EFFECTS OF REGULATORY CAPTURE: EVIDENCE FROM PATENT EXAMINERS*

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Abstract

Many regulatory agency employees are hired by the firms they regulate, typically at higher pay, which may lead them to "go easy" on their future as well as prospective employers. This paper studies regulatory capture at the US Patent and Trademark Office by linking patent grant decisions to the work histories of the examiners who made them. We find that examiners grant significantly more patents to firms that later hire them. Much of this leniency extends to firms for whom they were likely to work, which we infer from geographic location. Last we show that this leniency results in the Office granting patents of lower quality, which we measure in citation counts.

JEL: G18, D72, O34. Keywords: regulatory capture, intellectual property, revolving doors, patents.

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1 Introduction

Many regulatory agency employees follow brief, public sector experience with more lucrative work at the firms they used to regulate. In some industries, the process is so common that these agencies appear to have "revolving doors." The practice 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 [Stigler, 1971, Peltzman, 1976].[1] This exchange can have first order welfare consequences stemming from policies that are ineffective at correcting market failures, unmotivated to protect the public interest, and likely to erode trust in government.[2] Despite these facts, there is little empirical work on regulatory capture and no direct evidence that the prospect of lucrative, subsequent employment leads to biased decision-making in government agencies.

Studying regulatory capture presents empirical challenges. Agency employees that do and do not enter the private sector can fundamentally differ in many ways, so observing that the former are more lenient than the latter hardly provides proof of capture. The alternative research design compares behavior within-employee (across the firms they regulate). One can start by testing whether agency employees "go easy" on the firms that ultimately hire them. In many settings, this may be strongly indicative of capture; in others, the presence of employee-firm match-specific preferences shocks can yield false positives. For example, a defense department procurement agent who thinks Boeing has superior technology to its rivals may be more likely to both buy Boeing products and later work there—even in the absence of rent-seeking behavior. Thus, we propose additionally testing whether employees are lenient on the *prospective* employers as well, where the set of those firms is defined by attributes that are unlikely to be correlated with regulatory-related factors.

One agency that both regulates a significant share of commerce and faces potential capture vis-à-vis the revolving door is the United States Patent and Trademark Office ("USPTO"). The agency is tasked with issuing patents, which reward effort and disclosure by granting inventors a temporary right to exclude others from using the idea. When one applies for a patent, their application is quasi-randomly routed to an USPTO employee, called an examiner, who decides whether to allow, narrow, or reject the inventors' claims. Thus, examiner decisions have the potential to affect investment [Budish, Roin, and Williams, 2015], entrepreneurial activity [Farre-Mensa, Hegde, and Ljungqvist, February 2016],

[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 of revolving doors, e.g. Breyer and Stewart [1979].

[2]At the time of writing, polls reveal US public trust in government is at historically low levels (19%). Since the data was first collected in 1964, this has tracked most closely to (the inverse of) the sentiment that government is "run by a few big interests", which is also near a historic high of 75% [Pew, 2015]. This is precisely the concern mentioned above.

labor income [Kline, Petkova, Williams, and Zidar, 2016], the rate and direction of follow-on innovation [Moser, 2005, Williams, 2013, Galasso and Schankerman, 2015], and ultimately industrial organization and economic growth [Jaffe and Lerner, 2011]. Examination experience is valuable, though, in the private sector, particularly to law firms specializing in intellectual property. Many examiners leave the agency to become patent practitioners, i.e. individuals who have legally registered to represent others in the application process, and join one of these firms. This provides increased pay and mobility—in particular an ability to leave Alexandria, Virginia.[3]

This paper asks whether the prospect of lucrative, post-USPTO employment biases patent examiner decisions. In other words, do revolving door examiners appear to be captured and, if so, are lower quality patents the result? To answer this question, we construct an original dataset that ties application decisions to the examiners who made them, and then ties those examiners to their work histories. In other words, the paper links the firms affected by government regulation to the firms that hire regulatory agency employees *at the employee-decision level*. To our knowledge, it is the first to do so. The dataset also incorporates a quality measure for applications that result in grants. In particular, for each of the more than one million applications in our sample, we observe the name and location of the firm filing each application, the name and identifier of the examiner, his or her decisions and the dates on which they were made, and a set of technological classifications. For each of the applications that result in grants, we also observe the number of forward citations, i.e. those that accrue to the patent after it is published, which provide a commonly-used measure of quality. Moreover, for the subset of examiners whose work histories we can obtain, we observe we observe the name and location of their employers as well as the names and locations of institutions at which they were educated.

We find that revolving door examiners grant 13-16% (9-11 percentage points) more patents to firms that later hire them. Given our empirical approach and institutional details surrounding patent prosecution, this result on its own may provide evidence of capture. For example, one may be concerned that examiners who are particular eager to join a firm may simply select out high quality, easy-to-grant patent applications from the backlog and then approve these; however, the quasi-random allocation of applications to examiners rules this out [Williams, 2013].[4] Another concern, alluded to

[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.")

[4] Righi and Simcoe [2017] provide recent evidence of specialization among examiners across types of technology, although their findings likely do not affect our empirical strategy. They do not find evidence of specialization across application "importance" (a measure akin to quality). This rules out that certain examiners are being allocated or selecting easy-to-grant applications overall. Also, if one expects art unit management to allocate easy-to-grant applications to their friends or themselves,

above, is that some examiners are inherently more likely to join the private sector and grant patents. Similarly, some firms may be particularly successful at hiring examiners and filing applications. Our main specifications, however, include examiner and firm fixed effects, which absorb these differences. Moreover, the magnitudes of the estimates are largely invariant to limiting the sample to firms that hire examiners or examiners who become practitioners. A somewhat more nuanced concern is that certain firms may be more successful with *particular* examiners. If, for instance, an examiner is enamored with Google, then he or she may be more likely to apply to Google for a job and grant Google a patent (relative to other firms). However, the vast majority of companies conducting R&D outsource the patent application process to law firms, and these law firms account for over 90% of private sector examiner hires. While an examiner may have deep-seated preferences for Google relative to, say, Apple or Samsung, he or she is unlikely to feel as strong about, for example, Sughrue Mion relative to Fish & Richardson or Foley & Lardner.[5] Furthermore, USPTO employees are grouped into "art units," each of which cover narrowly-defined types of inventions. This severely limits the ability of examiners' grant and employment decisions to be correlated due to idiosyncratic preferences across types of technology.

To obtain stronger evidence, we propose an additional estimation strategy that depends on two premises. First, examiners face uncertainty about which firms will have future job openings. Second, conditional on the type of work, an employers 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]. If these are correct and if revolving door examiners are captured, then they will extend the aforementioned leniency to prospective employers as well, where that set comprises, on average, other firms in the same geographic area as the firm that ultimately hired them. For examiner-firm match-specific preferences to present a problem here, examiners would need to prefer the inventions developed in one region over those developed in another, even within a very narrow categorization of the underlying technology. We find that revolving door examiners extend their leniency to all firms in close proximity to the ones that later hire them. For example, they grant 7-11% (5-7 percentage points) more patents to firms in the same ZIP code.

To further support these results, we rely on the premise that prior residency—in particular, where one was educated—affects locational preferences [Richardson Jr, 1966]. For instance, a revolving door examiner that graduated the University of Maryland is more likely to join a DC area firm than one who graduated from the University of California system (but less likely to join a West Coast firm). The data

then this suggests that deducing quality from a cursory reading of the filing is difficult or impossible. Moreover, since most revolving door examiners leave after only a few years, specialization is presumably less of an issue for the employees we study.

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

supports this premise. If revolving door examiners are lenient towards their prospective employers, one should see higher grant rates to firms that are closer, on average, to their *almae matres*. The data support this as well. These findings strengthen the earlier identification argument, since the location of ones schooling is unlikely to be otherwise correlated with grant-related behavior—at least within the narrow confines of what examiners evaluate while working at the USPTO. Moreover, his relationship does not exist among non-revolving door examiners, i.e. those that do not become practitioners, which helps rule out "boosterism" in driving this result.

Last, we find that this leniency results in lower quality patent grants by the USPTO. In particular, we show that patents granted by revolving door examiners to the firms that later hire them receive 18-21% fewer citations than the others that are assessed by those employees. These effects extend to other firms in close proximity to the hiring firms: patents granted by revolving door examiners to the firms in the same city or ZIP code as the firm that later hired them receive 5-9% fewer citations than others assessed by those employees. The estimates are stable across specifications, and the ratio of firm- and location-specific coefficients in the quality regressions are in line with those determined by the grant regressions. Notably, though, the patents granted by revolving door examiners appear to be higher quality overall—they accrue roughly 5-7% more citations than those granted by their colleagues. We are reluctant, as stated above, to draw inference from mean differences across individuals, although this result should give pause to policymakers wishing to root out employees who are predisposed to or aiming for subsequent employment in the private sector.

These results are striking, especially since the US is typically given as an example of a low corruption regime (e.g. Shleifer and Vishny [1993]). Care should be given, however, to interpretation. On its own, this paper does not suggest immediate policy changes. Altering regulatory agency employment contracts can have adverse effects on hiring. For example, stipulating that examiners cannot work at any firm whose patents they examined for a year or more—akin to the "cooling off" period in public accounting—may dissuade talented people from joining the USPTO in the first place. Even if revolving door examiners are partial towards one firm over another, their work may be of higher quality overall.[6] It also does not imply explicit collusion. In fact, we suspect that the arrangement is tacit—that a norm has developed where, as the saying goes, "I will scratch your back if you scratch mine." Last, welfare calculations are absent. These would require assuming an optimal scope of IP protection, about which little is known. This paper focuses on revolving doors and regulatory capture; optimal patent policy is

[6] Kempf [2015], cited below, provides compelling evidence of this in a private sector setting. Results in Section 6 may provide some proof of this in our setting, too.

beyond its scope.

Empirical work on regulatory agency behavior has tended not to find evidence of capture. Agarwal, Lucca, Seru, and Trebbi [2014] study the rotation of US bank regulators between federal and state supervisor positions, and provide broad evidence that state supervisors are systematically more lax than federal ones. They argue this is due to institutional design problems and suboptimal incentive system but find no evidence of regulatory capture. For example, the state-level variation in turnover is not associated with leniency in their study. Lucca, Seru, and Trebbi [2014] use cross-state enforcement variation to show that numbers of employees who leave the regulating agency for private sector employment are higher during periods of intense regulatory enforcement. This is inconsistent with the view that as job prospects at regulated firms improve, the incidence of regulatory leniency and capture also increase. deHaan et al. [2015] show that private law firms that defend firms targeted by the SEC actually hire *harsher* prosecutors. Interestingly, they find some evidence of capture when they limit the sample to Washington, DC-based attorneys, but this result is not defendant-specific. Our paper expands this line of research, links agency employees to their subsequent employers, which allows for within-employee estimation, and strengthens identification using the notion of prospective employers.

This paper also relates to the literature studying revolving doors in the private sector, e.g. between credit rating analysts and the investment banks that they rate. These studies are primarily concerned with the extent to which *quid pro quo* arrangements contributed to the 2009 financial crisis. For example, Cornaggia, Cornaggia, and Xia [2016] show that corporate finance credit rating analysts issue higher ratings to firms for whom they later work, but the effect is modest—roughly 10% of a standard deviation in the ratings.[7] Kempf [2015] finds that leveraged finance credit analysts who are hired by investment banks also inflate their future employers' ratings, but this group of analysts is more accurate overall. These papers provide compelling results in their respective settings, though it is unclear how informative they are about the behavior of government agencies, which differ in important ways. For one, public entities are rarely disciplined by market forces. A credit rating agency that issues uninformative reports due to its analysts pandering will either remedy the situation or go out of business; however, an agency like the USPTO will maintain its monopoly over the provision of intellectual property protection, regardless of how deserving the recipients are.

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

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

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, 2003, Lemley and Shapiro, 2005, Alcacer and Gittelman, 2006, Lemley and Sampat, 2012, Frakes and Wasserman, 2016].

The outline of this paper is as follows. Section 2 describes the patenting process at the USPTO, and the roles that patent agents, attorneys, and examiners play in it. Section 3 describes the data. Section 4 introduces the empirical framework. Section 5 reports our findings with respect to grant behavior, while Section 6 report quality-related findings. 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.[8] Initial filing fees are several hundred dollars.[9] All new patent applications are sorted based on the type of technology, and directed to the appropriate "art unit"—the group of examiners specializing in 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,[10] most of whom work at the Alexandria, Virginia campus.[11]

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 property protection. As an example, Orville and Wilburn Wright claimed a "class of flying machines in which...one or more aeroplanes are moved through the air (...)".[12] Examiners decide whether to allow or reject the claims. They also provide reasons behind their decisions, which

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

[9] 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.

[10] See Footnote 4 for additional commentary on the randomness of application allocation.

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

[12] US Patent No. 821,393.

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

The process, called "patent prosecution," may go back and forth several times. Each submission, modification, and appeal, however, requires additional fees—often times stretching into the thousands of dollars. Patent prosecution ends with one of the following: the examiner allows a portion of the claims and the filer is satisfied with this allowance, in which case a patent is issued; or the filer abandons the application. As Roin [2016] points out, nearly all applications can, in practice, result in a grant—the filer can simply narrow the claim language to suit the examiner. Abandonment is best viewed as the case where the examiner insists on such narrow claims that filer deems the application no longer worth pursuing. Thus, the binary outcome of whether the application resulted in a grant approximates the somewhat more complicated result.[13]

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 of an art unit. Many 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. These equate to pay of roughly $63,000 and $87,000 per year, respectively.[14]

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."[15] 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

[13]This paper relies on this approximation throughout, as do nearly all others studying the examination process. A recent exception is Kuhn [2016]. See also Freilich [2016], which argues that intensive margin protection matters in litigation.

[14]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).

[15]See the initial draft of a report by the Office of the 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).

reporting cycle to dump a large number of cases on an SPEs desk and completely overwhelm them.[16] Lack of examiner oversight is a well-known problem at the USPTO and has triggered both a detailed report by the Office of the Inspector General[17] and a joint hearing by the US House of Representatives Ethics and Oversight Committees. As a result, even junior examiners have great discretion over grant decisions—an important factor driving the underlining data generating process in this paper.

Practitioners

The time and complexity of the patenting process leads most inventors to hire a patent practitioner to write the application and guide 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 filers. Many of the law firms that represent them specialize in matters related to intellectual property protection, and these firms seek out employees with past examination experience. In fact, examiner hires are a selling point for these 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,"[18] while Fish & Richardson states, "more than a dozen Fish attorneys had prior careers at the USPTO."[19]

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

Examiner–practitioner interactions

During prosecution, the USPTO and application filer exchange several documents. From the perspective of the firm, the most pivotal documents contain examiners decisions: the non-final rejection and final rejection and/or notice of allowance. The non-final rejection, for example, provides initial indication of which claims the examiners will allow, along with the justification for their decision. An unfavorable response forces the patent practitioner to go to the inventor with bad news; a favorable response allows the practitioner to boast that he or she has secured formal, legal protection over their idea. These

[16] *Ibid.*
[17] *Ibid.*
[18] See https://www.oliff.com/practice-areas/patent/ (retrieved January 2, 2017).
[19] See http://www.fr.com/services/patent-law/ (retrieved January 2, 2017).

transmissions are the precise point at which the firm learns whether the examiner has been lenient or not, and this fact raises an immediate question with respect to regulatory capture: how salient are the firm and examiner to one another in these documents?

To answer this, we need to go only as far as the cover page of these transmissions. 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. The Appendix provides concrete examples. In particular, it presents the cover pages of the non-final rejections to four well-known, relatively recent applications. These include Google's 1998 application for the PageRank algorithm, GoPro's 2004 application for an attachment of a camera to a body, Theranos's 2005 application for rapid diagnosis, and Square's 2010 application for a mobile phone attachment that captures credit card information. Several points are worth noting. First, the examiner is acutely aware of which firm will receive his or her decision. In the Google example, the transmission is clearly addressed to Harrity & Snyder of Fairfax, Virginia. (This firm and location appear below in Table III and Table IV, respectively). Tantamount to this, the examiner knows that the firm knows exactly whose decision this is. His or her first and last name are directly to the right of the recipient address. In contrast, nowhere does the form provide space to list the firm actually employing the named inventor. No mention of Google, GoPro, Theranos, or Square appear anywhere here.[20] Also of interest, the form is clearly identical across prosecution cases and unchanged since at least December 2000. This makes locating this information even easier than it otherwise would be.

3 Data

Sources

The data come from three 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.[21] The dataset covers substantively all filings between November 29, 2001 and December 31, 2015,[22]

[20] 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 cannot ascertain the identity of the firm that developed the technology but only that this information is much less salient than the law firm filing the application.

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

[22] This paper studies utility patents, which comprise roughly 94.3% of filings. (The remainder are plant and design patents.) Coverage for regular non-provisional utility patent applications is 95% [Graham et al., November 2015]. The small number of applications falling outside PatEx did so for idiosyncratic reasons, e.g. many related to ballistics and radar are non-public for

and for those applications that results in grants, coverage extends back to at least July 1995 [Graham, Marco, and Miller, November 2015].[23] PatEx is derived from the Public Patent Application Information Retrieval system ("Public PAIR"), carefully documented by the Office of the 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.[24] 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 we can retrieve "snapshots" of the roster for January 2009, October 2011, February 2013, and March 2015 using the Internet Archive[25] (in addition to the current roster). The office issues unique identification numbers to practitioners upon registration, so joining lists is straightforward, and turnover is low among this group. Thus, crucially, 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 patent citations. As a proxy for quality, we count forward citations, i.e. the number of patents that cite a given patent. The citations match precisely 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]

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 Appendix B for more details.

[23] 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, were merely begin the panel in July 1996.

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

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

[26] We exclude law schools. A large proportion of examiners attend law school on a part-time basis (in the DC metropolitan area) while working at the USPTO. Thus, the location of legal studies provide almost no information about locational preferences. However, their inclusion would bias our estimates downwards, both because they would introduce a large amount of right-hand side measurement error.

Variable descriptions

The first 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 outcome of interest, *Citations*, counts the forward citations to the patent in question. *Year* fixed effects correspond to the date of the examiner's last action on an application, i.e. an allowance preceding a grant or the last rejection preceding abandonment. *Experience* fixed effects correspond to the total number of complete years of experience that the examiner has accrued over the panel.[27] *Technology center* and *Patent class* fixed effects provide alternate, related ways to distinguish between types of technology.

To construct distance measures between educational institutions and filing firms, we rely on the latitude and longitude assigned to five digit US ZIP codes. Relative to addresses, five digit ZIP codes map easily and quickly to geospatial coordinates and do not result in any meaningful measurement error (relative to the variation that we consider). When an examiner has attended multiple schools, we use the minimum distance between the filing firm and the set of schools in question. For example, if the examiner holds degrees from both Georgetown and Clemson, we would use the distance to Georgetown if the filing firm was based in Washington, DC. We exclude law schools for an obvious reason: a large proportion of examiners are enrolled in DC-area night and weekend programs while they are working for the USPTO, so the bulk of these observations are independent of prior residency.

Summary statistics

Table I provides an initial summary of the data used to assess patent grant behavior. It covers about 1.2 million patents, 64% of which result in a grant. The mean examiner decision (across the longitudinal dimension of the panel) occurs early in 2011. Just under seven years of experience are acquired before examiners leave the agency. Nearly 30% of applications are filed by firms that hire at least one examiner, and 6% 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

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

an average of 11.54 citations, although the distribution is skewed right. Later in the paper, to ensure our results do not depend on extreme observations, we assess quality using both winsorized (at 1%) and unwinsorized citation counts. The second row of the table shows that while the mean drop by less than 20%, the maximum falls from over 3,000 to under 100. The other dimensions of the data are distributed similarly to the sample used to assess grant behavior.

[Table II about here.]

Table III provides more detail on the distribution of key variables across firms that 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 DC metropolitan area, including cities like Alexandria, Arlington, Reston, and McLean, Virginia. The balance spread throughout the country. New York City (Manhattan), NJ, Chicago, IL, San Francisco, CA, and Boston, MA are all heavily represented. Other large and/or R&D-focused cities are represented, including Dallas, TX, Philadelphia, PA, Minneapolis, MN, and San Diego, CA. Applications are distributed similarly to hires, with a minor exceptions. For example, cities near the USPTO headquarters are slightly overrepresented in terms of hiring. Cities such as Minneapolis, Houston, and Irvine are somewhat underrepresented.

[Table IV about here.]

4 Model

Conceptual framework

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

$$y_{i,j} = \theta x_{i,j} + \eta_{i,j} + \epsilon_{i,j}. \tag{1}$$

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

The presence of η in Equation 1 is not merely hypothetical. In many or even most industries, regulatory agency employees who are most enthusiastic about the activities of the regulated firms are the ones most likely to seek employment there. Similarly, high quality firms are more likely to attract job applicants and receive favorable judgments. At the USPTO, either will generate a correlation between grants and hires—even in the absence of self-interested behavior. A rich set of controls, W, can mitigate this problem. Our specifications, for example, include examiner and firm dummy variables, which subsume both of the aforementioned effects. Even with these controls, residual variation can create a material problem. $\tilde{\eta}$ reflect these examiner-firm match-specific preferences that drive grants and hires simultaneously. Using an earlier example, an examiner who (idiosyncratically) thinks Boeing has superior technology to its rivals may be more likely to both grant Boeing patents and later work there.

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

$$x_{i,j} = \lambda \ell_{i,j} + \kappa \eta_{i,j} + \nu_{i,j}. \tag{2}$$

ℓ reflects attributes of j that are observable to the econometrician and over which i has preferences. It may be a vector or scalar, and it may take continuous or discrete values. For simplicity, we assume that $\ell = 1$ if j has i's most preferred attribute and $\ell = 0$ otherwise. In our application, ℓ captures locational preferences. It takes a value of one if j resides in i's most preferred city and zero otherwise. Other factors that influence x but not y are denoted ν. Without loss of generality, let $\nu_{i,j} = W_{i,j}\mu + \tilde{\nu}_{i,j}$.

Estimation of effects on grant rates

We estimate a linear probability model, substituting *Grant* for y, so that the outcome of the examination process is binary. We consider four estimation strategies below. The first compares behavior across examiners. The second compares behavior within-examiner across-firms that did and did not hire the examiner. The third compares behavior within-examiner, across some firm attribute, where that attribute determines x but does not directly affect the grant decision. The fourth exploits an instrument for that attribute.

Comparisons across examiners.

Comparing the behavior of examiners who do and do not become practitioners is only informative about capture under strict conditions. Some examiners are naturally more enthusiastic than others about the technological developments in their respective fields, and it would not be surprising to see those examiners choose a career path in which they advocate for that technology, i.e. become practitioners. As we argue above, plausible stories exist in most if not all industries where lax supervision is correlated with subsequent employment for entirely benign reasons.

The rest of this section remains agnostic as to what systematic differences across examiners indicate, but this is not to say that heterogeneous behavior—especially across those who are and are not hired away from the regulator by the regulated firms—is policy irrelevant. It may reflect nefarious behavior, and it may not. In any case, big differences equate to very "inconsistent regulation" (in the parlance of Agarwal, Lucca, Seru, and Trebbi [2014]) and typically large inefficiencies. Section 5 returns to this point in more detail.

Comparisons within examiner but across firms.

The first within-examiner estimation strategy assesses whether examiners are especially lenient on firms that hire them. With time subscripts added, the estimating equation is

$$Grant_{i,j,t} = W_{i,j,t}\phi_1 + \beta_1 x^{\star}_{i,j} + \xi_{i,j,t}. \tag{3}$$

$x^{\star}_{i,j} = 1$ if j hires i and $x^{\star}_{i,j} = 0$ otherwise. The parameter of interest is β_1. This strategy relies on the fact that examiners apply to positions at—and hence are hired by—firms for which they most want to work. As a result, $\mathbb{E}[x|W, x^{\star} = 1]$ exceeds $\mathbb{E}[x|W, x^{\star} = 0]$.

The presence of $\tilde{\eta}$ is problematic. Since $\mathbb{E}[\tilde{\eta}|W, x^{\star} = 1] > \mathbb{E}[\tilde{\eta}|W, x^{\star} = 0]$, $\hat{\beta_1}$ is likely to exceed zero even when θ is zero. In other words, the econometrician will detect capture even when it does not exist. Granular right-hand side controls mitigate the problem by limiting the scope of confounding factors. In the main specifications below, for example, W comprises examiner and firm (as well as year) fixed effects. Thus, the remaining concern is that a portion of the residual, examiner-firm match-specific preferences simultaneously affect both hires and grants. This appears to be of second order in our setting, but it cannot be ruled out.

Comparisons within examiner but across firm attributes.

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

For this strategy, the estimating equation is

$$Grant_{i,j,t} = W_{i,j,t}\phi_2 + \beta_2 \ell^\star_{i,j} + \tau_{i,j,t}, \tag{4}$$

for observations in which $x^\star_{i,j} = 0$. $\ell^\star$ takes a value of one if j resides in the city in which i was hired and zero otherwise (which is not to say that j was in every case the city i most preferred). The parameter of interest is β_2. If $\tilde{\eta}$ is i.i.d. across firms, then $\mathbb{E}[\tilde{\eta}|l = l^\star, W, x \neq x^\star] - \mathbb{E}[\tilde{\eta}|l \neq l^\star, W, x \neq x^\star] \leq 0$. As a result, $\mathbb{E}[y|W, x \neq x^\star]$ is increasing in an indicator for whether $\ell = \ell^\star$ only when $\theta > 0$.

Estimating the effect on patent quality

We additionally study whether this leniency results in lower quality patents. η and ϵ reflect the degree to which i genuinely believes j should receive patent protection, so they relate to quality. If $\theta > 0$, then the expected value of η and ϵ, conditional on y, are lower for "captured" applications. Thus, in the data, conditional on observing *Grant*, if revolving door examiners are captured, then *Citations* are systematically lower for applications in which examiner i would like to work at work j.

For the linear model, the estimating equations are given by

$$Citations_{i,j,t} = W_{i,j,t}\phi_3 + \beta_3 x^\star_{i,j} + \psi_{i,j,t}. \tag{5}$$

and

$$Citations_{i,j,t} = W_{i,j,t}\phi_4 + \beta_4 \ell^{\star}_{i,j} + \varepsilon_{i,j,t}, \tag{6}$$

where, in the case of Equation 6, we restrict to (i, j, t) observations such that $x^{\star}_{i,j} = 0$. The parameters of interest are β_3 and β_4, respectively. As an alternative test, we also estimate a negative binomial model.

5 Effects on patent grants

We first study patent grant rates to assess whether revolving door examiners exhibit leniency towards firms that later hire them, and whether this leniency extends to prospective employers as well.

Do examiners favor their future employers?

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

[Table V about here.]

The table's first row reveals that revolving door examiners grant about 13-16% (9-11 percentage points) to firms that that subsequently hire them relative to those that do not. Adding firm fixed effects has almost no effect, while including examiner fixed effects—for example, moving from columns 2 to 3—attenuates the coefficients by less than 20%. Each of the estimates is 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.

The second row shows that revolving door examiners grant 7% (4.5 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 nearly 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 are, in the spirit of Agarwal, Lucca, Seru, and Trebbi [2014], very "inconsistent regulation." We should note here that while partly tangential to regulatory capture, that finding is still policy relevant. Several papers, back to at least Lemley and Shapiro [2005], document heterogeneous grant rates across examiners. More recent work asserts that ratcheted output targets drive much of this seemingly arbitrary behavior [Frakes and Wasserman, 2016]. Table V presents a different problem which, at least in part, changes to the intra-examiner incentive scheme will not fix.

Do examiners favor prospective employers as well?

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

[Table VI about here.]

Table VI shows that half or more of the leniency that revolving door examiners extend to the firms that hire them is extended to, on average, all other firms in the area as well. The final column, for example, which includes both examiner and city fixed effects, reports that revolving door examiners grant 7% (5 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 mean effect of the application being routed to a revolving door examiner is virtually unchanged from the prior table. For patent applications, geographic concentration is greater than firm concentration, so examiners are more likely to see an application from from a firm close to where they will work than from a firm for whom they will work.

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 the 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. Column 1 of Table VII addresses this concern. It reports estimates from an exercise analogous to the base-line firm-specific results, except it restricts the sample to firms that hired at least one examiner. This cuts the sample size by three-quarters. 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, the coefficients in the first row of Table VII will be much lower than those found in Table V. However, the overall magnitude and precision are essentially the same. For example, the estimate here is within 5% of the analogous one (provided in the final column) in Table V. 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. If this is true, then limiting the sample to locations that hire at least one examiner only—which discards 40% of the observations—will provide much lower coefficients than those found in the final column of Table VI. Column 3 of Table VII addresses this concern by restricting the sample in precisely this way. However, the estimates here are again very close to those reported in the earlier table.

Additional support from educational backgrounds

Most examiners join the agency directly after obtaining their undergraduate or graduate degree(s). While the USPTO presented the best job offer upon graduation, valuable opportunities arise after a few years of experience to examiners who would like to become practitioners. 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] and that workers prefer jobs near where they have lived and/or were educated [Richardson Jr, 1966].[28] In our context, this means that revolving door examiners want to work at firms that are closer, on average, to their *almae matras*. Moreover, if revolving door examiners exhibit leniency towards prospective employers, then they will grant more patents to firms that are closer, on average, to where they were educated. Assuming that the location of one's *alma mater* is otherwise uncorrelated with grant-related behavior, at least within the narrowly-defined type of technology that one is exposed to within the USPTO, we can exploit these facts to lend independent support to our findings.

We start by assessing whether the location of revolving door examiners' *almae matres* does, in fact, predict the location of the firm that subsequently hires them. Figure I establishes this connection. To maintain continuity with subsequent results, the unit of observation is the application. The x-axis measures the distance between the filing firm and the examiner's *alma mater*, while the y-axis measures the distance between the filing firm and the examiner's subsequent employer. The (unambiguous) positive relationship indicates that, holding fixed the filing firm, a revolving door examiner that was educated nearby is likely to join a firm that is nearby (and vice versa). This is consistent with revolving door examimers applying to and accepting offers from firms that are, for example, in close proximity to family and friends or in a city in which they are already familiar.[29]

[Figure I about here.]

Given what we learn from Figure **??**, the remaining empirical question is whether he or she granted more patents to firms near the school. Column 1 of Table VIII indicates this is so. *Grant* is increasing in the distance between filing firms and revolving door examiners' *almae matres*. Fixed effects at the examiner, city, and year level address analogous concerns to the ones discussed in the main specifications above. Schools and firms located in Hawaii create a long, thin right tail in the distribution of distances, so to ensure these observations are not driving our results, we winsorize distances at the top 1%. This reduces the top end of the support from 5,100 miles to just over 2,500 miles—effectively redistributing the distances to and from Hawaii to the largest values within the continental United States—but has only a trivial impact on the coefficients and their standard errors.

[Table VIII about here.]

[28] The author of that study, for example, 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."

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

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

6 Effects on patent quality

We now turn to the question of whether this leniency translates into lower quality grants, which we assess using *Citations*. Table IX reports the relevant estimates. Output from the negative binomial output appears in the first two columns, while output from the linear model appears in the remaining ones. Each specification includes firm or city fixed effects where appropriate. The linear models include either examiner-level controls or examiner fixed effects, with the latter accounted for in columns 4, 6, 8, and 10. The negative binomial model includes only examiner-level controls.[30]

[Table IX about here.]

Across 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% fewer citations, while those awarded to firms in the same ZIP code and city as the future employer receive 8% and 6% fewer. In the linear model, using the full set of controls, these figures translate to 6.3, 2.11, and 4.4 fewer citations, respectively. 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

[30]The non-linearity and the large number of individual examiner dummy variables rendered optimization infeasible. The relative proximity of the coefficients provided in columns 3 through 10 gave us confidence, however, that within estimates from the negative binomial model, were we able to estimate them, would not stray too far from the reported coefficients. This also forced us to use *Technology center* rather than *Patent class* fixed effects. For the sake of comparability, we carried through this practice through to columns columns 3, 5, 7, and 9. Varying the granularity of the technology-related controls does not meaningfully affect any estimated coefficients. See the Appendix for details.

the indicators based on close proximity to the hiring firm, which squares with earlier findings related to grant behavior. Here, however, the ZIP code-related coefficients are only marginally significant (and in two cases, are insignificant).

A final feature of the table that deserves discussion are the positive coefficients estimated on the indicator for revolving door examiners. In short, patents granted by revolving door examiners receive roughly 5.5-5.8% more citations relative to those granted by their colleagues. This figure is 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. Certainly one interpretation 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 private sector setting, wherein high quality employees leave for higher paid positions. We are reluctant to infer too much from mean differences across examiners, although this certain should give regulators pause when contemplating policies that would restrict *ex post* employment options.

7 Conclusion

The premise of this paper is that norms exist in which people are especially nice when interacting with those from whom they are about to ask for a job. This behavior becomes particularly salient when that job pays substantially more than the current one does. When the job seeker is considering multiple companies, he or she will likely be nice to all of them. This creates a conflict within regulatory agencies tasked with protection of the public interest and impartial supervision of economic activity. These regulatory bodies are staffed by many individuals who expect to follow a brief period of time at the regulator with longer-term, more lucrative work at one of the regulated, private firms. We provide initial evidence of this conflict using data that links the firms affected by regulation to the firms hiring regulatory agency employees at the employee-decision level.

This paper shows that the prospect of lucrative, subsequent employment biases regulatory agency employees' decisions. At the USPTO, this bias translates into more patents being awarded than otherwise would have been. In addition, these patents are on average lower quality, as measured by citation counts. The magnitude of these effects are meaningful: a patent application filed by a firm that later hires the examiner assessing the application is 13-16% more likely to result in a grant. Conditional on being granted, that patent will receive 21% fewer citations, i.e. be of considerably lower quality.

These findings are robust over a set of different specifications. Moreover, much of this effect extends to prospective employers as well. This implies that the effects of capture probably affect many more than just the subset of decisions that pertain to an agency employee's future employer.

This evidence on its own should not be used to advocate for specific policy changes but should, we hope, encourage encourage additional research. One open question is whether public employees who later join private sector firms produce higher quality work overall, even if a portion of their decisions are biased (as our results suggest in the Section 6 above). A related question is where those differences come from, i.e. whether they are driven by heterogeneous ability or effort. The answers to these questions map into very specific policy actions, such as how agencies should react to temporarily weak labor markets. If the agency's labor demand is mostly invariant to the business cycle, as is the case with at the USPTO, troughs in economic activity afford them an opportunity to pick up educated, ambitious individuals. However, these employees may also turnover at a high rate and be very "sympathetic" to the interests of industry. Finally, little is known about the effects on regulatory competition of competition among the firms or of the value of agency experience outside the industry, which makes agency employees less reliant on future employment at the regulated firms.

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Figures

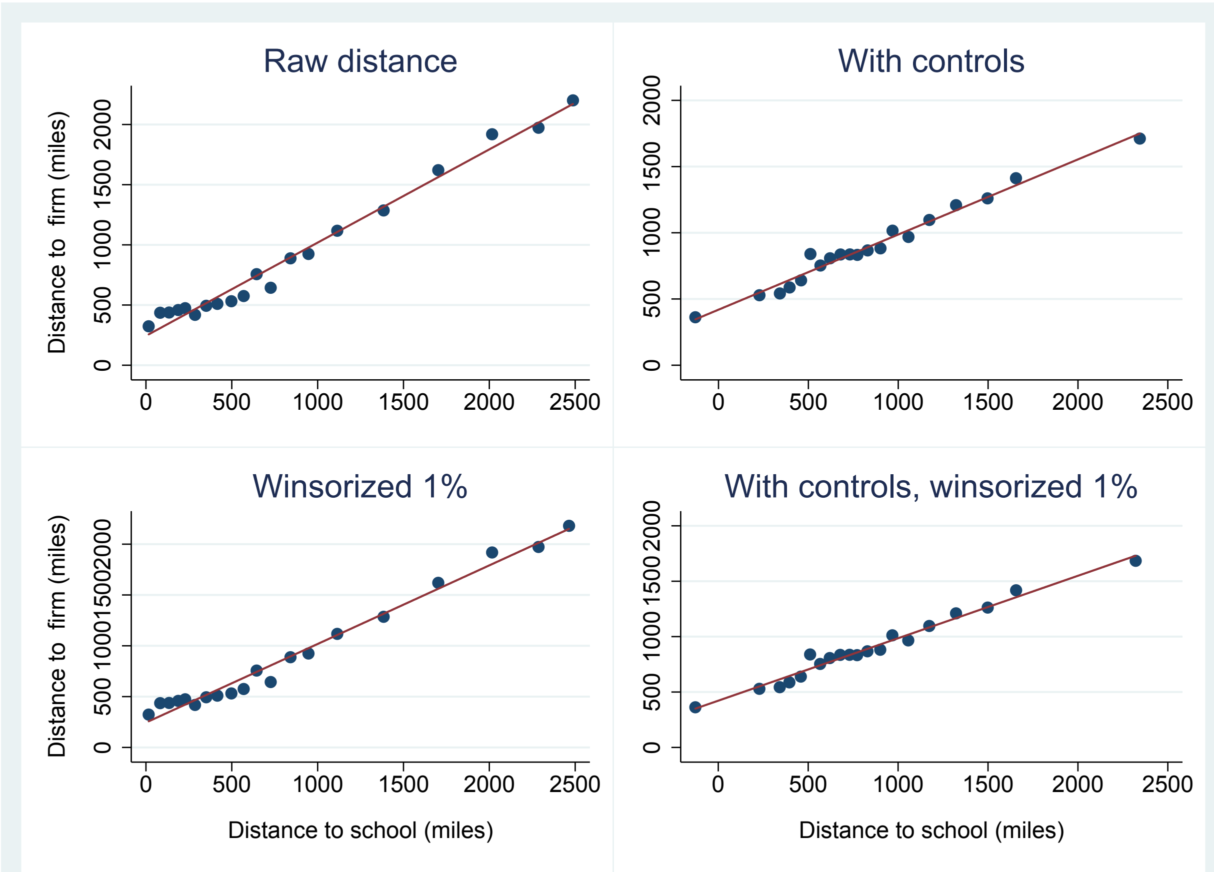


Figure I: *Where an examiner is education predicts his or her post-USPTO employment location*

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

Tables

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

	N	Mean	Std. Dev.	Minimum	Maximum
Grant	1,045,178	0.62	0.49	0	1
Year	1,023,669	2011.39	3.03	2001	2016
Experience	1,045,178	6.36	2.29	0	10
a_hiring	1,045,178	0.29	0.45	0	1
a_practicioner	1,045,178	0.07	0.25	0	1

The data on which these statistics are 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. Grant is a binary outcome that takes a value of one when a patent is issued and zero otherwise. See the text, above, for other variable definitions.

Table II: *Summary of data that assess quality*

	N	Mean	Std. Dev.	Minimum	Maximum
Citations	727,929	11.57	36.40	0	3072
Citations (winsorized, .025)	727,929	10.31	22.60	0	147
Year	727,920	2010.04	5.03	1995	2016
Experience	727,929	6.30	2.35	0	10
a_hiring	727,929	0.29	0.46	0	1
a_practicioner	727,929	0.11	0.31	0	1

The data on which these statistics are based covers filings from July 1995 through the end of 2015, i.e. all observations subsequent to the June 1995 law change affecting patent terms. Citations takes a count of forward citations, i.e. citations made by other patnets to the one in question. See the text, above, for other variable definitions.

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

City	Count		Mean	
	Examiner hires	Filed applications	Grant	Citations
Banner Witcoff	10	4014	.68	9.27
Birch Stewart	15	10958	.62	7.21
Buchanan Ingersoll	12	4829	.63	11.8
Cooley	8	2210	.62	19.7
Finnegan Henderson	48	7331	.51	13.0
Fish & Richardson	13	11488	.68	12.5
Fitzpatrick	9	7497	.77	10.0
Foley & Lardner	13	10059	.66	14.0
Greenblum	9	3135	.62	7.44
Harness Dickey	11	11026	.66	7.25
Harrity & Harrity	11	1051	.88	8.32
Hunton	10	836	.56	17.3
Knobbe	8	10233	.62	18.2
Lee & Morse	11	4040	.68	11.8
McDermott	8	8148	.56	10.3
Morgan Lewis	9	8611	.66	9.44
Oblon McClelland	16	17789	.68	7.96
Oliff	13	10432	.72	8.05
Sterne Kessler	15	3302	.75	12.6
Sughrue	17	13904	.56	7.78
Townsend (merged)	8	9662	.64	14.9
Venable	8	1558	.56	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. 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 entities. See the text, above, for other variable definitions.

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

	Count		Mean	
City	Examiner hires	Filed applications	Grant	Citations
Alexandria, VA	67	58029	.63	8.51
Arlington, VA	14	12200	.68	13.3
Atlanta, GA	16	14533	.5	19.2
Baltimore, MD	10	930	.50	13.8
Bethesda, MD	14	5315	.62	8.73
Boston, MA	30	27169	.58	14
Chicago, IL	39	41070	.64	18.0
Cleveland, OH	14	14351	.62	10.6
Dallas, TX	20	26578	.62	11.1
Fairfax, VA	41	22749	.58	6.80
Houston, TX	14	34566	.62	9.44
Irvine, CA	12	28576	.63	13.6
McLean, VA	28	20678	.57	7.96
Minneapolis, MN	14	51393	.64	12.2
New York City, NY	46	44632	.60	12.3
Philadelphia, PA	24	15581	.57	13.5
Pittsburgh, PA	10	5537	.56	11.6
Reston, VA	47	25517	.62	6.21
San Diego, CA	19	12375	.68	10.5
San Francisco, CA	30	33232	.63	14.4
San Jose area, CA	27	34937	.68	13.9
Seattle, WA	13	13429	.61	14.7
Troy, MI	10	12094	.63	9.63
Washington, DC	299	115125	.82	18.6

"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 for which there was either a grant or abandonment during the sample period. For comparison, the city with the most applications that did not hire any examiners is Scarsdale, NY, with about 20,000 filings. See the text, above, for other variable definitions.

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

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

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

The left-hand side measure is Grant. 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). Columns 2 and 4 include firm fixed effects, which columns 3 and 4 include examiner fixed effects. Standard errors are clustered at the examiner and firm level.

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

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

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

Grant is on the left-hand side of the estimating equation. The right-hand side measures comprise an indicator for whether an examiner was later hired as a practitioner (in the first row of coefficients) and indicators for whether the firm resides in a ZIP code or city in which the examiner was later hired (in the second and third rows of coefficients, respectively). Columns 2 and 4 include city fixed effects, which columns 3 and 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 when the sample is limited to hiring firms or cities*

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

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

Columns 1 and 2 restrict the sample to filing firms that hired at least one examiner. Columns 3 and 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 1 and 2 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 and 2 and are clustered at the examiner and city level in columns 3 and 4.

Table VIII: *Revolving door examiners grant more patents to firms closer to their almae matras*

VARIABLES	(1) Distance	(2) Distance	(3) Wins. distance	(4) 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 (miles, wins. 1%)			-.0098**	-.00988**
			(.00423)	(.00421)
Filing firm distance to non-revolver's school (miles, wins. 1%)				.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

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

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 forth rows and columns. Standard errors are clustered at the examiner and city level.

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

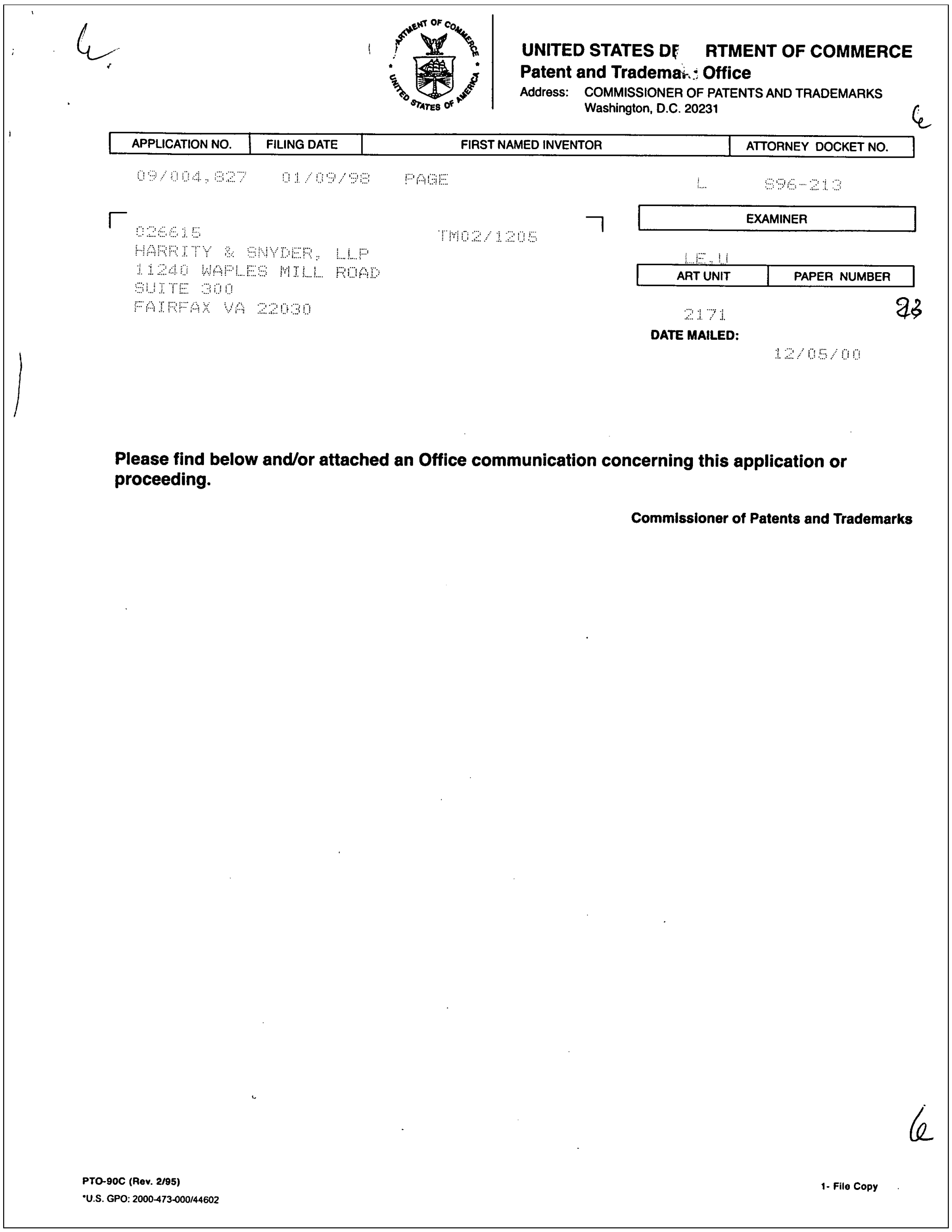
VARIABLES	(1) N.B. count	(2) N.B. count	(3) Count	(4) Count	(5) Count	(6) Count	(7) Wins. count	(8) Wins. count	(9) Wins. count	(10) Wins. count
1[Revolving examiner]	.0552***	.0585***	2.03**		2.34**		.9*		1.09*	
	(.00745)	(.00741)	(.811)		(1.17)		(.473)		(.656)	
1[Filing firm hires examiner]	-.21**		-9.78***	-6.3***			-5.6***	-4.09**		
	(.0995)		(2.32)	(2.11)			(1.9)	(1.62)		
1[Filing ZIP hires examiner]		-.0781			-4.73***	-2.11*			-2.87***	-.81
		(.0706)			(1.47)	(1.08)			(.781)	(.766)
1[Filing city, not ZIP, hires examiner]		-.0596*			-6.87***	-4.44***			-3.97***	-2.54***
		(.0309)			(.934)	(.766)			(.412)	(.439)
Observations	727,920	727,694	727,920	727,335	727,616	727,032	727,920	727,335	727,616	727,032
R-squared			.19	.325	.194	.327	.314	.417	.318	.419
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	Yes	No	No	Yes	Yes	No	No
City FE	No	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Technology class FE	Yes	Yes	Yes	No	Yes	No	Yes	No	Yes	No
Experience FE	Yes	Yes	No	No	No	No	No	No	No	No
Examiner FE	No	No	No	Yes	No	Yes	No	Yes	No	Yes

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

The left-hand side measure is the count of forward citations (winsorized at 1% in the final four columns). A negative binomial model is estimated for columns 1 and 2; a linear model is estimated for the remainder. 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 negative binomial model; in the other columns, standard errors are clustered at the examiner and either firm or city level, depending on the specification.

Appendix A: Selected examiner-firm communication

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



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

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

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

EXAMINER	
LE,U	
ART UNIT	PAPER NUMBER
2171	23

DATE MAILED: 12/05/00

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

Commissioner of Patents and Trademarks

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

1- File Copy

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

UNITED STATES PATENT AND TRADEMARK OFFICE

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

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

758 7590 01/19/2005

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

EXAMINER	
GRAY, DAVID M	

ART UNIT	PAPER NUMBER
2851	

DATE MAILED: 01/19/2005

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

PTO-90C (Rev. 10/03)

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

UNITED STATES PATENT AND TRADEMARK OFFICE

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

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

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

EXAMINER	
LAM, ANN Y	

ART UNIT	PAPER NUMBER
1641	

MAIL DATE	DELIVERY MODE
03/03/2011	PAPER

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

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

PTOL-90A (Rev. 04/07)

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

UNITED STATES PATENT AND TRADEMARK OFFICE

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

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

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

EXAMINER	
HAUPT, KRISTY A	
ART UNIT	**PAPER NUMBER**
2876	

NOTIFICATION DATE	DELIVERY MODE
11/14/2011	ELECTRONIC

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

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

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

Patentsv@goodwinprocter.com
dnakley@goodwinprocter.com

PTOL-90A (Rev. 04/07)

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

The America Inventors Protection Act ("AIPA") was passed late-November 2000 and went into effect one year later. It stipulated that all non-provisional patent applications are published. Prior to the AIPA, applications were published 18 months after filing. If the applicant chose to abandon the filing prior to this point, the application did not publish. Thus, it does not show up in our data. Figure A.I shows the affect this has on the number of filings over the longitudinal dimension of the data. This creates obvious complications when determining the factors that affect the likelihood of receiving a patent [Graham et al., November 2015]. To sidestep the issue, the sample we use to assess grant behavior begins on December 1, 2000, the day after the legislation went into affect.

[Figure II about here.]

Absolute and relative coverage before and after this period is fairly constant. This is evident when one expands the window around the AIPA effective data (see Figure A.II below).

[Figure III about here.]

Appendix C: Sensitivity to variation in technology controls

When we assess grant behavior, in particular in the specifications that reflect examiner level controls rather than fixed effects, we include *Patent class* fixed effects. These are preferable to the less specific *Technology center* fixed effects that we are limited to in our assessment of effects on quality. (See Footnote 30 in the body of the paper for more detail.) In Table A.I, below, we replace the *Patent class* with *Technology center* fixed effects in the relevant grant-related specifications and then re-estimate the model. Since our main specifications include examiner fixed effects, the forthcoming point is mostly moot, but in any case the estimates are robust to this change.

[Table X about here.]

In Table A.II, below, we replace the *Technology center* fixed effects with *Patent class* fixed effects in the relevant quality-related specifications and then re-estimate the model. Again, our main specifications include examiner fixed effects, the forthcoming point is mostly moot, but in any case the estimates are robust to this change.

[Table XI about here.]

Appendix E: Dataset construction

In process.

Figures

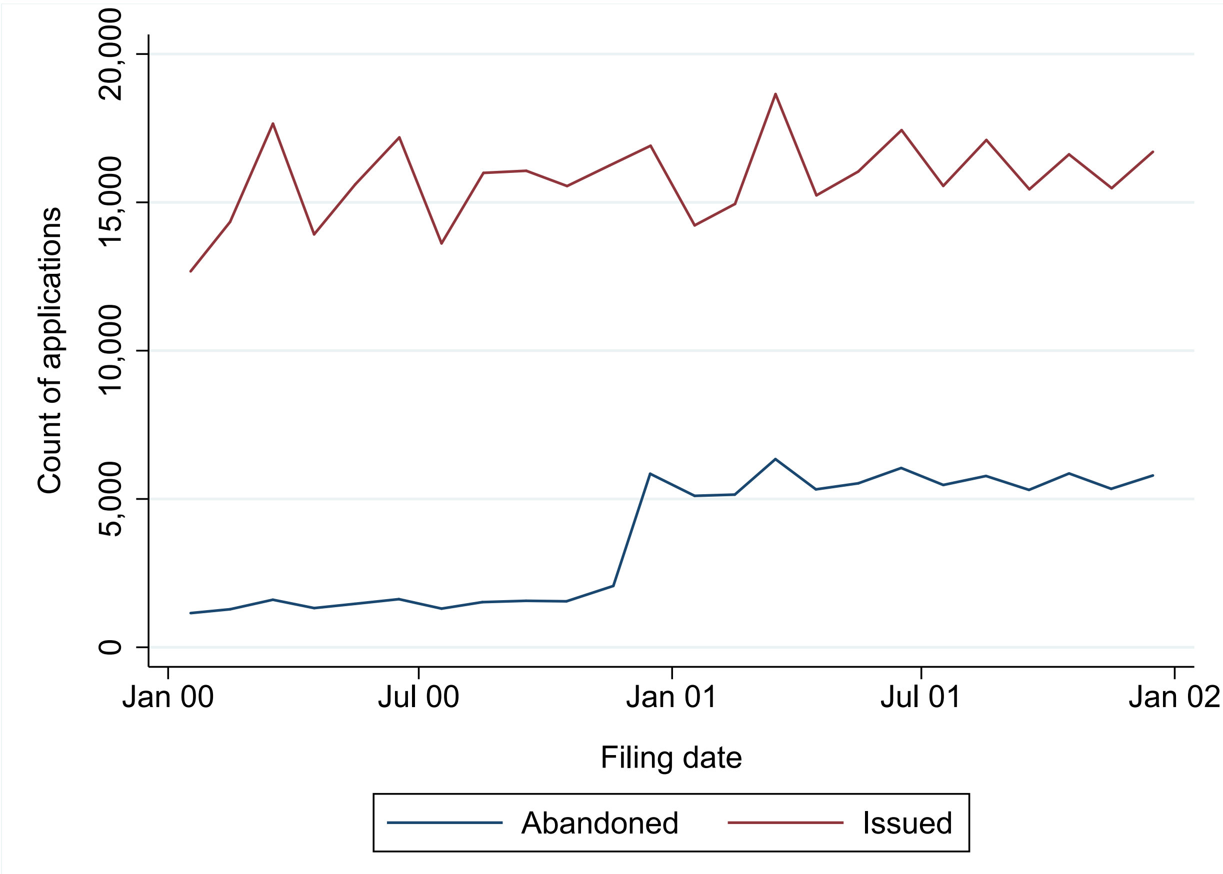


Figure A.I: *Applications that result in abandonments but not grants increase with AIPA legislation*

Values correspond to counts of applications by month and manner of eventual disposal, i.e. whether the application resulted in a grant or not.

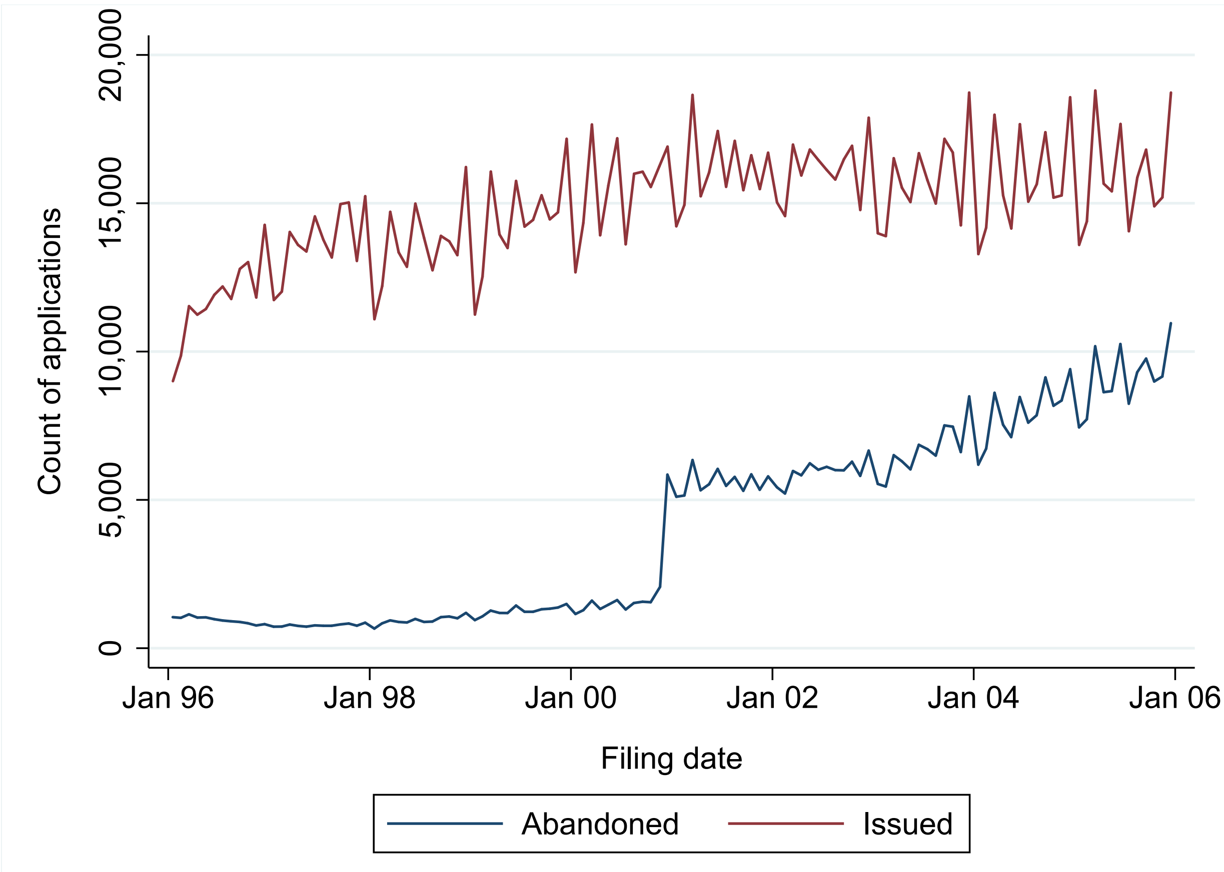


Figure A.II: *Coverage of the dataset is stable aside from the AIPA-related change*

Values correspond to counts of applications by month and manner of eventual disposal, i.e. whether the application resulted in a grant or not.

Tables

Table A.I: *Robustness of grant-related results to varying technology-related controls*

VARIABLES	(1) All firms	(2) All firms	(3) All firms	(4) All firms
1[Revolving examiner]	.059***	.0579***	.0447***	.0429***
	(.00769)	(.0076)	(.00697)	(.0069)
1[Filing firm hires examiner]	.115***	.104***		
	(.0334)	(.0233)		
1[Filing ZIP hires examiner]			.0658***	.0745***
			(.019)	(.0246)
1[Filing city, not ZIP, hires examiner]			.0197**	.0341***
			(.0081)	(.00947)
Observations	1,023,669	1,023,669	1,023,489	1,023,439
R-squared	.0806	.0972	.126	.144
Year FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No
City FE	No	No	No	Yes
Technology center FE	Yes	Yes	Yes	Yes
Experience FE	Yes	Yes	Yes	Yes
Examiner FE	No	No	No	No

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

Columns 1 and 2 here are analogous to the same columns from Table V in the body of the text. Columns 3 and 4 here are analogous to columns 1 and 2 of Table VI in the body of the text. They differ in that the coefficients reported here reflect Technology center rather than Patent class fixed effects. Standard errors are clustered at the examiner and firm level.

Table A.II: *Robustness of quality-related results to varying technology-related controls*

VARIABLES	(1) Count	(2) Count	(3) Wins. count	(4) Wins. count
1[Revolving examiner]	1.66**	1.99*	.723*	.919
	(.755)	(1.08)	(.404)	(.565)
1[Filing firm hires examiner]	-9.64***		-5.7***	
	(2.52)		(2.03)	
1[Filing ZIP hires examiner]		-3.27**		-1.74**
		(1.46)		(.884)
1[Filing city, not ZIP, hires examiner]		-6.97***		-3.94***
		(.983)		(.459)
Observations	727,911	727,607	727,911	727,607
R-squared	.201	.203	.333	.335
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	No	Yes	No
City FE	No	Yes	No	Yes
Patent class FE	Yes	Yes	Yes	Yes
Experience FE	No	No	No	No
Examiner FE	No	No	No	No

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

The columns here are analogous to columns 3, 5, 7, and 9 of Table IX, however they differ in that the coefficients reported here reflect Technology center rather than Patent class fixed effects. Standard errors are clustered at the examiner and firm level.