

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.

JEL Codes: I25, N13, O11, O43

Keywords: Institutions, Political Economy, Public Goods, Education, Human Capital, Growth, State Capacity.

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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

¹Strauss (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).

²Acemoglu, 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

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

⁴When 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

⁵Opportunity 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.

⁶We 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.

⁷On 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

⁸Competition 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.

⁹Our 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.”

¹¹An 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

¹²See [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.

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

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

¹⁵In 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.”

¹⁶We 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,

¹⁷We 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

¹⁸Most 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.

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

²⁰Schooling 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.

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

²²Anti-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.

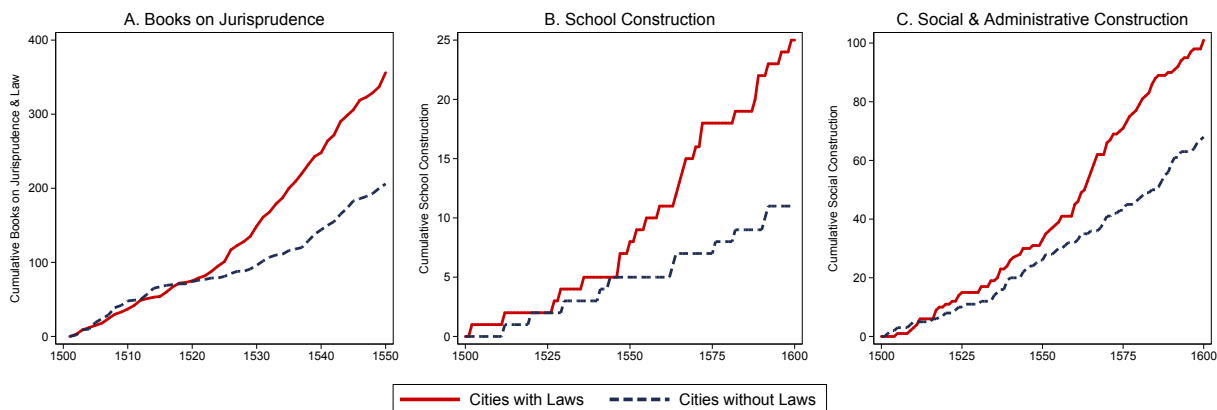
2.2 The Political Process

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

²³This 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,

²⁴Religious 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).

²⁵Princes’ 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.

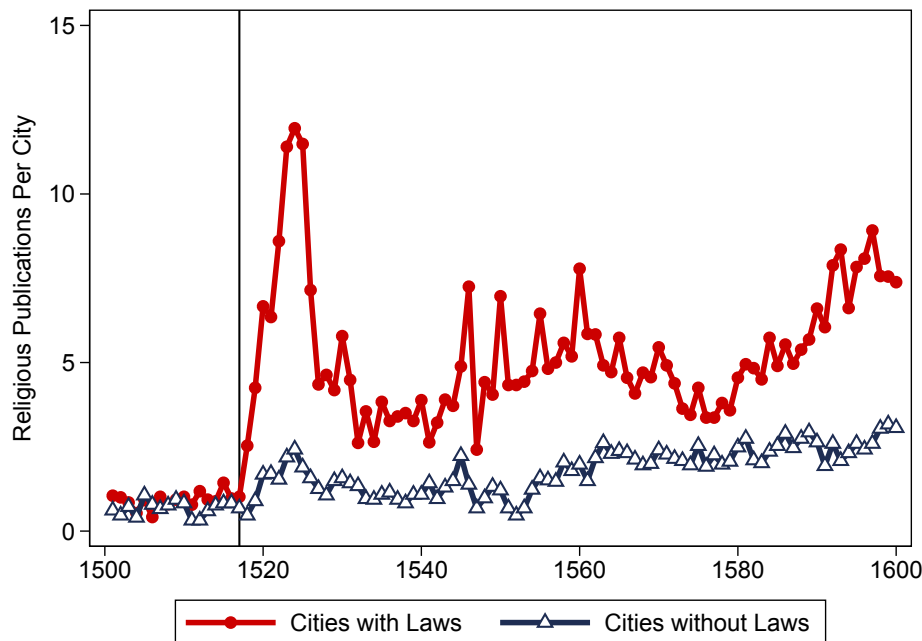
Religious printing provides unique, high frequency evidence on religious and political competition. The number of religious publications is an indicator of competition in the space of ideas and, more broadly, in religion and politics in the 1500s (Pettegree 2005; Dittmar and Seabold 2017). Figure 2 shows 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

²⁶The 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).

²⁷The 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.

²⁸Figure 2 examines the subset of cities that had printing presses in the 1500s (“ever printers”). Below we consider how religious and political printing increased during the Reformation in response to plague shocks.

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

These effects reflected increased 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

²⁹We 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.

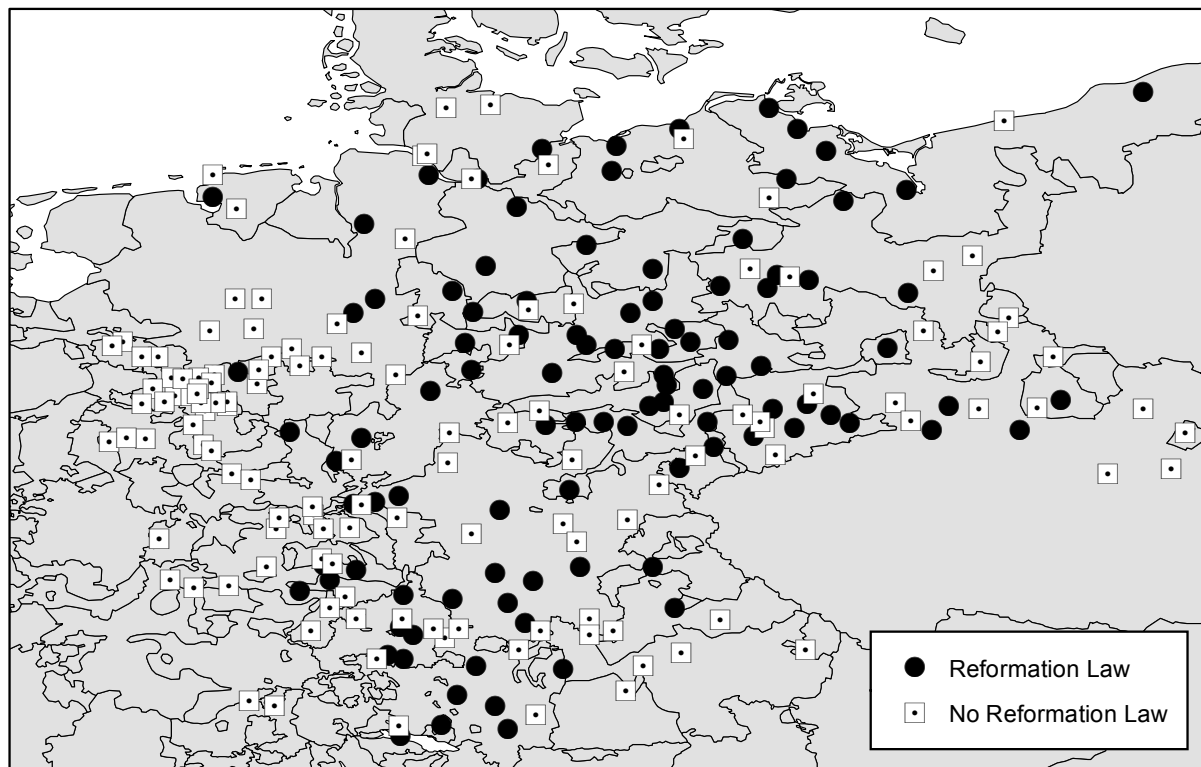
We provide discussion of the institutions and our classification in Appendix A and illustrative examples here. Bautzen is an example of a Protestant city which did not adopt institutional change, as discussed above (see also [Speer 2014](#)). Augsburg and Amberg are examples of cities where the institutions 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

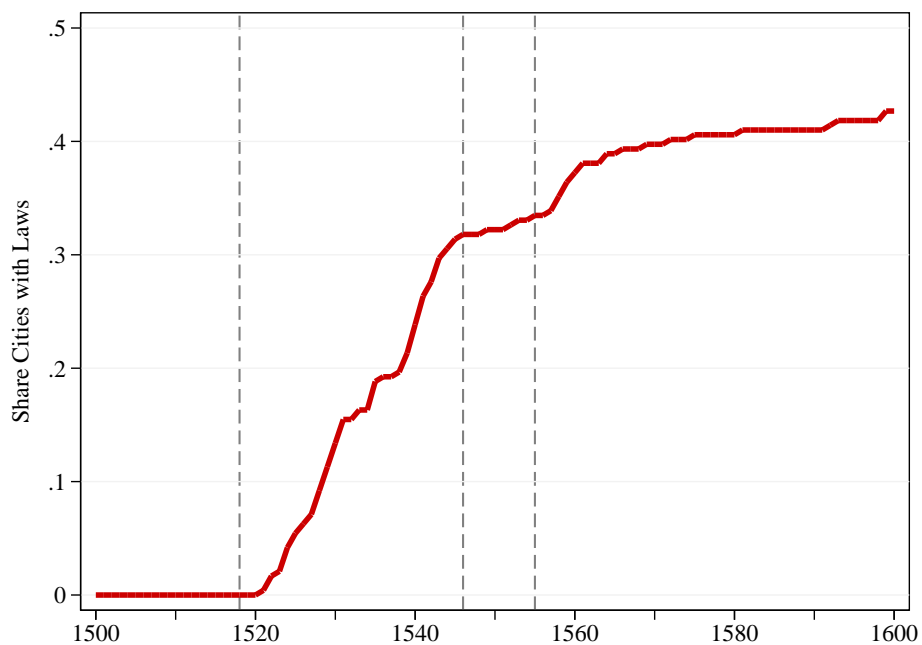
³⁰The settlement included a provision, *cuius regio, eius religio*, which allowed local rulers to dictate the religion in their realm, but maintained a complicated set of exceptions for cities where magistracies and offices were to be shared and largely respected facts on the ground ([Dittmar and Seabold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

³¹This 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.

³²We 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 cities.

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

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

³⁵For 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 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 \(2016; 2017\)](#) and the *Deutsches Städtebuch*. 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

³⁶The *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

Upper Tail Human Capital	Cities with Law			Cities without Law			Difference
	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***

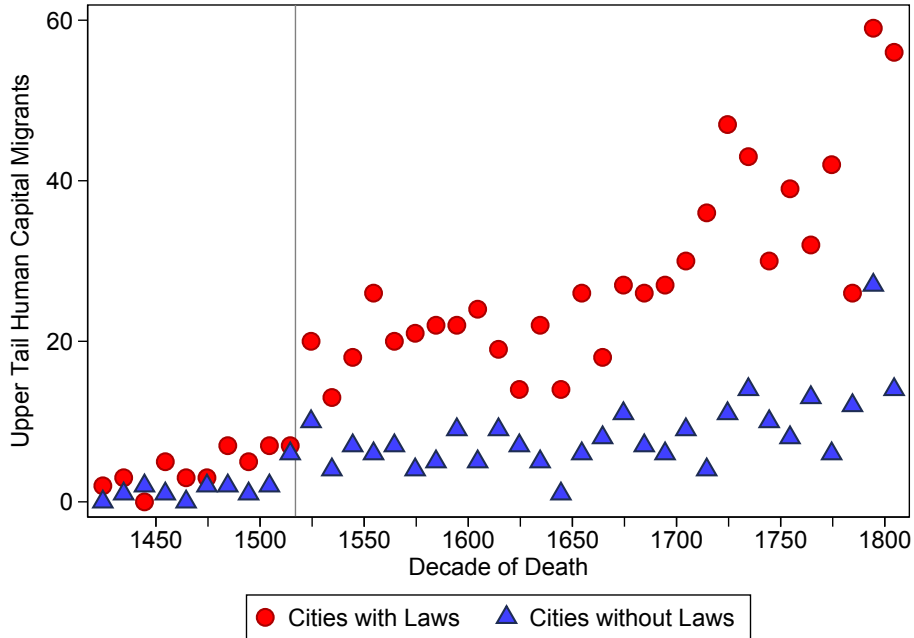
Upper tail human capital is measured by the number of people observed in the *Deutsche Biographie*. Locally born are people born in a given city i . Migrants to any given city i are individuals born in some other location j who died in city i . The last column presents the Hodges-Lehmann non-parametric 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.

³⁷In 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.

³⁸An 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

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

⁴⁰We 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]
<i>Panel A: Log Migration</i>										
	Complete Data: All Cities 1420-1819				Complete Data: All Cities 1420-1819			No Religious Heterogeneity		
Post × Law	0.22*** (0.06)	0.22*** (0.06)	0.26*** (0.07)	0.26** (0.11)	0.30** (0.15)	0.27*** (0.07)	0.33*** (0.07)	0.38*** (0.08)	0.35*** (0.10)	0.36*** (0.13)
Post × Trend × Law						0.07** (0.03)	0.09*** (0.04)	0.12*** (0.04)	0.11** (0.06)	0.09 (0.07)
Post × Protestant		-0.02 (0.05)	0.00 (0.06)	-0.05 (0.10)		-0.19*** (0.07)	-0.12 (0.09)	-0.12 (0.15)	-0.13 (0.15)	
Post × Trend × Protestant						-0.08* (0.05)	-0.08 (0.06)	-0.14 (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
<i>Panel B: Log Formation</i>										
	Complete Data: All Cities 1420-1819				Complete Data: All Cities 1420-1819			No Religious Heterogeneity		
Post × Law	0.33*** (0.06)	0.32*** (0.07)	0.37*** (0.07)	0.33*** (0.11)	0.33** (0.15)	0.24*** (0.06)	0.26*** (0.07)	0.23*** (0.08)	0.24** (0.10)	0.24* (0.12)
Post × Trend × Law						0.10*** (0.04)	0.11*** (0.04)	0.13*** (0.05)	0.08 (0.06)	0.07 (0.06)
Post × Protestant		0.03 (0.06)	0.01 (0.08)	-0.05 (0.13)		-0.07 (0.07)	-0.07 (0.07)	-0.12 (0.09)	0.03 (0.16)	
Post × Trend × Protestant						-0.03 (0.05)	-0.03 (0.05)	0.07 (0.05)	0.06 (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
<i>Controls</i>										
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

⁴¹We 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 B. We provide further historical evidence on religious heterogeneity in Appendix E. We do not observe the principality of 5 cities in the *Deutsches Städtebuch*.

⁴²When 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.

⁴³Detailed 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.

⁴⁴Our 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) and the *Deutsches Städtebuch*.

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:

$$\ln(\text{People}_{it} + 1) = \theta_i + \delta_t + \sum_{s=1420}^{1770} \beta_s(\text{Law}_i \times \delta_s) + \epsilon_{it} \quad (2)$$

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.

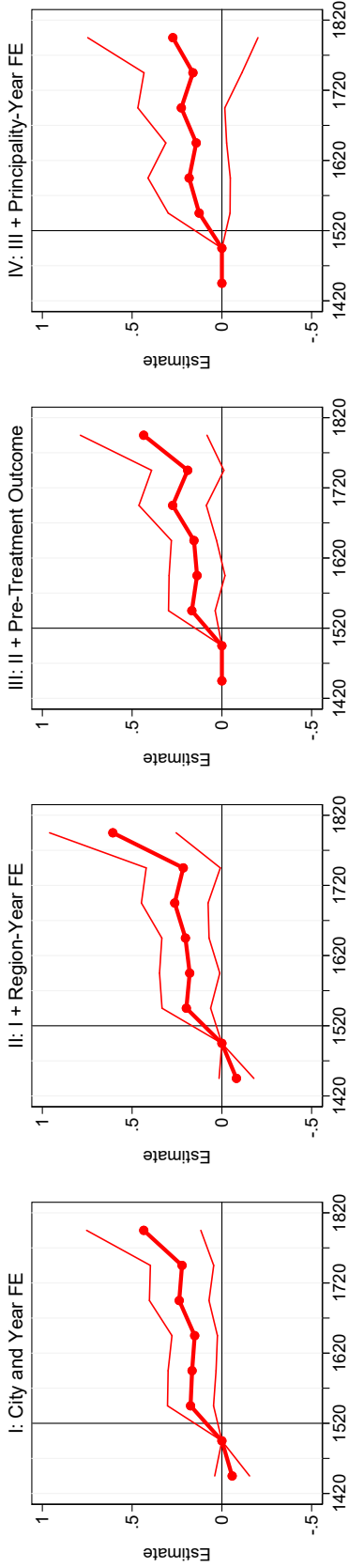
Our results in Panel A show that migration increased as and immediately after church ordinances were adopted. Our results in Panel B show that the local formation of human capital also increased following the adoption of ordinances, but that this response was more gradual and became statistically significant 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

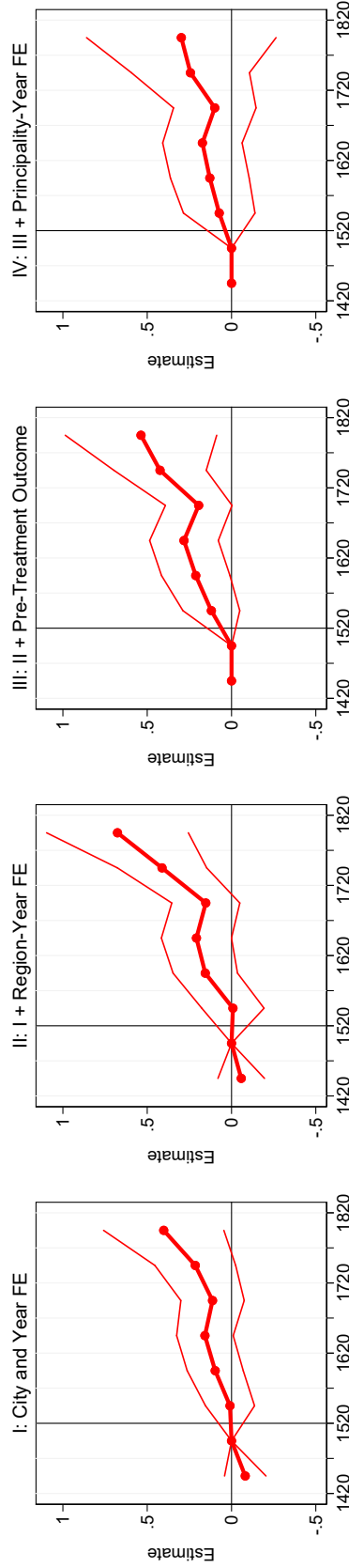
⁴⁵Our results are not explained by the selective inclusion of marginal individuals into the *Deutsche Biographie*. Our results hold for individuals for whom there is no ambiguity about inclusion, e.g. individuals with extended biographical essays in the *Deutsche Biographie* as discussed and shown below. 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

A. Migration of Human Capital



B. Formation of Human Capital



This figure presents regression estimates examining the relationship between institutional change and human capital outcomes. Outcomes are the logarithm of the number of 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; Deutsches Städtebuch). 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.

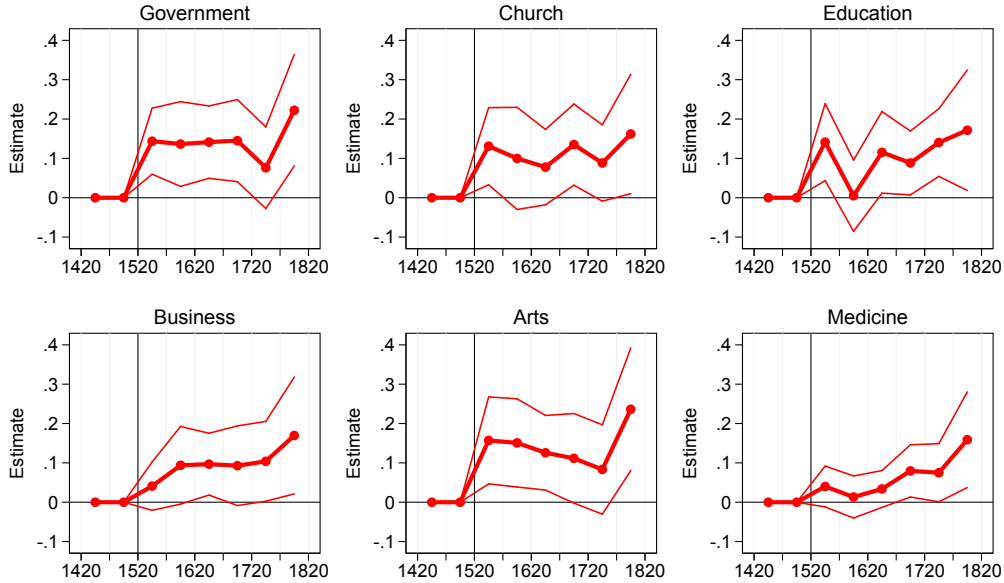
We examine the allocation of upper tail human capital across six occupational sectors: government (20%), church (15%), education (16%), business (18%), arts (26%), and medicine (5%). We measure the allocation of human capital by classifying the professions of all individuals in the *Deutsche Biographie* (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.

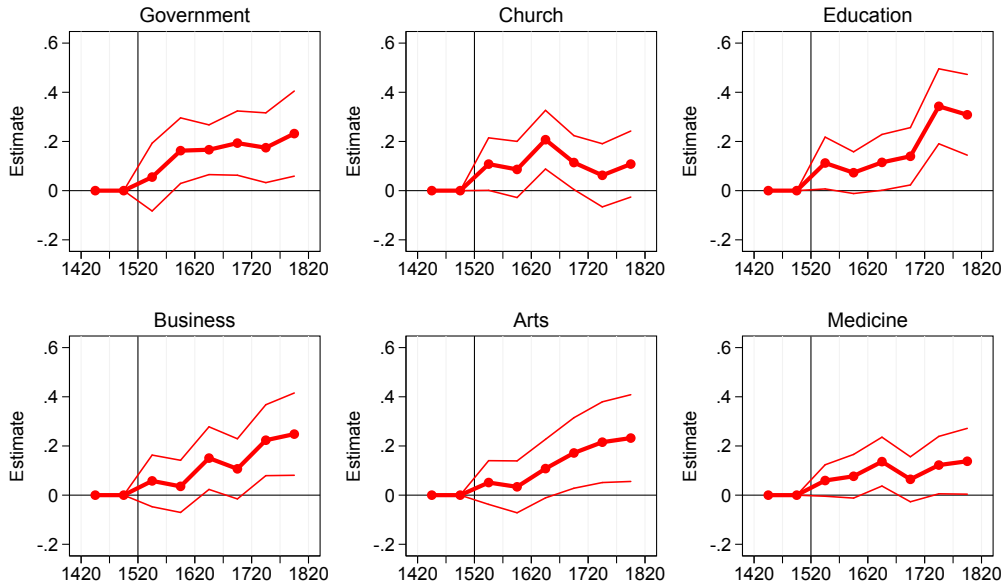
Panel B presents estimates studying the formation of upper tail human capital by sector. We find that the formation of human capital responded less immediately to the adoption of ordinances than migration did. The formation of local human capital in the government sector increased sharply one period 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

A: Migration of Human Capital



B: Formation of Human Capital



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.

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

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

⁴⁶We 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.

⁴⁷There 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.

⁴⁸A 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.

⁴⁹We 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 \times Law	0.07*** (0.02)		0.07*** (0.02)	0.01 (0.03)	0.12** (0.05)		0.13** (0.05)	-0.10 (0.09)
Post \times Protestant		0.03** (0.01)	0.02** (0.01)	-0.02* (0.01)		-0.01 (0.06)	-0.05 (0.06)	-0.06 (0.12)
Post \times Law \times Trend				0.03** (0.01)				0.12** (0.04)
Post \times Prot \times Trend				0.02** (0.01)				0.00 (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 Städtebuch*. 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 \times Law” interacts an indicator for institutional change and the post-1520 period. “Post \times 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]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
	Cities with Law			Cities without Law			Difference in Means	Share with Law	Share of Cities
	N	Mean	Sd	N	Mean	Sd			
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: $\ln(\text{population}/1000)$. 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:

$$Y_i = c + \alpha \cdot Law_i + \gamma \cdot X_i + \epsilon_i, \quad (4)$$

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

⁵⁰We 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.

⁵¹In 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]
<i>Panel A: City Population</i>							
	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
<i>Panel B: Human Capital</i>							
	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
<i>Panel C: Human Capital Intensity</i>							
	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
<i>Controls that Vary Across Specifications</i>							
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

⁵²Protestant 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

⁵³Almost 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).

⁵⁴Similar 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).

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

⁵⁶Because 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.

⁵⁷We 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

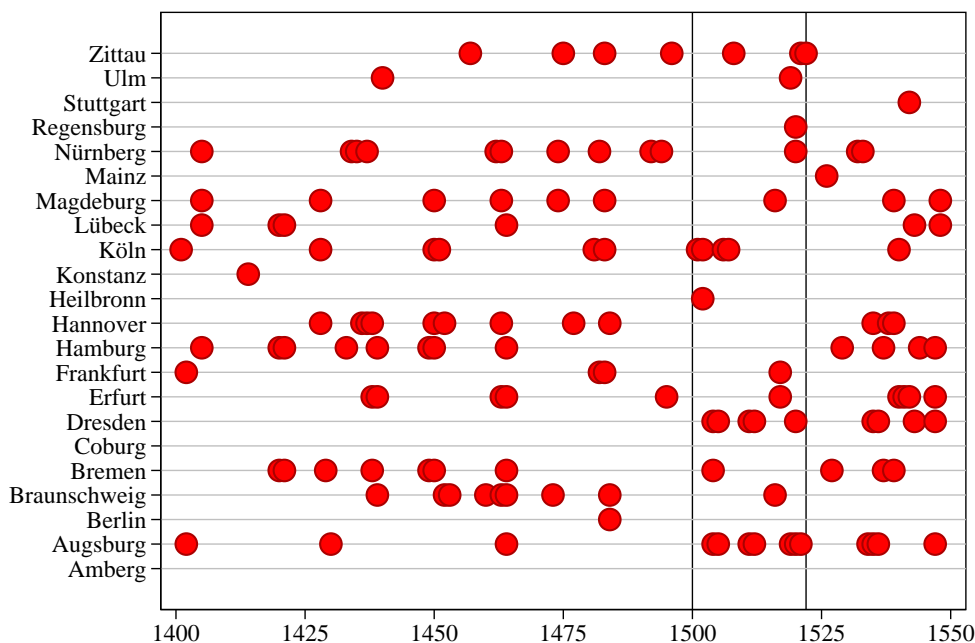
It is natural to wonder whether the plague shaped long-run outcomes directly, for example through its demographic impact or through its influence on other political institutions.

Here we preview two pieces of evidence that 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.

⁵⁸Our 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:

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

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

⁵⁹Our 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.

⁶⁰Note 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]
<i>Panel A: First Stage – Public Goods Institutions</i>						
	Outcome: Adoption of Law					
Plagues 1500-1522	0.14*** (0.02)	0.13*** (0.02)	0.13*** (0.03)	0.12*** (0.02)	0.12*** (0.04)	0.11*** (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
<i>Panel B: Instrumental Variable Outcomes – Population and Human Capital</i>						
	Outcome: Ln Population in 1800					
Law	1.61** (0.82)	1.76** (0.83)	1.94** (0.98)	2.17** (0.93)	2.13* (1.12)	2.30** (1.01)
	Outcome: Ln Upper Tail Human Capital 1750-1799					
Law	2.74** (1.24)	2.96** (1.29)	3.10** (1.38)	3.34** (1.40)	4.23** (1.81)	4.54*** (1.28)
	Outcome: Upper Tail Human Capital per 1,000					
Law	0.56** (0.27)	0.63** (0.25)	0.59* (0.31)	0.66** (0.29)	0.80** (0.40)	0.92*** (0.29)
<i>Controls</i>						
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

⁶¹This 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:

$$\ln(\text{population}_{i1800}) = \alpha + \sum_t \beta_t \text{plagues}_{it} + \gamma X_i + \epsilon_i \quad (6)$$

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

⁶²We 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.

⁶³We 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).

⁶⁴We 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 exogenous” (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) and the *Deutsches Städtebuch*. 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

⁶⁵In 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*** (0.05)	0.29*** (0.08)	0.29*** (0.08)	0.27*** (0.09)
Plagues 1375-1399	0.07 (0.10)	0.12 (0.12)	0.11 (0.12)	0.12 (0.15)
Plagues 1400-1324	-0.07 (0.19)	-0.08 (0.22)	-0.08 (0.22)	-0.06 (0.26)
Plagues 1425-1449	0.11 (0.09)	0.13 (0.08)	0.16* (0.08)	0.13 (0.09)
Plagues 1450-1474	-0.00 (0.11)	0.01 (0.09)	0.01 (0.09)	0.02 (0.09)
Plagues 1475-1499	0.20 (0.23)	0.13 (0.20)	0.13 (0.20)	0.10 (0.19)
Plagues 1500-1524	0.19** (0.07)	0.20** (0.07)	0.20** (0.08)	0.22** (0.09)
Plagues 1525-1549	0.03 (0.07)	0.03 (0.06)	0.02 (0.07)	0.02 (0.08)
Plagues 1550-1574	0.10 (0.07)	0.09 (0.05)	0.10 (0.06)	0.08 (0.08)
Plagues 1575-1599	0.00 (0.05)	-0.06 (0.06)	-0.06 (0.06)	-0.04 (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 “***”.

relationship between plagues 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

⁶⁶Between 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		Public		Guild	
	Elections		Representation		Constitution	
Plagues 1500-1522	-0.01 (0.02)	-0.01 (0.02)	0.03 (0.03)	0.02 (0.03)	-0.07 (0.05)	-0.08 (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) and the *Deutsches Stadtbuch*. 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

⁶⁷Over 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*.

⁶⁸When 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 \times Plagues 1500-1522	2.03***	2.04***	0.09***	0.09***
	(0.45)	(0.50)	(0.02)	(0.02)
City Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
Region \times 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 \times 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** (0.83)	2.17** (0.93)	1.33* (0.77)	1.96** (0.81)
F Statistic on IV	32.69	31.42	31.94	28.10
	First Stage Outcome: Law			
Plagues 1500-1522	0.13*** (0.02)	0.12*** (0.02)		
Plagues 1500-1550			0.19*** (0.03)	0.15*** (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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