FROM REVOLVING DOORS TO REGULATORY CAPTURE? EVIDENCE FROM PATENT EXAMINERS*

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Abstract

Many regulatory agency employees are hired by the firms they regulate, creating a "revolving door" between government and the private sector. We study these transitions using detailed data from the US Patent and Trademark Office. We find that patent examiners grant significantly more patents to the firms that later hire them, that much of this leniency extends to prospective employers, and that these effects are strongest in years when firms are actively hiring. Ultimately, this leads the agency to issue lower quality patents, which we measure in citations. We argue these results are suggestive of regulatory capture.

JEL: K23, D72, O34. Keywords: revolving doors, patents, regulatory capture, intellectual property.

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1 Introduction

Many regulatory agency employees follow brief, public sector experience with more lucrative work at the firms they used to regulate. In several industries, the practice is so common that these agencies appear to have "revolving doors." It may begin with—and partly be motivated by—firms' desires to hire workers with agency experience. The concern, however, is that it leads to a *quid pro quo*: lax supervision is exchanged for future employment. Whether explicit or tacit, this arrangement can have first order welfare consequences stemming from policies that are ineffective at correcting market failures and unmotivated to protect the public interest [Stigler, 1971, Peltzman, 1976].[1,2] Despite this concern, there is no empirical work studying individual public-to-private sector transitions among regulatory agency employees *at the decision level*.

One agency that both regulates a significant share of commerce and employs a large number of revolving door workers is the United States Patent and Trademark Office ("USPTO"). The agency is tasked with issuing patents, which incentivize effort and reward disclosure by granting inventors a temporary right to exclude others from using the idea. When one submits a patent application to the agency, that application is quasi-randomly routed to an USPTO employee, called an examiner, who decides whether or not to grant a patent. Thus, examiner decisions can affect investment [Budish, Roin, and Williams, 2015], entrepreneurial activity [Farre-Mensa, Hegde, and Ljungqvist, 2017], labor income [Kline, Petkova, Williams, and Zidar, 2016], the rate and direction of follow-on innovation [Moser, 2005, Williams, 2013, Moser, 2013, Galasso and Schankerman, 2015], and ultimately industrial organization and economic growth [Jaffe and Lerner, 2004]. Examination experience is valuable, though, in the private sector, particularly to law firms specializing in intellectual property. Many examiners leave the agency to become patent practitioners, i.e. individuals who have legally registered to represent others in the application process, and join one of these firms. This provides increased pay and mobility—in particular an ability to work outside Alexandria, Virginia[3]—but also creates a potential conflict of interest, since many examiners evaluate the applications of firms for whom they would like to work.

[1] See also Levine and Forrence [1990] and Laffont and Tirole [1991] for more comprehensive reviews of earlier work. These papers also reference helpful descriptive evidence on revolving doors, e.g. Breyer and Stewart [1979].

[2] It could also erode trust in government. This concern is particularly timely and sensitive given that the latest polls indicate only 19% of the public believes that the federal government "is run for the benefit of all the people" (as compared to 65% in 1964, which is the first year the poll was taken). About three-quarters (76%) believe it is "run by a few big interests" (as compared to 29% in 1964) [Pew, 2015].

[3] The office was in the neighboring town of Arlington until 2003. However, at the very end of our panel, the USPTO was in the process of opening satellite offices in Michigan, Texas, California, and Colorado. The new USPTO offices accounted for a negligible share of examiners in the panel, i.e. <1%, of examiners in our data.) The USPTO also introduced a teleworking option for examiners at the end of our panel. That particular change affected only experienced examiners and does not impact our analysis which focuses only on short-tenure junior employees, i.e. those who cycle through the revolving door.)

To study these transitions, we construct an original dataset that links patent grant decisions to the examiners who made them, and then ties these examiners to the firms that hired them. In particular, we observe, for over one million applications, the name and address of the filing firm, the name and unique identifier of the examiner, the decisions he or she made, and the dates on which they were made. For applications that receive grants, we observe the number of citations that the patent receives, which provides a commonly-used proxy of quality. Separately, we observe periodic snapshots of the full list of licensed practitioners, including their name and unique identifier, as well as the name and address of their employer, and supplement this data with biographical information (where available). In broad terms, the data links, *at the decision level*, the individuals setting regulation and the firms affected by that regulation to the individuals hired away from the regulator and firms hiring those individuals. To our knowledge, it is the first that does so.

We investigate whether revolving door examiners behave differently towards future and prospective employers, whether these differences are suggestive of regulatory capture, and whether they affect the quality of regulation. The first question relates to an empirical fact that we feel is important to establish on its own, since there is ongoing speculation about regulatory impartiality, regardless of the motivation behind it. The second presents a challenge. It requires distinguishing between actions whose intent is to gain favor with an employer and those that merely reflect idiosyncratic employee preferences. Features of the setting are helpful here. Unlike prior work that relied upon across-employee differences—or variation across other dimensions—to assess intent, we can study within-employee within-firm differences. This is important, since agency employees that do and do not leave for the private sector—as well as firms that do and do not intensively hire agency employees—can vary on a host of unobservable dimensions. Moreover, examiner-firm match-specific preferences are likely less important than in other settings. These can be problematic since an agency employee who is enamored with a particular brand will be more likely to both treat them favorably and subsequent work for them—even in the absence of capture. However, nearly all examiners-turned-practitioners are hired by law rather than R&D firms. Individuals may have strong preferences for, e.g., Google's brand over Samsung's or Apple's but probably feel less strongly about Foley & Lardner relative to, say, Sughrue Mion or Fish & Richardson.[4] Similarly, examiner-technology match-specific preferences are also likely less important here. These are problematic if examiners are exposed to a wide range of technology but law firms specialize. The former hypothetical, though, is ruled out, since examiners are administratively grouped within the USPTO by specialized "art units," each of which covers only a

[4]These are three commonly-observed law firms in our sample.

narrow amount of technological scope.[5] Other methods to distinguish these two are discussed below. The final question requires us to rule out that examiners merely *appear* favorable by selecting high quality, easy-to-grant applications from the queue. This would affect grant rates specific to certain examiner-firm dyads but on its own have no real effect. The allocation of applications to examiners at the USPTO, however, is quasi-random [Williams, 2013].[6]

We begin by showing that revolving door examiners grant 12.6-17.6% (8.5-11.4 percentage points) more patents to firms that later hire them. This result is robust to varying the level of controls, e.g. the inclusion or exclusion of examiner and firm fixed effects, or limiting the sample to only firms that hire at least one examiner, which cuts the sample by roughly two-thirds. While the "headline" number alone is not proof of capture, the robustness does suggest that unobservable differences—at least along the aforementioned dimensions—are small.

We next ask whether revolving door examiners extend this leniency to prospective employers as well. Here we rely on two premises: first, that examiners face uncertainty about which firms will have future job openings and, second, that conditional on the type of work, an employer's location is the most important attribute on which workers base their choices [Barber and Roehling, 1993, Turban, Eyring, and Campion, 1993, Powell and Goulet, 1996]. Thus, we test whether they grant more patents to other firms in close proximity to the firm that hired them (after excluding any observations where the filing firm later hired the examiner). We find that examiners extend much of the leniency afforded to their future employers to other firms that are nearby. For example, they grant 6.3-11.2% (4.2-7.4 percentage points) more patents to firms in the same ZIP code. Again, these results are robust to varying the granularity of the controls and restricting the sample to only firms or cities that hire at least one examiner. To be indicative of regulatory capture, this approach requires only that examiners' match-specific preference shocks are independent across locations, rather than across firms, so it provides somewhat stronger evidence.

To further assess intent, we consider three alternative approaches. First, we plot the coefficients of interest by year against measures of hiring intensity. If revolving door examiners exhibit leniency to gain favor—or at least not aggravate—prospective employers, then our estimates should load on periods when private sector firms are actually hiring. We find precisely this relationship. Second, we incorporate data on the educational backgrounds of the examiners. This approach relies on the

[5]As an example, art unit 2687 examines only "the mechanical parts of disk drives for dynamic storage systems."

[6]Recent work by Righi and Simcoe [2017] shows that examiners do not specialize in high or low value applications, which support this assumption, although they provide other evidence of specialization. See Footnote 11 for a more detailed discussion of their findings.

premise that prior residency—in particular, where one was educated—affects location preferences. Thus, if revolving door examiners are lenient towards their potential employers, then they should grant more patents to firms that are closer to their *alma maters*. The data are consistent with the premise as well as the conclusion. Since the location of one's schooling is unlikely to be otherwise correlated with grant-related preferences, at least within the narrow confines of what examiners evaluate while working at the USPTO, this result is particularly suggestive of regulatory capture. Importantly, this relationship does not exist among examiners that do not become patent practitioners, which helps rule out "boosterism" as an alternative explanation of this result. Third, we exploit recently-released patent claims data to construct two measures that the USPTO believes best capture how much patent scope is reduced by examination, i.e. proxy for how tough the regulator is. We then replicate the first set of tables while replacing the left-hand side of the estimating equations with these measures. We show that effects on the intensive margin of intellectual property protection are consistent with those on the extensive margin. This finding—and the one immediately below—support the notion that leniency is driving the grant-related results rather than signalling by revolving door examiners that they can effectively negotiate the prosecution process.

Finally, we ask whether this behavior results in lower quality patent grants. Examiner leniency naturally lowers the threshold for which an application results in a grant, so patents granted by revolving door examiners to their future and prospective employers may receive fewer citations. In line with prior findings, the results hereindicate that patents granted to the firm that later hires a revolving door examiner receive 19-24% fewer citations. Those granted to firms in the same city or ZIP code that later hired them receive 6-10% fewer citations.

We argue that although this research design boasts no "smoking gun," these results taken together are suggestive of regulatory capture. Care should be given, however, to their interpretation. First, note we do not claim that firms are blindly hiring examiners who treat them favorably—only that their ability to tell good from bad potential employees is not significantly hindered by this behavior. Several facts support our claim. Firms have access examiners' responses to all applications, not just their own, so there are many other observations on which they can gauge ability or ambition. Also, examiners can presumably still signal their type while granting a patent. (In the academic profession, this would be akin to submitting a referee report that acknowledges a paper's weaknesses in a sophisticated way but ultimately dismisses those shortcomings.) Moreover, firms still subject ex-examiners to an interview process as they would any other prospective employees. Beyond all this, patent practitioners face steep quotas on observable quantity and quality measures. Thus, self-selection may limit how careful firms must be in screening potential workers. Also, note that we do not claim that favor-seeking is the primary motivation for hiring examiners. Law firms focusing on intellectual property protection independently value agency experience. Whether or not regulatory capture is the main reason for the USPTO's revolving door should not diminish interest in quantifying its effects (although it will affect how policy addresses it, as discussed below). Further, note that nothing here implies explicit collusion. If anything, our findings tend to suggest the opposite—that a norm exists in this industry. As former Defense Secretary Robert Gates stated, "We need sharp-penciled negotiators...but don't provide the incentives. They'll go to work for a contractor some day, so they don't want to be perceived as a pain in the ass." Finally, note that we do not claim that the preferential treatment of prospective employers is the most pressing problem faced by the agency. Revolving door examiners account for on the order of ten percent of applications and grants, and only a fraction of these may be shown favoritism. Instead, we see the USPTO as a data-rich setting to answer basic questions about regulator behavior. That said, the *relative* narrowness of the examiner-practitioner pay gap means we likely understate the problem relative to many other regulator-regulated revolving door relationships.

In terms of policy, we caution that that these results do not in themselves suggest immediate changes, e.g. the implementation of a "cooling off" period akin to the ones faced by accountants and lobbyists. Altering employment contracts to prohibit examiners from working in private sector roles for which they are best suited can dissuade talented people from joining the USPTO in the first place. Even if revolving door examiners are biased towards some firms, their work may be higher quality overall. In fact, while we are reluctant to draw inference from across-examiner differences, we would be remiss to not point out that revolving door examiners decisions seem to be higher quality overall.[7] Patents granted by them receive more citations than those of their colleagues (even if patents granted to their future or prospective employers receive much fewer overall). The variation we exploit cannot cleanly evaluate competence-collusion tradeoffs, although we argue it provides evidence suggestive of regulatory capture and should additional scrutiny from social scientists.

This paper contributes to a growing empirical literature on regulator behavior, which has tended not to find evidence of capture. Agarwal, Lucca, Seru, and Trebbi [2014] study the rotation of US bank regulators between federal and state supervisor positions, and provide broad evidence that state supervisors are systematically more lax than federal ones. They argue this is due to institutional design problems and suboptimal incentive system but find no evidence of regulatory capture. For example, the state-level variation in turnover is not associated with leniency in their study. Lucca,

[7] Kempf [2015], cited below, provides compelling evidence of this in a private sector setting.

Seru, and Trebbi [2014] use longitudinal variation to show that numbers of employees who leave the regulating agency for private sector employment are higher during periods of intense regulatory enforcement, which is inconsistent with the view that as job prospects at regulated firms improve, the incidence of capture also increases. deHaan, Kedia, Koh, and Rajgopal [2015] show that private law firms defending firms targeted by the SEC actually hire *harsher* prosecutors. Interestingly, they do find some evidence of capture when they limit the sample to Washington, DC-based attorneys, but this result is not defendant-specific. Our paper expands this literature mainly by accounting for employee-firm relationships, incorporating the notion of "prospective employers" into the analysis, and intersecting these with novel supporting evidence, e.g. sample restrictions, the use of employees' *alma maters* to inform us about preferences and the use of citations to inform us about effects on quality.

This paper also relates to the literature studying revolving doors in the private sector, e.g. between credit rating analysts and the investment banks that they rate. These studies are primarily concerned with the extent to which *quid pro quo* arrangements contributed to the 2009 financial crisis. For example, Cornaggia, Cornaggia, and Xia [2016] show that corporate finance credit rating analysts issue higher ratings to firms for whom they later work, but the effect is modest—roughly 10% of a standard deviation in the ratings.[8] Kempf [2015] finds that leveraged finance credit analysts who are hired by investment banks also inflate their future employers' ratings, but this group of analysts is more accurate overall. These papers provide compelling results in their respective settings, though it is unclear how informative they are about the behavior of government agencies, which differ in important ways. For one, public entities are rarely disciplined by market forces. A credit rating agency that issues uninformative reports due to its analysts' pandering will either remedy the situation or go out of business; however, an agency like the USPTO will maintain its monopoly over the provision of intellectual property protection, regardless of how deserving the organization is.

Also related is work on lobbying [Vidal, Draca, and Fons-Rosen, 2012, Bertrand, Bombardini, and Trebbi, 2014]. This literature differs from our paper, however, in that previous relationships—rather than the prospect of future employment—influence behavior. It also differs in that policy outcomes are unobserved, so changes in administrative or regulatory behavior must be inferred. Lastly the paper aids in understanding how patent rights are allocated in the United States [Cockburn, Kortum, and Stern, 2002, Lemley and Shapiro, 2005, Alcacer and Gittelman, 2006, Lemley and Sampat, 2012, Frakes and Wasserman, 2016a].

[8] The measured effect is between 0.18 and 0.23 rating "notches," with a standard deviation across issues of approximately 1.3 to 2.3, depending on the agency. Relatedly, a study by Lourie [2014] of equity research analysts at financial institutions provides comparable findings.

The outline of this paper is as follows. Section 2 describes the patenting process at the USPTO, and the roles that patent agents, attorneys, and examiners play in it. Section 3 describes the data. Section 4 introduces the empirical framework. Section 5 reports our grant-related findings, while Section 6 reports quality-related results. Section 7 concludes.

2 Institutional background

Applying for US patent protection

The USPTO is tasked with issuing patents, which grant the inventor the temporary, monopoly right to exclude others from making or using the named invention in exchange for its public disclosure. The process begins with an application, which may be submitted by either the inventor or a licensed practitioner.[9] Initial filing fees are several hundred dollars.[10] All new patent applications are sorted based on the type of technology, and directed to the appropriate "art unit"—the group of examiners tasked with examining that subject matter. Art units are highly specialized. For example, units 2687, 2688, and 2689 all handle "Dynamic Storage Systems," but the first of these handles only the "Mechanical parts of the Disk Drives" while the latter two handle "Signal Processing & Control Processing" aspects. Once directed to the particular art unit, the new patent applications are then randomly allocated to that art unit's examiners,[11] most of whom work at the Alexandria, Virginia campus.[12]

The core of the application is a set of one or more claims. These define the patent's exclusionary scope, i.e. the extent of intellectual property protection. As an example, Orville and Wilbur Wright claimed a "class of flying machines in which...one or more aeroplanes are moved through the air (...)".[13] Examiners decide whether to allow or reject the claims. They also provide reasons behind their

[9] A patent can have one or more applicants which can be individual inventors or organizations—either public entities, universities or private firms.

[10] Filing fees depend on the size and the type of the patent applicant, and a slew of other factors. For example, requesting accelerated patent examination can cost between $1,000 and $4,000, depending on the applicant size and type. Requesting additional time to reply to examiner comments can cost between $50 and $200 for a one month extension, and between $750 and $3,000 for a five month extension, both depending on the applicant type and size.

[11] Recent work by Righi and Simcoe [2017] shows that examiners do not specialize in high or low value applications, which tends to rule out the possibility that revolving door examiners are selecting (and/or being allocated) easy-to-grant applications. Moreover, if one expects USPTO managers to allocate easy-to-grant applications to themselves or their close colleagues, then this result more broadly suggests that deducing quality from a cursory reading of a patent is simply difficult to do. The authors also show that examiners do not specialize in applications with long or short claims, although they do specialize in particular technologies. Since our main specifications condition on the examiner, specialization across examiners should not bias our results. Furthermore, specialization is less likely among revolving door examiners, who typically leave after only a few years at the agency.

[12] See Footnote 3 for more detail on the location of the examiners.

[13] US Patent No. 821,393.

decisions, which may aid the applicant in revising the claims to have them allowed. The criteria for claims to be allowed are novelty, usefulness, and non-obviousness. An initial rejection of some or all claims is very common and referred to as a "non-final rejection." In response, applicants can modify or remove claims.

The process, called "patent prosecution," may go back and forth several times. Each submission, modification, and appeal, however, requires additional fees—often times stretching into the thousands of dollars. Patent prosecution ends with one of the following: the examiner allows a portion of the claims and the filer is satisfied with this allowance, in which case a patent is issued; or the filer abandons the application. As Roin [2016] points out, nearly all applications can, in practice, result in a grant—the filer can simply narrow the claim language to suit the examiner. Abandonment is best viewed as the case where the examiner insists on such narrow claims that filer deems the application no longer worth pursuing. Thus, the binary outcome of whether the application resulted in a grant approximates the somewhat more complicated result.[14]

Examiners

Of more than 12,000 USPTO employees, 70% are patent examiners. The job requires a minimum of a bachelor's degree in the subject matter covered by an art unit. Many examiners have masters or doctoral degrees, and most are recruited directly out of school. Their pay depends on the General Schedule, i.e. the "GS" government pay scale. In an ongoing effort to retain staff, the USPTO has secured incentive pay to encourage higher production, although these are dull—typically not exceeding 10% of base pay. After two years at the agency, examiners typically reach pay grades of GS-9 to GS-11. With incentive pay, these equate to pay of roughly $63,000 and $87,000 per year, respectively.[15]

Before transitioning into an art unit, all new patent examiners are trained at the agency for several months. Statute requires that a supervisory patent examiner ("SPE") reviews all examined applications. Yet for several reasons this approval is mostly a "rubber stamp."[16] First, SPEs are busy with their own prosecution cases. Second, rejections of junior examiner submissions slow production, and SPEs are

[14]We rely on this approximation throughout the paper, with the exception of a brief discussion at the end of Section 5 and a more detailed discussion, with accompanying tables, in the Appendix, which show that effects on the intensive margin of patent protection are consistent with those on the extensive margin.

[15]See *http://federalpay.org* for individual and summary statistics for federal salaries (retrieved December 16, 2016). See *https://www.opm.gov/policy-data-oversight/pay-leave/salaries-wages/2016/general-schedule/* for GS salaries across metropolitan areas (retrieved December 16, 2016).

[16]See the initial draft of a report by the Office of Inspector General that was leaked to the *The Washington Post*. The report states, for example, that "investigators found that first-line supervisors feel powerless to discipline poor performers" and that "inconsistent enforcement ... has rendered the existing controls completely ineffective." The report can be found at https://www.washingtonpost.com/apps/g/page/politics/initial-report-on-us-patent-and-trademark-office-investigation-of-telework-fraud-allegations/1244/ (retrieved November 17, 2016).

incentivized to maximize the output of their art units. Third, examiners can wait until the end of a reporting cycle to dump a large number of cases on an SPEs desk and completely overwhelm them.[17] Lack of examiner oversight is a well-known problem at the USPTO and has triggered both a detailed report by the Office of Inspector General[18] and a joint hearing by the US House of Representatives Ethics and Oversight Committees. As a result, even junior examiners have great discretion over grant decisions—an important factor driving the underlining data generating process in this paper.

Practitioners

The time and complexity of the patenting process leads most inventors to hire a patent practitioner to write the application and manage correspondence with the USPTO throughout the process. These practitioners are employed primarily by law firms. Even large, frequent filers such as Lockheed Martin and General Electric outsource the bulk of their filings to law firms that specialize in matters related to intellectual property protection. These firms seek out employees with past examination experience. In fact, examiner hires are an important selling point for law firms and even mentioned in promotional materials, including their websites. For example, Oliff states "most of our associate[s] and other attorneys are registered to practice before the PTO, and many are former PTO Examiners,"[19] while Fish & Richardson states, "more than a dozen Fish attorneys had prior careers at the USPTO."[20]

The salaries of practitioners are much higher than those of examiners. This is one of the main factors driving high attrition at the USPTO, and it is a common feature of many "regulated firm-regulatory agency" relationships. Examiners without a law degree can register as patent agents and expect to earn $100,000 per year or more in most metropolitan areas. Those with a law a degree can register as patent attorneys and make as much as $160,000 per year starting out.

Examiner–practitioner interactions

Throughout prosecution, the patent examiner and correspondent are acutely aware of each other's identity. To see this, one need go only as far as the USPTO cover page, which sits atop the large volume of transmissions that pass from the agency to the filer. Displayed prominently on each, alongside very little additional information, are the name and address of the filing firm *and* the first and last name of the examiner. To make this concrete, the Appendix provides examples of cover sheets that preceded the

[17] *Ibid.*
[18] *Ibid.* See also Frakes and Wasserman [2016b].
[19] See https://www.oliff.com/practice-areas/patent/ (retrieved January 2, 2017).
[20] See http://www.fr.com/services/patent-law/ (retrieved January 2, 2017).

non-final rejection notice for four well-known, relatively recent applications. These include Google's 1998 application for the PageRank algorithm, GoPro's 2004 application for an attachment of a camera to a body, Theranos's 2005 application for rapid diagnosis, and Square's 2010 application for a mobile phone attachment that captures credit card information. Using the Google example, one can see that the transmission is addressed to Harrity & Snyder of Fairfax, Virginia. (This firm and location appear below in Table III and Table IV, respectively). Tantamount, the examiner knows that the firm knows exactly whose decision this is. Interestingly, nowhere does the form provide space to list the firm actually employing the named inventor.[21]

Filers can also request interviews with examiners to discuss a application. In these cases, examiners and filers speak on the phone or meet in person, typically at USPTO headquarters. The USPTO does not, unfortunately, strictly require examiners record these interviews, so we cannot observe their frequency. This provides the opportunity for richer interactions, even if these go unmeasured in our study.

3 Data

Sources

The data come from four main sources. First, the Patent Examination Research Dataset ("PatEx") provides application-level data including the name and unique identifier of the examiner, the name and address of the filing firm, the final decision made by the examiner, and the date on which it was made.[22] The dataset covers substantively all filings between November 29, 2001 and December 31, 2015,[23] and for those applications that results in grants, coverage extends back to at least July 1995 [Graham, Marco, and Miller, 2015].[24] PatEx is derived from the Public Patent Application Information Retrieval system ("Public PAIR"), carefully documented by the Office of Chief Economist of the USPTO, and posted "for public use and [to] encourage users to identify fixes and improvements."

[21]No mention of Google, GoPro, Theranos, or Square appears anywhere on these sheets. All were incorporated when these documents were mailed, and all but Google were incorporated when the applications were filed. Note that this is not to say that the examiner is always unaware of or cannot ascertain the identity of the firm that developed the technology—only that this information is much less salient than the law firm filing the application.

[22]USPTO. https://www.uspto.gov/learning-and-resources/electronic-data-products/patent-examination-research-dataset-public-pair (retrieved on October 16, 2016).

[23]This paper studies utility patents, which comprise roughly 94.3% of filings. (The remainder are plant and design patents.) Coverage for regular non-provisional utility patent applications is 95% [Graham, Marco, and Miller, 2015]. The small number of applications falling outside PatEx did so for idiosyncratic reasons, e.g. many related to ballistics and radar are non-public for reasons of national security. The data is incomplete prior to December 2000, since applications filed prior to that date were not subject to the America Inventors Protection Act ("AIPA"), which stipulated all non-provisional patent applications be published, even if abandoned. The effects of this legislation on the coverage of PatEx are evident in the data; see the Appendix for more details.

[24]Law changes that take effect June 8, 1995 affect, among other things, the term of US patents. These create a spike in filings, and may change other features of the process. To avoid these issues, we merely begin the panel in July 1996.

Second, the Patent Practitioner Roster lists all individuals registered to practice before the USPTO, as well as the name and address of their current place of business.[25] The list is publicly posted and constantly updated by the Office of Enrollment and Discipline of the USPTO. As stated above, only registered practitioners can legally file on behalf of inventors, and since the list is intended, at least in part, as a resource for inventors looking for representation, the office ostensibly takes availability and accuracy seriously. The USPTO does not maintain historical lists, but (in addition to the current roster) we can retrieve "snapshots" of the roster for January 2009, October 2011, February 2013, and March 2015 using the Internet Archive[26]. The office issues unique identification numbers to practitioners upon registration, so joining lists is straightforward, and turnover is low among this group. Thus, this data represents a nearly-complete list of the work histories of all examiners who leave the USPTO to practice patent prosecution. We confirmed the high degree of completeness using a randomly drawn 5% sample of examiners using several online resources (described two paragraphs below).

Third, Thomson Innovation provides forward patent citations, i.e. a count of the number of times a patent is cited by other published patents. This proxies for quality of the underlying innovation.[27,28] Citations accrue from patents published by all national offices.[29] This data merges exactly to PatEx on the USPTO-assigned patent number, which is a unique identifier.

Fourth, we compile educational histories by combining data from an employment-oriented social networking website, Martindale-Hubbard's attorney directory, and the employee profiles hosted on the websites of law firms. In particular, we record the name and address of any degree-granting institutions that the examiner attended[30] and merge to PatEx on the basis of first, middle, and last name.

Variable descriptions

The first main outcome of interest, *Grant*, takes a value of one if PatEx indicates the application's status is "issued" and zero if the status is "abandoned." Observations are dropped if the application

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[25] USPTO. https://oedci.uspto.gov/OEDCI/ (retrieved on October 16, 2016).

[26] Internet Archive. http://web.archive.org/web/*/https://oedci.uspto.gov/OEDCI/ (retrieved on October 16, 2016).

[27] One could also proxy for quality with the number or proportion of claims that are invalidated in the litigation process, but the incidence of litigation is far too low to be useful to our analysis. One could also consider the number or proportion of decisions that conflicted with those from the USPTO's European or Japanese counterparts, but again the incidence of multi-jurisdiction filing and discord is too low to be helpful.

[28] It includes examiner-added citations, since other data begins in 1995 but this distinction is only observable starting in 2001.

[29] A prior version of the paper erroneously stated that we include only US patents, although the results are not sensitive to this difference.

[30] A large number of examiners attend law school on a part-time basis in Washington, DC while working at the USPTO. Since graduation dates are not reliably observed, we cannot distinguish examiners who attend Washington, DC law schools prior to working at the agency, i.e. in the normal course of their education, from those who attend merely as an artifact of working at the agency. As a result, we exclude law schools. Including them provides very little information about location preferences and instead introduces a very large amount of right-hand side measurement error.

status is "pending." Pending applications are those that have yet to be examined and are in the USPTO examination backlog. The second is *Citations*, which counts references from other published patents. *Year* fixed effects correspond to the date of the examiner's last action on an application, i.e. an allowance preceding a grant or the last rejection preceding abandonment. *Experience* fixed effects correspond to the total number of complete years of experience that the examiner has accrued over the panel.[31] *Technology center* and the more specific *Patent class* fixed effects provide alternate, related ways to distinguish between types of technology.

To construct distance measures between educational institutions and filing firms, we rely on the latitude and longitude assigned to five digit US ZIP codes. Relative to addresses, five digit ZIP codes map easily and quickly to geospatial coordinates and do not result in any meaningful measurement error (relative to the variation that we consider). When an examiner has attended multiple schools, we use the minimum distance between the filing firm and the set of schools in question. For example, if the examiner holds degrees from both Berkeley and Clemson, we use the distance to Berkeley if the filing firm was based in San Francisco.

Summary statistics

Table I provides an initial summary of the data used to assess patent grant behavior. It covers just over one million patents, 63% of which result in a grant. The mean examiner decision (across the longitudinal dimension of the panel) occurs early in 2011. Examiners acquire roughly 6.4 years of experience before leaving the agency. Nearly 30% of applications are filed by firms that hire at least one examiner, and 7% of applications are examined by employees who become practitioners.

[Table I about here.]

Table II provides an initial summary of the data used to assess patent quality. Citations are only available for granted patents, so the sample size is smaller despite the longer panel. Patents accrue an average of 11.6 citations. The distribution include a large number of zeros and a small number of very highly cited patents. The other dimensions of the data are distributed similarly to the sample used to assess grant behavior. Since earlier patents have more time to accrue acknowledgements, there is a clear age pattern in forward citations. All empirical specifications include year fixed effects, though, controlling for this pattern.

[31] We proxy for the start date and, in the event of leaving the USPTO, end date of an examiner's tenure at the agency using the first and last observed last action on an application. The measure is somewhat noisy, but it serves only as a control variable and is subsumed by fixed effects in the main specifications anyway.

[Table II about here.]

Table III summarizes the data across the firms that most frequently hire examiners. Notably, all are law firms. Most specialize in intellectual property or at least heavily emphasize this part of their practice. Finnegan, Henderson, Farabow, Garrett & Dunner, LLP ("Finnegan") hires the largest number of examiners. Oblon, McClelland, Maier & Neustadt, LLP ("Oblon") files the most applications. Overall grant rates vary across firms although the majority fall close to the group average of 65%, which is close to the average rate for all filers.

[Table III about here.]

Table IV describes the geographic distribution. Almost half of the examiners who become practitioners remain in the Washington metropolitan area, including cities like Alexandria, Arlington, DC, McLean, and Reston. The balance spread is throughout the country. New York City, Chicago, San Francisco, and Boston are all heavily represented. Other large and/or R&D-focused cities are represented, including Dallas, Philadelphia, Minneapolis, and San Diego. Applications are distributed similarly to hires, with some minor exceptions. For example, cities near the USPTO headquarters are slightly over-represented in terms of hiring. Cities such as Minneapolis, Houston, and Irvine are somewhat under-represented.

[Table IV about here.]

4 Model

Conceptual framework

The decision of examiner i to grant intellectual property protection to firm j is given by[32]

$$y_{ij} = \theta x_{ij} + \eta_{ij} + \epsilon_{ij}. \tag{1}$$

[32]Note that we give agency to the examiners and take as given the human resources policies of the firm or customs of society, which are longer-lived entities. We remain agnostic throughout most of the paper as to whether examiners respond to a social norm, tacit collusion with firms playing a repeated game, or an explicit *quid quo pro*—the last of which we tend to think is uncommon and unlikely. Note also that for simplicity, we model the outcome of patent examination and abstract away from the underlying choice problem. Non-x terms map to, in the latent decision problem, the utility derived from accurate decisions, e.g. those that avoid censure by management. x maps to, in the latent decision problem, utility derived from, e.g., later working in a high paying private sector position or not violating a norm.

y denotes the extent of protection. x denotes how much i would like to work at j. θ reflects capture in a straightforward way: x directly influences y.[33] If the parameter is positive, for example, then examiners are lenient on the firms for whom they would like to work. The remaining terms, η and ϵ, reflect the degree to which the examiner believes the filer genuinely deserves intellectual property protection. They differ, though, in that η also determines x while ϵ does not. W denotes any factors observable to the econometrician (except for ℓ, described below). Without loss of generality, let $\eta_{ij} = W_{ij}\gamma + \tilde{\eta}_{ij}$ and $\epsilon_{ij} = W_{ij}\pi + \tilde{\epsilon}_{ij}$.

The presence of η in Equation 1 is not merely hypothetical. In most industries, regulatory agency employees who are most enthusiastic about the activities of the regulated firms are also most likely to seek employment at those firms. Similarly, firms that are the most skillful in attracting job applications may also be the most successful in receiving regulatory approvals. Either creates a correlation between grants and hires, even in the absence of rent-seeking behavior. A rich set of controls can mitigate this problem—examiner and firm level fixed effects, for example, will subsume both of the aforementioned relationships. Even with these controls, residual variation can create a material problem. We address these match-specific preference shocks, denoted by $\tilde{\eta}$, at length in the discussion below.

The examiner's desire to work at a firm is given by

$$x_{ij} = \lambda\ell_{ij} + \kappa\eta_{ij} + \nu_{ij}. \tag{2}$$

ℓ reflects firm attributes that are observable to the econometrician and determine where an examiner would like to work, but that are unlikely to otherwise influence examination. ℓ can be a vector or scalar and take continuous or discrete values. Since prior survey-based studies indicate that conditional on the type of work, the location of an employer is the most important attribute on which workers base their preferences [Barber and Roehling, 1993, Turban, Eyring, and Campion, 1993, Powell and Goulet, 1996], we base ℓ on geography. ν denotes other factors that influence x but not y. Without loss of generality, let $\nu_{ij} = W_{ij}\mu + \tilde{\nu}_{ij}$.

[33] We take as given that direct effect of x on y reflects the examiner's desire to gain favor with, or at least not aggravate, a prospective employer. An ostensibly plausible alternative explanation for this relationship is that examiners draw inference about a prospective employer's quality based on the quality of their applications, which correlates with the grant rate. This seems highly unlikely, since examiners have immediate access to much more precise information based on all applications, including informative summary statistics like average grant rates.

Estimating the effect on grants

We begin by estimating a linear probability model, substituting *Grant* for y, so that the outcome of the examination process is binary. Since comparisons between examiners who do and do not become practitioners or between firms that do and do not intensively hire examiners could reflect a host of factors besides regulatory capture, we remain agnostic as to what these differences indicate throughout much of the paper.[34] Instead, we focus on whether certain examiners are especially lenient towards particular firms.

We first assess whether examiners are especially lenient on firms that later hire them. With time subscripts added, the estimating equation is

$$Grant_{ijt} = W_{ijt}\phi_1 + \beta_1 x^{\star}_{ij} + \xi_{ijt}. \tag{3}$$

$x^{\star}_{ij} = 1$ if j hires i and $x^{\star}_{ij} = 0$ otherwise. The parameter of interest is β_1. This estimation strategy relies on the fact that examiners apply to positions at—and hence on average are hired by—firms for which they most want to work.[35]

The presence of $\tilde{\eta}$ is problematic.[36] Since $\mathbb{E}[\tilde{\eta}|W, x^{\star} = 1] > \mathbb{E}[\tilde{\eta}|W, x^{\star} = 0]$, if the variance of $\tilde{\eta}$ is non-zero, then $\hat{\beta_1}$ typically exceeds zero even when θ is zero. In other words, the econometrician will incorrectly infer capture even when it does not occur. However, note that granular right-hand side controls mitigate the problem by limiting the scope of confounding factors. In the main specifications below, for example, W comprises examiner and firm (as well as year) fixed effects. This confines the problem to whether examiner-firm match-specific preference shocks simultaneously affect both hires and grants. Also, as aforementioned, note that the vast majority of examiners hired to the private sector go to law rather than R&D firms, the former of which should contribute less to the variance of $\tilde{\eta}$ than the latter. Last, these residuals could also reflect heterogeneous preferences across types of technology. For example, if an examiner is more enthusiastic about innovations among automotive engines than among cellular phones, then he or she will grant more patents to and apply to work at firms specializing in the former rather than the latter. Since the art units expose examiners to very little

[34]This is not to say that heterogeneous employee behavior—especially across those who are and are not hired away from the regulator by the regulated firms—is policy irrelevant. It may reflect a *quid pro quo*, and it may not. In any case, differences equate to "inconsistent regulation" (in the parlance of Agarwal, Lucca, Seru, and Trebbi [2014]) and typically result in large inefficiencies. See their paper for a more in-depth discussion.

[35]Formally we have that $\mathbb{E}[x|W, x^{\star} = 1]$ exceeds $\mathbb{E}[x|W, x^{\star} = 0]$.

[36]A potential problem outside the framework presented here exists if examiners can choose which applications they work on. In an effort to appear favorable, they could pull easy-to-approve applications from firms for whom they would like to work out of the USPTO backlog. Recall, though, that the quasi-random allocation of applications to examiners rules this out. See Footnote 11 for a more detailed discussion.

technological scope, this can be effectively be ruled out.

To provide more credible evidence of capture, we propose also assessing whether the leniency that examiners show to the firms that actually hire them extends to the firms for whom they *might have worked*. We infer the set of prospective employers from location choices. Under weak conditions, ℓ is most likely to be equal to one for the city where i was actually hired. For example, if we compare two groups of revolving door examiners, with the first taking jobs in Chicago and the second taking jobs in New York, then we infer that Chicago was more preferable than New York for the first group relative to the second. To deal with the confounding influence of η, we discard all observations in which the filing firm hired the examiner.

For this strategy, the estimating equation is

$$Grant_{ijt} = W_{ijt}\phi_2 + \delta_2 \ell^{\star}_{1ij} + \rho_2 \ell^{\star}_{2ij} + \tau_{ijt}, \tag{4}$$

for observations in which $x^{\star}_{ij} = 0$. ℓ is a vector with two components. $\ell^{\star}_{1ij}$ takes a value of one if j resides in the ZIP code in which i was hired and zero otherwise, and $\ell^{\star}_{2ij}$ takes a value of one if j resides in the city, but not ZIP code, in which i was hired and zero otherwise. The parameters of interest are δ_2 and ρ_2.

Estimating the effects on patent quality

We also study whether this leniency results in lower quality patents. Applications affected by capture face lower thresholds to receive grants. If $\theta > 0$, then for the subset of applications that do result in grants, those affected by capture will be of systematically lower quality, which we measure in forward citations.

The estimating equations for the linear models are given by

$$Cites_{ijt} = W_{ijt}\phi_3 + \beta_3 x^{\star}_{ij} + \psi_{ijt} \tag{5}$$

and

$$Cites_{ijt} = W_{ijt}\phi_4 + \delta_4 \ell^{\star}_{1ij} + \rho_4 \ell^{\star}_{2ij} + \varepsilon_{ijt}, \tag{6}$$

where, in the case of Equation 6, we restrict to (i, j, t) observations such that $x^{\star}_{ij} = 0$. The parameters of interest are β_3, δ_4, and ρ_4. We also estimate a quasi-maximum likelihood Poisson model to more address the count-nature of the data [Wooldridge, 1999, Rysman and Simcoe, 2008, Bertanha and Moser, 2016].

5 Effects on patent grants

Do examiners favor their future employers?

Table V reports the estimates of Equation 3. These assess whether revolving door examiners are especially lenient on their future employers and, when the specification allows, assess whether revolving door examiners are more lenient overall. *Grant* is regressed on indicator for whether the filing firm subsequently hired the examiner and, in columns 1-2, an indicator for whether the examiner became a practitioner. Columns 1-2 include fixed effects to control for the type of invention and complete years of examiner experience, while columns 3-4 include examiner fixed effects. In this and later tables, increasing the number of fixed effects forces us to drop a very small number of observations—always less than 1% of the sample. Columns 2 and 4 include firm fixed effects whereas 1 and 3 do not. All columns include fixed effects for the calendar year.

[Table V about here.]

Revolving door examiners grant about 12.6-17.6% (8.5-11.4 percentage points) more patents to firms that subsequently hire them relative to those that do not. Including firm fixed effects has almost no impact, while including (the large number of) examiner fixed effects attenuates the estimates by one-quarter. All specifications yield coefficients that are significant at the 5% level or less. These results provide an initial indication that examiners are lenient on firms from whom they expect to later ask for a job.

Revolving door examiners also grant 7.1-7.4% (4.4-4.6 percentage points) more patents overall. For firms fortunate enough to have their applications routed to a revolving door examiner who they will later hire, the first and second row coefficients are added; the probability of receiving a patent increases by one-quarter. We are reluctant to interpret much, if any, of what appears in the second row as proof of capture. Nonetheless, the combined size of the coefficients means, in the language of Agarwal, Lucca, Seru, and Trebbi [2014], filers face "inconsistent regulation."

Do examiners favor prospective employers as well?

We now turn to Table VI, which reports estimates of Equation 4. These assess whether examiners are more lenient on prospective employers. The layout of the table is analogous to that of the previous table, except that the analysis here is across locations rather than across firms. Thus, whereas previous *Grant* was previously regressed on an indicator for whether the firm filing the application later hired the examiner, here it is regressed on an indicator for whether the filing firm is in the same city or ZIP code as the as the firm that hired the examiner. City fixed effects replace firm fixed effects. Recall that all observations in which the filing firm later hired the examiner are dropped. This limits or eliminates the ability of the examiner-firm match-specific preferences that affect both granting and hiring to drive the results.

[Table VI about here.]

Table VI shows that half or more of the leniency that revolving door examiners extend to the firms that hire them is extended to, on average, all other firms in the area as well. The final column, for example, which includes both examiner and city fixed effects, reports that revolving door examiners grant 7.4% (5.0 percentage points) more patents to firms in the same ZIP code as the firm that hired them. They also grant 3.5% (2.3 percentage points) more patents to firms in the same city (but not ZIP code) as the firm that hired them. The coefficient on the indicator for whether the examiner became a practitioner is virtually unchanged from the prior table.

Robustness to sample restrictions

Many filings are submitted by firms that never hire an examiner. Large, specialized intellectual property law firms are more likely to be among those hiring. A potential confounding factor in this analysis arises because revolving door examiners may be particularly enthusiastic about the inventions on which hiring firms' applications are based, even conditional on the type of technology. Columns 1-2 of Table VII address this concern. They replicate columns 2 and 4 of Table V but restrict the sample to firms that hired at least one examiner. This cuts the sample size by about seventy percent. If the results of Table V are merely driven by the fact that revolving door examiners are drawn to the work of firms who are likely to hire, then the coefficients in the second row of Table VII will be much lower than those found in Table V. However, the overall magnitude and precision are essentially the same. This suggests the aforementioned concern is second order.

[Table VII about here.]

The geographic distribution of applications gives rise to a similar concern. Cities may produce fundamentally different types of technology, even within the narrowly-defined scope of an art unit, and revolving door examiners may be especially enthusiastic about the specific kind of technology coming from cities that are otherwise the most likely to hire an examiner. Columns 3-4 of Table VII address this concern. They replicate columns 2 and 4 of Table V but restrict the sample to cities in which at least one examiner was hired. This cuts the sample size by about forty percent but results in estimates that are very close to those reported in the earlier table.

Are examiners more lenient when firms are actively hiring?

Unlike idiosyncratic preferences, regulatory capture should cause the effects above to be strongest when the revolving door is actually turning—i.e. when private sector firms are actively hiring examiners. To test this, we plot the coefficients of interest by year against a proxy for hiring intensity, year-over-year private seasonally-adjusted employment gains or losses.[37] Figure I reports this result of this exercise. On the top graph's y-axis, we plot the coefficients estimated on the terms that interact the year with an indicator for whether the filing firm hired the examiner. On the bottom graph's y-axis, we plot the coefficients estimated on the terms that interact the year with an indicator for whether the filing firm resides in a city in which the examiner was hired (excluding, as usual, any observations in which the firm later hired the examiner). Specifications include examiner and year fixed effects. They also include firm and city fixed effects in the top and bottom graph, respectively. The x-axis of both graphs plots the employment changes.

[Figure I about here.]

The figure shows that regardless of whether one plots firm-based or location-based effects by year, the main effects estimated above load on periods where hiring is robust. In the years suffering either weak gains or even losses, firm-based estimates average out to 7.4 percentage points and the city-based estimates average out to only 1.4 percentage points. In the years afforded strong gains, however, these numbers are two and three times as large, respectively. The firm-based estimates average out to almost 16 percentage points, and the city-based estimates average out to around 5.2 percentage points. Economy-wide employment changes are not a perfect proxy for the intensity of private sector interest in examiners, although they likely provide a good first-order approximation.[38] For an alternative x-axis specification that relies on the number of examiners actually hired in our data, see the Appendix.

[37]US Dept. of Labor Bureau of Labor Statistics. "Current employment survey." http://www.bls.gov (retrieved July 29, 2017).

[38]No perfect measure exists in the BLS data, so we find this preferable to making sharp cuts of that data based on subjective calls. For example, the left-most three x-axis values correspond to the Great Recession (2009 and 2010) and the tail end of the "Dot-com crash" (2002).

Additional support from educational backgrounds

Most examiners join the agency directly after obtaining their undergraduate or graduate degree(s). While the USPTO presented the best job opportunity upon graduation, valuable other opportunities arise after a few years of experience to examiners who would like to become practitioners. Prior survey-based studies indicate that workers prefer jobs near where they have lived and/or were educated.[39] In our context, this means that revolving door examiners want to work at firms that are closer, on average, to their *alma maters*. Moreover, if revolving door examiners exhibit leniency towards prospective employers, then they will grant more patents to firms that are closer, on average, to where they were educated. Assuming that the location of one's *alma mater* is mostly or otherwise uncorrelated with grant-related behavior, at least within the narrowly-defined type of technology that one is exposed to within the USPTO, we can exploit these facts to lend independent support to our findings.

We start by assessing whether the location of revolving door examiners' *alma maters* does, in fact, predict the location of the firm that subsequently hires them. Figure II establishes this connection. To maintain continuity with subsequent results, the unit of observation is the application. The x-axis measures the distance between the filing firm and the examiner's *alma mater*, while the y-axis measures the distance between the filing firm and the examiner's subsequent employer. The (unambiguously) positive relationship indicates that, holding fixed the filing firm, a revolving door examiner that was educated nearby is likely to join a firm that is nearby (and vice versa). This is consistent with revolving door examimers applying to and accepting offers from firms that are, for example, in close proximity to family and friends or in a city with which they are already familiar.[40]

[Figure II about here.]

Given what we learn from Figure II, the remaining empirical question is whether examiners grant more patents to firms near their school. Column 1 of Table VIII indicates this is so. *Grant* is increasing in the distance between filing firms and revolving door examiners' *alma maters*. Fixed effects at the examiner, city, and year level address concerns discussed in the main specifications above. Schools and firms located in Hawaii create a long, thin right tail in the distribution of distances, so to ensure these

[39] Boyd, Lankford, Loeb, and Wyckoff [2005] shows that most public school teachers take a job close to their hometowns or their *alma mater*, and Reininger [2012] shows this extends to college graduates more generally. A survey in Richardson Jr [1966] states that the preference among University of North Carolina graduates to stay in the Raleigh-Durham area may reflect a desire to preserve "the Southern way of life."

[40] We are agnostic as to precisely what drives the relationship. Proximity to family and friends is one likely explanation, especially considering that many examiners attend public colleges, so their *alma maters* is likely to be in the same state as where they grew up. (See http://www.collegexpress.com/lists/list/percentage-of-out-of-state-students-at-public-universities/360/ (retrieved January 2, 2017).

observations are not driving our results, we winsorize distances at the top 1%. This reduces the top end of the support from 5,100 miles to just over 2,500 miles—effectively redistributing the distances to and from Hawaii to the largest values within the continental United States—but has only a trivial impact on the coefficients and their standard errors.

[Table VIII about here.]

These findings do, however, raise the question of whether the results are simply driven by "boosterism"—a desire to support people and firms in close geographic proximity to their school for reasons that have nothing to do with gaining favor among prospective employers. To address this concern, we also include observations for which we have data on the distance between filing firms and non-revolving door examiners' *alma maters*. If boosterism rather than capture drives the aforementioned results, then *Grant* will be increasing in the distance between the filing firm and a *non*-revolving door examiners' *alma mater* as well. Column 2, however, reports that these coefficients differ sharply. The coefficient on the revolving door examiners' firm-school distance is negative and significant at under a 5% level, i.e. it remains virtually unchanged from the prior specification. The coefficients on the non-revolving door examiners' firm-school distance, though, is less than one-sixth as large, positive, and not statistically different from zero.

How is the intensive margin of patent protection affected?

Thus far we have considered only the extensive margin of intellectual property protection, i.e. whether an application resulted in a grant. Examiners also influence the scope of what the patent protects.[41] The USPTO Office of Chief Economist proposes two simple measures for scope: the length of the shortest independent claim and the number of independent claims [Marco, Sarnoff, and deGrazia, 2016]. During patent prosecution, strict examination equates to adding qualifying language to the broadest claim, thereby lengthening it, and to removing claims altogether. Lenient examination does not. While the study of patent scope is undoubtedly important to understanding intellectual property protection [Freilich, 2016, Kuhn, 2016], our discussion here is brief. On the one hand, the currently-available claims data from the USPTO supports previous findings in this paper—the estimates are signed in the direction of regulatory capture, with economically meaningful magnitudes and in most cases statistical significance, as described below. On the other hand, the first release of the claims data contains parsing errors. The underlying text comprises millions of claims with technical symbols

[41] We thank Iain Cockburn, Josh Lerner, and Carl Shapiro for each suggesting we consider the effects on scope.

and non-standard characters, so fully automated parsing is very difficult and manual correction is unfeasible. However, minor errors result in large skew in the outcome measures. For this reason, we summarize our claims-related findings here, leaving the accompanying tables and more detailed discussion to the Appendix.

Our sample comprises about 457,000 applications that result in grants.[42] The difference in the length of the shortest claim between the filed application and the published grant provides one measure of narrowing scope. On average, patent prosecution increases the shortest claim by about 65 words. In cases where the filing firm later hires the examiner, however, 11.5 to 13.9 fewer words are added. In cases where the filing firm resides in a ZIP code as the hiring firm (but is not the hiring firm), 4.7 to 8.5 fewer words are added. If we study firms in the same city (but outside the ZIP code), 0.2 to 4.2 fewer words are added. The ranges given span specifications where raw, 2.5% winsorized, and 5% winsorized differences are used on the left-hand side. All include year and examiner fixed effects as well as either firm or city fixed effects (depending on the specification).

The difference in the number of independent claims between the filed application and the published grant provide the other measure of narrowing scope. On average, patent prosecution removes 2.1 independent claims. In cases where the filing firm later hires the examiner, however, 0.4 to 1.4 fewer claims are removed. In cases where the filing firm resides in a ZIP code as the hiring firm (but is not the hiring firm), 0.3 to 1.1 fewer claims are removed. If we study firms in the same city (but outside the ZIP code), 0.1 to 0.3 fewer claims are removed.

6 Effects on patent quality

We now turn to the question of whether this leniency translates into lower quality grants, which we assess using citation counts. Table IX reports the estimates of Equations 5 and 6 on these outcomes, respectively. The negative binomial, quasi-maximum likelihood Poisson, and linear model estimates appear in columns 1-2, 3-4, and 5-8, respectively. Each specification includes year fixed effects and includes either firm or city fixed effects. The non-linear specifications include examiner level controls, while the linear specifications include either examiner level controls or examiner fixed effects.[43]

[42]Summary statistics and estimates will differ slightly from the prior version of the paper, primarily due to our correcting for cancelled claims. See Appendix 3 for more details.

[43]The non-linear models rendered optimization of a very large number of fixed effects infeasible. The proximity of the linear model coefficients—estimated with and without examiner fixed effects—gave us confidence, however, that the non-linear model estimates, were we able to estimate them, would not stray too far from the those that appear in the first four columns. This also forced us to use *Technology center* rather than *Patent class* fixed effects. For the sake of comparability, we used *Technology center* fixed effects in the final four here columns as well. In the Appendix, we re-estimate the linear model using *Patent class* fixed

[Table IX about here.]

Across all specifications we see that the patents awarded by revolving door examiners to their future and prospective employers receive many fewer citations—i.e. are of much lower quality—than the others they grant. To be precise, patents awarded to future employers receive 19-24% fewer citations, while those awarded to firms in the same ZIP code and city as the future employer receive 6-8% and 6-10% fewer. The linear model provides similar size effects.[44] Also across specifications, we see that the absolute value of the coefficients on the indicator for whether the filing firm hired the examiner are roughly twice as large as the coefficients on the indicators based on close proximity to the hiring firm, which squares with earlier findings related to grant behavior.

A final feature of the table that deserves discussion are the positive coefficients estimated on the indicator for revolving door examiners. These imply that patents granted by revolving door examiners receive roughly 6-9% more citations relative to those granted by their colleagues. This figure is much smaller in nearly all cases (in absolute value terms) than any of the coefficients on the variables we relate to capture, i.e. the indicators for whether a filing firm hired the revolving door examiner or was in close proximity to the one who did. One way to interpret this result is that revolving door examiners make higher quality grant decisions overall, except on patents related to future or prospective employers. Kempf [2015] reaches an analogous conclusion, albeit in a different, private sector setting. We are reluctant to infer too much from mean differences across examiners, although this finding should give regulators pause when contemplating policies that would restrict *ex post* employment options.

7 Conclusion

Many regulatory agency employee decisions affect firms for whom these employees hope to subsequently work. Societal norms, tacit collusion, or explicit agreements may pressure them to "go easy" on their prospective employers, creating a conflict of interest for individuals tasked with protection of the public interest and impartial supervision of economic activity. As a result, understanding the revolving door phenomenon and its effects is an important theoretical and empirical matter. Moreover,

effects and show that the coefficients differ very little—typically by 5-10% and at most by 30%. This, again, gave us confidence that the non-linear estimates are insensitive to the modeling choice.

[44] Revolving door examiners are present in greater proportions in the beginning of the panel relative to the end, and forward citations are increasing in the amount of time that has passed since the patent was granted. Hence, for revolving door examiners, a given within-examiner percent increase or decrease in citations will equate to, in absolute value terms, a larger level change than it would were revolving door examiners evenly dispersed across the longitudinal dimension of the panel. Thus, the coefficients reported in columns 5-8 appear large relative to the mean number of citations reported in Table II but are, in fact, consistent with the coefficients reported in columns 1-4.

policymakers have expressed and exhibited some willingness to address this problem.[45]

Data from the US Patent and Trademark Office provide some insight into the process. Many agency employees join the very firms whose patent applications they previously examined, and appear to treat the applications from these firms differently, e.g. by granting them more patents than they grant to other firms. This alone is not proof of rent-seeking behavior, although we argue that the preponderance of other evidence here points towards regulatory capture. For example, the relationship is robust to our treatment of examiner- and firm-specific unobservable attributes, extends to prospective employers, tends to abate in periods when firms are not hiring, and mirrors other measures of regulatory leniency that are on the intensive margin. Policy changes—for example, varying the level of agency compensation, monitoring, or post-employment restrictions—can have unintended consequences, so these results on their own cannot advocate for one specific policy over another. However, we hope they compel additional research in this area.

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Figures

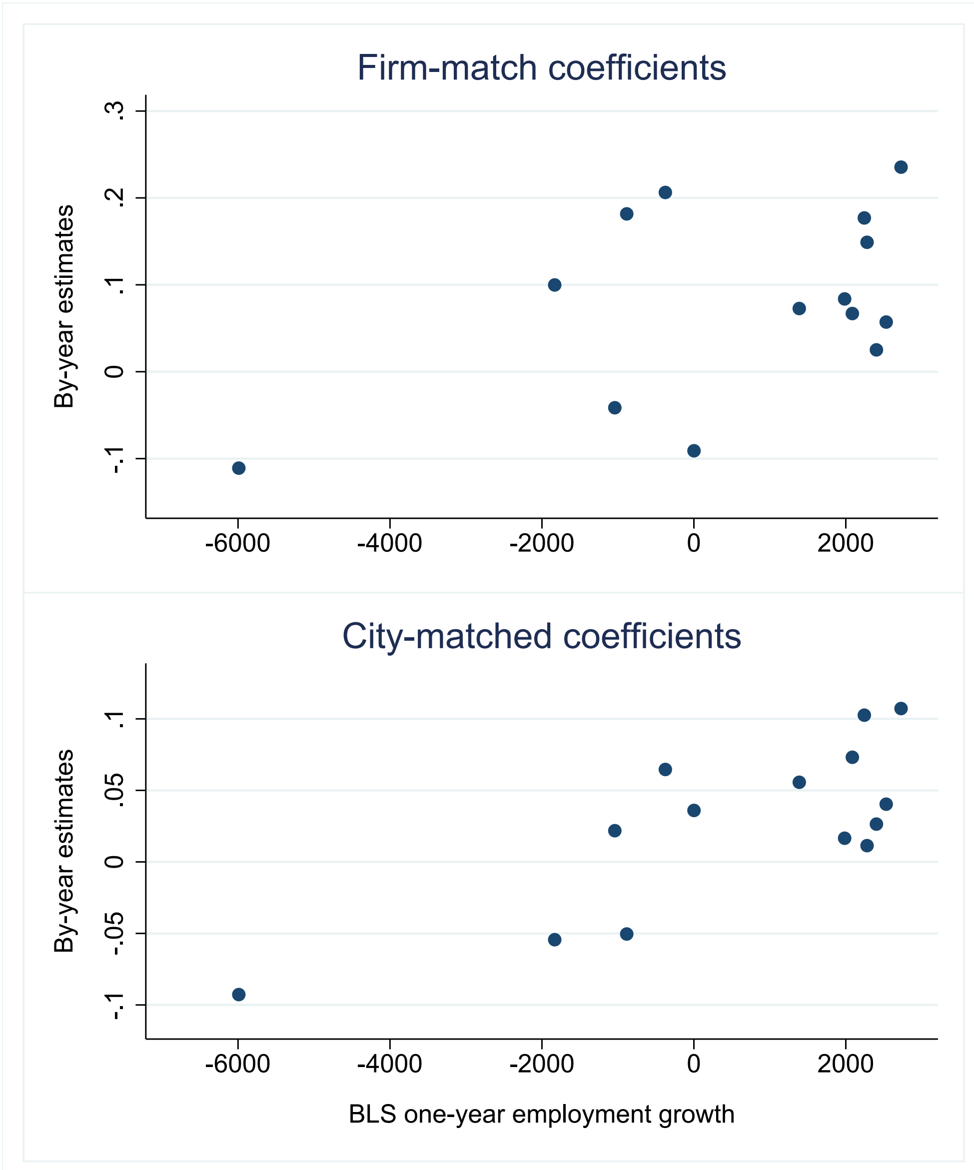


Figure I: *Effects are stronger in periods when hiring is more likely*

To arrive at the top graph, we regress Grant on an indicator for whether the examiner was later hired as a practitioner interacted with the year, an indicator for whether the filing firm hired that examiner interacted with the year, as well as fixed effects at the year, examiner, and firm level. We then plot the second set of interactions along the y-axis. To arrive at the bottom graph, we repeat this exercise, but we replace the second set of interactions with an indicator for whether the filing firm resides in a city in which the examiner was later hired interacted with the year, substitute city fixed effects for firm fixed effects, and restrict the sample to observations in which the filing firm did not hire the examiner. The x-axis measures the year-over-year change in private seasonally-adjusted US employment (in thousands), which proxies for the likelihood that filing firms are looking to hire examiners.

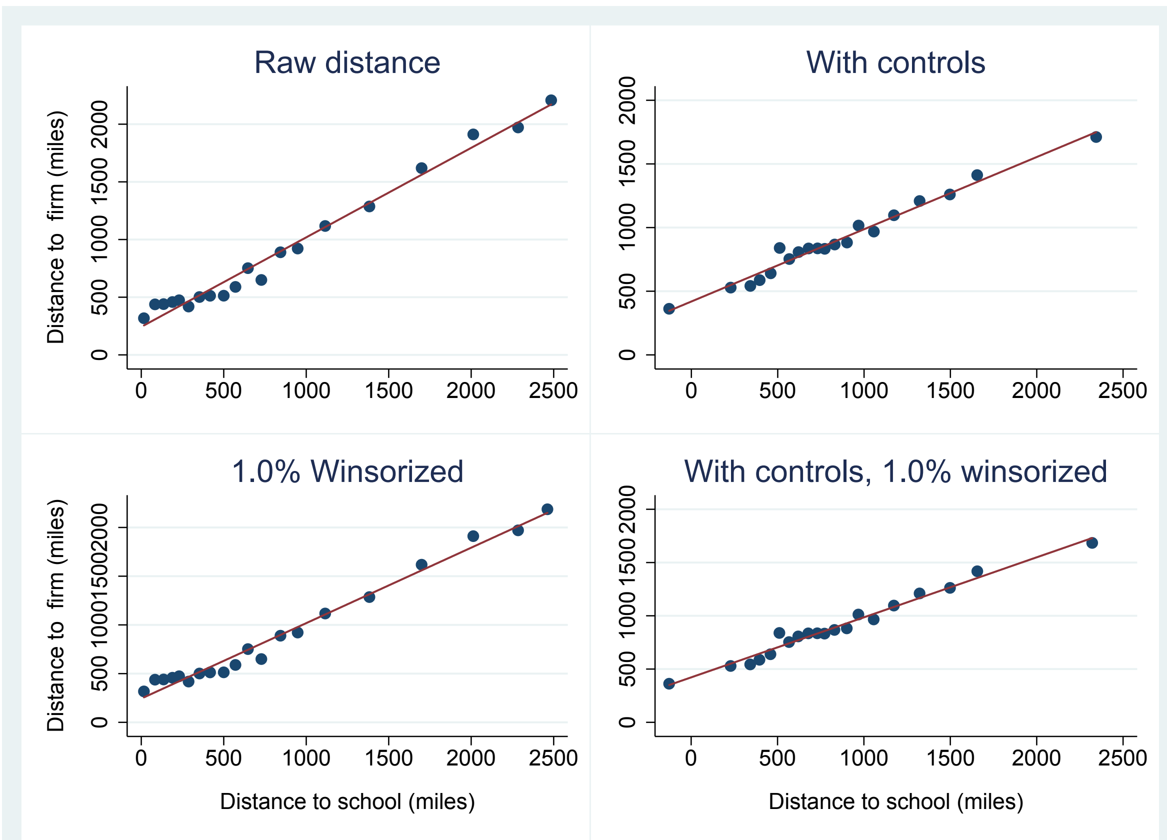


Figure II: *Where an examiner is educated predicts his or her post-USPTO employment location*

The unit of observations is the application (to remain consistent with forthcoming specifications). In the upper left-hand graph, the x-axis measures the distance from the filing firm to the examiner alma mater, and the y-axis measures the distance from the filing firm to the the firm that later hires the examiner. An increasing slope indicates that revolving door examiners tend to work, on average, close to where they were educated. In the upper right-hand and lower right-hand graphs, both the x-axis and y-axis measures are residualized values after controlling for the year, the identity of the examiner, and the identity of the city. In the lower left-hand and lower right-hand graphs, the x-axis and y-axis measures are winsorized at 1%.

Tables

Table I: *Summary of data that assesses grant behavior*

	N	Mean	Std. Dev.	Minimum	Maximum
Grant	1,023,669	0.63	0.48	0	1
Year	1,023,669	2011.39	3.03	2001	2016
Experience	1,023,669	6.37	2.27	0	10
1[Firm hires an examiner]	1,023,669	0.29	0.45	0	1
1[Revolving examiner]	1,023,669	0.07	0.25	0	1

This data based covers the period from November 2001 through the end of 2015, i.e. period for which PatEx has complete application data for both granted and abandoned patents. See the text, above, for variable definitions.

Table II: *Summary of data that assesses quality*

	N	Mean	Std. Dev.	Minimum	Maximum
Citations	727,920	11.57	36.40	0	3072
Year	727,920	2010.04	5.03	1995	2016
Experience	727,920	6.30	2.35	0	10
1[Firm hires an examiner]	727,920	0.29	0.45	0	1
1[Revolving examiner]	727,920	0.11	0.31	0	1

This data covers filings from July 1995 through the end of 2015, i.e. all observations subsequent to the June 1995 law change affecting patent terms. See the text, above, for variable definitions.

Table III: *Firms where revolving examiners are most frequently hired*

City	Count		Mean	
	Examiner hires	Filed applications	Grant	Citations
Banner Witcoff	10	3941	.69	9.27
Birch Stewart	15	10782	.62	7.21
Buchanan Ingersoll	12	4744	.66	11.8
Cooley	8	2117	.64	19.7
Finnegan Henderson	48	7127	.54	13.0
Fish & Richardson	13	11262	.68	12.5
Fitzpatrick	9	7452	.77	10.0
Foley & Lardner	13	9835	.68	14.0
Greenblum	9	3048	.63	7.44
Harness Dickey	11	10864	.67	7.25
Harrity & Harrity	11	1050	.88	8.32
Hunton	10	818	.57	17.3
Knobbe	8	9855	.64	18.2
Lee & Morse	11	3994	.68	11.8
McDermott	8	7922	.58	10.3
Morgan Lewis	9	8449	.67	9.44
Oblon McClelland	17	17343	.69	7.96
Oliff	13	10228	.73	8.05
Sterne Kessler	15	3235	.76	12.6
Sughrue	17	13602	.57	7.78
Townsend (merged)	8	9426	.67	14.9
Venable	8	1506	.60	8.44

"Examiner hires" counts the number of examiners who joined a firm in this city directly after leaving the employ of the USPTO. "Filed applications" counts the number of filings originating from this firm. Firm names are abbreviated. The firm of Townsend and Townsend and Crew merged with the larger firm of Kilpatrick Stockton to become Kilpatrick Townsend & Stockton. Here "Townsend" refers to the former of the pre-merger entities while "Kilpatrick" refers to the latter of the pre-merger entities and the post-merger entity.

Table IV: *Locations where revolving examiners are most frequently hired*

	Count		Mean	
City	Examiner hires	Filed applications	Grant	Citations
Alexandria, VA	68	56844	.64	8.51
Arlington, VA	14	11832	.70	13.3
Atlanta, GA	16	14224	.54	19.2
Austin, TX	7	20017	.75	12.5
Baltimore, MD	10	911	.51	13.8
Bethesda, MD	14	5201	.63	8.73
Boston, MA	30	26165	.61	14
Chicago, IL	39	40262	.68	18.0
Cleveland, OH	14	14124	.62	10.6
Dallas, TX	20	26308	.63	11.1
Denver, CO	8	5894	.69	14.3
Dist. of Columbia	299	112572	.82	18.6
Fairfax, VA	41	22387	.60	6.80
Houston, TX	14	34027	.62	9.44
Irvine, CA	12	27916	.64	13.6
McLean, VA	28	20313	.58	7.96
Minneapolis, MN	14	50585	.66	12.2
New York City, NY	46	43561	.62	12.3
Philadelphia, PA	24	15106	.60	13.5
Pittsburgh, PA	10	5440	.58	11.6
Reston, VA	47	25121	.62	6.21
San Diego, CA	19	12125	.68	10.5
San Francisco, CA	30	32572	.66	14.4
San Jose area, CA	27	34411	.68	13.9
Seattle, WA	13	13101	.62	14.7
Troy, MI	10	11938	.64	9.63

"Examiner hires" counts the number of examiners who joined a firm in this city directly after leaving the employ of the USPTO. "Filed applications" counts the number of filings originating from this city.

Table V: *Examiners grant more patents to firms that hire them*

VARIABLES	(1) All firms	(2) All firms	(3) All firms	(4) All firms
1[Revolving examiner]	.0458***	.0447***		
	(.00562)	(.00566)		
1[Filing firm hires examiner]	.119***	.114***	.0907***	.0845***
	(.0297)	(.0236)	(.0326)	(.0258)
Observations	1,023,660	1,023,660	1,023,287	1,023,287
R-squared	.126	.14	.204	.216
Year FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Patent class FE	Yes	Yes	–	–
Experience FE	Yes	Yes	–	–
Examiner FE	No	No	Yes	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Grant is on the left-hand side of the estimating equation. The right-hand side measures comprise an indicator for whether an examiner was later hired as a practitioner (in the first row of coefficients) and an indicator for whether the filing firm later hired the examiner (in the second row of coefficients). Standard errors are clustered at the examiner and firm level.

Table VI: *Examiners grant more patents to firms in close proximity the ones that hire them*

VARIABLES	(1) All firms	(2) All firms	(3) All firms	(4) All firms
1[Revolving examiner]	.0447***	.0429***		
	(.00697)	(.0069)		
1[Filing ZIP hires examiner]	.0658***	.0745***	.0419***	.0497**
	(.019)	(.0246)	(.014)	(.0194)
1[Filing city, not ZIP, hires examiner]	.0197**	.0341***	.0102	.023**
	(.0081)	(.00947)	(.00847)	(.00993)
Observations	1,023,489	1,023,439	1,023,116	1,023,066
R-squared	.126	.144	.204	.219
Year FE	Yes	Yes	Yes	Yes
City FE	No	Yes	No	Yes
Patent class FE	Yes	Yes	–	–
Experience FE	Yes	Yes	–	–
Examiner FE	No	No	Yes	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Grant is on the left-hand side of the estimating equation. The right-hand side measures comprise an indicator for whether an examiner was later hired as a practitioner (in the first row of coefficients) and indicators for whether the firm resides in a ZIP code or city in which the examiner was later hired (in the second and third rows of coefficients, respectively). Columns 2 and 4 include city fixed effects, while columns 3-4 include examiner fixed effects. The sample is restricted to observations where the filing firm did not subsequently hire the examiner. Standard errors are clustered at the examiner and city level.

Table VII: *Estimates are robust to limiting the sample to hiring firms or cities*

VARIABLES	(1) Hiring firms	(2) Hiring firms	(3) Hiring cities	(4) Hiring cities
1[Revolving examiner]	.0386***		.0448***	
	(.00827)		(.00731)	
1[Filing firm hires examiner]	.113***	.078***		
	(.0233)	(.0295)		
1[Filing ZIP hires examiner]			.0715***	.0503**
			(.0234)	(.0208)
1[Filing city, not ZIP, hires examiner]			.0343***	.0237**
			(.00982)	(.0102)
Observations	295,813	295,297	697,294	696,856
R-squared	.152	.244	.131	.213
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
City FE	No	No	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Columns 1-2 restrict the sample to filing firms that hired at least one examiner. Columns 3-4 restrict the sample to cities that did the same. Grant is on the left-hand side of the estimating equation. The right-hand side measures comprise an indicator for whether an examiner was later hired as a practitioner (in the first row of coefficients), an indicator for whether the filing firm later hired the examiner (in the second row of coefficients), and indicators for whether the firm resides in a city or ZIP code in which the examiner was later hired (in the third and fourth rows of coefficients, respectively). As in prior tables, for our location-based specifications, i.e. those whose results are reported in columns 3-4 here, the sample is restricted to observations where the filing firm did not subsequently hire the examiner. Standard errors are clustered at the examiner and firm level in columns 1-2 and at the examiner and city level in columns 3-4.

Table VIII: *Revolving door examiners grant more patents to firms closer to their alma maters*

VARIABLES	(1) Distance	(2) Distance	(3) 1.0% Wins. distance	(4) 1.0% Wins. distance
Filing firm distance to revolver's school (miles)	-.00843**	-.00849**		
	(.00331)	(.0033)		
Filing firm distance to non-revolver's school (miles)		.00139		
		(.00284)		
Filing firm distance to revolver's school (1.0% Wins. miles)			-.0098**	-.00988**
			(.00423)	(.00422)
Filing firm distance to non-revolver's school (1.0% Wins. miles)				.00132
				(.0029)
Observations	952,162	1,084,104	952,162	1,084,104
R-squared	.221	.22	.221	.22
Year FE	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Examiner FE	Yes	Yes	Yes	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

"Revolving examiner" is abbreviated "revolver" here. Grant is on the left-hand side of the estimating equation. On the right-hand side, we place the distance from the filing firm to the examiner's alma mater. In the first row, this distance is specific to revolving door examiners, and in the second row, it is specific to non-revolving door examiners, i.e. those that stay employed at the USPTO or subsequently join a government entity. To assess robustness to outliers, i.e. principally those derived from firms and schools in Hawaii and Alaska, we also winsorize the the left- and right-hand side measures at 1%. Those results are reported in the third and fourth rows and columns. Standard errors are clustered at the examiner and city level.

Table IX: *Patents granted to subsequent employers receive fewer citations*

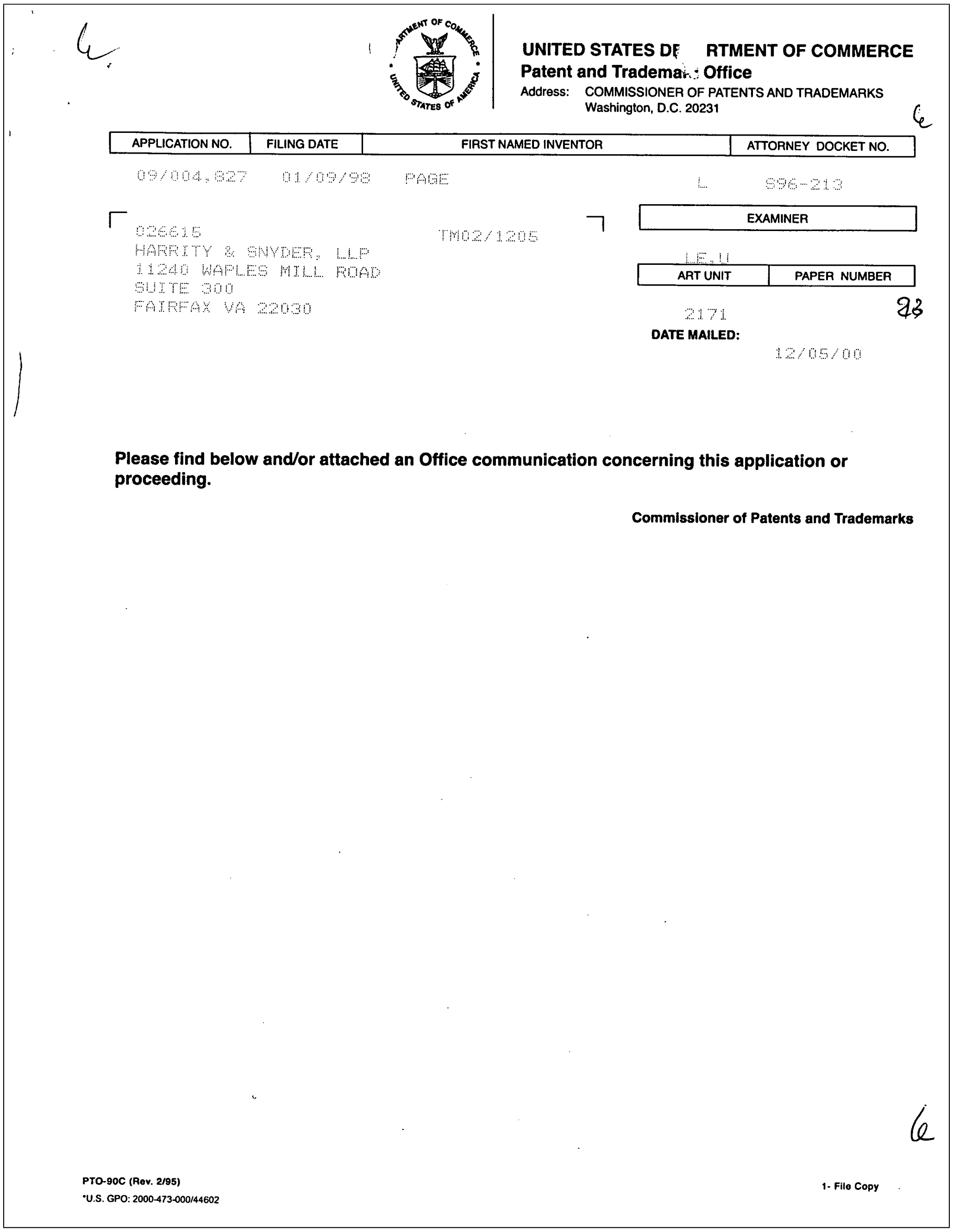
VARIABLES	(1) Neg. bin. count	(2) Neg. bin. count	(3) Poisson count	(4) Poisson count	(5) Count	(6) Count	(7) Count	(8) Count
1[Revolving examiner]	.0552***	.0585***	.085***	.0893***	2.03**		2.34**	
	(.00745)	(.00741)	(.00628)	(.0129)	(.811)		(1.17)	
1[Filing firm hires examiner]	-.21**		-.272***		-9.78***	-6.3***		
	(.0995)		(.104)		(2.32)	(2.11)		
1[Filing ZIP hires examiner]		-.0781		-.0573			-4.73***	-2.11*
		(.0706)		(.0724)			(1.47)	(1.08)
1[Filing city, not ZIP, hires examiner]		-.0596*		-.107***			-6.87***	-4.44***
		(.0309)		(.029)			(.934)	(.766)
Observations	727,920	727,694	727,920	727,610	727,920	727,335	727,616	727,032
R-squared					.19	.325	.194	.327
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No	Yes	Yes	No	No
City FE	No	Yes	No	Yes	No	No	Yes	Yes
Technology center FE	Yes	Yes	Yes	Yes	Yes	–	Yes	–
Experience FE	Yes	Yes	Yes	Yes	Yes	–	Yes	–
Examiner FE	No	No	No	No	No	Yes	No	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The left-hand side measure is the count of forward citations from other patents. The right-hand side measures comprise an indicator for whether an examiner was later hired as a practitioner (in the first row of coefficients), an indicator for whether the filing firm later hired the examiner (in the second row of coefficients), and indicators for whether the firm resides in a city or ZIP code in which the examiner was later hired (in the third and fourth rows of coefficients, respectively). As in prior tables, for our location-based specifications, i.e. those whose results are reported in even-numbered columns here, the sample is restricted to observations where the filing firm did not subsequently hire the examiner. Robust standard errors are provided for the non-linear specifications; in the other columns, standard errors are clustered at the examiner and either firm or city level, depending on the specification. Note that the within-examiner level changes reported in columns 5-8 appear large relative to the mean number of citations reported in Table II but are, in fact, in line with the percent changes implied by columns 1-4. See Footnote 44 for a brief explanation.

Appendix A: Selected examiner-firm communication

Example 1: Cover page of non-final rejection re: Google application for PageRank (1998)



UNITED STATES DE RTMENT OF COMMERCE
Patent and Tradema. Office
Address: COMMISSIONER OF PATENTS AND TRADEMARKS
Washington, D.C. 20231

APPLICATION NO.	FILING DATE	FIRST NAMED INVENTOR	ATTORNEY DOCKET NO.
09/004,827	01/09/98	PAGE L	S96-213

026615 TM02/1205
HARRITY & SNYDER, LLP
11240 WAPLES MILL ROAD
SUITE 300
FAIRFAX VA 22030

EXAMINER	
LE,U	
ART UNIT	**PAPER NUMBER**
2171	23

DATE MAILED: 12/05/00

Please find below and/or attached an Office communication concerning this application or proceeding.

Commissioner of Patents and Trademarks

PTO-90C (Rev. 2/95)
*U.S. GPO: 2000-473-000/44602

1- File Copy

Example 2: Cover page of non-final rejection re: GoPro application camera attachment to body (2004)

UNITED STATES PATENT AND TRADEMARK OFFICE

UNITED STATES DEPARTMENT OF COMMERCE
United States Patent and Trademark Office
Address: COMMISSIONER FOR PATENTS
P.O. Box 1450
Alexandria, Virginia 22313-1450
www.uspto.gov

APPLICATION NO.	FILING DATE	FIRST NAMED INVENTOR	ATTORNEY DOCKET NO.	CONFIRMATION NO.
10/777,287	02/11/2004	Nicholas D. Woodman	23769-07988	5427

758 7590 01/19/2005

FENWICK & WEST LLP
SILICON VALLEY CENTER
801 CALIFORNIA STREET
MOUNTAIN VIEW, CA 94041

EXAMINER	
GRAY, DAVID M	

ART UNIT	PAPER NUMBER
2851	

DATE MAILED: 01/19/2005

Please find below and/or attached an Office communication concerning this application or proceeding.

PTO-90C (Rev. 10/03)

Example 3: Cover page of non-final rejection re: Theranos application for rapid diagnosis (2005)

UNITED STATES PATENT AND TRADEMARK OFFICE

UNITED STATES DEPARTMENT OF COMMERCE
United States Patent and Trademark Office
Address: COMMISSIONER FOR PATENTS
P.O. Box 1450
Alexandria, Virginia 22313-1450
www.uspto.gov

APPLICATION NO.	FILING DATE	FIRST NAMED INVENTOR	ATTORNEY DOCKET NO.	CONFIRMATION NO.
11/202,206	08/12/2005	Elizabeth A. Holmes	30696-704.401	7759

21971 7590 03/03/2011
WILSON, SONSINI, GOODRICH & ROSATI
650 PAGE MILL ROAD
PALO ALTO, CA 94304-1050

EXAMINER	
LAM, ANN Y	

ART UNIT	PAPER NUMBER
1641	

MAIL DATE	DELIVERY MODE
03/03/2011	PAPER

Please find below and/or attached an Office communication concerning this application or proceeding.

The time period for reply, if any, is set in the attached communication.

PTOL-90A (Rev. 04/07)

Example 4: Cover page of non-final rejection re: Square application for mobile credit capture (2010)

UNITED STATES PATENT AND TRADEMARK OFFICE

UNITED STATES DEPARTMENT OF COMMERCE
United States Patent and Trademark Office
Address: COMMISSIONER FOR PATENTS
P.O. Box 1450
Alexandria, Virginia 22313-1450
www.uspto.gov

APPLICATION NO.	FILING DATE	FIRST NAMED INVENTOR	ATTORNEY DOCKET NO.	CONFIRMATION NO.
12/903,801	10/13/2010	Sam Wen	SQU 0003	4648

77845 7590 11/14/2011
Goodwin Procter LLP
Attn: Patent Administrator
135 Commonwealth Drive
Menlo Park, CA 94025-1105

EXAMINER	
HAUPT, KRISTY A	

ART UNIT	PAPER NUMBER
2876	

NOTIFICATION DATE	DELIVERY MODE
11/14/2011	ELECTRONIC

Please find below and/or attached an Office communication concerning this application or proceeding.

The time period for reply, if any, is set in the attached communication.

Notice of the Office communication was sent electronically on above-indicated "Notification Date" to the following e-mail address(es):

Patentsv@goodwinprocter.com
dnakley@goodwinprocter.com

PTOL-90A (Rev. 04/07)

Appendix B: Abandoned patent coverage pre- and post-AIPA

The America Inventors Protection Act ("AIPA") was passed late-November 2000 and went into effect one year later. The USPTO publishes applications 18 months after filing. Prior to AIPA, if the application was abandoned before the 18 month point, then it was never published. Following AIPA, all applications are published (regardless of whether or when they are abandoned). As a result, applications filed before late-November 2001 and abandoned prior to the 18 month point are absent from our data. The first panel of Figure A.I shows the effect this has on the number of filings over the longitudinal dimension of the data. This creates obvious complications when determining the factors that affect the likelihood of receiving a patent [Graham, Marco, and Miller, 2015]. To sidestep the issue, the sample we use to assess grant behavior begins on December 1, 2000, the day after the legislation went into affect. The number of abandonments and grants is otherwise very stable over the panel, which is evident from the bottom panel of Figure A.I. This expands the window around the AIPA effective date to coverage from 1996 through 2006.

[Figure A.I about here.]

Appendix C: Effects by intensity of hiring

Figure I plotted the main effects by year along the y-axis, with year-over-year changes in private seasonally-adjusted US employment gains on the x-axis. Figure A.II replicates this figure but replaces the x-axis measure with a count of the private sector examiner hires we observe in the data. Again, we see a strong relationship between the intensity of hiring and size of the effects. See Section 5 for a more detailed discussion of such a relationship.

[Figure A.II about here.]

Appendix D: Examiner toughness at the extensive margin

Data sources

The Patent Claims Research Dataset provides application- and patent-level data including the number of independent claims and the length of the shortest independent claim.[46] The dataset covers applications,

[46] USPTO. https://www.uspto.gov/learning-and-resources/electronic-data-products/patent-claims-research-dataset (retrieved on May 30, 2017).

i.e. pre-grant publications, filed after November 29, 2000 and published before January 1, 2015, and it covers patents granted between January 1, 1976 and December 31, 2015. Since we are interested in the change in scope due to examination, we limit the sample to only applications and grants that match to one another. (There is currently no data on changes to claims for applications that are abandoned [Marco, Sarnoff, and deGrazia, 2016]). We further exclude observations that are obviously in error, i.e. applications or patents with zero independent claims, which is legally impermissable, and then impose identical restrictions on the data, e.g. that we confine ourselves to utility patents, to applications where we observe the examiner name, etc. (For the step-by-step instructions on panel construction, see Appendix F.)

Data summary

We observe valid claim-related measures for 456,079 application-patent pairs. The difference in the number of claims from application to grant is -0.33 with a standard deviation of 2.07. The minimum is -173, and the maximum is 51. Winsorizing the distribution at the 5% level shifts the mean to -0.26, the standard deviation to 1.15, the minimum to -3, and the maximum to 2. The difference in the minimum claim length from application to grant is 65.69 with a standard deviation of 93.27. The minimum is -6531, and the maximum is 2864. Winsorizing the distribution at the 5% level shifts the mean to 64.5, the standard deviation to 63.76, the minimum to -2, and the maximum to 217.

Estimating the effects on patent claims

The estimating equations are the same as equations 5 and 6, i.e. the specifications applied to citations.

Results

Tables A.II and A.I report the relevant claims-based results. They regress the two measures of scope suggested by the USPTO on indicators for whether the filing firm later hires the examiner, is in the same ZIP code as the firm that later hires the examiner, or is in the same city (but not ZIP code) as the firm that later hires the examiners.

[Table A.I about here.]

[Table A.II about here.]

All 18 coefficients are signed consistent with our earlier estimates. For example, revolving door examiners increase the shortest independent claim by 14 words and eliminate 1.42 fewer claims for firms for whom they later work. Also, the relationship between the coefficients on the three right-hand side variables is the same as in the grant-based tables. Some estimates are sensitive to outlier values, i.e. fall when we winsorize the left-hand side measures, although in many or most cases the precision increases approximately commensurate with the coefficients so that the significance is not very effected.

(FOR ONLINE PUBLICATION)

Appendix E: Robustness to variation in technology controls

Table IX conditions on *Technology center* rather than *Patent class* fixed effects. The latter are more granular and used throughout the rest of this paper. To provide us confidence that the estimates are not too sensitive to that choice, we replicate the final four columns but substitute in *Patent class* fixed effects.

[Table A.III about here.]

Table A.III reports those results. Columns 1 and 3 replicate columns 5 and 7 from Table IX (for the sake of comparison). Despite a sharp increase in the number of discrete controls, the coefficients move very little as we vary the technology controls.

(FOR ONLINE PUBLICATION)

Appendix F: Dataset construction

PatEx

PatEx consists of individual files (“application_data.dta,” ‘transactions.dta,” “foreign.dta,” and “correspondence.dta”) that are merged to one another using the variable “application_number.” Cleaning the data consists of removing a very small number observations, i.e. always < 0.1% of the total, related to missing or erroneous data, including instances where the

- examiner name is missing
- examiner name is erroneous, i.e. it is listed as “None” or “Not defined”
- correspondence information is missing, i.e. no ZIP code is listed
- correspondence information is erroneous, i.e. the ZIP code is neither five digits alone or five digits followed by a hyphen and four additional digits

- status indicates a patent was issued but an abandonment date is given
- status indicates an abandonment but an issue date is given
- issue date and abandonment date are missing
- filing date or disposal date is missing
- art unit is missing

As per Section 3 in the body of the paper and the accompanying references in this appendix, we then

- keep regular utility patent applications, i.e. exclude provisional, PCT, divisional, or continuation applications
- keep applications filed on or after December 1, 2000 and grants filed on or after July 1, 1995
- remove observations related to examiners whom we have matched with the practitioner rosters but cannot confirm using the biographical-related sources, i.e. agent/attorney directors or social media websites
- remove observations where there is likely to be a pre-existing grant-related decision by another Patent Cooperation Treaty (PCT) nation, specifically ones in which the foreign country priority date is more than 365 days earlier than the US country priority date
- remove applications related to examiners whose total tenure at the USPTO exceeds ten years
- remove observations where multiple examiners have the same name, which makes it impossible to disambiguate and subsequently match to other data, e.g. more than one "Brian Johnson," "John Lee," "Michael Anderson," and "Huong Nguyen" have been employed by the USPTO

Claims supplement

We begin with the full texts of all published application claims, "pgpub_claims_fulltext.dta." This will provide us data on claims cancelled prior to publication of the application (which are not in the summary file, "pgpub_document_stats.dta.", and not properly counted in the more detailed summary file, "pgpub_claims_stats.dta."). We keep any observations for which the *claim_text* includes "cancelled" and is shorter than 25 characters, the latter of which eliminates claims related to actually "cancelling" something object. *claim_text* then provides us with a count of the claims cancelled prior to publication. Note that oftentimes contiguous cancelled claims are summarized with a dash, e.g. "2-5" to indicate that claims two through five are all cancelled. (Note further that "pgpub_claims_stats.dta" counts these as a single cancelled claim.) We merge this file to the published applications claims data summary file, "pgpub_document_stats.dta," on application number. Since the summary file explicitly does not count cancelled claims, we add the number of published application claims from the summary file to the cancelled claims obtained from the full text file. This completes the first intermediate step.

We proceed with the published patents claims data file, "patent_document_stats.dta." We merge to "application_data.dta" from PatEx on the patent number and then drop a small number of observations for which the application number in the claims data does not match with the application number in PatEx. This completes the second intermediate step.

We merge files from the intermediate steps on application number. The data contains obvious (but understandable) parsing errors. The text of older applications and grants is not digitally stored; the technical nature of many or most mean that highly non-standard characters included, exacerbating the already-difficult task of counting words; and the the sheer volume of claim text makes it prohibitively large to manually compute. Small errors can create large measurement problems, though. For example, in certain instances especially lengthy claims were erroneously broken into a very large number of single word claims, sharply skewing our measures away from the truth. Instead of a large number of subjective corrections to the data, we merely apply to blunt corrections: we exclude a small number of observations for which the number of independent claims is zero or missing (both of which are legally impermissible for a valid application or valid grant), and we exclude a small number of observations for which the minimum number of words among independent claims is less than eight (which in the examples we manually checked was always the result of an error) or missing (which is, again, impermissible).

Figures

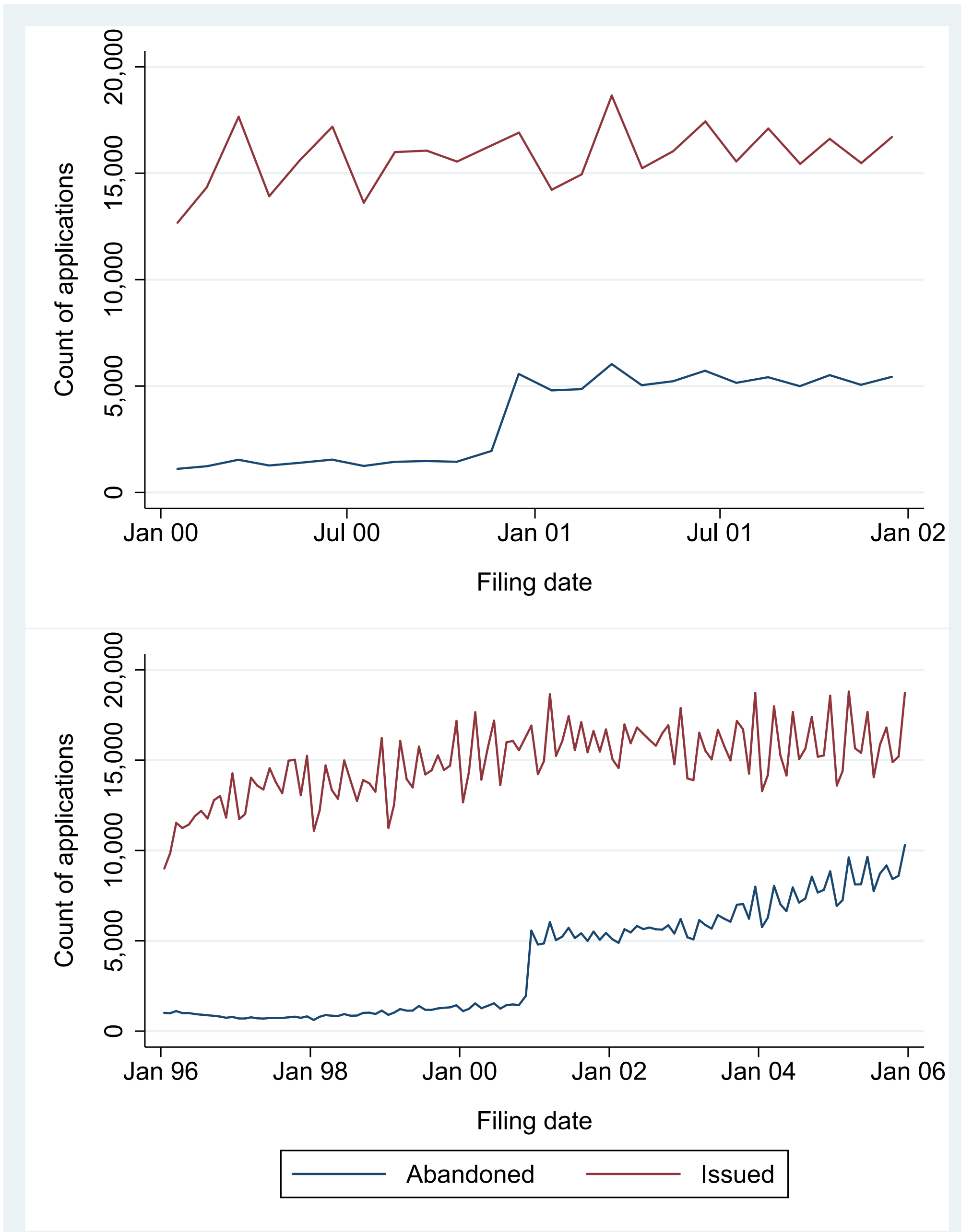


Figure A.I: *Applications that result in abandonments but not grants increase with AIPA legislation*

Values correspond to counts of applications by month and manner of eventual disposal, i.e. whether the application resulted in a grant or not. Note that the sharp increase in abandonments circa November 2000 is driven by a change in what applications are published rather than by a change in the grant rate.

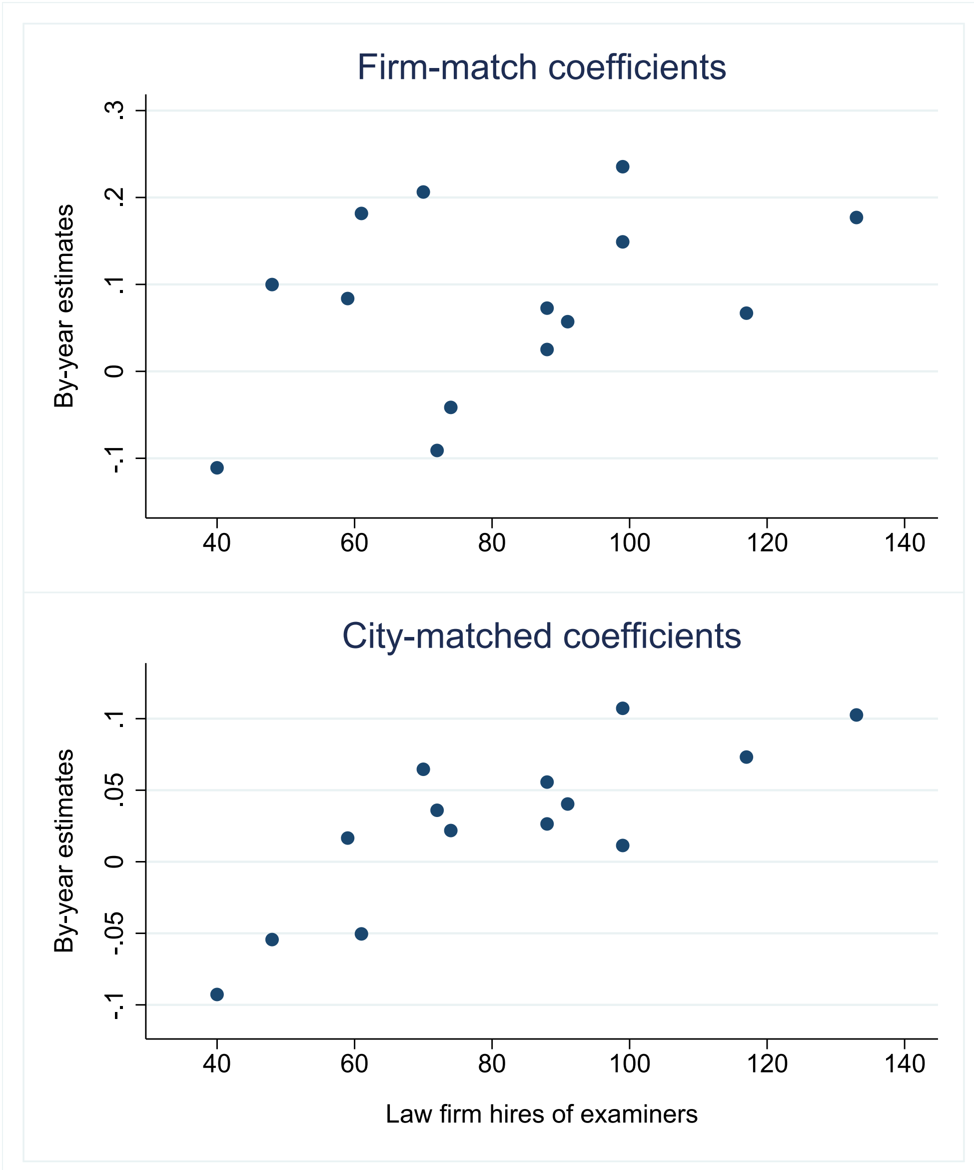


Figure A.II: *Effects are stronger in periods when hiring is more likely*

To arrive at the top graph, we regress Grant on an indicator for whether the examiner was later hired as a practitioner interacted with the year, an indicator for whether the filing firm hired that examiner interacted with the year, as well as fixed effects at the year, examiner, and firm level. We then plot the second set of interactions along the y-axis. To arrive at the bottom graph, we repeat this exercise, but we replace the second set of interactions with an indicator for whether the filing firm resides in a city in which the examiner was later hired interacted with the year, substitute city fixed effects for firm fixed effects, and restrict the sample to observations in which the filing firm did not hire the examiner. The x-axis counts the number of examiners hired by privat sector firms in that year.

Tables

Table A.I: *Patents granted to subsequent and prospective employers are allowed shorter independent claims*

VARIABLES	(1) Difference	(2) Difference	(3) 2.5% Wins. difference	(4) 2.5% Wins. difference	(5) 5.0% Wins. difference	(6) 5.0% Wins. difference
1[Filing firm hires examiner]	-13.9**		-12.7**		-11.5**	
	(6.21)		(5.72)		(5.53)	
1[Filing ZIP hires examiner]		-8.46***		-5.96**		-4.65*
		(2.35)		(2.49)		(2.48)
1[Filing city, not ZIP, hires examiner]		-4.22**		-1.04		-.234
		(1.76)		(1.39)		(1.3)
Observations	423,568	423,379	423,568	423,379	423,568	423,379
R-squared	.156	.158	.196	.198	.2	.202
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No	Yes	No
City FE	No	Yes	No	Yes	No	Yes
Examiner FE	Yes	Yes	Yes	Yes	Yes	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The left-hand side measure variable is the difference between the length of the shortest independent claim in the published patent and in the submitted application. More independent claims indicate more patent scope. Thus, the left-hand side measure gives one indication of how much scope is reduced in the patent prosecution process. That measure is winsorized at 2.5% in columns 3-4 and at 5% in columns 5-6. The right-hand side measures comprise an indicator for whether the filing firm later hired the examiner (in the first row of coefficients), and indicators for whether the firm resides in a city or ZIP code in which the examiner was later hired (in the second and third rows of coefficients, respectively). As in prior tables, for our location-based specifications, i.e. those whose results are reported in even-numbered columns here, the sample is restricted to observations where the filing firm did not subsequently hire the examiner. Standard errors are clustered at the examiner and firm level in odd-numbered columns and at the examiner and city level in even-numbered columns.

Table A.II: *Patents granted to subsequent and prospective employers are allowed more independent claims*

VARIABLES	(1) Difference	(2) Difference	(3) 2.5% Wins. difference	(4) 2.5% Wins. difference	(5) 5.0% Wins. difference	(6) 5.0% Wins. difference
1[Filing firm hires examiner]	1.42**		.698***		.438**	
	(.629)		(.263)		(.171)	
1[Filing ZIP hires examiner]		1.07**		.37**		.282**
		(.429)		(.187)		(.115)
1[Filing city, not ZIP, hires examiner]		.274		.0147		.0612
		(.386)		(.108)		(.0553)
Observations	456,637	456,445	456,637	456,445	456,637	456,445
R-squared	.0298	.0328	.085	.0846	.0897	.0898
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No	Yes	No
City FE	No	Yes	No	Yes	No	Yes
Examiner FE	Yes	Yes	Yes	Yes	Yes	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The left-hand side measure is the difference between the number of independent claims in the published patent and in the submitted application. Fewer independent claims indicate less scope. Thus, the left-hand side measure gives one indication of how much scope is reduced in the patent prosecution process. That measure is winsorized at 2.5% in columns 3-4 and at 5% in columns 5-6. The right-hand side measures comprise an indicator for whether the filing firm later hired the examiner (in the first row of coefficients), and indicators for whether the firm resides in a city or ZIP code in which the examiner was later hired (in the second and third rows of coefficients, respectively). As in prior tables, for our location-based specifications, i.e. those whose results are reported in even-numbered columns here, the sample is restricted to observations where the filing firm did not subsequently hire the examiner. Standard errors are clustered at the examiner and firm level in odd-numbered columns and at the examiner and city level in even-numbered columns.

Table A.III: *Quality estimates are robust to varying technology-related controls*

VARIABLES	(1) Count	(2) Count	(3) Count	(4) Count
1[Revolving examiner]	2.03**	1.66**	2.34**	1.99*
	(.811)	(.755)	(1.17)	(1.08)
1[Filing firm hires examiner]	-9.78***	-9.64***		
	(2.32)	(2.52)		
1[Filing ZIP hires examiner]			-4.73***	-3.27**
			(1.47)	(1.46)
1[Filing city, not ZIP, hires examiner]			-6.87***	-6.97***
			(.934)	(.983)
Observations	727,920	727,911	727,616	727,607
R-squared	.19	.201	.194	.203
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	No	No
City FE	No	No	Yes	Yes
Technology center FE	Yes	No	Yes	No
Patent class FE	No	Yes	No	Yes
Experience FE	Yes	Yes	Yes	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The left-hand side measure is citations. The right-hand side measures comprise an indicator for whether the filing firm later hired the examiner (in the first row of coefficients), and indicators for whether the firm resides in a city or ZIP code in which the examiner was later hired (in the second and third rows of coefficients, respectively). As in prior tables, for our location-based specifications, i.e. those whose results are reported in the final two columns here, the sample is restricted to observations where the filing firm did not subsequently hire the examiner. Standard errors are clustered at the examiner and firm level in the first two columns and at the examiner and city level in the last two columns.