Patents, Innovation, and Growth

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This version: December, 2024

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Abstract

How do patents influence innovation and growth? We leverage newly digitized data on examiners at the US Patent Office between 1919 and 1938 in a difference-in-differences framework. Newly appointed examiners grant 41% more patents to inventors residing close to their origin areas within their division. Patenting in other divisions, which the examiner does not oversee, increases by 10%. This effect is driven by divisions that are technologically closer to the examiner's division, indicating that patent protection presents substantial positive technology spillovers on innovation. We estimate large positive effects of patenting on local economic growth across multiple indicators.

Keywords: Patent Examiners, Growth, Innovation, Patents.

JEL Classification: O31, O34, O40.

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I gratefully acknowledge financial support from the University of Bristol and Northwestern University. Xueyan Li provided excellent research assistance in the construction of the dataset.

I Introduction

Innovation is the core driver of long-run economic growth (Romer, 1990; Aghion and Howitt, 1992). The patent system is often credited among the most relevant and widely adopted institutions fostering innovation.[1] Despite decades of theoretical and empirical research, however, our understanding of the effects of patents on innovation and, more broadly, economic growth remains limited and debated (e.g., see Boldrin and Levine, 2002, 2013; Lerner, 2002; Bryan and Williams, 2021).

This paper contributes to this literature by providing causal evidence on the effects of patents on innovation and growth. The impact of patents on innovation is *ex-ante* ambiguous. On the one hand, patents allow inventors to appropriate part of the social value of their idea, thus reducing the wedge between the private and the social value of innovation. On the other, patents give inventors monopoly power over a nonrival resource—knowledge—and may limit its diffusion. Since the theoretical effect of patents on innovation is not clear, their consequences on economic growth are similarly ambiguous. Ultimately, the answer to both questions needs to be empirical.

Providing causal evidence on the impact of patents on innovation, however, is challenging. Changes in patent laws are seldom exogenous and are often influenced by other policies and countries (Budish, Roin and Williams, 2016). Moreover, they are typically country-wide, making identifying an appropriate "control" group difficult (Moser, 2013). Studying the effect of innovation on growth poses even more daunting obstacles because of reverse causation. This paper advances and validates a new hypothesis to tackle these limitations.

We study the United States Patent (and Trademark) Office (USPTO) between 1919 and 1938. We conjecture that examiners may favor patent applications filed by inventors located close to their areas of origin. By exploiting newly digitized data on the universe of principal examiners active over this period and manually linked to the population census, we test this "location bias" hypothesis. In a difference-in-differences setting, we compare counties close to the area of origin of newly appointed examiners with other counties before and after the examiners are appointed. We estimate a 16% increase in aggregate patenting in treated counties. The effect manifests immediately after the appointment and grows in magnitude over time.

We leverage this novel stylized fact to estimate how shocks to local patenting stemming from the appointment of examiners influence innovation and growth. Because an examiner oversees patent

[1] As of December 2024, nine countries do not have a patent office: Eritrea, Maldives, Marshall Islands, Micronesia, Myanmar, Palau, South Sudan, East Timor, and Somalia. In many Western countries, patent offices date back to the Nineteenth century (Moser, 2019).

applications in a single technological division, or art, of the USPTO, changes in patenting in *other* divisions quantify the presence of technological spillovers from patent protection. We find that patenting in the same division of a newly appointed examiner increases by 41%. In other divisions, where the issuance of patents does not depend on the newly appointed examiner, we document increases of 10%. Importantly, the number of highly novel patents, measured through the text-based indicator developed by Kelly, Papanikolaou, Seru and Taddy (2021), also sharply increases after the examiner is appointed.

We uncover substantial treatment heterogeneity across divisions. Innovation in divisions that are technologically distant from the examiner's division does not display any response to increased patenting activity due to the examiner's appointment. By contrast, the average treatment effect is driven by increased innovation activity in divisions that are technologically closer to the examiner's division. This pattern is consistent with knowledge spillovers generated by increased patent protection as an underlying mechanism. Quantitatively, patenting in divisions in the top 40% of the technological similarity with the division of the examiner increases by almost 20%.

Analogously, we leverage the shocks to local patenting generated by the appointment of examiners to estimate the impact of patents on growth. Implicitly, these estimates conflate the effect of the technology spillovers generated by patent protection mentioned thus far. We document a consistently positive impact of patents on a wide range of proxies for county-level economic activity. The appointment of an examiner generates a 4% increase in population and a 10% increase in overseas and internal immigration. Employment grows by 6%, and we find similar responses in manufacturing and skilled employment. Using a proxy of income based on the occupational structure of counties, we estimate that income increases by 7% in counties close to the area of origin of a newly appointed examiner.

Economic theory predicts that innovation also influences growth at the intensive margin, e.g., output per worker, on top of the output level (Jones, 1995b). We test this proposition and find similarly positive effects of patenting. The appointment of an examiner leads to a 1.3% increase in overseas and internal immigration rates, a 1.2% increase in the employment rate, a 0.5% increase in manufacturing employment, and a 0.2% increase in the skilled employment rate. Partly because of the rise in employment, higher patenting activity and innovation produce a 36% increase in income per worker.

Using a novel identification strategy that leverages plausibly random variation in patenting activity at the local level, the results presented in this paper thus provide strong causal evidence of the positive effects of patents on innovation and economic growth.

Related Literature. This paper adds to three lines of literature. First, we contribute to a large literature on the effects of intellectual property protection, specifically, patents on innovation. This question has received theoretical (e.g., see Nordhaus, 1969; Scherer, 1972) as well as empirical (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). However, as noted by Williams (2017), providing credible causal evidence on the impact of patents on innovation is challenging because patent rights are endogenously determined. We inform this literature by providing causal evidence that patent protection has a positive and large effect on innovation. We show that patents generate sizable spillovers in technologically closer sectors, and we provide suggestive evidence that knowledge spillovers may explain these responses. Methodologically, this paper proposes a novel and operationalizable identification strategy to construct plausibly random variation in patenting activity leveraging the organizational structure of the patent office.

Second, we contribute to the largely theoretical literature on endogenous growth theory (e.g., see Romer, 1986, 1990; Aghion and Howitt, 1992; Mankiw, Romer and Weil, 1992). The ability of inventors to profit from their ideas, which patents supposedly enhance, is a common key feature of these models. Our results thus provide a rather direct test of the proposition that innovation and intellectual property rights foster economic growth. Importantly, we show that innovation influences economic growth in levels as well as in per-capita terms, as predicted by semi-endogenous models of growth (Jones, 1995a).

Third, we add to a smaller literature investigating the implications of the design of intellectual property protection institutions on innovation (Feng and Jaravel, 2020). 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, 2023) and gender (Avivi, 2024) of the inventors. To the best of our 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, which we label "location bias," introduces one novel dimension of bias of patent examiners, the "gatekeepers of quality [of patents]" (Bryan and Williams, 2021).

Outline of the Paper. The rest of the paper is structured as follows. Section II describes the data collection and the construction of the datasets. In section III, we present the empirical strategy and comment on its plausibility. Section IV presents the empirical results of the paper. We conclude in section V.

II Description of the Data

II.A Individual Examiner Data

We construct a novel individual-level dataset of principal examiners active at the patent office between 1919 and 1938.[2] We 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 yearly after that and contains, among others, information on the name, surname, and USPTO division where each examiner was active.

We manually link the examiners' records to genealogical documents provided by `Ancestry.com`. We match individuals by their first and last names and the occupations they list in the census.[3] Moreover, since there was only one USPTO office in Washington, DC, we 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, we map them to their county of birth and use this information to construct county-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. We 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 us to assign granted patents to USPTO divisions and, thus, examiners.

II.B Patents

We collect the universe of patents granted in the United States between 1919 and 1938 from Google Patents. We apply large language models to the full text of the patents to extract the residence address of each inventor, the filing and issue date, and the CPC class and to impute the USPTO division where the patent would be more likely to be examined. As noted by Coluccia and Patacchini (2024), large language models are substantially more flexible than traditional text-mining technologies, allowing us to confidently extract the information for the vast majority (98%) of patents. We geocode the inventors' residences using commercial software to map patents to 1930 counties.

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[2] The sample period is dictated by the historical context and limitations of the underlying data. Examiner data are available starting in 1915 and until 1950. The period 1919–1938 is selected to avoid disruptions arising from the First and Second World Wars.

[3] In all cases, we 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.

Patents vary extensively in terms of their economic and technological impact. We 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 our baseline indicator, we flag high-impact patents as those in the top quintile of the impact distribution.

We implement a simple methodology to measure the technological similarity between USPTO divisions. Each patent is mapped to one division and several CPC technological classes. We 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.[4] In robustness analyses, we validate this methodology using an alternative text-based approach.

II.C County-Level Variables

We construct various proxies of economic growth from the decennial population censuses (Ruggles, Flood, Sobek, Backman, Chen, Cooper, Richards, Rodgers and Schouweiller, 2024). Specifically, we compute population, overall and internal immigration, aggregate, manufacturing, high-skill employment, and a measure of income obtained from the occupational income scores.[5] All variables are computed at the county level between 1900 and 1950. While county borders remain largely stable throughout this period, we adopt the algorithm proposed by Eckert, Gvirtz, Liang and Peters (2020) to map all variables to harmonized 1930 county borders.

II.D Construction of the Datasets

We construct three datasets. Dataset "A" is a yearly panel of counties between 1919 and 1938. It contains information on the number of all and high-impact patents issued in each county-year pair. In addition, we construct a variable that returns the distance between each county's centroid and the closest examiner active each year.[6] Dataset "B" replicates "A" except that the observation unit is a county-division pair. In this case, we construct a variable equal to the distance between the county

[4]Formally, let divisions i and j be represented by vectors $\boldsymbol{d}_i = \{s_{i1}, \ldots, s_{iN}\}$ and $\boldsymbol{d}_j = \left\{s_{j1}, \ldots, s_{jN}\right\}$, 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 $\sigma_{ij} \equiv \left(\boldsymbol{d}_i \cdot \boldsymbol{d}_j\right) / \left(||\boldsymbol{d}_i|| \cdot ||\boldsymbol{d}_j||\right)$. By construction, $\sigma_{ij} \in [0,1]$ and $\sigma_{ii} = 1$.

[5]High-skill employment is constructed as the number of individuals employed as professionals or in managerial positions (i.e., occupational codes 0 to 299).

[6]By closest examiner, we mean the centroid of their county of birth.

and the examiner active in the given division and year for each county-division and year pair. Dataset "C" is a decade-level panel where each county is observed five times between 1900 and 1950. This dataset contains the outcome variables constructed from the federal census. Analogously to the other datasets, for every census year t, it includes a variable that returns the distance between the county and the closest examiner active over the preceding decade $[t, t-10)$.

III Empirical Strategy

This paper investigates two key questions. First, we ask whether patent protection influences innovation. Second, we explore the impact of innovation on economic growth. Answering the first question is challenging because variation in patent laws is scarce and typically country-wide, implying that constructing appropriate "control" groups to estimate their causal impact is often impractical. Answering the second question is, in turn, inherently plagued by reverse causality between innovation and growth.

To circumvent these issues, we exploit quasi-random variation in patenting activity arising from the appointment of patent examiners at the United States Patent Office. Our main hypothesis is that examiners issue relatively more patents to inventors residing in their area of origin. The first step of the empirical analysis is to provide causal evidence in favor of this claim.

Then, we exploit the organizational structure of the USPTO to estimate the impact of patents on innovation. As shown in the first step of the analysis, an examiner grants relatively more patents to inventors residing in "exposed" counties, i.e., those that are geographically close to the area of origin of the examiner. Examiners, however, are responsible for one single division. We thus look at exposed counties and divisions other than the examiner's to estimate how patent protection impacts innovation activity. This analysis thus quantifies the technology spillovers of patent protection.

Finally, we exploit the quasi-random cross-county variation in patenting activity generated by newly appointed examiners to estimate how innovation influences economic growth.

Formally, we estimate variations of the following difference-in-differences specification:

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

where i and t denote county and years or decades. The terms α_i and α_t denote county and time fixed effects. The treatment $I(t \geq \text{Examiner}_i)$ is an indicator variable equal to one after an examiner who is born within k kilometers from the centroid of county i is appointed at the patent office. In the baseline regressions, we set $k = 100$ Km when studying innovation and $k = 50$ Km when looking at the various proxies of growth.[7] The term X_{it} collects a set of county-level controls we include in various robustness regressions. The function $f(\cdot)$ depends on the outcome variable. Since patents are a count variable which exhibits substantial left skewness, we adopt a Poisson quasi-maximum likelihood (PQML) regression.[8] For all other variables, we employ a standard OLS specification.

Identification in this context requires a standard parallel trends assumption, which maintains that the outcomes—patenting and growth—in treated and untreated counties would not have diverged in the absence of the appointment of an examiner originating in proximity to the treated counties. While this assumption is not testable, we estimate a set of fully flexible specifications associated with regression (1):

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

where the terms $I(t - \text{Examiner}_i = k)$ code the periods since an examiner close to county i is appointed. In all cases, we 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 us to evaluate the dynamic treatment effects of new examiners on the outcome variables.

When looking at county-by-division outcomes (i.e., dataset "B"), the treatment $I(t \geq \text{Examiner}_{id})$ is equal to one after an examiner whose county of origin is within 100 kilometers from county i is appointed in division d and zero otherwise. When studying decade-level outcomes (i.e., dataset "C"), since the granularity of the examiner treatment is coarser, we include state-by-decade fixed effects to remove time-varying state-level factors that may influence patenting activity.

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, we thus adapt the stacked difference-in-differences estimator proposed by Cengiz, Dube, Lindner and Zipperer (2019) to the Poisson specification and find consistent and quantitatively very similar results to the baseline.

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

[8] 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 us to work with zeros without imposing arbitrary log-transformations (Chen and Roth, 2024).

IV Results

IV.A Examiners and Local Patenting

The starting point of the analysis is the hypothesis that examiners may favor patent applicants from their areas of origin. This "location bias" would generate increased observed patenting activity in those areas. We test this hypothesis explicitly by estimating regression (2) using the county-level patent count as the outcome variable. Figure I displays the results.

We estimate statistically insignificant coefficients before the examiner is appointed. This pattern supports the underlying identifying parallel trends assumption. To provide more formal evidence in this direction, the figure reports a test of the joint significance of the pre-and post-treatment coefficients, which rejects the possibility that the former is statistically different from zero. We estimate an immediate 10% increase in the number of patents issued in areas exposed to the examiner.[9] The treatment effect of examiners increases over time, and by the end of the estimation window, counties exposed to an examiner produce 34% more patents than counties without one. A formal test indicates that the post-treatment coefficients are highly jointly statistically significantly different from zero.

To further validate our starting hypothesis, we examine how the examiners' "location bias" varies across space. Our research design is motivated by the idea that examiners favor inventors originating from areas closer to their origin place. Figure II evaluates this prediction. We estimate a set of regressions analogous to (1), except that we allow for different proximity thresholds k, which determine whether a county is exposed to an examiner. We use 20 thresholds between 0 and 200 kilometers. The pattern of the estimated treatment effects strongly supports the "location bias" hypothesis. The treatment effect is largest for low levels of the proximity threshold below 100 Km and decreases to zero when counties further than 140 Km are included in the treatment pool. This figure also motivates our choice for the baseline exposure thresholds. We set 100 Km as the threshold for the innovation analysis because the estimated treatment effects are stable around this value, which allows us to enlarge the set of treated units. We set 50 Km as the threshold for the growth analysis because treatment effects are larger at this proximity threshold. Since we only observe output data at the decade level, further inflating the treatment definition artificially dilutes the effect of patenting on growth.

Panel A in table I presents the estimates associated with regression (1). Examiners, on average,

[9] 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 $\ln \mathbb{E}[y_{it} \,|\, X_{it}, \alpha_i, \alpha_t; I(t \geq \text{Examiner}_i) = 1] \equiv \ln \mathbb{E}[y \,|\, 1] = \hat{\beta}$ and $\ln \mathbb{E}[y_{it} \,|\, X_{it}, \alpha_i, \alpha_t; I(t \geq \text{Examiner}_i) = 0] \equiv \mathbb{E}[y \,|\, 0] = 0$. Hence, $\ln \mathbb{E}[y \,|\, 1] - \ln \mathbb{E}[y \,|\, 0] = \hat{\beta}$, $\mathbb{E}[y \,|\, 1] = e^{\hat{\beta}} \mathbb{E}[y \,|\, 0]$, and, therefore, the percentage change associated with the treatment activation is $(\mathbb{E}[y \,|\, 1] - \mathbb{E}[y \,|\, 0]) / \mathbb{E}[y \,|\, 0] \times 100 = (e^{\hat{\beta}} - 1) \times 100$.

generate a 16% increase in patenting in the counties within 100 Km of their county of origin (column 1). This effect is large and statistically significant even if we include state-by-year fixed effects (column 2). A plausible concern is that, since almost 15% of the examiners are born in Washington, DC, where the patent office is located, and in the neighboring states of Virginia and Maryland, these areas inflate the estimated treatment effect. In columns (3) and (4), we ensure this is not the case by dropping DC and its neighboring states, respectively. Innovation exhibits substantial geographic concentration. A natural question is, therefore, whether examiners can produce innovation clusters or if they foster innovation in already innovative areas. In columns (5) and (6), we split the sample into counties above and below the total median number of patents. The estimated response to new examiners is entirely driven by above-median innovation counties, indicating that newly appointed examiners foster innovation in already innovative areas. Finally, we explore the effect of examiners on the production of high-impact patents according to the text-based measure defined by Kelly *et al.* (2021). In column (7), we show that high-impact patents—i.e., those in the top quintile of the distribution of "impact"—increase by almost 60% in counties exposed to examiners. Similarly, the share of high-impact patents relative to the total number of patents increases by 55% (column 8).

Our results indicate that examiners promote patenting activity in their areas of origin. This stylized fact, which, to the best of our knowledge, has not been documented before, constitutes a first result. In the rest of the paper, we leverage this "location bias" pattern as a shock to local patenting and use it to study how patenting influences the production of novel knowledge and economic growth.

IV.B Patents and Innovation: Technology Spillovers of Patent Examiners

An important feature of the organization of the patent office throughout our analysis period is that only one principal examiner headed each division. We can thus exploit information on the division of each patent to study the heterogeneous responses to the appointment of examiners across divisions.

More specifically, we expect examiners to increase patenting activity within their division. Hence, patenting in the same division of the newly appointed examiners is not entirely informative about innovation. Ideally, however, examiners should not impact patents in other divisions because they have no jurisdiction over their acceptance. We thus interpret any spillover effects of examiners onto patenting outside of their division as evidence that patenting activity influences *innovation.*

In table I, we distinguish between patents in the same division of the newly appointed examiner (panel B) and patents in other divisions (panel C). Perhaps unsurprisingly, given the results described in the previous section, patenting in the same division of the examiners increases (panel B). Quantitatively, the increase is considerably larger than for overall patenting: counties exposed to an examiner produce

41% more patents in the same division as the newly appointed examiners, compared to a 16% increase in overall patenting. We interpret this difference as a sanity check, for it is plausible that the impact of the examiners' "location bias" is larger in their division.

Importantly, however, patenting also increases in divisions outside the examiner's pertinence (panel C). Quantitatively, the number of patents issued in divisions other than the newly appointed examiner's increases by 10% in exposed counties (column 1). This result holds upon including state-year fixed effects (column 2) and excluding DC and the surrounding states (columns 3–4). As in the previous section, innovative counties drive the effect (column 5), while we detect no response in relatively less innovative areas (column 6).

Interestingly, while examiners issue more patents in their division to inventors in their areas of origin, we find no effect on the number of high-impact patents (columns 7–8 of panel B). In this sense, it appears plausible that the "location bias" effect motivates examiners to "lower the bar" and issue relatively less important patents when the inventor resides in their areas of origin. By contrast, we estimate a large increase—60%—in the number of high-impact patents in divisions other than the examiner's (panel C, columns 7–8).

In Figure IIIa, we estimate regression (2) separately for patents in the same division of the examiner (in blue) and in other divisions (in red). In both cases, we find no evidence of statistically significant pre-treatment coefficients, once more supporting the parallel trends assumption. We estimate statistically significant and positive effects of examiner appointments on patenting within and outside the examiner's division. The treatment effects are larger on patenting in the same division of the examiner in all periods. However, patenting outside of that division also increases, and the effect grows over time. We interpret these patterns as further evidence that (i) examiners exhibit "location bias" in granting more patents in their division, and (ii) increased patent protection generates technology spillovers onto divisions other than the examiners'.

We conclude by exploring how the impact of examiners on patenting varies across the distribution of technology similarity with the division of the examiner. This exercise concentrates on patents issued in divisions other than the examiner's. As in the previous analysis, let the treatment variable (Examiner_i) be the year when an examiner from a county closer than 100 Km from county i is appointed. Let $\bar{d}$ be the division of that examiner. We estimate the following specification for patents in all divisions except $\bar{d}$:

$$\ln \mathbb{E}[y_{idt} \,|\, X_{it}] = \sum_{k=1}^{5} \beta_k \times I(t \geq \text{Examiner}_i) \times I(\xi_{id} = k) + \alpha_i + \alpha_\xi + \alpha_t, \tag{3}$$

where y_{idt} is the number of patents issued in county i and division d, the term ξ_{id} denotes the quintile of similarity between division d and the division $\bar{d}$ of the examiner to whom county i is exposed, and

$I(\xi_{id} = k)$ is an indicator variable equal to one when the similarity is in quintile k.

The terms $\{\beta_k\}_{k=1}^{5}$ quantify how the effect of examiners on local patenting varies depending on the similarity between each division and the division of the examiners. We interpret the effect of examiners on patenting in divisions other than their own as evidence of technology spillovers of patenting on innovation. This interpretation suggests that the effect of examiners should be smaller on patenting in technologically distant divisions (i.e., $k = 1, 2$) and larger for closer divisions (i.e., $k = 4, 5$). We evaluate these predictions in panel IIIb. The figure displays the point estimates and the standard errors of the estimates of the β_k of regression (3). The estimates confirm our interpretation. We find that the effect of examiners on divisions that are very distant from their own is statistically zero. By contrast, examiners exert a large, positive, and statistically significant effect on divisions that are technologically closer to their own.

Our results indicate that the increased local patenting activity generated by the appointment of new examiners presents large technology spillovers. In this sense, we document that patents 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.

IV.C Innovation and Growth

In the final part of the analysis, we leverage the appointments of examiners and the "location bias" stylized fact to investigate the impact of innovation on economic growth. Endogenous growth theory identifies innovation as the premier determinant of long-run growth prospects (Romer, 1990; Aghion and Howitt, 1992). Testing this proposition, however, is challenging because growth and innovation feed back into each other, thus generating issues of reverse causation. Our research design provides a clean solution to this empirical problem.

Unfortunately, historical measures of GDP and productivity at the county level are unavailable. The census did not collect systematic wage and income information until 1940. We thus follow the standard practice in the US economic history literature and examine various imperfect proxies constructed from the population census (e.g., Abramitzky, Ager, Boustan, Cohen and Hansen, 2023). More specifically, we construct county-level population, immigration from outside and inside the United States, overall, manufacturing, and skilled employment, and adopt the occupational income score provided by IPUMS as an imperfect income proxy. These variables are available at the decade level between 1900 and 1950.

Panel A in table II displays the impact of local examiners on the (log) level of these variables. Counties exposed to an examiner display, in the following years, a 4% increase in population, a 10% increase in overseas immigration, and an equivalent increase in the number of internal migrants. Overall, manufacturing and skilled employment increased by approximately five percentage points. Partly because of the increased labor force participation, overall income increases by 7%.

A 5% employment increase roughly corresponds to 600 more workers over a ten-year window. The estimated treatment effect associated with a 50 Km proximity threshold in figure II indicates that, over the same period, inventors in exposed counties would obtain approximately ten more patents issued because of the appointment of a local examiner. Indicatively, our estimates thus translate one additional patent into an employment gain of about 60 units.

Importantly, endogenous growth theory predicts that innovation should not only increase the *level* of income but also sustain its *per capita* growth (Jones, 1995a). We test this prediction in panel B of table II. Here, the outcome variables are normalized by population (columns 2–3) and adult population (columns 4–7). Notwithstanding data constraints, any response of these variables to the appointment of local examiners thus quantifies the impact of innovation on the economy's growth rate. We find consistently positive treatment effects of examiners on all per-capita indicators of economic activity. The appointment of a local examiner leads to a 1.3% increase in the rate of international and internal immigrants, a 1.2% increase in the employment rate, and a 0.2% increase in the employment share in highly skilled occupations.

These results provide consistent evidence that innovation has a positive, *causal* effect on economic growth. As predicted by semi-endogenous growth theories, new technologies propel the absolute level of economic growth while also increasing per-capita output.

V Conclusions

This paper provides novel evidence of the causal impact of patents on innovation and economic growth. To isolate exogenous variation in patenting activity, we advance and validate the "location bias" hypothesis, whereby examiners favor inventors located close to their areas of origin. Using newly digitized data on the universe of patent examiners active at the US patent office between 1919 and 1938, we show that newly appointed examiners grant 16% more patents to inventors close to their areas of origin.

Examiners have jurisdiction over patent grants in one technological division. To quantify the impact of patenting on innovation, we thus explore how newly appointed examiners influence innovation in *other* division. We find that patenting in divisions not directly covered by the examiners increased by almost 10%. The response is larger in technologically closer divisions while patenting in more distant sectors does not react. Increased patenting and innovation induced by newly appointed examiners had a large effect on local economic growth. Areas exposed to examiners display population gains, attract immigrants, and feature increased manufacturing and high-skilled employment. Using an occupational proxy for income, we find that innovation also increases income.

The evidence presented in this paper provide strong causal evidence of the positive effects of patent rights on innovation and growth. While further research is warranted to gauge the generalizability of our results, we offer a new empirical design to inform the literature on the economic implications of intellectual property protection institutions (Boldrin and Levine, 2013; Williams, 2017).

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Tables and Figures

Table I. Examiners, Patenting, and the Technology Spillovers of Patent Protection

	Number of Patents		Excluding Counties in...		Counties by Total Patents		High-Impact Patents	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	State FE	DC	DC Area	Above 50%	Below 50%	Number	Share
Panel A: Total Patents								
Local Examiner × Post	0.151***	0.068**	0.151***	0.152***	0.133***	-0.043	0.463***	0.454***
	(0.027)	(0.029)	(0.027)	(0.028)	(0.028)	(0.076)	(0.177)	(0.154)
# Counties	2,999	2,998	2,998	2,860	1,518	1,481	792	792
# Observations	59,980	59,960	59,960	57,200	30,360	29,620	15,840	15,840
R^2	0.700	0.709	0.700	0.701	0.627	0.157	0.766	0.414
Mean Dep. Var.	2.875	2.869	2.869	2.914	5.371	0.318	0.949	0.050
Std. Dev. Dep. Var.	5.710	5.700	5.700	5.759	7.171	0.633	5.397	0.235
Panel B: Patents in the Same Division of the Examiner								
Local Examiner × Post	0.345***	0.216***	0.346***	0.333***	0.297***	0.038	-0.269	-0.325
	(0.043)	(0.057)	(0.044)	(0.046)	(0.043)	(0.350)	(0.271)	(0.321)
# Counties	2,442	2,441	2,441	2,357	1,135	1,307	503	503
# Observations	48,840	48,764	48,820	47,140	22,700	26,140	10,060	10,060
R^2	0.597	0.604	0.596	0.598	0.577	0.152	0.699	0.321
Mean Dep. Var.	1.216	1.214	1.213	1.229	2.308	0.267	0.468	0.053
Std. Dev. Dep. Var.	2.919	2.917	2.916	2.949	3.968	0.554	3.311	0.239
Panel C: Patents in Different Divisions of the Examiner								
Local Examiner × Post	0.102***	0.050*	0.102***	0.101***	0.076***	0.009	0.477***	0.489**
	(0.024)	(0.027)	(0.024)	(0.024)	(0.024)	(0.073)	(0.175)	(0.208)
# Counties	2,976	2,975	2,975	2,839	1,410	1,566	785	785
# Observations	59,520	59,500	59,500	56,780	28,200	31,320	15,700	15,700
R^2	0.651	0.657	0.650	0.652	0.578	0.147	0.762	0.382
Mean Dep. Var.	1.940	1.936	1.936	1.975	3.790	0.275	0.922	0.065
Std. Dev. Dep. Var.	4.014	4.007	4.007	4.065	5.211	0.559	5.287	0.284
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	–	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	No	Yes	No	No	No	No	No	No

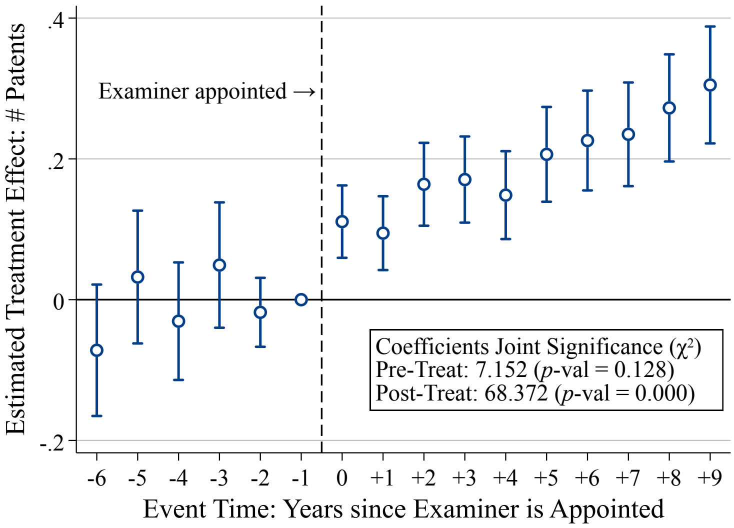
Notes. This table reports the effect of newly appointed examiners on patenting. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents (columns 1–6), the number of patents in the top 20% of the impact distribution (column 7), and the share relative to the total number of patents (column 8). Column (3) excludes Washington, DC; column (4) also excludes Maryland and Virginia; columns (5) and (6) 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 that are 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, patents are aggregated across USPTO divisions; in Panel B, we include only patents in the same division of the newly appointed examiner; in Panel C, we include only patents in divisions other than that of the examiner. The model is Poisson quasi-maximum likelihood. All regressions include county and year fixed effects; in column (2), we also include state-by-year fixed effects. Standard errors are clustered at the county level and are shown in parentheses.

Table II. Innovation and Economic Growth

	Population	Immigration		Employment			Income
	(1)	(2) Overall	(3) Internal	(4)	(5) Manuf.	(6) High-Skill	(7)
Panel A: Outcome Variables Expressed in Level							
Local Examiner × Post	0.037*	0.100***	0.116***	0.061***	0.052**	0.047*	0.070***
	(0.020)	(0.029)	(0.029)	(0.022)	(0.026)	(0.024)	(0.023)
R^2	0.927	0.926	0.915	0.925	0.929	0.932	0.925
Mean Dep. Var.	9.725	8.092	7.832	8.514	6.692	6.332	11.611
Std. Dev. Dep. Var.	1.110	1.407	1.349	1.141	1.605	1.286	1.211
Panel B: Outcome Variables Expressed as Share of the Population							
Local Examiner × Post		0.013***	0.013***	0.012***	0.005**	0.002**	0.367***
		(0.003)	(0.003)	(0.002)	(0.002)	(0.001)	(0.060)
R^2		0.971	0.959	0.676	0.862	0.878	0.799
Mean Dep. Var.		0.276	0.214	0.497	0.102	0.059	11.079
Std. Dev. Dep. Var.		0.200	0.166	0.060	0.071	0.021	1.855
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Counties	2,989	2,989	2,989	2,989	2,989	2,989	2,989
# Observations	17,718	17,718	17,718	17,718	17,718	17,718	17,718

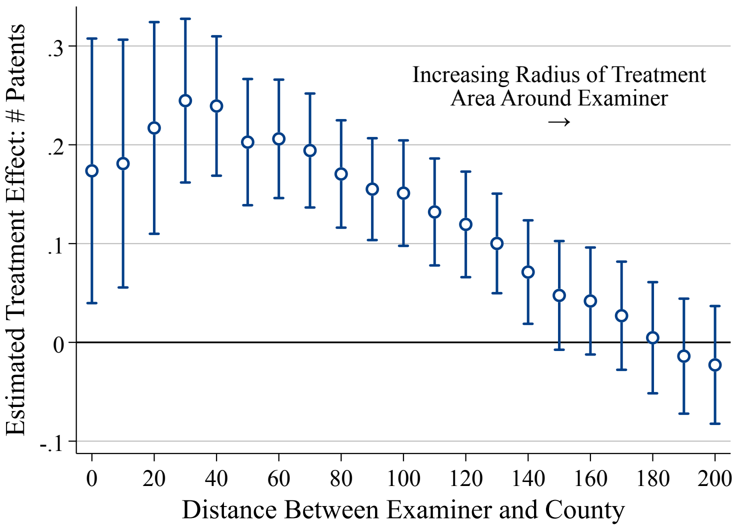
Notes. This table reports the effect of newly appointed examiners on proximate indicators of economic growth. The observation units are counties at a decade frequency between 1900 and 1950. The dependent variable is: population (column 1), overall and internal immigration (columns 2 and 3), overall, manufacturing, and high-skilled employment (columns 4, 5, and 6), and the occupational income score (column 7). 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 A, the dependent variables are expressed in logs; in Panel B, the outcome variables are expressed as population shares. All regressions include county and state-by-year fixed effects. Standard errors are clustered at the county level and are shown in parentheses.

FIGURE I. Effect of Patent Examiner on Local Patenting



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 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 pre- and 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.

FIGURE II. Geographic Spillovers of Patent Examiners on Local Patenting

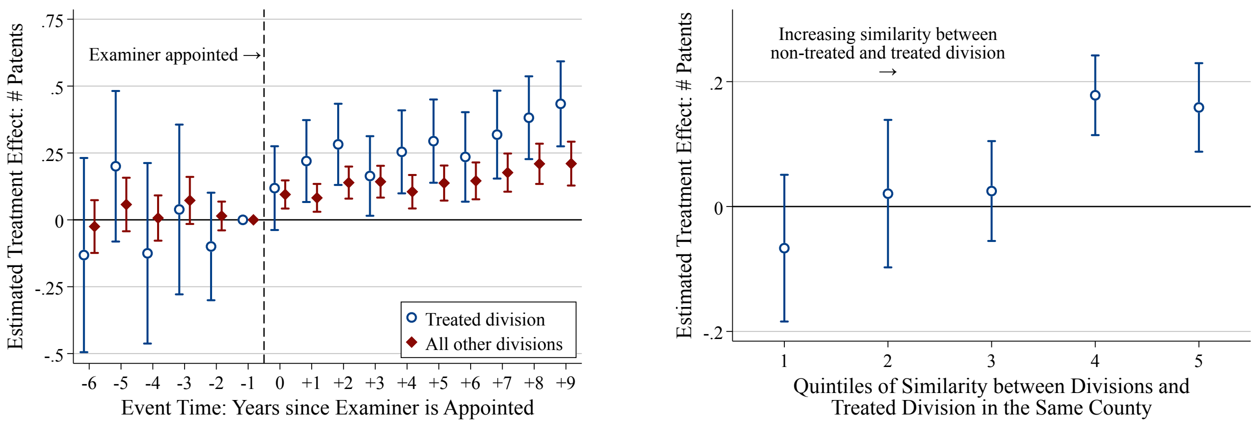


Notes. This figure reports the geographic spillovers of the appointment of an examiner on patenting activity in their area of origin. The observation units are counties at a yearly frequency between 1919 and 1938. The dependent variable is the number of patents. Each dot reports the estimated treatment effect for a treatment, which is an indicator variable equal to one in counties that are exposed to an examiner after the examiner is appointed and zero otherwise. The dots report the estimated treatment effect for various bandwidths of distance between the county and the examiner's county of origin. For example, the dot corresponding to 80 kilometers reports the estimated treatment effect for counties that are within 80 kilometers of the examiner's county of origin. 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.

FIGURE III. Technology Spillovers of Patent Examiners on Local Patenting

(A) Comparison between Treated and Untreated Divisions

(B) Heterogeneous Treatment Effects by Similarity with Treated Division



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. In Panel A, the observation units are counties at a yearly frequency between 1919 and 1938. In Panel B, the unit is a county-by-division pair over the same time period. The dependent variable is the number of patents. In Panel A, 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 blue dots report the estimated treatment effects on patenting in the same division of the examiner; the red dots report those for all other divisions. The regression includes county-by-division and year fixed effects. The last period before the examiner is appointed serves as the baseline category. Panel B concentrates on non-treated divisions: it displays the estimated treatment effect when estimating the main specification separately for each quintile of similarity with the division of the newly appointed examiner. The regression includes county, similarity quartile, 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.