

Origins of Persistent Differences in Human Capital Accumulation*

Jeremiah E. Dittmar Ralf R. Meisenzahl
London School of Economics Federal Reserve Board

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Abstract

We study the long-run effects of the first public mass education system. Protestant reformers implemented city- and county-level education laws in various parts of Prussia and the Holy Roman Empire. We find the cities with education laws by 1600 had twice as many schools and were larger in 1816. While fostering human capital accumulation and economic growth, education laws did not reinforce Protestantism. These results are robust to controlling for pre-Reformation city characteristics and even to using only the variation in laws that is not explained by the geographic nature of the diffusion process. Our results are consistent with endogenous growth theory that highlights the role of human capital accumulation for the transition over to modern economic growth.

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*E-mail: ralf.r.meisenzahl@frb.gov. Mailing address: 20th and C Streets NW, Washington, DC 20551. We would like to thank Sascha Becker and seminar participants at Northwestern University, 2015 SGE Meeting, and 2015 ARSEC conference for helpful comments. Russ Gusdia, Ana Houshmand, Luis Molestina-Vivar, David Rinnert, and David Schultz provided superb research assistance. The opinions expressed are those of the author and do not necessarily reflect the view of the Board of Governors of the Federal Reserve System.

1 Introduction

Whether human capital matters at early stages of economic development remains an open question. We study the consequences of the first experiments in public mass education in European history. During the Protestant Reformation of the 1500s, some cities in historical Germany passed laws establishing and organizing public schools. We provide evidence documenting that these legal reforms establishing public schools had long-run consequences stemming from higher human capital accumulation at the local level. Distinguishing between cities that did and did not pass education laws, we find that cities with education laws by 1600 had twice as many schools and were larger in 1816. We show that other dimensions of Protestant institutions do not explain our findings. In particular, we find that the laws establishing of educational institutions explain subsequent differences in schooling and city dynamism—as opposed to the passage of Reformation ordinances in general or the passage of, for example, Protestant laws governing public and private morality. While morality laws reinforced the protestant system of beliefs, education laws led to more economic growth without measurably reinforcing Protestantism. These findings suggest that public mass education facilitated the transition from the Malthusian epoch to modern economic growth.

Previous research has tied religion and Protestantism in particular to variations in human capital accumulation (e.g. Barro and McCleary (2005) and Becker and Woessmann (2009)). But the existing literature has not documented the underlying cause for this variation: the institutional nature of the relationship between religious change and educational outcomes. Neither has the existing literature documented or discriminated between the effects of the different concrete institutional innovations that formalized the Protestant Reformation. This research examines how the formal institutions supporting public mass education delivered persistent differences in human capital accumulation and economic outcomes. By examining the specific provisions of legislation, we are able to distinguish between the effects of legal institutions supporting public mass education and institutions designed to regulate and impact other dimensions of behavior, such as public and private morality.¹

The creation of the first European public mass education system by protestant reformers in sixteenth century in the Holy Roman Empire provides a unique opportunity to study the origins and consequences of differences in human capital accumulation within a country.² The Reformation itself was arguably exogenous and the Protestants' motivation for mass education was to create

¹We provide more detailed discussion of the laws of the Reformation below, including discussion of the fact that while education was clearly profoundly religious the evidence supports the view that educational institutions had social consequences that were distinct from other non-educational dimensions of the new legal framework.

²Prior to the Reformation, organized and institutionalized learning was reserved for the elites. One example is the Scottish Education Act 1496. This Act pre-dates our study but made grammar school attendance compulsory only for the sons of nobles and the rich.

new beliefs and new subjects.³ Despite these goals, the implementation of education laws was not uniform. Many cities, protestant and catholic alike, did not provide schooling during the sixteenth century. As Strauss (1978) puts it, “(f)rom the vantage point of the Prussian general territorial law of 1794, the school ordinances of the sixteenth century seem all patchwork and vacillation (p. 14).”

This patchwork of educational experiments of the Protestant reformation are among the most ambitious social policy programs of the last several hundred years. We exploit this variation in sixteenth century education experiments to show that differences in human capital accumulation persisted for over 200 years and can account for differences in economic development. To differentiate the effect of education laws from a potential effect of Protestantism, we follow Becker and Woessmann (2009) and use distance to Wittenberg as proxy for the variation in education laws that can be attributed to the diffusion of Protestantism. Using only the residual, random variation in education laws, we find that cities with education laws by 1600 had more schools in 1816. These persistent differences in human capital accumulation had economic consequences. Cities with education laws by 1600 were larger in 1816, before the industrial revolution began in historical Germany.

Protestant reformers established schools as one of the institutions that would formalize and reinforce the new system of beliefs. Yet, the Protestants created human capital that was not, so to speak, enterprise specific. The chief objective of the policy intervention was indoctrination and behavior modification. However, these educational interventions had, as we show, unintended consequences on economic dynamism at the local level that were not originally envisioned.

Arguing that human capital is crucial for early economic development is not new.⁴ So far, the existing literature has focused on the importance of elite education. For instance, Hornung (2014) finds that the resettlement of highly skilled refugees was associated with subsequent productivity in early manufacturing in Germany. Squicciarini and Voigtländer (forthcoming) find that scientific knowledge was associated with differences in early industrialization across French cities. Meisenzahl and Mokyr (2012) provide evidence that only the very top of the skill distribution mattered for innovation in the British industrial revolution while Mitch (1998) argues that average education attainment did not affect economic growth in that period. Our contribution is to show that persistent differences in basic mass education mattered for the transition to modern growth.

But why do these persistent differences in basic education matter for economic development?

³The aim of Protestant education was not only or even principally to produce believers able to read the bible. As discussed below, the center-piece of early childhood education became the study and recitation of Protestant catechism. See Strauss (1978).

⁴Human capital has also been stressed as the most important factors for sustained economic growth. For a survey, see (Galor, 2005)

One mechanism rationalizing our findings is that literacy improves the diffusion of information and enables commercial activity. In fact, in the later stages of the reformation, reformers appeared to recognize some benefits and also argued that educated citizens are essential to the success of the community and, perhaps as justification for why public funds should be used to support schooling, pointed to benefits of education for commerce.⁵ More generally, Dittmar (2011) finds that cities with printing presses grew more rapidly. A second, related mechanism is that literacy facilitates skill acquisition.⁶ Prussian education reformers implemented a series of reforms in the early nineteenth century aimed to improve schools, to establish trade schools, and to foster future skill acquisition.⁷ For instance, as of 1814, Kurhessen’s law, paragraph 313 required apprentices to present a transcript of a continuation school to complete their apprenticeship (Klauhold, 1855).

Our research documents changes in human capital accumulation and in economic outcomes over the transition to modern growth and before the diffusion of the industrial revolution in Germany. Our findings both support and qualify existing research on education and development in Europe. Becker and Woessmann (2009) argue that Protestantism delivered variations in literacy that had profound implications for economic development once the world technology frontier began to advance with the advent of the industrial revolution circa 1800. The evidence we examine suggests variations in local institutions supporting human capital accumulation were associated with variations in urban dynamism over the pre-industrial period.⁸

To be clear, changes in human capital accumulation did not only affect the transition to modern growth but also contributed to differences in industrialization. Becker et al. (2011) find that historically more literate Prussian regions experienced a faster catch-up during the industrial revolution. Formal education became even more relevant during the second industrial revolution, which was driven by advances in science. A workforce that can read machine instructions and can understand the basic science of the production process is not only more faster in adoption of new technologies but can also improve them, increasing overall productivity. Human capital then determines the rate of growth (Romer, 1990). Galor and Moav (2006) argue that for this reason industrialists supported the implementation of public mass education. All told, literacy—one form

⁵Luther and Melanchthon made this point in their later writings. See Witte Jr. (2002).

⁶The perhaps best example, though not German, of how literacy may aide skill acquisition and foster economic growth is the British locomotive engineer and inventor George Stephenson. Stephenson, who was illiterate at the age of 18, paid a tutor to teach him to read (and write), so that he could finally learn about the Watt engine he heard so much about (Smiles, 1879).

⁷Meisenzahl (2015) surveys myriad of educational institutions that emerged in Germany during the nineteenth century and argues that these institutions taught the skills necessary to be successful during the second industrial revolution.

⁸Our findings provide an interesting contrast with Cantoni (forthcoming), which finds Protestantism was not associated with higher growth in a panel of German cities. The explanation is as follows: The variation explore is variation in formal educational institutions as opposed to Protestantism per se. In addition, Cantoni’s time period includes the shocks of the Thirty Years War which imparted a differential negative shock to Protestant cities and dramatically raised the variance of city growth.

of human capital—is a pre-condition for economic advancement.

A large body of research studies the relationship between schooling and growth mostly in cross-county settings. Barro (1991), Benhabib and Spiegel (1994), and Sala-i Martin (1997) document a positive relationship between schooling and the growth rate of GDP per capita. Glaser et al. (1995) provide U.S. city-level evidence that more schooling is associated with higher income and population growth in the post-WWII period. Barro et al. (2013) summarize more recent research on human capital and development. However, most of these studies take initial differences in human capital as given, and cross-country comparisons are facing a serious identification challenge when trying to disentangle the effect of schooling from any other causes of economic growth such as political institutions.⁹ A related literature studies cross-cohort effects of educational interventions. Duflo (2001) studies cross-cohort impact of a school building program in contemporary Indonesia. Goldin and Katz (2011) study the effects of compulsory schooling laws introduced in the early 20th century in the US. To our knowledge, this paper is the first to exploit differences in education laws on the subnational level—that is, holding constant the political institutions—to study the origins and consequences of differences in human capital accumulation.

The next section describes our data on city-level protestant reformation laws. Section 3 lays out the empirical methodology and presents the results of our hypothesis tests. Section 4 concludes.

2 Data and Summary Statistics

Our main data source on city- and county-level protestant church ordinances are multiple volumes edited by Sehling (1902). These volumes reprint all protestant church ordinances in historical Germany and Prussia in their original. In total, the volumes contain XXX protestant church ordinances for XXX towns and XXX regions. We focus on protestant church ordinances, as to the best of our knowledge, there were no Catholic Church ordinances that had any provisions regarding schooling.

First, we collect which city had a church ordinance by 1600. Figure 1 shows the 5-year moving average of ordinances passed and the number of cities passing their first ordinance in a given year. Most cities passed their first ordinance by 1545, and the total number of ordinances dropped sharply after 1546. The timing is not coincidental. In 1546, the Schmalkaldic War (1546/47) between Protestant Allies (Schmalkaldic League) and Holy Roman Emperor Charles V broke out. The Augsburg Settlement (1555) ended hostilities between the protestant and catholic regions and

⁹Acemoglu et al. (2001) show that differences in a broad set of political institutions can account for differences in economic development. Acemoglu et al. (2005) survey this literature. However, Glaser et al. (2004) suggest that human capital is a more basic source of growth than institutional quality.

granted the Protestants some rights. Most notably, the settlement included a provision, *cuius regio, eius religio*, which allowed the local ruler to dictate the religion in their realm. Subsequently, ordinances were passed again throughout the rest of the sixteenth century.

These ordinances institutionalized the Protestant Reformation and led to a revolutionary change in legal institutions transferring control in many legal matters from the catholic church to the earthly rulers.¹⁰ All protestant reformation ordinances are reprinted in Sehling (1902), which is our key source on which cities got what type of legal institutions during the Reformation. The overarching theme of all ordinances is that the protestant reformers envisioned a radically different society. Hence, ordinances touched many parts of public life in order to educate and form this new society. Ordinances usually contained a subset of the following: instruction on the mass, church governance laws, education laws, poor laws, and morality laws. The reformers often included oversight provisions in the ordinances to ensure their efficacy.

We are focusing on the long-run effects of these legal changes. In particular, we are interested in which type of ordinances had lasting effects on economic growth but also on the reinforcement of Protestantism itself, the stated goal of reformers. To this end, we classify ordinances by category. For instance, if an ordinance contained information about curricula, school funding, school examination, or teachers' wages then we code this city as having a education law.

The following quote exemplifies how ordinances codified education in general terms: "*The schoolmaster and his assistants should diligently instruct the boys in catechisms, grammar, music and assiduously practise declension, conjugation, and constructions. Moreover, he should diligently instruct the children to read slowly and clearly, with good pronunciation, to speak Latin, and to write in a good, legible German script.*"¹¹ In some cases, as the next quote illustrates, the reformers were very specific about the curriculum: "*From seven to eight the first class should read with the schoolmaster Terence or a comedy by Plautus, which are pure and innocent, but should read Terence more often than the comedies by Plautus.*"¹² Moreover, the ordinances also implemented oversight. The ordinance of Doebeln prescribes quality control as follows: "*To ensure diligent teaching and learning in Latin and German schools, there shall be public examinations each quarter, but at least twice a year, with the local priest, mayor, and city council members in attendance.*"¹³ We argue that cities with these ordinances in the sixteenth century accumulated differentially more human capital.

We also code the presence of church governance laws, poor laws, and morality laws. Church governance laws codified how to hold mass and regulated wages of priests and their assistants.

¹⁰For a detailed discussion on how the Reformation affect law, see Witte Jr. (2002).

¹¹Church Ordinance of Pirna, cited in Sehling (1902), Vol. 1, p. 315.

¹²Church Ordinance of Leisnig, cited in Sehling (1902), Vol. 1, p. 606.

¹³Church Ordinance of Doebeln, cited in Sehling (1902), Vol. 1, p. 548.

Poor laws prescribed institutions that replaced catholic charities, such as orphanages. Morality laws include sumptuary laws (immodest apparel, wasteful living, extravagant feasts/funerals), Sabbath-day laws (minimal work, church attendance), and entertainment laws (public drunkenness, boisterous celebration, gambling and other games involving fate, luck, or magic). We will later use these to differentiate between the effects of education laws and other church laws, proxying for the diffusion of Protestantism in general, on human capital accumulation.

Table 1 shows the summery statistics of ordinances for cities on later Prussian territory. We will refer to these cities as Prussian cities thereafter. Prussian cities implemented, on average, three ordinances. Of these ordinances about 40 percent contained education laws. About 20 percent of ordinances included explicit provisions on teacher pay and about one quarter of ordinances provided guidelines for curricula. The reformers used the ordinances not only for education but also to regulate church governance (40 percent of ordinances), poor laws (35 percent of ordinances) and their oversight (25 percent of ordinances), as well as what was considered moral behavior (25 percent of ordinances).

Figure 2 shows the county-level share of Protestants in 1816 and Prussian cities with and without sixteenth century education laws. The figure shows that cities with education laws are generally in regions with a large share of Protestants in 1816. However, not all cities in deeply protestant counties had school ordinances, suggesting that the adoption of Protestantism alone did not necessarily lead to education laws.

We match the Prussian cities to the cities mentioned in the *Deutsches Städtebuch*, a series of volumes edited by Keyser (1939) summarizing characteristics, such as market rights and incorporation, of cities in historical Germany.¹⁴ For those cities, we add city-level information on market rights, town incorporation, and population as of 1500. We amended this data with city-level information on printing from XXX. In particular, we add whether books were printed in a city at the eve of the reformation and if so, how many books were printed.

For the city-level evidence, we restrict the sample to Prussian cities that are mentioned in the Mützell (1825). This source provides information about the number of schools in 1816 on the city level. This restriction reduces our sample to 172 cities. Of these 172 cities, 142 appear in the *Städtebuch*, 37 with education laws and 94 without. Table 2 shows the summary statistics of city-level data for Prussian cities with and without education laws. About 12 percent of cities without an education law had some church ordinance, for instance, a morality or church governance law, by 1600. Cities with education laws were less likely to have market rights at the eve of the Reformation, suggesting that cities with education laws were less likely to be (regional) commercial

¹⁴For a detailed description of the *Städtebuch* and summary statistics of all cities, see Cantoni and Yuchtman (2014).

centers. However, cities with laws were slightly more likely to be incorporated by 1517. Cities with education laws were also more likely to have printing presses by 1517 but more books were on average printed in towns without education laws. Perhaps surprisingly, for the subsample of cities with known population in 1500, there appears no difference in the population between cities with and without education laws. Ordinances written by Protestant reformers aimed to formalize and reinforce the new system of beliefs. Maybe indicating some success of the reformers, cities with education laws remained highly protestant with, on average, more than 90 percent being Protestants by 1816. Cities without education law had a considerably smaller protestant majority (about 60 percent). The key outcome variable of interest is number of schools in 1816. Cities with education law had more than twice as many schools as cities without education laws.

We code the geographic information of the cities described above and match them to the respective Prussian county in 1816. We then amend our dataset with county-level information on school enrollment, the fraction of children that attend school divided by all school-aged children in a county, and the share of Protestants in a county from the Prussian Census.¹⁵

3 Empirical Methodology and Results

We now describe how we use education law data to estimate the long-run persistence of difference in human capital accumulation. We first provide evidence from a city-level analysis on the number of schools in 1816. We then complement this analysis by assessing the effect of school law on city size measured as population in 1816 and on the share of Protestants in 1816. To address potential concern about education laws being a proxy of the Reformation in itself, we separate the effect of education laws from the overall diffusion of the Reformation. Finally, we study the factors associated with pre-Reformation towns becoming cities in the early 1800s. We document that the passage of education laws in the 1500s was a key determinant of the transition to later city status, and that the forms of selection this suggests support the view that educational laws contributed to local economic dynamism.

3.1 City-Level Evidence on Schools

The first piece of evidence on the long-run effect of sixteenth century education laws is the comparison of the distribution of the number of school in 1816 in cities with and without education law. Figure 3 plots the estimated distribution of the number of schools for these two groups. The distribution of the number of schools of cities with education laws is to the right of the distribution

¹⁵For a detailed description of the Prussian Census data, see Becker et al. (2014).

of the number of schools of cities without education laws. Therefore, this graphical evidence already suggests that large differences in the number of schools may be the consequence of sixteenth century education laws. Of course, a number of factors are potentially correlated with whether a city had an education law. For instance, city size and economic conditions could account for the persistent differences.

To assess the long-run persistence of differences in human capital accumulation on the city-level more formally, we estimate the following regression:

$$\text{Number of Schools}_{i,1816} = c + \alpha \cdot \text{Education Law}_{i,pre-1600} + \beta \cdot X_i + \epsilon_i, \quad (1)$$

where $\text{Education Law}_{i,pre-1600} = 1$ if city i had education provision in ordinances by 1600 as described in section 2 and 0 otherwise. We add controls for city characteristics X_i . We measure the city-level control variables that proxy for commercial activity (market rights), city size (incorporation), and media access (printing, number of educational book printed) in pre-1517—that is, before the reformation—to ensure that the control variables are not affected by the reformation itself. We also control for whether the city had a morality law to assess whether protestant reforms in general had a long-run effect on education. To better control for unobserved characteristics, we include territory fixed effects in some specifications.

Table 3 shows the results of estimating equation 1 using ordinary least squares (OLS). Column 1 shows the baseline estimate without any city-level control variables. The estimated effect of a city having an education law by 1600 is large and highly significant. A city with an education law by 1600 had, on average, 15 more schools in 1816. Of course other city characteristics, such as commercial activity or city size, could be correlated with the adoption of education laws during the Reformation. In column 2, we add indicator variables for whether the town had market rights before the Reformation to proxy for commercial activity (Cantoni and Yuchtman, 2014) and town incorporation before the Reformation as a proxy for city size. Including these controls leaves the point estimate on having an education law unchanged. Moreover, the control variables are comparatively small and statistically not significant. Dittmar and Seabold (2015) show that media markets mattered for the adoption of Protestantism. Therefore, we also control for media market size at the eve of the Reformation, measured by an indicator of whether books were printed in a city (column 3). Controlling for media markets attenuates the coefficient on education a little but does not change our main result. Becker and Woessmann (2009) argue that Protestantism itself led to higher human capital accumulation, and table 2 shows that towns with laws tended to be more protestant. We therefore control for the share of protestant population in 1816 and having a protestant morality law by 1600 in column 4 and find that the point estimate for education laws

remains unchanged.¹⁶ Pre-Reformation human capital accumulation may also be correlated with the adoption of education law. We include the number of education books as an additional control for pre-Reformation human capital accumulation. While pre-Reformation printing in itself does not seem to affect the number of school in 1816, the number of educational books does, suggesting that cities with more access to education before the Reformation remained more educated relative to their peers with little or no educational book production (column 5). The adoption of an education law could also be correlated with unobserved territory characteristics such as the legal system. We therefore control for territory fixed effects, and find a somewhat smaller but still significant effect of education laws on the number of schools in 1816 (column 6).¹⁷

One shortcoming of the data used above is that we only proxy for city size using town incorporation. To better control for city size, we use population in 1500 data. For 42 of our 172 Prussian cities (and 35 of the 142 Prussian cities that appear in the *Städtebuch*), the population in 1500 is known. Restricting ourselves to the sample of cities for which the population in 1500 is known biases the sample to larger cities. Moreover, as table 2 shows, population data are available for almost half of cities with education law but for only for about 20 percent of the cities without education law, considerably reducing our control group.

Table 4 shows the results of estimating equation 1 using OLS for the sample of cities with population in 1500 data. For comparison, column 1 shows the result without additional control for the sample of cities with known population in 1500. This sample is likely to include only larger cities. As one would expect when only considering larger cities, the point estimate is somewhat larger than in the full sample and remains statistically significant, suggesting that cities with an education law by 1600 had 24 additional schools in 1816. In column 2, we find that controlling for population in 1500 does not change the point estimate or its significance. This result is consistent with the fact that there are no differences in population in 1500 between cities with and without education law (table 2). Column 3 shows that adding controls for commercial activity (market rights) and city size (incorporation) does not change the estimate effect much. Similarly, controlling for printing, pre-Reformation access to education, share of Protestants in 1816 and having a morality law (column 4) increase our point estimate on education laws considerably. As before, we control for unobservable territory characteristics with territory fixed effect in column 5 and find that our point estimate on education laws remains unchanged. One potential explanation for the large point estimate is a disproportionately large outlier, in particular the city of Berlin. In column 6, we report the results when excluding Berlin from the sample. The point estimate on education laws remains large and

¹⁶The Protestant share of population in 1816 is of course endogenous to the laws of the 1500s. We include this measure of religion simply to document how variation in education in the 1800s relates to policy interventions in the 1500s conditional on later belief.

¹⁷In unreported results, we estimated equation 1 using only cities with a protestant share in 1816 of at least 50 percent and found similar results.

highly statistically significant, suggesting that our results are not driven by the largest Prussian city.

One potential issue with the sample of cities with population in 1500 data is that the dependent variable, number of schools, exhibits over-dispersion—that is, the conditional variance is higher than the conditional mean in the treatment groups. Here, negative binomial regressions may be more appropriate because over-dispersion is modeled explicitly. Table 5 shows the results of estimating equation 1 using negative binomial regressions for the sample of cities with population in 1500 data. As before, in column 1, we do not include any control variables and find that the estimated effect of having an education law on the number of schools in 1816 is large and statistically significant, suggesting that cities with an education law by 1600 had almost twice as many schools in 1816. Adding the city-level controls, column 2 through 4, does not change the results. Similarly, our point estimate for the effect of education laws on the number of schools remains large and highly significant when controlling for territory fixed effect (column 5) and excluding Berlin from the sample (column 6). In sum, the large and statistically highly significant point estimate on education laws suggests a long-run effect of education law on human capital accumulation. Cities with education laws by 1600 had, on average, twice as many schools in 1816.

3.2 City Size

Endogenous growth theory argues that increased human capital accumulation lead to economic growth and the demographic transition (Romer, 1990; Galor and Weil, 2000; Galor and Moav, 2004). One other potential consequence of school laws could be increased human capital accumulation. Literacy, while not being enterprise-specific, enables further skill acquisition and facilitates commercial activity. For a more recent time period, Glaser et al. (1995) document that U.S. cities with more schooling in 1960 experienced a faster income and population growth rate over the next 30 years. Hence, in the spirit of the endogenous growth theory, we hypothesize that cities with schools laws by 1600 experienced a more rapid population growth and were larger by 1816.

We test this hypothesis by estimating the following equation

$$\ln Population_{i,1816} = c + \alpha \cdot Education Law_{i,pre-1600} + \beta \cdot X_i + \epsilon_i. \quad (2)$$

As before, we add a number of variables to control for economic conditions: commercial activity (market rights), city size (incorporation), and media access (printing, number of educational book printed) in 1500.

Table 6 shows the results of estimating equation 2. Column 1 shows the baseline estimate without any city-level control variables. The estimated effect of a city having an education law by 1600 is large and highly significant. The point estimate suggests that cities with education laws by 1600 were, on average, X percent larger in 1816. Of course, population growth can be influenced by many other factors including the pre-existing economic conditions. Column 2 shows the regression result when controlling for pre-reformation economic conditions and column 3 also includes territory fixed effects. In both cases, the point estimate and its statistical significance remain unchanged.

One short-coming of these tests is that, without controlling for city size in 1500, the correlation of education law and population in 1816 could just reflect that larger cities were more likely to adopt education laws. With all cities growing at the same pace, the coefficient on education law would then be positive and significant but would proxy for city size in 1500 rather than for the impact of human capital accumulation. As shown in table 2, in the subset for which population is known in 1500, there are no differences in population for cities with and without laws. We estimate equation 2 on this subset and find that the point estimate is even larger and remains highly significant (table 6, column 4), suggesting that human capital accumulation already contributed to economic growth before the industrial revolution started in Prussia.

Another concern with interpreting the coefficient on education laws as a proxy for human capital accumulation is that it could also more generally better reflect legal institutions, which could also explain differential increases in population (Cantoni and Yuchtman, 2014). To assess whether laws in general proxy for better legal institutions which, in turn, lead to larger city size, column 5 shows the results for estimating equation 2 with morality laws instead of education laws. The estimated effect on morality laws is small and statistically not significant. This result suggests that having laws in itself did not improve economic conditions. Column 6 shows the estimated effect of education laws controlling for morality laws. The point estimate is the same as in columns 1-3. Overall, the findings presented in the section then indicate that, consistent with endogenous growth theory, education laws led to favorable economic outcomes through human capital accumulation. We conclude that education laws were the key mechanism driving the observation by Menschenfreund (1772) that protestant countries tended to be richer.

3.3 Reinforcement of Protestantism

The two previous sections show that education laws in the sixteenth century had lasting effects. They are associated with more schools and larger populations in 1816. But the key aim of education was to transform beliefs and behavior and thereby to construct new Protestant subjects. Strauss (1978) argues that this is distinct from producing people who could read the bible and that bible-

reading was not actually a primary goal of the interventions—that is, the key outcome of education laws that protestant reformers envisioned was a reinforcement of Protestantism. This feedback effect between education and Protestantism may affect the interpretation of the results shown above. Since we control for the share of Protestants in 1816, which could be the result of school laws, we may underestimate the effect of education laws.

To assess potential feedback effects between education laws and Protestantism, we estimate the following equation

$$\text{Share of Protestants}_{i,1816} = c + \alpha \cdot \text{Education Law}_{i,pre-1600} + \beta \cdot X_i + \epsilon_i. \quad (3)$$

Table 6 shows the results of estimating equation 3. Column 1 shows a positive and significant correlation between education laws and the share of Protestants in 1816. Column 2 shows the regression result when controlling for pre-reformation economic conditions. The point estimate remains unchanged and significant. However, the adoption of Protestantism in the sixteenth century was based on the local ruler’s denomination. Controlling for territory fixed effects, the point estimate drops by one half and becomes insignificant (column 3). Adding population in 1500 yields a similar result (column 4).

The results suggest that education laws had little if any effect on the share of Protestants in 1816. One potential issue with this interpretation is that historically whole territories converted when the ruler adopted Protestantism. Teasing out differential effects may therefore be challenging. To assess whether reformation laws reinforced Protestantism at all, we estimate equation 3 with morality laws instead of education laws as the main explanatory variable. Column 5 shows the results. Controlling for territory fixed effects, morality laws passed in the sixteenth century increased the share of Protestants in 1816 by 22 percentage points, suggesting reformist laws, in general, reinforced Protestantism. Column 6 includes both education and morality laws. The effect of morality laws remains large and significant. Hence, different types of law that protestant reformers past in the sixteenth century affected different outcome variables. While morality laws reinforced the protestant system of beliefs, education laws led to more economic growth without measurably reinforcing Protestantism.

3.4 Separating Human Capital and Diffusion of the Reformation

Up to this point, we have treated reformation laws in a vacuum. The Reformation itself can be seen as an exogenous shock, but interpreting the correlation between having an education law and the number of schools and other outcome variables as causal may raise a number of concerns. For

instance, Weber (1904) argues that the Reformation itself changed the work ethic, thus fostering economic growth. The Reformation is arguably at least as persistent as the education laws, and Nunn and Wantchekon (2011) show that persistent norms also affect economic outcomes. Hence, these laws may proxy for the persistence of the Reformation and its norms rather than human capital accumulation.

The key argument of our approach to disentangling the human capital channel from the diffusion of the Reformation is that the Reformation spread in concentric circles around Wittenberg (Becker and Woessmann, 2009).¹⁸ At the same time, many authors of ordinances either studied or were closely associated with the University of Wittenberg. Martin Luther himself authored a number of ordinances in cities close to Wittenberg. Hence, we estimate whether a city had an education law as a function of the city’s distance to Wittenberg. This distance measure proxies for the diffusion of the Reformation. We then use the residual variation in education law—that is, the part of the variation that cannot be explained by the diffusion of the Reformation—to estimate the causal effect of education laws on outcomes. We estimate the following equation in the first stage:

$$Education\ Law_{i,pre-1600} = c + \alpha \cdot Distance\ to\ Wittenberg_i + \beta \cdot X_i + \epsilon_i, \quad (4)$$

where $Education\ Law_{i,pre-1600} = 1$ if city i had education provision in ordinances by 1600 as described in section 2, and 0 otherwise. As before, we control for city characteristics at the eve of the Reformation X_i . The controls are market rights, town incorporation, printing, educational books and 6 population bins (including one bin for cities without 1500 population data).

Table 8 shows the results of the 2-stage regression. Column 1 shows the results of the first stage, estimating equation 2. As expected, distance to Wittenberg is negatively associated with a city having an education law. The estimated effect is large and statistically significant. Each 100 km distance to Wittenberg reduces the propensity of having an education law by 1600 by X percentage points.

The second stage only uses the variation in education laws that is not attributed to the diffusion of the Reformation. Column 2 shows that our results on number of schools in 1816 are highly robust to this specification. The estimated effect of education law on the number of schools in 1816 is statistically significant at the 1 percent level. These regression results suggest that education laws by 1600 had long-run effects, increasing the number of schools in cities with education laws by 9. This, though unintended by protestant reformers, advantage in human capital accumulation

¹⁸Dittmar and Seabold (2015) examine how variations in media market competition shaped the diffusion of the Reformation. Our setting does not provide enough cities with printing to study exogenous variation in media markets. Our data from historic Prussia include only 11 cities with printing as of 1517, where Dittmar and Seabold study all cities across German-speaking Europe.

led to more economic growth. Column 3 shows that our results for city size are also robust to the IV-specification. However, we cannot find a significant effect of education laws on the share of Protestants (column 4), suggesting that, while having positive effects on human capital accumulation and economic growth, this part of the reform agenda missed its target: reinforcing Protestantism.

Next, we provide additional evidence for the validity of our approach using morality laws. As for the education laws, we estimate equation 4 for morality laws and retrieve the residual of the first stage regression. We then estimate the effect of morality law on outcomes, using only the variation that is not explained as our proxy for the diffusion of the Reformation, distance to Wittenberg. Table 9 shows that results for the regressions with morality laws. As for education law, distance to Wittenberg is a strong predictor for a city having a morality law by 1600 (column 1). However, in the second stage, we again find no effect of morality laws on the number of schools in 1816 (column 2) or population in 1816 (column 3). We do find a marginally significant effect of morality laws on the share of Protestants, suggesting a persistence in and reinforcement of norms.

In sum, the findings in this section are consistent with the view that human capital accumulation accounted for differences in economic outcomes. Perhaps surprisingly, we find these effects even before the industrial revolution in Prussia started. Accounting for the diffusion of Protestantism, we find that cities with education laws by 1600 had more schools in 1816 and were larger in 1816. These results cannot be explained by more general Protestant reforms, as morality laws had no effect on economic outcomes.

3.5 Legal Reform as Determinant of City Status

The fact that our analysis examines the set of Prussian cities as they existed in 1816 raises a question: is the set of Prussian cities in some sense a selected sample? If so, how this impact the estimated relationship between historic legal reform and subsequent outcomes?

To study these questions we examine the complete set of towns and villages that existed in 1517 as candidates to become Prussian cities. The complete set of towns and villages is obtained from the *Deutsche Städtebuch* (Keyser, 1939). We consider all 887 towns and villages that fall within Prussian territory.

By studying the complete set of candidate locations we are able to answer questions about how selection may shape estimated treatment effects. The sort of concerns we and readers have in mind may be illustrated as follows. Suppose in 1500 there are some relatively big but ossifying cities. They will resist Protestantism and remain big enough to be cities in 1800, but will not appear to be dynamic. In 1500 there are small dynamic cities. These places are already bound for economic

glory. They are generally open to new ideas and hence will adopt Protestant legal innovations. We will conclude the laws cause growth. But really the estimated effect is down to selection or reverse causation.

We study selection into the set of Prussian cities. In section 3.4, we document that there are no *ex ante* observable differences (as of 1517) beyond distance to Wittenberg between locations that get laws and locations that do not. We now study what determines whether a city is observed in the sample used above.

Table 10 shows the regression results of estimating the propensity of observing a city mentioned in Keyser (1939) is also included in the sample of Prussian cities in Mützell (1825). We find that cities that had any ordinance are more likely to be observed in Mützell (1825). This finding suggests that Protestant cities are more likely to be selected in sample of the Prussian cities we use above. This points to a potential selection bias. However, when considering specific types of ordinances, we find that education laws predict future city status but that morality laws do not. This finding is robust to including controls, 1600 territory fixed effects and 1849 region fixed effects.

This selection implicit here may actually make our earlier estimates conservative. The control group we study to consider the impact of legal innovation is truncated and does not include the most stagnant and worst performing untreated locations.

4 Conclusion

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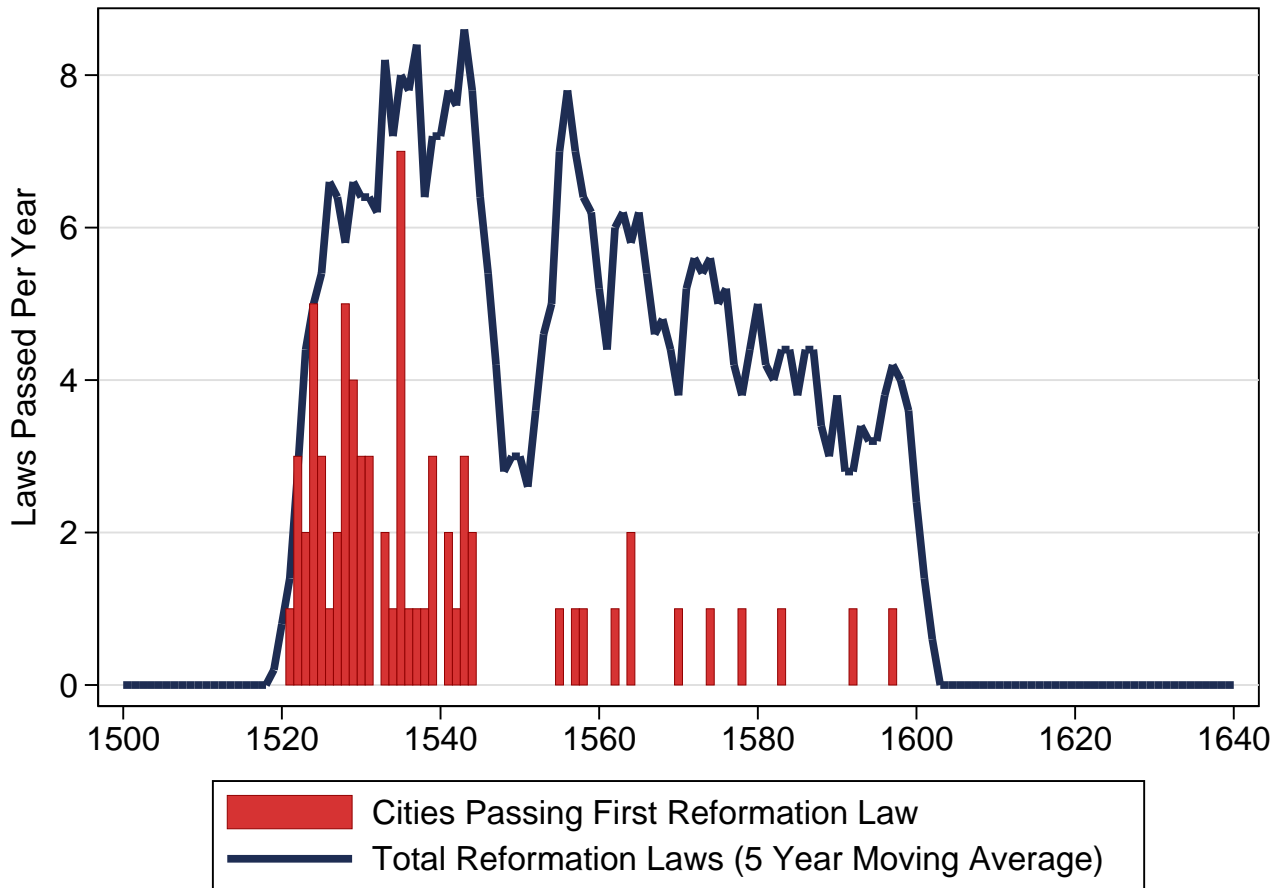


Figure 1: **Number of Reformation Laws:** This graph shows the number of cities²⁵ passing their first reformation law per year and the 5-year moving average of the total number of reformation laws passed between 1500 and 1640. The total number of reformation laws includes laws passed subsequent to the first law in a city.

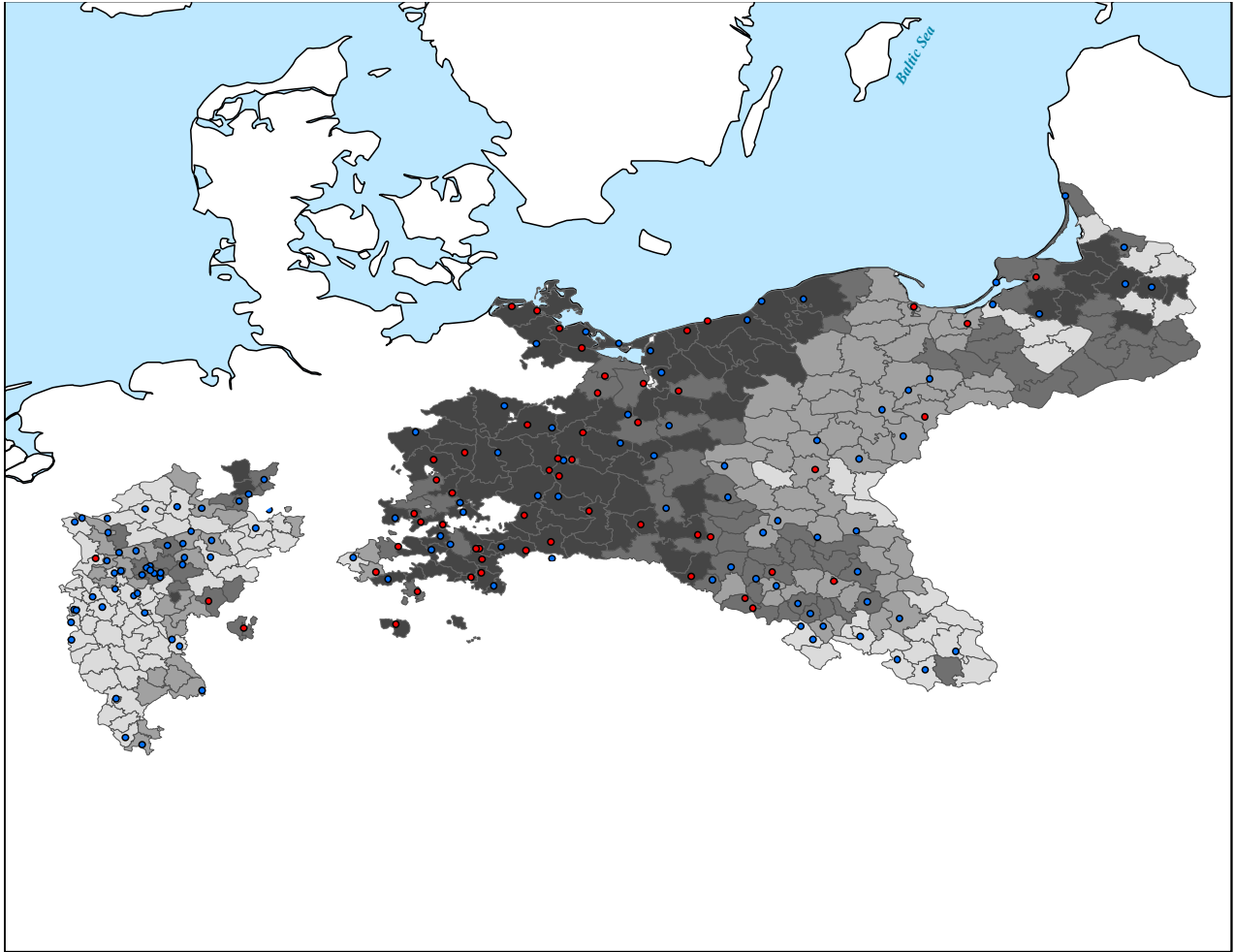


Figure 2: **School Laws (by 1600) and Protestant Share (1816)**: This graph shows cities with and without school laws (Red=City has a law. Blue=City does not have law.) and the county-level share of protestants. Darker shades indicate higher protestant share.

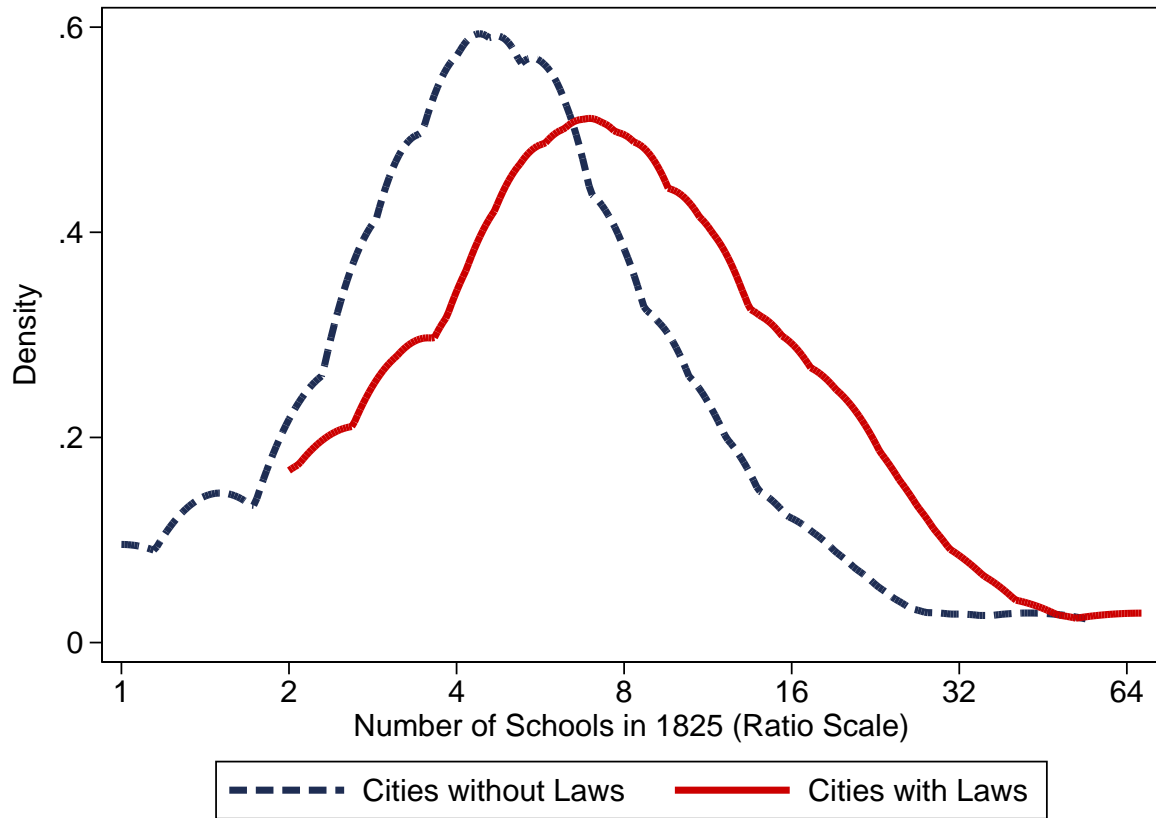


Figure 3: **Number of Schools in Cities with and without Ordinances during the Reformation:** This graph shows the distribution of the number of schools in 1816 for cities with and without school laws by 1600.

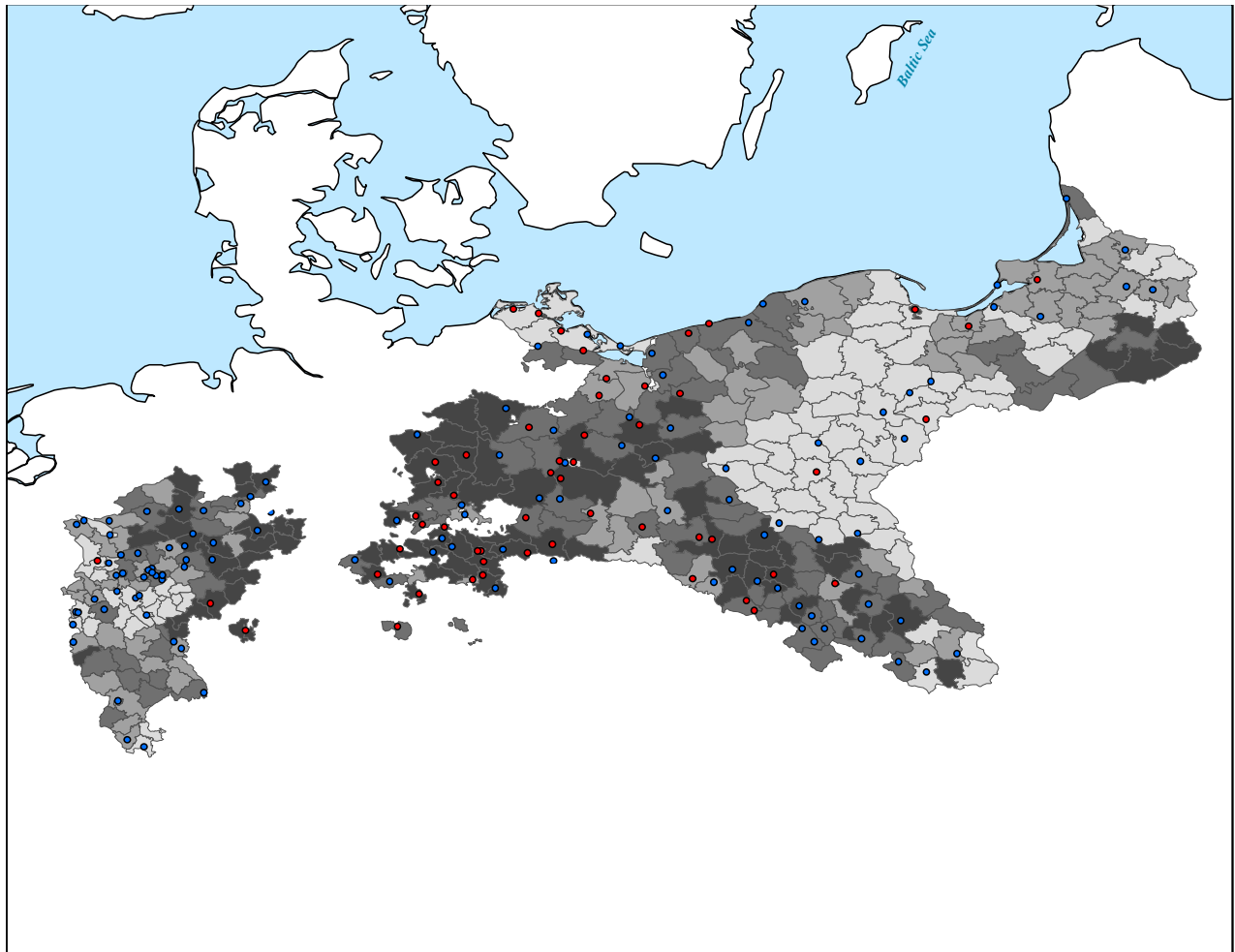


Figure 4: **School Laws (by 1600) and School Enrollment (1816)**: This graph shows cities with and without school laws (Red=City has a law. Blue=City does not have law.) and the county-level school enrollment measured as share of students of eligible school age population. Darker shades indicate higher school enrollment.

Table 1: Summary Statistics of Ordinance in Prussian Cities

This table provides the summary statistics for the implementation of ordinances as described in section 2.

	Observations	Mean	Std. Dev.	Min	Max
	[1]	[2]	[3]	[4]	[5]
All Cities					
Year First Law					
Total Number of Laws					
Education Laws					
Teacher Pay Provisions					
Curriculum Provisions					
Poor Laws					
Poor Law Oversight					
Morality Laws					
Church Governance Law					
Prussian Cities only					
Year First Law	56	1550	21.2	1521	1596
Total Number of Laws	56	3.0	2.9	1	15
Education Laws	56	1.2	1.3	0	6
Teacher Pay Provisions	56	0.6	1.0	0	4
Curriculum Provisions	56	0.7	1.0	0	4
Poor Laws	56	1.1	1.5	0	8
Poor Law Oversight	56	0.7	1.1	0	6
Morality Laws	56	0.8	1.2	0	6
Church Governance Law	56	1.3	1.6	0	8

Table 2: Summary Statistics of Prussian Cities by Education Ordinance Status

This table provides the summary statistics for the main regression variables by city education law status. Indicator in Städtebuch is equal to 1 if the city appears in the Städtebuch and 0 otherwise. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Count books is the number of education books that were printed in a city by 1517. Ln Population in 1500 is the logarithm of a city's population in 1500. Any church law is an indicator variable whether a city had any church ordinance as described in section 2 by 1600. Number of Schools is the total number of schools in a city in 1816.

	Cities with Laws			Cities without Laws		
	Cities	Mean	Std. Dev.	Cities	Mean	Std. Dev.
	[1]	[2]	[3]	[4]	[5]	[6]
Indicator in Städtebuch	40	0.93	0.27	132	0.80	0.40
Market Rights: Any pre-1517	37	0.41	0.90	94	0.73	1.25
Market Rights: Count pre-1517	37	0.24	0.44	94	0.32	0.47
Town Incorporated pre-1517	37	0.46	0.50	94	0.38	0.49
Town Mentioned pre-1517	37	0.84	0.39	94	0.75	0.43
Printing pre-1517	40	0.18	0.35	132	0.02	0.15
Count: Books pre-1517	40	5.5	15.25	132	25.42	226.65
Ln Population in 1500	18	9.61	7.97	24	9.71	10.21
Share Protestants in 1816	40	0.91	0.17	128	0.62	0.37
Number of Schools in 1816	40	25.95	36.73	132	10.20	9.26

Table 3: City-Level OLS Regression Results: Number of Schools

This table presents the regression results of estimating equation (1). The dependent variable is number of schools in 1816. Education law is an indicator variable whether a city had an education law by 1600. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Share of protestants is the population of with Evangelical-Reformist confession in 1816 divided by city population in 1816. Educational books is the number of education books that were printed in a city by 1517. Share of Protestants 1816 is the number of protestants in 1816 divided by total population in 1816. Any church law is an indicator variable whether a city had any church ordinance as described in section 2 by 1600. Standard errors are clustered on region in 1500 level. ***, **, * denotes 1%, 5%, and 10% statistical significance

Dependent Variable: Number of Schools in 1816						
	[1]	[2]	[3]	[4]	[5]	[6]
Education Law	15.15*** (3.26)	14.61*** (3.27)	13.16*** (4.03)	12.03*** (3.33)	13.12*** (3.86)	13.08*** (3.97)
Market Rights pre-1517		2.34 (2.92)	1.81 (2.70)	1.69 (3.42)	2.34 (3.38)	2.82 (3.63)
Town Incorporated pre-1517		4.60 (4.66)	4.31 (4.96)	3.81 (4.50)	4.43 (4.42)	5.32 (5.17)
Printing pre-1517			10.69 (6.96)	9.59 (14.93)	0.03 (10.68)	-0.44 (16.58)
Share of Protestants 1816				0.08 (2.36)	1.40 (1.62)	0.43 (1.92)
Morality Law				2.20 (11.18)	3.43 (11.05)	3.39 (12.84)
Educational Books pre-1517					0.15*** (0.05)	0.14** (0.04)
Territory Fixed Effects						Yes
Observations	172	142	142	138	138	138
R ²	0.11	0.11	0.13	0.12	0.16	0.18

Table 4: City-Level OLS Regression Results: Number of Schools

This table presents the regression results of estimating equation (1) for subsample of city with information on population in 1500. The dependent variable is number of schools in 1816. Education law is an indicator variable whether a city had an education law by 1600. Ln Population 1500 is the logarithm of a city's population in 1500. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Education books is the number of educational books that were printed in a city by 1517. Share of Protestants 1816 is the number of protestants in 1816 divided by total population in 1816. Morality law is an indicator variable whether a city had a morality as described in section 2 by 1600. Standard errors are clustered on region in 1500 level. ***, **, * denotes 1%, 5%, and 10% statistical significance.

	Dependent Variable: Number of Schools in 1816					
	[1]	[2]	[3]	[4]	[5]	[6]
Education Law	24.29** (11.96)	23.82** (8.39)	20.50* (10.31)	34.16** (14.89)	34.05*** (8.66)	29.45** (13.02)
Ln Population 1500		16.49*** (2.68)	15.35*** (4.72)	19.62*** (5.84)	28.05** (9.69)	12.42 (8.57)
Market Rights pre-1517			-5.21 (9.33)	-4.07 (11.28)	-0.95 (12.14)	5.20 (7.48)
Town Incorporated pre-1517			9.53 (11.90)	10.81 (11.41)	29.91 (17.31)	10.32 (10.83)
Printing pre-1517				-42.84 (39.05)	-63.99 (55.91)	-10.62* (4.81)
Share of Protestants 1816				-20.85 (16.83)	-24.38 (21.34)	-10.56 (6.59)
Morality Law				16.15 (29.15)	1.03 (28.08)	-17.41 (17.07)
Educational Books pre-1517				0.11 (0.08)	0.10 (0.08)	0.07** (0.03)
Territory Fixed Effects					Yes	Yes
Exclude Berlin						Yes
Observations	42	42	35	35	35	34
R ²	0.12	0.28	0.24	0.37	0.63	0.69

Table 5: City-Level Negative Binomial Regression Results: Number of Schools

This table presents the regression results of estimating equation (1) for subsample of city with information on population in 1500. The dependent variable is number of schools in 1816. Education law is an indicator variable whether a city had an education law by 1600. Ln Population 1500 is the logarithm of a city's population in 1500. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Education books is the number of educational books that were printed in a city by 1517. Share of Protestants 1816 is the number of protestants in 1816 divided by total population in 1816. Morality law is an indicator variable whether a city had a morality law as described in section 2 by 1600. Standard errors are clustered on region in 1500 level. ***, **, * denotes 1%, 5%, and 10% statistical significance.

Dependent Variable: Number of Schools in 1816						
	[1]	[2]	[3]	[4]	[5]	[6]
Education Law	0.86*** (0.31)	0.80*** (0.27)	0.74*** (0.24)	1.22*** (0.28)	1.15*** (0.12)	1.13*** (0.21)
Ln Population 1500		0.52*** (0.10)	0.49*** (0.12)	0.60*** (0.15)	0.65*** (0.10)	0.45** (0.18)
Market Rights pre-1517			-0.11 (0.22)	0.02 (0.15)	0.12 (0.14)	0.21*** (0.08)
Town Incorporated pre-1517			0.20 (0.27)	0.28 (0.28)	0.59*** (0.27)	0.40* (0.24)
Printing pre-1517				-1.03** (0.52)	-1.16* (0.60)	-0.41** (0.16)
Share of Protestants 1816				-0.65* (0.37)	-0.70** (0.32)	-0.50*** (0.19)
Morality Law				-0.29 (0.55)	-0.36 (0.46)	-0.72* (0.0.41)
Educational Books pre-1517				0.00* (0.00)	0.00** (0.00)	0.00*** (0.00)
Territory Fixed Effects					Yes	Yes
Exclude Berlin						Yes
Observations	42	42	35	35	35	34

Table 6: City-Level OLS Regression Results: Population

This table presents the regression results of estimating equation (2). The dependent variable is logarithm of the population in 1816. Education law is an indicator variable whether a city had an education law by 1600. Morality law is an indicator variable whether a city had an morality law by 1600. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Share of protestants is the population of with Evangelical-Reformist confession in 1816 divided by city population in 1816. Educational books is the number of education books that were printed in a city by 1517. Share of Protestants 1816 is the number of protestants in 1816 divided by total population in 1816. Morality law is an indicator variable whether a city had a a morality law as described in section 2 by 1600. Standard errors are clustered on region in 1500 level. ***, **, * denotes 1%, 5%, and 10% statistical significance

	Dependent Variable: Ln Population in 1816					
	[1]	[2]	[3]	[4]	[5]	[6]
Education Law	0.53*** (0.11)	0.52*** (0.16)	0.55*** (0.16)	1.14*** (0.26)		0.56*** (0.16)
Morality Law					0.30 (0.24)	-0.02 (0.28)
Market Rights pre-1517		0.30** (0.11)	0.25* (0.13)	0.50 (0.39)	0.23 (0.14)	0.25 (0.14)
Town Incorporated pre-1517		0.04 (0.12)	0.07 (0.15)	0.50 (0.46)	0.07 (0.15)	0.07 (0.13)
Printing pre-1517		0.47 (0.33)	0.49 (0.35)	-0.26 (0.45)	0.56 (0.44)	0.50 (0.49)
Share of Protestants 1816		-0.04 (0.10)	0.03 (0.13)	-1.10*** (0.24)	0.09 (0.10)	0.03 (0.11)
Educational Books pre-1517		0.01*** (0.00)	0.00*** (0.00)	0.00* (0.00)	0.00*** (0.00)	0.00*** (0.00)
Ln Population 1500				0.35 (0.29)		
Territory Fixed Effects			Yes	Yes	Yes	Yes
Observations	138	138	138	35	138	138
R ²	0.12	0.29	0.33	0.73	0.25	0.33

Table 7: City-Level OLS Regression Results: Share of Protestants

This table presents the regression results of estimating equation (1). The dependent variable is share of protestants in 1816. Share of Protestants 1816 is the number of protestants in 1816 divided by total population in 1816. Education law is an indicator variable whether a city had an education law by 1600. Morality law is an indicator variable whether a city had an morality law by 1600. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Share of protestants is the population of with Evangelical-Reformist confession in 1816 divided by city population in 1816. Educational books is the number of education books that were printed in a city by 1517. Any church law is an indicator variable whether a city had a morality law as described in section 2 by 1600. Standard errors are clustered on region in 1500 level. ***, **, * denotes 1%, 5%, and 10% statistical significance

	Dependent Variable: Share of Protestants in 1816					
	[1]	[2]	[3]	[4]	[5]	[6]
Education Law	0.28*** (0.06)	0.26*** (0.07)	0.14 (0.09)	0.17 (0.19)		0.07 (0.07)
Morality Law					0.22** (0.08)	0.18*** (0.05)
Market Rights pre-1517		-0.13* (0.07)	-0.01 (0.05)	0.05 (0.07)	-0.02 (0.07)	-0.02 (0.06)
Town Incorporated pre-1517		-0.00 (0.7)	0.00 (0.06)	0.06 (0.14)	-0.01 (0.07)	-0.01 (0.07)
Printing pre-1517		-0.04 (0.08)	-0.03 (0.09)	-0.14 (0.12)	-0.11 (0.08)	-0.12 (0.08)
Educational Books pre-1517		-0.00*** (0.00)	-0.00*** (0.00)	-0.00 (0.00)	-0.00*** (0.00)	0.00*** (0.00)
Ln Population 1500				-0.07 (0.07)		
Territory Fixed Effects			Yes	Yes	Yes	Yes
Observations	138	138	138	35	138	138
R ²	0.12	0.16	0.37	0.65	0.38	0.38

Table 8: City-Level IV Regression Results: Education Laws

This table presents the regression results of estimating equation (1) using the residual of the first stage regression of equation 4 that estimates the relationship between education laws and distance to Wittenberg. In the first stage, the dependent variable is whether the city had an education law by 1600. In the second stage, the dependent variable is number of schools in 1816, log population in 1816, and the share of protestants in 1816. Education law is an indicator variable whether a city had an education law by 1600. Distance to Wittenberg is measured in kilometres. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Education books is the number of education books that were printed in a city by 1517. Population bins are 6 indicator variables (population in 1500 data missing, 1,000, 2,000-5,000, 6,000-10,000, 11,000-20,000, and more than 20,000). ***, **, * denotes 1%, 5%, and 10% statistical significance.

	First Stage Education Law by 1600 [1]	Second Stage Number of Schools in 1816 [2]	Second Stage Population in 1816 [3]	Second Stage Share Protestant in 1816 [4]
Distance to Wittenberg	-0.001*** (0.000)			
Education Law Residual		9.051*** (2.083)	0.382*** (0.115)	0.069 (0.058)
Market Rights pre-1517	-0.027 (0.049)	-0.249 (3.748)	0.127 (0.118)	-0.018 (0.071)
Town Incorporated pre-1517	0.065 (0.068)	8.745 (6.163)	0.146 (0.123)	0.078 (0.059)
Printing pre-1517	0.234 (0.158)	-22.628 (17.030)	-0.349 (0.373)	-0.023 (0.201)
Educational Books pre-1517	-0.003*** (0.000)	-0.002 (0.032)	-0.000 (0.000)	-0.002*** (0.000)
Population Bin Fixed Effects	Yes	Yes	Yes	Yes
Observations	142	142	138	138
R ²	0.33	0.42	0.56	0.35
F-Statistic for IV				

Table 9: City-Level IV Regression Results: Morality Laws

This table presents the regression results of estimating equation (1) using the residual of the first stage regression of equation 4 that estimates the relationship between morality laws and distance to Wittenberg. In the first stage, the dependent variable is whether the city had a morality law by 1600. In the second stage, the dependent variables are number of schools in 1816, log population in 1816, and the share of protestants in 1816. Morality law is an indicator variable whether a city had an education law by 1600. Distance to Wittenberg is measured in kilometres. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Printing is an indicator variable whether books were printed in a city by 1517. Education books is the number of education books that were printed in a city by 1517. Population bins are 6 indicator variables (population in 1500 data missing, 1,000, 2,000-5,000, 6,000-10,000, 11,000-20,000, and more than 20,000). ***, **, * denotes 1%, 5%, and 10% statistical significance.

	First Stage Morality Law by 1600 [1]	Second Stage Number of Schools in 1816 [2]	Second Stage Population in 1816 [3]	Second Stage Share Protestant in 1816 [4]
Distance to Wittenberg	-0.001** (0.000)			
Morality Law Residual		12.257 (8.666)	0.349 (0.212)	0.123* (0.059)
Market Rights pre-1517	0.052 (0.067)	-0.040 (3.321)	0.132 (0.102)	-0.016 (0.070)
Town Incorporated pre-1517	0.086 (0.053)	9.015 (6.543)	0.153 (0.144)	0.010 (0.060)
Printing pre-1517	0.583*** (0.112)	-21.593 (15.969)	-0.319 (0.474)	-0.012 (0.059)
Educational Books pre-1517	-0.002*** (0.000)	0.002 (0.036)	-0.000 (0.000)	-0.002*** (0.000)
Population Bin Fixed Effects	Yes	Yes	Yes	Yes
Observations	142	142	138	138
R ²	0.31	0.42	0.44	0.36
F-Statistic for IV				

Table 10: City Selection

This table presents the regression results of estimating the propensity of a city being selected from all Prussian cities in Keyser (1939) into the sample of Prussia cities in Mützell (1825) used in our analysis. Any law is an indicator variable whether a city had any ordinance by 1600. Education law is an indicator variable whether a city had an education law by 1600. Morality law is an indicator variable whether a city had a morality law by 1600. Poor law is an indicator variable whether a city had a poor law by 1600. Market rights is an indicator variable whether a city had market right by 1517. Town incorporated is an indicator variable whether a city was incorporated by 1517. Books is the number of books that were printed in a city by 1517. Population bins are 6 indicator variables (population in 1500 data missing, 1,000, 2,000-5,000, 6,000-10,000, 11,000-20,000, and more than 20,000). ***, **, * denotes 1%, 5%, and 10% statistical significance. Standard errors in columns 1-8 are clustered at the territory level. Standard errors in column 9 are clustered at the 1849 county level.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Any Law	0.32*** (0.06)	-0.32*** (0.06)	0.22*** (0.05)	0.23*** (0.05)					0.05 (0.09)
Education Law					0.32*** (0.07)	0.26** (0.09)	0.22** (0.10)	0.22** (0.10)	0.28** (0.11)
Morality Law					0.00 (0.07)	-0.02 (0.06)	-0.05 (0.06)	-0.06 (0.06)	
Poor Law					0.11 (0.09)	0.11 (0.09)	0.11 (0.09)	0.11 (0.09)	
Church Law						0.06 (0.05)	0.07 (0.05)		
Market Rights pre-1517		0.14*** (0.03)	0.09** (0.03)	0.10** (0.04)	0.11*** (0.03)	0.11*** (0.03)	0.11*** (0.03)	0.11*** (0.03)	0.09* (0.05)
Town Incorporated pre-1517		0.03 (0.03)	0.01 (0.02)	0.02 (0.03)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.04 (0.04)
Books (in thsd)								0.28* (0.14)	
Population Bin Fixed Effects			Yes	Yes	Yes	Yes	Yes	Yes	Yes
Territory Fixed Effects			Yes	Yes	Yes	Yes	Yes	Yes	Yes
1849 County Fixed Effects	887	887	887	887	887	887	887	887	887
Observations	0.09	0.11	0.23	0.25	0.26	0.26	0.26	0.26	0.47
R ²									