NBER WORKING PAPER SERIES

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

Haris Tabakovic
Thomas G. Wollmann

Working Paper 24638
http://www.nber.org/papers/w24638

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2018

We thank Juan Alcacer, Marianne Bertrand, Eric Budish, Thom Covert, Janet Freilich, Kinda Hachem, Elisabeth Kempf, Josh Lerner, Ben Roin, Scott Stern, Chad Syverson, Dennis Yao, as well as conference discussants and seminar participants for helpful comments. We also thank Stu Graham, Alan Marco, Andrew Toole, and other members of the USPTO Office of Chief Economist for creating, documenting, and providing datasets that serve as the basis for much of this paper. Excellent research assistance was provided by Jordan Bell-Masterson, Kavya Garikipati, and Divya Vallabhaneni. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

From Revolving Doors to Regulatory Capture? Evidence from Patent Examiners
Haris Tabakovic and Thomas G. Wollmann
NBER Working Paper No. 24638
May 2018
JEL No. D72,K23,L51,O34

## 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 and that much of this leniency extends to prospective employers. These effects are strongest in years when firms are actively hiring, and these relationships hold for the intensive margin of intellectual property protection. Ultimately, this leads the agency to issue lower quality patents, which we measure in citations. Together with other supporting evidence, we argue these results are suggestive of regulatory capture.

Haris Tabakovic
Harvard Business School - Cotting Hall
Soldiers Field Road
Boston, MA 02163
htabakovic@hbs.edu

Thomas G. Wollmann
Booth School of Business
University of Chicago
5807 South Woodlawn Avenue
Chicago, IL 60637
and NBER
Thomas.Wollmann@chicagobooth.edu

A data appendix is available at http://www.nber.org/data-appendix/w24638

# 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." This 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 a patent application arrives at the agency, it is quasi-randomly routed to a USPTO employee, called an examiner, working in Alexandria, Virginia.[3] These examiners decide whether or not to grant a patent to the inventor(s), so their decisions affect investment [Budish, Roin, and Williams, 2015], the rate and direction of innovation [Moser, 2005, Williams, 2013, Moser, 2013, Galasso and Schankerman, 2015], labor income [Kline, Petkova, Williams, and Zidar, 2016], entrepreneurial activity [Farre-Mensa, Hegde, and Ljungqvist, 2017], and ultimately industrial organization and economic growth [Jaffe and Lerner, 2004]. Many examiners, though, leave the agency to join private sector law firms as patent practitioners—licensed specialists who represent others in the patenting process. This provides increased pay and mobility but creates a potential conflict of interest, since many examiners evaluate the applications of firms for whom they would like to work.

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

---

[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 given 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, the first year the poll was given). About three-quarters (76%) believe it is "run by a few big interests" (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.)

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. We also 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, this is the first study to make these connections.

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. Answering the first question establishes an empirical fact that we feel is important on its own, due to the importance of regulatory consistency. The second presents more of a challenge. It requires distinguishing between actions whose intent is to gain favor with a potential employer and those that merely reflect employee preferences or reverse causality. 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, for reasons discussed below, examiner-firm match-specific preferences are likely less important than in other regulator-regulated relationships. Additional tests, aided by supplementary data, also described below, further help distinguish these explanations. The last 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 pairs but on its own have no real effect. The allocation of applications to examiners at the USPTO, however, is quasi-random [Williams, 2013].[4]

We begin by showing that revolving door examiners grant 12.6-17.6% (8.5-11.9 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 very small.

---

[4] Work by Righi and Simcoe [2017] shows that examiners do not specialize in high or low value applications, which supports this assumption, though they provide other evidence of specialization. See Footnote 7 for more details.

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, and that 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. Hence it provides somewhat stronger evidence.

We then expand the analysis to include alternate research designs. First, we plot the coefficients of interest, by year, against measures of private sector 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 firms are frequently hiring. We find precisely this relationship. Since the level of economy-wide private sector hiring is unrelated to idiosyncratic examiner preferences, this result mitigates endogeneity concerns. Second, we incorporate data on the educational backgrounds of the examiners. This approach relies on the 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 alma mater location is "realized" prior to examination decisions, this mitigates reverse causality concerns. 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 our grant-related results are driven by leniency rather than examiners' attempts to signal they can effectively negotiate the prosecution process.

Finally, we ask whether this behavior results in lower quality patent grants. Examiner leniency lowers the threshold for which an application results in a grant, so patents granted by revolving door examiners to their future and prospective employers should receive fewer citations. In line with prior findings, the results here indicate that patents granted to the firm that later hires a revolving door examiner receive 21-27% fewer citations. Those granted to firms located near the one that hires the revolving door examiner receive many fewer, as well.

We argue that these results, taken together, suggest regulatory capture. The relative narrowness of the examiner-practitioner pay gap means we likely understate the problem relative to many other regulator-regulated relationships, although our results should be interpreted carefully. First, while the firms may not be blindly hiring examiners who treat them favorably, we believe 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 to 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, examination leniency may not be 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. Finally, the preferential treatment of prospective employers is probably not the most pressing problem faced by this particular agency. Instead, we see the USPTO as a data-rich setting to answer basic questions about regulator behavior.

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. The variation we exploit cannot cleanly evaluate competence-collusion tradeoffs, although we argue it provides evidence suggestive of regulatory capture and should merit additional scrutiny.

This paper contributes to a growing empirical literature on regulator behavior. Prior work exploiting across-state or across-time variation has tended not to find evidence of regulatory capture among, for example, bank examiners [Agarwal, Lucca, Seru, and Trebbi, 2014, Lucca, Seru, and Trebbi, 2014]. Related to this, private law firms defending companies targeted by the SEC actually hire *harsher* prosecutors, though this result is not defendant-specific [deHaan, Kedia, Koh, and Rajgopal, 2015]. These are provocative findings. We contribute by studying employer-firm relationships, incorporating the notion of "prospective employers" into the analysis, and intersecting these with novel supporting evidence and measures of decision quality. Analogous problems arise in the private sector, e.g., due to credit rating analysts join firms whose credit they previously rated. Interestingly, 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. Kempf [2015] finds roughly the same result among leverage finance credit analysts but also shows that this group of analysts is more accurate overall. These are compelling results, though it is somewhat unclear how informative they are about behavior among government employees, the quality of whose work is not disciplined by market forces.

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].

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 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.[5] Initial filing fees are several hundred dollars.[6] 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,[7] most of whom work at the Alexandria, Virginia campus.[8]

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 (...)".[9] Examiners decide whether to allow or reject the claims. They also provide reasons behind their 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

---

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

[6] 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.

[7] 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 hard to do. The authors also show examiners don't 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.

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

[9] US Patent No. 821,393.

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.[10]

## 2.1 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.[11]

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."[12] First, SPEs are busy with their own prosecution cases. Second, rejections of junior examiner submissions slow production, and SPEs are 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.[13] 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[14] 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

---

[10] 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.

[11] 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).

[12] 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).

[13] *Ibid.*

[14] *Ibid.* See also Frakes and Wasserman [2016b].

decisions—an important factor driving the underlining data generating process in this paper.

## 2.2 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,"[15] while Fish & Richardson states, "more than a dozen Fish attorneys had prior careers at the USPTO."[16]

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.

## 2.3 Examiner–practitioner interactions

Throughout prosecution, the patent examiner and correspondent are acutely aware of each others' 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 non-final rejection notice for three 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, 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).

---

[15] See https://www.oliff.com/practice-areas/patent/ (retrieved January 2, 2017).

[16] See http://www.fr.com/services/patent-law/ (retrieved January 2, 2017).

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.[17]

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

## 3.1 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.[18] The dataset covers substantively all filings between November 29, 2001 and December 31, 2015,[19] and for those applications that results in grants, coverage extends back to at least July 1995 [Graham, Marco, and Miller, 2015].[20] 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."

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.[21] 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)

---

[17] No mention of Google, GoPro, 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.

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

[19] This paper studies utility patents, which comprise roughly 94.3% of filings. (The remainder cover plants and designs.) Coverage for regular non-provisional utility patent applications is 95% [Graham, Marco, and Miller, 2015]. The small number of applications falling outside PatEx do so for idiosyncratic reasons, e.g. 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.

[20] 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.

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

we can retrieve "snapshots" of the roster for January 2009, October 2011, February 2013, and March 2015 using the Internet Archive.[22] 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.[23,24] Citations accrue from patents published by all national offices.[25] 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[26] and merge to PatEx on the basis of first, middle, and last name.

### 3.2 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 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. *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

---

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

[23] We considered the 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. We also considered the proportion of decisions that conflicted with those from the USPTO's European or Japanese counterparts, but the incidence of multi-jurisdiction filing and discord is also too low to be useful.

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

[25] A prior version of the paper erroneously stated that we cover only US patents, though the results are not sensitive to this.

[26] 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.

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.

### 3.3 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. 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.

[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

## 4.1 Conceptual framework

We model the decision of examiner $i$ to grant intellectual property protection to firm $j$ is given by[27]

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

$y$ denotes the extent of protection. $x$ denotes how much $i$ would like to work at $j$. $\theta$ reflects capture in a straightforward way: $x$ directly influences $y$.[28] If the parameter is positive, for example, then examiners are lenient on the firms for whom they would like to work. The remaining terms, $\eta$ and $\epsilon$, reflect the degree to which the examiner believes the filer genuinely deserves intellectual property protection. They differ, though, in that $\eta$ also determines $x$ while $\epsilon$ does not. $W$ denotes any factors observable to the econometrician (except for $\ell$, 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 $\eta$ 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

---

[27] 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 as to whether examiners are responding to social norms or an explicit *quid quo pro*, though in this setting we find the former more likely. Also, 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.

[28] We take as given that the 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 unlikely, since examiners have immediate access to much more precise information based on all applications, including informative summary statistics like average grant rates.

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 below.

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

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

$\ell$ 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. $\ell$ 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 $\ell$ on geography. $\nu$ denotes other factors that influence $x$ but not $y$. Without loss of generality, let $\nu_{ij} = W_{ij}\mu + \tilde{\nu}_{ij}$.

## 4.2 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—reflect factors besides regulatory capture, we focus on whether individual examiners are especially lenient on particular firms or sets of firms. We remain agnostic as to what systematic differences across examiners and firms indicate (though this is not to say that only "within" estimates are policy relevant).[29]

### 4.2.1 Initial specifications

We first assess whether examiners are lenient on firms that later hire them. We assume that examiners apply to positions at, and ultimatley are employed by, firms for whom they most want to work.[30] With

---

[29] Heterogeneous behavior across agency employees, for example, creates "inconsistent regulation" (in the language of Agarwal, Lucca, Seru, and Trebbi [2014]). Large inefficiencies often result. See their paper for a more in-depth discussion.

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

time subscripts, the estimating equation is

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

where $x^{\star}_{ij} = 1$ if $j$ hires $i$ and $x^{\star}_{ij} = 0$ otherwise. $\beta_1$ is the parameter of interest.

Endogeneity is a potential concern.[31] When $\mathbb{E}[\tilde{\eta}|W, x^{\star} = 1]$ exceeds $\mathbb{E}[\tilde{\eta}|W, x^{\star} = 0]$, non-zero variance of $\tilde{\eta}$ results in $\hat{\beta_1} > 0$ even when $\theta = 0$. In this case, the researcher infers regulatory capture when it does not occur. Aspects of our setting and research design, though, mitigate the problem. First, granular right-hand side control variables limit the scope of the confounding factors. In the main specifications below, for example, $W$ comprises examiner and firm (as well as year) fixed effects. This confines the source of the endogeneity to examiner-firm match-specific preference shocks that simultaneously influence hiring and granting behavior. These shocks can be problematic if examiners are particularly enthusiastic about certain brands relative to others, though substantively all revolving door employees of the USPTO leave for law rather than R&D firms. While individuals may feel strongly about, for example, Google relative to Samsung or Nokia, they are presumably much less likely to feel strongly about, say, Foley & Lardner relative to Sughrue Mion or Fish & Richardson.[32] These shocks can also be a problem when examiners have idiosyncratic tastes for some types of technology over others, are exposed to a range of technologies at the USPTO, and join firms that specialize. The second of these, though, is ruled out, since examiners are administratively grouped within the USPTO by specialized “art units,” each of which covers only a narrow amount of technological scope.[33] We believe endogeneity is not a principal concern in our setting. However, it cannot be completely ruled out on the basis of this argument alone, so it is addressed using alternate research designs below.

Reverse causality is another potential concern. We infer preferences that influence patent grant decisions from labor market outcomes, even if those outcomes are realized *after* examination decisions are made. This assumes that regulatory agency employees do not draw inference about the quality of private sector employers from the quality of their applications. If they do, then an examiner who receives, purely by chance, high quality submissions from a particular firm will be more likely to grant that firm patents as well as work there. However, law firms that hire examiners are large and well-known. A host of reputable media outlets and industry publications rank their practices by office

---

[31] A problem outside the framework presented here exists if examiners choose which applications to work on. To appear favorable, they might select easy-to-approve applications from firms for whom they would like to work. The quasi-random allocation of applications to examiners rules this out. See Footnote 7 for details.

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

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

location and by specialty. Again, while we believe reverse causality is not a first order concern, it cannot be ruled out on this basis alone, so it is addressed using alternate research designs below.

We then assess 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 observed location choices. 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 minimize the confounding influence of $\eta_{ijt}$, we discard all observations in which the filing firm hired the examiner.

Using this approach, 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 $x^{\star}_{ij} = 0$. $\ell$ is a two element vector. $\ell^{\star}_{1ij}$ 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. $\delta_2$ and $\rho_2$ are the parameters of interest.

Note that the threats to identification are somewhat smaller here. Examiner-firm match-specific preference shocks are no longer *per se* a problem: to bias estimates from Equation 4, $\tilde{\eta}$ must vary across geographic locations as well. For example, idiosyncratic tastes for Google over Samsung or for Foley & Lardner over Sughrue Mion are not on their own problematic (though idiosyncratic tastes for mobile phone patents filed in Boston rather than San Francisco still are).

### 4.2.2 Further addressing endogeneity

Examiner behavior intended to influence employers should be most pronounced when firms are actively hiring and subside in periods when they are not. The data indicates considerable variation along this dimension: more than three times as many examiners join private sector firms as patent practitioners in a "boom" year such as 2005 compared to a "bust" year such as 2009. Variation along this dimension, though, is plausibly unrelated to the magnitude of examiners' idiosyncratic preference shocks. As a result, we can further address endogeneity by plotting the estimated coefficients by year against measure of hiring intensity.

To do so, *Grant* is first regressed on (a) an indicator for whether the examiner was later hired as a practitioner interacted with the year, (b) an indicator for whether the filing firm hired that examiner interacted with the year, (c) year fixed effects, (d) examiner fixed effects, and (e) firm fixed effects.

We plot coefficients on terms in (b), which are year-specific, against the number of examiners hired by private sector firms in that particular year. Then, we repeat this exercise but use city-specific matches (with the appropriate sample and fixed effects adjustments). Finally, for a sense of robustness around these relationships and a broader, economy-wide measure of hiring intensity, we replicate these graphs but replace the number of examiner hires with year-over-year private sector seasonally-adjusted employment gains or losses reported by the Bureau of Labor Statistics[34] to similar effect.

### 4.2.3 Further addressing reverse causality

To correctly interpret estimates from Equations 3-4, we require that subsequent employment drives examination decisions, not the other way around. To support this interpretation, we turn to data on examiners' educational backgrounds—in particular, the location of their alma maters. As we argue immediately below, these locations provide examiner-specific observables that shift employment preferences and, critically, are realized *before* examination decisions are made.

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.[35] 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. We assume that, at least as a first order approximation, the location of one's alma mater does not directly influence grant decisions, at least within the narrowly-defined type of technology that one is exposed to within the USPTO.

We first test whether examiners do, in fact, take jobs nearer to where they were educated. We then test whether revolving door examiners grant more patents to firms located near their alma maters. A positive relationship, though, raises the question of whether the results merely reflect "boosterism"—a desire to support people and firms in close geographic proximity to their school for reasons that have

---

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

[35] 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 [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." We are agnostic, though, 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. (On this last point, see, e.g., http://www.collegexpress.com/lists/list/percentage-of-out-of-state-students-at-public-universities/360/ (retrieved January 2, 2017)).

nothing to do with gaining favor among prospective employers. To assess this concern, we separately test whether the same relationship exists for non-revolving door examiners, i.e. employees who did not join private sector firms as practitioners. This test mitigates the boosterism concern if the positive relationship exists only for revolving door examiners.

### 4.3 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 $\beta_3$, $\delta_4$, and $\rho_4$. We also estimate negative binomial and quasi-maximum likelihood Poisson models, the later of which better address the count nature of the data [Wooldridge, 1999, Rysman and Simcoe, 2008, Bertanha and Moser, 2016]. (Note that if the parameters of interest are negative, then these specifications also further minimize concerns over reverse causality, which were raised above.)

## 5 Effects on patent grants

### 5.1 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 technology, 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% (8.5-11.9 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 that filers face "inconsistent regulation."

## 5.2 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 Table VI is analogous to that of Table V, except that the analysis here is across locations rather than across firms. Thus, whereas *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. All observations in which the filing firm later hired the examiner are dropped. This limits 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 also 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.

## 5.3 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.

## 5.4 Are examiners more lenient when firms are actively hiring?

Figure I reports the results of the first exercise described in Section 4.2.2. Here we plot the coefficients of interest, by year, against the number of examiners hired as patent practitioners in that year. The top graph reports firm-specific results, while the bottom graph reports city-specific results. The figure shows that regardless of which set of results one considers, the effects are most pronounced when private sector hiring is robust.

[Figure I about here.]

Figure II then reports the results from the second exercise described in Section 4.2.2. Here we further assess the relationship by plotting the coefficients of interest, by year, against year-over-year private sector seasonally-adjusted employment gains or losses.[36] These provide a broader, economy-wide measure of hiring intensity. 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.

[Figure II about here.]

## 5.5 Are examiners more lenient on firms near prior residences?

We now turn to the specifications described in Section 4.2.3. We first assess whether the location of revolving door examiners' alma maters does, in fact, predict the location of the firm that subsequently hires them. Figure III 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.

[Figure III about here.]

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. This is consistent with revolving door examimers who apply to and accept offers from firms that are, for example, in close proximity to family and friends or in a city with which they are already familiar. Moreover, as the Appendix shows, these results are highly robust to instead considering raw (unresidualized) or winsorized distances, the latter of which account for a small number of Hawaiian schools and firms in the data.

Given the relationship evidenced by Figure III, we next test whether examiners grant more patents to firms near their school. Column 1 of Table VIII indicates this is so. *Grant* is decreasing in the distance

---

[36] BLS-reported economy-wide employment changes are perhaps not a perfect proxy for the private sector demand for examiners but should provide a good first-order approximation. For example, the left-most three x-axis values correspond to the Great Recession and tail end of the Dotcom crash. They are also preferable to taking sharp cuts of the employment data based on subjective calls.

between the filing firm and revolving door examiners' alma mater. Fixed effects at the examiner, city, and year level address concerns discussed in the main specifications above.

[Table VIII about here.]

One alternate explanation for this pattern is "boosterism," a mere desire to support people and firms in close proximity to one's school. To assess this, we incorporate educational information related to non-revolving door examiners. If boosterism rather than regulatory capture drives the relationship evidenced in Column 1 of Table VIII, then *Grant* will be decreasing in the distance between the filing firm and a *non*-revolving door examiners' alma mater as well. Column 2 indicates this is not so. The coefficient on the revolving door examiners' firm-to-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.

To ensure that that the small number of Hawaii-based schools and/or firms do not drive these results, we replicate the results in Columns 1 and 2 in Columns 3 and 4 but first winsorize the top 1% of the values. The size and precision of the estimates are effectively unchagned.

## 5.6 Impact on the intensive margin

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. 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 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.

The claims data comprises 457,000 applications that result in grants.[37] 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.[38]

[Table IX about here.]

---

[37] 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.

[38] 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 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.

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 21-27% fewer citations, while those awarded to firms in the same ZIP code and city as the future employer receive 6-8% and 6-11% fewer. The linear model provides similar size effects.[39] 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, policymakers have expressed and exhibited some willingness to address this problem.[40]

---

[39] 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.

[40] For example, 2007 legislation increased “cooling off” period—the length of time one must wait after leaving the government sector before one can begin lobbying—for members of Congress and their senior staff. In 2009, President Obama doubled the cooling off period for many senior officials in the executive branch. Separately, in 2013, the Securities and Exchange Commission eliminated loopholes that exempted senior officials from post-employment restrictions.

Data from the US Patent and Trademark Office provide 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.

# References

Sumit Agarwal, David Lucca, Amit Seru, and Francesco Trebbi. Inconsistent regulators: Evidence from banking. The Quarterly Journal of Economics, 129(2):889–938, 2014.

Juan Alcacer and Michelle Gittelman. Patent citations as a measure of knowledge flows: The influence of examiner citations. The Review of Economics and Statistics, 88(4):774–779, 2006.

Alison E Barber and Mark V Roehling. Job postings and the decision to interview: A verbal protocol analysis. Journal of Applied Psychology, 78(5):845, 1993.

Marinho Bertanha and Petra Moser. Spatial errors in count data regressions. Journal of Econometric Methods, 5(1):49–69, 2016.

Marianne Bertrand, Matilde Bombardini, and Francesco Trebbi. Is it whom you know or what you know? an empirical assessment of the lobbying process. The American Economic Review, 104(12): 3885–3920, 2014.

Donald Boyd, Hamilton Lankford, Susanna Loeb, and James Wyckoff. The draw of home: How teachers' preferences for proximity disadvantage urban schools. Journal of Policy Analysis and Management, 24(1):113–132, 2005.

Stephen G Breyer and Richard B Stewart. Administrative law and regulatory policy. Aspen Publishers, 1979.

Eric Budish, Benjamin N Roin, and Heidi Williams. Do firms underinvest in long-term research? evidence from cancer clinical trials. The American Economic Review, 105(7):2044–2085, 2015.

Iain M Cockburn, Samuel Kortum, and Scott Stern. Are all patent examiners equal? the impact of examiner characteristics. NBER Working Paper No. 8980, 2002.

Jess Cornaggia, Kimberly J Cornaggia, and Han Xia. Revolving doors on Wall Street. Journal of Financial Economics, 120(2):400–419, 2016.

Ed deHaan, Simi Kedia, Kevin Koh, and Shivaram Rajgopal. The revolving door and the SEC's enforcement outcomes: Initial evidence from civil litigation. Journal of Accounting and Economics, 60(2):65–96, 2015.

Joan Farre-Mensa, Deepak Hegde, and Alexander Ljungqvist. What is a patent worth? evidence from the u.s. patent "lottery". 2017.

Michael D. Frakes and Melissa F. Wasserman. Is the time allocated to review patent applications inducing examiners to grant invalid patents?: Evidence from micro-level application data. Review of Economics and Statistics, (forthcoming), 2016a.

Michael D. Frakes and Melissa F. Wasserman. Procrastination in the Workplace: Evidence from the US Patent Office. 2016b.

Janet Freilich. Pushing patent boundaries: An empirical assessment of how patent trolls and other litigants use patent scope. 2016. Working paper.

Alberto Galasso and Mark Schankerman. Patents and cumulative innovation: Causal evidence from the courts. The Quarterly Journal of Economics, 130(1):317–369, 2015.

Stuart J. H. Graham, Alan C. Marco, and Richard Miller. The USPTO Patent Examination Research Dataset: A Window on the Process of Patent Examination. 2015.

Adam B. Jaffe and Josh Lerner. Innovation and Its Discontents: How Our Broken Patent System Is Endangering Innovation and Progress, and What to Do about It. Princeton University Press, Princeton, NJ, 2004.

Elisabeth Kempf. The job rating game: The effects of revolving doors on analyst incentives. 2015. Working paper.

Pat Kline, Neviana Petkova, Heidi Williams, and Owen Zidar. Quasi-experimental estimates of rent-sharing: Evidence from administrative tax data. 2016.

Jeffrey M. Kuhn. Property rights and frictions in the sale of patents. 2016. Working paper.

Jean-Jacques Laffont and Jean Tirole. The politics of government decision-making: A theory of regulatory capture. The Quarterly Journal of Economics, pages 1089–1127, 1991.

Mark A Lemley and Bhaven Sampat. Examiner characteristics and patent office outcomes. Review of Economics and Statistics, 94(3):817–827, 2012.

Mark A Lemley and Carl Shapiro. Probabilistic patents. The Journal of Economic Perspectives, 19(2): 75–98, 2005.

Michael A. Levine and Jennifer L. Forrence. Regulatory capture, public interest, and the public agenda: Toward a synthesis. Journal of Law, Economics, & Organization, 6:167–198, 1990.

David Lucca, Amit Seru, and Francesco Trebbi. The revolving door and worker flows in banking regulation. Journal of Monetary Economics, 65:17–32, 2014.

Alan C. Marco, Joshua D. Sarnoff, and Charles A. deGrazia. Patent claims and patent scope. 2016.

Petra Moser. How do patent laws influence innovation? evidence from nineteenth-century world's fairs. The American Economic Review, 95(4):1214–36, 2005.

Petra Moser. Patents and innovation: evidence from economic history. The Journal of Economic Perspectives, 27(1):23–44, 2013.

Sam Peltzman. Toward a more general theory of regulation. The Journal of Law and Economics, 19(2): 211–240, 1976.

Pew. Beyond Distrust: How Americans View Their Government. Technical report, 2015.

Gary N Powell and Laurel R Goulet. Recruiters' and applicants' reactions to campus interviews and employment decisions. Academy of Management Journal, 39(6):1619–1640, 1996.

Michelle Reininger. Hometown disadvantage? it depends on where youre from: Teachers location preferences and the implications for staffing schools. Educational Evaluation and Policy Analysis, 34(2):127–145, 2012.

Louis A Jr. Richardson. Perceived monetary value of job type, company size, and location among college seniors. Journal of Applied Psychology, 50(5):412, 1966.

Cesare Righi and Timothy Simcoe. Patent Examiner Specialization. Boston University Questrom School of Business Research Paper, 2017.

Benjamin N. Roin. Discussant comments at the NBER Summer Institute workshop on Technology and Innovation. 2016.

Marc Rysman and Timothy Simcoe. Patents and the performance of voluntary standard-setting organizations. Management science, 54(11):1920–1934, 2008.

George J Stigler. The theory of economic regulation. The Bell Journal of Economics and Management Science, pages 3–21, 1971.

Daniel B Turban, Alison R Eyring, and James E Campion. Job attributes: Preferences compared with reasons given for accepting and rejecting job offers. Journal of Occupational and Organizational Psychology, 66(1):71–81, 1993.

Jordi Blanes I Vidal, Mirko Draca, and Christian Fons-Rosen. Revolving door lobbyists. The American Economic Review, 102(7):3731–3748, 2012.

Heidi L Williams. Intellectual property rights and innovation: Evidence from the human genome. Journal of Political Economy, 121(1):1–27, 2013.

Jeffrey M Wooldridge. Distribution-free estimation of some nonlinear panel data models. Journal of Econometrics, 90(1):77–97, 1999.

# Figures

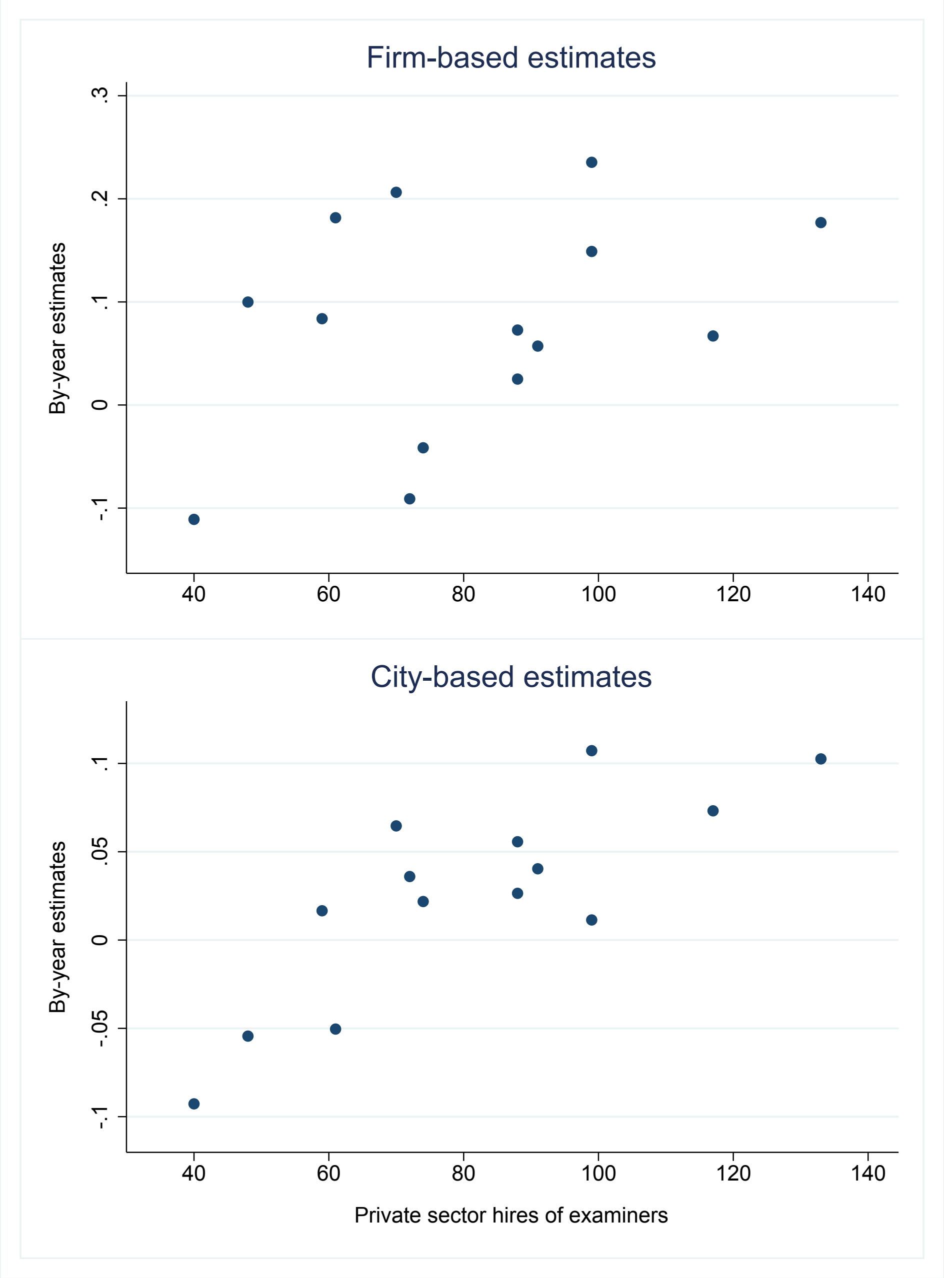

**Figure I:** *Effects are strongest when examiner hiring is most frequent*

*For the top graph, Grant is regressed on (a) an indicator for whether the examiner was later hired as a practitioner interacted with the year, (b) an indicator for whether the filing firm hired that examiner interacted with the year, and (c) year fixed effects, (d) year fixed effects, and (e) firm fixed effects. Then, coefficients on the second set of interactions are plotted on the y-axis. For the bottom graph, we repeat the exercise but base the second set of interactions on whether the filing firm resided in the same city as the firm that hire the examiner. (We also substitute city for firm fixed effects and drop any observations where the firm hired the examiner.) In both graphs, the x-axis counts the number of examiners hired by private sector firms in that year.*

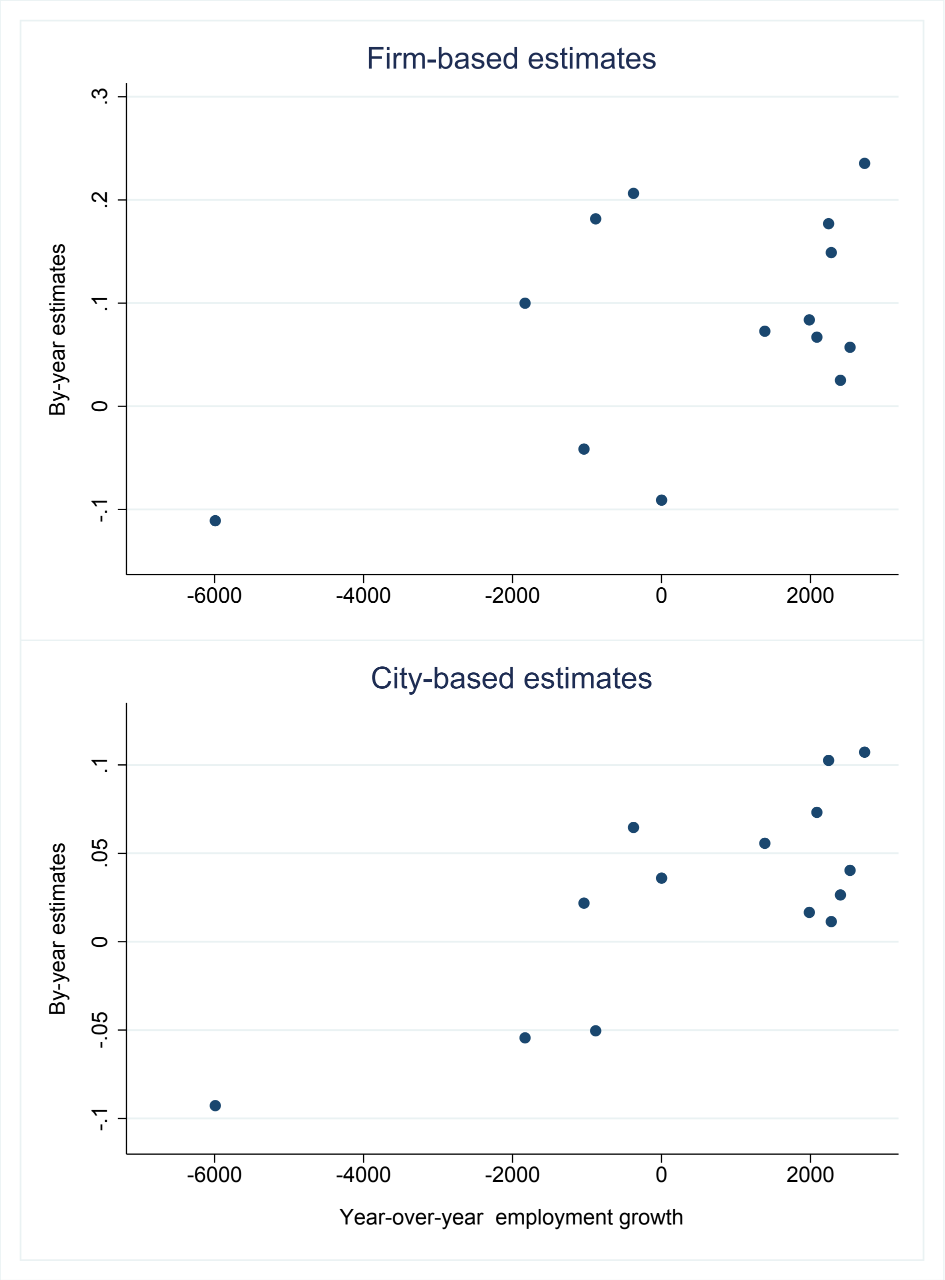

**Figure II:** *Economy-wide employment changes yield similar results to observed examiner hires*

*To arrive at the top graph, Grant is regressed on (a) an indicator for whether the examiner was later hired as a practitioner interacted with the year, (b) an indicator for whether the filing firm hired that examiner interacted with the year, and (c) year, examiner, and firm fixed effects. Then, coefficients on the second set of interactions are plotted on the y-axis. To arrive at the bottom graph, we repeat the exercise but base the second set of interactions on whether the filing firm resided in the same city as the firm that hire the examiner. (We also substitute city for firm fixed effects and drop any observations where the firm hired the examiner.) The x-axis measures the year-over-year change in private seasonally-adjusted US employment in thousands.*

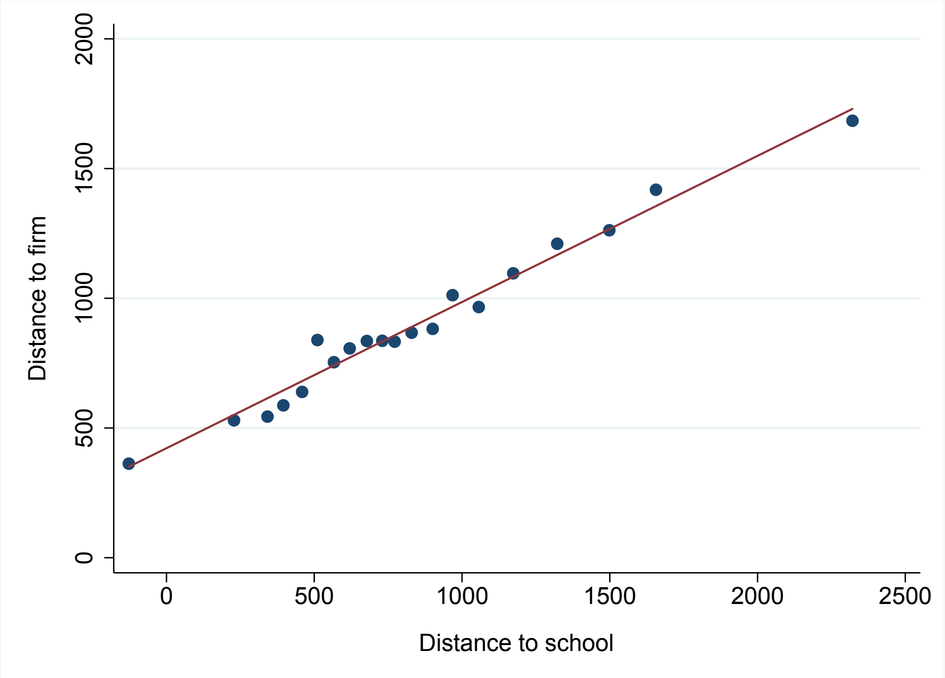

**Figure III:** *Where an examiner is educated predicts his or her post-USPTO employment location*

*The unit of observation is the application (to remain consistent with forthcoming specifications). The x-axis measures the distance, in miles, from the filing firm to the examiner alma mater, and the y-axis measures the distance, in miles, from the filing firm to the the firm that later hires the examiner. Distances are residualized using examiner, city, and year fixed effects. A positive slope indicates that revolving door examiners tend to work, on average, close to where they were educated.*

# 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*

| | Count | | Mean | |
|---|---|---|---|---|
| City | 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 |
| D. 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)<br>All firms | (2)<br>All firms | (3)<br>All firms | (4)<br>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)<br>All firms | (2)<br>All firms | (3)<br>All firms | (4)<br>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 hiring cities*

| VARIABLES | (1)<br>Hiring firms | (2)<br>Hiring firms | (3)<br>Hiring cities | (4)<br>Hiring cities |
|---|---|---|---|---|
| 1[Revolving examiner] | .0392*** | | .0451*** | |
| | (.00826) | | (.00739) | |
| 1[Filing firm hires examiner] | .113*** | .0764** | | |
| | (.0231) | (.0289) | | |
| 1[Filing ZIP hires examiner] | | | .0722*** | .0496** |
| | | | (.0241) | (.0229) |
| 1[Filing city, not ZIP, hires examiner] | | | .0342*** | .0244** |
| | | | (.00979) | (.0104) |
| Observations | 292,023 | 291,503 | 668,035 | 667,604 |
| R-squared | .152 | .244 | .13 | .213 |
| Year FE | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | No | No |
| City FE | No | No | Yes | Yes |
| Examiner FE | No | Yes | No | 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)<br>Distance | (2)<br>Distance | (3)<br>1.0% Wins. distance | (4)<br>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 39 for a brief explanation.*