

Disclosing Labor Demand

Gurpal Sran*

This Version: January 2023

(Original Version: November 2021)

Abstract

I study disclosure choices in job postings and the trade-off between two channels: detailed postings inform and attract optimal job applicants (i.e., a labor market channel) but could also inform competitors in labor and product markets (i.e., a proprietary costs channel). First, I provide evidence consistent with a proprietary costs channel: private firms and redacting firms are less specific in their postings, and postings are more often anonymous in industries with high levels of trade secrecy. Then, I exploit the introduction of federal trade secrecy protections (i.e., the Defend Trade Secrets Act, or DTSA) to assess the trade-off between the two channels. After the implementation of the DTSA, firms demand higher levels of skill in postings for innovative jobs, consistent with trade secrecy protections spurring innovative activities. However, job posting specificity decreases, in line with the proprietary costs channel, as trade secrecy protections are maximized when firms remain opaque regarding innovation. This decrease is attenuated for postings in tight labor markets, which is not only indicative of the importance of specificity in job postings, but also consistent with the proposed trade-off.

Keywords: human capital, labor demand, disclosure, proprietary costs, innovation, trade secrets, job postings

*New York University Stern School of Business. This paper is based on my dissertation at University of Chicago Booth School of Business, and I am grateful for the insights and support of my dissertation committee: Philip Berger, Hans Christensen, Christian Leuz (chair), and Thomas Rauter. I thank Julia Ariel-Rohr, Jonathan Craske, Jonas Dalmazzo, João Granja, Kalash Jain, Kasper Jønsson, Maria Khrakovsky, Anya Klyemenova, Shirley Lu, Maximilian Muhn, Fabian Nagel, Delphine Samuels, Haresh Sapra, Sandra Schafhäutle, Carol Seregini, Andrew Sutherland, Felix Vetter, Lauren Vollon, Matthew Walsh, Wanjia Zhao (discussant), and seminar participants at the AAA Annual Meeting, the Labor and Accounting Group Conference, the Taiwan Symposium on Innovation Economics and Entrepreneurship, Boston College, Dartmouth, Harvard Business School, London School of Economics, MIT, New York University, University of Chicago, University of Rochester, University of Texas-Dallas, and Wharton for comments and suggestions. I also thank Eric Addeo, Tamimi Ahmad, Lawrence Cabredo, Maheema Haque, Emily Underwood, and the University of Chicago Law School Innovation Clinic for various insights regarding firm innovation, trade secrecy, and hiring processes.

1 Introduction

Although labor is a key production input, firms do not systematically provide detailed human capital disclosures. The Securities and Exchange Commission (SEC) plans to introduce mandatory disclosure requirements in an effort to fill this gap and better inform investors.¹ Nonetheless, the persistent lack of *voluntary* disclosure could reflect the *proprietary costs* of providing information about labor investments. The theoretical literature predicts that a firm will strategically limit disclosure if granular information about investments would inform competitors and harm the firm's competitive position (e.g., [Verrecchia, 1983, 2001](#); [Bhattacharya and Ritter, 1983](#); [Ndofor and Levittas, 2004](#); [Heinle et al., 2020](#)). Yet, evidence on the proprietary costs of human capital disclosure is limited, as few traditional (and granular) human capital disclosures exist.

Arguably, the most ubiquitous and detailed human capital disclosures that firms make are their job postings. In acquiring human capital, firms use job postings to facilitate contracting with prospective employees. The existence of job postings—and the inclusion of detailed job descriptions and skill requirements therein—is driven by labor market frictions; detailed and targeted job postings inform and attract the optimal set of job applicants (e.g., [Menzio, 2007](#); [Walker and Hinojosa, 2014](#); [Banfi and Villena-Roldán, 2019](#); [Marinescu and Wolthoff, 2020](#)). However, because job postings are publicly available, disaggregated, timely, and directly related to the intention of a firm to invest in specific types of innovative human capital and projects, postings are informative not only to job applicants and investors ([Gutiérrez et al., 2020](#)), but plausibly to competitors as well. In this paper, I propose that job postings act as granular human capital disclosures, and a firm's *disclosure choices* when crafting its postings have both labor market *and* strategic implications.

For example, suppose a firm chooses to implement a new and innovative project. To realize the project, the firm requires a skillful and innovative engineer. The manager of the project will work

¹Paul Kiernan, *SEC Weighs Requiring Companies to Give More Details on Workers*, Wall Street Journal, August 20, 2021.

alongside the human resources department to craft a job posting and a labor search strategy. A detailed job posting—one that includes a description of the new and innovative project, a description of the specific job function and responsibilities of the engineer in that project, and the skill requirements for the position—is likely to inform and attract an optimal job applicant.² At the same time, a detailed job posting could be highly informative to both product market and labor market *competitors*, as the posting provides granular information about (i) the acquisition of human capital and its deployment in an innovative project and (ii) the firm’s idea of an innovative worker and job function, regardless of deployment. Because this disclosure could harm the firm’s competitive position, the posting carries high proprietary costs. Consequently, the firm could choose to be less specific in the posting or conduct its labor search discreetly, even though these more opaque hiring methods could make informing and attracting the right applicant more difficult.

Because job postings are a relatively unique source of public information regarding human capital investments, an examination of the aforementioned forces (and the trade-offs between them) is prudent. Furthermore, job postings are written by division managers and human resources departments (not investor relations departments), and postings are especially targeted toward potential employees. Thus, the extent to which firms make disclosure choices in job postings across the two channels—that is, as a reflection of labor demand to inform and attract the right job applicants (a *labor market channel*) and as a possible revelation of granular information to competitors about innovative human capital investments and activities (a *proprietary costs channel*)—is unclear.

I seek to answer the following questions: Do firms craft job postings with proprietary costs in mind, and if so, how do firms trade off the proprietary costs channel with the labor market channel?

Whereas the labor market channel incentivizes detail in job postings, in the presence of compet-

²It could be argued that more descriptive postings increase the duration of the firm’s job search by limiting the pool of applicants that deem themselves qualified. Industry research suggests otherwise; job postings with fewer than 1,000 characters have a click-to-apply rate of 3%, whereas those with about 5,000 characters have a click-to-apply rate of 12%-15% (Roy Maurer, *Crafting the Perfect Job Ad*, Society for Human Resource Management, February 26, 2016).

itive threats, employers could also adjust their postings to limit the level of detail. This trade-off should be especially salient for innovative firms with incentives to maintain opacity and engage in trade secrecy to reduce the proprietary costs of disclosure.³ To study these questions, I use a dataset of online job postings for manufacturing firms in the US from 2014 to 2017.⁴ The dataset contains the near universe of online job postings and categorizes each posting on multiple dimensions (e.g., job title, hiring industry, firm name, experience requirements, education requirements, skill requirements).

I measure disclosure choices related to the proprietary costs channel in two ways. My first measure assesses variation in the number of words in a job posting after controlling for both the interaction of key job posting features (the number of specialized skill requirements, the presence of an education requirement, the presence of an experience requirement) and the type of job being offered. Intuitively, this design enables the evaluation of disclosure choices within sets of job postings that have similar labor demand characteristics. An example of such a set is all postings seeking a mechanical engineer with five specialized skills, a college degree, and prior experience. *Within* this set of postings, I posit that a posting with *fewer words* reveals *less information* about why the firm is hiring that kind of mechanical engineer and what that mechanical engineer will do. I call this measure the *specificity* of a job posting. In addition, firms could reduce proprietary costs by using discreet hiring practices. Although I cannot observe the full scope of this behavior, I attempt to capture this strategy with a second measure: job posting *anonymity*. An anonymous posting—usually posted by a recruiter—omits the employer name, often replaced by an industry classification (e.g., “an employer in the computer manufacturing industry seeks to hire...”).

³Trade secrets are firm-specific proprietary information sets or processes that are key in producing revenue for the firm. Legal remedies and penalties protect trade secrets from misappropriation (e.g., theft by a rival, supplier, or former employee). To use and maximize these protections, the firm should not publicly disclose information related to trade secrets (e.g., [Li et al., 2018](#); [Glaeser, 2018](#)).

⁴Emsi Burning Glass (BGT) provides this dataset (www.economicmodeling.com). I focus on manufacturing firms, as survey evidence suggests that the manufacturing sector generally has a high propensity to engage in trade secrecy as a primary form of intellectual property protection (US Census Bureau BRDIS, 2011, 2012, 2015).

My analysis proceeds in three main steps. First, I provide evidence consistent with the *existence* of a proprietary costs channel. Comparing job postings from publicly held and privately held firms in the same industry *hiring for the same innovative job* at the same point in time (and after controlling for local labor market characteristics and firm size), I find that job posting specificity is significantly lower for privately held firms. Although this result is consistent with opaque firms (i.e., privately held firms) facing high proprietary costs of disclosure (relative to their less opaque peers), such a correlation could also be driven by other unobserved differences between publicly held and privately held firms. Therefore, in a second test with a similar research design, I focus on publicly held firms only and use variation in the number of confidential treatment orders (CTOs) issued by the SEC to capture opacity. A confidential treatment request, if approved by an issued CTO, allows a firm to redact specific information in mandatory disclosures if the revelation of that information would put the firm at a competitive disadvantage. Consistent with redacting firms facing high proprietary costs of granular disclosure, I find that firms with more CTOs are less specific in their postings. In a third test, I assess job posting anonymity and use differences in incentives for opacity across industries as a source of identifying variation (because employer-level characteristics are inherently unavailable for anonymous job postings). I find that job postings in industries with high levels of trade secrecy are more likely to omit the hiring firm's name than job postings in industries with relatively low levels of trade secrecy. Although these three tests depend on cross-sectional comparisons, they provide evidence that the proprietary costs channel induces decreases in job posting specificity and increases in job posting anonymity.

As a second step of analysis, I assess the *trade-off* between the labor market channel and the proprietary costs channel. To this end, I exploit a "shock" to both (i) firm incentives to demand innovative inputs and (ii) the benefits of opacity regarding innovative activities: federal trade secrecy protections. Specifically, in a difference-in-differences design, I exploit the passage of the 2016 De-

Defend Trade Secrets Act (DTSA) in the US. The DTSA provides a federal mechanism through which a firm can file legal action against parties that unlawfully utilize a trade secret of the firm (e.g., theft by a rival, supplier, or former employee). Such protections are provided to a focal firm's trade secrets with the intent to encourage focal firm innovation. Importantly, to maximize these protections and minimize the probability of reverse engineering, the focal firm should not publicly reveal information that is either directly or indirectly related to its innovative activities (e.g., [Li et al., 2018](#); [Glaeser, 2018](#)). Assuming a trade-off between the labor market channel and the proprietary costs channel, I hypothesize that the DTSA leads to (i) an increase in the demand for labor skills in trade secret-relevant occupations (as the DTSA spurs innovation) and (ii) a decrease in the specificity of job postings for those occupations (as the DTSA also incentivizes opacity).

While DTSA implementation has many desirable features, its federal scope does not allow for a geographical or staggered difference-in-differences research design. Therefore, my design exploits differences in job types. Specifically, I use job posting characteristics among non-innovation jobs (e.g., sales representatives) as a benchmark group for innovation jobs (e.g., mechanical engineers) because the contents of innovation job postings are more likely to reveal information related to innovative human capital investments and trade secrecy. The non-innovation job benchmark group controls for variation in job posting content related to general changes in technology used in the workplace, broad changes in "boilerplate" language, and business cycle-driven factors.

Consistent with DTSA implementation spurring the demand for innovative and skilled labor, I find that the average number of specialized skills in job postings for innovation jobs increases after the implementation of the DTSA, relative to postings for non-innovation jobs. The change in skills demand for the average innovation job is about six percentage points greater than the change for the average non-innovation job. These findings are not explained by time-invariant local labor market factors, industry-level shocks, or state-level regulations that affect all job postings.

Then, consistent with the DTSA increasing the marginal benefits of opacity regarding innovative human capital investments and activities, I also find that innovation job postings become less specific after the DTSA. Relative to the change in non-innovation jobs, the specificity change in innovation jobs is about three percentage points lower, consistent with the proprietary costs channel. This result could have an alternative explanation: the decrease in specificity could reflect the uncertainty involved with new activities. Yet, a coefficient stability test (Oster, 2019) with flexible employer-level uncertainty controls implies that such endogeneity concerns are unlikely to fully explain the decrease in specificity. In sum, after the implementation of the DTSA, employers convey their demand for labor skills but also seem to reduce the specificity of innovation job postings, consistent with firms balancing the labor market channel and the proprietary costs channel.⁵

As a third step of analysis, I consider *heterogeneity* in the specificity response after the implementation of the DTSA to shed further light on the trade-off between the labor market channel and the proprietary costs channel. From a labor market perspective, reductions in specificity could be especially costly when few candidates are available (i.e., tight labor markets), as specificity could inform and attract rarely available high-quality applicants in a timely fashion. Using a job-locality-level measure of the average time required to fill a job posting, I find that the decrease in specificity after the DTSA is strongest when labor markets are slack (i.e., when job postings historically take a short time to fill). By contrast, when labor markets are tight, this decrease is attenuated (i.e., less negative). This evidence is consistent with reductions in specificity (although favorable from a proprietary costs perspective) being costly from a labor market perspective when the right applicants are hard to come by.

⁵Because a strategy of job posting anonymity is likely to be a firm-level strategy that affects *both* innovation and non-innovation jobs, the baseline design is not well suited for an analysis of changes in job posting anonymity after the DTSA. In an alternative design, I compare changes in the anonymity of innovation job postings in industries with high trade secrecy propensity with those in industries with low trade secrecy propensity. Consistent with a proprietary costs channel, I find that innovation job postings in high-secrecy industries are more likely to be anonymous after the DTSA. See the Internet Appendix for details.

My paper makes three key contributions to the existing literature. First, it contributes to the literature on human capital disclosures, proprietary costs, and disclosure strategies. Although job postings are intended for labor market participants, recent work has shown that job postings can be informative to investors as well (e.g., [Gutiérrez et al., 2020](#); [Liu, 2021](#)). My paper extends this literature by providing evidence that *firms* treat their own postings as disclosures with both labor market and strategic implications, a timely exercise given the continued implementation of new human capital disclosure requirements ([Batish et al., 2021](#)).⁶ Job postings are both more disaggregated and timelier than most financial or nonfinancial disclosures, making the study of their strategic implications salient. Namely, the literature on firm disclosure strategies when facing disclosure-specific benefits and costs predicts that the proprietary costs of granular disclosures are particularly high (e.g., [Verrecchia and Weber, 2006](#); [Glaeser, 2018](#); [Heinle et al., 2020](#); [Barth et al., 2020](#)). Consistent with postings being highly granular and credible signals, I provide evidence that private firm status, redaction decisions, and trade secrecy are related in a complementary fashion to nondisclosure decisions in postings.⁷ Second, by studying postings in a trade secrecy setting, my paper provides evidence that transparency incentives affect the demand for innovative labor, thus adding to the literature on the real effects of transparency.⁸ Third, from a policy perspective, I provide an early

⁶In the broader disclosure and human capital context, I also contribute to the literature on the relation between job posting content, recruitment, and labor market outcomes (e.g., [Feldman et al., 2006](#); [Walker and Hinojosa, 2014](#); [Modestino et al., 2016, 2020](#); [Goldstein et al., 2017](#); [Banfi and Villena-Roldán, 2019](#); [Kircher, 2020](#); [Marinescu and Wolthoff, 2020](#); [Pacelli et al., 2022](#)), the labor market as an audience for various avenues of firm disclosure (e.g., [Jones et al., 2014](#); [Chang and Chin, 2018](#); [Boulland et al., 2021](#); [Choi et al., 2021](#); [deHaan et al., 2021](#)), and the properties of nonfinancial disclosures (e.g., [Dhaliwal et al., 2011](#); [Christensen et al., 2021](#)).

⁷The large body of literature on the proprietary costs of traditional financial disclosures, on the other hand, provides relatively mixed evidence, possibly because such disclosures require a large degree of aggregation and standardization. See [Berger \(2011\)](#) and [Leuz and Wysocki \(2016\)](#) for discussion and review of this literature. Concurrent work by [Cao et al. \(2022\)](#) considers the cross-sectional relation between technological peer pressure and skill requirements in job postings, finding evidence for the labor market channel (i.e., more specific skill requirements with increased peer pressure). In contrast, I examine a broader range of disclosure choices (i.e., skill requirements, specificity, and anonymity) and exploit plausibly exogenous variation in trade secrecy law along with cross-sectional sources of heterogeneous incentives, allowing me to characterize the proprietary costs and labor market channels in multifaceted ways. As a result, I find evidence for both channels.

⁸Although some studies evaluate the effects of transparency on general investment and innovation (e.g., [Biddle et al., 2009](#); [Cheng et al., 2013](#); [Badertscher et al., 2013](#); [Roychowdhury et al., 2019](#); [Breuer et al., 2020](#); [Rauter, 2020](#); [Simpson and Tamayo, 2020](#)), the literature studying the effects of transparency on *labor* investment is burgeoning. Most of these studies consider realized hiring, turnover, and wages (e.g., [Jung et al., 2014](#); [Ha and Feng, 2018](#); [Choi et al., 2019](#); [Cao and Chen, 2019](#)). The study of postings adds to this literature, as postings are fast-moving, detailed, and reflective of

study on the innovative investment and disclosure impacts of federal trade secrecy legislation; to my knowledge, my paper is one of the first to do so in this federal setting.

2 Background

2.1 Labor Demand and Job Postings

The labor economics literature formulates the firm’s hiring problem as maximizing the difference between an employee’s inferred productivity and her pay (Oyer and Schaefer, 2011). Because asymmetric information exists between employers and potential employees, inferences of productivity are formed by many imperfect signals. Through a costly labor search and matching process, employers gather these signals and attempt to hire from an optimal set of potential employees.

To reduce information asymmetry and search costs, employers often begin a job search by publicly disclosing a job posting that clearly describes the job available and the required skills for the position.⁹ This form of search presents trade-offs that drive the nature of job postings on various dimensions. One such dimension is the specificity of a job posting in terms of describing the job attributes. Posting a job opening with very low specificity could have the advantage of attracting a wide pool of job applicants and increasing the size of the job applicant pool. However, doing so could also result in many unqualified job applicants applying for a position or many qualified job applicants not finding the job posting informative or attractive enough. Therefore, in conveying their labor demand, employers are incentivized to include a certain level of specificity in job postings to optimize their search process, inform and attract appropriate job applicants, and maximize

real investment decisions (e.g., Deming and Kahn, 2018; Babina et al., 2020; Bloom et al., 2021).

⁹The contents of job postings—and the labor search strategy as a whole—are primarily the result of a joint decision between (i) a manager from the team seeking an external hire and (ii) the human resources department or an outsourced human resources group (*Recruitment and Selection Process*, Society for Human Resource Management). Compliance procedures may involve review of posting contents by compliance and/or legal subdivisions. Although this process may differ across firms, conversations with human resources professionals and managers of innovative teams confirm that this process is generally a representative one. Note that the lack of the investor relations department in this process motivates my focus on firms’ disclosure choices as they pertain to the labor market and proprietary costs channels (rather than also considering a capital markets channel).

match quality. For example, in an experimental design, [Feldman et al. \(2006\)](#) find that specificity in job postings leads to increased perceptions of informativeness, job fit, and company quality from the perspective of the job applicant.

Given that division managers and human resources departments (and not investor relations departments) are the primary authors of job postings, these matching factors are a key consideration in determining the nature of job postings. An article from the Society for Human Resource Management supports this assertion: job posting specificity is perceived to be an important characteristic in improving the click-to-apply rate. Job postings with fewer than 1,000 characters have a click-to-apply rate of 3%, whereas those with about 5,000 characters have a click-to-apply-rate of 12%-15%.¹⁰

Because external search usually involves the public availability of these (possibly quite specific) job postings, one could consider a job posting as being akin to a detailed public disclosure of a material contract for a production input. Consequently, job postings are informative not only to the potential job applicant pool but also to competitors. This informativeness is salient due to the limited mandatory disclosure of human capital investments. In fact, some companies offer competitive intelligence services that analyze the job postings of competitors to reveal early trends in hiring and investment, implying that the information in job postings—whether that be the existence of a posting, the job description, the job function, or the skill requirements—is likely to be actionable.¹¹ Therefore, I posit that job postings carry proprietary costs, as postings allow competitors to learn about early-stage human capital investments and activities in a relatively detailed fashion. To protect the rents of innovative human capital investments, employers could have incentives to limit the specificity of job postings, possibly at the expense of optimal labor search.¹² In

¹⁰Roy Maurer, *Crafting the Perfect Job Ad*, Society for Human Resource Management, February 26, 2016.

¹¹Ellie Mirman, *How to Decode Your Competitor's Strategy with Predictive Intelligence*, Crayon, January 2, 2018.

¹²Dan Gasser, *How Secretive are Your Job Postings?* Eagle, June 2017. Furthermore, because arriving and departing employees are a key source of information leakage between competitors, human resources departments are plausibly cognizant of proprietary information within the firm (*Protecting Trade Secrets in the Hiring and Firing Process*, Lorman,

fact, as an alternative strategy, employers could anonymize their labor search or use a headhunter with the express purpose of obfuscating details of their labor investments from competitors.¹³

An example with Twitter helps convey the disclosure friction between providing specific information to a potential job applicant pool and disclosing information about a human capital investment to competitors.¹⁴ In early July 2020, Twitter posted a job opening for a full-stack software engineer. Beyond the job title and skill requirements, Twitter also added the following:

Who are we? We are a new team, codenamed Gryphon. We are building a subscription platform, one that can be reused by others in the future. This is a first for Twitter! Gryphon is a team of engineers who are closely collaborating with the Payments team and Twitter.com team. We are looking for a full-stack engineer to lead the Payment and Subscription client work, someone who values collaboration as much as we do and can act as a bridge for the engineering team.

The specific purpose and job function for which the full-stack engineer was required—that is, for the development of a new subscription platform, an innovation for Twitter—was indeed news. After being quickly reported on by The Verge and Forbes (among others), Twitter shares jumped by more than 8% after the posting. Within a couple of hours, Twitter promptly *revised* the job posting, removing all mention of the new subscription service and the specific job function of the engineer:

What you'll do: We collaborate with other specialists, product managers, and designers to build tools that users can rely on to find out what's happening in the world, in real time. As an Android engineer, you will work with a bevy of backend engineering teams to build components that allow for experimentation to deliver the best experience possible to all of our users.

June 19, 2006).

¹³Sandy Reed, *Stupid Web tricks: what you shouldn't post on your company's Web site*, InfoWorld, July 12, 1999; Alison Doyle, *What is the Hidden Job Market?* The Balance Careers, January 30, 2020.

¹⁴Tom Warren, *Twitter is working on a new subscription platform, hints job listing*, The Verge, July 8, 2020; Carly Page, *Twitter Confirms It's Working On A Subscription Platform Codenamed "Gryphon"*, Forbes, July 8, 2020; Valentine Muhamba, *Twitter is apparently working on a subscription service*, Techzim, July 9, 2020.

Likely realizing that such a revision did not change the fact that they already revealed proprietary information, Twitter eventually restored the original posting.

This anecdote, although unique in terms of its revisionist nature, highlights the key trade-off firms face. Specificity about a job role could provide boons in the labor market, informing the highest-quality job applicants and optimizing directed search; for example, according to Forbes, a similar posting for the Twitter subscription team with similar initial specificity about the subscription service and job functions (but for a backend Scala engineer instead) quickly closed and no longer accepted new applications, implying it was filled. Nonetheless, such specificity also informs competitors about new and early investments into human capital and innovative projects, incentivizing the firm to “dial back” the specificity of the job posting to protect relatively proprietary information. Therefore, a firm with high marginal benefits of opacity regarding innovative human capital investments and activities must trade off the labor market and proprietary costs channels when formulating a job posting. However, given the various instances of firms inadvertently disclosing proprietary information in job postings (like the Twitter example presented here), the general extent to which firms treat job postings as disclosures that carry proprietary costs is unclear, thus inviting empirical study. I provide a visual representation of this key trade-off in Figure 1.¹⁵

[Figure 1]

¹⁵Note that some excerpts from the anecdotal evidence discussed in this section are presented in Internet Appendix Figures IA.1, IA.2, and IA.3. Although less revisionist in nature than the Twitter example, another example of an unintended disclosure involves NVIDIA, a chip-manufacturing firm that is in my estimation sample. In December 2016, in a job posting for a Senior Marketing Manager, NVIDIA revealed both the development of a new graphics card and the specific job responsibilities of the manager. After some coverage, that posting was deleted entirely (Dan Thorpe-Lancaster, *GTX 1080Ti and Club GeForce Elite subscription program outed in NVIDIA job posting*, Windows Central, December 19, 2016). In the extreme, a firm could explicitly (and inadvertently) disclose trade secrets in job postings and forfeit trade secrecy protections. In one particular trade secrecy case, a firm lost trade secrecy protections over its client list due to the disclosure of client names in job postings (*H & R Recruiters Inc. v. Kirkpatrick*, 243 A.D.2d 680, 663 N.Y.S.2d 865 (App. Div. 1997)).

2.2 Firm Opacity, Trade Secrets, and the Defend Trade Secrets Act (DTSA)

Firm opacity and trade secrecy foster scenarios in which firms rely on nondisclosure to protect unpatented investments, processes, and innovations, thus providing a natural setting in which to investigate job postings as disclosures to both labor market participants and competitors. Trade secrets are information sets that derive future value from not being known and appropriable by competitors (Glaeser, 2018). According to the Business R&D and Innovation Survey (BRDIS), trade secrecy is one of the most popular forms of innovation protection, especially in the manufacturing sector (US Census Bureau BRDIS (2011, 2012, 2015)). Because trade secrets by definition cannot be protected by publicly disclosed patents, laws provide legal remedies and protections in the case of trade secret misappropriation by a third party (e.g., theft by a former employee, supplier, or a rival).¹⁶ The intent of such laws is to protect unpatented innovations and thus spur further innovation.

However, for these laws to act as protection and as a credible threat, a key element of successful court action is that the information regarding a firm's secret could not have been acquired through any means other than the alleged misappropriation. This element was reiterated in the decision in *Economy Roofing & Insulating Co. v. Zumaris* by the Iowa Supreme Court regarding state-level trade secret law: "There is virtually no category of information that cannot, as long as the information is protected from disclosure to the public, constitute a trade secret."¹⁷ Importantly, if the firm makes public any ancillary details that allow a competitor to reverse engineer the trade secret, the competitor's use of that information provision does *not* constitute misappropriation. Therefore, a firm can maximally enforce its property rights over trade secrets if it remains opaque. Extending this concept to labor demand, if firms engage in trade secrecy and/or have high incentives for

¹⁶Two oft-studied laws include state-level Uniform Trade Secrets Acts, or UTSAAs (e.g., Png, 2017; Glaeser, 2018), and common-law inevitable disclosure doctrines, or IDDAs (e.g., Li et al., 2018; Klasa et al., 2018; Dey and White, 2021).

¹⁷538 N.W.2d 641, 647 (Iowa, 1995).

opacity, then they could be less specific in job postings to obfuscate information about innovative human capital inputs and unpatented innovative activities, thus maximizing the property rights over their trade secrets.

The implementation of the DTSA in 2016 represents a significant (yet understudied) change in trade secrecy protections in the US. [Cannan \(2017\)](#) provides a detailed legislative history of federal trade secrecy legislation. The DTSA was introduced in the Senate in July 2015. Preceded by multiple failed iterations of federal trade secrecy legislation in 2012 and 2014, the DTSA's final form and legislative fate at the time of its introduction was unclear. However, after various changes and amendments, the DTSA passed the Senate unanimously (and the House nearly unanimously) in April 2016 and was signed by President Barack Obama on May 11, 2016.¹⁸

The novel elements of its final form lie primarily along two dimensions. First, the DTSA provides redress in federal court for trade secrets, as was already the case with trademark, copyright, and patent protections. While the Economic Espionage Act of 1996 (EEA) provides some federal criminal enforcement, the DTSA adds an important layer of federal civil cause of action. Proponents view civil enforcement to be a less costly and more efficient means by which to prosecute trade secrets theft, especially in interstate cases ([Cannan, 2017](#)).¹⁹ Second, the DTSA introduces *ex parte* seizure procedure into trade secrecy protection. This procedure allows for the seizure of an allegedly stolen trade secret through an affidavit or verified complaint before final judgment if the misappropriating party's retention of that secret would lead to irreparable harm to the applicant. The DTSA also has other new features, such as updates to the criminal provisions in the EEA and limitations on disclosure requirements in court to limit trade secret exposure.

[Levine and Seaman \(2018\)](#) provide a review of the first year of cases under the DTSA and summarize some characteristics of these cases. First, the DTSA has proven to be an oft-used form

¹⁸Throughout the paper, I use May 2016 as the implementation month of the DTSA.

¹⁹The language of the DTSA in terms of civil cause of action is quite similar to that of the UTSA.

of trade secrecy protection; nearly 500 cases were filed in the first year of its existence, with an average of 35 cases per month from May to December 2016 and an average of 50 cases a month from January to May 2017. Second, unlike patent litigation, which is concentrated in very limited geographic areas, DTSA claims have been heard in 67 out of 90 (74%) district courts, with the Northern District of Illinois having the greatest number of claims (9% of first-year DTSA claims). Third, high complementarity seems to exist between federal claims and state claims: 84% of DTSA claims also assert state trade secret misappropriation, among other state-level claims. In this way, the DTSA provides an added layer of trade secrecy protection rather than preempting other federal or state law claims, which is often not the case for state-level trade secrecy protections such as UT-SAs. Fourth, the types of trade secrets that are allegedly misappropriated include business plans, software, algorithms, technical information, financial data, marketing information, and customer information, whereas misappropriators include former employees, business partners, and unrelated third parties. Fifth, in multiple cases, temporary restraining orders, preliminary injunctions, and (more rarely) *ex parte* seizures are granted, highlighting the protective measures provided by the DTSA.

Overall, the DTSA represents a shock to the expected property rights over unpatented innovations (if, of course, those innovations are treated as trade secrets). One effect of such a regulation is that firms expect to retain more rents of innovative investments and thus increase innovative activities (Png, 2017). Given that this rent retention is conditional on opacity, another effect of such a regulation is that firms are more incentivized to engage in trade secrecy because the marginal benefits of opacity increase. As a result, firms decrease the disclosure of specific information, especially when that information may directly (or indirectly) reveal innovative activities (e.g., Li et al., 2018; Glaeser, 2018; Callen et al., 2020).

Both of these effects are pertinent to job postings as demand disclosures. Through the first

effect, I hypothesize that firms are likely to increase their demand for labor skills (and perhaps be specific about those skills and the job at hand)—the labor market channel of job postings as demand disclosures. Through the second effect, firms will be disincentivized to disclose specific information in job postings that could reveal details relevant to innovative human capital investments and activities—the proprietary costs channel of job postings as demand disclosures.

3 Data

To study labor demand and job postings, I use a comprehensive dataset of online job postings from Emsi Burning Glass (BGT). BGT collects online job postings from thousands of posting boards, appends these data, and categorizes each posting on multiple dimensions. BGT also provides the full text of job postings.²⁰ The dataset represents the near universe of new online job postings at the employer-location-job-day level, and it is generally representative of true labor demand (Lancaster et al., 2019). I focus on job postings from employers in the manufacturing sector (NAICS 31-33), as survey evidence shows manufacturing firms are generally those that consider opacity and trade secrecy to be key protection tools for innovative activities (US Census Bureau BRDIS 2011, 2012, 2015). BGT determines the industry of the firm based on firm name and other characteristics. I use this classification at the three-digit NAICS level, when available. When this level is not available, I use three-digit industry classifications from Compustat after a fuzzy match on employer names to the Compustat database.²¹ Given that much of my analysis exploits the

²⁰In addition, BGT has a process of removing sufficiently similar duplicates across job postings and a refresh process for each job posting. *Across* job postings in the cross section, very similar postings are removed using the location, employer, and title fields. If all of those fields are the same, the duplicate posting is removed. *Across* job postings over time, the process above is conducted until the original posting is 60 days old. After 60 days, if a very similar posting is made (same location, employer, and title, but different URL), that posting becomes a new, original posting. *Within* an individual posting over time, postings stay open for six months at most. When the same job posting (same URL) is up for more than six months, it is characterized as a new job posting. Thus, the dataset reliably represents different job postings within the same employer with a reasonable refresh process.

²¹This matching process to Compustat, primarily conducted by BGT, uses an NLP model for fuzzy string matching and generates a probability score for matches on BGT employer name and on URL associated with the posting. It takes into account subsidiaries of publicly held companies and matches them to the parent. I use this match to differentiate between publicly held and privately held companies. I also use the identifiers from this match to merge in data on the number of CTOs issued by the SEC to each publicly held company.

passage of the DTSA in 2016, I focus on a sample period of 2014 to 2017. I omit jobs and employers with fewer than 10 postings, postings with no parsed skills, and postings with fewer than 10 total words for each parsed skill (specialized or baseline) because these postings are subject to classification issues.²² This process leads to a sample of just under three million postings from more than 6,000 manufacturing firms.

In Appendix A, I provide detailed definitions for the key variables in my paper. To measure specificity in job postings, I use the natural log of the word count in a job posting, while controlling for posting-specific features. Specifically, I define 124 interactions of key job posting features: the number of specialized skills, the presence of a degree requirement, and the presence of an experience requirement. I then further interact the combinations of these posting features with job group or job depending on the specification. Intuitively, when these posting feature controls are included as a set of fixed effects in the main tests, the remaining variation in the word count represents disclosure choices *within* a set of job postings with similar labor demand characteristics. I also define two key job groups, based on a professional services classification by [Deming and Kahn \(2018\)](#). I define jobs with two-digit Standard Occupational Classification (SOC) codes of 11 to 29 as innovation jobs because they are most likely to be human capital investments that are direct inputs to trade secrecy-related innovative activities. For example, in my sample, innovation job postings often represent engineer, computer scientist, and manager occupations. I call jobs with two-digit SOC codes outside that range—primarily sales representatives and production workers—non-innovation jobs. Summary statistics for the main job posting variables are provided in Panels A and B of [Table 1](#), split by job group. Summary statistics for firm-level characteristics are presented in Panel C.

[[Table 1](#)]

²²Note these omissions represent a small portion of the dataset, and the inclusion of these observations leads to similar results throughout.

4 Research Design

To establish a proprietary costs channel in job postings, I test for relations between opacity incentives and job posting specificity for innovation jobs. I run regressions of the following form:

$$Y_{ikct} = \beta FirmOpacity_k + \zeta_{vj} + \lambda_{cj} + \eta_{jdt} + \alpha_{zd} + \epsilon_{ikct}. \quad (1)$$

Above, i denotes a job posting for an innovation job, k denotes the employer making the posting, c denotes the county in which the job is located, t denotes the year-month of the posting, d denotes the three-digit NAICS code of the employer k , j denotes the eight-digit ONET code associated with job posting i , z denotes a decile rank of firm size, and v denotes the set of job posting features conveyed in job posting i .²³ In my first two tests, $FirmOpacity_k$ is either the listing status of firm k (i.e., publicly held or privately held) or, if publicly held, the number of CTOs issued from 2014 to 2017. I argue that these two types of firms—by revealed behavior—face high proprietary costs. The outcome Y is the natural log of the word count.

Of note in this design are the fixed effects. County-job fixed effects λ_{cj} control for time-invariant local labor market characteristics of a job posting for job j hiring in county c . Job-industry-time fixed effects η_{jdt} control for time-variant factors by job and hiring industry. The posting feature fixed effects ζ_{vj} control for variation explained by posting features v for job j . Finally, to control for other unobservable firm-level characteristics, I include the interaction of deciles of firm size (determined by number of job postings) and industry, a set of fixed effects represented by α_{zd} . In sum, this research design studies the relation between job posting specificity and firm opacity by comparing postings from two groups of similarly sized firms—one group opaque, the other less so—in the same industry hiring for the same innovation job at the same point in time, after controlling for local labor market characteristics and job posting features. I hypothesize that more opaque firms—that is, privately held firms and firms with more CTOs—have higher proprietary

²³An ONET code is a more granular version of the two-digit SOC code that captures the job being offered in the posting.

costs of disclosure and thus have lower specificity in job postings (i.e., fewer words conditional on a set of job posting features).

While reductions in specificity capture one form of a proprietary costs channel, firms could take a more “extensive margin” approach and opt to hire discreetly to reduce proprietary costs. In a third test, I define Y as the anonymity of the job posting, where a job posting is anonymous if the employer name is not disclosed. Because specific employer characteristics (other than industry) are inherently unavailable in this test, I define $FirmOpacity_k$ as one if the job posting is hiring for a position in three-digit NAICS industry d that utilizes high levels of trade secrecy (according to the BRDIS).²⁴ Although I retain the general structure of Equation (1), due to the level of identifying variation, I remove firm size and industry controls. This third test compares two groups of job postings—one group in high-secrecy industries, the other not—hiring for the same job at the same point in time, after accounting for local labor market and posting feature characteristics. I hypothesize that job postings in high-secrecy industries are more likely to be anonymous, as firms in high-secrecy industries likely face higher proprietary costs of disclosure regarding innovative human capital investments and activities.

The three aforementioned tests should give insight into the existence of a proprietary costs channel in job postings. However, the sources of variation in those tests primarily represent static characteristics, limiting the general extent to which I can examine both the labor market and proprietary costs channels of job postings. To further study these channels, I exploit the implementation of the DTSA in May 2016 (see Section 2). Plausibly, the DTSA spurs innovation by increasing the property rights over trade secrets, leading to both (i) a greater demand for skilled labor in innovation jobs and (ii) less specificity in those job postings in order to maximize trade secrecy protections

²⁴These industries are food manufacturing (311), petroleum and coal products manufacturing (324), chemicals manufacturing (325), and computer and electronic products manufacturing (334). Note that this classification correlates strongly with the average number of CTOs in an industry.

(because the contents of those postings are most relevant to trade secrets). Given that the DTSA is a federal piece of legislation (making geographic benchmark groups difficult to define), I use non-innovation jobs as the benchmark group to control for broad changes in job posting content. I implement the following difference-in-differences designs from 2014 to 2017, clustering standard errors two ways at the state-industry and job levels²⁵:

$$ihs(\text{SpecSkills})_{ikct} = \beta \text{PostDTSA}_t * \text{Innov.Group}_{J=1} + \lambda_{cdJ} + \eta_{dt} + \delta_{st} + \epsilon_{ikct}, \quad (2)$$

$$\ln(\text{WordCt})_{ikct} = \beta \text{PostDTSA}_t * \text{Innov.Group}_{J=1} + \zeta_{vJ} + \lambda_{cdJ} + \eta_{dt} + \delta_{st} + \epsilon_{ikct}. \quad (3)$$

All subscripts are defined as in Equation (1), except for an addition to differentiate between the treatment and benchmark groups: job group J indicates whether job j is an innovation job (SOC codes 11 to 29, $J = 1$) or a non-innovation job (other SOC codes, $J = 0$). $\text{PostDTSA}_t * \text{Innov.Group}_{J=1}$ is a “ $\text{post} \times \text{treat}$ ” indicator, taking a value of one in and after May 2016 for innovation jobs, and zero otherwise. County-industry-job group fixed effects λ_{cdJ} control for time-invariant local labor market characteristics of a firm in industry d hiring in county c for a job of type J . Industry-time fixed effects η_{dt} flexibly control for time-variant factors by hiring industry, reducing concerns that any results are explained by general industry-level shocks to job postings.²⁶ State-time fixed effects δ_{st} flexibly control for time-variant factors by state s , reducing concerns that any results are explained by general state-level shocks or regulations affecting job postings. In Equation (2), the outcome variable is the inverse hyperbolic sine of the number of specialized skills in a job posting.²⁷ As outlined in Section 2, if innovation is spurred by the DTSA due to increased

²⁵This clustering subsumes the primary time-invariant fixed effects in the main tests while plausibly satisfying homogeneity assumptions (Conley et al., 2018). Note that the main inferences are robust at conventional statistical levels to various other clustering choices. See Internet Appendix Table IA.1.

²⁶Note that because treatment is defined at the job group level (i.e., innovation jobs vs. non-innovation jobs), I cannot include job j -specific (or broader job group J -specific) fully flexible time trends as I do in Equation (1). Later tests in Table 5 and Internet Appendix Table IA.3 alleviate concerns regarding general job-level time trends.

²⁷This transformation ($\ln(x + \sqrt{x^2 + 1})$) is well-defined at zero, approximates the natural log transformation, and does not require the addition of a constant whose choice can affect the estimated magnitude of the coefficient in a regression. Although the transformation does introduce undesired convexity over negative values, my data contain no negative values. Note that this approximate transformation is not necessary for the word count, as it never takes a value of zero. Therefore, I use the natural log transformation for word count.

trade secrecy protections, firms demand more skills from the average innovation job and convey this demand in job postings, indicative of a labor market channel.²⁸

However, because this innovative investment is spurred due to better property rights under trade secrecy, the marginal benefits of opacity are plausibly increased, incentivizing firms to be less specific in a job posting for an innovation job. Equation (3) tests for this proprietary costs channel. After controlling for job posting features like skills demand (captured by adding fixed effects $\zeta_{v,j}$), I test for changes in the natural log of word count, thus representing changes in specificity for innovation job postings with similar labor demand characteristics before and after the DTSA.²⁹

Equations (2) and (3) allow for changes in the composition of jobs within each job group, likely an important element of the labor demand response exhibited before and after DTSA implementation. For example, in measuring the average number of skills in a job posting in a local labor market, such a design captures compositional changes (e.g., changes within innovation jobs of how many engineers are hired relative to computer scientists). However, also of interest is how job postings change *within* jobs and how this change could vary given the condition of local labor markets—especially for the specificity regressions. Thus, I implement a more granular difference-in-differences design³⁰:

$$\ln(\text{WordCt})_{ikct} = \beta \text{PostDTSA}_t * \text{Innov.Group}_{J=1} + \omega_{vj} + \iota_{cj} + \eta_{dt} + \delta_{st} + \epsilon_{ikct}. \quad (4)$$

The key changes are the more granular job posting feature controls (ω_{vj}) and county-job fixed effects ι_{cj} . In this design, β represents the change in the difference in job posting specificity for the average innovation job compared to the average non-innovation job around DTSA implementation,

²⁸In robustness exercises, I use related alternative proxies, such as the number of cognitive skills, degree requirements, and Bureau of Labor Statistics (BLS) job scores. See Internet Appendix Table IA.1.

²⁹Firms could also respond by more often posting anonymously. In Internet Appendix Table IA.3 and Figure IA.4, I compare changes in the anonymity of innovation job postings in industries with high trade secrecy propensity to those in industries with low trade secrecy propensity. Consistent with a proprietary costs channel, I find that innovation job postings in high-secrecy industries are more likely to be anonymous after the DTSA.

³⁰Although this job-level design is most pertinent for the specificity regressions, for completeness, I also report results of a job-level regression for specialized skills demand.

after controlling for *posting-job-specific* and *county-job-specific* characteristics.³¹

The design in Equation (4) allows for more granular job-specific analysis on labor market conditions and job posting specificity around DTSA implementation, providing further insight on the trade-off between the labor market and proprietary costs channels. Specifically, reductions in specificity could be costly in a labor market sense if labor markets are tight, as specificity may make job matching more efficient. BGT provides an average time-to-fill measure at the six-digit SOC code and Metropolitan Statistical Area (MSA) level for 50 of the largest MSAs in 2015.³² I use this measure as a proxy for the labor market tightness associated with labor search at the local job level; the longer it takes to fill a particular position in a locality, the tighter the labor market in that locality. For each job, I split counties into two groups $p = \{1, 2\}$ (and in an another specification, five groups) based on the average time-to-fill (TTF) by MSA in 2015 and run regressions of the following form³³:

$$\begin{aligned} \ln(\text{WordCt})_{ikct} = & \beta_L \text{PostDTSA}_t * \text{Innov.Group}_{J=1} * \text{LowTTF}_{cj}^{p=L} \\ & + \beta_H \text{PostDTSA}_t * \text{Innov.Group}_{J=1} * \text{HighTTF}_{cj}^{p=H} \\ & + \omega_{vj} + \iota_{cj} + \alpha_{pt} + \eta_{dt} + \epsilon_{ikct} . \end{aligned} \quad (5)$$

I hypothesize that $\beta_L < \beta_H$. When labor markets are slack, firms reduce specificity after the DTSA due to increased marginal benefits of opacity. However, when labor markets are tight, firms face a more severe trade-off; although reducing specificity could protect the revelation of information relevant to trade secrets, it could further exacerbate already high search costs in tight labor markets, inducing firms to attenuate their decrease in specificity to facilitate the hiring process.

³¹An even more robust analysis would account for time-invariant means at the county-industry-job level (i.e., ι_{cdj}). However, such a specification leads to further iterative singletons due to high-dimensional fixed effects, and I therefore retain the simpler design in Equation (4). Nonetheless, results are similar with these more granular fixed effects.

³²If a NECTA classification is available, BGT replaces the MSA with the NECTA classification.

³³The inclusion of α_{pt} and ι_{cj} fixed effects absorbs the time-variant and time-invariant main effects. To ensure conditional support for the quintile split, I only retain six-digit SOC codes that have all five quintiles populated. Almost all the job postings in the subsample with average time-to-fill data (98%) satisfy this requirement. Note that results are very similar if I also interact the posting features controls with p .

5 Results

5.1 Job Posting Specificity, Anonymity, and Opacity Incentives

To establish the proprietary costs channel, I first study the general relation between job posting specificity and opacity incentives. I posit that more opaque firms face high proprietary costs associated with disclosure and therefore disclose less specific job postings. I use three measures of opacity: the listing status of the posting firm (i.e., privately held or publicly held), the number of CTOs issued by the SEC from 2014 to 2017 for publicly held firms, and whether a job posting seeks to hire in a high-secrecy industry. Results are in Table 2.

In Panel A, I test whether privately held firms are more likely to have less specific job postings. In Column (1), I simply include a flexible size control by interacting deciles of firm size with industry classification, and I regress word count on listing status. In this baseline correlation, I find that privately held firms are more likely to have fewer words in a job posting. In Column (2), I introduce controls for posting features (i.e., key labor demand factors). I also account for time-invariant local labor market characteristics and time-variant characteristics for employers in an industry hiring for a particular job, as outlined in Section 4, Equation (1). With these controls included, I study the relation between job posting *specificity* (as I control for key labor demand factors) and firm opacity by comparing postings from privately held and publicly held firms in the same industry hiring for the same innovation job at the same point in time, after controlling for local labor market and size characteristics. Although the inclusion of these controls explains much more variation in specificity, the coefficient remains negative and statistically significant at conventional levels. This result is consistent with privately held firms being less specific in job postings because they face higher proprietary costs.

Although I do flexibly control for firm size, one may be concerned that this relation reflects unobservable characteristics of privately held firms that are not related to proprietary costs. For

example, privately held firms may face more lax thresholds in terms of required language in job postings. My next test alleviates some of these concerns by considering variation in opacity for publicly held firms only. I collect data on the number of CTOs issued by the SEC to each publicly held firm from 2014 to 2017. Firms that seek to redact regulated financial disclosures are likely facing high proprietary costs, relative to their peers. If a sizeable proprietary costs channel exists in labor search, these firms should be less specific in job postings as well.

In Panel B, for the subset of job postings by publicly held firms, I regress word count on the inverse hyperbolic sine of the number of CTOs for the posting firm from 2014 to 2017. I include controls identical to those in Panel A in Columns (1) and (2). In Column (1), comparing two publicly held firms of similar sizes in the same industry, I find that firms with more CTOs are less specific in job postings. In Column (2), I include the suite of controls that allows for the interpretation of the coefficient as the difference in specificity after controlling for size, local labor market characteristics, posting characteristics, and industry-job level shocks. While the R^2 increases significantly, the coefficient is nearly unchanged; in fact, it becomes more negative, and the estimate more precise. This result is once again consistent with a proprietary costs channel in labor search. In Figure 2, I replace the continuous CTO regressor in Column (2) with discrete indicators representing different numbers of CTOs issued: zero, one, and multiple.³⁴ If the proprietary costs channel is at play, firms with more CTOs could face higher proprietary costs and thus reduce specificity. Figure 2 supports this assertion. Relative to firms with zero CTOs, firms with one CTO convey slightly lower specificity; however, this difference is statistically insignificant. In line with expectations, firms with multiple CTOs exhibit significantly lower specificity in job postings. This difference is statistically different from both zero-CTO firms (as exhibited by the confidence interval) and one-CTO firms (an F -test of the difference in coefficients has a p -value of 0.046).

³⁴I limit this analysis to three categories to ensure estimation support, as a vast majority of firms have zero, one, or two CTOs.

Although the analysis of word count in my research design allows for a job posting specificity interpretation, other text measures could shed further light on the above specificity results. In preliminary and untabulated analyses, I take a small subsample of postings from publicly held firms and construct two alternative Y measures. The first measure is an “uncommon” word count based on the preemptive removal of the most common words from the subsample of job postings. The second measure is a specific item count based on a Named Entity Recognition (NER) Python module (e.g., [Weischedel et al., 2013](#); [Qi et al., 2020](#)) that recognizes mentions of specific items such as organizations, ordinal numbers, cardinal numbers, facilities, and locations.³⁵ Using these measures, I find supporting evidence that CTO firms are less specific in innovation job postings.

As discussed in Section 2, lowering specificity may not be the only margin on which firms reduce the proprietary costs of disclosing labor demand. Instead, firms could choose to hire more discreetly. In Panel C of Table 2, I partially capture this behavior by assessing the anonymity of job postings (i.e., the employer name is omitted from the posting) as the outcome variable. Because employer-level characteristics (other than industry) are not available for these job postings, I use industry classifications as a proxy for proprietary costs. Specifically, I define certain industries as having a high propensity for trade secrecy based on BRDIS data.³⁶ In Column (1) of Panel C, a simple regression shows that job postings in high-secrecy industries are more likely to obfuscate the hiring employer’s name. After controlling for posting feature variation, local labor market characteristics, and job-level shocks, the coefficient is essentially unchanged. Overall, the evidence in Table 2 and Figure 2 is consistent with the existence of a proprietary costs channel in labor search and job postings.

[Table 2]

[Figure 2]

³⁵For an example of the use of a similar measure in previous disclosure literature, see [Hope et al. \(2016\)](#).

³⁶See Appendix A.

5.2 The Effect of the DTSA on Labor Skills Demand

Although the results above are consistent with the proprietary costs channel of job postings, they rely on static characteristics and limit the extent to which I can assess both the labor market channel and the proprietary costs channel in concert. Therefore, as outlined in Sections 2 and 4, I implement a difference-in-differences design around the implementation of the DTSA. By increasing the level of trade secrecy protections, the DTSA plausibly represents a “shock” to both the demand for labor skills and the marginal benefits of opacity. Therefore, while firms could be incentivized to demand more labor skills in job postings for innovation jobs (the labor market channel), they could also be disincentivized to disclose specific information in postings for these trade secrecy-related occupations, especially in slack labor markets (the proprietary costs channel).

In Table 3, I assess the effects of the DTSA on labor skills demand for innovation jobs (relative to non-innovation jobs).³⁷ First, I provide estimates from a simple generalized difference-in-differences research design in Column (1). Here, I control for time-invariant local labor market (county-industry-job group) characteristics and include year-month fixed effects. The outcome variable is the inverse hyperbolic sine of the number of specialized skills. I find that after the implementation of the DTSA, the average innovation job posting requests a greater number of specialized skills relative to the average non-innovation job posting. I then implement the main difference-in-differences design outlined in Section 4, Equation (2). This design similarly accounts for time-invariant characteristics explained by local industries hiring for a job group (county-industry-job group fixed effects) and adds fully flexible time trends by both industry and hiring state. I still find a strong positive relation; compared to the average non-innovation job posting, the change in skills demand is about six percentage points higher for the average innovation job posting. This result is also robust to more granular fully flexible time trends at the county level (rather than at the state

³⁷Internet Appendix Section IA.1 and Table IA.1 report the results of a series of robustness tests for the evidence discussed in Sections 5.2 and 5.3, including alternative proxies, specifications, and standard error clustering.

level), as shown in Column (3). Results are virtually the same, and precision increases.

In Figure 3, I run an event-time specification for the main job group specification in Column (2). Specifically, this specification plots the difference in skills demand between the average innovation job posting and average non-innovation job posting for five periods before and after DTSA implementation, where the difference in the period just prior to DTSA implementation serves as a relative point. No obvious pre-trend or cyclicity in the difference exists. However, in the period of DTSA implementation, I document a significant increase in skills demand among innovation jobs relative to non-innovation jobs. Furthermore, this difference remains persistent through most of the estimation sample after the DTSA.

The main job group specification allows for changes in composition of the types of jobs within job group, which is likely important in documenting the total effect of the DTSA on labor skills demand. For example, firms may convey increased skills demand by hiring for a different composition of jobs.³⁸ However, one may also be interested in the job-level change in skills demand, which involves the inclusion of local labor market controls by *job*, as exhibited in Columns (4) and (5) of Table 3. Although the coefficient is unsurprisingly attenuated, I still document a strong positive skills demand increase after the DTSA.

In sum, I find evidence that trade secrecy protections spur the demand for innovative inputs and that firms convey this demand in job postings through an increase in the average number of specialized skills demanded. This finding is consistent with the labor market channel; in response to increased incentives to innovate, firms disclose their true demand for more skills to labor market participants to procure skilled labor (as an input to innovative activities).

[Table 3]

[Figure 3]

³⁸Indeed, in Internet Appendix Table IA.1, I show that the average creativity and analytical scores of jobs posted for (as measured by the BLS) increase in the innovation job group relative to the non-innovation job group.

5.3 The Effect of the DTSA on Job Posting Specificity

As discussed in Section 2, the DTSA also increases the benefits of opacity regarding activities related to trade secrecy. Because job postings for innovation jobs provide important information about human capital investments that are key to the secretive innovative activities spurred by the DTSA, firms are plausibly incentivized to be less specific in these postings. Therefore, I assess changes in job posting specificity around the implementation of the DTSA. As outlined in Section 4, Equation (3), I control for job posting features (skills demand, degree requirement, experience requirement) in an attempt to hold key labor demand factors constant.³⁹ Results are in Table 4.

Similar to the tests in Section 5.2, I begin with a simple research design, controlling for both county-industry-job group and posting characteristics. I find that, indeed, after the implementation of the DTSA, specificity decreases for the average innovation job posting (relative to the average non-innovation job posting). In Column (2), I implement the main job group specification from Section 4, Equation (3) that similarly controls for local labor market and posting characteristics and adds fully flexible time trends by industry and hiring state. The coefficient estimate remains quite similar: compared to the average non-innovation job posting, the change in specificity is just under three percentage points lower for the average innovation job posting. In Column (3), I find that this relative decrease in specificity is robust to the addition of county-time fixed effects.

In contrast to the skills demand estimation in Section 5.2, the primary purpose of this test is to assess changes in specificity at a granular level such that the job posting comparison before and after DTSA implementation is sound. Therefore, the county-*job*-level specification with more

³⁹The inclusion of these controls helps ensure that I compare similar job postings before and after the DTSA. As shown in Table 3, some of these posting features (i.e., Z) do change as a result of the DTSA (i.e., X), and Z likely affects the word count (i.e., Y) for mechanical and economic reasons. Including Z in a regression of Y on X does not inherently introduce a “bad control” problem if the effect of interest is a controlled direct effect, or CDE, as it is in my primary tests (Cinelli et al., 2021). However, if an unobserved confounder U of Z and Y exists, bias will be introduced in the CDE estimate due to the colliding path $X \rightarrow Z \leftarrow U \rightarrow Y$. In practice, this bias will only be sizeable if my features definition (in concert with my other controls) does not produce an effective mapping between Z and Y . While this is inherently an untestable bias concern, in Internet Appendix Table IA.1, I introduce controls for more posting features to somewhat reduce this concern. Results are similar.

granular job posting controls in Column (4) is of key interest (Section 4, Equation (4)). Although this more granular control structure increases explanatory power, the coefficient remains strongly negative. The addition of county-time fixed effects in Column (5) produces similar results.

I then estimate the differences between the average innovation job posting and non-innovation job posting in event time for the job-level specification in Column (4). Results are in Figure 4. Although some downward trend in specificity occurs from 2014 to 2015, in the periods just preceding DTSA implementation, no stark pre-trend or cyclicalities exist in the difference in specificity. However, in the period of DTSA implementation, a sharp drop in the difference occurs, and the coefficient estimate remains negative through 2017 (although statistical significance varies).⁴⁰ Overall, these results are consistent with the proprietary costs channel of job postings; firms are plausibly incentivized to disclose less information about innovative activities after DTSA implementation, and I consistently find a decrease in specificity for innovation jobs. In sum, the increases in skills demand and the decreases in specificity imply a trade-off between the labor market and proprietary costs channels. While firms convey demand for skilled labor, they are less specific in their disclosures, plausibly due to strategic nondisclosure considerations.

[Table 4]

[Figure 4]

5.3.1 Controlling for Business Uncertainty

Although the documented decrease in specificity is consistent with a proprietary costs channel, an alternative explanation for this decrease is that the DTSA induces new innovative activities that generally involve more uncertainty about required labor inputs, especially in innovation jobs.

Due to that uncertainty, employers are less specific in job postings for innovation jobs, and the

⁴⁰ Although the event-time pattern looks similar to that in Figure 3, note that the magnitudes of event-time fluctuations are much smaller in relative magnitude in Figure 4. This pattern also holds if I instead plot the event-time specification for Column (2) of Table 4.

proprietary costs channel may have minimal importance. Although such uncertainty concerns would not clearly explain the initial cross-sectional results in Table 2 regarding the existence of a proprietary costs channel, they may bias the difference-in-differences estimates in Table 4.

I attempt to address these concerns by (i) introducing a suite of controls that is likely correlated with this “selection” bias, (ii) assessing the changes in the coefficient estimate and R^2 as a result of the inclusion of the controls, and (iii) formally calculating the degree of endogenous selection on unobservables (relative to the selection on observables accounted for by the controls) that would be required to make the estimated coefficient zero (rather than the negative coefficient I document in Table 4, Column (4)). This formal coefficient sensitivity test is suggested by Oster (2019). Intuitively, if the inclusion of these controls results in only a small change in the coefficient and a relatively large change in the R^2 , the degree of selection on unobservables would have to be sizeable to “explain away” the result.

I control for business uncertainty on three dimensions. First, business uncertainty may affect the specificity of job postings for both innovation jobs and non-innovation jobs. Because this phenomenon is likely project- or employer-specific, this type of business uncertainty is likely captured by the inclusion of employer-time fixed effects. Second, business uncertainty may differentially impact the specificity of job postings for innovation jobs. I collect the publicly available firm risk and uncertainty measure from Hassan et al. (2019) and interact this measure with job group as an additional control.⁴¹ Because this measure is time varying, it captures any differential loading that business uncertainty has on innovation jobs above and beyond fully flexible employer shocks that affect all job postings. Third, new and uncertain projects may involve geographic expansion, and this expansion may differentially affect innovation jobs. For 2015 onward, I generate an indicator for whether a firm is hiring in a new county (i.e., a county they did not hire in for 2014), and I then

⁴¹This measure of overall firm-level risk counts the mentions of synonyms for risk or uncertainty in firms’ conference calls and divides by transcript length.

interact this indicator with job group. Overall, the collective inclusion of these controls lends itself to a short estimation window of 2015 to 2016 and a sample for which all measures are available.

In Internet Appendix Table IA.2, I first report the estimation results of the model in Column (4) of Table 4, limiting the sample as described above. Although not identical to the coefficient in Table 4, the estimated coefficient is comparable, negative, and statistically significant at conventional levels. Then, in Column (2), I add the aforementioned suite of controls to capture business uncertainty. The coefficient attenuates slightly, by about .001 in absolute difference. However, the R^2 increases by a relatively large margin. On the surface, this finding indicates that there is a low likelihood that *unobservable* business uncertainty concerns (not fully captured by the suite of controls) attenuate the result to zero. Specifically, the formal sensitivity test from Oster (2019) indicates that the unobservables would likely have to be 78% to 150% as important as the observables to produce a zero treatment effect, depending on the assumed R^2 of the hypothetical “full” model that would also control for unobservables. Overall, this coefficient sensitivity exercise conveys that although business uncertainty may be a valid confounding concern, it is unlikely to attenuate the documented treatment effect to zero, consistent with a robust proprietary costs channel.

5.3.2 The DTSA and Job Posting Anonymity

Although previous tests provide insight on job posting specificity changes around the DTSA, firms could also adjust labor search on another dimension: job posting anonymity. In conversations with hiring professionals, this type of “extensive margin” opacity strategy seems likely to occur at the firm level such that the firm does not reveal “quantity” information about hiring for *both* innovation jobs and non-innovation jobs. Therefore, the baseline difference-in-differences design in Table 4 is not necessarily conducive to testing this mechanism. Nonetheless, an examination of changes in job posting anonymity around the DTSA is prudent. The anonymity outcome variable

has a relatively clear nondisclosure interpretation even without controlling for key labor demand factors, a favorable measurement characteristic when both labor demand factors and disclosure incentives change as a result of the implementation of trade secrecy protections.

In the Internet Appendix, I test for changes in job posting anonymity among innovation jobs based on the level of trade secrecy in an industry. Although this industry-level definition is broad, it represents an intensity-to-treat split, as high-secrecy industries may respond more strongly to trade secret protections.⁴² I hypothesize that job postings in high-secrecy industries are more likely to be anonymous after the DTSA. The results in Table IA.3 support this assertion, even after controlling for fully flexible time trends by job (i.e., job-year-month fixed effects). Therefore, estimated fully within job, job postings are more likely to be anonymous in high-secrecy industries after the DTSA, relative to the pre-period difference. Internet Appendix Figure IA.4 shows no obvious pre-trend in this difference, with a sharp increase after the DTSA that persists for multiple periods. This alternative specification once again supports the assertion that proprietary costs considerations exist in labor search around DTSA implementation.

5.4 The DTSA, Job Posting Specificity, and Labor Market Tightness

The increases in skills demand and the decreases in specificity highlighted in Sections 5.2 and 5.3 convey a trade-off between the labor market and proprietary costs channels. Although firms convey their demand for skilled labor as inputs to innovative activities spurred by the DTSA, they are also less specific, plausibly due to the proprietary nature of those early and granular demand disclosures. However, a further trade-off between the labor market and proprietary costs channels likely exists *within* specificity responses. Although decreased specificity may protect relatively important details about innovative human capital investments and activities from competitors, it may

⁴²For example, in untabulated analysis, the skills demand result in Table 3 is stronger in high-secrecy industries. However, the specificity response is smaller, although the difference is not statistically significant at conventional levels.

also hamper the procurement of optimal job applicants (see Section 2). Therefore, when the right applicants are hard to come by, a prospective employer could be incentivized to be relatively more specific, even though doing so presents a possible trade-off with the proprietary costs channel.

As shown in Section 4, Equation (5), I implement the main job-level design and add interactions to assess labor market tightness heterogeneity in the specificity response.⁴³ I limit the sample to the MSAs for which I have data on the average time-to-fill (TTF) at the six-digit SOC level in 2015, and partition MSAs into high (above-median) and low (below-median) TTF groups within each six-digit SOC. I introduce fully flexible time trends for each of these groups in Column (1), and I then partition the treatment variable by TTF group in Column (2). Doing so produces within-TTF group estimates of the effect of the DTSA on job posting specificity. I find that when labor markets are slack, firms strongly reduce specificity. However, when labor markets are tight, firms attenuate that reduction, and the difference is near statistical significance at conventional levels. Furthermore, using a quintile split of MSAs within each six-digit SOC, I find stronger evidence for this relation in Column (3), and in Figure 5, I plot these coefficients. When labor markets are slack, firms face less of a trade-off in terms of reducing specificity to protect information on human capital investments related to trade secrecy. However, in the tightest labor markets with the most severe search frictions, specificity does not decrease as heavily; in fact, in the highest quintile, the effect is negative but statistically indistinguishable from zero at conventional testing levels. Overall, this evidence is consistent with specificity playing an important role in labor search when significant labor market frictions are present, highlighting a trade-off between the labor market and proprietary costs channels.

[Table 5]

[Figure 5]

⁴³To ensure such results are not purely driven by employer composition across localities, I replace industry-time fixed effects with employer-time fixed effects in Internet Appendix Table IA.4. Results are similar.

6 Conclusion

I study the extent to which firms treat job postings as disclosure channels with both labor market and strategic implications. I provide evidence that firms with high proprietary costs have incentives to be less specific in job postings. Furthermore, using a shock to these incentives in the form of federal trade secrecy protections, I am able to highlight the multiple channels through which firms treat their own job postings as disclosures. While firms convey their demand for skilled labor in innovation jobs after the implementation of the DTSA, they also decrease the specificity of these job postings. When search costs are high, firms face a further trade-off; lower specificity could facilitate the obfuscation of information related to innovative human capital investments and activities, but it could also hamper efficient job search and match quality. Consistent with this trade-off, I find that the decrease in specificity is muted when labor markets are tight. Overall, this evidence provides key insights on the following: (i) a demand disclosure related to human capital investment, an investment for which detailed disclosure is not widely available otherwise; (ii) the real labor demand responses of firms to changes in transparency incentives; and (iii) the disclosure trade-offs firms face in labor investment.

My evidence should be interpreted with three caveats in mind. First, I focus on labor demand in the manufacturing sector, as manufacturing firms likely face a salient trade-off between the labor market channel and the proprietary costs channel. However, I acknowledge that this focus could limit the external validity of the findings in this paper. Second, my primary measure of specificity—the number of words in a job posting after holding key posting features fixed—likely represents the *contextual* specificity of job postings. Although this measure of contextual specificity captures an important element of the discretion involved in disclosure choices in job postings and facilitates the separation of disclosure choices from labor demand factors, firms may also adjust specificity

on other margins as well (e.g., in obfuscating key posting features themselves). My anonymity measure takes one step in the direction of capturing these other margins, and further steps could be taken. Third, although I show the disclosure choices that firms make in their own job postings as they relate to the labor market channel and the proprietary costs channel, I do not observe the impact of direct disclosure costs on the trade-off between the two. For example, due to the financial and capacity constraints of division managers and human resources departments, the extent to which firms make dynamic disclosure choices in job postings could be heterogeneous. I leave the investigation of these pure firm-level human capital disclosure costs to future research.

Appendix A: Variable Definitions

Variable Name	Definition	Data Source
Job	Defined as the eight-digit ONET code of the job posting, an occupation code from the Department of Labor.	BGT
Job Group	Innovation job group (two-digit Standard Occupational Classification (SOC) codes 11 to 29, primarily engineer, computer scientist, and manager job postings) or non-innovation job group (all other SOC codes, primarily sales representative and production worker job postings).	BGT, own calculation
Innov. Group	An indicator variable equal to one if a posting is for an innovation job (two-digit SOC codes 11 to 29) and zero for a non-innovation job (all other SOC codes).	BGT, own calculation
Industry postDTSA	The three-digit NAICS level. An indicator variable equal to one in or after May 2016, and zero otherwise.	BGT, Compustat Legislation, own calculation
ln(Word Count)	The natural log of the number of words in a job posting, after the removal of hyperlinks and HTML tags.	BGT, own calculation
ihS(Num. Specialized Skills)	The inverse hyperbolic sine transformation of the number of specialized skills in a job posting: $\ln(\text{NumSpecializedSkills} + \sqrt{\text{NumSpecializedSkills}^2 + 1})$. This transformation closely approximates the natural log function while supporting values of zero. Specialized skills are determined based on a proprietary skill cleaning and clustering algorithm by BGT.	BGT
Asks for College Degree	An indicator variable equal to one if a posting asks for a college degree (bachelor's and above), and zero otherwise.	BGT, own calculation
Asks for Experience	An indicator variable equal to one if a posting asks for experience, and zero otherwise.	BGT, own calculation
Posting Features	The flexible interaction of the number of specialized skills, the presence of a degree requirement, and the presence of an experience requirement in a job posting. Job postings with more than 30 specialized skills (less than 1% of the sample) are "bunched" at 30 for conditional support, yielding a total of $31 \times 2 \times 2 = 124$ combinations. These are then interacted with job group or job.	BGT, own calculation
Firm Size	A decile rank of firm size based on the number of job postings in the sample.	BGT, own calculation
Privately Held Firm	An indicator variable equal to one if a posting is from a firm that is privately held, as determined (in complement) by a fuzzy match to Compustat firm names.	BGT, own calculation
ihS(Num. of CTOs)	The inverse hyperbolic sine transformation of the number of confidential treatment orders (CTOs) issued by the SEC to the firm from 2014 to 2017. Only defined for publicly held firms.	EDGAR, own calculation
1 (Anon. Job Posting)	An indicator variable equal to one if the employer name is not clearly available in the job posting, and zero otherwise. This is primarily due to a job posting being listed by a recruiter and/or on an anonymous job search board.	BGT, own calculation
High-Secrecy Industry	An indicator variable equal to one if the three-digit NAICS industry of the job posting is food manufacturing (311), petroleum and coal products manufacturing (324), chemicals manufacturing (325), or computer and electronic products manufacturing (334), and zero otherwise. This classification is based on Table 43 ("Importance of trade secrets for companies with and without R&D activity, by industry and company size") of the Business R&D and Innovation Survey (BRDIS, 2011).	BRDIS, BGT, Compustat
Time-to-Fill (TTF)	A partitioning variable based on the average time-to-fill of job postings in 2015 at the six-digit SOC-MSA level for 50 MSAs. A two-group definition defines the median by six-digit SOC and splits MSAs into high and low TTF groups. A five-group definition does the same by quintiles within six-digit SOC.	BGT, own calculation

References

- Babina, T., Fedyk, A., He, A. X., and Hodson, J. (2020). Artificial Intelligence, Firm Growth, and Industry Concentration. *SSRN Electronic Journal*.
- Badertscher, B., Shroff, N., and White, H. D. (2013). Externalities of public firm presence: Evidence from private firms' investment decisions. *Journal of Financial Economics*, 109(3):682–706.
- Banfi, S. and Villena-Roldán, B. (2019). Do high-wage jobs attract more applicants? Directed search evidence from the online labor market. *Journal of Labor Economics*, 37(3):715–746.
- Barth, M. E., Landsman, W. R., Tian, X. S., and Yu, M. (2020). Does Voluntary Disclosure Substitute for Mandatory Disclosure? Evidence from Contract Redactions. *SSRN Electronic Journal*.
- Batish, B. A., Gordon, A., Kelper, J. D., Larcker, D. F., Tayan, B., and Yu, C. (2021). What Do Companies Say About Their “ Most Important Asset ”? *SSRN Electronic Journal*.
- Berger, P. G. (2011). Challenges and opportunities in disclosure research — A discussion of ‘ the financial reporting environment : Review of the recent literature ’. *Journal of Accounting and Economics*, 51(1-2):204–218.
- Bhattacharya, S. and Ritter, J. R. (1983). Innovation and Communication: Signaling with Partial Disclosure. *The Review of Economic Studies*, 50(2):331–346.
- Biddle, G. C., Hilary, G., and Verdi, R. S. (2009). How does financial reporting quality relate to investment efficiency? *Journal of Accounting and Economics*, 48(2-3):112–131.
- Bloom, N., Hassan, T. A., Kalyani, A., Lerner, J., and Tahoun, A. (2021). The Diffusion of Disruptive Technologies. *SSRN Electronic Journal*.
- Boulland, R., Bourveau, T., and Breuer, M. (2021). Corporate Websites: A New Measure of Voluntary Disclosure. *SSRN Electronic Journal*.
- Breuer, M., Leuz, C., and Vanhaverbeke, S. (2020). Reporting Regulation and Corporate Innovation. *SSRN Electronic Journal*.
- Callen, J. L., Fang, X., and Zhang, W. (2020). Protection of proprietary information and financial reporting opacity: Evidence from a natural experiment. *Journal of Corporate Finance*, 64(January 2019):101641.

- Cannan, J. (2017). A (mostly) legislative history of the defend trade secrets act of 2016. *Law Library Journal*, 109(3):363–386.
- Cao, Y., Cheng, S., Tucker, J. W., and Wan, C. (2022). Technological Peer Pressure and Skill Specificity of Job Postings. *SSRN Electronic Journal*.
- Cao, Z. and Chen, S. (2019). Does Trade Secrets Protection Affect Labor Investment Strategy? Evidence From the Inevitable Disclosure Doctrine. *SSRN Electronic Journal*.
- Chang, E. and Chin, H. (2018). Signaling or experiencing: Commitment HRM effects on recruitment and employees' online ratings. *Journal of Business Research*, 84:175–185.
- Cheng, M., Dhaliwal, D., and Zhang, Y. (2013). Does investment efficiency improve after the disclosure of material weaknesses in internal control over financial reporting? *Journal of Accounting and Economics*, 56(1):1–18.
- Choi, B.-G., Choi, J. H., and Malik, S. (2021). Job Search with Financial Information: Theory and Evidence. *SSRN Electronic Journal*.
- Choi, J. H., Gipper, B., and Malik, S. (2019). Financial Reporting Quality, Turnover Risk, and Wage Differentials: Evidence from Worker-level Data. *SSRN Electronic Journal*.
- Christensen, H. B., Hail, L., and Leuz, C. (2021). Mandatory CSR and sustainability reporting: economic analysis and literature review. *Review of Accounting Studies*, 26(3):1176–1248.
- Cinelli, C., Forney, A., and Pearl, J. (2021). A Crash Course in Good and Bad Controls. *SSRN Electronic Journal*.
- Conley, T., Gonçalves, S., and Hansen, C. (2018). Inference with Dependent Data in Accounting and Finance Applications. *Journal of Accounting Research*, 56(4):1139–1203.
- deHaan, E., Li, N., and Zhou, F. (2021). Financial Reporting and Employee Job Search. *SSRN Electronic Journal*.
- Deming, D. and Kahn, L. B. (2018). Skill requirements across firms and labor markets: Evidence from job postings for professionals. *Journal of Labor Economics*, 36(S1):S337–S369.
- Dey, A. and White, J. T. (2021). Labor mobility and antitakeover provisions. *Journal of Accounting and Economics*, 71(2-3):101388.

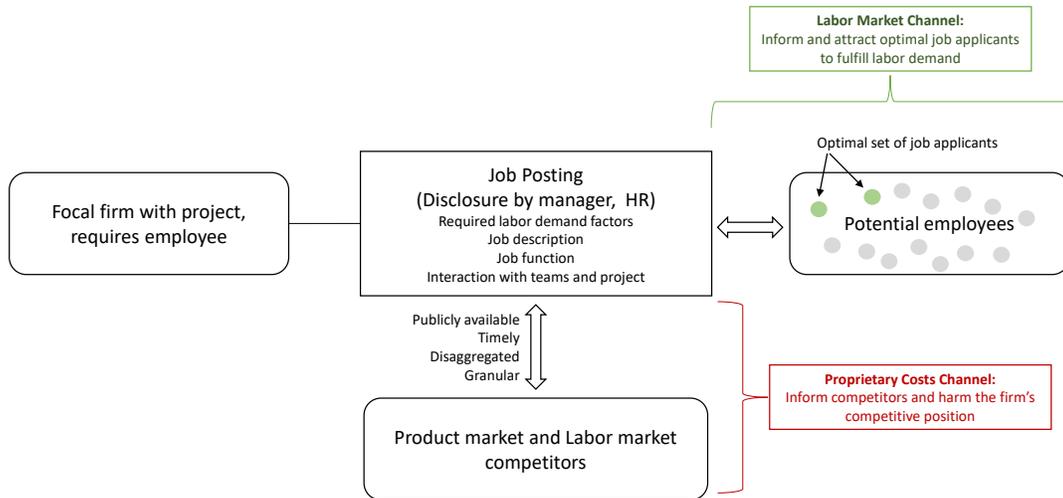
- Dhaliwal, D. S., Li, O. Z., Tsang, A., and Yang, Y. G. (2011). Voluntary Nonfinancial Disclosure and the Cost of Equity Capital : The Initiation of Corporate Social Responsibility Reporting. *The Accounting Review*, 86(1):59–100.
- Feldman, D. C., Bearden, W. O., Hardesty, D. M., Journal, S., Spring, N., and Taylor, P. (2006). Varying the Content of Job Advertisements : The Effects of Message Specificity. *Journal of Advertising*, 35(1):123–141.
- Glaeser, S. (2018). The effects of proprietary information on corporate disclosure and transparency: Evidence from trade secrets. *Journal of Accounting and Economics*, 66(1):163–193.
- Goldstein, H. W., Pulakos, E. D., Passmore, J., and Semedo, C. (2017). *The Wiley Blackwell Handbook of the Psychology of Recruitment, Selection and Employee Retention*. John Wiley and Sons.
- Gutiérrez, E., Lourie, B., Nekrasov, A., and Shevlin, T. (2020). Are online job postings informative to investors? *Management Science*, 66(7):3133–3141.
- Ha, J. and Feng, M. (2018). Conditional conservatism and labor investment efficiency. *Journal of Contemporary Accounting and Economics*, 14(2):143–163.
- Hassan, T. A., Hollander, S., Lent, L. V., and Tahoun, A. (2019). Firm-Level Political Risk: Measurement and Effects. *Quarterly Journal of Economics*, 134(4):2135–2202.
- Heinle, M. S., Samuels, D., and Taylor, D. (2020). Proprietary Costs and Disclosure Substitution: Theory and Empirical Evidence. *SSRN Electronic Journal*.
- Hope, O. K., Hu, D., and Lu, H. (2016). The benefits of specific risk-factor disclosures. *Review of Accounting Studies*, 21(4):1005–1045.
- Jones, D. A., Willness, C. R., and Madey, S. (2014). Why are job seekers attracted by corporate social performance? Experimental and field tests of three signal-based mechanisms. *Academy of Management Journal*, 57(2):383–404.
- Jung, B., Lee, W. J., and Weber, D. P. (2014). Financial Reporting Quality and Labor Investment Efficiency. *Contemporary Accounting Research*, 31(4):1047–1076.
- Kircher, P. A. (2020). Search design and online job search – new avenues for applied and experimental research. *Labour Economics*, 64:101820.

- Klasa, S., Ortiz-Molina, H., Serfling, M., and Srinivasan, S. (2018). Protection of trade secrets and capital structure decisions. *Journal of Financial Economics*, 128(2):266–286.
- Lancaster, V., Mahoney-Nair, D., and Ratcliff, N. J. (2019). Technology Report Review of Burning Glass Job-ad Data. *Working Paper, University of Virginia*.
- Leuz, C. and Wysocki, P. D. (2016). The Economics of Disclosure and Financial Reporting Regulation: Evidence and Suggestions for Future Research. *Journal of Accounting Research*, 54(2):525–622.
- Levine, D. and Seaman, C. (2018). The DTSA at One: An Empirical Study of the First Year of Litigation Under the Defend Trade Secrets Act. *Wake Forest Law Review*.
- Li, Y., Lin, Y., and Zhang, L. (2018). Trade Secrets Law and Corporate Disclosure: Causal Evidence on the Proprietary Cost Hypothesis. *Journal of Accounting Research*, 56(1):265–308.
- Liu, Y. (2021). Labor-Based Asset Pricing. *SSRN Electronic Journal*.
- Marinescu, I. and Wolthoff, R. (2020). Opening the black box of the matching function: The power of words. *Journal of Labor Economics*, 38(2):535–568.
- Menzio, G. (2007). A theory of partially directed search. *Journal of Political Economy*, 115(5):748–769.
- Modestino, A. S., Shoag, D., and Balance, J. (2020). Upskilling: Do employers demand greater skill when workers are plentiful? *Review of Economics and Statistics*, 102(4):793–805.
- Modestino, A. S., Shoag, D., and Ballance, J. (2016). Downskilling: changes in employer skill requirements over the business cycle. *Labour Economics*, 41:333–347.
- Ndofor, H. A. and Levitas, E. (2004). Signaling the strategic value of knowledge. *Journal of Management*, 30(5):685–702.
- Oster, E. (2019). Unobservable Selection and Coefficient Stability: Theory and Evidence. *Journal of Business and Economic Statistics*, 37(2):187–204.
- Oyer, P. and Schaefer, S. (2011). Personnel economics: Hiring and incentives. *Handbook of Labor Economics*, 4:1769–1823.

- Pacelli, J., Shi, T., and Zou, Y. (2022). Communicating Corporate Culture in Labor Markets: Evidence from Job Postings. *SSRN Electronic Journal*.
- Png, I. P. L. (2017). Law and Innovation : Evidence from the Uniform Trade Secrets Act. *Review of Economics and Statistics*, 99(1):167–179.
- Qi, P., Zhang, Y., Zhang, Y., Bolton, J., and Manning, C. D. (2020). Stanza: A Python Natural Language Processing Toolkit for Many Human Languages. *arXiv*.
- Rauter, T. (2020). The Effect of Mandatory Extraction Payment Disclosures on Corporate Payment and Investment Policies Abroad. *Journal of Accounting Research*, 58(5):1075–1116.
- Roychowdhury, S., Shroff, N., and Verdi, R. S. (2019). The effects of financial reporting and disclosure on corporate investment: A review. *Journal of Accounting and Economics*, 68(2-3):101246.
- Simpson, A. and Tamayo, A. (2020). Real effects of financial reporting and disclosure on innovation. *Accounting and Business Research*, 50(5):401–421.
- Verrecchia, R. E. (1983). Discretionary disclosure. *Journal of Accounting and Economics*, 5:179–194.
- Verrecchia, R. E. (2001). Essays on disclosure. *Journal of Accounting and Economics*, 32(1-3):97–180.
- Verrecchia, R. E. and Weber, J. (2006). Redacted disclosure. *Journal of Accounting Research*, 44(4):791–814.
- Walker, H. J. and Hinojosa, A. S. (2014). Recruitment: The role of job advertisements. *The Oxford Handbook of Recruitment*, pages 269–283.
- Weischedel, R., Palmer, M., Marcus, M., Hovy, E., Pradhan, S., Ramshaw, L., Xue, N., Taylor, A., Kaufman, J., and Franchini, M. (2013). OntoNotes Release 5.0. *Linguistic Data Consortium*.

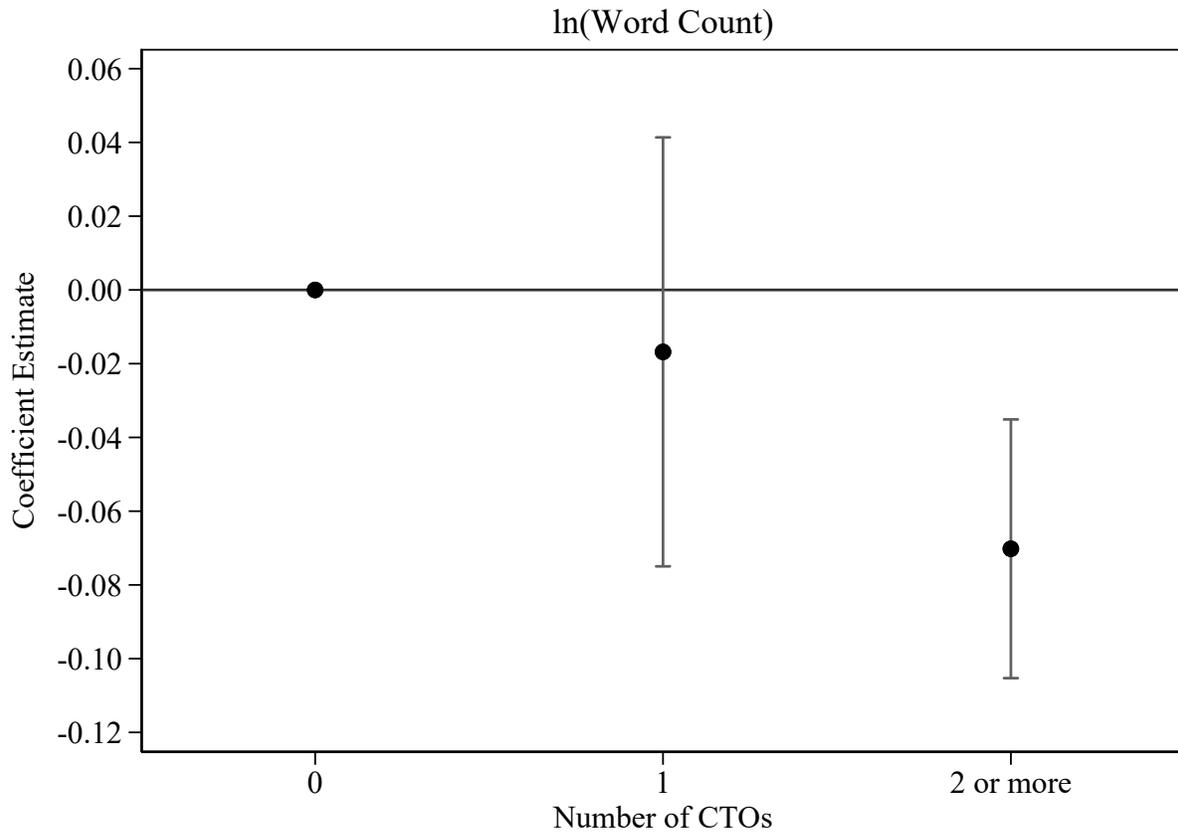
Figures and Tables

Figure 1: Conceptual Framework



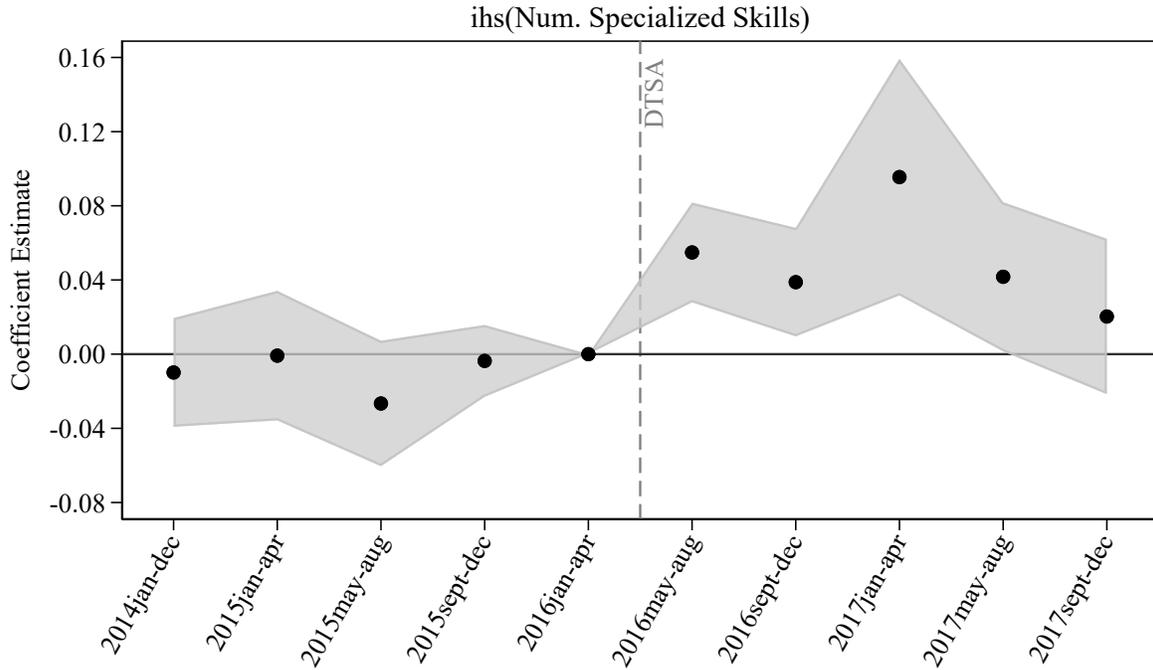
This figure presents a conceptual framework for job postings as disclosures to both labor market participants and competitors.

Figure 2: Job Posting Specificity by Public Firm Opacity Level



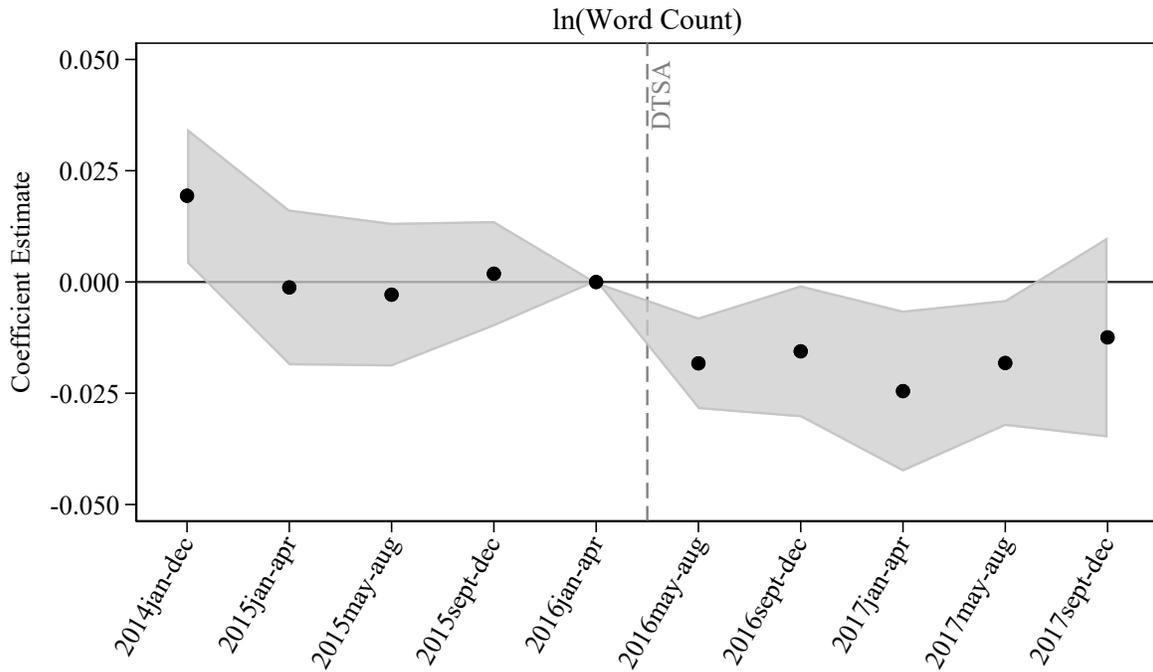
This figure plots an alternative specification of that in Table 2, Panel B, Column (2) by replacing the continuous $\ln(\text{Num. of CTOs})$ variable with three indicators: (i) zero CTOs issued, (ii) one CTO issued, and (iii) multiple CTOs issued from 2014 to 2017. A confidential treatment request, if approved by an issued CTO, allows a firm to redact specific information in mandatory disclosures if the revelation of that information would put the firm at a competitive disadvantage. The coefficient on multiple-CTO firms is statistically different from both zero-CTO firms (as exhibited by the confidence interval) and one-CTO firms (an F -test of difference in coefficients has a p -value of 0.046). Confidence bands, representing a 95% confidence interval, are based on standard errors clustered two ways at the state-industry and job levels. Variable definitions are provided in Appendix A.

Figure 3: The Effect of the DTSA on Labor Skills Demand in Event Time



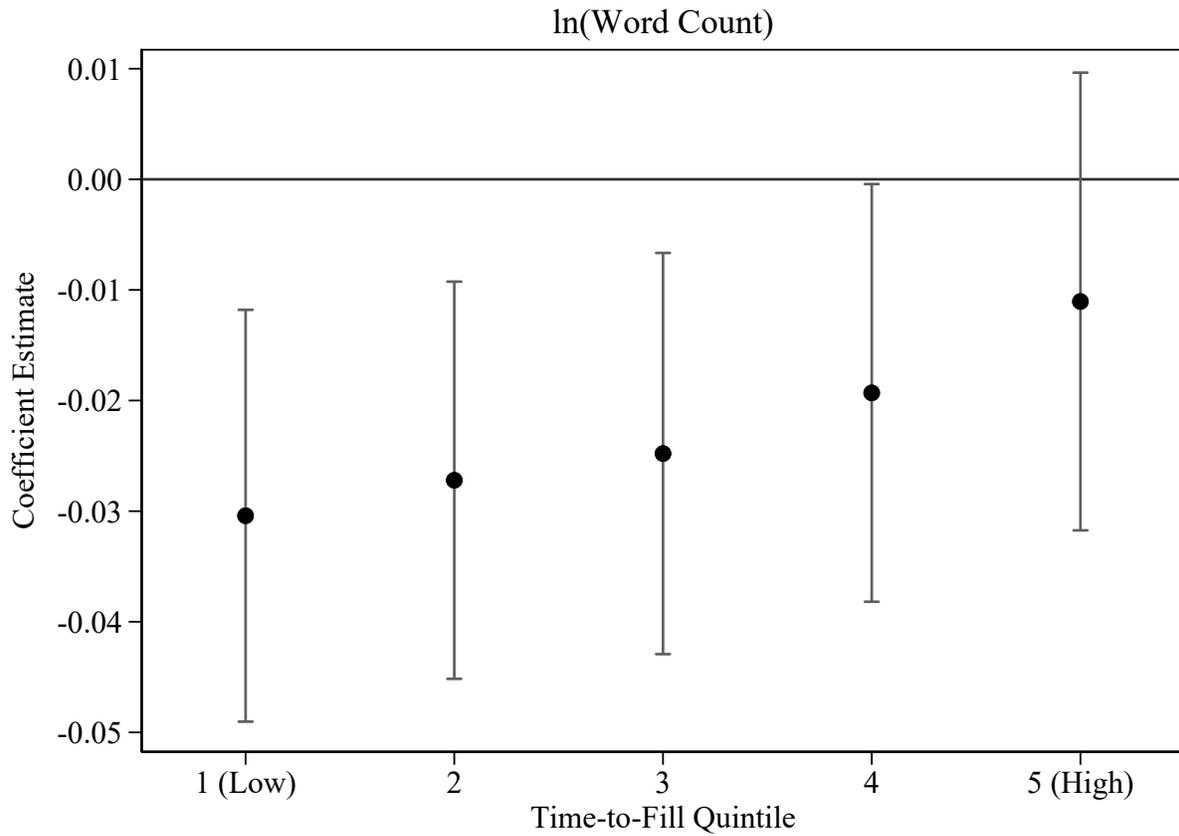
This figure plots the difference in specialized skills demand between the innovation job group and the non-innovation job group over time. The outcome variable is the skills demand of the job posting, measured as the inverse hyperbolic sine transformation of the number of specialized skills, a close approximation of the natural log transformation that admits zero: $\ln(\text{NumSpecializedSkills} + \sqrt{\text{NumSpecializedSkills}^2 + 1})$. The model is from Column (2) in Table 3 and interacts *Innov. Group* with 10 time indicators, the first indicator representing 2014 and those thereafter representing four-month periods. The period just before DTSA implementation serves as the relative-difference estimate (and therefore is zero with no confidence bands). The DTSA was signed into law in May 2016. Confidence bands, representing a 95% confidence interval, are based on standard errors clustered two ways at the state-industry and job levels. Variable definitions are provided in Appendix A.

Figure 4: The Effect of the DTSA on Job Posting Specificity in Event Time



This figure plots the difference in specificity between the innovation job group and the non-innovation job group over time. The outcome variable is the natural log of word count. The model is from Column (4) in Table 4 and interacts *Innov. Group* with 10 time indicators, the first indicator representing 2014 and those thereafter representing four-month periods. The period just before DTSA implementation serves as the relative-difference estimate (and therefore is zero with no confidence bands). The DTSA was signed into law in May 2016. Confidence bands, representing a 95% confidence interval, are based on standard errors clustered two ways at the state-industry and job levels. Variable definitions are provided in Appendix A.

Figure 5: The Effect of the DTSA on Job Posting Specificity by Labor Market Tightness Quintile



This figure plots the coefficients of the partitioned regression from Column (3) in Table 5. The outcome variable is the natural log of word count. The time-to-fill (TTF) partition is determined by a quintile split within each six-digit SOC of the average TTF by MSA. As reported in Table 5, a difference-in-coefficients test between the coefficients on the Low and High TTF quintiles has a p -value of 0.076. Confidence bands, representing a 95% confidence interval, are based on standard errors clustered two ways at the state-industry and job levels. Variable definitions are provided in Appendix A.

Table 1: Descriptive Statistics

Panel A: Summary Statistics for Job Posting Characteristics, Innovation Job Group								
Variable	N	Mean	SD	P5	P25	Median	P75	P95
lhs(Num. Specialized Skills)	1,755,614	2.87	0.70	1.44	2.49	3.00	3.33	3.83
ln(Word Count)	1,755,614	6.20	0.53	5.29	5.94	6.27	6.54	6.91
Num. Specialized Skills	1,755,614	10.80	6.69	2.00	6.00	10.00	14.00	23.00
Word Count	1,755,614	554.02	257.69	198.00	380.00	526.00	693.00	1003.00
Asks for College Degree	1,755,614	0.73	0.45	0.00	0.00	1.00	1.00	1.00
Asks for Experience	1,755,614	0.71	0.46	0.00	0.00	1.00	1.00	1.00
Posting by Privately Held Firm	1,673,330	0.33	0.47	0.00	0.00	0.00	1.00	1.00
Anonymous Job Posting	1,755,614	0.05	0.21	0.00	0.00	0.00	0.00	0.00

Panel B: Summary Statistics for Job Posting Characteristics, Non-Innovation Job Group								
Variable	N	Mean	SD	P5	P25	Median	P75	P95
lhs(Num. Specialized Skills)	1,202,213	2.40	0.76	0.88	1.82	2.49	3.00	3.47
ln(Word Count)	1,202,213	6.05	0.57	5.04	5.77	6.13	6.42	6.83
Num. Specialized Skills	1,202,213	7.02	5.03	1.00	3.00	6.00	10.00	16.00
Word Count	1,202,213	487.14	247.47	155.00	319.00	460.00	612.00	922.00
Asks for College Degree	1,202,213	0.25	0.44	0.00	0.00	0.00	1.00	1.00
Asks for Experience	1,202,213	0.54	0.50	0.00	0.00	1.00	1.00	1.00
Posting by Privately Held Firm	1,132,010	0.42	0.49	0.00	0.00	0.00	1.00	1.00
Anonymous Job Posting	1,202,213	0.06	0.23	0.00	0.00	0.00	0.00	1.00

Panel C: Summary Statistics for Firm Opacity								
Variable	N	Mean	SD	P5	P25	Median	P75	P95
Privately Held Firm	6,019	0.81	0.39	0.00	1.00	1.00	1.00	1.00
Num. of CTOs	1,126	1.08	2.99	0.00	0.00	0.00	1.00	7.00
lhs(Num. of CTOs)	1,126	0.46	0.88	0.00	0.00	0.00	0.88	2.64

This table presents summary statistics for key job posting variables by job group (Panels A and B) and summary statistics for firm-level opacity measures (Panel C). In Panels A and B, observations are at the job posting level, and in Panel C, observations are at the firm level. The innovation job group includes job postings with two-digit Standard Occupational Classification (SOC) codes 11 to 29, with most job postings hiring for (11) Management, (15) Computer and Mathematical, and (17) Architecture and Engineering. The non-innovation job group includes job postings with all other SOC codes, with most job postings hiring for (41) Sales, (43) Administrative Support, and (51) Production. The sample consists of job postings from manufacturing firms (NAICS codes 31-33) in the US from 2014 to 2017. Variable definitions are provided in Appendix A.

Table 2: Job Posting Specificity, Anonymity, and Opacity Incentives

Panel A: Specificity for Privately Held vs. Publicly Held Firms

$Y = \ln(\text{Word Count})$	(1)	(2)
Privately Held Firm	-0.053*** (-3.61)	-0.030** (-2.38)
<i>Fixed Effects:</i>		
Industry \times Firm Size	Y	Y
Posting Features \times Job		Y
County \times Job		Y
Industry \times Job \times YearMonth		Y
Observations	1,673,330	1,601,865
Adjusted R^2	0.033	0.495

Panel B: Specificity for More Opaque vs. Less Opaque Publicly Held Firms

$Y = \ln(\text{Word Count})$	(1)	(2)
ihs(Num. of CTOs)	-0.022** (-2.03)	-0.028*** (-3.11)
<i>Fixed Effects:</i>		
Industry \times Firm Size	Y	Y
Posting Features \times Job		Y
County \times Job		Y
Industry \times Job \times YearMonth		Y
Observations	1,118,100	1,056,616
Adjusted R^2	0.030	0.510

Panel C: Anonymous Job Posting in High-Secrecy vs. Low-Secrecy Industries

$Y = \mathbb{1}(\text{Anon. Job Posting})$	(1)	(2)
Posting in High-Secrecy Industry	0.041*** (6.18)	0.039*** (7.65)
<i>Fixed Effects:</i>		
Posting Features \times Job		Y
County \times Job		Y
Job \times YearMonth		Y
Observations	1,755,614	1,712,365
Adjusted R^2	0.009	0.156

This table reports the results of the following OLS regression (Section 4, Equation (1)) in Column (2):

$$Y_{ikct} = \beta \text{FirmOpacity}_k + \alpha_{zd} + \omega_{vj} + \lambda_{cj} + \eta_{jdt} + \epsilon_{ikct}.$$

Above, v denotes a set of posting features (number of specialized skills \times has degree requirement \times has experience requirement) conveyed in job posting i , k denotes the employer making the posting, c denotes the county in which the job is located, t denotes the year-month of the posting, d denotes the three-digit NAICS code of the employer k , j denotes the eight-digit ONET code associated with job posting i , and z denotes a decile rank of firm size. In Panels A and B, FirmOpacity_k is the listing status of firm k and the inverse hyperbolic sine of the number of confidential treatment orders (CTOs) issued, respectively. The natural log of word count is the outcome variable. In Panel C, FirmOpacity_k is determined by industry status, and job posting anonymity is the outcome variable (industry/firm controls are removed). The sample consists of manufacturing firms' job postings for innovation jobs (SOC codes 11 to 29, $J = 1$) from 2014 to 2017. t -statistics, in parentheses, are based on standard errors clustered two ways at the state-industry and job levels (***:1%, **:5%, *:10%). Variable definitions are provided in Appendix A.

Table 3: The Effect of the DTSA on Labor Skills Demand

$Y = \text{ihS}(\text{Num. Specialized Skills})$	Job Group Fixed Effects			Job Fixed Effects	
	(1) Basic Specification	(2) Main Job Group Specification	(3) Add County×Time Fixed Effects	(4) Main Job Specification	(5) Add County×Time Fixed Effects
postDTSA*Innov. Group	0.059*** (3.27)	0.060*** (4.56)	0.060*** (4.65)	0.033*** (3.15)	0.034*** (3.29)
<i>Fixed Effects:</i>					
County × Industry × Job Group	Y	Y	Y		
County × Job				Y	Y
YearMonth	Y				
Industry × YearMonth		Y	Y	Y	Y
State × YearMonth		Y		Y	
County × YearMonth			Y		Y
Observations	2,799,189	2,799,189	2,784,328	2,740,319	2,722,860
Adjusted R^2	0.203	0.209	0.217	0.341	0.343

This table reports the results of the following OLS regressions (Section 4, Equation (2)) in Columns (2) and (4), respectively:

$$\begin{aligned} \text{ihS}(\text{SpecSkills})_{ikct} &= \beta \text{PostDTSA}_t * \text{Innov.Group}_{J=1} + \lambda_{cdJ} + \eta_{dt} + \delta_{st} + \epsilon_{ikct}, \\ \text{ihS}(\text{SpecSkills})_{ikct} &= \beta \text{PostDTSA}_t * \text{Innov.Group}_{J=1} + \lambda_{cj} + \eta_{dt} + \delta_{st} + \epsilon_{ikct}. \end{aligned}$$

Above, k denotes the employer making job posting i , c and s denote the county and state in which the job is located, t denotes the year-month of the posting, d denotes the three-digit NAICS code of the employer k , j denotes the eight-digit ONET code associated with job posting i , and job group J indicates whether job j is an innovation job (SOC codes 11 to 29, $J = 1$) or a non-innovation job (other SOC codes, $J = 0$). The outcome variable is the skills demand of the job posting, measured as the inverse hyperbolic sine transformation of the number of specialized skills, a close approximation of the natural log transformation that admits zero: $\ln(\text{NumSpecializedSkills} + \sqrt{\text{NumSpecializedSkills}^2 + 1})$. The sample consists of manufacturing firms' job postings from 2014 to 2017 for which the employer name is available. t -statistics, in parentheses, are based on standard errors clustered two ways at the state-industry (965 clusters) and job (923 clusters) levels (***:1%, **:5%, *:10%). Variable definitions are provided in Appendix A.

Table 4: The Effect of the DTSA on Job Posting Specificity

$Y = \ln(\text{Word Count})$	Job Group Fixed Effects			Job Fixed Effects	
	(1) Basic Specification	(2) Main Job Group Specification	(3) Add County×Time Fixed Effects	(4) Main Job Specification	(5) Add County×Time Fixed Effects
postDTSA*Innov. Group	-0.029*** (-4.14)	-0.027*** (-4.39)	-0.026*** (-4.56)	-0.025*** (-4.40)	-0.025*** (-4.73)
<i>Fixed Effects:</i>					
Posting Features × Job Group	Y	Y	Y		
County × Industry × Job Group	Y	Y	Y		
Posting Features × Job County × Job				Y Y	Y Y
YearMonth	Y				
Industry × YearMonth		Y	Y	Y	Y
State × YearMonth		Y		Y	
County × YearMonth			Y		Y
Observations	2,799,189	2,799,189	2,784,328	2,731,066	2,713,531
Adjusted R^2	0.429	0.435	0.445	0.496	0.502

This table reports the results of the following OLS regressions (Section 4, Equations (3) and (4)) in Columns (2) and (4), respectively:

$$\ln(\text{WordCt})_{ikct} = \beta \text{PostDTSA}_t * \text{Innov.Group}_{J=1} + \zeta_{vJ} + \lambda_{cdJ} + \eta_{dt} + \delta_{st} + \epsilon_{ikct},$$

$$\ln(\text{WordCt})_{ikct} = \beta \text{PostDTSA}_t * \text{Innov.Group}_{J=1} + \omega_{vj} + \iota_{cj} + \eta_{dt} + \delta_{st} + \epsilon_{ikct}.$$

Above, v denotes a set of posting features (number of specialized skills×has degree requirement×has experience requirement) conveyed in job posting i , k denotes the employer making the posting, c and s denote the county and state in which the job is located, t denotes the year-month of the posting, d denotes the three-digit NAICS code of the employer k , j denotes the eight-digit ONET code associated with job posting i , and job group J indicates whether job j is an innovation job (SOC codes 11 to 29, $J = 1$) or a non-innovation job (other SOC codes, $J = 0$). The outcome variable is the natural log of word count. The sample consists of manufacturing firms' job postings from 2014 to 2017 for which the employer name is available. t -statistics, in parentheses, are based on standard errors clustered two ways at the state-industry (965 clusters) and job (923 clusters) levels (***:1%, **:5%, *:10%). Variable definitions are provided in Appendix A.

Table 5: The Effect of the DTSA on Job Posting Specificity by Labor Market Tightness Group

$Y = \ln(\text{Word Count})$	(1) Main Job Specification, Add Time-To-Fill \times Time Fixed Effects	(2) Split on 2 Time-To-Fill Groups	(3) Split on 5 Time-To-Fill Groups
postDTSA*Innov. Group	-0.022*** (-3.30)		
postDTSA*Innov. Group*Time-To-Fill, Low		-0.028*** (-3.90)	
postDTSA*Innov. Group*Time-To-Fill, High		-0.017** (-2.15)	
postDTSA*Innov. Group*Time-To-Fill, 1 (Low)			-0.030*** (-3.21)
postDTSA*Innov. Group*Time-To-Fill, 2			-0.027*** (-2.98)
postDTSA*Innov. Group*Time-To-Fill, 3			-0.025*** (-2.69)
postDTSA*Innov. Group*Time-To-Fill, 4			-0.019** (-2.01)
postDTSA*Innov. Group*Time-To-Fill, 5 (High)			-0.011 (-1.05)
$F\text{-test}(\beta_L \text{ v. } \beta_H), p\text{-value}$		0.111	0.076
<i>Fixed Effects:</i>			
Posting Features \times Job	Y	Y	Y
County \times Job	Y	Y	Y
Industry \times YearMonth	Y	Y	Y
Time-To-Fill Group \times YearMonth	Y	Y	Y
Observations	1,734,631	1,734,631	1,734,631
Adjusted R^2	0.462	0.462	0.462

This table reports the results of the following OLS regression (Section 4, Equation (5)) in Column (2):

$$\ln(\text{WordCt})_{ikct} = \beta_L \text{PostDTSA}_t * \text{Innov.Group}_{J=1} * \text{LowTTF}_{cj}^{p=L} + \beta_H \text{PostDTSA}_t * \text{Innov.Group}_{J=1} * \text{HighTTF}_{cj}^{p=H} + \omega_{vj} + \iota_{cj} + \eta_{dt} + \alpha_{pt} + \epsilon_{ikct}.$$

Above, v denotes a set of posting features (number of specialized skills \times has degree requirement \times has experience requirement) conveyed in job posting i , k denotes the employer making the posting, c and s denote the county and state in which the job is located, t denotes the year-month of the posting, d denotes the three-digit NAICS code of the employer k , j denotes the eight-digit ONET code associated with job posting i , and job group J indicates whether job j is an innovation job (SOC codes 11 to 29, $J = 1$) or a non-innovation job (other SOC codes, $J = 0$). The outcome variable is the natural log of word count. The time-to-fill (TTF) partition is determined by the average time to fill a job posting at the six-digit SOC level by MSA in 2015. The sample consists of manufacturing firms' job postings from 2014 to 2017 for which the employer name is available. t -statistics, in parentheses, are based on standard errors clustered two ways at the state-industry (726 clusters) and job (567 clusters) levels (**:1%, **:5%, *:10%). " F -test, p -value" reports the p -value of a difference-in-coefficients test. Variable definitions are provided in Appendix A.

Internet Appendix to
Disclosing Labor Demand

Gurpal Sran

January 2023

Table of Contents

Section IA.1: Robustness Tests

Figure IA.1: Competitive Intelligence Company Blog Discussing Job Posting Analysis

Figure IA.2: News Coverage of Revised Job Posting Specificity

Figure IA.3: Article Discussing the Competitive Advantages of Job Posting Anonymity

Figure IA.4: The Effect of the DTSA on Job Posting Anonymity in Event Time

Table IA.1: Robustness Tests

Table IA.2: The Effect of the DTSA on Job Posting Specificity, Controlling for Uncertainty

Table IA.3: The Effect of the DTSA on Job Posting Anonymity

Table IA.4: Alternative Specification, The Effect of the DTSA on Job Posting Specificity by Labor Market Tightness Group

IA.1 Robustness Tests

In Table IA.1, I present the findings of various robustness tests for the DTSA implementation results from Tables 3 and 4. I first report the baseline estimates from Column (2) of Tables 3 and 4. In the next four rows of Table IA.1, I test four alternative proxies for skills demand in job postings around the implementation of the DTSA. Note that the baseline design in Column (2) allows for changes in composition within each job group. To capture this change in composition more clearly, I utilize job-specific creativity and analytical scores from the Bureau of Labor Statistics (BLS).¹ These scores range from 0 to 100 and score occupations based on the work style of the typical job function. Using these scores as outcome variables, I show about a one point increase in these scores after the implementation of the DTSA in innovation jobs (relative to the change in non-innovation jobs), highlighting a change in composition to more creative and analytical jobs. Then, as a coarser alternative for the number of specialized skills, I instead use the presence of a degree requirement (college or above) as a measure of skills demand. The results indicate that innovation job postings are more likely to have a degree requirement relative to non-innovation job postings after the implementation of the DTSA, consistent with the number of specialized skills results in Table 3. Finally, I replace the specialized skills outcome variable with a more limited definition of skills demand that only considers cognitive skills, as these skills are likely to be inputs to innovative activities (Deming and Kahn, 2018). Although this more limited skills definition (especially for non-innovation job postings) likely leads to a noisy test, I document an increase in the inverse hyperbolic sine of cognitive skills demand in innovation jobs after the implementation of the DTSA.

I then test two alternative transformations of the word count variable for the job posting specificity results from Column (2) of Table 4. Note that I use the inverse hyperbolic sine transformation in Table 3, as the number of specialized skills in a job posting is sometimes zero. The inverse

¹For more details, see <https://www.onetonline.org/find/descriptor/browse/Work.Styles/>.

hyperbolic sine transformation approximates the natural log transformation (and has a similar interpretation) while also admitting zero without requiring the addition of a constant that affects the entire distribution of the underlying variable. In Table 4, however, this is unnecessary, as the word count in a job posting is never zero; a natural log transformation is feasible and functionally identical in distribution to the inverse hyperbolic sine transformation. Thus, when I replace the natural log of word count with the inverse hyperbolic sine of word count, results are almost literally unchanged. Another concern may be that outliers in the word count distribution introduce bias and/or estimation noise in my main estimates. Although the sample selection procedure described in Section 3 attempts to truncate these postings that are likely subject to classification issues, as a form of robustness, I also winsorize the natural log of word count within each job group and number-of-skills group at the 1st and 99th percentiles. Results are similar.

The next four tests focus on the job posting features controls that allow for the interpretation of the word count regressions as tests of job posting specificity. In my main tests in Table 4, I control for the interaction of the number of specialized skills in a job posting, the presence of a degree requirement, and the presence of an experience requirement. This definition admits 124 sets of posting features. I then interact these sets with job group. The inclusion of these controls helps ensure that I compare job postings with similar labor demand characteristics before and after the DTSA. Controlling for posting features, however, may introduce estimation bias. Specifically, as shown in Table 3, some of these posting features (i.e., Z) do change as a result of the DTSA (i.e., X), and Z likely affects the word count (i.e., Y) for mechanical and economic reasons. Including Z in a regression of Y on X does not inherently introduce a “bad control” problem if the effect of interest is a controlled direct effect, or CDE, as it is in my primary tests (Cinelli et al., 2021). Yet, if there exists an unobserved confounder U of Z and Y , there will be bias introduced in the CDE estimate due to the colliding path $X \rightarrow Z \leftarrow U \rightarrow Y$. In practice, this bias will only be

sizeable if my features definition (in concert with my other controls) does not produce an effective mapping between Z and Y . Although this is inherently an untestable bias concern, in Table IA.1, I introduce controls for additional features to somewhat sharpen the feature mapping into word count. First, “boilerplate” words are often determined at the employer level and could also vary over time as new employer-level hiring policies come into place. Although the existing inclusion of industry-time fixed effects should capture a large share of this variation, employer-time fixed effects would do so in a sharper fashion (unsurprisingly, doing so introduces more iterative singletons). The replacement of industry-time fixed effects with employer-time fixed effects yields similar results. Second, I interact the main posting feature controls with county and industry simultaneously. Such a design also leads to many iterative singletons, but note that results are generally similar. Third, there are other job posting features that are not in my job posting features definition: the presence of a certification requirement, salary inclusion, part-time status, and the number of baseline/character/cognitive/social/software skills. Adding all these features separately and flexibly interacted with job group leads to similar results. Fourth, in a much more stringent design, I flexibly interact the three main job posting features with time. These controls account for, say, any structural changes in word count purely explained by being any innovation or non-innovation job posting with a high number of specialized skills in a particular month. To a certain extent, this set of controls acts to subsume my treatment effect, as changes in elasticity between posting features and word count over time for high-skill innovation jobs is precisely one of the dimensions I intend to capture. Nonetheless, the inclusion of these very stringent controls still leads to a strongly negative coefficient estimate.

I then conduct four further robustness tests that address possible concerns with the results in both Table 3 and Table 4. First, there may be business cycle-driven factors in job posting content in the innovation job group that are not captured by time-invariant means by county-industry-job

group or over time by the non-innovation job group. Therefore, I introduce state-level cyclical controls within job group; results are similar. Second, larger firms (which naturally represent a sizeable portion of the sample because my analysis is at the job posting level) may face differential shocks to all of their job postings. Such shocks may bias the coefficient estimates. I introduce a set of firm size-industry-time fixed effects, and results are similar. Third, note that the job postings data represent a repeated cross-section, as firms are not hiring for the same positions or in the same locations every month. To more closely mimic a balanced panel structure, I limit the sample to county-industry-job groups with at least one job posting in each four-month period and replace year-month with those four-month periods in the time fixed effects. Results are similar. Fourth, my analyses throughout the paper rely on the assertion that posting features generally convey true labor demand. Although there is evidence that this is primarily the case ([Deming and Kahn, 2018](#)), certain scenarios may induce firms to overstate posting requirements. For example, when attempting to hire foreign workers through labor certification processes, firms may face disclosure requirements and often seek to establish that the particular worker they need is not available in the US. If my job-related fixed effects and posting feature controls do not account for these incentives adequately, my estimates of the effects of the DTSA on labor skills demand and job posting specificity may be biased. To address this concern, I drop the set of occupations that represent the majority of H-1B visa filings: computer-related occupations (i.e., four-digit SOC 15-11).² Results are essentially unchanged.

In the final three tests of Table [IA.1](#), I assess the sensitivity of my statistical inferences to alternative standard error clustering choices. In my main tests, I cluster standard errors two ways at the state-industry (965 clusters) and job (923 clusters) levels. These levels of clustering subsume the primary time-invariant fixed effects in the main tests while plausibly satisfying homogeneity

²PERM Labor Certification, Curran, Berger, & Kludt Immigration Law; M. Tia Johnson, *Characteristics of H-1B Specialty Occupation Workers*, U.S. Department of Homeland Security, March 17, 2016.

assumptions ([Conley et al., 2018](#)). Nonetheless, other clustering choices on dimensions of geography, industry, job type, and/or time are also reasonable. Statistical inferences are similar when clustering standard errors (i) two ways at the census division-industry-job group (342 clusters) and year-month (48 clusters) levels, (ii) one way at the industry-job group (38 clusters) level, and (iii) two ways at the census division-industry (171 clusters) and four-digit SOC (108 clusters) levels.

Figure IA.1: Competitive Intelligence Company Blog Discussing Job Posting Analysis

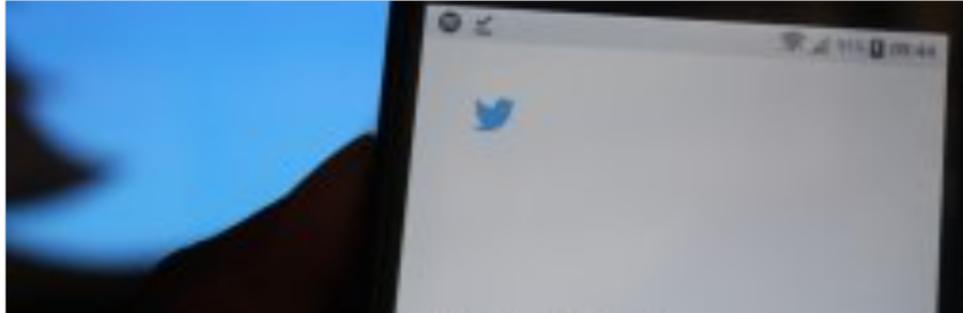
The screenshot shows the Crayon website's blog page. At the top, the Crayon logo is on the left, and navigation links for Product, Resources, Company, Blog, Get a Demo, Request Pricing, and Log In are on the right. Below the navigation is a link to 'BACK TO ALL POSTS'. The main heading is 'How to Decode Your Competitor's Strategy with Predictive Intelligence' by Ellie Mirman, dated Tue, Jan 2, 2018. The article text discusses annual plans and competitor analysis. A sidebar on the right contains a blue call-to-action box with the text 'Ready to compete like you mean it? Get a free analysis of your competitive landscape.' and a 'Get your intel' button. Below this is a subscription form with the text 'Get the latest CI intel in your inbox by subscribing to our blog!', an 'Enter your email' input field, and a 'Subscribe' button. At the bottom of the article content, there is a notification bar that says 'New open positions detected.' with a star icon.

This figure presents a partial excerpt from a blog post by a competitive intelligence company, Crayon, touting the importance of analyzing competitors' job postings on both extensive and intensive margins to understand changes in strategy and human capital investments. Source: Ellie Mirman, *How to Decode Your Competitor's Strategy with Predictive Intelligence*, Crayon, January 2, 2018.

Figure IA.2: News Coverage of Revised Job Posting Specificity

Twitter is apparently working on a subscription service

Posted on July 9, 2020 by Valentine Muhamba



Twitter seems to be working on some sort of subscription service model. A [job listing](#) on Twitter Careers is looking for a senior software engineer for a new project called Gryphon.

"We are a new team, codenamed Gryphon. We are building a subscription platform, one that can be reused by other teams in the future. This is a first for Twitter! Gryphon is a team of web engineers who are closely collaborating with the Payments team and the Twitter.com team. We are looking for a full-stack engineer to lead the Payment and Subscription client work."

Twitter Careers

Twitter, as anyone would expect, didn't reveal what it is exactly they were going to use this subscription service for. Their business makes money from ad revenue and data licensing. So to make the move to a subscription based service means we can speculate as to what they may intend to hide behind a paywall.

In an interesting twist, Twitter may have tried to cover their tracks. In a tweet by [Matthew Keys](#), they changed the listing to

"Work on a bevy of backend engineering teams to build components that allow for experimentation to deliver the best experience possible to all of our users"

Despite the cat and mouse game, word is already out, and we know that something is in the works.

This figure presents a partial excerpt from a news article—one of many articles—citing a job posting revision by Twitter in July 2020 (discussed further in Section 2). Twitter initially posted a job opening that gave specific details on the purpose and job function for which a software engineer was required. After some news coverage and a stock market response, Twitter revised the posting to make it less specific. Eventually, Twitter restored the original posting. Source: Valentine Muhamba, *Twitter is apparently working on a subscription service*, Techzim, July 9, 2020.

Figure IA.3: Article Discussing the Competitive Advantages of Job Posting Anonymity

FROM THE EDITOR IN CHIEF • SANDY REED

Stupid Web tricks: what you shouldn't post on your company's Web site

Something as simple as posting job openings on your Web site can reveal more than you should to your competitors. For instance, if your company has decided on a major initiative that requires technical expertise that you do not now have, you might decide that the fastest way to find candidates

is to post the jobs on your Web site.

However, the odds are that your competitors are among your Web visitors, and that the job postings will tip them off about your company's technical direction. A better strategy is to post the jobs on other sites and in printed publications without identifying your company as the employer.

Thinking before posting doesn't apply only to employment opportunities. As I noted last week, I got a

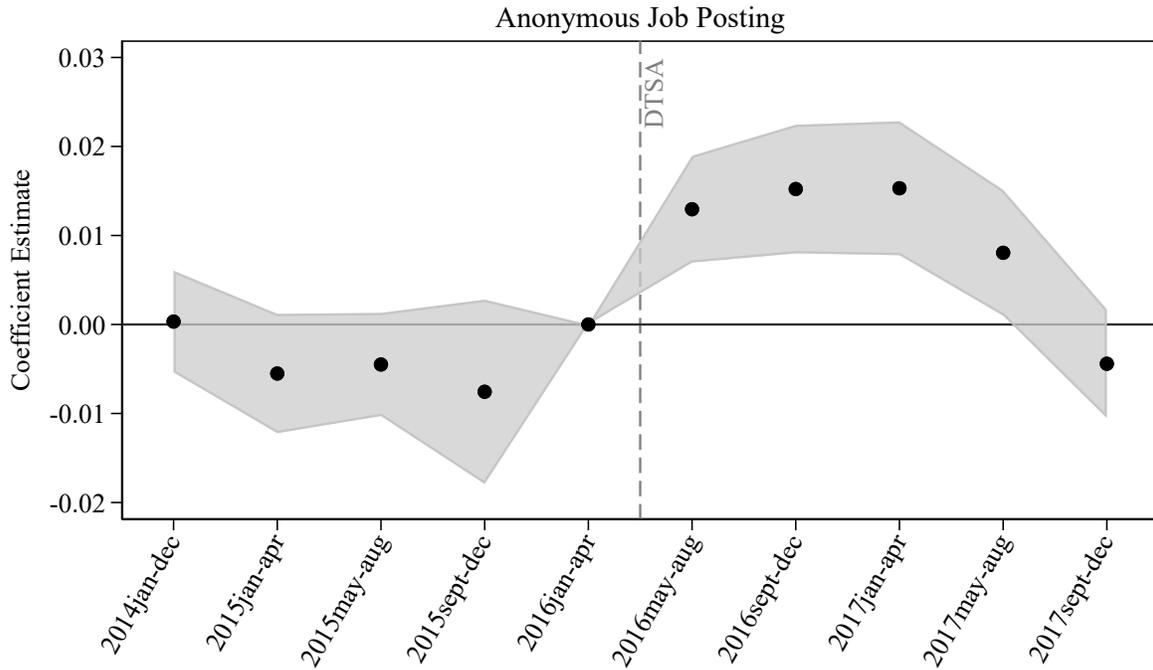
Protecting your company's crown jewels from competitive intelligence pros

What NOT to post on your Web site, by department

- ▶ Human resources departments
 - Detailed employment opportunities
 - Detailed descriptions of employee benefits
- ▶ Marketing/sales departments
 - Company literature that can be downloaded anonymously
 - Overspecific product specifications
- ▶ Other departments
 - Information on partners, suppliers, and licensees

This figure presents a partial excerpt from a news article in a magazine by *InfoWorld*, an information technology media company that caters content toward IT and business professionals. The editor-in-chief discusses the importance of job posting anonymity in order to protect proprietary information regarding innovative human capital investments and activities from competitors. The article also mentions the developing expertise of competitors in finding and acting upon this information. Source: Sandy Reed, *Stupid Web tricks: what you shouldn't post on your company's Web site*, *InfoWorld*, July 12, 1999.

Figure IA.4: The Effect of the DTSA on Job Posting Anonymity in Event Time



This figure plots the difference in job posting anonymity in innovation jobs between high-secrecy and low-secrecy industries over time. The outcome variable is an indicator equal to one if a job posting is anonymous. The model is from Column (4) in Table IA.3 and interacts *High Secrecy Industry* with 10 time indicators, the first indicator representing 2014 and those thereafter representing four-month periods. The period just before DTSA implementation serves as the relative-difference estimate (and therefore is zero with no confidence bands). The DTSA was signed into law in May 2016. Confidence bands, representing a 95% confidence interval, are based on standard errors clustered two ways at the state-industry and job levels. Variable definitions are provided in Appendix A.

Table IA.1: Robustness Tests

	N	Labor Skills Demand (Table 3)	Job Posting Specificity (Table 4)
<i>Baseline model:</i>			
- Column (2)	2,799,189	0.060*** (4.56)	-0.027*** (-4.39)
<i>Alternative proxies and specifications:</i>			
- Creativity score	2,769,191	0.830*** (2.62)	
- Analytical score	2,769,191	1.039*** (3.62)	
- Asks for college degree	2,799,189	0.042*** (4.30)	
- ihs(num. cognitive skills)	2,799,189	0.019* (1.87)	
- ihs(word count)	2,799,189		-0.027*** (-4.39)
- ln(word count), winsorized at 1st and 99th percentiles	2,799,189		-0.026*** (-4.34)
- Add employer “boilerplate” controls (employer×time fixed effects)	2,763,631		-0.026*** (-6.28)
- Add posting features×county×industry×job group fixed effects	2,576,527		-0.022*** (-4.38)
- Add other posting features×job group fixed effects	2,799,181		-0.025*** (-3.98)
- Add num. spec. skills, degree req., experience req.×time fixed effects	2,799,189		-0.018** (-2.41)
- Add state×job group×calendar month cyclical fixed effects	2,799,189	0.065*** (4.45)	-0.027*** (-4.18)
- Add firm size×industry×time fixed effects	2,798,808	0.059*** (4.60)	-0.028*** (-4.65)
- Keep balanced panel	2,330,218	0.064*** (4.07)	-0.027*** (-4.18)
- Drop high-non-US worker occupations (four-digit SOC 15-11)	2,427,494	0.064*** (4.89)	-0.027*** (-4.49)
<i>Alternative clusters (num. clusters in parentheses):</i>			
- division-industry-job group (342), year-month (48)	2,799,189	0.060*** (5.91)	-0.027*** (-4.56)
- industry-job group (38)	2,799,189	0.060*** (4.08)	-0.027*** (-4.34)
- division-industry (171), four-digit SOC (108)	2,799,189	0.060*** (4.32)	-0.027*** (-4.36)

This table reports sensitivity analyses for the results in Tables 3 and 4. If not stated differently, *t*-statistics, in parentheses, are based on standard errors clustered two ways at the state-industry and job levels (***:1%, **:5%, *:10%). “Creativity” and “Analytical” scores range from 0 to 100 and are provided by the Bureau of Labor Statistics (BLS). “Other posting features” include certification requirement, salary inclusion, part-time status, and number of baseline/character/cognitive/social/software skills, all separately and flexibly interacted with job group. The balanced panel is constructed by ensuring at least one job posting is observed within a county-industry-job group in every four-month period, and then year-month is replaced by four-month periods in the regressions. “Division” is the census division of the job posting.

Table IA.2: The Effect of the DTSA on Job Posting Specificity, Controlling for Uncertainty

$Y = \ln(\text{Word Count})$	(1) Main Job Specification, Uncertainty Subsample	(2) Add Uncertainty Controls
postDTSA*Innov. Group	-0.019*** (-2.59)	-0.018*** (-3.19)
$\delta \hat{\beta} = 0, R_{max} = 1$		0.782
$\delta \hat{\beta} = 0, R_{max} = 0.95$		0.906
$\delta \hat{\beta} = 0, R_{max} = 0.90$		1.078
$\delta \hat{\beta} = 0, R_{max} = 0.85$		1.329
$\delta \hat{\beta} = 0, R_{max} = 1.3\tilde{R} = 0.83$		1.492
<i>Fixed Effects:</i>		
Posting Features \times Job	Y	Y
County \times Job	Y	Y
Industry \times YearMonth	Y	
State \times YearMonth	Y	Y
Employer \times YearMonth		Y
c.Uncertainty \times Job Group		Y
New Geo. Hire \times Job Group		Y
Observations	752,620	752,620
Adjusted R^2	0.540	0.636

This table reports the results of the OLS regression from Section 4, Equation (4) in Column (1) (the same specification as that of Table 4, Column (4)), limited to the subsample of observations from 2015-2016 for which all necessary variables for the coefficient sensitivity test are available. Column (2) introduces (i) employer-time fixed effects (which subsume industry-time fixed effects), (ii) the interaction of job group with the continuous, time-varying, and employer-level measure of risk and uncertainty from Hassan et al. (2019), and (iii) the interaction of job group with an indicator for new geographic hires (since 2014). The outcome variable is the natural log of word count. t -statistics, in parentheses, are based on standard errors clustered two ways at the state-industry and job levels (***:1%, **:5%, *:10%).

As in Oster (2019), δ reflects the degree of selection on unobservables (relative to observables) that would be required to drive the treatment effect estimate to zero, conditional on a choice of what the “full” model R^2 would be if all unobservables were included in the estimation. δ is calculated as follows:

$$\delta = \frac{(\tilde{\beta} - \hat{\beta})(\tilde{R} - \hat{R})\hat{\sigma}_y^2\hat{\tau}_x + (\tilde{\beta} - \hat{\beta})\hat{\sigma}_x^2\hat{\tau}_x(\tilde{\beta} - \hat{\beta})^2 + 2((\tilde{\beta} - \hat{\beta}))^2 \left(\hat{\tau}_x(\hat{\beta} - \tilde{\beta})\hat{\sigma}_x^2 \right) + ((\tilde{\beta} - \hat{\beta}))^3 ((\hat{\tau}_x\hat{\sigma}_x^2 - \hat{\tau}_x^2))}{(R_{max} - \hat{R})\hat{\sigma}_y^2(\hat{\beta} - \tilde{\beta})\hat{\sigma}_x^2 + (\tilde{\beta} - \hat{\beta})(R_{max} - \hat{R})\hat{\sigma}_y^2(\hat{\sigma}_x^2 - \hat{\tau}_x) + ((\tilde{\beta} - \hat{\beta}))^2 \left(\hat{\tau}_x(\hat{\beta} - \tilde{\beta})\hat{\sigma}_x^2 \right) + ((\tilde{\beta} - \hat{\beta}))^3 ((\hat{\tau}_x\hat{\sigma}_x^2 - \hat{\tau}_x^2))}$$

Above, $\hat{\beta}$ and \hat{R} are the coefficient and R^2 from Column (1), $\tilde{\beta}$ and \tilde{R} are the coefficient and R^2 from Column (2), $\sigma_y^2 = 0.636$ is the sample variance of $\ln(\text{WordCount})$, $\sigma_x^2 = 0.034$ is the sample variance of $\text{postDTSA} * \text{Innov. Group}$ after residualizing by the controls in Column (1), $\hat{\tau}_x = 0.027$ is the sample variance of $\text{postDTSA} * \text{Innov. Group}$ after residualizing by the controls in Column (2), $\hat{\beta} = 0$ is the hypothesized “true” treatment effect, and R_{max} is the hypothesized R^2 of a regression of $\ln(\text{WordCount})$ on $\text{postDTSA} * \text{Innov. Group}$, observed controls, and unobserved controls. Measurement error in the outcome variable makes $R_{max} = 1$ unlikely. Therefore, I report δ for multiple candidate values of R_{max} , including $R_{max} = 1.3\tilde{R}$ as suggested by Oster (2019). For all calculations, I use the reported Adjusted R^2 values.

Table IA.3: The Effect of the DTSA on Job Posting Anonymity

$Y = \mathbb{1}(\text{Anon. Job Posting})$	Job Group Fixed Effects		Job Fixed Effects	
	(1) Basic Specification	(2) Main Job Group Specification	(3) Main Job Specification	(4) Add Job×Time Fixed Effects
postDTSA*High-Secrecy Industry	0.014*** (3.98)	0.013*** (4.23)	0.011*** (4.27)	0.012*** (4.44)
<i>Fixed Effects:</i>				
State × Industry × Job Group	Y	Y		
State × Industry × Job YearMonth	Y		Y	Y
Job × YearMonth				Y
State × YearMonth		Y	Y	Y
Observations	1,755,606	1,755,606	1,735,586	1,732,718
Adjusted R^2	0.042	0.049	0.148	0.156

This table reports the results of the following OLS regression in Column (4):

$$\mathbb{1}(\text{Anon.JobPosting})_{ikct} = \beta \text{PostDTSA}_t * \text{HighSecrecyIndustry}_d + \iota_{sdj} + \eta_{jt} + \delta_{st} + \epsilon_{ikct}.$$

Above, k denotes the employer making the posting i , c and s denote the county and state in which the job is located, t denotes the year-month of the posting, d denotes the three-digit NAICS code of the employer k , j denotes the eight-digit ONET code associated with job posting i , and *HighSecrecyIndustry* is equal to one for job postings in industries d that report a high importance of trade secrecy (three-digit NAICS 311, 324, 325, 334). The outcome variable is an indicator equal to one if a job posting is anonymous. The sample consists of manufacturing firms' job postings from 2014 to 2017 for innovation jobs (SOC codes 11 to 29, $J = 1$). t -statistics, in parentheses, are based on standard errors clustered two ways at the state-industry and job levels (***:1%, **:5%, *:10%). Variable definitions are provided in Appendix A.

Table IA.4: Alternative Specification, The Effect of the DTSA on Job Posting Specificity by Labor Market Tightness Group

$Y = \ln(\text{Word Count})$	(1) Main Job Specification, Add Time-To-Fill \times Time Fixed Effects	(2) Split on 2 Time-To-Fill Groups	(3) Split on 5 Time-To-Fill Groups
postDTSA*Innov. Group	-0.025*** (-6.59)		
postDTSA*Innov. Group*Time-To-Fill, Low		-0.033*** (-7.46)	
postDTSA*Innov. Group*Time-To-Fill, High		-0.018*** (-4.86)	
postDTSA*Innov. Group*Time-To-Fill, 1 (Low)			-0.033*** (-5.81)
postDTSA*Innov. Group*Time-To-Fill, 2			-0.033*** (-5.42)
postDTSA*Innov. Group*Time-To-Fill, 3			-0.027*** (-4.31)
postDTSA*Innov. Group*Time-To-Fill, 4			-0.017*** (-3.54)
postDTSA*Innov. Group*Time-To-Fill, 5 (High)			-0.019*** (-3.92)
$F\text{-test}(\beta_L \text{ v. } \beta_H), p\text{-value}$		0.000	0.018
<i>Fixed Effects:</i>			
Posting Features \times Job	Y	Y	Y
County \times Job	Y	Y	Y
Employer \times YearMonth	Y	Y	Y
Time-To-Fill Group \times YearMonth	Y	Y	Y
Observations	1,704,591	1,704,591	1,704,591
Adjusted R^2	0.626	0.626	0.626

This table reports the results of the following OLS regression in Column (2):

$$\ln(\text{WordCt})_{ikct} = \beta_L \text{PostDTSA}_t * \text{Innov.Group}_{J=1} * \text{LowTTF}_{cj}^{p=L} + \beta_H \text{PostDTSA}_t * \text{Innov.Group}_{J=1} * \text{HighTTF}_{cj}^{p=H} + \omega_{vj} + \iota_{cj} + \lambda_{kt} + \alpha_{pt} + \epsilon_{ikct}.$$

This alternative specification adds employer-time fixed effects. Above, v denotes a set of posting features (number of specialized skills \times has degree requirement \times has experience requirement) conveyed in job posting i , k denotes the employer making the posting, c and s denote the county and state in which the job is located, t denotes the year-month of the posting, d denotes the three-digit NAICS code of the employer k , j denotes the eight-digit ONET code associated with job posting i , and job group J indicates whether job j is an innovation job (SOC codes 11 to 29, $J = 1$) or a non-innovation job (other SOC codes, $J = 0$). The outcome variable is the natural log of word count. The time-to-fill (TTF) partition is determined by the average time to fill a job posting at the six-digit SOC level by MSA in 2015. The sample consists of manufacturing firms' job postings from 2014 to 2017 for which the employer name is available. t -statistics, in parentheses, are based on standard errors clustered two ways at the state-industry and job levels (***:1%, **:5%, *:10%). " F -test, p -value" reports the p -value of a difference-in-coefficients test. Variable definitions are provided in Appendix A.