Public Goods Institutions, Human Capital, and Growth: Evidence from German History*

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Abstract

What are the origins and consequences of the state as a provider of public goods? We study public goods provision established through new laws in German cities during the 1500s. Cities that adopted the laws subsequently began to differentially produce and attract human capital and to grow faster. Legal change occurred where ideological competition introduced by the Protestant Reformation interacted with local politics. We study plagues that shifted local politics in a narrow period as sources of exogenous variation in public goods institutions, and find support for a causal interpretation of the relationship between legal change, human capital, and growth.

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1 Introduction

For much of history states have provided minimal public goods and have instead served the objectives of elites (Tilly 1975; 1985; North 1990). A significant literature suggests that military conflict has provided one path to the provision of public goods, with states developing to provide defense (Besley and Persson 2011; Dincecco and Prado 2012; Gennaioli and Voth 2015). However, states today provide of broad set of public goods, including education, insurance, and health care. What are the origins and impacts of the state as a provider of non-defense public goods? And what do the short- and the long-run impacts indicate about how public goods provision may become a stable equilibrium?

In this paper we study the transition from a low public goods equilibrium to a high public goods equilibrium, using evidence on historic German cities. In the 1500s, some cities adopted laws that led to greater public goods provision. These laws expanded welfare provision, set up public education, and made secular city governments responsible for provision. The aim of education was largely political: schooling was designed to produce administrators to ensure the efficient provision of services and the stability of high public goods provision as an equilibrium.[1] The laws that formalized these changes were called church ordinances (*Kirchenordnungen*). We refer to these laws as "public goods" institutions, as distinct from economic institutions securing property rights or political institutions governing the selection of rulers.[2]

Shocks to religious and political competition drove the adoption of public goods institutions. The introduction of religious competition during the Protestant Reformation was a global shock to politics in central Europe. The collapse of the ideological monopoly of the Catholic Church created an opening for political entrepreneurs, who criticized the Catholic Church as a rent-seeking organization. These entrepreneurs developed the innovations in local public goods institutions that we study. The adoption of these

[1] Strauss (1988; p. 202) observes that, "little encouragement was given in the pupil's formal education to individual Bible reading. Most school plans make no mention of it at all." While Bible reading was not central, ideological persuasion was a key aim, as in contemporary settings (e.g. Cantoni et al. 2017).

[2] Acemoglu, Johnson, and Robinson (2005a) highlight the importance of economic institutions that facilitate factor accumulation. The institutions we study directly promoted the accumulation of human capital and may have indirectly promoted property rights through their influence on the quality of administration (North 1990; Gonzalez de Lara, Greif, and Jha 2008). The institutions we study expanded the provision of pure public goods and services which are arguably quasi-public or even private goods, such as education. We use "public goods" as broad label.

institutions reflected local politics, and was driven by popular political movements, not by local elites (Cameron 1991).[3] Local shocks that lowered the relative price of political action by citizens interacted with the global shock of the Reformation.

We describe the new public goods institutions, document shocks that generated variation in which cities adopted, and trace the consequences for human capital and growth. These changes occurred in only some Protestant cities. We find that public goods institutions, and not the adoption of Protestantism, explain economic development.

We focus our investigation on the mechanism leading from the adoption of church ordinances to long-run growth. Adoption marked a change in the political and institutional equilibrium. This change was secured by a shift in the formation and allocation of human capital towards public goods provision targeting education and welfare, and an increase in the quality of local administration. In the long run, adoption had unintended positive consequences for human capital in the business sector and city growth.

We also investigate the political mechanism that led to the adoption of church ordinances. The Reformation lowered the relative price of political action globally by introducing competition in the market for religion. However, political change remained costly. In the early 1500s, local plagues interacted with the global effects of religious competition. Plagues weakened incumbent elites and increased demand for public goods institutions. We use plague shocks as a source of exogenous variation in the adoption of church ordinances.

Our analysis generates specific predictions about the consequences of adopting church ordinances.[4] First, the formation and the migration of upper tail human capital are expected to respond to ordinances. Second, we expect short-term increases in human capital to be in targeted sectors: public administration, education, and the reformed church. Third, we expect that improvements in administration and public goods will generate human capital spillovers to other sectors, including the business sector. Fourth, we expect that these direct and indirect effects will lead to city growth. Fifth, we predict that these effects are driven by local variation in adoption, not the informal diffusion of Protestantism or regional factors.

Our analysis also generates specific predictions about the political process leading to

[3]These movements developed without the initial support from economic elites, city councils, or territorial rulers (Dickens 1982; Scribner 1979; Schilling 1983). We provide detail below.

[4]When we discuss the impact of church ordinances, we refer not simply to the legal documents but to the new political and institutional equilibrium they represent. The ordinances are indicators of the change in the political equilibrium towards public goods, which was reinforced by education.

the adoption of ordinances, including the role of the plague. First, we expect religious and political competition to be higher in cities that adopted and where plagues struck in the early 1500s. Second, we expect local plague shocks to predict adoption, even within territories and principalities. Third, we expect plagues in the early 1500s to have a unique relationship with long-run growth, due to the political implications of plagues following the global introduction of religious competition. This prediction corresponds to a test of the exclusion restriction when we use plague as an instrumental variable.

To test our predictions about the consequences of adoption for human capital and growth, we assemble data on city-level church ordinances, upper tail human capital, and city populations. We collect information on cities with church ordinances from Sehling (1902-2016). We construct microdata on upper tail human capital between 1420 and 1820 from the *Deutsche Biographie*, which is the definitive biographical dictionary of economic, cultural, and political figures in Germany (Hockerts 2008). We use data on city population from Bairoch, Batou, and Chèvre (1988).

In the data, we show that upper tail human capital increased in response to the adoption of church ordinances, as predicted. The initial response was in the public goods sectors targeted by the church ordinances: administration, education, and religion. Consistent with our predictions, we document that positive human capital spillovers into the business sector emerged with a lag. Finally, we show that adoption increased long-run city growth.

We use a difference-in-differences identification strategy to document the causal impact of the adoption of ordinances on upper tail human capital. We further show that the immediate increases in upper tail human capital were in targeted sectors, and that positive spillovers into the business sector emerged with a lag. We examine both the migration and the local formation of human capital. We find similar results, but that migration responses were more immediate, reflecting the manner in which upper tail administrators and educators moved to cities with ordinances to help establish the new political equilibrium. These responses hold controlling for whether or not a city adopted Protestantism; when we compare cities in the same region; and thus when we account for potential spatial variation in the diffusion of Protestantism similar to Becker and Woessmann (2009). This evidence is consistent with our prediction concerning the role of *local* factors and distinguishes our results from prior research. The effects we document hold controlling for underlying city-specific trends and

persist through later shocks such as the Thirty Years War (1618-1648). Our results thus indicate that the new equilibrium promoted economic opportunity and mobility.[5]

To study the growth effects of public goods institutions, we examine two long-run outcomes: population and human capital intensity. We show that cities that adopted church ordinances grew significantly larger and more human capital intensive by 1800.[6] To address questions about potentially selective adoption, we use plague outbreaks in the early 1500s as an instrumental variable (IV). We find large effects of public goods institutions studying variation in adoption induced by the quasi-experimental short-run variation in plague.

To test our predictions about the political process that led to adoption, we examine data on plagues, religious and political publications, and church ordinances. We use publication data to measure ideological competition. Political and religious competition increased where plagues struck in the early 1500s, and where ordinances were subsequently adopted, as predicted. Cities hit by plagues were more likely to adopt ordinances, even conditional on underlying plague trends. We show that plagues in the early 1500s have a unique relationship with long-run growth, supporting the exclusion restriction and reflecting how plagues in the Reformation era shifted the relative price of political action. We also show that plague shocks varied locally, hitting only some cities in a region, and that there were no trends in plagues overall or towards cities with trade network advantages. We provide historical evidence on how the plague shifted the relative price of political action in the early 1500s.

We present panel estimates that support our cross-sectional IV analysis of long-run city population outcomes. We show that the adoption of ordinances explains which towns *became* cities with population records and that the probability that city population is in the upper end of the distribution.

Our research contributes to the literature on the historic development of public goods provision.[7] Prior research has emphasized two political processes that drove increases in

[5]Opportunity and mobility may have been promoted through several channels. For example, by creating a more educated workforce, expanding opportunity for talented individuals from modest backgrounds, or limiting corruption and ensuring high quality administration of economic institutions regulating production, exchange, and property rights.

[6]We study long-run outcomes cross sectionally because city population is not consistently observed before the adoption of ordinances for many cities, limiting our ability to study growth in the panel as we discuss below. Around 1800, institutional changes and educational reforms impacted economic development in German cities (Strauss 1978; Acemoglu et al. 2011). We thus focus on city growth up to 1800.

[7]On contemporary public goods, see e.g. Banerjee and Iyer (2005) and Martinez-Bravo et al. (2014).

the provision of public goods: (1) extensions of democratic voting rights and (2) military competition between states.[8] The expansion of democratic voting rights has been tied to the growth of social spending at the national level starting in 1700s and 1800s (e.g. Lindert 2004). Prior research has also studied how military competition led to the introduction of mass public education starting in the 1800s (Aghion, Persson, and Rouzet 2012) and to the development of larger bureaucracies, tax authorities, and "state capacity" at the national level (Gennaioli and Voth 2015; Dincecco and Katz 2016; Dincecco and Prado 2012). Our research differs on two key dimensions. First, we study a different political process. We investigate how the collapse of the ideological monopoly of the Catholic Church shifted politics globally, and how this global shift interacted with local shocks to lower the relative price of political action. This process was not driven by military competition or by changes in voting rights. Second, our research documents the central role of human capital in the expansion of public goods provision. Political change drove responses in human capital formation, but human capital was itself allocated as a key input into sustaining a government that provides public goods.[9] In addition, we study pioneering changes in city-level public goods provision that provided models for provision at the national level.[10]

Previous research ties upper tail human capital to growth in history, focusing on human capital in science and business and taking the distribution of human capital as given (Mokyr 2009; Meisenzahl and Mokyr 2012; Squicciarini and Voigtländer 2015).[11] We advance the literature by studying political changes designed to produce an administrative elite. The immediate objectives were largely political: as Strauss (1988; p. 203) observes, "Preparing pupils for high office was always the salient objective." We show that political change first

[8]Competition among cities aiming to attract foreign merchants shaped public goods provision in other settings (Gelderblom 2013). The political process we study was not driven by competition over merchants, and acted on different margins from those Gelderblom (2013) examines, as discussed below.

[9]Our findings relate to Gonzalez de Lara, Greif, and Jha (2008) on self-reinforcing constitutions, and Greif and Iyigun's (2013) argument tying social insurance to reduced conflict and increased innovation.

[10]Hamm (1994) documents that change in public goods institutions in German cities provided models for, and influenced, later territory-level institutional changes, notably in education. Jütte (1994; p. 108) observes that city-level changes in welfare provision in the 1500s also, "paved the way for the development of a new social policy that favored secular systems of poor relief," and that their legacy was, "still visible when German towns embarked on another major reform of poor law at the end of the eighteenth century."

[11]An exception closer to our study is Cantoni and Yuchtman (2014), who study the impact of exogenous university foundation on local market institutions (market rights) across German towns during late 1300s. Cinnirella and Streb (2017) and Maloney and Valencia Caicedo (2017) examine upper tail human capital during the industrial revolution, and Serafinelli and Tabellini (2017) study clusters of creative human capital through European history.

led to increases in upper tail human capital in occupations supporting the provision of public goods, and later to increases in business. Narrative evidence documents that the quality of administrators rose, the beliefs of elites shifted, and innovations in monitoring worked to ensure a relatively efficient provision.[12] The sequence of changes we investigate indicates that human capital allocated to public goods provision had positive spillovers on growth.

Our study also relates to the literature on Protestantism and economic development.[13] Weber (1904) famously argued that Protestantism influenced development through its impact on norms and preferences, especially relating to saving and work. Becker and Woessmann (2009) argue that Protestantism fostered literacy, and that regional differences in literacy induced by religion led to differences in development during the industrial revolution. Rubin (2017) argues that Reformation influenced growth by reducing the power of religious elites, and focuses on shifts in religion and political economy at the country level. In contrast with Becker and Woessmann (2009), we study public goods institutions and their effect on upper tail human capital and pre-industrial growth, emphasizing differences across Protestant cities in the same regions.[14] Unlike Rubin (2017), we study changes in the political and institutional equilibrium at the city-level, where political pressure from below drove expansions in the provision of public goods. We find that local changes in public goods institutions drove differences in long-run growth, highlighting the importance of the interaction between religious competition and local political economy.[15]

Our contribution can also be situated relative to the literature on institutions and growth. Institutions that constrain executive authority and protect property rights contribute to development (Acemoglu, Johnson, and Robinson 2005a; 2001; North and Weingast 1989). We document the positive growth impact of political and institutional changes that promoted non-defense public goods and human capital formation.[16]

[12] See Hammond (2011), Strauss (1978), and Whaley (2012). We thus also contribute to research on the allocation of talent (e.g. Murphy, Shleifer, and Vishny 1991). In related research, Rauch and Evans (2000) find that meritocratic recruitment of government bureaucrats lowers country risk in contemporary settings.

[13] For a general review of Protestantism and development, see Becker, Pfaff, and Rubin (2016).

[14] Our results support Cantoni's (2015) finding that the adoption of Protestantism did not shift city growth.

[15] In complementary research, Cantoni, Dittmar, and Yuchtman (2018) find that Protestant princes, who were insulated from political pressure from below, reallocated resources to the construction of administrative buildings and palaces, but did not foster the construction of schools and hospitals. As Whaley (2012; p. 264) observes, Protestant princes appropriated monastic wealth and were "reluctant to part with it."

[16] We also present the first research to document the causal impact of institutions supporting local public goods on outcomes in targeted municipalities, to the best of our knowledge. Acemoglu, García-Jimeno, and Robinson (2015) study the *spillover* impacts of state capacity on outcomes across localities in Colombia.

2 The Expansion of Public Goods Provision

In this section we first describe the legal changes that expanded public goods provision at the city level. We then describe the political process that led to the adoption of public goods institutions, including the role of plague outbreaks as shocks to local politics.

2.1 Legal Institutions Supporting Public Goods

We study the passage and role of municipal laws that expanded government provision of public goods. We use the adoption of a city-level "church ordinance" law (*Kirchenordnungen*) in the early 1500s as our measure of "public goods institutions." These laws contained interlocking requirements that (1) expanded the set of services provided by the state, (2) transferred control of existing service provision from the Catholic Church to secular state authorities, and (3) institutionalized anti-corruption and oversight rules to improve quality and prevent the misappropriation of public and church finances (Strauss 1978).[17] These laws expanded the role of the secular state (Strauss 1988; Jütte 1994). Hamm (1994; p. 196) observes that the legal jurisdiction over public goods changed and that secular magistrates took control of "institutions that particularly concerned the laity," including schools, poor-houses, and hospitals.

The public goods institutions were designed to establish a new social order through their interlocking provisions. The ordinances contained provisions relating to education, social welfare, public morality, anti-corruption safeguards, as well as religion itself. Significantly, reformers targeted upper tail human capital in public administration as an input that would sustain the new political equilibrium and a government that provides public goods.

First, church ordinances established public education systems. Education was designed to produce secular administrators and churchmen, to ensure the efficient delivery of services and the persistence of the new equilibrium, consistent with Luther's demand for "men to govern" in his pamphlet *To the City Councillors* (1524). The laws "placed the supervision of all educational institutions firmly in the hands of. . . magistrates" (Strauss 1988; p. 193). In a 1528 church ordinance, Philip Melanchthon underlined that the institutions were designed,

[17] We examine variation in these legal institutions that coincided with changes in religious practices that were broadly shared across Protestant cities, including those that did not adopt institutional change in the 1500s, as discussed below.

"for raising up people who are skilled to teach in the church and govern in the world" (Strauss 1988; p. 196).[18] Institutions were also designed to support the *migration* of human capital, including the recruitment of talented schoolchildren: "Officials roamed the land looking for 'good minds' in town and village schools" (Strauss 1978; p. 178).[19] Schooling was compulsory, free for the poor, and promoted economic mobility.[20]

Second, the laws institutionalized social welfare provision. Municipal health care and poor relief featured prominently, reflecting Protestant views about the role of the state in the mitigation of suffering (Grell 2002).[21] "With the Reformation... medical care became a central ingredient of poor relief at least in cities ... Assistance might involve arrangements for a person to be admitted to a hospital, or for free medical care from physicians, surgeons, apothecaries, or midwives" (Lindemann 2010; p. 230). Welfare provision was combined with investigations of households to limit moral hazard (Jütte 1994; Kahl 2005; Hammond 2011).

Third, the laws include provisions regulating public morality. These provisions aimed to promote social order. They forbid gambling, drunkenness, prostitution, and similar offenses.

Fourth, anti-corruption provisions aimed to ensure the efficient allocation of public resources and political support. To reduce corruption, the laws instituted inspections ("visitations") of schools, including to audit *teacher* attendance, and introduced an audited "common chest" lock-box to ensure public and church finances were not misappropriated.[22] These provisions led to increases in both financial and human resources devoted to public goods provision, and represent a major instance of the rise of fiscal capacity.

The adoption of church ordinances marked a process of legal change and initiated new

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

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

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

[21]In contrast, Catholic theologians and statesmen rejected public participation (Roeck 1999).

[22]Anti-corruption safeguards, including the formal institutionalization of audits for public finances, were designed to reduce diversion. A concrete example of these innovations was the introduction of a "common chest." Wittenberg was a model: institutional change established an audited common chest in 1522, all church income was to be collected under one administration, these resources were to be used to provide care for the poor and sick and financial support to enable children of low-income parents to attend school and university (Sehling 1902-2016).

fixed investments in human capital and service provision (Hamm 1994; Strauss 1978). To show how church ordinances related to legal change and to fixed investments, we present data on the diffusion of jurisprudence and on the construction of schools and buildings used in administration and service provision. The publication of books on jurisprudence provides an indication of the diffusion of legal knowledge. In Figure 1 we show that publication of jurisprudence books was similar in cities that did and did not adopt church ordinances until the 1520s, and then diverged as cities adopting ordinances embarked on a process of legal and institutional change (panel A). Figure 1 also shows that church ordinances were also associated with subsequent differences in the construction of schools (panel B) and in the construction of buildings used in administration and social welfare provision (panel C).[23] Until the 1540s, these construction patterns were similar in cities that did and did not adopt church ordinances. In many cities that adopted institutional change, new schools and municipal services made use of former Catholic Church buildings (Ocker 2006). Hence new 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.

We provide an analysis of the church ordinances in our data, where we examine specific provisions in the ordinances and consider potential heterogeneity, in Appendix C.

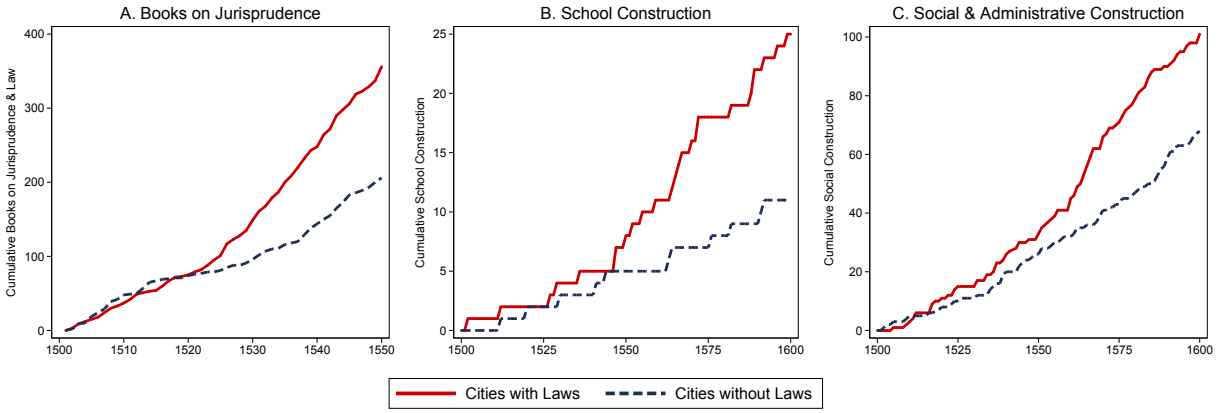
2.2 The Political Process

During the early 1500s, the introduction of religious competition interacted with local shocks to shift city politics.

The Reformation altered political competition across Germany. The circulation of Martin Luther's theses in 1517 led to the introduction of religious competition. The collapse of the religious monopoly of the Catholic Church provided an opening for political entrepreneurs. These entrepreneurs built on Luther's argument that the Catholic Church was a rent-extracting organization, and developed political arguments for lower cost provision of religious services and expanded provision of public goods (Cameron 1991). As a result, the

[23]This pattern of fixed investment is different from the pattern associated with the *territorial* adoption of Protestantism by princes. At the territory-level, the adoption of Protestantism is not associated with a significant shift towards school building and is associated with a large increase in the construction of palaces, reflecting (i) the seizure of Catholic Church assets by princes and (ii) princes' priorities (Cantoni, Dittmar, and Yuchtman 2018). Evidence on construction is assembled from the multi-volume *Deutsches Städtebuch*, which provides detailed histories urban development for of all cities in Germany.

Figure 1: Jurisprudence and Construction



This graph shows the cumulative number of (A) jurisprudence publications, (B) new school construction, and (C) administrative and social welfare construction in cities that did and did not adopt public goods institutions, measured by the passage of a church ordinance (cities with and without "Laws"). Publication and construction data are from the *Universal Short Title Catalogue* and the *Deutsches Städtebuch*, respectively, for the 239 German cities we study in this paper. We classify all new school construction as "School Construction" and all construction of buildings used by city governments and in non-education social welfare and service provision as "Social & Administrative Construction".

relative price of political action fell globally.[24] This price reduction reflected spiritual factors: notably, the reduced power of the threat of excommunication. It also reflected material factors: the willingness of temporal authorities to enforce Catholic doctrine declined.

Cities had characteristics that made them the centers of public goods expansion. Cities combined forms of jurisdictional autonomy and politically demanding middle classes that together made innovation feasible, and explain why political innovation appeared first and developed most in cities (Brady 2009; Ozment 1975). Hamm (1994; p. 201) observes: "Since the urban constitutions – unlike those of the territories – made the regimes susceptible to pressure from below, large groups of citizens could press successfully for religious changes and, for a time, influence the magistrates' will." To expand public goods provision, city governments adopted church ordinances.

The adoption of church ordinances depended on local politics. Citizens' movements drove city-level institutional change, and emerged without initial support from oligarchic city governments or territorial lords (Dickens 1979).[25] Cameron (1991; p. 240) observes,

[24]Religious competition changed the "market for salvation," in which the Catholic Church could threaten ex-communication. It also shifted the "market for legitimacy" in which rulers received religiously derived sanction for their rule in exchange for economic concessions. Rulers faced by competing religious elites did not enforce Catholic writ as before (Cantoni, Dittmar, and Yuchtman 2018).

[25]Princes' preferences and city elites are discussed further below and in Appendix E.

"As a rule neither the city patricians nor the local princes showed any sympathy for the Reformation in the crucial period in the late 1520s and early 1530s; they identified themselves with the old Church hierarchy. . . Popular agitation on a broad social base led to the formation of a 'burgher committee'." The constituency for institutional change came from citizens who were excluded from political power by oligarchic elites (Ozment 1975; Schilling 1983). While territorial princes did exert some influence over the process of institutional change, our results hold virtually unchanged when we study variation in institutions and outcomes across cities *within* the same region or principality (Sections 4 to 6).[26]

Narrative evidence indicates the importance of political competition. In Wittenberg, popular pressure drove the city council to defy the orders of the local prince and pass the first church ordinance in 1522 (Lindberg 1977).[27] In Augsburg, the patrician financial and political elite remained loyal to the Catholic church, but riots by artisans forced the city council to drop its policy of religious neutrality and led to legal change (Broadhead 1979; Johnson 2009). In Northern cities, such as Rostock, Stralsund, Greifswald, Lübeck, and Braunschweig the push for legal change was led by citizens excluded from power and had a *coup d'état* quality (Cameron 1991). In Zwickau, Lutheran publications were printed in 1523; the city council unsuccessfully attempted to suppress protests in 1524; the Reformation was adopted in law in 1529 (Scribner 1979). Further discussion is provided in Appendix E.

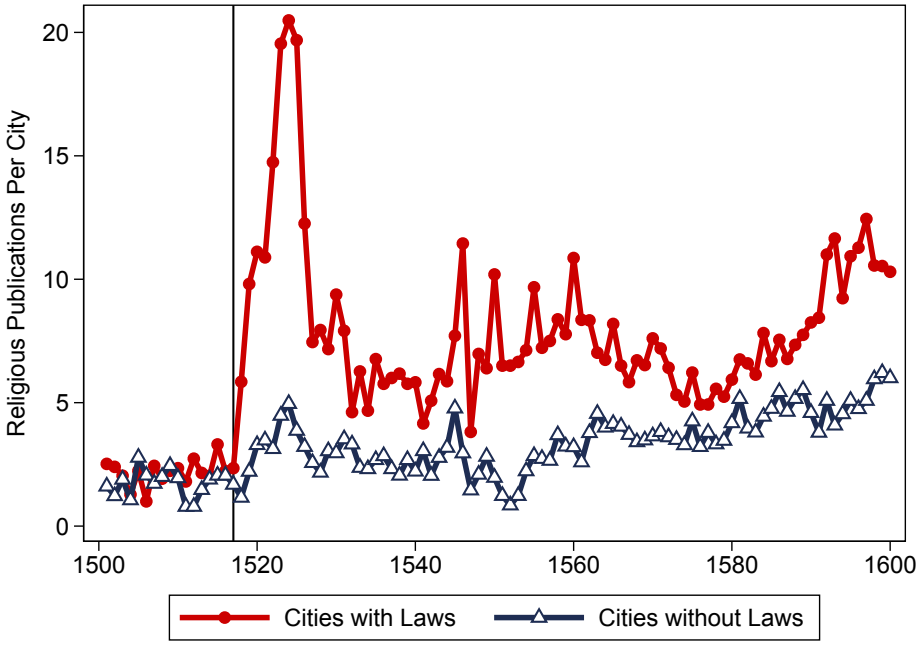
Religious printing provides unique, high frequency evidence on religious and political competition. The number of religious publications is an indicator of competition in the space of ideas and, more broadly, in religion and politics in the 1500s (Pettegree 2005; Dittmar and Seabold 2017). Figure 2 shows that religious publishing was similar in cities that did and did not adopt church ordinances before the Reformation, and diverged in the first years of the Reformation, before church ordinances began to be adopted starting in 1522. This evidence, and our investigation of economic outcomes before and after the adoption of church ordinances, invite the question of why some cities adopted and whether there are sources of exogenous variation in adoption.[28]

[26]The changes in politics and monitoring persisted after the early 1500s: "the 'common man' in the cities retained his capacity to challenge the magistrates" (Whaley 2012; p. 249).

[27]The city council passed the Wittenberg ordinance after it was unable to control popular protest (Scribner 1979). At the time, Luther was under house arrest at Wartburg castle, 200 kilometers away.

[28]Figure 2 examines the subset of cities with printing presses. Below we consider how religious and political printing increased during the Reformation in response to plague shocks. These effects reflected increased

Figure 2: Religious Publishing



This graph shows the mean number of religious publications in cities that did and did not adopt church ordinances ("Laws"). Data on religious publications are from the *Universal Short Title Catalogue*.

Plague outbreaks in the early 1500s delivered exogenous shocks to the political equilibrium. These shocks cut across other demand-side differences between cities. We use the timing of city-level plague shocks that interacted with the global shift in political competition as a source of exogenous variation in order to establish comparisons that do not reflect cultural and economic differences across cities that may have influenced demand for institutional change and directly influenced economic outcomes.

Plague outbreaks in the early 1500s influenced city politics in several ways. Plague outbreaks led to the breakdown of civic order and discredited city elites, who often fled (Dinges 1995; Isenmann 2012). Plagues also changed the composition of the population: in years following plagues new migrants typically arrived to replace the dead and acquired political rights with several year lag (Isenmann 2012). In addition, experience with plague shifted the salience of public goods provision. The public goods institutions developed by political entrepreneurs incorporated provision for healthcare, including care during plagues (Bugenhagen 1885; Grell 2002). These factors help clarify why the probability of institutional

ideological competition in cities that were already printing and increased competition in cities that had not previously been printing, as discussed below.

change increased for cities exposed to plagues in the early 1500s.[29] Given that religious publications provide a measure of political competition, it is natural to wonder whether they respond to plague shocks in a manner consistent with the narrative evidence. We show that plague shocks do drive religious publications below (Section 6.3).

Several factors explain why not all Protestant cities adopted public goods institutions. A large body of evidence documents that local elites did not initially favor institutional change (Cameron 1991) and that citizen mobilization and disorder drove city councils to adopt institutional change (Dickens 1982). In some cities, Protestantism diffused but political compromises between elites and the population prevented institutional change. For example, in Bautzen the Catholic bishop signed a contract agreeing to share the use of the Cathedral (this contract still governs the use of church space in 2018). Bautzen became a Protestant city but institutional change did not occur. Other cities adopted Protestantism under the influence of a territorial lord, but without a popular mobilization for institutional change.

While there were some *territorial* Catholic interventions in the counter-reformation that adopted innovations from the institutions we study (Strauss 1978), the consensus among historians is that the provision of public goods in Catholic Germany was limited (Roeck 1999) and that the presence of interventions in Catholic cities that borrowed from and responded to the institutions we study will lead us to conservatively underestimate the impact of church ordinances (Grell 2002).

2.3 Measuring Public Goods Institutions

We measure public goods institutions by the passage of a church ordinance in the 1500s. We consider cities that adopted this institutional change that persisted as "treated." In contrast, cities that remained Catholic or that became Protestant without changing legal

[29] We provide detailed discussion of these dynamics in Section 6. These variations in demand for institutional change are orthogonal to variations in the supply of Protestant ideas. Historians (Eisenstein 1980; Brady 2009) and economists (Rubin 2014) argue that the printing press shifted the supply of Reformist ideas. Recent research argues that the diffusion of Protestantism was driven by competition in the use of printing technology (Dittmar and Seabold 2017). Our research is fundamentally differentiated from this work in that it studies a larger set of cities, including more cities without printing, and examines shocks that were orthogonal to the supply-side shocks the research on printing has examined. Every printer death documented in Dittmar and Seabold (2017) occurred outside of plague outbreaks studied here. Similarly, our results hold when we study within-principality variation or control for distance from Wittenberg, which Becker and Woessmann (2009) identify as a determinant of the diffusion of Protestant ideas.

institutions are "untreated." While a small number of cities experienced religious changes after adopting these laws, our methodology provides a robust measure of institutional change. A small number of cities that experienced later re-Catholicization preserved the public goods institutions established by their church ordinances, however our findings are robust to excluding these cities (see Appendix for discussion). A small number of cities where institutional change was reversed after a few years are considered untreated in our baseline analysis, which considers cities with new institutions that persisted to 1600 as treated. However, we obtain virtually identical results when we conduct an intent-to-treat analysis including the few cities where institutional change was reversed in the early 1500s.

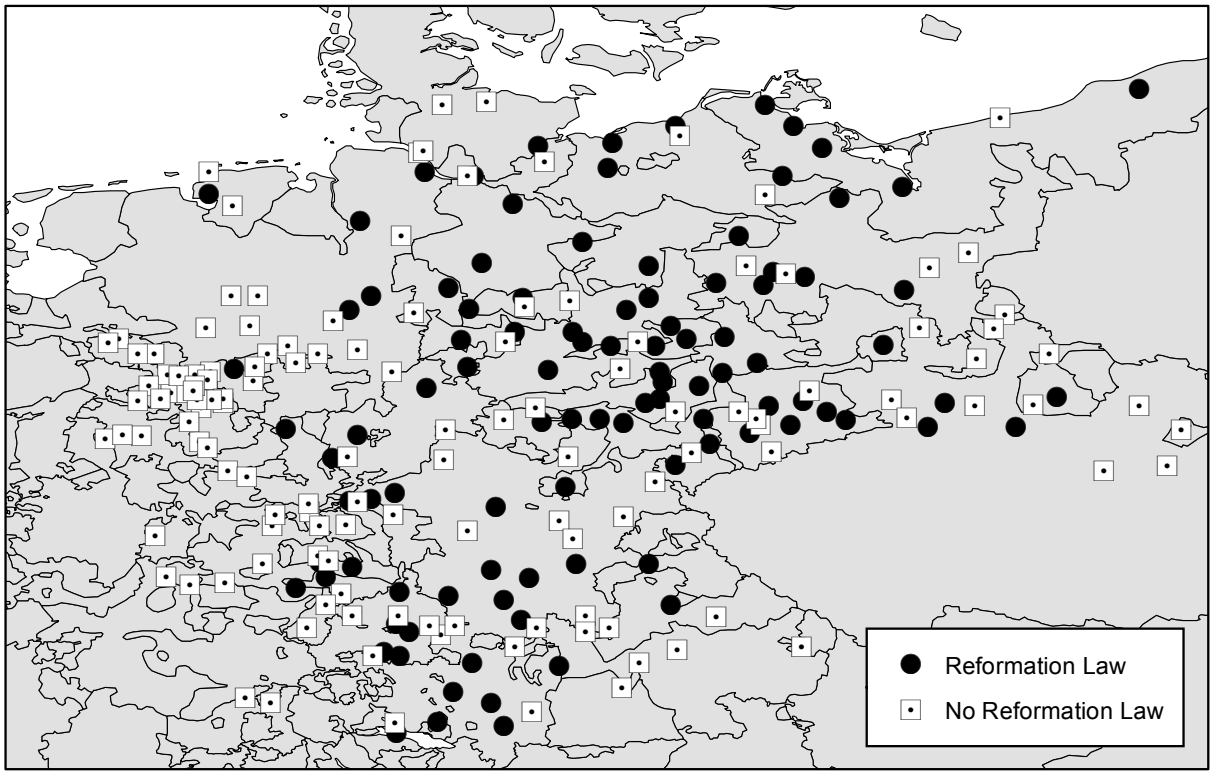
We provide discussion of the institutions and our classification in Appendix A and illustrative examples here. Bautzen is an example of a Protestant city which did not adopt institutional change, as discussed above (see also Speer 2014). Augsburg and Amberg are examples of cities where the institutions of the Reformation were established and persisted despite forms of re-Catholicization. Augsburg adopted the institutions of the Reformation 1534-1537, but was assigned a Catholic city council by the emperor in 1548. The council did not attempt to re-Catholicize the population and access to city services remained open to Protestants (Stein 2009). Amberg passed a church ordinance 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).

Figure 3 maps the cities in our data and illustrates the variation in which cities adopted church ordinances. Figure 4 shows the cumulative share of cities that had adopted as of each year. Most cities passed their first law by 1545. In 1546, the Schmalkaldic War broke out between Protestant and Catholic princes, largely arresting diffusion. The Augsburg Settlement (1555) established a new political equilibrium.[30] City level institutional change largely ended in 1555.

We focus on city-level ordinances for several reasons. City-level institutional change was salient because city governments had significant state capacity and were subject to pressure from citizens in ways territorial rulers were not (Cameron 1991; Hamm 1994). In the data, we

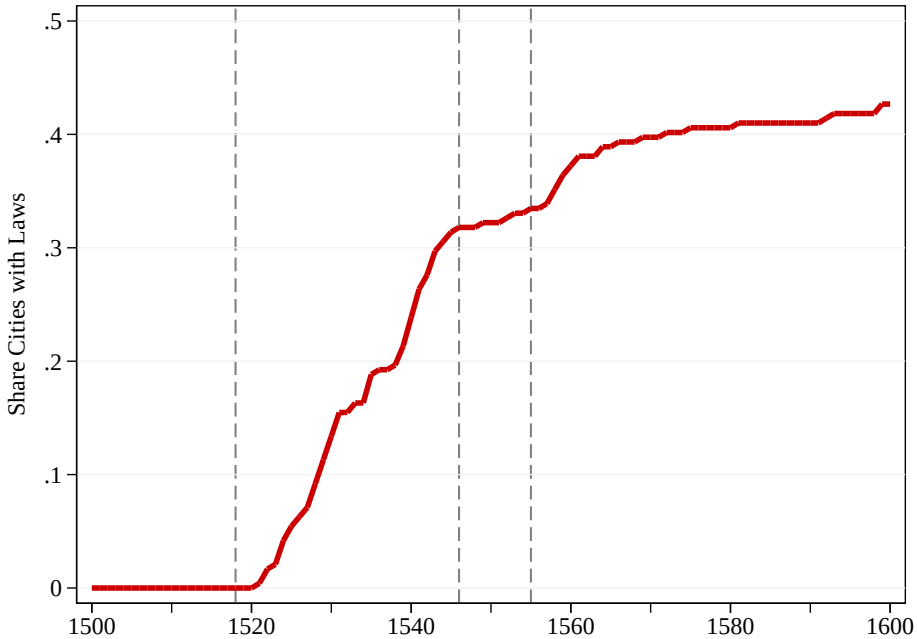
[30]The settlement included a provision, *cuius regio, eius religio,* which allowed local rulers to dictate the religion in their realm, but maintained a complicated set of exceptions for cities where magistracies and offices were to be shared and largely respected facts on the ground (Dittmar and Seabold 2017).

Figure 3: Cities and Institutional Change



This map shows cities with and without institutional change, measured by the adoption of a church ordinance ("Reformation Law"). This map also displays historic territories from Nüssli (2008).

Figure 4: The Share of Cities Adopting Institutional Change



This graph shows the share of cities with institutional change, measured by the adoption of a church ordinance ("Share Cities with Laws"). Dashed lines mark the spread of Luther's ideas in 1518, the Schmalkaldic War of 1546, and the Peace of Augsburg in 1555.

find that the within-principality difference in outcomes between "treated" and "untreated" cities is virtually identical to the difference across the entire population of cities.[31]

3 Data

Definition of Sample – We focus on institutions and outcomes in 239 German-speaking cities with population observed in 1800 in Bairoch, Batou, and Chèvre (1988) and information on the non-institutional diffusion of Protestantism recorded in Cantoni (2012).[32]

Legal institutions of the Reformation – Our principal data source on Protestant church ordinances is the multi-volume collection *Die evangelischen Kirchenordnungen des XVI. Jahrhunderts* (Sehling 1902-2016).[33] We review the text of the laws and manually code which cities adopted institutional change.

Upper Tail Human Capital – Data on individuals with upper tail human capital are from the *Deutsche Biographie* (Bayerischen Akademie der Wissenschaften 2015). The *Deutsche Biographie* is a project of the Historical Commission of the Bavarian Academy of Sciences (Reinert, Schrott, and Ebneth 2015), provides the most definitive record of upper tail human capital individuals in German history, and was designed to provide comprehensive coverage across regions and religions (Hockerts 2008). We identify over 3,000 individuals born in or migrating to our baseline set of cities from 1430 to 1819. We classify individual occupations in six principal sectors: (1) government; (2) church; (3) education; (4) business; (5) arts; and (6) medicine.[34] We provide detailed discussion of the nature and construction of the *Deutsche Biographie*, and our classification of occupations, in Appendix A.[35]

[31] This shows that important variation "lives" at the city-level and not the territorial-level. Consistent with this, we find differences in school construction by city-level ordinance, while Cantoni, Dittmar, and Yuchtman (2018) find no evidence that school construction was higher in Protestant territories as a whole.

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

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

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

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

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

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

City Characteristics – Data on the hometowns of individual students receiving university degrees from 1398 to 1517 are from Schwinges and Hesse (2015).[36] Data on books printed in each city are from the *Universal Short Title Catalogue*. Data on market rights and city incorporation are from Cantoni and Yuchtman (2014). Data on navigable rivers, the ecclesiastical status of cities, monasteries and mendicant orders, and the diffusion of Protestantism as the dominant city-level religion are from Cantoni (2012). Data on city institutions governing electoral participation, public (*Burgher*) representation in assemblies, and constitutions securing guild representation on city councils are from Wahl (2016; 2017). We study cities within 29 territories mapped by Nüssli (2008), and within 75 principalities recorded by the *Deutsches Städtebuch* as of circa 1500 (see Appendix A for details).

4 Public Goods Institutions and Human Capital

4.1 Motivation

In this section we test our hypotheses on the relationship between the adoption of public goods institutions and upper tail human capital using a difference-in-differences identification

[36] The *Repertorium Academicum Germanicum* project (Schwinges and Hesse 2015) provides individual-level data on graduates from historic German universities. These data are only available through 1550 due to the nature of the underlying sources. Because long-run data on university degree recipients after institutional change are not available we do not study degrees as outcome in this paper.

Table 1: Summary Statistics on Upper Tail Human Capital

	Cities with Law			Cities without Law			Difference
Upper Tail Human Capital	N	Mean	Sd	N	Mean	Sd	HL Statistic
Locally Born Pre-1520	103	1.02	3.37	136	0.20	0.64	0.00
Locally Born Post-1520	103	15.01	26.61	136	3.59	7.07	3.00***
Migrants Pre-1520	103	0.41	0.86	136	0.13	0.67	0.00
Migrants Post-1520	103	8.49	19.04	136	1.96	4.94	1.00
Total Pre-1520	103	1.42	3.89	136	0.32	1.13	0.00
Total Post-1520	103	23.50	44.89	136	5.54	11.55	5.00***

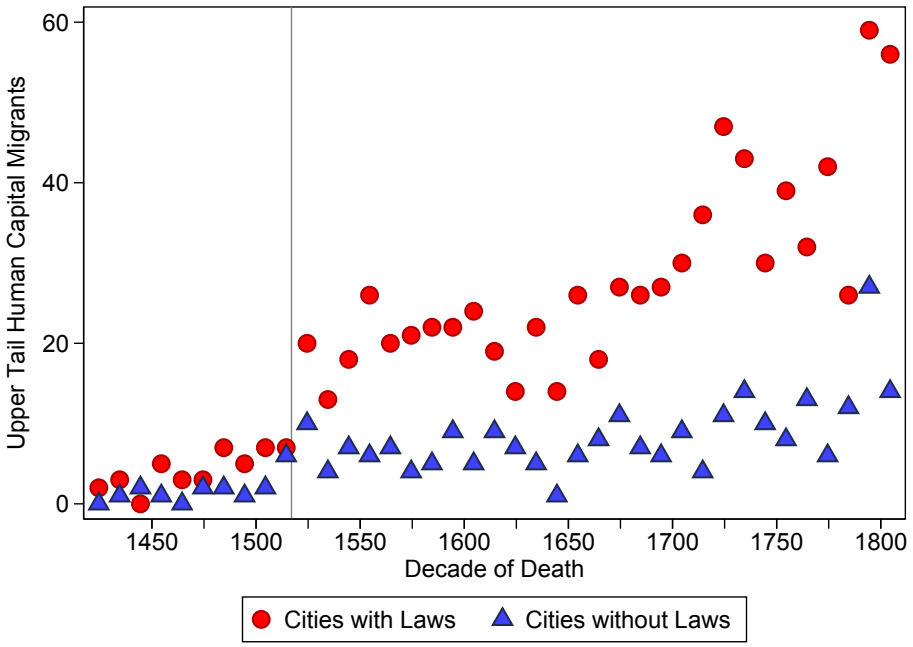
Upper tail human capital is measured by the number of people observed in the *Deutsche Biographie*. Locally born are people born in a given city i. Migrants to any given city i are individuals born in some other location j who died in city i. The last column presents the Hodges-Lehmann non-parametric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

strategy. We use the adoption of city-level church ordinances (laws) as a measure of public goods institutions. We show that the effects were most immediate in the sectors targeted by reformers – government, education, and church. We find lagged spillover effects on the business sector. We distinguish migration and local formation, motivated by the historical evidence on reformers' efforts to promote migration of talented school children and on the migration of educated elites who themselves participated in the establishment of the political equilibria. Observed migration reflects both human capital and opportunities.

To study the migration and formation of upper tail human capital we collect biographical data on all individuals in the *Deutsche Biographie* who either were born in or migrated to the 239 cities in our data between 1420 and 1820. We classify as a migrant any individual who died in a given city, but was born in some other location. Observed migrants thus comprise both individuals who migrated as adults and those who were identified as promising students and offered school places in cities while minors. We classify as local formation individuals born in a given city in our data. Table 1 presents summary statistics and shows significant differences in the period after institutional change.

Our econometric analysis is motivated by Figure 5, which plots the raw data and shows a sharp jump in migration into cities that adopted new laws (i.e. church ordinances) in the 1520s. Figure 5 shows that cities with and without these laws were attracting similar numbers of migrants and had similar trends before the Reformation, that there is a sharp

Figure 5: 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 church ordinances ("Laws"). Migrants are identified as people living and dying in town i but born in some other location j. The vertical line is at 1517, the year Luther's theses began circulating.

and persistent increase in migration observed in cities with laws starting in the 1520s, and that the evolution in the number of migrants in cities without laws does not change during the Reformation.[37] Significantly, cities with laws overwhelmingly attracted these migrants from smaller towns, not from cities without laws. Net migration from untreated to treated cities was virtually zero as shown in Appendix B.[38] We also find that our results are not explained by other institutional changes, examining shifts in city-level institutions governing (1) participatory elections for city councils, (2) citizen representation on assemblies, and (3) formal representation for guilds on city councils, as discussed below.

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

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

4.2 Results

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

$$\begin{aligned} People_{it} = \beta_0 + \beta_1(Post_t \times Law_i) + \beta_2(Post_t \times Trend_t \times Law_i) + \\ \beta_3(Post_t \times Protestant_i) + \beta_4(Post_t \times Trend_t \times Protestant_i) + \\ \phi_i Trend_{it} + \delta_{t,region} + \epsilon_{it} \end{aligned} \tag{1}$$

Here the parameters of interest are β_1 and β_2, which capture the level and trend shift, respectively, for cities that adopted church ordinances ("*Law*") in the post-period. Parameters β_3 and β_4 test for any level or trend shift associated with the adoption of Protestantism. The ϕ_i are city-specific time trends. The $\delta_{t,region}$ are region-time-period fixed effects (e.g. all cities in Saxony in a given period). We present specifications in which the regions are 29 territories from Nüssli (2008) and specifications in which the regions are 75 principalities (jurisdictions) recorded in the *Deutsches Städtebuch*. We normalize the linear trend to be time 0 in the period before treatment (i.e. 1470-1519).[39]

Table 2 reports our estimates. In Panel A, the outcome is the log of migration plus one. We examine upper tail human capital migration in fifty year periods from 1420 through 1819.[40] The post period begins 1520. We test for and find significant level shifts in Columns 1-5. Following the adoption of church ordinances the migration of upper tail human capital rises by 0.22-0.30 log points in treated cities. These effects hold when we control for the adoption of Protestantism, which is itself not a significant factor (column 2).

One might wonder whether the adoption of church ordinances reflected the geographic diffusion or intensity of Protestantism operating through distance to Wittenberg (Becker and Woessmann 2009) or characteristics shared by cities at the regional level. We address this question by studying variation within region-year (column 3) and within principality-year (column 4), and find consistent results. We study variation in adoption of church

[39] We present robustness exercises on considering the treatment time period and the outcome measure in the Appendix.

[40] We examine the data in 50 year periods to account for the fact that in the majority of observations we do not know the precise year or decade of migration.

Table 2: Public Goods Institutions and Upper Tail Human Capital

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Panel A: Log Migration										
	Complete Data: All Cities 1420-1819				No Religious Heterogeneity	Complete Data: All Cities 1420-1819				No Religious Heterogeneity
Post × Law	0.22***	0.22***	0.26***	0.26**	0.30**	0.27***	0.33***	0.38***	0.35***	0.36***
	(0.06)	(0.06)	(0.07)	(0.11)	(0.15)	(0.07)	(0.07)	(0.08)	(0.10)	(0.13)
Post × Trend × Law						0.07**	0.09***	0.12***	0.11**	0.09
						(0.03)	(0.04)	(0.04)	(0.06)	(0.07)
Post × Protestant		-0.02	0.00	-0.05			-0.19***	-0.12	-0.13	
		(0.05)	(0.06)	(0.10)			(0.07)	(0.09)	(0.15)	
Post × Trend × Protestant							-0.08*	-0.08	-0.14	
							(0.05)	(0.06)	(0.11)	
Cities	239	239	239	234	167	239	239	239	234	167
Observations	1912	1912	1912	1872	1336	1912	1912	1912	1872	1336
R^2	0.59	0.59	0.66	0.72	0.73	0.65	0.65	0.71	0.76	0.80
Panel B: Log Formation										
	Complete Data: All Cities 1420-1819				No Religious Heterogeneity	Complete Data: All Cities 1420-1819				No Religious Heterogeneity
Post × Law	0.33***	0.32***	0.37***	0.33***	0.33**	0.24***	0.26***	0.23***	0.24**	0.24*
	(0.06)	(0.07)	(0.07)	(0.11)	(0.15)	(0.06)	(0.07)	(0.08)	(0.10)	(0.12)
Post × Trend × Law						0.10***	0.11***	0.13***	0.08	0.07
						(0.04)	(0.04)	(0.05)	(0.06)	(0.06)
Post × Protestant		0.03	0.01	-0.05			-0.07	-0.12	0.03	
		(0.06)	(0.08)	(0.13)			(0.07)	(0.09)	(0.16)	
Post × Trend × Protestant							-0.03	0.07	0.06	
							(0.05)	(0.05)	(0.08)	
Cities	239	239	239	234	167	239	239	239	234	167
Observations	1912	1912	1912	1872	1336	1912	1912	1912	1872	1336
R^2	0.57	0.57	0.64	0.71	0.73	0.67	0.67	0.72	0.78	0.84
Controls										
City FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Trend	Yes	Yes	No	No	No	No	No	No	No	No
City-Specific Trend	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Region × Year FE	No	No	Yes	No	No	No	No	Yes	No	No
Principality × Year FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes

This table presents regression estimates documenting the relationship between the adoption of public goods institutions and the migration and formation of upper tail human capital. Panel A studies the migration of upper tail human capital, measured by the log of the number of upper tail human capital migrants plus one. Columns 1-4 and 6-9 study the complete data 1420-1819. Columns 5 and 10 restrict analysis to religiously homogeneous principalities. Public goods institutions are measured by the adoption of a church ordinance ("Law"). The variable "Post × Law" is the interaction between indicators for the post-1520 period and for ever-treated status. "Post × Trend × Law" is the interaction between treated, post, and the linear time trend. "Protestant" is an indicator for Protestant cities from Cantoni (2012). Time periods are 50 year intervals: starting with 1420-1469 and ending with 1770-1819. Panel B studies the formation of upper tail human capital outcome in a city-period, measured by the log of the number of home-grown upper tail human capital individuals plus one. Region-year fixed effects interact region and year fixed effects, using 29 regions from Nüssli (2008). Principality-year fixed effects use 75 principalities (jurisdictions) recorded in the *Deutsches Städtebuch*. Standard errors in parentheses are clustered at the city level. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***".

ordinances and religion within regions and principalities as constituted in 1500. We do this because jurisdictions frequently changed over time, in part as an outcome of the political processes we analyze (Whaley 2012).[41] To be clear, estimates without fixed effects and with region-year or principality-year fixed effects examine distinct counterfactuals. Region-year fixed effects absorb regional factors. Principality-year fixed effects absorb differences shared by cities that were near neighbors and subject to the same ruler in 1500. To limit the counterfactual to Protestant cities without laws, in column 5 we restrict analysis to cities in religiously homogeneous principalities.[42] The stability of our results indicates that the variation in outcomes is explained by local differences in public goods institutions, and not by the adoption of Protestantism or regional differences.[43]

We examine differences in human capital trends in columns 6-10. We control for underlying city-specific trends and find that adoption of public goods institutions was associated with both a positive level effect of 0.27-0.38 log points and positive shift in the post-trend of 0.07-0.12 log points. The estimated shift in the post period trend implies that after 100 years migration would increase about 14-16 percent, given that time is measured in 50 year periods. These estimates hold controlling for region-year and principality-year fixed effects (columns 6-9). However, the trend estimate is less precisely estimated when we study variation within religiously homogeneous principalities (column 10).[44]

In Panel B, we examine the local formation of human capital and find consistent results. We measure human capital formation with the log of the number of local individuals in the

[41] We also observe religious heterogeneity across cities in the same region after the Peace of Augsburg. We provide a complete list of the principalities and regions each city belonged to in Appendix A. We provide further historical evidence on religious heterogeneity in Appendix E. We do not observe the principality of 5 cities in the *Deutsches Städtebuch*.

[42] When we restrict to religiously homogeneous principalities, we drop "free" cities, which were legally free from the rule of a territorial lord but subject to the Holy Roman emperor. There are 167 cities in religiously homogeneous principalities, but 80 of these are in principalities without variation in law. The variation is across 14 Protestant principalities, containing a total of 87 cities, of which 43 have a law and 44 do not.

[43] Detailed formal records reveal that reformers did not achieve their goals in terms of shifting culture and individual behavior in the 1500s (Sehling 1902-2016; Strauss 1978). That said, the cultural effects of the new equilibrium may have influenced long-run development.

[44] Our results are not explained by other changes in local politics and institutions. In unreported results we study three key types of city-level changes in political institutions are observed in German cities between 1400 and 1800, involving the establishment and elimination of: (1) participatory elections, in which citizens were able to vote for city council members; (2) citizen representation, including the meeting of assemblies on which citizens were represented; and (3) guild constitutions, in which guilds were given representation on city councils (Wahl 2016). Our results do not change when we control separately for the presence of each of these dimensions of institutions using data from Wahl (2017).

Deutsche Biographie plus one. We find positive level and trend shifts in cities that adopted institutional change in the post-period. The level effects hold controlling for city-specific trends and region-year and principality-year fixed effects, whereas the trend effects remain positive but are not statistically significant within principality-years.

Flexible Model – We next flexibly study how migration and local human capital formation varied with "ever-treated" status (Law) period-by-period. We estimate regressions:

$$\ln(People_{it}+1) = \theta_i + \delta_t + \sum_{s=1420}^{1770} \beta_s (Law_i \times \delta_s) + \epsilon_{it} \tag{2}$$

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

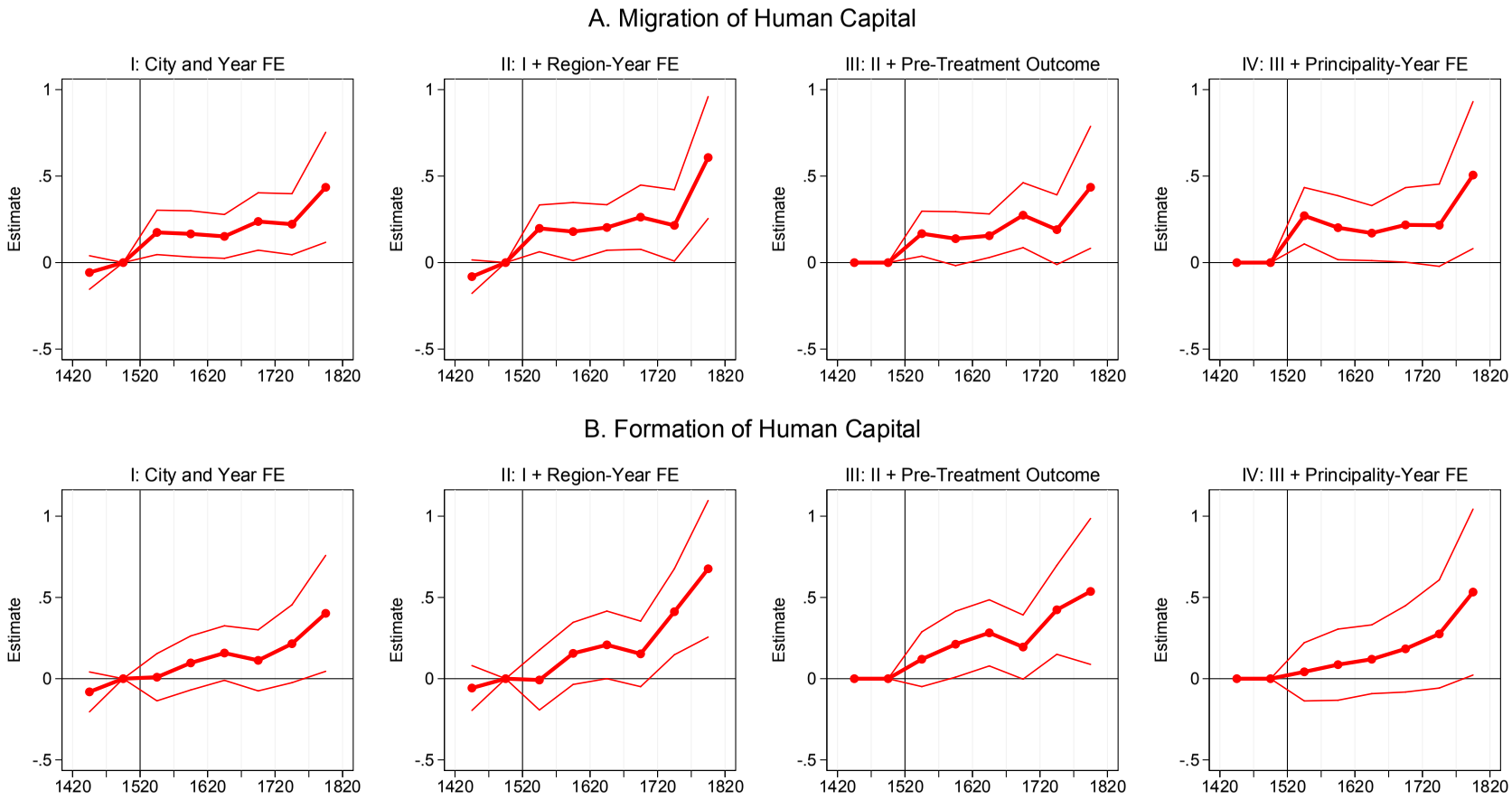
Figure 6 presents our estimates. We study migration in Panel A and the formation of human capital in Panel B. In each panel, we present four sets of comparisons. Graph I presents estimates with city and year fixed effects. Graph II adds region-year fixed effects to control for factors shared by all cities in a given region and time. We observe no significant differences between cities that adopted institutional change and cities that did not before the 1520s. However, in graph III we further account for differential pre-trends by controlling for pre-treatment outcomes interacted with the complete set of time-period indicators. In graph IV, we study variation within principality-year.

Our results in Panel A show that migration increased as and immediately after church ordinances were adopted. Our results in Panel B show that the local formation of human capital also increased following the adoption of ordinances, but that this response was more gradual and became statistically significant over time. This is consistent with the returns to institutional change interacting with evolving economic opportunities (Becker and Woessmann 2009; Acemoglu, Johnson, and Robinson 2002).[45]

Allocation of Human Capital Across Sectors – To provide additional evidence on the

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

Figure 6: Public Goods Institutions and Upper Tail Human Capital



This figure presents regression estimates examining the relationship between institutional change and human capital outcomes. Outcomes are the logarithm of the number individuals identified as upper tail human capital migrants or locals plus one, in Panels A and B, respectively. Institutional change is measured by the adoption of a church ordinance. The figure presents parameter estimates on interactions between time period fixed effects and an indicator for cities that adopted, with 95% confidence intervals. Graph I includes separate city and time-period fixed effects, and controls for time-varying participatory governance rules, measured with separate variables for (i) participatory elections, (ii) citizen representation in assemblies, and (iii) guild constitutions (Wahl 2016; 2017). Graph II adds region-time-period fixed effects, for 29 territories. Graph III adds controls for pre-1520 outcomes interacted with the complete set of time period fixed effects (i.e. outcomes for 1420-1469 and for 1470-1519, each interacted with all time indicators). Graph IV adds principality-time-period fixed effects, for 75 principalities, in place of region-time-period fixed effects. Time is measured in 50-year periods 1420 through 1819. Standard errors clustered at the city level are used to construct confidence intervals.

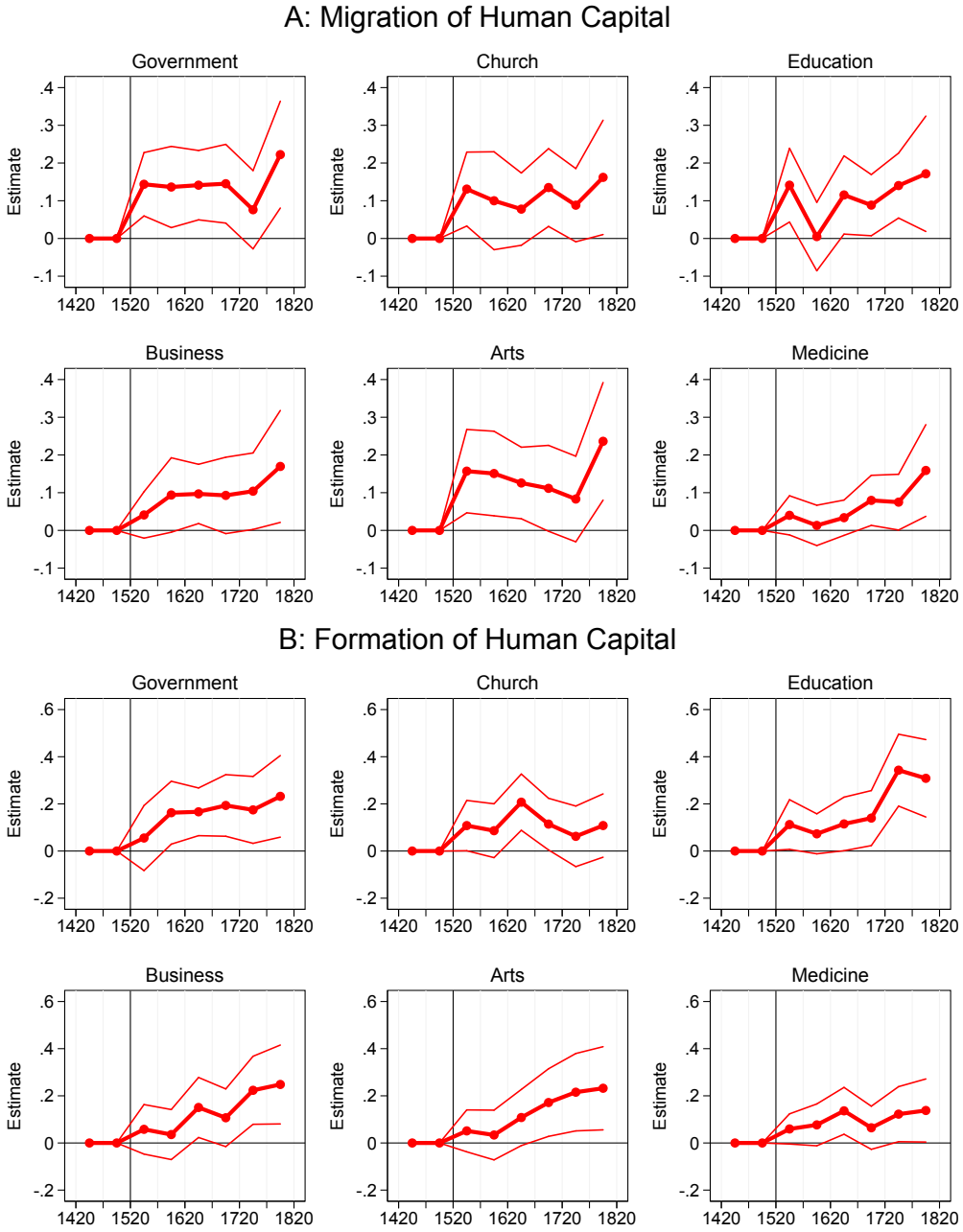
channels through which political and institutional change impacted the economy, we examine shifts in upper tail human capital across occupational sectors.

We examine the allocation of upper tail human capital across six occupational sectors: government (20%), church (15%), education (16%), business (18%), arts (26%), and medicine (5%). We measure the allocation of human capital by classifying the professions of all individuals in the *Deutsche Biographie* (see Appendix A). We study the allocation of upper tail human capital using the flexible difference-in-difference regression design of Equation 2, and maintaining the distinction between migration and local formation. We study the presence of upper tail human capital in a sector, measured 1/0. We examine this binary outcome because most of the variation at the city-sector-period level is between having zero or one observed individuals.

Figure 7 presents our estimates. We estimate regressions controlling for city fixed effects, region-year fixed effects, and pre-treatment outcomes (as in Figure 6). We use region-year fixed effects to study how the new equilibrium formalized in church ordinances drove increases in upper tail human capital even when we take as the counterfactual cities in the same region and time period. In Panel A, the outcome is a binary variable for the presence of any upper tail human capital migrants in a given city-period active in a specific occupational sector. Panel A shows that cities that adopted public goods laws in the 1500s experienced a large, immediate increase in migration in the government, church, and education sectors, consistent with our predictions. Migration in the arts also responded quickly. In contrast, the response in business sector migration was more gradual, increasing and becoming statistically significant over time.

Panel B presents estimates studying the formation of upper tail human capital by sector. We find that the formation of human capital responded less immediately to the adoption of ordinances than migration did. The formation of local human capital in the government sector increased sharply one period after adoption. But as with migration, the formation of human capital in the government sector rises relatively quickly, whereas the business sector increases over time and becomes highly significant in later periods. In contrast with migration, the formation of human capital for church in cities that adopted ordinances increases but then declines, and is no longer significant by the 1700s.

Figure 7: Upper Tail Human Capital by Sector



This figure graphs regression estimates examining the relationship between the adoption of church ordinances and upper tail human capital at the city-sector-time-period level. The outcomes are binary variables for any upper tail human capital individuals in a city-sector-time cell, estimated separately for migrants and local formation. The figure presents parameter estimates on interactions between time period fixed effects and an indicator for cities that adopted church ordinances, with 95% confidence intervals. All regressions control for city fixed effects, region-by-time-period fixed effects, and pre-1520 outcomes interacted with the complete set of time period indicators. Time is measured in 50-year periods 1420 through 1819. Sectors are as described in the text. Standard errors clustered at the city level are used to construct confidence intervals.

Discussion and Robustness – The nature of the migration process helps explain the immediate response in migration. Migration flows partly reflected geographic sorting by adults, specifically including people involved in the establishment of the new equilibrium. Cities that adopted church ordinances also directly promoted the migration of upper tail human capital *during* the educational process. Recruiters compiled extensive dossiers on promising children from small towns (Strauss 1978), who show up as migrants in our data. Both selective sorting and recruitment help explain why strong migration effects are observed in the immediate post-1520 periods.

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

An identifying assumption for our analysis is that treatment is not correlated with unobservables that could themselves drive the outcome. We address remaining questions about the potential for selective adoption using an IV strategy in Section 6.

5 Public Goods Institutions and Long-Run Outcomes

In this section, we test the hypothesis that political and institutional change embodied in church ordinances drove long-run population growth and upper tail human capital intensity. We study city population as a measure of local economic activity (De Long and Shleifer 1993; Glaeser, Scheinkman, and Shleifer 1995; Acemoglu, Johnson, and Robinson 2005a). We present panel and cross-sectional estimates in this section.

The challenge for panel research designs is that for the majority of treated cities, population data are observed only after treatment. For instance, we do not observe population in 1500 for 129 out of 239 cities because they were too small to be recorded.[46] We therefore consider two outcomes: (1) whether and when towns became cities with observed population and (2) whether and when towns became large enough to be in the top 100 cities ranked by population in a given year. We study these binary outcomes to avoid "conditional on positive" selection bias in a research design that includes unit fixed effects (Angrist and Pischke 2008). This selection bias is a concern because many initially small places only grew large enough to be observed after treatment.[47] We study being in the top 100 as a broad measure of size that we observe for all cities and periods.

We estimate panel regressions studying how the adoption of ordinances impacted whether and when towns become cities and when cities became relatively large.

$$Y_{it} = \theta_i + \delta_{jt} + \beta(Law_i \times Post_t) + \gamma(Protestant_i \times Post_t) + \epsilon_{it} \tag{3}$$

Y_{it} is a binary variable for locations (1) being cities with population observed in period t or (2) being a city with population in the top 100. Law_i and $Protestant_i$ are indicators for the adoption of church ordinances and Protestantism, respectively. The θ_i and δ_{jt} are city and region-by-time-period fixed effects. We examine data at the city-century level 1300 to 1800, with the "Post" period running 1600 to 1800.

Table 3 reports our estimates. Columns 1-4 study 2,230 German towns and show that towns that adopted institutional change were approximately 7 percent more likely to be observed as cities in the post-treatment period.[48] This result holds controlling for region-by-year fixed effects and whether a city adopted Protestantism.[49] Columns 5-7 show that cities were 12 percent more likely to be in the top 100 cities by population after adopting

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

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

[48] A small number of towns were absorbed into larger cities over our sample period. Here we analyze the balanced panel of towns that were not absorbed into other cities.

[49] We do not consider principality-year fixed effects because of ambiguities assigning each town to a principality.

ordinances, while Protestantism does not explain this variation. Column 8 shows that the post-period effect of adoption is entirely driven by a change in the trend, consistent with an underlying growth effect.

Table 3: Public Goods Institutions and City Population in the Panel

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Outcome: Population Observed Data on All Towns				Outcome: Population in Top 100 Data on Cities			
Post × Law	0.07***		0.07***	0.01	0.12**		0.13**	-0.10
	(0.02)		(0.02)	(0.03)	(0.05)		(0.05)	(0.09)
Post × Protestant		0.03**	0.02**	-0.02*		-0.01	-0.05	-0.06
		(0.01)	(0.01)	(0.01)		(0.06)	(0.06)	(0.12)
Post × Law × Trend				0.03**				0.12**
				(0.01)				(0.04)
Post × Prot × Trend				0.02**				0.00
				(0.01)				(0.06)
Observations	13380	13380	13380	13380	1434	1434	1434	1434
Town FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table presents regression estimates examining city population outcomes. In Columns 1-4, the outcome is a binary variable that takes the value of 1 if a town has population observed in Bairoch, Batou, and Chèvre (1988). Columns 1-4 examine the 2,230 towns recorded in the *Deutsches Städetebuch*. In columns 5-8 the outcome is a binary variable for cities with population in the top 100 and analysis is restricted to 239 cities observed in Bairoch, Batou, and Chèvre (1988). "Post × Law" interacts an indicator for institutional change and the post-1520 period. "Post × Protestant" interacts an indicator for cities that adopted Protestantism and the post-1520 period. Time trend interactions measure time in centuries from year 1500 (100 years is one time unit). We measure Protestantism using Cantoni (2012) for cities and the *Deutsches Städtebuch* for towns not in Cantoni (2012). Data on population are at 100-year intervals 1300 through 1800. Standard errors in parentheses are clustered at the town (city) level.

We next examine long-run growth in the cross-section. Table 4 presents two key facts on the relationship between initial city population, the adoption of church ordinances, and population in 1800. First, cities that adopted these institutional changes were 45 log points (57 percent) larger in 1800 than cities that did not. Second, cities that were large in 1500 were more likely to adopt (column 8), but the relationship between adoption and long-run population is significant and positive for all except for the few cities that were initially large. The positive relationship between adoption and growth is sharp in locations that had not been previously dynamic. Several factors may explain why adoption does not predict growth in the very largest cities. Large cities in this period were constrained by the need to transport food long-distance and thus grew slowly (Dittmar 2015), and the public goods institutions we

Table 4: Log Population in 1800 by Institutional Change Status and Initial Size

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

This table presents the summary statistics for log city population in 1800 by institutional change status and initial pre-Reformation city size. Institutional change is measured by an indicator variable for whether a city had a church ordinance ("Law") by 1600. Populations are measured in thousands: $\ln(population/1000)$. P-values for the statistical significance of differences in means in square brackets in column 7. Column 8 reports the share of cities with a church ordinance in each initial size category for population in 1500. Column 9 reports the share of total cities in each initial size category.

study may had limited impact on this constraint. In addition, there is suggestive case study evidence of negative selection into institutional change among the largest cities (Scribner 1976). We present complete summary statistics on all variables in the Appendix.

To study the relationship between the adoption of church ordinances (Law) and long-run population and human capital outcomes, we estimate the following regression:

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

where Y_i is the logarithm of population in 1800 and $Law_i = 1$ if city i had a law. The control variables (X_i) include our outcome measure of upper tail human capital before 1517 and the number of students from each city receiving university degrees separately decade-by-decade over the 1400s, to absorb human capital pre-trends. We also control for whether cities had market rights in 1517, were incorporated by 1517, indicators for printing, universities, Free-Imperial cities, the average number of pre-1500s plagues, the informal diffusion of Protestantism, and geographic controls. We control for initial population either with categorical fixed effects or for log population in 1500, setting this to 0 for unobserved cities and including an indicator for unobserved status. We present summary statistics in Appendix A.

Table 5 shows the results from estimating equation (4). The outcome in Panel A is

log population in 1800. The outcome in Panel B is upper tail human capital 1750-1799, measured as the log of the sum of migrants and local formation. The outcome in Panel C is the number of upper tail human capital individuals 1750-1799 per 1,000 population in 1800. Across specifications, we find that cities with laws institutionalizing public goods had 35-40 percent more upper tail human capital in the late 1700s, were 24-37 percent larger in 1800, and thus were more upper tail human capital intensive. In column 1 we control for region fixed effects, upper tail human capital 1470-1519 and 1420-1469 separately, and population in 1500 with categorical indicator variables.

Our main result holds when we control for initial conditions, human capital pre-trends, and the non-institutional diffusion of Protestantism. The estimate is slightly stronger and more precise when we control for initial conditions and human capital pre-trends in column 2. The point estimate is virtually unchanged when we include longitude, latitude, and their interaction as proxies for the potential growth advantages of proximity to Atlantic ports and city age in column 3. To distinguish the variation explained by laws from the variation explained by the non-institutional diffusion of Protestantism we use Cantoni's (2012) data on the adoption of Protestantism. We also control for distance from Wittenberg (Becker and Woessmann 2009), but most variation in distance is already absorbed in region fixed effects. In column 4, we use the same controls as Cantoni (2012) and find that the diffusion of Protestantism had no significant relationship with outcomes. We find the point estimate on laws is positive and significant controlling for the adoption of Protestantism in column 5. In column 6, we control for log population in 1500 and find the results are robust.[50] Column 7 shows our results hold when study variation within religiously homogeneous principalities, where counterfactual is Protestant cities without ordinances.

We also find no evidence that the adoption of public goods institutions interacted with initial city characteristics to predict outcomes, with one exception. In *ex ante* large cities we find a *negative* differential relationship between Reformation institutions and population growth.[51] We find no differential human capital or growth effect for institutions in Free-Imperial cities, cities with many university students, cities with printing, or cities with

[50] We assign a value of 0 for all cities with population unobserved in 1500 and include an indicator for unobserved status. We provide detailed evidence on these cities in the Appendix.

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

Table 5: OLS Analysis of Long-Run Outcomes

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A: City Population							
	Outcome: Ln Population in 1800						
Law	0.24*	0.26**	0.25**		0.28***	0.26***	0.37**
	(0.12)	(0.11)	(0.10)		(0.09)	(0.09)	(0.16)
Protestant				-0.08	-0.11	-0.12	
				(0.18)	(0.17)	(0.21)	
Observations	239	239	239	239	239	239	167
R^2	0.47	0.54	0.54	0.51	0.52	0.50	0.65
Panel B: Human Capital							
	Outcome: Ln Upper Tail Human Capital 1750-1799						
Law	0.35***	0.41***	0.39***		0.30***	0.30***	0.39*
	(0.11)	(0.12)	(0.13)		(0.10)	(0.10)	(0.21)
Protestant				0.20	0.19	0.19	
				(0.20)	(0.22)	(0.22)	
Observations	239	239	239	239	239	239	167
R^2	0.29	0.40	0.40	0.34	0.35	0.35	0.60
Panel C: Human Capital Intensity							
	Outcome: Upper Tail Human Capital per 1,000						
Law	0.09**	0.11***	0.11**		0.08*	0.08*	0.10
	(0.04)	(0.04)	(0.04)		(0.04)	(0.04)	(0.07)
Protestant				0.09	0.09	0.09	
				(0.08)	(0.08)	(0.09)	
Observations	239	239	239	239	239	239	167
R^2	0.25	0.42	0.42	0.30	0.31	0.30	0.59
Controls that Vary Across Specifications							
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Population FE	Yes	Yes	Yes	Yes	Yes	No	Yes
Main controls	No	Yes	Yes	No	No	No	No
Geo Controls	No	No	Yes	No	No	No	No
Cantoni Controls	No	No	No	Yes	Yes	Yes	Yes
Log Population 1500	No	No	No	No	No	Yes	No
Principality FE	No	No	No	No	No	No	Yes

This table presents the regression estimates of the relationship between the adoption of church ordinances ("Law") and long-run outcomes. Outcomes are: in Panel A log population in 1800; in Panel B the log of upper tail human capital plus one 1750-1799; and in Panel C upper tail human capital individuals 1750-1799 per 1,000 population in 1800. Upper tail human capital is the sum of locally born individuals and migrants from the *Deutsche Biographie*. "Law" is an indicator. "Protestant" is an indicator for Protestant cities (Cantoni 2012). All regressions control for Ln Upper Tail Human Capital in 1420-1469 and 1470-1519. Columns 1-6 control for 29 region fixed effects using Nüssli (2008). Column 7 controls for 75 principality fixed effects using the *Deutsches Städtebuch*. "Main Controls" are: Market rights by 1517; town incorporated by 1517; indicators for books printed pre-1517 (0, 1-100, 101-1000, 1001+), university by 1517; the number of students from each city receiving university degrees separately each 10-year period starting 1398 through 1508; and the average number of plagues from 1349 to 1499. "Geo Controls" are longitude, latitude, and their interaction. "Cantoni Controls" are year city founded and year turned Protestant, indicators for rivers, Hansa cities, Free-Imperial status, monasteries, university, printing, and distance to Wittenberg. Population fixed effects are indicators for 1500 population data: missing, 1,000-5,000, 6,000-10,000, 11,000-20,000, and 20,000+. Column 6 controls for log population in 1500, setting log population to 0 for cities with data unobserved, and an indicator for cities with data unobserved. Column 7 restricts analysis to religiously homogeneous principalities. Standard errors are clustered at the region (principality) level. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***".

market rights. We report these results in Appendix C.

6 Plague Shocks as a Source of Exogenous Variation

The fact that cities that adopted public goods institutions subsequently developed faster invites questions about (1) why some cities adopted and others did not and (2) whether there are sources of exogenous variation in adoption. One possibility is that the Reformation suddenly and exogenously allowed cities with different underlying characteristics to select different public goods institutions. In this case, differences in growth could have been driven in part by these underlying characteristics, and not only the adoption of public goods institutions. To address this potential concern, we use the timing of plague outbreaks in the early 1500s as an instrumental variable (IV) to isolate exogenous variation in adoption. The IV analysis also gives us a setting in which to test our predictions about the political process that leads to adoption.

6.1 Explaining the Instrumental Variable Methodology

In this section we explain our methodology, how plagues shifted politics and institutions, and why the timing of plagues provides exogenous variation. We also preview evidence indicating that plagues in the 1500s did not shift outcomes through other channels.

Mechanism: How The Timing of Plagues Shifted Politics and Institutions

Plagues in the early 1500s lowered the relative price of political action and increased local demand for public goods institutions. These plagues interacted with the global shift in politics due to the Reformation. The Reformation expanded the set of institutional arrangements in a non-democratic political marketplace.

The political implications of plague outbreaks changed with the introduction of religious competition. It was not unusual for 1/4 of a town's population to die (Slack 2012). During outbreaks, elites often died and fled, and civic order frequently broke down (Dinges 1995; Isenmann 2012). Experience with plague discredited elites, lowering the relative price of local political action.[52] Before religious competition led to the articulation of institutional

[52]Protestant reformers criticized past city council behavior during plagues and advocated institutional change. For example, in 1533 Andreas Osiander scolded Nürnberg's city council for previously abandoning the city during plague outbreaks in his famous "Plague Sermon" and authored a church ordinance.

alternatives, these shocks did not lead to institutional change. Political competition changed in the 1500s as a new institutional agenda for public goods provision was developed by ideological entrepreneurs. Church ordinances were institutional blueprints that aimed to ensure good governance. The plague and public health provision figured prominently in these ordinances, which explicitly formalize the provision of health and pastoral care.[53] In the 1500s, citizens who had and had not experienced plagues were thus suddenly given a new choice in political markets relating to social service provision.[54]

The historical evidence on political competition shapes how we construct our IV. The period characterized by increased political competition started with the dissemination of Luther's theses in 1517. City-level institutional change was arrested by the Schmalkaldic War (1544-5) and the Peace of Augsburg (1555).[55] In our baseline analysis, we use plague exposure between 1500 and 1522, the year of the first city ordinance, as an IV. By examining plagues only up to 1522, we ensure that we do not attribute adoption to plagues that occurred later.[56] However, we show that plagues after 1522 also predict adoption and growth, consistent with our baseline analysis, particularly among cities that were not early adopters.

Exogeneity: Randomness in the Timing of Plague Outbreaks

We use variation in plague within a narrow time period as our instrumental variable to isolate shocks. Clearly, plague outbreaks could reflect underlying differences across cities and be endogenous to economic development: cities on trade networks were in general more likely to experience outbreaks. To isolate random variation, we study outbreaks in the early 1500s conditional on underlying trends and long-run variation in plague that might be correlated with other characteristics that directly shaped economic development.[57]

[53] Almost all church ordinances contain provisions on directing priests to visit the sick and offer consolation. In contrast, Catholic theologians and statesmen, "rejected public participation entirely or wanted to allow it in only very reduced measure" (Roeck 1999; p. 286). Catholic cities outside Germany did develop strategies to address the plague, e.g. in Italy (Cipolla 1992).

[54] Similar dynamics are observed in contemporary research on AIDS in Africa, which shows that service provision drives conversion to a new religion (Trinitapoli and Weinreb 2012).

[55] City-level politics played a key role in institutional change before the Schmalkaldic war; afterwards political initiative largely shifted to territorial rulers (Hamm 1994).

[56] Because the adoption of ordinances led to more in-migration, it is possible that adoption could have *increased* the risk of plague, as we discuss below. In general, outbreaks shifted the institutional preferences of the survivors and changed the composition of the population by attracting a subsequent influx of migrants. Isenmann (2012) observes that typically the number of *new* property owning citizens with voting rights (*Neubürger*) rose dramatically after plagues, but new burghers only obtained voting rights after a period of 5 to 10 years residency, which may be one reason why political change often occurred with lags.

[57] We also condition on our measure of human capital outcomes observed over two periods before the

Our methodology is supported by research in historical epidemiology which emphasizes that the *short-run* distribution of plague outbreaks was random, conditional on observables, and was geographically localized (Biraben 1975; Slack 1988). Historic plagues outbreaks were characteristically observed in "compartmentalized" locations and *not* spreading neighbor-to-neighbor (Biraben 1975; p. 285). Among the notable "puzzling features in the spread of plague" was that it "missed some towns in its transit along major highways" and was characterized by "irregular timing" (Slack 1988; p. 435).

Figure 8 illustrates the short-run randomness of plague outbreaks and the variation in the IV for select cities: outbreaks from the beginning of the century to the passage of the first law in 1522. The timing and frequency of outbreaks was highly variable across cities. Neighboring cities experienced outbreaks at different times, for example Mainz and Frankfurt am Main, which are less than 50 kilometers apart. Others experienced few or no major outbreaks despite being important urban centers, like Frankfurt, Ulm, and Regensburg.

We provide several additional pieces of evidence that support the exogeneity assumption in our analysis. We show that there was no aggregate trend or periodicity in plague between 1400 and 1600 (Appendix D). We document that there were no non-linear increases in plagues in more connected cities in the IV period. This indicates that the variation in the instrument is not driven by a shift in plague towards locations that favor growth. We similarly show there were no differential plague trends in cities that were more connected to trade networks (see Appendix D). However, in our analysis we still control flexibly for long-run differences and pre-trends in plague that could reflect unobserved city characteristics related to development.

Exclusion Restriction: Public Goods Institutions as the Channel

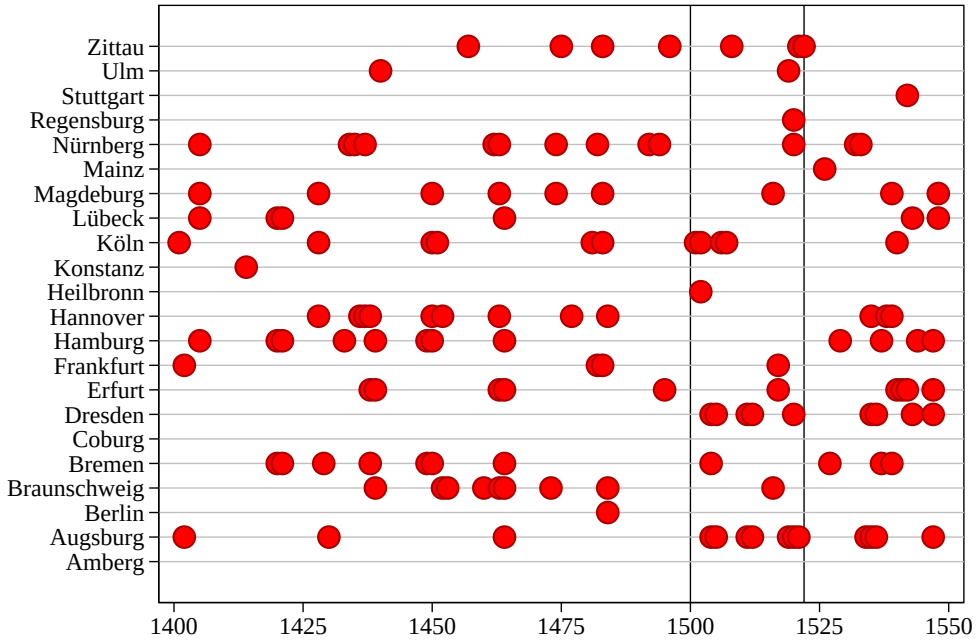
It is natural to wonder whether the plague shaped long-run outcomes directly, for example through its demographic impact or through its influence on other political institutions.

Here we preview two pieces of evidence that that support the exclusion restriction. First, we show that plagues in the early 1500s had a unique positive and statistically significant relationship with long-run city growth when compared to plagues across the 1400s and later 1500s.[58] Second, we find no relationship between plagues in the early 1500s and changes in

Reformation era and on the number of university students observed coming from each city decade-by-decade, thus flexibly accounting for differences in the level and trend of human capital before plague shocks and institutional change.

[58] Our results are further robust to relaxing the exclusion restriction to account for "plausible exogeneity" (Conley, Hansen, and Rossi 2012), as we discuss below.

Figure 8: 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.

other city institutions. We present these analyses below in Section 6.3.

6.2 Instrumental Variable Estimates

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

$$Law_i = c + \alpha \cdot Plagues_{i,1500-1522} + \beta \cdot g(Plagues_{i,1350-1499}) + \gamma \cdot X_i + \epsilon_i \tag{5}$$

In our baseline specification, the instrument shifting public goods institutions is the number of plague outbreaks between 1500 and 1522, the year the first church ordinance was passed. Our instrument recovers how plagues that hit the generation in place when the Reformation began shifted the probability of adoption. The impact of plagues across the early 1500s, including through 1545, is similar and is discussed below. We control for long-run variation in plague because over the long-run outbreaks may have been more frequent in cities that were "open" or "good" and already bound to grow. To isolate plausibly exogenous variation in outbreaks we control for: the average annual level of outbreaks 1350 to 1499 and,

independently, the number of plague outbreaks each quarter-century starting in 1350. We denote these controls with $g(Plagues_{i,1350-1499})$. The vector X_i contains the same control variables as in Section 5, including our measure of upper tail human capital and the number of university graduates over multiple pre-Reformation periods.[59]

Table 6 shows our IV results. In Panel A, column 1 shows that $Plagues_{i,1500-1522}$ is a strong predictor for the adoption of a law and that each additional plague outbreak between 1500 and 1522 increases the propensity of adopting a law by 14 percentage points. The F-statistic on the excluded instrument is above 34. In Panel B, the point estimate of the second stage implies that a city with a law by 1600 was 1.61 log points larger in 1800 than a city without a law. We also find strong positive effects of adoption on both total upper tail human capital and the intensity of upper tail human capital, measured by the number of upper tail human capital individuals relative to total population. Our second stage results are slightly stronger when we control for plague flexibly by period, accounting for potential pre-trends (column 2). The results strengthen when we introduce region fixed effects and identify off within-region variation (columns 3 and 4) and strengthen further when we introduce principality fixed effects (columns 5 and 6).

To gauge the magnitudes of our IV estimates, we compare the implied annual growth rates across our regression designs. The OLS estimates for upper tail human capital imply an annual growth rate advantage of 0.1 percent for a treated city. The difference-in-differences estimates imply an annual advantage of about 0.5 percent. The IV estimates imply an annual growth advantage of approximately 1.1 percent. For city population, the OLS and IV estimates imply annual growth rate advantages of 0.1 percent and 0.7 percent, respectively.[60]

There are several possible explanations for the fact that the IV estimates are much larger than the OLS estimates. The first is that the IV isolates exogenous variation in treatment whereas the OLS is attenuated by unobserved city characteristics. One might assume that cities positively selected into treatment. However, there is little evidence that the institutional change was adopted for directly economic reasons. In a few notable wealthy

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

[60] Note that our difference-in-difference estimates imply a total advantage of 1.2-1.9 log points in late 1700s (Section 5). For comparison, Acemoglu, Johnson, and Robinson (2005b) study city growth and find that European cities with access to Atlantic trade were 0.8-1.1 log points larger in 1800, controlling for time invariant city characteristics and time fixed effects shared across cities.

Table 6: Instrumental Variable Analysis of Long-Run Outcomes

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: First Stage – Public Goods Institutions						
	Outcome: Adoption of Law					
Plagues 1500-1522	0.14***	0.13***	0.13***	0.12***	0.12***	0.11***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.04)	(0.03)
R^2	0.29	0.30	0.51	0.52	0.62	0.63
F Statistic on IV	34.02	32.69	18.92	31.42	8.70	12.02
Panel B: Instrumental Variable Outcomes – Population and Human Capital						
	Outcome: Ln Population in 1800					
Law	1.61**	1.76**	1.94**	2.17**	2.13*	2.30**
	(0.82)	(0.83)	(0.98)	(0.93)	(1.12)	(1.01)
	Outcome: Ln Upper Tail Human Capital 1750-1799					
Law	2.74**	2.96**	3.10**	3.34**	4.23**	4.54***
	(1.24)	(1.29)	(1.38)	(1.40)	(1.81)	(1.28)
	Outcome: Upper Tail Human Capital per 1,000					
Law	0.56**	0.63**	0.59*	0.66**	0.80**	0.92***
	(0.27)	(0.25)	(0.31)	(0.29)	(0.40)	(0.29)
Controls						
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Plagues Pre-1500: Level	Yes	Yes	Yes	Yes	Yes	Yes
Plagues Pre-1500: Non-Linear	No	Yes	No	Yes	No	Yes
Region Fixed Effect (n=29)	No	No	Yes	Yes	No	No
Principality Fixed Effect (n=75)	No	No	No	No	Yes	Yes
Observations	239	239	239	239	234	234

The first stage outcome variable in Panel A is an indicator for the adoption of a church ordinance (Law). "Plagues 1500-1522" is the number of plagues 1500 to 1522. The outcome variables in Panel B are: log population in 1800; log of the number of upper tail human capital individuals observed between 1750 to 1799 plus one; and the number of upper tail human capital individuals per thousand population. In first stage regressions, the dependent variable is an indicator for the passage of a Reformation ordinance by 1600. All regressions control separately for the log of upper tail human capital observed 1420-1469 and in 1470-1519, and include the complete set of controls from Table 5. Upper tail human capital is measured by the sum of the number of migrants dying in a city-period and the number of people locally born people reaching age forty in a city-period. Region fixed effects control use territories recorded by Nüssli (2008). Principality fixed effects use principalities recorded in the *Deutsches Städtebuch*. "Plagues Pre-1500: Level" is the average number of plagues from 1350 to 1499. "Plagues Pre-1500: Non-Linear" indicates independent controls for the number of years with plague outbreaks in each of the twenty-five year periods: 1350-1374 through 1475-1499. Standard errors are clustered at the 1500 Region level. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***".

and well-connected cities, the municipal leadership was successful in preventing institutional change. Cologne was Germany's largest city in 1500 and is the classic example of a city in which elites' interest in preserving trade relationships motivated anti-Protestant behavior (Scribner 1976). A second possibility is that the instrumental variable design recovers a cleaner measure of the intensity of treatment. The adoption ordinances produced what North (1990) would recognize as local "institutional matrices." It is possible that the IV captures underlying variation in institutions that is lost in the binary treatment variable on which OLS relies. A third possibility is that the IV recovers underlying heterogeneity in the returns to treatment across cities.

To examine whether the IV recovers heterogeneity in returns, we study whether the interaction between plague shocks and city characteristics shaped the adoption of church ordinances in Appendix D. We find no significant interaction between plagues and prior printing, plagues and university students, or plagues and market rights. We do find evidence that the plague effect on adoption was muted in free cities. This suggests that the effect of plagues on adoption was concentrated in cities subject to feudal lords, where the barriers to political change were higher.[61] If cities subject to lords had higher returns to adoption, our IV could recover these returns. However, we find no differential correlation between adoption and growth in cities subject to lords (Appendix C).

6.3 Evidence Supporting the Exclusion Restriction

We use several tests to address the plausibility of the exclusion restriction. First, we test whether plagues in general had a significant positive relationship with long-run population, as would be predicted if they had direct growth effects. Second, we test for a relationship between plague outbreaks in the 1500s and city-level institutional change along other dimensions than the public goods institutions we study. Third, we test whether plagues in the early 1500s drove increases in religious and political publications during the Reformation, as we would expect if plague outbreaks in this period shifted political competition.

The relationship between plagues and long-run growth

Plagues could have influenced long run growth through channels not specific to the

[61] This is consistent with the finding in Dittmar and Seabold (2017) that variations in media market competition mattered most for the diffusion of the Reformation ideas in cities subject to lords.

political and institutional changes of the 1500s, for example by shifting the capital-to-labor ratio and factor returns. Were this the case, we would expect to find that variation in plague before, during, and after the early 1500s influenced long-run growth.

To test this possibility, we study the direct relationship between long-run city population outcomes and plagues in different periods from the mid-1300s through the late-1500s. We estimate regressions of the form:

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

The parameters of interest are the β_t, which capture the relationship between long-run population and $plagues_{it}$, which measures the number of plagues in city i in 25-year intervals starting 1350-1374 and running to 1575-1599.[62] The controls X_i include indicators for cities with market rights by 1300, cities legally incorporated in 1300, region fixed effects, and initial city population in 1300, measured categorically.

Table 7 shows that while early 1500s plagues predict city population in 1800, plagues in other periods across the 1400s and 1500s do not. After the Black Death mega-shock of the mid-1300s, the positive relationship between long-run population and plagues in the early 1500s was unique in its magnitude, precision, and robustness.[63] For example, while plagues in the period 1475-1499 were also positively associated with long run outcomes, the estimated relationship is imprecise and declines in magnitude when we control for initial population in 1300 (column 2), city incorporation and market rights as of 1300 (column 3), and region fixed effects (column 4). In contrast, the relationship between early 1500s plagues and long-run outcomes is robust to controlling for initial observables and studying within-region variation.[64]

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

[63] We find no systematic changes in city-level institutions in the 1300s associated with differential exposure to plagues in this era examining, for example, formal city incorporation, forms of city governance (Wahl 2017), and market rights (e.g. Cantoni and Yuchtman 2014). This suggests that the Black Death did not shape variation in city-level outcomes through institutional channels, at least in Germany. It is consistent with evidence showing that the Black Death raised growth through its effects on factor prices and consequent changes in family structure, fertility, and human capital investments (as suggested by Voigtländer and Voth 2013).

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

Our results strongly suggest that long-run outcomes were not driven by the plague's direct economic impact, with the exception of the Black Death. Our key results are also robust to relaxing the exclusion restriction. We find our results hold when we treat the instrument as only "plausibly exogeneous" (Conley, Hansen, and Rossi 2012), as shown in Appendix D. Our estimates are also consistent with historical evidence indicating that plagues had *long-run* development impacts precisely when outbreaks occurred in critical junctures (Biraben 1975). While political competition extended until the mid-1500s, and we observe no significant relationship between plagues 1525-1549 and long-run growth, this is conditional on variation in plagues in other periods including 1500-1524. For cities that were not early adopters of institutional change, plagues in the period 1525-1549 remained highly significant predictors of institutional change and growth, as we show below.

Plagues in the early 1500s did not shift other institutions

Did plagues in the early 1500s, which had a special relationship to long-run growth, drive other institutional changes that could explain our findings? Changes in political institutions occurred in some cities in the 1500s, including in institutions securing more participatory governance (Wahl 2016). More participatory political institutions could be expected to promote growth, and are the most likely alternative growth channel to the public goods institutions we investigate. In the 1500s, we observe changes in political institutions that established or eliminated: (1) participatory elections; (2) public representation on city assemblies; and (3) municipal constitutions that secured representation for guilds on city councils (Whaley 2012; Brady 2009; Wahl 2016).

We test whether plagues in the 1500s drove changes in these political institutions, using data from Wahl (2016; 2017). We estimate models that are structurally identical to the first stage in our IV analysis examining public goods institutions. Table 8 presents our estimates and shows there was no significant relationship between plague shocks and changes in political institutions on any of these dimensions. This result holds when we examine all the variation in the data and when we study within-region variation (odd and even columns, respectively).[65] There is similarly no significant relationship between plagues

[65]In Table 8 we examine shifts towards and away from "participative political institutions": the outcome variables take values +1 when elections, representation, or a guild constitution are established; 0 when no institutional change is observed; and -1 when elections, representation, or a guild constitution are eliminated.

Table 7: Historic Plague Outbreaks and City Population in 1800

	[1]	[2]	[3]	[4]
	Outcome: Ln Population in 1800			
Plagues 1350-1374	0.42***	0.29***	0.29***	0.27***
	(0.05)	(0.08)	(0.08)	(0.09)
Plagues 1375-1399	0.07	0.12	0.11	0.12
	(0.10)	(0.12)	(0.12)	(0.15)
Plagues 1400-1324	-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
	(0.11)	(0.09)	(0.09)	(0.09)
Plagues 1475-1499	0.20	0.13	0.13	0.10
	(0.23)	(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	0.09	0.10	0.08
	(0.07)	(0.05)	(0.06)	(0.08)
Plagues 1575-1599	0.00	-0.06	-0.06	-0.04
	(0.05)	(0.06)	(0.06)	(0.07)
Observations	239	239	239	239
Population in 1300	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Region Fixed Effects	No	No	No	Yes

This table presents results from regressions estimating the relationship between city population in 1800 and historic plague exposure between 1350 and 1599. "Plagues 1350-1374" is the count of plague outbreaks in that period. Other plague variables are similarly defined. Controls include indicators for city incorporation and for city market rights granted by 1300. Population fixed effects are for categorical variables: population in 1300 data missing; 1,000-5,000; 6,000-10,000; 11,000-20,000; and more than 20,000. Region fixed effects control for regions from the *Deutsches Städtebuch*. Standard errors are clustered at the region level. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***".

and institutional change when we separately estimate for (i) cities without participative institutions in 1500, i.e. candidates for adoption, and (ii) cities with participative institutions in 1500, i.e. candidates for disestablishment.[66]

The fact that plagues did not shift participatory institutions is to be expected, given that reformers supported local secular power structures and targeted the Catholic Church as a

[66]Between 1500 and 1600, changes in "Participatory Elections," "Public Representation," and "Guild Constitution" are observed in 11, 25, and 24 of the 239 cities in our data, respectively. In 1500, 61 cities in our data had participative elections, 76 had public representation, and 80 had guild constitutions. Separate estimates for adoption candidates and disestablishment candidates are available on request.

foreign, rent-seeking organization (Hamm 1994; Blickle 1994; Ozment 1975).

Table 8: Plagues and Other Potential Institutional Changes in the 1500s

	[1]	[2]	[3]	[4]	[5]	[6]
	Outcome: Change in Institutions in the 1500s					
	Participatory Elections		Public Representation		Guild Constitution	
Plagues 1500-1522	-0.01	-0.01	0.03	0.02	-0.07	-0.08
	(0.02)	(0.02)	(0.03)	(0.03)	(0.05)	(0.06)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	239	239	239	239	239	239

This table presents results from regressions estimating the relationship between plague shocks and institutional change in the 1500s. We examine changes in formal institutions establishing or eliminating: (1) participatory elections; (2) public representation, specifically formal representative bodies for citizens (*Burghers*); and (3) constitutions securing guild representation on city councils. The outcome measures of institutional change are constructed from data in Wahl (2016; 2017). Institutional change outcomes take values of: 1 when institutional indices go from 0 in 1500 to 1 in 1600; 0 when institutional indices do not change between 1500 and 1600; and -1 when institutional indices go from 1 in 1500 to 0 in 1600. Specifications and the complete set of controls match the first stage regression estimates reported in Table 6. Standard errors are clustered by region.

Increased Religious and Political Competition After Plague Shocks

An additional prediction of our analysis is that plague shocks led to increases in religious and political competition. We use data on religious and political publications across cities as proxies for competition, and examine the relationship between plagues and publications in Table 9. Our data measure the number of religious publications and political publications in a given city-year.[67] We find religious and political publication increased significantly in response to plague shocks in the early 1500s. This result is not driven by a decline in publications during plague outbreaks and mean reversion afterwards, as we show by (i) examining only plagues between 1510 and 1521 and (ii) dropping the years 1510-1521 from our analysis in Appendix D. Our results reflect both increases in religious and political publications in cities that had printing prior to 1517 and increases in cities that did not have printing prior to 1517, but where printers subsequently settled.[68] Significantly, over

[67] Over 95% of religious publications were *not* bibles. We identify religious publications, political tracts, and discourses on government using the classification in the *Universal Short Title Catalogue*.

[68] When we restrict the sample to only cities without any printing before 1517, we find that plague shocks drove highly significant increases in religious publications in the post period. Narrative evidence on cities without any printing suggests a similar chronological pattern, with political competition increasing sharply in the 1520s, especially where legal changes later occurred (Scribner 1979).

the early 1500s printing activity in cities that subsequently did and did not adopt public goods institutions was similar (Figures 1 and 2 above). We thus study a different form of variation from Dittmar (2011), which examines the relationship between pre-1500 printing and subsequent economic development.

Table 9: Plagues and Religious and Political Publications

	[1]	[2]	[3]	[4]
	Religious Publications		Political Publications	
Post × Plagues 1500-1522	2.03***	2.04***	0.09***	0.09***
	(0.45)	(0.47)	(0.02)	(0.02)
City Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
Region × Year Fixed Effects	No	Yes	No	Yes
Observations	24139	24139	24139	24139
Mean Outcome	1.46	1.46	0.04	0.04

This table presents regression estimates examining the relationship between plagues and religious and political publishing outcomes. The outcome is the number of religious or political publications in a given city-year. Data on religious and political publications are from the *Universal Short Title Catalogue*. The analysis examines printing across cities from 1500 through 1600. "Post × Plagues 1500-1522" is an interaction between an indicator for the post-1517 period ("Post") and the number of plagues in the early 1500s (Plagues 1500-1522). Standard errors are clustered by city. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***".

6.4 Plagues, Institutional Change, and Growth Across the 1500s

Our baseline analysis relies on variation in plague in the early 1500s. In this section, we show our results are robust when we study plagues across the first half of the 1500s.

We define the instrument time period in our baseline analysis to ensure we do not reverse the direction of causation and to eliminate measurement error. While reverse causation is unlikely, one could for example wonder whether the risk of plague outbreaks might have increased with rising in-migration – as an unintended result of institutional change. Measurement error could arise due to the staggered timing of plague outbreaks and the adoption of church ordinances. By the mid-1500s, cities had been exposed to early plague shocks and, partly in response, many had already adopted. When we study the impact of plagues after 1522, the control group for whom the instrument is "off" in later periods contains cities that appear as "always takers" by virtue of previously being exposed to plagues and adopting. We might thus expect potential bias in estimates using post-1522 plagues as

the IV. Our baseline set-up rules out these forms of reverse causality and measurement error.

While our baseline rules out reverse causality and measurement error, it excludes from consideration plagues in the 1520s and 1530s that may have influenced the adoption of ordinances in cities that were not early adopters. Consider the example of the city of Hannover which had no plagues from 1500 to 1522, meaning the IV is "turned off" in our baseline analysis. Hannover survived without a law into the 1530s, experienced a plague in 1535, and adopted an ordinance in 1536. This example invites us to ask whether plagues across the first half of the 1500s operated similarly.

In the data, we find that our results are robust to examining plagues across the first half of the 1500s. In Table 10, columns 1 and 2 present our baseline results using plagues between 1500 and 1522 as the IV, with and without region fixed effects. Columns 3 and 4 present our estimates using plagues across the first half of the 1500s as the IV (for comparability with the baseline, we normalize to consider plagues on a consistent per-23-year basis). The IV estimates are broadly similar across specifications.

Table 10: IV Estimates Using Plague Shocks in Different Time Periods

	[1]	[2]	[3]	[4]
	IV Outcome: Ln Population in 1800			
Law	1.76**	2.17**	1.33*	1.96**
	(0.83)	(0.93)	(0.77)	(0.81)
F Statistic on IV	32.69	31.42	31.94	28.10
	First Stage Outcome: Law			
Plagues 1500-1522	0.13***	0.12***		
	(0.02)	(0.02)		
Plagues 1500-1550			0.19***	0.15***
			(0.03)	(0.03)
Observations	239	239	239	239
Region Fixed Effects	No	Yes	No	Yes

This table presents IV regression estimates. Columns 1 and 2 present our baseline estimates using plague 1500-1522 as the IV. Columns 3 and 4 present corresponding estimates using plague 1500-1550 as the IV. For the period 1500-1550, we normalize the count of plagues to a 23 year basis to ensure comparability of the first stage estimates (i.e. for Plagues 1500-1550 we divide the observed number of plagues by 51 and multiply by 23). Regression specifications and controls correspond to Table 6 column 2 (without region fixed effects) and column 4 (with region fixed effects). Standard errors are clustered by region. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***".

Our results are further supported by another set of comparisons: how the plague instrument shifted adoption and growth year-by-year in the subset of cities that had not

adopted up to that point. We thus compare plagues, adoption, and growth in the set of cities that survived as candidates for adoption in any given year. We find that the first stage relationship between recent plagues and institutional change initially strengthened after 1522 and that the relationship between induced institutional change and growth remained relatively stable over the first half of the 1500s. This analysis comparing the effects of the instrument as it gets "turned on" at different times for different cities provides an additional external validity check on our baseline estimates and is presented in Appendix D.

7 Conclusion

Public goods may be under-provided by private markets and entrenched political interests. A large literature studies how democratic voting or military competition may promote the provision of public goods. In contrast, we focus on citizen-led, quasi-revolutionary political action. This action occurred when the introduction of religious competition during the Protestant Reformation transformed the political landscape, resulting in expansions of public goods provision in non-democratic settings. The expansion of public goods provision fostered human capital accumulation and ultimately led to long-run growth.

Our research shows how the introduction of religious competition precipitated profound changes in the role of the state. The Reformation interacted with local political economy, leading to the expansion of public goods provision. This expansion of public goods – and not the adoption of Protestantism – had unintended positive consequences for growth.

While the Reformation was a unique event, our results are general. To be sustained, expanded public goods provision must be a political and institutional equilibrium. We provide evidence on canonical changes in public goods institutions, including expansions of social welfare provision and the establishment of public education. Critically, the state expanded public education in order to produce the skilled administrators it needed to efficiently provide a broad set of public goods. Our findings thus indicate how institutions and human capital inputs directed to public goods provision are mutually self-reinforcing.

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Appendices – For Online Publication

A Data

A.1 Summary Statistics for Cross-Sectional Analysis

Table A1 presents summary statistics for cities that did and did not adopt Reformation laws.

Table A1: Summary Statistics on City Populations and Characteristics

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

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

A.2 Institutional Change – Legal Institutions

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

- Emil Sehling editor, Volume I Sachsen und Thüringen nebst angrenzenden Gebieten (1902) (Leipzig: O.R. Reisland).
- Emil Sehling editor, Volume II Sachsen und Thüringen nebst angrenzenden Gebieten (1904) (Leipzig: O.R. Reisland).
- Emil Sehling editor, Volume III Brandenburg, Ober- und Niederlausitz, Schlesien (1909).
- Emil Sehling editor, Volume IV Preußen, Polen, Pommern (1911).
- Emil Sehling editor, Volume V Baltische Länder, Mecklenburg, Lübeck, Lauenburg, Hamburg (1913).
- Institut für evangelisches Kirchenrecht der EKD, Volume VI/1 Niedersachsen (1955).
- Institut für evangelisches Kirchenrecht der EKD, Volume VI/2 Niedersachsen (1957).
- Institut für evangelisches Kirchenrecht der EKD, Volume VII/1 Niedersachsen (1963).
- Institut für evangelisches Kirchenrecht der EKD, Volume VII/2 Niedersachsen (1980).
- Institut für evangelisches Kirchenrecht der EKD, Volume VIII Hessen I: Landgrafschaft bis 1582 (1965).
- Institut für evangelisches Kirchenrecht der EKD, Volume XI Franken, (1961).
- Institut für evangelisches Kirchenrecht der EKD, Volume XII Schwaben (1963).
- Institut für evangelisches Kirchenrecht der EKD, Volume XIII Altbayern (1966).
- Institut für evangelisches Kirchenrecht der EKD, Volume XIV Kurpfalz (1969).

- Institut für evangelisches Kirchenrecht der EKD, Volume XV Baden-Württemberg I: Hohenlohe (1977).
- Heidelberger Akademie der Wissenschaften, Volume XVI Baden-Württemberg II: Württemberg, Baden u. a. (2004).
- Heidelberger Akademie der Wissenschaften, Volume XVII/1; XVII/2 Baden-Württemberg III/IV: Reichsstädte (2007/09).
- Heidelberger Akademie der Wissenschaften, Volume XVIII Rheinland-Pfalz I: Zweibrücken, Veldenz, Sponheim u. a. (2006).
- Heidelberger Akademie der Wissenschaften, Volume XIX Rheinland-Pfalz II: Wild- und Rheingrafschaft, Leiningen, Wied u. a. (2008).
- Heidelberger Akademie der Wissenschaften, Volume XX/1 Elsass I: Straßburg (2011).
- Heidelberger Akademie der Wissenschaften, Volume IX Hessen II: Landgrafschaft ab 1582, Waldeck, Solms, Frankfurt u. a. (2011).
- Heidelberger Akademie der Wissenschaften,Volume X Hessen III: Nassau, Hanau-Münzenberg, Ysenburg (2012).
- Heidelberger Akademie der Wissenschaften, Volume XX/2 Elsass II: Hanau-Lichtenberg, Colmar, Mülhausen, Weißenburg u.a. (2013).
- Heidelberger Akademie der Wissenschaften, Volume XXI Nordrhein-Westfalen I: Die Vereinigten Herzogtümer Jülich-Kleve-Berg, das Hochstift und die Stadt Minden, das Reichstift und die Stadt Herford, die Reichsstadt Dortmund, die Reichsabtei Corvey, die Grafschaft Lippe, das Reichsstift und die Stadt Essen (2015).
- Heidelberger Akademie der Wissenschaften, Volume VII/2/2/2 Niedersachsen: Die außerwelfischen Lande; Grafschaft Schaumburg, Goslar, Bremen (2016).

We also review Richter (1846), *Die evangelischen Kirchenordnungen des sechszehnten Jahrhunderts*. For select additional cities we consult additional sources: Thomas A. Brady, Heiko Augustinus Oberman, James D. Tracy, *Handbook of European History 1400 - 1600:*

Late Middle Ages, Renaissance and Reformation (Leiden: Brill, 1994); Joseph Guerber, *Haguenau et la Réforme* (Lyon: Le Roux L.F., 1861); Jürgen Sanowsky, "Vorgeschichte und Anfänge der Reformation in der Ballei Brandenburg des Johanniterordens", in Johannes Mol et al. eds., *Military Orders and the Reformation: Choices, State Building, and the Weight of Tradition* (Amersfoort: Uitgeverij Verloren, 2006); Peter Blickle, *Communal Reformation: The Quest for Salvation in Sixteenth-Century Germany* (Leiden: Brill, 1992); and Cameron (1991).

A.3 Institutional Treatment and Religious Heterogeneity

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

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

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

The city of Amberg in Bavaria provides an instructive example showing how Reformation institutions typically persisted in treated cities once long-run benefits of Reformation

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

The city of Augsburg provides another example of institutional persistence. In Augsburg, the Reformation was formally adopted and municipal social services were reorganized between 1534 and 1537, when Protestant guildsmen gained control of the city council (Broadhead 1979). In 1548, the Holy Roman Emperor Charles V reestablished a form of Catholic rule in Augsburg that allowed Protestant institutions to persist: rule by an elite of Catholic patricians, with reserved control of key government positions, over a Protestant majority city with active public service institutions shaped by the Reformation. Under this arrangement Augsburg experienced peaceful co-existence without institutional reversals into the 1600s. Stein (2009; p. 73) observes: "At no point did the Catholic-dominated patrician council attempt to re-catholicise the city; instead it governed with discretion in order to minimise [sic] tensions with the Protestant community... The admission practices of all the city-run health care institutions reflected this strategy of confessional tolerance."[69]

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

[69] 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).

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 a Reformation ordinance.[70] In our data there are several cities with ambiguous religious histories. For example, Erfurt adopted the Reformation in 1521 but became officially biconfessional in 1530. Ravensburg also became biconfessional. Konstanz adopted the Reformation but was later re-Catholicized. Regensburg adopted the Reformation but the Catholic bishop formally retained his seat.

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

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

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

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

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

In this section we discuss how the *Deutsche Biographie* was designed to provide a comprehensive, universal, biographical dictionary of important economic, political, and cultural figures within German speaking Europe. We highlight the fact that the *Deutsche Biographie* was constructed with the specific objective to record the biographies of important economic, political, and cultural figures from all time, places, and social and religious groups within German-speaking Europe. We provide a review of the scholarly literature examining the scope and representativeness of the *Deutsche Biographie*. We then describe the nature of the information the *Deutsche Biographie* contains and how we use this information to construct our database on upper tail human capital.

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

The Deutsche Biographie as a Data Source

Deutsche Biographie is the definitive biographical dictionary of political, cultural, and economic figures in German history. The design and objectives of the *Deutsche Biographie*, scholarly assessments of its coverage, and our econometric results all strongly weigh against the possibility that our key findings are driven by a selective inclusion process into the *Deutsche Biographie*.[71]

The current *Deutsche Biographie* builds on and extends the original *Allgemeine Deutsche Biographie*, a 56 volume reference collection published 1875-1912. The online portal, www.deutsche-biographie.de, which we use to collect biographical data, also includes

[71] In addition, we document that the shifts in human capital in cities treated by Reformation laws were in the specific sectors targeted by these laws (government administration and church employment). The fact that the effects were in the targeted sectors and not universal across occupations suggests that our results are not picking up a general effect of cities with public goods institutions either producing, attracting, or recording more upper tail human capital in general.

articles published in *Neue Deutsche Biographie*, the successor of the *Allgemeine Deutsche Biographie*, that was initiated in 1953. The coverage of *Allgemeine Deutsche Biographie* included all German-speaking parts of Europe. It was organized with entries on important Jewish figures frequently written by Jewish scholars, entries on Austrians written by Austrian experts, and entries on Catholics written by experts on Catholic culture.

The *Deutsche Biographie* is regarded as the authoritative biographical dictionary for all regions of Europe in which German is spoken and German culture is prevalent. The *Deutsche Biographie* records individuals who made a "significant impact on developments in politics, economics, social life, scholarship, technology or the arts."[72] A strict structure is imposed on all biographical articles, simplifying the extraction of relevant information. In particular, the available information contained in the articles is structured as follows: 1) the full name, occupation, data and place of birth, date and place of death, tomb, religious denomination, 2) family (genealogy), 3) Career, achievements, critical evaluations, 3) List of selected works, 4) List of selected works, 5) list of sources and secondary literature, 6) references of portraits, 7) name of the author.[73]

The *Deutsche Biographie* was designed to address and eliminate selective inclusion. The original *Allgemeine Deutsche Biographie* was designed to document the "thoroughly pluralistic foundation" of German cultural achievement and to that end explicit effort was made to include religious and social minorities (Hockerts 2008: p. 238).[74] The editorial inclusion criteria targeted everyone, "whose deeds or works contributed to the development of Germany in history, science, art, trade, or business, in short in every corner of political or cultural life" (von Liliencron and Wegele 1875; p. V-VI). However, evidence suggests that the original *Allgemeine Deutsche Biographie* over-represented people active in the humanities and social sciences and under-represented scientists, entrepreneurs, and technicians (Hockerts 2008). The *Neue Deutsche Biographie* was initiated in 1953 to update the *Allgemeine Deutsche Biographie* and was specifically designed to eliminate the prior over-representation

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

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

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

of activities in the humanities and social sciences (Hockerts 2008). Hockerts (2008; p. 257-58) further observes that the *Neue Deutsche Biographie* includes, "relatively unknown, but yet important personalities from the back rows....which is the real value of a biographical lexicon." As such, the current edition and digital version, which we use in this paper, attempt to be universal in coverage and contain both new entries as well as updated information on entries in prior editions.

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

Construction of Database on Upper Tail Human Capital

Scope of the Data

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

For our study of the formation of human capital, we restrict our study to people born in our baseline set of 239 cities between the 1380s and 1770s with birthdate known. For our study of migration, we restrict our study to people migrating to our 239 cities and dying between the 1420s and 1810s. Our evidence on migrants includes people whose birthplaces

are rural towns, cities outside Germany, and in some cases unknown. Our evidence on migrants also includes people whose dates of birth are not known.

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

Classification of Types of Upper Tail Human Capital

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

We classify professions in occupational sectors as follows: (1) the *government* sector comprises all government, public administration, law, and legal services careers; (2) the *church* sector comprises all religious or Church occupations; (3) the *education* sector comprises all research, teaching, and science occupations; (4) the *business* sector comprises all careers in trade, finance, services, crafts, proto-industry, and industry; (5) the *arts* sector comprises all occupations in the visual, performing, and literary arts; (6) the *medicine* sector comprises all medical occupations.[75]

To illustrate how we classify upper tail human capital careers, Table A2 presents the top careers by frequency for the principal occupational sectors and the share of careers by sector.

When we construct our baseline data for analysis, we take the career sector as the unit to be classified. Of the individuals observed born between 1380 and 1769 in the *Deutsche Biographie*, approximately 62 percent have one career occupation and fewer than 100 have

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

four recorded occupations. In the data, approximately 78 percent of people born 1480 to 1769 pursued their significant professional and life activities in just one sector and 98 percent pursued their activities in no more than two sectors.[76] Most people with multiple occupations remain in one sector. For example, we observe people who were priests and theologians, both *church* occupations. However, in instances where a given individual worked in two sectors, his or her city is credited as having both upper tail human capital careers. An example of this kind of human capital, career structure, and data classification is Christian Lorentz von Adlershelm (1608-1684) of Leipzig, who was a jurist and merchant (*Kaufmann*) and is classified as *government* and *business*. An example of human capital in both education and government administration is Albert Philipp Frick (1733-1798), who was active as a professor (*Hochschullehrer*) and as a jurist. An example of human capital in *business* and *government* is Detlev Karl Graf von Einsiedel (1737-1810), who was a state functionary in Saxony (*Sächsischer Minister*) and an industrialist in the metal industries (*Eisenindustrieller*).

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

Table A2: Classification of Upper Tail Human Capital by Occupational Sector – Illustration

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

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

A.5 Data on City Population

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

This paper relies on Bairoch, Batou, and Chèvre (1988) as the principal source for data on city populations. The Bairoch, Batou, and Chèvre (1988) data (the "Bairoch data") are designed to record the populations of all European cities that had populations of at least 5,000 some time between 1000 CE and 1800 CE. The data are recorded at 100 year intervals through 1700 and then at 50 year intervals through 1850. Bairoch, Batou, and Chèvre (1988; p. 289) record data on the populations of urban agglomerations and make a special effort to include, "the 'fauborgs', the 'suburbs', 'communes', 'hamlets', 'quarters', etc. that are directly adjacent" to historic city centers.[77]

The Bairoch, Batou, and Chèvre (1988) database provides an unbalanced panel. Table A3 documents that over 70 percent of cities have population unobserved in one or more periods 1500, 1600, 1700. Over the period 1500 to 1800 only 66 of 239 cities are in the balanced panel. Over the period 1400 to 1800 37 of 239 cities are in the balanced panel. Our baseline analysis examines long-run populations for all 239 cities, including those that were small and for which population was not observed in 1500 in Bairoch, Batou, and Chèvre (1988).

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

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

Table A3: Summary of Cities in Balanced Panel for Population Data

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

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

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

We gather evidence on locations for which population data in 1500 are not observed in Bairoch, Batou, and Chèvre (1988) from the *Deutsches Städtebuch*. The *Deutsches Städtebuch* provides evidence on city populations and city sizes for all periods where these data are available. The *Deutsche Städtebuch* data (i) confirm that the cities that are not observed in Bairoch, Batou, and Chèvre (1988) were small settlements and (ii) document when settlements first appear with population data. Table A4 provides evidence on each individual city for which population data are missing in 1500, which in our baseline cross-sectional regressions we class together as the "population missing" category.

Table A4: Evidence on Cities with Data Missing in 1500

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

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Table A4 – continued from previous page

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

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Table A4 – continued from previous page

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

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Table A4 – continued from previous page

City	Evidence on Population and Size
Koblenz	First record is 674 hearths in 1560, 1,409 persons in 1663, destructions and epidemics (Vol. 4.3)
Koenigswinter	No information
Koethen	First record is 2,000 before 1418, ca 4,000 in 1758 (Vol. 2)
Konstanz	First record is ca 5,000 in 1418, 3,714 in 1763, many epidemics and destructions (Vol. 4.2)
Kuestrin	First record is 1,490 in 1623, town nearly completely destroyed by fire in 1490, many floodings throughout time
Krefeld	First record is 350 in 1604, 1,350 in 1650, 4,576 in 1740, many destructions by war (Vol. 3.3)
Krempe	First record is 4,000-5,000 in ca 1600 (Vol. 1)
Landshut	First record is 9,000 in 1450, 8,600 in 1560, many epidemics (Vol. 5.2)
Langensalza	First record is 5,029 in 1414, 3,930 in 1447, 3,145 in 1551, fire in 1506 (Vol. 2)
Leer	First record is 3,500 in 1600 (Vol. 3.1)
Lauban	First record is 3,915 in 1706 (Vol. 1)
Ludwigsburg	No information
Lüneburg	First record is 14,000 in 1618, many epidemics (Vol. 3.1)
Mannheim - Sandhofen	First record is ca 570 in 1450, ca 800 in 1572, 3,000 in 1663 (Vol. 4.2)
Marburg	First record is 3,404 in 1696, many epidemics 16th/17th century (Vol. 4.1)
Marienberg	First record is 1,100 in 1550, many epidemics (Vol. 2)
Merseburg	First record is 5,000 in 1600, many epidemics (Vol. 2)
Muelheim a.d. Ruhr	No information
Nauen	First record is 1,700 in 1705, 1514 severe fire (Vol. 1)
Neubranden-burg	First record is 4,711 in 1797 (Vol. 1)
Neuburg a.d. Donau	No information
Neuss	First record is 3,555 in 1771 (Vol. 3.3)
Neustrelitz	First record is 1,619 in 1745 (Vol. 1)
Norden	No information
Neisse	First record is 4,500 in 1424, 7,344 in 1551 (Vol. 1)
Offenbach	First record is 480 in 1540 (Vol. 4.1)
Oppenheim	First record is 1,000+ between 600-1118, 4,000-5,000 between 1400s and 1600s (Vol. 4.3)
Paderborn	First record is 500+ in 1222, 4,500 in 1551, 1506 severe fire (Vol. 3.2)
Pforzheim	1501 epidemic (Vol. 1)
Pirna	First record is 600+ in 1547 (Vol. 2)

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Table A4 – continued from previous page

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

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Table A4 – continued from previous page

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

Table A4 also shows several cities that had attained populations above 5,000 at previous points in time. Our baseline results are robust to excluding these cities from the analysis or to instead incorporating indicator variables for population size observed in the *Städtebuch* at different moments in time that absorb variation in past population missed in the Bairoch data.

A.6 Regions and Principalities

Our analysis assigns cities to regions and to principalities as constituted in 1500. We map cities to 29 regions recorded in Nüssli (2008) and use the *Deutsches Städtebuch* to assign cities to 75 principalities (jurisdictions). The *Deutsche Städtebuch* is a multi-volume encyclopedia of German-speaking towns that provides information on the historical development of towns and cities, including legal jurisdiction (a complete list of the volumes is provided below).

We first present evidence on the assignment of cities to regions and principalities, and then further discussion. A list of cities and their regions and principalities follows. Each city is listed followed by: its region from Nüssli (2008); its principality or ruling house from the *Deutsches Städtebuch*.

> *Aachen*: Small States of HRE; Aachen (imperial free city). *Alt-Saarbruecken*: Lordships of the House of Nassau; Nassau-Saarbruecken. *Altenburg*: Electorate of Saxony; Ernestiner. *Altona*: Small States of HRE; Schaumburg. *Amberg*: Upper

Palatinate; Pfalz. *Andernach*: Small States of HRE; Koeln. *Anklam*: Duchy of Pomerania; Pommern. *Ansbach*: Small States of HRE; Ansbach, Brandenburg-Ansbach. *Aschaffenburg*: Electorate of Mainz; Mainz. *Aschersleben*: Small States of HRE; Halberstadt. *Augsburg*: Duchy of Lower Bavaria-Landshut; Augsburg (imperial free city). *Bad Kreuznach*: Electoral Palatinate; Pfalz. *Baden-Baden*: Margravate of Baden; Baden. *Bamberg*: Small States of HRE; Bamberg. *Bautzen*: Bohemia; Boehmen. *Bayreuth*: Small States of HRE; Ansbach, Brandenburg-Ansbach. *Beckum*: Small States of HRE; Muenster. *Berlin*: Electorate of Brandenburg; Brandenburg. *Berlin-Charlottenburg*: Electorate of Brandenburg; Brandenburg. *Berlin-Spandau*: Electorate of Brandenburg; Brandenburg. *Bernburg*: Principality of Anhalt; Anhalt-Koethen. *Biberach*: Small States of HRE; Biberach (imperial free city). *Bielefeld*: Small States of HRE; Paderborn. *Bingen*: Electorate of Mainz; Mainz. *Bochum*: Duchy of Cleves and County of Mark; Kleve-Mark. *Bonn*: Electorate of Cologne; Koeln. *Brandenburg*: Electorate of Brandenburg; Brandenburg. *Braunschweig*: Duchy of Brunswick-Wolfenbuettel; Braunschweig-Lueneburg. *Breisach*: Small States of HRE; Breisgau. *Bremen*: Small States of HRE; Bremen. *Breslau*: Bohemia; Boehmen. *Brieg*: Bohemia; Boehmen. *Bruchsal*: Small States of HRE; Speyer. *Burg b.M.*: Small States of HRE; Magdeburg. *Burscheid*: Duchies of Juelich and Berg; Juelich-Berg. *Celle*: Duchy of Brunswick-Lueneburg; Braunschweig-Lueneburg. *Chemnitz*: Duchy of Saxony; Albertiner. *Clausthal-Zellerfeld*: Duchy of Brunswick-Wolfenbuettel; Braunschweig-Grubenhagen. *Coburg*: Electorate of Saxony; Ernestiner. *Coesfeld*: Small States of HRE; Muenster. *Cottbus*: Electorate of Brandenburg; Brandenburg. *Darmstadt*: Landgraviate of Hesse; Hessen. *Dessau*: Principality of Anhalt; Anhalt. *Dinkelsbuehl*: Small States of HRE; Reichsbesitz/Kaiserlicher Besitz (imperial free city). *Doebeln*: Duchy of Saxony; Albertiner. *Dortmund*: Small States of HRE; Dortmund (imperial free city). *Dresden*: Duchy of Saxony; Albertiner. *Dueren*: Duchies of Juelich and Berg; Juelich-Berg (imperial free city). *Duesseldorf*: Electorate of Cologne; Juelich. *Duisburg*: Duchies of Juelich and Berg; Kleve-Mark (imperial free city). *Ebersbach*: Bohemia; Boehmen. *Eichstaett*: Small States of HRE; Eichstaett. *Eilenburg*: Electorate of Saxony; Ernestiner. *Einbeck*: Duchy of Brunswick-Wolfenbuettel; Braunschweig-Grubenhagen. *Eisenach*: Electorate of Saxony; Ernestiner. *Eisleben*: Small States of HRE; Halberstadt. *Emden*: County of East Friesland; Ostfriesland. *Emmerich*: Duchy of Cleves and County of Mark; Kleve-Mark. *Erfurt*: Small States of HRE; Mainz. *Erlangen*: Small States of HRE; Nuernberg. *Eschweiler*: Duchies of Juelich and Berg; Juelich-Berg. *Essen*: Duchies of Juelich and Berg; Essen. *Esslingen/Neckar*: Duchy of Wuerttemberg; Esslingen (imperial free city). *Forchheim*: Small States of HRE; Bamberg. *Frankenberg*: Duchy of Saxony; Albertiner. *Frankenthal*: Small States of HRE; Wormatiensis pagus. *Frankfurt*: Small States of HRE; Frankfurt (imperial free city). *Frankfurt (Oder)*: Electorate of Brandenburg; Brandenburg. *Freiberg*: Duchy of Saxony; Albertiner. *Freiburg/Br.*: Habsburg Monarchy; Habsburg. *Freising*: Small States of HRE; Freising. *Fulda*: Small States of HRE; Fulda. *Gelnhausen*: Small States of HRE; Reichsbesitz/Kaiserlicher Besitz (imperial free city). *Giessen*: Landgraviate of Hesse; Hessen. *Glatz*: Bohemia; Boehmen. *Glogau*: Bohemia; Boehmen. *Glueckstadt*: Kingdom of Denmark; N/A. *Goeppingen*: Duchy of Wuerttemberg; Wuerttemberg. *Goerlitz*: Bohemia; Boehmen. *Goettingen*: Duchy of Brunswick-Calenberg; Braunschweig-Wolfenbuettel (imperial free city). *Goldberg*: Bohemia; Boehmen. *Goslar*: Duchy of Brunswick-Wolfenbuettel; Goslar (imperial free city). *Gotha*: Electorate of Saxony; Ernestiner. *Greifswald*: Duchy of Pomerania; Pommern.

Grossenhain: Duchy of Saxony; Albertiner. *Gruenberg*: Bohemia; Boehmen. *Guben*: Bohemia; Boehmen. *Guestrow*: Duchy of Mecklenburg; Mecklenburg. *Halberstadt*: Small States of HRE; Halberstadt. *Halle a.d.S.*: Small States of HRE; Unbekannte Herrschaft. *Hamburg*: Small States of HRE; Hamburg (imperial free city). *Hameln*: Duchy of Brunswick-Calenberg; Welfen. *Hamm*: Duchy of Cleves and County of Mark; Kleve-Mark. *Hanau*: Small States of HRE; Hanau-Muenzenberg. *Hannover*: Duchy of Brunswick-Calenberg; Calenberg. *Harburg*: Small States of HRE; Oettingen. *Hattingen*: Duchy of Cleves and County of Mark; Kleve-Mark. *Heidelberg*: Electoral Palatinate; Worms. *Heilbronn*: Small States of HRE; Heilbronn (imperial free city). *Helmstedt*: Duchy of Brunswick-Wolfenbuettel; Braunschweig-Wolfenbuettel. *Herford*: Small States of HRE; Herford (imperial free city). *Hersfeld*: Small States of HRE; Hersfeld. *Hildesheim*: Duchy of Brunswick-Wolfenbuettel; Hildesheim. *Hirschberg*: Bohemia; Boehmen. *Hof*: Small States of HRE; Ansbach, Brandenburg-Ansbach. *Hueckeswagen*: Duchies of Juelich and Berg; Juelich-Berg. *Ibbenbueren*: Small States of HRE; Tecklenburg. *Ingolstadt*: Duchy of Upper Bavaria-Munich; Bayern-Landshut. *Iserlohn*: Duchy of Cleves and County of Mark; Kleve-Mark. *Jena*: Electorate of Saxony; Ernestiner. *Kaiserslautern*: Electoral Palatinate; Kaiserslautern. *Karlsruhe*: Margravate of Baden; N/A. *Kassel*: Landgraviate of Hesse; Hessen. *Kaufbeuren*: Small States of HRE; Kaufbeuren (imperial free city). *Kempten (Allgaeu)*: Small States of HRE; Kempten (imperial free city). *Kiel*: Kingdom of Denmark; Daenemark. *Kleve*: Duchy of Cleves and County of Mark; Kleve-Mark. *Koblenz*: Electorate of Trier; Trier. *Koeln*: Small States of HRE; Koeln (imperial free city). *Koenigswinter*: Electorate of Cologne; Koeln. *Koethen*: Principality of Anhalt; Anhalt-Koethen. *Kolberg*: Duchy of Pomerania; Cammin. *Konstanz*: Small States of HRE; Konstanz (imperial free city). *Krefeld*: Electorate of Cologne; Moers. *Krempe*: Kingdom of Denmark; Daenemark. *Kuestrin*: Electorate of Brandenburg; Brandenburg. *Landau*: Electoral Palatinate; Landau in der Pfalz (imperial free city). *Landsberg*: Electorate of Brandenburg; Brandenburg. *Landshut*: Duchy of Lower Bavaria-Landshut; Bayern-Landshut. *Langensalza*: Duchy of Saxony; Albertiner. *Lauban*: Bohemia; Boehmen. *Lauingen (Donau)*: Duchy of Lower Bavaria-Landshut; Bayern-Landshut. *Leer*: County of East Friesland; Ostfriesland. *Leipzig*: Duchy of Saxony; Albertiner. *Liegnitz*: Bohemia; Boehmen. *Lippstadt*: Small States of HRE; Kleve-Mark. *Ludwigsburg*: Duchy of Wuerttemberg; Wuerttemberg. *Luebeck*: Small States of HRE; Luebeck (imperial free city). *Lueneburg*: Duchy of Brunswick-Lueneburg; Lueneburg. *Magdeburg*: Small States of HRE; Semi-independent. *Mainz*: Electorate of Mainz; Mainz. *Mannheim*: Electoral Palatinate; N/A. *Marburg*: Landgraviate of Hesse; Hessen. *Marienberg*: Duchy of Saxony; N/A. *Meissen*: Duchy of Saxony; Albertiner. *Memmingen*: Small States of HRE; Memmingen (imperial free city). *Merseburg*: Duchy of Saxony; Mersburg. *Mettmann*: Duchies of Juelich and Berg; Juelich-Berg. *Minden*: Small States of HRE; Minden. *Mittweida*: Duchy of Saxony; Albertiner. *Muelheim a.d. Ruhr*: Duchies of Juelich and Berg; Broich. *Muenchen*: Duchy of Upper Bavaria-Munich; Bayern-Muenchen. *Muenster*: Small States of HRE; Muenster (imperial free city). *Nauen*: Electorate of Brandenburg; Brandenburg. *Naumburg*: Duchy of Saxony; Naumburg. *Neisse*: Bohemia; Boehmen. *Neu Ruppin*: County of Ruppin; Ruppin. *Neubrandenburg*: Duchy of Pomerania; Mecklenburg. *Neuburg a.d. Donau*: Duchy of Lower Bavaria-Landshut; Bayern-Landshut. *Neuss*: Electorate of Cologne; Koeln. *Neustrelitz*: Duchy of Mecklenburg; Mecklenburg-Schwerin. *Neviges*: Duchies of Juelich and Berg; Juelich-Berg. *Noerdlingen*: Small States of HRE; Noerdlingen (imperial free city). *Norden*: County of East Friesland; Ostfriesland.

Nordhausen: Small States of HRE; Nordhausen (imperial free city). *Nuernberg*: Small States of HRE; Nuernberg (imperial free city). *Offenbach*: Small States of HRE; Isenburg, Ysenburg. *Oppeln*: Bohemia; Boehmen. *Oppenheim*: Electoral Palatinate; Reichsbesitz/Kaiserlicher Besitz. *Paderborn*: Small States of HRE; Paderborn. *Passau*: Prince-Bishopric of Passau; Passau. *Pforzheim*: Margravate of Baden; Pfalz. *Pirna*: Duchy of Saxony; Albertiner. *Plauen*: Electorate of Saxony; Boehmen. *Potsdam*: Electorate of Brandenburg; Brandenburg. *Prenzlau*: Electorate of Brandenburg; Brandenburg. *Quedlinburg*: Small States of HRE; Quedlinburg. *Radevormwald*: Duchies of Juelich and Berg; Kleve-Mark. *Ratzeburg*: Small States of HRE; Sachsen-Lauenburg. *Ravensburg*: Small States of HRE; Ravensburg (imperial free city). *Regensburg*: Small States of HRE; Regensburg (imperial free city). *Reichenbach i. V.*: Electorate of Saxony; Boehmen. *Remscheid*: Duchies of Juelich and Berg; Kleve-Mark. *Rendsburg*: Kingdom of Denmark; Daenemark. *Reutlingen*: Duchy of Wuerttemberg; Reutlingen (imperial free city). *Rheine*: Small States of HRE; Muenster. *Rheydt*: Electorate of Cologne; Moers. *Rochlitz*: Duchy of Saxony; Albertiner. *Rostock*: Duchy of Mecklenburg; Mecklenburg. *Rothenburg o.d. Tauber*: Small States of HRE; Reichsbesitz/Kaiserlicher Besitz (imperial free city). *Rottenburg*: Small States of HRE; Habsburg. *Saarlouis*: Lordships of the House of Nassau; N/A. *Sagan*: Electorate of Saxony; Albertiner. *Salzwedel*: Electorate of Brandenburg; Brandenburg. *Schorndorf*: Duchy of Wuerttemberg; Wuerttemberg. *Schwaebisch Gmuend*: Small States of HRE; Schwaebisch Gmuend (imperial free city). *Schwaebisch Hall*: Small States of HRE; Schwaebisch Hall (imperial free city). *Schweinfurt*: Small States of HRE; Schweinfurt (imperial free city). *Schwerin*: Duchy of Mecklenburg; Mecklenburg. *Siegen*: Lordships of the House of Nassau; Nassau-Dillenburg. *Soest*: Duchy of Cleves and County of Mark; Kleve-Mark (imperial free city). *Solingen*: Duchies of Juelich and Berg; Siegburg. *Speyer*: Small States of HRE; Speyer (imperial free city). *Stade*: Small States of HRE; Bremen. *Stendal*: Electorate of Brandenburg; Reichsbesitz/Kaiserlicher Besitz. *Stettin*: Duchy of Pomerania; Pommern. *Stolp*: Duchy of Pomerania; Pommern. *Stralsund*: Duchy of Pomerania; Stralsund. *Straubing*: Duchy of Upper Bavaria-Munich; Bayern-Muenchen. *Stuttgart*: Duchy of Wuerttemberg; Wuerttemberg. *Torgau*: Electorate of Saxony; Ernestiner. *Trier*: Electorate of Trier; Trier. *Tuebingen*: Duchy of Wuerttemberg; Wuerttemberg. *Ueberlingen*: Small States of HRE; Ueberlingen (imperial free city). *Ulm*: Small States of HRE; Ulm (imperial free city). *Unna*: Duchy of Cleves and County of Mark; Kleve-Mark. *Velbert*: Duchies of Juelich and Berg; Juelich-Berg. *Verden*: Small States of HRE; Verden (imperial free city). *Warburg*: Small States of HRE; Paderborn (imperial free city). *Weimar*: Electorate of Saxony; Ernestiner. *Weissenfels*: Duchy of Saxony; Albertiner. *Wesel*: Duchy of Cleves and County of Mark; Juelich-Berg (imperial free city). *Wetzlar*: Small States of HRE; Wetzlar (imperial free city). *Wiesbaden*: Lordships of the House of Nassau; Nassau-Idstein. *Wismar*: Duchy of Mecklenburg; Mecklenburg. *Wittenberg*: Electorate of Saxony; Ernestiner. *Wolfenbuettel*: Duchy of Brunswick-Wolfenbuettel; Braunschweig-Wolfenbuettel. *Worms*: Electoral Palatinate; Worms (imperial free city). *Wuelfrath*: Duchies of Juelich and Berg; Juelich-Berg. *Wuerzburg*: Small States of HRE; Wuerzburg. *Wuppertal-Barmen*: Duchies of Juelich and Berg; Aachen. *Wuppertal-Elberfeld*: Duchies of Juelich and Berg; Juelich-Berg. *Xanten*: Duchy of Cleves and County of Mark; Kleve-Mark. *Zeitz*: Duchy of Saxony; Naumburg-Zeitz. *Zerbst*: Principality of Anhalt; Anhalt. *Zittau*: Bohemia; Boehmen. *Zuellichau*: Bohemia; Brandenburg. *Zweibruecken*: Electoral Palatinate; Pfalz. *Zwickau*: Electorate of Saxony; Ernestiner.

Our assignment above abbreviates the Holy Roman Empire with "HRE" and uses the "Small States" regional category as defined by Nüssli (2008).

It is important to observe that jurisdiction changed over time and could be overlapping or contested (Whaley 2012). For example, between 1462-1750, Pforzheim was a fief of the Margravate of Baden – i.e. under Baden's control – while owned by the Electoral Palatinate. Magdeburg was formally part of the territoral Archbishopric of Magdeburg (Erzstift Madgeburg), but was effectively self-governing and in constant conflict – including in military conflict – with the Archbishop of Magdeburg in our period. Indeed, in 1503 the Archbishop of Magdeburg retained his title but moved his residence to Halle an der Saale. Another example relates to the town of Glogau, which was a fief of the Bohemian Crown, and Gruenberg (today Zelenia Gora), which was itself a fief of Glogau. We use the 1521 tax register (*Reichsmatrikel*) of the Holy Roman Empire to identify cities constitutionally designated as "imperial free 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.)

A.7 *Deutsches Städtebuch* Data

The *Deutsches Städtebuch* provides information on notable historic construction events in German cities (Cantoni, Dittmar, and Yuchtman 2018). We present information on school construction from the *Deutsches Städtebuch*. Volume I: Erich Keyser (editor), *Deutsches Städtebuch, Handbuch städtischer Geschichte. Bd. I Nordostdeutschland* (Stuttgart: Kohlhammer, 1939). Volume II: Erich Keyser (editor), *Deutsches Städtebuch, Handbuch städtischer Geschichte. Bd. II Mitteldeutschland* (Stuttgart: Kohlhammer, 1941). Volume III: Erich Keyser (editor), *Deutsches Städtebuch, Bd. III Nordwest-Deutschland. 2, Westfalen. Westfälisches Städtebuch* (Stuttgart: Kohlhammer, 1954). Erich Keyser (editor), *Deutsches Städtebuch, Bd. III Nordwest-Deutschland. 3, Landschaftsverband Rheinland. Rheinisches Städtebuch* (Stuttgart: Kohlhammer, 1956). Volume IV: Erich Keyser (editor), *Deutsches Städtebuch, Bd. IV Südwest-Deutschland. 1, Land Hessen. Hessisches Städtebuch*

(Stuttgart: W. Kohlhammer, 1957). Erich Keyser (editor), *Deutsches Städtebuch: Handbuch städtischer Geschichte, Bd. IV Sudwest-Deutschland. 2, Land Baden-Wurttemberg. Teilbd. Württemberg. Württembergisches Städtebuch* (Stuttgart: Kohlhammer, 1962). Erich Keyser (editor), *Städtebuch Rheinland-Pfalz, Saarland, Mainz, Stadtkreis* (Stuttgart: W. Kohlhammer, 1964). Volume V: Erich Keyser (editor), *Deutsches Städtebuch, Bd. V Bayern. 1, Bayerisches Stadtebuch* (Stuttgart: Kohlhammer, 1971). Erich Keyser (editor), *Deutsches Städtebuch, Bd. V Bayern. 2, Bayerisches Stadtebuch* (Stuttgart: Kohlhammer, 1974).

A.8 Publications Data

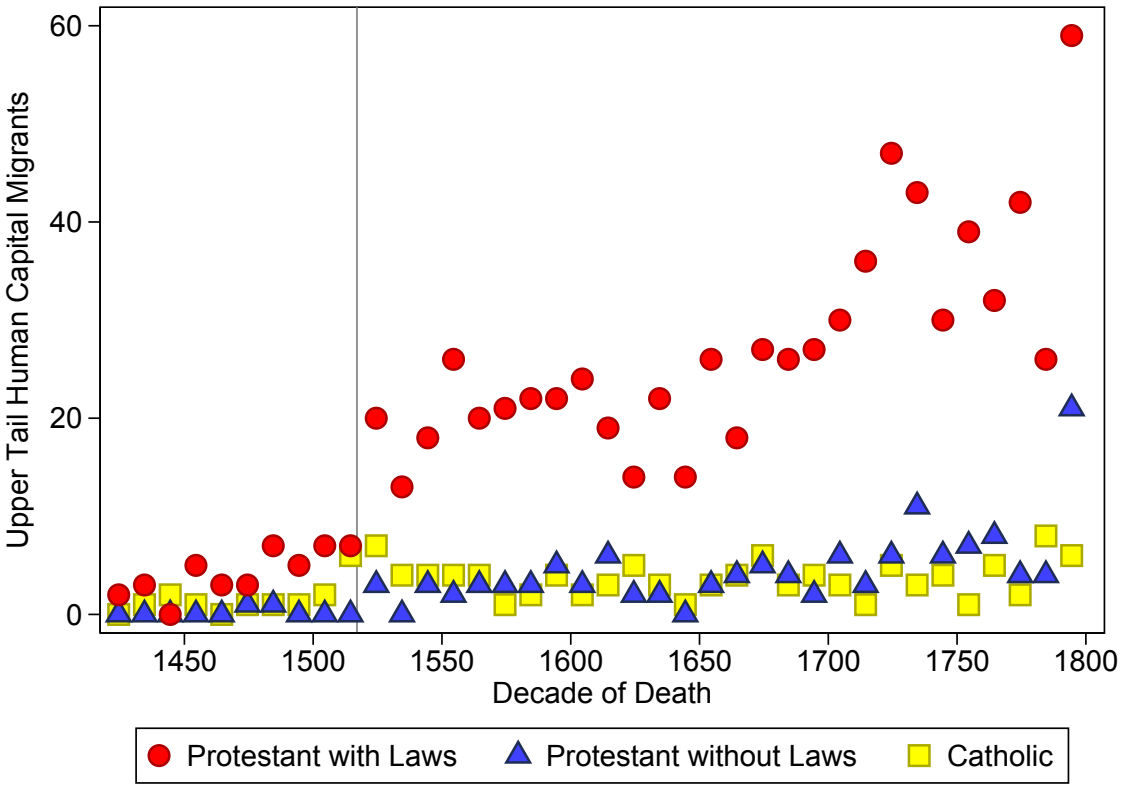
Data on publications are from the *Universal Short Title Catalogue* (USTC), which catalogues all known books and pamphlets printed in Europe before 1600: www.ustc.ac.uk/. Data downloaded in 2012. We use the *Universal Short Title Catalogue* classification of religious, political, and jurisprudence publications. Dittmar and Seabold (2017) provide a detailed review of the *Universal Short Title Catalogue*.

B Upper Tail Human Capital

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

B.1 Disaggregating Untreated Cities

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



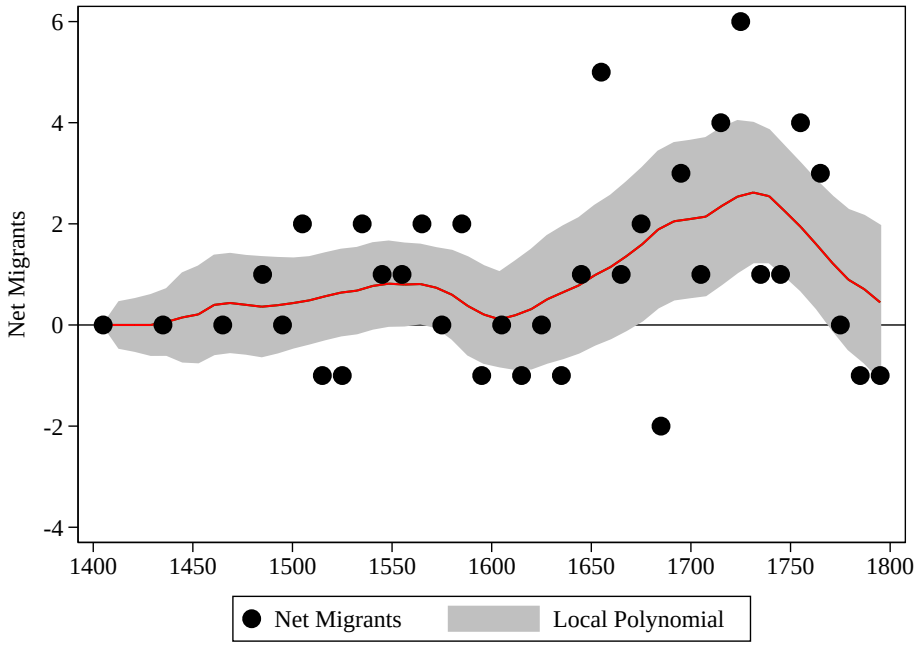
This graph plots the number of migrants observed in the *Deutsche Biographie* at the decade level for cities grouped as (1) Protestant cities with Reformation laws, (2) Protestant cities without Reformation laws, and (3) Catholic cities also without laws. Migrants are identified as people living and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

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

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

B.2 Net Migration from Untreated to Treated Cities

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



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

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

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

B.3 Defining the Treatment Period

In the main body of the paper, we use regression analysis to quantify differences in upper tail human capital across cities. To interpret our findings, it is is important to understand the research design. In the regression analysis, we study the formation and migration of upper tail human capital over 50 year periods. To study the formation of human capital we assign individuals to the fifty-year period in which they were 40 years old.[78] 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.[79]

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.

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

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

Table B1: Formation of Upper Tail Human Capital

	[1]	[2]	[3]	[4]	[5]	[6]
	Log	Binary Outcome: Number of People Crosses Threshold				
	People	Any	50th Pct.	75th Pct.	90th Pct.	95th Pct.
Post	0.71***	0.51***	0.40***	0.13***	0.01	0.01
	(0.11)	(0.07)	(0.08)	(0.04)	(0.01)	(0.01)
Post × Law	0.79***	0.10	0.26***	0.33***	0.19**	0.11***
	(0.19)	(0.12)	(0.07)	(0.08)	(0.07)	(0.03)
Observations	478	478	478	478	478	478
R^2	0.77	0.75	0.72	0.65	0.61	0.54

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 adopted church 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 50th, 75th, 90th, and 95th percentiles in the post-period distribution of upper tail human capital. 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 regional level. Regions are from Euratlas.

B.4 Count Data Outcome

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

$$People_{it} = \theta_i + \alpha \cdot Post_t + \beta \cdot (Post_t \times Law_i) + \epsilon_{it}, \tag{7}$$

Table B1 documents the relationship between the adoption of a church ordinance ("Law") and the formation of upper tail human capital using using estimating equation (7). Column 1 studies shows that cities with Reformation laws produced significantly more upper tail human capital formation in the post-Reformation period. Column 1 measures upper tail human capital by the logarithm of the number of people in the *Deutsche Biographie* born in a given city plus one. Columns 2-6 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 3 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. Cities with public goods institutions were 22 percent more likely to be above the 75th percentile, 19 percent more likely to be above the 90th percentile, and 11 percent more likely to be above the 95th percentile.

Consistent with Table B1, our baseline results in the main text study the logarithm of upper tail human capital as an outcome, but are supported by analysis of the relationship between public goods institutions and the number (count) of upper tail human capital migrants and locally formed individuals observed in the data. Figure B3 presents parameter estimates of the relationship between public goods institutions and count outcomes using the specification from equation (2). Figure B3 plots the parameter estimates through 1745 for ease of presentation: The migration advantage for cities with laws becomes very large post-1750 – and when plotted this dominates and obscures the early period advantages highlighted by this graph and our broader analysis.

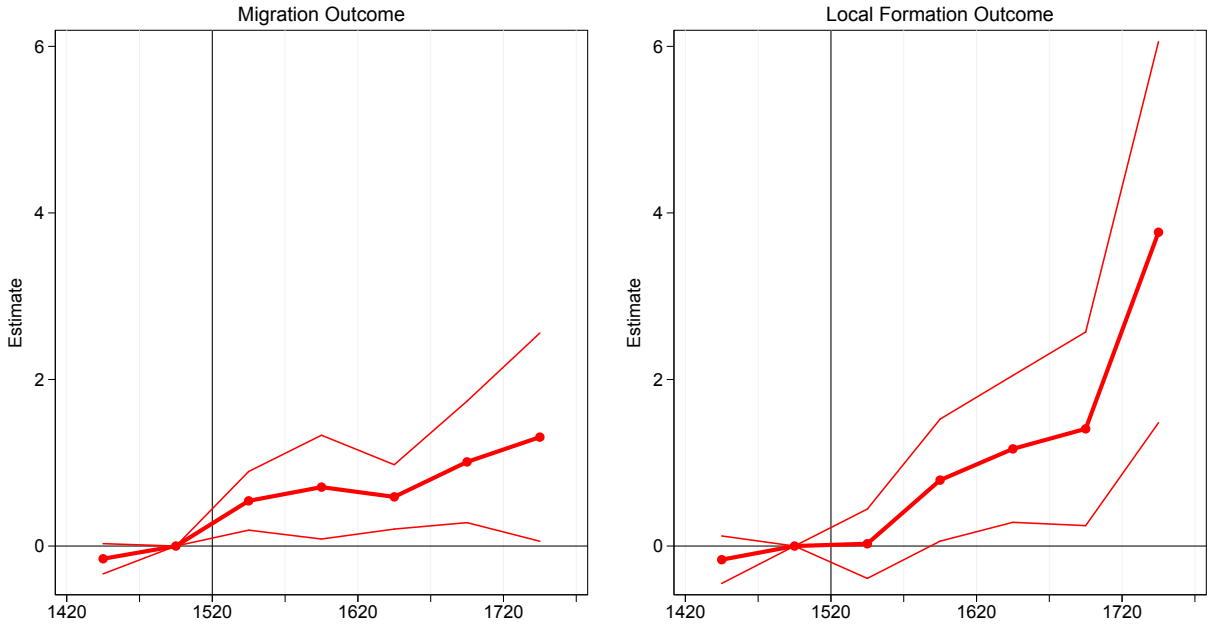
B.5 Human Capital Pre-Trends

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

To document similar human capital pre-trends, we study microdata on university students *by home town location* from Schwinges and Hesse (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.[80] Figure B4 plots an index of the number of students receiving degrees (the average number of degrees between 1400 and 1450 is indexed to 1). We use the index to compare the flow of degrees to students from cities that

[80]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 B3: Count Data Estimates of Institutional Change and Upper Tail Human Capital



This graph plots parameter estimates from regression analysis examining the differential migration and formation of upper tail human capital for cities that adopted Reformation law. The outcome variable is the count of the number of migrants(local formation) in city i at time t. 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 region-time periods and is estimated over data from 1420 through 1769. The post-Reformation periods begin with the 1520-1570 period (centered on 1545).

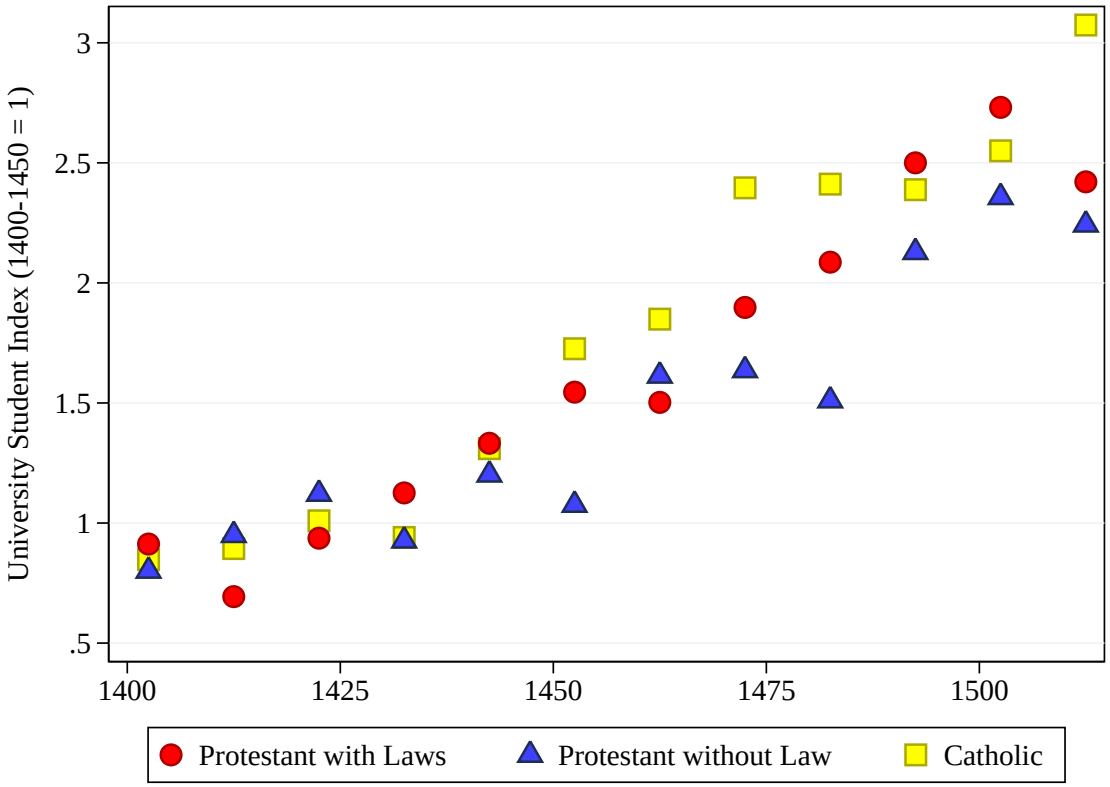
got Protestant laws, cities that adopted Protestantism in religion but did not get Protestant laws, and always Catholic cities. Figure B4 shows that cities that did and did not get Reformation laws exhibited similar pre-Reformation trends in university degrees earned by hometown students.

We use regression analysis to test for prior differences in university student trends and find no significant pre-1520 differences between cities that did and did not adopt institutional change. We estimate regressions of the form:

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

Here α_i and δ_t are city and time fixed effects and the parameter of interest is β which

Figure B4: Pre-Trends in University Degrees



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

estimates the differential trend for cities with laws ($Trend_t \times Law_i$).

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

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

In our baseline analysis, we examine all individuals in the *Deutsche Biographie*. In this subsection, we restrict analysis to upper tail "super-star" individuals. We focus on super-stars in order to study individuals (1) who were especially important and (2) for whom

Table B2: University Student Trends Before Institutional Change

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

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

selective inclusion into the *Deutsche Biographie* is not plausible. We show that our baseline results are robust in data on super-stars.

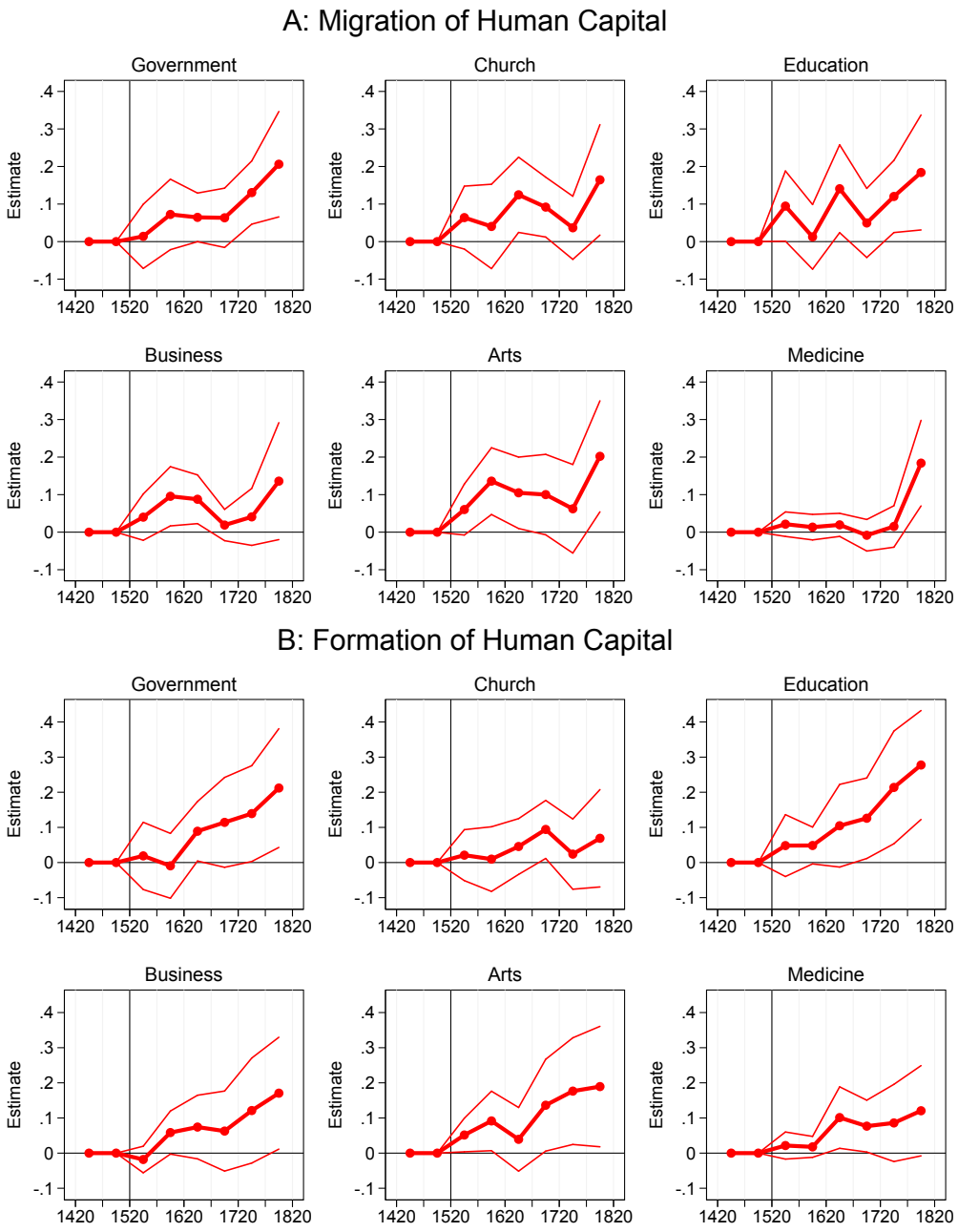
We restrict the data to "super-star" individuals who were the most important figures *within* the *Deutsche Biographie*. We do this by restricting analysis to indviduals with biographies with extended biographical essay, which account for slightly more than 25 percent of individuals in the *Deutsche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

We examine "super-stars" within the *Deutsche Biographie* in order to study (1) individuals for whom potential selection into the *Deutsche Biographie* is not salient and (2) the effects of institutions on human capital *within* the upper tail. We define "super-stars" as those individuals for whom the *Deutsche Biographie* provides an extended biographical essay.[81] These super-stars account for just over 25 percent of entries. We examine super-stars using our baseline difference in differences designs.

Figure B5 presents our results on the sectoral allocation of super-stars and shows strong responses in migration to Reformation laws, as well as some differences in which sectors

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

Figure B5: Institutions and Upper Tail Human Capital



This figure graphs regression estimates examining the relationship between institutional change and 'super-star' upper tail human capital at the city-sector-time-period level. The outcomes are binary variables for any super-star human capital individuals in a city-sector-time cell, estimated separately for migrants and local formation and defined as individuals with an extended biographical essay in the *Deutsche Biographie*. The figure presents parameter estimates on interactions between time period fixed effects and an indicator for cities that adopted Reformation Laws. All regressions control for city fixed effects, region-by-time-period fixed effects, and pre-1520 outcomes interacted with the complete set of time period indicators. Time is measured in 50-year periods 1420 through 1819. Sectors are as described in the text. Standard errors clustered at the city level are used to construct confidence intervals.

have the strongest responses when compared to our baseline results. In Panel A, we examine super-star migration. In Panel B, we examine super-star local formation and observe more gradual effects as in our baseline analysis. We find that by the 1700s, treated cities were producing more super-stars in business and the arts. These results are broadly consistent with our baseline findings.

C Institutional Change and City Growth

This section provides additional evidence to characterize the relationship between the adoption of church ordinances and city growth. We first study whether institutional change interacted with initial city characteristics to predict growth. We then consider evidence relating to the question of whether human capital was a channel for growth. We finally consider potential heterogeneity in church ordinances.

C.1 Interactions Between the Adoption of Church Ordinances and Prior City Characteristics

Our baseline analysis examines how the adoption of church ordinances predicts city growth. In this section we test whether these institutional change interacted with prior city characteristics to predict city growth.

We find no evidence that institutional change interacted with initial city characteristics to predict outcomes, with one exception. In *ex ante* large cities we find a *negative* differential relationship between the adoption of ordinances and population growth.[82] We find no differential human capital or growth effect for institutions in Free-Imperial cities, cities with many university students, cities with printing, or cities with market rights.

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

$$\ln(Population_{i,1800}) = \alpha_0 + \alpha_1 Law_i + \alpha_2(Law_i \times x_i) + \gamma X_i + \epsilon_i \tag{9}$$

Table C1 presents our results. Panel A studies upper tail human capital outcomes and shows that the direct effect of adoption is stable and significant and all the interactions are insignificant. Panel B studies long-run population outcomes and shows that the interaction between Reformation law and initial city size is negative and significant when it enters alone and maintains its magnitude but loses statistical significance when other interactions are introduced. The measure of initial size we study here is an indicator for "Large City in

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

1500" which distinguishes cities with populations of 6,000 or more. Cities with 6,000 or more were in the far upper tail: 80 percent of cities in our data had less than 6,000 inhabitants in 1500.

C.2 Human Capital as a Channel for Growth

City growth in pre-industrial Europe was driven by migration, not by demographic growth of resident populations (Bairoch 1991; De Vries 1984). We observe the migration of upper tail human capital, however data on total migration do not exist.[83] We examine observed migration of upper tail human capital individuals over the period 1520 through 1770 as a proxy for unobserved total migration flows.

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

$$\ln(Pop_{i,1800}) = \alpha + \beta Law_i + \theta(Migration_{i,1520-1770}) + \gamma X_i + \epsilon_i \tag{10}$$

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

Table C2 presents our estimates and shows (1) that migration of upper tail human capital is a powerful and statistically significant predictor of population in 1800 and (2) that conditional on the migration of upper human capital, there is a very small positive and statistically insignificant relationship between the adoption of ordinances and long-run populations. In Column 1 we replicate our baseline estimates showing that cities that adopted ordinances were approximately 25 percent larger in 1800 than observably similar cities that did not. In Column 2 we show that the church ordinance ("Law") effect becomes

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

Table C1: Church Ordinance Interactions with Pre-Characteristics

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

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

Table C2: Church Ordinances and Migration as Determinants of Long-Run City Population

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

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

insignificant and almost zero when we introduce migration flows as an explantory factor. In Column 3 we show these effects hold when we also control for the non-institutional diffusion of Protestantism. The fact that the effect of observed upper tail human capital migration dominates the church ordinance effect is striking, given that there is likely to be measurement error in our upper tail human capital variable operating as a proxy for unobserved total migration.

C.3 Potential Heterogeneity in Church Ordinances

Our investigation treats the adoption of a church ordinance as a measure of public goods institutions. Historical evidence supports the conclusion that church ordinances were public goods institutions broadly defined. Consider the case of the 1522 church ordinance in Wittenberg, the first ordinance in our data. The 1522 Wittenberg ordinance contains provisions on medical care, poor relief, and education. This ordinance also contains directly

religious provision (e.g. new regulations for church mass and the removal of images from the parish church). However, "Of the seventeen articles in the order, all but three were concerned with alleviating the plight of both the unemployed and the underemployed poor" (Lindberg 1977; p. 322), including through the provision of income support, medical care, and education to low income citizens.

In our analysis, the adoption of church ordinances is measured 1 or 0. However, our conceptual framework emphasizes that church ordinances were public goods institutions that expanded the provision of education, social welfare (notably through "poor law" provisions), and health care.

In this section, we provide additional evidence on the content of church ordinances. We provide evidence that church ordinances did in fact contain provisions on education, social welfare (poor law), and health care. We then use evidence on the content of church ordinances to consider potential heterogeneity and to test whether church ordinance provisions relating to the provision of these specific public goods explains variation in city growth.

C.3.1 Classifying Content of Ordinances

To support our analysis, we investigate the content of the church ordinances themselves, as reproduced in Sehling (1902-2016). To document the nature and characteristics of church ordinances more fully, individual ordinances were handcoded to indicate whether they contain provisions on: (1) education, (2) health care, (3) poor law, (4) public morality, and (5) church governance. Our methodology defines education provisions as any provisions making explicit provisions or rules for formal education, including the organization of schools, the pay of teachers, etc. We define health care provisions as any provisions making explicit provisions or rules for health care, including the organization and staffing of hospices or hospitals and access to services by doctors, midwives, or other practicioners. We define poor law provisions as any provisions that regulate the administration and oversight of poor law practice and resources (e.g. income or food support available through the "common chest"). We define public morality provisions as any provisions that explicitly govern public morality and conduct (e.g. involving public drunkenness and behavior). We define church governance provisions as any provisions regulating the human management and the political structure of the church (e.g. the pay, hiring, promotion of priests; regulation of church finances;

regulation of fees charged for religious services; and sources of income for church activities). To be clear, all church ordinances contain provisions and language that relate to religious belief and doctrine.[84]

It is important to understand the precise nature of the underlying data and our methodology for classifying church ordinances. Our analysis of the nature and characteristics of church ordinances focuses on the presence of different provisions and not the length of provisions for two reasons. First, length is a noisy measure of the intensity of treatment. The ordinances are "immensely prolix documents" (Strauss 1988; p. 193). Second, in some instances the complete text of ordinances is not reproduced in full in Sehling (1902-2016), limiting our ability to comprehensively measure the length of all provisions. For example, the Sehling (1902-2016) collection records the legal history for Chemnitz. Chemnitz passed a church ordinance with educational provisions and an ordinance with church governance provisions in the same year (Sehling 1902-2016). However, while Sehling reproduces the church governance ordinance, the text of the education provision is not reproduced (Sehling 1902-2016; Vol. 1, p. 539). This form of "missingness" in the complete text of laws means that we are unable to calculate the length of all provisions in all laws, although we are able to code whether a given ordinance had speciific provisions where this is explicitly recorded in Sehling (1902-2016). Moreover, in a number of cases Sehling (1902-2016) does not indicate explicitly what the content was in ordinances or provisions that are not reproduced. This generates a further ambiguity which we discuss below.[85] That said, we handcode both the binary presence of heterogeneous provisions (measured 1/0) and the length of observed provisions (measured in pages).

Table C3 presents summary statistics on the provisions observed in church ordinances. Column 1 reports summary statistics on the presence of different provisions. In the data, we find that 82 percent of cities with ordinances have educational provisions. We similarly find 82 percent had church governance provisions. The share with poor law and health care provisions observed is 46 and 44 percent, respectively. We find that public morality provisions are observed in 72 percent of cities. However, in 48 percent of cities there are ordinances or provisions that are not reproduced in full and which we are therefore unable

[84]The coding was completed by a team of native German-speaking graduate students.
[85]Our research assistants specifically measured length in quarter-page units.

Table C3: Provisions Observed in Church Ordinances

	[1] Indicator	[2] Length
Provision: Education	0.82	1.72
	(0.39)	(2.89)
Provision: Poor Law	0.46	0.84
	(0.50)	(1.86)
Provision: Health Care	0.44	0.34
	(0.50)	(1.16)
Provision: Public Morality	0.72	2.11
	(0.45)	(4.52)
Provision: Church Governance	0.82	3.19
	(0.39)	(4.36)
Provision: Not Reproduced in Full	0.48	
	(0.50)	

This table presents summary statistics on different types of church ordinance provisions at the city level across the 103 cities with ordinances in our data. Column 1 provides the mean (standard deviation in parentheses) for indicators that measure the presence of different types of provisions in church ordinances. The interpretation is that, for example, 82 percent of these cities have explicitly observed educational provisions. The last row reports the mean of an indicator recording whether some provisions are not reproduced in Sehling (1902-2016), and indicates that 48 percent of cities had one or more ordinances or provisions that are not reproduced in full. Column 2 provides summary statistics on the length of the observed provisions measured in pages.

to classify in terms of content. Column 2 presents summary statistics on the length of observed provisions. To be clear, a considerable number of ordinances and/or provisions are not reproduced in full.

C.3.2 Evidence on Ordinance Content and Long-Run Growth

To provide supplementary analysis on the relationship between content and long-run growth, we use our handcoded evidence on the specific provisions observed in church ordinances.

We estimate the relationship between variation in long-run city growth and different provisions in church ordinances. We estimate regressions that correspond to our baseline model for studying long-run city growth:

$$Y_i = \alpha_0 + \alpha_1 Law_i + \alpha_2 PublicGoods_i + \alpha_3 Church_+ \alpha_4 Morality_i + \gamma X_i + \epsilon_i$$

The outcome Y_i is log city population in 1800. Law_i measures the presence of a church ordinance (1/0). Similarly, $PublicGoods_i$ measures the presence of a provision for education,

poor law, or healthcare (1/0). We report estimates examining this composite measure of treatment, and where we examine education, poor law, and health care provisions separately and independently. $Church_i$ is an indicator for provisions on church governance, including the pay and assignment of priests and other organizational regulations, but not regulations concerning theology or the conduct of mass. $Morality_i$ is an indicator for provisions regulating public morality, e.g. drunkenness, violence, lewd behavior, and so on.

Table C4 reports our estimates. Column 1 presents our baseline OLS estimate of the relationship between the adoption of church ordinances and long-run population. Our specification corresponds to Table 5, column 5 (in the main text). All estimates thus control for the adoption of Protestantism and study variation within regions, which will absorb both (1) any historical characteristics shared by cities in the same region and (2) any variation in ordinance reproduction in Sehling (1902-2016) that is shared across cities in a given territory.[86] Column 2 introduces an indicator for the presence of an education or poor law or health care provision. Column 2 shows that the presence of these public goods provisions is a strong positive predictor of city growth, and that once we control for these public goods provisions the presence of an ordinance *per se* no longer explains variation in growth. Column 3 introduces indicators for provisions regulating public morality and church governance. This analysis shows that whereas education, poor law, and health care provisions are large and significant explanatory factors for long-run growth, public morality provisions have a weak and negative correlation with outcomes and that church governance provisions have effectively no correlation with growth. Column 4 introduces separate indicators for each type of provision, as well an an indicator for the presence of any church ordinance. We find that educational provisions are the largest explanatory factor and are statistically significant at the 90 percent level. Poor law and health care provisions are also positive explanatory factors. Individually, poor law and health care provisions are not statistically significant conditional on other provisions, however, they are jointly significant. We find that there is effectively no relationship between the presence of an ordinance ("Law") or the presence of a church governance provision and long-run city growth. We find that public morality provisions have a negative but statistically insigificant relationship to long-run growth. To be clear, in a small number of cities our coding suggests that church ordinances have purely

[86]The Sehling (1902-2016) volumes are organized by geographic territory.

religious content. This is what identifies the "Law" effect, which is insigificant conditional on public goods provisions.

Several important observations shape our interpretation of the estimates in Table C4. First, these estimates are suggestive correlations, not causal effects. In addition, the fact that some ordinances are not reproduced in full in Sehling (1902-2016) limits our ability to provide a comprehensive classification of content in every ordinance. For this reason, and because the classification of provisions is subject to some residual ambiguity, our estimates may reflect measurement error. With these caveats, our findings indicate that church ordinances explain growth insofar as they shifted the provision of key public goods (education, social welfare via the poor law, and health care), but not through other religious provisions or provisions concerning morality or church governance. In this respect, these findings are consistent with our broader analysis and interpretation.

C.3.3 Further Evidence on Religion, Institutions, and Development

In the main text, we present motivating evidence on printing and construction across cities that did and did not adopt public goods institutions. Here we provide further detail comparing (1) Protestant cities that adopted, (2) Protestant cities that did not adopt, and (3) Catholic cities that did not adopt.

In the main text, Figure 1 shows (A) books on jurisprudence, (B) school construction, and (C) social and administrative construction. In Figure C1 we present these data for all three types of cities. We observe that construction in Protestant cities without laws and in Catholic cities without laws follow similar trends (panels B and C), but that Protestant cities without laws printed fewer books on jurisprudence. In the data, there are only 6 Protestant cities without laws that were printing in 1517. More generally, the printing data suggests that Catholic cities may be a good counterfactual for Protestant cities with laws.

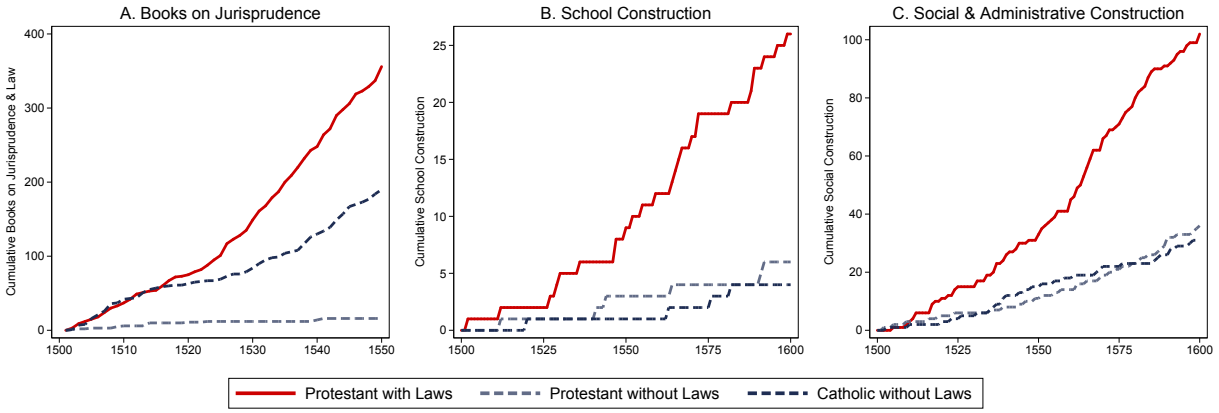
Figure C2 presents evidence religious printing across (1) Protestant cities that adopted, (2) Protestant cities that did not adopt, and (3) Catholic cities that did not adopt. It shows that printing was similar before 1517 in Protestant cities that adopted and Catholic cities that did not, and that all cities followed similar trends. The few Protestant cities with printing presses that did not adopt public goods institutions, initially printed few religious books, but by the late 1500s were printing as many as Catholic cities.

Table C4: Heterogeneity in Laws and City Populations

	[1]	[2]	[3]	[4]
	Outcome: Ln Population in 1800			
Law	0.28***	-0.11	-0.07	0.02
	(0.09)	(0.10)	(0.13)	(0.15)
Provision: Education, Poor Law, Health Care		0.49***	0.51***	
		(0.12)	(0.18)	
Provision: Education				0.37*
				(0.20)
Provision: Poor Law				0.18
				(0.11)
Provision: Health Care				0.14
				(0.09)
Provision: Public Morality			-0.14	-0.21
			(0.17)	(0.15)
Provision: Church Governance			0.05	0.02
			(0.12)	(0.15)
Controls	Yes	Yes	Yes	Yes
Observations	239	239	239	239
P-value: Poor Law & Health Care				0.02

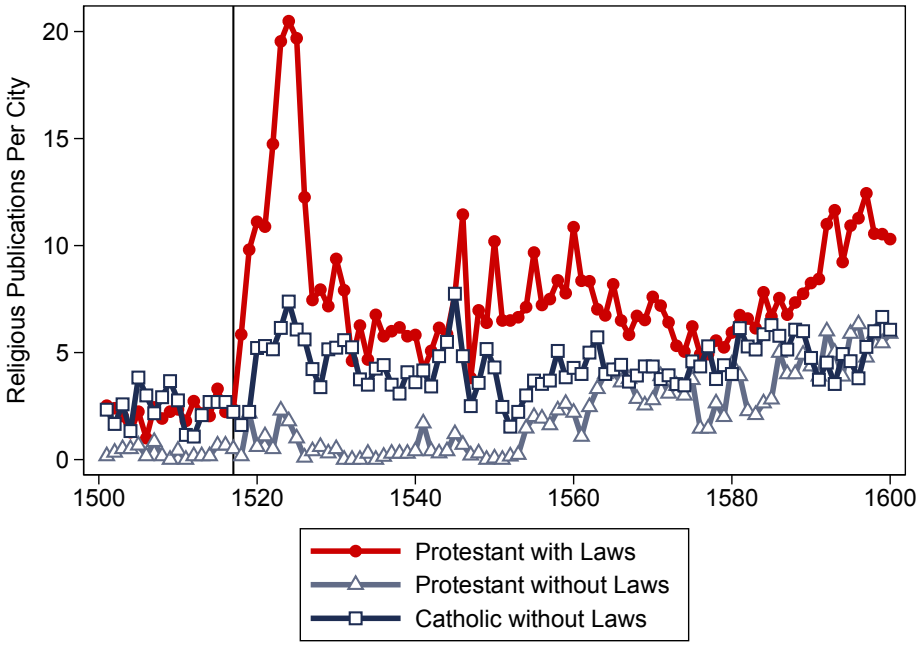
This table presents results from regressions estimating the relationship between the passage of potentially heterogeneous church ordinances and city population in 1800. The independent variables are indicators. "Law" is an indicator for the adoption of a church ordinance. "Provision: Education, Poor Law, Health Care" is an indicator for any provision on education, poor law, or health care. The remaining independent variables are indicators for the presence of specific provisions. Controls from Cantoni (2012): adoption of Protestantism; year city founded; indicators for rivers, Hansa cities, Free-Imperial status, monasteries, university, and printing; and distance to Wittenberg. All specifications include population fixed effects are indicators for 1500 population data: missing, 1,000-5,000, 6,000-10,000, 11,000-20,000, and 20,000+. All specifications include region fixed effects. Standard errors are clustered at the region level. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***". "P-value: Poor Law & Health Care" reports the p-value on the joint significance test for Poor Law and Health care provisions.

Figure C1: Jurisprudence and Construction



This graph shows the cumulative number of (A) jurisprudence publications, (B) new school construction, and (C) administrative and social welfare construction in Protestant cities that adopted public goods institutions, measured by the passage of a church ordinance ("Laws"), Protestant cities that did not adopt, and Catholic cities. Publication and construction data are from the *Universal Short Title Catalogue* and the *Deutsches Städtebuch*, respectively, for the 239 German cities we study in this paper. We classify all school construction as "School" construction and all construction of buildings used by city governments and in non-education social welfare and service provision as "Social & Administrative".

Figure C2: Religious Publishing



This graph shows the mean number of religious publications in (1) Protestant cities that adopted church ordinances ("Laws"); (2) Protestant cities that did not; and (3) Catholic cities that did not. Data on religious publications are from the *Universal Short Title Catalogue*.

D Plague as Instrumental Variable

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

D.1 The Mechanism – Plague and City Politics

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

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

or do not exist. Indeed, this motivates Biraben (1975) to collect evidence on the presence of major outbreaks rather than mortality counts. However, surviving evidence indicates remarkable dislocations. During the 1533 plague outbreak in Nürnberg a large 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).[87] Perhaps the starkest high-level evidence of the collapse of authority during plague outbreaks was the temporary dissolution of the Reichskammergericht, the highest court of the Holy Roman Empire, in 1540 when Speyer experienced a plague outbreak (Ahl 2004). In addition to the dissolution of the local order, the supply of other public goods frequently collapsed. To avoid contagion, city markets remained closed (Ibs 1993). Doctors, priests, and lawyers were among the first to flee, leaving the remaining population without help or means to care for the sick, bury the dead, write testaments, or settle the care for new orphans. With family solidarity and structures disappearing and local order dissolving, a "brutalization" of society frequently followed (Isenmann 2012). When the plague subsided, surviving populations usually had

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

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

Following plague outbreaks, cities experienced a large inflow of migrants that, at least in part, replaced the victims of disease. Significantly, a considerable number of migrants were wealthy enough to register as burgher and thus obtain formal political rights (city voting rights). The number of Neubürger (new burgher) often rose dramatically after plague outbreaks, indicating considerable mobility of skilled craftsmen, merchants, and wealthy elites. However, migrants usually did not receive the right to vote immediately. New migrants often had to reside in city for 5 to 10 years to be eligible to vote in the city council and therefore acquired a political voice through voting channels only years after plague outbreaks (Isenmann 2012).[88] This provides one reason why the effect of plague outbreaks on institutional change through the political process developed with lags.

In the main text, we examine religious and political publications as proxies for ideological competition. We show that plagues in the early 1500s led to increases in religious and political publications during the Reformation era. A natural question is whether our baseline analysis could embody a form of mean reversion: perhaps religious and political publications were low in the pre-period due to the plague shocks themselves. To address this question, we restrict our analysis to examine a pre-period free of potential contamination. We focus our analysis on plagues in the period 1510-1521 and drop these years from our analysis, so that the pre-period comprises only the first decade of the 1500s.

Table D1 presents both our baseline analysis (columns 1-4) and the results examining the restricted sample (columns 5-8). We find large and highly significant effects of plague shocks in the restricted sample. In fact, the results are stronger than in the baseline. This strong effect in the restricted sample indicates that religious and political publications did not decline in response to plague shocks in the pre-period. In fact, religious and political publications rose somewhat in response to plagues even in the pre-period, before their further large increase during the Reformation.

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

Table D1: Plagues and Religious and Political Publications

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Data: Complete Panel 1500 to 1600				Data: Drop Observations 1510 to 1522			
	Outcome: Religious Publications		Outcome: Political Publications		Outcome: Religious Publications		Outcome: Political Publications	
Post × Plagues 1500-1522	2.03***	2.04***	0.09***	0.09***				
	(0.45)	(0.47)	(0.02)	(0.02)				
Post × Plagues 1510-1522					6.48**	7.01**	0.21**	0.22***
					(2.82)	(2.96)	(0.09)	(0.09)
City Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	24139	24139	24139	24139	21032	21032	21032	21032
Mean Outcome	1.46	1.46	0.04	0.04	1.54	1.54	0.04	0.04

This table presents regression estimates examining the relationship between plagues and religious and political publishing outcomes. The outcome is the number of religious or political publications in a given city-year. Data on religious and political publications are from the Universal Short Title Catalog. The analysis examines printing across cities from 1500 through 1599. "Post × Plagues 1500-1522" is an interaction between an indicator for the post-1517 period ("Post") and the number of plagues in the ealry 1500s (Plagues 1500-1522). Columns 1 to 4 examine the complete data. Columns 5 to 8 drop the years 1510 through 1522 from the analysis. Standard errors are clustered by city. Statistical significance at the 90, 95, and 99 percent confidence level denoted "*", "**", and "***".

D.2 The Stability of Plague in Well-Connected Cities

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

Our IV analysis studies how plague shocks in the early 1500s shifted institutions conditional on long-run city-level plague propensity. We control for long-run plague propensity because historical evidence suggests that more connected cities were in general more likely to experience plague. In particular, cities on trade routes experienced plague more frequently but were also likely to grow because of their transportation and network advantages. To address this possibility, our primary results control for both long-run, time invariant plague prevalence before the 1500s and independently for plague in each quarter century before 1500 (Table 6).

The rate of plague outbreaks was stable in aggregate for over 100 years prior to the Reformation and during the Reformation. Figure D1 shows that the aggregate plague time series is stable over the period from 1400 to 1550.

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

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

overall stability of plague outbreak rates over this period.

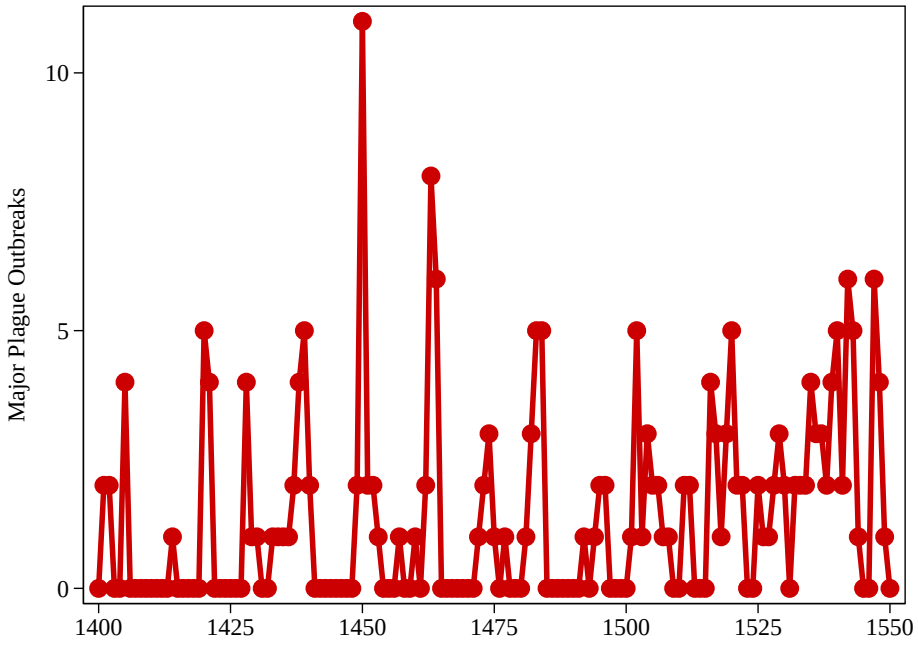
We examine three measures of city connectedness. We observe which cities were located on rivers, members of the Hanseatic League, and located in close proximity to neighboring cities. Navigable rivers provided access to cheap waterborne transport in early modern Europe. The cities of the Hanseatic League were located along the Baltic litoral, had locational advantages due to their access to Atlantic trade, and legal arrangements designed to support trade. More broadly, the density of the urban network varied across space as shown in Figure 3 (main text). To test for time-varying plague dynamics, we examine the relationship between plague outbreaks over 25-year periods and time varying functions of these measures of connectedness.

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

$$Plagues_{it} = \alpha_i + \delta_t + \beta(Connected_i \times Trend_t) + u_{it} \tag{11}$$

The dependent variable *Plagues* is the number of years with plagues observed in the city-level

Figure D1: Aggregate Plague Outbreaks



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

in 25-year periods. We estimate this equation using three measures of $Connected$: indicators for rivers ($River$), indicators for Hanseatic cities ($Hansa$), and the count of cities within 100 km ($Proximity$). We test whether there was any differential time trend in plagues in connected cities by regressing plagues on the interaction between measures of connectedness and a time trend ($Trend_t$). We estimate over the period 1400-1599 and include include city and time fixed effects (α_i and δ_t) in all specifications.

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

$$Plagues_{it} = \alpha_i + \delta_t + \sum_{s=1425}^{1575} \beta_s (Connected_i \times Time_s) + u_{it} \tag{12}$$

Table D2 presents our estimates and shows that the relationship between plague and city connectedness was stable between 1400 and 1599. 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. Specifically, there is no evidence that connected cities were differentially exposed to plagues in the early 1500s, when plague interacted with the introduction of religious competition during the Reformation.

D.3 The IV Period – Plagues Across First Half of the 1500s

This section expands on our baseline analysis by examining how plagues acted as institutional shifters across the first half of the 1500s. Our baseline IV for institutional change is the number of plagues between 1500 and 1522, the year the first church ordinance was adopted. In this section we first document how the relationship between plague shocks, institutional change, and growth evolved after 1522. By examining plagues after 1522 we provide evidence on the local average treatment effect the IV recovers and the dynamics of the Reformation. We then study plagues across the early 1500s through 1550 as the IV.[89] Our findings support our baseline estimates, as shown below.

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

Table D2: Plague Shocks and City Locations

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	Dependent Variable: Plagues per 25-Year Period							
River × Trend	0.00			0.00				
	(0.00)			(0.00)				
Hansa × Trend		0.00		0.00				
		(0.00)		(0.00)				
Proximity × Trend			-0.00	-0.00				
			(0.00)	(0.00)				
River × 1425					-0.12*			-0.12*
					(0.06)			(0.07)
River × 1450					-0.06			-0.12
					(0.08)			(0.10)
River × 1475					-0.10			-0.07
					(0.07)			(0.06)
River × 1500					-0.03			-0.00
					(0.11)			(0.10)
River × 1525					0.02			0.00
					(0.10)			(0.11)
River × 1550					0.13			0.11
					(0.10)			(0.10)
River × 1575					-0.02			-0.08
					(0.11)			(0.11)
Hansa × 1425						0.01		0.04
						(0.23)		(0.23)
Hansa × 1450						0.50		0.53
						(0.32)		(0.33)
Hansa × 1475						-0.23		-0.21
						(0.22)		(0.21)
Hansa × 1500						-0.15		-0.14
						(0.25)		(0.24)
Hansa × 1525						0.22		0.22
						(0.22)		(0.22)
Hansa × 1550						0.13		0.10
						(0.16)		(0.16)
Hansa × 1575						0.59		0.62
						(0.40)		(0.40)
Proximity × 1425							0.00	0.00
							(0.00)	(0.00)
Proximity × 1450							0.00	0.00
							(0.00)	(0.00)
Proximity × 1475							0.00	0.00
							(0.00)	(0.00)
Proximity × 1500							0.00	0.00
							(0.00)	(0.00)
Proximity × 1525							-0.00	0.00
							(0.00)	(0.00)
Proximity × 1550							-0.00	-0.00
							(0.00)	(0.00)
Proximity × 1575							0.00	0.00
							(0.00)	(0.00)
Observations	1912	1912	1912	1912	1912	1912	1912	1912

This table reports regression estimates examining the number of plague outbreaks in city-quarter-century cells. "River" and "Hansa" are indicators for navigable rivers and Hanseatic League membership. "Proximity" measures the number of cities within 100 kilometers. These measures of location are interacted with a linear time trend ("Trend") or time period indicators. Estimates include city and time period fixed effects and examine plagues 1400 through 1599. Standard errors clustered at city level.

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

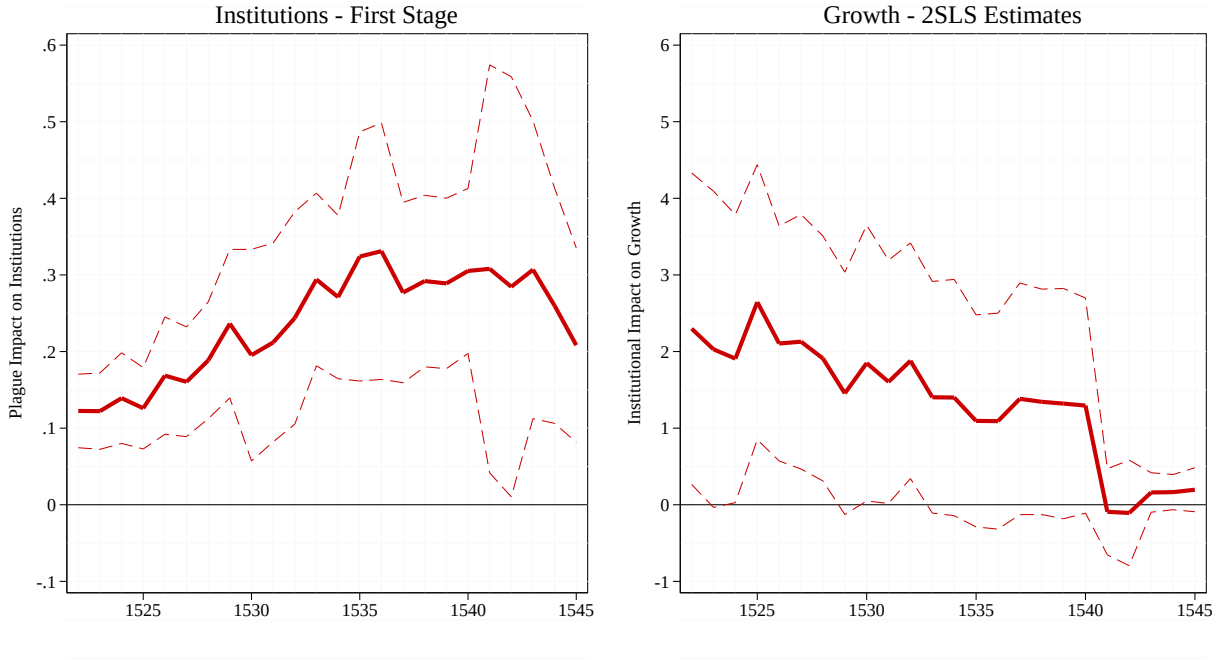
These changes in the instrument provide one kind of external validity check on our baseline IV estimates. They allow us to compare how shocks shaped institutional change and how institutions shaped outcomes for cities that had the instrument "turned on" at different times.

To understand how the instrument varies over time, consider the example of the city of Hannover. Hannover had no major plagues from 1500 to 1522. For Hannover the instrument is thus "turned off" in our baseline analysis (Table 6). 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 church ordinance in 1536.[90] When we now use the IV set-up to study the plague and induced institutional change during the Reformation, we keep Hannover in the sample each year through 1536, when it gets a law and is no longer a candidate for institutional change.

Figure D2 plots year-by-year (i) the first-stage estimates of the relationship between institutional change and plagues and (ii) the 2SLS estimates of the population growth impact of induced variation in institutions. Figure D2 shows that the first stage relationship between plague and institutional change strengthened over the initial years of the Reformation and then declined. In the early 1520s, one additional recent plague raised the probability of

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

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



This graph presents estimates from instrumental variable regressions for cities surviving without Reformation laws year-by-year. In each year, we estimate regressions examining the set of "surviving" cities that had not yet adopted a Reformation law as of that year. For each year, the instrument is the number of plague outbreaks in the previous twenty years. The left panel presents first stage regression estimates. The outcome is a binary variable for institutional change (measured by the adoption of a church ordinance). First stage regressions estimate how the probability of institutional change varied with the count of recent plague shocks, defined as outbreaks in the previous twenty years. The right panel presents 2SLS regressions that document how long-run city populations (log population in 1800) responded to induced variation in institutions (Reformation law). In each year, the regressions are estimated using recent city-level plague shocks as of that year. This graph plots how the annual estimates evolve. All regressions include the same control variables as in Table 6, including the mean level of plague in the 1400s and period-by-period plague across the 1400s. Standard errors are clustered at the region level. The red dashed line represents the 95 percent confidence interval.

institutional change by about 15 percent. By the mid-1530s, the estimate is over 25 percent. Figure D2 also shows that the relationship between institutions and growth is more stable, but slowly declines over time and then collapses to zero in the early 1540s, just before the Schmalkaldic war, which initiated a new era in which relatively few cities adopted institutional change, as discussed above.

D.4 Local Average Treament Effects – Interactions between Plague and City Characteristics

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

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

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

$$Law_i = c + \alpha\ Plagues_i + \beta(Plagues_i \times Characteristic_i) + \gamma X_i + \epsilon_i, \tag{13}$$

The outcome *Law* is a binary indicator for cities adopting church ordinances. 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 D3 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 (2017) that variations in media market competition mattered most in cities subject to lords.

Table D3: Plagues, City Characteristics, and Church Ordinances

	[1]	[2]	[3]	[4]	[5]	[6]
	Dependent Variable: Adoption of Church Ordinance					
Plagues	0.14***	0.19***	0.16***	0.15***	0.10***	0.18***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Plagues × Free City		-0.19***				-0.27***
		(0.05)				(0.09)
Plagues × Any Printing			-0.10**			0.13
			(0.05)			(0.10)
Plagues × Students				-0.00*		-0.00
				(0.00)		(0.00)
Plagues × Market Rights					0.13	0.06
					(0.10)	(0.10)
Free City	0.33***	0.35***	0.33***	0.32***	0.33***	0.36***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)
Any Printing		0.02	0.05	0.02	0.04	-0.01
		(0.13)	(0.14)	(0.13)	(0.14)	(0.13)
Students	-0.01	-0.00	-0.00	0.00	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Market Rights	-0.07	-0.06	-0.07	-0.07	-0.08	-0.07
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)
Observations	239	239	239	239	239	239
R^2	0.29	0.30	0.29	0.29	0.29	0.30

This table presents results from regressions estimating the relationship between plague outbreaks in the early 1500s and the adoption of church ordinances. The outcome is is an indicator variable whether a city had adopted a church 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 include all direct effects and the complete set of controls discussed in the main text (Table 6, column 1). 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 region level. Territories are from Euratlas.

D.5 Exclusion Restriction and Plausible Exogeneity

In the main text, we provide evidence on the relationship between plagues in different periods and long-run growth that is consistent with the exclusion restriction assumption for our IV estimates. Specifically, we show that plagues in the early 1500s had a strong and highly significant relationship to long-run city population, but that other plagues across the 1400s and 1500s do not (Table 7).

Here we provide a second test of our results by instead relaxing the exclusion restriction assumption. We use the Conley, Hansen, and Rossi (2012) methodology to estimate confidence intervals on our IV estimates that account for the fact that the IV may be only "plausibly exogenous", and that there could be some direct relationship between plagues and growth. When we do so, we find support for our baseline estimates and interpretation.

The Conley, Hansen, and Rossi (2012) methodology estimates confidence intervals on IV estimates that account for potential violations of the exclusion restriction. To fix ideas, consider an estimating model:

$$Y = \beta X + \delta Z + \epsilon$$

Here X is a potentially endogenous treatment and Z is an instrument. The corresponding "first stage" model is:

$$X = \Pi Z + \nu$$

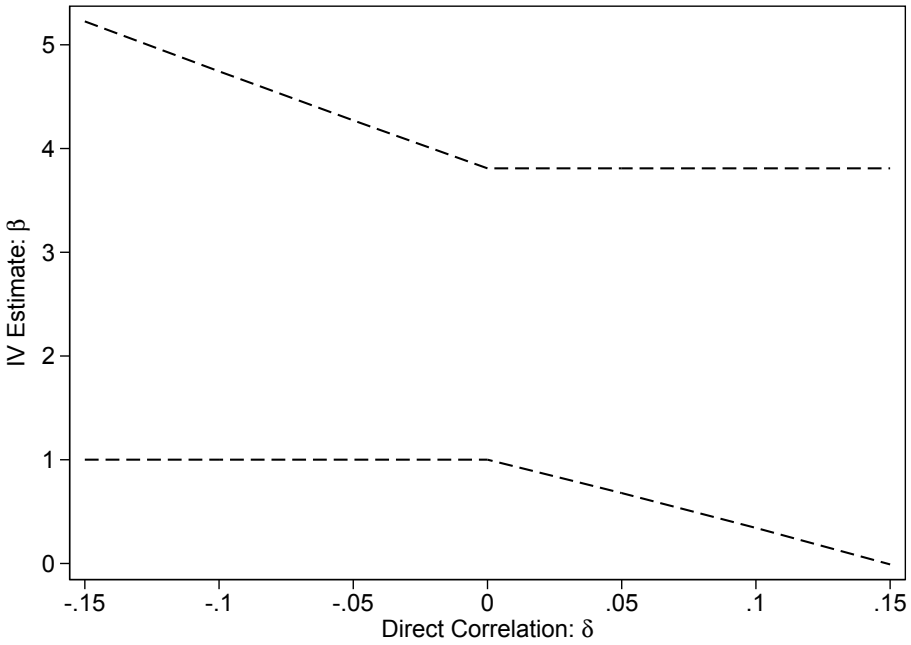
The Conley, Hansen, and Rossi (2012) methodology exploits the fact that the econometrician's priors may be used to investigate potential violations of the exclusion restriction, in particular the implications of a range of plausible non-zero values for δ, the direct correlation between the instrument Z and the outcome Y.

Our analysis of the reduced form relationship between the plague and long-run city population shapes our priors over δ. In Table 7 (column 4), we provide evidence on the correlation between plague shocks in a given time period and long run city population. Between 1400 and 1600, the point estimates on these conditional correlations lie between -0.06 and 0.13, except in our key treatment period running from 1500 through 1524. This evidence suggests that the direct correlation between plagues and outcomes could be expected to lie in the interval between -0.15 and 0.15. As a practical matter, our interest will be in

values where $\delta > 0$.

We implement the Conley, Hansen, and Rossi (2012) methodology and obtain confidence intervals around our IV estimate that vary with δ. Over the range of plausible δ's, our confidence interval remains positive, using Conley, Hansen, and Rossi's (2012) "union of confidence intervals" approach. We illustrate by plotting the estimated 95 percent confidence interval bounds for $\delta \in (-0.15, 0.15)$ in Figure D3 below.

Figure D3: Boundaries on IV Estimates Under Plausible Exogeneity



This graph shows Conley, Hansen, and Rossi (2012) union of confidence interval boundaries for IV estimates of the impact of church ordinances on the logarithm of city population in 1800. The specification corresponds to Table 6, column 3.

E Historical Evidence on Politics and Institutional Diffusion

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

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

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

The diffusion of the Reformation at the city level involved civil disobedience and a broad range of actions forbidden by or at odds with the policy of city councils. In Frankfurt am Main, the guilds revolted against the city council after the council ordered an end Protestant church services in 1524. A Bürgerausschuss (a burgher council) published 46 articles, which city council was forced to adopt when facing mobilized and armed citizens. With the defeat of the Protestant peasants in the Peasant's War (1525), the burgher council was dissolved and the 46 articles nullified. However, public pressure continued to build. By 1533 the city officially converted, and the legal articles were implemented.[91] Speyer, Worms,

[91] Sehling (1902-2016) Vol. IX, p. 473f.

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

The political power struggle berween citizens and city councils was often a lengthy one (Ozment 1975). The adoption of the Reformation in Hildesheim exemplifies the struggle between the city council and other disenfranchised interest groups. From the beginning, the city council of Hildesheim actively opposed the Reformation. In 1524, the council banned Protestant books. In 1525, the council and guilds urged the Catholic church to fight heretics (i.e. reformers). Nevertheless, clerics integrated Lutheran songs into their masses. These songs were subsequently outlawed in 1528, but in 1530 a Protestant preacher was welcomed with Lutheran songs. The city council responded with passing new law against Lutheran songs the next day, prompting 150 Protestants to go to the vesper in an act of civil disobedience. The city council then outlawed Lutheran or "Wittenberg" teachings and instituted fines for Protestants. While the local Protestants received some forms of support from the *Städtetag* (assembly of the free cities), they remained unable to establish Protestantism in Hildesheim in the 1520s. In 1532, the Hildesheim guild masters first sent for a Protestant preacher and in September of that year 150 burghers stormed the city hall to demand Protestant preaching. In response, the city council asked Catholic duke of Wolfenbüttel for protection. However, in 1534 the city also signed an alliance with Protestant cities ensuring mutual protection. After the death of the staunch anti-Reformation mayor Wildefüers (elected 1526), the city council relented and openly adopted Protestantism in 1542.[92]

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

[92] Sehling (1902-2016) Vol. VII/2/1, p. 797-806.

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

While the local rulers were typically opposed to the early Reformation, the strength of their opposition could vary. Local histories provide insight into how citizens navigated this opposition. In Göttingen, Protestant preaching was initiated in 1518, but the city council was concerned about the reaction of the local Catholic duke and did not adopt the Reformation. The first openly Protestant service was held in Göttingen in 1529. In that year, the burghers revolted against the city council and succeeded in forcing the provision of Protestant services. When the local duke subsequently demanded that the Protestant preachers leave Göttingen, the council obeyed. Shortly thereafter, the city council, fearing a further burgher revolt, sent the duke a formal statement declaring for Protestantism.[93] The Reformation movement in Hannover was similarly resisted by the local duke (Duke Erich d. Ältere) and the city council. In agreement with the duke, the city council passed an ordinance against Luther's teachings. After civil unrest in 1532, Lutheran teaching became dominant in Hannover, but the city council would not allow Protestant services. In 1533, renewed unrest forced the city council to flee. The duke was unable to broker a compromise. On July 31, 1534, the newly elected (Protestant) city council entered into a transaction in which the city provided a payment to compensate the duke for accepting Protestant preaching in the city.[94] 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.[95] In contrast, the Dukes of Bavaria emerged as particularly ardent defenders of Catholic orthodoxy in the 1520s, but over the

[93] Sehling (1902-2016) Vol. VI/2, p. 902.
[94] Sehling (1902-2016) Vol. VI/2, p. 941.
[95] Sehling (1902-2016) Vol. VI/2, p. 919.

1500s failed to eliminate the diffusion of Protestantism in their territory.[96]

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

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