ESSAYS ON THE ECONOMICS OF JOB SEARCH

PhD Dissertation

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Jonas Fluchtmann
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## 4 Nudging and Self-Efficacy Intervention

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Summary

Unemployment affects both the unemployed and the economy as a whole. For the individual, joblessness often results in a substantial loss of income and for the society, the provision of unemployment insurance (UI) benefits to secure subsistence is costly. While a speedy return into employment is desirable for most, the actual search for employment is a complex endeavour. It entails a continuous series of important decisions and predictions that each individual job seeker needs to make. During this process, individuals not only need to identify jobs they are interested in - taking into account the expected pay, distance from home, and other aspects of the job - but each job seeker also needs to predict the chances of receiving a job offer upon sending an application. Further, the individual search for a job might be influenced by particular behavioral biases that can induce procrastination, overconfidence and loss aversion - all of which may have important implications for search behavior.

This dissertation comprises four independent chapters that contribute to the research on job search during unemployment. The first three chapters address behavioral and dynamic aspects of job search as well as its importance for gender gaps in job outcomes. The fourth chapter finishes with an intervention to enhance labor market attachment for long-term unemployed, a group that faces additional barriers to job search.

In the first chapter, *Policy Evaluation under Biased Job Search*, I focus on the presence of behavioral biases in job search. These affect not only how (and how much) individuals search during unemployment but an understanding of their mechanisms may also help to design a more effective unemployment insurance system. To get a sense of the importance of these behavioral features, I develop a broad and tractable behavioral job search model that incorporates multiple behavioral biases. Some recent job search frameworks incorporate these biases in one-at-a-time piece-meal approaches. However, there is a lack of comprehensive models encompassing multiple features. My model builds upon the work of Paserman (2008), Spinnewijn (2015) and DellaVigna et al. (2017) and combines their behavioral extensions. I explicitly allow for loss aversion induced by reference-dependence, present bias, and biased beliefs about the job offer arrival rate. I further extend the model by allowing the agents to learn about their actual job offer probability over time, which erodes their initially biased confidence. To recover the model parameters, I structurally es-
imate the framework on Danish labor market data. The model fits the observed unemployment-employment exit rates and re-employment wages significantly better than traditional settings and prior behavioral frameworks. By testing several hypothetical policies in this setting, I show that a changing to a front-loaded system or a job search monitoring scheme may increase the individual welfare while simultaneously reducing the expected duration in unemployment and the government expenditure on UI benefits. I highlight that focusing on a single behavioral feature can lead to biased policy prescriptions.

In the second chapter, *The Dynamics of Job Search in Unemployment*, Jonas Maibom and I shed light on the specific job characteristics that job seekers target over an unemployment spell. We use a novel administrative data set containing comprehensive search diaries that Danish unemployed need to report continuously to be eligible for UI benefits. This data covers a large fraction of the job applications made by the universe of Danish UI recipients and is linkable to administrative data on individuals and firms. We can thus add detailed information on the characteristics of the job that these applications are directed to. Our approach relates to the recent spread of literature using comprehensive data on individual applications sourced from online job boards (e.g. Faberman and Kudlyak, 2019). However, because of the administrative nature of this data and the possibility to link it to the Danish registers, we can cover job search in greater detail. We show that the characteristics individuals target change very little over the unemployment spell, yet important differences exist between the job search portfolio and the eventually successful application. We further document that caseworker meetings and active labor market policies do not substantially change the jobs individuals apply to, though we observe hints of threat- and lock-in effects in the number of applications sent.

In the third chapter, *Gender Gaps in Job Applications and Hiring Outcomes*, jointly written with Anita Glenny, Nikolaj Harmon and Jonas Maibom, we examine the importance of gender differences in job search for observed gender differences in post-unemployment job outcomes. Using the same data set as in chapter two, we document novel facts about gender differences in job search along several dimensions. We show that women, even when controlling for observable differences in education and labor market history, tend to direct a larger share of their applications to lower-paying jobs. The gaps in the eventual job outcomes closely track these gender gaps in application behavior. This suggests that differences in application behavior are responsible for a substantial part of differences in outcomes, such as the gender wage gap and the allocation into different occupations and industries. To get a sense of how much of the post-unemployment wage gap these gender differences can explain, we adopt a semi-parametric decomposition method by DiNardo et al. (1996). Our results show that gender differences in application behavior explain a large part of the gap. We further explore potential mechanisms behind differences in job search and find that women might trade-off wages against other amenities of the job, such
as shorter commute times, fewer hours and more family-friendly employers.

In the last and fourth chapter, Nudging and Self-Efficacy Intervention for Long-Term Unemployed, co-authored with Alexander Koch and Michael Rosholm, we develop and test a low-cost nudging intervention for the group of long-term unemployed on the edge of the labor market. This group differs from the populations studied in chapters one to three, as strong personal barriers to job search and employment often challenge these individuals. This includes, among others, mental- and physical health challenges, substance abuse and a general lack of (job search) skills. We use a randomized control trial to test whether an SMS- and video-based intervention can enhance participation in the labor market for this vulnerable group. The treatment aims to boost the individual’s self-efficacy to increase the beliefs in the possibility to work for a few hours per week. Participants receive testimonial videos of successful citizens challenged by similar barriers and short clips with targeted job search guidance, both based on a supplementary homepage. Because of a substantial lack of statistical power, we can make no clear conclusions on the effects of this intervention. Acknowledging these shortcomings, we can report small positive, yet insignificant, effects on the likelihood of having any employment or internship in the three months after the intervention initially starts.
References


DANSK RESUMÉ

Arbejdsløshed påvirker både de arbejdsløse og økonomien som helhed. For individet medfører arbejdsløshed ofte et betydeligt indkomsttab og for samfundet, er udgiften til arbejdsløshedsforsikring, som et gode til sikring af tilværelsen, bekostelig. Mens en hurtig tilbagevenden til beskæftigelse er ønskeligt for de fleste, er den faktiske søgen efter beskæftigelse en kompleks bestræbelse. Det indebærer en kontinuerlig række vigtige beslutninger og forudsigelser, som hver enkelt jobsøgende skal foretage sig. Under denne proces, skal enkeltpersoner ikke blot identificere job de er interesserede i at tage - de skal også forholde sig til den forventede løn, pendlingsafstand og andre aspekter af jobbet. Den jobsøgende skal også forudsige chancerne for at modtage et jobtilbud, når de sender en ansøgning afsted. Desuden kan den enkeltes jobsøgning blive påvirket af særlige adfærdsmæssige bias, der kan lede til overspringshandlinger, tabsaversion og overmod, som kan have afgørende konsekvenser for den individuelles søgeadfærd.


DANSK RESUMÉ

ger med hensyn til, hvornår et jobtilbud kommer. Jeg udvider modellen yderligere ved at tillade individer at lære om den faktiske sandsynlighed for jobtilbud over tid, hvilket undergraver jobsøgerens oprindelige forudindtaget tillid til, hvornår det sker. For at nå frem til modellparametrene estimator jeg modellen på danske arbejdsmarkedssdata. Modellen er betydeligt bedre tilpasset den observerede arbejdsløshedssrate samt genbeskæftigelses løsningerne end traditionelle modeller, samt tidligere adfærdsmaessige versioner. Ved at afprøve flere hypotetiske dagpensystemer i denne model, viser jeg, at en ændring til et front-loaded system eller et monitoreringssystem for jobsøgning, kan øge den enkeltes velfærd, mens det samtidigt nedarbringer den forventede varighed af arbejdsløshed og dermed det offentliges udgifter til arbejdsløshedsforsikring. Jeg fremhæver, at ved at fokusere på én enkelt adfærdsmaessig funktion, kan det føre til partiske politiske løsninger.


I det tredje kapitel, *Gender Gaps in Job Applications and Hiring Outcomes*, skrevet i fællesskab med Anita Glenny, Nikolaj Harmon og Jonas Maibom - undersøger vi betydningen af kønsforskelle i jobsøgningsadfærd for de observerede kønsforskelle i jobresultater efter arbejdsløshed. Ved at bruge det samme datataset som i kapitel 2, dokumenterer vi nyt fakta om kønsforskelle i jobsøgning der tages højde for observerbare forskelle dimensioner. Vi viser, at kvinder, selv når de tager højde for kendte forskelle i uddannelses- og arbejdsmarkedshistorik, har tendens til at sende en større andel af deres ansøgninger til lavlønsjobs. Disse forskelle i fundne jobs afspejler nøje de konstbetingede forskelle i søgeadfærd. Dette antyder, at forskelle i søgeadfærd er ansvarlige for en væsentlig del af forskellene i resultaterne, såsom lønforskelle blandt køn og kønsfordelingen i forskellige erhverv og industrier. For at få en fornemmelse af,
hvor meget af lønforskellen efter arbejdsløshed, som disse kønsforskelle kan forklare, antager vi en semi-parametrisk dekomponeringsmetode af DiNardo et al. (1996). Vo-
res resultater viser, at kønsforskelle i søgeadfærd forklarer en stor del af lønforskellen. Vi udforsker yderligere potentielle mekanismer bag forskelle i jobsøgning og finder ud af, at kvinder måske er mere villige til at bytte lønninger mod andre faciliteter i forbindelse med et job, f. eks. kortere pendlertid, nedsat tid og mere familievenlige arbejdsgivere.

I det sidste og fjerde kapitel, Nudging and Self-Efficacy Intervention for Long-Term Unemployed, skrevet sammen med Alexander Koch og Michael Rosholm, udvikler og afprøver vi en billig nudgingintervention for gruppen af langtidsledige på kanten af arbejdsmarkedet. Denne gruppe adskiller sig fra de øvrige grupper af ledige som er undersøgt i kapitel et til tre, da der her er stærke personlige barrierer forbundet med jobsøgning og beskæftigelse. Dette omfatter blandt andet mentale og fysiske helbredsmæssige udfordringer, stofmisbrug og en generel mangel på jobsøgnings-
færdigheder. Vi bruger et randomiseret kontrolleret forsøg til at teste, om en SMS og en videobaseret intervention kan øge tilknytningen på arbejdsmarkedet for denne sårbare gruppe. Interventionen har til formål at styrke den enkeltes tro på egne mulig-
heder for at opnå beksæftigelser fra få timer om ugen. Deltagere modtager videoer, der viser andre borgere i tilsvarende situationer, som har haft succes med at vende tilbage til arbejdsmarkedet, samt korte klip med målrettet vejledning til jobsøgning, baseret på en supplerende hjemmeside. På grund af en betydelig mangel på stati-
stisk styrke, kan vi ikke give klare konklusioner for effekterne af denne intervention. Trods disse svagheder, kan vi rapportere små positive, men ikke betydelige effekter på sandsynligheden for, at der er beskæftigelse eller praktikophold tre måneder efter, at interventionen startede.
Litteratur


CHAPTER 1

POLICY EVALUATION UNDER BIASED JOB SEARCH

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Abstract

In this paper I estimate a broad and tractable behavioral job search model that incorporates multiple behavioral biases which have mainly been applied in a one-at-a-time piecemeal approach in the previous literature. I allow for loss aversion induced by reference-dependence, present bias and biased beliefs about the job offer arrival rate. Additionally, I explicitly allow the agents to learn about their actual job offer probability over time, which erodes their initially biased confidence. My model fits the observed data significantly better than traditional settings and prior behavioral frameworks, even when allowing for unobserved heterogeneity in the pool of unemployed. I use this partial equilibrium framework to evaluate several hypothetical reforms of the Danish UI system. This exercise highlights that focusing on a single behavioral feature can lead to biased policy prescriptions. It further suggests that, given the estimated model, a policy change from the status-quo to a multi-step policy or a job search monitoring scheme could have overall beneficial effects as the non-standard preferences and beliefs of unemployed agents are exploited.

Keywords: job search, cognitive biases, structural estimation, policy evaluation

JEL Codes: D83, H30, J64
1.1 Introduction

Fueled by Kahneman and Tversky’s (1979) prospect theory, a vast and increasing amount of research has established non-standard preferences, beliefs and decision making in economic modelling. These behavioral models seek to include real-world divergences from a neoclassical standard theory of economics (DellaVigna, 2009). One area that is not only actively debated in academia and public alike, but also subject to particularly salient behavioral triggers, is the process of job search and the design of the unemployment insurance (UI) system.\(^1\) Standard economic theory often struggles to fit the distinct paths of unemployment-to-employment exits without imposing unobserved heterogeneity in the pool of unemployed.\(^2\) Additionally, the presence of these biases might have important implications for policy design as model based ex-ante policy recommendations can often differ between behavioral models and traditional settings (Chetty, 2015). In the job search literature, behavioral features have so far only been added separately in a piecemeal approach. While each method significantly increases the fit of the models to empirical observations on the labor market, we still lack answers on how significant each of these features are when applied together. Further, there is no clear guidance on how these features affect policy outcomes when combined, which inhibits the full potential of behavioral job search theories for policy design.

The innovation of this paper is to combine in a single tractable model three notable extensions to standard job search framework - reference dependence in conjunction with loss aversion, present bias and biased beliefs about the job offer probability. First, I allow for time inconsistent preferences by introducing (quasi-)hyperbolic discounting. This follows Paserman (2008) who quantitatively estimates the degree of present bias in a job search model with (quasi-)hyperbolic discounting. This extension induces procrastination in search activities.\(^3\) Second, I include a bias in the job seeker’s perceived probability of getting a job offer by altering the functional form of the job offer arrival rate. This is related to Spinnewijn (2015), who examines the influence of overconfidence on job search. He shows empirically that unemployed job seekers strongly overestimate their probability to find a job by taking three times longer than they expected to find employment. This has important implications for UI design.\(^4\) I extend this framework by explicitly allowing for the possibility that job seekers learn about the true

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1. The costs of search, such as browsing job-boards and writing applications, are immediate while the potential benefits, i.e. job offers, are delayed. If individuals are present biased, this can lead to procrastination in the job search activities. Individuals might be uncertain about their own job search efficiency and thus over- or underestimate their likelihood of getting a job offer given their exerted efforts. Taking into account behavioral biases is a promising path to an improved model fit. Additionally, if job seekers are loss averse, one may observe rather extreme behavior around income transfer changes.

2. Note that other methods, for example stock-flow matching (Gregg and Petrongolo, 2005) or optimal stopping problems (Shimer, 2008; Alvarez et al., 2016) can fit a part of these patterns (e.g. duration dependence).

3. The (quasi-)hyperbolic discounting parameter \( \beta \) is estimated below 1, in line with the present bias assumption. Paserman further evaluates policy changes that aim to increase search-effort, which, as a result of present bias, is below its optimal level. In this context, job search monitoring efforts may increase the search intensity and social welfare whilst decreasing the duration of the unemployment spell and the overall government expenditure on the UI system.

4. Spinnewijn calibrates his model such that beliefs about the job offer probability are biased upward and benefits are financed through taxes levied on wages in employment. In this setting, an overconfident individual might prefer a lower benefit level, resulting in lower taxation, as the job seeker expects to stay unemployed shorter than the actual realized duration.
probability of finding employment over the course of the unemployment spell, which embeds the assumption of a constant bias that Spinnewijn makes. My more flexible approach also allows for eroding self-confidence as in Falk et al. (2006a, 2006b). Third, the unemployed job seekers in my model are loss averse. Including a reference dependent utility function allows for sharply declining unemployment-to-employment hazard rates after shifts in income levels, even under the absence of unobserved heterogeneity in the pool of unemployed. This follows DellaVigna et al. (2017), who introduce a job search model with present bias as well as reference dependence and estimate it on Hungarian unemployment data around a major UI reform.\footnote{DellaVigna et al. hypothesize that a decreasing multi-step benefit system might be desirable if the unemployed are reference dependent. See Lindner and Reizer (2016) for an empirical assessment of benefit frontloading as part of the Hungarian UI reform.}

I recover the model parameters through structural estimation using detailed Danish register data on labor market hazard rates and re-employment wages around a major reform of the UI entitlement length between 2008 and 2011. All three model extensions have distinct effects on the simulated hazard rates. Differences in the empirical exit rates into employment around the UI reform identify the key behavioral parameters. My behavioral model, even without imposing unobserved heterogeneity, outperforms other frameworks. These models need high degrees of unobserved heterogeneity in the pool of unemployed to reach comparable degrees of data fit. In particular, the simple introduction of biased beliefs to a DellaVigna et al. (2017)-type model almost halves the weighted deviation from the empirical moments.

As one strength of the structural approach, I can, based on the estimated parameters in the given partial equilibrium setting, evaluate a set of hypothetical policies. This makes it possible to gauge the extent to which policy and welfare conclusions differ from those reached with a traditional model or frameworks that include behavioral features separately. I evaluate the implications on individual welfare, unemployment duration, government expenditure, and the share of individuals that eventually transition into the welfare system. I present suggestive evidence that a policy change from the status-quo to a multi-step policy or a job search monitoring scheme could have overall beneficial effects. This results from exploiting how non-standard preferences and beliefs shape the behavioral responses of agents. Importantly, the implications of certain policies can not only vastly differ between my model and a standard setting, but also compared to the one-bias-at-a-time piecemeal approaches. This finding offers an important caveat about focusing on a single behavioral feature, showing that this can lead to biased policy prescriptions.

This paper contributes by adding a unifying model to the literature on behavioral labor economics which examines cognitive biases and their impact on individual behavior on the labor market.\footnote{Applications focus for example on the influence of reference points on reservation wages (Koenig et al., 2016), non-search behavior (Damgaard, 2017), labor supply in general (Camerer et al., 1997; Goette et al., 2004; Farber, 2008; Thakral and Tô, 2017) and its reactions to wage changes (Doerrenberg et al., 2016) as well as wage rigidity induced by loss aversion in search and matching models (Eliaz and Spiegler, 2014). DellaVigna and Paserman (2005) as well as Van Huizen and Plantenga (2013) explore the influence of impatience through (quasi-)hyperbolic discounting on job search behavior. Ganong and Noel (2019) and Gerard and Naritomi (2019) relate potential present bias of unemployed job seekers to the lack of consumption adjustment around the exhaustion of UI benefits. Caliendo et al. (2015) and Dohmen et al. (2009) relate biased probabilistic expectations to job search.} It relates in particular to the field of behavioral job search with the before-mentioned contributions. It further contributes to the large literature on optimal design of the UI system (e.g. Kolsrud et al., 2018; Schmieder and Von Wachter, 2016; Chetty, 2008; Boone...}
et al., 2007). I structure the paper as follows: Section 2 introduces the theoretical framework with its behavioral extensions. Section 3 briefly outlines the Danish UI system and the data that is used to structurally estimate the underlying model parameters. The estimation itself is laid out in Section 4. Following, I examine effects of hypothetical policy reforms under this model in Section 5, before concluding in Section 6.

1.2 Theoretical Framework

The theoretical framework of this paper builds on the before-mentioned discrete time job search intensity model of DellaVigna et al. (2017). This model in turn builds on the settings developed by Card et al. (2007) and Lentz and Tranæs (2005) by allowing for endogenous saving. In this work I abstract from endogenous saving as the computational intensity increases enormously. Additionally, my prior work to this paper has shown little importance for savings decisions in this context, possibly due to the overall generous UI system (Fluchtmann, 2016).

In each of the periods an unemployed job seeker needs to choose the utility maximizing job search effort \( s_t \in [0,1] \) which gives the probability of receiving a job offer in the upcoming period. The exerted search effort results in costs each period represented by the twice continuously differentiable cost function \( c(s_t) \) with \( c'(s_t) > 0 \), \( c''(s_t) < 0 \), \( c(0) = 0 \) and \( c'(0) = 0 \). The wage \( w_t \) of a job offer is drawn from the cumulative wage offer distribution \( F(w) \).

During the unemployment spell the agent receives transfers \( b_t \) in each of the periods, which are consumed instantaneously. For the first \( T \) periods, the UI benefits are set at a level \( b_t = \bar{b} \). After this, the eligibility of UI benefits is exhausted and the agent enters the welfare system receiving significantly lower social security transfers that are characterized by \( b = \psi \bar{b} \). Here \( \psi < 0 \) governs the fraction of UI benefits \( \bar{b} \) that the job seeker receives after transitioning to the welfare system.

The agent is loss averse which follows the large body of evidence on asymmetric preference of economic gains relative to equal sized losses (e.g. Kahneman and Tversky, 1979). In the context of this model one can think of an agent who feels particularly strong pain by comparing the income transfers under UI to the previous income under employment. To reduce this pain she initially searches harder in order to increase the likelihood of employment in the next period. As she becomes gradually used to unemployment this effect vanishes and search effort decreases. In this framework the agent has a reference-dependent gain-loss utility in the fashion of Köszegi and Rabin (2006). Thus, she derives instantaneous utility \( v(y_t) \) from current income \( y_t \in \{b_t, w_t\} \) as well as gain or loss utility from comparing \( y_t \) to a reference point \( r_t \), given by

\[
    u(c_t | r_t) = \begin{cases} 
    v(c_t) + \eta [v(c_t) - v(r_t)] & \text{if } c_t \geq r_t \\
    v(c_t) + \eta \lambda [v(c_t) - v(r_t)] & \text{if } c_t < r_t 
    \end{cases}
\]  

where \( \eta \) is the weight on gain-loss utility and \( \lambda \geq 1 \) is the parameter specifying the degree of loss-aversion. Under this setting, the individual weights losses with respect to the reference point \( r_t \) stronger than gains of equivalent size.

Following DellaVigna et al. (2017) and Koenig et al. (2016), the reference point \( r_t \) is modelled as a backward-looking moving average of the current income and the preceding \( N \)
1.2. Theoretical Framework

Therefore:

\[ r_t = \frac{1}{N+1} \sum_{k=t-N}^{t} y_k \]  

(1.2)

While unemployed, the reference point \( r_t \) is always higher than or equal to the current benefit level and thus the individual experiences a loss and weights the difference between \( b_t \) and \( r_t \) with \( \eta \lambda \).

Given the importance of time inconsistent preferences documented for behavior around layoffs and UI benefit exhaustion (Ganong and Noel, 2019; Gerard and Naritomi, 2019) as well as job search behavior in general (DellaVigna and Paserman, 2005; Paserman, 2008; Van Huizen and Plantenga, 2013), I assume quasi-hyperbolic discounting with a present bias parameter \( \beta \in (0,1] \) between the present and future periods (Laibson, 1997). The agent therefore discounts future periods stronger and puts more utility weight on the present. I assume that she behaves naive with respect to her time preference (O’Donoghue and Rabin, 1999), i.e. she assumes that she will discount exponentially in the future. Reaching the next period however, she applies \( \beta \) again and thus deviates from her initial plans that assumed exponential discounting. The naivité assumption is backed by recent experimental evidence that finds very little sophistication in real effort tasks (Augenblick and Rabin, 2018).

Agents do not seem to hold correct beliefs about their employment prospects (Spinnewijn, 2015; Caliendo et al., 2015; Mueller et al., 2019) and probability judgments about uncertain economic outcomes are often biased (Dohmen et al., 2009). Thus, I assume that agents hold biased beliefs about the job offer arrival rate, i.e. the probability of obtaining a job offer given the exerted search effort. In contrast to Spinnewijn (2015), I allow for approximated learning over time by formulating the bias as an exponential decay. This then decreases at a rate proportional to its current value over time:

\[ \phi_t(s_t) = s_t \cdot (\pi e^{-\xi t} + 1), \quad \xi \geq 0 \]  

(1.3)

where \( s_t \) is the true job offer arrival rate, \( \pi \) is the baseline bias at the beginning of the unemployment spell and \( \xi \) models the speed of the adaption to the true arrival rate. For \( \xi > 0 \) the bias vanishes over time as the perceived job offer arrival rate \( \phi_t(s_t) \) approaches \( s_t \). For \( \pi = 0 \), the perceived job arrival rate coincides with the true one. If \( \xi = 0 \), I get a constant bias as in Spinnewijn (2015). The extension is thus relatively flexible as multiple hypotheses are nested within (3) and the chosen specification makes approximated learning over time computationally tractable. In this set-up, a high baseline bias \( \pi \) leads to a large discrepancy between perceived and actual job offer arrival rate. As mentioned before, Spinnewijn found that at the

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7 Koenig et al. (2016) show that reservation wages significantly respond to backward-looking reference points in job search and DellaVigna et al. (2017) show that reference points as forward-looking expectations are not able to fit unemployment-employment hazard rates.

8 Note that if I set \( \eta \) to 0 I arrive back at a basic model without gain-loss utility.

9 Note: This nests the standard case of dynamic consistency for \( \beta = 1 \).

10 Further, DellaVigna and Paserman (2005) find no qualitative and quantitative behavioral differences between naive and sophisticated agents in a job search model.

11 Note that my chosen functional form only addresses the baseline bias, i.e. the level of the bias, and is less flexible than Spinnewijn (2015) regarding the control, i.e. the margins. My model assumes that agents who are baseline optimistic about the job finding probability are additionally control optimistic about the returns to search. The model is in that sense restrictive as the functional form does not allow to be e.g. baseline optimistic while being control pessimistic.
onset of a new unemployment spell individuals expected to find employment roughly three times as fast as they did in reality, which would imply $\pi = 2$ in (3). Further, a higher level of the approximated learning rate $\xi$ implies a faster adaption to the true job offer probability.

The assumption that job seekers update their beliefs is related to a model proposed by Falk et al. (2006a) where agents are uncertain about their relative ability. Unsuccessful search erodes their confidence and they become less likely to search as confidence about being a high type falls. This erosion of self-confidence over time implies a declining hazard out of unemployment. Falk et al. (2006b) present experimental evidence in support of this model. This extension is also related to Potter (2019) who structurally estimates a job search model with Bayesian learning about the job offer arrival rate. He shows empirically that unemployed job seekers underestimate the rate at which job offers arrive, though learn about the true rate from accumulated search experience over time. Both of these findings are inconsistent with Spinnewijn (2015) and Mueller et al. (2019). Additionally, Conlon et al. (2018) present evidence of non-Bayesian learning about the wage offer distribution.

The model set-up is completed with the formulation of the job seeker’s value functions under the introduced behavioral extensions. While the individual is unemployed, her utility is given by:

$$V_{t}^{U,n} = \max_{s_t} u(y_t(r_t) - c(s_t) + \beta \delta \left[ \varphi_t(s_t) \int_{\varphi_{t+1}}^{\infty} \left[ V_{t+1}^{E}(w) - V_{t+1}^{U} \right] dF(w) + V_{t+1}^{U} \right] \right) \tag{1.4}$$

where $V_{t+1}^{E}(w_{t+1})$ is the value of being employed in period $t+1$ conditional on finding a job starting in period $t+1$. $V_{t+1}^{U}$ is the future value of being unemployed under exponential discounting. In every period she draws a wage offer from $F(w)$ with an arrival rate equal to her search effort $s_t$. The job seeker however expects this arrival rate to be $\varphi_t(s_t)$. She only accepts wage offers that exceed her reservation wage $\varphi_{t+1}$. For lower wage offers she finds employment less attractive than to search and wait for another offer in the next period while staying unemployed. The value function under employment is:

$$V_{t+1}^{E}(w_{t+1}) = u(w_{t+1}|r_{t+1}) + \delta V_{t+2}^{E}|_{t+1} = \frac{\nu(w_{t+1})}{1 - \delta} + \eta \sum_{i=1}^{N} \delta^i \{ u(w_{t+1}) - v(r_{t+i}|_{t+1}) \} \tag{1.5}$$

where $w_{t+1}$ is the realized wage in period $t+1$ after accepting an offer in period $t$. The reservation wage $\varphi_{t+1}$ is the single wage offer that satisfies:

$$\varphi_{t+1} = \left\{ w \mid V_{t+1}^{E}(w) = V_{t+1}^{U} \right\} \tag{1.6}$$

The interior optimality condition is then found by taking the first order derivative of (4) with respect to $s_t$. This implies:

$$c'(s_t^*) = \beta \delta (\pi e^{-\xi t} + 1) \left[ \int_{\varphi_{t+1}}^{\infty} \left( V_{t+1}^{E}(w) - V_{t+1}^{U} \right) dF(w) \right] \tag{1.7}$$

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12 If $\pi = 2$, then, at $t = 0$, we have $\varphi_0(s_0) = s_0 \cdot (2e^{-10} + 1) = s_0 \cdot 3$.
13 $V_{t}^{U,n}$ denotes the value of unemployment under which the agent is discounting hyperbolic (naive) in the present period where $V_{t+1}^{U}$ denotes the value of unemployment under exponential discounting in the future.
14 Additionally check for solutions at the boundaries of $s_t$ in the model computations.
The hazard rate $h_t$ in period $t$ depends on the true job offer arrival rate, i.e. the search effort $s_t$, as well as on the acceptance rate given the reservation wage $\phi_{t+1}$. Therefore:

$$h_t = s_t (1 - F(\phi_{t+1}))$$  \hspace{1cm} (1.8)

Further, the expected re-employment wage in every period $t$ is given as:

$$E[\ln w | w \geq \phi_t] = \int_{\phi_t}^{\infty} \ln w \, dF(w) \frac{(1 - F(?))}{(1 - F(\phi_t))}$$  \hspace{1cm} (1.9)

Exactly $N$ periods after the benefits drop, i.e. at $T + N$, the search effort becomes stationary as there are no income related shocks anymore. The model is solved from this point of steady state for every preceding period by dynamic programming (see Appendix A.1.1 for details).

Figure A.1 in Appendix A.3 displays simulated hazard rates for the full model as well as a DellaVigna type model\textsuperscript{15} and contrasts them with a standard setting without the extensions of biased beliefs, present bias and reference dependence.\textsuperscript{16} The standard model predicts increasing hazards up until benefit exhaustion from where they stay on a constant level. Both the full and the DellaVigna type model can exhibit a declining initial hazard rate and a resulting negative duration dependence. The biased beliefs allow for some flexibility as it embeds cases where agents search too much as in Falk et al. (2006a) or too little as in Spinnewijn (2015). This thus allows for complex search dynamics as in Potter (2019), depending on the parameter constellations (see Appendix A.1.2). Due to the nature of the exponential decay in (3), the perceived job offer arrival rate gradually adapts to its true value. Thus, over time the reference dependence induced loss aversion becomes more important for the hazard rates. This loss aversion induces an additional decline in the hazard rate as a result of lower transfers in welfare contrary to the standard setting.

1.3 Institutional Setting and Data

1.3.1 The Danish UI System

The Danish unemployment insurance (UI) system is based on a voluntary scheme where benefits are paid to unemployed who have been a member of a UI fund for a minimum amount of one year. Additionally, individuals need to have worked at least 1,924 hours in the preceding three years (approximately one year of full-time work).\textsuperscript{17}

The UI benefit amounts to up to 90% of the prior income, but is capped at 871 DKK per day for 5 days a week as per 2016. This is roughly 4,355 DKK per week for recently employed workers. The UI system was subject to several major reforms over the last decades. Relevant for my purpose is a cut of the maximum UI benefit entitlement from four to two years. This change in the UI system was gradually introduced from 2008 to 2011 by decreasing the entitlement

\textsuperscript{15}A model as in DellaVigna et al. (2017) which includes both reference dependent utility and hyperbolic discounting.

\textsuperscript{16}This is embedded by setting: $\lambda = 0$, $N = 0$, $\pi = 0$, $\xi = 0$, $\beta = 1$.

\textsuperscript{17}To regain access to the benefits after their exhaustion an unemployed individual needs to fulfill this criteria again.
length for new cohorts every half year. After exhausting the UI entitlement, the unemployed enter the welfare system and receive welfare/social security benefits. These amount to roughly 80% of the UI benefit for recipients with children and to 60% for recipients without children. Contrary to UI benefits which are independent of household wealth, these benefits are subject to means testing.

The initial aim of significantly reducing the unemployment rate through the UI reform was not fulfilled and the reform itself was subject to strong opposition from the Danish trade unions (Madsen, 2013). In response the government implemented "acute-measures" that delayed the benefit exhaustion by another six months for long-term unemployed who otherwise would have lost their right to benefits in late 2012. These measures thus affected recipients with up to six months to benefit exhaustion, whereas individuals further away from the end of benefit entitlement were not affected. The acute-measures moved the final implementation of the two year entitlement to people who filed for UI benefits from the beginning of 2011 and onward. Figure A.2 in the appendix shows the transition between pre- and post-reform UI regimes in terms of their maximum benefit entitlement length from the start of the unemployment spell.

1.3.2 Data

The main data source is the DREAM database of the Danish Ministry of Employment, which contains information on weekly income transfers for the whole Danish population. Data is available from mid 1991 and updated frequently. Transfers are distinguished by type and it is possible to calculate the unemployment duration in weeks for every person registered in Denmark as well as the transition into several other labor market states. Information on employment and wages comes from the BFL register of Statistics Denmark, which contains detailed monthly information on received income and hours worked starting from 2008.

The empirical basis for the structural estimation of this paper is two cohorts of insured unemployed individuals who entered the system with full benefits eligibility. That is on the one hand the 2008 cohort, which was the last cohort to receive benefits for 4 years, as well as the 2011 cohort which was the first group of recipients who were faced with only 2 years of benefits after the reform of the UI system.

I exclude individuals potentially affected by pension schemes and other labor market programmes: Individuals older than 49 years and younger than 25 years. I allow for up to four weeks in self-support, without benefits or income from employment, before exit into employment and the job must be held for four consecutive weeks at least. Spells that end with leaving the labor force or transitioning into self-support longer than four weeks are treated as censored observations. Due to the fact that interruptions in the unemployment spell occur rather frequently, I allow the individuals to temporary leave the unemployment category for up to four weeks and still regard the spell as uninterrupted. Further, I exclude individuals who did not hold a regular job prior to the unemployment spell for at least four consecutive weeks.

In order to avoid the misclassification of short breaks with unemployment benefit reception during job-to-job transitions as proper unemployment spells, I exclude spells that last four weeks or less. This follows Lise (2012) who excludes spells that are shorter than three weeks.

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18 Individuals entering the system in 2008 were the last ones receiving benefits for four years and the duration was cut step-by-step until individuals entering the system in 2011 were the first ones subject to two years of benefit entitlement.
1.3. Institutional Setting and Data

I extend this criterion to four weeks due to the nature of the data which gives me sufficient employment information only on a monthly basis.

The structural model of this paper is estimated on bi-weekly empirical hazard rates out of unemployment and the corresponding re-employment wages.\textsuperscript{19} Figure 1.1 plots the raw hazard rates for the 2008 and 2011 cohorts. The hazard rates under the 2008 regime are dropping steadily from right after the beginning of the unemployment spell while stabilizing around week 50. In anticipation of the transition to social security, the exit rates eventually begin to increase with a peak at week 208. For most of the spell and between those two extremes the hazards are essentially flat. The sample size after week 208 is too small to observe more of the dynamics that follow the benefit exhaustion.

\textbf{Figure 1.1:} 14-day unemployment-employment hazard rates for two different benefit regimes with 95% confidence bands, vertical lines indicate benefit exhaustion.

In the 2011 cohort, this transition occurs much earlier and one can thus observe the exit rate patterns also for individuals who have been unemployed for longer than their UI entitlement length. While the hazard still decreases steadily after entering unemployment, it does so more slowly than in the pre-reform regime and settles on a higher level. Nevertheless, I observe that both cohorts show stable exits from the same time around week 50.

As expected, the hazard rates show a distinct spike at the point of benefit exhaustion and decline sharply afterwards, falling roughly to the same level as the 2008 cohort. It is striking that the decline of the hazard rates at the end of UI entitlement is much sharper than at the beginning of the UI spell. With roughly 16 weeks after the transition into the welfare system the unemployment exits also stabilize considerably faster than before the reform, roughly 16 weeks after the transition into the welfare system.

The average bi-weekly re-employment wages, plotted in logs in Figure 1.2, are a relatively noisy measure with high variance. For both regimes, the re-employment wages are falling slowly but steadily right from the start of the unemployment spell with wide confidence bands,

\textsuperscript{19}See Appendix A.1.3 for estimation of the hazards and re-employment wages.
especially after the two year mark. The 2008 wages become rather stable after two years, even though the second half shows high volatility in the data.

![Graph showing 14-day re-employment wages for two different benefit regimes in Denmark with 95% confidence bands, vertical lines indicate benefit exhaustion.]

Figure 1.2: 14-day re-employment wages for two different benefit regimes in Denmark with 95% confidence bands, vertical lines indicate benefit exhaustion.

Table A.1 in Appendix A.2 shows the basic descriptive statistics for both cohorts that are used in the structural estimation. It is evident that the two cohorts are not fully comparable, yet the differences are generally small. I nevertheless look at estimates of the hazard rates and re-employment wages controlling for background characteristics. Figure A.3 in Appendix A.3 shows that there are no qualitative differences between estimations that control and those that do not control for observables. Unfortunately, both cohorts are affected by different business cycle conditions due to the onset of the great recession and elevated unemployment rates in the late 2000’s. This may very well affect the hazard rates in general, though business cycle effects on unemployment-to-employment hazard rates seem to be rather small in comparison to e.g. the general UI benefit level (Bover et al., 2002). Further, I expect business cycle effects to not have a major impact on the underlying behavioral biases, but rather only on the general level of the hazards. In order to assess whether this difference in business cycle conditions has a major effect on the validity of the estimated parameters in the later sections, I run an out-of-sample consistency check on the 2010 cohort which was subject to roughly similar macroeconomic conditions as the 2011 cohort (see Section 4.3).

1.4 Estimation

To solve the model and to estimate the parameters of interest, I need to establish some further assumptions. Following Paserman (2008) and DellaVigna et al. (2017), the search cost function takes power form with \( c(s_t) = \frac{k_s t^{1 - \gamma}}{1 + \gamma} \), where \( \gamma \) represents the inverse search effort elasticity with

\[\gamma\]
respect to the individuals net valuation of employment. Utility is a simple log function, $v(b) = \ln(b)$. Following the common practice of fixing delta at pre-specified values (Paserman, 2008), I set the exponential two-week discount factor at $\delta = 0.995$. It is not separately identifiable from other estimated parameters like the search costs $k$ and features of the benefit structure, i.e. $\psi$. The weight on the gain-loss utility $\eta$ is fixed at 1 as it is not separately identifiable from loss aversion. The wage offer distribution is assumed to be a time invariant log-normal distribution and independent of the search effort $s_t$. It depends only on the distributional mean $\mu_w$ as well as standard deviation $\sigma_w$ and can be recovered from the distribution of accepted re-employment wages that are observed in the data (Flinn and Heckman, 1982).

In some of the estimations I assume unobserved heterogeneity in the search costs. With two types of unemployed this adds the extra parameters $p_l$, $k_l$ and $k_h$ to the estimation. The former of these is the population share of the low type, whereas the latter ones specify the search cost level for low and high types respectively.

The bi-weekly pre-unemployment income is set to the rounded bi-weekly median income of the individuals who became unemployed and claimed benefits, that is 13,300 DKK for 2011 and 13,500 DKK for 2008. The median claimed UI benefit is 7,660 DKK in 2011 and 7,480 DKK in 2008, both corresponding to the maximum available benefit level in the respective years.\(^{21}\)

1.4.1 Method of Simulated Moments

Parameters are estimated using the method of simulated moments approach (MSM), as described in McFadden (1989) and DellaVigna (2018). MSM is a minimum-distance estimation technique that allows to estimate model parameters in settings where maximum likelihood is not feasible. It replaces the likelihood function with the distance between empirical and parametric moments and minimizes this objective function to estimate the parameters of interest.

The parametric moments are the hazard rates for every period obtained from the model, which was described in Section 2, while the empirical moments are the hazard rates out of unemployment as well as the average re-employment wages. The vector of model parameters is then estimated by:

$$\hat{\theta} = \arg\min_{\theta} (m(\theta) - \hat{m})^T W (m(\theta) - \hat{m})$$  \hspace{1cm} (1.10)

where $\hat{\theta}$ is the minimum-distance estimate of the underlying parameters of the model, i.e. the magnitude of the loss aversion $\lambda$, the adjustment speed of the reference point $N$, the hyperbolic-discounting parameter $\beta$, the parameters defining the bias in the beliefs about the subjective job finding probability $\pi$ and $\xi$, the search costs $k$ and their curvature $\gamma$, the share of the benefits that is payed out as welfare transfers $\psi$ as well as the mean and standard deviation of the wage offer distribution $F(w)$. The moments generated by these model parameters are $m(\theta)$, the empirical moments are $\hat{m}$ and the estimator is weighted by a weighting matrix $W$.

\(^{21}\)All transfers and incomes are in 2011 price levels. This corresponds to a bi-weekly income of roughly 2,450 USD for 2011 and 2,480 USD for 2008. The UI benefits correspond to roughly 1,425 USD for 2011 and 1,395 USD for 2008.
Given this, the minimum-distance estimator (10) achieves normality with variance:

\[
V(\hat{\theta}) = \frac{(\hat{G}'W\hat{G})^{-1} \cdot (\hat{G}'W \text{Var}(\hat{m})W\hat{G}) \cdot (\hat{G}'W\hat{G})^{-1}}{N}
\]  

(1.11)

where \( \hat{G} = N^{-1} \sum_{i=1}^{N} \frac{\partial m_i(\theta)}{\partial \theta} |_{\hat{\theta}} \). Choosing the weighting matrix \( W \) as the inverse of the variance-covariance matrix may lead to undesirable finite sample properties and instability in the parameters estimates (Altonji and Segal, 1996). I therefore follow the common practice of choosing the inverse of the diagonal of the variance-covariance matrix for the weighting.

In the estimation of the parameters I supply 384 bi-weekly moments from the empirical data. I start the estimation on a grid of 300 random starting points and additionally restrict the bounds for most parameters to a wide, yet economically reasonable interval. I am aware that under the highly non-linear structure of the minimization problem I can never guarantee to find the true global minimum, but I am confident that the number of starting points is sufficient, especially as the best solutions all tend to converge to roughly the same values.

1.4.2 Identification

Due to the chosen estimation strategy all parameters are identified jointly in the MSM process. The identification thus depends on a rich supply of moments and sufficient variation within those. Just like in DellaVigna et al. (2017) and Paserman (2008) I can roughly classify the central sources of identification of the parameters.

The inverse of the search cost elasticity towards utility gains of becoming employed, i.e. the search cost curvature \( \gamma \), is identified by the sharpness of the hazard rate reactions towards changes in the income. This happens at the start initial unemployment spell or the transition into social security at week 104 or 208. The search costs \( k_i \) are identified by the path of the hazard rates over the spell as well as by their general level. As argued by Paserman, the short-run discounting parameter \( \beta \) is identified by the relative difference between search efforts and the distribution of accepted re-employment wages. The parameter has no effect on the wages which are a future payoff (and thus independent of \( \beta \)). However, it directly influences the search decisions which lead to costs in the present. The belief parameters \( \pi \) and \( \xi \) are identified by dynamics of the hazard rate at the beginning of the unemployment spell relative to the paths after benefit exhaustion. The loss aversion parameter \( \lambda \) is identified by the dynamics around income shocks, especially around the point of transition into social security. Lastly, the time until the search effort becomes constant after the transition into social security identifies the adjustment speed of the reference point \( N \).

1.4.3 Main Results

Figures 1.3 and 1.4 show the performance of the full behavioral model that includes all biases introduced in Section 2. Table A.2 in Appendix A.2 displays the parameter estimates in column

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22 Under some specifications specific parameters are not well identified. In order to invert \( \hat{G}'W\hat{G} \) I drop these parameters from \( \hat{G} \). By doing this I am able to still calculate standard errors for all other parameters.

23 This then simplifies the variance to \( V(\hat{\theta}) = N^{-1}(\hat{G}'\text{Var}(\hat{m})\hat{G})^{-1} \)

24 88 hazard moments and 88 re-employment wages from 2011 as well as 104 hazard moments and 104 re-employment wages from 2008
1. The model exhibits a strong data fit over both benefit regimes. The simulated hazard rates are close to the empirical moments for the most part and the distinct difference between the 2008 and 2011 cohorts is captured well. The spike at benefit exhaustion is not fully matched, though one of the reasons for the higher hazards here could be some sort of exploitation of the UI system by delaying starting times for newly found jobs. Boone and van Ours (2012) find some evidence for such subsidized leisure with Slovenian data. In addition, there is slight overshooting on the 2011 cohort at the beginning of the unemployment spell.

![Figure 1.3: Empirical and estimated hazard rates for two benefit regimes.](image)

Up until benefit exhaustion the re-employment wages are captured well by the estimated model. As these wages are somewhat noisy and volatile after this point, it is hard to draw proper conclusions with respect to the data fit here. The modeled re-employment wages are nevertheless able to capture the general level adequately and fall within the confidence bands of the empirical moments.

As pointed out in Paserman (2008), the proper identification of the model ultimately depends on the confidence intervals around the estimated parameters and whether these are sufficiently narrow. Throughout column one in Table A.2 there are low standard errors on the estimated coefficients which hints at a proper identification of the model. The loss aversion parameter $\lambda$ is estimated at a higher magnitude than the usual estimates from prior experimental studies (e.g. Tversky and Kahneman, 1991; Tversky and Kahneman, 1992). However, Farber (2008) also finds particularly high loss aversion for a (stochastic) reference point in a labor supply setting. Compared to the estimates of DellaVigna et al. (2017), the loss aversion I estimate is roughly twice as high and the reference point adapts approximately twice as fast. This implies that losses are felt to a stronger degree, but are in turn not felt as long as DellaVigna et al. estimate on Hungarian data.

The perceived job offer arrival rate at the beginning of the unemployment spell is about four and a half times as high as the objective job finding probability. This hints at a substantial degree of biased beliefs and overconfidence in job search where individuals expect to enter employment faster then they eventually will. The bias gradually adjusts over time by eroding...
the overly optimistic confidence in one’s ability to secure a job offer, similarly to Falk et al. (2006a,b). The extent of the bias in the beliefs about the job finding rate is not too far off from Spinnewijn (2015) who finds that unemployed expect to enter employment more than three times as fast as they actually will. By the end of the second year it has approximately adapted to the true job offer arrival rate. The fast speed of learning is especially apparent at the beginning of the unemployment spell as the bias in the beliefs has already shrunk to one half of its initial magnitude after roughly 2.5 months. Further, I estimate the (quasi-)hyperbolic discounting parameter $\beta$ at 0.89 which matches the estimate of Paserman (2008) on NLSY data for high wage samples.\footnote{Note that the previous wage is significantly higher here than in Paserman’s NLSY sample.} This modest, yet still significant present bias is also in line with Falk et al. (2018) who find relatively high levels of patience for populations in Northern-Europe as well as Augenblick et al. (2015) who estimate a population present bias parameter of $\beta = 0.9$. The unemployment duration elasticity with respect to the entitlement length of the estimated model is 0.53, whereas the elasticity with respect to the benefit level is 0.98. Both of these fall well in the range of the estimated elasticities for Europe and the United States (Schmieder and Von Wachter, 2016).

In order to further examine the performance of the model and to assess whether the different business cycle conditions between the cohorts influence the model and parameter validity, I perform an out-of-sample consistency check. This is done on the cohort of individuals that entered the system in the second half of 2010. These individuals were subject to roughly similar business cycle conditions as the 2011 cohort and were directly affected by the extension of the benefit period in their last six months before benefit exhaustion. The extension was likely not expected by benefit recipients while entering unemployment in the first place (see Section 3.1). I thus simulate the model under the assumption that individuals expected a two year benefit entitlement ex-ante, but get updated about the extension during their last half year of the UI period. As it is not clear at what point of their spell individuals became aware of this
1.4. Estimation

extension, I am explicitly not modelling hazard rates from week 79 to 104. The performance on the empirical hazard rates for this cohort can be observed in Figure A.4 in Appendix A.3. The model performs relatively well, with an exception of slight initial overshooting, just as in other versions. In particular, the onset of increasing exit rates in anticipation of the ex-ante expected benefit exhaustion at week 104 is captured, as well as the shifted spike at the time of the actual benefit exhaustion.

1.4.4 Relative Model Fit

To show that the full model leads to improved data fit over prior behavioral frameworks, I estimate three alternative models on the same data. This exercises illustrates the importance of the introduction of biased beliefs as the full model outperforms alternative frameworks strongly. This holds for the fit to the exit rates and re-employment wages as well as on matching the expected unemployment duration of the individuals in the estimation pool. The alternative frameworks estimated here include: 1) A standard model with 2-type heterogeneity in the search costs $k$, 2) a reference dependence DellaVigna et al. (2017)-type model (incl. present bias) without heterogeneity, 3) the latter model with 2-type heterogeneity in the search costs.

The performance of the reference dependence model without search cost heterogeneity is shown in Figure 1.5 (a) as well as column 2 in Table A.2. This framework struggles to fit the data, which is not only apparent in the data fit measure, but also in the dynamics of the hazard rates. These over- or undershoot the empirical exit rates over large portions of the unemployment spell in contrast to Figure 1.3. Further, re-employment wages are essentially flat, induced by a very low wage offer standard deviation. While this set-up reveals a substantial degree of present bias on Hungarian data, I estimate a (quasi-)hyperbolic discounting parameter $\beta$ that is not significantly different from one. The loss-aversion parameter $\lambda$ is again quite high and the adjustment speed of the reference point is considerably slower than in the estimates of DellaVigna et al..

The model needs this slow adjustment speed in order to fit the initial negative duration dependence which leads to slowly falling hazard rates over the first year. This however comes at the cost of not being able to capture the sharp decline in the exit rates after benefit exhaustion in week 104 as the adjustment takes too long. The full model in contrast can fit both of these patterns. The bias in the job offer beliefs allows to fit the initial slower decline in the exit rate while keeping a fast adjustment speed of the reference point. This is due to the interplay of both reference dependence and biased beliefs (see Appendix A.1.2).

The standard model with 2-type unobserved heterogeneity can be found in column 3 in Table A.2 as well as in Figure 1.5 (b). The model requires a very low search cost estimate for the low type in order to fit the initial decline in the hazard rates. In this setting low cost types leave the UI system particularly fast. While the hazards are corresponding well to the 2010 cohort, this fact leads to a clear divergence from the data in the 2011 cohort which is already apparent from around week 45. Most importantly, here the hazards become practically flat for agents on social welfare which does not fit the spike with sharp decline at after benefit exhaustion. Further, re-employment wages become flat as well after roughly one year of slight decline for both cohorts. Overall, the data fit measure confirms this poor performance of the model.

A reference dependence model with 2-type unobserved heterogeneity is shown in Figure 1.5 (c) as well as in column 4 in Table A.2. The comparison to this model is especially interesting.

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26 The estimate for $N$ is additionally not well identified in this setting.
(a) Reference dependence model

(b) Standard 2-type model

(c) Reference dependence 2-type model

Figure 1.5: Empirical estimated hazard rates and (log) re-employment wages for two benefit regimes in alternative model frameworks.

as the full model extension with biased beliefs naturally increases the degrees of freedom in the minimum-distance estimation due to the added parameters. By allowing for 2-type heterogeneity in the reference dependence model, I equate the degrees of freedom to those
1.4. Estimation

of the full model. The reference dependence model has a strong fit on both cohorts with an exception of the benefit exhaustion spike for the 2008 regime. Overall, the data fit increases marginally in contrast to the full model. The reference dependence parameter $\lambda$ is estimated at a lower magnitude that is closer to the estimates of DellaVigna et al. on Hungarian data, albeit a bit smaller. On the other hand, I estimate an adjustment speed $N$ that is about three weeks slower. The re-employment wages strictly decrease up until benefit exhaustion after which they stay on a roughly constant level.

Looking closer at the estimates, two issues appear: First, the welfare fraction of the original benefits $\psi$ falls to an extremely low level of around 33%. This induces the stark increase in the hazards prior to benefit exhaustion as well as the strong drop afterwards. Further, following DellaVigna et al. (2017) who also find strong model performances under unobserved heterogeneity, I look at differences in the dynamic selection throughout the UI spell. I regress the individual unemployment duration on a rich set of observable characteristics and predict the expected duration in unemployment for all individuals.\footnote{The unemployment duration is estimated on observables containing age, gender, having kids, being married, ethnicity (dane/western/nonwestern), region of residence, living in city, education (compulsory/secondary/tertiary), pre-unemployment wage (log), prior occupation as well as general labor market experience and generates a $R^2$ of 0.05.}

Figure 1.6: Expected unemployment duration of individuals exiting at $t$ for observed data with 95% confidence bands and simulated models including type-shifts.

When plotting these values over time together against the simulated counterparts in Figure 1.6 it becomes apparent that the reference dependent model with two cost types needs a high degree of heterogeneity and type-shifts to fit the dynamics of the data. These strong type shifts are induced by the wide differences in search-costs for low- and high type. While the predicted durations do not exhibit a constant pattern over time, the path is nevertheless relatively flat and the full model without type-shifts falls close. I conclude that the good fit of the reference dependence model with 2-type unobserved heterogeneity is mainly driven by the high degree of heterogeneity relative to the data, even though only a part of the selection is observed.
To sum up, this Section has shown that the full model exhibits a better performance than the reference dependent model proposed by DellaVigna et al. (2017). It leads to a better fit to the empirical hazard rates and re-employment wages or to a more realistic matching of the expected unemployment duration, even though minor dynamic selection is apparent in the data.

### 1.5 Policy Evaluation

The purpose of this section is to evaluate hypothetical changes in the UI system. This is especially interesting when considering the before-mentioned cognitive biases and the implications these have for policy outcomes. Focusing on a single behavioral bias or omitting a crucial one can lead to worse predictive power as seen above. Additionally, it can lead to ill-directed policy advice as not all of the major divergences from standard theory are captured. It is therefore a crucial and pragmatic approach to employ a framework that models all major behavioral biases and leads to better empirical predictions than more sparse alternative settings (Chetty, 2015).

For the evaluation of policies I use the full model specification with the complete set of added behavioral extensions, all of which might have crucial implication for policy design. For example, under reference dependence, a hypothetical multi-step UI system will trigger loss aversion multiple times over the unemployment spell. Under present bias and biased beliefs, some systems that change the level of costs incurred in the present or future periods, such as sanctions or re-employment bonuses, might have implications that diminish self-control problems. Following the main evaluation, I show how that policy prescriptions can vastly differ if not all biases are accounted for.

I consider four main outcomes of interest: 1) The ex-ante expected duration in unemployment, 2) the ex-ante expected government expenditure per unemployed individual in terms of UI benefits and welfare transfers, 3) the fraction of individuals entering the welfare system, 4) the individual welfare of the unemployed. For the latter, the estimated degree of present bias induces dynamic inconsistency as plans, actions and expectations differ over time. I follow Paserman (2008) who evaluates the utility of the long-run selves of the job seekers, just like in O’Donoghue and Rabin (2001). This is done by evaluating the decision paths of a present biased agent using normal exponential discounting. As Paserman points out, this welfare measure resembles a non-employed voter deciding about possible changes of the UI system. I evaluate all of the following policies in the next section by comparing them against a benchmark setting which resembles the policy set-up in place since 2011.\(^{28}\)

### 1.5.1 Hypothetical Policies

**Benefit cut:** This is standard in the policy evaluation literature and simply cuts the benefit level \(\bar{b}\) over the whole range of UI entitlement by a certain amount so that the unemployed job seekers receive less benefits. Similar policies that involve cuts or increases of the benefit level have been studied widely (Carling et al., 2001; Lalive et al., 2006; Eugster, 2015). Evidence

\(^{28}\)The expected duration is calculated as the integral under the estimated survival curve and the expected government expenditure is obtained by multiplying the respective point estimate of the survival curve with the transfer for every period and then summing it up afterwards.
generally points to positive effects of benefit cuts on unemployment duration, i.e. a lower (higher) benefit level decreases (increases) the unemployment duration.

*Entitlement-length cut:* As another standard policy I evaluate a reduction of the UI entitlement length. This is a cut of the duration the benefits $\tilde{b}$ paid to the unemployed before transition into social security. The shortening and extension of benefit entitlement were also subject to prior examination (Card and Levine, 2000; Van Ours and Vodopivec, 2006; Lalive, 2008) and the evidence leads to roughly similar conclusions as benefit cuts.

*Re-employment bonus:* I consider a re-employment bonus that is paid out as a lump-sum transfer if employment has been found during the first year of unemployment. This makes obtaining a job more attractive relative to continued unemployment and thus increases search effort. Reference dependence also has an additional effect here, as the bonus is evaluated as a gain during employment until reference point adaption. The literature on re-employment bonuses is not conclusive as evidence has shown positive effects on re-employment speed (Woodbury and Spiegelman, 1987; Anderson, 1992; Decker and L'Leary, 1995) as well as null-effects in more recent settings (Van der Klaauw and Van Ours, 2013).

*Effort monitoring and sanctions:* Denmark recently introduced a new job search monitoring scheme that involves electronic *joblogs*, i.e. weekly recording of job applications on an online platform which is a requirement for UI benefits receipt. 29 I thus evaluate a related scheme that introduces a search-effort requirement $s_{ij}$ for the agents under which I assume perfect monitoring. If this requirement is not met, benefits in the next period will be reduced to the level of social-security. An impatient or present biased individual might not comply with these requirements as sanctions are only imposed in the future. 30 Paserman (2008), Boone et al. (2007) and Van den Berg and Van Der Klaauw (2019) find positive effects of similar policies, however others come to less optimistic conclusions when considering imperfect monitoring schemes (e.g. Cockx et al., 2014) or potential substitution of search channels (Van den Berg and Van der Klaauw, 2006). In the implementation I disregard monitoring costs as the marginal costs of monitoring a benefit recipient are usually negligible relative to the overall cost of the UI system. 31

*Front-loading the benefit path:* At last, I evaluate a relatively recent idea of re-designing the UI system by front-loading the benefit path. DellaVigna et al. (2017) propose an UI system that pays higher benefits at the beginning for $T_1$ periods as well as lower benefits for the remaining $T - T_1$ periods. The total amount available over the whole UI entitlement length stays fixed. This would change the system from $\tilde{b}' = \tilde{b}'' = \tilde{b} > b$ (the initial case) to $\tilde{b}' > \tilde{b} > \tilde{b}'' > b$ where $\tilde{b}'$ are the higher initial benefits and $\tilde{b}''$ the final lower ones. For simplicity I introduce this extra step at exactly half of the entitlement length. The new step after $T_1$ exploits the reference dependence of job seekers to induce higher search efforts around the additional benefit drop. As it is straight-forward, I additionally evaluate benefit systems that add further steps over time. I still keep the total possible amount of benefits payed out over time fixed. Note that,
the more steps introduced in the hypothetical UI systems, the smaller the transfer decreases between the different tiers. Losses might thus not be very salient under systems with many tiers (Bordalo et al., 2012). As far as evidence on front-loading goes, Lindner and Reizer (2016) have shown positive and expenditure neutral effects on the speed of re-employment.

1.5.2 Evaluation

The outcomes of changing the benefit level can be seen in Figure A.5. One easily observes that a cut of the status-quo benefit level leads to a shorter expected duration in unemployment. This measure is decreasing with the level of the benefit cut, in line with prior evidence. The increased speed of exit into employment is due to higher hazards, induced by a lower value of unemployment relative to employment. On the other hand, the effect of loss aversion weakens around the transition into welfare benefits as the difference between benefits and welfare transfers shrinks. Further, reductions of the government expenditure as well as the share of the people that would exhaust their right to UI benefits can be observed. While these effects of a benefit cut are positive, the cut also has consequences for the long-run utility of the individuals. As one would expect, a decrease of the benefit level lowers the long-run utility even though employment is found faster.

Similar effects can be found when looking at the outcomes of a change of the entitlement length of UI benefits as shown in Figure A.6. A shorter entitlement length shifts the benefit exhaustion spike to the left and raises the hazards in anticipation of this point. The effects on government expenditure and expected duration in unemployment are weaker than under a change of the benefit level. However, there is a strong effect on the share of individuals exhausting their benefits. Interestingly, even though a shortening of the entitlement length means that the point of benefit exhaustion approaches faster, the amount of people reaching this point decreases. The reactions on the long-run utility are practically flat around the status-quo, as individuals expect to leave unemployment faster than actual rate and discount the future hyperbolically. The effects of shortening the entitlement length nevertheless decreases the long-run utility slightly.

Next I turn to the introduction of a re-employment bonus whose outcomes can be found in Figure A.7. The hazard paths are slightly raised over the period the bonus is payed and drop sharply once it is not available anymore. The difference between benchmark hazards and the ones under the bonus regime increases strongly the closer one comes to the end of the first year. This a possible implication of the reference dependence and the higher gains once employment is found. When looking at the policy outcomes, one can observe roughly null-effects on unemployment duration which is in line with Van der Klaauw and Van Ours (2013). As expected, government expenditure and long-run utility increase under these bonus schemes.

The effects of introducing a search effort monitoring scheme are highly dependent on the chosen minimum requirement for exerted search effort, as evident in Figure A.8. Up until a requirement of roughly $s_t = 0.2$ unemployed are not affected as search effort is in any case high enough even under the absence of monitoring. Therefore, there is virtually no effect on the policy outcomes for low levels of $s_t$. If $s_t$ is however chosen to be larger than 0.2, positive and Pareto-improving effects start to materialize. In this case, present bias induced self-control problems are partly overcome as additional sanctions are imposed on future periods in the
1.5. POLICY EVALUATION

This raises the hazard rates for individuals in unemployment longer than 36 weeks, both by increasing search efforts and the job offer acceptance rate. Before this point individuals search enough to not be threatened by the sanctions. The increased hazard rates last up to the point of benefit exhaustion and thus reduce expected duration and government expenditure strongly. Interestingly, and just as in Paserman (2008), this also increases the individuals long-run welfare significantly as the search effort requirement works as an efficient commitment device. This moves the behavior closer to the long-run self’s optimal behavior.

At last I examine the effects of the front-loaded UI system, which is a change from a one-tier to a two-tier benefit path with $T_1 = 52$ weeks. Figure A.9 displays the effect of this policy change. The drop in the benefits after one year induces an additional spike in the hazards as loss aversion kicks in. A reduction in the difference between prior wages and benefits at the start of the unemployment spell dampens the hazards initially. This is also in effect at benefit exhaustion where the the difference between unemployment and welfare benefits shrinks. It is striking that long-run utility is increasing with the level of the first tier benefits, while the expected duration as well as amount of people entering social security is decreasing. This increase in the first tier benefits is almost expenditure-neutral on the government side and even decreasing slightly over certain intervals. Higher levels of $\bar{b}'$ eventually lead to increases in the government spending overall. Thus, this change of the benefit path seems to lead to Pareto improvements over specific intervals. The effects are nevertheless relatively weak in comparison to a standard cut in the overall benefits. Multiple tier system as well as systems that decrease the benefits monthly over the spell show comparable effects as evident in Figures A.10 to A.12. Here additional and weaker spikes are observed. Interestingly, the government expenditure shrinks the more tiers are added in the system. One needs to be aware that with multiple tiers the discrepancy between each of them also decreases. Additionally, the effect of the loss aversion might weaken due to a lower salience of the losses as mentioned earlier.

In conclusion, a change to a front-loaded benefit structure or a scheme with search effort monitoring and related sanctions might lead to desirable changes on all considered measures. This is under the assumption that the model represents the actual data generating process well enough. But how do these policies compare? In order to evaluate this, I look at policies that roughly lead to the same change in expected duration, which can be seen in Table A.3 in Appendix A.2. The comparison of the different policies reveals that, for the same impact on the expected duration, the search efforts monitoring scheme is the most effective. It reduces the government expenditure and increases the individual welfare of the unemployed job seekers more than other polices. Further, within the set of front-loading regimes, there are also differences among the level of chosen tiers. Here, no single policy is uniformly more effective than the other ones. That is, while the 3-tier system is most effective in increasing individual welfare it is also the least effective in terms of reducing government expenditure and the opposite holds for the gradual front-loading scheme.

1.5.3 Importance of Model Selection

In order to assess the importance of model selection for policy evaluation, I evaluate the policy outcomes across across the alternative models. This also highlights that omitting important biases can lead to differing outcomes and therefore to potentially ill-directed policy advice. The outcomes are presented in Table A.4 for the various front-loading regimes as well as Table
A.5 for re-employment bonuses and search effort monitoring schemes. For the front-loading policies, there are evident differences in magnitudes between the models along the outcome dimensions of expected unemployment duration and individual welfare. More strikingly are further differences in sign on the government expenditure. The expenditure reduces slightly in most of the settings for both full and reference dependent models without heterogeneity. It further increases across the board in the standard and reference dependent model with unobserved heterogeneity, partly up to 28 percent. In terms of the long-run utility, the reference dependent model in column 2 sticks out by having relatively large effects. Other models are affected to lower degrees by a change of the benefit path.

For the re-employment bonus, there are differences between the models with and without unobserved heterogeneity. The models with two cost types show somewhat shrinking government expenditure. The models without heterogeneity however exhibit clear and sizable increases in spending on the UI system by introducing the bonus. Vast differences between the full model and the other frameworks are evident under search effort requirements. Effects under the full specification are moderate in size yet Pareto-improving and non-existent for minimum requirements of $s_t \leq 0.2$. For all other frameworks however, effects are large in magnitude and additionally welfare reducing. The search effort requirement thus does not seem to be a well functioning commitment device in order to overcome self-control problems under these frameworks. This can be anticipated for the models in column 2 and 3 as present bias was either not modelled or particularly weak. It is however surprising for the 2-type reference dependence model in column 4 with modest self-control problems under which long-run individual utility decreases the most.

The sometimes vast differences in magnitude on the policy outcomes as well as differences in sign confirm that ex-ante model based policy advice is critically dependent on the model choice. Even if two models exhibit comparable data fit, there might be important divergences between them if one of the frameworks lacks the inclusion of an important behavioral feature we repeatedly observe in the real world.

1.6 Conclusion

In this work I combined approaches of prior behavioral job search frameworks in one tractable model which comprises several cognitive biases. In addition to this, I introduced a model extension that approximates learning about the true job offer arrival rate for individuals who are overconfident about their job finding capabilities. I estimated the model with a minimum-distance approach to recover the structural parameters. By doing so I was able to achieve a strong and increased fit to the empirical hazard rates on the Danish labor market in contrast to alternative frameworks used in the prior literature. In particular, the combination of reference dependence and the extension of decaying overconfidence in job search led to strong improvements. These were most notable in capturing the patterns of negative duration dependence and sharp declines of exit rates after transitions into the welfare system.

Following the estimations, I used the full behavioral model to evaluate hypothetical changes of the Danish UI system ex-ante. I showed that the alternative models, which do not include all three behavioral biases, offer policy advice that differs from the full model. This reaffirms the importance of model selection and clear consideration of behavioral triggers that might be at play in economic situations. In terms of policy outcomes, I tested the effects of
introducing multiple tiers in the UI benefit path as the reference-dependence induced loss aversion might have positive implications in this setting. It turns out that this front-loading to a multiple-tier system, ranging from two tier to a gradual monthly benefit decrease, reduces government expenditure and unemployment duration. It additionally increases the long-run utility of the individuals, thus this policy appears Pareto-improving. To further address self-control problems of present biased and overconfident individuals, I tested the effects of a re-employment bonus and the introduction of a search effort monitoring scheme. Both policies are able to assist in overcoming self-control problems, in fact raising the long-run utility of the job seekers and reducing expected duration in unemployment. However, only the search effort monitoring scheme also reduces the overall spending on the UI system. To assess which of the Pareto-improving policies promises to be most effective, I compared this to front-loading systems of similar impact on the expected unemployment spell duration. This showed that the monitoring regime is somewhat more effective.

While the results of the policy evaluation exercise appear promising, the results need to be viewed with some caution. This is due to the search models nature as a partial equilibrium framework. The setting lacks the government side that finances UI benefits through taxation as well as firms that hire workers and offer wages. Further, the welfare criterion only concerns the individuals ex-ante long-run utility in the UI system before actually entering it. An unemployed job-seeker might as well feel very different about the introduction of a multiple-step benefit system, especially once she has been unemployed for a longer period of time and is potentially facing lower benefit levels. Further work should therefore shed more light on tractable variations of welfare criteria in behavioral settings as well as possible general equilibrium applications of behavioral job search frameworks.

Acknowledgements

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1.7 References


1.7. REFERENCES


A.1 Derivations and Estimation

A.1.1 Solving the Model

The model is solved from a point of steady state, i.e. a point where search effort and reservation wages are stationary indefinitely. In the given set-up this is a certain point after the final benefit exhaustion where 1) the reference point has fully adjusted to the social welfare transfer level b and 2) the beliefs about the subjective beliefs about job offer arrival rate have fully adjusted to the objective job offer arrival rate. From this point it is possible to solve the model first numerically for the steady state and then, taking the steady state solution as given, by backwards induction for all previous periods.

To start, recall that the value of unemployment in period \( t \) is given by:

\[
V^U_t = \max_{s_t} \left[ u(y_t|r_t) - c(s_t) + \beta \delta \left( \phi_t(s_t) \int_{\phi_{t+1}}^{\infty} V^E_{t+1|t+1}(w) - V^U_{t+1} \right) \right] \tag{A.1}
\]

Similarly, the value of employment:

\[
V^E_{t+1|t+1}(w_{t+1}) = u(w_{t+1}|r_{t+1}) + \delta V^E_{t+2|t+1} = \frac{u(w_{t+1})}{1 - \delta} + \eta \sum_{i=1}^{N} \delta^i [v(w_{t+1}) - v(r_{t+1})] \tag{A.2}
\]

Under this the optimal search effort was characterized by:

\[
c'(s_t^*) = \beta \delta (\pi e^{-\xi t} + 1) \int_{\phi_{t+1}}^{\infty} V^E_{t+1|t+1}(w) - V^U_{t+1} \, dF(w) \tag{A.3}
\]

While unemployed, the individual accepts all wage offers \( w_{t+1} \) where \( V^E_{t+1}(w_{t+1}) \geq V^U_{t+1} \), therefore the reservation wage \( \phi_{t+1} \) is characterized by \( V^E_{t+1}(\phi_{t+1}) = V^U_{t+1} \). This hold true regardless of the period we are in, thus we can simply use \( V^E_t(\phi_t) = V^U_t \) in order to keep the notation a bit simpler. We can start from here:

\[
\frac{v(\phi_t)}{1 - \delta} + \eta \sum_{i=1}^{N} \delta^i [v(\phi_t) - v(r_t)]
\]

\[
= u(y_t|r_t) - c(s_t) + \beta \delta \left( \phi_t(s_t) \int_{\phi_{t+1}}^{\infty} V^E_{t+1|t+1}(w) - V^U_{t+1} \right) \tag{A.4}
\]

Now we can insert the reservation wage condition \( V^U_{t+1}(\phi_t) = V^E_{t+1}(\phi_t) \) for the next period:

---

Note that, while the adjustment of the reference point happens always \( N \) periods after the benefit exhaustion, the beliefs about the job offer arrival rate are only adjusting asymptotically with \( \lim_{t \to \infty} (e^{-\xi t}) = 0 \). In practice, most choices for \( \xi \) lead to approximate adjustment in reasonable time. I consider adjustment when \( |\phi_t(s_t) - s_t| < \epsilon \) with \( \epsilon = 0.0001 \).
We know that the decision problem for the job seeker anymore. 34 Then:

\[
\frac{v(\phi_t)}{1-\delta} + \eta \sum_{i=1}^{N} \delta^i [v(\phi_t) - v(r_i)]
\]

\[
= u(y_t | r_t) - c(s_t) + \beta \delta \left[ \phi_t(s_t) \int_{\phi_t}^{\infty} V^E_{t+1}(w) - V^E_{t+1}(\phi_{t+1}) dF(w) + V^E_{t+1}(\phi_t) \right] \tag{A.5}
\]

We see now that \( \phi_t \) is the only unknown and can thus easily be determined numerically with root-finding routines common to most numerical computing environment such as Matlab. Once we have \( \phi_t \), we can then solve for \( V_t^U \) and \( s_t \), having this we can solve for \( \phi_{t-1} \) and continue in this backwards induction fashion until the initial period is reached.

With these characterizations of \( \phi_t \) and \( s_t \) for every period of the model we get \( h_t = s_t(1 - F(\phi_{t+1})) \) as well as \( E[\ln w | w \geq \phi_t] = \frac{\int_{\phi_t}^{\infty} \ln w dF(w)}{1 - F(\phi_t)} \) and have thus fully solved the model.

### A.1.2 Biased Beliefs and Search Effort

Let \( \Delta_t = \int_{\phi_t}^{\infty} V^E_t(w) - V^U_t dF(w) \). Then solving the optimality condition for \( s_t^* \) we get:

\[
s_t^* = c'^{-1} \left[ \beta \delta (\pi e^{-\xi t} + 1) \Delta_{t+1} \right]
\]

We know that \( c'^{-1}() > 0 \) as the cost function itself \( c() \) is strictly increasing, and therefore the inverse has to be increasing as well (Binmore, 1982). Search effort is then increasing in \( \pi \) if:

\[
\frac{\partial}{\partial \pi} \left[ \beta \delta (\pi e^{-\xi t} + 1) \Delta_{t+1} \right] > 0
\]

Re-arranging:

\[
\beta \delta e^{-\xi t} \Delta_{t+1} + \frac{\partial}{\partial \pi} \beta \delta (\pi e^{-\xi t} + 1) > 0
\]

It is important to note that the reference point in \( V^E_{t+1}(w_{t+1}) \) is a future event and only enters in expectation. It has therefore yet to enter and is naturally not adjusted, thus we still keep \( \eta \sum_{i=1}^{N} \delta^i [v(\phi_t) - v(r_i)] \) in the equation.
Which then leads to the following condition, where both sides are strictly positive:

\[
\frac{e^{-\xi t} \Delta_{t+1}}{\pi e^{-\xi t} + 1} > -\frac{\partial \Delta_{t+1}}{\partial \pi}
\]

### A.1.3 Estimating Hazard Rates and Re-Employment Wages

Prior to estimation I group the data to bi-weekly exits and then use a simple linear probability model to estimate the hazard rates for pre- and post-reform separately for each period after entering the spell, following DellaVigna et al. (2017) as well as a similar linear regression model for the re-employment wages:

\[
h(t^*_i = t | t^*_i \geq t) = h_{0,t} + h_{1,t} d_{2011,i} + X_i \theta_t + \epsilon_{it} \tag{A.8}
\]

\[
w(t^*_i = t | t^*_i \geq t) = w_{0,t} + w_{1,t} d_{2011,i} + X_i \phi_t + \sigma_{it} \tag{A.9}
\]

where \(h_{0,t}\) is the baseline hazard for the 2008 cohort, \(h_{0,t} + h_{1,t}\) marks the hazard in the 2011 regime and \(X_i\) is a matrix of time-invariant background control characteristics. I estimate both with and without background controls, where adding controls does not change the patterns quantitatively or qualitatively.
### A.2 Tables

**Table A.1:** Descriptive statistics of the population used in the structural estimation: Means, standard deviation (in parenthesis) and difference between means

<table>
<thead>
<tr>
<th>Benefit regime</th>
<th>2008</th>
<th>2011</th>
<th>Diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ethnic dane</td>
<td>0.88</td>
<td>0.88</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td>(0.32)</td>
<td>(0.31)</td>
<td></td>
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<tr>
<td>Female</td>
<td>0.46</td>
<td>0.51</td>
<td>0.05***</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.50)</td>
<td></td>
</tr>
<tr>
<td>Age at spell start</td>
<td>36.7</td>
<td>36.4</td>
<td>0.03***</td>
</tr>
<tr>
<td></td>
<td>(7.1)</td>
<td>(7.4)</td>
<td></td>
</tr>
<tr>
<td>Living in city</td>
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<td>0.32</td>
<td>0.01***</td>
</tr>
<tr>
<td></td>
<td>(0.46)</td>
<td>(0.47)</td>
<td></td>
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<tr>
<td>Compulsory education†</td>
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<td>0.18</td>
<td>0.07***</td>
</tr>
<tr>
<td></td>
<td>(0.43)</td>
<td>(0.39)</td>
<td></td>
</tr>
<tr>
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<td>0.51</td>
<td>0.01***</td>
</tr>
<tr>
<td></td>
<td>(0.50)</td>
<td>(0.50)</td>
<td></td>
</tr>
<tr>
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<td>(0.45)</td>
<td></td>
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<tr>
<td>Prior wage (DKK/h)‡</td>
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<td>199.1</td>
<td>7.9***</td>
</tr>
<tr>
<td></td>
<td>(164.9)</td>
<td>(140.0)</td>
<td></td>
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<tr>
<td>Years in labor force</td>
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<td>17.1</td>
<td>0.1***</td>
</tr>
<tr>
<td></td>
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<td>(8.1)</td>
<td></td>
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<tr>
<td>Employment rate§</td>
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<td>0.62</td>
<td>0.00</td>
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<tr>
<td></td>
<td>(0.27)</td>
<td>(0.28)</td>
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<tr>
<td>No. of distinct spells</td>
<td>44,719</td>
<td>58,612</td>
<td></td>
</tr>
</tbody>
</table>

Notes: *** p<0.01, ** p<0.05, * p<0.1. (†) Education: Degree level of highest finished education. (‡) Wage: All wages in 2011 price levels. (§) Employment rate: Fraction of employment in the time since entering the labor force with full-time work equivalent 160.33 hours per month.
Table A.2: Estimates of the different structural model parameters for various specifications, standard errors in parentheses.

<table>
<thead>
<tr>
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<th>Standard 2-type (3)</th>
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<td>Discount factor $\beta$</td>
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<td>0.973</td>
<td>-</td>
<td>0.783</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.015)†</td>
<td></td>
<td>(0.065)</td>
</tr>
<tr>
<td>Loss aversion $\lambda$</td>
<td>8.757</td>
<td>6.966</td>
<td>-</td>
<td>3.535</td>
</tr>
<tr>
<td></td>
<td>(0.972)</td>
<td>(1.891)</td>
<td></td>
<td>(0.183)</td>
</tr>
<tr>
<td>Adjustment speed in days $N$</td>
<td>85.693</td>
<td>211.178</td>
<td>-</td>
<td>189.001</td>
</tr>
<tr>
<td></td>
<td>(22.522)</td>
<td>(.)‡</td>
<td></td>
<td>(4.364)</td>
</tr>
<tr>
<td><strong>Belief parameters</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Baseline bias $\pi$</td>
<td>3.651</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>(0.506)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Decay $\xi$</td>
<td>0.117</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Search cost parameters</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Curvature $\gamma$</td>
<td>0.391</td>
<td>0.280</td>
<td>0.401</td>
<td>0.944</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.043)</td>
<td>(0.021)</td>
<td>(0.161)</td>
</tr>
<tr>
<td>Costs $k$</td>
<td>13.250</td>
<td>125.770</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>(1.107)</td>
<td>(21.574)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population share low type $p_l$</td>
<td>-</td>
<td>-</td>
<td>0.611</td>
<td>0.194</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Costs low type $k_l$</td>
<td>-</td>
<td>-</td>
<td>1.945</td>
<td>5.289</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.100)</td>
<td>(2.150)</td>
</tr>
<tr>
<td>Costs high type $k_h$</td>
<td>-</td>
<td>-</td>
<td>136.720</td>
<td>908.262</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(9.934)</td>
<td>(394.662)</td>
</tr>
<tr>
<td><strong>Other parameters</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wage offer mean (log)</td>
<td>8.989</td>
<td>9.444</td>
<td>9.390</td>
<td>9.341</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.008)</td>
<td>(0.004)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Wage offer std. deviation</td>
<td>0.295</td>
<td>0.100</td>
<td>0.103</td>
<td>0.123</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.034)†</td>
<td>(0.030)†</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Welfare fraction $\psi$</td>
<td>0.797</td>
<td>0.800</td>
<td>0.611</td>
<td>0.345</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.006)†</td>
<td>(0.026)</td>
<td>(0.061)</td>
</tr>
<tr>
<td><strong>Model fit</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Used moments</td>
<td>384</td>
<td>384</td>
<td>384</td>
<td>384</td>
</tr>
<tr>
<td>Estimated parameters</td>
<td>10</td>
<td>8</td>
<td>7</td>
<td>10</td>
</tr>
<tr>
<td>Data Fit*</td>
<td>833.9</td>
<td>1574.1</td>
<td>1163.1</td>
<td>821.7</td>
</tr>
<tr>
<td><strong>Duration elasticities†</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Entitlement length</td>
<td>0.53</td>
<td>0.73</td>
<td>0.23</td>
<td>0.49</td>
</tr>
<tr>
<td>Benefit level</td>
<td>0.98</td>
<td>2.10</td>
<td>1.37</td>
<td>1.14</td>
</tr>
</tbody>
</table>

Notes: (∗) Data fit: Value of the objective function at the estimated parameters. (†) Standard errors need to be viewed with caution as the parameter estimates lie at the boundary of the parameter space. (‡) The parameter is not well identified under this specification and thus dropped from $\hat{G}$ in order to invert $\hat{G}^T \hat{W} \hat{G}$ and calculate other standard errors. (†) Unemployment duration elasticities $\frac{dD}{dT}$ (increasing entitlement length by 10 weeks) and $\frac{dD}{dB}$ (increasing benefits by 10 percent)
Table A.3: Comparison of policies with similar impact on expected unemployment duration, change on outcomes in percent.

<table>
<thead>
<tr>
<th></th>
<th>Duration</th>
<th>Expenditure</th>
<th>Welfare</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Frontloading</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2-Tier</td>
<td>-8.4</td>
<td>-2.6</td>
<td>0.022</td>
</tr>
<tr>
<td>3-Tier</td>
<td>-8.4</td>
<td>-2.3</td>
<td>0.026</td>
</tr>
<tr>
<td>4-Tier</td>
<td>-8.4</td>
<td>-3.1</td>
<td>0.025</td>
</tr>
<tr>
<td>Gradual</td>
<td>-8.4</td>
<td>-4.8</td>
<td>0.014</td>
</tr>
<tr>
<td><strong>Monitoring</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-8.4</td>
<td>-7.7</td>
<td>0.049</td>
</tr>
</tbody>
</table>

*Notes: Table cells show changes in the outcomes as a result of a policy change that leads to same expected duration relative to benchmark situation. Policy parameters with same duration change: 2-tier with 8.8% front-loading, 3-tier with 9.8% front-loading, 4-tier with 9.3% front-loading, gradual system with 9.3% front-loading and search monitoring with $s_T = 0.31$. (*) Frontloading first tier benefit increase, other tiers are calculated in a way such that $\bar{b}'_T T_1 + \bar{b}''(T - T_1) = \bar{b}T$. 
<table>
<thead>
<tr>
<th></th>
<th>Full (1)</th>
<th>Ref. Dep. (2)</th>
<th>Standard 2-type† (3)</th>
<th>Ref. Dep. 2-type† (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Frontloading</strong></td>
<td>0.1</td>
<td>0.2</td>
<td>0.3</td>
<td>0.1</td>
</tr>
<tr>
<td><strong>Duration</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1-tier</td>
<td>-9.3</td>
<td>-13.9</td>
<td>-15.5</td>
<td>-9.9</td>
</tr>
<tr>
<td>2-tier</td>
<td>-8.5</td>
<td>-13.8</td>
<td>-16.7</td>
<td>-8.8</td>
</tr>
<tr>
<td>3-tier</td>
<td>-8.9</td>
<td>-15.3</td>
<td>-19.1</td>
<td>-9.6</td>
</tr>
<tr>
<td>Gradual</td>
<td>-9.0</td>
<td>-15.8</td>
<td>-20.4</td>
<td>-9.7</td>
</tr>
<tr>
<td><strong>Expenditure</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1-tier</td>
<td>-2.6</td>
<td>0.8</td>
<td>8.2</td>
<td>-3.2</td>
</tr>
<tr>
<td>2-tier</td>
<td>-2.3</td>
<td>-0.6</td>
<td>3.9</td>
<td>-2.5</td>
</tr>
<tr>
<td>3-tier</td>
<td>-3.2</td>
<td>-3.2</td>
<td>-0.1</td>
<td>-3.8</td>
</tr>
<tr>
<td>Gradual</td>
<td>-5.1</td>
<td>-7.7</td>
<td>-8.0</td>
<td>-5.7</td>
</tr>
<tr>
<td><strong>Welfare</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1-tier</td>
<td>0.03</td>
<td>0.05</td>
<td>0.10</td>
<td>0.13</td>
</tr>
<tr>
<td>2-tier</td>
<td>0.03</td>
<td>0.06</td>
<td>0.09</td>
<td>0.12</td>
</tr>
<tr>
<td>3-tier</td>
<td>0.03</td>
<td>0.05</td>
<td>0.08</td>
<td>0.11</td>
</tr>
<tr>
<td>Gradual</td>
<td>0.02</td>
<td>0.03</td>
<td>0.04</td>
<td>0.06</td>
</tr>
</tbody>
</table>

**Notes:** Table cells show changes in the outcomes as a result of a policy change relative to benchmark situation. (*) Frontloading first tier benefit increase, other tiers calculated such that $b'T_1 + b''(T - T_1) = b'T$. (†) Percentage change with respect to the average outcome for models with unobserved heterogeneity.
Table A.5: Outcomes for multiple policies and different models, change on outcomes in percent

<table>
<thead>
<tr>
<th></th>
<th>Full (1)</th>
<th>Ref. Dep. (2)</th>
<th>Standard 2-type‡ (3)</th>
<th>Ref. Dep. 2-type† (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Bonus</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>*</td>
<td>0.5</td>
<td>1</td>
<td>1.5</td>
<td>0.5</td>
</tr>
<tr>
<td><strong>Duration</strong></td>
<td>-1.2</td>
<td>-1.9</td>
<td>-2.4</td>
<td>-1.2</td>
</tr>
<tr>
<td><strong>Expenditure</strong></td>
<td>5.1</td>
<td>10.6</td>
<td>16.3</td>
<td>5.0</td>
</tr>
<tr>
<td><strong>Welfare</strong></td>
<td>0.02</td>
<td>0.04</td>
<td>0.05</td>
<td>0.02</td>
</tr>
<tr>
<td><strong>Effort req.‡</strong></td>
<td>0.15</td>
<td>0.3</td>
<td>0.45</td>
<td>0.15</td>
</tr>
</tbody>
</table>

|                  |          |               |                      |                       |
| **Duration**     | 0.0      | -7.5          | -21.9                | -52.9                 |
| **Expenditure**  | 0.0      | -6.9          | -20.3                | -50.4                 |
| **Welfare**      | 0.00     | 0.05          | 0.08                 | -0.09                 |

Notes: Table cells show changes in the outcomes as a result of a policy change relative to benchmark situation. (*) Re-employment bonus over the first year of unemployment as a multiple of the benefit level. (†) Minimum requirement for search effort $s_t$, if not fulfilled benefits reduced to welfare transfers $b$. (‡) Percentage change with respect to the average outcome for models with unobserved heterogeneity.
A.3 Figures

Figure A.1: Simulated search efforts for different model frameworks.

Note: Figure displays model simulations for different behavioral and non-behavioral frameworks. Parameters of the standard model are set to $\gamma = 0.3$, $k = 80$, $\lambda = 0$, $N = 0$, $\pi = 0$, $\xi = 0$, $\beta = 1$. For the DellaVigna type model set $\lambda = 2$, $N = 20$ and $\beta = 0.95$. For the full model additionally set $\pi = 0.3$ and $\xi = 0.2$.

Figure A.2: Benefit entitlement length for individuals entering the UI system in first/second half of various years, including temporary extension.

Note: Figure displays the maximum entitlement length for cohorts entering the UI system at different points in time. First bar is the first half for a given year, second bar is the second half. The “acute-measures” delayed the benefit exhaustion for long-term unemployed who would lose their right to benefits in late 2012 by six months.
Figure A.3: 14-day hazard rates and re-employment wages, with (blue/red) and without controls (green/orange).

Note: Controls contain age, gender, having kids, being married, ethnicity, region of residence, living in city, education, pre-unemployment wage, prior occupation as well as general labor market experience.

Figure A.4: Out of sample performance for the 2010 (2nd half) benefit regime.

Note: Figure shows the model fit on the 2010 (2nd half) cohort assuming individuals expected a two year benefit entitlement ex-ante, but get updated about the extension during their last half year of the UI period. I exclude weeks 78 to 104 from the simulation as news about benefit extension reached individuals at different points in this time-frame.
Figure A.5: Effects of changing the benefit level, hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of reducing the benefit level $\bar{b}$. The left panel displays the impact of reducing the benchmark benefits by 10% on the hazard rates. The right panel shows the effects on policy outcomes with percentage change relative to the benchmark situation on the x-axis.

Figure A.6: Effects of changing the entitlement length, hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of changing the entitlement length $T$. The left panel displays the impact of shortening entitlement by 26 weeks on the hazard rates. The right panel shows the effects on policy outcomes with change in weeks relative to the benchmark situation on the x-axis.
Figure A.7: Effects of a re-employment bonus, hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of a re-employment bonus paid out for the first year. The left panel displays the impact of a bonus of the size of the benefit level $\bar{b}$. The right panel shows the effects on policy outcomes with a bonus as a multiple of the monthly benefit level on the x-axis.

Figure A.8: Effects of search effort monitoring with sanctions, hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of a search effort requirement with sanctions $(\bar{b} - \bar{b})$. The left panel displays the impact of a search effort requirement of $s_t = 0.3$. The right panel shows the effects on policy outcomes with the search effort requirement on the x-axis.
**Figure A.9:** Effects of front-loading the benefit path with two tiers, hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of front-loading the benefit path with two tiers such that \( \bar{b}'T_1 + \bar{b}''(T - T_1) = \bar{b}T \). The left panel displays the impact of front-loading such that the first tier is 10% higher than under the benchmark. The right panel shows the effects two-tier front-loading with the change in first tier benefits on the x-axis.

**Figure A.10:** Effects of front-loading the benefit path with three tiers, hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of front-loading the benefit path with three tiers such that the total amount of benefits available over the spell remains the same. The left panel displays the impact of front-loading such that the first tier is 10% higher than under the benchmark. The right panel shows the effects three-tier front-loading with the change in first tier benefits on the x-axis.
Figure A.11: Effects of front-loading the benefit path with four tiers, hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of front-loading the benefit path with four tiers such that the total amount of benefits available over the spell remains the same. The left panel displays the impact of front-loading such that the first tier is 10% higher than under the benchmark. The right panel shows the effects four-tier front-loading with the change in first tier benefits on the x-axis.

Figure A.12: Effects of gradual front-loading of the benefit path hazard rates (left) and policy outcomes (right)

Note: The figure shows the effects of gradually front-loading the benefit path, i.e. a lower tier every month, such that the total amount of benefits available over the spell remains the same. The left panel displays the impact of front-loading such that the first tier is 10% higher than under the benchmark. The right panel shows the effects gradual front-loading with the change in first tier benefits on the x-axis.
Little is known about the nature of job search and its dynamics over the unemployment spell. In this paper we shed light on search behavior, in terms of the number of applications and search method used as well as the specific characteristics of the jobs that unemployed workers target during their unemployment spell. We use a novel Danish administrative data set containing actual job applications made by the universe of UI recipients. Because of the administrative nature of this data and the possibility to link it to the Danish registers, we can cover job search in greater detail than previous work.

We show that the targeted job characteristics change very little over the unemployment spell, yet important differences exist between the overall job search portfolio and the specific application that leads to employment. We further document that caseworker meetings and active labor market policies do not substantially change search behavior as such, but we do observe hints of threat effects on the number of applications made. In sum, our results suggest that while there may be large gains for unemployed workers by changing job search, this may be hard to accomplish as unemployed job seekers generally do not seem willing (or able) to change application behavior along the dimensions we study.

Keywords: job search, unemployment

JEL Codes: E24, J64
2.1 Introduction

Job search during unemployment is an important process, both for the individual and the economy as a whole. It affects aggregate performance through unemployment dynamics and, for the job seekers who are eventually successful, the allocation into jobs. Theory predicts that job search may change as benefit exhaustion approaches and that it depends on features of the environment, such as the returns to search. These can, for instance, vary with availability of vacancies or the time elapsed in unemployment (duration dependence). Other models explore the role of learning and reference point adjustments over the time spent searching. They predict that search may change as the perceived return to search is updated (e.g. Falk et al., 2006), or as individuals adjust their reference point and become “more familiar with unemployment” (e.g. DellaVigna et al., 2017).

Theory thus suggests that job search is a continuous and dynamic process that may vary over the duration of an unemployment spell. Empirically, we know less about how individuals choose which jobs to apply to and how search behavior changes over time. Further, many open questions regarding the determinants of successful job search remain unanswered: Does it matter where individuals search, which job characteristics they target, or which search methods they use? Previous empirical research on these issues has been challenged by the lack of sufficient data sources. In order to answer how reservation wages and the time spent searching change over the unemployment spell, most of the literature used access to survey data or questionnaires at inflow to unemployment (e.g. Krueger et al., 2011; Krueger and Mueller, 2016).

With the increased usage of online job search, broad and detailed data are now more often available to researchers. Yet, recent work still pays most attention to the dynamics of individual reservation wages or target wages and the overall level of search intensity (we review the empirical literature below).¹ Both can arguably serve as an indicative statistic for job search as a whole. However, wages are only one dimension of the job characteristics that individuals may value, and there is little knowledge on how individuals trade off different job characteristics against each other.² Below, we aim to fill a part of this gap, exploiting the continuous availability of job search measures throughout the unemployment spell using a novel administrative data source on job applications in Denmark.

In this paper, we shed more light on the dynamics of job search. We are interested in characterizing the extent to which job search is dynamic, that is, whether it changes over the course of an unemployment spell or in response to interactions with the job center and activation programs. We also try to understand whether changes in job search could improve job finding by analyzing whether search changes prior to the successful application, and to what extent this application differs from previous applications earlier in the spell. If, for instance, the successful application differs from the general pool of applications in terms

¹Note that targeted wages and reservation wages may be very different entities. Targeted wages are measures of the monetary pay associated with the specific job which an individual applies to. Reservation wages reflect the lowest possible job/pay which a specific individual would accept (summary index with potentially monetary and non-monetary elements).
²Le Barbanchon et al. (2019) decompose the elasticity of job finding with respect to UI benefits into a part attributable to changes in reservation wages and a part attributable to changes in job search and show that the latter part is the primary force in job finding (over 90%). This result underlines the importance of a good understanding of the underlying driver behind job search patterns and dynamics over the unemployment spell.
of job characteristics, it suggests that job seekers might increase their likelihood of success by changing the type of jobs they target. Our main contribution is to document how job search evolves over the unemployment spell, in response to interactions with job centers and around the time of successfully applying for a job. Documenting whether and how job search changes is important to understand the underlying reasons leading to prolonged exposure to unemployment. It is useful for policy makers, particularly if certain job search strategies are more likely to be successful than others. Understanding job search is also important in order to distinguish between different theoretical explanations and assess the relative importance of different mechanisms and thus enhance our understanding of job search. We look at these different sources of dynamics which have been discussed in the previous literature but in which cases data sources provided limited ability to look at actual search patterns and behavior.

We exploit access to data on mandatory search diaries that all Danish unemployment insurance (UI) recipients have to report during their unemployment spell. These search diaries, called joblogs, form the basis for UI eligibility assessments and are thus actively used by caseworkers and UI funds. This gives individuals a clear (financial) incentive to document their job search, at least until the requirements set by the respective UI fund are met (below we discuss the validity of the data further). The joblog data thereby directly links to actual applications sent while unemployed. Importantly, the applications are logged continuously throughout the spell which makes our data suitable to analyze dynamics. Additionally, we can link this data to the general Danish registers and observe both job search during and job outcomes after unemployment. As the data covers the universe of UI recipients for roughly two years, the dimensions are large. Overall, our analysis sample covers over 4.5 million applications of about 114,000 individuals. For each logged application, we have information on the search method (formal/informal, online application, search channels, etc.) and job characteristics such as occupational codes, firm identifiers, location of the firm and more. We also identify the typical wage associated with the job applied for. This enables us to provide a rich characterization of the observed job search behavior.

We base our empirical analysis on regression models in which we estimate changes in application patterns while controlling for unemployment spell fixed effects. We start out focusing on the dynamics over the unemployment spell and quantify how search changes along several dimensions including the used search method and the job characteristics which job seekers target. We then reorganize the data and focus on the dynamics around important labor market institutions. Specifically, we analyze the dynamics of search and applications around caseworker meetings in the municipal job centers and around the placement in mandatory activation measures, such as education- and skill development programs. The job center meetings serve as a measure for guidance and monitoring, and thus potentially alter search strategies and the exerted effort. Activation measures aim to teach new and enhance existing skills that could lead to altered job search targets. Last, based on the knowledge about the extent to which job search changes over the unemployment spell and around interactions with the job center, we look closer at what characterizes successful job search to quantify potential benefits from changing the search strategy. In particular, we focus on the composition and the dynamics of applications around the time of the (eventually) successful application. We thus restructure the panel and order the applications relative to

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3These measures are typically initiated by the job center but in the latter case potentially conducted by third parties (e.g. private contractors).
the time until an eventually successful application is sent. By doing this, we are able to focus
the analysis on search patterns leading up to the eventual success and examine whether there
are important changes associated with this event, i.e. whether this application differs from the
rest of the individual’s applications.

We show that, consistent with previous work, job search is (remarkably) stable over the
unemployment spell. The (typical) wage associated with an application is declining, suggesting
that individuals change the type of jobs they target, yet the magnitude of this change is very
small. Some other job characteristics show slightly larger adjustments (e.g. people are more
willing to commute or work part time), but the overall patterns and magnitudes suggest very
limited changes in job search. This result may be read as a reflection of the lack of job seekers’
rational belief adjustment over the unemployment spell (Mueller et al., 2019). Similarly, there is
little evidence in the data that participation in meetings or activation programs fundamentally
changes job search. We do, however, observe some evidence of threat effects in the sense that
individuals log more applications in the weeks prior contact with the Public Employment
Services (PES). For some of our measures we also observe changes in the short run after
participation, but again, the quantitative magnitude is limited and vanishes when controlling
for the time spent in unemployment. At face value, our results thus suggest that one reason
activation and similar types of programs are often shown to provide limited short term benefits
in terms of job finding (see e.g. Card et al., 2017) is that they are largely ineffective in changing
the type of jobs and the search methods which unemployed use.

Finally, we show that the successful application, i.e. the single application we can connect
to the eventual post-unemployment job, differs from the general application portfolio. This
particular application is more likely to be sent through informal channels, including the
use of the individual’s coworker network and it is sent to a job with shorter commute times.
Furthermore, the firm applied to is more likely to be in an industry related to the previous
job of the individual, possibly reflecting that employers have a preference for job seekers with
previous relevant experience in their sector.

Overall, these results suggest that the job which the average individual finds after unem-
ployment is somewhat different from the jobs targeted during unemployment. In consequence,
it may be problematic to infer general job search behavior from the observed job outcomes
alone. While this difference also reflects employer preferences and the availability of job op-
opportunities, the results also suggest that there could be substantial gains linked to changing
the application behavior of job seekers.

This paper proceeds as follows: First, we briefly review previous work on job search dynam-
ics and application behavior with a focus on the recent spread of available application-level
data. In Section 2 we then follow with an overview of the institutional setting and detailed
introduction to our data source. The third section describes our econometric approach and
is followed by the fourth section with our empirical results on job search dynamics over the
unemployment spell and around specific events. This section concludes with an exploration
of the differences between the job search portfolio and the eventually successful application.
Finally, Section 5 concludes on this paper’s findings.
2.1.1 Literature and previous work

Our paper is directly related to the recent, yet rapidly growing literature that explores the nature of job search. Below, we first look at papers which exploit the availability of detailed data from specific online job search platforms.\textsuperscript{4} We then continue on the literature that can link individual applications with administrative data.

While online job search was rather uncommon and ineffective at the turn of the century (Kuhn and Skuterud, 2004), it has since become a widespread and efficient tool for job seekers (Kuhn and Mansour, 2014). Some of the literature that focuses on specific job boards is concerned with the dynamics of job search over the unemployment spell.\textsuperscript{5} Kudlyak et al. (2014) use data on individual applications made on SnagAJob.com, a job search and vacancy posting platform mainly used for hourly-paid jobs. They find a negative relationship between the duration of the UI spell and sorting relative to the individual’s educational background. Much of this adjustment happens very early in the spell. The authors interpret this as learning through negative search outcomes at the beginning of job search and relate it to the work on declining reservation wages (Kiefer and Neumann, 1979; Brown et al., 2011). The hypothesis on dynamics in the role of reservation wages over the spell only finds modest support in the recent literature however (see e.g. Addison et al., 2009; Schmieder et al., 2016; Krueger and Mueller, 2016).

Faberman and Kudlyak (2019) also use data from SnagAJob.com to study the evolution of search intensity. Since there is no information on job finding in this data, they deduce it from individuals stopping to apply through the job search engine. They show that the number of applications a job seeker makes is declining over the course of an unemployment spell. This is in line with the general notion that effort devoted to job search activities declines over the UI spell as found in time-use surveys (Krueger et al., 2011), but could also be driven by substitution of search activities away from the specific platform. Additionally, the average number of applications made per week is positively related to the realized duration of the unemployment spell, i.e. individuals that are unemployed for a longer time generally send more applications per week than individuals who are unemployed shorter.

Banfi et al. (2019a) use data from Trabajando.com with postings from their Chilean job board and show that unemployed job seekers have a decreasing probability of making an application over time. Further, with increasing duration in unemployment, applications to vacancies with lower wages and education requirements increase. Over time the probability to apply to occupations misaligned with the worker’s characteristics increases, while the likelihood to apply to jobs that are geographically more distant decreases. The authors also

\textsuperscript{4}While most of these studies observe the number of applications sent on the specific platform, there is little to no information on the actual search effort involved. Survey evidence on time use shows that applications sent and time spent searching for work, as reported by job seekers, track each other quite closely when dis-aggregated by labor force status (Faberman et al., 2017). Thus, the number of applications appears to be a good measure of the overall search effort.

\textsuperscript{5}Further work has also shed light on aspects of job search other than dynamics over time. Marinescu and Rathelot (2018) use one of the largest US job boards, CareerBuilder.com, to study the relation of static application behavior to the geographic location of the posted vacancies. Marinescu (2017) uses the same data to estimate the impact of UI benefit extensions on job application rates and job vacancies. Banfi and Villena-Roldan (2019) use data from Trabajando.com (see below) to present evidence that higher wages attract more applicants, even under the absence of explicit wage-posting. Banfi et al. (2019b) use the same data and find strong evidence for positive assortative matching.
find some evidence for stock-flow-matching (Gregg and Petrongolo, 2005).

Most related to our data is the literature that can link administrative data to individual job seekers and their search behavior. Le Barbanchon et al. (2019) use French administrative data on individually reported reservation wages at the beginning of a new unemployment spell as well as desired hours and commuting distance. The French PES require each person to report these numbers once at the onset of a new unemployment spell. The authors are not able to reject that there is zero elasticity between the remaining time of benefit entitlement and the reservation wages. There is further non-responsiveness on hours and commuting time. Skandalis and Marinescu (2019) use data from the most popular French job search platform, administered by the country’s unemployment agency, and can link it to administrative data on job seekers. The authors examine search intensity as well as job finding around the exhaustion of UI benefits and find increases in search intensity at the end of UI entitlement followed by a sharp decline, corresponding well to the observed job finding rates. Further, individuals apply to jobs associated with lower wages over the duration of the unemployment spell, mostly driven by targeting lower paying occupations. Skandalis (2019) uses the same data and shows that job seekers react to informational shocks, e.g. news about plant expansions, and increase the number of applications sent in the immediate aftermath.

That job seekers are affected by new information has also been shown by Belot et al. (2018a). The authors test the effects of providing targeted advice to job seekers’ in an artificial job search lab by supplying information on a broad set of occupations related to the job seekers skill transferability. This information broadens the set of considered occupations, especially for initially narrow searchers, and leads to increased interview invites (they do not observe job outcomes).  

In relation to the papers above, our paper has the potential to extend and improve the literature of the dynamics of job search in several dimensions. First, our paper is among the first papers that have comprehensive panel data on job search. This implies that we can study within-individual dynamics of job search and avoid cross-sectional comparisons likely affected by dynamic selection (as shown below). At the same time, we observe applications that are not specific to a particular job search platform but instead based on mandatory reporting requirements for the job seekers. This may decrease the concern about substitution effects and that online platforms only cover a part of the pool of vacancies. Second, the ability to link the data on logged applications to the administrative registers further enables us to provide a rich analysis of the dynamics of job search while also linking it to eventual job finding. We can thus observe the dual outcome of job search and job finding and avoid potential biases from indirectly imputing job finding by stopping to apply. Last, while most research on job search focuses on search intensity and reservation wage dynamics, we can directly study non-wage aspects of job search. Reservation wages are typically thought of as a summary index of the value of the job, including both wages and non-pecuniary aspects, such as amenities, commuting times, hours, tasks and promotion prospects.  

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6In the same setting, Belot et al. (2018b) show that job seekers trade off posted wages and the expected competition over the respective vacancy.
7Using rich survey data, Hall and Mueller (2018) measure non-wage aspects as a function of the ratio of the wages offered to reservation wages and find that these play a substantial role in job search. Taber and Vejlin (2016) find similar evidence of the importance of non-pecuniary job aspects using Danish matched-employer-employee data and job-to-job transitions.
2.2 Data

2.2.1 Institutional setting

Broadly speaking, two types of transfer payments may be available for unemployed job seekers in Denmark: regular UI benefits or (means-tested) social assistance. We focus on the UI system below as our sample contains UI recipients only. Being eligible for UI requires at least one year of UI fund membership prior to unemployment as well as an accumulation of a minimum of one year of full-time work over the past three years. Registering at one of the 24 different UI funds is voluntary, yet involves a monthly membership fee. Some funds target specific occupations and industries exclusively, while others are open for all workers. Individuals are entitled to benefits for a maximum of two years after which they need to fulfill the before-mentioned requirements again. Due to an upper bound on the payouts, which is binding for the vast majority of the unemployed, the actual transfers are identical for most. This implies that the effective replacement rate shows a large dispersion across the sample.

Throughout the unemployment spell, benefit recipients are required to demonstrate availability and active search for employment. This includes applying for suitable vacancies and following instructions from the job center. Interacting with the UI fund and the municipal job center, in the form of caseworker meetings and ALMP measures, is also a mandatory requirement to receive unemployment benefits. A failure to show up to these events or missing other scheduled activities (e.g. registering a CV or booking meetings) may cause a partial or full withdrawal of benefits.

Meetings are an important feature of the UI system and are used for guidance and eligibility assessment throughout the unemployment spell (Maibom et al., 2017; Andersen and Svarer, 2007). Within the first 6 months of unemployment, job seekers have to take part in at least 6 meetings at the job center. Some of these are joint meetings with an UI fund representative and a municipal caseworker. At these meetings, previous job search activity is discussed (more below), suitable application targets may be identified, and future activation measures can be planned. After 6 months of unemployment, the frequency falls to a meeting at least every third month. There is a number of different types of ALMP programs, some of which are administered by the job centers, while private providers offer others. Broadly, these activation
programs are of 3 types: work practice schemes, wage subsidy schemes, and guidance and qualification programs. The latter category involves programs with varying content, such as formal education, skill clarification and job search assistance. Later, we focus on the guidance and qualification programs and distinguish between more traditional ordinary education and shorter skill development/counseling. These measures in particular aim to improve job seekers’ skills and potentially encourage participants to search for new types of jobs.

Based on previous literature, we may expect to see threat-, lock-in- and post-program effects around both ALMP measures (e.g. Black et al., 2003; Rosholm and Svarer, 2008). While these effects have been identified in the exit rates out of unemployment, previous work has been challenged by the inability to observe the actual threat in retroactive data. With these approaches, one can only observe threat effects in the case individuals find employment and thus don’t take part in the respective ALMP. Furthermore, there is limited knowledge on how threat effects materialize as search behavior may also change along other dimensions (commute times, target wages, etc.). Our data (see below) offers a new way to look at threat effects as we observe measures of search effort, application methods and targeted job characteristics for the subset of individuals who do not find employment prior to program participation.

2.2.2 Joblog

In 2015, the Act on Active Employment (Beskæftigelsesreform I) obliged UI recipients to document their job search activity in detailed job search diaries on jobnet.dk. Jobnet is the central online platform of the National Labor Market Authorities on which individuals also claim for UI benefits initially and stay in contact with the job center. Job seekers also register their CV and book caseworker meetings on this platform. In addition, Jobnet also offers a job board itself, containing most of the posted vacancies in Denmark.

The individual joblogs, i.e. the specific applications that are registered on the Joblog platform, are typically used as a basis for the regular caseworker meetings in the job centers. The new tool offers a more detailed assessment of the individuals’ search activity and supports the caseworkers in guiding their unemployed clients through the search for employment more efficiently. During the first weeks of unemployment, the UI funds are legally required to instruct the unemployed in the use of joblog. The law further requires them to specify
a personal minimum amount of weekly or monthly applications that an individual needs to register. Over the unemployment spell, UI funds monitor their clients’ search activity, and if it does not adequately fulfill the requirements, potentially issue sanctions after giving a short deadline. The National Labor Market Authorities further incentivize UI funds and municipalities to guarantee sufficient usage among their clients as their allocation of financial resources depends on active joblog usage among all their UI recipients (in January 2019 this rate was 90%).

Our source data is directly extracted from the joblog database on Jobnet and delivered by the National Labor Market Authorities. Each application registered in Joblog contains the individual’s personal identification number, which we can link to administrative data, and has a unique observation for each edit of a joblog. To create a new entry for an application, information about the specific job, the search channel used and the status of the application is mandatory. We pre-process this source data to identify applications that were actually sent by only selecting the first joblog entry after excluding those which have the status of “not yet applied”. A part of the mandatory details is the specific job title, which is most often the title of the vacancy posting, and information on the firm in terms of name and address. Further compulsory information we have access to include whether the job is a full- or part-time position, the search channel, and the method used to apply. We use this to distinguish formal search, i.e. applications going to officially advertised vacancies, from informal search, i.e. those that are sent through some other method (personal contacts, being contacted by headhunters or placing unsolicited applications). In terms of the specific method used to place the application, we distinguish traditional- (letter or e-mail applications), personal- (personal inquiry and phone calls) and online methods (web forms and social media). We add to this by identifying whether the application was sent to a firm that employs a member of the individual’s network of former coworkers.

In Appendix A.1.1 we lay out how we use the information on individuals, job titles and firms to get a wide range of additional job characteristics using the Danish administrative data and a rigorous matching procedure. In summary, for each application we have information

\[17\] § 36, pcs. 1 in the law about availability (“Bekendtgørelse om rådighed”). The UI funds determine the job seekers requirement based on individual characteristics, such as the job seekers education, work experience, skills and the regional labor demand. Despite no universal threshold of logging requirements, UI funds often state a rough guideline of their general expectations which vary across funds but are mostly around 1.5-2 applications a week (see Figure A.1 in Appendix A.2).

\[18\] The National Labor Market Authorities can cut reimbursements to the UI funds if the benefits have not been administered according to the legal requirements, such as eligibility assessments and joblog screening. See also https://star.dk/it/bорger-it/tjek-jobforslag-find-job-og-joblog/min-joblog-borgerens-værktøj-til-en-struktureret-jobsoegning/status-for-brug-af-joblog-for-dagpengemodtagere/ (in Danish).

\[19\] The Joblog tool can also be used to schedule applications planned to send out later. Other status options include "applied", "called for an interview", "job offer" or "rejection".

\[20\] Individuals can also upload documents (e.g. the actual application) or enter a link to the vacancy, individual deadlines and information on the contact person at the applied firm. See here for a step by step guide in how to create a joblog: https://www.ca.dk/sites/default/files/PDF/Vejledninger/saadan_joblogger_du_paa_jobnet.pdf (in Danish).

\[21\] Note that informal applications often do not count fully when assessing the job search activity of benefit recipients. However, these applications are regularly counted as a supplement to formal search, see https://ma-kasse.dk/dagpenge/kort-om-dagpenge/krav-til-jobsoegning/ (in Danish).
on occupational and industry classifications in major (1 digit), sub-major (2 digit) and minor (3 digit) groupings as well as whether these are highly related to the individual's prior job in terms of skills and tasks.\footnote{Occupations are grouped in DISCO codes with 9 (major), 55 (sub-major) or 153 (minor) respective occupations. DISCO is the Danish equivalent of ISCO. The industries are grouped in an aggregated version of NACE Rev. 2 with 10 (major), 21 (sub-major) or 38 (minor) respective industries. See appendix section A.1.1 for the exact definition of the relatedness measures.} We have the approximated commute time from the individual's municipality of residence to the job's location as well as the competition over these jobs, proxied by the within commute area unemployment rate in the respective occupation or industry. Further, we obtain firm fixed effects for each firm in Joblog by running an AKM model on the full Danish matched employer-employee data (Abowd et al., 1999). Our data does not contain wages associated with the job as these are not commonly reported for vacancies in Denmark. For each application in our data we therefore estimate the typical wage for this position based on detailed characteristics of the application we observe and the re-employment wages individuals are paid upon entering a new job.\footnote{Details on this procedure can also be found in the appendix section in Fluchtmann et al. (2019).}

In Figure A.1 in Appendix A.1 we plot the distribution of AKM firm fixed effects and commuting distances as well as typical- and actual re-employment wages.

While the Joblog data brings several advantages over previously used data sources that study job search, it is also subject to some challenges. First, the only measure of search activity is the number of logged applications which disregards other important activities of the search process (browsing job ads, networking, the writing of the actual application and many more; see Krueger et al., 2011). Second, while the logging requirement for UI benefit reception is a strong incentive to register individual search behavior, individuals are only required to log their activities up to this threshold. There is no direct incentive to log any additional applications, yet we observe some degree of excess logging.\footnote{Showing progress to caseworkers or using joblog as a personal tool to organize search could be a reason to do so. Naturally the job seekers may also be selective in the kind of applications they choose to register/log. Therefore while the incentive to register applications gives our data and analysis some clear advantages over previous work, the same incentive may also affect the type of applications we see. Thereby our data source nicely complements previous data.}

Fluchtmann et al. (2019), working with the same sample, argue that the majority only rarely applies to more jobs than what they enter in Joblog, thus joblog appears to cover a large part of individual applications made over the unemployment spell. Explicit requirements could potentially create incentives to log ‘pro forma’ applications, i.e. sending applications only to comply with the UI funds' demands. In practice there might thus be a fraction of applications not logged as well as a fraction of applications logged without the intention to accept a potential job offer. Note however that caseworkers and UI funds use joblog as a monitoring tool, and turning down job offers during unemployment can ultimately lead to a loss of UI benefits.

These facts therefore suggest that we might not observe all of job search, yet a substantial fraction of the actual applications made by Danish UI recipients as reporting is financially incentivized. In the following, we assume that the applications we observe are a random subset of an individual's application portfolio, and that the propensity to log an application does not change during the spell or around particular events. With this assumption in mind, we can use the data to provide insights into previously undocumented aspects of job search such as the targeted job characteristics and used search channels.
2.2.3 Sample

Our sample consists of individuals entering new unemployment spells between the first week of September 2015 to the last week of September 2017. As special UI rules may apply for immigrants, we exclude them from the sample. We require each spell to have at least one observed application in Joblog. We construct the panel on the spell level, thus some individuals may enter as spells multiple times, though in practice this is somewhat limited by the additional sample restrictions we make below. In total, we observe 114,448 individuals with 141,551 spells and 4,636,217 logged applications in our final sample.

To obtain our final sample we make three important sample restrictions in order to observe actual search in the data. For an overview of the individuals, spells and joblog entries we lose in this process, we refer to Table A.2 in Appendix A.2.2. First, we restrict our sample to individuals with a full two-year availability of UI benefits at spell start. By doing this, we make sure to have a correct measure of the time spent in the spell to properly identify dynamics. Without this restriction, individuals that start a new unemployment spell in our data might partly just come out of a prolonged interruption of their ongoing spell.

Second, at the beginning of a new unemployment spell the unemployed are usually subject to a ‘phasing-in’ period in which individuals slowly get introduced to joblog and other components of the UI system. Over these weeks, more time is spent on meetings that “prepare” job search and a proper CV with the help of caseworkers. Further, some individuals might only use UI benefits in the transition between jobs. This suggests that a part of the shorter unemployment spells is not subject to actual search for employment and therefore also lacks log activity. For these reasons we observe low usage of joblog over the first weeks of a new spell (see Figure 2.2 in Section 2.4.1). We therefore impose the restriction that individuals need to be unemployed for at least 8 weeks to capture actual unemployment spells. While this restriction reduces the number of spells by roughly 23 percent, we only lose about 5 percent of joblogs. This suggests that only a minor part of recorded search behavior is lost by imposing this restriction.

Third, we censor the last 4 weeks before individuals enter their subsequent employment. We do this to avoid confounding our analysis with the peculiar application behavior of individuals that have already accepted a job offer and might only log ‘pro forma’ applications to still fulfill the UI funds requirement. These are still enforceable until the day employment starts.

We report these uncommon application patterns after the successful application, i.e. the last application to a specific firm which we see the individual eventually entering (we identify successful application for 46 percent of our sample entering employment), in Appendix A.3.2 Figure A.26. We see how the average number of applications and applications to e.g. related industries drop after a successful application has been made. We exclude the last 4 weeks based on the median transition time between a successful application and starting a job.

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25 The start and end sampling points reflect that we observe Joblog entries from September 2015 and currently have labor market data available until September 2017. Our unemployment spell definitions require at least 4 weeks of consecutive UI benefit payments and no payout in the 4 weeks prior to spell start.

26 See for example https://www.min-a-kasse.dk/moder_jobcenter_og_a-kasse (in Danish).

27 See for example UI fund CA-Kasse: “Even if you have signed a contract and start that job in a week, in a month or in 3 months, you still have to be actively looking for a job until you start if you want unemployment benefits” (translation from Danish, see https://www.ca.dk/nyheder/hvor-mange-job-skal-du-soege)

28 The median transition time is 6 weeks, but we assume it takes 2 weeks from the application to the
We enrich our final sample of joblogs with Danish administrative data including basic demographics, attained education levels as well as detailed weekly labor market status including monthly worked hours and income.\textsuperscript{29} We further use the firm identifiers to add data on the firms in which the individuals were employed before and after the respective unemployment spell.

In the following, we briefly illustrate some descriptive facts sample of unemployed which we study. Looking at the 9 major occupation groups, 85 percent of the spells end up in one occupation identified in their pool of applications while 56 percent end in the most searched for occupation.\textsuperscript{30} The fraction of spells that end in an occupation we can identify among the logged applications is increasing to 89 percent for spells with more than 25 registered applications. The fraction ending in the most searched for occupation slightly decreases to 55 percent here. The individuals send 38 applications on average (median 26) during the whole period of each unemployment episode, and it takes 9 weeks on average (median 6) to transition from a successful application to the eventual job.\textsuperscript{31}

Figure 2.1: Survivor curve and week of spell start

![Survivor curve and week of spell start](image)

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

In the left panel of Figure 2.1 we show the survivor curve as a function of unemployment duration. The Figure illustrates that unemployment is to a large extent a temporary state in the Danish labor market - after half a year more than 50\% of our sample will have found employment. The fraction of individuals remaining unemployed continues to decline rapidly until it becomes stable after around 80 weeks of unemployment. Taken together, we observe joblogs throughout the unemployment spell and thus for many unemployment durations

\textsuperscript{29}The employment data comes from the BFL register, available until September 2017. This data set is based on Statistics Denmark’s E-income register which contains income reports from employers to the tax authorities. We get information about basic demographics from the IDA database at Statistics Denmark.

\textsuperscript{30}Similarly, 89 percent end up in an industry we observe in their applications (10 major groups) and 81 percent in an AKM firm fixed effect decile they applied for. Further, 48 percent enter the most searched for industry and 29 percent in the most frequent firm fixed effect decile. All these numbers are conditional on entering employment over our sample period.

\textsuperscript{31}A successful application is defined as the latest application to a specific firm we eventually see the individuals working in.
(even for the same individual). The right panel illustrates another feature of our data: the ability to observe unemployment spells starting at different points in time throughout our sample period. This variation is important for us to distinguish e.g. seasonal effects from dynamics linked to unemployment duration. In Appendix A.1 we confirm the same patterns for the start of ALMP measures and caseworker meetings.

### Table 2.1: Summary statistics - Final sample

<table>
<thead>
<tr>
<th>Unemployment Duration</th>
<th>Total</th>
<th>&lt;3 Months</th>
<th>3-6 Months</th>
<th>6-12 Months</th>
<th>&gt;12 Months</th>
</tr>
</thead>
<tbody>
<tr>
<td>Demographics</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.52</td>
<td>0.51</td>
<td>0.52</td>
<td>0.54</td>
<td>0.54</td>
</tr>
<tr>
<td>Age</td>
<td>38.49</td>
<td>37.34</td>
<td>38.62</td>
<td>39.02</td>
<td>40.35</td>
</tr>
<tr>
<td>Married</td>
<td>0.38</td>
<td>0.36</td>
<td>0.39</td>
<td>0.40</td>
<td>0.40</td>
</tr>
<tr>
<td>Living in city</td>
<td>0.36</td>
<td>0.36</td>
<td>0.35</td>
<td>0.39</td>
<td>0.41</td>
</tr>
<tr>
<td>Education</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High-School</td>
<td>0.25</td>
<td>0.24</td>
<td>0.26</td>
<td>0.24</td>
<td>0.23</td>
</tr>
<tr>
<td>Vocational</td>
<td>0.51</td>
<td>0.53</td>
<td>0.51</td>
<td>0.49</td>
<td>0.49</td>
</tr>
<tr>
<td>Academic</td>
<td>0.22</td>
<td>0.22</td>
<td>0.22</td>
<td>0.25</td>
<td>0.27</td>
</tr>
<tr>
<td>Previous job</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Previous wage (DKK/hour)</td>
<td>235.13</td>
<td>233.68</td>
<td>234.72</td>
<td>236.30</td>
<td>234.25</td>
</tr>
<tr>
<td>Spell</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Replacement rate</td>
<td>0.48</td>
<td>0.46</td>
<td>0.48</td>
<td>0.49</td>
<td>0.46</td>
</tr>
<tr>
<td>Joblogs p. week</td>
<td>1.51</td>
<td>1.42</td>
<td>1.50</td>
<td>1.58</td>
<td>1.64</td>
</tr>
<tr>
<td>Entered activation</td>
<td>0.51</td>
<td>0.17</td>
<td>0.49</td>
<td>0.84</td>
<td>0.96</td>
</tr>
<tr>
<td>Caseworker Meetings</td>
<td>6.71</td>
<td>2.55</td>
<td>5.40</td>
<td>10.47</td>
<td>17.18</td>
</tr>
<tr>
<td>Right censored</td>
<td>0.21</td>
<td>0.18</td>
<td>0.16</td>
<td>0.24</td>
<td>0.45</td>
</tr>
<tr>
<td>Found employment</td>
<td>0.69</td>
<td>0.74</td>
<td>0.74</td>
<td>0.63</td>
<td>0.40</td>
</tr>
<tr>
<td>Of those:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Success log$^§$</td>
<td>0.46</td>
<td>0.42</td>
<td>0.47</td>
<td>0.52</td>
<td>0.53</td>
</tr>
<tr>
<td>Recall$^§§$</td>
<td>0.21</td>
<td>0.26</td>
<td>0.23</td>
<td>0.13</td>
<td>0.10</td>
</tr>
<tr>
<td>Wage (DKK/hour)</td>
<td>186.54</td>
<td>188.95</td>
<td>187.04</td>
<td>183.64</td>
<td>178.18</td>
</tr>
</tbody>
</table>

| N                     | 141,551 | 41,124 | 56,906 | 31,342 | 12,179 |

Notes: ($^†$) Education: Degree level of highest finished education. ($^‡$) Employment rate: Fraction of time spent in employment in the year prior to unemployment. ($^§$) Success log: Fraction of spells ending in employment for which we can identify the successful application in Joblog. ($^§§$) Recall: Fraction returning to former employer.

Table 2.1 contains summary statistics for the individuals in our sample. We observe slightly more women than men, and the individuals are on average 38.5 years old when they start an unemployment spell. About half of these obtained a vocational degree. Women and older individuals seem to be more represented at longer unemployment durations and also

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32Because of logging requirements, our sample of job seekers is naturally more representative of the general population on UI benefits than other comparable papers. Both Skandalis and Marinescu (2019) as well as Faberman and Kudlyak (2019) have an over-representation of women, the young, and white-collar workers as they are more likely to use online job boards and to search online.
individuals with academic degrees. On average, there are no significant differences in terms of the prior wage across the varying unemployment durations.

Individuals in our sample log about 1.5 applications per week over the whole spell duration. This number is larger than in most related studies using data on individual applications, suggesting that we can observe a larger degree of job search.\textsuperscript{33} About half of the individuals in our sample enter at least one ALMP program over the duration of their unemployment spell, and individuals meet on average 6.7 times with their caseworkers. These numbers are increasing in the realized duration of the spell, insofar that with shorter durations there are fewer meetings, and less job seekers have exposure to activation.

In Table A.3 in Appendix A.2.3 we report some basic descriptives of the search portfolio for the job seekers in our sample (we report more detailed dynamics below). This overview suggests that there are differences in the breadth of the portfolios for individuals at various realized unemployment spell lengths. In general, individuals with longer durations have lower variance in their search targets which suggests that they may be rather narrow searchers, especially relative to those that find employment fast.

Coming back to Table 2.1 we see that a substantial fraction of our spells with longer durations is right censored (around 20\%) as they exceed September 2017. However, we observe eventual job finding for over two-thirds of our sample and an additional 10\% of our sample eventually leave the labor force. A considerable share of the successful job seekers returns to the same firm for which they previously worked (we refer to this as recalls). We can link 46\% of the formed jobs out of unemployment to an application in Joblog, i.e. we observe the eventually successful application. We may not be able to identify this application for the rest for two main reasons: 1) the successful application might not be logged, or 2) we could not match the reported employer to the firm identifiers.\textsuperscript{34} In Table A.4 in Appendix A.2.4 we explore differences in search patterns between those for which we observe the successful application and those for which we don't. Here we regress a dummy capturing whether we are able to observe a successful application for each spell on the search portfolio while sequentially controlling for demographics and characteristics of the previous or future job. We see that individuals that apply to a job with higher typical wages, as well as those that send more applications to related occupations/industries are more likely to have a recorded successful application. Perhaps surprisingly, we are more likely to observe a successful application for those that use their co-worker networks and search for part-time jobs. A higher fraction of informal applications is related to a lower likelihood of observing the success. However, when

\textsuperscript{33}Skandalis and Marinescu (2019) observe between 0.5 and 0.1 monthly applications per individual and assume to capture about 4\% of job search activity. Banfi et al. (2019a) report an average of 1.5 applications per unemployment spell and observe search for about 8\% of the average spell duration. Le Barbanchon et al. (2019) do not observe applications, but have access to reported reservation wages as well as desired hours and maximum commuting distances at the onset of an unemployment spell. While Faberman and Kudlyak (2019) see about 1.8 applications per week over the time individuals are active on the search platform, they observe only 2.1 weeks of search on average. In Section 2.4.1 we see that during active search (the search until the successful application is sent), our data shows around 1.75 logged applications per week.

\textsuperscript{34}We can match 84\% of the applications to a firm identifier and, as presented in Fluchtmann et al. (2019), individuals in our sample log about 80\% of jobs they apply to. Both of these fractions reduce the number of successful applications we could in expectation observe, assuming that it is random which applications we can match to a firm and which are logged. Further, the return to the former employer may also contribute to a lack of observing a particular successful application, assuming that this process may happen informal in many cases.
controlling for the job characteristics after (before) search, these patterns reverse (vanish). This might be driven sectoral differences in application requirements and behavior, i.e. specific sectors might not require formal applications. Our analysis of the patterns around successful applications may thus be slightly biased against sectors that rely to a larger degree on informal applications and individuals with smaller co-worker networks.

2.3 Econometric design

Our analysis of the dynamics of job search focuses on within unemployment spell changes in application patterns and on changes around particular events, such as contacts with the job center, exposure to active labor market policies and/or the eventual job finding. We estimate simple linear regressions of the number of logged applications or the share of specific job characteristic in the monthly application portfolio on a spell month dummy over our panel of unemployment spells. The main challenge in quantifying these patterns over the unemployment spell is dynamic selection. If finding employment correlates with, for instance, the number of applications sent or targeting of specific job characteristics, then individuals with longer unemployment durations will be a selected sample of those who did not send a lot of applications or targeted other job characteristics. Thereby the “raw” change in e.g. targeted job characteristics for different unemployment durations may simply reflect that different individuals contribute to this average. To take such patterns into account we include unemployment spell fixed effects in our primary specification:

\[ y_{ijt} = \alpha_i + \tau_j + \epsilon_{ijt} \]  

(2.1)

where \( y_{ijt} \) is the outcome for spell \( i \) at time \( t \) with unemployment duration \( j \). The dependent variable \( y_{ijt} \) thus measures the average characteristics of the application portfolio defined at a given level of aggregation \( t \) (e.g. a month or a week). Note that we set the specific job characteristic entries in the portfolio to missing if there is no recorded application in the given month while keeping the zero entry for the number of logs. In \( \alpha_i \) we capture unemployment spell fixed effects and \( \tau_j \) is an unemployment-duration fixed effect measured in weeks/months. The spell fixed effects account for observable and unobservable individual time-invariant heterogeneity and thus offer us the analysis of within-spell changes in job search. We cluster standard errors at the level of the unemployment spell.\(^{35}\)

Note that because we only include newly unemployed with full benefit eligibility in constructing our sample, unemployment duration \( j \) is also directly linkable to time until UI benefit exhaustion (UI eligibility is 2 years). In the figures below we show our estimates of \( \tau_j \) with and without the spell fixed effects included. Our empirical analysis has three distinct parts: First, in Section 2.4.1, we analyze the dynamics of job search as a function of unemployment duration. Second, in Section 2.4.2, we restructure the panel and focus on dynamics around contacts with the PES or exposure to ALMP. Finally, in Section 2.4.3, we compare successful applications to unsuccessful applications to identify differences between the two. For the first part of the analysis we look at the number of logs or the characteristics of the application portfolio defined

\(^{35}\)In parts of the analysis we are dealing with the number of applications which can be thought of as count data. To get a sense of the real relative magnitude of the estimated effects we therefore also run Poisson regressions. We can view the obtained coefficients as semi-elasticities and thus infer the effects relative to the base level.
at the monthly level, i.e. we look at the average or share of the respective job characteristic per month. For the second and third part, we focus the analysis on the weekly level to enable a richer characterization of job search around events. Our results are robust to these choices concerning the level of aggregation.

2.4 Empirical results

2.4.1 Job search during the unemployment spell

In this section, we explore the application behavior over the unemployment spell. We start by describing search behavior and application methods used by the job seekers. We then look closer at the characteristics of the jobs applied to. In all steps below we focus on the first year of the spell, as the majority of individuals (approx. 80%) will have either found employment or left the labor force by the 52 week mark. We provide a range of robustness checks in Appendix A.3.2 and discuss parts of them at the end of this subsection.

Search portfolio

In the left panel of Figure 2.2 we display the number of logged applications per month over the duration in unemployment. The before-mentioned phasing-in period is striking as the number of applications in the first month is about a third lower than in the following month. The raw time profile suggests that the number of logged applications is fairly stable over the unemployment spell. However, when controlling for spell fixed effects, we see the number of logs gradually decline from around 7.1 applications in the second month to 5.8 applications at the end of the first year. This is qualitatively similar to Faberman and Kudlyak (2019) and it is also in line with previous work suggesting that the time spent on job search in declining over the unemployment spell (Krueger et al., 2011).

The change in the time profile when including spell fixed effects suggests that individuals who end up with longer unemployment spells are sending more applications than individuals who find jobs earlier in the spell. In this sense, dynamic selection goes opposite of what one might have expected. We illustrate this in more detail in the right panel of Figure 2.2. This plot shows the average number of logged applications separately for individuals with different unemployment durations. The dynamics suggest that individuals with shorter unemployment spells log fewer applications on average over most of the spell with the exceptions of the first month. Here all groups seem to log roughly similar numbers. Interestingly, these separate duration groups exhibit gradual declines in logged applications after roughly half of their spell length. In the first half of the respective spells, almost all groups log about 1.7 applications per week which falls close to the average logging requirements set by the UI funds, see Table A.1 in Appendix A.2. Faberman and Kudlyak (2019), who report similar evidence, note that this is at odds with canonical job search models such as Pissarides (2000). In their interpretation,

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36We do not look at the dynamics around benefit exhaustion (104 weeks) for two reasons. First, the current version of available labor market data implies that we have very few people who are unemployed for these durations. Second, given that the majority of those initially unemployed finds jobs within the first year, focusing on the successful and unsuccessful applications within the first year implies that our results are valid for a much larger pool of unemployed. For an analysis of the change in job search around benefit exhaustion, see e.g. Skandalis and Marinescu (2019).
individuals with lower prospects of getting a job offer might substitute lower returns to search
with the provision of more search effort in the form of sending more applications throughout
their spell.

While these patterns appear consistent with explanations of stock-flow matching (e.g.
Gregg and Petrongolo, 2005), the declining pattern in the number of logs may also be driven
by reductions in made and/or logged applications after the successful application is sent (see
Section 2.4.3). In order to examine whether this behavior drives the results in the right panel
of Figure 2.2 we run the same analysis on the pool of spells for which we can identify the
successful application and exclude all logs after this point. Figure 2.3 plots these patterns and
shows that, with the exception of spells lasting shorter than 10 weeks, individuals log roughly
the same amount of applications over the spell, irrespective of the realized spell length. Thus
our results do not correspond to the findings of Faberman and Kudlyak (2019).

In the first two panels of Figure 2.4 we plot the evolution of formal and informal appli-
cations over the unemployment spell. Formal channels cover applications sent to officially
posted vacancies, making up most of the application portfolio at all times. The share devoted
to this channel is increasing from about two-thirds in the first month of unemployment to
80% of all applications by the end of the one year mark. Naturally, the inverse proportion
of applications, those sent through informal channels (e.g. unsolicited applications, being
contacted by a headhunter or using the network), is decreasing proportionally. As informal
applications may also link to coworker referrals we examine the share of applications going
to firms that employ a former coworker in the third panel (b). Note that this may also include
recalls as the coworker network comprises former colleagues (21% of all exits to employment
in our sample are exits to the same firm id as that held prior to unemployment, see Table 2.1).
While the raw dynamics of the number of logged applications are gradually decreasing after a
phasing-in period, similar to the number of applications, controlling for spell fixed effects flat-
tens the path to around 5% of the portfolio. In contrast to the number of applications, dynamic
selection only plays a very minor role for the search channels as controlling for individual spell

Figure 2.2: Spell dynamics - Avg. number of logs

![Figure 2.2: Spell dynamics - Avg. number of logs](image)

Note: The left Figure shows the coefficients from regressing the number of joblogs on unemployment spell
month dummies. The right panel shows the coefficients from regressing the number of joblogs on
unemployment spell week dummies, separately for groups leaving unemployment at different points of
the spell. This version controls for individual spell fixed effects. Standard errors are clustered on the spell
level and vertical bars display 95% confidence bands.
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**Figure 2.3:** Avg. number of logs per week before success

![Avg. No. of Logs By Spell Duration - Spell FE](image)

Note: The Figure shows the coefficients from regressing the number of joblogs on unemployment spell week dummies, separately for groups leaving unemployment at different points of the spell and conditional on having a recorded successful application. This version controls for individual spell fixed effects and excludes all applications after the successful one. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.

The last three panels of Figure 2.4 display dynamics in the methods used to send the applications. We see that over the spell, traditional applications, i.e. sending letters or e-mails, are most common and increasing. The share of applications classified as personal is gradually declining. Spell fixed effects reverse these patterns for traditional methods and flatten the dynamics for personal applications. This suggests that individuals who are successful in quickly finding employment are more likely to use personal channels, relying less on traditional methods. The exhaustion of personal channels, such as coworker networks, may also partly drive these findings. Over the spell, both in the raw setting and when controlling for spell fixed effects, the share of applications sent through web-platforms (and social media) is growing. This may indicate that over time individuals may broaden their search to consider more firms they are initially not aware of and which they can find on online job boards.

**Job characteristics**

Next, we zoom in on the specific characteristics of the jobs that individuals apply to. In Figure 2.5 (a) we show the time profile of the average typical wage and AKM firm fixed effects in the

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37 In principle, this could also be driven by differences in how dynamic these individuals are, i.e. by having been employed in many companies they were exposed to a larger network. If individuals with a higher return to search are more likely to be employed at larger firms, then this can also rationalize these patterns. The inclusion of spell fixed effects should however capture these individual differences.
2.4. Empirical Results

**Figure 2.4:** Spell dynamics - Application channels and methods

(a) Application channels

(b) Coworker network and application methods

(c) Application methods, continued

Note: This Figure shows the coefficients from regressing the share of certain application channels (a) or methods (b2 and c) on unemployment spell month dummies. Formal search channels contain applications that go to officially posted vacancies, informal applications contain unsolicited applications, being headhunted or using the network. We zoom into the coworker network separately (b1). Traditional applications are sent via letter/e-mail, whereas personal applications through a phone call or personally showing up. Online submissions contain applications sent through online application forms or social media. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
application portfolio. Although there is a small decrease in their time profiles over the duration of the unemployment spell when controlling for spell fixed effects, the main takeaway is that there are only minor dynamics in this dimension of job search. The change in wages (AKM firm fixed effects) is less than 0.015 (0.004) log points over 12 months of unemployment. Even though the typical wage measure is not synonymous with the individuals’ reservation wage, this finding relates to previous empirical work which generally reports low dynamic reservation wage dynamics over the duration in unemployment (Addison et al., 2009; Schmieder et al., 2016; Krueger and Mueller, 2016). The flat “raw” dynamics suggest that individuals at longer spell duration on average target jobs with higher typical wages. The small changes in typical wages may partly result from a large degree of wage coordination and collective agreements on minimum wages which may be binding out of unemployment in the Danish labor market. However, it could also mirror that individuals only marginally change the type of jobs they target, at least for factors which affect the payment.

As evident in Figure 2.5 (b), only a small fraction of applications are sent to part-time positions and again, there is very little gradual adjustment. At the end of the first year, the number of applications to these positions increase only by about 2-3%. There is a minor evolution in the average commuting distance (in minutes) as commuting times increase from 43.5 to about 45 minutes over the first four months of the unemployment spell. There is thus only a modest tendency that individuals spatially broaden their search during early durations of an unemployment spell. After these initial 4 months, the average commuting distance becomes fairly stable. This limited spatial adjustment is in line with a distaste for distance (Marinescu and Rathelot, 2018; Le Barbanchon et al., 2019).

Figure 2.6 (a) shows the evolution in the extent to which individuals apply to occupations/industries highly related to their previous position (at our level of aggregation). We think of this as a measure of the broadness of search, that is, how mobile the job seekers are in the occupations and industries they apply to. This can be seen as somewhat related to Belot et al. (2018a) who show that broader search may be linked to increased success in search for employment. Again, the figure reveals only little dynamics once we account for spell fixed effects. Comparing the raw profile to the spell fixed effects version shows that there is some dynamic selection on whether individuals apply to occupations/industries highly related to their previous position. This difference suggests that individuals with longer unemployment durations send fewer applications to jobs that relate to their skills and experience. This observation might also be driven by a lack of available vacancies or employment histories in rather narrow markets. In Figure 2.6 (b) we further see that individuals do not seem to adjust their search across the within commute-area occupation and industry specific unemployment rate, which we think of as a proxy for the competition in a local labor market. There is some dynamic selection such that individuals at a longer duration apply to occupations with lower

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38 Note that by construction there are no (typical) wage differences for the same position in our dataset, and therefore these results do not speak to the role of frictional wage dispersion. The flat “raw” dynamics instead reflect that individuals are not making large changes in the characteristics of the job they apply to. Furthermore, as is evident in Figure A.1 in Appendix A.3.1, the decrease in typical wages is very small relative to their overall distribution.

39 Marinescu and Rathelot (2018) do not report dynamics, yet find a strong distaste for distance. Le Barbanchon et al. (2019) do not find any significant effects of of the potential benefit duration on the desired number of hours, duration of labor contract and commuting time/distance. Both thus suggest that a) individuals do not like distance and b) therefore do not adjust their search on this dimension as a response to the UI environment (e.g. benefit exhaustion).
2.4. Empirical results

Figure 2.5: Spell dynamics - Job characteristics

(a) Typical wages and AKM firm fixed effects

(b) Hours and commuting

Note: This Figure shows the coefficients from regressing specific job characteristics on unemployment spell month dummies. Typical wages are average wages for a certain firm/occupation combination. Firm fixed effects are obtained from an AKM model using matched-employer-employee data. Full-time positions are classified as positions with at least 37 hours of work per week. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the jobs’ postal code. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure 2.6: Spell dynamics - Relative to previous job characteristics

(a) Related industries and occupations

(b) Competition

Note: This Figure shows the coefficients from regressing job characteristics relative to the individuals’ prior occupation/industry on unemployment spell month dummies. Relatedness measures in (a) are based on the top 10 most related 3-digit occupations/industries in the O*NET career mover matrix or Nefikε and Henning (2013). Competition measures in (b) are based on the within commute-area unemployment rate in the 2-digit occupation/industry applied for. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
2.4. Empirical results

competition, yet the magnitude of this change is low. In general, these dynamics correspond well to the measures above and might thus be driven by the same factors.

Job characteristics for specific worker groups

While the summary statistics in Table 2.1 do not suggest that individuals differ a lot across groups of varying unemployment duration, there might still be some nuances in search strategies across subgroups of our sample. Thus, we additionally report a subset of the dynamics for specific worker groups in Appendix A.3.2. We report differences between different age groups and gender. While there are some interesting level differences, the dynamics over the spell are rather stable and very similar across groups. In terms of notable differences in levels, it seems that younger unemployed at the beginning of their career log more applications and also consider a larger spatial distance for the jobs they apply to. This is likely driven by a higher mobility earlier in life. Men apply to jobs with higher typical wages, but also consider positions that are farther away from their municipality of residence. On the other hand, women are less likely to use informal channels and devote three times as much of their applications to part-time positions. For a more detailed examination of gender differences in job search, we refer to Fluchtmann et al. (2019).

Robustness checks

In Appendix A.3.2 and Figures A.4 to A.6 we present two types of robustness checks. First, we show how the estimates of $\tau_j$ change after including quarter dummies to our specification with spell fixed effects in order to account for seasonality and take out time trends other than the spell dynamics. This exploits that we have variation in when the spell starts as we have different unemployment durations for each time $t$. Seasonality may affect patterns if e.g. certain vacancies are not available at specific points of the year and there at the same time is an over-representation of specific long term unemployed. In this case we may wrongly conclude that the long-term unemployed do not apply for such vacancies while in fact they may when these vacancies are available. Second, we assess the robustness of our results to the impact of extreme values for certain job characteristics by additionally reporting the baseline model while trimming the dependent variable below the 10th and above the 90th percentile. The dynamics in our robustness checks generally change very little, but unsurprisingly we see some differences in the levels when trimming the outcome characteristics. This results in shorter average commute times of the positions applied for, a smaller amount of applications to network contacts and lower competition as well as through less informal channels and to part-time jobs. This suggests that a part of the outcome levels is driven by more extreme values, such as far commuting distances or exclusively coworker network applications in a given month. Nevertheless, none of these changes significantly affect the dynamics we observe.

For completeness, we extend the time window to 20 months in Figure A.7. Note that we have only few people who are unemployed for these durations, and the majority of those

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40 We have also looked at other dimensions such as educational background and previous unemployment history, but did not find interesting differences in terms of dynamics.

41 This concern is likely to be more relevant at lower levels of aggregation. Our broad measures of industries and occupations ensure that there is a large number of unemployed and vacancies in the different bins.
initially unemployed find jobs within the first year. However, we do not see major changes in the dynamics relative to the first year in unemployment.

Discussion

Our results suggest that the targeted job characteristics and the number of logged applications remain largely unchanged over an unemployment spell (at least in the dimensions and the level of aggregation which we focus on). It therefore does not appear that individuals are to a large extent changing their search strategy or the jobs they target in response to prolonged unemployment. Note that we focus on the first 12 months of unemployment, thus these results do not convey what happens closer to benefit exhaustion. Previous work suggests that changes in job search may occur at this point in response to the upcoming end of benefit payments (e.g. Skandalis and Marinescu, 2019). The 12 months window instead speaks to the general role of dynamics in job search - or the lack thereof. Our results suggest that even though individuals send a large number of applications over the spell, they generally do not change the type of jobs they target nor their search method. These results are therefore partially conflicting with models of learning during continued exposure to unemployment as well as with other models in which job search is a dynamic process, and where individuals are trying to find out which jobs they could get. Our results are in line with previous empirical work on this issue (Addison et al., 2009; Schmieder et al., 2016; Krueger and Mueller, 2016; Le Barbanchon et al., 2019) which primarily examines changes in reservation wages. Mueller et al. (2019) show that job seekers are not revising their job search related beliefs downward, even under prolonged exposure to unemployment. The lack of notable changes along targeted job search characteristics might therefore be driven by the absence of adjustments in beliefs. Our results further conflict with the findings of Faberman and Kudlyak (2019) who find that the number of weekly applications is positively related to the realized length of the unemployment spell. In contrast to their data, which may be subject to attrition as job finding is deduced from stopping to apply on the SnagAJob.com job search platform, we can properly identify job finding and spell lengths through administrative data.

Nevertheless, there is some adjustment in the search channels as individuals use more formal search channels at longer unemployment durations, yet rely less on traditional application methods. Further, we see small differences across specific subgroups in the data. While there are some distinct level differences across job characteristics and application channels, these discrepancies are rather stable over the unemployment spell and thus exhibit no significant differences in the dynamics.

Our findings are also interesting given the importance of dynamic selection that we have shown above. While individuals with longer unemployment durations are rather “similar” on observables at inflow into unemployment, they seem to search quite differently in terms of the number of logged applications, the use their network et cetera. As previously argued, individuals may in fact be rather narrow job searchers, at least relative to those that find employment fast. This could suggest gains from altering the search strategy of some of the unemployed, for instance through interactions the job center or other type of information.

\footnote{Note that the results do not rule out dynamics on the effort dimensions. Therefore, learning and dynamic adjustment might still play a role for search intensity and the number of applications sent.}
To look closer at the extent to which job search may change in response to new information or changes in the overall job search environment, we next analyze the dynamics of job search around interactions with the public employment services. As these interactions happen regularly throughout the unemployment spell, small changes in search behavior may be hard to detect in the aggregate patterns studied above. We therefore refocus the analysis to event type studies and examine the evolution in job search around these interactions.

2.4.2 Dynamics around interactions with the job center

In this section, we analyze dynamics around caseworker meetings or exposure to ALMPs. The aim of this analysis is to quantify whether (and how much) job search changes in response to these interactions. We can think of these events as either carrot- or stick type interventions that may change the information- or skill set of the unemployed and/or serve as a tax on leisure. The latter would reduce the individual valuation of unemployment and thus increase the gain from finding employment. As mentioned in Section 2.2.1, we may expect to see threat-, lock-in- and post-program effects. Naturally, opposite effects may also arise if individuals are viewing upcoming program participation as attractive. Our empirical analysis below can to some extent be used to assess the importance of these competing interpretations. To analyze these effects, we select all the spells in our sample that have recorded participation in either ALMP or caseworker meetings. We then restructure the panel to define the time relative to the event, i.e. either the activation start week or week of the meeting. Note that there is potentially some dynamic selection in both the dynamics prior to and especially after participation in ALMP. Individuals may enter activation very early in their unemployment spell or find jobs after a few weeks of activation, thus entering the panel late or leaving it quickly. We therefore only report versions controlling for spell fixed effects. Also note that if there were large changes in job search over the unemployment spell, it would also be important (and possible) to control for unemployment duration fixed effects. This would distinguish dynamic patterns from systematic patterns that relate to individuals being unemployed for long. Note that we control for these patterns using the whole panel of unemployment spells, and that spell fixed effects are also identified from the whole sample, not only the window we display on the graphs.

Dynamics around activation

As a starting point, Figure A.8 in Appendix A.3.3 shows the evolution in the number of logged applications approaching program participation for different types of activation programs.

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43 Below we assume that the reporting effect, which is the choice of the job seeker on what type and how many applications to log, is unchanged around the time of participation in meetings and activation programs. This implies that we abstract from situations where job seekers purposefully log a higher number of applications right before a meeting to signal sufficient search. In the robustness section we show that individuals who already log a lot of applications before a caseworker meeting show similar dynamics to those we report below, this suggests that even individuals who are already logging sufficiently also increase their search effort.

44 We only have data on actual activation or meetings, but not on intended events. Individuals need to have taken part in these measures to appear in this part of the analysis. This implies that those who respond to the "threat" by searching higher and securing employment prior to activation will not appear in our sample.
The vertical red bar at week 0 marks the start week of program participation. We can thus interpret changes in job search prior to week 0 as evidence of threat effects, while later changes are evidence of either lock-in effects or post program effects. Our setup is thus well-suited to study pre-program effects while post-program effects will be a mixture. Note that the program duration differs (even within ALMP measures). Hence, some individuals will have completed participation early while others are subject to longer durations. The figure documents stark differences in the dynamics of logged applications: some activation types show evidence of threat effects while others show the opposite pattern. Self-selected education programs, which are programs where the content is to a large extent actively chosen by the unemployed themselves, show large lock-in effects and an absence of threat effects. For programs such as wage subsidies and internships, it is similarly not surprising that individuals are searching less and that threat effects are not very pronounced as these schemes are often planned together with the unemployed. Additionally, these set-ups partly resemble “regular” employment. Since the ALMP measures are very different, we focus our analysis below on skill development and ordinary education measures only.\footnote{Skill development is the most common ALMP measure (50\%) while ordinary education is less common (12\%), yet still an important part of the activation regime and focused on the upgrading of skills.} In these programs, and especially for skill development, individuals are to a larger extent forced into participation. This makes the analysis of threat effects for these activation measures particularly interesting. Further both type of programs aim to improve skills and allow participants to search for new types of jobs, potentially affecting post-program dynamics.

**Figure 2.7:** Avg. no of logs around activation - Skill development (left) and education (right)

Note: This Figure shows the coefficients from regressing the specific number of logged applications on weekly dummies relative to a starting skill development or ordinary education measures in week 0. Base-level (red square) is three months prior to activation. Standard errors are clustered on the spell level and vertical bars display 95\% confidence bands.

Figure 2.7 shows the evolution in the number of logged applications approaching participation in either ordinary education or skill development programs. For skill development, we see a clear increase in the number of logged applications until the early weeks of program participation. The number of applications declines slowly after the start of participation. This is interesting as individuals, despite being “locked-in” to the program (average duration of 3 weeks), don’t immediately reduce their application behavior to pre-threat levels. Further, the logging dynamics settle on a stable level, approximately 0.1 applications per week higher than...
at the base level, about 5 weeks after the program started. This could hint at the presence of a
counterpart "post-program" effect after the completion of the course. The specific effects seem to
dampen at longer durations as controlling for the time spent in unemployment reduces both
threat- and post-program effects by a third.

As we are dealing with count data and in order to get a sense of the real relative magnitude
of the effects which are hard to infer from the figures above, we also run Poisson regression on
the number of logged applications. In this case we can view the obtained coefficients as semi-
elasticities and thus infer the effects relative to the base level. We present these outcomes in
Figure A.12 in Appendix A.3.3 which shows that at peak, the threat effect for skill development
programs increases logged applications by about 14% (10% when controlling for UI duration).
The post-program effects range about 5% above the base level. As before, threat effects are not
significant for ordinary education, yet lock-in- and post-program effects reduce the number of
logged applications by about 9 to 10%.

Further, the application patterns around activation could also pick up behavior related to
caseworker meetings. These show concentrated increases in the number of logged applications
(see next subsection below) and, while being of much higher frequency during the spell,
typically precede the enrollment in activation measures. Thus, we examine the logging behavior
around activation while controlling for time since the last and time until the next meeting in
Figure A.13 in Appendix A.3.3. In practice, this does not lead to major changes relative to the
specification only controlling for spell fixed effects, except for a slightly reduced threat effect
leading up to skill development courses.

While the number of logged applications increases in response to expected participation in
skill development, we see limited changes in the type of applications individuals send. Figure
A.10 (in Appendix A.3.3) shows that time profiles are mostly flat. We do not see large changes
in the dynamics regarding typical wages and firm fixed effects, especially when controlling
for unemployment duration along with spell fixed effects. This is not surprising given our
previous results that the targeted wages generally change very little over the unemployment
spell. There is some evidence in favor of applications becoming less likely to be informal prior
to activation start. This decrease continues after program participation starts. However, all the
threat effects on informal applications vanish once we control for unemployment duration
at program exposure and instead exhibit possible lock-in and post-program effects. In this
specification we also observe small reductions in the applications to part-time positions. As
before, all effects are small in magnitude however.

For ordinary education programs, we do not see evidence of threat effects in Figure 2.7.
Here we instead see a clear change in the number of logged applications during and after
program participation (average duration of 6 weeks). The effects we see correspond well to
potential lock-in effects as the individuals have a decreased and rather stable application
profile for 6 weeks after program start. Following this, the number of logged applications
stays well below pre-activation levels (about 0.15 applications per week). We may interpret
this as a sign of post-program effects where individuals send less, but perhaps more targeted

\[\text{In Figure A.9 in Appendix A.3.3 we additionally report a Poisson regression version for all activation}
\text{measures.}\]

\[\text{As reactions to caseworker meetings are concentrated on a short interval, we control for weeks}
\text{until/since meeting on the [0,4] interval.}\]

\[\text{We only report a selected range of outcomes in order to economize the space. We do not observe}
\text{changes along the other dimensions of job characteristics which are not included here.}\]
applications. It could also be driven by an increase in the quality of each application at the expense of the quantity. Controlling for unemployment duration only marginally shifts the dynamics downward. The effects on types of applications sent are roughly similar to those in skill development in that we don’t see major changes in how and where individuals apply. Note that the small effects on informal applications after controlling for unemployment duration are not present here.

An interesting finding above is the change in the number of logged applications, especially for programs with skill development. In order to get a sense on whether the potential threat effects on logged applications before exposure to skill development programs translate into actual employment effects, we regress exit-to-employment indicators on a dummy for the two months after after skill-development exposure for all spells in our panel. If there are actual employment effects, we would expect the exit-to-employment probability to be significantly higher than at base-line. However, controlling for the number of applications placed in the weeks before activation should in principle remove (or at least substantially reduce) these effects. We present this regression in Table A.5 in Appendix A.2.5. In all of the regressions we restrict on applications between weeks 6 and 52 in order to reduce the potential impact of joblog phasing-in, duration dependence and the approach of benefit exhaustion on the hazard rates.

As it turns out when looking at columns (1) and (2), the exit-to-employment in the two months after skill development programs is in fact about 1.5% higher than at base-level and reduces to roughly 0.6% when controlling for the number of applications sent in the weeks immediately before entering the program. We thus see this as a hint that the threat effect on reported applications raises the exit to employment. The table also shows that the exit probabilities are reduced in the early weeks of activation (see columns (3) and (4)).

Dynamics around meetings

Next, we turn to the effect of caseworker meetings at the municipal job center. As caseworkers are actively using the logged search activity to assess UI benefit eligibility, threat effects can still be present, though of perhaps a somewhat different nature. To signal active search, individuals might increase their search effort prior to the meeting.

As job search guidance is a major component of these contacts, job seekers may also significantly alter their search strategy, either to comply with requirements set by the caseworker or to incorporate advice. Lock-in effects are unlikely in this setting as meetings do not strongly crowd out time available for job search. We plot the dynamics in the number of logs in Figure 2.8. In contrast to activation

---

49 Note that if an individual can show that she will start employment within 4-6 weeks, she is typically exempt from activation in the weeks until the job starts. This means that we will usually not observe any employment exits during program exposure. As skill development courses last on average 4 weeks, we thus do not expect to see a high number of employment exits over these weeks. See https://info.jobnet.dk/jobs%C3%B8ger/det-praktiske/hvis-du-bliver-ledig/n%C3%A5r-du-f%C3%A5r-job (in Danish).

50 In all of the regressions we restrict on applications between weeks 6 and 52 in order to reduce the potential impact of joblog phasing-in, duration dependence and the approach of benefit exhaustion on the hazard rates.

51 As evident in columns (5) and (6), there are by construction no employment exits in the weeks before starting skill development (individuals only enter our data if they actually enter a program, not for intended programs).

52 This can potentially also be driven by a reporting effect, i.e., those that did not meet the requirements for active search before the meeting do so immediately before to convince the caseworker of active search.
measures, threat effects appear concentrated in the week of the respective meeting itself as well as in the week immediately before.\footnote{This might be driven by the fact that many meetings are scheduled on short notice. See https://www.ca.dk/sites/default/files/Ledig/PDF/worth_knowing_june_2019.pdf: “With a day’s notice you must be able to attend all meetings and seminars to which the job center, secondary operator or [the UI fund] invites you.”} We see that individuals increase the number of applications by about 0.11 in the meeting’s week. This is followed by 0.18 applications above the baseline level in the immediately following week. After this, the number of logged applications drops sharply and falls back to the baseline level about a month after the meeting.

\begin{figure}[h]
\centering
\includegraphics[width=0.5\textwidth]{figure28.png}
\caption{Avg. no of logs around caseworker meeting}
\end{figure}

Note: This Figure shows the coefficients from regressing the specific number of logged applications on weekly dummies relative to a caseworker meeting in week 0. Base-level (red square) is three months prior to the meeting. Standard errors are clustered on the spell level and vertical bars display 95\% confidence bands.

During the meetings, individuals might get motivated to search more or threatened with potential sanctions in case of insufficient search activity. Both could drive the spike in logged applications, though from these dynamics it is unclear whether it is simply a reporting effect or reflects a true increase in the number of applications sent. To identify whether these threat responses are likely to be driven by a lack of reported activity prior to the meeting, we perform the analysis on those that already report a high number of applications earlier. We chose individuals above the 75th percentile in terms of average number of logs in the month prior to the base level (thus excluding the threat effect).\footnote{Here we also exclude exposure to the first and the last meeting for each spell. See Section 2.4.2 for details on why behavior might differ here.} In Figure A.15 in Appendix A.3.3 we report these dynamics, and it turns out that the threat effects on those that continuously report rather high numbers of applications are comparable to the overall sample. However, after the meeting the number of applications falls to a lower level, and the threat effect has a somewhat earlier onset. This is contradictory to a story where the spike in logged applications around meetings is driven by a reporting effect and supports the hypothesis that individuals react to the meeting itself by increasing the search effort. We again report a Poisson regression version in Figure A.16 in Appendix A.3.3. We infer that the number of logged applications is raised 7.5\% above
the base level at the week of the meeting and about 10% in the week thereafter (7-7.5% when additionally controlling for UI duration).\textsuperscript{55}

We report the effects on selected job characteristics in Appendix A.3.3. The Figures suggest that meetings change job search along some dimensions. However, due to the rather high frequency of meetings over the unemployment spell, the specific time of these events might be very important. Including controls for unemployment duration removes all dynamics preceding and following the meeting. We therefore conclude that meetings actually appear rather ineffective in changing the type of jobs individuals target. On the extensive margin, i.e. the number of logged applications, we see increases immediately around the meeting. This suggests that threat effects are still active, yet only on the dimension of logged applications - similar to the patterns for ALMP programs. Interestingly, we see negative post-meeting effects with about 0.3 less applications per week than at the base level.

\textbf{Robustness checks}

In Appendix A.3.3 we report robustness checks where we additionally control for quarter-fixed effects as well as trimming the respective outcome below the 10th- and above the 90th percentile. Controlling jointly for time until event, unemployment duration and quarter fixed effects is possible because at different calendar months we have different individuals with different unemployment durations and different times until events $e$ take place. In Figures A.17 and A.20 we consider ALMP measures. We see that these controls do not significantly change any of the previously observed patterns on job outcomes, search channels or the number of logged applications - neither for ordinary education nor for skill development. Figure A.23 shows the corresponding checks on caseworker meetings. We again see that the effects on the number of applications and other dimensions do not change significantly relative to the spell fixed effects model above.

The effects of activation programs, especially the threat effect, might differ at first exposure to ALMPs. Further, the last activation measure before job finding may induce specific changes in search behavior that lead to the eventual success. Therefore we single out the first- as well as the last activation measure for all individuals in our sample, shown in Figures A.18 and A.19 for skill development as well as in Figures A.21 and A.22 for ordinary education programs. Overall, we do not see vast changes relative to the prior regressions that focus on the whole spell. Note that there is a lot less variation in the unemployment durations for these regressions, thus we do observe particularly large standard errors when controlling for the time spent in unemployment. We run a similar exercise for caseworker meetings in Figures A.24 and A.25. Except for a much higher threat- and post-exposure effect on the number of applications around the first meeting, potentially due to the simultaneous introduction to joblog, we do not see interesting changes here.\textsuperscript{56}

\textsuperscript{55}We do not pursue exit-to-employment regressions similar to Section 2.4.2 here as identifying these with respect to meetings is harder. As evident in Table 2.1, meetings occur at a much higher frequency than activation measures and by construction, most meetings won't lead to employment. In fact, only the last meeting we observe for each individual can potentially have an immediate employment effect. At the same time many individuals might have already secured a job offer at the time they meet with their caseworker a last time before exiting to employment (see Section 2.4.2 below).

\textsuperscript{56}At the last meeting we do not observe any threat effects any more. This could have two possible reasons: First, individuals might have gotten used to the UI system, that is, which search behavior is
Discussion

The above analysis shows that activation measures have a rather limited impact on the jobs that individuals target and the channels that job seekers use. While we see some changes on typical wages, commuting time, informal applications and part-time jobs around meetings with caseworkers, controlling for the time spent in unemployment removes these dynamics. However, both caseworker meetings and activation measures change the number of logged applications with hints of threat- and lock-in effects. It is perhaps not surprising that individuals are mobile in adjusting the effort dimension, rather than the jobs they target as these changes can entail more long-term consequences than solely devoting more time and effort to search. If individuals consider more distant and lower-paying jobs, they will have to live with lower wages and longer commute times if successful. This might well be an acceptable trade-off for some individuals, especially for present biased job seekers who disproportionately discount these consequences (DellaVigna and Paserman, 2005). However, our analysis shows that the program effects are concentrated on the number of applications logged which is a proxy for the effort involved. The results are not necessarily surprising given the modest (short-term) employment impacts of ALMPs (Card et al., 2017; Kluve, 2010) and the absence of a significant adjustment of search in general. However, we see some indicative hints of increased exits to employment after exposure to skill development.

The evidence on threat effects is also in line with previous work. To the extent that these increase search activity, which translates into an increase in job finding, our results are consistent with Maibom et al. (2017) who show that case worker meetings can be a very effective tool in improving job finding (see also Rosholm and Svarer, 2008). Furthermore, Maibom (2019) suggests that an important driver behind this change in job finding is individuals responding to the threat of meetings (see also Black et al., 2003). Furthermore, we see similar dynamics to those reported above when we only focus on individuals who generally log many applications in the pre-phase and thus should not be at risk of searching inadequately. Finally, note that we do not see the number of applications drop immediately after a meeting at the job center which should be expected if individuals are simply changing when and what they log. The findings instead suggest that the threat effects accumulate into real changes in the number of logged applications.

2.4.3 Job finding

Our analysis above has shown that job search, in terms of the characteristics of the targeted jobs, is fairly stable as it does not seem to change with time spent on unemployment benefits nor as a response to participation in ALMPs or caseworker meetings. From our analysis it is, however, not clear whether the “stability in job search” is good or bad. In this section, we examine whether gains could be made from changing job search by comparing the type of jobs individuals get to the type of applications they send. We do this in two ways: First, we look closer at the dynamics of job search around job finding and compare the characteristics of the (eventually) successful application to applications earlier in the spell. Following this, expected from them and to which degree potential sanctions are enforced. Second, the individuals might have already secured a job offer at the time they get together with the caseworker. Following, we see a strong decline in the number of logged applications, which hints at the latter explanation where caseworkers might become more lenient in the search expectations if the unemployed already have a job offer.
we examine the relative success rate among different application types, i.e. whether targeting specific job characteristics or using certain search channels improves the chances of finding a job.

If the successful application is "similar" to the pool of applications, it suggests that getting employed is to a large extent related to waiting for a "lucky" draw from the stock or flow of vacancies or just the right match. Similarly, eventual job finding may require a gradual change in the jobs targeted until individuals have identified jobs where employers want to hire them. If alternatively, search behavior around the weeks of the successful application differs from previous weeks in unemployment, it may suggest that there could be economic gains from helping job seekers change behavior earlier in the spell. A similar point holds if we observe that the successful application across individuals is different to the (within individual) pool of applications. This would suggest that job seekers are spending time and effort sending out applications that are less likely to result in job offers. Note that the job seekers' success depends not only on the application, but also on the pool of competitors and the employers' preferences. Our results are therefore only partial and should thus be interpreted with care.

To distinguish the potential stories, we focus the analysis on the dynamics leading up to the successful application. As laid out earlier, we identify the successful application as the last logged application to the same occupation and firm combination as the job we observe individuals holding in the administrative data after unemployment. As before, we restructure our panel to measure time until sending the successful application (week 0). We again consider the time on the weekly level to capture even short-term changes in search behavior and application targets that might lead to a job offer.

**Dynamics and the successful application**

Figure A.27 in Appendix A.3.4 shows the evolution of job search targets and channels leading up to the successful application. While the targeted typical wages are fairly stable leading up to success, we see that the successful application itself has a significantly lower commute time than applications in the weeks before. Interestingly, the successful application is also more likely to go through the individual's coworker network as well as to an industry related to the individual's previous employment. We also see that the overall number of applications to the specific firm/occupation combination is vastly higher in a three-week window around the successful application as we see in Figure A.27c in Appendix A.3.4. This thus the job seeker's applications often seem to target rather new vacancies which suggests that individuals may wait for the right vacancy to occur. While the figures show some smaller changes in targeted job characteristics as we approach week 0, the key takeaway is that the successful application differs notably from behavior in the weeks prior to success. This is incompatible with the hypothesis of dynamic adjustment until placing the successful application.

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57 In Figure A.27c we plot the distribution of applications going to the same occupation/firm combination (we interpret this a weak proxy for a specific vacancy) relative to the week of the successful application. As we see more applications to the same job around the week of the successful application, we interpret this as an indicator for the recent opening of the respective vacancy.

58 Note that the availability of several job offers the job seeker can choose from might also drive the patterns we observe. In this case it would not be surprising that the successful application has more favorable characteristics, such as a higher wage and less commute time, as the individual could choose the most appealing offer. However, we do not consider this to be a likely explanation for the observed differences to the application portfolio for two reasons: First, the individual job seekers have been unsuccessful in
To illustrate this further, we run a series of regressions comparing the successful application to the pool of applications sent earlier in the spell. The left-hand side for each separate regression is a specific characteristic of the job or the search portfolio (e.g. the share of applications sent through informal channels), while the independent variables are a dummy for the successful application and several potential controls and fixed effects that we add sequentially. Table 2.2 displays the estimate of the coefficient on the successful dummy from these regressions. We also estimate a version where we control for a vast set of job characteristics, excluding the left-hand side job attribute and a version additionally controlling for potential recalls to the previous employer.

Our estimates suggest that, when comparing the within-individual variation across our sample, we do see systematic patterns in the characteristics of the successful application. This is not a result of particular individuals using a specific search strategy more often. Instead it suggests that the average applicant - all else equal, including prior search - might be more likely to be successful if she targeted specific job characteristics. In particular, the successful application has a higher AKM firm fixed effect, it is closer to the job seekers’ municipality of residence, more likely to go through informal channels and to a firm employing a member of the coworker-network. It is also more likely to be a part-time position and surprisingly one that has a higher local competition, on both the respective occupation and the industry (this effect is quantitatively very small however). What stands out further is that it is also more likely to go to a firm in an industry highly related to the individuals prior position, and to a lesser extent also to related occupations. The qualitative conclusions of these results are relatively robust across specifications. Obviously, controlling for other job characteristics decreases most of the coefficients magnitudes and recalls seem to explain a large part on the coworker network and related industry measure.

What determines success?

In the above section, we characterized the extent to which the successful application differs from the pool of applications that individuals send over their unemployment spell. Our findings suggest that the successful application is in fact different in a similar manner across individuals. However, identifying whether the successful application differs from the pool of previous applications might only hold limited information about potential gains of a change in behavior. It is uninformative about the immediate return as having success may require sending many applications of the favorable kind.

To substantiate this point further, we therefore add regressions that aim to identify the relative success rates for different job characteristics. In other words, we ask what the marginal return to targeting a related industry is, all else equal. We report regressions of the successful application indicator on various job characteristics and search channels in Appendix A.2.6.

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59 These estimates are not informative about the marginal return to a change in e.g. targeted job characteristics, we address this question below.
### Table 2.2: Regression results: Job characteristics on successful application dummy

<table>
<thead>
<tr>
<th>Job Characteristic</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
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<td>Typical Wage (log)</td>
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<td>0.003***</td>
<td>0.001***</td>
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<td>0.002***</td>
<td>0.002***</td>
<td>0.002***</td>
<td>0.003***</td>
<td>0.004***</td>
<td>0.002***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.001)</td>
<td>(0.000)</td>
<td>(0.001)</td>
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<tr>
<td>Commute time (min)</td>
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<td>-5.869***</td>
<td>-5.904***</td>
<td>-5.91***</td>
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<td>-3.857***</td>
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<td></td>
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<td>(0.177)</td>
<td>(0.178)</td>
<td>(0.185)</td>
<td>(0.178)</td>
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<td>Related industry (0/1)</td>
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<td>0.110***</td>
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<td>0.092***</td>
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<td>0.029***</td>
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<td>0.021***</td>
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<tr>
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<td>Informal channel (0/1)</td>
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<td>0.100***</td>
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<td>0.106***</td>
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<td>(0.002)</td>
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<td>(0.002)</td>
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<td>Coworker network (0/1)</td>
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<td>0.084***</td>
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Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Spell ID clustered standard errors in parentheses. The table displays the coefficients obtained from regressing specific job characteristics on a success dummy (1 for successful application, 0 otherwise). We regress these characteristics separately on all applications made before the successful application for the sample of individuals on which we can identify this application. Each coefficient thus represents a separate model and shows how the successful application differs from all other application in terms of the specific characteristics of the job (listed on the left side of the table). We show versions controlling for spell-, quarter- and duration fixed effects, as well as the unemployment rate in the prior occupation. In the last two columns we control for detailed characteristics of the job, namely all characteristics in this table excluding the respective left hand side as well as a version where we additionally control for successful applications tied to a recall to the previous employer.

These regressions tell us something about the relative success rate of each application strategy. It indeed turns out that similar patterns to Table 2.2 hold: Applying to industries/occupations related to the prior job or to jobs that are spatially closer to the
individuals residence are more likely to lead to success. Similar patterns hold for applications that go through informal channels, the coworker network, or to part-time positions.

The estimates for example suggest that “sending” an application through informal channels (the coworker network) increases the likelihood of being successful by around 3% (5%). These two application methods seem to be particularly relevant as their share in the average portfolio is relatively small and decreasing with continued exposure to unemployment (see Table A.3 in Appendix A.2.3 for average portfolio shares). For individuals that are unsuccessful in their search for a long time, it might thus be beneficial to switch to informal methods. Given the large share of applications that are already going to related industries and occupations, even at longer realized durations, these might not be as relevant. Similarly, the effect on the likelihood of success when sending applications to jobs with a shorter commute seems to be rather small relative to the average distance in the search portfolio.\textsuperscript{61}

Discussion

The results above suggest that the successful application differs from the prior application behavior on a wide range of dimensions and that changes in search behavior may improve the chances of finding employment. This stark difference in the successful application is perhaps not surprising as job search success also a function of employer preference - for an application to be successful it also requires that the employer makes a job offer. Nevertheless, our results show that, while individuals are sending out a rather stable set of applications during their unemployment spell, some applications have a higher probability of leading to a job. The results suggest that employers prefer individuals with experience in their specific occupation and especially so on the respective industry. The results also hint at a preference for candidates who live nearby, potentially as it increases the prospects of employee retention in the long run. Similarly, personal references of existing employees may make the screening process easier for employers as they can use their networks to secure applications for hard to fill vacancies. This is in line with recent work on the importance of labor market referrals and network contacts (Topa, 2011; Hensvik and Skans, 2016; Glitz and Vejlin, 2019).

The results are also important for a broader point: it’s problematic to derive predictions about general job search behavior and preferences over different job types from observed job outcomes only. Our results indicate that the characteristics of the eventual post-unemployment job outcome are, at least along some dimensions, different from the pool of applications the individual sends during unemployment. This suggests that deriving search behavior indirectly from accepted jobs might be questionable and requires a deeper understanding of the role of employer preferences. The results show that small changes in the jobs that individuals target could affect job finding substantially. On the other hand, large changes in search activity could lead to prolonged unemployment if the “wrong” characteristics are targeted.

In all the above analysis we can not rule out the role of luck. Individuals may for long stretches of their unemployment spell target jobs out of a necessity to apply for jobs given the setting of the Danish UI system.\textsuperscript{62} Within in their spatial search area, there might be a lack of

\textsuperscript{61}While the coefficients on the competition measures are particularly large, their importance vanishes when taking the scale into account. Applying to an industry with a 1% higher within commute-area unemployment rate would, under the linearity of the model, only increase the likelihood of success by less than 0.5%.

\textsuperscript{62}Note that a necessity for active job search was in place even before the introduction of joblog.
positions in the stock and flow of vacancies until the “right” one gets advertised.

2.5 Conclusion

In this paper we examine potential dynamics in job search along several dimensions using a novel data source that covers continuously reported application behavior for the universe of Danish UI recipients. This data allows us to not just look at the extensive margin of applications sent, but also to study the specific job characteristics individuals target and the search channels they use. We examine the evolution of job search over the unemployment spell and potential search responses to interactions with the job center, such as mandatory caseworker meetings and ALMP programs. Further, we also look closer at dynamics that lead up to the particular application directly linked to the eventual post-unemployment job outcome. To examine whether this specific application is different to earlier ones, we compare its characteristics to the pool of unsuccessful applications (within individuals).

Our results show that job search in terms of targeted job characteristics is largely stable as dynamic adjustments, such as changes in target wages or commute time, are particularly small in magnitude. This suggests that individuals do not change the types of jobs they apply to, even under prolonged exposure to unemployment. However, we see some moderate adjustment toward the use of formal channels and the use of online application methods.

Interactions with the public employment services, in the form of caseworker meetings or exposure to active labor market policies, also have a limited impact on the jobs individuals apply for and the search channels they use. We nevertheless observe the presence of threat- and lock-in effects as well as post-program changes in the number of applications that individuals report in their mandatory job search diaries. While we cannot fully rule out reporting effects, our results are suggestive of search effort adjustments in response to the threat of ALMP participation or caseworker meetings, resulting in increased employment exits.

In the last step of our analysis, we find stark differences between the job seekers’ average application portfolio and the specific application that we can link to the post-unemployment job. The results suggest that this particular application is going to a job with shorter commute times and more likely to be facilitated through coworker referrals. It is also more likely to be a job in the individuals’ previous industry. We read these results as driven by employer preferences for candidates who live nearby and have experience in the respective industry. In summary, it may be problematic to infer general job search behavior from the observed job outcomes alone, as these differ significantly along a number of dimensions.

Overall, our results suggest that while individuals have a very stable portfolio of applications over the unemployment spell, and primarily change the number of applications they send, some applications in the portfolio have a higher likelihood of being successful. Thus there may be large gains from changing the portfolio slightly.

Acknowledgements

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2.6 References


A.1 Appendix

A.1.1 Measures of job search and job characteristics

While we observe occupations, wages and industry affiliations of jobs held before and after unemployment in the Danish administrative data, joblog comes with relatively sparse information on the characteristics of the jobs individuals apply to. However, we can make use of the job title as well as the firm’s name and address to construct detailed measures of these characteristics. The job title can link the application to standard occupational codes (DISCO) as the vast majority of the titles contain the name of the respective occupation. We usually consider major (1 digit) as well as sub-major (2 digit) and minor (3 digit) disco grouping with 9, 55 or 153 respective occupations.

We further match the firm’s name and address to firm identifiers in the official Danish firm register. As both firm and occupation information are self-reported in the source data, we apply a rigorous matching procedure to map applications to the official codes and identifiers (details below in A.1.2). In the source data, we can match 84% of the applications to firms and 83% to occupations with an overlap of 69%. We use the obtained firm identifiers and merge them with the IDA firm register at Statistics Denmark. This allows us to get the industry affiliation of each firm that we observe in the applications (an aggregated version of NACE). We consider major (1 digit) as well as sub-major (2 digit) and minor (3 digit) industry groupings with 10, 21 or 38 respective industries.

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 make use of 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 this set as the group of related occupations. 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 industries. 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. We further use the joblog’s occupations and industry affiliations to classify a specific application as targeting a job with a higher, lower or similarly ranked occupation/industry relative to their previous position. To do this, we rank

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63 This information comes from registers such as BFL, DREAM and IDA.
64 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.
65 Information in the aggregation of the NACE nomenclature for industries can be found here: https://www.dst.dk/klassifikationsbilag/8cf95f88-8153-43b5-a82a-fa89adf6f214 (pp. 463-477)
66 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 3-digit DISCO codes using readily available translation keys. 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.
67 The industry skill-relatedness data uses labor flows among industries in the Swedish economy.
1 digit DISCO codes and industry affiliation in terms of their average pay on the Danish labor market and compare the application to the prior job.

We define the individuals coworker network as the group of former coworkers which contains all individuals that were registered as working at the same time at the same firms the respective person worked before (since 2008). We then create an indicator on whether an application goes to a firm where a former member of the network is currently employed. By doing this, we can get a sense of the role of labor market referrals and network contacts which seem to be important (Topa, 2011; Hensvik and Skans, 2016; Glitz and Vejlin, 2019). Using the firm's address reported in Joblog and the individuals' residence municipality, we determine distance measures from Google maps API which we report in terms of commuting times. We get a measure of the within commute-area competition in a certain industry/occupation by dividing the number of unemployed individuals with a prior job in the given 2-digit group by the number of employed persons in the industry/occupation.68

We further use matched employer-employee data to estimate an AKM model (Abowd et al., 1999) and use the implied firm fixed effects as a measure of the respective firm's attractiveness in terms of their overall wage policy. In general, the model captures the firms wage premium from moves of workers from one firm to another while controlling for worker fixed effects. As worker movements are critical for identification, the firm fixed effects can only be estimated for the largest set of connected firms. As in Fluchtmann et al. (2019) we use the administrative BFL data set covering monthly salaries for workers in Denmark and build a matched employer-employee panel from 2008 to 2015 covering 306,900 firms. Of these, 290,108 firms are connected through worker movements (94.5 percent) for which we can estimate the firm fixed effects.

Our data does not contain wages associated with the job as these are not commonly reported for vacancies in Denmark. Similar to Fluchtmann et al. (2019), we thus estimate the typical wage for each application in our data based on detailed 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 on the 1-, 2- and 3- digit level as well as the within-industry-demeaned AKM firm fixed effect measures. Further, we also include pair-wise interactions between all of these measures which results in a total set of 10,568 variables. To condition the set of variables included in the eventual model we employ the Rigorous-LASSO regression of Belloni et al. (2012). This procedure selects 181 variables we then include in a standard log wage regression using OLS.69 For each job applications that has information on both occupations and firms we can then predict the typical wage that is paid in a job with similar characteristics. We further run a Rigorous-LASSO using data only on occupations or industries and AKM fixed effects and add typical wage predictions for joblog entries where occupation or firm information is not present.

A.1.2 Matching algorithm

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

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68 The commute areas are based on definitions from Statistics Denmark and are readily available in the register data.
69 The post model-selection OLS explains about 20 percent of the variance in actual wages. Note that the typical wage does not include individual observable characteristics.
and target files and correct basic spelling mistakes. As a first step in the actual matching, we use the self-reported job titles and link these to the official Danish occupational codes (DISCO). We exploit that many of the self-reported job titles have the actual occupation as a part of the self-reported title. Thus, we identify occurrences of the DISCO occupations in the reported job titles. We only consider as 1:1 matches (43.4%), i.e. if a certain job title links to several occupations we do not treat it as a match. For remaining unmatched entries we manually match some job titles to occupations as many job titles use acronyms that do not match to the standard classification (i.e. ‘social og sundhedshjælper’, Danish for social and health-care workers, are most often reported as ‘sosu-hjælper’). This adds about 27.2% to the matches. Finally, we also use some fuzzy matching techniques on the remaining unmatched observations to circumvent misspellings in the 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%. Overall, we can thus map 81.5% 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% of the applications in Joblog. We further add the sub-string matches which are spatially the closest to the reported firm address (13.9%). To link applications which we cannot match exactly on firm names, we employ a fuzzy matching procedure using the Matchit command in STATA to identify the 50 closest matches. We then test these 50 potential matches using several scoring functions besides the one obtained from Matchit. For each of the scores (5 in total) we calculate the ranking of the 50 potential matches (rank 1 is the best) and identify the “correct” match as the match which receives the best average rank (the scores we use are Bi-gram Similscore, Token, TokenSound from Matchit and the Compget and Speedist functions in SAS). This adds further 6.2% to the matches, so we end up with an overall firm match of 86.4%.

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70 The first four digits of DISCO are identical to the international ISCO classification, details are here [url](https://www.dst.dk/da/Statistik/dokumentation/nomenklaturer/disco-08)
A.2 Tables

A.2.1 Overview of UI fund requirements

Table A.1: Reference logging requirement by UI fund

<table>
<thead>
<tr>
<th>UI Fund</th>
<th>Requirement</th>
<th>Weekly Logs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Akademikernes A-kasse</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>A-kassen Ase</td>
<td>Yes</td>
<td>0.5</td>
</tr>
<tr>
<td>A-kassen Frie</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>A-kassen LH</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>A-kassenfor Journalistik, Kommunikation og Sprog</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>Børne- og Ungdomspædagogernes Landsdækkende Akasse</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>Byggefagenes Akasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>CA A-kasse</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>DANA Akasse for Selvstændige</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Det Faglige Hus - A-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Din Sundhedsfaglige A-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>El-fagets Akasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Faglig Fælles A-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>FOAs A-kasse</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>Fødevareforbundet NNFs Akasse</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>Funktionærernes og Tjenestemændenes Fælles-Akasse</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>HK/Danmarks A-kasse</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>Kristelig A-kasse</td>
<td>Yes</td>
<td>0.25</td>
</tr>
<tr>
<td>Lærernes a-kasse</td>
<td>Yes</td>
<td>0.5</td>
</tr>
<tr>
<td>Magistrenes Akasse</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>Metalarbejdernes Akasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Min a-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Socialpædagogernes Landsdækkende Akasse</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>Teknikernes Akasse</td>
<td>Yes</td>
<td>1</td>
</tr>
</tbody>
</table>

Notes: The Table shows the minimum logging requirement for all Danish UI funds. Note that the minimum requirements are only reference values as individual requirements are set in caseworker meetings. This applies similarly to UI funds without a minimum requirement.
A.2.2 Sample selection

Table A.2: Sample selection

<table>
<thead>
<tr>
<th></th>
<th>Individuals</th>
<th>Spells</th>
<th>Joblogs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Inflow</td>
<td>179,530</td>
<td>261,529</td>
<td>7,516,412</td>
</tr>
<tr>
<td>- Min. 1 logged application</td>
<td>174,746</td>
<td>250,647</td>
<td>7,516,412</td>
</tr>
<tr>
<td>- Full UI entitlement</td>
<td>141,113</td>
<td>184,599</td>
<td>5,647,234</td>
</tr>
<tr>
<td>- Minimum 8 weeks spell length</td>
<td>115,207</td>
<td>142,652</td>
<td>5,358,257</td>
</tr>
<tr>
<td>- Censoring last 4 weeks of applications</td>
<td>114,448</td>
<td>141,551</td>
<td>4,636,217</td>
</tr>
<tr>
<td>Final sample</td>
<td>114,448</td>
<td>141,551</td>
<td>4,636,217</td>
</tr>
</tbody>
</table>

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.

A.2.3 Search portfolio

Table A.3: Summary statistics - Search portfolio

<table>
<thead>
<tr>
<th></th>
<th>Unemployment Duration</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Total</td>
</tr>
<tr>
<td>Typical Wage (log)</td>
<td>5.195</td>
</tr>
<tr>
<td></td>
<td>(0.123)</td>
</tr>
<tr>
<td>AKM firm fixed effect</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.067)</td>
</tr>
<tr>
<td>Commute time (min)</td>
<td>44.50</td>
</tr>
<tr>
<td></td>
<td>(30.14)</td>
</tr>
<tr>
<td>Related occupation (0/1)</td>
<td>0.59</td>
</tr>
<tr>
<td></td>
<td>(0.37 )</td>
</tr>
<tr>
<td>Related industry (0/1)</td>
<td>0.41</td>
</tr>
<tr>
<td></td>
<td>(0.29 )</td>
</tr>
<tr>
<td>Competition industries</td>
<td>0.057</td>
</tr>
<tr>
<td></td>
<td>(0.01 )</td>
</tr>
<tr>
<td>Competition occupations</td>
<td>0.057</td>
</tr>
<tr>
<td></td>
<td>(0.03 )</td>
</tr>
<tr>
<td>Informal channel (0/1)</td>
<td>0.30</td>
</tr>
<tr>
<td></td>
<td>(0.31 )</td>
</tr>
<tr>
<td>Coworker network (0/1)</td>
<td>0.05</td>
</tr>
<tr>
<td></td>
<td>(0.12 )</td>
</tr>
<tr>
<td>Part-time</td>
<td>0.11</td>
</tr>
<tr>
<td></td>
<td>(0.19 )</td>
</tr>
</tbody>
</table>

Notes: The table displays the average characteristics of the search portfolio over the first 52 weeks in unemployment. Standard deviation in parentheses.
## A.2.4 Success portfolios

**Table A.4: Summary statistics - Success portfolio**

<table>
<thead>
<tr>
<th></th>
<th>(1) Success</th>
<th>(2) Success</th>
<th>(3) Success</th>
<th>(4) Success</th>
</tr>
</thead>
<tbody>
<tr>
<td>Typical Wage (log)</td>
<td>0.0613***</td>
<td>0.0471***</td>
<td>0.0445***</td>
<td>0.0416***</td>
</tr>
<tr>
<td></td>
<td>(0.0024)</td>
<td>(0.0024)</td>
<td>(0.0025)</td>
<td>(0.0024)</td>
</tr>
<tr>
<td>AKM firm fixed effect</td>
<td>0.0275</td>
<td>0.0837***</td>
<td>0.105***</td>
<td>0.105***</td>
</tr>
<tr>
<td></td>
<td>(0.0229)</td>
<td>(0.0229)</td>
<td>(0.0235)</td>
<td>(0.0235)</td>
</tr>
<tr>
<td>Commute time (min)</td>
<td>-0.0006***</td>
<td>-0.0004***</td>
<td>-0.0004***</td>
<td>-0.0004***</td>
</tr>
<tr>
<td></td>
<td>(0.0001)</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
</tr>
<tr>
<td>Related occupation (0/1)</td>
<td>0.0487***</td>
<td>0.0513***</td>
<td>0.0038</td>
<td>0.0067</td>
</tr>
<tr>
<td></td>
<td>(0.0041)</td>
<td>(0.0041)</td>
<td>(0.005)</td>
<td>(0.0042)</td>
</tr>
<tr>
<td>Related industry (0/1)</td>
<td>0.0549***</td>
<td>0.0716***</td>
<td>0.0687***</td>
<td>0.0611***</td>
</tr>
<tr>
<td></td>
<td>(0.0054)</td>
<td>(0.0054)</td>
<td>(0.0057)</td>
<td>(0.0053)</td>
</tr>
<tr>
<td>Competition industries</td>
<td>1.660***</td>
<td>1.740***</td>
<td>2.114***</td>
<td>1.893***</td>
</tr>
<tr>
<td></td>
<td>(0.151)</td>
<td>(0.149)</td>
<td>(0.15)</td>
<td>(0.146)</td>
</tr>
<tr>
<td>Competition occupations</td>
<td>-2.064***</td>
<td>-1.401***</td>
<td>-1.031***</td>
<td>-0.0007***</td>
</tr>
<tr>
<td></td>
<td>(0.0698)</td>
<td>(0.0724)</td>
<td>(0.0745)</td>
<td>(0.0733)</td>
</tr>
<tr>
<td>Informal channel (0/1)</td>
<td>-0.0539***</td>
<td>-0.0202***</td>
<td>0.0022</td>
<td>0.0129**</td>
</tr>
<tr>
<td></td>
<td>(0.0055)</td>
<td>(0.0055)</td>
<td>(0.0055)</td>
<td>(0.0054)</td>
</tr>
<tr>
<td>Part-time (0/1)</td>
<td>0.244***</td>
<td>0.193***</td>
<td>0.135***</td>
<td>0.0934***</td>
</tr>
<tr>
<td></td>
<td>(0.0092)</td>
<td>(0.0094)</td>
<td>(0.0096)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Coworker network (0/1)</td>
<td>0.364***</td>
<td>0.337***</td>
<td>0.298***</td>
<td>0.222***</td>
</tr>
<tr>
<td></td>
<td>(0.0133)</td>
<td>(0.0132)</td>
<td>(0.0132)</td>
<td>(0.0122)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.12***</td>
<td>0.0299</td>
<td>0.0517</td>
<td>-0.484***</td>
</tr>
<tr>
<td></td>
<td>(0.0122)</td>
<td>(0.0211)</td>
<td>(0.0425)</td>
<td>(0.0872)</td>
</tr>
</tbody>
</table>

Observations                | 96.733      | 96.733      | 96.733      | 96.733      |
R-squared                   | 0.043       | 0.058       | 0.072       | 0.135       |
Demographics                | No          | Yes         | Yes         | Yes         |
Prior Job Characteristics   | No          | No          | Yes         | No          |
Future Job Characteristics  | No          | No          | No          | Yes         |

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The table displays the regression outcomes for a linear regression of a dummy indicating that we can identify the successful application for an individual on the individual search portfolio over the whole spell. We show versions controlling for demographics (age, gender, education, city, married) as well as characteristics of the prior or future job. Standard errors are clustered at the spell level.
### A.2.5 Post-activation hazards

#### Table A.5: Employment effects of skill development

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Time dummies</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>After (4 &lt; t \leq 13)</td>
<td>0.0149***</td>
<td>0.0061***</td>
<td>0.0137***</td>
<td>0.0048***</td>
<td>0.0089***</td>
<td>0.0011*</td>
</tr>
<tr>
<td></td>
<td>(0.0004)</td>
<td>(0.0007)</td>
<td>(0.0004)</td>
<td>(0.0007)</td>
<td>(0.0004)</td>
<td>(0.0007)</td>
</tr>
<tr>
<td>During (0 \leq t &lt; 4)</td>
<td>-0.0103***</td>
<td>-0.0103***</td>
<td>-0.0170***</td>
<td>-0.0170***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0004)</td>
<td>(0.0004)</td>
<td>(0.0005)</td>
<td>(0.0005)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Before ((-4 \leq t &lt; 0))</td>
<td>-0.0346***</td>
<td>-0.0344***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0003)</td>
<td>(0.0003)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Joblogs</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td># Applications</td>
<td>0.0015***</td>
<td>0.0015***</td>
<td>0.0013***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>((-4 \leq t &lt; 0))</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
<td>(0.0001)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.0279***</td>
<td>0.0280***</td>
<td>0.0284***</td>
<td>0.0284***</td>
<td>0.0304***</td>
<td>0.0304***</td>
</tr>
<tr>
<td></td>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
<td>(0.0000)</td>
</tr>
<tr>
<td>Observations</td>
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<td>3,666,415</td>
<td>3,666,415</td>
<td>3,666,415</td>
<td>3,666,415</td>
<td>3,666,415</td>
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</table>

Notes: *** \(p < 0.01\), ** \(p < 0.05\), * \(p < 0.1\). The table displays the regression outcomes for a linear regression of employment exits on time dummies around entering a skill development program at \(t = 0\). In all of the regressions we restrict on applications between weeks 6 and 52. In (2), (4) and (6) we additionally control for the number of applications placed immediately before entering the program (4 weeks window). Standard errors are clustered at the spell level.
### Table A.6: Regression results: Success rates

<table>
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<th>Job Characteristics</th>
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<td>No</td>
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Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Spell ID clustered standard errors in parentheses. The table displays the coefficients obtained from regressing a success dummy (1 for successful application, 0 otherwise) on specific job characteristics of the applications made before the respective successful application. The table thus shows us the relative success rate for specific types of applications (or the marginal effects for continuous measures). We show versions controlling for spell fixed effects in all columns. In the last column we regress on all job characteristics jointly.
Table A.7: Regression results: Success rates (recall fixed effects)

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<td>AKM firm fixed effect</td>
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<td>Commute time (min)</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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</table>

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Spell ID clustered standard errors in parentheses. The table displays the coefficients obtained from regressing a success dummy (1 for successful application, 0 otherwise) on specific job characteristics of the applications made before the respective successful application. The table thus shows us the relative success rate for specific types of applications (or the marginal effects for continuous measures). We show versions controlling for spell fixed effects and recalls in all columns. In the last column we regress on all job characteristics jointly.
A.3 Figures

A.3.1 Distributions

Figure A.1: Distributions

Note: This Figure shows the distribution of start weeks/months for activation measures and the week/month of caseworker meetings as well as distributions of specific job characteristics. Wages, commuting distances and AKM firm fixed effects are trimmed below the 1st and the 99th percentile.
A.3.2 Job search during the unemployment spell

Job characteristics by worker group

**Figure A.2:** By gender

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on unemployment spell month dummies for men and women, controlling for spell fixed effects. Typical wages are average wages for a certain firm/occupation combination. Firm fixed effects are obtained from an AKM model. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Part-time applications are those directed to jobs with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.3: By age groups

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on unemployment spell month dummies for different age groups at spell start, controlling for spell fixed effects. Typical wages are average wages for a certain firm/occupation combination. Firm fixed effects are obtained from an AKM model. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Part-time applications are those directed to jobs with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Robustness

Figure A.4: Robustness: Spell dynamics

Note: This Figure shows the coefficients from regressing specific the number of logged applications and job characteristics on unemployment spell month dummies for different age groups at spell start, adding additional fixed effects and trimming below the 10th and above the 90th percentile. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals' municipality of residence and the job's postal code. Firm fixed effects are obtained from an AKM model using matched-employer-employee data. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Note: This Figure shows the coefficients from regressing specific the number of logged applications and job characteristics on unemployment spell month dummies for different age groups at spell start, adding additional fixed effects and trimming below the 10th and above the 90th percentile. Traditional applications are sent via letter/e-mail, whereas personal applications through a phone call or personally showing up. Online submissions contain applications sent through online application forms or social media. Network applications are those sent to firms that currently employ a former coworker. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.6: Robustness: Spell dynamics - Continued

Note: This Figure shows the coefficients from regressing specific the number of logged applications and job characteristics on unemployment spell month dummies for different age groups at spell start, adding additional fixed effects and trimming below the 10th and above the 90th percentile. Relatedness measures are based on the top 10 most related occupations/industries in the O*NET career mover matrix or Neffke and Henning (2013). Competition measures are based on the within commute-area unemployment rate in the occupation/industry applied for. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.7: Robustness: Spell dynamics - Extended window

Note: This Figure shows selected spell dynamics on an extended time window. Typical wages are average wages for a certain firm/occupation combination. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
A.3.3 Dynamics around interactions with the job center

Activation Programmes - General

**Figure A.8: ALMP dynamics - All programs**

Note: This Figure shows the coefficients from regressing the specific number of logged applications on weekly dummies relative to starting a specific activation measure in week 0. Base level (red square) is three months prior to activation. The regressions control for spell fixed effects. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.

**Figure A.9: Activation - All programs, Poisson regression**

Note: This Figure shows the poisson regression coefficients from regressing the specific number of logged applications on weekly dummies relative to starting a specific activation measure in week 0. Base level (red square) is three months prior to activation. The regressions control for spell fixed effects. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Activation Programmes - Skill development & ordinary education

Figure A.10: Activation - Skill development

Note: This Figure shows the coefficients from regressing specific job characteristics on weekly dummies relative to starting a skill development measures in week 0. Base level (red square) is three months prior to activation. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job's postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are those with less than 37 hours per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
**Figure A.11**: Activation - Ordinary education

Note: This Figure shows the coefficients from regressing specific job characteristics on weekly dummies relative to starting an ordinary education measure in week 0. Base level (red square) is three months prior to activation. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are those with less than 37 hours per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.12: Activation - Poisson regression, skill development (left) and ordinary education (right)

![Graph 1](image1)

Note: This Figure shows the poisson regression coefficients from regressing the number of logged applications on weekly dummies relative to starting an activation measure in week 0. Base level (red square) is three months prior to activation. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.

Figure A.13: Activation - Meeting controls, skill development (left) and ordinary education (right)

![Graph 2](image2)

Note: This Figure shows the coefficients from regressing the number of logged applications on weekly dummies relative to starting an ordinary education measure in week 0, adding controls for time until next meeting and time since last meeting in weeks (interval [0,4]). Base level (red square) is three months prior to activation. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Caseworker meetings

**Figure A.14:** Casework meetings

Note: This Figure shows the coefficients from regressing specific job characteristics on weekly dummies relative to a caseworker meeting in week 0. Base level (red square) is three months prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are those with less than 37 hours per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.15: Caseworker meetings - High effort types

Note: This Figure shows the coefficients from regressing the number of logged applications on weekly dummies relative to a caseworker meeting in week 0. We restrict the sample to individuals above the 75th percentile in terms of average applications in the month prior to the base level. Base level (red square) is three months prior to the meeting. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.

Figure A.16: Caseworker meetings - Poisson regression

Note: This Figure shows the poisson regression coefficients from regressing the number of logged applications on weekly dummies relative to a caseworker meeting in week 0. Base level (red square) is three months prior to the meeting. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Robustness

Figure A.17: Robustness: Activation - Skill development

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to a starting a skill development measure in week 0, adding various fixed effects and other controls. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.18: Robustness: First activation - Skill development

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to a starting a the first activation measure (skill development) in week 0. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
**Figure A.19:** Robustness: Last activation - Skill development

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to a starting a the last activation measure (skill development) in week 0. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.20: Robustness: Activation - Ordinary education

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to a starting an ordinary education measure in week 0, adding various fixed effects and other controls. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.21: Robustness: First activation - Ordinary education

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to a starting a the first activation measure (ordinary education) in week 0. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job's postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.22: Robustness: Last activation - Ordinary education

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to a starting a the last activation measure (ordinary education) in week 0. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals' municipality of residence and the job's postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.23: Robustness: Caseworker meetings

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative a caseworker meeting in week 0, adding various fixed effects and other controls. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.24: Robustness: Caseworker meetings - First meeting

Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to the first caseworker meeting in week 0. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Note: This Figure shows the coefficients from regressing the number of logged applications and specific job characteristics on weekly dummies relative to the last caseworker meeting in week 0. Base-level (red square) is three month prior to the meeting. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Part-time positions are classified as positions with less than 37 hours of work per week. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
A.3. Figures

A.3.4 Job finding

Figure A.26: Successful applications

Note: This Figure shows the coefficients from regressing the specific number of logged applications and informal channel on week relative to successful application is sent dummies. Informal applications contain unsolicited applications, being headhunted or using the network. Coworker-network applications are those sent to firms that currently employ a former coworker. The related industry measure is based on the top 10 most related industries in Neffke and Henning (2013). Commute areas are based on definitions from Statistics Denmark. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Figure A.27: Pre-success dynamics and application bunching

(a) Typical wages and commuting distance

(b) Related industry and network applications

(c) Application bunching around success

Note: This Figure (a+b) shows the coefficients from regressing the specific number of logged applications and job characteristics on week until successful application is sent dummies. Typical wages are average wages for a certain firm/occupation combination. Commuting distances are calculated with Google Maps API using the individuals’ municipality of residence and the job’s postal code. Informal applications contain unsolicited applications, being headhunted or using the network. Coworker-network applications are those sent to firms that currently employ a former coworker. The successful application is sent in week 0. Sub-Figure (c) shows the distribution of applications going to the same occupation/firm combination around the week of the successful application. Standard errors are clustered on the spell level and vertical bars display 95% confidence bands.
Gender Gaps in Job Applications and Hiring Outcomes

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Abstract

A large literature has documented gender differences in the types of jobs men and women tend to hold. Do these gender differences exist also in which jobs men and women apply to? To answer this question, we use a novel administrative data set containing actual job applications made by the universe of Danish UI recipients, which we link to additional administrative data on hiring outcomes and the jobs applied for. We use these data to document large gender differences in the jobs men and women apply to, both in terms of occupation and industry as well as the firm type. These gaps persist even after conditioning on a rich set of characteristics and closely mirror the observed gaps in actual hiring outcomes. In particular, we show that women systematically apply to lower-paying jobs. We then apply a standard decomposition method to examine whether differences in application behavior can help explain the gender gap in wages. After conditioning on individual observables, we find that differences in application behavior can explain virtually all of the wage gap related to job characteristics and about 60 percent of the overall wage gap. Finally, we provide suggestive evidence that non-wage job characteristics and gender differences in overconfidence may explain why men and women apply to very different types of jobs.

Keywords: gender gap, wage dispersion, job search, unemployment

JEL Codes: E24, J29, J31, J71
3.1 Introduction

In most labor markets, there are large and persistent differences in the types of jobs men and women tend to hold. This is true both in terms of job characteristics, such as industries or occupations, as well as in terms of the typical wage level of the job. In most cases, women tend to hold jobs that pay systematically less than those men more often work in.\footnote{For a broad overview of the recent literature on gender differences in earnings and wages see Blau and Kahn (2017) or Olivetti and Petrongolo (2016). For Danish evidence see Gallen et al. (2019).}

In this paper, we examine the extent to which these observed gender differences in job outcomes are mirrored in the types of jobs men and women tend to apply for. Conceptually, we can imagine two extreme benchmarks here. Under one extreme benchmark, gender differences in applications exactly mirror gender differences in job outcomes. Under the other benchmark, there are no differences in the jobs men and women apply for and the observed gender gaps in job outcomes are shaped only by gender differences in which job applications are successful.

As we return to further below, we know relatively little about which of these benchmarks are closer to the truth. Identifying the extent to which men and women are applying to very different jobs is important, both for understanding the process that shapes gender gaps in the labor market and the effect of policies that try to close the gender gap by influencing women’s job search and career plans. Examples of such policies include family-friendliness policies that alleviate constraints on women’s time or informational campaigns that aim to influence women’s career plans.

In this paper, we provide new evidence on how gender gaps in job applications compare to gender gaps in actual hiring outcomes. We do this by contrasting job applications and hiring outcomes for unemployed men and women in Denmark. Utilizing a novel administrative data source containing data on actual job applications made by the universe of Danish UI recipients, we link individuals and job applications to administrative data on the universe of workers and firms in Denmark. The resulting data set allows us to examine the characteristics of the jobs that all male and female UI recipients apply to and compare them to the characteristics of the jobs they eventually end up in.

We start our analysis by providing descriptive results on differences in the types of jobs men and women apply to. The main job characteristics we focus on are the industry and the occupation of the job and whether the job is at a high- or low-wage firms, as measured by the firm’s AKM fixed effect (a two way fixed effect model; see Abowd et al., 1999). In addition, we use administrative data on actual wage payments to compute a predicted wage for each type of job given its characteristics. We refer to this as the typical wage for this type of job and use it to shed light on the relationship between job application behavior and the gender wage gap. Based on these data, we compute the share of job applications going to different types of jobs for each male and female UI recipient and compare these shares across gender.

Across all job characteristics, we see large gender differences in application behavior, which also remain after conditioning on a very rich set of individual labor market observables. In most cases, women tend to send more job applications to jobs that typically pay lower wages. Looking at firms’ wage levels for example, after conditioning on observables, women send 4 percentage points more of their application to firms in the bottom 30 percent, and 6 percentage points less of their applications to firms in the top 30 percent. These gender differences in job characteristics add up to a stark gender gap in the typical wages of the jobs
men and women apply for: After conditioning on observables, women send 13 percentage points more of their applications to jobs that are in the bottom third in terms of typical pay and send 11 percentage points less of their application to jobs in the top third.

Next, we quantify how much of the observed gender gaps in job outcomes may be explained by differences in application behavior. Because our data contains information on job applications and job outcomes for the same individuals, we can apply the standard semi-parametric wage decomposition approach of DiNardo et al. (1996), hereafter DFL. Based on the DFL method and its standard assumptions, we use a reweighting estimator to compute the counterfactual job outcomes that would have occurred if women applied to the same types of jobs as men. We then compare this to the observed job outcomes to decompose what part of the observed gender gaps in outcomes can be explained by gender differences in job applications. Under the ignorability assumption underlying the DFL method, job applications are capable of explaining a very substantial share of observed gender gaps in job outcomes. After condition out individual observables, job applications are able to explain about 30 percent of the remaining gender segregation across industry and occupation and 86 percent of the gap in typical wages. For the overall gender wage gap, we find that gender differences in job applications can explain about 60 percent. These results indicate that gender differences in the jobs men and women apply for play an important role in shaping gender gaps on the labor market. For the purpose of combating gender gaps via policy, the results suggest an important role for policy initiatives that aim to influence women’s job search and career plans.

Finally, in the last part of our analysis, we provide suggestive evidence regarding some potential explanations for why women apply to different types of jobs than men. We find little support for the idea that women are shying away from jobs in which they have lower probabilities of getting hired than men. In particular, gender gaps in where men and women apply are only weakly correlated with gender differences in the probability of getting hired conditional on applying. Consistent with the idea that systematic (over)confidence plays a role for men’s job search behavior, however, we show that relative to men, women are much less likely to apply to jobs which would represent a step up the occupational ladder relative to their previous job. In line with recent evidence that women may be more geographically constrained in their job search (Le Barbanchon et al., 2019a), we also provide evidence that women are systematically applying to jobs that involve a significantly shorter commute. Finally, consistent with recent evidence that gender gaps in the labor market are particularly tied to motherhood (e.g. Kleven et al., forthcoming; Hotz et al., 2018; Lundborg et al., 2017), we see that women are more likely to apply to jobs at family friendly firms, proxied by the average length of maternity leave per birth at the firm. We also repeat our main analysis separately for two samples of young workers respectively with and without children. Gender differences in application behavior and hiring outcomes are present among both samples, yet these differences are more pronounced among men and women with children, suggesting that having children may play a particular important role for the overall gender differences in application behavior that we observe.

Our paper is closely related to a recent literature that explores the role of labor market frictions in explaining the gender pay gap. This work suggests that there are gender differences in sorting into different types of jobs and pay as well as gender differences in the impact of search frictions in the market. We extend this work by considering the role of job search and

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2 See also Fortin et al. (2011) for a more recent treatment.
3 Barth et al. (2016) show establishment differences in pay are very important in driving aggregate
the outcomes of the search process in shaping the gender wage gap. Our work is further related to the literature exploring sexual discrimination in hiring. While Goldin and Rouse (2000) have found early evidence for discrimination against female applicants, more recent audit studies show that sex-biased hiring is not exclusively affecting women as discrimination against men seems to be an issue in female-dominated occupations while women face these barriers in male-dominated fields.4

Our paper should also be seen as complementary to some of the recent literature exploring job search behavior with detailed data on individual applications or search criteria, such as surveys (e.g. Krueger et al., 2011; Pager and Pedulla, 2015), data stemming from specific job boards (e.g. Faberman and Kudlyak, 2019; Banfi et al., 2019; Skandalis and Marinescu, 2019), artificial job search platforms in a lab (Belot et al., 2018a,b) or administrative sources (Le Barbanchon et al., 2019a,b). Our work adds to this by being able to look beyond pure job search with additional information on job outcomes. Most notably, we do not only observe the universe of UI recipients and a substantial degree of their search, but also the subsequent employment outcomes by linking applications and employer-employee register data. This novel feature of our data allows us to go beyond an exclusive focus on either job search or employment outcomes, specifically, we are able to study the trajectory from job search to employment and relate this to gender gaps in job applications and hiring outcomes.5

The rest of the paper is structured as follows: In the next section we briefly present the data, including the institutional framework around joblog as well as validity checks relating to its relevance as a measure of individuals search behavior. We then proceed by presenting a number of “stylized” facts about the gender wage gap out of unemployment. In the following we present our results along multiple dimensions of job characteristics as well as a series of decompositions of the gender wage gap which shed light on the importance of the different mechanisms outlined above. As a least exercise we provide suggestive evidence for potential mechanism that lead to differential search behavior between men and women. Finally, we conclude.

3.2 Data and institutional setting

The crux of the present paper is the use of administrative data on actual job applications made by Danish UI recipients between 2015 and 2017, which we link to a range of other wage inequality. Barth and Dale-Olsen (1999) find evidence that female labor supply is less elastic than male labor supply. Wiswall and Zafar (2017) show that women have a higher average willingness to pay for more flexible and secure jobs which explains a quarter of the early career gender wage gap. Card et al. (2016) show that 5 percent of the overall gap is driven by gender differences in bargaining.

4Some audit studies find no evidence of employers displaying discriminatory behavior (e.g. Albert et al., 2011; Bygren et al., 2017) while others find that men face discrimination in more female-dominated occupations (e.g. Booth and Leigh, 2010; Carlsson and Eriksson, 2017). Likewise, women seem to be discriminated against in male-dominated occupations (e.g. Riach and Rich, 2006). Within occupations, women appear to have systematically lower call back and job offer rates at workplaces that are associated with high earnings (Neumark et al., 1996). Hence, discriminatory behavior of employers seems to be occupation dependent.

5In this literature, only Le Barbanchon et al. (2019a) focus primarily on gender differences in job seekers’ search criteria and employment outcomes. Banfi et al. (2019) provide some descriptive evidence of gender differences on the selective margin of job search, yet do not consider eventual job outcomes. Kuhn and Shen (2012) do not have data on application but provide evidence of gender discrimination in Chinese job ads.
administrative data sets on workers, firms and employment relationships. The next sections describe the institutional details of the Danish UI system and the analysis data set we construct by combining the data on job applications (the so-called Joblog data) with other administrative data sources.

### 3.2.1 The Danish UI system and the Joblog application data

The focus of the present paper is on job applications and hiring outcomes for Unemployment Insurance (UI) recipients in Denmark over the period from 2015 to 2017. UI eligibility in Denmark is dependent on having signed up and contributed to one of Denmark’s 24 UI fund sufficiently well in advance of becoming unemployed (typically a year).\(^6\) The vast majority of Danish workers satisfy eligibility requirements. In 2015, 76 percent of Danish employees were member of a UI fund.

During the period we study, UI was available for up to two years at a replacement rate of 90 percent of previous income and a cap of 18,500 DKK (2,500 Euro in 2017).\(^7\) To maintain UI eligibility when unemployed, the UI law stipulates that workers have to be available to work and actively search for jobs. Following a large labor market reform in 2015, strict requirements were imposed about how workers were to document that they were actively searching. As part of this, workers are required to register specific jobs that they have applied to in a system called Joblog. It is the data on these registered job applications that serves as our main data source in this paper.

The Joblog system works as follows: To register an application in the system, unemployed workers need to login on the central online platform of the Danish public employment services (Jobnet) which individuals also use to claim their UI benefits. This platform serves as the main means for communication between UI recipients and public authorities and also functions as a job-board, where job seekers can find most posted vacancies in Denmark.\(^8\) After logging in, unemployed workers fill out a joblog form regarding their job application with information on the job, including the job-title, hours (part-/fulltime), search channels/methods and application status, as well as on the potential employer, including firm name and address. Individuals can further upload the CV and cover letter as well as key tasks in relation to the application. Of

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\(^6\)The different UI funds typically cater to different occupational groups and are formally private organizations. Their UI payments are heavily subsidized and regulated by the government however. Broad eligibility requirements, replacement rates, caps on payouts and eligibility periods are thus all completely determined by public policy. As we return to below, the UI funds do have some leeway in their administration of the UI system. The general eligibility rules require an unemployed worker to be a member of a UI fund at least 12 months before claiming benefits as well as having had a minimum of 1,924 working hours within the previous three years. From 2017 onward the rules on minimum working hours were converted to a minimum income level of 223,000 kr. (30,000 Euro in 2017). Newly graduates are eligible for a reduced benefit level (70-80 percent) either immediate after graduation or one month later depending on whether the graduate have been a free UI member 12 months prior of graduation or become a member no later than 2 weeks post of graduation.

\(^7\)The cap is binding for the majority of workers. Workers whose UI eligibility expires or workers without UI eligibility can instead be eligible for a lower level of means tested public social assistance while unemployed.

\(^8\)The portal lists vacancies that are directly posted by firms through Jobnet, as well as scrapped vacancies from other job portal banks. The coverage rate is around 85 percent of all online posted jobs in Denmark.
these, submitting information on the job and employer is mandatory in order to submit the
joblog form.\footnote{when reporting information on the employer, the address is not necessarily mandatory, as individuals
can state they know neither the postal code or the city of the respective position.}

While the 2015 reform introduced the requirement for UI recipients to log job applications,
administrating the details of the requirement was left to the individual UI funds. During the
first weeks of unemployment, the UI funds are legally required to instruct the unemployed
in the use of joblog.\footnote{\$12 pcs. 2 in the law of UI-fund' s obligations to supervise etc. ("Bekendtgørelse om en a-kassespligt
til vejlede mv.").} The law further requires them to specify a minimum amount of weekly or
monthly applications that an individual needs to register.\footnote{\$ 36, pcs. 1 in the law about availability ("Bekendtgørelse om rådighed"). This requirement is 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 no universal threshold
of logging requirements, UI funds often state a rough guideline of their general expectations
which vary across funds, but are mostly between 1.5 and 2 applications a week (see Appendix
A.1.8). Over the unemployment spell, UI funds monitor their clients search activity and if
it does not adequately fulfill the requirements, they can issue sanctions after giving a short
deadline.\footnote{To incentivize UI funds to comply with the rules and impose sanctions when applicable, the
national Labor Market authorities also incentivize UI funds and municipalities to guarantee sufficient
usage among their clients. Their allocation of financial resources from the public depends on
active joblog usage among all their UI recipients (in January 2019 this rate was 90 percent). If for
instance the National Labor Market Authorities judge that a UI fund has not administered according
to the law (i.e. assessing eligibility and screening job logs) and thus paid out "illegal" UI benefits
the UI fund will loose the reimbursements of UI and hence pay these expenses by themselves.
See also https://star.dk/it/borger-it/tjek-jobforslag-find-job-og-joblog/min-joblog-borgerens-v
aerktoej-til-en-struktureret-jobsoegning/status-for-brug-af-joblog-for-dagpengemodtagere/}

UI recipients thus face a clear economic incentive to comply with the requirements
and register submitted job applications in Joblog. As we return to when discussing the validity
and coverage of the Joblog data in section 3.2.3, these incentives have resulted in a very high
level of usage.

### 3.2.2 Data construction

Our data construction starts from data on all submitted job applications that UI recipients have
registered in Joblog between September 2015 and September 2017.\footnote{Besides documenting search activity to qualify for UI, the joblog section of Jobnet website was developed also with the goal of helping job seekers keep track of their job search. As a result, non-Ul recipients can also log in and register applications to the Joblog system. In addition to submitting information on jobs that the worker has applied for, workers can also use the joblog form to register and keep track of vacancies that the worker is considering to apply for in the future, and to register other job search events such as being called for an interview or being rejected. Since the 2015 reform only requires UI recipients to register information on submitted applications to maintain eligibility, however, very few non UI recipients use the Joblog system and very few UI recipients register information on things other than submitted applications. We therefore limit ourselves to look at submitted applications for UI recipients.} For each UI recipient, we
observe all the listed information their submitted job applications along with the timing of
their application.\footnote{The joblog database maintains information on job title, working hours, name and address of the
applied firm as well as search method/channel. We base our measure of timing on the time where Joblog
was fully integrated and the end date is the last month with available labor market data. We pre-process}
For each UI recipient, the Joblog data contains individual identifiers that uniquely allow us to merge the data to the universe of UI recipients with information from a wide range of administrative data sets, including 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.

For each submitted job application in the Joblog data, we perform a string matching on the job title to determine the occupation of the job according the Danish version of the ISCO classification (DISCO). We also perform string matching on the firm name listed in the application, as well as its address, to match job applications to firms in the central Danish firm register.\(^\text{15}\) This allows us to determine the identity of the firm the worker has applied to and link to administrative data on the industry, location and the wage-level at the firm. In the source data, we match 84 percent of the applications to a firm and 83 percent to occupations with an overlap of 69 percent. Additional details of the matching process are provided in Fluchtmann and Maibom (2019).

The final baseline data comprises linked administrative data on the universe of Danish UI recipients, including firm-linked administrative data on their full employment history and all their submitted applications registered in Joblog. For the submitted applications, we have access to the occupation of the applied for job as well as administrative data on the firm.

### 3.2.3 Coverage of the Joblog application data

One of the key advantages of the Joblog application data is that, by construction, it covers the universe of UI recipients. Since logging applications in the data is done entirely by UI recipients themselves, however, the coverage and validity of the applications data warrants some additional discussion.

As noted previously, an important feature of the data is that UI recipients face a clear economic incentive to register job applications in Joblog to maintain their UI eligibility. This requirement has resulted in a very high degree of usage. Across all UI spells lasting at least 8 weeks, 98 percent have logged an application in Joblog.\(^\text{16}\)

In terms of the number of applications that individuals log, we noted previously that many UI funds use 1.5 to 2 applications per week as their general requirement for maintaining UI eligibility. This tendency is borne out strongly in the data. As we show in Figure A.4 in Appendix A.1.9, the distribution of logged applications centers strongly between these requirements, with the mean number of applications per week being 1.53 (or 1.78 when ignoring the initial ‘phasing-in’). Most UI recipients seem to log exactly enough applications to satisfy the requirements of the UI fund. While this high degree of compliance is encouraging, it raises a concern\(^\text{15}\) about the data source to identify applications that were actually sent by only selecting the latest edit to the joblog entry and excluding those which have the status of “not yet applied”.

\(^{15}\)The Danish central firm register (CVR-register in short) contains information on companies officially registered in Denmark. The register covers all firms, with the exception of privately held companies with an annual turnover below 50,000 DKK (about 7,500 USD). Each firm is registered with a uniquely identifiable CVR number that linkable to Danish administrative data sets.

\(^{16}\)Across all UI spells 96 percent have logged an application. This lower fraction reflects that logging is low at the beginning of each UI spell, likely reflecting that UI recipients are gradually learning about the use of Joblog.
that the Joblog data may miss many applications that UI recipients make beyond the required 1.5 to 2 per week. In Appendix A.1.9 we have assessed the extent of this lack of coverage by comparing Joblog data with survey data on job applications among Danish UI recipients. The comparison suggests that the Joblog data captures on average 70 percent of applications, with the majority of individuals only rarely applying for more jobs than reported in joblog.

Since the focus in the present paper is on where individuals send their applications rather than the total number of applications sent, the lack of full coverage is less problematic, as long as the subset of applications that are being logged is representative. Importantly, we note that UI recipients face no formal incentives to selectively log some applications over others. This limits concerns about whether individuals are logging only a particular subset of their actual applications. Perhaps a bigger concern is that to satisfy requirements, UI recipients may log fictitious job applications that they have in fact never sent. We have no way of ruling out that such behavior occurs, however, we again note that UI recipients can face economic sanctions if they are caught submitting fictitious applications in Joblog.

Overall, we expect the Joblog data to have a high coverage and representativeness. Fluchtmann and Maibom (2019), working with the same data, provide a range of analyses that confirms this view. For example, individuals exhibit pronounced threat- and lock-in effects in logging behavior around active labor market programmes and contacts with the public employment services.

3.2.4 Selecting the analysis sample

For our baseline sample, we restrict our attention to individuals of Danish nationality entering new UI spells from September 2015 to September 2017. The start date is the time where Joblog requirements was fully integrated and the end date is the last month with available labor market data. Here we observe 227,515 individuals, covering 261,529 spells with a total of 7,516,412 job applications in Joblog.

In constructing our analysis sample, we make four further sample restrictions (we display the effect of these individual selection steps on the sample in Table 3.1). For all of these steps we provide robustness checks in Appendix A.1.13.

First, from the sample of new UI spells, we only consider the ones lasting at least 8 weeks. During the early weeks of a new unemployment spell the unemployed are usually subject to a 'phasing-in' period in which they slowly get introduced to joblog and other components of the UI system. Our results remain qualitatively unchanged if we instead consider all identified unemployment spells in our sample, regardless of their length.

Second, we restrict our sample to individuals who log at least 4 applications in joblog during their respective unemployment spell. The restriction guarantees that we observe actual job search behavior in our data, which is crucial for our analysis in which we are interested in how search behavior links to employment outcomes. We confirm the robustness
of this by considering spells with a minimum requirement of only one joblog during the unemployment spell.

Third, we restrict our sample on individuals who leave UI for employment within the first year of their unemployment spell. We do this for two reasons: The first is that our focus in this paper is on the intensive margin: We are interested in the types of job men and women apply for and how these applications shape where they are hired, rather than whether or not their job search results in a hire. Second, much of the existing literature that we relate to focuses on gender gaps in earnings or wages conditional on being employed, which makes the focus on employed individuals natural. In Appendix A.1.13 we show alternative results that includes the full sample of UI recipients and treats not finding a job as a separate outcome. All of our main conclusions remain unchanged.\(^{20}\)

Fourth, for each individual, we drop all applications made in the last four weeks before entering employment. As shown in Fluchtmann and Maibom (2019), there is a sharp drop in the number of applications that people log in Joblog about one month before they enter employment, reflecting that individuals have already accepted their new job at this point and are just waiting for it to start. We therefore drop applications from the last four weeks before the new jobs start as applications made while waiting for the new job to start may not represent an individual’s general application behavior. None of our results are sensitive to this restriction.

Our final analysis sample consists of 106,266 individuals, covering 114,809 UI spells with a total of 2,952,499 job applications in Joblog. Each of the UI spells in this data ends with the individual transitioning into a job. In the rest of the paper we refer to these jobs as the UI recipients’ new jobs. In Sections 3.2.6 and 3.4 we additionally trim our sample above and below certain propensity score thresholds (more details in the respective sections). As evident at the bottom of Table 3.1, this further reduces our sample for these exercises.

### 3.2.5 Measuring job characteristics and wages

Our analysis uses data on a range of characteristics of the jobs that men and women apply for and the new jobs they are hired into. For both job applications and new jobs, our analysis data contains information on the occupation of the job in terms of the standard ISCO classification and information on the industry of a firm in an aggregated version of the NACE Rev. 2 nomenclature.\(^{21}\) To construct a measure for whether jobs in our data are at high or low paying firms, we use matched employer-employee data to estimate an AKM model on log wages and obtain an estimated firm wage fixed effect for all Danish firms (see Appendix A.1.1 for details). We then link this to our job application and new jobs via the unique firm identifier.

Besides job characteristics, we are also interested in examining wages. For each of the new jobs that our sample of UI recipients are hired into, we observe actual wage payments. We use

\(^{20}\) The restriction that people must leave for unemployment introduces some censoring on our sample because our employment data only runs until September 2017. The amount of censoring is very small however. Most individuals find jobs within the first 30 weeks (see Figure A.4 in Appendix A.1.9, after 30 weeks most have found employment and there is only a slight gender difference). Redoing our analysis for the sample people who are not censored also yields virtually identical results.

\(^{21}\) The occupation and industry classifications are available to several degrees of detail, grouped in major, sub-major and minor groups. Occupations are thus grouped with 9, 55 or 153 respective occupations. The industries are grouped in 10, 21 or 38 respective 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](https://www.dst.dk/klassifikationsbilag/8cf95f88-8153-43b5-a82a-fa89adf6f214) (pp. 463-477)
this to construct a single wage measure as the wage that is paid to the individuals one month after entering a new job. For job applications, we have no direct measure of the potential wage the applicant would have received if hired. Instead, we predict a wage for each job based on its characteristics. Using the wages and characteristics of the new jobs that UI recipients are hired into, we apply a LASSO-based machine learning approach to predict the actual wage paid based on the full set of observed job characteristics. We compute this predicted wage for each of the job applications in Joblog as well as for each of the new jobs, and refer to it as the typical wage for a job with these characteristics. We refer to Appendix A.1.2 for details on this procedure. Note that since vacancies in Denmark rarely post a wage figure, our typical wage prediction approximates the way job seekers have to infer wages at the time they apply for jobs.

3.2.6 Conditioning on observables

The focus of our paper is to examine differences in the types of jobs men and women apply for and how these differences relate to gender gaps in hiring. As is well known, men and women often differ on a number of labor market characteristics that are likely to relate strongly to which jobs they apply for and are hired into. As we will see later, for example, many more men

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Table 3.1: Sample selection

<table>
<thead>
<tr>
<th></th>
<th>Individuals</th>
<th>Spells</th>
<th>Joblogs</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Inflow</strong></td>
<td>227,515</td>
<td>261,529</td>
<td>7,516,412</td>
</tr>
<tr>
<td>1. Minimum 8 weeks spell length</td>
<td>177,145</td>
<td>194,660</td>
<td>7,104,721</td>
</tr>
<tr>
<td>2. Spells with ( \geq 4 ) joblogs</td>
<td>170,638</td>
<td>186,352</td>
<td>7,094,045</td>
</tr>
<tr>
<td>3. Employment within 52 weeks</td>
<td>106,266</td>
<td>114,809</td>
<td>3,493,035</td>
</tr>
<tr>
<td>4. Censoring last 4 weeks of applications</td>
<td>106,266</td>
<td>114,809</td>
<td>2,952,499</td>
</tr>
<tr>
<td><strong>Analysis sample</strong></td>
<td>106,266</td>
<td>114,809</td>
<td>2,952,499</td>
</tr>
<tr>
<td>1. Trimming (observables)*</td>
<td>99,106</td>
<td>106,832</td>
<td>2,811,698</td>
</tr>
<tr>
<td>2. Trimming (observables &amp; behavior)**</td>
<td>92,054</td>
<td>98,857</td>
<td>2,611,351</td>
</tr>
</tbody>
</table>

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. On the analysis sample we eventually perform two additional trimming exercises in order to reduce extreme weights (propensity scores below 0.01 and above 0.99). In (*) we trim our sample when conditioning on observables (see Section 3.2.6) and in (**) we trim when jointly conditioning on observables and application behavior for our decomposition exercises in Section 3.4.

---

22. In our main analysis, our wage prediction is based on the Rigorous LASSO estimator of Belloni et al. (2012). We obtain very similar results if we use other Machine Learning approaches or if we simply regress log wages on the firm’s AKM fixed effect and occupation and industry dummies. See Appendix A.1.2 for details.

23. Note that this measure abstracts from wage differences *within* each specific job type by instead considering the average wage paid to employees in these job types.
than women in our sample of UI recipients have past experience from the construction sector, which naturally could make them more likely to both apply for and get hired into jobs in this sector.

In our main analysis, we are interested in conditioning out these observable differences among men and women to focus on differences in job applications and hiring outcomes among men and women with the same labor market observables. To do this, we employ a standard propensity score reweighting procedure and reweight the women in our sample to have the same observables as the men. As will become clear later, the use of propensity score reweighting ties in naturally with the decomposition we consider later (see Section 3.4).

Letting $i$ index individuals in our data, letting $m_i$ be an indicator for being male and letting $x_i$ be the vector of observable characteristics we wish to condition on, we compute the propensity score $\hat{p}_i$ for each woman by the conditional probability of being male given observables, $P(m_i = 1|x_i)$. We then weight woman $i$ by $\frac{\hat{p}_i}{\hat{p}_i}$. Appendix A.1.4 provides additional details.

In terms of which exact observables to focus on and include in $x_i$, the richness of the administrative means that we have data on an unusually large number of potentially relevant characteristics - almost certainly more characteristics than what we could reasonably include given our sample size. Rather than making ad-hoc decisions about which characteristics to include, we therefore discipline our set of conditioning variables using a LASSO procedure. After constructing a very large set of potential characteristics, we use the double-LASSO (Belloni et al., 2014) to select the subset of these variables that are most important for explaining wage differences between men and women. In practice, we let our full set of potential characteristics contain information on age, educational background and detailed labor market history, including labor market status and experience in both broad and narrow occupation and industry groups, and allow for a full set of interactions with age, experience and education length (4,196 variables in total). The double-LASSO selects 333 of these variables. In our main analysis we reweight the women in our sample based on estimated propensity scores using these 333 characteristics (see Appendix A.1.4 for details).

Finally, to avoid the usual issues of non-overlapping support when reweighting, in our main results, we trim individuals with a propensity score above 0.99 or below 0.01. This reduces our sample by 6.9 percent. We note that none of our main conclusions are sensitive to using other trimming cutoffs or using different sets of characteristics for computing weights, including controls for seasonality. Results controlling for seasonality are shown in Appendix A.1.13. Raw results that do not condition on observables are shown in Appendix A.1.10. The qualitative conclusions are the same as those presented later although gender gaps in both applications and hiring are generally much larger.

Table 3.2 shows characteristics at the beginning of the unemployment spell for the men in our analysis sample, and contrast with the characteristics of the women in our analysis sample both before and after reweighting. As noted, gender differences in observables are large in the raw data but become much smaller after reweighting.

---

24 The labor market history includes work experience in hours since 1980, weeks worked in every 1-, 2- and 3-digit occupation/industry for the last five years before unemployment as well as 1-, 2- and 3-digit information on the job prior to unemployment. We further include detailed information in the highest attained education level and the education field as well as the individuals age and weeks spent in unemployment, self-support and employment over the last five years.

25 Results controlling for seasonality are shown in Appendix A.1.13
### Table 3.2: Characteristics of the analysis sample

<table>
<thead>
<tr>
<th></th>
<th>Men Raw</th>
<th>Women Weighted</th>
<th>Women Raw</th>
<th>Men Raw</th>
<th>Women Weighted</th>
<th>Women Raw</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Age</strong></td>
<td>38.63</td>
<td>39.03</td>
<td>37.65</td>
<td></td>
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<td></td>
</tr>
<tr>
<td><strong>Education: Level</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Lower secondary</td>
<td>0.32</td>
<td>0.28</td>
<td>0.21</td>
<td>0.03</td>
<td>0.03</td>
<td>0.02</td>
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<tr>
<td>Upper secondary</td>
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<td>0.56</td>
<td>0.54</td>
<td>0.16</td>
<td>0.16</td>
<td>0.24</td>
</tr>
<tr>
<td>Tertiary</td>
<td>0.14</td>
<td>0.15</td>
<td>0.24</td>
<td>0.08</td>
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<tr>
<td><strong>Education: Field</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>General</td>
<td>0.49</td>
<td>0.46</td>
<td>0.42</td>
<td>0.02</td>
<td>0.02</td>
<td>0.03</td>
</tr>
<tr>
<td>Education</td>
<td>0.01</td>
<td>0.01</td>
<td>0.02</td>
<td>0.01</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td>Humanities</td>
<td>0.04</td>
<td>0.04</td>
<td>0.06</td>
<td>0.04</td>
<td>0.04</td>
<td>0.04</td>
</tr>
<tr>
<td>Social sciences</td>
<td>0.02</td>
<td>0.02</td>
<td>0.03</td>
<td>0.02</td>
<td>0.02</td>
<td>0.02</td>
</tr>
<tr>
<td>Business</td>
<td>0.12</td>
<td>0.13</td>
<td>0.18</td>
<td>0.12</td>
<td>0.12</td>
<td>0.12</td>
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<tr>
<td>Natural sciences</td>
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<td>0.01</td>
<td>0.01</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td>Information</td>
<td>0.01</td>
<td>0.01</td>
<td>0.00</td>
<td>0.01</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td>Engineering</td>
<td>0.19</td>
<td>0.21</td>
<td>0.05</td>
<td>0.26</td>
<td>0.26</td>
<td>0.26</td>
</tr>
<tr>
<td>Agriculture</td>
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<td>0.04</td>
<td>0.02</td>
<td>0.04</td>
<td>0.04</td>
<td>0.04</td>
</tr>
<tr>
<td>Health</td>
<td>0.03</td>
<td>0.03</td>
<td>0.16</td>
<td>0.04</td>
<td>0.04</td>
<td>0.04</td>
</tr>
<tr>
<td>Service</td>
<td>0.04</td>
<td>0.04</td>
<td>0.04</td>
<td>0.04</td>
<td>0.04</td>
<td>0.04</td>
</tr>
<tr>
<td><strong>Region</strong></td>
<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
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<tr>
<td>Hovedstaden</td>
<td>0.30</td>
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<td>0.70</td>
<td>0.50</td>
</tr>
<tr>
<td>Midtjylland</td>
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<td>0.22</td>
<td>0.22</td>
<td>0.12</td>
<td>0.12</td>
<td>0.12</td>
</tr>
<tr>
<td>Nordsjaelland</td>
<td>0.13</td>
<td>0.12</td>
<td>0.12</td>
<td>0.14</td>
<td>0.15</td>
<td>0.13</td>
</tr>
<tr>
<td>Sjaelland</td>
<td>0.14</td>
<td>0.15</td>
<td>0.13</td>
<td>0.22</td>
<td>0.23</td>
<td>0.21</td>
</tr>
<tr>
<td>Sydjylland</td>
<td>0.22</td>
<td>0.23</td>
<td>0.21</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Previous job/year</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wage</td>
<td>227.15</td>
<td>219.03</td>
<td>214.10</td>
<td>19.96</td>
<td>20.12</td>
<td>20.09</td>
</tr>
<tr>
<td>AKM Firm FE</td>
<td>0.01</td>
<td>-0.00</td>
<td>-0.01</td>
<td>1.46</td>
<td>1.54</td>
<td>1.60</td>
</tr>
<tr>
<td>Weeks of employment</td>
<td>35.90</td>
<td>35.61</td>
<td>33.70</td>
<td>18.70</td>
<td>18.69</td>
<td>21.32</td>
</tr>
<tr>
<td>Weeks of public transfers</td>
<td>18.70</td>
<td>18.69</td>
<td>21.32</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>UI Spell</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Length (weeks)</td>
<td>219.03</td>
<td>219.03</td>
<td>214.10</td>
<td>19.96</td>
<td>20.12</td>
<td>20.09</td>
</tr>
<tr>
<td>Logs p. week</td>
<td>35.61</td>
<td>35.61</td>
<td>33.70</td>
<td>1.46</td>
<td>1.54</td>
<td>1.60</td>
</tr>
<tr>
<td>Logs p. week (cond.)</td>
<td>18.70</td>
<td>18.69</td>
<td>21.32</td>
<td>1.74</td>
<td>1.78</td>
<td>1.83</td>
</tr>
</tbody>
</table>

Notes: The table shows summary statistics for our sample. All characteristics, except for the spell details, are measured at the beginning of the respective unemployment spell. The weighted samples are trimmed below propensity scores of 0.01 and above 0.99.

#### 3.2.7 Gender gaps in wages

A major motivation for the analysis in this paper is to shed light on how gender differences in job applications and hiring help shape the overall gender gap in wages. Before continuing with our analysis, it is therefore instructive to examine the gender wage gap that exists in the new jobs that our UI recipients transition into.

The teal bar on the left of Figure 3.1 shows the raw gender gap in log wages in these new jobs. On average, the male UI recipients in our data earn 0.07 log points more in their new jobs than the female UI recipients. As described in the previous section, our main interest in this paper is to examine differences in job applications and job outcomes for men and women with similar observables. Accordingly, in the bars on the right of Figure 3.1, we condition on observable variables using our reweighting procedure. The gender gap in our UI recipients’ new jobs is almost unchanged by doing this: after condition on observables, male UI recipients still earn...
0.061 log points more in their new jobs. This result reflects that women have caught up to or even overtook men in terms labor market characteristics, such as the level of schooling and experience (Goldin, 2014, 2006).

Instead of looking at the gap in actual wages, the blue bars in Figure 3.1 next shows the gap in typical wage of the new jobs. Recall from Section 3.2.5 that the typical wage is a measure of what each job could be expected to pay given its characteristics. The blue bars in Figure 3.1 show that after conditioning on observables, the gender gap in typical wage represents about a third of the gender gap in actual wages (two thirds for the raw version). In other words, male UI recipients tend to end up in types of jobs that typically pay more than females. Moreover, these gender differences in the characteristics of the jobs can explain a substantial part of the overall wage gap. This confirms the importance of looking at gender gaps in the characteristics of the jobs men and women apply for and get hired into.

Note that gender wage gap we consider here is the gender gap at entry into new jobs after an unemployment spell. Our measure thus considers a selected subsample of individuals on the Danish labor market and abstracts from differences that may arise over time. Differences in wage growth rates across jobs as well as differences in career progression and promotions may further reinforce the wage gaps at entry to employment. In order to get a rough comparison of the gender wage gap in our analysis sample to the overall working population in Denmark we examine a 10 percent sample of all those individuals regularly employed in August 2015 (the month before our sample starts). On this sample we do not estimate the set of weights we use to condition on observables elsewhere as the weights in Section 3.2.6 are based on a

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26In Appendix A.1.12 we provide evidence that men and women in fact target jobs with different wage growth rates which might increase the wage gap over time. Furthermore, Merlino et al. (2018) find that Danish women are less likely to get promoted than Danish men.
particular set of characteristics that have a different interpretation on the sample of individuals in employment.\textsuperscript{27} We thus focus on the raw wage gap in actual and typical wages.\textsuperscript{28} In Figure A.2 in Appendix A.1.5 we see that the wage gap on actual wages for the employed sample is about 50\% higher than the gap in starting wages in our sample of unemployment spells. The gap in typical wages is only slightly larger in the employment sample. This suggests that the differential allocation into job types explains a lower share of of the wage in the overall population, potentially driven by gender differences in wage growth and promotion rates that lead to increasing wage gaps over time. However, these numbers do not account for differences in observables between men and women.

### 3.3 Gender differences in job applications

We start our empirical analysis with a descriptive examination of the differences in the types of jobs men and women apply for when unemployed. For each of the job characteristics and for each UI recipient in our data, we compute what share of all their job applications has gone to a specific type of job. We then average these shares by gender to figure out what share of men and women’s job applications are on average going to a specific type of job and compare these average applications shares across gender. When looking at occupations, for example, we first compute what share of each individuals job applications have been sent to jobs in services, jobs in agriculture, clerical jobs, etc. Next we average these shares across men and women separately and compute the gender gaps in each of the shares.\textsuperscript{29} As discussed in Section 3.2.6, when computing averages, we apply propensity score reweighting to the women in our sample so that we are comparing women and men with the same observables. Appendix A.1.10 shows that unweighted results are qualitatively similar to what we present here. We further confirm the general robustness of our results in relation to our sample restrictions, and other potential confounders, in Appendix A.1.13.

#### 3.3.1 Application differences across occupations

In Figure 3.2 we look at differences in the occupations men and women are applying to. The teal bars show the female-male gap in the average share of applications going to different 1-digit occupation groups. We see clear gender differences. Most noticeably, women send on average almost 12 percentage points more of their applications to service occupations and about 5 percentage points fewer to craft occupations. We note that, because these results are conditional on observables (including several measures of past education and occupation), these large occupation differences are not be explained by men being more likely to have education or experience from a particular occupation.

\textsuperscript{27}In our main analysis we rely heavily on the characteristics of the previous job in order to condition on observables. These characteristics may have a different interpretation on the individuals currently in employment.

\textsuperscript{28}We do not re-estimate the full Rigorous-LASSO used to obtain typical wages in our analysis sample. Instead we use the set of variables already selected on the unemployed in Appendix A.1.2 and re-estimate the regression coefficients on those variables for the sample of employed.

\textsuperscript{29}We subtract the male share from the female share, thus positive gaps indicate that women are typically sending more of their applications to these specific jobs whereas negative gaps indicate that men typically send more.
To provide a point of comparison for the gaps in job applications, the blue bars in Figure 3.2 show the gaps in the share of men and women in our sample who are hired into each of the occupation groups at the end of their unemployment spell. To think about how these gaps relate to job application gaps, note that if men and women are treated exactly the same by employers and if each job application has the same probability of resulting in a hire, the gap in the share of hires should be very close to the gap in the average application share.\footnote{The probability of getting hired also depends on the number of applications sent and not only on the share sent to specific job types. If there are no gender differences in the number of applications then the application and hiring shares would be identical if men and women have the same probability of getting hired. Table A.3 in Appendix A.1.9 suggests that there are only small gender differences, both in the number of actual applications sent and the number of applications registered in Joblog.} Figure 3.2 indeed shows a clear correspondence between hiring gaps and application gaps. Occupations with a large positive gap in application shares also tend to have larger gaps in hiring and vice versa for occupations with negative gaps. In terms of magnitudes, however, the observed gender gaps in application shares are uniformly bigger than the gaps in hiring outcomes. This is consistent with a form of decreasing returns to targeting applications towards specific occupations.\footnote{Decreasing returns to targeting implies a concave relationship between the probability of landing a job in some occupation and how many applications have been targeted there. A given gender gap in the application share going to some occupation will in this case translate to a smaller gender gap in the probability of ending up in this occupation.}

Finally, for the purpose of linking these results with our later results on the gender wage gap, the occupations on the X-axis of Figure 3.2 have been sorted left-to-right according to the average wage paid in the occupations. Looking across the figure, we note that there is a noticeable tendency that women apply less towards higher paying occupations: The female-male gap is negative for all occupations in the right hand side of Figure 3.2.

### 3.3.2 Application differences across industries and firm wage levels

In Figures 3.3 and 3.4, we examine application gender gaps for (1-digit) industries and in terms of firm’s wage levels (measured by deciles of the estimated AKM firm fixed effects). Both figures follow the sample template as Figure 3.2: Teal bars show the female-male gap in average applications sent to some type of job while blue bars show the corresponding gaps in hiring shares. On the X-axis of Figure 3.3, industries have also been arranged left-to-right according to wage level.

In Figure 3.3 we see that the pattern of results for industries mirror the results for occupations: There are clear gender gaps in which industries men and women apply to and these gap closely mirror the gaps in where men and women are typically hired. Relative to occupations, however, we note that the overall magnitude of the gaps in application shares is smaller. The pattern of application gaps in terms of high-paying vs low-paying industries is also less clear.

Figure 3.4 examines whether men and women have different propensities to apply to high- or low-paying firms. We see a very clear gender gap here as women are sending a systematically larger share of their applications to lower paying firms: The female-male gap in application shares is positive for each of the bottom 6 deciles and negative for the top 4.\footnote{Note that we calculate all ranks within each 1-digit industry group as the specific industries might reflect segmented markets. The ranks are further weighted by the number of employees. For details see Appendix A.1.1.}
### 3.3.3 Application differences across typical wages

In Figure 3.5 we explore the gender gaps in applications and hiring in typical wage deciles. Here the teal bars show the female-male gap in average applications sent to the different typical wage deciles while blue bars show the corresponding gaps in hiring shares.

Following the same patterns as Figure 3.4, the gender gaps in Figure 3.5 show that women are sending a significantly larger share of their applications to jobs with lower typical wages. We see that women send 13 percentage points more of their applications to the bottom third deciles of typical wages and 11 percentage points less of their applications to jobs in the top third. Over most deciles, the gender gap in applications follow the gender gap in hiring outcomes, yet from the 5th to the 7th decile they are not significantly different from zero.

One obvious question is whether men and women face very different typical wages, in which case gender differences in the type of jobs that men and women target may be fully rational. In Appendix A.1.11 we report the same analysis for alternative typical wage measures that are exclusively estimated on the re-employment wages for either men or women. Figure 3.5 shows how our results qualitatively do not change if we deploy a typical wage measure that is estimated on a purely female (male) sample.

Overall, our analysis shows that there is a significant gap in the typical wages that men and women are paid after transitioning from unemployment into employment. However, differences in the within-firm growth rate of wages may further reinforce the gaps over time. In Appendix A.1.12 we thus examine gender gaps across wage growth deciles, calculated over one or five years. Figure A.8 shows that there are indeed distinct gender differences along these
3.3. Gender Differences in Job Applications

**Figure 3.3:** Gender gaps in job applications and hiring, industry groups

![Gender gaps in job applications and hiring, industry groups](image)

Note: Figure plots gender gaps in shares of applications going to specific industries and gaps in where job-seekers are hired. All gaps are conditional on observable characteristics. Industries are ranked from lowest paying on the left to highest paying on the right. The 95% confidence bars are based on standard errors clustered on the individual level.

**Figure 3.4:** Gender gaps in job applications and hiring, firm wage level

![Gender gaps in job applications and hiring, firm wage level](image)

Note: Figure plots gender gaps in shares of applications going to specific firm fixed effect deciles and gaps in which decile job-seekers are hired. All gaps are conditional on observable characteristics. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure 3.5: Gender gaps in job applications and hiring, typical wage

Note: Figure plots gender gaps in shares of applications going to specific typical wage deciles and gaps in which decile job-seekers are hired. All gaps are conditional on observable characteristics. The 95% confidence bars are based on standard errors clustered on the individual level.

lines, insofar that women tend to apply more to firms with lower short- and long-term wage growth, thus likely leading to gradual increases in the wage gap.

3.4 Can gender differences in applications explain gender gaps?

In the previous section we have seen that there are distinct differences in the types of jobs men and women apply to, even when conditioning on observable differences in their labor market characteristics. A natural question to ask is to what extent these differences in application behavior are capable of explaining the observed gender gaps in hiring outcomes.

To provide an answer to this question, we leverage the fact that our data contains joint information on both job applications and actual hiring outcomes for the same individuals. This allows us to apply the standard semi-parametric decomposition method introduced by DiNardo et al. (1996) (see Fortin et al. (2011) for a more recent treatment). The decomposition allows us to quantify what part of the observed gender gaps in hiring outcomes can be explained by applications under a standard ignorability or conditional independence assumption.

Over the next two sections we first briefly lay out the relevant methodology and then present results.

3.4.1 Decomposition and counterfactuals, methodology

For some UI recipient in our sample, let $m$ be an indicator for being male, let $x$ be the vector of other observables that we have been conditioning on throughout (see Section 3.2.6) and let $a$ be some vector capturing which types of jobs the person has applied to. In our main results we
3.4. Can Gender Differences in Applications Explain Gender Gaps?

Let $a$ consist of the share of applications sent to each 2-digit occupation, each 2-digit industry, each decile of the firm fixed effect distribution and each decile of the typical wage distribution. Let $y$ be a measure of job type (occupation, industry, wage etc.). For expositional convenience we will assume that $y$ is discrete, while $x$ and $a$ are absolutely continuous.

Now let $P_M(y)$ and $P_W(y)$ denote the probability of ending ones UI spell by being hired into a job of type $y$, conditional on being a man and a woman respectively. Further let $P_M(y|a,x)$ and $P_W(y|a,x)$ be the corresponding conditional probabilities when additionally conditioning on having observable characteristics $x$ and having made applications $a$. To decompose gender gaps in hiring outcomes, we are interested in estimating counterfactual versions of the hiring probabilities $P_M(y)$ and $P_W(y)$ that show how women’s hiring outcomes would differ if they had the same observables and/or job application behavior as men. Now, to do so we start by noting that,

$$P_M(y) = \int \int p_M(y|a,x) f_{M|X}(a|x) f_{M}(x) \, da \, dx$$

$$P_W(y) = \int \int p_W(y|a,x) f_{W|X}(a|x) f_{W}(x) \, da \, dx$$

Here $f_{M}$ and $f_{W}$ are the distributions of observables among men and women respectively, while $f_{M|X}$ and $f_{W|X}$ are the distribution of applications among men and women after conditioning on observable characteristics.

### Conditioning on observables

As before, we are primarily interested in differences in job application behavior between men and women with similar observable characteristics. Instead of doing decompositions on the raw differences in hiring probabilities, we therefore first wish to condition out differences in observables. For this purpose let the counterfactual hiring probability for women conditional on observables, $\tilde{P}_W^X(y)$, be the hiring probability that women would have faced if they had the same distribution of observables as men:

$$\tilde{P}_W^X(y) = \int \int p_W(y|a,x) f_{W|X}(a|x) f_{X}(x) \, da \, dx$$

Based on this, we can define the gender gap in the hiring probability for job type $y$ after conditioning on observables. We will refer to as the **baseline gap**:

$$BaselineGap(y) = \tilde{P}_W^X(y) - P_M(y)$$

To compute this gap, we need construct estimates of $\tilde{P}_W^X(y)$ and $P_M(y)$. $P_M(y)$ is straightforward to estimate as the share of men in our sample who get hired into job type $j$. As shown by DiNardo et al. (1996), $\tilde{P}_W^X(y)$ can be estimated simply by propensity score reweighting the sample of women to have the same observables as the men, followed by computing the share of women who get hired into job type $j$ on the reweighted sample. In implementing this, we apply the exact same reweighting procedure as used previously. If the sample is kept constant,
our estimates of $BaselineGap(y)$ will thus be numerically the same as the computed gender gaps in hiring shares shown in Figures 3.2 to 3.5.\footnote{As we return to in the next section, the decomposition requires additional trimming of our baseline sample so the baseline gaps presented later will be slightly different than the hiring share gaps presented earlier.}

**Conditioning also on application behavior**

$GapX$ shows the gender gap in hiring probabilities that remains when the differences in observables between and men and women have been taking into account. The key question for our decomposition exercise is how much of this gap can be explained by application behavior. To answer this we define $\tilde{\beta}_{W,X}^W(y)$ to be the hiring probability that women would have faced if they had the same distribution of application behavior as men (as well as the same distribution of observables):

$$\tilde{\beta}_{W,X}^W(y) = \int \int p_W(y|a,x) f_M^M(a|x) f_M^M(x) da dx$$

Based on this, we define the gender gap in hiring probability that remains after conditioning also on applications. We refer to this as the *residual gap* in hiring probabilities after application behavior has been accounted for:

$$ResidualGap(y) = \tilde{\beta}_{W,X}^W(y) - P_M(y)$$

We can then define our decomposition of interest:

$$BaselineGap(y) = \left( \tilde{\beta}_{X}^W(y) - \tilde{\beta}_{A,X}^W(y) \right) + ResidualGap(y)$$

Explained by applications Remaining gap after applications

To implement this decomposition, we need an estimate of $\tilde{\beta}_{W,X}^W(y)$. As shown in Appendix A.1.6, however, such an estimate can be obtained simply by propensity score reweighting the sample of women to have the same observables and application behavior as the men and then simply computing the share of women who get hired into job type $j$ on the reweighted sample. As before, we compute the weights by estimating propensity scores using a logit model with both the observables, $x$, and the application shares, $a$, as explanatory variables.

Before proceeding to discuss identification, we note two things about this decomposition:

First, the decomposition above was presented for the gaps in conditional hiring probabilities but computation of decompositions and counterfactuals can directly be applied to other measures that depend on the conditional hiring probabilities. In the next section, we thus apply the decomposition also to the gender gap in average typical wages and the Duncan measure of gender segregation (Duncan and Duncan (1955)).

Second, since the residual gap for job type $y$ shows the gender gap in hiring probabilities into job type $y$ when men and women have the same application behavior, this residual gap is a useful summary measure of whether women are more or less likely to be hired into job type $j$ conditional on applying for it.\footnote{Formally, $ResidualGap(y)$ reflects the gap in conditional hiring probability faced by the average man.} We exploit this later in Section 3.5.
3.4. CAN GENDER DIFFERENCES IN APPLICATIONS EXPLAIN GENDER GAPS?

Identification of counterfactuals in the decomposition

Before proceeding, it is important to be clear about what assumptions we use to identify the counterfactual hiring probabilities used in the decomposition. As discussed and formalized in Fortin et al. (2011), the key identifying assumption can be viewed as a standard ignorability or conditional independence assumption. The assumption is reflected by the fact that throughout the definition for the counterfactual hiring probabilities, \( \tilde{P}_{W|A,X}(y) \), the conditional hiring probability for women \( P^W(y|a,x) \) is assumed to be unchanged relative to what is observed in the data. In other words, if some woman who is not currently applying to more male jobs starts to apply to male jobs, we are assuming that she will face the same hiring outcomes as women with similar observables who currently are already applying to more male jobs.

There are obvious concerns with this identifying assumption. Perhaps the most salient concern is the possibility that women who are currently applying to more male jobs are different along dimensions not captured by our vector of unobservables and that this also affects their hiring outcomes. Although our vector of observable characteristics aims to include very detailed measures of the relevant labor market characteristics (see Section 3.2.6), we are still likely to miss some important characteristics. Women applying to more male jobs could, for example, have a more competitive personality, which may impact their hiring probabilities. In this case, the hiring outcomes of these women are likely to serve as poor counterfactuals for what would happen if other women apply to more male jobs.

Finally, in implementing the reweighting procedure, we again need to deal with the issue of thin or non-overlapping support as there may be some combinations of characteristics and application behavior that is (virtually) only ever observed among either men or women. We again do this by trimming the sample based on the propensity score. As discussed in Section 3.2.6, our base sample has been trimmed to ensure that propensity scores based on observables are all in between 0.01 and 0.99. Throughout this section of the paper, however, we additionally sample to exclude observations where the estimated propensity scores based also on applications are above 0.99 or below 0.01. Relative to the baseline sample, this reduces our sample by an additional 7,052 observations or 7.1 percent.

3.4.2 Decomposition and counterfactuals, results

We now present results from the decomposition. Starting with occupations, the teal bars in Figure 3.6a ("Observables") show the baseline gap in hiring probabilities across 1-digit occupations after conditioning on observables. The blue bars ("Observables and behavior") show the corresponding residual gap that remains after also conditioning on applications. Comparing the two sets of gaps, we see that applications can explain a significant part of the gender gap in hiring into service, clerical and associate professional occupations. For both craft and service occupations, women are about 3 percentage points more likely to be hired in the

\[
ResidualGap(y) = \tilde{P}_{W,A,X}^W(y) - P^M(y) = \int \int \left( P^W(y|a,x) - P^M(y|a,x) \right) \frac{f^M_{a|x}(a|x)}{f^M(x)} \, da \, dx \\
= E \left[ P^W(y|a,x) - P^M(y|a,x)|m = 1 \right]
\]
baseline, however, after accounting for where men and women apply, this gap falls to 1 around percentage point in both cases. For associated professional occupations, women are roughly 1.5 percentage point less likely than men to be hired than men at baseline, but accounting for application behavior reduces this gap to roughly zero. Accounting for applications also has a particularly large effect on the hiring gap into professional occupations. In the baseline women are about 2 percentage points less likely to be hired, after accounting for applications, however, this gap flips and women are almost three percentage points more likely to be hired. For the remaining occupations, the hiring gaps are relatively unaffected by accounting for applications as baseline and residual gaps are very similar. In particular, we note that application behavior can not explain why women are noticeably under-represented in the craft occupations.

**Figure 3.6:** Decomposing gaps in hiring shares

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wages

Note: Figure plots gender gaps in gaps in the types of jobs job-seekers are hired after sequentially conditioning on observable characteristics and application behavior. The 95% confidence bars are based on standard errors clustered on the individual level.

Figure 3.6b shows corresponding results looking at the industry dimension. Accounting for applications here primarily affects the hiring gap into manufacturing and communication. In the baseline, women are almost 3 percentage points more likely to be hired into manufacturing. However, after accounting for application behavior, they are in fact slightly less likely to be so. Women are about 2 percentage point less likely to be hired into communications at baseline,
but after accounting for applications this gap essentially disappears. For the other industries, we see that women are slightly over-represented in the public sector at baseline and noticeably under-represented in the construction sector. Neither of these differences can be explained by applications. For the remaining industries, hiring gaps are small both before and after conditioning on applications.

Figure 3.6c shows results focusing on the firm wage level (as before measured by deciles of the AKM firm fixed effect distribution). Accounting for applications can explain a very noticeable part of the hiring gaps we see here. For most deciles, the gender gap in hiring shares essentially disappears once we account for applications. While at baseline there is thus a very clear tendency for women to be hired at systematically lower paying firms, a markedly different pattern emerges once we account for differences in application behavior. After this, women face higher hiring shares at firms around the middle of the wage distribution (the 30th and 50th percentile in particular) but face markedly significantly lower hiring rates in the top and bottom deciles.

Finally, Figure 3.6d shows results looking at the typical wage of the job. Similar to the overall firm wage level, we see that conditioning on application behavior reduces a particularly large fraction of the gender gaps in hiring. The clear gaps at the lower end of the typical wage distribution practically disappear. At the upper end of the typical wage distribution we still see a pronounced gap on the 90th percentile, yet contrary to the substantially higher hiring rate for men in the highest typical wage decile at baseline, women are more likely to enter these jobs once accounting for application behavior. Overall, we see a reduced tendency for women to end up in the lower-paying jobs.

In Table 3.3 we provide a summary of the decomposition, showing how much of the observed gender gaps in hiring can be explained by applications. The first two rows focus on gender gaps in terms of occupations of industries. As our summary measure for the overall gender gaps across occupations and industries we use the Duncan measure of gender segregation (Duncan and Duncan, 1955). This measure shows what share of men or women needs to be shifted to a different occupation (industry) in order to have perfect gender balance across occupations (industries). For both industries and occupations, gender differences in application behavior can explain about 30 percent of the baseline segregation by gender: Occupation and industry segregation is 0.068 and 0.053 in the baseline but the residual segregation after accounting for applications is only 0.047 and 0.035.

Next, we examine the wage level of the hiring firm. We see that the baseline men work at firms with a 0.012 higher firm wage level, as measured by the firms’ AKM fixed effect. After continuing on application behavior, this gender difference reduces greatly, such that the residual gender gap amounts to 0.003. Application behavior thus explains about 80 percent of the baseline gap in the firm wage level.

The next row decomposes the gender gap in the typical wage of the job given its characteristics. At baseline men are hired into jobs with typical wages that are on average 2.4 percent higher than women. Almost all of this difference can be explained by differences in where men and women apply. After conditioning on applications, the gap in typical wages is only 0.3 percent, suggesting that differences in applications can explain 86 percent of the baseline gap.

Finally, we can also perform the decomposition using the actual wage paid in the new job. Relative to the typical wage for a job, the actual wage paid can be higher or lower depending things like individual wage bargaining or whether the job has characteristics that influence
### Table 3.3: Decomposing gender gaps

<table>
<thead>
<tr>
<th></th>
<th>Baseline</th>
<th>Explained by applications</th>
<th>Residual</th>
</tr>
</thead>
<tbody>
<tr>
<td>Occupational segregation</td>
<td>0.068</td>
<td>0.021</td>
<td>0.047</td>
</tr>
<tr>
<td>(Duncan index, 1-digit)</td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
<tr>
<td></td>
<td>[0.31]</td>
<td>[0.69]</td>
<td></td>
</tr>
<tr>
<td>Industry segregation</td>
<td>0.053</td>
<td>0.018</td>
<td>0.035</td>
</tr>
<tr>
<td>(Duncan index, 1-digit)</td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
<tr>
<td></td>
<td>[0.34]</td>
<td>[0.66]</td>
<td></td>
</tr>
<tr>
<td>Firm wage level</td>
<td>0.012</td>
<td>0.009</td>
<td>0.003</td>
</tr>
<tr>
<td>(Male-female gap, AKM fixed effect)</td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td></td>
<td>[0.80]</td>
<td>[0.20]</td>
<td></td>
</tr>
<tr>
<td>Typical wage for job</td>
<td>0.024</td>
<td>0.021</td>
<td>0.003</td>
</tr>
<tr>
<td>(Male-female gap, log typical wage)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td></td>
<td>[0.86]</td>
<td>[0.04]</td>
<td></td>
</tr>
<tr>
<td>Actual Wages</td>
<td>0.061</td>
<td>0.039</td>
<td>0.022</td>
</tr>
<tr>
<td>(Male-female gap, log wage)</td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.006)</td>
</tr>
<tr>
<td></td>
<td>[0.64]</td>
<td>[0.36]</td>
<td></td>
</tr>
</tbody>
</table>

Notes: The table displays decomposition results after accounting for application behavior. All results account for observable differences between men and women and are displayed with bootstrapped standard errors in parenthesis (2,000 replications). Share of contribution to the baseline gap in brackets. The second column shows the parts explained by application behavior, the third column displays the residual after controlling for applications behavior. DFL re-weighting uses logit specifications where \( a_i \) includes the share of applications sent to each 2-digit occupation, each 2-digit industry, each deciles of the firm fixed effect distribution and each decile of the typical wage distribution.

Pay but are not considered in our measure of typical wage. In the baseline, men are paid 6.1 percent than women in their new job. Differences in where men and women apply can account for 3.9 percentage points or 64 percent of this gap.

Overall, we conclude that differences in where men and women apply are capable of explaining are very large fraction of the observed differences in where men and women are hired.  

---

\[35\] We repeat this exercises for the unweighted sample as well as for male/female typical wages in Appendix A.1.14 and A.1.15.
3.5 Explaining gender differences in job applications

The previous section has shown some clear gender differences in the type and the payment of jobs that men and women apply to. Using our semi-parametric decomposition approach, we can attribute 86 percent of the post-unemployment typical wage gap to differences in application patterns. Naturally, the question of what drives these differences in application behavior may arise. In this section, we provide some indicative evidence for potential explanations behind these differences. One could think of at least two different explanations that could rationalize our results (they may also be related to each other): First, women may target other job characteristics that correlate negatively with pay (aside from the ones we studied before). If women value these other characteristics and thus apply to these jobs more frequently, it would also imply that they would be more likely to end up in lower-paying jobs (conditional on being hired). Second, the application behavior of women may, at least to some extent, result from self-fulfilling statistical discrimination and or social norms. In other words, the application behavior of women might reflect perceived preferences from the demand side: Women apply less to higher-paying jobs because they expect that the likelihood of being hired in these positions is smaller. This response to an expected difference in hiring probabilities could be rational, i.e. reflecting true differences in preferences across gender on the demand side, but it could also be a misperception and potentially even reflect over-/underconfidence on the supply side.

Distinguishing between these explanations is hard, and both types of mechanisms may be at play jointly. Below we argue that the explanations linked to differential valuation of job characteristics are important in shaping the gender gap in applications. In particular, we show that there are differences along other characteristics of the jobs and that these correlate negatively with pay. Obviously, this does not imply that the behavior of women is not partly shaped by the demand side. We also show that men and women differ in the type of jobs they target relative to their previous jobs in systematic ways: Men show signs of overconfidence as they are more likely to target higher paying occupations relative to their previous job, while women are more often applying to jobs that represent a step down on the occupational letter. Nevertheless, these differences do not seem to be significantly driven by self-fulfilling discrimination as women tend to apply only slightly less to those positions in which they are less likely to be hired in conditional on applying.

3.5.1 The role of other job characteristics

As a first step, we show that women target specific job characteristics to a higher degree than males. This suggests that in choosing which jobs to apply to, women don’t exclusively focus on wages, industries and occupations, but also put considerable weight on other characteristics of the job. We analyze three types of job characteristics that have been discussed in the previous literature and correlate negatively with typical wages in our sample: First, women target jobs with a shorter commute than men. Second, women are more likely to apply to jobs that involve part-time work. Third, women are more likely to target jobs in firms where the average maternal leave length is longer, a measure that we think of as a crude proxy for family-friendliness.

Another channel could be differences in the search channels and methods men and women use to apply. In Appendix A.1.16 we show that there are no substantial differences on these dimensions.
In Figure 3.7a we group applications into broad categories based on the estimated commute time for each application. The figure shows that women are targeting more jobs with shorter commute times. This finding is consistent with Le Barbanchon et al. (2019a) who argue that around 10 percent of the gender wage gap can be attributed to women’s willingness to trade off shorter commuting times with lower pay. It further confirms Banfi et al. (2019) who show that unemployed women are typically applying to jobs that are spatially closer than those unemployed men apply to.

As wage differentials between part- and full-time work contracts can have a profound effect on the gender wage gap, we analyze gender differences along these lines in Figure 3.7b. We distinguish between full-time (37 hours/week) and positions below full-time. The figure shows that women are more likely to apply to positions that involve contracts with fewer hours, while also being more likely to be hired there. This may not only contribute to the gender wage gap through part-time/full-time wage differentials (see e.g. Manning and Petrongolo, 2008; Bardasi and Gornick, 2008), but it might also spur a long-term divergence in wage rates as part-time jobs are linked to wage stagnation (Costa Dias et al., 2018; Russo and Hassink, 2008).

Last, in Figure 3.8, we group applications into deciles of the average length of maternal leave which women take in the respective firm, see Appendix A.1.7 for the construction and the distribution of this measure. The length of maternal leave in particular can for example be linked to parenting stress (Chatterji et al., 2013), we thus think of this as a crude proxy for family friendliness. This relates to Hotz et al. (2018) who build a more advanced index of family friendliness for Swedish firms. Figure 3.8 shows that men are over-represented at firms that have no woman giving birth in our sample period, both for applications and hiring. This also implies that women apply more to firms in which we observe other women giving birth. The gender gaps in applications and hiring are also gradually increasing in the length of average maternal leave. Thus, women are more likely to apply and get hired to firms with longer maternal leave durations.

In Table 3.4 we display the estimates from separately regressing the job characteristics we examined above on the typical wages at entry to employment. Shorter commute times, part-time contracts and longer average maternal leave lengths all correlate negatively with our wage measure. This suggests that there might be a trade-off between pay and certain amenities of the job that materializes in the gender differences in application behavior we observe.

---

37 The commute time is measured between the location of the job, based on the postal code, and the individuals municipality of residence. The commute time measure is obtained from Google Maps API and accounts for congestion.

38 Despite generous overall parental leave policies in Denmark, there seems to be sufficient variation in the actual length of the leave taken to relate it to identify potential differences in application behavior (see Figure A.3 in Appendix A.1.7)

39 Note that the coefficient on commute time has a positive sign. This means that shorter commute times are linked to lower wages.

40 Note that by construction these negative correlations in typical wages also reflect heterogeneity across sub-markets (in terms of industry/occupation) as none of these characteristics enter directly into determining typical wages (see Section A.1.2).
### Figure 3.7: Gender gaps in job applications and hiring

**(a) Commuting distances**

![Graph showing gender gaps in commuting distances](image)

<table>
<thead>
<tr>
<th>Commuting Distance</th>
<th>Female-Male Gap</th>
<th>Avg. Share of Applications</th>
<th>Share of Hires</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-30 min</td>
<td>-0.07</td>
<td>0.01</td>
<td>0.03</td>
</tr>
<tr>
<td>30-60 min</td>
<td>-0.04</td>
<td>0.02</td>
<td>0.04</td>
</tr>
<tr>
<td>60-90 min</td>
<td>-0.01</td>
<td>0.03</td>
<td>0.05</td>
</tr>
<tr>
<td>&gt;90 min</td>
<td>0.01</td>
<td>0.04</td>
<td>0.06</td>
</tr>
</tbody>
</table>

**(b) Hours**

![Graph showing gender gaps in hours](image)

<table>
<thead>
<tr>
<th>Hours</th>
<th>Female-Male Gap</th>
<th>Avg. Share of Applications</th>
<th>Share of Hires</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full-time</td>
<td>-0.09</td>
<td>0.01</td>
<td>0.03</td>
</tr>
<tr>
<td>Part-time</td>
<td>-0.06</td>
<td>0.02</td>
<td>0.04</td>
</tr>
<tr>
<td>Avg. Share of Applications</td>
<td>-0.04</td>
<td>0.03</td>
<td>0.05</td>
</tr>
<tr>
<td>Share of Hires</td>
<td>0.01</td>
<td>0.04</td>
<td>0.06</td>
</tr>
</tbody>
</table>

Note: Figure plots gender gaps in shares of applications going to jobs with specific characteristics and gaps in where job-seekers are hired. All gaps are conditional on observable characteristics. Commuting distances are measured using Google API from the individuals municipality of residence to the job and account for congestion. Full-time jobs are those with at least 37h of work per week, part-time jobs are those with less.

### Figure 3.8: Gender gaps in job applications and hiring, avg. maternal leave length

![Graph showing gender gaps in average maternal leave length](image)

<table>
<thead>
<tr>
<th>Decile</th>
<th>Female-Male Gap</th>
<th>Avg. Share of Applications</th>
<th>Share of Hires</th>
</tr>
</thead>
<tbody>
<tr>
<td>No birth</td>
<td>-0.2</td>
<td>0.01</td>
<td>0.03</td>
</tr>
<tr>
<td>10th</td>
<td>-0.12</td>
<td>0.02</td>
<td>0.04</td>
</tr>
<tr>
<td>20th</td>
<td>-0.04</td>
<td>0.03</td>
<td>0.05</td>
</tr>
<tr>
<td>30th</td>
<td>0.04</td>
<td>0.04</td>
<td>0.06</td>
</tr>
<tr>
<td>40th</td>
<td>0.12</td>
<td>0.05</td>
<td>0.07</td>
</tr>
<tr>
<td>50th</td>
<td>0.2</td>
<td>0.06</td>
<td>0.08</td>
</tr>
<tr>
<td>60th</td>
<td>0.2</td>
<td>0.07</td>
<td>0.09</td>
</tr>
<tr>
<td>70th</td>
<td>0.12</td>
<td>0.08</td>
<td>0.10</td>
</tr>
<tr>
<td>80th</td>
<td>0.04</td>
<td>0.09</td>
<td>0.11</td>
</tr>
<tr>
<td>90th</td>
<td>0.01</td>
<td>0.10</td>
<td>0.12</td>
</tr>
<tr>
<td>100th</td>
<td>-0.04</td>
<td>0.11</td>
<td>0.13</td>
</tr>
</tbody>
</table>

Note: Figure plots gender gaps in shares of applications going to firms with different average parental leave lengths and gaps in which decile job-seekers are hired. All gaps are conditional on observable characteristics. The average maternal leave length is calculated among all women giving birth in a specific firm. The 95% confidence bars are based on standard errors clustered on the individual level.
Table 3.4: Correlation between job characteristics and typical wages

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Typical wage</td>
<td>Typical wage</td>
<td>Typical wage</td>
<td>Typical wage</td>
</tr>
<tr>
<td>Commute (min)</td>
<td>0.00003***</td>
<td>0.00003***</td>
<td>(0.00000)</td>
<td>(0.00001)</td>
</tr>
<tr>
<td>Part-time (0/1)</td>
<td>-0.02130***</td>
<td>-0.02080***</td>
<td>(0.00094)</td>
<td>(0.00094)</td>
</tr>
<tr>
<td>Maternal leave (days)</td>
<td>-0.00034***</td>
<td>-0.00032***</td>
<td>(0.00001)</td>
<td>(0.00000)</td>
</tr>
<tr>
<td>Constant</td>
<td>5.174***</td>
<td>5.189***</td>
<td>5.224***</td>
<td>5.231***</td>
</tr>
<tr>
<td></td>
<td>(0.0007)</td>
<td>(0.0008)</td>
<td>(0.0023)</td>
<td>(0.0025)</td>
</tr>
<tr>
<td>Observations</td>
<td>114,809</td>
<td>114,809</td>
<td>114,809</td>
<td>114,809</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.001</td>
<td>0.005</td>
<td>0.005</td>
<td>0.010</td>
</tr>
</tbody>
</table>

Notes: ***p < 0.01, **p < 0.05, *p < 0.1. The table shows coefficients of regression typical wages at employment entry on different job characteristics. Standard errors are clustered at the spell level.

3.5.2 Motherhood

An increasing body of evidence, much of which is based on Danish data, points to the role of motherhood in shaping gender differences in earning and wages. For example, Kleven et al. (forthcoming) find a substantial motherhood penalty in earnings that is to approximately equal parts attributable to reductions in labor force participation, hours worked and a decline in wages. Similarly, Lundborg et al. (2017) explain this motherhood penalty in wages by a move to lower-paying jobs that are spatially closer to the mothers’ home.41

In order to understand whether motherhood also affects job search, we explore the female-male gaps in applications and hiring for a subsample of young workers with and without children. Specifically, we are looking at men and women aged 25 to 40 years and compare those who do not live with a child with those who do live with at least one child under the age of 5.42 In this section we show differences in applications and hiring along typical wage deciles and report gender differences along further characteristics of the job in Appendix A.1.17. In Figure 3.9 we see that the clear patterns we observed earlier, namely that women apply more to jobs associated with lower typical wages compared to men, is present both among unemployed with and without children. Yet, many of these differences are small and not statistically significant. For the sample of men and women with children below the age of 5 however, the gender differences are indeed more exacerbated and statistically significant.

41 Further evidence shows that a part of the penalty can be attributed to skill depreciation and a loss of earnings opportunities during early motherhood (Adda et al., 2017). For Sweden, Angelov et al. (2016) show that giving birth results in a 10 percent wage gap between fathers and mothers after 15 years.

42 Our data does not come with readily available information on the children’s birthdays. We can however use yearly information from the FAM database to identify the number of children’s as well as the age of the youngest child of every cohabiting family. We define individuals not living with a child at home as those who are not listed as living with a kid prior to the start of their UI spell. Further, we define individuals living with a child below the age of five as those who are listed as living with at least one kid below 5 prior to the start of their UI spell.
For both subsamples, the eventual hiring corresponds well to gaps in application behavior. This might suggest that women, more so than men, adjust their job search as a result of recent parenthood which may lead to the observed motherhood penalties the previous literature identified.

**Figure 3.9:** Gender gaps in matching and applications, parenthood

(a) Typical wage: No children

(b) Typical wage: Child below 5

Note: Figure plots gender gaps in shares of applications going to specific typical wage deciles and gaps in which decile job-seekers are hired. All gaps are conditional on observable characteristics. Panel (a) restricts the sample to individuals aged between 25 and 40 who do not live with a child at home whereas the panel (b) considers individuals of the same age that live with a child below the age of 5 at home. The 95% confidence bars are based on standard errors clustered on the individual level.

In Figure A.19 in Appendix A.1.17 we additionally report that there are no clear and significant gender differences in geographical mobility for men and women without children, measured as above by commutes times. For those with young children, however, we see that women send fewer applications to jobs that are far away, likewise applying more to jobs with shorter commute compared to men. It therefore suggests that mothers adjust their search by considering lower-paying jobs with shorter commute times. The results are qualitatively comparable to Le Barbanchon et al. (2019a) who find lower acceptable commute times for mothers at the expense of increased gaps in reservation wages. Similarly, we see comparable changes on the hours worked as the gender gaps in applications and hiring are increase after birth (see Figure A.19 in Appendix A.1.17).

### 3.5.3 Overconfidence and beliefs

As argued above, differential application behavior across gender may also reflect a response to expectations (rational and biased respectively) about the chance of being successful when sending an application. For the analysis in Figure 3.10 we have ranked applications relative to the job the applicant held before entering unemployment. We group applications based on whether they are directed to occupations which involve a higher, lower or similar pay than in the previous job.\(^\text{43}\) The figure reveals a clear difference across gender. Males are more likely to

\(^{43}\)These rankings are similar to the ranking on the X-axis in Figure 3.2 in Section 3.3 and use the average within occupation pay on the Danish labor market.
apply to occupations up the ladder whereas women are more likely to apply to occupations at a lower level (in terms of pay) than the job which they previously had. Importantly, the 10 percent gap in application rates for downward movements also translates to some degree into a 5 percent gap in hiring rates, thus directly affecting the allocation into jobs out of unemployment. On the other hand, the fact that men apply more to jobs up the occupation ladder does not seem to translate well into equivalent gender differences in the hiring rates on occupations.

**Figure 3.10**: Gender gaps in job applications and hiring, occupational ranks relative to prior job

![Gender gaps in job applications and hiring](image)

Note: Figure plots gender gaps in shares of applications going to jobs ranked relative to the individuals prior occupation and gaps where job-seekers are hired. All gaps are conditional on observable characteristics. The 95% confidence bars are based on standard errors clustered on the individual level.

In Appendix A.1.18 we provide more evidence on this mobility dimension and show that a similar patterns emerge if we consider industries or AKM firm fixed effects. However, when we consider typical wages instead, much of the male application behavior directed to higher typical wage deciles translates well into hiring outcomes.

The patterns we observe in Figure 3.10 and Appendix A.1.18 could reflect women’s perceptions over the likelihood of being hired in certain jobs. In this sense, women may send fewer applications to jobs in which they think chances of being hired are small, potentially driven by the expectation of statistical discrimination in higher-paying occupations or industries. In turn, this would reinforce the gender gaps. The literature on statistical discrimination has found clear differences across occupation in terms discriminatory behavior, such that women seem to face lower chances of being hired in in male-dominated occupations (e.g. Riach and Rich, 2006) as well as men in more female-dominated occupations (e.g. Booth and Leigh, 2010).

To examine how important this channel might be, we focus on the residual gender gap that remains after jointly controlling for observable characteristics and application behavior. In other words, the residual gender gaps in hiring outcomes that remain after the decomposition exercise. As we argue in Section 3.4, these residual gaps are a useful summary measure of whether women are more or less likely to be hired into a specific job type conditional on apply-
ing for it. Relating them to gender differences in application behavior tells us whether women systematically send fewer of their applications to those jobs where they face discrimination.

**Figure 3.11:** Relation between decomposition residuals and gender application gaps, occupations

![Graph showing the relation between decomposition residuals and gender application gaps, occupations.](image)

Note: Figure plots the relation between gender gaps in shares of applications and the decomposition residuals along 2-digit occupation groups. All gaps are conditional on observable characteristics. Decomposition residuals are the remaining gaps in hiring shares after jointly controlling for observables and application behavior.

In Figure 3.11 we examine this relation between decomposition residuals and applications behavior along 2-digit occupations groups. On the X-axis we proxy for the extent of gender based statistical discrimination by the size of the decomposition residuals in employment exists for each occupation and relate it to the female-male application gaps on the Y-axis. We see a small positive trend which tells us that the gender gaps in applications indeed seem to be positive for occupations where they have a greater chance of getting hired, i.e. women have a larger share of applications directed to these jobs than men. However, the extent to which women react to these differences in the likelihood of being hired conditional on applying is rather small and the trend seems partly driven by a few outlier occupations. Most of the gender gaps in applications along occupations are very close to zero.

We see a similar pattern in Appendix A.1.19 when considering 2-digit industries or firm wage level/typical wage percentiles. Over all of these job characteristics the correlations between decomposition residuals and the gender gaps in application behavior are only weakly positive. We thus conclude that the fact that women often apply to different jobs than men does not seem to be driven by the expectation of discrimination.

### 3.6 Conclusion

In this paper, we document differences in the types of jobs that men and women apply to during an unemployment spell and those they are eventually hired in. We exploit access to a novel administrative data set from an online platform, administered by the National Labor
Market Authorities, on which unemployed have to continuously register job applications as a part of their eligibility assessment for UI. We link these applications to occupations and firms and characterize each application along several important dimensions of job characteristics. Finally, we link the data to administrative registers, which enables us to add information about the UI recipients and their future jobs.

We analyze application behavior and hiring outcomes along a wide range of job specific characteristics and find that relative to men, women direct a larger share of their applications to characteristics associated with lower-paying jobs. These gender gaps in applications often mirror gender gaps in outcomes very closely and these patterns remain even after controlling for an expansive set of individual labor market observables. In consequence, the gender differences in application behavior add up to a sizable difference in the typical wages of the jobs women and men apply for and likewise in the job they are hired into.

To quantify what share of the observed gender gaps in job outcomes after unemployment can be explained by these differences in application behavior, we adopt a semi-parametric decomposition method that estimates the counterfactual job outcomes women would have if they had applied to the same jobs as men. We find that job applications are capable of explaining a substantial part of observed differences in job outcomes between men and women. After condition out individual labor market characteristics, gender differences in application behavior are able to explain about 30 percent of the post-unemployment gender segregation across industry and occupations. Similarly, we can attribute around 86 percent of the gap in typical wages to these differences as well as about 60 percent of the overall wage gap. Our results highlight that gender differences in the jobs men and women target during job search are an important driver shaping gender gaps in labor market outcomes.

In a last step, we provide suggestive evidence for potential reasons driving the differential application behavior. We show that, consistent with recent literature, women are more likely to consider jobs that would entail a shorter commute and shorter hours than those that men target. Recent parenthood exacerbates gender differences along most of our dimensions. Along with an overall tendency for women to target more family-friendly firms, proxied by the average length of maternal leaves per firm, this corresponds well to the recent evidence on the important role of motherhood for gender gaps. We also show that women are more likely to apply to firms that would represent a step down the occupational ladder. Finally, the differences in application behavior do not seem to be driven by the potential for self-fulfilling discrimination as gender gaps in application behavior are only weakly correlated with gender gaps in the probability of getting hired conditional on applying.

Acknowledgements

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3.7 References


3.7. REFERENCES


A.1 Appendix

A.1.1 Obtaining AKM firm fixed effects

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

In practice, we get a set of 290,108 firms connected by worker movements (5.5 percent of all firms are not connected to this set). We can therefore estimate the AKM firm fixed effects for 94.5 percent of the firms we observe in the labor market data between 2008 and 2015. After the estimation, we rank firms into deciles according to their firm fixed effect. As the model estimates imprecise effects for smaller firms and to guarantee equal size of the decile bins, we employment weight these rankings with the number of employees in each firm as of August 2015, the month before we observe applications in Joblog. Because the specific industries might reflect segmented markets, we calculate all ranks within each 1-digit industry group.

A.1.2 Constructing typical wages

To construct our measure of the typical wage paid in a job with certain characteristics, we use data on all the new jobs in our analysis sample. On this data, we perform a LASSO regression of the log actual wage paid in the job on the main job characteristics we consider in this paper. Specifically, we include industry and occupation dummies at both the 1-, 2- and 3-digit level and the within-industry-demeaned AKM firm fixed effect measure, along with all pair-wise interactions (10,568 variables in total). We rely on the Rigorous-LASSO of Belloni et al. (2012) to choose the regularization parameters. Because some individuals show up with several UI spells in our data, we allow for clustered disturbances at the individual level in estimation. The Rigorous Lasso selects 181 variables, which we then include in a standard log wage regression using OLS (Post-Lasso OLS). We use this regression to predict the log wage for each job outcome in our sample as well as for the 69 percent of job applications in our data that contain information on both occupation and firm. These predictions serve as our measure of the typical wage in the job.

To compute typical wages for job applications where occupation or firm information is unavailable, we repeat the above procedure once using data only on occupations and once using data only on industry and the within-industry-demeaned AKM firm fixed effect.

Experimenting with a range of other approaches to compute typical wages (including other variations of the LASSO or simple OLS regressions using a smaller, pre-specified set of variables) does not change any of our main results. We present results exclusively constructing typical wages from the sate of female (male) actual wages in Appendices A.1.11 and A.1.15.
A.1.3 Selecting our set of conditioning variables

To discipline which observable characteristics we condition on, we rely on the double LASSO of Belloni et al. (2014). This method selects the most important variables for explaining the gender wage gap. Using data on all the individuals in our analysis sample, along with the wages in their new jobs, the double-LASSO procedure involves an initial Rigorous-LASSO variable selection in a regression with log wage as the outcome variable and an additional Rigorous-LASSO variable selection with a gender dummy as the outcome variable. Combining the variables selected from these two regressions gives the set of most important variables for explaining the gender gap in wages. Belloni et al. (2014) provide additional discussion and formal results.

The baseline set of variables we select from in the two regressions contains the age, educational background, and detailed labor market history for the UI recipient. The education measure includes information on the highest attained education level as well as the education field. The labor market history contains work experience in hours since 1980, weeks worked in every 1-, 2- and 3-digit occupation/industry for the last five years before unemployment, 1-, 2- and 3-digit information on the job prior to unemployment, as well as weeks spent in unemployment, self-support and employment over the last five years. This gives us 4,196 variables in total in the baseline set. The double-LASSO selects 333 of these variables, which we use as our observable characteristics to condition on throughout the main analysis.

A.1.4 Propensity score reweighting for descriptive results

As discussed in Section 3.2.6, we use propensity score reweighting to condition out observable characteristics in all of our analysis. Using the notation introduced in Section 3.2.6, the reweighting scheme involves reweighting woman $i$ by a weight equal to $\frac{\hat{p}_i}{1-\hat{p}_i}$, where $\hat{p}_i$ is an estimate of the conditional probability of being male given observables, $P(m_i = 1|x_i)$.

Figure A.1: Male propensity distribution, trimmed

![Figure A.1: Male propensity distribution, trimmed](image)

Note: Figure plots the distribution of male propensity score estimates for men and women. We trim the distribution outside of the range $[0.01,0.99]$ to avoid extreme weights.
A.1.3), we follow the standard in the literature and estimate a logit model for the probability of being male, using the variables in $x_i$ as our explanatory variables. We then obtain the $\hat{p}_i$s as the predicted probabilities from this model and use these to reweight the women in our sample. In Figure A.1 we show the distribution of the estimated propensity scores in our sample.

A.1.5 Gender wage gaps in the employment population

Figure A.2: Gender wage gaps, jobs in employment population sample

Note: Figure plots gender gaps in wages and typical wages for a 10% sample of all regular employees in Denmark in August 2015. Typical wages for the employment sample are re-estimated using the set of variables already selected with the Rigorous-LASSO on the unemployed in Appendix A.1.2.

A.1.6 Decomposition additional details

Below we show how propensity score reweighting can be used to construct estimates of the counterfactual hiring probabilities underlying our decomposition exercise. We start by considering the counterfactual hiring probability for women conditional on observables:

$$\tilde{P}_W^X(y) = \int \int p_W^W(y|a, x) f_{a|x}^W(a|x) f_M^M(x) da \, dx$$

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

$$\tilde{P}_X^W(y) = \int \int p_W^W(y|a, x) f_{a|x}^W(a|x) f_{x}^W(x) \Psi_x(x) da \, dx$$

Here we have defined $\Psi_x(x) = \frac{f_M^M(x)}{f_{x}^W(x)}$. The first key insight is that the quantity of the right is the weighted expectation of $p_W^W(y|a, x)$ over the set of all women weighted by $\Psi_x(x)$:

$$\tilde{P}_X^W(y) = E \left[ \Psi_x(x) p_W^W(y|a, x) | m = 0 \right]$$

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

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

We use this approach to estimate the counterfactual hiring probability $\tilde{P}_X^W(y)$. We estimate a logit model for the likelihood of being male as a function of our observable characteristics $x$ and then use the predicted probabilities from this model to reweight the women in our sample before computing hiring probabilities. This equivalent to the to the propensity score reweighting we use to condition out observable differences between men and women, as introduced in Section 3.2.6.

Our estimation of the counterfactual hiring probability that also conditions on applications follows a similar approach. We start from the expression given in the main text:

$$ \tilde{P}_{A,X}^W(y) = \int \int p_W^W(y|a,x) f_{a,x}^M(a|x) f_X^M(x) \, da \, dx $$

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

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

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

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

We use this approach to estimate the counterfactual hiring probability $\tilde{P}_X^W(y)$. We estimate a logit model for the likelihood of being male as a function of our observable characteristics $x$ and application behavior and then use the predicted probabilities from this model to reweight the women in our sample before computing hiring probabilities.

---

44To see this more clearly let $I(y)$ be an indicator for ending ones UI spell by being hired into job type $y$ and note that we have:

$$ \tilde{P}_X^W(y) = E \left[ \Psi_X(x)p_W^W(y|a,x)|m = 0 \right] = E \left[ \Psi_X(x)E \left[ I(y|a,x) | m = 0 \right] \right] $$

$$ = E \left[ E \left[ \Psi_X(x)I(y|a,x) | m = 0 \right] \right] = E \left[ \Psi_X(x)I(y|m = 0) \right] $$

The direct empirical counterpart of the last expectation is then the share of women hired into job type $y$ after applying the $\Psi_X(x)$ weights: $\frac{1}{N_W} \sum_{i=0} I(y_i) \Psi_X(x_i)$ (here subscript $i$ refers to individuals in the data and $N_W$ is the total number of women in the data).
A.1.7 Family friendliness and maternal leave

In order to get a rough proxy of the firm’s family friendliness, we use the average length of maternal leave per birth in each company. Danes are entitled to 52 weeks of paid parental leave that can be extended by further 8 weeks at the cost of lower benefits in the period before. The data we use to calculate this measure is obtained from the ILME and BFL registers and covers parental leave payments from the government or from employers between 2011 and 2017. We measure the length as the number of weeks with paid-out parental leave benefits after each birth for those that eventually return to the same pre-birth employer. We only use births and parental leave for women as Danish men only take about 11 percent of the total leave per birth, by far the lowest share in Scandinavia (Nordic Council of Ministers, 2018). Unfortunately, our data does not come with readily available information on birthdays for children. However, we can infer these from starting a new maternal leave spell which corresponds well with the official birth statistics. As we show in Figure A.3, the length of maternal leave spells shows some wide variation, with the majority lasting less than 200 days.

Figure A.3: Maternal leave: Distribution of average length

Note: Figure plots the distribution of the average length of maternal leave for women in days.

\[\text{Note that some women may transition from one leave to another, thus we censor the length of leaves at 14 months and regard any subsequent leave as a new birth/spell. This is however very limited in our data.}\]
### A.1.8 Overview of UI fund requirements

**Table A.1:** Reference logging requirement by UI fund

<table>
<thead>
<tr>
<th>UI Fund</th>
<th>Requirement</th>
<th>Weekly Logs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Akademikernes A-kasse</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>A-kassen Ase</td>
<td>Yes</td>
<td>0.5</td>
</tr>
<tr>
<td>A-kassen Frie</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>A-kassen LH</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>A-kassenfor Journalistik, Kommunikation og Sprog</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>Børne- og Ungdomspædagogernes Landsdækkende Akasse</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>Byggefagenes Akasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>CA A-kasse</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>DANA Akasse for Selvstændige</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Det Faglige Hus - A-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Din Sundhedsfaglige A-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>El-fagets Akasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Faglig Fælles A-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>FOAs A-kasse</td>
<td>Yes</td>
<td>1.5</td>
</tr>
<tr>
<td>Fødeværeforbundet NNFs Akasse</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>Funktionærernes og Tjenestemændenes Fælles-Akasse</td>
<td>Yes</td>
<td>2</td>
</tr>
<tr>
<td>HK/Danmarks A-kasse</td>
<td>Yes</td>
<td>1</td>
</tr>
<tr>
<td>Kristelig A-kasse</td>
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</tr>
<tr>
<td>Lærernes a-kasse</td>
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</tr>
<tr>
<td>Magistrenes Akasse</td>
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<tr>
<td>Metalarbejdernes Akasse</td>
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<td></td>
</tr>
<tr>
<td>Min a-kasse</td>
<td>No</td>
<td></td>
</tr>
<tr>
<td>Socialpædagogernes Landsdækkende Akasse</td>
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<td>1</td>
</tr>
<tr>
<td>Teknikernes Akasse</td>
<td>Yes</td>
<td>1</td>
</tr>
</tbody>
</table>

**Notes:** The table shows the minimum logging requirement for all Danish UI funds. Note that the minimum requirements are only reference values as individual requirements are set in caseworker meetings. This applies similarly to UI funds without a minimum requirement.
A.1.9 Further documentation of application behavior

Figure A.4: Survival rates and logged applications

(a) Kaplan-Meier survival curve until employment exit

(b) Average number of logged applications

Note: Figure plots gender Kaplan-Meier survival rate estimates over the first year in unemployment for the unrestricted sample (left) and the average number of logged applications by gender (right).

Figure A.5: Distribution of logs per week

(a) Inflow sample

(b) Analysis sample

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

Table A.2: Survey question "Which of these statements best describes your use of joblog?"

<table>
<thead>
<tr>
<th></th>
<th>Female</th>
<th>Male</th>
<th>Diff.</th>
</tr>
</thead>
<tbody>
<tr>
<td>N</td>
<td>721</td>
<td>515</td>
<td>206</td>
</tr>
</tbody>
</table>

**Logging behaviour**
- Fulfill req., often applied to more jobs: 32% Female, 42% Male (-9%)
- Fulfill req., rarely applied to more jobs: 23% Female, 19% Male (4%)
- All applied jobs: 44% Female, 38% Male (6%)
- Never: 1% Female, 2% Male (-1%)

Notes: The table shows the logging behavior of individuals surveyed during a pilot for a randomized control experiment commenced on the 5th of March 2018. For more information, see pre-trial information at Mahlstedt et al. (2018).

Table A.3: Self-reported and registered applications in the previous month

<table>
<thead>
<tr>
<th></th>
<th>Female</th>
<th>Male</th>
<th>Diff.</th>
</tr>
</thead>
</table>

**Survey**
- # of applied jobs: 11.7 Female, 11.2 Male (0.5)
- # of applied jobs not registered: 2.1 Female, 2.7 Male (-0.6)

**Joblog**
- # of registered joblogs: 7.9 Female, 8.1 Male (-0.2)

**Difference**
- 1.7 Female, 0.4 Male (1.3)

Notes: The table shows the logging behavior of individuals surveyed during a pilot for a randomized control experiment commenced on the 5th of March 2018 and relates it to the observed number of these individuals applications in joblog. For more information, see pre-trial information at Mahlstedt et al. (2018).
A.1.10 Raw gender gaps in application and hiring outcomes

In this section, we reproduce the results of section 3.3 for our sample unconditional on observable characteristics. Figure A.6 illustrates that, while the female-male gaps indeed appear to be magnified for the unconditional sample, the overall patterns show that women, compared to men, send more of their applications to job types that typically pay a low wage, qualitatively similar to our main conclusions. They also send less of their applications to the highest paying firms and occupations. Similar to our main results, we likewise see that gaps in the share of applications and the share of hires covary closely. Thus, our results are robust in the unconditional sample regarding our main conclusion, yet conditioning on observables quantitatively matters for the size of the observed female-male gaps.

Figure A.6: Gender gaps in matching and applications, raw

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. All gaps are raw, not accounting for observable differences. The 95% confidence bars are based on standard errors clustered on the individual level.
A.1.11 Alternative wage measures - Application and hiring

For the investigation of female-male gaps in the typical wages that men and women apply to and are hired in, i.e. Section 3.3.3, we constructed the measure of typical wages based on our full sample of new jobs (see Appendix A.1.2). A natural question that arises is whether men and women indeed face equal typical wages for a given job type, or whether there are gender differences in the typical wage that men and women are being paid. To further investigate whether gender differentials in typical wages can be a cause of female-male gaps in applications, we compute typical wages measures that are exclusively based on the actual wages either men or women in our sample get paid after entering employment. Based on the observable job characteristics of the applications and new jobs in our sample, we thus construct the typical wage that females get paid. Similarly, we also assign a typical wage that males would get paid in a given job type.

In Figure A.7 it can be seen how the female-male gaps over the typical wage distribution are qualitatively unchanged for these alternative measures of typical wages compared to our main results, though gaps between the 3rd and 8th decile are somewhat smaller and often insignificant for female typical wages.

**Figure A.7:** Gender gaps in job applications and hiring, alternative wage measures

(a) Typical wages (women only)

(b) Typical wages (men only)

Note: Figure plots gender gaps in shares of applications going to specific typical wage deciles and gaps in which decile job-seekers are hired. Typical wage deciles are computed on women only (left) and men only (right). The 95% confidence bars are based on standard errors clustered on the individual level.
A.1.12 Wage growth measures

In Section 3.2.7, we have seen that there are distinct differences in the typical wages that men and women apply to. These differences largely correspond to differences in the eventual hiring outcome, leading to the observed gender wage gaps in typical wages. By construction, the wage gaps are only a momentary snapshot at entry into the new position and thus may mask potential differences in the wage growth rates that come with seniority on the job. If there are substantial differences in how fast or how strong wages increase, the gender wage gap for this sample of employment entries may change over time. To get a sense on whether this is important, we examine the gender gaps in applications and hiring along wage growth rate deciles after one- and five years. We calculate these wage growth rates as the average relative increase in paid wages for each firm between the wage we observe at employment entry, for all jobs starting between 2008 and 2016, and the wage paid after one or five years, respectively. In order to separate general time trends from the growth rates as well as structural differences between industries, we control for year by industry fixed effects. As many individuals will have left their jobs by the one and especially the five year mark, we censor individuals that are not anymore employed by these firms at these times.

In Figure A.8 we see that women are more likely to apply to firms with lower wage growth rates over both periods, with the exception of the lowest decile. Likewise, men apply substantially more to those firms in the 9th and 10th decile. The share of hires corresponds closely to the application shares, thus we conclude that the gender wage gaps we observe are likely to increase over time.

Figure A.8: Gender gaps in job applications and hiring, wage growth

(a) Wage growth (1 year)  
(b) Wage growth (5 year)

Note: Figure plots gender gaps in shares of applications going to specific wage growth deciles and gaps in which decile job-seekers are hired. Wage deciles are computed as the relative difference between the starting wage with the wage one year (left) or five years (right) after entering the respective job. The 95% confidence bars are based on standard errors clustered on the individual level.
A.1.13 Robustness tests

In this section, we present robustness tests relating to the main results presented in section 3.3. In order to get as close as possible to actual search behavior, we imposed several restrictions on our main sample as laid out in Section 3.2.4. Naturally, and to make sure our results are not driven by the selection of a specific subsample of unemployment spells, we test whether our results hold when relaxing these restrictions below.

Figures A.9 to A.13 replicate the female-male gaps in average application and hiring shares for a sample were we do not sequentially restrict our sample to the selections that were used for our main analysis (see Section 3.1). Figure A.9 is based on a sample were we do not exclude the last 4 weeks of applications, whereas in Figure A.10 we focus on unemployment spells that find a job within 26 weeks (in contrast to the 52 week requirement used above). In Figure A.11 we relax our sample restriction of only including unemployment spells that have at least logged 4 applications during the respective unemployment spell, by selecting all spells that have at least registered one application instead. Figure A.12 relaxes the restriction on the minimum unemployment spell length of 8 week to a consideration of all unemployment spells we observe in the sample period. Figure A.13 replicates the results for a sample that is not restricted to end in a new hire. Here we treat unemployment as a separate category to the hiring outcomes. Common to all of our robustness tests on the sample selection criteria is that the results do not change qualitatively. In fact, female-male gaps are remarkably stable across the different samples.

Aside from our sample selection criteria, we can study further potential versions of our analysis. In Figure A.14, for example, we examine the gender gaps in application and hiring shares when conditioning on having placed at least one application to the respective occupation/industry group or firm fixed effect/typical wage decile. The qualitative patterns to not change in this settings, yet standard errors are particularly wide, so many of the observed gaps are not statistically significant. Figure A.15 displays the results for our main sample while additionally including quarterly dummies in the weighting exercise of Section 3.2.6. The purpose of this is to control for seasonality, i.e. whether entering the sample at different times is important for the differential in application behavior and hiring outcomes. Again the female-male gaps are rather stable when explicitly controlling for seasonality differences in the entrance quarter.

Rather than studying the share of applications sent to a specific job type over the spell, Figure A.16 illustrates the absolute number of applications sent. While the female-male-gaps quite naturally change due to a different scale, the overall patterns of women applying to lower paying job types in terms of occupations, industries, AKM firm fixed effects and typical wages are evident. Further, gaps in the number of applications sent and the hiring shares likewise covary closely similar to our main results. Hence, the overall patterns of our data is preserved if we assess an absolute measure of application rather than the share of applications sent.

Note that logging at least one application is necessary to appear in the Joblog data delivery we received.

Nevertheless, these unemployment spells need to be at minimum 4 weeks long in order for us to properly identify them.
**Figure A.9**: Gender gaps in matching and applications, no exclusion of last 4 weeks

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wage

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we relax the restriction on the last 4 weeks of unemployment and consider all applications. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.10: Gender gaps in matching and applications, 26 week sample

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wage

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we only consider spells lasting at most 26 weeks. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.11: Gender gaps in matching and applications, only 1 application logging requirement

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we relax the requirement of having min. 4 logged applications to at least 1 application. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.12: Gender gaps in matching and applications, no spell length requirement

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wage

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we relax the restriction on min. 8 weeks spell length and consider all spells. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.13: Gender gaps in matching and applications, no employment requirement

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wage

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we relax the restriction on employment and also consider spells that do not end in employment. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.14: Gender gaps in matching and applications, conditional

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wage

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we restrict each application/hiring bar to those individuals having placed at least one application to the specific job category. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.15: Gender gaps in matching and applications, seasonality controls

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wage

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we control for quarter of inflow into unemployment to take out seasonality. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.16: Gender gaps in matching and applications, absolute measure for applications

(a) Occupations

(b) Industries

(c) Firm wage level

(d) Typical wage

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we consider absolute measures of applications. The 95% confidence bars are based on standard errors clustered on the individual level.
A.1.14 Decomposition - Raw

In this section, we reproduce the results of section 3.4 for our sample unconditional on observable characteristics. In Table A.4 we present the decomposition results, similar to Table 3.3 in Section 3.4. In general, we observe larger gender gaps as controlling for observables reduces many of these differences among men and women in hiring (see Appendix A.1.10). In consequence, we see that application behavior, without jointly conditioning on observables, accounts for slightly more of the typical and actual wage gap than in Table 3.3. We further see in Table A.4 that the gender segregation across occupations and industries is substantially larger when not accounting for observables. After reweighting the sample this reduces a substantially larger fraction of the segregation than in Table 3.3. However, this is not necessarily surprising as we do not account for the differential labor market history between men and women here, which, when doing so, reduces a large part of the segregation.

Table A.4: Decomposing gender gap, raw sample

<table>
<thead>
<tr>
<th></th>
<th>Baseline</th>
<th>Explained by applications</th>
<th>Residual</th>
</tr>
</thead>
<tbody>
<tr>
<td>Occupational segregation</td>
<td>0.235</td>
<td>0.177</td>
<td>0.058</td>
</tr>
<tr>
<td>(Duncan index, 1-digit)</td>
<td>(0.007)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td></td>
<td>[0.75]</td>
<td>[0.25]</td>
<td></td>
</tr>
<tr>
<td>Industry segregation</td>
<td>0.144</td>
<td>0.114</td>
<td>0.030</td>
</tr>
<tr>
<td>(Duncan index, 1-digit)</td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
<tr>
<td></td>
<td>[0.79]</td>
<td>[0.21]</td>
<td></td>
</tr>
<tr>
<td>Firm wage level</td>
<td>0.016</td>
<td>0.013</td>
<td>0.003</td>
</tr>
<tr>
<td>(Male-female gap, AKM fixed effect)</td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td></td>
<td>[0.80]</td>
<td>[0.20]</td>
<td></td>
</tr>
<tr>
<td>Typical wage for job</td>
<td>0.041</td>
<td>0.038</td>
<td>0.003</td>
</tr>
<tr>
<td>(Male-female gap, log typical wage)</td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td></td>
<td>[0.92]</td>
<td>[0.08]</td>
<td></td>
</tr>
<tr>
<td>Actual Wages</td>
<td>0.070</td>
<td>0.052</td>
<td>0.017</td>
</tr>
<tr>
<td>(Male-female gap, log wage)</td>
<td>(0.002)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td></td>
<td>[0.75]</td>
<td>[0.25]</td>
<td></td>
</tr>
</tbody>
</table>

Notes: The table displays decomposition results after accounting for application behavior for the raw sample. All results are displayed with bootstrapped standard errors in parenthesis (2,000 replications). Share of contribution to the baseline gap in brackets. The second column shows the parts explained by application behavior, the third column displays the residual after controlling for applications behavior. DFL re-weighting uses logit specifications where \( a_i \) includes the share of applications sent to each 2-digit occupation, each 2-digit industry, each deciles of the firm fixed effect distribution and each decile of the typical wage distribution.
A.1.15 Decomposition - Alternative wage measures

Based on the alternative wage measure from Appendix A.1.11, we re-run our decomposition exercise on the wages that women and those that men would typically get paid in the jobs they apply for. Table A.5 displays the outcome of this exercise. We see that the share of the gender wage gap that can be attributed to differential search behavior does not change a lot when considering the typical wages of men, yet it increases slightly by 4 percent. We may interpret this as evidence that the benchmark decomposition exercise in Section 3.4 allocates more women to jobs that typically employ few women. The typical wages we compute for these jobs are then mainly driven by higher earning men. However, we see considerable increases in these share of the gender wage gap attributable to differences in application behavior along women's typical wages on which application behavior explains almost all of the gender differences.

Table A.5: Decomposing gender gap, alternative wage measures

<table>
<thead>
<tr>
<th></th>
<th>Baseline</th>
<th>Explained by applications</th>
<th>Residual</th>
</tr>
</thead>
<tbody>
<tr>
<td>Female typical wage</td>
<td>0.018</td>
<td>0.018</td>
<td>0.001</td>
</tr>
<tr>
<td>for job</td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>(Male-female gap, log typical wage)</td>
<td>[0.96]</td>
<td>[0.04]</td>
<td></td>
</tr>
<tr>
<td>Male typical wage</td>
<td>0.024</td>
<td>0.021</td>
<td>0.002</td>
</tr>
<tr>
<td>for job</td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>(Male-female gap, log typical wage)</td>
<td>[0.90]</td>
<td>[0.10]</td>
<td></td>
</tr>
</tbody>
</table>

Notes: The table displays decomposition results after accounting for application behavior. All results account for observable differences between men and women and are displayed with bootstrapped standard errors in parenthesis (2,000 replications). Share of contribution to the baseline gap in brackets. The second column shows the parts explained by application behavior, the third column displays the residual after controlling for applications behavior. DFL re-weighting uses logit specifications where \( a_i \) includes the share of applications sent to each 2-digit occupation, each 2-digit industry, each deciles of the firm fixed effect distribution and each decile of the typical wage distribution (female/male typical wages separately).
A.1.16 Search channels and application methods

For most of this paper we focus on differences in application behavior in terms of the characteristics of the jobs that men and women apply to. However, differences in application behavior may also occur along other dimensions, such as the specific search channels or the methods used to place the applications. If these differ vastly between men and women we might lack an important explanation for differences in observed job outcomes. In Figure A.17 we thus examine whether there are significant differences in how men and women apply for jobs.

In panel (a) we see that gender differences in the used search channels are generally small, yet men are slightly more likely to use informal channels, such as unsolicited applications and network contacts. Women on the other hand apply slightly more to jobs that are formally posted vacancies. In panel (b) we still rather small differences that correspond well to panel (a). Men are more likely to apply by directly contacting employers by phone, corresponding well to informal channels, whereas women send more applications through web-forms or letters/email. Overall, the gender differences are small and thus likely not driving our results.

**Figure A.17:** Gender gaps in job applications and hiring, search channels and application methods

Note: Figure plots gender gaps in shares of applications channels/methods used. The 95% confidence bars are based on standard errors clustered on the individual level.
A.1.17 Motherhood - Details

This section explores additional job characteristics for the subsamples of young workers with and without children (see Section 3.5.2). Figure A.18 illustrates how the gap in average share of applications sent to the lowest paying occupation, service and sales, is larger between men and women with children compared to the those with no children. Furthermore, there is a clear pattern of women with children to send a lower share of their applications and being less likely to get hired into all occupations above clerical work, relative to men. While this pattern is also present among men and women with no children, the differences are greater in magnitude for the former.

When considering the different sectors the applications are directed to, we see a reversal of the gender gap over the manufacturing sector which results in a substantial increase in the hiring gap. Likewise, we see a reduction (and partly a reversal) of the gender gap in the public and culture sectors. The clear patterns of women applying more to job types and firms that are associated with lower wage levels (in terms of AKM firm fixed effects) compared to men is present both among workers with and without children. However, the gender differences are indeed more exacerbated between men and women with children.

Regarding commute times, there are no significant gender differences between men and women without children in Figure A.19, yet we see that women with children send fewer of their applications to jobs that are far away, whereas applications to jobs with shorter a commute increase relative to men with children. Further, we see comparable changes on the hours worked as the gender gaps in applications and hiring are substantially increased after birth. We also see slight increases in the number of applications and hires for women at more family-friendly firms. Overall, these findings are in line with the recent evidence on the effect of motherhood in gender differences and the preference for more family-friendly workplaces.
Figure A.18: Gender gaps in matching and applications, parenthood

(a) Occupations: No children
(b) Occupations: Child below 5

(c) Industries: No children
(d) Industries: Child below 5

(e) Firm wage level: No children
(f) Firm wage level: Child below 5

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we compare individuals living with no child to those with a child below the age of 5 at home. The 95% confidence bars are based on standard errors clustered on the individual level.
Figure A.19: Gender gaps in matching and applications, parenthood

(a) Commute times: No children

(b) Commute times: Child below 5

(c) Hours: No children

(d) Hours: Child below 5

(e) Avg. maternal leave length: No children

(f) Avg. maternal leave length: Child below 5

Note: Figure plots gender gaps in shares of applications going to specific jobs and gaps in where job-seekers are hired. Here we compare individuals living with no child to those with a child below the age of 5 at home. The 95% confidence bars are based on standard errors clustered on the individual level.
A.1.18 Additional mobility measures

In this section we add to the mobility measures presented in Section 3.5.3. In Figure A.20 we find that women display relatively more downward movements in their application behavior compared to men, both in terms of the industry hierarchy relative to their previous job, but also in terms of the firm attractiveness in terms of pay. Thus, women, to a greater extent than men, revise which industries and firms they consider, and systematically target industries and firms that are associated with lower wages compared to the industry and firm of their previous job. On the contrary men are more optimistic in their search behavior as they sent more applications to industries and firms that are associated with higher wages compared to their previous job. Yet, to a lesser degree does this translate into realized upward movements in the hiring shares, which indicate that also in terms of the industries and firms that they consider, men display some potential overconfidence in their application behavior.

Figure A.20: Gender gaps in job applications and hiring, industry and firm wage level ranks relative to prior job

Note: Figure plots gender gaps in shares of applications going to jobs ranked relative to the individuals prior industry or firm wage level decile and gaps in where job-seekers are hired. All gaps are conditional on observable characteristics. The 95% confidence bars are based on standard errors clustered on the individual level.

Finally, in Figure A.21 we see similar patterns when considering the typical wage that men and women apply to. Women are 6 percent more likely to apply to jobs that are in a lower typical wage decile relative to their previous position, while men send 7 percent more of their applications to jobs in a higher typical wage decile. Interestingly, the hiring gaps are considerably closer to the application gaps than when considering occupations or industries.
**Figure A.21:** Gender gaps in job applications and hiring, typical wage ranks relative to prior job

Note: Figure plots gender gaps in shares of applications going to jobs ranked relative to the individuals prior typical wage decile and gaps in which decile job-seekers are hired. All gaps are conditional on observable characteristics. The 95% confidence bars are based on standard errors clustered on the individual level.

A.1.19 Gender application gaps and decomposition residuals

In this section we add to Section 3.5.3 by considering the relation between the decomposition residuals and gender gaps in application behavior along additional characteristics of the job. In Section 3.5.3 we saw a rather modest relationship between the two for occupations and as it turns out in Figure A.22, similar conclusions can be drawn when considering 2-digit industries and the percentiles of the overall firm wage level. The weak positive relationship may simply be a result of outlier industries as the vast majority of gaps are centered closely around 0.

This pattern is also strongly evident in Figure A.23 where we consider the percentiles of the typical wage distribution. Here we see no actual positive trend. In conjunction with the results in Section 3.5.3 we thus conclude that the fact that women often apply to different jobs than men does not seem to be driven by the expectation of discrimination.
Figure A.22: Relation between decomposition residuals and gender application gaps

(a) Industries

(b) Firm wage level

Note: Figure plots the relation between gender gaps in shares of applications and the decomposition residuals along 2-digit industries (left) and firm wage level percentiles. All gaps are conditional on observable characteristics. Decomposition residuals are the remaining gaps in hiring shares after jointly controlling for observables and application behavior.

Figure A.23: Relation between decomposition residuals and gender application gaps, typical wage

Note: Figure plots the relation between gender gaps in shares of applications and the decomposition residuals along typical wage percentiles. All gaps are conditional on observable characteristics. Decomposition residuals are the remaining gaps in hiring shares after jointly controlling for observables and application behavior.
Abstact

In this randomized control trial, we test whether an SMS- and video-based intervention for long-term unemployed can enhance participation in the labor market. Most targeted individuals face strong personal barriers to job search and regular employment. Prior evidence shows that a lack of perceived self-efficacy is a good predictor of job-search intentions, behavior, and the future employment status. This holds especially for vulnerable groups similar to the target group of this intervention. The treatment aims to boost the individual’s self-efficacy to increase the beliefs in the possibility to work for a few hours per week. We distribute videos of successful citizens that were challenged by similar barriers as well as targeted job search guidance videos through SMS. Due to a substantial lack of statistical power, we can make no clear conclusions on the effects of this intervention.

Keywords: job search, information provision, unemployment, randomized control trial

JEL Codes: C93, D83, J22, J68
4.1 Introduction

Most long-term welfare recipients in Denmark are classified as not immediately ready to perform in full-time regular employment (activation-ready). While this acknowledges their situation under prolonged exclusion from the labor market, public employment services and labor market authorities encourage marginal and part-time employment. Despite lacking concrete evidence on their effects on long-term attachment to the labor market, these modest employment experiences have become a prominent target in getting long-term unemployed into regular jobs. Yet, many of these individuals face strong personal barriers that prevent active engagement in job search activities (Danneris, 2016). These factors act as an added obstacle on top of other factors that come with long-term exclusion from the labor market.

In the Danish context, Rosholm et al. (2017a) examine the progression of activation-ready long-term unemployed over several years. Five strong markers have a significant connection to active job search and job search success. These predictive indicators relate to common barriers to employment for long-term welfare recipients (Singley, 2003) and any of these markers might be strongly associated with individual self-efficacy (Bandura, 1977). A vast psychological literature examines the importance of the concept of self-efficacy on job search and success in the labor market. Many studies have linked perceived self-efficacy to job search intentions and outcomes, especially for vulnerable and disadvantaged groups (e.g. Zenger et al., 2013; Moynihan et al., 2003; van Hooft, 2014; Andersson, 2015; Wanberg et al., 1999, 2005). Further, there has been evidence that an enhancement of self-efficacy can increase the probability of engaging in job search (Eden and Aviram, 1993). In a meta-analysis, Liu et al. (2014) study a large amount of (quasi-) experimental job search interventions. They find that interventions aimed at boosting self-efficacy and teaching job search skills are most effective in terms of job search success. The effects are the strongest for interventions that target individuals with special needs and conditions.

Despite the documented importance of the self-efficacy concept, most measures taken by public employment agencies focus on education and skill enhancement. These programs are less effective in raising self-efficacy, job-orientation and the likelihood of getting any employment than more work-oriented activation measures (Arendt et al., 2017).

We thus propose a low-cost intervention that aims to increase individuals’ belief of being able to

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1 Individuals that are considered to be ready for full-time employment are classified as job-ready. They are required to work in certain measures determined by the job center in order to receive welfare benefits (internships, wage subsidies or public work programs). Individuals not ready for full-time work are classified as activation-ready and are subject to extra support. These individuals are not required to work in order to receive welfare benefits.

2 For example, Edin and Gustavsson (2008) find that a year of unemployment is associated with a move of 5-percentile points down the general skill distribution. Further, self-reported health measures are declining over the course of an unemployment spell, with a significant increase in mental health symptoms (Burgard et al., 2007). Long periods of joblessness may also affect children and their emotional well-being (McLoyd et al., 1994) as well as probability to commit violent crime (Nordin and Almén, 2017).

3 The perception about how good one self is in working together with others, the capacity to cope with everyday life, the perceived mental and physical health, the perceived readiness to work as well as the knowledge about job search strategies to increase the chances of obtaining employment.

4 Bandura defines self-efficacy as ones’ belief and confidence in the possibility to be able to to achieve desired outcomes. This is related to the locus-of-control concept, yet not fully similar (Ajzen, 2006)

5 Through direct engagement in work activities such as internships, wage subsidies or public employment, the individuals might learn that it is actually possible to work in (at least) marginal employment.
work. We focus on promoting marginal employment with less than 10 ordinary hours of work per week, as the target group of this intervention is not immediately ready for full-time employment.

We use an easy to scale design by sending out SMS text messages in combination with targeted audio-visual job search guidance and positive testimonials in the form of videos. Text message interventions have for example seen successful use in medical applications promoting healthy behaviors (e.g. Patrick et al., 2009; Rami et al., 2006; Strandbygaard et al., 2010). Further, even simple text messages have had success in changing individuals’ beliefs about the ability to cope with certain issues (Petrie et al., 2012; Franklin et al., 2006). We choose the testimonials to relate to the concept of vicarious learning to enhance perceived self-efficacy through indirect learning about others’ success in comparable situations (Bandura, 1977). Using positive testimonials can help in overcoming personal barriers (Pruitt et al., 2012) and reducing negative expectations (Ditre et al., 2010). All of the testimonials relate to the target groups’ common barriers to job search and the audio-visual approach may be more efficient than distributing information only in written form (Braverman, 2008). To test the effectiveness of the intervention, we use a randomized control trial with a treatment and a control group.

Our intervention relates to the literature on information provision for unemployed and other actors on the labor market, which has seen an increased interest in recent years. For instance, Altmann et al. (2018) examine the impact of an informational brochure, distributed via postal mail, for newly unemployed. While the effects are small and insignificant on the overall sample, the impact on employment and earnings is significant and positive for individuals at risk for long-term employment. A related approach by Liebman and Luttmer (2015) tests an information brochure about key social security provisions for workers nearing retirement. These brochures include short written testimonials of actual retirees. Despite the low-cost nature of this treatment, it raises the labor force participation rate by four percentage points. Van den Berg et al. (2017) find an increased take-up of a targeted wage-support program for displaced workers aged 55-59 after receiving an informational brochure. To the best of our knowledge, we are the first to test an audio-visual and text message intervention among individuals on the edge of the labor market.

Due to issues in recruiting a sufficient sample of long-term unemployed to our intervention, we are unfortunately lacking enough statistical power to identify potential effects. Over the course of the implementation period, we invited individuals in our target group to take part in the intervention. To do this, caseworkers distributed information and sign up forms during the regular meetings at the responsible municipal job centers. A post-intervention survey among the caseworkers involved in the project revealed a considerable lack of time to introduce the intervention to potential participants. A significant share of both caseworkers and clients also seem to not see the package benefiting our target group enough.

We structure the paper as follows. Section 2 introduces the experimental setup, including sample recruitment. Section 3 lays out the empirical strategy and details the data sources used. Section 4 presents the results before Section 5 identifies some issues that may have played a role in the low intervention sign-up. Finally, we conclude in Section 6.
4.2 Experimental Design

To test the effect of an SMS/video-nudge in combination with information provision and positive testimonials, we conducted a randomized control trial on the Danish labor market. The experiment ran between August 2018 and February 2019 and targeted long-term unemployed social welfare recipients. These individuals are not considered immediately ready for full-time employment but are nevertheless encouraged to work for a few hours per week in marginal or casual jobs. The main content of this intervention are short videos distributed via SMS text messages. The video clips, embedded on a supplementary webpage, either contain motivational information, job search guidance or testimonials of former social welfare recipients that successfully found marginal employment. We additionally use a survey to elicit the barriers to job search faced by the individuals in our intervention.

The following section first lays out the initial recruitment of intervention participants and follows with an overview of the experimental design of the RCT, introducing the use of SMS, videos and the survey. After this, we will explain the contents of videos and SMS in more detail.

4.2.1 Sample and Recruitment

The target group of the intervention is the group officially classified as aktivitetsparate kontanthjælpsmodtagere, which roughly translates to activation-ready recipients of social welfare. These individuals receive social welfare benefits between 11,282 - 14,993 DKK per month if 30 years or older and slightly lower benefits if younger. In contrast to other groups subject to similar transfers, the recipients do not need to search for employment actively as they are not considered being immediately ready for full-time work. Individuals in this group are, besides their usual long-term unemployment status, often challenged by substantial personal problems, such as physical and mental illnesses, a lack of social skills or substance abuse (Danneris, 2016). Rosholm et al. (2017a) identify the importance of these barriers for job search intentions and success. These barriers are nevertheless not considered severe enough to qualify for early retirement or disability benefits. Mandatory activation measures, such as training and qualification courses or less often ordinary education and supported work programs, facilitate an eventual return to the labor market.

Prior to implementation, we recruited four public job centers across Denmark to take part in the intervention. We added three additional job centers shortly after the intervention started. All locations are indicated in Figure 4.1. In contrast to related experiments, which have access to information on individual addresses and use these to provide brochures via postal mail (e.g. Altmann et al., 2018), we sign up each participant individually to the experiment. The main reason for this is the legal requirement of getting informed consent from participants to contact them via SMS text messages. Participants were recruited from the stock of clients in the target group during caseworker meetings at the municipal job centers. Meetings are typically scheduled once every quarter for each client and thus occur at individual times for each prospective participant (they might occur more often where necessary). The caseworkers were instructed to devote a few minutes of the meeting to introduce each of their clients in the

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6 Roughly 1,700 - 2,300 USD, depending on household status.
7 Aarhus, Norddjurs, Middelfart and Esbjerg were recruited prior to the intervention, whereas Randers, Haderslev and Odense were added shortly after the start.
target group to the intervention and, if the client is interested, to provide more information on the procedure as well as to assist in signing up. In practice however, many individuals were not introduced to the intervention and thus were unaware of the treatment and unable to sign up (see Section 4.5).

The participation in the intervention required signing a consent form and stating a mobile phone number along with the individual’s Danish personal identification code.\(^8\) We asked for these details on the informed consent form, which caseworkers hand out to each prospective participant.\(^9\) Along with the consent form (reproduced in the appendix), participants received additional details on the treatment and a rough overview of its content and motivation. Individuals had the possibility to reach out to us for questions or feedback, though no one used this option.

We made the consent forms available to the participating job centers along with detailed instructions for the caseworkers on how to present the intervention to prospective participants. We further included a list of answers to frequently asked questions to make the process for the caseworkers as convenient as possible. Job centers electronically forwarded signed consent forms. Following this, we transcribed the mobile phone and personal identification number from the hand-filled documents.\(^{10}\)

---

\(^8\) The Danish personal identification number (CPR) is necessary for us to merge the collected data with official administrative records.

\(^9\) There was just one single individual which provided consent without a mobile phone number. This person was discarded from the population subsequently.

\(^{10}\) In order to not send out SMS messages to wrong individuals, we had two research assistants transcribe the forms separately. We only contacted individuals for whom both transcriptions matched which lead to the discarding of two consent forms.
4.2.2 General Experimental Set-Up

After transcription of the consent forms, we randomized the weekly inflow of individuals who signed up for the intervention into either treatment or control groups of approximately equal size.\(^{11}\) While the control group followed the status-quo job center contact, the treatment group received additional efforts that linked SMS with videos.

We based the intervention content on a webpage that collects all videos on respective sub-pages. It additionally presented supplementary background information and linked to common job search platforms and temporary work agencies. The website also contained an inspirational list of marginal or part-time jobs that other former social welfare recipients have found. A conversation guide to aid the treatment recipients through first telephone contacts with potential employers was also available. As we contacted participants via SMS, the homepage was optimized for mobile usage to make interaction with it as straight-forward as possible.\(^{12}\)

During the intervention, we contacted participants multiple times via SMS text messages, up to a maximum of eight SMS depending on multiple factors. The benefit of using SMS messages to contact people, rather than for example email, is a usually high read- and interaction rate.\(^{13}\) To send out SMS, we used a third party SMS service that delivers messages after we post requests through a secure gateway.

The content of the SMS highlighted either the benefits of taking up marginal or part-time employment, briefly introduced a testimonial case or guided through some aspects of job search, depending on the actual step in the intervention flow (the following section provides the details). Each SMS contained a shortened URL that re-directed to the supplementary webpage that contained the respective videos in a mobile friendly layout. We personalized the links to the webpages by tagging the URLs with an ID for every participant. This enables us to analyze whether the individuals opened the links. At the end of each SMS, we allowed for the option to unsubscribe from the intervention by answering with ‘stop’.

The individual intervention started roughly one month after the job center contact in which the informed consent form was collected. With this, we made sure not to significantly interfere with the usual job center efforts. We sent the SMS text messages either on a Monday or a Friday, depending on the respective stage of the intervention. Initially, the individuals received an SMS with a link to the motivational video and survey on a Monday. If there is no fully completed survey answer, we sent a single reminder containing the same link on the following Friday. In the next step, we sent out SMS with links to the testimonial video on the following Monday.\(^{14}\) Last, the individuals got a series of four SMS containing links to the four

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\(^{11}\) While we aimed (though failed) to invite the whole stock to the intervention, consent forms were only signed in the quarterly meetings. We therefore received a continuous weekly inflow of new participants that all came from the stock of individuals in the target group. Randomization was performed electronically by drawing a random number when adding new individuals to the intervention sample. We did not stratify the sample as we only added background information once we uploaded the data to the servers of Statistics Denmark after the intervention was completed.

\(^{12}\) The webpage is archived under [http://trinfortrinitiljob-archived.mozello.dk/](http://trinfortrinitiljob-archived.mozello.dk/).


\(^{14}\) In fact two videos with SMS on Monday and Friday if the individual has multiple barriers and one or two random videos if no survey response is recorded. If the only barrier to job search is the knowledge about job search itself this step is skipped and we directly continue with the job search guidance part.
job search guidance videos. We spread these over four weeks and sent them on Mondays to give enough time to internalize the content and information. The intervention thus lasted between five and six weeks in total. We present an overview of this structure in Table 4.1 and a general treatment flow in Figure A.1 in the appendix.

Table 4.1: Intervention Timeline

<table>
<thead>
<tr>
<th>Week 1</th>
<th>Week 2</th>
<th>Week 3</th>
<th>Week 4</th>
<th>Week 5</th>
<th>Week 6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mon</td>
<td>Fri</td>
<td>Mon</td>
<td>Fri</td>
<td>Mon</td>
<td>Fri</td>
</tr>
<tr>
<td>No Barrier</td>
<td>I</td>
<td>R</td>
<td>G1</td>
<td>G2</td>
<td>G3</td>
</tr>
<tr>
<td>One Barrier</td>
<td>I</td>
<td>R</td>
<td>T1</td>
<td>G1</td>
<td>G2</td>
</tr>
<tr>
<td>Two Barriers</td>
<td>I</td>
<td>R</td>
<td>T1</td>
<td>T2</td>
<td>G1</td>
</tr>
</tbody>
</table>

Note: Letters correspond to the following SMS/videos: I=introductory, R=reminder, T1=testimonial_1, T2=testimonial_2, G1=guidance_1, G2=guidance_2, G3=guidance_3, G4=guidance_4. The reminder is only send when there’s no survey response.

4.2.3 SMS, Survey and Video Content

This section details the content of the videos, the accompanying SMS, and a survey we used to elicit the individual’s job search barrier. During the intervention, we used two sets of videos besides the first introductory clip. One set were testimonial videos relating to the survey responses and the determined barriers. The second set was the set of job search guidance videos that introduced efficient strategies for job search, especially targeted to the group of activation-ready social welfare recipients. We distributed these videos through personalized links in the SMS text messages and embedded on the webpage. The content directly linked the content of the SMS to the respective video. Translations to English are available in the appendix.

4.2.3.1 Intervention Introduction

The introductory video introduced benefits of the take-up of marginal employment. For this video we recruited a professional actor who presented information in an easily accessible manner. The benefits that the video highlighted are higher income, better chances of obtaining regular employment in the long-run, having the satisfaction of contributing to a meaningful cause, and getting access to a broader social circle with new colleagues. Further, it underscored the large number of marginal jobs available in the Danish labor market. At the end of this video, we invited the individuals to fill out a brief online survey that aimed to elicit the specific

15 The videos (in Danish) are available on the archived webpage or here: https://www.youtube.com/playlist?list=PLEaDltjY_Pp3F72-ibAhCk4mSbEpCqXOP
16 Approximately 650.000, see Thorsen et al. (2016).
job search barriers faced by the individuals. We informed the participants that filling out the survey helps to get the most valuable and personalized information out of the intervention.

4.2.3.2 Survey

We designed the survey to be short, concise, and optimized for mobile usage to make it as easy as possible to respond. Further, the introductory video and the survey were both embedded on the same webpage. This offered a direct interaction with the survey after video completion. We based the respective survey items on Rosholm et al. (2017a) who surveyed a group of Danish activation-ready social welfare recipients and followed their labor market trajectories in the Danish register data. The particular survey items we used have shown to have predictive power on the probability of engaging in job search activities. The original items from Rosholm et al. (2017a) contained one question that only asked for problems concerning overall health, including both mental and physical health issues. We split this question into mental and physical health to achieve a better matching with the testimonial videos. The original Danish survey questions and answer possibilities are in the appendix, including English translations. We expect these measures to be highly related to the general self-efficacy concept. We randomized the order of the items in the survey.\footnote{We cluster the two questions regarding mental and physical health together, yet randomized within this cluster.}

We used the outcomes of the survey to determine the exact (or strongest) barriers to job search to achieve a high relatability to the testimonial videos. We classified a survey item as an item with a barrier if the response to this question was lower or equal to three on the five-point scale. In this case, the individuals judged the respective item as a serious problem that hinders them from successfully engaging in job search or to work. We restricted ourselves to a maximum of two main barriers. We did this to not overload participants with testimonial videos afterwards. To do this, we first chose the item with the lowest score and then added the one with the second lowest score. If there were multiple ones with the same score, we randomized the barrier we chose. This way, we ended up with a maximum of two barriers. If no score was below three on the scale, we defined the individual as not challenged by severe job search barriers and skipped the testimonial stage.\footnote{The same happened if the item relating to job search knowledge is the only low score.} With no survey response, even after a reminder SMS, we randomized the individual into one or two barrier items.

4.2.3.3 Testimonials

Having a measure of the barriers that prevent individuals from engaging in job search allowed us to provide targeted advice that aimed to enhance self-efficacy through vicarious learning. To do this, we anchored specific positive testimonials at the barriers that were determined after completion of the survey. For example, we provided an individual we determined to have a job search barrier relating to physical health with a testimonial of a former social assistance recipient who successfully overcame challenges of the same kind. From the individual client’s perspective, these success stories are rather rare and unusual which might revise their pessimistic priors (Danneris and Caswell, 2019). Further, observing others having success when being in comparable situations is known to be a relatively strong way to enhance perceived
self-efficacy in individuals. This is especially powerful if these people score low on the usual self-efficacy scales (Bandura, 1977). The testimonials highlighted the individual trajectory from long-term unemployment to the eventual success and the life in employment. This included the process of job search and the motivating factors. We covered testimonials relating to mental health, physical health, a lack of professional skills, substance abuse, social exclusion, and issues with the Danish language.

It was not easy to recruit former social welfare recipients who were successful in finding employment to stand in front of the camera and tell their story - for obvious reasons. We thus substituted a part of these 'success-stories' with hired actors that present actual, yet anonymized, testimonials obtained from former unemployed and case-workers in the recruited job centers. The videos contained a disclaimer whenever an actor substituted an actual former social welfare recipient. We informed the viewers that the presented story is nevertheless real.

### 4.2.3.4 Job Search Guidance

The testimonials are followed up with job search guidance. We designed the content of these videos in close collaboration with Væksthusets Forskningscenter.\(^\text{19}\) We interviewed both researchers and caseworkers to identify job search strategies that are effective means to find marginal employment for our target group. Further, we used these interviews to script the job search guidance videos and SMS content together with the researchers of Væksthusets Forskningscenter. These clips highlight important steps and tricks that have helped other people of a similar background into employment. They set a focus on individuals with comparably low skills and their best way to approach the labor market. This includes a general introduction to organized job search, help on how to approach (phone-) conversations with companies, the benefits of informal search and the opportunities of using temporary work agencies or internships. We paid particular attention to the wording to make them easy to grasp and to not sound condescending or patronizing. The same actor as in the introductory video presents the information on job search strategies. The exact content of these messages and their English translations are available in appendix.

### 4.3 Empirical Strategy and Data

The main data source for this project is the DREAM data of the Danish Ministry of Employment. This rich administrative data set contains information on weekly transfers for the whole Danish population. The data is collected by the Ministry of Employment, the Ministry of Education, the Danish Ministry of Finance and other population registers. It is possible to use these transfer payments to identify the weekly employment status. The data additionally contains monthly information on the number of hours worked in regular employment. Using this, we can identify whether individuals find employment and how many hours of ordinary work they collect. We can also determine participation in internship programmes while still receiving social welfare benefits.

\(^{19}\)The research centre is itself financed by Væksthuset, a social enterprise which offers consulting programs and courses for vulnerable unemployed all across Denmark, often in partnership with municipal job centers.
We set up the intervention as a simple randomized control trial with assignment into treatment or control group at inflow. Our main specification separately regresses the dependent variables of interest on a dichotomous treatment indicator:

\[ Y_i = \alpha + \beta T_i + \epsilon_i \]  

(4.1)

where \( T_i \) is the treatment dummy for each individual \( i \) and \( Y_i \) are the outcomes of interest detailed below. There is no guarantee that individuals read the SMS, open the links or watch the videos. We are thus estimating an intention-to-treat effect (ITT). Using third-party web analytics software, we can identify whether a personalized link contained in the SMS has been opened. Using this data, we can get closer to the estimation of a treatment-on-the-treated effect (ATT). We therefore report additional regressions for these responders, where we exclude treated units that opened no link. One needs to be careful with the interpretation, as there is no guarantee that the time spent on the webpage is actually used to watch the video.

The intervention aims to enable job search or increase the search effort of long-term unemployed on the edge of the labor market. Hypothetically, this would increase the probability of finding a job. The main outcomes of interest are thus the share of individuals who find employment in the treatment and control groups. We are also interested in the intensive margin, i.e. the average of obtained ordinary hours of employment in both groups.

Further, there is also an interest in the effect on the share of people taking part in internship programs (\( \text{virksomhedspraktik} \)) which we highlight throughout the educational videos. Arendt et al. (2017) find that these programs have a positive short run effect on employment for social welfare recipients, partly mediated through an increase in self-efficacy and job orientation. We therefore expect that a short-run increase in internship participation, especially if self-motivated, will increase the likelihood of finding employment in the mid- to long-run. Thus, the outcomes of interest in \( Y_i \) also include the extensive and intensive margin for this measure. We have weekly information on the instance of participation in internships, yet no information on the hours performed in these. We therefore use the number of weeks in \( \text{virksomhedspraktik} \) as the intensive margin here.

Table A.1 reports the descriptive background statistics for the treatment and control groups. The low sign-up rate of 198 individuals is striking. As a result, the random allocation of individuals into treatment and control does not achieve sufficient balancing. Though we do not reject the null hypothesis of no significant difference between both groups for most variables, differences are substantial. Especially the big divergence in labor market experience over the five years prior to intervention start and the instance of mental health diagnoses is worrying.

Due to the particularly small sample, resulting in the insufficient balance between treated and controls, we additionally estimate a version that controls for individual background characteristics:

\[ Y_i = \alpha + \beta T_i + X_i \gamma_t + \epsilon_i \]  

(4.2)

where \( X_i \) collects the set of background variables which include gender, ethnicity, age, educational level, a dummy on diagnoses with mental illness, the employment history as well as job center and intervention start month fixed effects, all before randomization.

Card et al. (2017) show that there is often a substantial delay between labor market interventions and their materialized effects. While this concerns mostly larger interventions, other
small-scale treatments also show delayed effects (e.g. Altmann et al., 2018). As the intervention lasts up to six weeks, we evaluate all outcomes at three months post intervention start. We base this on a trade-off between allowing for sufficient time to observe treatment effects and limitations in the data, which is so far only available until February 2019. We do not expect effects to materialize already during the intervention or shortly after the end. The reason for this is that many of the targeted individuals are far removed from the labor market and would likely need additional support from the caseworkers throughout their job search on top of the guidance videos we provide. Once further data is available, we will extend the analysis frame to at least six months. Focusing on the three-month window since intervention start reduces our sample size further from 198 to 144 units. A substantial part of participants entered the intervention just before the end of the current window of labor market data. We expect to re-evaluate the outcomes, for 3 and 6 month windows, once further labor market data is available. This will permit us to include all individuals in the regressions.

We report the outcomes of the survey determining the specific barriers to job search and employment in Table A.2. About 32 percent of all treated units responded to the survey. We thus only have a small group of individuals that we can supply with targeted testimonials. Most of the respondents face a multitude of barriers with a lack of search skills being the most pervasive item. Over two-thirds are affected by issues to physical and mental health and do not consider themselves to be ready for work or able to cope with their daily life to a sufficient degree. These factors show that providing job search guidance may be a very important aspect of our intervention, but also that the strong doubts regarding the readiness to work may be hard to overcome by a nudging intervention.

As individuals actively select into the intervention it is important to check whether the group of participating individuals differs from the overall population at risk. In the last two columns of Table A.1 we thus compare the intervention population to the overall target group in the recruited jobcenters as well as in the country as a whole. Note that we do not know which individuals were introduced and invited to the intervention but didn't sign up as we only observe those individuals for which we received consent forms from the jobcenters. We thus face two layers of potential selection into treatment: 1) The caseworker decides whether to introduce the client to the intervention and 2) the client decides whether to participate. We are not able to distinguish between these two.

Overall, the very low participation rate of about 2% of the group of potential participants is striking. We see hints at strong differences in the success rates of signing up prospective participants to the intervention across municipalities. While 32% of the members of the target group in the participating jobcenters are registered in Odense, only 14% of the participants of the intervention are. Similarly, we see that only 4% of potential participants are registered in Norddjurs, yet 12% of actual participants are recruited from this municipality. This fact might indicate differences in the time constraints across jobcenters, resulting in lower introduction and sign-up rates (see Section 4.5). In terms of demographics, we see that intervention participants are on average slightly younger, higher educated and less likely to have had a diagnosis of mental illness than the population at risk in the target groups in the collaborating jobcenters and across the country. Further, the participants have on average accumulated somewhat

\[20\text{We are left with 68 treated and 76 control units.}\]

\[21\text{The individuals consider mental health to be an issue more often than the incidence of mental health diagnoses in the administrative data (see Table 4.1).}\]
less hours of employment over the last five years before the intervention. Even though the differences between actual and prospective participants are not overly large, they hint at some degree of selection into the intervention which would call the external validity of potential results into question.

4.4 Results

In this section we present the results of the intervention in terms of the average treatment effects on the intensive and extensive margins of regular employment as well as internship participation. We do this despite issues with the balance between the treatment and control groups and the overall low statistical power due to the low intervention turnout. Because of these limitations, we are also restricting ourselves to show results on the overall sample, as subgroup analysis would have even lower statistical power. To deal with the missing balance between both groups, we report estimates with regression controls besides the standard raw estimate. It is important to stretch that we are estimating an intention-to-treat effect (ITT) here. To approximate a treatment-on-the-treated effect (ATT) we also report results on the treatment subgroup of responders, i.e. individuals that have opened at least one link in the distributed SMS. We therefore discard individuals in the treatment group who never responded to the intervention. This specification thus discards 37 further treatment units.

Table 4.2 reports the average treatment effects of the intervention package. We find no significant effects, most likely because of missing power. The effects on the extensive margin of employment are nevertheless positive throughout columns (1)-(4). The fraction of individuals in employment is 4 percentage points higher for treated units in the basic specification and 3 percent higher for the responders. Despite a lack of statistical significance, the standard errors on the treatment coefficient are relatively small. Interestingly, the number of hours worked reduces by about 2.3 hours, yet here the standard errors are particularly large. Controlling for background characteristics flips the sign on this coefficient. For the responders, the treatment even reduces employment by 6.15 hours. As the intervention effects might not result in ordinary labor market attachment in the short-run, we also estimate the effects on entering internship programmes (virksomhedspraktik). Here, despite none being significant, we find overall positive effects of pronounced magnitude. Treated individuals are 9 percent more likely to have been in these programmes for at least one week in the three months post intervention. There are no big differences in the coefficient between the overall sample and the responders or when controlling for background characteristics. Accumulating the weeks in internships over this period, we estimate an increase of 0.35 weeks on average for the treated units.

Despite mostly positive directions, we cannot make any statistically significant conclusions on the treatment. This is a result of the severe lack of power due to low participation in the intervention. Assuming that there would in fact be a significant 0.04 effect on employment, we can calculate the sample size needed to detect this. For a significance level of 0.05 and power of 0.8, we would have needed a sample size of about 1,600 individuals to detect a treatment effect of 0.04. This confirms that the experiment was in fact severely under-powered. Our power calculations prior to implementation required even larger sample sizes, yet the initial confidence among most job centers in reaching a sufficient number of participants was high.

See the pre-registration of the trial under https://www.socialscienceregistry.org/trials/3142.
Table 4.2: Three month treatment effects: Treatment coefficients and standard errors (in parenthesis)

<table>
<thead>
<tr>
<th>Outcome</th>
<th>All (1)</th>
<th>All (2)</th>
<th>Responders (3)</th>
<th>Responders (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Any employment:</td>
<td>0.04</td>
<td>0.06</td>
<td>0.03</td>
<td>0.03</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Hours in employment:</td>
<td>-2.38</td>
<td>1.53</td>
<td>-6.15</td>
<td>-1.98</td>
</tr>
<tr>
<td></td>
<td>(8.27)</td>
<td>(8.53)</td>
<td>(9.37)</td>
<td>(9.88)</td>
</tr>
<tr>
<td>Any internship:</td>
<td>0.09</td>
<td>0.08</td>
<td>0.06</td>
<td>0.10</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.07)</td>
<td>(0.09)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>Weeks on internship:</td>
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<td>0.59</td>
<td>0.24</td>
<td>0.81</td>
</tr>
<tr>
<td></td>
<td>(0.66)</td>
<td>(0.68)</td>
<td>(0.77)</td>
<td>(0.80)</td>
</tr>
</tbody>
</table>

Controls | No | Yes | No | Yes
N        | 144 | 144 | 118 | 118

Notes: *** p<0.01, ** p<0.05, * p<0.1. Treatment effects are estimated only on the sample with three months of available data post individual intervention start. Regression controls include gender, ethnicity, age, educational level, a dummy on diagnoses with mental illness, employment history, and as well as job center and intervention start month fixed effects, all before randomization. Regressions on responders disregard treated units that did have no recorded interaction with the survey or webpage.

4.5 Assessing Implementation Issues

To assess the issues that led to the low intervention sign-up, we distributed an additional survey to the caseworkers and responsibles in the municipal job centers. The survey included questions concerning the caseworkers beliefs about the most important barriers and the reasons for why many individuals did not sign up to the intervention. We kept the questionnaire as short as possible as we expected time issues to be one of the main important drivers on the caseworker side. We received 48 responses in total. Unfortunately, many of these were partial, so we recorded only 28 full responses. We take these partial responses into account and acknowledge that this might be a further sign for issues regarding time pressure among the caseworkers.

About 60 percent of the caseworkers did not present the intervention package to all of their citizens. In fact, this group of caseworkers only informed 30 percent of the eligible citizens that the intervention existed. The reasons for this appear mainly driven by the caseworkers expectation that the project would not benefit a particular citizen. However, most of the respondents reported that they saw at least some benefit in the intervention package. This is interesting considering the importance of caseworkers beliefs about the clients’ prospects of being able to find work (Rosholm et al., 2017b). Clients with caseworkers who have a

23 The survey report is available under https://ql.tc/w0ixqt
higher employment-related belief in them are more successful in securing employment. Thus, withholding the intervention from the clients might have barred clients from potentially beneficial treatment.

The caseworkers time constraints also seem to be an important factor that potentially prevented them from engaging citizens in the intervention. Many job centers are involved in various programs for our target group, and there might only be a limited amount of time available to introduce the clients to more interventions. Despite this, many clients have actively been introduced and invited to the intervention, yet the vast majority did not sign-up. Most caseworker report that many clients did not see the benefit of external support through the intervention. Further, a substantial amount of responses declare that citizens do not believe they will be able to find a job or even work in marginal employment at all.

In conclusion, this intervention would have needed a much closer coordination with the caseworkers to identify potential bottlenecks. This way we potentially could have determined other means of inviting clients to the intervention and optimized the caseworkers time-use on this project.

4.6 Conclusion

In this paper, we developed and tested a video-based text message intervention for long-term unemployed with complex personal challenges. The treatment aims to increase the participants believe in the possibility of working in marginal employment to gain attachment to the regular labor market in the long-run. We use a novel design that combines targeted job search guidance with positive testimonials of former long-term unemployed with relateable personal challenges. Due to a very low intervention sign-up, we face a substantial lack of statistical power and clear issues in the statistical balancing of our sample. As a result, we cannot draw definite conclusions about the interventions’ treatment effects. While acknowledging the shortcomings, we report small positive effects on the likelihood of having any employment or internship in the three months after the intervention initially starts and negative effects on accumulated hours in regular employment. However, none of these effects are statistically significant.

Acknowledgements

We are grateful for funding by Væksthusets Forskningscenter and the Interacting Minds Centre at Aarhus University. We thank all collaborating job centers and the respective caseworkers therein for the significant efforts involved in implementing this intervention. We further thank Johanna Rantzau and Mathias Varming Jacobsen for their assistance in transcribing the consent forms. The experiment was pre-registered under AEA RCT Registry trial number: 3142.
4.7 References


Appendix

A.1 Figures

Figure A.1: Treatment Flow

Note: The figure displays an overview of the intervention, starting with the introductory video that invites to the motivational SMS embedded in the survey. If there is no survey response a single reminder will be sent. Next, the individuals receive one or two testimonial SMS with links to the homepage and the respective video, depending on the survey response (two if multiple barriers are detected). Once this stage is completed participants receive four job search guidance SMS with videos on the homepage.
## A.2 Tables

**Table A.1:** Descriptive statistics by treatment status and for the population at risk in the participating jobcenters and the overall country: Means, standard deviation (in parenthesis) and difference between means for treatment population

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Treat</th>
<th>Diff</th>
<th>Jobcenter</th>
<th>Country</th>
</tr>
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<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Female</strong></td>
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<td>0.53</td>
<td>0.05</td>
<td>0.49</td>
<td>0.49</td>
</tr>
<tr>
<td></td>
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<td>(0.50)</td>
<td>(0.50)</td>
<td>(0.50)</td>
<td>(0.50)</td>
</tr>
<tr>
<td><strong>Ethnic dane</strong></td>
<td>0.73</td>
<td>0.69</td>
<td>-0.04</td>
<td>0.67</td>
<td>0.71</td>
</tr>
<tr>
<td></td>
<td>(0.45)</td>
<td>(0.46)</td>
<td>(0.47)</td>
<td>(0.45)</td>
<td>(0.45)</td>
</tr>
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<td><strong>Age</strong></td>
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<td>39.69</td>
<td>-2.96***</td>
<td>43.43</td>
<td>44.59</td>
</tr>
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<td></td>
<td>(11.62)</td>
<td>(12.61)</td>
<td>(9.85)</td>
<td>(9.99)</td>
<td></td>
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<tr>
<td><strong>Primary Education</strong></td>
<td>0.44</td>
<td>0.51</td>
<td>0.11</td>
<td>0.52</td>
<td>0.52</td>
</tr>
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<td></td>
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<td>(0.50)</td>
<td>(0.50)</td>
<td>(0.50)</td>
<td>(0.50)</td>
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<td>(0.35)</td>
<td>(0.38)</td>
<td>(0.38)</td>
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<tr>
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<td>0.08</td>
<td>0.46</td>
<td>0.45</td>
</tr>
<tr>
<td></td>
<td>(0.48)</td>
<td>(0.50)</td>
<td>(0.50)</td>
<td>(0.50)</td>
<td>(0.50)</td>
</tr>
<tr>
<td><strong>Hours of employment (5 years)</strong></td>
<td>382.41</td>
<td>460.12</td>
<td>77.70</td>
<td>453.92</td>
<td>552.95</td>
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<td>(857.9)</td>
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<td>(1095.04)</td>
<td>(1209.52)</td>
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**Jobcenter**

<table>
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<th>Control</th>
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<th>Jobcenter</th>
<th>Country</th>
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<td>(0.25)</td>
<td>(0.11)</td>
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</table>

**N**: 107 91

**Notes:** *** p<0.01, ** p<0.05, * p<0.1. Individual characteristics are measured in the month before intervention start (individual starting date for treatment population, month before general intervention start for population at risk). (1) Education levels refer to highest attained education level. (2) Mental illness measures previous official diagnosis with a mental illness. (3) Hours of employment are calculated over the 5 years before treatment start.
Table A.2: Survey outcomes: Barriers to job search and employment

<table>
<thead>
<tr>
<th>Share affected</th>
<th>Avg. severity</th>
</tr>
</thead>
<tbody>
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<td>Readiness to work</td>
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</tr>
<tr>
<td>Physical Health</td>
<td>0.78</td>
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<tr>
<td>Mental health</td>
<td>0.67</td>
</tr>
<tr>
<td>Coping</td>
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<tr>
<td>Danish skills</td>
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<tr>
<td>Search skills</td>
<td>0.74</td>
</tr>
</tbody>
</table>

N: 29

Notes: The table shows the outcomes of the survey used to elicit barriers to job search and employment. Rows relate to specific barriers and the respective survey questions (see appendix). Survey item are ranked on a five-point scale, where 1 equals a very severe barrier and 5 equals no barrier. The first column displays the fraction of respondent we define as affected by the barrier (response less or equal to 3). The second column shows the average answer on the five-point scale among all respondents.
A.3  Experimental Material

A.3.1  Project Description on Consent Form

Projektbeskrivelse - Tro på det!


Hvilke oplysninger behandles?

Med dette samtykke giver du tilladelse til, at vi registrerer følgende oplysninger om dig:

1. Navn
2. CPR-nummer
3. Mobil-nummer

Hvad bruges oplysningerne til, og hvem har adgang til dem?

Oplysningerne bruges til at sende sms-tekstbeskeder der indeholder links til korte videoer med hjælp på vej mod småjob til nogle tilfældigt udvalgte deltagerne. Oplysningerne anvendes i forbindelse med register data til at undersøge om korte videoer og sms-beskeder kan hjælpe kontanthjælpsmodtagere til lønnet arbejde. Det er alene medarbejdere på Institut for Økonomi, Aarhus Universitet der har adgang til oplysningerne. Alle oplysninger behandles fortroligt.

**English translation:**

**Project description - Believe in it!**
The project is a research project at the Department of Economics at Aarhus University. The project investigates whether short videos with simple tips can help welfare recipients get into paid work. The project is a so called randomized experiment. Some randomly selected participants will receive free text messages containing links to short videos with help and guidance to get into marginal jobs. This means here is a possibility that you will not receive these SMS text messages. The tips presented have proven to work well for others in similar situations. The videos are sent without costs and obligations to you, and the project is not associated with the job center or caseworker. To get the most from these videos, we ask you to answer 6 short and simple questions during the project, which you can do directly from your phone. It will take less than 5 minutes. There will be a maximum of 8 text messages spread over a period of 5 to 6 weeks and you can choose to stop receiving these text messages at any time. Participation in the project is voluntary and it is without consequences for you to refuse to participate in the project. Your participation in the research project is anonymous, but we need your mobile number to send you text messages and your social security number in order to follow you in the data.

**What information is being processed?**
With your consent, you authorize us to record the following information about you:

1. Name
2. Social security number
3. Mobile phone number

**What is the information used for and who has access to it?**
The information is used to send SMS text messages containing links to short videos with help on the way to marginal jobs for some randomly selected participants. The information is used in connection with register data to investigate whether short videos and text messages can help welfare recipients into paid work. Only employees at the Department of Economics, Aarhus University, have access to the information. All information is treated confidentially.

As soon as the processing of the incoming data has been completed (no later than December 2021), your contact information and CPR number will be deleted. The rest of the collected data will only be used as unidentified individual data at Statistics Denmark. This means that it will no longer be possible for us to identify you directly in the data. You have the option to withdraw your consent at any time, after which all information will be deleted unless they have been transferred to unidentified form before. You do this by writing to Jonas Fluchtmann, Department of Economics, Aarhus University at the e-mail address: fluchtmann@econ.au.dk.
A.3.2 SMS Content

**Introductory (I):**

"Sidst i job center hørte du om projektet 'Tro på det!' fra Aarhus Universitet. Tak for at du er med. Der er måske nogle barrierer, der forhinder dig i at søge efter et job, men der er mange personer, der har fundet et småjob på trods af lignende udfordringer. Brug fem minutter nu og se vores video på [URL] og få information om alle de positive ændringer, et skridt i retning af ordinære løntimer kan betyde for dig! Svar med 'stop' for at afmelde."

Translation:

"In the job center you recently heard about the "Believe in it!" project from Aarhus University. Thank you for joining. There may be some barriers that prevent you from searching for a job, but there are many people who have found marginal employment despite similar challenges. Use five minutes now and watch our video at [URL] to get information on all the positive changes a step toward a few hours in regular pay can mean for you! Reply with "stop" to unsubscribe."

**Reminder (R):**

"Husk: At arbejde et par timer om ugen har mange fordele for dig! Det tager kun et par minutter at se vores video og besvare vores spørgsmål under [URL] hvorefter vi hjælper dig med trinvis vejledning. Hvorfor ikke prøve det? Svar med 'stop' for at afmelde dig."

Translation:

"Remember: Working a few hours a week has many benefits for you! It only takes a few minutes to watch our video and answer our questions at [URL] after which we will help you with step by step instructions. Why not try it? Reply with 'stop' to unsubscribe."

**Testimonial 1 (T1 or T2):**

"Carsten havde voldsomme smerter i ryggen, som forhindrede ham i at arbejde. Han fandt dog en arbejdsgiver, der tilbød ham at se om han kunne udføre et arbejde i en praktik. I dag er han ved at starte et nyt job som lastbilchauffør hvilket er grunden til at han er virkelig glad for sin fremtid. Se videoen hvor han fortæller om sin vej på [URL]! Svar med 'stop' for at afmelde."

Translation:

"Carsten had severe back pain which prevented him from working. He nevertheless found an employer who offered him to see if he could work in an internship. Today he is starting a new job as a truck driver, which is why he is really happy about his future. Watch the video where he talks about his way at [URL]! Reply with 'stop' to unsubscribe."

**Testimonial 2 (T1 or T2):**

"Marie har alvorlige problemer med voldsom angst og depression. Hun overvandt dog denne udfordring og fandt et job hos en arbejdsgiver, der giver hende meget fleksibilitet. I dag er hun lykkeligt ansat som sosu-hjælper i 15 timer om ugen og har det så godt som hun ikke har haft det længe. Se hende fortælle om sin vej mod arbejde på [URL]. Svar med 'stop' for at afmelde."

Translation:

"Marie has serious problems with severe anxiety and depression. However, she overcame this challenge and found a job with an employer that gives her a lot of flexibility. Today, she is happily employed as a social worker for 15 hours a week and feels as good as she hadn't for a long time. See her talk about her way to work at [URL]. Reply with 'stop' to unsubscribe."
Testimonial 3 (T1 or T2):
"Brian har søgt efter et job i lang tid og blev altid afvist af potentielle arbejdsfirere. Han har aligevel aldrig givet op og dermed til sidst fundet to job, som han kan udføre på trods af at han er uudannet. I dag arbejder han som rengøringsassistent i nogle timer om ugen og også som opvasker nogle aften. Se ham fortælle om sin vej mod job på [URL]. Svar med 'stop' for at afmelde."

Translation:
"Brian has been looking for a job for a long time and has always been rejected by potential employers. He has never given up and eventually found two jobs that he can work in despite lacking a formal education. Today he works as a cleaning assistant for a few hours a week and also as a dishwasher some nights. See him talk about his way to work at [URL]. Reply with 'stop' to unsubscribe."

Testimonial 4 (T1 or T2):
"Per har været arbejdsløs på grund af depression, angst og alkohol. Han lukkede sig ud fra hele sit sociale liv, men fandt sig til sidst i beskæftigelse alligevel. I dag arbejder han som arbejdsmand og er begyndt at betale af på sin gæld. Han er glad fordi han tog kontrollen over sit liv tilbage. Se ham fortælle om sin vej mod job på [URL]! Svar med 'stop' for at afmelde."

Translation:
"Per has been unemployed as a result of depression, anxiety and alcohol abuse. He estranged himself from his social life, but eventually found employment anyway. Today he works as a factory worker and has started paying off his debt. He is happy because he took control of his life back. See him talk about his way to work at [URL]! Reply with 'stop' to unsubscribe."

Testimonial 5 (T1 or T2):

Translation:
"Alisa has had both physical and mental issues and has been unemployed for a long time. She did not see these conditions as a problem that excluded her from the labor market and therefore began to search for part-time jobs. She called many companies and asked if she could help them in any way. Today, Alisa works as a distributor for 9 hours a week. Now she is happy and content with her life! See her talk you about her way to work at [URL]! Reply with 'stop' to unsubscribe."

Guidance 1 (G1):
"Dagens tips: Forbered din jobsøgning! Tænk på det som en proces der kræver en smule struktur og en del tålmodighed. Vi hjælper dig med at få denne struktur på plads. Det er ikke så svært som du tror! Brug fem minutter nu og tag det første skridt i retning af ordinære timer ved at se vores video med nyttig hjælp på [URL]. Svar med 'stop' for at afmelde."

Translation:
"Today's tip: Prepare your job search! Think of it as a process that requires a little structure and some patience. We help you get this structure in place. It's not as hard as you think! Use five minutes now and take the first step toward regular hours by viewing our video with helpful tips at [URL]. Reply with 'stop' to unsubscribe."
Guidance 2 (G2):
"Dagens tip: Lær virksomheden at kende, før du ringer til dem! Det kan virke svært at ringe til et firma, men det er faktisk nemmere end du tror. Vi hjælper dig med nogle vejledninger til, hvordan du kan gribe det an. Brug fem minutter nu og gør dig klar til at ringe til et firma ved at se vores video på [URL]. Svar med 'stop' for at afmelde."

Translation:
"Tip of the day: Research the company before you call them! Calling a company may seem difficult, but it's actually easier than you think. We will help you with some guidance on how to deal with it. Use five minutes now and get ready to call a company by viewing our video at [URL]. Reply with 'stop' to unsubscribe."

Guidance 3 (G3):
"Dagens tip: Søg uopfordret! Mange kontanthjælpsmodtagere har fundet et småjob efter selv at have kontakttet en virksomhed. Rigtig mange jobs annonceres slet ikke via officielle kanaler, og der er mange muligheder for at finde målrettede opgaver for dig! Brug fem minutter nu og se vores video med nyttig hjælp på [URL]. Svar med 'stop' for at afmelde."

Translation:
"Tip of the day: Search unsolicited! Many long-term unemployed have found a part-time job after having contacted a company themselves. Many jobs are not advertised at all through official channels and there are many opportunities to find suitable tasks for you! Use five minutes now and watch our video with useful help at [URL]. Reply with 'stop' to unsubscribe."

Guidance 4 (G4):

Translation:
"Tip of the day: Consider temp work agencies! In recent years, companies have employed more and more employees through temporary employment agencies, even with only a few hours of work. The agencies find possible temporary employees and it is you who can decide when and how much you want to work. Use five minutes now and see how you can use temporary agencies at [URL]. Reply with 'stop' to unsubscribe."
A.3.3 Survey Questions

Q1 Hvordan vil du alt i alt vurdere dine faglige færdigheder ift. at kunne varetage et arbejde?
1. Mine faglige færdigheder forhindrer, at jeg kan finde arbejde
2. Jeg tvivler meget på, at jeg kan finde et arbejde
3. Jeg er usikker på, om jeg kan finde et arbejde
4. Jeg er nogenlunde sikker på, at jeg kan finde et arbejde
5. Jeg er helt sikker på, at jeg kan finde et arbejde

Q2 Hvordan vil du alt i alt vurdere dine sproglige færdigheder (på dansk) ift. at kunne varetage et arbejde?
1. Mine sproglige færdigheder forhindrer, at jeg kan finde arbejde
2. Jeg tvivler meget på, at jeg kan finde et arbejde, mine sproglige færdigheder er ikke så gode
3. Jeg er usikker på, om jeg kan finde et arbejde pga. mine sproglige færdigheder
4. Jeg er nogenlunde sikker på, at jeg kan finde et arbejde med mine sproglige færdigheder
5. Jeg er helt sikker på, at jeg kan finde et arbejde, jeg har gode sproglige færdigheder

Q3 Har du overskud i hverdagen til at fokusere på at få et arbejde/følge et aktiverings eller uddannelsesforløb?
1. Jeg kan næsten aldrig overskue at fokusere på det
2. Jeg kan for det meste ikke overskue at fokusere på det
3. Det svinger. Nogle gange kan jeg overskue det, andre gange ikke
4. Jeg kan for det meste overskue at fokusere på det
5. Jeg kan sagtens overskue at fokusere på det

Q4 Ved du, hvad du skal gøre for at forbedre dine muligheder for at få et job?
1. Jeg ved ikke, hvordan jeg kan nærme mig et job
2. Jeg ved kun lidt om, hvordan jeg kan nærme mig et job
3. Jeg ved nogenlunde, hvordan jeg kan nærme mig et job, men er også noget i tvivl
4. Jeg ved en del om, hvad jeg skal gøre for at nærme mig et job
5. Jeg ved helt klart, hvad jeg skal gøre for at nærme mig et job

Q5 Hvordan vil du alt i alt vurdere dit fysiske helbred ift. at kunne varetage et arbejde?
1. Mit fysiske helbred forhindrer, at jeg kan arbejde
2. Mit fysiske helbred sætter store begrænsninger for, at jeg kan arbejde. Jeg vil måske kunne varetage et job på få timer
4. Mit fysiske helbred er ikke i vejen for, at jeg kan arbejde, men kan sætte enkelte begrænsninger
5. Mit fysiske helbred er ikke i vejen for at jeg kan arbejde

Q6 Hvordan vil du alt i alt vurdere dit psykiske helbred ift. at kunne varetage et arbejde?
1. Mit psykiske helbred forhindrer, at jeg kan arbejde
2. Mit psykiske helbred sætter store begrænsninger for, at jeg kan arbejde. Jeg vil måske kunne varetage et job på få timer
4. Mit psykiske helbred er ikke i vejen for, at jeg kan arbejde, men kan sætte enkelte begrænsninger
5. Mit psykiske helbred er ikke i vejen for at jeg kan arbejde
**English translation:**

**Q1 How would you evaluate your professional skills in terms of being able to handle a job?**

1. My skills prevent me from finding work
2. I very much doubt that I can find a job
3. I'm not sure if I find a job
4. I'm pretty sure I can find a job
5. I'm absolutely sure I can find a job

**Q2 How would you all in all evaluate your language skills (in Danish) in terms of being able to handle a job?**

1. My language skills prevent me from finding work
2. I very much doubt that I can find a job, my language skills are not so good
3. I am unsure whether I can work because of my language skills
4. I'm pretty sure I can find a job with my language skills
5. I'm absolutely sure I can find a job, I have good language skills

**Q3 Are you able to focus on getting a job/following an activation or training course in your daily life?**

1. I can almost never focus on it
2. I mostly can't focus on it
3. It depends. Sometimes I can, sometimes not
4. I can mostly focus on it
5. I can easily focus on it

**Q4 Do you know what to do to find a job?**

1. I don't know how to search for a job
2. I know little about how I can search for a job
3. I know how I can search for a job, but I also have some doubts
4. I know a lot about what to do to search for a job
5. I clearly know what to do to search for a job

**Q5 How would you assess your physical health in terms of being able to work?**

1. My physical health prevents me from working
2. My physical health limits my ability to work. I might be able to do a job with few hours
3. It depends. Occasionally my physical health makes it difficult to work. I might be able to do a part-time job with the right kind of help and support
4. My physical health is not in the way for me to work, but can set some limitations
5. My physical health is not in the way for me to work

**Q6 How would assess your mental health in terms of being able to work?**

1. My mental health prevents me from working
2. My mental health limits my ability to work. I might be able to do a job with few hours
3. It depends. Occasionally my mental health makes it difficult to work. I might be able to do a part-time job with the right kind of help and support
4. My mental health is not in the way for me to work, but can put some limitations
5. My mental health is not in the way for me to work
A.3.4 Video Content

Introductory (I):
The video is available here:
http://trinfortrinitiljob-archived.mozello.dk/hvorfor/

Translation:
"Hi and welcome! Are you tired of trying to find work? Here is what worked for others who felt that way: they switched their effort to finding work for just a few hours a week. Employers have a need for these jobs and value them highly!

We know that it is hard, sometimes very hard to find work. Some people even face serious challenges in getting into work, but everyone has competences that employers value. A lot found their way in by starting with a job that had a few hours of work, a job that got them started living a happier and more fulfilling life!

What kind of jobs are these? Many people now clean for a few hours or do the gardening in small and medium sized firms, but there are countless possibilities and probably also one that will fit right for you. We have prepared a list of possible jobs below. It might be a place where you can exploit some competencies you perhaps didn't even realize that you had! So far, there are around 450.000 jobs with less than 10h a week in Denmark and many firms are willing to create more.

But can this pay off for me? First of all, there is an economic benefit. Under the new welfare ceiling there is a ceiling on how much you can get in public support. When starting on a job, only a part of your paycheck is deducted from the benefits that you are already receiving. That means you can obtain a higher income for yourself and your family! Additionally, you may be eligible for job premium, which would further increase your income. If you were considering an internship, you can also combine this with some ordinary hours – the only requirement is that the tasks differ a little. This is a great way to gain foothold in the labor market and increase your skills in the meanwhile!

Besides that, the many people that found a job report a higher level of happiness as they have something to get up for every morning, receive their own paychecks directly from the company and have great connection to their new colleagues. It is something to be really proud of!

Do these benefits convince you? Then why not give it a try? You are not alone on this: We have prepared a little help on this way towards a job for you – the only thing you need to do now is to take 5 minutes and answer six short and easy questions below the video. After this, we will come back with some help that fits just right for you – completely free and without any commitments!"
Testimonial 1 (T1 or T2):
The video is available here:
http://trinfortrniljob-archived.mozello.dk/successhistorier/carsten/

Translation:
"For years, I was struggling with problems in my back. I have been working as a truck driver before, but since the problems in my back started I have been absent from work most of the time. Eventually I quit the job as I could not bear the pain I was feeling during work. I spent many hours at doctors' offices, physiotherapists and I have had many surgeries. None of them really helped me I thought. I was still feeling the pain every day. This led me to stay on welfare benefits for more than 10 years, and even though I wanted to, I never considered myself being able to work again. Life on welfare benefits is extremely boring, lonely and depressing. This is also what led me to drink too much alcohol in order to pass the time to the next day - which would be the same all over again.

I always wanted to get out of the ‘system’ and have a regular life with a job and my own self-earned money – no welfare benefits -, but I simply thought I could not do it due to the problems with my back and my struggles with alcohol. My dream was to become a truck driver again, but since I have turned 50 some years ago, I needed a new driving license and it is not easy to get it granted by the job center. With the welfare benefits I receive, I could not have payed for it myself. I also did not know whether my back would allow me to work after all the operations.

I eventually got into a internship as a support driver to try to see whether I can manage to work in a similar job again. Here I was joining a truck driver on the passenger seat to see whether my back problems allowed me to sit and keep up with the job. I helped loading and unloading the trucks and it felt good to be back in a trucks cockpit after such a long time. I still felt some pain, but it was manageable. After all, it seems that the surgeries had helped – at least to some degree. I managed to work all of the days during the internship and my boss was satisfied with me. My caseworker at the job center and my boss set up a declaration that they would offer me paid hours if I would recover my truck driver's license. Due to this declaration, the job center granted me to pay for the driver's license.

It was great to join my colleagues on the passenger seat, but I feel ready now to drive a truck all by myself again. I am on the way to get my driver's license now and look forward to finish it very soon. I have just the final exam in front of me and cannot wait to be less dependent on welfare benefits and sit in the cockpit all by myself. Since I started the internship I felt stable for the first time in years as I had something to get up for in the mornings. I was able work in the job I always wanted to get back to! For the first time in a long while, I feel happy and look forward to the future."
Testimonial 2 (T1 or T2):
The video is available here:
http://trinfortrintiljob_archived.mozello.dk/successhistorier/marie/

Translation:
"I was struggling a lot with my mental health and this put me on welfare benefits for a long time. I am a trained social- and health worker and have worked in this job for a while, but the job was always stressful and with a lot of pressure. I simply could not handle it. It was hard for me to deal properly with all the patients I was responsible for which made me feel extremely guilty to personally let them and my colleagues down. I thought I simply did not achieve what I thought everyone expected from me. I got severe anxiety attacks during the job as well as outside of it, because I simply could not handle all the stress and disappointments. I thought I really could not manage to work in any job and stayed passive in welfare benefits for a long time.

I was introduced to a mentor, but initially refused this offer. Back then I thought I had a problem no one could really help me with, as my anxiety would not just simply go away. I eventually agreed to meet him and he helped me to figure out that I was still passionate about my former job: assisting people who are in need for help! However, he still acknowledged my anxiety issues and saw that a job where I get attached to people and environments would not work for me.

Initially we tried a internship in a supermarket, to see whether this would work for me. The supermarket was very satisfied with me and even offered me some hours of paid work in the closing shifts. My anxiety still hit me as I could not deal with the duty to be the sole responsible person to close the shop at the end of the day. I had to refuse this job. We concluded that I could try to sign up for a vikarbureau, which would give me the flexibility I need, and I would get less attached to patients and people. I signed up and got a job as a vikar social and health care helper - the job I am still passionate about. Here I work some hours a week in varying institutions. I know very well what led to my anxiety problems and this job helps me as I avoid long-term relations to colleges and patients. At the vikarbureau, I decide by my own which shifts and duties I want to take and can handle. Most often I pick solitary night shifts, that are quiet without too much of the pressure I felt before. They suit me very well as I have only a little bit of contact to With my mentor, I calculated that I have to take 15 hours a week to live without provision to have the same income as when I was on full provision. I decided to do this and feel very happy about it now. The anxiety is of course not fully gone, but I have learnt to work with it. I do not need to exclude myself from the work I am passionate about because of it! Now I can pay and support my children and myself without money from the state. It is my own money and I earned it myself. This is a great feeling and means a lot to me!"
Testimonial 3 (T1 or T2):
The video is available here:
http://trinfortrindiljob-archived.mozello.dk/successhistorier/brian/

Translation:
"For a very long time I had been searching for jobs and sent a lot of applications to countless companies. Every time the employers told me “Sorry, we are not hiring right now!” or “Come back to us later. We may need you at that point in time, but right now there is no job for you here.” It is extremely frustrating if you really want to work, but every attempt to find a job is always turned down, no matter how much effort you put into every application. I wanted to get out of the social benefit system and earn my own money, but even though I tried hard, it was just not possible for me. I just wanted to get out of sitting at home all day having nothing to do which is really boring and depressing. Because all my applications to jobs have been unsuccessful and depressing, I tried to search for smaller jobs with less hours of work. I had heard that companies often hire people to help out in smaller tasks and they rarely ask for high qualifications here. In addition, if they are particularly busy you can work for more hours – this means more salary at the end of the day!

I usually turned up at the given place of work to deliver my application by hand after I found a job advertisement. Sometimes I also went to companies that did not put up a job advertisement – simply to see whether there is something for me to do. I think it is always better to go to company directly or to call them and to be pro-active! That way they can understand that I am really motivated to work. Actually, I found one job this way! I just went straight to a restaurant and asked whether they have some work for me. The employer was skeptical at first, but he offered me a job as a dishwasher in the evenings for a few hours a week.

Besides my job as a dishwasher, I also found a few hours of work in a cleaning company. For one hour a day I help out in the cleaning of offices and other places. It is not much, but together with the restaurant job I am able to have some more money at the end of the day. Especially when days are busy in the restaurant I can work for a lot of hours which then makes me really happy! I am not out of social benefits, but I think it feels much better being able to, at least, earn a bit of money on your own."
Testimonial 4 (T1 or T2):
The video is available here:

http://trinfortrintiljob-archived.mozello.dk/successhistorier/per/

Translation:
"Due to my long-term depression and anxiety issues, I have been on social welfare benefits for many years. The depression and anxiety turned me to drink too much alcohol. I basically only stayed at home and barely left the house or talked to anyone. I was not even able to talk to my children even though I miss them every day. Over the years, I also developed severe pain in my feet, which never got proper treatment because I did not go the doctors.

To be honest, I did not bother to search for a job for a long while, because I simply thought I could not do it with my feet, my alcohol problems and my mental health issues. How are you supposed to work if it gives you anxiety and you cannot walk without pain? However, as I never picked up calls from the job center my caseworker personally turned up at my door and brought me to the doctor. Over the following months, I got the proper treatment and my feet improved somewhat. Additionally, I had just gotten a new girlfriend and she showed me that it could be a good thing to be in contact with people and not just sulk at home behind closed doors. I also did not want to disappoint her and earn some money by myself, especially as I had stacked up a lot of debt over the years.

After my feet got better, I got the chance to participate in an internship in the business-center in my municipality as well as afterwards in a local factory where my job was to drive the forklift and load and unload trucks. The job suited me well as I could sit most of the time and my feet would not hurt too much. It felt good to be there with colleagues and finally get out of the house doing something purposeful. After the internship ended, my boss offered me to come in for a few hours each week and help out in the company – pretty much doing the same job as I had been doing before in the internship. The job is relatively flexible, so when my anxiety and depression are especially bad I can let him know and he understands it.

Due to the job in the factory, I am having a higher income these days and I can therefore pay off some of the debt I collected over the years before. It is a really good feeling to regain control over your life! All of my initial problems are of course still there to some degree – my depression as well as my anxiety still pull me down on bad days and my feet are not pain free every day. However, I learned to live and work with it! It is also good to have some colleagues around you at work and this helped me to enjoy talking with people in general again. It is still a long way for me, but I think I am on the right track!"
Guidance 1 (G1):
The video is available here:
http://trinfortriljjob-archived.mozello.dk/jobsogning/start/

Translation:
"Hi and welcome back! In the last videos, you saw that getting a job with a few hours of work is worth aiming for and that many people found such a job, even though they faced serious challenges. We are now going to help you on the way to find the job that will work for you!

When you start your job search, it is very important to not simply rush into something. That way you can get easily overwhelmed by the task and lose the focus you need. Think of it as a process that needs a little bit of structure and patience.

To get started, brainstorm about the work that you would be comfortable with. Below you’ll find a list with examples of jobs that other people already found and which are highly valued by their employer, but you can be creative and might have an idea what you could bring to a firm. It is important to focus on what you can do – and not what you can’t. A potential employer can get financial support to help you being able to work, so don’t be afraid if a job would need some special accommodation or assistance!

When you do search for a job, make sure you do not only focus on one way. A better strategy is to use a variety of approaches, such as newspapers, the internet, unsolicited applications, your own network and temp-work agencies. The more ways you use, the higher the chances of success! A good way to start is also an internship, especially if you do not feel immediately ready to work and think you need to acquire some skills before starting.

Brainstorm about who you know (family, friends, former schoolmates and colleagues, people you met at events and sport, neighbors) and let them know that you are searching for a job. When we think of these networks, we tend to think in the present. But try to think back in time - you’ve met many more people in your life than you might even think. It is also a good idea to share your job search intentions on social media such as Facebook! Make sure that as many people as possible know that you are looking for a job.

In the following videos, we will show you how to actually contact a firm. It is not as scary as it may sound! Additionally, we will show you some special ways to search for a job."
Guidance 2 (G2):
The video is available here:
http://trinfortrintiljob-archived.mozello.dk/jobsogning/kontakt/

Translation:
"One of the parts of job search many people are afraid of is the contact to potential employers. It is actually not scary as you might think! Most employers are kind and nice to motivated job seekers and curious to get to know you. So don't be afraid to call – there is nothing to lose!

Before you contact a firm, it is important to be prepared. Therefore, take some time and look at the firms webpage to make yourself familiar with what it exactly does. Here you can also try to find the right person to contact, but if there is no information, you can also ask for this during the phone call later. If you feel nervous before the call, try talking to people you don't know personally, like someone you sit next to on the bus and talk a bit about the weather.

Be ready before you pick up the phone and have a rough idea on what you want to say. We have prepared a few sentences that you could use below this video. For the first contact, keep it short and simple. Ask who the responsible person is, introduce yourself and ask for a short moment of their time. If the other person is busy, simply ask when you can call back.

If you are calling for a posted position, refer to the job ad and express your interest. If you call unsolicited, let them know that you are currently searching for a few hours of work and ask whether there are any work needs that you could fill in their company.

Tell them something about you, for example what your education is, where you have been on internships, what you have learned there or why you find their firm interesting. It is always a good idea to ask whether a meeting is possible. It is important to focus on what you can do – and not what you can't. Remember - If you need to ask for an accommodation, phrase it in positive terms ("As long as my work hours are flexible, I can work.").

You can also offer the firm to start with an internship to see whether you fit to the job. This is also a great way to learn the necessary skills you need if you do not feel immediately ready to work.

At the end of the call, thank the other person for their time. If the employer agrees to meet you – congratulations, you just made a good step towards work! If not, don't worry – there are plenty of other places out there. It is important to stay curious and open to opportunities and be ready to strike your luck."
Guidance 3 (G3):
The video is available here:
http://trinfortrintiljob-archived.mozello.dk/jobsogning/uopfordret/

Translation:
"There is a large amount of jobs in Denmark that are not officially announced and this is especially true for smaller companies and jobs with a few hours of work each week. Many firms are happy to get contacted to fill such jobs without having to go through the process of posting the position.

An important benefit of an unsolicited application is that you show great initiative and motivation - the company will appreciate your pro-activity! You will also not be in competition with other job seekers like for ordinary posted positions.

Focus on smaller firms in your area. They often hire unsolicited and might have work needs they are initially unaware of – that is where you come in! To start, let your network of friends, former colleagues, acquaintances and schoolmates know that you are searching and ask them whether their current workplace might have some work needs. It is a good idea to use social media such as Facebook to share your job search intentions and to find out where the people in your network are employed.

If you know which jobs would be interesting for you – make use of the internet! You can use Google maps to find all firms of a sector that interests you in your area. Make a list of these firms, use the internet to find their contact details and familiarize yourself with what the firm exactly does. This is important once you start to contact them! If you found the firm through your network, you can ask this person for more information and who to contact. Consider whether an internship might be a good way to get a foot inside the company.

When it comes to contacting the firm, stick to the approach we have given you before. You can always re-watch the video here."
Guidance 4 (G4):
The video is available here:
http://trinfortrintiljob-archived.mozello.dk/jobsoegning/vikarjob/

Translation:
"In recent years, firms are hiring more and more workers via temporary work agencies. Especially in jobs that relate to the fields of office and IT, construction, cleaning, kitchen and warehouse many jobs – from just a few hours to full-time - are available.

Signing up at a one of these agencies is a great way obtain ordinary hours and foothold on the labor market – if you are hired you will gain valuable experience and skills while working for varied and interesting tasks. Additionally, there is a chance to be permanently hired by one of the firms!

One of the biggest benefits of this approach is that the job search becomes a lot easier! The temp-work agencies will try to match you to potential employers while taking into account your needs and your situation. You decide when and how much you want to work

When signing up for a temp-work agency you usually need to upload a CV and declare some preferences about the jobs you would like and the amount of hours and days you want to work. Then the agency will start to match your profile to firms that are currently searching for a vikar. The CV contains your education as well as places where you have worked or been on internships Most of the temp-work agencies help you out with simple guidelines on how to prepare a good CV on their homepage. Simply follow their steps. Tip: increase your opportunities by signing up with more than one agency. We have collected some links to their webpages below. While writing the CV focus on what you can do – and not what you can't.

Now it is time to wait, once the temp-work agency has found a job for which you are well suited, you will be contacted. Tip: make the most of your time by searching also through the job databases on their web-pages!"
**Declaration of co-authorship**

Full name of the PhD student: Jonas Fluchtmann

This declaration concerns the following article/manuscript:

<table>
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<tr>
<th>Title:</th>
<th>The Dynamics of Job Search in Unemployment</th>
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<tr>
<td>Authors:</td>
<td>Jonas Fluchtmann, Jonas Maibom</td>
</tr>
</tbody>
</table>

The article/manuscript is: Published [ ] Accepted [ ] Submitted [ ] In preparation [x]

If published, state full reference:

If accepted or submitted, state journal:

Has the article/manuscript previously been used in other PhD or doctoral dissertations?

No [ ] Yes [x] If yes, give details:

The PhD student has contributed to the elements of this article/manuscript as follows:

A. Has essentially done all the work
B. Major contribution
C. Equal contribution
D. Minor contribution
E. Not relevant

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**Signatures of the co-authors**

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<td>Jonas Maibom</td>
<td>[Signature]</td>
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In case of further co-authors please attach appendix

*As per policy the co-author statement will be published with the dissertation.*
Declaration of co-authorship

Full name of the PhD student: Jonas Fluchtman

This declaration concerns the following article/manuscript:

Title: Gender Gaps in Job Search and Hiring Outcomes
Authors: Jonas Fluchtmann, Anita Glenzy, Nikolaj Harmon, Jonas Maibom

The article/manuscript is: Published ☐ Accepted ☐ Submitted ☐ In preparation ☒
If published, state full reference:
If accepted or submitted, state journal:
Has the article/manuscript previously been used in other PhD or doctoral dissertations?
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In case of further co-authors please attach appendix

Signature of the PhD student

*As per policy the co-author statement will be published with the dissertation.
Declaration of co-authorship

Full name of the PhD student: Jonas Fluchtmann

This declaration concerns the following article/manuscript:

Title: Nudging and Self-Efficacy Intervention for Long-Term Unemployed

Authors: Jonas Fluchtmann, Alexander Koch, Michael Rosholm

The article/manuscript is: Published □ Accepted □ Submitted □ In preparation ☑

If published, state full reference:

If accepted or submitted, state journal:

Has the article/manuscript previously been used in other PhD or doctoral dissertations?

No ☑ Yes □ If yes, give details:

The PhD student has contributed to the elements of this article/manuscript as follows:

A. Has essentially done all the work
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Date | Name           | Signature
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20.8.19 | MICHAEL ROUSHOLM | Rosholm

In case of further co-authors please attach appendix

Date: 20-08-2019

Signature of the PhD student

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