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FROM REVOLVING DOORS TO REGULATORY CAPTURE? EVIDENCE FROM PATENT EXAMINERS

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ABSTRACT

Many regulatory agency employees are hired by the firms they regulate, creating a "revolving door" between the public and private sectors. 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 leniency extends to prospective employers. Effects are strongest in years when firms are actively hiring examiners (e.g., all else equal, our estimates are attenuated during recessions). Conditional on being granted, affected patents have broader scope, as measured by claims, and lower quality, as measured by forward citations.

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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 ineffectively address market failures and incompletely protect public interests (Stigler, 1971; Peltzman, 1976; Breyer and Stewart, 1979).[1][2] Despite this concern, there is very little empirical analysis of public-to-private sector transitions by 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"). When a patent application arrives at the agency, it is routed to a USPTO employee, called an examiner, working in Alexandria, Virginia.[3] These examiners decide whether or not to grant a patent, 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 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. Doing so provides increased pay and mobility but creates a potential conflict of interest, since many *examiners evaluate 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. For over one million applications, we observe the name and address of the filing firm, the name and unique identifier of the examiner, the decisions he or she made, and the dates on which they were made. For applications that receive grants, we observe the number of citations that the patent receives, which provides a commonly used proxy of quality. Separately, we observe periodic snapshots of the full list of licensed practitioners, including their name and unique identifier, as well as the name and address of their employer. We also supplement this data with biographical information. At the decision level, our data links the individuals setting regulation and the firms affected by regulation to the individuals hired by regulators and the firm that hire those individuals. To our knowledge, it is the first paper to make these connections.

We investigate whether revolving door examiners behave differently towards future and prospective employers, whether these differences suggest regulatory capture, and whether they affect the quality of regulation. While the first question is a straightforward test of regulatory consistency, the second is challenging. It requires us to distinguish actions whose intent is to gain favor with an employer from actions that reflect employee preferences or reverse causality. Features of the setting aid this process. For instance,

[1] See also Levine and Forrence (1990) and Laffont and Tirole (1991) for more comprehensive reviews of earlier work.

[2] It could also erode trust in government. This concern is particularly timely given polls that indicate only 19% of the public believes that the federal government "is run for the benefit of all the people." By comparison, in 1964, when Pew first administered the poll, they obtained a 65% share. Likewise, about three-quarters (76%) believe it is "run by a few big interests," which contrasts with the 29% share obtained 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.)

unlike prior work that relied upon across-employee differences or variation along other dimensions, we can exploit within-employee within-firm differences. Additional tests, described below, incorporate auxiliary data. The third question is also challenging. It requires us to rule out examiners merely *appearing* favorable by selecting high quality, easy-to-grant applications from the queue, as this would have no real effect on intellectual property (IP) protection. The allocation of applications to examiners at the USPTO, however, is quasi-random along the dimensions about which we are concerned (Williams, 2013; Righi and Simcoe, 2017).[4]

We begin by showing that revolving door examiners grant 12.6-17.6% more patents to firms that later hire them. This result is robust to varying the level of controls, such as examiner and firm fixed effects. Likewise, it is robust to limiting the sample to firms that hire at least one examiner, which reduces the number of observations by roughly two-thirds.

We then ask whether revolving door examiners extend this leniency to prospective employers. 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 revolving door examiners grant more patents to other firms in close proximity to the one that hired them. We find, for example, that grant rates are 6.2-11% higher for firms that are located in the same ZIP code as the firm that hired examiner and that are location-based estimates are robust to the types of specification changes and sample restrictions described above.

Next, we consider complementary 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 the main effect should load on periods when firms are actively hiring. We find precisely this relationship. Since the level of economy-wide private sector hiring is plausibly independent of idiosyncratic examiner preferences, this result mitigates endogeneity concerns.

Second, we collect data on where each examiner was educated prior to joining the USPTO. We show that the location of a revolving door examiner's alma mater predicts where he or she will be hired after leaving the agency. We then show that revolving door examiners grant more patents to firms located near their alma mater. Moreover, we find that this result does not hold for "non-revolving door" examiners—ones that continue to work for the USPTO through the end of our sample period. Since educational backgrounds are "realized" prior to examination decisions, this result mitigates reverse causality concerns.

We also study patent claims. From them, we construct two measures that the USPTO suggests capture how much patent scope is reduced in the examination process, which proxy for examiner toughness. We find that effects on the *intensive margin* of IP protection are consistent with those on the *extensive margin*.

Finally, we study patent quality. Examiner leniency lowers the threshold at which applications result in grants, so patents granted by revolving door examiners to their future and prospective employers should receive fewer citations. We find exactly this pattern in the data. For instance, we estimate that a patent granted to a firm that subsequently hired its examiner receives 21-27% fewer citations on average.

While these results are consistent with regulatory capture, we caution that they do not, on their own, suggest restricting where or when federal employees may work after they leave government. "Cooling off" periods, akin to ones faced by accountants and lobbyists, can adversely affect regulators. For one, prohibiting examiners from working in private sector roles for which they are best suited can dissuade

[4]See Footnote 7 for a discussion of assignment that is informed by the cited works.

talented people from joining the USPTO in the first place. For another, even if the revolving door biases examiners, it may induce significantly more effort from them. The variation we exploit cannot cleanly evaluate these tradeoffs. Likewise, we emphasize that none of the patterns we report imply explicit agreements; tacit understandings or even social norms have the same empirical signature and create the same distortions. Similarly, we clarify that the alleged leniency towards prospective employers need not be the main factor determining which examiners are hired or the most pressing problem faced by this particular agency. Instead, the USPTO merely serves as a data-rich environment that provides insight into the revolving door phenomena.

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 issues arise in the private sector. Cornaggia, Cornaggia, and Xia (2016) study credit rating analysts that join firms whose credit they previously rated. They 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 leveraged finance credit analysts, although she also finds that this group of analysts is more accurate overall. Both studies are carefully executed but leave open the question of how easily the results extend to government employees, whose work undisciplined by market forces.

Regulatory capture can also permeate lobbying. However, for lobbyists, it is typically past relationships rather than future employment that influence behavior (Vidal, Draca, and Fons-Rosen, 2012; Bertrand, Bombardini, and Trebbi, 2014). Notably, this literature also differs in that policy outcomes are often unobserved, so changes in statutes and laws must be inferred. Likewise, we relate to a broad literature that aims to understand how US patent rights are allocated (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 played in it by patent agencies, attorneys, and examiners. Section 3 describes the data. Section 4 presents our model. Section 5 describes our estimation strategy and reports results, while Section 6 concludes.

2 Institutional background

Patent application and grant process

The USPTO is tasked with issuing patents, which grant the inventor the temporary 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

[5] A patent can have multiple applicants, such as individuals, government agencies, universities, or firms.

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.

The core of the application is a set of one or more claims. These define the patent's exclusionary scope—the extent of IP 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" (US Patent No. 821,393). 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. 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. Thus, abandonments are best viewed as cases in which examiners insist on such narrow claims that filers no longer deem their applications worthy of pursuit. By extension, grants are a discrete measure that approximates a slightly more complicated result.

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, commonly called the "GS" 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 $75,000 and $104,000 per year, respectively (at the time of writing in 2024).[8]

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.

[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] Righi and Simcoe (2017) show 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 *http://federalpay.org* for individual and summary statistics for federal salaries (retrieved June 17, 2024). See *https://www.opm.gov/policy-data-oversight/pay-leave/salaries-wages/2024/general-schedule/* for GS salaries across metropolitan areas (retrieved June 17, 2024).

Yet for several reasons this approval is mostly a "rubber stamp."[9] 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. 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 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.

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. 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 IP 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, one prominant firm states "most of our associate[s] and other attorneys are registered to practice before the PTO, and many are former PTO Examiners,"[10] while another states, "more than a dozen [of our] attorneys had prior careers at the USPTO."[11]

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 other's identity. To see this, one need go only as far as the USPTO cover page, which sits atop the large volume of transmissions that pass from the agency to the filer. Displayed prominently on each, alongside very little additional information, are the name and address of the filing firm *and* the first and last name of the examiner.

To make this concrete, Appendix A provides examples of cover sheets that preceded the non-final rejection notice for three well-known 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 Tables III and IV, respectively). Tantamount, the examiner knows that the firm knows

[9] 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). See also Frakes and Wasserman (2016b).

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

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

exactly whose decision this is. Interestingly, nowhere does the form provide space to list the firm actually employing the named inventor.[12]

Filers can also request interviews with examiners to discuss an application. In these cases, examiners and filers speak on the phone or meet in person, typically at USPTO headquarters, providing for richer interactions. (The agency does not require examiners to record interviews, however, so we cannot observe how frequently they occur.)

3 Data

3.1 Sources

The data come from four main sources. 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.[13] PatEx also reports the technology center and patent class associated with each application, which characterize different types of technology. The dataset covers substantially all filings between November 29, 2001 and December 31, 2015 and, for applications that result in grants, extends back to at least July 1995 (Graham, Marco, and Miller, 2015). We restrict attention to utility patents, which comprise just over 94% of the PatEx.[14] We also restrict attention away from "pending" applications, as their relevant outcomes have not yet been realized.

Patent Practitioner Rosters list all individuals registered to practice before the USPTO, as well as the name and address of their current place of business.[15] 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. Bi-annual snapshots are available through archived versions of the agency's website.[16] 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, five percent sample of examiners and various online resources (described below).

Thomson Innovation counts "forward" patent citations (i.e., the number of times a patent is cited by other

[12]Notably, neither Google, GoPro, nor Square appears anywhere on these sheets. This is not to say that the examiner is always unaware of or cannot ascertain the identity of the firm that developed the technology; it is only to point out that this information is much less salient than the law firm filing the application.

[13]USPTO. https://shorturl.at/xnq0q (retrieved on October 16, 2016).

[14]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). Applications prior to December 2000 may be incomplete, as they were not subject to the America Inventors Protection Act ("AIPA"), which stipulated that all non-provision patent applications must be published. The effects of this legislation on the coverage of PatEx are evident in the data; see Appendix B for more details. Law changes took effect on June 8, 1995 that, among other things, affected the lifespan of US patents. To ensure comparability over time, we begin our analysis in July 1996.

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

[16]January 2009, October 2011, February 2013, and March 2015 versions are available through the Internet Archive. http://web.archive.org/web/*/https://oedci.uspto.gov/OEDCI/ (retrieved on October 16, 2016).

published patents). This measure proxies for quality of the underlying innovation.[17][18] Citations accrue from patents published by all national offices. This data merges exactly to PatEx on the USPTO-assigned patent number, which is a unique identifier.

Educational histories combine information from an employment-oriented social networking website, Martindale-Hubbard's attorney directory, and the employee profiles hosted on the websites of law firms. We record the name and address of any degree-granting institutions that the examiner attended[19] and merge to PatEx on the basis of first, middle, and last name. We measure the distance between educational institutions and filing firms using the latitude and longitude of the five-digit ZIP assigned to each. 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.

3.2 Summary statistics

We define *Grant* equal to one if PatEx indicates the application's status is "issued" and zero if the status is "abandoned." Also, we define *Citations* equal to the number of references from other published patents. Finally, we define *Year* equal to the calendar year of the examiner's last action on the application (i.e., the date of allowance if *Grant* = 1, and the date of the last rejection preceding abandonment if *Grant* = 0).

Table I summarizes the data used to study grants. 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: *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 summarizes the data used to study quality. Citations are only available for granted patents,

[17] We considered using the proportion of claims that are invalidated during litigation, but litigation is infrequent, and most suits settle before trial. We also considered using the proportion of decisions that conflicted with those of the European and Japanese patent offices, but multi-jurisdictional differences also arise too infrequently.

[18] Since the USPTO starts falling examiner-added citations in 2001 and our sample begins in 1995, we count all citations.

[19] 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, so 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.

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: *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 summarizes the data across the firms that most frequently hire examiners. Notably, all are law firms. Most specialize in IP or at least heavily emphasize this part of their practice. Grant rates vary across firms, but most are close to average among this subset of firms, which equals 65% (not explicitly shown).

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 is spread 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.

To understand how these figures relate to one another, we turn to our model.

Table III: *Summary of firms that frequently hire examiners*

	Count		Mean	
Firm	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. "Grant" reports the average rate across patents. Firm names are abbreviated. Townsend and Townsend and Crew merged with the larger firm of Kilpatrick Stockton to become Kilpatrick Townsend & Stockton. We abbreviate Townsend and Townsend and Crew as "Townsend" and abbreviate its merging partner and successor as "Kilpatrick."

Table IV: *Summary of cities in which examiners are 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. "Grant" reports the average rate across patents.

4 Conceptual framework

Examiners, indexed by i, evaluate patent applications filed by firms, indexed by j. In our model, examiners have agency: they take as given the norms and policies of the firms, industry, and society at large, which are longer-lived entities, and then make decisions.[20] We denote the extent of IP protection that the examiner grants the firm by y_{ij}, and we denote i's desire to work at j by x_{ij}. We let η_{ij} and ϵ_{ij} measure the protection that the examiner believes the firm should receive, with the difference being that η may be co-determined with x while ϵ is not. The examiner sets

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

Regulatory capture corresponds to $\theta > 0$: in the broadest terms, all else equal, a regulatory agency employee's desire to be hired by a firm it regulates results in special treatment.

We let ℓ represent firm attributes that are observable by the econometrician and influence examiner preferences about where he or she would like to work but do not otherwise influence the examination process. Various 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), so we let ℓ reflect geographical differences. We then let

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

where λ measures how strongly location influences post-USPTO employment preferences.

Finally, we let W represent other factors observable by the econometrician. (Precisely which factors are included in W is estimation-specific, so we postpone this discussion until the following section.) We define $\eta_{ij} \equiv W_{ij}\gamma + \tilde{\eta}_{ij}$, $\epsilon_{ij} \equiv W_{ij}\pi + \tilde{\epsilon}_{ij}$, and $\nu_{ij} \equiv W_{ij}\mu + \tilde{\nu}_{ij}$.

If we abstract away from location-based preferences (i.e., set $\lambda \equiv 0$), then

$$y_{ij} = \theta \tilde{\nu}_{ij} + W_{ij}\zeta_1 + (\theta\kappa + 1)\tilde{\eta}_{ij} + \tilde{\epsilon}_{ij}, \tag{3}$$

where $\zeta_1 = (\theta\kappa + 1)\gamma + \theta\mu + \pi$. In this case, variation in $\tilde{\nu}$ traces out θ. If instead we focus on location-based preferences (but abstract away from other factors shifting x but not y by assuming the distribution of $\tilde{\nu}$ is degenerate), then

$$y_{ij} = \theta\lambda\ell_{ij} + W_{ij}\zeta_2 + (\theta\kappa + 1)\tilde{\eta}_{ij} + \tilde{\epsilon}_{ij}, \tag{4}$$

where $\zeta_2 = (\theta\kappa + 1)\gamma + \pi$. In this other case, variation in ℓ informs us about θ.

[20] We remain agnostic as to whether equation 1 reflects implicit or explicit understandings, though we find the former more likely in this setting. For simplicity, we also model the outcome of patent examination and abstract away from the underlying choice problem. In the latent problem, non-x terms map to the utility derived from accurate decisions (e.g., those that avoid censure by USPTO management), while x maps to utility derived from securing a high-paying private sector position and/or not violating a social norm.

5 Estimation and estimates

5.1 Patent grants

Our main measure of IP protection is the binary outcome *Grant*.

5.1.1 Hiring firms

We start by assessing whether examiners are lenient on firms that hire them. To interpret our initial estimates, we abstract away from location differences, and we assume that examiners apply to positions at, and ultimately are employed by, firms for whom they most want to work (but we relax this restriction below). Formally, we define $x^{\star}_{ij}$ equal to one if j hires i and zero otherwise, set $\lambda \equiv 0$, and assume that $\mathbb{E}[\tilde{v}|W, x^{\star} = 1] > \mathbb{E}[\tilde{v}|W, x^{\star} = 0]$. We then index years by t and estimate the following linear probability model:

$$Grant_{ijt} = W_{ijt}\phi_1 + \beta_1 x^{\star}_{ij} + \xi_{ijt}. \tag{5}$$

Before turning to our results, we emphasize that care must be taken when estimating these types of specifications. In particular, interpretations of β_1 are typically context-specific and deserve additional validation. The reason is easy to see. $\mathbb{E}[\tilde{\eta}|W, x^{\star} = 1] > \mathbb{E}[\tilde{\eta}|W, x^{\star} = 0]$, so if $\tilde{\eta}$ has meaningful variance, then β_1 can be positive even when θ is exactly zero. In other words, there is a risk that the econometrician infers regulatory capture when it does not exist. Features of our setting, data, and research design mitigate this risk, as described below.

The most common sources of bias arise from coincidental relationships that cause revolving door examiners to grant more patents than average or cause firms hiring examiners to be granted more patents than average. Unlike many prior studies, we can include employee and employer (i.e., examiner and firm) fixed effects in W. Doing so eliminates this particular problem and confines endogeneity to examiner-firm match-specific preference shocks that simultaneously influence hiring and granting behavior.

More nuanced sources of bias can emerge from the residual disturbances. One involves examiner-specific preferences for certain "brands" over others. For example, if an individual has a "taste" for Google, then he or she may be more likely to grant Google patents *and* work for Google after leaving the agency. Mitigating this concern, nearly all revolving door examiners join law firms, over which idiosyncratic tastes are plausibly much more homogeneous.[21][22]

Reverse causality can also occur. Our approach infers preferences that influence regulatory decisions from labor market outcomes, even though these outcomes are realized *after* the decisions are made. In some settings, agency employees may learn about the quality of a potential employer from their submissions, meaning that one who (purely by chance) receives high quality submissions from a particular firm will be more likely to grant it patents and apply to work for it. In our setting, however, potential employers are respected, established law firms whose offices and specialties are ranked by multiple, reputable industry publications, meaning that priors over quality are plausibly very tightly distributed (i.e., there is little room for updating beliefs). Even so, we revisit these issues below.

[21] An analogous problem arises when examiners have preferences over types of technology, examine a wide variety of them at the agency, and join firms that specialize. Limiting this possibility, examiners are administratively assigned to "art units," which are very narrowly defined. See Section 2 for examples of how narrowly art units are defined.

[22] Separately, notice that bias can also arise when 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.

Table V reports estimates from equation 5. 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 firms that do not. Including firm fixed effects has almost no impact, while including (a large number of) examiner fixed effects attenuates the estimates by a small amount. Across all specifications, coefficients are significant at the 5% level or less.

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

VARIABLES	(1) Grant	(2) Grant	(3) Grant	(4) Grant
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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *This table reports the result of estimating equation 5. The first row reports the coefficient on an indicator for whether an examiner was later hired as a practitioner, and the second reports* $\hat{\beta}_1$. *Standard errors are clustered at the examiner and firm level.*

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 first row as proof of capture. Nonetheless, the combined size of the coefficients means that filers face "inconsistent regulation."

5.1.2 Prospective employers

Next, we assess whether examiners' leniency extends to 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 limit the influence that $\tilde{\eta}$ may have on our estimates, we discard all observations in which the filing firm hired the examiner for the purposes of this exercise.

Formally, we restrict attention to $x^{\star}_{ij} = 0$, and we assume that ℓ can be broken down into two components. We define $\ell^{\star}_{1ij}$ equal to one if j resides in the ZIP code in which i was hired and zero otherwise, and we define $\ell^{\star}_{2ij}$ equal to one if j resides in the city (but not ZIP code) in which i was hired and zero otherwise. We then estimate

$$Grant_{ijt} = W_{ijt}\phi_2 + \delta_2\ell^{\star}_{1ij} + \rho_2\ell^{\star}_{2ij} + \tau_{ijt}. \tag{6}$$

Table VI reports the result. Half or more of the leniency that revolving door examiners extend to firms

that hire them is extended to other firms in the area. For example, the final column's coefficients, which reflect examiner, firm, and year fixed effects imply that revolving door examiners grant 5.0 percentage points more patents to firms in the same ZIP code as the firm that hired them. The comparable figure in Table V is 8.45 percentage points.

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

VARIABLES	(1) Grant	(2) Grant	(3) Grant	(4) Grant
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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *This table reports the result of estimating equation 6. The first row reports the coefficient on an indicator for whether an examiner was later hired as a practitioner, while the second and third rows report* $\hat{\delta}_2$ *and* $\hat{\rho}_2$*, respectively. The sample is restricted to observations for which* $x^\star = 0$*. Standard errors are clustered at the examiner and city level.*

5.1.3 Differences in hiring intensity over the business cycle

To strengthen our interpretation of the coefficients, we exploit the fact that hiring intensity varies considerably over the sample period. For example, three times as many examiners are hired by private sector firms in years with robust growth, such as 2005, than in years periods of little to no growth, such as 2009. We can test whether smaller estimates are obtained in periods when firms are rarely hiring examiners and vice versa. Formally, we let $\mathbb{1}\{t = \tau\}$ denote an indicator that equals one when the examination occurs in year τ and estimate

$$Grant_{ijt} = W_{ijt}\phi_3 + \sum_{\tau=2001}^{2016} \beta_{3\tau}\mathbb{1}\{t = \tau\}x^\star_{ij} + \omega_{ijt}, \tag{7}$$

where W includes firm and year fixed effects and terms that interact an indicator for revolving examiner with year dummies (i.e., $\mathbb{1}[\text{Revolving examiner}_{ij}] \times \sum_{\tau=2001}^{2016} \mathbb{1}\{t = \tau\}$). We then plot estimates of $\beta_{3\tau}$ against two separate measures of hiring intensity, described below.

The top panels of Figures I and II report these results. In the first instance, we measure hiring intensity using the number of examiners hired in a given year. For the second, we use a broader, economy-wide measure of hiring intensity: year-over-year private sector seasonally-adjusted net job gains, as measured by

the Bureau of Labor Statistics.[23] Consistent with the idea that examiners are more lenient towards future employers when they are being considered for employment, both panels depict positive relationships. For instance, in years experiencing weak private sector job growth, $\hat{\beta}_{3\tau}$ averages out to 7.4 percentage points, whereas in years with strong growth, that same figure is 16 percentage points.

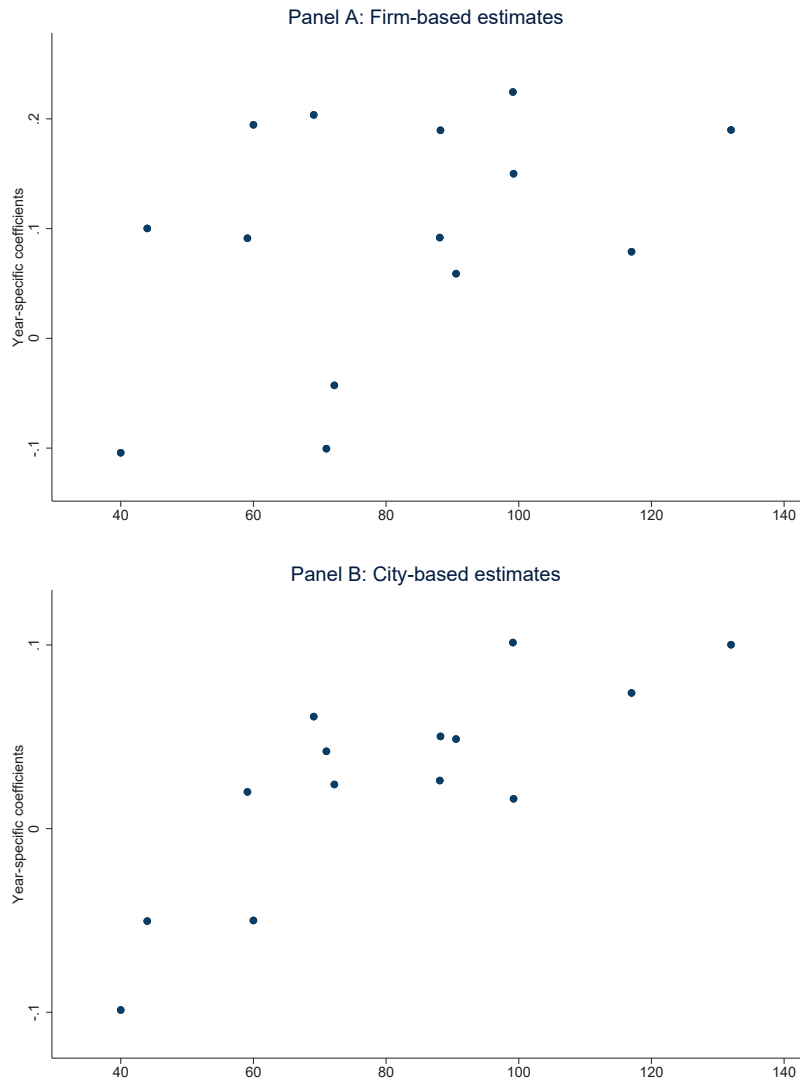

Figure I: *Main effects are largest when examiner hiring is most frequent*

Each plotted point corresponds to a different calendar year. The y-axes of Panels A and B measure $\hat{\beta}_{3\tau}$ and $\hat{\delta}_{4\tau}$, respectively, while the x-axes measure the number of examiners hired.

We then repeat this exercise in the spirit of Section 5.1.2, exploiting location- rather than firm-based differences. To ease exposition, we construct a single examiner-location match variable by defining

[23] US Dept. of Labor, Bureau of Labor Statistics. "Current employment survey." http://www.bls.gov (retrieved July 29, 2017). BLS-reported economy-wide employment changes cannot perfectly proxy for examiner openings in the private sector. Instead, they provide a reasonable approximation (e.g., the left-most three x-axis values correspond to the Great Recession and tail end of the Dotcom crash). Sharper cuts of employment data would require subjective calls over what "slice" is most appropriate.

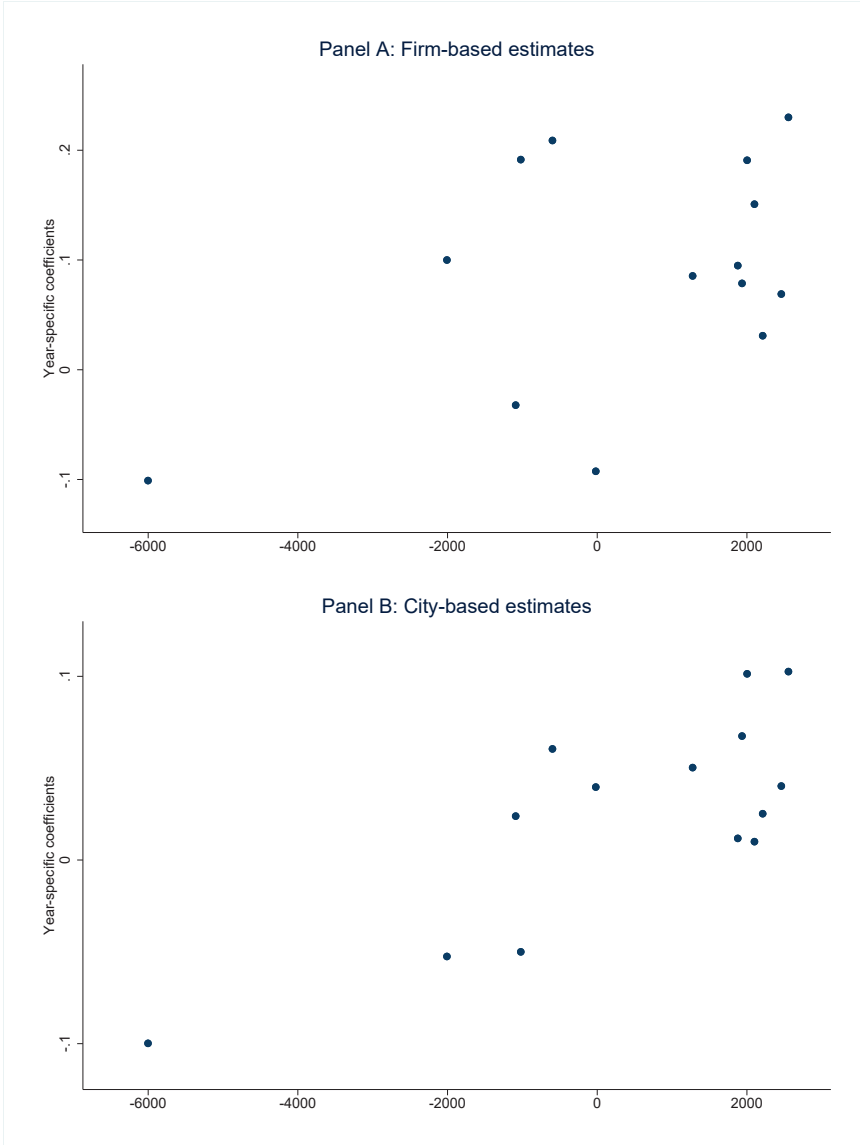

Figure II: *Economy-wide employment changes yield similar results to observed examiner hires*

Each plotted point corresponds to a different calendar year. The y-axes of Panels A and B measure $\hat{\beta}_{3\tau}$ and $\hat{\delta}_{4\tau}$, respectively, while the x-axes measure year-over-year private sector seasonally-adjusted net job gains.

$\ell^{\star}_{ij} \equiv \max\{\ell^{\star}_{1ij}, \ell^{\star}_{2ij}\}$. Following the logic we described above, we also restrict the sample such that $x^{\star}_{ij} = 0$ and substitute firm-specific dummies for location-specific ones. We then estimate

$$Grant_{ijt} = W_{ijt}\phi_4 + \sum_{\tau=2001}^{2016} \delta_{4\tau}\mathbb{1}\{t = \tau\}\ell^{\star}_{ij} + \omega_{ijt}. \tag{8}$$

Finally, we plot estimates of $\delta_{4\tau}$ against our measures of hiring intensity.

The bottom panels of Figures I and II report the results. In short, both depict even clearer positive

relationships than the top panels do.

5.1.4 Differences in location preferences formed before USPTO employment

To further address reverse causality, we propose constructing a variable that shifts the preferences of agency employees over future employers before they are hired by the regulator but does not directly affect their decisions while employed by the agency. Survey-based studies indicate that workers prefer jobs near where they have lived and/or were educated. Boyd, Lankford, Loeb, and Wyckoff (2005), for instance, find that most public school teachers take jobs close to their alma maters, while Reininger (2012) extends the result to all college graduates.[24]

Based on these findings, we collect information about where examiners received their post-secondary education. For each application assigned to a revolving door examiner, we calculate (a) the distance from the filing firm to the examiner's closest alma mater and (b) the distance from the filing firm to the firm that hired the examiner away from the USPTO. We residualize each measure using examiner, city, and year fixed effects, and plot the resulting values against one another. A positive relationship between x- and y-axis values implies that, conceptually speaking, examiners are likely to be hired by firms near their alma maters.

Figure III reports the result. The unambiguous upward slope is consistent with the idea that revolving door examiners apply to and accept offers from firms that are, for example, close to family and friends and/or in cities with which they are already familiar. Appendix Figure A.II shows that if we use "unresidualized" (i.e., raw) distances or winsorize them, nearly identical graphs are obtained.

Given these results, we restrict attention to applications evaluated by revolving door examiners and regress *Grant* on the distance from the filing firm to the examiner's closest alma mater and control variables. We then relax the sample restriction and re-estimate the parameters.

Table VII reports the results. Revolving door examiners grant significantly more patents to firms near their alma maters, mitigating concerns about reverse causality. In contrast, grant rates of non-revolving door examiners do not vary along this dimension, reducing the risk that *boosterism* drives the main result.[25] Winsorizing 1% of distances produces similar estimates, ensuring robustness to how we treat a small number of large distances attributable to Hawaii.

5.1.5 Robustness to sample restrictions

Many filings are submitted by firms that never hire an examiner. Firms that do are disproportionately large law firms with established IP practices. At least in theory, even within the narrow confines of a technology center (i.e., administrative unit within the USPTO), revolving door examiners may be especially predisposed towards the types of inventions on which these firms' applications are based.

To address this final concern, we restrict attention to applications filed by firms that hire at least one examiner and re-estimate the specifications that produced columns 1 and 4 of Table V. For completeness, we

[24] 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)).

[25] Supporting organizations near or related to one's home or school, independent of how deserving those organizations are, is commonly called "boosting." Were this driving our odd-numbered rows' estimates, we would expect our even-numbered rows' estimates to also be positive.

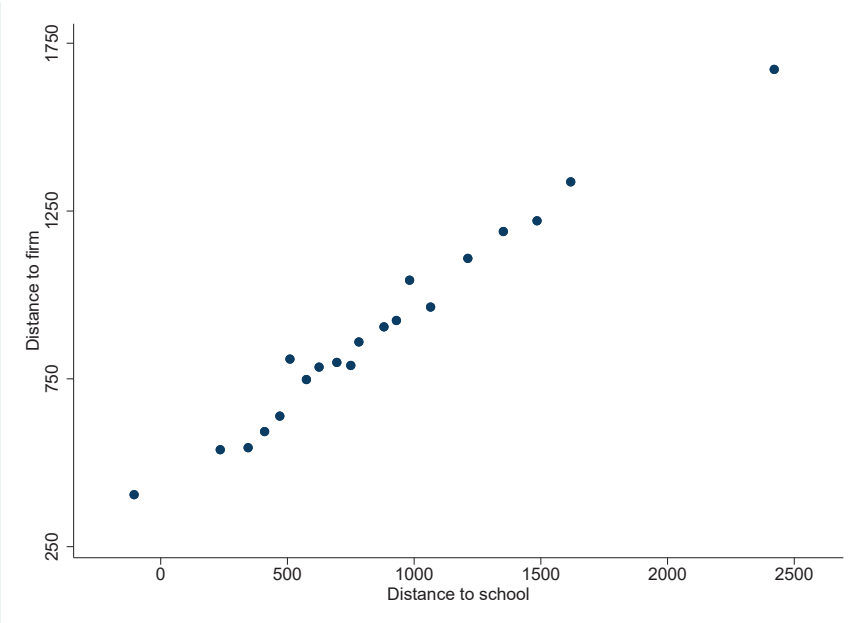

Figure III: *Alma mater location predicts post-USPTO employer location*

The unit of observation is an application. The x-axis measures the distance from the filing firm to the examiner's closest alma mater, while the y-axis measures the distance from the filing firm to the firm that hired the examiner (so the sample is implicitly restricted to applications evaluated by revolving door examiners). Distances are measured in miles and residualized using examiner, city, and year fixed effects. Observations are binned according to x-axis values, and means within each bin are plotted along the axes.

then restrict attention to applications filed by firms in cities that hire at least one examiner and re-estimate the specifications that produced columns 1 and 4 of Table VI.

Table VIII reports the results. Despite the fact that our sample restrictions remove around 70% and 30%, respectively, of the observations, all of the coefficients are similar to the ones we previously reported. For instance, here we estimate β_1 equal to 0.113 and 0.076, depending on the specification, whereas we obtained 0.119 and 0.0845 in Table V. Likewise, here we find that δ_2 equal to 0.0722 and 0.0496, whereas earlier we obtained 0.0658 and 0.497, respectively (and for ρ_2, 0.0342 and 0.0244 in comparison to 0.0197 and 0.023).

5.1.6 Intensive margin estimates

Examiners also affect patent scope—*they make decisions about how much IP protection to grant on the intensive margin* (Freilich, 2016; Kuhn, 2016). Following the USPTO Office of the Chief Economist, we measure scope two ways: 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 removing claims altogether. Lenient examination does not.

Although our claims-based findings are consistent with those obtained using the binary measure *Grant*, our discussion here is comparatively brief. In short, the claims data available to us contains parsing errors that are infeasible to correct—millions of claims comprising non-standard ASCII characters both manual

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

VARIABLES	(1) Grant	(2) Grant	(3) Grant	(4) Grant
Filing firm distance to RE's school	-.00843**	-.00849**		
	(.00331)	(.0033)		
Filing firm distance to non-RE's school		.00139		
		(.00284)		
Filing firm wins. distance to RE's school			-.0098**	-.00988**
			(.00423)	(.00422)
Filing firm wins. distance to non-RE's school				.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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *Examiners are characterized as either revolving ("RE") or non-revolving ("non-RE"). In columns 1 and 3, we restrict the sample to revolving examiners; in columns 2 and 4, we include all examiners. All distances are measured in miles. "Wins." means 1% of values are winsorized. Standard errors are clustered at the examiner and city level.*

and automatic correction impossible or unreliable. As these errors can affect our estimates, summarize them here and report detailed statistics in Appendix C.3.

The sample comprises about 457,000 applications that resulted in grants. Our first outcome equals the difference between the lengths of the shortest independent claim in the original application and the published patent. On average, patent prosecution increases the shortest independent claim by about 65 words. In cases where the filing firm later hires the examiner, however, we find that between 11.5 and 13.9 fewer words are added, depending on the specification. Fewer words are also added to applications filed by firms located near the one that later hired the examiner.

Our second outcome equals the difference between the number of independent claims in the original application and the published patent. 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. Expectedly, fewer claims are also removed from applications filed by firms located near the one that later hired the examiner.

5.2 Patent quality

$\theta > 0$ implies that, all else equal, grant rates are rising in x. By extension, conditional on *Grant*, expected quality is falling in x. We can test this implication in our data. Following convention, we measure quality using forward citations and estimate a linear model given by

$$Cites_{ijt} = W_{ijt}\phi_5 + \beta_5 x^{\star}_{ij} + \psi_{ijt}. \tag{9}$$

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

VARIABLES	(1) Grant	(2) Grant	(3) Grant	(4) Grant
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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *For columns 1 and 2, we restrict the sample to filing firms that hired at least one examiner and then report the result of estimating equation 9. For columns 3 and 4, we (a) restrict the sample to filing firms located in cities where at least one examiner was hired, (b) restrict the sample such that $x^\star = 0$, and (c) report the result of estimating equation 10. Standard errors are clustered at the examiner and firm level in columns 1 and 2 and at the examiner and city level in columns 3 and 4.*

We then restrict attention to $x^\star = 0$ and estimate

$$Cites_{ijt} = W_{ijt}\phi_6 + \delta_6 \ell^\star_{1ij} + \rho_6 \ell^\star_{2ij} + \varepsilon_{ijt}. \tag{10}$$

where, in the case of Equation 10, we restrict to (i, j, t) observations such that $x^\star_{ij} = 0$. To address the count nature of the data (Wooldridge, 1999; Rysman and Simcoe, 2008; Bertanha and Moser, 2016), we also estimate negative binomial and quasi-maximum likelihood Poisson models.

Table IX reports the results. 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.[26]

Across all specifications we see that the patents awarded by revolving door examiners to their future and prospective employers receive many fewer citations—*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

[26]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.

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

VARIABLES	(1) Neg. bin. cites	(2) Neg. bin. cites	(3) Poisson cites	(4) Poisson cites	(5) Cites	(6) Cites	(7) Cites	(8) Cites
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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *Odd-numbered columns report results from estimating equation 9. Even-numbered columns report results from estimating equation 10 on the subset of observations for which* $x^* = 0$. *Robust standard errors are reported for non-linear specifications. In columns 5 and 6, standard errors are clustered at the examiner and firm level, while in columns 7 and 8, they are clustered at the firm and city level. 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 27 for a brief explanation.*

linear model provides similar size effects.[27] 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.

6 Conclusion

The premise of our analysis was simple. First, many government employees regulate firms for whom they desire to work. Second, though elaborate schemes are possible, social norms typically suffice for disparate, lenient treatment. Informally speaking, no grand conspiracy is required for a revolving door to produce regulatory capture; one may simply avoid aggravating an individual from whom they are about to ask an important favor. Asymmetries typically exacerbate the risk. For one, regulatory decisions often have a concentrated effect on the regulated firm and its associates but a very diffuse effect on everyone else. For another, being hired by a regulated firm usually involves a terrific increase in compensation.

We studied this risk using data on patent examiners. Examiners, in effect, regulate IP protection by deciding the contours of patent rights. Each has considerable discretion—individuals rather than teams reach decisions, and oversight is necessarily limited by the volume of applications that the agency receives. Moreover, grants confer important benefits to filing firms. Namely, they justify the large fees that agents and attorneys bill their clients for patent application and prosecution. Furthermore, at least anecdotally, agents and attorneys have opportunities to personally interact with their examiners.

Yet, many examiners are hired by the firms for whom these agents and attorneys work, i.e., the entities whose applications they evaluate. We assembled data that links examiners with firms *through the decisions they made while working at USPTO and the places they worked after leaving the agency*. Our analysis indicated regulatory capture. We exploited variation in which firm hired the examiner, which location the examiner was hired, which location the examiner was educated, and macroeconomic performance, and we measured differences in patent grant rates, scope, and quality—all pointed the same direction. How and whether policy should address the risks we describe remain an open question that merits additional research.

[27] 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.

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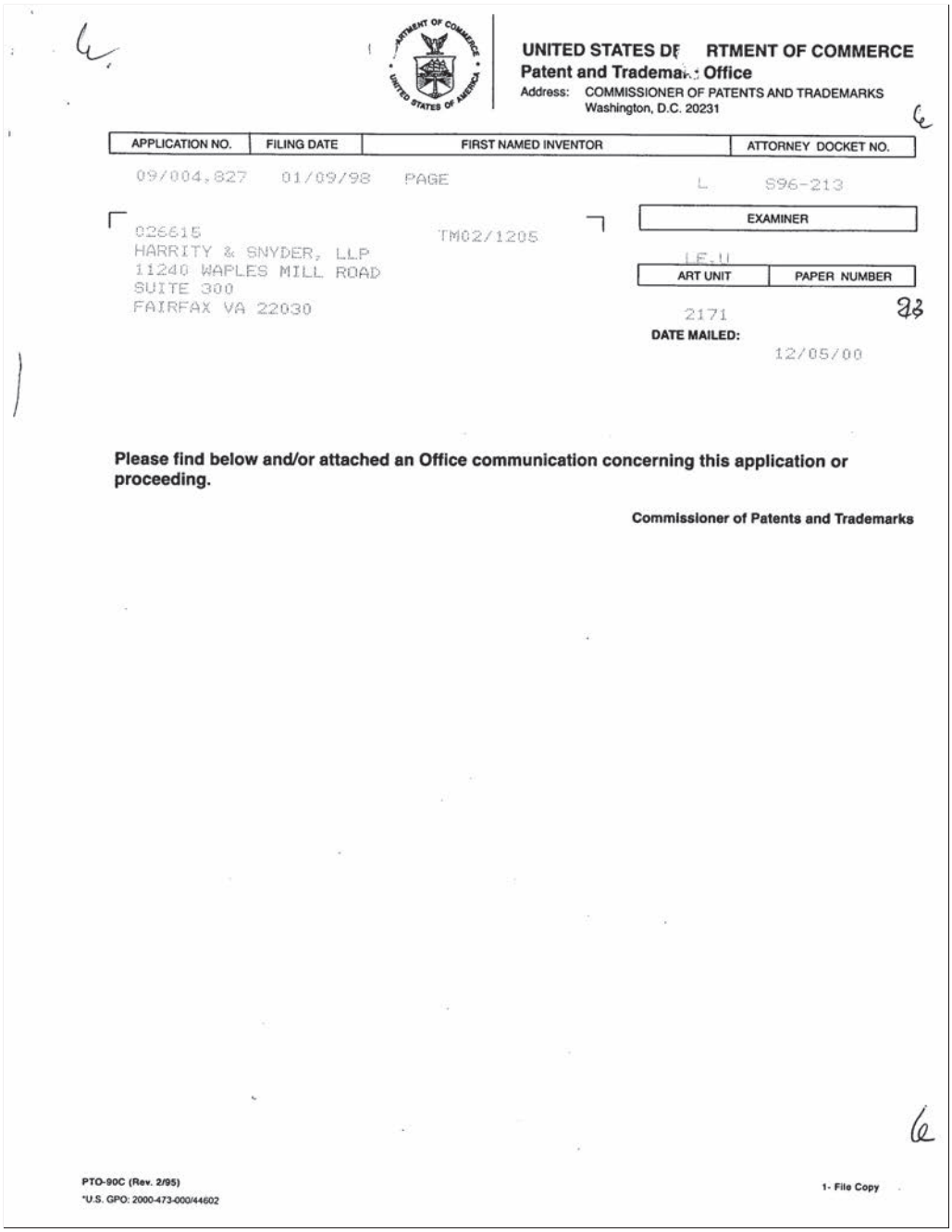
Online Appendix

A Selected examiner-firm communication

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



UNITED STATES DEPARTMENT OF COMMERCE
Patent and Trademark 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
HARRITY & SNYDER, LLP
11240 WAPLES MILL ROAD
SUITE 300
FAIRFAX VA 22030

TM02/1205

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: 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)

B Abandoned patent coverage pre- and post-AIPA

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

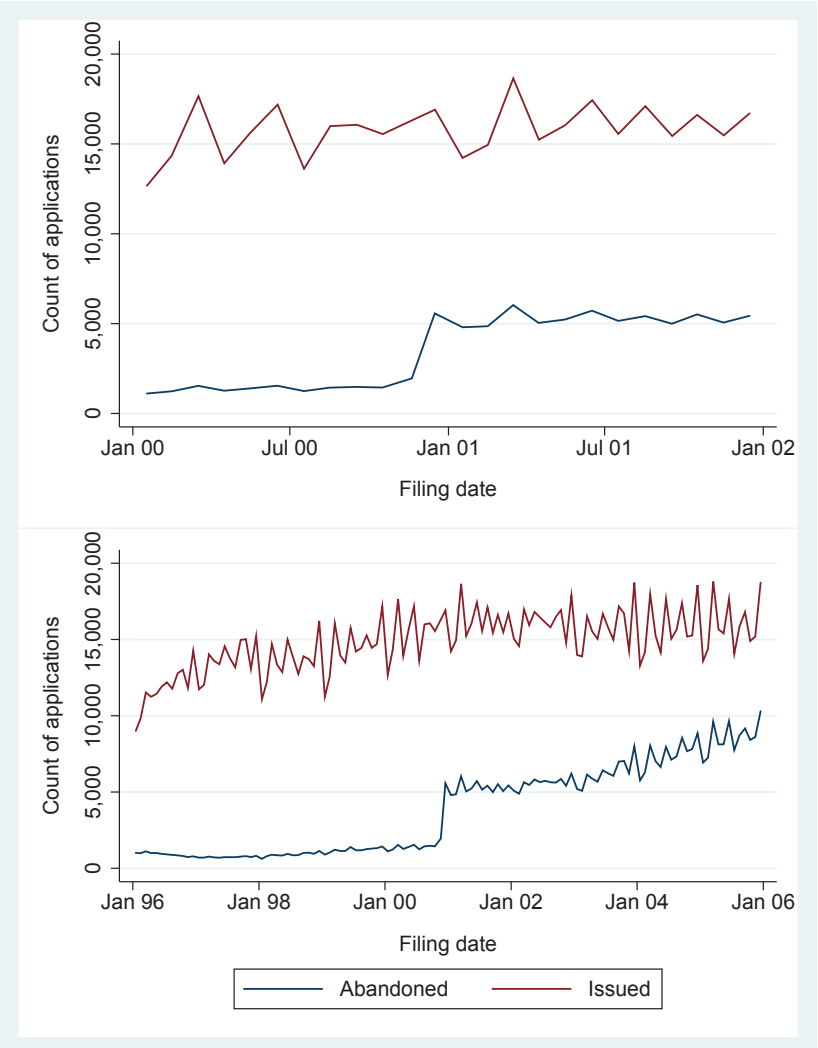


Figure A.I: *Abandoned but not granted applications increase with AIPA legislation*

The x-axis measures calendar month, while the y-axis measures the number of applications and grants. The sharp increase in abandoned applications around November 2000 is driven by a change in what applications are published rather than by a change in the grant rate.

C Supplementary analysis

C.1 Robustness to alternative measures of distances

To construct Figure III, we residualize distances using examiner, firm, and year fixed effects and plot the resulting values. In Figure A.II, below, we assess the robustness to alternative measures. In the top and middle panels, we plot raw (i.e., "unresidualized") distances, while in the middle and bottom panels, we winsorize 1% of the distances. Each panel closely resembles our original specific specification: all report unambiguously positive relationships between the x- and y-axes values.

C.2 Robustness to alternative technology classifications

Table IX in the body of the main text reports citation-related estimates when W includes *Technology center* fixed effects. *Patent class* fixed effects are more granular but create convergence problems for our non-linear specifications, as they involve many dummy variables. To assess whether our choice of controls affects our results, we re-estimate coefficients from our linear specifications and compare them to the ones we previously obtained.

Table A.I contains the comparison. In columns 1 and 3, we reproduce columns 5 and 7 of Table IX, while in columns 2 and 4, we report the new results (i.e., ones that include *Patent class* fixed effects rather than *Technology center* fixed effects in W). Despite a sharp increase in the number of discrete controls, the coefficients are very close to the ones we previously obtained.

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

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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *This table reports the result of replacing Technology center fixed effects with Patent class fixed effects and re-estimating coefficients that appear in the final four columns Table IX. Please see that table for more details.*

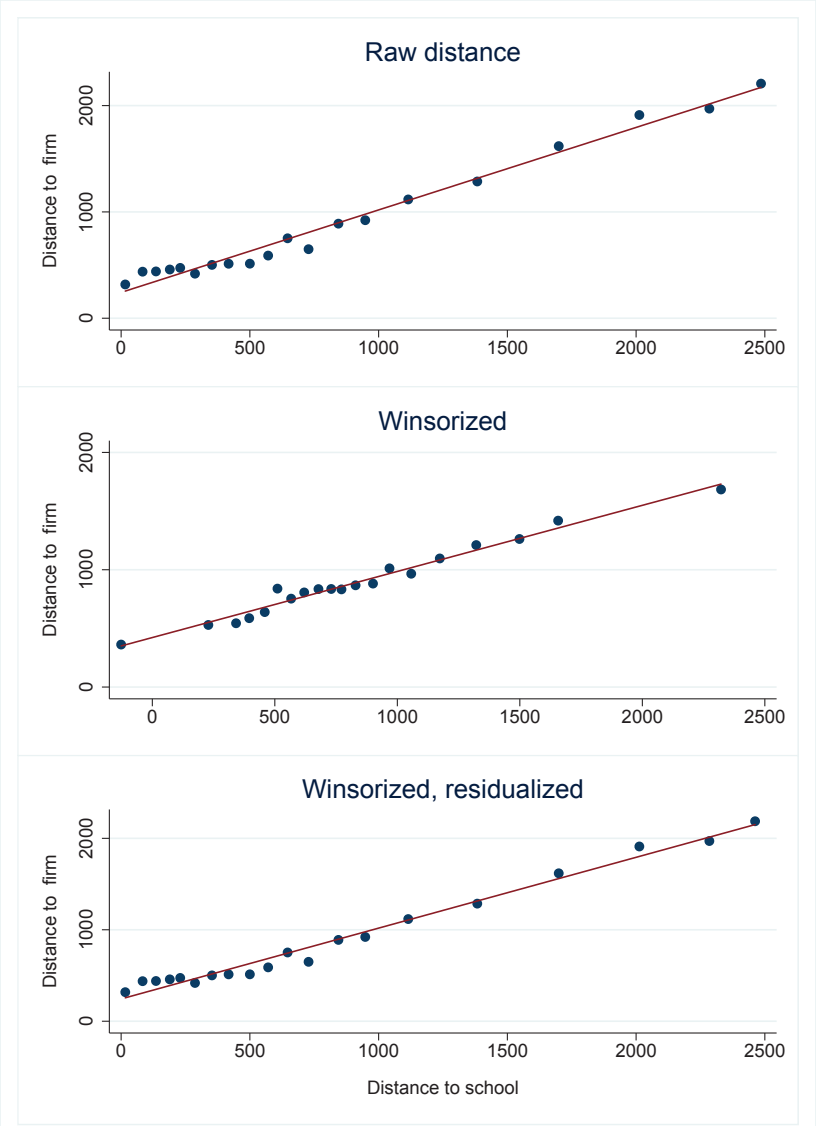


Figure A.II: *Robustness of relationship reported by Figure III*

The x-axis measures the distance from the filing firm to the examiner's closest alma mater, while the y-axis measures the distance from the filing firm to the firm that hired the examiner (so the sample is implicitly restricted to applications evaluated by revolving door examiners). Distances are measured in miles. Observations are binned according to x-axis values, and means within each bin are plotted along the axes.

C.3 Assessing claims

C.3.1 Data sources and summary

The Patent Claims Research Dataset provides application- and patent-level data including the number of independent claims and the length of the shortest independent claim.[28] The dataset covers applications (i.e., pre-grant publications) filed after November 29, 2000 and published before January 1, 2015, and it covers patents granted between January 1, 1976 and December 31, 2015. Since we are interested in the change in scope due to examination, we limit the sample to only applications and grants that match to one another. (There is currently no data on changes to claims for applications that are abandoned (Marco, Sarnoff, and deGrazia, 2016)). We further exclude observations that contain obvious errors. Namely, we exclude applications or patents without any independent claims, which are not legally permissible. We then impose identical restrictions on the claims-based dataset that we imposed on our grant-based dataset—we confine ourselves to utility patents, omit applications where the examiners name is missing, et cetera. (For the step-by-step instructions on dataset construction, see Appendix D.)

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

C.3.2 Estimation and estimates

We estimate equation 5 on all applications and report the result in Table A.III. We then restrict attention to application for which $x^\star = 0$, estimate equation 6, and report the result in A.II. All eighteen coefficients have the same signs as their grant-based counterparts.

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

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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *The outcome variable is the difference between the length of the shortest independent claim in the published patent and the submitted application. An increase in length of the shortest independent claim indicates less scope. Standard errors are clustered at the examiner and firm level in odd-numbered columns and at the examiner and city level in even-numbered columns.*

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

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

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

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *The outcome variable is the difference between the number of independent claims in the published patent and the submitted application. An increase in the number of independent claims indicates more scope. Standard errors are clustered at the examiner and firm level in odd-numbered columns and at the examiner and city level in even-numbered columns.*

D Dataset construction

PatEx

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

- Examiner name is missing
- Examiner name is erroneous, i.e. it is listed as "None" or "Not defined"
- Correspondence information is missing, i.e. no ZIP code is listed
- Correspondence information is erroneous, i.e. the ZIP code is neither five digits alone or five digits followed by a hyphen and four additional digits
- Status indicates a patent was issued but an abandonment date is given
- Status indicates an abandonment but an issue date is given
- Issue date and abandonment date are missing
- Filing date or disposal date is missing
- Art unit is missing

As per Section 3 in the body of the paper and the accompanying references in this appendix, we then take the following steps to prepare the dataset.

- Keep regular utility patent applications, i.e. exclude provisional, PCT, divisional, or continuation applications
- Keep applications filed on or after December 1, 2000 and grants filed on or after July 1, 1995

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

Claims supplement

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

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

We merge files from the intermediate steps on application number. The data contains obvious (but understandable) parsing errors. The text of older applications and grants is not digitally stored; the technical nature of many or most mean that highly non-standard characters included, exacerbating the already-difficult task of counting words; and the the sheer volume of claim text makes it prohibitively large to manually compute. Small errors can create large measurement problems, though. For example, in certain instances especially lengthy claims were erroneously broken into a very large number of single word claims, sharply skewing our measures away from the truth.

Instead of making a large number of subjective corrections to the data, we make a few blunt changes. First, we exclude a small number of observations for which the number of independent claims is zero or missing. (Both are legally impermissible.) We also exclude a small number of observations for which the minimum number of words among independent claims is less than eight. Manually inspection revealed that all claims shorter than eight words involving parsing errors.