The Dynamics of Job Search in Unemployment: Beyond search effort and reservation wages*

Jonas Fluchtmann[†] Anita M. Glenny [‡] Nikolaj Harmon [§] Jonas Maibom [¶]

Abstract

Using novel administrative data on applied-for jobs for Danish UI recipients, we provide new evidence on how application behavior change over the unemployment spell. i) The average wage and hours of applied-for jobs declines, ii) reflecting a downward shift in the distribution of wages. iii) The proximity of jobs in terms of geography/industry/occupation decreases early on but then remains fixed. iv) The channels and methods through which job seekers search for jobs change. v) Changes in search behavior are similar for workers with short and long spells. We discuss the implications for theory and empirical work on job search.

Keywords: job search, unemployment

JEL: E24, J64

*This paper has benefited from numerous questions and comments from participants at several seminars, conferences and workshops. We thank Alexander Koch, Rune Vejlin, Michael Rosholm, Andrea Weber, Roland Rathelot and Daniel le Maire for comments on early versions of the paper. We are grateful to the Danish Agency for Labor Market and Recruitment and the ECONAU project database (Department of Economics and Business Economics, Aarhus University) for making their data available. This paper was supported by the Danish Research Council grant "How do unemployed job seekers apply for jobs and can we help them do better?".

[†]Aarhus University; present: OECD; E-mail: jonas.fluchtmann@oecd.org; The views expressed here are those of the authors and do not necessarily reflect the official views of the OECD or its member countries.

[‡]University of Copenhagen; present: Aarhus University; E-mail: glenny@econ.au.dk;

[§]University of Copenhagen ; E-mail: nikolaj.harmon@econ.ku.dk

[¶]Aarhus University and DTMC; E-mail: maibom@econ.au.dk;

1 Introduction

Job search among the unemployed is a fundamentally dynamic problem. When making search decisions, unemployed workers need to weigh their own changing economic situation against future search prospects. Accordingly, a large literature examines how unemployed workers may change behavior over time during their unemployment spell. The focus in this literature has typically been on changes in which wage offers workers are willing to accept - the *reservation wage* - and possibly on changes in how hard workers search for jobs - the *search effort*. This has been a very successful focus for both understanding and explaining typical labor market data on the speed of job finding and on reemployment wages.

At the same time, the concepts of reservation wages and search effort are only indirectly linked to many of the micro-level search decisions that unemployed workers make: Besides the decision of how hard to search and which wages to accept, unemployed workers make daily decisions about which specific jobs to target and about where to look for and apply for these jobs. Many modern policy initiatives have the explicit goal of affecting these micro-level decisions. To design and evaluate such policy initiatives in turn requires an empirical and theoretical understanding of where and how unemployed job seekers are applying for jobs, and whether these decisions change over time.

To inform theoretical work and policy discussions, this paper provides representative evidence on where and how unemployed job seekers are searching and applying for jobs over the course of their unemployment spell. We are able to do this by using a novel administrative data set that contain information on applied-for jobs for Danish UI recipients. Since 2015, Danish UI recipients have been required to document job search activities electronically with the Danish employment agency. By linking this data set with administrative data on UI recipients and on the applied-for jobs, we create a panel data set containing information on applied-for jobs during each month of the UI recipients' unemployment spells. 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 search behavior. We use these data to examine changes over time in UI recipients job search behavior along a number of dimensions.

Our analysis is based on simple event-study regressions that examine within-spell variation in job search behavior. We regress measures of job search behavior in a given month on a set of dummies for the number of months spent in unemployment, while controlling for person-spell fixed effects. The estimated coefficients on the month dummies shows how job search behavior (on average) changes over the spell. We focus on the first year of the unemployment spell, which is when the vast majority of individuals find jobs and/or exit the UI system. Our analysis proceeds in five parts.

First, we examine average wages and hours of applied-for jobs. Consistent with existing theory and evidence on declining reservation wages, we find that the average (predicted) wage level of applied-for jobs gradually decreases throughout the unemployment spell. We also see a similar pattern for the estimated firm wage level or the firm type (in the spirit of Abowd et al., 1999). Over the first year of the spell, the typical wage of applied-for jobs drops by about 1 percent. We find similarly behavior for hours: over the unemployment spell, the share of job applications going to full time jobs gradually decreases, leading to a reduction in average applied-for hours of 1.4 percent over the first year. Theoretically, this behavior is well understood using insights from standard job search models when the value of unemployment gradually declines over time, for example because job seekers get closer to the expiration point of UI benefits. As unemployment becomes less attractive over time, UI recipients become more willing to target less lucrative jobs that are likely also easier to get.

Second, we examine the distribution of applied-for wages. Leveraging the fact that our data contains multiple applications for each job seeker at each point in time, we show that the drop in applied-for wages over time occurs throughout the distribution. This sits awkwardly with the canonical random search models where job seekers decisions are characterized by a declining reservation wage over time. If application behavior is governed by a gradually declining reservation wage that makes lower and lower wages acceptable, changes in applied-for wages over time should be concentrated in the lower part of the applied-for wage distribution. In contrast, our finding of a general decline in applied-for wages, is consistent with simple modifications of the standard framework in which search decisions are characterized by workers choosing a *target wage* (e.g., Nekoei and Weber 2017).

Third, we examine the proximity of UI recipients applied-for jobs, both geographically and in terms of the industry and occupation of the applied-for jobs vis-a-vis job seekers past occupation and industry. For both types of proximity, we find a very different pattern than for wages and hours. As for hours and wages, we do find that over the first few months of unemployment, UI recipients gradually apply to jobs that are further away from their home and in occupations and industries that are less closely related to their past jobs. After a few months, however, we see essentially no additional changes in where job seekers apply along these dimensions. From a theoretical perspective, this pattern of behavior appears somewhat puzzling, especially given the results on wages and hours; if continual reductions in the value of remaining unemployed are causing workers to apply to gradually lower wage jobs over time, then it would be reasonable to expect them to also continually broaden the type of jobs they apply for and thus also target jobs in more and more remote locations, industries and occupations. Potential explanations for these observed patterns include vacancy supply effects, as in the so-called stock-flow models of job search (Coles and Smith, 1998; Ebrahimy and Shimer, 2010), learning during unemployment (Gonzalez and Shi, 2010; Burdett and Vishwanath, 1988) or particular assumptions on preferences such as highly convex commuting costs or reference dependence (Dellavigna et al., 2017). In a set of supplementary analyses, however, we find that vacancy supply effects and learning are unlikely to explain the observed pattern, suggesting that the nature of preferences over commuting and occupation/industry switching are the most likely explanations.

Fourth, we examine the methods and channels through which job seekers find and apply for jobs. Here we utilize that when job seekers register their applied-for jobs, they are required to also provide information about how they found the job (the search method) and the method through which they applied for the job (the application channel). Based on these data, we show that UI recipients change methods and channels substantially over the course of an unemployment spell. This is a new fact, which is not easily understood using existing job search models that rarely contain explicit modeling of the choice of search and application channel. Of particular interest, we find that UI recipient's use of applications through digital job application platforms increases markedly during the course of an unemployment spell. This provides a cautionary tale for inferring the dynamics of job search from data sets based on online job platforms, which are commonly used in the literature.

Finally, we examine whether changes in job search over time differs between different workers. We focus specifically on differences between groups of workers who experience different total unemployment duration. Such heterogeneity has received much attention in both research and policy work. While we find large differences in search behavior overall, we find that changes in job search behavior over time are close to parallel. Theoretically, this suggests that the mechanisms that lead to changes in search behavior over time have very similar impacts on workers facing short or long expected employment durations. Empirically, the very stable parallel trends across groups also serves as a validation of the parallel trends assumption that underlie the standard difference-in-difference designs employed in the literature when examining job search or unemployment dynamics.

Our results contribute to a nascent literature that uses micro data on job search behavior to improve our understanding of job search for the unemployed. Relative to previous work, a major contribution of our analysis is the scope and coverage of our data. First, our data contains rich job search information for the same individual over time, including exact information on the start and end time of unemployment spells. Second, since our administrative data cover the universe of UI recipients, we are able to analyze behavior for a large and well-defined subset of unemployed job seekers, thus sidestepping some of the representativeness issue that plague many studies on non-administrative data. Finally, a particular strength of our job search 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. We are not aware of any other data or analyses that combine these features.

The existing work most closely related to ours is Marinescu and Skandalis (2021). Using administrative data from France linked with information on applied-for jobs from the job search platform of the french Public Employment Services they also study changes in job search behavior over time. Relative to our work, a main drawback of their data is that they cover a much smaller and more selected subset of job search behavior. An important advantage, however, is that their data also cover individuals who exhaust their UI benefits and leave the UI system, as well as variation across individuals in their potential duration of benefits. Accordingly, Marinescu and Skandalis (2021) focus their analysis on understanding changes in behavior around benefit exhaustion. We complement this focus by instead examining changes in behavior at the beginning of an unemployment spell and by exploring a wider set of job search outcomes, including the distribution of applied-for wages, the firm type (in the spirit of Abowd et al., 1999) and the search methods and application channels that job seekers use. Another highly related paper in the same vein is Kudlyak et al. (2013). Using data from an online job website, and proxying wage levels by the education level of other applicants, they provide evidence that workers over time apply to jobs that pay lower wages. This is consistent with our findings on applied-for wages.

As noted previously, our paper builds on and expands a large previous literature that studies changes in job seekers' reservation wages and/or search effort over time. Most closely related here are a string of recent papers using individual longitudinal micro data. For instance, Dellavigna et al. (2020); Krueger and Mueller (2016, 2011) all use longitudinal survey data on job search behavior over time. Other recent papers use other micro data sources to study job search behavior. For example, Le Barbanchon et al. (2020) use information about reservation wages and maximum acceptable commuting distances that job seekers report to the French Employment Agency at inflow into unemployment, while others use data from various online job platforms (e.g., Marinescu and Rathelot (2018); Banfi and Villena-Roldán (2019); Banfi et al. (2019a,b); Kuhn and Shen (2013); Faberman and Kudlyak (2019)). Lastly, some papers look at how job search changes in response to e.g. policies or new information. For example, Belot et al. (2019), take an experimental approach to study job search behavior based on micro data from an "controlled" job-search lab in Edinburgh. The authors show how tailored advice broadens the set of jobs that job seekers consider. Correspondingly, other studies find that search behavior can be altered by new information (e.g., Altmann et al. (2018), Skandalis (2018), Gee (2019)) or changes in the UI benefit system (e.g. Lichter and Schiprowski (2021); Le Barbanchon et al. (2019)).

The stylized facts uncovered in this paper serve as input into the theoretical literature on job search in unemployment going back to e.g., McCall (1970); Mortensen (1977) and Van Den Berg (1990). In particular, the results help tie the dynamics of job search behavior with so-called directed search models in which workers make active decisions about where to search and apply for jobs (see e.g., Nekoei and Weber, 2017). In the paper we also compare our results to theoretical work on stock-flow matching in the labor market (Coles and Smith, 1998; Ebrahimy and Shimer, 2010) and on job search in the face learning and/or behavioral biases (Burdett and Vishwanath, 1988; Gonzalez and Shi, 2010; Spinnewijn, 2015; Dellavigna et al., 2017; Mueller et al., 2021).

The rest of the paper is structured as follows: First, in Section 2 we present the institutional setting and the data sources used in the paper. This includes an extensive discussion of the validity and content of our data on applied-for jobs. In Section 3 we present our empirical specification. In Section 4 we discuss predictions from existing theories regarding our analyses. In Section 5 we present results and discuss their implications. Section 6 concludes.

2 Data and institutional setting

Over the next subsections, we first present the details of our empirical setting and the data set we use which combines data on applied-for jobs (the so-called Joblog data) with other administrative data sources. The presentation of the setting and job search measures is adapted from Fluchtmann et al. (2019), in which we use the same data sources to study gender differences in job search.

2.1 The Danish UI system and the Joblog application data

The Danish UI system is based on voluntary membership. To be eligible for UI, workers are required to sign up and contribute to one of the 24 UI funds sufficiently well in advance of becoming unemployed. The vast majority of Danish workers satisfy these eligibility requirements.[1] When a UI eligible worker becomes unemployed, benefits are available for up to two years. UI benefits are determined at a replacement rate of 90 percent of previous income and a cap of 18.500 DKK (2.500 Euro in 2017) per month. The cap is binding for the majority of workers. For workers who exit unemployment, the two year eligibility period resets after one year of employment. For workers who run out of UI benefits, social assistance benefits are instead available. Social assistance benefits are means tested and thus not available for individuals with e.g., savings or other assets.

To remain eligible for UI while unemployed, UI recipients have to be actively searching and applying for jobs.[2] Further they regularly have to document that they are satisfying eligibility criteria, i.e., show examples of submitted applications etc. Since 2015 this documentation has been centralized through an online system called *Joblog*. The Joblog system works as follows: To register applied-for jobs in the system, unemployed workers need to log in to the central online platform of the Danish public employment service *(Jobnet)*. This platform is also where UI recipients register for UI benefits at the start of their unemployment spell and where they book meetings with case workers and the municipal job centers. The website also serves a job board with posted vacancies. Through the online platform they can enter the Joblog system, and fill out a form describing the particular job they have applied for. 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. In addition, the job seekers must also provide information on how they found the job and the method by which they applied for it.[3] As we describe further below, it is the information entered on these forms that we use to measure job search behavior and link with

[1] In 2015, 76 percent of Danish employees were members of a UI fund while over 70 percent of the gross unemployed were UI recipients. Among the residual group of gross unemployed, more than 20 percent receive means-tested social assistance and is therefore likely to have exhausted UI prior to this (see e.g., Danish Economic Council, 2014).

[2] 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.

[3] The UI recipients also have to regularly attach the actual application as well to the job log. In our data we can see that around a third of joblogs contains an attachment, but we cannot see the actual content (for half of all the joblogs there is also a link to the specific vacancy). Further, note that the Joblog system also allows the UI recipients to register additional information. This includes registering jobs that they plan to apply for in the future and registering other activities such as participating in a job interview. Since UI recipients are not required to use most of these features however, fewer UI recipients register these activities. In our analysis, we only use data on jobs that UI recipients report having applied for.

additional administrative data.

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.[4] 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.[5] Failure to comply with documentation requirements results in sanctions in the form of lost or reduced UI payments. 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 or fake applications) similar requirements apply.

The above shows that UI recipients face a clear economic incentive to comply with the requirements and register submitted job applications in Joblog. As we discuss below, these incentives have resulted in a very high level of usage, and correspondingly a high level of coverage for our data.

2.2 Selecting the analysis sample

Our baseline sample is constructed from administrative data on UI payment and consists of all individuals of Danish origin entering new unemployment spells between September 2015 and September 2017.[6] We restrict our sample to individuals with a full two-year availability of UI benefits at the start of their unemployment spell. We do this to ensure that individuals in our analysis face the same incentives in regards to the timing of future benefit exhaustion.

In the baseline data, we follow each new UI recipient for as long as possible given our sample window or until a given UI spell ends. As the available data ends in September 2017, this implies

[4]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 risk losing the reimbursements of UI. The National Labor Market Authorities further use the Joblog data to create monthly scoring cards (or performance assessments) across different UI funds and municipalities of e.g., the share of UI recipients satisfying the UI eligibility requirements and log at least 1.5 applications per week, see https://va.star.dk/.

[5]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 (see also footnote 4).

[6]The start and end sampling points reflect that we observe Joblog entries from September 2015 and have labor market data available until September 2017. We construct the panel on the spell level, thus some individuals may enter multiple times, though in practice the additional sample restrictions we make below means that only a very small fraction of individuals show up with more than one spell.

that some spells in our data are right-censored, see Table 1. We define an unemployment spell to end when there are 4 consecutive weeks where no UI is paid out. This implies that a UI exit can be associated with entering other public benefits e.g., sickness benefits (where job search requirements differ) or employment. As evident below in Table 1 it means entering employment in the vast majority of cases.

For each UI recipient in this data, we use a unique person identifier to identify all applied-for jobs that they have registered in the Joblog system during their 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. 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 also Appendix A.1 for additional details on these data sets). The resulting data thus contains a rich set of background characteristics, information on applied-for jobs throughout the UI spell, information on the start and end date of the UI spell and characteristics of the new job for those UI recipients who transition into employment.

To arrive at our final analysis sample, we impose four restrictions on the baseline sample: First, we require that the individual has registered at least one applied-for job during the unemployment spell so that some information on search behavior is available. Because of the high level of usage of the Joblog system, this only removes very few individuals. Second, we impose the restriction that the UI spell lasts at least 8 weeks. We do this to remove individuals who are de facto making a job-to-job transition but who are temporarily receiving UI while waiting for their new job to start. Third, for UI recipients who eventually leave UI for employment we drop information on jobs that they apply for in the last 4 weeks before starting their new jobs. This gets rid of applications that UI recipients are making after successfully landing a job but before this job has actually started.[7]

The final restriction we impose is that we limit our analysis to focus only on the first year of each UI spell. We note that this focus differs from much of the existing literature on the dynamics of job search. Much of this work focus on behavior later in the unemployment around the time UI benefits expire. Our main motivation for instead focusing on the early part of an unemployment spell has to do with quantitative importance. While studying behavior at the time of benefit exhaustion can be extremely useful for testing different theories of job search (see e.g., Card et al., 2007; Ganong and Noel, 2019; Dellavigna et al., 2020; Marinescu and Skandalis, 2021), benefit exhaustion is in fact a relatively rare event in many settings - particularly in settings with relatively generous UI duration (Danish Economic Council (2014)). In the Danish setting where UI benefit duration is 2 years, more than 80 percent of individuals exit the UI system within the first year. This underscores the

[7] 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 raw 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. As consequence of the wording of the Danish UI rules during our sample frame, however, such individuals were in principle required to both apply for and register applications in Joblog, despite the fact that they had a new job lined up with a known start date. They obviously face a very peculiar set of incentives in their application decisions. By dropping applied-for jobs in the last 4 weeks before a new job start we get rid of these applications.

importance of understanding job search behavior in earlier parts of the spell.[8]

Table 2 in Appendix A.1 shows how the sample size changes with each of these four restrictions. Our final analysis sample consists of 136.291 individuals covering 141,551 UI spells and 4,3 million applied-for jobs. In Table 1 we provide some summary statistics on our final sample for the analysis. We report statistics for both the full sample and different unemployment duration groups.

2.3 Measuring job search behavior

We use the information on applied-for jobs in Joblog to construct monthly measures of job search behavior. From the raw Joblog data, we directly observe the date of each applied-for job, whether the job is part-time or full-time, as well as how the job was found and how the individual applied for the job. Using the firm's address reported in Joblog and the individuals' residence municipality we determine commuting times.[9]

Based on a string matching procedures, we further use the job title of the applied-for job to determine the occupation of the job according to the Danish version of the ISCO classification (DISCO). We also use string matching to link the job applications to firms in the administrative registers based on the reported firm name and address. Thereby we can attach information about the occupation, industry, location and the firm wage level to the application. In the source data, we successfully match 86 percent of applications to a firm and 82 percent to an occupation. See Appendix Section A.2 for additional details on the matching procedure and occupation and industry measures.

Our application data does not contain direct measures of the wage an applicant would have been paid in each applied-for job. In our analysis of applied-for wages we instead rely on two indirect measures of applied-for wages. First, we use realized data on wage payments for new hires to predict the (log) wage for each potential job based on the job's observable characteristics.[10] Since this measure will reflect the typical wage paid in a given type of job, we refer to this measure as the *typical wage* of the applied-for job. Second, we use the full Danish matched employer-employee data to estimate an AKM model (Abowd et al. (1999)) for all employees and use the implied firm fixed effects to capture the wage-level at the applied-for firm.

For hours, our data contain information on whether each applied-for job is indicated to be

[8]The fact that most people leave UI relatively fast in our data implies that our data is less useful for learning about behavior around the time of benefit exhaustion simply because sample sizes become small when looking at longer spells. This issue is compounded by the institutional setting surrounding the Joblog data. In particular, the requirement to register applied-for jobs only applies to individuals receiving UI. After exhausting UI benefits, job seekers in our setting thus face no formal incentive to register applied-for jobs leading to a large (and discontinuous) drop in the coverage of our application data.

[9]The commute areas are based on definitions from Statistics Denmark and are readily available in the register data. The commuting times are based on distance measures from Google maps API, this data come from Harmon (2013).

[10]In this prediction exercise we include the occupation and industry of the applied-for job (up to the three digit level) as well as the estimated wage level of the employing firm based on the estimated firm fixed effect from an AKM model estimated across all employees in Denmark (Abowd et al. (1999)). The prediction is based on a linear log wage regression that allows for a rich set of interactions between the included variables and uses the Rigorous LASSO of Belloni et al. (2011) to avoid overfitting. See Appendix A.3 for details.

Table 1: Summary statistics - Final sample

		By Realized Unemployment Duration				
	Total	<3 Months	3-6 Months	6-12 Months	>12 Months	Right Censored (and <12 Months)
Demographics						
Female	0.52	0.50	0.51	0.55	0.54	0.55
Age	38.49	37.27	38.74	38.54	40.35	38.69
Married	0.38	0.36	0.40	0.39	0.41	0.37
Living in city	0.36	0.35	0.33	0.38	0.41	0.41
Education†						
High-School	0.25	0.25	0.26	0.24	0.23	0.22
Vocational	0.51	0.54	0.50	0.50	0.49	0.51
Academic	0.23	0.21	0.23	0.25	0.27	0.26
Previous job						
Previous wage (DKK/hour)	187.99	186.81	188.53	188.94	186.99	189.13
Employment rate†††	0.71	0.74	0.73	0.69	0.66	0.69
Previous UI experience†††	0.04	0.04	0.05	0.04	0.02	0.01
Spell						
Average Spell lenght (weeks)	24.76	9.88	18.27	36.01	69.61	..
Joblogs p. week	2.07	2.04	2.08	2.09	2.09	2.06
Observed spell ends with employment	0.69	0.90	0.89	0.84	0.40 (0.45)‡	..
Of those:						
Successful log††	0.47	0.43	0.47	0.52	0.54	..
N (number of spells)	141,551	33,763	47,842	23,720	12,179	24,047

Notes: †Education: Degree level of highest finished education. †††Employment rate (Previous UI experience): Fraction of time spent in employment (UI) in the year prior to entering the sample. †† Success log: Fraction of spells ending in employment for which we can identify the successful application in Joblog. ‡the number in parenthesis gives the share of spells which are above 12 months but right-censored in our panel.

part-time or full-time. We primarily use this binary distinction in our analysis. To quantify the effect on average hours of applied for jobs, however, we also convert the information into a rough proxy for actual hours assuming that part time jobs are 20 hours per week, while full time jobs are 37 hours per week. This follows a common labeling of full and part time in the Danish labor market.[11]

Our analysis also focuses on the occupation and industry of the applied for jobs. Here we specifically focus on the proximity (or similarity) of applied-for jobs' industry and occupation relative to the workers' previous jobs. The motivation is that jobs which require a very different skill sets to that of the job seekers' previous job are likely less attractive to apply for. For occupations, we base our measure of relatedness on the latest version of the O*NET Related Occupations Matrix. This data contains, for each occupation, the top 10 related occupations in terms of skills and experience (Allen et al., 2012). In order to get a similar measure for skill relatedness across industries, we use data from Neffke and Henning (2013). This data contains skill-relatedness estimates across NACE Rev. 2 industries. We select the top 10 most related 3-digit industries to resemble the occupation measure and define this set as the group of related industries. Based on these measures, we say that an occupation or industry is *unrelated* to the job seekers past occupation or industry if it is not among the 10 most related occupations or industries. See Appendix Section A.4 for additional details and Appendix Section B.2 for robustness checks using alternative measures of related occupations which are build using the Danish register data.

For each applied-for job, we lastly also use data on how the UI recipient found the job and the method by which they applied for it. In terms of the methods by which the job was found, we examine whether the job seeker reports finding the job via a publicly posted vacancy, whether they heard about the job through their social network, whether they where directly contacted by the firm (or other headhunters) or whether they simply opted to apply to the firm despite not knowing whether they had vacancy or not (referred to below as an uninvited application). In terms of the application method, we examine whether the job seekers submitted the application in writing (via mail or e-mail), verbally (phone or in-person), through some online platform or via other means.

Since we conduct our analysis at monthly frequency, we aggregate all the measures of application behavior to monthly averages, shares or percentiles. Our final data set takes the form of a monthly panel. For each individual and spell, the data set contains information on the applied-for jobs in each month, from the beginning of the spell and until the spell ends, until the individual has been unemployed for 12 months or until the last month where data is available if the individual is censored. In Table 3 in Appendix A.1 we describe our sample in terms of average applied-for job characteristics across months in our final sample and across the different unemployment duration groups similar to Table 1. The table also shows the fraction of applied-for jobs with missing information on different measures.[12]

[11]Full time positions are always defined as 37 hours per week in the Danish context. The exact number of hours involved in a part-time position varies in the Danish labor market. Sometimes anything below 37 hours can be categorized as part-time work. Part-time work with 20 hours per week is common however and we therefore work with the assumption that all part-time jobs involve 20 hours of work.

[12]As explained above there are some applied-for jobs which we cannot link to firms and/or occupations in the

2.4 Coverage of the 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 information on 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 semi-official 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 is just above 2 (see Table 1) and the distribution of applications per week is strongly centered just around 2 applications per week (see Figure 12 for a histogram of monthly and 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.5 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.

Since the focus of this paper is on *how* and *where* individuals search for and apply for jobs, imperfect coverage is not a problem as long as the subset of applied-for jobs that an individuals register in a given month are representative of their overall application behavior in that month. 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.5.

registers, or where we do not have the necessary information on the UI job seeker's previous job. We simply drop these applications when constructing the relevant monthly averages or shares.

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.

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, recall that our raw data is estimated to cover between 69 and 80 percent of all applications and that we successfully match 86 percent of these applications to the corresponding firm. Moreover, survey data from Denmark suggests that around 73 percent of new hires out of unemployment involve the job seeker actively applying for the job.[13] This suggest a simple test of the representativenes of the application data: If the applied-for jobs contained in our data are a representative subset of all applied-for jobs, we should be able to match somewhere between 43 and 50 percent of new hires to a past application.[14] 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 our raw data, we are in fact able to match 47 percent to a past application, consistent with the data being representative.[15]

Finally, a particular concern for our analysis is whether our data meaningfully captures changes in a person's application behavior over time. As a check on this, we examine whether application data from the end of the unemployment spell are able to predict later job outcomes, also *after* we first condition on application data from the early months of the spell. We find that the characteristics of applied-for jobs in the last months of a spell remain highly predictive of the type of job that the UI seeker finds even after conditioning on past application behavior. In other words, two job seekers who have the same application behavior in the first part of their UI spell but who later diverge in their search behavior, also face very different job outcomes. This confirms that changes in application behavior over time in our data captures meaningful changes in behavior. For further details see Appendix A.5.

[13]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).

[14]If the applications in our data are a representative subset of all applications, this further implies that when some application leads to a new hire, the likelihood that this application is in our data should be between 59 and 69 percent. In practice of course, not all new hires stem from a job application. Based on survey data, we estimate that 73 percent of new hires out of unemployment in Denmark stem from a job application (see Appendix A.5). Across all the new hires, we should thus be able to link the hire to an application in our data between 43 percent and 50 percent of the time ($0.69 \cdot 0.86 \cdot 0.73$ and $0.80 \cdot 0.86 \cdot 0.73$).

[15]Further, in Appendix A.6 we also show results focusing only on the spells where we have this direct link from an application to later hiring outcomes. The results are very similar to those reported below based on the full sample suggesting that spells where the link to a successful application exists are not selected in terms of job search dynamics.

3 Empirical specification

The aim of our analysis is to document how individuals are changing their search behavior over time within their unemployment spell. Accordingly, our main empirical approach is to estimate event-study regressions for individual job search behavior. Let i index individuals in our data, let s index unemployment spells for each individual and let $m = 1, 2..., 12$ denote months since the start of the unemployment spell (with $m = 1$ denoting the first month of the unemployment spell in which the spell starts).[16]

Let $SearchBehavior_{ism}$ denote some measure of job search behavior for person i, in spell s, in month m of their unemployment spell. An example of this could be the average wage level of the applied-for jobs during the month in question. Using OLS, we then estimate a simple panel regression with unemployment-duration-month and person-by-spell fixed effects, while clustering standard errors at the level of the individual:

$$SearchBehavior_{ism} = \alpha_{is} + \tau_m + \varepsilon_{ism} \tag{1}$$

The parameters of interest here are the duration-month fixed effects, $\tau_1, \tau_2, ..., \tau_{12}$. These capture how individuals (on average) change their search behavior over time within an unemployment spell. The person-by-spell fixed effect in this regression, α_{is}, absorbs differences in the overall level of search behavior across individuals and unemployment spells. If these were not included, the estimated duration-month fixed effects would be affected by dynamic selection (*in levels*): If some individuals have systematically higher or lower levels of $SearchBehavior_{ism}$ throughout their spell and also tend to exit unemployment faster, the estimated duration-month fixed effects would tend to show a mechanical downward or upward trajectory because the duration-month fixed effects for later months are estimated on a sample where these individuals have already exited unemployment.[17] Including the person-by-spell fixed effects addresses this issue however, by absorbing any level differences across individuals and spells.

While the inclusion of person-by-spell fixed effects flexibly allows for differences in the level of search behavior, we note that the duration-month fixed effects in equation (1) still impose a single path of search behavior during unemployment for all individuals in the sample. If job search behavior in fact follows different paths for different individuals, the estimated duration-month fixed effect for duration m will reflect an average of these different paths based on those individuals who are in fact still unemployed at duration m. This can create a different form of dynamic selection *on changes*. In Section 5.6, we directly explore heterogeneity in the time path of search behavior by estimating equation (1) separately for groups workers who exit unemployment at different times in

[16] Note that individuals in our data enter unemployment at different times. As a result, m does not correspond to calendar time.

[17] The summary statistics presented in Table 1 indeed show evidence that such dynamic selection occurs in our setting: individuals who leave unemployment earlier are younger and more likely to hold a vocational education. Further they are likely to have been employed immediately before UI entry.

their spell. As will be clear, we see no indications that dynamic selection on changes is significantly affecting our results; the estimated time paths across different groups of workers are remarkably similar.

Because equation (1) includes both a person-by-spell fixed effect and a full set of duration-month fixed effects, it is necessary to impose a normalization for estimation. A common normalization is to impose that τ_1 equals zero. Throughout most of our analysis, we take a different approach and normalize τ_1 to equal the mean of the outcome variable in the first month of unemployment. This makes it straightforward to compare the estimated changes over time to the level of the outcome variable in the overall sample.[18] When we split the sample to compare different groups of workers, however, we instead normalize τ_1 to zero so as to facilitate comparisons between the groups.

4 Theoretical predictions on changes in search behavior

A main motivation of our empirical analysis is to inform future theoretical work. Before presenting our empirical results on how UI recipients change where and how they apply for jobs over time, it is useful to briefly discuss what major existing theoretical work predicts regarding the dynamics of job search. We discuss this below.

The canonical random search model

The canonical model of random job search simply assumes that during unemployment, job offers arrive exogenously at some rate, λ, with wage offers drawn from some exogenous distribution, F (see e.g., Mortensen, 1977; McCall, 1970; Rogerson et al., 2005). The only decision that UI recipients make in this model is whether to accept a given job offer or instead remain unemployed and continue searching. This decision is summarized by the lowest acceptable wage, the reservation wage. In this model, decisions change when the expected value of remaining unemployed changes. Typically, this value slowly decreases over time, for example because individuals anticipate that their UI benefits will eventually expire.[19] The implication is that an individual's reservation wages gradually declines over time.

A standard extension of the canonical model assumes that the arrival rate of job offers, λ, is influenced by the job seeker's decision about how much search effort the job seeker exerts. Since higher search effort increases the probability of leaving unemployment, this generates the additional

[18] Of course, the choice of normalization has no effect on the estimated changes in behavior. Normalizing τ_1 to zero would simply shift down all our estimated coefficient by the sample mean of the outcome in the first period.

[19] Similar predictions arise if the value of staying unemployed decreases for other reasons, for example because individuals gradually run down their savings over the unemployment spell (see e.g., Lentz and Tranæs, 2005; Mortensen, 1986) or if future job opportunities worsen over time because of human capital depreciation or screening (see e.g., Lockwood, 1991; Pissarides, 1992; Acemoglu, 1995). Machin and Manning (1999) provide a general discussion of features which may lead to changes in job search and/or the hazard rate out of unemployment when unemployment duration increases. Krueger and Mueller (2016) present a calibration and discuss the quantitative importance of the decline in reservation wages during the unemployment spell in a model where wages is the only characteristic of a job.

prediction that search effort should be gradually increasing over the course of an unemployment spell as the value of staying unemployed falls (see e.g., Van Den Berg, 1990).

Finally, if the model is extended so that jobs vary in other characteristics besides the wage, the reservation wage is replaced by a reservation utility that acceptable jobs must provide given their characteristics (see e.g., Hall and Mueller, 2018). Just like the reservation wage, this reservation utility will be decreasing over time, implying that job seekers will gradually be willing to accept jobs with less and less attractive characteristics over time.

Application decisions and directed search

As written, the canonical random search model implies that job seekers make no systematic decisions about where to search and apply for jobs and therefore have no influence over the *types* of job offers they receive. Under this strict interpretation, the model would imply that we should see no changes over time in the types of jobs people apply for in our data.

A simple reinterpretation of the model primitives relaxes this implication however. Instead of letting λ denote the arrival rate of job offers, we can interpret it as the rate at which the job seekers learns about a potential job and interpret F as the distribution of wages across these potential jobs. After learning about a potential job with a wage drawn from F, the job seeker's key decision is then whether to apply for the job or not. As before, this decision can be summarized by a reservation wage that now indicates the lowest wage job that a worker is willing to apply for. Also as before, this reservation wage will be decreasing over time if the value of unemployment decreases. For our data, this interpretation implies that the average wage of applied-for jobs should decrease over an unemployment spell, as UI recipients gradually lower their reservation wages and they also start applying to lower and lower wage jobs.

A different approach to modeling application behavior, comes from the literature on so-called directed search which explicitly deals with workers' decision about which types of jobs to search and apply for. Standard models in this literature emphasize that when deciding where to search and apply for jobs, workers face a trade-off between the attractiveness of a job and the likelihood of getting the job if applying (see e.g., Moen 1997; Rogerson et al. 2005; Wright et al. 2020). Building on a set of empirical findings, a recent paper by Nekoei and Weber (2017) embed this trade-off in a dynamic setting where workers must continuously choose a target wage or target utility level for the jobs they search and apply for. If the value of remaining unemployed is declining over time in the usual fashion, this yields a prediction similar to the prediction of declining reservation utility in the random search framework: Over the course of their unemployment spell, job seekers will continually lower their target wage or utility level and apply for jobs offering lower wages and/or less attractive characteristics.

In terms of applied-for wages, the only difference between the "target wage" implication of Nekoei and Weber (2017) and the "reservation wage" implications generated by versions of the random search framework lies in the exact nature of why applied-for wages fall over time: If changes in job application decisions are explained by a gradually declining reservation wage, changes in behavior should show up as a gradual reduction in the lowest applied-for wages. If changes are instead explained by a shift in the target job, we would expect the entire distribution of applied-for wages to gradually shift down.

Stock-flow matching, learning and behavioral biases

Another theoretical framework that generates predictions for our empirical analysis, is the stock-flow matching framework (see e.g., Coles and Smith, 1998; Ebrahimy and Shimer, 2010; Coles and Petrongolo, 2008). This framework emphasizes the distinction between the flow of new vacancies into the labor market and the stock of already existing vacancies. On the worker side, this distinction implies that newly unemployed workers face a larger set of potential vacancies to apply to during the early periods of their unemployment spell. A newly unemployed worker can apply to the entire set of vacancies that are available. In contrast, workers who have been unemployed for a while have already sent unsuccessful applications to most of the existing vacancies, or considered them inappropriate, and are therefore limited to wait for entirely new vacancies to post. The broad implication is that workers who are in the very beginning of a new unemployment spell may have different job application behavior simply because they face a larger set of available vacancies; a vacancy supply effect.[20] Empirically, this would generate sharp changes in application behavior that occur only at the beginning of the unemployment spell hereafter the job seeker should primarily apply to new vacancies.

Job search models with learning also have potential implications for our empirical analysis. If job seekers have incomplete information about their job finding prospects, they are likely to gradually learn about them over their unemployment spell as they experience the job search process in practice. This learning may in turn impact their job search behavior. Existing theoretical work on job search and learning have emphasized the prediction that learning should lead job seekers to target and accept lower and lower wage jobs over time. This is because each additional week of unsuccessful job search is likely to be a signal that employment prospects are worse than expected (see e.g., Burdett and Vishwanath, 1988; Gonzalez and Shi, 2010). If job seekers are assumed to also learn about the hiring probability for different types of jobs and/or the effectiveness of different methods of search, learning could also lead to broader changes in job search behavior over time.[21] In terms of timing, changes in search behavior due to learning may be particularly pronounced early in the spell if job seekers start out with biased beliefs as has been suggested in recent work

[20] In the stock-flow framework, unemployed workers and vacant jobs coexist because they cannot form sufficiently productive matches (i.e not due to search frictions as such). In its standard form job search dynamics would arise solely from the fact that a set of vacancies have been exhausted and that the unemployed is focusing only on the inflow of new vacancies (the environment is otherwise stationary). Non-stationarities in the economic environment such as e.g., time-limited benefits or human capital depreciation may lead the "longer term unemployed" to partly revisit vacancies in the stock later on in the unemployment spell if his/her preferences over matches change. We note that if this mechanism is present we should expect applied for job characteristics to differ depending on whether vacancies are from the flow or the stock.

[21] Recent empirical work have suggested gains from more tailored advice to job seekers suggesting that the assumption about e.g., complete information about both future employment chances and the exact location of jobs are at best approximations (see e.g., Altmann et al., 2018; Belot et al., 2019).

(see e.g., Spinnewijn, 2015; Conlon et al., 2018; Mueller et al., 2021). Moreover, learning models in which job seekers beliefs eventually converge to the truth imply that changes in job search should dissipate over time. The speed with which this occurs will depend on the speed of learning.

Finally, models of other types of behavioral biases may also have implications for the dynamics of job search. Models of reference-points and reference-dependent preferences have received particular attention recently, see e.g., Dellavigna et al. (2017).[22] In particular, these models also predict declining reservation or target wages over time as workers become "accustomed" to a lower income or consumption. The timing of this decline depends on the speed with which reference points adjust but the decline should be focused in the early part of unemployment spell. Estimates in Dellavigna et al. (2017) imply that reference points should have completely adjusted about 6 months into the unemployment spell. Finally, while reference-dependence have mostly been considered in relation to income or consumption, we note that it could also apply to other variables with implications for job search behavior. If preferences are reference dependent over commuting time or occupation choice, for example, theory would predict gradual adjustments in search behavior on these dimensions as well.

5 Results

We now turn to our empirical analysis. We structure the presentation around five main facts and discuss the results in relation to the theoretical predictions from Section 4. We do this in the spirit of Occam's razor by discussing which theoretical mechanisms are capable (or necessary) to match the patterns in the data. At the same time, we emphasize at the outset that the aim of our analysis is *not* to provide a sharp test between different theories of job search - as made clear in the previous section, many of the empirical predictions of the different theories are similar and/or depend on concepts that are fundamentally unobserved in our data, such as the speed of learning or the rate at which reference-points adjust. In addition, we do not view the theories as mutually exclusive - the mechanisms of one theory may well explain one pattern of changes in job search behavior, while the mechanism of a different theory explains changes in behavior in a different dimension or over a different time horizon.

5.1 Wages and hours of applied-for jobs

In the top part of Figure 1 we examine how the wages of applied-for jobs changes across the unemployment spell. Specifically, the figure plots the estimated duration-month fixed effects from our event study specification (equation (1)) when using measures of average applied-for wages as the outcome variable in our event study specification.

In Figure 1a, we examine the average (log) wage of applied-for jobs using our measure of the typical (log) wage for jobs given their characteristics. In Figure 1b, we instead focus on the average

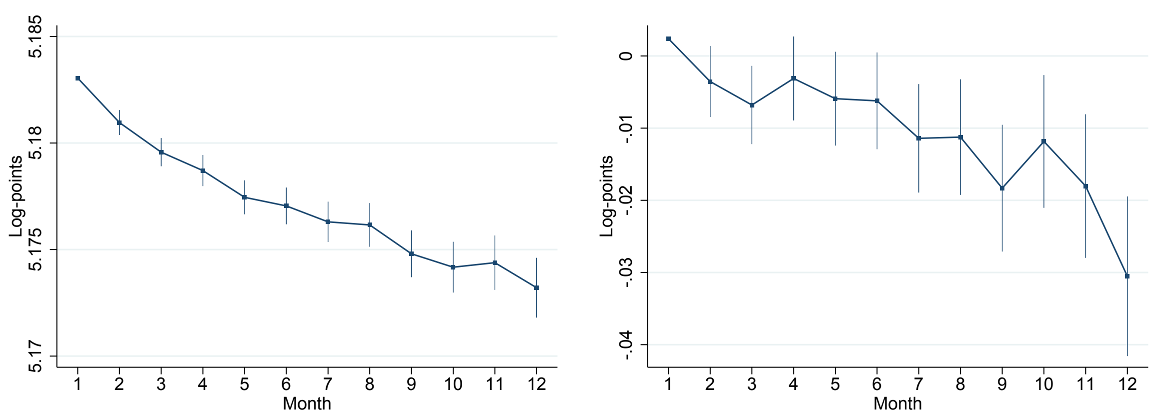
[22] Brown et al. (2011) presents an overview of other behavioral biases which could also explain declining reservation wages throughout the unemployement spell such as e.g., the sunk cost fallacy or subjective search costs. They analyze these competing mechanisms in the lab.

firm fixed effects from an AKM wage equation, thus zooming in on the firm component of the job's wage level.

For both measures of wages, we see the same pattern. Over the course of the unemployment spell, workers gradually apply to gradually lower-paying jobs. This behavior is completely in line with theoretical predictions from standard search models and with most of the previous evidence on reservation wages. If the value of remaining unemployed falls over time - for example because UI benefit exhaustion moves closer - workers should gradually be willing to apply more to jobs that pay lower wages but are easier to get.[23] In terms of the magnitude, the results based on typical wages in Figure 1a suggest that the average wage of applied-for jobs drops by about 1 log point (1 percent) over the first year of the unemployment spell. This magnitude is consistent with evidence from France presented in Marinescu and Skandalis (2021).

Figure 1: Changes in wages of applied-for jobs

(a) Average typical wage of applied-for jobs (b) Average firm wage level of applied-for jobs



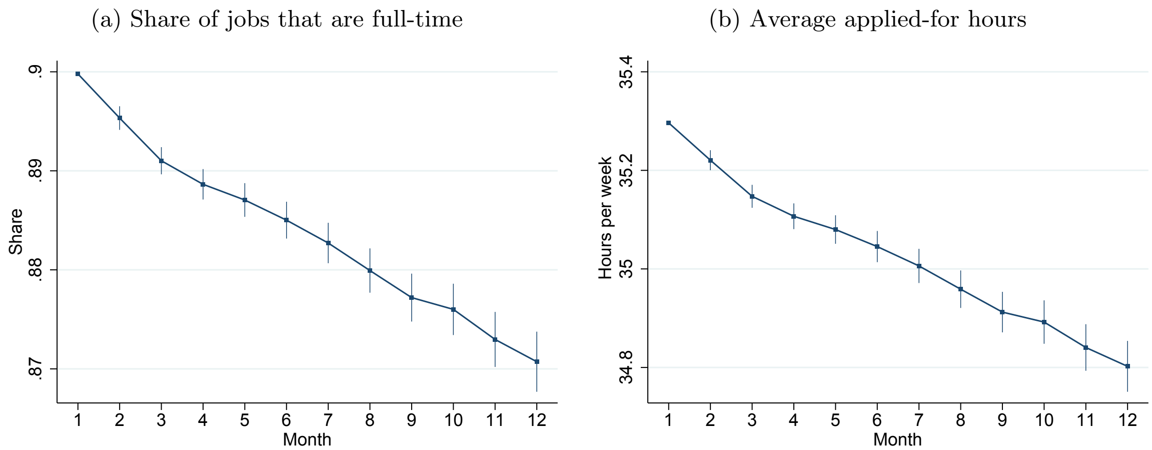
Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 for two outcomes related to the wage of the applied-for job. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. The average typical wage is the average applied-for (predicted) wage in a given month, and the firm wage level is estimated firm fixed effects from an AKM regression, see Appendix Section A.3 for further details. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

In Figure 2, we examine another main determinant of re-employment earnings, namely the hours offered in the applied-for jobs. Using the share of applied-for jobs that are full-time as the outcome variable in the event study regression, Figure 2a shows a similar pattern as for wages. Over time, the share of applications going to full-time jobs decreases gradually. Assuming that most workers prefer jobs offering full-time hours and earnings, the decreasing pattern we see in most months

[23]The changes with respect to firm type (Figure 1b) can be seen as one channel though which changes in the average applied-for typical wage (Figure 1a) arise. Later in the paper we show that underlying the changes in applied-for typical wages are also adjustments on applied-for occupation and industries. Further, note that the average firm fixed effects are net of industry specific means. Therefore changes over the duration of the UI spell in Figure 1b do not reflect changes in the targeted industries but instead reflect within industry changes in the type of firms targeted. In Appendix B.1 we show very similar patterns when we employ two alternative measures of a "firm type" suggesting that the result is robust.

here is again easily explained by a standard search model in which the value of staying unemployed gradually decreases. Over the first year of unemployment, the share of applications going to full-time jobs decreases by 2.9 percentage points. This corresponds to a decrease in average applied-for work hours of around 1.4 percent (see Figure 2b). Combined with the drop in applied-for wages documented above, the average earnings of applied-for jobs thus drops about 2.4 percent over the first year of unemployment.

Figure 2: Changes in hours of applied-for jobs



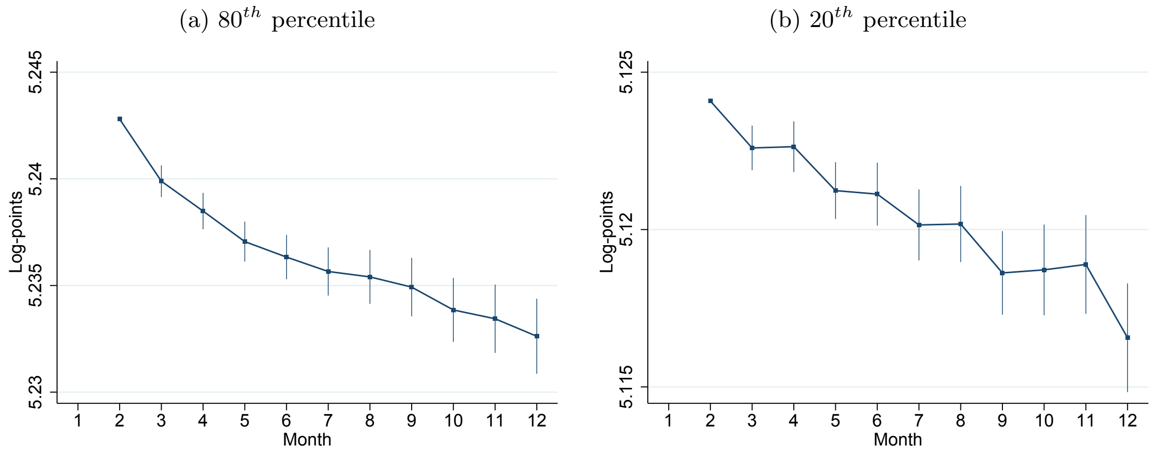
Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 for two outcomes related to the hours of the applied-for job. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. See Section 2.3 for further details on these outcomes. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

5.2 The distribution of applied-for wages

The fact that average applied-for wages are gradually declining over time is in line with theoretical predictions from standard search models. As noted in Section 4, however, different theoretical frameworks make different predictions about how the gradual drop in applied-for wages should come about. Models in which worker decisions are summarized by a reservation wage decision - as in a random search framework - predict that the drop of average applied-for wages is caused by workers gradually starting to also send applications to lower and lower wage jobs. In contrast, models in which search decisions are characterized by a target wage predict that over time the job seeker shift all applications to lower wage jobs. Because our data contains several applications per months for each job seeker, it is possible for us to shed some light on this distinction by examining changes in the full distribution of applied-for wages over time.

In Figure 3, we thus also examine changes in the distribution of applied-for wages. Using data on the typical wages of all applied-for jobs, for each job seeker we construct measures of the 80th and 20th percentile of applied-for wages in each month. In each panel of Figure 3 we then presents event study estimates with these percentiles as the outcome variable: Figure 3a examines

Figure 3: Changes in the distribution of applied-for wages



Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 when the outcome is the 80^{th} (20^{th}) percentile in the distribution of applied-for wages in a given month. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

the 80th percentile of applied-for wages and Figure 3b examines the 20th percentile. As noted, meaningfully measuring these percentiles is possible because our data contains several applications for each worker in each month - on average each worker in our data registers about 6.5 applications per month (see also Table 3). Because we tend to observe markedly fewer applications in the very first month of an unemployment spell, however, we exclude this month from the analysis when examining wage percentiles. We instead use month 2 as the baseline month in this analysis.[24]

As Figure 3 shows, the decrease in applied-for wages over time are not driven by particular changes in the top or bottom of the distribution of applied-for jobs; both the 80th and 20th percentile of applied-for wages shows a decrease that is similar to the one seen for the the average wage.[25] The changes in applied-for wages over time thus seem most in line with the existence of a gradually declining target wage rather than a declining reservation wage: Over time, workers shift all their applications towards jobs with a gradually lower wage level. They do not merely extend the set of jobs they apply to by including more lower paying jobs.

5.3 Proximity of applied-for jobs

We next look at the characteristics of applied-for jobs beyond hours and wages. One of the major advantages of our data on applied-for jobs is its linkage with additional administrative data on firms and jobs. This allows us to characterize changes in applied-for jobs over time along a number of dimensions.

[24]Because workers take some time to learn about and get instruction in the use of the Joblog registration system, we see a clear pattern that the number of registered jobs in the first month is relatively low. On average workers in our analysis register 4.3 jobs in the first month of their unemployment spell.

[25]Results are similar if we focus on other percentiles than the 90th or the 10th percentile.

We focus our analysis here on the proximity of the applied-for jobs in three dimensions. Geographically, we measure how far the worker would have to commute to the job. In terms of occupation, we measure to what extent applied-for jobs are in occupations that are closely related to the worker's previous occupation. Finally, we also examine whether the applied-for jobs are in industries that are closely related to the workers previous industry. The motivation is that both commuting and occupation/industry switching are likely to incur costs for workers.[26]

The top part of Figure 4 shows estimated changes in the share of applied-for jobs that are in unrelated industries or occupations using our event study specification.[27] The bottom part of Figure 4 shows corresponding results for the average commuting time to the applied-for jobs.

For both industry, occupation and geographical proximity, we see a similar pattern. Over the early periods of the unemployment spell, there is a clear change in the characteristics of applied-for jobs as UI recipients start to search more broadly in all three dimensions, i.e., they search less in related occupations/industries and they consider jobs with higher commuting time. However, after this initial adjustment during the first 3-4 months, estimates are basically a flat line and we see little change in search behavior in these dimensions.

5.4 Understanding changes in the proximity of applied-for jobs

The results above regarding the proximity of applied-for jobs sit awkwardly with standard versions of canonical search models, especially given the previous results on declines in applied-for wages and hours. If the value of unemployment is continually decreasing - as the results on hours and wages suggest - standard search models predict that workers should also continually be shifting their applications to less and less attractive jobs along other dimensions in order to increase their likelihood of getting hired. Figure 4 however shows that for the majority of the first year in unemployment, job seekers are not changing the locations, industries and occupations they apply to. In this section we discuss and examine possible explanations for this observed pattern based on alternative theories of job search or extensions to the basic search framework.

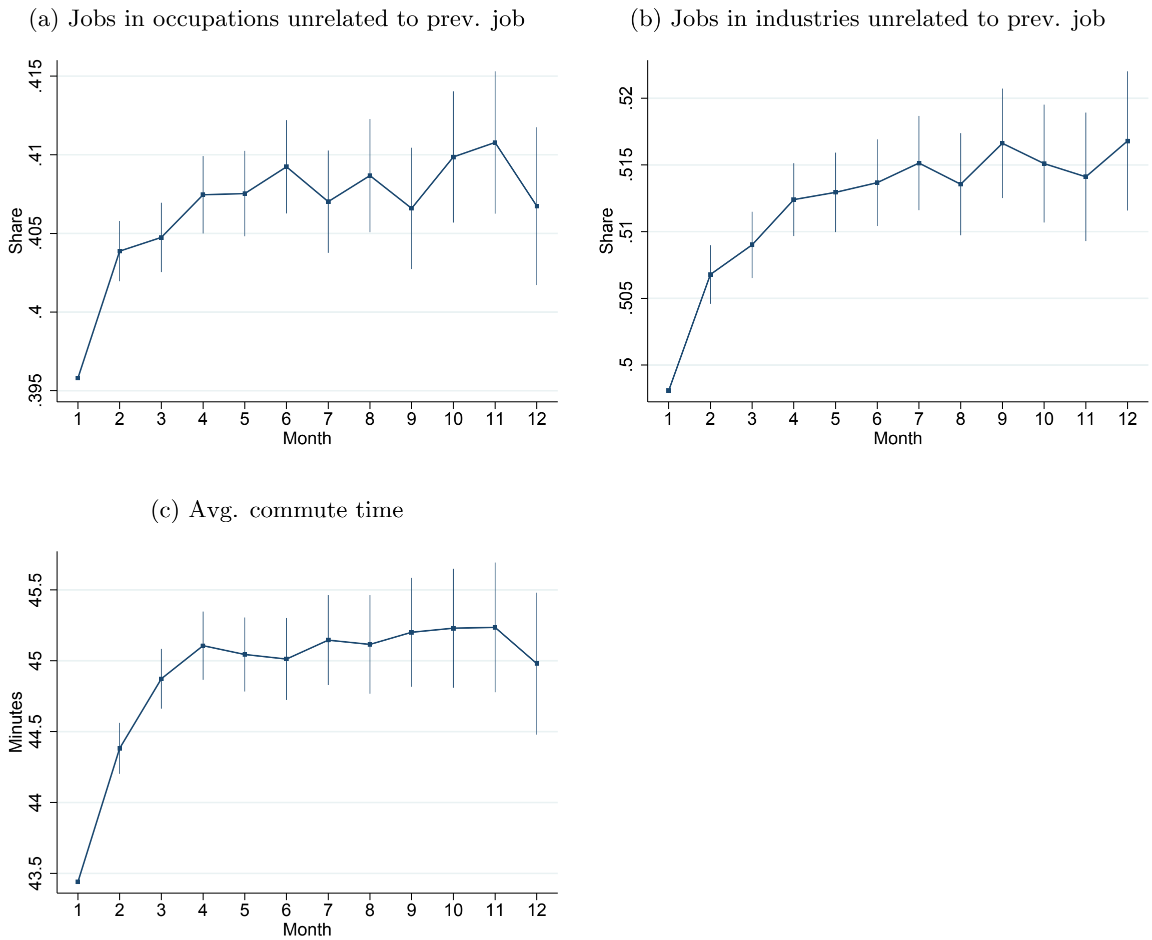
Stock-flow matching and vacancy supply effects

One possible explanation for why job seekers adjust the proximity of applied-for jobs at the beginning of the unemployment spell is vacancy supply effects, as in the stock-flow model of job search.

[26] The cost of switching occupation or industry may be a pure utility cost as the job seeker needs to exert effort to acquire new skills. Alternatively, it may also be a pecuniary cost if occupation or industry switching is associated with a lower wage offer because firms need to provide additional training and/or live with temporarily lower productivity. Note that none of the wage measures considered in the previous section take into account the workers previous industry and occupation. Wage losses from occupation/industry switching would thus be in addition to the changes in applied-for wages documented earlier.

[27] As described in detail in Section 2.3, relatedness is defined in terms of how similar tasks/skills are across occupations and how often transitions between the different industries occur in the data. Results are similar if we use other measures of relatedness, including simply the share of applications going to the UI recipients previous occupation. We also see qualitatively similar patterns when we consider higher levels of aggregation such as 2-digit occupations. See Appendix Section B.2 for further robustness checks using alternative measures of related occupations which are build using the Danish register data.

Figure 4: Changes in the proximity of applied-for jobs



Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. "Occupations unrelated to the prev. job" measure the share of applications sent to occupations which are not among the top 10 related occupations in the O*NET Related Occupations Matrix. "Industries unrelated to the prev. job" measure the share of applications sent to firms in industries which are not among the top 10 most related 3-digit industries. Commute time is the (estimated) commuting time from the municipality of the job seeker to the municipality of the applied-for firm. See Section 2.3 for further details on all these measures. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

The idea is that unemployed workers face a large stock of potential vacancies to apply to but this set quickly narrows as the unemployed worker exhausts the relevant options. This can exactly generate a pattern where workers are pushed to broaden their search over the first few months but not later on.[28] If the dynamics, and in particular the absence of dynamics after the initial

[28] In existing theoretical models, unemployed workers are assumed to exhaust all existing vacancies in the vacancy stock very early on in the UI spell. This implies that changes in job search behavior due to stock-flow effects should materialize early on in the spell. In practice, it may take some time before a newly unemployed worker has applied to all relevant jobs in the existing vacancy stock. This would imply a more gradual change in behavior over the few months of unemployment. Note that the most common predictions from the stock flow framework would concern the number of submitted applications which should experience a (large) decline as the unemployed exhaust the stock.

adjustment, in the proximity of applied-for jobs are driven by such vacancy supply effects we would expect large changes in the probability of applying for vacancies in the stock versus the flow around months 3 to 5. Further it is reasonable to expect applied for job characteristics to differ based on the duration of the vacancy if we expect vacancies in the stock to partly consist of vacancies which where previously found non-suitable (see also footnote 20).

To provide some evidence on this mechanism, we therefore need a measure of whether each applied-for job is a newly posted vacancy or a vacancy that has been posted for some time. Our data does not contain information on vacancy posting dates. For each applied-for job in our data, however, we can construct a rough proxy of whether the job is likely to be a newly posted vacancy by checking whether the data contain any other applications to the same firm over the preceding two weeks. If no other applications have gone to this firm over the preceding two weeks, we label the job as representing a new vacancy and record the vacancy opening week, otherwise we label it as an existing vacancy. Using this measure of new vacancies, Figure 5 examines how the share of applications going to new vacancies change over the unemployment spell based on our event study specification.[29] The overall direction of change is consistent with the stock-flow framework: Over time, the share of applications going to new vacancies tends to increase systematically. The size of this increase is small, however; at most one percentage point over the entire period (corresponding to around 3 percent of a standard deviation of the variation across spells in the first month). More importantly, the timing of the increase is inconsistent with the idea that stock-flow effects explain changes in the proximity of applied-for jobs over the first 4 months of unemployment only since the share of applications going to new jobs is essentially flat for the first 4 months and then slowly increases throughout the remaining spell. Although stock-flow effects may well be at work in the data, they are thus unlikely to explain the observed changes in the proximity of applied-for jobs. In Appendix Section B.3 Figure 17 we substantiate this further by showing the resulting dynamics profiles when we split applications by whether they are sent to "new" vacancies or not for wages, commuting and unrelated occupations. We generally see very similar adjustment patterns for "stock" and "flow" vacancies supporting our findings above.

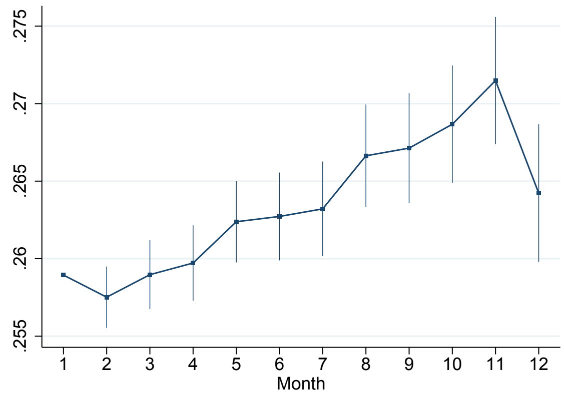
Learning during the unemployment spell

An alternative explanation for the observed changes in the proximity of applied-for jobs is learning. If unemployed workers are initially uncertain about the search environment but learn about job search over the first few months this can generate changes in search behavior that are concentrated only over these first months. For example, new unemployed workers may systematically have upwards biased beliefs about the likelihood of being hired when applying to local jobs and to jobs

Below we take a broader view and consider stock flow effects as driving dynamics in applied-for job characteristics.

[29]We define applying to a new vacancy as applying to a vacancy which has opened during the last two weeks. A brand new vacancy should obviously never have received any applications in the past. Since many firms periodically post new vacancies and hire, however, some time frame needs to be imposed on how far back to look for past applications to the same firm. In Appendix B.3 we show that our findings below are similar if we use other time frames than 2 weeks, or if distinguish "vacancies" by the combination of firm id and occupation codes at the 1-digit level.

Figure 5: Share of applications going to new vacancies



Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. The outcome variable is the share of applications which are submitted to firms which have not received other applications in our data in a 2-week time window preceding the submitted application. See also Section B.3 for exact definition, additional results and robustness checks. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

that are in occupations and industries related to their previous job. If gradual learning corrects these beliefs over the first few months of an unemployment spell, this could lead to rapid adjustments in the proximity of applied-for jobs over these same months.

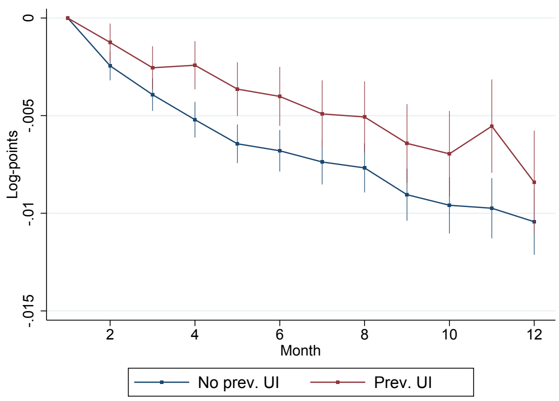
To provide some evidence on whether such learning effects may be at play, we split our sample of unemployed job seekers according to whether or not they have had any previous UI spells over the past 4 years.[30] If the adjustments in proximity of applied-for jobs reflect learning while unemployed, we would expect these adjustments to be less pronounced in the sample of job seekers who have previous experience with unemployed job search. Accordingly, Figure 6 estimates changes in the proximity of applied-for jobs separately for the group of job seekers with and without previous UI experience. The estimates are based on our event study specification. To facilitate comparisons between the two groups, however, we here normalize the estimated coefficient for month one to be zero for both groups.

Results do not indicate that the adjustments in the proximity of applied-for jobs are related to learning. Across the two groups, we see very similar patterns of adjustments in the average commute of applied-for jobs. Moreover, for the share of applied-for jobs in a related occupation we in fact see a stronger response for the group of job seekers with previous UI experience than for the group of job seekers without. This is the exact opposite of what we would expect if learning while unemployed was a main driver of the observed adjustments.

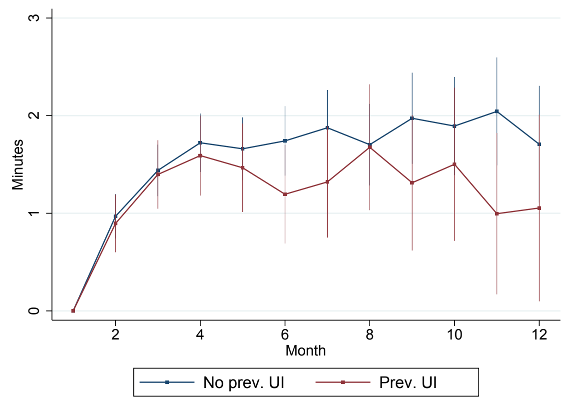
[30] As is evident from Table 1 a small share of our sample have received UI during the past year (in part also because we only sample spells with a full UI eligibility period, see Section 2), we therefore consider UI spells going further back in order to secure a reasonable sample size across subgroups.

Figure 6: Heterogeneous effects by past UI experience

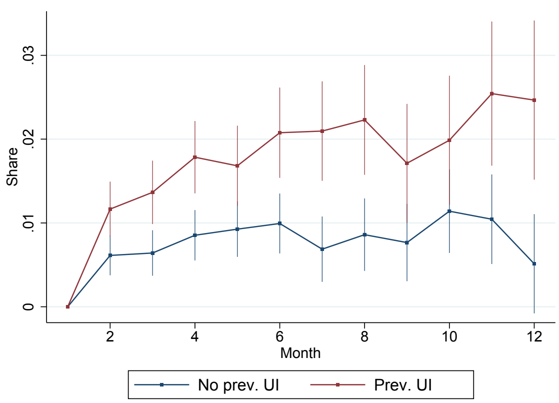
(a) Average applied-for wage



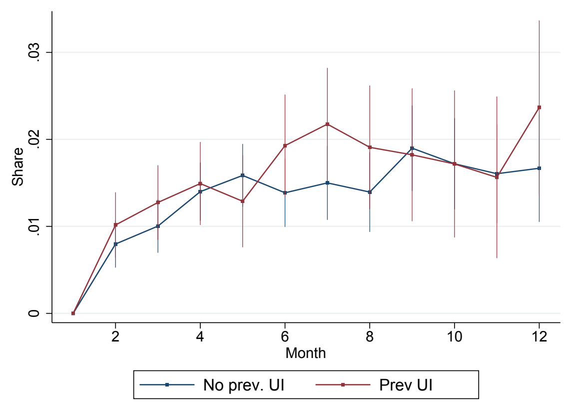
(b) Average applied-for commute



(c) Jobs in occupations unrelated to prev. job



(d) Jobs in industries unrelated to prev. job



Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 estimated separately for individuals who have (have not) been on UI for the past 4 years prior to entering our sample. Note that we have normalized τ_1 to 0 to facilitate comparisons across groups. The average typical wage is the average applied-for (predicted) wage in a given month, and the firm wage level is estimated firm fixed effects from an AKM regression, see Appendix Section A.3 for further details. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

Other explanations

Another way to reconcile the lack of adjustment is to note that theoretical models only predict that workers should target jobs with less attractive characteristics if this in fact increases their likelihood of getting a job. In relation to targeting jobs offering lower wages and fewer hours this is likely to be the case because wages and hours vertically differentiates jobs: assuming most workers prefer higher paying, full-time jobs, fewer unemployed workers should be applying to lower paying, part-time jobs so application to these jobs should be more likely to succeed. In contrast, geography, occupation and industry are more horizontal characteristics: A job that is unattractive to one job seeker because it is in a far away location will in fact be very attractive to all the job seekers who live in that location. The same is true for jobs in industries or occupations that are unrelated to some job seekers previous job but highly related to the previous job for other job seekers. Because of this applications to jobs in locations, occupation or industries that are unattractive to a given job seeker are not necessarily more likely to result in a hire, or alternatively there may simply be very limited room to adjust along these dimensions in order to increase hiring chances. As a result, job seekers face no incentive to apply for these less attractive locations, occupation and industries, regardless of whether the value of unemployment is decreasing over time. However, note that while this simple argument may reconcile the lack of adjustment in later months of the UI spell in Figures 4, it raises the question of why we *do* in fact see systematic adjustments over the first 3-4 months of the unemployment spell.

A final explanation for the observed changes in the proximity of applied-for jobs is that they are driven by the exact nature of job seekers' preferences over commuting, industry and occupation. For commuting for example, it may be natural to assume that the costs of commuting are highly convex. This can generate a pattern where job seekers initially adjust their search to apply for jobs farther away but where it eventually would be too costly (or even infeasible) to increase the search radius and consider even longer commutes.

For industry and occupation, we note that some form of reference-dependence could explain the observed patterns. If job seekers have reference-dependent preferences over occupation and industry they will start out their unemployment spell with a strong preference for working in industries or occupations that are similar to their previous job. This would lead them to apply more to these industries and occupations at the beginning of the spell. After spending time in unemployment rather than in their previous occupation/industry, however, their reference point will shift and their preferences for specific industries and occupations will weaken and eventually disappear.

Unfortunately, our data does not offer any direct evidence on the role played by preferences and/or commuting costs. Regarding the possibility of reference dependent preferences, however, we note that the timing of the adjustments in applications to related industry and occupation do align well with the timing of reference point adjustments estimated in Dellavigna et al. (2017).[31]

[31] Dellavigna et al. (2017) estimate that job seekers' reference points for income should have fully adjusted after 5-6 months in unemployment. As noted, our data shows that the change in the share of applications going to related industries and occupations occurs over the first 4 months but that essentially no changes occur from month 5 and onward.

5.5 Search and application channels

Next we examine changes in the methods and channels that UI recipients use to find and apply for jobs. This has received less attention in previous work on the dynamics of job search but is a potentially important margin on which UI recipients may adjust their search behavior.

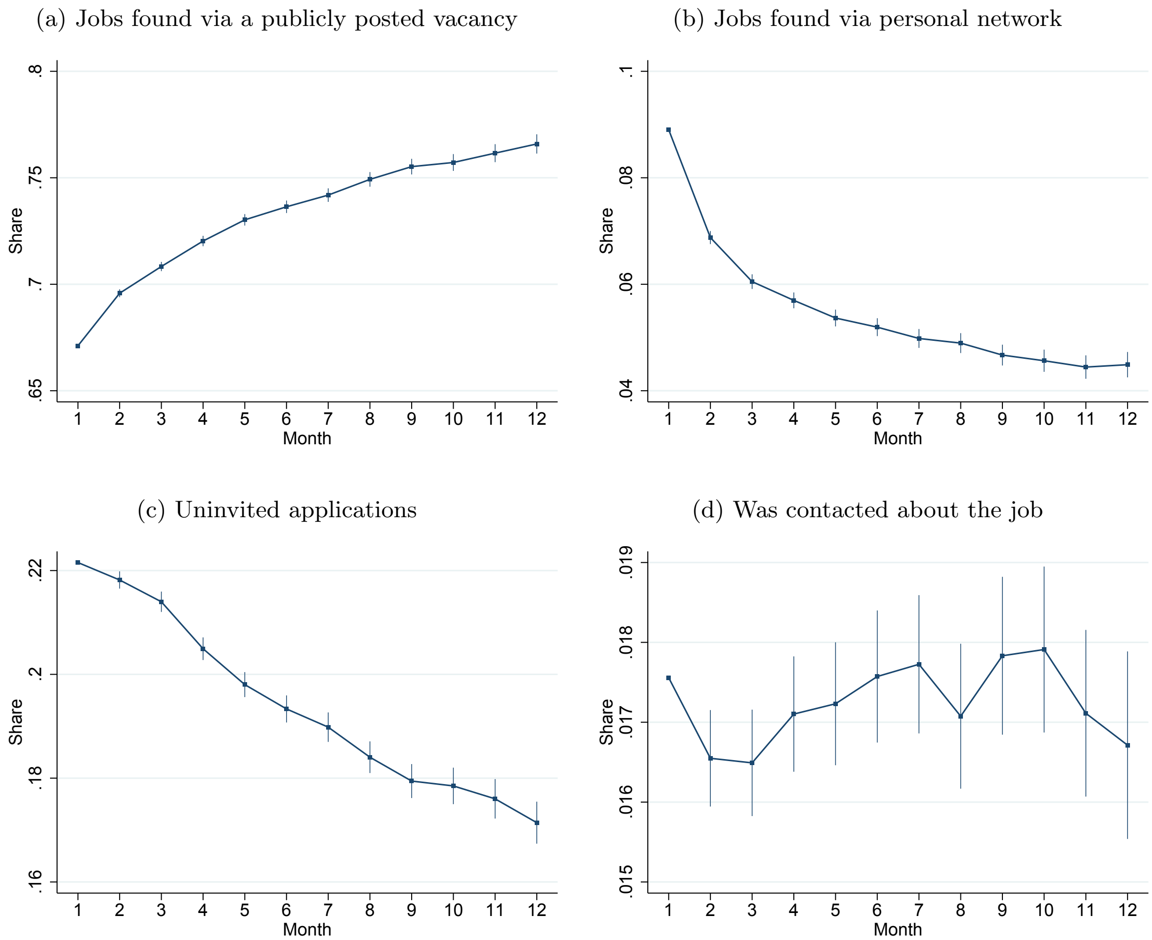
Our data on applied-for jobs contain information on how the UI recipient found the job, as well as on the method by which they applied for it. Based on this data and our event study specification, Figure 7 show changes over time in how job seekers find the jobs they apply for, while Figure 8 show changes in the channels through which job seekers are applying. Both figures show that job seekers are making systematic changes both in how they find and how they apply for jobs throughout the unemployment spell. Jobs found because of a formal posted vacancy for example make up a larger share over time. This comes in large part at the expense of job found through social networks which makes up a smaller share over time. One interpretation of this pattern is that job seekers initially prefer to search for jobs through their social network but that they eventually start to run out of relevant contacts in their network and shift their emphasis to job search through public sources. This interpretation is consistent with recent evidence that social networks can be an effective channel of job search and that getting hired through personal contacts may offer certain advantages (Caldwell and Harmon, 2020; Glitz and Vejlin, 2020). Additionally, situations where the job seekers sent uninvited application materials to a firm (i.e. without a posted vacancy and without having any personal connections) also make up a smaller share of applications over time.

In Figure 8, we examine the methods through which job seekers actually submit their application. Likewise, these change systematically over the course an unemployment spell. Most notably, the share of applications that are submitted through an online form increases markedly throughout the spell. This result has implications for much existing and ongoing research on job search. As discussed in the introduction, much of the existing work using micro data on job search is based on data on applications and search behavior from one or more online job platforms. The results in Figure 8 show that - at least in the Danish case - job seekers are very systematically selecting into these platforms over time, which raises a number of potential concerns with these types of data. For example, if the number of applications made on an online platform is used to infer total sent application for example, the gradual switch to online platforms shown in Figure 8 will bias the analysis towards finding an increase in applications over time, even if the total number of applications made online and offline is constant over time. For data sets that only include individuals who are active on the search platform and where the start of a job search spell must be inferred from activity on the platform, the pattern in Figure 8 also suggest that observed samples of "new searchers" can in fact be skewed towards individuals who have been unemployed for an extended period of time.

5.6 Heterogeneity by realized unemployment duration

In the results presented so far we have used person-by-spell fixed effects to flexibly absorb level differences in job search across individuals but have otherwise estimated a single time path of

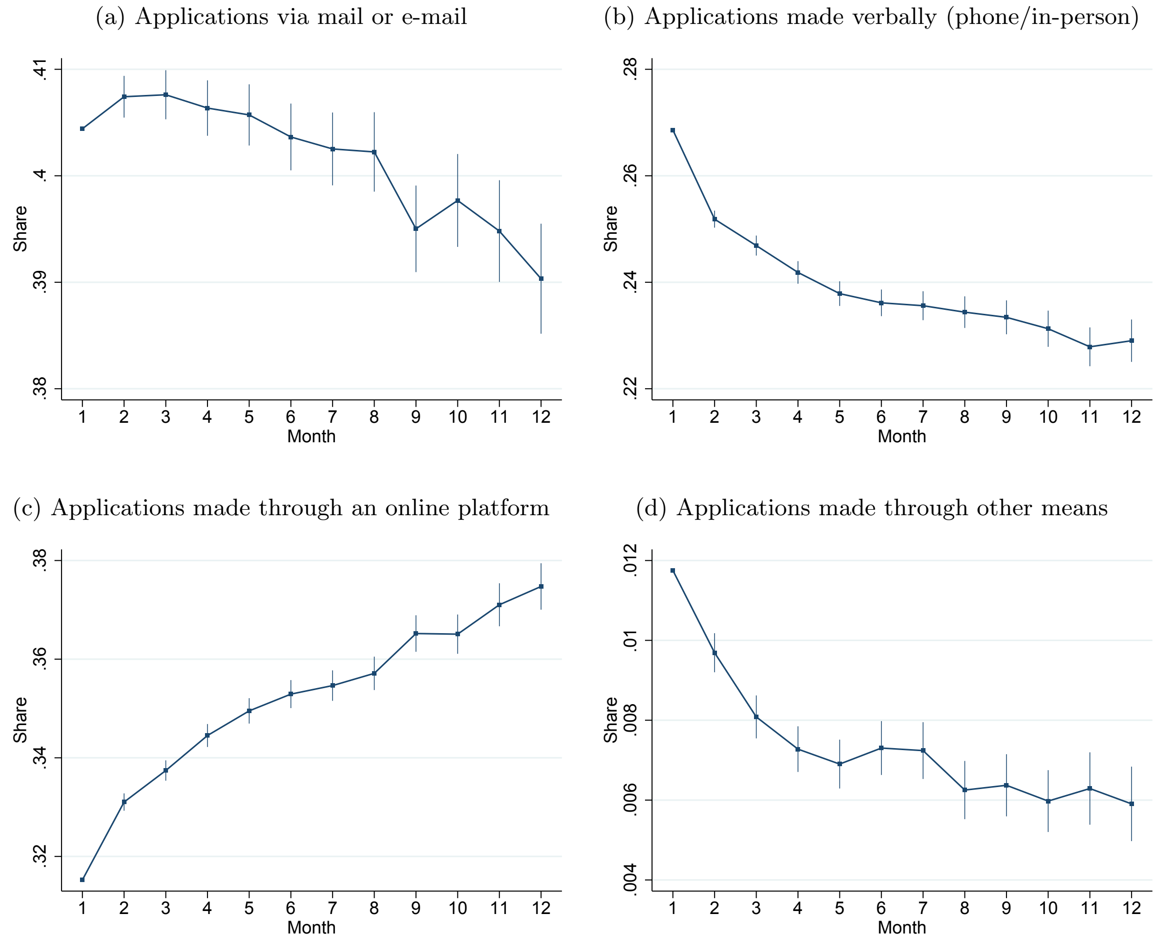
Figure 7: Changes in how applied-for jobs were found



Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. The outcome variable is the share of submitted applications which where found using a specific search method (answers to the question: "how did you find the job?"). Uninvited applications cover applications submitted without knowing whether a vacancy existed, see Section 2.3 for additional details on the different measures. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

changes in application behavior for all job seekers. In this section, we examine possible heterogeneity in how job search changes over time. Specifically we focus on differences in the time path of search behavior between individuals who experience unemployment spells of different lengths. We do this for two reasons: The first reason is the possibility that our previous results are affected by dynamic selection on changes. As discussed in Section 3, our use of person-by-spell fixed effects rules out dynamic selection in levels but not in changes. If individuals who tend to have longer unemployment durations systematically adjust their search behavior more or less over time than others, our fixed effects specification will show faster or slower changes in job search behavior over time even if the rate of change is constant for any given individual. The second reason is that potential heterogeneity between long- and short-term unemployed is often of direct interest for

Figure 8: Changes in application channels used to apply



(a) Applications via mail or e-mail

(b) Applications made verbally (phone/in-person)

(c) Applications made through an online platform

(d) Applications made through other means

Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. The outcome variable is the share of submitted applications which where found using a specific application method (answers to the question: "how did you apply for the job?"), see Section 2.3 for additional details. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

both policy work and research.

To examine heterogeneity by unemployment duration, we simply split our sample into 3 groups based on the realized total unemployment duration: individuals who exit from UI in months 5-8, individuals who exit UI in month 9-12 and individuals who remain on UI after one year. For each of these groups we then separately estimate our event study specifications using data on application behavior in the months before the group exits (e.g., for individuals who exit in months 5-8, we include data from months 1-4).[32] Figures 9 and 10 show the resulting estimated time paths for all

[32]We exclude data in the last months where the group exits unemployment to avoid generating mechanical changes in the time path of job search. For example, if targeting lower paying jobs increases the likelihood of exiting unemployment, individuals who lower their target wage in months 5 for idiosyncratic reasons will tend to be overrepresented among individuals who exit in months 5-8 thus generating a mechanical downward shift in target wages for this group.

our main measures of job search behavior. Note that to facilitate comparisons between the groups we have here normalized estimates to be zero in the first month for all groups and outcomes (we discuss level differences further below).

Looking across the panels of Figures 9 and 10, we see that the estimated time path of search behavior are remarkably similar across the three groups. Both in terms of the type of jobs they apply for, in terms of search methods and in terms of application channels, individuals with very different final unemployment durations appear to adjust their search behavior in the same way over time. The main exception to this are the measures of job proximity of applied-for jobs. For these measures - commuting time in particular - we see evidence that changes in search behavior are larger in magnitude for the group of long-term unemployed. At the same time, however, the qualitative pattern is very similar for all groups.

Importantly, the results in Figures 9 and 10 do not reflect that job search behavior is the same for the three groups of job seekers. In Appendix B.4, we show that there are large level differences in our measures of search behavior across individuals with different final unemployment durations. In other words, the data on search behavior data and unemployment duration is well described by the event study specification with time and individual fixed effects: Different individuals - who will eventually experience different total unemployment spell lengths - start out with very different patterns of job search behavior. Over time, however, they adjust their behavior in the same way.

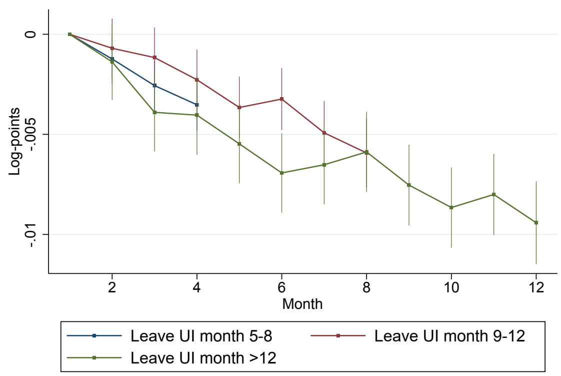
This finding has implications for both theoretical and empirical work. Theoretically, the findings suggest that the mechanisms that lead job seekers to change behavior over time - be it eventual benefit expiration, learning or something else - have very similar effects on groups of job seekers who experience different unemployment spell lengths. Empirically, the findings lend credence to the use of difference-in-difference research designs when examining job search behavior. Such research designs rest on an assumption that job search behavior between different groups evolve along similar time trends.

5.7 Additional robustness checks

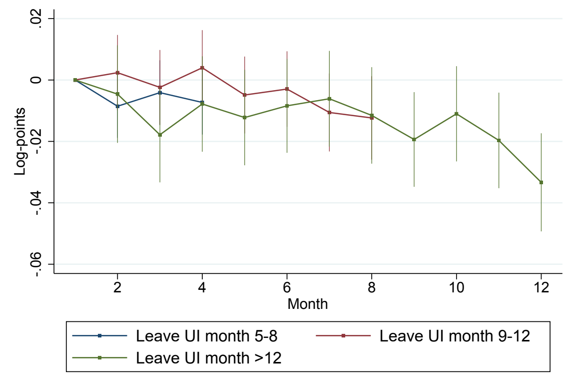
In Appendix C we subject the results above to a set of additional robustness checks, including the inclusion of calendar-month fixed effects to control for seasonality and winsorization of the the outcome variables at the 90^{th} and 10^{th} percentiles to check whether extreme observations are affecting the estimates. See also Appendix B for additional results and robustness for alternative measures of firm type, unrelated occupations and the importance of stock-flow effects in the data. None of these checks challenge our findings. Lastly, we note that in Section 2.4 and Appendix A.5 we have extensively discussed the coverage and validity of our data. In particular, we have shown that changes in application behavior over time in our data correlates with differences in final job outcomes. Hence the application dynamics documented above are highly relevant for the future allocation into jobs.

Figure 9: Changes in applied-for jobs by duration of unemployment

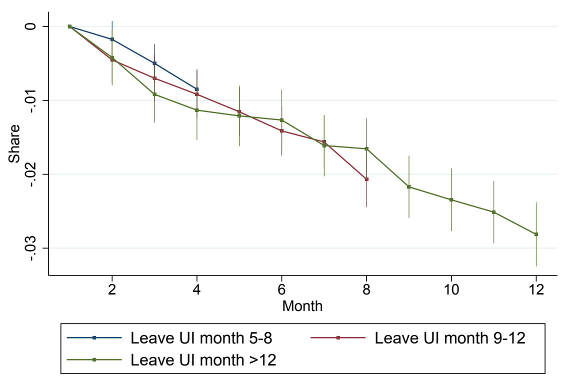
(a) Average typical wage of applied-for jobs



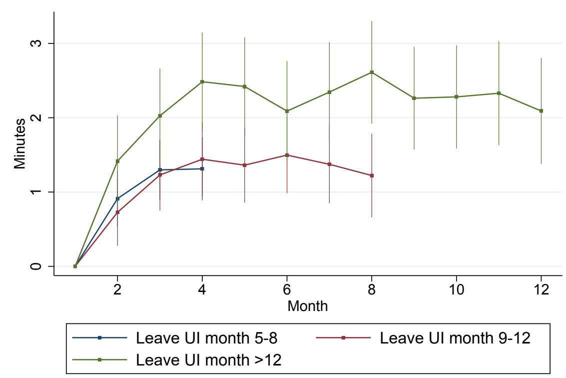
(b) Average firm wage level of applied-for jobs



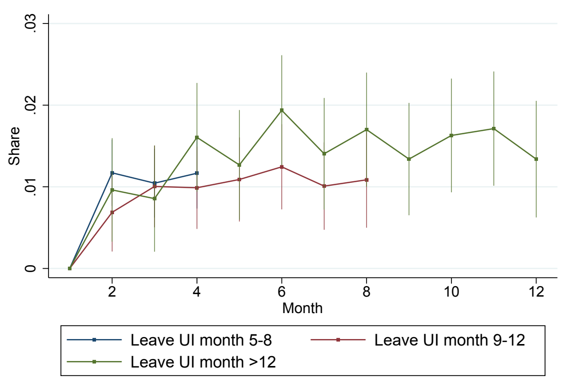
(c) Share of jobs that are full-time



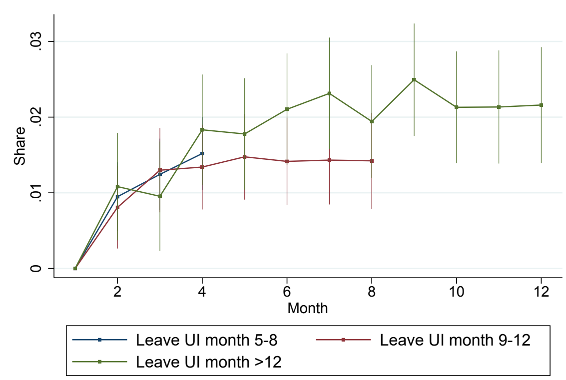
(d) Avg. commute time



(e) Jobs in occupations unrelated to prev. job

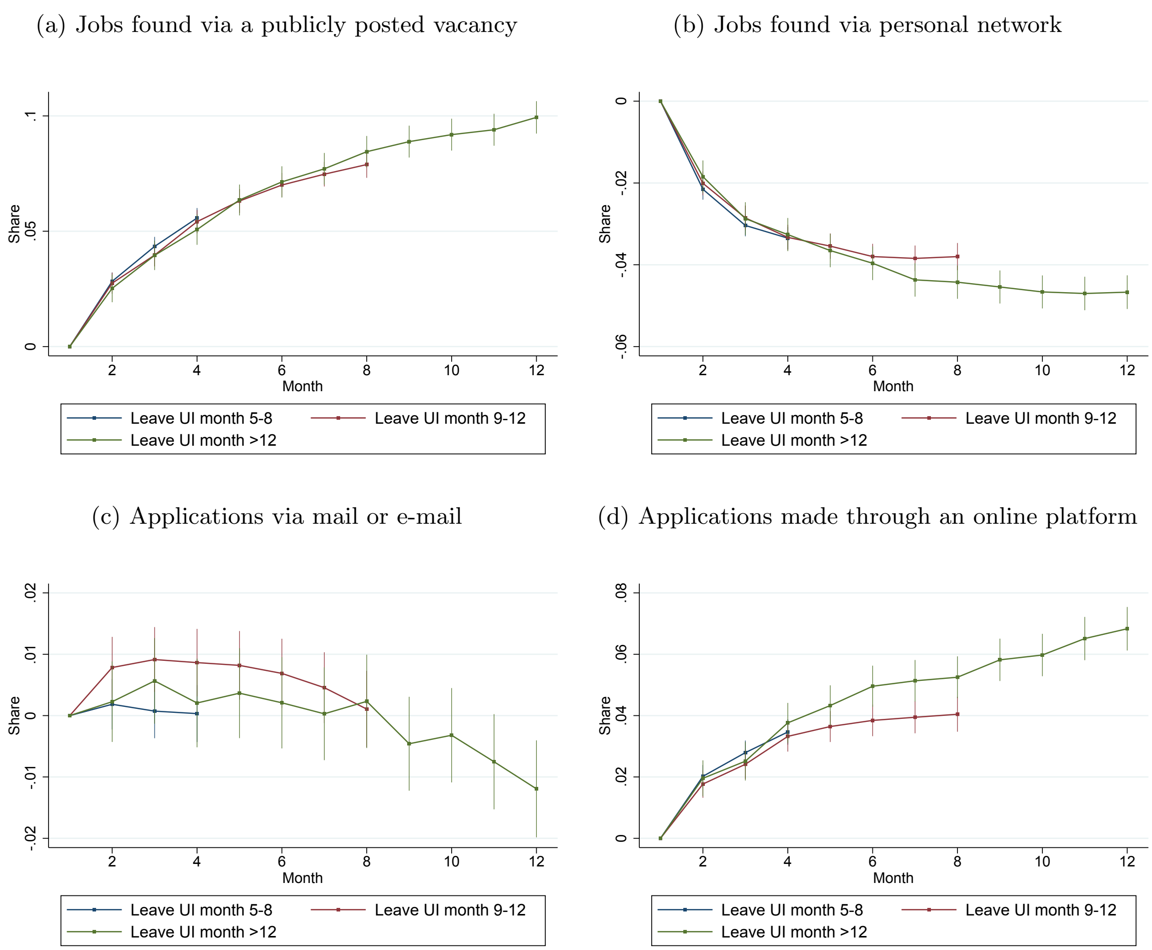


(f) Jobs in industries unrelated to prev. job



Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. To facilitate comparison across groups we have normalized τ_1 to 0. The average typical wage is the average applied-for (predicted) wage in a given month, and the firm wage level is estimated firm fixed effects from an AKM regression. "Occupations unrelated to the prev. job" measure the share of applications sent to occupations which are not among the top 10 related occupations in the O*NET Related Occupations Matrix. "Industries unrelated to the prev. job" measure the share of applications sent to firms in industries which are not among the top 10 most related 3-digit industries. Commute time is the (estimated) commuting time from the municipality of the job seeker to the municipality of the applied-for firm. See Section 2.3 for further details on all these measures. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

Figure 10: Changes in search channels and methods by duration of unemployment



Note: This Figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. The outcome variable is the share of submitted applications which where found using a specific application method or a specific search method (i.e. answers to the question: “how did you apply for the job?” and “how did you find the job?”), see Section 2.3 for additional details. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

6 Conclusion

In this paper we provide new evidence on how UI recipients change their job search behavior over time. We do this using novel linked administrative data on job search behavior and applied-for jobs for the universe of Danish UI recipients. We use this data to estimate event study regressions that uncover changes in job search behavior within individual unemployment spells.

We document five facts: First, we show that job seekers gradually apply for jobs that pay lower and lower wages throughout their unemployment spell. Similarly, workers also gradually apply to jobs that offer fewer and fewer hours. This is in line with standard theoretical predictions and empirical evidence regarding declining reservation wages over the unemployment spell.

Second, examining the distribution of applied-for wages, we find that decreases in applied-for wages over time occur throughout the distribution. This is most consistent with theoretical frameworks in which search decisions over time can be summarized by a declining target wage, as opposed to a declining reservation wage.

Third, we document that over the first few months of unemployment job seekers gradually broaden their job search by applying for jobs that are located further away from their home as well as jobs that are in industries and occupations which are less similar to their previous job. After these first few months, however job seekers do not broaden their job search any further in these dimensions. This finding sits awkwardly with predictions from the simplest version of canonical job search models. Potential explanations include stock-flow effects, learning or that job seekers have particular preferences over job proximity, such as reference-dependence and/or convex commuting costs. Based on auxiliary analysis we find no evidence that stock-effects or learning are primary explanations of this pattern, suggesting that the observed behavior has to do with the exact nature of preferences over commuting, occupation and industry.

Fourth, we show that over the course of an unemployment spell job seekers very systematically change the methods and channels through which they find and apply for jobs. In particular, job seekers continually increase the share of applications they submit via online platforms over the course of the unemployment spell. This systematic selection serves as a cautionary tale for studying job search using only data from online job platforms.

Finally, we document that changes in job search behavior over time are remarkably similar across groups with different unemployment duration. Theoretically this suggest that mechanisms that shape changes in job search behavior over time operate similarly across workers with different expected unemployment durations. Empirically, this last fact also validates the usefulness of difference-in-difference research designs when studying job search behavior. Differences-in-difference designs rely crucially on parallel trends between different groups.

References

Abowd, J. M., Kramarz, F., and Margolis, D. N. (1999). High wage workers and high wage firms. *Econometrica*, 67(2):251–333. 1, 2.3, 10, A.3

Acemoglu, D. (1995). Public Policy in a Model of Long-term Unemployment. *Economica*, 62(246):161. 19

Ahrens, A., Hansen, C. B., and Schaffer, M. E. (2019a). Lassopack: Model Selection and Prediction with Regularized Regression in Stata. *IZA Discussion Papers*. A.3

Ahrens, A., Hansen, C. B., and Schaffer, M. E. (2019b). LASSOPACK: Stata module for lasso, square-root lasso, elastic net, ridge, adaptive lasso estimation and cross-validation. *Statistical Software Components*. A.3

Allen, M. T., Waugh, G., Shaw, M., Tsacoumis, S., Rivkin, D., Lewis, P., Brendle, M., Craven, D., Gregory, C., and Connell, D. (2012). The Development and Evaluation of a New ONET Related Occupations Matrix Volume I : Report. *National Center for ONET Development*, I. 2.3, A.4

Altmann, S., Falk, A., Jäger, S., and Zimmermann, F. (2018). Learning about job search: A field experiment with job seekers in Germany. *Journal of Public Economics*, 164:33–49. 1, 21

Banfi, S., Choi, S., and Villena-Roldan, B. (2019a). Deconstructing Job Search Behavior. *Working Paper*. 1

Banfi, S., Choi, S., and Villena-Roldán, B. (2019b). Sorting On-line and On-time. *University of Bristol Discussion Paper 19/706*. 1

Banfi, S. and Villena-Roldán, B. (2019). Do high-wage jobs attract more applicants? Directed search evidence from the online labor market. *Journal of Labor Economics*, 37(3):715–746. 1

Belloni, A., Chernozhukov, V., and Hansen, C. (2014). Inference on treatment effects after selection among high-dimensional controls. *Review of Economic Studies*, 81(2):608–650. A.3

Belloni, A., Chernozhukov, V., Hansen, C., and Kozbur, D. (2016). Inference in High-Dimensional Panel Models With an Application to Gun Control. *Journal of Business and Economic Statistics*, 34(4):590–605. A.3

Belloni, A., Chernozhukov, V., and Hansen, C. B. (2011). Inference for high-dimensional sparse econometric models. *Advances in Economics and Econometrics: Tenth World Congress Volume 3, Econometrics*, pages 245–295. 10

Belot, M., Kircher, P., and Muller, P. (2019). Providing advice to jobseekers at low cost: An experimental study on online advice. *Review of Economic Studies*, 86(4):1411–1447. 1, 21

Brown, M., Flinn, C. J., and Schotter, A. (2011). Real-time search in the laboratory and the market. *American Economic Review*, 101(2):948–974. 22

Burdett, K. and Vishwanath, T. (1988). Declining reservation wages and learning. *Review of Economic Studies*, 55(4):655–666. 1, 4

Caldwell, S. and Harmon, N. (2020). Outside Options, Bargaining, and Wages: Evidence from Coworker Networks — Department of Economics. *Working Paper*. 5.5

Card, D., Chetty, R. A., and Weber, A. (2007). The spike at benefit exhaustion: Leaving the unemployment system or starting a new job? *American Economic Review*, 97(2):113–118. 2.2

Coles, M. and Petrongolo, B. (2008). A test between stock-flow matching and the random matching function approach. *International Economic Review*, 49(4):1113–1141. 4

Coles, M. G. and Smith, E. (1998). Marketplaces and Matching. *International Economic Review*, 39(1):239. 1, 4

Conlon, J., Pilossoph, L., Wiswall, M., and Zafar, B. (2018). Labor Market Search With Imperfect Information and Learning. *National Bureau of Economic Research Working Paper Series*. 4

Danish Economic Council (2014). Danish Economy, Autumn Report. 1, 2.2

Dellavigna, S., Heining, J., Schmieder, J. F., and Trenkle, S. (2020). Evidence on Job Search Models from a Survey of Unemployed Workers in Germany. 1, 2.2

Dellavigna, S., Lindner, A., Reizer, B., and Schmieder, J. F. (2017). Reference-dependent job search: Evidence from Hungary. *Quarterly Journal of Economics*, 132(4):1969–2018. 1, 4, 5.4, 31

Ebrahimy, E. and Shimer, R. (2010). Stock-flow matching. *Journal of Economic Theory*, 145(4):1325–1353. 1, 4

Engmann, T. S. and Weiskopf, S. V. (2019). Mænd får oftere job gennem netværk og bliver lidt oftere headhuntet end kvinder. *Statistics Denmark*. A.5

Faberman, R. J. and Kudlyak, M. (2019). The intensity of job search and search duration. *American Economic Journal: Macroeconomics*, 11(3):327–357. 1

Fluchtmann, J., Glenny, A. M., Harmon, N., and Maibom, J. (2019). Gender gaps in job search and hiring outcomes. *In: Essays on the Economics of Job Search (PhD dissertation).* 2, A.3, A.5

Ganong, P. and Noel, P. (2019). Consumer spending during unemployment: Positive and normative implications. *American Economic Review*, 109(7):2383–2424. 2.2

Gee, L. K. (2019). The more you know: Information effects on job application rates in a large field experiment. *Management Science*, 65(5):2077–2094. 1

Glitz, A. and Vejlin, R. (2020). Learning through coworker referrals. *Review of Economic Dynamics*. 5.5

Gonzalez, F. M. and Shi, S. (2010). An Equilibrium Theory of Learning, Search, and Wages. *Econometrica*, 78(2):509–537. 1, 4

Hall, R. E. and Mueller, A. I. (2018). Wage dispersion and search behavior: The importance of nonwage job values. *Journal of Political Economy*, 126(4). 4

Harmon, N. a. (2013). Are Workers Better Matched in Large Labor Markets ? *Working Paper*. 9

Krueger, A. B. and Mueller, A. (2011). Job search, emotional well-being, and job finding in a period of mass unemployment: Evidence from high-frequency longitudinal data. *Brookings Papers on Economic Activity*, (1):1–57. 1

Krueger, A. B. and Mueller, A. I. (2016). A contribution to the empirics of reservation wages. *American Economic Journal: Economic Policy*, 8(1):142–179. 1, 19

Kudlyak, M., Lkhagvasuren, D., and Sysuyev, R. (2013). Systematic Job Search: New Evidence from Individual Job Application Data. *SSRN Electronic Journal*. 1

Kuhn, P. and Shen, K. (2013). Gender discrimination in job ads: Evidence from china. *Quarterly Journal of Economics*, 128(1):287–336. 1

Le Barbanchon, T., Rathelot, R., and Roulet, A. (2019). Unemployment insurance and reservation wages: Evidence from administrative data. *Journal of Public Economics*, 171:1–17. 1

Le Barbanchon, T., Rathelot, R., and Roulet, A. (2020). Gender Differences in Job Search: Trading off commute against wage. *The Quarterly Journal of Economics*, 136(1):381–426. 1

Lentz, R. and Tranæs, T. (2005). Job search and savings: Wealth effects and duration dependence. *Journal of Labor Economics*, 23(3):467–489. 19

Lichter, A. and Schiprowski, A. (2021). Benefit duration, job search behavior and re-employment. *Journal of Public Economics*, 193:104326. 1

Lockwood, B. (1991). Information Externalities in the Labour Market and the Duration of Unemployment. *The Review of Economic Studies*, 58(4):733. 19

Machin, S. and Manning, A. (1999). Chapter 47 The causes and consequences of longterm unemployment in Europe. 19

Mahlstedt, R., Sebald, A., Cairo, S., and Altmann, S. (2019). Complexity and the Effectiveness of Public Policy. *Manuscript*. A.5

Marinescu, I. and Rathelot, R. (2018). Mismatch unemployment and the geography of job search. *American Economic Journal: Macroeconomics*, 10(3):42–70. 1

Marinescu, I. E. and Skandalis, D. (2021). Unemployment Insurance and Job Search Behavior. *Forthcomming in Quarterly Journal of Economics*. 1, 2.2, 5.1

McCall, J. J. (1970). Economics of Information and Job Search. *The Quarterly Journal of Economics*, 84(1):113. 1, 4

Moen, E. R. (1997). Competitive search equilibrium. *Journal of Political Economy*, 105(2):385–411. 4

Mortensen, D. T. (1977). Unemployment Insurance and Job Search Decisions. *Industrial and Labor Relations Review*, 30(4):505. 1, 4

Mortensen, D. T. (1986). Job search and labor market analysis. *Handbook of Labor Economics*, 2(C):849–919. 19

Mueller, A. I., Spinnewijn, J., and Topa, G. (2021). Job Seekers' perceptions and employment prospects: Heterogeneity, duration dependence, and bias. *American Economic Review*, 111(1):324–363. 1, 4

Neffke, F. and Henning, M. (2013). Skill relatedness and firm diversification. *Strategic Management Journal*, 34(3):297–316. 2.3, A.4

Nekoei, A. and Weber, A. (2017). Does extending unemployment benefits improve job quality? *American Economic Review*, 107(2):527–561. 1, 4

Pissarides, C. A. (1992). Loss of Skill During Unemployment and the Persistence of Employment Shocks. *The Quarterly Journal of Economics*, 107(4):1371–1391. 19

Rogerson, R., Shimer, R., and Wright, R. (2005). Search-theoretic models of the labor market: A survey. *Journal of Economic Literature*, 43(4):959–988. 4, 4

Skandalis, D. (2018). Breaking News: The Role of Information in Job Search and Matching. *Mimeo*. 1

Spinnewijn, J. (2015). Unemployed but optimistic: Optimal insurance design with biased beliefs. *Journal of the European Economic Association*, 13(1):130–167. 1, 4

Van Den Berg, G. J. (1990). Nonstationarity in job search theory. *Review of Economic Studies*, 57(2):255–277. 1, 4

Wright, R., Kircher, P., Julien, B., and Guerrieri, V. (2020). Directed search: A guided tour. *Journal of Economic Literature*, 23884:1–120. 4

A Data construction and validity checks

A.1 Final sample statistics

Our baseline sample is constructed from administrative data on UI payments and consists of all individuals of Danish origin entering new unemployment spells between September 2015 to September 2017. We link all UI spells to 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,[33] hours, wages, industry[34] and firm identifiers for the employing firms.[35] We use the DREAM register data base (see footnote 35) to identify new UI spells with full UI eligibility. In addition we only include spells with at least 4 weeks of consecutive UI benefit payments and no payout in the 4 weeks prior to spell start. As special UI rules may apply for immigrants, we exclude them from the sample.

As explained in Section 2.2 we do not consider spells with less than 8 weeks of duration and we require at least one job log - the latest possible entry in our data is thus June 2017. We follow these spells for up to the first 12 months and we disregard the last 4 weeks of applications before individuals enter the job they eventually are hired into. Table 2 shows the effect of these additional sample restrictions in terms of the reduction in sample size.

Given these restrictions we arrive at our final analysis sample consisting of 136.291 individuals with 141,551 UI spells and 4,3 million applications. In Figure 11 we provide some descriptives on our UI spells by plotting the survivor function and the distribution our spells starting points. The fact that we have spells starting at different points in time is what allows us to control for time effects (calendar quarter fixed effects) in a robustness check of our main results (see Section C.1). In Figure 12 we show histograms of the number of logged job applications at the monthly or weekly level of aggregation. Clearly the distribution of the number of logged applications centers around the semi-official registration incentives set forward by the UI funds, see Sections 2.4 and 2.1.

In Table 3 we describe our sample in terms of average applied-for job characteristics across months in our final sample and across the different unemployment duration groups similar to Table 1. As explained above there are some applications which we cannot link to firms in the registers

[33]DISCO is the Danish equivalence of the standard international ISCO classifications. e.g., medical doctors have the minor code 221 which relates to the sub-major group of health professionals with code 22, which is part of the major group of professionals with number 2. Similar classifications apply for the industries. The first four digits of DISCO are identical to the international ISCO classification, details are here https://www.dst.dk/da/Statistik/dokumentation/nomenklaturer/disco-08

[34]We use the NACE Rev. 2 nomenclature to classify industries. Information in the aggregation of the NACE Rev. 2 nomenclature for industries can be found here: https://www.dst.dk/klassifikationsbilag/8cf95f88-8153-43b5-a82a-fa89adf6f214 (pp. 463-477)

[35]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 data on jobs, paid hours of work and earnings for the universe of employed individuals. 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/Forskningsservice).

or where information on previous occupation or occupations applied for are not available. We treat these observations as missing when constructing the monthly averages. In Table 3 we report summary statistics on this dimension also by e.g., the share of observations (months) where we have no information on e.g., typical wages.

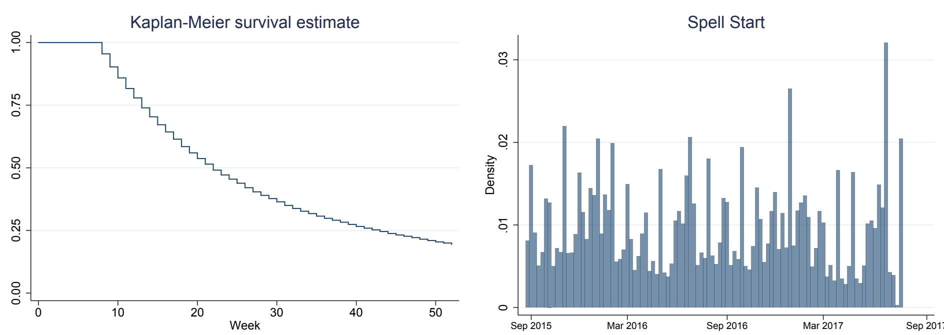
Table 2: Sample selection

	Individuals	**Spells**	**Joblogs**
Initial sample with full UI entitlement	181,113	191,840	5,647,234
- Min. 1 logged application	175,318	184,599	5,647,234
- At least 2 months (8 weeks) in UI	137,309	142,652	5,358,257
- Censoring last 4 weeks of applications	136,291	141,551	4,636,217
- Remove logs after 12 months in UI	136,291	141,551	4,335,245
Final sample	**136,291**	**141,551**	**4,335,245**

Notes: The Table shows the amount of individuals, respective unemployment spells as well as number of joblogs for multiple stages in our sample selection process. The last row indicates the final sample used for the main analysis of this paper.

Figure 11: Distributions

(a) Survivor curve and week of spell start



Note: Left panel of (a) plots the Kaplan-Meier estimate of the survival curve for our sample (until employment). The right panel (b) displays the distribution of the start week for each respective spell in our final sample.

A.2 Matching algorithm

Below we provide additional details in how we process the job log entries in order to obtain links to the standard Danish registers available at Statistics Denmark. 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 target files and correct basic spelling mistakes.

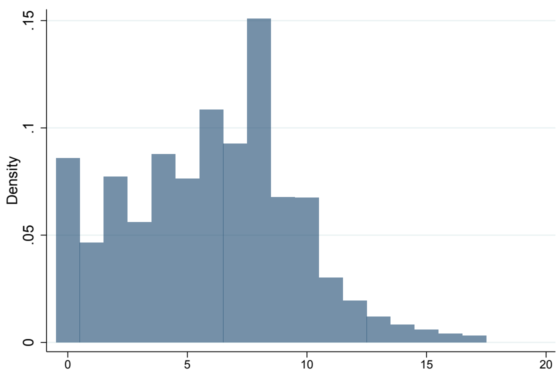
Table 3: Summary statistics across monthly averages - Measures of applied-for job characteristics

		Unemployment Duration				
	Total	<3 Months	3-6 Months	6-12 Months	>12 Months	Right censor
Typical Wage (log)	5.18	5.18	5.18	5.18	5.18	5.20
	(0.12)	*(0.12)*	*(0.12)*	*(0.11)*	*(0.12)*	*(0.12)*
AKM firm fixed effect	0.01	0.01	0.00	0.01	0.01	0.02
	(0.43)	*(0.55)*	*(0.42)*	*(0.33)*	*(0.29)*	*(0.40)*
Fulltime	0.89	0.89	0.89	0.88	0.89	0.88
	(0.19)	*(0.21)*	*(0.19)*	*(0.18)*	*(0.17)*	*(0.19)*
Commute time (min)	43.80	43.27	44.15	44.64	44.76	42.47
	(29.40)	*(31.44)*	*(29.28)*	*(28.14)*	*(28.16)*	*(28.57)*
Unrelated occupation (0/1)	0.40	0.36	0.38	0.42	0.46	0.43
	(0.36)	*(0.38)*	*(0.36)*	*(0.35)*	*(0.35)*	*(0.37)*
Unrelated industry (0/1)	0.49	0.45	0.49	0.51	0.53	0.50
	(0.34)	*(0.38)*	*(0.34)*	*(0.31)*	*(0.29)*	*(0.33)*
No information on typical wages	0.12	0.15	0.13	0.11	0.08	0.07
	(0.18)	*(0.23)*	*(0.19)*	*(0.16)*	*(0.13)*	*(0.14)*
No information on firm effects	0.12	0.15	0.14	0.11	0.08	0.07
	(0.19)	*(0.23)*	*(0.19)*	*(0.16)*	*(0.13)*	*(0.14)*
No information on hours	0.08	0.11	0.09	0.08	0.06	0.05
	(0.16)	*(0.20)*	*(0.16)*	*(0.14)*	*(0.11)*	*(0.12)*
No information on commuting time	0.09	0.12	0.11	0.09	0.06	0.05
	(0.17)	*(0.21)*	*(0.17)*	*(0.14)*	*(0.12)*	*(0.13)*
No information on unrelated occupations	0.11	0.13	0.12	0.11	0.07	0.06
	(0.18)	*(0.22)*	*(0.18)*	*(0.16)*	*(0.14)*	*(0.14)*
Average number of logs per individual	31.36	9.40	23.08	51.77	84.03	30.25
	(27.84)	*(5.40)*	*(12.77)*	*(23.76)*	*(27.99)*	*(24.65)*
Number of logs in sample	4,335,245	299393	1078389	1221097	1022101	714265

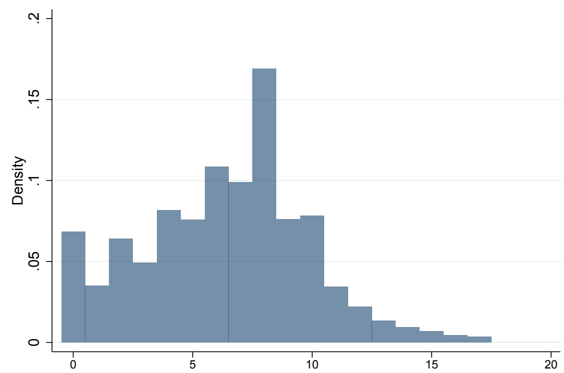
Notes: The table displays the average characteristics of the search portfolio over the first 52 weeks in unemployment. Averages are calculated across the monthly averages/measures of applied-for job characteristics. Standard deviation in parentheses.

Figure 12: Histograms over number of applications

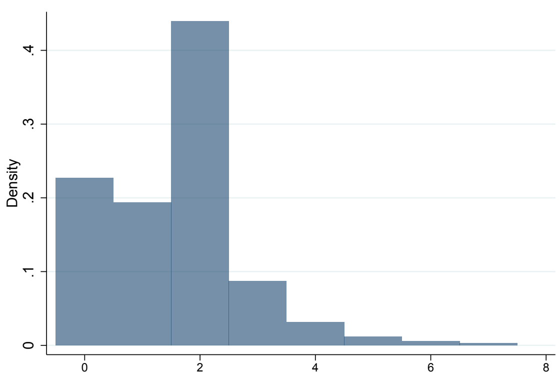
(a) Monthly level



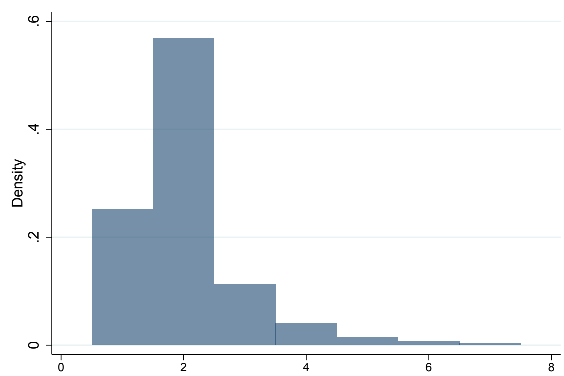
(b) Monthly level - excluding first month on UI



(c) Weekly level



(d) Weekly level - conditional on logging



Note: The figure shows histograms of the distribution of the number of logged applications at the monthly or weekly level of aggregation. We further show a version where we exclude submitted applications in the first month of UI and a version where we only include weeks where the individual logged any applications. Due to Statistics Denmark's confidentiality rules we have excluded observations above the 99th percentile in all figures (18 applications at the monthly level).

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, we identify occurrences of the DISCO occupations in the reported job titles. We only consider 1:1 matches (43.4 percent), 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 (i.e., 'social og sundhedshjælper', Danish for social and health-care workers, are most often reported as 'sosu-hjælper'). This adds about 27.2 percent to the matches. Finally, we also use some fuzzy matching techniques on the remaining unmatched observations to circumvent misspellings in the joblog 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 percent. Overall, we can thus map 81.5 percent of the applications to a DISCO group.

In 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 developed a matching procedure which matches this information to the official firm registers recording all Danish firms (CVR-register). We can then use these links to identify firms in the registers at Statistics Denmark. 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 percent of the applications in Joblog. We further add the sub-string matches (i.e., where we have a perfect match for a subset of the firm name string) and if several exists we choose the one which is spatially the closest to the reported firm address. This add 13.9 percent to the matches. 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 are Bi-gram Similscore, Token, TokenSound from matchit and the compget and speedist functions in SAS). This adds further 6.2 percent to the matches, so we end up with an overall firm match of 86.4 percent.

A.3 Information on applied-for wages

Our data does not contain wages associated with the applied-for job as these are not commonly reported for vacancies in Denmark. Instead we construct two different proxies for the wage associated with a given applied-for job leveraging that we have access to the rich Danish register data.

First, we use estimate from an AKM model (Abowd et al. (1999)) and use the implied firm fixed effects as a measure of the firm type. We take advantage of the rich administrative data on the whole Danish working population, in particular the BFL data (see footnote 35) set covering all salaries in Denmark, to construct a matched employer-employee panel from 2008 to 2015 with 290,108 (connected) firms. We include year-month fixed effects in the AKM wage regression to absorb any aggregate time trends. Post-estimation we remove (worker-weighted) industry specific means from the estimated firm effects. Second, for each application in our data we estimate the typical wage for this position based on detailed observable characteristics of the job we observe and the re-employment wages individuals are paid upon entering a new job. In constructing this measure, we use occupation and industry codes of different levels as well as a AKM firm fixed effects. Further, we also include various interactions between all of these measures and use a LASSO regression for model selection (Belloni et al., 2014). 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 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 over-fitting. 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 the estimation . The estimation was conducted using the LASSOPACK implementation of Ahrens et al. (2019a,b). Out of the 10,407 baseline explanatory variables,[36] 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. Further details on the estimation procedure for typical wages are available in Fluchtmann et al. (2019). These typical wages are then a measure of the wage a person would typically be payed in the applied jobs.

A.4 Building occupation and industry relatedness measures

In terms of occupations, many jobs might have tasks and skill requirements that are easily transferable from the individual's previous jobs. To get a sense on whether applications go to occupations that are highly related to the previous position, we use the latest version of the O*NET Related Occupations Matrix. This data contains, for each occupation, the top 10 related occupations in terms of skills and experience (Allen et al., 2012). We map this matrix to our 3-digit DISCO codes and use this to define the set of related occupations.[37] Based on our measure of related occupations we then say that an occupation is *unrelated* to the job seekers past occupation if it is not in the set of related occupations. To test the robustness of our findings based on this measure we also construct an alternative measure as explained in Appendix B.2.

In order to get a similar measure for skill relatedness across industries, we use data from Neffke and Henning (2013). This data contains skill-relatedness estimates across NACE Rev. 2 industries based on labor flows among industries in the Swedish economy. We select the top 10 most related 3-digit industries to roughly resemble the occupation measure and define this set as the group of related industries and continue as above by constructing our final measure of unrelated industries.

A.5 Joblog coverage and validity

In this section we complement the discussion in Section 2.4 with additional details regarding the coverage and validity of joblog data. Note that some of the analysis below is adapted from Fluchtmann et al. (2019), in which we use the same data sources to study gender differences in job search.

[36] 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, a dummy for whether we were able to obtain an estimated AKM fixed effect for the employing firm and the within-industry-demeaned AKM firm fixed effect when this is available. In addition we include all pairwise interactions between these variables.

[37] The O*NET Related Occupations Matrix is based on US data. In this classification, plumbers are e.g., coded to be highly related to the occupation of heating and air condition mechanics. The matrix defines related occupations in terms of tasks and requirements using a classification of occupations that we can map to the Danish 3-digit DISCO codes after appropriate translation. Some of these codes are more detailed than the DISCO codes. Sometimes, we therefore get over 10 related occupations for a single 3-digit DISCO code.

Table 4: Survey question "Which of these statements best describes your use of Joblog?"

Answer:	Share of respondents:
Fulfill requirements, often applied to more jobs	36%
Fulfill requirements, rarely applied to more jobs	21%
Always register all applied-for jobs	41%
Never register applications	1%
Number of respondents	1236

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).

First, Table 4 and 5 present results from a survey conducted among Danish UI recipients by Mahlstedt et al. (2019) in March 2018.[38] Table 4 reports survey answers about how individuals log applications in Joblog. 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.

To get a sense of how many applications may be missed by the Joblog data, Table 5 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 respondents on average say they 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.[39]

Next, for the sub-sample of UI recipients who eventually find a job, we analyze how the Joblog data on applied jobs applied for relate to the actual hiring outcomes we observe in the data. If

[38]We thank the authors for making this data available.

[39]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).

Table 5: Self-reported and registered applications in the previous month

	Mean
Survey answers	
# of applied-for jobs	11.5
# of applied-for jobs not registered	2.4
Joblog data	
# of applied-for jobs	8.0

Notes: The to part of the table shows the reported number of job applications sent over the last month and the reported number of these jobs applications that were not registered in Joblog based on the 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.

differences in applied-for job characteristics across individuals also correlates with differences in the type of employment the unemployed eventually enter, this would of course be a strong signal that the Joblog data accurately capture actual job search behavior. We benchmark the predictive value of the Joblog data against a known strong predictor of job outcomes: the characteristics of the UI recipient's previous job. In Table 6 we show how the characteristics of applied-for jobs 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 the previous job or the share of applications going to each occupation. 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 6, we see that models that predict job outcomes only using data on applied-for jobs perform quite similarly to models that instead use prior job characteristics. The data on applied-for jobs does 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, data on applied-for jobs 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 data on applied-for jobs (columns (3), (6), (9) and (12)), we see that the characteristics of applied-for jobs remains highly predictive even after prior job characteristics have been conditioned on; adding the applied-for job variables alongside prior job characteristics always leads to sizable increases in the (*pseudo-*)R^2 relative to models that only use prior job characteristics. Moreover the applied-for job variables are always highly statistically significant in the combined models. Overall, we conclude that the Joblog data is highly predictive of later job outcomes.

Next, we examine how often we are able to trace a new hire back to a job application that is contained in 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 5 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 A.2, $s = 0.86$ in our data matching procedure. Finally, to provide bounds on the share of hires that stem from a job application, j, we rely from Statistics Denmark's official survey *Arbejdskraftsundersøgelsen* on how unemployed Danes report landing their first job out of unemployment (Engmann and Weiskopf (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).[40] 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 $j = 0.73$.[41]

[40]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.

[41]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 $0.89 \cdot 0.80 \cdot 0.86 = 0.61$.

Table 6: 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)
Explanatory variables:												
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	90	90	171	72	72	136	2	2	3	2	2	3
(pseudo-)R-squared	0.278	0.257	0.361	0.338	0.423	0.516	0.044	0.079	0.106	0.257	0.287	0.387
p-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: 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. 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.

Plugging in these values, we 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 $0.73 \cdot 0.68 \cdot 0.86 = 0.43$ 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.

We also focus on the measured changes in job search behavior over time. Since these changes are at the heart of our analysis, it is important that they reflect meaningful changes in behavior rather than some form of dynamic measurement error or changes in selective reporting of applied-for jobs over time. To check this, we examine whether changes in job search behavior in our data are predictive of later job outcomes. Specifically, we focus on the subset of our analysis sample that remain unemployed for at least 3 months but eventually find a job. For this sample, we then construct two different types of job search measures: the first type measures characteristics of applied-for jobs only in the last two months of the unemployment spell, the second type measures the characteristics of applied-for jobs in all months prior to the last two. After conditioning on the latter measure, we then ask whether the former measure predicts the type of job an individual ends up in. In other words, we check if job outcomes tend to be different between two individuals who have the same search behavior up until 2 months prior to job finding but who later diverge in behavior. If they do, this suggests that changes in measured job search behavior over time indeed capture meaningful changes in behavior.

Table 7 shows the results of this type of exercise. In Column (1), we regress the typical wage of the new job on the average applied-for wage of the last two months, while controlling for the average applied-for wage in all months prior to this. In Columns (2)-(11) we instead consider the industry of the new job. Here we regress a dummy variable for ending up in a particular 1-digit industry on the share of applications going to that industry over the last two months, while controlling for the share of applications going to the industry over all months prior to this. We see that job search behavior over the last two months of a spell is a strong predictor of the type of job that a UI recipient finds, even after conditioning on past job search behavior. For wages, a 1 percent increase in the average applied-for wage of the last two months is associated with a 0.32 percent increase in the wage paid in the new job. For industries, we see that a one percentage point increase in the share of applications going to a particular industry over the last two months is associated with an increase in the probability of ending up in this industry of between 0.31 and 0.46 percentage points. These results confirm that measured changes in application behavior in our data is meaningful; individuals who show different changes in behavior over time also tend to have very different job outcomes.

Finally, we note that the coefficient on search behavior in previous months in Table 7 shows that past search behavior also predicts later job outcomes. This is unsurprising. For some new jobs the gap between applying for the job and starting can be larger than 2 months, implying a causal link between past application behavior and later outcomes. Additionally, individuals who apply heavily to a particular type of job early in the spell are likely to stand out on other dimensions that make them likely to end up in this type of job also at a later date.

Table 7: Job outcomes and changes in applied-for job characteristics

		Industry of new job:									
	Log typical wage in new job	Agricult., forestry and fish.	Manufact., materials and util.	Con-struction	Retail and transport.	Information and communic.	Finance and insurance	Real estate and rental	Business services	Public adm., education and health	Culture and leisure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Avg. log wage of applied-for jobs, last 2 months	0.322*** (0.124)										
Avg. log wage of applied-for jobs, all other months	0.286*** (0.119)										
Share of applied-for jobs in this industry, last 2 months		0.459*** (0.035)	0.356*** (0.012)	0.380*** (0.019)	0.346*** (0.009)	0.439*** (0.020)	0.328*** (0.022)	0.308*** (0.024)	0.332*** (0.014)	0.446*** (0.010)	0.378*** (0.019)
Share of applied-for jobs in this industry, all other months		0.389*** (0.124)	0.312*** (0.012)	0.379*** (0.019)	0.302*** (0.009)	0.394*** (0.020)	0.319*** (0.023)	0.233*** (0.024)	0.299*** (0.014)	0.416*** (0.010)	0.367*** (0.020)
Observations	46,502	46,238	46,238	46,238	46,238	46,238	46,238	46,238	46,238	46,238	46,238

Notes: Each column of the table show OLS estimates from a different regression, estimated on the subset of the analysis sample that end their UI spell by finding a new job. In Column (1) the outcome variable is the log typical wage for the new job and the right hand side variables are the average log wage of applied-for jobs over the last 2 months of the UI spell and the average log wage of applied-for jobs over all other months of the spell. In each of the columns (2)-(11) the outcome variable is an indicator for whether the new job is in a different 1-digit industry and the right hand side variables are the share of applied-for jobs in this industry over the last 2 months of the spell and the share of applied-for jobs in this industry over all other months. Variation in observation numbers are caused by missing industry information on new jobs. Standard errors clustered at the individual level are in parenthesis. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$.

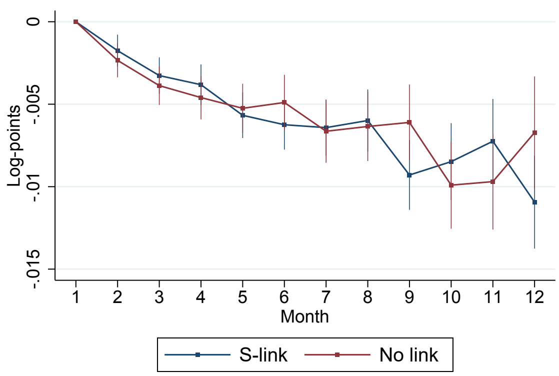
A.6 Robustness check: Requiring a successful link between application and new hire

In Section 2.4 we carry out a check of the representativenes of our data by using the possibility of linking eventual hiring outcomes with a specific past application in our job search data. In this section, we instead use this for a simple robustness check by redoing key parts of our our main analysis only for those individuals in our sample who eventually find a job at some firm and where we are able to link this job outcome to a past application from the individual to the firm. The motivation is that if some individuals are less diligent or truthful in registering applied-for jobs in Joblog, then these individuals should be (partially) removed by considering only individuals with a successful link.

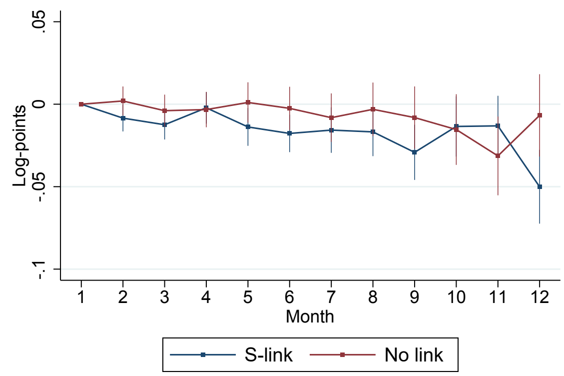
For this sub-sample, Figure 13 estimates our event-study specification across three outcomes: the typical wage, the firm type and commuting time. Reassuringly, we see that results are virtually identical to those found in the main sample.

Figure 13: Results only using matched new hires

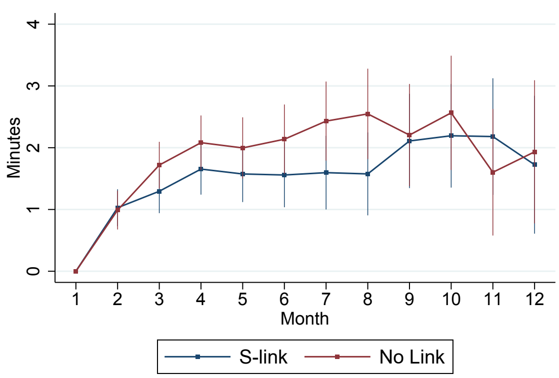
(a) Average typical wage of applied-for jobs



(b) Average firm wage level of applied-for jobs

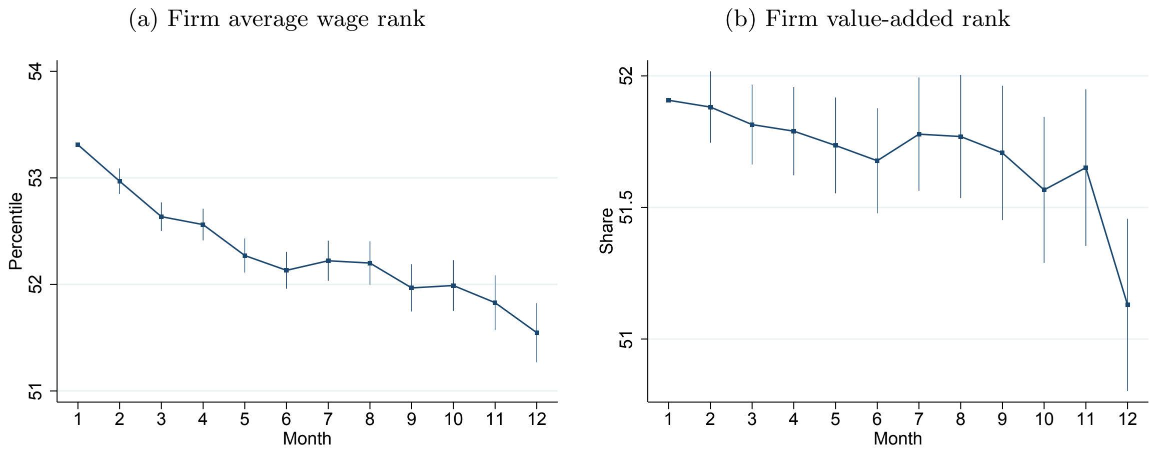


(c) Avg. commute time



Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment (see Figure 9 for a version where τ_1 is normalized to 0). The average typical wage is the average applied-for (predicted) wage in a given month, and the firm wage level is estimated firm fixed effects from an AKM regression. Commute time is the (estimated) commuting time from the municipality of the job seeker to the municipality of the applied-for firm. See Section 2.3 for further details on all these measures. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

Figure 14: Different measures of firm type



Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 for two different classifications of a firm type, see the discussion in Section B.1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

B Additional results

B.1 Firm type measures

In the main text (Figure 1b) we show that the average applied-for firm type is decreasing throughout the unemployment spell. The measure of a firm type is there determined as the firm fixed effects from an AKM wage regression. Below we test the robustness of that result as we use alternative measures of the firm type. First, in Figure 14a we show the evolution in the average applied-for firm wage rank which is determined by simply ranking firms by their average wage level across all employment spells in the BFL data (see footnote 35) covering all salaries in Denmark from 2008 to 2015. Second, in Figure 14b we report the results obtained when we instead rank firms by their value added per worker. Value added per worker is here determined by subtracting firm purchases from firm sales and dividing through by firm level employment (in full-time equivalents). The measures of firm sales and firm purchases are obtained from the FIKS database at Statistics Denmark[42], and the measure of firm level employment is calculated based on the BFL data.

B.2 Unrelated occupations - alternative measures

In the main text we classify whether applied-for occupations are unrelated or not based on the O*NET Related Occupations Matrix (see Section A.4). To test the robustness of our results in Figure 4 we also construct alternative measures using the Danish register data. First, we build

[42]The FIKS database is constructed based on the mandatory VAT reports which are submitted from the universe of (VAT eligible) Danish Firms, for further information see: https://www.dst.dk/da/TilSalg/Forskningsservice/Dokumentation/hoejkvalitetsvariable/firmaernes-koeb-og-salg

a occupational-code to occupational-code transition matrices using changes in occupational codes within the Danish BFL data (see footnote 35). We follow individuals over time from 2009-2016 and sample all events where the occupational code change for an employee, allowing for a maximum of 365 days in between payments. Thereby we use occupational code changes within jobs (e.g., promotion) as well as changes arising in the change of employer (also allowing for shorter periods of unemployment). Using these transitions we construct a matrix which describes the transition probabilities over future occupational codes conditional on a specific previous occupation code. To be consistent with the definition in the main text we then categorize applied-for jobs in terms of how unrelated they are by focusing on the inverse event (i.e., not making a transition to this specific occupation code).[43] We construct measures at both the 1st, 2nd and 3rd digit level. Compared to the O*NET based measure our "BFL measure" has the advantage that we can classify all occupation-to-occupation transitions with their corresponding transition probability thereby avoiding making choices of how many occupations to classify as (un)related, and further we obtain a richer characterization of how (un)related occupations are.

In Figure 15 we report the dynamics in the probability of applying to occupations which are unrelated based on these measure(s). The dynamics are remarkably similar to the dynamics reported in Figure 4, i.e., across all three measures we see a larger initial adjustment during the first 3-4 months, and thereafter the change in estimates decreases substantially and we see small changes in search behavior in these dimensions.[44]

B.3 Constructing the vacancy panel

In Section 5.4 we test for the presence of stock-flow effects driving our results. To test the relevance of stock-flow effects we leverage the fact that we have firm identifiers of the applied-for firms and use our application panel to construct "vacancy" indicators. In other words we browse through all applications in our raw data (i.e., the full set of job logs we have available and not just our final analysis sample) and record the first time we see an application to a specific firm id and code this as a vacancy opening. If more than 2 weeks pass without applications to a given vacancy we close it again and it may then open again as a new vacancy later on in the panel. In Figure 5 we show the evolution in the probability of a applying to a vacancy which is "new", defined as having opened during the last 2 weeks. In Figure 16 we show how the dynamics profile change as we 1) change the "opening" period, i.e., include vacancies having opened during the last X weeks, and 2) distinguish vacancies by the combination of firm id and occupation codes (1-digit). Across all figures we see the same overall patterns, job seekers are gradually applying to more new vacancies over time, but again the magnitude of the change is very limited supporting the results in the main text.

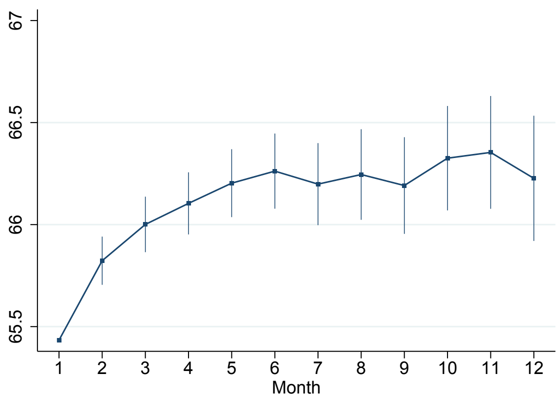
Finally, in Figure 5 we explicitly show that stock flow-effects are unlikely to drive the particular

[43] Note that opposite to the categorization in the main text the categorization of each applied-for job is no longer binary (unrelated or not) but now instead a probability.

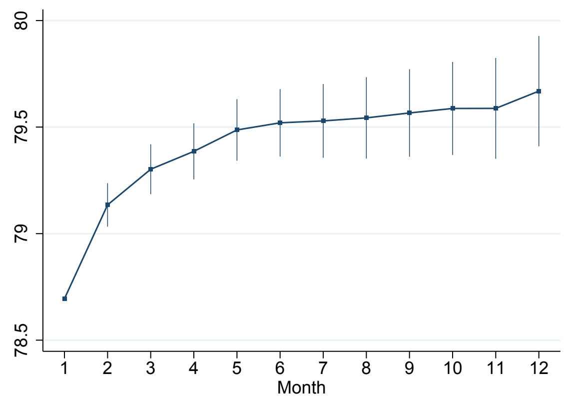
[44] Note also that the baseline (the average of the outcome variable in month 1) is increasing as we move to higher level occupation codes. This makes sense as for higher digits the probability of a specific transition decreases and hence the probability of the inverse is increasing.

Figure 15: Jobs in occupations unrelated to prev. job - alternative measures

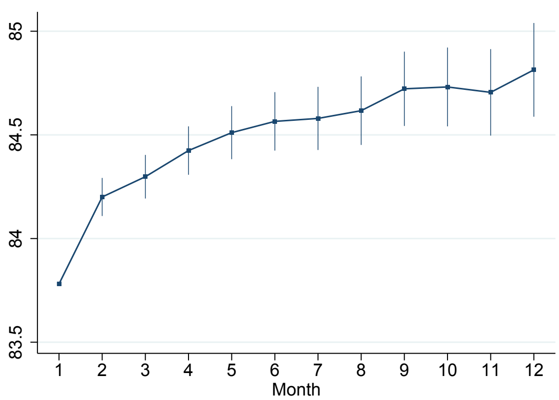
(a) Unrelated occupations at the 1 digit level



(b) Unrelated occupations at the 2 digit level

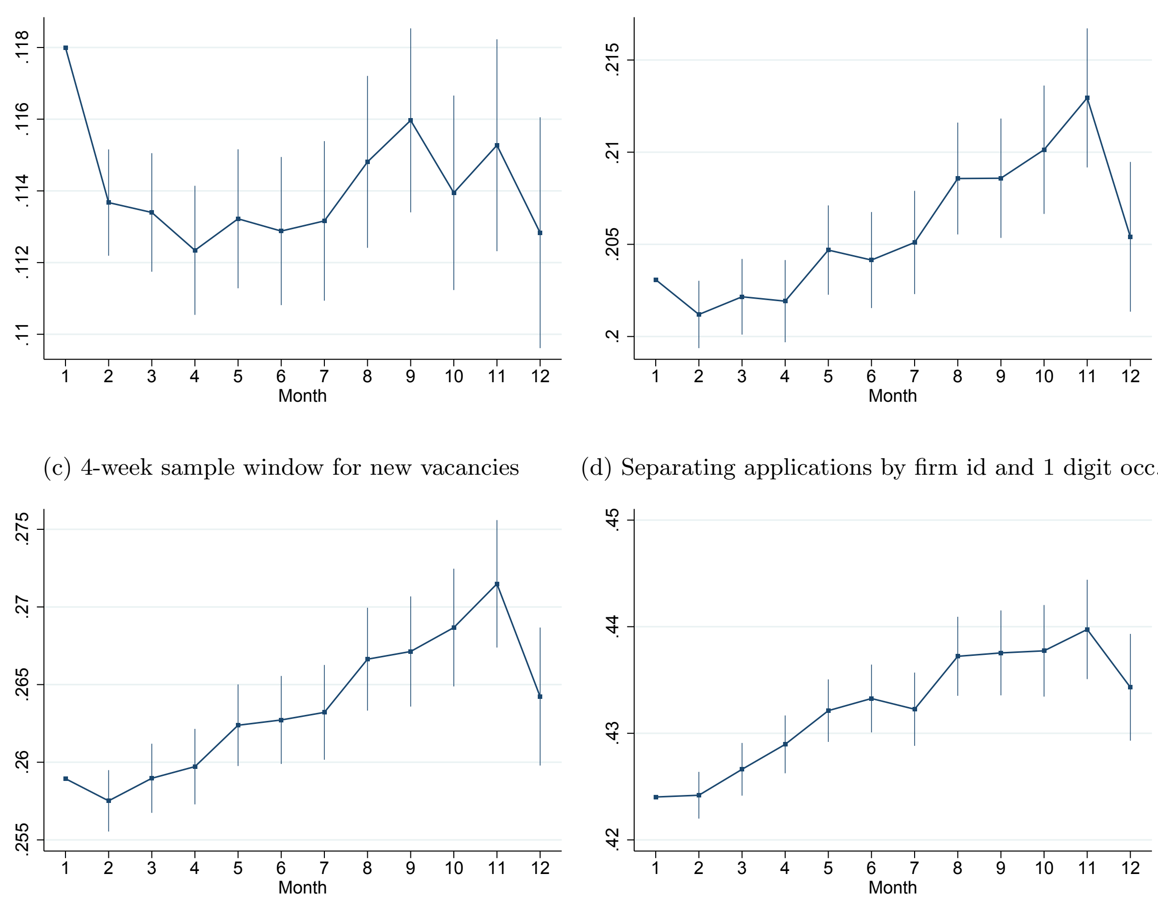


(c) Unrelated occupations at the 3 digit level



Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 for alternative measures of unrelated occupations which are created using the Danish registers, see Section B.2. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

Figure 16: Share of applications sent to a new vacancy - using different definitions



(a) 1-week sample window for new new vacancies

(b) 2-week sample window for new vacancies

(c) 4-week sample window for new vacancies

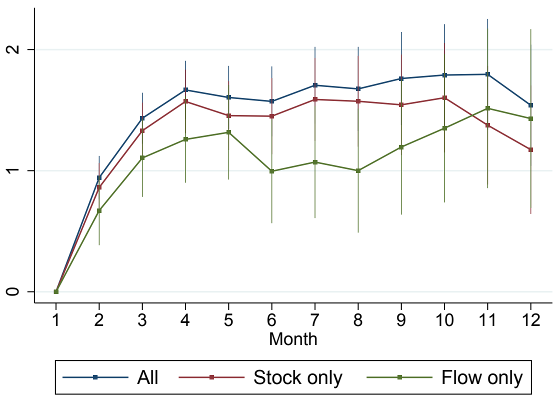
(d) Separating applications by firm id and 1 digit occ.

Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 for different categorizations of whether a given application constitutes a new vacancy or not. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

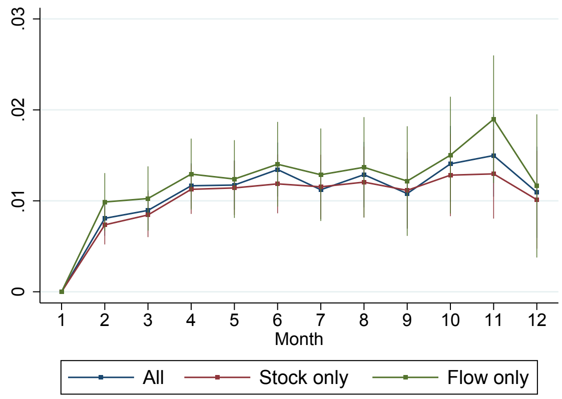
adjustment patterns we observe for our measures of proximity where we observe a large initial adjustment but then very little adjustment from around 4 months and onward. In the figure we first report the baseline dynamic profiles as reported in the main text for our three measures of proximity, and then we add dynamics profiles when we only use applications to new vacancies(flow vacancies) or existing vacancies (stock vacancies). As is evident from the figure the dynamics are very similar across all three profiles, leading us to conclude that although stock-flow effects may well be at work in the data, they are unlikely to explain the observed changes in the proximity of applied-for jobs.

Figure 17: Distinguishing applications to new and older vacancies

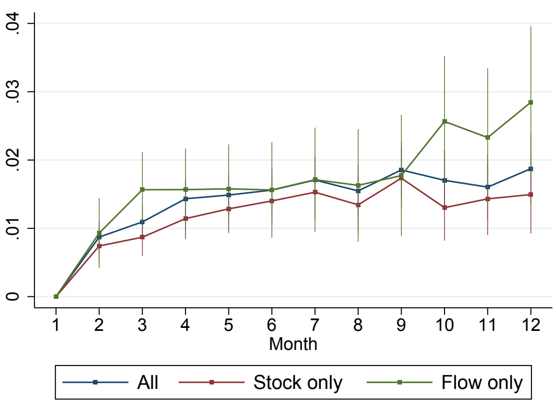
(a) Avg. commute time



(b) Jobs in occupations unrelated to prev. job



(c) Jobs in industries unrelated to prev. job



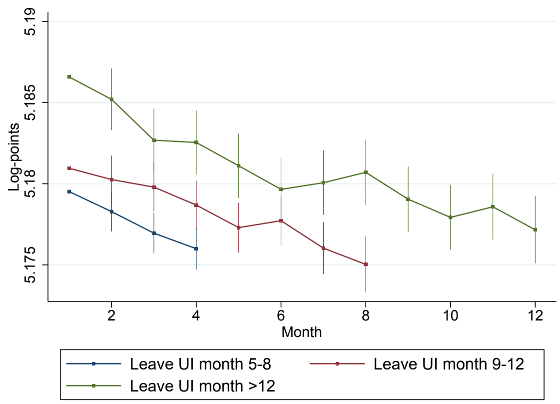
Note: This figure shows the estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1 for three specifications, one where we use outcomes from all applications (thus identical to the baseline estimates in Figure 4), one where we only use applications to vacancies we classify as new (flow only) and one where we only use applications to existing vacancies (stock only). Note that we have normalized τ_1 to 0 to ease comparison of the dynamics profiles. "Occupations unrelated to the prev. job" measure the share of applications sent to occupations which are not among the top 10 related occupations in the O*NET Related Occupations Matrix. "Industries unrelated to the prev. job" measure the share of applications sent to firms in industries which are not among the top 10 most related 3-digit industries. Commute time is the (estimated) commuting time from the municipality of the job seeker to the municipality of the applied-for firm. See Section 2.3 for further details on all these measures. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

B.4 Heterogeneity by realized unemployment duration - including level differences

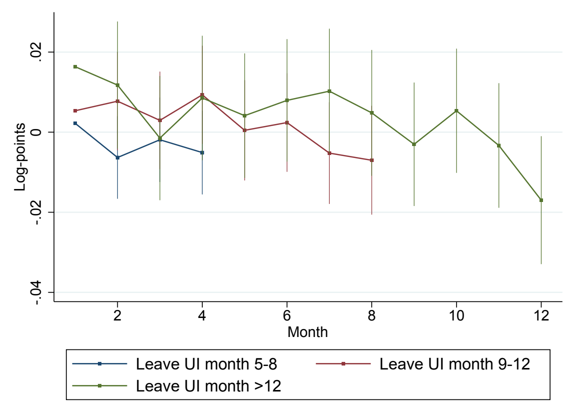
In Figure 18 and 19 we plot the resulting estimated time paths for all our main measures of job search behavior, search methods and channels across realized unemployment duration groups. The figures are the same as Figures 9 and 10 in the main text with the exception that in the main text we normalized the estimate of τ_1 to be zero in the first month for all groups and outcomes to facilitate comparisons of the changes in $\tau_i' s$ across the groups. In Figures 18 and 19 we instead normalize τ_1 to equal the mean of the outcome variable in the first month of unemployment for the specific group to illustrate level differences across the different groups. Figure 18 show there there are generally level differences in our measures of search behavior across individuals with different final unemployment durations. For example, individuals with longer realized unemployment durations (above 12 months) apply to jobs with higher typical wages and to jobs in more unrelated industries and occupations on average. With respect to search methods and channels (Figure 19) the level differences across groups are somewhat smaller, but still present. For example, individuals with shorter unemployment durations (leave UI in months 5-8) use publicly posted vacancies less but use their personal network slightly more in their job search. They are also less likely to apply for the jobs using online platforms.

Figure 18: Changes in applied-for jobs by duration of unemployment

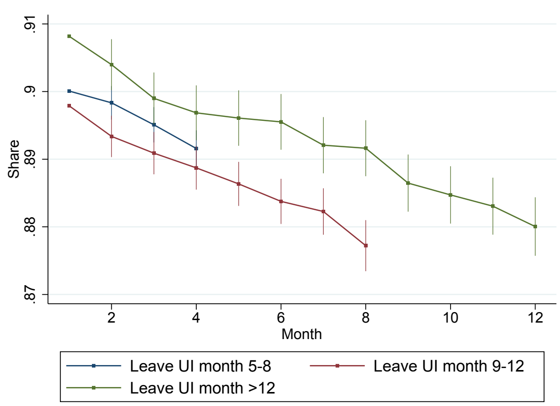
(a) Average typical wage of applied-for jobs



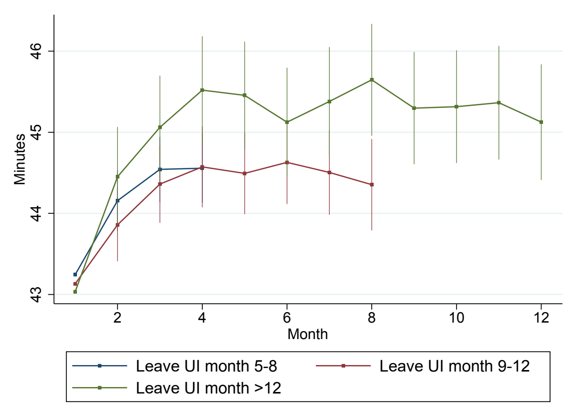
(b) Average firm wage level of applied-for jobs



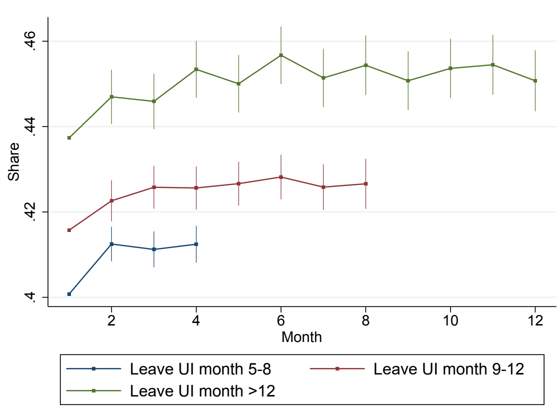
(c) Share of jobs that are full-time



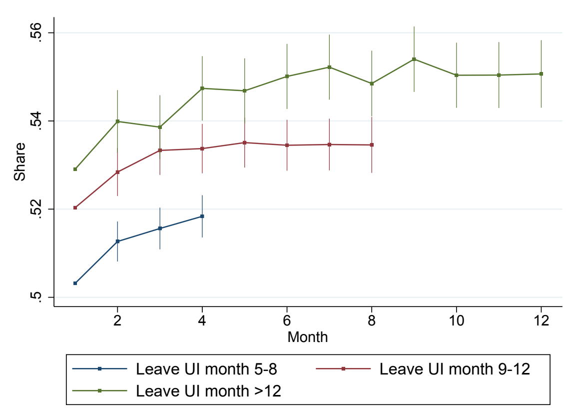
(d) Avg. commute time



(e) Jobs in occupations unrelated to prev. job



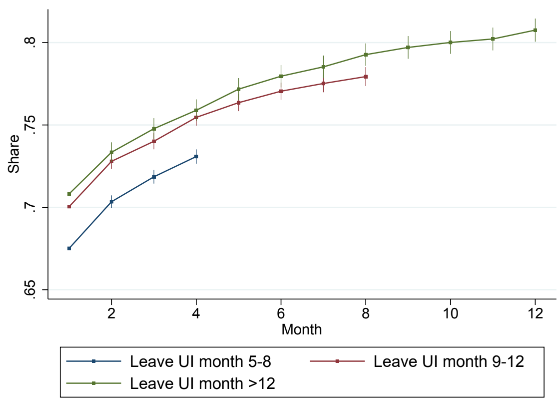
(f) Jobs in industries unrelated to prev. job



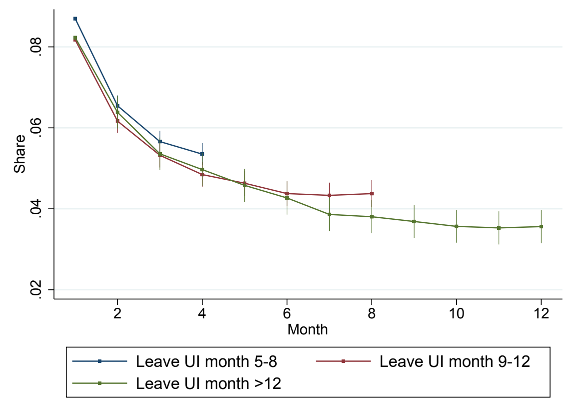
Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment (see Figure 9 for a version where τ_1 is normalized to 0). The average typical wage is the average applied-for (predicted) wage in a given month, and the firm wage level is estimated firm fixed effects from an AKM regression. "Occupations unrelated to the prev. job" measure the share of applications sent to occupations which are not among the top 10 related occupations in the O*NET Related Occupations Matrix. "Industries unrelated to the prev. job" measure the share of applications sent to firms in industries which are not among the top 10 most related 3-digit industries. Commute time is the (estimated) commuting time from the municipality of the job seeker to the municipality of the applied-for firm. See Section 2.3 for further details on all these measures. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

Figure 19: Changes in search channels and methods by duration of unemployment

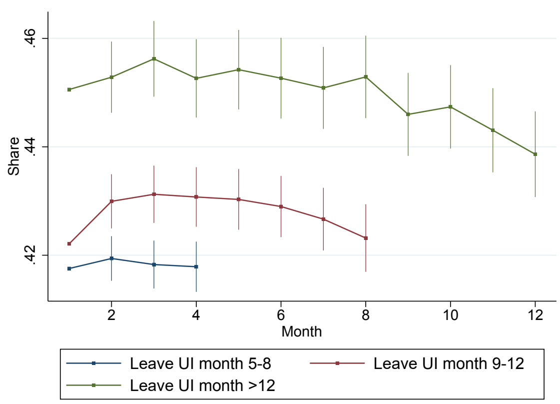
(a) Jobs found via a publicly posted vacancy



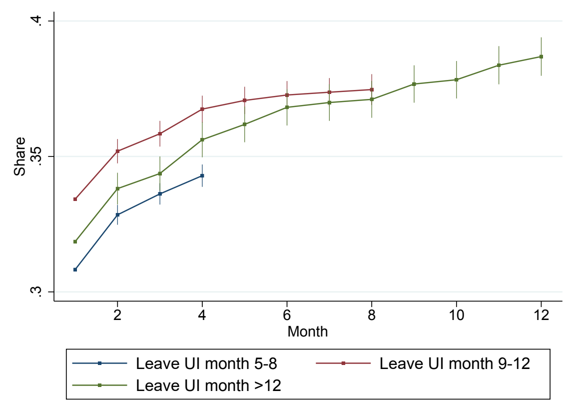
(b) Jobs found via personal network



(c) Applications via mail or e-mail



(d) Applications made through an online platform



Note: This Figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ in Equation 1. Note that we have normalized τ_1 to equal the mean of the outcome variable in the first month of unemployment (see Figure 10 for a version where τ_1 is normalized to 0). The outcome variable is the share of submitted applications which where found using a specific search method ("how did you find the job?") or a specific application method ("how did you apply for the job?"), see Section 2.3 for additional details. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.

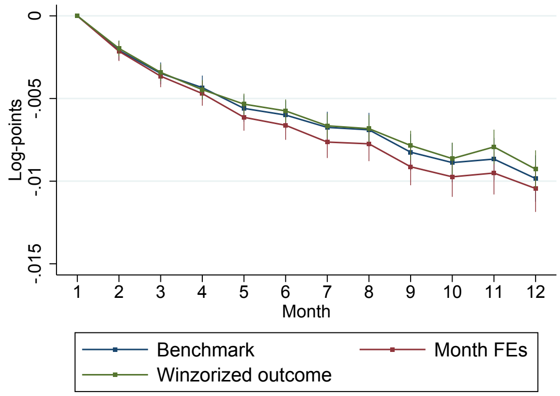
C Additional robustness checks

C.1 Time effects and trimming

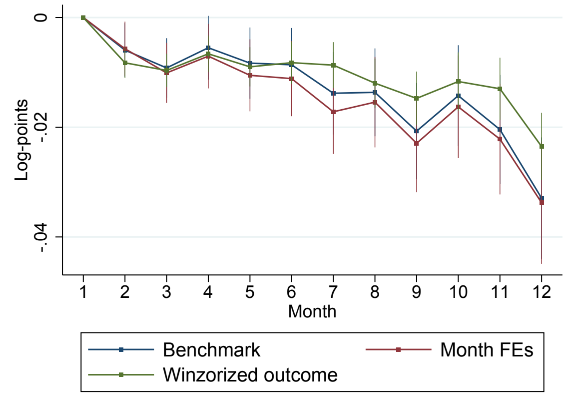
In Figure 20 we repeat our main results (based on Equation 1) and contrast the resulting dynamics profiles to the profiles we obtain from two robustness checks. First, we add calendar-month fixed effects to the set of control variables, we thereby leverage that we have variation in the starting time of a given UI spell which allows us to check whether seasonality drives our results. Second, we run our main specification after we winsorize the outcome variables at the 10^{th} and 90^{th} percentile within each month of unemployment duration. The estimated time paths changes very little leading us to conclude that our main results are robust.

Figure 20: Changes in applied-for jobs

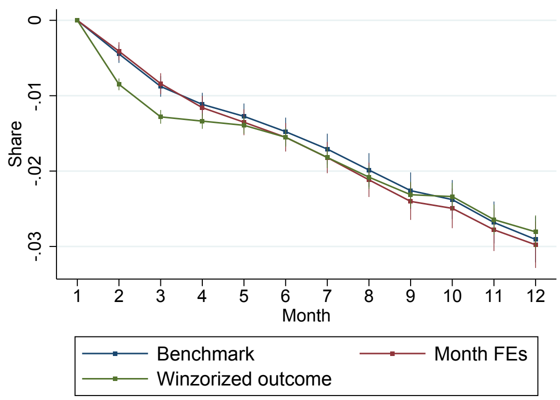
(a) Average typical wage of applied-for jobs



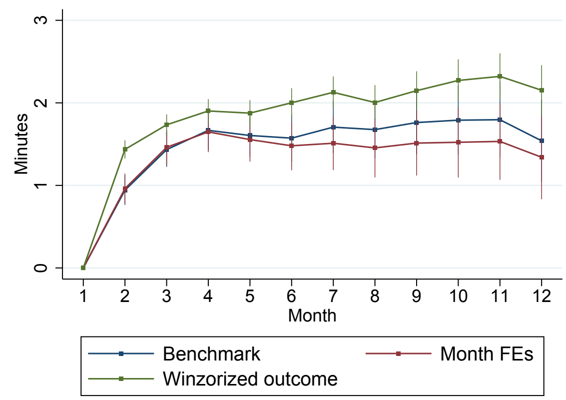
(b) Average firm wage level of applied-for jobs



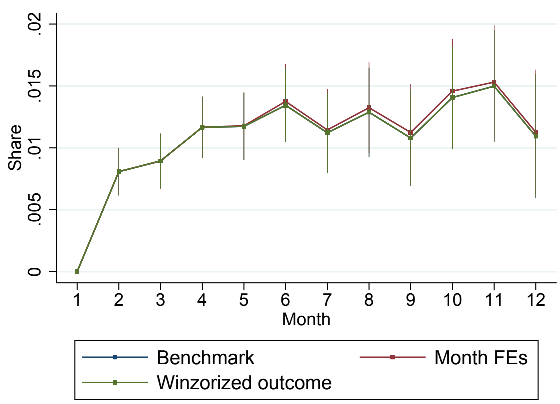
(c) Share of jobs that are full-time



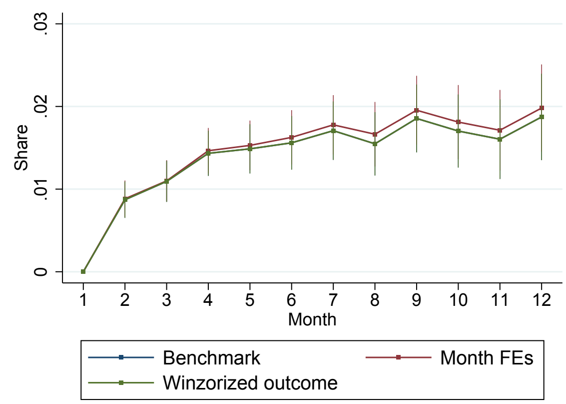
(d) Avg. commute time



(e) Jobs in occupations unrelated to prev. job



(f) Jobs in industries unrelated to prev. job



Note: This figure shows the resulting estimates of the duration-month fixed effects, $\tau_1, \tau_2, ...\tau_{12}$ obtained when running 1) our benchmark model as in Equation 1, 2) a specification where we add calendar month fixed effects to the specification and 3) running our benchmark model on outcome variables where we have winzorized observations below the 10^{th} or above the 90^{th} percentile within a given unemployment duration month. To facilitate comparison across specifications we have normalized τ_1 to 0. See Section 5 and 2.3 for definitions of the outcomes. Standard errors are clustered at the level of the individual and vertical bars display 95% confidence bands.