Time Is of The Essence: Experimental Evidence on The Effects of Job Application Timing

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Abstract

Leveraging a nationwide correspondence experiment that sent over 83,000 fictitious applications to large US firms, I provide novel evidence on the causal effects of job application timing. Specifically, I uncover a significant "early application advantage": the early half of applications submitted to a job in the experiment are 1.2 percentage points more likely to be contacted, an effect size equivalent to 57% of the Black-White contact gap observed in the same context. Furthermore, each day earlier an application is submitted increases the likelihood of contact by 0.4 percentage points. This advantage is driven by recruiters' evaluate-while-searching strategy, wherein early applicants are contacted and evaluated while later applications continue to be accepted. By constructing the contact history prior to an application, I uncover two competing mechanisms, Quota and Learning, and demonstrate how they jointly shape recruiters' decision-making dynamics. Together, these findings add a new dimension to our understanding of labor market discrimination, highlighting the importance of considering the timing aspect in further research and policy discussions.

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1 Introduction

Labor market discrimination persists despite extensive legislative efforts aimed at its eradication. Through the approach of resume correspondence experiments, researchers have documented robust evidence of employers' discriminatory biases across various contexts (Bertrand and Duflo, 2017; Quillian et al., 2017; Baert, 2018; Gaddis, 2018; Neumark, 2018; Gaddis and Crabtree, 2021). However, the timing of job applications, a critical aspect of the job search and hiring process, remains largely unexplored. This paper provides novel causal evidence on employers' preference for early applications. Leveraging a massive nationwide resume correspondence experiment targeting large US firms, I uncover a previously overlooked "early application advantage" and demonstrate the underlying mechanisms.

The experiment, conducted by Kline, Rose and Walters (2022), offers a unique context for understanding the effects of application timing compared to other resume correspondence studies. Up to 125 entry-level jobs in distinct US counties were sampled for each of 108 Fortune 500 firms. Eight fictitious resumes were submitted sequentially for the majority of sampled positions, providing substantial variation in application timing. With over 83,000 applications in total, this experiment is the largest of its kind, granting uniquely precise statistical inference. The comprehensive set of resume characteristics, including signals of applicants' sex, race, age, sexual orientation, gender identity, and political leaning, allows for a nuanced understanding of the heterogeneity in application timing effects across different types of applicants. Overall, 24% of the applications were contacted by employers within 30 days, validating the design the fictitious resumes.

The first main finding of this paper is the significance of the early application advantage within the context of the experiment. Controlling for all resume characteristics, the first four fictitious applications submitted out of the eight for each job are 1.2 percentage points more likely to be contacted, an effect size equivalent to 57% of the observed Black-White contact gap (2.1 p.p.). Additionally, the early application advantage exhibits a linear trend with respect to the number of days an application is submitted earlier than the last one in the experiment: each day earlier increases the likelihood of being contacted by 0.4 percentage points. This early application advantage is remarkably robust across various specifications and different methods of constructing the early application measures.

Second, I demonstrate that the early application advantage largely stems from recruiters contacting qualified applicants while still accepting late applications. The constructed contact history within jobs prior to a submission negatively predicts the probability of contact for that submission: each additional prior application contacted reduces the probability of contact for the current application by 4.5 to 5.9 percentage points, depending on the method of contact history construction. This suggests that recruiters in the experiment likely adopt an "evaluate while searching" strategy, where they contact and evaluate qualified early candidates while still accepting later applications, rather than setting a firm deadline and evaluating all applications together.

The third main finding addresses the intricate interplay of application timing and two widely studied discriminatory factors: race and sex. The early application advantage is smaller for Black applicants compared to White applicants, while it is larger for female applicants relative to male applicants. Consistent with the differences in the early application advantage, additional previous applicant contacted reduces the contact probability for Black applicants by 1.8 percentage points more than for their White counterparts. Conversely, female applicants' contact probability is reduced marginally less than that of male applicants. Using group-specific contact histories, I demonstrate that these results can be explained by two competing mechanisms in recruiter behavior: Quota, where previous contacts of applicants from a minority group reduce the contact probability of subsequent applicants from the same group; and Learning, where previous contacts of minority applicants correct bias and increase the contact probability for subsequent applicants from the same group. The results suggest that the Quota mechanism dominates for Black applicants, while the Learning mechanism is more predominant for female applicants, adding another dimension to the discrimination based on protected characteristics in the hiring process.

Lastly, I leverage substantial industry, geographic, and firm variation to explore the heterogeneity of the observed early application advantage. This advantage is present across all industries, ranging from nearly null in the wholesale trade industry to over 5 percentage points in the transportation/warehousing industry. Two significant negative predictors of the early application advantage are the employment size of an industry and the importance of analytical skills. The heterogeneity of the early application advantage across states is even more pronounced, varying

from a 3 percentage point disadvantage to over a 10 percentage point advantage. A higher state-level wage-to-income ratio for median households significantly predicts a smaller early application advantage. Another state-level negative predictor is labor market tightness, measured as the unemployed persons per job opening ratio. This suggests that the effects of application timing are embedded in the interplay of labor market competition on both the supply (job seekers) side and the demand (firms) side in equilibrium. Firm-level results are consistent with this narrative: the labor market power of firms, proxied by the share of job postings from a firm in the local labor market, also negatively predicts the size of the early application advantage. In addition, human resource management practices and financial performance of firms explain some of the variation in the early application advantage.

Contribution to Literature This paper contributes to two strands of literature.

The first is on labor market discrimination. Robust evidence has been established for the existence and significance of various discriminatory factors in the labor market, including gender (Neumark, Bank and Van Nort, 1996), race (Bertrand and Mullainathan, 2004; Kline, Rose and Walters, 2022), ethnicity (Lippens et al., 2023), education background (Deming et al., 2016), immigration experience (Oreopoulos, 2011), unemployment history (Kroft, Lange and Notowidigdo, 2013; Farber et al., 2019), criminal records (Agan and Starr, 2018), soft skills (Valencia et al., 2024), etc. However, to the best of my knowledge, no experimental studies have focused on the effects of application timing, potentially due to the large sample size required for substantial time variation. This paper, paralleling recent work on offer acceptance timing (Cortés et al., 2023), fills the gap by demonstrating the significance of job application timing, both on its own and in its interplay with discriminatory factors, adding another dimension to our understanding of discrimination in the labor market. The early application advantage uncovered in this paper also helps reconcile the disparity between null effects in resume correspondence experiments (e.g., the null gender effect in Kline, Rose and Walters, 2022) and the observed discrimination in the field.

Second, this paper contributes to the literature on belief-based discrimination and firm hiring practices. A growing body of work studies how potentially biased beliefs drives discrimination (Bordalo et al., 2016; Bohren, Imas and Rosenberg,

2019; Bohren et al., 2023; Coffman, Exley and Niederle, 2021), and how information can help correct such bias (Vuletich and Payne, 2019; Alesina et al., 2024). Some recent work focuses on the endogenous acquisition of information. Cavounidis, Lang and Weinstein (2024) find that employers are more likely to monitor and then punish Black employees; Eyting (2022) shows in a series of online experiments that individuals incline to acquire information matched their motivation to justify discriminatory behavior; Coffman, Kostyshak and Saygin (2024) shows that endogenous information acquisition leads to under-acquisition and misevaluation in a stylized hiring experiment. Using field data in the financial advisory industry, Egan, Matvos and Seru (2022) show female advisers are more severely punished for misconduct. Leveraging the large-scale lab-in-the-field resume correspondence experiment, this paper provides novel evidence on the belief updating of recruiters and its interaction with the competing Quota mechanism in the real-world hiring decision-making process.

Structure of the Paper The rest of the paper is organized as follows: In Section 2, I describe the correspondence experiment and complementary labor market data; Section 3 discusses the empirical strategy and the construction of application timing indicators; Section 4 presents the causal evidence on the impact of application timing; Section 5 elaborates the potential mechanisms of the observed application timing effects; Section 6 discuss the industry, state and firm level heterogeneity of the application timing effects; Section 7 concludes.

2 The Experiment and Data

In this section, I briefly summarize the key elements of the resume correspondence experiment, and complementary labor market data.

2.1 Experiment

The main analysis of this paper is based on the large-scale resume correspondence experiment conducted by Kline, Rose and Walters (2022). The sampling frame started with the 2018 Fortune 500 firm list. To ensure the feasibility of audit methods, 108 companies of separate brands with distinct hiring websites and sys-

tems were selected due to sufficient geographic variation and consistently posted enough entry-level jobs. These firms spanned a sufficiently wide variety of industries industries and collectively employed around 15 million workers in 2020.

From each employer, 125 entry-level vacancies were sampled over five waves, with 25 jobs per wave, from October 2019 to April 2021. Each vacancy corresponded to an establishment in a distinct U.S. county. A majority of 72 firms were included in all waves. Some firms were excluded from the first wave due to COVID-19 disruptions, while others were excluded from the last wave due to technological barriers. The order of firms was randomized at the beginning of each wave.

For each sampled firm, all vacancies were scraped from its proprietary hiring portal every day. The focus was on entry-level jobs that did not require a bachelor's or advanced degree, substantial prior experience, or a specialized license. The *RandRes* platform monitored the scraped jobs, adding 5 to 10 jobs for each firm at a time to match the maximum submission capacity.

For each sampled vacancy, eight applications with randomized characteristics were submitted in four pairs, each pair consisting of one Black applicant and one White applicant. Some vacancies stopped accepting applications before all eight submissions were completed. The final sample included approximately 84,000 applications for 11,000 jobs. For each vacancy, it typically took no more than eight days to submit all eight applications (94% of all), with a gap of one to two days between consecutive pairs. This design introduced a uniquely rich variety of application timings within each job.

Following previous studies (Bertrand and Duflo, 2017), the experiment signaled race and gender with disinctive names. The initial set included nine unique first names for each race and gender group, as proposed by Bertrand and Mullainathan (2004), supplemented with 10 additional first names per group from a database of speeding tickets issued in North Carolina between 2006 and 2018. A name was classified as racially distinctive if over 90% of individuals with that name belonged to a particular race. The most common distinctive Black and White names for those born between 1974 and 1979 were selected. Distinctive last names were chosen from those occuring at least 10,000 times in the 2010 U.S. Census with high race-sepcific shares. This process generated 500 unique full names for each race and gender group. Within each pair, one application was randomly assigned,

without replacement, a distinctively Black name white the other assigned a distinctively White name. 50% of all applications were randomly assigned a distinctively female name without stratification, resulting in the number of applications with distinctively female names per job varying from 0 to 8.

Three additional potential discriminatory factors were considered: applicants were randomly assigned an age from a uniform distribution over the range from 22 to 58 years old; 10% of applicants were randomly assigned to list LGBTQ high school clubs on the resumes to imply their sexual orientations; Another 10% of applications were assigned to list pronouns on resumes, with half using gender-neutral pronouns (they/them) and the other half using gender-typical pronouns.

Additionally, a large set of supplementary characteristics were randomly assigned to each application, including whether the applicant had an associate degree (50%) instead of a high school diploma, and employment histories comprising two or three jobs with relevant experiences.

As in most correspondence experiments, the primary outcome was whether an employer attempted to contact the applicant. The main analysis focuses on the 30-day period post application. Each applicant was assigned a unique phone number and email address, both of which were monitored to record any interview requests from employers. More details of the experiment design are available in Kline, Rose and Walters (2022).

2.2 Complementary Data

To provide further insights into how the findings relate to various conditions in the labor market, I collect the following supportive data from various sources.

First, I collect industry-level information on firms. This includes measures of required skills, typical tasks, and job conditions, constructed from the characteristics of typical occupations within each industry. Additionally, I gather monthly data on employment size and compensation conditions for each industry to capture the dynamic nature of these industries over time.

Second, I compile detailed state-level labor market information. Leveraging newly available granular income data, I construct measures of income levels, wage levels, and income mobility for each state. I also collect historical minimum wage data to reflect wage regulations for each state over time. Furthermore, I include

state-by-month indicators of labor market conditions, such as employment, hirings, separations, and tightness, to provide a nuanced view of local labor market dynamics.

Lastly, I assemble a set of firm-level information, including labor law violations, human resource management practices, employee reviews, and financial performance. For firms sampled in Wave 1, I also utilize job posting data to create a measure of hiring shares for each firm in local labor markets.

These diverse datasets support the heterogeneity analysis of the application timing effects across different contexts. Section B in the appendix provides a detailed account of the gathering and construction of the full list of covariates.

3 Specification

In this section, I describe the construction of the application timing measures and outline the empirical strategies.

3.1 Measuring Application Timing

Order of submissions The submission date and time were recorded for each fictitious application to construct the order of submission. Of the 11,114 jobs in the full sample, 9,829 (88.4%) received eight applications each (*Complete* hereafter), totaling 78,632 submissions (94.0% of all submissions). In Column (2) of Table 1, I summarize the submission order for jobs with less than 8 submissions (*Incomplete* hereafter).

As described in Section 2.1, applications were submitted in pairs by design. Therefore, I also consider the designed order of pairs as an alternative measure of submission order. Table A.1 compares the submission order with the pair order. Due to technical barriers, a small number of applications were submitted later than planned, resulting in negligible inconsistencies between constructed submission orders and designed pair orders.

Early submission indicators Using the two orders described above, I construct three indicators for early submissions.

Table 1: Order of Submissions

Order	(1) Full	(2) Full (< 8)	(3) Wave 1	(4) Wave 2
1	11114	1285	2063	9051
2	11031	1202	2040	8991
3	10682	853	1883	8799
4	10606	777	1879	8727
5	10278	449	1748	8530
6	10204	375	1734	8470
7	9899	70	1604	8295
8	9829	0	1590	8239
N applications	83643	5011	14541	69102
N jobs	11114	1285	2063	9051
N firms	108		85	

Note: This table summarizes the number of applications ordered from 1 to 8. *Full* refers to full sample, *Full* (< 8) refers to the jobs with less than 8 submissions in the full sample, *Wave 1* refers to the subsample of Wave 1, *Wave 2-5* refers to the subsample of Wave 2-5.

- $early^1$: The main early submission indicator is defined such that the first four submissions to a job are coded as early if there are more than four submissions. For jobs with no more than four submissions (836 jobs, 7.52%; 2321 submissions, 2.77%), all submissions are coded as late. In full sample, 41112 submissions (49.15%) are coded as early submissions.

- $early^2$: The first alternative early submission indicator, mirroring $early^1$, is defined such that the last four submissions to a job are coded as late. For jobs with no more than four submissions, all submissions are coded as late. In full sample, 40210 (48.07%) are coded as early submissions.

- $early^3$: The second alternative early submission indicator, based on the designed pair orders, is defined such that the last two pairs of submissions to a job are coded as late. For jobs with no more than two pairs of submissions (836 jobs, 7.52%; 2321 submissions, 2.77%), all submissions are coded as late. In full sample, 40354 submissions (in 20177 pairs, 48.25%) are coded as early submissions.

Table A.2 summarizes the three constructed early submission indicators, show-

ing that they are largely consistent with each other. Going forward, I report results using the primary indicator $early^1$ for the main analysis, and report results with $early^2$ and $early^3$ for robustness.

Days to the last submission In addition to orders and early indicators dummies, I use the submission date to construct a continuous measure $D2L$, representing the number of days between each submission and the last submission to a job. As shown in Figure A.1, the distribution of this variable is heavily right-skewed, with 99.3% of all submissions having no more than 10 days' separation from the last submission for each job. When used for analysis, I winsorize $D2L$ at the top 1% level to adjust for this skewness.

By design, the days to the last submission should be at most 6, as the 8 applications for each job were planned to be submitted in 4 pairs either every other day or daily. Therefore, I regroup all observations with $D2L$ larger than 6 and code them as 7, creating the measure used in the main analysis $D2L^t$. Figure A.2 displays its distribution.

One should be cautious about the differences in the distribution of $D2L^t(D2L)$ between Wave 1 and Waves 2-5: In Wave 1, each pair of applications was planned to be submitted every other day, while in Waves 2-5, pairs were planned to be submitted daily. Specifically, in Wave 1, around 95.6% of all applications were submitted no more than 8 days before the last submission, whereas in Waves 2-5, around 96.6% were submitted no more than 4 days before the last submission. Although the full sample is used in the main analysis, I report results separately for Wave 1 and Waves 2-5 for robustness.

Figure A.3 plots the distribution of $D2L^t$ by submission order to cross-validate the two measures. Two key observations can be made: First, the submissions, despite technical barriers, submissions largely align with the planned pair orders. Submissions in the same pair have very similar distributions of $D2L^t$. Second, when comparing the *Complete* with the Incomplete jobs, we see a shift in the distribution for applications of the same pair order towards a concentration around zero for *Incomplete* jobs This suggests that the early closing of a vacancy, resulting in a certain order of submission being the last, is rather random with respect to the submission of fictitious applications.

3.2 Empirical Strategies

The main analysis aims to provide causal evidence on whether applying to a job earlier affects the probability of being contacted for interviews, which is the first step towards employment, and if so, to what extent. To do so, I first estimate, for fictitious applicant i

$$Y_i = \alpha_0 + \beta \cdot early_i^* + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \tag{3.1}$$

where the outcome Y_i is whether applicant i is contacted within 30 days, $early_i^*$ is one of the three constructed early submission indicators, taking the value of 1 if an application is classified as early, and $\mathbf{X}_i$ is the full set of characteristics, including race and gender, the two main discriminatory factors examined in the literature.

To examine the application timing effect in greater details, I further estimate

$$Y_i = \alpha_0 + \sum_{j=2}^{8} \beta_j \cdot \mathbf{1}\left(Order_i = j\right) + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \tag{3.2}$$

where $\mathbf{1}\left(Order_i = j\right)$ is the order indicator that takes the value 1 if fictitious applicant i is the $j-$th submission to a job[1]. The coefficient β_j can be interpreted as the contact gap between the $j-$th submission and the first submission, conditional on controlled characteristics.

I also use the constructed continuous measure $D2L^t$ as a regressor and estimate

$$Y_i = \alpha_0 + \delta \cdot D2L_i^t + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \tag{3.4}$$

where the coefficient δ represents how much the contact gap changes in response to one additional day before the last submission.

Model 3.4 assumes a linear impact of $D2L^t$ on the probability of contact, which might be overly simplistic. Therefore, I also estimate a model with indicators for

[1]When considering pairs, I instead estimate

$$Y_i = \alpha_0 + \sum_{j=2}^{4} \beta_j \cdot \mathbf{1}\left(Order_i^{pair} = j\right) + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \tag{3.3}$$

where $\mathbf{1}\left(Order_i^{pair} = j\right)$ is the pair order indicator, taking the value 1 if ficticious applicant i is designed to be in the $j-$th pair of submission to a job.

each value of $D2L^t$:

$$Y_i = \alpha_0 + \sum_{d=1}^{7} \delta_d \cdot \mathbf{1}\left(D2L_i^t = d\right) + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \tag{3.5}$$

the coefficients $\{\delta_d\}_{d=1,\cdots,7}$ then provide a non-parametric function showing how the contact probability changes over time.

For the main analysis, I focus on the within-job effect of application timing by including job fixed effects in all models outlined above. Standard errors are clustered at the job level to account for potential heterogeneity in treatment effects (Abadie et al., 2023). For robustness, I also report two alternative specifications for the main results: (a) with job fixed effects and robust standard errors, and (b) with region and wave fixed effects and job-level clustered standard errors, as in Kline, Rose and Walters (2022).

Before presenting the main results, I first check the balance of the constructed timing measures with respect to the resume characteristics $\mathbf{X}$. Table A.3 shows that in all three specifications, the constructed timing measures are well randomized.

4 Main Results

In this section, I discuss the main findings on the application timing effect, employing the empirical strategy outlined in Section 3. Section 4.1 presents the main estimation results with the three groups of constructed application timing measures: early submission indicators, submission orders (and designed order of pairs), and $D2L^t$, days to the last submission. Section 4.2 discusses how the application timing effect interacts with various resume characteristics.

4.1 Application Timing

Early submission indicators First, I estimate Model 3.1 to test whether a relatively early submission increases the likelihood of being contacted. The results with job fixed effects and job-level clustered standard errors are reported in Table 2. For the full sample, early submissions are significantly more likely to be contacted by 1.1 to 1.2 percentage points across three different early submission dummies. This difference is economically significant, compared to the 2.1 percent-

age points Black-White contact gap. Regarding resume characteristics, consistent with Kline, Rose and Walters (2022), Black applicants and applicants over 40 experience significant contact penalties. Listing pronouns, whether gender-typical or gender-neutral, leads to a marginally significant lower contact rate. Other characteristics, including gender (female versus male), do not have a significant impact in this context.

When focusing on the *Incomplete* jobs, the early application advantage becomes marginally significant. This change from the full sample results can potentially be attributed to the fact that a large number of jobs received fewer than five fictitious applications, meaning all received submissions are coded as late, and to the much smaller sample size ($N = 5011$). The estimated coefficients of the resume characteristics also change significantly, supporting such speculations. An important observation is that the constant for the *Incomplete* job sub-sample is significantly lower than that of the full sample, further supporting the existence of the early application advantage: Submissions to the *Incomplete* jobs can be considered inherently late, as they occur near the closing of these vacancies.

In Table A.4 and Table A.5, I report the estimation results of the two alternative specifications. In the specification with robust standard errors and job fixed effects, the standard errors are smaller, resulting in less conservative confidence intervals compared to the main specification. In the specification with region and wave fixed effects and job-level clustered standard errors, the results are largely consistent with the main findings.

Order of submissions The results so far indicate a significant early application advantage in this experimental setting, as early applications are more likely to be contacted, conditional on comparable resume characteristics. Next, I leverage the large number of submissions for each job to further explore the effect of the relative timing of applications. Specifically, I estimate Model 3.2 job fixed effects and cluster the standard errors at the job level.

Figure 1 plots the estimated coefficients of interest $\hat{\beta}_j$. It is shown that within a job sampled in the experiment, the first submission is always more likely to be contacted compared to later submissions. The order effect is weakly monotonic, meaning a later submission is not more likely to be contacted than an earlier one, based on submission order. Another important observation is that submissions of

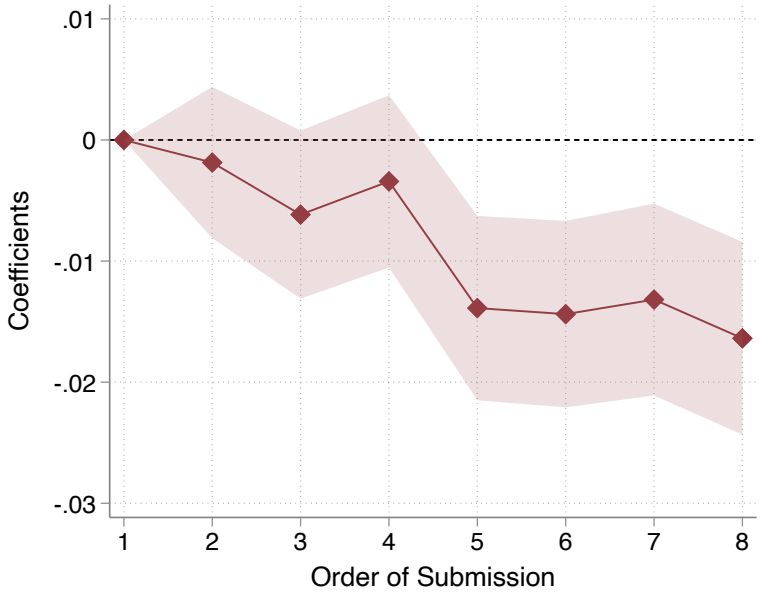
Table 2: Estimation of Model 3.1, Main Specification

	(1) Full	(2) < 8	(3) Full	(4) < 8	(5) Full	(6) < 8
$early^1$	0.0116	0.0240				
	(0.0024)	(0.0127)				
$early^2$			0.0113	0.0135		
			(0.0023)	(0.0118)		
$early^3$					0.0105	0.0121
					(0.0023)	(0.0119)
Black	-0.0205	-0.0125	-0.0205	-0.0124	-0.0205	-0.0125
	(0.0018)	(0.0066)	(0.0018)	(0.0066)	(0.0018)	(0.0066)
Female	-0.0021	-0.0006	-0.0021	-0.0009	-0.0021	-0.0008
	(0.0022)	(0.0080)	(0.0022)	(0.0081)	(0.0022)	(0.0081)
Over 40	-0.0043	0.0041	-0.0043	0.0038	-0.0043	0.0040
	(0.0020)	(0.0083)	(0.0020)	(0.0083)	(0.0020)	(0.0083)
Pol. club	0.0059	0.0037	0.0059	0.0021	0.0059	0.0020
	(0.0051)	(0.0251)	(0.0051)	(0.0252)	(0.0051)	(0.0252)
Acad. club	0.0049	0.0136	0.0049	0.0135	0.0049	0.0140
	(0.0051)	(0.0175)	(0.0051)	(0.0174)	(0.0051)	(0.0175)
LGBTQ club	-0.0003	0.0152	-0.0003	0.0152	-0.0003	0.0154
	(0.0036)	(0.0171)	(0.0036)	(0.0171)	(0.0036)	(0.0171)
Typical pron.	-0.0077	0.0056	-0.0077	0.0042	-0.0077	0.0043
	(0.0051)	(0.0203)	(0.0051)	(0.0201)	(0.0051)	(0.0201)
Neutral pron.	-0.0086	0.0065	-0.0086	0.0068	-0.0085	0.0069
	(0.0051)	(0.0217)	(0.0051)	(0.0216)	(0.0051)	(0.0216)
Assoc. degree	0.0021	0.0082	0.0021	0.0080	0.0021	0.0080
	(0.0020)	(0.0085)	(0.0020)	(0.0085)	(0.0020)	(0.0085)
Constant	0.2475	0.1680	0.2477	0.1746	0.2481	0.1744
	(0.0025)	(0.0097)	(0.0024)	(0.0085)	(0.0024)	(0.0088)
N	83643	5011	83643	5011	83643	5011

Note: This table presents the effects of early submission and other characteristics on the probability of employer contact within 30 days. Columns titled "Full" include all jobs, while columns titled "<8" include jobs with less than 8 submissions. All regressions include job fixed effects. Standard errors in parentheses are clustered at the job level.

Order 3 and 4 experience a small late penalty compared to those of Order 1 and 2. Meanwhile, there is a large decrease (around 1 percentage point) in the contact rate for submissions of Order 5 compared to Order 4. After this significant drop, the contact rate remains stable for subsequent submissions.

Figure 1: Contact Gap and Order of Submission



Notes: The figure plots the estimated coefficients $\hat{\beta}_j, j \in \{2, \cdots, 8\}$ of Model 3.2 with the order of submission on the x-axis, controlling for resume characteristics. The regression includes job fixed effects, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals. Sample: full sample ($N = 83643$).

In Figure A.4, I re-estimate Model 3.2 with region and wave fixed effects. The results are very consistent with the main estimations. In Figure A.5, I employ the same within-job specification as in Figure 1 for *Complete* jobs and *Incomplete* jobs separately. Since 94% of submissions come from *Complete* jobs, the results on *Complete* job sub-sample, shown in Panel (a) of A.5, are nearly identical to the full-sample results. For the *Incomplete* job subsample, shown in Panel (b) of Figure A.5, the within-job early application advantage remains significant and even larger in scale, but the results are noisy due to the much smaller sample size.

As discussed above, the realized order of submissions does not perfectly align with the designed order due to technical issues. Since the race of the fictitious applicants is pair-level stratified by design, I estimate Model 3.3 with job fixed effects in Table 3 and with wave and region fixed effects in Table A.6, both controlling for the full set of resume characteristics. The results are highly consistent with those using the order of submissions.

Estimating the effects of the order of submissions reaffirms the significance of the early application advantage. It also further validates the constructed early sub-

Table 3: Estimation of Model 3.3, w. Job Fixed Effects

	(1) Full	(2) $= 8$	(3) < 8
$\mathbf{1}(Order_i^{pair} = 2)$	-0.0036	-0.0021	-0.0211
	(0.0027)	(0.0028)	(0.0091)
	[0.0024]	[0.0025]	[0.0074]
$\mathbf{1}(Order_i^{pair} = 3)$	-0.0126	-0.0113	-0.0327
	(0.0031)	(0.0032)	(0.0141)
	[0.0025]	[0.0025]	[0.0105]
$\mathbf{1}(Order_i^{pair} = 4)$	-0.0127	-0.0116	-0.0572
	(0.0032)	(0.0033)	(0.0346)
	[0.0025]	[0.0026]	[0.0314]
Constant	0.2601	0.2641	0.1901
	(0.0028)	(0.0030)	(0.0090)
	[0.0026]	[0.0027]	[0.0086]
N	83643	78632	5011

Note: This table presents the effects of the order of submission pairs on the probability of employer contact within 30 days. The column titled "Full" includes all jobs, the column titled "=8" includes all *Complete* jobs with 8 submissions, the column titled "<8" includes only *Incomplete* jobs with less than 8 submissions. All regressions include the full set of resume characteristics and job fixed effects. Standard errors clustered at the job level are reported in parentheses, robust standard errors are reported in brackets.

mission indicators $early^*$: The estimated gap is largely attributed to the difference in the contact rate between the first four submissions (mostly coded as early) and the last four submissions (mostly coded as late).

Days to the last submission Thus far, I have provided causal evidence of the existence and magnitude of the contact reward for early submissions using measures of relative application timing. Next, I use the number of days between a submission and the last submission for the same job $D2L^t$ to quantify the early application advantage by the number of days.

First, I estimate Model 3.4, which uses the timing measure $D2L^t$ as a continuous regressor. The estimation results controlling for job fixed effects are summarized in Table 4. Column (1) reports the estimation results for the full sample: compared to the last application submitted, submitting one day earlier increases the contact rate by 0.40 percentage points, conditional on resume characteristics. Given that

the regressor measures the number of days between an application and the last application for the same job, there may be concerns about the *Incomplete* jobs, where the last actual submissions were not the planned last submissions due to early closing. Column (2) of Table 4 shows that the main result still holds for *Incomplete* jobs: submitting one day earlier than the last successful submission for a job increases the contact rate by 0.87 percentage points.

Another concern might be the difference in submission timing design between Wave 1 and Wave 2 to 5, as discussed in Section 3.1. To address this, I re-run the estimation on the subsamples of Wave 1 and Wave 2 to 5 separately. As summarized in Column (3) to (6) of Table 4, the full-sample results hold robustly in both subsamples. The estimated effect of submitting one day earlier than the last submission to a job is 0.36 percentage points in Wave 1 and 0.43 percentage points in Wave 2 to 5. This difference is naturally linked to the fact that fictitious applications after Wave 1 were submitted every day instead of every other day, making the average $D2L^t$ bigger in Wave 1 (see Figure A.2).

As discussed in Section 3.1, the measure $D2L^t$ is constructed such that all submissions more than 6 days earlier than the last submission are grouped together and coded as 7. One might worry that this regrouping could bias the results. To address this concern, I reestimate Model 3.4 directly using the raw measure $D2L$ winsorized at the top 1% level to adjust for the right-skewness.. The results, shown in Table A.7, are virtually the same as those presented in Table 4. This is not surprising, since only a negligible number of applications (2.35%) were submitted more than 6 days prior to the last submission. Employing the specification with region and wave fixed effects does not change the conclusion (Table A.8).

Using days to the last submission as a continuous measure, I further demonstrate the existence of the early application advantage. One limitation of the results discussed above is the linearity assumption made in Model 3.4. To address this, I estimate the nonparametric Model 3.5 and plot the estimated coefficients $\hat{\delta}_d, d \in \{1, \cdots, 7\}$. This approach provides a comprehensive view of how the contact rate evolves over the application timeline, allowing for a detailed examination of the relationship between submission timing and contact probability.

Figure 2 shows the full sample results. It appears that the linearity assumption may not be a poor choice: the earlier an application is submitted, as measured by a large number of the days to the last submission, the more likely it is to be con-

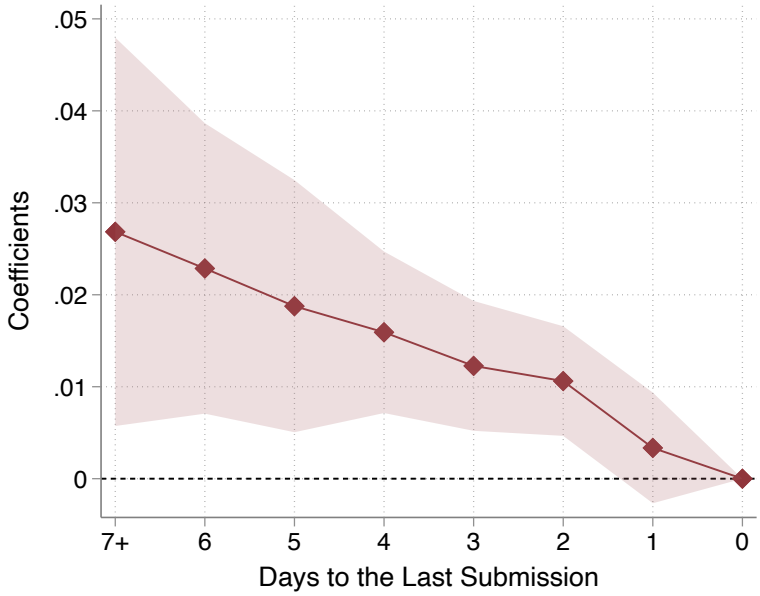
Table 4: Estimation of Model 3.4, w. Job Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Wave 1		Wave 2-5	
	Full	< 8	Full	< 8	Full	< 8
$D2L^t$	0.0040	0.0087	0.0036	0.0083	0.0043	0.0093
	(0.0008)	(0.0035)	(0.0013)	(0.0042)	(0.0010)	(0.0064)
Black	-0.0205	-0.0125	-0.0205	-0.0112	-0.0205	-0.0132
	(0.0018)	(0.0066)	(0.0041)	(0.0113)	(0.0020)	(0.0080)
Female	-0.0021	-0.0007	0.0013	-0.0045	-0.0028	0.0014
	(0.0022)	(0.0080)	(0.0051)	(0.0131)	(0.0024)	(0.0102)
Over 40	-0.0043	0.0034	-0.0041	0.0035	-0.0043	0.0035
	(0.0020)	(0.0083)	(0.0048)	(0.0147)	(0.0022)	(0.0100)
Pol. club	0.0060	0.0026			0.0060	0.0026
	(0.0051)	(0.0253)			(0.0051)	(0.0252)
Acad. club	0.0050	0.0136			0.0050	0.0136
	(0.0051)	(0.0174)			(0.0051)	(0.0173)
LGBTQ club	-0.0002	0.0154			-0.0002	0.0153
	(0.0036)	(0.0171)			(0.0036)	(0.0171)
Typical pron.	-0.0078	0.0040			-0.0078	0.0040
	(0.0051)	(0.0202)			(0.0051)	(0.0202)
Neutral pron.	-0.0085	0.0061			-0.0085	0.0059
	(0.0051)	(0.0216)			(0.0050)	(0.0216)
Assoc. degree	0.0021	0.0083	-0.0013	0.0037	0.0028	0.0108
	(0.0020)	(0.0085)	(0.0050)	(0.0150)	(0.0022)	(0.0103)
Constant	0.2453	0.1668	0.1930	0.1744	0.2561	0.1623
	(0.0027)	(0.0093)	(0.0062)	(0.0150)	(0.0030)	(0.0121)
N	83643	5011	14541	1821	69102	3190

Note: This table presents the effects of early submission measured by days before the last submission and other characteristics on the probability of employer contact within 30 days. Columns titled "Full" include all jobs, while columns titled "<8" include jobs with less than 8 submissions. Column (1) and (2) are on full sample, Column (3) and (4) are on Wave 1 where the variables "Pol. club", "Acad. club", "LGBTQ club", "Typical pron." and "Neutral pron." were not designed to vary, Column (5) and (6) are on Wave 2-5. All regressions include job fixed effects. Standard errors in parentheses are clustered at the job level.

tacted. The larger confidence intervals for the indicators of more than 4 days earlier reflect the smaller sample size. When examining Wave 1 and Waves 2 to 5 separately in Figure A.6, the early application advantage remains pronounced. The effects are mostly monotonic in $D2L^t$, but the submissions made 3 days prior to

Figure 2: Contact Gap and Days to the Last Submission



Notes: The figure plots the estimated coefficients $\hat{\delta}_d, d \in \{1, \cdots, 7\}$ of Model 3.5 with days to the last submission $D2L^t$ on the x-axis, controlling for resume characteristics. The regression includes job fixed effects, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals. Sample: full sample ($N = 83643$).

the last one have a higher-than-trend probability of contact. The estimation results for the subsample of Waves 2 to 5 largely align with the full sample results, with the exception that the contact rate stays fairly constant for applications submitted more than 3 days before the last submission. Since $D2L^t$ is constructed as the days elapsed backwards with the last submission to each job as the benchmark, *Incomplete* jobs that were closed in the middle of the experiment process of submitting all 8 applications might exhibit different trends. Figure A.7 shows that, in addition to the robust significance of the early application advantage, the weak monotonicity still holds for the most part, but the estimations are noisier due to a much smaller sample size. These results are robust to the alternative specification with region and wave fixed effects included (Figure A.8).

4.2 Application Timing and Resume Characteristics

So far, I have provided robust causal evidence on the economic and statistical significance of the "early reward" in the context of the experiment, with various con-

structed application timing measures and specification. In this section, I investigate the heterogeneity of such early application advantages across different groups of applicants.

First, I reestimate Model 3.1 and 3.4, adding interaction terms between the early submission indicators and race/gender. This approach allows for a straightforward examination of whether these two widely studied discriminatory factors affect the observed early application advantages. The results are summarized in Table 5: Using the dummy of the first four submissions to a job, $early^1$, I show in Column (1) that Black applicants experience a smaller early application advantage by 0.27 percentage points, which is roughly 21% lower than that of White applicants. However, this difference is statistically insignificant. On the other hand, Column (2) shows that female applicants experience a larger early application advantage by 0.22 percentage point (21.0%) comparing to their male counterparts. This difference is also not statistically significant. In Column (3), I estimate a model with triple interactions of early submission, race, and gender. The results show that Black female applicants face a significant contact penalty compared to all other demographic groups and experience the smallest early application advantage at the same time, making them the most vulnerable group of job seekers in this setting. Using the continuous measure $D2L^t$ yields similar results in Column (4) to (6).

To account for pair-level stratification of applicants' race, I reestimate Model 3.3 and and add interaction terms of $\mathbf{1}\left(Order_i^{pair} = j\right), j = 2, 3, 4$, the designed pair order indicators, with race and gender indicators. Table A.9 summarizes the results. Black applicants do not experience noticeable differences in early application advantages, while female applicants experience a sizably bigger but still statistically insignificant early application advantage compared to male applicants. Adding the three-way interaction terms, Column (3) of Table A.9 shows that Black male applicants experience more early application advantages. White female applicants experience even bigger early application advantages. However, the picture for Black female applicants is very different: they actually experience significant early submission *disadvantages* compared to their Black male and White female counterparts. These results are consistent with those in Table 5, suggesting the potential heterogeneity of early application advantages across demographic groups defined by race and gender.

In Figure A.9 and A.10, I visualize in detail how the race and gender contact

Table 5: Early Application, Race and Gender

	(1)	(2)	(3)	(4)	(5)	(6)
		$early^1$			$D2L^t$	
E.S.I.	0.0129	0.0105	0.0109	0.0041	0.0032	0.0029
	(0.0029)	(0.0031)	(0.0041)	(0.0009)	(0.0010)	(0.0013)
Black	-0.0192	-0.0205	-0.0147	-0.0200	-0.0205	-0.0164
	(0.0024)	(0.0018)	(0.0037)	(0.0027)	(0.0018)	(0.0040)
E.S.I. × Black	-0.0027		-0.0009	-0.0002		0.0006
	(0.0033)		(0.0051)	(0.0010)		(0.0015)
Female	-0.0021	-0.0032	0.0012	-0.0021	-0.0054	-0.0017
	(0.0022)	(0.0029)	(0.0040)	(0.0022)	(0.0032)	(0.0044)
E.S.I. × Female		0.0022	0.0040		0.0017	0.0025
		(0.0039)	(0.0056)		(0.0012)	(0.0017)
Black × Female			-0.0089			-0.0074
			(0.0055)			(0.0059)
E.S.I. × Black × Fem.			-0.0034			-0.0016
			(0.0078)			(0.0023)
Constant	0.2468	0.2480	0.2451	0.2450	0.2469	0.2448
	(0.0026)	(0.0027)	(0.0031)	(0.0028)	(0.0029)	(0.0035)
N				83643		

Note: This table presents the effects of early submission interacted with race and gender on the probability of employer contact within 30 days, controlling for other characteristics. "E.S.I." stands for "Early Submission Indicator", referring to the dummy of first four submissions to each job $early^1$ in Column 1-3 and the days to the last submission $D2L^t$ in Column 4-6. All regressions are on the full sample, including job fixed effects and all resume characteristics. Standard errors in parentheses are clustered at the job level.

gaps evolve over time. Specifically, I reestimate Model 3.2 and 3.5, including interaction terms of gender/race indicators with indicators of order with indicators of order $\mathbf{1}(Order_i = j)$, $j = 2, \cdots, 8$ and indicators of days to last submission $\mathbf{1}(D2L_i^t = d)$, $d = 1, \cdots, 7$, respectively. The contact penalty experienced by Black applicants is larger among early submissions, consistent with the observed smaller early application advantages for Black applicants in Table 5. Conversely, early female applicants experience a small contact reward compared to early male applicants, which diminishes over time. This also aligns with the slightly larger early application advantages for female applicants discussed earlier.

Lastly, I examine whether the effects of various resume characteristics differ for early and late applications in Table 6. Compared to White male applicants,

Black male applicants do not experience a different early application advantage, while White female applicants experience a greater early reward. Black female applicants, however, face an early penalty. These results are consistent with those in Column (3) of Table 5. Applicants over 40 experience an early penalty of 0.31 percentage points and are significantly less likely to be contacted by 0.59 percentage points among the early applicants. Listing membership in an academic club increases the contact rate by 0.88 percentage points for late applicants but has a minor impact on early applicants. Listing membership in an LGBTQ club decreases the contact rate by 0.46 percentage points for early applicants but increases it by 0.39 percentage points for late applicants. Having an associate degree has no effect on early applicants but boosts the probability of contact by 0.44 percentage points for late applicants. Listing gender-typical pronouns leads to a contact penalty of 1.12 percentage points for early applicants, higher than the 0.43 percentage points penalty for late applicants. Listing gender-neutral pronouns, on the contrary, results in a smaller contact penalty for early applicants (0.67 percentage points) comparing to late applicants (1.06 percentage points). However, a joint test cannot reject the null hypothesis of no interaction effects. In Table A.10, I rerun this analysis, comparing submissions of the four designed pairs. While there are changes of effects for individual characteristics, joint tests cannot reject the null hypothesis of no interaction between the characteristics and the designed order of submission pairs.

Taken together, the results, though statistically less significant, consistently suggest an intricate interplay between the timing of application and the two main discriminatory factors, race and gender, in this experimental setting. In Section 5, I delve deeper into this interplay and provide evidence on the potential underlying mechanisms.

5 Application Timing: Mechanisms

The results in the previous session indicated that firms, at least within the context of the experiment involving entry-level jobs without specific requirements targeting a narrow group of applicants with particular traits, exhibit a preference for early applicants during the contact period. This raises the question: why does this preference exist? If employers were to wait until they had collected all applications

Table 6: Early Application Interacted with Resume characteristics

	(1) Late $early^1 = 0$	(2) Early $early^1 = 1$	(3) Diff. $(2) - (1)$
Black	-0.0148	-0.0157	-0.0009
	(0.0037)	(0.0036)	(0.0051)
Female	0.0012	0.0052	0.0040
	(0.0040)	(0.0041)	(0.0056)
Black × Female	-0.0089	-0.0123	-0.0034
	(0.0055)	(0.0055)	(0.0078)
Over 40	-0.0028	-0.0059	-0.0031
	(0.0028)	(0.0029)	(0.0040)
Pol. club	0.0046	0.0072	0.0026
	(0.0070)	(0.0074)	(0.0101)
Acad. club	0.0088	0.0012	-0.0076
	(0.0069)	(0.0072)	(0.0099)
LGBTQ club	0.0039	-0.0046	-0.0085
	(0.0051)	(0.0050)	(0.0071)
Typical pron.	-0.0043	-0.0112	-0.0069
	(0.0071)	(0.0073)	(0.0101)
Neutral pron.	-0.0106	-0.0067	0.0039
	(0.0072)	(0.0072)	(0.0102)
Assoc. degree	0.0044	-0.0001	-0.0045
	(0.0028)	(0.0029)	(0.0040)
Constant	0.2428	0.2585	0.0157
	(0.0035)	(0.0035)	(0.0051)
N	42531	41112	83643
$F-$ stats for joint significance			0.58
$p-$value			0.8344

Note: This table presents the effects of early submission interacted with resume characteristics on the probability of employer contact within 30 days. Column (1) displays the results for late applicants (not the first 4 submissions to a job), Column (2) displays the results for early applicants (the first 4 submissions to a job), Column (3) shows the difference between early and late applicants. All regressions include job fixed effects. Standard errors in parentheses are clustered at the job level. $F-$ statistics and joint $p-$values come from tests that all differences in reported coefficients other than the constant term are zero.

before starting the evaluation process, we would not expect such an advantage for early applications.

Figure A.11 plots the days it took for a submission to be contacted, conditional

on that submission being contacted within 30 days after submission. It shows thatin the context of this study, employers were quick to respond: The average time from submission to contact was 6.04 days for the 20,102 applications contacted within 30 days post-submission. Notably, 21.55% were contacted on the same day of submission, 52.28% within 3 days, and 67.91% within 6 days.

The quick response of employers suggests that later submissions likely arrived after some contact decisions had already been made. In Table A.11, I summarize the number of contacted submissions before the submission of application i, by the submission order of i. On Average, there is a positive number of submissions contacted prior to the submission of i, and the number is monotonically bigger for later submissions. One thing to notice is that there are often multiple contacts within a day. In fact, only 23.35% of submissions were the only one contacted on a specific day. This complicates the calculation since the time of contact was not reported and some submissions were contacted on the day of submission. To tackle this issue, I use two measures as the upper and lower bounds of the number of contacted submissions prior to submission i: For the upper bound, NoC_i^{up}, I include all other applications contacted no later than the day of the submission of i; For the lower bound, NoC_i^{low}, I only include all other applications contacted at least one day before the submission of i. Table A.12 illustrates the construction of the measures with an example. Table A.13 presents the pairwise correlations between the constructed contact history measures and various application timing measures. The conclusion holds robustly that later submissions are more likely to have a higher number of prior contacted submissions.

After constructing the contact history for a job prior to submission i, the natural question to ask is whether having contacted more applicants prior to submission i drives the timing effect we observed. I reestimate Model 3.1, 3.4 and 3.2, adding the two constructed contact history measures NoC_i^{up} and NoC_i^{low}. The results presented in Table A.14 show two key findings: First, for a given job, the more submissions that have already been contacted prior to it, the less likely the current submission is to be contacted, controlling for resume characteristics. Second, adding NoC_i^{up} and NoC_i^{low} significantly reduces or even reverses the early application advantages, suggesting that the *early rewards* shown in Section 4 are, in fact, largely driven by the prior contact history for a job.

These results are not surprising: An employer facing a stream of applications

is likely to stop considering new ones if they have already encountered and contacted enough candidates to move to the next step of the hiring process, even if the remaining applicants are equally qualified. In Table 7, I show how the overall contact history affects applicants of different races and genders differently, controlling for job fixed effects and submission order fixed effects. The results indicate that Black applicants are more significantly negatively impacted by the prior contact history than White applicants, while there are no significant differences between female and male applicants.

Table 7: Prior Contact History and Race/Gender

	(1)	(2) NoC^{up}	(3)	(4)	(5) NoC^{low}	(6)
NoC	-0.0445	-0.0415	-0.0447	-0.0587	-0.0550	-0.0589
	(0.0024)	(0.0024)	(0.0026)	(0.0029)	(0.0030)	(0.0032)
Black	-0.0203	-0.0181	-0.0203	-0.0205	-0.0186	-0.0205
	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)
$NoC \times$ Black		-0.0060			-0.0074	
		(0.0018)			(0.0024)	
Female	-0.0022	-0.0022	-0.0024	-0.0024	-0.0024	-0.0025
	(0.0022)	(0.0022)	(0.0022)	(0.0022)	(0.0022)	(0.0022)
$NoC \times$ Female			0.0004			0.0005
			(0.0023)			(0.0031)
N	83643					

Note: This table presents the effects of prior contact histories, controlling for resume characteristics on the probability of employer contact within 30 days. In Column (1) - (3), NoC refers to NoC_i^{up}, the number of other submissions already contacted no later than the day when a submission was made; In Column (4) - (6), NoC refers to NoC_i^{low}, the number of other submissions already contacted at least one day before a submission was made. All regressions include submission order fixed effects and job fixed effects. Standard errors in parentheses are clustered at the job level.

In Table A.15, I construct two dummies, $\mathbf{1}\left(NoC_i^{up} > 0\right)$ and $\mathbf{1}\left(NoC_i^{low} > 0\right)$, to indicate any submissions having been contacted prior to i for a job, and repeat the analysis in Table 6 to explore how various resume characteristics affect the submissions affect submissions with versus without any prior submissions contacted differently. The results show that Black applicants, especially Black female applicants, experience a lower contact rate compared to their peers when at least one prior submission has been contacted. Applicants listing an LGBTQ club on their resumes experience a higher contact rate when at least one prior submission has

been contacted. Joint tests reject the null hypothesis of no interaction effects when $\mathbf{1}\left(NoC_i^{up} > 0\right)$ is used, suggesting that the impact of resume characteristics varies significantly across submissions with different traits of contact history.

So far, the results leverage the constructed overall contact history and show that the consistently observed early application advantage is due to the quick response of recruiters, who likely stop contacting applicants once they have contacted enough candidates. Next, I delve deeper to understand the impacts of race- and gender-specific contact histories, providing evidence on the evolution of discrimination over time.

Consider applicant i from a demographic minority group g, defined by race or gender, with comparable resume characteristics to their counterparts of the majority group g'. There are three mechanisms the contact history of group g can potentially impact the probability of i getting contacted.

- **Quota**: As suggested by a large body of theoretical and empirical literature (Welch, 1976; Lundberg, 1991; Coate and Loury, 1993; Fang and Moro, 2011; Ahern and Dittmar, 2012; Bertrand et al., 2019; Eckbo, Nygaard and Thorburn, 2019; Bijkerk et al., 2021), hiring quotas of various forms can play a prominent role in labor market discrimination. Here, I consider a spontaneous quota where recruiters are more likely to stop contacting an applicant from group g after having already contacted candidates from the same group. This quota can be symmetric, meaning that recruiters set a quota for the majority group g' as well, leading to a higher chance of contacting an applicant from group g if some candidates from group g' have already been contacted. The quota can be understood as a form of discrimination when employed only for the minority group, while it serves as a diversifying recruiting strategy when employed symmetrically.

- **Taste**: Extensive studies have shown that taste-based discrimination is a significant contributor to labor market inequality (Becker, 2010; Gneezy, List and Price, 2012; Rubinstein and Brenner, 2014; Feld, Salamanca and Hamermesh, 2016; Hedegaard and Tyran, 2018). Absent updating, taste-based discrimination is expected to be relatively stable. Therefore, if a recruiter's discrimination is purely taste-based, we should not see any changes in the contact gap between groups following different traits of contact history.

- **Learning**: Rooted in statistical discrimination and the ambiguity of the productivity of individual employees (Arrow, 1973; Bohren, Imas and Rosenberg, 2019; Lang and Spitzer, 2020), a growing strand of literature has focused on how employers update their beliefs about worker quality (Altonji and Pierret, 2001; Altonji, 2005; Lang and Lehmann, 2012; Campos-Mercade and Mengel, 2024). In the setting of this experiment, if recruiters have learned through evaluating and contacting applicants from group g prior to i, whose resume characteristics are randomized to be equivalent to those of applicants from group g', the recruiters should be able to correct their bias against i following a higher realized contact rate of prior applicants from group g, which signals the true quality of the group.

Consider the model for submission i of minority group g

$$\begin{aligned} Y_i =& \alpha_0 + \eta_1 \cdot \text{Contact History}_i^g + \eta_2 \cdot \mathbf{1}\left(\text{Group}_i = g\right) \\ &+ \eta_3 \cdot \text{Contact History}_i^g \times \mathbf{1}\left(\text{Group}_i = g\right) + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \end{aligned} \tag{5.1}$$

where $\text{Contact History}_i^g$ indicates the contact history of applicants from group g up to i; $\mathbf{X}_i$ includes resume characteristics, job fixed effects and submission order fixed effects. The three mechanisms then predict that:

> Under the **Quota** mechanism, $\eta_3 < 0$; Under the **Taste** mechanism, $\eta_3 = 0$; Under the **Learning** mechanism, $\eta_3 > 0$ [2].

By replacing $\text{Contact History}_i^g$ with $\text{Contact History}_i^{g'}$, the contact history of applicants from the majority group g', This model can be used to estimate cross-group effects and test the potential asymmetry in the **Quota** mechanism and the **Learning** mechanism between minority and majority groups.

For the same purpose, I also estimate

$$\begin{aligned} Y_i =& \alpha_0 + \eta_1 \cdot \text{Contact History}_i^g + \eta_2 \cdot \mathbf{1}\left(\text{Group}_i = g\right) \\ &+ \eta_3 \cdot \text{Contact History}_i^g \times \mathbf{1}\left(\text{Group}_i = g\right) \\ &+ \eta_1 \cdot \text{Contact History}_i^{g'} + \eta_2 \cdot \text{Contact History}_i^{g'} \times \mathbf{1}\left(\text{Group}_i = g\right) + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \end{aligned} \tag{5.2}$$

[2] Due to the design of the experiment, no information on the evaluation procedure following the contact stage is observed. Therefore, the predictions focus only on the contact stage, under the assumption that applicants are contacted because they are deemed qualified. This ensures that, under the Learning mechanism, more previous applicants being contacted always leads to a non-negative update in the perceived quality of applicants from the same group.

where the contact histories of both groups are included.

Race-specific contact history The first set of results focus on how the race-specific contact history affects later submissions.

In Table 8, I estimate Model 5.1 and 5.2 using the two constructed contact history measures for Black and White applicants separately up to i. In Columns (1) to (3), I use the upper bound NoC_i^{up}, which includes all other submissions of the same group contacted no later than the day when i was submitted. Column (1) shows evidence of a quota for contacting Black applicants: one additional previous Black applicant contacted is associated with an extra contact penalty of 3.3 percentage points for an upcoming Black applicant. Column (2) suggests a quota for contacting White applicants as well: one additional previous White applicant contacted is associated with a contact reward of 0.93 percentage points for an upcoming Black applicant. Both of these results are highly significant. However, there is an asymmetry in the effects of race-specific contact histories. The negative impact of the contact history of previous Black applicants is larger in scale than the positive impact of the contact history of previous White applicants. Column (3) further confirms this asymmetry, indicating another dimension of racial discrimination in this context.

In Column (4) to (6), I reestimate both models with the lower bound NoC_i^{low}, which only includes submissions of the same group contacted at least one day before the submission of i. Column (4) confirms the existence of the quota for contacting Black applicants: the additional contact penalty from each Black applicant contacted at least one day before the submission of i is 1.6 percentage points. Column (5) shows a reversal of the effect of the contact history of White applicants on an upcoming Black applicant: each previous White applicant contacted at least one day before the submission of i is associated with a contact penalty of 1.09 percentage points, instead of a reward. These results suggest that, at least for White applicants, the Learning mechanism is also active and dominates when only considering applicants contacted at least one day before i's submission. Column (6) confirm the directions of such effects, but they are not statistically significant.

In Table A.16, I repeat the exercise in Table 8 using indicators $\mathbf{1}\left(NoC_i^{up} > 0\right)$ and $\mathbf{1}\left(NoC_i^{low} > 0\right)$ instead of the numeric measures. The results are very consistent.

Table 8: Race-Specific Contact History

	(1)	(2)	(3)	(4)	(5)	(6)
	History : NoC^{up}			History : NoC^{low}		
Black	-0.0183	-0.0184	-0.0182	-0.0192	-0.0184	-0.0186
	(0.0019)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)
History$^{\text{Black}}$	-0.0615		-0.0157	-0.0977		-0.0525
	(0.0041)		(0.0067)	(0.0058)		(0.0097)
History$^{\text{Black}}$ $\times$ Black	-0.0330		-0.0455	-0.0162		-0.0035
	(0.0039)		(0.0095)	(0.0047)		(0.0099)
History$^{\text{White}}$		-0.0827	-0.0697		-0.0974	-0.0574
		(0.0050)	(0.0078)		(0.0056)	(0.0096)
History$^{\text{White}}$ $\times$ Black		0.0093	0.0341		-0.0109	-0.0108
		(0.0037)	(0.0092)		(0.0047)	(0.0098)
N	83643					

Note: This table presents the effects of race-specific prior contact histories, controlling for resume characteristics on the probability of employer contact within 30 days. In Column (1) - (3), History refers to NoC_i^{up}, the number of other submissions already contacted no later than the day when a submission was made, by race (Black/White); In Column (4) - (6), History refers to NoC_i^{low}, the number of other submissions already contacted at least one day before a submission was made, by race (Black/White). All regressions include submission order fixed effects and job fixed effects. Standard errors in parentheses are clustered at the job level.

Gender-specific contact history Next, I estimate Model 5.1 and 5.2 with gender-specific contact histories. The results are summarized in Table 9. Using the upper bound NoC_i^{up} as the contact history measure, Column (1) and (2) show that both the female and male contact histories up to submission i have a negative impact on the contact rate of i, and this impact does not differ significantly based on the gender of i. Column (3) suggests that Learning could be an underlying mechanism: each additional previous female applicant contacted is associated with an extra contact reward of 0.78 percentage points for an upcoming female applicant. On the other hand, each additional previous male applicant contacted is associated with an extra contact penalty of 0.77 percentage points for an upcoming female applicant. These results are not statistically significant. Replacing the contact history measure with the lower bound NoC_i^{low} yields very consistent results in Column (4) to (6).

Again, in Table A.17, I replace the numeric measures NoC_i^* with indicators $\mathbf{1}\left(NoC_i^* > 0\right)$ and restimate the models. The results are consistent with those ob-

Table 9: Gender-Specific Contact History

	(1)	(2)	(3)	(4)	(5)	(6)
	History : NoC^{up}			History : NoC^{low}		
Female	-0.0061	0.0011	-0.0029	-0.0061	0.0008	-0.0032
	(0.0022)	(0.0022)	(0.0022)	(0.0022)	(0.0022)	(0.0022)
History$^{\text{Female}}$	-0.0699		-0.0546	-0.0955		-0.0734
	(0.0046)		(0.0055)	(0.0057)		(0.0065)
History$^{\text{Female}}$ × Female	0.0001		0.0078	0.0038		0.0087
	(0.0042)		(0.0053)	(0.0054)		(0.0063)
History$^{\text{Male}}$		-0.0626	-0.0347		-0.0803	-0.0447
		(0.0048)	(0.0056)		(0.0057)	(0.0064)
History$^{\text{Male}}$ × Female		0.0010	-0.0077		-0.0021	-0.0079
		(0.0043)	(0.0054)		(0.0055)	(0.0064)
N	83643					

Note: This table presents the effects of gender-specific prior contact histories, controlling for resume characteristics on the probability of employer contact within 30 days. In Column (1) - (3), History refers to NoC_i^{up}, the number of other submissions already contacted no later than the day when a submission was made, by gender (female/male); In Column (4) - (6), History refers to NoC_i^{low}, the number of other submissions already contacted at least one day before a submission was made, by gender (female/male). All regressions include submission order fixed effects and job fixed effects. Standard errors in parentheses are clustered at the job level.

served in Table 9, and more significantly significant. Taken together, these results suggest that the Learning mechanism might be an active factor in how gender-specific contact history influencing the contact rate of subsequent submissions. The learning process appears symmetric for female and male applicants, which aligns with the absence of gender discrimination observed in this context.

One might be concerned about how the two types of contact history are constructed, as they rely on the time span between the submission and the contact for an application, conditional on it being contacted. For those not contacted, no information is available on how long it takes for a recruiter to evaluate and decide not to contact. As a robustness check, I construct an alternative measure of contact history NoC_i^{alt}, solely relying on the order of submission (see Table A.12 for an example of construction). All applications for a job submitted prior to submission i are included to calculate the contact history. Using this measure and the corresponding indicator $\mathbf{1}\left(NoC_i^{alt} > 0\right)$, I reestimate the models and report results in Table A.18 and Table A.19. These results are very consistent with those estimated

using the two contact-date-based measures, demonstrating the robustness of the findings.

Taken together, the results presented in this section provide supportive evidence for two intuitions: First, the significant early application advantages observed in Section 4 are largely driven by recruiters moving quickly to contact applicants and stopping once enough candidates have been contacted. Second, the contact history by demographic groups has asymmetric effects within and across groups, which seems to result from the interplay between two underlying mechanisms: Quota and Learning. However, even though the results are both economically and statistically significant, they should be interpreted with caution. The applications submitted in the experiments are limited to a maximum of eight per job, likely accounting for only a part of the total application pool and the full contact history.

6 Heterogeneity

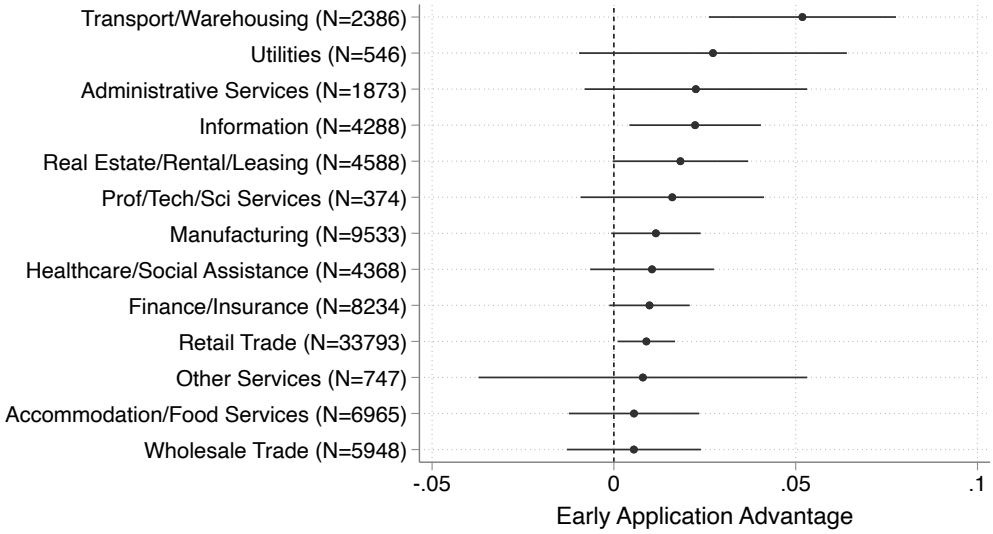
The results thus far have established the significance of early application advantages in the context of entry-level jobs. A key driving force behind these advantages is recruiters moving quickly to contact candidates while continuing to accept other applications, rather than waiting for all applications to come in and then evaluating them together. In this section, I explore the heterogeneity of these findings to further our understanding of the recruiters' decision-making process in contacting candidates.

6.1 Industry Heterogeneity

First, I estimate the early application advantage using Mode 3.1 and the early application indicator $early_i^1$ for each of the thirteen industries and plot the estimated early reward $\hat{\beta}$ in Figure 3. There is significant heterogeneity across industries with respect to the early application advantage. In the transport/warehousing industry, an earlier submission experiences a 5 percentage point higher contact rate, which is more than twice the racial contact gap observed in the experiment. The early application advantage is smallest in the wholesale trade and accommodation/food service industries, though still positive, it is not statistically significant. In the in-

dustry of retail trade, where 40% of the fictitious applications were made, there is a significant early application advantage of about 1 percentage point.

Figure 3: Early Application Advantage by Industry



Notes: The figure plots the estimated coefficients $\hat{\beta}$ of Model 3.1 for each industry, with the early indicator $early^1$ which equals 1 for the first four applications to each job, controlling for resume characteristics. The regressions include job fixed effects, the standard errors are clustered at the job level. The line depicts the 95% confidence intervals. The nubmer of applications in each industry is reported in parentheses.

To better understand the driving force behind the industry heterogeneity of application timing effects, I construct industry-specific task-based characteristics by aggregating the characteristics of typical occupations within each industry (see Section B.1 for more details) and estimate

$$Y_i = \alpha_0 + \mu \cdot early_i^1 + \nu_m \cdot early_i^1 \times \text{Covar}_{m,i} + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \tag{6.1}$$

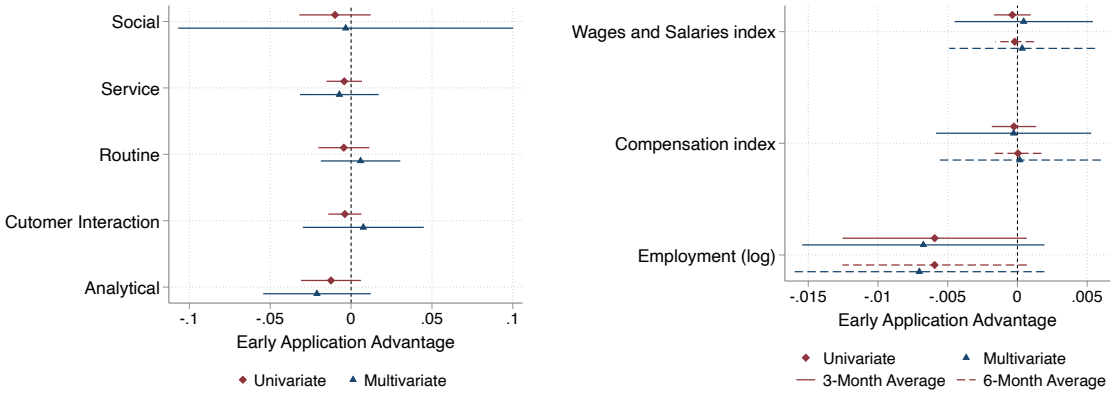
where $\text{Covar}_{m,i}$ is the value of covariate m for application i. I adopt the main specification with job fixed effects and standard errors clustered at the job level. To account for potential correlations between covariates, I also estimate a multivariate model

$$Y_i = \alpha_0 + \phi \cdot early_i^1 + \sum_m \psi_m \cdot early_i^1 \times \text{Covar}_{m,i} + \boldsymbol{\gamma}\mathbf{X}_i + \epsilon_i \tag{6.2}$$

In the left panel of Figure 4, I plot the estimated coefficients of interest $\hat{\nu}_m$ and $\hat{\psi}_m$ for the five task-based industry characteristics: social, service, routine, cus-

tomer interaction, analytical. Univariate estimates show that for all five characteristics, industries that require higher levels of these characteristics exhibit a smaller early application advantage. This suggests that recruiters in these industries are likely to spend more time to compile and compare applications. Controlling for correlations between covariates, the multivariate estimates show that industries requiring more analytical skills and those that are more service-based have a smaller early application advantage. Conversely, industries featuring more routine work and customer interaction exhibit a larger early application advantage. However, due to the broad industry classification, these results are not statistically significant.

Figure 4: Early Application Advantage and Industry-Level Covariates



(a) Task-Based Characteristics

(b) Employment Statistics

Notes: The figures plot how industry level covariates affect the early application advantage. The left panel plots the estimated effects of task-based characteristics; The right panel plots the estimated effects of industry-level employment statistics. "Univariate" refers to estimates of Model 6.1; "Multivariate" refers to estimates of Model 6.2. "3-Month Average" and "6-month Average" refers to the the two choices of time windows to calculate the average employment statistics, only covariates calculated with the same time-window choice enter the same multivariate regression. All regressions include job fixed effects and resume characteristics, the standard errors are clustered at the job level. The lines depict the 95% confidence intervals of the estimates. The construction of covariates is described in Section B.1.

In the right panel of Figure 4, I estimate the effects of three covariates to capture the pre-experiment industry-level labor market conditions: the Wages and Salaries index, the Total Compensation index from the Employment Cost Index (ECI), and the seasonally adjusted monthly employment size prior to the month of the first submission to a job in the experiment (see Section B.1 for more details). The results show that industry-level wages, salaries, and total compensation do not significantly impact the early application advantage. On the other hand, the

employment size of industries is negatively associated with the size of the early application advantage. This suggests that larger industries may experience less urgency in responding to early applications compared to smaller industries. These results are robust across the two time windows for calculating the average value of the covariates (3 months and 6 months prior to the month of the first submission). Univariate and multivariate specifications yield very similar results.

The evidence so far suggests significant industry heterogeneity in the early application advantage. Echoing the results in Table 5, I test whether such heterogeneity exists in the racial and gender gap of the early application advantage. The estimated coefficients of the interaction terms between race/gender indicators and the early submission indicator for each industry are plotted in Figure A.12. Both the racial gap and the gender gap in the early application advantage exhibit noticeable heterogeneity: In the industries of information, real estate/rental/leasing, manufacturing, accommodation/food services, and other services, Black applicants experience a lower early application advantage than their White counterparts. In the industries of professional/technology/science services, administrative services, healthcare/social assistance, and accommodation/food services, female applicants experience a lower early application advantage than their male counterparts. In the transport/warehousing industry, where the early application advantage is the largest, there is no significant gender or racial gap in the early application advantage.

Figure 4b suggests that employment size could be a potential explainer of the industry heterogeneity in the early application advantage. Using annual data on the demographic composition of employment for each industry sourced from the Current Population Survey (see Section B.1), I test whether similar effects can be observed for the racial and gender gap heterogeneity For each industry-year unit, I estimate the racial and gender gap in the early application advantage and plot the estimates against the share of Black and female employees in the industry the year prior to the first submission to a job in the experiment. Figure A.13a shows that in industries with a higher share of Black employees, Black applicants experience a larger early application advantage compared to their White counterparts; On the contrary, Figure A.13b shows a negligible negative correlation between the share of female employees and the gender gap in the early application advantage. Estimating Model 6.1 with the share of employment as the covariate returns consistent

results.

6.2 State Heterogeneity

The second dimension of heterogeneity stems from the location of the jobs. Specifically, I focus on the geographic heterogeneity of the early application advantage at the state level.

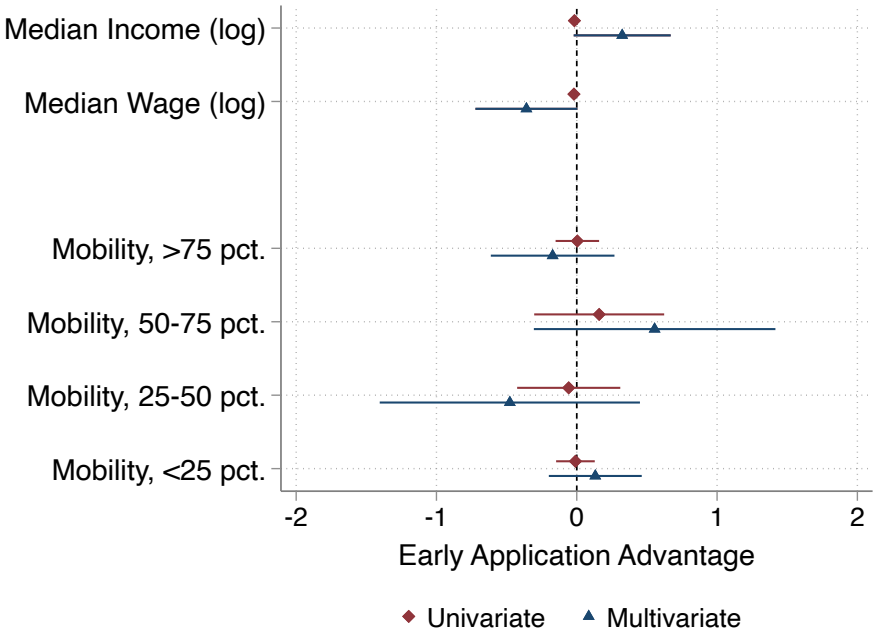
Similarly to Figure 3, I estimate Model 3.1 for each state and plot the estimated early application advantage $\hat{\beta}$ in Figure A.14a. A significant early application advantage is observed in most states, with only 12 states out of the 51 in the sample showing zero or negative advantage. Figure A.14b plots the estimated coefficients of the interaction terms between the early submission indicator and the indicators for Black and female applicants. The results show significant heterogeneity across states in the racial and gender gaps in the early application advantage. In Figure A.15, I plot the state-level racial gaps against gender gaps in the early application advantage. The two gaps exhibit an insignificant positive correlation, indicating that in states where Black applicants experience a larger early application advantage relative to their White counterparts, female applicants are also more likely to experience a larger early application advantage compared to male applicants. In Figure A.16, I repeat the exercise in Figure A.13 with Local Area Unemployment Statistics (LAUS) annual data of state-level demographic composition of the employment and show a similar null effect of the shares of Black/female employees on the racial/gender gaps in the early application advantage.

Next, I examine the explanatory power of three sets of covariates on the observed state-level heterogeneity. The construction of these covariates is detailed in Section B.2.

First, using the newly available granular Income Distributions and Dynamics in America (IDDA) data (Kondo et al., 2023), I construct the pre-experiment median income and wage, as well as the mobility of four income bins, $\{< 25$pct., $25-50$pct., $50-75$pct., > 75pct.$\}$ for each state by averaging 2015-2019 annual data. I then use these six constructed covariates to estimate $\hat{\nu}_m$ in Model 6.1 and $\hat{\psi}_m$ in Model 6.2. As shown in Figure 5, the median income and median wage of a state prior to the experiment do not significantly affect the early application advantage individually. However, they jointly have an impact: in a bivariate regression with both as

regressors, the log median wage of a state is negatively correlated with the early application advantage, while the median income is positively correlated with it. Both results are significant, indicating that the wage-to-income ratio is a negative predictor of the early application advantage at the state level. One interpretation of this result is that in states where wages constitute a larger component of income for median households, recruiters tend to wait and compile more applications rather than moving forward quickly to contact applicants as more applications come in.

Figure 5: Early Application Advantage and State Income/Mobility Conditions



Notes: The figure plots how state pre-experiment income and mobility conditions affect the early application advantage. "Univariate" refers to estimates of Model 6.1; "Multivariate" refers to estimates of Model 6.2. The two income measures and the four mobility measures are estimated separately as two groups of covariates in two multivariate regressions. All regressions include job fixed effects and resume characteristics, with standard errors clustered at the job level. The line depicts the 95% confidence intervals.

The mobility of the four income bins does not have a significant impact on the early application advantage individually. However, in a multivariate regression where all four mobility measures are used as regressors, I observe a positive correlation between the early application advantage and the mobility of both the $50-75$pct. and the <25pct. bins. Conversely, the correlation between the early application advantage and the mobility of both the $25-50$pct. and the >75pct. bins is negative. As summarized in Table B.1, the mobility of the $50-75$pct. and

the > 75pct. bins is negative (*downward mobility*), while the mobility of the < 25pct. and the $25 - 50$pct. bins is positive (*upward mobility*). Thus, the results are symmetric: for states where richest (> 75pct.) and the poorest (< 25pct.) groups are more likely to converge to the middle, the early application advantage is larger; for states where the middle groups ($50 - 75$pct. and $25 - 50$pct.) are more likely to converge to the middle, the early application advantage is smaller. However, none of these results are statistically significant potentially due to the small number of observation units (51 states).

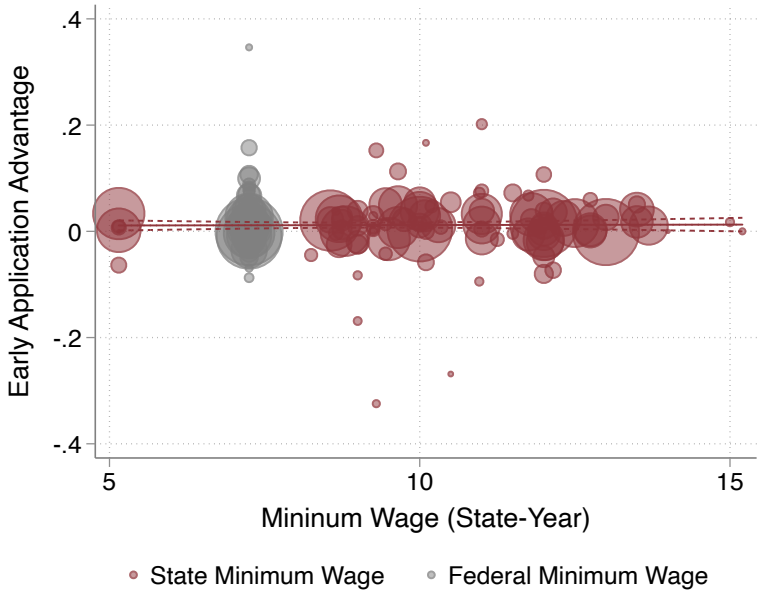
The next covariate, the minimum wage in the state where a sampled job was located in the year when the first application for the job was submitted, reflects the impact of state-level labor protection on the early application advantage. I group jobs by states and the year of the first submission, and estimate Model 3.1 for each state-year group. The estimated coefficients are then plotted against the corresponding minimum wage in Figure 6. The results show no correlation between the early application advantage and state minimum wages in the context of the experiment. In Table A.20, I estimate Model 6.1 with the minimum wages as the covariate and obtain a robust null result. Using state minimum wages instead of the federal minimum wage does not differentiate the early application advantage of states (Table A.20, Column (3)). Among states not using the federal minimum wage, I do not observe any significant effect of the minimum wages on the early application advantage either (Table A.20, Column(2)).

However, these results do not rule out the impact of minimum wages on the application timing effects. In the experiment, all firms were sampled specifically for having branches in all states. They are competitive employers in the labor market, typically offering above-minimum compensations. For firms that are actually bounded by minimum wage regulations, the results could be very different.

The last set of state-level covariates aims to capture monthly labor market conditions. The six covariates, sourced from the Job Openings and Labor Turnover Survey (JOLTS), are described in Section B.2. For each job, I include the value of a covariate for the month when the first submission was made, as well as the average value of the covariate over the three months prior to the month of the first submission, in Model 6.2. The bivariate estimations for the six covariates are plotted in Figure 7.

Neither the current month nor the previous 3-month average share of job open-

Figure 6: Early Application Advantage and State Minimum Wages

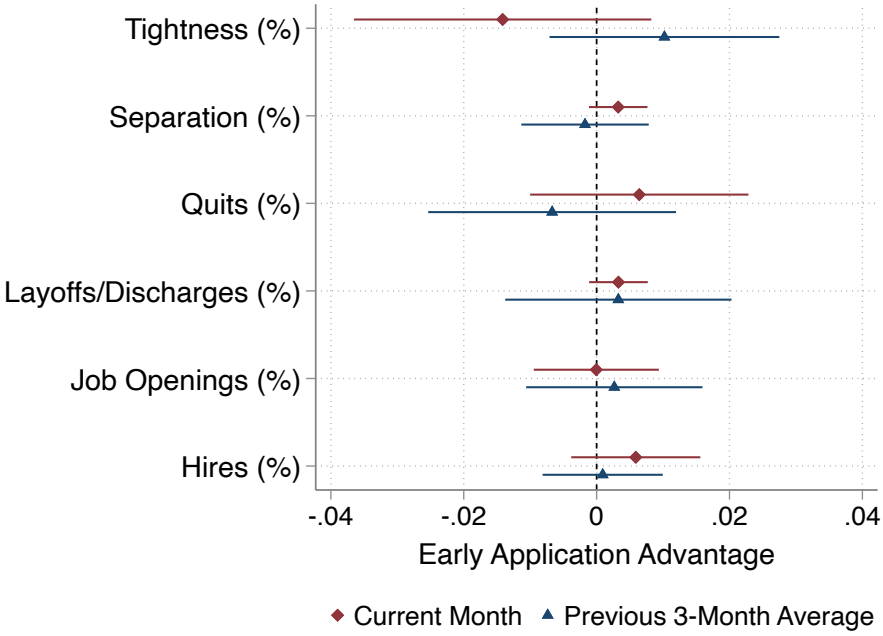


Notes: The figure plots how state minimum wages affect the early application advantage. Each dot represents the estimated early application advantage for a state-year group. The size of the dots represents the number of submissions in the corresponding state-year group. The dots in grey represent states without minimum wages. All regressions include job fixed effects and resume characteristics, with standard errors clustered at the job level.

ings relative to the sum of job openings and employment has a significant impact on the early application advantage. Meanwhile, states with a higher share of new hires out of employment in the current month exhibit a larger early application advantage, while the previous new hire rates do not have any impact. The state with a higher share of new hires out of employment in the current month also exhibit a larger early application advantage, while the previous new hire rates do not have any impact. This result is not surprising. As more hirings are made, recruiters are naturally forced to move quicker in moving the hiring decisions. And there are no carry-over effects from the previous new hire rates.

As for separations, the current quit rate out of employment is positively associated with the early application advantage, while the previous average quit rates are negatively correlated with it. The current layoff/discharge rate out of employment is also positively correlated with the early application advantage, but unlike the previous average quit rate, the previous average layoff/discharge rate is also positively correlated with the early application advantage. When summed

Figure 7: Early Application Advantage and State Labor Market Conditions



Notes: The figure plots how state monthly labor market conditions affect the early application advantage. The six labor market condition measures are obtained from JOLTS (see Section B.2 for details). "Current Month" refers to the value of the covariates of the month when the first submission was made for a job in the state where the job was located; "Previous 3-Month Average" refers to the average value of the covariates over the 3 months prior to the month of first submission for a job. For each covariate, the estimates were obtained from Model 6.2, including both the current month measure and the previous 3-month average measure as regressors. All regressions include job fixed effects and resume characteristics, with standard errors clustered at the job level. The line depicts the 95% confidence intervals.

together, the positive correlation between the current separation rate and the early application advantage persists. However, the impact of the previous quit rate and the layoff/discharge rate cancels out, leading to a null effect for the previous average total separation rate. One can interpret the effects of separations as a mix of a demand-driven effect, where separations leave positions to be filled, and a supply-driven effect, where more unemployed workers could apply for a job. As the results show, application timing can play an intricate role in this dynamic.

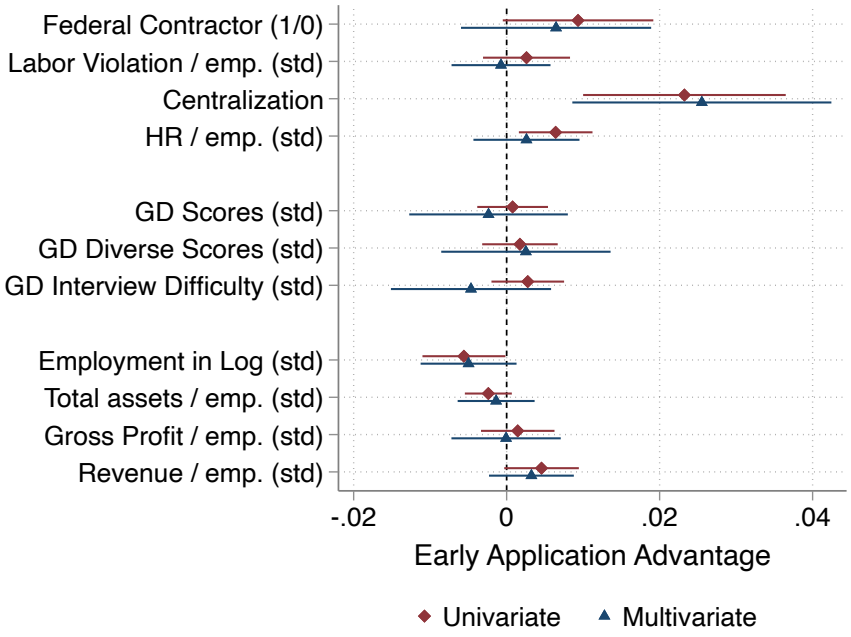
When examining labor market tightness, defined as the ratio of unemployed persons per job opening, I observe that current labor market tightness is negatively correlated with the early application advantage, while previous labor market tightness is positively correlated with it. One explanation for these results is that increased competition between job seekers grants employers more market power,

allowing them to wait longer and make hiring decisions with a larger pool of candidates to consider.

6.3 Firm Heterogeneity

Lastly, I focus on firm-level heterogeneity in the early application advantage. For each of the sampled firms, I collect a set of pre-experiment covariates on human resource management practices, employee satisfaction, and financial performance (see Section B.3 for detailed descriptions) and plot both univariate and multivariate estimates in Figure 8.

Figure 8: Early Application Advantage and Firm characteristics



Notes: The figure plots how pre-experiment firm characteristics affect the early application advantage. "Univariate" refers to estimates of Model 6.1; "Multivariate" refers to estimates of Model 6.2. The definition of the covariates is described in Section B.3. All covariates are normalized to have a standard deviation of 1. All regressions include job fixed effects and resume characteristics, with standard errors clustered at the job level. The line depicts the 95% confidence intervals.

Firms that are federal contractors exhibit a larger early application advantage. Additionally, those with a higher number of labor law violations per employee also show a larger, though statistically insignificant, early application advantage. Firms with a larger human resource management department relative to their total employment size also experience a bigger early application advantage. The

most significant predictor is the callback centralization measure constructed by Kline, Rose and Walters (2022): for firms where hiring responsibility is more concentrated, the early application advantage is greater. Taken together, these results show that human resource management practices are significant predictors of the early application advantage.

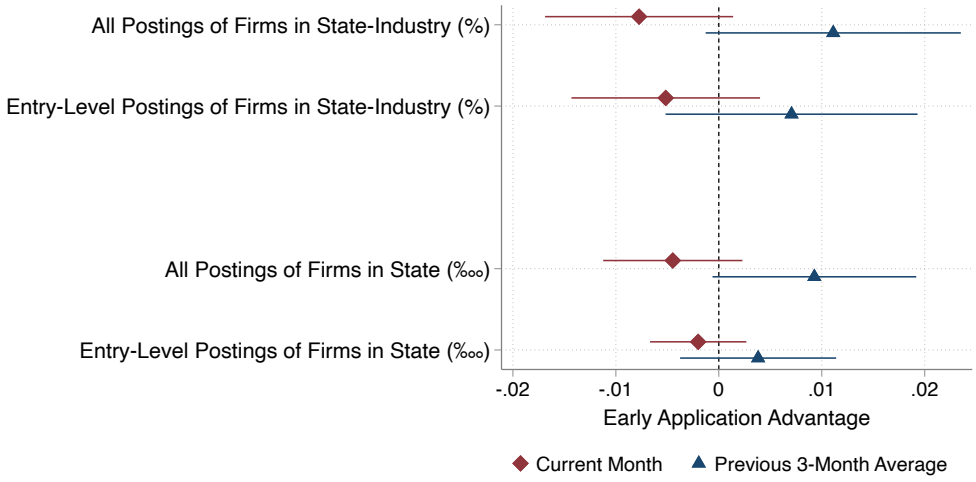
Using GlassDoor review scores, I show that the overall scores and diversity scores given by employees do not explain the firm-level heterogeneity of the early application advantage. Meanwhile, the interview difficulty scores are negatively associated with the early application advantage, consistent with the intuition that firms with a more rigorous recruiting process tend to wait longer to compile more candidates.

As for the financial performance of firms, consistent with the industry-level results in Figure 4b, employment size is negatively correlated with the early application advantage. Firms with a higher level of total assets per employee also exhibit a smaller early application advantage. While total revenue per employee is positively associated with the early application advantage, gross profit per employee shows a positive but not statistically significant correlation. These results suggest that firms of larger scale, as measured by assets and employment size, exhibit a smaller early application advantage. On the contrary, firms with better revenue performance tend to exhibit a larger early application advantage.

Lastly, I utilize the LightCast job posting data from June 2019 to March 2020 to create measures of firms' market power in state-by-industry and state level labor markets for firms sampled in Wave 1. These measures are calculated as the number of postings by a firm out of all postings in the local labor market each month. The construction of these market power measures is detailed in Section B.3. I then adopt the same empirical strategy as in Figure 7: for a job at a firm in a state, I include both the value of the market power covariate of the firm in the month when the first submission was made and the 3-month average value prior to the month of the first submission. The estimated results are plotted in Figure 9.

Consistent with the effect of state labor market tightness on the early application advantage shown in Figure 7, firms that hold a larger share of total postings in both state-by-industry and state-level labor markets exhibit a smaller early application advantage. The results are robust for the measures constructed using all postings and for postings of entry-level jobs, defined as jobs in O*NET Job Zone

Figure 9: Early Application Advantage and Firm Market Power



Notes: The figure plots how the monthly firm market power affects the early application advantage. The firm market power measures are constructed as discussed in Section B.3. "Current Month" refers to the value of the covariates of the month when the first submission was made for a job in the state where the job was located; "Previous 3-Month Average" refers to the average value of the covariates over the 3 months prior to the month of first submission for a job. "Entry-Level" refers to occupations with O*NET codes classified in Job Zone 1 and 2. For each covariate, the estimates were obtained from Model 6.2, including both the current month measure and the previous 3-month average measure as regressors. All regressions include job fixed effects and resume characteristics, with standard errors clustered at the job level. The line depicts the 95% confidence intervals. Sample: Wave 1.

1 and 2. One way to interpret these results is that firms posting more positions relative to the market are more visible to applicants, thereby gaining some market power from having a larger pool of potential candidates. Consequently, recruiters feel less pressure to move quickly when making the decision to contact qualified candidates. Mirroring the state labor market tightness results, the observed outcomes here stem from the interplay of competition on both sides of the labor market, where the timing of application is a key factor to consider in the equilibrium[3].

7 Conclusion

This paper leverages a large-scale resume correspondence study, in which over 83,000 fictitious applications were sent to 108 large US firms for over 10,000 jobs across various US counties, to explore the causal effects of application timing on

[3]These results align with recent work on modelling the composition of the applicant pool with on-the-job search (Eeckhout and Lindenlaub, 2019; Engbom, 2021; Bradley, 2022).

employer responses.

The results provide compelling evidence of a significant "early application advantage": The early half of applications submitted to a job in the experiment are 1.2 percentage points more likely to receive a response from employers, an effect size equivalent to 57% of the Black-White contact gap observed in the same context. The early application advantage follows a nearly linear relationship with the number of days prior to the last submitted application: each day an application is submitted earlier increases the likelihood of contact by 0.4 percentage points.

By constructing the contact history prior to an application, I show that the early application advantage is primarily driven by recruiters adopting a hiring strategy where early applicants are contacted for evaluation while later applications continue to be received. Each additional prior application contacted reduces the probability of contact for the current application by 4.5 to 5.9 percentage points. Furthermore, I construct group-specific contact histories and demonstrate two key mechanisms, Quota and Learning, that jointly shape recruiters' decision-making processes over time. These mechanisms are not symmetric: the Quota mechanism dominates for Black applicants, while the Learning mechanism is more significant for female applicants.

I also explore the driving force behind the substantial heterogeneity in the early application advantage across industries, states, and firms. Two key results emerge: States with a higher unemployment-to-opening ratio exhibit a smaller early application advantage. Firms that hold a larger share of job postings in the local labor market also exhibit a smaller early application advantage. These observations are consistent with the intuition of job-search models, which suggest that employers become more selective when faced with a longer queue of applicants.

In sum, these results underscore the importance of incorporating the timing aspect into research on labor market discrimination. The dimension of timing has a direct impact and can influence the effects of discriminatory factors through mechanisms such as belief updating or quota. The timing effect can also potentially reconcile the disparities between null effects in resume correspondence studies and the observed discrimination in the field. For instance, in the case of gender discrimination, while recent correspondence experiments suggest that recruiters do not discriminate against female applicants at the contact stage (Kline, Rose and Walters, 2022), female applicants may still experience lower contact rates due to

late application submissions. This delay could arise from spending too much time overpreparing applications due to underconfidence (Aksoy, Exley and Kessler, 2024), or facing tighter time constraints due to domestic responsibilities (Philippe and Skandalis, 2023). The findings of this paper suggest that researchers should be especially careful when designing resume correspondence studies and take the timing aspect into consideration.

Timing has started to gain attention in recent studies on labor market discrimination, as it is a key element of various job search models. The results of this paper provide empirical insights for modelling labor market dynamics, where competition on both the supply side (job seekers) and the demand side (firms) shape the equilibrium.

It is also important to note that results of this paper specifically pertain to entry-level jobs and represent a small step towards understanding the timing aspect of job search and hiring process. As an increasing amount of detailed real-world data become available for various types of jobs (e.g., Davis and de la Parra, 2024), more empirical studies in non-experimental contexts can hopefully provide further insights into the labor market implications of application timing.

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A Figures and Tables

Table A.1: Consistency of Early Submission Indicators

	Pair Order			
Order	1	2	3	4
Panel A: full sample				
1	11090	21	3	0
2	10549	479	3	0
3	341	10322	14	5
4	114	9980	498	14
5	23	371	9865	19
6	10	60	9539	595
7	5	23	469	9402
8	13	32	91	9693
Total	22145	21288	20482	19728
Panel A: jobs with 8 submissions				
1	9812	14	3	0
2	9384	443	2	0
3	316	9495	13	5
4	95	9243	477	14
5	23	350	9437	19
6	10	58	9176	585
7	5	23	459	9342
8	13	32	91	9693
Total	19658	19658	19658	19658

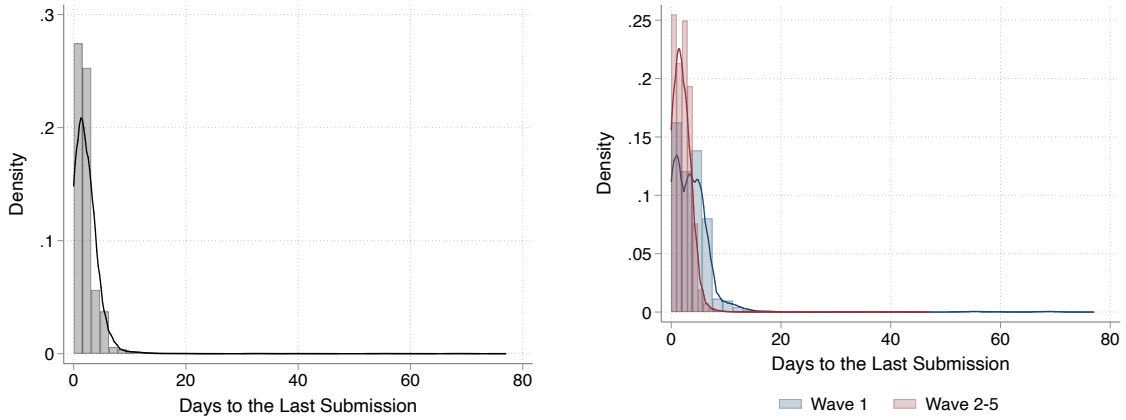
Note: Full sample in Panel A, Jobs with 8 submissions in Panel B. Order is constructed with the date and time of actual submissions; Pair Order is constructed by design.

Table A.2: Consistency of Early Submission Indicators

		$early^1$		$early^2$		$early^3$	
		$=0$	$=1$	$=0$	$=1$	$=0$	$=1$
Panel A: full sample							
$early^1$	$=0$			42531	0	42005	526
	$=1$			902	40210	1284	39828
$early^3$	$=0$					42755	678
	$=1$					534	39676
total		42531	41112	43433	40210	43289	40354
				$N = 83643$			
Panel B: jobs with 8 submissions							
$early^1$	$=0$			39316	0	38802	514
	$=1$			0	39316	514	38802
$early^3$	$=0$					38802	514
	$=1$					514	38802
total		39316	39316	39316	39316	39316	39316
				$N = 78632$			

Note: Full sample in Panel A, Jobs with 8 submissions in Panel B. The four early submission indictors are constructed as outlined in Section 3.1.

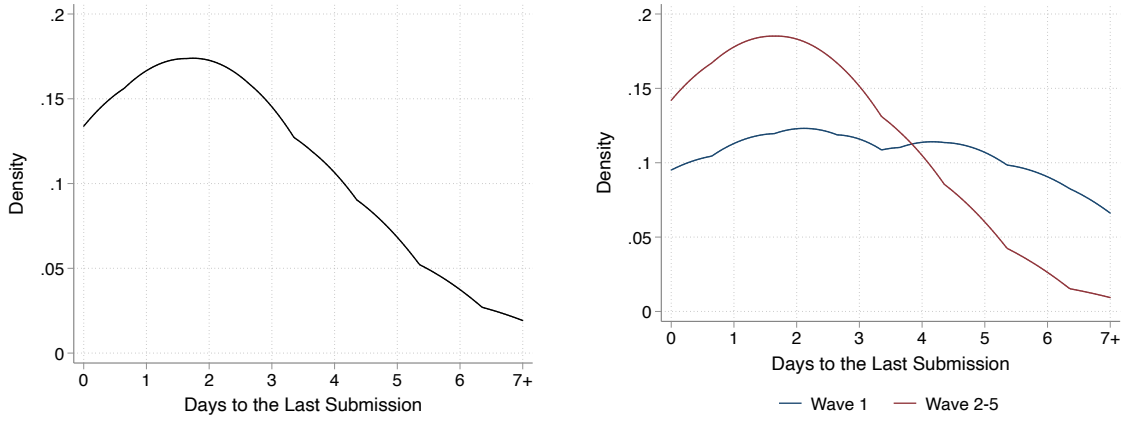
Figure A.1: Distribution of Days to the Last Submission $(D2L)$



(a) Full Sample (b) Wave 1 v.s. Wave 2-5

Notes: The variable $D2L$ is calculated as in Section 3.1. The distribution of all applications is depicted in the left panel; the distributions of applications in Wave 1 and Wave 2-5 are depicted separately in the right panel. In both panels, Epanechnikov kernel is used, the kernel half-width is set as 1.

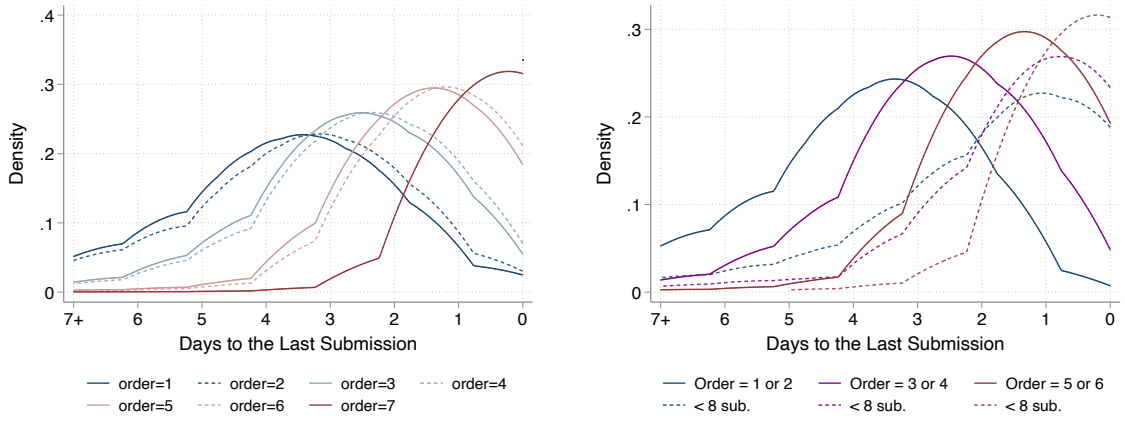
Figure A.2: Distribution of Days to the Last Submission, Truncated $(D2L^t)$



(a) Full Sample (b) Wave 1 v.s. Wave 2-5

Notes: The variable $D2L^t$ is calculated as in Section 3.1. The distribution of all applications is depicted in the left panel; the distributions of applications in Wave 1 and Wave 2-5 are depicted separately in the right panel. In both panels, Epanechnikov kernel is used, the kernel half-width is set as 1.5.

Figure A.3: Distribution of $D2L^t$, by Submission Order



(a) Full Sample (b) Complete v.s. Incomplete

Notes: The variable $D2L^t$ is calculated as in Section 3.1. In the left panel, applications are grouped by the within-job orders of submission. In the right panel, applications of order 1/2, 3/4, 5/6 are grouped together, the solid lines represent jobs with 8 submissions (*Complete*), the dashed line represent jobs with less than 8 submissions due to early closing (*Incomplete*). In both panels, Epanechnikov kernel is used, the kernel half-width is set as 1.

Table A.3: Randomization Test of Constructed Timing Measures

Dep.Var.	(1) $early^1$	(2) $early^2$	(3) $early^3$	(4) $Order$	(5) $Order^{pair}$	(6) $D2L^t$
Specification A: with job FEs, clustering on job level						
Balanced	1.55	1.37	1.19	0.73	1.02	1.25
	[0.1228]	[0.1938]	[0.2967]	[0.6864]	[0.4212]	[0.2568]
Full	1.07	0.98	0.83	0.63	0.83	0.97
	[0.3831]	[0.4564]	[0.5857]	[0.7692]	[0.5876]	[0.4618]
Full$(=8)$	0.99	0.99	0.82	0.53	1.11	0.92
	[0.4481]	[0.4481]	[0.5988]	[0.8540]	[0.3519]	[0.5040]
Full(<8)	1.10	1.06	0.87	0.93	1.03	0.75
	[0.3566]	[0.3931]	[0.5478]	[0.5007]	[0.4142]	[0.6610]
Specification B: with job FEs, robust						
Balanced	1.24	1.25	1.21	0.81	0.86	0.62
	[0.2647]	[0.2582]	[0.2823]	[0.6096]	[0.5593]	[0.7791]
Full	1.03	1.01	0.87	0.68	0.59	0.61
	[0.4130]	[0.4252]	[0.5497]	[0.7313]	[0.8043]	[0.7879]
Full$(=8)$	0.93	0.93	0.83	0.59	0.56	0.51
	[0.4989]	[0.4989]	[0.5871]	[0.8066]	[0.8282]	[0.8676]
Full(<8)	1.26	1.16	0.91	0.94	1.15	0.95
	[0.2520]	[0.3192]	[0.5191]	[0.4856]	[0.3255]	[0.4792]
Specification C: with wave and region FEs, clustering on job level						
Balanced	1.97**	1.66*	1.16	1.13	2.03**	1.38
	[0.0388]	[0.0938]	[0.3181]	[0.3395]	[0.0319]	[0.1929]
Full	1.33	1.19	0.90	0.81	1.39	1.09
	[0.2129]	[0.2943]	[0.5221]	[0.6086]	[0.1864]	[0.3692]
Full$(=8)$	1.13	1.13	0.94	0.61	1.27	1.05
	[0.3383]	[0.3383]	[0.4911]	[0.7934]	[0.2454]	[0.4004]
Full(<8)	0.55	0.82	0.52	1.61	1.69*	0.32
	[0.8377]	[0.5972]	[0.8627]	[0.1065]	[0.0860]	[0.9691]

Note: All regressions include the list of covariates in Table 2 of Kline, Rose and Walters (2022). In each unit, joint $F-$ statistic (Degree of Freedom $= 9$) and $p-$values are reported in the square parenthesis. Each column refers to a constructed timing measure indicated by the column title. The four rows in each panel corresponds to balanced sample, full sample, full sample of jobs with 8 submissions, full sample of jobs with less than 8 submissions. Significant level: *** 0.01, ** 0.05, * 0.1.

Table A.4: Estimation of Model 3.1, Alternative Specification 1

	(1) Full	(2) < 8	(3) Full	(4) < 8	(5) Full	(6) < 8
$early^1$	0.0116	0.0240				
	(0.0018)	(0.0094)				
$early^2$			0.0113	0.0135		
			(0.0018)	(0.0093)		
$early^3$					0.0105	0.0121
					(0.0018)	(0.0092)
Black	-0.0205	-0.0125	-0.0205	-0.0124	-0.0205	-0.0125
	(0.0017)	(0.0059)	(0.0017)	(0.0059)	(0.0017)	(0.0059)
Female	-0.0021	-0.0006	-0.0021	-0.0009	-0.0021	-0.0008
	(0.0019)	(0.0068)	(0.0019)	(0.0068)	(0.0019)	(0.0068)
Over 40	-0.0043	0.0041	-0.0043	0.0038	-0.0043	0.0040
	(0.0019)	(0.0071)	(0.0019)	(0.0071)	(0.0019)	(0.0071)
Pol. club	0.0059	0.0037	0.0059	0.0021	0.0059	0.0020
	(0.0048)	(0.0216)	(0.0048)	(0.0217)	(0.0048)	(0.0217)
Acad. club	0.0049	0.0136	0.0049	0.0135	0.0049	0.0140
	(0.0047)	(0.0159)	(0.0047)	(0.0159)	(0.0047)	(0.0159)
LGBTQ club	-0.0003	0.0152	-0.0003	0.0152	-0.0003	0.0154
	(0.0034)	(0.0155)	(0.0034)	(0.0155)	(0.0034)	(0.0155)
Typical pron.	-0.0077	0.0056	-0.0077	0.0042	-0.0077	0.0043
	(0.0047)	(0.0192)	(0.0047)	(0.0190)	(0.0047)	(0.0190)
Neutral pron.	-0.0086	0.0065	-0.0086	0.0068	-0.0085	0.0069
	(0.0047)	(0.0182)	(0.0047)	(0.0182)	(0.0047)	(0.0182)
Assoc. degree	0.0021	0.0082	0.0021	0.0080	0.0021	0.0080
	(0.0019)	(0.0071)	(0.0019)	(0.0071)	(0.0019)	(0.0071)
Constant	0.2475	0.1680	0.2477	0.1746	0.2481	0.1744
	(0.0023)	(0.0089)	(0.0023)	(0.0080)	(0.0023)	(0.0082)
N	83643	5011	83643	5011	83643	5011

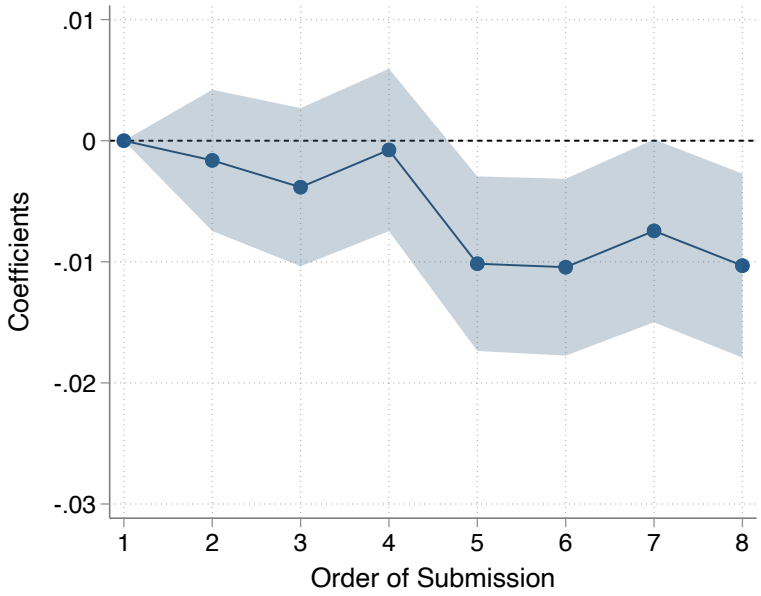
Note: This table presents the effects of early submission and other characteristics on the probability of employer contact within 30 days. Columns titled "Full" include all jobs, while columns titled "<8" include jobs with less than 8 jobs. All regressions include job fixed effects. Robust standard errors are in parentheses.

Table A.5: Estimation of Model 3.1, Alternative Specification 2

	(1) Full	(2) < 8	(3) Full	(4) < 8	(5) Full	(6) < 8
$early^1$	0.0123	0.0048				
	(0.0023)	(0.0160)				
$early^2$			0.0141	0.0028		
			(0.0023)	(0.0145)		
$early^3$					0.0131	0.0061
					(0.0023)	(0.0150)
Black	-0.0206	-0.0118	-0.0206	-0.0118	-0.0205	-0.0118
	(0.0017)	(0.0059)	(0.0017)	(0.0059)	(0.0017)	(0.0059)
Female	0.0001	0.0134	0.0001	0.0134	0.0001	0.0134
	(0.0030)	(0.0107)	(0.0030)	(0.0107)	(0.0030)	(0.0107)
Over 40	-0.0058	0.0071	-0.0058	0.0071	-0.0058	0.0071
	(0.0030)	(0.0113)	(0.0030)	(0.0113)	(0.0030)	(0.0113)
Pol. club	-0.0018	-0.0440	-0.0018	-0.0440	-0.0018	-0.0440
	(0.0074)	(0.0296)	(0.0074)	(0.0295)	(0.0074)	(0.0295)
Acad. club	0.0096	0.0185	0.0095	0.0185	0.0095	0.0186
	(0.0076)	(0.0339)	(0.0076)	(0.0340)	(0.0076)	(0.0340)
LGBTQ club	-0.0052	0.0317	-0.0052	0.0317	-0.0052	0.0317
	(0.0055)	(0.0228)	(0.0055)	(0.0228)	(0.0055)	(0.0228)
Typical pron.	-0.0138	-0.0174	-0.0139	-0.0175	-0.0138	-0.0174
	(0.0073)	(0.0294)	(0.0073)	(0.0294)	(0.0073)	(0.0294)
Neutral pron.	-0.0105	-0.0322	-0.0105	-0.0323	-0.0105	-0.0323
	(0.0076)	(0.0301)	(0.0076)	(0.0301)	(0.0076)	(0.0301)
Assoc. degree	0.0012	0.0176	0.0012	0.0175	0.0012	0.0176
	(0.0030)	(0.0110)	(0.0030)	(0.0110)	(0.0030)	(0.0110)
Constant	0.2009	0.1854	0.2003	0.1866	0.2008	0.1859
	(0.0113)	(0.0324)	(0.0113)	(0.0322)	(0.0113)	(0.0322)
N	83643	5011	83643	5011	83643	5011

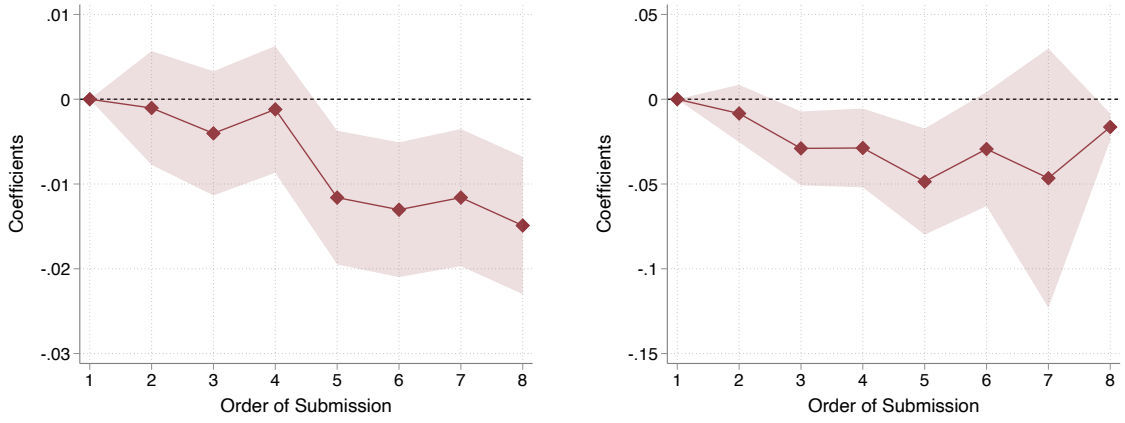
Note: This table presents the effects of early submission and other characteristics on the probability of employer contact within 30 days. Columns titled "Full" include all jobs, while columns titled "<8" include jobs with less than 8 jobs. All regressions include wave and region fixed effects. Standard errors in parentheses are clustered at the job level.

Figure A.4: Contact Gap and Order of Submission, Alternative Specification



Notes: The figure plots the estimated coefficients $\hat{\beta}_j, j \in \{2, \cdots, 8\}$ of Model 3.2 with the order of submission on the x-axis, controlling for resume characteristics. The regression includes region and wave fixed effects, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals.

Figure A.5: Contact Gap and Order of Submission, *Complete* v.s. *Incomplete* Jobs



(a) *Complete* Jobs ($N = 78632$)

(b) *Incomplete* Jobs ($N = 5011$)

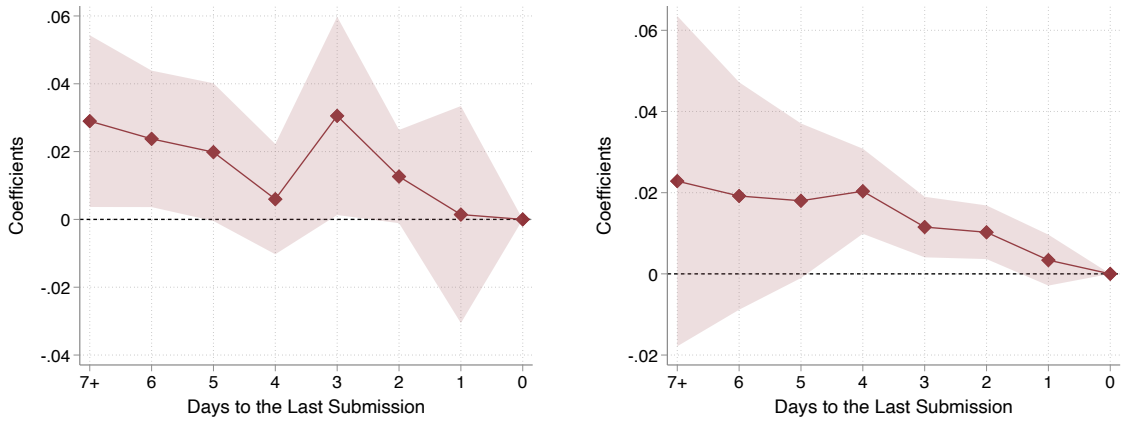
Notes: The figures plot the estimated coefficients $\hat{\beta}_j, j \in \{2, \cdots, 8\}$ of Model 3.2 with the order of submission on the x-axis, controlling for resume characteristics. In the left panel, the sample consists of jobs with 8 submissions. In the right panel, the sample consists of jobs with less than 8 submissions. The regression includes job fixed effects, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals.

Table A.6: Estimation of Model 3.3, w. Wave and Region Effects

	(1) Full	(2) $= 8$	(3) < 8
$\mathbf{1}(Order_i^{pair} = 2)$	-0.0012	-0.0020	-0.0109
	(0.0025)	(0.0026)	(0.0095)
$\mathbf{1}(Order_i^{pair} = 3)$	-0.0089	-0.0113	-0.0302
	(0.0029)	(0.0030)	(0.0155)
$\mathbf{1}(Order_i^{pair} = 4)$	-0.0069	-0.0116	-0.0304
	(0.0031)	(0.0031)	(0.0418)
Constant	0.2108	0.2136	0.1958
	(0.0113)	(0.0121)	(0.0319)
N	83643	78632	5011

Note: This table presents the effects of the order of submission pairs on the probability of employer contact within 30 days. The column titled "Full" includes all jobs, the column titled "=8" includes all *Complete* jobs with 8 submissions, the column titled "<8" includes only *Incomplete* jobs with less than 8 submissions. All regressions include the full set of resume characteristics and wave and region fixed effects. Standard errors clustered at the job level are reported in parentheses.

Figure A.6: Contact Gap and $D2L^t$, Wave 1 v.s. Wave 2-5



(a) Wave 1 ($N = 14541$) (b) Wave 2-5 ($N = 69102$)

Notes: The figures plot the estimated coefficients $\hat{\delta}_j, j \in \{1, \cdots, 7\}$ of Model 3.5 with days to the last submission $D2L^t$ on the x-axis, controlling for resume characteristics. The left panel shows Wave 1 results. The right panel, shows Wave 2-5 results. The regression includes job fixed effects, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals.

Table A.7: Estimation of Model 3.4, Winsorized $D2L$

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Wave 1		Wave 2-5	
	Full	< 8	Full	< 8	Full	< 8
$D2L$	0.0039	0.0084	0.0034	0.0079	0.0042	0.0092
	(0.0008)	(0.0032)	(0.0013)	(0.0038)	(0.0010)	(0.0064)
Black	-0.0205	-0.0125	-0.205	-0.0112	-0.0205	-0.0132
	(0.0018)	(0.0066)	(0.0041)	(0.0113)	(0.0020)	(0.0080)
Female	-0.0021	-0.0007	0.0013	-0.0044	-0.0028	0.0014
	(0.0022)	(0.0080)	(0.0051)	(0.0131)	(0.0024)	(0.0102)
Over 40	-0.0043	0.0034	-0.0041	0.0036	-0.0043	0.0034
	(0.0020)	(0.0083)	(0.0048)	(0.0147)	(0.0022)	(0.0100)
Pol. club	0.0060	0.0026			0.0060	0.0027
	(0.0051)	(0.0253)			(0.0051)	(0.0252)
Acad. club	0.0050	0.0136			0.0050	0.0136
	(0.0051)	(0.0174)			(0.0051)	(0.0173)
LGBTQ club	-0.0002	0.0154			-0.0002	0.0153
	(0.0036)	(0.0171)			(0.0036)	(0.0171)
Typical pron.	-0.0078	0.0040			-0.0078	0.0040
	(0.0051)	(0.0202)			(0.0051)	(0.0202)
Neutral pron.	-0.0085	0.0062			-0.0085	0.0059
	(0.0051)	(0.0216)			(0.0050)	(0.0216)
Assoc. degree	0.0021	0.0084	-0.0013	0.0040	0.0028	0.0108
	(0.0020)	(0.0085)	(0.0050)	(0.0150)	(0.0022)	(0.0103)
Constant	0.2454	0.1669	0.1932	0.1743	0.2561	0.1624
	(0.0026)	(0.0092)	(0.0061)	(0.0148)	(0.0030)	(0.0121)
N	83643	5011	14541	1821	69102	3190

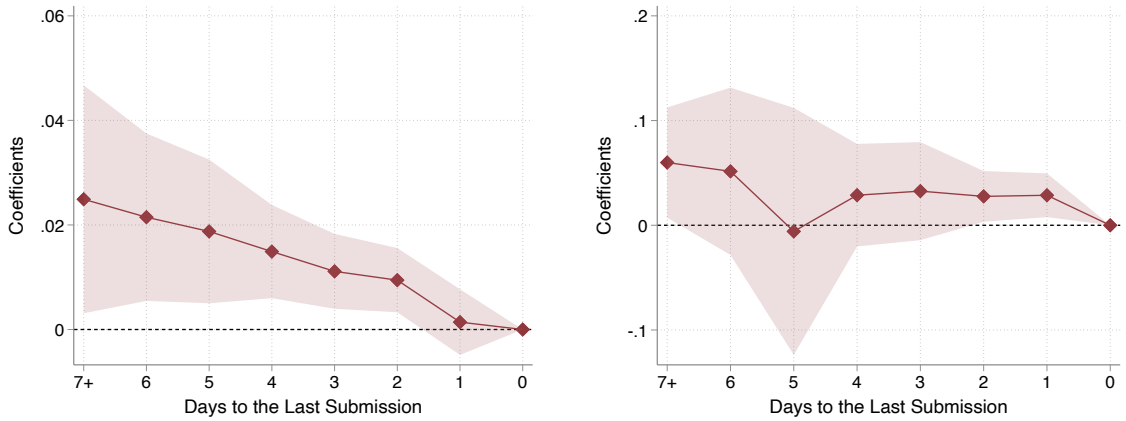
Note: This table presents the effects of early submission measured by days before the last submission and other characteristics on the probability of employer contact within 30 days. Columns titled "Full" include all jobs, while columns titled "<8" include jobs with less than 8 submissions. Column (1) and (2) are on full sample, Column (3) and (4) are on Wave 1 where the variables "Pol. club", "Acad. club", "LGBTQ club", "Typical pron." and "Neutral pron." were not designed to vary, Column (5) and (6) are on Wave 2-5. All regressions include job fixed effects. Standard errors in parentheses are clustered at the job level.

Table A.8: Estimation of Model 3.4, w. Wave and Region Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	All		Wave 1		Wave 2-5	
	Full	< 8	Full	< 8	Full	< 8
$D2L^t$	0.0054	0.0161	0.0056	0.0254	0.0053	-0.0034
	(0.0010)	(0.0051)	(0.0014)	(0.0066)	(0.0014)	(0.0073)
Black	-0.0206	-0.0118	-0.0216	-0.0136	-0.0204	-0.0108
	(0.0017)	(0.0059)	(0.0038)	(0.0099)	(0.0019)	(0.0074)
Female	0.0002	0.0131	0.0045	0.0279	-0.0007	0.0047
	(0.0030)	(0.0106)	(0.0065)	(0.0172)	(0.0034)	(0.0134)
Over 40	-0.0059	0.0063	-0.0024	0.0167	-0.0067	-0.0000
	(0.0030)	(0.0113)	(0.0066)	(0.0195)	(0.0033)	(0.0138)
Pol. club	-0.0018	-0.0436			-0.0018	-0.0431
	(0.0074)	(0.0298)			(0.0074)	(0.0296)
Acad. club	0.0095	0.0184			0.0096	0.0195
	(0.0076)	(0.0339)			(0.0076)	(0.0341)
LGBTQ club	-0.0051	0.0316			-0.0051	0.0310
	(0.0055)	(0.0228)			(0.0055)	(0.0228)
Typical pron.	-0.0140	-0.0181			-0.0139	-0.0168
	(0.0073)	(0.0294)			(0.0073)	(0.0295)
Neutral pron.	-0.0104	-0.0335			-0.0103	-0.0332
	(0.0076)	(0.0301)			(0.0076)	(0.0301)
Assoc. degree	0.0012	0.0180	0.0026	0.0028	0.0009	0.0278
	(0.0030)	(0.0110)	(0.0068)	(0.0196)	(0.0034)	(0.0131)
Constant	0.1902	0.1604	0.1844	0.1274	0.2518	0.1806
	(0.0116)	(0.0326)	(0.0195)	(0.0488)	(0.0130)	(0.0469)
N	83643	5011	14541	1821	69102	3190

Note: This table presents the effects of early submission measured by days before the last submission and other characteristics on the probability of employer contact within 30 days. Columns titled "Full" include all jobs, while columns titled "<8" include jobs with less than 8 submissions. Column (1) and (2) are on full sample, Column (3) and (4) are on Wave 1 where the variables "Pol. club", "Acad. club", "LGBTQ club", "Typical pron." and "Neutral pron." were not designed to vary, Column (5) and (6) are on Wave 2-5. All regressions include region and wave fixed effects. Standard errors in parentheses are clustered at the job level.

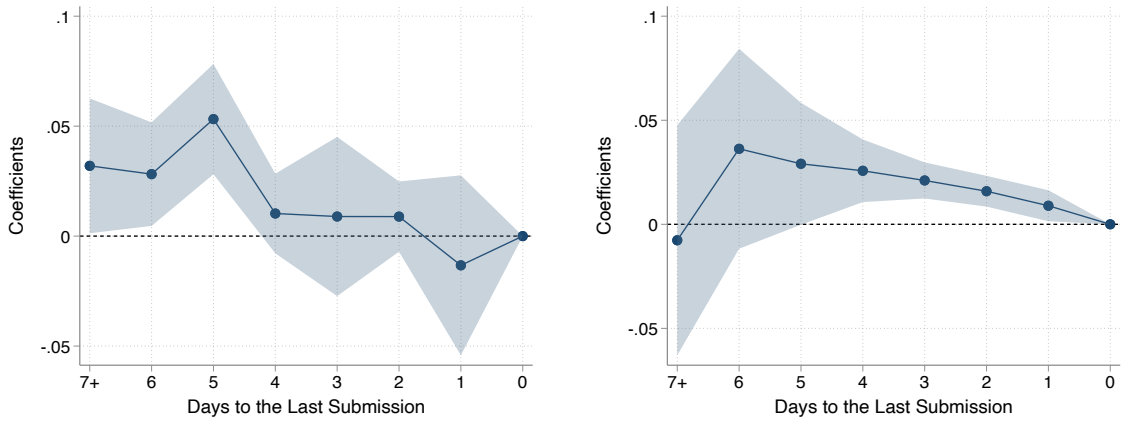
Figure A.7: Contact Gap and $D2L^t$, *Complete* v.s. *Incomplete* Jobs



(a) *Complete* Jobs ($N = 78632$) (b) *Incomplete* Jobs ($N = 5011$)

Notes: The figures plot the estimated coefficients $\hat{\delta}_j, j \in \{1, \cdots, 7\}$ of Model 3.5 with days to the last submission $D2L^t$ on the x-axis, controlling for resume characteristics. The left panel shows full sample results. The right panel, . The regression includes job fixed effects, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals.

Figure A.8: Contact Gap and $D2L^t$, w. Wave and Region Fixed Effects



(a) Wave 1 ($N = 14541$) (b) Wave 2-5 ($N = 69102$)

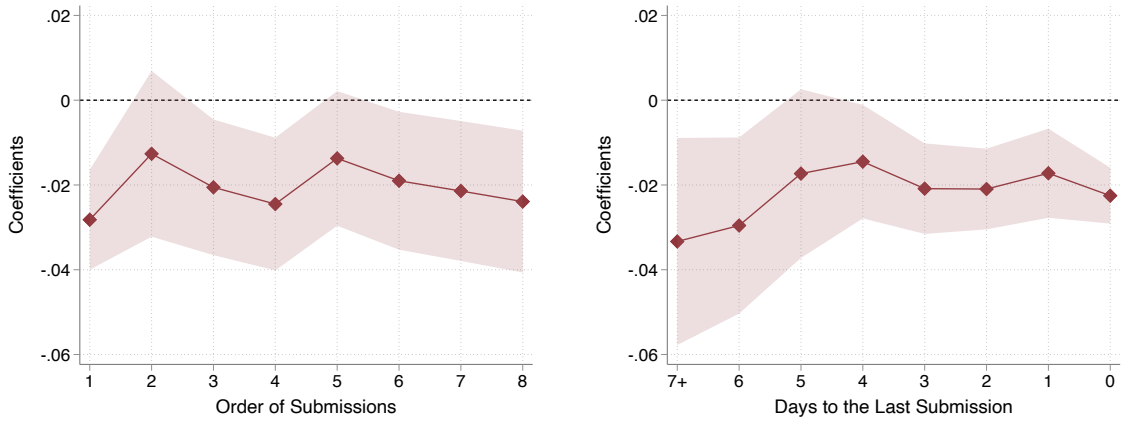
Notes: The figures plot the estimated coefficients $\hat{\delta}_j, j \in \{1, \cdots, 7\}$ of Model 3.5 with days to the last submission $D2L^t$ on the x-axis, controlling for resume characteristics. The left panel shows Wave 1 results. The right panel, shows Wave 2-5 results. The regression includes wave and region fixed effects, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals.

Table A.9: Application Pair Order, Race and Gender

	(1)	(2)	(3)
$\mathbf{1}(Order_i^{pair} = 2)$	-0.0023	-0.0015	0.0055
	(0.0036)	(0.0038)	(0.0052)
$\mathbf{1}(Order_i^{pair} = 3)$	-0.0136	-0.0113	-0.0107
	(0.0039)	(0.0040)	(0.0054)
$\mathbf{1}(Order_i^{pair} = 4)$	-0.0121	-0.0091	-0.0040
	(0.0040)	(0.0043)	(0.0057)
Black	-0.0200	-0.0205	-0.0089
	(0.0032)	(0.0018)	(0.0049)
$\mathbf{1}(Order_i^{pair} = 2) \times$ Black	-0.0026		-0.0142
	(0.0046)		(0.0071)
$\mathbf{1}(Order_i^{pair} = 3) \times$ Black	0.0020		-0.0011
	(0.0046)		(0.0071)
$\mathbf{1}(Order_i^{pair} = 4) \times$ Black	-0.0011		-0.0102
	(0.0046)		(0.0073)
Female	-0.0021	0.0012	0.0123
	(0.0022)	(0.0041)	(0.0057)
$\mathbf{1}(Order_i^{pair} = 2) \times$ Female		-0.0041	-0.0156
		(0.0055)	(0.0078)
$\mathbf{1}(Order_i^{pair} = 3) \times$ Female		-0.0025	-0.0055
		(0.0054)	(0.0077)
$\mathbf{1}(Order_i^{pair} = 4) \times$ Female		-0.0071	-0.0161
		(0.0057)	(0.0081)
Black $\times$ Female			-0.0223
			(0.0078)
$\mathbf{1}(Order_i^{pair} = 2) \times$ Black $\times$ Fem.			0.0232
			(0.0109)
$\mathbf{1}(Order_i^{pair} = 3) \times$ Black $\times$ Fem.			0.0059
			(0.0108)
$\mathbf{1}(Order_i^{pair} = 4) \times$ Black $\times$ Fem.			0.0182
			(0.0111)
Constant	0.2599	0.2584	0.2527
	(0.0032)	(0.0032)	(0.0040)
N		83643	

Note: This table presents the effects of designed submission pair orders interacted with race and gender on the probability of employer contact within 30 days, controlling for other characteristics. $\mathbf{1}(Order_i^{pair} = j), j = 2, 3, 4$ indicates whether application i was designed to be submitted in Pair j. All regressions are on the full sample, including job fixed effects and all resume characteristics. Standard errors in parentheses are clustered at the job level.

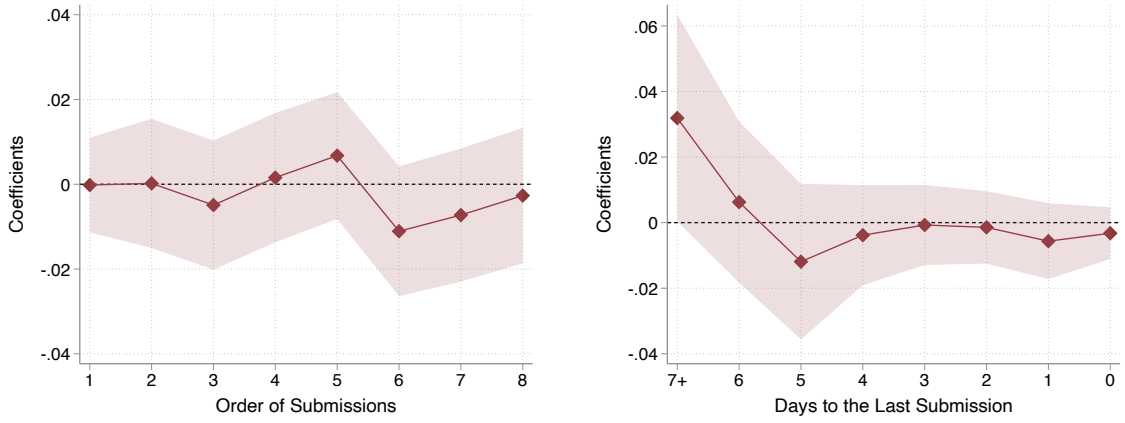
Figure A.9: Race Contact Gap Over Time



(a) $\mathbf{1}\left(Order_i = j\right) \times$ Black (b) $\mathbf{1}\left(D2L_i^t = j\right) \times$ Black

Notes: The figures plot how the race contact gap evolves over time. The left panel plots the estimated coefficients of $(Order_i = j) \times$ Black plus the estimated coefficient of Black for $j = 2, \cdots, 8$ and the coefficient of Black for $j = 1$; The right panel plots the estimated coefficients of $\left(D2L^t = d\right) \times$ Black plus the estimated coefficient of Black for $d = 1, \cdots, 7$ and the coefficient of Black for $d = 0$. The regression includes job fixed effects and all resume characteristics, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals.

Figure A.10: Gender Contact Gap Over Time



(a) $\mathbf{1}\left(Order_i = j\right) \times$ Female (b) $\mathbf{1}\left(D2L_i^t = j\right) \times$ Female

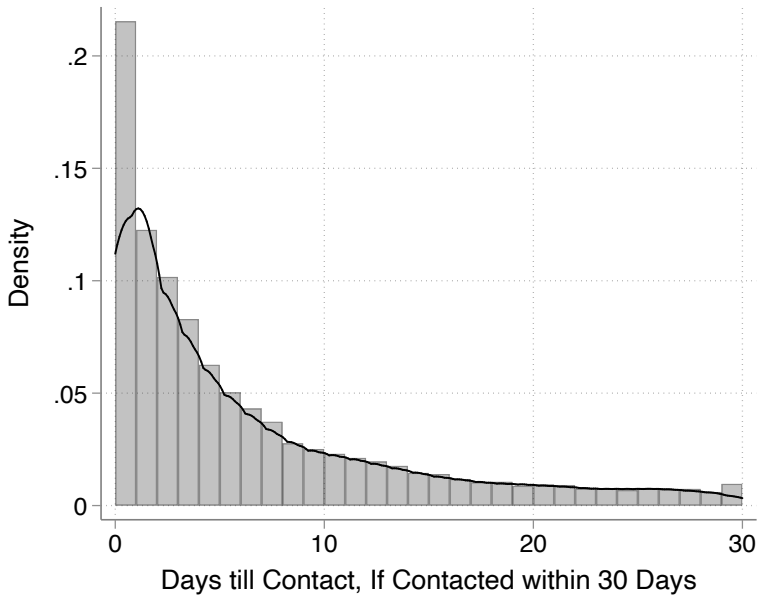
Notes: The figures plot how the gender contact gap evolves over time. The left panel plots the estimated coefficients of $(Order_i = j) \times$ Female plus the estimated coefficient of Female for $j = 2, \cdots, 8$ and the coefficient of Female for $j = 1$; The right panel plots the estimated coefficients of $\left(D2L^t = d\right) \times$ Female plus the estimated coefficient of Female for $d = 1, \cdots, 7$ and the coefficient of Female for $d = 0$. The regression includes job fixed effects and all resume characteristics, the standard errors are clustered at the job level. The area depicts the 95% confidence intervals.

Table A.10: Order of Submission Pairs and Resume characteristics

	(1) Pair 1 - 2	(2) Pair 1 - 3	(3) Pair 1 - 4	(4) Pair 2 - 3	(5) Pair 2 - 4	(6) Pair 3 - 4
Black	0.0132	-0.0024	0.0135	-0.0118	-0.0041	0.0059
	(0.0078)	(0.0080)	(0.0083)	(0.0080)	(0.0085)	(0.0080)
Female	0.0132	0.0014	0.0168	-0.0091	0.0010	0.0062
	(0.0087)	(0.0090)	(0.0095)	(0.0089)	(0.0094)	(0.0088)
Black $\times$ Female	-0.0216	0.0012	-0.0242	0.0150	0.0054	-0.0059
	(0.0121)	(0.0127)	(0.0131)	(0.0127)	(0.0132)	(0.0124)
Over 40	0.0037	0.0038	-0.0043	-0.0001	-0.0076	-0.0012
	(0.0061)	(0.0064)	(0.0067)	(0.0063)	(0.0067)	(0.0063)
Pol. club	-0.0086	0.0077	-0.0036	0.0180	0.0172	-0.0058
	(0.0168)	(0.0169)	(0.0181)	(0.0160)	(0.0169)	(0.0159)
Acad. club	0.0156	-0.0035	-0.0104	0.0028	-0.0145	-0.0123
	(0.0159)	(0.0168)	(0.0174)	(0.0155)	(0.0171)	(0.0160)
LGBTQ club	0.0030	-0.0019	-0.0187	0.0006	-0.0130	-0.0098
	(0.0109)	(0.0118)	(0.0120)	(0.0115)	(0.0121)	(0.0114)
Typical pron.	-0.0004	-0.0104	-0.0028	-0.0087	-0.0003	0.0164
	(0.0164)	(0.0166)	(0.0171)	(0.0165)	(0.0167)	(0.0159)
Neutral pron.	0.0000	0.0035	0.0120	0.0056	0.0103	0.0025
	(0.0158)	(0.0164)	(0.0173)	(0.0151)	(0.0178)	(0.0154)
Assoc. degree	-0.0079	-0.0044	-0.0035	0.0036	0.0008	-0.0019
	(0.0063)	(0.0066)	(0.0068)	(0.0063)	(0.0067)	(0.0063)
Constant	-0.0029	0.0126	0.0086	0.0138	0.0140	-0.0018
	(0.0076)	(0.0079)	(0.0084)	(0.0077)	(0.0085)	(0.0078)
N	43433	42627	41873	41770	41016	40210
$F-$ stats, joint	0.70	0.17	0.76	0.42	0.52	0.34
$p-$value	0.7237	0.9980	0.6644	0.9376	0.8798	0.9704

Note: This table presents the effects of designed submission pairs interacted with resume characteristics on the probability of employer contact within 30 days. Column (1)-(3) display the differences of between submissions of Pair 2 to 4 and Pair 1; Column (4)-(5) display the differences of between submissions of Pair 3 to 4 and Pair 2; Column (6) displays the differences of between submissions of Pair 3 and Pair 4. All regressions include job fixed effects. Standard errors in parentheses are clustered at the job level. χ^2 statistics and joint $p-$values come from tests that all differences in reported coefficients other than the constant term are zero.

Figure A.11: Days till Contact, If Contacted



Notes: The figure plots the distribution of days from submission to contact given a submission was contacted within 30 days after submission. Epanechnikov kernel is used, the kernel half-width is set as 1.

Table A.11: Number of Submissions Contacted before A Submission

Order	N	NoC^{up}	NoC^{low}	NoC^{alt}
1	11114	0.0501	0	0
2	11031	0.0935	0.0154	0.2451
3	10682	0.2322	0.1127	0.4936
4	10606	0.2626	0.1355	0.7362
5	10278	0.4558	0.3070	0.9876
6	10204	0.5025	0.3410	1.2249
7	9899	0.7153	0.5359	1.4757
8	9829	0.7703	0.5913	1.7177

Note: This table summarize the number of submissions already contacted when a submission was made, by the order of that submission. In Column titled NoC^{up}, other submissions contacted no later than the day of the current submission; In Column titled NoC^{low}, only other submissions contacted at least one day before the current submission are included; In Column titled NoC^{alt}, all submissions before the current submission are included. Order is constructed with the date and time of actual submissions.

Table A.12: Construction of Contact History, Example

Order	Date: Submission	Date: Contact	Contacted	NoC^{up}	NoC^{low}	NoC^{alt}
1	10/8/19	10/8/19	1	1	0	0
2	10/8/19	10/8/19	1	1	0	1
3	10/12/19		0	3	2	2
4	10/12/19	10/12/19	1	2	2	2
5	10/14/19	10/15/19	1	3	3	3
6	10/14/19	10/15/19	1	3	3	4
7	10/16/19		0	5	5	5
8	10/16/19	10/20/19	1	5	5	5

Note: This table presents an example of the construction of the three contact history measures (job ID: 80). NoC^{up} is calculated including all other submissions contacted no later than the day of the current submission; NoC^{low} is calculated including other submissions contacted at least one day before the current submission; NoC^{alt} is calculated including all submissions ordered before the current submission. Order is constructed with the date and time of actual submissions.

Table A.13: Number of Contacted Submissions and Timing Measures

	NoC_i^{up}	NoC_i^{low}	$early^1$	$early^2$	$early^3$	$Order$	$D2L^t$
NoC_i^{up}	1						
NoC_i^{low}	0.8843	1					
$early^1$	-0.1899	-0.2048	1				
$early^2$	-0.1859	-0.2006	0.9786	1			
$early^3$	-0.1850	-0.1993	0.9569	0.9710	1		
$Order$	0.2238	0.2385	-0.8164	-0.8076	-0.8006	1	
$D2L^t$	-0.1747	-0.1790	0.7169	0.7195	0.7146	-0.7406	1

Note: This table summarize the pairwise correlation between the constructed measures. NoC_i^{up} refers to the number of other submissions already contacted no later than the day a submission was made; NoC_i^{low} refers to that while only including other submissions contacted at least one day before. $early^1$, $early^2$, $early^3$ are the 3 constructed early submission dummies; Order is constructed with the date and time of actual submissions; $D2L^t$ is the number of days before the last submission to a job. All coefficients are significant at 1% level.

Table A.14: Prior Contact History and Early Submission Advantages

	(1)	(2)	(3)	(4)	(5)	(6)
	Model 3.1		Model 3.2		Model 3.4	
NoC^{up}	-0.0435		-0.0445		-0.0440	
	(0.0024)		(0.0024)		(0.0024)	
NoC^{low}		-0.0574		-0.0587		-0.0581
		(0.0028)		(0.0029)		(0.0029)
$early^1$	-0.0078	-0.0099				
	(0.0022)	(0.0022)				
$\mathbf{1}(Order = 2)$			0.0001	-0.0010		
			(0.0032)	(0.0032)		
$\mathbf{1}(Order = 3)$			0.0018	0.0004		
			(0.0035)	(0.0035)		
$\mathbf{1}(Order = 4)$			0.0060	0.0045		
			(0.0036)	(0.0036)		
$\mathbf{1}(Order = 5)$			0.0041	0.0041		
			(0.0038)	(0.0038)		
$\mathbf{1}(Order = 6)$			0.0057	0.0056		
			(0.0038)	(0.0038)		
$\mathbf{1}(Order = 7)$			0.0163	0.0182		
			(0.0038)	(0.0039)		
$\mathbf{1}(Order = 8)$			0.0155	0.0182		
			(0.0039)	(0.0039)		
$D2L^t$					-0.0030	-0.0038
					(0.0007)	(0.0007)
N	83643					

Note: This table presents the effects of prior contact histories and early submission, controlling for resume characteristics on the probability of employer contact within 30 days. NoC_i^{up} refers to the number of other submissions already contacted no later than the day when a submission was made; NoC_i^{low} refers to the number of other submissions already contacted at least one day before a submission was made. $early^1$ is the constructed early submission dummy; $\mathbf{1}(Order = j)$, $j = 2, \cdots, 8$ are the indicators of the submission orders; $D2L^t$ is the number of days before the last submission to a job. All regressions include job fixed effects. Standard errors in parentheses are clustered at the job level.

Table A.15: Prior Contact History and Resume Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
		NoC^{up}			NoC^{low}	
	$= 0$	$= 1$	$(2) - (1)$	$= 0$	$= 1$	$(5) - (4)$
Black	-0.0140	-0.0215	-0.0075	-0.0149	-0.0200	-0.0051
	(0.0026)	(0.0095)	(0.0099)	(0.0026)	(0.0111)	(0.0114)
Female	0.0019	0.0088	0.0069	0.0007	0.0179	0.0172
	(0.0029)	(0.0105)	(0.0108)	(0.0030)	(0.0120)	(0.0123)
Black $\times$ Female	-0.0075	-0.0289	-0.0214	-0.0071	-0.0359	-0.0288
	(0.0038)	(0.0148)	(0.0152)	(0.0038)	(0.0171)	(0.0175)
Over 40	-0.0027	-0.0122	-0.0095	-0.0037	-0.0066	-0.0029
	(0.0020)	(0.0079)	(0.0082)	(0.0020)	(0.0090)	(0.0092)
Pol. club	0.0080	-0.0049	-0.0129	0.0071	0.0006	-0.0066
	(0.0051)	(0.0189)	(0.0196)	(0.0051)	(0.0218)	(0.0223)
Acad. club	0.0044	0.0081	0.0037	0.0040	0.0143	0.0103
	(0.0050)	(0.0179)	(0.0186)	(0.0051)	(0.0201)	(0.0207)
LGBTQ club	-0.0045	0.0265	0.0310	-0.0034	0.0299	0.0333
	(0.0036)	(0.0132)	(0.0137)	(0.0036)	(0.0159)	(0.0163)
Typical pron.	-0.0059	-0.0178	-0.0119	-0.0082	0.0010	0.0092
	(0.0052)	(0.0177)	(0.0184)	(0.0051)	(0.0204)	(0.0210)
Neutral pron.	-0.0085	-0.0089	-0.0003	-0.0085	0.0002	0.0087
	(0.0050)	(0.0187)	(0.0193)	(0.0050)	(0.0221)	(0.0226)
Assoc. degree	0.0008	0.0082	0.0073	0.0023	-0.0044	-0.0067
	(0.0020)	(0.0074)	(0.0077)	(0.0020)	(0.0086)	(0.0089)
Constant	0.2638	0.1382	-0.1256	0.2591	0.0818	-0.1773
	(0.0034)	(0.0120)	(0.0127)	(0.0033)	(0.0131)	(0.0133)
N	71662	11981	83643	75074	8569	83643
$F-$ stats, joint			1.77			1.44
$p-$value			0.0598			0.1563

Note: This table presents the effects of the prior contact history interacted with resume characteristics on the probability of employer contact within 30 days. NoC_i^{up} refers to the number of other submissions already contacted no later than the day when a submission was made; NoC_i^{low} refers to the number of other submissions already contacted at least one day before a submission was made. Column (3) displays the differences of between Column (2) and Column (1). Column (6) displays the differences of between Column (5) and Column (4). All regressions include job fixed effects and submission order fixed effects. Standard errors in parentheses are clustered at the job level. χ^2 statistics and joint $p-$values come from tests that all differences in reported coefficients other than the constant term are zero.

Table A.16: Race-Specific Contact History Dummy

	(1)	(2)	(3)	(4)	(5)	(6)
	History : $\mathbf{1}\left(NoC^{up} > 0\right)$			History : $\mathbf{1}\left(NoC^{low} > 0\right)$		
Black	-0.0189	-0.0175	-0.0179	-0.0193	-0.0183	-0.0185
	(0.0019)	(0.0018)	(0.0018)	(0.0018)	(0.0018)	(0.0018)
History$^{\text{Black}}$	-0.1295		-0.0689	-0.1803		-0.1054
	(0.0094)		(0.0117)	(0.0102)		(0.0144)
History$^{\text{Black}}$ $\times$ Black	-0.0386		-0.0449	-0.0230		-0.0120
	(0.0073)		(0.0140)	(0.0082)		(0.0144)
History$^{\text{White}}$		-0.1521	-0.1183		-0.1777	-0.1086
		(0.0094)	(0.0110)		(0.0098)	(0.0138)
History$^{\text{White}}$ $\times$ Black		-0.0012	0.0222		-0.0189	-0.0121
		(0.0072)	(0.0137)		(0.0080)	(0.0140)
N	83643					

Note: This table presents the effects of race-specific prior contact histories, controlling for resume characteristics on the probability of employer contact within 30 days. In Column (1) - (3), History refers to $\mathbf{1}\left(NoC_i^{up} > 0\right)$, the indicator of positive number of submissions already contacted no later than the day when a submission was made, by race (Black/White); In Column (4) - (6), History refers to $\mathbf{1}\left(NoC_i^{low} > 0\right)$, the indicator of positive number of submissions already contacted at one day before a submission was made, by race (Black/White). All regressions include submission order fixed effects and job fixed effects. Standard errors in parentheses are clustered at the job level.

Table A.17: Gender-Specific Contact History Dummy

	(1)	(2)	(3)	(4)	(5)	(6)
	History : $\mathbf{1}\left(NoC^{up} > 0\right)$			History : $\mathbf{1}\left(NoC^{low} > 0\right)$		
Female	-0.0058	0.0005	-0.0031	-0.0056	-0.0003	-0.0035
	(0.0022)	(0.0022)	(0.0022)	(0.0022)	(0.0022)	(0.0022)
History$^{\text{Female}}$	-0.1515		-0.1318	-0.1941		-0.1457
	(0.0102)		(0.0121)	(0.0109)		(0.0131)
History$^{\text{Female}}$ × Female	0.0094		0.0324	0.0127		0.0197
	(0.0092)		(0.0129)	(0.0106)		(0.0138)
History$^{\text{Male}}$		-0.1205	-0.0572		-0.1685	-0.0887
		(0.0105)	(0.0122)		(0.0111)	(0.0134)
History$^{\text{Male}}$ × Female		-0.0062	-0.0323		-0.0023	-0.0137
		(0.0091)	(0.0127)		(0.0104)	(0.0136)
N	83643					

Note: This table presents the effects of gender-specific prior contact histories, controlling for resume characteristics on the probability of employer contact within 30 days. In Column (1) - (3), History refers to $\mathbf{1}\left(NoC_i^{up} > 0\right)$, the indicator of positive number of submissions already contacted no later than the day when a submission was made, by gender (female/male); In Column (4) - (6), History refers to $\mathbf{1}\left(NoC_i^{low} > 0\right)$, the indicator of positive number of submissions already contacted at one day before a submission was made, by gender (female/male). All regressions include submission order fixed effects and job fixed effects. Standard errors in parentheses are clustered at the job level.

Table A.18: Test Alternative Prior Contact History NoC^{alt}

	(1)	(2)	(3)	(4)	(5)	(6)
NoC^{alt}	-0.0377	-0.0411	-0.0375	-0.0411	-0.0386	-0.0414
	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0015)	(0.0016)
Black				-0.0202	-0.0160	-0.0202
				(0.0018)	(0.0016)	(0.0018)
$NoC^{alt} \times$ Black					-0.0050	
					(0.0012)	
Female				-0.0020	-0.0020	-0.0023
				(0.0022)	(0.0022)	(0.0020)
$NoC^{alt} \times$ Female						0.0004
						(0.0015)
$early^1$	-0.0250					
	(0.0019)					
$\mathbf{1}(Order = 2)$		0.0082				
		(0.0031)				
$\mathbf{1}(Order = 3)$		0.0141				
		(0.0034)				
$\mathbf{1}(Order = 4)$		0.0268				
		(0.0035)				
$\mathbf{1}(Order = 5)$		0.0266				
		(0.0036)				
$\mathbf{1}(Order = 6)$		0.0359				
		(0.0035)				
$\mathbf{1}(Order = 7)$		0.0471				
		(0.0035)				
$\mathbf{1}(Order = 8)$		0.0538				
		(0.0035)				
$D2L^t$			-0.0079			
			(0.0007)			
N				83643		

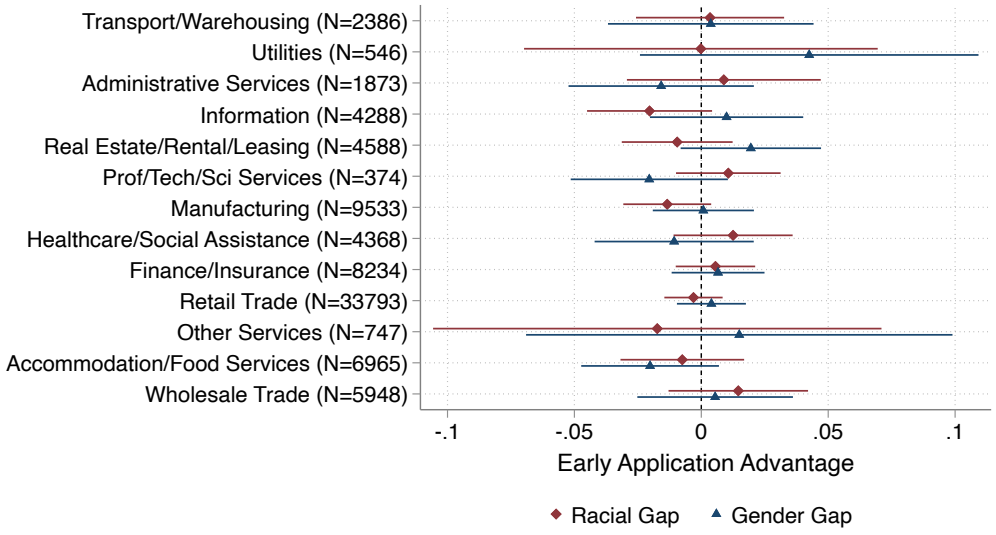
Note: This table presents the effects of prior contact histories and early submission, controlling for resume characteristics on the probability of employer contact within 30 days. NoC_i^{alt} refers to the number of submissions before the current submission. $early^1$ is the constructed early submission dummy; $\mathbf{1}(Order = j)$, $j = 2, \cdots, 8$ are the indicators of the submission orders; $D2L^t$ is the number of days before the last submission to a job. All regressions include job fixed effects. All regressions in Column (4) to (6) include submission order fixed effects as well. Standard errors in parentheses are clustered at the job level.

Table A.19: Group-Specific Contact History, Alternative Measure

	(1)	(2)	(3)	(4)	(5)	(6)
	History : NoC^{alt}			History : $\mathbf{1}\left(NoC^{alt} > 0\right)$		
Panel A: Race-specific contact history						
Black	-0.0205	-0.0124	-0.0153	-0.0202	-0.0085	-0.0125
	(0.0018)	(0.0016)	(0.0016)	(0.0018)	(0.0016)	(0.0015)
History$^{\text{Black}}$	-0.0592		-0.0366	-0.1160		-0.0909
	(0.0027)		(0.0053)	(0.0073)		(0.0096)
History$^{\text{Black}}$ × Black	-0.0202		0.0048	-0.0256		0.0055
	(0.0026)		(0.0066)	(0.0055)		(0.0101)
History$^{\text{White}}$		-0.0721	-0.0414		-0.1304	-0.0922
		(0.0032)	(0.0061)		(0.0069)	(0.0086)
History$^{\text{White}}$ × Black		0.0018	-0.0120		-0.0215	-0.0322
		(0.0026)	(0.0065)		(0.0054)	(0.0100)
Panel B: Gender-specific contact history						
Female	-0.0119	0.0071	-0.0028	-0.0115	0.0059	-0.0036
	(0.0021)	(0.0021)	(0.0020)	(0.0021)	(0.0021)	(0.0019)
History$^{\text{Female}}$	-0.0618		-0.0517	-0.1386		-0.1343
	(0.0028)		(0.0035)	(0.0075)		(0.0087)
History$^{\text{Female}}$ × Female	0.0060		0.0172	0.0229		0.0561
	(0.0026)		(0.0034)	(0.0066)		(0.0092)
History$^{\text{Male}}$		-0.0537	-0.0307		-0.0998	-0.0562
		(0.0030)	(0.0036)		(0.0074)	(0.0083)
History$^{\text{Male}}$ × Female		-0.0051	-0.0166		-0.0207	-0.0545
		(0.0028)	(0.0036)		(0.0066)	(0.0092)
N	83643					

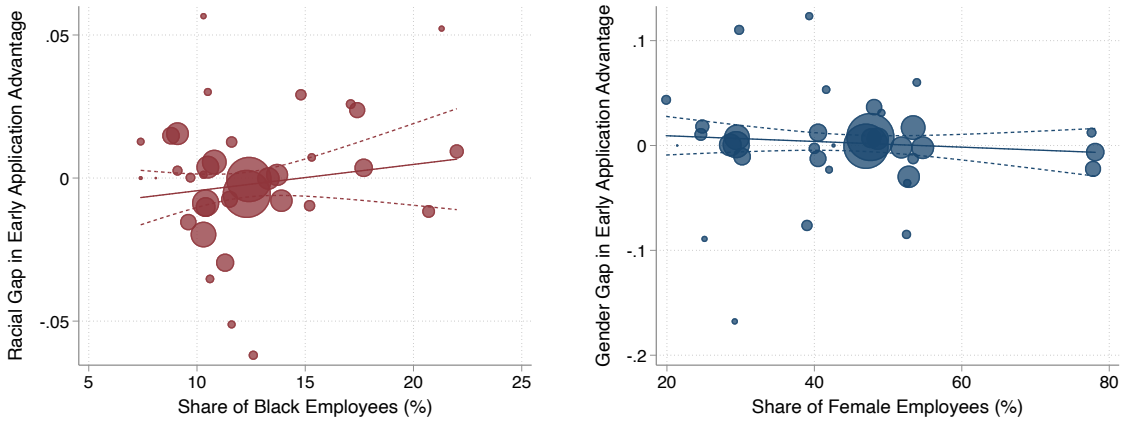
Note: This table presents the effects of group-specific prior contact histories with the alternative measure based on submission order, controlling for resume characteristics on the probability of employer contact within 30 days. In Column (1) - (3), History refers to NoC^{alt}, the number of applications submitted prior to a submission was made, by race (Black/White) or gender (female/male); In Column (4) - (6), History refers to $\mathbf{1}\left(NoC_i^{alt} > 0\right)$, the indicator of positive number of applications submitted prior to a submission was made, by race (Black/White) or gender (female/male). All regressions include submission order fixed effects and job fixed effects. Standard errors in parentheses are clustered at the job level.

Figure A.12: Racial/Gender Gap in Early Application Advantage by Industry



Notes: The figure plots the estimated coefficients of the interaction term of the early indicator $early^1$ which equals 1 for the first four applications to each job and the Black (in red)/female applicant indicator (in blue) in Model 3.1, controlling for resume characteristics. The regressions include job fixed effects, the standard errors are clustered at the job level. The line depicts the 95% confidence intervals. The nubmer of applications in each industry is reported in parentheses.

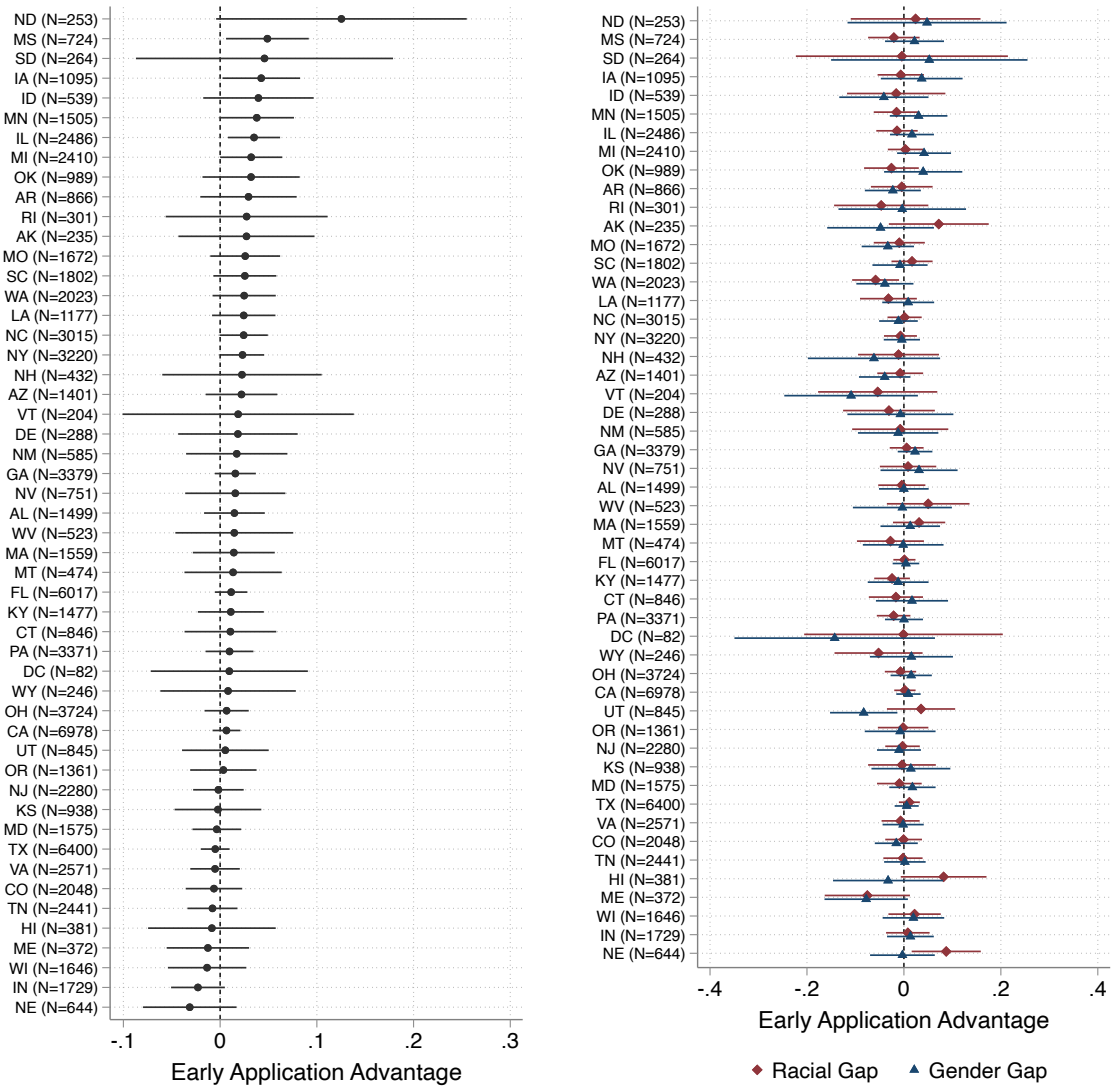
Figure A.13: Early Application Advantage and Industry Demographics



(a) Racial Gap (b) Gender Gap

Notes: The figures plot the gender and race gap in the early application advantage against the share of Black/female employees in an industry in the year prior to the year of first submission to a job in the experiment. In both panels, each dot represents an industry-year unit, weighted by the number of submissions in that unit during the experiment. The racial/gender gap is estimated as in Column (1) and (2) of Table 5 for each industry-year unit. All regressions include job fixed effects and resume characteristics.

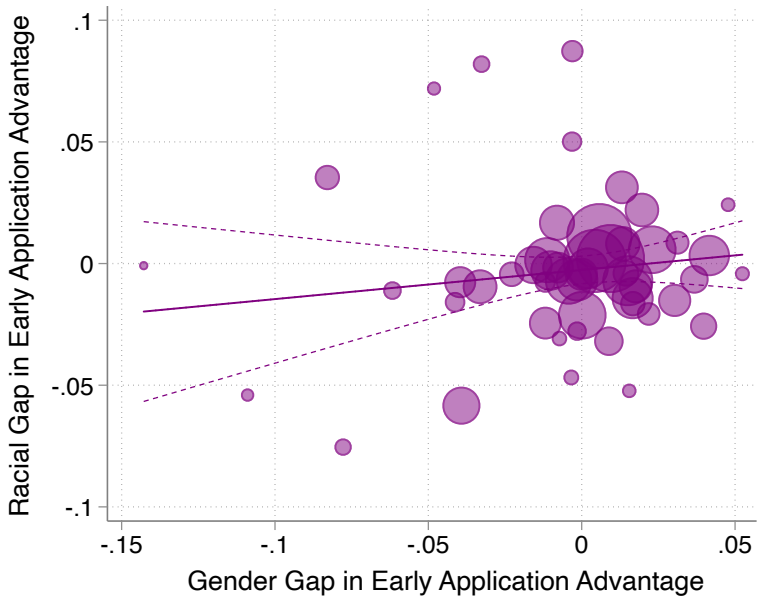
Figure A.14: Early Application Advantage by State



(a) State-Level Heterogeneity (b) Gender/Racial Gap Heterogeneity

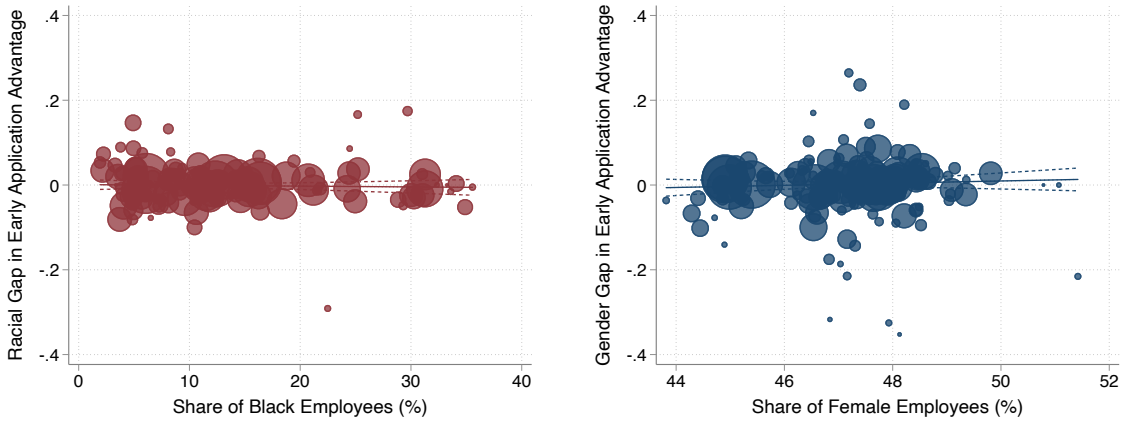
Notes: The figures plot the geographic heterogeneity of the early application advantage at the state level. The left panel plots the estimated coefficients $\hat{\beta}$ of Model 3.1 with the early indicator $early^1$ which equals 1 for the first four applications to each job; The right panel plots the estimated coefficients of the interaction term of $early^1$ and the Black (in red)/female applicant indicator (in blue) in Model 3.1. All regressions include job fixed effects and resume characteristics, the standard errors are clustered at the job level. The line depicts the 95% confidence intervals. The nubmer of applications in each state is reported in parentheses.

Figure A.15: Racial/Gender Gap in Early Application Advantage by State



Notes: The figure plots the racial gaps and gender gaps in the early application advantage in each state as estimated in Figure A.14b. The size of points reflects the size of submissions in a state in the experiment.

Figure A.16: Early Application Advantage and State Demographics



(a) Racial Gap (b) Gender Gap

Notes: The figures plot the gender and race gap in the early application advantage against the share of Black/female employees in a state in the year prior to the year of first submission to a job in the experiment. In both panels, each dot represents a state-year unit, weighted by the number of submissions in that unit during the experiment. The racial/gender gap is estimated as in Column (1) and (2) of Table 5 for each state-year unit. All regressions include job fixed effects and resume characteristics.

Table A.20: Early Application Advantage and State Minimum Wages

	(1)	(2)	(3)
$early^1 \times$ Minimum Wages	0.0002	-0.0007	
	(0.0010)	(0.0014)	
$early^1 \times \mathbf{1}$ (State Min. Wages)			0.0040
			(0.0048)
N	83643	51968	83643

Note: This table reports the estimated coefficents of the interaction term of the early application indicator and the covariates in Model 6.1. In Column (1), the covariate is the minimum wages of states; In Column (2), the covariate is the mininum wages of states, the sample is all states that have a minimum wage different from the federal minimum wage; In Column (3), the covariate is an indicator of states with minimum wages different from the federal minimum wage. All regressions include job fixed effects and resume characteristics. Standard errors are clustered at the job level.

B Details of Complementary Data

This appendix section details how the complementary data used for the heterogeneity analysis in Section 6 were sourced and constructed.

B.1 Industry-Level Covariates

The industries are defined by the North American Industry Classification System (NAICS) 2-digit codes. In the original data released by Kline, Rose and Walters (2022), industries are defined by Standard Industrial Classification (SIC) 2-digit codes. I recover the 2-digit NAICS codes for firms in two steps: First, I use the 6-digit NAICS codes listed on Compustat for firms to generate the 2-digit industry codes. For firms not listed on Compustat, I use the 2017 NAICS-SIC crosswalk to map the 2-digit SIC codes to 2-digit NAICS codes.

Industry task measures Following Kline, Rose and Walters (2022), I use the Occupation Information Network (O*NET) database to construct task-based characteristics of U.S. industries. The ONET database provides a full list of occupations for each NAICS-2 industry. For each occupation, the database includes a projected number of job openings and the proportion of employment of that occupation within the industry. I calculate industry-level task measures as the weighted sum of occupation-level task measures, using the projected number of employment for each occupation in the industry as weights.

Five task-based measures are constructed to reflect the importance of the *abilities*, *skills*, *knowledge* required to perform the work, the typical *activites* and the *context* of a job:

- **Analytical**: This measure is the average importance of 3 components: mathematical reasoning ability, mathematics knowledge, and mathematics skills. These variables measure how much mathematical skills and knowledge are required to perform the work.

- **Customer interaction**: This measure is the average importance of 4 components: 2 activities variables, establishing and maintaining interpersonal relationships and performing for or working directly with the public, 1 knowledge variable, customer and personal service, and 1 context variable contact

with others. These variables measure how much an industry requires workers to be in contact with others to perform their job.

- **Social**: This measure is the average importance of 4 skill components: social perceptiveness, coordination, persuasion, and negotiation. These variables measure to what extent developed capacities are used to work with people.
- **Routine**: This measure is the average importance of 2 context components: degree of automation and importance of repeating same tasks. These variables measure the relative amounts of routine work performed as part of a job.
- **Service**: The service measure is the average importance of 2 components: 1 activity variable, assisting and caring for others; 1 skill variable service orientation. These variables measure how important assisting people is for performing a job.

All variables are on a scale of 1 to 5. To better match the jobs sampled in the experiments, I focus on entry-level jobs, classified in the O*NET database as those in Job Zone 1 (little to no preparation needed) and 2 (some preparation needed). These jobs mostly do not require an associate degree or job-specific experiences. The five constructed measures are treated as time-invariant.

Industry employment by race and gender To understand whether the race and gender composition of employment affects the racial and gender gaps in early application advantages, I use the Labor Force Statistics on employment by industry, gender, and race from the Current Population Survey. For each sampled job, I use the share of Black and female employment in industries one year prior to the year when the first application was submitted to capture the industry level pre-experiment race and gender composition of workers.

Monthly industry employment measures To better capture the dynamics of labor market conditions by industry, I use three monthly industry employment measures:

- **Employment size**: The seasonally adjusted monthly employment size of each industry, obtained from the U.S. Bureau of Labor Statistics.

- **Compensation index**: The quarterly constant dollar index of *total compensation* and *total wages/salaries* for private-industry workers in all occupations of each industry, obtained from the Employment Cost Index. Both indices represent the change in compensation costs over time relative to a base period. The data are quarterly, and transformed into monthly for analysis.

For each sampled job, I calculate the rolling average of the three variables over the 3 months or 6 months prior to the month when the first application was submitted in the experiment to proxy the labor market conditions in the industry of that job at the start of the experimental submission procedure. I use the natural log of the average employment size for analysis.

B.2 State-Level Covariates

I constructed three groups of state-level covariates: (1) income and mobility measures prior to the start of the experiment, (2) yearly minimum wage regulations, and (3) monthly labor market conditions.

Income and mobility Leveraging the granular annual income and mobility data from the Income Distributions and Dynamics in America (IDDA) for each state[4], I calculate the 2015-2019 averages of the following measures as pre-experiment income and mobility condition proxies for each state:

- **Median income and wages**: I use the median total income of the population and calculate the median wages of workers as the difference between the median total income and the median total non-wage income in each state. All variables are seasonally adjusted.

- **Income mobility**: IDDA provides a detailed yearly transition matrix for the population with respect to the income bins $b \in \{< 25\text{pct.}, 25-50\text{pct.}, 50-75\text{pct.}, > 75\text{pct.}\}$. To construct a measure of mobility, I assign a value of n for moving by n bins, e.g., moving from < 25pct. to $25-50$pct. is assigned 1, while moving from > 75pct. to < 25pct. is assigned -3. I then calculate the expected value of mobility as $\sum \Pr\left((b_t, b_{t-1}) \mid b_{t-1}\right) \cdot n\left(b_t, b_{t-1}\right)$, where

[4] I use variables constructed from household income and earnings data aggregated from Form 1040. For more details, see Kondo et al. (2023).

$\Pr((b_t, b_{t-1}) \mid b_{t-1})$ is the probability of moving from income bin b_{t-1} to b_t during year t, conditional on being in income bin b_{t-1} in year $t-1$. $n(b_t, b_{t-1})$ is the assigned value of moving. This procedure generates a mobility measure for each income bin b for each year.

Table B.1: Summary of Firm-Level Covariates

	Mean	SD	Min	Max
Income and Wage				
Median Income (log)	11.3232	0.1630	11.0154	11.6375
Median Wage (log)	11.2806	0.1557	10.9964	11.5965
Mobility by Income Bins				
Mobility: < 25pct.	0.3707	0.0397	0.3052	0.4971
Mobility: $25 - 50$pct.	0.1181	0.0174	0.0948	0.1655
Mobility: $50 - 75$pct.	-0.0967	0.0095	-0.1194	-0.0748
Mobility: > 75pct.	-0.3343	0.0402	-0.4357	-0.2806

Note: This table summarizes state-level covariates for the 51 states in the sample. Data are sourced from IDDA 1040 (household) data.

State minimum wage To capture state-level labor regulations, I use the list of changes in basic minimum wages in non-farm employment under state law made public by the U.S. Department of Labor. For states without a minimum wage, I use the federal minimum wage instead.

State employment by race and gender Similar to the covariate of industry employment by race and gender, I use the Expanded State Employment Status data from the Local Area Unemployment Statistics. For each sampled job, I use the share of Black and female employment in the state where the job is located one year prior to the year when the first application was submitted to capture the state-level pre-experiment race and gender composition of workers.

State labor market conditions The last set of state-level covariates are sourced from the Job Openings and Labor Turnover Survey (JOLTS). JOLTS provides a rich set of variables and state-month level estimations[5]. I include the following covariates for the non-farm employment in the analysis:

[5]For the state-month level estimation methodology and more details of JOLTS, see offical page

- **Job opening rate**: The number of positions that are open on the last business day of the month divided by the sum of employment and job openings.
- **Hire rate**: The number of additions to the payroll during the month divided by employment.
- **Total separation rate**: The number of employees separated from the payroll during the calendar month divded by employment. I also include two subcategories of separations: **quit rate**, defined as the number of employees who left voluntarily except for retirements and transfers to other locations, divided by employment; and **layoff/discharge rate**, defined as the number of involuntary separations initiated by the employer, divided by employment.
- **Tightness**: The number of unemployed persons per job opening.

For each sampled job, I consider the six labor market measures for the state where the job is located and the month of the first application submitted in the experiment. I also calculate the average of these measures over the three months in that state prior to the first submission as proxies for pre-experiment conditions.

B.3 Firm-Level Covariates

For the 108 sampled firms, I collect a wide range of covariates to capture aspects of human resource management, financial performance, employee satisfaction, and labor market power[6]:

Human resource management I use the following measures to capture the human resource management practices of the sample firms:

- 1 (**federal contractor**): the indicator for a firm being a federal contractor.
- **Labor law violations per employee** *(std.)*: I collect the total count of employment related law violations since 2000 from the Good Job First violation tracker, and divide it by total employment from Compustat.

[6]Covariates marked with *(std.)* are normalized to have a standard deviation of 1 in the analysis.

- **Contact centralization**: This measure was constructed by Kline, Rose and Walters (2022), defined as

$$\frac{\text{total number of unique phone numbers from a firm to contact applicants}}{\text{total number of jobs where at least one contact was made} - 1}$$

  This ratio serves as a proxy for the concentration of hiring responsibility within the firm.

- **HR per employee** *(std.)*: For each sampled firm, I collect the total number of LinkedIn profiles associated with the firm that hold a human resource management-related position and divide it by the total number of LinkedIn profiles listing the firm as the employer. The search was conducted in July 2024. The human resource service provider ADP was excluded.

Employee satisfaction To construct proxies for employee satisfaction, I follow Kline, Rose and Walters (2022) and collect three ratings from GlassDoor.com in July 2024 for each firm:

- **GD scores** *(std.)*: the overall rating of a firm (scale 1-5)
- **GD diversity scores** *(std.)*: the diversity rating of a firm (scale 1-5)
- **GD interview difficulty scores** *(std.)*:: the interview diff (scale 1-5)

Financial performance Following Kline, Rose and Walters (2022), I sourced the annual financial performance data from Compustat:

- **Log employment** *(std.)*: the nature log of the 2010-2018 average employment.
- **Total assets per employee** *(std.)*: total sales divided by employment, averaged over 2010-2018.
- **Gross profit per employee** *(std.)*: gross profit divided by employment, averaged over 2010-2018.
- **Total revenue per employee** *(std.)*: total revenue divided by employment, averaged over 2010-2018.

The firms that do not have Compustat data are omitted.

Within-state labor market power Lastly, I leverage the LightCast pre-pandemic job posting data to construct a set of labor market power measures for firms in each state in each month:

- **Share of postings within industry-state unit**: calculated as the number of postings by a firm in a state in a month, divided by the total number of postings by all firms in the same industry (NAICS-2) within the same state in that month.
- **Share of postings within state**: calculated the number of postings by a firm in a state in a month, divided by the total number of postings by all firms within the same state in that month.

For each of these measures, I calculate a parallel measure specifically for entry-level jobs, defined as those with O*NET occupation codes of Job Zone 1 and 2.

Similar to the construction of labor market conditions with JOLTS data, for each sampled job, I consider the four constructed measures for the state where the job is located and the month of the first application submitted in the experiment. I also calculate the average of these measures over the three months prior to the first submission in that state as proxies for pre-experiment conditions. These measures are constructed for firms sampled in Wave 1 (September 2019 to March 2020).

Table B.2: Summary of Firm-Level Covariates

	Mean	SD	Median	No.(firms)
Human resource management				
1(federal contractor)	0.6667	0.4736	1.0000	108
Violations per emp.	0.0004	0.0007	0.0002	94
Contact centralization	-1.1204	0.3685	-1.0842	100
HR per emp.	0.0254	0.0092	0.0251	107
Employee satisfaction				
GD scores	3.5226	0.3059	3.5000	106
GD diversity scores	3.7858	0.2667	3.8000	106
GD interview difficulty	2.4717	0.3195	2.5000	106
Financial performance				
log(employment)	11.2432	1.0384	11.0745	94
Total assets per emp. ($M)	0.8484	2.1732	0.1979	94
Gross profit per emp. ($M)	0.1042	0.0806	0.0819	94
Total revenue per emp. ($M)	0.3563	0.3828	0.2613	94

Note: This table summarizes firm-level covariates related to **human resource management**, **employee satisfaction** and **financial performance**. The data here are not normalized. No.(firms) refers to the number of firms with non-missing values for the covariates.