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## ABSTRACT

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# The Gender Application Gap: Do Men and Women Apply for the Same Jobs?\*

Men and women tend to hold different jobs. Are these differences present already in the types of jobs men and women apply for? Using administrative data on job applications made by the universe of Danish UI recipients, we provide evidence on gender differences in applied-for jobs for the broader labor market. Across a range of job characteristics, we find large gender gaps in the share of applications going to different types of jobs even among observationally similar men and women. In a standard decomposition, gender differences in applications can explain more than 70 percent of the residual gender wage gap.

**JEL Classification:** E24, J29, J31, J71

**Keywords:** job search, wage decomposition, firm wage premium, gender earnings gap

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# 1 Introduction

In most labor markets, there are large and persistent differences in the types of jobs men and women tend to hold. This is true both in terms of job characteristics, such as industries or occupations, as well as in terms of the typical wage level of the job. In most cases, women tend to hold jobs that pay systematically less.<sup>1</sup> These gender differences in job outcomes may arise at different possible stages of the labor market: They may arise through gender differences in wage bargaining or promotion rates in ongoing employment relationships. They may arise earlier at the hiring stage, through gender differences in how firms select among job applicants or seek out candidates through other means. Alternatively, gender differences in job outcomes may arise already at the job application stage when male and female job seekers decide which jobs to apply for.

In this paper, we track gender gaps in job outcomes back to the job application stage and ask a simple question: To what extent are gender gaps present already in the types of jobs men and women apply for? The answer to this question has implications for ongoing debates about the origin of gender gaps and for policies that aim to promote gender equality. Outside of some very specific settings, however, empirical evidence on gender gaps in applied-for jobs is virtually nonexistent. This is primarily due to data constraints: Detailed data on job applications is typically only available for a small and/or selected subset of individuals and jobs. The ability to link job application data with rich background characteristics and with actual job outcomes is even more limited.

We overcome these data constraints and provide new evidence on gender differences in the jobs that men and women apply for. We do this by exploiting a unique administrative data source that contains job application data for the universe of Danish unemployment insurance (UI) recipients. Since 2015, all UI recipients in Denmark have been required to systematically register applied-for jobs on an online web portal run by the Danish employment agency. Based on a unique person identifier, we are able to link the resulting data set of applied-for jobs with worker characteristics and hiring outcomes. Based on firm and job information in the job application data, we are further able to link each job application to administrative data on firms and jobs. The resulting data set allows us to examine the characteristics of the firms and jobs that male and female UI recipients

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<sup>1</sup>For a broad overview of the recent literature on gender differences in earnings and wages, see e.g. [Blau and Kahn \(2017\)](#) or [Olivetti and Petrongolo \(2016\)](#).

apply to and compare them to the characteristics of the firms and jobs they eventually end up in. A key feature of these data is their coverage. By construction, the administrative data includes the universe of individual UI recipients, and we use a range of validity checks to establish the coverage and representativeness of the data on applied-for jobs. Across all UI recipients, we estimate that the data cover between 69 and 80 percent of all applied-for jobs and that the covered subset is both highly representative and provides meaningful measures of actual job application behavior. The data thus enables us to provide representative evidence on gender differences in applied-for jobs for a substantial part of the overall labor market.

The primary job characteristics we focus on are a set of standard wage determinants, including the industry and occupation of the job. In addition, a particular advantage of our linked administrative data is that we can construct measures of whether an applied-for job is at a high- or low-wage firm. We do this based on the firm fixed effects from a two-way fixed effects regression for wages (Abowd et al., 1999; Card et al., 2013). Finally, we use administrative data on actual wage payments to compute a predicted wage for each type of job given its characteristics. We refer to this as the *typical wage* for each type of job and use it to shed light on the relationship between job application behavior and the gender wage gap.

The first part of our analysis provides descriptive evidence on gender differences in job applications. We split jobs according to the different job characteristics and then compute differences in the share of applications that men and women send to each type of job. We refer to these differences as *gender application gaps*. Across all the job characteristics we focus on, gender application gaps are substantial and remain also when we condition on a rich set of individual labor market observables, including detailed information on education and prior work experience from different industries and occupations. These gender application gaps tend to closely mirror gender gaps in job outcomes. Across most characteristics, women tend to target jobs that pay lower wages. These gender differences in targeted job types therefore add up to a stark gender gap in the typical wages of the jobs men and women apply for: After conditioning on individual observables, women send 8.2 percentage points more of their applications to jobs whose typical wage is in the bottom decile. In contrast, women send 4.8 percentage points less of their applications to jobs in the top decile. On average, women apply to jobs with a typical wage that is 1.9 percent lower than men.

Next, we use the linked nature of our data to quantify how much of the observed gender gaps

in job outcomes may be explained by the gender application gaps that we find. Because our data contains information on both job applications and job outcomes for the same individuals, we can apply standard decomposition methods from the wage gap and inequality literature. We use a sequential implementation of the semi-parametric decomposition of DiNardo et al. (1996) which allows us to focus on the part of the gender gap that is not explained by individual labor market observables (as in Butcher and DiNardo, 2002 and Altonji et al., 2012). Specifically, we first condition out individuals' observable characteristics and then examine how much of the residual gender gaps in job outcomes can be explained by gender differences in where men and women apply. Our decomposition shows that differences in applied-for jobs are able to explain 86 percent of the residual gender gap in the typical wage level of the jobs males and females hold and 73 percent of the residual gender gap in realized starting wages. For industry and occupation, results vary: For some industries and occupations, gender differences in applied-for jobs can explain virtually all of the observed gender segregation. For others, the observed gender segregation appears unrelated to application behavior.

Although our decompositions rest on the usual strong assumptions regarding decomposition counterfactuals, the results suggest that gender differences at the job application stage may play a very important role in shaping overall gender gaps in the labor market. A natural next question is *why* men and women are applying to such systematically different jobs. Our data is less well suited to answer this question, however, we are able to provide some suggestive results. We thus finish our analysis by looking for evidence in support of different mechanisms that may explain the gender application gap. This includes the possibility that women simply apply less for job types where they face a lower likelihood of being hired (so-called “self-fulfilling discrimination”, Lundberg and Startz, 1983; Glover et al., 2017; Coate and Loury, 1993), the possibility that men and women differ in their beliefs, degree of (over)confidence or risk preferences (Cortes et al., 2020) and the possibility that men and women differ in their valuation of non-wage job characteristics (Le Barbanchon et al., 2021; Wiswall and Zafar, 2018; Maestas et al., 2019; Hotz et al., 2018).

Within the limitations of our data, we are only able to find evidence for the latter of these explanations: Women indeed target systematically different non-wage job characteristics. Relative to men, women send more of their applications to jobs that are part-time, that involve a shorter commute and that are at more family-friendly firms. At the same time, such jobs tend to pay lower

wages. We also find some support for the idea that these differences in job application behavior may be partly related to motherhood. Gender application gaps are larger among men and women with young children, however, there are substantial gender application gaps also among individuals without children.

Our paper is directly motivated by the large literature on gender gaps in the labor market (see [Blau and Kahn \(2017\)](#) or [Olivetti and Petrongolo \(2016\)](#) for an overview). A pervasive finding in this literature is that the types of jobs that men and women hold are important for the overall gender gap in wages and earnings (see [Gallen et al. \(2019\)](#) for Danish evidence). Moreover, recent work has also emphasized differences in the types of firms that men and women work at ([Card et al., 2016](#)). This motivates our focus on tracking gender differences in job outcomes back through the job search and job application process. It also motivates our particular focus on the firm type.

To our knowledge, our paper is the first to provide evidence on gender differences in the jobs that men and women apply for in the broader labor market. Outside of our paper, the most direct evidence on overall gender differences in job search comes from data on UI recipients' minimum acceptable jobs. In earlier work, [Eriksson and Lagerström \(2012\)](#) use data from the job website of the Swedish employment agency to study differences in the geographical areas that men and women say they are willing to work in, while [Caliendo et al. \(2017\)](#) use survey data on the reservation wages German UI recipients. In work concurrent with our own, [Le Barbanchon et al. \(2021\)](#) use data from the French employment agency on UI recipients' self-reported job preferences. [Eriksson and Lagerström \(2012\)](#) and [Le Barbanchon et al. \(2021\)](#) both document that men are willing to accept jobs further away from where they live. [Caliendo et al. \(2017\)](#) and [Le Barbanchon et al. \(2021\)](#) also document that women report a systematically lower reservation wage. In this paper, we move beyond passive differences in reservations wages or acceptable commutes and instead document gender differences in the active decision about which specific jobs to apply for. In combination with our detailed data, this focus also allows us to characterize gender differences in applied-for jobs along a range of new dimensions, including firm type.

A different set of existing papers conduct case studies of gender differences in job search and application behavior using data from very specific settings. [Kuhn et al. \(2018\)](#), [Gee \(2019\)](#), [Banfi et al. \(2019a\)](#) and [Rousille \(2021\)](#) each study job search on a particular online job platform, [Flory et al. \(2014\)](#) and [Samek \(2019\)](#) use field experiments to study applications to a particular job or

career, while [Barbulescu and Bidwell \(2013\)](#) and [Cortes et al. \(2020\)](#) study job search behavior among students from two different business schools. A key advantage of this approach is that the authors can often leverage (quasi-)experimental variation to identify gender differences in how job search responds to various external factors. A drawback however is that the data and settings often only represent a small subset of the overall labor market and that selection into the data is not well understood. This is limiting because gender differences may vary a lot across settings and worker groups.<sup>2</sup> A major contribution of our paper is that we provide descriptive evidence on gender differences in job search using data on the universe of UI recipients. UI recipients constitute a large and well-defined subgroup of workers and are quantitatively very important for the overall labor market: hires out of unemployment cover about half of all new hires in the Danish labor market and roughly half of a given cohort receive UI at some point during their labor market career.

The results in the present paper have implications for ongoing debates about the origins of gender gaps. Several recent papers have argued that gender differences in the valuation of non-wage job characteristics are important for the gender wage gap. By combining their data on job preferences with a structural search model, [Le Barbanchon et al. \(2021\)](#) find that women have a higher willingness to pay for avoiding long commutes, and that this can explain around 10 percent of the gender gap in earnings. Based on surveys where respondents choose between hypothetical jobs with different characteristics, [Wiswall and Zafar \(2018\)](#) and [Maestas et al. \(2019\)](#) also estimate that women have a higher willingness to pay for a range of non-wage job characteristics, such as time flexibility and paid leave, and that this can explain a substantial part of the gender wage gap.<sup>3</sup> Finally, several recent studies have emphasized that the gender gap in earnings appears to be closely linked to motherhood and to mothers seeking out jobs in more family-friendly firms (see e.g., [Kleven et al., 2019](#); [Hotz et al., 2018](#); [Lundborg et al., 2017](#)). All these findings and conclusions are directly consistent with our finding that women systematically apply for jobs that pay lower wages but are more family-friendly, offer shorter commutes and involve fewer hours.

Our results also have implications for policy initiatives that aim to close the gender gap. In particular, our results suggest an important role for policies that directly aim to influence the job

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<sup>2</sup>In their sample of 1,255 MBA graduates for example, [Barbulescu and Bidwell \(2013\)](#) find no evidence that women target lower paying jobs. This stands in stark contrast to the pattern we document for UI recipients at large.

<sup>3</sup>In a field experiment among applicants to a call center job, [Mas and Pallais \(2017\)](#) also find that women have a higher willingness to pay for flexible work arrangements. They conclude however that the differences are too small to be quantitatively important for the gender wage gap.

and career choices of women. Examples of such policies include public family-friendliness policies that may ease the time constraints faced by women (e.g. [Andresen and Nix, 2019](#)) or policies based on gender role models that may encourage women to pursue different career paths (e.g. [Porter and Serra, 2019](#)).

Finally, our paper is related to a large empirical literature that estimates gender discrimination in the hiring process using either natural (e.g. [Goldin and Rouse, 2000](#)) or controlled experiments (see e.g. [Neumark, 2004](#); [Neumark, 2018](#); [Rich, 2014](#); [Riach and Rich, 2002](#)). The central focus in this literature has been to estimate how the probability of being hired (or interviewed) differs across similar men and women when they apply for the same job. This is directly complementary to the present paper which documents to what extent men and women in fact tend to apply to the same types of jobs.

The rest of the paper is structured as follows: In [Section 2](#) we describe our data and institutional framework and discuss the validity of the data for measuring of individual job application behavior and for analyzing gender gaps. In [Section 3](#), we present descriptive evidence and document gender application gaps across a range of job characteristics. [Section 4](#) covers the decomposition methodology we use and presents the results of the decomposition. [Section 5](#) provides suggestive evidence on why women and men are applying for such different jobs. [Section 6](#) concludes.

## 2 Data and institutional setting

In Denmark, UI is available for up to two years at a replacement rate of 90 percent of previous income and a cap of 18.500 DKK (2.500 Euro in 2017). The cap is binding for the majority of workers. UI eligibility requires membership and quarterly membership fees to one of the 24 different UI funds sufficiently well in advance of becoming unemployed. Although such membership is voluntary, a large majority of Danish employees are members of a UI funds and UI recipients make up the vast majority of unemployed job seekers.<sup>4</sup>

To remain eligible for UI while unemployed, UI recipients have to document that they are ac-

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<sup>4</sup>In 2015, 76 percent of Danish employees were members of a UI fund. Among the gross unemployed 70 percent were currently UI recipients. Of the remaining 30 percent, however, more than two-thirds received mean-tested social assistance, which typically means that they were former UI recipients whose UI benefits had expired (see e.g. [Danish Economic Council \(2014\)](#)).

tively searching for jobs.<sup>5</sup> Since 2015 this documentation has been centralized through an online system called *Joblog*. The Joblog system works as follows: To register an application in the system, unemployed workers need to log in to the central online platform of the Danish public employment service (*Jobnet*). This platform serves as the main means of communication between UI recipients and public authorities and also functions as a job board, where job seekers can find most posted vacancies in Denmark. After entering the Joblog system, unemployed workers fill in a form describing their job application. It is mandatory to provide information on the applied-for job, including the job-title and hours (part-/full-time), and about the potential employer, including firm name and address.<sup>6</sup> This information serves as the basis for our analysis.

Administration and payout of UI in Denmark is carried out by the UI funds. This includes administration of the job search documentation requirements in Joblog. During a UI recipient's first weeks of unemployment, the UI fund is legally required to instruct the UI recipient in the use of the Joblog system.<sup>7</sup> Over the subsequent unemployment spell, the fund is required to assess whether the UI recipient is complying with the documentation requirements necessary to maintain eligibility. Formally, this is to be done on case-by-case basis, however, as a general rule of thumb, UI recipients are instructed that they need to register somewhere between 1.5 and 2 applications per week in the Joblog system to maintain eligibility.<sup>8</sup> Failure to comply with documentation requirements results in sanctions in the form of lost or reduced UI payments.<sup>9</sup> UI recipients thus face a clear economic

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<sup>5</sup>Additional requirements for maintaining eligibility are that the UI recipient accepts appropriate job offers and participates in activities (such as meetings and activation programs) at the municipal job centers and at the UI funds.

<sup>6</sup>The Joblog system also allows the UI recipients to register additional information. This includes registering jobs that they plan to apply to in the future and registering other activities such as participating in a job interview or reaching out about jobs through their personal network. Since UI recipients are not required to use most of these features however, they are much more seldom used. In our analysis, we only use data on the formal job application that UI recipients report that they have applied for.

<sup>7</sup>The UI funds are incentivized to comply with the rules by the National Labor Market Authorities. If the National Labor Market Authorities decide that a UI fund has not administered according to the law (i.e. assessing eligibility and screening registered applications) and thus paid out "illegal" UI benefits, the UI fund risks losing the reimbursements of UI.

<sup>8</sup>The law always requires the UI fund to specify a minimum amount of weekly or monthly applications that each individual needs to register, however, this amount should in principle be based on a specific assessment of the workers' education, work experience and competencies, as well as the demand for labor in the area that the worker needs to be available for. Despite the lack of a formal universal threshold of registration requirements, the vast majority UI funds often post general guidelines of their expectations and it is generally well-known that registering between 1.5 and 2 applications per week should be sufficient for recipients to fulfill eligibility requirements.

<sup>9</sup>In the case of non-compliance with the job search requirements, UI recipients will typically be given a short time period to prove eligibility and register previously unregistered (or ongoing) job search after which the UI fund will make its final assessment. The size of the sanctions ranges from a loss of benefits for a couple of days to a permanent loss of benefits depending on the severity of the non-compliance. In cases where registered job applications are not considered adequate (due to e.g. an assessed risk of proforma search, fake applications), similar requirements apply.

incentive to comply with the requirements and register submitted job applications in Joblog. As we show below, these incentives have resulted in a very high level of usage. Our analysis leverages this by combining the comprehensive information on applied-for jobs from the Joblog data with a range of other administrative data sets.

## 2.1 Selecting the analysis sample

Our base sample is constructed from administrative data on UI payments and consists of all UI recipients of Danish nationality entering new UI spells from September 2015 to September 2017. September 2015 is the time where the Joblog system was fully operational and September 2017 is the last month with available labor market data. For each UI recipient, we use a unique person identifier to identify all applied-for jobs that have been registered in the Joblog system during the unemployment spell. We further use this person identifier to merge in data from a wide range of other administrative data sets maintained by Statistics Denmark (DST). These data sets includes demographic information, education and the full history of public benefit payments and employment, including information on occupation, hours, wages and firm identifiers for the employing firms (see additional details on the data sources and data construction in Appendix A.1). In selecting the final sample, we make four sample restrictions:

1. We only consider UI spells lasting at least 8 weeks.
2. We only consider individuals who registered at least 4 job applications during their spell.
3. We only consider UI spells that end with the individual finding a job within one year.
4. We exclude job applications made in the last four weeks before a transition to employment.

Restriction 1 ensures that we are not looking at individuals who already have a new job lined up when leaving their old employer but receive UI while they wait for this job to start. Restriction 2 removes a small number UI recipients who never start using the Joblog registration system before exiting from UI.<sup>10</sup> Restriction 3 reflects that we are interested in gender gaps in earnings or wages

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<sup>10</sup>Although the UI funds are required to instruct UI recipients in the use of the Joblog system at the beginning of their UI spells, the data shows that unemployed are usually subject to a “phasing-in” period in which they slowly get introduced to Joblog and other components of the UI system, and realize that they have to register applications regularly. For some individuals who leave UI very quickly this can imply that they only register very few applications before exiting.

conditional on being employed. Our analysis thus aims to understand how gender differences in job search relate to differences in the jobs men and women are hired for, rather than whether their search results in a hire.<sup>11</sup> Finally, Restriction 4 gets rid of applications that UI recipients are making after successfully landing a job but before this job has actually started.<sup>12</sup>

The top part of Table A.1 in Appendix A.1 show how each of these restrictions affect our sample. After imposing all restrictions we are left with sample of 105,879 individuals, covering 114,375 UI spells with a total of 2,911,585 job applications in Joblog.<sup>13</sup> Each of the UI spells in this data ends with the individual transitioning into a job. In the rest of the paper we refer to these jobs as the UI recipients’ *new jobs*.

As noted in the introduction, a key contribution of the present paper is the coverage and representativeness of the data sources we use. To ensure that this representativeness is not foiled by the specific sample restrictions we impose, Appendix B.1 conducts an extensive set of robustness checks to examine whether any of the four restrictions substantially affect our results. None of the conclusions presented later are sensitive to the sample restrictions above.

## 2.2 Measuring job characteristics and wages

Our analysis uses data on a range of characteristics of the jobs that men and women apply for and the new jobs they are hired into. For each applied-for job in the data, we use string matching on the job title, firm name and firm address to determine the occupation of the job and to merge in firm information from the administrative data. We successfully match 86 percent of applications to a firm and 82 percent to an occupation. Additional details of the matching process are outlined in Appendix A.2.<sup>14</sup>

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<sup>11</sup>Note that Restriction 3 implies that we may slightly oversample shorter unemployment spells because our employment data only runs until September 2017, and therefore UI spells starting e.g. late in 2017 are only included in our sample if the unemployed find work quickly. In practice this does not affect our results, and redoing our analysis using only individuals unemployed prior to e.g. September 2016 yields similar results.

<sup>12</sup>Many jobs do not start right away which implies that UI recipients typically continue receiving UI for some weeks after they have accepted a new job. In the data, we see a clear drop in the number of applications that people register in Joblog about one month before they enter employment, likely reflecting that the individuals have already accepted their new job at this point in time and are simply waiting for it to start. Applications made while waiting for the new job to start may not represent an individual’s general application behavior.

<sup>13</sup>In Sections 2.4 and 4 we additionally trim our sample above and below certain propensity score thresholds (more details in the respective sections). As evident at the bottom of Table A.1 in Appendix A.1, this further reduces our sample for these exercises.

<sup>14</sup>In our analyses of the various job characteristics, we exclude applications with missing information on the relevant job characteristic. In Table A.2 in Appendix A.1 we show the share of missing values for different job characteristics. We have also tried versions where we included a separate category for missing values. This does not change any of

After matching, we immediately observe the industry and occupation for each UI recipient both for the applied-for jobs and the new job they are actually hired into.<sup>15</sup> To further construct a measure for whether jobs are at a high- or low-paying firm, we use the matched administrative data and estimate an (AKM) log wage regression with worker and firm fixed effects on the universe of Danish workers and firms (Abowd et al. (1999), details of the procedure are given in Appendix A.3). We use the estimated firm fixed effects from this regression as our measure of whether applied-for and actual new jobs are at high- or low-paying firms. Since we conduct separate analyses focused on industry, we demean the estimated firm fixed effects within industry so that they reflect within-industry differences in firms' wage levels.<sup>16</sup>

We also use information on wages in our analysis. For each new job, we observe monthly earnings in the administrative registers and construct a measure of the hourly wage that is paid to the individuals one month after entering a new job.<sup>17</sup> For job applications, we have no direct measure of the wage an unsuccessful applicant would have received if hired. Instead, we use the actual wages and characteristics of the new jobs that UI recipients are hired into to estimate a model that predicts the wage in a given job from the full set of job characteristics. We use this model to compute a predicted wage for each of the jobs in our job application data as well as for each of the actual new jobs. We refer to this prediction as the *typical wage* for the job given its type. Appendix A.4 discusses the details of the prediction procedure. Appendix B.6 presents results from an alternative approach that computes typical wages separately for men and women. We note that since vacancies in Denmark rarely post a wage figure, our *typical wage* prediction could be seen as an approximation of the information job seekers have available at the time they apply for jobs.

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our conclusions.

<sup>15</sup>The occupation and industry classifications are available to several degrees of detail, grouped in major, sub-major and minor groups. Occupations are based on the Danish version of the ISCO classification (DISCO) and are grouped with 9, 55 or 153 respective occupations (referred to below as 1-, 2- or 3-digit groupings). The industries are based on NACE Rev. 2 and are grouped in 10, 21 or 38 respective industries.

<sup>16</sup>We obtain very similar gender gaps in both application and hiring if we forego the demeaning. This reflects that men and women do not apply for and get hired into industries with systematically different wage levels. See Figure 2b, where industries are organized left-to-right according to their average wage level.

<sup>17</sup>The hourly wage measure we use is based on recorded monthly earnings divided by the recorded monthly hours in the job in the first full month of employment. This measure has the highest coverage in our sample as it is available already after one month of employment. The gender wage gap based on e.g. an average over 4 months of earnings is very similar, and the gap is likewise similar when we exclude observations where hours worked have been imputed by Statistics Denmark, see <https://www.dst.dk/da/Statistik/dokumentation/Times/beskaeftigelse-for-loenmodtagere/ajo-loentimer>.

## 2.3 Coverage of the Joblog application data

Relative to many other data sets with information on job search, a key advantage of the data sources we use is their coverage. By definition, the administrative data sets we build on include the universe of UI recipients in Denmark. In addition, a particular advantage of the Joblog application data is that they cover all types of applied-for jobs rather than being limited to job applications made via a certain channel or platform, or being limited to a certain subset of potential jobs. At the same time, since registering jobs in the Joblog data is done entirely by UI recipients themselves, the coverage and validity of these data warrants further discussion and analysis.

A priori, a reassuring feature of the Joblog data is that UI recipients face very clear incentives to register job applications in the data and to do so truthfully. As discussed previously, UI recipients face sanctions if they fail to register the required number of applications or if they are caught registering fictitious applications. These incentives are borne out in a very high level of registration activity. In the raw data 96 percent of new UI recipients register at least one applied-for job during the UI spell. Among UI spells lasting at least 8 weeks - which we focus on - the number is even higher. For 98 percent of these UI spells we observe at least one applied-for job.

In terms of the number of jobs that each UI recipient applies to, the rule of thumb requirements to register between 1.5 and 2 jobs per week is also strongly borne out in the data. In our final sample, the average number of applied-for jobs per week is strongly centered just below 2 applications per week (see Figure A.2 for a histogram of weekly applications). This indicates that many UI recipients respond to the registration incentives by registering just enough jobs to satisfy requirements. To the extent that job seekers sometimes apply to more jobs than the required number, however, this means the data may not cover all applied-for jobs. In Appendix A.9.1 we use auxiliary survey data on Danish UI recipients to look closer at the degree of coverage. We find a high level of coverage: Survey results suggest that between 69 and 80 percent of all applied-for jobs are registered in the Joblog data. Coverage also appears very similar across gender: Depending on the method we use, we find that the Joblog data contains between 68 and 82 percent of all applications made by women and between 72 and 76 percent of all applications made by men.

Since the focus of our analysis is on *where* individuals send their applications rather than the total number of applications sent, any potential lack of full coverage is less problematic as long as

the subset of applications that are being registered is representative of overall application behavior. Again the incentive structure around Joblog is reassuring here. Danish UI recipients face no formal incentives to selectively register some applications over others. Moreover, UI recipients who register fictitious or erroneous applications would be subject to economic sanctions if discovered. Since we cannot rule out that some selective logging occurs, however, we have subjected the data to a range of validity checks. These checks exploit the fact that - independently of the application data - we also observe actual job outcomes. The checks are summarized below but are presented at length in Appendix [A.9.2](#).

First, we show that our data on applied-for jobs is highly predictive of later job outcomes; data on applied-for jobs predicts the characteristics of a UI recipient's new job about as well as the characteristics of their previous job. Moreover, the data on applied-for jobs continue to be predictive even after conditioning on the characteristics of the previous job. This is true both overall and when analyzing male and female job seekers separately.

Second, we examine how often we are able to trace a new hire back to a job application that is contained in our data. Specifically, for each UI recipient who finds a job at some firm, we check whether we see that the UI recipients has previously applied for a job at this firm according to our data. To see the usefulness of this exercise, consider the following: Since our raw data is estimated to cover between 69 and 80 percent of all applications and since we successfully match 86 percent of these applications to the corresponding firm, our firm-matched sample of applications should cover between 59 and 69 percent of all applications. Survey data from Denmark suggests that around 73 percent of new hires out of unemployment involve the job seeker actively applying for the job.<sup>18</sup> This suggest a simple test of the representativeness of the application data: If the subset of applied-for jobs contained in our data are a representative subset of all applied-for jobs, the likelihood that we are able to match a given new hire to an application in our data should be between  $0.59 \cdot 0.73 = 0.43$  and  $0.69 \cdot 0.73 = 0.50$ . In contrast, if the application data is non-representative, the data is likely to either over- or under-represent applications that end up turning into a new hire, which would imply a higher or lower match rate. Looking at the new hires in

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<sup>18</sup>The remaining 27 percent reflect jobs or internships assigned to job seekers by their educational institution or through a temp agency. It also reflects instances where the job may be offered to the worker without the worker making an active application (i.e., when a firm directly recruit workers via headhunting, recalls past workers or recruits workers actively through social networks). See Appendix [A.9.2](#) for additional details.

our data, however, the share of new hires that we are able to link to an application is in fact 0.47, consistent with the data being representative.<sup>19</sup> Appendix A.9.2 provides more details and presents additional validity checks.

## 2.4 Conditioning on observables

In our analysis, we primarily want to focus on differences in job applications and hiring outcomes among men and women with the same labor market observables. Throughout the main text, we therefore analyze gender differences in application behavior after conditioning out other labor market observables.

To condition out observables we employ a standard propensity score reweighting procedure and reweight the women in our sample to have the same distribution of observables as the men.<sup>20</sup> We opt for propensity score reweighting because this ties in naturally with the decomposition methodology we use later in the paper (see Section 4).

In terms of which exact observables to condition on, the richness of our administrative data means that we have access to an extremely large number of potentially relevant variables. Rather than making ad hoc decisions about which subset of variables to include, we instead use a Machine Learning procedure to discipline variable selection, following recent suggestions in the literature (see e.g. [Athey and Imbens, 2019](#); [Angrist and Frandsen, 2019](#); [Mullainathan and Spiess, 2017](#)). Specifically, we construct a very large baseline set of potential variables and then use the double-LASSO of [Belloni et al. \(2014\)](#) to select the subset of these variables that is most important for explaining wage differences between men and women. We then condition on this set of variables throughout our analysis.

Our baseline set of potential variables consists of 4,196 variables containing detailed information on age, work experience, level and field of education, prior industries and occupations, and

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<sup>19</sup>We can also compute this match rate separately for male and female job seekers. Doing so we find a corresponding match rate of 53 percent for women and 41 percent for men. Qualitatively, the difference here is consistent with previous evidence that overall men are somewhat more likely to find a job in ways that do not involve a formal job application ([Engman \(2019\)](#)), which will mechanically lower the match rate. Since we do not have reliable data on how often this occurs for male and female UI recipients, however, we cannot perform the quantitative benchmarking exercise for men and women separately.

<sup>20</sup>Our propensity score reweighting works as usual: For some individual in our data, let  $m$  be an indicator for being male, and let  $x$  be the vector of labor market observables we wish to condition on. The propensity score is now defined as the probability of being male given these other characteristics,  $p = P(m = 1|x)$ . For each person in the data, we compute an estimated propensity score,  $\hat{p}$ , and then reweight each woman in the data by  $\frac{\hat{p}}{1-\hat{p}}$ .

dependence on public transfers.<sup>21</sup> Importantly, these variables both include detailed measures of stable characteristics such as education and detailed information about recent labor market history. In addition to capturing stable differences in the characteristics of men and women, this also allows us to deal with the possibility that men and women may self-select into UI at particular point in their labor market career.

From the baseline set of 4,196 variables, the double-LASSO selects 332 variables as being important. In our main analysis we reweight the women in our sample based on estimated propensity scores using these 332 variables. To avoid the usual issues of non-overlapping support when propensity score-reweighting, we exclude individuals with a propensity score above 0.99 or below 0.01 throughout our descriptive analysis. This reduces our sample of UI spells by 5.4 percent (see Table A.1). Appendix A.5 and A.6 provides additional details regarding the reweighting procedure and the choice of variables to include. Table 1 provide descriptive statistics for the men and women in our analysis sample both before and after reweighting.

## 2.5 Gender gaps in the analysis sample and overall

Our analysis focuses on gender gaps among UI recipients transitioning into new jobs. These make up a substantial part of the overall labor market; hires out of unemployment cover about half of all new hires in the Danish labor market and in historical data roughly half of a given cohort receives UI at some point during their labor market career. Before proceeding with our analysis of gender gaps, however, Figure 1 compares gender gaps in reemployment wages in our analysis sample to the overall gender wage gap in Denmark.

The two bars furthest to the left in Figure 1 show the gap in actual and typical log wages across the stock of employed individuals in Denmark in August 2015. The gray bar shows the substantial gender wage gap that prevails in the Danish labor market: men in Denmark are on average paid

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<sup>21</sup>To capture educational differences, the baseline set contains years of education, as well as dummies for the field of study. To capture additional differences in general human capital, the baseline set includes age, total work experience and work experience over the last five years. To capture additional differences in specific human capital, the baseline set includes dummies for the sector, industry and occupation of the previous job as well as continuous measures for the total work experience over the last five years in each of the different industries and occupations. To capture differences in dependence on public transfers, the baseline set includes the total time spent receiving unemployment insurance, social assistance and other public transfers over the last five years. Finally, all variables are also interacted with both age, years of education, total work experience and work experience over the last five years. Appendix A.5 provides additional details.

12 percent (log-points) more than women.<sup>22</sup> The black bar shows the corresponding gap when focusing on our measure of jobs' typical wage given their characteristics. The typical wage level for the average job held by a man is around 5 percent higher than for women. This mirrors previous evidence suggesting that differences in firm and job characteristics can explain a substantial part of the gender wage gap.

The two bars in the middle of Figure 1 shows comparable figures for our analysis sample (before reweighting). We see that the majority of the overall gender gaps in wages is present already in the first job following an unemployment spell; from our sample of UI recipients, the men are being hired into jobs with an actual wage that is 7 percent higher than women and a typical wage that is 4 percent higher. As we return to later, the jobs that women find out of unemployment also tend to be associated with lower wage growth on the job, meaning that the effect of differences in job outcomes among unemployed men and women grows over time (see Section 3.4).

Finally, the two rightmost bars in Figure 1 show the corresponding wage gaps after conditioning on observables (after reweighting). As is commonly found in the literature, conditioning on observables reduces gender gaps somewhat but substantial gaps remain.

In the rest of the paper, we examine the extent to which there are gender differences in job applications in this (reweighted) sample of observational similar men and women, and whether these differences in applications can explain the substantial gender wage gap that arise when transitioning out of unemployment.

### 3 Descriptive results: Do men and women apply to the same jobs?

We begin our analysis by providing descriptive evidence on differences in the type of jobs that men and women apply for. To do this, we first categorize jobs according to one of the job characteristics we consider (occupation, industry, firm wage-level or typical wage). For each category of jobs, we then compute what share of their applications women and men are on average sending to jobs in this category. We refer to this difference between these averages as the gender gap in applications

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<sup>22</sup>A 12 percent wage gap is in the range of the numbers reported by [Larsen and Larsen \(2018\)](#) who report the wage gap for different measures of wages. [Larsen and Larsen \(2018\)](#) also compare the gender wage gap in Denmark to other European and Scandinavian countries. The gender wage gap in Denmark is close to the EU average, similar to the gap in e.g. Norway, France and smaller than the gap in Germany. [Gallen et al. \(2019\)](#) show the evolution of the gender wage gap in Denmark 1980-2010.

for this type of job. Throughout the main text, we focus on gender gaps computed *after* reweighting the sample on observables (see Section 2.4). This implies that we are measuring gender gaps in applications among men and women with similar labor market observables.

### 3.1 Occupation and industry

In Panels (a) and (b) of Figure 2 we examine gender gaps in applications across occupations and industries. Specifically, the gray bars in the two panels show the female-male gap in the average share of applications going to each one-digit occupation and industry (the black bars in the figure show corresponding gender gaps in hiring shares; we return to these further below).

There are substantial gender gaps in applications across both occupations and industries. In terms of occupations, for example, the average woman sends almost 12 percentage points more of her applications to service occupations than does the average man. In contrast, she sends 4.7 and 6.1 percentage points less of her applications to machine and craft occupations respectively. For industries, gender gaps in applications are also substantial. For example, the average woman sends about 5.6 percentage point fewer of her applications to jobs in construction than does the average man. Importantly, because these results are conditional on observables (including several measures of past education and occupation), these differences are not simply explained by men or women being more likely to have education or experience from a particular occupation or industry.<sup>23</sup>

To assess the magnitudes, it is instructive to compare the observed gender gaps in applications to the observed gender gaps in the likelihood of ending up in a particular occupation or industry. For this purpose, the black bars in Panels (a) and (b) of Figure 2 show corresponding gender gaps in the share of women vs. men that ends up being hired into each occupation and industry. Comparing the gray and black bars visually, two broad patterns stand out. First, in terms of their sign and relative magnitude, gender gaps in hiring outcomes closely mirror gender gaps in applications; the industries and occupations that women are much less likely to end up in are also the ones that women are applying much less to (and vice versa). Second, in terms of their absolute magnitudes, gender gaps in applications tend to be systematically *larger* than gaps in hiring outcomes.

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<sup>23</sup>We note that our reweighting procedure need not impose exact balance on all covariates in finite samples. As shown in Table 1, however, gender differences in the previous occupation are much smaller than the observed gender gaps in occupations in the reweighted sample. Changing our reweighting procedure to ensure exact balance on previous occupation or industry also does not eliminate the gender gaps in applications to different occupations and industries (see Appendix B.2).

Table 2 and 3 quantifies these two patterns. In Table 2, we apply a standard measure of industry or occupational segregation - the Duncan index - to measure the extent of segregation in both applications and hiring outcomes (Duncan and Duncan, 1955).<sup>24</sup> Across both industries and occupations, the Duncan index suggests that gender segregation in applications is larger than gender segregation in actual hiring outcomes.

In Table 3, we compute the correlation between gender gaps in applications and in hiring outcomes across occupations and industries. As noted, we see a strong positive correlation for all the different measures.

Finally, given the existence of the gender wage gap, it is natural to ask whether the gender application gaps across occupations and industries reflect that women are applying less (or more) to high-paying industries or occupations. With this purpose in mind, the occupations and industries on the x-axis in Panels (a) and (b) of Figure 2 have been arranged left-to-right in terms of their average wage. Visual inspection of Panel (a) thus shows a notable tendency that women apply more to lower paying occupations. No clear pattern is visible for industries in Panel (b) however.

### 3.2 Firm wage levels

Next we examine differences in the wage levels of the firms that men and women are applying to. The gray bars in Panel (c) of Figure 2 shows gender gaps in applications to jobs categorized by the deciles of the employing firms' wage level, as measured by their within-industry demeaned AKM fixed effect (see Section 2.2). As before, the black bars show corresponding gender gaps in the share of workers who end up working at firms in each decile.

Gender gaps in applications to firms with different wage levels follow a striking pattern. Starting at the bottom two deciles, we see a gender gap in applications of about 0.8-1.4 percentage points. Thus, the average woman is sending slightly more of her applications to jobs at these low-paying firms. Moving up the distribution of firms' wage levels to higher deciles, however, the gender gap in applications decreases until it reaches about -2.2 percentage points at the two top deciles. This

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<sup>24</sup>Applied to applications, the Duncan index measures which fraction of the average woman's (or man's) applications must be changed in order to eliminate the gender application gap. Applied to hiring outcomes, the index measures which fraction of men and women need to be moved to a different job in order to achieve gender balance across occupations or industries. Formally, let  $y$  index job types, let  $a_w^y$  and  $a_m^y$  be the share of applications sent to job  $y$  by the average woman and man respectively and let  $s_w^y$  and  $s_m^y$  be the share of women and men that are hired into job  $y$ . The Duncan index for applications is then defined as  $\frac{1}{2} \sum_y |a_w^y - a_m^y|$ , while the Duncan index for hiring outcomes is defined as  $\frac{1}{2} \sum_y |s_w^y - s_m^y|$ .

adds up to a very systematic difference in the types of firms men and women are targeting. Overall, women are sending 3.2 percentage points more of their applications to firms that are in the bottom three deciles and 4.6 percentage points less of their applications to firms that are in the top three deciles.

Contrasting gender application gaps with the corresponding gender gaps in hiring outcomes in Panel (c) (black bars), we again see that the two follow each other closely. In Table 3, we compute the correlation between gender gaps in applications and in hiring shares across the deciles, which turns out to be 0.945. In terms of the relative magnitudes of the two gaps, however, the pattern is less systematic: For some deciles, application gaps are larger than gaps in hiring shares. For other deciles the reverse is true. To get an overall sense of these magnitudes, in Table 2 we compute gender gaps in the wage level of both the average firm applied-for and in the wage level of the actual firms where men and women end up. On average, men are applying to firms with a wage level that is 0.052 standard deviations higher than women. The gap in the wage level of the firms they end up at is slightly higher at 0.068 standard deviations.

### 3.3 Jobs' typical wage levels

The results in the previous section showed that when it comes to occupation and firm type, women tend to apply more to types of jobs that pay less. We now examine how such gender differences in the characteristics of applied-for jobs add up to create differences in the overall wage levels of the jobs men and women apply for. In Panel (d) of Figure 2 we thus show gender gaps in applications to jobs that fall in different deciles of the distribution of typical wages. Recall that a job's typical wage refers to the wage level that is typical for a job given its characteristics (see Section 2.2). The gender gaps in Panel (d) thereby summarize how women are targeting jobs with high-paying or low-paying characteristics overall.

There are striking gender gaps in the typical wage level of applied-for jobs. Starting at the bottom, women are sending 8.2 percentage points more of their applications to jobs whose typical wage are in the bottom decile. Moving up the wage distribution, the gender gap in applications decreases almost monotonically and eventually flips. For jobs that are in the top decile, women are sending about 4.8 percentage points less of their applications to these jobs. As shown in the second panel of Table 2, these differences in applications imply that women are on average applying for

jobs with a typical wage that is 1.9 percent lower than those men apply to.

Contrasting the gender gap in applications in Panel (d) (gray bars) with the corresponding gender gaps in hiring outcomes (black bars) shows a familiar pattern: Gender gaps in applications closely mirror hiring outcomes; across the deciles of jobs' typical wages, the correlation between gender gaps in applications and in hiring is 0.897 (Table 3). Moreover, gender gaps in applications are on average slightly lower than gender gaps in hiring outcomes; while women are on average applying for jobs with a typical wage that is 1.9 percent lower than men, the gender gap in typical wages for the jobs that men and women end up in is 2.5 percent (second panel of Table 2).

### 3.4 Additional results and robustness

We finish this section by discussing some additional results and robustness checks regarding the documented gender gaps in applications that are presented at length in the appendix.

First, we show that our conclusions are robust to using different approaches to condition out observables, including approaches that ensure exact balance on previous industry or occupation (Appendix B.2). We also show that the conclusions are not sensitive to the choice of analysis sample (Appendix B.1).

Second, we show the raw gender gaps in applications before conditioning on any observables. Unsurprisingly, these gaps are generally larger than when conditioning on observables (Appendix B.4).

Third, our analysis examines gender gaps in the share of applications going to different jobs throughout the entire unemployment spell. A potential drawback of this approach is that it obscures any changes in application behavior that occurs over time within an unemployment spell. As we show in the appendix, however, changes in application behavior over time are modest and are very similar for men and women (Appendix A.10). Examining gender gaps in applications at different times throughout the unemployment spell therefore yields very similar results.<sup>25</sup> Furthermore, we also show that our conclusions are not sensitive to looking at gender differences in the total number of applications going to different jobs instead of focusing on shares (Appendix B.5).

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<sup>25</sup>In unreported results, we have conducted our main analysis separately for subsamples of job applications that are made after individuals have been unemployed for a specific amount of time. Such changes in time horizons yielded no qualitative differences in the observed patterns. See also Section B.1 Figure B.2 where we show that results are similar if we instead focus on individuals who find employment within 26 weeks.

Third, our measure of the typical wages paid in different kinds of jobs does not depend on gender. In practice, however, it is possible that some jobs tend to pay higher wages to men than women while the reverse is true in other jobs. In the appendix we therefore compute two alternative measures of typical wages that are estimated either exclusively using data on wages paid to women or exclusively using data on wages paid to men. This provides measures of the typical wage that a given type of job pays to either men or women. Using either of these alternative measures in our main analysis yields qualitatively similar results to the ones shown above (Appendix B.6).

Fourth, our measure of typical wages captures differences in the typical *starting* wages for different jobs. In addition to differences in starting wages, however, different jobs may also differ in their wage trajectories over time. In the appendix, we construct simple measures that capture the extent of on-the-job wage growth. We find that women are in fact both applying to and getting hired into lower wage-growth jobs, consistent with the gender wage gap growing over the course of an employment spell (Appendix C.1).

Finally, we show that the gender gap in applied-for wages exists also when examining wages relative to the previous job. Men are significantly more likely than women to apply to jobs whose typical wage is higher than that of their previous job (Appendix B.3). The gender gap in applied-for wages thus does not simply reflect that men had a higher wage in their previous job and are using this higher wage as a reference point in their job search.

## 4 Decomposition: Can applications explain gender gaps in hiring?

As documented in the previous section, there are substantial differences in the jobs men and women apply to. Moreover these gender gaps in applications closely mirror gender gaps in hiring outcomes. A natural question to ask is therefore to what extent the observed differences in application behavior are capable of explaining the observed gender gaps in hiring outcomes. To answer this question, we decompose the observed gender gaps in hiring outcomes (including wages) into a part that can be explained by gender differences in applications and a part explained by other factors.

Our decomposition exercise leverages the fact that our data contains joint information on both job applications and actual hiring outcomes for the same individuals. This allows us to apply the standard semi-parametric decomposition method introduced by DiNardo et al. (1996). Since this

methodology has not previously been used to decompose job application behavior, we first lay and discuss the relevant methodology for our setting over the next two (sub)sections.<sup>26</sup> We then present results.

#### 4.1 Decomposition and counterfactuals, methodology

Consider some individual in our (unweighted) analysis sample. Let  $m$  be an indicator for being male, let  $x$  be the vector of other labor market observables that we have been conditioning on throughout (see Section 2.4), and let  $a$  be some vector capturing which types of jobs the person has applied for. In our main results we let  $a$  consist of the share of applications sent to each two-digit occupation, each two-digit industry, each decile of the firm wage distribution and each decile of the typical wage distribution. Let  $y$  be a measure of job type (capturing occupation, industry, wage-level etc.). For expositional convenience we will assume that  $y$  is discrete, while  $x$  and  $a$  are absolutely continuous.

Now let  $P^M(y)$  and  $P^W(y)$  denote the probability of being hired into a job of type  $y$ , conditional on being a man or a woman respectively. In large samples, these gender-specific hiring probabilities will equal the share of men and women hired into job type  $y$ , so we will refer to these probabilities interchangeably also as *hiring shares*. Next, let  $P^M(y|a, x)$  and  $P^W(y|a, x)$  be the corresponding conditional hiring probabilities when conditioning on observable characteristics  $x$  and applications  $a$ . To decompose gender gaps in hiring, we are interested in estimating counterfactuals of  $P^W(y)$  that show how women’s hiring outcomes would differ if they had the same observables and job application behavior as men. With this objective in mind, note that by definition  $P^M(y)$  and  $P^W(y)$  can be written as

$$P^M(y) = \iint P^M(y|a, x) f_{a|x}^M(a|x) f_x^M(x) da dx$$

$$P^W(y) = \iint P^W(y|a, x) f_{a|x}^W(a|x) f_x^W(x) da dx$$

Here  $f_x^M$  and  $f_x^W$  are the distributions of labor market observables among men and women

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<sup>26</sup>In their study of sorting, [Banfi et al. \(2019b\)](#) have previously applied a weighting scheme based on [DiNardo et al. \(1996\)](#) to job application data from an online job board in Chile. The implementation details are different however because their aim is not to decompose application gaps.

respectively, while  $f_{a|x}^M$  and  $f_{a|x}^W$  are the conditional distributions of applications among men and women after conditioning on labor market observables.

Throughout the descriptive analysis in Section 3, we focused on gender gaps in applications and hiring among men and women with similar labor market observables. We adopt the same focus and approach in our decomposition. To do this we follow [Butcher and DiNardo \(2002\)](#) and [Altonji et al. \(2012\)](#) and apply a two-step version of the semi-parametric decomposition method of [DiNardo et al. \(1996\)](#) (see also [Fortin et al. \(2011\)](#) for a more recent treatment). In an auxiliary first step, we use the method to condition out labor market observables. In a second step, we then decompose which part of the remaining gender gap can be explained by differences in applications among men and women with similar labor market observables. Additional details regarding the implementation of the decomposition are given in Appendix Section A.7. Appendix B.7 provides a decomposition of the raw gaps, without conditioning on observables.

### Step 1: Measuring hiring gaps among men and women with similar characteristics

To set up the first step, let  $P_X^{\tilde{W}}(y)$  be the hiring probability that women would have faced if they had the same distribution of labor market observables as men:

$$P_X^{\tilde{W}}(y) = \iint P^W(y|a, x) f_{a|x}^W(a|x) f_x^M(x) da dx$$

Based on this, we can define the gender gap in the hiring probability for job type  $y$  after conditioning on labor market observables. Since the focus of our analysis will be on decomposing this gap, we will refer to this simply as our *baseline hiring gap*:

$$\text{BaselineHiringGap}(y) \equiv P_X^{\tilde{W}}(y) - P^M(y) \tag{1}$$

To compute this gap in the data, we need to compute (or estimate)  $P^M(y)$  and  $P_X^{\tilde{W}}(y)$ . Computing  $P^M(y)$  is straightforward: We simply compute the share of men in our sample who get hired into job type  $y$ . As for  $P_X^{\tilde{W}}(y)$ , the key insight from [DiNardo et al. \(1996\)](#) is that it can be reliably estimated by propensity score reweighting the sample of women to have the same labor market observables,  $x$ , as men and then computing the share of women who get hired into job type  $y$  in the reweighted sample.

Reweighting and computing these baseline hiring gaps constitutes the auxiliary first step in our decomposition. In the implementation, we follow the standard practice of using a logit model for the propensity score. The set of labor market observables we include is again the same set as we have used up until this point (see Section 2.4). This implies that the reweighting procedure used in this step of the decomposition is *exactly the same* as the reweighting procedure that was used for the descriptive results in Section 3. There is thus a direct link between the decomposition and the descriptive results presented previously: the descriptive gender gaps in hiring shares shown in Figure 2 are formally equivalent to the *baseline hiring gaps* that we focus on in our decomposition.<sup>27</sup> As in Section 3, we continue to trim the sample by dropping observations with an estimated propensity score above 0.01 and 0.99.

In addition to decomposing gender gaps in hiring into particular job types, we are also interested in decomposing the average gender gap in wages and firm wage levels, as well as the overall extent of gender segregation across industries and occupations (as measured by the Duncan index). This proceeds completely analogously in two steps. In the first step, we measure e.g. the baseline wage gap that would prevail if women had the same distribution of labor market observables as men. Letting  $w(y)$  be the wage in a job of type  $y$ , the baseline wage gap is:

$$BaselineWageGap \equiv \sum_y w(y) \cdot P_X^{\tilde{W}}(y) - \sum_y w(y) \cdot P^M(y) \quad (2)$$

Similarly, letting  $y$  index industries and occupation, the baseline level of industry or occupation segregation is:

$$BaselineSegregation \equiv \frac{1}{2} \sum_y \left| P_X^{\tilde{W}}(y) - P^M(y) \right| \quad (3)$$

Again the equivalence of the reweighting procedure implies that these baseline wage gaps and the level of baseline segregation are will be formally equivalent to the descriptive results presented previously in Table 2.<sup>28</sup>

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<sup>27</sup>As we return to in the next section, implementation of the decomposition requires additional trimming of our analysis sample so in the implementation, the baseline hiring gaps we decompose will differ slightly from the gender gaps presented earlier in Figure 2 and Table 2. See Appendix A.1 and Table A.1 for additional details.

<sup>28</sup>As we return to in the next section, small changes to the employed sample imply that the baseline wage and segregation gaps considered in the decomposition will numerically slightly different from those presented previously. See footnote 27.

## Step 2: Decomposing the hiring gaps

$BaselineGap(y)$ ,  $BaselineWageGap$  and  $BaselineSegregation$  show gender gaps in hiring outcomes for men and women with similar observables. The key question that we answer in our second step of the decomposition is how much of these gaps can be explained by differences in application behavior. To do this, we define  $P_{A,X}^{W\tilde{}}(y)$  to be the hiring probability which women would have faced if they had the same application behavior as men (as well as the same distribution of observables):

$$P_{A,X}^{W\tilde{}}(y) = \iint P^W(y|a, x) f_{a|x}^M(a|x) f_x^M(x) da dx$$

We can now define the gender gap in hiring (for job type  $y$ ) that remains after conditioning on both applications and labor market observables. We refer to this as the *residual hiring gap* after application behavior (and observables) have been accounted for:

$$ResidualHiringGap(y) = P_{A,X}^{W\tilde{}}(y) - P^M(y) \tag{4}$$

The baseline gender gaps in hiring into job  $y$  can be decomposed into a part that is explained by differences in applications and a residual gap as follows:

$$BaselineHiringGap(y) = \underbrace{\left( P_X^W(y) - P_{A,X}^{W\tilde{}}(y) \right)}_{\text{Explained by applications}} + \underbrace{ResidualHiringGap(y)}_{\text{Explained by other factors}} \tag{5}$$

This is the key equation that defines our decomposition of interest. Focusing on men and women with similar labor market observables, this equation decomposes gender differences in hiring shares into two parts. The first part is the part that can be explained by differences in the types of jobs men and women apply as measured by the Joblog application data. The second part is a residual that contains all other factors that can explain gender gaps in hiring shares. This residual will include discrimination and any other differences in how employers treat observationally similar men and women.<sup>29</sup> The residual may also include other potential explanations, however, such as

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<sup>29</sup>Note that “observationally similar” refers to the rich set of observables that we include in the vector  $x$  (see Section 2.4). As usual, however, men and women may also differ in unobserved ways that may influence employers hiring decisions even in the absence of gender discrimination.

measurement error or gender differences in the likelihood of accepting a job offer.<sup>30</sup>

To implement Equation 5, we need an estimate of  $P_{A,X}^{W\tilde{}}(y)$ . As shown by Fortin et al. (2011), however, such an estimate can be obtained simply by propensity score reweighting the sample of women to have both the same observables,  $x$ , and the same application behavior,  $a$ , as the men, and then computing the share of women who get hired into job type  $y$  on the reweighted sample.

In implementing this second reweighting step, we again follow standard practice and estimate the propensity score using a logit model. We also continue to trim observations where the estimated propensity score is above 0.99 or below 0.01. This reduces our sample of UI spells by 8.5 percent (see Table A.1 in the Appendix).<sup>31</sup>

Finally, we can extend this second step of the decomposition to decompose wage gaps and gender segregation by industry and occupation. Again letting  $w(y)$  be some measure of wages in job type  $y$ , we can define the average residual wage gap as:

$$ResidualWageGap \equiv \sum_y w(y) \cdot P_{A,X}^{W\tilde{}}(y) - \sum_y w(y) \cdot P^M(y) \quad (6)$$

This immediately gives rise to the following decomposition for wages and firms' wage levels:

$$BaselineWageGap = \underbrace{\sum_y w(y) \cdot \left( P_X^{W\tilde{}}(y) - P_{A,X}^{W\tilde{}}(y) \right)}_{\text{Explained by applications}} + \underbrace{ResidualWageGap}_{\text{Explained by other factors}} \quad (7)$$

Letting  $y$  index occupations and industries, we further extend this approach to decompose gender segregation across industry and occupation:

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<sup>30</sup>As discussed in Section, 2.3, the Joblog application appears to have very high coverage and reliability so we expect measurement error in these data to be limited. In addition, UI recipients in Denmark have to accept all job offers that may be considered reasonable if they wish to maintain UI eligibility. This suggest that gender differences in the rejection of job offers should also play a limited role. It is still possible, however, that men and women - with the same labor market observables and similar job application behavior - can make different decisions in situations where they receive more than one job offer at the same time.

<sup>31</sup>In total, we thus trim our sample twice before performing the decomposition. The first trimming removes observations where the estimated propensity score lies below 0.01 or above 0.99 when the propensity score is estimated only based on labor market observables ( $x$ ). The second trimming removes observations where the estimated propensity score lies below 0.01 or above 0.99 when the propensity score is estimated based on both labor market observables and application behavior ( $x$  and  $a$ ). Since the second trimming step is not needed for the descriptive analysis in Section 3, the sample UI spells used throughout the decomposition is 8.5 percent smaller than the sample used in Section 3. Comparing the (baseline) hiring gaps in Figure 2 and 3, however, we see that the additional trimming affects gender gaps relatively little.

$$ResidualSegregation \equiv \frac{1}{2} \sum_y \left| P_{A,X}^{W\tilde{}}(y) - P^M(y) \right| \quad (8)$$

$$BaselineSegregation = \underbrace{\frac{1}{2} \sum_y \left( \left| P_X^{W\tilde{}}(y) - P^M(y) \right| - \left| P_{A,X}^{W\tilde{}}(y) - P^M(y) \right| \right)}_{\text{Explained by applications}} + \underbrace{ResidualSegregation}_{\text{Explained by other factors}} \quad (9)$$

## 4.2 Identification of counterfactuals in the decomposition

The decomposition method relies on estimating the counterfactual hiring outcomes that women would have faced if they had the same application behavior as men (and the same labor market observables). As a result, it is worth discussing the underlying assumptions that identify these counterfactuals.

As discussed and formalized in Fortin et al. (2011), the key identifying assumption can be viewed as an ignorability or conditional independence assumption. In the derivations above, this is most clearly seen from the fact that the conditional hiring probability for women only depends on application behavior and labor market observables,  $P^W(y|a, x)$ . This in particular implies that any unmodeled or unobserved factors that affect hiring outcomes must be independent of application behavior (conditional on labor market observables). Put differently, if a woman who is currently not applying to “male jobs” were to start applying to such jobs, we are assuming that she will face the same hiring outcomes as those women in the data who are already applying to these jobs and have similar labor market observables.

While this identifying assumption is standard in decomposition exercises, it raises several concerns. Perhaps the most salient concern is the possibility that women who are currently applying to more male jobs differ from other women along dimensions not captured by our vector of labor market observables and that these differences affect hiring outcomes. Although our vector of observables aims to include very detailed measures of the relevant labor market observables (see Section 2.4), unobservable characteristics may still play a role.<sup>32</sup>

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<sup>32</sup>Given the existing literature on gender differences in competitiveness (see e.g. Niederle and Vesterlund, 2007), we might for example expect that women who apply to more “male” jobs could have a more competitive personality.

Regardless, we view the decomposition as providing a useful benchmark, especially given the lack of previous evidence. In particular, the decomposition shows how much of the observed gender gaps in outcomes can be explained by differences in applications under a standard assumption used in the literature.

### 4.3 Decomposition and counterfactuals, results

We now present results from the decomposition. Figure 3 provides a detailed decomposition of the gender gaps in hiring outcomes across occupations, industries, firm types and typical wages. These results follow from applying the decomposition outlined in Equation 5. The black bars in the figure show the baseline hiring gaps after conditioning on labor market observables. The solid-white and dashed-white bars then decompose these hiring gaps into a part that can be explained by gender differences in applications (solid bars) and a residual gap (dashed bars).

The decomposition suggests that in many cases gender differences in applications are capable of explaining substantial parts of the observed gaps in hiring outcomes. For example, gender differences in applications can explain about one third of the observed hiring gap into Service and Clerical occupations and can explain virtually all of the hiring gap into the Information & Communication industry and in the Craft occupations. Across most deciles of both firms' wage levels and jobs' typical wages, gender differences in applications also explain a substantial part of observed hiring gaps. There are even some instances where application differences can more than explain the observed hiring gaps between genders. For Professional occupations, for example, the baseline hiring gap is negative, but the application gap is positive. The decomposition results thus suggest that if women applied to the same jobs as men, women would go from being underrepresented to overrepresented in this occupational group (all else equal).

At the same time, there are also some hiring gaps which cannot be explained by differences in applications, especially for some industries and occupations. Differences in applications cannot explain why women are underrepresented in Technicians occupations or in the Construction industry, for example. For both of these job types, the residual hiring gap is essentially the same size as the baseline gap.

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This may impact their hiring probabilities as well but may not be captured well by the set of variables we condition on in our analysis.

The fact that residual hiring gaps are big for some industries and occupations but very small for others is consistent with the existing literature on gender discrimination in the hiring process. A common finding in this literature is that in some occupations or industries, observationally equivalent women and men are treated very differently in hiring, while for others there are no differential treatment by gender (see Rich, 2014; Riach and Rich, 2002). As noted previously, such gender discrimination in hiring will show up as part of the residual hiring gap in the decomposition presented here.

Turning next to overall gender gaps, Table 4 presents decompositions for the overall level of gender segregation by industry or occupation (based on Equation 9) and for average gender gaps in wages or firms' wage levels (based on Equation 7). The first four rows decomposes the level of gender segregation across industries and occupations. The results mirror the heterogeneous pattern of results from Figure 3: Gender differences in applications are generally capable of explaining a substantial part of the observed gender segregation by industry and occupation, but a substantial part of the observed gender segregation also remains unexplained. Depending on the level of aggregation considered, gender differences in application are capable of explaining 29 to 42 percent of observed gender segregation across occupations among men and women with similar labor market characteristics. For segregation across industries, gender differences in applications are able to explain 23 to 24 percent of the observed industry segregation.

The next row shows the decomposition of the gender gap in the employing firms' wage levels. Results suggest that gender differences in the types of firms that men and women apply to can explain a large part of the observed gap. Among men and women with similar labor market observables, women on average work at firms whose wage level is 0.068 standard deviation lower than men. After accounting for differences in application behavior, however, this difference drops to 0.017 standard deviations, implying that differences in applications can explain 75 percent of the baseline gap.

Finally, the last two rows of the table focus on gender gaps in wages using two different measures. The first is our measure of the *typical wage* paid in jobs with different characteristics: Among men and women with similar labor market observables, women are on average hired into types of jobs that pay 2.5 percent less than men. The second to last row shows a decomposition of this gap. The second wage measure is the *actual wage* of the new job. Among newly employed men and

women with similar labor market observables, women in our data are on average paid 5.6 percent less in their new job than are men. The last row decomposes this gap. In both decompositions, gender differences in applications can explain a very substantial part of the gender wage gap. After accounting for differences in where men and women are applying, the gender gap in typical wages drops to 0.4 percent, while the gender gap in actual wage drops to 1.5 percent. Overall, the gender application gap is capable of explaining about 86 percent of the gap in typical wages and 73 percent of the gap in actual wages.

## 5 Suggestive evidence on mechanisms

Overall, the decomposition exercise in the previous section shows that gender differences in application behavior are capable of explaining a very large part of observed gender gaps in hiring outcomes. Although such decomposition exercises rest on strong assumptions on counterfactuals, the results suggest that differences in applications may play a very important role in shaping overall gender gaps in the labor market. This raises the natural question of *why* men and women are applying to such different jobs. Unfortunately, while our data offer a unique opportunity to document gender differences in application behavior for the broader labor market, they are less well suited to analyze why these differences exist. Doing so typically requires convincing sources of (quasi-)experimental variation that is not readily available in our data. Nevertheless, in this section we discuss some basic empirical checks of possible explanations for the observed gender differences.

### 5.1 Gender differences in the valuation of non-wage job characteristics

Several recent papers have suggested that women have a higher valuation of a range of non-wage job characteristics (see e.g. [Wiswall and Zafar, 2018](#), [Maestas et al., 2019](#), [Le Barbanchon et al., 2021](#), [Hotz et al., 2018](#); [Goldin, 2014](#)). If these characteristics correlate negatively with jobs' wage levels, this can explain why women are applying to lower paying jobs more than men. To examine this explanation in the data, we construct measures of three non-wage job characteristics that have been emphasized in previous work and compute gender gaps in the share of applications going to jobs with these characteristics. As in [Section 3](#), we compute these gender gaps after reweighting on labor market observables (see [Section 2.4](#)).

The non-wage job dimensions we consider are part-time vs. full-time jobs, jobs with different implied commute lengths and jobs with different levels of family-friendliness. Measures of all these are readily available in our data: One of the mandatory fields that UI recipients fill in when registering a job application is whether the applied-for job is full-time (37 hours per week in Denmark) or part-time (less than full-time work). Using information on the address or zip code of applied-for jobs as well as information on the zip code of the UI recipient, we can compute the implied commuting for each applied-for job.<sup>33</sup> Finally, based on the linked administrative data we compute a simple measure of a firm’s family-friendliness based on how much parental leave the average employee takes when they or their partner give birth. We correct the measure for the gender of the employees that take leave and treat firms that never experience an employee or partner giving birth as a separate category.<sup>34</sup> Appendix A.8.1 provides additional details.

Figure 4 shows gender gaps in applications and hiring to part-time vs. full-time jobs, to jobs with different implied commute lengths and to firms in different deciles in terms of family-friendliness. There is clear support for the idea that women are targeting jobs with different non-wage characteristics than men. Women are sending a markedly smaller share of their applications to full-time jobs, to jobs that are more than 60 minutes away and to firms that score low on our family-friendliness measure.<sup>35</sup> Further, in Appendix A.8.1 we verify that part-time jobs, jobs involving shorter commutes and jobs at family-friendly firms all tend to offer lower typical wages in our data. Overall, these results indicate that gender differences in the valuation of non-wage job characteristics may be one of the explanations for the gender application gap that we document in this paper.

Additionally, as we show in Appendix C.2, we also find some support for the idea that gender differences in the valuation of these job characteristics are related to motherhood (see e.g. Kleven et al. (2019); Hotz et al. (2018)). Focusing on individuals in an age window around childbearing (age 25-40), gender gaps in job applications tend to be larger between men and women with young kids than between men and women without kids. At the same time, however, we find substantial

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<sup>33</sup>Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the jobs’ postal code, see Harmon (2015) for further details on this data.

<sup>34</sup>We adjust for the gender of the employee because - as is well-known - men take systematically much shorter leaves than women in Denmark. Average leave length is of course undefined for firms that never experience an employee or partner giving birth, however, since these firms may arguable have low levels of family-friendliness, we opt to leave them in the analysis as a separate category.

<sup>35</sup>We see that women are particularly unlikely to apply to firms that do not experience a birth over the time period we consider. Among firms that do experience a birth, however, there is also a clear pattern of women applying more to firms where long parental leaves are common.

gender gaps in job application behavior also among individuals without kids.

## 5.2 General discrimination, self-fulfilling discrimination, overconfidence, beliefs and risk-aversion

A number of other possible explanations exists for the observed gender differences in application, including so-called self-fulfilling discrimination, gender differences in beliefs, (over)confidence and risk-aversion, or broad gender discrimination against women. Since we are not able to find any evidence for these explanations with our data, however, we only briefly discuss these below and relegate corresponding empirical results to Appendix C.

One possible explanation for the observed gender application gaps is a version of the “self-fulfilling discrimination” mechanism that has been proposed and documented in other settings (Lundberg and Startz, 1983; Coate and Loury, 1993; Glover et al., 2017). In the context of the job application process, the basic idea behind this is as follows: Gender discrimination in hiring implies that women face a lower likelihood of being hired when applying for some jobs. As a result women have an incentive to apply less to these jobs and more to jobs where the chance of being hired is higher. In this way, gender differences in the likelihood that an application turns into a hire may explain why there are gender differences in applications.

To look for evidence that this mechanism is at play in our data, we compute simple measures of how much more/less likely a woman is to be hired into a particular type of job when she applies for it, relative to when a man applies for it. We then correlate this measure with the observed gender gaps in applications to different jobs. To find evidence of self-fulfilling discrimination, we should see a positive correlation here; women should be applying less than men exactly to those jobs where women also face systematically lower returns to applying. This is not what we see, however. If anything the correlation is slightly negative in our data. While we thus find no evidence of self-fulfilling discrimination in our data, we note that these findings also do not rule out self-fulfilling discrimination. Ruling out this mechanism would require us to also examine gender differences in the counterfactual success probabilities of potential job applications that men and women *could have sent* but opted not to. This is not possible with our data. Appendix C.3 provides additional details and discussion.

Another possible explanation for the observed gender application gaps are gender differences

in beliefs or risk tolerance (see e.g. [Cortes et al., 2020](#)). If men are more (over)confident and systematically judge their labor market prospect to be better than women do - or if men simply are less risk averse - this could lead men to systematically target more high-paying but harder-to-get jobs than women.

To look for empirical evidence for this mechanism, we examine the speed of job finding for men and women; if gender differences in beliefs or risk preferences are causing men to systematically target harder-to-get jobs, this implies that men should be finding jobs at a slower rate than women. This is not borne out in the data. If anything, men tend to find jobs faster than women both in the raw data and after conditioning on observables. While this finding rules out gender differences in beliefs or risk preferences as the only source of differences in application behavior, we again note that it does not rule out that gender differences in beliefs or risk preferences are at play. If other differences across men and women also affect job finding rates, these other differences may explain why women are not finding jobs faster than men even if men do in fact have more optimistic beliefs or lower levels of risk aversion. [Appendix C.4](#) provides additional details and discussion.

Finally, we note that general discrimination against women in the hiring process could also generate the patterns we see in our data. If women face a lower probability of being hired throughout the labor market, standard models would predict that women should respond by applying for less attractive jobs that are easier to get. This would explain why women tend to apply for lower-paying jobs. At the same time, if the hiring discrimination faced by women is not fully offset by women targeting easier-to-get jobs, women might still end up with lower job finding rates than men.

## 6 Conclusion

In this paper we provide evidence on gender differences in the types of jobs men and women apply for. We do this by exploiting new administrative data on job applications made by the universe of Danish UI recipients which we link to additional administrative data on hires, firms and UI recipients characteristics.

We document substantial gender differences in applied-for jobs, even among men and women with similar labor market observables. These gender gaps in applications closely mirror observed gender gaps in actual hiring outcomes. In particular, women apply for systematically lower-paying

jobs.

Applying a standard decomposition method, we further show that gender differences in applications are able to explain a substantial part of observed gender gaps in hiring outcomes among men and women with similar labor market characteristics. Focusing on measures of wages in particular, the gender application gap can explain more than 70 percent of the residual gender gap in wages.

In the final part of our empirical analysis we provide suggestive evidence on why men and women are applying to such different jobs. We particularly find support for the idea women and men apply for different jobs because they have different valuations of non-wage job characteristics, such as hours, commuting time and family-friendliness.

Our results are consistent with recent work emphasizing that differences in the valuation of non-wage job characteristics are important drivers of gender gaps in the labor market. In terms of policies that try to combat these gender gaps, our results suggest an important role for policies that aim to influence the types of jobs and careers women seek out.

Finally, by documenting the importance of gender difference in job search and job applications, our results also point to several avenues for future research. First, the present paper shows that gender differences in job applications and hiring among unemployed men and women are important for overall gender gaps. It remains an open question how gender differences in job search and job applications among employed individuals further contribute to observed gender gaps over the course of a career.

Second, although our results suggest that non-wage job characteristics are important for understanding the gender application gap, many other mechanisms may also be at play. Unpacking the different mechanisms behind differences in job application in the general labor market therefore remains an important topic for future work.

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Table 1: Descriptives of the analysis sample

	Men Raw	Women Weighted	Women Raw		Men Raw	Women Weighted	Women Raw
Age	38.56	38.87	37.57	<b>Occupation</b>			
				Manager	0.03	0.03	0.02
<b>Education: Level</b>				Professional	0.16	0.16	0.24
Lower secondary	0.32	0.28	0.21	Associate professional	0.08	0.08	0.08
Upper secondary	0.48	0.51	0.50	Clerical support	0.08	0.08	0.13
Tertiary	0.18	0.20	0.28	Service/sales	0.14	0.15	0.28
Length (years)	13.21	13.41	13.76	Agricultural	0.02	0.02	0.01
				Craft	0.16	0.10	0.02
<b>Education: Field</b>				Plant/machine	0.08	0.10	0.03
General	0.50	0.46	0.42	Elementary	0.17	0.14	0.10
Education	0.01	0.01	0.02				
Humanities	0.04	0.03	0.06	<b>Industry</b>			
Social sciences	0.02	0.02	0.03	Agriculture	0.03	0.02	0.01
Business	0.12	0.13	0.18	Manufacturing	0.14	0.15	0.07
Natural sciences	0.01	0.01	0.01	Construction	0.10	0.08	0.02
Information	0.01	0.01	0.00	Trade	0.23	0.24	0.20
Engineering	0.19	0.20	0.05	Communication	0.04	0.05	0.03
Agriculture	0.03	0.04	0.02	Finance	0.02	0.02	0.01
Health	0.03	0.03	0.15	Real estate	0.01	0.02	0.01
Service	0.04	0.04	0.04	Services	0.14	0.14	0.11
				Public	0.20	0.21	0.44
<b>Region</b>				Culture	0.05	0.05	0.06
Hovedstaden	0.30	0.28	0.32				
Midtjylland	0.22	0.22	0.22	<b>Sector</b>			
Nordsjælland	0.13	0.12	0.12	Private sector	0.71	0.69	0.50
Sjælland	0.14	0.15	0.13	Public Sector	0.12	0.13	0.30
Syddanmark	0.22	0.22	0.21	Other sector	0.17	0.17	0.19
<b>Previous year</b>				<b>UI Spell</b>			
Employment, weeks	35.87	35.38	33.64	Length (weeks)	19.85	19.95	20.10
Public transfers, weeks	18.84	18.94	21.40	Applications per week	1.46	1.55	1.60

Notes: The table reports summary statistics for our analysis sample. Means or shares for the different variables are reported separately for men, for women before applying propensity score reweighting and for women after applying propensity score reweighting. All characteristics, except for the UI spell details, are measured prior to the unemployment spell.

Table 2: Summary table for descriptive gender gaps

	<b>Applications</b>	<b>Hiring</b>
Occupational segregation (Duncan index, 1-digit)	0.173 (0.004)	0.068 (0.006)
Occupational segregation (Duncan index, 2-digit)	0.231 (0.004)	0.105 (0.006)
Industry segregation (Duncan index, 1-digit)	0.099 (0.004)	0.074 (0.006)
Industry segregation (Duncan index, 2-digit)	0.108 (0.004)	0.081 (0.006)
Gap in mean firm wage level (Male-female gap, std. AKM fixed effect)	0.052 (0.026)	0.068 (0.010)
Gap in mean typical wage (Male-female gap, log typical wage)	0.019 (0.002)	0.025 (0.002)

Notes: In the top panel of this table we report the Duncan index measure of industry or occupational segregation at the 1- and 2-digit level, calculated based on application and hiring shares respectively (see footnote 24 for a definition of the Duncan index). The lower panel of the table reports the gender gaps in the mean firm wage level (standardized AKM fixed effect) and (log) typical wage across applied-for jobs and actual new jobs. The results are computed for the main analysis sample after applying propensity score reweighting to condition out differences in observables. Standard errors in parenthesis are calculated by bootstrapping individuals.

Table 3: Correlation between application and hiring shares

<b>Correlation between application and hiring shares</b>	
Occupations, 1-digit	0.953 (0.027)
Occupations, 2-digit	0.863 (0.031)
Industries, 1-digit	0.962 (0.022)
Industries, 2-digit	0.941 (0.028)
Firm wage level, deciles	0.945 (0.053)
Typical wage level, deciles	0.897 (0.042)

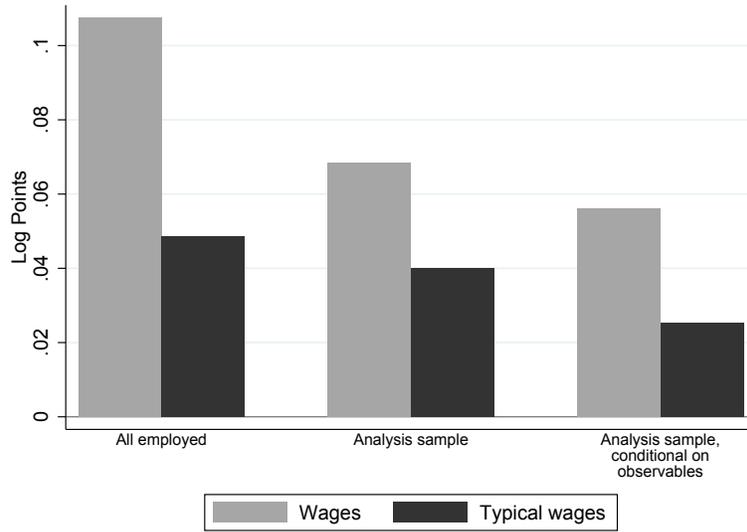
Notes: In this table we report the correlation between the gender gap in application shares and the gender gap in hiring shares across 1- and 2-digit occupations, across 1- and 2-digit industries, across deciles of the firm wage level distribution and across deciles of the typical wage distribution. The results are computed for the main analysis sample after applying propensity score reweighting to condition out differences in observables. Standard errors in parenthesis are calculated by bootstrapping individuals.

Table 4: Decomposing gender gaps

	<b>Baseline</b>	<b>Explained by applications</b>	<b>Residual</b>
Occupational segregation (Duncan index, 1-digit)	0.068 (0.006)	0.029 (0.008) [0.42]	0.039 (0.008) [0.58]
Occupational segregation (Duncan index, 2-digit)	0.105 (0.006)	0.031 (0.009) [0.29]	0.074 (0.008) [0.71]
Industry segregation (Duncan index, 1-digit)	0.074 (0.006)	0.018 (0.008) [0.24]	0.057 (0.007) [0.76]
Industry segregation (Duncan index, 2-digit)	0.081 (0.006)	0.018 (0.008) [0.23]	0.063 (0.007) [0.77]
Firm wage level (Male-female gap, std. AKM fixed effect)	0.068 (0.10)	0.051 (0.016) [0.75]	0.017 (0.018) [0.25]
Typical wage for job (Male-female gap, log typical wage)	0.025 (0.002)	0.022 (0.002) [0.86]	0.04 (0.003) [0.14]
Actual Wages (Male-female gap, log wage)	0.056 (0.006)	0.041 (0.005) [0.73]	0.015 (0.005) [0.27]

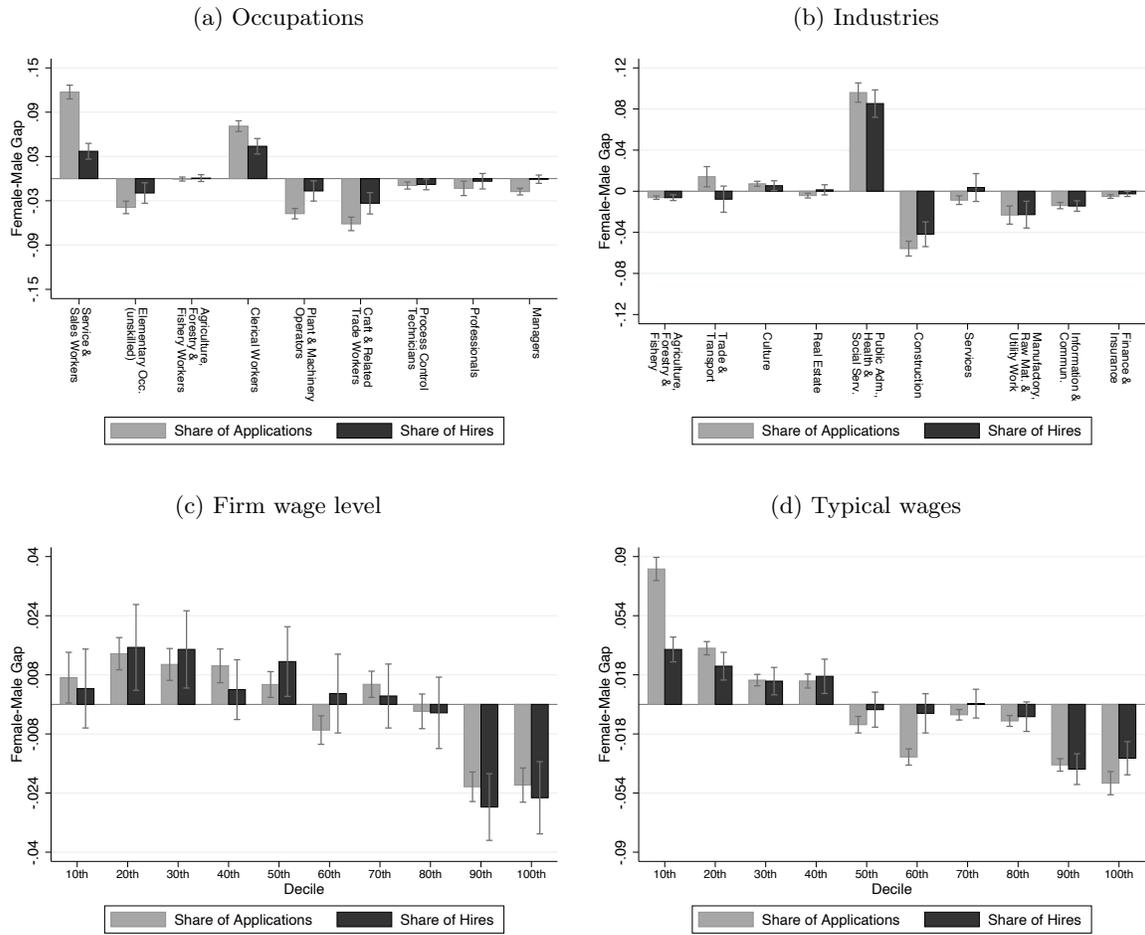
Notes: The table decomposes the baseline gaps in hiring outcomes after conditioning on observables. The gaps are decomposed into a part explained by applications and a residual gap. Standard errors based on bootstrapping individuals are shown in parenthesis. Brackets report the share of the baseline gap explained by each component.

Figure 1: Gender wage gaps



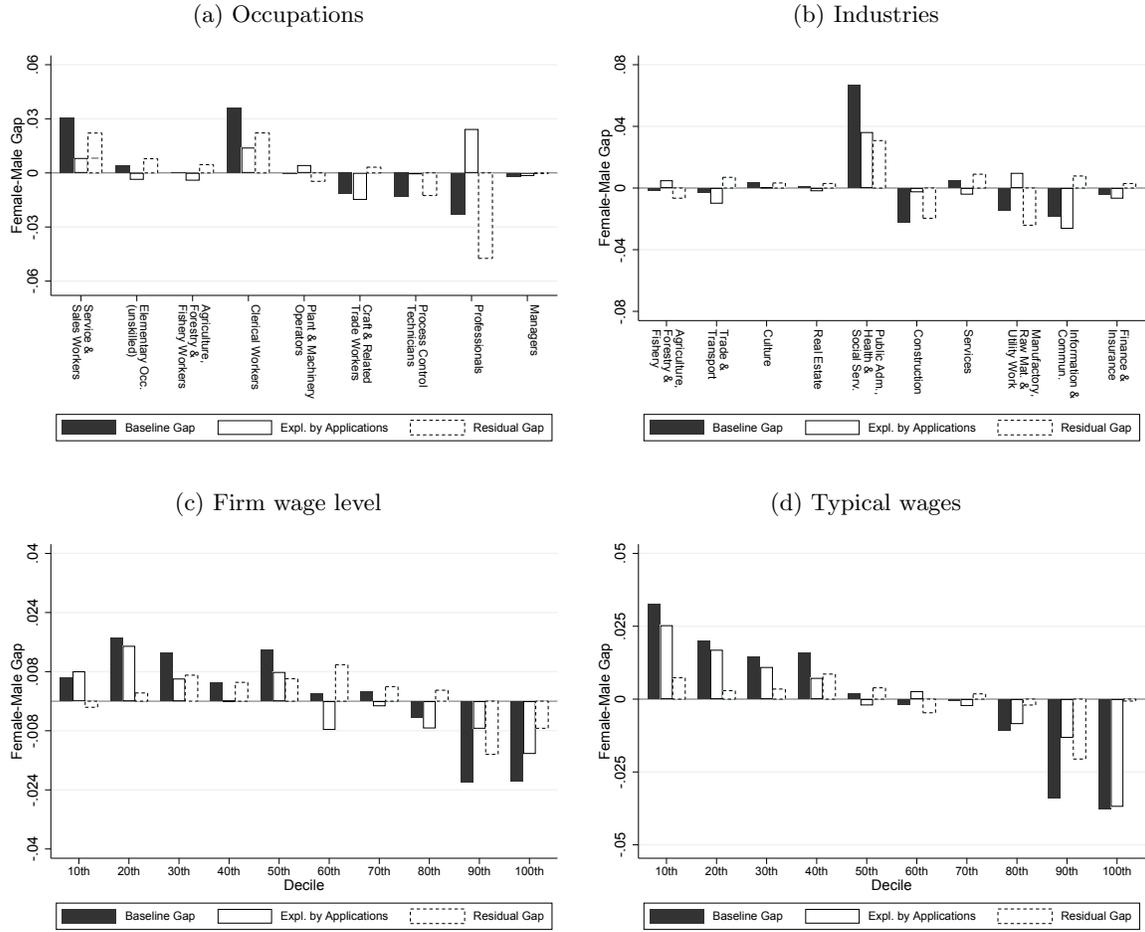
Note: Figure plots raw (unweighted) gender gaps in wages and typical wages for employed workers in 2015 ("All employed") and for our sample of unemployed ("Analysis sample"). The bars to the right ("Analysis sample, conditional on observables") control for observable differences between men and women by applying propensity score reweighting. The "All employed" sample is based on 5 percent sample of all individuals in regular employment in August 2015.

Figure 2: Gender gaps in applications and hiring outcomes



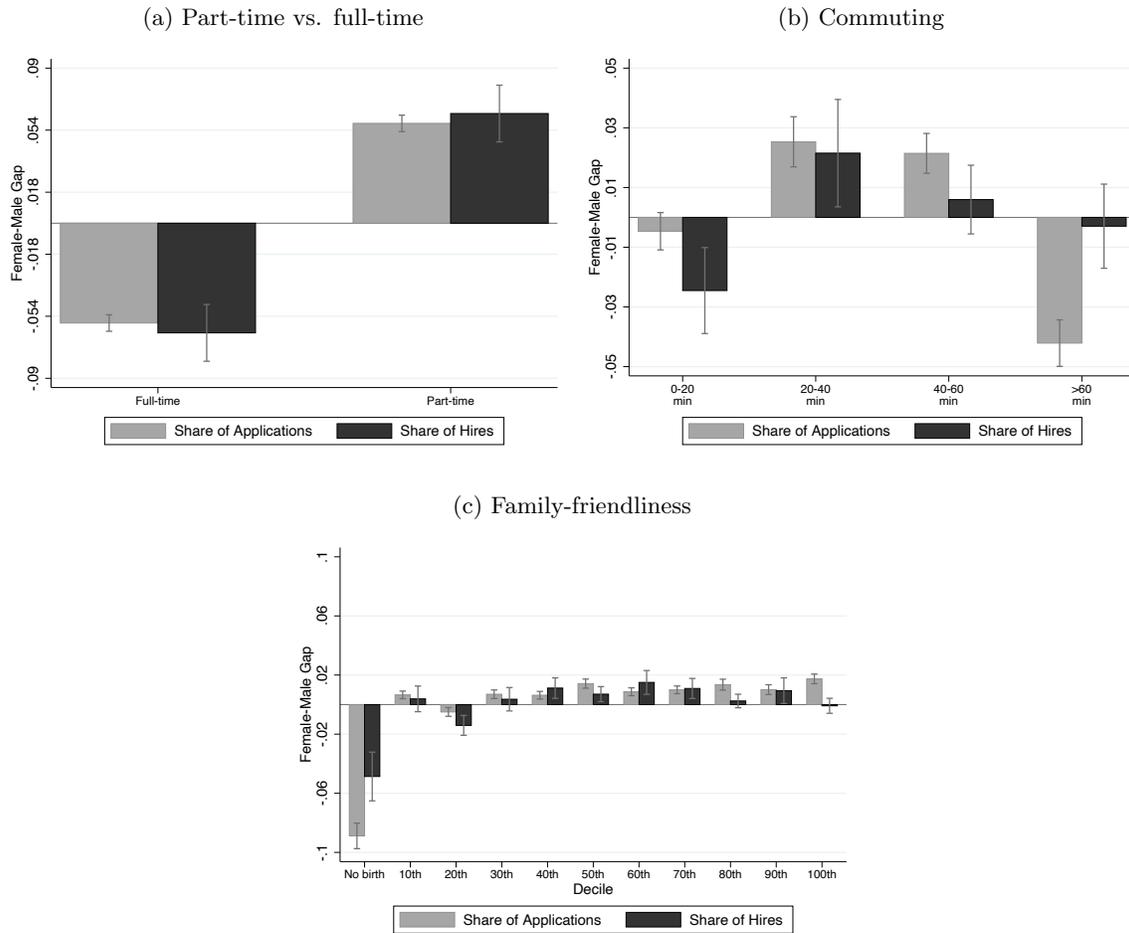
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. All gaps are based on the reweighted sample and are therefore conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure 3: Decomposing gaps in hiring shares



Note: Figure decomposes baseline gaps in the share of men and women hired into different types of jobs after conditioning on labor market observables. The gaps are decomposed into a part that explained by applications and a residual gap (see Equation 5). The computation of the baseline gaps is numerically equivalent to the computation of gender gaps in hiring shares in Section 3. Because the decomposition relies on additional trimming, the sample used is slightly different however (see Table A.1).

Figure 4: Gender gaps in applications and hiring across now-wage job characteristics



Note: The figures plot gender gaps in shares of applications going to specific types of jobs and corresponding gender gaps in hiring outcomes. Family-friendliness is measured based on the average parental leave taken when an employee or their partner gives birth. The measure is corrected for the gender of the employee. Firms where no employee or partner gives birth is included as a separate category as indicated on the x-axis (see Appendix A.8.1 for details). All gaps are based on the reweighted sample so are conditional on observables. The 95% confidence bars are based on standard errors clustered on the individual level.

## A Appendix: Data and measurements

### A.1 Data sets and sample selection steps

Our sample consists of UI recipients of Danish nationality entering new UI spells from September 2015 to September 2017. A new UI spell is defined when an individual who has not received UI benefits in the previous 4 weeks is observed with at least 4 consecutive weeks of UI payouts. For each UI recipient we identify and add all submitted job applications that have been registered in the Joblog system during the unemployment spell. The raw Joblog data is organized in several different databases with information on each edit (and save) of a given application entry. To generate the Joblog data used in this paper, we therefore pre-process these data sources and only include applications that were actually sent, and we further only select the first version of a given Joblog entry.<sup>36</sup>

To construct our final data set we make some additional sample restrictions, the effect of which we show in Table A.1 and also discussed in the main text. First, from the sample of new UI spells, we only consider the UI spells lasting at least 8 weeks. Second, we restrict our sample to individuals who register at least 4 applications in Joblog during their respective unemployment spell. Note that this condition is effective after dropping the last 4 weeks of applications, the individuals therefore need to have at least 4 registered applications during their UI spell. Third, we restrict our sample to individuals who leave UI for employment within the first year of their unemployment spell. Fourth, we drop all applications made in the last four weeks before entering employment. The data shows a drop in the number of applications that people register in Joblog about one month before they enter employment, reflecting that individuals have already accepted their new job at this point and are just waiting for it to start. We therefore drop applications from the last four weeks before the new jobs start based on the median transition time between a successful application and starting a job, as applications made while waiting for the new job to start may not represent an individual's general application behavior.<sup>37</sup>

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<sup>36</sup>Besides documenting search activity to qualify for UI, the Joblog section of the Jobnet website was developed with the goal of helping job seekers keep track of their job search. In addition to submitting information on jobs that the worker has applied for, workers can also use the Joblog form to register and keep track of vacancies that the worker is considering to apply for in the future and to register other job search events such as being called for an interview or being rejected. However, the coverage of these other events is much lower, and we therefore focus on applications only.

<sup>37</sup>The median transition time is 6 weeks, but we assume it takes 2 weeks from the application to the eventual job

To the job application data we add data from three administrative data bases: These data sources are IDA, BFL and DREAM. IDA, the Integrated Database for Labor Market Research, is a matched employer-employee panel containing socioeconomic information on the entire Danish population. BFL, the Employment Statistics for Employees, contains monthly data on jobs, paid hours of work and earnings. DREAM, is an event-history data set created by the ministry of employment tracing the participation of individuals in public income support programs at a weekly level. All data sets are available through servers at Statistics Denmark (see <https://www.dst.dk/en/TilSalg/Forskningservice>). We link applied-for firms to firms in the BFL registers using a string matching procedure which we explain in Section A.2.

Our final data set allows us to examine the characteristics of the firms and jobs that all male and female UI recipients apply to and compare them to the characteristics of the firms and jobs they eventually end up in. For some of our measures of job characteristics we have missing values for some of the applications, in Tables A.2 we document the prevalence.<sup>38</sup> In the analysis we simply leave out job applications when the job characteristics in question are missing.

In Figure A.1 we plot the survivor function for a version of our main analysis sample where we do not require individuals to find employment within 52 weeks. We also plot the average number of registered applications for each week in unemployment for the main analysis sample. As we discuss in Section 2.3 the average weekly number of applications during the unemployment spell is around 1.5 applications. Finally, in Figure A.2 we report the distribution of the number of submitted applications per week in our initial and final sample. Note that we discuss the dynamics in job search further in Section A.10.

## A.2 Data matching algorithm

Before matching reported job titles and firms to official classifications and registers, we perform an extensive cleaning of these entries. In this step, we streamline the notation between source and

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offer. The law requires active job search until the actual UI benefits stop (and the job starts), but naturally the type of jobs applied to are different, and the UI funds explicitly write that permanent positions should not be applied to when future employment is already secured. From 2019 the requirement was changed such that individuals with less than 6 weeks until the beginning of a new job will no longer have to register applications.

<sup>38</sup>The typical reason for missing job characteristics is that we were unable to link the application to either the firm id or the specific occupation, see Section A.2. The higher shares of missing applications for the industry and firm wage levels (AKM firm fixed effects) reflects that either the firm match was unsuccessful or the firm is so new (small) that e.g. the industry affiliation is not recorded in the employment register (BFL) or it is not a part of the connected set. See also Section A.3.

target files and correct obvious spelling mistakes.

As a first step in the actual matching, we use the self-reported job titles and link these to the official Danish occupational codes (DISCO). We exploit that many of the self-reported job titles have the actual occupation as a part of the self-reported title. Thus, as a first step we identify occurrences of the DISCO occupations in the reported job titles. We only consider as 1:1 matches in this step (43.4%), i.e. if a certain job title links to several occupations we do not treat it as a match. For remaining unmatched entries, we manually match some job titles to occupations as many job titles use acronyms that do not match to the standard classification.<sup>39</sup> This adds about 27.2% to the matches. Finally, we also use some fuzzy matching techniques on the remaining unmatched observations to circumvent misspellings in the job titles, adding the manual titles from the step before. We rank the potential matches along several scoring functions and only pick consistently high-ranked matches. For this we use `compget`, `speedist` and `soundex` routines from SAS as well as sub-string occurrences which adds 10.9%. Overall, we can thus map 81.5% of the applications to a DISCO group.

As the second matching step, we link the reported firm information to firm identifiers. With the mandatory reporting of firm name, zip code and city, we develop a matching procedure which matches this information to the official firm registers recording all Danish firms (CVR-register).<sup>40</sup> We can then use these links to identify firms in the registers at Statistics Denmark (BFL). Our matching procedure on firms also starts with perfect matches, using both firm name and zip codes. Here we have a 1:1 match for 66.3% of the applications in Joblog. We further add the sub-string matches which are spatially the closest to the reported firm address (13.9%). To link applications which we cannot match exactly on firm names, we employ a fuzzy matching procedure using the `matchit` command in STATA to identify the 50 closest matches. We then test these 50 potential matches using several scoring functions besides the one obtained from `matchit`. For each of the scores (5 in total), we calculate the ranking of the 50 potential matches (rank 1 is the best) and identify the “correct” match as the match which receives the best average rank (the scores we use

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<sup>39</sup>For example ‘social og sundhedshjælper’, Danish for social and health care workers, are most often reported as ‘sosu-hjælper’.

<sup>40</sup>The Danish central firm register (CVR-register in short) contains information on companies officially registered in Denmark. The register covers all firms, with the exception of privately held companies with an annual turnover below 50,000 DKK (about 7,500 USD). Each firm is registered with a uniquely identifiable CVR number that’s linkable to Danish administrative data sets.

are Bi-gram Similscore, Token, TokenSound from matchit and the compget and speedist functions in SAS). This adds further 6.2% to the matches, so we end up with an overall firm match of 86.4%.

### A.3 Measuring firm wage levels using AKM firm fixed effects

We use our matched employer-employee data to estimate an AKM model (Abowd et al., 1999) and use the estimated firm fixed effects as a measure of the firms wage premium. The AKM model captures implied firm fixed effects on wages, i.e. the firms' wage premium, by identifying moves of workers from one firm to another while simultaneously absorbing individual wage components in worker fixed effects. The model identification relies on the connection between firms in terms of worker movements. Thus, the firm fixed effects can only be recovered for the set of connected firms. We take advantage of the rich administrative data on the whole Danish working population, in particular the BFL data set (see Section A.1) covering monthly salaries, to construct a matched employer-employee panel from 2008 to 2015 with 306,900 firms.

In practice, we get a set of 290,108 firms connected by worker movements (5.5 percent of all firms are not connected to this set). We can therefore estimate the AKM firm fixed effects for 94.5 percent of the firms we observe in the labor market data between 2008 and 2015.<sup>41</sup> After the estimation, we subtract industry specific averages of firm effects from the estimated firm effects to ensure that the rankings we obtain account for industry differences in the level of firm effects. Further, to guarantee equal size of the decile bins, we employment weight these rankings with the number of employees in each firm as of August 2015, the month before we observe applications in Joblog.<sup>42</sup>

### A.4 Constructing typical wages

To construct our measure of the typical wage paid in a job with certain characteristics, we use data on the new jobs in our analysis sample to estimate a model that predicts wages based on the characteristics of the job. Since this is a pure prediction problem, we use a LASSO-based machine learning approach. Specifically, we consider a linear regression with log wages as the outcome variable and a very large number of potential explanatory variables based on the available job

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<sup>41</sup>We additionally include month fixed effects in the estimation to absorb any aggregate time trends.

<sup>42</sup>This additionally implies that we ensure that our results are not driven by estimated firm fixed effects from smaller firms which are known to be imprecisely estimated, see e.g. Andrews et al. (2008).

characteristics in our data. We then use LASSO estimation to select the subset of these variables that most efficiently trades off predictive power in-sample against the risk of overfitting.

As the baseline set of explanatory variables we include dummies for the industry and occupation of the job at both the 1-, 2- and 3- digit levels,<sup>43</sup> a dummy for whether we were able to obtain an estimated AKM fixed effect for the employing firm (see Section A.3) and the within-industry-demeaned AKM firm fixed effect when this is available.<sup>44</sup> In addition we include all pairwise interactions between these variables for a total of 10,407 baseline explanatory variables. We rely on the Rigorous-LASSO approach of Belloni et al. (2016) to choose the regularization parameters for the LASSO estimation. Because some individuals show up with several UI spells in our data, we allow for clustered disturbances at the individual level in estimation following. The estimation was conducted using the LASSOPACK implementation of Ahrens et al. (2019a,b).

Out of the 10,407 baseline explanatory variables, the Rigorous-LASSO selects 233 variables. As the final step, we run a standard OLS regression with log wage as the outcome variable and these 233 variables as explanatory variables (so-called Post-LASSO OLS) to arrive at our final prediction model.<sup>45</sup> Table A.3 summarizes the estimation and final model.<sup>46</sup>

We use this prediction model to compute our measures of the *typical* wage for new jobs as well as for applied-for job in our application data by simply predicting the log wage from the job’s characteristics. Since some applied-for jobs in our application data cannot be linked with firm/or occupation information (see Section A.2), we estimate two alternative prediction models by applying exactly the same procedure as above but excluding either firm or occupation information from the baseline set of explanatory variables. We use these alternative models to fill in typical wages also for the applications that cannot be linked with firm/or occupation information.

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<sup>43</sup>Note that the resulting set of dummies thus exhibit perfect multicollinearity by definition. Because of the penalization term in the LASSO objective function, however, perfect does not create a problem for estimation. Including all the dummies, however, is advantageous because the LASSO aims to select a parsimonious model with high predictive power. An obvious candidate for such a model is one that includes dummies for most of the aggregate 1-digit categories but in addition includes dummies for some 2- or 3-digit categories. The final prediction model indeed has this flavor (see Table A.3).

<sup>44</sup>Formally, we adopt the usual normalization that the within-industry-demeaned AKM firm fixed effect is zero for firms where it could not be obtained.

<sup>45</sup>As is common, some of the 233 variables selected by the Rigorous-LASSO turn out to be collinear and thus drop out in the OLS regression. As a result, the total number of variables in the final OLS regression is 116.

<sup>46</sup>As a crude benchmark for the prediction model, we can compare it to a simple OLS regression of log wages on the demeaned firm fixed effect variables and dummies for 3-digit industries and occupations. Despite having more parameters than the final Post-LASSO OLS (139 vs. 117), this benchmark model has a lower  $R^2$  in sample (0.188 vs. 0.202).

When comparing our analysis sample to the stock of employed individuals in Figure 1, we need to compute a version of our typical wage measure for the sample of employed individuals. We again do this using the approach from above and rerun the final Post-LASSO OLS estimation on the employment sample before predicting wages in this sample. <sup>47</sup>

Finally, note that the prediction models we use to compute typical wages only ever include job and firm characteristics but never include any worker characteristics. Our measure of the typical wage in a given job thus makes no attempt to capture that workers with different characteristics might face different wages in the same job.<sup>48</sup> Throughout our analysis of gender differences in applications and hiring outcomes we condition on observables so that we are in fact comparing men and women with similar labor market observables. A particular issue arises however if the typical wage offered in a particular type of job depends directly on gender (see Section 3.4). As a robustness check we therefore also present results where our measure of typical wages in a given job is based only on the typical wage paid to either men or women (See Appendix B.6). We do this simply by redoing the final Post-LASSO OLS estimation on either the male or female half of the sample.

In unreported results, we have also experimented with using alternative prediction approaches to the construction of our typical wage measure, including other machine learning approaches or simple linear regressions with a smaller set of variables. Our results are not sensitive to using these other approaches.

## A.5 Selecting our set of conditioning variables

To discipline which labor market observables we condition on we follow recent suggestions in the literature (e.g. Angrist and Frandsen, 2019; Athey and Imbens, 2019; Mullainathan and Spiess, 2017) and rely on a Machine Learning procedure. Specifically we use the double-LASSO procedure of Belloni et al. (2014) to select the most important variables for explaining the gender wage gap. We start by specifying a very large baseline set of variables that ex ante could be important to

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<sup>47</sup>Using our sample of employed individuals, we regress the log wage on the same set of 233 variables that was selected in the LASSO regression above and use the resulting model to predict log wages. An alternative approach would be to redo the full LASSO procedure on the sample of employed individuals. This would generally imply that a different set of explanatory variables would be selected, which makes the comparison in Figure 1 less meaningful. A major aim of the comparison in Figure 1 is to verify that the job characteristics we focus on in our analysis sample of UI recipients are relevant for the overall gender wage gap. This is obscured if our measure of jobs' typical wages are based on different sets of characteristics in the two samples.

<sup>48</sup>Formally, our measure of typical wage in a given job will reflect the wage paid to the average individual that is hired into this type of job in the data.

condition on when analyzing the gender wage gap. Using data on all the individuals in our analysis sample, along with the wages in their new jobs, the double-LASSO procedure then involves two separate LASSO regressions: First the LASSO is applied to a regression that has log wage in the new job as the outcome variable and includes the full set of baseline variables as regressors. Intuitively, this step selects out any variables in the baseline set that are relevant predictors of wages. Second, the LASSO is applied to a regression that has a female dummy as the outcome variable but again includes the full set of baseline variables as regressors. This step selects out variables that are significant correlates of gender. Combining all the variables selected in each of these two steps then gives the set of most important variables for explaining the gender gap in wages.<sup>49</sup> In each of the steps in the double-LASSO, a data-driven procedure is used to determine the penalty parameter for the LASSO estimation. See [Belloni et al. \(2014\)](#) and [Urminsky et al. \(2016\)](#) for additional discussion and formal results.

The baseline set of potential variables that we include in the two regressions consists of a set of 4,196 variables: To capture educational differences, the set contains years of education, as well as dummies for the field of study. To capture additional differences in general human capital, the set includes age, total work experience and total work experience over the last five years. To capture additional differences in specific human capital, the set includes dummies for the sector, industry and occupation of the previous job as well as continuous measures for the total work experience over the last five years in each of the different industries and occupations. To capture differences in dependence on public transfers, the set includes the total time spent on unemployment insurance, social assistance and other public transfers over the last five years. When including dummies for industry, occupation or field of education and when including continuous measures of industry or occupation-specific experience, we always include all possible measures at both the 1-, 2- and 3-digit level.<sup>50</sup> Finally, all variables are also interacted with both age, years of education, total work

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<sup>49</sup>The intuition here is that in order to play a significant role in explaining gender differences in wages a variable has to be strongly correlated with either wages or with gender. Variables that are weakly correlated with both however should not play an important role in explaining gender differences in wages. These are exactly the variables excluded by the double-LASSO.

<sup>50</sup>Our coding of occupations, industries and fields of education are based on the official definitions by Statistics Denmark, see also footnote 15. We note that the resulting set of included dummies exhibit perfect multicollinearity by definition. Because of the penalization term involved in the LASSO objective function, however, perfect multicollinearity does not create a problem for estimation. Including all the dummies, however, is advantageous because the LASSO aims to select a parsimonious set of variables with high predictive power. An obvious candidate for such a set is one that includes dummies for most of the aggregate 1-digit categories but in addition includes dummies for some 2- or 3-digit categories. The final set of selected variables indeed has this feature (see [Table A.4](#)).

experience and work experience over the last five years.<sup>51</sup>

The double-LASSO selects 332 of these variables which we use as our observable characteristics to condition on throughout the main analysis.<sup>52</sup> The estimation was carried out using the PDSLASSO implementation of Ahrens et al. (2018). Table A.4 summarizes the final selected set of variables that we condition on.

## A.6 Propensity score reweighting for descriptive results

As discussed in Section 2.4 and Section A.5, we use propensity score reweighting in all of our analysis. Using the notation introduced in Section 2.4, the reweighting scheme involves reweighting woman  $i$  by a weight equal to  $\frac{\hat{p}_i}{1-\hat{p}_i}$ , where  $\hat{p}_i$  is an estimate of the conditional probability of being male given observables,  $P(m_i = 1|x_i)$ .

After selecting the set of variables to include in our vector of observables,  $x_i$ , we follow the standard in the literature and estimate a logit model for the probability of being male, using the variables in  $x_i$  as our explanatory variables. We then obtain the  $\hat{p}_i$ s as the predicted probabilities from this model and use these to reweight the women in our sample.

In Figure A.3 we show the distribution of the estimated propensity scores in our sample. Note that we trim our sample to avoid very small or very large weights, see Table A.1. Specifically we trim all observations with an estimated propensity score larger than 0.99 or smaller than 0.01.

## A.7 Decomposition additional details

In this section we briefly translate the main insights and methodology of DiNardo et al. 1996 and Fortin et al. 2011, showing how propensity score reweighting can be used to construct estimates of the counterfactual hiring probabilities  $P_{A,X}^{W\tilde{}}(y)$  and  $P_X^{W\tilde{}}(y)$  underlying our decomposition exercise and introduced in Section 4.

We start by considering the counterfactual hiring probability for women if they had the same distribution of observables as men.:

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<sup>51</sup>In implementing this, we allow for variables to be interacted with themselves so that our baseline set includes squared terms in age, years of education, total work experience and work experience over the last five years.

<sup>52</sup>As is common, some of the 332 variables selected by the Rigorous-LASSO turn out to be collinear and thus in practice drop out from our conditioning set when we implement our propensity weighting procedure (see Section A.6). As a result, the total number of variables in the model that we use to estimate propensity scores is 302.

$$P_X^{\tilde{W}}(y) = \iint P^W(y|a, x) f_{a|x}^W(a|x) f_x^M(x) da dx$$

Multiplying and dividing by  $f_x^W(x)$  inside the integral, we can rewrite this as follows:

$$P_X^{\tilde{W}}(y) = \iint P^W(y|a, x) f_{a|x}^W(a|x) f_x^W(x) \Psi_X(x) da dx$$

Here we have defined  $\Psi_X(x) = \frac{f_x^M(x)}{f_x^W(x)}$ . The first insight is that  $P_X^{\tilde{W}}(y)$  is simply a weighted expectation of  $P^W(y|a, x)$  over the set of all women weighted by  $\Psi_X(x)$ :

$$P_X^{\tilde{W}}(y) = E [\Psi_X(x) P^W(y|a, x) | m = 0]$$

It follows that if the weighting function  $\Psi_X(x)$  was known,  $P_X^{\tilde{W}}(y)$  could be estimated by applying the weighting function and then simply computing the share of women hired into job type  $j$  in the weighted sample.<sup>53</sup> Now, by an application of Bayes rule,  $\Psi_X(x)$  is proportional to a simple function of the conditional probability for being male conditional on observable characteristics  $x$  (the propensity score):

$$\Psi_X(x) \propto \frac{P(m = 1|x)}{1 - P(m = 1|x)}$$

It follows that  $P_X^{\tilde{W}}(y)$  can be estimated via propensity score reweighting. We estimate a logit model for the likelihood of being male as a function of our observable characteristics  $x$  and then use the predicted probabilities from this model to reweight the women in our sample before computing hiring probabilities. As noted in the main text, this is equivalent to the propensity score reweighting we use to condition out observable differences between men and women, as introduced in Section 2.4.

Next consider the counterfactual hiring probability that women would have faced if they also

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<sup>53</sup>To see this more clearly, let  $I(y)$  be an indicator for ending one's UI spell by being hired into job type  $y$  and note that we have:

$$\begin{aligned} P_X^{\tilde{W}}(y) &= E [\Psi_X(x) P^W(y|a, x) | m = 0] = E [\Psi_X(x) E [I(y)|a, x] | m = 0] \\ &= E [E [\Psi_X(x) I(y) | a, x] | m = 0] = E [\Psi_X(x) I(y) | m = 0] \end{aligned}$$

The direct empirical counterpart of the last expectation is then the share of women hired into job type  $y$  after applying the  $\Psi_X(x)$  weights:  $\frac{1}{N_W} \sum_{m_i=0} I(y_i) \Psi_X(x_i)$  (here subscript  $i$  refers to individuals in the data, and  $N_W$  is the total number of women in the data).

had the same distribution of application behavior as men:

$$P_{A,X}^{W\tilde{}}(y) = \iint P^W(y|a,x) f_{a|x}^M(a|x) f_x^M(x) da dx$$

Letting  $f_{a,x}^M$  and  $f_{a,x}^W$  denote the joint distribution of application behavior and observables for men and women respectively, we rewrite this in a similar way as before:

$$P_{A,X}^{W\tilde{}}(y) = \iint P^W(y|a,x) f_{a,x}^W(a,x) \Psi_{A,X}(a,x) da dx$$

Here we have defined  $\Psi_{A,X}(a,x) = \frac{f_{a,x}^M(a,x)}{f_{a,x}^W(a,x)}$ . Similar to before we see that this implies that the counterfactual hiring probability can be estimated by reweighting the women according to the weighting function  $\Psi_{A,X}(a,x)$ . Also as before, an application of Bayes rule shows that the weighting function is proportional to a simple function of the conditional probability for being male conditional on both observable characteristics  $x$  and application behavior  $a$  (a different propensity score):

$$\Psi_{A,X}(a,x) \propto \frac{P(m=1|a,x)}{1-P(m=1|a,x)}$$

It follows that  $P_X^{W\tilde{}}(y)$  can also be estimated using propensity score reweighting. We estimate a logit model for the likelihood of being male as a function of our observable characteristics  $x$  and application behavior and then use the predicted probabilities from this model to reweight the women in our sample before computing hiring probabilities.

## A.8 Additional details on non-wage characteristics

### A.8.1 Measuring family-friendliness

To construct a simple measure of how family-friendly a firm is, we use data on how much parental leave employees at the firm tend to take when they become parents. The basic idea here is employees will tend to take longer leave if their firms offers more generous parental leave terms and/or are very tolerant towards employees going on leave.<sup>54</sup> Family-friendly firms that offer generous leave

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<sup>54</sup>During our sample window, government-mandated parental leave rules are as follows: Mothers are entitled to the following weeks on leave with compensation by the government: 4 weeks of leave just before birth, 14 weeks of maternity leave post birth, and subsequently 32 weeks of parental leave which can also be used by the father. Fathers are further entitled to 2 weeks of paternity leave immediately after birth. The government compensation during leave is at the UI benefit level, however most employment contracts in Denmark offer periods of leave where the

packages and are supportive of employees leave-taking should thus see longer parental leave periods among their employees.

We extract information on the duration of leave through the Danish register on sickness benefit claims (SGDP) which contains all information about benefits paid out by the government in connection with sickness and childbirths. Because this data covers both reimbursements made to firms with workers on leave and payments made directly to the workers on leave, it can be used to infer how much parental leave an employee takes. We focus on benefit claims after 2011 for both men and women and select all payouts which are related to childbirth and where the worker eventually returns to the same pre-birth employer. We then accumulate days with payments within individuals. As our data does not come with readily available information on birthdays for children, we infer these from starting a new parental leave spell (or having more than half a year between payments).<sup>55</sup> Finally, we calculate the average days on leave per birth at the firm level.

In Figure A.4 we plot the distribution of average leave lengths at the firm level. Before computing the the average leave taken at each firm, we correct for the gender of the employee to account for the fact that women take much longer leaves than men. We do this by simply demeaning the leave periods by gender.

Obviously, the average parental leave length will be undefined for firms that do not experience an employee or their partner giving birth at any point during the time period we consider. Since such firms may be likely to be at the bottom of the distribution in terms of family-friendliness, however, we do not exclude them from the analysis but include “no birth” as a separate category when analyzing family-friendliness.

### A.8.2 Non-wage characteristics and wage correlations

In Table A.5 we show the result from simple linear regressions where the dependent variable is the typical wage associated with a given application (see Section 2.2) and the independent variables are

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worker receives additional compensation by the firm. The duration of these employer coverage periods differ by firms, occupations and collective agreements. During the employer coverage periods the government subsidy is instead paid to the firm, and the firm simply continues to pay the worker his/her wage. When the employer coverage period ends the government payment is redirected and paid directly to the worker.

<sup>55</sup>Focusing on mothers this corresponds well with the official birth statistics. Note that some women may transition from one leave to another, thus we censor the length of leave at 14 months and regard any subsequent leave as a new birth/spell. This is however very limited in our data.

our selected non-wage characteristics.<sup>56</sup> The sample contains all the submitted applications, and we cluster standard errors at the level of the unemployment spell. The estimates show that part-time jobs, jobs involving shorter commutes and jobs at family-friendly firms all tend to offer lower typical wages in our data. As a result, if women send more applications to jobs characterized by e.g. shorter commute compared to males, this should translate into gender application gaps suggesting that women to a larger extent apply to jobs with lower typical wages.

## A.9 Further documentation of application behavior and data

### A.9.1 Survey about Joblog usage

Table A.6 and A.7 present results from a survey conducted among Danish UI recipients by [Mahlstedt et al. \(2019\)](#) in March 2018.<sup>57</sup> Table A.6 reports survey answers about how individuals log applications in Joblog. Looking at the first column, 41 percent of respondents report that they always log all the jobs they have applied for in Joblog regardless of whether they have fulfilled the logging requirements. An additional 21 percent report that they only log applications up to the point where they have satisfied their logging requirements but that they rarely apply for more jobs than what is required. Putting these together suggest that Joblog has close to full coverage for 63 percent of respondents. For the remaining 37 percent, however, the survey responses suggest that the Joblog data often misses some job applications that they have made beyond the required number. The second and third columns report corresponding numbers by gender. These show a similar pattern overall, although men are somewhat more likely to say that they often apply to jobs that they do not register. 42 percent of men say that they often apply to more jobs than they register, while only 32 percent of women say so.

To get a sense of how many applications may be missed by the Joblog data, Table A.7 presents survey responses about the total number of job applications sent the past month and the number of job applications sent that were not registered in Joblog. In addition, the Table also shows the actual number of registered Joblog applications made by the survey respondents in the month before the survey. This was computed by linking survey responses with the actual Joblog data. On average, survey respondents report applying for 11.5 jobs in total over the past month. Of those jobs, survey

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<sup>56</sup>We remove applications where the relevant job characteristics are missing from the analysis when necessary.

<sup>57</sup>We thank the authors for making this data available.

respondents on average say they average failed to register 2.4 jobs in Joblog. This suggest that Joblog covers 80 percent of actual applications. The bottom of the table instead shows that average number of jobs respondents actually registered in Joblog was 8.0. Relative to the total number of reported applications, this suggest that respondents on average failed to register 3.5 applications, implying that Joblog on average covers 69 percent of all applications.<sup>58</sup> The table also reports separate numbers for men and women. Women self-report failing to register 2.1 jobs on average, implying that joblog covers 82 percent of applied-for jobs for women. Alternatively, comparing total reported applications to actual joblog registrations, suggest that women on average fail to register 3.8 applications, corresponding to a coverage of 68 percent. Corresponding calculations for men suggest that Joblog covers between 72 and 76 percent of applied-for jobs for men.

### **A.9.2 Coverage and representativeness of the Joblog data**

In this section we present additional evidence and validity checks regarding the quality of the Joblog application data. In doing so we exploit the fact that the data is linked to actual job outcomes.

First, we verify how the application data relates to actual hiring outcomes. If the Joblog application data accurately capture actual application behavior, we would expect the application data to be highly predictive of the type of job that each UI recipient ends up being hired into. To assess whether this is the case, we benchmark the predictive value of the application data against a known strong predictor of job outcomes: the characteristics of UI recipients previous job.

Table A.8 compares how the Joblog application data and prior job characteristics predict respectively the industry, the occupation, the firm wage level or the typical wage level of a UI recipient's new job. Each column of the table corresponds to a different prediction model estimated on our analysis sample. When predicting the industry of the new job, we use a simple multinomial logit model that includes either dummies for the industry of the previous job, the share of applications going to each industry or both sets of variables. Similarly, when predicting the occupation of the new job we use a multinomial logit model that includes either dummies for the occupation of

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<sup>58</sup>This difference could reflect imperfect recall among survey respondents or could relate to measurement error from the timing of registered jobs and/or the precise interpretation of the survey question. Registering applied-for jobs in Joblog can be done retroactively so the interpretation of the survey question could either refer to the date at which applications were sent or to the date at which the application was entered into the Joblog system. Additionally, the fact that UI recipients are able to register other activities besides formal job applications introduces some ambiguity about the interpretation of the survey question (if for example UI recipients have registered that they reached out to a friend about a specific job).

the previous job or the share of applications going to each occupation. For both industries and occupations, we exclude one small industry/occupation for which the model obtains near perfect predictions for a few observations.<sup>59</sup> When predicting the firm wage level or the typical wage of the new job, we use a simple linear regression that includes either the firm wage level or the typical wage of the previous job, or includes the mean of the firm wage level or typical wage across the applied-for jobs. For the linear regression models, we measure the predictive power simply using the regression  $R^2$ . For the multinomial logit models, we use McFadden's *pseudo- $R^2$* .

Looking across Table A.8, we see that models that predict job outcomes only using application data perform quite similarly to models that instead use prior job characteristics. Application data does do markedly worse than prior job characteristics when predicting the occupation of the new job (column (4) vs (5)) but only slightly worse for firm wage level and the typical wage (columns (7) and (10) vs. (8) and (11)). At the same time, application data actually does better than prior job characteristics when predicting the industry of the new job (column (1) vs. (2)).

For models that include both prior job characteristics and application data (columns (3), (6), (9) and (12)), we see that application data remains highly predictive even after prior job characteristics have been conditioned on. Adding the application variables alongside prior job characteristics always leads to sizeable increases in the (*pseudo- $R^2$* ) relative to models that only use prior job characteristics. Moreover the application variables are always highly statistically significant in the combined models. Overall, we conclude that the Joblog application data is highly predictive of later job outcomes.

In Tables, A.9 and A.10, we repeat the prediction exercise separately for the men and women in the data. Results are very similar and we see little indication of systematic differences in the predictiveness of job applications for later job outcomes. While job application information appears to predict occupational outcomes slightly better for men, they instead appear to predict wage outcomes slightly better for women. Moreover, the differences are small throughout.

As a further check on the representativeness and quality of the Joblog application data, we examine how often we are able to trace a new hire back to a job application that is contained in

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<sup>59</sup>Specifically, we exclude individuals from the sample who find a job in the smallest industry or occupation, respectively, as well as individuals whose prior job was in this industry or occupation. Results are almost identical if these observations are included, however, we see indications that the likelihood function becomes ill-behaved in some specifications in this case, reflecting that some observations are predicted nearly perfectly.

our data. For 47 percent of the new hires, we are able to identify a previous application that the UI recipient sent to the firm in question. This is informative about the representativeness of the data. To see why, assume that the Joblog data covers a share  $r$  of all applications and that the share of applications that we successfully match to firms in our data matching procedure is  $s$ . In this case, our data will contain firm information for a share  $s \cdot r$  of all applied-for jobs. Next assume that the fraction of jobs that stem from a job application is  $j$ . If the applied-for jobs in our data are a representative subset of all applied-for jobs, the share of new hires that we should be able to trace back to an application,  $t$ , should then be:

$$t = j \cdot r \cdot s$$

Based on independent survey data from Table A.7 we estimated that the raw Joblog data contain between 69 and 80 percent of all applied-for jobs, that is  $r$  is between 0.69 and 0.80. Furthermore, as described in Section 2.1,  $s = 0.86$  in our data matching procedure. Finally, to gauge the share of hires that stem from a job application,  $j$ , we rely from Statistics Denmark’s official survey *Arbejdskraftundersøgelsen* on how unemployed Danes report landing their first job out of unemployment (Engman (2019)). In these data, 11 percent of respondents report landing their job in a way that is very unlikely to have involved the worker applying for the job (the job resulted from work at a temp agency, they got the job via their educational institution as an internship or the job seekers themselves advertised publicly), while 58 percent of respondents report landing their job in a way that almost surely involved making a formal job application (they themselves applied to a posted position, they applied to a firm with no posted positions or they were directed to the job by the employment agency or other authorities).<sup>60</sup> To arrive at an estimate for the fraction of hires that stem from workers applying for the job, we simply assume that half of the remaining jobs involved a job application. This implies that about 73 percent of new hires out of unemployment involve the worker applying for the job at some point so that  $j = 0.73$ .<sup>61</sup>

Plugging in these values, we see that if the applied-for jobs in our data is representative, the

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<sup>60</sup>The remaining respondents report landing their jobs through channels that may have involved applying for the job application but may also have involved receiving a job offer more directly. This includes finding the job through an acquaintance or finding a job after having been contacted by the firm.

<sup>61</sup>Alternatively, we could use 0.58 as a lower bound on  $r$  and use 0.89 as an upper bound. Plugging into the formulate above, we then see that if the applied-for jobs in our data is representative, the share of new hires that we should be able to match,  $t$ , should be between  $0.58 \cdot 0.69 \cdot 0.86 = 0.34$  and  $0.89 \cdot 0.80 \cdot 0.86 = 0.61$ .

share of new hires that we should be able to match,  $t$ , should be between  $0.73 \cdot 0.68 \cdot 0.86 = 0.43$  and  $0.73 \cdot 0.80 \cdot 0.86 = 0.50$ . As noted, we in fact have  $t = 0.47$  in our data, consistent with the data containing a representative subset of all applied-for jobs.

Finally, we can also compute how often we are able to trace a new hire back to a job application that is contained in our data for men and women respectively. For 41 percent of all new male hires, we are able to identify a previous application that the UI recipient sent to the firm in question. For women, the corresponding number is 53 percent. A likely reason for this difference is that Danish men are more likely to find jobs in ways that do not involve a formal job application (see e.g. [Engman \(2019\)](#) for evidence of this for the overall labor market). This mechanically implies that we should be able to match a smaller share of new male hires to a job application in our data. Because we do not have reliable data on how often this occurs for unemployed men and women, however, we cannot assess quantitatively how the gender-specific match rates match up with our other data.

## A.10 Changes in application behavior over time

In our analysis we pool all applications sent during the unemployment spell and analyze the composition of this pool across different individuals. Thereby we are potentially neglecting interesting gender differences in dynamics or changes in application behavior occurring over time during the unemployment spell. To quantify the importance of such dynamics we create a panel data that for each person and unemployment spell contains an observation that for each month of the spell. We then run an event study regression that uses the the monthly average typical wage of applied-for job as the outcome variable and includes dummies for the number of months since entering unemployment as the explanatory variables.<sup>62</sup> We run this separately for men and women and include person-by-spell fixed effects.<sup>63</sup> The results of this regression shows how men and women change their application behavior over time within an unemployment spell.

Figure [A.5](#) shows the estimates of gender specific time profile of job search from this regression. Changes in application behavior over time are modest and are extremely similar for men and women. Examining gender gaps in applications at different times throughout the unemployment

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<sup>62</sup>We use the month of entry into unemployment as the baseline and omit a dummy for this month in the regression.

<sup>63</sup>We let the fixed effect be specific to each unemployment spell because some individuals in our data show up with more than one unemployment spell. We cluster standard errors at the level of the unemployment spell.

spell therefore yields very similar results to the pooled results presented in the main text. For additional evidence on the dynamics of job search in our setting see [Glenny et al. \(2020\)](#).

Table A.1: Sample selection and trimming

	Individuals	Spells	Applications
<b>Inflow</b>	<b>227,515</b>	<b>261,529</b>	<b>7,422,148</b>
1. Minimum 8 weeks spell length	177,145	194,660	7,019,513
2. Spells with $\geq 4$ applications	170,304	185,959	7,008,284
3. Employment within 52 weeks	105,879	114,375	3,439,690
4. Censoring last 4 weeks of applications	105,879	114,375	2,911,585
<b>Analysis sample</b>	<b>105,879</b>	<b>114,375</b>	<b>2,911,585</b>
After trimmning for descriptive analysis*	100,267	108,172	2,790,216
After trimming for decomposition **	92,128	98,924	2,599,222

Notes: The top of the table shows the number of individuals, unemployment spells as well as number of applications in the base UI inflow data and after applying each of our four sample restrictions. The bottom of the table shows how the analysis sample changes when we apply trimming to remove observations with extreme propensity scores. (\*) refers to the trimming we use in our descriptive analysis where propensity scores are estimated based only on labor market observables (see Section 2.4). (\*\*) refers to the trimming we use in the decomposition exercise where propensity scores are estimated based on both labor market observables and application behavior (see Section 4).

Table A.2: Share of missing job characteristics

	Firm ID	Occupation	Industry	Firm Wage Level	Typical Wage	Wage
Applications	0.183	0.190	0.328	0.330	0.067	1.000
Hires	0.000	0.078	0.062	0.062	0.000	0.099

Notes: The table shows the share of missing job characteristics both for applications and for the jobs that UI recipients are hired into in the analysis sample.

Table A.3: Prediction model summary

Model and estimation summary:	
Explanatory variables in baseline set:	10,407
Variables selected in Rigorous-LASSO:	233
Parameters in final Post-LASSO OLS model:	117
$R^2$ for Post-LASSO OLS model (in sample):	0.202
Summary of selected variables:	
Demeaned firm fixed effect	
7 dummies for 1-digit occupations	
7 dummies for 2-digit occupations	
17 dummies for 3-digit occupations	
7 dummies for 1-digit industries	
7 dummies for 2-digit industries	
7 dummies for 3-digit occupations	
161 occupation-industry interactions	
9 occupation-firm fixed effect interactions	
10 industry-firm fixed effect interactions	

Notes: The table summarizes the main prediction model used to construct the measures of typical wages. The difference between the number of selected variables in the Rigorous LASSO and the number of parameters in the final Post-LASSO OLS model reflect that some of the selected variables are perfectly multicollinear (see footnote 45).

Table A.4: Summary of observables selected in double-LASSO

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*Non-interacted continuous variables:*

Age, years of education  
2 continuous measures of experience from 1-digit industries  
9 continuous measures of experience from 2-digit industries  
12 continuous measures of experience from 3-digit industries  
4 continuous measures of experience from 1-digit occupations  
1 continuous measures of experience from 2-digit occupations  
32 continuous measures of experience from 3-digit occupations

*Non-interacted dummy variables:*

4 dummies for the 1-digit occupation of most recent job  
13 dummy for the 2-digit occupation of most recent job  
29 dummies for the 3-digit occupation of most recent job  
3 dummies for the 1-digit industry of most recent job  
6 dummies for the 2-digit industry of most recent job  
10 dummies for the 3-digit industry of most recent job  
2 dummies for the sector of the most recent job  
3 dummies for education field at the 1-digit level  
8 dummies for education field at the 2-digit level  
17 dummies for education field at the 3-digit level

*Interactions involving only continuous variables:*

5 interactions involving only combinations of age, years of education or experience  
2 interactions involving continuous measures of past receipt of public transfers  
47 interactions involving continuous measures of experiences from specific occupations  
10 interactions involving continuous measures of experiences from specific industries

*Interactions involving discrete variables:*

42 interactions involving dummies for the occupation of most recent job  
16 interactions involving dummies for the industry of most recent job  
3 interactions involving dummies for the sector of the most recent job  
50 interactions involving dummies for field of education

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Notes: The table summarizes the set of 332 variables selected in the double-LASSO. Interaction terms always involve either age, years of education, total work experience or work experience over the last five years as one of the two interacted variables. We also allow for variables to be interacted with themselves, corresponding to a squared term (see footnote 51).

Table A.5: Correlation between job characteristics and typical wages

	(1)	(2)	(3)	(4)
	Log typical wage	Log typical wage	Log typical wage	Log typical wage
Commute (minutes)	0.00025*** (0.00000)			0.00024*** (0.00000)
Part-time (indicator)		-0.07170*** (0.00057)		-0.07140*** (0.00057)
Family-friendliness (days of leave)			-0.00014*** (0.00000)	-0.00014*** (0.00000)
Constant	5.155*** (0.0005)	5.179*** (0.0004)	5.180*** (0.0004)	5.175*** (0.0005)
Observations	2,752,177	2,752,177	2,752,177	2,752,177
R-squared	0.014	0.0024	0.006	0.045

Notes: \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ . The table shows the result of regressing the log typical wages of an applied-for job in the main analysis sample on different non-wage job characteristics. Jobs with missing data on any of the involved variables have been dropped in all specifications. Standard errors are clustered at the spell level.

Table A.6: Survey question "Which of these statements best describes your use of Joblog?"

<b>Answer:</b>	<b>Overall:</b>	<b>Men:</b>	<b>Women:</b>
Fulfill requirements, often applied to more jobs	36%	42%	32%
Fulfill requirements, rarely applied to more jobs	21%	19%	23%
Always register all applied-for jobs	41%	38%	44%
Never register applications	1%	2%	1%
Number of respondents	1236	515	721

Notes: The table shows answers to the question "Which of these statements best describes your use of Joblog?" based on the survey of UI recipients conducted in [Mahlstedt et al. \(2019\)](#).

Table A.7: Self-reported and registered applications in the previous month

	Mean, overall	Mean, men	Mean, women
<b>Survey answers</b>			
# of applied-for jobs	11.5	11.2	11.7
# of applied-for jobs <u>not</u> registered	2.4	2.7	2.1
<b>Joblog data</b>			
# of applied-for jobs	8.0	8.1	7.9

Notes: The top part of the table shows the reported number of job applications sent over the last month and the reported number of these job applications that were not registered in Joblog based on the survey of UI recipients conducted in [Mahlstedt et al. \(2019\)](#). The bottom part of the table shows the actual number of jobs registered in Joblog by the survey respondents in the month prior to the survey.

Table A.8: Predicting job outcomes from application data vs. prior job characteristics

Job Outcome:	Industry (1-digit)			Occupation (1-digit)			Firm wage level			Typical wage		
Model:	Multinomial logit			Multinomial logit			Linear regression			Linear regression		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Explanatory variables:</i>												
Characteristics of previous job	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Characteristics of applied-for jobs	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
# of parameters	72	72	136	56	56	105	2	2	3	2	2	3
(pseudo-)R-squared	0.280	0.259	0.365	0.334	0.411	0.506	0.044	0.079	0.106	0.257	0.287	0.387
<i>p</i> -value, test of excluding applied-for job variables	< 0.01		< 0.01	< 0.01		< 0.01	< 0.01		< 0.01	< 0.01		< 0.01

Notes: The table examines the predictiveness of job applications and past job characteristics for the sample overall. Columns (1)-(3) correspond to multinomial logit models for the 1-digit industry of the UI recipients new job. Explanatory variables in these models are dummies for the 1-digit industry of the previous job or the share of job applications sent to jobs in each 1-digit industry. Columns (4)-(6) correspond to multinomial logit models for the 1-digit occupation of the UI recipients new job. Explanatory variables in these models are dummies for the 1-digit occupation of the previous job or the share of job applications sent to jobs in each 1-digit occupation. Specifications in Columns (1)-(6) exclude individuals who found a job in the smallest industry or occupation, or who had their previous job in this industry or occupation (see footnote 59). Columns (7)-(9) correspond to linear regressions where the outcome variable is the industry-demeaned firm fixed effect for the UI recipients new job. Explanatory variables in these models are the industry-demeaned firm fixed effect for the UI recipients previous job or the average industry-demeaned firm fixed effect across all the applied-for jobs. Columns (10)-(12) correspond to linear regressions where the outcome variable is the typical wage of the UI recipients new job. Explanatory variables in these models are the typical wage of the UI recipients previous job or the average typical wage across all the applied-for jobs. The table reports the  $R^2$  for the linear regression models. For the multinomial logit models, the table reports the McFadden's pseudo- $R^2$ . The last row of the table show the  $p$ -value for testing the exclusion of all explanatory variables pertaining to applied-for jobs.

Table A.9: Predicting job outcomes from application data vs. prior job characteristics, men only

Job Outcome:	Industry (1-digit)			Occupation (1-digit)			Firm wage level			Typical wage		
Model:	Multinomial logit			Multinomial logit			Linear regression			Linear regression		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Explanatory variables:</i>												
Characteristics of previous job	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Characteristics of applied-for jobs	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
# of parameters	72	72	136	56	56	105	2	2	3	2	2	3
(pseudo-)R-squared	0.258	0.257	0.354	0.308	0.431	0.506	0.038	0.097	0.117	0.181	0.266	0.338
<i>p</i> -value, test of excluding applied-for job variables	< 0.01		< 0.01	< 0.01		< 0.01	< 0.01		< 0.01	< 0.01		< 0.01

Notes: The table examines the predictiveness of job applications and past job characteristics for the male half of the sample. Columns (1)-(3) correspond to multinomial logit models for the 1-digit industry of the UI recipients new job. Explanatory variables in these models are dummies for the 1-digit industry of the previous job or the share of job applications sent to jobs in each 1-digit industry. Columns (4)-(6) correspond to multinomial logit models for the 1-digit occupation of the UI recipients new job. Explanatory variables in these models are dummies for the 1-digit occupation of the previous job or the share of job applications sent to jobs in each 1-digit occupation. Specifications in Columns (1)-(6) exclude individuals who found a job in the smallest industry or occupation, or who had their previous job in this industry or occupation (see footnote 59). Columns (7)-(9) correspond to linear regressions where the outcome variable is the industry-demeaned firm fixed effect for the UI recipients new job. Explanatory variables in these models are the industry-demeaned firm fixed effect for the UI recipients previous job or the average industry-demeaned firm fixed effect across all the applied-for jobs. Columns (10)-(12) correspond to linear regressions where the outcome variable is the typical wage of the UI recipients new job. Explanatory variables in these models are the typical wage of the UI recipients previous job or the average typical wage across all the applied-for jobs. The table reports the  $R^2$  for the linear regression models. For the multinomial logit models, the table reports the McFadden's pseudo- $R^2$ . The last row of the table show the  $p$ -value for testing the exclusion of all explanatory variables pertaining to applied-for jobs.

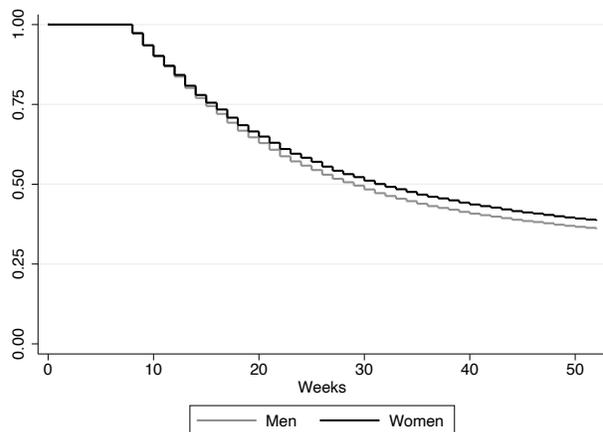
Table A.10: Predicting job outcomes from application data vs. prior job characteristics, women only

Job Outcome:	Industry (1-digit)			Occupation (1-digit)			Firm wage level			Typical wage		
Model:	Multinomial logit			Multinomial logit			Linear regression			Linear regression		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Explanatory variables:</i>												
Characteristics of previous job	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Characteristics of applied-for jobs	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
# of parameters	72	72	136	56	56	105	2	2	3	2	2	3
(pseudo-)R-squared	0.245	0.213	0.322	0.324	0.382	0.490	0.050	0.063	0.097	0.299	0.281	0.405
<i>p</i> -value, test of excluding applied-for job variables	< 0.01		< 0.01	< 0.01		< 0.01	< 0.01		< 0.01	< 0.01		< 0.01

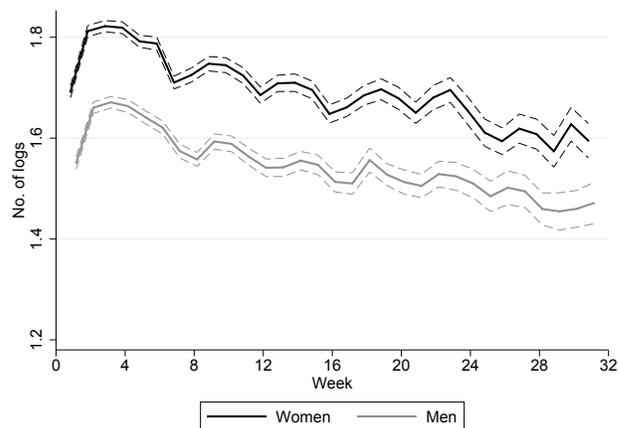
Notes: The table examines the predictiveness of job applications and past job characteristics for female half of the sample. Columns (1)-(3) correspond to multinomial logit models for the 1-digit industry of the UI recipients new job. Explanatory variables in these models are dummies for the 1-digit industry of the previous job or the share of job applications sent to jobs in each 1-digit industry. Columns (4)-(6) correspond to multinomial logit models for the 1-digit occupation of the UI recipients new job. Explanatory variables in these models are dummies for the 1-digit occupation of the previous job or the share of job applications sent to jobs in each 1-digit occupation. Specifications in Columns (1)-(6) exclude individuals who found a job in the smallest industry or occupation, or who had their previous job in this industry or occupation (see footnote 59). Columns (7)-(9) correspond to linear regressions where the outcome variable is the industry-demeaned firm fixed effect for the UI recipients new job. Explanatory variables in these models are the industry-demeaned firm fixed effect for the UI recipients previous job or the average industry-demeaned firm fixed effect across all the applied-for jobs. Columns (10)-(12) correspond to linear regressions where the outcome variable is the typical wage of the UI recipients new job. Explanatory variables in these models are the typical wage of the UI recipients previous job or the average typical wage across all the applied-for jobs. The table reports the  $R^2$  for the linear regression models. For the multinomial logit models, the table reports the McFadden's pseudo- $R^2$ . The last row of the table show the  $p$ -value for testing the exclusion of all explanatory variables pertaining to applied-for jobs.

Figure A.1: Survival rates and registered applications

(a) Kaplan-Meier survivor functions in non-employment (un-weighted)



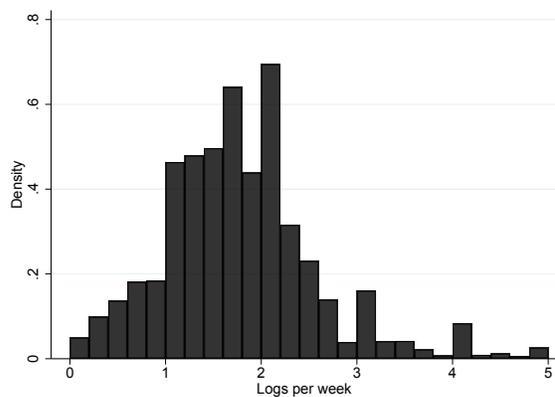
(b) Average number of logged applications



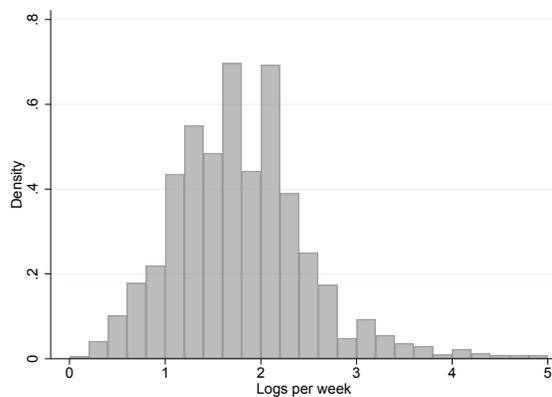
Note: The figures plot gender-specific Kaplan-Meier survival rate in nonemployment estimates (left) and the average number of registered applications (right). The X-axes measure weeks since the start of the UI spell. The Kaplan-Meier estimates of the survivor function in nonemployment are estimated on a version of the main analysis sample where the requirement of finding a job within a year has not been imposed. Average number of registered applications is shown for the main analysis sample.

Figure A.2: Distribution of registered applications per week

(a) Baseline sample

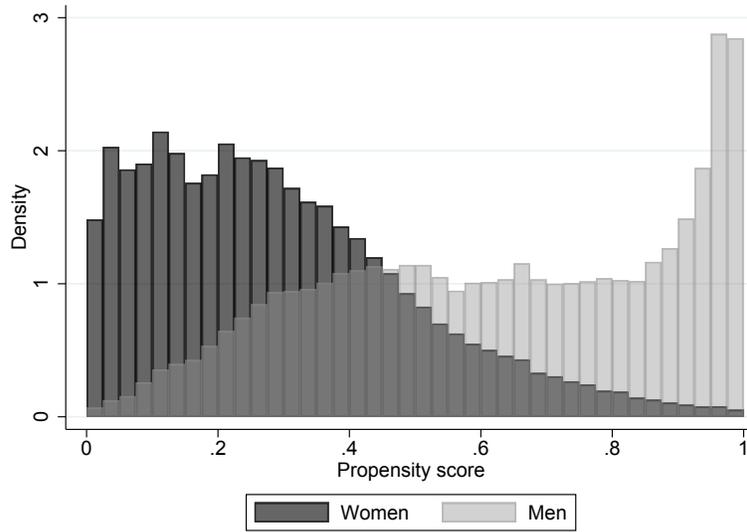


(b) Analysis sample



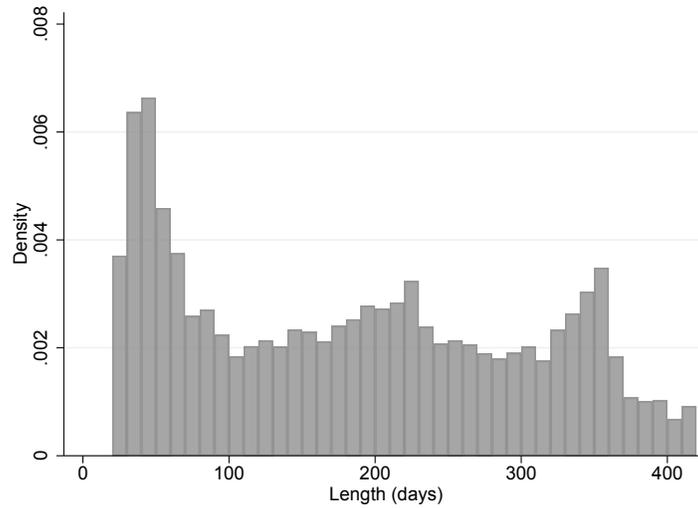
Note: Figure plots the distribution of average applications per week for the unrestricted sample (left) and the analysis sample (right).

Figure A.3: Distribution of estimated propensity scores for descriptive analysis



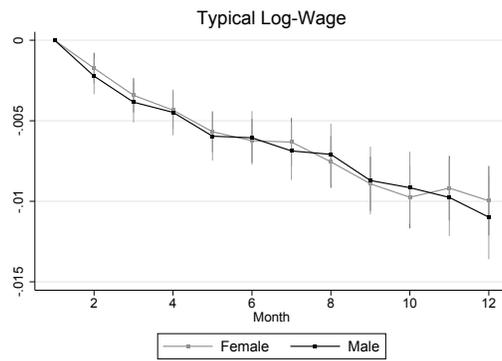
Note: Figure plots the distribution of male propensity score estimates for men and women. Propensity scores outside the range [0.01,0.99] have been trimmed to avoid extreme weights.

Figure A.4: Parental leave: Distribution of average length



Notes: The figure plots the distribution of the average length of parental leave in days across firms with more than 28 days of leave on average.

Figure A.5: Change in job search over time, event study estimates



Notes: The figure plots estimates from an event study regression that regresses the average log typical wage of applied-for jobs on dummies for the number of months since entering unemployment. The figure plots the estimated coefficients on the month dummies. The regression includes a person-by-spell fixed effect and uses the month of entry (month 0) as the omitted baseline month. Results are shown separately for men and women. Standard errors are clustered at the spell level.

## B Appendix: Robustness checks

In this section we present a range of robustness checks for our main results.

First, we focus on the results presented in Section 3, i.e. the gaps in application and hiring shares. In Section B.1 we show that these results are not sensitive to the specific rules employed in the sample selection steps discussed in Section 2.1. In Section B.2 we show the results still prevail under alternative weightings schemes. In Section B.4 we show our unweighted results. Finally, in Section B.5 we report our results under some alternative measurements including for instance focusing on the number of applications instead of on the share of applications sent to specific job characteristics.

Second, we focus on the results presented in Section 4 from our decomposition approach. In Section B.7 we present the results from our decomposition excluding the first stage in our decomposition, that is without accounting for the role of observables. Lastly, in Section ?? we illustrate that our results are not simply driven by differential application behaviour in the tails of e.g. the typical wage distribution or other job characteristics.

### B.1 Alternative sample definition

We impose several restrictions on our main sample as laid out in Section 2.1. Below we show that our findings are not sensitive to these restrictions. Figures B.1 to B.5 replicate the female-male gaps in average application and hiring shares for a sample where we do not sequentially restrict our sample to the selections that were used for our main analysis. Figure B.1 is based on a sample where we do not exclude the last 4 weeks of applications, whereas in Figure B.2 we focus on unemployment spells that find a job within 26 weeks (in contrast to the 52 week requirement used in the main text). In Figure B.3 we relax our sample restriction of only including unemployment spells with at least 4 registered applications by selecting all spells that have at least one application instead.<sup>64</sup> Figure B.4 removes the restriction of at least 8 weeks of unemployment to enter the sample, and instead include all available unemployment spells in the sample period.<sup>65</sup> Figure B.5 replicates the results for a sample that is not restricted to end in a new hire. Here we treat unemployment as a separate

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<sup>64</sup>Note that registering at least one application is necessary to appear in the Joblog data delivery we received.

<sup>65</sup>Nevertheless, these unemployment spells need to be at minimum 4 weeks long in order for us to properly identify them.

category to the hiring outcomes. Common to all of our robustness tests on the sample selection criteria is that the results do not change qualitatively. In fact, female-male gaps are remarkably stable across the different samples.

## **B.2 Conditioning on different observables**

In the main text, our descriptive analysis conditions on observables by propensity score reweighting on a set of variables selected through a LASSO procedure. We have, however, also experimented with several other approaches to conditioning on observables. None of these change our conclusions. In this Section we present results from some of these alternative approaches.

In figure B.6 we present results after propensity score reweighting only on the 3-digit industry of the previous job, thus imposing exact balance on previous industry across men and women. Similarly, in Figure B.7 we present results after propensity score reweighting only on the 3-digit occupation of the previous job, thus imposing exact balance on previous occupation. Finally, in Figure B.8 in which we use the same set of conditioning variables as the main analysis but include dummies for the quarter of entry into UI in the propensity score estimation 2.4. The purpose of this is to control for seasonality, i.e. whether entering the sample at different times is important for the differences in application behavior and hiring outcomes we observe.

Throughout the various alternative approaches, we see a similar pattern of gender gaps as in the main text.

## **B.3 Wage of applied for jobs relative to previous job**

In Figure B.9 we show gender application and hiring gaps when grouping applications and hires by whether the typical wage is higher or lower than the previous job. The figure reveal a stark difference between women and men, where men target higher typical wages compared to the typical wage of their previous job.

## **B.4 Raw gender gaps in application and hiring outcomes**

In Figure B.10, we show raw gender gaps in applications and hiring outcomes without conditioning on unobservables. We see that the overall patterns are similar to the conditional results presented in the main text but that, unsurprisingly, the raw gaps tend to be much larger in magnitude.

## B.5 Gender gaps in number of applications instead of shares

In our main analysis we measure gender gaps in the share of applications going to different jobs. In Figure B.11 we instead show gender gaps in the absolute number of applications sent to different jobs. We see that the overall patterns of results from the main analysis remains.

## B.6 Gender-specific measures of jobs' typical wage level

One drawback of the measure of a job's typical wage level that we use in the main analysis is that it does not allow for the possibility that men and women face different wages in the same job. A particular concern here is the possibility that some types of jobs tend to pay high wages to women but not men, while other jobs are the reverse. If this is the case we would expect women to apply much more for jobs in which they face higher wages and vice versa for men. These differences would be obscured in our main analysis and could bias our results regarding the wage levels of the jobs men and women apply for.

To examine this possibility, this section presents results based on two alternative measures wages that capture the typical wage level faced by either women or men in a given job. We construct these measures simply by repeating the last step of the wage prediction exercise underlying our typical wage only for the male or female half of the sample (see Appendix A.4).

Figure B.12 shows gender gaps in applications and hiring splitting jobs according to the typical wage level they pay to either women or men. Although the exact size of the application and hiring gaps change slightly, the overall pattern of results is identical to what we see in the main analysis.

## B.7 Decomposing raw gender gaps

In this section, we present a decomposition of the raw gender gaps in hiring outcomes. We do this by simply forgoing the first step of the decomposition used in the main analysis: starting from the raw gender gaps in hiring outcomes in the data, we propensity score reweight the women in the sample to the same application behavior as men to see how much of this raw gap applications can explain.

Table B.1 shows the decomposition. Unsurprisingly, we see that raw gender gaps before conditioning on observables are generally larger than the baseline hiring gaps considered in the main

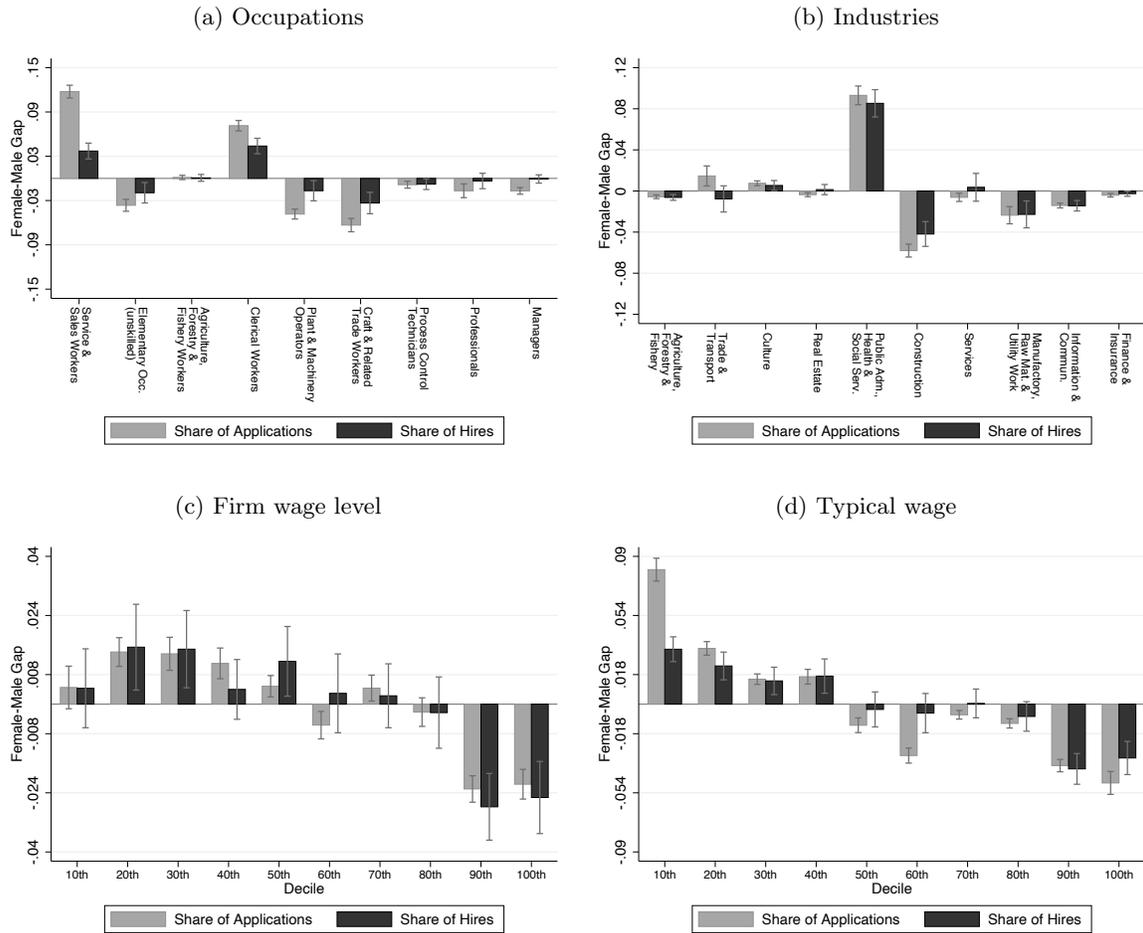
analysis. We also see that applications are capable of explaining a larger share of these raw gaps. The only exception to this is firm-wage level, where the raw gap is smaller than the baseline gap and where applications are capable of explaining a smaller share of the raw gap.

Table B.1: Decomposing gender gap without conditioning on observables

	Raw gender gap	Explained by applications	Residual
Occupational segregation (Duncan index, 1-digit)	0.228 (0.004)	0.180 (0.007) [0.79]	0.048 (0.007) [0.21]
Occupational segregation (Duncan index, 2-digit)	0.351 (0.003)	0.225 (0.008) [0.64]	0.126 (0.007) [0.36]
Industry segregation (Duncan index, 1-digit)	0.253 (0.003)	0.215 (0.007) [0.85]	0.038 (0.006) [0.15]
Industry segregation (Duncan index, 2-digit)	0.262 (0.003)	0.205 (0.007) [0.78]	0.058 (0.007) [0.22]
Firm wage level (Male-female gap, std. AKM fixed effect)	0.026 (0.006)	0.013 (0.021) [0.49]	0.013 (0.022) [0.51]
Typical wage for job (Male-female gap, log typical wage)	0.040 (0.001)	0.037 (0.002) [0.92]	0.003 (0.002) [0.08]
Actual Wages (Male-female gap, log wage)	0.069 (0.002)	0.055 (0.004) [0.92]	0.013 (0.004) [0.08]

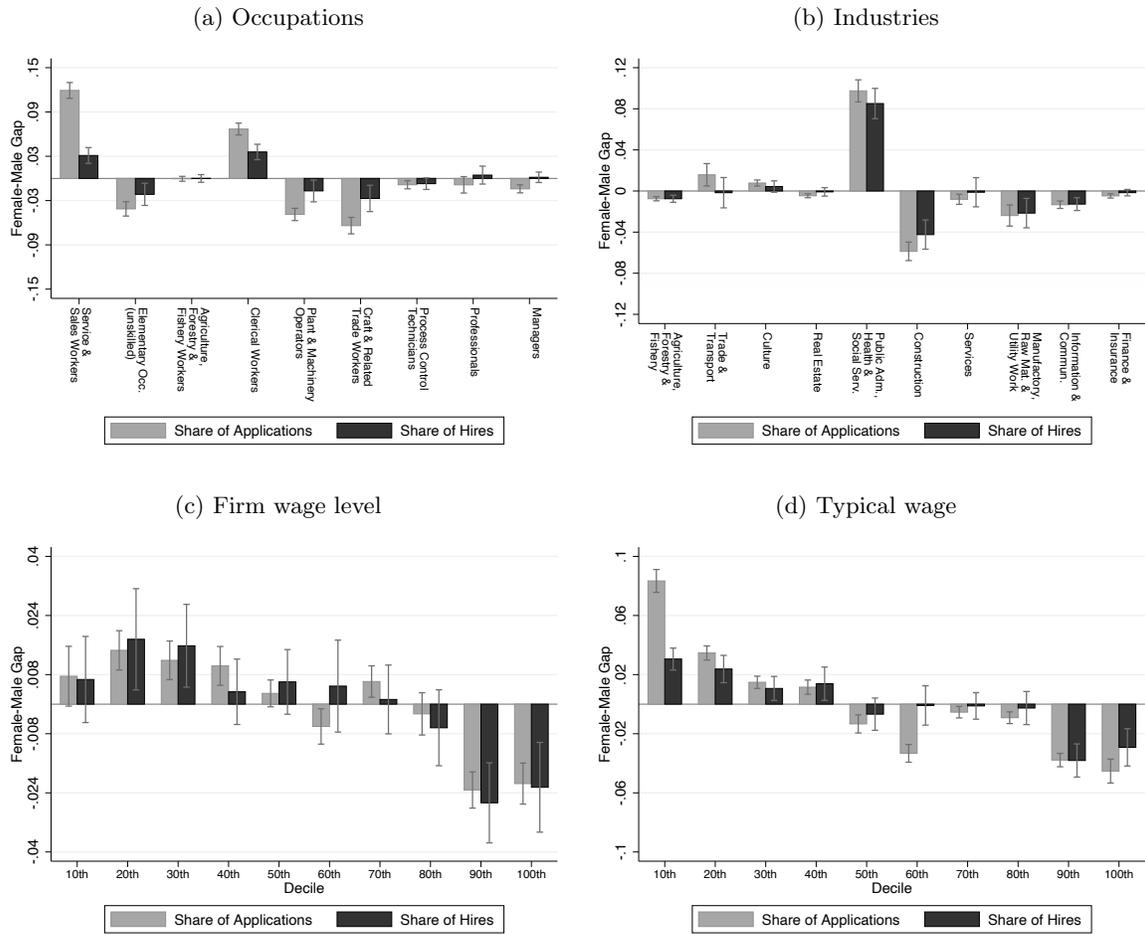
Notes: The table decomposes the raw gaps in hiring outcomes. The gaps are decomposed into a part explained by applications and a residual gap. Standard errors based on bootstrapping individuals are shown in parenthesis. Brackets report the share of the raw gap explained by each component

Figure B.1: Gender gaps in applications and hiring outcomes, no exclusion of last 4 weeks



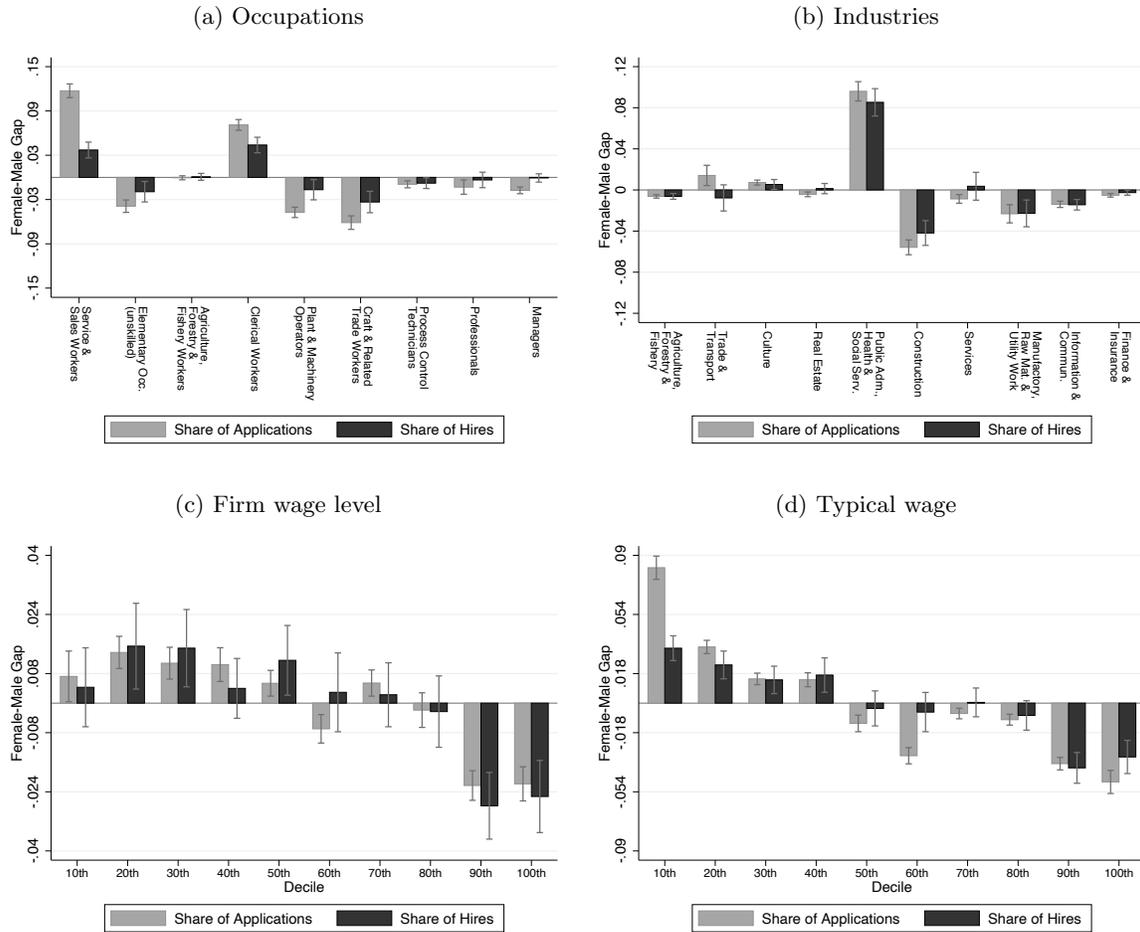
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. The figure relaxes the sample restriction on the last 4 weeks of unemployment and consider all applications. All gaps are based on a reweighted sample so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.2: Gender gaps in applications and hiring outcomes, 26 week sample



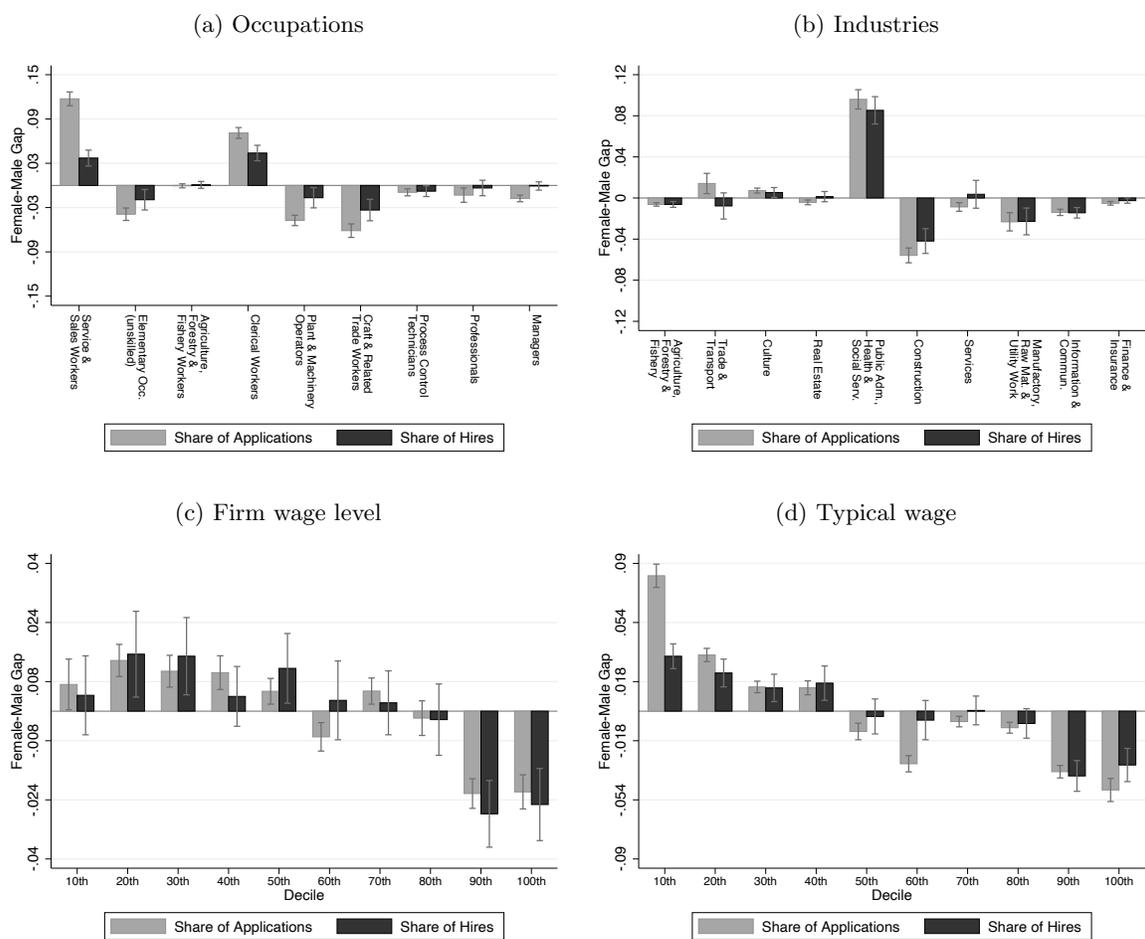
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. The figure changes the sample to only consider spells lasting at most 26 weeks. All gaps are based on a reweighted sample so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.3: Gender gaps in applications and hiring outcomes, only 1 application registration requirement



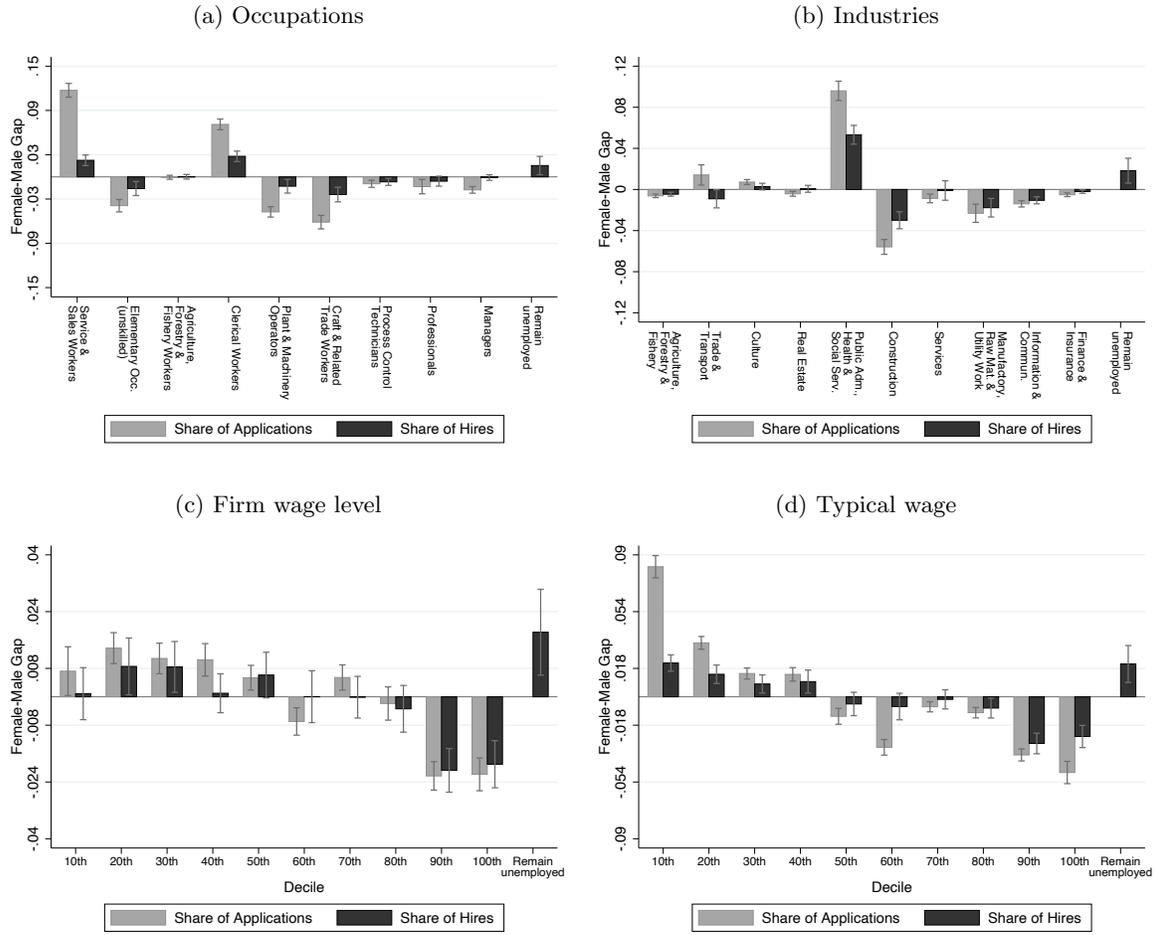
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. The figure relaxes the samples requirement of having min. 4 registered applications to at least 1 application. All gaps are based on a reweighted sample so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.4: Gender gaps in applications and hiring outcomes, no spell length requirement



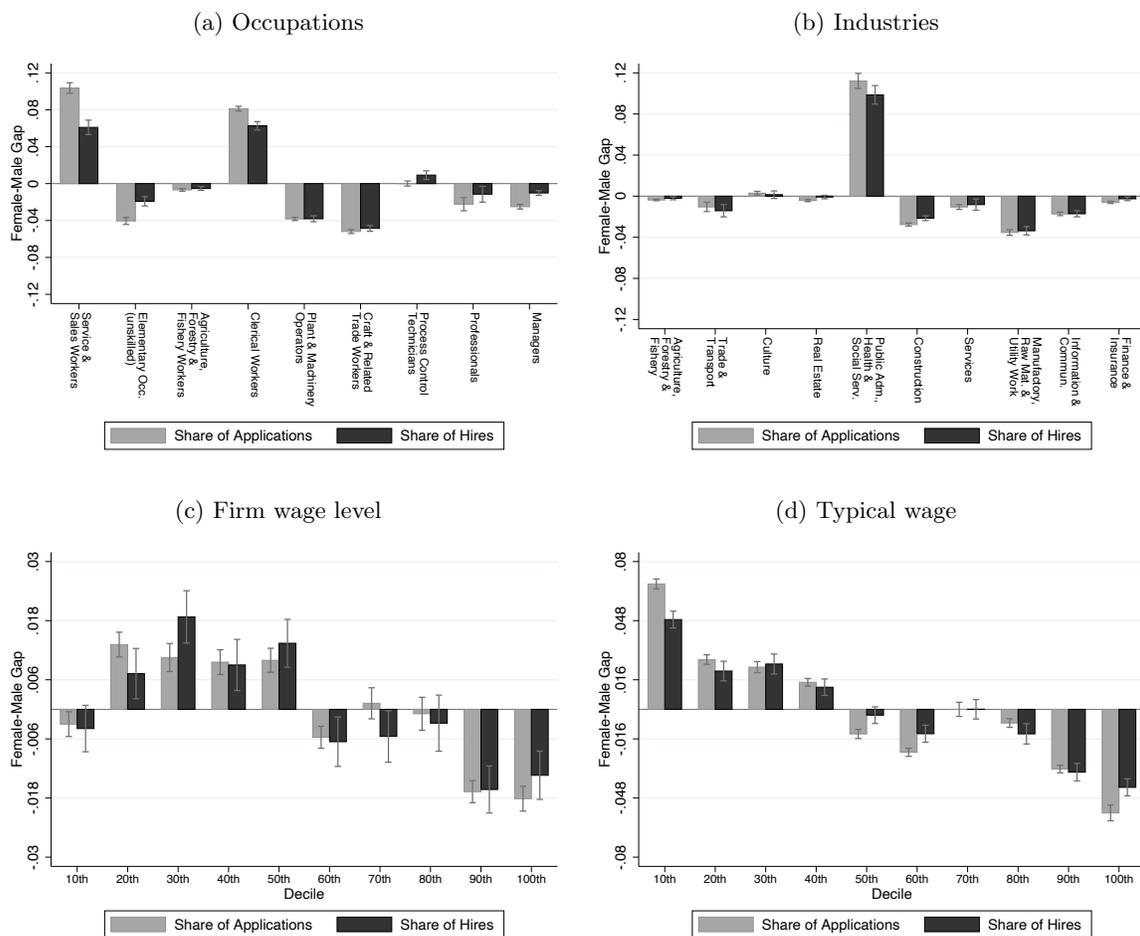
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. The figure relaxes the sample restriction on min. 8 weeks spell length and considers all spells. All gaps are based on a reweighted sample so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.5: Gender gaps in applications and hiring outcomes, no employment requirement



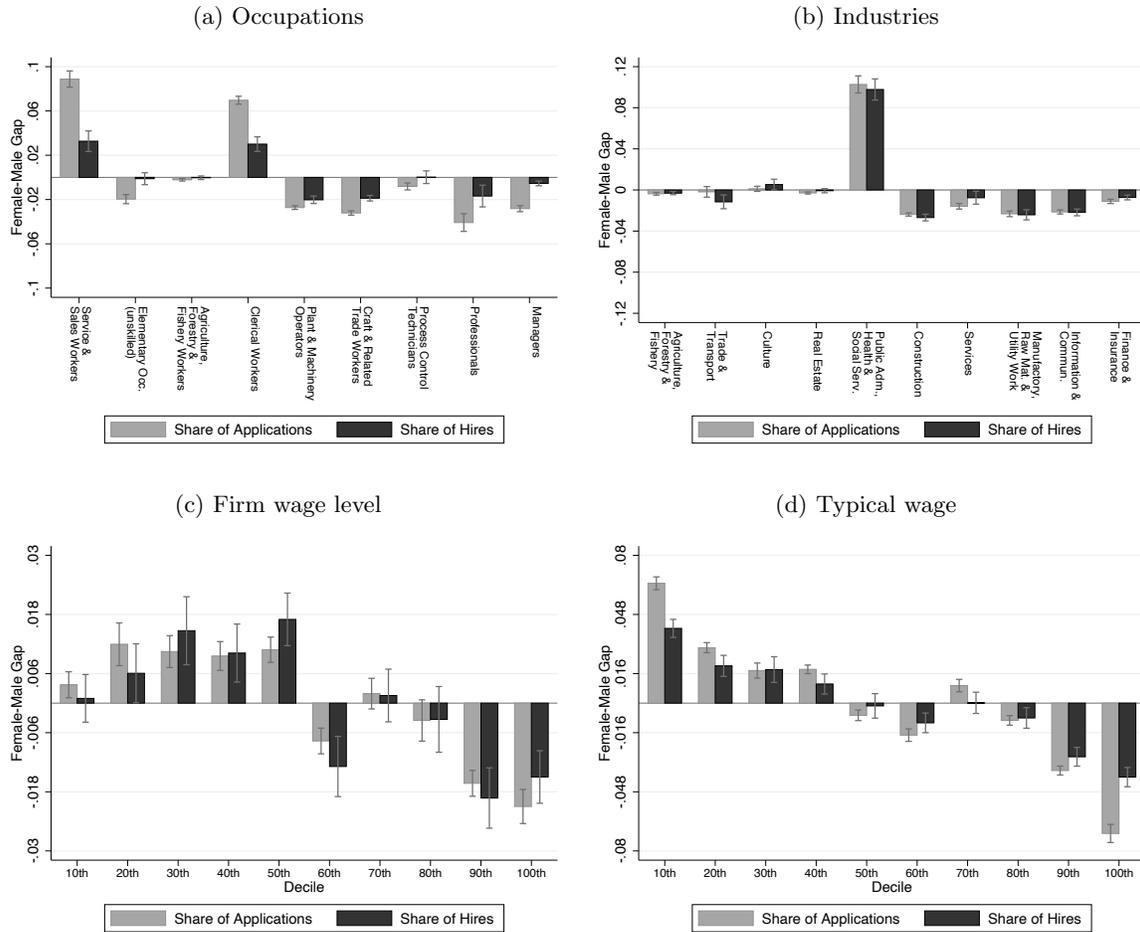
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. The figure relaxes the sample restriction requiring the UI recipient to find employment and thus also consider spells that do not end in employment. Staying unemployed is treated as a separate category throughout. All gaps are based on a reweighted sample so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.6: Gender gaps in applications and hiring outcomes, conditioning only on previous industry



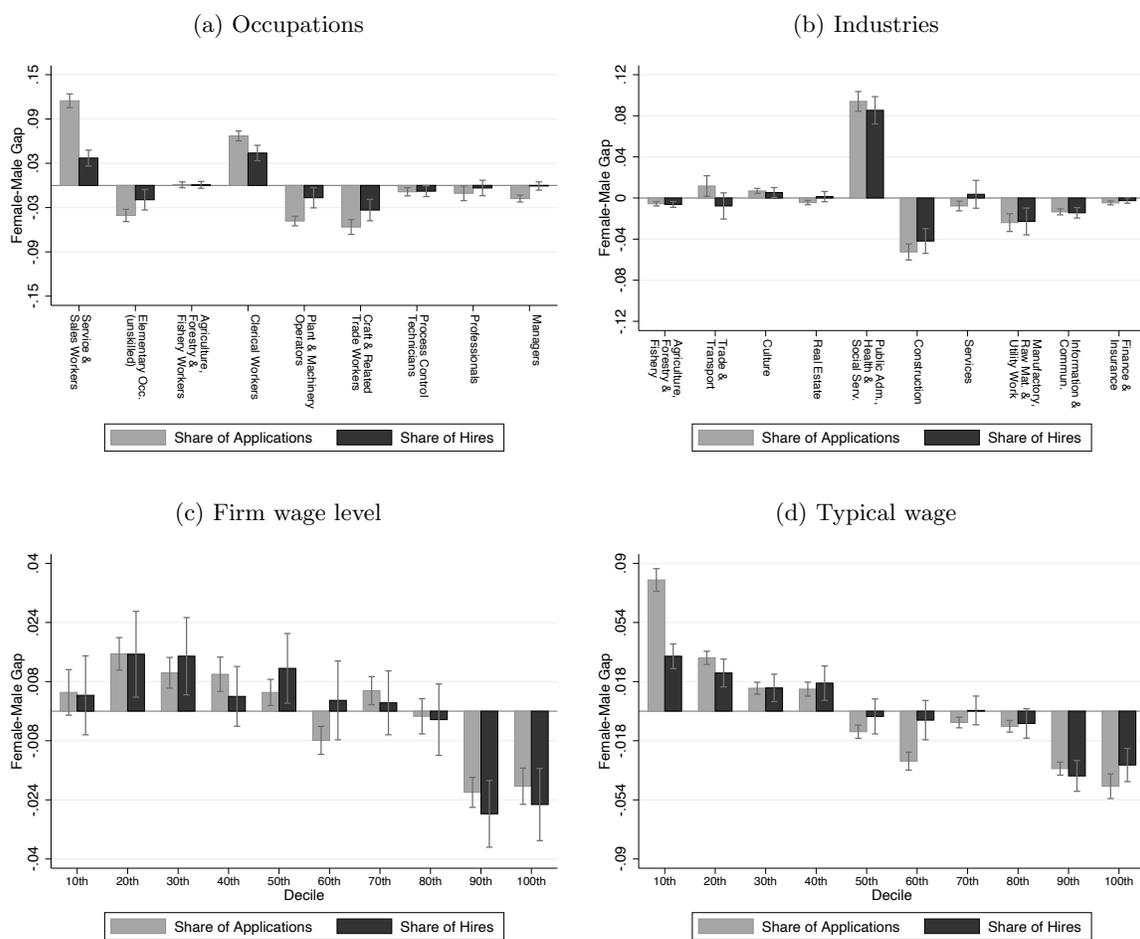
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. All gaps are computed after propensity score reweighting on only the 3-digit industry of the previous job. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.7: Gender gaps in applications and hiring outcomes, conditioning only on previous occupation



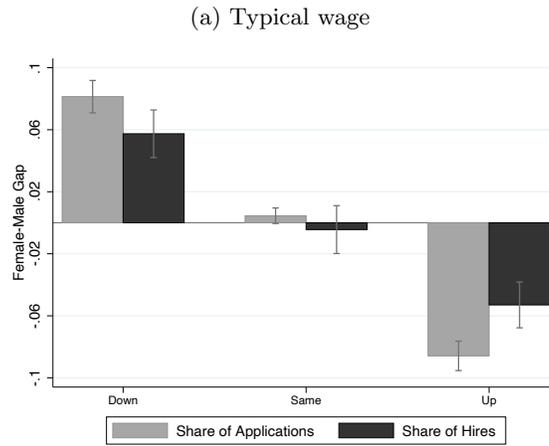
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. All gaps are computed after propensity score reweighting on only the 3-digit occupation industry of the previous job. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.8: Gender gaps in applications and hiring outcomes, seasonality controls



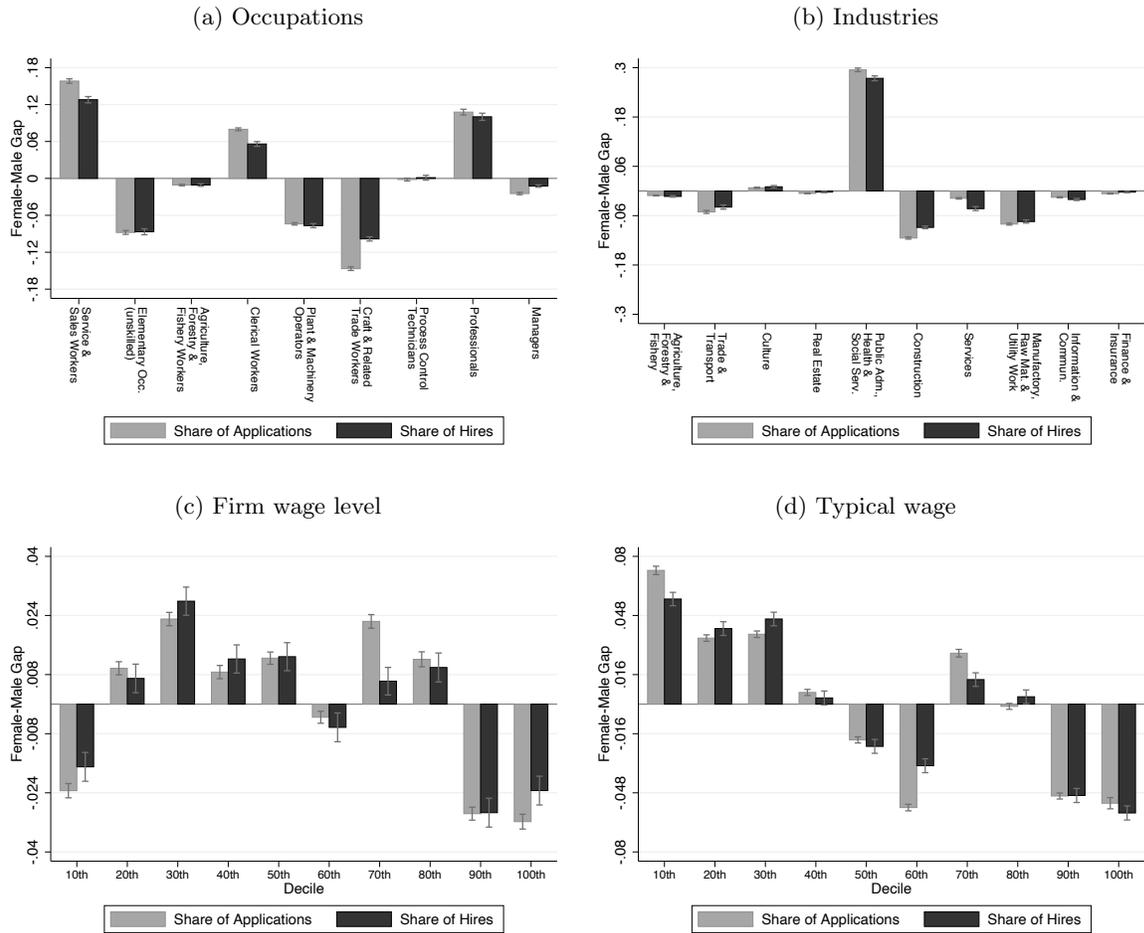
Notes: The figures plot gender gaps in the share of applications going to specific types of jobs and corresponding gender gaps in hiring shares. All gaps are computed after propensity score reweighting on the quarter of inflow into unemployment along with all the conditioning variables used in the main analysis. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.9: Gender gaps in job applications and hiring, typical wage ranks relative to previous job



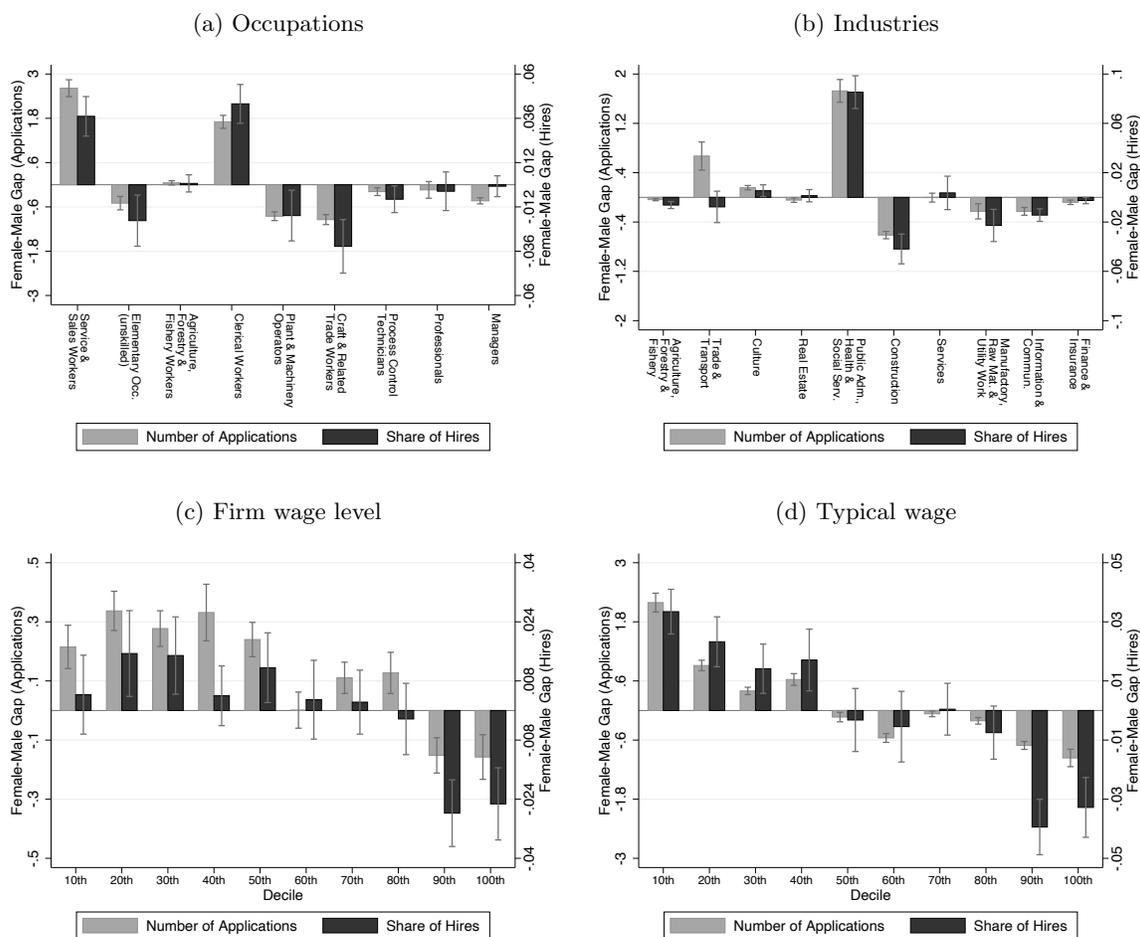
Notes: The figures plot gender gaps in the share of applications going to jobs that are in a higher, lower or the same decile as the previous job. The figure also plots corresponding gender gaps in hiring shares. All gaps are computed after propensity score reweighting so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.10: Gender gaps in applications and hiring outcomes, raw



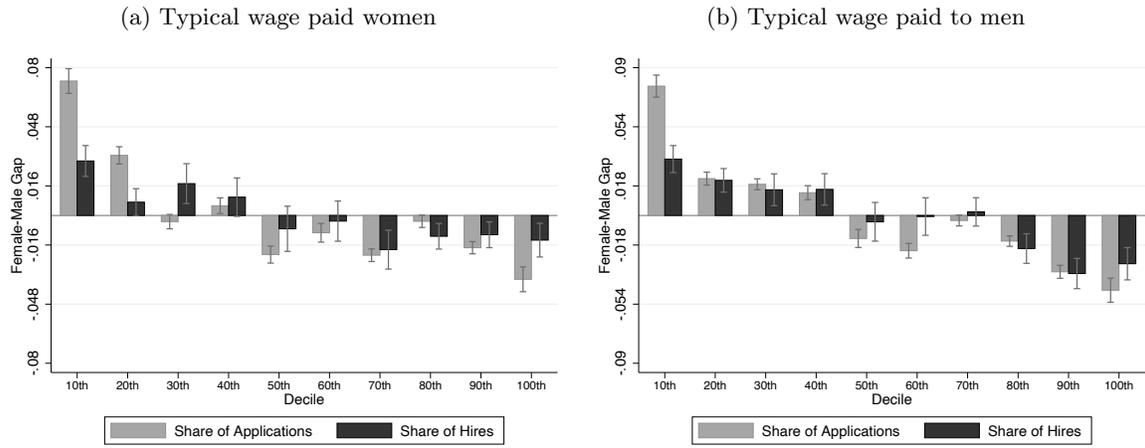
Notes: The figures plot gender gaps in the share of applications going to different types of jobs along with corresponding gender gaps in hiring shares. The figure shows raw gaps without conditioning on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.11: Gender gaps in applications and hiring outcomes, absolute measure for applications



Notes: The figures plot gender gaps in the number of applications going to different types of jobs (left axis) along with corresponding gender gaps in hiring shares (right axis). All gaps are computed after propensity score reweighting so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure B.12: Gender gaps in job applications and hiring, gender-specific typical wage measures



Notes: The figures plot gender gaps in the share of applications going different jobs with different level of typical wages paid to women (left) and men (right). All gaps are computed after propensity score reweighting so are conditional on observables. Occupations and industries on the x-axis are ordered from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

## C Appendix: Additional results

### C.1 Do women apply for jobs with lower wage growth?

In Section 2.5, we have seen that there are men and women apply to jobs with different typical wages. These differences largely correspond to differences in the eventual hiring outcome, leading to the observed gender wage gaps in typical wages. As described in Section, 2.2, however, the wage measures used in our main analysis reflect the wage level at the start of a workers new job. If there, however, are substantial differences in how fast or how strong wages increase over the initial years in a firm, results based on these measure may obscure how the gender wage gap change over time as men and women progress in their career.

To get a sense on whether this is important, this section constructs a measure of the wage growth that a given job offers and examine whether there are gender gaps in applications and hiring also into high vs low wage growth jobs. Specifically, we calculate firm specific 1 and 5 year wage growth rates based on all jobs starting between 2008 and 2016 in a given firm. In order to separate general time trends from the growth rates as well as structural differences between industries, we control for year by industry fixed effects. As many individuals will have left their jobs by the one and especially the five year mark, we censor individuals that are no longer employed by these firms at these times.<sup>66</sup>

In Figure C.1 we thus split firms into deciles based on the typical wage growth they offer over either 1 or 5 years. We see that women are more likely to apply to firms with lower wage growth rates, with the exception of the lowest decile. Likewise, men apply substantially more to those firms in the very top deciles. In addition to the fact that women are applying and getting hired more into jobs with lower starting wages, women thus are thus also applying and getting hired into firms that offer lower rates of wage growth. These differences in applications and hiring outcomes contribute to a widening of the gender gap over time.

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<sup>66</sup>Obviously it is not random who stays in a firm up to 5 years, and our numbers may therefore partly be driven by dynamic selection. Results should be interpreted with this in mind.

## C.2 Are gender gaps in applications related to motherhood?

Several recent papers have emphasized that gender gaps in the labor market are particularly related to motherhood and its effects on the valuation of non-wage job characteristics. In Figures C.2 and C.3, we attempt shed some light on how motherhood relates to gender differences in job applications. In these figures, we limit our sample to UI recipients in an age window around the prime childbearing years, specifically 25-40 years. We then repeat our descriptive analysis of gender gaps in application and hiring separately for men and women with young children (0-5 years) and for men and women without children. For most of the non-wage job characteristics considered in Section 5.1 and for typical wages, we see that gender gaps in both applications and hiring outcomes tend to be larger when comparing men and women with young children. At the same time, however, we note that very substantial gender differences do exist in both groups.

## C.3 Gender differences in the returns to applications (self-fulfilling discrimination)

One explanation for the observed gender application gap is a version of the “self-fulfilling discrimination” mechanism that has been proposed and documented in other settings (Lundberg and Startz, 1983; Coate and Loury, 1993; Glover et al., 2017). In the context of the job application process, the basic idea behind this is as follows: Gender discrimination in hiring implies that there are gender differences in the likelihood that an application turns into a hire for some jobs. As a result women have an incentive to apply less to these jobs and more to jobs where the chance of being hired is higher. In this way, gender differences in the likelihood than an application turns into a hire may explain why there are gender differences in applications.

To test for whether we can detect this phenomenon in our data, we first construct measures of gender differences in the likelihood that an application turns into a hire. We do this in the context of a simple linear regression. For some individual in our analysis sample, let  $y$  denote a type of job (an occupation, industry or decile of a wage distribution), let  $a^y$  be the share of their applications that the individual sent to jobs of type  $y$ , and let  $d^y$  be an indicator for whether the individual was hired into a job of type  $y$ . We then consider estimating the following regression on our weighted analysis sample:

$$d^y = \beta_0^y + \beta_1^y a^y + \varepsilon \tag{10}$$

If  $a^y$  is measured in percentage points, the coefficient  $\beta_1^y$  in this regression captures how much the likelihood of being hired into job type  $y$  increases if one additional percentage point of applications is targeted to this type of job. If given a causal interpretation, this is a measure of the returns to applications for job type  $y$ . To obtain a simple measure of gender differences in the likelihood that an application turns into a hire for job type  $y$ , we therefore estimate equation 10 separately for men and women and compute the difference in the estimate of  $\beta_1^y$  across genders. We refer to this as the *gender gap in returns to applications* for job type  $y$ . We note that there are obvious concerns with treating the estimate of  $\beta_1^y$  as causal. In particular, if individuals tend to send more applications to jobs they are more likely to get due to e.g. differences in unobservables, we would expect  $\beta_1^y$  to overstate the returns to search.<sup>67</sup> Absent a source of exogenous variation in application behavior, we have no way of removing this potential bias. To the extent that the resulting bias is relatively constant across job types, however, the estimates from Equation 10 may still give a useful ranking of the types of jobs where men face relatively higher returns to search than women.

In Figure C.4 we examine whether differences in the returns to applications appear to explain the observed gender gaps in applications. Each data point in the figure corresponds to a job type, defined as either a two-digit occupation, a two-digit industry, a decile of the firm wage level distribution or a decile of the typical wage distribution. The y-axis shows the gender gap in applications to the different job types after conditioning on labor market observables. Finally, the x-axis contains our (standardized) measure of the gender gap in returns to applications for the different job types.<sup>68</sup> If the observed gender gaps in applications were driven by women applying more to jobs where their applications have a higher relative likelihood of turning into a hire, we would expect to see an upward sloping relationship in Figure C.4. This is not what we see. If anything the relationship seems to be slightly downward sloping.<sup>69</sup>

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<sup>67</sup>Since we estimate Equation 10 on the reweighted sample, we are ensuring that labor market observables are balanced across men and women. There may of course still be unobserved differences across individuals that affect the likelihood of a successful application and likely correlates with application behavior as well.

<sup>68</sup>Specifically, we standardize the measure within each category of job types (across industries, across occupations, across deciles of firm wage levels and across deciles of typical wages). Standardizing the measure jointly across all the categories or using a non-standardized version of the measure does not change the results.

<sup>69</sup>The correlation between the gender gap in applications and our measure of the gender gap in returns to applications for the different job types is -0.368 overall and range from -0.762 (typical wage deciles) to 0.257 (firm wage

The fact that gender gaps in applications are not positively correlated with gender gaps in the likelihood that an application turns into a hire is also borne out in the relative magnitudes of the application and hiring gaps documented in Section 3. If the jobs that women apply less to than men are also the ones where they face a lower chance of being hired when they apply, this will compound to make overall gender gaps in hiring outcomes even larger than gender gaps in applications.<sup>70</sup> As shown in Section 3, however, gender gaps in applications actually tend to be larger than gender gaps in hiring.

In sum, with the data we have available, we cannot find evidence that “self-fulfilling discrimination” underlie the observed patterns of application behavior across genders in our data. As discussed, however, this does not rule out that “self-fulfilling discrimination” is at play and could be detected with different data.<sup>71</sup>

#### C.4 Gender differences in beliefs, overconfidence and risk preferences

Another possible explanation for our results is that gender differences in preferences and beliefs lead men to systematically apply for more high-paying and harder-to-get jobs than women. If men and women have different beliefs about their general labor market prospects such that men are more (over)confident than women, this could lead men to systematically target more higher-paying but harder-to-get jobs.<sup>72</sup> Similar predictions also arise if men are less risk-averse than women or if some form of social norms lead women to hold themselves to a higher standard when deciding where to apply for jobs. The existence of these types of gender differences have received significant attention in previous work and have also found empirical support in some settings (see in particular

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level deciles) if computed across one of the four job categories.

<sup>70</sup>Put differently, overall gender gaps in hiring outcomes is the product of two gaps: 1) the gap in how likely women are to apply for a particular type of job and 2) the gap in how likely an application from a woman is to result in a hire for a particular type of job. If the types of jobs where gap 1 is big are also the ones where gap 2 is big, the aggregate gap in hiring outcomes should be bigger than the application gap alone (gap 1).

<sup>71</sup>As noted, our simple regression-based measure of returns to applications does not account for unobservable characteristics that correlate with both application behavior and the likelihood of getting hired. In addition, the most extreme models of “self-fulfilling discrimination”, can imply that women *never* apply to any jobs where they face a lower probability being hired than men do. In this case it would never be possible to measure any meaningful differences in the likelihood of being hired conditional on applying. In our data, women are of course sending many applications to each of the observed job types we consider, however, this does not rule out that men and women are differentially applying to jobs with different unobserved characteristics within these job types.

<sup>72</sup>It is commonplace to refer to this explanation for gender differences as reflecting male overconfidence. Of course, it is in principle also possible that men have approximately correct or even downward biased beliefs about their labor market prospects, while women are (more) underconfident and have (more) downward biased beliefs about their labor market prospects. This could also explain the gender application gaps that we see in our data through exactly the same mechanisms.

Cortes et al. (2020)).

To provide some evidence on this possible mechanism, we can examine the speed with which men and women find jobs in our data. To this end, we construct a version of our main analysis sample where we drop the restriction that individuals must find employment within a year (see Section 2.1).<sup>73</sup> For this sample we then examine how many men and women still have not found employment after each week of their unemployment spell. Specifically, Figure C.5 shows Kaplan-Meier estimates of the survivor function in nonemployment, estimated separately for men and women. To account for differences in observable characteristics of men and women, the survivor curves are estimated after reweighting on observables (see Section 2.4).<sup>74</sup>

If simple gender differences in beliefs or preferences are systematically causing women to apply to less ambitious and easier-to-get jobs than men, we should see women find jobs faster than men. Looking at Figure C.5, however, this is the opposite of what we see in the data. Throughout the unemployment spell, the survivor curve for women is slightly above the survivor curve for men, implying that women in fact find jobs slightly more slowly than men do.

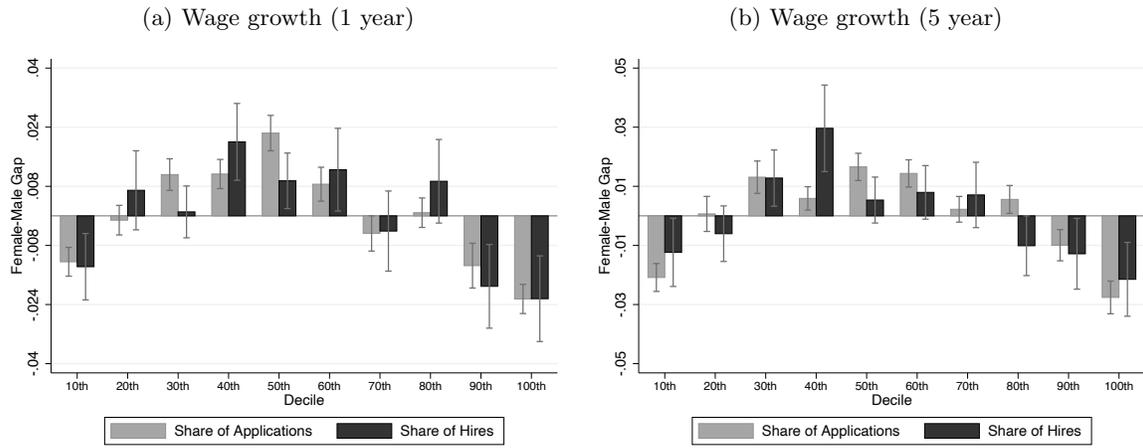
In sum, we cannot find evidence that simple gender differences in beliefs or risk preferences underlie the observed patterns of application behavior across genders in our data. Of course, this not rule out that gender differences in beliefs or risk preferences exist and contribute to the observed application behavior - if men and women differ in other dimensions that influence job finding rates, for example, this may obscure the effect of beliefs or risk preferences on job finding rates.

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<sup>73</sup>We find a similar pattern if we instead focus on the main analysis sample including the restriction that individuals must find employment within a year.

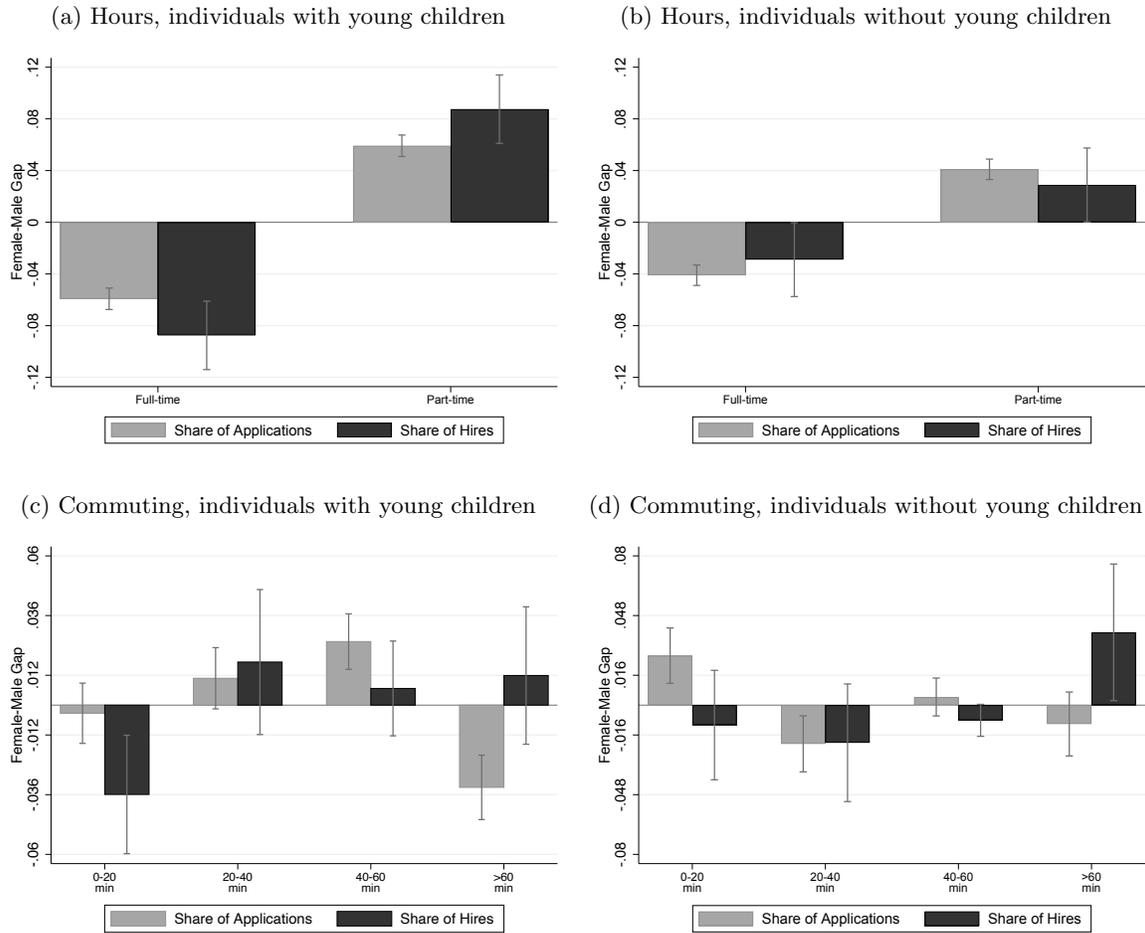
<sup>74</sup>Figure A.1 shows corresponding estimates without reweighting. This leads to a larger difference between men and women but again with women again finding jobs slower than men.

Figure C.1: Gender gaps in job applications and hiring, wage growth



Note: Figure plots gender gaps in shares of applications going to specific wage growth deciles and gaps in which decile job-seekers are hired. Wage deciles are computed as the relative difference between the starting wage with the wage one year (left) or five years (right) after entering the respective job. The 95% confidence bars are based on standard errors clustered on the individual level.

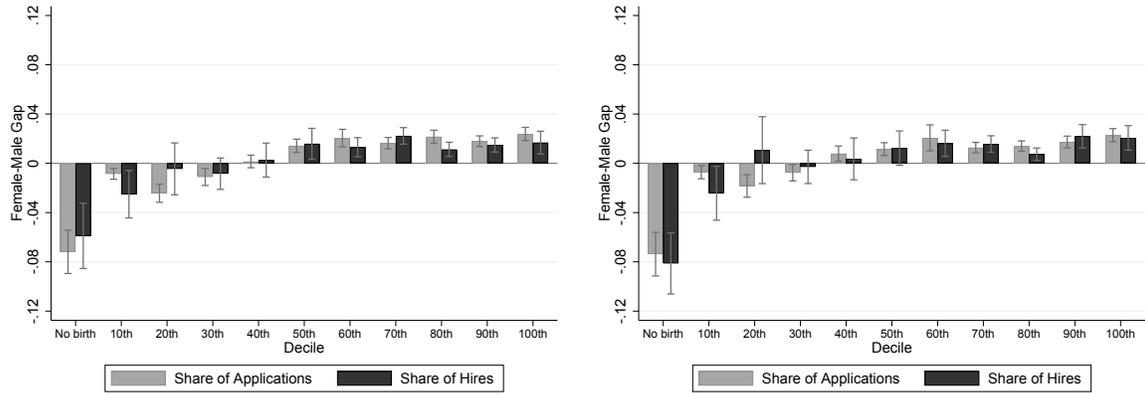
Figure C.2: Gender gaps in applications and hiring outcomes, individuals with/without young children I



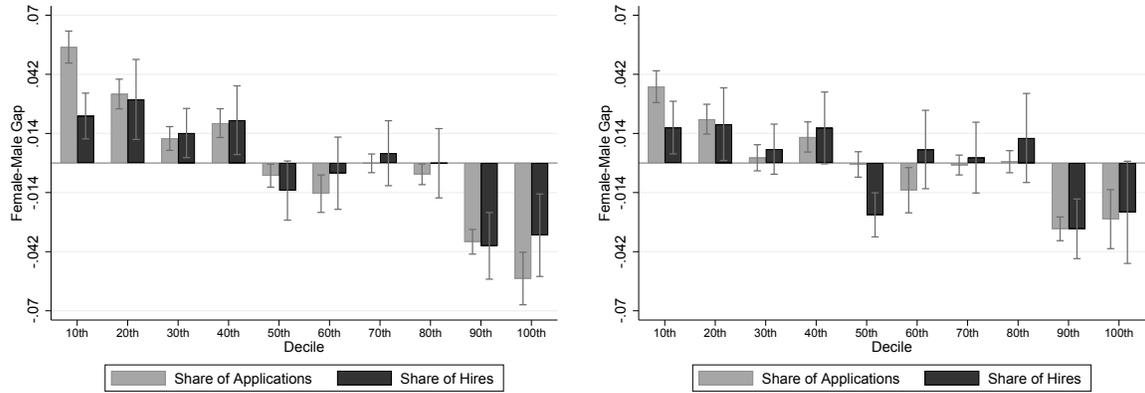
The figures plot gender gaps in shares of applications going to specific types of jobs and corresponding gender gaps in hiring outcomes separately for individuals (age 25-40) with young (0-5 years) children and without children. All gaps are based on the reweighted sample so are conditional on observables. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure C.3: Gender gaps in applications and hiring outcomes, individuals with/without young children II

(a) Family friendly, individuals with young children (b) Family friendly, individuals without young children



(c) Typical wages, individuals with young children (d) Typical wages, individuals without young children



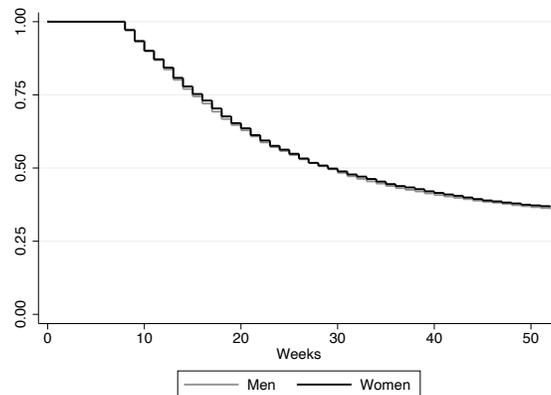
The figures plot gender gaps in shares of applications going to specific types of jobs and corresponding gender gaps in hiring outcomes separately for individuals (age 25-40) with young (0-5 years) children and without children. See Appendix A.8.1 for details on the measure of family friendliness. All gaps are based on the reweighted sample so are conditional on observables. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure C.4: Gender application gaps and the return to applications



Note: The figure plots the corresponding gender application gap and the gender gap in the return to applications. Each data point is thus a job type defined as either a two-digit occupation, a two-digit industry, a decile of the firm wage level distribution or a decile of the typical wage distribution. Military occupations and job types, where fewer than 100 individuals finds employment are excluded. The y-axis shows the gender gap in applications to the different job types after conditioning on labor market observables. The x-axis contains our (standardized) measure of the gender gap in returns to applications for the different job types, see Equation 10.

Figure C.5: Kaplan-Meier survivor functions in nonemployment



Note: The figure plots Kaplan-Meier estimates of the survivor function in nonemployment, estimated separately for men and women. The two curves thus show the share of men and women that still have not found a new job after each week since starting their UI spell. The curves are estimated on a version of the main analysis sample where the requirement of finding a job within a year has not been imposed. The curves are estimated after reweighting on observables.