You Only Weave Twice
Espionage And Industrialization in 18th Century France [*]

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Abstract

Can state-sponsored industrial espionage promote innovation and lead to self-sustained growth? I study the effect of 18th-century French industrial espionage on French innovation and industrial activity in the 19th century. Between 1730 and 1800 the French Bureau of Commerce promoted an ambitious plan aimed at stealing from Britain the new technologies of the Industrial Revolution, bribing British entrepreneurs and inventors to leave England and bring their expertise to France. I assemble a novel database with a comprehensive list of French espionage and combine it with newly digitized 17th- and 18th-century industrial surveys, 1800s industrial censuses, and the full list of early French patents. I find large, positive, and persistent effects of industrial espionage on industrial activity and innovation.

Keywords: industrial espionage, industrialization, France, technology adoption, innovation.

JEL Classification: O33, O14, N73, F63, O38.

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Successful technologies always invite imitation. When copying is illegal, some firms resort to theft, stealing their rivals' ideas to catch up and compete (Wired, 2018). At times, the state itself joins the effort, coordinating the spying to promote domestic growth. Examples from the past show the strategy can pay off: during the Cold War, East German *Stasi* planted agents in Western companies to smuggle industrial secrets across the Iron Curtain, and Glitz and Meyersson (2020) shows that these operations helped East German productivity to keep up with the West. But industrial spies are not unique to the Soviet Bloc, and as nations as diverse as France and Iran engage in espionage (Pezeshki et al., 2010; Foreign Policy, 2013), we still do not know whether the practice works outside the specific context of Soviet Germany, whether it can promote long-run industrialization, and whether stolen ideas can lead to genuine innovation. Answers to these questions remain elusive because espionage is, almost by definition, hard to measure.

This paper evaluates the impact of French 18$^{\text{th}}$-century industrial espionage on long-run French industrialization and innovation. Before the Revolution of 1789, the French government attempted to industrialize by bribing British artisans to bring industrial technology to France. During the seventy years leading to the Revolution, the program brought to France between 1,000 and 2,000 British artisans (Linant de Bellefonds, 1971; Ó Gráda, 2019). The French firms that hired them obtained technologies illegally, ranging from steel-making to chemicals and ship-building. Above all, they obtained the new machines used in textiles—the fastest-growing sector of the time (Crafts, 1985). Smuggling technology in this way was illegal under British law.[1] Because these spies left British workshops for good, their operations differ from Soviet-era espionage, when *Stasi* agents would exfiltrate secrets while remaining inside Western firms. Their activity, however, resembles recent episodes of espionage, e.g. the expert engineers poached by state-controlled Chinese firms in chips and wind turbines who were paid to smuggle advanced technology to China (Financial Times, 2025; Reuters, 2009, 2018; Wall Street Journal, 2024). Whether this espionage delivers the same results as Soviet-type operations is an empirical question that this paper aims to address.

I analyze the effect of espionage on industrialization and innovation with novel data on espionage and industrial development. To measure espionage, I compile a new list of British workers paid by the French *Bureau de Commerce* from the records of the "*payment à faire sur le demy pour cent*." I complement this source with information from historians to compile the first comprehensive list of French 18$^{\text{th}}$-century espionage. I use this list to study industrial development during the 1700s and 1800s, assembling from primary sources a novel city-

[1] See Harris (1998) "Industrial espionage and technology transfer," especially chapter 18. The relevant legislation was passed in 1719, 1750 and 1774: 5 Geo. I c. 27; 23 Geo. II c. 13; 14 Geo. II c. 71.

level database of textile production spanning almost two hundred years. The data provide information on employment, textile establishments and plant size between 1692 and 1860. While the panel has low frequency, two observations before the start of industrial espionage allow me to study pre-trends. To investigate mechanisms, I take post-1789 data on patents to proxy for French innovative activity after spies brought new industrial technology.

To pin down the causal effect of espionage, I use two complementary strategies. The first leverages a panel of textile activity to estimate two-way fixed effects models in which growth in towns hosting spies is compared to growth in towns with no spy visits. This difference-in-differences approach identifies causal effects if treated and control towns follow similar trends of industrial development. The assumption is plausible because pre-industrial economies, including France, displayed little dynamism. Moreover, the historical record indicates that French bureaucrats in charge of espionage sent spies to both growing and struggling towns, a claim I validate with formal tests of pre-trends. Parallel pre-trends in treated and control towns suggest that the results of these difference-in-differences models are not contaminated by differential trends in towns receiving the spies.

The second identification strategy looks at all manufacturing sectors and exploits additional information on French efforts to acquire British technology. Over the seventy years of the espionage program, not every spy succeeded: several artisans recruited in Britain were arrested before making the trip to the Continent. Detailed information on their failed migration plans, including the planned destination in France, allows me to study growth in "placebo towns" that were considered to receive industrial secrets but eventually did not. Because the British ability to detect and apprehend spies was primitive, these arrests are likely to be unrelated to the growth potential of French destinations: in other words, these placebo towns were likely similar to the towns that eventually saw their spies arriving. Formal statistical tests confirm this intuition, showing arrested and successful spies heading to destinations that were similar along all observable characteristics. The strategy allows me to evaluate the impact of espionage in sectors different from textiles, for which pre-espionage data is not available and the difference-in-differences strategy is not feasible.

French espionage proved an effective industrial policy. Between 1692 and 1708 textile employment grew more slowly in towns that would eventually receive spies, though not significantly so (p=0.247). During the following century textiles boomed, with employment growing at a compound yearly rate of 0.7%. Growth was uneven, though, and towns visited by spies saw yearly growth of 1.6%. I find no effects for the total number of plants and large effects on plant size, suggesting that the arrival of spies created large industrial concerns that replaced the many self-employed weavers working on domestic looms in the early 1700s.

Small and statistically insignificant effects of arrested spies, together with the absence of significant pre-trends in the towns of successful espionage, suggest that these effects may be causal.

In the long run, the technologies stolen from Britain helped France industrialize. The confusion of the Napoleonic Wars (1803–15) interrupted the flow of British spies, and espionage did not resume after the Congress of Vienna (1815), as tensions between the restored French monarchy and Britain subsided. Yet, in the industrial surveys of 1839 and 1860 textiles are more developed in the towns that had become home to British spies almost one hundred years before. The effect is roughly half what it was in 1806, at the end of the espionage program, but it is still economically and statistically significant.

The surveys of 1839 and 1860 report statistics for the entire manufacturing sector and show that espionage, which was not confined to textiles, promoted growth broadly. Performance of towns where arrested spies never arrived is not significantly better than the rest of France, though formal statistical tests can reject the null of similar effects of successful and arrested spies only in the case of textile spies. Thus, I conclude that espionage had a positive causal effect on textile development, and the effect may have been positive in other sectors too.

The nature of the technology shock makes this persistence perhaps surprising. A large literature documents long-run agglomeration effects of geographic advantage (Bleakley and Lin, 2012) or permanent migration (Peters, 2022; Ciccone and Nimczik, 2022). Contrary to these shocks, however, the new technology introduced by British spies only granted a temporary advantage, as ideas are portable and non-rival, and good ideas spread fast. Indeed, looking at technology adoption in 1806, I find that spy presence predicts the adoption of recently stolen technologies (the *mule*), but cannot predict adoption of technologies smuggled thirty years earlier: the *jenny* had arrived in France in 1771 and at the time of Napoleon was everywhere. The diffusion of the technologies brought by the spies would dampen the effect of spies over time, reducing persistence.

In a world where technology spreads, how do towns that initially receive stolen secrets stay ahead? I hypothesize that agglomeration turns a temporary advantage into a permanent one through local innovation. As British spies settle in France, they bring new technologies which, before their diffusion, allow some French towns to pull ahead and grow. This temporary expansion attracts new workers with employment opportunities and higher wages. In the medium run, this added manpower generates new ideas, as in endogenous and semi-endogenous growth models (Romer, 1990; Jones, 1995), increasing productivity and compensating the erosion of competitiveness that technology diffusion generates. I find suggestive evidence of this mechanism in patent data: towns that received a spy during the 1700s file more patents throughout the first half of the 1800s. The results are only suggestive as lack of pre-espionage data prevents estimation of difference-in-differences models with innovation. Nevertheless, these results are consistent with continued innovation helping towns that received spies in the 1700s to remain ahead one hundred years later.

Conclusions are broadly robust. All results survive the inclusion of region × year fixed effects: these controls absorb flexibly time-varying differences in market conditions across some 20 French regions as well as different levels of accuracy in data collection at the time of early surveys. Bounding difference-in-differences estimates using pre-espionage coefficients (Rambachan and Roth, 2023) strengthens the conclusions. Accounting for spatial autocorrelation of the outcome with the method of Conley (1999) does not affect the significance of results, and I find limited evidence of local spillovers. Overall, the conclusions appear robust.

The main contribution of this paper is to show that stolen secrets have lasting effects on industrialization, and that industrial spies can spur growth even if they permanently move away from the country at the technological frontier. These results complement existing evidence on *Stasi* espionage, which helped East German firms through the work of spies that maintained constant access to Western technologies. Additional results indicate that long-lasting effects are at least partly driven by follow-on innovation, suggesting that industrial espionage can provide a springboard to promote further innovation and kick-start self-sustained growth.

Related literature. Because the French government used espionage to promote industrial growth, this study contributes to the literature of industrial policy.[2] Governments have historically used many tools to steer the economy, and recent work has shown cheap credit and subsidies to be effective: Lane (2025) and Choi and Levchenko (2025) show that South Korean credit subsidies supported the expansion of heavy industries, and Kalouptsidi (2018); Barwick et al. (2025) and Goldberg et al. (2024) demonstrate the importance of different types of subsidies in shipbuilding, EV, and chip manufacture. Locations receiving preferential treatment enjoy long-lasting benefits (Kline and Moretti, 2014), as agglomeration turns temporary support into lasting advantage (Greenstone et al., 2010). Trade barriers have a mixed record: Luzio and Greenstein, 1995 finds that the Brazilian PC import ban of the 1980s did not allow Brazilian manufacturers to become competitive. However, in the case of 18$^{\text{th}}$-century France, Juhász (2018) shows that temporary import protection caused by

[2] Economists' view of industrial policy has evolved over the past two decades, and distinctly negative views (Pack and Saggi, 2006) have recently given way to more positive assessments (Harrison and Rodríguez-Clare, 2010; Juhász et al., 2024).

the Napoleonic blockade gave French textile manufacturers the time to adopt new technologies and become more productive. French espionage differs from these interventions as it supported specific industries through the import of foreign technology.

Technological transfer has proven effective: Giorcelli (2019) finds large effects of a US policy that transferred managerial practices to Italy, and Giorcelli and Li (2021) shows that Soviet steel making machines donated to Chinese plants in the 1950s made them permanently more productive only when it was accompanied by knowledge transfer.[3] Unlike these examples, industrial espionage imports ideas developed abroad without the cooperation of the technological leader: thus, it may be less effective, as ideas are often hard to transfer (Keller and Yeaple, 2013; Kantor and Whalley, 2019). The closest precedent in this literature is Glitz and Meyersson (2020) that studies how Stasi's spies helped East German manufacturing to keep up with the West. Relative to this work, this paper makes three contributions. First, it looks at a different type of espionage: unlike *Stasi* agents who remained inside Western firms, British artisans taking new technologies to France left their companies for good. Their presence may have made adoption easier in the short run, while at the same time it may have made it difficult to follow further technological development in the medium run. Second, it studies the impact of espionage over almost one century, and third, it studies its effect on French innovation.

The paper is also closely related to the literature on the origins of the Industrial Revolution (Crafts, 1985; North and Weingast, 1989; Wrigley, 1990; Inikori, 2002; McCloskey, Deirdre N, 2011; Clark, 2005; Allen, 2009; Voth et al., 2023). Most relevant are Mokyr's theories (1992; 2002; 2009) which hold that Britain industrialized because it possessed an unusually abundant supply of human capital. Despite low literacy (Mitch, 2004; A'hearn, 2014), Britain had access to both highly skilled mechanics and learned *savants*, who together had accumulated large amounts of useful, tacit knowledge and used it to design and improve new industrial machines. Recent work finds empirical support for these theories: Kelly et al. (2023) finds that mechanics predict sectoral dynamism across 45 British counties. This paper has a different focus, as it studies how these mechanics allowed the Industrial Revolution to spread beyond Britain, thus isolating their contribution from other important confounders. This speaks to the importance of human capital in the industrialization of Europe (Squicciarini and Voigtländer, 2015; Dittmar and Meisenzahl, 2020; Cinnirella et al., 2025). Relative to these works, this paper measures human capital differently: instead of looking at "upper-tail

[3]The movement of people is essential to successful technology transfer: Hornung (2014) shows how the Huguenot diaspora helped spread new technology to Prussia. Different from these episodes of mass migration, British artisans moving to France as spies were few. Their impact on innovation is thus reminiscent of the contribution on US science of Jewish chemists fleeing Nazi Germany (Moser et al., 2014).

human capital" as proxied by Encyclopedia subscribers, research universities and economic societies, I look at (often illiterate) expert craftsmen moving to France. These "industrious mechanics" are central to Mokyr's narrative, and thus this paper complements earlier results showing the importance of learned philosophers and *savants* for growth.

As the Industrial Revolution spread unevenly, it magnified global inequality: the aim of the "Great Divergence" literature is to explain why 19^{th}-century growth was concentrated in a handful of Western countries (Juhász and Steinwender, 2024). Scholars have suggested that active public policies promoted 19^{th}-century growth (List, 1841; Studwell, 2013), and economists have found evidence of positive effects for some of them, e.g. new transportation infrastructure such as trains, telegraphs and steamboats (Donaldson, 2018; Hornung, 2015; Juhász and Steinwender, 2018; Pascali, 2017). These studies have two limitations: first, the spread of these technologies was not the result of deliberate public policies (Juhász and Steinwender, 2024, p.24). Second, some of these policies cannot explain the timing of industrialization: trains, telegraph and steamboats spread when growth was well under way across the West. In contrast, this paper looks at how an earlier public policy induced industrialization, providing new evidence of how industrial policies may have driven the Great Divergence between the West and the Rest.

1 French growth in the 18^{th} and 19^{th} centuries

Over the past three hundred years, France escaped Malthusian stagnation and experienced two centuries of sustained growth. Change was slow during most of the 18^{th} century (Daudin, 2012; Crouzet, 2003), but growth picked up in the following century: France posted average annual growth of 1.4% in the 1800s and 2.4% in the 1900s (Dormois, 2004). The timing of the take-off may not be coincidental: the second half of the 18^{th} century witnessed the introduction and spread in France of many of the new technologies of the British Industrial Revolution. While lagging behind Britain, France was receptive to British ideas and adopted them quickly, which may have facilitated the early take-off of the country.

How did the Industrial Revolution come to France? As none of the major inventions of the period were developed in France, French firms had to import new technology from abroad—mostly from Britain. Technological transfer was promoted directly by the French state, which for half a century sponsored a top-down program of industrial espionage through the *Bureau de Commerce* (Ballot and G'evel, 1923; Harris, 1998). The *Bureau* was the brainchild of Jean-Baptiste Colbert (1619–1683) and until the 1730s was mainly concerned with collecting business taxes and compiling industrial statistics (Minard, 1998). Starting from 1735, the

Bureau came under the guidance of successive ministries who promoted an aggressive policy of industrial espionage targeting British industrial technology (Parker, 1993). The program slowed after the 1789 Revolution and was discontinued around 1800 (Jeremy, 1977). The end of the espionage experiment allows us to evaluate its long-term impact.

Industrial espionage took many forms and achieved considerable success. The *Bureau* paid French spies to visit British manufactures and bring back advanced technologies through blueprints, memoirs, and smuggling. These visits were seldom useful, and the Ministry quickly realized that the most effective way to import technology was to bribe British workers and entrepreneurs to move to France (Harris, 1998): starting from 1735, between 1,000 and 2,000 British citizens became spies in this way (Linant de Bellefonds, 1971; Ó Gráda, 2019). Depending on their secrets, France offered wages, state pensions, housing, and even business loans and grants. While the scheme brought to France a few important inventors (e.g. Kay, inventor of the flying shuttle, and Wilkinson, pioneer of cast iron), most of the spies were skilled mechanics with knowledge in advanced British technologies. It is also credited with the introduction in France of the Arkwright frame, the mule, and the jenny. While the scheme was judged a success by contemporaries and historians Harris, 1998, to my knowledge no systematic empirical analysis studies its long-term economic consequences.

Casual empiricism suggests that the success of the program may have promoted the geographical reorganization of economic activity. Before 1735, industrial production consisted mostly of woolen manufactures (*draps*) concentrated in the South-West region of Languedoc (Bondois, 1943). Some industries did not exist (e.g., chemicals) and others were rudimentary (e.g., iron foundries; Harris, 1985). The arrival of new ideas from Britain scrambled production techniques as well as regional specialization. New cotton manufactures employing British technology sprang up in the North and North-East. Innovative chemical businesses located close by, providing compounds to dye cotton fabrics. Both contemporaries and historians noticed that the development of these innovative sectors happened in the same areas where British spies first brought new technology.

Where did industrial spies bring the new technologies? The *Bureau de Commerce* negotiated with the spies the terms of technological transfer, including where the spies should settle. In selecting spies' destinations, the *Bureau de Commerce* balanced conflicting priorities, as it strove to ensure the success of the spies while at the same time using the new technologies to reduce endemic unemployment. Both advanced and backward destinations received spies as a result. Moreover, once it was decided to direct the new ideas to a broad area, the exact town within this area to receive industrial spies was often selected for idiosyncratic reasons such as personal preferences of the spies (e.g., the metal goods producer Alcock insisted on working at La Charité). Finally, some spies planned the move to France, but were arrested in Britain before sailing for the Continent. For example, in 1791 one Charles Albert recruited six cotton workers in Lancaster for a Toulouse workshop, but was arrested after one of the workers denounced him. This and similar episodes provide an opportunity to study placebo locations which were considered to receive spies but eventually did not.

The impact of industrial espionage is likely to have persisted, even as the French economy changed beyond recognition. During the 1800s, the sectors most affected by industrial espionage moved past the technologies first introduced by British spies. The machines brought to France in the 1700s were operated by hand (e.g., flying shuttle, carding machine) or water (e.g., mule, Arkwright frame). In contrast, most 19$^{\text{th}}$-century inventions took advantage of mineral power through the steam engine. Over the century, entirely new sectors emerged thanks to new technologies (e.g., railroads, electricity). Despite these profound changes, decades after spies arrived in France, the towns that received them were specialized in the same industries that benefited from their contributions.

2 French data from the Early Modern Period

To study the effect of espionage on growth I use primary and secondary sources to assemble a novel town-level database of spy activity, industrial development and innovation spanning two hundred years.

Industrial espionage. Several historians describe how French espionage smuggled British technologies across the Channel (Ballot and G'evel, 1923; Linant de Bellefonds, 1971; Chassagne, 1991; Harris, 1998). These works use primary sources from French and British archives to recount the stories of both successful and unsuccessful spies. I compile a list of 267 espionage events from these secondary sources, recording for each of them when and where in France British spies settled. I use spies' histories to classify espionage as "successful"—when spies reached their planned destination in France—and "unsuccessful"—when British authorities foiled the scheme and the spies never arrived. Detailed information on the technology smuggled allows me to assign these events to one of 16 broad sectors. Figure 1-Panel A shows the geographic distribution of these eventsI use this measure of industrial espionage as the main explanatory variable of industrial development.

Industrial development. Reconstructing French industrial activity from the late 1600s is possible thanks to the pioneering surveys of the *Bureau de Commerce*. These surveys were compiled by local public servants (*intendants du commerce*) for the finance ministry (*contrôleur général des finances*). These *intendants* were primarily responsible for collecting taxes on manufactured goods in their region (around twenty *généralités*), but they were also requested periodically to send to Paris information about the state of industry. Early surveys carry the name of the ministry that commissioned them: I use the Pontchartrain (1692) and Desmarets (1708) surveys to measure town-level textile activity before spies' arrival. These surveys were initially transmitted as letters (Appendix Figure A.1-Panel A), and in the case of the 1692 survey these letters were used to compile tables (Appendix Figure A.2-Panel B). Surveys in 1806, 1839 and 1860 asked *intendants* to record information on pre-printed tables (Appendix Figure A.2-Panels A–C). I digitize the surveys of 1692, 1708, and 1806 and source the digitized surveys of 1839 and 1860 from Chanut et al. (2000).[4] Together, these sources allow me to track textile development since 1692.

The main outcomes are employment, plants and plant size, which can be measured consistently throughout the period. The surveys of 1692 and 1708 report town-level textile employment. They also contain enough information to decide whether these workers were employed by the same concern, allowing me to count plants and compute average plant size. As in many developing countries today, most of these workers are self-employed, and average plant size is small (1.1 in 1692 and 1.5 in 1708). The 1800s surveys record information at the plant level, and in addition to employment, contain information on production and technology. In 1839 and 1860 all manufacturing sectors are covered. While the absence of pre-espionage information on non-textiles, technology and production prevents a difference-in-differences analysis with these outcomes, textiles were the most dynamic sector of the time (Crafts, 1985) and a close reading of the surveys of 1692 and 1708 indicates that before 1730 textile technology was rudimentary and similar across France.

Textiles expanded during this period: the sector employed 27,000 in 1692 and 432,000 in 1860. Relative to the French population the growth was even more impressive, as population grew from 20 million in 1692 to 37.4 million in 1860 (Goldstone, 2012). When we observe the entire manufacturing sector in 1839 and 1860, textiles account for 59% and 43% of manufacturing employment. Before 1730, the southwestern region of Languedoc was an important textile center, specializing in woolen manufactures (Figure 1-Panel B). During the 18$^{\text{th}}$ century British spies brought new cotton technology to the northeast (Figure 1-Panel A), a region not particularly dynamic before 1730 (Figure 1-Panel C) and one that experienced strong growth afterwards (Figure 1-Panel D).

Innovation. During the 19$^{\text{th}}$ century textiles were a dynamic sector with considerable innovation both internationally and in France. I track inventive activity in this industry with data from the *Institut National de la Propriété Industrielle* (INPI), which maintains the records

[4] Juhász (2018) and Juhász et al. (2024) use the Revolutionary survey of 1806.

of the first French patent office. The office was created in 1791 as the *bureau des patentes* and awarded inventors "brevets"—temporary monopolies on the use of a novel technology modeled after British patents. INPI records, for every patent granted between 1791 and the 1860s, the residence of the inventor and the sector where the technology is applied. Few inventors patented before the end of the Napoleonic Wars (848, of which 205 were textile); after 1815, however, activity picks up and between 1816 and 1860 35947 inventions receive a patent. Textile patents represent an important part: 5748, or 16%. Because patents did not exist before the French Revolution and the arrival of spies, no difference-in-differences analysis is possible for this outcome.

Other data. To study the characteristics of towns receiving spies I collect additional demographic, economic and geographical data for France during the Early Modern period. Town-level population data in 1600–700 is from Bairoch et al. (1988). I use male literacy rate in 1686 from Squicciarini and Voigtländer (2015) to proxy for pre-industrial human capital. From the same source I take several measures of "upper-tail human capital", including: proximity to a printing press and number of books printed in 1500, the sales of books, of *Encyclopédias* and of the manual *Description des Arts et Métiers* in the 1700s, the number of famous scientists born between 1500 and 1700 and the presence of the *Académie Royale des Sciences* and the *Académie Française*. I proxy proto-industrial development with the Huguenot population in 1670 (also from Squicciarini and Voigtländer, 2015) and the presence of iron works before 1600 from Sprandel (1968). To measure market access I take proximity to Atlantic and Mediterranean ports and to navigable rivers from Squicciarini and Voigtländer (2015). In additional analyses, I aggregate sectoral trade data at the port × product level from Charles and Daudin (2015) and use the 1841 input-output table for Great Britain from Horrell et al. (1994) to proxy sectoral linkages in 19^{th}-century France.

3 The Effect of Espionage on Industrialization

Identification of the causal effect of espionage requires finding suitable counterfactuals to towns where spies settled, a task made difficult by the *Bureau de Commerce*'s active planning of the program. The chief concern is selection: as the *Bureau* took decisions to attain its goals, it sent British spies to locations whose growth potential was systematically different from the one of non-selected towns. Difference-in-differences and a placebo analysis allow us to address some of these concerns.

3.1 Textile Espionage: Difference-in-Differences Estimates

Assumptions. The first approach to identify causal effects exploits the longitudinal nature of textile data to estimate difference-in-differences models. When treated and control towns differ in pre-determined characteristics, considering the *changes* in their outcomes can mitigate selection bias. These estimates pin down the causal effects of spies on industrialization under the standard assumptions of (1) no anticipation, (2) parallel trends, (3) no correlated shocks, and (4) no spillovers. The setting and some of the design choices make these assumptions plausible.[5]

If workers and employers of future spy towns prepared in anticipation of industrial secrets arrival, pre-trends may be contaminated by these preparations, and before/after comparisons would conflate the anticipation and the actual effect of espionage. The context makes a failure of this assumption unlikely. First, pre-espionage surveys were collected two decades before espionage started, making anticipation improbable. Moreover, as migration of British artisans was illegal, everyone involved strived to conceal it: few would be able to know and prepare in advance. Finally, espionage was dangerous and uncertain: British spies were unsure where they would settle until the very end, and often made last-minute changes to their migration plans (Harris, 1998). In short, the timeframe, as well as the secrecy and uncertainty of espionage make anticipation unlikely.

Difference-in-differences addresses selection bias only if potential growth is similar in treated and control towns—absent spying, towns with and without spies would have been on parallel trends. The assumption is plausible. First, France showed little dynamism before the mid-1700s: between 1650 and 1700 population was constant or even declined slightly (Goldstone, 2012). Differential growth may be less of a concern in a stagnant economy. Second, French bureaucrats deciding where to send British textile spies selected both promising and struggling locations, as they balanced the desire to see successful technology adoption with the hope to use the new textile machines to boost employment of declining areas. As negative and positive selection were both present, the average bias may be small. Third, to account for the possibility that influential *intendants* may obtain more technologies for their regions, I show results with year $\times$ region fixed effects, comparing treated and control towns within areas under the same French bureaucrat. Finally, and most importantly, parallel trends are supported by formal tests on pre-trends.

[5] Roth et al. (2023) summarizes recent methodological advances in the difference-in-differences literature, including staggered treatments, inference with few clusters and differential pre-trends. Below I follow its advice and bound the effect using pre-trends coefficients. Although spies arrived in France over 70 years, I observe no industrial survey during espionage, and staggered treatment is not relevant in this context.

I use the industrial surveys of 1692 and 1708 to test for differential trends before the spies' arrival. Table 1 shows estimates of β_1 from:

$$\log Y_{i,1708} - \log Y_{i,1692} = \beta_0 + \beta_1 \text{Spy}_i + u_i \quad (1)$$

Where *Spy* is an indicator for any espionage activity in town i, explaining changes in textile activity between 1708 and 1692—the dependent variable.[6] I look at number of workers, number of plants and average plant size, and show both estimates of unconditional regressions (Panel A) as well as regressions including 17 region fixed effects (Panel B). Across specifications, spy and non-spy locations display broadly similar pre-trends: if anything, the negative coefficients indicate that selected towns may have been doing slightly worse in the decades before the start of espionage, consistent with some of the historical accounts. Parallel pre-trends make the parallel trends assumption more plausible.

Results in Table 1 do not imply that treated and control towns were similar along all dimensions. Appendix Figure A.3 shows that textile spies settled in towns that were already more populous, better educated and more likely to host one of the important *Ancien Régime* institutions. They also had a larger Huguenot population in 1670 and while they were farther from Mediterranean ports and not particularly close to Atlantic ones, they were often on rivers. The figure shows β-coefficients: Panel A from an unconditional regression and Panel B from a regression that includes 17 region fixed effects. Overall, the small but often significant coefficients suggest that treated towns were already "on the map," making it important to account for these pre-existing differences with town fixed effects. On the other hand, absence of significant pre-trends suggests that this positive selection was not made on pre-espionage textile growth, supporting the assumption that growth in control towns represents a valid counterfactual for the growth observed in treated towns.

Even if they were on parallel trends, a comparison of spy and non-spy towns would not reveal the effect of espionage if this was accompanied by other events. The possibility of correlated shocks is relevant because France spied for more than 70 years, during which much happened. A particularly important confounder is the Napoleonic blockade (21 November 1806–11 April 1814), that disproportionally benefited textile workshops in northeast France by shielding them from British competition (Juhász, 2018). As some of the textile spies settled in the same areas (Figure 1-Panel A), difference-in-differences estimates may conflate the effect of espionage with that of the blockade. The timing of the first post-spy survey helps

[6] I follow Chen and Roth (2024) and explicitly assign -2 to log(0), implying that the treatment effect on the extensive margin is 200% of the one on the intensive margin. Different assumptions do not affect the results.

address this concern, as the 1806 survey recorded activity *before* the start of the Napoleonic blockade (it is the "pre" period of Juhász, 2018). Thus, estimates of the effect of espionage on 1806 textile activity are not contaminated by the blockade that followed, and French textiles during the early 19th century provide evidence of the growth potential of two separate public policies: import protection and technological transfer through espionage, supporting the broad case for industrial policy (Juhász et al., 2024).

The last threat to identification comes from spillovers. Positive spillovers in the form of technological diffusion would promote the growth of non-treated towns, reducing differences with treated ones and dampening treatment effects. On the other hand, French companies not receiving new British technologies may find themselves at a disadvantage if they had to compete for clients and inputs with the firms hiring British spies. The towns where these firms operate may struggle as a result: if this was the case, a comparison of treated and control towns would exaggerate the treatment effect. I postpone a formal discussion of positive spillovers to Section 4, but anticipate that I find suggestive evidence of technology diffusion across the entire country over the long run (30 years), but limited evidence of localized spatial spillovers. On the other hand, negative spillovers are less likely. First, high transport costs, internal tariffs and strict regulations for internal trade limited exchanges across different regions of France. Second, labor migration was constrained by the "*billet de congé*" which required workers to obtain the permission of their current employers to change job (Horn, 2008). Thus, both output and labor markets were highly segmented. Consistent with this observation, I find similar results with and without region fixed effects, suggesting that negative spillovers across regions do not contaminate estimates.

Results. I estimate the following difference-in-differences model:

$$\log Y_{it} = \alpha_i + \alpha_t + \sum_{\substack{k=1692 \\ k \neq 1708}}^{1860} \beta_k \mathrm{Spy}_i + e_{it} \tag{2}$$

I measure espionage exposure of town i with the logarithm of espionage events or with an indicator for any event in town.[7] Town fixed effects α_i absorb time-invariant differences across treated and controls, and year fixed effects α_t account for conditions in the textile industry across the entire country at the time of survey t. In the most conservative specification, I replace these year fixed effects with year $\times$ region fixed effects α_{rt}, which help control for different conditions in the inputs and output markets of 17 regions, as well as heterogeneous

[7] For consistency, I assign -2 to log(0) also when it appears on the right-hand side of the regression. Different assumptions do not affect results.

accuracy in data collection across regions. The coefficients of interest are β_k, which identify the impact of espionage and are allowed to vary over time. Estimates are relative to the reference year 1708, and in the baseline estimate I exclude all towns that are missing in that year.

Espionage promoted textile growth. Figure 2 plots β_k of log spies for the three outcomes, in specifications controlling for region $\times$ year fixed effects. Full estimates are in Table A.1, that reports β_k of Equation (2) with town and year fixed effects (cols. 1, 3, 5) and with town and year $\times$ region fixed effects (cols. 2, 4, 6). Outcomes are textile employment (cols. 1–2), number of plants (cols. 3–4) and average plant size (cols. 5–6), which I observe consistently between 1692 and 1860. Panel A reports coefficients of the log of espionage events; Panel B of a dummy for any event. Across outcomes and specifications, I find no evidence of differential pre-trends, confirming the results of Table 1. I also find large effects of espionage on employment and plant size in 1806, at the end of the espionage program. Effects decline during the 1800s, but persist until 1860, when they are still large and significant. In contrast, I find smaller and generally non-significant results when the outcome is number of plants.

The effects are economically significant. For both employment and plant size, estimates of Table A.1-Panel A imply elasticities of 1 and 0.5 in 1806 and 1860, suggesting that doubling the number of espionage events would lead to roughly a doubling of textile employment in 1806 and a 50% larger sector in the second half of the century. Similarly, Table A.1-Panel B implies that receiving any industrial secret through espionage leads to +170% more employment in 1860. While these effects appear large, the new technologies represented a radical change in textile manufacture, and allowed firms to replace antiquated domestic processes with modern factory production. The small effects of espionage on the number of plants should be interpreted in this light: the new technologies helped displace the myriads of weavers that in the surveys of 1692–1708 declared to be working on their own account with few, large establishments employing labor and modern machines. All in all, Table A.1 suggests that France industrialized thanks in part to espionage.

Robustness of results. Results are robust to several checks. First, the conclusions of Table A.1 are strengthened when pre-trends are used to bound difference-in-differences estimates. Appendix Figure A.4 uses estimates of β_{1692} to construct bounds to post-spy coefficients. For this exercise, I pool post-espionage periods to estimate a single coefficient and then use the method of Rambachan and Roth (2023) to construct bounds of confidence intervals. These bounds never contain zero, confirming the robustness of the results to violations of the parallel trends assumption informed by observed pre-trends. Second, despite the spatial correlation of regressions' residuals (Moran's I statistics in Appendix Table A.2 show spatial dependence up to 600 km), Appendix Table A.3 shows that estimates remain significant with standard errors corrected to account for spatial dependence with the formula of Conley (1999).

3.2 Beyond Textiles: the Spies That Never Arrived

During the 1700s, Britain pulled ahead in most manufacturing sectors—by the end of the century, the Industrial Revolution was in full swing, and British entrepreneurs produced with the most advanced technologies in industries ranging from shipbuilding to metal objects, from steel-making to chemicals. As French governments saw British advantage grow across manufacturing, they tried to catch up by acquiring these new technologies through industrial espionage. Expert workers from Britain brought new technologies across many sectors: Alcock came with the secrets of metal goods production, Holker Jr. taught the chemical processes necessary to produce artificial dyes and several British workers introduced the sheathing of vessels with copper. The industrial surveys of 1839 and 1860 cover all manufacturing sectors and provide an opportunity to evaluate the success of the entire espionage program.

Two limitations constrain the analysis of sectors other than textiles. First, historians suggest that in the case of non-textile espionage, positive selection of French destinations may have dominated, possibly because employment benefits of new technologies were less important in these industries than in textiles, and possibly because geographical advantages determined the viability of some of these ventures. So iron works went close to Le Creusot's rich deposits of iron and coal, and masters skilled in sheathing ships with copper headed for Romilly, within easy access of the seas and the English Channel. It is possible that the same characteristics that made these destinations attractive to British spies would have promoted growth even without their presence.

The second limitation of this analysis is the lack of pre-1800 data. The surveys of 1692 and 1708 only covered textiles and, while it is possible that other sectors were surveyed on other occasions, I have not been able to locate other pre-espionage surveys. Thus, the results of this section must be interpreted with caution, as lack of data prevents the estimation of two-way fixed effects models and, different from textiles, selection may bias estimates upwards. With these *caveats* in mind, I first show correlations of several industrial outcomes with espionage, and then introduce a different identification strategy that exploits failed attempts at espionage.

I start by estimating the following regression:

$$\log Y_{ist} = \alpha_s + \alpha_t + \sum_{k=1839}^{1860} \beta_k \cdot \log S_{is} + e_{ist} \tag{3}$$

Outcomes here are observed across two years, 16 sectors, and 374 towns. Appendix Table A.4 reports coefficients from models with year and sector fixed effects α_t, α_s (Panel A), sector and year $\times$ region fixed effects α_s, α_{rt} (Panel B) and sector and year $\times$ department fixed effects α_s, α_{dt}. Departments are roughly equally sized areas introduced in 1790 by the Revolutionary government (Chambru et al., 2024): as they contained between three and four towns (*arrondissements*), these estimates compare industrial development of spy and non-spy towns in close proximity. As in the difference-in-differences analysis, I allow the effect to differ in 1839 and 1860. Different from that analysis, however, early surveys do not constrain the analysis to variables that can be measured consistently since 1692, and I can study the correlation of espionage with a richer set of industrial outcomes.

Growth was fast in every sector affected by espionage. Appendix Table A.4 shows the effect of espionage on number of workers (col 1), plants (col 2), plant size (col 3), production (col 4), value added (col 5) and value added per worker (col 6). Across outcomes and specifications, I find that towns and sectors visited by spies perform better during the 1800s: they have more workers, more and larger plants, produce more and have higher labor productivity as proxied by value added per worker. In short, the French towns and sectors that received the stolen technologies of Britain during the 1700s were larger and more productive one hundred years later.

While causal effects remain elusive when pre-espionage data is not available, episodes of failed espionage provide an opportunity to address positive selection. Some French towns were selected for spying, but never received stolen technology because the spies they were waiting for were arrested in Britain before they could sail to the Continent. As these towns were considered for espionage, they arguably resemble the other towns that eventually received British technology. And because arrests in Britain were often a matter of chance, they were unlikely to be related to conditions in France, including the growth potential of spies' destinations. In other words, selection bias is likely minor when comparing treated towns where successful spies arrived to placebo towns where arrested spies never did. And since treatments that did not happen should change nothing, the performance of these placebo destinations provides a useful benchmark against which to contrast estimates of successful espionage.

Formal statistical tests support the intuition that arrested and successful spies headed to similar French destinations. I estimate the correlation of pre-espionage outcomes of destinations of successful and arrested spies with the following regression:

$$\log Y_i = \beta_0 + \beta_1 \text{Spy}_i + \gamma \text{Arrested Spy}_i + u_i \tag{4}$$

I then calculate $\beta - \gamma$ and plot it on Figure 3, along with its estimated confidence interval. Panel A shows estimates from regressions without controls, Panel B adds 17 region fixed effects. The figure demonstrates that all French towns considered for espionage had similar pre-existing characteristics, whether or not British spies reached them. This provides strong support for the assumption that these towns shared similar potential outcomes and that selection bias was minor.

Towns of arrested spies did not grow faster. I augment Equation (3) with the number of arrested spies. Table 2 shows that destinations of these arrested spies have more plants in 1839–60 but are otherwise not significantly different from locations that were never considered for espionage. Coefficients of failed espionage are smaller than those of successful espionage, but the difference is mostly not significant at standard levels. In sum, while spy towns are ahead of the rest of France in 1839–60, results with placebo events cannot rule out that some or all of this growth would have happened even without the contribution of British technology. However, within the textile sector, the same strategy gives sharper results.

To validate and complement estimates in Section 3.1, I estimate on textile outcomes the modified difference-in-differences model:

$$\log Y_{it} = \alpha_i + \alpha_t + \beta_{\text{post}} \text{Spy}_i + \gamma_{\text{post}} \text{Arrested Spy}_i + e_{it} \tag{5}$$

Equation (5) pools all post-1800 periods to study the textile growth of both successful and failed espionage. Table 3 reports β_{post} and γ_{post}: odd columns include town and year fixed effects, even columns include town and year $\times$ region fixed effects. The table shows that spies promoted growth only when they found their way to France: growth performance of the destinations of arrested spies is small and not significantly different from zero. When the outcome is employment, the difference between successful and failed espionage is 1.5–2.2 (p=0.11–0.05). Together with the difference-in-differences results, this exercise thus provides strong support for the conclusion that textile espionage was decisive for the long-run industrialization of the French sector.

Taken together, the evidence in this section indicates that espionage helped France industrialize. Destinations of British textile spies were not selected based on past growth and had arguably similar growth potential to other towns in the same regions. Moreover, the textile sector of towns chosen by spies expanded fast during the 1700s, but only when these spies actually brought new textile technologies: arrested spies had no effect on growth of their destinations, supporting a causal interpretation of these estimates. Overall, textile espionage appears to have promoted the growth of the sector in France, and results from other sectors provide suggestive evidence of positive effects in these sectors too. But ideas travel fast: how did new British production methods give lasting advantage to the towns that received them first?

4 The Long-Run Impact of Ideas

The lasting impact of British technology brought to France in the 1700s on the geography of French industry requires an explanation. After all, ideas are portable and "infinitely replicable" (Jones, 2022). Even if entrepreneurs were jealous of their inventions, and failed espionage demonstrates the challenges of technology transfer, we may expect good ideas to spread over time, dissipating the initial advantage of those towns that learned these ideas first. Long-run effects documented in Section 3 suggest that diffusion was either slow or compensated by other forces. This section shows that technology did in fact diffuse, and that towns that saw the new British technologies first stayed ahead through continued innovation.

Technology diffusion is evident in the 1806 survey. In that year, *intendants* recorded the use of two modern weaving technologies: the *jenny* and the *mule*, which combined the jenny with the water frame of Arkwright. British spies brought to France both technologies: the jenny arrived with John Holker Jr. and the mule was first rebuilt in France by Pickford (Linant de Bellefonds, 1971). Importantly, the two technologies came at different times: the jenny in 1771 and the mule twenty years later, in 1791. The geographic distribution of these machines in 1806 gives clues on the speed of adoption. Table 4 shows that textile spies do not predict the location of jennies in 1806: 35 years were enough to allow wide adoption of this technology. In contrast, textile espionage is a strong predictor of mule adoption in the Napoleonic period. These results provide suggestive evidence that technology diffusion was not immediate, but it did occur over time.

If data indicate broad technology diffusion, evidence of *local* diffusion is more limited. To investigate the diffusion of British technologies over short distances I augment Equation (3) with an indicator of espionage activity in one of the neighboring towns. Appendix Table A.5 shows that proximity to spy towns did not support local industry, perhaps because entrepreneurs introducing the new machines sought to keep the technological secrets for themselves. They could not always avoid being victim of espionage, but when this happened the French workers stealing their secrets settled far from their factories, contributing to the national spread of the technologies but limiting local spillovers.

In sum, available evidence indicates a slow but positive diffusion of new ideas. This in turn implies true effects of espionage that are likely larger than those documented in Section 3, for two reasons. First, as stolen technology eventually spread throughout France, it increased aggregate national productivity. These aggregate positive effects are not identified by difference-in-differences models which suffer from a "missing intercept" problem (Wolf, 2023). Second, as these technologies spread to non-treated towns, they contributed to their growth, thus reducing the gap with treated towns and biasing estimates downwards. For these reasons, I conclude that difference-in-differences estimates in Section 3.1 likely represent lower bounds for the full effect of espionage on French industrial development.

Espionage had long-lasting effects, despite technology diffusion, likely because it spurred further innovation. The office of the *Institut National de la Propriété Industrielle* (INPI) maintains all patents granted since the French Revolution. Few inventions were patented before the end of the Napoleonic Wars (1815) and none before 1792, when patents were introduced by the revolutionary government, making an analysis of the pre-trends of this outcome impossible. However, detailed information on technology and inventors allows me to compare patenting in treated and control towns during the first half of the 1800s, providing clues on French innovation in the towns of espionage.

I estimate a variation of Equation (3) where the logarithm of textile patents is the outcome and the effect of espionage is allowed to vary over the five decades ending in 1850:

$$\log \text{Patents}_{it} = \alpha_t + \sum_{k=1810s}^{1860s} \beta_k \cdot \log S_i + e_{it} \tag{6}$$

Figure 4 reports estimates of β_k in regressions with decade fixed effects α_t (Panel A), decade $\times$ region fixed effects α_{rt} (Panel B), and decade and department fixed effects α_{dt} (Panel C). Towns that received British technologies through espionage during the 1700s patented more in the 1800s: the effect is largest in the first decade, declines over time, but remains large and significant into the 1860s.

Appendix Figure A.5 reports correlations between espionage and patents across all sectors and paints a similar picture: espionage is associated with more inventive activity years later, suggesting that after stealing British technology, French spy towns remained ahead through continued innovation.

The results on diffusion and innovation speak to the literature of semi-endogenous growth, which posits that an economy's growth rate is a function of its population growth. Peters (2022) provides empirical support to these theories by showing the growth effect of a permanent shock to the stock of *people* provoked after World War II by the millions of East German refugees moving to West Germany. French espionage shows that a shock to the stock of *ideas* has a similar effect on growth, and for the same reasons. Peters (2022) argues that larger populations allowed German towns to produce more ideas, thus promoting growth. In the case of France, British ideas attracted people to work with the new technologies. These newly established industrial clusters in turn proved fertile ground to develop the next wave of inventions that appeared during the 1800s.

5 Conclusions

This paper shows how 18^{th}-century espionage helped France adopt the new technologies of the Industrial Revolution, despite British efforts to ban these transfers. The introduction of these technologies had long-lasting effects in the textile sector and beyond, likely because they spurred follow-on innovation. These results indicate that technology transfer can be an effective industrial policy even without the cooperation of the technological leader, provided that it involves expert workers with knowledge of the new processes. They also suggest that industrial spies can help close the technology gap even when they abandon companies at the technological frontier, thus extending the results of Glitz and Meyersson (2020) on East German espionage, which operated from inside rival companies. One important question not addressed in this paper is the impact of French espionage on British competition and innovation. This is left to future research.

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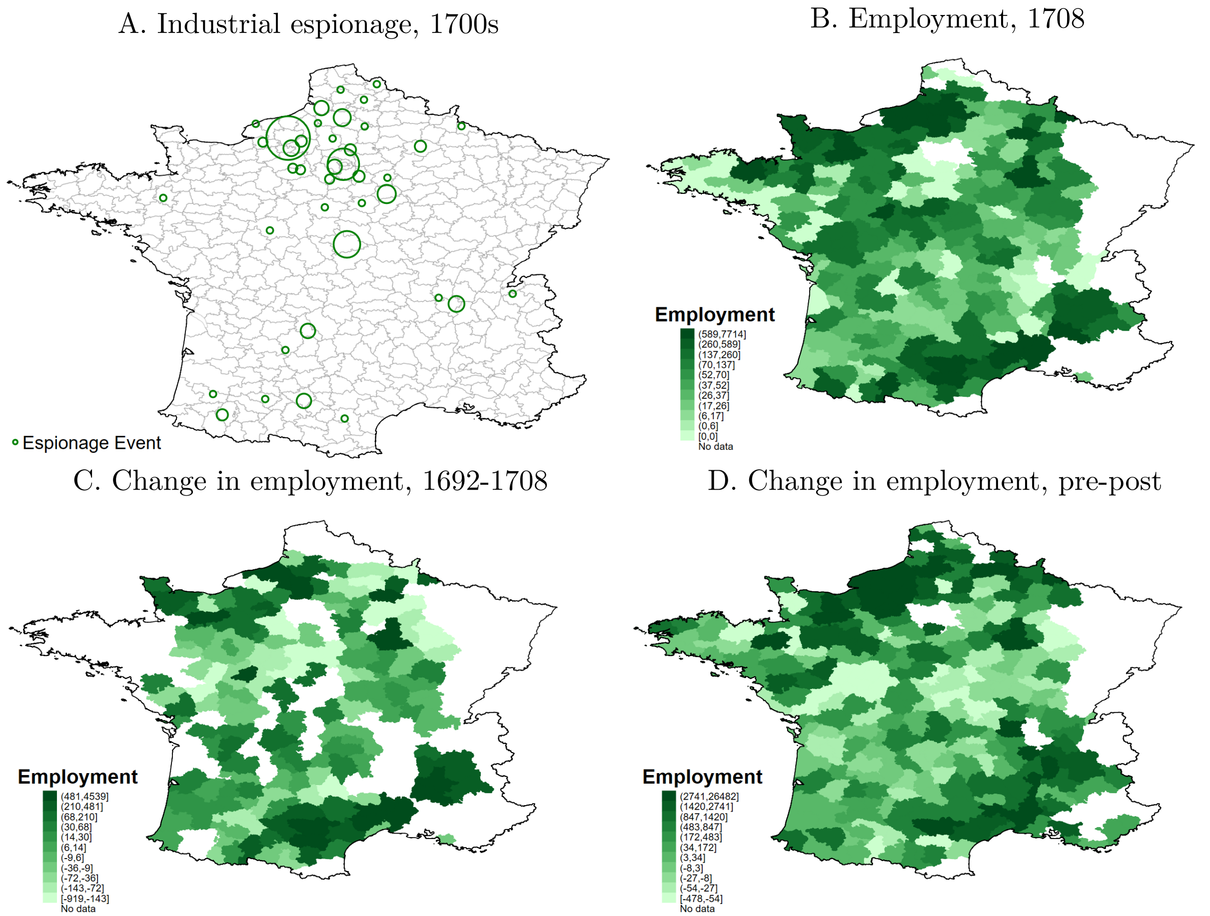
Figures

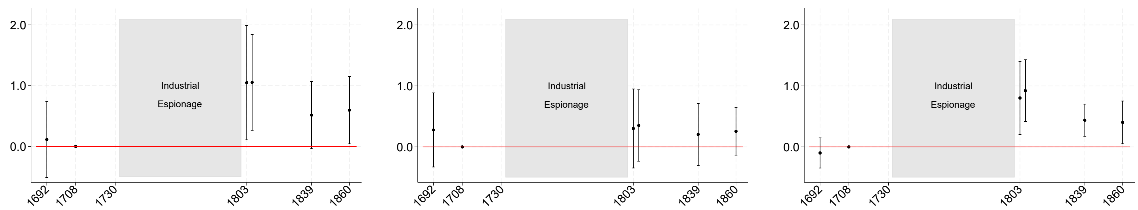
Figure 1: French industrial espionage and textile employment in the 1700s and 1800s



Notes: Panel A: destinations of British spies settling in France during the 1700s. Panel B: textile employment in 1708. Panel C: 1708–1692 change in textile employment. Panel D: average change in textile employment: 1692–1708 vs 1804–1860. Sources: spies: Harris (1998); Linant de Bellefonds (1971); employment: industrial surveys.

Figure 2: The impact of 1700s textile espionage on textile growth

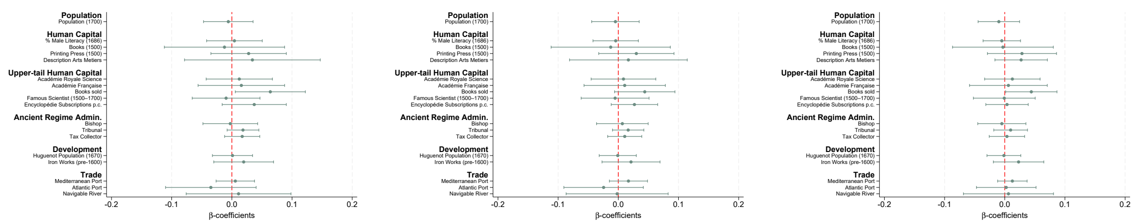
A. Employment B. Plants C. Plant size



Notes: Difference-in-differences estimates of β_k in equation (2). Dependent variable: Panel A: log textile employment. Panel B: log number of textile plants. Panel C: log average size of textile plants. Sources: spies: Harris (1998); Linant de Bellefonds (1971); employment: industrial surveys.

Figure 3: Pre-existing differences in destinations of successful and arrested spies

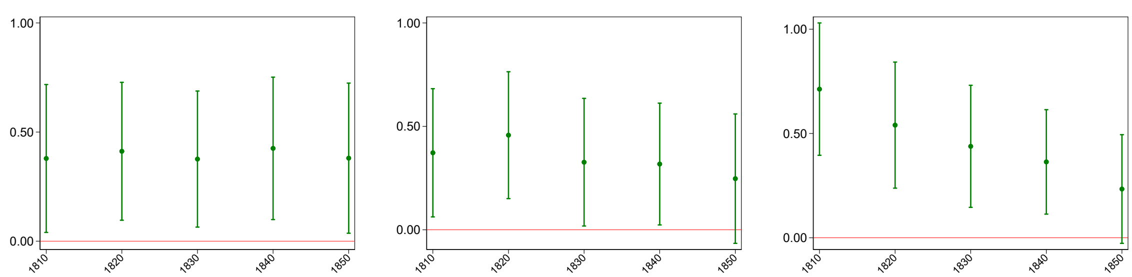
A. Unconditional B. Region fixed effects C. Department fixed effects



Notes: Differences in pre-espionage characteristics in destinations of successful and arrested spies. The graph reports the difference and 95% confidence interval of $\beta_1 - \gamma$ from Equation (4). Confidence intervals are calculated with robust standard errors. Panel A: regression only includes indicators for successful and arrested spies. Panel B: includes region fixed effects. Panel C: includes department fixed effects.

Figure 4: Textile espionage in the 1700s and textile patenting in the 1800s

A. Decade fixed effects B. Decade × region fixed effects C. Decade × department fixed effects



Notes: Textile espionage and patenting. The graph reports the point estimate and the 95% confidence interval of β_k from equation (6). Confidence intervals are calculated with standard errors clustered at town level. The dependent variable is the log of textile patents. Panel A: regression includes decade fixed effects. Panel B: includes decade × region fixed effects. Panel C: includes decade × department fixed effects.

Tables

Table 1: Changes in textile activity before the arrival of spies

	Treated (s.d.)	Control (s.d.)	Difference (s.e.)	p-value
Δ log workers (1708-1692)				
Unconditional	-0.640	0.804	-1.444	0.247
	(3.506)	(2.987)	(1.198)	
Region F.E. (16)			-1.603	0.119
			(0.969)	
Δ log plants (1708-1692)				
Unconditional	-1.152	0.659	-1.811	0.129
	(3.439)	(2.957)	(1.126)	
Region F.E. (16)			-1.994	0.044
			(0.909)	
Δ log plant size (1708-1692)				
Unconditional	0.262	0.368	-0.105	0.790
	(1.188)	(0.990)	(0.388)	
Region F.E. (16)			-0.118	0.692
			(0.293)	

Notes: Differences in the 1692–1708 changes in number of workers, number of plants and plant size. Treated towns have at least one spy, control towns have none. For every outcome the table reports the unconditional difference between treated and controls and the difference after netting out 16 region fixed effects. Sample consists of 160 towns for which textile data is available in both 1692 and 1708. Standard errors clustered at region level.

Table 2: Placebo: Effect of espionage on growth of all manufacturing sectors

Panel A. year & sector FEs	log plants (1)	log workers (2)	log size (3)	log production (4)	log VA (5)	log VA / worker (6)
log spies	0.487***	1.030***	1.016***	1.775***	1.743***	1.145***
	[0.084]	[0.128]	[0.120]	[0.202]	[0.192]	[0.131]
log arrested spies	0.542**	0.554	0.362	0.785	0.592	0.429
	[0.234]	[0.353]	[0.342]	[0.619]	[0.659]	[0.425]
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Sector FEs	Yes	Yes	Yes	Yes	Yes	Yes
p-value spy = arrested	0.831	0.253	0.103	0.164	0.119	0.135
Average dep. var	0.01	1.18	0.71	5.30	3.87	2.23
R^2	0	0	0	0	0	0
Observations	11,648	11,648	11,648	11,648	10,148	10,148

Panel B. year & region × sector FEs	log plants (1)	log workers (2)	log size (3)	log production (4)	log VA (5)	log VA / worker (6)
log spies	0.450***	0.943***	0.926***	1.592***	1.565***	1.023***
	[0.079]	[0.125]	[0.118]	[0.200]	[0.196]	[0.136]
log arrested spies	0.510**	0.474	0.291	0.638	0.445	0.336
	[0.245]	[0.370]	[0.359]	[0.658]	[0.695]	[0.453]
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Region × sector FEs	Yes	Yes	Yes	Yes	Yes	Yes
p-value spy = arrested	0.824	0.280	0.125	0.204	0.148	0.177
Average dep. var	0.01	1.18	0.71	5.30	3.87	2.23
R^2	0	0	0	0	0	0
Observations	11,648	11,648	11,648	11,648	10,148	10,148

Panel C. year & department × sector FEs	log plants (1)	log workers (2)	log size (3)	log production (4)	log VA (5)	log VA / worker (6)
log spies	0.410***	0.883***	0.886***	1.485***	1.504***	0.986***
	[0.071]	[0.129]	[0.126]	[0.210]	[0.205]	[0.139]
log arrested spies	0.424**	0.345	0.193	0.412	0.314	0.243
	[0.211]	[0.368]	[0.367]	[0.653]	[0.657]	[0.433]
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Department × sector FEs	Yes	Yes	Yes	Yes	Yes	Yes
p-value spy = arrested	0.955	0.203	0.089	0.138	0.101	0.122
Average dep. var	0.01	1.18	0.71	5.30	3.87	2.23
R^2	1	0	0	0	0	0
Observations	11,648	11,648	11,648	11,648	10,148	10,148

Notes: Estimates of β_{Post} and γ_{Post} in Equation (5). Dependent variables: col 1: log workers; col 2: log plants; col 3: log plant size; col 4: log production; col 5: log value added; col 6: log value added per worker. All regressions include 16 sectors fixed effects; regressions in Panel A include two year fixed effects; regressions in Panel B include year × 21 region fixed effects; regressions in Panel C include year × 95 department fixed effects. Sample includes the 325 towns across 16 sectors and two years (1839 and 1860). Standard errors clustered at town level in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Table 3: Placebo: Effect of arrested spies on textile growth in destination towns

	log workers		log plants		log size	
	(1)	(2)	(3)	(4)	(5)	(6)
log spies × post 1800	0.830**	0.920**	0.303	0.390	0.638***	0.662***
	[0.363]	[0.419]	[0.281]	[0.312]	[0.201]	[0.239]
log arrested spies × post 1800	-0.651	-1.243*	-0.315	-0.749	-0.331	-0.583
	[0.567]	[0.723]	[0.428]	[0.589]	[0.315]	[0.379]
Spies & arrested spies × 1692	Yes	Yes	Yes	Yes	Yes	Yes
Year & arr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year × region FEs	No	Yes	No	Yes	No	Yes
R^2	0.234	0.393	0.295	0.477	0.353	0.448
Mean dep. var.	3.21	3.21	1.68	0.98	0.98	0.98
p-value spy = arrested × post	0.108	0.052	0.380	0.184	0.058	0.040
Observations	1240	1240	1240	1240	1237	1237

Notes: Estimates of β_{Post} and γ_{Post} in Equation (5). Dependent variables: cols 1–2: log workers; cols 3–4: log plants; cols 5–6: log plant size. All regressions include town and year fixed effects; regressions on even columns include year × region fixed effects. The p-value at the bottom of the table tests the null that $\beta_{\text{Post}} = \gamma_{\text{Post}}$. Sample includes the 226 towns observed in 1708 whenever data is available in one of the survey years: 1692, 1708, 1804, 1806, 1839 and 1860. Standard errors clustered at town level in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Table 4: Effect of 1700s espionage on adoption of stolen technology in 1806

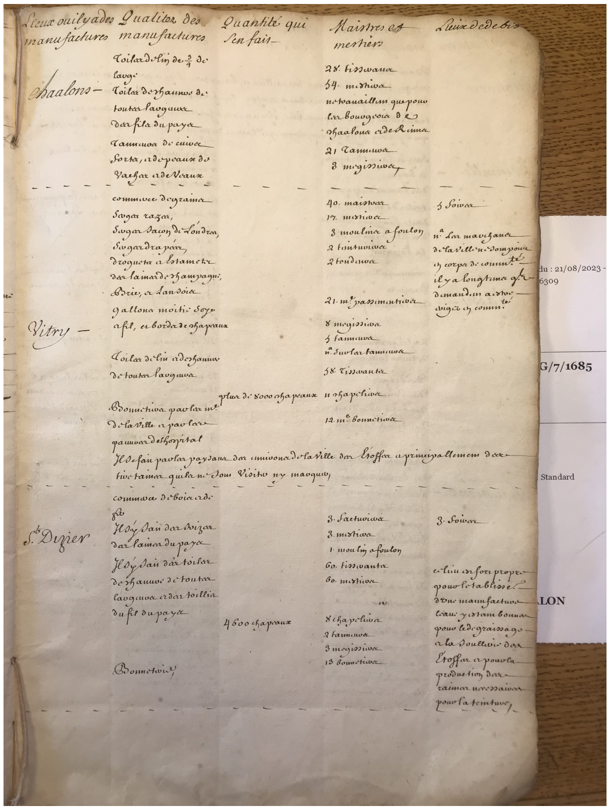
	log jennies (1806)		log mules (1806)	
	(1)	(2)	(3)	(4)
log spies	0.345	0.387	0.739***	0.611**
	[0.297]	[0.285]	[0.254]	[0.263]
Region FEs	No	Yes	No	Yes
R^2	0.012	0.113	0.041	0.172
Mean dep. var.	-1.14	-1.14	-0.92	-0.92
Observations	289	289	288	288

Notes: Correlation of espionage activity with diffusion of two stolen technologies in 1806. The "jenny" arrived in France with Holker Jr. in 1771. The first "mule" built in France was made by Pickford in 1791. Adoption is measured in 1806. Robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1.

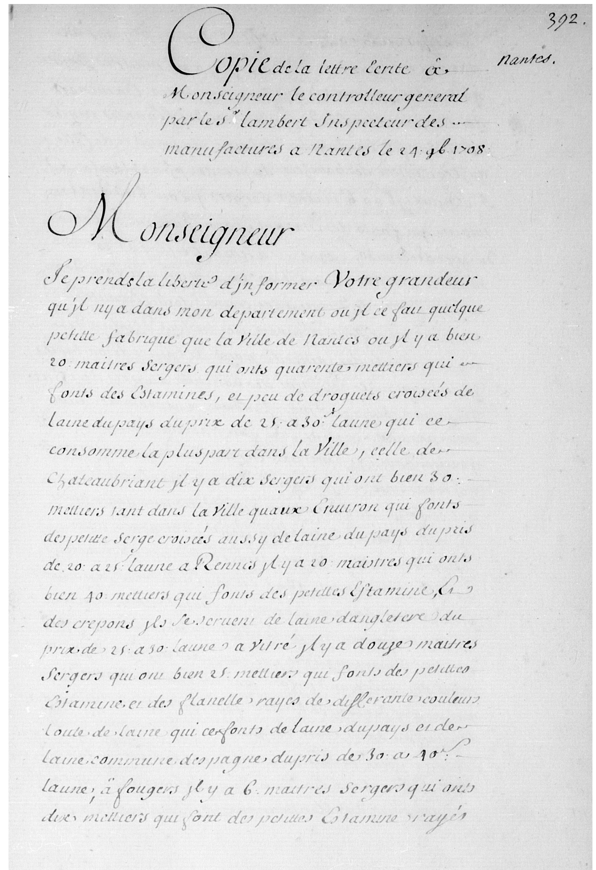
A Appendix Figures

Figure A.1: Examples of industrial surveys

(a) A. Survey 1692



B. Survey 1708



392.

Nantes.

Copie de la lettre écrite à Monseigneur le controlleur general par le S.r Lambert Inspecteur des manufactures a Nantes le 24 9bre 1708

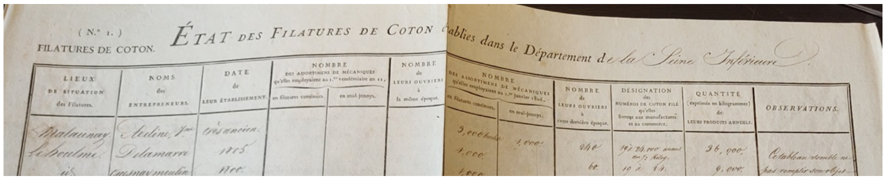
Monseigneur

Je prends la liberté d'informer Votre grandeur qu'il n'y a dans mon departement ou il se fait quelque petitte fabrique que la Ville de Nantes ou il y a bien 20 maitres Sergers qui onts quarente metiers qui fonts des Estamines, et peu de droguets croisées de laine du pays du prix de 25 a 30 s. l'aune qui se consomme la pluspart dans la Ville, celle de Chateaubriant il y a dix Sergers qui ont bien 30 metiers tant dans la Ville qu'aux Environ qui fonts des petitte serge croisées aussy de laine du pays du pris de 20 a 25 s. l'aune a Rennes il y a 20 maistres qui onts bien 40 metiers qui fonts des petittes Estamine, et des crepons ils se servent de laine d'anglettere du prix de 25 a 30 l'aune a Vitré il y a douze maitres Sergers qui ont bien 25 metiers qui fonts des petittes Estamine et des flanelle rayes de differante couleurs toute de laine qui se fonts de laine du pays et de laine commune d'espagne du pris de 30 a 40 s. l'aune, a Fougers il y a 6 maitres Sergers qui onts dix metiers qui font des petittes Estamine rayés

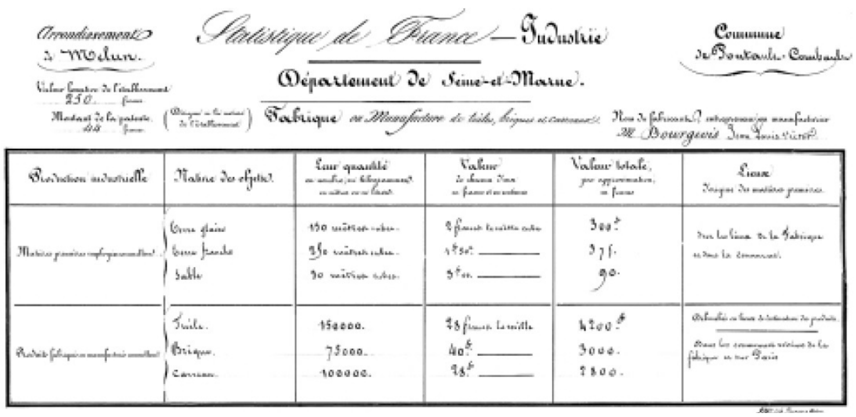
Notes: Examples of pages taken from the industrial surveys of 1692 (Panel A), 1708 (Panel B). Information in 1708 was extracted from letters written by *intendants* to the Ministry of Commerce.

Figure A.2: Examples of industrial surveys

A. Survey 1806



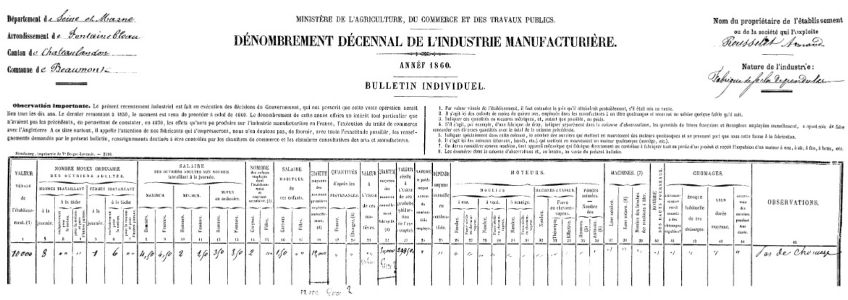
(N.° 1.)
FILATURES DE COTON.
ÉTAT DES FILATURES DE COTON établies dans le Département de la Seine Inférieure



Statistique de France — Industrie
Département de Seine-et-Marne.

B. Survey 1839

C. Survey 1860

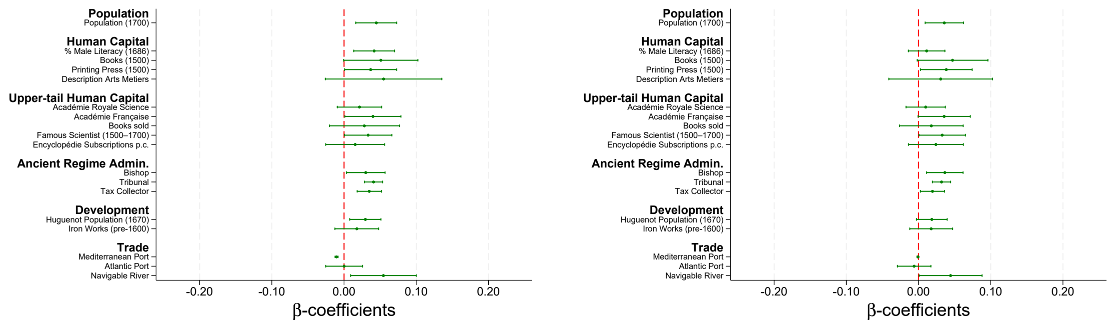


MINISTÈRE DE L'AGRICULTURE, DU COMMERCE ET DES TRAVAUX PUBLICS.
DÉNOMBREMENT DÉCENNAL DE L'INDUSTRIE MANUFACTURIÈRE.
ANNÉE 1860.
BULLETIN INDIVIDUEL.

Notes: Examples of pages taken from the industrial surveys of 1806 (Panel A), 1839 (Panel B) and 1860 (Panel C). The 1806 survey asked about textile activity in 1803.

Figure A.3: Pre-existing differences in destinations of successful and arrested spies

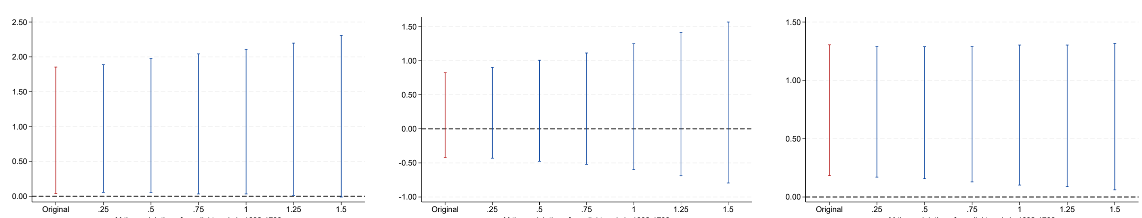
A. Unconditional B. Region fixed effects



Notes: Differences in pre-espionage characteristics in towns with and without spies. The graph reports the point estimate and the 95% confidence interval of an indicator for textile espionage in regressions where the dependent variable is one of the outcomes reported on the right of the graph. Confidence intervals are calculated with robust standard errors. Panel A: regression only includes indicators for successful spies. Panel B: includes region fixed effects.

Figure A.4: Robustness to violations of the pre-trends assumption

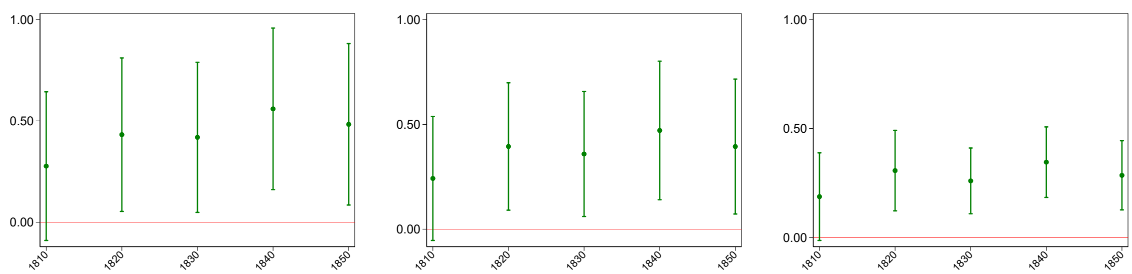
A. Employment B. Plants C. Plant size



Notes: The figure reports bounds for the coefficients β_{Post} from Equation (2) calculated with the method of Rambachan and Roth (2023) to account for possible violations of the parallel trends assumption. Dependent variables: Panel A: log textile employment. Panel B: log number of textile plants. Panel C: log average size of textile plants. We allow non-parallel trends with the Relative Magnitude method (RM) and report bounds for deviations from the parallel trends assumption that are equal to M times observed pre-trends in 1692–1708, with $M \in \{0.25, 0.50, 0.75, 1.00, 1.25, 1.50\}$. We also report in red the bounds when the parallel trends assumption holds ($M = 0$). Lines show 90% confidence bounds.

Figure A.5: Espionage in the 1700s and manufacturing patenting in the 1800s

A. Decade fixed effects | B. Decade × region fixed effects | C. Decade × department fixed effects



Notes: Espionage and patenting across 16 manufacturing sectors. The graph reports the point estimate and the 95% confidence interval of β_k from equation (6). Confidence intervals are calculated with standard errors clustered at town level. The dependent variable is the log of patents. Panel A: regression includes sector and decade fixed effects. Panel B: includes sector and decade × region fixed effects. Panel C: includes sector and decade × department fixed effects.

B Appendix Tables

Table A.1: The impact of 1700s textile espionage on textile growth

	log workers		log plants		log size	
Panel A. number of espionage events	(1)	(2)	(3)	(4)	(5)	(6)
log spies × 1860	0.596**	0.687**	0.257	0.376*	0.402**	0.416**
	[0.283]	[0.319]	[0.200]	[0.220]	[0.179]	[0.209]
log spies × 1839	0.515*	0.625*	0.205	0.345	0.438***	0.438**
	[0.282]	[0.318]	[0.259]	[0.267]	[0.135]	[0.188]
log spies × 1806	1.054***	0.994**	0.351	0.283	0.923***	0.943***
	[0.402]	[0.468]	[0.299]	[0.349]	[0.258]	[0.285]
log spies × 1803	1.049**	0.947*	0.302	0.201	0.802***	0.744**
	[0.480]	[0.552]	[0.330]	[0.378]	[0.306]	[0.341]
log spies × 1692	0.114	0.185	0.278	0.341	-0.099	-0.063
	[0.318]	[0.264]	[0.310]	[0.251]	[0.126]	[0.113]
Year & arr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year × region FEs	No	Yes	No	Yes	No	Yes
R^2	0.237	0.393	0.295	0.476	0.357	0.451
Mean dep. var.	3.21	3.21	1.68	1.68	0.98	0.98
Observations	1240	1240	1240	1240	1237	1237

	log workers		log plants		log size	
Panel B. any espionage event	(1)	(2)	(3)	(4)	(5)	(6)
Any spy × 1860	1.698**	1.932**	0.776	1.102*	1.132**	1.140*
	[0.825]	[0.931]	[0.577]	[0.645]	[0.527]	[0.600]
Any spy × 1839	1.459*	1.730*	0.681	1.035	1.142**	1.118**
	[0.854]	[0.978]	[0.752]	[0.802]	[0.460]	[0.561]
Any spy × 1806	2.726**	2.622*	0.788	0.678	2.519***	2.553***
	[1.302]	[1.417]	[0.928]	[1.001]	[0.839]	[0.893]
Any spy × 1803	2.797*	2.572	0.612	0.418	2.211**	2.041**
	[1.428]	[1.575]	[1.010]	[1.061]	[0.918]	[1.007]
Any spy × 1692	0.441	0.579	0.983	1.098	-0.316	-0.241
	[0.962]	[0.780]	[0.896]	[0.709]	[0.401]	[0.362]
Year & arr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year × region FEs	No	Yes	No	Yes	No	Yes
R^2	0.235	0.393	0.295	0.477	0.357	0.452
Mean dep. var.	3.21	3.21	1.68	1.68	0.98	0.98
Observations	1240	1240	1240	1240	1237	1237

Notes: Estimates of β_k in Equation (2). Dependent variables: cols 1–2: log workers; cols 3–4: log plants; cols 5–6: log plant size. All regressions include town and year fixed effects; regressions on even columns include year × region fixed effects. Panel A: espionage is measured with log espionage events. Panel B: espionage is an indicator for any espionage event. Sample includes the 226 towns observed in 1708 whenever data is available in one of the survey years: 1692, 1708, 1804, 1806, 1839 and 1860. Standard errors clustered at town level in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Table A.2: Spatial inference: Moran's I (p-value)

	log workers		log plants		log plant size	
100 km	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
200 km	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
400 km	0.0000	0.0000	0.0000	0.0000	0.0000	0.3052
600 km	0.0000	0.5861	0.0000	0.0018	0.0000	0.6585
Town FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	No	Yes	No	Yes	No
Region × year FEs	No	Yes	No	Yes	No	Yes

Notes: The table reports the p-value of Moran's I statistics at different bandwidths. The null hypothesis is no spatial correlation in the residuals of regressions (2). Dependent variables: cols 1–2: log workers; cols 3–4: log plants; cols 5–6: log plant size. All regressions include town and year fixed effects; regressions on even columns include year × region fixed effects. Sample includes the 226 towns observed in 1708 whenever data is available in one of the survey years: 1692, 1708, 1804, 1806, 1839 and 1860.

Table A.3: Robust spatial inference: standard errors corrected with the formula of Conley (1999)

	Workers (ihs)		Plants (ihs)		Size (ihs)	
log spies × post 1800	0.765	0.788	0.272	0.311	0.606	0.601
Cluster: town	[0.311]**	[0.360]**	[0.240]	[0.269]	[0.172]***	[0.205]***
Conley s.e.: 25 Km cutoff	[0.257]***	[0.275]***	[0.203]	[0.208]	[0.144]***	[0.160]***
Conley s.e.: 50 Km cutoff	[0.252]***	[0.267]***	[0.199]	[0.205]	[0.141]***	[0.155]***
Conley s.e.: 100 Km cutoff	[0.274]***	[0.272]***	[0.221]	[0.217]	[0.150]***	[0.156]***
Conley s.e.: 200 Km cutoff	[0.288]***	[0.302]***	[0.232]	[0.236]	[0.153]***	[0.172]***
Conley s.e.: 400 Km cutoff	[0.265]***	[0.278]***	[0.203]	[0.211]	[0.148]***	[0.166]***
Conley s.e.: 600 Km cutoff	[0.221]***	[0.223]***	[0.157]*	[0.166]*	[0.133]***	[0.145]***
Conley s.e.: 800 Km cutoff	[0.189]***	[0.178]***	[0.130]**	[0.133]**	[0.123]***	[0.130]***
Region × year FEs	No	Yes	No	Yes	No	Yes

Notes: Robustness to correcting standard errors for spatial correlation with the formula of Conley (1999). The table reports the coefficient of log spies interacted with a "Post 1800" indicator. Dependent variables: cols 1–2: log workers; cols 3–4: log plants; cols 5–6: log plant size. All regressions include town and year fixed effects; regressions on even columns include year × region fixed effects. The first row reports point estimates; the following rows report standard errors corrected in different ways. Row 2: standard errors clustered at town level. Rows 3–9: standard errors corrected with the formula of Conley (1999). The cutoff is 25 (row 3), 50 (row 4), 100 (row 5), 200 (row 6), 400 km (row 7), 600 km (row 8) and 800 km (row 9). Sample includes the 226 towns observed in 1708 whenever data is available in one of the survey years: 1692, 1708, 1804, 1806, 1839 and 1860.

Table A.4: Effect of espionage on growth of all manufacturing sectors

Panel A. year & sector FEs	log plants (1)	log workers (2)	log size (3)	log production (4)	log VA (5)	log VA / worker (6)
log spies × 1860	0.544***	1.037***	0.899***	1.612***	1.704***	0.985***
	[0.139]	[0.140]	[0.104]	[0.221]	[0.197]	[0.127]
log spies × 1839	0.577***	1.177***	1.245***	2.173***	1.918***	1.393***
	[0.090]	[0.132]	[0.133]	[0.230]	[0.223]	[0.162]
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Sector FEs	Yes	Yes	Yes	Yes	Yes	Yes
Average dep. var	0.01	1.18	0.71	5.30	3.87	2.23
R^2	0.45	0.35	0.28	0.34	0.33	0.32
Observations	11,648	11,648	11,648	11,648	10,148	10,148

Panel B. year & region × sector FEs	log plants (1)	log workers (2)	log size (3)	log production (4)	log VA (5)	log VA / worker (6)
log spies × 1860	0.476***	0.894***	0.762***	1.318***	1.393***	0.772***
	[0.122]	[0.125]	[0.105]	[0.202]	[0.209]	[0.134]
log spies × 1839	0.562***	1.127***	1.185***	2.067***	1.826***	1.330***
	[0.088]	[0.132]	[0.132]	[0.236]	[0.226]	[0.167]
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Region × sector FEs	Yes	Yes	Yes	Yes	Yes	Yes
Average dep. var	0.01	1.18	0.71	5.30	3.87	2.23
R^2	0.47	0.38	0.31	0.37	0.36	0.35
Observations	11,648	11,648	11,648	11,648	10,148	10,148

Panel C. year & department × sector FEs	log plants (1)	log workers (2)	log size (3)	log production (4)	log VA (5)	log VA / worker (6)
log spies × 1860	0.395***	0.773***	0.670***	1.110***	1.251***	0.677***
	[0.067]	[0.129]	[0.138]	[0.226]	[0.238]	[0.142]
log spies × 1839	0.543***	1.098***	1.175***	2.006***	1.806***	1.324***
	[0.082]	[0.133]	[0.136]	[0.242]	[0.231]	[0.169]
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Department × sector FEs	Yes	Yes	Yes	Yes	Yes	Yes
Average dep. var	0.01	1.18	0.71	5.30	3.87	2.23
R^2	0.52	0.43	0.36	0.42	0.41	0.40
Observations	11,648	11,648	11,648	11,648	10,148	10,148

Notes: Estimates of β_{1860} and β_{1839} in Equation (3). Dependent variables: col 1: log workers; col 2: log plants; col 3: log plant size; col 4: log production; col 5: log value added; col 6: log value added per worker. All regressions include 16 sectors fixed effects; regressions in Panel A include two year fixed effects; regressions in Panel B include year × 21 region fixed effects; regressions in Panel C include year × 95 department fixed effects. Sample includes the 325 towns across 16 sectors and two years (1839 and 1860). Standard errors clustered at town level in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Table A.5: Spillovers: Effect of textile espionage on growth in neighboring towns

	log workers		log plants		log size	
	(1)	(2)	(3)	(4)	(5)	(6)
log spies × post 1800	0.778**	0.832**	0.283	0.340	0.608***	0.622***
	[0.310]	[0.355]	[0.238]	[0.265]	[0.171]	[0.204]
log spies (spillover) × post 1800	-0.264**	-0.347**	-0.242**	-0.233**	-0.049	-0.169**
	[0.129]	[0.137]	[0.104]	[0.110]	[0.082]	[0.082]
Any spy (direct & spillover) × 1692	Yes	Yes	Yes	Yes	Yes	Yes
Year & arr. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year × region FEs	No	Yes	No	Yes	No	Yes
R^2	0.238	0.398	0.300	0.480	0.354	0.450
Mean dep. var.	3.21	3.21	1.68	1.68	0.98	0.98
Observations	1240	1240	1240	1240	1237	1237

Notes: Effect on textile growth of any espionage activity in a town or in the surrounding towns. Dependent variables: cols 1–2: log workers; cols 3–4: log plants; cols 5–6: log plant size. All regressions include town and year fixed effects; regressions on even columns include year × region fixed effects. Sample includes the 226 towns observed in 1708 whenever data is available in one of the survey years: 1692, 1708, 1804, 1806, 1839 and 1860. Standard errors clustered at town level in parentheses. ***p<0.01, **p<0.05, *p<0.1.

Table A.6: Industrial espionage in the 1700s and patents in the 1800s

	log textile patents			log all patents		
	(1)	(2)	(3)	(4)	(5)	(6)
log spies × 1810	0.379**	0.372**	0.713***	0.277	0.242	0.187*
	[0.173]	[0.158]	[0.162]	[0.187]	[0.151]	[0.103]
log spies × 1820	0.412**	0.457***	0.540***	0.432**	0.395**	0.307***
	[0.161]	[0.156]	[0.154]	[0.193]	[0.155]	[0.094]
log spies × 1830	0.376**	0.327**	0.438***	0.419**	0.359**	0.260***
	[0.159]	[0.157]	[0.149]	[0.189]	[0.152]	[0.077]
log spies × 1840	0.425**	0.318**	0.364***	0.560***	0.471***	0.346***
	[0.166]	[0.150]	[0.128]	[0.204]	[0.169]	[0.082]
log spies × 1850	0.380**	0.247	0.234*	0.483**	0.394**	0.285***
	[0.176]	[0.160]	[0.133]	[0.203]	[0.164]	[0.081]
R^2	0.065	0.233	0.516	0.153	0.198	0.308
Decade FEs	Yes	No	No	Yes	No	No
Decade × region FEs	No	Yes	No	No	Yes	No
Decade × department FEs	No	No	Yes	No	No	Yes
Sector FEs	No	No	No	Yes	Yes	Yes
Mean dep. var.	-0.66	-0.65	-0.59	-1.31	-1.31	-1.31
Observations	1073	1069	989	16095	16095	16095

Notes: Industrial espionage during the 1700s and patents during the 1800s. Dependent variable: log patents. Cols 1–3: textile patents. Cols 4–6: all manufacturing patents. All regressions include decade fixed effects; col. 2 and 5 include decade × region fixed effects; cols 3 and 6 include decade × department fixed effects; cols 4–6 include sector fixed effects. Standard errors clustered at town level in parentheses. ***p<0.01, **p<0.05, *p<0.1.