

Generative AI and Labor Market Matching Efficiency

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Most recent draft [here](#).

Abstract

Reductions in private search costs due to advances in information technology can theoretically improve market efficiency. However, this improvement is not guaranteed—changes in private search costs can have unclear welfare implications if they lead to negative externalities. We consider the effects of the introduction of a generative AI tool in a large scale field experiment on an online labor market, which lowered employers’ search costs by randomly offering them AI-written first drafts of their job post. The assistance was widely accepted and treated employers were 19% more likely to post a job. It also saved employers time—treated employers spent 44% less time writing their job posts. Despite the substantial increase in job posts, there was no discernible increase in matches in the treatment group. The lack of match formation was mostly due to marginal jobs being posted by employers with lower hiring intent, while up to a fifth of missing matches resulted from lowered hiring probability among inframarginal jobs. We provide evidence that the treated job posts were more generic and less informative to jobseekers. This combination of increased job post volume and reduced informativeness diluted signals of employer seriousness. A time-cost accounting implies jobseeker application time costs are roughly six times larger than employer time savings per treated employer. In a subsequent platform roll-out expanding the access of the AI tool, posting responses attenuated while lower hiring conditional on posting remained, suggesting that learning and increased adoption did not eliminate these frictions.

1 Introduction

Economic decisions require market participants to acquire information and to communicate preferences and constraints, neither of which is costless. Because of these frictions, information and communication technologies (ICT) that lower the cost of search and communication are often expected to improve market outcomes—especially when they increase the amount or quality of information used for matching and allocation (Stigler, 1961; Goldfarb and Tucker, 2019). However,

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lower private costs need not translate into higher social surplus when they change participation and information transmission in ways that generate externalities, such as congestion or information dilution (Rao and Reiley, 2012; Crépon et al., 2013; Ashlagi et al., 2020; Kleinberg and Raghavan, 2021).

Generative AI constitutes a new ICT shock that is especially relevant for matching markets. Whereas earlier ICT waves primarily reduced the cost of finding and transmitting information, generative AI also reduces the cost of producing and processing text—job posts, resumes, cover letters, and other messages that are central inputs to hiring. Labor markets are therefore a natural setting in which generative AI might deliver large gains, given the substantial information and communication challenges participants face (Autor, 2001). Yet the diffusion of earlier ICT, including the Internet, dramatically lowered search and dissemination costs without clear evidence of increased match formation in labor markets (Kroft and Pope, 2014; Kuhn and Mansour, 2014; Kuhn and Skuterud, 2004). For market designers, this history sharpens a concrete design question: whether and how to offer generative AI tools to increase postings without generating congestion or information dilution.

In this paper, we estimate the effect of demand-side use of generative AI on labor market matching efficiency¹. In a large-scale field experiment on an online labor market, a randomly selected treatment group of would-be employers were offered AI-written first drafts of their job posts. We observe large uptake of the offered service, considerable time-savings for those using it, and a large increase in the number of job posts in the marketplace. Despite this, we observe no net increase in total matches formed. Given the time savings for employers, the intervention still could have a net social benefit. This is not the case: although the intervention saved employers time, this was more than undone by degraded matching efficiency.

We present evidence that the causal mechanism for degraded matching efficiency—no more matches despite more job posts—was that (a) the marginal jobs induced by the treatment were less likely to hire, and (b) even among jobs that would have been posted regardless of treatment, AI assistance made them less informative to jobseekers about the employer’s needs and intent. We find that (a) accounts for a larger share of the decline in matching efficiency than (b). This compositional change in job posts in turn wasted jobseeker time, as jobseekers applied to jobs they otherwise would not have. When we quantify the value of this time wasted and time saved, our estimates suggest that the per job post loss to jobseeker welfare is six times larger than the increase to employer welfare. These negative efficiency outcomes persist after most of the market was offered the technology.

Our findings have direct implications for the design and governance of two-sided marketplaces where one side produces text postings that the other side must screen. Matching market design

¹We use ‘matching efficiency’ to mean match formation net of posting volume (hires per posting).

emphasizes that market clearing depends on the availability of informative signals (and, often, platform recommendations that leverage them) (Ashlagi et al., 2020). A central lesson of this paper is that reducing job post creation effort can increase participation while simultaneously reducing conversion, because it relaxes a screening hurdle that previously deterred low intent entrants and weakens the informational content of the job post itself. As a result, platform interventions that are evaluated solely by increases in posting volume risk creating congestion and shifting time costs onto the other side of the market.

A practical implication is that platforms can often preserve the private time savings from AI assistance while mitigating adverse selection by reintroducing a lightweight screening device that is less wasteful than manual writing effort. In our setting, the dominant driver of reduced hires comes from marginal postings with low hiring intent, suggesting that the relevant design objective is not “more posts” but “more posts with serious intention.” This points to substituting the time-based “ordeal” of drafting a detailed job post with alternative frictions that better target low intent entry, such as a small posting fee, a refundable deposit (returned upon a hire or upon meeting a minimum interviewing threshold), identity verification, or throttling/limits for first time employers.

A second implication concerns information quality. When AI makes it easy to produce polished and lengthy text, it can dilute the informativeness of that text, increasing misdirected applications and time spent screening. Platforms can mitigate this information pollution by designing the drafting workflow to encourage customization rather than one-click posting. More generally, a relevant product decision is not only whether to offer AI assistance, but how to bundle it with commitment and information-verification mechanisms so that reduced writing costs translate into higher match conversion rather than congestion.

These results were not *ex ante* obvious. AI assistance could have simply helped employers express their needs more clearly, as has been shown with other types of algorithmic writing assistance (Wiles, Munyikwa and Horton, 2025).² On the jobseeker side there is evidence that AI written cover letters improve application quality (Cui, Dias and Ye, 2025). However, the AI could also undermine market efficiency through two channels. First, when posting becomes cheaper, more low-value jobs enter the market, creating negative externalities for jobseekers—an ‘information pollution’ effect similar to how spam proliferated when email costs fell (Rao and Reiley, 2012). Second, AI-assisted writing reduces the effort needed to create detailed job posts, making

²Although at first blush our results seem counter to those in Wiles et al. (2025), which show that algorithmic writing assistance improved the writing on applicants’ resumes without any negative impacts to employers, the underlying technology is fundamentally different. In Wiles et al. (2025), algorithmic writing assistance only edits worker written resumes, so it complements jobseekers’ underlying effort and can make existing information easier for employers to interpret. The tool we study here is generative, substantially substituting for writing effort and thereby weakening the informational value of a well-written post as a signal of employer seriousness. We view these findings as complementary: AI that complements costly communication can strengthen information transmission, whereas AI that makes communication close to costless can dilute signals and create congestion.

the text of the job post less informative to jobseekers. This aligns with cheap talk models where costless communication between parties with misaligned preferences often fails to convey meaningful information (Crawford and Sobel, 1982).³ We formalize this trade-off between reducing writing costs and applicant match quality in a model where employers endogenously decide how much effort to put into their job post.

When private costs fall, we typically expect direct benefits to market participants to translate into social benefits of a similar magnitude. For example, Le Barbanchon, Hensvik and Rathelot (2023) conduct a large-scale, two-sided field experiment creating and evaluating a machine-learning recommender system that gives personalized suggestions for jobseekers. This decrease in search costs resulted in a higher employment rate and suggests that traditional, predictive AI can improve search and matching efficiency. When private cost reductions fail to translate into market-level gains, it is often because the lower costs alter behavior in ways that generate negative externalities. For example, with reduced costs of finding jobs, jobseekers' reservation utilities might increase (Martellini and Menzio, 2020) or jobseekers might apply to more jobs and crowd out other workers (Crépon, Duflo, Gurgand, Rathelot and Zamora, 2013). Our findings are consistent with this concern, with the intervention changing employers' behavior in a way that imposed a cost on jobseekers, making their matching problem harder. Despite this negative finding, the intervention did substantially lower job posting costs, suggesting that if market designers can selectively reduce entry costs for high intent employers, such tools might still provide value.

The experimental intervention was simple. Newly registered employers wrote a short description of their needs—which we call a “prompt”—and received an AI-generated job post in response. Employers could modify the text as needed—even discarding all of it. Employers could then, at their discretion, add their job posts to the marketplace. Once posted, jobseekers could then apply, potentially leading to hires. We can observe all aspects of this process from initial job-post creation to the conclusion of any contracts formed.

Access to the AI technology clearly made possible job posts that would otherwise have been abandoned during creation. Treated employers were highly interested in the AI assistance, with 75% of them opting in to receive the AI written first draft. These AI-written draft posts were widely used by employers, with 66% posting them directly without edits. Treated employers were 19% (or 6 percentage points) more likely to post a job than employers in the control group. Despite increasing the number of job posts, treated employers who posted a job were 15% less likely to make a hire. This decline in hiring fully offset the increase in job posts: the unconditioned probability that a match is formed is the same in the treatment and control groups.

³Aside from this muddled signaling perspective, if the AI hallucinates requirements and the employer is unwilling to correct them, both employer and jobseeker could be made worse-off. Gans (2024) proposes a model of communication with generative AI as one where the AI hinders effective communication by making it harder to distinguish high-quality and low-quality communication.

Given the lack of an increase in matches, a potential saving grace is that the treatment saved employers time. Among those treated employers who posted a job, they spent about 44% less time writing the job post (the average time is 8 minutes) than employers in the control group. This shift manifested itself in a difference in the relationship between employer effort and the length of the job post. In the control group, there is a strong positive correlation between the time spent on the job post and its length. However, this relationship largely disappears in the treatment group: the elasticity in the control group is 0.61, whereas the treatment group elasticity is just 0.13.

The decline in employer writing costs benefits employers in a vacuum, but creates challenges for jobseekers who use the job post to assess their fit and probability of being hired. Consider that jobseekers must make inferences about an employer based on the content of a job post. Jobseekers are clearly interested in their individual probability of being hired and the quality⁴ and type of the job in question (Bhole, Fradkin and Horton, Forthcoming). However, these factors are rarely communicated directly, and so jobseekers pay attention to signals. One such proxy is the effort invested in creating detailed job posts.⁵ If AI makes it cheap to write long, detailed and seemingly carefully written job posts, jobseekers might draw the wrong conclusion about the seriousness of the employer. Before jobseekers recognize the new equilibrium, they might think the employer made it through an economic “ordeal” when they, in fact, did not (Nichols and Zeckhauser, 1982; Alatas, Banerjee, Hanna, Olken and Tobias, 2012). If job post writing costs select for employers with higher intent, lowering that hurdle might make the pool of job-posters relatively adversely selected. This simple adverse selection mechanism is an example of how a seeming reduction in the private costs of generating information destroyed public information.

We quantify the contribution of the negative hiring effect which is driven by the selection of “worse” jobs versus a direct negative treatment effect on the job posts. We use two complementary methods to decompose the overall negative hiring effect into what is driven by (a) marginal jobs having a lower baseline probability of hire and (b) inframarginal jobs that were written with AI (but would have still been posted without AI had they been in the control) also had their probability of hire reduced by the treatment. We use time- and job-category variation in the size of the extensive margin effect on posting to decompose effects. The two methods give effects with different magnitudes, but both suggest that most of the negative hiring effect is driven by selection. This is a promising finding, as it suggests that it is possible to throw out the bath water and keep the baby—i.e., time costs could be lowered if employers faced some other ordeal (ideally a financial one) rather than a wasteful time ordeal.

To the extent there is a reduction in hiring among inframarginal employers, this problem is

⁴Prior work shows that when jobseekers have better information about firm quality, they can allocate their search efforts towards higher quality firms (Bryan, Hoffman and Sariri, 2022).

⁵As Athey, Bryan and Gans (2020) shows, when AI makes predictions easier, human agents may reduce their effort levels suboptimally.

potentially fixable as the AI technology improves. In this setting, the job posts created by treated employers are more “generic” in that they are more semantically similar to each other.⁶ This genericness is likely to reduce the information content of the job post and seems to be a feature of AI-generated writing both empirically (Cowgill, Hernandez-Lagos and Wright, 2024) and in theory (Gans, 2024).

As there was no net change in matches formed, we focus solely on time costs for welfare analysis. Being offered the treatment saved employers approximately 3.6 minutes per job post and generated \$0.41 in employer benefits. However, jobseekers faced increased costs from applying to more jobs that didn’t lead to hires and treated jobs received 5% more applications, with each application taking 4.5 minutes.⁷ The total welfare loss to jobseekers (\$2.57 per treated employer offered the treatment) was six times larger than employer gains, resulting in a net welfare loss of \$2.16 per treated employer.

A natural concern is that these negative effects might be temporary—either disappearing as market participants learn to use the technology more effectively, or changing once AI adoption becomes widespread rather than limited to our experimental sample. These effects could attenuate through several channels: employers might abandon or better calibrate their use of AI, jobseekers might develop new screening strategies, or employers might find new ways to signal fit or seriousness. Months after the experiment concluded the technology was made available to 95% of newly registered employers (covering most of new-employer posting activity during that period) with a random 5% of new entrants withheld. Compared to results from the earlier experiment, we find the posting effect attenuated but the negative hiring effects remained. This suggests that if there was employer learning, it was not enough to eliminate the inefficiencies introduced by the AI written posts.

Although our setting is an online labor market, the mechanisms we document—lower listing-creation costs increasing entry and reducing the informational content of text-based listings—apply broadly to two-sided platforms that rely on user-generated descriptions and screening. The fundamental challenges of information asymmetry, signaling posting intent, and the costs of creating informative postings exist across all two-sided markets. There is anecdotal evidence that these tools are being used in conventional labor markets as well (Mok, 2023; Smith, 2023) and there’s evidence of wide-spread use of AI in writing application materials (Awuah, Krenk and Yanagizawa-Drott, 2024) found incidentally in field experiments by economists in developing countries.

This paper makes several contributions. First, we provide causal evidence on the marketplace effects of generative AI in an online labor platform, observing the complete sequence from draft

⁶This is an example of the risk of algorithmic monoculture discussed in Kleinberg and Raghavan (2021). We measure this in a variety of ways using NLP approaches showing far less diversity among treated employer job posts.

⁷Because application writing is only one component of job search, this jobseeker time-cost accounting is a conservative lower bound on total jobseeker costs.

creation to posting, applications, and hires. Second, we document that lowering listing-creation effort does not necessarily translate into more successful matches: the tool increases postings but reduces hires per posting, driven primarily by low intent marginal entry and secondarily by reduced informativeness among inframarginal postings. Third, we translate these mechanisms into actionable implications for platform design. If AI tools were paired with even small posting costs to screen out low intent employers, while allowing the technology to reduce writing effort, markets could capture the efficiency gains without substantially degrading signals. Lastly, the paper speaks to a central prediction of search and matching models of the labor market (Diamond, 1984; Mortensen and Pissarides, 1994) by providing rare randomized variation in firms' vacancy creation costs and observing postings and realized hires in the same setting. Consistent with theory, lowering vacancy creation costs increases vacancy posting. However, counter to theory, reducing the effort required to draft the listing increases postings without increasing hires. Together with evidence that lowering vacancy costs via recruitment assistance can increase both postings and hires (Algan et al., 2020), this contrast suggests that the mechanism by which vacancy costs are reduced—whether by facilitating recruitment or by simplifying ad creation—can critically shape whether additional vacancies translate into successful matches.

The rest of the paper proceeds as follows. Section 2 describes the online labor market which serves as the focal market for this experiment. Section 3 describes the experimental design. In Section 4 we describe the effect of the treatment on the production of the job posts. In Section 5 we present a simple model. Section 6 reports the experimental results of the treatment on job posting and hiring. Section 7 provides a welfare analysis. In Section 8 we report results from the market-wide roll-out of the feature. In Section 9 we provide a decomposition of the hiring effects into composition and direct treatment effects. Section 10 concludes.

2 Empirical context

The experiment was conducted on a large online labor market (Horton, 2010; Stanton and Thomas, 2016; Pallais, 2014). In online labor markets, employers search for and hire workers to complete jobs that can be done with only a computer and an internet connection.⁸ These markets can differ in their scope and focus, and platforms have different responsibilities they provide to employers and workers. Some common services platforms provide include soliciting and promoting job openings, hosting profile pages, processing payments, certifying worker skills, and maintaining a reputation system (Filippas et al., 2022; Horton, 2010).

⁸We use the terms “employer,” “job opening,” and “application” for consistency with the economics literature and not as a commentary on the legal nature of the relationships created on the platform.

2.1 Vacancy creation

Would-be employers register for the platform, after which they can write and publish job posts. During registration they provide their name, email address, and country they are operating in. After registration they are taken to a home page where they see a button that says “Post a job”. These job posts include a narrative description of the job, required skills, and indications of the scope of the project.

As part of the posting process, a would-be employer first gives the title of the job, for example, “E-commerce website copywriter,” “Web developer,” or “Executive assistant.” They then list the skills necessary for the job, selecting from a pre-defined list. The employer classifies the job into a broad category from a pre-specified list. Examples of categories include “Administrative Support,” “Data Entry,” “Software Development,” among others. The employer is asked to estimate how long the job will last, choosing from a collection of pre-defined lengths, e.g., less than one month, more than 6 months, and so on.

In terms of compensation, what employers state in the job post depends on the contractual structure the employer has chosen. Contracts can either be fixed price or hourly. If the job is hourly, the employer can optionally report a wage range; if fixed price, the employer reports a budget. Both of these reports are not binding, though job seekers do use them to decide which jobs to apply to and on what terms.

Despite the low technical costs to posting jobs, as we will show, many new employers “fall off” during the job posting process and fail to post a job: 30% of new would-be employers who begin the job posting process post a job. This substantial fall-off is presumably because some employers are still curious about the job posting process and have low intent; others likely find filling out several text boxes to be more onerous than they expected. Consistent with this learning explanation, employers with experience on the platform have a much higher completion rate—greater than 90%.

2.1.1 Worker job search

Workers find jobs in the platform via active search, emailed messages from the platform, and from direct inquiries from employers. Employers receive organic applications from workers who find the job opening independently, or they search for workers themselves and invite specific workers to apply. To find workers they might want to invite, employers can search through worker “profiles.” Employers are also recommended workers immediately after posting a job. These worker profiles contain workers’ history of work on the platform (jobs, hours, hourly rates, ratings) as well as their education history and skills. For both workers and employers, the platform verifies some of the information available to the other side of the market. Employers are particularly interested in

past experience on the platform (Pallais, 2014), and generally are looking for signals to overcome information asymmetries (Stanton and Thomas, 2016).

Workers apply to job openings with a cover letter and a price. For fixed price jobs, the price is the total amount; for hourly jobs, the price is a wage bid. Employers can review applicants whenever they like and can choose to hire—or not.

Hired workers for hourly jobs install custom tracking software that serves as a digital punch clock. This information is all sent to the platform’s servers and made available to the employer for monitoring in real-time. At the end of the contract, both parties give a reason for ending the contract (usually that the project was completed successfully) and provide both written and numerical feedback about each other.

3 Experimental design

Would-be employers were added to the experiment at the moment they began their first job. The point of allocation was when the newly registered employers first clicks the “Post a job” button. From June 07, 2023, through July 20, 2023, new employers on the platform were randomly allocated to either the status quo control experience or the treated experience.⁹ The realized number in each was 90,048 treated and 90,276 control. These counts are consistent with random allocation (χ^2 test p-value is 0.26). Each employer account is randomized only at their first attempt to create a job post; subsequent attempts keep the same assignment. Appendix Table 17 reports the geographic balance of employers at the moment of randomization. Because allocation occurs before any additional firm or job-level information is entered, the only observable attribute at that stage is the country reported during registration. The table shows that treatment and control groups are geographically similar, suggesting that random assignment was implemented as intended.¹⁰

We identify individual-level treatment effects under the Stable Unit Treatment Value Assumption (SUTVA). For outcomes determined before the job post is published—such as whether an employer initiates or completes a posting, how long they take to write it, or how specific the text is—interference between treated and untreated employers is unlikely. By contrast, SUTVA could be violated for outcomes determined after posting, such as applications and hires, where treated employers might draw applicants away from control employers. Because only half of new entrants were assigned to treatment during the experiment, treated employers represented a small share of total market activity¹¹, so potential spillovers are likely limited. To assess the potential importance of such spillovers, we exploit a later market-wide expansion of the treatment: starting on October

⁹Appendix Figure 14 shows the daily allocations of employers into the treatment and control groups.

¹⁰The marginally significant difference for Africa ($p = 0.05$) represents six percent of the employers in each treatment group and is within the range expected by chance when testing multiple regions.

¹¹Over the course of the experiment, treated job posts made up less than 7% of job posts in the market.

10, 2023, the fraction of new entrants assigned to treatment increased from 50% to 95%, allowing us to observe potential market-wide effects.

3.1 Employers posting jobs offered AI assistance

From the home page, would-be employers can click a button that says “Post a job.” Employers in the control group who click this button receive the standard job posting experience described in Section 2. Employers in the treatment group who click the button are given two options: (1) “I’ll do it without AI” or (2) “Get started using AI.” Those who choose the first option receive the standard experience. Those who choose the second option are asked to describe the job they want to post in a sentence or two. We refer to these initial sentences as the “initial prompt,” which we observe. See Figure 1 for a stylized version of the interface. Importantly, at this stage treated employers have not yet entered structured fields such as job title, required skills, job category, or compensation. Instead, the AI tool uses the initial prompt to generate a draft job post, which populates those fields on the employers behalf. Employers can then edit or overwrite any of the AI-generated text or fields before submitting their posting.

As an example, after being asked to describe the job in a sentence or two, one employer wrote:

I need someone to generate a an [sic] Excel database showing the frequency of a search term in a list of targeted business media

This is incorporated into a prompt, calling a popular generative AI service.¹² The employer is then shown the job post written by the AI as well as a proposed list of required skills. The model also infers a high-level category for the work. In the case of the above input, the employer was shown:

We are looking for an expert who can generate an Excel database that displays the frequency of a specific search term in a list of targeted business media. The ideal candidate should have the following skills:

- *Strong knowledge of Excel*
- *Ability to work with large sets of data*
- *Research skills*
- *Attention to detail*
- *Time management skills*

The AI-generated job post was presented in an editable text box, instructing the employer to “take time to review your job post and make it your own.” Employers could modify the job

¹²The exact prompt is listed in Appendix A.8.

post as they saw fit, including deleting all of the AI text if they chose. We observe both the initial AI-generated text and the final job post text posted to the market. In addition to freely editing the post, the employer was shown options for how the AI could edit the job post for them. These “personalization” options were “Make it casual,” “Make it formal,” “Shorten it,” “Add more details,” “Rewrite it.” We observe use of these personalization editing options. We also observe the time the employer spent on the job post writing process.

3.2 Description of the data

The primary dataset used in our analysis includes all job posts made by employers in the experimental sample from the time they were assigned to the experiment up until August 3, 2023, which is 14 days post-allocation. We compile job post-level data encompassing all posts, applications, and hires occurring within 14 days of the posting date. We track the complete evolution of job posts from conception to submission. For each treated job post, we observe: (1) the employer’s initial prompt to the AI system, (2) the AI-generated first draft, (3) all subsequent employer modifications, and (4) the final published version. This granular tracking allows us to analyze not just outcomes, but the entire production of the job post.

Our primary economic outcomes of interest are: (1) whether the employer ultimately completed the job posting, (2) the number of applications received for the job posts, and (3) whether or not a hire was made for the job. We also look at the difference in the production process for treated and control employers, with outcomes including time spent writing, post length and its distribution, and semantic similarity between posts (“genericness” of writing), and the relationship between writing time and perceived job post quality.

4 Employer usage of the AI feature in the production of a job post

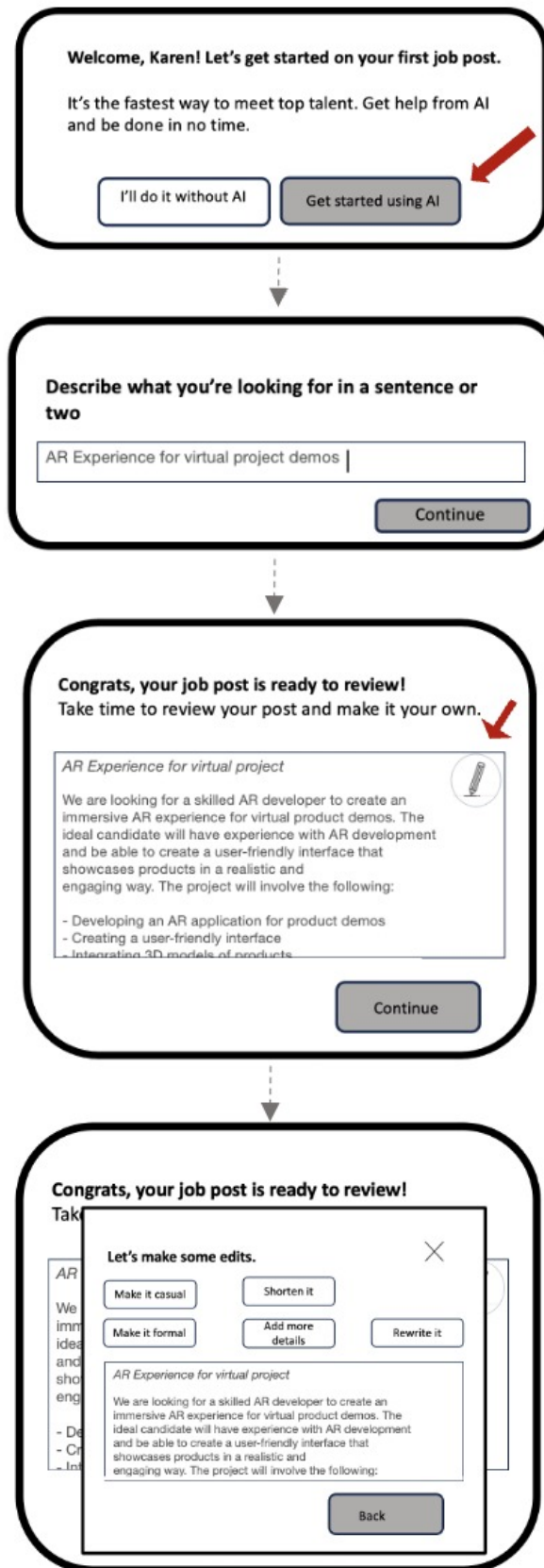
We start our analysis by focusing on how the AI treatment affected the job post writing process.

4.1 Use of the AI feature was widespread

Among treated employers, 75% chose to receive an AI-generated first draft post. While opting for the first draft was common, the personalization features were not frequently used.¹³

¹³Employers utilized the “Make it casual” feature the most, 3.3% of the time. The next most used feature was “Add more details,” which appeared in 2% of the job posts. “Shorten it,” “Make it formal,” and “Rewrite it” were each used in between 1 and 2% of the job posts.

Figure 1: Stylized job post process for employers in the treatment group



In the treated group, employers using AI still had to exert effort at two points in the process: (1) drafting their initial one or two sentence prompt sent to the AI, and (2) (potentially) editing the AI-generated text. Employers who provide high-quality initial prompts are more likely to edit AI-generated text, but higher-quality AI drafts are less frequently edited, indicating a latent factor such as employer motivation influencing both prompt quality and editing probability. See Appendix A.2 for this analysis.

4.2 Treated employers spent less time writing the job post

Treated employers spent 44% less time writing their job posts. We measure the time the employer spent writing the job post as the difference in the timestamps from when the employer first clicks on the page to post a job and when the employer finally presses “submit” on the job post. This time is only measured for employers that post a job. In Table 1 Column (1) we show that employers in the control group spent on average 8.1 minutes writing the job post while treated employers spent only 4.5 minutes. Length of time spent writing the job post is only measured for employers (1) whose efforts result in a job post and (2) who write the job post in one sitting. In Column (2) we show that the employers in the treatment group who posted a job were much less likely to get timed out of the job description writing page.

Table 1: Effects of generative AI on job post writing, conditional on posting

	<i>Dependent variable:</i>		
	Minutes	Timed Out	Num Words
	(1)	(2)	(3)
GenAI Treatment Assigned	-3.592*** (0.072)	-0.069*** (0.004)	1.888** (0.801)
Constant	8.142*** (0.055)	0.384*** (0.003)	94.089*** (0.589)
Observations	38,203	58,495	58,495
R ²	0.061	0.005	0.0001

Notes: This table analyzes the effect of the treatment on outcomes, conditional on the employer posting job. An observation is one employer’s first job post. Minutes is the number of minutes the employer spends writing the job post. Minutes is NA if the employer got timed out while writing the job post or closed and reopened the browser window while writing. Timed Out is a binary indicator if the employer got timed out via logging off or closing the window while writing the job post. Num Words is the length of the job post. The sample is made up of all employers in the experimental sample who post a job within 14 days of being allocated into the experiment. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

4.3 Job posts from treated employers were longer and more similar to each other in terms of length

Job posts by treated employers were slightly longer but the distribution of lengths was far more compressed. In Table 1 Column (3) we show the average length of job posts by treated employers was nearly identical to those in the control: 96 words in the treatment and 94 words in the control. However, this masks a large change in how the distribution of lengths changed: the standard deviation of job post length for treated employers was 82 but 111 in the control. The median job in the control group was 53 words long, while the median job in the treatment group was 84 words long. For a CDF plot of lengths, see Appendix A.3.

4.4 The treatment likely weakened a signal of employer effort

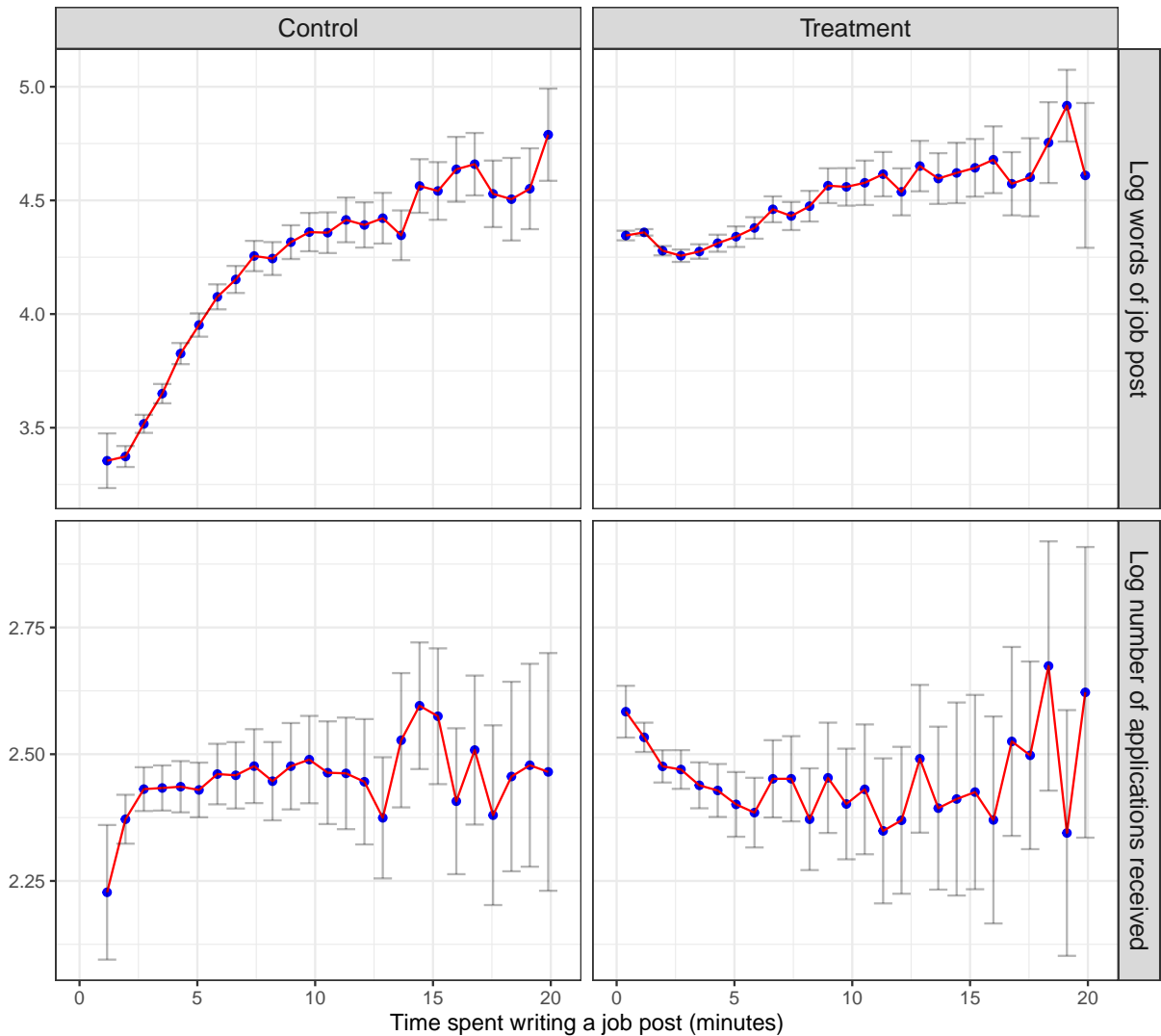
Jobseekers may interpret the attributes of a job post as indicators of the employer's seriousness and intent. An employer who invests more effort and care into crafting a job post might be more committed to making a hire and could also be a more conscientious employer. Of course, this is only a proxy. Some employers might have dedicated HR teams or use automated tools to generate detailed job posts without much effort. Conversely, smaller companies or startups might not have the resources to create extensive job posts but could still be highly committed and conscientious employers.

Given this possibility, we explore the relationship between job post length and time spent on the job post. Figure 2 is a scatter plot of job post length and applications versus time spent on the job post, by treatment status. In the first panel we plot log number of words in the job post against the number of minutes the employer spent writing the job post. In the control panel of job post length, we can see a clear and obvious monotonic relationship, at least on average. If we regress log length on log time spent, the elasticity in the control group is 0.61. In contrast, in the treatment group, the relationship is considerably weakened. There are many jobs near the median length where the employer put in almost no time. The treatment group elasticity is just 0.13.

In the second panel, we plot log number of applications a job post received against the number of minutes the employer spent writing the job post. At the lower end of the distribution, the job posts in the control group have a similarly high elasticity as job posts which employers spent longer on get more applications. Like with number of words, in the treatment group this relationship breaks down, and in fact job posts which employers spent less time on get *even more* applications than those which employers spent more time on.¹⁴

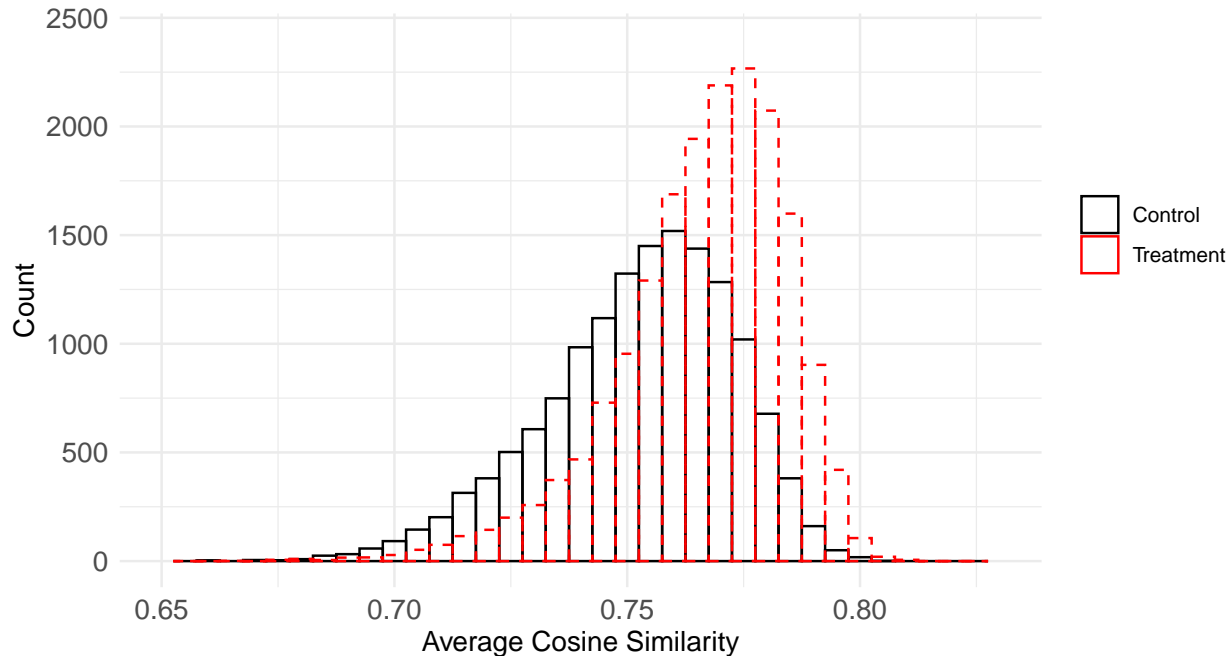
¹⁴Complementary work shows that jobseekers who spend more time editing AI generated cover letters are more likely to receive a job offer (Cui, Dias and Ye, 2025).

Figure 2: Cross-sectional bin scatter relationship between job post length and volume of applications versus job post writing time



Notes: This figure is a bin-scatter plot of the log length of job post and log number of applications versus the log of the time spent writing the job post. The left panels shows the control group and the right panels shows the treatment group. Log number of minutes is truncated at 20 minutes, and is split into 25 buckets. We plot 95% confidence intervals around each point estimate.

Figure 3: Average cosine similarity of job post text embeddings, by treatment status



Notes: This plot shows the average cosine similarity in the treatment and control cells for all employers in the sample who posted a job. Appendix Table 30 shows the regression output.

4.5 Treated job posts were semantically more generic

Job posts from treated employers were semantically less distinctive than job posts from control employers. We characterize this lack of distinctiveness by comparing the text embedding of a particular job post with the text embeddings of all other job posts in that cell. These embeddings are high-dimensional vectors that are intended to capture the semantic attributes and content of the job descriptions. This transformation of text into a Euclidean vector allows us to compute numerical measures of similarity.¹⁵ In Figure 3, we plot the distributions of mean similarity, by treatment assignment, to show how much this treatment effect shifts the distribution. The treatment effect on the mean cosine similarity between job post embeddings is small in absolute terms but notable and statistically significant, covering 10% of the range between the minimum and maximum average cosine similarities. See Appendix A.1 for more details and the regression output.

¹⁵For our embeddings, we use OpenAI’s model “text-embedding-ada-002”. In Appendix Section A.10, we plot the embeddings in two dimensions to show that they can pick up meaningful differences between job types. We also plot the treatment effects.

5 Conceptual framework

Having described the experimental setup and context, we now present a simple model to formalize the key mechanisms at play. The model captures how AI assistance affects both the extensive margin (which employers post jobs) and the intensive margin (how much effort employers invest in describing their needs). By reducing the cost of posting and providing automated assistance with job descriptions, the technology could increase market efficiency. However, as we will show, these benefits may be offset if the technology leads to less informative job posts or induces posting by employers with lower intent to hire.

There is, of course, a vast literature on the labor market and job search, but we focus on a few key aspects that are relevant to our context. To start, we should note that a foundational assumption in matching models of the labor market is that creating vacancies is costly ([Mortensen and Pissarides, 1994](#); [Rogerson et al., 2005](#)). In these models, firms gain a benefit from posting a vacancy. Given these benefits, one might wonder, “Why don’t firms post an infinite number of vacancies?” The answer lies in the costliness of hiring activities ([Acemoglu, 2014](#)). Hiring one skilled worker is estimated to cost about 10–17 weeks of wages, and these costs increase with the skill requirements of the position ([Blatter et al., 2012](#)). Hiring costs are, at least in part, research and writing costs: understanding the company’s needs, determining the right kind of candidate, finding out market rates, describing the opportunity in an attractive manner, and so on. These are precisely the kinds of white-collar tasks that Generative AI appears to be most helpful in performing ([Noy and Zhang, 2023](#)).

The comparative statics of these equilibrium models of search and matching suggest that lowering vacancy creation costs should increase match formation. Whether job posts are under- or over-supplied in a market depends on the [Hosios \(1990\)](#) condition. Firms creating vacancies need to be compensated for their contributions to match formation. Each new job post would, in these models, increase the probability that a jobseeker would find a match, but also have a negative externality on other employers who have a reduced probability of hiring. This AI assistance might be less useful in our digital context where technical search and screening costs are presumably lower because of platform-mediated assistance ([Oestreicher-Singer and Sundararajan, 2012](#); [Horton, 2017](#)). As such, in more conventional contexts, AI assistance might be potentially more important than in our context.

5.1 The setup

There is a unit mass of would-be employers (“employers”) considering posting one job each. There are two periods. In period 1, employers decide whether to post the job and whether to exert effort. If they post the job, then in period 2 they receive applications and decide whether to hire a worker.

If they do not post the job, nothing happens in period 2. AI technology is modeled as a substitute for human effort.

5.2 Period 2: The decision to hire

We first describe period 2. Each job j is defined by a location on a Hotelling line, $\theta_j \in (\underline{\theta}, \bar{\theta})$, which reflects the type of skills needed to complete the job. If the employer exerted effort in period 1, they receive N applications, with skills $\{\theta_i\}_{i=1}^N$, drawn iid from $U[\theta_j - \gamma, \theta_j + \gamma]$, where $\gamma > 0$ is a parameter that captures the fact that the employer cannot perfectly describe the skills needed in the job post.¹⁶ If the employer did not exert effort in period 1, they instead receive N applications drawn iid from $U[\theta_j - \rho\gamma, \theta_j + \rho\gamma]$, where $\rho > 1$ captures the fact that exerting no effort to specify the skills required results in a vague job post and thus draws applicants with a wider—and less relevant—set of skills.

Intuitively, exerting effort shrinks the support of the distribution of applicant skills and makes it more likely that the employer will receive an application close to θ_j . An employer is able to fill the job iff at least one application is within distance $m > 0$ of θ_j . If the employer is unable to fill the job—because they did not receive any application within distance $m > 0$ of θ_j , they receive period 2 utility of 0.

If the employer has at least one such application, they can choose whether to make a hire. If they make a hire, they receive value $v_j \sim G$ from completing the job and pay wage w .¹⁷ They also must pay idiosyncratic utility cost $\epsilon_j \sim U[0, 1]$, which reflects various hiring costs like search and screening. Therefore, conditional on being able to hire, they will hire iff $v_j - w - \epsilon_j \geq 0$.

5.3 Period 1: The decision to post

In period 1, employers decide both whether to post the job, $p \in \{0, 1\}$ and, if they do post, whether to exert effort, $e \in \{0, 1\}$. Posting incurs cost $c > 0$ and effort incurs cost $c_e > 0$. They know v_j , but do not know ϵ_j nor whether they will receive an application sufficiently close to θ_j to be able to hire, so must form expectations over these objects when making their period 1 decisions. In particular, their utility if they post is given by

$$U(p = 1, e) = \pi(e, v_j) (v_j - w - \mathbb{E}[\epsilon_j | v_j - w - \epsilon_j \geq 0]) - c - ec_e,$$

where $\pi(e, v_j)$ is the probability of hiring, which happens if they are able to hire and ϵ_j is sufficiently low relative to v_j . If they do not post, they receive utility 0.

¹⁶We define $\underline{\theta}$ and $\bar{\theta}$ such that this and subsequent ranges of applications are always interior to $(\underline{\theta}, \bar{\theta})$.

¹⁷We assume an exogenous and fixed wage because our experiment only affects a small subset of the market.

We now compute the objects $\mathbb{E}[\epsilon_j | v_j - w - \epsilon_j \geq 0]$ and $\pi(e, v_j)$. Since $\epsilon_j \sim U[0, 1]$, we can write $\mathbb{E}[\epsilon_j | v_j - w - \epsilon_j \geq 0] = (v_j - w)/2$.¹⁸ To obtain $\pi(e, v_j)$, note that this is given by $\Pr(\text{at least one application is within distance } m \text{ of } \theta_j | e) \cdot \Pr(v_j - w - \epsilon_j \geq 0)$. The latter term is just $v_j - w$. For the former term, denote an application as θ_i . Assume for now that $e = 1$. Then, this probability can be written as $\Pr_{e=1}(\min_i |\theta_j - \theta_i| < m) = 1 - \Pr_{e=1}(|\theta_j - \theta_i| > m)^N$. Since $\theta_i \sim U[\theta_j - \gamma, \theta_j + \gamma]$, this is $1 - (1 - \frac{m}{\gamma})^N$. Figure 4 shows the intuition for this: the probability of not being able to hire is simply the probability that all N draws fall outside of the shaded area, each of which occurs with probability $1 - \frac{2m}{2\gamma}$. If instead the employer did not exert effort in period 1, then this probability falls to $1 - (1 - \frac{m}{\rho\gamma})^N < 1 - (1 - \frac{m}{\gamma})^N$. Intuitively, if the support from which applications are drawn is wider, the probability of receiving an application within distance m of θ_j is lower.

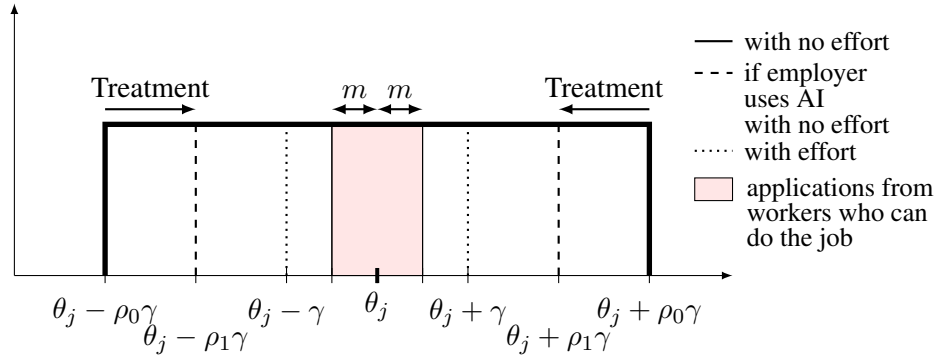


Figure 4: Stylized version of the distribution of applications job post j receives, and effect of the treatment

Thus, plugging these objects in and simplifying, period 1 utility of posting is given by

$$U(p = 1, e) = \frac{1}{2} \left[1 - \left(1 - \frac{m}{(1 + (1 - e)(\rho - 1))\gamma} \right)^N \right] (v_j - w)^2 - c - ec_e.$$

Note that effort and value of the job are complements: $\partial^2 U / \partial e \partial v_j > 0$. Intuitively, if v_j is higher, then the return to effort in terms of increased likelihood of finding a suitable applicant is also higher.

The employer can choose one of three sets of actions: not post, post without effort, and post with effort. Their choice will be governed by v_j , as shown in Figure 5.¹⁹ For $v_j < \underline{v}_l$, they will not post, where \underline{v}_l is the unique value of v_j such that $U(p = 1, e = 0; v_j) = 0$. Intuitively, if the value

¹⁸We assume for simplicity that $v_j - w \in (0, 1)$. This is not a substantively important assumption—it merely simplifies the algebra. More generally, we could write $\epsilon_j \sim U[0, \bar{v} - w]$ where \bar{v} is the upper bound of v_j .

¹⁹This depiction imposes a technical assumption that the first threshold for v_j is for the employers to post without effort, and the second threshold is that they will post with effort. This assumption is required for the effort choice to

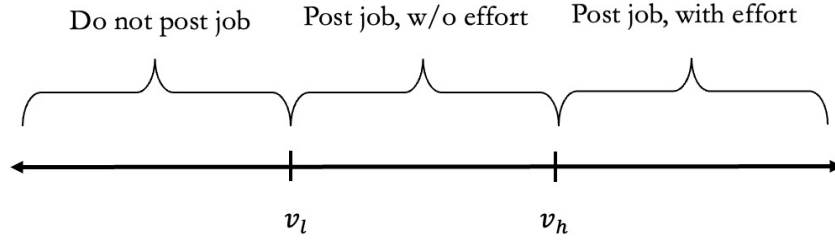


Figure 5: Possible values of v_j and what action the employer takes

of the job is low, it is not worthwhile for the employer to pay the posting cost c . For $v_j \in (\underline{v}_l, \underline{v}_h)$, they will post the job and not exert effort. Intuitively, for these workers the value of the job is high enough to justify the posting cost c , but not so high that the incremental gain from exerting effort to shrink the application pool exceeds the effort cost c_e . Finally, for $v_j > \underline{v}_h$, employers will post the job and exert effort. Intuitively, for very valuable jobs, the increased hiring probability from exerting effort is sufficient to justify the effort cost c_e .

5.4 Effect of technology shock

We now introduce a technology (AI) that does two things. First, it lowers the cost of posting a job from c_0 to c_1 , where $0 < c_1 < c_0$.

Proposition 1. *A fall in the cost of posting from c_0 to c_1 , where $0 < c_1 < c_0$ will cause more jobs to be posted.*

Proof of Proposition 1 is trivial—because the employer’s utility from posting is decreasing in cost c , while their utility from not posting is not dependent on c .

Second, it shrinks the support of the application distribution when the employer does not exert effort by lowering ρ from ρ_0 to ρ_1 , where $1 < \rho_1 < \rho_0$. Intuitively, AI writing software clarifies key elements of the job post if the employer’s original post was vague, but is still not as precise as the employer would be if they exerted effort to clearly specify the skills required.

Proposition 2. *A technology which lowers ρ from ρ_0 to ρ_1 , where $1 < \rho_1 < \rho_0$, will raise the hiring probability of marginal employers who do not exert effort in period 1 and lower the hiring probability of employers on the margin of exerting effort or not.*

The probability an employer makes a hire if they put in effort is $(1 - (1 - \frac{m}{\gamma})^N)(v_j - w)$. If they do not put in effort, $(1 - (1 - \frac{m}{\rho\gamma})^N)(v_j - w)$. Both of these effects cause \underline{v}_l to shift left in

have bite: because effort and value are complements, if even the employer on the margin of posting preferred to exert effort, then all employers that post would exert effort (in which case the decision over effort would be irrelevant for the model). This assumption holds when c_e is sufficiently large—i.e., effort is costly enough that at least some employers that post prefer not to exert effort.

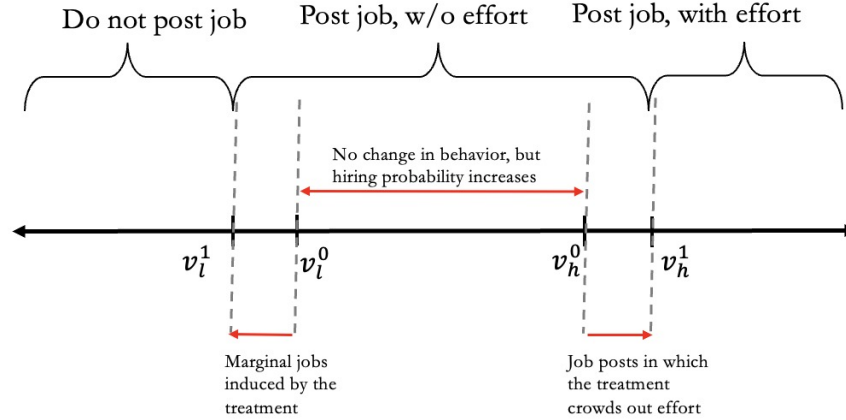


Figure 6: Impact of AI treatment on possible values of v_j and what action the employer takes

Figure 5. The lower cost of posting induces a previously-marginal employer to post as the cost has decreased. As the marginal employer was not exerting effort, the shift in ρ also increases their likelihood of being able to hire and thus further increases the return to posting. Intuitively, the cost of posting has decreased and the probability of hiring has increased, both of which cause employers with lower v_j to post who otherwise would not have.

The reduction in ρ causes v_h to shift right. For an employer who was previously indifferent between exerting effort or not, the technology increases the probability that they will be able to hire if they do not exert effort, and thus they now prefer to not exert effort. Employers who have a very high value of v_j will still exert effort as $\rho_1 > 1$ —i.e., the incremental hiring probability is still worthwhile paying the effort cost for very valuable jobs.

Treatment causes changes in the share of jobs that get posted, the likelihood of making a hire conditional on posting, and the unconditional likelihood of making a hire. We can see this in Figure 6, which shows that treatment causes a change for three groups. First, those with $v_j \in (v_l^1, v_l^0)$ post a job in treatment but not in control. These marginal jobs are less likely to hire than the inframarginal jobs because they are less valuable ($v_j < v_l^0$) and so require even lower draws of the period 2 hiring cost ϵ_j .²⁰ Thus, for these jobs, the share that get posted increases, the probability of hiring conditional on posting decreases, and the unconditional probability of hiring increases. We will call this impact the treatment has on the unconditional probability of hiring δ_1 .

Second, those with $v_j \in (v_l^0, v_h^0)$ do not change their behavior—they post without effort in both treatment and control—but their probability of hiring increases as the shift in ρ from the technology increases their probability of finding a suitable applicant. We will call this impact the treatment has on the unconditional probability of hiring δ_2 .

Third, those with $v_j \in (v_h^0, v_h^1)$ exert effort in control but not treatment. This does not affect the

²⁰The probability that a job j posted without effort hires is $(1 - (1 - \frac{m}{\rho\gamma})^N)(v_j - w)$. As v_j is for these marginal jobs is lower than v_j for all inframarginal jobs, this probability decreases.

probability of posting because these jobs are always posted. It does reduce the probability for these jobs of making a hire, because the reduction in effort lowers the probability of finding an application with θ_i sufficiently close to θ_j . Thus, for these jobs, the share that gets posted is unaffected, and both the conditional and unconditional probability of hiring decreases. We will call this impact the treatment has on the unconditional probability of hiring δ_3 .

Combining the previous three ranges of v_j , the model predicts that treatment increases the share of jobs that get posted. The effect to the probability of hiring conditional on posting is ambiguous, and will decrease if the effects to the first and third regions dominate the effects to the second. The effect of treatment on the unconditional probability of hiring is ambiguous. On the one hand, the increase in posted jobs increases the probability of a hire. On the other hand, the probability of hiring conditional on posting a job is lower for both marginal jobs (as they are less valuable) and inframarginal jobs (as some of them stop exerting effort). The net effect to the unconditional probability of hiring depends on which force dominates, which depends on the relative masses of v_j in the two regions as well as the various parameters.

Proposition 3. *The net effect of lowering ρ and c on the unconditional number of hires is equal to $\delta_1 + \delta_2 + \delta_3$, which can be positive, negative, or zero.*

Formally, the effect to the unconditional probability of hiring is given by $\delta_1 + \delta_2 + \delta_3$. The first two terms are positive and the third term is negative, and the net effect will depend on which forces dominate. The part of the effect driven by marginal job posts δ_1 is positive, the effect driven by the hiring probability of inframarginal posters who never put in effort δ_2 is also positive, and the effect of the treatment crowding out effort from employers who would otherwise have put in effort given by δ_3 is negative. If the forces are balanced and $\delta_1 + \delta_2 = \delta_3$, then the unconditional impact to hires is 0. For more details see Appendix Section [A.7](#).

6 Experimental results from the marketplace

We estimate the effects of the assistance on whether a job was posted. Given that employers could opt-in to AI assistance, we estimate the intent-to-treat effects. Our specification for posting is:

$$\text{JOBPOSTED}_i = \beta_0 + \beta_1 \text{TRTAI}_i + \epsilon, \quad (1)$$

where JOBPOSTED_i is a binary indicator for whether the employer posted a job, and TRTAI_i is an indicator for being assigned to the treatment. Each observation is an employer who began the process of posting a job. As we will see, the treatment had a large effect on the probability that a job was posted. This creates an econometric problem because many outcomes of economic interest are

defined conditional upon posting a job. As such, we will repeat regressions of this unconditional form for all of the main outcomes,²¹ with results in Table 2.

For all outcomes other than JOBPOSTED we will also estimate conditional regressions, e.g.,

$$Y_i = \beta_0 + \beta_1 \text{TRTAI}_i + \epsilon \quad \Bigg| \quad \text{JOBPOSTED}_i = 1 \quad (2)$$

where each observation is an employer who posted a job. Results from these specifications can be found in Table 3.

With this kind of conditional regression, we will caveat that the estimate for any conditional regression is a mix of two potential causes. For any outcome, a change could be driven by compositional effects, as the treatment group has more marginal employers induced by the treatment, and these marginal job posts might be made by employers who differ from inframarginal employers. But there are also potential effects due to having an AI-written job post among inframarginal employers in the treatment group. These employers would have still posted had they been in the control group, but with a “hand-written” job post. When they are in the treatment group, having an AI-written job post might affect the kinds of applicants the job attracts, how they bid, whether they are willing to interview, and so on. In Sections 6.9 and 9, we will be able to decompose these effects. In addition to reporting results in tables, Figure 7 summarizes all of our main results graphically, reporting effects in percentage terms.

6.1 Treated employers were far more likely to post a job

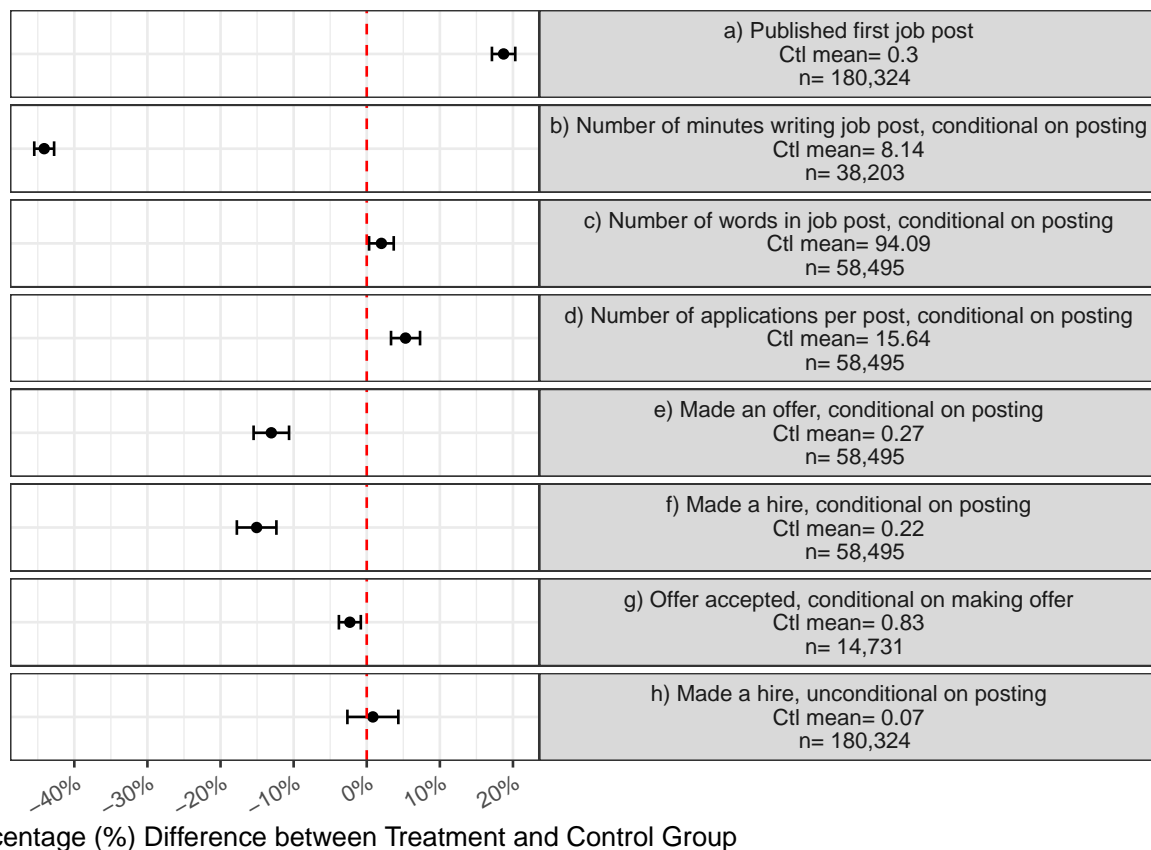
Treated employers are 19% more likely to post a job than employers in the control group. We estimate Equation 1 and report the results in Column (1) of Table 2. We can see from $\hat{\beta}_0$ that 30% of first-time would-be employers in the control group finish the process and post a job. Adding the coefficient on the treatment indicator, we can see that 35% of treated employers finish and post a job.

6.2 The treatment changed the composition of jobs posted to the marketplace

When posting a job, an employer self-categorizes their job into a number of distinct high-level categories. The distribution of jobs by category was not the same in the treatment and control groups. Figure 8 shows the number of job posts in each category, by treatment and control group. Above each pair of bars, the estimated percentage treatment effect is reported. The treatment effect

²¹Appendix Table 20 reports summary statistics of variables used to analyze marketplace outcomes.

Figure 7: Experimental estimates of effects of offering AI written job post on would-be employer outcomes



Notes: This plot shows the effect of being assigned treatment on outcomes for would-be employers in the experimental sample. The x-axis is the percentage difference in the mean outcome between employers in the treated group and the control group. The outcome a) published first job post is a 0 if the employer never submits a job post after beginning the job post and 1 if they do. The outcome b) number of minutes is conditional on the employer posting a job and not being timed out on the job posting page, either from inactivity or by closing the page. The outcomes c), d), e), and f) are all conditional on the employer posting a job. The outcome g) offer accepted is conditional on the employer posting a job and making an offer. The outcome h) made a hire is unconditional on posting a job, it is 0 if the employer doesn't hire anyone after allocations and 1 if they do. A 95% confidence interval based on standard errors calculated using the delta method is plotted around each estimate. The experimental sample is of employers who started to post a job between June 7th and July 20th, 2023, with $N = 180,324$ for outcomes unconditional on posting and $N = 58,495$ for those conditional on posting. Regression details on main outcomes unconditional on posting can be found in Table 2. Regression details on minutes and number of words in job post are in Table 1. Regression details on hiring outcomes conditional on posting are in Table 3.

Table 2: Effects of generative AI on hiring outcomes, unconditional on posting

	<i>Dependent variable:</i>			
	Job Posted	Num Apps	Employer Makes Offer	Hired
	(1)	(2)	(3)	(4)
GenAI Treatment Assigned	0.056*** (0.002)	1.161*** (0.061)	0.003** (0.001)	0.001 (0.001)
Constant	0.297*** (0.002)	4.639*** (0.043)	0.080*** (0.001)	0.066*** (0.001)
Observations	180,324	180,324	180,324	180,324
R ²	0.004	0.002	0.00002	0.00000

Notes: This table analyzes the effect of the treatment on hiring outcomes, unconditional on any behavior of the employer. Job Posted is 1 if the employer submits a job post within 14 days of beginning their job post and being allocated into the experiment. Num Apps is the number of applications the job post receives if the employer posts a job, or zero if else. Employer Makes Offer is 1 if the employer makes any offer within 14 days of being allocated into the experiment. Hire, unconditional on post is 1 if the employer makes any hire within 14 days. The sample is made up of all employers in the experimental sample. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

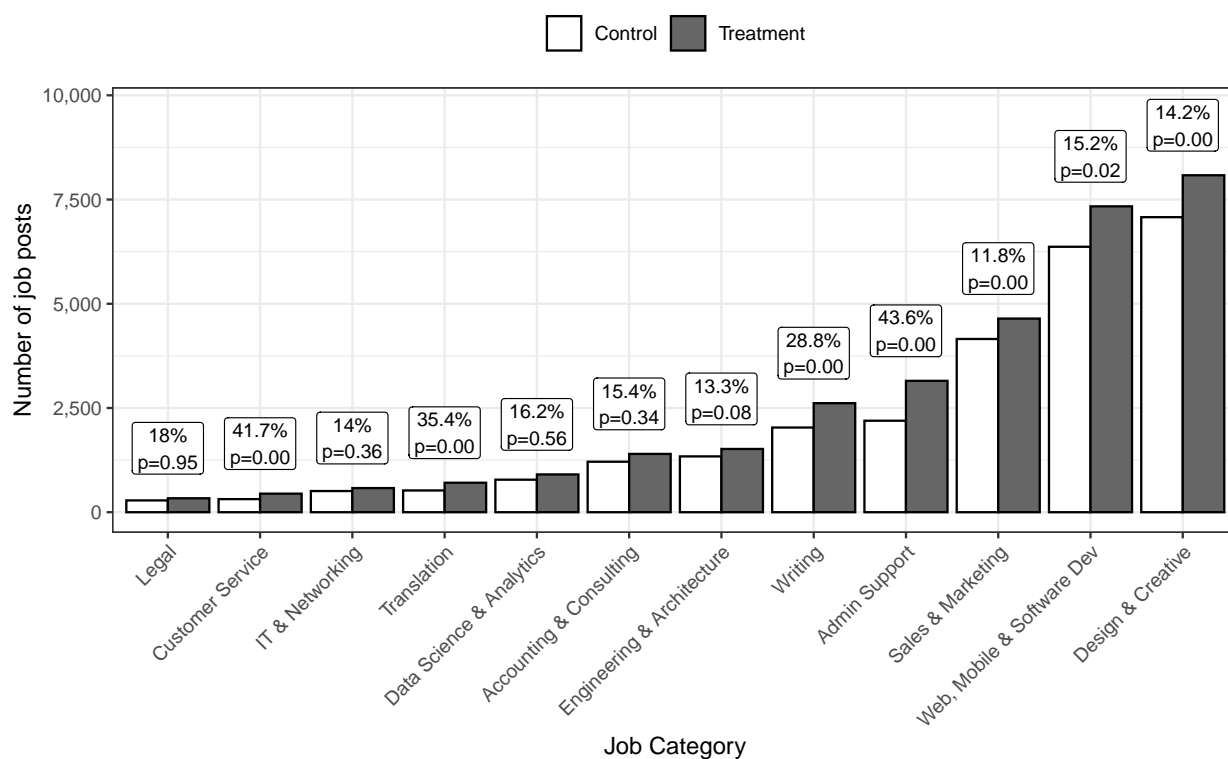
to number of job posts in each category range from a 13% increase in Engineering & Architecture jobs to a 44% increase in Administrative Support jobs.

6.3 Treated job posts received more applications, both unconditionally and conditionally

Both conditional and unconditional on whether or not they post a job, treated employers receive approximately one additional application. For would-be employers not posting a job, we can record them as having received zero applications. Control employers received on average 4.64 applications, whereas treated employers received 5.8, as shown in Table 2.

While one might think that treated employers' increase in applications might be mechanical, we show in Table 3 that even conditional on posting a job, treated employers received more applications on average, though the percentage difference is much smaller: treated employers who posted a job received 16.8 versus 15.64 in the control. Employers in the treatment group received more applications, both because they were more likely to post a job and because the jobs posted in the treatment group attracted more applications. This represents a large increase in applications sent by jobseekers—job posts in the control group received 418,796 applications overall, while those in the treatment group received 522,253, a difference of 103,457 applications.

Figure 8: Number of jobs posted in each primary category of work, by employer treatment status



Notes: This plot shows the number of jobs posted in the treatment and control group for each category of work. The numbers shown on the plot reflect the percentage difference between number of job posts from employers in the treatment group relative to employers in the control group. We also report p-values derived from a Chi-squared test, which assess whether the treatment effects by job category are statistically significant.

Table 3: Effects of generative AI on hiring outcomes, conditional on posting

	<i>Dependent variable:</i>			
	Num Apps	Employer Makes Offer	Accepted Offer	Hired
	(1)	(2)	(3)	(4)
GenAI Treatment Assigned	0.830*** (0.154)	-0.035*** (0.004)	-0.019*** (0.006)	-0.034*** (0.003)
Constant	15.638*** (0.113)	0.271*** (0.003)	0.825*** (0.005)	0.224*** (0.002)
Observations	58,495	58,495	14,731	58,495
R ²	0.0005	0.002	0.001	0.002

Notes: This table analyzes the effect of the treatment on outcomes, conditional on the employer posting job. An observation is one employer's first job post. Number of apps is the number of applications the job post receives if the employer posts a job. Employer Makes Offer is 1 if the employer makes any offer within 14 days of being allocated into the experiment. Accepted offer is not only conditional on posting a job, but also conditional on making an offer, and is 1 if the applicant accepts an offer made. Hired is 1 if the employer makes any hire within 14 days, conditional on posting a job. The sample is made up of all employers in the experimental sample who post a job within 14 days of being allocated into the experiment. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

6.4 Treated job employers were more likely to post accidentally and let job post expire

Table 4: Effects of generative AI on employer stated reason for not hiring, conditional on posting

	<i>Dependent variable:</i>		
	Job Cancelled	Posted Accidentally	Job Post Expired
	(1)	(2)	(3)
GenAI Treatment Assigned	-0.0005 (0.002)	0.003** (0.001)	0.032*** (0.004)
Constant	0.042*** (0.001)	0.019*** (0.001)	0.701*** (0.003)
Observations	58,495	58,495	58,495
R ²	0.00000	0.0001	0.001

Notes: This table analyzes the effect of the treatment on employers who did not hire, conditional on the employer posting job. An observation is one employer's first job post. Job Cancelled is an indicator for if the employer explicitly cancels the job post. Posted Accidentally is an indicator for if the employer reports that the job post was accidental. Job Post Expired is an indicator for if the job post got taken down by the platform because the employer did not make any hires, and the employer gave no other reason for not hiring. The sample is made up of all employers in the experimental sample who post a job within 14 days of being allocated into the experiment. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

In Table 4, we examine how the treatment affected employers' stated reasons for not hiring,

conditional on posting a job. Column (1) shows no significant difference in job cancellation rates between treatment and control groups, suggesting the treatment did not affect employers' likelihood of actively withdrawing their posting. Column (2) shows that treated employers are more likely to report that their job was posted by accident, although the point estimate is small. Column (3) shows that these employers are also more likely to let their job post expire without making a hire.

6.5 Jobseeker behavior

We will exploit the data from job applications to analyze the experiment at the level of the individual jobseeker. The jobseeker does not observe the treatment status of the applied-to job. This allows us to directly observe how the treatment assignment of a job affects the behavior of the jobseeker, with some caveats.

One econometric challenge when interpreting jobseeker-level outcomes is that we know that the treatment has heterogeneous effects on the types of jobs posted. For instance, there is an increase in administrative support jobs (see Figure 8). This shift in job composition could complicate a purely cross-sectional analysis of applications. For example, if our outcome was wage bids and administrative jobs typically pay less, we might mistakenly conclude that the treatment lowered wage bids when it merely changed the composition of jobs available to workers. However, we can address this issue by leveraging the fact that jobseekers can and do send multiple applications during a job search spell. This allows us to observe how the same jobseeker behaves when interacting with a treated versus control job post. This analysis is conditioned on jobseekers who applied to at least one job in each treatment cell. We compile a dataset of all job applications sent to job posts in either the treatment or control group. This dataset includes 1,174,442 total applications to 67,682 distinct jobs, submitted by 480,951 jobseekers. Summary statistics are reported in Appendix Table 22.

Our application-level specification is

$$y_{ij} = \alpha_i + \beta_1 \text{TRTAI}_j + \epsilon_{ij} \quad (3)$$

where y_{ij} is some outcome from jobseeker i to job-opening j , α_i is a jobseeker-specific fixed effect, and TRTAI_j is the treatment assignment of job opening j . We cluster standard errors at the level of the jobseeker and job post. For comparison, we also report regression results without the jobseeker effect to illustrate the importance (or lack thereof) of selection.

In order to investigate how jobseekers interacted with treated and control job posts, we estimate Equation 3 on application level data, with the results in Tables 5 and 6. All outcomes are conditional on jobs which are posted, and include jobseeker fixed effects. So we can interpret β_1 as the

difference in the outcome for jobs that jobseekers apply to in the treatment group vs the control group.

In Table 5, we see that when jobseekers apply to treated job posts, those job posts were written in 53% less time and are 23% longer than jobs they apply to in the control group. These workers then apply to those jobs with cover letters that are 1% longer than the cover letters they write for control group jobs, a small but statistically significant effect.

In Table 6 we see that jobseekers are less likely to have an interview (Column 1) or receive an offer (Column 2) from a treated job. Column (3) shows that when a jobseeker applied to a treated job, that job was also less likely to make an offer to someone else. Column (4) shows that overall, jobseekers receive 12% fewer offers from jobs they apply to in the treatment group. The point estimate is a 0.06 percentage point reduction, from a baseline mean of 0.51 offers in the control group. And with Num Offers we see that when jobseekers applied to treated jobs, those jobs were less likely to make an offer to anyone than when they applied to jobs in the control group. Lastly, Column (5) shows that there is little evidence that workers adjusted their hourly bid based on the treatment status of the job, with slightly hire wage bids when they applied to a job in the treated group.

Table 5: Effects of AI-written draft on experience of jobseekers during hiring process

	<i>Dependent variable:</i>		
	Log Minutes	Log Job Post Length	Log Cover Letter Length
	(1)	(2)	(3)
GenAI Treatment Assigned	-0.532*** (0.010)	0.234*** (0.010)	0.014*** (0.002)
Jobseeker FE	X	X	X
Observations	646,626	941,049	1,174,206
R ²	0.553	0.508	0.892

Notes: This table provides estimates of the difference in jobseekers' experiences when applying to treated vs control jobs. An observation is an application. All specifications involve jobseeker fixed effects. Log Minutes refers to the log number of minutes employers spent writing the job posts. Log Job Post Length is the log number of words in the job posts. Log Cover Letter Length refers to the log number of words in the jobseekers' cover letters. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

In Table 7 we continue to investigate to what extent jobseekers are able to sort around jobs with low intent to hire, this time using the employer provided reasons they did not hire (cancelling the job or reporting that they posted it accidentally) and whether or not the job post expired without the employer taking any action. While jobseekers appear able to avoid the most extreme cases—accidentally posted jobs (Column 2)—they are significantly more likely to apply to jobs that ultimately expire without hiring when those jobs are in the treatment group (Column 3). This

Table 6: Effects of AI-written draft on experience of jobseekers, hiring outcomes

	<i>Dependent variable:</i>				
	Interview	Received Offer	Hired Elsewhere	Num Offers	Log Wagebid
	(1)	(2)	(3)	(4)	(5)
GenAI Treatment Assigned	-0.012*** (0.001)	-0.002*** (0.0004)	-0.060*** (0.010)	-0.062*** (0.011)	0.003*** (0.001)
Jobseeker FE	X	X	X	X	X
Observations	1,174,442	1,174,442	1,174,442	1,174,442	753,536
R ²	0.476	0.481	0.629	0.633	0.948

Notes: This table provides estimates of the difference in jobseekers' experiences when applying to treated vs control jobs. An observation is an application. All specifications involve jobseeker fixed effects. Interview is an indicator for whether the applicant received an interview to the focal job. Received Offer is 1 if the applicant received an offer for the focal job within 14 days. Hired Elsewhere is 1 if the focal job hired a different applicant. Num Offers is the number of offers the focal job makes across all applicants. Log Wagebid is the log hourly bid jobseeker submit with their applications to treated vs control jobs, and is conditional on hourly jobs which result in a hire. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Table 7: Effects of AI-written draft on experience of jobseekers during hiring process

	<i>Dependent variable:</i>		
	Job Cancelled	Posted Accidentally	Job Post Expired
	(1)	(2)	(3)
GenAI Treatment Assigned	0.0005 (0.002)	0.001 (0.001)	0.030*** (0.005)
Jobseeker FE	X	X	X
Observations	941,049	941,049	941,049
R ²	0.424	0.402	0.437

Notes: This table provides estimates of the difference in jobseekers' experiences when applying to treated vs control jobs. An observation is an application. All specifications involve jobseeker fixed effects. Outcomes are defined at the job post level. Job Cancelled is an indicator for if the employer explicitly cancels the job post. Posted Accidentally is an indicator for if the employer reports that the job post was accidental. Job Post Expired is an indicator for if the job post got taken down by the platform because the employer did not make any hires, and the employer gave no other reason for not hiring. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

aligns with our main experimental results in Table 4, which showed treated employers were more likely to both post accidentally and let jobs expire without hiring. Together, these results suggest that while jobseekers can screen out obviously unintentional posts, they struggle to identify the broader set of low intent jobs created by the treatment.

6.6 Treated employers with job posts do less search and screening

We analyze employers' behavior by estimating Equations 1 and 2 in Table 8, with Table 8 showing results both with and without conditioning on whether the employer posted a job. We compare employers' propensity to invite workers to apply, shortlist applicants, or conduct interviews in the treatment and control group. Columns (1), (3), and (5) report unconditional estimates, where we impute zero invites, shortlists, and interviews for employers who did not post a job.²² Columns (2), (4), and (6) report the estimates for only employers who posted a job.

The outcome in Column (1) is the number of invitations employers sent to potential applicants. In Column (3) the outcome is the number of applicants an employer shortlisted, and in Column (5) the outcome is the number of interviews initiated by the employer.²³ Treated employers do slightly more inviting and interviewing than employers in the control group, perhaps unsurprisingly given the large increase in number of job posts.

In order to see how the composition of employers who posted jobs changed due to the treatment, we then condition on only employers who posted a job in Columns (2), (4), and (6). Employers in the treatment group exhibit significantly lower search and screening effort across all three measures compared to those in the control group. The magnitudes of these effects are all small but statistically significant, suggesting that the treatment induces more job posts, but that these employers do less search and screening for these jobs.

6.7 Treated would-be employers were no more likely to make a hire

Our intention-to-treat estimate is that the AI assistance did not increase the number of matches. Treated would-be employers are no more likely to make a hire. This is despite the large increase in employers' propensity to post a job and an increase in applications. Unconditional on whether or not the employers post a job, the likelihood of hiring in the control group is only 6.6% and less than 6.7% in the treatment group, as we can see from Column (4) of Table 2. These results imply that either none of the marginal jobs induced by the treatment resulted in a hire, or that the treatment reduced hiring likelihood among inframarginal jobs.

²²Employers who do not post a job, by definition, cannot invite, shortlist, or interview candidates.

²³Because interviews happen over multiple mediums in this setting, we define an employer initiated interview as an indicator for whether or not the employer sent a direct message to an applicant.

Table 8: Employer search and selection in treatment and control group

	<i>Dependent variable:</i>					
	Number of invites		Number of shortlists		Number of interviews	
	(1)	(2)	(3)	(4)	(5)	(6)
GenAI Treatment Assigned	0.015*** (0.002)	-0.016*** (0.004)	0.001 (0.001)	-0.022*** (0.003)	0.003** (0.002)	-0.059*** (0.004)
Constant	0.111*** (0.001)	0.376*** (0.003)	0.047*** (0.001)	0.157*** (0.002)	0.130*** (0.001)	0.437*** (0.003)
Conditional on posting:		X		X		X
Observations	180,324	58,495	180,324	58,495	180,324	58,495
R ²	0.001	0.0003	0.00000	0.001	0.00002	0.004

Notes: This table analyzes the impact of the treatment on employer behavior. Number of invites is the number of times a would-be employer reached out to a potential applicant and invited them to apply. Number of shortlists is the number of applications an employer put on their short list of potential hires. And number of interviews is defined as a 1 if the employer direct messaged a jobseeker after receiving their application. In the second specification, the sample is conditioned on employers who posted a job which received at least one application. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

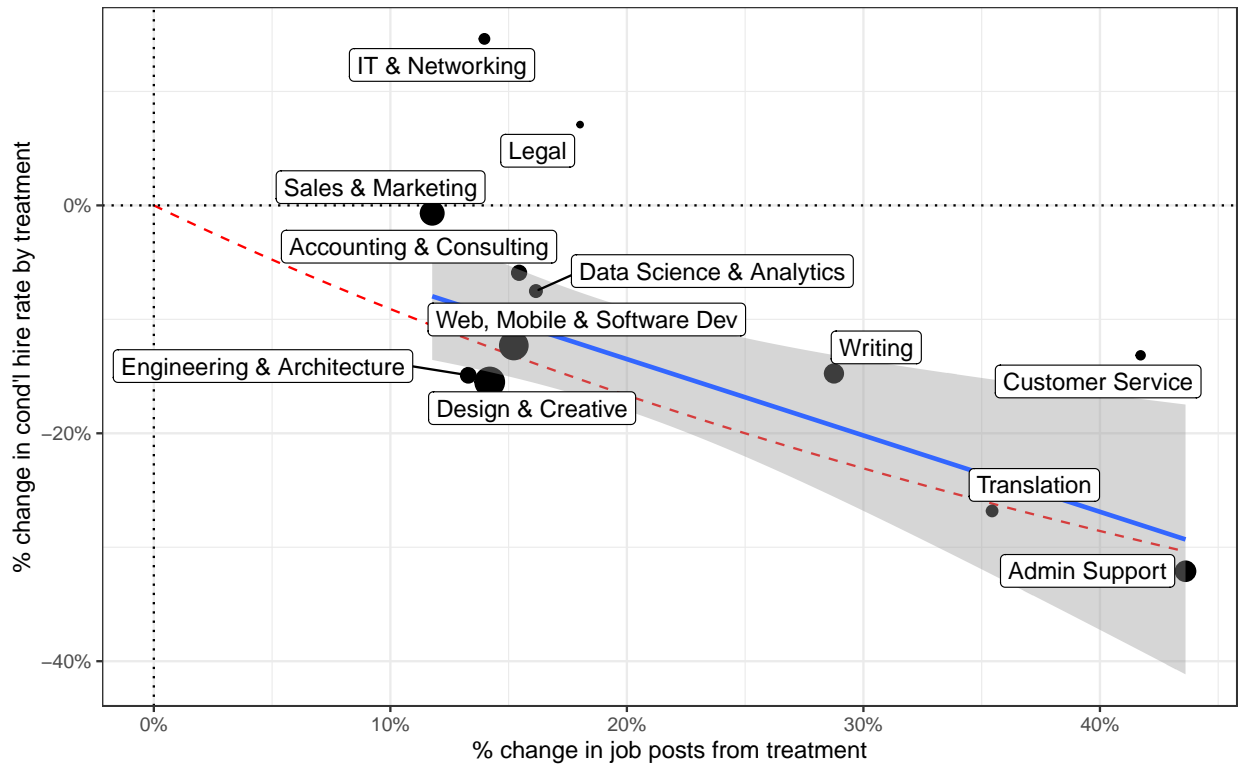
6.8 Conditional on posting a job, treated employers job posts were much less likely to hire

Table 3 shows that conditional on posting a job, treated employers were 15% less likely to make a hire relative to the control group. In the control group, 22% of job posts resulted in a hire within 14 days, compared to 19% in the treatment group (Column 4). This reduction stems from two channels: treated employers were significantly less likely to make offers (Column 2), and conditional on making an offer, their offers were more likely to be rejected (Column 3). We find that offers made by employers in the treatment group are 2 percentage points less likely to be accepted than those in the control group, which could be rationalized by the AI written job posts leading to employers giving offers to worse fitting applicants.

6.9 Category-level decomposition

Figure 9 plots, on the y-axis, the percentage change in hiring in treatment versus control, and on the x-axis, the percentage change in the number of job posts. The dotted line indicates the percentage change in hiring rate, $\hat{\tau}_h$, that would be required to leave the number of jobs unchanged, which is $-(1 + \hat{\tau}_p)^{-1}$, where $\hat{\tau}_p$ is the treatment effect on the posting rate. This represents the expected treatment effects if marginal jobs had zero fill probability while inframarginal jobs remained unaffected.

Figure 9: Category-level hiring rate treatment effect estimate versus category level posting rate treatment effect, in percentage terms



Notes: The y-axis is the percentage change in hiring in treatment versus control employers, by job category. The x-axis is the percentage change in the number of job posts in treatment versus control employers, by job category. The blue line is the smoothed OLS line. The red dashed line indicates the expected relationship if marginal jobs had zero probability of filling and the inframarginal jobs were not affected. The point size is weighted by how many jobs are posted in each category.

To quantify this relationship, we regress the hiring treatment effect for each category on their posting treatment effect.

$$\hat{\tau}_{hc} = \beta_0 + \beta_1 \hat{\tau}_{pc} + \epsilon \quad (4)$$

We calculate the treatment effect to hires $\hat{\tau}_{hc}$ as the percentage difference between the hiring rate between the treatment and control group, where the hiring rate is the fraction of posted jobs which make at least one hire. The treatment effect to posting $\hat{\tau}_{pc}$ is the percentage difference between the number of jobs posted between the treatment and control group, in each category. Categories are weighted by the number of jobs in each category posted in the control group.

The results of the regression in Equation 4 give a coefficient β_1 suggests that for a 1% increase

in the posting treatment effect there was a 0.674% decrease in the hiring treatment effect, suggesting that almost the entirety of the negative hiring effect is driven by marginal job posts. See Appendix Table 29 for full table.

7 AI and the efficiency of the matching process

In this section, we use the results above to do a time-cost accounting to understand the overall effect of the technology to jobseeker and employer welfare. We find that the loss to jobseeker welfare from time wasted applying to jobs that do not hire is six times larger than the increase to employer welfare from time saved writing the job post, with an overall loss to welfare of \$2.16 per employer given the treatment.

We conceptualize the total welfare effect as the welfare effect of allocating one additional would-be employer into the treatment at the moment they are considering posting a job. We consider two employers, one in the control group and one in the treatment group. There are four possible outcomes with respect to posting:

1. both employers post a job, occurring with probability 0.1
2. the employer in the treatment group posts a job and the control employer does not, occurring with probability 0.25
3. both employers do not post a job, occurring with probability 0.46
4. the treated employer does not post a job but the control employer does, occurring with probability 0.19

We calculate the net costs and benefits to workers and employers under each of the four conditions, weighted by the probability of each condition occurring.

Based on the empirical results, we assume that no more hires will result from jobs posted in the treatment group than from the control group. We also assume that an employer who does not post a job has no cost to jobseekers or themselves. Lastly, we assume that an employer receiving the treatment has no impact on the outcomes of other employers.

Jobseekers' welfare: The large increase in posts increased jobseekers' search costs. Jobseekers would have to do additional searching, applying, interviewing, and checking the status of their applications. For applicants, we only know how much time they spent writing their applications. As such, we can only construct a conservative lower bound on the time lost to jobseekers due to increased search costs.

As we showed in Table 3, the treatment induced 5% more applications in the treatment group than in the control group. On average, jobseekers spend 4.5 minutes per application, and their average hourly wage bid is \$29.56. Based on the time they wasted on applying to marginal jobs, active jobseekers lost \$2.22 per additional application to job posts. On average, job posts in the control group receive 15.6 applications and 16.5 applications in the treatment group. This suggests that the costs induced by an additional job post are \$34.66 for jobs in the control group and \$36.51

for those in the treatment group, with higher costs from a treated job being driven by the increase in applications to treated jobs.

Calculating the effect of the treatment on the jobseekers application costs and weighting each of these effects of the treatment to the costs to workers by their probabilities, the impact of one employer being given the treatment is a loss to jobseekers of \$2.57.

Employer’s welfare: To understand the impacts to employer welfare, we use the amount of time it took employers to write a job post to estimate the time saved from the technology. In Table 1 we show that employers in the treatment group saved 3.6 minutes per job post. Because this platform is a gig economy where the employer is deciding between whether to complete a job themselves or hire someone else to do it, we approximate employers’ value of time with how much employers in the control group spend per hour if they make a hire. This is \$30.41 in the control group, which may be an overestimate as \$30.41 is well above the \$23 per hour that the median US worker makes (IPUMS, 2024).

Calculating the effect of the treatment on the employer’s time savings in each scenario and weighting each of these effects of the treatment to the costs to workers by their probabilities, the impact of one employer being given the treatment benefits them \$0.41.

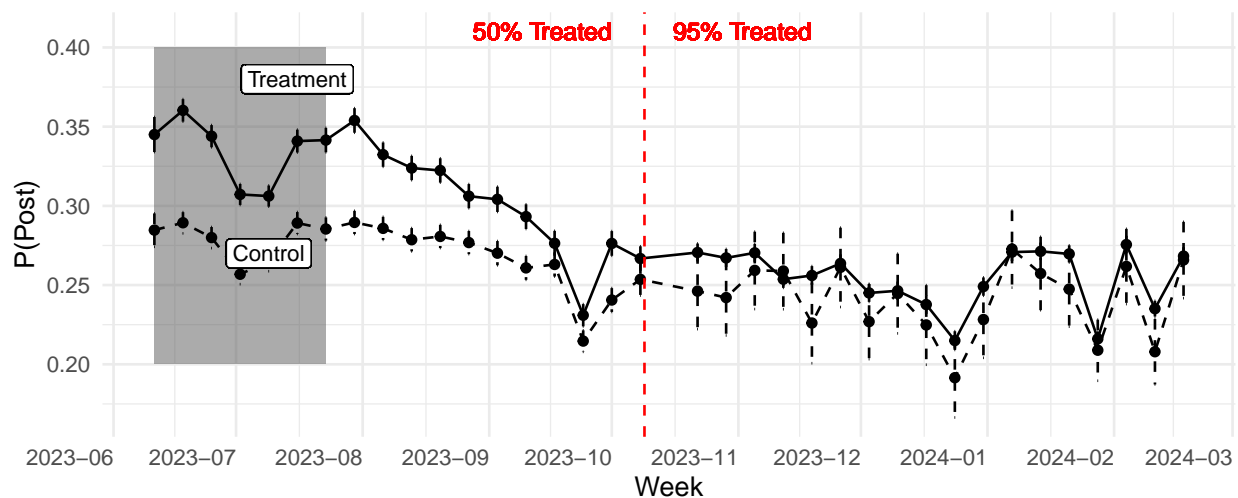
Overall, we find that the introduction of AI-written first drafts of job postings on this subset of the market slightly increased employer welfare, but that these gains are six times smaller than the loss to jobseeker welfare, resulting in a \$2.16 loss per employer given the treatment.

8 Large scale randomized roll-out

After the conclusion of the experiment, the platform expanded access to the AI drafting feature to 95% of newly registered employers, leaving a randomized 5% holdout without access in perpetuity. As in the experiment, this change was not publicly announced and was likely a surprise to new entrants. This design creates a second, large-scale randomized evaluation in a high-adoption environment: because most new employers now have access, any remaining holdout employers operate in a marketplace where AI-assisted posting is prevalent among other entrants. Accordingly, estimates in the roll-out period can be interpreted as the effect of access under high saturation—potentially incorporating any spillovers (like congestion or crowd-out) generated by broader availability of the technology. This allows us to test whether treatment effects attenuate as the tool becomes more familiar and as off-platform AI use may diffuse, reducing the effective contrast between treated and control employers.

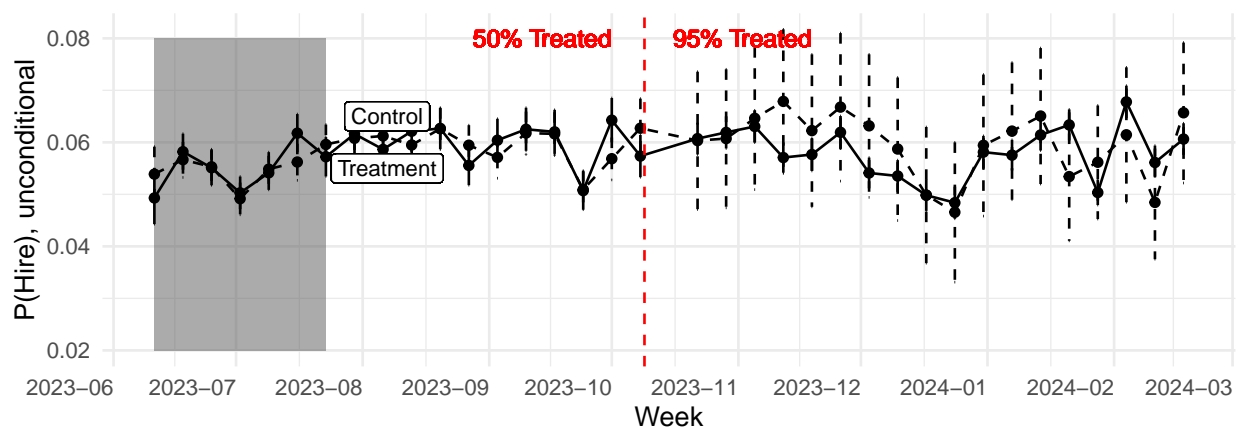
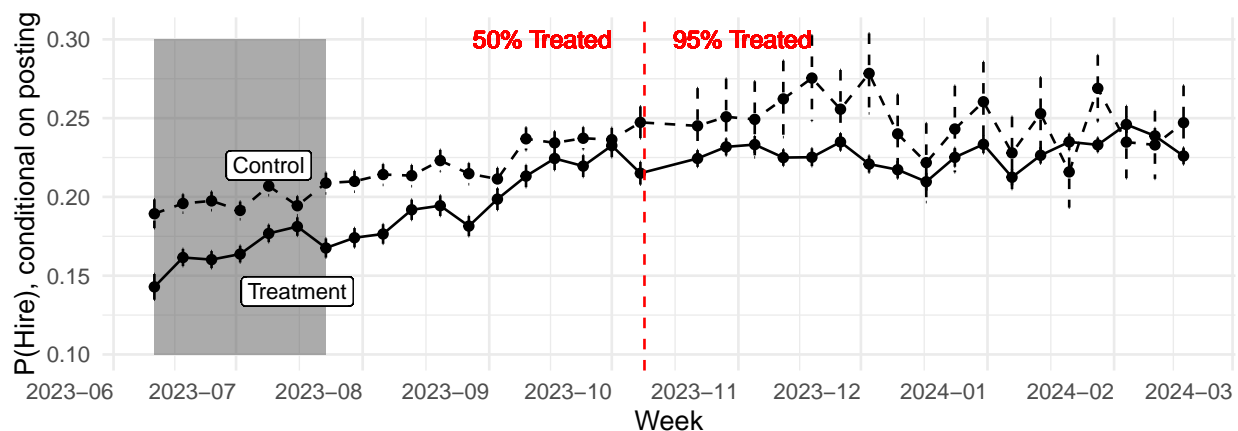
In Figure 10 we plot the main outcomes for each treatment group before and after the roll out. Given the presence of both treatment and control groups during and after the experiment, we can employ a difference-in-differences (DiD) analysis to evaluate the equilibrium effects of

Figure 10: Posts and hires by treatment status during the experiment and after



(a) Fraction of started job posts that are completed, during the experiment and after

(b) Fraction of completed job posts that make a hire, during the experiment and after



(c) Fraction of employers that make a hire, unconditional on posting, during the experiment and after

Notes: Panel A shows the fraction of employers who post a job, by treatment status. Panel B shows the fraction of job posts that make a hire. Panel C shows the fraction of employers that make a hire, regardless of if they post a job. The sample is all first job posts by employers allocated into a treatment cell. The original experiment is shaded in grey, between June 07, and July 20, 2023. Before the dashed red line 50% of all new employers were allocated to the treatment group, after the red line it was 95% of all new employers.

the treatment. A notable strength of our experimental design is its capacity to implement a form of DiD analysis that bypasses the need for the parallel trends assumption because of the random assignment of the two groups. This approach addresses several issues associated with DiD methods, as highlighted by recent advancements in the literature (Sun and Abraham, 2021; Roth and Sant’Anna, 2023; Roth, Sant’Anna, Bilinski and Poe, 2023).

The basic empirical strategy is to estimate a regression of the form

$$y_i = \alpha + \beta_1 \text{ROLLOUT}_i + \beta_2 \text{TRTAI}_i + \beta_3 (\text{ROLLOUT}_i \times \text{TRTAI}_i) + \varepsilon_i \quad (5)$$

on a dataset where each row is an employer initiating their first job post, indexed by i . ROLLOUT_i is 1 if the employer is posting this job after the near full roll-out and 0 if before, and TRTAI_i is 1 if they are allocated into the treatment group. The primary outcomes y_i we will investigate are whether or not the employer submits a job post, whether they hire conditional on posting, and whether they hire whether or not they posted a job.

In Table 9 we report the difference-in-differences estimates for the primary outcomes.²⁴ In Column (1) we find that prior to the roll-out, the treatment had a 16.5% increase in the probability an employer completed their first job post. This is lower than the 19% increase during the primary experiment, and the decrease in the treatment effect might reflect learning in the control group as employers became more aware of AI. After the near full roll-out, we see that the effect of the treatment substantially decreased. In the ROLLOUT period, treated employers are only 1.1 percentage points more likely than control employers to post a job, on a base of 27%, or a 4% treatment effect. In Panel (a) of Figure 10 we show this outcome over time, for treated and control employers. The plot shows the shrinking treatment effect over time, although the difference between the treatment and control groups remains significant.

In Table 9 Column (2) we show that the treatment effect on the fraction of hires from posted jobs is negative and similar in magnitude before and after the policy change. In Column (3) we show that the number of total hires in the treatment and control group is significantly lower after the policy roll out.

Overall, the large scale randomized roll-out yields two clear patterns. First, the extensive-margin posting response attenuates substantially when access expands from 50% to 95% of newly registered employers. Second, the reduction in hiring conditional on posting persists under near-universal availability, implying continued deterioration in match conversion even as entry responses weaken. The attenuation of the posting effect is consistent with market learning: marginal employers initially induced to post by the AI tool may have realized that their probability of hiring was lower than expected and gradually refrained from posting. However, the negative effect

²⁴In Appendix Figure 18 we report all of the labor market outcomes in Figure 7, in only the post period.

Table 9: Effect of AI treatment pre and post near full roll out

	<i>Dependent variable:</i>		
	Post	Hire [cond]	Hire [uncond]
	(1)	(2)	(3)
GenAI Treatment Assigned (TRTAI)	0.045*** (0.001)	-0.028*** (0.002)	0.001 (0.001)
ROLLOUT	-0.030*** (0.003)	0.037*** (0.005)	0.002* (0.001)
ROLLOUT x TRTAI	-0.034*** (0.003)	0.008 (0.005)	-0.003* (0.001)
Constant	0.272*** (0.001)	0.211*** (0.002)	0.057*** (0.0005)
Observations	1,033,681	281,214	1,033,681
R ²	0.003	0.002	0.00000

Notes: This table analyzes the effect of the AI treatment during the experiment and after the treatment was rolled out to almost all new employers. Prior to the red line, 50% of employers starting their first job post are allocated into the treatment group. In the week of October 8th, 2023, the tool was rolled out to 95% of all employers starting their first job post, with 5% remaining in a control group. ROLLOUT means the job was posted during the time that 95% of new entrants were in the treatment group. Allocation into treatment occurs when the employer begins a job post. Post is 1 if the employer allocated into the experiment posts a job. Hire [cond] is hires conditional on the employer posting a job. It is 1 if the job post that the employer was working on when they were allocated into the experiment makes a hire within 14 days. Hire [uncond] is hires unconditional on whether the employer posts. It is 1 if the employer allocated into the experiment makes a hire within 14 days, whether or not they complete a job post. The sample is made up of all new entrants to the platform and their outcomes during their first time starting a job post. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

on hiring conditional on posting persists even after the roll-out. This pattern indicates that while the market adjusts on the extensive margin of who posts, the core inefficiency introduced by AI-generated posts remains in converting vacancies into hires.

9 Decomposing hiring reduction into composition and treatment effects

In the experiment, employers in the treatment group who posted a job had a clearly lower probability of hiring. However, a natural question is how much of this is due to composition—the AI intervention made it easier for relatively lower-intent employers to post jobs—versus a direct treatment effect from the job posts’ lower likelihood of hiring. In the experiment, these two effects are impossible to separate because we do not know who the “marginal” employer was in the treatment group—though we can estimate the number of such employers. These marginal job posts are what drive the effects on the composition of posts in the market, but we also give evidence that the inframarginal job posts are negatively impacted by the treatment.

As a thought experiment, consider if we had measurable variation in the number of “marginal” employers, say across multiple experiments, for exogenous reasons. In that case, we could observe changes in the treatment effect on hire probability as a function of the number of marginal employers. If we saw, for example, there was no variation in the reduction in the *treatment effect* on hire probability, we could conclude that the negative hiring effects are driven primarily by the inframarginal employers who would have posted a job regardless of the treatment.

We can approximate this situation by using variation over time in the size of the treatment effect on posting, which varies the number of marginal employers, to see how it relates to the treatment effect on hiring probability. In Figure 11 we show the correlation between these two treatment effects over time.

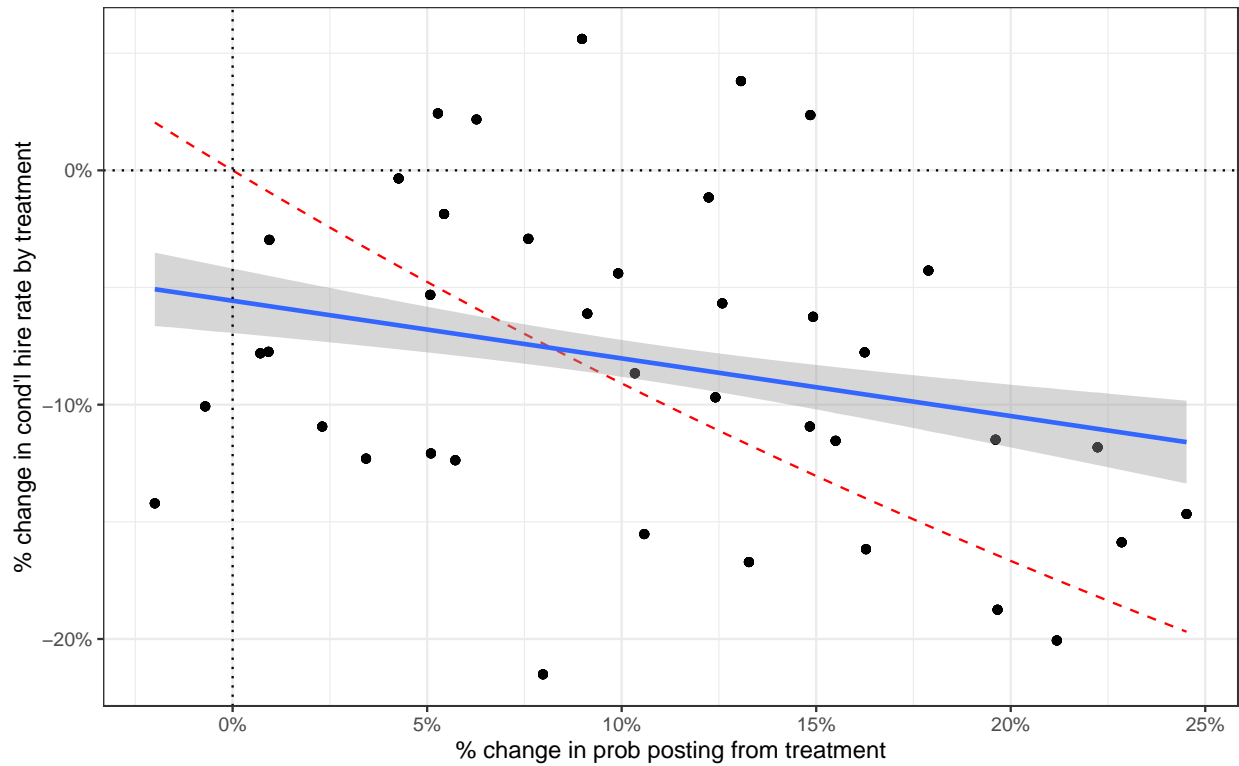
We can then run the regression of time-period t estimates of treatment effects on hiring, regressed on the treatment effect on posts.

$$\hat{\tau}_{ht} = \beta_0 + \beta_1 \hat{\tau}_{pt} + \epsilon \quad (6)$$

In this regression, τ_{ht} describes for each period t , the effect of the treatment on the probability an employer makes a hire, conditional on posting. τ_{pt} is each period’s treatment effect on the probability an employer makes a post. When $\tau_{pt} \approx 0$ (no marginal employers), β_0 is the effect of the AI intervention on inframarginal employers. And β_1 tells us how this hiring effect differs as we add more marginal employers.

Using this method we find that both the marginal and the inframarginal employers drive the

Figure 11: Weekly hiring rate treatment effect estimate versus weekly posting rate treatment effect, in percentage terms



Notes: The y-axis is the percentage change in hiring in treatment versus control employers, by week. The x-axis is the percentage change in the number of job posts in treatment versus control employers, by week. The blue line is the smoothed OLS line. The red dashed line indicates the expected relationship if marginal jobs had zero probability of filling and the inframarginal jobs were not affected. The x-axis is the job posting treatment effect, or the percentage difference between the posting rate in the treatment group and the control group. The y axis is the hiring treatment effect conditional on posting, or the percentage difference between the hiring rate in the treatment and control group. Each point refers to the treatment effect's for a given week.

negative hiring effect. In Table 10 we give results from running Equation 6 on the dataset of daily and weekly treatment effects. In Column (1) we show that when treatment effects are calculated at the daily level, the contribution from the inframarginal job posts is -0.038. The additional impact of the marginal posters is -0.078. However, if we look at Column (2) we see that when the treatment effects are calculated at the weekly level, the contribution of the marginal posts is much higher. The different effect sizes depending on the unit of time suggests that the daily data are picking up a lot of noise, suggesting that $\hat{\tau}_{pt}$ is measured with error. We believe that the weekly data are more reliable than the daily data. The intercept in Column (2) shows that even when the treatment causes no increase in job posts, there is still a negative 5.4% effect on hiring, suggesting that some of the negative effects are driven by the inframarginal jobs. The coefficient on “Posting TE” suggests that for every 1% increase in the posting treatment effect, there is a -0.28% decrease in hiring.

In Column (3) we use a “correlation correction” to rid the coefficient of attenuation bias due to measurement error (Spearman, 1987; De Winter et al., 2016). With this correction, the additional impact of the marginal posters is -0.56. This is much larger than the coefficient on the weekly estimate because of the large variation in both the daily and weekly data. The overall impact of the marginal employers is -0.56.

We find that most of the negative hiring effect is driven by changes in the composition of job posts. We multiply the above treatment effects from our preferred specification in Column (3) by their proportion of the job posts. The resulting overall contribution from the marginal employers is -0.56 multiplied by the fraction of job posts which are marginal.²⁵ The overall contribution from the inframarginal employers is -0.038 multiplied by the fraction of job posts which are inframarginal. Direct effects on the inframarginal employers make up 25% of the overall negative hiring effect, while 75% come from changes in the composition of job posts.

Table 10: Correlation between weekly treatment effects to posting and treatment effects to hiring

	<i>Dependent variable:</i>		
	Hiring TE		
	(1)	(2)	(3)
Posting TE	-0.078 (0.115)	-0.277* (0.154)	-0.560*** (0.115)
Constant	-0.038* (0.021)	-0.054*** (0.019)	-0.038* (0.021)
TE Calculated by:	Daily	Weekly	Daily
Attenuation bias correction:			X
Observations	269	39	269
R ²	0.002	0.081	0.002

Notes: This table decomposes the negative effect of the treatment to hiring into the inframarginal and marginal effects. We use the sample of all new entrants on the platform during and after the experiment. For each day in our data we calculate a treatment effect on posting and hiring. Column (1) reports the correlation between those treatment effects. Column (2) reports the same regression, but on treatment effects calculated by week. In Column (3), we still use daily treatment effects, but scale the coefficient using the variance of weekly treatment effects to correct for attenuation bias from daily measurement noise. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

This approach raises the question—what drives the variation in τ_{pt} and might this also affect τ_{ht} ? Our assumption is not that baseline hire rates and posting rates are constant over time or that the populations are otherwise constant—just that treatment effects are constant. A confounder could be that over time employers became better at using or less excited about using AI features.

²⁵While we do not know which job postings were induced by the treatment, we do know how many were. The number of marginal job postings is the number of jobs posted in the treatment group less the number posted in the control group.

We find no evidence that this is the case, as AI uptake remains level after the wider roll-out.²⁶

10 Conclusion

Matching markets depend on meaningful information to coordinate effort and attention. For example, jobseekers infer from job postings which vacancies are likely to hire them, and allocate search and application effort accordingly. When generative AI makes communication cheaper, markets can become congested—with more vacancies and more applications—but not necessarily more matches if the induced vacancies or applications are low quality or if the tool weakens signals of fit. We study this tension in a large, randomized field experiment on an online labor market, where the platform introduced a generative AI tool that provides newly registered employers with AI written first drafts of their job posts.

Treated employers were 19% more likely to post a job. The tool also delivered private benefits to employers, treated employers spent 44% less time writing their job posts, and those resulting job posts received at least as many applications as those from employers in the control group. Nonetheless, treated job posts were 15% less likely to make a hire. We decompose this decline into extensive and intensive margin channels: at least 75% of the negative treatment effect is driven by the marginal employers induced by the treatment, with the rest coming from direct treatment effects on inframarginal employers who would have posted a job with or without the treatment. We find that the treatment changed the information available to jobseekers. Treated job posts were more generic and more uniform in length. The treatment also impacted jobseekers' behavior. Jobseekers invested more in treated jobs—writing longer cover letters, bidding higher, and applying more frequently. However, their applications to treated jobs were less likely to result in an offer.

Despite the large increase in the number of job posts, the group of treated employers saw no more hires. Treated job posts received (a) more applications and (b) applications from jobseekers who were no worse on observables than control group job posts. Nevertheless, many fewer treated employers made offers, and those who did were more likely to have their offers rejected by the applicant. We rationalize these results with a model where the treatment induces more job posts, but these marginal job posts are relatively less valuable to employers and therefore less likely to result in a hire. Additionally, for the inframarginal job posts, the use of AI crowds out effort that employers would have put in themselves—resulting in more generic job posts and attracting worse fitting applicants.

A time-cost accounting exercise suggests that the loss to jobseekers is about six times larger than the gains to employers, and that each additional employer allocated to the experiment had a

²⁶In Appendix Figure 15 we show weekly opt in rates among treated employers and find that across all weeks the mean percentage of employers who opt in are between 70 and 75%.

negative effect on total welfare of \$2.15.

After the conclusion of the experiment, the treatment was rolled out to almost all new employers, and difference-in-differences estimates show at least as negative outcomes as in the experiment, suggesting that if there was employer learning it was not enough to eliminate the inefficiencies introduced by the AI written posts.

The results highlight a key insight for platform-design implication: when AI makes entry and communication close to costless, privately beneficial innovations can generate negative externalities. Platforms operating in high-AI environments may therefore benefit from mechanisms that restore credible signals of intent—for example, modest posting fees or other forms of friction that screen out low-quality postings. We contribute to research on AI in hiring, which has largely emphasized algorithmic search and screening (Tambe, Cappelli and Yakubovich, 2019; Le Barbanchon, Hensvik and Rathelot, 2023; Van den Broek, Sergeeva and Huysman, 2021), by documenting how generative AI changes behavior at the vacancy-creation stage and can reduce matching efficiency through weakened signals and increased congestion. Lastly, we provide novel evidence on core predictions from search and matching theory: while reducing job posting costs does increase job posts as predicted by Diamond (1984) and Mortensen and Pissarides (1994), it doesn't increase matches, highlighting the importance of job heterogeneity.

One limitation is that our estimates only capture effects in the first year of the roll-out; long-run equilibrium responses may differ as participants and platforms adapt (Bhuller, Havnes, McCauley and Mogstad, 2024; Autor, Dorn and Hanson, 2016). Nonetheless, we find the response to the introduction of generative AI in a large, real two-sided market shows that even a “perfect” AI that could frictionlessly communicate employers' preferences would not necessarily improve hiring outcomes if it simultaneously weakens signals of employer intent that guides jobseekers' search.

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A Appendix

A.1 Cosine similarity of job post text embeddings

In Table 30, our outcome of interest is the mean of the cosine similarities between the embedding of job post i and the embeddings for each other job post $-i$. A cosine similarity of 1 means the texts are identical, and a cosine similarity of 0 means they are completely orthogonal. The treatment effect is small, only 0.014 on a base of 0.75. However, the range of average cosine similarities is very narrow—the lowest average cosine similarity is 0.65 while the highest is 0.81²⁷. This is because, despite the fact that these job posts can be in very different industries, they are all job posts. This treatment effect covers 10% of the distance between the minimum and maximum average cosine similarity.

Table 11: Mean cosine similarity of job posts by treatment cell

	<i>Dependent variable:</i>	
	Mean cosine similarity	Rank
	(1)	(2)
GenAI Treatment Assigned	0.014*** (0.0002)	-6,565.200*** (98.146)
Constant	0.753*** (0.0002)	19,883.000*** (72.941)
Observations	32,513	32,513
R ²	0.112	0.121

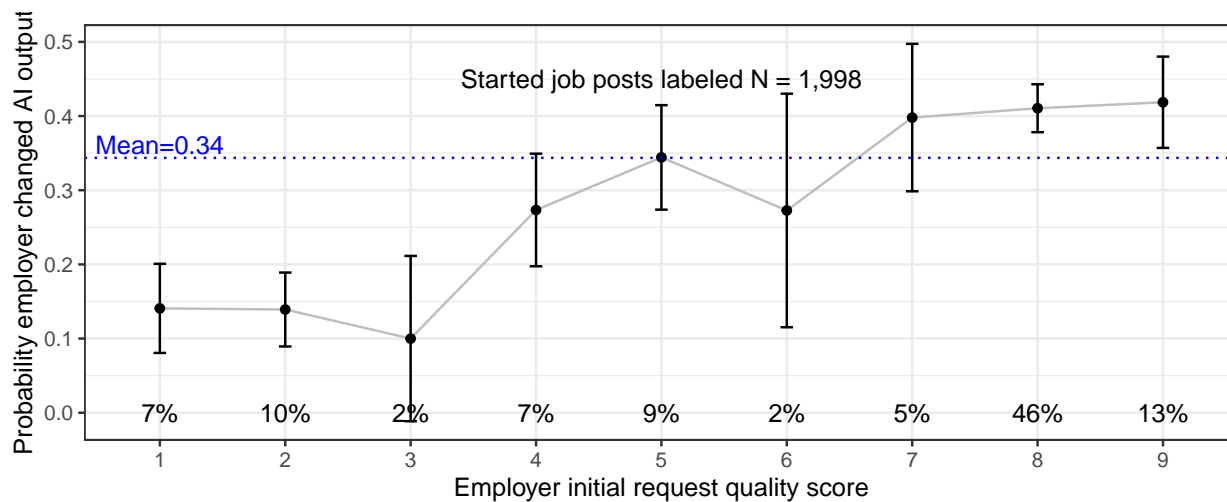
Notes: This table analyzes the effect of the treatment on how different job posts are from each other. For each job post we get the embeddings using OpenAI’s ‘text-embedding-ada-002’ model, we then create a matrix of the cosine similarity between each job post and each other job post in the experiment. Then for each job post we take the mean of all of the cosine similarities, as a proxy for how generic a job post is. The outcome in column (1) is the mean cosine similarity between the ego and all other job posts in the experiment. The outcome in column (2) is the rank of those job posts in descending order. The sample consists of the subset of the experimental sample which post a job, and randomization occurs at the job post (and employer) level. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

A.2 Employer effort

Employers that wrote better initial prompts were also more active editors of the AI-generated text. We can see this in Figure 12, where we plot the relationship between probability of editing and the prompt quality of the initial human prompt. About a 1/3 of all employers edited the AI-generated

²⁷See Appendix Section A.8 for examples of job posts with average cosine similarities at the minimum, maximum, and mean average cosine similarity.

Figure 12: Initial employer-written prompt quality score and probability of that employer editing the AI-generated text



Notes: This figure shows the mean edit probability for treatment employers posting a job, by a GPT4o generated scores of the informativeness of the original prompt.

text. But among those writing the most informative initial prompts, it was close to 40%, while those with the lowest scores, only about 15% did any editing at all.

This apparent complementarity of effort is potentially surprising if these two points of effort could be substitutes: more work upfront in writing a great prompt leads to a better AI generated text that requires less editing. Employers that wrote short, uninformative requests would be the most active editors, changing details or augmenting the AI text to create a suitable final job post.

While the cross-sectional relationship between prompt quality and editing shows complementarity, this actually conceals a substitution that occurs: if we score the quality of the AI generated job post, we can see that the better the job post, the less likely it is edited. Table 12 Column (1) shows that one standard deviation increase in the quality of the AI written draft is associated with an employer being 4.6% less likely to make changes. Column (2) shows that one standard deviation increase in prompt quality is associated with employers being 10% less likely to make changes.

Note that the negative association between AI text quality and editing is even stronger if we control for the initial prompt quality. What this suggests is that there is some latent factor like “Employer interest” or “Employer motivation” that affects both the initial prompt quality and the probability of editing. If this latent factor is also correlated with employers’ propensity to hire—which seems likely—then it suggests that the treatment selected for relatively low intent employers.

Table 12: Whether the employer made changes to the AI-generated text

<i>Dependent variable:</i>		
Did the employer make changes to AI-generated post?		
	(1)	(2)
AI Text Quality	-0.046*** (0.011)	-0.059*** (0.010)
Prompt Quality		0.108*** (0.010)
Constant	0.344*** (0.011)	0.344*** (0.010)
Observations	1,999	1,999
R ²	0.010	0.061

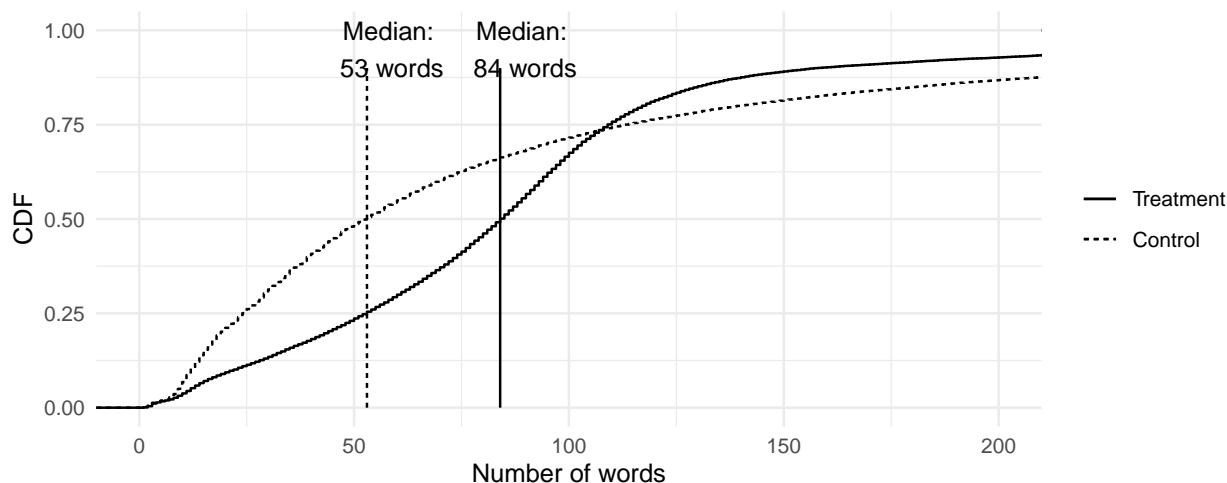
Notes: This table measures the correlation between whether or not an employer who received the AI-written job post draft made any edits to the writing. The sample is 1,999 randomly drawn job posts from employers in the treatment group who submitted a job post. The dependent variable is an indicator for employers who made at least one edit to the AI-written draft. AI Text Quality is the z score of a measure of the quality of the AI-written draft. Prompt Quality is the z score of the quality of the prompt that employers provide to the AI giving a “one or two sentence description of what [they’re] looking for.” Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Table 13: Effects of generative AI on length of job post

<i>Dependent variable:</i>			
Number of words in job post			
	OLS	Quantile Regression	
	(1)	(2)	(3)
GenAI Treatment Assigned (Trt)	1.888** (0.801)	1.975* (1.178)	31.000*** (0.563)
Anglophone		5.892*** (1.185)	
Anglophone X Trt		0.188 (1.606)	
Constant	94.089*** (0.589)	90.834*** (0.881)	53.000*** (0.488)
Comparing	Means	Means	Medians
Observations	58,495	58,495	58,495
R ²	0.0001	0.001	

Notes: This table analyzes the effect of the treatment on the number of words in the job posts. In Columns (1) and (2) are Ordinary Least Squares models. Column (3) compares the median number of words in job posts. “Anglophone” is 1 if the employer registers from an anglophone country, defined as the United States, Canada, United Kingdom, Ireland, or New Zealand. The sample is conditional on jobs which were posted. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Figure 13: Cumulative distribution function of the number of words in job posts



Notes: CDF of the number of words in employer job posts, by treatment status.

A.3 Difference in distributions of job post length

The difference in distributions is readily apparent graphically. Figure 13 plots the CDF of job post length for both groups. The median job in the control group had 53 words, while the median number of words in job posts in the treatment group was 84 words.

A.4 Applicant pools were not worse overall

The lack of additional hires in the treated group is not caused by worse applicant pools. Treated jobs actually received more applications, as shown in Table 3, but perhaps job posts induced by the treatment had lower interest from high-quality applicants.

When an employer collects applications for a job post, the platform recommends some applicants based on their wages, ratings, and employment history on the platform. In Table 14, the outcome of interest is the share of a job's applications which came recommended from the platform. Jobs in the treatment group saw a larger share of their applications come from recommended applicants.

A.5 Treated job posts had a higher fraction of their applications in common with other job posts

Treated job posts were more generic. As such, they might receive less distinctive pools of applications, perhaps making it harder for the employer to find a suitable candidate. To quantify this, we create a two-dimensional matrix of job posts where the (m, n) entry in the matrix is the cosine

Table 14: Effects of generative AI on quality of applicant pool

	<i>Dependent variable:</i>	
	Share of apps recommended	Number of recommended apps
	(1)	(2)
GenAI Treatment Assigned	0.007*** (0.003)	0.454*** (0.070)
Constant	0.302*** (0.002)	4.994*** (0.052)
Observations	58,495	58,495
R ²	0.0001	0.001

Notes: This table analyzes the impact of the treatment on the quality of a jobs' applicant pool. Share of apps recommended is the mean of applications a job post receives which the platform flags as recommended. Number of recommended apps is the number of application a job post receives which are recommended. The sample is conditioned on employers who posted a job which received at least one application. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

similarity of the n th and m th job posts in terms of the applications they received. For each job post i , we then take the mean across all other job posts $-i$. This gives each job post an average measure of application overlap.

Table 15: Average share of applications in common with other job posts, times 100

	<i>Dependent variable:</i>
	Mean share of apps in common
GenAI Treatment Assigned	0.004*** (0.001)
Constant	0.053*** (0.001)
Observations	54,145
R ²	0.001

Notes: This table analyzes the effect of the treatment on how many applications job posts share with other job posts. We construct a matrix of all job posts, where the m by n th element is the fraction of the m th job posts' applications which come from a freelancer who also applies to the n th job. For each job post we take the mean of this measure across all other job posts. This is the independent variable. The sample is conditioned on employers who posted a job post which received at least one application. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

In Table 15, we show that the mean share of applications in common is higher for jobs in the treatment group. In the control group, a job post's cosine similarity in terms of the applications it shares with other job posts in the experiment is 0.053. In the treatment group, it is 0.057. In both cases, the cosine similarity is very low but in the treatment group the similarity between job posts

is higher. However, this might be because treated job posts get a larger number of applications overall.

A.6 Treated non-native English speakers experienced more rejections after making an offer

Most offers are accepted. In our sample, 83% of job offers result in a hire. Table 16 Column (1) shows that workers given offers by employers in the treatment group were 2 percentage points less likely to accept. However, we can see from Column (2) that this result is driven entirely by non-native English speaking employers.

Table 16: Effects of generative AI on the share of offers that are accepted

	<i>Dependent variable:</i>	
	Offer accepted	
	(1)	(2)
GenAI Treatment Assigned (Trt)	-0.019*** (0.006)	-0.037*** (0.010)
Anglophone		0.162*** (0.009)
Anglophone X Trt		0.031** (0.013)
Constant	0.825*** (0.005)	0.721*** (0.007)
Observations	14,731	14,731
R ²	0.001	0.049

Notes: This table analyzes the effect of the treatment on the share of offers that are accepted. Offer accepted is 0 if an offer is made which does not lead to a hire and 1 if it does lead to a hire. The sample is made up of all employers in the experimental sample who post a job and make at least one offer. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

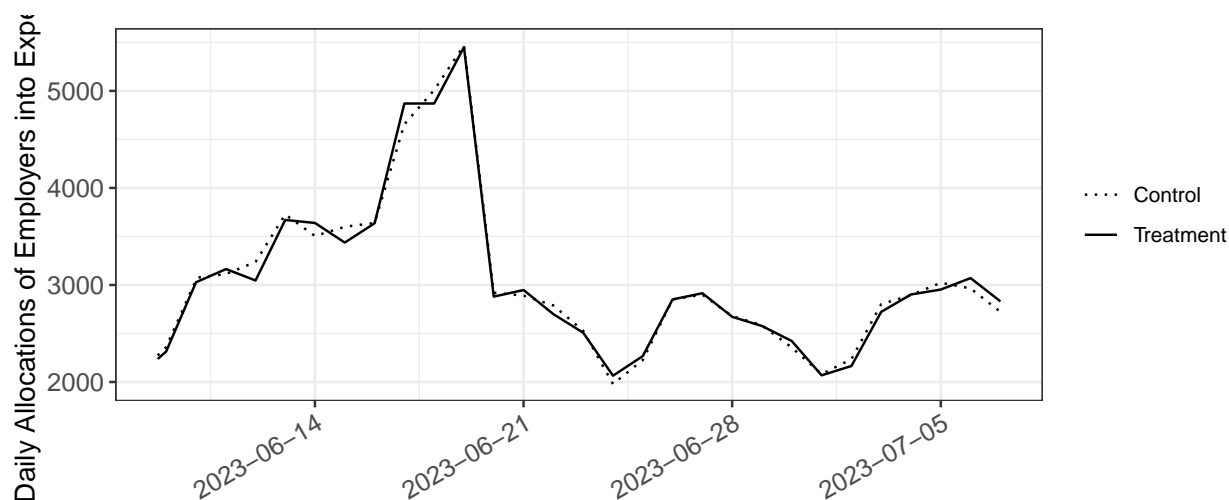
A.7 Proposition 3

Proposition 3. *The net effect of lowering ρ and c on the unconditional number of hires is equal to $\delta_1 + \delta_2 + \delta_3$, which can be positive, negative, or zero.*

Formally, the effect to the unconditional probability of hiring is given by $\delta_1 + \delta_2 + \delta_3$. The first two terms are positive and the third term is negative, and the net effect will depend on which forces dominate. The part of the effect driven by marginal job posts δ_1 is positive, where $\delta_1 = \int_{\underline{v}^1}^{\underline{v}^0} (1 - (1 - \frac{m}{\rho_1 \gamma})^N)(v - w)dG(v)$. The effect driven by the hiring probability of inframarginal posters who

never put in effort δ_2 is also positive, where $\delta_2 = \int_{v^l}^{v^0} (v - w) \left(\left(1 - \frac{m}{\rho_0 \gamma}\right)^N - \left(1 - \frac{m}{\rho_1 \gamma}\right)^N \right) dG(v)$. And lastly, the effect of the treatment crowding out effort from employers who would otherwise have put in effort is given by $\delta_3 = \int_{v_h^0}^{v_h^1} (v - w) \left(\left(1 - \frac{m}{\rho_1 \gamma}\right)^N - \left(1 - \frac{m}{\gamma}\right)^N \right) dG(v)$. If the forces are balanced and $\delta_1 + \delta_2 = \delta_3$, then the unconditional impact to hires is 0.

Figure 14: Daily allocations of employers into experimental cells



Notes: This plot shows the daily allocations into the treatment and control cells for the experimental sample of 180,324 employers.

A.8 Additional tables and figures

Table 17: Balance table: Geographic distribution by treatment assignment

Variable	Control (N)	Control (%)	Treatment (N)	Treatment (%)	Difference (%)	P-value
United States	28,547	31.34	28,694	31.57	0.23	0.29
North America	32,245	35.4	32,333	35.58	0.18	0.43
South America	1,299	1.43	1,316	1.45	0.02	0.71
Europe	16,626	18.25	16,558	18.22	-0.03	0.86
Asia	24,434	26.82	24,259	26.69	-0.13	0.53
Africa	5,941	6.52	5,721	6.3	-0.23	0.05

Notes: This table shows the balance of employers between the treatment and control group. Randomization occurs immediately following registration at the moment the employer clicks on “Post a job.” The only information available at this stage is the country the employer reports registering from.

Table 18: Summary statistics by treatment assignment

treatment	mean	sd	se	median	min	max	n
Employer from anglophone country							
0	0.440	0.496	0.002	0.0	0.0	1.0	90,276
1	0.443	0.497	0.002	0.0	0.0	1.0	90,048
Opt-in rate for AI feature							
0	1.000	NA	NA	1.0	1.0	1.0	1
1	0.752	0.432	0.003	1.0	0.0	1.0	27,111
Job posting rate							
0	0.297	0.457	0.002	0.0	0.0	1.0	90,276
1	0.352	0.478	0.002	0.0	0.0	1.0	90,048
Minutes spent on job post							
0	8.142	8.621	0.067	5.2	1.4	63.3	16,489
1	4.549	5.453	0.037	2.6	0.5	40.7	21,714
Number of words in job post							
0	94.089	111.365	0.681	53.0	0.0	1,074.0	26,781
1	95.977	81.804	0.459	84.0	0.0	1,126.0	31,714

Notes: This table provides summary statistics for the main outcomes in the paper. Opt-in, Post, and Hired (Unconditional) are determined for the entire experimental sample. Minutes, Skill Count, Number of Words, Number of Applications, Any Interviews, Any Invites, Any Shortlists, Made an offer, and Hired (Conditional on Posting) are all conditional on the employer posting a job. Minutes is NA if the employer got timed out while writing the job post or closed and reopened the browser window while writing. Mean Bid per Hour is the average wage bid for hourly jobs, conditional on the employer posting an hourly job and receiving at least one application. Accepted offer is conditional on the employer making an offer. Max Hourly Rate is the highest hourly wage the employer paid any worker for that job post, conditional on hiring at least one hourly worker.

Table 20: Summary statistics on employer market outcomes, by treatment assignment

treatment	mean	sd	se	median	min	max	n
Number of applications received							
0	15.638	18.039	0.110	11.0	0.0	974.0	26,781
1	16.468	18.977	0.107	11.0	0.0	508.0	31,714
Any interviews by employer							
0	0.437	0.496	0.003	0.0	0.0	1.0	26,781
1	0.378	0.485	0.003	0.0	0.0	1.0	31,714
Any invites by employer							
0	0.376	0.484	0.003	0.0	0.0	1.0	26,781
1	0.360	0.480	0.003	0.0	0.0	1.0	31,714
Any shortlists							
0	0.157	0.364	0.002	0.0	0.0	1.0	26,781
1	0.135	0.342	0.002	0.0	0.0	1.0	31,714
Mean wage bid							
0	29.556	16.595	0.133	26.0	4.0	100.0	15,589
1	27.326	14.598	0.106	24.5	4.0	100.0	18,875
Employer made a job offer							
0	0.271	0.444	0.003	0.0	0.0	1.0	26,781
1	0.236	0.424	0.002	0.0	0.0	1.0	31,714
Job offer accepted							
0	0.825	0.380	0.004	1.0	0.0	1.0	7,258
1	0.806	0.395	0.005	1.0	0.0	1.0	7,473
Hired (conditional on posting)							
0	0.224	0.417	0.003	0.0	0.0	1.0	26,781
1	0.190	0.392	0.002	0.0	0.0	1.0	31,714
Hired (Unconditional)							
0	0.066	0.249	0.001	0.0	0.0	1.0	90,276
1	0.067	0.250	0.001	0.0	0.0	1.0	90,048
Max Hourly Rate							
0	30.408	35.941	0.721	20.0	3.0	700.0	2,483
1	28.619	33.525	0.670	20.0	3.0	495.0	2,503

Notes: This table provides summary statistics for the main outcomes. Opt-in, Post, and Hired (Unconditional) are determined for the entire experimental sample. Minutes, Number of Applications, Any Interviews, Any Invites, Any Shortlists, Made an offer, and Hired (Conditional) are conditional on the employer posting a job. Mean wage bid is the mean wage bid for hourly jobs, conditional on the employer posting an hourly job and receiving at least one application. Accepted offer is conditional on the employer making an offer. Max Hourly Rate is the highest hourly wage the employer paid any worker for that job post, conditional on hiring at least one worker.

Table 22: Summary statistics on job applications, by treatment assignment of applied-to job

treatment	mean	sd	se	median	min	max	n
Hourly rate							
0	28.544	20.885	0.036	25.0	4.0	100.0	330,033
1	26.118	19.327	0.030	20.0	4.0	100.0	423,503
Any offers							
0	0.088	0.283	0.000	0.0	0.0	1.0	528,677
1	0.072	0.258	0.000	0.0	0.0	1.0	645,765
Hired							
0	0.024	0.153	0.000	0.0	0.0	1.0	528,677
1	0.020	0.140	0.000	0.0	0.0	1.0	645,765
Interviewed							
0	0.111	0.314	0.000	0.0	0.0	1.0	528,677
1	0.093	0.290	0.000	0.0	0.0	1.0	645,765
Short-listed							
0	0.031	0.175	0.000	0.0	0.0	1.0	528,677
1	0.025	0.156	0.000	0.0	0.0	1.0	645,765

Notes:

Table 24: Effects of AI written draft on wage bids for hourly job posts

	<i>Dependent variable:</i>	
	Mean bids (1)	Log mean bids (2)
GenAI Treatment Assigned	-2.230*** (0.168)	-0.055*** (0.006)
Constant	29.556*** (0.124)	3.274*** (0.004)
Observations	34,464	34,464
R ²	0.005	0.003

Notes: This table analyzes the effect of the treatment on the average wage bids the employer receives within 14 days of posting a job. Mean bid for hourly jobs is the mean bid an hourly job post received within 14 days. The first specification is in levels while the second specification is logged hourly wage bids. The sample is made up of all employers in the experimental sample. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Table 25: Downstream impacts of Gen AI on wagebill, conditional on hiring

	<i>Dependent variable:</i>		
	Wagebill (1)	Hours worked (2)	Hourly wage (3)
GenAI Treatment Assigned	78.908 (81.091)	3.510 (2.779)	-1.789* (0.984)
Constant	478.849*** (57.424)	43.460*** (1.970)	30.408*** (0.697)
Observations	12,017	4,330	4,986
R ²	0.0001	0.0004	0.001

Notes: This table analyzes the effect of the treatment on the size of contracts for all employers in the experimental sample. All outcomes are conditional on the employer making a hire. The outcome in Column (1) is what the employer pays any hire for the job, conditional on them making at least one hire. Wagebill includes both hourly and fixed price jobs. The outcomes in Columns (2) and (3), hours worked and hourly rate, are only for hourly jobs. Hourly rate is the maximum hourly rate the employer pays any worker for the job. The samples are made up of all employers in the experimental sample. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Table 26: Effects of generative AI on number of hires

	<i>Dependent variable:</i>	
	Offer, conditional on posting a job	
	(1)	(2)
GenAI Treatment Assigned (Trt)	-0.035*** (0.004)	-0.033*** (0.005)
Anglophone		0.106*** (0.005)
Anglophone X Trt		0.001 (0.007)
Constant	0.271*** (0.003)	0.212*** (0.004)
Observations	58,495	58,495
R ²	0.002	0.017

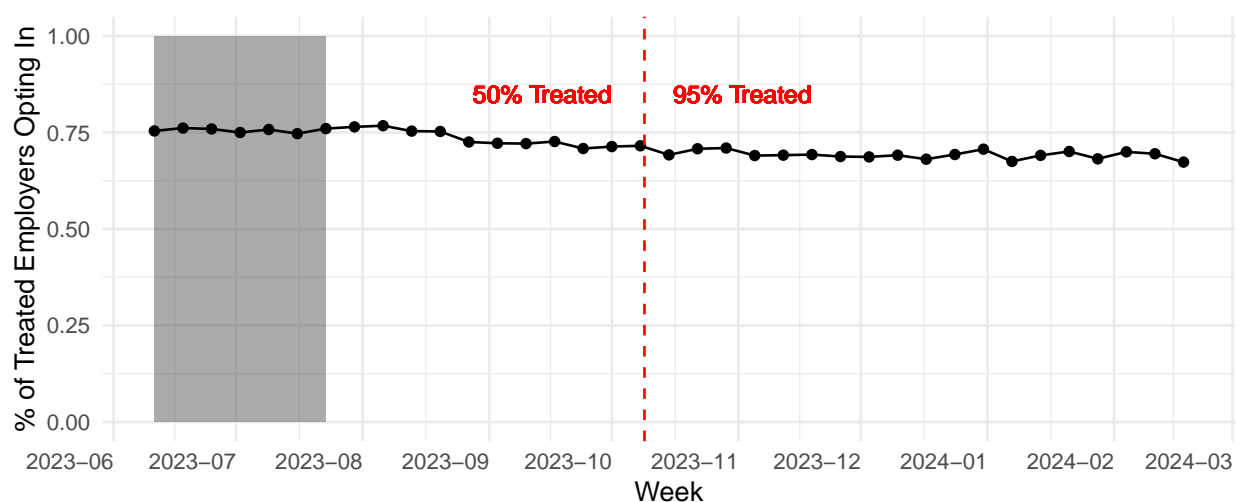
Notes: This table analyzes the effect of the treatment on if employer makes an offer. Ever offer, unconditional on post is 1 if the employer makes any offer within 14 days of being allocated into the experiment. Offer, conditional on post is 1 if the job post that the employer was working on when they were allocated into the experiment makes an offer within 14 days. Number of hires is the number of distinct contracts that form as a result of the job post. The sample is made up of all employers in the experimental sample. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Table 27: Effects of generative AI on main outcomes, unconditional on posting

	<i>Dependent variable:</i>			
	Job Post	Num Apps	Employer Makes Offer	Hired
	(1)	(2)	(3)	(4)
GenAI Treatment Assigned (Trt)	0.066*** (0.003)	1.422*** (0.081)	0.004** (0.002)	0.001 (0.002)
Anglophone	0.136*** (0.003)	2.974*** (0.086)	0.068*** (0.002)	0.068*** (0.002)
Anglophone X Trt	-0.024*** (0.004)	-0.610*** (0.122)	-0.004 (0.003)	-0.001 (0.002)
Constant	0.237*** (0.002)	3.331*** (0.057)	0.050*** (0.001)	0.036*** (0.001)
Observations	180,324	180,324	180,324	180,324
R ²	0.021	0.013	0.015	0.018

Notes: This table analyzes the effect of the treatment on hiring outcomes, unconditional on any behavior of the employer. Job Posted is 1 if the employer submits a job post within 14 days of beginning their job post and being allocated into the experiment. Num Apps is the number of applications the job post receives if the employer posts a job, or zero if else. Employer Makes Offer is 1 if the employer makes any offer within 14 days of being allocated into the experiment. Hire, unconditional on post is 1 if the employer makes any hire within 14 days. The sample is made up of all employers in the experimental sample. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Figure 15: Fraction of treated employers who opt in to receive AI written first draft



Notes: This plot shows the fraction of treated employers who opt in to receiving the AI written first draft of their job post. The sample is all first job posts by employers allocated into a treatment cell. The original experiment is shaded in grey, between June 07, and July 20, 2023. Before the dashed red line 50% of all new employers were allocated to the treatment group, after the red line it was 95% of all new employers.

Table 28: Effects of generative AI on main outcomes, conditional on posting

	<i>Dependent variable:</i>			
	Num Apps	Employer Makes Offer	Accepted Offer	Hired
	(1)	(2)	(3)	(4)
GenAI Treatment Assigned (Trt)	1.652*** (0.226)	-0.033*** (0.005)	-0.037*** (0.010)	-0.030*** (0.005)
Anglophone	2.863*** (0.228)	0.106*** (0.005)	0.162*** (0.009)	0.128*** (0.005)
Anglophone X Trt	-1.406*** (0.308)	0.001 (0.007)	0.031** (0.013)	0.001 (0.007)
Constant	14.056*** (0.169)	0.212*** (0.004)	0.721*** (0.007)	0.153*** (0.004)
Observations	58,495	58,495	14,731	58,495
R ²	0.004	0.017	0.049	0.027

Notes: This table analyzes the effect of the treatment on hiring outcomes, conditional on the employer posting job. An observation is one employer's first job post. Number of apps is the number of applications the job post receives if the employer posts a job. Employer Makes Offer is 1 if the employer makes any offer within 14 days of being allocated into the experiment. Accepted offer is not only conditional on posting a job, but also conditional on making an offer, and is 1 the applicant accepts an offer made. Hired is 1 if the employer makes any hire within 14 days, conditional on posting a job. The sample is made up of all employers in the experimental sample who post a job within 14 days of being allocated into the experiment. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Table 29: Correlation between category treatment effects to posting and treatment effects to hiring

	<i>Dependent variable:</i>
	Hiring TE
Posting TE	-0.670*** (0.201)
Constant	-0.001 (0.043)
Observations	12
R ²	0.526

Notes: This table decomposes the negative effect of the treatment to hiring into the inframarginal and marginal effects. We use the sample of all new entrants on the platform during and after the experiment. For each category in our data we calculate a treatment effect on posting and hiring. Categories are weighted by the number of jobs posted in each category in the control group. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

A.9 Embeddings in table form

Table 30: Mean cosine similarity of job posts by treatment cell

	<i>Dependent variable:</i>	
	Mean cosine similarity	Rank
	(1)	(2)
GenAI Treatment Assigned	0.014*** (0.0002)	−6,565.200*** (98.146)
Constant	0.753*** (0.0002)	19,883.000*** (72.941)
Observations	32,513	32,513
R ²	0.112	0.121

Notes: This table analyzes the effect of the treatment on how different job posts are from each other. For each job post we get the embeddings using OpenAI’s ‘text-embedding-ada-002’ model, we then create a matrix of the cosine similarity between each job post and each other job post in the experiment. Then for each job post we take the mean of all of the cosine similarities, as a proxy for how generic a job post is. The outcome in column (1) is the mean cosine similarity between the ego and all other job posts in the experiment. The outcome in column (2) is the rank of those job posts in descending order. The sample consists of the subset of the experimental sample which post a job, and randomization occurs at the job post (and employer) level. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

In Table 30, our outcome of interest is the mean of the cosine similarities between the embedding of job post i and the embeddings for each other job post $-i$. A cosine similarity of 1 means the texts have the same embedding, and a cosine similarity of 0 means they are completely orthogonal.

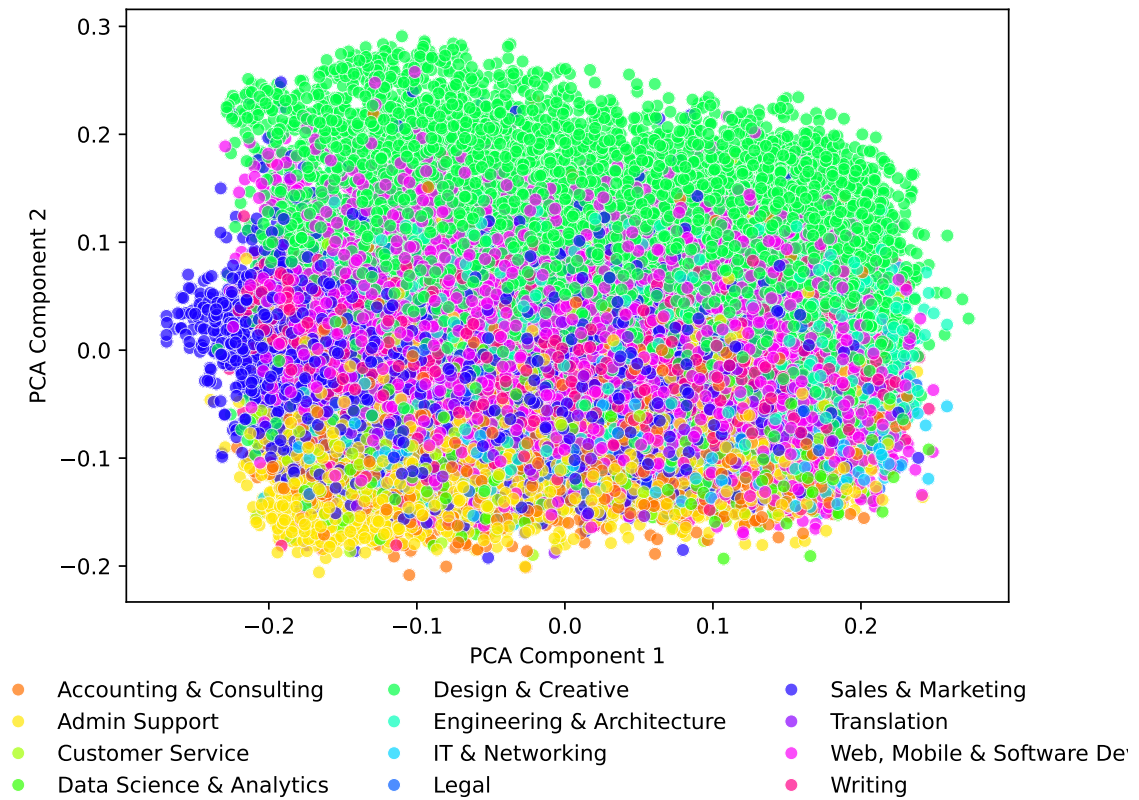
We find that job posts in the treatment group were on average closer to the mean job post embedding than job posts in the control group. The treatment effect is small, only 0.014 on a base of 0.75. However, the range of average cosine similarities is very narrow—the lowest average cosine similarity is 0.67 while the highest is 0.81.²⁸ This is because, despite the fact that these job posts can be in very different industries, they are all job posts. This treatment effect covers 10% of the distance between the minimum and maximum average cosine similarity.

A.10 Job post embeddings, additional figures

Given the high-dimensional nature of the embeddings, we cannot directly visualize them. To this end, we apply Principal Component Analysis (PCA) to reduce the dimensionality of the embeddings to two principal components, allowing us to visualize the embeddings in a 2D space. This

²⁸See Appendix Section A.10.1 to see examples of job posts with average cosine similarities at the min, max, and mean average cosine similarity.

Figure 16: Embeddings of Job Posts Reduced to 2 Dimensions, by Job Category



Notes: This plot shows the text of all job posts reduced to two dimensions. We use OpenAI’s “text-embedding-ada-002” model to turn the text of job posts into embeddings, and then use PCA to reduce the dimensionality of the embeddings into two dimensions.

reduction preserves as much of the variance in the data as possible in 2D. In Appendix Figure 16 we plot the job posts by category to show that this way of visualizing the job posts picks up meaningful differences between them.

We plot the embeddings for the treatment and control group in Appendix Figure 17. While the principal components themselves are not directly interpretable due to their composite nature, they still can facilitate a visual comparison of the job postings' embeddings. Most notably, the treatment appears to cause a shift in the distribution along the first principal component. We plot the 2D embeddings for the control group in Appendix Figure 17a. We investigate the treatment group in Appendix Figure 17b. Here we break down the job posts in the treatment group into those that opted in to receive the AI-written first draft, plotted in blue, and those that opted out, plotted in red. For 75% of the job posts the employer opted in to receive the draft, and therefore the vast majority of embeddings are in blue. While the red embeddings for those that opted out are placed more uniformly across the distribution of the first component, the ones that opted in are clustered to the right.

A.10.1 Range of average cosine similarities of job posts

The interpretation of the 0.014 effect of the AI treatment on the average cosine similarity of job posts does not have an obvious interpretation. A higher average cosine similarity means a job post is more similar to other job posts in the experiment, a lower average cosine similarity means a job post is more unique. The range of average cosine similarities amongst these job posts is 0.67 to 0.81, with a mean of 0.75. The following are examples of job posts at each of these points.

Most unique: Job post with average cosine similarity of 0.67

What needs to be done: in `main.cpp` file, I am calculating the force for all the particles. I want to optimize it. There are two linked lists in `int main()`. I want you to use them (`list_cell` and `list_particle`) basically, you have to fill the `list_cell` with `list_particle`'s ID. and calculate force (in `compute_acceleration`) only for the particles which are in the same cell and neighbor cells, not for all. That's all. I would like to understand how you did it. Here is the code: [\(link removed\)](#)
 Run the project: get into the build folder...in terminal type `Make` then: `./md 100 10 0.01` (particles, Time, delta Time) For visualization, just download Paraview and open VTK file.

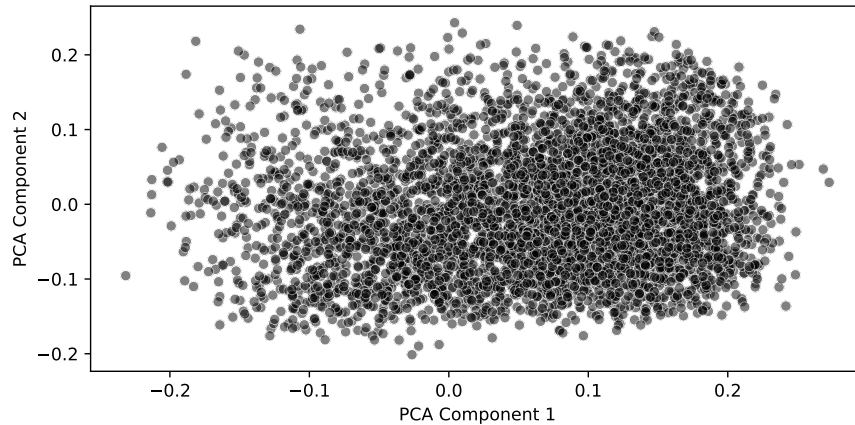
Mean uniqueness: Job post with average cosine similarity of 0.75

I'm looking for a logo for a small health and wellness company. I am a naturopathic doctor with an emphasis on weight loss counseling. I'm looking to have a design within the next month. I'm looking for a simple and clean logo that is modern yet whimsical and will transfer easily between Instagram, Facebook, a website, and other business materials like treatment plans, recipe books, etc.

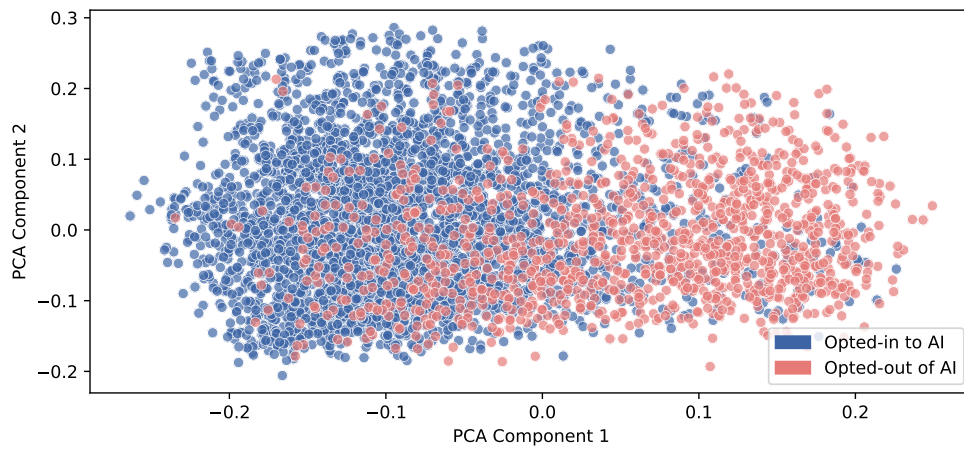
Most generic: Job post with average cosine similarity of 0.81

We are looking for a skilled professional to assist us in creating a website and driving business growth. The ideal candidate will have expertise in web development and marketing strategies. The responsibilities include designing and developing a user-friendly and visually appealing website that aligns with our brand image and business objectives. The candidate should also possess knowledge of SEO techniques, social media marketing, and content creation to drive organic traffic and increase conversions. Excellent communication and project management skills are essential for effectively collaborating with team members and delivering satisfactory results within the specified timeframe.

Figure 17: Embeddings of Job Posts Reduced to 2 Dimensions



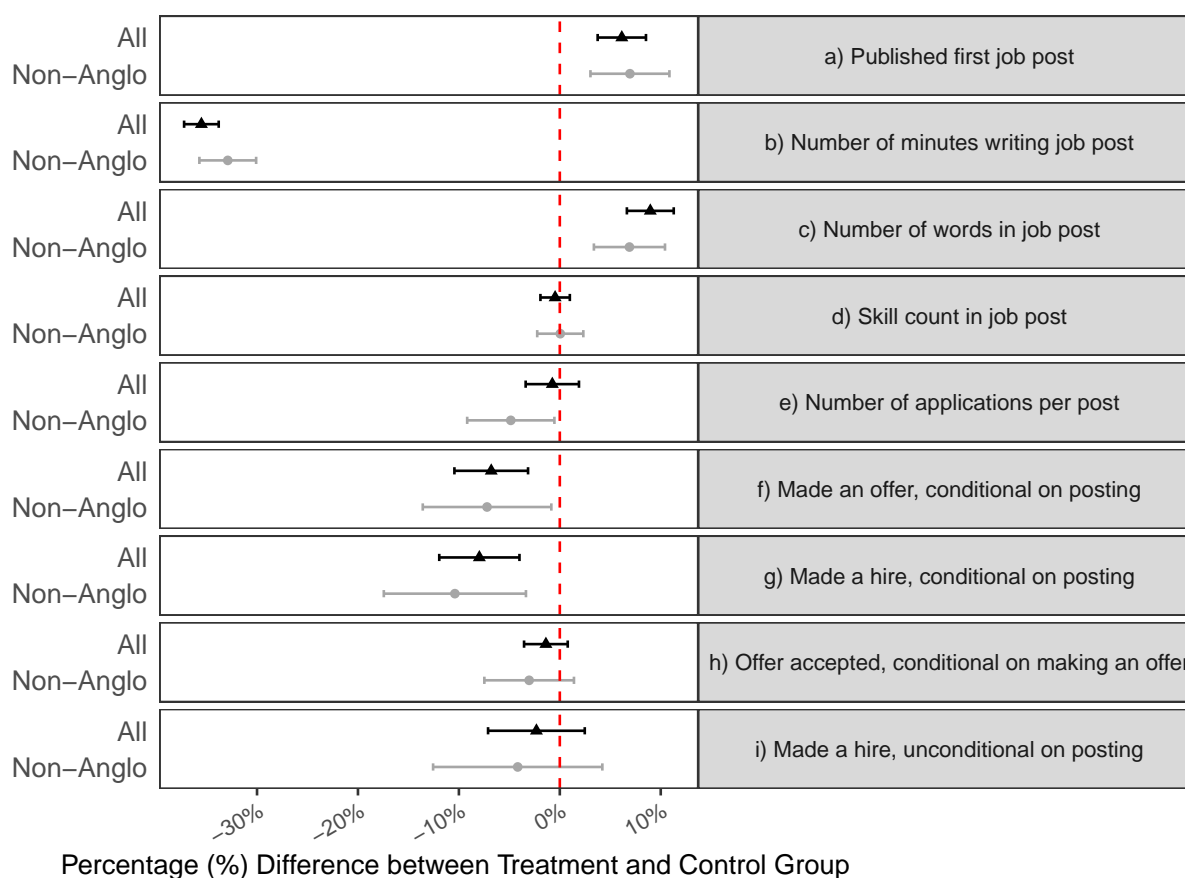
(a) Embeddings of Job Posts Reduced to 2 Dimensions for Control Group



(b) Embeddings of Job Posts Reduced to 2 Dimensions for Treatment Group

Notes: These plots show the job posts' embeddings reduced to two dimensions. We use OpenAI's "text-embedding-ada-002" model to turn the text of job posts into embeddings, and then use PCA to reduce the dimensionality of the embeddings into two dimensions. We then take a random sample of 5,000 job posts in the treatment group and 5,000 job posts in the control group, for ease of visualization.

Figure 18: Experimental impacts after the close to market wide roll out



Notes: This analysis looks at the effect of being assigned treatment on outcomes for employers after the near market wide roll out on new entrants, where 95% of the newly registered employers were randomly assigned the treated group. The x-axis is the percentage difference in the mean outcome between employers in the treated group and the control group. The outcome a) published first job post is a 0 if the employer never posts a job after allocations and 1 if they do. The outcomes b), c), d), e), f), g), h) are all conditional on the employer posting a job. The outcome i) made a hire is unconditional on posting a job, it is 0 if the employer doesn't hire anyone after allocations and 1 if they do. A 95% confidence interval based on standard errors calculated using the delta method is plotted around each estimate. The sample is of all new employers who started to post a job between October 10th, 2023 and February 18, 2024, with $N = 525,119$, where 95% were in the treatment group and 5% in the control group.

A.11 Prompts

```
<basicSystemPrompt> You are a(n) [platform] client posting a
  ↪ job.
```

```
<basicUserPrompt> Based on the following job requirements,
  ↪ write:
```

```
# Title
```

```
# Detailed job description:
```

```
## Around 100 words in length
```

```
## List relevant skills with bullet-points
```

```
# Choose the most relevant size. Choose one of: 'small', 'medium
  ↪ ', or 'large'
```

```
# Choose the most relevant duration. Choose one of: 'under 1
  ↪ month', '1 to 3 months', '3 to 6 months', or 'more than 6
  ↪ months'
```

```
# Choose the most relevant expertise level. Choose one of: 'entry
  ↪ ', 'intermediate', or 'expert'
```

```
Respond with JSON! Keys should be ONLY 'title', 'description', '
  ↪ size', 'duration', 'expertise'.
```

```
Requirements: "" {{requirements}}
```

```
<basicUserPrompt> A person writing a job post was given GPT3.5
  ↪ assistance in response to the job-poster's request.
```

```
The initial prompt by the human was: '{{ prompt }}'.
```

```
In response, the AI generated text: '{{ ai_text }}'.
```

```
How good was the original prompt in conveying the job poster
  ↪ wanted done and/or what skills were needed?
```

```
Example scores:
```

```
"I need someone to help me edit my podcasts" ---> 9
```

```
"Devoted individuals who like to help others" --> 1
```

```
"Help" --> 1
```



B Second experiment to understand selection into receiving the AI generated first draft

In the previous experiment, employers could choose to opt out of receiving the AI-generated job posts. Since these employers were all posting on the platform for the first time, we were not able to investigate which types of employers selected to receive help from AI. To investigate this selection, we examined another experiment run by the platform on a sample of employers who had previously posted at least one job on the platform.

From April 20, 2023, through June 6, 2023, returning employers on the platform who posted a job were randomly allocated into treatment and control groups. The sample included all employers who had ever posted a job on the platform before. For treated employers, any job they posted beginning at the time they were allocated into the experiment was considered treated.

The experimental sample includes 101,601 employers who posted 164,382 openings between them. Appendix Figure 19 shows the daily allocations of employers into the treatment and control groups. Table B reports pre-experiment attributes of these employers and shows the sample of employers was well balanced in terms of their experience on the platform.

Table 31: Pre-randomization employer attributes by treatment status

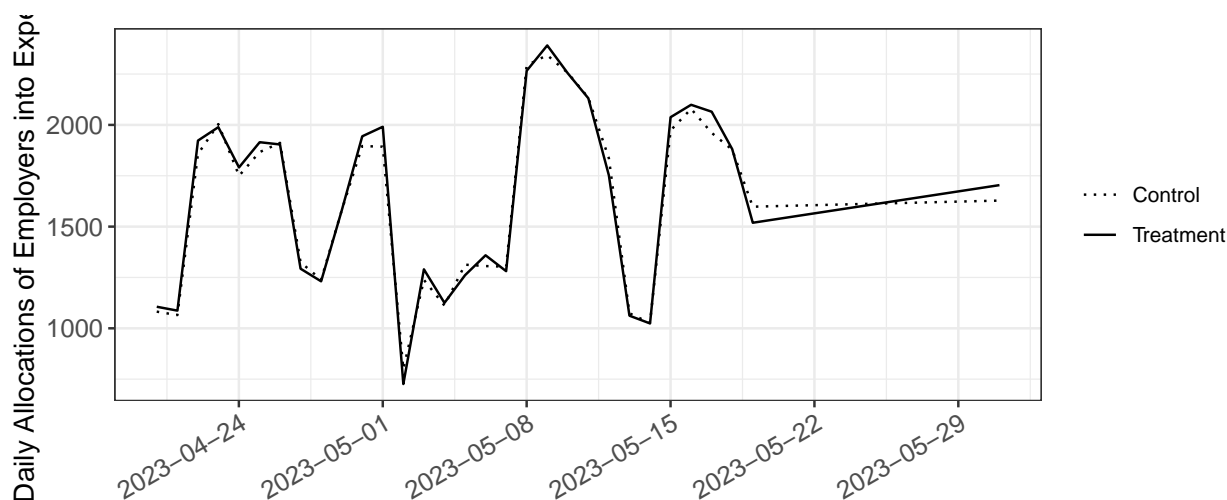
Variable	Control	Treatment	Difference	P_value
From English-speaking country	0.59	0.59	-0.002	0.61
US-based	0.42	0.41	-0.01	0.09
Years since platform registration	3.13	3.13	-0.003	0.89
Num posts, year before allocation	5.49	5.54	0.06	0.25
Num hires, year before allocation	3.40	3.43	0.03	0.36
Hourly wagebill, year before allocation	64,416.28	68,908.11	4,491.83	0.57
FP wagebill, year before allocation	44,184.90	51,323.58	7,138.68	0.67
Total hours demanded, year before allocation	3,925.41	3,241.50	-683.91	0.45
Mean hourly wages, year before allocation	9.81	9.92	0.12	0.39

Notes: This table shows the difference between treatment and control workers for means of pre-experiment covariates, as well as a t-test comparing the difference between those means. Age is defined as the number of years between the employers' registration date and when they were allocated into the experiment. Mean hourly wages is conditional on the employer having made an hourly hire in the year prior to the experiment.

B.1 Description of data used in the analysis

The dataset we use in this analysis consists of all job posts posted by employers in the experimental sample between the moment they were allocated into the experiment and June 6, 2023 when allocation stopped. We construct job post-level data with all posts, applications, and hires they have

Figure 19: Daily allocations of employers into experimental cells, pilot experiment



Notes: This plot shows the daily allocations into the treatment and control cells for the experimental sample of 101,601 employers.

within 14 days of posting. While in general we are interested in many outcomes related to posting and hiring, for these purposes we primarily want to 1) show that the take-up in this experiment was comparable with the main experiment and then 2) use the employer histories to understand if there is non-random selection into treatment.

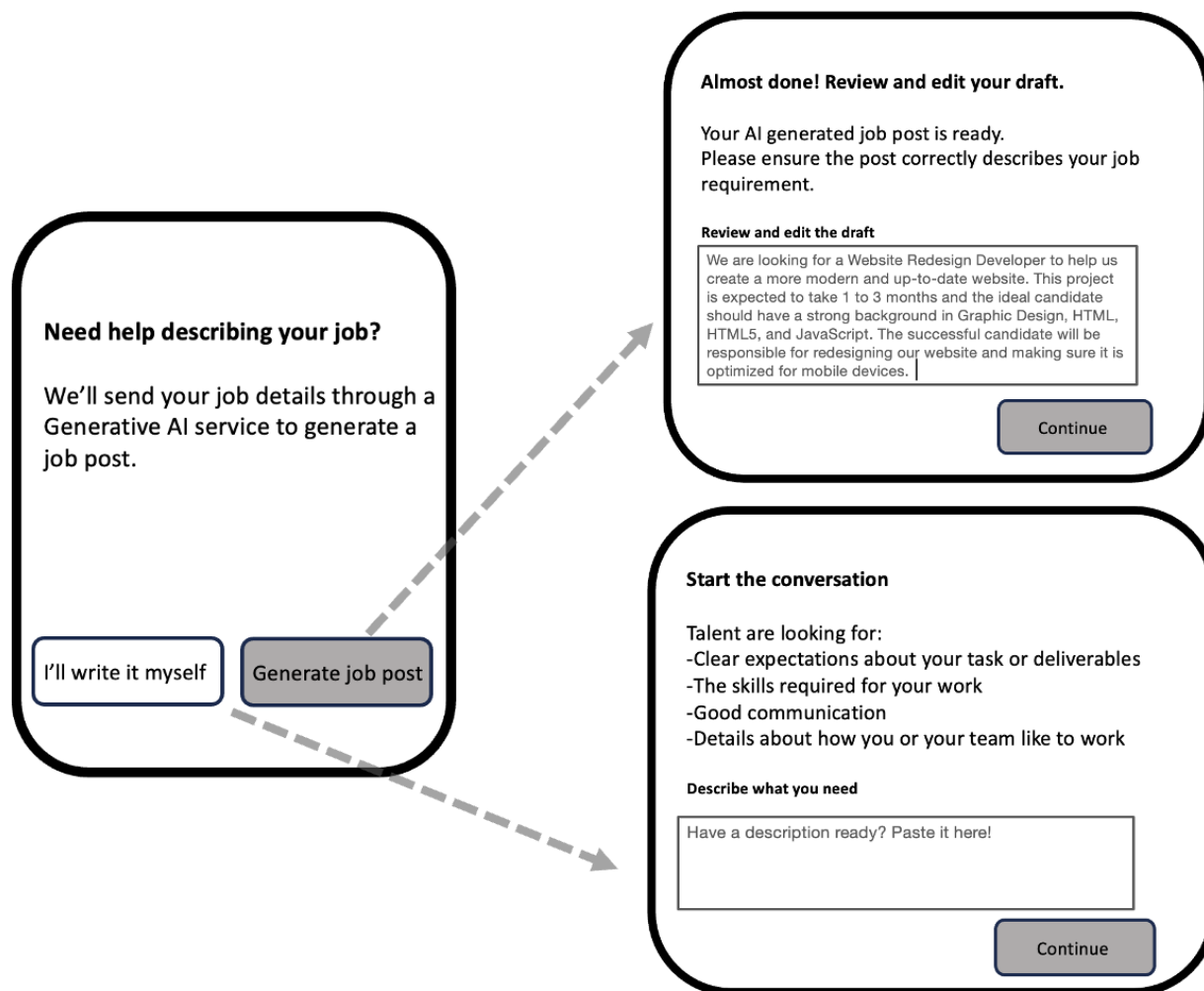
B.2 Experimental intervention at the job description writing stage of job posting

When an employer on the platform wants to post a job, they go through a series of steps. First they provide a job title, the length of time they expect the job to last, and a list of skills required or demanded of the job. After they provide this information, they report some information on their expected budget and then move on to a page where they can input a job description. For employers in the control group, here they type in their job description and then submit the job to be posted.

For employers in the treatment group, after they input the basic information about the job and complete the budget step, they encounter an additional page that asks if they'd like help describing their job. If they select "yes" they have the option to click "Generate job post." The information they have entered so far is incorporated into a prompt, calling a popular generative AI service. The exact prompt is listed below.

```
# Given a job title of '{{title}}'
# Given a job length of '{{duration}}'
# Given job skills of {{skillNames}}
```

Figure 20: The “describe your job post” page in the job post process for the treatment group



Notes: This is a stylized version of the page of the job post process where employers write their job post for employers in the treatment group. For employers in the control group, they only see the bottom page titled “Start the conversation.”

```
# Write a detailed job description, without a title
# Ask the candidate to submit a proposal
# The candidate should describe how they can help with the
  ↪ project
# The candidate should include some links to past completed
  ↪ projects
```

If the employer is not interested in the service, they click a button that says “I’ll write it myself,” and they are sent to the basic page employers in the control group would see.

C First stage

Most employers used the AI-generated job posts at least once. The platform records every action and even click taken by each user to the microsecond. Appendix Table 32 helps us to see the ‘first-stage’ of the treatment. Of all employers in the treatment group, 53% opted-in to having the generative AI write their first job post. Of employers who made it through this stage, 62% opted-in. Of the employers who opted in, 78% edited the proposed job description, meaning 22% of employers posted the job without changing anything themselves.

Table 32: Treatment take up

Opted In	Count	Percent	Edited job after opting in
Yes	27,192	53%	78%
No	16,707	33%	NA
Never got this far	7,081	14%	NA

Notes: This table provides summary statistics on employers in the treatment group. “Opting in” means the employer chose to have GenAI generate at least one job post for them. Some employers drop off the job post process before getting to that step, these are labeled ‘Never got this far.’

D Results

D.1 Treated employers were more likely to post a job

Treated employers were 10% more likely to post a job. In Table 33 Column (1) we see that on this sample of returning employers, 92% who start a job post end up finishing it. This 10% increase is only about half of the size of the treatment effect we saw in the main experiment, which may be because it was run in April 2023, when employers may have been less familiar with generative AI.

D.2 Employers who opt-in to treatment are slightly positively selected

In Table 34, we compare employers who opted in to the treatment with those who opted out on pre-experiment platform experience. We find that the employers who opted in to receive the AI-written draft are slightly positively selected on observables. This suggests that negative treatment effects on likelihood of hiring in the first experiment are unlikely to be due to the selection of “worse” employers taking up the treatment.

Table 33: Effects of generative AI on employer proclivity to post jobs

	<i>Dependent variable:</i>	
	Indicator for if first job is posted	Number of job postings
	(1)	(2)
GenAI Treatment Assigned	0.024*** (0.002)	0.056*** (0.012)
Constant	0.921*** (0.001)	1.570*** (0.008)
Observations	101,601	101,601
R ²	0.002	0.0002

Notes: This table analyzes the effect of the treatment on the number of jobs the employer posts over the experimental period. Likelihood of completing first job post is a binary variable for the job post that the employer was working on when they were allocated into the experiment. The sample is made up of all employers in the experimental sample. Number of job posts excludes any spam postings. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.

Table 34: Selection into opt-ing into the treatment, from the treatment group

	<i>Dependent variable:</i>			
	Hourly earnings	Fixed price earnings	Hours demanded	Hourly wages
	(1)	(2)	(3)	(4)
Opted-In to GenAI	17,871.920* (9,304.224)	18,859.110 (34,388.340)	1,043.654** (437.399)	0.177 (0.204)
Constant	60,942.970*** (7,322.737)	44,858.690* (27,064.780)	2,844.011*** (344.248)	9.947*** (0.160)
Observations	43,899	43,899	43,899	43,899
R ²	0.0001	0.00001	0.0001	0.00002

Notes: This table compares pre-experiment observable characteristics of employers in the treatment group who opt-ed in to the treatment to those who opt-ed out of it. Earnings, hours, and hourly rates are averages calculated from the month prior to when they were allocated into the experiment. Significance indicators: $p \leq 0.10$: *, $p \leq 0.05$: ** and $p \leq .01$: ***.