DISCUSSION PAPER SERIES

DP16212

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DEVELOPMENT ECONOMICS

ECONOMIC HISTORY

FINANCIAL ECONOMICS

MACROECONOMICS AND GROWTH

CEPR

ISSN 0265-8003

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Discussion Paper DP16212
Published 02 June 2021
Submitted 31 May 2021

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www.cepr.org

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JEL Classification: O31, O43, N13, N43, K40

Keywords: Innovation, patents, institutions, Economic Growth

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*We thank Bo Bian, Maximilian Eber, Umit Gurun, Isaac Hacamo, Walker Hanlon, Giampaolo Lecce, Maria-Teresa Marchica, Vikas Mehrotra, Joel Mokyr, Elias Papaioannou, Dimitris Papanikolaou, Sahil Raina, David Schoenherr, Max-Stephan Schulze, Felix Selgert, Henri Servaes, Janis Skrastins, Jan Starmans, Jochen Streb, Paolo Volpin, Fabian Wahl, Kirsten Wandschneider, and Xintong Zhan, along with conference participants at the AEA, the CAGE/HEDG Workshop, the CICF, the Cliometric Society ASSA Session, the DIIE Research Symposium, the EEA, the EHA, the EHS, the EFA, the GSWG, the NFA , the SSHA, the Summer Research Conference at the ISB, the TADC, the VfS, the WFA, and the World Congress of Cliometrics, and seminar participants at London Business School, London School of Economics, the University of Exeter, and the University of Mannheim for discussions and comments. We are grateful to Jörg Baten and Jochen Streb for sharing patent data, to David de la Croix and Omar Licandro for sharing data on famous people, and to Nico Voigtländer and Hans-Joachim Voth for sharing city population data. Thanks also to Anna-Sophia Elm and David Müller for research assistance. Meier thanks the Deloitte Institute of Innovation and Entrepreneurship at London Business School for providing financial support.

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1 Introduction

Economic prosperity depends on the ability of firms and individuals to generate innovative products and to improve production techniques (Solow, 1957; Mokyr, 1992; Kogan et al., 2017). The local socio-economic environment is crucial in determining the ability of individuals and firms to innovate. Consequently, innovative activity tends to be clustered in regions where the local conditions are more conducive to innovation (Chatterji et al., 2014).

The contribution of this paper is to show that *inclusive* institutions—institutions that provide broad access to economic opportunities instead of favoring the few at the expense of the many—create such an innovative environment, and thus positively impact innovation.

It is challenging to empirically study the role of inclusive institutions as a determinant of innovation, since it is difficult to obtain exogenous variation in institutional quality, especially at a granular level. We take advantage of a historical setting with exogenous within-country variation in the inclusiveness of institutions by using novel, hand-collected, county-level data on Imperial Germany. Until the late 18$^{\text{th}}$ century, *extractive institutions* were still widespread across the German states (Blum, 1978). These institutions provided privileges and rents to the few at the expense of the many. A fundamental change occurred after the French Revolution of 1789, when the French occupied large parts of Germany and spread their revolutionary ideas of personal freedom and equal rights. As a consequence, the power of local elites was reduced by the implementation of inclusive institutions, such as the French *Code civil* (Fehrenbach, 2008). These reforms created a more level playing field in terms of economic opportunities, by lowering entry barriers and reducing distortions in labor and product markets.

Using the length of the French occupation as an instrument for the quality of local institutions (Acemoglu et al., 2011), we find an economically large and highly statistically significant effect of institutions on patents per capita in Imperial Germany. Counties with the longest period of occupation, which implemented better institutions earlier, had more than twice as many patents per capita around 1900 as unoccupied counties with more extractive institutions.[1] This result points to inclusive institutions as a first order determinant of innovation.

Two aspects of our setting make it a good laboratory to study the impact of institutions on

[1] We focus on the period around 1900, since there is no nationwide patent data available for the years before 1877, when a harmonized patent law was introduced in the German Empire.

innovation. First, the timing and geography of occupations was imposed by the French, not chosen by the Germans. Second, the motives behind the French occupations were military and geostrategic, not economic. Napoleon wanted to expand the French borders to create a territorial buffer between France and its rivals, Austria-Hungary and Prussia. The choice of German regions to be occupied was thus not driven by the potential for future innovation or future economic growth of these regions, making subsequent increases in innovation an unintended consequence of the French occupation.

While we provide historical and quantitative evidence that the French occupation was determined by military and geostrategic considerations, one might still be concerned that the occupation was, by chance, correlated with the growth potential of the occupied areas. We use a historical event to address this concern. As compensation for its war efforts against France, Prussia wanted to occupy Saxony, which was at the time considered one of the most prosperous German regions, with high potential for economic growth (Flockerzie, 1991). However, Great Britain and Austria-Hungary were wary of giving such an economic powerhouse to Prussia. Instead, Prussia was compensated with Rhineland-Westphalia. We thus compare the part of Germany that Prussia wanted with the part that it obtained. Contrary to unoccupied Saxony, Rhineland-Westphalia had undergone the deepest institutional changes, due to experiencing the longest period of French occupation. As a consequence, it became significantly more innovative than Saxony ex-post. This historical event provides evidence for the assumption that, at the time of occupation, the occupied regions were economically neither more prosperous nor more promising.

The granularity of our county-level data allows us to provide evidence against alternative explanations for the regional differences in innovation that we document in this paper. By using sectoral employment shares, reflecting differences in economic development, we show that the effect of institutions on innovation is unlikely to be driven by a general increase in economic activity due to better institutions. We also discuss whether the transmission of French culture, knowledge, or technology could explain the results. In doing so, we document that counties that bordered France, which were more exposed to such transfers, did not become more innovative, and we find no evidence that the availability of steam engines and the availability of mechanical spinning mills, which we use as proxies for technology transfer, explain the effect of institutions on innovation. Furthermore, we find no link between innovation and the presence of intellectual elites in the period before the occupation. Trade and market integration are other potential channels. However, we find no evidence that innovation was driven by trade with France, and the impact of institutions on innovation remains strong after controlling for market-integration effects. Moreover, we control for human capital by using different proxies (including, e.g., the location of universities and literacy rates), for inequality by using data on landownership concentration, for financial development by using data on the location of stock exchanges and the banking workforce, for differences with regard to the discrimination and persecution targeting the Jewish minority, and for migration. We also show that past differences in patent law–the German patent law was not harmonized until 1877–are not driving the findings. Finally, we use additional county-level data on innovative products exhibited at two world's fairs (1876 and 1893) as a non-patent based proxy for innovation. As for patents, we find a significantly positive effect of institutions on world's fair exhibits, suggesting that differences in the propensity to patent cannot explain the results.

As described above, the index of institutional quality that we employ is based on the implementation of four institutional reforms. Yet it is challenging to identify the impact of each individual reform on innovation because the timing of the implementation of a reform is highly correlated with that of the other reforms–regions that implemented one reform earlier also tended to implement all other reforms earlier. To investigate which reforms mattered the most, we first compare the effect of institutions in counties where one would expect some institutional reforms to be muted with counties where some reforms should be particularly relevant. Furthermore, we distinguish between patents filed by individual inventors and patents filed by firms, since some reforms should have been more relevant for the inventive activity of firms than for individuals, and vice versa. Our results suggest that, among the four institutional reforms, the introduction of the *Code civil* and the abolition of guilds had a quantitatively stronger effect on innovation than the abolition of serfdom and the agricultural reforms.

In tracing the implications of our findings for economic growth, we show that the effect of inclusive institutions was particularly pronounced for innovation in chemicals and electrical engineering, which were the two high-tech sectors of the second industrial revolution. Counties with the longest period of occupation, which implemented better institutions earlier, had more than 2.5 times as many high-tech patents per capita around 1900 as unoccupied counties with more extractive institutions. Many inventions in these industries were disruptive and path-breaking, powering subsequent economic growth and improvements in living standards (Landes, 2003). The fact that the economic magnitude of the effect of institutions is larger in high-tech industries highlights innovation as a quantitatively plausible channel through which inclusive institutions affect economic prosperity.

Consequently, by using a three-stage least squares (3SLS) model, we show that the increase in patents per capita due to better institutions is associated with higher economic growth. The increase in innovation due to better institutions can explain about one third of the variation in economic growth across counties. Given that there are other plausible channels through which inclusive institutions may have affected growth, for example through a better allocation of factor inputs (Hsieh and Klenow, 2009) or the promotion of effort and an increase in working time (Voth, 2001), we interpret this result as an upper bound for the effect of innovation on growth. Our results also reflect the findings of Acemoglu et al. (2011), who show that inclusive institutions fostered urbanization in the German states.

This paper relates to three literatures. First, it contributes to the literature on the causes of innovation by documenting that inclusive institutions are a first order determinant of innovation. Other determinants of innovation include patent law (Moser, 2005), labor laws (Acharya et al., 2013), taxes (Mukherjee et al., 2017), competition (Aghion et al., 2005), human capital (Cinnirella and Streb, 2017), tolerance for failure (Manso, 2011), and myopia (Bian and Meier, 2021). Moreover, there is a voluminous literature on the role of finance as a determinant of innovation (for a survey, see Kerr and Nanda, 2015).

Second, our paper contributes to the extensive and influential literature highlighting the importance of institutions for the generation of economic growth (e.g. North, 1990; De Long and Shleifer, 1993; Hall and Jones, 1999; Acemoglu et al., 2001). We add to this literature by showing that innovation is a quantitatively plausible channel through which institutions affect growth. Other studies take a critical stance and attribute less importance to institutions. Glaeser et al. (2004), for example, argue that economic growth is not an outcome of inclusive institutions, but rather a precondition for better institutions, and Michalopoulos and Papaioannou (2014) show that within the homelands of African ethnicities extending into more than one country, institutions have no effect on economic performance. Our evidence is more consistent with the former view than with the latter. However, by the nature of our setting, which includes only data for one specific country in a specific period–the German Empire–we refrain from taking a strong stand on this debate.

Third, since we use a historical setting to exploit exogenous variation in the inclusiveness of institutions, this paper is related to the more recent literature on the historical determinants of social norms, financial outcomes, and entrepreneurship, focusing both on the role of formal historical institutions but also cultural factors. Examples are Grosfeld et al. (2013), who analyze settlement restrictions for Jews in Russia and show that antisemitism led to persistent antimarket culture and trust among the non-Jewish population; Bazzi et al. (2020), who provide evidence that former US frontier counties exhibit more individualism and opposition to redistribution; and Natividad (2019), who analyzes how disadvantageous taxation in Peru's mita regions affected trust and economic performance. Studies on the historical roots of finance include Pascali (2016) showing a link between Catholic doctrine in Renaissance Italy, the local presence of Jewish communities, and the development of modern banks, D'Acunto et al. (2019) studying the link between past antisemitism and demand for finance in contemporaneous Germany, and Suesse and Wolf (2020) exploring the emergence of micro-finance institutions in Prussia, while Pierce and Snyder (2018) and Levine et al. (2020) analyze how the slave trade affected financial development in Africa. Other examples are D'Acunto (2015), who documents the long-run persistence of basic education and its role in innovation in traditional industries, and Mao and Wang (2018), who study how access to finance and labor exploitation affected innovation in the antebellum US. For a recent survey of this literature, see D'Acunto (2017).

While scholars have long proposed the necessity of inclusive institutions in stimulating innovation (e.g. North and Thomas, 1970), evidence has been limited to anecdotes (see, e.g., Acemoglu and Robinson, 2012).[2] To the best of our knowledge, our paper is the first that uses a setting with exogenous variation in the quality of institutions to identify, isolate, and quantify the effect of inclusive institutions on innovation. Moreover, our paper suggests that innovation is a quantitatively important channel through which inclusive institutions foster economic growth.

2 Historical and Institutional Background

2.1 The French Occupation of Germany and Territorial Changes

In the 18th century, the Holy Roman Empire of the German Nation was a loose federation of states (Whaley, 2012). The French Revolution, starting in 1789, provoked the fall of the Empire. In 1792,

[2] d'Agostino and Scarlato (2019) and Tebaldi and Elmslie (2013) analyze the link between institutions and innovation, but lack exogenous variation in the quality of institutions and granular data to address alternative explanations.

the German monarchies joined a military coalition to counter the revolutionary ideas in France, but the French prevailed. To improve their strategic position, the French occupied all German territories west of the river Rhine by 1795. The result was a buffer zone, with the Rhine as a natural border to protect France from its continental rivals, Prussia and Austria-Hungary. In 1806, Napoleon forced the remaining medium-sized German states to establish the Confederation of the Rhine and to form a coalition with France. As a result, the Holy Roman Empire ceased to exist.

Prussia, the most powerful German state, was directly threatened by the Napoleonic expansion and declared war against France in the autumn of 1806. However, the French prevailed once more. Prussia had to accept the peace treaty of Tilsit (1807), which resulted in large territorial losses, in particular all provinces west of the River Elbe. Parts of these territories were integrated into the Grand Duchy of Berg and the Kingdom of Westphalia, which were ruled by relatives of Napoleon. From a strategic perspective, Berg and Westphalia became French bridgeheads on the eastern side of the Rhine. Furthermore, Napoleon occupied the free cities of Bremen, Hamburg, and Lübeck, as well as the remaining territories in the North. This control over the coast was a strategic necessity, since otherwise the continental blockade would have been undermined by contrabandists trading with Britain (Fehrenbach, 2008). By 1810, the Rhineland, Westphalia, the North Sea coast, Hanover, and parts of Hesse were under French control (see Panel A of Figure 1).

The military setbacks of 1813 triggered the end of the French occupation. Since the French had to withdraw their troops rapidly, the occupation ended in all territories at almost the same point in time, varying only by a couple of weeks.[3] After Napoleon's final defeat in the Battle of Waterloo, the great powers restored the political order of pre-revolutionary Europe. The German states, however, refrained from the restoration of the Holy Roman Empire (Fehrenbach, 2008). As a result of the French expansion and the subsequent territorial reorganization, a large number of previously independent territories were integrated into larger states. These changes were not revised, so most of the surviving states experienced lasting territorial gains.

2.2 Institutional Change under French Rule

The occupied territories suffered from the demand for troops and confiscations used to supply the French forces (Whaley, 2012). Economic activity was suppressed in times of war, and the blockade

[3]This implies that the geographical variation in the length of the French occupation resulted from the variation in the starting date (but not the end date) of the French occupation.

against Britain restricted trade. Thus, in the short run, the French occupation might have reduced innovation.[4] However, we document that the occupation had positive effects on innovation in the long run, as it fostered economic modernization and social progress through institutional reforms.

In the 18$^{\text{th}}$ century, economic development and the potential for innovation were limited by backward institutions which preserved the power and privileges of small elites.[5] However, along with the French troops, the ideas of the French Revolution spread across Germany and induced institutional change (Schubert, 1977). In occupied areas, the French forced substantial reforms which cut the elites' privileges. These reforms included the establishment of commercial freedom through the dissolution of guilds, the introduction of the *Code civil*, the abolition of serfdom, and the implementation of agrarian reforms (Acemoglu et al., 2011).[6] Economic motives played a minor role in the decision to impose these reforms. Besides having ideological reasons, the French wanted to restrict the power of the local elites, to facilitate the administration of the occupied areas. After the retreat of the French troops, the German sovereigns recalled some of the Napoleonic institutions in the formerly occupied lands. However, the process of restoration varied between regions, depending on the duration of the French occupation. In territories where the French occupation lasted only a couple of years, the pushback was much stronger than in territories with a long period of French rule.[7] As a result, the inclusiveness of institutions differed between German regions. Even after the formation of the German Empire in 1871, there were still significant institutional differences that could be traced back to the French invasion. One manifestation of this is the absence of a single, unified, nationwide civil law until 1900.

2.3 How Can Institutional Change Affect Innovation?

The institutional changes brought by the French created an economic environment that was more conducive to innovation. The establishment of commercial freedom through the abolition of guilds and other restrictions on trade and production directly increased the potential for innovation. In

[4]Rhenish textile firms, for example, could not import superior English machinery during the continental blockade. These companies struggled after 1815 due to antiquated equipment (Kisch, 1989).

[5]The nobility dominated economically and politically, and it retained juridical privileges, in particular in rural areas (Blum, 1978). City oligarchies limited entrepreneurship through guilds (Ogilvie, 1996).

[6]Reform efforts also took place in non-occupied states, but local elites often prevented the implementation. In Nassau and Hesse-Darmstadt, for example, the introduction of the French *Code civil* was discussed, but the consultations ended without results (Schubert, 1977). Reforms were also introduced in the non-occupied parts of Prussia, but the nobility retained privileges until the end of the century (Wagner, 2005).

[7]In the Rhineland, which was under French rule for 19 years, inclusive institutions remained in place, while restoration occurred in northern and central Germany, where the French occupation was shorter-lived.

the 18th century, guilds were still widespread in all German states (Ogilvie, 1996). Guilds created entry barriers and impeded change by protecting their members' interests and by preventing the introduction of labor-saving production techniques (Mokyr, 1992; Ogilvie, 2014).[8] Guilds acted like cartels, controlling local monopolies. Since low competition can reduce incentives for innovation (Aghion et al., 2005), the dissolution of guilds may have impacted innovation not only directly by breaking up rigid rules, but also indirectly through the creation of more competitive markets.

Factory-based industries were typically not controlled by guilds. However, launching a factory required a trading license, and the rulers seldom distributed such licenses (Fischer, 1962). Entrepreneurs had to rely on the goodwill of the ruler, but had no legal claim. Requests were rejected for various reasons, e.g., when the interests of incumbents were challenged.[9] Therefore, restrictive trade licenses affected innovation in a similar manner to guilds. The decrees on the dissolution of guilds also weakened other restrictions on competition and led to easier access to trade licenses and a general economic liberalization, so the timing of the dissolution of guilds proxies for a broad reduction of entry barriers.

Another consequence of the occupation was the introduction of the French *Code civil*. The French established a legal system that separated the judiciary from the public administration, and under which all citizens were to be treated equally before the law (Schubert, 1977). Clear rules were set in both civil and trade law, which may have fostered the creation of social capital (Buggle, 2016) and improved the conditions for establishing a business, thereby increasing the potential for innovation. Before the French invasion, patrimonial jurisdiction existed in all German states (Werthmann, 1995). Under patrimonial jurisdiction, the judiciary was not separated from the local administration. In rural districts, the local lord of the manor was often not only the largest landowner but also the mayor, the judge, and in charge of the local police. While the introduction of the *Code civil* revoked patrimonial jurisdiction in territories under French rule, this practice survived in most of the non-occupied German regions for a long time.[10] Overall, the French *Code*

[8] In the 17th century, the use of engine looms for silk ribbon production was inhibited in Cologne, Frankfurt, and other German cities (Pfister, 2008), and the guilds in Aachen impeded innovations in the metal industry (Kellenbenz, 1977). See Kisch (1989) and Lindberg (2009) for further examples.

[9] Arns (1986) mentions an entrepreneur who wanted to build a cotton spinning mill in Urach (Württemberg) in the mid-19th century, but officials did not grant a license, to protect the local weavers' guild.

[10] In Prussia, patrimonial courts and the police powers of the local lords of the manor were abolished in 1849. However, in 1853, the police powers were reestablished in some regions (Werthmann, 1995), and finally abolished only in 1927 (Clark, 2007). The East Elbian lords dominated the local public administration until World War II (Wagner, 2005).

civil provided a superior legal system, not only regarding the more equal treatment of people but also because of a stronger endorsement of private property rights (Clark, 2007), thereby creating an environment that was more conducive to innovation. By contrast, under a system of extractive institutions, the lack of legal protection may lead to poor incentives to innovate.

The French occupation also fostered innovation through the abolition of serfdom and subsequent agrarian reforms. In the 18$^{\text{th}}$ century, the manorial system still existed in the German states (Blum, 1978). In the manorial system a small group of noblemen owned a large proportion of the land, and their estates were subdivided into parcels, which were individually cultivated by tributary peasants. The French abolished this system in two steps. First, the peasants gained individual freedom through the abolition of serfdom, which increased labor market mobility. Second, agricultural reforms were introdruced to transfer land ownership from the aristocratic landowners to the peasants.

Tributary peasants were allowed to take ownership of the land that they had cultivated, though they had to compensate the landowners to some extent (Achilles, 1993). Therefore, the abolition of serfdom and the agrarian reforms reduced the power of the local elites. These reforms did not only take place in occupied regions. However, in most non-occupied regions, the rulers started agricultural reforms later and the implementation took much longer (Ashraf et al., 2020). Furthermore, the implementation of these reforms differed between and within the German states. For example, in unoccupied East Elbia, the agricultural reforms were not effective in breaking the economic power of the old elites, since the compensation rules were designed in favor of the lords (Clark, 2007).

As a whole, these institutional changes were a revolution in the way local communities operated, and we find that they significantly increased the economic incentives to innovate.

3 Data

We introduce novel hand-collected county-level data for Imperial Germany. Due to data availability, we focus on the years 1890, 1900, and 1910. Before 1877, German patent law was not harmonized and there was no nationwide patent register (Donges and Selgert, 2019a). The selection of years is also dictated by the availability of census data. The analysis ends in 1910 to avoid contamination by the economic disruptions of the World Wars. We use county-level data, the smallest unit for which data is available. After adjusting for changes in the administrative structure, we have 881 counties per year, spread over 80 regions, covering all 25 federal states of the German Empire (see Appendix B for more information on the data).

3.1 Institutions and French Occupation

We measure the inclusiveness of local institutions with the variable *Institutions*. This index includes four measures of institutions: (i) introduction of the *Code civil*, (ii) abolition of serfdom, (iii) implementation of agrarian reforms, and (iv) dissolution of guilds. The index is the mean of the number of years between the implementation of each reform and the year a patent was filed (1890, 1900, or 1910). The Rhineland, for example, has an index value of 100.25 for 1900, based on the average of the following (year of reform in brackets): *Code civil*: 98 (1802); serfdom: 102 (1798); agrarian reform: 96 (1804); guilds: 105 (1795).

This index was introduced by Acemoglu et al. (2011) to study the impact of institutional reforms on urbanization, which they use as a proxy for economic development.[11] There are two main differences between our institutions index and theirs. First, they use highly aggregated data, since their index is constructed at the level of German states and at the province level for Prussia. We collected additional data in order to include smaller German states and regions that were not covered in their paper. Second, we use the data at the county level in order to alleviate measurement error concerns inherent in a province-level analysis. As a consequence, our panel consists of 881 counties per year, instead of the 19 provinces and states used by Acemoglu et al. (2011). Out of these 881 counties, 211 counties are in states or regions not included in their data set. Table 1 contains information on the average institutions index in occupied and non-occupied areas in the sample. Consistent with the view that the French occupation led to more inclusive institutions, the institutions index is about 12 years higher in occupied counties than in non-occupied counties.

We use the variable *Years French Occupation* as an instrument for *Institutions*. *Years French Occupation* is the number of years for which a county was occupied. A county is classified as occupied if it was under direct French rule or was ruled by a member of Napoleon's family. Panel A of Figure 1 illustrates the regional differences in the length of French occupation and Panel B of Figure 1 shows regional differences in institutions.

[11] See Appendix A.6 for a discussion of the institutions index.

3.2 Patents

In the 18th century, the legal concept of patents did not exist in the German lands. Consequently, it is not possible to account for patenting activity before the French occupation. After 1815, the German states gradually introduced patent laws, but a harmonized German patent law was not established until 1877. In contrast to civil law and many other aspects of the legal system, the introduction of patent laws was not a result of the French occupation, nor did the French shape German patent laws. Consequently, there is no correlation between differences in patent laws before 1877 and the years of French occupation (Donges and Selgert, 2019a). Throughout the paper, we use patent data for the whole German Empire, which is available for the period after 1877 but not for earlier years, when each German state used an individual patent law.

As a proxy for innovation, we compute the number of high-value patents per million inhabitants by county. The data on high-value patents is from Streb et al. (2006), who provide information about patentee location, whether the patentee was an individual or a firm, and the technology class. High-value patents are defined as patents with a lifespan of at least 10 years, with a maximum length of 15 years. Since a patentee had to pay an increasing annual fee to renew a patent, these patents represent financially valuable products or production technologies (Streb et al., 2006). The mean number of patents per capita is distinctly higher in occupied counties (Table 1). Panel C of Figure 1 illustrates the spatial distribution of patenting activity.

4 Main Results

4.1 Determinants of French Occupation

In Section 2, we explain why the French occupation was driven by military and geostrategic considerations, not by economic motives. We now provide empirical support for this statement. We test whether we can predict the length of occupation with pre-1789 variables that account for potential determinants of economic prosperity, such as large cities, rivers, or coal deposits. If the argument that the occupation was not related to the underlying economic characteristics is valid, we would expect the coefficients of these variables to be insignificant and the R^2 close to zero. The results are presented in Table 2. In addition to equally weighted observations in column 1, we use area weighted observations in column 2 to account for differences in county size.[12] We cluster standard

[12]We cannot weight by the county population, since this data is not available for the early 19th century.

errors at the region level. Overall, the findings in columns 1 and 2 support the assumption that the French occupation was not related to the underlying economic determinants of the occupied counties, given that the adjusted R^2 of both models is 3%.

To illustrate the military and geostrategic motives behind the French occupation, we re-estimate the models in columns 1 and 2 by adding *Distance to Paris* as an explanatory variable. Columns 3 and 4 show that *Distance to Paris* is highly significant with a negative point estimate, and the adjusted R^2 increases by about 20 percentage points. These patterns strengthen the argument that the occupation was driven by military and geostrategic considerations.[13] The significant effects of *City State* (columns 1 and 3) and *Hanseatic League* (columns 3 and 4) align with this argument. *City State* controls for Hamburg, Bremen, and Lübeck, which were occupied in November 1806 due to their ports, to enforce the continental blockade. *Hanseatic League* indicates former cities of the Hanseatic League (a trade federation in the late medieval period). The positive effect of *Hanseatic League* can be explained by most of these cities being located in the North and near to the coast.

4.2 The Impact of Institutions on Innovation

To analyze the impact of institutions on innovation, we use an IV approach. In the first stage, we test whether we can predict the inclusiveness of institutions (*Institutions*) with the length of French occupation (*Years French Occupation*). In the second stage, we study the effect of *Institutions,* instrumented with *Years French Occupation*, on patents per capita, our proxy for innovation. By including a battery of county-level controls in both stages, we account for other potential determinants of innovation. Table 1 contains summary statistics for these variables. We also include year fixed effects, and cluster standard errors at the region level. We weight observations by population to avoid the possibility of small counties (population-wise) biasing the estimates.

The results of the first stage regression are presented in column 1 of Table 3. We find a statistically significant positive relationship between *Years French Occupation* and *Institutions.* The magnitude of the estimated effect is large. An additional year of French occupation is associated with inclusive institutions being in place for an additional 1.6 years. Moving from no occupation to the maximum length of French occupation (19 years) implies a 70% increase in the number of

[13]In the IV models later on, we do not control for distance to Paris, since this would control for a determinant of the French occupation, while instrumenting with its length. However, the results remain significant in all models if we use distance to Paris as an instrument for *Institutions*.

years inclusive institutions have been in place. The F-statistic for the instrument is 64.44, which attests to its strength. Furthermore, this regression model explains a large part of the variation in *Institutions* in Imperial Germany, as can be inferred from the adjusted R^2 of 81%.

The results of the second stage regression–the main finding of the paper–are presented in column 2 of Table 3. We find a strong relationship between the inclusiveness of institutions and innovation. The coefficient of 0.384 implies that going from 0 to 19 years of French occupation, which is equivalent to a change from no treatment to maximum treatment, leads to a 122% increase in the number of patents per capita, evaluated at the mean, through the implied change in institutions.[14] The effect is economically large and highly significant.

This finding holds after including year fixed effects and controlling for other potential determinants of innovation. First, we account for population density computed at the county level (*Population/*Km^2, see Figure A1), which is a well-established proxy for economic prosperity (Ciccone and Hall, 1996; Acemoglu et al., 2002). A one standard deviation increase in population density leads to a 105% increase in patents per capita when evaluated at the mean. Since market access may also affect innovation (Sokoloff, 1988), we control for important harbors (*Harbor),* access to navigable rivers (*River),* and access to both (*River*Harbor).* The effects for *River* and *Harbor* are not significant, while *River*Harbor* is negative and statistically significant. A potential explanation is that counties with major harbors, which were connected with their hinterland by rivers, specialized in trade at the expense of manufacturing, which typically produces more patents. We also account for the effect of foreign culture and cross-border trade by including dummies for counties at an external border (*Border)* and for counties at the border with France (*Border France*). The coefficient of *Border* is significantly negative, and the negative effect of *Border France* is even stronger. We also control for counties with coal deposits (*Coal Deposits*) and metal ore deposits (*Ore Deposits*), but find no significant effects. To account for potential pre-occupation differences in the propensity to innovate, we use a dummy that is equal to one if a city with more than 5,000 inhabitants was located within the respective county in 1750. The results show that patents per capita were on average significantly higher in these counties. This could be due to proto-industrial

[14]This magnitude is computed as follows. Subtract the average of *Institutions* for counties with the maximum occupation from the average of *Institutions* for counties with no occupation. Multiply this difference with the estimate for *Institutions* in the second stage of the IV. Dividing the resulting number by the mean number of patents per capita across all counties results in an implied increase of 122%. All averages are population weighted. All similar magnitudes reported in the paper are computed in the same way.

manufacturing businesses with a skilled labor force, or to the concentration of 'upper-tail' knowledge elites in these cities (Squicciarini and Voigtländer, 2015). In addition, we use *University 1789* to control for the existence of a university in 1789, since universities provide training that could lead to innovation and economic growth (Cantoni and Yuchtman, 2014). However, the estimated coefficient of *University 1789* is negative. This could be due to these old universities focusing on law, theology, and philosophy rather than the sciences or engineering. *Hanseatic League* indicates whether a former member city of the Hanseatic League was located in the county. We find a significantly negative relationship with patents per capita, which is consistent with the findings in the literature that document a relative economic decline of these cities after the medieval period (Lindberg, 2009). The coefficient of *Protestant %*, the share of Protestants in each county, is not statistically significant. This result differs from the findings of Becker and Woessmann (2009), who argue that Protestant regions became more developed than Catholic regions because of higher incentives to accumulate human capital. Interestingly, the Protestant effect becomes significant if one does not control for *Huguenots*, an indicator for settlements of the French Protestants who escaped from religious persecution in the 17$^{\text{th}}$ century. The effect of *Huguenots* is positive and highly significant. This finding is consistent with the results in Hornung (2014), who finds a positive effect of Huguenot settlements on productivity in textile production. Another factor that could have influenced innovation is the local presence of minorities with a native language other than German. In total, these minorities represented only 7% of the population, but they were clustered in a small number of counties. For this reason, we control for minorities non-parametrically with the dummy variable *Non-German Language*, which is equal to one if the fraction of the population whose native language was not German is above 50%.[15] *Non-German Language* is not significant. We also control for Prussia in its 1816 borders (the territory that belonged to Prussia in 1816) with *Prussia 1816*, which is akin to including a geography fixed effect for Prussia.[16] The coefficient of *Prussia 1816* is significantly negative. One explanation for this finding is the relative importance of agriculture in the eastern part of Prussia. Finally, we use *City State* to control for Hamburg, Bremen, and Lübeck (the city states of the German Empire) for two reasons. First, these major

[15] By the sample period of this paper, the Huguenots had already adopted the German language. The correlation between *Non-German Language* and *Huguenot* is -0.08.

[16] Prussia is the only state large enough to have variation in institutions to allow for such a "geography fixed effect", while the other German states are usually the size of a Prussian province, if not smaller.

harbor cities were occupied by Napoleon in 1806, to maintain the blockade against Great Britain. Second, the index captures only three of the four institutional reforms in these city states, since agricultural reforms played no role due to scarcity of agricultural land. The significant coefficient of *City state* suggests that these cities were on average more innovative.

4.3 Correlation between Occupation and Economic Potential

While we provide historical (Subsection 2.1) and quantitative evidence (Subsection 4.1) that the French occupation was determined by military and geostrategic considerations and not by economic ones, one might be worried that the French occupation was correlated, by chance, with the occupied areas' future growth potential. We use a historical case study to argue that, if anything, the French occupation might have been negatively correlated with the underlying growth potential of the occupied areas–at least in the eyes of contemporaries such as the rulers of Prussia, Austria-Hungary, and the United Kingdom.

We start by restricting the sample to the Rhineland, Westphalia, and Saxony (see Appendix B.1.2). This is motivated by the territorial reorganization after the Congress of Vienna. While the French had occupied the Rhineland, Westphalia, and the northern part of the Prussian province of Saxony, the old territories of the Kingdom of Saxony, one of Napoleon's military allies, had not been under French rule. After the French defeat, Prussia was pushing to annex the whole territory of the Kingdom of Saxony, which was considered to be one of the German regions with the highest growth potential (Kiesewetter, 2004). However, the United Kingdom and Austria-Hungary did not want to give such an economic powerhouse to Prussia. As a consequence, Prussia could only annex the economically less important northern part of Saxony, and not its prosperous heartland (Flockerzie, 1991). In addition, Prussia was compensated with the Rhineland and Westphalia. From a Prussian perspective, these regions were considered economically and strategically less attractive than Saxony. Importantly, the Rhineland and Westphalia, which Prussia gained against its initial intentions, had been occupied by the French and thus underwent substantial institutional reforms during the occupation. The result in column 1 of Table 4 provides evidence that the Rhineland and Westphalia became significantly more innovative *ex-post* due to the earlier implementation of institutional reforms, even though (unoccupied) Saxony appeared to be economically more promising *ex-ante*. If the French occupation was determined by the growth potential of the occupied regions, then Prussia should have been aiming to gain the Rhineland and Westphalia. Instead, Prussia pushed for Saxony. This test provides evidence against the view that our findings are driven by occupied regions being more likely to innovate in the absence of French occupation.

4.4 Robustness of the Main Result

Next, we perform a series of robustness tests. In column 2 of Table 4, we exclude East Elbia from the sample (see Appendix B.1.3). East Elbia represents the Prussian provinces east of the River Elbe, which were dominated by agriculture and a rural aristocracy, and could thus be a worse control group for the occupied areas. The effect of *Institutions* remains economically and statistically significant. Column 3 shows that the result also holds if we perform the analysis only for Prussia in its 1816 borders. The idea is that a within-state analysis may provide a more homogeneous sample than the German Empire as a whole.

In column 4, we replace the instrument *Years French Occupation* with an occupation dummy, which takes the value of one if the county was occupied by the French. On average, occupied areas implemented institutional reforms 12 years earlier than non-occupied areas. This leads to a 76% increase in patents per capita, compared to the mean in the sample. *ln(Institutions)* is the natural logarithm of *Institutions*, which we use in column 5 to allow for a non-linear effect. We find that the difference in institutional quality associated with going from 0 to 19 years of occupation leads to a 130% increase in innovation. In column 6, we use an alternative index of institutional quality that differs from the main index by including the abolition of patrimonial courts as an additional institutional reform (see Appendix B.2.2). The end of patrimonial justice was a major step towards a society in which all people were treated equally before the law. In some regions, in particular in the Rhineland where the French occupation lasted longest, patrimonial justice was abolished with the introduction of the French *Code civil*. However, it persisted for longer in non-occupied states and states where the old order was restored after the occupation (Werthmann, 1995). The effect of institutions on innovation remains highly significant and the magnitude of the effect is almost unchanged. In Appendix A.6, we also document that the results are robust to using a different index of institutional quality that is proposed by Kopsidis and Bromley (2016). However, as we discuss in Appendix A.6, the data for the index used by Acemoglu et al. (2011) is historically documented and more accurate than the data advocated by Kopsidis and Bromley (2016). For this reason, we follow Acemoglu et al. (2011) in our main tests. In column 7, we also estimate the effect of *Institutions* on innovation using OLS, instead of the instrumental variables approach. The effect of *Institutions* on innovation is 9% smaller than in the main regression specification of the paper, but the estimate remains highly significant.

We conduct further robustness tests, which we report in Appendix A.3. First, we show that the effect of *Institutions* remains highly significant and economically large when weighting observations equally. This addresses the concern that the results might be driven by a few heavily populated counties. Second, we document that the results hold if we run separate regressions for the years 1890, 1900, and 1910. We find a 212% increase in patents per capita for 1890, 115% for 1900, and 95% for 1910. In 1890, there were still regional differences in institutions; 1900 is the first year with harmonized institutions, and by 1910 all counties had already experienced at least 10 years of inclusive institutions. The fact that we find significant effects even in 1910 is testament to the long-term impact of inclusive institutions on innovation. This highlights the notion that the effect of institutional change on innovation is a relatively slow-decaying process with long-lasting effects. Furthermore, if we include only data from one cross-section, the effect of *Institutions* is also significant in all other tests reported in the paper.

The map of the French occupation in Panel A of Figure 1 shows occupied exclaves surrounded by non-occupied counties and non-occupied enclaves surrounded by occupied counties. While the overall occupation was not driven by economic considerations or correlated with growth potential, one might be worried that this might not hold for exclaves or enclaves, which could influence the results. However, the historical evidence excludes this possibility, and the results remain virtually unaffected if we exclude exclaves and enclaves from the sample, or if we code non-occupied enclaves as occupied and occupied exclaves as non-occupied. The findings are also unaffected if we "smooth" the occupation border by excluding all "bulges" that protrude into (non-)occupied areas, or code the corresponding counties such that the border between occupied and non-occupied areas is "smooth".

In Panel C of Figure 1, one can observe a small number of counties with a very high number of patents per capita. Such clustering of innovative activity is common. In the US, for instance, Silicon Valley stands out as a small but highly innovative cluster. Nevertheless, we show that the results are robust to winsorizing patents per capita (columns 1 and 2 of Table A4) and that the effect of *Institutions* is also significant and large when we estimate a Poisson model in the second stage of the IV, to account for the relatively high number of zero observations (column 3 of Table A4). Moreover, the results are robust to dropping the top 5, top 10, top 15, top 20, top 25, and top 30 most innovative counties from the sample (see Table A5). Since the 30 most innovative counties roughly 10% of the population, this represents a significant restriction on our sample. Lastly, we address the concern that, instead of clustering at the regional level, clustering should be at the level of the geographical units with a homogeneous institutional setting, since these geographical units could correspond to the treatment units (Table A6). We also report results using spatially-correlated standard errors (Conley, 1999) in Table A7. Reassuringly, the results are unaffected by these alternative clustering choices.

5 Alternative Explanations

5.1 Economic Development and Industrialization

The findings so far indicate that the institutional change caused by the French occupation fostered innovation. An alternative explanation could be a general increase in economic activity due to more inclusive institutions, which in turn affected innovation. To account for this concern, in all regressions, we control for population density, which is a well-established proxy for economic development (Ciccone and Hall, 1996; Acemoglu et al., 2002). We now provide additional evidence that institutions directly affected innovation, and that local economic development is unlikely to be driving the results.

To control for differences in local economic development that are not captured by population density, we use workforce data. Counties with a high employment share in manufacturing, mining, and services were on average more prosperous than counties where agriculture still dominated (Kuznets, 1971). To account for this, we use *Manufacturing+Mining Workforce %* (employment share in manufacturing and mining) and *Services Workforce %* (employment share in the private service sector). Due to data availability, we match the patent and population data of 1890, 1900, and 1910 with data from the employment censuses of 1882, 1895, and 1907, respectively. In addition, we introduce *Coal Mining 1850* (coal production in 1850 divided by the population in this year) to control for the importance of coal mining during the industrialization.[17]

[17] We already include *Coal Deposits* as a baseline control. In contrast, *Coal Mining 1850* measures the amount of extracted coal per capita in 1850. *Coal Mining 1850* is computed at the region level for data availability reasons.

In column 1 of Table 5, we add *Manufacturing+Mining Workforce %, Services Workforce %,* and *Coal Mining 1850* as controls to the baseline model. For all three variables, the estimated coefficients are positive, but only the coefficient of *Services Workforce %* is significant. The impact of *Institutions* remains economically and statistically significant. The coefficient of 0.288 indicates that going from 0 to 19 years of French occupation is associated with an increase in the inclusiveness of local institutions that leads to a 93% increase in patents per capita, compared to an increase of 122% for the baseline specification. The effect, although large in economic terms, decreases with the inclusion of these additional variables. This is not surprising, since innovation positively affects economic growth (see Section 8). When we account for economic activity we are controlling away part of the very effect we want to capture.[18]

5.2 Culture, Technology, and Knowledge Transfer

The French occupation induced institutional change, but the French influence may have extended beyond institutions. Other potential channels include an entrepreneurial culture or the transfer of technology and knowledge. Such transfers should be higher for counties that border France, since these counties were not only more likely to be occupied, but German-French interactions should also have been stronger at the border. We would thus expect a positive coefficient for *Border France*. However, the impact of *Border France* is significantly negative in the second stage regression (column 2 of Table 3).[19] Apart from border counties, the potential for the transfer of technology, knowledge, or ideas from France could have been higher in counties with Huguenot settlements (Hornung, 2014). We account for this effect by including *Huguenots* in all regressions. The estimate for *Huguenots* is positive and significant, but it does not explain the effect of *Institutions*.

We also note that Germany was particularly innovative in the chemical industry and electrical engineering (the high-tech sectors of that time), accounting for 23 percent of all high-value patents in our sample.[20] These new industries expanded at the end of the 19$^{\text{th}}$ century, long after the French occupation. By that time, Germany was the leading producer of dyestuffs, pharmaceuticals, and other chemicals, while France lagged behind in these sectors (Mokyr, 1992). If the French brought

[18] In Appendix A.4.1, we show additional specifications, also including coal production in 1880, for robustness.

[19] In Appendix A.4.2, we show that the effect of *Institutions* remains positive and significant when controlling for the distance to the French border. We also show results using distance to the Rhine as an alternative instrument and additional specifications using various subsamples that exclude parts of the occupied areas along the Rhine.

[20] In Section 7, we show that the effect of Institutions is much larger when we include only high-tech patents.

superior knowledge, it had to be technology that would become valuable for innovation several decades later, and in areas where the French did not seem to have a comparative advantage. By contrast, France had a competitive edge in the leading sectors of the First Industrial Revolution, for example, in the textile industry. During the Napoleonic Wars, the French textile industry flourished, since British imports had to be replaced by domestic production as a result of the blockade (Juhász, 2018), and there was also a textile boom in the occupied German Rhineland (Fehrenbach, 2008). However, historical case studies provide no evidence that import substitution fostered the transfer of French technology in this sector. Kisch (1989) even argues that the Rhenish textile firms suffered from antiquated machinery after the French occupation, because they could not import modern British technology during the blockade.

Despite the lack of evidence in the literature, we test whether occupied territories were endowed with superior production technologies by using hand-collected data on steam engines and mechanical cotton spinning mills. The steam engine was a general-purpose technology used in almost all industries, and mechanical cotton spinning mills were advanced textile-production facilities. If technology import was crucial, we would expect a positive correlation between the past endowment of steam engines and mechanical cotton spinning mills and patents per capita. We therefore include *Steam Engines 1861* (steam engines per million inhabitants within each region in 1861) and *Spinning Mills 1861* (mechanical cotton spinning mills per million inhabitants within each region in 1861) in column 2 of Table 5.[21] The data is based on a survey that was conducted in the Zollverein (the customs union that existed before the formation of the German Empire) in 1861. Thus, the sample is restricted to the Zollverein states. The coefficients for *Steam Engines 1861* and *Spinning Mills 1861* are both positive, but only *Spinning Mills 1861* is significant. The effect of *Institutions* remains strong and significant.

Territories that came under French rule might also have profited from closer geographic proximity to the centers of enlightenment in Western Europe, allowing a more intense intellectual exchange. Mokyr (2002) argues that the First Industrial Revolution emerged in an intellectual and social environment that was conducive to the generation of scientific knowledge and, eventually, technological change, creating a "culture of growth". Following this line of argument, one can construct a link between the scientific revolution of the 18$^{\text{th}}$ century–the Age of Enlightenment–and the subsequent

[21] We show additional model specifications in Appendix A.4.2.

industrial revolution (Squicciarini and Voigtländer, 2015). If there was already a superior culture of growth in occupied territories already before the French rule, these territories may have profited thereof from this in the 19th century, when scientific knowledge became increasingly important for the generation of innovation. To account for these potential pre-occupation differences in intellectual and scientific potential, we obtained data from de la Croix and Licandro (2015) on the birth places and birth years of famous individuals. This data allows us to control for the local presence of scientific and intellectual elites in the period *before* the French Revolution.

In column 3 of Table 5, we add the dummy variable *Intellectual Elites,* indicating counties in which at least five famous scientists and intellectuals were born in the period between 1650 and 1750 (for a more detailed description of the data, see Appendix B.5.4). The effect of *Institutions* remains significant, while the estimated effect of *Intellectual Elites* is not significant. Overall, the German states may have benefited from the scientific revolution of the 18th century and an "enlightened" culture that fostered innovation, but we find no evidence that this development was more pronounced in territories that later came under French rule.

5.3 Trade and Market Integration

If previously occupied territories traded more with France, this could explain a higher number of patents per capita in these regions. To address this concern, we control for border effects in all regressions. If trade was crucial, we would expect a higher number of patents per capita in counties that border France, since these counties were more exposed to German-French trade. However, we find a significantly negative coefficient of *Border France* in column 2 of Table 3. The coefficient of *Border* is also significantly negative. We cannot formally test whether France accounted for a higher share of foreign trade in previously occupied counties because trade statistics are only available at the highest aggregated level (German Empire and, for the period before 1871, the Zollverein). However, the aggregated import and export statistics of the Zollverein and the German Empire reveal that trade with France was less important than trade with other major European countries. The share of imports and exports from and to France was rather low, given the size of the French economy and its geographic proximity. In 1841, a year for which data is available, France ranked fourth in the trade statistic, accounting for only 8.4% of the imports and 11.4% of the exports of the Zollverein states, and it even lost shares in the subsequent decades.[22] Furthermore, we find no evidence that occupied areas profited from preferential trade agreements with France after the occupation.

A further channel through which the French occupation might have affected innovation is market integration (Keller and Shiue, 2016). As described in Subsection 2.1, the French occupation initiated a territorial reorganization through the dissolution of formerly independent small principalities, ecclesiastical states, and imperial cities. It is possible that the areas that could afford to be more politically fragmented ex-ante were those with higher economic potential, so the territorial reorganization could have unleashed economic growth through market integration. Failing to control for this could bias the estimates if the French occupied politically fragmented territories. We address this concern in column 4 of Table 5, by measuring the potential gains from internal market integration with *Old territories/km^2*. *Old territories/km^2* is the number of independent territories that existed in 1789 within each region, divided by the area of the region. In addition, we include the dummy variable *Internal Border,* which is one if the county was at a state border in 1816 and if the neighboring state was part of the German Empire in 1871. This variable accounts for market integration effects that appeared after the French occupation, particularly effects caused by the formation of the Zollverein (Keller and Shiue, 2014). We also include the dummy variable *Zollverein 1842*, which is one if the county was part of a Zollverein state in 1842.[23] The specification reported in column 4 adds *Old territories/km^2*, *Internal Border*, and *Zollverein 1842*. We find a significantly positive effect of *Zollverein 1842* and a significantly negative effect of *Old territories/km^2*. More importantly, the coefficient of *Institutions* remains highly significant and the economic magnitude even increases in comparison with the baseline model.[24]

5.4 Human Capital

Human capital is another factor that could drive the results, since regions with a more educated population were more innovative (see, e.g., Cinnirella and Streb, 2017). It could be that occupied regions were already better endowed with human capital before the occupation. However, controlling for differences in human capital before the late 19$^{\text{th}}$ century is challenging due to a lack of data.

[22]We report aggregated import and export statistics in Appendix A.4.3.
[23]The results are robust to earlier or later benchmark years (see Appendix B.5.7 for more information).
[24]For additional tests on market integration, see Appendix A.4.3.

Therefore, we employ several different proxies. We control for universities in 1789 in all models, and find a negative effect on patents per capita (column 2 of Table 3). This might be driven by the fact that these old universities focused on law, theology, philosophy, and the classics, which may not be conducive to innovation. We also control for large cities in 1750 (*Large City 1750*) in our baseline specification, which represent centers of proto-industry and "upper-tail knowledge" (Squicciarini and Voigtländer, 2015). We observe a significantly positive effect of *Large City 1750.*

In column 5 of Table 5, we add further proxies for human capital. First, we employ the dummy variable *Printing Press*, indicating places with a printing press between 1450 and 1500. Printing presses facilitated the exchange of knowledge and, eventually, the formation of human capital, leading to a positive link between the location of printing presses and economic development in pre-industrial Europe (Dittmar, 2011). Second, we add three variables that capture not only pre-occupation differences in human capital, but also differences in human capital in the period of observation, since the French occupation could also, for instance, have affected the provision of human capital. Such an effect would provide an alternative to institutions as the main channel. The three post-occupation human capital variables are: *Illiteracy* (the share of the population aged 10 or older that is illiterate, using the same data as in D'Acunto (2015)), *University* (indicating whether a university was located in a county in the respective year), and *Technical University* (indicating whether a technical university or mining academy was located in a county in the respective year). We control specifically for the presence of technical universities, since these universities trained engineers who may have been more likely to innovate than graduates from the non-technical sciences. In column 5 of Table 5, we simultaneously include *Printing Press*, *Illiteracy*, *University,* and *Technical University.* The coefficient for *Institutions* is almost unchanged compared to the baseline specification. In Appendix A.4.4, we provide additional regression specifications.

5.5 Inequality

Counties with high inequality could be less innovative (Akcigit et al., 2017), as inequality could affect inventors' ability to finance innovation (Rajan and Ramcharan, 2011) or the provision of education (Cinnirella and Hornung, 2016). To examine this, we use hand-collected, county-level data on landownership concentration, which is a well-established measure of wealth inequality for this period. This measure has two main strengths. First, the data was collected as part of the agricultural census of 1895 (see Appendix B.5.11), so there is (virtually) no measurement error. Second, agricultural land still accounted for a substantially larger fraction of total wealth than other assets, so it captures a large fraction of total wealth.

The data reveals considerable geographical variation in the concentration of landownership. Some parts of Germany, such as Baden or Württemberg, had a much lower degree of landownership concentration than other parts, such as East Elbia. If inequality was higher in non-occupied counties, differences in inequality, and not institutions, could drive the results. The results in column 2 of Table 4, where we document that the results are robust to excluding East Elbia, alleviate this concern. In addition, we use the aforementioned data on landownership to compute county-level Gini coefficients of landownership concentration. Column 1 of Table 6 indicates that the effect of inclusive institutions on innovation is practically unaffected by the inclusion of the Gini coefficient of landownership concentration as an additional control. The point estimate for *Inequality* is negative and statistically insignificant.

5.6 Discrimination and Persecution targeting the Jewish Population

From the medieval period, German laws systematically discriminated against Jews, for example by their free choice of occupation. Moreover, there were many periods when discrimination culminated in the persecution and the expulsion of the Jewish minority. Voigtländer and Voth (2012) document that, in the 1920s and early 1930s, antisemitism was more pronounced in regions where pogroms had taken place in the late medieval age, suggesting a long-run persistence of antisemitism. Antisemitism may have affected inventive activity in late-19$^{\text{th}}$-century Germany in several ways. Because of legal restrictions, Jews were not able to work in many occupations, but they were allowed to provide financial services. Therefore, antisemitism and, eventually, the persecution and expulsion of the Jewish population may have affected not only local financial development but also investment behavior (D'Acunto et al., 2019), and thus the financing of inventive activity. Moreover, if the Jewish minority was more educated, there would have been a loss of human capital in regions where the local population forced Jews to emigrate, while the receiving regions might have profited from the immigration of Jews (Moser et al., 2014). Finally, in regions with pronounced antisemitism, there might have been a culture that was in general less conducive to innovation, compared with regions where the local population was more accepting of minorities and open-minded.

To control for the potential effects of discrimination against and persecution of Jews, we employ data from Voigtländer and Voth (2012) to construct a dummy variable indicating places in which pogroms happened (*Pogrom*) and a dummy variable indicating major Jewish settlements in the late medieval period (*Medieval Jewish Settlement*). Moreover, we use census data to control for the number of Jewish people per 1,000 inhabitants *(Jewish Population per 1,000)* in the period of observation. By controlling for the Jewish population, we aim to account for potential effects of the introduction of emancipation laws in the 19$^{\text{th}}$ century. For the Jewish minority, discriminatory laws remained in place even after the introduction of inclusive institutions, and in most German states, the Jewish population did not get full civil rights until the late 1860s. Thus, it could be that Jews migrated to regions where emancipation laws had been introduced earlier, which might have affected innovation.

In column 2 of Table 6, we control for *Pogrom*, *Medieval Jewish Settlement*, and *Jewish Population per 1,000. Pogrom* is positive and significant. The reason why counties with a pogrom might have been more innovative is that pogroms might have been likelier to occur in places where the Jewish community was particularly successful in economic and business terms. Such places might also have been particularly innovative. Thus, it is not surprising that the (significant) point estimate of *Institutions* is slightly reduced, when compared with the baseline specification.[25]

5.7 Financial Development

The link between financial development and economic growth, with innovation as a central channel, is well-established in the literature (e.g., King and Levine, 1993). To test whether differences in local access to finance affected inventive activity, we use three proxies for financial development. First, we control for regional financial centers in the period before the French occupation with the dummy variable *Old Financial Center,* indicating the existence of an exchange for financial assets in a county before 1789.[26] Second, we use the dummy variable *New Financial Center,* indicating the local presence of a stock exchange in our period of observation. Apart from Berlin, which was the major financial center at the time, there were several smaller stock exchanges, including for

[25]In Appendix A.4.5, we document that the results are robust to a modified institutions index that also accounts for the Jewish emancipation. We also provide additional model specifications.

[26]Measuring financial development *prior* to the French occupation is challenging, since there is no data on credit volumes or financial-sector employment. To identify historical financial centers, we use information from Kaufhold (1992).

example Frankfurt am Main (Burhop and Lehmann-Hasemeyer, 2016). Focusing on the location of stock exchanges as a proxy for access to finance is supported by research on 19$^{\text{th}}$-century Germany, highlighting the role of IPOs for the financing of innovative companies (Lehmann-Hasemeyer and Streb, 2016). Third, we use county-level data from the 1895 and 1907 employment censuses to identify the share of the workforce employed in banking, which we then match with the patent and population data for 1900 and 1910 (*Banking Workforce in %)*.[27] For counties with a relatively high share of people employed in banking, we expect better access to financial services.

We include *Old Financial Center*, *New Financial Center*, and *Banking Workforce in %* as additional controls in column 3 of Table 6, respectively. While the estimated coefficients of *Old Financial Center* and *New Financial Center* are not significant, *Banking Workforce in %* has a positive, significant point estimate. Yet, the effect of *Institutions* remains highly significant with a similar magnitude as in the baseline specification.[28]

5.8 Migration

Regions with strong economic growth might attract migrants, while also being more innovative. Therefore, migration could reflect unobserved economic fundamentals that are not captured by the proxies for economic development and industrialization included in the previous tests. We use region-level data from the 1885 census, which provides the number of inhabitants born outside the respective province, to capture both international and intra-German migration.[29] Column 4 of Table 6 documents that *Migration*, the fraction of non-native inhabitants in each region, does not affect the relationship between the inclusiveness of institutions and innovation, while the point estimate of *Migration* is insignificant.[30]

5.9 Differences in Patent Law and Validity of Patents as a Proxy for Innovation

Differences in patent law can also affect innovation (Moser, 2005). Before 1877, the German patent law was not harmonized and the patent laws differed distinctly across German states, which is

[27]The 1882 employment census does not provide banking-workforce data, so we can include only two years.

[28]Given that patents could be used as collateral, inventors might have had incentives to patent their inventions to raise financing. If inventors were not innovating more but simply patenting more to improve their access to credit, we would expect a reduced point estimate for Institutions when controlling for local financial development, which is not what we find. In Subsection 5.9, we also use a non-patent-based measure of innovation to provide evidence that it is unlikely that higher incentives to file patents explain the effect of institutions on patenting. In Appendix A.4.6 we provide additional results on the topic of financial development.

[29]Census publications do not report county-level migration data and region-level data is only available for 1885.

[30]A qualitative analysis of the patentees' names shows that there is no evidence for French migration.

why we only use data for the period after 1877. As noted in Section 3, French patent law was not incorporated in the German states after the French occupation. In 1815, the Prussian government proclaimed a royal ordinance including the first German patenting rules. This Prussian patent law differed significantly from the French patent law at the time because it required the technical examination of patent applications and it used a very narrow definition of novelty (for details, see Donges and Selgert, 2019a). The legal design was influenced by the free trade supporters in the Prussian administration, but there is no evidence of French influence. Moreover, in contrast to (general) civil law, there were no regional differences in patent law within Prussia and only one central Prussian patent office in Berlin. Thus, the patent law in former French-occupied territories did not differ from the patent law in the non-occupied parts of Prussia. The smaller German states introduced patent laws later, but, as for Prussia, there is also no evidence of French influence.

Even if patent laws were not directly influenced by the French, it could be that the various patent laws influenced innovation differently, for example, by creating different incentives (Donges and Selgert, 2019b). Although we cannot directly control for possible legacy effects resulting from past differences in patent law, we circumvent this issue by restricting the analysis to counties that were part of Prussia in 1816, where there was a single patent law, so that the concern does not apply. Column 3 of Table 4 documents that the estimate of *Institutions* is virtually unaffected for this subsample.

So far, we have relied on patents as a proxy for innovation, but patents constitute only a subset of all innovative products and production technologies (see e.g., Moser, 2005, Moser, 2012). German patent law explicitly excluded some type of innovations from patent protection (e.g., agricultural commodities and foodstuffs). Moreover, in counties with more inclusive institutions, inventors may have had more trust in the rule of law and the ability to use courts to enforce their patents, making them more likely to protect their innovation with patents than with trade secrets. If this was the case, cross-county differences in patenting could be driven not by differences in innovative activity, but instead by differences in the propensity to patent.

To address these concerns, we use world's fair data to construct an alternative proxy for innovation. In the 19^{th} century, world's fairs were events where firms presented the cutting-edge products of their time. Moser (2005) shows that only a small fraction of these exhibits were actually patented and that there were distinct differences in the propensity to patent across industries.

To test the effect of inclusive institutions on the number of world's fair exhibits that originated from exhibitors of a certain county, we use data for two exhibitions: Philadelphia (1876) and Chicago (1893). We hand-collected the places of residence of the exhibitors from the official exhibition catalogs, aggregated this data, matched it with our data set, and computed the number of exhibits per million inhabitants. In Column 5 of Table 6, we present the results for the effect of inclusive institutions (instrumented with the years of French occupation) on the number of world's fair exhibits per million inhabitants.[31] We find a positive and significant coefficient of *Institutions* on this non-patent-based measure of innovation.[32]

By using world's fair exhibits, we show that the effect of inclusive institutions is not driven by the choice of our innovation proxy. While better legal institutions may have raised the motivation to file patents, the fact that we also find a positive effect on world's fair exhibits makes it unlikely that the estimated effect of inclusive institutions on patenting is only driven by a higher propensity to patent and not by more innovation.

6 Which Institutional Reforms Matter the Most?

An important question is which of the four institutional reforms included in the institutions index (introduction of the *Code civil*, abolition of serfdom, implementation of agrarian reforms, dissolution of guilds) mattered the most for innovation. One challenge in addressing this question is that the timing of the implementation of a reform is highly correlated with that of the other reforms. Thus, we cannot conduct an instrumental variables analysis using individual measures of institutions without violating the exclusion restriction that the instrument (*Years French Occupation*) is correlated with the variable of interest (one individual reform), but uncorrelated with any other determinants of the dependent variable (the other reforms). To circumvent this issue, we compare the effect of *Institutions* in counties where one would expect some institutional reforms to be muted with counties where some institutions should be particularly relevant. Moreover, we distinguish between patents filed by individual inventors and patents filed by firms, since some reforms should have been more relevant for the inventive activity of firms than for individuals, and vice versa.

To test the relative importance of the *Code civil*, we use data on the location of trade courts.

[31] Because we have data for only two world's fairs, we only include two years in the regression.

[32] In Appendix A.4.7 we also show that the effect of *Institutions* persists when only including chemical and electrical-engineering exhibits, and we find a significantly positive effect when including only Prussia in its 1816 borders in the sample to rule out any effects resulting from previous patent-law changes.

Trade courts specialized in dealing with legal disputes related to business, commerce, and trade.[33] One distinguishing factor across different parts of Germany was the ease with which people had access to trade courts. In regions without trade courts, legal disputes were brought before regular courts, which might have been inferior, for instance in terms of the business knowledge of the local judges. Thus, we consider transaction costs and other frictions to be lower in regions with trade courts. Most of the trade courts were founded in the early 19^{th} century and influenced by the French model, since trade courts had existed in France for a long time. Importantly, not only were trade courts established in regions that had been under French rule, but the density of trade courts was distinctly higher in the occupied territories.[34]

We compare the effect of *Institutions* in counties that are in a region with a trade court to that in counties without one. In column 1 of Table 7, we find a coefficient of 0.448 with a p-value of 0.1% if our baseline regression is run only for those counties that lie within a region with a trade court. In contrast, when we estimate our baseline regression in the subsample of counties that lie within a region without a trade court, the coefficient is -0.139 with a p-value of 30.9% (column 2 of Table 7). The difference between the coefficients for *Institutions* across the two models is significant at the 1% level. The fact that we find a stronger effect in regions with trade courts suggests that among the four institutional reforms the introduction of the *Code civil* may be particularly important.

Next, to tease out the relative importance of the abolition of serfdom and the agricultural reforms, we study the effect of *Institutions* in city and rural counties separately. By their nature, the abolition of serfdom and the agricultural reforms should have had a stronger effect on innovation in rural than in city counties. Therefore, if the effect of *Institutions* is stronger in rural than in city counties, this would be evidence that the abolition of serfdom and the agricultural reforms were important drivers of innovation. To identify city counties, we take advantage of the administrative structure that existed in Bavaria, Prussia, and Oldenburg at the time. These states distinguished between city and rural counties so we restrict the sample in the following regressions to these three states.[35] We then estimate our baseline regression model separately for the city and rural

[33] Concerning the role, development, and location of trade courts, we use information from 19^{th}-century sources, which we explain in Appendix B.5.18.

[34] Seven out of the eight states that were occupied had at least one trade court, with the exception of Oldenburg. 14 out of 21 states that were not occupied did not have a single trade court. In addition, there was also substantial variation in access to trade courts within states and within regions. For a detailed discussion of the geographical distribution of trade courts, see Appendix A.5.

[35] See Appendix B.1.5 for information on the distinction between city and rural counties.

counties. In columns 3 and 4 of Table 7, the coefficient of *Institutions* is 1.601 for the city-county sample (p-value 0.6%) and 0.113 (p-value 14.4%) for the rural counties. The difference between the coefficients for *Institutions* across the two models is significant at the 5% level. We interpret this as evidence for the notion that the abolition of serfdom and the agricultural reforms were less crucial for innovation than the introduction of the *Code civil* and the abolition of guilds.

Last, we distinguish between patents filed by individual inventors and patents filed by firms. The abolition of serfdom and the agricultural reforms (which transferred landownership from aristocratic landowners to the peasants) should have had a stronger effect on individuals than on firms. Therefore, if the abolition of serfdom and the agricultural reforms were quantitatively more important than the abolition of guilds and the introduction of the *Code civil*, we should observe a stronger effect of *Institutions* on patents filed by individual inventors than on firm patents. Yet, we find the opposite effect. *Institutions* had a stronger impact on patents filed by firms than on those filed by individuals. The coefficient for *Institutions* with firm patents per capita as the dependent variable is 0.292, with a p-value of 0.3% (column 5 of Table 7), while the corresponding estimate with individual patents per capita as the dependent variable is 0.092, with a p-value of 7.4% (column 6 of Table 7). The change in institutions associated with going from 0 to 19 years of French occupation implies an increase in firm patents of 189%, but of only 57% for individual patents. These results suggest that the introduction of the *Code civil* and the abolition of guilds may have been more important for innovation than the agricultural reforms and the abolition of serfdom.

Taken as a whole, the evidence suggests that among the four institutional reforms that comprise our index, the introduction of the *Code civil* and the abolition of guilds had a quantitatively stronger effect on innovation than the abolition of serfdom and the agricultural reforms.[36]

7 Effect of Institutions on Patents in High-Tech Industries

In this section, we study whether the impact of institutions was different in high-tech industries. We distinguish patents related to chemicals or electrical engineering, which we call "high-tech" patents, from the remaining patents in "non-high-tech" industries (see Appendix B.3.3). Concern-

[36]While the aforementioned correlation in the timing of the introduction of reforms makes us wary of employing individual institutions as independent variables, it is reassuring that our results hold if we focus only on the introduction of the *Code civil* and the abolition of guilds, which our findings suggest are the most relevant for innovation.

ing the creation of innovation, the chemical and electrical-engineering industries differed from other industries. R&D activities in these industries required a larger amount of physical and human capital, as shown by the fact that the leading companies in these sectors founded large research labs (Murmann, 2003). In these labs, researchers developed path-breaking and disruptive innovations such as the Haber–Bosch process, which became a major invention of the 20th century, allowing for example for the synthetic production of fertilizers.[37] Both the chemical industry and the electrical-engineering industry were important drivers of the second industrial revolution (Mokyr, 1992; Landes, 2003). The electrification of production processes, for example, allowed productivity gains compared with steam-engine technologies. Therefore, the finding that inclusive institutions impacted high-tech patents not only suggests that our proxy for high-tech innovation is capturing important technological discoveries, but also provides suggestive evidence that innovation might be a key channel through which institutions ultimately affect economic growth.

In columns 1 and 2 of Table 8, we divide the sample into high-tech and non-high-tech patents. The findings hold for both subsamples separately, but the effect is larger for high-tech patents. For ease of interpretation, we present the economic magnitudes implied by these results in the row below the point estimates in Table 8. Relative to their sample means, the change in institutions associated with going from 0 to 19 years of French occupation implies a 264% increase in high-tech patents, while the same institutional change leads to an 80% increase in non-high-tech patents. In addition, we examine high-tech patents filed by firms in column 3. The coefficient of *Institutions* remains highly significant, and the magnitude of the effect of 329% is the strongest in comparison to the other subsamples. In this regard, we note that high-tech patents filed by firms include patented innovations of many well-known chemical and electrical-engineering companies such as BASF or Siemens, which the literature considers highly innovative (e.g. Murmann (2003)).[38]

The above results provide suggestive evidence that inclusive institutions may be especially relevant for high-tech innovation. Thus, inclusive institutions might have been a precondition for the rise of the German chemical and electrical industries at the end of the 19$^{\text{th}}$ century.

[37]In 1911, BASF filed a patent for the "process for the synthesis of ammonia from the elements", which became well-known as the Haber–Bosch process.

[38]In Appendix A.4.7, we provide additional results showing that we also find a strong effect of Institutions on high-tech innovation, when using world's fair exhibits as a proxy.

8 The Effect of Innovation on Economic Growth

So far, we have documented a strong positive link between the inclusiveness of institutions and patents per capita. We now proceed to show that inclusive institutions, through their effect on innovation, affected economic growth, using a three-stage least squares (3SLS) model. Since county-level GDP data is not available for this period, we follow the literature and employ population growth as a proxy for economic growth (e.g., De Long and Shleifer, 1993, Ciccone and Hall, 1996, and Acemoglu et al., 2002).

In the first stage, we instrument the inclusiveness of institutions with the years of French occupation. We use the estimated *Institutions* in the second stage to test the effect on patents per capita. In the third stage, we test whether differences in patents per capita in 1890 and 1900 affect population growth in 1890-1900 and 1900-1910, respectively. The exclusion restrictions are twofold: First, the French occupation impacts innovation only through institutional reforms; second, institutional reforms affect economic growth exclusively via increases in innovation. Since the impact of inclusive institutions on growth may not operate exclusively through innovation, the results in this section are only suggestive in nature.

The controls used in the baseline specification in Table 3 are included in all three stages. Furthermore, we control for past population growth (*Past Growth*), to account for a possible growth trend. As before, we weight observations by population (for details, see Appendix B.5.19). The estimated coefficients of the 3SLS model are reported in Table 9. The results of the first (column 1) and second stages (column 2) confirm the previous findings and show that the length of French occupation explains differences in the inclusiveness of institutions, and that, in turn, institutions affect patents per capita. In column 3, we extend the findings by documenting a significant effect of patents per capita on future economic growth. The change in institutional quality associated with moving from no occupation to the maximum length of occupation is associated with an increase of 0.36 percentage points in the annual county-level population growth rate, through its impact on patents per capita in the second stage. The magnitude of the estimated effect is economically large, when compared with the mean population growth rate of 1.14 percentage points. Put differently, our results can explain up to 32% of the variation in future population growth. We note, however, that this estimate is an upper bound, as it is possible that inclusive institutions may also have affected growth through other channels (e.g., by reducing the misallocation of factor inputs (Hsieh and Klenow, 2009) or by higher provision of effort and an increase in working hours).

Acemoglu et al. (2011) show that the introduction of inclusive institutions fostered economic development (which they proxy with urbanization growth) in French-occupied regions. The results in this section are in line with their findings, since we also find evidence for an increase in economic growth by using a similar proxy (county-level population growth). Moreover, our finding supports the view that innovation is a plausible channel through which institutions affect economic growth.

9 Conclusion

We investigate the impact of inclusive institutions on innovation using novel, hand-collected, county-level data for Imperial Germany. After the French Revolution, the French occupied parts of the German states, for geostrategic reasons. We use this occupation, which was uncorrelated with economic fundamentals, as an instrument for differences in local institutions. Regions occupied for longer were early implementers of more inclusive institutions, creating an economy with lower barriers to entry and fewer distortions in labor and product markets.

We provide evidence for a quantitatively large effect of institutional quality on patenting activity. Counties with the longest occupation, which implemented better institutions earlier, had more than twice as many patents per capita around 1900 as unoccupied counties with more extractive institutions. We discuss various alternative explanations for the observed effect, but we find no evidence that a different channel explains the effect of inclusive institutions. Our findings are stronger for innovation in the chemical industry and electrical engineering. Therefore, inclusive institutions may have been instrumental for the Second Industrial Revolution, which was driven by these industries. We also show that the increase in patenting activity due to better institutions was associated with higher economic growth. Overall, the findings point to institutions as a first order determinant of innovation, thereby also helping us to understand the history of technological change.

Future research could explore the channels of transmission for the effect of institutions on innovation in more depth. Concerning the role of the *Code civil*, which we consider a particularly important institution, one could ask whether the related improvement of the legal systems directly affected innovation by lowering transaction costs or whether an indirect effect is more likely. Buggle

(2016), for example, shows that regions where the *Code civil* was introduced early exhibit higher social capital today. This raises the question of whether social capital might be such a potential channel of transmission. Similarly, one can ask whether the abolition of guilds fostered innovation via an increase in competition, which might be the most obvious channel, or whether cultural factors were also crucial. In this context, further research could analyze whether the introduction of guilds affected entrepreneurial spirit or the attitude towards risk and failure.

We study the determinants of innovation in the German Empire, since this historical setting provides us with variation in institutional quality within a single country driven by an event in the past–the French occupation–that we exploit for identification. However, our results may have broader implications for the understanding of the determinants of innovation. Even today, countries might be able to stimulate innovation by creating a more inclusive and efficient legal system with lower transaction costs. Similarly, reforms and policies aiming to increase competition and to create a level-playing field could be especially beneficial. Today, as in the case of guilds or trade licenses, there are institutions such as occupational licensing that restrict competition. Lifting such restrictions may foster innovation.

Moreover, in the early 19th century, Germany was economically and technologically backward compared with other countries in Western Europe. However, by the end of the century, Germany had become one of the leading industrial countries with highly innovative and internationally competitive companies, in particular in the high-tech industries of chemicals and electrical engineering. In this regard, developing and emerging economies may find it highly beneficial to implement the type of inclusive institutions that we study in our paper, as doing so could make them more innovative and help them to catch up to the technological frontier.[39]

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[39] This argument is in line with evidence in Bian et al. (2021), who document that countries with weak domestic institutions experience a particularly strong catch-up to the technological frontier when they sign bilateral investment treaties with technologically more advanced countries.

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Table 1: Summary Statistics

This table presents summary statistics for the main variables by occupation status. The last two columns report the differences in the variables between occupied and non-occupied counties and the associated p-values of a univariate test of the differences. Institutions is an index that measures the inclusiveness of local institutions. The index is the average of the number of years between the implementation of four institutional reforms (introduction of the Code civil, abolition of serfdom, implementation of agrarian reforms, dissolution of guilds) and the filing of a patent. Years French Occupation is the number of years of French occupation. Patents is the number of patents per million inhabitants. Population/km^2 is population divided by square kilometers. River (Harbor) is a dummy that takes the value of one if a county had a navigable river (sea harbor). River*Harbor is the interaction of the two variables. Border (Border France) is a dummy that takes the value of one if a county was at an external border of the German Empire (with France). Coal Deposits (Ore Deposits) is a dummy that takes the value of one if a county had deposits of coal (iron ore or non-ferrous metals). Large City 1750 is a dummy that takes the value of one if the county had a city with more than 5,000 inhabitants in 1750. University 1789 is a dummy that takes the value of one if a county had a university in 1789. Hanseatic League is a dummy that takes the value of one if a city in the county was a member of the Hanseatic League in the medieval period. Protestants % is the percentage of the population that was Protestant. Huguenots is a dummy that takes the value of one if a Huguenot settlement was in the respective county. Non-German Language is a dummy that takes the value of one if the fraction of the population whose native language was not German was above 50%. Prussia 1816 is a dummy that takes the value of one if a county was part of Prussia in 1816. City State is a dummy that takes the value of one if a county was part of one of the city states: Hamburg, Bremen, or Lübeck. All variables are weighted by population, using the population weights of 1900. Dummy variables are multiplied by 100 to facilitate their display. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details, see Appendix B.

	Occupied			Not Occupied			Difference in Means	
	Mean	St. Dev.	N	Mean	St. Dev.	N		
Institutions	70.21	21.20	251	58.33	9.73	630	11.88***	(0.000)
Years French Occupation	9.30	5.81	251	0.00	0.00	630	9.30***	(0.000)
Patents	12.14	24.81	251	8.95	17.89	630	3.20**	(0.030)
Population/km^2	1,141	1,933	251	2,442	6,770	630	-1,301***	(0.001)
River	53.45	49.98	251	39.91	49.01	630	13.54***	(0.000)
Harbor	9.25	29.04	251	5.36	22.53	630	3.90**	(0.029)
River*Harbor	8.18	27.46	251	2.18	14.63	630	6.00***	(0.000)
Border	10.93	31.27	251	16.43	37.08	630	-5.50**	(0.030)
Border France	3.43	18.25	251	1.07	10.29	630	2.37**	(0.014)
Coal Deposits	36.70	48.29	251	25.55	43.65	630	11.15***	(0.001)
Ore Deposits	16.42	37.12	251	13.13	33.80	630	3.29	(0.191)
Large City 1750	30.03	45.93	251	23.81	42.63	630	6.21**	(0.048)
University 1789	11.97	32.52	251	12.57	33.17	630	-0.60	(0.801)
Hanseatic League	24.73	43.23	251	12.06	32.59	630	12.68***	(0.000)
Protestants %	59.60	33.96	251	65.90	35.89	630	-6.29**	(0.013)
Hugenots	26.25	44.09	251	20.22	40.19	630	6.03**	(0.043)
Non-German Language	0.00	0.00	251	10.33	30.46	630	-10.33***	(0.000)
Prussia 1816	58.05	49.45	251	49.49	50.04	630	8.56**	(0.017)
City State	6.13	24.03	251	0.00	0.00	630	6.13***	(0.000)

Table 2: The Determinants of French Occupation

This table presents estimates of the determinants of French occupation using OLS. The dependent variable is the number of years of French occupation. Distance to Paris is the great circle distance in kilometers between the main city in each county and Paris. All other variables are defined in Table 1. One cross-section is used in the analysis. In columns 1 and 3, observations are equally-weighted. In columns 2 and 4, observations are weighted by county area. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	Years French Occupation			
	(1)	(2)	(3)	(4)
Distance to Paris			-0.009***	-0.008***
			(0.000)	(0.000)
River	1.056	0.452	1.050	0.596
	(0.170)	(0.413)	(0.138)	(0.235)
Harbor	-1.500**	-1.430**	-0.144	-0.700
	(0.025)	(0.034)	(0.828)	(0.261)
River*Harbor	0.066	3.554***	-0.120	1.118
	(0.958)	(0.002)	(0.883)	(0.372)
Coal Deposits	0.960	0.499	1.032	0.181
	(0.222)	(0.390)	(0.144)	(0.724)
Ore Deposits	1.483	1.472	0.628	-0.091
	(0.214)	(0.174)	(0.524)	(0.925)
Large City 1750	-0.231	-0.536	-0.252	-0.332
	(0.651)	(0.393)	(0.547)	(0.487)
University 1789	0.805	2.115	0.156	0.797
	(0.435)	(0.127)	(0.860)	(0.444)
Hanseatic League	1.060	1.080	1.579**	1.333**
	(0.193)	(0.221)	(0.018)	(0.049)
Huguenots	1.142	1.271	0.534	0.280
	(0.277)	(0.196)	(0.558)	(0.720)
City State	3.208***	1.397	2.376***	1.850
	(0.002)	(0.323)	(0.004)	(0.108)
Adj. R^2	0.032	0.032	0.230	0.253
N	881	881	881	881
Cluster	Region	Region	Region	Region
Weighting	Equal	Area	Equal	Area

Table 3: The Impact of Institutions on Innovation

This table presents estimates of the impact of institutions on innovation using an instrumental variables approach. In the first stage, we instrument Institutions with the years of French occupation. In column 2, we present the second-stage regression of patents per million inhabitants on the instrumented Institutions and control variables. All variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Institutions	(2) Patents
Years French Occupation	1.602***	
	(0.000)	
Institutions		0.384***
		(0.001)
Population/km^2	0.000	0.002***
	(0.780)	(0.000)
River	0.590	2.929
	(0.457)	(0.135)
Harbor	3.268	-1.372
	(0.238)	(0.592)
River*Harbor	-3.680	-14.399**
	(0.179)	(0.018)
Border	0.934	-3.802*
	(0.354)	(0.051)
Border France	16.024**	-18.409***
	(0.018)	(0.004)
Coal Deposits	-3.234***	1.451
	(0.006)	(0.210)
Ore Deposits	1.479	-0.709
	(0.154)	(0.691)
Large City 1750	-0.327	9.382**
	(0.778)	(0.013)
University 1789	4.361**	-8.845*
	(0.038)	(0.075)
Hanseatic League	-6.200***	-7.798**
	(0.000)	(0.013)
Protestants %	-0.053***	0.013
	(0.009)	(0.508)
Huguenots	1.486	9.999**
	(0.206)	(0.030)
Non-German Language	-1.902	-0.929
	(0.238)	(0.581)
Prussia 1816	14.949***	-10.298***
	(0.000)	(0.001)
City State	-7.995**	15.878***
	(0.015)	(0.005)
Adj. R^2	0.808	0.411
N	2,643	2,643
F-Stat. Ex. Instr.		64.44
Year FE	Yes	Yes
Cluster	Region	Region
Weighting	Population	Population

Table 4: Robustness Tests: Varying Sample, Instrumental Variables and Variables of Interest

This table presents estimates of the impact of institutions on innovation under different specifications. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation in all models, except for the model in column 4. The dependent variable is patents per million inhabitants. In column 1 (3), only counties in the Rhineland, Westphalia, and Saxony (Prussia) are included in the sample. In column 2, East Elbia is excluded from the sample. In column 4, Institutions is instrumented with a dummy that is one if a county was occupied by the French. In column 5, we use the logarithm of Institutions. In column 6, we use an alternative index of institutional reforms. In column 7, we estimate the impact of Institutions on patents per million inhabitants using ordinary least squares. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	Patents						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Institutions	0.154*	0.289*	0.398***	0.815***			0.348***
	(0.079)	(0.063)	(0.000)	(0.004)			(0.001)
ln(Institutions)					30.533***		
					(0.001)		
Alternative Institutions I						0.351***	
						(0.001)	
Adj. R^2	0.488	0.410	0.532	0.379	0.407	0.414	0.411
N	480	2,013	1,110	2,643	2,643	2,640	2,643
F-Stat. Ex. Instr.	607.69	49.00	447.19	10.79	39.98	110.35	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	West vs. Saxony	Excl. East Elbia	Prussia 1816	All	All	All	All
IV French Occ.	Years	Years	Years	Dummy	Years	Years	OLS

Table 5: Alternative Explanations

This table presents estimates of the impact of institutions on innovation, controlling for additional variables. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured by patents per million inhabitants. Institutions is instrumented by the years of French occupation. Manufacturing+Mining Workforce % (Services Workforce %) is the employment share of manufacturing and mining (private sector services in %). Coal Mining 1850 is the coal production in tons divided by the population in 1850. Steam Engines 1861 (Spinning Mills 1861) is the number of steam engines (spinning mills) per million inhabitants in a region in 1861. Intellectual Elites is a dummy that is equal to one if at least five famous scientists and intellectuals were born in the respective county between 1650 and 1750. Old territories/km^2 is the number of old territories per km^2 in a region. Internal border (Zollverein 1842) is one if a county was at a German state border in 1816 (if a county was in a Zollverein member state in 1842). Printing press is a dummy that is one if a printing press was adopted in a county between 1450 and 1500. Illiteracy is defined as the share of the population aged 10 or older that is illiterate. University (Technical University) is one if a (technical) university was located in the county in the respective year. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents	(5) Patents
Institutions	0.288**	0.343**	0.385***	0.576***	0.369**
	(0.017)	(0.015)	(0.001)	(0.000)	(0.011)
Manufacturing+Mining Workforce %	0.125				
	(0.202)				
Services Workforce %	0.796*				
	(0.075)				
Coal Mining 1850	0.816				
	(0.780)				
Steam Engines 1861		0.004			
		(0.435)			
Spinning Mills 1861		0.041***			
		(0.009)			
Intellectual Elites			1.453		
			(0.787)		
Old territories/km^2				-2.235**	
				(0.043)	
Internal Border				1.723	
				(0.462)	
Zollverein 1842				4.470*	
				(0.075)	
Printing Press					-2.935
					(0.333)
Illiteracy					-3.490
					(0.448)
University					2.977
					(0.589)
Technical University					-7.196
					(0.380)
Adj. R^2	0.434	0.416	0.411	0.422	0.416
N	2,643	2,514	2,643	2,643	2,556
F-Stat. Ex. Instr.	64.71	47.67	64.14	60.81	29.58
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Province
Weighting	Population	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes	Yes
Sample	All	Zollverein	All	All	Literacy Data

Table 6: Alternative Explanations (Continued)

This table presents estimates of the impact of institutions on innovation, controlling for additional variables. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured by patents per million inhabitants, except in column 5. In column 5 the dependent variable is world's fair exhibits per million inhabitants. Institutions is instrumented by the years of French occupation. Inequality is the Gini coefficient of the ownership concentration of agricultural land. Pogrom indicates counties in which pogroms occurred. Medieval Jewish Settlement indicates counties with a Jewish settlement in Medieval times. Jewish Population per 1,000 measures the number of Jewish people per 1,000. Old Financial Center indicates financial centers that existed before the French occupation. New Financial Center is a dummy variable indicating the location of an active stock exchange during the period of observation. Banking Workforce in % is the percentage of the workforce employed in banking. Migration is the share of non-native inhabitants relative to the total population of a region. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents	(5) Exhibits
Institutions	0.373***	0.308***	0.401***	0.399***	0.580**
	(0.001)	(0.005)	(0.004)	(0.001)	(0.025)
Inequality	-7.893				
	(0.256)				
Pogrom		4.698**			
		(0.024)			
Medieval Jewish Settlement		-0.571 (0.748)			
Jewish Population per 1,000		-0.411 (0.212)			
Old Financial Center			-3.450		
			(0.577)		
New Financial Center			7.584		
			(0.226)		
Banking Workforce in %			3.093*** (0.003)		
Migration				-0.258	
				(0.409)	
Adj. R^2	0.412	0.421	0.492	0.413	0.307
N	2,643	2,643	1,758	2,643	1,762
F-Stat. Ex. Instr.	62.57	60.18	63.95	63.68	64.47
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All (2 Years)	All	All (2 Years)

Table 7: Which Institutional Reforms Matter the Most?

This table presents estimates of the impact of institutions on innovation, investigating which of the four institutional reforms matters most. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured by patents per million inhabitants in columns 1 to 4, firm patents per million inhabitants in column 5, and individual patents per million inhabitants in column 6. Firm patents are those filed by corporations, and individual patents are those filed by individuals. Institutions is instrumented by the years of French occupation. In model 1 (2), only counties in a region with a (no) trade court are included in the sample. In model 3 (4), only city (rural) counties in Bavaria, Oldenburg, and Prussia are included in the sample. In models 5 and 6 the baseline sample is used. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents	(5) Firm Patents	(6) Individual Patents
Institutions	0.448***	-0.139	1.601***	0.113	0.292***	0.092*
	(0.001)	(0.309)	(0.006)	(0.144)	(0.003)	(0.074)
Adj. R^2	0.444	0.226	0.494	0.204	0.304	0.311
N	1,617	1,026	285	1,671	2,643	2,643
F-Stat. Ex. Instr.	76.36	18.32	79.28	185.06	64.44	64.44
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region	Region
Weighting	Population	Population	Population	Population	Population	Population
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Trade Court	No Trade Court	City	Rural	All	All

Table 8: The Impact of Institutions on Innovation in High-tech Industries

This table presents estimates of the impact of institutions on innovation in different industries. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured by patents per million inhabitants. Institutions is instrumented by the years of French occupation. The dependent variables are High-tech Patents in column 1, Non-high-tech Patents in column 2, and High-tech Firm Patents in column 3. Patents are categorized as high-tech if they are associated with chemicals and electrical engineering. Non-high-tech patents are all other patents. The economic magnitudes are the increase in patents associated with comparing the institutions in a county with no occupation to a county with the longest French occupation. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) High-tech Patents	(2) Non-high-tech Patents	(3) High-tech Firm Patents
Institutions	0.191**	0.193**	0.179**
	(0.034)	(0.016)	(0.041)
Economic Magnitudes	264%	80%	329%
Adj. R^2	0.194	0.373	0.157
N	2,643	2,643	2,643
F-Stat. Ex. Instr.	64.44	64.44	64.44
Year FE	Yes	Yes	Yes
Cluster	Region	Region	Region
Weighting	Population	Population	Population
Controls	Yes	Yes	Yes

Table 9: The Effect of Institutions on Growth

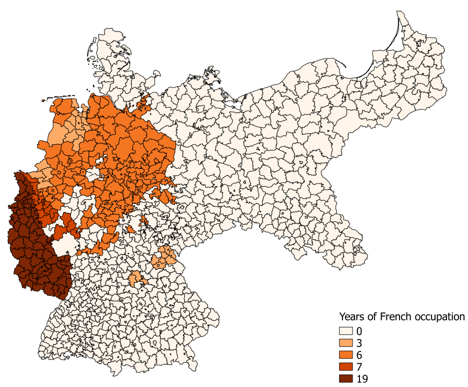
This table presents estimates of the impact of institutions on growth, using a three-stage least squares model. In the first stage, we instrument Institutions with the years of French occupation (column 1). In the second stage, we examine the relationship between the estimated Institutions and patents per million inhabitants (column 2). We take the estimated patents per million inhabitants in the third stage to estimate the effect on future county-level population growth rates (column 3). All control variables from Table 3 are included but not displayed. In addition, we control for past county-level population growth rates. The observations are weighted by county population. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details, see Appendix B.

	(1) Institutions	(2) Patents	(3) Growth
Years French	1.588***		
Occupation	(0.000)		
Institutions		0.257***	
		(0.000)	
Patents			0.034**
			(0.017)
Past Growth	-0.096	0.922***	0.316***
	(0.358)	(0.000)	(0.000)
R^2	0.781	0.307	0.297
N	1,762	1,762	1,762
Year FE	Yes	Yes	Yes
Weighting	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes
Sample	1890+1900	1890+1900	1890+1900

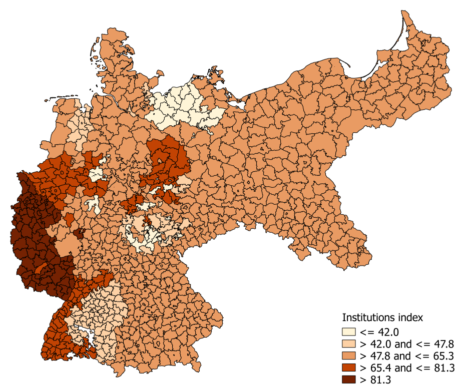
Figure 1: Maps of French Occupation, Institutions Index, and Patents per Million Inhabitants

This figure shows the map of the length of the French occupation in Panel A, the map of the Institutions index in Panel B, and the map of the distribution of the mean number of patents per million inhabitants in Panel C. In all three maps, the borders represent the county structure in the data set. See Appendix B for further information about the data.

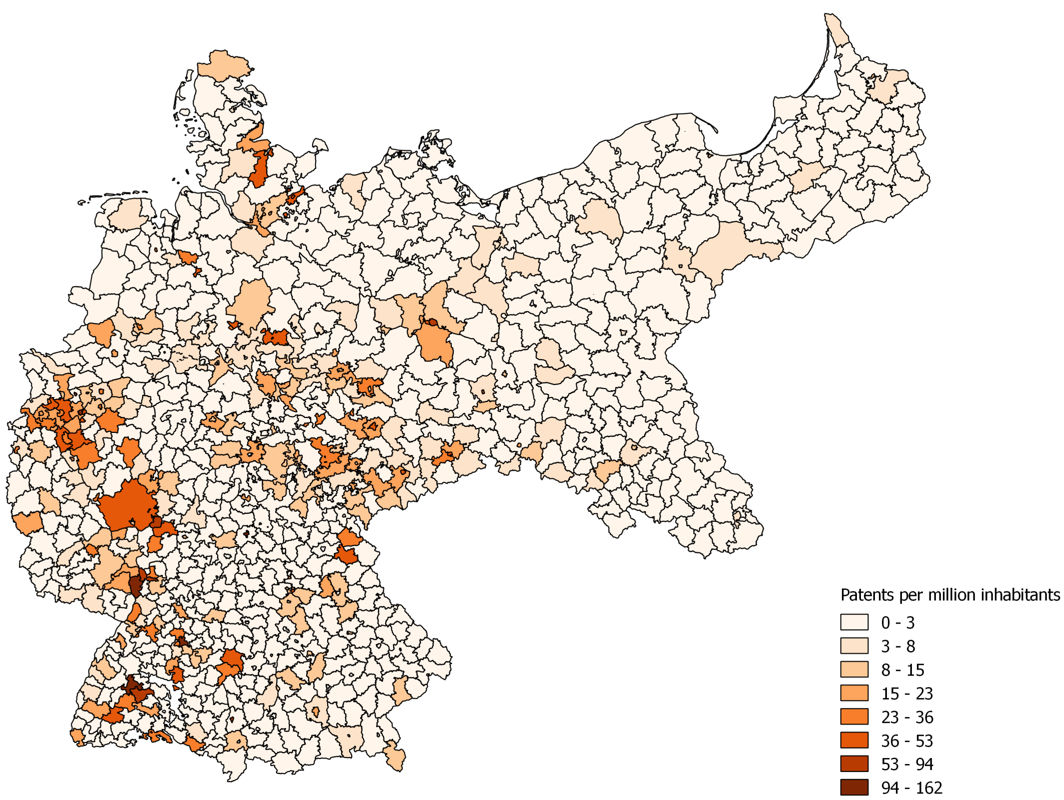
Panel A: French Occupation



Panel B: Institutions Index



Panel C: Patents per Million Inhabitants



For Online Publication

Appendix to "The Impact of Institutions on Innovation"

A Additional Results and Discussions

A.1 Additional Descriptive Statistics

Table A1 presents the summary statistics for the time-varying variables for each year of the sample. The general pattern from Table 1 holds for each individual year, with the level of the variables *Institutions* and *Patents* increasing over time. This is in line with better institutions being in place for longer (*Institutions*) and with the growth of innovative activity during the second industrial revolution (*Patents*). In contrast, *Protestants* is stable over the years. For the population density (*Population/km*), we report both an equally-weighted measure (where, for example, a county with 250,000 inhabitants gets the same weight as a county with 25,000 inhabitants) and a population-weighted measure (where, for example, a county with 250,000 inhabitants gets ten times the weight of a county with 25,000 inhabitants). The population-weighted population density is more informative, since in a national population-density measure, the large population of Berlin, for instance, naturally carries a larger weight than the small population of a rural county. When using the equally-weighted population density, we observe a decline in the population density in the occupied counties from 1900 to 1910 of -7.12%, while the population density in non-occupied counties increases by 2.39%. However, this pattern is explained by the population growth in occupied counties being more skewed towards the more populous counties (in particular due to the booming industrial cities) than in the non-occupied counties. When we population-weight the population density variable, the growth rate of population density from 1890 to 1900 is 37.67% in occupied counties compared to 26.14% in non-occupied counties. The corresponding numbers for 1900 to 1910 are 12.61% in occupied counties and 10.27% in non-occupied counties.

When considering the equally-weighted population density, the occupied counties have a higher population density than the non-occupied counties. By contrast, when considering the population-weighted population density variable, the non-occupied counties have a higher population density than the occupied counties in all three sample years. However, this effect is completely driven by Berlin. In the year 1900, for instance, Berlin had a population density of 29,816 people per square kilometer, while the second highest value for population density in 1900 was 13,612 people per square kilometer. Moreover, in 1900, Berlin represented 3.46% of the German population, making it easily the most populous county. If we exclude Berlin from the population-weighted population-density measure, then the population density of the occupied counties is larger than that of the non-occupied counties in all three sample years. To give an example, for the year 1900, the values without Berlin are 1,141 people per square kilometer for the occupied counties and 960 people per square kilometer for the non-occupied counties.

Table A1: Yearly Summary Statistics for the Time Varying Variables

This table presents yearly summary statistics for the time varying variables by occupation status. The last two columns report the differences in the means between occupied and non-occupied counties and the associated p-values of a univariate t-test for the differences in means. Patents is the number of patents per million county population. Institutions is an index that measures the inclusiveness of local institutions. The index is the average of the number of years between the implementation of four institutions (introduction of the Code civil, abolition of serfdom, implementation of agrarian reforms, dissolution of guilds) and the filing of a patent. Population/km^2 (equal-w.) is the equally-weighted ratio of population and area. Population/km^2 (population-w.) is the population-weighted ratio of population and area. Protestants % is the percentage of the population that was Protestant. All other variables are weighted by population. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details, see Appendix B.

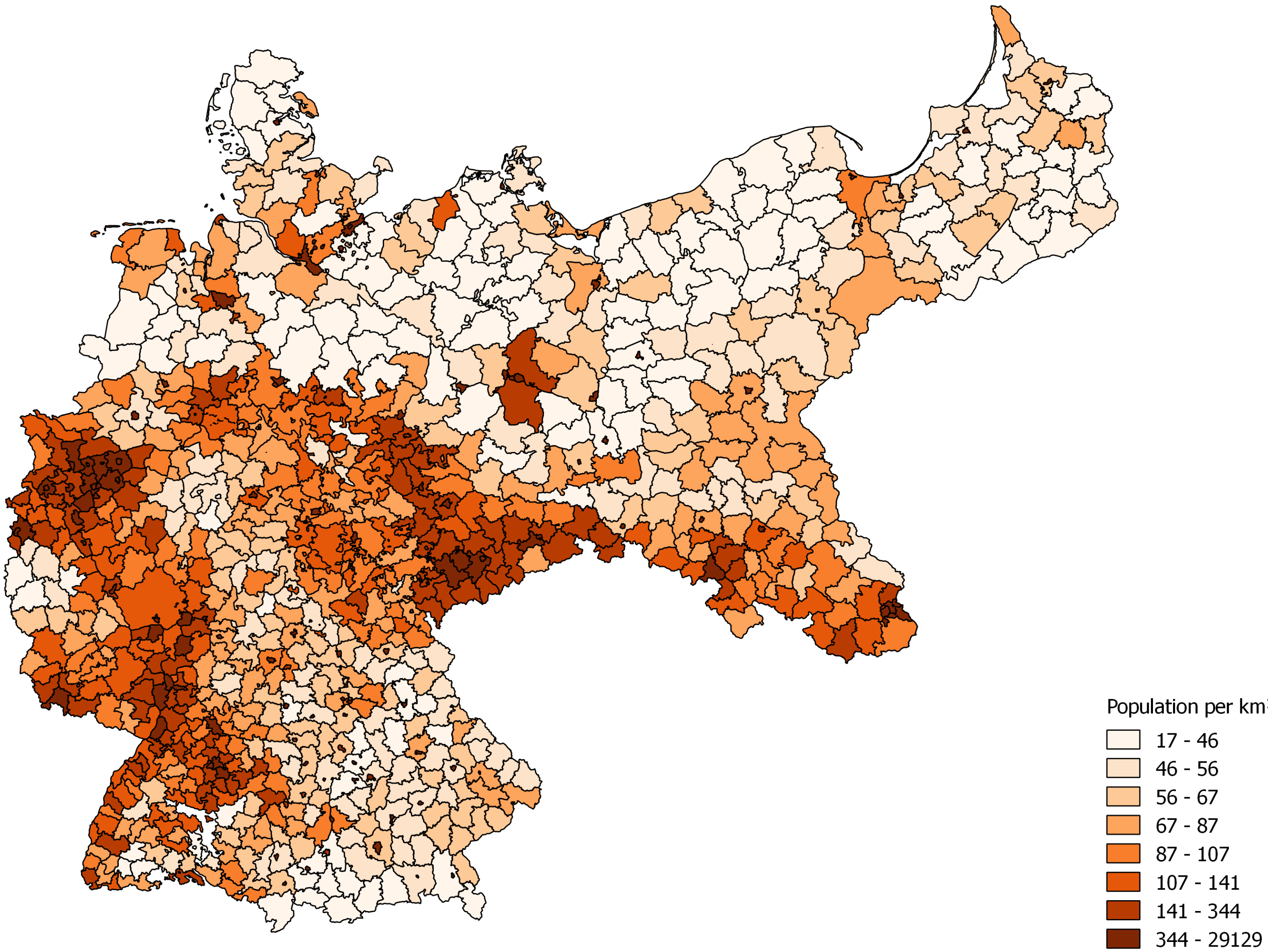
	Occupied			Not Occupied			Difference in Means	
	Mean	St. Dev.	N	Mean	St. Dev.	N		
1890								
Patents	8.74	17.79	251	3.63	9.15	630	5.11***	(0.000)
Institutions	62.01	20.42	251	50.33	9.86	630	11.68***	(0.000)
Population/km^2 (equal-w.)	537	1,262	251	406	1,628	630	131	(0.249)
Population/km^2 (population-w.)	829	1,498	251	1,935	5,593	630	-1,106***	(0.001)
Protestants %	60.07	34.71	251	65.43	36.27	630	-5.36**	(0.040)
1900								
Patents	12.14	24.81	251	8.95	17.89	630	3.2**	(0.030)
Institutions	70.21	21.20	251	58.33	9.73	630	11.88***	(0.000)
Population/km^2 (equal-w.)	664	1,618	251	435	1,748	630	228*	(0.074)
Population/km^2 (population-w.)	1,141	1,933	251	2,442	6,770	630	-1,301***	(0.000)
Protestants %	59.60	33.96	251	65.90	35.89	630	-6.29**	(0.013)
1910								
Patents	26.90	39.54	251	21.73	39.38	630	5.17*	(0.066)
Institutions	80.08	20.96	251	68.40	9.69	630	11.68***	(0.000)
Population/km^2 (equal-w.)	616	1,304	251	446	1,802	630	171	(0.173)
Population/km^2 (population-w.)	1,285	1,789	251	2,693	7,297	630	-1,408***	(0.001)
Protestants %	58.28	32.87	251	65.16	35.03	630	-6.89***	(0.005)

A.2 Population Density Map

Figure A1 shows the mean population per square kilometer for Imperial Germany over the years 1890, 1900, and 1910. The borders represent the county structure in the data set, which is explained in Appendix B.1.1. A comparison with the map on the number of patents per capita (Panel C of Figure 1) indicates that counties with a higher population density on average also produced more patents per capita.

Figure A1: Population Density

This map shows the mean population per square kilometer. The borders represent the county structure in the data set. See Appendix B for further information about the data.



A.3 Additional Robustness Tests

A.3.1 Equal Weighting and Individual Years

Throughout the empirical analysis, we weight observations by population, to avoid the possibility that a few small counties could determine the results. We now repeat the analysis of column 2 of Table 3, without weighting observations by population. Although equal-weighting observations could cause the magnitudes to be unrepresentative of the average effect of institutions on innovation, it allows us to exclude the concern that the results are driven by just a few heavily populated counties. We present the results in column 1 of Table A2. The results remain highly statistically significant and economically large.

In columns 2, 3, and 4 of Table A2, we test whether the results hold when we separately analyze the years 1890, 1900, and 1910, respectively. We want to exclude the possibility that the pattern we document is driven by a single year of data, which could raise doubts about the validity and general nature of the findings. We find that the results hold for all the years in the sample. When we contrast the impact of more inclusive institutions on innovation in the longest-occupied counties against the data for unoccupied counties, we find a 212% increase in patents per capita in 1890. The magnitudes are 115% in 1900 and 95% in 1910. All effects are evaluated at the respective sample mean. The reason for the results being stronger for earlier years is that institutions were not harmonized across the entire German Empire until 1900. In 1890, there were still regional differences in institutions; 1900 was the first year with harmonized institutions, and by 1910 all counties had already experienced at least 10 years of inclusive institutions. The fact that we find significant effects even in 1910 is testament to the long-term impact of inclusive institutions on innovation. This highlights the notion that the effect of institutional change on innovation is a relatively slow-decaying process with long-lasting effects. Furthermore, if we include only data from one cross-section, the effect of *Institutions* is also significant in all other tests reported in the paper.

Table A2: Robustness Tests: Equal Weighting of Observations and Individual Panel Years

This table presents estimates of the impact of institutions on innovation under different specifications. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. The dependent variable is patents per capita. In column 1, we equally weight observations. In all other columns, counties are weighted by population. In columns 2, 3, and 4, the sample is restricted to the year 1890, 1900, or 1910, respectively. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1)	(2)	(3)	(4)
	Patents	Patents	Patents	Patents
Institutions	0.254***	0.279***	0.276**	0.542***
	(0.001)	(0.000)	(0.027)	(0.004)
Adj. R^2	0.136	0.213	0.375	0.519
N	2,643	881	881	881
F-Stat. Ex. Instr.	72.67	63.99	63.43	62.82
Year FE	Yes	No	No	No
Cluster	Region	Region	Region	Region
Weighting	Equal	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes
Sample	All	1890	1900	1910
IV French Occ.	Years	Years	Years	Years

A.3.2 Exclaves and Enclaves

The map of the French occupation in Panel A of Figure 1 shows occupied exclaves surrounded by non-occupied territories, and non-occupied enclaves surrounded by occupied territories. While the overall French occupation was not driven by economic fundamentals or correlated with the underlying growth fundamentals of the occupied areas, one might worry that this might not hold for the exclaves or enclaves and that this could affect the results. We eliminate this concern using both historical evidence and quantitative analysis. All enclaves were part of small principalities located in strategically unimportant regions, in contrast to, for example, the territories along the North Sea, which had to be occupied to maintain the blockade against Great Britain. Likewise, the existence of exclaves reflects the political history of these territories. As a result of the Treaty of Tilsit (1807), Prussia lost all territories west of the Elbe—including these exclaves. In Appendix B.1.4, we provide a list of all enclaves and exclaves.

In the following tests, we provide quantitative evidence that the exclaves and enclaves do not affect the results (for details on the coding, see Appendix B.1.4). In column 1 of Table A3, occupied exclaves surrounded by unoccupied areas are dropped from the sample. In column 2 of Table A3, occupied exclaves surrounded by unoccupied areas are coded as unoccupied. In both cases, the coefficient for *Institutions* is virtually unaffected. In column 3 of Table A3, unoccupied enclaves surrounded by occupied areas are dropped from the sample. In column 4 of Table A3, unoccupied enclaves surrounded by occupied areas are coded as occupied. In both cases, the coefficient for *Institutions* is slightly larger than in the baseline specification. It also does not affect the findings if we "smooth" the border of French occupation by excluding all "bulges" that protrude into (non-)occupied areas (column 5 of Table A3), or if we code the corresponding counties such that the border between occupied and non-occupied areas is "smooth" (column 6 of Table A3). Finally, column 7 and column 8 combine all prior adjustments either by dropping the affected counties from the sample (column 7), or by coding them in the manner previously discussed (column 8). In both specifications, the point estimate for Institutions increases slightly.

Table A3: Robustness Tests: Exclaves and Enclaves

This table presents estimates of the impact of institutions on innovation under different specifications. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. The dependent variable is patents per capita. In column 1, occupied exclaves surrounded by unoccupied areas are dropped from the sample. In column 2, occupied exclaves surrounded by unoccupied areas are coded as unoccupied. In column 3, unoccupied enclaves surrounded by occupied areas are dropped from the sample. In column 4, unoccupied enclaves surrounded by occupied areas are coded as occupied. In column 5, the border of French occupation is "smoothed" by excluding from the sample all "bulges" that protrude into (non-)occupied areas. In column 6, all "bulge" counties are coded such that the border between occupied and non-occupied areas is "smooth". Column 7 and column 8 combine all the prior adjustments, by either dropping the affected counties from the sample (column 7), or by coding them in the previously discussed manner (column 8). All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B and in particular Appendix B.1.4.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents	(5) Patents	(6) Patents	(7) Patents	(8) Patents
Institutions	0.386***	0.385***	0.412***	0.404***	0.415***	0.383***	0.417***	0.404***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Adj. R^2	0.413	0.411	0.409	0.411	0.410	0.411	0.412	0.411
N	2,592	2,643	2,580	2,643	2,556	2,643	2,505	2,643
F-Stat. Ex. Instr.	65.54	65.62	63.16	59.63	63.41	63.73	64.52	58.76
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	No Exclaves	All	No Enclave	All	Smooth	All	All Adj.	All
IV French Occ.	Years	Exclave Adj.	Years	Enclave Adj.	Years	Smooth	Years	All Adj.

A.3.3 Winsorization and Excluding the Most Innovative Counties, and Poisson Regression

In Panel C of Figure 1, one can observe a small number of counties with a very high number of patents per capita, for example the cities of Berlin, Cologne, and Frankfurt am Main. Such a clustering of innovative activity in a small number of highly innovative regions is common. Today in the United States, for instance, Silicon Valley stands out as a small but highly innovative cluster. Nevertheless, we account for the concern that these observations constitute outliers that might drive the results. We winsorize the population-weighted number of patents per capita across all counties at the 1% and 5% levels (columns 1 and 2 of Table A4). The effect of *Institutions* remains highly significant and economically large in these specifications.

In column 3, we present an additional variation on the estimation procedure. Instead of employing a linear regression model, we estimate a Poisson regression in the second stage of the IV to account for the relatively high number of zero observations. Specifically, we estimate the impact of *Institutions* on the absolute number of patents per county (*Number Patents*), instead of patents per capita as in the main specification. Consequently, we add the county population as an additional control in the regression. We confirm the main result of the paper in this alternative specification, as *Institutions* remains highly statistically significant. The estimates imply that implementing all institutional reforms one year earlier leads to an increase of roughly 5.2% in the number of patents in the county.

We further address the concern that our results could be driven by a small number of outliers, by excluding the top 5, 10, 15, 20, 25, and 30 counties ranked by patents per capita in columns 1 to 6 of Table A5, respectively. A county's rank by patents per capita is computed by first averaging a county's patents per capita across 1890, 1900, and 1910, and then ranking the counties by average patents per capita. Since the 30 most innovative counties account for roughly 10% of the population, this represents a significant restriction on our sample. As can be seen, our main finding is robust to these non-random exclusions of counties.

Table A4: Robustness Tests: Winsorization and Poisson Regression

This table presents estimates of the impact of institutions on innovation under different specifications. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. In columns 1 and 2, the dependent variable is patents per capita. In column 3, the dependent variable is the number of patents. In columns 1 and 2, a standard instrumental variable approach is used. In column 3, an IV-Poisson approach is used. In column 1 (2), the number of patents per capita is winsorized at the 1% (5%) level. In column 3, county population is added as an additional control variable. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents Winsorized 1%	(2) Patents Winsorized 5%	(3) Number Patents
Institutions	0.354***	0.219***	0.052***
	(0.001)	(0.006)	(0.005)
Population			0.000
			(0.191)
Adj. R^2	0.458	0.450	
N	2,643	2,643	2,643
F-Stat. Ex. Instr.	64.44	64.44	
Estimation	IV	IV	IV-Poisson
Year FE	Yes	Yes	Yes
Cluster	Region	Region	Region
Weighting	Population	Population	Population
Controls	Yes	Yes	Yes

Table A5: Robustness Tests: Excluding the Most Innovative Counties

This table presents estimates of the impact of institutions on innovation, excluding the most innovative counties. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. The dependent variable is patents per capita. In columns 1 to 6, we exclude from the sample the top 5, 10, 15, 20, 25, and 30 counties ranked by patents per capita, respectively. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1)	(2)	(3)	(4)	(5)	(6)
	Patents	Patents	Patents	Patents	Patents	Patents
Institutions	0.262***	0.178**	0.176**	0.204***	0.179**	0.167***
	(0.008)	(0.038)	(0.040)	(0.009)	(0.013)	(0.009)
Adj. R^2	0.494	0.363	0.341	0.310	0.330	0.317
N	2,628	2,613	2,598	2,583	2,568	2,553
F-Stat. Ex. Instr.	69.56	77.66	77.14	79.23	77.06	77.06
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region	Region
Weighting	Population	Population	Population	Population	Population	Population
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Excl. Top 5	Excl. Top 10	Excl. Top 15	Excl. Top 20	Excl. Top 25	Excl. Top 30

A.3.4 Alternative Clustering

In this subsection, we show that our main findings are robust to different choices concerning the clustering of standard errors and we provide results with Conley (1999) standard errors to account for spatial autocorrelation in the residuals.

In column 1 of Table A6, instead of clustering at the regional level, we cluster at the level of the geographical units with a homogeneous institutional setting, since these geographical units could correspond to the treatment units. Reassuringly, the results are unaffected by this alternative clustering.

By using standard errors clustered at the region level, we account for the potential spatial autocorrelation within a region (a region consists on average of 11 counties), but not for the autocorrelation in the residuals of the counties that are close to each other but are part of different regions. In light of recent debates about the validity of empirical results with regard to the existence of spatial autocorrelation in the residuals (see Kelly (2019) and Voth (2021)), we conduct robustness tests in which we estimate our regression model with Conley standard errors (Conley (1999)), allowing for spatial autocorrelation in the residuals. In these tests, we use the code posted on Timothy Conley's website which uses equal-weighted observations.

The results, which we present in Table A7, show that the effect of *Institutions* remains statistically significant when we allow for spatially correlated standard errors. Across the different columns of this table, we vary the size of the geographical radius where we allow standard errors to be correlated. To ease the comparison of these results, in column 1 we also report standard errors using region-level clustering, which is the level of clustering employed throughout the paper. In column 2, standard errors allow for spatial correlation within 1 degree longitude and latitude; in column 3, standard errors allow for spatial correlation within 2 degrees longitude and latitude; in column 4, standard errors allow for spatial correlation within 4 degrees longitude and latitude; and in column 5, standard errors allow for spatial correlation within 8 degrees longitude and latitude. In all models, the coefficient for Institutions is significant at the 1% level. For context, the standard deviation of latitude is 2.36 and that of longitude is 3.72. Consistent with the evidence in Voth (2021), our results are robust to standard errors that allow for spatial correlation. In fact, it appears that the standard errors associated with the coefficient of interest are very similar under our original clustering and with spatially clustered standard errors.

Table A6: Robustness Tests: Alternative Clustering

This table presents estimates of the impact of institutions on innovation when the clustering is at the level of geographical units with homogenous institutions. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. The dependent variable is patents per capita. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents
Institutions	0.384**
	(0.021)
Adj. R^2	0.411
N	2,643
F-Stat. Ex. Instr.	24.12
Year FE	Yes
Cluster	Homogeneous Institutions
Weighting	Population
Controls	Yes

Table A7: Robustness Tests: Standard Errors Allowing for Spatial Correlation

This table presents estimates of the impact of institutions on innovation with standard errors that are spatially clustered. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. The dependent variable is patents per capita. In all columns, we estimate our baseline regression model, except that observations are equally weighted. In column 1, we report standard errors that are clustered at the region level, as throughout. In columns 2 to 5, we report standard errors that are spatially clustered according to the procedure of Conley (1999). In column 2, standard errors allow for spatial correlation within 1 degree longitude and latitude; in column 3, standard errors allow for spatial correlation within 2 degrees longitude and latitude; in column 4, standard errors allow for spatial correlation within 4 degrees longitude and latitude; and in column 5, standard errors allow for spatial correlation within 8 degrees longitude and latitude. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents	(5) Patents
Institutions	0.254***	0.254***	0.254***	0.254***	0.254***
	(0.076)	(0.090)	(0.081)	(0.067)	(0.060)
Adj. R^2	0.136	0.136	0.136	0.136	0.136
N	2,643	2,643	2,643	2,643	2,643
F-Stat. Ex. Instr.	72.67	72.67	72.67	72.67	72.67
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Region	1 Degree	2 Degrees	4 Degrees	8 Degrees
Weighting	Equal	Equal	Equal	Equal	Equal
Controls	Yes	Yes	Yes	Yes	Yes

A.4 Additional Results

A.4.1 Economic Development and Industrialization

In Subsection 5.1, we provide evidence that local economic development is unlikely to be driving the paper's main results (see column 1 of Table 5). We now provide evidence from further model specifications to support this assertion. As explained before, to control for differences in local economic development that are not captured by population density, we use data on the sectoral composition of the workforce. One new variable that we use is *Coal Mining 1880*, which measures the coal production in 1880 divided by the population in 1880. Similarly to *Coal Mining 1850*, this variable is computed at the region level due to data availability.

In column 1 of Table A8, we add *Manufacturing+Mining Workforce %* and *Services Workforce %* to the baseline model as controls. The coefficients are positive and significant. In columns 2 to 5, we show various specifications with *Coal Mining 1850* and *Coal Mining 1880* included. When added individually, the coefficients are positive and statistically significant (columns 2 and 3), but they lose significance when we also control for the employment shares (columns 4 and 5). The impact of *Institutions* remains economically and statistically significant in all columns. The coefficient of 0.292 in column 1 indicates that going from 0 to 19 years of French occupation is associated with an increase in the inclusiveness of local institutions that in turn leads to a 93% increase in patents per capita, compared to an increase of 122% for the baseline specification. The effect, although large in economic terms, decreases with the inclusion of these additional variables. This is not surprising. Since innovation positively affects economic growth (see Section 8), when we account for economic activity we are controlling away part of the very effect we want to capture.

Table A8: Alternative Explanation: Economic Development and Industrialization

This table presents estimates of the impact of institutions on innovation, controlling for economic growth and industrialization. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured by patents per capita. Institutions is instrumented by the years of French occupation. Manufacturing+Mining Workforce % (Services Workforce %) is the employment share of manufacturing and mining (private sector services in %). Coal Mining 1850 (Coal Mining 1880) is the coal production in tons divided by the population in 1850 (1880). All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	Patents				
	(1)	(2)	(3)	(4)	(5)
Institutions	0.292**	0.347***	0.370***	0.288**	0.292**
	(0.018)	(0.004)	(0.002)	(0.017)	(0.018)
Manufacturing+Mining	0.136*			0.125	0.133
Workforce %	(0.062)			(0.202)	(0.176)
Services Workforce	0.790*			0.796*	0.793*
%	(0.080)			(0.075)	(0.074)
Coal Mining 1850		4.276*		0.816	
		(0.072)		(0.780)	
Coal Mining 1880			0.515*		0.036
			(0.063)		(0.922)
Adj. R^2	0.434	0.415	0.414	0.434	0.434
N	2,643	2,643	2,643	2,643	2,643
F-Stat. Ex. Instr.	58.34	68.40	69.68	64.71	61.33
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region
Weighting	Population	Population	Population	Population	Population
Controls	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	All

A.4.2 Culture, Technology, and Knowledge Transfer

In this section, we present additional evidence to rule out the notion that technology and knowledge transfers from the French drove the subsequent boom in innovation in the occupied areas.

Distance to the French Border In all specifications, we include the dummy variable *Border France* to control for potential border effects that may have affected innovation. Among other things, this variable accounts for potentially more intense cultural exchange and technology transfers at the border with France. However, by only including counties directly at the German-French border, which we indicate with this dummy, we might underestimate these effects.

Therefore, we computed the closest distance of each county in our sample to a French-German border county. The variable *ln(Distance to French Border)* indicates the natural logarithm of this distance (before computing the logarithm, we added one to the distance, since the distance is zero for all border counties). We use the baseline specification of our paper except that we add this control variable and remove the dummy variable *Border France* from the specification, since we would otherwise be controlling for two variables related to the French border in the same specification. Column 1 of Table A9 indicates that the estimated coefficient of *Institutions* is almost unchanged, compared to the baseline specification, while the effect of *ln(Distance to French Border)* is insignificant.

Table A9: Distance to the French Border

This table presents estimates of the impact of institutions on innovation, controlling for the distance to the French border. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured using patents per capita. Institutions is instrumented by the years of French occupation. ln(Distance to French Border) is the natural logarithm of (1 + distance to the closest French-German border county). All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. For more details on the data, see Appendix B.

	(1) Patents
Institutions	0.356**
	(0.019)
ln(Distance to French Border)	0.487
	(0.766)
Adj. R^2	0.406
N	2,643
F-Stat. Ex. Instr.	37.83
Year FE	Yes
Cluster	Region
Weighting	Populat.
Controls	Yes
Sample	All

Rhineland, Distance to Rhine, and Excluding Parts of the Sample Next, we discuss additional tests showing that the effect of *Institutions* on patents per capita is robust to different specifications and subsamples.

One concern could be that the parts of Germany that were located west of the river Rhine or close to the Rhine were historically different from other German regions (e.g. in terms of socioeconomic pre-conditions or a specific Rhenish culture). Since these were the regions with the longest period of French occupation and the most inclusive institutions, such differences could affect our results. However, there is strong evidence that this is not the case.

First, in Table 2, we show that the years of French occupation are not correlated with potential proxies for economic development in the period before the French occupation. Moreover, in Subsection 4.3, we provide historical evidence that (unoccupied) Saxony was economically more developed than the Rhineland. Importantly, Prussia wanted to annex Saxony and not the Rhineland (and Westphalia), but against its intentions received the Rhineland and Westphalia instead of Saxony.

Second, we note that we cannot control directly for the distance to the Rhine or the Rhineland, since the first wave of occupation had the military aim of establishing the Rhine as the French-German border. In the later waves, the French then occupied parts east of the Rhine. Put differently, since the distance to the Rhine was the single most important determinant of the French occupation, we cannot use the French occupation as an instrument for *Institutions* while controlling for the single most important determinant of the French occupation, the distance to the Rhine. However, we can use the distance to the Rhine as an alternative instrument for the French occupation. Column 1 of Table A10 shows that the results are robust when using this alternative instrument.

Third, another test that we employ to address the concern that the parts of Germany west of the Rhine may have been special is to remove from the sample all counties occupied for 19 years, which are all counties west of the Rhine. As a result, in this test, we are only exploiting the later waves of occupation. The results in column 2 of Table A10 document that our results are robust to this specification. Alternatively, we can also observe that our results are robust to only including in the sample the areas west of the Rhine and all unoccupied parts of Germany. As can be seen in column 3 of Table A10, our results are also robust to this specification. In addition, we exclude from the sample the Prussian "Rhine Province", which covers most of the German territory west of the Rhine. The results are also robust to this specification (column 4 of Table A10).

Fourth, we use a slightly different approach to document that our results are robust to excluding the parts of Germany west of the Rhine from the sample. In Table A11, we gradually remove all counties that were located west of different major cities along the Rhine. We start by excluding all counties located west of Cologne in column 1, then we exclude all counties west of Koblenz in column 2, all counties west of Mainz in column 3, and all counties west of Worms in column 4. West of a city means that we take the longitude on which the respective city is located and exclude all counties west of this longitude. A county is west of the respective longitude if its main city (in most cases the city after which the county is named) is located west of this longitude. The number of observations that are excluded increases as we go from column 1 to column 4, since the cities are ordered according to the geographic location, with Cologne being furthest west and Worms furthest east (and all the cities being located along the Rhine). By imposing stricter restrictions on the sample, as we move from left to right in the table, we gradually reduce the number of treated counties. The results, reported in Table A11, show that the effect of *Institutions* remains significant in all four columns. In columns 1 and 2, the coefficient is of similar magnitude as in the unrestricted sample. In columns 3 and 4, the magnitude of the coefficient is much larger. Yet, we note that the explanatory power of our instrument, years of French occupation, decreases massively since we exclude most counties that were occupied for the longest amount of time. For example, in column 4, we only include three counties that were occupied for 19 years. Consequently, the F-statistic of the excluded instrument is only 4.68. Given the change in the explanatory power of the instrument due to the reduction of the number of treated counties, we have to be cautious when interpreting the different coefficient estimates. Nevertheless, the tests for the restricted samples suggest that the estimated effect of *Institutions* is not only driven by the main parts of the Prussian Rhine Province.

Overall, the results of these tests show that our results are robust to restricting the sample in various ways. We read this evidence as indicating that it is unlikely that other, non-observable factors, which are potentially correlated with the Rhineland (e.g. a specific Rhenish culture), explain the observed effect of *Institutions* on *Patents*.

Table A10: Distance to the Rhine and Excluding Some of the Occupied Counties

This table presents estimates of the impact of institutions on innovation for several different specifications. The dependent variable is patents per capita. We present second-stage estimates of the impact of institutions on innovation. In column 1, Institutions is instrumented by the log of the distance to the Rhine. In columns 2 to 4, Institutions is instrumented by the years of French occupation. In column 2, we exclude counties occupied for 19 years from the sample. In column 3, we exclude counties occupied for 3, 6, or 7 years from the sample. In column 4, we exclude the Rhine Province from the sample. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1)	(2)	(3)	(4)
	Patents	Patents	Patents	Patents
Institutions	0.726***	3.178**	0.286**	0.437*
	(0.000)	(0.015)	(0.011)	(0.083)
Adj. R^2	0.390	0.030	0.467	0.445
N	2,643	2,439	2,094	2,427
F-Stat. Ex. Instr.	41.10	2.49	140.29	10.82
Year FE	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.
Controls	Yes	All	Yes	Yes
Sample	All	Excl. 19 Years Occupation	Excl. 3, 6, 7 Years Occupation	Excl. Rhine Province
Instrument	ln(Distance to Rhine)	Years	Years	Years

Table A11: Excluding Parts of the Sample

This table presents estimates of the impact of institutions on innovation when parts of the sample are excluded. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. The dependent variable is patents per capita. In column 1, we exclude all counties west of Cologne from the sample. In column 2, we exclude all counties west of Koblenz from the sample. In column 3, we exclude all counties west of Mainz. In column 4, we exclude all counties west of Worms. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents
Institutions	0.454**	0.434*	1.357*	1.970*
	(0.035)	(0.100)	(0.094)	(0.078)
Adj. R^2	0.417	0.454	0.421	0.359
N	2,520	2,385	2,190	2,157
F-Stat. Ex. Instr.	27.56	20.10	6.08	4.68
Year FE	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes
Sample	Ex. west of Cologne	Ex. west of Koblenz	Ex. west of Mainz	Ex. west of Worms

Additional Specifications for the Effect of Steam Engines and Spinning Mills: In Subsection 5.2 of the paper, we argue that the concern about technology transferring from the French to the Germans would be most acute for technologies in which France might have had a competitive advantage relative to the German states: steam engines and mechanical cotton spinning mills. We therefore obtained additional data from the Zollverein census of 1861 to compute the number of mechanical cotton mills per million inhabitants that were in operation in each region in 1861 (*Spinning Mills 1861*), and the number of steam engines per million inhabitants within each region in 1861 (*Steam Engines 1861*). In column 1 of Table 5 of the paper, we jointly include both variables as additional controls. We now report estimates from additional specifications to underline the robustness of this result.

In column 2 of Table A12, we first present the results of the baseline estimation for the restricted sample, since data on steam engines and spinning mills is only available for the Zollverein member states, which excludes some territories in the North. The effect of *Institutions* remains almost unchanged compared to the full sample. In column 2 of Table A12, we add only *Steam Engines 1861* as an additional control variable, and in column 3 of Table A12, we add only *Spinning Mills 1861* as an additional control variable. While the effect for *Steam Engines 1861* is not significant, *Spinning Mills 1861* is significant at the 1% level. Importantly, the effect of *Institutions* remains large and significant. For comparison, column 4 includes both *Steam Engines 1861* and *Spinning Mills 1861,* which is the specification reported in column 2 of Table 5. Again, *Steam Engines 1861* is not significant, while *Spinning Mills 1861* is significant at the 1% level. Finally, in column 5 of Table A12, we test whether the coefficient of *Institutions* remains significant if we include the employment shares used in Subsection 5.1 of the paper to proxy for economic development (*Manufacturing+Mining Workforce %* and *Services Workforce %*), in addition to *Steam Engines 1861* and *Spinning Mills 1861.* The magnitude of the effect of *Institutions* decreases when we include the employment shares, which reflects the findings of Subsection 5.1, but the effect still remains large in economic terms and highly significant. In contrast, *Spinning Mills 1861* is no longer significant. Overall, these additional results strengthen the argument that the results are unlikely to be driven by French technology transfer.

Table A12: Alternative Explanation: Technology Import

This table presents estimates of the impact of institutions on innovation, controlling for potential technology import. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured using patents per capita. Institutions is instrumented by the years of French occupation. Steam Engines 1861 measures the number of steam engines that were in operation in 1861 per one million inhabitants. Spinning mills 1861 measures the number of mechanical spinning mills that were in operation in 1861 per million inhabitants. Manufacturing+Mining Workforce % (Services Workforce %) is the employment share of manufacturing and mining in % (private sector services in %). All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. For more details on the data, see Appendix B.

			Patents		
	(1)	(2)	(3)	(4)	(5)
Institutions	0.390***	0.331**	0.390***	0.343**	0.286**
	(0.001)	(0.020)	(0.001)	(0.015)	(0.049)
Steam Engines 1861		0.004		0.004	0.001
		(0.335)		(0.435)	(0.736)
Spinning Mills 1861			0.046***	0.041***	0.013
			(0.010)	(0.009)	(0.527)
Manufacturing+Mining					0.099
Workforce %					(0.239)
Services Workforce					0.906*
%					(0.056)
Adj. R^2	0.413	0.415	0.414	0.416	0.437
N	2,514	2,514	2,514	2,514	2,514
F-Stat. Ex. Instr.	62.33	46.45	61.71	47.67	46.98
Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes	Yes
Sample	Zollverein	Zollverein	Zollverein	Zollverein	Zollverein

A.4.3 Trade and Market integration

One potential concern, which we attempt to rule out, relates to the possibility that the French-occupied regions could have experienced a larger increase in international or domestic trade. In Subsection 5.3 of the paper, we present several pieces of evidence that alleviate this concern. We now present further evidence that an improvement in institutions, and not trade, is the most plausible driver of the findings.

In Table A13, we present additional statistics on German foreign trade. We show that between 1841 and 1910, France did not dominate the German import statistics (Panel A), nor the German export statistics (Panel B). Trade with France was less important than trade with other European countries like the United Kingdom, Austria-Hungary, Switzerland, or the Netherlands. We therefore argue that there is no empirical evidence that the French occupation fostered foreign trade with Germany in the long-run, nor that such French-German trade could drive the results.

As discussed in Subsection 5.3 of the paper, market integration is another potential channel though which the occupation could have fostered innovation, since the German territories were reorganized in the early 19^{th} century. In column 4 of Table 5, we address this concern by measuring the degree of potential gains from internal market integration using *Old territories/km²*. *Old territories* represents the number of independent territories that existed in 1789 within each region (Regierungsbezirk) of the sample, and *Old territories/km²* is *Old territories* divided by square kilometers. In addition, we use the dummy variable *Internal Border*, which is one if the county was at a state border in 1816 and if the neighboring state was part of the German Empire in 1871, and the dummy variable *Zollverein 1842* to control for Zollverein membership in 1842. In addition to the results presented in column 4 of Table 5, where *Old territories/km²*, *Internal Border* and *Zollverein 1842* are included in the regression model, we now report additional specifications in Table A14, in which these additional variables enter separately into the regressions.

In column 1 of Table A14, we find no significant effect for *Old territories,* and the coefficient of *Institutions* remains large and highly significant. In column 2 of Table A14, we find a significantly negative coefficient of *Old territories/km²*. The effect of *Institutions* is highly significant and the magnitude of the effect is even larger than in the baseline model. We include only *Internal Border* in column 3, and only *Zollverein 1842* in column 4 of Table A14. There is neither a significant effect of *Internal Border* nor a significant effect of *Zollverein 1842,* and the effect of *Institutions* is almost unchanged compared to the baseline model. In column 5 (6) of Table A14 we control for *Old territories* (*Old territories/km²*), *Internal Border,* and *Zollverein 1842* simultaneously. Again, the results are robust to these specifications.

Table A13: Alternative Explanation: International Trade

This table presents data on Germany's international trade statistics for the 1841 to 1910 period. Panel A reports the share of imports from Germany's main trade partners relative to total imports in percent. Panel B presents the share of exports relative to total exports in percent. Exports and imports for 1841 and 1851 include both Zollverein member and non-member states that became part of the German Empire in 1871. The category Others includes all countries for which no separate figures are available. For more information on the data source, see Appendix B.6.

Panel A: German Import Statistics

Year	1841	1851	1890	1900	1910
Austria-Hungary	13.3	9.4	14.0	12.0	8.5
Belgium	4.4	11.4	7.4	3.6	3.6
Denmark	1.3	1.2	1.4	1.2	1.8
France	8.4	6.1	6.3	5.1	5.7
Netherlands	19.6	20.0	7.2	3.6	2.9
Russia	2.8	3.6	12.7	11.9	15.5
Sweden	1.0	0.7	1.1	1.7	1.8
Switzerland	8.3	9.3	4.1	2.8	1.9
United Kingdom	23.1	25.4	15.0	13.9	8.6
USA	3.0	2.8	9.5	16.9	13.3
Others	14.9	10.1	21.3	27.3	36.3
Total	100	100	100	100	100

Panel B: German Export Statistics

Year	1841	1851	1890	1900	1910
Austria-Hungary	17.2	22.5	10.3	10.7	11.0
Belgium	3.0	9.1	4.4	5.3	5.2
Denmark	3.3	3.0	2.2	2.6	3.0
France	11.4	5.4	6.8	5.8	7.3
Netherlands	11.7	12.4	7.6	8.3	6.7
Russia	6.4	7.3	6.1	6.8	7.3
Sweden	1.4	1.4	2.7	2.9	2.5
Switzerland	11.2	9.2	5.3	6.1	6.1
United Kingdom	20.9	15.9	20.7	19.2	14.7
USA	1.3	4.9	12.2	9.2	8.5
Others	12.2	9.0	21.8	22.8	27.7
Total	100	100	100	100	100

Table A14: Alternative Explanations: Trade and Market Integration

This table presents estimates of the impact of institutions on innovation after controlling for different measures of trade and market integration. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured by patents per capita. Institutions is instrumented by the years of French occupation. Old territories represents the number of independent territories that existed in 1789 within each region (Regierungsbezirk). Old territories/km^2 is Old territories divided by square kilometers. Internal border (Zollverein 1842) is a dummy equal to one if a county was at a state border in 1816 and if the state was part of the German Empire in 1871 (if a county was in a Zollverein member state in 1842). All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	Patents					
	(1)	(2)	(3)	(4)	(5)	(6)
Institutions	0.349***	0.528***	0.383***	0.402***	0.369***	0.576***
	(0.004)	(0.000)	(0.001)	(0.000)	(0.002)	(0.000)
Old territories	0.157				0.129	
	(0.421)				(0.531)	
Old territories/km^2		-1.958*				-2.235**
		(0.059)				(0.043)
Internal Border			0.110		0.063	1.723
			(0.958)		(0.976)	(0.462)
Zollverein 1842				2.421	1.996	4.470*
				(0.215)	(0.380)	(0.075)
Adj. R^2	0.412	0.421	0.411	0.411	0.412	0.422
N	2,643	2,643	2,643	2,643	2,643	2,643
F-Stat. Ex. Instr.	59.02	46.53	63.88	89.83	83.77	60.81
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes	Yes	Yes

A.4.4 Human Capital

In Subsection 5.4 of the paper, we address the concern that regional differences in the endowment of human capital could drive the results. We now present additional robustness tests for the results reported in column 5 of Table 5, where we jointly add the variables *Printing Press*, *Illiteracy, University,* and *Technical University* to the baseline specification. More precisely, we now test in Table A15 whether these variables have an effect on the results when entering separately into the regressions. We also provide more context on the proxies for innovation that we use.

To start, we turn to the paper of Dittmar (2011). Dittmar documents the importance of the printing press for economic development. The printing press represented a supply-side shock to the price of books and thus a shock to the provision of education. Dittmar collects information on which localities adopted the printing press between 1450 and 1500. He relies on three data sources for this adoption of the printing press: Clair (1976), Febvre and Martin (1958), and ISTC (1998). For our tests, we collected the data from Clair (1976). Although this is only one of the three data sources used by Dittmar (2011), we believe this does not affect our results, since footnote 21 on page 1,143 of Dittmar (2011) argues that the three data sources provide highly overlapping information on the adoption of the printing press. In column 1 of Table A15, we add a dummy variable indicating that a printing press was adopted within the county borders between 1450 and 1500 to the baseline specification. The point estimate for this dummy variable is insignificant, while the coefficient for Institutions is essentially unaffected.

Next, we turn to the paper by D'Acunto (2015), who analyses the effect of basic education on innovation in a European setting. He shows that basic education persists for decades and measures it with literacy rates around 1880 in Europe. In the case of Germany, he uses (il)literacy data from 1871. We have collected the same illiteracy data for Germany as that used in D'Acunto (2015) and refer to his data section, which starts on page 7 of his paper. In column 2 of Table A15 we control for the illiteracy rate, which is defined as the share of the population aged 10 or older that is illiterate. The point estimate for this illiteracy variable is insignificant, while the coefficient for *Institutions* barely changes.

In column 3 of Table A15, we control for contemporaneous universities (at the time when the patents where filed, 1890, 1900, and 1910) and for contemporaneous technical universities (indicating whether a technical university or mining academy was located in a county in the respective year). We control specifically for the presence of technical universities, since these universities trained engineers who may have been more likely to innovate than graduates from the non-technical sciences. Every technical university is also coded as a university. The point estimates for University and Technical University are both insignificant, while the coefficient for Institutions is basically unchanged.

In column 4 of Table A15, we control for the printing press, university and technical university dummies and for the illiteracy variable. None of these four proxies for education is significant, while the coefficient for *Institutions* barely changes.

Table A15: Alternative Explanations: Human Capital

This table presents estimates of the impact of institutions on innovation after controlling for human capital. We present second-stage instrumental variable estimates of the impact of institutions on innovation, as measured by patents per capita. Institutions is instrumented by the years of French occupation. Printing press is a dummy that is one if a printing press was adopted in a county between 1450 and 1500. Illiteracy is defined as the share of the population aged 10 or older that is illiterate. University is one if a university was located within a county in the respective year. This includes general universities, technical universities, mining academies, medical universities and higher trade colleges. Technical University is one if a technical university or mining academy was located in the county in the respective year. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the province level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more detail on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents
Institutions	0.383***	0.369***	0.384***	0.369**
	(0.001)	(0.002)	(0.000)	(0.011)
Printing Press	-0.937			-2.935
	(0.816)			(0.333)
Illiteracy		-3.918		-3.490
		(0.363)		(0.448)
University			1.588	2.977
			(0.751)	(0.589)
Technical University			-3.620	-7.196
			(0.612)	(0.380)
Adj. R^2	0.411	0.415	0.411	0.416
N	2,643	2,556	2,643	2,556
Year FE	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.
Sample	All	Literacy Data	All	Literacy Data
Controls	Yes	Yes	Yes	Yes

A.4.5 Discrimination against and Persecution of the Jewish Population

In this subsection, we extend the analysis on whether the discrimination against and persecution of the Jewish population drive the effect of institutions on innovation.

Before we describe our data on the persecution of and discrimination against Jews, we want to lay out why there are good reasons to doubt that our results might be driven by this factor. D'Acunto et al. (2019) convincingly document that antisemitism lowers the demand for financial services and is associated with higher distrust in financial markets. They also document that antisemitism was frequent in the western part of Germany. This corresponds to the part of Germany with more inclusive institutions. Therefore, it appears that the "treatment areas" of our paper and those of D'Acunto et al. (2019) are overlapping. Importantly, however, the "treatment" of D'Acunto et al. (2019)–antisemitism–will intuitively have a negative effect on innovation or, at best, no effect on innovation. In contrast, we document that more inclusive institutions have a positive effect on innovation. While the treatment areas overlap, the treatments arguably have opposing effects on innovation. Thus, it seems unlikely that the persecution of Jews or antisemitism is driving our results. Rather, it seems likely that the persecution of Jews may have weakened our results and that we find results *despite, not because of*, the areas with better institutions also being more antisemitic and less open to minorities.

Even though, conceptually, antisemitism would likely work against our findings, we conduct additional tests to show that the discrimination against and persecution of Jews are unlikely to explain our results. We follow D'Acunto et al. (2019), who use data on the persecution of Jews from Voigtländer and Voth (2012).

We construct a variable that is one if any of the different pogrom variables provided by Voigtländer and Voth (2012) indicate that there was a pogrom in a county. Column 1 of Table A16 shows that *Pogrom* has a positive and significant effect, while the point estimate for *Institutions* is slightly muted, when compared with the basic specification. As discussed above, it seems implausible that places that were more antisemitic were, all else equal, more innovative. We speculate that the reason why counties with a pogrom might have been more innovative is that pogroms might have been likelier to occur in places where the Jewish community was particularly successful in economic and business terms. Such places might also have been particularly innovative.

Next, Voigtländer and Voth (2012) provide several variables on medieval Jewish settlements. We construct the dummy variable *Medieval Jewish Settlement* that is one if any of the different variables provided by Voigtländer and Voth (2012) for medieval Jewish settlements indicate that there was a medieval Jewish settlement in a county. Column 2 of Table A16 shows that the effect of *Medieval Jewish Settlement* is insignificant, while the effect of *Institutions* is positive and significant.

Antisemitism and the persecution of Jews could have affected the settlement patterns of the Jewish population in Germany, which in turn could have affected innovation. Therefore, we use census data to control for the share of Jewish people per 1,000 inhabitants in 1890, 1900, and 1910 (*Jewish Population per 1,000*). The data is available at the province level for Prussia and at the state level for most other German states. In addition, we use a second variable for the Jewish population share: *Jewish Population per 1,000 (Alternative)*. The advantage of this measure is that it is available at the region level, while the disadvantage is that it is only available for 1890. When using this measure, we assume that the fraction of the population in a region that is Jewish has stayed constant from 1890 to 1900 and 1910. The correlation of the two variables is 70% for the whole sample and 74% if we compute the correlation of the two variables for the year 1890 only. Columns 3 and 4 of Table A16 indicate that the coefficient of Institutions is highly robust to the inclusion of the *Jewish Population per 1,000* (column 3) and the *Jewish Population per 1,000 (Alternative)* (column 4) as controls. The coefficients of *Institutions* are positive and highly significant.

In column 5 (6) of Table A16, we jointly control for *Pogrom*, *Medieval Jewish Settlement*, and *Jewish Population per 1,000 (Jewish Population per 1,000 (Alternative))*. Our results are robust to these specifications.

Next, we provide additional evidence against the concern, approaching the issue from a different angle. For the Jewish minority, discriminatory laws remained in place even after the introduction of inclusive institutions, and in most German states, the Jewish population did not get full civil rights until the late 1860s. The emancipation of Jews could affect our results by, for instance, causing migration by the Jewish population within Germany. Therefore, we create a modified institutions index that also includes the Jewish emancipation as an additional reform. Information on the reform year is from Brenner et al. (1996) and Diekmann (2013). Column 7 of Table A16 documents that our results are highly robust to the use of this modified institutions index.

Taken as a whole, the additional analysis that we conduct suggests that the discrimination against and persecution of the Jewish population are unlikely to provide an alternative explanation for the observed effect of institutions on innovation.

Table A16: Alternative Explanations: Discrimination against and Persecution of the Jewish Population

This table presents estimates of the impact of institutions on innovation, controlling for the discrimination against and persecution of the Jewish population. We present second-stage estimates of the impact of institutions on innovation, where Institutions is instrumented by the years of French occupation. The dependent variable is patents per capita. Pogrom indicates counties in which pogroms occurred. Medieval Jewish Settlement indicates counties with a Jewish settlement in medieval times. Jewish Population per 1,000 measures the number of Jewish people per 1,000. Jewish Population per 1,000 (Alternative) measures the number of Jewish people per 1,000 based on an alternative data source. Alternative Institutions II includes Jewish emancipation (the year when Jews got full civil rights) as an additional reform. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents	(5) Patents	(6) Patents	(7) Patents
Institutions	0.326***	0.351***	0.366***	0.395***	0.308***	0.333***	
	(0.005)	(0.003)	(0.001)	(0.000)	(0.005)	(0.004)	
Pogrom	4.174**				4.698**	4.707**	
	(0.017)				(0.024)	(0.017)	
Medieval Jewish		2.583			-0.571	-0.171	
Settlement		(0.109)			(0.748)	(0.926)	
Jewish Population			-0.412		-0.411		
per 1,000			(0.220)		(0.212)		
Jewish Population				-0.194		-0.208	
per 1,000 (Alternative)				(0.409)		(0.360)	
Alternative							0.856***
Institutions II							(0.003)
Adj. R^2	0.415	0.413	0.418	0.413	0.421	0.417	0.334
N	2,643	2,643	2,643	2,643	2,643	2,643	2,643
F-Stat. Ex. Instr.	58.81	61.02	64.89	68.18	60.18	62.26	13.22
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	All	All	All

A.4.6 Financial Development

In this section, we provide more comprehensive evidence than in the main body of the paper that financial development does not affect our results. One concern is that individual inventors or firms may have incentives to file patents if doing so can improve access to external capital (e.g., if patents could be used as collateral). However, for the period of investigation, we have no quantitative evidence on the use of patents as collateral. The most relevant academic work on this topic is Burhop (2010), which analyzes the transfer of patents in Imperial Germany but gives no information on the role of patents used as collateral. While there is an old literature that argues that the provision of credit was important for German industrialization, highlighting the role of the large universal banks (see the seminal study of Gerschenkron (1962)), more recent studies question the importance of debt financing and argue that equity financing via stock markets had a more prominent role, in particular for innovative firms (see, e.g., Lehmann-Hasemeyer and Streb (2016)). If debt was less relevant for corporate financing, the use of patents as collateral would also have been less relevant, even though we have no data to empirically test the use of patents as collateral.

Next, we document that our results are robust to controlling for various proxies for financial development. We first provide evidence that the effect of inclusive institutions is not driven by persistent differences in financial development that existed even before the French occupation by using the variable *Old Financial Center* (see column 1 of Table A17). *New Financial Center* is a dummy variable indicating the location of an active stock exchange during the period of observation, and *Banking Workforce in %* is the percentage of the workforce employed in banking. By controlling for *Banking Workforce in %* we aim to account for differences in the ability to obtain bank loans, since a high share of employees in banking reflects a high density of banks. If the institutional change induced by the French occupation fostered the emergence of stock exchanges, banks, and financial development in general, then higher financial development would be an alternative channel explaining the observed increase in innovation. To address this concern, we include *New Financial Center* and *Banking Workforce in %* as additional control variables (see columns 2 and 3 in Table A17, respectively). When we control for these variables, the effect of Institutions on patents per capita remains positive and highly significant. In column 4 of Table A17, we control for all 3 proxies for financial development simultaneously. The effect of Institutions on patents per capita remains positive and highly significant.

Finally, with regard to the concern that the potential use of patents as a collateral may have raised the incentives for patenting, but not necessarily for innovation, we also note that we use world's fair exhibits as an alternative proxy for innovation. When using world's fair exhibits per million inhabitants as the outcome variable, the effect of Institutions remains positive and significant (see Subsection 5.9).

These findings make it less likely that financial development or the use of patents as collateral can explain our results. For details on the data for the 3 proxies of financial development, see

Appendix B.5.14.

Table A17: Alternative Explanations: Financial Development

This table presents estimates of the impact of institutions on innovation, controlling for financial development. We present second-stage instrumental variable estimates of the impact of institutions on innovation, as measured by patents per capita. Old Financial Center indicates financial centers that existed before the French occupation. New Financial Center is a dummy variable indicating the location of an active stock exchange during the period of observation. Banking Workforce in % is the percentage of the workforce employed in banking. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents	(2) Patents	(3) Patents	(4) Patents
Institutions	0.385***	0.381***	0.403***	0.401***
	(0.001)	(0.001)	(0.003)	(0.004)
Old Financial Center	4.682			-3.450
	(0.524)			(0.577)
New Financial Center		11.149*		7.584
		(0.090)		(0.226)
Banking Workforce in %			3.272***	3.093***
			(0.001)	(0.003)
Adj. R^2	0.412	0.418	0.491	0.492
N	2,643	2,643	1,758	1,758
F-Stat. Ex. Instr.	64.35	64.43	64.17	63.95
Year FE	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes
Sample	All	All	All (2 Years)	All (2 Years)

A.4.7 Differences in Patent Law and Validity of Patents as a Proxy for Innovation

In Subsection 5.9, we introduce world's fair exhibits as an additional proxy for innovation. We show that there is a significantly positive effect of *Institutions* on *Exhibits,* which measures the number of world's fair exhibits originated in a given county per million inhabitants. We now extend the analysis by reporting three additional specifications in Table A18 that underline the robustness of this result. For comparison, column 1 of Table A18 shows the effect of *Institutions* on *Exhibits* (world's fair exhibits per million inhabitants), as it is also shown in the main part of the paper.

In column 2 of Table A18, we use *High-tech Exhibits* as the outcome variable. *High-tech Exhibits* is the number of exhibits in the chemical industry and electrical engineering per million inhabitants. The literature considers the chemical industry and the electrical-engineering industry to be the leading sectors of the "second industrial revolution" at the end of 19^{th} century and in the early 20^{th} century. Innovation in these industries was particularly important for long-run productivity growth (e.g., the electrification of production processes allowed productivity gains in manufacturing, and innovation in the chemical industry included nitrogen fertilizer, increasing agricultural productivity). The estimated coefficient for the effect of *Institutions* on *High-tech Exhibits* is positive and significant. This finding is consistent with the effect of *Institutions* on high-tech patents (which we define analogously) that we discuss in Section 7. The stronger effect for high-tech innovation (proxied with patents or world's fair exhibits) suggests that inclusive institutions were particularly important for the development of these modern industries, which were key drivers of economic growth.

In column 3 of Table A18, we show that the effect of *Institutions* on innovation is not entirely driven by these high-tech industries, but rather broadly based, by including all non high-tech exhibits per million inhabitants (*Non High-tech Exhibits*) in the regression (*Non High-tech Exhibits* is constructed by excluding *High-tech Exhibits* from *Exhibits*). We find a positive and significant effect of *Institutions* on *Non High-tech Exhibits*, suggesting that inclusive institutions fostered innovation in a wide range of industries.

Finally, in column 4 of Table A18, we again use *Exhibits* (all exhibits per million inhabitants) as the outcome variable, but we restrict the sample to Prussia in its 1816 borders (the territory that belonged to Prussia in 1816). This test is motivated by the fact that there were differences and changes in patent law before the 1877 patent-law harmonization, which might have affected not only patenting but also innovation in general. To rule out historical differences in patent-law as the main driver of the results, we restrict the sample to Prussia in its 1816 borders, since there were no patent law differences within this territory before 1877. In column 4 of Table A18, when using the restricted sample, the effect of *Institutions* on the number of world's fair exhibits per million inhabitants is positive and highly significant. Therefore, we conclude that pre-1877 differences and changes in patent laws could have affected innovation, but are unlikely to explain the effect of inclusive institutions on innovation that we show in this paper.

Table A18: Alternative Explanations: Differences in Patent Law and Validity of Patents as a Proxy for Innovation

This table presents estimates of the impact of institutions on innovation. We present second-stage instrumental variable estimates of the impact of institutions on innovation, measured with world's fair exhibits per million inhabitants. Institutions is instrumented by the years of French occupation. We show the results for world's fair exhibits per million inhabitants in column 1, for high-tech exhibits per million inhabitants in column 2, and for all non-high-tech exhibits per million inhabitants in column 3. In column 4, we show the result for world's fair exhibits per million inhabitants when restricting the sample to Prussia in its 1816 borders. All control variables from Table 3 are included but not displayed. The observations are weighted by county population. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Exhibits	(2) High-tech Exhibits	(3) Non-high-tech Exhibits	(4) Exhibits
Institutions	0.580**	0.063**	0.517*	0.938***
	(0.025)	(0.023)	(0.056)	(0.000)
Adj. R^2	0.307	0.128	0.293	0.544
N	1,762	1,762	1,762	740
F-Stat. Ex. Instr.	64.47	64.47	64.47	482.21
Year FE	Yes	Yes	Yes	Yes
Cluster	Region	Region	Region	Region
Weighting	Populat.	Populat.	Populat.	Populat.
Controls	Yes	Yes	Yes	Yes
Sample	All (2 Years)	All (2 Years)	All (2 Years)	Prussia (2 Years)

A.5 The Geographical Distribution of Trade Courts

One distinguishing factor across different parts of Germany is the ease of access to trade courts. While 68 counties in our sample had a trade court, these were not homogeneously distributed. Of the 25 states in our sample, 14 states did not have a single trade court, including, for instance, Anhalt with a population of more than 300,000 in 1900, Mecklenburg-Schwerin with a population of more than 600,000 in 1900, or Oldenburg with a population of about 400,000. The state of Württemberg with a population of more than two million in 1900 had only one trade court. Neighboring Baden with a slightly smaller population had two trade courts, although both were located in the north of the state. In contrast, Bavaria with a population of 6.2 million in 1900 had 16 trade courts, which compares favorably with the 32 trade courts in Prussia for 34.5 million inhabitants in 1900. Hesse with a population of about 1.1 million in 1900 had 5 trade courts. Notably, seven out of the eight states that were (partly) occupied had at least one trade court, with the exception of Oldenburg. 14 out of 21 that were not (fully) occupied did not have a single trade court (Bavaria, Hesse, Oldenburg, and Prussia were partly occupied).

In addition to the considerable variation in trade courts across states, there is also substantial variation in access to trade courts within states. Out of the 32 trade courts in Prussia, ten were in the Rhine Province (5.8 million people in 1900), while five were in Westphalia (3.2 million people in 1900). In contrast, some populous Prussian provinces had one trade court or no trade court at all: Brandenburg (0 trade courts, 3.1 million inhabitants in 1900), Posen (one trade court, 1.9 million people in 1900), Silesia (one trade court, 4.7 million inhabitants in 1900), Schleswig-Holstein (one trade court, 1.4 million inhabitants). Similar differences in access to trade courts can be found when the presence of trade courts is considered at a regional instead of a state or provincial level.

A.6 Measuring Institutional Quality

As a measure of institutional quality, we use an index that was proposed by Acemoglu et al. (2011a). Acemoglu et al. use this index to analyze the effect of institutional reforms on urbanization. This approach is criticized by Kopsidis and Bromley (2016a). Kopsidis and Bromley argue that the index used by Acemoglu et al. (2011a) underestimates the institutional quality of non-occupied territories compared to occupied territories, and provide a list of alternative years of reform implementation. However, we use the original coding, since, as we illustrate below, the coding of Kopsidis and Bromley artificially and arbitrarily overestimates institutional quality in non-occupied territories. In addition, Kopsidis and Bromley do not use strict and consistent criteria to identify the years of reform implementation, nor do they provide references that allow the verification of their coding (see their brief online appendix, Kopsidis and Bromley (2016b)). In contrast, Acemoglu et al. (2011a) describe in detail both the criteria used to define the year of reform implementation and the sources for each individual reform (see the online appendix of their paper, Acemoglu et al. (2011b)). The following examples illustrate the general shortcomings in the approach of Kopsidis and Bromley, who artificially and arbitrarily overestimate institutional quality in non-occupied territories:

(1) Introduction of the *Code civil*: Kopsidis and Bromley (2016a) argue that the Prussian October Decree (Oktoberedikt) of 1807 marked the introduction of a civil code in the eastern provinces of Prussia, while Acemoglu et al. (2011b) use 1900, the year when the nationwide German Civil Law (Bürgerliches Gesetzbuch) was introduced. The latter is motivated by the fact that under the General State Laws for the Prussian States (Allgemeines Landrecht für die Preußischen Staaten), which existed until 1900, people were not treated equally before the courts. Thus, taking 1807 as the reform year overestimates the institutional quality of these provinces, since highly discriminatory legal institutions had not yet been abolished, in comparison with the territories that had been under French rule. In particular, patrimonial justice persisted in the eastern Prussian provinces until 1849. The local lords of the manor also lost their police powers in 1849, but this privilege was officially reintroduced in 1853, at least in some parts of Prussia (see in particular Werthmann (1995) for the dissolution of patrimonial courts in German states). The special police jurisdiction of the local lords of the manor in Prussia was not finally abolished until 1927 (Clark (2007), pp. 650f). Moreover, there are detailed historical case studies on the society and the daily lives of people in these provinces that point out the persistence of non-inclusive institutions during the 19th century (see, for example, Wagner (2005)). Overall, the French *Code civil* provided a superior legal system, not only regarding the more equal treatment of people but also because of a stronger endorsement of private property rights (Clark (2007), p. 406). Therefore, we use 1900 as the year when nationwide German Civil Law was introduced. Nevertheless, we account for this concern using the variable *Alternative Institutions I* (see Subsection 4.4), which includes the

abolition of patrimonial courts as an additional measure of institutional quality.

(2) Dissolution of guilds: For the Kingdom of Württemberg, Kopsidis and Bromley (2016a) suggest 1828 as the year when guilds were abolished. In their appendix, they justify this by the fact that the Common Trade Regulation Act (Allgemeine Gewerbeordnung) was introduced in 1828. This trade act caused the dissolution of some guilds, but there is strong evidence from historical studies that the remaining guilds still had a negative impact on economic development. Furthermore, as we have argued in Subsection 2.3, even sectors that were not regulated by guilds were heavily restricted, since trade licenses were necessary to establish a business. The allocation of trade licenses was very restrictive and, in the case of Württemberg, this system was also used to protect the interests of powerful guilds (see, for example, Arns (1986) or Fischer (1962) for the restrictiveness of the trade license system). Since a strict definition is more appropriate, we follow the approach of Acemoglu et al. (2011a) and use the year when commercial freedom (Gewerbefreiheit) was established (in the case of Württemberg, 1862).

(3) Serfdom and agricultural reforms: Kopsidis and Bromley (2016a) argue that the abolition of serfdom is an economically meaningless reform, and therefore should not be considered a measure of institutional quality in the 19^{th} century, since serfdom was no longer practiced in the western parts. In the late 18^{th} century, serfdom was indeed less strict than in earlier periods in the western regions. However, under the manorial system, the lives of the serfs were still restricted to the extent that they owed duties to their lord of the manor. Depending on how serfdom was organized locally, the people had to pay monetary contributions, deliver payments in kind, or fulfill work obligations (see, for example, Achilles (1993)). These measures in turn restricted both social and labor market mobility. The formal abolition of serfdom was the first step in a series of agricultural reforms in which the former serfs were relieved of these duties. In territories where serfdom was abolished earlier, other agricultural reforms could also be implemented earlier. The year when a law was finally implemented to regulate the redemption of feudal lands, which reflects the reform measure 'agricultural reforms' in Acemoglu et al. (2011b), marks the end of this reform process. However, it is worth pointing out that excluding this variable does not affect the results. In unreported robustness checks, we computed an institutions index that only includes the introduction of the *Code civil* and the dissolution of guilds, and we also computed an index that only includes the abolition of guilds. In both cases, the results suggest an economically and statistically strong effect of institutions on innovation. However, as we argue in the main body of the paper, using only one institution (or two institutions) would violate the exclusion restriction that the instrument (*Years French Occupation*) is correlated with the variable of interest (one individual institution), but uncorrelated with any other determinants of the dependent variable (the other institutions).

Even though the index of Acemoglu et al. (2011a) is well documented and historically more accurate than the one advocated by Kopsidis and Bromley (2016a), we show that our results hold when using this inferior index. In Table A19, we replace our index of institutional quality with an index that is based on the reform years proposed by Kopsidis and Bromley (2016a) (which, consequently, also does not include the abolition of serfdom as proposed by these authors). As in our

Table A19: A Further Measure of Institutional Quality

This table presents estimates of the impact of institutions on innovation using a further alternative measure of institutional quality, KB Institutions, based on Kopsidis and Bromley (2016a). We present second-stage instrumental variable estimates of the impact of institutions on innovation, as measured by patents per capita. KB Institutions is instrumented by the years of French occupation. All control variables from Table 3 are included but not displayed. All remaining variables are defined in Table 1. The observations are weighted by county population. Standard errors are clustered at the regional level. P-values are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. For more details on the data, see Appendix B.

	(1) Patents
KB Institutions	1.440**
	(0.041)
Adj. R^2	0.169
N	2,643
F-Stat. Ex. Instr.	3.58
Year FE	Yes
Cluster	Region
Weighting	Populat.
Controls	Yes
Sample	All

main regression model, we take the years of French occupation as an instrument for institutional quality, which we now measure with a different index using the information from Kopsidis and Bromley (2016a). We name this alternative index *KB Institutions*. The estimated results show that the effect of *KB Institutions* (instrumented with *Years French Occupation*) on patents per capita is even larger than our baseline estimate in column 2 of Table 3, while the point estimate is significant at the 5% level. However, as explained above, the results based on the index by Kopsidis and Bromley (2016a) might be biased because of an inaccurate measure of institutional quality.

B Description of Data and Variables

B.1 Structure of the Data Set

B.1.1 General Structure

The structure of the data set reflects the administrative structure of the German Empire, which consisted of 25 federal states. Prussia was by far the most important federal state, accounting for a population share of 61.2% in 1900 (see Deutsches Reich (1903a) for population figures). It was subdivided into provinces (Provinzen), regions (Regierungsbezirke), and counties (Kreise). The medium-sized states (e.g. the Kingdom of Bavaria) were organized into regions and counties, while the smaller principalities were only subdivided into counties. We use county data, the lowest level for which the census publications of the Imperial Statistical Office provide information. To account for potential correlation within regions, standard errors are clustered at the regional level in all regressions. Every small state that was not subdivided into regions is treated as an independent region. The data set includes all 25 German federal states (excluding the Prussian exclave Hohenzollern, with only 66,780 inhabitants in 1900). Alsace-Lorraine, which was annexed in 1871 after the French defeat in the Franco-German War, is excluded since it was not a federal state, but a territory with a minor status (Reichsland), directly subordinated to the German Emperor, where the inhabitants had less rights than the citizens of the federal states, and it was ruled by an Imperial governor coming from outside Alsace-Lorraine, who was often a high-ranking Prussian officer (see Wehler (1995), p. 325 and pp. 1012-1014).

The county borders changed over time as a result of administrative reforms. In most cases, the size of the counties was reduced, so the number of counties increased. The data set represents the administrative structure in the year 1882, since this is the earliest year for which we use county data from the official census publications (we use data from the employment census of 1882 to control for employment by sector in Table 5 and Table A8). In order to use the same balanced panel in all regressions, we match the counties in all subsequent years to the administrative structure in the year 1882 (for example: Witten (city) was separated from Bochum in 1899. Thus, we merged Witten (city) and Bochum in order to maintain the structure of 1882). Furthermore, for some smaller German federal states (e.g. the Principality of Lippe), some data was only available on a higher aggregated level, meaning that we had to merge counties. After all adjustments, the data set includes 881 county-level observations per year.

B.1.2 Definition of West vs. Saxony

In column 1 of Table 4, we use the sub-sample West vs. Saxony to compare economically leading regions in the western and eastern parts of Germany. We include the Prussian Rhine Province (except for the county of St. Wendel which only became Prussian in 1834), the Prussian Province of Westphalia, and the Kingdom of Saxony (in its pre-1815 borders) in this sub-sample. Due to the complicated territorial structure in this part of Germany with dozens of exclaves, enclaves and even counter-enclaves (enclaves of one state in an enclave of another state), we approximate the (pre-1815) borders of the Kingdom of Saxony with the territories of the Kingdom of Saxony post-1815, the Prussian province of Saxony, and those Thuringian states that gained territory, which Prussia had claimed, at the expense of Saxony at the Congress of Vienna (Grand-Duchy of Saxe-Weimar-Eisenach, Duchy of Saxe-Altenburg, Principality of Reuss, Younger Line, and Principality of Reuss, Older Line).

B.1.3 Definition of East Elbia

In column 2 of Table 4, we use a sub-sample which excludes East Elbia (Ostelbien), the eastern Prussian provinces that were dominated by agriculture. In the historical literature, this geographical area is typically associated with economic backwardness, a high landownership concentration, and extractive institutions (see, for example, Eddie (2008) and Wagner (2005)). We define the following Prussian provinces as part of East Elbia: Brandenburg, Silesia, Pomerania, Posen, West Prussia, and East Prussia. Note that we do not treat the German capital Berlin, which was administered as a separate province, as part of East Elbia, since it lacks the relevant socioeconomic features characterizing East Elbia.

B.1.4 Exclaves and Enclaves

In Subsection 4.4 and Appendix B.1.4, we discuss the extent to which the estimated results are affected by non-occupied enclaves surrounded by occupied territories, or occupied exclaves surrounded by non-occupied territories.

The following territories are coded as *enclaves*, since they were not under French rule: Grand Duchy of Hesse-Darmstadt (Oberhessen and Arnsberg with surrounding counties), Duchy of Nassau, Principality of Lippe, Principality of Schaumburg-Lippe, Principality of Waldeck and Pyrmont.

The following territories are coded as *exclaves*, since they were (temporarily) under French rule: Prussian province of Bayreuth (occupied by the French as a result of the 1807 Treaty of Tilsit; France ceded this territory to the Kingdom of Bavaria in 1810 in the course of a territorial exchange), Erfurt (Prussian territory since the German Mediatization of 1803; occupied by the French as a result of the 1807 Treaty of Tilsit), Schmalkladen (exclave of the Electorate of Hesse; occupied by the French in 1807, when the whole Electorate of Hesse came under French rule).

When we "smooth" the borders, we drop the following territories (code the following counties as not occupied): Free City of Lübeck, Duchy of Lauenburg, Prussian territories in Saxony (including the following counties: Calbe, Halle an der Saale (city), Mansfelder Seekreis, Mansfelder Gebirgskreis, Saalkreis).

B.1.5 Definition of City Counties vs. Rural Counties

In Section 6, we estimate the effect of *Institutions* separately for city counties and rural counties. A city county included only one larger city but no villages or smaller cities of the surrounding area, while we use the term rural county for all other counties. Note that the administrative structure differed across the federal states of the German Empire. Some states did not distinguish between city and rural counties (e.g. Baden); some states established city counties only for the capital or very large cities (e.g. in Württemberg); and in some cases we had to merge city counties with rural counties to construct a balanced panel, since some city counties were founded after 1890. In Section 6, we restrict the sample to counties located in Prussia, Bavaria, and Oldenburg. Moreover, for Bavaria we only include the main part of Bavaria (which is almost identical to the present state of Bavaria in Germany) in the sample, and not the Palatinate (which is today part of the German state Rhineland-Palatinate). The Palatinate, which was an exclave of Bavaria, had a very different county structure to the rest of Bavaria with no city counties. In the aforementioned states, a relatively large number of city counties existed, so we consider city counties a good proxy for urban areas. This is not the case for the excluded states, for the reasons mentioned above.

B.2 Institutions and French Occupation

B.2.1 Institutions Index

The index of institutional quality (*Institutions*) is based on four measures of institutional quality: the introduction of the *Code civil*, the abolition of serfdom, the implementation of agrarian reforms and the dissolution of guilds. We determine the year of reform implementation according to Acemoglu et al. (2011a). For the following German states, the reform index is based on information published in their online appendix (Acemoglu et al. (2011b)): **Kingdom of Prussia**, **Kingdom of Bavaria**, **Palatinate** (Bavarian exclave west of the Rhine), **Kingdom of Saxony**, **Kingdom of Württemberg**, **Grand Duchy of Baden**, **Grand Duchy of Brunswick**, **Grand Duchy of Hesse-Darmstadt**, and **Grand Duchy of Mecklenburg-Schwerin**. Since we use county-level data, we adjust and extend the data for some territorial entities. The information about the political affiliation of counties or border changes is based on various maps that are published online on the server for digital historical maps at the Leibniz Institute of European History in Mainz (IEG-MAPS) (link: http://www.ieg-maps.uni-mainz.de, last accessed May 25, 2021). Furthermore, we have collected additional information about the year of reform implementation for all small federal states that are not considered by Acemoglu et al. (2011a). In the following, we document all changes compared to the original coding of Acemoglu et al. (2011a), as well as the sources for information on all newly added territories:

Kingdom of Prussia:

(a) Provinces of Posen and West Prussia: Acemoglu et al. (2011a) do not include the Prussian provinces of Posen and West Prussia in their data set. Since institutional reforms were implemented in the same period as in the Prussian mainland, we use the same institutions index value as for the Prussian provinces of Brandenburg, East Prussia, Pommerania, and Silesia, which are reported in Acemoglu et al. (2011a).

(b) Provinces of Brandenburg, Saxony, and Silesia: We adjust for those counties that formed the northern part of the Kingdom of Saxony before 1815. These counties were annexed by Prussia and became part of the Prussian provinces of Brandenburg, Saxony, and Silesia. Institutional change did not take place before 1815 in these counties. We therefore adjust the reform index accordingly.

(c) Province of Hesse-Nassau: As a result of its victory in the Austro-German War, Prussia annexed several territories (Electorate of Hesse, Duchy of Nassau, Landgraviate of Hesse-Homburg, and Free City of Frankfurt am Main), which constituted the Prussian province of Hesse-Nassau in the subsequent years. Acemoglu et al. (2011a) report reform data for the northern part (former Electorate of Hesse), but not for the region in the South-West (former Duchy of Nassau). Information about the years of reform implementation for the missing part is from Schüler (2006). Furthermore, we adjust the values for the county of Biedenkopf, which belonged to Hesse-Darmstadt until 1866, and thus differed in terms of the years of reform implementation.

(d) Rhine Province: The main part of the province is located west of the Rhine, but there were also several counties on the east side (core territory of the Grand Duchy of Berg until 1815). Acemoglu et al. (2011a) use province-level data based on the institutions in the western part.

Since the years of reform implementation differ slightly for the counties on the east side, we use additional information from Klippel (1996) and Schubert (1977) to adjust the data. Furthermore, the counties of Altenkirchen and Neuwied belonged to Nassau until 1866, and thus differed in institutional quality. We use information from Schüler (2006) to get data for the years of reform implementation in Nassau (see also the comments in (b)).

(e) Province of Schleswig-Holstein: Information about institutional reforms in Schleswig-Holstein is reported in Acemoglu et al. (2011a). Lauenburg was part of the Kingdom of Hanover until 1866, and therefore the years of reform implementation differed. After the Prussian annexation of Hanover, Lauenburg became part of Schleswig-Holstein. We take this change into account and adjust the reform values. Information about reforms in Hanover, which we use for Lauenburg, is reported in Acemoglu et al. (2011a).

(f) Province of Westphalia: Until 1815, the southern part of the province belonged to Hesse-Darmstadt, where reforms were implemented in different years. We therefore adjust the reform data for the respective counties based on information about reform implementation in Hesse-Darmstadt.

Kingdom of Bavaria:

Acemoglu et al. (2011a) only consider the southeastern part of Bavaria (the mainland around Munich), and not the Frankonian and Swabian regions. Since we identify no differences in terms of the institutional quality (according to the definition described above), we use the same variables as for the southeastern part of Bavaria.

Grand Duchy of Hesse-Darmstadt:

The western part on the left side of the Rhine (Rheinhessen) became part of Hesse-Darmstadt as a result of the Congress of Vienna. Acemoglu et al. (2011a) do not treat this region separately. Rheinhessen was under French rule and thus implemented reforms earlier. Furthermore, institutions remained in place after the French withdrew. We therefore assign the same reform values for Rheinhessen as for the Palatinate.

Grand Duchy of Mecklenburg-Strelitz:

Mecklenburg-Schwerin and Mecklenburg-Strelitz were strongly linked in economic and political terms, so reforms were implemented in the same years and the same way (see Mast (1994), pp. 113-153). We therefore use the same years of implementation as for Mecklenburg-Schwerin (see Acemoglu et al. (2011a)).

Grand Duchy of Oldenburg:

Abolition of serfdom: Eckhardt and Schmidt (1987), pp. 717-719; agricultural reforms: Eckhardt and Schmidt (1987), pp. 717-719; dissolution of guilds: Eckhardt and Schmidt (1987), p. 354; *Code civil*: Klippel (1996). Besides its main territory in the north-west of Germany, Oldenburg possessed two small exclaves: Birkenfeld (in the Rhineland) and the Principality of Lübeck (north of the independent city of Lübeck). The introduction of some reforms differed for both exclaves, in particular for the territory of Birkenfeld, which was under French occupation for 19 years. We thus adjusted the corresponding index values based on information in Eckhardt and Schmidt (1987) and Schubert (1977).

Grand Duchy of Saxe-Weimar-Eisenach:

Abolition of serfdom: Patze and Schlesinger (1978), p. 41; agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Duchy of Anhalt:

The Duchy was created in 1863 by the unification of Anhalt-Dessau and Anhalt-Bernburg. We treat the preceding territories separately since the years of reform implementation differed. We used the following sources: abolition of serfdom: Kraaz (1898), pp. 190-206 and p. 214; agricultural reforms: Kraaz (1898), pp. 218-223; dissolution of guilds: Norddeutscher Bund (1869); *Code civil*: Klippel (1996) and Schubert (1977).

Duchy of Saxe-Altenburg:

Abolition of serfdom: Patze and Schlesinger (1978), p. 41; agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Duchy of Saxe-Coburg-Gotha:

Abolition of serfdom: Patze and Schlesinger (1978), pp. 141-142; agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Duchy of Saxe-Meiningen:

Abolition of serfdom: Patze and Schlesinger (1978), p. 141; agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Principality of Lippe:

Abolition of serfdom and agricultural reforms: Arndt (1992), pp. 266-272; dissolution of guilds: Arndt (1992), p. 295; *Code civil*: Klippel (1996).

Principality of Reuss, Older Line:

Abolition of serfdom and agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Principality of Reuss, Younger Line:

Abolition of serfdom: Patze and Schlesinger (1978), p. 41; agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Principality of Schaumburg-Lippe:

Abolition of serfdom: Havliza (1975), p. 13-34; agricultural reforms: Schneider (1983); dissolution of guilds: Norddeutscher Bund (1871), p. 714; *Code civil*: Klippel (1996).

Principality of Schwarzburg-Rudolstadt:

Abolition of serfdom: Patze and Schlesinger (1978), pp. 141-142; agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Principality of Schwarzburg-Sondershausen:

Abolition of serfdom: Patze and Schlesinger (1978), pp. 141-142; agricultural reforms: Patze and Schlesinger (1978), p. 142; dissolution of guilds: Patze and Schlesinger (1978), p. 144; *Code civil*: Klippel (1996).

Principality of Waldeck and Pyrmont:

Abolition of serfdom and agricultural reforms: Seidel (1964), pp. 181-182; dissolution of guilds: Brand (2006), p. 97; *Code civil*: Klippel (1996).

Free and Hanseatic City of Bremen:

Abolition of serfdom: Schubert (1977), pp. 381-382; dissolution of guilds: Schulz (1995), p. 157; *Code civil*: Klippel (1996) and Schubert (1977), pp. 153-161. We have no information about agrarian reforms, since Bremen was a city state. Thus, the index of institutional quality is constructed over three institutions.

Free and Hanseatic City of Hamburg:

Abolition of serfdom: Schubert (1977), pp. 381-382; dissolution of guilds: Schulz (1995), p. 145; *Code civil*: Klippel (1996) and Schubert (1977), pp. 153-161. We have no information about agrarian reforms, since Hamburg was a city state. Thus, the index of institutional quality is constructed over three institutions.

Free and Hanseatic City of Lübeck:

Abolition of serfdom: Schubert (1977), pp. 381-382; dissolution of guilds: Endres (1926), p. 145; *Code civil*: Klippel (1996) and Schubert (1977), pp. 153-161. We have no information about agrarian reforms, since Lübeck was a city state. Thus, the index of institutional quality is constructed over three institutions.

B.2.2 Alternative Institutions Index

In column 6 of Table 4, we use an alternative index (*Alternative Institutions I*) to test whether the results are robust to different measures of institutional quality. *Alternative Institutions I* includes the reforms used in the original index (see B.2.1), as well as an additional reform: the year when patrimonial courts were abolished in the respective territory. Consequently, we compute the average index value over five reform measures. Information about the abolition of patrimonial courts is from Werthmann (1995), and information for all Thuringian states (Saxe-Weimar-Eisenach, Saxe-Altenburg, Saxe-Coburg-Gotha, Saxe-Meiningen, Reuss, Younger Line, Reuss, Older Line, Schwarzburg-Rudolstadt, and Schwarzburg-Sondershausen) is from Heß (1993), p. 64. We use additional sources for Oldenburg (Eckhardt and Schmidt (1987), pp. 352-353), Anhalt (Kraaz (1898), pp. 192-218), Schaumburg-Lippe (Havliza (1975), pp. 31-36), and Waldeck and Pyrmont (Seidel (1964), p. 182). Comparable patrimonial courts were not in operation in the city states (Bremen, Hamburg and Lübeck). Consequently, *Alternative Institutions I* only contains three reforms (abolition of serfdom, dissolution of guilds, and introduction of the *Code civil*) for these three city states.

B.2.3 French Occupation

We follow the approach of Acemoglu et al. (2011a) to determine the years of French occupation. A territory is defined as occupied if it was under direct French rule or under the rule of a French-controlled satellite state, which includes the Grand Duchy of Berg, the Kingdom of Westphalia, and the Grand Duchy of Frankfurt. These satellite states were all ruled by family members of Napoleon. The period of French occupation ranges between zero and 19 years. Since Acemoglu et al. (2011a) only provide data on the state level (on the province level for Prussia), we use various historical maps to identify the years of French occupation on the county level. The maps are available on IEG-MAPS (link: http://www.ieg-maps.uni-mainz.de, last accessed May 25, 2021).

Note that we treat counties that were part of the Duchy of Warsaw as not occupied. The Duchy of Warsaw was created as a French satellite state in 1807, but the French lost control over this satellite in early 1813 as a result of Napoleon's failed campaign against Russia. In contrast to other French satellite states, the Duchy of Warsaw, which was partly located in the eastern part of the provinces of Posen and West Prussia, was not ruled by a French administration, but by King Frederick Augustus I of Saxony. However, this rule was only formal, since the Polish nobility remained in power and in control of the public administration. In contrast to German territories that were under French rule, the ideas of the French *Code civil* were not put into practice by the Polish elites, so extractive institutions persisted. The peasants, for example, remained under the control of the nobility (for the history of the Duchy of Warsaw, see Grab (2003)). Due to the lack of effective French influence and the lack of effective institutional change, the Duchy of Warsaw is not comparable with the German territories in the West and North that were under direct French control or the French satellite states ruled by Napoleon's family members (e.g. the Grand Duchy of Berg), which we treat as occupied.

B.2.4 Distance to Paris

In columns 2 and 3 of Table 2, we use the variable *Distance to Paris*. *Distance to Paris* is the great circle distance (in kilometers) between the main city of the county and Paris.

B.3 Patent Data

B.3.1 Patents per Capita

We extracted the patent data from the Baten/Streb patent database (see Streb et al. (2006) for a description of the data set). It contains all patents granted in the German Empire between 1877 and 1913 that were renewed for at least 10 years, out of a maximum length of 15 years permitted by patent law. The data set includes information about the location of the patentee, the technological class of the patent, and whether the patentee was a firm or a private individual. We include all patents granted to German individuals and firms. We assign every patent to the historic German county where the patentee was located. The variable *Patents* is defined as the total number of patents that originated from the respective county in the respective year, divided by the county population. Population figures for 1890, 1900, and 1910 are extracted from the official census records of the Imperial Statistical Office (1890: Deutsches Reich (1894); 1900: Deutsches Reich (1903b); 1910: Deutsches Reich (1915)). To ease the display of coefficients, *Patents* represents patents per million inhabitants.

B.3.2 Firm Patents and Individual Patents

In Table 7, we distinguish between patents filed by firms and patents filed by individuals. The variable *Firm Patents* (*Individual Patents*) is defined as the total number of firm (individual) patents that originated from the respective county in the respective year, divided by the county population. To ease the display of coefficients, all variables represent patents per million inhabitants.

B.3.3 High-tech and Non-high-tech Patents

In Table 8, we use information about the technology class of the patents to create sub-samples. Technology classes were used by the Imperial Patent Office to classify inventions. In total, there are 89 different major classes. Due to the emergence of new technologies and the rising number of patents, the Imperial Patent Office extended its classification scheme by introducing sub-classes in 1900. For example, class 21 (electrical engineering) was subdivided into 8 sub-classes, ranging from 21a (communications engineering) to 21h (processes and installations for electrical heating and smelting, including metalworking based on electrically generated heat). We use this classification to distinguish between high-tech and non-high-tech patents. High-tech patents are defined as all patents related to the chemical industry and electrical engineering, the two leading sectors of the second industrial revolution (see for example Henderson (1975) and Streb et al. (2006)). For the chemical industry, we include general chemical processes and applications (class 12), fertilizers (class 16), dyestuffs (class 22), and textile chemistry (class 8i to 8o). For electrical engineering, we include general electrical engineering (class 21), as well as electric trains and electric railway equipment (20k and 20l). All other classes are defined as non-high-tech. The variable *High-tech Patents* (*Non-high-tech Patents*) represents the total number of high-tech (non-high-tech) patents that originated from the respective county in the respective year, divided by the county population. We also use the variable *High-tech Firm Patents* for all high-tech patents filed by firms (see definition in Appendix B.3.2) from the respective county in the respective year, divided by the county population. To ease the display of coefficients, all variables represent patents per million inhabitants.

B.4 Basic Control Variables

B.4.1 Population Density

The variable *Population/Km²* represents the population density, which is the number of inhabitants of a county, divided by the county's area in square kilometers. County population and area size are extracted from the official German census publications (for 1890: Deutsches Reich (1894); for 1900: Deutsches Reich (1903b); for 1910: Deutsches Reich (1915)).

B.4.2 Rivers and Harbors

River is a dummy variable that is equal to one if the county was located by a navigable waterway, and zero otherwise. We include all rivers and canals that were navigable in 1850, based on the map *Schiffahrtsstrassen im Deutschen Zollverin 1850*, which is available on IEG-MAPS (link: http://www.ieg-maps.uni-mainz.de/gif/w850d_a4.htm, last accessed May 25, 2021). We use the same map to identify major seaports. *Harbor* is equal to one if a seaport was located in the county, and zero otherwise. *River*Harbor* is equal to one if a county had access to both a navigable river and a seaport, and zero otherwise.

B.4.3 Border Counties

Border is a dummy variable that is equal to one if a county was located at an external border of the German Empire, and zero otherwise. Since Alsace-Lorraine was part of France until 1871, we treat the border between Alsace-Lorraine and the adjoining German federal states (Prussia, Bavaria (Palatinate), and Baden) as an external border. Thus, this border definition reflects the situation after the Congress of Vienna (1815), which shaped economic activity in these counties in the long run. The coding is based on the map *Der Deutsche Bund nach dem Frankfurter Territorialrezess um 1820*, which is available on IEG-MAPS (link: http://www.ieg-maps.uni-mainz.de/gif/d820_a4.htm, last accessed May 25, 2021)). In the same way, we define the dummy variable *Border France,* which is equal to one if the county was located at the border to France after the Congress of Vienna, and zero otherwise.

B.4.4 Coal and Mineral Ore Deposits

Coal Deposits is a dummy variable that is equal to one if coal deposits were located in the respective county, and zero otherwise. In the same way, *Ore Deposits* is defined as dummy variable that is equal to one if deposits of iron ore or other important metals (e.g. copper) were located in the respective county. The data is based on map BI (coal mining) and map BII (metal ore mining) in Pfohl and Friedrich (1928).

B.4.5 Large Cities in 1750

Large City 1750 is a dummy variable that is equal to one if at least one city with more than 5,000 inhabitants was located in the respective county in 1750, and zero otherwise. City population data is from Voigtländer and Voth (2013) (which is based on the data set of Bairoch et al. (1988)).

B.4.6 Universities in 1789

University 1789 is a dummy variable that is equal to one if a university was located in the respective county in 1789, and zero otherwise. We use De Ridder-Symoens (1992), De Ridder-Symoens (1996), and Rüegg (2004) to obtain information about the formation of universities in German states.

The data includes information about the year when a university was opened, whether and when a university was closed, and whether and when it was reopened. This data allows us to determine the universities that were in operation in 1789. We include general universities, technical universities, mining academies, medical and veterinary universities, and higher schools of commerce. Theological universities, academies of arts, and academies of music are not taken into account.

B.4.7 Hanseatic League

Hanseatic League is a dummy variable that is equal to one if a former member city of the Hanseatic League, a trade federation of the late-medieval period, was located in the respective county. The data is based on Dollinger (2012), pp. 592-593.

B.4.8 Huguenots

Huguenots is a dummy variable that is equal to one if at least one Huguenot settlement was in the respective county, and zero otherwise. The coding is based on the map *Hugenotten, Waldenser und Wallonen in deutschen Territorien* from the German Huguenot Museum in Bad Karlshafen. The map is available online via the following link:

https://www.hugenottenmuseum.de/hugenotten/karte-siedlungsorte-franz-ref-gemeinden-im-roemischen-reich.pdf (last accessed April 28, 2021).

B.4.9 Protestants

Protestants % is the share of the Protestant population as a percentage of the total population in each county. We extracted the data from the official census publications of the Imperial Statistical Office for each year of observation (1900: Deutsches Reich (1903a); 1910: Deutsches Reich (1915)). The census publications provide county-level information about the Protestant and Catholic populations living in each county. Together, Protestants and Catholics account for over 98% of the German population. Note that the census publications only report data on the regional level for 1890 (Deutsches Reich (1894)). Since there is no evidence of a significant change in the spatial distribution of Protestants on the regional level, we use the county data on the share of Protestants in 1900 for 1900 and 1890.

B.4.10 Non-German Language

Non-German Language is a dummy variable that is equal to one if the share of the population with a mother language other than German was higher than 50% in the respective county in 1900, and zero otherwise. The county data is based on the official census publication of the Imperial Statistical Office, which provides information about non-German-speaking minorities on the county level (Deutsches Reich (1903b)).

B.4.11 Prussia in 1816

Prussia 1816 is a dummy variable that is equal to one if the county was part of Prussia after the Congress of Vienna (1815), and zero otherwise. See the map *Der Deutsche Bund nach dem Frankfurter Territorialrezess um 1820* for the Prussian territory after the Congress of Vienna. This map is available online on IEG-MAPS (link: http://www.ieg-maps.uni-mainz.de/gif/d820_a4.htm, last accessed May 25, 2021).

B.4.12 City States

City State is a dummy variable equal to one if the county was part of one of the city states that existed in the German Empire (Bremen, Hamburg, and Lübeck), and zero otherwise.

B.5 Additional Variables

In this section, we describe all other variables used in Section 5 (Alternative Explanations), in later sections, and in the Appendix. The variables are ordered according to the discussion in the main text.

B.5.1 Employment by Sector

In Table 5 and Table A8, we introduce *Manufacturing+Mining Workforce %*, which represents the share of people employed in manufacturing and mining relative to the total number of people employed in the respective county, and *Services Workforce %,* which represents the share of employees in the private service sector. We use the official German employment census publications of the Imperial Statistical Office to get information about employment by sector. In order to avoid double-counting, we only consider the main occupation. Employment census data is available for the years 1882, 1895, and 1907 (1882: Deutsches Reich (1884); 1895: Deutsches Reich (1898b) and Deutsches Reich (1898c); 1907: Deutsches Reich (1910)). We match the employment data for 1882 with patents per capita in 1890, the employment data for 1895 with patents per capita in 1900, and the employment data for 1907 with patents per capita in 1910.

B.5.2 Coal Mining

In Table 5 and Table A8, we define *Coal Mining 1850* (*Coal Mining 1880*) as the total coal production in tons in a region in 1850 (1880), divided by the population of this region in 1850 (1880). Data on coal production is only available on the regional level. We use the HGIS Germany database (link: http://www.ekomp.digihist.de/Dokumentation_Datensaetze/Zeitreihen/, last accessed May 25, 2021) to get information about coal production and population for 1850 and 1880. For earlier years (e.g. 1840), coal mining data is not available for all regions.

B.5.3 Technology Transfer

In Table 5 and Table A12, we define *Steam Engines 1861* (*Spinning Mills 1861*) as the number of steam engines (mechanical cotton spinning mills) in operation in the respective region in 1861 divided by the number of inhabitants (in millions) in this region in 1861. Note that data on steam engines and mechanical cotton spinning mills is only available on the regional level. The data is based on the 1861 industrial survey of the Zollverein (Zollverein (1861)). Therefore, the data includes information about the member states of the Zollverein only (which excludes the city states of Bremen, Hamburg, and Lübeck, as well as Mecklenburg-Schwerin, Mecklenburg-Strelitz, and Schleswig-Holstein). Region-level population figures are available on the HGIS Germany database (link: link: http://www.ekomp.digihist.de/Dokumentation_Datensaetze/Zeitreihen/, last accessed May 25, 2021).

B.5.4 Intellectual Elites

In Table 5, we use *Intellectual Elites* to control for the local presence of knowledge elites prior to the French occupation. *Intellectual Elites* is a dummy variable that is equal to one if at least five famous scientists and intellectuals were born in the respective county between 1650 and 1750, and zero otherwise. To construct this dummy, we use data from de la Croix and Licandro (2015). Their data set includes information about the place of birth and year of birth of famous individuals, and it distinguishes between several categories. We include famous individuals from the categories "education", "science", and "humanities". Note that we only focus on people born between 1650 and 1750, since people born later might have been influenced by the French Revolution and the subsequent institutional change. In order to minimize measurement error, we only focus on potential centers of enlightenment with at least five famous individuals.

B.5.5 Old Territories

In Table 5 and Table A14, we use *Old territories*, which is defined as the number of independent principalities that existed in 1789 within each region (Regierungsbezirk). *Old territories/km^2* equals *Old territories* divided by the area in square kilometers of the respective region. *Old territories* is also used in Acemoglu et al. (2011a), but at a higher level of aggregation. *Old territories* includes secular principalities that were immediate to the emperor (reichsunmittelbar), independent ecclesiastical territories (electorates, prince-Bishoprics, prince-abbeys, and territories of religious orders of knights), free imperial cities, and territories of the Imperial Knights. The latter represent a large number of micro-states that were organized into different leagues and cantons (e.g. the canton Odenwald of the Franconian Circle). Since even very detailed maps only report the territories of the Imperial Knights as a whole or by canton, but not separately, we treat these micro states as one old territory. Territories that were ruled under a dynastic union are only counted once. To get accurate information on the regional level, we use the following sources: for a general register for all former German territories: Köbler (1992); for a general overview (and the eastern territories in particular):

the map *Deutschland 1792*, which is available on IEG-MAPS (link: http://www.ieg-maps.uni-mainz.de/gif/w850d_a4.htm, last accessed May 25, 2021); for the territories of the Rhineland: map 5.1 in Irsigler (1982); for the territories in the Palatinate: map 001 in Alter (1964); for the territories in Hesse: map 22 in Hessisches Landesamt für Geschichtliche Landeskunde (1984); for the territories in the South-West (subsequent federal states of Baden and Württemberg): the map *Herrschaftsgebiete und Ämtergliederung in Südwestdeutschland 1790* in Schröder and Miller (1988); for the territories in the South-East (subsequent state of Bavaria): Bayrische Staatsbibliothek München (2009).

B.5.6 Internal Border

In Table 5 and Table A14, we define *Internal Border* as a dummy variable which equals one if a county was located at an internal state border, and zero otherwise. We define internal state border as a border between states that were located on the territory of the subsequent German Empire. The coding is based on the borders that were established after the Congress of Vienna (see the map *Der Deutsche Bund nach dem Frankfurter Territorialrezess um 1820*, which is available online on IEG-MAPS (link: http://www.ieg-maps.uni-mainz.de/gif/d820_a4.htm, last accessed May 25, 2021)). We therefore classify a county as being at an internal border if it was at the border of two states that became part of the German Empire in 1871 (e.g. the border between the kingdoms of Bavaria and Württemberg), or if it was at the border to a state that was dissolved before 1871 (e.g. the border between the Kingdom of Hanover and Prussia; the Kingdom of Hanover was annexed by Prussia after the Austro-Prussian War in 1866).

B.5.7 Zollverein in 1842

In Table 5 and Table A14, we define *Zollverein in 1842* as a dummy variable that is equal to one if the county was located within a state that belonged to the German Customs Union (Zollverein) in 1842, and zero otherwise. See the map *Deutscher Zollverein 1842*, which is available on IEG-MAPS (link: http://www.ieg-maps.uni-mainz.de/map4.htm, last accessed May 25, 2021). The Zollverein was founded in 1834 under the leadership of Prussia. By 1842, most German states had joined the customs union, except for the states in the North that had access to the coast. We choose 1842 as a benchmark year for the following reasons: First, the effect of market integration should be stronger for the states that had no access to the coast, second we wanted to select a year in which a significant number of states were still not part of the union. The latter argument no longer holds for 1854, the year of the subsequent major enlargement, when the Kingdom of Hanover and the Grand Duchy of Oldenburg joined.

B.5.8 Printing Press

In Table 5 and Table A15, we use *Printing Press* as a proxy for human capital. Dittmar (2011) uses information on which localities adopted the printing press between 1450 and 1500. He relies on three data sources for this adoption of the printing press: Clair (1976), Febvre and Martin (1958), and ISTC (1998). We collected the data from Clair (1976). While this is only one of the three data sources used by Dittmar (2011), we believe this does not affect our results, since footnote 21 on page 1,143 of Dittmar (2011) argues that the three data sources provide highly overlapping information on the adoption of the printing press.

B.5.9 Illiterates

In Table 5 and Table A15, we use *Illiteracy* as a proxy for human capital. We have collected the same illiteracy data for Germany as that used by D'Acunto (2015). *Illiteracy* is defined as the share of the population aged 10 or older that is illiterate.

B.5.10 Universities

In Table 5 and Table A15, we define *University* as a dummy variable that is equal to one if a university was located in the respective county in the respective year, and zero otherwise. We use De Ridder-Symoens (1992), De Ridder-Symoens (1996), and Rüegg (2004) to get information about the formation of universities in German states. The data includes information about the year when a university was opened, whether and when a university was closed, and whether and when it was reopened. This data allows us to determine which universities were in operation for each year in the sample. *University* includes general universities, technical universities, mining academies, medical and veterinary universities, and higher schools of commerce. Theological universities, academies of arts, and academies of music are not taken into account. *Technical University* is a dummy variable that is defined in the same way, except that it includes only technical universities and mining academies.

B.5.11 Inequality

In Table 6, we use the variable *Inequality* as a proxy for wealth inequality. The variable is defined as the Gini coefficient of landownership concentration based on data from the agricultural census of 1895 (Deutsches Reich (1898a)). The census records provide information on how much land in a county is owned by how many farms. The data is reported for 18 size categories (farms up to 0.1 are, 0.1-2 ares, 2-5 ares, 5-20 ares, 20-50 ares, 50-100 ares, 1-2 hectares, 2-3 hectares, 3-4 hectares, 4-5 hectares, 5-10 hectares, 10-20 hectares, 20-50 hectares, 50-100 hectares, 100-200 hectares, 200-500 hectares, 500-1,000 hectares, more than 1,000 hectares). We use data from the 1895 agricultural census for all years in the panel data set.

B.5.12 Pogroms and Medieval Jewish Settlements

In Table 6, we control for the potential effects of discrimination against and persecution of Jews, by using data from Voigtländer and Voth (2012). Based on their data, we construct the dummy variable *Pogrom,* which is equal to one if any of the pogrom variables provided by Voigtländer and

Voth (2012) indicates that there was a pogrom in a county, and zero otherwise. *Medieval Jewish Settlement* is a dummy variable that is equal to one if any of the different variables on medieval Jewish settlements provided by Voigtländer and Voth (2012) indicates that there was a medieval Jewish settlement in a county, and zero otherwise.

B.5.13 Jewish Population

In Table 6 and Table A16, we use census data to control for differences in the Jewish population share. The variable *Jewish Population per 1,000* indicates the number of Jewish people per 1,000 inhabitants in the respective year of observation (1890, 1900, and 1910). The data is available on the province level for Prussia and on the state level for most other German states (for Bavaria, the Jewish population living in the Palatinate is reported separately) . Data is from the population census publications (1890: Deutsches Reich (1894); 1900: Deutsches Reich (1903b); 1910: Deutsches Reich (1915)).

The variable *Jewish Population per 1,000 (Alternative)*, which we use in Table A16, is different from the variable mentioned before, in that it is available at the region level but only for 1890, since only the 1890 census (Deutsches Reich (1894)) reports region-level data on the Jewish population (in addition to the aforementioned province- and state-level data) .

B.5.14 Financial Centers

In Table 6 and Table A17, we measure financial development with *Old Financial Center,* which is a dummy variable that is equal to one if there was an exchange for financial assets in the respective county before 1789, and zero otherwise. Information on the location of historical financial centers is from Kaufhold (1992). In addition, we use the variable *New Financial Center*, which is a dummy variable that is equal to one if there was an active stock exchange in the respective county in our period of observation. To identify active stock exchanges, we use information from Burhop and Lehmann-Hasemeyer (2016)

B.5.15 Banking Workforce

In Table 6 and Table A17, we use the variable *Banking Workforce in %* to measure differences in financial development at the county level. *Banking Workforce in %* measures the share of the workforce employed in the banking sector, based on the official employment-census records. To avoid double-counting, we only consider the main occupation. For banking, employment census data is only available for the years 1895 and 1907 (1895: Deutsches Reich (1898b) and Deutsches Reich (1898c); 1907: Deutsches Reich (1910)). We match the employment data for 1895 with patents per capita in 1900, and the employment data for 1907 with patents per capita in 1910. Note that the 1882 employment census (which we use when measuring employment shares in manufacturing and private services) does not provide separate information on the banking workforce.

B.5.16 Migration

In Table 6, we use the variable *Migration* to control for both international and intra-German migration. The variable is defined as the fraction of all non-native inhabitants living in the respective region. Non-native inhabitants are those who were not born in the same state or in the same Prussian province. The data is based on the German population census of 1885 (Deutsches Reich (1888)), which only provides information on the regional level. We use data from the 1885 census for all years in the panel data set.

B.5.17 World's Fair Exhibits

In column 5 of Table 6 and Table A18, we test the effect of Institutions on the number of world's fair exhibits per million inhabitants. We use data that we extracted from the official exhibition catalogues of two world's fairs: the 1876 Centennial International Exhibition in Philadelphia and the 1893 World's Columbian Exposition in Chicago. Data for 1876 is from the *Official Catalogue of the U.S. International Exhibition 1876. Revised Edition* (published for the Centennial Catalogue Company by John R. Nagle and Company, Philadelphia) and data for 1893 is from *Columbische Weltausstellung in Chicago. Amtlicher Katalog der Ausstellung des Deutschen Reiches* (printed by Reichsdruckerei, Berlin). We only include commercially relevant products, but exclude artwork (e.g. paintings), ethnological exhibits, and exhibits describing the educational system in Germany (section "Educational Systems, Methods, and Libraries" in the 1876 catalogue and section "Erziehungswesen" in the 1893 catalogue). For 1893, we also exclude all exhibits by book traders because book-trade exhibits do not represent innovation and there was no specific book-trade section in 1876, so we have to exclude it to make the data comparable. The catalogues indicate where the exhibitor was located. We use this information to assign the exhibits to the counties in our data set and aggregate the number of exhibits at the county level. To match the world's fair exhibits with our data set, we combine the 1876 data with data for the benchmark year 1890, and the 1893 data with data for the benchmark year 1900.

Exhibits is defined as the number of world's fair exhibits per million inhabitants. *High-tech Exhibits* is the number of exhibits of the chemical and electrical-engineering industries per million inhabitants, and *Other Exhibits* is the number of non high-tech exhibits per million inhabitants.

B.5.18 Trade Courts

In Section 6 we analyze the effect of trade courts. For (qualitative) information on the role and development of trade courts, we rely on German sources from the 19$^{\text{th}}$ century, since there is a lack of modern research on these courts. A detailed description of the role of trade courts, including an overview of the historical development, is, for instance, given in the chapter *Die Organisation der Handelsgerichte nach dem Entwurfe eines Gerichtsverfassungsgesetzes* in Hirth (1875). Information on the geographic location of trade courts is from the accompanying booklet (Orientierungsheft) of Klippel (1996).

B.5.19 Population Growth

In Table 9, we use a three-stage least squares model to test whether differences in patents per capita across counties affect future county-level economic growth, which we proxy with the future average annual compound growth rate of the county population (*Growth*). We compute *Growth* over the period 1890-1900 for the year 1890, and over the period 1900-1910 for the year 1900. Likewise, *Past Growth* is the average annual compound growth rate of the county's population during the preceding period. *Past Growth* is computed over the period 1890-1900 for the year 1900 and over the period 1885-1890 for 1890. To compute the average annual growth rate for the period 1885-1890, we use additional data from the German population census of 1885 (Deutsches Reich (1888)). The county's population in 1885 is also used to compute the weights for the population-weighted regression in Table 9. If we had not used population data from a date before the date of the outcome variables, then we would have the problem that innovation would simultaneously affect the outcome variable, population growth, and population weights. This problem is avoided by fixing population data before the sample years 1890, 1900, and 1910.

B.6 Foreign Trade Statistics

In Table A13, we report German import and export statistics by country of origin and destination, for several years. Exports and imports for 1841 and 1851 include Zollverein member and non-member states that became part of the German Empire in 1871. The data for 1841 and 1851 are from von Borries (1970), table 42. Exports and imports for 1890, 1900, and 1910 are based on the official trade statistics published in the yearbooks of the Imperial Statistical Office (Deutsches Reich (1892), p. 65; Deutsches Reich (1905), pp. 169-171; Deutsches Reich (1913), pp. 241-242). "Others" includes all countries for which no separate figures are available in von Borries (1970).

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