WORKING PAPER SERIES

Institutional Transplant and Cultural Proximity: Evidence from Nineteenth-Century Prussia

Giampaolo Lecce and Laura Ogliari

Working Paper n. 598

This Version: March, 2017

Institutional Transplant and Cultural Proximity: Evidence from Nineteenth-Century Prussia[*]

Giampaolo Lecce[†] and Laura Ogliari[‡]

This version: March 2017

Abstract

The economic impact of exported institutions depends on the underlying cultural environment of the receiving country. We present evidence that cultural proximity between the exporting and the receiving country positively affects the adoption of new institutions and the resulting long-term economic outcomes. We obtain this result by combining new information on pre-Napoleonic kingdoms with county-level census data from nineteenth-century Prussia. This environment allows us to exploit a quasi-natural experiment generated by radical Napoleonic institutional reforms and deeply rooted cultural heterogeneity across Prussian counties. We show that counties that are culturally more similar to France, in terms of either religious affiliation or historical exposure to French culture, display better long-term economic performance. We analyze a range of alternative explanations and suggest that our findings are most easily explained by cultural proximity facilitating the adoption of new institutions.

Keywords: Institutions, Institutional Transplants, Culture, Economic Growth
JEL classification: N13, N43, O47, Z10, Z12

1

1 Introduction

Economists have long argued about the importance of good institutions for economic growth. Rule of law, better enforcement of contracts, and secure property rights are usually associated with better economic outcomes, such as higher investment in physical and human capital, and technological progress. Some countries lacking good legal institutions have attempted to import them from abroad with the goal of boosting economic growth. However, the adoption of good foreign institutions does not always lead to positive economic outcomes.

What are the elements of successful institutional transplants? In principle, the effectiveness of transplanted formal institutions hinges on their reception by local communities. Shared values, language, ethnicity and religion – collectively defined as *cultural proximity* – play a relevant role for at least three reasons. First, the new institutions, pervaded with the values of the exporting country, could be better tolerated inasmuch as their content resembles the customs and social norms of the receiving communities.[1] Second, irrespective of the content of the new rules, the receiving country may be more accommodating of institutions from closer and more familiar exporting countries. Finally, a given exporter may adopt a friendlier approach, engaging elites and local population in the reform process and making the new institutions more acceptable, in more culturally similar receiving countries.

We investigate the economic consequences of the interplay between new institutions and cultural proximity by exploiting a well-known historical natural experiment: the Napoleonic invasion of German territories and the consequent imposition of French institutions. Our novel finding is that transplants of these institutions had heterogeneous effects on economic performance across German areas characterized by different cultural traits. Transplanting French institutions was more effective in areas that were culturally closer to France, while it had virtually no effect in culturally distant territories. This result highlights the relevance of the interplay between culture and institutions for economic development, and it provides the first evidence that cultural proximity plays an essential role in transferring an institution from its original environment to a different context.

To perform our analysis, we build a novel, hand-collected dataset on pre-Napoleonic kingdoms, their rulers and the adoption of progressive reforms and then combine them with rich county-level information from different waves of the Prussian census. The resulting dataset consists of a cross-section of 447 counties belonging to 36 kingdoms and 17 independent or imperial cities controlled by 18 different rulers. We construct three measures of cultural proximity to France. First, since countries that share a religion are expected to have similar cultures (see Guiso et al., 2009 and Fisman et al., 2017), we exploit the fact that France was predominantly Catholic at that time and use the share of Protestants in the population as an inverse measure of cultural proximity. Second, we construct two proxies based on hand-collected data. These measures exploit pre-Napoleonic cultural linkages to separately capture ruler and local population familiarity with French culture.

We find evidence that cultural proximity is crucial for the long-run economic success of institutional transplants. In particular, the effect of good institutions is positive only in culturally similar counties. When measured as the share of Protestants in the population, cultural proximity increases the wages of primary school teachers (our main proxy for economic performance) by a sizable 12-20%. Moreover, in culturally similar areas, the positive effect of good

---

[1]In a relevant paper, Dal Bo et al. (2010) set up an experiment and argue that policies and rules are more effective – triggering private cooperation – when they are chosen democratically by the subjects than when they are exogenously imposed. Similarly, greater cooperation from subjects is likely if the imposed rule is perceived as familiar rather than foreign. For example, when US law was imposed in California after it was annexed, Mexican law continued to be *de facto* applied. Indeed, Californians perceived US law as foreign and distant from their social norms and traditions. Other interesting examples can be found in Mailath et al. (2016).

institutions increases with the number of years of French domination and with the number of progressive reforms implemented. On the contrary, we find no effect in culturally distant territories, irrespective of the intensity of the institutional treatment. Finally, we find that cultural proximity is at least as important in moderating the long-run economic effects of transplants as the alternative channels proposed in the literature, such as state capacity, institutional proximity, religious fractionalization and Napoleonic war severity.

We perform a battery of robustness checks to ensure that our results are not spuriously driven by observable and unobservable characteristics of Prussian counties. We show that the results are robust to the inclusion of a large set of controls, the adoption of different proxies for economic performance and religious affiliation, and the use of different samples. Then, we discuss our empirical strategy and show that the main findings are unaffected by pre-Napoleonic characteristics, both by conditioning the estimation on a full set of pre-Napoleonic kingdom fixed effects and by implementing a difference-in-differences specification.

Our paper contributes to an emerging literature on the interplay between culture and institutions. While the individual roles of culture and institutions on economic outcomes have been widely investigated (see Alesina and Giuliano, 2015 for a survey), studies on the interaction between these two drivers remain scant. Acemoglu and Jackson (2016) model the interaction between law enforcement and social norms. Similarly, Bisin and Verdier (2015) develop a model of culture, institutions and their joint dynamics.[2] We address an analogous question from an empirical perspective and show that cultural proximity generates complementarities between local cultures and transplanted institutions, thereby enhancing economic performance.

Our work is also connected to three additional strands of research. First, it relates to the literature on the transplantation of legal systems (Berkowitz et al., 2003a,b). While these studies focus on the effectiveness of imported legal institutions and attribute differences in their adoption to the process of lawmaking and the demand for law, we test the channel of cultural similarity as a moderating factor in the reception of transplanted institutions. We analyze the long-term economic effects of the interplay between new legal institutions and local cultures.

Second, our work is related to the literature investigating the importance of good institutions for economic growth. Starting with the seminal work of North (1990), many scholars have emphasized that institutions "matter".[3] Among these studies, Acemoglu et al. (2011) is the most closely related to this paper, as it is the first to exploit variation in institutional reforms during the Napoleonic campaigns in eighteenth- and nineteenth-century Germany.[4] In contrast to the existing contributions, our analysis does not focus on the positive effects of adopting good institutions but on the heterogeneity of their reception due to underlying cultural traits and on the resulting differences in long-term economic outcomes.

Finally, our work contributes to the rich literature analyzing the link between culture and economic performance.[5] In particular, Fisman et al. (2017) stress the role of cultural proximity, as measured by religious affiliation and social class, in mitigating informational asymmetries in the Indian lending market. The works by Becker and Woessmann (2009) and Cantoni (2015) are especially relevant to this paper. They both test the Weber hypothesis using data on early modern Germany – the same historical environment that we exploit – and draw mixed con-

---

[2]Tabellini (2008) analyzes the co-evolution of persistent cultural traits and legal institutions.

[3]See, for example, Acemoglu et al. (2001, 2002). On the relations between institutions and the legal origins of countries, see Glaeser and Shleifer (2002) and La Porta et al. (2008).

[4]See also Acemoglu et al. (2012) and Buggle (2016).

[5]See Alesina and Giuliano (2010), Alesina et al. (2013), Algan and Cahuc (2010), Bisin and Verdier (2000), Doepke and Zilibotti (2008), Fernandez et al. (2004), Galor and Moav (2002), Galor et al. (2016), Giavazzi et al. (2014), Greif (1993), Guiso et al. (2008), Harutyunyan and Ozak (2016), Nannicini et al. (2013), Nunn and Wantchekon (2011) and Tabellini (2010). Fernandez (2011) provides a detailed review of this literature.

clusions.[6] We provide the first empirical evidence that cultural proximity affects institutional transplants, thereby marrying the literature on culture to that on institutions.

The rest of this paper is organized as follows. Section 2 reviews the historical background, discussing the political situation of German territories before the French invasion, the French military campaigns, and the introduction of new institutions. Section 3 illustrates our data and provides some descriptive statistics. Section 4 presents our empirical strategy, while Section 5 illustrates the results, discusses their robustness and investigates the effect of the intensity of the institutional treatment. Several alternative explanations for our findings are explored in Section 6, while Section 7 investigates the effects of cultural proximity measures other than religious affiliation. Finally, Section 8 concludes.

2 Historical Overview

2.1 The Holy Roman Empire before 1800

The territories of the Holy Roman Empire had always been characterized by a considerable degree of heterogeneity. Since its founding in 962 AD, the Holy Roman Empire had been a multi-ethnic, multi-cultural, and multi-lingual ensemble of several entities – eventually numbering in the hundreds – governed by kings, dukes, counts and bishops that were collectively known as princes. These different layers of political power gradually became more autonomous, as the Holy Roman emperors shifted their attention to their local kingdoms.

The Protestant Reformation was pivotal to the progressive disintegration of the Empire. Starting as a protest against the corruption of the Roman Catholic Church, the Reformation quickly spread throughout central Germany, gaining the support of several princes who wanted to stress their political and religious independence. In 1555, after several years of war, the Emperor and the Protestant German princes signed a peace treaty in Augsburg. The *cuius regio, eius religio* ("whose realm, his religion") principle was affirmed, making Lutheranism an official religion of the Empire. The ambition of the Emperor to centralize power and rule over a unified empire was thus permanently shattered. Religious and political wars continued to afflict the Holy Roman Empire until a stable resolution was reached with the Peace of Westphalia. By 1648, the Empire was merely a confederation of German princes with the right, in their own lands, to legislate, impose taxes, organize an army, issue currency and engage in foreign policy.

The political fragmentation of the Empire (see Figure 1) gave rise to persistent institutional and cultural heterogeneity. This diversity was reinforced by the internal migration of religious minorities, notably Jews and French Protestants, who imported their own values and customs. All these elements contributed to an extremely variegated landscape wherein each territory displays its own identity. Religious affiliation, albeit just a facet, is the first evident aspect of these cultural differences.

2.2 Napoleonic Campaigns in Germany

The emergence of revolutionary France as an aggressive and strong military power at the end of the eighteenth century marked the end of the Holy Roman Empire. The first victories of the French army created a power vacuum in the central German territories, which Napoleon

---

[6]Cantoni (2015) analyzes the impact of Protestantism on urbanization using seventeenth-century city-level data and finds no significant effect of a Protestant ethic on economic development. Becker and Woessmann (2009) make a similar argument to that proposed by Botticini and Eckstein (2005, 2012), stressing the importance of human capital for explaining economic prosperity. Using cross-county variation in nineteenth-century Prussia, they find evidence of a higher level of human capital in Protestant areas, thus providing an alternative explanation for the greater prosperity of Protestant regions.

Figure 1: Holy Roman Empire in 1789
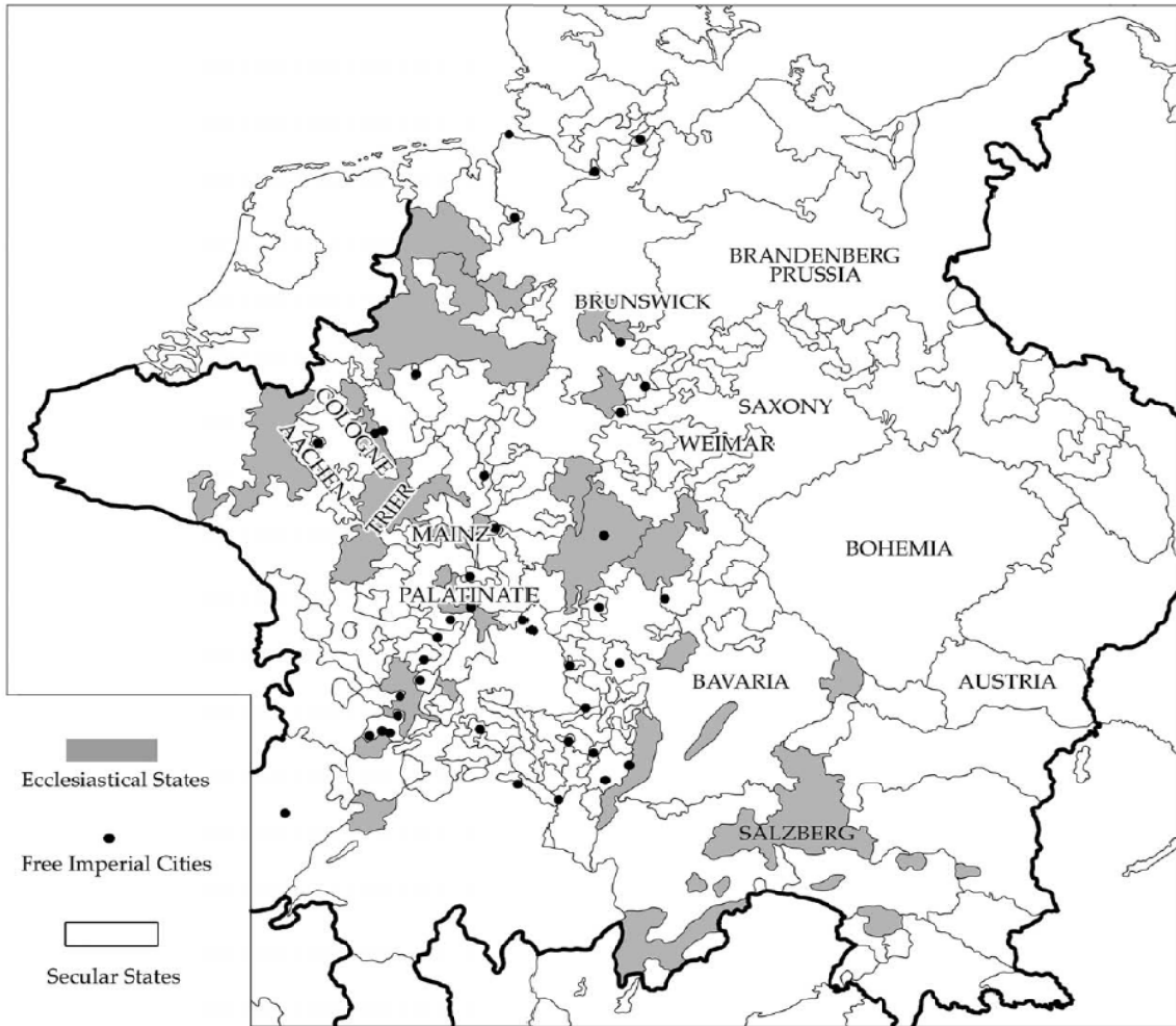
Source: Eric D. Brose. *German History 1789-1871: From the Holy Roman Empire to the Bismarckian Reich.* Berghahn Books, 1997.

exploited to create a "cordon sanitaire" between France and its traditional Eastern enemies: Austria and Russia. By 1795, Napoleon was in control of the left bank of the Rhine, which was formally annexed by the French Empire in the treaty of Luneville (1801).[7] When the House of Habsburg ceded some of their German estates to Napoleon's allies in 1805, the end of the Holy Roman Empire was essentially determined. The following year, central Germany was unified as the *Confederation of the Rhine*, a formally independent confederation of sixteen states whose protector and unofficial ruler was Napoleon.[8] French expansion continued until Napoleon's downfall after the Russian campaign in 1812. By that time, the French sphere of influence extended to Poland (with the creation of the Duchy of Warsaw in 1807) and Northern Germany (with the annexation of the Hanseatic cities of Hamburg, Lubeck, and Bremen in 1810). By the first decade of the nineteenth century, Napoleon had taken over the majority of German states. Figure 2 shows the counties in territories controlled by Napoleon differentiating between annexed areas and satellite states.[9]

As Acemoglu et al. (2011) argue, Napoleon's expansion of the French Empire was mainly driven by ideological and geopolitical concerns rather than by the economic outlook of the region. Besides the desire to maintain influence over a buffer region separating France from the two main Eastern powers, the Revolutionary rhetoric of *France's natural borders* drove his military campaigns.[10] Accordingly, we will exploit the quasi-natural shock of the Napoleonic invasions to identify the effect of institutional transplants.

2.3   The Imposition of French Institutions

Despite the marked institutional heterogeneity of the Holy Roman Empire resulting from high territorial fragmentation, some useful general features can be identified. At the dawn of the nineteenth century, the institutions of the *ancien regime* still pervaded German territories, and feudal privileges were the norm. In rural areas, peasants were subject to several restrictions and burdened by a list of duties and services they had to provide to their lords, even where serfdom had been abolished. In the cities, guilds regulated access to different trades, often limiting the development and growth of the industry they controlled.[11] Equality before the law was still far away: members of the aristocracy, clergy, and military benefited from particular exemptions, while other groups were discriminated against (e.g., Jews).

The arrival of Napoleon was a disruptive force. His rule over central Europe meant the imposition of a series of institutional reforms, the most important of which was the introduction of the Civil Code. Emblematic of the values promoted during the French Revolution, the *Code*

---

[7]According to Fisher (1903), this treaty has also been called the "First Revolution of Germany" given that a "territory of 150,000 square miles, peopled by 3,500,000 inhabitants, and amounting to about a seventh part of the population and territory of the whole Empire was transferred to foreign non-German powers" (pag. 38).

[8]The members of the Confederation promised to "supply their protector with a military contingent of sixty-three thousand men"(Lefebvre, 1969, pag. 207), and in return for their support, they were given higher status or territories. For example, the free cities of Augsburg and Nuremberg were annexed by Bavaria. Frankfurt was assigned to Karl von Dalberg, and Nassau became a duchy. Additionally, Dalberg became Prince Primate of the Confederation of the Rhine.

[9]We consider the German northwest territories (the Duchy of Arenberg and the Duchy of Oldenburg) as satellite states even though they were later annexed, in December 1810, by the French Empire.

[10]Discussions of the Rhine question began well before hostilities broke out in 1792. The idea of France's natural borders became prominent among Jacobin revolutionaries. On January 21, 1793, Georges Jacques Danton argued in favor of the annexation of Belgium during a national convention saying, "the limits of France are marked by nature, we will reach the four corners of the horizon, to the edge of the Rhine, to the edge of the ocean, to the edge of the Pyrenees, to the edge of the Alps. The boundaries of our Republic must be there". For more details, see Smets (1998).

[11]In the Rhenish area, for example, guilds imposed strict limitations on the adoption of new technologies (Kisch, 1989).

Figure 2: Counties Under Napoleonic Influence

	French Empire
	Satellite States
	No Napoleonic Influence
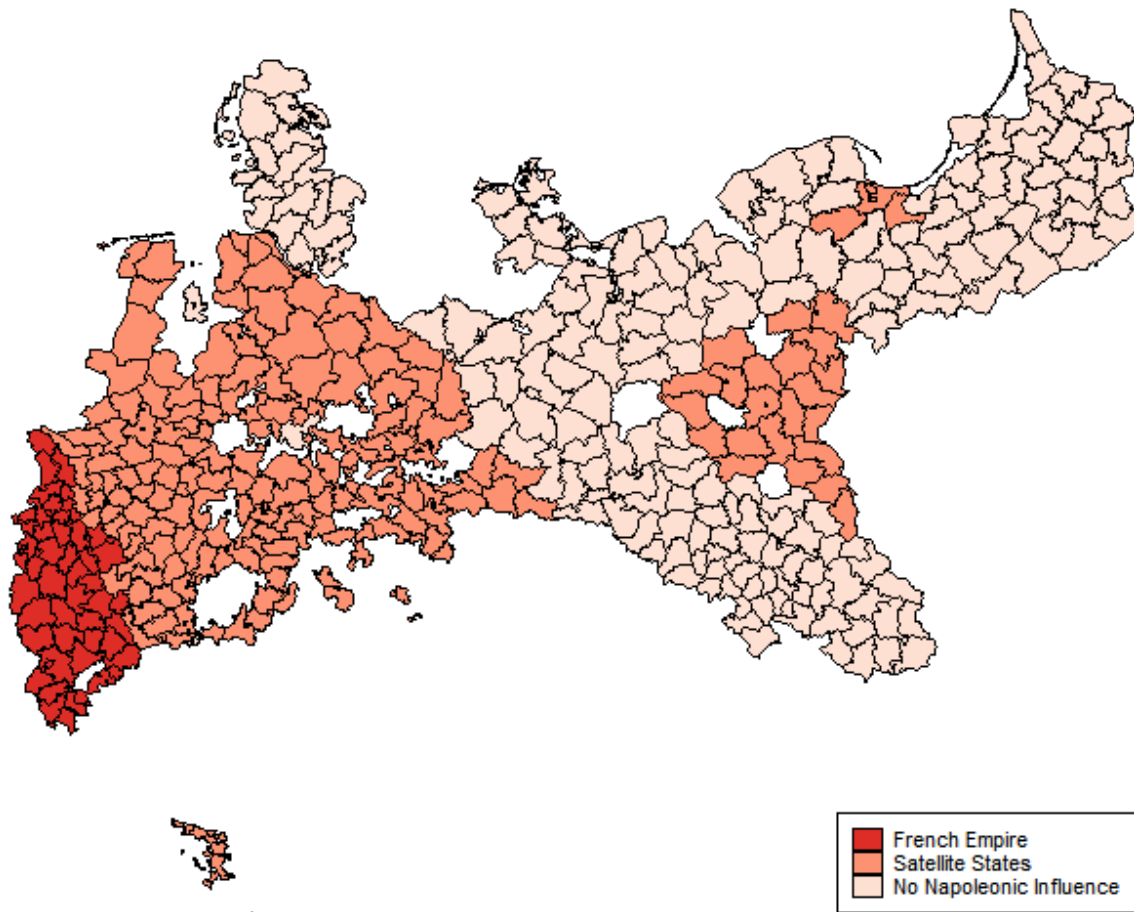
Napoleon (1804) introduced equality before the law for all men, regardless of their social or economic status. Moreover, it consecrated absolute property rights, to which the Code dedicated a total of 1776 articles.[12] Finally, the Code provided a modern legal framework that regulated all aspects of social interaction, from family matters to economic contracts. Interestingly, the drafting of the Code was driven not only by revolutionary ideals but also by conservative French values. According to Ellis (2003), at several points in the Code, "the legislative reforms of the Revolution were diluted or abandoned"(pag. 49). Additionally, several sources stress the similarities between some parts of the Code – especially the articles regulating family matters – and traditional French practices.[13] The process of codification continued with the promulgation of the Code of Civil Procedure (1806), the Commercial Code (1807), the Criminal Code, the Code of Criminal Procedure (1808) and the Penal Code (1810). All these codes were imposed on the satellites states under Napoleon's control. Interestingly, some states decided to retain the Code

---

[12]This is a vast sum compared to the 515 articles regulating personal behavior. See Woolf (2002).

[13]As Stetson (1987) observes, "[the Civil Code] was not a new set of laws. The family code was based on traditional ideals and customs dominant in France since the thirteenth century" (pag. 83). Smithers (1901) argues, "The substantial elements [of the Civil Code] are drawn from the Civil Law, Feudal Customs, Canon Law, Royal Ordinances, and Laws of the Revolutionary Assemblies, which predominate in the order named, showing how firmly tradition held the French people despite the annihilating processes of the Revolution" (pag. 142). Finally, Lobingier (1918) profiles the four members of the Commission who drafted the Code and writes, "Of the four who were selected everyone was past middle age and a conservative, at heart attached to the old regime, and Napoleon knew it" (pag. 117).

after Napoleon's fall, and even in those that formally abandoned the *Code Napoleon*, institutions were permanently affected.[14] Beyond judicial innovations, French rule implied a more efficient model of administration and the implementation of fiscal reforms that introduced budgeting and the rationalization of public expenditures.

In his quest to build a pan-European empire, Napoleon sought to assimilate conquered territories and to forge a class of loyal new *Frenchmen* to support him in the administration of the Empire. Historical evidence shows that, in some areas, local elites willingly fit themselves into Napoleonic society because they held the same values, and this facilitated the implementation of new institutions (as was the case in Piedmont and the Rhineland). In other areas, the amalgamation policy pursued by Napoleon imposed French culture on a reluctant population, and the Napoleonic Code was simply too alien to be enforced by local communities.[15] The introduction of legal, judicial and bureaucratic reforms occasionally generated hostility among the indigenous population; the same French recognized the complexity of exporting a system; as Woolf (2002) notes, "They recognized that acceptance of their procedures and values required trust and collaboration on the part of those they regarded as men of influence in society. They acknowledged that the imposition of so comprehensive a new set of rules would inevitably clash with many established practices, which had emerged from and expressed often long-accepted values" (pag. 185).[16] Anecdotal evidence suggests that in some areas, the Code met the opposition of not only the aristocracy, which was deprived of its privileges, but also the very social classes the revolution meant to emancipate. This indicates that the transition from the *ancien regime* to the modern era occurred at different speeds across German counties, and in some states, the attempt to transplant French institutions failed, likely because of cultural clashes.

3   Data and Variables

To investigate how cultural proximity influences the effects of institutional transplants on long-run economic outcomes, we build a novel dataset containing cross-sectional information on 447 Prussian counties immediately after German unification (1871).[17] We combine census data from the Ifo Prussian Economic History Database (iPEHD) with new detailed information on pre-Napoleonic kingdoms we collected and digitized from historical sources.[18] In particular, we map all the counties surveyed in the census into 36 eighteenth-century states: this allows us to build pre-Napoleonic, kingdom-level variables that will serve as measures of cultural and institutional proximity.[19] We complement the resulting dataset with information on historical

---

[14]In the Rhineland territories annexed by Prussia after the Congress of Vienna, a commission was set up to decide whether to extend Prussian law or keep French law. The Commission ended its work in 1818 and recommended the preservation of the French judicial system. French law remained in force in the Rhineland until 1900. Local populations and business communities were at the forefront of efforts to retain the Code. See Rowe (2000) and Diefendorf (1975) for more details. Another notable example is the Duchy of Baden, which decided to retain the Napoleonic Code after 1815.

[15]For a thorough discussion, see Parsons (2010).

[16]For example, Rowe (2003) pag. 130, documents the existence of distrust and animosity between Rhinelanders and "*émigrés* and the corrupt hangers-on who had entered the region with the revolutionary army".

[17]We chose this period because it guarantees the widest geographical coverage (including the former members of the Confederation of the Rhine) and a sufficiently long time lag for the new institutions to affect economic performance. The difference between our sample and that used in Becker and Woessmann (2009) is that five counties could not be mapped onto pre-Napoleonic kingdoms. Our results are not affected if we include those observations in the specifications with no pre-Napoleonic controls.

[18]For additional information on Prussian census data, see Becker et al., 2014.

[19]Details on the construction of these variables are provided in Section 7. The list of pre-Napoleonic kingdoms is reported in the Data Appendix.

Figure 3: Share of Protestants in 1871

▉	>=96%
▉	84%-96%
▨	25%-84%
▢	0-25%
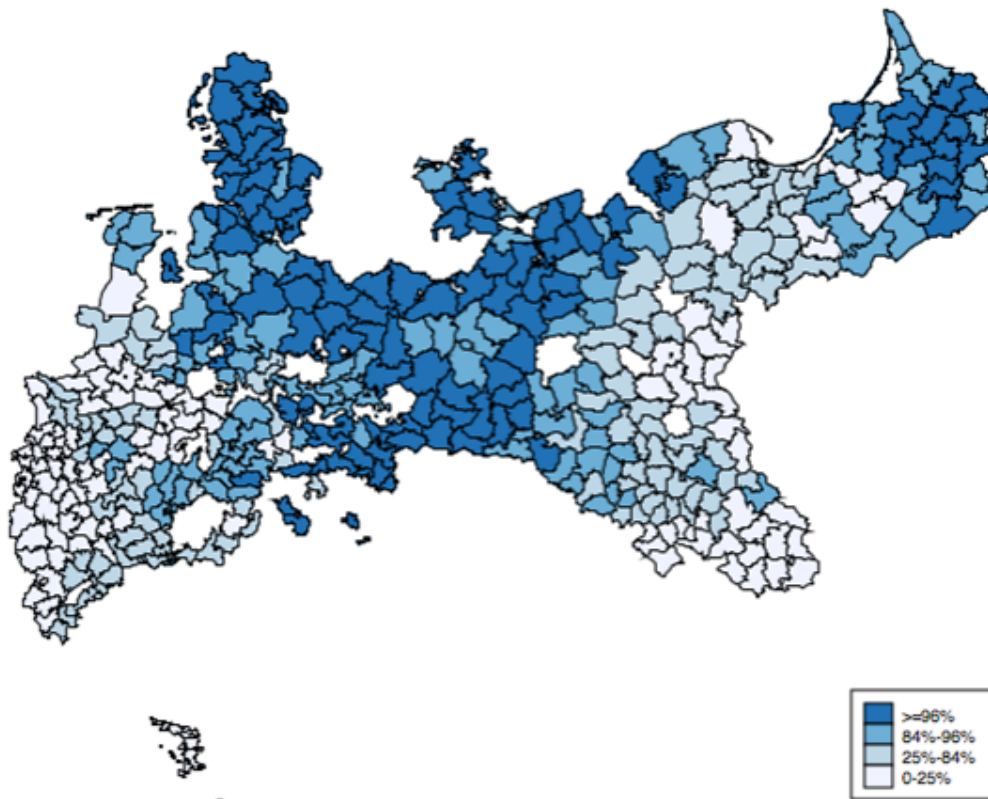
religious affiliations from Cantoni (2012) and Spenkuch (2010).

Following an established literature (Allen, 2001; Galloway et al., 1994; Becker and Woessmann, 2009), we use wages to proxy for economic performance. Our main measure of county-level income is the average annual wage of male elementary school teachers from the 1886 Education Census. This is the earliest direct measure of income available for all counties. Teacher salaries, albeit representative of a single occupational group, reflect the development of the county, as its main determinants are local contributions.[20] One drawback of this proxy is that it may be influenced both by the value that the local community attaches to education and by other benefits provided to teachers (e.g., free housing). In our empirical analysis, we address these issues by controlling for factors that affect both the demand for teachers (e.g., the number of pupils, demographic structure) and the supply of teachers (e.g., free housing and the total number of teachers). By exploiting later waves of the Prussian census, we assess the robustness of our results using the wages of (unskilled) day laborers in 1892 and income tax revenue per capita, an additional income proxy used by Galloway et al. (1994) and Becker and Woessmann (2009). Our main measure of institutional transplantation is a binary variable, which takes the value 1 if the county is either in a province annexed by the French Empire (e.g., the Rhineland) or in a satellite state (e.g., the Kingdom of Westphalia). This variable reflects the presence of French institutions since, in our sample, all territories under direct or indirect control of Napoleon were subject to the Civil Code and, at least partially, the set of modernizing

---

[20]For more details, see Becker and Woessmann (2009) and Schleusen (1989).

reforms.[21]

Exposure to the new institutions was not homogeneous across kingdoms, as it varied according to the fortune of the military campaigns. Therefore, we employ alternative proxies for institutions that take into account the varying intensity of institutional treatment. Specifically, in Section 5.4, we first differentiate territories annexed by the French Empire, which faithfully imported all French institutions, from satellite kingdoms. We then use duration of French presence, which ranges from 5 to 20 years, as a proxy for the intensity of the transplant. Finally, we exploit the index of reforms constructed by Acemoglu et al. (2011), which combines the different durations of four reforms (the introduction of the Code, the abolition of serfdom, the abolition of guilds and agrarian reforms). Consistent with the existing literature, we expect the average effect of good transplanted institutions on economic performance to be stronger with more intense treatment.

Following the existing literature (e.g., Guiso et al., 2009, Spolaore and Wacziarg, 2009 and Fisman et al., 2017), we use religious affiliation as our main proxy for cultural similarity to France. Falck et al. (2012) show that there is a strong geographical correlation between local dialect (their proxy for cultural identity) and religious affiliation in post-unification Germany. Moreover, as previously mentioned, religion was of paramount importance in shaping the politics of the Holy Roman Empire, determining alliances, wars, marriages and lineages. Hence, religious affiliation synthesizes various cultural aspects beyond the mere spiritual dimension. Catholic territories were closer to the identity of the French authority and to the content of the new set of rules that was imbued with centuries of French culture. Accordingly, we use the share of Protestants in the county as an inverse proxy for cultural similarity. We draw this information from the 1871 Prussian census. Because religious affiliation is highly persistent across centuries, cross-county variation in this measure is mostly driven by long-run differences in cultural traits, which likely existed prior to the French invasion.[22] The geographical distribution of this measure is displayed in Figure 3, showing that Protestants are mainly concentrated in the central parts of modern Germany, particularly around the city of Wittemberg where Protestantism was born.

Finally, in our analysis, we use a rich set of controls, including historical, geographic and contemporaneous controls. *Historical controls* are meant to capture pre-existing differences across counties. We construct a dummy variable indicating the presence of Hanseatic or free imperial cities in the sixteenth century, since they benefited from particular economic and diplomatic privileges. We also control for pre-Napoleonic economic development using the urban population in 1500 from Becker and Woessmann (2009).[23] *Geographic controls* include the latitude (in radians), the log of total area of the county and a dummy variable for Polish-speaking provinces (which are mainly located in the east and underdeveloped).[24] Using in-

---

[21]The only exception in our sample is the Duchy of Nassau, where the Code was formally adopted but never entered into force. In our main specification, we include the two counties of Nassau as satellite states, since the Duchy joined the Confederation of the Rhine. We drop the Nassau territories from our sample in a robustness check; see Section 5.2.

[22]Cantoni (2015) finds a correlation of 0.98 between the Protestant shares in 1820 and 1900 across Germany. See Cantoni (2012) and Falck et al. (2012) for other examples of papers documenting the persistence of denominational affiliation. In Section 5.3, we address potential endogeneity concerns by using three alternative Protestant measures: (i) a dummy variable indicating the religious affiliation of the kingdom at the beginning of the seventeenth century taken from Cantoni (2012) and Spenkuch (2010); (ii) the share of Protestants in the aftermath of the Congress of Vienna, which is available for a subsample of 349 counties; and (iii) instrumenting the 1871 Protestant share with the distance from Wittemberg.

[23]See also, De Long and Shleifer (1993).

[24]We do not include longitude in our specifications because it is strongly correlated with the institutional variable. Indeed, the Napoleonic invasion followed a west-east trajectory starting from neighboring territories and moving toward Russia. This renders longitude a proxy for the intensity of French presence. In Section 5.3.1, we

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Napoleon	0.528	0.499	0	1	447
French Empire	0.121	0.326	0	1	447
Satellite States	0.407	0.492	0	1	447
Years of French Invasion	4.647	5.917	0	19	447
Income of Male Elem. School Teachers (1886)	983.123	201.322	711.961	1954.194	447
Protestant Share	0.644	0.377	0.003	0.999	447
French Exposure	0.210	0.408	0	1	447
Pro-French Ruler	0.761	0.427	0	1	447
Institutional Proximity	0.469	0.361	0	1	447
% of County Population in Urban Areas	0.276	0.22	0	1	447
% Females	0.51	0.015	0.44	0.546	447
% Age under 10	0.247	0.025	0.153	0.299	447
Total Population (log)	10.804	0.416	9.359	13.625	447
County Area (log)	10.798	1.152	5.313	12.955	447
Universities Holy Roman Empire	0.06	0.238	0	1	447
Hanseatic or Imperial City	0.098	0.298	0	1	447

formation from the 1871 and 1886 censuses, we control for the demographic and social characteristics of the population and the industrial features of the county, as well as other aspects that may affect the wages of teachers (*Socioeconomic and Education Controls*).[25] Table 1 reports summary statistics for our main variables.[26] The table shows that one-half of the sample was under the direct (12%) or indirect control (43%) of Napoleon. Moreover, we observe significant variability in the annual wages of male elementary school teachers, which range from 711.961 to 1954.194 gold marks. Table 1 also reveals that while eighteenth-century Prussia was mainly Protestant (the average Protestant share is 64.4%), we observe sizable differences in counties' religious affiliation. Table 2 compares the unconditional means of our main dependent variable across religious affiliations for counties that received or did not receive French institutions. In particular, institutional transplantation is associated with higher wages, but the effect is much stronger in Catholic territories and produces a statistically significant difference-in-differences coefficient. This first simple result points to the heterogeneous economic effects of institutional transplants. In what follows, we test this effect by exploiting the rich variation offered by our county-level data.

4   Empirical Strategy

This section presents the empirical model we use to test our central hypothesis, namely, that institutional transfer was more effective – and hence induced better economic performance – in places that were more culturally similar to France.

Our baseline specification is as follows:

---

carefully address this issue by conditioning on different sets of fixed effects.

[25]Specifically, *Socioeconomic Controls* include the log of total population size, the percentage of the county population in urban areas in 1871, the percentage of Jews, the percentage of the labor force in mining in 1882, the number of farms in 1882, the log of the distance from the imperial capital (Berlin) and from the district capital, and the year of annexation by Prussia. *Education Controls* include the percentage of pupils traveling a distance of over 3 km to attend school, the log of the total number of pupils in 1886, the log of the total number of teachers in 1886 and the number of free apartments for male teachers in 1886.

[26]Summary statistics are divided by treatment and control group and are reported in Table A1 in the Appendix.

Table 2: Annual Wages Elementary School Teachers – Comparison of Means

	Napoleon=0	Napoleon=1	Diff
Catholic	856.50	1017.66	-161.16
	(14.71)	(21.82)	(38.21)
Protestant	941.31	972.35	-31.04
	(15.54)	(16.85)	(24.01)
Diff	-84.81	45.31	-130.12
	(26.11)	(33.12)	(44.73)

Notes: A county is defined as Catholic if it is in the first quartile of the distribution of *Protestant Share*, while is defined as Protestant if it is in the fourth quartile. Standard errors in parentheses.

$$
\begin{aligned}
y_i = {} & \alpha + \beta_1 Culture_i + \beta_2 Napoleon_i + \beta_3 Culture_i \times Napoleon_i \\
& + \mathbf{H}_i\beta_4 + \mathbf{G}_i\beta_5 + \mathbf{E}_i\beta_6 + \mathbf{X}_i\beta_7 \\
& + \mathbf{H}_i \times Napoleon_i\beta_8 + \mathbf{G}_i \times Napoleon_i\beta_9 + \varepsilon_i
\end{aligned} \tag{1}
$$

where y_i is the log of the average income of male elementary school teachers in county i; $Culture_i$ is one of our measure of cultural proximity; $Napoleon_i$ is a binary variable indicating the adoption of the Napoleonic Code; H_i, G_i, E_i and X_i are vectors of historical, geographical, educational and socioeconomic controls, respectively (details below); and ε_i is an error term. The regression equation also includes an interaction between the institutional dummy, $Napoleon_i$, and pre-Napoleonic and geographic variables that account for the differential effects of the institutions depending on pre-existing characteristics that are not related to culture. We interact only controls that were fixed at the time that our variable of interest, Napoleonic invasion, was determined.

Our aim is to identify the differential impacts of these institutions on economic outcomes across levels of cultural similarity. The coefficient of interest is therefore β_3, which captures the differential effects of institutions on economic outcomes across counties with different degrees of cultural proximity to France.

The identification of β_3 requires that other characteristics correlated with prosperity did not influence Napoleon's invasion across areas with different degrees of cultural proximity to France. As discussed in Section 2.2, the French invasions had primarily geopolitical, military and ideological motives. This makes it unlikely that Napoleon systematically selected territories based on their cultural traits. Indeed, the placebo regressions estimated in Section 5.3 reveal no correlation between $Culture_i \times Napoleon_i$ and all available proxies for economic development prior to the French invasion.

5 Results

5.1 Baseline Estimates

The benchmark results are reported in Table 3. Column (1) shows the results of the most parsimonious specification, which includes only geographical controls. Our coefficient of interest (β_3) is negative and significant, indicating that Napoleonic institutions had virtually no impact on economic performance in culturally distant (i.e., Protestant) areas. Specifically, the positive

effect of institutions vanishes for a county whose Protestant share of the population equals 82%. In columns (2)-(5), we progressively add different sets of controls. Column (2) includes historical variables that take into account counties' pre-Napoleonic differences. Columns (3) and (4) add socio-economic and education controls, respectively. These variables not only capture the economic and social outlook of the county after the Congress of Vienna but are also meant to account for elements that influence the demand for and/or supply of teachers, thereby directly affecting our preferred proxy for economic performance. Unlike previous controls, these can be considered 'bad' controls (Angrist and Pischke (2009)) since they are themselves potential outcomes of institutional transplant. However, our results are robust, and neither the magnitudes nor the statistical significance of our coefficients of interest are affected. Finally, to eliminate the possibility that our coefficient of interest is spuriously driven by the interplay of transplanted institutions with pre-existing county-level characteristics, column (5) implements our baseline model and includes interactions between Napoleonic institutions and historical and geographic controls, thereby obtaining the final model discussed in Section 4.

Table 3: Institutions and Religious Affiliation

Log Average Wage for Male Elementary Teachers in 1886	(1)	(2)	(3)	(4)	(5)
Napoleon	0.118***	0.114***	0.143***	0.127***	-0.676
	(0.0232)	(0.0227)	(0.0257)	(0.0227)	(0.835)
Protestant Share	0.175***	0.189***	0.176***	0.170***	0.193***
	(0.0260)	(0.0252)	(0.0303)	(0.0320)	(0.0387)
Napoleon $\times$ Protestant Share	-0.105***	-0.117***	-0.192***	-0.173***	-0.218***
	(0.0348)	(0.0339)	(0.0319)	(0.0297)	(0.0399)
Geographic Controls	yes	yes	yes	yes	yes
Historical Controls	no	yes	yes	yes	yes
Socioeconomic Controls	no	no	yes	yes	yes
Education Controls	no	no	no	yes	yes
Hist & Geo Interactions	no	no	no	no	yes
R^2	0.371	0.399	0.659	0.667	0.675
Obs.	447	447	447	447	447

Notes: Geographic Controls: latitude, area of the county (log) and Polish-speaking area. *Historical Controls*: population in 1500 and Hanseatic or Imperial cities. *Socioeconomic Controls*: total population size (log), percentage of county population in urban areas in 1871, percentage of Jews, percentage of labor force in mining in 1882, number of farms in 1882 (log), distance from the imperial capital (Berlin), distance from the district capital and year of annexation by Prussia. *Education Controls*: percentage of pupils traveling over 3 km to school, total number of pupils in 1886 (log), total number of teachers in 1886 (log), and number of free apartments for male teachers in 1886. *Hist & Geo Interactions*: interaction between *Napoleon* and both *Geographic Controls* and *Historical Controls*. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Consistent with the existing literature, the linear effects of both good institutions (*Napoleon*) and Protestant affiliation (*Protestant Share*) on our measure of economic performance is positive and statistically significant across all specifications, suggesting that transplanting good institutions and the presence of a Protestant majority can indeed improve economic outcomes.[27]

---

[27]Note that in column (5), the coefficient associated with French institutions cannot be interpreted as the linear effect of good institutions since, in that specification, we include all interactions between historical and geographical controls and the Napoleon dummy. The effect of *Napoleon* is 0.04, significant at 10%, when we evaluate it at the

Notably, β_3 is always negative and significant and is remarkably stable across specifications.[28]

5.2 Robustness Checks

This section presents a series of checks to verify the robustness of the baseline estimates. First, we use different proxies for the dependent variable. Then, we test our model specification by adding both historical and contemporaneous controls, using different clustering and performing the analysis on different samples. The results are reported in Table 4.

Dependent Variable Panel a) of Table 4 shows that the baseline results hold for alternative proxies for economic prosperity. In row (1), we use the average income of male elementary school teachers in levels. The results are consistent with the baseline model, and the interaction term is statistically significant and negative. The coefficients differ in terms of magnitude compared to our main specification wherein the dependent variable is measured as the logarithm. In row (2), we use another direct measure of income, that is, the log wage of a daily laborer in 1892.[29] Then, as in Becker and Woessmann (2009), we use income tax revenue per capita as the dependent variable in row (3). The coefficient of the interaction term remains consistently negative and statistically significant.[30]

Contemporary Controls In panel b), we introduce additional post-Napoleonic controls that may affect our results. A possible concern is that our result is induced by differences in purchasing power across regions or by other drivers of the demand for teachers and, consequently, of their wages. Hence, we first include a price measure to capture potential differences in purchasing power across counties (row 4). This proxy is constructed as the ratio of total expenditures on new school buildings in 1886 to the total number of new school buildings, which should capture variation in housing prices. We then add a group of demographic variables from 1871 – including household size, the share of females and the share of the population under 16 – that might influence the demand for teachers. We also include the share of the population of Prussian origin and the share of the population born in the county to control for the stock of both internal and foreign migrants (row 5). In row (6), we include the literacy rate in 1871. Since Protestants are, on average, more literate, one concern is that our effect may be confounded by the value that people in different counties attribute to schooling.[31] The results consistently confirm the baseline estimates.[32]

Clustering To allow for an arbitrary variance-covariance matrix capturing potential serial correlation in the residual error term, panel c) considered clustered standard errors at the pre-Napoleonic-reign level (row 7); at the pre-Napoleonic-ruler level (row 8), since many king-

---

average value of the continuous variables when all the dummies equal zero, while it is not statistically different from zero when all the dummies equal one.

[28]The coefficients of the interaction terms are also very stable when we replicate Table 3 using the Protestant Majority Dummy (this measure is discussed in Section 5.3.3) and when we include Protestant Share Squared in order to capture non-linear effects. See Tables 8 and A2 in the Appendix.

[29]Table 4 displays the results for male laborers in urban areas. The coefficients are virtually the same when using the wage of a rural male daily laborer or the wage of a female daily laborer. These results are available upon request.

[30]The complete results of specifications (1), (4) and (5) with these alternative dependent variables are reported in the Appendix. See Table A3.

[31]For the effect of human capital on economic growth, see, for example, Barro (2001) and Gennaioli et al. (2014).

[32]The robustness of the coefficients is confirmed by the results of Oster tests. The bias-adjusted estimated effect of the interaction term, *Napoleon × Protestant Share*, is always strictly negative and much larger than the OLS estimate (Oster, 2017), suggesting that the degree of omitted variable bias is unlikely to explain the size of the estimated effect.

Table 4: Robustness Checks – Specification

| | Napoleon × Protestant Share | | | |
	Coeff.	Std.Err.	Obs.	R^2
a) Dependent Variables				
1) Wage Elem. Teacher (level)	-219.7***	(45.950)	447	0.671
2) Wage Urb. Male Lab. 1892 (log)	-0.268***	(0.0524)	430	0.695
3) Income Tax Revenue p.c. 1877	-0.858***	(0.2080)	421	0.384
b) Additional Contemporary Controls				
4) Price (Real estate unit price)	-0.213***	(0.0411)	441	0.674
5) Demographic	-0.160***	(0.0448)	447	0.702
6) Literacy Rate	-0.122***	(0.0396)	447	0.696
c) Clustering Levels				
7) Pre-Napoleonic Kingdom	-0.218***	(0.0675)	447	0.675
8) Pre-Napoleonic Ruler	-0.218***	(0.0623)	447	0.675
9) Post-Napoleonic Kingdom	-0.218***	(0.0538)	447	0.675
10) 1871 District	-0.218***	(0.0580)	447	0.675
d) Alternative Samples				
11) Trimming	-0.207***	(0.0321)	439	0.672
12) Winsorizing	-0.213***	(0.0373)	447	0.679
13) Df Beta	-0.256***	(0.0280)	425	0.736
14) Excluding Polish-speaking Areas	-0.252***	(0.0514)	328	0.656
15) Excluding County of Nassau	-0.247***	(0.0406)	438	0.702
16) Excluding Rhineland	-0.203***	(0.0442)	393	0.671
17) Excluding Areas Annexed after 1810	-0.255***	(0.0424)	411	0.697
18) Excluding Confederation of the Rhine	-0.195**	(0.0931)	265	0.713

Notes: Dependent variable: Log average wage of male elementary school teachers in 1886. All specifications include *Geographic Controls*, *Historical Controls*, *Socioeconomic Controls*, *Education Controls* and *Hist & Geo Interactions*. See also, the notes to Table 3. Robust standard errors in parentheses, unless otherwise specified in the table.
*** p<0.01, ** p<0.05, * p<0.1

doms were under the control of the same authority; at the pre-unitary-state level after the Napoleonic German Mediatization, as defined by Acemoglu et al. (2011) (row 9); and at the Prussian political-district level in 1871 (row 10). The coefficients of interest are always statistically significant, as in the baseline estimates.

Samples Finally, in panel d), we show that our evidence is not driven by influential observations. First, we trim (row 11) and winsorize (row 12) the extreme 1% of observations of our dependent variable. In row 13, we compute a measure of the influence of each observation on the estimated coefficient. In particular, an observation is considered influential when the difference between the regression coefficient estimated using the whole sample and that calculated excluding the observation is above a standard cut-off value.[33] We then exclude all the influential observations for the coefficient of interest (*Protestant × Napoleonic Code*). Moreover, in rows (14)-(18), we consider alternative subsamples. In row (14), we exclude Polish-speaking areas, as these territories are mostly Catholic and have below-average economic performance. We then exclude the Duchy of Nassau (row 15), since it joined the Confederation of the Rhine

---

[33]The cut-off value we use for a highly influential observation is $2/\sqrt{(n)}$, but our results are robust to the use of different cut-offs.

but did not implement the Code despite formally adopting it. (See Arvind and Stirton (2010)). Then, we exclude the territories under the direct control of the French Empire and consider as treated only those territories in the Confederation of the Rhine (i.e., satellite states) that adopted the Code (row 16) in order to exclude the possibility that our results are mainly driven by the Rhineland. We also exclude territories annexed after 1810 (row 17) because they were under French influence for only a few months. Finally, in row (18), of the territories under the control of Napoleon, we keep only those annexed by the French Empire. These different specifications show that the interaction between institutions and cultural distance is statistically significant, negative and remarkably stable.

5.3 Threats to Identification

In this section, we discuss three possible threats to our identifying assumptions, omitted variable bias, unobserved heterogeneity and measurement error in the cultural proxy, and present our empirical strategies for dealing with these issues.

5.3.1 Omitted Variables: Fixed Effect Specification

A first concern with our identification strategy is that the presence of unobserved characteristics may influence both the economic outcomes and the cultural traits. In this section, we address this issue by conditioning on a series of different fixed effects. Specifically, we add two sets of kingdom fixed effects and three groups of geographic dummies to the baseline specification. This allows us to take into account all pre-Napoleonic kingdom-level characteristics (e.g., institutional setting, infrastructure) and exploit only the within-kingdom variation in the explanatory variables. Table 5 reports the results.

The first set of fixed effects we include is defined at the ruler level (*Ruler FE*) and controls for all features common to territories under the same ruler (e.g., institutional reforms, legal framework). We identify 18 different rulers at the time of the Napoleonic invasions and include a dummy for each (column 1).[34] Although ruled by the same prince at the time of French invasion, some territories could have developed distinctive attributes over time. Accordingly, we include pre-Napoleonic kingdom fixed effects (column 2). The coefficient of interest, although slightly smaller, is always negative and statistically significant when we exploit within-kingdom variation, suggesting that the economic effect of French institutions crucially relies on the cultural background.

It is worth noting that, when using within-kingdom variation, the average east-west distance we exploit is 160 km, approximately 10% of the total extension of eighteenth-century Prussia. Hence, the inclusion of kingdom fixed effects largely captures the influence of unobserved characteristics correlated with longitude and alleviates concerns that geography biases our results. To further tackle this issue, we compute the distance quintiles between each county and (i) Paris, (ii) the French Border and (iii) Berlin and include a dummy variable for each quintile. The results are reported in columns (3)-(5). Reassuringly, the estimated coefficient on the interaction term is negative and significant across all specifications. Adding the distance dummies, especially the distance from the French border, reduces the magnitude of this coefficient. This is not surprising given that the distance from the French border also captures the trajectory and timing of the military expansion of the French Empire and, thus, the intensity of the institutional treatment.

---

[34] As noted above, several kingdoms were under the same ruler; thus, the number of ruler fixed effects, 18, is smaller than the number of kingdom fixed effects, 36.

Table 5: Fixed Effects Specification

Log Average Wage of a Male Elementary School Teacher in 1886	(1)	(2)	(3)	(4)	(5)
Napoleon	-0.910	-0.773	-0.505	-0.387	0.0785
	(0.696)	(1.445)	(0.819)	(0.787)	(0.867)
Protestant Share	0.144***	0.152***	0.206***	0.116***	0.105***
	(0.0262)	(0.0397)	(0.0422)	(0.0403)	(0.0378)
Napoleon × Protestant Share	-0.133***	-0.116**	-0.215***	-0.0768*	-0.0653*
	(0.0386)	(0.0538)	(0.0400)	(0.0449)	(0.0386)
Specification	Ruler FE	Kingdom FE	Dist. Paris	Dist. Berlin	Dist. FR. Border
R^2	0.798	0.803	0.681	0.731	0.722
Obs.	447	447	447	447	433

Notes: All specifications include *Geographic Controls, Historical Controls, Socioeconomic Controls, Education Controls* and *Hist & Geo Interactions*. See also, the notes to Table 3. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Note that the coefficient capturing institutions' linear effect is no longer statistically significant. We are exploiting within-kingdom variation, and the identification relies on a few kingdoms that were only partially invaded; hence, the positive effect of institutions is not easily identifiable.[35]

5.3.2 Unobserved Heterogeneity

A second concern with the identification of our main coefficient is that the presence of observed or unobserved cross-county differences or trends are correlated with institutional transplants. In this section, we use two different approaches to address this concern.

Placebo Tests Our first strategy consists of estimating placebo regressions using the pre-Napoleonic variables available in our dataset. Significant coefficients may be symptomatic of the presence of pre-existing characteristics that simultaneously influence culture, institutions and economic performance. We regress the new dependent variables on the same covariates as in columns (1) and (5) of Table 3.[36] The results reported in Table 6 show that, reassuringly, the coefficient of the interaction term *Napoleon × Protestant Share* is never significant.

Difference-in-Differences Specification Our second strategy is to test our hypothesis using a different dataset, which allows us to implement a difference-in-differences specification. We use the data compiled by Acemoglu et al. (2011), which contains information on urbanization and religious affiliation for a panel of 19 independent German states (or provinces of larger states) for the years 1750, 1800, 1850, 1875 and 1900. Even though these data are available at a higher level of aggregation (19 states vs more than 440 counties), they allow us to test our hypothesis using within-region variation over time. Accordingly, our baseline specification is as follows:

$$urb\_rate_{it} = \mu_i + \alpha_1 Post_t + \alpha_2 Post_t \times Napoleon_i + \alpha_3 Post_t \times Prot_i$$
$$+ \alpha_4 Post_t \times Napoleon_i \times Prot_i + \varepsilon_{it} \tag{2}$$

---

[35]Specifically, two counties in the Electorate of Trier and one in the Electorate of Mainz were annexed by the Duchy of Nassau, which was a satellite state that did not adopt the Code. Moreover, one county in Nassau was given to the Grand Duchy of Berg. Two districts of the Electorate of Brandenburg to the west of the Elbe were annexed by the Kingdom of Westphalia, and one district in the Kingdom of Prussia was annexed by the Duchy of Warsaw, which all adopted the Napoleonic Code.

[36]To address the high number of zeros (84% of the observations) we estimate a Poisson regression in columns (1) and (2).

Table 6: Placebo Tests

	% Urban Pop in 1500		Hanseatic/Imperial city		University		Schools	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Napoleon	1.354	38.43*	0.0581	-3.150	0.0643**	2.954***	0.0721**	-1.039
	(0.968)	(21.30)	(0.0590)	(2.715)	(0.0327)	(1.028)	(0.0339)	(1.472)
Protestant Share	-2.653**	0.0728	-0.138**	-0.00178	0.0181	0.0266	-0.0647**	-0.00585
	(1.161)	(1.340)	(0.0688)	(0.103)	(0.0212)	(0.0461)	(0.0259)	(0.0444)
Napoleon × Protestant Share	-0.833	-0.00835	0.124	-0.0995	-0.0466	0.0549	0.0515	-0.0588
	(1.319)	(1.442)	(0.0868)	(0.128)	(0.0502)	(0.0668)	(0.0579)	(0.0691)
Geographic Controls	yes	yes	yes	yes	yes	yes	yes	yes
Historical Controls	no	yes	no	yes	no	yes	no	yes
Socioeconomic Controls	no	yes	no	yes	no	yes	no	yes
Education Controls	no	yes	no	yes	no	yes	no	yes
Hist & Geo Interactions	no	yes	no	yes	no	yes	no	yes
R^2	0.359	0.462	0.0878	0.182	0.0711	0.167	0.0654	0.133
Obs.	447	447	447	447	447	447	447	447

Notes: The dependent variable is in the column heading. Poisson regressions in columns (1)-(2). See also, the notes to Table 3. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

where urb_rate_{it} is the urbanization rate of state i in year t (the proxy for economic outcomes in this dataset), *Post* is a dummy variable that equals one in the years after the Napoleonic invasion (i.e., in the second half of the nineteenth century), *Napoleon* is a time-invariant dummy variable that captures French presence (treatment) in the state, and *Prot* is the share of Protestants around 1800 (constant over time).

The results are reported in Table 7. Column (1) shows the baseline estimates. The results are remarkably similar to those in Table 3: there is a negative interaction between French institutions and Protestant share after institutional transplant, and the positive effect of good institutions vanishes for a share of Protestants of approximately 85% (the median Protestant share in this sample is 80%). In column (2), we show that the results are robust to weighting the regression by the total population in 1750 as in Acemoglu et al. (2011). In column (3), we reach the same conclusion using a different measure of French institutions, i.e., the number of years of French presence. Finally, in column (4), we replace the *Post* indicator with a full set of year dummies. The coefficients become significant starting in 1875, i.e., 60 years after the Congress of Vienna.[37]

5.3.3 Measurement Error: Religious Persistence

Our baseline measure of cultural proximity is the Protestant share of the population in 1871 (the year of German unification). In this section, we use different measures of religious affiliation to study the effects of culture on institutions. Table 8 reports the results obtained using the same specification as in column (5) of Table 3.

First, to ensure that our results are not driven by minor changes in the Protestant share variable, we use counties' ranking in terms of the Protestant share (column 1) or a Protestant (absolute) majority dummy (column 2).

---

[37]Compared to Acemoglu et al. (2011), we find a positive effect of institutions on economic performance 25 years earlier.

Table 7: Difference-in-Differences Estimation

Dep. Var.: Urbanization Rate	Baseline (1)	Weighted (2)	Years of French (3)	Napoleon × Years (4)
After	9.925***	10.38***	11.64***	
	(2.442)	(2.325)	(2.949)	
Napoleon × After	21.78***	21.23***	23.29***	
	(7.467)	(4.889)	(6.842)	
After × Nap. × Prot. Share	-28.13***	-29.18***		
	(8.458)	(6.563)		
Years of French × After			1.454***	
			(0.272)	
Post 1850 × French Yrs. × Prot. Share			-2.233***	
			(0.751)	
Napoleon × 1750				10.49
				(12.01)
Napoleon × 1800				20.68
				(14.45)
Napoleon × 1850				19.38
				(12.50)
Napoleon × 1875				37.67**
				(16.93)
Napoleon × 1900				50.93**
				(20.67)
Prot. Share × Nap. × 1750				-12.46
				(14.67)
Prot. Share × Nap. × 1800				-24.65
				(16.57)
Prot. Share × Nap. × 1850				-24.65
				(15.06)
Prot. Share × Nap. × 1875				-47.80**
				(18.84)
Prot. Share × Nap. × 1900				-62.55**
				(21.95)
Number of id	19	19	19	19
R^2	0.506	0.530	0.503	0.878
Obs.	109	109	109	109

Notes: All regressions have territory fixed effects that subsume the linear effects of *Napoleon*, *Prot. Share* and *Napoleon × Prot. Share* (all of which are constant over time). Column (4) includes year fixed effects. All specifications include interactions between post (or year) and the Protestant share. The results are not reported for the sake of readability. Regressions are weighted by territories' total population in 1750. Robust standard errors clustered by territory.   *** p<0.01, ** p<0.05, * p<0.1

Log Average Wage of a Male Elementary School Teacher in 1886	(1)	(2)	(3)	(4)	(5)	(6)
Napoleon	-1.147	-0.0206	-0.0133	-0.606	0.133	-3.237***
	(0.810)	(0.774)	(0.822)	(0.673)	(0.737)	(1.063)
Prot. Share Rank	0.000697***					
	(0.000114)					
Nap × Prot. Sh. Rank	-0.000747***					
	(0.000110)					
Prot. Dummy		0.098***	0.117***			
		(0.022)	(0.026)			
Nap × Prot. Dummy		-0.134***	-0.149***			
		(0.028)	(0.032)			
Prot. Share 1816				0.165***		
				(0.028)		
Nap. × Prot. Share 1816				-0.159***		
				(0.032)		
Prot Dummy 1600					0.0770	
					(0.074)	
Nap. × Prot Dummy 1600					-0.140*	
					(0.075)	
Prot. Share (IV)						0.749***
						(0.186)
Nap. × Prot. Share (IV)						-0.601***
						(0.010)
R^2	0.690	0.665	0.666	0.859	0.665	0.467
Obs.	447	447	428	349	438	447

All specifications include *Geographic Controls, Historical Controls, Socioeconomic Controls, Education Controls* and *Hist & Geo Interactions*. See also, the notes to Table 3.
Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Second, we exclude counties with a Protestant share between 40–60%, since those areas are more likely to have switched from a Protestant to a Catholic majority, or vice versa (column 3). Note that the coefficients have the same sign as in the baseline specification, and they remain significant at conventional levels. Then, we compute the Protestant share using the first available wave of the Prussian census, which was conducted in 1816 immediately after the Congress of Vienna. While this variable is available only for the 349 counties that formed Prussia at that time, its correlation with the Protestant share in 1871 is extremely high (0.98).[38] Accordingly, our main results are unchanged (column 4).

Third, one may still worry that the persistence of religious affiliation after the Napoleonic wars is not sufficiently informative of the religious composition at the end of the eighteenth century given that war itself may have caused religious migration. To address this concern, we construct a historical measure of the Protestant majority in the seventeenth century using data from Cantoni (2012) and Spenkuch (2010). Although, it is defined at the kingdom (rather than at the county) level, and it is not available for the entire sample, the advantage of this variable is that it was measured two centuries before the arrival of Napoleon. The main evidence is unaffected even when using the historical Protestant share (column 5).

Finally, we follow the literature and instrument the Protestant share using the distance from Wittenberg. This should isolate exogenous variation in religious affiliation using the concentric diffusion of Protestantism through Prussia from its origins in Luther's city (column 6).[39] The

[38]Of the invaded territories, Prussia was given the Rhineland and the Duchy of Warsaw. All states that we consider satellites remained independent after the Congress of Vienna and were annexed by the Kingdom of Prussia only later.

[39]The *t*-statistic of the first stage is approximately 14.

results, reported in column 6, are qualitatively unchanged, and the coefficient of interest is even larger than that for the baseline estimate.

5.4 Intensity of the Institutional Treatment

In this section, we investigate how the moderating effect of cultural proximity changes with the length of exposure to the new institutions. To explore this question, in Table 9, we use three proxies that capture different facets of the intensity of the institutional treatment.

Table 9: Intensity of the Institutional Treatment

	Log Average Wage Male Elementary School Teacher in 1886								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Protestant Share	0.157***	0.142***	0.143***	0.187***	0.172***	0.197***	0.264***	0.279***	0.389***
	(0.0226)	(0.0324)	(0.0372)	(0.0253)	(0.0324)	(0.0392)	(0.0403)	(0.0472)	(0.0647)
French Empire	0.153***	0.172***	-0.393						
	(0.0305)	(0.0266)	(1.143)						
Empire × Prot. Sh.	-0.215*	-0.236***	-0.239***						
	(0.119)	(0.0870)	(0.0823)						
Satellite States	0.0904***	0.0904***	-1.305						
	(0.0264)	(0.0255)	(0.881)						
Sat. × Prot. Sh.	-0.0839**	-0.132***	-0.186***						
	(0.0406)	(0.0339)	(0.0426)						
French Years				0.0065***	0.0082***	-0.0340			
				(0.0016)	(0.0014)	(0.0623)			
Fr. Yrs. × Prot. Sh.				-0.0053	-0.0115***	-0.0121**			
				(0.0038)	(0.0038)	(0.0047)			
ACJR Index							0.0029***	0.0036***	-0.0629***
							(0.0006)	(0.0006)	(0.0212)
ACJR In. × Prot. Sh.							-0.003***	-0.0042***	-0.0065***
							(0.0008)	(0.0008)	(0.0012)
Geographic Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Historical Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Socioecon. Controls	no	yes	yes	no	yes	yes	no	yes	yes
Education Controls	no	yes	yes	no	yes	yes	no	yes	yes
Hist & Geo Interact.	no	no	yes	no	no	yes	no	no	yes
R^2	0.398	0.667	0.671	0.404	0.674	0.688	0.423	0.673	0.688
Obs.	447	447	447	447	447	447	431	431	431

*Notes: Empire × Prot. Sh. is the interaction between French Empire and Protestant Share. Sat. × Prot. Sh. is the interaction between Satellite States and Protestant Share. Fr. Yrs. × Prot. Sh. is the interaction between French Years and Protestant Share. ACJR In. × Prot. Sh. is the interaction between ACJR Index and Protestant Share. See also, the notes to Table 3. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

First, we differentiate between territories that were annexed by the French Empire and those belonging to the Confederation of the Rhine (i.e., satellite states). In the former group, the effects of treatment might be stronger, both because the Code was imposed and implemented and because the administrative structure and local governors were replaced with French ones. Indeed, columns (1)-(3) show that the linear effects of institutions on economic outcomes are significantly stronger in the annexed areas than in the satellite states, suggesting that a more intense institutional treatment had a stronger impact on long-term economic performance. At the same time, the coefficients on the interaction terms remain negative and significant. The point estimates show that, irrespective of the intensity of the exposure, the impact of good institutions vanishes in highly Protestant areas.

In the remaining columns, we use the number of years of French domination (columns 4-6) and an index (columns 7-9) that summarizes four different reforms: the enactment of the French Civil Code, the restructuring of agricultural relations, the abolition of guilds and the

abolition of serfdom.[40] Both measures show that more intense exposure to French institutions had a stronger impact on economic performance in culturally similar areas but not elsewhere.

6 Competing Explanations

This section investigates alternative explanations for our findings. We focus on factors, other than culture, that may interact with the new institutions, thereby affecting long-run economic outcomes. The results are reported in Table 10.

State Capacity Dittmar and Meisenzahl (2016) provide evidence on the links among Protestantism, enhanced state capacity and growth. In particular, they show that the 103 reformed cities that boosted public good provision in the sixteenth century by adopting city-level laws, called church ordinances (*kirchnordnung*), experienced significantly higher population growth in the long run. The coefficient on the interaction between cultural proximity and institutions may be explained by the fact that French institutions were ineffective in Protestant counties with high levels of public good provision, since they already had good institutions. This could be true if the pre-existing and new institutions are substitutes. In order to rule out this possibility, we collect data on the *kirchnordnung*, following Dittmar and Meisenzahl (2016). Specifically, we identify 45 cities in our sample that adopted this particular legal institution in the sixteenth century, and we construct a dummy variable (*Church Ordinances*) that equals one if at least one city in the county promulgated a *kirchnordnung*.[41] The results reported in column (1) confirm the hypothesis that counties with institutionalized public good provision were on a higher growth path, as the coefficient on *Church Ordinances* is positive and significant. Importantly, however, the results also show that the interaction between the measure of state capacity and French institutions has a small and insignificant coefficient. Accordingly, including this interaction in the main specification does not alter our results.

Institutional Proximity Our results might be induced by institutional rather than cultural proximity. During the eighteenth century, some rulers, perhaps inspired by Enlightenment principles, enacted reforms in their states to promote literacy and simplify justice and administration (See Arvind and Stirton, 2010). It is possible that by the time Napoleon arrived the local population was already used to a modern legal framework in places where these early reforms were implemented and were, hence, more likely to accept Napoleonic institutions. In order to disentangle the contribution of *institutional similarity* from that of cultural commonality in moderating the economic effect of the transplant, we construct a measure of historical institutional proximity. In particular, we collect data on progressive reforms of the educational, judicial or administrative systems implemented in each state between 1701 and 1790. We classify rulers who implemented at least one modernizing reform as *Reformists*, and we create an index based on the fraction of years the progressive king was in power. For example, Frederick the Great implemented innovative educational reforms and held power in Brandenburg for 46 years; hence, his contribution to the Brandenburg *institutional-proximity* index is 0.51 (i.e., 46 over 90 years). The coefficient on the interaction term in column (2) shows that the similarities of Napoleonic institutions and pre-existing ones positively affect the success of the transplant. However, our coefficient of interest remains negative and highly significant, albeit slightly smaller in magnitude, confirming that cultural traits – in particular, cultural proximity – play a role beyond pre-existing institutional characteristics.

---

[40]Both these measures are available for 19 German pre-unitary independent states or provinces of larger states, and we complemented the index with territories not considered by Acemoglu et al. (2011), considering 33 entities.

[41]The geographical area we examine does not perfectly overlap with that analyzed by Dittmar and Meisenzahl (2016). Approximately one-half of the cities they consider are included in our sample.

Legitimacy of the Pre-Napoleonic Ruler The intricate web of family ties characterizing the European aristocracy, coupled with complex succession laws, implied that the same prince frequently ruled several – occasionally non-contiguous – kingdoms. This implied prolonged absences that could erode the ruler's legitimacy and, in turn, entail varying receptions of new institutions. In addition to the inclusion of kingdom fixed effects in Table 5, we explore this possible alternative explanation by constructing a dummy variable, *Peripheral Ruler*, which identifies 21 peripheral kingdoms (277 counties) with respect to the ruler's main residence. For example, Charles Theodore (1724-1799) was Prince Elector of Bavaria, where he maintained his main residence, but he also ruled the Electorate Palatinate and Duchy of Julich and Berg. For those three territories, the *Peripheral Ruler* dummy takes the value 1 in our sample. Reassuringly, column (3) confirms that our results are not affected by the physical presence of the ruler.

Education Policies Among other reforms, Napoleon restructured the educational system. His main objective was to breed well-prepared military and administrative elites, and interventions principally targeted higher education, leaving primary schooling in the hands of Catholic religious institutions and old local-community schools.[42] A fruitful interaction could have arisen where an already-developed primary education system was combined with innovative Napoleonic educational policies. In order to test this potential channel, we use information on the presence of schools and monasteries – the most prominent primary educational centers at the time – in 1517. We define a dummy variable that takes the value 1 if a school or a monastery was present in the county and interact this variable with our institutional measure. In order to control for pre-existing hubs of higher education, we include a dummy variable identifying the presence of universities in the county before the Napoleonic invasions. The results in column (4) show that the presence of universities is relevant in moderating the effect of French institutions on long-run economic outcomes, whereas no synergy arises with centers of primary education. The addition of these controls does not affect the sign, magnitude or significance of the coefficient of interest.

Previous French Invasion Central Europe was plagued by continuous conflict following the creation of the Holy Roman Empire, and some areas in our sample had been repeatedly invaded by France before the Napoleonic wars. On the one hand, this could have forged a historical collective memory identifying France as *the* traditional enemy and Bonaparte as the villain insofar as he was the legitimate successor of the French kings. The rejection of French institutions could thus be driven by animosity originating from previous invasions rather than from cultural distance.[43] On the other hand, protracted occupations could have improved the reception of new institutions, as they imply interactions with the French military and cultural exchanges with the local population. We construct a dummy variable that equals one if the area was occupied by French troops after the Peace of Westphalia (1648). The results suggest that previous French occupations positively interact with Napoleonic institutions, supporting the hypothesis that military occupations involve cultural exchange. In fact, this result is in line with our theory to the extent that interactions with French people (the army, in this case) foster cultural assimilation. However, our main message is not affected by the introduction of this control variable, and the coefficient on *Napoleon × Protestant Share* remains negative and significant.

---

[42]According to Ellis (2003) ch. 3, Napoleon paid very little attention to primary education – especially for girls – while promoting technical training and higher education by establishing polytechnics, conservatories of art and trades, and lycées.

[43]We did not find any anecdotal evidence that conflicts were harsher in Protestant areas than in Catholic ones. Rather, French invasions often involved the Catholic lands of the Rhenish area. For example, Trier was besieged and occupied by French troops three times between 1632 and 1675, and in 1673, the French military destroyed all its churches and abbeys.

Table 10: Competing Explanations

Log Average Wage of a Male Elementary School Teacher in 1886	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Napoleon	-1.205	-1.989**	-1.345	-1.265	-1.741	-1.196	-1.203	-1.003	-0.876
	(0.988)	(0.873)	(1.073)	(0.945)	(1.138)	(0.981)	(0.979)	(1.029)	(0.872)
Protestant Share	0.195***	0.170***	0.216***	0.196***	0.203***	0.195***	0.185***	0.174***	0.150***
	(0.0555)	(0.0586)	(0.0551)	(0.0556)	(0.0555)	(0.0560)	(0.0513)	(0.0539)	(0.0481)
Nap. × Prot. Share	-0.211***	-0.160***	-0.248***	-0.213***	-0.233***	-0.214***	-0.201***	-0.188***	-0.115*
	(0.0630)	(0.0548)	(0.0711)	(0.0590)	(0.0616)	(0.0607)	(0.0512)	(0.0544)	(0.0585)
Church Ordinances	0.0342**								0.0486***
	(0.0160)								(0.0152)
Nap. × Ch. Ordinances	-0.0286								-0.0185
	(0.0297)								(0.0289)
Institutional Proximity		-0.321***							-0.471***
		(0.0695)							(0.0506)
Nap. × Inst. Prox.		0.372***							0.515***
		(0.0681)							(0.0488)
Peripheral Ruler			0.0739						-0.0678
			(0.0505)						(0.0616)
Nap. × Periph. Ruler			-0.0434						0.126*
			(0.0664)						(0.0619)
Universities				-0.0527**					-0.0611***
				(0.0258)					(0.0202)
Nap. × Uni.				0.0757**					0.0776**
				(0.0337)					(0.0369)
Monasteries or Schools				0.123*					-0.00395
				(0.0710)					(0.0298)
Nap. × Monasteries/Schools				-0.0971					0.0177
				(0.0715)					(0.0355)
Previous French Presence					-0.0861*				-0.331***
					(0.0476)				(0.0749)
Nap. × Fr. Presence					0.114*				0.363***
					(0.0582)				(0.0746)
Battles						-0.0152			0.0191
						(0.0357)			(0.0327)
Nap. × Battles						0.0281			0.0243
						(0.0462)			(0.0320)
Religious Fragmentation							0.0664		-0.0826
							(0.0783)		(0.0562)
Nap. × Rel. Frag.							-0.0454		0.106
							(0.0924)		(0.0757)
Rel. Dist. from Neighbors								-0.0866	0.0498**
								(0.0920)	(0.0233)
Nap. × Rel. Dist.								0.0821	-0.122*
								(0.125)	(0.0642)
R^2	0.674	0.726	0.673	0.677	0.671	0.673	0.674	0.674	0.752
Obs.	447	447	429	447	429	447	447	431	413

Notes: All specifications include *Geographic Controls*, *Historical Controls*, *Socioeconomic Controls*, *Education Controls* and *Hist & Geo Interactions*. See also, the notes to Table 3. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Severity of Napoleonic Conflict An essential characteristic of the institutional transfer we examine is that it was forceful, often achieved through conquest, and carried out in one of the major theaters of the Napoleonic wars. If destruction from war has a persistent economic effect (longer than the 70-year period over which our dependent variable is constructed) and if harsher conflicts occurred in invaded Protestant areas, then our results may be contaminated by the severity of Napoleonic conflict. To control for this potential confounding factor, we collect data on all major battles during the Napoleonic military campaigns (1796-1815) and create a dummy variable that equals one if there was a relevant battle in the county.[44] The results are robust to the inclusion of this additional control variable (column 6).

Religious Fragmentation A low Protestant share might reflect a high level of religious fractionalization. Several papers have investigated the costs and benefits of diversity, whether racial, ethnic, religious, or linguistic (e.g., Alesina and La Ferrara, 2005). On the one hand, fragmented societies are more prone to poor policy management and pose more political economic challenges than do homogeneous societies. Accordingly, in counties characterized by high fragmentation, the functioning of French institutions could be impaired. On the other hand, a diverse cultural or ethnic mix may provide a variety of abilities and experiences that boost productivity through innovation and creativity. In this case, a highly fractionalized county may be a better recipient of French institutions. To test this alternative explanation, we construct and include in the baseline specification a Herfindahl Index using the shares of the three largest religious groups (Protestants, Catholics and Jews) and use these measures and their interactions with the Napoleonic dummy. The coefficients of these new controls are not statistically significant, and our main results are unchanged.

Religious Divergence The western part of Prussia includes few Protestant counties surrounded by Catholic ones. Being surrounded might have put such Protestant areas at an economic and political disadvantage. To control for this possibility, we create a *Religious Distance* variable, which is the difference between the Protestant share of the county population and the average Protestant share of the neighboring counties. Column (8) shows that controlling for religious difference from the surrounding counties does not affect the negative interaction term between Napoleonic institutions and Protestant share.

Summary Finally, in column (9), we implement a horse-race model, including all controls for the alternative explanations. Our main result survives this demanding exercise. These findings confirm that, despite complex interactions among institutions and historical and socioeconomic factors, cultural similarity does play a role in institutional transplants and long-term economic outcomes.

7   Additional Dimensions of Cultural Proximity

We devote this section to investigating whether there are other cultural dimensions, not embedded in religious affiliation, that promote the reception of Napoleonic institutions. To this end, we construct alternative measures of cultural similarity, which we then add to religious affiliation in our main specification. One factor that may be relevant to the success of transplanted institutions is exposure to French culture; indeed, frequent interactions with French people and opportunities to experience their attitudes and understand their mindsets may trigger transfers of knowledge, values, and ideas (*horizontal cultural transmission*), thereby encouraging cultural

---

[44]We define major battles as those with at least 1,000 deaths. Of a total of 23 battles, only 5 occurred in the counties included in our sample: Dennewitz in 1813 (approximately 30,000 casualties); Friedland in 1807 (30,000); Heilsberg in 1807 (7,400); Lutzen in 1813 (31,000) and Eylau in 1807 (40,000).

similarity. Hence, we expect that populations that had been previously exposed to French culture were better recipients of the new institutions.

Two levels of such exposure may be relevant: the local population and the ruling class. The top-down imposition of a norm requires private cooperation to be successful. The more familiar the local population is with French customs, the more private cooperation there is likely to be. Similarly, the adoption of institutions crucially hinges on the mediation of the ruling class, whose inclinations and attitudes toward the new rules could facilitate their implementation.

We thus construct two measures that summarize these aspects of cultural similarity with France: *French Exposure* and *Pro-French Ruler*. To build the former variable, we consider the presence of books published in French after the diffusion of the printing press in the sixteenth century.[45] We collect these data from the Universal Short Title Catalogue, a comprehensive database of all books published in Europe between the introduction of the printing press and the end of the sixteenth century.[46] The underlying idea is that the existence of manuscripts in French should be associated with the presence of French natives or, at least, a French-speaking population. Second, we investigate whether the kingdoms received Huguenot migrants during the seventeenth century, exploiting county-level data collected by Hornung (2014) and complemented by Poole (1880). Many Huguenots left France after Louis XIV revoked the Edict of Nantes in 1685, and the majority of them migrated to neighboring Protestant countries. Some sovereigns even competed to attract these skilled French immigrants by offering them special privileges.[47] The typical Huguenot immigrant was a hard-working, urban, middle-class man who, while preserving his own traditions and identity, quickly integrated into the local community. It is likely that fruitful interaction with the local population generated a favorable image of these Frenchmen and a positive inclination toward French culture. We construct a dummy variable, *French Exposure*, that equals one if a book in French language was printed in that county, or if the county registered the presence of Huguenot colonies. We expect the transplant to be more effective if the local population had previous exposure to French culture.

The second variable we construct, *Pro-French Ruler*, captures the existence of a positive attitude of eighteenth-century rulers toward both Enlightenment ideals and French culture. Using essays, biographies and books, we investigate whether local rulers during the period 1701-1790 (i) had a direct French relative (mother, father, spouse) and, thus, an explicit link to the French aristocracy; (ii) displayed a positive disposition toward the customs and traditions of the French court;[48] (iii) embraced French Enlightenment ideals;[49] (iv) had a long-standing relationship with the French Royal House.[50] The *Pro-French ruler* dummy equals 1 if at least one of the above conditions is satisfied. This variable summarizes the rulers' disposition toward this foreign culture. We expect rulers with a more favorable attitude toward France to better accept and enforce the transplanted institutions.

These two variables, which are essentially orthogonal (their correlation is 0.052), are added to our baseline specification. Table 11 presents the results. In columns (1) and (3), we include

---

[45]See Dittmar (2011) and Rubin (2014) for more information on the diffusion of the printing press and its economic impact on European Cities in the sixteenth century.

[46]We identify 19 German cities where French books were published. We are able to map 8 of them onto our sample because of geographical coverage. For example, Mainz, the city where the printing press was invented, is not in our sample because it was part of the Grand Duchy of Hessen in the second half of the nineteenth century.

[47]A prominent example is the Electorate of Brandenburg, which with the Edict of Potsdam, granted Huguenots tax-free status for ten years and allowed them to hold church services in their native language.

[48]For instance, the Landgrave of Hesse-Kassel, Frederick II, was strongly attracted to French culture and art. During his reign, French influence could be detected not only in architecture, opera and theater performances but also in the choice of French as the language of his inner administration and the local elites.

[49]For instance, the Elector Palatine, Charles Theodore, had assiduous correspondence with Voltaire.

[50]For instance, William Henry, Prince of Nassau-Saarbrucken, often traveled to Paris where he even received military honors.

Table 11: Ruler and Population French Exposure as Measures of Cultural Similarity

Log Average Wage of a Male Elementary School Teacher in 1886	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Napoleon	0.495	-1.218	-0.788	-2.247**	-0.763	-0.112	-2.230**	-1.425
	(0.723)	(0.938)	(0.772)	(0.840)	(0.777)	(0.909)	(0.830)	(0.879)
Protestant Share	0.0478	0.196***	0.0436	0.161***	0.0432	0.0622*	0.162***	0.155***
	(0.0379)	(0.0540)	(0.0351)	(0.0533)	(0.0348)	(0.0313)	(0.0525)	(0.0464)
Napoleon × Protestant Share		-0.221***		-0.178***			-0.181***	-0.145**
		(0.0593)		(0.0505)			(0.0495)	(0.0560)
French Exposure	-0.00311	-0.0259*			0.00809	-0.0455***	-0.00768	-0.0387**
	(0.0152)	(0.0144)			(0.0159)	(0.0162)	(0.0128)	(0.0152)
Napoleon × French Exposure	0.0601**	0.0873***			0.0362	0.0766**	0.0548**	0.0701**
	(0.0281)	(0.0300)			(0.0254)	(0.0319)	(0.0267)	(0.0306)
Pro-French Ruler			-0.348***	-0.314***	-0.346***	-0.418***	-0.311***	-0.384***
			(0.0315)	(0.0283)	(0.0314)	(0.0358)	(0.0278)	(0.0416)
Napoleon × Pro-French Ruler			0.400***	0.384***	0.396***	0.481***	0.379***	0.459***
			(0.0347)	(0.0239)	(0.0345)	(0.0336)	(0.0232)	(0.0345)
Competing Explanations	no	no	no	no	no	yes	no	yes
R^2	0.656	0.679	0.737	0.751	0.740	0.758	0.755	0.765
Obs.	447	447	447	447	447	413	447	413

Notes: All the specifications include *Geographic Controls*, *Historical Controls*, *Socioeconomic Controls*, *Education Controls* and *Hist & Geo Interactions*. See also, the notes to Table 3. Standard errors clustered at the pre-Napoleonic-kingdom level in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

the two proxies for exposure to French culture and their interactions with the institutional dummy. In columns (2) and (4), we further include the *Napoleon × Protestant Share* interaction. The results show that institutional transfer has stronger effects on economic outcomes in counties with previous exposure to French culture. We next include the two measures together (columns 5 and 7) and control for all competing explanation discussed in the previous section (columns 6 and 8). All our measures of cultural similarity are significant, retain the expected sign and, notably, survive the inclusion of a battery of controls for alternative explanations. Interestingly, the cultural similarity measures seem to capture different facets of the concept, as implied by the remarkable stability of the coefficients of interest.

8 Conclusion

In this work, we measure how the economic impact of transplanted institutions depends on cultural proximity between the exporting and the receiving countries. Our historical context is well suited to exploiting both the quasi-natural experiment generated by the Napoleonic military campaign and cultural heterogeneity across Prussian counties. We present evidence that cultural proximity, measured as either religious affiliation or previous exposure to French culture, interacts with the adoption of transplanted institutions, thereby generating positive long-term economic outcomes.

We are able to combine novel data on pre-Napoleonic kingdoms with waves of the Prussian census to conduct a comparative analysis of the economic impact of institutional transplants across areas characterized by different degrees of cultural proximity. Our results suggest that the new institutions had a differential effect on economic performance of approximately 13% when comparing counties in the first and fourth quartiles of the share of Protestants in the population. Overall, our findings are best explained by cultural proximity facilitating the adoption and implementation of new institutions through a mix of a better disposition toward the French

exporter and the content of the institutions in the receiving communities and of a friendlier approach of the exporter in imposing the same formal institutions in culturally similar areas.

Although we analyze a very specific historical environment and extrapolation to other contexts might be hazardous, our findings call for care before deciding to export seemingly good institutions. The transplant may fail if it conflicts with local culture, and the population may be reluctant to accept institutional innovations that are perceived as alien or that are imposed by a culturally distant entity.

References

Acemoglu, D., Cantoni, D., Johnson, S., and Robinson, J. A. (2011). The Consequences of Radical Reform: The French Revolution. *The American Economic Review*, 101(7):3286–3307.

Acemoglu, D., Cantoni, D., Johnson, S., and Robinson, J. A. (2012). From Ancien Regime to Capitalism: The Spread of the French Revolution as a Natural Experiment. In *Natural Experiments of History*. Harvard University Press.

Acemoglu, D. and Jackson, M. O. (2016). Social Norms and the Enforcement of Laws. *Journal of the European Economic Association*, forthcoming.

Acemoglu, D., Johnson, S., and Robinson, J. A. (2001). The Colonial Origins of Comparative Development: An Empirical Investigation. *American Economic Review*, 91(5):1369–1401.

Acemoglu, D., Johnson, S., and Robinson, J. A. (2002). Reversal of Fortune: Geography and Institutions in the Making of the Modern World Income Distribution. *The Quarterly Journal of Economics*, 117(4):1231–1294.

Alesina, A. and Giuliano, P. (2010). The power of the family. *Journal of Economic Growth*, 15(2):93–125.

Alesina, A. and Giuliano, P. (2015). Culture and Institutions. *Journal of Economic Literature*, 53(4):898–944.

Alesina, A., Giuliano, P., and Nunn, N. (2013). On the Origins of Gender Roles: Women and the Plough. *Quarterly Journal of Economics*, 128(2):469–530.

Alesina, A. and La Ferrara, E. (2005). Preferences for redistribution in the land of opportunities. *Journal of Public Economics*, 89(5-6):897–931.

Algan, Y. and Cahuc, P. (2010). Inherited Trust and Growth. *American Economic Review*, 100(5):2060–92.

Allen, R. C. (2001). The Great Divergence in European Wages and Prices from the Middle Ages to the First World War. *Explorations in Economic History*, 38(4):411–447.

Angrist, J. and Pischke, J. S. (2009). *Mostly Harmless Econometrics*. Princeton: Princeton University Press.

Arvind, T. and Stirton, L. (2010). Explaining the reception of the Code Napoleon in Germany: a fuzzy-set qualitative comparative analysis. *Legal Studies*, 30(1):1–29.

Barro, R. J. (2001). Human Capital and Growth. *American Economic Review*, 91(2):12–17.

Becker, S. O., Cinnirella, F., Hornung, E., and Woessmann, L. (2014). iPEHD–The ifo Prussian Economic History Database. *Historical Methods: A Journal of Quantitative and Interdisciplinary History*, 47(2):57–66.

Becker, S. O. and Woessmann, L. (2009). Was Weber Wrong? A Human Capital Theory of Protestant Economic History. *The Quarterly Journal of Economics*, 124(2):531–596.

Berkowitz, D., Pistor, K., and Richard, J.-F. (2003a). Economic development, legality, and the transplant effect. *European Economic Review*, 47(1):165–195.

Berkowitz, D., Pistor, K., and Richard, J.-F. (2003b). The Transplant Effect. *The American Journal of Comparative Law*, 51(1):163–203.

Bisin, A. and Verdier, T. (2000). "Beyond The Melting Pot": Cultural Transmission, Marriage, And The Evolution Of Ethnic And Religious Traits. *Quarterly Journal of Economics*, pages 955–988.

Bisin, A. and Verdier, T. (2015). On the joint evolution of culture and institutions. *Mimeo*.

Botticini, M. and Eckstein, Z. (2005). Jewish Occupational Selection: Education, Restrictions, or Minorities? *The Journal of Economic History*, 65(04):922–948.

Botticini, M. and Eckstein, Z. (2012). *The Chosen Few: How Education Shaped Jewish History, 70-1492*. Princeton University Press, Princeton, NJ.

Buggle, J. C. (2016). Law and Social Capital: Evidence from the Code Napoleon in Germany.

European Economic Review, 87:148–175.

Cantoni, D. (2012). Adopting a New Religion: the Case of Protestantism in 16th Century Germany. *The Economic Journal*, 122(560):502–531.

Cantoni, D. (2015). The Economic Effects of the Protestant Reformation: Testing the Weber Hypothesis in the German Lands. *Journal of the European Economic Association*, 13(4):561–598.

Dal Bo, P., Foster, A., and Putterman, L. (2010). Institutions and Behavior: Experimental Evidence on the Effects of Democracy. *The American Economic Review*, 100(5):2205–29.

De Long, B. J. and Shleifer, A. (1993). Princes and Merchants. *Journal of Law and Economics*, 36.

Diefendorf, J. M. (1975). *Businessmen and Politics in the Rhineland, 1789-1834, Diss.* Berkeley.

Dittmar, J. and Meisenzahl, R. (2016). State capacity and public goods: Institutional change, human capital, and growth in early modern germany. Mimeo.

Dittmar, J. E. (2011). Information technology and economic change: the impact of the printing press. *The Quarterly Journal of Economics*, 126(3):1133–1172.

Doepke, M. and Zilibotti, F. (2008). Occupational Choice and the Spirit of Capitalism. *The Quarterly Journal of Economics*, 123(2):747–793.

Ellis, G. (2003). *The Napoleonic Empire*. Palgrave Macmillan.

Falck, O., Heblich, S., Lameli, A., and Sudekum, J. (2012). Dialects, cultural identity, and economic exchange. *Journal of Urban Economics*, 72(2–3):225–239.

Fernandez, R. (2011). Does Culture Matter? In *Handbook of Social Economics*, volume 1A. J. Benhabib, M. O. Jackson and A. Bisin (Eds.), North Holland.

Fernandez, R., Fogli, A., and Olivetti, C. (2004). Mothers and Sons: Preference Formation and Female Labor Force Dynamics. *The Quarterly Journal of Economics*, 119(4):1249–1299.

Fisher, H. A. L. (1903). *Studies in Napoleonic Statesmanship: Germany*. Clarendon Press.

Fisman, R., Paravisini, D., and Vig, V. (2017). Cultural proximity and loan outcomes. *American Economic Review*, 107(2):457–492.

Galloway, P. R., Lee, R. D., and Hammel, E. A. (1994). Fertility decline in Prussia: Estimating influences on supply, demand, and degree of control. *Demography*, 31(2):347–373.

Galor, O. and Moav, O. (2002). Natural selection and the origin of economic growth. *Quarterly Journal of Economics*, pages 1133–1191.

Galor, O., Ozak, O., and Sarid, A. (2016). Geographical origins and economic consequences of language structures. *Mimeo*.

Gennaioli, N., La Porta, R., De Silanes, F. L., and Shleifer, A. (2014). Growth in regions. *Journal of Economic Growth*, 19(3):259–309.

Giavazzi, F., Petkov, I., and Schiantarelli, F. (2014). Culture: Persistence and evolution. NBER Working Paper w20174, National Bureau of Economic Research.

Glaeser, E. L. and Shleifer, A. (2002). Legal origins. *Quarterly Journal of Economics*, pages 1193–1229.

Greif, A. (1993). Contract enforceability and economic institutions in early trade: The Maghribi traders' coalition. *The American Economic Review*, pages 525–548.

Guiso, L., Sapienza, P., and Zingales, L. (2008). Long term persistence. NBER Working Paper w14278, National Bureau of Economic Research.

Guiso, L., Sapienza, P., and Zingales, L. (2009). Cultural biases in economic exchange? *The Quarterly Journal of Economics*, 124(3):1095–1131.

Harutyunyan, A. and Ozak, O. (2016). Culture, Diffusion, and Economic Development. MPRA Paper 70502, University Library of Munich, Germany.

Hornung, E. (2014). Immigration and the diffusion of technology: The Huguenot diaspora in Prussia. *The American Economic Review*, 104(1):84–122.

Kisch, H. (1989). *From Domestic Manufacture to Industrial Revolution: The Case of the Rhineland Textile Districts: The Case of the Rhineland Textile Districts*. Oxford University Press, New York.

La Porta, R., Lopez-de Silanes, F., and Shleifer, A. (2008). The Economic Consequences of Legal Origins. *Journal of Economic Literature*, 46(2):285–332.

Lefebvre, G. (1969). *Napoleon*. Columbia University Press, New York.

Lobingier, C. S. (1918). Napoleon and His Code. *Harvard Law Review*, 32(2):114–134.

Mailath, G. J., Morris, S., and Postlewaite, A. (2016). Laws and authority. *Mimeo*.

Nannicini, T., Stella, A., Tabellini, G., and Troiano, U. (2013). Social capital and political accountability. *American Economic Journal: Economic Policy*, 5(2):222–250.

North, D. C. (1990). *Institutions, institutional change and economic performance*. Cambridge university press, Cambridge.

Nunn, N. and Wantchekon, L. (2011). The Slave Trade and the Origins of Mistrust in Africa. *The American Economic Review*, 101(7):3221–52.

Oster, E. (2017). Unobservable selection and coefficient stability: Theory and validation. *Journal of Business Economics and Statistics*, forthcoming.

Parsons, T. (2010). *The rule of empires: those who built them, those who endured them, and why they always fall*. Oxford University Press, Oxford.

Poole, R. L. (1880). *A History of the Huguenots of the Dispersion at the Recall of the Edica of Nantes*. Macmillan and Company, London.

Rowe, M. (2000). The Napoleonic legacy in the Rhineland and the politics of reform in Restoration Prussia. *Napoleon's Legacy: Problems of Government in Restoration Europe*, pages 129–143.

Rowe, M. (2003). *From Reich to state: the Rhineland in the revolutionary age, 1780-1830*. Cambridge University Press, Cambridge.

Rubin, J. (2014). Printing and protestants: An empirical test of the role of printing in the reformation. *The Review of Economics and Statistics*, 96(2):270–286.

Schleusen, K. A. (1989). *Schooling and Society: The Politics of Education in Prussia and Bavaria 1750-1900*. Berg Publishers, London.

Smets, J. (1998). Le Rhin, frontiere naturelle de la France. *Annales historiques de la Revolution française*, 314(1):675–698.

Smithers, W. W. (1901). The Code Napoleon. *The American Law Register*, 49.

Spenkuch, J. L. (2010). The Protestant Ethic and Work: Micro Evidence from Contemporary Germany. MPRA Paper 26444, University Library of Munich, Germany.

Spolaore, E. and Wacziarg, R. (2009). The diffusion of development. *The Quarterly Journal of Economics*, 124(2):469–529.

Stetson, D. M. (1987). *Women's rights in France*. Greenwood Press, Westport, Connecticut.

Tabellini, G. (2008). The Scope of Cooperation: Values and Incentives. *The Quarterly Journal of Economics*, 123(3):905–950.

Tabellini, G. (2010). Culture and Institutions: Economic Development in the Regions of Europe. *Journal of the European Economic Association*, 8(4):677–716.

Woolf, S. (2002). *Napoleon's integration of Europe*. Routledge.

Appendix A - Additional Tables

Table A1: Summary Statistics (by Counties with/without French Institutions)

Variable	Mean	Std. Dev.	Min.	Max.	N
PANEL: NO FRENCH INSTITUTIONS					
Income of male elem. school teachers (1886)	934.67	177.97	722.22	1954.19	211
Protestant Share	762	0.318	0.016	0.999	211
French Exposure	0.076	0.265	0	1	211
Pro-French Ruler	0.896	0.301	0	1	211
Institutional Proximity	0.681	0.280	0	.84	211
% of county population in urban areas	0.254	0.191	0	1	211
% females	0.515	0.012	0.467	0.541	211
% age below 10	0.248	0.025	0.158	0.297	211
Total Population (log)	10.88	0.392	9.768	13.625	211
County Area (log)	11.025	0.961	5.989	12.899	211
Universities Holy Roman Empire	0.033	0.179	0	1	211
Hanseatic or Imperial City	0.057	0.232	0	1	211
PANEL: FRENCH INSTITUTIONS					
Income of male elem. school teachers (1886)	1026.437	211.25	711.96	1838.764	236
Protestant Share	0.539	0.394	0.003	0.998	236
French Exposure	0.331	0.471	0	1	236
Pro-French Ruler	0.640	0.481	0	1	236
Institutional Proximity	0.279	0.316	0	1	236
% of county population in urban areas	0.295	0.241	0	1	236
% females	0.506	0.016	0.44	0.546	236
% age below 10	0.246	0.025	0.153	0.299	236
Total Population (log)	10.74	0.42	9.36	11.91	236
County Area (log)	10.59	1.261	5.313	12.955	236
Universities Holy Roman Empire	0.084	0.279	0	1	236
Hanseatic or Imperial City	0.136	0.343	0	1	236

Table A2: Institution and Religious Affiliation (Protestant Share Squared)

Log average wage male elementary teacher 1886	(1)	(2)	(3)	(4)	(5)
Napoleon	0.122***	0.116***	0.140***	0.123***	-0.714
	(0.0235)	(0.0228)	(0.0259)	(0.0226)	(0.840)
Protestant Share	0.278***	0.259***	0.0974	0.0843	0.121*
	(0.101)	(0.0977)	(0.0796)	(0.0698)	(0.0718)
Protestant Share Squared	-0.0937	-0.0637	0.0750	0.0839	0.0702
	(0.0918)	(0.0889)	(0.0705)	(0.0692)	(0.0748)
Napoleon × Protestant Share	-0.112***	-0.121***	-0.188***	-0.170***	-0.214***
	(0.0349)	(0.0339)	(0.0320)	(0.0295)	(0.0390)
Geographic Controls	yes	yes	yes	yes	yes
Historical Controls	no	yes	yes	yes	yes
Socio-Economic Controls	no	no	yes	yes	yes
Education Controls	no	no	no	yes	yes
Hist & Geo Interactions	no	no	no	no	yes
R^2	0.373	0.400	0.660	0.668	0.676
Obs.	447	447	447	447	447

Notes: All the specifications include *Geographic Controls*, *Historical Controls*, *Socio-Economic Controls*, *Education Controls* and *Hist & Geo Interactions*. See also notes to Table 3. Robust standard errors in parenthesis.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A3: Robustness Checks - Dependent Variable

	Wage Elem. Teacher (Level)			Log Wage Urb. Male Lab. 1892			Income Tax revenue p.c		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Napoleon	105.1***	128.5***	-796.7	0.198***	0.255***	0.510	0.294**	0.320**	-5.909
	(22.92)	(23.90)	(982.1)	(0.0332)	(0.0347)	(0.926)	(0.115)	(0.125)	(3.803)
Protestant Share	173.0***	171.8***	195.5***	0.113***	0.109***	0.105**	0.602***	0.0238	0.201
	(27.53)	(38.98)	(48.92)	(0.0371)	(0.0411)	(0.0440)	(0.119)	(0.150)	(0.151)
Nap. $\times$ Prot. Share	-103.8***	-175.5***	-219.7***	-0.107**	-0.245***	-0.268***	-0.327**	-0.477***	-0.858***
	(36.48)	(32.90)	(45.95)	(0.0440)	(0.0423)	(0.0524)	(0.165)	(0.162)	(0.208)
Geographic Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Historical Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Socio-Econ. Controls	no	yes	yes	no	yes	yes	no	yes	yes
Education Controls	no	yes	yes	no	yes	yes	no	yes	yes
Hist & Geo Interact.	no	no	yes	no	no	yes	no	no	yes
R^2	0.393	0.663	0.671	0.464	0.676	0.695	0.0880	0.353	0.384
Obs.	447	447	447	430	430	430	421	421	421

Notes: Robust standard errors in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$