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WE'VE GOT YOU COVERED:
FIRMS' POLITICAL STANCES ON ABORTION AND LABOR MARKET SORTING

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ABSTRACT

Amidst rising political polarization, firms engage more frequently with political issues through public statements and policies. This paper examines how firms' stances on polarizing issues impact worker sorting, leveraging announcements from hundreds of employers following the Supreme Court's ruling in *Dobbs v. Jackson*, overturning federal abortion rights. We introduce a methodology to uncover labor market competitors for each announcing firm based on job seekers' revealed preferences. While announcing firms received more applications from job seekers, particularly in Democratic-leaning states and female-dominated jobs where abortion was outlawed, current employees began searching for jobs elsewhere as employee satisfaction declined, particularly among male-dominated jobs. Smaller companies with less-established reputations experienced the largest effects. When deciding whether to take a sociopolitical stance, firms face a complicated trade-off: attracting culturally-aligned workers at the expense of alienating current ones.

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1 Introduction

Over fifty years ago, [Friedman \(1970\)](#) argued that a firm’s only social responsibility is to maximize profits. Under this widely held view, firms should avoid engaging with unrelated political causes. However, the United States has become increasingly politically polarized ([Gentzkow, 2016](#); [Gentzkow et al., 2019](#); [Bertrand and Kamenica, 2023](#)). Amidst this rise in polarization, firms have more frequently engaged with politically and socially controversial subject matter, including guns, LGBTQ issues, climate change, and racial equality ([Chatterji and Toffel, 2019](#); [Cassidy and Kempf, 2022](#)). This paper examines whether this political engagement impacts the firm’s workforce. If workers care little about a firm’s political stance relative to other aspects of the job, firms taking such political positions should have minimal impact on worker sorting. Alternatively, taking a stand might attract value-aligned workers while alienating those with opposing views, leading to politically-motivated sorting across firms.

We consider this question in the context of the June 24, 2022 *Dobbs v. Jackson* U.S. Supreme Court decision, which overturned *Roe v. Wade* and returned decisions over abortion access to individual U.S. states. In the wake of *Dobbs* and the immediate loss of access to abortion in so-called "trigger" states (i.e., Republican states with abortion bans triggered by the overturning of *Roe v. Wade*), some firms publicly announced policies offering additional financial support for their employees to access abortions and related care in other states. Given polarized perspectives on abortion ([Saad, 2023](#)) and labor-market sorting along gender and political lines ([Mas and Pallais, 2017](#); [Cortés and Pan, 2017](#); [Folke and Rickne, 2022](#); [Colonnelli et al., 2022b](#)), we explore whether these announcements induce sociopolitical sorting by current and prospective employees.

To answer this question, we collect a set of "announcing firms" and merge it with data on job seeker behavior from Indeed and employee satisfaction from Glassdoor. Indeed is the largest job search site in the United States and the world by traffic, with over 70 million unique monthly U.S. visitors. Glassdoor is a widely used website with over 50 million unique monthly visitors that aggregates employer reviews from current and former employees. These large-scale data sources (3 billion clicks on U.S. job postings and 6.5 million company reviews for the time period we

consider) allow us to study aspects of the labor market that are absent from administrative data and traditional labor-market surveys, such as granular job-search behavior and workers’ views on firm culture and management. One contribution of this paper is to demonstrate how these large-scale, high-frequency data from private companies can capture labor market dynamics, in this case, workers’ reactions to shocks in the political environment.

To clarify the labor-market trade-offs that can result from firms’ sociopolitical stances, we first introduce a conceptual framework in which current and prospective workers attain different levels of utility depending on how much workers value alignment with the firm’s stance. Firms internalize the potential labor-market sorting and profit implications and decide whether or not to announce. The framework offers empirical predictions about which firms announce and about the sorting behavior of current and prospective workers into and out of announcing firms. Consistent with our framework, firms announce a sociopolitical stance after *Dobbs* if their current and prospective workforces are more female and lean Democrat.

The ideal experiment to explore these potential labor market effects would compare outcomes of prospective and current employees at announcing firms with the same outcomes for a comparison set of firms that did not announce after *Dobbs* but between which job seekers would have been indifferent otherwise. We introduce a methodological approach that mimics this experiment by recovering, for each announcing firm, the most common set of non-announcing firms that workers also click on during an Indeed search session. This revealed preference approach produces a hands-off, data-driven set of firms workers view as close alternatives—sharing some similarities with studies that use realized employee mobility to define markets or rank firms ([Schmutte, 2014](#); [Sorkin, 2018](#); [Schubert et al., 2022](#); [Nimczik, 2023](#)).

These firms serve as the controls in a difference-in-differences (DiD) design. Our methodology differs from a traditional matching-on-observables approach by selecting comparison firms that workers view as similar based on observable and *unobservable* characteristics during job search. Nevertheless, our results are robust to alternative choices for comparison firms, e.g., non-announcing firms we would have predicted to have announced based on observables and firms that do not compete for the same applicants where spillovers are unlikely.

To understand if announcing attracted new employees, we examine whether prospective job seekers increasingly clicked on and applied to job postings of announcing firms. Before *Dobbs*, inbound clicks and applications for announcing firms trended similarly to those for non-announcing firms. After *Dobbs*, clicks on announcers’ postings increased 9% compared with similar firms that did not announce. This effect size is large, equivalent to the increase in clicks that would result from raising the posted wage by 13% based on our estimated elasticity of clicks to posted wages. In turn, applications increased 11%. The increase is concentrated primarily among vacancies that advertise female-dominated roles as well as those in non-trigger states, i.e., states where abortion was not automatically banned after *Dobbs* and where the Democratic vote share was 53% (compared with only 39% in trigger states). In trigger states, we observe a positive response only for vacancies advertising female-dominated jobs, suggesting individuals most directly affected by the ruling do respond to these announcements. The effects are particularly large for smaller firms, consistent with workers having less information about the firm’s sociopolitical slant before the announcement. These heterogeneous effects suggest announcing increased a firm’s ability to recruit gender- and politically-aligned workers.

In contrast, the impacts on current workers suggest firms face a trade-off between attracting new workers and retaining existing ones. Using the same DiD design, we find that announcing reproductive care in the wake of *Dobbs* reduced workers’ satisfaction with their firms, leading to a sudden and persistent 5% decline in ratings of senior management. This reduction is larger than declines observed following news of tax avoidance ([Lee et al., 2021](#)) or the public revelation of corporate misconduct ([Gadgil and Sockin, 2020](#)).¹ The free-response text workers write in their reviews provides evidence for what might be driving this decline: newfound political misalignment. For announcers’ reviews compared with those of non-announcers, the word “woke” occurs 209% more often in the ‘Cons’ section and “culture” 19% less often in the ‘Pros’ section—although we caution that these effects are relative to small baseline means. This decline in satisfaction is most pronounced among smaller firms, whose sociopolitical positions may be less established,

¹While our estimates following these announcements are larger in magnitude, we caveat that the samples are different. Whereas our sample of announcers spans the spectrum of firm size, [Lee et al. \(2021\)](#) examine S&P500 firms, and [Gadgil and Sockin \(2020\)](#) consider only firms that garner national media attention. Given that we observe larger declines among small firms, we caution against interpreting our results as an ordinal ranking of events.

and in part, reflects an increased likelihood that workers voice their views on the platform.

Consistent with the decline in job satisfaction, we observe *increased* turnover intentions among current employees in announcing firms. Compared with non-announcing firms, current employees of announcing firms send 17% more clicks and 12% more applications outbound toward other employers compared with current employees of non-announcing firms. Consistent with sorting along gender and political lines, these effects are larger for employees in trigger (highly Republican) states and weaker among female-dominated jobs in more Democratic-leaning states. Based on workers' resumes, we find evidence that these increased intentions of turnover translate into realized turnover: Among the set of employees we observe employed at the time of the *Dobbs* ruling, those at announcing firms are 3.7 percentage points less likely to still be with the firm one-and-a-half years later compared with those at non-announcers.

Last, we investigate whether these announcements were accompanied by a broader shift in recruitment behavior. We first consider changes in recruitment intensity by analyzing the number and type of vacancies a firm posts, as well as the composition of vacancies across jobs and states. Compared with non-announcers, we detect no shift along any of these dimensions. Next, we examine wages and find that announcing firms increase the wages they advertise relative to non-announcers, resulting in faster wage growth for new hires. We rule out, however, that the rise in posted wages causes the increased interest from prospective employees.

The story that emerges from our analysis is that firms taking sociopolitical stances facilitates sorting along political and gender dimensions, meaningfully altering labor market dynamics. Some existing (likely male) employees are more dissatisfied and exit, but this is offset by increased interest from outside job seekers, particularly women and seemingly co-partisan workers aligned with the firm's political stance. Taken together, the evidence demonstrates that firm-level sociopolitical announcements contribute to an increasingly polarized U.S. labor market.

These findings contribute to three research areas. First, research in economics, management, and political science increasingly recognizes the interplay between politics and the workplace (McConnell *et al.*, 2018; Burbano, 2021; Bondi *et al.*, 2023). Prior work shows managers prefer to hire co-partisan workers (Gift and Gift, 2015; Colonnelli *et al.*, 2022b), political alignment shapes

employment relationships and worker retention (Colonnelli *et al.*, 2022b), and firms can influence employees’ political activities (Babenko *et al.*, 2019). A closely related literature examines firms’ public stances on controversial social and political issues. Ottonello *et al.* (2024) find that on average 4% of corporate earnings calls discuss politics, and since 2020, there has been a sharp increase in sociopolitical stances from firms, while Barari (2024) shows sociopolitical speech is common on corporate brands’ social media outlets. In terms of impacts, Bhagwat *et al.* (2020) and Klostermann *et al.* (2021) document corporate sociopolitical activism generally causes negative investor reactions and consumer brand perceptions, respectively. Most closely related, Conway and Boxell (2023) find that politically aligned consumers are more likely to purchase from firms after sociopolitical announcements, whereas misaligned consumers reduce their consumption.

Rather than studying firm value, consumer responses, or manager choices, we focus on workers. Our findings indicate that workers perceive firms’ political stances as an integral part of workplace culture, with implications for sorting across firms (Guiso, Sapienza and Zingales, 2015). The relevance of firms’ sociopolitical signaling for labor-market sorting is heightened by the increasing polarization of American society (Gentzkow, 2016), firms’ growing use of political speech (Cassidy and Kempf, 2022), and rising political polarization among corporate leaders (Fos *et al.*, 2022). At the same time, workplaces have historically been less politically segregated than families or social networks (Gentzkow and Shapiro, 2011), making firm-level political signaling a potentially important new mechanism through which political polarization can propagate.

Our results also connect to work on private politics, which studies how firms engage in political and social issues outside formal electoral channels (Diermeier, 2017). This work emphasizes that firms’ political actions can shape stakeholder behavior and market outcomes even in the absence of regulation or government intervention. We show that private political actions by firms, specifically, public announcements taking a sociopolitical stance over a controversial issue, can have real consequences through labor-market sorting.

Second, many papers demonstrate that women sort into different firms than men based not only on wages (Card *et al.*, 2016; Cortés *et al.*, 2023) but also non-wage amenities such as work arrangements (Niederle and Vesterlund, 2007; Goldin and Katz, 2011; Mas and Pallais, 2017, 2020;

Cortés and Pan, 2017; Babcock *et al.*, 2017; Emanuel *et al.*, 2023; Bloom *et al.*, 2022), harassment risk (Folke and Rickne, 2022; Folke, Rickne, Tanaka and Tateishi, 2020; Adams-Prassl, Huttunen, Nix and Zhang, 2024), and female friendliness (Hotz *et al.*, 2017; Fluchtmann *et al.*, 2021; Liu *et al.*, 2022). We contribute to this literature by showing that firms announcing a politically charged, gender-focused policy differentially affects how men and women sort across firms, highlighting political signaling as a previously understudied firm attribute shaping gendered labor outcomes.

Last, many studies document large costs to women of denied access to abortion and the importance of contraction in determining female labor supply (Goldin and Katz, 2002; Bitler and Zavodny, 2002; Bailey, 2006; Ananat *et al.*, 2009; Myers, 2017; Lu and Slusky, 2019; Miller *et al.*, 2023; Gershoni and Low, 2021a,b; Zandberg, 2021; Gallen *et al.*, 2023; Londoño-Vélez and Saravia, 2025). A very recent literature documents unintended spillovers of *Dobbs*, e.g., public financing (Lu and Ye, 2023) and defensive contraceptive investments (Pennington and Venator, 2023). Our finding that there is a spike in job seeker interest among female-dominated jobs in states where abortions were immediately banned suggests some women may partly mitigate the loss of access to abortion care after *Dobbs* through labor market sorting. However, firms with fewer employees in trigger states were more likely to make such announcements, suggesting that firm-level policies do not substitute for state-level legal access.

2 *Dobbs v. Jackson Women’s Health Organization* and Firm Responses

To study the impacts of firms’ political statements on the workforce, we focus on firm responses to the *Dobbs v. Jackson* decision rendered by the U.S. Supreme Court on June 24, 2022.² This ruling had broad implications for women (and men) in the United States. “Trigger laws” tied to the *Dobbs* ruling immediately outlawed abortion in many states. We focus on firms’ reactions to this event for two reasons. First, immediately following the ruling, a wide swath of firms made a series of announcements that were politically controversial, given the highly polarizing nature of the *Dobbs* ruling. Second, aside from a draft of the opinion being publicly leaked beforehand

²A preview of the decision was leaked on May 2, 2022. Since our main analysis is quarterly, and the leak and decision occurred in the same quarter, the treatment timing includes both.

(but in the same calendar quarter), this ruling was largely unexpected—as *Roe v. Wade* was the current precedent for fifty years—engendering an unanticipated slate of politically-charged firm announcements that we use to study labor market sorting.

These announcements, examples of which are provided in Online Appendix [A](#), have proven contentious, given the strong sentiments surrounding abortion ([Saad, 2023](#)). As an example of their political divisiveness, the Conservative Political Action Coalition (CPAC) drafted a list of “woke companies” based on firms’ post-*Dobbs* policies. Incidentally, every firm on this list of ‘woke’ companies is in our database of firms that announced travel coverage for abortion-related care, even though we collected the list of announcers separately.³ Disagreements over post-*Dobbs* policies in the workplace extended into the public sector. U.S. Senator Tommy Tuberville (R-Alabama) dominated news headlines by blocking military promotions for over 250 service men and women over the Department of Defense’s policy of travel coverage for out-of-state abortions.⁴

After the ruling, several media outlets profiled U.S. workers’ views on the implications of the *Dobbs* ruling for their lives and their relationships with employers. One prototypical example is [Goldberg \(2022\)](#), from which we highlight a few vignettes. One woman urged her daughter to find an employer willing to cover abortion-related travel expenses, stating “It would be awesome for her to move to a state that offers it, or at least work for a company that says, ‘Hey, we’ll foot the bill.’” Another woman worried access to such jobs might be limited, saying “How many people truly have that opportunity, especially in states where the bans are in place?” Some workers also noticed when their employers did not announce they would cover such expenses. One woman stated, “I wish they would do something” and contrasted her employer with those that did by saying, “They cared enough that they would send you to go get the help and care you need.” Another woman was even helping her daughter find a job by starting their search with workplaces that would cover abortion-related travel, saying, “It shows they’re listening to workers.”

Together, these anecdotal accounts are consistent with such announcements (or lack thereof) potentially affecting both inbound recruitment and outbound turnover, creating an uncertain

³Though two companies in the CPAC list, OKCupid and YouTube, are not on our list, they are owned by companies in our list. See the CPAC list [here](#).

⁴See, for instance, this [NPR article](#).

tradeoff for firms. Andrea Hagelgans, director of social issues engagement at Edelman, summarizes this tension firms face: “This is something that companies are going to have to grapple with...There’s a risk around action, absolutely. But there’s also a risk around inaction if you can’t recruit people to work for your company and you’re losing talent to other companies” (Agovino, 2022). In this paper, we present the first large-scale evidence of how these announcements were received by existing and prospective workers, and whether they impacted the firm’s ability to retain and recruit workers.

3 Conceptual Framework

We develop a stylized model that yields predictions about which firms make announcements following *Dobbs* and the effects of these announcements on inbound recruitment and outbound turnover. For brevity, we summarize the structure and main implications here and refer the reader to Appendix B for the formal model, propositions, and derivations.

There are two types of workers, Blue and Red. Firms begin with an initial share of Blue workers θ_0 and operate in labor markets where the external share of Blue workers is λ_0 . These composition variables are taken as exogenous at the time when firms decide whether to make a sociopolitical and polarizing announcement. Firms choose between a Blue-coded stance (e.g., covering abortion-related travel expenses) and a Red-coded stance (e.g., remaining silent).

Workers derive (dis-)utility from (mis-)alignment with the firm’s stance, in addition to deriving utility from wages and idiosyncratic match quality. We allow alignment and misalignment effects to differ between current and prospective workers. Following the firm’s decision on its stance, workers have the opportunity to leave, thereby generating sorting across both recruitment and retention margins. After sorting occurs, production—which depends in part on the worker’s alignment with the firm’s stance—takes place.

The firm chooses its stance to maximize profits, weighing the effects of announcements on recruitment, retention, and productivity, given its internal composition θ_0 , the external market composition λ_0 , and the strength of workers’ alignment preferences.

The model delivers sharp predictions about worker sorting. A Blue-coded announcement

increases the likelihood that Blue workers stay or join while decreasing the likelihood that Red workers do so, with potentially different magnitudes for current and prospective workers. This asymmetry allows the model to rationalize patterns in which firms experience increased inbound interest from job seekers alongside increased outbound turnover among current workers.

Following a Blue-coded announcement, net recruitment increases if and only if the relative increase in Blue workers’ willingness to join exceeds the relative prevalence of Red workers in the external labor market. In other words, even though Red workers become less likely to join after a Blue-coded announcement, overall recruitment can increase if the external market is sufficiently Blue or if Blue workers’ alignment preferences are sufficiently strong. Symmetrically, turnover rises if and only if the relative increase in exit probabilities among misaligned incumbents outweighs the relative prevalence of aligned incumbents in the firm’s workforce.

Finally, the firm’s decision balances a tradeoff between scale and alignment-driven productivity. Announcements can expand or contract employment while simultaneously changing average productivity through sorting. The model—which can also be extended to incorporate hiring and turnover costs and market-level equilibria in announcement behavior—yields intuitive comparative statics linking announcements to workforce and market composition. Firms with higher initial shares of blue workers (θ_0) and those operating in more blue labor markets (λ_0) have stronger incentives to announce, as they are more likely to retain aligned current workers and attract aligned prospective workers. We also extend the model to include firm types and allow workers to be imperfectly informed about them. This helps explain why sorting effects may be greater for smaller and less well-known firms.

4 Data

Announcing Firms To compile a comprehensive set of firms that publicly announced they would cover the costs to obtain an out-of-state abortion, we draw on lists from two online sources. The first is [Leopard.fyi](#), a platform designed to help women sort across job opportunities by providing information on, for instance, company culture and compensation. Although the platform primarily targets female engineers and focuses on technology companies, the list of employers

spans multiple sectors. There are 444 firms included in this set. These firms either posted an announcement on the social network LinkedIn, made announcements shared through spokespeople, or posted directly on their websites. The second is [Rhia Ventures](#), whose stated mission is to “create a vibrant US market for sexual, reproductive, and maternal health that produces equitable outcomes for all.” This database includes 147 large firms (500+ employees) and 72 small firms (fewer than 500 employees). Finally, we add a handful of firms not present in either database but mentioned in news sources as announcing travel coverage for reproductive care.⁵

All firms in our sample appear to have announced within a narrow window immediately following the *Dobbs* decision. Upon revisiting the list of announcing firms, only a handful of additions were identified, suggesting little variation in treatment timing; accordingly, we omit the very few firms that announced later. Further, while we are certain every firm we designate as announcing did so, we may overlook firms that announced internally with no public documentation. Given that our empirical strategy assigns equal weight to each announcer, of which there are hundreds, the omission of any single firm is likely to be immaterial to our findings. Moreover, omitting a firm that announced, if anything, would likely bias against finding an effect. Two pieces of empirical evidence further limit this concern. First, to the extent that an omitted announcer could have been mistakenly assigned to a control firm, if we split the sample to include only half of the control firms, we find very similar results for each half (see Appendix E). Second, the effects are more pronounced for firms that made these announcements on social media (see Appendix F), suggesting that the public nature of the announcement matters.

While the ruling was leaked roughly one month in advance—raising the possibility of private deliberation or coordination—any such interactions are not observable in our data, and we do not study strategic announcement timing. The final database consists of 481 unique firms.

Indeed Data A key innovation of this paper is the use of rich, proprietary Indeed data on job searches and job postings, which allows us to observe granular search and application patterns

⁵We identified only six such firms: Johnson & Johnson, L’Oréal, TPG, and Walgreens ([Reuters article](#)) and Giant Eagle and the Cleveland Cavaliers ([Cleveland.com article](#)). We also looked for mentions of out-of-state abortion travel benefits in the text of job postings, but were not able to find any such cases.

among millions of individuals who browse millions of job postings. With the advent of online job sites, the internet has become the dominant method for U.S. workers to search for jobs. According to the Computer and Internet Use Supplement of the Current Population Survey, more than 72% of unemployed workers in November 2021 used the Internet to search for jobs.⁶ By studying job-seeker interactions with job postings for firms that announced a policy after the *Dobbs* decision alongside firms that did not, we can isolate individuals’ preferences over employment opportunities directly rather than attempting to make inferences through equilibrium outcomes such as realized hires or separations. In addition, since we can observe workers’ job transitions and firm tenure through the resumes they upload to Indeed, our work builds on the growing literature using data obtained through online platforms, including workers’ job histories, to understand factors influencing worker mobility (Schubert *et al.*, 2022; Amanzadeh *et al.*, 2024)..

Indeed is the largest job site in the United States and globally based on web traffic, with its U.S. site receiving approximately 70 million unique visitors each month.⁷ Estimates suggest Indeed reaches 93% of U.S. online job seekers. Additionally, both the job search and job posting data are collected frequently enough for us to narrow in on the quarters just before and after firms announced their policies. We observe the universe of Indeed’s job seekers and their search behavior. This includes every search that every job seeker who interacts with the Indeed website or mobile application makes and their interactions with every vacancy listed on the platform.

We use the Indeed data on job seeker search activity in three main ways. First, we use within-individual search behavior to construct a set of firms to compare with announcing firms. We detail this methodological innovation in Section 6. Second, using profile and resume data from Indeed users, we isolate the behavior of job seekers while employed at either announcing or non-announcing firms.⁸ This allows us to examine how the intensity of *outbound* job-search activity toward other firms, i.e., the number of outbound clicks and outbound application starts, evolves for announcers and non-announcers after *Dobbs*. Third, we can examine how the intensity of

⁶Authors’ calculations using data made available by Flood *et al.* (2020) through IPUMS.

⁷See the [Indeed website](#) for more information about the platform, as well as this [TechCrunch report](#) describing how in 2010, Indeed moved past Monster to become the largest job site in the United States.

⁸From worker resumes, we can observe their job histories, i.e., past firms, job titles, and locations. What we do not have is the resume for *every* employee of each firm at *every* point in time.

inbound job-search activity, i.e., the number of inbound clicks and inbound application starts, evolves for announcers' and non-announcers' job openings. A click on a job posting reveals the full job description and enables the job seeker to start an application or continue to the employer's website.⁹ We caution that some workers will not apply for jobs directly through Indeed, perhaps applying through another online platform, and some postings direct workers to submit their applications elsewhere, motivating us to consider both clicks and application starts.

Glassdoor Data Glassdoor is an online platform that provides prospective employees with information on employers. It has more than 50 million unique visitors per month. The website primarily consists of information voluntarily provided by visitors through a 'give-to-get' mechanism, whereby visitors gain access to information others have provided after contributing themselves. To satisfy the 'give-to-get' requirement, a user "must submit a piece of content based on their personal experience at a company."¹⁰ Access expires after twelve months, which can motivate respondents to contribute more than once.

Glassdoor offers a unique window into the views of personnel at firms and their establishments. The data are employee-employer matches, updated in real time, and cover a wide array of U.S. private-sector firms before and after the *Dobbs* decision. Based on each employer's size as recorded in the Glassdoor database, we estimate that, on average, 4% of employees at the firms in our sample contribute an employer review each year. Negative reviews on Glassdoor can impede firm recruitment, as they are often listed as a major 'red flag' for prospective employees.¹¹ A survey of 4,600 workers found that almost half used Glassdoor as part of their job search.¹²

We focus on employer reviews, as they capture workers' sentiment towards their employers.

⁹Jobs are presented as a queue. Although we cannot account for queue order (since it is not recorded), we do not believe it is material for our results. For one, firms are given equal weight in our regressions, implying that queue order would need to have meaningfully shifted for many announcers, which seems unlikely. Second, as firms did not make these announcements on Indeed, there is little reason to believe the queue order on Indeed was systematically altered by content elsewhere on the internet.

¹⁰A 'piece of content' can consist of either: "Leaving a company review based on personal experience working at a company...Writing a salary for a position you currently or previously held...Reviewing the benefits at your current or previous employer...Leaving an interview review for a company you've interviewed with." For additional details regarding the give-to-get policy, see https://help.glassdoor.com/s/article/Give-to-get-policy?language=en_US.

¹¹For example, see this [Fortune](#) article.

¹²See this [Cornerstone](#) article.

When submitting an employer review, a current or former employee will include free-response descriptions of the ‘pros’ and ‘cons’ of their jobs, along with 1–5 stars Likert scale ratings for job satisfaction overall and for satisfaction with five sub-categories (career opportunities, compensation, culture, management, and work-life balance). Respondents disclose their firms and may include their job titles and locations. Demographic information, such as gender, is missing for most workers.¹³ For summary statistics at the review and firm level, see Appendix Table [H.1](#).

Limitations The main limitation of our data is that they reflect the Indeed and Glassdoor user bases. As described above, there are 70 million unique monthly visitors to Indeed, reaching 93% of U.S. job seekers who search online. Still, we do not observe the universe of job-seeker activity. For instance, we will miss workers who apply only in person or at another job site.

Turning to Glassdoor, because we observe sentiment only from workers who provide employer reviews, we will not capture the sentiments of all workers in the firm. Reassuringly, [Sockin \(2022\)](#) shows that satisfaction ratings and free-response text from Glassdoor reviews correlate strongly with measures observed in smaller representative surveys of workers, including the National Longitudinal Survey of Youth 1997 and the American Working Conditions Survey. Further, [Graham et al. \(2022\)](#) show that Glassdoor ratings of culture and review text capture survey-level differences in culture across firms observed in a survey of North American executives, and ([Green et al., 2019](#)) find Glassdoor reviews predict stock returns, suggesting they contain truthful information about the company. That said, negative reviews may be omitted due to concerns about employer retaliation ([Sockin and Sojourner, 2023](#)), and positive reviews may be fabricated to promote firms ([Fuller and Winkler, 2019](#)). We recognize these potential biases and implement several sensitivity tests—such as estimating a within-worker specification and excluding suspect reviews—to ensure neither these issues nor selection into who reports after *Dobbs* drives our findings. For a detailed discussion and array of robustness checks ruling out alternative explanations for our Glassdoor results, see Appendix [E](#).

¹³We only observe gender for employees who voluntarily provide it with a pay report or include it when creating a user profile on Glassdoor. For employer reviews, we observe gender for approximately 25% of respondents.

5 Which Firms Announce?

If firms announce to advance strategic recruitment and retention goals, then, as in our conceptual framework, the composition of their current and prospective workforces should predict whether they announce. Given the salience of the *Dobbs* ruling for women and the intense political polarization surrounding abortion, we focus on workforce gender composition and political leaning.

We measure workforce gender composition using two complementary proxies. First, to capture the gender composition of *current* employees, we construct the share female based on the fraction of Glassdoor employer reviews and pay reports submitted by women between 2019 and June 2022.¹⁴ Second, to capture the gender composition of *prospective* employees, we use Quarterly Workforce Indicators (QWI) data to calculate the share of college graduates who are female within each four-digit NAICS industry, following [Liu et al. \(2022\)](#).¹⁵

To measure political alignment, we use Federal Election Commission (FEC) data that details political contributions made by firm CEOs and employees. We restrict attention to donations made to Democrat or Republican candidates in 2020-2021, ensuring all contributions predate *Dobbs*. Each donation records the contributor’s name, employer, and job title. We identify CEO political affiliations by focusing on individuals where the job title mentions ‘CEO’ or ‘Chief Executive Officer.’ Since employers are not uniformly recorded, we textually match employers in FEC data to employers in Glassdoor. Then, for each CEO-employer pair, we calculate the share of donations to Democratic candidates. If multiple individuals list themselves as the CEO of an employer, we retain the most recent entry to capture the most recent CEO before *Dobbs*.¹⁶ We are able to assign CEO political leaning to about 4,000 firms. Because CEO ideology may not reflect the political preferences of the firm’s workforce more broadly, we separately measure employee political alignment using the fraction of the firm’s non-CEO employees who donated to Democrats in the FEC data.

¹⁴[Sockin and Sockin \(2019\)](#) find a correlation of 0.95 in female employment share between industry-occupation pairs with the American Community Survey and differences between the two are normally distributed around zero.

¹⁵The takeaway is identical if we instead consider the female share of non-college-educated workers.

¹⁶To verify these individuals are indeed CEOs of these companies, we match them to Glassdoor’s database of employer information from January 2022 and keep only CEOs whose last names align in the two datasets.

Table 1 reports estimates relating announcement decisions to the sociopolitical characteristics of current workers using Glassdoor data, and of prospective workers using Compustat data. Column (1) shows that firms with female CEOs were more likely to announce reproductive care coverage than firms with male CEOs. Column (2) further shows that firms with a higher share of female employees were significantly more likely to announce, even after controlling for CEO gender.¹⁷ Turning to political alignment, column (3) shows that Democratic-leaning CEOs were significantly more likely to announce coverage for reproductive care.¹⁸ This finding is consistent with other work documenting differences between Democrat and Republican CEOs, e.g., Democratic CEOs increasing female representation and closing gender gaps in the executive suite (Cohen *et al.*, 2021) as well as spending more on corporate social responsibility (Di Giuli and Kostovetsky, 2014). Column (4) shows a similar pattern for employees: Firms with more Democrat-leaning workforces were more likely to announce, suggesting the political preferences of rank-and-file workers also contributed to firms' decisions.

Although the announced coverage would constitute a meaningful increase in pecuniary compensation for those who take up the benefit, we anticipate few, if any, employees take it up.¹⁹ Not only would it not apply to workers in non-trigger states, but employees who find themselves with an unwanted pregnancy in a trigger state may hesitate to take up the benefit if they do not wish to share such sensitive information with an employer, even though several firms did state their intent to protect the privacy of employees.²⁰ For these reasons, while we might expect women in trigger states to be most affected, it is possible that these announcements mostly operate as a broader signal of company culture rather than an announcement of a fringe benefit.

Considering geographic exposure to *Dobbs* and whether a firm announced in Table 1 supports this line of reasoning. Using Glassdoor data, we first construct an indicator for whether a firm employs any workers in trigger states. Column (5) shows that firms with at least one employee

¹⁷Appendix Table D.3 shows that while female representation on corporate boards strongly predicts announcing, the association with female CEOs is not crowded out by including the female share of non-CEO board members.

¹⁸We find a similar pattern for firms in Compustat whose CEOs are listed in Execucomp (Appendix Table D.4).

¹⁹In Online Appendix C, we estimate that the dollar amount firms promised in maximum coverage for travel expenses was, on average, about \$4,500, or 5% of the average wage.

²⁰For instance, this CNN article mentions the examples of Match.com and Yelp: "Match Group's reproductive benefits are structured through third parties to ensure privacy and confidentiality for employees...Yelp will never receive any information on who incurred a claim and/or received reimbursement."

Table 1: Predicting Whether a Firm Announces Reproductive Care

	1(Firm Announced Reproductive Care)							
	Glassdoor						Compustat	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Female CEO)	0.004*** (0.001)	0.003*** (0.001)						
Female employment share		0.023*** (0.005)						
Share of CEO donations to Democratic party			0.037*** (0.005)	0.025*** (0.006)				
Share Democrat employees				0.046** (0.022)				
1(Operates in trigger state)					0.001*** (0.000)	0.003*** (0.000)		
Share employment in trigger states						-0.004*** (0.000)		
Female share of college graduates in sector							0.154** (0.067)	
1(HQ in trigger state)								-0.028*** (0.011)
Mean DV	0.006	0.006	0.025	0.025	0.001	0.001	0.026	0.029
Firms	53,201	53,201	2,465	2,465	426,136	426,136	5,481	5,263

Notes: Table 1 reports the relationship between a firm announcing reproductive care and the gender and political composition of its workforce and the sector in which it operates. Estimates reflect marginal effects from logit specifications. For columns (1)-(2), the sample is restricted to firms with at least 10 workers on Glassdoor. CEO gender (imputed based on first name), female employment share, whether operates in a trigger state, and share employment in trigger states are derived from the Glassdoor database. In columns (3)-(4), donations to Democrat candidates is based on Federal Election Commission (FEC) data. For column (7), female share of college graduates is calculated within 4-digit North American Industry Classification System (NAICS) sectors from Quarterly Workforce Indicators (QWI) data. Each specification controls for the logarithm of firm size. Glassdoor specifications include an indicator for whether the firm is publicly traded and industry fixed effects. Compustat specifications include two-digit NAICS sector fixed effects. Standard errors are clustered by industry for Glassdoor (see Appendix Table D.1) and by two-digit NAICS sector for Compustat. Significance levels: * 10%, ** 5%, *** 1%.

in a trigger state were more likely to announce, consistent with responding to workers directly affected by the ruling. However, column (6) shows that firms with a larger share of employees located in trigger states were *less* likely to announce. Together, these patterns suggest that announcements were less about providing a narrowly targeted fringe benefit and more about signaling firm culture in ways that reflect and cater to the political alignment of the majority of current workers.

Finally, we turn to prospective workers using Compustat data. With regards to gender, we show in column (7) that firms operating in industries with a higher share of female talent were more likely to announce. Turning to politics, we find in column (8) that firms headquartered in trigger states, where prospective workers are more likely to be politically misaligned with

an announcement, were significantly less likely to announce. This implies that firms were more likely to announce when operating primarily in Democratic-leaning markets. Again, we interpret these patterns as suggestive evidence that these announcements were less about providing a fringe benefit and more about establishing the firm’s culture surrounding gender and politics.

Consistent with our theoretical framework, these results suggest that whether a firm announced coverage for out-of-state abortions was a function of both the firm’s existing employee composition and the composition of its broader labor market. Firms were increasingly likely to offer reproductive care the more that women and Democrat-leaning workers were represented among current and prospective employees. These announcements may thus have been the product of strategic recruitment and retention goals — the success of which we test below.

6 Identifying Comparison Firms Using Worker Revealed Preferences

To determine the impact these announcements had on retention and recruitment requires a valid set of non-announcing firms to compare with announcing firms in a standard event study framework. A coarse approach would use every firm that abstained from announcing. However, such an approach is both computationally demanding, would overlook the heterogeneous nature of announcers, and fails to obtain a set of firms that face similar labor market shocks.

6.1 Obtaining a Set of Comparison Firms for Each Announcer

We propose a methodological approach to identify, from the perspective of prospective employees, the most relevant comparison firms. Intuitively, we use internal Indeed search data to identify, for each announcing firm, its closest labor-market competitors among all firms that did not make a post-*Dobbs* announcement. Accordingly, each announcing firm is assigned a separate set of comparison firms that workers view as close substitutes.

Formally, we take the universe of Indeed user search sessions over a set time period. For computational tractability, we limit the dates to the last date of each month from January 2022 to April 2022. Operationally, we narrow in on the first 30-minute window of each job seeker’s search activity, capturing over 13 million unique sessions. Within this universe of search sessions, we

identify users who clicked on a job posting for an announcing firm.²¹ For these users, we observe all other job postings they clicked on during their search sessions using their unique IP address. This identifies all other firms in which the worker showed interest. Aggregating across users, we then have an ordinal ranking of competitor firms for each announcer based on a key common feature: job seekers’ interest in where else they wish to work. For tractability, we retain only the 20 closest competitors as our baseline. However, our results are robust to using the top 15, top 10, or top 5 nearest competitors, or to applying inverse weights to control firms, which place greater emphasis on more closely related control firms (Appendix E). This novel approach allows us to observe which companies job seekers interact with organically, without imposing our own a priori restrictions on which firms we believe compete with one another.

By leveraging realized search behavior, this “revealed preference” approach offers a hands-off, data-driven procedure for identifying a plausible subset of comparison firms. Whatever aspects of a treated unit produce closeness with non-treated units—whether they are readily (e.g., industry and occupation) or difficult (e.g., social networks) to observe—will be implicitly captured by these realized choices. For comparison, propensity score matching or coarsened exact matching can match only treatment and control observations based on preselected observables. Further, while synthetic control methods allow researchers to match treatment and control observations on unobservables by matching on the outcome of interest before the treatment (e.g., [Acemoglu et al., 2016](#)), in this setup there is no inherent relationship between the control and treatment beyond aligning on aggregate outcomes. Under our approach, each firm in a treatment firm’s control set has economic meaning: the two are direct labor-market competitors.

Our approach allows researchers to *dynamically* identify labor market competitors. In this setting, the control firms provide a snapshot of each firm’s competitors immediately prior to the *Dobbs* ruling. More broadly, the approach can be used to study how a firm’s competitive set evolves over time, recognizing that each firm’s labor-market competitors are likely to fluctuate, especially for smaller firms. One inherent drawback of this procedure is that it requires both announcing and non-announcing firms to be actively hiring. If an announcing or non-announcing

²¹Given the nature of this procedure, we consider only sessions in which a worker clicked on a job ad for at most one announcer, so we are able to assign each session to a single announcing firm.

firm does not have an active job posting during the dates used to capture job seeker sessions, then that firm will necessarily be omitted. Of the 487 announcing firms, we can match 452 to Glassdoor data and derive a comparison set for 373 using job seeker activity on Indeed. That we cannot include all announcing firms further motivates assigning equal weight in the difference-in-differences equations to each announcer and its respective comparison set; this way, the omission of any one employer should not materially impact our results.²²

6.2 Examples of Labor Market Competitors

The full list of all firms and their corresponding matches under this approach consists of many thousands of firms. To demonstrate how this process works in practice, consider the following examples of announcing firms and their associated control firms that compete over labor; competing firms are listed in descending order according to their ranking.

Alaska Airlines — American Airlines, Delta, United Airlines, Southwest Airlines, United States Postal Service, Hawaiian Airlines, Whole Foods Market, Frontier Airlines, Boeing, Transportation Security Administration, Avelo Airlines, City of Seattle, Pearson, SkyWest Airlines, Spirit Airlines, Swissport, Enterprise Holdings, Republic Airways, Labcorp, University of Washington.

Starbucks — McDonald's, ALDI, Barnes & Noble, Whole Foods Market, Applebee's, Bath & Body Works, Chipotle, Planet Fitness, Chick-fil-A, Five Below, Panera Bread, Dunkin', Sally Beauty, Big Lots, United States Postal Service, PetSmart, GameStop, Burlington Stores, Michaels.

Bank of America — Truist Financial, Morgan Stanley, U.S. Bank, PNC Financial Services Group, State Farm Mutual, UBS, Obsidian Financial Services, Ally Financial, United States Postal Service, VMware, Fifth Third Bank, Charles Schwab, Northwestern Mutual, Liberty Mutual Insurance, Atrium Health, Navy Federal Credit Union, KeyBank, TIAA, Novant Health, M&T Bank.

Evidently, a naive classification of competitors for labor based on only industry classification or occupational title would not reproduce these sets. While American Airlines, United Airlines, and many other airlines are labor-market competitors with Alaska Airlines (and share an indus-

²²A plausible alternative would be to weight by firm size to obtain worker-level estimates; however, this would have neglected the effects, if any, on small firms, and muddled the interpretation of our treatment effects for the marginal announcing firm.

try classification), there are additional ex post obvious competitors based on other dimensions. For example, location clearly plays a role in who competes for labor with Alaska Airways, which is headquartered in Seattle, with both the University of Washington and the City of Seattle appearing in this list of top 20 competitors using our methodology. The same is true for Starbucks, which has many fast-food service chains but also a number of retail stores.

6.3 Comparison between Announcers and Matched Non-Announcers

Our methodology identifies which firms compete for the same personnel and systematically categorizes competitors as closer versus more distant in labor markets across the near-universe of U.S. firms. Table 2 summarizes the extent to which conventional markers of labor market competition manifest in practice by reporting, under various thresholds for determining closeness in competition, the share of control firms that share an industry, occupation, or location with their matched announcing firm.

Table 2: Degree of Similarity Between Announcing Firms and Their Labor Market Competitors

Outcome	Threshold for control employers				
	Top 20	Top 15	Top 10	Top 5	Random
	(1)	(2)	(3)	(4)	(5)
Firm has same industry	30.0%	38.0%	42.1%	49.2%	11.2%
Job postings have same occupation	22.8%	22.6%	22.9%	22.7%	7.3%
Job postings have same county	59.8%	59.7%	60.3%	60.0%	0.7%

Notes: Table 2 reports the percent of overlap for key observable characteristics between the announcing firms and the control firms obtained from the revealed-preference procedure. We consider four different thresholds based on rankings of closeness: the top 20 competitors, top 15, top 10, and top 5. The share of postings for control employers that were a match with that of the announcing firm by county or by occupation is derived using data from all job seeker accounts that had click activity on January 30th, 2022, who clicked on a single announcer and at least one control employer. Only unique combinations of control company and county or occupation were considered.

We find approximately 30% of a firm’s 20 closest competitors operate in the same industry, although that fraction rises to about 50% when we narrow in on the five closest competitors. For occupation (of which there are 50 broad categories), we observe a much smaller overlap of 23%, highlighting that workers do not primarily search for jobs along occupational lines. This emphasizes the relevance of outside-occupation options (Schubert *et al.*, 2022). Consistent with previous

work suggesting workers tend to search locally (Manning and Petrongolo, 2017; Marinescu and Rathelot, 2018; Sprung-Keyser *et al.*, 2022), the largest overlap we observe between announcers and competitors is hiring in the same highly granular location: 60% of competitors’ clicked-on job postings share the same county as the announcer’s clicked-on job posting. For comparison, in column (5) of Table 2, we calculate the probability that announcers and competitors randomly overlap along each dimension. Given the comparatively low rates of random overlap, it is clear that workers direct their search along each dimension, especially geography.

7 Effects of Firms Announcing Political Stances on Current Workers

Current Employee Satisfaction We first investigate how employees’ perceptions of their workplaces changed when their firms announced reproductive care after *Dobbs*. We focus on the 1–5 star Likert scale Glassdoor rating workers provide about firm management. We also consider ratings about firm culture with similar takeaways (Appendix Figure H.1). Figure 1 Panel (a) reports the raw average rating among announcers. We observe an improvement from the start of 2019 until 2021. After that, reviews remain steady and then fall sharply upon announcing and decline thereafter. The timing of the announcement is indicated by the dashed vertical line.

However, this drop may not be caused by the announcement. Other factors may explain the sudden decline in ratings from the second half of 2022 onward. To uncover the causal impact on current employees, we estimate a dynamic differences-in-differences (DiD) research design comparing the evolution of ratings at announcing firms with ratings among the set of comparison firms described in Section 6. Formally, let $\mathbb{A}$ represent the set of announcing firms. We estimate:

$$Y_{i,k,t,a} = \sum_{\tau \neq -1} \beta_{\tau} \mathbb{1}\{k \in \mathbb{A}\} + \gamma X_{i,k,t,a} + \lambda_{k,a} + \lambda_{j(i,k,t),t,a} + \lambda_{s(i,k,t),t,a} + \varepsilon_{i,k,t,a}, \quad (1)$$

where $Y_{i,k,t,a}$ represents the satisfaction rating for worker i employed with firm k in calendar year-quarter t for the grouping a of an announcing firm and its 20 matched comparison firms. The benchmark specification includes fixed effects for the firm $\lambda_{k,a}$, for job titles $j(i, k, t)$ over time $\lambda_{j(i,k,t),t,a}$, and for U.S. states $s(i, k, t)$ over time $\lambda_{s(i,k,t),t,a}$. We also include an indicator for

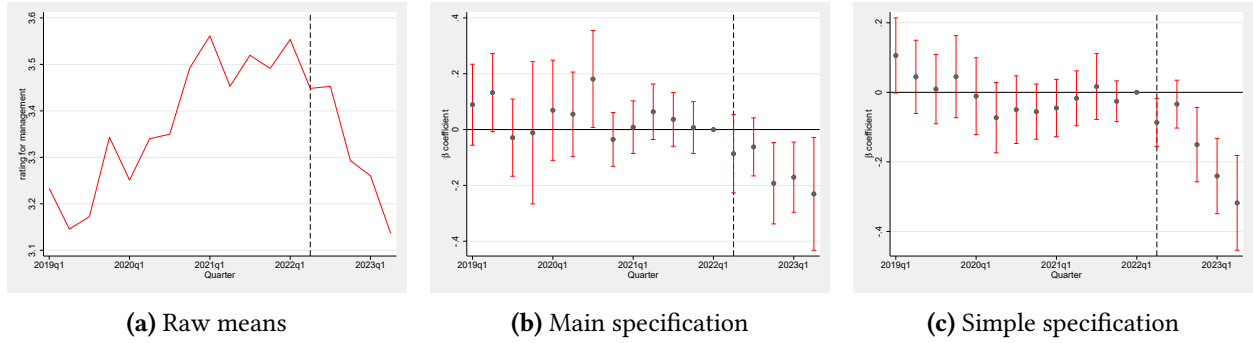
whether the individual is a current employee at the firm when the review is written, $X_{i,k,t,a}$. Since the treatment is assigned at the firm level, we cluster standard errors by grouping a and firm k in all specifications (Abadie *et al.*, 2022). All announcements occurred in the same time period after the ruling, so our research design avoids concerns over staggered timing of treatment.

Figure 1 Panel (b) reports event study estimates from equation (1). We observe no clear pre-trend before the announcement, suggesting that our method for identifying comparison firms captures a reasonable counterfactual. While the absence of pre-trends provides the clearest evidence in favor of our estimation strategy’s ability to recover causal effects, we note two additional features that enable us to recover average treatment effects on the treated (ATT). First, while firms select into announcing, our conceptual framework models this selection as based on time-invariant factors, which still allows our estimates to be interpreted causally (Ghanem *et al.*, 2022).²³ Second, while firms endogenously choose to announce, individual workers do not choose whether their employer announces. For current employees, the treatment is thus working at a firm that announces reproductive coverage following *Dobbs*. Conditional on observables that predict announcing behavior, announcing was plausibly unexpected from workers’ perspectives, as such statements would not have arisen prior to the ruling.

Directly after the firms’ announcements, we observe a sharp and statistically significant decline in satisfaction with management that persists throughout the post-announcement period. As shown in Panel (c) of Figure 1, we observe the same dynamics under a simpler version of equation (1) that leaves out the high-dimensional job title-time and state-time fixed effects.

We report the DiD results for each Glassdoor rating category under the benchmark specification in Panel A of Table 3. The results across all five sub-categories tell a consistent story: sentiment within the firm declines following these announcements. The most salient effects are observed among career opportunities, culture, and management, with average ratings for announcing firms falling 0.13–0.17 stars compared with their non-announcing counterparts. Relative to the sample means, these effects are non-trivial, translating to 3-5% declines in satisfaction. Compared with estimates from the literature, these effects are larger than declines following news

²³What would violate the parallel trends assumption is if the firms that chose to announce had private information about firm-specific shocks to recruitment or retention that would have occurred even if they remained silent.

Figure 1: Raw Means and Event Studies of Firms' Announcements on Management Ratings

Notes: Figure 1 panel (a) plots the mean Glassdoor management rating for announcers each year-quarter over the sample window, equally weighting each announcer. The figure also reports the estimated impact from equation (1) on management ratings under the benchmark specification in panel (b) and the simple specification in panel (c). Panel (b) includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Panel (c) includes event-firm and event-quarter fixed effects. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-value for the test of joint significance for all pre-*Dobbs* coefficients is 0.204 for panel (b) and 0.108 for panel (c).

Table 3: Effect of Firms' Announcements on the Five Rating Categories

	Career opportunities (1)	Compensation & benefits (2)	Culture & values (3)	Senior management (4)	Work-life balance (5)
Panel A: Main Specification					
After announcement	-0.127*** (0.029)	-0.025 (0.026)	-0.131*** (0.031)	-0.174*** (0.034)	-0.065** (0.027)
Observations	4,918,142	4,899,323	4,862,322	4,819,454	4,870,004
Panel B: Simple Specification					
After announcement	-0.129*** (0.025)	-0.028 (0.020)	-0.105*** (0.026)	-0.161*** (0.030)	-0.059*** (0.020)
Observations	7,568,412	7,546,954	7,501,307	7,449,140	7,512,880

Notes: Table 3 reports DiD estimates from equation (1) capturing the change in Glassdoor ratings between announcing and non-announcing firms after *Dobbs*. Panel A includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Panel B includes event-firm and event-quarter fixed effects. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

of tax avoidance (Lee *et al.*, 2021) and the public revelation of corporate misconduct (Gadgil and Sockin, 2020), yet smaller than those observed following an Accounting and Auditing Enforcement Release declaring financial misconduct (Zhou and Makridis, 2021).

Given that firm leadership orchestrated these announcements, which speak to the firm's cultural fabric, it is not surprising that ratings for culture and management respond most strongly. That career opportunities respond similarly negatively suggests that workers may perceive their

futures with the firm, or the firm’s future prospects, as worsening after these announcements; perhaps politically misaligned workers feel ostracized and at increased risk of being forced out or overlooked for promotions.²⁴ Sentiment over work-life balance deteriorates slightly as well, suggesting potential spillovers into other aspects of work (e.g., a disgruntled worker indiscriminately rating their firm poorly). However, the effect on work-life balance is much smaller, and we detect no statistically significant effect on compensation, suggesting negative reviews are concentrated on items most directly linked to announcements. We draw the same conclusions under the simple specification with fewer fixed effects in Panel B.²⁵

One concern with our main estimates is that data from small firms only have a small number of reviews, such that a single negative review could have an outsized impact. To rule out this possibility, we demonstrate in Appendix E that the results are similar if we restrict to firms with 50 or more reviews before the *Dobbs* announcement.

We also rule out an alternative explanation: post-*Dobbs* reviews could be submitted disingenuously by individuals never employed by these firms, i.e., “trolling.” This explanation is highly unlikely. First, in addition to committing to check every contribution, Glassdoor requires users to certify their employee relationship through their terms of service and verify a permanent email address or social networking account.²⁶ Second, the reduction in ratings persists throughout the entire post-*Dobbs* period, whereas we would expect large and short-lived impacts from a surge in trolling. Third, we equally weight each announcing firm, implying that a coordinated campaign across many firms would be necessary for fake reviews to drive our results. Fourth, our results are robust to removing any reviews that may be candidates for farmed fake reviews, as well as considering a specification with worker fixed effects that requires within-worker variation in ratings before and after *Dobbs*. We report tables and figures for all of these robustness exercises and

²⁴We observe larger impacts for those who conceal personal characteristics (job title and/or location) in Appendix Table E.15, which may reflect a desire for greater anonymity when relaying more negative information out of concern for employer retaliation (Sockin and Sojourner, 2023). This fear could be more acute if politically misaligned.

²⁵The decline in employee satisfaction could stem from a fear of being replaced, particularly if the worker is politically misaligned. This interpretation would be consistent with the decline in ratings of career opportunities. While we cannot fully rule this out, we will show that the jobs that experience the largest increases in applications do not exhibit the largest corresponding declines in satisfaction. Moreover, we find no evidence of an increase in job postings (see Table 9), which would be a precursor to replacing misaligned current workers.

²⁶For more details on this policy, see [here](#).

a more extended discussion in Appendix E.

Another natural concern is that the decline in Glassdoor ratings following these announcements could be mechanically driven by changes in who reviews on the platform, rather than by a deterioration in workplace experiences among current employees. In Appendix E, we show that while there is a modest increase in the probability that smaller announcing firms receive *any* review in a given period, there is no meaningful change in the intensive margin of review volume for firms that consistently receive reviews. We also do not see a shift toward first-time Glassdoor users or first-time reviewers, and the decline in satisfaction persists within reviewers over time and across a wide range of specifications and alternative control groups.

Last, we further demonstrate in Appendix E the robustness of our main estimates to less- and more-saturated DiD specifications, to alternative rank thresholds, and to various alternative counterfactual firms, e.g., matching on industry and size, or a comparison to most likely announcers who did not announce. Beyond these sensitivity checks, we rule out several other concerns, including layoffs in the information technology sector and spillovers to other firms.

Deterioration of Culture To better understand the impacts on satisfaction, we examine respondents’ free-response descriptions of the positive (‘pros’) and negative (‘cons’) aspects of their jobs. These announcements were ostensibly politically polarizing, as evidenced by the overlap between the list of announcers and the companies CPAC labeled as ‘woke’ based on their post-*Dobbs* policies. Based on this fact, we record whether reviewers specifically mention the term ‘woke’ in their reviews. Formally, we re-estimate equation (1) using an indicator for whether ‘woke’ appears in the pros or cons sections as the outcome. Table 4 reports the results. While we find little change in the presence of this phrase in the pros section in column (7), we find workers increasingly disparage their firms by referencing ‘woke’ in the cons section after *Dobbs*. This effect is large relative to the sample average: under the benchmark specification in Panel A, ‘woke’ occurs roughly 200% more often, although we caution the initial incidence of this phrase is rare (at 0.06%). Similarly, under the simpler DiD specification with fixed effects in Panel B, the increase remains large at 100%, again off the low pre-*Dobbs* baseline of 0.06%.

The phrase “woke” is generally used pejoratively. The increase in this negative adjective used to describe announcers may be offset by positive statements from value-aligned workers. To assess this possibility, we examine whether there was an impact on respondents using more neutral or plausibly positive phrases like “culture”, variations on “feminist, female-friendly, etc.” through the phrase “fem”, or variations on “inclusive, inclusivity, etc.” through the phrase ‘inclus’ in the pros or cons sections. For announcing firms after *Dobbs*, we find ‘culture’ is 8–19% *less* likely to be mentioned in the pros section and ‘inclus’ is 25-34% less likely to be mentioned.²⁷

We interpret this as evidence that the decline in satisfaction is partly fueled by a shift in how employees view their companies politically. Excerpts from announcers’ reviews after *Dobbs* that mention ‘woke’ in the cons section are consistent with this narrative. One reviewer writes that their workplace is “not friendly to conservatives.” Another said that their employer is a “very liberal organization that forces woke agendas on employees.” A third stated their workplace has “super woke leadership,” which is “either a plus or minus depending on your political views.” In each of these instances, political (mis-)alignment within the firm is invoked. However, misalignment need not be the only reason satisfaction declines when firms enter politics. Workers may also be concerned their firms will become less profitable; as one reviewer writes, their firm is “going woke, not focusing on profit.”

Current Employee Search and Turnover Next, we examine whether dissatisfaction with management translates to current workers searching for and applying for jobs at other firms. To focus on the firm while still allowing for heterogeneity by gender and politics, we bin current employees according to the announcer or non-announcer they work for, whether they list a job title in the upper tercile by female employment share, whether they work in a trigger state, and the calendar year-quarter they are searching. We then employ the following DiD specification:

$$Y_{k,f,d,t,a} = \sum_{\tau \neq -1} \beta_{\tau} \mathbb{1}\{k \in \mathbb{A}\} + \lambda_{k,a} + \lambda_{t,a} + \lambda_{f,d,a} + \varepsilon_{k,t,f,d,a}, \quad (2)$$

²⁷Under the simpler specification, we find ‘fem’ is about 80% less likely to be mentioned in the cons section, and this decrease is statistically significant. This could suggest that some aligned workers are less likely to view their firms negatively on gender or female-friendliness. Possibly consistent with this result, we find a more muted response in job satisfaction among jobs that are female-dominated (see Appendix F).

Table 4: Effect of Firms' Announcements on Reviewers Mentioning Culture-Related Phrases

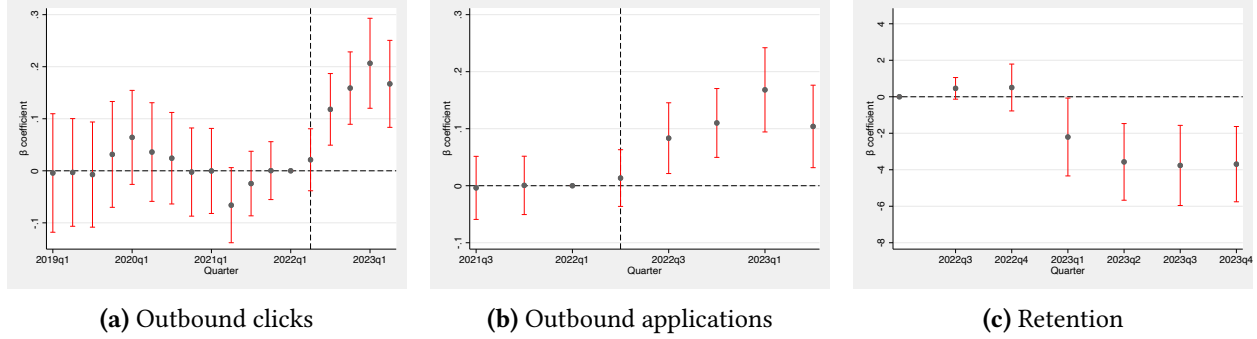
	Culture		Inclus		Fem		Woke	
	Pros	Cons	Pros	Cons	Pros	Cons	Pros	Cons
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Main Specification								
After announcement	-2.014***	0.429	-0.355*	0.018	0.025	-0.124	0.008	0.134*
	(0.601)	(0.318)	(0.189)	(0.165)	(0.030)	(0.111)	(0.008)	(0.077)
Pre-announcement mean DV	10.595	4.229	1.379	0.412	0.035	0.178	0.004	0.064
Observations	6,854,574	6,854,574	6,854,574	6,854,574	6,854,574	6,854,574	6,854,574	6,854,574
Panel B: Simple Specification								
After announcement	-0.984**	0.281	-0.587**	-0.080	0.006	-0.176**	-0.000	0.062*
	(0.480)	(0.316)	(0.262)	(0.060)	(0.019)	(0.089)	(0.005)	(0.036)
Pre-announcement mean DV	12.110	5.291	1.717	0.446	0.042	0.218	0.005	0.062
Observations	9,984,689	9,984,689	9,984,689	9,984,689	9,984,689	9,984,689	9,984,689	9,984,689

Notes: Table 4 reports DiD estimates capturing the change in the incidence of specific phrases in the pros and cons text of Glassdoor reviews between announcing and non-announcing firms after *Dobbs*. Each dependent variable is an indicator equal to one if the worker mentions the phrase listed in the header of each column and zero otherwise. For ease of exposition, the dependent variable is multiplied by 100. Panel A includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Panel B includes event-firm and event-quarter fixed effects. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

where $Y_{k,f,d,t,a}$ represents the logarithm of total clicks or total application starts on postings outside the firm, by all workers employed at firm k in calendar year-quarter t , is in a trigger state or not with indicator d for a job title for which the female employment share is in tercile f , for the grouping a that includes an announcing firm and its 20 matched comparison firms. The benchmark specification includes firm fixed effects $\lambda_{k,a}$, calendar year-quarter fixed effects $\lambda_{t,a}$, and female employment tercile-trigger state indicator fixed effects $\lambda_{f,d,a}$. We again cluster standard errors by grouping a and firm k . We also assess the robustness of our estimates to potential zeros in the log specification in Appendix E, following the suggestion of [Chen and Roth \(2023\)](#) to consider the extensive and intensive margins separately.

Figure 2 presents event-study estimates of workers' outbound job search behavior. Directly following firms' announcements, Panels (a) and (b) document statistically significant increases in the number of clicks and application starts that current employees of the firm send to job postings of external firms.²⁸ Both panels exhibit flat and stable pre-trends prior to *Dobbs*. The effects are economically meaningful. As shown in Table 5, outbound clicks and application starts to other

²⁸As the identity of which worker made which application is only available in the Indeed data starting in the third quarter of 2021, our pre-treatment window for outbound applications is truncated to this time frame.

Figure 2: Event Study of Firms' Announcements on Outbound Search and Turnover

Notes: Figure 2 plots the DiD estimates capturing the change in the logarithm of outbound clicks and outbound application starts by workers employed at announcing versus non-announcing firms after *Dobbs* in panels (a) and (b), respectively. Observations are cells grouped by firm, quarter, state category (trigger vs. non-trigger), and female employment share tercile. Panel (c) shows retention, measured as the estimated mean gap in the percentage of remaining workers over time who were employed with the firm in June 2022 at announcing versus non-announcing firms, where observations are firm-quarter cells. Each panel includes event-firm and event-quarter fixed effects. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-value for the test of joint significance for all pre-*Dobbs* coefficients is 0.184 for panel (a) and 0.984 for panel (b).

Table 5: Effect of Firms' Announcements on Outbound Search by Firm Size

	Outbound clicks		Outbound applications	
	(1)	(2)	(3)	(4)
After announcement	0.159*** (0.038)	0.125*** (0.034)	0.112*** (0.025)	0.098*** (0.025)
After announcement x Ln(Firm size)		-0.038*** (0.014)		-0.000 (0.009)
Observations	793,152	640,980	352,512	284,880

Notes: Table 5 reports the DiD estimates capturing the change in the logarithm of outbound clicks and outbound application starts by workers employed at announcing versus non-announcing firms after *Dobbs*, allowing for heterogeneous effects by firm size. Observations are cells grouped by firm, quarter, state grouping (trigger vs. non-trigger), and female employment share tercile. Each specification includes event-firm and event-quarter fixed effects. We proxy for the logarithm of firm size by using total postings in 2019, and this variable has been demeaned. Even columns have fewer observations because some firms had no postings in 2019. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

employers increase 17% and 11%, respectively. These results are robust to excluding zeros, and can be observed along the extensive margin: announcing firms become more likely to have at least one click and one application toward jobs elsewhere on Indeed made by current workers (see Appendix E).

These estimates capture the pure supply-side effect of these announcements on current em-

employees’ search behavior. Whether these workers’ turnover intentions translate into realized turnover depends on the demand side of the market. For example, if workers wish to leave the firm but there is limited demand for their labor from announcing firms, an intention to exit may not translate into actual turnover. To measure realized impacts on turnover and the firm’s retention of current employees after *Dobbs*, we collected all resumes on Indeed to identify job-to-job transitions. Formally, we estimate the fraction of workers who remain employed in announcing firms and non-announcing firms, among the set of workers we observed employed in the quarter of the *Dobbs* ruling. Given that we benchmark against the firm’s employees in the second quarter of 2022, we can only estimate retention differences after the announcement.²⁹ We find, as depicted in Panel (c) of Figure 2, that employees of announcers are 3.7 percentage points less likely to remain with the firm one-and-a-half years later compared with employees in non-announcing control firms. Relative to non-announcers’ retention rates (see Appendix Table H.3), this represents an 8% lower rate of retention after *Dobbs* for announcers.

Together, the results present a clear pattern. Not only are workers at announcing firms less satisfied, but they also demonstrate a significant increase in outbound search and reduced retention. We conclude that after firms made politically-charged announcements, workers expressed dissatisfaction, began searching for and applying to new jobs, and increasingly left the firm.

Firm Reputation We next consider whether firms with less-established sociopolitical reputations had more to gain (or lose) from wading into sociopolitical issues. To proxy for firm reputation, we focus on firm size. Larger firms are more likely to be well-known household names with established public identities, whereas smaller firms may struggle to stand out in labor markets and convey information about workplace culture (Benson *et al.*, 2020; Bryan *et al.*, 2022; Sockin and Sojourner, 2023). As a result, workers may have noisier priors about smaller firms, implying that announcing a sociopolitical stance may be especially revealing. We thus re-estimate our baseline specifications, allowing treatment effects to vary with the logarithm of firm size.

The results, recorded in Table 5, indicate larger responses among smaller firms. Columns (2)

²⁹We caveat that this depends on workers updating their resumes, which is more likely when switching jobs.

and (4) show that the interaction between the post-announcement indicator and log firm size is negative, and for outbound clicks specifically, is statistically significant and large. The increase in outbound clicks, for instance, is roughly 4 percentage points smaller for firms that are one-unit of log firm size larger. Taken together, these estimates indicate the increase in outbound search is stronger for workers at smaller firms. While the interaction with firm size is more muted for outbound applications, we find a similar and consistent relationship between firm size and the magnitude of the decline in employee satisfaction (see Appendix E).³⁰

To better understand this pattern, Appendix E examines extensive-margin outcomes. We show that smaller announcing firms experience statistically significant increases in the probability of receiving *any* Glassdoor review and in the likelihood that current employees generate *any* outbound click or application. These extensive-margin responses are largely absent for larger firms and help explain why the average effects are larger among smaller employers.

Overall, these results suggest sociopolitical announcements are particularly salient for employees at smaller firms, where prior beliefs about firm values may be less well-formed.

Announcement Features Announcing is a bundled treatment that includes a dollar benefit, possible revelations about CEO ideology, and indications of firm sociopolitical culture. In Appendix F, we use variation across announcements and firms to test for differential effects by whether the announcement: (i) listed a dollar amount of maximal coverage (see Appendix C) or introduced additional family-friendly benefits such as IVF, (ii) led to different reactions based on CEO ideology, (iii) was made on the social media platform LinkedIn, or (iv) mentioned donations to organizations such as the American Civil Liberties Union (ACLU) or Planned Parenthood.

We observe little difference if either a maximal dollar amount was included or if the announcement included additional family-friendly benefits—further demonstrating our effects are not the product of introducing a new fringe benefit. We next examine whether differences exist by CEO characteristics—as current employees may associate the decision to announce with firm leader-

³⁰Further evidence that these announcements caused a greater shift in firm reputation for smaller firms can be seen in the arrival rate of Glassdoor reviews, as smaller firms are more likely to receive any review on Glassdoor in a given calendar half-year after announcing. This extensive margin response is less apparent in larger firms.

ship, which may differ by whether the CEO is a Democrat or female—and find no evidence of differential effects. Thus, while the CEO’s political lean and gender predict whether firms announce, they do not predict how employees respond. Last, we find that firms that announced on social media experienced sharper increases in outbound and inbound search, which we interpret as evidence that digital information-sharing platforms play key roles in labor market sorting (e.g., [Belot et al., 2018](#); [Bryan et al., 2022](#); [Sockin and Sojourner, 2023](#); [Wheeler et al., 2022](#)).³¹

We conclude from these results that workers were not responding to the benefit itself or the CEO’s personal ideology, but that the public nature of the announcement matters. We also find that sentiment declines more sharply when announcements explicitly reference political donations, rendering the statement more overtly political. However, fewer than one-tenth of announcers include such references, and the estimated difference is not statistically significant. Still, this pattern is consistent with the interpretation that these announcements signaled broader dimensions of firm culture, which, as we show next, induced worker sorting along sociopolitical lines.

Sorting Across Gender and Politics Given that the firm’s gender and political composition are correlated with whether the firm announced, Table 6 examines whether dissatisfaction and outbound sorting after *Dobbs* vary systematically by gender and political affiliation. Since we do not observe gender for individuals on Indeed, nor for three-quarters of Glassdoor reviewers, we proxy for gender using the female employment share in the worker’s job title, partitioning job titles into terciles by female employment share. As we do not observe political leanings among Indeed or Glassdoor reviewers, we proxy for political affiliation by whether the worker is in a trigger state or a non-trigger state. Trigger states generally lean very Republican: the Democratic vote share was only 39% in trigger states, compared with 53% in non-trigger states.

Using these proxies, column (1) suggests that women are significantly less likely to engage in outbound search after their firm announces abortion-care coverage following *Dobbs*. While announcements significantly increase the likelihood that workers click and apply to external

³¹While smaller firms were more likely to announce on social media, possibly because they are less likely to have spokespeople or articles written about them, still about three-fifths of medium firms and one-quarter of large firms had their announcements shared on social media. This effect thus seems related to, but distinct from, firm size.

postings, the effects are substantially muted for female-dominated job titles. Women generate 17 percentage points fewer outbound clicks than men, indicating less outward mobility among female workers.

Column (2) shows that this gender-based heterogeneity is not present in trigger states, where both men and women are similarly likely to search for and apply to jobs elsewhere following the announcement. This pattern suggests that in places where firms' announcements are more likely to be politically misaligned with the local workforce, outbound sorting is broad-based and does not vary systematically by gender.

In contrast, the heterogeneity is concentrated among women in non-trigger states. Column (3) shows that women at announcing firms in non-trigger states are significantly less likely to search for and click on job ads for alternative employers compared with men in non-trigger states at announcing firms. This pattern indicates that the most aligned workers—women in Democrat-leaning states—are not deterred by these announcements and instead remain relatively insulated from the broader increase in employee turnover.

8 Effects of Firms Announcing Political Stances on Prospective Workers

Section 7 focused on *current* employees, but ignored the potential impacts on *prospective* employees. While misaligned workers sort out of announcing firms, aligned workers might sort in. To understand the salience of these announcements for prospective workers, we first examine the keywords individuals type into the Indeed search bar. Appendix Figure H.3 depicts the share of searches that include “abortion” or related words and phrases relative to the same share in January 2019, well before the Supreme Court considered the *Dobbs* case. The share of searches that include “abortion” or related terms increased nearly 3,000% in the days immediately after the ruling. The effect dissipated but remained elevated thereafter: compared with job seekers in January 2019, job seekers from July 2022 through January 2023 explicitly searched for jobs on Indeed using abortion and related words 147% more often.

Next, we examine whether these announcements shifted job seekers' behavior by analyzing total inbound clicks and application starts for Indeed job postings. For disclosure reasons, we

Table 6: Effect on Current Employees by Female Employment Share and State Political Lean

	All States	Trigger State	Non-Trigger State
	(1)	(2)	(3)
Panel A: Ratings of management			
After announcement	-0.138*** (0.040)	-0.107* (0.058)	-0.127*** (0.047)
After announcement x Top tercile of female employment share	0.093 (0.059)	0.007 (0.109)	0.040 (0.064)
Observations	2,632,846	523,772	1,825,984
Panel B: Outbound clicks			
After announcement	0.256*** (0.052)	0.138*** (0.052)	0.264*** (0.053)
After announcement x Top tercile of female employment share	-0.169*** (0.064)	-0.047 (0.071)	-0.204*** (0.065)
Observations	396,576	371,358	393,444
Panel C: Outbound applications			
After announcement	0.140*** (0.040)	0.084** (0.038)	0.152*** (0.041)
After announcement x Top tercile of female employment share	-0.021 (0.059)	0.018 (0.061)	-0.060 (0.059)
Observations	176,256	165,048	174,864

Notes: Table 6 reports the DiD estimates capturing the change in ratings of management and the logarithm of outbound clicks and application starts by workers employed at announcing versus non-announcing firms after *Dobbs*, allowing for heterogeneous effects by whether the job title is in the top tercile of female employment share. Observations in Panels B-C are cells grouped by firm, quarter, state category (trigger vs. non-trigger), and female employment share tercile. Panel A includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Panels B-C include firm-group and quarter-group fixed effects as well as interactions of an indicator for whether top tercile of the female employment share with indicators for group and treatment. All regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

cannot report raw means; however, we present the raw differences between announcers and non-announcers over time in Appendix Figure H.4. We observe a sudden and stark jump in application starts for announcers' job postings after the announcements in the raw differences.

For computational tractability, we aggregate job postings to the job title-firm-state-year-quarter level. At this level of aggregation, there are still 80 million observations (see Table H.1). Our DiD specification follows equation (1), estimating:

$$Y_{j,k,s,t,a} = \sum_{\tau \neq -1} \beta_{\tau} \mathbb{1}\{k \in \mathbb{A}\} + \gamma X_{j,k,s,t,a} + \lambda_{k,a} + \lambda_{j,t,a} + \lambda_{s,t,a} + \epsilon_{j,k,s,t,a} \quad (3)$$

where $Y_{j,k,s,t,a}$ represents the logarithm of job seeker clicks or application starts for all postings

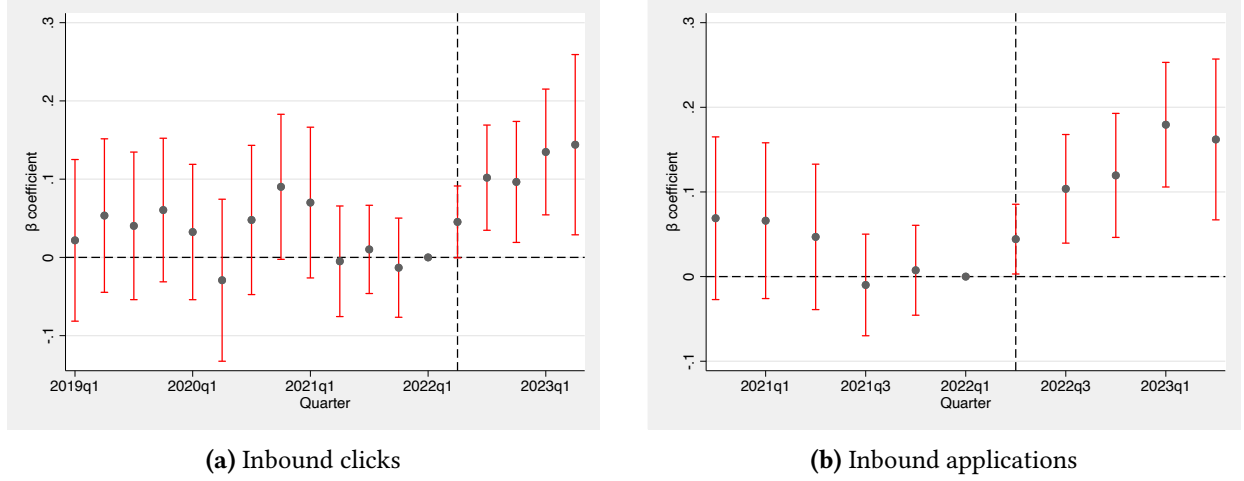
with job title j at firm k located in state s in calendar year-quarter t for the grouping a of an announcing firm and its 20 matched comparison firms. The benchmark specification includes the same fixed effects as in equation (1): for the firm $\lambda_{k,a}$, job titles over time $\lambda_{j,t,a}$, and states over time $\lambda_{s,t,a}$. We also include the logarithm of total postings in each cell, $X_{j,k,s,t,a}$, to account for the data being aggregated across postings.

We plot the event-study estimates for each outcome in Figure 3. In both panels, we observe no significant pre-trend between announcers and non-announcers before *Dobbs*. Shortly after these announcements, we observe an immediate and persistent jump in clicks and application starts for job postings of announcing firms in Panels (a) and (b), respectively.³² Table 7 reports overall DiD estimates for the benchmark specification in Panel A, as well as the simpler specification with fewer fixed effects in Panel B. The benchmark specification shows these announcements caused a statistically significant increase in clicks and application starts of about 9% and 11%, respectively. The effects are similar under the simple specification.

More clicks and applications may not always be unambiguously beneficial for a firm. An increase may cause congestion, and stem from candidates the firm would not consider hiring—both of which serve to raise vacancy costs through increased screening. That said, receiving more than zero applications is undoubtedly positive, assuming the firm is interested in filling the posted opening. In Appendix E, we report a significant positive impact on announcers’ job postings receiving at least one click or at least one application start, in addition to the increase overall. We also show that the increases in inbound clicks and applications are robust to dropping zero cells. We conclude that these announcements were likely positive developments for recruitment.

One way to put these effects in context is to approximate how much posted wages would need to increase to achieve the same increase in clicks or application starts. Building on the approach of Marinescu and Wolthoff (2020), we estimate that a 10% increase in the posted wage is associated with a 6.8% increase in clicks (Appendix Table H.4), implying that for job seekers, the signal from announcing was as valuable as a 13% greater posted wage. For application starts, we estimate that the signal from announcing was as valuable as a 26% greater posted wage.

³²Note that for application starts, data are only available beginning in the fourth quarter of 2020.

Figure 3: Event Study of Firms' Announcements on Inbound Clicks and Applications

Notes: Figure 3 plots the DiD estimates capturing the change in the logarithm of inbound clicks and inbound application starts between announcing versus non-announcing firms after *Dobbs*. Each specification includes event-firm, event-state-quarter, and event-job title-quarter fixed effects, and controls for the logarithm of total job postings. Observations are firm-job-title-state-quarter cells, weighted such that each event is given equal weight. Application starts are only observed in the data beginning in the fourth quarter of 2020. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-value for the test of joint significance for all pre-*Dobbs* coefficients is 0.256 for panel (a) and 0.321 for panel (b).

Table 7: Effect of Firms' Announcements on Inbound Search by Firm Size

	Inbound clicks		Inbound applications	
	(1)	(2)	(3)	(4)
Panel A: Main Specification				
After announcement	0.090*** (0.030)	0.078** (0.030)	0.109*** (0.027)	0.095*** (0.028)
After announcement x Ln(Firm size)		-0.028** (0.013)		-0.017 (0.011)
Observations	75,655,221	69,479,336	56,612,847	50,500,097
Panel B: Simple Specification				
After announcement	0.108*** (0.036)	0.076** (0.038)	0.114*** (0.032)	0.084** (0.034)
After announcement x Ln(Firm size)		-0.026** (0.012)		-0.023* (0.012)
Observations	81,192,640	74,844,040	60,104,374	53,822,824

Notes: Table 7 reports the DiD estimates capturing the change in the logarithm of inbound clicks and inbound application starts between announcing versus non-announcing firms after *Dobbs*, allowing for heterogeneous effects by firm size. Panel A includes fixed effects for event-firm, event-state-quarter, and event-job-title-quarter. Panel B includes event-firm and event-quarter fixed effects only. Both control for the logarithm of total postings. We proxy for the logarithm of firm size by total postings in 2019 and this variable is demeaned. Even columns have fewer observations because some firms had no postings in 2019. All regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Firm Reputation In the even columns of Table 7, we consider whether there are heterogeneous effects between announcers by their relative size. In column (2), we observe that the increase in clicks is significantly stronger for smaller firms. Inbound applications exhibit a similar pattern: while applications increase sharply on average, the interaction with firm size is negative, suggesting greater effects among firms with less-established reputations, although the estimates are less precise. This result reinforces the interpretation that sociopolitical announcements are most informative, and therefore most consequential, for firms whose reputations are less established. For larger firms with more entrenched public identities, announcements generate more incremental changes in attracting workers.

Sorting Across Gender and Politics Table 8 shows that the surge in job-seeker interest following these announcements is sharply polarized along political lines. The increase in inbound activity is driven almost entirely by jobs located in non-trigger states. As shown in column (3), postings in non-trigger states experience a statistically significant rise on average of at least 7-8% in inbound interest from job seekers after the announcement. In contrast, there is no detectable change in either clicks or applications for jobs located in trigger states broadly in column (2), where point estimates are small and statistically indistinguishable from zero. This stark geographic split suggests that workers' responses are closely aligned with political leanings, with job seekers in more Democratic-leaning states reacting positively to firms' announcements.

The response is even stronger when we proxy for job seekers' gender by allowing heterogeneous effects for female-dominated job titles, i.e., those in the top tercile of female employment share. In non-trigger states, postings for female-dominated jobs experience an additional 4 log-point increase in clicks and applications beyond the baseline post-announcement effect. In total, female-dominated jobs experience a roughly 10% increase in clicks and application starts. This pattern is consistent with firms attracting ideologically aligned workers, and especially women in Democratic-leaning states, following the announcement.

In trigger states as well, we observe a gendered response. While there is no apparent shift in clicks or application starts for announcers' postings in male-dominated or more gender-balanced

Table 8: Effect on Prospective Workers by Female Employment Share and State Political Lean

	All States	Trigger State	Non-Trigger State
	(1)	(2)	(3)
Panel A: Inbound clicks			
After announcement	0.055* (0.033)	-0.012 (0.036)	0.065** (0.033)
After announcement x Top tercile of female employment share	0.041** (0.019)	0.051 (0.033)	0.037* (0.019)
Observations	67,385,265	16,136,747	48,764,549
Panel B: Inbound applications			
After announcement	0.067** (0.029)	0.003 (0.033)	0.075** (0.029)
After announcement x Top tercile of female employment share	0.044** (0.019)	0.057** (0.029)	0.044** (0.021)
Observations	49,470,514	12,275,262	35,606,433

Notes: Table 8 reports DiD estimates of the change in the logarithm of inbound clicks and inbound application starts between announcing and non-announcing firms after *Dobbs*, allowing for heterogeneous effects by whether the job title is in the top tercile of female employment share. Each specification includes fixed effects for event-firm, event-state-quarter, and event-job-title-quarter, and controls for the logarithm of total job postings. All regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

jobs, we find a meaningful increase among female-dominated jobs. In column (2), we observe a 6% rise in applications to announcers for female-dominated jobs, even though the average effect in trigger states is otherwise muted. This suggests that some of the women most directly impacted by *Dobbs*—women in Republican-leaning states where abortion was outlawed—reoriented their job search toward firms that publicly committed to covering abortion-related care.

9 Is Announcing Part of a Broader Change in Recruitment Strategy?

In making these declarations, announcing firms not only signaled a cultural stance, but revisited their compensation policies by introducing a new fringe benefit—equivalent to 5% of the average wage if used (Appendix C). A key question is whether firms announced this new benefit to signal their company culture and sociopolitical affiliation, or whether announcing was part of a broader suite of changes to their personnel priorities and strategies. If the latter, then the sorting patterns we document in Sections 7 and 8 may not reflect cultural (mis)alignment but rather workers responding to different yet simultaneous actions undertaken by these firms. This section explores

whether announcing firms simultaneously revisit recruiting intensity and wages.

Recruiting Intensity To capture recruiting intensity, we again turn to the Indeed data. For each announcing firm, we first determine whether they choose to hire at all—creating an indicator variable equal to one if the firm posts an advertisement in a given calendar half-year and zero otherwise. Column (1) of Table 9 reports the DiD estimate with this measure as the outcome, and reveals no effect. However, since large firms consistently hire, any change along this margin is more likely to materialize among small firms. When we examine heterogeneous effects across the distribution of firm size (Appendix Table F.3), we again find no change between announcers and non-announcers. Next, we consider the total vacancies posted by each firm each calendar half-year to understand whether the intensity of hiring changes among firms that regularly hire. We observe no discernible shift in hiring intensity on average in column (2) nor by firm size in Appendix Table F.3. Together, these results suggest no shift in the extensive or intensive margin of hiring intensity among announcing firms.

Table 9: Additional Recruitment Responses Taken Alongside Announcing

	Recruiting Intensity				Wages	
	1(Posts ad)	Total ads in logs Always ≥ 1	Share ads in trigger states Always ≥ 1	Share ads in top female tercile Always ≥ 1	Posted wage in logs	Realized wage in logs
	(1)	(2)	(3)	(4)	(5)	(6)
After announcement	0.028 (0.021)	-0.055 (0.101)	-0.002 (0.010)	-0.001 (0.012)	0.036*** (0.010)	-0.007* (0.004)
After announcement $\times$ 1(New hire)						0.017*** (0.005)
Pre-announcement mean DV	0.90	7.82	0.20	0.34	10.09	11.20
Announcing firms	273	202	202	202	193	330
Observations	31,401	23,499	23,499	23,499	6,439,390	7,748,166

Notes: Table 9 reports the DiD estimates capturing the change in the number and composition of job ads posted, posted wages, and realized wages between announcing versus non-announcing firms after *Dobbs*. Each specification for recruiting intensity outcomes is aggregated to the firm-quarter level and includes fixed effects for event-firm and event-industry-half-year. Each specification for wages includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-hourly wage indicator, with realized wages additionally including event-new hire indicator fixed effects. New hires are employees with fewer than two years of experience. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Although announcers do not change whether they hire and the number of job ads they post, they may have shifted the composition of their hiring towards markets and jobs in which workers would increasingly align with their stance. To examine this, we focus on firms that regularly hire

and estimate the share of ads each firm posts each calendar half-year that are located within trigger states or for female-dominated jobs. The corresponding DiD estimates reported in columns (3) and (4) of Table 9, respectively, are precisely zero. These results suggest firms do not time announcements to coincide with shifts into labor markets or occupations for which an announcement would be advantageous for attracting workers. Moreover, that we do not observe a shift in hiring towards more Democrat-leaning states suggests announcing was not part of a marketing strategy to drive sales (and thus, firm growth) from more value-aligned customers (Conway and Boxell, 2023).

Wages Since workers respond to posted wages in job ads (Belot *et al.*, 2022; Jalal, 2026), announcers may have revisited their posted wages, either simultaneously or dynamically, in response to changes in the recruiting environment following these announcements. Since posted wages are advertised among new vacancies, they likely react more immediately to shifts in hiring conditions than the wages of incumbent workers, as the latter involve decentralized bargaining with workers individually. While not all job ads list a wage, Indeed data provide a large sample that closely tracks aggregate wage dynamics.³³ Since posted wages are advertised as a range, we focus on the median, but results are similar if we instead use the top or bottom of the range. As shown in column (5), posted wages increase 3.6% on average among announcing firms.

To determine whether this increase in advertised wages translates into higher realized wages, we use Glassdoor pay reports. These data are employee-employer matched and capture the annual base pay a worker receives in a given year, as well as the worker’s years of experience.³⁴ To distinguish between the wages of incumbents and new hires, we identify workers as new hires if they have fewer than two years of experience, and incumbents otherwise. Estimating our benchmark DiD specification in column (6) indicates wages indeed grow faster for new hires—consistent with the growth in posted wages—but not necessarily for incumbent workers, high-

³³Fewer than one-half of U.S. ads include wage information (Stahle, 2023). Nevertheless, a regression of 2019-2023 wage growth from the Indeed Wage Tracker, which uses posted wage data, on the Federal Reserve Bank of Atlanta’s job switcher wage growth series with a six-month lag has an adjusted R-squared of 0.93 (Adrian and Lydon, 2024).

³⁴Workers are incentivized to contribute a pay report to Glassdoor through the same “give-to-get” mechanism as with employer reviews. For more details and evidence of external validity for Glassdoor pay reports, see Karabarbounis and Pinto (2019); Martellini *et al.* (2024); Sockin and Sockin (2025).

lighting a bifurcation that studying posted wages in isolation misses.³⁵

Despite the change in wages among announcers, we can rule out that this phenomenon drives the rise in inbound clicks and application starts we document: In Appendix E, we highlight that there is an increase in inbound clicks and application starts even when we exclude all ads with posted wage information.

We consider two potential explanations for why announcers raise wages after *Dobbs*. The first is that announcers may have become more profitable after announcing through an impact on consumer behavior (e.g., Chatterji and Toffel, 2019; Conway and Boxell, 2023). Data on firm sales from Compustat, however, do not support this hypothesis. In Appendix G, we show that there does not appear to be a shift in sales or sales per worker for publicly-traded announcers after *Dobbs*, even in the retail sector, where consumers are more directly involved.

The second is compensating differentials. The theory of equalizing differences (Rosen, 1986) yields three predictions for why wages could diverge after these announcements. First, a wage penalty could arise among female-dominated occupations since announcing introduced a fringe benefit (even if infrequently used) for female employees.³⁶ Second, a wage penalty could arise among workers in Democrat-leaning states if workers derive amenity value from working for a sociopolitically-aligned firm (Chinoy and Koenen, 2024; Colonnelli *et al.*, 2023). Third, a wage premium could arise among announcing firms for which sentiment falls most sharply since job satisfaction has amenity value to workers (Akerlof *et al.*, 1988; Sockin, 2022).

To test these theories, we investigate heterogeneity along each dimension for posted wages and realized wages in Appendix F. We find little evidence in support of the existence of compensating differentials. Moreover, that we observe an increase in pay only among new hires and not incumbents (column (7) of Table 9) does not support a compensating differentials narrative: the shift in amenities underlying these three hypotheses should apply throughout the firm, not just to new hires.

The increase in wages, therefore, appears consistent with neither increased profitability nor compensating differentials. Moreover, it does not cause the rise in inbound clicks and applica-

³⁵We report event studies for both Indeed posted wages and Glassdoor realized wages in Appendix Figure H.5.

³⁶Although men may also use this benefit if their employer-provided health insurance covers a female partner.

tions. Alternative explanations for the growth in wages among new hires are plausible. For one, announcers may believe that new hires will be more productive after the announcement, as our conceptual framework suggests. Additionally, announcers may believe that new hires will stay longer and, therefore, offer them efficiency wages. Research over the longer run on firms that take sociopolitical stances could prove insightful in examining these alternative theories.

10 Conclusion

Our findings highlight the strength of workers' preferences for non-pecuniary amenities such as firm culture and political affiliation and the corresponding challenges firms face when navigating politically polarizing issues. After the *Dobbs* ruling, firms took into account the political and gender composition of their workforce when deciding whether to cover travel expenses related to abortion. We find that these announcements meaningfully altered subsequent labor market dynamics along gender and political lines. Job seekers in Democrat-leaning locations, and especially women, increasingly expressed interest in working for such employers. In contrast, current workers expressed dissatisfaction with their employers, applied for other jobs, and increasingly left the firm, but this negative effect is not apparent among women in majority Democrat states. Small firms — where the new signal of firm culture was presumably strongest — experienced the largest increases in interest from prospective workers, but also the largest jump in dissatisfaction and turnover among current employees. These results suggest workers would sort differently in the labor market if they could more readily learn firm culture from the outside ([Tadelis and Zettelmeyer, 2015](#); [Sockin et al., 2022](#)).

Our results also speak to discussions about access to abortion and female careers in a post-*Dobbs* world. Prior work suggests that access to contraception and abortion facilitates career advancement for women ([Goldin and Katz, 2002](#); [Myers, 2017](#); [Miller et al., 2023](#)). Since many states made abortion illegal after *Dobbs*, it is natural to wonder whether firms' reproductive care policies could feasibly substitute for the lack of local access to abortion care. Our findings suggest this is highly unlikely. While female-dominated jobs in trigger states do experience increased interest among job seekers, firms are less likely to announce such benefits in the first place when

more of their workers are located in trigger states.

Although we study employers' responses to a change in the political landscape around a single issue, abortion, our results speak broadly to the growing importance of politics and gender in the workplace—and how this changing landscape impacts hiring, retention, and firm culture. Taken together, our findings underscore that firms' political speech has real economic consequences in the United States, not only by shaping consumer or investor behavior, as shown in prior work, but also by actively restructuring labor markets through sociopolitical sorting.

Would we observe the same effects if we studied employers' public responses to a different politically divisive issue, such as gun control (Hou and Poliquin, 2023) or immigration? While female representation throughout the firm may not predict which firms make such announcements when the issue is not directly related to gender, our results regarding job seeker interest would likely hold for these other issues: prospective workers whose sociopolitical views align with the firm's position would increasingly sort towards that firm, while misaligned current employees would express dissatisfaction and start searching for jobs at other firms.

Our findings also offer clear motivation for future research. For one, we are unable to observe individual productivity, so whether the workers who sort into the firm are more or less productive than the ones who sort out of the firm is unknown. Hedblom *et al.* (2019) suggest the former, but in the context of politics rather than corporate social responsibility (CSR), it remains unclear. While we find no immediate impact on firm revenue, if the most productive workers are those who exit, profitability could falter. Alternatively, if increased interest from job seekers enables firms to hire higher-quality workers over time, profitability might rise. Understanding how firms' sociopolitical speech more broadly affects profits is critical to determining if there is a “business case” for firms to step into politics in this way.³⁷ Such sociopolitical speech may also have unintended consequences for firms beyond affecting sales or productivity directly.³⁸ Furthermore, while we gain some insights from the written reviews of current employees, our work invites future qualitative investigations into the workplace following firms' sociopolitical stances. Are there fundamental changes that workers perceive afterward, for instance, in dialogue among

³⁷This is distinct from political donations, for which there may be a clearer “business case” (Cowgill *et al.*, 2023).

³⁸Advertising positive CSR, for instance, can reduce public support for a company bailout (Colonnelli *et al.*, 2022a).

coworkers, or is it business as usual?

Additionally, our methodological approach to identifying labor-market competitors through job-search behavior could shed light on a variety of labor-market questions. For example, a key issue in labor economics is how to define a labor market. Our analysis suggests occupations are a poor proxy for a local labor market, industries are slightly better, and granular location is the most informative. These insights can help guide how we study and define labor market competition. As the U.S. Federal Trade Commission recently released new guidelines for considering the implications of mergers and acquisitions on the welfare of workers,³⁹ defining the relevant labor market for each merger and acquisition will be crucial to measuring changes in concentration.

Beyond the labor market, our results highlight a deeper societal issue. What does increased political homophily in the workplace mean for society? Historically, the workplace has been an important social context in which individuals discuss politics (Finifter, 1974; Conover *et al.*, 2002). While survey evidence suggests about two-fifths of workers believe their coworkers' political beliefs are important when considering where to work (Hertel-Fernandez, 2020), we show that firms unexpectedly signaling that they are Democrat-leaning causes Democrat-leaning workers to sort toward them. Taken to its natural conclusion, as sociopolitical speech becomes more common among firms (Cassidy and Kempf, 2022) and it becomes more apparent which firms lean Democrat and which Republican, individuals will increasingly work among co-partisan workers. In other words, workers will become increasingly less exposed to coworkers with dissimilar views to their own. What can we expect to happen from this bifurcation? Leaning on the findings of Mutz and Mondak (2006), we can expect less political tolerance as people's knowledge of rationales for political perspectives other than their own declines. If the traditional channels by which individuals become exposed to differing viewpoints evaporate, what does that mean for the functioning of a democratic society more broadly?

³⁹See this July 2023 [statement](#) from the Chair of the Federal Trade Commission.

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Online Appendix for

We've Got You Covered: Employer and Employee Responses to Abortion Restrictions

A Firms Offer Support

In this appendix, we provide anecdotal accounts from firms describing how and why they responded to the *Dobbs* ruling with formal announcements of care.

Google: Fiona Cicconi, Google's Chief People Officer, stated in a letter to employees after the ruling, "This is a profound change for the country that deeply affects so many of us, especially women." She went on to state that "Googlers can also apply for relocation without justification, and those overseeing this process will be aware of the situation" and that "to support Googlers and their dependents, our US benefits plan and health insurance covers out-of-state medical procedures that are not available where an employee lives and works."⁴⁰

Salesforce: Marc Benioff, CEO of Salesforce, stated directly after the ruling, "I believe CEOs have a responsibility to take care of their employees – no matter what. Salesforce moves employees when they feel threatened or experience discrimination. To our Ohana – we always make sure you have the best benefits & care, & we will always have your back. Always."

Eventbrite: Julia Hartz, CEO and co-founder stated "I'm reflecting on what it means to have full and complete access to healthcare in the United States. How much of that do we take for granted? How do we decide who gets the proper care they need? Eventbrite stands behind the basic human need for safe reproductive healthcare. I'm grateful to be in a position at #eventbrite to support our teammates in getting the care they need, when they need it. We accept this responsibility with a deep sense of purpose and humility. Because it's the right thing to do."

Clari: Andi Byrne, CEO at Clari announced "I'm disappointed and upset at the news of the U.S. Supreme Court's ruling today overturning *Roe vs. Wade*...The impact this ruling will have on access to reproductive healthcare across the United States cannot be ignored." She went on to

⁴⁰The full letter to employees is available [here](#).

state, "We joined many other companies in adding travel reimbursement benefits for all Clarians to help ensure equal access to reproductive healthcare no matter where our employees live. For those CEOs who may be on the fence about whether to offer this benefit to your employees – now is the time to act. Business leaders must make their voices heard and act to protect the health and well-being of their employees. Of course, corporations offering reimbursement and support is only a small step. We know many women will be excluded from new corporate policies like ours. As I said to all Clarians earlier today: 'It's OK to not be OK.'"

Pinterest: Christine Deputy, Chief People Officer at Pinterest wrote, "It's been a difficult few days in the wake of last week's Supreme Court ruling. I've spent time reflecting with my family, holding space for conversations with my team, and ensuring our Pinterest employee community feels safe and supported...At @Pinterest, we believe access to healthcare is a fundamental human right. We provide our employees and their families with a comprehensive set of benefits designed to support their mental, physical, and financial well-being. As of May 2022, we expanded our medical travel benefits in the U.S. to include travel reimbursement for employees who cannot access abortion care where they live."

State Street: Ronald O'Hanley, Chairman and CEO at State Street wrote, "We believe that access to reproductive healthcare has been essential to greater workplace equality over the past 50 years. The US Supreme Court's overturning of Roe vs. Wade is a detrimental step back in women's rights. Regardless of people's personal views on the issue, we stand firm in our belief that women should have equal access to reproductive healthcare and the ability to make decisions that support their individual rights...In light of the US Supreme Court's decision, State Street is working to ensure that all US employees will have access to reproductive healthcare no matter what state they live in."

These six examples comprise only a small subset of the firms that made such announcements, but they suggest that firms made these announcements with their workforce in mind.⁴¹

⁴¹See also this [The New York Times article](#).

B Expanded Discussion of Conceptual Framework for Announcing

Model Overview. The firm plays a one-shot game within a large labor market. Workers differ based on their sociopolitical orientation (Blue or Red). The firm takes as given their initial composition of blue workers and the share of blue workers in the external labor market. The firm chooses to either make a blue-coded announcement or remain silent, which is implicitly red-aligned. Due to polarization, workers aligned with the firm's stance gain utility and become more productive when their type is aligned. Workers then sort across firms based in part on the extent to which they value alignment with their firm's stance. After sorting, production occurs and outcomes are realized.

Workers and population composition. The labor market is comprised of a unit mass of potential workers. Each worker t has a Blue or Red type, $t \in \{B, R\}$.

Firms and action set. Each firm begins with a workforce of size L_0 with blue share θ_0 . Each firm faces a broader external labor market with a blue share given by λ_0 . We take these as exogenously given pre-announcement. Firms decide whether to make a sociopolitically polarizing announcement $a \in \{b, r\}$, where b refers to a blue-coded announcement (i.e., offering to pay for out-of-state abortions) and r reflects red-coded silence.

Worker Utility. Workers value alignment with the firm's stance. Let $S \in \{A, M\}$ denote alignment status, with $S(t, a) = A$ (aligned) if worker type t and firm announcement a are in the set $(t, a) \in \{(B, b), (R, r)\}$ and $S(t, a) = M$ (misaligned) otherwise. For incumbent workers ($k = I$) and external workers ($k = E$), define the value of aligning with the firm's announcement as $V_A^k \geq 0$ and the value of misaligning as $V_M^k \leq 0$. Worker i of type t and external/incumbent status k at a firm choosing a obtains utility

$$U_{it}^k(a) = w + V_{S(t,a)}^k + \varepsilon_i,$$

where w is the market wage and ε_i is an idiosyncratic taste for the firm. We assume ε_i is mean zero and follows the standard logistic distribution with CDF $\Lambda(x) = 1/(1 + e^{-x})$.

Worker Retention and Inbound Interest. Following the firm's announcement, workers may receive outside opportunities. A worker's effective outside option is defined as $q\bar{U}_t$, where $\bar{U}_t = w + \bar{V}_t^E$ is expected outside option utility,⁴² and $q \in [0, 1]$ is an outside option intensity parameter, which captures how salient and available alternative jobs are (e.g., search intensity, recruiter contact rates, etc.).⁴³ We model the stay/join decision as a comparison between utility at the focal firm and the effective outside option. The incumbent worker stays if $U_{it}^I(a) > q\bar{U}_t$. Similarly, an external worker is willing to join the focal firm if $U_{it}^E(a) > q\bar{U}_t$. Given that ε_i follows a symmetric logit distribution, the resulting probability that a worker of type t and incumbent/external status k stays at or is willing to join a firm choosing a is

$$p_t^k(a) \equiv P(U_{it}^k(a) > q\bar{U}_t) = \Lambda(w + V_{S(t,a)}^k - q\bar{U}_t). \quad (4)$$

Note that since $V_A^k > V_M^k$ and $\Lambda(\cdot)$ is strictly increasing then it follows that a Blue-type worker will be more likely to join a blue-announcing firm, and vice versa for a red-type worker. That is,

$$p_B^k(b) > p_B^k(r), \quad p_R^k(b) < p_R^k(r), \quad k \in \{I, E\}.$$

The role of alignment differences in sorting behavior. The analysis above shows that the direction of sorting following an announcement is unambiguous: Blue workers are more likely to stay at and join blue-announcing firms, while Red workers are more likely to exit or avoid them. However, the *magnitude* of these sorting responses depends on the strength of workers' alignment preferences. Under the logit specialization the stay/join probability (4) admits a simple

⁴²Note that, as with the focal firm, alignment with an external firm depends on worker type and what stance the external firm took. Letting ϕ be the probability the external firm makes blue-coded announcement b , then the expected alignment value from an alternative firm for a worker of type B is $\bar{V}_B^E = \phi V_A^E + (1 - \phi)V_M^E$, while the expected alignment value for a type R worker is $\bar{V}_R^E = (1 - \phi)V_A^E + \phi V_M^E$.

⁴³One can alternatively model q as a literal probability of meeting an alternative firm, which yields a two-stage mixture probability. We adopt the reduced-form outside-option intensity formulation because it preserves the log-odds representation used below while delivering the same qualitative comparative statics.

log-odds representation, since $\log \frac{p_t^k(a)}{1-p_t^k(a)} = w + V_{S(t,a)}^k - q\overline{U}_t$. Evaluating the log-odds change from the firm choosing r to choosing b for a Blue worker gives

$$\log \frac{p_B^k(b)}{1-p_B^k(b)} - \log \frac{p_B^k(r)}{1-p_B^k(r)} = V_A^k - V_M^k \equiv \Delta^k.$$

Thus, the difference in the value of alignment Δ^k is exactly the change in log-odds of staying/joining induced by switching the firm's stance.

The fact that differences in valuation of alignment matters for sorting, both for Blue and Red types as well as incumbent versus external workers, implies the potential for asymmetric responses to polarizing announcements. Indeed, a significant body of prior research broadly suggests that responses to sociopolitical behaviors are asymmetric between aligned and misaligned individuals (Hou and Poliquin, 2023; Burbano, 2021; Hurst, 2023; Carpenter and Gong, 2016; McConnell *et al.*, 2018).

Inbound recruitment and outbound turnover. Assume for simplicity the firm hires a fraction of the external workers willing to join, given by $\Omega > 0$. Given that the external labor market initially had λ_0 blue workers and $(1 - \lambda_0)$ red workers, define $L_B^E(a) = \Omega \lambda_0 p_B^E(a)$ as the number of blue external workers who join the firm following announcement a . Similarly define $L_R^E(a) = \Omega(1 - \lambda_0)p_R^E(a)$ as the number of red external workers who join the firm following announcement a . Then total external new hires following announcement a is $L^E(a) = L_B^E(a) + L_R^E(a)$.

Given that the firm initially had $L_0\theta_0$ blue workers and $L_0(1-\theta_0)$ red workers, define $L_B^I(a) = L_0\theta_0 p_B^I(a)$ as the number of blue incumbent workers who are retained, and similarly define $L_R^I(a) = L_0(1 - \theta_0)p_R^I(a)$ as the number of red incumbents who are retained. Then total incumbents retained following announcement a is $L^I(a) = L_B^I(a) + L_R^I(a)$.

Final employment is given by: $L(a) = L^I(a) + L^E(a)$ and the share of blue workers is given by $\theta(a) = \frac{L_B^I(a) + L_B^E(a)}{L(a)}$.

Proposition 1 (Inbound recruitment). *Inbound recruitment increases following a Blue-coded an-*

nouncement relative to a red one ($L^E(b) \geq L^E(r)$) if and only if

$$\frac{p_B^E(b) - p_B^E(r)}{p_R^E(r) - p_R^E(b)} \geq \frac{1 - \lambda_0}{\lambda_0}.$$

Proof. From $L^E(b) \geq L^E(r)$ we have

$$\begin{aligned} L_B^E(b) + L_R^E(b) &\geq L_B^E(r) + L_R^E(r) \\ \Omega \lambda_0 p_B^E(b) + \Omega(1 - \lambda_0) p_R^E(b) &\geq \Omega \lambda_0 p_B^E(r) + \Omega(1 - \lambda_0) p_R^E(r) \\ \lambda_0(p_B^E(b) - p_B^E(r)) &\geq (1 - \lambda_0)(p_R^E(r) - p_R^E(b)) \\ \frac{p_B^E(b) - p_B^E(r)}{p_R^E(r) - p_R^E(b)} &\geq \frac{1 - \lambda_0}{\lambda_0} \end{aligned}$$

□

The right hand side of this proposition reflects the odds that a randomly drawn worker is Red. The left side reflects how much more likely external Blue workers are to join the firm, relative to how much less likely external red workers are to join. This proposition reveals that net inbound recruitment rises if and only if the relative increase in Blue workers' willingness to join exceeds the relative prevalence of Red workers in the labor market. In turn these joining probabilities depend inherently on the extent to which external workers value alignment with firm's stance.

Proposition 2 (Outbound turnover). *Outbound turnover increases following a Blue-coded announcement if and only if*

$$\frac{p_R^I(r) - p_R^I(b)}{p_B^I(b) - p_B^I(r)} \geq \frac{\theta_0}{1 - \theta_0}$$

Proof. The number of blue worker departures following an announcement a is given by $L_0 \theta_0 (1 - p_B^I(a))$ while the number of red worker departures is given by $L_0 (1 - \theta_0) (1 - p_R^I(a))$. Total departures following any announcement decision is then the sum of red and blue worker departures.

Then departures weakly increase from blue announcements relative to red silence when

$$\begin{aligned}
\underbrace{L_0\theta_0(1 - p_B^I(b)) + L_0(1 - \theta_0)(1 - p_R^I(b))}_{\text{Turnover following Blue announcement}} &\geq \underbrace{L_0\theta_0(1 - p_B^I(r)) + L_0(1 - \theta_0)(1 - p_R^I(r))}_{\text{Turnover following Red announcement}} \\
\theta_0(p_B^I(r) - p_B^I(b)) &\geq (1 - \theta_0)(p_R^I(b) - p_R^I(r)) \\
\frac{p_R^I(r) - p_R^I(b)}{p_B^I(b) - p_B^I(r)} &\geq \frac{\theta_0}{1 - \theta_0}
\end{aligned}$$

□

This proposition mirrors the prior result, but for incumbent workers. It reveals that the exits among misaligned Red workers must be sufficiently large, relative to the reduction in exits among aligned Blues, given their initial shares.

Production, Profits, and Announcements Firm output equals the sum of individual productivities. Following the literature finding that sociopolitical aligned workers exert more effort (Carpenter and Gong, 2016; Burbano, 2021), productivity depends on alignment. Each worker produces $y + \psi V_{S(t,a)}^k$, where $y > 0$ is baseline productivity and $\psi \geq 0$ governs the pass-through from (mis-)alignment utility $V_{S(t,a)}^k$ —where $S(t, a) \in \{A, M\}$ —to output. Note that when workers are misaligned then $V_M^k < 0$ and productivity falls. Firm profits are

$$\Pi(a) = \sum_{k \in \{I, E\}} \sum_{t \in \{B, R\}} L_t^k(a) (y + \psi V_{S(t,a)}^k) - wL(a).$$

Firms choose $a \in \{b, r\}$ to maximize $\Pi(a)$. Note that because a is binary, we can define $\Delta\Pi = \Pi(b) - \Pi(r)$. A firm announces if and only if $\Delta\Pi > 0$.

Given the profit equation above, we can decompose $\Delta\Pi$ into a scale effect and an alignment effect:

$$\Delta\Pi = \underbrace{(y - w)(L(b) - L(r))}_{\text{Scale (employment) effect}} + \underbrace{\psi \left(\sum_{k \in \{I, E\}} \sum_{t \in \{B, R\}} V_{S(t, b)}^k L_t^k(b) - \sum_{k \in \{I, E\}} \sum_{t \in \{B, R\}} V_{S(t, r)}^k L_t^k(r) \right)}_{\text{Alignment-productivity (pass-through) effect}}. \quad (5)$$

This conceptualization reveals the core tradeoff that firms face when deciding whether to announce. An announcement may increase profits by attracting more workers than it loses, as long as $y > w$.⁴⁴ Alternatively, the firm may make an announcement that reduces the size of the firm so long as it improves sociopolitical alignment within the firm which results in higher productivity per worker.

Announcements and Workforce and External Labor Market Composition. How do the initial blue-share of the workforce (θ_0) and the initial blue-share of the external labor market (λ_0) affect whether the firm announces? While the model does not admit an unambiguous prediction absent additional model restrictions, the formulation above makes clear the central tradeoffs.

A firm with a high-blue share with a large blue-share external labor market largely benefits from announcing because its incumbent workers are likely to stay and be more productive, and the firm is likely to grow from attracting more external workers. The same intuition applies in reverse to the symmetric case of a low-blue-share firm in a low-blue-share labor market, where there is a stronger incentive not to announce. Alternatively, when the firm has a high blue-share workforce but operates in a low blue-share labor market (or has a low blue-share but high blue share labor market), the benefits from announcing are more ambiguous.

Model Extensions. The model above is designed to capture the core sorting decisions of workers and understand the various tensions related to the firm's choice to make sociopolitically polarizing announcements. In this section we offer a discussion about four natural extensions, which

⁴⁴If $y = w$ then equation (5) reveals that firms care only about alignment and not about scale. Note that we leave wages as exogenously determined, but in an extension we describe how endogenizing the wage does not change these qualitative predictions.

complicate the model but generally do not change the core logic identified above.

First, in Section B.1 we allow each firm to have its own intrinsic type, with workers having imperfect information about firm type. In general, such information results in some degree of ex ante sorting. The prediction arises that announcements deliver greater sorting effects where there is more uncertainty about firm type. To the extent that larger firms are more likely to have well-known types, these results imply that sorting effects should be greater for smaller firms.

Second, we could include hiring and turnover costs, which naturally blunt sorting behavior and make announcement decisions more sensitive to the current workforce relative to the broader labor market—though their inclusion does not alter the fundamental predictions of the model.

Third, we could allow firms to choose wages jointly with announcements. That is, we can rewrite the firm’s problem as

$$\max_{a,w} \Pi(a,w) = \sum_{k \in \{I,E\}} \sum_{t \in \{B,R\}} L_t^k(a,w) (y + \psi V_{S(t,a)}^k) - w L(a,w). \quad (6)$$

When wages are endogenous, firms face a standard monopsonistic tradeoff given idiosyncratic worker utility: higher wages increase retention and recruitment across all worker types but reduce per-worker surplus. Announcements, by contrast, differentially affect aligned and misaligned workers. As a result, wages and announcements act as imperfect substitutes. Depending on intrinsic values from the announcement, the pass-through to productivity, and workers’ sensitivity to wages, firms may raise wages to mitigate turnover following a polarizing announcement, or lower wages when alignment increases effective labor supply among aligned workers. While equilibrium wage responses are generally ambiguous, allowing wages to adjust does not alter the qualitative predictions of the model regarding sorting, inbound recruitment, outbound turnover, or the scale versus alignment-productivity tradeoff that governs the announcement decision.

Fourth, in the baseline model the fraction of firms making Blue-coded announcements, denoted ϕ , is treated as exogenous. An extension could endogenize ϕ as a market-level equilibrium object. With a continuum of heterogeneous firms, each firm chooses whether to announce taking ϕ as given, while workers’ outside options depend on ϕ through the likelihood of encountering

a firm with a given stance. An equilibrium ϕ^* satisfies a fixed-point condition equating ϕ^* to the measure of firms for which announcing is profitable at ϕ^* . Endogenizing ϕ introduces strategic interactions across firms mediated by workers' outside options and can generate market-level clustering or tipping in announcement behavior. Importantly, conditional on ϕ , the sorting, recruitment, turnover, and scale versus alignment-productivity tradeoffs characterized in the baseline model continue to hold.

B.1 Extension: Imperfect Information, Reputation, and Firm Size

This extension formalizes how announcements have stronger effects for smaller firms because workers have less precise information about the firm's sociopolitical stance pre-announcement. The key mechanism is belief updating: when pre-announcement beliefs are diffuse, a truthful announcement causes a larger change in expected alignment and thus stronger sorting responses.

Firm type. Each firm j has an intrinsic sociopolitical type

$$\tau_j \in \{b, r\},$$

where $\tau_j = b$ denotes a Blue-type firm and $\tau_j = r$ denotes a Red-type firm. Let $\rho \equiv \Pr(\tau_j = b) \in (0, 1)$ denote the population share of Blue-type firms.

Pre-announcement information. Prior to any announcement, both incumbent and external workers observe only a noisy public reputation signal

$$x_j \in \{b, r\}$$

about τ_j . The signal is correct with probability $\pi(L_{j0})$:

$$\Pr(x_j = \tau_j \mid \tau_j, L_{j0}) = \pi(L_{j0}), \quad \pi(\cdot) \in \left(\frac{1}{2}, 1\right), \quad \pi'(L_{j0}) > 0. \quad (7)$$

Thus, larger firms, i.e., those with greater L_{j0} , are better known and generate more precise pre-announcement beliefs among workers.

Given (x_j, L_{j0}) , workers form a posterior belief that firm j is Blue-type,

$$\mu_{j0} \equiv \Pr(\tau_j = b \mid x_j, L_{j0}),$$

which is increasing in signal precision $\pi(L_{j0})$. For larger firms, μ_{j0} is closer to 0 or 1; for small firms, beliefs are more diffuse.

Pre-announcement sorting. Before any announcement is made, both incumbent and external workers make recruitment/retention decisions based on expected utility under beliefs μ_{j0} . For a type- $t \in \{B, R\}$ worker of status $k \in \{I, E\}$, the expected alignment value from firm j is

$$\mathbb{E}[V^k \mid t, \mu_{j0}] = \begin{cases} \mu_{j0} V_A^k + (1 - \mu_{j0}) V_M^k, & t = B, \\ (1 - \mu_{j0}) V_A^k + \mu_{j0} V_M^k, & t = R. \end{cases}$$

As in the baseline model, wages and idiosyncratic match quality enter additively.

Then, under the logit specification, for any worker type $t \in \{B, R\}$ and status $k \in \{I, E\}$, the recruitment/retention probability at firm j can be written as

$$p_{tj}^k(\mu) = \Lambda\left(w + \mathbb{E}[V^k \mid t, \mu] - q\bar{U}_t\right), \quad \Rightarrow \quad \log \frac{p_{tj}^k(\mu)}{1 - p_{tj}^k(\mu)} = w + \mathbb{E}[V^k \mid t, \mu] - q\bar{U}_t,$$

Remark 1 (Pre-announcement composition). *The firm's initial share of Blue workers θ_{j0} is increasing in μ_{j0} . Because μ_{j0} is closer to the extremes for larger firms, large Blue-type firms begin with more Blue-skewed workforces, while small firms tend to have more mixed initial compositions.*

Announcements and post-announcement sorting. Announcements are assumed to be truthful and fully revealing:

$$a_j = \tau_j.$$

After an announcement, beliefs become degenerate for all workers:

$$\mu_{j1} = \begin{cases} 1, & a_j = b, \\ 0, & a_j = r. \end{cases}$$

Post-announcement sorting. Following the announcement, both incumbent and external workers update beliefs from μ_{j0} to μ_{j1} and reoptimize as in the baseline model.

Remark 2. (*Announcement effects and firm size.*) Fix worker type t and status k . For a Blue-type firm ($\tau_j = b$), the magnitude of the change in log odds of staying or joining induced by the announcement (since $\mu_{j1} = 1$ when $a_j = b$) is

$$\left| \log \frac{p_{tj}^k(1)}{1 - p_{tj}^k(1)} - \log \frac{p_{tj}^k(\mu_{j0})}{1 - p_{tj}^k(\mu_{j0})} \right| = (1 - \mu_{j0}) \Delta^k, \quad t = B, R$$

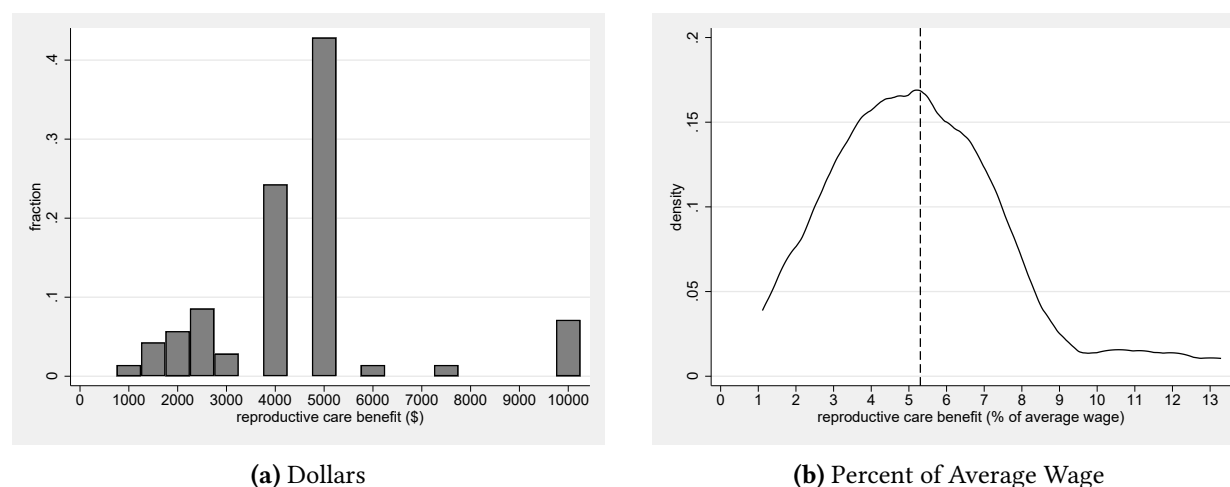
where $\Delta^k \equiv V_A^k - V_M^k > 0$. Because μ_{j0} is closer to the extremes for larger firms, the magnitude of the announcement-induced sorting response is weakly decreasing in firm size L_{j0} . Thus, announcements generate larger changes in recruitment and turnover for smaller firms.

Interpretation. When a firm is already very well known, a public announcement conveys little new information and induces limited newfound sorting. By contrast, for firms that are less well known, a public announcement substantially update workers' beliefs about firm type, leading to larger recruitment and retention responses.

C Quantifying The Announcements

In this appendix, we quantify this newly-created fringe benefit that firms announced, using public statements or comprehensive summaries that accompanied employer communications about these new policies. From these excerpts, we identify 76 firms that announced a maximum dollar amount for how much they would cover each year in expenses incurred while traveling to obtain reproductive care.⁴⁵ The distribution of these maximum dollar amounts per annum is displayed in panel (a) of Figure C.1. If realized, the promised amounts are non-negligible: The mean is \$4,500, and the amounts range from \$1,000 to \$10,000. To gauge the size of this benefit as a share of a worker's income, we calculate the average wage of these firms using pay reports in Glassdoor from January 2021 through June 2022. The distribution of these promised amounts relative to the average wage we observe is shown in panel (b). Though we observe a long right tail, the distribution appears normally distributed with an average of about 5%. To put this number in context, fringe benefits accounted for on average 31% of employee compensation in June 2022.⁴⁶

Figure C.1: Distribution of Travel Coverage for Reproductive Care



Notes: Figure C.1 plots the distribution of the maximum amount that the employer announced it would cover specifically for reproductive care, in dollars in panel (a) and as a percent of the average wage of the firm in panel (b). The average wage is calculated using Glassdoor pay data from January 2021 through June 2022.

⁴⁵We implement this procedure by identifying whether the excerpt included a number for expenses covered preceded by a dollar sign, and excluding when the dollar amount refers to family planning more broadly, not just abortion-related expenses. We exclude two instances where the dollar amount refers to a lifetime benefit and incorporate four where the maximum annual contributions were available elsewhere online.

⁴⁶See https://www.bls.gov/news.release/archives/ecec_09202022.pdf.

D Further Analysis of Which Firms Announce Post-*Dobbs*

In this appendix, we bolster the discussion in Section 5 by further summarizing which firms announce, offering robustness exercises, and considering additional outcomes.

In Table D.1, we break down announcers by Glassdoor industry, of which there are 24. Firms for which there is no assigned industry are grouped together into an “Unassigned” category. Two facts warrant highlighting. The first is that very few firms announced reproductive care after *Dobbs*. While there are about 415,000 firms in the Glassdoor database, only 430 announced reproductive care, which equates to about 0.1%. The second is that announcers are not uniformly distributed across industries. Announcers are concentrated primarily in Information Technology, Retail & Wholesale, Media & Communications, and Financial Services. As a fraction of firms in the industry, Information Technology has the largest coverage at 0.39%.

Table D.1: Firms that Announced Reproductive Care by Glassdoor Industry

Industry	Announcing Firms	Non-announcing Firms	Percent Announcing
Aerospace & Defense	0	2,626	0.00
Agriculture	0	2,969	0.00
Arts, Entertainment & Recreation	9	9,271	0.10
Construction, Repair & Maintenance	1	33,362	0.00
Education	4	27,237	0.01
Energy, Mining & Utilities	3	8,165	0.04
Financial Services	35	20,994	0.17
Government & Public Administration	0	17,881	0.00
Healthcare	14	41,544	0.03
Hotels & Travel Accommodation	0	5,268	0.00
Human Resources & Staffing	7	7,139	0.10
Information Technology	157	40,513	0.39
Insurance	1	5,372	0.02
Legal	1	7,919	0.01
Management & Consulting	15	20,165	0.07
Manufacturing	32	43,739	0.07
Media & Communication	55	23,681	0.23
Nonprofit & NGO	3	16,390	0.02
Personal Consumer Services	3	7,288	0.04
Pharmaceutical & Biotechnology	14	5,218	0.27
Real Estate	9	9,282	0.10
Restaurants & Food Service	1	14,882	0.01
Retail & Wholesale	58	30,768	0.19
Telecommunications	3	3,627	0.08
Unassigned	5	10,837	0.05
Total	430	416,137	0.10

Notes: Table D.1 displays, by Glassdoor industry, the number of firms that announced reproductive care, the number of firms in the sample that did not, and the share of firms that announced within each industry.

Next, recognizing that female CEOs tend to be more Democratic-leaning than male CEOs (Cohen *et al.*, 2019), in Table D.2 we demonstrate that the relation between announcing reproductive care and the political lean of the firm and its employees shown in Table 1 persists even when accounting for CEO gender. Since we can assign CEO gender to only a subset of firms, we leave the specifications in Table D.2 as robustness checks. Even after accounting for the gender of the CEO, a firm was more likely to announce reproductive care if: (i) the CEO donated to Democrats, (ii) the firm's employees donated to Democrats, (iii) the firm had an establishment in a Trigger state, and (iv) a greater share of the firm's employees were outside a Trigger state.

Table D.2: Politics and Whether Firm Offers Reproductive Care Accounting for CEO Gender

	1(Firm Announced Reproductive Care)				
	(1)	(2)	(3)	(4)	(5)
1(Female CEO)	0.017*** (0.006)	0.014** (0.006)	0.014** (0.006)	0.001*** (0.000)	0.001*** (0.000)
Share of CEO donations to Democratic party		0.039*** (0.005)	0.029*** (0.006)		
Share Democrat employees			0.040* (0.021)		
1(Operates in trigger state)				0.003*** (0.000)	0.005*** (0.000)
Share employment in trigger states					-0.011*** (0.001)
Mean DV	0.025	0.025	0.025	0.003	0.003
Firms	2,245	2,245	2,245	131,232	131,232

Notes: Table D.2 reports the relation between whether a firm announced reproductive care and various characteristics related to the political leaning of the firm, accounting for CEO gender. Estimates reflect the marginal effects from a logit specification. CEO gender is imputed by first name for each CEO listed in the Glassdoor database. Each specification includes the logarithm of firm size, an indicator for whether the firm is publicly-traded, and industry fixed effects. Standard errors are clustered by Glassdoor industry. Significance levels: * 10%, ** 5%, *** 1%.

We also show in Table D.3 that the Compustat-derived measures that predict a firm announcing in Table 1 still predict a firm announcing even when accounting for CEO gender. Again, since we are only able to assign CEO gender for a subset of firms, specifically those for whom we observe CEO gender in Execucomp, we leave Table D.3 as robustness. Even after accounting for the gender of the CEO, a firm was more likely to announce reproductive care if: (i) a greater share of

the board is female, (ii) a greater share of college-educated workers in the firm's sector is female, and (iii) the firm's headquarters is not in a Trigger state.

Table D.3: Whether Firm Offers Reproductive Care Accounting for CEO Gender, Compustat

	1(Firm Announced Reproductive Care)			
	(1)	(2)	(3)	(4)
1(Female CEO)	0.046*** (0.015)	0.041*** (0.015)	0.029** (0.014)	0.045*** (0.016)
Female share of non-CEO executives		0.072** (0.036)		
Female share of college graduates in sector			0.262* (0.135)	
1(HQ in trigger state)				-0.043* (0.026)
Mean DV	0.061	0.061	0.055	0.062
Firms	1,468	1,468	1,340	1,424

Notes: Table D.3 reports the relation between whether a firm announced reproductive care and the gender composition of the Board and the sector in which it operates, and if its headquarters is in a Trigger state, accounting for CEO gender. Estimates reflect the marginal effects from a logit specification. CEO gender is obtained from Execucomp. Female share of college graduates calculated within 4-digit NAICS sector using Quarterly Workforce Indicators (QWI) data. Each specification controls for the logarithm of firm size and includes two-digit NAICS sector fixed effects. Standard errors are clustered by two-digit NAICS sector. Significance levels: * 10%, ** 5%, *** 1%.

In Table 1, we document that a firm was more likely to announce reproductive care if the CEO donated to Democrats. This result was based on mapping FEC donation data, which is by firm and name, to the Glassdoor database of CEOs, which is also by firm and name. As a robustness exercise, in Table D.4, we restrict the sample to only employers for which we can map FEC donation data, by firm and name, to the Execucomp database of CEOs. This necessarily restricts the sample to only public firms, and the sample is much smaller. Even among this sample of roughly 120 firms, we still observe that firms with Democrat-leaning CEOs were significantly more likely to announce reproductive care.

In Table 1, we document that a firm was more likely to announce reproductive care if not just the CEO donated to Democrats, but also if the firm's employees more broadly donated to Democrats. To further establish that a firm announcing reproductive care reflected a broader culture within the firm beyond the current CEO, we introduce data on former CEOs' donations. More specifically, we determine the political lean of past CEOs, covering the years 2000 to 2018,

Table D.4: CEO Political Leaning and Whether Firm Offers Reproductive Care, Compustat

	1(Firm Announced Reproductive Care)
	(1)
Share of donations to Democratic party	0.212*** (0.032)
Mean DV	0.132
Firms	121

Notes: Table D.4 reports the relationship between whether a firm announced reproductive care and the political lean of its CEO using data from Execucomp and the Federal Election Commission (FEC). Estimates reflect the marginal effects from a logit specification, which controls for the logarithm of firm size and includes two-digit NAICS sector fixed effects. Standard errors are clustered by two-digit NAICS sector. Significance levels: * 10%, ** 5%, *** 1%.

by matching FEC donation data, by firm and name, to CEOs of public firms in Execucomp, by firm and name. In Table D.5, we show that the political lean of past CEOs is a significant predictor of whether the firm announced reproductive care. This is true even after accounting for the political lean of the current CEO, though we recognize the sample in this instance is considerably thin.

Table D.5: Former CEOs' Political Leaning and Whether Firm Offers Reproductive Care

	1(Firm Announced Reproductive Care)		
	(1)	(2)	(3)
Share of former CEOs donations to Democratic party	0.076*** (0.022)		0.227* (0.124)
Share of current CEO donations to Democratic party		0.500*** (0.125)	0.443*** (0.150)
Mean DV	0.181	0.271	0.271
Firms	622	48	48

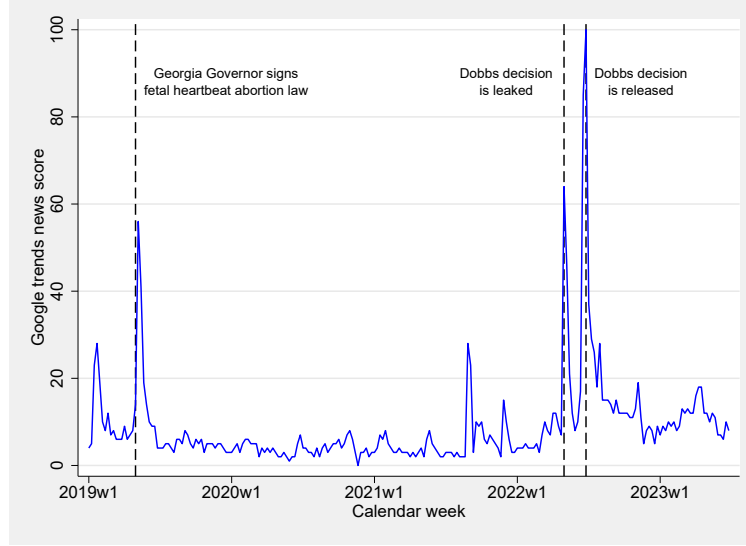
Notes: Table D.5 reports the relationship between whether a firm announced reproductive care and the political lean of its CEO and former CEOs using data from Execucomp and the Federal Election Commission (FEC). Estimates reflect the marginal effects from a logit specification, which controls for the logarithm of firm size and includes two-digit NAICS sector fixed effects. Sample of former CEOs includes CEOs from 2000–2018. Standard errors are clustered by two-digit NAICS sector. Significance levels: * 10%, ** 5%, *** 1%.

E Eliminating Other Possible Explanations

The substantive and discontinuous changes in satisfaction, clicks, and applications relative to the firms' pre-announcement outcomes and those of their matched control firms, the fact that these differences persist for many quarters after announcing, the lack of pre-trends in these same outcomes before announcing in the event studies, and the collection of evidence on politics and gender that are all consistent with our main story, rules out many alternative explanations for our findings. In this section, we implement several additional checks to confirm that our key results are not the byproduct of confounding alternative forces.

Media Coverage Although we cannot rule out that media coverage generates some interest in announcing firms—and acknowledge that some media coverage or wide reach through social media is necessary for prospective workers to learn about the announcements—we believe media coverage itself does not drive our results for two reasons. First, if our results just reflected the causal effect that increased media coverage may have on interest in a firm, regardless of the type of coverage, then we would not expect to observe sorting by gender or politics—since media coverage, especially on the internet, is not geographically or gender constrained. Second, the dynamics of the results we document are inconsistent with media coverage itself being the principal mechanism. If it were simply media coverage that drove the results, we would expect an immediate increase in interest, followed by a subsequent decline as media coverage wanes and other news cycles rise. Indeed, we show in Figure E.1 that media interest in *Dobbs* wanes and remains low after these announcements. Inbound clicks and applications toward announcers, however, not only respond immediately but persist even a year-and-a-half later.

Figure E.1: News Coverage Over Time of *Dobbs* and Abortion



Notes: Figure E.1 plots the weekly Google trends score for “Dobbs + abortion” among News sources during our sample period. Google trends scores are normalized relative to the maximum score, which is set at 100.

Spillovers in Job Search to Non-Announcers Since announcers and non-announcers are competitors for labor (Table 2), the interest non-announcers receive from job seekers might be affected by their competitors’ announcements, i.e., a spillover effect may occur. For instance, if a worker is drawn to a specific announcer after Dobbs and is interested in working in a given U.S. zip code, they may shift their application from a non-announcer to an announcer. Our main identification strategy would capture two effects: (i) the new application to the announcer and (ii) the loss of an application to the non-announcer. While we view this as part of the overall treatment, it is interesting to disentangle the extent to which the effects we document are the product of announcers cannibalizing applications from their competitors. To do so, we consider an alternative comparison set in which such spillovers should be almost entirely absent: the 20 *least* connected firms. More specifically, among all firms also clicked on by job seekers in the same search session, we select the 20 (large) firms that were least likely to also be clicked on when a job seeker was interested in that announcing firm.⁴⁷ Appendix Table E.6 reports the DiD estimates. We find similarly large effects under this alternative, suggesting our results are not the

⁴⁷To ensure the control set is not populated by firms with very few vacancies, but just disconnected, we identify the least connected, considering only large firms. That they are large is immaterial to the purpose of this sensitivity check, although our results are similar if we used the 20 least connected regardless of size.

product of spillovers but rather a broad-based increase in job seeker interest.

Table E.6: Effect on Inbound Clicks and Applications, Most- vs. Least-Connected Firms

	Inbound clicks		Inbound applications	
	Most Connected	Least Connected	Most Connected	Least Connected
After announcement	0.090*** (0.030)	0.153*** (0.036)	0.109*** (0.027)	0.136*** (0.033)
Observations	75,655,221	11,022,437	56,612,847	8,054,550

Notes: Table E.6 reports the DiD estimates capturing the change in the logarithm of inbound clicks and inbound application starts between toward announcing versus non-announcing firms after *Dobbs*, for two alternative sets of control firms based on job seeker click behavior. “Most Connected” reflects the baseline sample comprised of the 20 most connected firms according to job seeker click behavior, whereas “Least Connected” considers the 20 least connected firms according to job seeker click behavior. Each specification includes fixed effects for event-firm, event-state-quarter, and event-job-title-quarter, and controls for the logarithm of total job postings. All regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

IT Sector and Layoffs Around the time of the *Dobbs* ruling, many large IT firms experienced large and public layoffs.⁴⁸ Any effects related to these layoffs could confound the identification of our post-announcement estimates for job satisfaction since more than one-third of announcers operate in IT (see Appendix Table D.1). We address this concern in three ways. First, although we observe particularly strong satisfaction effects in IT, we still observe a significant downturn when we exclude IT announcers (Appendix Table E.7). Second, although employee sentiment likely declines after a mass layoff, the effect appears more pronounced for satisfaction with work-life balance than for satisfaction with culture (Ayas and Arslan, 2023) — but we document the reverse (Appendix Table 3). Third, we observe a sharp decline in sentiment under our alternative, matching-on-observables approach for deriving comparison firms; since this approach matches firms within the same industry, industry-wide shocks cannot explain our findings.

⁴⁸For details, see this [Crunchbase tracker](#) of layoffs among U.S. technology companies in 2022 and 2023.

Table E.7: Effect on Management Ratings, Separately for Announcers in IT and not in IT

	IT sector		Not IT sector	
	(1)	(2)	(3)	(4)
After announcement	-0.243*** (0.046)	-0.326*** (0.072)	-0.100*** (0.031)	-0.087*** (0.030)
DiD Specification	Simple	Benchmark	Simple	Benchmark
Mean DV	3.254	3.231	3.111	3.080
Observations	2,333,980	1,400,251	5,115,160	3,419,203

Notes: Table E.7 reports DiD estimates capturing the change in Glassdoor ratings of management between announcing and non-announcing firms after *Dobbs*, splitting the sample by whether the firm is in the Information Technology industry. Each specification includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Alternative Model Specification To specify our differences-in-differences framework, we built a model that accounted for differences across employers, over time by state, over time by job title, and between former and current employees by including fixed effects by event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator, respectively. To establish that our results are not sensitive to the coarseness or the granularity of our econometric specification, we consider two alternative sets of fixed effects.

First, we consider a coarser set of controls, including only event-firm and event-quarter fixed effects. The results for Glassdoor ratings and inbound search are recorded in Panel B of Tables E.8 and E.10, respectively. The takeaways under this simpler specification are the same.

Second, we consider a tighter set of controls, including event-firm-state-job title fixed effects in lieu of event-firm fixed effects. (We still include fixed effects for event-state-quarter, event-job-title-quarter, and event-former employee indicator). Under this more saturated specification, we now identify how Glassdoor ratings and inbound search evolve after *Dobbs* between announcers and non-announcers for the same roles in the same labor markets. The results for Glassdoor ratings and inbound search are recorded in Panel C of Tables E.8 and E.10, respectively. The takeaways again are similar.

Alternative Comparison Firms Difference-in-differences estimates rely on the control set used to form a counterfactual. In the main paper, our control set was derived from job seekers’

revealed preferences. However, this is not the only plausible control set. For each of our main outcomes, we examine the sensitivity of our conclusions to alternative control sets.

The first alternative we consider is matching firms based on two observable characteristics: industry and size. For the former, we restrict attention to firms that operate within the same Glassdoor industry (listed in Appendix Table D.1) as the announcing firm. For the latter, we partition firms into twenty 5-percentile bins by their total employment (according to an employer lookup table maintained by Glassdoor). Each announcer's comparison set comprises firms operating in the same industry and belonging to the same size-based bin. The difference-in-differences results for Glassdoor ratings are recorded in Panel D of Table E.8, outbound search in Panel B of Table E.9, and inbound search in Panel D of Table E.10. The takeaways are unchanged.

The second alternative is a set of control firms that workers would have anticipated to also announce (based on observables), but did not. This addresses the concern that *Dobbs* itself, as opposed to announcing, differently affected announcing firms. For instance, current employees may have also reacted negatively to the *lack* of an announcement after *Dobbs* if they had expected their firm to do so. In this case, our main estimates would overstate the true decline in satisfaction from announcing. Further, workers may have sorted toward female- and Democrat-leaning firms after *Dobbs* broadly, not just to those that made announcements. In this case, we would identify a positive effect on job search related to *Dobbs* itself rather than announcing.

We rule out this possibility through a two-step process. First, we identify the firms that were most likely to have made an announcement but did not, based on a propensity score model using the observable characteristics of firms that did announce, as summarized in Table E.11. Second, we select the top 2 percent of firms by predicted probability of announcing and use them as the comparison set in a DiD framework. This approach effectively compares firms that announced with firms that should have been most likely to announce but did not. The difference-in-differences results, recorded in Panel E of Table E.8 for Glassdoor ratings, Panel C of Table E.9 for outbound search, and Panel E of Table E.10 for inbound search, produce the same takeaways as under the baseline. Thus, even compared with this set of most-probable announcers, firms that made announcements following *Dobbs* experienced inbound and outbound sorting.

The third alternative combines the benchmark with the two prior alternatives. We again construct an announcer-specific control set of firms, but each firm now satisfies three conditions: (i) is within the top 20 labor market competitors according to job seeker click behavior (as under the benchmark), (ii) operates in the same industry as the announcing firm, and (iii) we would have anticipated to have announced based on observable characteristics but did not, i.e., falls in the top 2 percent of firms according to predicted probabilities. The difference-in-differences results, recorded in Panel F of Table E.8 for Glassdoor ratings, Panel D of Table E.9 for outbound search, and Panel F of Table E.10 for inbound search, are very similar or larger to our main specification in Panel A of all three tables, with the exception of outbound applications where the point estimate is slightly smaller and no longer significant.

Alternative Choice of Ranks In the main analysis, we use the 20 closest labor-market competitors identified by job seekers’ click behavior as the control set. This choice is arbitrary, so we also re-estimate each of our main DiD estimates using an increasingly narrow set of control firms, i.e., the top 15, top 10, and top 5 labor market competitors, as well as only considering even or odd ranked competitors.⁴⁹ Table E.12 affirms our findings are robust to alternative degrees of closeness.

Additionally, each competitor receives equal weight under the baseline and in each of the alternatives in Table E.12. Since higher-ranked firms are, in practice, closer competitors with the announcing firm compared with lower-ranked firms, we also consider a specification in which higher-ranked firms receive greater sample weight. Specifically, a control firm with ranking r is assigned a weight equal to $\frac{1}{r}$. The results, presented in Panel H of Table E.8, Panel E of Table E.9, and Panel H of Table E.10, produce similar findings to the benchmark.

⁴⁹For Glassdoor management ratings, the estimate for odd rankings (−0.167) is remarkably similar to that for even rankings (−0.163). The same is true for outcomes based on the Indeed data (e.g., 0.170 versus 0.163 for outbound clicks and 0.074 versus 0.082 for inbound clicks).

Table E.8: Effect of Firms' Announcements on the Five Rating Categories

	Career opportunities (1)	Compensation & benefits (2)	Culture & values (3)	Senior management (4)	Work-life balance (5)
Panel A: Main Specification					
After announcement	-0.127*** (0.029)	-0.025 (0.026)	-0.131*** (0.031)	-0.174*** (0.034)	-0.065** (0.027)
Observations	4,918,142	4,899,323	4,862,322	4,819,454	4,870,004
Panel B: Simple Specification					
After announcement	-0.129*** (0.025)	-0.028 (0.020)	-0.105*** (0.026)	-0.161*** (0.030)	-0.059*** (0.020)
Observations	7,568,412	7,546,954	7,501,307	7,449,140	7,512,880
Panel C: Saturated Specification					
After announcement	-0.143*** (0.039)	-0.008 (0.031)	-0.139*** (0.042)	-0.177*** (0.044)	-0.077** (0.036)
Observations	3,641,913	3,626,057	3,595,294	3,557,564	3,601,687
Panel D: Match on Industry and Size					
After announcement	-0.096*** (0.024)	-0.023 (0.023)	-0.098*** (0.026)	-0.142*** (0.030)	-0.058** (0.026)
Observations	9,844,049	9,829,911	9,783,497	9,733,920	9,795,707
Panel E: Control Group of Likely Announcers					
After announcement	-0.179*** (0.035)	-0.076** (0.031)	-0.149*** (0.036)	-0.197*** (0.038)	-0.077** (0.033)
Observations	648,284	645,847	641,643	636,965	642,294
Panel F: Match on Job Search, Industry, Likelihood					
After announcement	-0.186*** (0.058)	-0.013 (0.048)	-0.188*** (0.050)	-0.234*** (0.063)	-0.102* (0.053)
Observations	1,094,996	1,089,278	1,082,131	1,073,670	1,083,439
Panel G: Weight by Job Seeker Interest					
After announcement	-0.155*** (0.030)	-0.032 (0.028)	-0.147*** (0.032)	-0.187*** (0.034)	-0.084*** (0.029)
Observations	4,918,142	4,899,323	4,862,322	4,819,454	4,870,004
Panel H: Within-Worker Specification					
After announcement	-0.055 (0.040)	-0.003 (0.033)	-0.097** (0.039)	-0.162*** (0.044)	-0.056 (0.041)
Observations	748,481	746,008	741,191	735,394	742,407
Panel I: Firms with >50 Reviews Pre-Dobbs					
After announcement	-0.127*** (0.030)	0.002 (0.026)	-0.107*** (0.033)	-0.159*** (0.035)	-0.050 (0.031)
Observations	3,632,720	3,616,820	3,586,160	3,548,497	3,592,563
Panel J: Removing Suspect Reviews					
After announcement	-0.102*** (0.028)	-0.004 (0.022)	-0.091** (0.035)	-0.130*** (0.033)	-0.038 (0.029)
Observations	3,356,151	3,342,195	3,315,867	3,287,294	3,321,556

Notes: Table E.8 reports DiD estimates capturing the change Glassdoor ratings between announcing and non-announcing firms after *Dobbs*. Panels A, D-G, and I-J include fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Panels B and G include event-firm and event-quarter fixed effects, with the latter additionally include event-worker fixed effects. Panel C includes fixed effects for event-firm-state-job title, event-state-quarter, event-job-title-quarter, and event-former employee indicator.. In Panel D, we construct a control group based on each announcer's industry and firm size (where size reflects total employees for Glassdoor and total job postings for Indeed). In Panel E, we construct a single control group using the firms with the highest predicted probabilities of announcing based on the estimates of Table E.11. In Panel F, the main control group is reduced to only include firms in the same industry and with a high probability of announcing. In Panel G, control firms are inversely weighted by their rank of closeness to the announcer. In Panel I, we restrict to firms with at least 50 Glassdoor reviews before *Dobbs*. In Panel J, we remove suspect reviews, i.e., months in which the frequency of reviews is more than 5 percent greater than that over the preceding and following three months; under this threshold, 28% of reviews are flagged. All regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Table E.9: Effect of Firms' Announcements on Outbound Search

	Outbound clicks (1)	Outbound applications (2)
Panel A: Main Specification		
After announcement	0.159*** (0.038)	0.112*** (0.025)
Observations	793,152	352,512
Panel B: Match on Industry and Size		
After announcement	0.267*** (0.055)	0.160*** (0.033)
Observations	244,404	108,624
Panel C: Control Group of Likely Announcers		
After announcement	0.356*** (0.059)	0.095** (0.042)
Observations	11,340	5,040
Panel D: Match on Job Search, Industry, Likelihood		
After announcement	0.358*** (0.074)	0.076 (0.055)
Observations	51,318	22,808
Panel E: Weight by Job Seeker Interest		
After announcement	0.153*** (0.039)	0.095*** (0.025)
Observations	793,152	352,512
Panel F: Drop Zero Cells		
After announcement	0.136*** (0.031)	0.080*** (0.030)
Observations	605,945	269,870
Panel G: Extensive Margin		
After announcement	0.028*** (0.007)	0.028*** (0.005)
Observations	793,152	352,512

Notes: Table E.9 reports DiD estimates capturing the change in outbound clicks and outbound application starts between announcing and non-announcing firms after *Dobbs*. Each specification includes event-firm and event-quarter fixed effects. In Panel B, we construct a control group based on each announcer's industry and firm size (where firm size reflects total job postings). In Panel C, we construct a single control group using the firms with the highest predicted probabilities of announcing based on the estimates of Table E.11 and run the regressions on a firm-quarter panel. In Panel D, the main control group is reduced to only include firms in the same industry and with a high probability of announcing. In Panel E, control firms are inversely weighted by their rank of closeness to the announcer. In Panel F, zero cells are excluded. In Panel G, the dependent variable is an indicator for any clicks or any application starts. Observations are cells grouped by firm, quarter, state grouping (trigger vs. non-trigger), and female employment share tercile. All regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Table E.10: Effect of Firms' Announcements on Inbound Search

	Inbound clicks (1)	Inbound applications (2)
Panel A: Main Specification		
After announcement	0.090*** (0.030)	0.109*** (0.027)
Observations	75,655,221	56,612,847
Panel B: Simple Specification		
After announcement	0.108*** (0.036)	0.114*** (0.032)
Observations	81,192,640	60,104,374
Panel C: Saturated Specification		
After announcement	0.066** (0.032)	0.079*** (0.028)
Observations	69,291,731	50,916,831
Panel D: Match on Industry and Size		
After announcement	0.108*** (0.039)	0.080** (0.033)
Observations	45,387,104	33,610,231
Panel E: Control Group of Likely Announcers		
After announcement	0.068** (0.030)	0.084*** (0.028)
Observations	4,928,086	3,528,349
Panel F: Match on Job Search, Industry, Likelihood		
After announcement	0.097*** (0.035)	0.113*** (0.031)
Observations	42,088,934	31,567,476
Panel G: Weight by Job Seeker Interest		
After announcement	0.105*** (0.034)	0.111*** (0.030)
Observations	75,655,221	56,612,847
Panel H: Drop Zero Cells		
After announcement	0.089*** (0.030)	0.114*** (0.026)
Observations	73,708,922	46,157,260
Panel I: Extensive Margin		
After announcement	0.002* (0.001)	0.014*** (0.004)
Observations	75,655,221	56,612,847
Panel J: Exclude Ads with Posted Wages		
After announcement	0.059* (0.033)	0.072** (0.029)
Observations	66,934,544	48,986,314

Notes: Table E.10 reports DiD estimates capturing the change in inbound clicks and application starts on Indeed between announcing and non-announcing firms after *Dobbs*. Each specification in Panels A and D-J includes fixed effects for event-firm, event-state-quarter, and event-job-title-quarter. Panel B includes event-firm and event-quarter fixed effects. Panel C includes event-firm-state-job-title, event-state-quarter, and event-job-title-quarter fixed effects. In Panel D, we construct a control group based on each announcer's industry and firm size (where firm size reflects total employees for Glassdoor and total job postings for Indeed). In Panel E, we construct a single control group using the firms with the highest predicted probabilities of announcing based on the estimates of Table E.11. In Panel F, the main control group is reduced to only include firms in the same industry and with a high probability of announcing. In Panel G, control firms are inversely weighted by their rank of closeness to the announcer. In Panel H, zero cells are excluded. In Panel I, the dependent variable is an indicator for any clicks or any application starts. In Panel J, we exclude ads that have wage information. All regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Table E.11: Predicting if a Firm Announces Reproductive Care, Deriving Alternative Control Set

	(1)
Share Democrat employees	0.006*** (0.001)
Female CEO	0.003*** (0.001)
Female employment share	0.006*** (0.001)
Operates in a trigger state	0.008*** (0.001)
Share of employment in trigger states	-0.011*** (0.001)
Logarithm of firm employment	0.003*** (0.000)
Publicly traded company	0.017*** (0.002)
Aerospace and defense industry	-0.017*** (0.002)
Agriculture industry	-0.016*** (0.002)
Arts, entertainment, and recreation industry	-0.008*** (0.003)
Construction, repair, and maintenance industry	-0.013*** (0.001)
Education industry	-0.017*** (0.001)
Energy, mining, and utilities industry	-0.018*** (0.002)
Financial services industry	-0.010*** (0.002)
Government and public administration industry	-0.018*** (0.001)
Healthcare industry	-0.016*** (0.002)
Hotels and travel accommodation industry	-0.018*** (0.001)
Human resources and staffing industry	-0.012*** (0.002)
Insurance industry	-0.018*** (0.002)
Legal industry	-0.015*** (0.001)
Management and consulting industry	-0.011*** (0.002)
Manufacturing industry	-0.015*** (0.002)
Media and communication industry	-0.005* (0.002)
Nonprofit industry	-0.014*** (0.002)
Personal consumer services industry	-0.008 (0.005)
Pharmaceutical and biotechnology industry	-0.015*** (0.003)
Real estate industry	-0.012*** (0.002)
Restaurants and food services industry	-0.018*** (0.001)
Retail and wholesale industry	-0.004* (0.002)
Telecommunications industry	-0.013*** (0.004)
Transportation and logistics industry	-0.015*** (0.002)
Mean DV	0.005
Firms	56,572
Adjusted R ²	0.02

Notes: Table E.11 reports estimates from an OLS regression of an indicator the firm announced reproductive care and a vector of Glassdoor observables. The omitted industry is Information Technology. Standard errors are bootstrapped. Significance levels: * 10%, ** 5%, *** 1%.

Table E.12: Robustness to Choice of Labor Market Competitors Included in the Control Set

Outcome	Alternative ranks included in control set of employers					
	Top 20	Top 15	Top 10	Top 5	Odd	Even
Panel A: Ratings of management						
After announcement	-0.174*** (0.034)	-0.194*** (0.047)	-0.191*** (0.042)	-0.225*** (0.058)	-0.167*** (0.060)	-0.163*** (0.048)
Panel B: Outbound clicks						
After announcement	0.159*** (0.038)	0.161*** (0.038)	0.158*** (0.038)	0.169*** (0.040)	0.170*** (0.040)	0.163*** (0.036)
Panel C: Outbound applications						
After announcement	0.112*** (0.025)	0.109*** (0.025)	0.097*** (0.025)	0.094*** (0.025)	0.116*** (0.026)	0.121*** (0.024)
Panel D: Inbound clicks						
After announcement	0.090*** (0.030)	0.067** (0.031)	0.068** (0.032)	0.085** (0.040)	0.074** (0.032)	0.082*** (0.028)
Panel E: Inbound applications						
After announcement	0.109*** (0.027)	0.075*** (0.028)	0.073** (0.029)	0.074** (0.036)	0.085*** (0.030)	0.090*** (0.025)

Notes: Table E.12 repeats the baseline specification for each main outcome of interest, adjusting the rank threshold for including non-announcing firms. The estimates reported in the first column (Top 20) mirror the benchmark specification. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Sensitivity of Glassdoor Sample In the main text, we document a decline in ratings of satisfaction disclosed on Glassdoor. In this appendix, we address a number of concerns regarding selection into who reviews and show that our results cannot be rationalized by changes in reviewing behavior after *Dobbs*.

First, we consider the possibility that the announcement altered who chooses to post on Glassdoor, rather than what workers think. To address this concern, we examine all individuals who disclose information on Glassdoor for an announcing firm or for a firm in the control set, and characterize the type of content they contribute.⁵⁰ For each review, we can determine: (i) whether the worker is new to Glassdoor, i.e., this is the first piece of content disclosed on Glassdoor, and (ii) whether this employer review is the first employer review provided by the worker to Glassdoor. If we observed a shift in either measure for announcers after *Dobbs*, it would suggest that selection in who reports to Glassdoor is one mechanism behind the decline in satisfaction. However,

⁵⁰Workers can disclose an employer review, pay report, interview report, or fringe benefit rating on Glassdoor.

difference-in-differences estimates for both outcomes, reported in Table E.13, are not statistically significant and are near zero—suggesting announcing did not cause a shift in who reviews.

Table E.13: Effect of Firms’ Announcements on Attracting Workers to Glassdoor

	New to Glassdoor	First employer review
	(1)	(2)
After announcement	-0.004 (0.006)	0.002 (0.005)
Mean DV	0.588	0.823
Observations	9,984,689	9,984,689

Notes: Table E.13 reports DiD estimates capturing the change in the likelihood of being new to Glassdoor (column 1) or reflecting the first employer review the worker has written on Glassdoor (column 2) between announcing and non-announcing firms after *Dobbs*. Each specification includes fixed effects for event-firm and event-quarter. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Second, given the opportunity cost of submitting an employer review, workers may only come forward with a review if their responses are particularly extreme. The “give-to-get” framework reduces extreme response bias (Marinescu *et al.*, 2021). However, it is possible that announcing causes workers with the most extreme responses to review, whereas those with more moderate responses do not. In particular, if workers perceive highly negative reviews as a strategic tool to punish their employer or impede recruitment (Benson *et al.*, 2020; Sockin and Sojourner, 2023), the observed decline could reflect strategic reporting rather than a generalized decline in sentiment. We rule out this possibility by examining the response in overall satisfaction ratings, rather than in management or culture-specific ratings. If the decline we document reflects a shift toward extreme responses or strategic retribution (whether from actual employees or online trolling), we would expect a shift toward the most negative responses: one-star reviews. Instead, while the average overall rating declines, this appears to reflect a broad shift toward one-, two-, and three-star reviews.

In Table E.15, we consider whether there is a differential response in ratings of management by whether the reviewer chooses to conceal their job title or location. Although there is a more negative response among concealers, the effect is not driven by them alone. That there is a larger

Table E.14: Effect of Firms' Announcements on the Distribution of Overall Satisfaction Ratings

	Overall rating	1(Overall star rating)				
		One	Two	Three	Four	Five
	(1)	(2)	(3)	(4)	(5)	(6)
After announcement	-0.093*** (0.025)	0.011 (0.007)	0.011* (0.006)	0.006 (0.006)	0.002 (0.007)	-0.030*** (0.008)
Mean DV pre-2022	3.617	0.097	0.099	0.211	0.275	0.318
Observations	6,854,574	6,854,574	6,854,574	6,854,574	6,854,574	6,854,574

Notes: Table E.14 reports DiD estimates for Glassdoor ratings of overall job satisfaction, as well as indicators for each star rating. Each specification includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

response among concealers may reflect workers' concerns over employer retaliation for speaking out (Sockin and Sojourner, 2023; Sockin *et al.*, 2022).

Table E.15: Effect on Management Ratings, Concealing of Personally-Identifying Information

	Simple specification		Main specification	
	(1)	(2)	(3)	(4)
After announcement	-0.148*** (0.027)	-0.100*** (0.024)	-0.174*** (0.034)	-0.125*** (0.029)
After announcement x 1(Conceals PII)		-0.109*** (0.033)		-0.082** (0.034)
1(Conceals PII)		-0.042*** (0.007)		-0.089*** (0.034)
Observations	7,449,140	7,449,140	4,819,454	4,819,454

Notes: Table E.15 reports DiD estimates for senior management ratings between announcing and non-announcing firms after *Dobbs*, with interaction terms for the worker concealed their job title or location. Simple specification includes event-firm and event-quarter fixed effects. Main specification includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Third, we consider whether the announcement affected review volume by prompting workers to report their experiences, even if it did not change the composition of reviewers. To explore this possibility, we construct a firm-half year panel for the number of employer reviews written on Glassdoor. We first examine the intensive margin, estimating a difference-in-differences design for the logarithm of the number of reviews among firms that always record at least one review.

The results, recorded in the first two columns of Table E.16, suggest there is no shift in the number of reviews received after announcing.

Table E.16: Effect of Firms' Announcements on the Arrival of Glassdoor Reviews

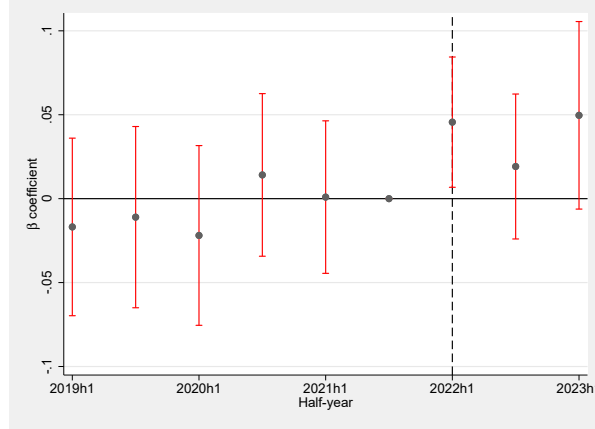
	Total reviews in logs		1(Has review)		Senior management rating	
	Always ≥ 1					
	(1)	(2)	(3)	(4)	(5)	(6)
After announcement	-0.018 (0.052)	-0.016 (0.062)	0.033** (0.015)	0.016 (0.011)	-0.175*** (0.034)	-0.314*** (0.067)
After announcement x Ln(Firm size)		-0.006 (0.032)		-0.020*** (0.006)		0.057*** (0.016)
Pre-announcement average	3.54	3.54	0.86	0.86	3.47	3.47
Announcing firms	225	225	330	330	348	348
Observations	31,122	31,122	42,822	42,822	4,816,297	4,816,297

Notes: Table E.16 reports DiD estimates for the arrival of Glassdoor reviews and ratings of management between announcing and non-announcing firms after *Dobbs*, allowing for heterogeneous effects by firm size. Each specification in columns (1)-(4) includes fixed effects for event-firm and event-industry-half year. Each specification in columns (5)-(6) includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. The logarithm of firm size has been demeaned. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

To assess the extensive margin, we estimate a difference-in-differences design with an indicator for receiving at least one review as the outcome. In column (3) we find a significant increase of 3.3 percentage points (relative to a baseline of 86%) in the probability of receiving a review. An event study for this outcome, displayed in Figure E.2, suggests there are parallel trends between announcers and non-announcers before *Dobbs*, followed by a positive jump thereafter. Consistent with the fact that smaller firms receive reviews less frequently, column (4) shows that this effect is concentrated among smaller firms.

To put this result into context, the last two columns of Table E.16 present the corresponding difference-in-differences results under the benchmark specification, allowing for heterogeneous effects by firm size. Two takeaways emerge. First, because declines in satisfaction occur across the distribution of firm size, whereas only smaller firms experience an increase in the arrival of reviews, it cannot be that the overall decline in satisfaction simply reflects an increased flow of reviews following these announcements. Second, because the decline in satisfaction is larger for smaller firms, the accelerated arrival of reviews among these firms may plausibly contribute to the larger magnitude of the observed decline among smaller firms.

Figure E.2: Event Study of Firms' Announcements on Receiving a Glassdoor Review



Notes: Figure E.2 plots the estimated impact on the likelihood each half-year of receiving a Glassdoor review, corresponding to column (3) of Table E.16. Regression is weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-value for the test of joint significance for all pre-*Dobbs* coefficients is 0.768.

We next show the decline in sentiment persists even within worker. In Table 3, we document a decline under a simple specification with only firm and year-quarter fixed effects. A benefit of working with this simpler model is that it provides sufficient statistical power to account for unobservable differences among reviewers by incorporating a fixed effect for each worker. Doing so restricts the sample to workers who provide multiple reviews, thereby removing workers for whom reporting sentiment was a one-time decision—perhaps out of a new and acute sense of frustration. In Panel H of Table E.8, we find sentiment declines similarly under this alternative specification.

Next, we show that our results are not driven by small-sample variation by restricting the analysis in Panel I of Table E.8 to firms with at least 50 pre-*Dobbs* reviews. Although the estimated declines in Glassdoor ratings are smaller in magnitude, they persist.

Last, we consider the possibility that some reviews may have been disingenuously posted as sock puppetry. Channeling Sockin and Sojourner (2023), in Panel J of Table E.8, we exclude reviews from months in which the frequency of reviews is more than 5 percent greater than that over the preceding and following three months; under this conservative threshold, 28% of reviews are flagged as suspect. Removing these reviews does not alter the takeaway results.

Intensive and Extensive Margins of Job Search Since inbound and outbound clicks and applications on Indeed can yield zero-value outcomes, we disentangle effects across both the extensive and intensive margins and assess robustness to our treatment of zero cells. The extensive margin captures whether there is *any* click or application recorded, while the intensive margin captures *how many* are recorded.

To capture the intensive margin alone for firms that have at least one click or application, in Panel F of Table E.9 and Panel H of Table E.10, we exclude cells with zero values and still document significant positive effects.

To capture the extensive margin, in Panel G of Table E.9 and Panel I of Table E.10, we change the outcome of interest to be an indicator equal to one if there are any clicks or applications recorded, and zero otherwise. The estimates are significant and positive, indicating an increase in there being any search activity for announcers, both for inbound and outbound search. Our qualitative takeaways are thus not sensitive to the treatment of zeros, and we interpret this as further evidence of newfound sorting caused by the firm’s announcement. In Tables E.17 and E.18, we allow for heterogeneous effects along the extensive margin by firm size. We find that small firms experience a more pronounced extensive margin effect. This jump in any clicks or applications could help explain the larger impacts we observe for smaller firms in the main text.

Table E.17: Effect of Announcements on Outbound Search by Firm Size (Extensive Margin)

	Any outbound clicks		Any outbound applications	
	(1)	(2)	(3)	(4)
After announcement	0.028*** (0.007)	0.016*** (0.005)	0.028*** (0.005)	0.016*** (0.005)
After announcement x Ln(Firm size)		-0.010*** (0.003)		-0.003* (0.002)
Observations	793,152	640,980	352,512	284,880

Notes: Table reports DiD estimates in which the dependent variable is an indicator of any clicks or application starts by workers employed at announcing versus non-announcing firms after *Dobbs*, allowing for heterogeneous effects by firm size. Observations are cells grouped by firm, quarter, state grouping (trigger vs. non-trigger), and female employment share tercile. Each specification includes event-firm and event-quarter fixed effects. The logarithm of firm size is proxied for by total postings in 2019 and has been demeaned. Even columns have fewer observations because some firms had no postings in 2019. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Table E.18: Effect of Announcements on Inbound Search by Firm Size (Extensive Margin)

	Any inbound job seeker clicks		Any inbound job seeker applications	
	(1)	(2)	(3)	(4)
After announcement	0.002*	0.001	0.014***	0.013***
	(0.001)	(0.001)	(0.004)	(0.004)
After announcement x Ln(Firm size)		-0.001***		-0.003
		(0.000)		(0.002)
Observations	75,655,221	69,479,336	56,612,847	50,500,097

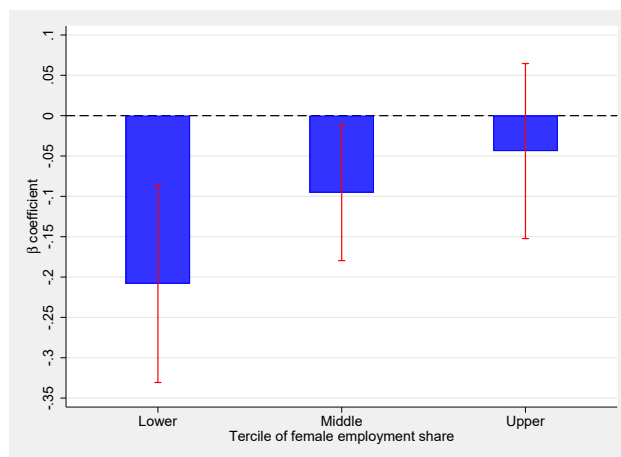
Notes: Table reports DiD estimates in which the dependent variable is an indicator of any clicks or application starts on job postings at announcing versus non-announcing firms after *Dobbs*, allowing for heterogeneous effects by firm size. Each specification controls for the logarithm of total job postings and includes fixed effects for event-firm, event-state-quarter, and event-job-title-quarter. The logarithm of firm size is proxied for by total postings in 2019 and has been demeaned. Even columns have fewer observations because some firms had no postings in 2019. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Posted Wages Not Driving Inbound Search Given that there is an increase in posted wages among announcers after *Dobbs*, it is plausible that the growth in wages drives the increased interest from outside job seekers in the firm's vacancies (Belot *et al.*, 2022). To rule this mechanism out, in Panel J of Table E.10 we estimate the effects on inbound clicks and applications when we exclude all ads that include wage information. Even with these postings excluded, we still document positive and significant estimates.

F Additional Heterogeneity Results

Glassdoor Sentiment by Gender Terciles In Figure F.1, we expand upon the heterogeneity test presented in Panel A of Table 6. More specifically, we estimate separate coefficients for each tercile of female employment share and test for differences between them, as opposed to testing for differences between the upper tercile and the other two. We can observe that ratings of management fall the most (0.21 stars) for male-dominated jobs and the least (0.04 stars) for female-dominated jobs. The difference between the two is statistically significant at conventional levels. For jobs that are more gender-balanced, the decline (0.10 stars) falls between the two.

Figure F.1: Effect on Management Ratings by Tercile of Female Employment Share



Notes: Figure F.1 displays the estimated impact from equation (1) on management ratings under the benchmark specification, which includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Regression is weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-values for the test of equality between coefficients are: lower and middle terciles = 0.092; middle and upper terciles = 0.409; lower and upper terciles = 0.030.

CEO Political Affiliation In Table F.1, we consider characteristics of the CEO of the announcing firm. This includes whether the CEO is female and whether the CEO donated only to Democrat political candidates. We observe no significant difference along either dimension.

Announcement Features In Table F.2, we consider other content-related aspects of the announcement as it pertains to ratings of management, inbound search behavior, and outbound

Table F.1: Effect on Management Ratings, Heterogeneity by Characteristics of the CEO

	Senior management rating	
	(1)	(2)
After announcement	-0.152*** (0.046)	-0.165*** (0.040)
After announcement x 1(CEO donates only to Democrats)	0.015 (0.070)	
After announcement x 1(Female CEO)		0.038 (0.079)
Observations	1,479,317	4,166,141

Notes: Table F.1 reports DiD estimates for senior management ratings between announcing and non-announcing firms after *Dobbs*, with interactions as indicated in the first column. There are 63 announcers with a CEO that only donated to Democrats and 49 with a female CEO. Each specification includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

search behavior. We find that the effects on job search are significantly larger when the announcement was made on social media, consistent with media acting as an amplification message for reaching workers. We find no differential effect by whether the announcement listed a specific dollar amount covered for abortion care, nor when the announcement also introduced additional family-friendly fringe benefits such as IVF, adoption, or egg freezing, further suggesting that the response of workers to these announcements was not driven by fringe benefit concerns.

Table F.2: Heterogeneity in Effects of Firms' Announcements by Content of Announcements

	Management ratings		Outbound clicks		Inbound clicks	
	(1)	(2)	(3)	(4)	(5)	(6)
After announcement	-0.174*** (0.034)	-0.104*** (0.037)	0.159*** (0.038)	0.114** (0.054)	0.090*** (0.030)	0.029 (0.040)
After announcement x 1(Announces dollar amount covered)		-0.135 (0.104)		-0.088 (0.111)		0.024 (0.065)
After announcement x 1(Announces family-friendly fringe benefits)		0.073 (0.087)		-0.039 (0.096)		0.094 (0.061)
After announcement x 1(Announces on social media)		-0.112 (0.068)		0.151** (0.069)		0.140*** (0.050)
After announcement x 1(Announces donating to organizations)		-0.297 (0.215)		-0.071 (0.125)		0.218 (0.149)
Observations	4,819,454	4,819,454	793,152	793,152	75,655,221	75,655,221

Notes: Table F.2 reports DiD estimates for senior management ratings between announcing and non-announcing firms after *Dobbs*, allowing for heterogeneous effects by the content of the announcement as indicated in the first column. There are 47 firms announcing a specific dollar amount, 29 announcing additional family-friendly fringe benefits (i.e., mentions adoption, cryopreservation, fertility, freezing, IVF, or surrogacy), 168 announcing on LinkedIn, and 26 announcing donating to organizations. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Recruitment Intensity by Firm Size In Table F.3, we consider whether announcers differ in recruiting intensity by their size. The odd columns reproduce the estimates from Table 9, and the even columns incorporate an interaction with the logarithm of firm size. We find no differential effects by firm size for any outcome.

Table F.3: Recruiting Intensity, Allowing for Heterogeneity by Firm Size

	1(Posts ad)		Total ads in logs Always ≥ 1		Share ads in trigger states Always ≥ 1		Share ads in top female tercile Always ≥ 1	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After announcement	0.028 (0.021)	0.026 (0.020)	-0.055 (0.101)	-0.046 (0.117)	-0.002 (0.010)	-0.003 (0.012)	-0.001 (0.012)	-0.001 (0.016)
After announcement x Ln(Firm size)		-0.013 (0.014)		-0.015 (0.045)		0.002 (0.005)		0.001 (0.007)
Pre-announcement mean DV	0.90	0.90	7.82	7.82	0.20	0.20	0.34	0.34
Announcing firms	273	273	202	202	202	202	202	202
Observations	31,401	31,401	23,499	23,499	23,499	23,499	23,499	23,499

Notes: Table 9 reports the DiD estimates capturing the change in job ads between announcing versus non-announcing firms after *Dobbs*, allowing for heterogeneous effects by the size of the announcing firm. Observations are firm-quarter cells. Each specification includes fixed effects for event-firm and event-industry-half-year. The logarithm of firm size is proxied for by total postings in 2019 and has been demeaned. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Wages for New Versus Incumbent Workers and Wage Heterogeneity In Table F.4, we consider whether there is heterogeneity in the dynamics of posted wages along three dimensions: (i) whether the job title is in the top tercile of female employment share, (ii) whether the job is in a trigger state, and (iii) whether management ratings for the announcer declined more than the median. We find no significant differences along any of the three cases.

Table F.4: Effect of Firms' Announcements on Posted Wages

	Logarithm of posted wages			
	(1)	(2)	(3)	(4)
After announcement	0.036*** (0.010)	0.023** (0.010)	0.032*** (0.010)	0.035** (0.015)
After announcement x 1(Top female share tercile)		0.023 (0.019)		
After announcement x 1(Trigger state)			0.017 (0.012)	
After announcement x 1(Management rating decline worse than median)				0.001 (0.019)
Observations	6,439,390	6,339,195	6,439,390	6,439,390

Notes: Table F.4 reports DiD estimates capturing the change in the logarithm of posted wages on Indeed (using the midpoint in the case of a wage range) between announcing and non-announcing firms after *Dobbs*. Observations are individual job postings, weighted such that each announcing firm is given equal weight. Each specification includes fixed effects for event-firm-state-job title, event-state-quarter, event-job title-quarter, and event-hourly wage indicator. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

In Table F.5, we consider whether there is heterogeneity in the dynamics of realized wages, both for incumbent workers as well as new hires, along three dimensions: (i) whether the job title is in the top tercile of female employment share, (ii) whether the job is in a trigger state, and (iii) whether management ratings for the announcer declined more than the median decline. We find no clear differences along any of the dimensions.

Table F.5: Effect of Firms' Announcements on Realized Pay for Incumbents and New Hires

	Logarithm of realized wage			
	(1)	(2)	(3)	(4)
After announcement	-0.007* (0.004)	-0.008* (0.004)	-0.007** (0.004)	-0.007* (0.004)
After announcement $\times$ 1(New hire)	0.017*** (0.005)	0.018*** (0.006)	0.016*** (0.005)	0.023*** (0.006)
After announcement $\times$ 1(Top female share tercile)		0.005 (0.006)		
After announcement $\times$ 1(Trigger state)			0.005 (0.005)	
After announcement $\times$ 1(Management rating decline worse than median)				0.002 (0.007)
After announcement $\times$ 1(New hire) $\times$ 1(Top female share tercile)		-0.002 (0.009)		
After announcement $\times$ 1(New hire) $\times$ 1(Trigger state)			0.004 (0.007)	
After announcement $\times$ 1(New hire) $\times$ 1(Management rating decline worse than median)				-0.012 (0.009)
Observations	7,748,166	7,748,166	7,748,166	7,748,166
Adjusted R ²	0.88	0.88	0.88	0.88

Notes: Table F.5 reports DiD estimates for log total pay from Glassdoor pay reports between announcing and non-announcing firms after *Dobbs*, with interaction terms as indicated in the first column. Each specification includes fixed effects for event-firm-state-job title, event-state-quarter, event-job-title-quarter, event-hourly wage indicator, and event-new hire indicator. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

G Evolution of Sales for Announcing Firms in Compustat

Was there an effect from these announcements on firm performance? To answer this question, we use data from Compustat on sales and sales per worker, the latter as a proxy for average labor productivity (Cronqvist *et al.*, 2009), since a rise in firm productivity could explain the uptick in posted wages (given the close link between labor productivity and wages). Estimating a standard DiD with non-announcing firms in Compustat as the control set reveals limited evidence that announcers experienced a material change in sales or labor productivity (Table G.1). When we estimate event studies, we find no evidence of pre-trends and no evidence of effects consistent with the DiD estimates (available on request). This null result holds even when looking only within the retail sector, where products are sold directly to consumers and a shift in preferences for employers' products would be most likely to materialize. Thus, productivity growth from rising sales does not appear to rationalize the increase in announcers' posted wages.

Table G.1: Effect of Sales and Productivity for Announcers in Compustat

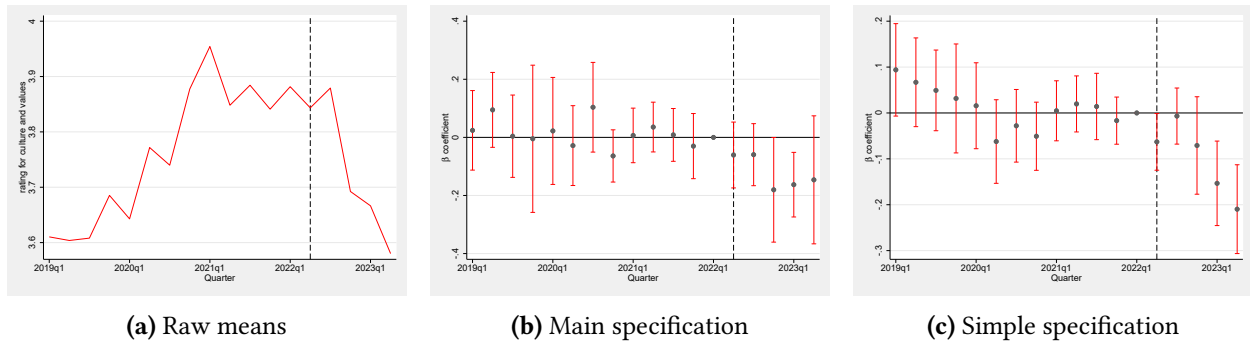
	All sectors		Retail sector	
	Log sales	Log sales per worker	Log sales	Log sales per worker
After announcement	-0.002 (0.023)	-0.014 (0.024)	0.042 (0.036)	-0.013 (0.043)
Firm FE	✓	✓	✓	✓
Four-digit NAICS x Year-Quarter FE	✓	✓	✓	✓
N	91,640	91,640	3,390	3,390
Adjusted R ²	0.97	0.86	0.98	0.85

Notes: Table G.1 reports DiD reports the estimated impact on the logarithm of sales and sales per worker between announcing and non-announcing firms after *Dobbs*. Regressions are weighted to give each announcing firm equal weight. Quarterly firm sales and annual firm employment are from Compustat. Standard errors are clustered by firm. Significance levels: * 10%, ** 5%, *** 1%.

H Additional Results

In this appendix, we present additional summary information and outcomes. In Figure H.1, we replicate the analyses of Figure 1, but with ratings of culture as the outcome of interest as opposed to ratings of management. We observe a similar stark drop in the raw means in Panel (a) and clear event studies that show no evidence of pre-trends and reductions in ratings for culture thereafter.

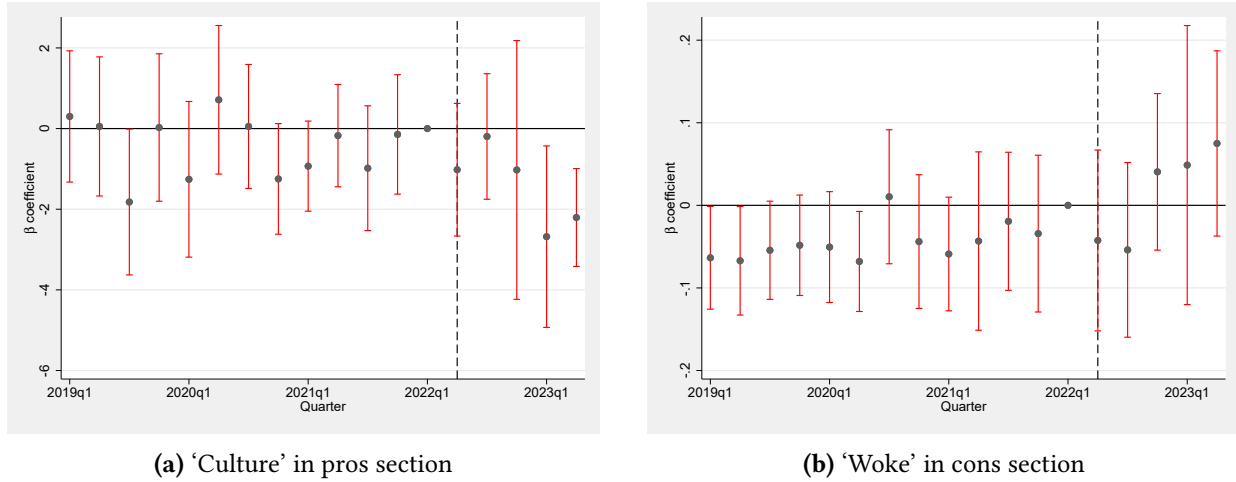
Figure H.1: Raw Means and Event Study of Firms' Announcements on Culture Ratings



Notes: Figure H.1 plots the mean Glassdoor culture rating for announcers each year-quarter over the sample window in panel (a), equally weighting each announcer. The figure also reports the estimated impact from equation (1) on culture ratings under the benchmark specification in panel (b) and the simple specification in panel (c). Panel (b) includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Panels (c) includes event-firm and event-quarter fixed effects. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-value for the test of joint significance for all pre-*Dobbs* coefficients is 0.277 for the main specification and 0.104 for the simple specification.

Figure H.2 plots dynamic estimates for whether employer reviews on Glassdoor mention 'culture' in the pros section or 'woke' in the cons section. We caveat that these events are rare: the baseline rates for being mentioned are 12% for the former and 0.06% for the latter.

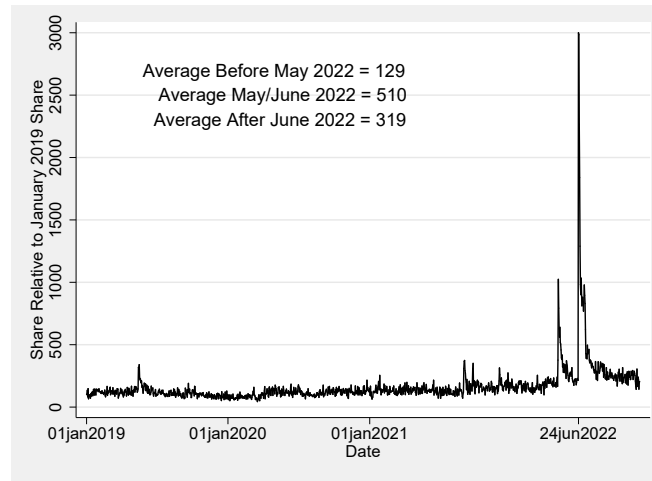
Figure H.2: Event Study of Firms' Announcements on Culture-Related Phrases



Notes: Figure 3 plots the DiD estimates capturing the change in the incidence of the phrase 'culture' in the Pros section (panel a) and 'woke' in the Cons section (panel b) of Glassdoor reviews between announcing versus non-announcing firms after *Dobbs*. Each specification includes event-firm and event-quarter fixed effects. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-value for the test of joint significance for all pre-*Dobbs* coefficients is 0.155 for panel (a) and 0.421 for panel (b).

To assess the saliency of abortion for job seekers, Figure H.3 records the fraction of job seekers on Indeed who enter abortion-related terms into the Search Bar of the platform before and after *Dobbs*. Values are normalized relative to the level in January 2019. We observe two spikes, when the ruling was leaked on May 2, 2022, and when the ruling was handed down on June 24, 2022.

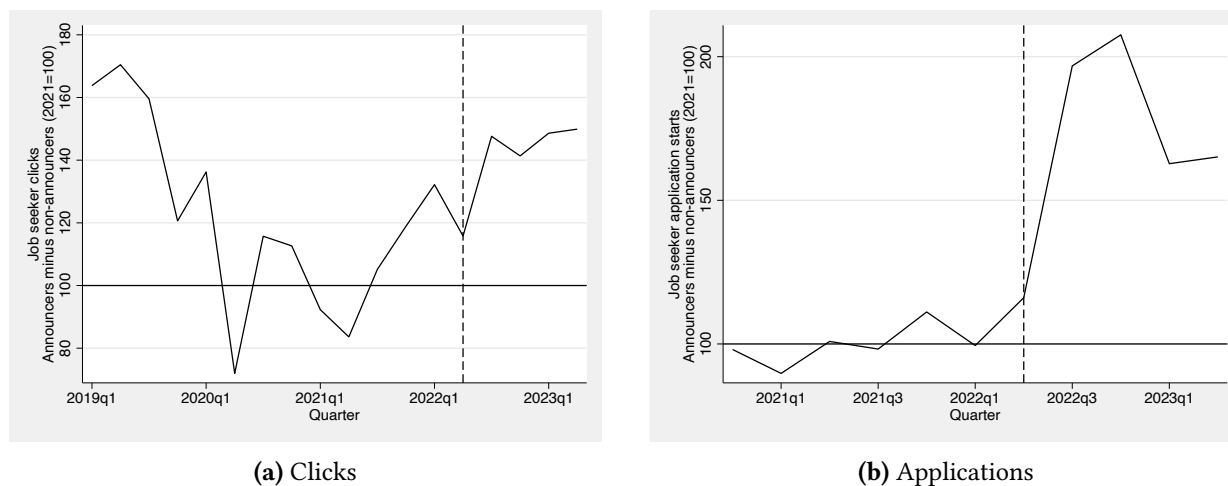
Figure H.3: Abortion-Related Terms in the Search Bar on Indeed Before and After *Dobbs*



Notes: Figure H.3 reports the number of searches by workers on Indeed that include the word "abortion" or other related words, as a share of the total number of such searches on January 1, 2019. The *Dobbs* ruling occurred on June 24, 2022 as indicated in the graph, and the leak occurred on May 2, 2022.

In Figure H.4, we plot the difference in inbound clicks and application starts between announcers and non-announcers over time. For disclosure reasons, we cannot plot raw means for announcers. Values are normalized to the 2021 level. In Panel (b), the jump in application starts just after the announcements is particularly stark. These results suggest that the impacts of announcements are clearly apparent even in the raw means.

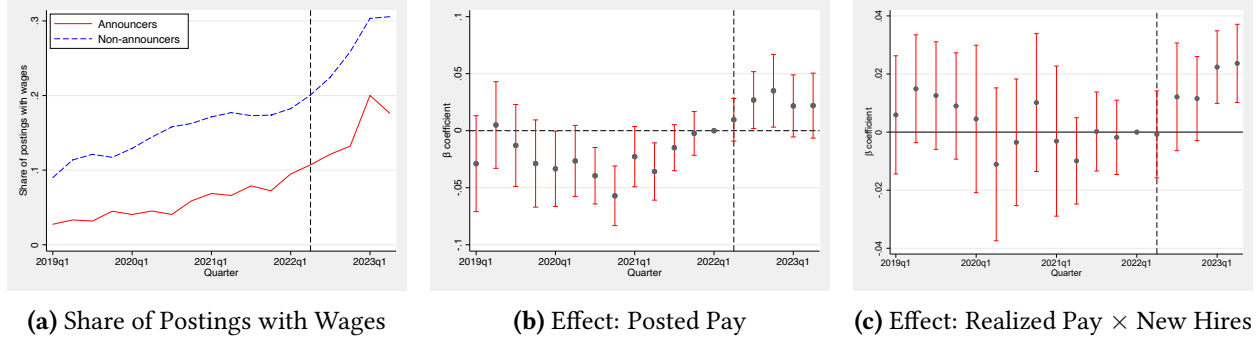
Figure H.4: Raw First Differences for Job Seeker Clicks and Applications



Notes: Figure H.4 plots the first difference in the average number of inbound clicks in panel (a) and the average number of inbound application starts in panel (b) for job postings between announcing and non-announcing firms. We cannot show trends in the raw means by group for disclosure reasons. Firms are equally weighted.

In Figure H.5, we plot the share of ads with posted wage information for announcers and non-announcers over time, as well as event-study DiD estimates for posted wages and realized wages for new hires. We observe an increase in posted wages and realized wages for new hires among announcing firms after *Dobbs*.

Figure H.5: Share of Postings with Wages and Effect of Announcing on Pay



Notes: Figure H.5 panel (a) reports the share of job postings with wage information included in the posting over time. Announcing firm trends are depicted by the solid line, and non-announcing firms are depicted by the dashed line. Panels (b) and (c) plot the DiD estimates for the logarithm of posted base pay in job postings on Indeed (using the midpoint in the case of a wage range) and the logarithm of realized pay for workers with less than 2 years of experience, respectively, between announcing and non-announcing firms after *Dobbs*. Each specification includes event-firm-state-job title, event-state-quarter, and event-job-title-quarter fixed effects. Panel (c) additionally includes event-new hire indicator fixed effects. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Vertical bars indicate 95% confidence intervals around each point estimate. The p-value for the test of joint significance for all pre-*Dobbs* coefficients in panel (b) is 0.001 and in panel (c) is 0.163.

In Table H.1, we provide summary statistics for the Glassdoor data at the review level and firm level, as well as the Indeed dataset for outbound search, inbound search, and posted wages.

Table H.1: Summary Statistics for Glassdoor and Indeed Datasets

Measure	Announcing firms			Non-announcing firms		
	N	Mean	Standard deviation	N	Mean	Standard deviation
<i>Panel A. Glassdoor review level data</i>						
Overall rating	524,867	3.75	1.31	9,460,004	3.64	1.27
Career opportunities rating	391,995	3.65	1.37	7,176,608	3.50	1.35
Compensation and benefits rating	390,464	3.75	1.25	7,156,682	3.61	1.25
Culture and values rating	388,177	3.74	1.44	7,113,323	3.52	1.42
Senior leadership rating	385,193	3.39	1.49	7,064,140	3.16	1.45
Work-life balance rating	388,452	3.57	1.41	7,124,617	3.40	1.41
Female employment share	465,578	0.46	0.22	8,389,276	0.47	0.24
Share Democrat employees	516,516	0.87	0.18	9,290,932	0.73	0.24
Logarithm of firm employment	524,867	9.22	2.78	9,457,052	10.37	2.02
1(Current employee)	524,867	0.63	0.48	9,460,004	0.62	0.48
1(Trigger state)	329,199	0.18	0.39	5,831,417	0.24	0.43
1(Majority Democrat 2020 state vote share)	329,199	0.69	0.46	5,831,417	0.58	0.49
1(Top female share tercile)	465,578	0.28	0.45	8,389,276	0.33	0.47
1(Conceals PII)	524,867	0.42	0.49	9,460,004	0.41	0.49
<i>Panel B. Glassdoor firm level data</i>						
Female employment share	436	0.29	0.13	436,364	0.27	0.34
Share Democrat employees	361	0.88	0.21	119,582	0.61	0.45
Logarithm of firm employment	437	7.06	2.84	498,196	4.31	1.62
1(Publicly traded company)	437	0.46	0.50	506,628	0.13	0.33
1(Female CEO)	359	0.17	0.38	147,990	0.16	0.37
1(Operates in trigger state)	436	0.77	0.42	459,477	0.28	0.45
Share employment in trigger states	436	0.10	0.15	459,477	0.17	0.34
Share of CEO donations to Democratic party	63	0.87	0.30	4,403	0.53	0.49
<i>Panel C. Indeed firm-quarter-female share tercile-trigger state status level data on outbound search</i>						
Logarithm of clicks	40,284	4.4	3.97	752,868	5.89	4.13
Logarithm of application starts	40,284	3.04	3.46	752,868	4.19	3.78
1(Top female share tercile)	40,284	0.33	0.47	752,868	0.33	0.47
1(Trigger state)	40,284	0.5	0.5	752,868	0.5	0.5
<i>Panel D. Indeed firm-state-job title-quarter level data on inbound search</i>						
Logarithm of clicks	2,634,799	3.94	1.67	78,557,904	4.15	1.76
Logarithm of application starts	1,889,499	2.23	1.62	58,214,932	2.32	1.73
1(Top female share tercile)	2,537,414	0.33	0.47	75,984,720	0.35	0.48
1(Trigger state)	2,634,799	0.22	0.41	78,557,904	0.25	0.43
<i>Panel E. Indeed job postings with posted wages data</i>						
Logarithm of the posted hourly wage	137,382	7.68	0.44	3,452,133	7.56	0.42
Logarithm of the posted annual salary	21,236	16.11	0.52	3,335,427	15.99	0.57
1(Top female share tercile)	152,709	0.5	0.5	6,623,903	0.46	0.5
1(Trigger state)	158,618	0.18	0.39	6,787,560	0.23	0.42

Notes: Table H.1 displays the total observations, mean, and standard deviation for each observable in the Glassdoor and Indeed data. Panel A summarizes the dataset of reviews. Panel B summarizes across firms, rather than reviews. Panel C summarizes the dataset for studying outbound job search on Indeed. Panel D summarizes inbound job seeker clicks and applications on Indeed. Panel E summarizes the dataset of job listings with posted wages on Indeed.

Table H.2 considers alternative approaches to handling standard errors for the DiD estimate on each Glassdoor rating category, including adjusting for multiple hypothesis testing. Our results are robust to these alternative approaches to standard errors.

Table H.2: Effect on Glassdoor Ratings, Alternative Approaches to Standard Errors

	Career opportunities	Compensation & benefits	Culture & values	Senior management	Work-life balance
	(1)	(2)	(3)	(4)	(5)
Panel A: Main Specification					
Cluster by announcing-firm sub-sample and employer	0.0000	0.3227	0.0000	0.0000	0.0155
Cluster by employer	0.0000	0.3167	0.0000	0.0000	0.0154
Cluster by employer, Bonferroni-Holm multiple hypotheses	0.0000	0.3167	0.0000	0.0000	0.0308
Panel B: Simple Specification					
Cluster by announcing-firm sub-sample and employer	0.0000	0.1689	0.0001	0.0000	0.0031
Cluster by employer	0.0000	0.1455	0.0000	0.0000	0.0015
Cluster by employer, Bonferroni-Holm multiple hypotheses	0.0000	0.1455	0.0001	0.0000	0.0031

Notes: Table H.2 reports the corresponding p-values for DiD estimates of the sub-category star ratings on Glassdoor between announcing and non-announcing firms after *Dobbs*. Panel A includes fixed effects for event-firm, event-state-quarter, event-job-title-quarter, and event-former employee indicator. Panel B includes event-firm and event-quarter fixed effects. All regressions are weighted to give each announcing firm equal weight. Standard errors are as described in the first column.

In Table H.3, we report the point estimates, standard errors, and sample means (for non-announcers) behind Figure 2 panel (d). The econometric specification follows,

$$Y_{k,t,a} = \sum_{t > 2022:Q2} \beta_t \mathbb{1}\{k \in \mathbb{A}\} + \lambda_{k,a} + \lambda_{t,a} + v_{k,t,a}, \quad (8)$$

where our outcome of interest $Y_{k,t,a}$ is the fraction of workers employed with firm k in period t that were employed with firm k in 2022:Q2 according to their resumes, for the grouping a of an announcing firm and its 20 matched comparison firms. The sample period is 2022:Q2 through 2023:Q4, and by construction, $Y_{k,t,a} = 100$ for all firms in 2022:Q2. As was clear from Figure 2 panel (d), we observe a significant drop in retention among announcing firms compared with non-announcing firms that levels off at about 3.7 percentage points one-and-a-half years later.

Last, Table H.4 estimates the value of clicks and applications by posted wage equivalence.

Table H.3: Effect of Firms' Announcements on Employee Turnover

	Fraction of workers still employed with firm
	(1)
After announcement x 2022:Q3	0.461 (0.302)
After announcement x 2022:Q4	0.508 (0.655)
After announcement x 2023:Q1	-2.206** (1.088)
After announcement x 2023:Q2	-3.569*** (1.072)
After announcement x 2023:Q3	-3.764*** (1.121)
After announcement x 2023:Q4	-3.693*** (1.052)
Mean DV for non-announcers in 2023:Q4	47.84
Observations	49,728

Notes: Table H.3 lists the estimated mean gap in the percentage of remaining workers over time who were employed with the firm in June 2022 at announcing versus non-announcing firms, where observations are firm-quarter cells. Each panel includes event-firm and event-quarter fixed effects. Regressions are weighted to give each announcing firm equal weight. Standard errors are two-way clustered by event and employer. Significance levels: * 10%, ** 5%, *** 1%.

Table H.4: Elasticity of Job Seeker Clicks and Applications with Respect to the Posted Wage

	Logarithm of clicks		Logarithm of applications	
	(1)	(2)	(3)	(4)
Logarithm of the posted wage	0.599*** (0.002)	0.694*** (0.003)	0.367*** (0.002)	0.443*** (0.003)
Job title FE	✓	✓	✓	✓
Job characteristics	✓	✓	✓	✓
Firm FE		✓		✓
Observations	4,552,795	4,125,983	4,513,089	4,086,431

Notes: Table reports the elasticity of clicks and application starts with respect to posted wages in our dataset in 2019. Based on column (2), a 10% increase in the posted wage is associated with a 6.8% increase in clicks ($= (1.10^{0.694} - 1) * 100\%$). Thus, our estimated 9.4% (9.0 log point) rise in clicks (column (1) of Table 7) is equivalent to the increase in clicks that would result from a 13.2% increase in the posted wage ($= ((1.090)^{1/0.694} - 1) * 100\%$). Based on column (4), the equivalent estimate for application starts is 26.3% ($= ((1.109)^{1/0.443} - 1) * 100\%$). Job characteristics include posting duration, a dummy for whether the job is paid hourly, county, and calendar month. The specification in column (2) is similar to that in column (5) of Table 6 in [Marinescu and Wolthoff \(2020\)](#), who use CareerBuilder.com data. Robust standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%.

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