

# Algorithmic Writing Assistance on Jobseekers’ Resumes Increases Hires

Emma van Inwegen  
MIT

Zanele Munyikwa  
MIT

John J. Horton  
MIT & NBER

July 18, 2023

## Abstract

There is a strong association between writing quality in resumes for new labor market entrants and whether they are ultimately hired. We show this relationship is, at least partially, causal: in a field experiment in an online labor market with nearly half a million jobseekers, treated jobseekers received algorithmic writing assistance on their resumes. Treated jobseekers were hired 8% more often. Contrary to concerns that the assistance takes away a valuable signal, we find no evidence that employers were less satisfied. We present a model where better writing does not signal ability but helps employers *ascertain* ability, rationalizing our findings.

## 1 Introduction

For most employers, the first exposure to a job candidate is typically a written resume. The resume contains information about the applicant—education, skills, past employment, and so on—that the employer uses to draw inferences about the applicant’s suitability for the job. A well-written resume might influence an employer’s perception of a candidate. One perspective is that a better-written resume—without any change in the underlying facts—might make it easier for the employer to draw the correct inferences about a candidate’s abilities, potentially improving the chance of an interview or job offer. We call this the “clarity view” of the role of resume writing quality. From another perspective, a resume might not merely be a conduit for match-relevant information; the resume’s writing itself could signal ability. In particular, writing quality might provide signals about the jobseeker’s communication skills, attention to detail, or overall quality, potentially leading to a greater chance of a positive outcome. We call this the “signaling view” of the role of resume writing quality.

In this paper, we explore the mechanics of how resume writing quality affects the hiring process. First, using observational data from a large online labor market, we document

a strong positive relationship between writing quality in resumes and hiring that persists even after controlling for obvious confounders. Second, we report the results of a field experiment in which we exogenously vary writing quality in the same market. This experiment directly tests whether there is a causal effect of writing quality on job market outcomes and provides a testing ground to distinguish between the clarity and signaling views.

Our main substantive finding is evidence for the “clarity view.” Evidence for this conclusion is possible because we trace the whole matching process from resume creation all the way to a measure of post-employment satisfaction with a sample of 480,948 jobseekers. This sample size is an order of magnitude larger than the next largest experiments.

Treated jobseekers were more likely to get hired (consistent with both signaling and clarity explanations), but we find no evidence that employers were later disappointed in the quality of work by the treated group, which refutes what the “signaling view” explanation would predict.

To create random variation in writing quality, we intercept new jobseekers at the resume-writing stage of registering for the online labor market. We randomly offer some of them—the treatment group—algorithmic writing assistance, while others—the control group—write their resume under the status quo experience of no assistance. We will discuss this assistance in depth, which we refer to as Algorithmic Writing Service, but, at a high level, it improves writing by identifying and providing suggestions to resolve common errors. After resume creation, we observe both treated and control jobseekers as they engage in search and, in the case of completed jobs, receive reviews.

In the experimental data, there are quantifiable improvements to resume-writing quality among the treatment group. For example, we find fewer spelling and grammar errors in the resumes of the treated group of jobseekers, as well as fewer instances of commonly confused words. These positive effects were concentrated among the low-end of the distribution in writing quality, as jobseekers with already excellent resumes benefited little from writing assistance.

One might worry that the treatment could affect behavior. However, we find that, during job search, treated workers did not send out more applications than workers in the control group, nor did they propose higher wage bids. This is a convenient result, as it allows us to focus on the decision-making of the employers. If jobseekers had altered their application behavior—perhaps sending more applications with their stronger resumes—we might wrongly attribute greater job-market success to the resume rather than this endogenous change in effort.

As for the effect of writing assistance on hiring, we find that treated jobseekers had a 8% increase in their probability of being hired within their first month on the platform relative

to the control group. If hired, treated workers' hourly wages were 10% higher than the hourly wages of workers in the control group. We show this result is due to composition changes in which workers are hired—when we control for the workers opening wage bid, the effect to wages disappears.

The data make the impact of resume quality on hiring decisions apparent. In order to differentiate between the “signaling view” and “clarity view,” we look at the effect of the treatment on a few different proxy's for employers' satisfaction with the quality of work. We do not find any significant treatment effects to revealed preference measures like hours worked or whether or not workers were ever rehired.

Unique to our setting, we have explicit measures of employer disappointment, as both sides privately rate each other at the conclusion of the contract. Employer ratings provide a direct way to analyze the “signaling view” of the role of the resume. Specifically, since the treatment removes or at least weakens a credible signal of jobseeker ability, the “signaling view” would suggest that hiring decisions made without this signal should leave employers disappointed. We find no statistically or economically significant treatment effects for any of these ratings. Given the 10% higher average wages in the treatment group, if employers were simply tricked into hiring worse workers generally, these higher wages should have made it even more likely to find a negative effect on ratings (Luca and Reshef, 2021). Moreover, we find that workers are hired for at least as many hours of labor, and are just as likely to be rehired.

One possible explanation for our results is that employers are simply wrong to consider resume writing quality as informative about ability. However, the “clarity view” can also rationalize our results without making this assumption. It is helpful to formalize this notion to contrast it with the more typical signaling framing of costly effort and hiring. To that end, we present a simple model where jobseekers have heterogeneous private information about their productivity but can reveal their type via writing a “good” resume. This is not a signaling model where more productive workers face lower resume-writing costs—any worker, by writing a good resume, will reveal their information. This cost is assumed to be independent of actual productivity. We show that writing assistance shifting the cost distribution can generate our findings of more hires, higher wages, and equally satisfied employers.

We perform this study in the context of a large literature on how experimentally varying applicant attributes affects callback rates (Moss-Racusin et al., 2012; Bertrand and Mullainathan, 2004; Kang et al., 2016; Farber et al., 2016). More specifically, we contribute by showing the importance of text in understanding matching (Marinescu and Wolthoff, 2020). The notion that better writing can help a reader make a better purchase decision is well-

supported in the product reviews literature (Ghose and Ipeirotis, 2010) but is a relatively novel finding in labor markets.

While we are not the first to investigate how writing matters in receiving callbacks from employers<sup>1</sup> (Sterkens et al., 2021; Martin-Lacroux and Lacroux, 2017), we believe we are the first to do so in a field experiment with natural variation in writing quality. In one related example, Sajjadiani et al. (2019) analyze resumes of applicants to public school teaching jobs and find that spelling accuracy is correlated with a higher probability of being hired. Hong, Peng, Burtch and Huang (2021) further show that workers who directly message prospective employers (politely) are more likely to get hired, but the politeness effect is muted when the workers’ messages contain typographic errors. Weiss et al. (2022) conducts a lab experiment and finds that the use of AI in jobseekers’ writing resulted in employer perceptions of lower competence, warmth and social desirability (however, of particular importance is that in their experiment, the use of AI was disclosed to employers).

These results come at a key time for the evolution of hiring decisions—the practical implications of these two views can inform employers who need to adapt their hiring practices to a world in which AI can provide substantial quality improvements to application materials. AI capable of generating text is already leaving its mark on labor markets (Eloundou et al., 2023; Felten et al., 2023), and understanding the role of individuals’ writing abilities in predicting their quality is becoming increasingly crucial. Recent research has demonstrated that Large Language Models like ChatGPT can significantly improve worker productivity, particularly by raising bottom of the skill distribution (Noy and Zhang, 2023; Brynjolfsson et al., 2023). Similar findings have been reported in other studies on technological advancements, such as the benefit that surgical robots provide to the least proficient surgeons (Tafti, 2022). Our own findings are consistent with these results, as we observed the greatest effects of our treatment among individuals with lower writing quality.

These results can only describe a partial equilibrium. Our study cannot speak to crowd-out effects (Crépon et al., 2013; Marinescu, 2017), which are relevant to discuss the welfare implications of any labor market intervention. However, our purpose is understanding how employers make decisions with respect to resumes and their role as a tool for lessening information frictions. Additionally, given that in our setting, new entrants compete with established jobseekers on the platform, we anticipate the crowd-out effect will be small, and perhaps even welcome if at the expense of more established workers, given the obstacles new entrants face (Pallais, 2014).

If the “clarity view” is more important to future hiring decisions, then any intervention

---

<sup>1</sup>While the reason this preference exists is not known, recruiters report, anecdotally, caring about a resume’s writing quality (Oreopoulos, 2011).

that encourages better writing is likely to be beneficial for all parties. There will likely be little loss in efficiency if parties are better informed. Even better, as we show, this kind of assistance can be delivered algorithmically. These interventions are of particular interest because they have zero marginal cost (Belot et al., 2018; Briscese et al., 2022; Horton, 2017), making a positive return on investment more likely, a consideration often ignored in the literature (Card et al., 2010). On the other hand, if the “signaling view” is more important, then providing such writing assistance will mask important information and lead to poor hiring decisions. As for the treatment itself, unlike general advice, algorithmic interventions are adaptive. In our study, the algorithm took what the jobseeker was trying to write as input and gave targeted, specific advice that likely improved it.<sup>2</sup> This is likely more immediately useful than more vague recommendations, such as telling jobseekers to “omit needless words.”

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 provides evidence on the relationship between writing quality and labor market outcomes from observational data from the market before any intervention. Section 4 reports the experimental results of the treatment effects on writing quality and subsequent labor market outcomes. In Section 5 we present a simple model that can rationalize our findings. Section 6 concludes.

## 2 Empirical context and experimental design

The setting for this experiment is a large online labor market. Although these markets are online, with a global user base, and with lower search costs (Goldfarb and Tucker, 2019), they are broadly similar to more conventional markets (Agrawal et al., 2015). Employers post job descriptions, jobseekers search among job posts and apply. Employers then decide if and who to interview or hire. One distinctive feature of online labor markets is that both the employer and the worker provide ratings for each other at the end of a contract.

Because of the many similarities between on and offline labor markets, a substantial body of research uses online labor markets as a setting, often for randomized experiments. Many researchers have used platforms to study the role of information in hiring, as they are difficult to study elsewhere (Stanton and Thomas, 2016; Agrawal et al., 2016; Chan and Wang, 2018; Kokkodis and Ransbotham, 2022). Online labor markets also allow researchers to broaden the range of hypotheses to test (Horton, 2010) because platforms store detailed data on things like applications, text, length of time spent working on an application, speed of hire, and more.

---

<sup>2</sup>The Algorithmic Writing Service does not provide whole paragraphs of text, nor is it able to be prompted.

The online labor market which serves as the setting for this experiment is a global marketplace, and not representative of, say, the US workforce. About 20% of the sample comes from Anglophone countries. However, less than 6% of the world’s population comes from Anglophone countries. The sample also overweights places like India, which make up 18% of the global population but 24% of the workers in our sample. As a global marketplace, this market has features that distinguish it from local labor markets. All of the work is Internet-mediated, removing frictions based on geography. Still, there exist frictions based on language and communication skills, which is one of the reasons it makes a good setting to study the role of the resume in hiring. We provide summary statistics about jobs worked on the platform in Appendix Table A1. The average job lasts two months, takes 201 hours of labor with average wages of \$17 per hour. Most of the work measured by the wagebill on the platform consists of hourly jobs, but fixed price jobs make up two-thirds of the total number of contracts formed.

## 2.1 Search and matching on the platform

A would-be employer writes job descriptions, labels the job opening with a category (e.g., “Graphic Design”), lists required skills, and then posts the job opening to the platform website. Jobseekers generally learn about job openings via electronic searches. They submit applications to jobs they are interested in and are required to include a wage bid and a cover letter.

In addition to jobseeker-initiated applications, employers can also use the interface to search worker profiles and invite workers to apply to particular jobs. The platform uses jobseekers’ on-platform history and ratings to both recommend jobseekers directly to would-be employers and to rank them in order of relevance and quality. At no point do these algorithmic recommendations consider the writing quality of the jobseeker’s resume. By using recommendation systems, algorithms can help reduce randomness in the hiring process and provide employers with quality signals about potential hires (Horton, 2017). In terms of selection, Pallais (2014) shows that employers in an online labor market care about workers’ reputation and platform experience when hiring. After jobseekers submit applications, employers screen the applicants. The employers can highlight applications of interest through the platform interface to save in a separate tab, their “shortlist.” Then the employer decides whether to conduct interviews, and whether to make an offer(s). If a match is formed, the platform observes the wages, hours worked, earnings, and ratings at the conclusion of the contract. Although these ratings have been shown to become inflated over time and can be distorted when they are public and reciprocal (Bolton, Greiner and Ockenfels, 2013), they

are still a useful signal of worker performance (Fradkin et al., 2021; Cai et al., 2014). We consider the impact of the treatment to the public and private numerical ratings the employers give to the workers, as well as the “sentiment” of the written text of reviews, which are less prone to inflation (Filippas et al., 2022).

## 2.2 Experimental intervention at the resume-writing stage of profile creation

When new jobseekers sign up to work on the platform, their first step is to register and create their profile. This profile serves as the resume with which they apply for jobs. This profile includes a list of skills, education, and work experience outside of the platform. It also includes a classification of their primary job category (e.g., “Graphic Design”), mirroring what employers select when posting a job. The interface consists of a text box for a profile title and a longer text box for a profile description. Their finished profile will include their profile description and a “profile hourly wage,” which is the wage they offer to employers searching for workers.

During the experimental period, jobseekers registering for the platform were randomly assigned to an experimental cell. The experimental sample comprises jobseekers who joined the platform between June 8th and July 14th, 2021. For treated jobseekers, the text boxes for the profile description are checked by the algorithmic writing assistance software, which we will call the Algorithmic Writing Service. Control jobseekers received the status quo experience. The experiment included 480,948 jobseekers, with 50% allocated to the treated cell. Table 1 shows that it was well-balanced and the balance of pre-treatment covariates was consistent with a random process.<sup>3</sup>

## 2.3 The algorithmic writing assistance

Words and phrases that a language model determines to be errors are underlined by the Algorithmic Writing Service. See Figure 1 for an example of the interface. By hovering a mouse cursor over the underlined word or phrase, the user sees suggestions for fixing spelling and grammar errors. The Algorithmic Writing Service also advises on punctuation, word choice, phrase over-use, and other attributes related to clarity, engagement, tone, and style.

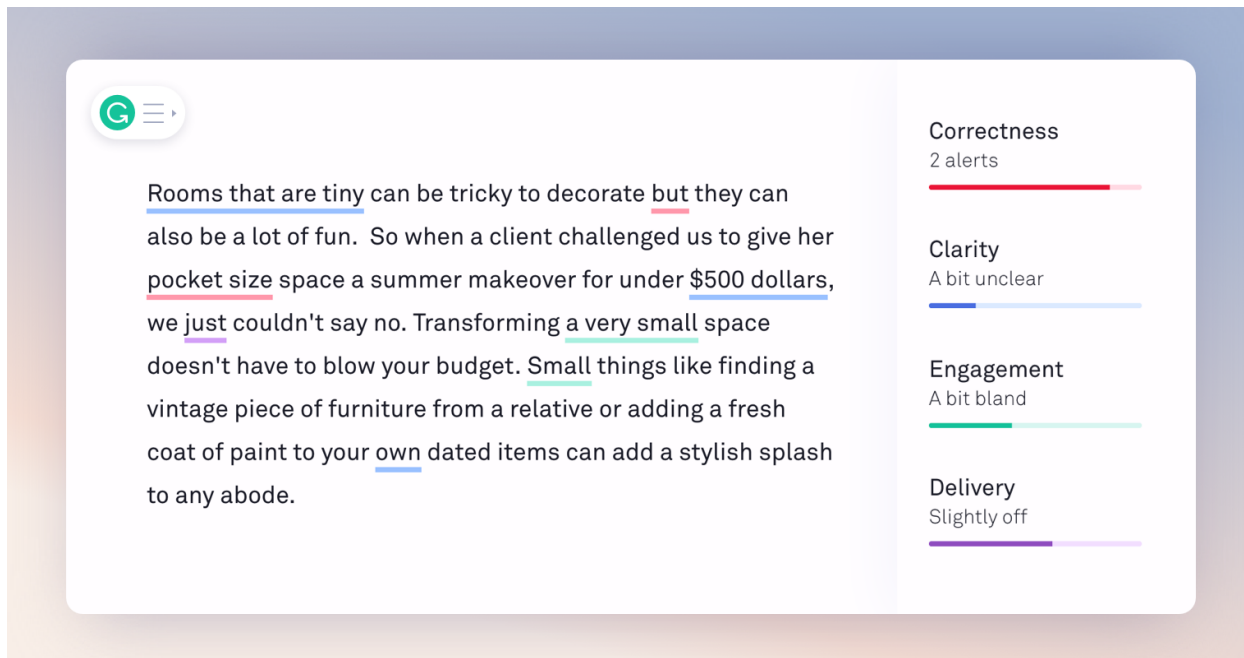
According to the Algorithmic Writing Services website, the software uses a combination of transformer models and rule-based systems to provide its recommendations. Unlike Large Language Models like ChatGPT or BingChat, this system is not generative—it cannot

---

<sup>3</sup>In Appendix Figure A1 we show the allocations by treatment status over time and it also looks balanced.



Figure 1: Example of the Algorithmic Writing Service's interface showing suggestions on how to improve writing



Notes: Example of the Algorithmic Writing Service applied to a paragraph of text. To receive the suggestions, users hover their mouse over the underlined word or phrase. For example, if you hover over the first clause "Rooms that are tiny" underlined in blue, "Tiny rooms" will pop up as a suggestion.

be prompted or asked questions, it simply takes the text the user has provided and suggests improvements to it.

## 2.4 Platform profile approval

Of the experimental sample, 46% of workers allocated into the experiment upon registration complete and submit their profiles. When jobseekers finish setting up their profiles, they have to wait to be approved by the platform. The platform approves jobseekers who have filled out all the necessary information, uploaded ID, and provide bank details so they can be paid. The platform can reject jobseekers at their discretion. However, platform rejection is somewhat rare. About 10% of profiles are rejected, usually as a part of fraud detection or because the jobseekers leave a completely empty profile. About 41% of workers who begin registering get all the way through the approval process.

As approval is downstream of profile creation, this step creates a potential problem for interpreting any intervention that changes profile creation. For example, it could be that the treatment leads to a greater probability of platform approval. Or, the treatment could have made jobseekers more likely to complete the registration process and submit their profile,



both of which could effect hiring. While unlikely given the mechanistic rules the platform applies, this is possible, and we investigate this potential issue in multiple ways.

First, we check whether there is any evidence of selection and find no evidence that treated jobseekers are no more likely either to submit their profiles and or to receive approval.<sup>4</sup>

Second, in our main analysis, we condition on profile approval in our regressions. We also perform robustness checks where we report the same analysis not conditioned on profile approval and where we control for profile approval as a covariate. All findings are robust to these strategies, a result described in Section 4.5.

## 2.5 Description of data used in the analysis

The dataset we use in the analysis consists of the text of jobseekers’ resumes as well as all of their behavior on the platform between the time they registered—between June 8th and July 14th 2021—and August 14th, 2021. We construct jobseeker level data, including the title and text of their profile, the number of applications they send in their first month on the platform, the number of invitations they receive to apply for jobs, the number of interviews they give, and the number of contracts they form with employers. The most common categories listed as worker’s primary job categories are, in order of frequency, Design & Creative, Writing, Administrative Support, and Software Development.

In Table 1 we present summary statistics about the jobseekers in the full experimental sample, as well as the sample conditioned on platform approval. Jobseekers with writing as their primary area of work make up 17% of the sample. Only 14% of jobseekers are based in the US, and over 80% are based in a country where English is not the native language.

## 2.6 Constructing measures of writing quality

We do not observe the changes that the Algorithmic Writing Service suggested—we simply observe the resumes that result. As such, we need to construct our own measures of writing quality to determine if the treatment was delivered.

Algorithmic Writing Service provides text improvement suggestions along several dimensions. Perhaps the most straightforward measure of writing quality is spelling. To see if the treatment impacted spelling errors, we take each worker’s profile and check if each word appears in an English language dictionary. We use the dictionary `hunspell`, which is based on MySpell dictionaries and is the basis for the spell checker for Google Chrome, Firefox, and Thunderbird.

---

<sup>4</sup>See Appendix Table A3 for regression output.

Table 1: Comparison of jobseeker covariates, by treatment assignment

	Treatment mean: $\bar{X}_{TRT}$	Control mean: $\bar{X}_{CTL}$	Difference in means: $\bar{X}_{TRT} - \bar{X}_{CTL}$	p-value
<i>Full sample description: N = 480,948</i>				
Resume submitted	0.456 (0.001)	0.455 (0.001)	0.001 (0.001)	0.452
Platform approved	0.407 (0.001)	0.406 (0.001)	0.002 (0.001)	0.186
Resume length	32.911 (0.116)	32.860 (0.117)	0.051 (0.165)	0.755
Profile hourly rate	18.843 (0.126)	18.917 (0.126)	-0.074 (0.178)	0.676
<i>Flow from initial allocation into analysis sample</i>				
	<i>Treatment (N)</i>	<i>Control (N)</i>	<i>Total (N)</i>	
Total jobseekers allocated	240,231	240,717	480,948	
↪ who submitted their profiles	109,638	109,604	219,242	
↪ and were approved by the platform	97,859	97,610	195,469	
↪ with non-empty resumes	97,479	97,221	194,700	
<i>Pre-allocation attributes of the analysis sample: N = 194,700</i>				
From English-speaking country	0.182 (0.001)	0.183 (0.001)	-0.002 (0.002)	0.362
US-based	0.141 (0.001)	0.143 (0.001)	-0.002 (0.002)	0.222
Specializing in writing	0.173 (0.001)	0.176 (0.001)	-0.003 (0.002)	0.105
Specializing in software	0.115 (0.001)	0.115 (0.001)	0.000 (0.001)	0.770
Resume length	70.394 (0.222)	70.260 (0.222)	0.135 (0.314)	0.668

*Notes:* This table reports means and standard errors of various pre-treatment covariates for the treatment group and the control group. The first panel shows the post-allocation outcomes of the full experimental sample i) profile submission, ii) platform approval, iii) length of resume in the number of words, iv) profile hourly wage rate in USD. The means of profile hourly rate in treatment and control groups are only for those profiles which report one. The reported p-values are for two-sided t-tests of the null hypothesis of no difference in means across groups. The second panel describes the flow of the sample from the allocation to the sample we use for our experimental analysis. The complete allocated sample is described in the first line, with each following line defined cumulatively. The third panel looks at pre-allocation characteristics of the jobseekers in the sample we use for our analysis, allocated jobseekers with non-empty resumes approved by the platform. We report the fraction of jobseekers i) from the US, UK, Canada, or Australia, ii) from the US only, iii) specializing in writing jobs, iv) specializing in software jobs, and v) the mean length of their resumes in the number of words.

As many of the resumes are for technical jobs, they often contain industry-specific terms such as “UX” or brand names like “Photoshop.” To prevent these from being labeled as errors, we augmented the list of words in the dictionary by checking the 1,000 most commonly “misspelled” words in our sample and adding non-errors manually.

Spelling is not the only measure of writing quality and we understand that employers may care about measures of quality beyond this. To broaden our measures, we use LanguageTool,<sup>5</sup> an open-source software that finds many errors that a simple spell checker cannot detect. LanguageTool is a rule-based dependency parser that identifies errors (rule violations) and categorizes them. Some example categories include “Nonstandard Phrases,” “Commonly Confused Words,” “Capitalization,” and “Typography.” For example, the non-standard phrase “I never have been” would be flagged with a suggestion to replace it with “I have never been.”<sup>6</sup>

### 3 Observational results

Before presenting results of the field experiment, we explore the relationship between resume writing quality and hiring using observational data from this market. We begin by showing a negative relationship between spelling errors and hiring. Then we show the negative relationship persists using more nuanced measures of writing quality and hiring.

#### 3.1 Spelling errors are associated with lower hiring probabilities in the observational data

We begin by studying the most unambiguous measure of writing quality: spelling. In Figure 2, we plot jobseekers’ hiring outcomes versus the percentage of words spelled correctly for a sample of jobseekers. The sample is the resumes of all jobseekers who registered for the platform over the month of May 7th through June 7th, 2021, prior to the experiment. Because the distribution of percent correctly spelled is so left skewed, we truncate the sample to only those who spell at least 75% of the words in their resumes correctly. This window includes 97% of jobseekers. The x-axis is deciles between 75% and 100% of words spelled correctly.

Generally jobseekers with resumes with fewer spelling errors are more likely to be hired. In the left facet, the y-axis is the number of contracts a jobseeker forms in their first month on the platform; the right facet, the y-axis is the probability that a jobseeker is ever hired in their first month on the platform. Jobseekers with over 99% of the words in their resume spelled correctly contract nearly three times more in their first month on the platform than those with less than 90% spelled correctly.

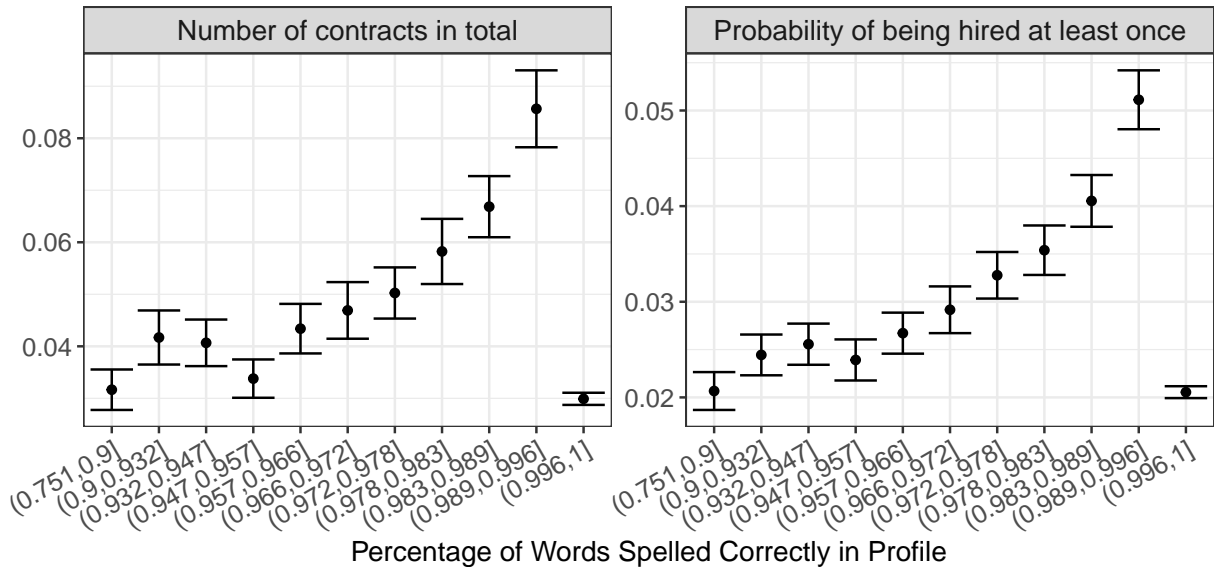
One complication which is visible in both facets is that resumes with 100% of words

---

<sup>5</sup>This is a different software than the Algorithmic Writing Service.

<sup>6</sup>For a more detailed explanation of all of the rule categories, see Appendix Table A4.

Figure 2: Association between spelling errors and hiring outcomes in the observational data



Notes: These data show the relationship between the percentage of correctly spelled words on a jobseekers' resume with various hiring outcomes. A 95% confidence interval is plotted around each estimate. The sample is of all new jobseekers who registered and were approved for the platform between June 1st and June 7th, 2021, and had resumes with more than 10 words. Plots are truncated at those who spelled at least 75% of the words in their resume correctly.

spelled correctly are much *less* likely to receive interest from employers. This is likely because those resumes tend to be much shorter than the others—the average length of a resume that has zero spelling errors is only 47 words long compared to the average which is 64.

### 3.2 The association between various kinds of writing errors and hiring probabilities

Moving beyond spelling, Table 2 shows the correlation between hiring outcomes and each type of language error in the observational data.<sup>7</sup> In the first specification, we show the correlation between the error rate for the various types of language errors and the number of contracts formed over the jobseeker's first month. In the second specification, the outcome is simply whether or not the jobseeker was ever hired in their first month. In Columns (3) and (4), we control for the jobseekers' profile hourly rate and primary category of work.

Resumes with more per word grammar errors, typos, typography errors, and miscellaneous errors are all hired less. This linear model places some unreasonable assumptions like constant marginal effects on the relationship between various writing errors and hir-

<sup>7</sup>In Appendix Table A2 we summarize the frequency of these error types in the observational data.

ing. There may be interactions between these error types. However, it is still useful to summarize the relationships. We can see generally negative relationships between writing error rate and hiring. In Columns (3) and (4) where we add controls, we see coefficients get smaller in magnitude as we would expect, but the significance does not disappear.

In terms of magnitude, one additional error of any type is associated with that jobseeker being hired 1.4% less. In Appendix Table A5 we collapse all error types into one measure of Total Errors and report these results in both levels and normalized by resume length. For robustness we repeat these analysis in levels in Appendix Table A6. The negative relationship between writing errors and hiring persists in all specifications.

## 4 Experimental results

We look at three main kinds of experimental results. First, we examine how the treatment affected the text of resumes. Next, we look at employment outcomes for those treated workers. Third, we will look at how this impacted the quality of work, as assessed by employer reviews and whether or not a worker is rehired.

### 4.1 Algorithmic writing assistance improved writing quality

The first step of our analysis is to measure the effect that the Algorithmic Writing Service has on the treatment group’s writing. Starting with the fraction of words in the resume spelled incorrectly, even the worst spellers spell most of the words correctly. An average resume in the control group has 96.4% of the words spelled correctly. In the treatment group, they have 96.5% spelled correctly.

To understand the effects of the treatment on other types of writing errors we turn to the more fine-grained LanguageTool definitions of writing errors. Figure 3 displays the effect of treatment on the number of each type of writing error, normalized by resume length.<sup>8</sup> Our outcomes of interest are the error rate for each type, so we normalize each type of error to the number of words in the resume. For treatment effects measured in percentage terms, we calculate the standard errors using the delta method.

In the first facet of Figure 3, we find that jobseekers in the treatment group made 5% fewer errors in their resumes. Breaking these down by error type, we find that jobseekers in the treatment group had a significantly lower rate of errors of the following types: capitalization, collocations, commonly confused words, grammar, spelling, possible typos, miscellaneous, and typography. We find larger treatment effects for errors associated with writing

---

<sup>8</sup>The treatment had no effect on the length of resumes—see Appendix Table A7.

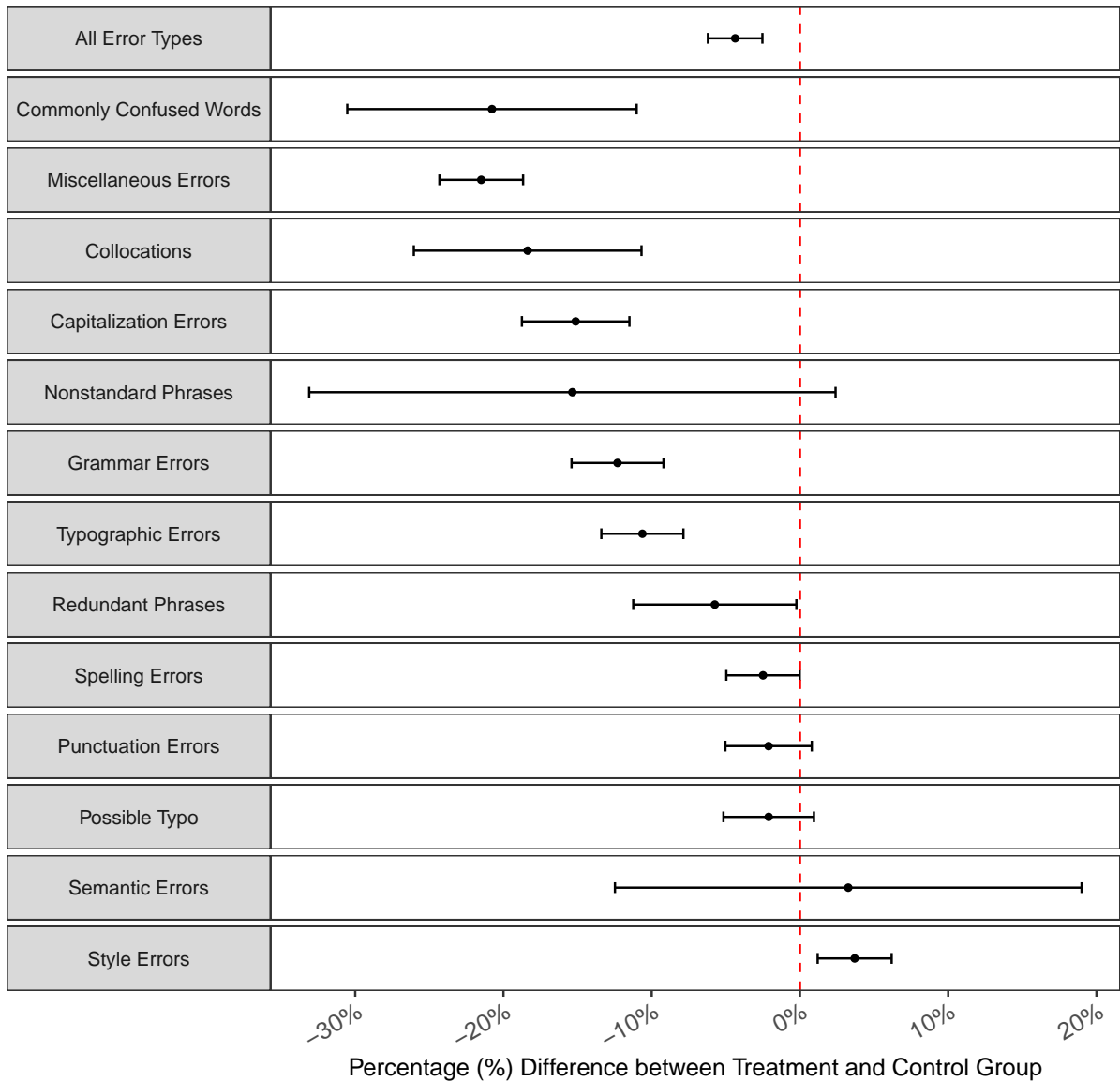
Table 2: Hiring outcomes predicted based on language errors (normalized by word count) in observational data

	<i>Dependent variable:</i>			
	Number of Contracts	Hired	Number of Contracts	Hired
	(1)	(2)	(3)	(4)
Capitalization Error	-0.075 (0.048)	-0.038 (0.025)	-0.055 (0.045)	-0.026 (0.023)
Possible Typo	-0.030** (0.013)	-0.022*** (0.007)	-0.016 (0.012)	-0.013** (0.006)
Grammar Error	-0.534*** (0.097)	-0.314*** (0.051)	-0.360*** (0.092)	-0.210*** (0.047)
Punctuation Error	-0.0001 (0.006)	0.0002 (0.003)	-0.0002 (0.006)	0.0001 (0.003)
Typography Error	-0.098*** (0.026)	-0.069*** (0.014)	-0.066*** (0.025)	-0.050*** (0.013)
Style Error	0.261** (0.119)	0.130** (0.062)	0.234** (0.112)	0.115** (0.058)
Miscellaneous Error	-0.414*** (0.151)	-0.220*** (0.079)	-0.252* (0.143)	-0.121 (0.074)
Redundant Phrases	-0.433 (0.437)	-0.264 (0.229)	-0.240 (0.414)	-0.149 (0.213)
Nonstandard Phrases	0.804 (1.681)	-0.124 (0.882)	0.699 (1.591)	-0.193 (0.819)
Commonly Confused Words	-0.761 (0.618)	-0.331 (0.324)	-0.531 (0.584)	-0.190 (0.301)
Collocations	-0.637 (0.434)	-0.380* (0.228)	-0.438 (0.411)	-0.262 (0.211)
Semantic Error	-0.340 (1.112)	-0.532 (0.583)	-0.191 (1.052)	-0.445 (0.541)
Constant	0.053*** (0.002)	0.036*** (0.001)	0.036*** (0.002)	0.026*** (0.001)
Controls			X	X
Observations	65,114	65,114	65,114	65,114
R <sup>2</sup>	0.001	0.002	0.106	0.140

*Notes:* This table analyzes correlation between various writing errors on jobseekers' resumes and their hiring outcomes. The independent variables, writing errors, are divided by the number of words in the jobseekers' resume. Number of Contracts is defined as the number of unique jobs they work over the month after they register for the platform. Hired is defined as 1 if the jobseeker was ever hired over that month, and 0 if else. Columns (3) and (4) include controls for profile hourly rate and job category. Writing errors are defined by LanguageToolR. The sample is made up of all jobseekers who registered for the platform in the week before the experiment who submitted non-empty resumes.

Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Figure 3: Effect of the algorithmic writing assistance on writing quality measures



*Notes:* This plot shows the effect of the treatment on various writing errors in jobseekers' resumes. Point estimates are the percentage change in the dependent variable versus the control group for the treatment groups. A 95% confidence interval based on standard errors calculated using the delta method is plotted around each estimate. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021, and had non-empty resumes, with  $N = 194,700$ . Regression details can be found in Appendix Tables [A8](#) and [A9](#).



clarity than for many others. For example, two of the largest magnitudes of differences in error rate were commonly confused words and collocations, where two English words are put together that are not normally found together. Interestingly, the treatment group had more “style” errors, paralleling our results from the observational data (see Table 2).

#### **4.1.1 Heterogeneous treatment effects to writing quality**

A natural question is which jobseekers benefited most from the treatment. Appendix Table A13 interacts pre-randomization jobseeker attributes with the treatment. We can see that jobseekers from the US, from English-speaking countries,<sup>9</sup> and who are writers all have fewer errors in “levels.”

We find that jobseekers from non-native English-speaking countries experience significantly larger treatment effects to their error rate. Still, effects to their Anglophone counterparts are negative and significant.

## **4.2 Effects to workers employment outcomes**

Access to the treatment impacted whether or not jobseekers were hired. Figure 4 summarizes the treatment effects on the primary hiring outcomes.

#### **4.2.1 Treated workers did not change their job search strategy or behavior**

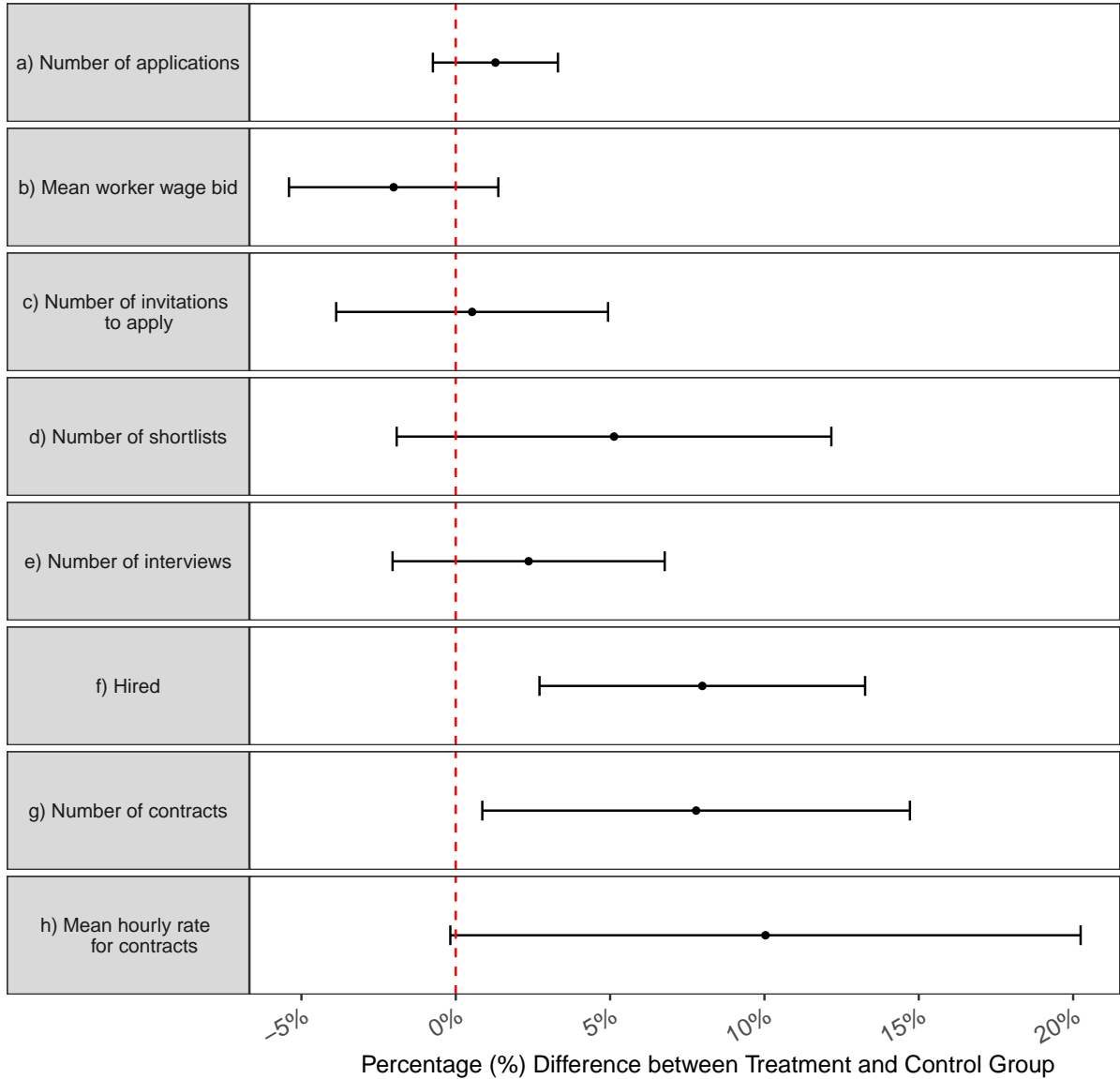
The potential for the treatment to impact jobseeker search behavior or intensity could complicate our desire to focus on employer decision-making. Job applications have been shown to be costly (Abebe, Caria and Ortiz-Ospina, 2021). It is possible that treated jobseekers realized they were in an experiment and increased their search efforts, knowing they had higher quality resumes. In that case, we could not interpret our treatment effect as being driven by employers’ improved perceptions of treated jobseekers. We therefore plot the percentage change in job search metrics for jobseekers in the treatment versus those in the control group in Figure 4a) and Figure 4b) and find no evidence that jobseekers changed their search behavior.

In Figure 4a), the outcome is the number of applications a jobseeker sends out over their first 28 days after registering. We find no effect of the treatment on the number of applications sent. In Figure 4b), the outcome is the mean wage bid proposed by the jobseekers on their applications in their first 28 days on the platform. Average wage bids in both the treat-

---

<sup>9</sup>We define whether a jobseeker is from a native English-speaking country, by whether they login to the platform from USA, UK, Canada, or Australia.

Figure 4: Effect of algorithmic writing assistance on hiring outcomes



*Notes:* This analysis looks at the effect of treatment on hiring outcomes on jobseekers in the experimental sample. The x-axis is the difference in the mean outcome between jobseekers in the treated group and the control group. A 95% confidence interval based on standard errors calculated using the delta method is plotted around each estimate. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021, and had non-empty resumes, with  $N = 194,700$ . Regression details on the number of applications and wage bid can be found in Table 3. Regression details on invitations, interviews, hires, and the number of contracts can be found in Table 4. Regression details on hourly wages can be found in Table 5.

ment and control groups were \$24 per hour. This lack of impact makes sense as jobseekers were not made aware of the treatment.

Table 3 provides regression results for the effects of the treatment on jobseekers’ search behavior. Column (1) demonstrates that treated jobseekers did not apply for more jobs than those in the control group over the experimental period. In Column (2), we find that treated jobseekers do not apply to more hourly jobs than those in the control group. They also could have bid for higher wages knowing they had better-looking resumes. In Column (3), where we narrow the sample to only applications to hourly jobs, we see no evidence of a treatment effect on hourly wage bids.

Table 3: Effects of writing assistance on jobseekers’ application behavior

	<i>Dependent variable:</i>		
	Num Applications (1)	Num Hourly Applications (2)	Mean Hourly Wage Bid (3)
Algo Writing Treatment	0.023 (0.018)	0.012 (0.011)	-0.492 (0.427)
Constant	1.768*** (0.013)	0.919*** (0.008)	24.425*** (0.302)
Observations	194,700	194,700	59,854
R <sup>2</sup>	0.00001	0.00001	0.00002

*Notes:* This table analyzes the effect of the treatment on jobseekers’ application behavior. The experimental sample is made up of all new jobseekers who registered and were approved by the platform between June 8th and July 14th, 2021 and had non-empty resumes. The outcome in Column (1) is the number of total applications a jobseeker sent out between the time the experiment began and one month after it ended. The outcome in Column (2) is the number of specifically hourly applications sent out in that same time period. The outcome in Column (3) is the mean hourly wage bid they proposed for those hourly jobs, and the sample narrows to only jobseeker who submitted at least one application to an hourly job.

Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

#### 4.2.2 The treatment did not affect employer recruiting

Employers are able to seek out workers using the platform’s search feature to invite jobseekers to apply to their job openings. In Figure 4c), the outcome is the number of invitations to apply for a job that the jobseeker receives in their first month. We find the effect of the treatment on employer invitations is a precise zero.

Although it may seem surprising given the results on hires and contracts, it makes sense given that our experimental sample consists of only new jobseekers to the platform. New entrants almost never appear in the search results when employers search for jobseekers, given that their rank is determined by their platform history. Therefore, we would not

expect the treatment to affect invitations to apply. Table 4 Column (1) provides the details of this regression.

After jobseekers apply, employers can sort through the applications to their job and highlight applications they are especially interested in through a feature called shortlisting. In Figure 4d) we observe that jobseekers in the treatment group had applications shortlisted 5% more than jobseekers in the control group, although this effect is not significant. Table 4 Column (2) provides the details of this regression.

The treatment had no significant impact to number of interviews. In Figure 4e), we show no effect of the treatment on number of interviews. Interviews, while technically feasible, are rare on this platform, and do not correspond the types of interviews given in offline labor markets. Here, an interview is defined as any correspondence via message between the employer and applicant, prior to an offer being made. In the control group the average jobseeker gives 0.18 interviews over the course of their first month after registering, with the treatment group receiving 2.5% more interviews. Table 4 Column (3) provides the details of this regression.

### **4.2.3 Treated jobseekers were more likely to be hired**

The treatment raised jobseekers' hiring probability and the number of contracts they formed on the platform. In Figure 4f), the outcome is a binary indicator for whether or not a jobseeker is ever hired in their first 28 days on the platform. During the experiment, 3% of jobseekers in the control group worked at least one job on the platform. Treated jobseekers see an 8% increase in their likelihood of being hired in their first month on the platform.

Jobseekers in the treated group formed 7.8% more contracts overall. In Figure 4g), the outcome is the number of contracts a jobseeker worked on over their first month. In Table 4 Column's (4) and (5) we report these results in levels.

### **4.2.4 Hourly wages in formed matches were higher**

Treated workers had 10% higher hourly wages than workers in the control group. In Figure 4h), the outcome is the mean hourly rate workers earned in jobs they worked over their first month on the platform.<sup>10</sup>

In Table 5 we show that in the control group, workers on average made \$17.25 per hour. In the treatment group, workers made \$18.48 per hour, a significant difference at the 0.042 level. Since workers did not bid any higher, this result suggests that employers are hiring

---

<sup>10</sup>Hourly wage rates for new entrants are not representative of rates on the platform. If a new entrant gets hired for their first job, they tend to experience rapid wage growth.

Table 4: Effect of algorithmic writing assistance on hiring outcomes

	<i>Dependent variable:</i>				
	Num Invitations	Num Shortlists	Num Interviews	Hired x 100	Num Contracts
	(1)	(2)	(3)	(4)	(5)
Algo Writing Treatment	0.001 (0.003)	0.002 (0.001)	0.004 (0.004)	0.247*** (0.080)	0.004** (0.002)
Constant	0.142*** (0.002)	0.039*** (0.001)	0.178*** (0.003)	3.093*** (0.057)	0.047*** (0.001)
Observations	194,700	194,700	194,700	194,700	194,700
R <sup>2</sup>	0.00000	0.00001	0.00001	0.00005	0.00003

*Notes:* This analysis looks at the effect of treatment on hiring outcomes on jobseekers in the experimental sample. The Column (1) outcome Invitations is the number of times they were recruited to a job over their first month. Column (2) is the number of times their application was shortlisted over that month. Column (3) is the number of interviews they gave over that month. Column (4) defines Hired x 100 as one hundred times the probability the jobseeker was hired over that month. Column (5) defines Number of Contracts as the number of unique jobs they work over the month after they register for the platform. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021 and had non-empty resumes. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

more productive workers, or that they thought the treated workers were more productive. If it is the latter, the “signaling view” would predict that employers would then be disappointed with the workers they hired, which we should be able to observe in worker ratings.

Higher wages could be a result of bargaining or due to a composition effect. We find that the initial wage bids are almost always the same as the hourly wage and there is very little evidence of bargaining. In this sample of hires, in only 0.2% of contracts the freelancer proposes more than one bid before being hired. Initial wages and bids are 92% correlated for hourly jobs and 95% correlated for fixed price jobs.

Moreover, as we show in Table 3 there is no effect of the treatment to asking wage bids. We also show that the effect to wages disappears when we control for the worker’s wage bid and we have no evidence the treatment changed wage bids, pointing to a composition effect (see Column (2) of Table 5.) Therefore, we believe this increase in hourly wages to be a composition effect, as employers are able to hire more productive workers.

#### 4.2.5 Hours worked were unaffected by the treatment

After examining the effects of the treatment on hiring outcomes, we now turn our attention to employer satisfaction with treated workers’ labor. One proxy for employer satisfaction is the total number of hours worked by treated workers, as this can be an indication of how much demand there is for their services. In Table 6 Column (1) we show that treated workers worked no fewer hours than workers in the control group. This sample for this

Table 5: Effect of algorithmic writing assistance on average contract wages

	<i>Dependent variable:</i>	
	Hourly wage rate	
	(1)	(2)
Algo Writing Treatment	1.763* (0.909)	0.075 (0.374)
Wage bid		0.839*** (0.091)
Constant	17.247*** (0.573)	1.025 (1.789)
Clustered SEs	X	X
Observations	3,305	1,949
R <sup>2</sup>	0.001	0.801

*Notes:* This analysis looks at the effect of treatment on hourly wages of contracts for jobseekers in the experimental sample. The sample is at the job level, and we cluster standard errors at the worker level. The outcome hourly wage rate is defined as the max hourly wage rate a worker receives for that job. In Column (2) we control for the max wage bid that the worker proposes for that job. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021 and had non-empty resumes, for all jobs they worked within 28 days of registering for the platform. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table 6: Effects of algorithmic writing assistance on hours worked and rehires

	<i>Dependent variable:</i>			
	Hours worked	Log hourly earnings	Log total earnings	Ever rehired
	(1)	(2)	(3)	(4)
Algo Writing Treatment (Trt)	0.412 (0.303)	0.010*** (0.003)	0.010*** (0.004)	-0.003 (0.007)
Constant	2.649*** (0.214)	0.061*** (0.002)	0.134*** (0.003)	0.079*** (0.005)
Observations	194,700	194,700	194,700	6,263
R <sup>2</sup>	0.00001	0.0001	0.00003	0.00003

*Notes:* This table analyzes the effect of the treatment on measures of contract size and value. In Column (1) the outcome is the number of total hours worked by a worker in their first 28 days on the platform. In Column (2) the outcome is the log hourly earnings a worker is paid over this time period. In Column (3) the outcome is the log total earnings a worker is paid over this time period. In Column (4) the outcome is the fraction of workers who are ever rehired for different jobs by the same employer. The experimental sample is of all new jobseekers who registered and were approved by the platform between June 8th and July 14th, 2021 and had non-empty resumes. Column's (1) through (3) are unconditional on ever being hired, while the analysis in Column (4) is conditional on being hired at least once in their first month on the platform. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

analysis is the entire experimental sample who finished registration and were approved by the platform. The average worker in the control group only works for 2.6 hours during their first month on the platform. However, among those who are ever hired, the average worker in the control group works 238 hours. In Column (2) the outcome is workers' log of 1 plus hourly earnings. As with total hours, there are a lot of zeros, but among workers who have any hourly earnings, the average in the control group is \$1,529 in their first month. In Column (3) the outcome is the log of 1 plus the sum of workers earnings from both hourly and fixed price jobs, unconditional on ever being hired. Among workers who have any earnings, the average in the control group is \$2,957. Small positive effects on hourly earnings are mechanical due to the increase in hourly wages, and there does not seem to be any additional earnings effect to fixed price jobs. Because of the large number of jobseekers who have zero earnings, we report results from specifications which deal better with overdispersion in Appendix Figure 2. The treatment effect estimates are small and positive, but point estimates are sensitive to specification.

Lastly in Column (4) we show the impact of the treatment to the fraction of workers that are ever rehired. Unlike the other outcomes, rehires are conditional on a worker being hired at least once over their first month on the platform. Rehires are defined as an number of unique job posts an employer hires a worker for, minus 1. About 8% of all workers are rehired by the same employer at least once over the course of the experiment. This fraction does not differ in the treatment and control group.

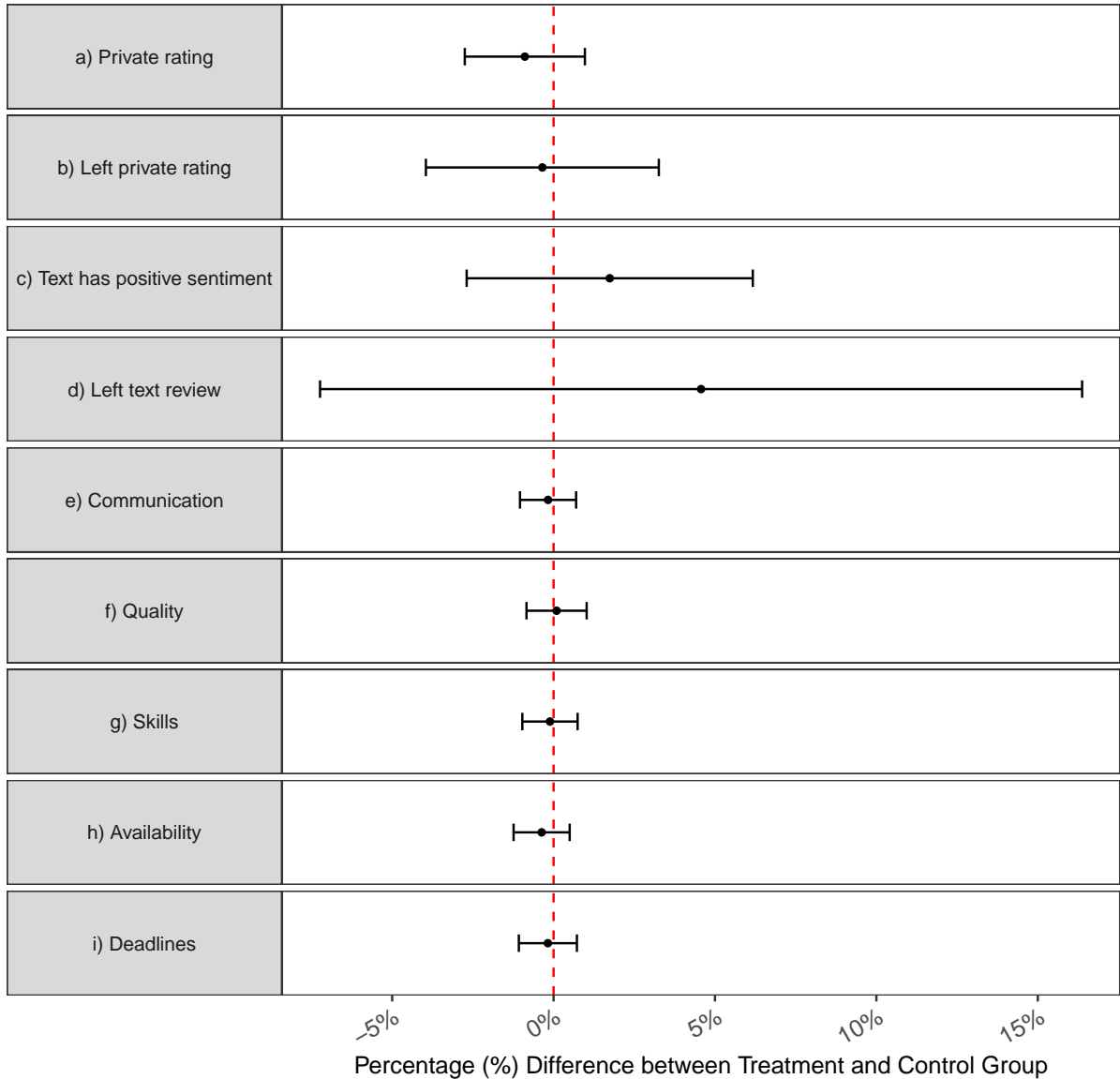
### **4.3 Employers satisfaction was unaffected by the treatment**

At the end of every contract, employers rate and review the workers by reporting both public and private rating to the platform. Private ratings are not shared with the worker. In the control group, workers had an average private rating of 8.63. In Figure 5a) we show that treated workers who formed any contracts over the experimental period did not have statistically different private ratings than workers in the control group. In Column (1) of Table 7 report the results from this regression. We show that workers in the treated group have an average private rating of 8.56 with a standard error of 0.08. We may also worry that if employers are less happy with the workers quality or productivity, that they may be more or less likely to leave a review at all. Figure 5b) we show that workers in the treatment group are not more or less likely to receive any rating than workers in the control group.

When the employers give these ratings they are also able to leave text reviews. While numerical ratings have become inflated in recent years, [Filippas et al. \(2022\)](#) show that the sentiments associated with the text of reviews has increased significantly less over time.



Figure 5: Effect of algorithmic writing assistance on ratings



*Notes:* This analysis looks at the effect of treatment on ratings outcomes on jobseekers in the experimental sample. Private ratings are on a scale from one to ten. Communication, Quality, Skills, Availability, and Deadlines ratings are public and left as star ratings, on a scale from one to five. The x-axis is the difference in the mean outcome between jobseekers in the treated group and the control group. A 95% confidence interval based on standard errors calculated using the delta method is plotted around each estimate. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021, and had non-empty resumes that were hired in their first month on the platform, with  $N = 4,250$ . Regression details on private ratings and text reviews can be found in Table 7. Regression details on public ratings can be found in Appendix Table A15.

Table 7: Effect of algorithmic writing assistance on contract ratings

	<i>Dependent variable:</i>			
	Private rating	Positive text review	Left any rating	Left any text review
	(1)	(2)	(3)	(4)
Algo Writing Treatment	-0.077 (0.082)	0.015 (0.019)	-0.002 (0.012)	0.006 (0.008)
Constant	8.633*** (0.059)	0.859*** (0.014)	0.624*** (0.008)	0.138*** (0.006)
Observations	4,250	1,185	6,263	6,263
R <sup>2</sup>	0.0002	0.001	0.00001	0.0001

*Notes:* This analysis looks at the effect of treatment on contract outcomes for jobseekers in the experimental sample. Column (1) defines private rating as the mean private rating on all jobs given by employers to the workers after the job ended, at the worker level. In Column (2) we take the text of the reviews left by employers on each job and use sentiment analysis (model: distilbert-base-uncased-finetuned-sst-2-english) to impute whether the review is positive, neutral, or negative, labeled one if it is positive or neutral. The outcome is the mean of these ratings over all contracts in the sample. Column (3) is the percentage of contracts worked where the freelancer received any private rating. And Column (4) is the percentage of contracts worked where the freelancer received any text based review. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021 and had non-empty resumes, for all jobs they worked within 28 days of registering for the platform. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

This means that text reviews are likely more informative about the workers’ quality than the numerical ratings. We use a BERT text classification model (HF canonical model maintainers, 2022) to label each review as having positive or negative sentiment. These classifications are significantly correlated with the private ratings, with a Pearson correlation coefficient of 0.54. In Figure 5c) we show that the treated workers’ average text reviews are not statistically different from the average sentiment of the reviews for control workers. In Figure 5d) we show that workers in the treatment group are not more or less likely to receive any text review than workers in the control group. Results from these regressions can be found in Table 7.

In Figure 5e) through i) we report the results of the effect of the treatment on the employers’ public ratings of the workers. Each outcome is a public rating the employers give to the workers at the end of a contract. Employers rate the workers communication, skills, quality of work, availability, cooperation, and ability to make deadlines. Each rating is given on a five point scale. There is less variation in the public ratings than in the private ones, and the average rating for each attribute is over 4.75 stars. Like the private ratings, there are no significant effects of the treatment to any of the ratings, including to workers’ communication skills. And the point estimate of the treatment effect to the quality of the work done is even positive. Results from these regressions can be found in Appendix Table A15.

### 4.3.1 How much power do we have to detect worse contractual outcomes?

Given the null effect of the treatment to ratings, a natural question is how much power is available to detect effects. While we do find a substantial increase in hiring—8%—these marginal hires are mixed in with a much larger pool of “inframarginal” hires that would likely be hired anyway, but for our intervention. How much worse could those marginal applicants have been and still get our results to private ratings in the treatment?

Let  $I$  indicate “inframarginal” jobseekers who would have been hired in the treatment or control. Let  $M$  indicate “marginal” jobseekers who are only hired in the treatment. For workers in the control group, the average private rating will be  $\bar{r}_C = \bar{r}_I$ . But for the treatment, the mean rating is a mixture of the ratings for the inframarginal and the ratings for the induced, marginal applicants, and so

$$\bar{r}_T = \frac{\bar{r}_I + \tau \bar{r}_M}{1 + \tau} \quad (1)$$

where  $\tau$  is the treatment effect. We assume no substitution, making our estimates conservative. The sampling distribution of the mean rating for the marginal group is

$$\bar{r}_M = \frac{\bar{r}_T(1 + \tau) - \bar{r}_C}{\tau} \quad (2)$$

Our course,  $\bar{r}_T$ ,  $\tau$  and  $\bar{r}_C$  are all themselves random variables. Furthermore, they are not necessarily independent. To compute the sampling distribution of  $\bar{r}_M$ , we bootstrap sample both the hiring regressions and the private feedback regressions on the experimental sample.<sup>11</sup> Because we do not have feedback on workers who are never hired, we use the estimates values to calculate  $\bar{r}_M$ . Figure 6 shows the sampling distribution of  $\bar{r}_M$ .

The treatment actual rating is plotted as a dotted line and control actual rating is plotted as a solid vertical line. The distribution is centered at these mean values.

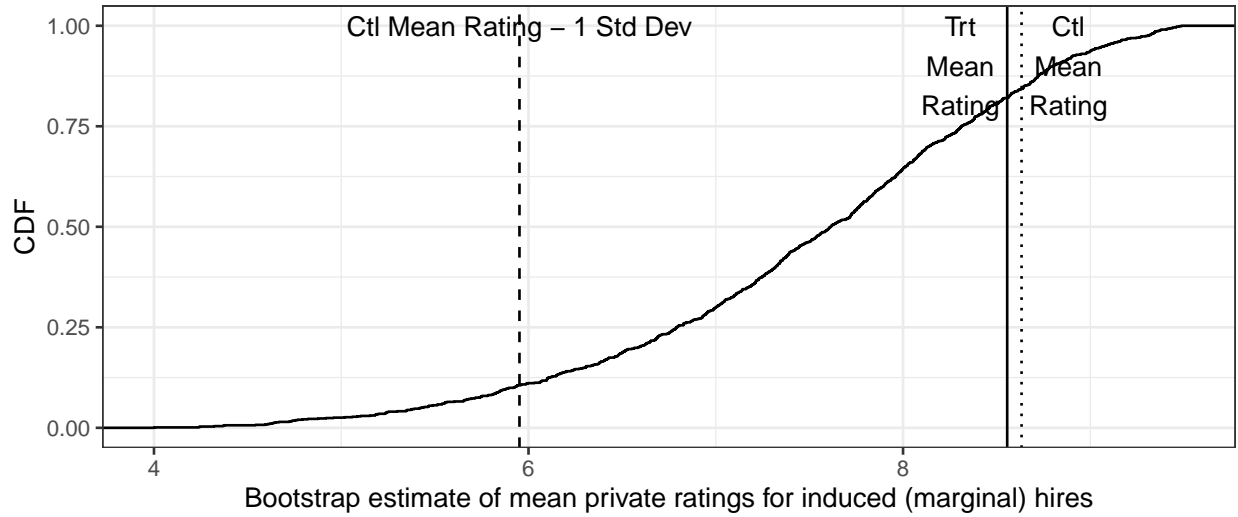
The dashed line indicates the control mean rating minus one standard deviation in the private ratings (where the standard deviation is 2.4). Comparing this value to the distribution of  $\bar{r}_M$ , this value (at the dashed line) lies at less than 0.1 of the density. In short, it would be quite surprising for us to get the results we have—an 8% increase in hires and slightly higher (but not significant ratings) if the actual marginal hires were a standard deviation worse.

Due to concerns about the loss of information in ratings caused by ratings inflation, it

---

<sup>11</sup>We define this sample as the workers allocated into the experiment who were approved by the platform and had non-empty resumes. From this we bootstrap sample with replacement. We run the hiring regressions on this sample and the ratings regressions on the same samples, narrowed to only those workers who were ever hired.

Figure 6: Sampling distribution of the private ratings of marginal hired jobseekers



is reasonable to question the level of variation that could realistically be observed, even in the presence of real effects. We do find variation in the ratings given to workers on the platform. In particular, workers with profiles written in a language other than English have an average private rating of 7.9 out of 10, which is lower than the average rating of 8.6 out of 10 for workers with profiles in English. Among workers with profiles in English, those based in the US have an average rating of 9.08 (with a standard deviation of 2.8), while workers from outside the US have an average rating of 8.46 (with a standard deviation of 2.14).

We can conduct a power analysis to determine the smallest effect size we could rule out with confidence. With 80% power and a 0.05 significance level, we can rule out any effects larger than 0.2 of a standard deviation. The overall standard deviation of ratings is 2.4, so an effect size of 0.2 standard deviations corresponds to a difference of 0.48 in ratings. This effect size is within the range of variation in ratings that we see within the data. Therefore, we can be reasonably confident that our study design would have been able to detect effects of practical significance.

#### 4.4 Heterogeneous treatment effects to hiring

We have already shown above in Appendix Table A13 that the treatment disproportionately impacted the fraction of words spelled correctly in non-native English speakers' resumes. If we look downstream to hiring outcomes, in Appendix Table A14, we interact the same groups with the treatment and look at their effect on the probability they were hired. The

point estimates are generally quite imprecise and we lack the power to conclude much. While non-native English speakers’ writing might benefit more from the treatment, it does not translate into more hires relative to native English speakers. In fact, we actually see positive point estimates for effects to hiring for US and Anglophone workers. This may appear surprising, but it is important to remember that those workers are much more likely to be hired to begin with. Absent the treatment, the average worker from an Anglophone country is about twice as likely to ever be hired within their first 28 days on the platform. While workers from Anglophone countries have smaller treatment effects to their writing error rate in Table A13 than their Anglophone counterparts, but they still have significant positive treatment effects.

Lastly, in Appendix Table A16 we report the same specifications but look for heterogeneity in the effects to private ratings or whether the text of the review had a positive sentiment. We can see from Column (2) that Anglophone workers are generally higher rated and Column (3) that US workers are as well. However neither have any additional effect when interacted with the treatment.

While our results suggest robust evidence for the “clarity view”, it is certainly possible that there are some types of work where ones writing ability is an important indicator of their on the job performance. We look specifically at jobseekers whose primary work is in writing in Appendix Table A12. Unfortunately, workers who specialize in writing make up less than 10% of the sample, and the standard errors are too large to be able to very confidently say anything about the effect of the treatment to ratings. Therefore, we do not reject the possibility that the signaling view could be important in jobs where writing is an important part of the output.

## 4.5 Robustness checks

In our main analysis, we narrow the sample to only those jobseekers whose profiles were approved by the platform. In Appendix Table A10 we run a similar regression on the full experimental sample, but we include profile approval as a control to see if it affects the estimates. In this analysis, we find that the treatment effect on the number of hires is slightly smaller than in the analysis conditional on platform approval—conditioning the sample on only jobseekers whose profiles were approved has an estimate of 7.8% while it is 10% in the full sample. The effect on the probability of any hire is 8% in the sample of only approved jobseekers and 8% in the unconditional sample. This approach and narrowing the sample to only approved jobseekers would “block” the approval channel. In Appendix Table A11 we report the same analysis not conditioned on profile approval. None of these

robustness checks change the direction or significance of any of the hiring estimates, and the slightly larger estimates in the unconditional sample are unsurprising because platform approval is a necessary condition for a jobseeker to be hired.

## 5 A simple model of the “clarity view” of resume writing

In this section, we formalize a rational model of how the writing intervention could (a) increase hiring but (b) not lead to worse matches. We formalize the argument that better writing allowed employers to better ascertain who was a potential match with a simple model, and show how this kind of interplay between resume quality and hiring could exist in equilibrium.

### 5.1 A mass of jobseekers with heterogeneous productivity

There is a unit mass of jobseekers. If hired, their productivity is  $\theta_i$ . Workers are either high-type ( $\theta = \theta_H$ ) or low-type ( $\theta = \theta_L$ ), with  $\theta_H > \theta_L$ . Workers know their own type. It is common knowledge that the fraction of high types in the market is  $\gamma$ . All workers, if hired, are paid their expected productivity, from the employer’s point of view. Hires only last one unit of time.

### 5.2 Jobseekers decide whether to put into resume-writing

Before being hired, jobseekers write resumes. Jobseekers must decide whether to put effort  $e \in \{0, 1\}$  into writing that resume. Effort itself is not observable. The cost of this effort is jobseekers-specific and there is a distribution of individual resume effort costs. The support of the cost distribution is  $[0, \bar{c}]$ . The distribution has mass everywhere and the CDF is  $F$  and PDF is  $f$ . Jobseekers who put in no effort have resume costs of 0, while those that put in effort have a cost of  $c_i$ . Critically, this cost is independent of a jobseeker’s type i.e., there is no Spence-like assumption that better workers find it cheaper to create better resumes (Spence, 1973).

Before making an offer, firms observe a signal of jobseekers’ type on their resume,  $R \in \{0, 1\}$ . With effort, a high-type jobseeker generates an  $R = 1$  signal; without effort,  $R = 0$ . A low-type jobseeker generates  $R = 0$  no matter what.

Clearly, low-types will never put in effort. The question is whether a high type will put in effort. The decision hinges on whether the cost of resume effort is worth the wage premium it creates. Let  $w_{R=0}$  be the wage paid in equilibrium to jobseekers with  $R = 0$ . Note that  $w_{R=1} = \theta_H$ , as there is no uncertainty about a jobseeker’s type if  $R = 1$ .

A jobseeker  $i$  who is a high-type will choose  $e = 1$  if  $\theta_H - w_{R=0}(c_i) > c_i$ . The marginal high-type jobseeker is indifferent between putting in effort or not, and has a resume-writing cost



of  $\hat{c}$ , where

$$\hat{c} = \theta_H - w_{R=0}(\hat{c}). \quad (3)$$

This implies that there are  $F(c)\gamma$  jobseekers that choose  $e = 1$ . These are the high-type jobseekers with relatively low resume writing costs. The remaining  $[1 - F(c)]\gamma$  high-type jobseekers choose  $e = 0$ . They are pooled together with the  $1 - \gamma$  jobseekers that choose  $e = 0$  because they are low-types.

From the employer's perspective, if they believe that the resume effort cost of the marginal high-type jobseekers is  $\hat{c}$ , the probability an  $R = 0$  jobseekers is high-type is

$$p_H^{R=0}(\hat{c}) = \frac{1 - F(\hat{c})}{1/\gamma - F(\hat{c})}. \quad (4)$$

The wage received by an  $R = 0$  worker is

$$w_{R=0}(\hat{c}) = \theta_L + (\theta_H - \theta_L)p_H^{R=0}(\hat{c}) \quad (5)$$

When the cost of the marginal jobseeker is higher, more jobseekers find it worth choosing  $e = 1$ , as  $F'(\hat{c}) > 0$ . This leaves fewer high-types in the  $R = 0$  pool, and so

$$\frac{dp_H^{R=0}}{d\hat{c}} < 0. \quad (6)$$

### 5.3 The equilibrium fraction of high-type workers putting effort into resume-writing

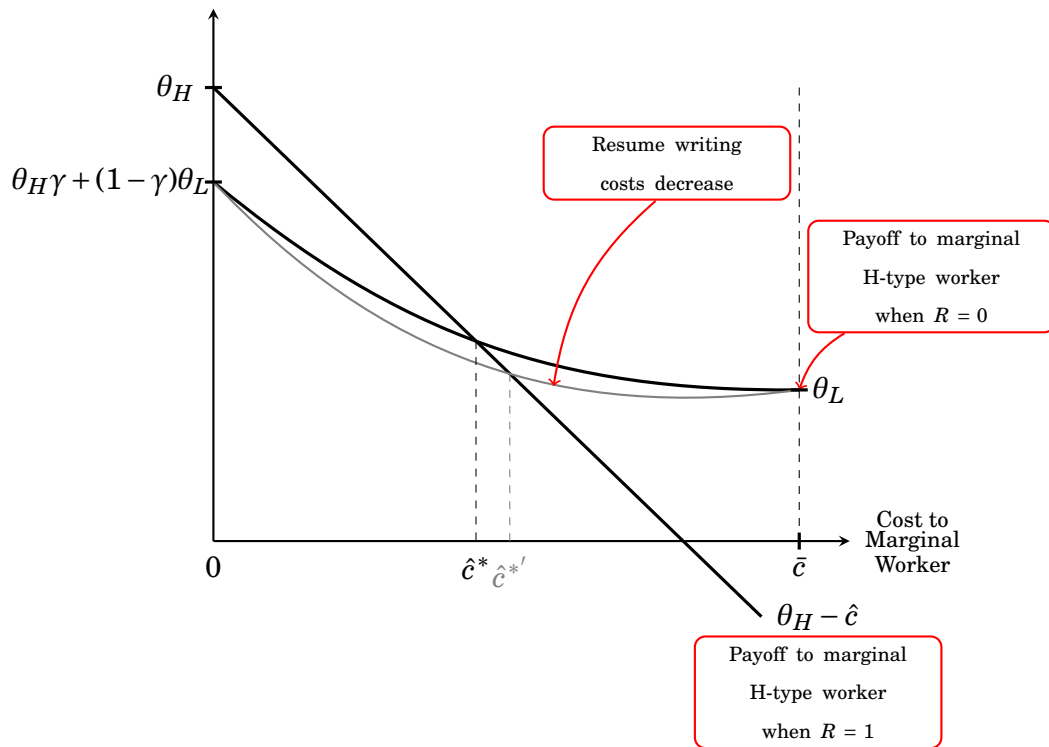
In equilibrium, there is some marginal high-type jobseeker indifferent between  $e = 0$  and  $e = 1$ , and so

$$(\theta_H - \theta_L)(1 - p_H^{R=0}(\hat{c}^*)) = \hat{c}^*.$$

Figure 7 illustrates the equilibrium i.e., the cost where the marginal jobseeker is indifferent between  $e = 0$  and  $e = 1$ . The two downward-sloping lines are the pay-offs to the marginal jobseeker for each  $\hat{c}$ . The pay-off to  $R = 1$  is declining, as the wage is constant (at  $\theta_H$ ) but the cost is growing linearly. The pay-off to  $R = 0$  is also declining, from Equation 6. Both curves are continuous.

Note that when the marginal jobseeker has  $\hat{c} = 0$ , there is just a point-mass of high-types that have a cost that low, i.e.,  $f(\hat{c})$ . Because the marginal jobseeker is indifferent between

Figure 7: Equilibrium determination of the marginal high-type jobseeker indifferent between putting effort into a resume



putting in effort and not putting in effort, jobseekers with costs of even  $\varepsilon$  will not put in effort. Since no one finds it worthwhile to put in effort the  $R = 0$  pool is just the expected value of all jobseekers. And the wage is  $w_{R=0}(\hat{c}) = \gamma\theta_H + (1 - \gamma)\theta_L$ . The marginal jobseeker pays nothing, so the pay-off is  $\theta_H$ .

At the other extreme,  $\hat{c} = \bar{c}$ , all but a point mass of jobseekers have a cost less than this. Since the marginal jobseeker is indifferent between putting in effort at a cost of  $\bar{c}$ , any jobseeker with cost  $\bar{c} - \varepsilon$  or below will put in effort. Then the  $R = 0$  pool is purely low-types and the wage is  $\theta_L$ . For the  $R = 1$  market, the marginal jobseeker has a cost of  $\hat{c}$  so the pay-off is  $\theta_H - \hat{c}$ . We know  $\theta_H > \gamma\theta_H + (1 - \gamma)\theta_L$ . And by assumption,  $\theta_L > \theta_H - \hat{c}$ , and so by the intermediate value theorem, an equilibrium  $\hat{c}^*$  exists on  $(0, \bar{c})$ .

#### 5.4 A shift in the resume writing cost distribution leads to more high-type workers choosing to exert effort

Now suppose a technology comes along that lowers—or at least keeps the same—resume writing costs for all jobseekers. This would shift  $F$  higher for all points except the endpoints of the support, creating a new distribution of costs that first-order stochastically dominates

the other.

Before determining the new equilibrium, note that no matter the marginal  $\hat{c}$ , when  $F$  increases, the probability that an  $R = 0$  worker is a high-type declines, as

$$\frac{dp_H}{dF} = -\frac{1}{(F-2)^2} < 0. \quad (7)$$

This shifts the  $w_{R=0}$  curve down everywhere, without changing the endpoints.

Because  $w_{R=1} - \hat{c}$  is downward sloping, it intersects  $w_{R=0}(\hat{c})$  at a higher value of  $\hat{c}$ . At the new equilibrium, the marginal jobseeker has resumes costs of  $\hat{c}^{*'} > \hat{c}^*$ . At this new equilibrium, more jobseekers choose  $e = 1$ , causing more  $R = 1$  signals. This lowers wages for the  $R = 0$  group.

## 5.5 The effects of lower costs are theoretically ambiguous

Note that this shift in costs is not Pareto improving—low-types are made worse off as they find themselves in a pool with fewer high-types. Furthermore, because workers are all paid their expected product, the surplus maximizing outcome would be for everyone to choose  $R = 0$ . Resume effort purely changes around the allocation of the wage bill, not the total amount. Total surplus is

$$\theta_H \gamma + (1 - \gamma) \theta_L - \int_0^{\hat{c}} cf(c)dc, \quad (8)$$

which is maximized at  $\hat{c} = 0$ , i.e., when no one finds it worthwhile to choose effort. However, with a *shift* in cost distribution (raising  $F$ ), what matters is whether the marginal decrease in costs for all inframarginal workers i.e, those with  $c < \hat{c}$  outweighs the costs borne by the (newly) marginal jobseekers who choose to put in effort.

In our model, all job offers are accepted. However, if we think of jobseekers as having idiosyncratic reservation values that determine whether they accept an offer, the shift in costs makes it more likely that high-types will accept an offer, while making it less likely that low-types will accept an offer. This is consistent with results where there is a greater chance an employer hires at all in the treatment. It is also consistent with our result of higher wages. Finally, if we think of employer ratings being a function of surplus, our finding of no change in employer satisfaction is also consistent, as employers are, in all cases, just paying for expected productivity.

## 6 Conclusion

Employers are more likely to hire new labor market entrants with better-written resumes. We argue that better writing makes it easier for employers to judge the match quality of a particular worker. We show results from a field experiment in an online labor market where treated workers were given algorithmic writing assistance. These jobseekers were 8% more likely to get hired and formed 7.8% more contracts over the month-long experiment. While one might have expected writing quality to be a valuable indicator of worker quality, the treatment did not affect employers’ ratings of hired workers. We provide a model of the hiring process where the cost of exerting effort on a resume is lowered by the algorithmic writing assistance, which helps employers to distinguish between high and low-productivity workers.

One possibility is that the benefits to treated workers came at the expense of other workers, as both treated- and control-assigned workers compete in the same market. Crowd-out concerns have been shown to be important with labor market assistance (Crépon et al., 2013). However, even if additional hires came from experienced workers, this is likely still a positive result. New labor market entrants are uniquely disadvantaged (Pallais, 2014) in online labor markets. To the extent that the gains to new workers come partially at the expense of experienced workers, this is likely a good trade-off.

Conceptualizing AI/ML innovation and proliferation as a fall in the cost of prediction technology fits our setting (Agrawal et al., 2018b,a). Writing a resume is, in part, an applied prediction task—what combination of words and phrases, arranged in what order, are likely to maximize my pay-off from a job search? The Algorithmic Writing Service reduces the effort or cost required for making these decisions. When revising their resumes, rather than identifying errors in their own predictions themselves, jobseekers with access to the Algorithmic Writing Service are given suggestions for error correction and cleaned up writing. Furthermore, the treatment, by lowering the costs of error-free writing for at least some jobseekers, causes them to do better at writing their resumes.

These kinds of algorithmic writing assistance will likely “ruin” writing as a signal of ability. With the proliferation of writing technologies with capabilities far beyond what is explored here (Brown et al., 2020), even if the “signaling view” was at one time true, technological changes are likely to make it not true in the future.

## References

- Abebe, Girum, A Stefano Caria, and Esteban Ortiz-Ospina**, “The selection of talent: experimental and structural evidence from Ethiopia,” *American Economic Review*, 2021, *111* (6), 1757–1806.
- Agrawal, Ajay, John Horton, Nicola Lacetera, and Elizabeth Lyons**, “Digitization and the contract labor market,” *Economic Analysis of the Digital Economy*, 2015, 219.
- , **Joshua Gans, and Avi Goldfarb**, “Prediction, judgment, and complexity: a theory of decision-making and Artificial Intelligence,” in “The economics of Artificial Intelligence: an agenda,” University of Chicago Press, 2018, pp. 89–110.
- , —, and —, *Prediction machines: the simple economics of Artificial Intelligence*, Harvard Business Press, 2018.
- , **Nicola Lacetera, and Elizabeth Lyons**, “Does standardized information in online markets disproportionately benefit job applicants from less developed countries?,” *Journal of international Economics*, 2016, *103*, 1–12.
- Belot, Michèle, Philipp Kircher, and Paul Muller**, “Providing advice to jobseekers at low cost: an experimental study on online advice,” *The Review of Economic Studies*, 2018, *86* (4), 1411–1447.
- Bertrand, Marianne and Sendhil Mullainathan**, “Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination,” *American Economic Review*, 2004, *94* (4), 991–1013.
- Bolton, Gary, Ben Greiner, and Axel Ockenfels**, “Engineering trust: reciprocity in the production of reputation information,” *Management Science*, 2013, *59* (2), 265–285.
- Briscese, Guglielmo, Giulio Zanella, and Veronica Quinn**, “Providing government assistance online: a field experiment with the unemployed,” *Journal of Policy Analysis and Management*, 2022, *41* (2), 579–602.
- Brown, Tom, Benjamin Mann, Nick Ryder, Melanie Subbiah, Jared D Kaplan, Prafulla Dhariwal, Arvind Neelakantan, Pranav Shyam, Girish Sastry, Amanda Askell et al.**, “Language models are few-shot learners,” *Advances in Neural Information Processing Systems*, 2020, *33*, 1877–1901.
- Brynjolfsson, Erik, Danielle Li, and Lindsey R Raymond**, “Generative AI at work,” Technical Report, National Bureau of Economic Research 2023.

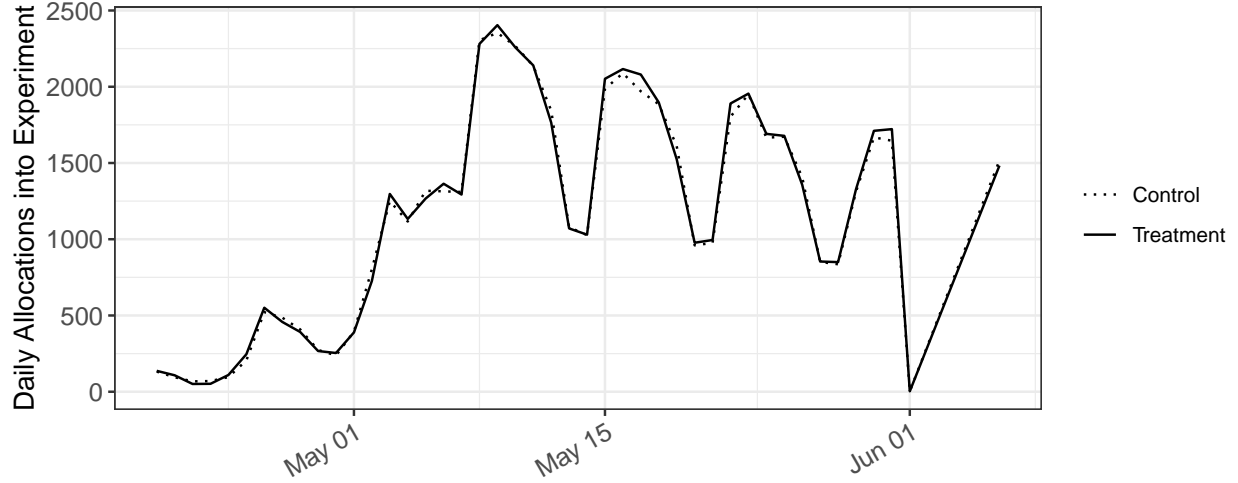
- Cai, Hongbin, Ginger Zhe Jin, Chong Liu, and Li an Zhou**, “Seller reputation: from word-of-mouth to centralized feedback,” *International Journal of Industrial Organization*, 2014, 34, 51–65.
- Card, David, Jochen Kluge, and Andrea Weber**, “Active labour market policy evaluations: A meta-analysis,” *The Economic Journal*, 2010, 120 (548), F452–F477.
- Chan, Jason and Jing Wang**, “Hiring preferences in online labor markets: Evidence of a female hiring bias,” *Management Science*, 2018, 64 (7), 2973–2994.
- Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora**, “Do labor market policies have displacement effects? Evidence from a clustered randomized experiment,” *The Quarterly Journal of Economics*, 2013, 128 (2), 531–580.
- Eloundou, Tyna, Sam Manning, Pamela Mishkin, and Daniel Rock**, “GPTs Are GPTs: An early look at the labor market impact potential of large language models,” *arXiv preprint arXiv:2303.10130*, 2023.
- Farber, Henry S, Dan Silverman, and Till Von Wachter**, “Determinants of callbacks to job applications: An audit study,” *American Economic Review*, 2016, 106 (5), 314–18.
- Felten, Edward W, Manav Raj, and Robert Seamans**, “Occupational heterogeneity in exposure to generative AI,” *SSRN*, 2023.
- Filippas, Apostolos, John Joseph Horton, and Joseph Golden**, “Reputation inflation,” *Marketing Science*, 2022.
- Fradkin, Andrey, Elena Grewal, and David Holtz**, “Reciprocity and unveiling in two-sided reputation systems: evidence from an experiment on airbnb,” *Marketing Science*, 2021, 40 (6), 1013–1029.
- Ghose, Anindya and Panagiotis G Ipeirotis**, “Estimating the helpfulness and economic impact of product reviews: Mining text and reviewer characteristics,” *IEEE transactions on knowledge and data engineering*, 2010, 23 (10), 1498–1512.
- Goldfarb, Avi and Catherine Tucker**, “Digital economics,” *Journal of Economic Literature*, 2019, 57 (1), 3–43.
- HF canonical model maintainers**, “DistilBERT-base-uncased-finetuned-sst-2-English (Revision bfdd146),” 2022.

- Hong, Yili, Jing Peng, Gordon Burtch, and Ni Huang**, “Just DM me (politely): direct messaging, politeness, and hiring outcomes in online labor markets,” *Information Systems Research*, 2021, 32 (3), 786–800.
- Horton, John J.**, “Online labor markets,” *Internet and Network Economics: 6th International Workshop, WINE 2010, Stanford, CA, USA, December 13-17, 2010. Proceedings*, 2010.
- , “The effects of algorithmic labor market recommendations: evidence from a field experiment,” *Journal of Labor Economics*, 2017, 35 (2), 345–385.
- Kang, Sonia K, Katherine A DeCelles, András Tilcsik, and Sora Jun**, “Whitened résumés: race and self-presentation in the labor market,” *Administrative Science Quarterly*, 2016, 61 (3), 469–502.
- Kokkodis, Marios and Sam Ransbotham**, “Learning to successfully hire in online labor markets,” *Management Science*, 2022.
- Luca, Michael and Oren Reshef**, “The effect of price on firm reputation,” *Management Science*, 2021, 67 (7), 4408–4419.
- Marinescu, Ioana**, “The general equilibrium impacts of unemployment insurance: Evidence from a large online job board,” *Journal of Public Economics*, 2017, 150, 14–29.
- **and Ronald Wolthoff**, “Opening the black box of the matching function: the power of words,” *Journal of Labor Economics*, 2020, 38 (2), 535–568.
- Martin-Lacroux, Christelle and Alain Lacroux**, “Do employers forgive applicants’ bad spelling in résumés?,” *Business and Professional Communication Quarterly*, sep 2017, 80 (3), 321–335.
- Moss-Racusin, Corinne A., John F. Dovidio, Victoria L. Brescoll, Mark J. Graham, and Jo Handelsman**, “Science faculty’s subtle gender biases favor male students,” *Proceedings of the National Academy of Sciences*, 2012, 109 (41), 16474–16479.
- Noy, Shakked and Whitney Zhang**, “Experimental evidence on the productivity effects of generative Artificial Intelligence,” *SSRN*, 2023.
- Oreopoulos, Philip**, “Why do skilled immigrants struggle in the labor market? A field experiment with thirteen thousand resumes,” *American Economic Journal: Economic Policy*, 2011, 3 (4), 148–71.

- Pallais, Amanda**, “Inefficient hiring in entry-level labor markets,” *American Economic Review*, nov 2014, 104 (11), 3565–3599.
- Sajjadiani, Sima, Aaron J Sojourner, John D Kammeyer-Mueller, and Elton Myk-  
erezi**, “Using machine learning to translate applicant work history into predictors of performance and turnover,” *Journal of Applied Psychology*, 2019, 104 (10), 1207.
- Spence, Michael**, “Job market signaling,” *The Quarterly Journal of Economics*, Aug 1973, 87 (3), 355–374.
- Stanton, Christopher T and Catherine Thomas**, “Landing the first job: The value of intermediaries in online hiring,” *The Review of Economic Studies*, 2016, 83 (2), 810–854.
- Sterkens, Philippe, Ralf Caers, Marijke De Couck, Michael Geamanu, Victor Van  
Driessche, and Stijn Baert**, “Costly mistakes: why and when spelling errors in resumes jeopardise interview chances,” *Working paper*, 2021.
- Tafti, Elena Ashtari**, *Technology, skills, and performance: the case of robots in surgery*, Institute for Fiscal Studies, 2022.
- Weiss, Daphne, Sunny X. Liu, Hannah Mieczkowski, and Jeffrey T. Hancock**, “Effects of using Artificial Intelligence on interpersonal perceptions of job applicants,” *Cyberpsychology, Behavior, and Social Networking*, 2022, 25 (3), 163–168.



Figure 1: Daily allocations of jobseekers into experimental cells



Notes: This plot shows the daily allocations into the treatment and control cells for the experimental sample of 480,948 new jobseekers to the platform.

## A Appendix

### A.1 Summary statistics and descriptives

Table A1: Summary statistics of jobs worked in the control group

	Mean	Std Deviation
Length of job in days	59	(114)
Fraction of jobs that are hourly	0.34	(0.47)
Total hours worked	201	(482)
Earnings from hourly jobs (\$)	2,524	(2,524)
Earnings from fixed-price jobs (\$)	386	(386)
Min hourly rate (\$)	17.04	(18.42)
Max hourly rate (\$)	17.22	(19)

Notes: This table reports summary statistics for jobs worked by workers in the control group of the experiment. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021 and had non-empty resumes. The sample of jobs include any job where the worker was hired between June 08,2021 and August 14, 2021, with N = 4,521.

Table A2: Summary statistics on error counts and rates in the control group

	Total Errors	Error Rate
All Error Types	4.390 (10.257)	0.080 (0.252)
Capitalization Errors	0.140 (0.543)	0.004 (0.029)
Possible Typo	2.312 (8.768)	0.041 (0.106)
Grammar Errors	0.219 (0.589)	0.004 (0.014)
Punctuation Errors	0.425 (1.629)	0.008 (0.213)
Typographic Errors	0.821 (2.985)	0.016 (0.051)
Style Errors	0.317 (0.890)	0.004 (0.011)
Miscellaneous Errors	0.103 (0.391)	0.002 (0.009)
Redundant Phrases	0.025 (0.162)	0.000 (0.003)
Nonstandard Phrases	0.002 (0.050)	0.000 (0.001)
Commonly Confused Words	0.010 (0.117)	0.000 (0.002)
Collocations	0.012 (0.121)	0.000 (0.003)
Semantic Errors	0.003 (0.061)	0.000 (0.001)

*Notes:* This table reports means and standard errors of the writing errors in the resumes of the control group. The first column displays the average total error count and the second column displays the average error rate (total errors normalized by word count). Writing errors are defined by LanguageToolR. The sample is made up of all jobseekers in the control group of the experimental sample who submitted non-empty resumes and were approved by the platform.

Table A3: Effects of writing assistance on profile submission and platform approval

	<i>Dependent variable:</i>		
	Profile submitted x 100	Approved x 100	
	(1)	(2)	(3)
Algo Writing Treatment	0.106 (0.144)	0.199 (0.133)	0.186 (0.142)
Constant	45.532*** (0.102)	89.057*** (0.094)	40.550*** (0.100)
Observations	480,948	219,242	480,948
R <sup>2</sup>	0.00000	0.00001	0.00000

*Notes:* This table analyzes the effect of the treatment on whether or not a jobseeker’s profile was submitted and approved. In Column (1) the outcome is 100 times a binary indicator for whether or not the jobseeker completed platform registration and submitted their resume, on the full experimental sample. In Column (2) the outcome is 100 times a binary indicator for whether or not the platform approved the resume, on the sample of only those jobseekers who submitted their resumes. In Column (3) the outcome is 100 times a binary indicator for whether or not the platform approved the resume, on the full experimental sample. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A4: Description of Error Rule Categories with Examples

Category	Description	Examples
American English Phrases	Sentence favors the American English spelling of words.	<i>apologize, catalog, civilization, defense</i>
British English, Oxford Spelling	Sentence favors the British English spelling of words.	<i>apologise, catalogue, civilisation, defence</i>
Capitalization	Rules about detecting uppercase words where lowercase is required and vice versa.	<i>This house is old. it was built in 1950. I really like Harry potter.</i>
Collocations	A collocation is made up of two or more words that are commonly used together in English. This refers to an error in this type of phrase.	<i>Undoubtedly, this is the result of an extremely dynamic development of Lublin in the recent years. I will take it in to account. It's batter to be save then sorry.</i>
Commonly Confused Words	Words that are easily confused, like 'there' and 'their' in English.	<i>I have my won bed. Their elicit behavior got the students kicked out of school.It's the worse possible outcome.</i>
Grammar	Violations related to system of rules that allow us to structure sentences.	<i>Tom make his life worse. A study like this one rely on historical and present data.This is best way of dealing with errors.</i>
Miscellaneous	Miscellaneous rules that do not fit elsewhere.	<i>This is best way of dealing with errors. The train arrived a hour ago. It's nice, but it doesn't work. (inconsistent apostrophes)</i>
Nonstandard Phrases		<i>I never have been to London. List the names in an alphabetical order. Why would a man all of the sudden send flowers?</i>
Possible Typo	Spelling issues.	<i>It'a extremely helpful when it comes to homework. We haven't earned anything.This is not a HIPPA violation.</i>
Punctuation	Error in the marks, such as period, comma, and parentheses, used in writing to separate sentences and their elements and to clarify meaning.	<i>"I'm over here, she said. Huh I thought it was done already. The U.S.A is one of the largest countries.</i>
Redundant Phrases	Redundant phrases contain words that say the same thing twice. When one of the words is removed, the sentence still makes sense. Sometimes the sentence has to be slightly restructured, but the message remains the same.	<i>We have more than 100+ customers. He did it in a terrible way. The money is sufficient enough to buy the sweater.</i>
Semantics	Logic, content, and consistency problems.	<i>It allows us to both grow, focus, and flourish. On October 7, 2025 , we visited the client.This was my 11nd try.</i>
Style	General style issues not covered by other categories, like overly verbose wording.	<i>Moreover, the street is almost entirely residential. Moreover, it was named after a poet. Doing it this way is more easy than the previous method. I'm not very experienced too. Anyways, I don't like it.</i>
Typography	Problems like incorrectly used dash or quote characters.	<i>This is a sentence with two consecutive spaces. I have 3dogs.The price rose by \$12,50. I'll buy a new T—shirt.</i>

Table A5: Hiring outcomes predicted based on language errors in observational data

	<i>Dependent variable:</i>							
	Number of Contracts		Hired		Number of Contracts		Hired	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total errors	-0.0003** (0.0001)		-0.0002** (0.0001)		-0.0002 (0.0001)		-0.0001 (0.0001)	
Num words	0.0004*** (0.00002)		0.0002*** (0.00001)		0.0003*** (0.00002)		0.0001*** (0.00001)	
Error rate		-0.017*** (0.005)		-0.011*** (0.003)		-0.010** (0.005)		-0.007*** (0.003)
Constant	0.020*** (0.002)	0.049*** (0.001)	0.017*** (0.001)	0.033*** (0.001)	0.014*** (0.002)	0.034*** (0.001)	0.014*** (0.001)	0.024*** (0.001)
Normalized		X		X		X		X
Controls					X	X	X	X
Observations	65,114	65,114	65,114	65,114	65,114	65,114	65,114	65,114
R <sup>2</sup>	0.007	0.0002	0.007	0.0002	0.108	0.105	0.142	0.139

*Notes:* This table analyzes correlation between all writing errors on jobseekers' resumes and their hiring outcomes. The first independent variable is the total number of writing errors on a jobseekers' resume. The second independent variable is the total number of errors divided by the length of their resume, in number of words. Number of Contracts is defined as as the number of unique jobs they work over the month after they register for the platform. Hired is defined as 1 if the jobseeker was ever hired over that month, and 0 if else. Columns (5) through (8) include controls for profile hourly rate and job category. Writing errors are defined by LanguageToolR. The sample is made up of all jobseekers who registered for the platform in the week before the experiment who submitted non-empty resumes.

Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A6: Hiring outcomes predicted based on language errors in the observational data

	<i>Dependent variable:</i>			
	Number of Contracts	Hired	Number of Contracts	Hired
	(1)	(2)	(3)	(4)
Number of words	0.0004*** (0.00002)	0.0002*** (0.00001)	0.0003*** (0.00002)	0.0001*** (0.00001)
Capitalization Error	-0.006*** (0.002)	-0.002* (0.001)	-0.004* (0.002)	-0.001 (0.001)
Possible Typo	-0.00000 (0.0002)	-0.00001 (0.0001)	0.0001 (0.0002)	0.00003 (0.0001)
Grammar Error	-0.012*** (0.002)	-0.007*** (0.001)	-0.008*** (0.002)	-0.004*** (0.001)
Punctuation Error	0.002* (0.001)	0.001*** (0.0004)	0.0004 (0.001)	0.001* (0.0004)
Typography Error	-0.001** (0.0005)	-0.001*** (0.0002)	-0.001* (0.0004)	-0.001*** (0.0002)
Style Error	0.006*** (0.002)	0.003*** (0.001)	0.006*** (0.001)	0.003*** (0.001)
Miscellaneous Error	-0.011*** (0.003)	-0.007*** (0.002)	-0.005 (0.003)	-0.003** (0.002)
Redundant Phrases	-0.009 (0.008)	-0.003 (0.004)	-0.006 (0.008)	-0.001 (0.004)
Nonstandard Phrases	0.009 (0.026)	-0.001 (0.014)	0.005 (0.025)	-0.003 (0.013)
Commonly Confused Words	-0.021* (0.011)	-0.008 (0.006)	-0.015 (0.011)	-0.003 (0.006)
Collocations	-0.013 (0.011)	-0.003 (0.006)	-0.013 (0.010)	-0.003 (0.005)
Semantic Error	0.035 (0.021)	0.004 (0.011)	0.031 (0.020)	0.002 (0.010)
Constant	0.022*** (0.002)	0.018*** (0.001)	0.015*** (0.002)	0.015*** (0.001)
Controls			X	X
Observations	65,114	65,114	65,114	65,114
R <sup>2</sup>	0.008	0.009	0.109	0.142

*Notes:* This table analyzes correlation between various writing errors on jobseekers' resumes and their hiring outcomes. Number of Contracts is defined as the number of unique jobs they work over the month after they register for the platform. Hired is defined as 1 if the jobseeker was ever hired over that month, and 0 if else. Columns (3) and (4) include controls for profile hourly rate and job category. Writing errors are defined by LanguageToolR. The sample is made up of all jobseekers who registered for the platform in the week before the experiment who submitted non-empty resumes.

Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A7: Effects of writing assistance on length of resume

	<i>Dependent variable:</i>
	Number of words in resume
Algo Writing Treatment	0.127 (0.314)
Constant	70.541*** (0.223)
Observations	194,700
R <sup>2</sup>	0.00000

*Notes:* This table analyzes the effect of the treatment on the number of words in a jobseeker’s resume. The sample is made up of all jobseekers in the experimental sample who submitted non-empty profiles and were approved by the platform. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A8: Effect of Treatment to Writing Errors, Page 1

	<i>Dependent variable:</i>						
	Spelling (1)	Capitalization (2)	Possible Typo (3)	Grammar (4)	Punctuation (5)	Typography (6)	Style (7)
Algo Writing Treatment	-0.001* (0.0005)	-0.0005*** (0.0001)	-0.002*** (0.0005)	-0.0005*** (0.0001)	-0.0004 (0.0004)	-0.002*** (0.0003)	0.0003*** (0.0001)
Constant	0.036*** (0.0003)	0.003*** (0.00005)	0.041*** (0.0003)	0.004*** (0.00004)	0.010*** (0.0003)	0.015*** (0.0002)	0.004*** (0.00004)
Observations	194,700	194,700	194,700	194,700	194,700	194,700	194,700
R <sup>2</sup>	0.00002	0.0003	0.0001	0.0004	0.0000	0.0004	0.0001

*Notes:* This table analyzes the effect of the treatment on all types of writing errors, normalized by resume length. Writing errors are defined by LanguageToolR, and divided by the number of words in a jobseekers' resume to calculate their error rate. The sample is made up of all jobseekers in the experimental sample who completed the platform registration page and submitted non-empty resume. Significance indicators:  $p \leq 0.10$  :

\*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.



Table A9: Effect of Treatment to Writing Errors, Page 2

		<i>Dependent variable:</i>					
	Miscellaneous	Redundant Phrases	Nonstandard Phrases	Commonly Confused Words	Collocations	Semantics	
	(1)	(2)	(3)	(4)	(5)	(6)	
Algo Writing Treatment	-0.0004*** (0.00003)	-0.00003* (0.00001)	-0.00001 (0.00000)	-0.00005*** (0.00001)	-0.0001*** (0.00001)	0.00001 (0.00001)	
Constant	0.002*** (0.00002)	0.0004*** (0.00001)	0.00003*** (0.00000)	0.0001*** (0.00001)	0.0003*** (0.00001)	0.0001*** (0.00000)	
Observations	194,700	194,700	194,700	194,700	194,700	194,700	
R <sup>2</sup>	0.001	0.00002	0.00001	0.0001	0.0001	0.00000	

*Notes:* This table analyzes the effect of the treatment on all types of writing errors, normalized by resume length. Writing errors are defined by LanguageToolR, and divided by the number of words in a jobseekers' resume to calculate their error rate. The sample is made up of all jobseekers in the experimental sample who completed the platform registration page and submitted non-empty resume. Significance:  $p \leq 0.10$  : †,  $p \leq 0.05$  : \*,  $p \leq 0.01$  : \*\* and  $p \leq .001$  : \*\*\*.

Table A10: Effect of algorithmic writing assistance on hiring outcomes, controlling for platform approval

	<i>Dependent variable:</i>				
	Num Invitations	Num Shortlists	Num Interviews	Hired x 100	Num Contracts
	(1)	(2)	(3)	(4)	(5)
Algo Writing Treatment	0.0004 (0.001)	0.001 (0.001)	0.002 (0.002)	0.100*** (0.032)	0.001** (0.001)
Approved by Platform	0.142*** (0.001)	0.040*** (0.001)	0.179*** (0.002)	3.204*** (0.033)	0.049*** (0.001)
Constant	0.00001 (0.001)	-0.0003 (0.0005)	-0.001 (0.001)	-0.050* (0.027)	-0.001 (0.001)
Observations	480,948	480,948	480,948	480,948	480,948
R <sup>2</sup>	0.023	0.010	0.024	0.019	0.011

*Notes:* This analysis looks at the effect of treatment on hiring outcomes on jobseekers in the experimental sample. The Column (1) outcome Invitations is the number of times they were recruited to a job over their first month. Column (2) is the number of times their application was shortlisted over that month. Column (3) is the number of interviews they gave over that month. Column (4) defines Hired x 100 as one hundred times the probability the jobseeker was hired over that month. Column (5) defines Number of Contracts as the number of unique jobs they work over the month after they register for the platform.

The sample used in this analysis is the entire experimental sample. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A11: Effect of algorithmic writing assistance on hiring outcomes, unconditional on platform approval

	<i>Dependent variable:</i>				
	Num Invitations	Num Shortlists	Num Interviews	Hired x 100	Num Contracts
	(1)	(2)	(3)	(4)	(5)
Algo Writing Treatment	0.001 (0.001)	0.001 (0.001)	0.002 (0.002)	0.106*** (0.033)	0.002** (0.001)
Constant	0.058*** (0.001)	0.016*** (0.0004)	0.072*** (0.001)	1.249*** (0.023)	0.019*** (0.0005)
Observations	480,948	480,948	480,948	480,948	480,948
R <sup>2</sup>	0.00000	0.00001	0.00000	0.00002	0.00001

*Notes:* This analysis looks at the effect of treatment on hiring outcomes on jobseekers in the experimental sample. The Column (1) outcome Invitations is the number of times they were recruited to a job over their first month. Column (2) is the number of times their application was shortlisted over that month. Column (3) is the number of interviews they gave over that month. Column (4) defines Hired x 100 as one hundred times the probability the jobseeker was hired over that month. Column (5) defines Number of Contracts as the number of unique jobs they work over the month after they register for the platform. The sample used in this analysis is the entire experimental sample. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A12: Effect of algorithmic writing assistance on writers

	<i>Dependent variable:</i>			
	Error Rate X 100	Hires X 100	Private rating	Positive text review
	(1)	(2)	(3)	(4)
Algo Writing Treatment	-0.030 (0.102)	0.190 (0.184)	-0.053 (0.214)	-0.064 (0.060)
Constant	2.890*** (0.072)	2.855*** (0.130)	8.456*** (0.153)	0.874*** (0.046)
Observations	33,907	33,907	672	149
R <sup>2</sup>	0.00000	0.00003	0.0001	0.008

*Notes:* This analysis looks at the effect of treatment on contract outcomes for jobseekers in the experimental sample whose primary job category is listed as Writing. In Column (1) the outcome is 100 times the error rate based on any error type in their resume. In Column (2) the outcome is 100 times whether or not the jobseeker ever is hired over their first month on the platform. In Column (3) the outcome is the mean private rating of jobseeker for any jobs they work in their first month on the platform. In Column (4) we take the text of the reviews left by employers on each job and use sentiment analysis (model: distilbert-base-uncased-finetuned-sst-2-english) to impute whether the review is positive, neutral, or negative, labeled one if it is positive or neutral. The outcome is the mean of these ratings over all contracts in the sample. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021 and had non-empty resumes, for all jobs they worked within 28 days of registering for the platform. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A13: Effects of writing assistance on error rate

	<i>Dependent variable:</i>		
	Total Error Rate x 100		
	(1)	(2)	(3)
Algo Writing Treatment (Trt)	-0.557*** (0.071)	-0.676*** (0.079)	-0.644*** (0.077)
Anglophone Country		-4.755*** (0.130)	
Trt x Anglo		0.605*** (0.184)	
US			-4.676*** (0.144)
Trt x US			0.548*** (0.203)
Constant	7.958*** (0.051)	8.831*** (0.056)	8.629*** (0.054)
Observations	194,700	194,700	194,700
R <sup>2</sup>	0.0003	0.012	0.010

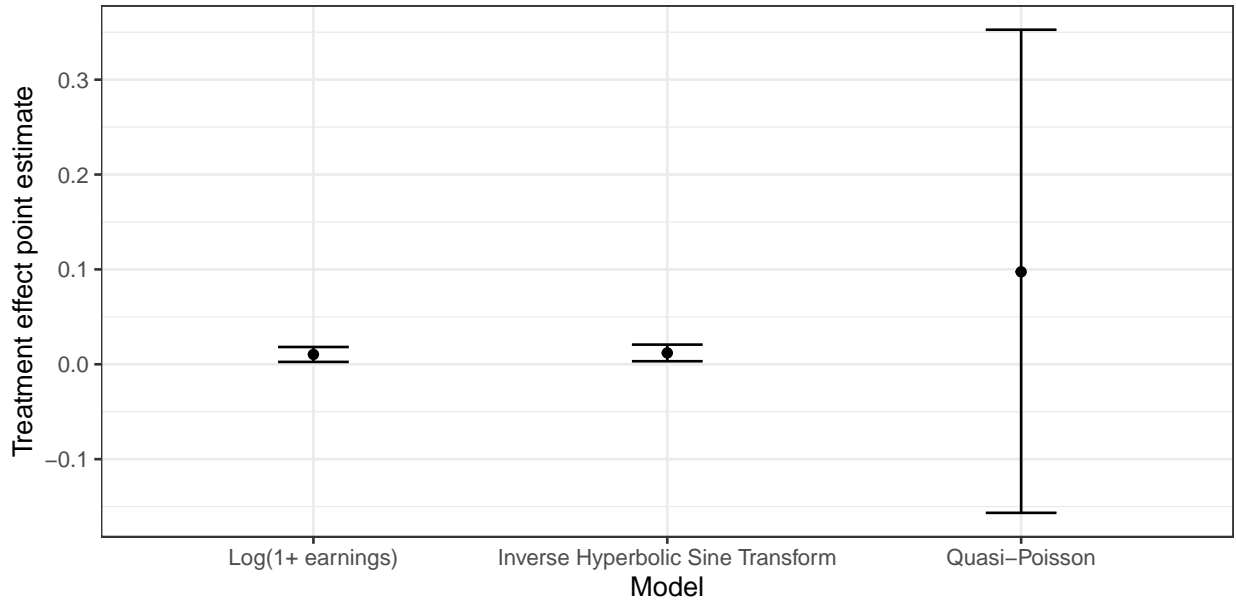
*Notes:* In Column (1) we show the overall effect of the treatment to the number of errors on a jobseekers' resume divided by the number of words. In Column (2) we interact the treatment with a dummy variable for if the jobseeker is from the US, UK, Canada, or Australia. In Column (3) we interact the treatment with a dummy for if the jobseeker is in the US. The experimental sample is of all new jobseekers who registered and were approved by the platform between June 8th and July 14th, 2021 and had non-empty resumes. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A14: Effects of writing assistance on hiring, by sub-groups

	<i>Dependent variable:</i>		
	Hired x 100		
	(1)	(2)	(3)
Algo Writing Treatment (Trt)	0.247*** (0.080)	0.223** (0.088)	0.242*** (0.086)
Anglophone Country		2.508*** (0.146)	
Trt × Anglo		0.155 (0.207)	
US			2.602*** (0.161)
Trt × US			0.072 (0.228)
Constant	3.093*** (0.057)	2.632*** (0.063)	2.719*** (0.061)
Observations	194,700	194,700	194,700
R <sup>2</sup>	0.00005	0.003	0.003

*Notes:* This table analyzes the effect of the treatment on whether or not a jobseeker was ever hired on the platform in the month after they joined, times 100. In Column (1) we show the overall effect of the treatment to hiring. In Column (2) we interact the treatment with a dummy variable for if the jobseeker is from the US, UK, Canada, or Australia. In Column (3) we interact the treatment with a dummy for if the jobseeker is in the US. The experimental sample is of all new jobseekers who registered and were approved by the platform between June 8th and July 14th, 2021 and had non-empty resumes. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Figure 2: Robustness tests for total earnings treatment effect



Notes: This plot shows the effect of the treatment on the total earnings on all contracts from the workers first month on the platform. We report results from specifications meant to deal with the fact that most of the observations are zeros. The first specification is an OLS regression where the outcome is  $\log(1+ \text{earnings})$ , the second specification is OLS where the earnings variable has had an Inverse Hyperbolic Sine transformation. The third is a quasi-poisson generalized linear model. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021, and had non-empty resumes, with  $N = 194,700$ .

Table A15: Effect of algorithmic writing assistance on workers' public ratings

	<i>Dependent variable:</i>					
	Communication (1)	Skills (2)	Quality (3)	Cooperation (4)	Deadlines (5)	Received public rating (6)
Algo Writing Treatment	-0.008 (0.021)	-0.005 (0.021)	0.004 (0.023)	-0.008 (0.020)	-0.009 (0.022)	0.002 (0.012)
Constant	4.811*** (0.015)	4.801*** (0.015)	4.768*** (0.016)	4.840*** (0.015)	4.803*** (0.016)	0.534*** (0.009)
Observations	3,745	3,745	3,745	3,745	3,745	6,263
R <sup>2</sup>	0.00004	0.00002	0.00001	0.00004	0.00004	0.00001

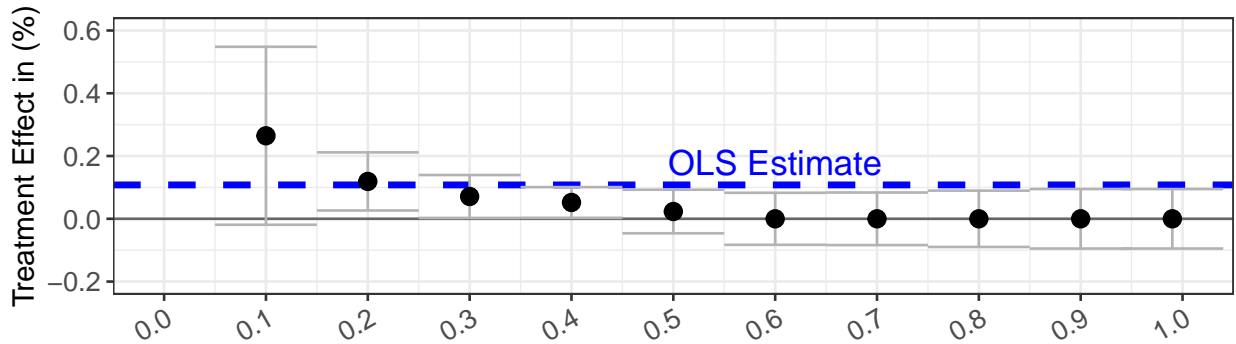
Notes: This analysis looks at the effect of treatment on the average public ratings of contracts for jobseekers in the experimental sample. All ratings are on a scale of 1 to 5 and averaged at the worker level. In Column (1) the outcome is the rating the employer gives to the workers' communication ability. In Column (2) the outcome is the rating the employer gives to the workers' skills. In Column (3) the outcome is the rating the employer gives to the overall quality of work completed. In Column (4) the outcome is the rating the employer gives to the workers' cooperation. In Column (5) the outcome is the rating the employer gives to the workers' ability to make deadlines. In Column (6) the outcome is the percentage of jobs worked where the worker was left any public rating by their employer. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021 and had non-empty resumes. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Table A16: Effects of writing assistance on private ratings and reviews

	<i>Dependent variable:</i>					
	Private rating			Positive text review		
	(1)	(2)	(3)	(4)	(5)	(6)
Algo Writing Treatment (Trt)	-0.076 (0.082)	-0.132 (0.098)	-0.126 (0.094)	0.016 (0.019)	0.015 (0.022)	0.014 (0.021)
Anglophone Country		0.481*** (0.126)			0.014 (0.033)	
Trt × Anglo		0.213 (0.177)			0.003 (0.044)	
US			0.516*** (0.135)			-0.002 (0.036)
Trt × US			0.244 (0.190)			0.009 (0.049)
Constant	8.631*** (0.059)	8.480*** (0.071)	8.502*** (0.068)	0.858*** (0.014)	0.854*** (0.016)	0.858*** (0.016)
Observations	4,318	4,318	4,318	1,189	1,189	1,189
R <sup>2</sup>	0.0002	0.011	0.011	0.001	0.001	0.001

*Notes:* In this table we report the effect of the treatment to two measures of worker ratings. In Columns (1) through (3) the outcome is jobseekers average private ratings. In Columns (4) through (6) the outcome is the average percent of jobseekers reviews flagged as having a positive or neutral sentiment. In Column (2) and (5) we interact the treatment with a dummy variable for if the jobseeker is from the US, UK, Canada, or Australia. In Column (3) and we interact the treatment with a dummy for if the jobseeker is in the US. The experimental sample is of all new jobseekers who registered and were approved by the platform between June 8th and July 14th, 2021 and had non-empty resumes. Significance indicators:  $p \leq 0.10$  : \*,  $p \leq 0.05$  : \*\* and  $p \leq .01$  : \*\*\*.

Figure 3: Effect of treatment on percentage of words spelled correctly, by deciles



*Notes:* This plot shows the effect of the treatment on the percentage of words spelled correctly in jobseekers' resumes, by deciles. The experimental sample is of all new jobseekers who registered and were approved for the platform between June 8th and July 14th, 2021, and had non-empty resumes, with  $N = 194,700$ .