GATEKEEPERS OF GROWTH: PATENT EXAMINERS, INNOVATION, AND INDUSTRIAL GROWTH IN THE UNITED STATES, 1919–1938 *

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Abstract

Patent protection is the most prevalent form of intellectual property protection, yet its impact on innovation and economic growth remains unclear. I introduce a difference-in-differences strategy exploiting the appointment of patent examiners at the U.S. Patent Office from 1919 to 1938. Newly appointed examiners grant 14% more patents to inventors from their home regions. Using examiners' appointments to isolate exogenous spatial variation in patent protection over time, I find that increased patenting fosters growth in manufacturing and income per capita. Increased patenting generates knowledge spillovers, which amplify subsequent innovation, in sectors technologically related to those of the newly appointed examiners.

Keywords: Patent Examiners, Knowledge Spillovers, Innovation, Economic Growth.

JEL Classification: O31, O34, O38, R11.

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INTRODUCTION

Innovation is the central engine for long-run growth (e.g., Romer, 1986; Aghion and Howitt, 1992). Market competition, however, generally underprovides innovation relative to the socially optimal level (Arrow, 1962). Governments thus frequently engage in policies to promote technological progress (Bloom, Van Reenen and Williams, 2019). Patent law constitutes the most widespread innovation policy.[1] Despite substantial theoretical and empirical research, however, the impact of patents on innovation and growth remains unclear (Boldrin and Levine, 2002, 2013; Lerner, 2002).[2]

Theoretically, patents have ambiguous effects on innovation.[3] On the one hand, patents may foster innovation by granting inventors temporary monopoly power over their ideas, thereby reducing the wedge between the private and social returns to innovation (Nordhaus, 1969). On the other hand, patents may generate inefficient "races," limit the diffusion of new ideas, inflate prices, and distort the direction of innovation (Loury, 1979; Green and Scotchmer, 1995). Despite this theoretical tension, providing credible causal evidence on the effects of patents on innovation has proven challenging. As noted by Budish, Roin and Williams (2016), changes in patent laws are seldom exogenous and are often influenced by other policies and countries. Moreover, they are typically country-wide, making identifying an appropriate "control" group difficult (Moser, 2013). Due to reverse causation, studying the effect of patent protection on broader economic growth presents even greater challenges.

The main contribution of this paper is to develop a strategy for estimating the causal impact of patent protection on innovation and growth. Using novel data on patent examiners at the United States Patent Office (USPTO) between 1919 and 1938, I first document that newly appointed examiners grant more patents to inventors from areas close to their city of origin. The appointment of examiners thus generates localized shocks to patent protection. Second, I leverage these shocks to estimate how patent protection impacts economic activity. Increased patenting from newly appointed examiners has a positive and substantial effect on manufacturing. Third, I compare patenting in the division of newly appointed examiners with that in other divisions to estimate how patent protection affects innovation. Patents increase in divisions other than the examiner's, especially in technologically closer areas, indicating that patent protection generates knowledge spillovers that promote innovation.

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[1] As of 2025, only eleven countries— Afghanistan, Eritrea, Marshall Islands, Maldives, Micronesia, Niue, Palau, South Sudan, Somalia, Suriname, and Timor-Leste—lack enforceable patent laws. A full list of existing patent offices is available at `wipo.int`.

[2] According to Boldrin and Levine (2013, p. 3), "[...] there is no empirical evidence that they [patents] serve to increase innovation and productivity, unless productivity is identified with the number of patents awarded." Conversely, Haber (2015, p. 814) states that "the weight of the evidence supports the claim of a positive causal relationship between the strength of patent rights and innovation."

[3] Bryan and Williams (2021) provide a complete discussion of the theoretical and empirical implications of patent laws.

I contribute newly digitized individual-level data on the universe of patent examiners active at the USPTO between 1919 and 1938. I manually link the examiners' records to genealogical documents to observe their city of origin. Additionally, the data contains information on the division where each examiner was active. To measure innovation activity, I construct a dataset covering the universe of patents granted in the United States between 1910 and 1950 by applying state-of-the-art large language models to patent texts available from Google Patents. I compile employment and income county-level data from the US federal censuses (1910–1950, Ruggles, Fitch, Goeken, Hacker, Nelson, Roberts, Schouweiler and Sobek, 2021) and city-level manufacturing outcomes from the Census of Manufactures (1910–1940, Lafortune, Lewis and Tessada, 2019).

The first result relates patent examiners to the volume of granted patents. In a difference-in-differences setting, I compare counties close to the city of origin of newly appointed examiners with other counties before and after the examiner is appointed. The identification assumption requires that patenting in treated and control counties would not have diverged had the examiners not been appointed. I corroborate the empirical plausibility of this assumption through event-study estimates showing no statistically significant pre-treatment differences between treated and control units. I find that the number of patents granted to inventors residing in treated counties immediately and persistently increases by approximately 14% after the examiner is appointed. I employ the text-based "impact" measure developed by Kelly, Papanikolaou, Seru and Taddy (2021) to show that the appointment of examiners increases the number of economically relevant patents granted in treated counties. I evaluate the spatial spillovers of examiners by looking at changes in patenting over increasingly distant areas from the examiner's city of origin. I find that examiners grant more patents to inventors located within 150 km of their city of origin.

Second, as the appointment of patent examiners generates local patenting shocks in their areas of origin, I leverage these shocks to estimate the causal impact of increased patent protection on economic growth in an instrumented difference-in-differences setting. The exclusion restriction requires that the appointment of patent examiners affected changes in economic activity only through the examiners' impact on patenting. Event-study results confirm the empirical credibility of this assumption. I find that increased patenting from examiners' appointments yields a 2% increase in the employment rate, which corresponds to 6.7% of the mean. The effect is entirely driven by an uptake in skilled manufacturing employment, especially in machinery and chemistry. Conversely, agricultural employment decreases in response to more intense patent protection, suggesting that patents may facilitate labor reallocation toward higher-value-added manufacturing. Average income per capita increases by 7.6% in response to higher labor force participation and shift toward better-paying occupations.

Data from the Census of Manufactures reveal similarly beneficial effects of patent protection on manufacturing growth. Manufacturing in the cities of origin of patent examiners displays substantial gains after examiners are appointed across a battery of outcomes, including the number of establishments, labor and material costs, and the total production value. For example, my estimates indicate that total manufacturing employment and production value, respectively, increase by 30% and 39% over the two decades that follow the appointment of patent examiners. As I estimate similar increases in labor and material costs, I find no evidence that innovation significantly alters the labor share in production. If anything, my results indicate that innovation complements labor as more intense innovation activity increases overall labor demand.

Third, I ask whether the increased patenting generated by the appointment of patent examiners partly reflects more intense innovation activity. To answer this question, I distinguish between patents granted in the same division of the newly appointed examiners and all other patents. Since each examiner oversees one division, patenting in other divisions reflects the response of innovation to increased patent protection through examiners' appointments. I find that patenting in divisions other than the examiners' increases by approximately 13% in the years after the appointment. As a comparison, patenting in the same division of the examiner increases by nearly 35%. To shed further light on the knowledge spillovers of patent protection on innovation, I show that the treatment effect of newly appointed examiners is largest for divisions that are technologically closer to their division and decreases for more distant sectors. My evidence thus provides consistent evidence that increased patent protection through newly appointed examiners has a positive and sizable effect on innovation.

Governments worldwide employ patent laws as part of a broader toolkit of policies to promote innovation (Bloom et al., 2019). Despite their ubiquity, the economics of innovation literature has produced "remarkably little empirical research" on the causal effect of patents on innovation (Williams, 2017, p. 443). Leveraging a novel insight into the effect of USPTO examiners on the intensity of patent protection, this paper provides novel causal evidence that patents promote innovation and fuel downstream economic growth.

Related Literature This paper adds to three lines of literature. First, a large literature studies the effects of patents on innovation both theoretically (e.g., see Nordhaus, 1969; Scherer, 1972) and empirically (e.g., Williams, 2013; Galasso and Schankerman, 2015; Sampat and Williams, 2019; Moscona, 2021; Hegde, Herkenhoff and Zhu, 2023) attention, also in historical contexts (Moser, 2005, 2012, 2013; Mokyr, 2009). North and Thomas (1973) argued that the emergence of the patent system in Britain was a key element within the broader institutional developments that enabled the Industrial Revolution. However, as noted by Williams (2017), providing credible evidence on the impact of patents on innovation is challenging because patent rights are endogenously determined. I inform this literature by providing causal evidence that patent protection has a positive and large effect on innovation. Methodologically, this paper proposes a novel identification strategy to construct plausibly random variation in patenting activity by leveraging the appointment of patent examiners.

Second, I add to a growing literature investigating how the design of patent offices, and, more specifically, patent examiners influence innovation (Cockburn, Kortum and Stern, 2003; Feng and Jaravel, 2020).[4] Recent studies have examined the role of patent examiners in the patenting process (Lemley and Sampat, 2012; Gaule, 2018; Righi and Simcoe, 2019) and unveiled patterns of discrimination based on the race (Coluccia, Dossi and Ottinger, 2024) and gender (Avivi, 2024) of the inventors. To the best of my knowledge, this is the first paper to document that examiners are more likely to issue patents to inventors close to their area of origin. This pattern introduces one novel dimension of bias of patent examiners, who Bryan and Williams (2021) dub as the "gatekeepers of quality [of patents]," which influences aggregate innovation dynamics.

Third, my findings are related to the extensive literature studying the link between agglomeration and innovation (for a review, see Carlino and Kerr, 2015; Duranton and Kerr, 2018).[5] Evidence on the effect of spatial proximity on innovation remains mixed, with existing studies finding both positive (Jaffe, Trajtenberg and Henderson, 1993; Agrawal, Kapur and McHale, 2008; Bosquet and Combes, 2017; Moretti, 2021) and no relationship between agglomeration and innovation (Rosenthal and Strange, 2008; Azoulay, Graff Zivin and Wang, 2010; Waldinger, 2012; Moser, Voena and Waldinger, 2014). My contribution is twofold. First, I provide novel evidence that institutions—patent examiners—influence the spatial distribution of innovation. Second, my strategy enables the joint study of spatial and knowledge spillovers. I find strong evidence of knowledge spillovers, whereas the positive effects of spatial agglomeration on innovation exhibit fast decay.

Outline of the Paper The rest of the paper is organized as follows. Section I presents the key features of the historical U.S. patent system. In Section II, I describe the data sources and the methods I adopt to construct the analysis datasets. In Section III, I introduce the empirical framework and the underlying identification assumptions. Section IV presents the results on the effect of examiners on patenting activity; Section V discusses the causal effects of patents on economic growth; in Section VI, I analyze the knowledge spillovers of patents on innovation. The last section concludes.

[4]Methodologically, most papers leverage the "quasi-random" assignment of patents to examiners. This paper, instead, proposes a new design which allows for non-random assignment by exploiting the newly documented geographical preferences of examiners.

[5]A broader literature in urban economics studies the consequences of agglomeration forces on a range of outcomes, including industry location (Ellison and Glaeser, 1997; Duranton and Overman, 2005; Ellison, Glaeser and Kerr, 2010) and productivity (Greenstone, Hornbeck and Moretti, 2010; Combes, Duranton, Gobillon, Puga and Roux, 2012).

I INSTITUTIONAL BACKGROUND

The United States Patent Office (henceforth, USPTO) is widely credited as one of the first modern intellectual property protection institutions (e.g., see Khan and Sokoloff, 2004; Khan, 2020). Since its founding in 1836, the USPTO has maintained low patent application fees to ensure widespread access to intellectual property protection. For example, in 1925, patenting in the United States was approximately ten times less expensive than in the United Kingdom (Lerner, 2002). Moreover, a technical examination of each application has to ascertain its novelty. The 1836 Patent Act establishes that this examination has to be performed by qualified officers—patent examiners—who are forbidden from obtaining patents.[6]

Until 1969, the USPTO's only office was in Washington, DC.[7] During my study period, the application process unfolded in three phases. First, the applicant mailed the application to the USPTO office. Second, the application was assigned to one division based on its content. Each application was overseen by a single division, and a single principal examiner headed each division. Third, examiners could grant patent protection or demand further changes. The records of issued patents have survived and are used in this paper, but denied applications were destroyed (Andrews, 2021).

Importantly, the text of patent applications contained the first and last name of each inventor as well as their city of residence. This information was available to examiners who evaluated the novelty of the invention. This observation is crucial for my empirical design since I advance and validate the hypothesis that examiners "favored" applicants living in areas close to their city of origin.

Between 1900 and 1950, innovation in the United States underwent tumultuous changes. As corporations gained prominence, the share of independent inventors dropped from 80% to approximately 50%, even though they remained a source of economically valuable innovation (Nicholas, 2010). The Great Depression severely disrupted innovation by hampering access to credit (Babina, Bernstein and Mezzanotti, 2023). The USPTO, however, remained remarkably stable throughout this period. The consistency of the institutional aspects of the patent protection system is a convenient feature of this historical setting. Additionally, unlike today, each principal examiner oversaw one and only one division, making the examiner-patent mapping straightforward.

[6] In 1925, for example, there were 43 principal examiners. The average tenure was 9.3 years, and the average wage was approximately 70,000$ at 2010 prices.

[7] During World War 2, part of the USPTO was temporarily relocated to Alexandria, VA.

II DATA

This section presents the data I compile to perform the empirical analysis. Appendix A provides additional technical details. I describe the new examiner dataset I assemble in Section II.A. Section II.B presents the patent data, which constitute the primary outcome variable; Section II.C discusses the manufacturing data I employ to measure the downstream effects of patent protection. In Section II.D, I briefly explain the methodology I follow to construct the final analysis datasets.

II.A Individual Examiner Data

I construct a novel individual-level dataset of principal examiners active at the patent office between 1919 and 1938. The historical context and limitations of the underlying data dictate the choice of the sample period. Examiner data are available from 1915 to 1950. The period 1919–1938 is selected to minimize the influence of the collapse in international science during World War I (Iaria, Schwarz and Waldinger, 2018) and the profound changes in the US innovation system that followed World War II (Gross and Sampat, 2023).

I collect information on 184 examiners from the "Official Register of the United States," a source first used in economics by Aneja and Xu (2022). The Register was published biannually until 1921 and then yearly and contains information on the name, surname, and USPTO division where each examiner was active.

I manually link the examiners' records to genealogical documents provided by `Ancestry.com`. I match examiners by their first and last names and the occupations they list in the census.[8] Moreover, since there was only one USPTO office in Washington, DC, I can further narrow the search to individuals residing in DC, Maryland, and Virginia. The final sample comprises 176 out of 184 examiners uniquely linked to their census records. By following examiners over their lifetime, I map them to their city of birth and use this information to construct county- and city-level exposure to newly appointed examiners.

Practically, upon arriving at the USPTO, patent applications would be assigned to a division depending on their content. Each principal examiner was responsible for one division. I obtain the precise subjects covered by each division—and, hence, each examiner—from various "Classification of subjects of invention," historical publications of the Patent Office intended as guides to patent applicants. This information allows me to assign granted patents to USPTO divisions and, thus, examiners.

Appendix Table C.1 lists sample statistics on the final dataset. I match 96% of the examiners to the

[8]In all cases, I find at least one census entry whose listed occupation is a variant of "Patent Examiner." This pattern provides strong evidence in support of the linking procedure.

genealogical data, which, in all but one case, allows me to infer the year of birth. All examiners are white and only one is female. There are no examiners born outside of the United States, but approximately 22% are second-generation immigrants. States in the Northeast, Midwest, and, especially, the DC area—Washington, DC, Virginia, and Maryland—are overrepresented as the states of origin of the examiners compared to the broader population. Examiners serve for long periods, with an average tenure of 9 years.

II.B Patent Data

I collect the universe of patents granted in the United States between 1910 and 1950 from Google Patents. Following Coluccia and Dossi (2025), I apply large language models (LLMs) to the full text of the patents to extract the residence address of each inventor, the filing and issue date, and the CPC class. LLMs are substantially more flexible than traditional text-search approaches. This methodology allows me to confidently extract the data for the vast majority (98%) of patents. I geo-reference the inventors' residences using commercial software to map patents to 1930 counties.

Moreover, I apply an LLM to the full text of each patent to assign patents to USPTO divisions. The "Classification of subject of invention" lists all the subjects covered by each division. The LLM allows me to map patents to the most appropriate division based on their content as described in the patent document. I use this information to estimate the knowledge spillovers of examiner-induced patenting.

Patents vary extensively in terms of their economic and technological impact. I employ the "impact" measure proposed by Kelly et al. (2021) to account for this heterogeneity. According to their measure, a patent is more important if it introduces a word that has not been used before and that appears in subsequent grants. As a baseline indicator, I flag high-impact patents as those in the top quintile of the impact distribution.

I implement a simple methodology to measure the technological similarity between USPTO divisions. Each patent is mapped to one division and several CPC technological classes. I thus compute, for each division, a vector that collects the share of patents in that division by CPC class. The technological proximity between the two divisions is the cosine similarity between their vector representation in the technology space. Intuitively, two divisions are more similar if the patents assigned to those divisions belong to the same CPC technology categories.[9] To assess the robustness of this measure, I employ a document embedding algorithm to measure the pairwise similarity between each patent

[9] Formally, let divisions i and j be represented by vectors $D_i = \{s_{i1}, \ldots, s_{iN}\}$ and $D_j = \left\{s_{j1}, \ldots, s_{jN}\right\}$, where the generic term s_{ik} denotes the share of patents in division i belonging to CPC class k. Then, the similarity σ_{ij} between the two divisions is $\sigma_{ij} \equiv D_i \cdot D_j^{\intercal} / \left(||D_i|| \cdot ||D_j||\right)$. By construction, $\sigma_{ij} \in [0, 1]$ and $\sigma_{ii} = 1$.

and aggregate them at the division level. In Appendix Figure D.1, I show that the resulting metric positively correlates with the baseline "cross-posting" approach.

II.C Manufacturing Data

I leverage two data sources to investigate the downstream effects of innovation on economic growth.

From the full-count population census, I compile several indicators of economic activity at the county level and decade frequency between 1910 and 1950 (Ruggles et al., 2021). I construct the overall employment rate, defined as the share of the population in formal employment and the share of workers employed in blue-collar and white-collar occupations. Within the manufacturing sector, I distinguish between durable and non-durable goods industries. Additionally, I compute the employment share by other major industry groups, such as public administration, trade, and transportation, and more granular sectors within manufacturing, such as machinery, metallurgy, and textiles. While income was not recorded until 1940, I adopt a standard occupation-based income score proxy to measure gains in labor income. I aggregate these measures at the county-by-decade level and harmonize the resulting series to consistent county borders.

The population census provides rich information on employment dynamics but provides no insight into firm dynamics. I thus employ city-level data from the Census of Manufactures between 1910 and 1940 to investigate the firm response to increased patenting activity. I aggregate the underlying industry-by-city data digitized by Lafortune et al. (2019) into city-level indicators of economic activity. I consolidate the information contained in the different census waves into six categories: the number of establishments, the number of workers, total labor costs, total material costs, production value, and value-added. The original data source contains information on 147 cities; however, since my analysis exploits within-state variation, I exclude all states with a single city in the Census, which reduces the total sample size to 99 cities. The sample is not balanced because not all cities are observed in all census waves. Using a commercial geo-reference tool, I assign each city to precise latitude and longitude coordinates. To assess the robustness of the results to selection on observables, I employ the population census and data from Berkes, Karger and Nencka (2023) to compile city-level demographic characteristics. Appendix A.IV provides additional details.

II.D Sample Construction

I construct four samples. Samples "A" and "B" contain information on patenting activity. Sample "A" is a yearly panel that follows counties between 1919 and 1938. It contains data on the total number of patents and the number of "high-impact" patents issued in each county over time. In addition, I construct a variable that returns, for each year, the distance between each county's centroid and the closest city of origin among the examiners active in that year. Sample "B" replicates sample "A," except it follows county-division pairs over time. In sample "B," I pair each county-division-year entry with the minimum distance between the centroid of the county and the closest origin city of an examiner active in the given year and division.

Sample "C" follows counties across census decades between 1910 and 1950, containing employment and other demographic characteristics tabulated from the population census. Here, too, I construct a variable that returns the minimum distance between each county's centroid and the closest origin city of an examiner active in the previous decade.

Finally, dataset "D" is a city-level panel dataset at a decade frequency between 1910 and 1940, containing data tabulated from the Census of Manufactures and additional controls compiled from the population census. For each city and decade in the sample, I define an indicator variable equal to one if at least one examiner from that city was active in the previous decade and zero otherwise.

Appendix Table C.2 provides descriptive statistics for the outcome variables in the various samples (Panels A–C) and on control variables included in robustness exercises (Panel D).

III EMPIRICAL FRAMEWORK

The purpose of this paper is to estimate the causal effect of patents on innovation and broader economic growth. Disentangling the effect of patents on innovation is challenging because variation in patent laws is scarce and typically country-wide, which makes constructing an "appropriate" control group challenging (Moser, 2013; Budish et al., 2016). Estimating the causal impact of innovation on growth is inherently affected by reverse causality. On the one hand, a well-established endogenous growth literature argues that innovation is the primary driver of long-term growth (e.g., Romer, 1986, 1990; Aghion and Howitt, 1992). On the other hand, larger returns to innovation fueled by higher growth incentivize innovation (e.g., Acemoglu, 2002, 2007). This circular loop invalidates any strategy that does not single out exogenous variation in innovation activity.

The methodological contribution of this paper is to propose a novel empirical design to estimate the causal effect of patent protection on innovation and growth. My argument is that *if* patent examiners are more lenient towards inventors from their area of origin, then the appointment of a new examiner generates a positive shock to local patenting in the examiner's area of origin. Since this shock would arguably be orthogonal to economic conditions in that area, I can use it to estimate the effect of patents on growth. Moreover, since each examiner oversees one division, the response of patenting activity in divisions other than the examiner's reflects the innovation response to increased patent protection.

The paper thus follows the following logical steps. First, I document that the appointment of an examiner at the USPTO results in a substantial and enduring increase in patents granted to inventors residing near the examiner's city of origin. Second, I employ this shock as an instrument to estimate the causal effect of patent protection on economic growth. Finally, I examine how the appointment of an examiner impacts innovation by analyzing patenting activity in divisions other than the examiner's own division.

I estimate variations on the following difference-in-differences (DiD) model:

$$\mathbb{E}[y_{it} \mid X_{it}] = f\left(\beta \times I(t \geq Examiner_i) + \alpha_i + \alpha_t\right), \tag{1}$$

where i denotes counties or cities and t stands for years or decades. The terms α_i and α_t denote unit and time fixed effects.[10] The baseline treatment term is $I(t \geq Examiner_i)$. In the county-level regressions, the treatment is activated whenever an examiner who is born within k kilometers of the centroid of county i is appointed at the patent office. I set $k = 100$ kilometers when studying innovation and $k = 50$ kilometers when looking at the various proxies of growth.[11] In the city-level regressions, the treatment is equal to one after an examiner from city i is appointed and zero otherwise. The term X_{it} collects a set of unit-level controls we include in various robustness regressions. Appendix Figure D.2a displays the counties included in the treatment group when studying output (in red) and patenting (in red and yellow). Appendix Figure D.2b reports the spatial distribution of treated and control cities in red and blue, respectively.

The functional specification $f(\cdot)$ depends on the outcome variable. I use patents to measure the patenting response to the appointment of examiners, and manufacturing employment and firm outcomes to explore the downstream effect of innovation on economic growth. Since patents are a count variable that exhibits substantial left skewness, I adopt a Poisson quasi-maximum likelihood (PQML) regression.[12] For all other variables, I employ a standard OLS specification. Standard errors are clustered at the county or city level, i.e. the level of variation of the treatment.

Identification in this context requires a standard parallel trends assumption, which maintains that the

[10] When working with decade-level data, I substitute time fixed effects (α_t) with state-by-time fixed effects ($\alpha_{s\times t}$) to control for time-varying heterogeneity at the state level and increase the comparability of units over time.

[11] I explain why we choose two thresholds in the next section. I also display how $\hat{\beta}$ in regression (1) varies for different values of k.

[12] The key advantage of the PQML estimator is that it remains consistent when dealing with non-negative dependent variables, such as patents in the presence of fixed effects without requiring to model the underlying distribution explicitly (Correia, Guimarães and Zylkin, 2020). Another advantage of the PQML estimator is that it allows me to work with zeros without imposing arbitrary log transformations (Chen and Roth, 2024).

outcomes—patenting and growth—in treated and untreated units would not have diverged in the absence of the appointment of an examiner originating in proximity to the treated units. While this assumption is not testable, I estimate a set of fully flexible specifications associated with regression (1):

$$\mathbb{E}[y_{it} \mid X_{it}] = f\left(\sum_{\substack{k=-a \\ k\neq -1}}^{b} \beta_k \times I(t - Examiner_i = k) + \alpha_i + \alpha_t\right), \tag{2}$$

where the terms $I(t - Examiner_i = k)$ code the periods since an examiner close to unit i is appointed. In all cases, I estimate pre-treatment coefficients $\hat{\beta}_{k<0}$ that are never statistically different from zero, hence providing empirical support for the plausibility of the parallel trends assumption. Importantly, specification (2) also allows me to evaluate the dynamic treatment effects of new examiners on the outcome variables.

To provide further evidence in support of the parallel trends assumption, Table I compares treated and untreated counties and cities in terms of the key outcome and the controls included in the robustness analyses. Columns (1–2) report the average growth rate of each variable in control counties (Panels A–B) and cities (Panel C). Columns (3–4) refer to the treated units. In columns (5–6), I report the difference between treated and control units and its standard error. All regressions include state and year fixed effects, and as in the rest of the analysis, standard errors are clustered at the unit-level: counties in Panels A and B and cities in Panel C. The estimates indicate that treated and control units are comparable along most dimensions, as their difference is quantitatively small and almost always statistically insignificant.

Since examiners are appointed at different times, the treatment roll-out across counties is staggered. As evidenced by Goodman-Bacon (2021), this circumstance implies that the two-way fixed effects estimator may fail to yield the average treatment effect. In robustness exercises, I thus adopt the stacked difference-in-differences estimator proposed by Cengiz, Dube, Lindner and Zipperer (2019) and obtain consistent and quantitatively similar results to the baseline.

Regressions (1)–(2) are "reduced-form" specifications that quantify the effect of newly appointed examiners on patenting and growth outcomes. This setting, however, allows me to estimate the impact of innovation on growth by employing the appointment of patent examiners as an instrument. Specifically, I complement the reduced-form estimates using an instrumented difference-in-differences model (IV-DiD) where I employ the baseline treatment $I(t \geq Examiner_i)$ as an instrument for the log-number of patents $Patents_{it}$. Appendix Figure D.3 reports the first-stage positive correlation between examiner appointment and patenting both at the county (Figure D.3a) and the city level (Figure D.3b). The identifying exclusion restriction in the IV-DiD setting requires that the appointment of examiners must affect changes in the outcome variable only through changes in patenting activity. Compared to a standard DiD, the IV-DiD estimator is thus robust to time-varying endogeneity of the main explanatory variable because the instrument is not subject to time-varying unmeasured confounding (Ye, Ertefaie, Flory, Hennessy and Small, 2023). Under this assumption, Miyaji (2024) shows that the IV-DiD estimator quantifies the local average treatment effect on the treated (LATT). The LATT measures the effect of the treatment on exposed units induced by the treatment after the treatment is activated. In my context, it is therefore natural to suppose that the LATT will be larger than the average treatment effect on the treated (ATT), which is the target estimand in the standard DiD setting.

IV EXAMINERS AND PATENTING ACTIVITY

I begin the empirical analysis by providing evidence that, as an examiner is appointed at the USPTO, they grant more patents to inventors living in an area close to their place of origin. In Section VI, I further elaborate on whether this behavior can be qualified as a "bias."

IV.A The Causal Effects of Examiners on Patenting

I start with a descriptive, stylized fact. Appendix Figure D.4 reports the correlation between the number of examiners born in each state and active over the period 1919–1938, the number of patents (Figure D.4a), and the number of patents per capita (Figure D.4b). In both cases, the graphs display a positive and statistically significant correlation. Quantitatively, one additional examiner born in state s is associated with a 12.7% increase in the number of patents, and a 6.1% increase in the number of patents per capita granted to inventors from state s. Washington, DC is the single most represented "state" among examiners, possibly given that the USPTO was hosted in the federal capital. Even when excluding DC from the sample, however, a positive correlation remains between examiners and patents.

This exercise does not necessarily reflect a causal effect of examiners on patenting. An omitted variable that correlates with the number of examiners appointed at the USPTO and the number of patents granted to inventors from each state would generate a correlation between these two variables. In the rest of the section, I employ the causal design discussed in the previous section to provide evidence against this possibility.

In Figure Ia, I construct the total number of patents granted before and after counties are "exposed" to a newly appointed examiner over a 20-year window around the appointment of the examiners. A county is exposed to an examiner if that examiner was born within 100 km of that county's centroid. This count thus excludes never-treated counties. The black dashed line marks the year when the examiner is appointed, and the solid black line fits a linear trend on the pre-appointment patenting data. The time series exhibits a clear and substantial jump between the period before the examiner's appointment and the following year. Before the examiner is appointed, the treated counties produced approximately 1,500 patents each year. This figure nearly triples after the examiner is appointed. Naturally, this exercise yields only suggestive evidence because it lacks a control group and aggregates counties into a single time series.

Figure Ib reports the event-study estimates obtained from the Poisson DiD model (2) using the number of patents as the outcome variable. In this regression, I include county fixed effects to control for time-invariant unobserved heterogeneity at the county level, arising, for example, from geography. One such confounding factor would be the distance from the USPTO, which may predict the number of appointed examiners. In addition, I include year fixed effects to control for time-varying aggregate changes in patenting. This is particularly relevant since Figure Ia indicates that treated counties display an upward trend in patenting, which does not reflect examiners.

The event-study estimates indicate no statistically significant difference between treated and control counties before the examiner's appointment. This pattern provides evidence in support of the parallel trends assumption under which the DiD estimates are consistent estimators of the average treatment effect. After the examiner is appointed, on the other hand, patenting in treated counties increases relative to the control units. The increase is highly statistically significant, as indicated by the test for the joint significance of the post-treatment coefficients ($p < 0.01$). Besides the average post-treatment effect, the event-study estimates indicate that the increase in patenting activity manifests immediately after the examiner is appointed.[13] Patenting in treated counties increased by approximately 17% after the examiner is appointed.[14] Patenting further increases over the following years and, ten years after the examiner is appointed, patenting in exposed counties is approximately 34% higher than in control counties. Therefore, I do not find evidence of mean reversion over time.

Table II reports the estimated average treatment effect from equation (1). Column (1) displays the average treatment effect of my preferred specification. The appointment of an examiner produces a 14% increase in patenting in treated relative to control counties over the sample period. Quantitatively, this effect translates into 0.42 additional patents granted every year. In columns (2) and (3),

[13] It is worth noting that the "year" dimension is the year when patents are granted, as opposed to the year of application. The immediacy of the effect is thus not surprising.

[14] To compute the magnitude of the coefficients in the PQML setting, consider regression (1) and suppose $\hat{\beta}$ is the estimated β coefficient. Then, it is $\ln \mathbb{E}[y_{it} \mid X_{it}, \alpha_i, \alpha_t; I(t \geq \text{Examiner}_i) = 1] \equiv \ln \mathbb{E}[y \mid 1] = \hat{\beta}$ and $\ln \mathbb{E}[y_{it} \mid X_{it}, \alpha_i, \alpha_t; I(t \geq \text{Examiner}_i) = 0] \equiv \mathbb{E}[y \mid 0] = 0$. Hence, $\ln \mathbb{E}[y \mid 1] - \ln \mathbb{E}[y \mid 0] = \hat{\beta}$, $\mathbb{E}[y \mid 1] = e^{\hat{\beta}} \mathbb{E}[y \mid 0]$, and, therefore, the percentage change associated with the treatment activation is $(\mathbb{E}[y \mid 1] - \mathbb{E}[y \mid 0]) / \mathbb{E}[y \mid 0] \times 100 = (e^{\hat{\beta}} - 1) \times 100$.

the sample respectively excludes Washington, DC, and the "DC Area," defined as DC proper and the neighboring states of Virginia and Maryland. The underlying rationale is that the number of examiners in these states is considerably higher than the national average, possibly due to their proximity to the patent office. The appointment of examiners from these states is thus considerably more frequent and, hence, possibly less salient. The estimates, however, remain essentially unchanged relative to the baseline.

Do examiners produce new innovation clusters, or do they foster already innovative areas? To answer this question, I split the estimation sample into two groups: above-median and below-median total issued patents. My evidence suggests that the appointment of patent examiners increases patenting activity in already innovative areas (column 4), whereas it has no statistically significant effect on less innovative counties. This difference indicates that examiners do not "create" innovation, but rather support already active inventors.

In columns (6–7), I focus on high-impact patents according to the text-based measure of Kelly et al. (2021). Looking at high-impact patents is important because most patents bear little economic value (Abrams, Akcigit and Grennan, 2013). Hence, if examiners increased patenting of low-value innovation, their economic impact would be modest. In column (6), the dependent variable is the number of patents in the top 20% of the impact distribution. I estimate a positive and large effect of examiners on the number of high-impact patents granted in treated relative to control counties. Quantitatively, I estimate a 52% increase in high-impact patents following the examiners' appointments. In column (7), I use the share of high-impact patents relative to the total number of patents and find that, since the increase in high-impact patents is larger than the overall increase in patenting, the share of economically relevant innovations granted patent protection in treated counties increases after an examiner is appointed. These findings indicate that while examiners are more likely to grant patent protection to inventors from their area of origin, this practice does not translate into worse quality or reduced innovative content of the patents.

I perform a battery of tests to gauge the robustness of these findings.[15] In columns (1–2) of Appendix Table C.3, I report the standard errors obtained using estimators. Importantly, I employ the estimator discussed in Conley (1999) to account for potential spatial autocorrelation at different cutoffs and find that the statistical significance remains unchanged. The appointment of examiners is staggered across counties. In Appendix Table C.4, column (1) reports the estimates obtained using the estimator described in Cengiz et al. (2019), which, if anything, are larger than the baseline two-way fixed effects results. Columns (1–2) in Appendix Table C.5 report the confidence intervals for the estimate of

[15] Appendix B lists all the robustness analyses, the key challenge they address, and a brief explanation of the results.

the average treatment effect when allowing for a violation of the parallel trends in the pre-treatment period up to $\bar{M}$, as discussed in Rambachan and Roth (2023). The results indicate that the baseline estimate is considerably robust to violations of parallel trends. To assess the robustness of my results to selection on observable characteristics, in Appendix Figure D.6a, I include a set of county-level characteristics measured in 1920 and interacted with a time trend in regression (1). The resulting estimated treatment effect remains stable and statistically indistinguishable from the baseline estimates. Finally, in Appendix Figure D.7a, I re-estimate the baseline model, excluding one state at a time, to ensure that the results are not driven by any specific part of the United States. The estimates remain stable regardless of which state is excluded.

IV.B Spatial Spillovers of Examiners on Patenting

Thus far, I have assumed that examiners are more lenient towards inventors who reside in cities within 100 km of their city of birth, and apply no differential treatment to inventors from outside this area. Since this is an assumption, I now relax it and estimate the effect of examiners on patenting for different distance cutoffs. Implicitly, the results thus shed light on the spatial spillovers of examiners on patenting activity.

I consider two alternative definitions of the treatment associated with each examiner. In the first case, displayed in Appendix Figure D.5a, I progressively increase the radius τ around the origin city of the examiner and include the counties whose centroid lies within the circle in the treatment group. All other counties are included in the estimation sample as control units. In the second case, which I label "doughnut" treatment and display in Figure D.5b, the treatment is composed of counties whose centroid lies within the intersection between the circle centered around the examiner's city of origin of radius τ km and the concentric circle of radius $\tau - 20$ km. Counties closer than $\tau - 20$ km from the examiner's city of origin are excluded from the estimation sample, and those further than τ km constitute the control group. In both cases, I expect to estimate treatment effects that decrease in magnitude as the distance from the examiner's origin city τ increases.

Figure II reports a set of treatment effects obtained from the baseline estimating equation (1) using the number of patents as the outcome variable and distance cutoffs τ between 0 and 200 km. Figure IIa reports the estimates when the treatment comprises all counties whose centroid lies within τ km of the city of origin of the examiner, whereas Figure IIb reports the results of the "doughnut" treatment definition. In both cases, the treatment effect peaks in magnitude between 0 and 50 km of the city of origin of the examiner and declines thereafter. In Figure IIa, the treatment effect remains statistically significant until 150 km, whereas in Figure IIb, I find no statistically significant effects beyond 110 km.

This pattern indicates that the effect of the examiners goes beyond county borders.[16]

The spatial decay of the average treatment effect provides a rationale for the treatment cutoffs in the baseline specification. I pick 100 km in the patenting regression because the treatment effect is relatively stable around this cutoff. Output variables, however, are only available at 10-year intervals every census year. At this coarser frequency, it is plausible that measurement error makes the effect of examiners on patenting and, in turn, output, harder to observe. I thus pick a more conservative 50-km distance cutoff that maximizes the impact of examiners on patents. The drawback of this choice is that it makes the associated estimates of the effect of patenting on growth more local and less suited to compute aggregate effects.

V THE DOWNSTREAM EFFECTS OF PATENT PROTECTION

In this section, I discuss how the appointment of examiners affects economic growth through increased patenting. This analysis is grounded in standard models of endogenous growth, where the long-run growth rate of the economy is ultimately determined by the rate of technological progress (e.g. Romer, 1990; Aghion and Howitt, 1992). I organize this section into two parts. First, in Section V.A, I discuss the effect of patenting on employment growth across sectors. Second, in Section V.B, I focus on manufacturing.

V.A Patent Protection and Employment

As a first indicator of economic activity and sectoral specialization, I calculate the employment rate across sectors using data from population censuses conducted between 1910 and 1950. The underlying microdata enables me to tabulate highly disaggregated county-industry employment. I estimate the baseline regression (1) at the (census) decade level, and, because at this time-frequency, counties may undergo shocks that are correlated with the appointment of patent examiners, I include state-by-time fixed effects to compare counties within the same state and mitigate this concern.

Table III reports the results. Panel A reports the "reduced-form" estimates from the baseline post-examiner treatment. The reduced-form estimates quantify the effect of newly appointed examiners on the outcome variables. In Panel B, I estimate an instrumented difference-in-differences regression where the instrument is the treatment of Panel A, and the endogenous variable is the (log) number of patents. Appendix Figure D.3a displays the statistically significant correlation between the instrument and patenting activity. The Table further reports the first-stage F-statistic, which supports the instrument's relevance.

[16]This pattern is evident in Figure D.2 (specifically, in Figure D.2a), where the counties of the examiners are colored in blue and all other treated counties within the baseline 100 km distance cutoff are displayed either in red or yellow.

All variables are normalized by population in columns (1–6). In column (1), I show that the employment rate increases in response to the appointment of an examiner originating from a city within 50 km of the county. Quantitatively, the employment rate increases by approximately 2%, which corresponds to 6.7% of the mean. A back-of-the-envelope calculation allows me to—tentatively—quantify the net effect of patents on employment. The cumulated gain in innovation produced by an examiner's appointment over a decade is, on average, five more patents. This increase translates into an employment gain of approximately 900 workers. On average, each patent thus increases employment by 180 workers. Benchmarking this magnitude is challenging because credible causal evidence on the effect of innovation on employment is scarce. However, my estimates provide strong evidence that innovation increases labor demand, as documented by, among others, Van Reenen (1997).

The granularity of the census micro-data allows me to explore the employment effects of innovation across sectors. In column (2), I use the white-collar employment rate as the dependent variable. White-collar employment was generally more skilled and, hence, better paid, but did not directly benefit from product innovations.[17] I find that white-collar employment does not react to changes in innovation activity that follow the appointment of patent examiners. By contrast, in column (3), I focus on manufacturing employment and find that employment gains in blue-collar occupations entirely drive the aggregate employment positive response reported in column (1).

In columns (4–5), I further distinguish between durable and non-durable manufacturing employment. The employment gains are concentrated in durable manufacturing, whereas I find no statistically significant effect of patenting on non-durable manufacturing. Figure IIIa reports the estimated treatment effect across a wider range of industries. The appointment of patent examiners generates large employment gains only in durable manufacturing. The other sectors display either an insignificant response or, in the case of agriculture, a negative shift. The opposing employment responses of durable manufacturing and agriculture to innovation are consistent with early literature on structural change linking factor reallocation across sectors, productivity, and innovation (Lewis, 1954; Lucas, 1988).

Figure IIIb reports the treatment effect of examiners across sectors within manufacturing. Unsurprisingly, I estimate larger employment responses in durable goods sectors. Additionally, the magnitude of the estimates is larger in relatively more skill-intensive industries, such as machinery and chemistry. This pattern suggests that innovation activity boosts labor demand in relatively more skill-intensive sectors. In contrast, I find no effect on low-skill industries like construction, textiles, and

[17] In the 1930 census, white-collar occupations commanded a higher occupational income score (29.78) than blue-collar jobs (25.60).

food-processing manufacturing.

In column (6) of Table III, I employ the occupational income proxy available in the census to construct a measure of income per capita. Income per capita displays a positive and statistically significant response to the appointment of patent examiners. The IV-DiD estimate indicates that income per capita increases by approximately 7.6% in response to the treatment. The larger increase in income per capita compared to the employment rate suggests that innovation triggers a reallocation of labor toward better-paying occupations, presumably more skill-intensive, especially in manufacturing.

Finally, in columns (7–8), I present a "falsification" exercise where I employ the overall population (column 7) and the working-age population (column 8) as the outcome variables. The estimates presented thus far may reflect broader population changes that are unrelated to innovation activity. Suppose, for example, that the probability of an examiner's appointment rose as the county population increased. Then, my estimates may reflect the underlying population dynamics rather than the causal effect of examiners—and patenting—on employment growth. However, the results reported in columns (7–8) do not support this interpretation. Neither the population nor the working-age population increases after an examiner is appointed. It thus appears unlikely that my estimates reflect correlated shocks rather than the effect of examiners and innovation on economic activity.

As in the previous section, I perform a set of exercises to assess the robustness of these results.[18] In Appendix Table C.6, I check that the statistical significance of the results is unaffected when employing alternative standard error estimators. Appendix Table C.7 and Table C.8 show that the estimates are quantitatively unchanged when employing a heterogeneity-robust DiD estimator and remain remarkably robust to violations of the parallel trends assumption. Appendix Figure D.8 repeats the DiD estimation in the event-study regression (2) to evaluate the empirical plausibility of the parallel trends assumption. Reassuringly, I find no statistically significant difference between treated and control counties before examiners are appointed. Finally, Appendix Figure D.9 and Figure D.10 indicate that the baseline results remain stable when including county-level controls in the estimating equation and when excluding one state at a time from the estimation sample.

V.B Patent Protection and Manufacturing Growth

I complement the analysis of the downstream effects of patents on economic growth by looking at supply-side outcomes from the Census of Manufactures at the city level between 1910 and 1940 (Lafortune et al., 2019). Table IV reports the baseline DiD estimates. As in the previous analysis, I

[18]For the sake of brevity, I report the robustness exercises only on the outcomes which, in the baseline specification, yield statistically significant results. The unreported outcomes yield similarly statistically insignificant results in the respective robustness exercises.

include state-by-year fixed effects to compare cities in the same states by exposure to patent examiners. Panel A reports the reduced-form DiD estimates, while Panel B displays the IV-DiD results. As in the county-level analysis, Figure D.3b reports the positive first-stage correlation between examiners and innovation, and the F-statistic reported in the Table provides reassuring evidence of first-stage instrument relevance.

I construct six indicators of economic activity: the number of establishments (column 1), the number of workers (column 2), the total wage bill (column 3), material costs (column 4), production value (column 5), and value-added as reported in the census returns (column 6). I estimate consistently positive effects of examiners and innovation on all outcomes. Patenting yields a 30% increase in the number of active establishments and 30% higher employment.[19] Total labor costs increase by 36%, indicating a moderate but statistically insignificant increase in the average wage. Expenses related to raw materials increased by 38%. Innovation in response to examiners' appointment triggers a 39% higher production value and 29% increase in value added. The magnitude of these estimates suggests that, as innovation increases in response to the appointment of patent examiners, it diffuses among firms in the exposed cities. As the increase in labor and material costs is comparable, I find no evidence that innovation significantly alters the labor share in manufacturing.

In Appendix Figure D.11, I estimate flexible event-study designs as in (2). I find no statistically significant differences in the various manufacturing outcomes between treated and non-treated cities before the appointment of examiners. This pattern supports the identifying parallel trends assumption, requiring that manufacturing activity in exposed and non-exposed cities would not have diverged if an examiner from exposed cities had not been appointed at the patent office.

I perform a battery of complementary exercises to ascertain the robustness of the results to alternative empirical strategies. Specifically, Appendix Table C.9 shows confidence intervals for the baseline estimates obtained using alternative standard error estimators and finds that statistical significance remains unchanged. Table C.10 reports the estimates obtained using the method described in Cengiz et al. (2019) to account for the staggered roll-out of the treatment across cities, and Table C.11 shows that the estimated treatment effects—particularly on labor costs, material costs, and the value of production—are robust to substantial deviations from the parallel trends assumption. In Appendix Figure D.12, I report the estimated DiD coefficients obtained by controlling for one covariate at a time in the baseline regression and find that the baseline estimate remains remarkably stable. Finally, Figure D.13 repeats the estimation by dropping cities located in one state at a time from the sample and

[19]The increase in manufacturing employment in Table III amounts to 28.9% of the mean. The small difference between this figure and the estimates obtained from the Census of Manufactures may be due to the different observation units.

finds mostly quantitatively similar results.

Taken together, the results presented in this section and Section V.A provide consistent evidence that patenting has a positive and sizable causal effect on manufacturing growth, both in terms of employment growth and supply-side (firm) economic activity. Technological change in this context complements labor, hence an increase in innovation raises the demand for labor and overall employment. Interestingly, increased patenting activity benefits manufacturing but not other sectors. If anything, my results suggest that increased manufacturing activity comes at the expense of agricultural employment, thus indicating that innovation may fuel broader structural change dynamics.

VI THE KNOWLEDGE SPILLOVERS OF PATENT PROTECTION

Section IV documents that newly appointed examiners issue more patents to inventors residing close to their city of origin. This novel fact allowed me to isolate exogenous variation in patenting to estimate, in Section V, the effect of patents on economic growth. How should the increased patenting activity promoted by examiners be interpreted? One possibility is that it is merely a mechanical consequence of examiners' preferences. On the other hand, increased patenting may generate knowledge spillovers that promote the production of genuine innovation. In this section, I evaluate the empirical plausibility of these non-mutually exclusive explanations.

VI.A Examiners and Patenting in Other Divisions

The organizational structure of the USPTO allows me to distinguish between the direct effect of examiners' preferences on patenting activity and the ensuing innovation promoted by increased patent protection. Upon reception, each patent application was assigned to one division depending on its textual content. Importantly, each examiner was in charge of one single division. Therefore, they would have direct jurisdiction over patents assigned to their division but could not affect the acceptance probability in other divisions. I can thus leverage variation across USPTO divisions to disentangle the direct examiner effect and innovation promoted by examiners' appointments.

I hypothesize that examiners are more lenient towards inventors residing in an area close to their city of origin. This location preference would influence patenting activity within their division by increasing the number of patents granted to inventors close to their city of origin. Since examiners did not influence patenting in divisions other than their own, any change in patenting activity in such divisions would reflect the impact of increased patent protection on innovation. Under this hypothesis, the previous analyses reflect the aggregate effect of examiners in terms of both sheer patenting and actual innovation on economic growth.

Since patents do not report their examination division, I apply large language models to infer it from their text. I provide additional details on this approach in Appendix A.II. I use this information to assemble a county-by-division yearly panel. In this panel, a given county c-division d cell is "treated" after an examiner originating from within 100 km of the county c's centroid is appointed in division d. I aggregate the resulting dataset so that, for each treated county-year pair, the dataset reports the total number of patents inside and outside the division of the examiner.[20] I then estimate the baseline regressions (1)–(2) separately on treated and non-treated cells to quantify the effect of examiners on their divisions and other divisions, thereby measuring the direct effect of examiners on patenting and, in turn, the impact of patents on innovation.

Figure IV reports the event-study estimates. The dots displayed in blue (Figure IVa) refer to patents issued in the same divisions of the examiners, whereas those in pink (Figure IVb) display patents issued in other divisions. The difference-in-differences estimates, which include county and year fixed effects to control for time-invariant county-level and time-varying aggregate unobserved heterogeneity, indicate a large and sudden increase in the number of patents in the examiners' and other divisions. Patenting in treated and control county-division pairs is on parallel trends before the examiner is appointed, and increases in both exposed and non-exposed divisions thereafter. However, the patenting uptake in the same examiner division is larger and more immediate. The effect on patenting activity in divisions other than the examiners' is, on average, smaller and takes approximately five years to build up. This difference is consistent with the initial hypothesis. Within their division, the location preference of newly appointed examiners raises the number of patents granted to inventors residing close to their city of origin. However, examiners cannot directly influence the patenting process outside of their division. Hence, in divisions not under the examiner's oversight, the number of patents reflects the innovation response to increased patent protection. The observed relative delay is thus unsurprising because it is plausible that the development of new technologies is not immediate.

Table V reports the mean-shift estimates obtained from model (1). Panel A refers to patenting activity within the same division of the examiner, while Panel B reports the estimates on patenting in other divisions. Column (1) reports the baseline estimates. After a local examiner is appointed, patenting increases by 35% in the same division of the examiner and by 15% in other divisions. The direct examiner effect is thus considerably larger than the aggregated effect documented in Section IV. However, the knowledge spillovers of increased patent protection are economically large and persistent. Distinguishing between treated and non-treated divisions thus allows me to provide explicit evidence that patent protection positively affects *innovation* on top of more generic "patenting" activity. The estimates remain robust when excluding DC and the surrounding states of Maryland and Virginia

[20] For non-treated counties, the dataset returns the yearly number of patents.

(columns 2 and 3). In columns (4) and (5), I split counties by the total number of patents granted. As in the aggregate case, examiners positively affect patenting in innovative areas, but I estimate no statistically significant effect on other counties.

Finally, in columns (6–7), I use the text-based impact measure constructed by Kelly et al. (2021) to focus on the number and share of the most economically relevant patents. This exercise reveals that the aggregate results conceal substantial heterogeneity between the examiners' divisions and the others. Compared to non-treated units, the number of high-impact patents *decreases* in exposed counties after the examiner is appointed (column 6). Because the overall number of high-impact patents decreases and the number of patents increases, the share of high-impact patents decreases (column 7). This finding is rationalizable in a model of taste discrimination where the examiner derives utility from granting patent rights to inventors living close to their home city (e.g. Becker, 1957 [2010]). In such a model, the utility gained from granting patent protection to such inventors implies that the marginal quality of an approved patent to inventors in this group would be lower than that required to issue a patent to inventors outside of it. In this sense, the examiners' behavior would qualify as a "bias." By contrast, in Panel B, I find that the number of high-impact patents granted in divisions other than the examiners' displays a sizable increase both in absolute value (column 6) and as a share of the overall patents (column 7). This pattern indicates that the innovation incentives generated by increased patent protection foster novel and economically relevant innovation activity.

I perform a battery of exercises along the lines of the previous analyses to assess the robustness of these results. Statistical significance is largely preserved when using alternative standard errors estimators, as shown in columns (3–6) of Appendix Table C.3 and when estimating the DiD model using the estimator proposed by Cengiz et al. (2019), as displayed in columns (2–3) of Appendix Table C.4. Using the approach described in Rambachan and Roth (2023), in columns (3–6) of Table C.5, I show that the results are robust to considerable deviations from the parallel trends assumption. Additionally, the baseline estimates are quantitatively unchanged when controlling for time-varying county-level controls, as displayed in Figure D.6b and Figure D.6c, and when excluding one state at a time from the estimation sample, as shown in Figure D.7b–Figure D.7c.

VI.B Examiners and Patenting Across the Technology Space

I conclude this section by evaluating the heterogeneous effects of examiners' appointments across the technology space. Specifically, I ask how the appointment of a patent examiner influences innovation in divisions other than the examiner's division as a function of the technological expertise of the examiner's division. For concreteness, suppose an examiner coming from county c is appointed in division j. Let $D_{-j} = \{d_1, \ldots, d_N\}$ denote the set of divisions other than j arranged such that

$\delta_{d_1,j} \leq \delta_{d_2,j} \leq \cdots \leq \delta_{d_N,j}$, where $\delta_{d_i,j}$ is the technological distance between division d_i and division j.[21] The previous analysis shows that the appointment of the examiner increases the number of patents granted in county c in both division j and $j' \in D_{-j}$. In this section, I evaluate how the increase in patenting within D_{-j} is correlated with the underlying technological similarity between divisions $j' \in D_{-j}$ and j. Specifically, the knowledge spillovers argument implies that the gains in patenting generated by the examiner decrease as $\delta_{d_i,j}$ increases.

To test this intuition, I compute the quintiles of pairwise distance between each division and the division of the newly appointed examiners and aggregate the baseline county-division yearly panel into county-by-quintile pairs observed yearly between 1919 and 1938. I estimate the following variation of the baseline model (1) at the county-by-quintile level:

$$\ln \mathbb{E}\left[y_{idt}\right] = \beta \times I(t \geq Examiner_i) + \sum_{k=1}^{4} \gamma_k \times I(t \geq Examiner_i) \times I(d = k) + \alpha_i + \alpha_d + \alpha_t, \qquad (3)$$

where y_{idt} is the number of patents issued in county i and similarity quintile d and year t, and $I(d = k)$ is an indicator variable equal to one for quintile k. The last quintile—i.e., the pairs where the distance between the examiner's division and the observed division is largest—serves as the baseline category. I estimate the model through Poisson quasi-likelihood, as in the other patent regressions. My hypothesis suggests that the estimated quintile-specific treatment effect is expected to decrease as k increases.

Figure V reports the estimated $\{\hat{\gamma}_k\}$ coefficients. The bars report the point estimates, and the bands overlay 95% confidence bands. As expected, the treatment effect of newly appointed examiners decreases as the technological distance from the examiner's division increases. The estimated effect is approximately 40% larger for the first quintile of distance relative to the fifth quintile and fades thereafter. This pattern is consistent with the hypothesis that the knowledge spillovers generated by newly appointed examiners are largest in divisions that are technologically closer to the division that receives increased patent protection through the examiner's location bias.

Taken together, the evidence strongly suggests that increased local patenting activity resulting from the appointment of new examiners generates significant technology spillovers. Patents thus positively impact the production of novel knowledge. Increased innovation activity is concentrated in divisions that are closer, in the technology space, to the divisions of the examiners, suggesting knowledge spillovers as a plausible underlying mechanism.

[21] In Section II, I explain how I construct the pairwise technological similarity between divisions.

CONCLUSIONS

Patents are the most widespread intellectual property protection institution, but their effects on innovation and growth remain debated and theoretically ambiguous (Boldrin and Levine, 2002, 2013; Lerner, 2002). I introduce a novel stylized fact: examiners at the US patent office grant more patents to inventors residing in areas close to their city of origin. I leverage this observation to isolate exogenous variation in local patenting stemming from the appointment of patent examiners. In a difference-in-differences setting, I show that employment increases in response to increased local patenting. Skilled manufacturing sectors entirely drive the effect and propel structural reallocation of labor into manufacturing. By examining USPTO divisions that are not directly affected by the appointment of new examiners, I show that patent protection generates sizable knowledge spillovers that foster innovation, particularly in sectors technologically closer to the divisions of the examiners.

Methodologically, this paper advances and validates a novel method to estimate the causal effects of patent protection on innovation, an area that has been subject to intense empirical and theoretical debate. From a policy perspective, my findings indicate that patents benefit innovation and broader growth, as predicted by workhorse endogenous growth models. However, they also suggest that the discretion of patent examiners may have costly implications for the quality of patented innovation.

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Table I. Comparison of Treated and Non-Treated Units

	Non-Treated		Treated		Difference	
	Mean	Std. Err.	Mean	Std. Err.	Mean	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Patenting Activity (County-Year)						
Patents	-0.028	(0.003)	-0.038	(0.009)	-0.008	(0.012)
High-Impact Patents	-0.005	(0.002)	-0.048	(0.022)	-0.042*	(0.025)
Share High-Impact Patents	-0.025	(0.005)	-0.024	(0.044)	0.012	(0.048)
Panel B. Population Census Employment (County-Decade)						
Immigrants	-0.027	(0.002)	-0.062	(0.006)	0.002	(0.009)
Internal Immigrants	0.018	(0.002)	0.002	(0.008)	0.002	(0.010)
White-Collar Employment	0.107	(0.001)	0.070	(0.004)	-0.001	(0.006)
Income	0.015	(0.001)	0.002	(0.003)	0.030***	(0.004)
Manufacturing Employment	0.129	(0.004)	0.174	(0.012)	0.024	(0.016)
Durable Manufacturing Empl.	0.061	(0.004)	0.173	(0.016)	0.031	(0.020)
Non-durable Manufacturing Empl.	0.200	(0.004)	0.182	(0.015)	0.025	(0.019)
Panel C. Manufacturing Census (City-Decade)						
Establishments	0.008	(0.013)	0.339	(0.075)	0.217***	(0.083)
Number of Employees	0.104	(0.013)	0.530	(0.061)	0.111*	(0.060)
Labor Costs	0.503	(0.025)	1.162	(0.019)	0.077	(0.076)
Material Costs	2.089	(0.035)	4.409	(0.244)	0.270	(0.165)
Value of Production	0.500	(0.019)	1.149	(0.047)	0.104	(0.074)
Value Added	0.507	(0.023)	1.196	(0.011)	0.092	(0.076)

Notes. This Table compares the growth rate of a selected set of variables in treated and control units before the treatment period (*i.e.*, the appointment of an examiner from a nearby area). In Panel A (resp. B), the observation unit is a county at a yearly (resp. decade) frequency; in Panel C, the observation units are cities at a decade frequency. Panel A reports patenting outcomes; in Panel B, the outcomes are compiled from the population census; in Panel C, the outcomes are compiled from the manufacturing census. All variables are expressed as a growth rate. Columns (1) and (2) (resp. 3 and 4) report the average and the standard error of the mean for each variable in non-treated (resp. treated) units. Columns (5) and (6) report the difference between treated and control units. Each regression includes state and time fixed effects. Standard errors are clustered at the county level in Panels A and B and the city level in Panel C and are shown in parentheses. Referenced on page 11.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table II. Examiners' Appointment and Innovation

	Baseline	Excluding Counties in...		Counties by Total Patents		High-Impact Patents	
	(1)	(2) DC	(3) DC Area	(4) Above 50%	(5) Below 50%	(6) Number	(7) Share
Local Examiner × Post	0.131***	0.131***	0.134***	0.111***	0.024	0.421**	0.423***
	(0.029)	(0.029)	(0.030)	(0.029)	(0.082)	(0.181)	(0.159)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Counties	3,001	3,000	2,861	1,539	1,462	798	798
# Observations	60,020	60,000	57,220	30,780	29,240	15,960	15,960
Mean Dep. Var.	2.992	2.985	3.033	5.528	0.323	0.988	0.050
Std. Dev. Dep. Var.	5.953	5.943	6.005	7.451	0.640	5.668	0.237

Notes. This Table reports the effect of newly appointed examiners on patenting. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents (columns 1–5), the number of patents in the top 20% of the impact distribution (column 6), and the share relative to the total number of patents (column 7). Column (2) excludes Washington, DC; column (3) also excludes Maryland and Virginia; columns (4) and (5) split the sample between counties above and below the median number of patents, respectively. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 100 kilometers. The model is Poisson quasi-maximum likelihood. All regressions include county and year fixed effects. Standard errors are clustered at the county level and are shown in parentheses. Referenced on page 13.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table III. Examiners' Appointment and Employment Growth

	Employment		Manufacturing Employment			Income	Population	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	White Collar	All	Durables	Non Durables		All	Adults
Panel A: Difference-in-Differences Estimates								
Local Examiner $\times$ Post	0.005***	0.001*	0.003***	0.003***	0.000	0.133***	0.020	0.012
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.034)	(0.014)	(0.014)
# Counties	3,095	3,095	3,095	3,095	3,095	3,095	3,095	3,095
# Observations	15,409	15,409	15,409	15,409	15,409	15,409	15,409	15,409
Mean Dep. Var.	0.307	0.039	0.044	0.024	0.020	6.935	9.803	9.315
Std. Dev. Dep. Var.	0.043	0.016	0.043	0.027	0.024	1.587	1.021	1.052
Panel B: IV Difference-in-Differences Estimates								
$\widehat{\text{Patents}}$	0.020***	0.002	0.013***	0.014***	0.001	0.536***	0.081	0.046
	(0.006)	(0.001)	(0.005)	(0.004)	(0.003)	(0.174)	(0.052)	(0.054)
# Counties	3,094	3,094	3,094	3,094	3,094	3,094	3,094	3,094
# Observations	14,718	14,718	14,718	14,718	14,718	14,718	14,718	14,718
K-P F-stat	32.062	32.062	32.062	32.062	32.062	32.062	32.062	32.062
Mean Dep. Var.	0.308	0.040	0.045	0.024	0.020	6.967	9.836	9.348
Std. Dev. Dep. Var.	0.043	0.016	0.044	0.027	0.025	1.597	1.019	1.051
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This Table reports the effect of newly appointed examiners on variables capturing employment growth tabulated from the population census. The observation units are counties at a decade frequency between 1910 and 1950. The dependent variable is employment (column 1), white-collar employment (column 2), aggregate, durable, and non-durable manufacturing employment (columns 3–5), and income (column 6). These outcomes are expressed as shares of the population. In columns (7–8), I report a "placebo" exercise where I employ population and working-age population as the dependent variable to rule out that the estimated treatment effects reflect broader trends in size growth. In Panel A, I estimate a difference-in-differences regression where the treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 50 kilometers. In Panel B, I use the treatment as an instrument for patenting activity in an IV-DiD setting and report the associated Kleibergen-Paap F-statistic. All regressions include county and state-year fixed effects. Standard errors are clustered at the county level and are shown in parentheses. Referenced on pages 16, 18, 19.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table IV. Examiners' Appointment and Manufacturing Growth

	Number of Establishments	Number of Workers	Labor Costs	Material Costs	Value of Production	Value Added
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Difference-in-Differences Estimates						
Local Examiner $\times$ Post	0.269**	0.263***	0.306***	0.321***	0.335***	0.261**
	(0.124)	(0.087)	(0.085)	(0.119)	(0.108)	(0.107)
Panel B: IV Difference-in-Differences Estimates						
$\widehat{\text{Patents}}$	0.348*	0.341**	0.397***	0.416**	0.434***	0.339**
	(0.190)	(0.148)	(0.119)	(0.174)	(0.162)	(0.156)
K-P F-stat	18.045	18.045	18.045	18.045	18.045	18.045
City FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# Cities	99	99	99	99	99	99
# Observations	296	296	296	296	296	296
Mean Dep. Var.	6.104	9.948	16.661	16.792	18.410	17.593
Std. Dev. Dep. Var.	1.058	1.055	1.222	2.333	1.221	1.238

Notes. This Table reports the effect of newly appointed examiners on variables capturing manufacturing growth tabulated from the Census of Manufactures. The observation units are cities at a decade frequency between 1910 and 1940. Not all cities appear in all censuses. The dependent variable is the number of establishments (column 1), the number of workers (column 2), total labor costs, equal to the number of workers times the average wage (column 3), the cost of materials (column 4), total nominal production value (column 5), and the value added (column 6). The data aggregate industry-level entries from the original Census records. In Panel A, I estimate a difference-in-differences regression where the treatment is an indicator variable equal to one in cities exposed to an examiner after the examiner is appointed and zero otherwise. A city is exposed to examiners who are born in that city. In Panel B, I use the treatment as an instrument for patenting activity in an IV-DiD setting and report the associated Kleibergen-Paap F-statistic. All regressions include city and state-year fixed effects. Standard errors are clustered at the city level and are shown in parentheses. Referenced on page 18.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

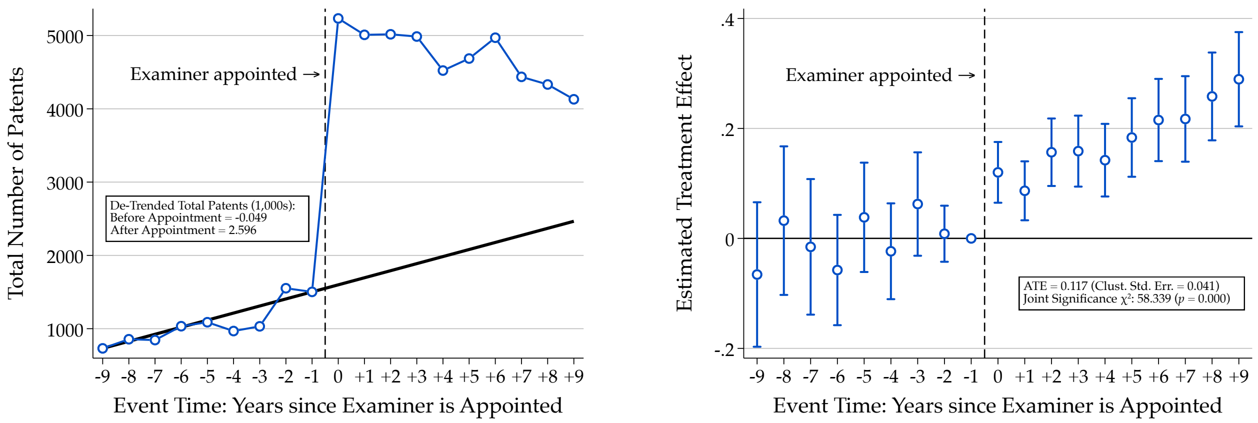
Table V. Examiners' Appointment and Knowledge Spillovers

	Baseline	Excluding Counties in...		Counties by Total Patents		High-Impact Patents	
	(1)	(2) DC	(3) DC Area	(4) Above 50%	(5) Below 50%	(6) Number	(7) Share
Panel A: Patents in the Same Division of the Examiner							
Local Examiner × Post	0.310***	0.310***	0.313***	0.256***	0.305	-0.484*	-0.540*
	(0.050)	(0.050)	(0.053)	(0.050)	(0.451)	(0.289)	(0.287)
# Counties	2,415	2,414	2,326	1,234	1,181	334	334
# Observations	48,300	48,280	46,520	24,680	23,620	6,680	6,680
Mean Dep. Var.	0.816	0.816	0.826	1.417	0.189	0.409	0.073
Std. Dev. Dep. Var.	1.524	1.524	1.536	1.898	0.462	2.351	0.368
Panel B: Patents in Different Divisions of the Examiner							
Local Examiner × Post	0.138***	0.139***	0.143***	0.105***	0.072	0.473***	0.284*
	(0.035)	(0.035)	(0.036)	(0.036)	(0.104)	(0.178)	(0.157)
# Counties	2,889	2,888	2,759	1,510	1,379	555	555
# Observations	57,780	57,760	55,180	30,200	27,580	11,100	11,100
Mean Dep. Var.	1.811	1.801	1.838	3.289	0.193	0.639	0.054
Std. Dev. Dep. Var.	4.709	4.680	4.736	6.136	0.466	3.130	0.274
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This Table reports the effect of newly appointed examiners on patenting. In Panel A, the observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents (columns 1–5), the number of patents in the top 20% of the impact distribution (column 6), and the share relative to the total number of patents (column 7). Column (2) excludes Washington, DC; column (3) also excludes Maryland and Virginia; columns (4) and (5) split the sample between counties above and below the median number of patents, respectively. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 100 kilometers. In Panel A, the outcomes include patents issued in the same division as the newly appointed examiner; in Panel B, the outcomes are constructed from patents in divisions other than the examiner's. The model is Poisson quasi-maximum likelihood. All regressions include county and year fixed effects. Standard errors are clustered at the county level and are shown in parentheses. Referenced on page 21.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

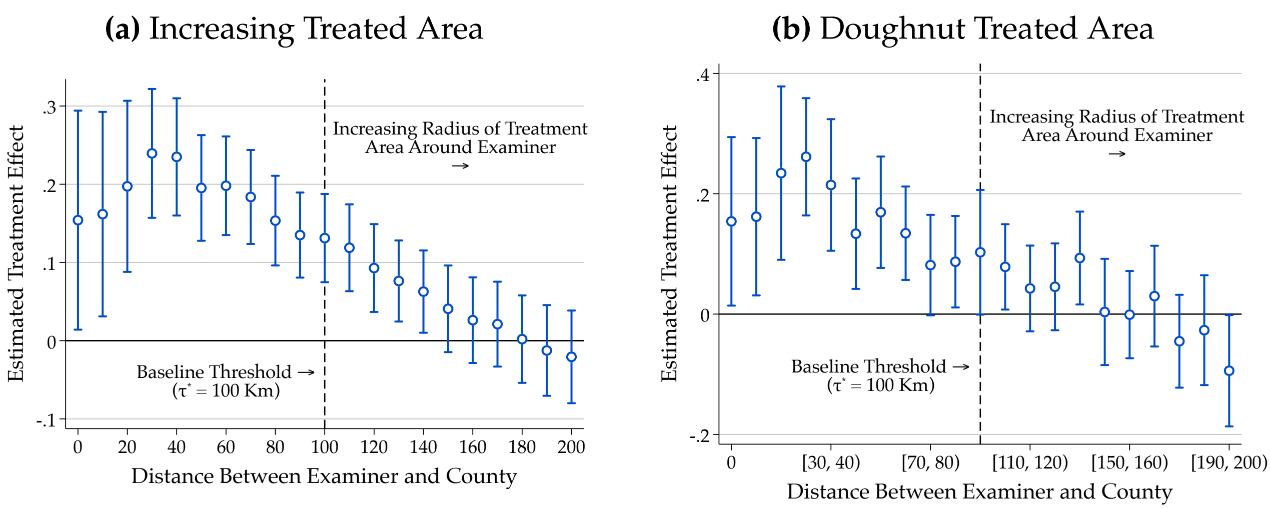
Figure I. Examiners' Appointment and Innovation Over Time

(a) Raw Data **(b)** Difference-in-Differences



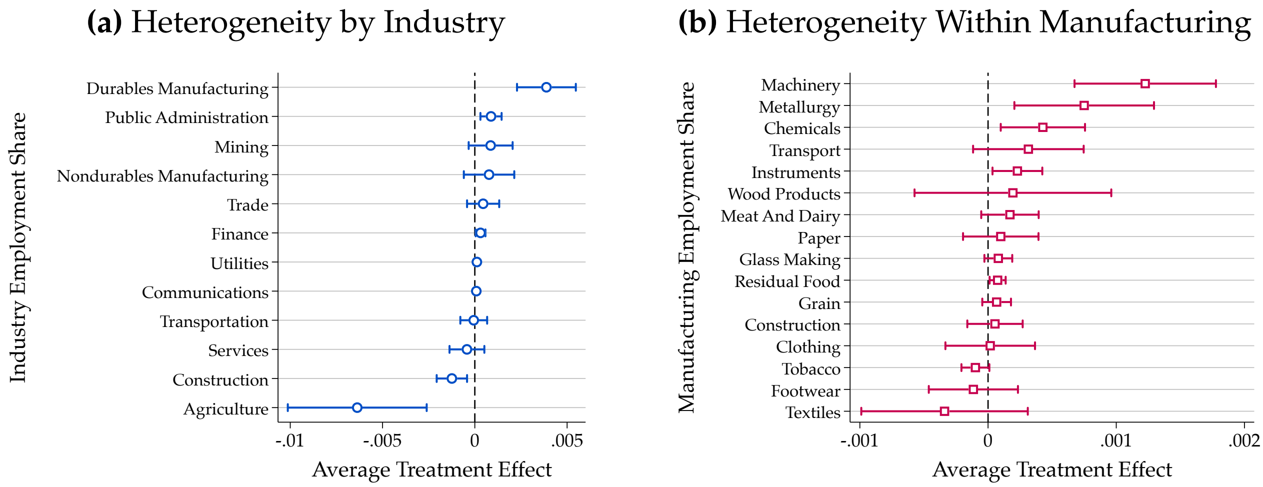
Notes. This Figure reports the effect of the appointment of an examiner on patenting activity in their area of origin. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. In Figure Ia, each dot reports the total number of patents in treated units before and after an examiner from a nearby area (within 100 km) is appointed. The black-dashed line indicates the year when the examiner is appointed. The black line reports a linear fit based on the pre-appointment patenting activity. I report the total number of patents (net of the pre-treatment linear trend). In Figure Ib, each dot reports the estimated coefficient associated with the years since an examiner in the proximity of a county is appointed. A county is exposed to examiners who are born in a county within 100 kilometers. The last period before the examiner is appointed serves as the baseline category. The figure reports tests of joint significance for the post-treatment coefficients. The regression is estimated through Poisson quasi-maximum likelihood and includes county and year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on pages 12, 13.

Figure II. Examiners' Appointment and Innovation Across Space



Notes. This Figure reports the effect of the appointment of an examiner on patenting activity in their area of origin. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. Each dot reports the treatment effect obtained using the baseline difference-in-differences regression model at various thresholds of examiner proximity. Specifically, each dot reports the coefficient of an indicator equal to one after an examiner in the proximity of a county is appointed and zero otherwise. In Figure IIa, for each threshold $\tau \in [0, 200]$, I consider a county exposed to examiners born within τ kilometers of the examiner's origin city. In Figure IIb, for each threshold $\tau \in [0, 200]$, I consider a county exposed to examiners born within τ kilometers of the examiner's origin city, but I exclude all counties closer than $\tau - 20$ kilometers to the examiner's origin city. The treatment area is thus a doughnut centered around the examiner's origin city. The regression is estimated through Poisson quasi-maximum likelihood and includes county and year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on page 15.

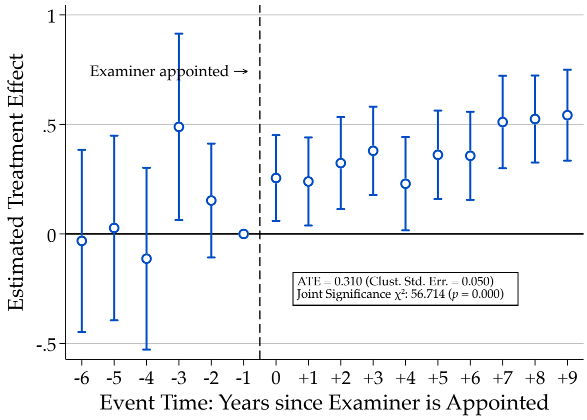
Figure III. Examiners' Appointment and Employment Across Sectors



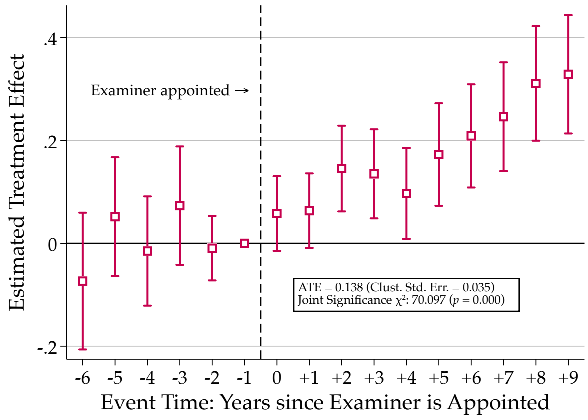
Notes. This Figure reports the effect of the appointment of an examiner on patenting activity in their area of origin. The observation units are counties at a decade frequency between 1910 and 1950. In Figure IIIa, the dependent variable is the (log) employment share by industry; in Figure IIIb, the dependent variable is the employment share by industry within manufacturing. Each dot reports the estimated treatment effect from a difference-in-differences regression where the treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 50 kilometers. The regressions include county and state-by-year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on page 17.

Figure IV. Examiners' Appointment and Innovation Across Divisions Over Time

(a) Examiner's Division

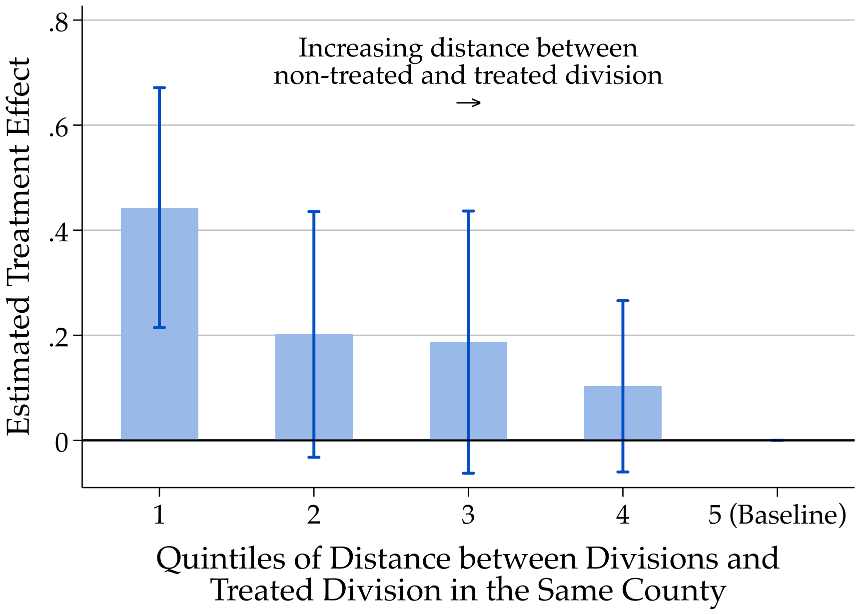


(b) Not Examiner's Division



Notes. This Figure reports the effect of the appointment of an examiner on patenting activity in their area of origin in their division (Figure IVa) and in other divisions (Figure IVb). The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. Each dot reports the estimated coefficient associated with the years since an examiner in the proximity of a county is appointed. A county is exposed to examiners who are born in a county within 100 kilometers. The last period before the examiner is appointed serves as the baseline category. The figures report tests of joint significance for the post-treatment coefficients. Regressions are estimated through Poisson quasi-maximum likelihood and include county and year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on page 21.

Figure V. Examiners' Appointment and Knowledge Spillovers



Notes. This figure reports the spillovers of the effect of the appointment of an examiner on patenting activity in their area of origin across USPTO divisions. The unit of observation is a county at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents in divisions other than the division of the newly appointed examiner. We compute the similarity between each USPTO category i and j as the share of patents in i that are also in j. Then, for each county, we assign the similarity between each division and the division of the newly appointed examiner and aggregate the dataset at the county-quintile level. Each bar then displays the estimated treatment effect of an interaction term between the baseline post-examiner treatment and quintile dummies. The last quintile serves as the baseline category. The regression includes county, similarity quintile, and year fixed effects. All regressions are estimated through Poisson quasi-maximum likelihood. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on page 23.

ONLINE APPENDIX

Gatekeepers of Growth: Patent Examiners, Innovation, and Industrial Growth in the United States, 1919–1938

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July, 2025

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A DATA APPENDIX

This section presents a technical discussion that complements the information provided in the main text on the data sources and the procedure I follow to assemble the analysis sample.

A.I Novel Examiner Data

To assemble the dataset, I collected the name, surname, and USPTO division for all 204 examiners active at the patent office over the period 1919–1938. There are 65 divisions at the patent office in this period. However, divisions 47–65 were established after 1919. I thus exclude them and their patents (approximately 5% of the total) from the sample. This leaves me with 184 examiners. I successfully matched 176 of them (over 96%) to Ancestry data. The matching is performed manually, and, in all cases, I match the records of the "Register" to only one person who, at some point, in the census, indicates that their profession is "Patent Examiner" (or variants thereof). Their city of birth is identified either through census records close to the date of birth of the examiner or through World War 2 enlistment records, which indicate the town of birth. I then assign coordinates to the towns using a commercial geo-coding algorithm (Google Maps API). Table C.1 reports descriptive statistics on the final dataset.

A.II Patent Data

I collected the text of all patents issued in the United States between 1919 and 1938 from Google Patents. This repository also contains the CPC technology class(es) of each patent. I apply large language models to the patent texts to extract the address of residence of the inventors (along with their name and surname, which I do not use). I geo-code the address using Google API to the 1930 county borders. Patent grants indicate the county of residence of the inventor but do not report more disaggregated information. Therefore, I run the analysis at the county level. I also apply large language models to the patent texts to assign each patent to a USPTO division based on the description of each division's coverage, which I extract from the "Classification of subjects of invention," historical manuals for patent examiners. Below, I provide the exact query I used to assign the USPTO divisions.

`gpt-4o-mini` Prompt for Division Imputation

> You are an expert patent examiner tasked with assigning patents to the corresponding technology categories.
>
> Consider the following list of categories: `< List of USPTO Subjects >`.
>
> Below is the text of a US patent.
>
> Your task is to carefully read the patent and, in a JSON object, return a list of all categories that are clearly and directly relevant to the patent. If none of the categories apply with high

> certainty, return an empty list.
>
> Avoid uncertain guesses. Only assign categories if the connection is unambiguous and absolutely clear from the patent text.
>
> Always return the result in the following JSON format: < ``categories'': [ ] >
>
> Text of US Patent: < Text of Patent >

I construct an alternative text-based similarity indicator to validate the baseline division similarity measure. Given the entire corpus of patent texts, I apply basic cleaning routines—remove stop words, lemmatization—and compute the term-frequency inverse-document frequency (TF-IDF) on the resulting dataset. The TF-IDF allows me to represent each patent as a 7,000-dimensional vector where each feature is one weighted word. For each couple of divisions d and d', I then compute the cosine similarity between all patents in d and in d' and take the average as the text-based similarity between the two divisions. Figure D.1 reports the correlation between the text-based and baseline measures.

A.III Data Compiled from the Census of Manufactures

I employ city-level data from the Census of Manufactures at a decade frequency between 1910 and 1940. The census tabulations were digitized by Lafortune et al. (2019).

I harmonize the information contained in the census records into six categories: the number of establishments, the number of workers (i.e., the number of salaried employees), total labor costs (i.e., the average amount spent on wages), material costs (i.e., the average amount spent on fuel, transportation, raw materials, and contract work), the value of production, and the value added. The returns are available at the city-industry level, but given the relatively small sample size, I aggregate them at the city level.

I geo-reference all cities using the Google Maps API to assign them their respective latitude and longitude coordinates. I assign patents to cities using a nearest-neighbor approach: a patent is assigned to the closest city within 20 kilometers of the inventor's residence. The data cover 147 cities scattered across the US territory. However, my empirical design employs state-by-decade fixed effects to compare cities in the same state, thus excluding states with only one city appearing in the Census of Manufactures. Ultimately, the sample comprises 99 cities. Importantly, this is not a balanced panel because not all cities appear in all census waves. Figure D.2b displays the spatial distribution of treated and control cities.

A.IV Data Compiled from the Population Census

From the population censuses 1910 to 1950, I extract information on population, adult population (aged over 18), employment, manufacturing employment ("IND1950" 306 to 499, i.e., "Durables" and "Nondurables"), durable ("IND1950" 306 to 399) and non-durable ("IND1950" 406 to 499) manufacturing employment, white-collar employment ("OCC1950" 0 to 99 and 200 to 290, i.e., "Professionals" and "Managers"), occupational score ("OCCSCORE"), international immigrants and individuals born in another state (whom I label as "internal migrants"). As further outcomes, I compute employment in agriculture and mining ("IND1950" 100 to 239), construction ("IND1950" 246), transportation and communications ("IND1950" 506 to 579), and trade and services employment ("IND1950" 606 to 699), and the number of illiterate individuals. Finally, I compile employment within manufacturing industries according to the standard IPUMS classification.

I apply the method described by Eckert, Gvirtz, Liang and Peters (2020) to cross-walk all these variables to consistent 1930 county borders. The shares of all employment and imputed income variables are computed relative to the adult population. All other variables are relative to the overall population.

I employ the Census Place Project (CPP, Berkes et al., 2023) to assign the variables compiled from the Population Census to cities. The CPP assigns latitude and longitude coordinates to individual census records based on the recorded residence. I employ a nearest-neighbor algorithm to assign each coordinate pair to the closest city within 20 kilometers that appears in the Lafortune et al. (2019) dataset.

A.V Sample Construction

I assemble four datasets: panel "A" is a yearly panel of counties with information on patenting between 1919 and 1938; panel "B" follows counties over the same period but distinguishes between "treated" divisions (i.e., the divisions of the newly appointed examiners from each coutny) and "untreated" divisions (all other divisions); panel "C" contains data from the population census at the decade level between 1900 and 1950; panel "D" is a city-level panel at a decade frequency with information compiled from the Census of Manufactures between 1910 and 1940.

All patenting outcomes at the 5% level to avoid the possibility that outliers drive our estimates. I obtain very similar results without the winsorization. I use the measure developed by Kelly et al. (2021) to identify high-impact patents as those in the top 20% of the novelty distribution of their measure. I obtain similar results using alternative thresholds. In panels "A" (resp. panel "B"), the main treatment variable is equal to one in county c (resp. county c and division d) after an examiner originating from a county closer than 100 Km from c (resp. and active in division d) is appointed.

In panel "C," I adopt the same definition except that the proximity threshold is set at 50 Km, and the examiner must have been appointed in the preceding decade. In panel "D", a city is simply exposed to examiners originating from that city. All manufacturing variables in panels "C" and "D" are winsorized at the top and bottom 1%.

B SUMMARY OF THE ROBUSTNESS CHECKS

1. Are the results sensitive to alternative standard error estimators?

Table C.3, Table C.6, Table C.9	Specification	I employ different clustering strategies and estimate Conley-corrected standard errors to allow for spatial autocorrelation at different cutoffs. Significance remains largely unaffected.

2. Are the results robust to heterogeneity-consistent difference-in-differences estimators?

Table C.4, Table C.7, Table C.10	Specification	I employ the estimator developed by Cengiz et al. (2019) to address the staggered treatment timing issue and find that the results remain unchanged and, if anything, the magnitude slightly increases.

3. Are the results robust to violations of parallel trends?

Table C.5, Table C.8, Table C.11	Identification	I adopt the method developed by Rambachan and Roth (2023) to assess the robustness of the results to violations of the parallel trends assumption. When the baseline results are significant, they also withstand large deviations from parallel trends.

4. Is there an aggregate effect of examiners on patenting?

Figure D.4	Measurement	I compare the number of patents, the number of patents per capita, and the number of examiners from each state, and find a statistically significant positive correlation.

5. Is the instrument a relevant predictor of patenting activity?

Figure D.3	Specification	Besides the F-statistics shown in the main text, I compare patenting and the appointment of examiners at the county- and town-level and find a robustly positive association even when controlling for the same fixed effects included in the main regressions.

6. Are the knowledge spillovers results driven by the division similarity metric?

Figure D.1	Measurement	I apply a document embedding algorithm to patent texts and compute the pairwise similarity between each division using the document embeddings. I compare this metric to the baseline cross-posting measure and find that the two are robustly positively correlated.

7. Is the parallel trends assumption at the county level likely to hold?

Figure D.8	Identification	I estimate event-study regressions and find no statistically significant differences between treated and control counties before examiners are appointed.

8. Is the parallel trends assumption at the city level likely to hold?

Figure D.11	Identification	I estimate event-study regressions and find no statistically significant differences between treated and control cities before examiners are appointed.

9. Are the results robust to controlling for selection on observables?

Figure D.6, Figure D.9, Figure D.12	Confounding Factors	I re-estimate each baseline regression, including one control at a time interacted with a time trend, as well as all controls at the same time, and find quantitatively very similar results.

10. Are the results driven by specific parts of the sample?

Figure D.7, Figure D.10, Figure D.13	Confounding Factors	I re-estimate each baseline regression, dropping one state at a time from the estimation sample, and find that the results remain qualitatively and quantitatively unchanged.

C ADDITIONAL TABLES

Table C.1. Descriptive Statistics on Examiners

	(1) Mean	(2) Std. Dev.	(3) Min.	(4) Max.	(5) Median	(6) Observations
Panel A. All Examiners						
Matched to Ancestry Data	0.962	0.192	0.000	1.000	1.000	184
Birth Year	1881.403	14.872	1844.000	1909.000	1881.000	176
White	1.000	0.000	1.000	1.000	1.000	177
Male	0.994	0.075	0.000	1.000	1.000	177
First-generation Immigrant	0.000	0.000	0.000	0.000	0.000	177
Second-generation Immigrant	0.220	0.415	0.000	1.000	0.000	173
From North-east	0.333	0.473	0.000	1.000	0.000	177
From South	0.090	0.288	0.000	1.000	0.000	177
From Midwest	0.294	0.457	0.000	1.000	0.000	177
From West	0.028	0.166	0.000	1.000	0.000	177
From DC Area (DC, VA, MD)	0.147	0.355	0.000	1.000	0.000	177
Panel B. Geo-coded Examiners						
Tenure (Years)	9.308	7.392	0.000	35.000	8.000	198
Latitude	40.291	2.644	29.324	45.108	40.242	176
Longitude	-80.807	8.750	-120.620	-67.162	-77.069	176

Notes. This table presents key descriptive statistics for the main variables used in the analysis. In panel A, the unit of observation is a county at a yearly level between 1918 and 1939. The panel reports patent and examiner—i.e., treatment—outcomes. In panel B, the unit of observation is a county at a decade frequency between 1910 and 1950. The panel reports the outcome variables constructed from the population censuses in levels and as shares of the population. The variables in levels are expressed in thousand units; the population shares are expressed in percentage terms. In panel C, the table reports the control variables constructed from the 1920 population census, which, in the horse-race regressions, we interact with a post-treatment indicator to assess the robustness of the estimates. These variables are expressed in thousand units. Referenced on pages 6, A2.

Table C.2. Descriptive Statistics

	(1) Mean	(2) Std. Dev.	(3) Min.	(4) Max.	(5) Median	(6) Units	(7) Observations
Panel A. Yearly Panel							
N. of Patents	8.608	65.757	0.000	3052.667	0	3,102	62,040
N. of Breakthrough Patents	0.254	2.907	0.000	168.167	0	3,102	62,040
Share of Breakthrough Patents	0.005	0.034	0.000	0.750	0	3,102	62,040
Year of First Local Examiner	1922.249	5.189	1919.000	1937.000	1,919	862	17,240
Distance to Closest Examiner (Km)	315.417	247.749	0.000	1419.370	240	3,102	46,530
Panel B. County Decade-Level Panel							
Population (1,000)	39.123	132.214	0.037	4528.215	18	3,099	15,417
Employment (Share)	0.307	0.045	0.000	0.750	0	3,099	15,417
Manufacturing Employment (Share)	0.045	0.044	0.000	0.346	0	3,099	15,417
Durable Mfg. Employment (Share)	0.025	0.029	0.000	0.256	0	3,099	15,417
Non-durable Mfg. Employment (Share)	0.020	0.027	0.000	0.288	0	3,099	15,417
White-Collar Employment (Share)	0.040	0.016	0.000	0.149	0	3,099	15,417
Income per Capita	6.939	1.622	0.000	15.063	7	3,099	15,417
Panel C. City Decade-Level Panel							
Establishments	0.114	0.409	0.004	5.903	0	143	382
Workers	4.250	10.747	0.152	144.982	2	143	382
Labor Costs	4232.967	11513.336	0.000	144944.828	1,531	143	382
Material Costs	12797.194	39426.250	0.000	494853.312	3,623	143	382
Production Value	25752.193	73381.070	0.000	932176.750	8,907	143	382
Value Added	11523.255	34045.539	0.000	437323.438	3,854	143	382
Panel D. Cross-Sectional Controls (in 1920)							
Agriculture & Mining	0.830	0.094	0.468	1.000	1	3,094	3,094
Transportation & Utilities	0.022	0.021	0.000	0.189	0	3,094	3,094
Trade & Services	0.047	0.025	0.000	0.184	0	3,094	3,094
Illiteracy Rate	0.296	0.086	0.148	0.683	0	3,094	3,094
Foreign-Born Immigrants	0.071	0.082	0.000	0.540	0	3,094	3,094
Internal Immigrants	0.217	0.169	0.000	0.831	0	3,094	3,094

Notes. This table presents key descriptive statistics for the main variables used in the analysis. In panel A, the unit of observation is a county at a yearly level between 1918 and 1939. The panel reports patent and examiner—i.e., treatment—outcomes. In panel B, the unit of observation is a county at a decade frequency between 1910 and 1950. The panel reports the outcome variables constructed from the population censuses in levels and as shares of the population. The variables in levels are expressed in thousand units; the population shares are expressed in percentage terms. Panel C reports city-level statistics from the Census of Manufactures. In panel D, the table reports the control variables constructed from the 1920 population census, which, in the horse-race regressions, we interact with a post-treatment indicator to assess the robustness of the estimates. These variables are expressed in thousand units. Referenced on page 9.

Table C.3. Alternative Standard Errors: Patent Outcomes

	Patents (Pooled)		Examiner's Division		Not Examiner's Division	
	(1)	(2)	(3)	(4)	(5)	(6)
	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%
No Adjustments	0.074	0.123	0.102	0.174	0.227	0.394
White	0.074	0.123	0.102	0.174	0.227	0.394
Cluster: Unit	0.055	0.142	0.069	0.207	0.213	0.408
Cluster: State	-0.012	0.209	0.037	0.238	0.208	0.413
Conley: 10 Km	0.074	0.123	0.102	0.173	0.308	0.313
Conley: 20 Km	0.081	0.116	0.112	0.163	0.309	0.312
Conley: 50 Km	0.087	0.110	0.122	0.154	0.309	0.312
Conley: 100 Km	0.091	0.106	0.126	0.149	0.310	0.311
Conley: 200 Km	0.093	0.104	0.130	0.146	0.310	0.311

Notes. This Table reports alternative standard errors for the estimated effect of newly appointed examiners on patenting. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 100 kilometers. In columns (1–2), we pool together all patents; in columns (3–4), the outcomes include patents issued in the same division as the newly appointed examiner; in columns (5–6), the outcomes are constructed from patents in divisions other than the examiner's. All regressions include county and year fixed effects. The table reports the 95% confidence intervals for the estimated average treatment effect using different standard error estimators: unadjusted, White adjusted for heteroskedasticity, clustered by counties and state, and adjusting for spatial autocorrelation at various distance cutoffs following Conley (1999). Conley standard errors are estimated using the method described in Bertanha and Moser (2016). Referenced on pages 14, 22, B6.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.4. Alternative Staggered Estimator: Examiners' Appointment and Innovation

	Dependent Variable: Number of Patents		
	(1)	(2)	(3)
	Pooled	Examiner's Division	Not Examiner's Division
Local Examiner × Post	0.197***	0.259***	0.259***
	(0.030)	(0.037)	(0.034)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
# Counties	3,001	2,893	2,893
# Observations	447,920	446,920	4,172,920
R^2	0.674	0.479	0.455
Mean Dep. Var.	2.181	0.976	0.891
Std. Dev. Dep. Var.	4.881	2.271	1.693

Notes. This Table reports the effect of newly appointed examiners on patenting. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 100 kilometers. In column (1), we pool together all patents; in column (2), the outcomes include patents issued in the same division as the newly appointed examiner; in column (3), the outcomes are constructed from patents in divisions other than the examiner's. The model is Poisson quasi-maximum likelihood. The estimates are obtained using the staggered difference-in-differences estimator discussed in Cengiz et al. (2019). All regressions include county and year fixed effects. Standard errors are clustered at the county level and are shown in parentheses. Referenced on pages 14, 22, B6.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.5. Robustness to Violations of Parallel Trends: Patent Outcomes

	Patents (Pooled)		Examiner's Division		Not Examiner's Division	
	(1)	(2)	(3)	(4)	(5)	(6)
$\bar{M}$	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%
Original	0.103	0.220	0.108	0.205	0.204	0.406
0	0.102	0.221	0.108	0.205	0.203	0.407
0.1	0.095	0.228	0.095	0.216	0.147	0.463
0.2	0.085	0.235	0.079	0.230	0.062	0.548
0.3	0.075	0.244	0.058	0.248	-0.033	0.640
0.4	0.063	0.254	0.036	0.269	-0.131	0.741
0.5	0.050	0.265	0.015	0.290	-0.231	0.838
0.6	0.035	0.277	-0.009	0.312	-0.333	0.940
0.7	0.020	0.291	-0.031	0.334	-0.433	1.043
0.8	0.006	0.305	-0.055	0.357	-0.536	1.146
0.9	-0.010	0.319	-0.078	0.381	-0.639	1.249
1.0	-0.025	0.334	-0.102	0.404	-0.743	1.350

Notes. This Table reports the robustness of violations of the parallel trends assumption for the estimated effect of newly appointed examiners on patenting using the method of Rambachan and Roth (2023). The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 100 kilometers. In columns (1–2), we pool together all patents; in columns (3–4), the outcomes include patents issued in the same division as the newly appointed examiner; in columns (5–6), the outcomes are constructed from patents in divisions other than the examiner's. All regressions include county and year fixed effects. The table reports the 95% confidence intervals for the estimated average treatment effect at different cutoffs, and the baseline confidence intervals at the top of the table. Referenced on pages 14, 22, B6.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.6. Alternative Standard Errors: Employment Outcomes

	Employment		Manufacturing Employment		Durable Manufacturing Employment		Income	
	(1) Bottom 5%	(2) Top 95%	(3) Bottom 5%	(4) Top 95%	(5) Bottom 5%	(6) Top 95%	(7) Bottom 5%	(8) Top 95%
No Adjustments	0.002	0.005	0.002	0.005	0.002	0.004	0.006	0.021
White	0.002	0.005	0.002	0.005	0.002	0.004	0.007	0.021
Cluster: Unit	0.002	0.005	0.001	0.005	0.002	0.005	0.005	0.022
Cluster: State	0.000	0.007	-0.000	0.006	0.000	0.006	0.001	0.026
Conley: 10 Km	0.002	0.005	0.002	0.004	0.002	0.004	0.007	0.020
Conley: 20 Km	0.002	0.005	0.002	0.004	0.002	0.004	0.007	0.020
Conley: 50 Km	0.002	0.005	0.001	0.005	0.002	0.005	0.006	0.021
Conley: 100 Km	0.001	0.005	0.001	0.005	0.002	0.005	0.006	0.022
Conley: 200 Km	0.001	0.006	0.001	0.005	0.001	0.005	0.005	0.022

Notes. This Table reports alternative standard errors for the estimated effect of newly appointed examiners on variables capturing employment growth tabulated from the population census. The observation units are counties at a decade frequency between 1910 and 1950. The dependent variable is employment (columns 1–2), aggregate and durable manufacturing employment (columns 3–4 and 5–6), and income (columns 7–8). These outcomes are expressed as shares of the population, and the treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 50 kilometers. All regressions include county and state-by-year fixed effects. The table reports the 95% confidence intervals for the estimated average treatment effect using different standard error estimators: unadjusted, White adjusted for heteroskedasticity, clustered by counties and state, and adjusting for spatial autocorrelation at various distance cutoffs following Conley (1999). Conley standard errors are estimated using the method described in Bertanha and Moser (2016). Referenced on pages 18, B6.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.7. Alternative Staggered Estimator: Examiners' Appointment and Employment Growth

	Employment Rate	Manufacturing Employment Rate	Durable Mfg. Employment Rate	Income per Capita
	(1)	(2)	(3)	(4)
Local Examiner × Post	0.005***	0.003***	0.003***	0.127***
	(0.001)	(0.001)	(0.001)	(0.034)
County FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
# Counties	3,095	3,098	3,098	3,098
# Observations	28,560	54,874	107,490	212,722
R^2	0.794	0.895	0.855	0.890
Mean Dep. Var.	0.307	0.041	0.023	6.818
Std. Dev. Dep. Var.	0.043	0.041	0.026	1.535

Notes. This Table reports the effect of newly appointed examiners on variables capturing employment growth tabulated from the population census. The observation units are counties at a decade frequency between 1910 and 1950. The dependent variable is employment (column 1), aggregate and durable manufacturing employment (columns 2–3), and income (column 4). These outcomes are expressed as shares of the population. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 50 kilometers. The estimates are obtained using the staggered difference-in-differences estimator discussed in Cengiz et al. (2019). All regressions include county and year fixed effects. All regressions include county and state-year fixed effects. Standard errors are clustered at the county level and are shown in parentheses. Referenced on pages 18, B6.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.8. Robustness to Violations of Parallel Trends: Employment Outcomes

	Employment		Manufacturing Employment		Durable Manufacturing Employment		Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\bar{M}$	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%
Original	0.002	0.008	0.001	0.006	0.002	0.005	0.061	0.217
0	0.002	0.008	0.001	0.006	0.002	0.006	0.060	0.218
0.1	0.002	0.008	0.001	0.006	0.002	0.006	0.054	0.229
0.2	0.001	0.008	0.001	0.007	0.001	0.006	0.046	0.240
0.3	0.001	0.009	0.001	0.007	0.001	0.006	0.036	0.253
0.4	0.001	0.009	0.001	0.008	0.001	0.006	0.027	0.267
0.5	0.000	0.010	0.000	0.008	0.001	0.007	0.016	0.282
0.6	-0.000	0.010	-0.000	0.008	0.001	0.007	0.004	0.298
0.7	-0.001	0.011	-0.000	0.009	0.000	0.007	-0.008	0.314
0.8	-0.001	0.012	-0.001	0.009	0.000	0.008	-0.021	0.331
0.9	-0.002	0.012	-0.001	0.010	-0.000	0.008	-0.036	0.346
1.0	-0.002	0.013	-0.002	0.010	-0.000	0.009	-0.049	0.364

Notes. This Table reports the robustness of violations of the parallel trends assumption for the estimated effect of newly appointed examiners on variables capturing employment growth tabulated from the population census using the method of Rambachan and Roth (2023). The observation units are counties at a decade frequency between 1910 and 1950. The dependent variable is employment (columns 1–2), aggregate and durable manufacturing employment (columns 3–4 and 5–6), and income (columns 7–8). These outcomes are expressed as shares of the population, and the treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 50 kilometers. All regressions include county and state-by-year fixed effects. The table reports the 95% confidence intervals for the estimated average treatment effect at different cutoffs, and the baseline confidence intervals at the top of the table. Referenced on pages 18, B6.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.9. Alternative Standard Errors: Manufacturing Outcomes

	Establishments		Workers		Labor Costs		Material Costs		Production Value		Value Added	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%
No Adjustments	0.053	0.484	0.018	0.508	-0.259	0.900	-0.065	0.734	-0.165	0.688	-0.102	0.714
White	0.007	0.530	0.059	0.467	0.040	0.602	0.088	0.582	-0.027	0.549	0.104	0.509
Cluster: Unit	0.026	0.511	0.093	0.433	0.088	0.554	0.123	0.546	0.051	0.471	0.139	0.473
Cluster: State	0.031	0.506	0.159	0.367	0.107	0.535	0.101	0.568	0.031	0.491	0.137	0.476
Conley: 10 Km	0.085	0.452	0.120	0.406	0.124	0.517	0.162	0.508	0.060	0.463	0.164	0.448
Conley: 20 Km	0.082	0.455	0.119	0.407	0.118	0.524	0.141	0.528	0.048	0.475	0.159	0.454
Conley: 50 Km	0.074	0.463	0.101	0.425	0.111	0.531	0.141	0.529	0.034	0.489	0.140	0.473
Conley: 100 Km	0.093	0.444	0.123	0.403	0.094	0.548	0.104	0.565	0.021	0.501	0.121	0.491
Conley: 200 Km	0.086	0.451	0.136	0.390	0.091	0.550	0.098	0.572	0.035	0.488	0.128	0.484

Notes. This Table reports alternative standard errors for the estimated effect of newly appointed examiners on variables capturing manufacturing growth tabulated from the Census of Manufactures. The observation units are cities at a decade frequency between 1910 and 1940. Not all cities appear in all censuses. The dependent variable is the number of establishments (columns 1–2), the number of workers (columns 3–4), total labor costs, equal to the number of workers times the average wage (columns 5–6), the cost of materials (columns 7–8), total nominal production value (columns 9–10), and the value added (columns 11–12). The data aggregate industry-level entries from the original Census records. The model is a difference-in-differences regression where the treatment is an indicator variable equal to one in cities exposed to an examiner after the examiner is appointed and zero otherwise. All regressions include city and state-by-year fixed effects. The table reports the 95% confidence intervals for the estimated average treatment effect using different standard error estimators: unadjusted, White adjusted for heteroskedasticity, clustered by city and state, and adjusting for spatial autocorrelation at various distance cutoffs following Conley (1999). Conley standard errors are estimated using the method described in Bertanha and Moser (2016). Referenced on pages 19, B6.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.10. Alternative Staggered Estimator: Examiners' Appointment and Manufacturing Growth

	Number of Establishments	Number of Workers	Labor Costs	Material Costs	Value of Production	Value Added
	(1)	(2)	(3)	(4)	(5)	(6)
Local Examiner × Post	0.252**	0.251***	0.302***	0.268***	0.306***	0.334***
	(0.120)	(0.071)	(0.084)	(0.102)	(0.118)	(0.108)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
# Cities	110	140	140	140	140	140
# Observations	648	1,388	2,748	21,788	5,468	10,908
Mean Dep. Var.	6.070	9.790	16.500	17.425	16.742	18.261
Std. Dev. Dep. Var.	0.988	1.002	1.176	1.180	2.258	1.174

Notes. This Table reports the effect of newly appointed examiners on variables capturing manufacturing growth tabulated from the Census of Manufactures. The observation units are cities at a decade frequency between 1910 and 1940. Not all cities appear in all censuses. The dependent variable is the number of establishments (column 1), the number of workers (column 2), total labor costs, equal to the number of workers times the average wage (column 3), the cost of materials (column 4), total nominal production value (column 5), and the value added (column 6). The data aggregate industry-level entries from the original Census records. The treatment is an indicator variable equal to one in cities exposed to an examiner after the examiner is appointed and zero otherwise. A city is exposed to examiners who are born in that city. The estimates are obtained using the staggered difference-in-differences estimator discussed in Cengiz et al. (2019). All regressions include city and state-year fixed effects. Standard errors are clustered at the city level and are shown in parentheses. Referenced on pages 19, B6.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

Table C.11. Robustness to Violations of Parallel Trends: Manufacturing Outcomes

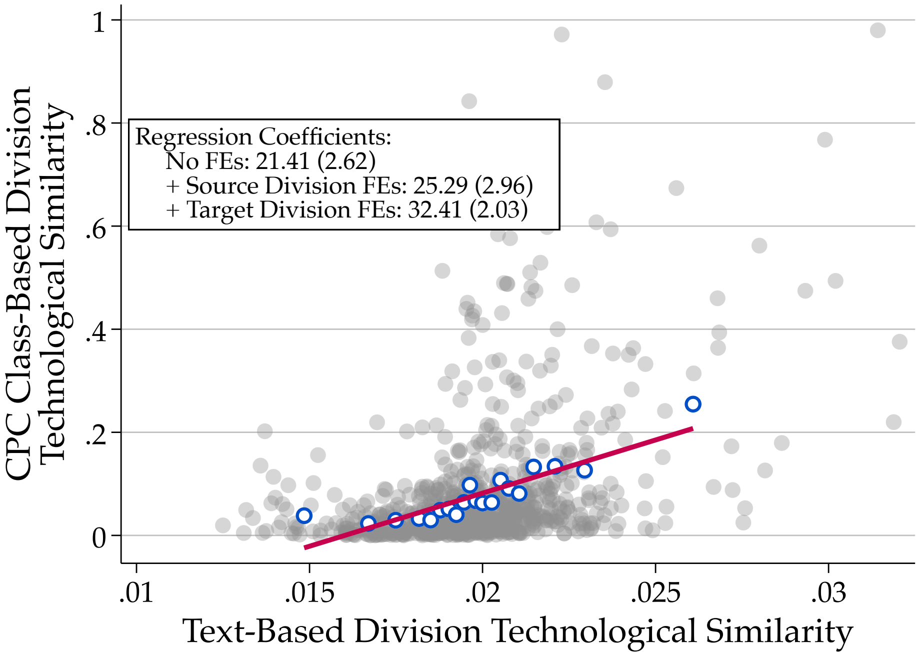
	Establishments		Workers		Labor Costs		Material Costs		Production Value		Value Added	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\bar{M}$	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%	Bottom 5%	Top 95%
Original	0.053	0.665	0.036	0.645	0.128	0.536	0.120	0.760	0.164	0.674	-0.032	0.394
0	0.050	0.668	0.033	0.648	0.125	0.538	0.116	0.764	0.161	0.677	-0.034	0.397
0.1	0.035	0.696	0.018	0.675	0.111	0.556	0.100	0.800	0.159	0.706	-0.045	0.416
0.2	0.026	0.718	-0.003	0.696	0.093	0.579	0.076	0.844	0.151	0.735	-0.060	0.436
0.3	0.010	0.753	-0.024	0.724	0.070	0.602	0.052	0.889	0.149	0.764	-0.080	0.461
0.4	-0.005	0.782	-0.039	0.752	0.038	0.626	0.027	0.935	0.141	0.799	-0.100	0.490
0.5	-0.021	0.818	-0.060	0.780	0.001	0.654	-0.011	0.988	0.134	0.835	-0.130	0.525
0.6	-0.044	0.854	-0.089	0.815	-0.036	0.683	-0.044	1.049	0.115	0.872	-0.160	0.564
0.7	-0.061	0.891	-0.117	0.850	-0.079	0.716	-0.084	1.105	0.085	0.914	-0.196	0.605
0.8	-0.084	0.935	-0.140	0.886	-0.126	0.758	-0.132	1.161	0.054	0.957	-0.232	0.646
0.9	-0.107	0.974	-0.175	0.922	-0.169	0.801	-0.173	1.218	0.023	0.995	-0.268	0.687
1.0	-0.138	1.019	-0.205	0.959	-0.213	0.849	-0.223	1.277	-0.014	1.040	-0.310	0.729

Notes. This Table reports the robustness of violations of the parallel trends assumption for the estimated effect of newly appointed examiners on variables capturing manufacturing growth tabulated from the Census of Manufactures using the method of Rambachan and Roth (2023). The observation units are cities at a decade frequency between 1910 and 1940. Not all cities appear in all censuses. The dependent variable is the number of establishments (columns 1–2), the number of workers (columns 3–4), total labor costs, equal to the number of workers times the average wage (columns 5–6), the cost of materials (columns 7–8), total nominal production value (columns 9–10), and the value added (columns 11–12). The data aggregate industry-level entries from the original Census records. The model is a difference-in-differences regression where the treatment is an indicator variable equal to one in cities exposed to an examiner after the examiner is appointed and zero otherwise. All regressions include city and state-by-year fixed effects. The table reports the 95% confidence intervals for the estimated average treatment effect at different cutoffs, and the baseline confidence intervals at the top of the table. Referenced on pages 19, B6.

***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.10$

D ADDITIONAL FIGURES

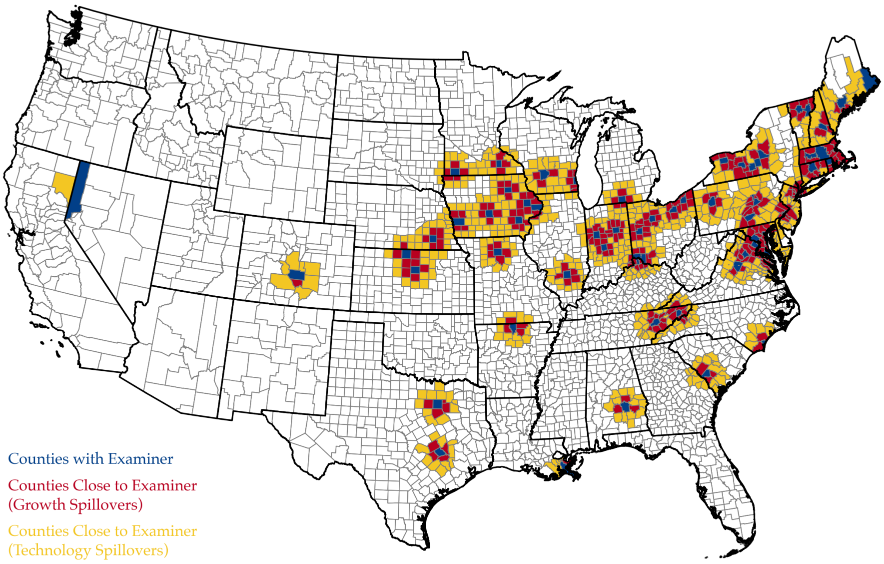
Figure D.1. Correlation Between Text-Based and Class-Based Division Similarities



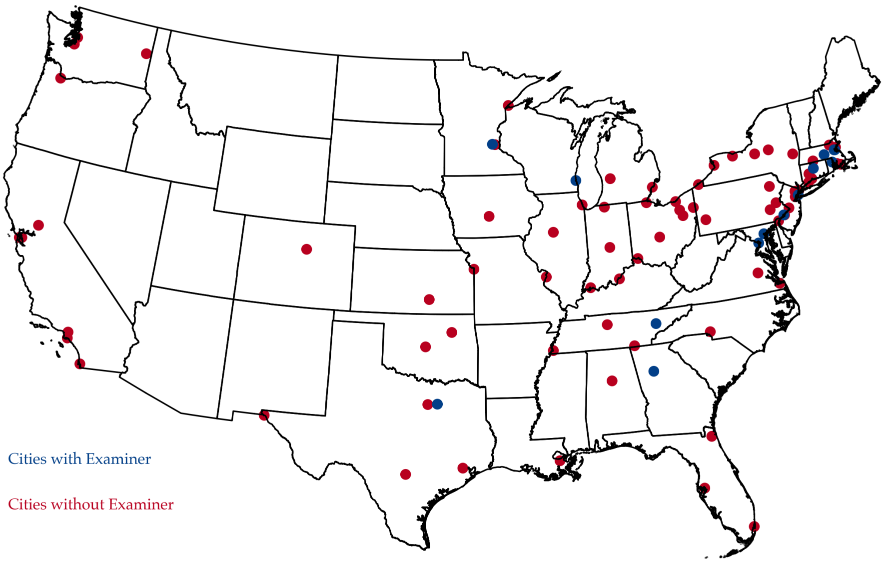
Notes. This figure reports the correlation between the baseline division similarity obtained through CPC technologies (y-axis) and the text-based division similarity obtained from the document embedding model (x-axis). Each gray dot refers to a division-division pair. The figure overlays the binned means in blue and a linear fit in red. In addition, we report the regression coefficient between the two measures without any controls, including fixed effects for the initial (y-axis) division, and including division-division dyadic fixed effects. Referenced on pages 7, A3, B6.

Figure D.2. Geographic Distribution of Examiners' Counties and Cities

(a) Counties



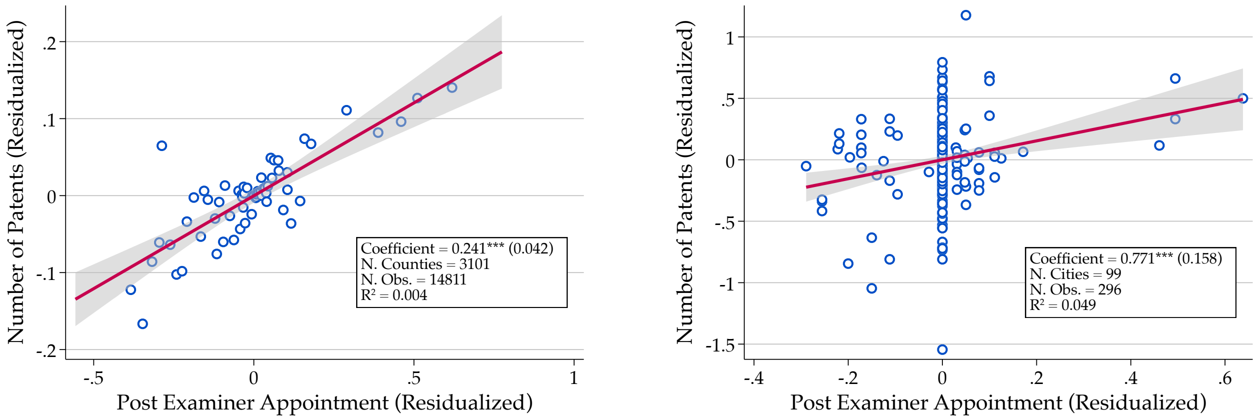
(b) Cities



Notes. These figures report the distribution of treatment and control counties (Figure D.2a) and cities (Figure D.2b) across the various empirical exercises. The counties and cities of origin of at least one principal examiner active at the USPTO between 1919 and 1938 are displayed in blue; counties whose centroid lies within 50 Km from the blue counties are marked in red and are considered treated in the growth analysis; counties whose centroid lies within 100 Km from the blue counties are marked in yellow and are considered treated in the innovation analysis. All other counties and cities are part of the control group. The county borders refer to 1930. The solid black lines superimpose the contemporaneous state borders. Referenced on pages 10, 15, A3.

Figure D.3. First-Stage Instrument Correlations

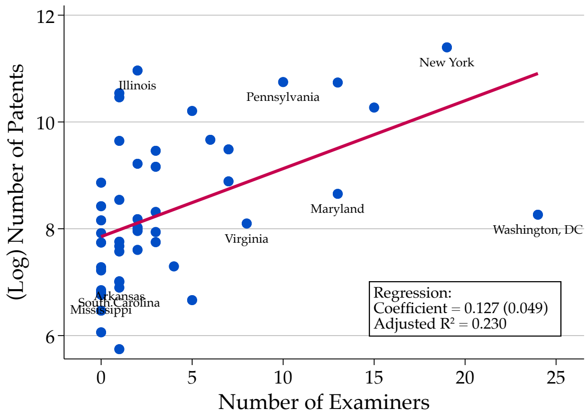
(a) County-Level First Stage **(b)** City-Level First Stage



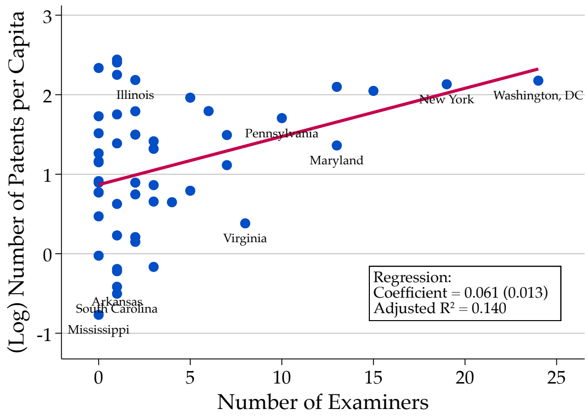
Notes. This figure reports the first-stage correlation between the instrument (the appointment of an examiner) and the number of patents. In Figure D.3a, the observation units are counties at a decade frequency between 1910 and 1950; in Figure D.3b, the observation units are cities at a decade level between 1910 and 1940. The x-axis reports the baseline treatment, i.e., a categorical variable equal to one after the county (Figure D.3a) or the city (Figure D.3b) is exposed to a newly appointed examiner and zero otherwise. The y-axis reports the total number of patents. Both variables are residualized against unit (county or city) and state-by-year fixed effects, which are the controls included in the two-stage least-squares estimates. Figure D.3a is a binned scatter plot. Each graph superimposes a linear fit and the associated 95% confidence bands and reports the regression coefficient along with its clustered standard error, the number of observations, and the adjusted R^2 of the regression. Referenced on pages 11, 16, 18, B6.

Figure D.4. Examiners' Origins and Patenting

(a) Total Patents

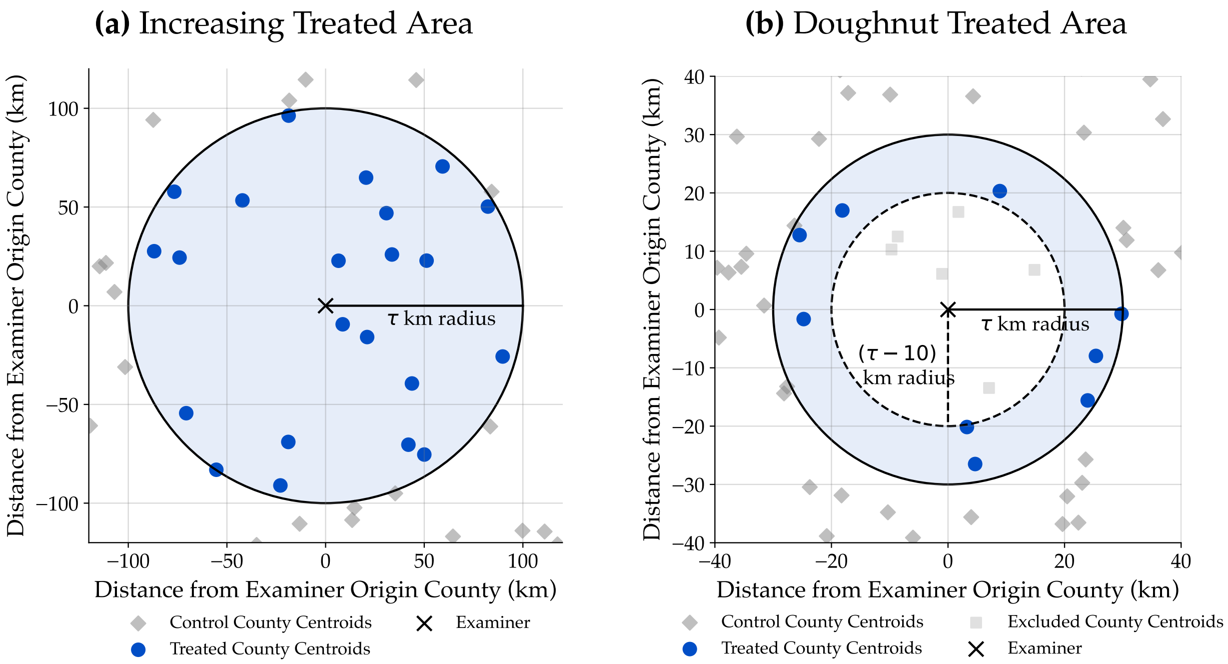


(b) Patents per Capita



Notes. The graphs display the correlation between patenting and the number of USPTO principal examiners. Each dot refers to one state. On the x-axis, we display the number of examiners active between 1919 and 1938 who originated from each state. On the y-axis, Figure D.4a reports the (log) number of patents issued to inventors residing in the given state over the same period; Figure D.4b reports the (log) number of patents issued to inventors residing in the given state over the same period normalized by the average state population. The red line superimposes a linear regression. Each graph reports the coefficient of the associated regression along with its standard error clustered at the state level and the adjusted R^2. The labels highlight the dots of the DC area (Washington, DC, Virginia, and Maryland), the three most innovative states (New York, Illinois, and Pennsylvania), and the three least innovative states outside of the West (Mississippi, Arkansas, and South Carolina). Referenced on pages 12, B6.

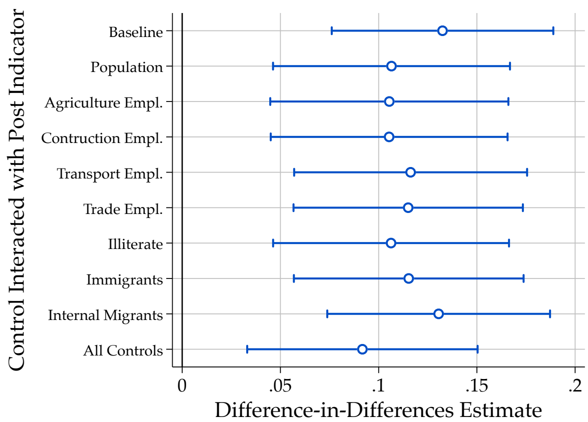
Figure D.5. Visual Representation of the Spatial Spillovers Design



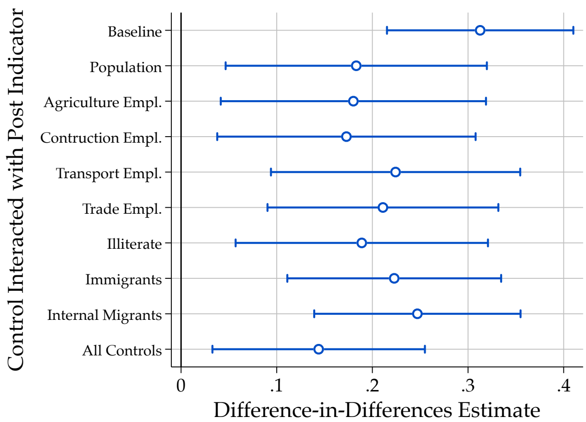
Notes. This figure provides a visual representation of the research design employed to estimate the spatial spillovers of the appointment of new examiners on innovation. The origin city of the examiner is displayed at the center of each picture with a cross; the blue dots are the centroids of counties included in the treated group at the given τ-distance cutoff (in Km); the gray rhombuses are counties in the control group; the light gray squares are counties excluded from the estimation sample. In Figure D.5a, the treatment group comprises all counties whose centroid lies within τ kilometers from the origin city of the examiner, and all other counties enter the control group. In Figure D.5b, the treatment group comprises all counties whose centroid lies in the "doughnut" area within τ and $\tau - 20$ kilometers from the origin city of the examiner: counties closer than $\tau - 20$ are excluded from the sample, and counties further than τ are part of the control group. Referenced on page 15.

Figure D.6. Horse-Race Estimates: Examiners' Appointment and Innovation

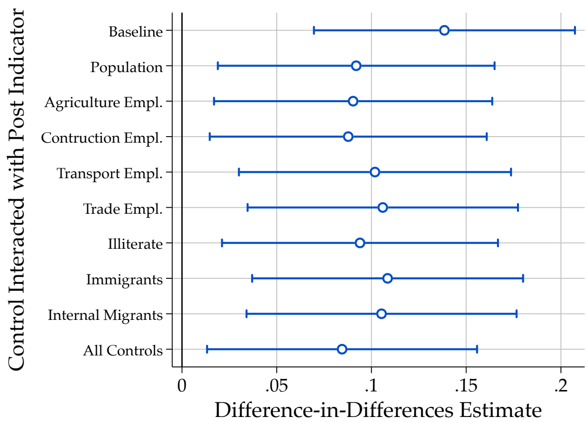
(a) Pooled Patents



(b) Patents in Examiners' Division



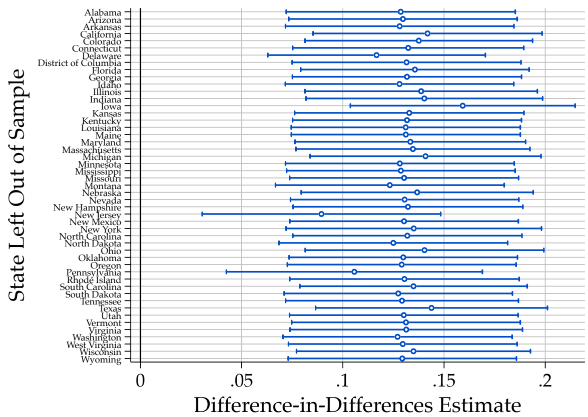
(c) Patents not in Examiners' Division



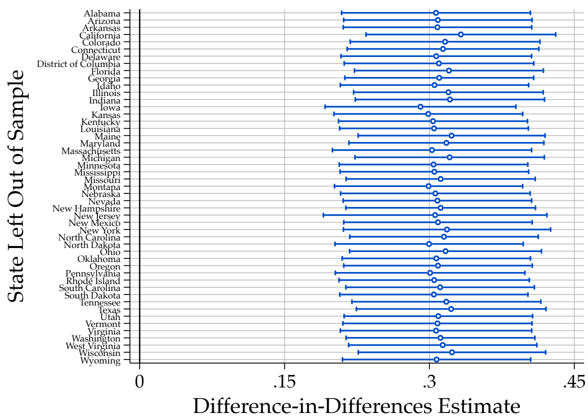
Notes. This figure reports robustness regressions for the estimated effect of newly appointed examiners on patenting. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 100 kilometers. The first dot in each plot reports the baseline average treatment effect; each subsequent dot reports the treatment coefficient where we include one control measured in 1920, interacted with a time trend as an additional control; the last dot reports the treatment coefficient where all controls are included at the same time. In Figure D.6a, we pool together all patents; in Figure D.6b, the outcomes include patents issued in the same division as the newly appointed examiner; in Figure D.6c, the outcomes are constructed from patents in divisions other than the examiner's. All regressions include county and year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on pages 14, 22, B6.

Figure D.7. Leave-Out Estimates: Examiners' Appointment and Innovation

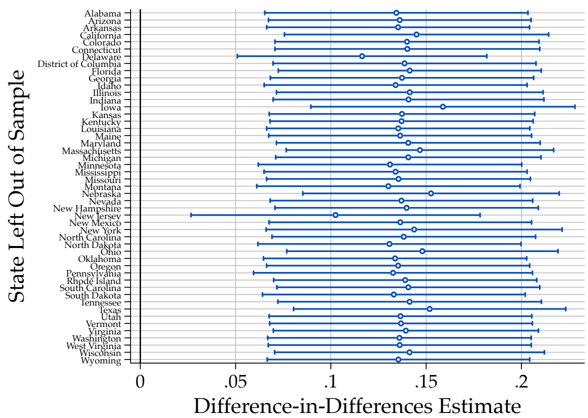
(a) Pooled Patents



(b) Patents in Examiners' Division



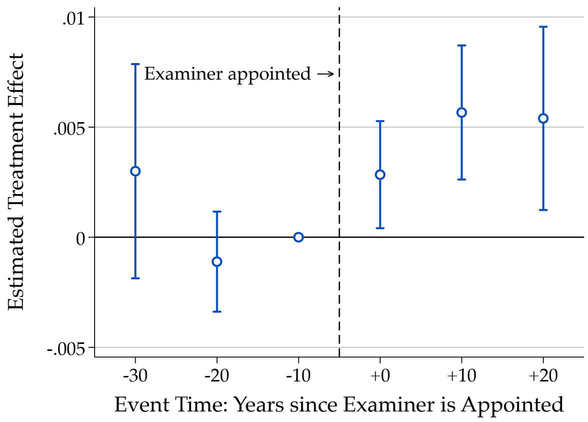
(c) Patents not in Examiners' Division



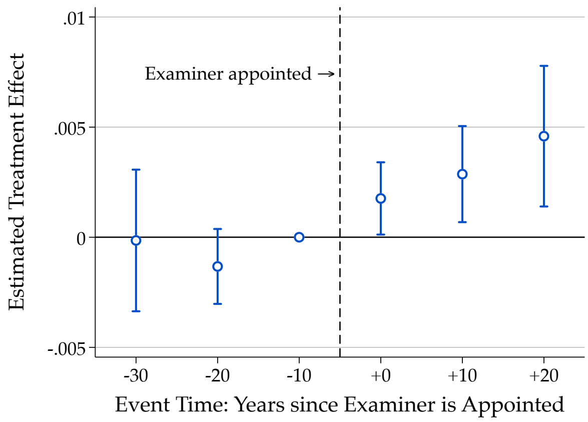
Notes. This figure assesses the stability of the estimated effect of newly appointed examiners on patenting when one state is dropped from the estimation sample at a time. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. The treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 100 kilometers. Each dot reports the treatment coefficient when each state is excluded from the estimation sample. In Figure D.7a, we pool together all patents; in Figure D.7b, the outcomes include patents issued in the same division as the newly appointed examiner; in Figure D.7c, the outcomes are constructed from patents in divisions other than the examiner's. All regressions include county and year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on pages 14, 22, B6.

Figure D.8. Event-Study Estimates: Examiners' Appointment and Employment Growth

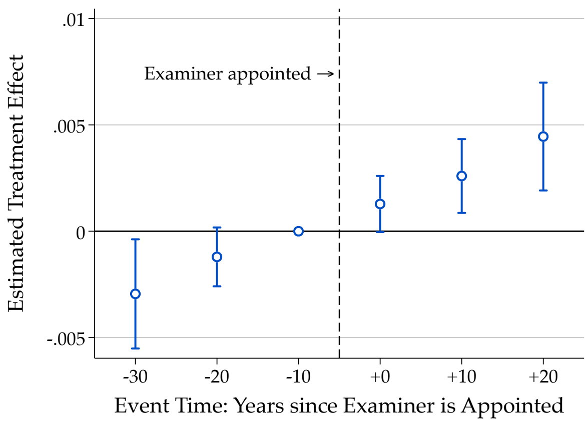
(a) Employment Rate



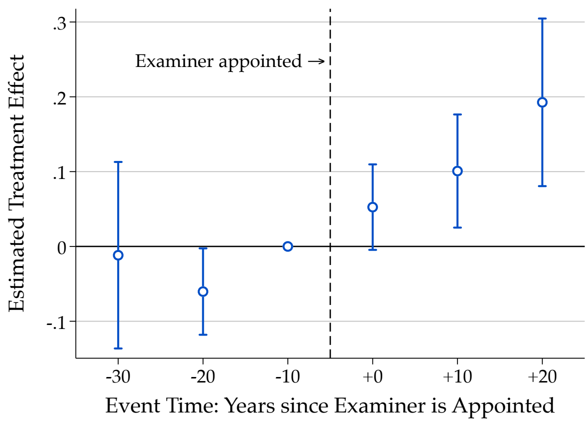
(b) Manufacturing Employment Rate



(c) Durable Manufacturing Employment Rate

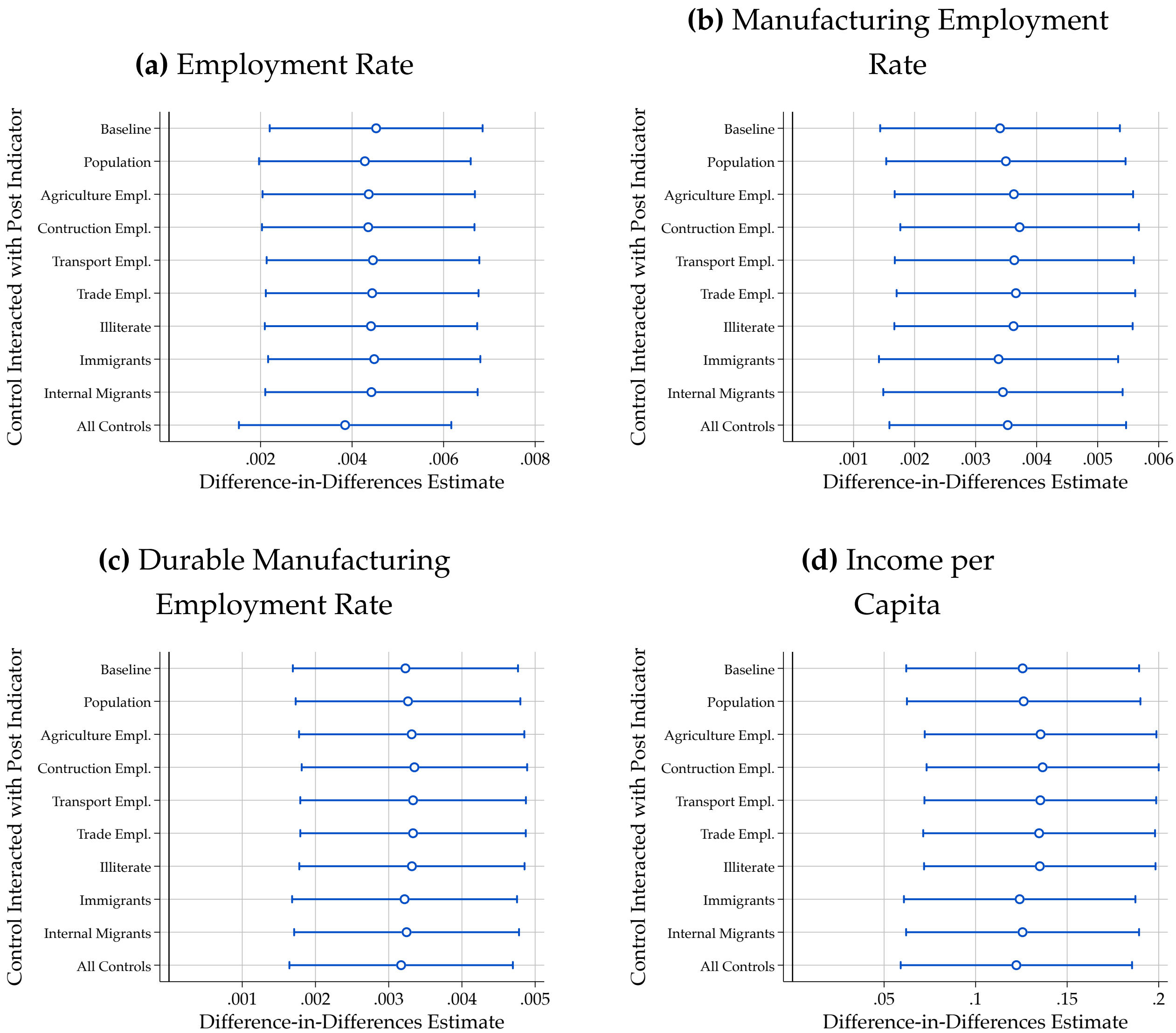


(d) Income per Capita



Notes. This figure reports the effect of newly appointed examiners on variables capturing employment growth tabulated from the population census. The observation units are counties at a decade frequency between 1910 and 1950. The dependent variable is employment (Figure D.8a), aggregate and durable manufacturing employment (panels Figure D.8b–Figure D.8c), and income (Figure D.8d). These outcomes are expressed as shares of the population. Each dot represents the coefficient associated with indicator variables that code the time since the county is exposed to an examiner. A county is exposed to examiners who are born in a county within 50 kilometers. All regressions include county and state-year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. The dashed line marks the treatment period. Referenced on pages 18, B6.

Figure D.9. Horse-Race Estimates: Examiners' Appointment and Employment Growth



(a) Employment Rate

(b) Manufacturing Employment Rate

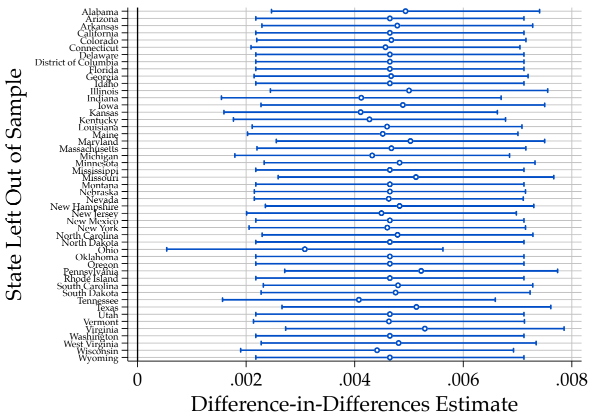
(c) Durable Manufacturing Employment Rate

(d) Income per Capita

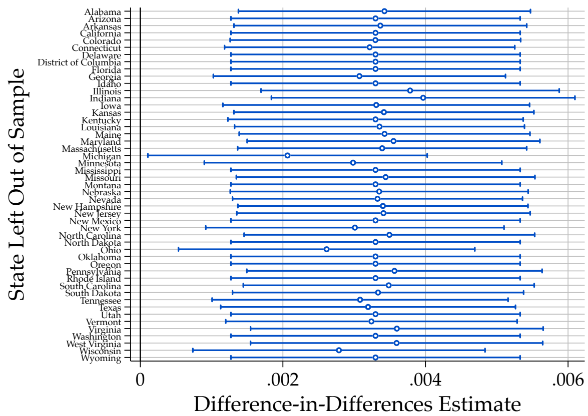
Notes. This figure reports robustness regressions for the estimated effect of newly appointed examiners on variables capturing employment growth tabulated from the population census. The observation units are counties at a decade frequency between 1910 and 1950. The dependent variable is employment (Figure D.9a), aggregate and durable manufacturing employment (panels Figure D.9b and Figure D.9c), and income (Figure D.9d). These outcomes are expressed as shares of the population, and the treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 50 kilometers. The first dot in each plot reports the baseline average treatment effect; each subsequent dot reports the treatment coefficient where we include one control measured in 1920, interacted with a time trend as an additional control; the last dot reports the treatment coefficient where all controls are included at the same time. All regressions include county and state-by-year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on pages 18, B6.

Figure D.10. Leave-Out Estimates: Examiners' Appointment and Employment Growth

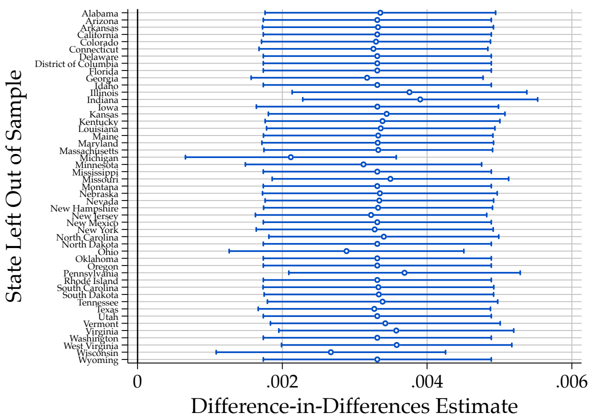
(a) Employment Rate



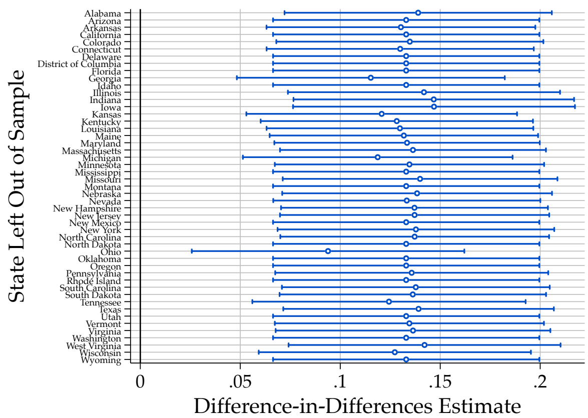
(b) Manufacturing Employment Rate



(c) Durable Manufacturing Employment Rate

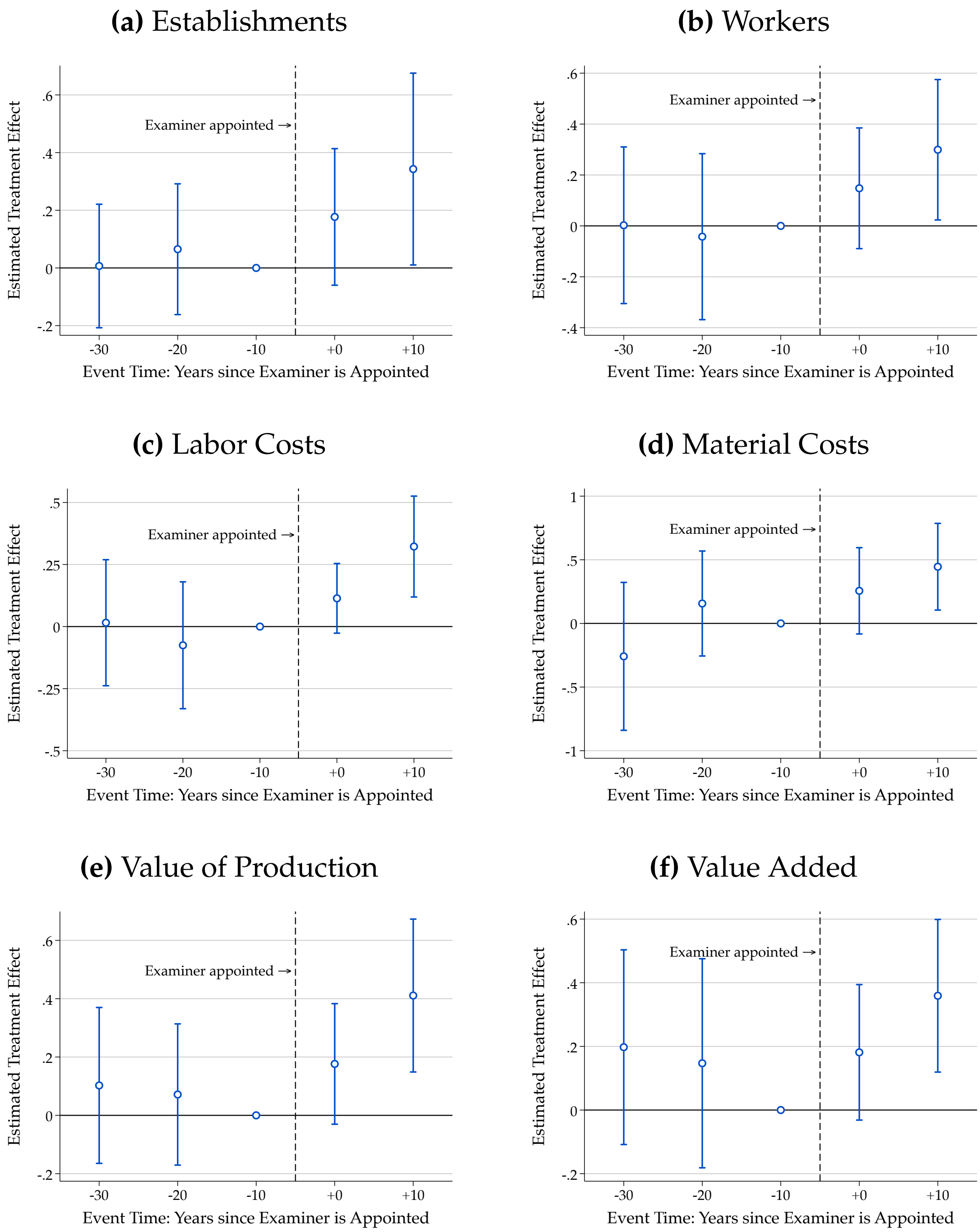


(d) Income per Capita



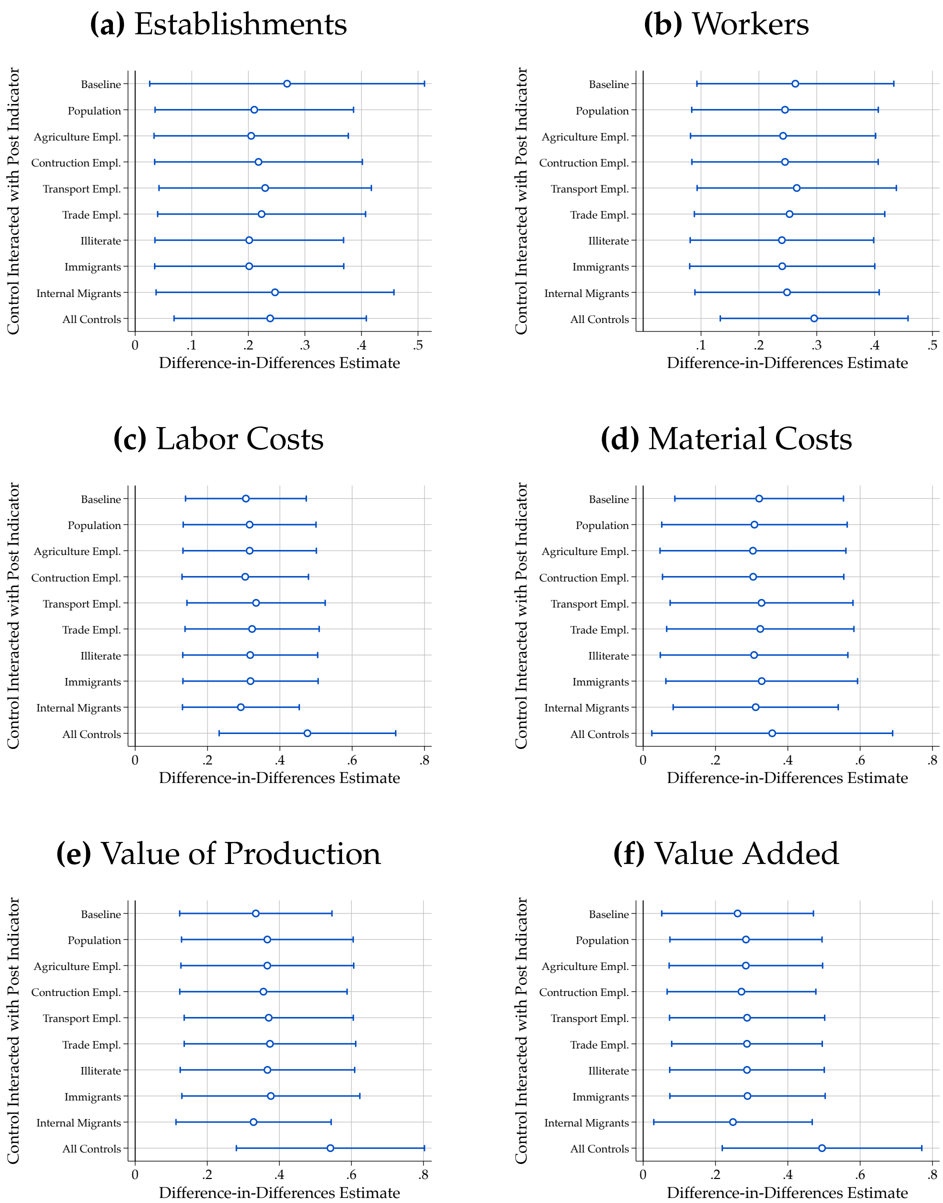
Notes. This figure assesses the stability of the estimated effect of newly appointed examiners on variables capturing employment growth tabulated from the population census when one state is dropped from the estimation sample at a time. The observation units are counties at a decade frequency between 1910 and 1950. The dependent variable is employment (Figure D.10a), aggregate and durable manufacturing employment (panels Figure D.10b and Figure D.10c), and income (Figure D.10d). These outcomes are expressed as shares of the population, and the treatment is an indicator variable equal to one in counties exposed to an examiner after the examiner is appointed and zero otherwise. A county is exposed to examiners who are born in a county within 50 kilometers. Each dot reports the treatment coefficient when each state is excluded from the estimation sample. All regressions include county and state-by-year fixed effects. Standard errors are clustered at the county level; bands report 95% confidence intervals. Referenced on pages 18, B6.

Figure D.11. Event-Study Estimates: Examiners' Appointment and Manufacturing Growth



Notes. This figure reports the effect of newly appointed examiners on variables capturing manufacturing growth tabulated from the Census of Manufactures. The observation units are cities at a decade frequency between 1910 and 1940. Not all cities appear in all censuses. The dependent variable is the number of establishments (Figure D.11a), the number of workers (Figure D.11b), total labor costs, equal to the number of workers times the average wage (Figure D.11c), the cost of materials (Figure D.11d), total nominal production value (Figure D.11e), and the value added (Figure D.11f). The data aggregate industry-level entries from the original Census records. Each dot represents the coefficient associated with indicator variables that code the time since the city is exposed to an examiner. A city is exposed to examiners who are born in that city. All regressions include city and state-year fixed effects. Standard errors are clustered at the city level; bands report 95% confidence intervals. The dashed line marks the treatment period. Referenced on pages 19, B6.

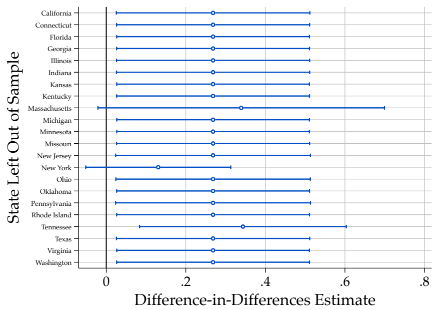
Figure D.12. Horse-Race Estimates: Examiners' Appointment and Manufacturing Growth



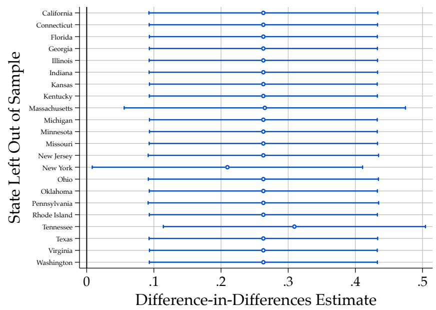
Notes. This figure reports robustness regressions for the estimated effect of newly appointed examiners on variables capturing manufacturing growth tabulated from the Census of Manufactures. The observation units are cities at a decade frequency between 1910 and 1940. Not all cities appear in all censuses. The dependent variable is the number of establishments (Figure D.12a), the number of workers (Figure D.12b), total labor costs, equal to the number of workers times the average wage (Figure D.12c), the cost of materials (Figure D.12d), total nominal production value (Figure D.12e), and the value added (Figure D.12f). The data aggregate industry-level entries from the original Census records. The model is a difference-in-differences regression where the treatment is an indicator variable equal to one in cities exposed to an examiner after the examiner is appointed and zero otherwise. The first dot in each plot reports the baseline average treatment effect; each subsequent dot reports the treatment coefficient where we include one control measured in 1920, interacted with a time trend as an additional control; the last dot reports the treatment coefficient where all controls are included at the same time. All regressions include city and state-by-year fixed effects. Standard errors are clustered at the city level; bands report 95% confidence intervals. Referenced on pages 19, B6.

Figure D.13. Leave-Out Estimates: Examiners' Appointment and Manufacturing Growth

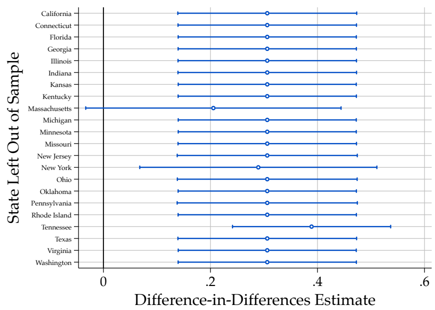
(a) Establishments



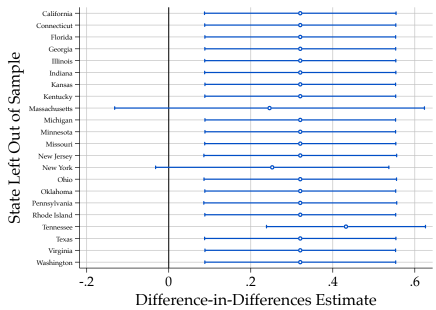
(b) Workers



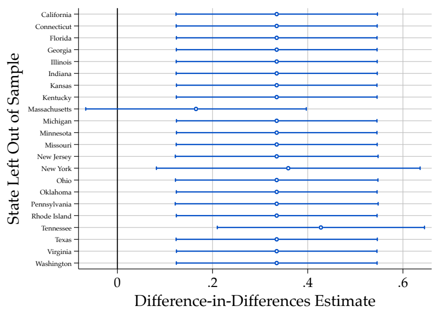
(c) Labor Costs



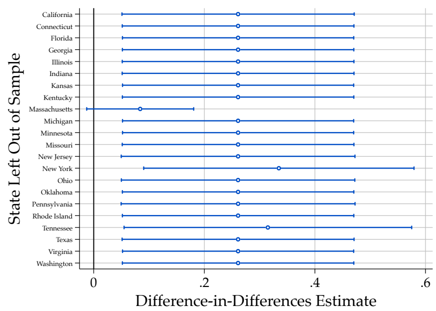
(d) Material Costs



(e) Value of Production



(f) Value Added



Notes. This figure assesses the stability of the estimated effect of newly appointed examiners on variables capturing manufacturing growth tabulated from the Census of Manufactures when one state is dropped from the estimation sample at a time. The observation units are cities at a decade frequency between 1910 and 1940. Not all cities appear in all censuses. The dependent variable is the number of establishments (Figure D.13a), the number of workers (Figure D.13b), total labor costs, equal to the number of workers times the average wage (Figure D.13c), the cost of materials (Figure D.13d), total nominal production value (Figure D.13e), and the value added (Figure D.13f). The data aggregate industry-level entries from the original Census records. The model is a difference-in-differences regression where the treatment is an indicator variable equal to one in cities exposed to an examiner after the examiner is appointed and zero otherwise. Each dot reports the treatment coefficient when each state is excluded from the estimation sample. All regressions include city and state-by-year fixed effects. Standard errors are clustered at the city level; bands report 95% confidence intervals. Referenced on pages 19, B6.

APPENDIX REFERENCES

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