American Economic Review 2012, 102(1): 396–427
http://dx.doi.org/10.1257/aer.102.1.396

Compulsory Licensing: Evidence from the Trading with the Enemy Act†

By Petra Moser and Alessandra Voena*

Compulsory licensing allows firms in developing countries to produce foreign-owned inventions without the consent of foreign patent owners. This paper uses an exogenous event of compulsory licensing after World War I under the Trading with the Enemy Act to examine the effects of compulsory licensing on domestic invention. Difference-in-differences analyses of nearly 130,000 chemical inventions suggest that compulsory licensing increased domestic invention by 20 percent. (*JEL* D45, L24, N42, O31, O34)

Compulsory licensing allows firms in developing countries to produce foreign inventions without the consent of foreign patent owners.[1] Countries such as Brazil, Thailand, and India have used the policy to procure life-saving drugs for millions of patients and are proposing it as a means to access foreign technologies to combat climate change.[2] Opponents of compulsory licensing, however, fear that the policy may reduce access to critical innovations that are invented abroad, as it weakens incentives for foreign firms to transfer new technologies into developing countries. For example, the US pharmaceutical company Merck criticized Brazil's licensing of its HIV drug efavirenz as an "expropriation of intellectual property" which will "hurt patients who require new life-saving therapies" (http://www.ip-watch.org/, May 7, 2007).

* Moser: NBER and Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94705 (e-mail: pmoser@stanford.edu); Voena: Harvard Kennedy School, 79 JFK Avenue, Cambridge, MA 02138 (e-mail: alessandra_voena@hks.harvard.edu). We thank David Autor, Tim Bresnahan, Iain Cockburn, Giacomo De Giorgi, Christina Gathmann, Eric Hilt, Todd Newman, Tom Nicholas, Joerg Ohmsted, Andrea Pozzi, Mike Scherer, Gavin Wright, and seminar participants at Arizona, Berkeley, Chicago Booth, ISNIE, the NBER, and Stanford, for helpful comments. We owe special thanks to Ryan Lampe for helping us collect the US patent data, and Kathryn Steen for sharing copies of archival records. Shaun Hayes at the American Heritage Center of the University of Wyoming helped us to access the personal papers of the Alien Property Custodian. Adam Tarhouni, Emily Rains, Leah Karlins, Marina Kutyavina, and Stephanie Lee provided excellent research assistance. Petra Moser thanks the National Science Foundation for its financial support through grant SES0921859, and the NBER's Group on Innovation Policy and the Economy and the Kauffman Foundation for funding the early stages of this project. Alessandra Voena acknowledges financial support through the Ely Fellowship at SIEPR and the Clayman Institute for Gender Studies at Stanford.

† To view additional materials, visit the article page at http://dx.doi.org/10.1257/aer.102.1.396.

[1] In general, TRIPS Art.31 allows compulsory licenses after negotiations for voluntary licenses have failed. In cases of emergency, TRIPS allows governments to grant compulsory licenses without first trying to negotiate. The World Trade Organization (WTO) Doha Declaration of 2001 emphasized developing countries' rights to issue compulsory licenses: "Each member has the right to grant compulsory licenses and the freedom to determine the grounds upon which such licenses are granted" (WT/MIN(01)/DEC/1, Art. 5.b).

[2] Thailand and Brazil have used compulsory licenses to produce antiretrovirals for AIDS patients and India has indicated plans to use compulsory licensing to combat swine flu (Kremer 2002; Galvão 2002; Gostin 2006; Steinbrook 2007).

Policy debates have, however, neglected an important aspect of compulsory licensing: does compulsory licensing increase or discourage *domestic* invention in countries that license foreign technologies? Compulsory licensing may discourage domestic invention if access to foreign inventions at below-market rates weakens incentives to develop alternative technologies domestically. The ability to produce foreign inventions could, however, also enable domestic firms to establish their own independent production, which strengthens incentives to invest in complementary research and skills and creates opportunities for learning by doing (e.g., Arrow 1962; Stokey 1988; Irwin and Klenow 1994).

To test whether compulsory licensing increases or decreases domestic invention, we take advantage of an exogenous episode of compulsory licensing as a result of World War I. On October 6, 1917, Congress passed the Trading with the Enemy Act (TWEA). Section 10 of the Act permitted US firms to violate enemy-owned patents if they contributed to the war effort.[3] As the war dragged on, the TWEA became more and more punitive (Steen 2001). One week before the Armistice at Compiègne on November 11, 1918, Congress amended the TWEA to confiscate all enemy-owned patents. By February 1919, German-owned patents were systematically licensed to US firms.

To measure the effects of compulsory licensing, we compare changes in the number of patents by domestic inventors across technologies that were differentially affected by the TWEA. This allows us to control for alternative factors that may have encouraged domestic invention across chemical technologies, such as improvements in education and scientific training (e.g., Landau and Rosenberg 1992) or tariff barriers intended to protect the US chemical industry (Eichengreen 1989; Irwin 1998). Technologies are measured at the level of subclasses of United States Patent and Trademark Office (USPTO) patents in organic chemistry. Chemical inventions in all of these subclasses were affected by tariff barriers and improvements in education, but only some subclasses were affected by compulsory licensing.

Three complementary variables measure compulsory licensing. A binary variable identifies subclasses that received at least one license under the TWEA. Two additional variables control for differences in the number and in the novelty of licensed patents.

Changes in domestic invention are measured by the number of US patents granted to US inventors per subclass and year. To construct the data, we collected information on all 19 USPTO classes of organic chemicals that received at least one of 727 compulsory licenses of enemy-owned patents under the TWEA. These 19 classes produced a total of 128,953 patents between 1875 and 1939 and covered 7,248 subclasses; 336 of these subclasses were treated.

These data reveal a substantial increase in domestic invention in subclasses that were affected by compulsory licensing. In subclasses that received at least one license, domestic inventors produced an average of 0.151 additional patents per year after the TWEA compared with other subclasses. This implies an increase in domestic patents of nearly 25 percent relative to an average of 0.619 patents per subclass between 1919 and 1939. Tests that control for the number of compulsory licenses

[3] 12 USC. § 95a. Today, Cuba is the only country still affected by the TWEA.

indicate that each additional license generated 0.072 additional patents per subclass and year. In subclasses where US firms licensed patents that were 10 years younger, domestic inventors produced 0.060 additional patents per year.

We also examine the timing of effects, which may help shed some light on the mechanisms by which licensing encourages domestic invention. If licensing increases domestic invention through learning by doing, effects may take several years to materialize, as domestic firms learn to produce foreign inventions and build their own production capacities. This process might be especially slow if domestic inventors need "time to learn," as Arora and Rosenberg (1998, p. 79) suggest to have been the case for organic chemicals in the United States.[4] In fact, our data on US patents suggest that pre-TWEA levels of domestic invention were especially low in treated subclasses.

Estimates of annual treatment effects confirm that the full impact of compulsory licensing occurred with a lag of eight to nine years. Enemy-owned patents were licensed from 1919 to 1926, with most licenses being granted from 1919 to 1922 (Steen 2001). Although annual treatment effects become significant as early as 1927, the strongest effects occur for patents that were granted after 1931. Given that patent grants occur two to three years after applications in our data, this implies that the largest effects on applications began in 1928—six to nine years after most patents had been licensed. Effects remained large and significant at nearly 60 percent additional patents per subclass and year throughout the 1930s.

One caveat with these results is that the licensing decisions of US firms may not have been exogenous, even though the timing of the TWEA and the types of technologies that could be licensed were exogenous. Most importantly, US inventors may have been especially eager to license foreign inventions in subclasses where the demand for domestically produced goods was high, so that the observed effect may be the result of an interaction between the demand for domestic production and the ability to license foreign inventions. On the other hand, the demand for licenses may have been highest in subclasses where levels of domestic invention were initially low. In those subclasses domestic invention is likely to have increased more slowly because US firms had to bridge a larger gap to the technological frontier before they could patent their own inventions.

To control for the potential influence of alternative factors, we subject the data to a series of additional tests. Triple difference regressions account for unobservable characteristics that may have encouraged patenting by *all* non-German inventors in treated subclasses. Specifically, we compare changes in patenting by domestic inventors with changes in patenting by other non-German inventors before and after the TWEA. Triple difference estimates confirm that licensing encouraged patenting by domestic inventors, even relative to other non-German inventors. An alternative test artificially exposes French inventors, who could not license enemy patents under the TWEA to "treatment" by compulsory licensing. In this test, compulsory licensing has no effect.

[4] Also see Haber (1971); Aftalion (2001); Mowery and Rosenberg (1998). In 1923 chemical trials during a court case established that a skilled US chemist could not reproduce synthetic organic chemicals based on confiscated German patents: Louis Freedman, who had earned degrees from Yale and Columbia proved unable to produce cincophen, a drug to treat gout (Steen 2001). Additional delays may result from incomplete information in patent documents. The German firm BASF, for example, withheld critical information about the Haber-Bosch process from its patent application and US firms took nearly a decade to replicate its process (Haynes 1945).

To assess the direction and size of selection bias, we estimate intent-to-treat (ITT) and instrumental variable (IV) regressions, where the number of enemy-owned patents that US firms could have licensed under the TWEA measures the ITT and IV variables. ITT estimates are slightly smaller than OLS estimates, while IV estimates are somewhat larger, which indicates that selection bias (such as the concentration of licensing in subclasses with low initial skill levels) may indeed lead us to underestimate the true effects of compulsory licensing.

Additional robustness checks control for preexisting time trends and variation above the subclass level, regressions on a restricted sample of primary subclasses, and regressions for changes in patenting within a specific chemical (indigo dyes).

In a final section of the paper, we perform a firm-level analysis that distinguishes the effects of patents that were licensed to a specific US firm (Du Pont) from the effects of patents that were licensed to other firms. Effects of own licenses are more likely to result from learning that occurs when a firm produces foreign inventions, while other licenses capture factors that benefit the industry more broadly, such as improvements in education. Our results suggest that both types of mechanisms were important, but effects of own licenses were roughly four times as large as effects of other firms' licenses.

The remainder of this paper is structured as follows. Section I summarizes basic features of the TWEA. Section II presents our empirical strategy. Section III details the data collection and discusses potential sources of bias and measurement error. Section IV presents estimation results, Section V presents robustness checks, and Section VI summarizes results of our firm-level analysis. Section VII concludes.

I. The TWEA as a Natural Experiment of Compulsory Licensing

Created by an Act of Congress on October 6, 1917, the TWEA was intended to "dislodge the hostile Hun within our gates" (Alien Property Custodian 1919, p. 17) to destroy "Germany's great industrial army on American soil," its "spy centers," and "nests of sedition" (Alien Property Custodian 1919, p. 14). To this end, the TWEA placed all enemy property "beyond the control of influence of its former owners, where it cannot eventually yield aid or comfort to the enemy" (Alien Property Custodian 1919, p. 13).[5]

On March 28, 1918, the TWEA was amended to give the Custodian the power to sell enemy property, including all enemy-owned patents, "as though he were the owner thereof" (Alien Property Custodian 1919, p. 22). Thus, the Alien Property Custodian began to appropriate any patent owned by "enemy persons" and corporations doing business in Germany, Austria-Hungary, Bulgaria, and Turkey, as well as the occupied parts of Belgium, France, Russia, and the Balkans (Alien Property Custodian 1919, p. 7), administering these properties as a trust.

[5] The destruction of German property was also intended to prevent Germany from starting another war: " … the great overshadowing result which has come from this war is the assurance of peace almost everlasting amongst the peoples of the earth. It would help to make that an absolute certainty by refusing to permit Germany to prosecute a war after the war... if she can get out of the war with her home territory intact, rebuild a stable government and still have her foreign markets subject to her exploitation, by means no less foul and unfair than those which she has employed on the field of battle, we shall not be safe from future onslaughts different in methods … " (Alien Property Custodian 1919, p. 16).

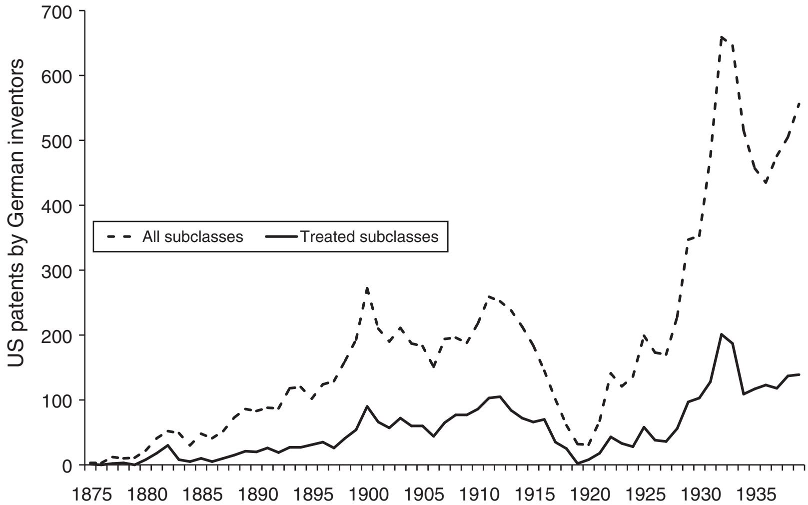

Figure 1. US Patents by German Inventors (1875–1939)

Notes: Data from www.uspto.gov and the *LexisNexis Chronological Patent Files* (*1790–1970*) include all 128,953 patents between 1875 and 1939 in 19 USPTO classes that received at least one license under the TWEA. These 21 main classes cover 7,248 subclasses, 336 of which are treated. Data on inventor nationality are based on a keyword search for country names in *LexisNexis*.

By February 22, 1919, Mitchell Palmer, the Alien Property Custodian and President of the Bureau of Investigation (today's Federal Bureau of Investigation) felt comfortable to say that "practically all known enemy property in the United States has been taken over by me and is administered according to the provisions of the trading with the enemy act" (Alien Property Custodian 1919, p. 7); 35,400 reports of alien property had been received, and 27,274 trusts had been created, with a total value exceeding $500 million in 1919, equivalent to $4.7 billion in 2008 (online Appendix Table A1).[6]

At the time of the TWEA, the US organic chemical industry was largely based on natural, wood-based products, and lagged behind in more complex processes, including organic synthesis (e.g., Aftalion 2001, Arora and Rosenberg 1998). In these areas, foreign patentees dominated US markets. For example, 70 percent of all US patents for synthetic organic compounds between 1900 and 1910 were granted to German firms (United States Tariff Commission 1918, Haynes 1945, Steen 2001). While World War I temporarily suspended German competition, German firms swiftly returned to US markets and resumed patenting in the 1920s (Figure 1; also see Aftalion 2001, and Genesove 2006).

The TWEA granted US firms access to all patents that had been owned by enemies during the war. On behalf of the US government, the Chemical Foundation began

[6] Using the GDP deflator as a conservative measure; based on relative shares of GDP, the 2008 equivalent would be $88 billion (Williamson 2008).

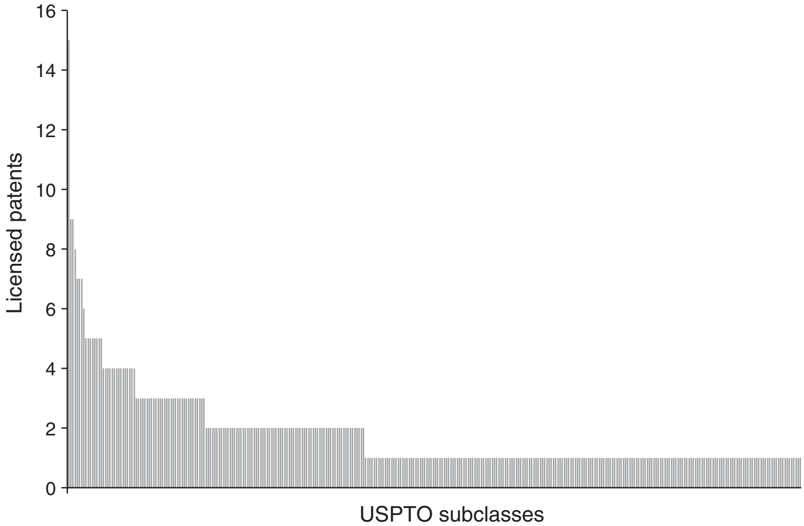

Figure 2. Licensed Patents per Treated Subclass

Notes: Data from Haynes (1945) and www.uspto.gov. The y-axis records the number of licensed patents in a treated subclass. Treated subclasses are defined as subclasses that received at least one license under the TWEA; 336 subclasses in our data where treated.

to issue nonexclusive licenses of enemy patents in 1919.[7] Licensing continued until 1926, though most licenses were granted from 1919 to 1922 (Steen 2001).

II. The Data

Our treatment variable consists of 727 enemy-owned chemical patents that were licensed to US firms; the outcome variable includes all 128,953 US patents in 19 USPTO (main) classes that received at least one compulsory license under the TWEA.

A. *Data on the Treatment: Licensed Enemy Patents*

Under the TWEA, the United States confiscated over 4,500 enemy-owned patents for chemical inventions. Of these patents, 727 were licensed by the Chemical Foundation to one or more of 326 US firms from 1919 to 1926 (Haynes 1945). Exact data on the grant dates of licenses are unavailable, although we know that most licenses occurred from 1919 to 1922 (Steen 2001). Licensed patents belong to 336 primary and secondary subclasses, which we define as treated. Most subclasses received one license (Figure 2), but a small number of subclasses received up to 15 licenses; the average subclass received patents that were valid for another 23 years (Figure 3).

[7] In 1921 the Chemical Foundation owned 4,764 patents, 874 trademarks, and 492 copyrights. Although licenses were sold below market rates, the foundation collected nearly $700,000 in royalties (approximately 7 million 2008 dollars, using the GDP deflator).

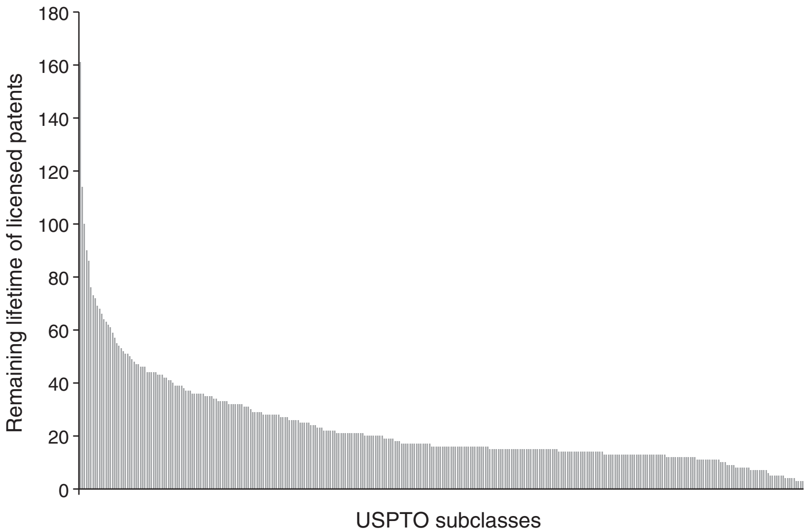

Figure 3. Remaining Years of Patent Life per Treated Subclass

Notes: Data from Haynes (1945) and www.uspto.gov. The y-axis records the total years of remaining patent life for all licensed patents in a treated subclass. For each licensed patents, the remaining years of patent life are calculated by subtracting the patent's age in 1919 from 17 years (patent life in the United States in 1919). Treated subclasses are defined as subclasses that received at least one license under the TWEA; 336 subclasses in our data where treated.

B. *Data on the Outcome: US Patents 1875–1945*

Domestic invention is measured as the number of US patents by domestic inventors per subclass and year. We have collected these data from the USPTO database *US Patent Master Classification File* (www.uspto.gov) for all 19 USPTO classes of chemicals that received at least one dyestuff license under the TWEA (online Appendix Table A2). Between 1875 and 1945, these 19 classes produced 128,953 patents in 7,248 subclasses, including 336 treated subclasses.

Ideally, we would measure changes in domestic invention based on the application (rather than grant) dates of US patents. Because data limitations only allow us to measure grant dates, we estimate the length of the lag between patent applications and grants. In a sample of 493 dyestuff patents between 1930 and 1933, the median patent is granted three years after the application (with a 25th percentile of two and a 75th percentile of four years).[8]

Patents by domestic inventors are measured by subtracting foreign patents from the total number of US patents per year. Foreign patents are US patents by inventors from Argentina, Australia, Austria, Belgium, Brazil, China, England, France, Germany, India, Italy, the Netherlands, Russia, Scotland, Spain, and Switzerland. Inventors' countries of origins are identified through keyword searches for country

[8] More generally, the lag between applications and grants has been shown to vary over time and across technologies, depending, among other factors, on the complexity of patent applications and the workload of examiners (Popp, Juhl, and Johnson 2004). To measure the size of the lag in our sample, we searched the site www.google.com/patents for patents that include the word "dye." Google capped our search at 600 patents; 536 of these patents included application dates, and 493 belong to our sample.

names in the *LexisNexis Chronological Patent Files (1790–1970)*. For example, we assign a patent to be of a German inventor if it contains the word "Germany" anywhere in title or in the description of the invention.

Data on inventor nationality reveal that German firms quickly reentered the United States after the war, despite the potential incentive effects of the TWEA (Figure 1).[9]

C. *Measurement Error and Attenuation Bias*

Our data may be subject to measurement error in the way we assign patents to inventor nationalities. Specifically, we may overestimate the number of patents by domestic inventors if countries that are not included in our search patented a significant number of inventions; this error, however, is likely to be small. Another type of measurement error results from using Optical Character Recognition (OCR) to identify patents by foreign inventors, because OCR is worse at recognizing misspelled names or untidy script than the human eye.[10]

Although there is no reason to believe that these errors vary systematically across treated and untreated subclasses, we hand-collected inventor nationalities of 625 patents of alizarin, indigo, azo dyes, and aniline, which Delamare and Guineau (2000) consider the most important dyes in the early twentieth century, to check for systematic bias. For these patents we identify inventors' nationalities by carefully reading the full text of each patent. A comparison of the hand-collected and machine-collected data reveals no significant differences in inventor nationalities across subclasses (Table 1 and online Appendix Figure A1).

Another type of measurement error results from our use of the USPTO classification system. Specifically, inventors' propensity to patent may vary across subclasses (Scherer 1977, Lerner 1995, and Moser 2009) and we may underestimate patenting in subclasses that are narrowly defined. To address these issues, all regressions include subclass-specific fixed effects.

Most importantly, however, the narrow definition of treated technologies at the level of USPTO subclasses may lead us to underestimate the effects of compulsory licensing: our estimation assumes that treatment effects are limited to inventors in a specific subclass. Given the narrow definition of USPTO subclasses it is, however, likely that some effects of compulsory licensing spill over to other subclasses that are included in our control.

[9] German discoveries in the 1920s and 30s include the production of insulin in 1922 (using pancreas glands from slaughterhouses), estradiol (progynone) in 1928, and Raschig's phenol synthesis via the catalytic chlorination of benzene in 1935 (Aftalion 2001). According to contemporary accounts, Germany's quick re-entry to chemical research was partly fueled by wartime profits from the production of combat gases and explosives (Aftalion 2001).

[10] To identify as many foreign inventors as possible, we search for the name of a foreign country anywhere in the document. This overestimates the number of foreign inventors, if patent applications use the country name in a different context. For example, we wrongly assign USPTO patent 1,674,085 to Great Britain, because its inventors (who came from Massachusetts) also applied for a patent in Britain and mentioned this in their patent document. Several cross-checks of our data, however, indicate that such errors are rare. Improvements in the quality of OCR over time will be captured by annual fixed effects.

Table 1— Hand-Collected versus Algorithm-Assigned Nationalities

Inventor Nationality	Hand-collected	Algorithm-assigned
United States	240	289
German	225	197
Other foreign	160	139
Total	625	625

Notes: Data from Haynes (1945), www.uspto.gov, the *LexisNexis Chronological Patent Files* (*1790–1970*) and www.google.com/patents. To collect data on inventor nationality, we create an algorithm that performs keyword searches on LexisNexis. This algorithm relies on Optical Character Recognition (OCR), which is worse at recognizing misspelled names or untidy script than the human eye. To check for measurement error, we hand-collected an alternative data set that includes all 625 patents for the most important dyes of the early 20th century (Delamare and Guineau 2000): alizarin, indigo, azo dyes, and aniline. In the hand-collected sample, inventors come from Argentina, Australia, Austria, Belgium, Brazil, China, England, France, Germany, India, Italy, the Netherlands, Russia, Scotland, Spain, Switzerland, and the United States.

III. Results

Our empirical strategy compares changes in domestic invention between 1875 and 1939 across chemicals that were differentially affected by the TWEA. The dependent variable is the number of patents by US inventors per USPTO subclass and year:

$$\textit{Patents by US inventors}_{c,t} = \alpha_0 + \boldsymbol{\beta}'\mathbf{TREAT_c} \cdot \textit{postTWEA}_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t},$$

where **TREAT** is a vector of treatment variables and *postTWEA* equals 1 for every year after 1918. In our most basic specification, we define a subclass as treated if it contained at least one enemy-owned patent that was licensed to a US firm. The control variable Z measures the total number of foreign patents; it controls for unobservable factors, such as technological progress within subclasses. The variable δ indicates year fixed effects and f subclass fixed effects.[11] The coefficient β on the interaction term between $\mathbf{TREAT_c}$ and $\textit{postTWEA}_t$ is the standard difference-in-differences estimator (e.g., Duflo 2001).

Regression results reveal a high and statistically significant correlation between compulsory licensing and patenting by domestic inventors: in subclasses where domestic firms benefited from compulsory licensing, domestic inventors produced between 0.151 and 0.255 additional patents per year after 1919 (Table 2, columns 1–2, significant at 1 percent). Compared with an average of 0.619 annual patents in the average subclass after 1919, this implies a 24 to 40 percent increase in domestic

[11] Fixed effects include estimates for α_1 and α_2, from the standard difference-in-differences equation $\textit{Patents by US inventors}_{c,t} = \alpha_0 + \boldsymbol{\alpha}_1'\mathbf{TREAT_c} + \alpha_2 \cdot \textit{postTWEA}_c + \boldsymbol{\beta}'\mathbf{TREAT_c} \cdot \textit{postTWEA}_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$. In our simplest specification TREAT equals 1 if the subclass includes at least one licensed patent. In alternative specifications TREAT is a vector of the number of licensed patents per subclass and the total years of remaining patent life of all licensed patents, which enters linearly and nonlinearly.

Table 2— OLS Regressions, Dependent Variable is Patents by US Inventors per USPTO Subclass And Year (1875–1939)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subclass has at least one license	0.151***	0.255***						
	(0.036)	(0.038)						
Number of licenses			0.110***	0.072***	0.115***			
			(0.025)	(0.017)	(0.022)			
Number of licenses squared			−0.007***					
			(0.002)					
Remaining lifetime of licensed patents						0.009***	0.006***	0.010***
						(0.002)	(0.001)	(0.002)
Remaining lifetime of licensed patents squared (× 100)						−3.60e-05*		
						(2.19e-05)		
Number of patents by foreign inventors	0.283***		0.282***	0.283***		0.282***	0.282***	
	(0.018)		(0.018)	(0.018)		(0.018)	(0.018)	
Subclass fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471,120	471,120	471,120	471,120	471,120	471,120	471,120	471,120
Number of subclasses	7,248	7,248	7,248	7,248	7,248	7,248	7,248	7,248

Notes: Data from www.uspto.gov and the *LexisNexis Chronological Patent Files* (*1790–1970*). Our data consist of all 128,953 patents between 1875 and 1939 in 19 USPTO main classes that contained at least one licensed enemy dyestuff patent. These 19 main classes are subdivided into 7,248 subclasses. Data on inventor nationality are based on a keyword search for country names in LexisNexis. Regressions that include a two-year lag for number of patents by foreign inventors drop the first two years of data. Robust standard errors clustered at the subclass level in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

invention. Coefficients stay highly significant when standard errors are computed by a block bootstrap clustered at the subclass level to account for serial correlation in domestic patenting (online Appendix Table A3).[12] Controls for patents by foreign inventors have a measurable influence on treatment effects, but treatment effects remain large and statistically significant.

As a difference-in-differences estimator β is subject to a number of limitations: most importantly, β is consistent only if differences in patenting between treated and untreated subclasses that are not due to the TWEA remain constant over time. In the case of the TWEA, the reduced effectiveness of German competitors after 1914 may have had a larger effect on treated subclasses, where German competition was

[12] A potential problem with difference-in-differences estimation is that, in the presence of serial correlation in the dependent variable, standard errors may be underestimated even with clustering. For difference-in-differences estimations with a large number of groups a block bootstrap, which maintains the autocorrelation structure within groups by keeping observations that belong to the same group together in a "block," has been shown to perform best (Bertrand, Duflo, and Mullainathan 2004). Applied to our specific case, the block bootstrap maintains the structure of autocorrelations within subclasses, as it samples subclasses instead of observations. We draw a large number of (79) bootstrapped samples (the computer crashed at 79), and reject the hypothesis that $\beta = 0$ at a 99 percent confidence interval (online Appendix Table A3).

stronger prior to 1914; this implies that β may overestimate the effect of compulsory licensing on invention.[13]

To check for the effects of weakened competition, we compare changes in US patenting by German inventors in treated and untreated subclasses before and after the TWEA. As expected, the data show that US patents by German inventors declined sharply after 1914, dropping from 259 patents in 1911 to 61 patents in 1918, 32 patents in 1919, and 68 patents in 1920 (Figure 1). US patents by German inventors, however, recovered quickly after 1919, reaching 199 patents in 1925, and 353 patents in 1930. Moreover, there is no evidence that German competitors were more affected in treated subclasses after 1919. In treated subclasses, the number of US patents by German inventors declined from 103 patents in 1911 to 25 patents in 1918, 2 patents in 1919, and 18 patents in 1920. German inventors, however, recovered quickly in treated subclasses after 1919, reaching 58 patents in 1925 and 103 US patents in 1930.[14] These data are consistent with evidence that German firms had reentered US markets by 1921 to compete with US firms (Haynes 1945; Hounshell and Smith 1988; Arora and Rosenberg 1998; Genesove 2006).[15]

In the next step, we extend the analysis to control for variation in the number and in the age of licensed patents. Most subclasses received only one license under the TWEA, but a small number of subclasses received many licenses (Figure 2). Subclass 106/402, "compositions: coating or plastic—lakes," for example, received eight licenses. Similarly, most subclasses received licenses with 40 or fewer years of remaining patent life (measured as the total number of years that licensed patents will be valid after 1918 (Figure 3)). Licenses with more years of remaining patent life may be more valuable to US firms.[16] For example, compare a patent that was granted in 1903 with another that was granted in 1915. If both patents are licensed under the TWEA and technologies improve over time, the old patent becomes obsolete more quickly, and a license for the new patent conveys greater benefits.

An additional license increases domestic patents by 0.072 to 0.115 per year, equivalent to a 12 to 19 percent increase (Table 2, columns 3–5, significant at 1 percent).[17] An additional year of patent life increases the number of patents by 0.006 to 0.010 per year (Table 2, columns 6–8, significant at 1 percent), which implies that licensing a new patent in 1918 (with 17 years of remaining patent life) adds 0.102 to 0.17 patents per year (17 years $\times$ 0.006 to 0.010 patents per year), while licensing

[13] Factors that may have reduced the effectiveness of German firms include high ad valorem tariffs on chemical imports (Eichengreen 1989, Irwin 1998), and the confiscation of German subsidiaries (Mann and Plummer 1991; Hounshell and Smith 1988; Arora and Rosenberg 1998).

[14] A comparison of patents by German inventors as a share of all patents confirms their quick reentry after the war. After 1914, the share of patents by German inventors declined sharply from 15 percent in 1911 to 3, 1, and 1 percent in 1918, 1919, and 1920, respectively. After 1920, however, it recovered quickly, reaching 8 percent in 1925, and 13 percent in 1930. In treated subclasses, the share of patents by German inventors fell from 67 percent in 1911 to 20 percent in 1918, 2 percent in 1919, and 8 percent in 1920. Again, it recovered quickly, reaching 32 percent of all patents in 1925 and 37 percent in 1930, despite the increase in domestic US invention.

[15] The real profits of German chemical firms increased by 44 percent between 1913 and 1917, in part fueled by the production of combat gases and other war-related supplies (Baten and Schulz 2005; Aftalion 2001).

[16] For example, empirical evidence from patent citations suggests that patents with additional years of remaining patent life are more valuable (Hall, Jaffe, and Trajtenberg 2005).

[17] Consistent with the idea that the marginal benefits of additional knowledge are decreasing, coefficients on the square of licensed patents are negative. Taken to the extreme, this implies that, in subclasses that had already received more than 16 licenses, an additional license may discourage domestic invention. In practice, however, none of the 336 treated subclasses in our data received more than 15 licenses.

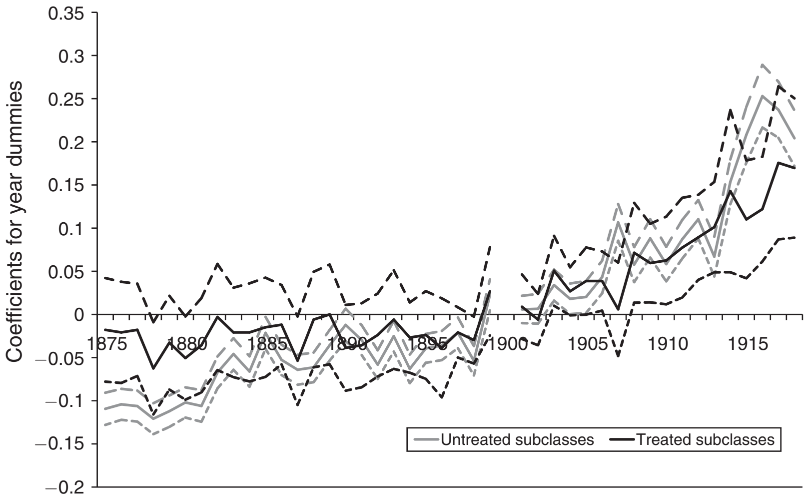

FIGURE 4. PRE-TWEA TIME TRENDS IN PATENTING BY DOMESTIC INVENTORS: TREATED VERSUS UNTREATED SUBCLASSES

Notes: Data from www.uspto.gov and the *LexisNexis Chronological Patent Files* (*1790–1970*) include all 128,953 patents between 1875 and 1939 in 19 USPTO classes that received at least one license under the TWEA. These 21 classes cover 7,248 subclasses, 336 of which are treated; the omitted year is 1900.

an old patent (with just one year of remaining patent life) adds no more than 0.010 (1 year × 0.006 to 0.010 patents per year).[18]

A. *Comparing Pretreatment Trends for Treated and Untreated Subclasses*

A potential challenge to the difference-in-differences strategy is that differential changes between treated and untreated subclasses may be driven by preexisting differences in the time trends of patenting. To address this issue, we allow β_t to vary across treated and untreated subclasses prior to the TWEA, using 1900 as the baseline.

$$\textit{Patents by US inventors}_{c,t} = \alpha_0 + \beta_t \cdot \textit{YEAR}_t \cdot \textit{TREAT}_c \cdot \textit{pre}1919_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}.$$

This test reveals no systematic differences in pre-trends across treated and untreated subclasses (Figure 4).

B. *Measuring Annual Treatment Effects*

In addition to average effects, we estimate annual treatment effects to examine the timing of changes in domestic invention. If compulsory licensing encourages invention

[18] To control for differences in the quality of licensed patents, we also match our data with citations in US patents between 1975 and 2002 (Hall, Jaffe, and Trajtenberg 2001); 154 of our 727 licensed patents were cited at least once. Adjusting treatment variables for citations has no significant effect on estimated effects.

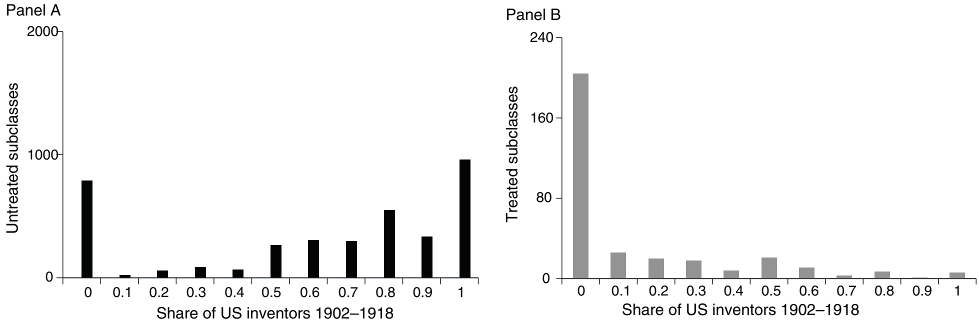

Figure 5. Pre-Twea Shares of Domestic Inventors: Treated versus Untreated Subclasses

Notes: Data on annual patents and inventor nationalities were constructed from www.uspto.gov and the *Lexis Nexis Chronological Patent Files* (*1790–1970*). Treated subclasses received at least one license under the TWEA. Data include 7,248 subclasses, 336 of which are treated.

through experience and learning-by-doing (e.g., Arrow 1962) the most significant changes should occur with some delay. Low initial skill levels in the licensing country (which, as we will show below, may disproportionately affect treated technologies) imply that domestic firms may need "time to learn," as Arora and Rosenberg (1998, p. 79) suggest to have been the case for the United States. In late 1919, for example, US dye companies succeeded in producing only a hundred more dyes than they had made before the war. The prospects of duplicating German inventions seemed almost hopeless. For example, Du Pont's initial runs of indigo turned out green, rather than blue (Hounshell and Smith 1988).

Incomplete information in patent documents may create further delays. At the time of the TWEA, the German chemical company BASF, for example, had "effectively bulwarked its discovery [of the Haber-Bosch process] with strong, broad patents which detailed meticulously the apparatus, temperatures and pressures, but cleverly avoided particulars as to the catalysts employed or their preparation" (Haynes 1945, pp. 86–87). As a result, a "prolonged learning experience was necessary to understand the two sides of catalysis, the chemical side and the engineering and design side."[19]

Even access to the physical capital of German-owned firms was not in and of itself sufficient to jump start US production. The Winthrop Chemical Company, which had acquired all of Bayer's production machinery in addition to its patents,

> *could not figure out how to make the sixty-three drugs that were supposed to be* (*its*) *stock-in-trade … The former German supervisors having been jailed or deported*, *nobody knew how to run the machines; … the patents*,

[19] Mowery and Rosenberg (1998, p. 75, citing Haber 1971, pp. 205–206). Additional delays may result from variation in business cycles, which constrain investments in R&D. For example, personnel cuts during the recession of 1920 deeply affected DuPont's research team on dyestuffs, which "had already been struggling with the burden of catching up with chemists in the German dye industry" (Hounshell and Smith 1988, p. 89). Between mid- and end-1920, the team's salary roll fell from 565 to 217, so that "(r)esearch chemists washed their own dishes, ran their own errands and did all of the experimental work" (Hounshell and Smith 1988, p. 89).

which were supposed to specify manufacturing processes, were marvels of obfuscation (Mann and Plummer 1991, pp. 52–53).

Annual treatment effects β_t help to evaluate the extent of such delays:

$$\textit{Patents by US inventors}_{c,t} = \alpha_0 + \beta_t \cdot \textit{TREAT}_c \cdot \textit{YEARpostTWEA}_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t},$$

where β_t measures the differential change in domestic patenting between treated and untreated subclasses in year t after the TWEA.

Consistent with historical accounts, annual coefficients indicate that the full effects of compulsory licensing took several years to materialize. Coefficients become statistically significant in 1927 (Figure 6), implying an increase in patent applications around 1924.[20] The full effects of licensing, however, begin in 1931, implying an increase in applications in 1929, six to nine years after most licenses had been granted. Effects remain strong and significant throughout the 1930s. After 1932, treated subclasses produced from 0.246 to 0.595 additional patents per year, implying an increase above 40 percent.

Regressions that control for the number and the age of licenses confirm that the full effects of licensing materialized in the early 1930s, although effects were statistically significant as early as 1927. In the 1930s, an additional license increased domestic patents by up to 0.242 patents per year (Figure 7). Regressions that control for the novelty of licensed patents confirm that the strongest effects of licensing occurred in the early 1930s, although less precisely estimated treatment effects are observable by 1928 (Figure 8).[21]

C. *Triple Differences and "Treatments" for French Inventors*

Another concern is that unobservable factors, such as the temporary absence of German competitors from US markets, may have encouraged domestic invention independently of the TWEA.[22] As discussed above, the basic difference-in-differences estimator may be inconsistent if such increases differentially favored domestic invention in treated subclasses. Although historical accounts and data on US patents by German inventors yield no evidence of such effects, we estimate triple difference regressions as an additional test. These regressions compare changes in annual patents by US inventors with changes in annual patents by all

[20] For patents in our data, grants occur with a three year lag. See the data section for a detailed description.

[21] We also estimate $\textit{Patents by US inventors}_{c,t} = \alpha_0 + \beta \cdot \textit{TREAT}_c \cdot \textit{postTWEA}_t + \xi \textit{Number of licenses}_c \cdot \textit{YEARpostTWEA}_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$, which confirms the results in Figures 8 and 9.

[22] For example, historical accounts suggest that the absence of German competitors from overseas markets opened the field to integrated producers of dyestuffs from England, the United States, France, Japan, and Switzerland (Aftalion 2001).

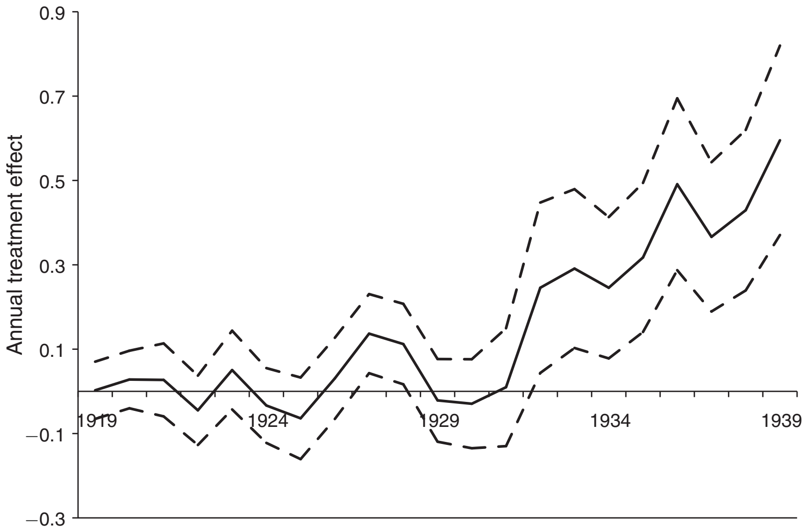

FIGURE 6. ANNUAL TREATMENT EFFECTS: TREATMENT = 1 FOR SUBCLASSES THAT RECEIVED AT LEAST ONE LICENSE UNDER THE TWEA

Notes: For a 95 percent confidence interval of the regression *Patents by U.S. inventors*$_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$, where $TREAT = 1$ if a subclass received at least one license under the TWEA. Data include all 165,400 patents between 1875 and 1939 in 19 USPTO classes that received at least one license. These 21 classes cover 7,248 subclasses, 336 of which are treated.

FIGURE 7. ANNUAL TREATMENT EFFECTS OF AN ADDITIONAL LICENSE

Notes: For a 95 percent confidence interval of the regression *Patents by U.S. inventors*$_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$, where *TREAT* measures the number of licenses in one of 335 treated subclasses. Data include all 128,953 patents between 1875 and 1939 in 21 treated main classes.

Figure 8. Annual Treatment Effects of an Additional Year of Patent Life

Notes: For a 95 percent confidence interval of the regression *Patents by U.S. inventors*$_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$, where *TREAT* measures the total remaining years of patent life for all licensed patents in a treated subclasses. Data include 128,953 patents between 1875 and 1939.

other non-German inventors across treated and untreated subclasses before and after the TWEA:

$$\begin{aligned} Patents_{n,c,t} = {} & \alpha_0 + \alpha_1 USA_n + \alpha_{2t} TREAT_c \cdot YEARpostTWEA_t \\ & + \alpha_3 USA_n \cdot TREAT_c \\ & + \alpha_{4t} USA_n \cdot YEARpostTWEA_t \\ & + \beta_t \cdot USA_n \cdot TREAT_c \cdot YEARpostTWEA_t \\ & + \delta_t + f_c + \varepsilon_{c,t}, \end{aligned}$$

where the subscript *n* distinguishes US and other non-German inventors, and USA distinguishes patents by US inventors. The triple-differences estimator β_t measures the additional effect of compulsory licensing on US inventors relative to other non-German inventors. It consistently estimates the effect of compulsory licensing on US invention if unobservables, such as the absence of German competitors, had the same effect on US and other non-German inventors.

Triple difference estimates confirm that licensing encouraged patenting by U.S inventors, even relative to other non-German inventors. In treated subclasses, domestic inventors produced 0.087 additional patents per year after 1919 compared with other non-German inventors (significant at 5 percent). The timing of these

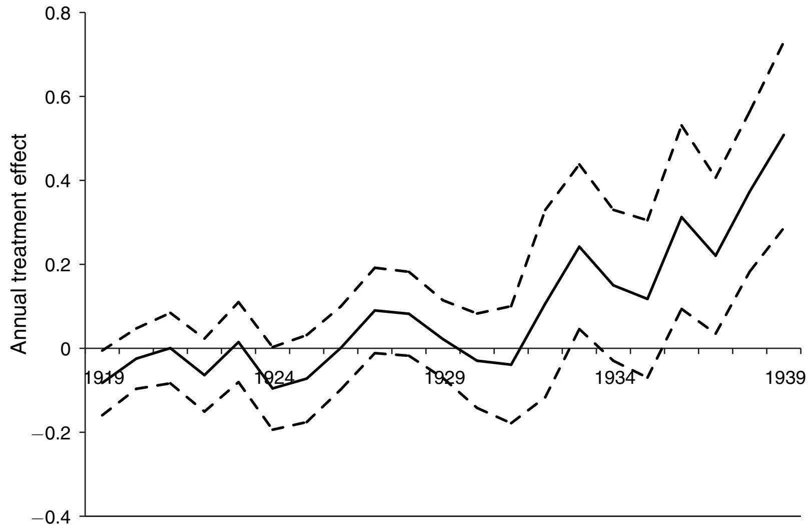

Figure 9. Annual Treatment Effects: Triple Differences Comparing US Inventors with other Non-German Inventors

Notes: For a 95 percent confidence interval of the regression $Patents_{n,c,t} = \alpha_0 + \alpha_3 USA_n + \alpha_4 TREAT_c \cdot YEARpostTWEA_t + \alpha_5 USA_n \cdot TREAT_c + \alpha_6 USA_n \cdot YEARpostTWEA_t + \beta_t \cdot USA_n \cdot TREAT_c \cdot YEARpostTWEA_t + \delta_t + f_c + \varepsilon_{c,t}$, where *TREAT* measures the total remaining years of patent life for all licensed patents in a treated subclasses. Data include 128,593 patents between 1875 and 1939.

effects also closely matches the results from our basic specifications. Beginning in 1933, domestic inventors produced an additional 0.118 to 0.508 patents per year in treated subclasses (Figure 9, significant at 1 percent). The true effects of compulsory licensing may be even larger, because the control includes a large number of British inventors who were affected by their own version of the TWEA.[23] Triple difference regressions that account for number and the age of licensed patents (not reported) further strengthen these results.

An alternative test artificially exposes French inventors, who were also lagging behind in organic chemistry (e.g., Aftalion 2001), to treatment under the US TWEA. Specifically, we reestimate the basic specification with annual treatment effects under the counterfactual that French inventors, who could not take advantage of compulsory licensing provisions, did in fact benefit from them.

$$Patents\ by\ French\ inventors_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}.$$

[23] In September 1914, the House of Commons passed an Act forbidding all transactions "that would improve the financial or commercial position of a person trading or residing in an enemy country" (*House of Commons Debate* 08 *August* 1916 *vol.* 85 *column* 871). In parallel with the TWEA, the British Act was extended in 1919 to allow for compulsory licensing. The amended Act required "the Comptroller to grant a compulsory license under a food or medicine patent to anyone who seemed competent to work the invention" (Davenport 1979, p. 81). We include British inventors in the triple difference control to be conservative.

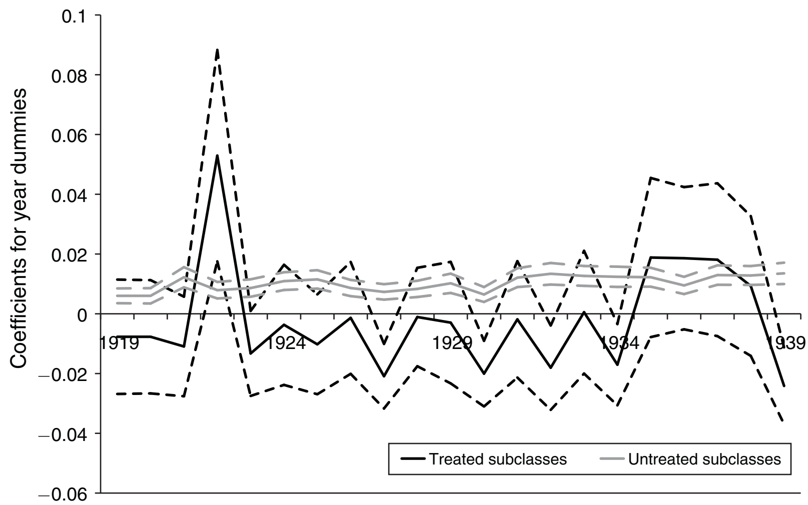

FIGURE 10. ANNUAL TREATMENT EFFECTS: PLACEBO ON FRENCH INVENTORS

Notes: For a 95 percent confidence interval of the regression *Patents by French inventors*$_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$ where $TREAT = 1$ for subclasses where US firms received at least one license under the TWEA. Data include all 3,000 US patents in treated subclasses between 1875 and 1939 that were granted to French inventors.

If unobservables, such as the absence of German competitors during the war, caused US inventors to patent more after 1919, French inventors should experience a similar increase.

Results from this counterfactual regression reveal no measurable changes in annual patents by French inventors for treated subclasses (Figure 10), confirming that the effects of the TWEA were limited to US firms.

D. *Intent to Treat and Instrumental Variable Regressions*

Perhaps the most important threat to our identification strategy is that the licensing decisions of US firms may not have been exogenous, even though the TWEA itself and the technologies that US firms could license were exogenous. In fact, patent data indicate that subclasses where US inventors chose to license were substantially different from other subclasses: US firms were more likely to license in subclasses where initial levels of domestic invention were weak (Figure 5). Under the TWEA, enemy-owned patents became available for licensing in 1,377 subclasses; the pre-TWEA share of domestic invention in these subclasses was 85 percent. US firms chose to license in 336 of these subclasses; the pre-TWEA share of domestic inventions in these (treated) subclasses was 50 percent. Thus, the data suggest that US firms were more likely to license in subclasses where their pre-TWEA inventive capacity was weak. As a result, the effects of compulsory licensing may have been delayed (which is consistent with historical accounts cited above), and OLS may underestimate the true effects of compulsory licensing.

ITT (e.g., Imbens and Wooldridge 2009) regressions allow us to identify the direction of this selection bias. We define ITT as the number of enemy patents that were available for licensing under the TWEA.[24]

$$\textit{Patents by US inventors}_{c,t} = \alpha_0 + \beta \cdot \textit{Enemy patents}_c \cdot \textit{postTWEA}_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}.$$

Results from this regression confirm the findings of OLS: Each additional enemy patent that was available for licensing increased the number of domestic patents per year by 0.055 (Table 3, column 1, significant at 1 percent), implying a 9 percent increase for each additional patent. Similarly, each additional year of patent life increased the number of domestic patents by 0.007 (Table 3, column 3, significant at 1 percent) implying a 1.1 percent increase for each additional year of patent life. If all enemy patents had the same probability of being licensed, ITT estimates would be equal to OLS estimates multiplied by the probability that a subclass with a confiscated patent is treated, which is about one-fourth.[25] The fact that ITT estimates are only slightly smaller than OLS (0.072 for an additional license and 0.006 for an additional year of patent life) suggests that selection bias may lead OLS to under- rather than overestimate the true effects of licensing.

An alternative test uses the number of enemy patents as an instrument for licensed patents. *Enemy patents* is highly correlated with the *number of licenses* that were granted to US firms, but variation in *enemy patents* (other than those that were licensed) should not by itself increase domestic invention.

$$\text{First stage: } \textit{Number of licenses}_{c,t} = \eta_0 + \phi \cdot \textit{Enemy patents}_c \cdot \textit{postTWEA}_t + \mu_t + g_c + \omega_{ct};$$

$$\text{Second stage: } \textit{Patents by US inventors}_{c,t} = \alpha_0 + \beta \cdot \textit{Number of licenses}_c \cdot \textit{postTWEA}_t + \delta_t + f_c + \varepsilon_{c,t}.$$

IV regressions confirm that OLS estimates are downward biased. An additional license adds 0.306 domestic patents per year, while an additional year of patent life

[24] Specifically, we construct a list of all 4,767 enemy-owned patents that the Chemical Foundation had made available for licensing by 1922 (Alien Property Custodian 1922). The alternative, binary, definition of ITT as a subclass that included at least one enemy patent would assign nearly 50 percent of subclasses to the ITT. In the IV regressions, this binary treatment variable would consistently estimate the sign of the average per-unit treatment effect but overestimate the size of the effect if treatment is continuous (Angrist and Imbens 1995; Angrist, Imbens, and Rubin 1990).

[25] For binary treatment variables, ITT = TOT $\cdot$ P(treatment), where TOT represents unbiased estimates of treatment on the treated (Angrist and Imbens 1995; Wooldridge 2002). Here P(treatment) equals $336/1{,}377 = 0.244$ (subclasses where US firms licensed enemy patents/subclasses where enemy patents were available for licensing), implying that unbiased TOT estimates would be $0.227/(336/1{,}377) = 0.930$. Because estimating binary treatment variables may yield inflated IV estimates if the "real" treatment is continuous (Angrist, Imbens, and Rubin 1996), we perform IV and ITT with continuous treatment variables.

Table 3—Intent To Treat Regressions. Dependent Variable Is Patents By US Inventors Per USPTO Subclass and Year

	(1)	(2)	(3)	(4)
Number of enemy patents	0.055***	0.070***		
	(0.007)	(0.008)		
Remaining lifetime of enemy patents			0.007***	0.008***
			(0.001)	(0.001)
Number of patents by foreign inventors	0.279***		0.278***	
	(0.017)		(0.017)	
Subclass fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	471,120	471,120	471,120	471,120
Number of subclasses	7,248	7,248	7,248	7,248

Notes: Data from www.uspto.gov and the *LexisNexis Chronological Patent Files* (*1790–1970*) consist of all 128,953 patents between 1875 and 1939 in 19 USPTO main classes that contained at least one licensed enemy dyestuff patent. These 19 main classes are subdivided into 7,248 subclasses. Robust standard errors clustered at the subclass level in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

adds 0.024 domestic patents (Table 4, columns 3–4, significant at 1 percent).[26] This is consistent with data on the weak presence of US inventors in treated subclasses (Figure 5), which suggests that US firms were more likely to license German patents in technologies where US invention was weak.

IV. Robustness Checks

This section presents a series of robustness checks, including controls for preexisting time trends in patenting, interactions between broader technology classes and time dummies, and changes in the USPTO classification system.

A. *Controlling for Preexisting Time Trends*

One potential problem with difference-in-differences is that it may confound the dynamic effects of compulsory licensing with preexisting differences in time trends across treated and untreated subclasses. In other words, subclasses that were affected by compulsory licensing may have experienced an increase in domestic patenting after the TWEA due to differences in time trends that *preceded* the TWEA. Although a comparison of pretrends does not yield any evidence for significant differences (Figure 4), we include an additional test, which extends

[26] For a binary ITT variable that is uncorrelated with the error term in the second stage of the IV regression, the IV coefficient consistently estimates TOT as TOT $=$ ITT$/P$(treatment). In our data, this implies TOT $= 0.070/0.228 = 0.307 =$ IV. A Hausman specification test rejects consistency for OLS estimates at the 1 percent level under the assumption that IV estimates are consistent.

Table 4—Instrumental Variable Regressions. Dependent Variable Is Patents by US Inventors per USPTO Subclass and Year

	First Stage		Second Stage	
	(1)	(2)	(3)	(4)
Number of enemy patents	0.228***			
	(0.003)			
Remaining lifetime of enemy patents		0.354***		
		(0.004)		
Number of licenses			0.306***	
			(0.009)	
Remaining lifetime of licensed patents				0.024***
				(0.001)
Subclass fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	471,120	471,120	471,120	471,120
Number of subclasses	7,248	7,248	7,248	7,248

Notes: Data from www.uspto.gov and the *LexisNexis Chronological Patent Files* (*1790–1970*) consist of all 128,953 patents between 1875 and 1939 in 19 USPTO main classes that contained at least one licensed enemy dyestuff patent. These 19 main classes are subdivided into 7,248 subclasses. Robust standard errors clustered at the subclass level in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

our regressions to include a linear time trend for all treated subclasses for the pre-TWEA period:

$$\begin{aligned} \textit{Patents by US inventors}_{c,t} = {} & \alpha_0 + \beta_t \cdot \textit{TREAT}_c \cdot \textit{YEARpostTWEA}_t \\ & + \gamma \cdot Z_{c,t} + \delta_t + f_c \\ & + \phi \cdot \textit{TREAT}_c \cdot t + \varepsilon_{c,t,} \end{aligned}$$

where β_t measures treatment effects in year t and δ_t captures year fixed effect controlling for a preexisting time trend $\phi \cdot \textit{TREAT}_c \cdot t$. Results of this regression confirm that patenting by domestic inventors increased significantly more for treated than for untreated subclasses after the TWEA, even controlling for preexisting time trends (Figure 11).[27]

An alternative test controls for *subclass*-specific linear and quadratic time trends:

$$\begin{aligned} \textit{Patents by US inventors}_{c,t} = {} & \alpha_0 + \beta_t \cdot \textit{TREAT}_c \cdot \textit{YEARpostTWEA}_t \\ & + \gamma Z_{c,t} + \delta_t + f_c \\ & + \phi_{1c} \cdot t + \phi_{2c} \cdot t^2 + \varepsilon_{c,t}. \end{aligned}$$

[27] Regressions with quadratic time trends yield larger standard errors but nearly identical coefficients β_t.

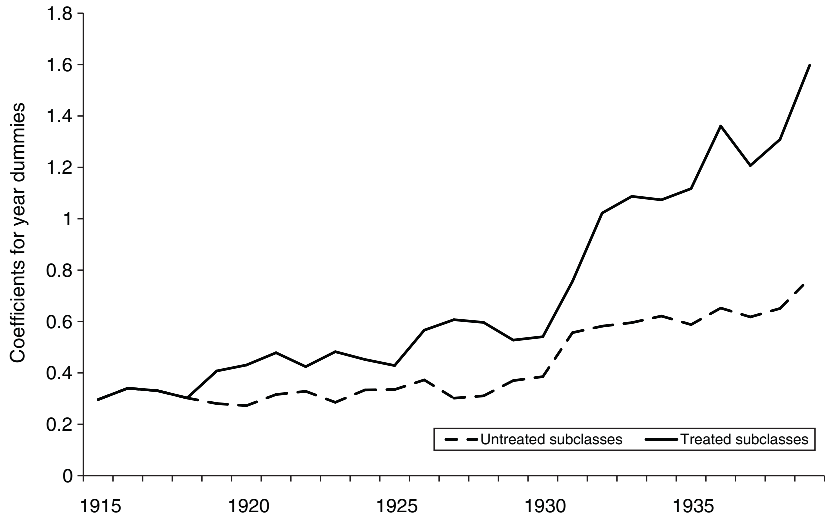

Figure 11. Annual Treatment Effects, Controlling for Linear Time Trends

Notes: The regression equation is *Patents by US inventors*$_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varphi \cdot TREAT_c \cdot t + \varepsilon_{c,t}$ where $TREAT = 1$ for subclasses where US firms received at least one license under the TWEA. The y-axis plots coefficients for the year-specific treatment β_t, and the year fixed effects δ_t where a subclass is defined as treated if it received at least one license under the TWEA. Line for untreated subclasses represents δ_t, line for treated subclasses represents $\beta_t, + \delta_t$.

In these regressions (not reported) treatment effects are also positive and statistically significant, further strengthening the results.[28]

B. *Interactions between Main Classes and Year Fixed Effects*

As an alternative way to account for the potential of differential growth paths across treated and untreated subclasses, we include interaction terms between year dummies and each of the broader 19 USPTO main classes.

$$\text{Patents by US inventors}_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot postTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \lambda_{mt} Year_t \cdot Class_c + \varepsilon_{c,t},$$

where λ_{mt} represents a fixed effect for USPTO class m and year t. Results from this exercise indicate that our estimates are robust to controlling for class-specific time trends (Table 5).[29]

[28] Running this test on the entire sample would require estimating 7,248 subclass fixed effects, 7,248 linear time trends, and 7,248 quadratic time trends in addition to treatment variables and controls. To limit the number of parameters, we run the regression separately for each of the 19 (main) classes. Results are comparable or larger than results in the entire sample for 15 of 19 classes.

[29] We also estimate regressions separately for all 19 main classes; class-specific regressions confirm that domestic patenting increased in treated subclasses after the TWEA. In two of four classes with more than 20 licenses treatment effects were strongest in the late 1920s (8:bleaching and dyeing and 552:azides); in the two other classes with more than 20 licenses treatment effects were strongest in the early 1930s (534:organic compounds containing a noble gas and 548:organic compounds containing 5-membered hetero rings).

Table 5—OLS with Interactions between USPTO Main Classes and Years. Dependent Variable is Patents by US Inventors per USPTO Subclass and Year

	(1)	(2)	(3)
Subclass has at least one license	0.263***		
	(0.033)		
Number of licenses		0.113***	
		(0.019)	
Remaining lifetime of licensed patents			0.009***
			(0.001)
Number of patents by foreign inventors	0.285***	0.281***	0.281***
	(0.017)	(0.018)	(0.018)
Subclass fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Main class × year fixed effects	Yes	Yes	Yes
Observations	471,120	471,120	471,120
Number of subclasses	7,248	7,248	7,248

Notes: Data from www.uspto.gov and the *LexisNexis Chronological Patent Files* (*1790–1970*) consist of all 128,953 patents between 1875 and 1939 in 19 USPTO main classes that contained at least one licensed enemy dyestuff patent. These 19 main classes are subdivided into 7,248 subclasses. Robust standard errors clustered at the subclass level in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

C. *Dropping Newly Created Subclasses and Secondary Subclasses*

Two additional tests address potential problems with the USPTO classification system. Most importantly, we account for the fact that the USPTO periodically adds new subclasses to accommodate new areas of invention. In our data, 2,664 new subclasses were added after 1919. Because domestic inventors could not patent in these subclasses prior to 1919, patenting increases mechanically in new subclasses after 1919, which may lead us to underestimate the true effects of licensing. To address this problem we restrict the sample to include only subclass year pairs for subclasses c that produced at least one patent in a year before t; this excludes subclasses that do not yet exist in year t.

Regressions with a restricted sample of preexisting subclasses indicate that including newly created subclasses does not affect the estimates. In subclasses that received at least one license under the TWEA, domestic inventors produced 0.142 additional patents per year (Table 6, column 2 significant at 1 percent). Compared with a mean of 0.884 patents per subclass and year in the restricted sample, this implies a 16 percent increase in domestic invention. Similarly, each additional license increases domestic patents by 0.060 per year (Table 6, column 5, significant at 1 percent), and each additional year of patent life increased domestic patents by 0.006 per year (Table 6, column 8, significant at 1 percent).

Another potential concern is that the USPTO assigns patents to several secondary subclasses (in addition to primary subclasses) to cross-reference related technologies. Our analysis includes secondary subclasses because they are affected by compulsory licensing. Their inclusion may, however, give too much weight to patents

Table 6—OLS, Restricting the Sample to Subclasses that Existed Prior to the TWEA. Dependent Variable is Patents by US Inventors per Subclass and Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subclass has at least one license	0.142***	0.213***						
	(0.044)	(0.048)						
Number of licenses			0.120***	0.060***	0.086***			
			(0.030)	(0.022)	(0.029)			
Number of licenses squared			−0.010***					
			(0.003)					
Remaining lifetime of licensed patents						0.010***	0.006***	0.008***
						(0.003)	(0.002)	(0.002)
Remaining lifetime of licensed patents squared (× 100)						−6.38e-05*		
						(3.37e-05)		
Number of patents by foreign inventors ($t-2$)								
Number of patents by foreign inventors	0.284***		0.283***	0.284***		0.283***	0.284***	
	(0.020)		(0.020)	(0.020)		(0.020)	(0.020)	
Subclass fixed effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	221,673	221,673	221,673	221,673	221,673	221,673	221,673	221,673
Number of subclasses	4,584	4,584	4,584	4,584	4,584	4,584	4,584	4,584

Notes: Data from www.uspto.gov and the *LexisNexis Chronological Patent Files* (*1790–1970*). Our data consist of all 128,953 patents between 1875 and 1939 in 19 USPTO main classes that contained at least one licensed enemy dyestuff patent. These 19 main classes are subdivided into 7,248 subclasses. Subclasses created after 1919 have been dropped and subclasses not yet created have been given a missing value in the years that preceded their creation. Regressions that include a two-year lag drop the first two years of data. Robust standard errors clustered at the subclass level in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

that were assigned to many subclasses. For example, 25 percent of patents in our data were assigned to at least four secondary subclasses. To address this issue, we restrict the sample to the 5,656 primary subclasses in the data.

Regressions for the restricted sample confirm the results from the full sample. In primary subclasses that received at least one license under the TWEA, domestic inventors produced 0.024 additional patents per year after 1919 (Table 7, column 1). This implies an 8 percent increase in patenting compared with an average of 0.309 of patents per year and primary subclass after 1919. Each additional license increased domestic patents by 0.025 per year, and each additional year of patent life increased domestic patents by 0.002 patents (Table 7, columns 2–3, significant at 1 percent).

D. *Effects within Indigo*

An additional test examines whether a shock to the demand for domestically produced dyes as a result of World War I can explain the observed increase in domestic invention without compulsory licensing. By cutting off German suppliers, World War I created an acute “dye famine” in the United States from 1914 to 1921, when German firms reentered the US market (Genesove 2006).

Table 7—OLS, Restricting the Sample to Primary Subclasses. Dependent Variable is Patents by US Inventors per Subclass and Year

	(1)	(2)	(3)
Subclass has at least one license	0.024		
	(0.017)		
Number of licenses		0.025***	
		(0.009)	
Remaining lifetime of licensed patents			0.002***
			(0.001)
Number of patents by foreign inventors	0.165***	0.165***	0.165***
	(0.013)	(0.013)	(0.013)
Subclass fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Main class × year fixed effects	No	No	No
Observations	367,640	367,640	367,640
Number of subclasses	5,656	5,656	5,656

Notes: Data include all 5,656 primary subclasses in the 19 main classes treated by the TWEA. Primary subclasses in this sample include an average of 0.183 patents per year. Robust standard errors clustered at the subclass level in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

We examine changes in domestic invention for indigo, which was disproportionately affected by changes in demand. In 1914, 90 percent of the US demand for indigo was imported from Germany. In 1915, Britain's naval blockade cut US markets off from German imports so effectively that the last shipment of German dyes arrived in March 1915 (Haber 1971). At the same time, the United States' entry into the war increased demand for domestically produced indigo to create the blue shade of Navy uniforms (Navy Department 1917).[30] Congress established a five-year tariff barrier in September 1916 (Aftalion 2001, pp. 123–124).[31] As a result, the price of indigo rose from 20 cents per pound in 1914 to nearly 70 cents in 1917. While prices for other dyes recovered quickly to their prewar levels, indigo remained expensive at 40 cents in 1919, double its prewar level (online Appendix Figures A2 and A3, Haynes 1945).[32]

Regressions within indigo patents confirm that compulsory licensing encouraged domestic invention. Each additional license is associated with an increase of 0.027 patents by domestic inventors per subclass and year (Table 8, column 2, significant

[30] The Navy's personnel increased from 60,376 in 1916 to 194,617 in 1917 and 530,338 in 1918. By 1919, the Navy's personnel strength fell back to 272,144, and 121,845 in 1920; it declined to 94,094 in 1923 and remained around 90,000 for the 1920s and early 1930s (Bureau of Naval Personnel Annual Report 1960).

[31] Tariff protection continued throughout the 1920s and 1930s. In 1922 the Fordney McCumber Act imposed ad valorem tariffs of nearly 30 percent on chemical imports; it covered indigo, alizarin and vat dyes. In 1930, the Smoot-Hawley Act raised tariff rates to 36 percent (United States Tariff Commission 1930).

[32] Indigo was also subject to a technology sharing agreement, which may have transferred knowledge of German production processes to US firms. In November 1916, the British chemical firm Herbert Levinstein agreed to share with Du Pont its secrets of producing synthetic indigo dyes, which included knowledge that Levinstein had acquired when it purchased a confiscated British plant of the German company Hoechst (Hounshell and Smith 1988). Historical records, however, suggest that Du Pont wrestled with the problem of producing indigo for several years and succeeded "only after long experimentation" (Haynes 1945, p. 245). In addition to the within-indigo test we also restrict the sample to non-Du Pont firms, which leaves results qualitatively unchanged.

Table 8—OLS, Restricting the Sample to Indigo Patents. Dependent Variable is Patents by US Inventors per Subclass and Year

	(1)	(2)	(3)
Subclass has at least one license	0.044***		
	(0.015)		
Number of licenses		0.027***	
		(0.010)	
Remaining lifetime of licensed patents			0.002***
			(0.001)
Number of patents by foreign inventors	0.004	0.004	0.004
	(0.003)	(0.003)	(0.003)
Subclass fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	46,670	46,670	46,670
Number of subclasses	718	718	718

Notes: Data consist of all 843 patents in our data that contain the word "indigo." In the indigo sample, the average number of patents per subclass and year is 0.038. Robust standard errors clustered at the subclass level in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

at 1 percent). Compared with an average of 0.04 indigo patents per subclass and year in this sample, this implies a 68 percent increase in domestic patenting within indigo, which is larger than the effects in the overall sample. Under the assumption that all technologies within indigo were affected by the same demand shock, this differential increase for treated subclasses measures the effects of compulsory licensing in the presence of a strong positive shock to the demand for domestically produced inventions.

Regressions that control for the number of licenses confirm these results (Table 8, columns 1–3), suggesting that demand effects may have reinforced the effects of compulsory licensing. The timing of effects closely mirrors the effects in the overall sample. Annual treatment effects become stable and statistically significant in 1931, though there are some statistically significant effects as early as 1928 (Figure 12).

V. Firm-Level Analysis

As a final test, we analyze firm-level data for Du Pont de Nemours and Co. to shed some light on the mechanisms by which compulsory licensing encouraged domestic invention.[33] Specifically, we compare the effects of Du Pont's own licenses with the effects of licenses that were issued to other US firms. Licenses that were issued to Du Pont created learning opportunities for Du Pont, while licenses to other firms

[33] The data for this firm-level analysis consist of all 234 licenses and 1,618 chemical patents that were granted to Du Pont between 1875 and 1939. We identify these patents by searching LexisNexis for all known variants of the company's name, including E. I. Du Pont de Nemours and Co., Du Pont Ammonia Corp., Du Pont Cellophane Co, Du Pont Everdur Co, Du Pont Fibersilk Co, Du Pont Film and Picture Co, and Du Pont Rayon Co. This search yields a total of 3,571 patents in 241 classes and 5,716 subclasses; 1,618 of these patents are in one of the 21 classes that were affected by the TWEA.

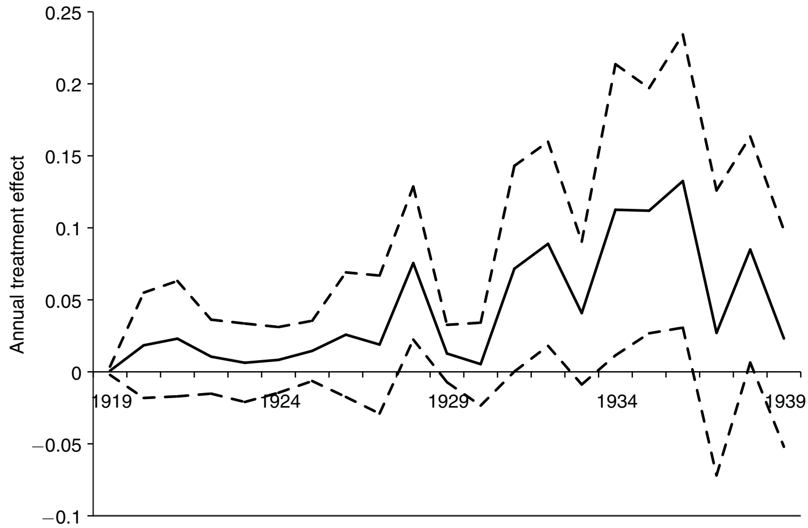

Figure 12. Annual Treatment Effects: Indigo Patents

Notes: For a 95 percent confidence interval of the regression *Indigo patents by U.S. inventors*$_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$, where $TREAT = 1$ if a subclass received at least one license under the TWEA. Data include all 843 patents between 1875 and 1939 in 19 USPTO classes that received at least one license. These 21 classes cover 718 subclasses, 127 of which are treated. The average number of indigo patents in each subclass-cell is 0.035.

benefitted the US industry more broadly, for example, by strengthening incentives to invest in skills and education.

$$\begin{aligned} \textit{Du Pont Patents}_{c,t} = {} & \alpha_0 + \beta_1 \cdot TREATDuPont_c \cdot postTWEA_t \\ & + \beta_2 \cdot TREATotherFirms_c \cdot postTWEA_t + \gamma \\ & \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}. \end{aligned}$$

It is important to keep in mind, however, that this test is descriptive (rather than measuring a causal effect) because Du Pont may have strategically chosen to license German technologies to complement or substitute for its own R&D. Specifically, Du Pont may have chosen to license German technologies that matched its own research, so that progress after compulsory licensing may have been faster even without licensing. On the other hand, Du Pont may have been more likely to license foreign technologies in areas where its own research was comparatively weak (Haynes 1945; Hounshell and Smith 1988).[34]

[34] In these areas, Du Pont's "problems stemmed from the company's trying to do in months what had taken the German six or seven decades to achieve," including the development of a corps of expert dye chemists and technical personnel who had accumulated craft knowledge of dye synthesis and a whole gamut of tricks inherent to dye manufacture (Hounshell and Smith 1988, p. 83). Similar to the case of the Winthrop company cited above, Du Pont was unable to produce German chemicals despite entering an agreement with Levinstein, who had access to a German plant in Britain: "Even with such extensive though incomplete information, questions remained about how to proceed, which intermediates and dyes to produce first, how to organize for dyestuffs research, and how to put together such technical parts of the business as testing and marketing support" (Hounshell and Smith 1988, p. 84).

Table 9—OLS, Regressions at the Firm-Level, Dependent Variable is Patents by Du Pont per Subclass and Year

	(1)	(2)	(3)	(4)	(5)	(6)
Subclass has at least one license to Du Pont	0.094***	0.098***				
	(0.014)	(0.012)				
Subclass has at least one license to other firms	0.021	0.025***				
	(0.016)	(0.010)				
Licenses to Du Pont			0.051***	0.059***		
			(0.009)	(0.008)		
Licenses to other US firms			0.014*	0.009*		
			(0.008)	(0.005)		
Remaining lifetime of Du Pont licenses					0.004***	0.004***
					(0.001)	(0.001)
Remaining lifetime of other licenses					0.001*	0.001*
					(0.001)	(0.001)
Patents by foreign inventors	0.030***		0.030***		0.029***	
	(0.005)		(0.005)		(0.004)	
Subclass fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	72,694	222,924	72,694	222,924	72,694	222,924
Number of subclasses	1,913	5,716	1,913	5,716	1,913	5,716

Notes: The data consist of all 3,571 US patent grants between 1875 and 1939 that include the word "Du Pont" or variations of the company's name. These patents cover a total of 5,716 subclasses; 1,618 of the 3,571 Du Pont patents belong to one of 19 treated USPTO main classes. Data on patents by foreign inventors are available for 1,913 subclasses. Robust standard errors clustered at the subclass level in parentheses.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Firm-level regressions indicate that both own and other firms' licenses encouraged patenting, though the coefficients for own licenses are substantially larger. In subclasses where DuPont received a license under the TWEA, the company's annual patents increased by 0.094 to 0.098 patents after 1919 (Table 9, columns 1–2, significant at 1 percent). In subclasses where other US firm received a license, Du Pont's annual patents increased by 0.021 to 0.025 patents roughly one third this effect (Table 9, columns 1–2).

These results match up closely with empirical estimates on learning-by-doing and knowledge spillovers in the late twentieth century, which indicate that within-firm learning effects are more than three times as large as effects of knowledge spillovers across firms (Irwin and Klenow 1994).

Controlling for the number and age of patents strengthens these results. An additional license granted to Du Pont increased Du Pont's patents per year by 0.051, compared to an effect of 0.014 for other firm's licenses (Table 9, column 3). Again, both effects are significant, but the effects of a firm's own licenses are about four times larger. Regressions that control for the novelty of patents further strengthen these results (Table 9, columns 5–6). For all regressions, Wald tests reject the hypothesis that treatment effects of own and other licenses are equal at 0.01 percent significance.[35]

[35] Estimates of annual treatment effects indicate that the most significant change in patent grants occurred around 1933, although some effects occur as early as 1927 (Figure 13, significant at 5 percent). In terms of patent applications, this implies that the full effects of licensing set in 3 to 9 years after most licenses had been granted.

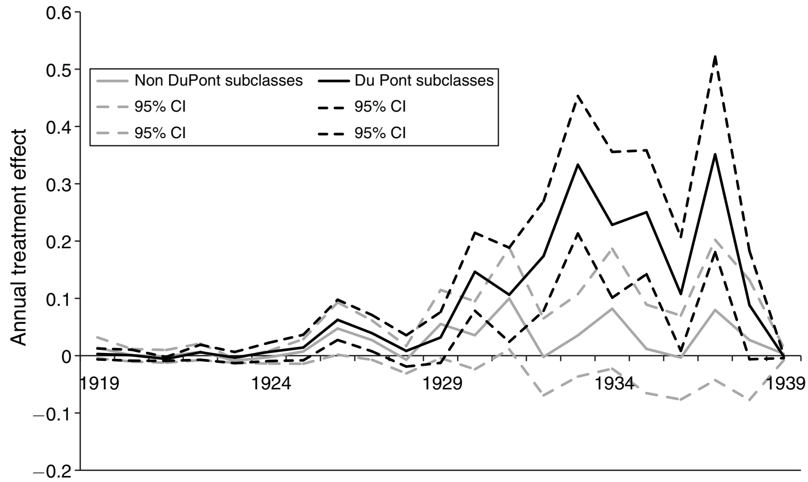

Figure 13. Year-Specific Treatment Effects: Du Pont

Notes: For a 95-percent confidence interval of the regression *Patents by U.S. inventors*$_{c,t} = \alpha_0 + \beta_t \cdot TREAT_c \cdot YEARpostTWEA_t + \gamma \cdot Z_{c,t} + \delta_t + f_c + \varepsilon_{c,t}$, where $TREAT = 1$ if Du Pont received at least one license in this subclass. Data include 3,571 US patents between 1875 and 1939 that include variation of the company name. These patents cover 5,716 subclasses, 402 of which are treated.

VI. Conclusions

This paper has used the TWEA as a natural experiment to examine whether compulsory licensing encourages invention by nationals in nascent industries. Data on chemical patents by US inventors after the TWEA indicate that compulsory licensing has a strong and persistent positive effect on domestic invention. In USPTO subclasses, where at least one enemy-owned patent was licensed to a domestic firm under the TWEA, domestic patenting increased by about 20 percent after the TWEA (compared with subclasses that were not affected). These results are robust to controlling for the number of licenses that were granted and by accounting for the novelty of licensed patents. Results are also robust to a variety of alternative tests, including triple differences (comparing changes in the number of patents by US inventors before and after the TWEA with changes in the number of patents by other, non-German inventors), controls for subclass- and treatment-specific time trends, and placebo tests for other non-German inventors.[36] ITT and instrumental variable regressions further suggest that the analysis may under-, rather than overestimate the true effects of licensing.

The historical nature of the data also allows us to examine the timing of such effects. Estimates of annual treatment effects indicate that the strongest effects of

[36] Even without any effects on innovation, compulsory licensing may create significant positive welfare effects on consumers in developing countries as a mechanism to maintain product variety. For example, welfare losses of extending patent protection to pharmaceuticals on Indian consumer have been shown to be substantially smaller under policies, such as compulsory licensing, that maintain product variety (Chaudhuri, Goldberg, and Jia 2006). As a mechanism to address anticompetitive patenting behavior in domestic markets, compulsory licensing is expected to increase overall welfare by encouraging the optimal trade-off between incentives for R&D and the deadweight loss of long-lived patents (Tandon 1982; Gilbert and Shapiro 1990).

licensing set in around 1929 (measured in terms of patent applications) and persisted throughout the 1930s. Compulsory licenses gave US firms the right to produce German inventions, but even with access to confiscated patents and in some cases physical capital, it took several years for US firms to acquire the knowledge and skills necessary to produce these inventions domestically. Our data indicate that US invention took off after this prolonged period of learning. These findings are mirrored in changing patterns of scientific citations (e.g., Thackeray et al. 1985), which indicate that the US chemical industry gained prominence as an originator of knowledge in the 1930s.[37]

While our analysis suggests that compulsory licensing encourages domestic invention in the licensing country, the policy's long-run effects include potentially important incentive effects on invention in the country whose inventions are licensed. Ex ante these effects are unclear because, for example, increased competition may either encourage or discourage innovation. In the case of the TWEA, the quick reentry of German patentees suggest that negative incentive effects may be limited if compulsory licensing is a one-shot response to an emergency situation. Systematic analyses with additional data, however, are required to evaluate these effects. The response of US pharmaceuticals to compulsory licensing provisions in India and more recently under TRIPS offers a promising contemporary setting.

Finally, the difficult learning process that US firm experienced after the TWEA suggest that human capital and tacit knowledge are essential in facilitating rapid technology transfers across countries. World War II provides an opportunity to measure these effects: On April 7, 1933, Adolf Hitler's "Law for the Restoration of the Professional Civil Service" led to the dismissal of 1,100 scientists from German universities (Hartshorne 1937). Many of these scientists moved to the United States in the mid-1930s, several years after compulsory licensing had helped to jump-start the organic chemical industry. Their contributions to US invention deserve further study.[38]

REFERENCES

Aftalion, Fred. 2001. *A History of the International Chemical Industry*. Translated by Otto Benfey. Philadelpia: University of Pennsylvania Press.

Alien Property Custodian. 1922. *A Detailed Report by the Alien Property Custodian of All Proceedings Had by Him under the Trading with the Enemy Act during the Calendar Year 1918 and to the Close of Business on February 15, 1919.* Washington, DC: Government Printing Office.

Angrist, Joshua D., and Guido W. Imbens. 1995. "Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity." *Journal of the American Statistical Association* 90(430): 431–42.

Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association* 91(434): 444–55.

Arora, Ashish, and Nathan Rosenberg. 1998. "Chemicals: A U.S. Success Story." In *Chemicals and Long-Term Economic Growth: Insights from the Chemical Industry*, edited by A. Arora, R. Landau, and N. Rosenberg, 71–102. Hoboken, NJ: Wiley.

[37] Based on citations in the top seven US journals and the German journal *Chemische Berichte*.

[38] Between 1933 and 1939, 129 scientists were dismissed from German universities (Deichman 2001). We have been able to collect emigration and employment histories for 62 of them from Strauss (1983); 32 eventually arrived in the United States, 5 as early as 1933, and another 16 throughout the 1930s.

Arrow, K. J. 1962. "The Economic Implications of Learning by Doing." *Review of Economic Studies* 29: 155–73.

Baten, Joerg, and Rainer Schulz. 2005. "Making Profits in Wartime: Corporate Profits, Inequality, and GDP in Germany during the First World War." *Economic History Review* 58(1): 34–56.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119(1): 249–75.

Bureau of Naval Personnel Annual Report. 1960. *Navy and Marine Corps Military Personnel Statistics.* Washington, DC: Government Printing Office.

Chaudhuri, Shubham, Pinelopi K. Goldberg, and Panle Jia. 2006. "Estimating the Effects of Global Patent Protection in Pharmaceuticals: A Case Study of Quinolones in India." *American Economic Review* 96(5): 1477–1514.

Chien, Colleen. 2003. "Cheap Drugs at What Price to Innovation: Does the Compulsory Licensing of Pharmaceuticals Hurt Innovation?" *Berkeley Technology Law Journal* 18(3): 859–907.

Davenport, A. Neil. 1979. *The United Kingdom Patent System: A Brief History with Bibliography*. Homewell, UK: Kenneth Mason Publications.

Deichmann, Ute. 2001. *Flüchten, Mitmachen, Vergessen. Chemiker und Biochemiker im Nationalsozialismus*. Weinheim, Germany: Wiley.

Delamare, François and Bernard Guineau. 2000. *Colors – The Story of Dyes and Pigments.* New York: Harry N. Adams, Inc.

Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review* 91(4): 795–813.

Eichengreen, Barry. 1989." The Political Economy of the Smoot-Hawley Tariff." In *Research in Economic History*, vol. 12, edited by R. L. Ransom, 1–43. Greenwich, CT: JAI Press, Emerald Group Publishing Ltd.

Field, Alexander J. 2003. "The Most Technologically Progressive Decade of the Century." *American Economic Review* 93(4): 1399–1413.

Galvão, J. 2002. "Access to Antiretroviral Drugs in Brazil." *The Lancet* 360(9348): 1862–65.

Genesove, David. 2006. "The Dye Famine and its Aftermath: Knowledge Diffusion and Entry." Centre for Economic Policy Research Discussion Paper 5890.

Gilbert, Richard, and Carl Shapiro. 1990. "Optimal Patent Length and Breadth." *RAND Journal of Economics* 21(1): 106–12.

Gostin, Lawrence O. 2006. "Medical Countermeasures for Pandemic Influenza: Ethics and the Law." *Journal of the American Medical Association* 295(5): 554–6.

Haber, Ludwig Fritz. 1971. *The Chemical Industry, 1900–1930: International Growth and Technological Change*. Oxford: Clarendon Press.

Hall, Bronwyn H., Adam Jaffe, and Manuel Trajtenberg. 2001. "The NBER Patent Citation Data File: Lessons, Insights and Methodological Tools." National Bureau of Economic Research Working Paper 8498.

Hall, Bronwyn H., Adam Jaffe, and Manuel Trajtenberg. 2005. "Market Value and Patent Citations." *RAND Journal of Economics* 36(1): 16–38.

Hartshorne, Edward Yarnall. 1937. *The German Universities and National Socialism*. Cambridge: Harvard University Press

Haynes, Williams. 1945. *American Chemical Industry – The World War I Period 1912–1922*. New York: D. Van Nostrand Company.

Hounshell, David A., and John Kenly Smith, Jr. 1988. *Science and Corporate Strategy: Du Pont R&D, 1902–1980*. Cambridge, UK: Cambridge University Press.

Imbens, Guido W., and Jeffrey M. Wooldridge. 2009. "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature* 47(1): 5–86.

Irwin, Douglas A. 1998. "The Smoot-Hawley Tariff: A Quantitative Assessment." *Review of Economics and Statistics* 80(2): 326–34.

Irwin, Douglas A., and Peter J. Klenow. 1994. "Learning-by-Doing Spillovers in the Semiconductor Industry." *Journal of Political Economy* 102(6): 1200–27.

Kremer, Michael. 2002. "Pharmaceuticals and the Developing World." *Journal of Economic Perspectives* 16(4): 67–90.

Landau, Ralph, and Nathan Rosenberg. 1992. " Successful Commercialization in the Chemical Process Industries." In *Technology and the Wealth of Nations*, edited by N. Rosenberg, R. Landau, and D. C. Mowery, 73–119. Palo Alto, CA: Stanford University Press.

Lerner, Josh. 1995. "Patenting in the Shadow of Competitors." *Journal of Law and Economics* 38(2): 463–95.

Lexis Nexis. 2009. "Chronological Patent Files—Full Text, Older (1790–1970)." http://www.lexisnexis.com/hottopics/lnacademic/?verb=sf&sfi=AC00NBGenSrch (accessed December 4, 2011).

Mann, Charles C. and Mark L. Plummer. 1991. *The Aspirin Wars: Money, Medicine, and 100 Years of Rampant Competition*. Cambridge MA: Harvard Business School Press.

Moser, Petra. 2009. "Innovation Without Patents: Evidence from the World Fairs" http://ssrn.com/abstract=930241.

Moser, Petra, and Alessandra Maria Voena. 2012. "Compulsory Licensing: Evidence From the Trading with the Enemy Act: Dataset." *American Economic Review.* http://dx.doi.org/10.1257/aer.102.1.396.

Mowery, David C., and Nathan Rosenberg. 1998. *Paths of Innovation: Technological Change in Twentieth-Century America*. Cambridge, UK: Cambridge University Press.

Navy Department. 1917. *Uniform Regulations, United States Navy, together with Regulations together with Regulations Common to both Navy and Marine Corps*. Washington, DC: Government Printing Office.

Popp, David, Ted Juhl, and Daniel K. N. Johnson. 2004. "Time in Purgatory: Examining the Grant Lag for U.S. Patent Applications." *Topics in Economic Analysis and Policy* 4(1): 1–43.

Rubin, Donald B. 1990. "Formal Modes of Statistical Inference for Causal Effects." *Journal of Statistical Planning and Inference* 25(3): 279–92.

Scherer, Frederic M. 1977. *The Economic Effects of Compulsory Patent Licensing*. New York: Center for the Study of Financial Institutions.

Steen, Kathryn. 2001. "Patents, Patriotism, and 'Skilled in the Art' USA v. The Chemical Foundation, Inc., 1923–1926." *Isis* 92(1): 91–122.

Steinbrook, Robert. 2007. "Thailand and the Compulsory Licensing of Efavirenz." *New England Journal of Medicine* 356(7): 544–6.

Stokey, Nancy L. 1988. "Learning by Doing and the Introduction of New Goods." *Journal of Political Economy* 96(4): 701–17.

Strauss, Herbert A, ed. 1983. *International Biographical Dictionary of Central European Emigrés 1933–1945*. New York: Research Foundation for Jewish Immigration.

Tandon, Pankaj. 1982. "Optimal Patents with Compulsory Licensing." *Journal of Political Economy* 90(3): 470–86.

Thackeray, Arnold, Jeffrey L. Sturchio, P.Thomas Carroll and Robert Bud. 1985. *Chemistry in America, 1876–1976*. Dordrecht, The Netherlands: D. Reidel Publishing Company.

United States Patents and Trademarks Office. 2010. "US Patent Grant Master Classification File." http://www.google.com/googlebooks/uspto-patents-class.html (accessed December 4, 2011).

United States Tariff Commission. 1918–1923. *Census of Dyes and Coal-tar Chemicals*. Tariff Information Series No.11. Washington, DC: Government Printing Office.

United States Tariff Commission. 1930. *The Tariff Review*, *July 1930*. Washington, DC: Government Printing Office.

Williamson, Samuel H. 2008. "Five Ways to Compute the Relative Value of a U.S. Dollar Amount, 1790 to Present." www.measuringworth.com/uscompare.

Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

World Trade Organization. 2001. "Ministerial Declaration, November, 20, 2001." WT/MIN(01)/DEC/1.

This article has been cited by:

1. Ashish Arora, Sharon Belenzon, Andrea Patacconi, Jungkyu Suh. 2020. The Changing Structure of American Innovation: Some Cautionary Remarks for Economic Growth. *Innovation Policy and the Economy* **20**, 39-93. [Crossref]
2. Yunsen Chen, Jianqiao Huang, Sheng Xiao, Ziye Zhao. 2020. The "home bias" of corporate subsidiary locations. *Journal of Corporate Finance* **62**, 101591. [Crossref]
3. Da Gao, Yi Li, Qiuyue Yang. 2020. Can pollution charges reform promote industrial SO 2 emissions reduction?—Evidence from 189 China's cities. *Energy & Environment* **10**, 0958305X2091941. [Crossref]
4. Jing Li, Xing Shi, Huaqing Wu, Liwen Liu. 2020. Trade-off between economic development and environmental governance in China: An analysis based on the effect of river chief system. *China Economic Review* **60**, 101403. [Crossref]
5. Kai Tang, Yuan Qiu, Di Zhou. 2020. Does command-and-control regulation promote green innovation performance? Evidence from China's industrial enterprises. *Science of The Total Environment* **712**, 136362. [Crossref]
6. Haiwei Jiang, Shiyuan Pan, Xiaomeng Ren. 2020. Does Administrative Approval Impede Low-Quality Innovation? Evidence from Chinese Manufacturing Firms. *Sustainability* **12**:5, 1910. [Crossref]
7. Chao Wang, Yue-Jun Zhang. 2020. Does environmental regulation policy help improve green production performance? Evidence from China's industry. *Corporate Social Responsibility and Environmental Management* **27**:2, 937-951. [Crossref]
8. Chen Feng, Beibei Shi, Ming Xu. 2020. The political origin of differences in long-term economic prosperity: centralization versus decentralization. *Cliometrica* **91**. . [Crossref]
9. Yuchen Gao, Zehao Liu, Ruipeng Li, Zhidan Shi. 2020. Long-Term Impact of China's Returning Farmland to Forest Program on Rural Economic Development. *Sustainability* **12**:4, 1492. [Crossref]
10. Fenghua Wen, Nan Wu, Xu Gong. 2020. China's carbon emissions trading and stock returns. *Energy Economics* **86**, 104627. [Crossref]
11. Chuanwang Sun, Yanhong Zhan, Gang Du. 2020. Can value-added tax incentives of new energy industry increase firm's profitability? Evidence from financial data of China's listed companies. *Energy Economics* **86**, 104654. [Crossref]
12. Xiaomin Fan, Yingzhi Xu, Yongqing Nan, Baoli Li, Haiya Cai. 2020. Impacts of high-speed railway on the industrial pollution emissions in China. *Kybernetes* **ahead-of-print**:ahead-of-print. . [Crossref]
13. Xian Zheng, Wenwei Peng, Mingzhi Hu. 2020. Airport noise and house prices: A quasi-experimental design study. *Land Use Policy* **90**, 104287. [Crossref]
14. Elisabetta Gentile. 2020. Intellectual Property Rights and Foreign Technology Licensing in Developing Countries: An Empirical Investigation. *Economic Development and Cultural Change* **68**:2, 655-698. [Crossref]
15. Joel F. Houston, Chen Lin, Sibo Liu, Lai Wei. 2019. Litigation Risk and Voluntary Disclosure: Evidence from Legal Changes. *The Accounting Review* **94**:5, 247-272. [Crossref]
16. Jose M. Carbo, Daniel J. Graham, Anupriya, Daniel Casas, Patricia C. Melo. 2019. Evaluating the causal economic impacts of transport investments: evidence from the Madrid–Barcelona high speed rail corridor. *Journal of Applied Statistics* **46**:9, 1714-1723. [Crossref]
17. Hang Yuan, Kun Wang, Hangjun Yang, Cong Wang. 2019. The impact of rail dispatch system reform on import by rail --- Empirical evidence from a natural experiment in China. *Transport Policy* **79**, 165-176. [Crossref]

18. Changyuan Luo, Qingyuan Chai, Huiyao Chen. 2019. "Going global" and FDI inflows in China: "One Belt & One Road" initiative as a quasi-natural experiment. *The World Economy* **42**:6, 1654-1672. [Crossref]
19. Ricardo Mora, Iliana Reggio. 2019. Alternative diff-in-diffs estimators with several pretreatment periods. *Econometric Reviews* **38**:5, 465-486. [Crossref]
20. Youxing Huang, Yan Zhang, Li Lin, Guanghua Wan. 2019. Foreign direct investment and cleaner production choice: Evidence from Chinese coal-fired power generating enterprises. *Journal of Cleaner Production* **212**, 766-778. [Crossref]
21. Xinshuo Hou. 2019. High-Speed Railway and City Tourism in China: A Quasi-Experimental Study on HSR Operation. *Sustainability* **11**:6, 1512. [Crossref]
22. Yue-Jun Zhang, Jing-Yue Liu. 2019. Does carbon emissions trading affect the financial performance of high energy-consuming firms in China?. *Natural Hazards* **95**:1-2, 91-111. [Crossref]
23. Daniel P. Gross. 2019. The Consequences of Invention Secrecy: Evidence from the USPTO Patent Secrecy Program in World War II. *SSRN Electronic Journal* . [Crossref]
24. Julian Boulanger. 2019. Patents and Follow-On Innovation: Evidence From Patent Renewal Decisions. *SSRN Electronic Journal* . [Crossref]
25. Alberto Galasso, Hong Luo. 2019. Risk-Mitigating Technologies: The Case of Radiation Diagnostic Devices. *SSRN Electronic Journal* . [Crossref]
26. Haoyuan Ding, Cong Qin, Kang Shi. 2018. Development through electrification: Evidence from rural China. *China Economic Review* **50**, 313-328. [Crossref]
27. Beibei Shi, Chen Feng, Meng Qiu, Anders Ekeland. 2018. Innovation suppression and migration effect: The unintentional consequences of environmental regulation. *China Economic Review* **49**, 1-23. [Crossref]
28. Xing Li, Minyue Jin. 2018. Technology transfer in prescription drug market. *Journal of Modelling in Management* **13**:2, 495-518. [Crossref]
29. Alessandro Iaria, Carlo Schwarz, Fabian Waldinger. 2018. Frontier Knowledge and Scientific Production: Evidence from the Collapse of International Science*. *The Quarterly Journal of Economics* **133**:2, 927-991. [Crossref]
30. Petra Moser, Joerg Ohmstedt, Paul W. Rhode. 2018. Patent Citations—An Analysis of Quality Differences and Citing Practices in Hybrid Corn. *Management Science* **64**:4, 1926-1940. [Crossref]
31. Brian S. Silverman. 2018. Markets for (Codified, Tacit, Encapsulated, and Firm-Specific) Knowledge. *SSRN Electronic Journal* . [Crossref]
32. Alberto Galasso, Hong Luo. 2018. How Does Product Liability Risk Affect Innovation? Evidence from Medical Implants. *SSRN Electronic Journal* . [Crossref]
33. Deepa Mani, Anand Nandkumar, Anandhi Bharadwaj. 2018. Market Returns to Digital Innovations: A Group Based Trajectory Approach. *SSRN Electronic Journal* . [Crossref]
34. Yunsen Chen, Jianqiao Huang, Sheng Xiao, Ziye Zhao. 2018. Corporate Subsidiary Locations, Politics, and Firm Value. *SSRN Electronic Journal* . [Crossref]
35. Jun Wang, Juan Yang, Bo Li. 2017. Pain of disasters: The educational cost of exogenous shocks evidence from Tangshan Earthquake in 1976. *China Economic Review* **46**, 27-49. [Crossref]
36. Rafael Pinho Senra de Morais. 2017. Compulsory licensing of pharmaceuticals by the developing south. *Economics Letters* **161**, 74-77. [Crossref]
37. Nancy Gallini. 2017. Do patents work? Thickets, trolls and antibiotic resistance. *Canadian Journal of Economics/Revue canadienne d'économique* **50**:4, 893-926. [Crossref]

38. Shuai Shao, Zhihua Tian, Lili Yang. 2017. High speed rail and urban service industry agglomeration: Evidence from China's Yangtze River Delta region. *Journal of Transport Geography* **64**, 174-183. [Crossref]

39. Pavel Chakraborty, Chirantan Chatterjee. 2017. Does environmental regulation indirectly induce upstream innovation? New evidence from India. *Research Policy* **46**:5, 939-955. [Crossref]

40. Lu-Yi Qiu, Ling-Yun He. 2017. Can Green Traffic Policies Affect Air Quality? Evidence from A Difference-in-Difference Estimation in China. *Sustainability* **9**:6, 1067. [Crossref]

41. Joerg Baten, Nicola Bianchi, Petra Moser. 2017. Compulsory licensing and innovation – Historical evidence from German patents after WWI. *Journal of Development Economics* **126**, 231-242. [Crossref]

42. Leonid Kogan, Dimitris Papanikolaou, Amit Seru, Noah Stoffman. 2017. Technological Innovation, Resource Allocation, and Growth*. *The Quarterly Journal of Economics* **132**:2, 665-712. [Crossref]

43. Mike W. Peng, David Ahlstrom, Shawn M. Carraher, Weilei (Stone) Shi. 2017. History and the Debate Over Intellectual Property. *Management and Organization Review* **13**:1, 15-38. [Crossref]

44. Alireza Chavosh. 2017. Patent Nonuse: Are Patent Pools a Possible Solution?. *SSRN Electronic Journal* . [Crossref]

45. Elisabetta Gentile. 2017. Intellectual Property Rights and Foreign Technology Licensing in Developing Countries: An Empirical Investigation. *SSRN Electronic Journal* . [Crossref]

46. Lili Dai, Rui Shen, Bohui Zhang. 2017. Does the Media Spotlight Burn or Spur Innovation?. *SSRN Electronic Journal* . [Crossref]

47. Michael Berlemann, Vera Jahn. 2016. Regional Importance of Mittelstand Firms and Innovation Performance. *Regional Studies* **50**:11, 1819-1833. [Crossref]

48. Petra Moser. 2016. Patents and Innovation in Economic History. *Annual Review of Economics* **8**:1, 241-258. [Crossref]

49. Aaron K. Chatterji, Kira R. Fabrizio. 2016. Does the market for ideas influence the rate and direction of innovative activity? Evidence from the medical device industry. *Strategic Management Journal* **37**:3, 447-465. [Crossref]

50. K. Saggi. Trade, Intellectual Property Rights, and the World Trade Organization 433-512. [Crossref]

51. Marinho Bertanha, Petra Moser. 2016. Spatial Errors in Count Data Regressions. *Journal of Econometric Methods* **5**:1. . [Crossref]

52. Petra Moser. 2016. Patents and Innovation in Economic History. *SSRN Electronic Journal* . [Crossref]

53. Yugank Goyal. Economic and Procedural Constraints of Compulsory Licences for Medicines 437-458. [Crossref]

54. Sumit Agarwal, Yunqi Zhang. 2015. The Effects of Government Bail-out on Mortgage Modification. *SSRN Electronic Journal* . [Crossref]

55. Joel F. Houston, Chen Lin, Sibo Liu, Lai Wei. 2015. Shareholder Litigation and Management Earnings Forecasts. *SSRN Electronic Journal* . [Crossref]

56. Petra Moser, Alessandra Voena, Fabian Waldinger. 2014. German Jewish Émigrés and US Invention. *American Economic Review* **104**:10, 3222-3255. [Abstract] [View PDF article] [PDF with links]

57. Eric W. Bond, Kamal Saggi. 2014. Compulsory licensing, price controls, and access to patented foreign products. *Journal of Development Economics* **109**, 217-228. [Crossref]

58. Mericcan Usta, Feryal Erhun, Warren H. Hausman. 2014. Supply licensing when the manufacturer strategically commits to invest in R&D. *Naval Research Logistics (NRL)* **61**:4, 341-350. [Crossref]

59. Marinho Bertanha, Petra Moser. 2014. Spatial Errors in Count Data Regressions. *SSRN Electronic Journal* . [Crossref]

60. Joerg Baten, Nicola Bianchi, Petra Moser. 2014. Does Compulsory Licensing Discourage Invention? Evidence from German Patents after the US Trading-with-the-Enemy Act. *SSRN Electronic Journal* . [Crossref]
61. Barbara Biasi, Petra Moser. 2014. Does Cheap Access Encourage Science? Evidence from the WWII Book Replication Program. *SSRN Electronic Journal* . [Crossref]
62. Michela Giorcelli, Petra Moser. 2014. Copyright and Creativity: Evidence from Italian Operas. *SSRN Electronic Journal* . [Crossref]
63. Petra Moser. 2013. Patents and Innovation: Evidence from Economic History. *Journal of Economic Perspectives* **27**:1, 23-44. [Abstract] [View PDF article] [PDF with links]
64. B. Zorina Khan. 2013. Selling Ideas: An International Perspective on Patenting and Markets for Technological Innovations, 1790–1930. *Business History Review* **87**:1, 39-68. [Crossref]
65. Mericcan Usta, Feryal Erhun, Warren H. Hausman. 2013. Supply Licensing When the Manufacturer Strategically Commits to Invest in R&D. *SSRN Electronic Journal* . [Crossref]
66. Petra Moser. 2012. Innovation without Patents: Evidence from World's Fairs. *The Journal of Law and Economics* **55**:1, 43-74. [Crossref]
67. Ryan Lampe, Petra Moser. 2012. Patent Pools: Licensing Strategies in the Absence of Regulation. *SSRN Electronic Journal* . [Crossref]
68. Jeffrey P. Clemens. 2012. The Effect of U.S. Health Insurance Expansions on Medical Innovation. *SSRN Electronic Journal* . [Crossref]
69. Petra Moser. 2012. Patent Laws and Innovation: Evidence from Economic History. *SSRN Electronic Journal* . [Crossref]
70. Leonid Kogan, Dimitris Papanikolaou, Amit Seru, Noah Stoffman. 2012. Technological Innovation, Resource Allocation, and Growth. *SSRN Electronic Journal* . [Crossref]