Providing Advice to Job Seekers at Low Cost: An Experimental Study on On-Line Advice.

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Abstract

We develop and evaluate experimentally a novel tool that redesigns the job search process by providing tailored advice at low cost. We invited job seekers to our computer facilities for 12 consecutive weekly sessions to search for real jobs on our web interface. For half, instead of relying on their own search criteria, we use readily available labor market data to display relevant alternative occupations and associated jobs. The data indicates that this broadens the set of jobs they consider and increases their job interviews especially for participants who otherwise search narrowly and have been unemployed for a few months.

Keywords: Online job search, occupational breadth, search design.

JEL Codes: D83, J62, C93

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1 Introduction

Getting the unemployed back into work is an important policy agenda and a mandate for most employment agencies. In most countries, one important tool is to impose requirements on benefit recipients to accept jobs beyond their occupation of previous employment, at least after a few months.[1] Yet there is little guidance how they should obtain such jobs and how one might advise them in the process. This reflects the large literature on active labor market policies which is predominantly silent about the effective provision of job search advice, where most studies do not distinguish between advice and enforcement. In their meta-study on active labor market policies Card et al. (2010) merge "job search assistance or sanctions for failing to search" into one category.[2] Ashenfelter et al. (2005) assert a common problem that experimental designs "combine both work search verification *and* a system designed to teach workers how to search for jobs" so that it is unclear which element generates the documented success. Only few studies, reviewed in the next section, have focused exclusively on providing advice, mostly through labor-intensive counselling on multiple aspects of job search. Our study aims to contribute by providing and assessing low-cost, automated occupational advice to job seekers.

Even before evaluating the effects of advice on job search, a prime order question is what advice should be provided and how? In most countries, the provision of advice is usually done by trained advisors who meet job seekers on a regular basis, yet financial constraints often mean that such advice can only be limited in scope. Our first contribution is to propose an innovative low-cost way of providing tailored advice to job seekers online. It has long been argued that occupational information is something job seekers have to learn.[3] Recent evidence both for the US and the UK shows a pronounced occupational mismatch (Sahin et al. (2014), Patterson et al. (2016)): job seekers search in occupations with relatively few available jobs while at the same time other occupations with relatively more jobs are available but attract little interest. This "mismatch" has seen a further persistent increase since the great recession. Incomplete information could be a contributor if job seekers do not fully know which occupations currently have favorable conditions and whether their skills allow them to transit there. The tool we propose aims to address this by suggesting occupations (and shows the jobs that are currently available in them) using an algorithm based on representative labor market statistics. In a nutshell, it recommends additional occupations in which relevant other job seekers have successfully found jobs and where skill transferability is high, and visualises where market tightness is favorable.

Our second contribution is to evaluate how the advice provided through our tool affects job search behavior, i.e., to see if and how job seekers adjust their job search strategies in response to the suggestions they receive and whether this affects job interviews. To do this, we conduct a randomized study in a highly controlled and replicable environment. We recruited job seekers in Edinburgh from local Job Centres and transformed the experimental laboratory into a job search facility resembling those in "Employability Hubs" which provide computer access to job seekers throughout the city.

[1] See Venn (2012) for an overview of requirements across OECD countries.

[2] See the clarification in Card et al. (2009), p. 6.

[3] For example, Miller (1984), Neal (1999), Gibbons and Waldman (1999) , Gibbons et al. (2005), Papageorgiou (2014) and Groes et al. (2015) highlight implications of occupational learning and provide evidence of occupational mobility consistent with a time-consuming process of gradual learning about the appropriate occupation.

Participants were asked to search for jobs via our search platform from computers within our laboratory once a week for a duration of 12 weeks. The main advantage of this “field-in-the-lab” approach is that it allows us to obtain a complete picture of the job search process. Not only do we observe participants’ activities on the job search platform, such as the criteria they use to search for jobs and which vacancies they consider; but we also collect information via weekly surveys on which jobs they apply for, whether they get interviews and job offers. Furthermore, we also collect information about other search activities that job seekers undertake outside the job search platform, which is important if one is worried that effects on any one search channel might simply induce shifts away from other search channels. This allows us to have measures of total search effort and total job interviews that include such effects. These are key advantages of this approach that complement the alternatives reviewed in the next section: Studies that rely on data from large on-line job search platforms typically do not have information on activities outside the job search platform nor on job search success. They also currently lack a randomized design. Studies that use administrative data usually only have information about final outcomes (i.e. job found) but know little about the job search process. Because of the logistics required for our field-in-the-lab setup, our sample is limited to 300 participants. As a twelve week panel this is a large number for experimental work but limited relative to usual labor market studies, with associated limits in terms of power. Since it is the first study on the use of on-line advice, we found that the advantages warranted this approach.

Most of the literature in labor economics focuses on evaluating interventions that have been designed by policy makers or field practitioners. We add to this tradition here, not only by evaluating a novel labor market intervention, but also by leading the design of the intervention itself, using insights from labor economics to integrate existing labor market data right into a job search platform. To our knowledge our study is the first to use the expanding area of online search to provide advice by re-designing the jobs search process on the web, and allows for a detailed analysis of the effects on the job search “inputs” in terms of search and application behavior and the amount of interviews that participants receive.

Internet-based job search is by now one of the predominant ways of searching for jobs (Kuhn and Mansour (2014)). In the UK where our study is based, roughly two thirds of both job seekers and employers now use the internet for search and recruiting (ONS (2013), Pollard et al. (2012)). We set up two search platforms for internet-based job search that access the database of live vacancies of Universal Jobmatch, the official job search platform provided by the UK Department of Work and Pensions, which features a vacancy count at over 80% of the official UK vacancies. One platform replicates “standard” designs where job seekers themselves decide which keywords and occupations to search for, similar to interfaces used on Universal Jobmatch and other commercial job search sites. The second “alternative” platform provides targeted occupational advice. It asks participants which (target) occupation they are looking for - which often coincides with the occupation of previous employment. Then a click of a button provides them with two lists containing the most related occupations. The first is based on common occupational transitions that people who have worked in the target occupation make. The second contains occupations for which skill requirements are similar to that in the target occupation. Another click then triggers a consolidated query over all jobs that fall in any of these occupations within their geographic area. Participants can also take a look at maps to see in which occupations the ratio of unemployed workers to available jobs is more favorable - but data availability limits this to aggregated occupational groups. The maps provide direct information on the competition for jobs in an occupation, skill transferability provides information on the occupations in which the job seeker has realistic chances to fulfill the needs of a job opening, and information on successful transitions combines both because successful transitions require the availability of jobs in the new occupation and the skills to secure those jobs. The benefit of this intervention is that it provides job search advice in a highly controlled manner based on readily available statistical information, entails only advice and no element of coercion (participants were free to continue with the "standard" interface if they wanted to) and constitutes a low-cost intervention.

Job search occurs precisely because people lack relevant information that is costly and time-consuming to acquire. The main benefit of the internet is the ability to disseminate information at lower cost. In our study we investigate the following hypotheses. First, whether it stimulates job seekers to consider a wider set of occupations than they would normally do. Second, whether this broader search leads to more job interviews. While we expect both to be the case for job seekers that would normally search over a narrow set of occupations, it might not be the case for those that already explore many occupations. In particular, our set of suggestions might be smaller than what they normally consider, thereby making their search more narrow. How this affects job interviews depends on how they re-target their job search effort. We also expect a stronger impact of the intervention on job seekers that have been unemployed longer, as they might be more open to new suggestions. While these predictions arise naturally, we provide an illustrative theory model that lays out these considerations in Section 6. We test these predictions relative to the obvious null hypothesis (that there will be no effect of the intervention). This might be the case if the information that we provide is already known to job seekers or if the real problem is incentives to search rather than information.

All participants searched with the standard interface for the first three weeks, which provides a baseline on how participants search in the absence of our intervention. After three weeks, half of the participants was offered to try the alternative interface. We report the overall impact on the treatment group relative to the control group. Overall, we find that our intervention exposes job seekers to jobs from a broader set of occupations, increasing our measure of breadth by 0.2 standard deviations (which corresponds to the broadening that would occur naturally over three months of unemployment). Job applications become broader, and the total number of job interviews increases by 44%. These effects are driven predominantly by job seekers who initially search narrowly. These narrow searchers experience a two-fold increase in total job interviews (compared to similarly narrow searchers in the control group). Among those, the effects are mostly driven by those with above-median unemployment duration (more than 80 days), for whom the effects on interviews are even larger. Since we collected information on job interviews obtained through other channels, we can assess possible spillovers. We find positive effects for such other channels, which indicates that our information is helpful beyond the search on our particular platform. In fact, the statistical significant impact on job interviews is driven by significantly larger reported interviews due to search outside the lab. This re-enforcing effect is in contrast to crowding-out found in studies on monitoring and sanctions which led to offsetting reductions in non-monitored activities (Van den Berg and Van der Klaauw (2006)).

We find similar overall patterns across a number of robustness checks in terms of empirical specifications. Significance does vary somewhat with the specification and outcome variable. Most robust are the significant increase in the occupational breadth of search and the increase in interviews for initially-narrow job seekers. We do find heterogeneity in effects. For example, initially-broad job seekers become more narrow in their search and we find no sign of increased interviews. This group also uses the new interface less. The fact that some individuals search broader initially — e.g., because they have less specialized skills as in Moscarini (2001) — should not be surprising. They might already search in many occupations beyond their most preferred one, so recommendations from the alternative interface may be less valuable. If they do use it and new information ends up moving their perceived skills further apart, they become less broad.[4] The heterogeneity in adoption and impact between different groups provides one reason why overall effects are weaker and lack robustness. While we do not find any significant negative effects of interviews for any subgroup some point estimates remain economically sizeable. This warrants further analysis and caution, and it might be promising to target advice to particular subgroups such as those who otherwise search narrowly and experienced somewhat longer unemployment. This is particularly interesting because targeting could be included directly into an online advice tool. Moreover, if the effects are positive either overall or for a targeted subgroup, the near zero marginal costs of our type of intervention should make it an attractive policy tool.[5]

Yet, any of these conclusions needs to be viewed with caution. Apart from concerns about the power of our study, a true cost-benefit analysis would need further evaluation of effects on job finding probabilities as well as on whether additional jobs are of similar quality (e.g. pay similarly and can be retained for similar amounts of time). On that point, our approach shares similarities with the well-known audit studies (e.g. Bertrand and Mullainathan (2004)) on discrimination. The main outcome variable in these studies is usually the employer call-back rate rather than actual hiring decisions. As we elaborate in Section 5, it is evident that our study was not intended to pick up effects on job finding because of its size compared to the very low baseline rate of job finding. We find indeed no indication of increased job finding - even in point estimates (though also no significant difference between the point estimate for job finding and the large positive impact on job interviews). We acknowledge that this might not only be due to power issues, though. For example, the conversion rates of interviews into jobs in broader occupations could be lower.[6] A larger-scale assessment would be necessary here. Moreover, a broader roll-out in different geographic areas would also be needed to uncover any general equilibrium effects, which could reduce the effects if search by some job seekers negatively affects others, or could boost the effects if firms react to more efficient search with more job creation. Such general equilibrium effects may be important (as highlighted by Crépon et al. (2013) and Gautier et al. (2018)). We hope that future work with conventional large-scale search providers will marry the

[4]If they are already nearly indifferent between occupations, information can only increase differences. We lay this out in our own toy model that includes learning.

[5] Designing the alternative interface cost £20,000, and once this is programmed, rolling it out more broadly would have no further marginal cost of an existing platform such as Universal Jobmatch.

[6]For example, Moscarini (2001) outlines a model where those who search narrow have particular advantages in those narrow sectors which would not extend equally to search over a broader set of occupations. This might be reflected only in lower interview rates, but could conceivably also affect the conversion rates.

benefits of our approach with their large sample sizes.

The essence of our findings can be captured in a simple learning theory of job search that is presented in the pan-ultimate section. It also exposes why narrow individuals with slightly longer unemployment duration might be particularly helped by our intervention. In essence, after losing their job individuals might initially search narrowly because jobs in their previous occupation appear particularly promising. If the perceived difference with other occupations is large, our endorsement of some alternative occupations does not make up for the gap. After a few months, unsuccessful individuals learn that their chances in their previous occupation are lower than expected, and the perceived difference with other occupations shrinks. Now alternative suggestions can render the endorsed occupations attractive enough to be considered. Our intervention then induces search over a larger set of occupations and increases the number of interviews. One can contrast this with the impact on individuals who already search broadly because they find many occupations roughly equally attractive. They can rationally infer that the occupations that we do not endorse are less suitable, and they stop applying there to conserve search effort. Their breadth declines, but effects on job interviews are theoretically ambiguous because search effort is better targeted, which might be the reason for the insignificant effects on job interviews for this group in our empirical analysis.

The subsequent section reviews related literature. Section 3 outlines how our study is set up. Section 4 sets the stage by providing basic descriptives about the job search process and the subject pool, covering also issues of representativeness, sample balance, and attrition. Section 5 assesses the impact of our intervention within our main empirical specification as well as in a number of robustness checks. Section 6 uses a simple model to illustrate the forces that might underlie our findings, and the final section concludes.

2 Related Literature

As mentioned, most studies on job search assistance evaluate a combination of advice and monitoring/sanctions. An example in the UK is the work by Blundell et al. (2004) that evaluates the New Deal for the Young Unemployed. The program instituted bi-weekly meetings with a personal adviser to "encourage/enforce job search". The authors establish significant impact of the program, but cannot distinguish whether "assistance or the "stick" of the tougher monitoring played the most important role" [p. 601]. More recently, Gallagher et al. (2015) of the UK government's Behavioral Insights Team undertook a randomized trial in Job Centres that re-focuses the initial meeting on search planning, introduced goal-setting but also monitoring, and included resilience building through creative writing. They find positive effects of their intervention, but cannot attribute it to the various elements.[7] Their study indicates the potential of additional information provision.

Despite the fact that a lack of information is arguably one of the key frictions in labor markets, we are only aware of a few studies that exclusively focus on the effectiveness of information interventions in the labor market.[8] Prior to our study the focus has been on the provision of counseling services by

[7] This resembles findings by Launov and Waelde (2013) that attribute the success of German labor market reforms to service restructuring (again both advice and monitoring/sanctions) with non-experimental methods.

[8] There are some indirect attempts to distinguish between advice and monitoring/sanction. Ashenfelter et al. (2005)

traditional government agencies and by new entrants from the private sector. Behaghel et al. (2014) and Krug and Stephan (2013) provide evidence from France and Germany that public counseling services are effective and outperform private sector counseling services. The latter appear even less promising when general equilibrium effects are taken into account (Crépon et al. (2013)). Bennmarker et al. (2013) finds overall effectiveness of both private and public counseling services in Sweden. The upshot of these studies is their larger scale and the access to administrative data to assess their effects. The downside is the large costs that range from several hundred to a few thousand Euro per treated individual, the multi-dimensional nature of the advice and the resulting "black box" of how it exactly affects job search. Our study can be viewed as complementary. It involves nearly zero marginal cost, the type of advice is clearly focused on occupational information, it is standardized, its internet-based nature makes it easy to replicate, and the detailed data on actual job search allow us to study the effects not only on outcomes but also on the search process.

Contemporaneously, Altmann et al. (2018) analyze the effects of a brochure that they sent to a large number of randomly selected job seekers in Germany. It contained information on i) labor market conditions, ii) duration dependence, iii) effects of unemployment on life satisfaction, and iv) importance of social ties. They find some positive impact, but only for those at risk of long-term unemployment. In our intervention we also find the strongest effects for individuals with longer unemployment duration, but even overall effects are significant and occur much closer in time to the actual provision of information. Their study has low costs of provision and is easily replicable. On the other hand, it is not clear which of the varied elements in the brochure drives the results, there are no intermediate measures on how it affects the job search process, and the advice is generic to all job seekers.

Our study is also complementary to a few recent studies which analyze data from commercial online job boards. Kudlyak et al. (2014) analyze U.S. data from Snagajob.com and find, among other things, that job seekers lower their aspirations over time. Faberman and Kudlyak (2014) analyze the same data source and show that there is little evidence that declining search effort causes the declining job finding rate. The data lacks some basic information such as (un)employment status, but they document some patterns related to our study: Occupational job search is highly concentrated and absent of any exogenous intervention it broadens significantly but only slowly over time.

Marinescu and Rathelot (2018) investigate differences in market tightness as a driver of aggregate unemployment using data from Careerbuilder.com and concur with earlier work that differences in market tightness are not a large source of unemployment. In their dataset search is rather concentrated, with 82% of applications staying in the same city.[9] Using the same data source, Marinescu (2017) investigates equilibrium effects of unemployment insurance. Marinescu and Wolthoff (2016) use data from Careerbuilder.com and document that job titles are informative above and beyond wage and

cite experimental studies from the US by Meyer (1995) which have been successful but entailed monitoring/sanctions as well as advice, and they then provide evidence from other interventions that monitoring/sanctions are ineffective in isolation. This leads them to conclude indirectly that the effectiveness of the first set of interventions must be due to the advice. Yet subsequent research on the effects of sanctions found conflicting evidence: e.g., Micklewright and Nagy (2010) and Van den Berg and Van der Klaauw (2006) also find only limited effects of increased monitoring, while other studies such as Van der Klaauw and Van Ours (2013), Lalive et al. (2005) and Svarer (2011) find strong effects.

[9]Based on Figure 5 in the 2013 working paper. The "distaste" for geographical distance backed out in this work for the US is lower than that backed out by Manning and Petrongolo (2017) from more conventional labor market data in the UK, suggesting that labor markets in the UK are even more local.

occupational information for attracting applications. As mentioned, these studies have large sample size and ample information of how people search on the particular site, but none involves a randomized design nor do they have information on other job search channels. Also, their focus is not on advice.

Our weekly survey of job search activity outside the lab is related to the seminal study by Krueger and Mueller (2016) that conducted weekly interviews regarding reservation wages with a panel of job seekers in the US over the course of half a year. Our recommendation to target occupational information to job seekers that otherwise search narrowly is in the spirit of recent discussion of profiling in active labor market policy. Profiling singles out subsets of individuals for treatment according to a probabilistic assessment of the benefits (see, e.g., Berger et al. (2000) for a comprehensive discussion). Interestingly, in our environment the profiling could be integrated directly into a standard job search engine in which individuals first search "normally" and subsequently, depending on the breadth of their search, occupational information could be offered or not. To our knowledge, our study is the first that undertakes job-search platform design and evaluates it. While the rise in internet-based search will render such studies more relevant, the only other study of search platform design that we are aware of is Dinerstein et al. (2018), who study a change of the presentation of search results at the online consumer platform Ebay.

3 The Set-Up of the Study

Two main contributions underlie our study: first, we design of a novel online tool that provides labor market information that is readily available to researchers but usually not to job seekers. The aim is to make this available in an easily accessible cost-effective form and to enable a direct link to potential jobs. Second, we evaluate the new tool experimentally in a randomized controlled experiment for which we invited job seekers for a period of 12 weeks. We used a "standard" interface for comparison. We now describe the experimental design and provide descriptives on the sample and the job search process.

3.1 Description of the Advice Interface

We designed an on-line job search interface in collaboration with professional programmers from the IT Applications Team at the University of Edinburgh. The main feature of the interface is to provide a tailored list of suggestions of possible alternative occupations that may be relevant to job seekers, based on a preferred occupation that job seekers pre-specify (but can change at any time).

We use two methodologies to compile a list of suggested alternative occupations. The first methodology builds on the idea that successful labor market transitions experienced by people with a similar profile contain useful information about occupations that may be suitable alternatives to the preferred occupation: the fact that others found jobs there indicates that skills might be transferable and jobs available. It is based on the standard idea in the experimentation literature that others have already borne the cost of experimentation and found suitable outcomes.

To do this, we use information on labor market transitions observed in the British Household

Panel Survey and the national statistical database of Denmark (because of larger sample size).[10] Both databases follow workers over time and record in what occupation they are employed. We then match the indicated preferred occupation to the most common occupations to which people employed in the preferred occupation transition to.[11] This methodology has the advantage of being highly flexible and transportable. Many countries now have databases that could be used to match this algorithm. That is, the tool we propose can easily be replicated and implemented in many different settings.

The second methodology uses information on transferable skills across occupations from the US based website O*net, which is an online "career exploration" tool sponsored by the US department of Labor, Employment & Training Administration. For each occupation, they suggest up to 10 related occupations that require similar skills. We retrieved the related occupations and presented the ones related to the preferred occupation as specified by the participant. This provides information on skill transferability only, not on job availability.

The tool was directly embedded in the job search interface. That means that once participants had specified their preferred occupation, they could click "Save and Start Searching" and were taken to a new screen where a list of suggested occupations was displayed. The occupations were listed in two columns. The left column suggests occupations based on the first methodology (labor market transitions) and the right column suggests occupations based on the second methodology (O*net related occupations). A screenshot is presented in Online Appendix 8.5.6. Participants were informed of the process by which these suggestions came about, and could select or unselect the occupations they wanted to include or exclude in their search. By default all were selected. By clicking the "search" button, the program searched through the same underlying vacancy data as in the control group.[12]

In addition, the interface provides visual information on the labor market tightness for broad occupational categories across regions in Scotland. The goal here is to provide information about how competitive the labor market is for a given set of occupations - which is closest to the idea of search mismatch in Sahin et al. (2014) and provides information on the competition for jobs but not on skill transferability. We constructed "heat maps" that use labor market statistics for Scotland and indicate visually (with a colored scheme) where jobs may be easier to get because there are many jobs relative to the number of interested job seekers. These maps were created for each broad occupational category (two-digit SOC codes).[13] Participants could access the heat maps by clicking on the button "heat map" which was available for each of the suggested occupations based on labor market transitions.

In principle this tool can be used with any database of vacancies that includes occupational codes; for our experimental approach we combine it with one of the largest databases in the UK.

[10]The name of the database is IDA - Integrated Database for Labour Market Research administered by Statistics Denmark. We are grateful to Fayne Goes for providing us with the information.

[11]For each occupation, we created a list of three to five common transitions using information from both datasets. The list contained all occupations that occur in the top-10 common transitions in both datasets (if there were more than five of these, we selected the five highest occurring occupations). In case this resulted in less than 3 occupations, we added the highest ranked transitions from each dataset until the list contained at least three occupations.

[12]Occupations in O*net have a different coding and description and have a much finer categorization than the three-digit occupational code available in the British Household Panel Survey (BHPS) and in Universal Jobmatch. We therefore asked participants twice for their preferred occupation, once in O*net form and BHPS form. The query on our vacancy database relies on keyword search, taking the selected occupations as keywords, to avoid problems of differential coding.

[13]These heat maps are based on statistics provided by the Office for National Statistics, (NOMIS, claimant count, by occupations and county, see https://www.nomisweb.co.uk/). An example of a heat map is in Online Appendix 8.5.6.

3.2 Control Treatment: Standard Search Interface

We designed a standard job search engine that replicates the search options available at the most popular search engines in the UK (such as Monster.com and Universal Jobmatch). As in the treatment group this allowed us to record precise information about how people search for jobs (what criteria they use, how many searches they perform, what vacancies they click on and what vacancies they save), as well as collecting weekly information (via the weekly survey) about outcomes of applications and search activities outside the laboratory.

A screenshot of the standard search interface is provided in Online Appendix 8.5.6. Participants can search using various criteria (keywords, occupations, location, salary, preferred hours), but do not have to specify all of these. Once they have defined their search criteria, they can press the search button at the bottom of the screen and a list of vacancies fitting their criteria will appear. The information appearing on the listing is the posting date, the title of the job, the company name, the salary (if specified) and the location. They can then click on each individual vacancy to reveal more information. Next, they can either choose to "save the job" (if interested in applying) or "not save the job" (if not interested). If they choose not to save the job, they are asked to indicate why they are not interested in the job from a list of possible answers.

As in most job search engines, they can modify their search criteria at any point and launch a new search. Participants had access to their profile and saved vacancies at any point in time outside the laboratory, using their login details. They could also use the search engine outside the laboratory, but this turned out to be only a very small share compared to the search activities performed in the lab.

The key feature of this interface is that job seekers themselves have to come up with the relevant search criteria, as is shared by commercial and public jobsearch sites at the time of our study. In order for the study to provide a valid environment to study search behavior, it is important that the platforms are used seriously and are not viewed as inferior to alternative search environments. In an exit survey we asked all users to evaluate the interface and found that it was evaluated very positively. The responses to the question "How would you rate the search interface compared to other interfaces?" were: Poor (7%) Below average (7%) Average (14%) Good (46%) Very Good (26%). These responses were very similar across the two interfaces.

3.3 Vacancies

In order to provide a realistic job search environment, both search interfaces access a local copy of the database of real job vacancies of the government website Universal Jobmatch. This is a very large job search website in the UK in terms of the number of vacancies, which is crucial because results can only be trusted to resemble natural job search if participants use the lab sessions for their actual job search. The large set of available vacancies combined with our carefully designed job search engine assures that the setting was as realistic as possible.

Each week there are between 800 and 1600 new vacancies posted in our dataset in Edinburgh (see Online Appendix 8.1 for further details). Furthermore, there is a strong correlation between vacancy posting in Edinburgh and the UK. Comparing the number of vacancies in our database with he official national vacancy statistics suggests that our coverage is above 80%, which is a very extensive coverage compared to other online platforms.[14] It is well-known that not all vacancies on online job search platforms are genuine, so the actual number might be somewhat lower.[15] We introduced ourselves a small number of additional posts (below 2% of the database) for a separate research question (addressed in Belot et al. (2018)).[16]

3.4 Job Seekers

To study the effect of information provision through the new interface, we recruited job seekers in the area of Edinburgh in two waves: wave 1 was conducted in September 2013 and wave 2 in January 2014. Labor market conditions in Edinburgh are broadly consistent with national ones, displaying monotonically decreasing unemployment between 2012 and 2015 for both (see Online Appendix 8.2).

The eligibility criteria for participating to the study were: being unemployed, searching for a job for less than 12 weeks (a criterion that we did not enforce), and being above 18 years old.[17] We imposed no further restrictions in terms of nationality, gender, age or ethnicity. We aimed to recruit 150 participants per wave, which constitutes about 2% of the stock of JSA claimants.[18]

As a background on the institutional setting, individuals on job seeker allowance (JSA) receive between £52.25 and £72 per week depending on age. Eligibility depends on sufficient contributions during previous employment or on sufficiently low income.[19] This is linked to the requirement to be available and actively looking for work. In practice, this implies committing to agreements made with a work coach at the job centre, such as meeting the coach at regular (usually bi-weekly) intervals, applying to suggested vacancies, or participating in suggested training. They are not entitled to reject job offers because they dislike the occupation or the commute, except that the work coach can grant a period of up to three months to focus on offers in the occupation of previous employment, and required commuting times are capped at 1.5 hours per leg. The work coach can impose sanctions on benefit payments in case of non-compliance to any of the criteria.

We obtained the collaboration of several local public unemployment agencies (called Jobcentre

[14] For comparison, the largest US jobsearch platform has 35% of the official vacancies; see Marinescu (2017), Marinescu and Wolthoff (2016) and Marinescu and Rathelot (2018). The size difference might be due to the fact that the UK platform is run by the UK government.

[15] For Universal Jobmatch fake vacancies covering 2% of the stock have been reported, posted by a single account (Channel 4 (2014)) and speculations of higher total numbers of fake jobs circulate (Computer Business Review (2014)). Fishing for CV's and scams are common, including on Carreerbuilder.com (The New York Times (2009)) and Craigslist.

[16] Participants were fully informed about this. They were told that "we introduced a number of vacancies (about 2% of the database) for research purposes to learn whether they would find these vacancies attractive and would consider applying to them if they were available". They were asked for consent and were informed if they expressed interest in them before any actual application costs were incurred, so any impact was minimized. This small number is unlikely to affect job search, and there is no indication of differential effects by treatment group: In an exit survey the vast majority of participants (86%) said that this did not affect their search behavior, and this percentage is not statistically different in the treatment and control group (p-value 0.99). This is likely due to the very low numbers of fake vacancies and to the fact that fake advertisements are common (see footnote 15) and discussed in search guidelines (e.g., Joyce (2015)).

[17] We drop one participant from the sample because this participant had been unemployed for over 30 years and was therefore an extraordinary outlier. We also exclude two participants who showed up once without searching and never returned. Including them in the analysis has no effects on the qualitative findings.

[18] The stock of JSA claimants In Edinburgh during our study is 9,000 with a monthly inflow of 1,800, approximately.

[19] Benefits of £56.25 per week apply to those aged up to age 24, and £72 per week for those aged 25 and older. Contribution-based JSA lasts for a maximum of 6 months. Afterwards - or in the absence of sufficient contributions - income-based JSA applies, with is identical to weekly benefits but with extra requirements. Once receiving JSA, the recipient is not eligible for income assistance, however they may receive other benefits such as housing benefits.

Plus) to recruit job seekers on their premises during a two-week window prior to each wave. Since most individuals on job seeker allowance meet their advisers bi-weekly, this gives us a chance to encounter most of them. The counselors were informed of our study and were asked to advertise the study. We also placed advertisements at public places in Edinburgh (including libraries and cafes) and posted a classified ad on an on-line platform (Gumtree). Sign up and show up rates are presented Table 14 in Online Appendix 8.3. Of all participants, 86% were recruited in the Jobcentres. Most of the other participants were recruited through our ad on Gumtree. Out of the visitors at the Jobcentres that we could talk to and who did not indicate ineligibility, 43% percent signed up. Out of everyone that signed up, 45% showed up in the first week and participated in the study, which is a substantial share for a study with voluntary participation. These figures display no statistically significant difference between the two waves of the study.

We also conducted an online study in which job seekers were asked to complete a weekly survey about their job search. These participants did not attend any sessions, but simply completed the survey for 12 consecutive weeks. This provides us with descriptive statistics about job search behavior of job seekers in Edinburgh and it allows us to compare the online participants with the lab participants. These participants received a £20 clothing voucher for each 4 weeks in which they completed the survey. The online participants were recruited in a similar manner as the lab participants.[20] The sign up rate at the Jobcentres was slightly higher for the online survey (58%). However, only 21% completed the first survey, which is partly caused by one-fourth of the email addresses not being active.

In Section 4.1 we discuss in more detail the representativeness of the sample, by comparing the online and the lab participants with population statistics.

3.5 Experimental Procedure

Job seekers were invited to search for jobs once a week for a period of 12 weeks (or until they found a job) in the computer facilities of the School of Economics at the University of Edinburgh. We conducted sessions at six different time slots, on Mondays or Tuesdays at 10 AM, 1 PM or 3:30 PM. Participants chose a slot at the time of recruitment and were asked to keep the same time slot for the twelve consecutive weeks.

Participants were asked to search for jobs using our job search engine for a minimum of 30 minutes.[21] Afterwards they could continue to search or use the computers for other purposes such as updating their CV or applying for jobs. They could stay up to two hours. No additional job search support was offered. Participants received a compensation of £11 per session attended (corresponding to compensation for meal and travel expenses as advized by Jobcentre Plus) and we provided an additional £50 clothing voucher for job market attire for participating in 4 sessions in a row. [22]

Participants would register in an office at the beginning of each session and were then told to sit at one of the computer desks in the computer laboratory. Before the first session they received a

[20] Participants were informed of only one of the two studies, either the on-site study or the on-line study. The did not self-select into one or the other.

[21] The 30 minute minimum was chosen as a trade-off between on the one hand minimizing the effect of participation on the natural amount of job search, while on the other hand ensuring that we obtained enough information.

[22] Our study did not affect the entitlements or obligations that participants face at the local Jobcentre. All forms of compensation effectively consisted of subsidies, i.e. they had no effect on the allowances the job seekers were entitled to.

Table 1: Randomization scheme

	Monday			Tuesday		
	10 AM	1 PM	3:30 PM	10 AM	1 PM	3:30 PM
Wave 1	Control	Treatment	Control	Treatment	Control	Treatment
Wave 2	Treatment	Control	Treatment	Control	Treatment	Control

description of the study and a consent form (see Online Appendix 8.5.1). We handed out instructions on how to use the interface (see Online Appendix 8.5.2). We had assistance in the laboratory to answer clarifying questions. Participants were explicitly asked to search as they normally would.

Once they logged in, they were automatically directed to our own website. They were first asked to fill in a survey. The initial survey asked about basic demographics, employment and unemployment histories as well as beliefs and perceptions about employment prospects, and measured risk and time preferences. From week 2 onwards, they only had to complete a short weekly survey asking about job search activities and outcomes. For vacancies saved in their search in our facility we asked about the status (applied, interviewed, job offered). We asked similar questions about their search through other channels than our study. The weekly survey also asked participants to indicate the extent to which they had personal, financial or health concerns (on a scale from 1 to 10). The complete survey questionnaires can be found in the Online Appendices 8.5.4 and 8.5.5.

After completing the survey, the participants were re-directed towards our search engine and could start searching. A timer located on top of the screen indicated how much time they had been searching. Once the 30 minutes were over, they could end the session. They would then see a list of all the vacancies they had saved which could also be printed. This list of printed vacancies could be used as evidence of required job search activity at the Jobcentre. We received no information about the search activities or search outcomes from the Jobcentres. This absence of linkage was important to ensure that job seekers did not feel that their search activity in our laboratory was monitored by the employment agency. They could then leave the facilities and receive their weekly compensation. Participants could still access our website from home, for example in order to apply for the jobs they had found.

3.6 Randomization

All participants used the standard interface in the first 3 weeks of the study. Half of the participants was offered the "alternative" interface, which incorporates our suggestions tool, from week 4 onwards. Participants were randomized into control (no change in interface) and treatment group (alternative interface) based on their allocated time slot. We randomized the first time slot into treatment and control, and assigned each following time slot in an alternating pattern, to avoid any correlation between treatment status and a particular time slot. Each time slot that was allocated to control (treatment) in the first wave was assigned to treatment (control) in the second wave (see Table 1). The change of interface was not previously announced, apart from a general introductory statement to all participants that included the possibility to alter the search engine over time.

Participants received a written and verbal instruction of the alternative interface (see Online Appendix 8.5.3), including how the recommendations were constructed, in the fourth week. For them, the new interface became the default option when logging on, but it was made clear that using the new interface was not mandatory. Rather, they could simply switch back and forth between interfaces. This ensures that we did not restrict choice, but rather expanded their means of searching for a job.

3.7 Measures of Job Search

The main goal of the study is to evaluate how tailored advice affects job search strategies. Our data allow us to examine each step of the job search process: the listing of vacancies to which job seekers are exposed, the vacancies they apply to and the interviews they receive. In the weekly survey that participants complete before starting to search, we ask about applications and interviews through channels other than our study. The intervention may affect these outcomes as well, since the information provided in the alternative interface could influence people's job search strategies outside the lab. Of course, ultimately one would also like to evaluate the effects on job finding and the characteristics of the job found (occupation, wage, duration, etc.), which would be important to evaluate the efficiency implications of such an intervention. This is however not the prime goal of this study and given the small sample of participants, we should be cautious when interpreting results on job finding as we discuss in a separate part in Section 5.5. We summarize the outcome variables of interest in Table 2. The main outcome variables relate to (1) listed vacancies, (2) applications and (3) interviews. The precise definition of each of these is presented next.

The most immediate measure of search relates to listed vacancies, i.e., all vacancies that appear on the participants' screen as a return to their search queries in a given week. For a given search query, up to 25 results are presented on the screen and only these are included in the set of listed vacancies.[23] If the query returned more vacancies, the participant can click to move to the next screen where again up to 25 vacancies are shown. These are added as "listed" for this week. This means that vacancies are only recorded as "listed" if the applicant has seen them on the screen. All our analyses are at the weekly level and, thus, we group listings from all search queries in a week together.[24] We note that listings are not mechanical even in the treatment group but, rather, remain an outcome of their choice: on the new interface users still decide how many pages of results to move through, which geographical radius to explore, how many recommended alternative occupations to keep, and how many preferred occupations and associated alternatives to explore in a given week - not to mention that participants can revert back to standard keyword search to explore some options more deeply (we document the use of each interface later on).

The second measure of search behavior relates to applications, which is a more direct measure of interest.[25] For each vacancy that was saved in the laboratory, we asked participants to indicate

[23] The default ordering of results is by posting date, but alternative orderings can be chosen such as location or salary.

[24] Since the alternative interface tends to return more search results (due to the additional suggested occupations), it may necessitate less search queries. For that reason the weekly analysis seems more appropriate compared to results at the level of an individual query. In a given week each vacancy is counted at most once.

[25] We also record "viewed vacancies" (vacancies that the job seeker clicks on in order to view all job details) and "saved vacancies", but we prefer to focus on applications as they constitute a more direct measure of interest. Results for viewed and saved vacancies are reminiscent of those for listed and applied vacancies and are omitted for brevity.

Table 2: Outcome variables

	Listed vacancies	Applications	Interviews
In lab:			
Number	√	√	√
Occupational breadth	√	√	
Geographical breadth	√	√	
Core/Non-core occ's			√
Outside lab:			
Number		√	√

whether they actually applied to it or not.[26] We can therefore precisely map applications to the timing of the search activity. This is important as there may be a delay between the search and the actual application; so applications that are made in week 4 and after could relate to search activity that took place before the actual intervention. For applications conducted based on search outside the laboratory, we do not have such precise information. We asked how many applications job seekers made in the previous week but we do not know the timing of the search activity these relate to. For consistency, we assume that the lag between applications and search activity is the same inside and outside the laboratory (which is one week) and assign applications to search activity one week earlier. As a result, we cannot use information on search activity in the last week of the experiment, as we do not observe applications related to this week.

For listed vacancies and applications we look at the number as well as measures of breadth (occupational and geographical). For occupational breadth we focus on the UK Standard Occupational Classification code (SOC code) of a particular vacancy, which consists of four digits.[27] The structure of the SOC codes implies that the more digits two vacancy codes share, the more similar they are. Our measure of diversity within a set of vacancies is based on this principle, defining for each pair within a set the distance in terms of the codes. The distance is zero if the codes are the same, it is 1 if they differ on the last digit, 2 if they differ on the last two digits, et cetera. This distance, averaged over all possible pairs within a set, is the measure that we use in the empirical analysis (we discuss robustness to alternative measures in Section 5.6). Note that this distance is increasing in breadth (diversity) of a set of vacancies. We compute this measure for the set of listed and applied vacancies in each week for each participant. For geographical breadth we use a simple measure. Since a large share of searches restricts the location to Edinburgh, we use the weekly share of a participant's searches that goes beyond Edinburgh as the measure of geographical breadth.[28]

Our third outcome measure is interviews - which is the measure most closely related to job prospects. As was done for applications, we assign interviews to the week in which the search activity was performed, and assign interviews through channels other than the lab to search activity two weeks earlier. As a result we do not use information on search activity in weeks 11 and 12 of the experiment,

[26]If they had not applied, they are asked whether they intended to apply and only if they answered affirmatively they were asked again the next week. A similar procedure was followed for interviews.

[27]The first digit defines the "major group" , the second the "sub-major group", the third the "minor group" and the fourth the "unit group". Examples are "Social science researchers" (2322) and "Call centre agents/operators" (7211).

[28]The surroundings of Edinburgh contain only small towns, the nearest city (Glasgow) takes 1-1.5 hours of commuting.

because for job search done in these weeks we do not observe interviews. The number of interviews is too small on average to compute informative breadth measures. As an alternative, we asked individuals at the beginning of the study about three "core" occupations in which they are looking for jobs, and we estimate separately the impact of the treatment for interviews in core and non-core occupations.

4 Descriptive Statistics on Job Seeker Characteristics and Job Search Behavior

This section provides descriptive statistics about the characteristics of the sample of job seekers in our study. We indicate how our experimental sample compares to the (limited) information we have on the overall set of JSA claimants in Edinburgh and to those participating in the online survey, and we demonstrate balance between treatment and control group. The control group faces no intervention throughout the study, and we document how they change their job search over time. For the treatment group we document to which extent they adopt the new interface. Finally, we discuss attrition.

4.1 Job Seeker Characteristics and Job Search History

Demographic variables, based on the first week baseline survey and presented in Table 3, show that 43% of the lab participants are female, the average age is 36 and 43% have some university degree. 80% classify themselves as 'white' and 27% have children. We can compare this to aggregate statistics about the population of job seekers available from The Office of National Statistics (NOMIS) where we truncate unemployment duration to obtain a sample with similar median.[29] Unfortunately this provides only few variables presented in the last column of the table. It indicates that we oversample women and non-whites, while the average age is very similar. Another comparison group are the participants in our online survey which arguably face a lower hurdle to participation in the study. Results are presented in the intermediate columns, and in column 7 the p-value of a two-sided t-test for equal means relative to the lab participants is shown. The online survey participants differ somewhat in composition: they are more likely to be female, are slightly younger and have less children.

The lower part of Table 3 shows variables related to job search history, also based on the first week baseline survey. The lab participants have on average applied to 64 jobs during the unemployment spell preceding the participation in our study. These led to 2.3 interviews and 0.42 job offers.[30] Only 20% received at least one offer. Mean unemployment duration at the start of the study is 260 days, while the median is 80 days. About three-fourth of the participants had been unemployed for less than half a year. Participants typically receive job seekers allowance and housing allowance, while the amount of other benefits received is quite low. The online survey participants are not significantly different on each of these dimensions.

To check the balance between treatment and control group we also report demographics and job

[29] Source: Office for National Statistics: NOMIS Official Labour Market Statistics. Dataset: Claimant Count conditional on unemployment duration$<$ 12 months, average over the duration of the study. Restricting attention to less than 12 months ensures similar median unemployment duration between the NOMIS query and our dataset.

[30] We censor the response to the survey question on the number of previous job offers at 10 and the question on interviews at 50.

Table 3: Characteristics of lab participants and online survey participants (based on the first week initial survey)

	Lab participants			Online survey			T-test[a]	Pop.[b]
	mean	min	max	mean	min	max	pval	
Demographics:								
gender (%)	43			52			.09	33
age	36	18	64	34	18	64	.08	35
high educ (%)	43			43			1.00	
white (%)	80			77			.43	89
number of children	.53	0	5	.28	0	2	.02	
couple (%)	23			23			.96	
any children (%)	27			23			.41	
Job search history:								
vacancies applied for	64	0	1000	75	0	1354	.53	
interviews attended	2.3	0	50	2.7	0	20	.42	
jobs offered	.42	0	8	.51	0	10	.52	
at least one offer (%)	20			24			.36	
days unempl. (mean)	260	1	5141	167	8	2929	.15	111
days unempl. (median)	80			118				81
less than 183 days (%)	76			75			.76	
less than 366 days (%)	85			91			.13	
job seekers allowance (£)	52	0	1005	58	0	280	.49	
housing benefits (£)	64	0	660	48	0	400	.36	
other benefits (£)	14	0	700	12	0	395	.81	
Observations	295			103				

[a] P-value of a t-test for equal means of the lab and online participants. [b] Average characteristics of the population of job seeker allowance claimants in Edinburgh over the 6 months of study. The numbers are based on NOMIS statistics, conditional on unemployment duration up to one year. [c] High educated is defined as a university degree.

search history separately by group in Table 4. Only one out of 19 variables - the number of children - displays significant differences between the groups. This indicates balance of the sample. Balance is further corroborated by the fact that also none of the 14 measures of search behavior during the first three weeks of the study shown in the lowest panel in Table 4 displays any significant differences. We discuss these further in the next subsection. A formal assessment of balance through a Holm-Bonferroni test across all 19 baseline variables or across all 33 variables including initial job search does not reject equality between the groups even at the 10% level.

4.2 Descriptives of Job Search Behavior During the Study

In terms of job search behavior in our study over the first three weeks, we find that the control group lists on average 493 vacancies, of which 25 are viewed, and 10 are saved (see third panel in Table 4). Out of these, participants report to have applied to 3 and eventually get an interview in 0.1 cases. Furthermore, they report about 9 weekly applications through channels outside our study, leading to 0.5 interviews on average. For the sets of listed vacancies and applications we compute a measure

Table 4: Characteristics of the treatment and control group

	Control group			Treatment group			T-test
	mean	min	max	mean	min	max	p-value
Demographics:							
female (%)	42			43			0.83
age	36	18	62	36	18	64	0.85
high educ [a] (%)	44			41			0.63
survey qualification level	4.2	1	8	4.4	2	8	0.36
white (%)	80			80			0.97
number of children	0.66	0	5	0.38	0	5	0.01
couple (%)	25			21			0.41
any children (%)	31			24			0.17
Job search history:							
expect job within 12 weeks (%)	0.59			0.58			0.93
vacancies applied for	75	0	1000	53	0	1000	0.18
interviews attended	2.4	0	50	2.2	0	50	0.68
jobs offered	0.37	0	5	0.48	0	8	0.43
at least one offer (%)	20			20			0.91
days unemployed (mean)	290	1	5028	228	1	5141	0.39
days unemployed (median)	81	1	5028	77	1	5141	
less than 183 days (%)	75			78			0.60
less than 366 days (%)	84			87			0.54
job seekers allowance (£)	49	0	225	56	0	1005	0.46
housing benefits (£)	65	0	600	62	0	660	0.90
other benefits (£)	9.7	0	280	18	0	700	0.41
Weekly search activities in weeks 1-3:							
listed	493	4.3	3049	493	1	1966	1.00
viewed	25	3	86	26	0	119	0.57
saved	10	0	65	11	0	79	0.54
applied	3.3	0	45	2.5	0	33	0.14
interview	0.098	0	3.3	0.083	0	1.5	0.66
applications other	9.3	0	68	7.4	0	37	0.13
interviews other	0.54	0	4	0.47	0	5	0.48
broadness listed[b]	3.2	0	3.7	3.3	1	3.7	0.50
broadness applied[b]	3	0	4	3.2	0	4	0.34
hours spent searching[c]	11	0.5	43	12	1	43	0.15
met caseworker (%)	0.32			0.28			0.48
Observations	152			143			

Demographics and job search history values are based on responses in the baseline survey from the first week of the study. Search activities are mean values of search activities over the first 3 weeks of the study. [a] High educated is defined as a university degree. [b] Occupational broadness, as defined in section 3.7. [c] The number of hours spent on job search per week, as filled out in the weekly survey, averaged over week 2 and 3.

Table 5: Job search activity over time (only control group)

	(1) Hours search per week	(2) Breadth of listed vac.	(3) Number of listed vac.	(4) Breadth of applications	(5) Number of applications	(6) Number of interviews
Time trend	0.040	0.015***	8.91**	-0.0052	-0.15**	-0.0072*
	(0.063)	(0.0048)	(4.10)	(0.014)	(0.059)	(0.0043)
Individual FE	yes	yes	yes	yes	yes	yes
Mean dep. var.	12.2	3.29	536.1	3.07	3.38	0.082
Weeks	1-12	1-12	1-12	1-11	1-11	1-10
N	1040	1193	1196	504	1125	1049

All regressions contain only control group individuals. "Time trend" is a linear weekly trend. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

of occupational breadth (as described in subsection 3.7), of which the average values are also shown. Participants in the control group report 11 hours of weekly job search in addition to our study. In the weekly survey, participants were also asked to rate to what extent particular problems were a concern to them. On average, health problems are not mentioned as a major concern, while financial problems and strong competition in the labor market seem to be important. Finally, about 30% met with a case worker at the Jobcentre in a particular week.

Comparing job search behavior and outcomes after week three between treatment and control group is at the heart of the empirical assessment of the next section. Here we simply report some additional observations regarding job search behavior to provide some background.

First, about a third of job seekers search for jobs in the exact same occupation of their previous employment. We compare the occupations that they list in their employment history (obtained in the initial survey) with the three "preferred occupations" that they list when asked in which occupations they would prefer to find a job.[31] We find that for 35%, all of their previous occupations are now listed as preferred occupations. For 27%, some of their previous occupations are listed as preferred occupations, and for 38%, none of their previous occupations are indicated to be preferred occupations.

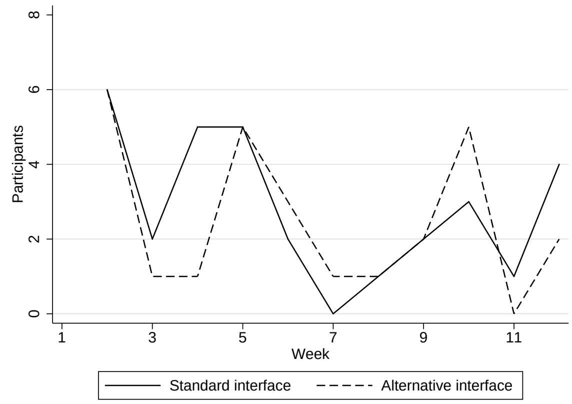
Second, most applications go to recently posted vacancies. The median age of a vacancy at the time of an application is 12 days. Of all applications to jobs from our search interface, 85% goes to a vacancy that is at most three weeks old at the time the application is reported.

Third, the breadth and the number of vacancies that job seekers list increase over time, while the numbers of applications and interviews decrease over time. The weekly increase in breadth of listed vacancies is about 2.2 % of a standard deviation. There is no significant trend for breadth of applications (though this is imprecisely measured), nor on weekly hours spent on job search. These results follow from regressing the outcome on a linear (weekly) time trend using only the control group and including individual fixed effects. The focus on the control group is to avoid any confounding with the treatment. The results are presented in Table 5.[32]

[31] These 3 preferred occupations are good proxies for actual search. When comparing them to the first occupation that is specified in the alternative interface we find that for 51% of the job seekers this first occupation is one of the three preferred occupations.

[32] One may worry that the results in Table 5 are affected by dynamic selection as some participants leave the study over time. In Table 15 in the Online Appendix we show the results for the subsample of participants that are still present in the final weeks of the study (i.e., attended at least one session in week 10, 11 or 12), and results are very robust.

Figure 1: Attrition of participants in the standard and alternative interface groups (excluding job finding)



Fourth, we investigate whether the requirement to search on our platform has an effect on job search per se by comparing the patterns we just described for the control group to those for online participants who were only surveyed, including on the weekly number of job applications and interviews. Lab participants report both the numbers associated with the search in the lab and through other channels. Both groups receive no information information but one has to come physically to our lab to search on our standard interface. Lab participants report a similar number of job applications through other channels as online participants, but together with the applications associated with search in the lab the total number of applications by lab participants is significantly higher in most weeks (see figures 10 and 11 in the Online Appendix). This difference could be the result of additional search induced through our intervention, even though we cannot rule out that it is the result of selection of more motivated participants into the lab study. In either case it clearly shows the need of a control group. The number of job interviews does not differ significantly between the groups in any week.

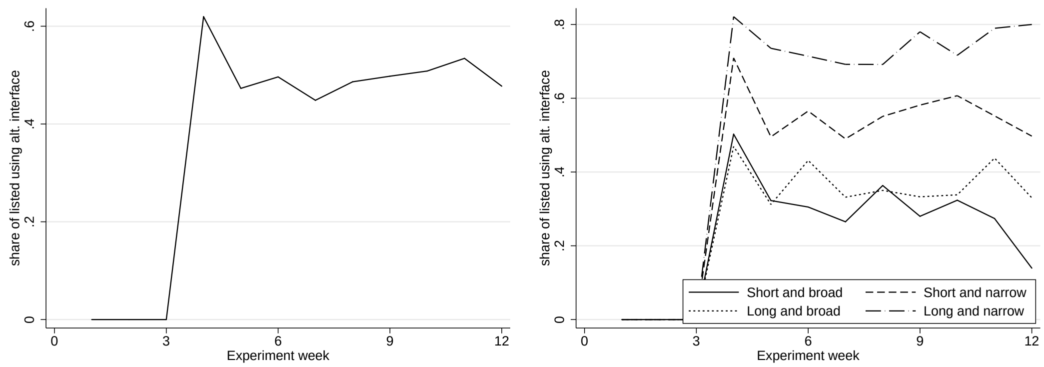
4.3 Attrition

The study ran for 12 weeks, but job seekers could obviously leave the study earlier either because they found a job or for other reasons. Whenever participants dropped out, we followed up on the reasons for dropping out. In case they found a job, we asked for details, and in many cases we were able to obtain detailed information about the new job. Since job finding is a desirable outcome related to the nature of our study, we present attrition excluding job finding in Figure 1. An exit from the study is defined to occur in the week after the last session in which the individual attended a lab session. In most weeks, we lose between 2 and 4 participants, and these numbers are very similar in control and treatment groups. On average, we have 8.3 observations per participant.[33]

We now investigate whether the composition of the control and treatment group changes over time due to attrition, by looking at observable characteristics of those that remain in the study. We compute

[33] In the online appendix we show the distribution of the number of attended weeks per participant, split by pre-intervention (weeks 1-3) and post-intervention (weeks 4-12). See figures 7, 8 and 9.

Figure 2: Share of listed vacancies that results from using the alternative interface



(a) Average in treatment group

(b) Average in treatment group by type (unemployment duration and occupational breadth)

mean values of the same set of variables as in Table 4, for individuals remaining in the study in week 1, 4 and 12. For each of these groups of survivors, we test whether the treatment and control group are significantly different. Since we present 32 variables for three groups of survivors, this implies 96 tests. The resulting p-values are presented in Table 31 in the Online Appendix. Only 6 of the p-values are smaller than 0.10, so there is no indication that attrition leads to systematic differences in the composition of the treatment and control group. Also a Holm-Bonferroni test for joint significance does not reject the null hypothesis of identical values.

The apparent lack of selection is on the one hand helpful to study how the intervention may have affected search outcomes, on the other hand it hints that we are unlikely to capture differences in job finding rates, which are low overall. We will come back to the analysis of drop out and job finding in more detail in Subsection 5.5.

4.4 Use of alternative interface

An obvious question regarding our treatment intervention is whether participants actually use the alternative interface. They are free to revert back to the standard interface, and in this sense our intervention can be considered an intention-to-treat. We are hesitant to adopt this interpretation since all participants in the treatment group used the alternative interface at least once and were therefore exposed to recommendations and suggestions based on their declared "desired" occupation. It could be that they used this information when they reverted back to searching with the standard interface. With this in mind, we report information on actual usage. Panel (a) of Figure 2 plots the fraction of users of the alternative interface over the 12 weeks. On average we find that around 50% of the listed vacancies of the treated participants come from searches using the alternative interface over the 8 weeks and this fraction remains quite stable throughout.[34] We discuss panel b) that considers

[34]The variation in usage results from both between and within users. The participants in the treatment group use the alternative interface for at least one search in 75 % of the weeks on average, and in 35 % of the weeks the alternative interface was used solely. These findings are shown in Figure 13 (panel (a)) in the Online Appendix. Further statistics on usage are shown in panel (b) and (c).

subgroups of participants later on.

5 Analysis and Results

As outlined in the introduction, the hypothesis behind the intervention is that providing information about other occupations will allow individuals to explore vacancies from a larger set of occupations, which may lead to more job interviews. This should hold in particular for individuals that otherwise explore few occupations. Exploring more occupations could go along with more search, or with the same search effort concentrated on more occupations but in a closer geographic region. The following lays out the empirical strategy to investigate this.

5.1 Econometric Specification

Our data is a panel and our unit of observation is at the week/individual level. That is, we compute a summary statistic for each individual of her search behavior (vacancies listed, applications, interviews) in a given week; see Section 3.7 for a description of the outcome measures of interest. Since it is a randomized controlled experiment in which we observe individuals for three weeks before the treatment starts, the natural econometric specification is a model of difference-in-differences. To take account of the panel structure we include individual random effects. By design, there should be no correlation between individual characteristics (observable and unobservable) and treatment assignment, at least initially. To test whether the random effects specification is appropriate for the entire duration of the study, we have estimated a fixed effects model and performed a Hausman test for each of the main specifications (see Table 17 in Online Appendix 8.3). In none of the cases we could reject that the random effects model is consistent, such that we decide in favor of the random effects model for increased precision. We discuss robustness at the end of this section (Subsection 5.6) and show that point estimates are similar when using individual fixed effects yet precision is lower. As has been emphasized by Bertrand et al. (2004), serial correlation is an issue in difference-in-differences models. We follow their suggestion and average the weekly observations into two observations per individual, one before (weeks 1-3) and one after the intervention (weeks 4-12), but again report robustness to alternative specifications at the end of this section.

We compare a variable measuring an outcome (Y) in the control and treatment group before and after the week of intervention, controlling for time period fixed effects (α_t, before or after the intervention), time–slot $\times$ wave fixed effects (δ_g) and a set of baseline individual characteristics (X_i) to increase the precision of the estimates. The treatment effect is captured by a dummy variable (T_{it}), equal to 1 for the treatment group in the period after the intervention. The specification is:

$$Y_{it} = \alpha_t + \delta_g + \gamma T_{it} + X_i \beta + \eta_i + \epsilon_{it} \tag{1}$$

where i relates to the individual, t to the time period and $\eta_i + \epsilon_{it}$ is an error term consisting of an individual specific component (η_i) and a white noise error term (ϵ_{it}). Individual characteristics X_i include gender, age and age squared, unemployment duration and unemployment duration squared and dummies indicating financial concerns, being married or cohabiting, having children, being highly educated and being white. Standard errors are clustered at the individual level in the regressions, to account for any remaining correlation of an individual's observations.

As mentioned earlier, one important challenge with such approach has to do with attrition. If there is differential attrition between treatment and control groups, it could be that both groups differ in unobservables following the treatment. We proceed in two ways to address this potential concern. First, in Section 4.3 we document attrition across treatment and control groups and find no evidence of asymmetric attrition in terms of observable characteristics. Second, our panel structure allows us to control for time-invariant heterogeneity and use within-individual variation. When we estimate a random and fixed effects model, as mentioned above the Hausman test fails to reject the latter. Even though the treatment itself is assigned at the group-level and it is unlikely to be correlated with unobserved individual characteristics, differential attrition could create correlation between the treatment and unobservable individual characteristics. This would then lead to rejection of the random-effects model. The fact that we can never reject this model is thus another indication against differential attrition between treatment and control groups.

It is likely that the treatment affects different individuals differentially. In order for our intervention to affect job search and job prospects, it has to open new search opportunities to participants and participants have to be willing to pursue those opportunities. Participants may differ in terms of their search strategies. We expect our intervention to broaden the search for those participants who otherwise search narrowly, which we will measure by their search in the weeks prior to the intervention. For those who are already searching broadly in the absence of our intervention it is not clear whether we increase the breadth of their search. We therefore estimate heterogeneous treatment effects by initial breadth (splitting the sample at the median level of breadth over the first three weeks).[35]

Second, the willingness to pursue new options depends on the incentives for job search, which change with unemployment duration for a variety of reasons. Longer-term unemployed might be those for whom the search for their preferred jobs turned out to be unsuccessful and who need to pursue new avenues, while they are also exposed to institutional incentives to broaden their search (the Jobcentres require job seekers to become broader after three months). We therefore also interact the treatment effect with a dummy for above median unemployment duration.[36]

Apart from these dimensions for which we have clear reasons for separate investigation we do not explore other dimensions of heterogeneity to avoid data mining. Nevertheless it might be interesting

[35] To check the robustness of our classification of job seekers as narrow or broad searchers, we computed three different classifications (based on listed vacancies in week 1, week 2 and week 3). We find that the classifications of week 1 and 2 agree on 69 % of the job seekers, those of week 1 and 3 agree on 67 % of the job seekers and those of week 2 and 3 agree on 86% of the job seekers.

[36] When estimating heterogeneous effects we adapt our specification to include all necessary additional terms. Define D_i to be an indicator equal to one for individuals belonging to group 1 (for example narrow searchers) and equal to zero for individuals belonging to group 2 (for example broad searchers). We estimate:

$$Y_{it} = \theta D_i + \alpha_{1t} D_i + \alpha_{2t}(1 - D_i) + \delta_g + \gamma_1 T_{it} D_i + \gamma_2 T_{it}(1 - D_i) + X_i \beta + \eta_i + \epsilon_{it} \tag{2}$$

Thus, the specification contains an additional baseline difference between the groups (θ), differential time period effects for the the groups (α_{1t} and α_{2t}) and differential treatment effects between the groups (γ_1 and γ_2). Note that since we average observations into two period (before and after the intervention), α_{1t} and α_{2t} simply contain a time effect for the second period. Note also that, just as in the baseline model, the specification contains time-slot X wave dummies (δ_g) and since treatment is assigned at the time-slot-level, these control for any baseline differences between the control and treatment group.

Table 6: Effect of intervention on listed vacancies

	Breadth of listings		Number of listings
	(1)	(2)	(3)
	Occupational	Geographical	Lab
Treatment	0.13**	-0.01	-34.99
	(0.06)	(0.02)	(52.09)
Treatment			
X occupationally broad	-0.07**	0.01	-23.71
	(0.04)	(0.03)	(90.08)
X occupationally narrow	0.34***	-0.03	-41.84
	(0.10)	(0.03)	(64.01)
Model	Linear	Linear	Linear
Observation weeks	1-12	1-12	1-12
N	540	541	541

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

to know whether breadth of search is correlated with other factors that might drive the observations we report. We investigate this by regressing it on a number of individual characteristics. Results are presented in columns (1) and (2) in Table 16 in the online appendix. We find that breadth of search is not easily predicted based on individual characteristics. Almost all variables are not statistically different from zero, and the R^2 of the regression is low (0.18). The same holds for unemployment duration (columns (3) and (4)).

For the sake of brevity, in the main body we only present the results on the treatment effect (γ) as well as the interaction effects between the treatment and the subgroups of interest. In Table 22 in the Online Appendix we report full results including all other covariates for the main regressions.

5.2 Effects on Listed vacancies

We first look at the effects on listed vacancies - both in terms of number and breadth. We have two variables measuring how broad participants search, one in terms of occupation (as described in Section 3.7), the other in terms of geography (fraction of vacancies outside Edinburgh metropolitan area).

We estimate equation (1)) and present results in Table 6. The first row shows a significant positive overall effect on breadth of search in terms of occupation. The breadth measure increases with 0.13, which amounts to approximately one-fifth of a standard deviation. Another way to assess the magnitude of this effect is to compare it to the natural increase in breadth of listings over time (as shown in Table 5), which implies that the treatment effect is equivalent to the broadening that on average happens over 9 weeks. We find no significant evidence of an overall effect on geographical breadth or on the number of listed vacancies.

In rows two and three in Table 6 we split the sample according to how occupationally broad job

Table 7: Effect of intervention on listed vacancies - interactions

	Breadth of listings		Number of listings
	(1)	(2)	(3)
	Occupational	Geographical	Lab
Treatment			
X long unempl. and occ. broad	-0.10**	0.06	189.12
	(0.05)	(0.04)	(135.01)
X short unempl. and occ. broad	-0.05	-0.04	-252.80**
	(0.05)	(0.05)	(120.19)
X long unempl. and occ. narrow	0.36**	-0.04	23.35
	(0.15)	(0.05)	(62.51)
X short unempl. and occ. narrow	0.32**	-0.01	-112.82
	(0.13)	(0.05)	(116.52)
Model	Linear	Linear	Linear
Observation weeks	1-12	1-12	1-12
N	540	541	541

Each column represents one regression. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

seekers searched in the first three weeks. We find clear heterogeneous effects: those who looked at a more narrow set of occupations in the first three weeks become broader, while those who were broad become more narrow as a result of the intervention. Note that these effects are not driven by 'regression to the mean' since we compare narrow/broad searchers in our treatment group to similarly narrow/broad searchers in our control group.[37] We again find no significant effects on the geographic distance of job search nor on the number of job applications.[38,39] The total effects on job prospects remains in either case an empirical matter that we take up in subsequent sections. The different effects on occupational breadth can be reconciled in a setting where broad searchers find many occupations plausible and use the additional information to narrow down the suitable set, while narrow searchers find few occupations suitable and use the additional information to broaden this set. This mechanism is more formally described in Section 6.

Finally, we split the effect further depending on how long job seekers have been searching for a job and present the results in Table 7. We interact the intervention effect with two groups: short term unemployed (with unemployment duration of less than the median of 80 days) and long term unemployed (with unemployment duration above the median). We find that results do not change much, though

[37] In Figure 12 in the online appendix we show the mean breadth of the different groups before and after the intervention to clarify further that these results are not caused by regression to the mean.

[38] In the Online Appendix we also report estimates where we split the sample according to breadth along the geographical dimension at the median (see Table 19). The results are similar (those who were searching broadly become more narrow and vice versa, and there is some trade-off with occupational breadth). This could still be driven by initial occupational breadth, since this is negatively correlated with initial geographical breadth (coefficient -0.36) and is not controlled for. Indeed, when we split both by occupational and geographical breadth the effects are driven by the occupational dimension, which we will henceforth focus on.

[39] The difference in the number of observations between the columns in Table 6 and similar tables that follow is due to the fact that we can only compute the occupational (geographical) breadth measure if the number of listed is two (one) or larger, which excludes different numbers of observations depending on the variable of interest.

Table 8: Effect of intervention on applications

	Breadth of applications		Number of applications		
	(1) Occupational	(2) Geographical	(3) Lab	(4) Outside lab	(5) Total
Treatment	0.03	-0.06*	0.09	-0.03	0.01
	(0.20)	(0.03)	(0.16)	(0.09)	(0.09)
Treatment					
X occupationally broad	-0.43*	-0.02	-0.08	-0.06	-0.05
	(0.22)	(0.05)	(0.19)	(0.13)	(0.12)
X occupationally narrow	0.49*	-0.09**	0.27	-0.02	0.08
	(0.29)	(0.04)	(0.27)	(0.13)	(0.13)
Model	Linear	Linear	Neg. Bin.	Neg. Bin.	Neg. Bin.
Observation weeks	1-11	1-11	1-11	1-11	1-11
N	305	363	541	490	487

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (3)-(5) are Negative Binomial regression models where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors in parentheses (clustered by individual in column (1) and (2)). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

standard errors are larger. We still find that occupationally narrow searchers become broader while those that were already broad become more narrow, irrespective of unemployment duration. Shorter unemployed job seekers seem to consult less listings in the treatment group, significantly so for broader ones. If the new information allowed them to focus their search better this might not necessarily harm their job prospects, as outlined in our theoretical model, but nevertheless this remains a concern that we return to when we consider the effect on job interviews.

5.3 Effects on Applications

The second measure of search behavior relates to applications. We have information about applications based on search activity conducted inside the laboratory as well as outside the laboratory. The distribution of applications contains a large share of zeros (in almost 50% of the weekly observations there are zero applications through the lab). Therefore we estimate a negative binomial model, with individual random effects.[40] For these models we report [exp(coefficient) $-$ 1], which is the percentage effect.

The results are presented in Table 8. We find no overall treatment effect on applications, except for a decrease in their geographical breadth (approximately one-fifth of a standard deviation). When

[40] Due to overdispersion in the distribution of applications, we prefer a negative binomial model over a Poisson model. However, negative binomial regressions are sometimes less robust and in addition no consensus exists on how to include fixed effects (Allison and Waterman (2002)). Furthermore, we can not cluster standard errors with the random effects negative binomial regressions. Therefore we also report results from Poisson regressions in Online Appendix 8.3 (Table 18). The findings are similar. Furthermore, as we average weekly observations into a before and after period, the outcome variable is changed from discrete into continuous. The distribution still resembles a Poisson distribution and a Poisson regression model or a negative binomial model can still be used (see Gourieroux et al. (1984)).

Table 9: Effect of intervention on applications - interactions

	Breadth of applications		Number of applications		
	(1) Occupational	(2) Geographical	(3) Lab	(4) Outside lab	(5) Total
Treatment					
X long unempl. and occ. broad	-0.67***	-0.07	-0.24	-0.22	-0.20
	(0.25)	(0.06)	(0.21)	(0.14)	(0.13)
X short unempl. and occ. broad	-0.18	0.02	0.17	0.17	0.17
	(0.33)	(0.07)	(0.36)	(0.22)	(0.21)
X long unempl. and occ. narrow	0.51	-0.10**	0.42	-0.11	0.00
	(0.34)	(0.05)	(0.40)	(0.16)	(0.17)
X short unempl. and occ. narrow	0.40	-0.08	0.25	0.14	0.22
	(0.41)	(0.06)	(0.37)	(0.20)	(0.21)
Model	Linear	Linear	Neg. Bin.	Neg. Bin.	Neg. Bin.
Observation weeks	1-11	1-11	1-11	1-11	1-11
N	305	363	541	490	487

Each column represents one regression. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (3)-(5) are Negative Binomial regression models where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors in parentheses (clustered by individual in column (1) and (2)). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

we split the sample according to initial occupational breadth, we find a similar pattern as for listings. Those who searched more narrowly in terms of occupation become occupationally broader, while those that searched broadly become more narrow. The estimates are significantly different from zero at the 10 % level. We find no effects on the number of applications for either group (columns (3) - (5)), though point estimates might indicate a pattern where the initially-narrow group expands broadness through more applications and vice versa for the initially-broad subgroup. There is also a negative effect on geographical breadth for the occupationally narrow job seekers (column (2)), which indicates that narrow job seekers search for occupationally broader jobs closer to home.[41]

Again, we split these effects by the duration of unemployment. Column (1) in Table 9 shows that occupational breadth goes down significantly for long term unemployed broad searchers. It increases most for long term unemployed narrow searchers, yet this is insignificant due to large standard errors. This increase is accompanied by a significant decrease in geographical distance. Estimates on the number of applications are insignificant, though point estimates are economically large. As noted earlier, even decreases in occupational breadth can be beneficial if job search becomes better targeted.

5.4 Effects on Interviews

We now turn to interviews, the variable that is most closely related to job prospects. Since the number of interviews per week is always very small, we cannot compute breadth measures. So we only look at

[41] When splitting the sample according to how narrow people searched in terms of geography, we find no evidence of heterogeneous effects. Results are presented in the Online Appendix in Table 20.

Table 10: Effect of intervention on interviews

	Number of interviews		
	(1) Lab	(2) Survey	(3) Total
Treatment	0.61	0.40*	0.44*
	(0.79)	(0.27)	(0.28)
Treatment			
X occupationally broad	-0.37	-0.00	-0.07
	(0.43)	(0.28)	(0.24)
X occupationally narrow	1.13	0.86**	1.03***
	(1.26)	(0.47)	(0.55)
Model	Poisson	Poisson	Poisson
Observation weeks	1-10	1-10	1-10
N	540	466	464

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (1)-(3) are Poisson regression models where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a measure of the number of interviews obtained as a result of search conducted inside the laboratory and outside the laboratory.[42] Because of the large share of zeros, we estimate a Poisson model with individual random effects. Again we report [exp(coefficient) $-$ 1], which is the percentage effect.

Results are presented in Table 10. There is a positive effect of the treatment of 44% on the total number of interviews, which is significant at the 10% level. We also find positive effects on interviews on the two separate dimensions of search in the lab and search outside the lab, but even though the point estimate for the effect within the lab is highest only the increase in out-of-lab interviews is statistically significant. This can be explained by the difference in base rate which is lower in the lab making statistical inference more difficult: In the pre-treatment period the number of interviews through the lab was 0.09, while the number of interviews through other channels was 0.53.

When splitting the sample according to breadth of search, we find that the effect is entirely driven by those who searched narrowly in terms of occupation. For this group the number of interviews increases for search activity conducted both in the lab and outside (though again, only the increase of the out-of-lab interviews is statistically significant). This seems to indicate that the additional information is not only helpful for search on our platform, but also guides behavior outside.[43] The

[42] For interviews reported outside the lab we censor observations at 3 interviews per week, because of some outliers. Results are similar when no such restriction is imposed. As a check of consistency, we also check whether interviews are ever reported without preceding applications. We find that in 98.2 % of the weeks in which an interview is reported, a positive number of applications was reported in at least one of the two preceding weeks.

[43] We find some evidence of heterogeneity in treatment effects when we split the sample according to initial geographical breadth, with a large positive significant treatment effect for those who searched broadly geographically. Results are presented in the Online Appendix in Table 21.

Table 11: Effect of intervention on interviews - interactions

	Number of interviews		
	(1)	(2)	(3)
	Lab	Survey	Total
Treatment			
X long unempl. and occ. broad	-0.27	-0.23	-0.21
	(0.72)	(0.25)	(0.26)
X short unempl. and occ. broad	-0.37	0.17	0.01
	(0.52)	(0.47)	(0.37)
X long unempl. and occ. narrow	13.12***	2.44***	3.39***
	(9.25)	(1.19)	(1.42)
X short unempl. and occ. narrow	-0.26	0.31	0.30
	(0.51)	(0.40)	(0.44)
Model	Poisson	Poisson	Poisson
Observation weeks	1-10	1-10	1-10
N	540	466	464

Each column represents one regression. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (1)-(3) are Poisson model regressions where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

point estimates for the occupationally broad group are all insignificant and in absolute value much smaller, but point estimates are negative.

When we further split the sample according to length of unemployment duration, we find that the positive treatment effects on the narrow searchers is mainly driven by the long term unemployed narrow searchers. This group gets a significant increase in the number of interviews both as a result of search activity inside the lab and outside the lab.[44] This highlights that our intervention is particularly beneficial to people who otherwise search narrowly and who have been unemployed for some months. It might be encouraging that there are no significant negative effects on the groups that became occupationally narrower, but some negative point estimates might warrant further investigation.

The set of weekly interviews is too small to compute breadth measures. We did, however, ask individuals at the beginning of the study to indicate three core occupations in which they search for jobs, and we observe whether an interview was for a job in someone's core occupation or for a job in a different occupation. We had seen earlier that the alternative interface was successful in increasing the occupational breadth of listed vacancies and applications, and separate treatment effects on interviews in core vs non-core occupations allow some assessment of whether this lead to more "breadth" in job interviews. Results are presented in Table 12. We indeed find that the increase in the number of interviews relative to the control group comes from an increase in non-core occupations that were not their main search target at the beginning of our study, though due to low precision the effect is not

[44]The extremely large value of the increase in lab interviews for the long term narrow searchers is partly due an individual outlier that reported an average of 3.5 interviews per week in the treatment period. If we exclude this individual, the coefficient is still large, positive and statistically significant (6.75***).

Table 12: Effect of intervention on interviews: core and non-core occupations

	Number of interviews (in the lab)	
	(1)	(2)
	Core occupations	Non-core occupations
Treatment	-0.14	0.75
	(0.72)	(0.85)
Model	Poisson	Poisson
Observation weeks	1-10	1-10
N	540	540

Each column represents one regression. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (1)-(2) are Poisson model regressions where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

statistically significant. As the number of interviews becomes small when splitting between core and non-core, we cannot split the sample further by subgroups.

One may worry that the increase in interviews in non-core occupations is associated with different quality of the interviews. For example, the suggestions could lead to interviews for jobs with different wages. We have investigated this by comparing the average wage of listed vacancies, applications and interviews and find that the alternative interface does not significantly change the wage of any of these.[45]

Our findings suggest that the alternative interface may be more beneficial to those that search narrowly and have been relatively long unemployed. This finding is supported by statistics on usage of the interface over time. Panel (b) of Figure 2 shows the evolution of the fraction of treated participants using the interface, splitting the sample by occupational breadth and unemployment duration. We find that long term narrow searchers are indeed using the interface more than the other groups (with around 75% of them using the interface in contrast to around 45% for the other groups), and this difference is statistically significant. This finding supports the intuition that some groups of job seekers benefit more from the intervention and are therefore more willing to use the alternative interface. This group, the long-term unemployed narrow searchers is exactly the group for which we find the most pronounced positive effects.[46]

[45] We computed for every individual in every week the average wage of listed vacancies, applications or interviews and performed regressions similar to our main specifications.

[46] The idea that these groups are more willing to use the alternative interface is supported by responses from the baseline survey in the first week. The participants were asked to specify how long they expected it would take to find a job. Within the group of short-term unemployed the median response is "less than 3 months" which might indicate a rather clear idea of how to obtain a job, while for the long-term unemployed group the median response is "less than 6 months" which might indicate a less clear view and more scope to provide successful alternatives.

5.5 Effects on Job Finding

We now briefly turn to the analysis of job finding. As mentioned earlier, the study was not designed to evaluate effects on job finding and, given the size of the sample, we should be cautious in interpreting any results we have. Also, one should keep in mind that attrition from one week to the next for unexplained reasons is low but of the same order of magnitude as the confirmed job finding rate.[47]

At the end of the 12 weeks, 28% of the participants using the standard interface have found a job, against 22% of the participants using the alternative interface. A similar proportion (15%) of participants have dropped out of the study with an unclear outcome, so it is difficult to draw conclusions based on these numbers.

These numbers are nevertheless useful to get a sense of the sample size one would need in order to capture significant effects on job finding. We perform a simple sample size calculation to illustrate how the required sample size for finding an effect on job finding exceeds the sample size required for finding an effect on the number of interviews. To detect a 44% increase in interviews due to the intervention (see Table 10), a sample size of 70 observations per treatment is required (so 140 in total). For job finding, detecting a similar sized effect requires around 3794 observations per treatment, due to a much lower base rate.[48] Even if one takes the (at most) 8 observations per individual in our study into account, it is clear that we lack power to identify any realistic effect on job finding.

Bearing this in mind, we estimate a simple duration model where the duration is the number of weeks we observe an individual until she/he finds a job. Since we know when each individual became unemployed, we can calculate the total unemployment duration and use this as a dependent variable. This variable is censored for individuals who drop out of the study or who fail to find a job before the end of the study. We estimate a proportional Cox hazard model with the treatment dummy as independent variable, controlling for additional individual characteristics and group session dummies.

We report estimates for the entire sample and for the sub-samples conditioning on initial search type (narrow vs broad search). The results are presented in Table 13. We fail to find significant differences in the hazard rates across treatments. That is, we have no evidence that the job seekers exposed to the alternative interface were more or less likely to find a job (conditionally on still being present in week 4). Despite the negative point estimate for the treatment group, even increases in the hazard of the treatment group of the magnitude of the increase in interviews overall (29%) or for narrow individuals (52%) are well within the confidence interval of these estimates. That is not to say that lack of power is the only plausible reason for finding no effect. As mentioned in the introduction, interviews in broader occupations might not convert to jobs at the same rate. We return to advocating

[47]We tried to follow-up by calling them at least 3 times, though for a non-trivial share of the attrition we still do not observe whether the person found a job or just quit the study.

[48]The precise computation is as follows. We observe in the first three weeks that, on average, participants have a total of 0.61 interviews per week through the lab and other channels (see Table 4). To detect a 44% increase in interviews due to the intervention (see Table 10), such that the interview rate becomes 0.89, a sample size of 70 observations per treatment is required (so 140 in total). This number is based on an one-sided test with type-I error probability $\alpha = 0.10$ and power $1 - \beta = 0.80$. The standard deviation is assumed to be 0.75 in both groups, based on the numbers reported in Table 4. For job finding, we observe 19 people finding a job in the first 3 weeks, which implies a weekly job finding rate of approximately 0.02. If we make the (strong) assumption that the additional interviews are equally likely to result in a job as the initial interviews, we would expect a 44% increase in job finding. Note that this is a conservative choice as this would be a very large effect. Still, to be able to pick up the increase in job finding from 0.02 to 0.0288 requires a sample size of 3794 people per treatment (similar test as for interviews).

Table 13: Treatment effects on job finding rate

	(1)	(2)
Treatment	-0.13	-0.18
	(0.25)	(0.31)
Treatment x Occupationally narrow		0.10
		(0.56)
N	253	253

Proportional Cox Hazard model, with time-slot fixed effects, and individual characteristics. We exclude observations censored at 3 weeks or less. Reported values are coefficients. * $p < 0.10$.

larger studies in the conclusion.

5.6 Robustness: Alternative Specifications

In our analysis we made some choices regarding the empirical specification and the definition of variables. Below we briefly discuss alternative choices and investigate robustness of our results (more details can be found in Online Appendix 8.4). We consider (1) individual fixed effects instead of random effects, (2) weekly observations instead of aggregated data, (3) linear models instead of count data models, (4) excluding the last one or two observations per individual, (5) an alternative breadth measure and (6) IV regressions with the use of the alternative interface as the treatment intensity.[49]

Our specifications include individual random effects to increase precision. A Hausman test does not reject validity of the random effects model. In Table 23 of the Online Appendix we show our baseline regressions using individual fixed effects instead of random effects. We find very similar overall patterns but reduced precision and significance. In particular, changes in occupational breadth are similar (for listed vacancies and applications), while we find large positive coefficients for interviews for narrow searchers, which are, due to slightly reduced precision not statistically significant.

All data in our estimations have been averaged into two periods, before and after the intervention (as suggested by Bertrand et al. (2004)). In Table 24 in Online Appendix 8.3 we show that results are very similar when including weekly observations, both for changes in breadth and for the number of interviews.

Since the number of applications and interviews are count variables, we use Poisson regressions or negative binomial regressions in our analysis. In Table 25 in Online Appendix 8.3 we present linear regressions for these outcomes. We find similar patterns: there is no clear impact on applications, but the point estimate for interviews is economically large, and significant for narrow searchers.

In our analysis we excluded week 12 (for applications) and weeks 11 and 12 (for interviews), because for vacancies saved in these weeks we can not follow up on whether an application was sent or an interview was secured. Alternatively, we can exclude *for each individual* their final one or two

[49] We also thank an anonymous referee for requesting additional analysis of heterogeneous effects by educational level. We find no evidence. In the paper we just focus on two obvious dimensions of heterogeneity, to prevent data mining.

attended sessions. The results when using this approach are shown in the online appendix in Table 26. All findings are very similar, both in magnitude and statistical significance.

Fifth, we consider our definition of occupational breadth. In our approach the distance between two occupations is based on the number of common digits of the two occupational codes (see section 3.7). We consider two alternatives. First, one can call occupations identical if they share a particular number of digits, which leads to the well-known Gini-Simpson index. Such measures are highly correlated with our measure (for listed vacancies our measure has a correlation above 0.95 with 4 different Gini-Simpson measures, see Tables 29 and 30 in the Online Appendix). Not surprisingly we find very similar results when we adopt this alternative measure. Second, we could use a more elaborate alternative which is to use empirically observed transitions between occupations in labor market surveys to measure the "closeness" of the two occupations.[50] The results of the main regressions using this approach are presented in Table 27 in Online Appendix 8.3. We find, again, that results are very robust against this alternative breadth measure.

Finally we consider an interpretation that all our results are intention-to-treat effects (due to voluntary usage of the alternative interface). As discussed, we are hesitant to emphasize this interpretation too much, because suggestions about alternative occupations can affect job seekers even after a user switches back to the standard interface. However, for the sake of comparison, we can consider treatment assignment as an instrument for actual usage when estimating our empirical models.[51] The results of such an approach are presented in Table 28. As expected, the estimates are larger in magnitude. We find that breadth of listed vacancies increases (with a coefficient of 0.24** compared to 0.13** in the baseline results). Additionally, we also find that breadth of applications increases significantly for narrow searchers. Interviews increase significantly for narrow searchers.

6 An Illustrative Model

In the empirical section we saw that our information intervention increases occupational breadth: listings are broader and more job interviews are obtained, possibly driven by jobs outside the core occupations. Effects are concentrated on those who initially search narrowly, while breadth decreases for those who initially already search broadly. Finally and possibly least obvious, effects are strongest for the longer-term unemployed. Here we briefly sketch a very stylized occupational job search model that is capable of organizing our thoughts about the driving forces. It is based on the idea that workers learn about the occupations in which they search for jobs, in the spirit of e.g. Neal (1999), with the difference that workers start with heterogeneous beliefs about different occupations and that we study information provision.

A job seeker can search for jobs in different occupations, indexed $i \in \{1, .., I\}$. For each occupation she decides on the level of search effort e_i. Returns to searching in occupation i are given by an increasing but concave function $f(e_i)$.[52] The returns to getting a job are given by wage w and are the same across occupation, and b denotes unemployment benefits. The cost of search is given by an

[50] We thank an anonymous referee for this suggestion.
[51] We define actual usage as the share of listings that is performed using the alternative interface in a particular week.
[52] This could embed a standard matching function with search effort - we do not go into the foundations here.

increasing and convex function $c(\sum e_i)$. A limiting case is a fixed total search effort $\bar{e}$, such that costs are zero up to that point and infinite thereafter.

The individual is not sure of her job prospects within the various occupations. In a given occupation i her job prospects are either good (in which case we denote her H - high - type) and she obtains a job with arrival probability $a_H f(e_i)$. Otherwise her prospects are bad (in which case we denote her L - low - type) and her job probability is $a_L f(e_i)$, where $a_H > a_L = 0$, where the equality is only for simplicity. The individual does not know whether she is a high or low type in occupation i, but assigns probability p_i to being a high type. So the individual's belief is a vector $(p_1, p_2, ..., p_I)$ of probabilities for each occupation. Types are drawn iid and therefore the type vector is all that is relevant for the individual. Still, to model the information content of the alternative interface later on, it will be convenient to make the additional assumption that the individual is unsure of the exact value of the probability in each of the occupations, and only knows its distribution Q_i with support $[\underline{q}_i, \overline{q}_i]$ among people that are like her. Then p_i can be interpreted as the average belief according to Q_i. For technical convenience assume that types are not too good, i.e., $\overline{q}_i \leq 1/2$, so that the average belief is also bounded by this number. This ensures that an occupation with higher belief also has higher variance and both increase the incentives to search in this occupation in such a simple bandit problem, which makes search incentives monotone in p_i.

Given a belief vector $p = (p_1, ..., p_I)$ and a vector of search effort in the various occupations $e = (e_1, ..., e_I)$, her overall expected probability of being hired is

$$H(p, e) = 1 - \prod_i [1 - f(e_i)(p_i a_H + (1 - p_i) a_L)]$$

where the product gives the probability of not getting a job offer in any occupation.

Assume the unemployed job seeker lives for T periods, discounts the future with factor δ, and there are no job separations. An individual who has a prior p_i^t about his type in occupation i at the beginning of period t and spends effort e_i^t during the period but does not get a job will update her beliefs by Bayes rule. Let $B(p_i^t, e_i^t)$ denote this new belief. For interior beliefs we have[53]

$$p_i^{t+1} = B(p_i^t, e_i^t) = \begin{cases} = p_i^t & if \quad e_i^t = 0 \\ < p_i^t & if \quad e_i^t > 0, \end{cases} \tag{3}$$

since the individual becomes more pessimistic if unsuccessful unless she put zero effort in the first place. Let $B(p, e) = (B(p_1, e_1), ..., B(p_I, e_I))$ denote the vector of updates.

An individual who enters period t of his life with prior p $(= p^t)$ chooses optimal effort vector e $(= e^t)$ across occupations to maximize

$$R_t(p) = \max_e \left(b - \delta R_{t+1}(B(p, e)) + H(p, e)\Big(W_t - (b + \delta R_{t+1}(B(p, e)))\Big) - c(\sum_i e_i) \right). \tag{4}$$

She can ensure herself the unemployment benefit and the value from continued search. If she finds a job, she looses those but gains the lifetime value of wages (W_t). She also has to pay the search costs.

[53]The exact formula in this case is $B(p_i^t, e_i^t) = p_i^t[1 - f(e_i^t)a_H]/[1 - p_i^t f(e_i^t)a_H - (1 - p_i^t)f(e_i^t)a_L]$. Note also that beliefs do go up if the person finds a job, but under the assumption that the job is permanent this does no longer matter.

For our purposes a two-period model suffices (for which $R_3 = 0$, $W_2 = w$ and $W_1 = w(1+\delta)$).[54] The first period captures the newly unemployed, and the second period the longer-term unemployed.

The alternative interface provides a list of occupations suitable for someone like her. To formalize this, assume that an occupation is only featured on the list if the objective probability q_i of having good job prospects exceeds a threshold $\hat{q}$. In the first period of unemployment this means that for any occupation on the list the individual updates her belief upward to the average of q_i conditional on being larger than $\hat{q}$ (i.e., $p_i^1 = \int_{\hat{q}}^{\overline{q}_i} q_i dQ_i / \int dQ_i$). For occupations that are not on the list her beliefs decline to the average of q_i conditional of q_i being below $\hat{q}$ (i.e., $p_i^1 = \int_{\underline{q}_i}^{\hat{q}} q_i dQ_i / \int dQ_i$). Obviously these updates also apply if the alternative interface is introduced at a later period of unemployment as long as the individual has not yet actively searched in this occupation.[55] The alternative interface induces an update in belief p_i^t when it is introduced, but given this update problem (4) still applies.

In order to gain some insights into in how an unanticipated introduction of the alternative interface affects the occupational breadth of search, consider for illustration two classes of occupations. Occupations $i \in 1, ..., I_1$ are the "core" ones where the job seeker is more confident and holds first period prior $Q_i = Q_H$ leading to average belief $p_i = p_h$, while she is less confident about the remaining "non-core" occupations to which she assigns prior $Q_j = Q_L$ with average $p_j = p_L$ such that $p_L \leq p_H$. Assume further that core occupations enter the list in the alternative interface for sure (i.e., $\underline{q}_H > \hat{q}$), which means that the alternative interface provides no information content for them. For non-core occupations we assume that there is information content (i.e., $\hat{q} \in (\underline{q}_L, \overline{q}_L)$) so that the alternative interface changes the prior positively if this occupation is featured on the alternative interface and negatively if it is not. For ease of notation, denote by e_H the search effort in the first period in core occupations, and by e_L the same for non-core occupations.

The following results are immediately implied by problem (4): given the search period, the number of core occupations and the current belief about them, there exists a level $\bar{p}$ such that the individual puts zero search effort on the non-primary occupations iff $p_i^t \leq \bar{p}$ for each non-core occupation i. Intuitively, when the average belief about being a high type in the non-core occupations is sufficiently close to zero, then it is more useful to search in the core occupations and search effort in non-core occupations is zero. More or better core occupations increase the level of $\bar{p}$ since this leads to more search there which drives up the marginal cost of search in non-core occupations.

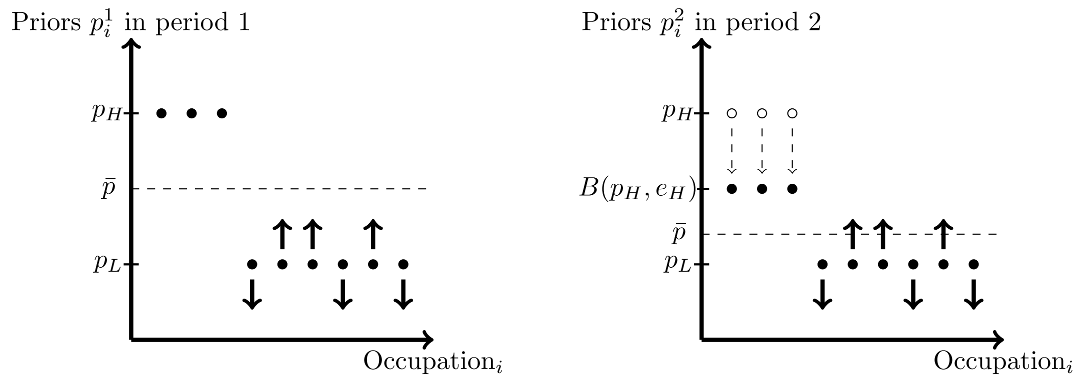
An individual who is recently unemployed and narrow is depicted in Figure 3 (a). She is narrow because her beliefs in her core occupations (p_H) are high enough that she does not want to search in the secondary occupations ($\bar{p} > p_L$). This individual concentrates so much effort onto the primary occupations that marginal effort costs are large and exceed the marginal gain from exploring the less likely occupations. In fact, even small changes in the prior p_L induced by the alternative interface - indicated by the thick arrows in the figure - do not move them above the threshold $\bar{p}$.[56] So there is no difference in search behavior with or without the alternative interface.

[54] Infinitely lived agents would correspond to a specification with $W_t = w/(1-\delta)$ and $R_t(p) = R(p)$.

[55] After effort the updating is more complicated but obviously being on the list continues to be a positive signal.

[56] The alternative interface induces small changes if its informativeness is low enough (e.g., $\overline{q}_L - \underline{q}_L < \epsilon$ for sufficiently small ϵ so that the support of initial beliefs is not very dispersed). We do not explore the alternative case due to its counterfactual implication that already recently unemployed individuals would turn broad with the alternative interface.

Figure 3: Model Illustration: narrow search



(a) Narrow search, short-term unemployed: High belief (p_H) in first three occupations relative to other occupations (p_L). Small changes in beliefs (arrows) do not move beliefs above the threshold $\bar{p}$ to be included into the search.

(b) Narrow search, longer-term unemployed: Updating in first three occupations lowers beliefs (dashed arrows). This lowers threshold $\bar{p}$ and additional information moves some beliefs above it and broadens search (solid arrows for occupations 4-10).

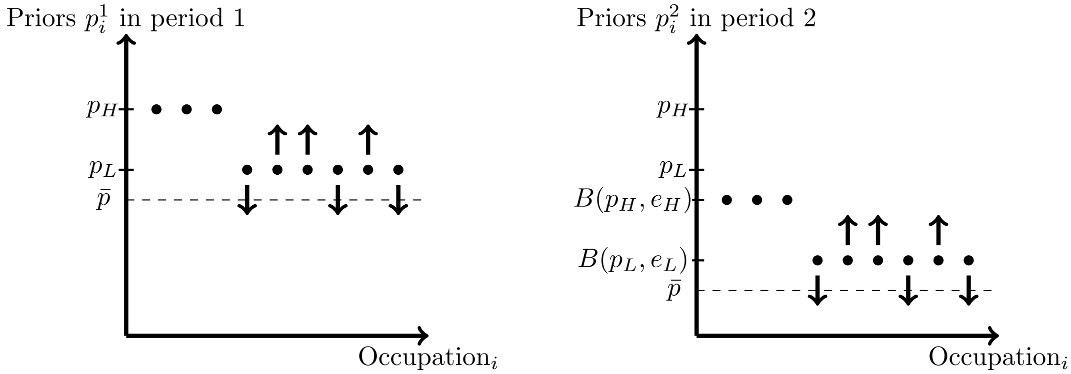
In panel (b) we depict our notion of the same individual after a period of unemployment. Her prior at the beginning of the second period is derived by updating from the previous one. After unsuccessful search in the core occupations it has fallen there, as indicated by the lower priors for the first three occupations. Since she did not search in non-core occupations, her prior about them remains unchanged. So beliefs are now closer, and the utility of applying to either of them are also closer and $\bar{p}$ falls.[57] In a model with many rounds beliefs about the core occupations would eventually fall so low that individuals would start searching more broadly even without access to the alternative interface (as we see in our data for the control group, see section 4). Panel (b) depicts a shorter time frame where $\bar{p}$ falls too little to induce additional search without further information, but enough such that being featured on the alternative interface now moves beliefs about such non-core occupations above it. It thereby induces broader search. Search effort weakly expands, and strictly so if the cost function is smooth, leading to better job prospects.[58] So this rationalizes why longer-unemployed individuals in the treatment group become broader and their number of interviews increases, relative to the control group without the alternative interface. It also implies a weak increase in search effort relative to the control group. At low unemployment durations to the contrary there is little effect.

Figures 4 (a) and (b) depict individuals who are already broad in the absence of an information intervention, since the threshold $\bar{p} < p_L$. This could be a recently unemployed individual who started with rather equal priors, as shown in panel (a). Alternatively it could be a person whose beliefs fell over the course of the unemployment spell to a more even level, as shown in (b) (possibly from an initially uneven profile such as in Figure 3 (a)). In both cases, beliefs about occupations that are not recommended on the alternative interface might fall so low that the person stops searching there and becomes narrow. Effects on search effort and job prospects are ambiguous: search effort can now be

[57] Additional monetary sanctions for failing to search broader over time would further push down $\bar{p}$ over time.

[58] Marginal benefits of search in core occupations are not affected by the alternative interface, so optimality implies marginal cost (and thereby search effort) cannot fall. Such effort is only optimal under higher job finding probabilities.

Figure 4: Model Illustration: broad search



(a) Broad search, short-term unemployed: Beliefs are rather similar and all beliefs are above the search cutoff $\bar{p}$. Changes in beliefs (arrows) move some beliefs below this cutoff, making the person narrower.

(b) Broad search, longer-term unemployed: Similar to part (a) but at a lower level of beliefs.

concentrated more effectively on promising occupations which raises job prospects if search effort does not fall too much or even rises; alternatively the negative information on some occupations can lead to such reductions in search effort that overall job prospects fall. Both are theoretically possible.[59] And the offsetting effects can lead to a decrease in breadth without significant employment effects. This can rationalize this empirical finding for initially broad searchers. Thus, the model is able to replicate differential effects by breadth and unemployment duration.

7 Conclusion

We provided an information intervention in the labor market by redesigning the search interface for unemployed job seekers. Compared to a "standard" interface where job seekers themselves have to specify the occupations or keywords they want to look for, the "alternative" interface provides suggestions for occupations based on where other people find jobs and which occupations require similar skills. While the initial costs of setting up such advice might be non-trivial, the intervention shares the concept of a "nudge" in the sense that the marginal cost of providing the intervention to more individuals is essentially costless and individuals are free to opt out and continue with the standard interface. While our intervention has a clear information component that falls within classical economic theory, a major aim of the intervention was to keep things simple for participants so little cognitive effort is required to learn on the alternative interface, which might be considered a nudge element.

We find that the alternative interface significantly increases the overall occupational breadth of job search in terms of listed vacancies. In particular, it makes initially narrow searchers consider a broader set of options, but decreases occupational breadth for initially broad searchers, even though overall the former effect dominates. We find a positive effect on job interviews especially for those

[59] With cost functions that induce constant search effort better information must improve job finding. See our previous working paper for the construction of cost functions such that search effort falls and job finding decreases.

which otherwise search narrowly and have an above-median unemployment duration. The effect of unemployment duration is illustrated in our model where those who just got unemployed concentrate their efforts on those occupations where they have most hopes in and are not interested in investing time into new suggestions. If this does not lead to success, their confidence in these occupations declines and they become more open to new ideas.

Some words of caution in line with those in the introduction are warranted. While we find no statistically significant negative effects on job interviews for any subgroup, we cannot rule out that some of them get hurt through less interviews. Moreover, the size of the current study precludes any precise assessment of the effects on job finding, and currently we find no evidence of improvements on this dimension. We have limited information on the types of job found, which jeopardizes our ability to provide a convincing analysis on the duration and quality of jew jobs. At this stage, we can therefore not conclude that the increase in interviews is beneficial. Finally, additional larger-scale roll-out of such assistance would be required to document the full employment effects. The current study does not allow the assessment of equilibrium effects that would arise if everyone obtained information.

With these caveats in mind, our findings suggest that targeted job search assistance to those who otherwise search narrowly and with somewhat longer unemployment duration could be effective, in a cost-efficient way. The programming for the study cost £20,000 ($30,000). If a large-scale website such as Universal Jobmatch would roll out such a scheme for millions of job seekers, it is obvious that the cost per participant is at the order of a few pence.[60] So any meaningful positive employment effects would swamp the costs. As a first study on job search design on the web, it offers a new route how to improve market outcomes in decentralized environments and hopefully opens the door to more investigations in this area.

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[60] The study also devoted substantial resources (£80,000/$120,000) to attracting participants, compensating participants, and for research assistants.

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8 Appendix - For Online Publication

8.1 Vacancies in the UK and in Edinburgh

The number of posted vacancies available through our search engine in Edinburgh and in the UK is shown in Panel (a) of Figure 5 for each week of the study (the vertical line indicates the start of wave 2). In panel (b) the total number of active vacancies in the UK is shown over the second half of 2013 and 2014.[61] As a comparison the total number of active vacancies in the database used in the study in both waves is shown.

8.2 Aggregate State of the Labor Market

The unemployment rate in the UK overall and in Edinburgh in particular between 2011 and 2014 is shown in part a) of Figure 6 where the vertical lines indicate the start of each wave of our study. These statistics are based on the Labour Force Survey and not the entire population. Therefore we present the number of job search assistance (JSA) claimants in the Edinburgh and the UK in panel (b), which is an administrative figure and should be strongly correlated with unemployment.

8.3 Extended results

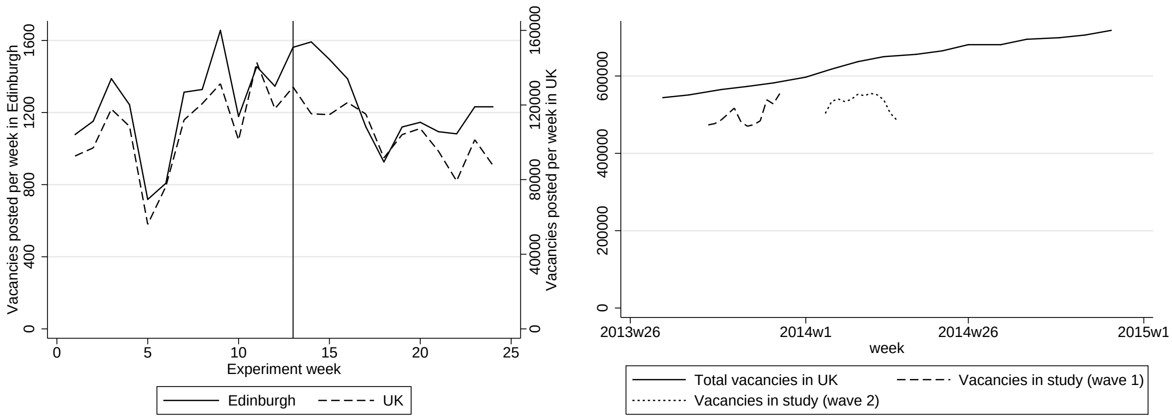
Table 14: Recruitment and show-up of participants

	Full sample	Wave 1	Wave 2
Recruitment channel participants:			
Job centres	86%	83%	89%
Gumtree or other	14%	17%	11%
Sign up rate jobcentre for lab study[a]	43%	39%	47%[c]
Show up rate lab study	45%	43%	46%
Sign up rate jobcentre for online study[a]		60%	
Show up rate online study[b]	21%	21%	21%

[a] Of those people that were willing to talk to us about the study, this is the share that signed up for the study. [b] About a fourth of those that signed up for the online study had a non-existing email address, which partly explains the low show up rate. [c] The sign up rate at Jobcentres for the lab study in wave 2 is based on only one day of recruitment for the following reason: We asked our assistants to write down the number of people they talked to and the number that signed up. Unfortunately these have not been separated for the online study and the lab study. In the first wave there were different assistants for the two studies, such that we can compute the sign up shares separately. In the second wave we asked assistants to spend parts of their time per day exclusively on the lab study and parts exclusively on the online study, so we only have sign-ups for the total number. One day was an exception, as recruitment was done only for the lab study on this day, such that we can report a separate percentage based on this day. We do not have a separate number for sign-up for the online study.

[61] Panel (b) is based on data from our study and data from the Vacancy Survey of the Office of National Statistics (ONS), dataset "Claimant Count and Vacancies - Vacancies", url: www.ons.gov.uk/ons/rel/lms/labour-market-statistics/march-2015/table-vacs01.xls

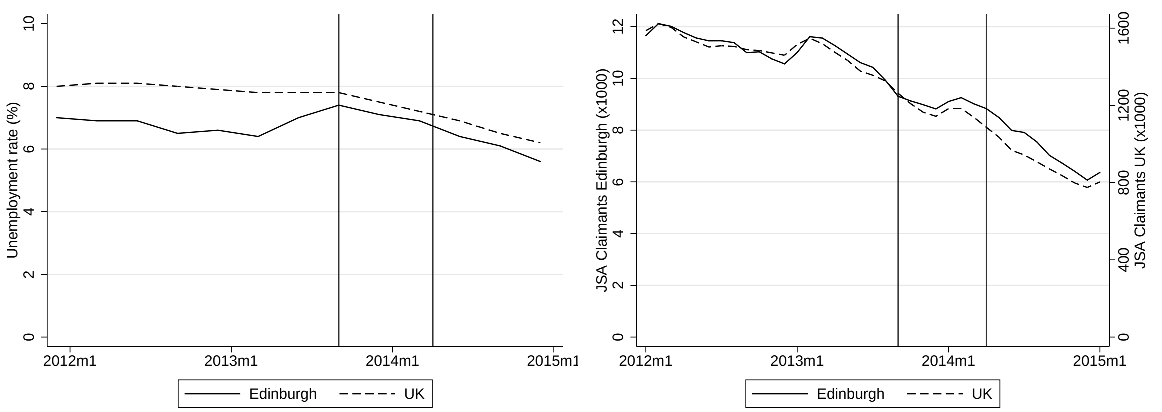
Figure 5: Number of vacancies



(a) Posted vacancies in our study

(b) Active vacancies in our study and in UK

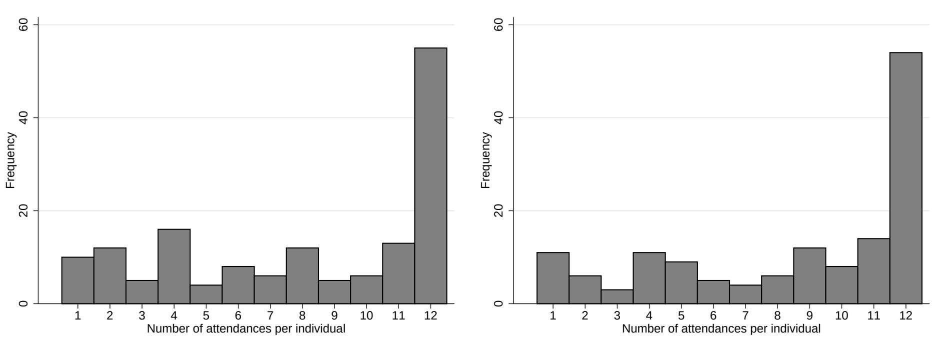
Figure 6: Aggregate labor market statistics



(a) Unemployment rate

(b) JSA claimants

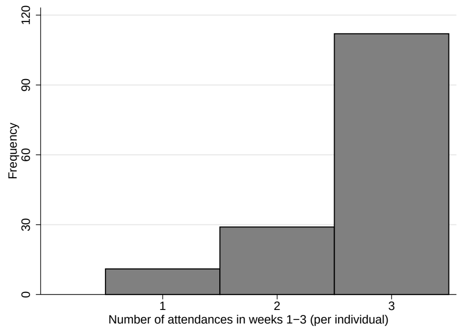
Figure 7: Histogram of the total attendance in weeks per individual



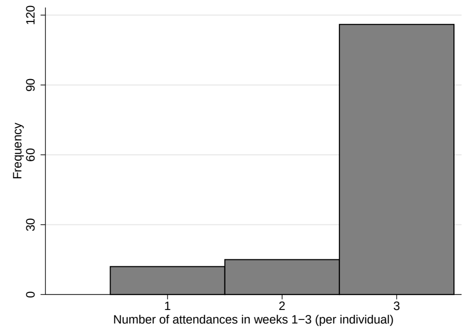
(a) Control group

(b) Treatment group

Figure 8: Histogram of the attendance in weeks 1-3 per individual

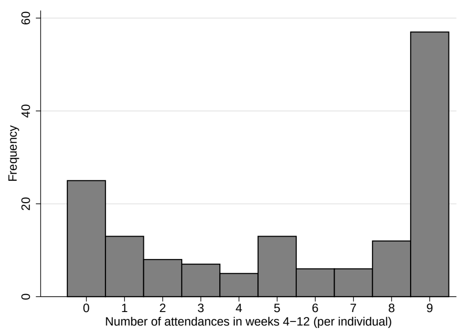


(a) Control group

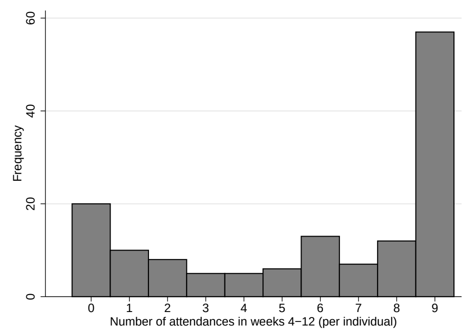


(b) Treatment group

Figure 9: Histogram of the attendance in weeks 4-12 per individual

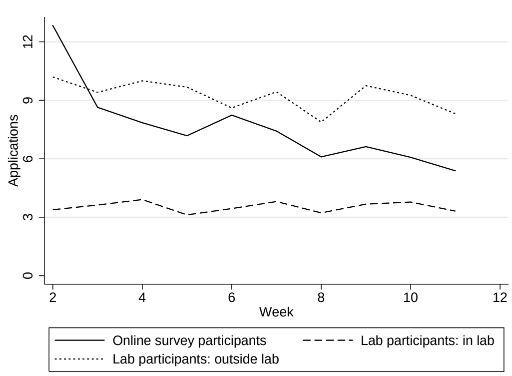


(a) Control group

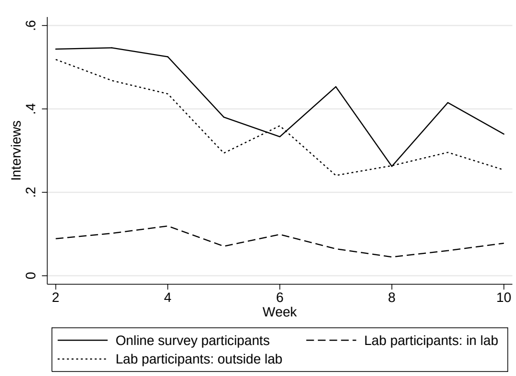


(b) Treatment group

Figure 10: Jobsearch behavior online and lab participants

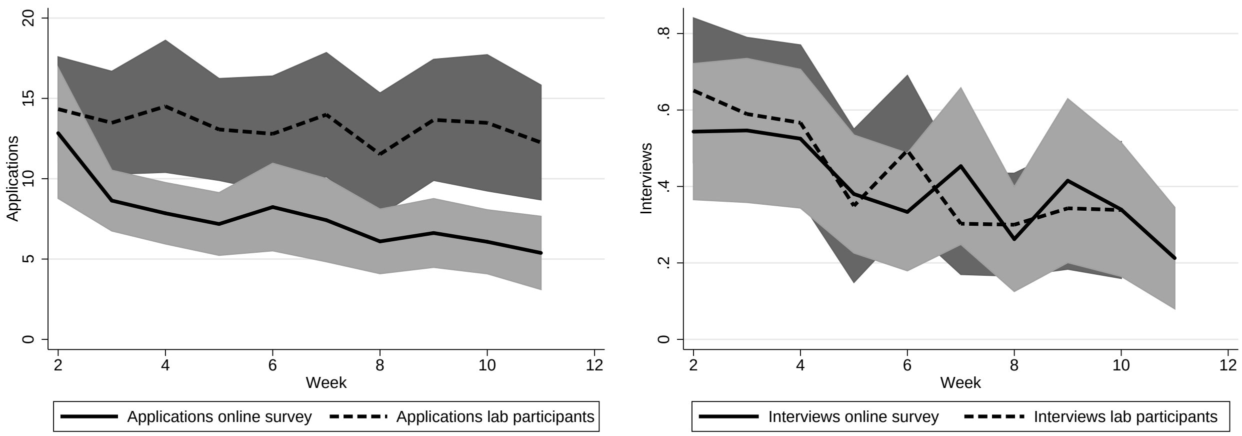


(a) Applications



(b) Interviews

Figure 11: Applications and interviews of lab participants and online survey participants with 95% confidence interval



(a) Applications

(b) Interviews

Figure 12: Mean values breadth of listed by initial occupational breadth

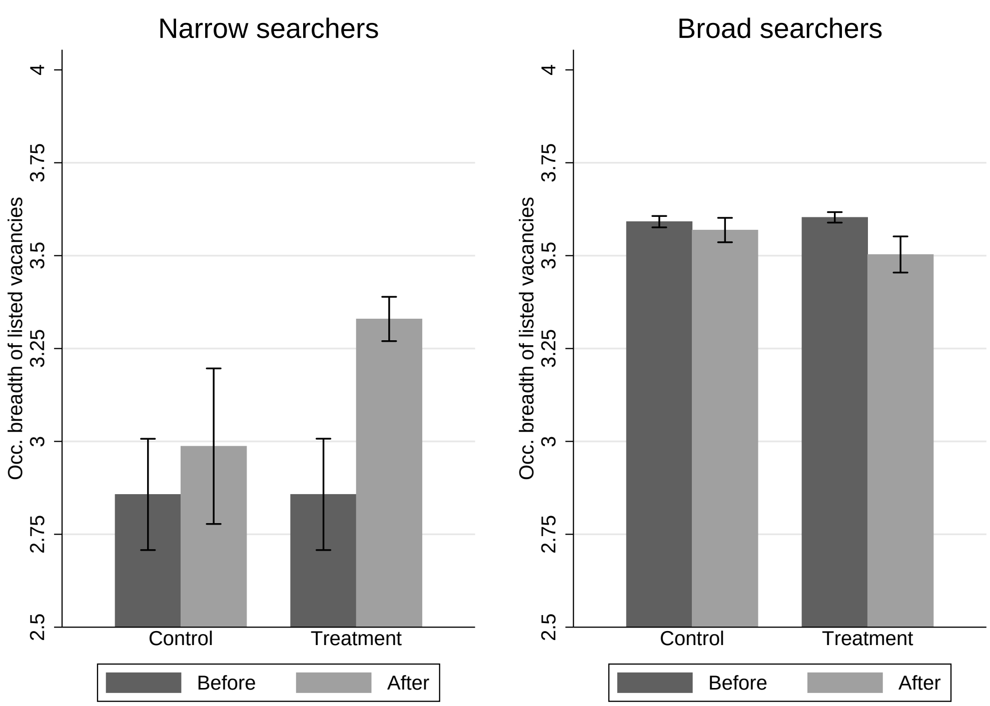


Figure 12 shows that both in the treatment and in the control group there is regression to the mean at least in point estimates: Narrow searchers before the intervention become broader after the intervention both in the control and in the treatment group, and broad searchers before the intervention become narrower after the intervention both in the control and treatment group. But the magnitude is larger for the treatment group.

Table 15: Job search activity over time (only control group survivors until week 10)

	(1) Hours search per week	(2) Breadth of listed vac.	(3) Number of listed vac.	(4) Breadth of applications	(5) Number of applications
Time trend	0.057	0.014***	7.86*	-0.0046	-0.12*
	(0.066)	(0.0050)	(4.28)	(0.015)	(0.064)
Individual FE	yes	yes	yes	yes	yes
Mean of dep. var.	12.1	3.29	542.4	3.07	3.86
Weeks	1-12	1-12	1-12	1-11	1-11
N	833	918	920	418	849

All regressions contain only control group individuals. "Time trend" is a linear weekly trend. Standard errors clustered by individual in parentheses. Sample contains only control group individuals that attended at least one session in week 10, 11 or 12. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Relation between breadth/unemployment duration and individual characteristics

	(1) Breadth dummy	(2) Breadth continuous	(3) Unemployment duration dummy	(4) Unemployment duration continuous
Age	-0.04**	0.01	0.01	3.10
	(0.02)	(0.02)	(0.02)	(3.21)
Age^2	0.03	-0.04	-0.02	-4.35
	(0.02)	(0.03)	(0.02)	(4.12)
Gender	0.07	0.08	0.04	0.91
	(0.06)	(0.07)	(0.06)	(10.78)
Weeks unemployed	0.00	-0.00		
	(0.00)	(0.00)		
Weeks unemployed^2	-0.00	0.00		
	(0.00)	(0.00)		
Financial problems	0.04	0.04	0.10	-14.67
	(0.06)	(0.07)	(0.06)	(10.60)
Married/cohabiting	-0.01	-0.05	0.06	-16.71
	(0.07)	(0.09)	(0.07)	(12.61)
Children	-0.08	-0.05	-0.13*	22.56*
	(0.07)	(0.09)	(0.08)	(13.45)
High educated	-0.08	-0.05	-0.01	23.14**
	(0.06)	(0.08)	(0.06)	(11.17)
White	0.02	0.22**	0.16**	4.09
	(0.07)	(0.09)	(0.08)	(13.41)
Constant	1.44***	3.36***	0.15	-19.97
	(0.32)	(0.41)	(0.34)	(59.68)
Observations	295	295	295	295
R^2	0.178	0.213	0.044	0.044

Standard errors in parentheses. The dependent variable is a dummy for searching broad in weeks 1-3 in column (1), a continuous breadth measure in column (2), a dummy for having unemployment duration above the median in column (3) and the continuous unemployment duration (in weeks) in column (4). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: Random effects vs Fixed Effects: Hausman tests

	Listed	Applications				Interviews		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Breadth	Breadth	In lab	Outside lab	Total	In lab	Outside lab	Total
Treatment (fe model)	0.13**	0.062	0.066	-0.031	0.011	0.57	0.25	0.29
	(0.062)	(0.21)	(0.16)	(0.095)	(0.094)	(0.95)	(0.38)	(0.35)
Treatment (re model)	0.15***	0.12	0.0081	-0.077	-0.033	0.29	0.18	0.20
	(0.06)	(0.15)	(0.14)	(0.09)	(0.09)	(0.47)	(0.22)	(0.21)
P-val Hausman test[a]	0.58	0.69	0.28	0.18	0.19	0.68	0.83	0.74
Model	Linear	Linear	Neg. Bin	Neg. Bin	Neg. Bin	Poisson	Poisson	Poisson
Included weeks	1-12	1-11	1-11	1-11	1-11	1-10	1-10	1-10
N	540	305	410	428	424	134	306	314

Standard errors in parentheses. A time period dummy is included in all regressions (but not reported). [a] P-value of a chi-squared test of equal estimates for the treatment effect. Column (1) concerns listed vacancies, columns (2)-(5) concern applications and columns (6)-(8) concern interviews. We report [exp(coefficient) − 1] in columns (3)-(8), which is the percentage effect. Estimates from the random effects models differ from other tables because no other variables are included here (individual characteristics and time-slot fixed effects).

Table 18: Effect of intervention on the number of applications - alternative specifications

	Number of Applications		
	(1)	(2)	(3)
	In lab	Outside lab	Both
Treatment	-0.01	-0.07	-0.02
	(0.17)	(0.11)	(0.11)
Treatment			
X occupationally broad	-0.08	-0.05	-0.02
	(0.23)	(0.18)	(0.19)
X occupationally narrow	0.05	-0.09	-0.04
	(0.25)	(0.12)	(0.13)
Model	Poisson RE	Poisson RE	Poisson RE
Observation weeks	1-11	1-11	1-11
Observations	541	490	487

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. We report [exp(coefficient) − 1], which is the percentage effect. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19: Effect of intervention on listed vacancies - extensions (split by geographical breadth)

	Breadth of listings		Number of listings
	(1)	(2)	(3)
	Occupational	Geographical	Lab
Treatment	0.13***	-0.01	-34.99
	(0.06)	(0.02)	(52.09)
Treatment			
X geographically broad	0.22**	-0.03	30.68
	(0.09)	(0.04)	(66.28)
X geographically narrow	0.02	0.03	-111.03
	(0.06)	(0.03)	(81.42)
Treatment			
X occ. broad and geo. broad	-0.07	0.00	126.87
	(0.05)	(0.06)	(168.51)
X occ. broad and geo. narrow	-0.09*	0.04	-123.80
	(0.05)	(0.04)	(98.25)
X occ. narrow and geo. broad	0.40***	-0.05	-11.08
	(0.13)	(0.05)	(56.80)
X occ. narrow and geo. narrow	0.21*	0.01	-83.32
	(0.11)	(0.03)	(141.01)
Model	Linear	Linear	Linear
Observation weeks	1-12	1-12	1-12
N	540	541	541

Each column represents three separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: Effect of intervention on applications - extensions (split by geographical breadth)

	Breadth of applications		Number of applications		
	(1)	(2)	(3)	(4)	(5)
	Occupational	Geographical	Lab	Outside lab	Total
Treatment	0.03	-0.06*	0.09	-0.03	0.01
	(0.20)	(0.03)	(0.16)	(0.09)	(0.09)
Treatment					
X geographically broad	-0.03	-0.10**	0.06	-0.07	0.00
	(0.26)	(0.04)	(0.21)	(0.12)	(0.12)
X geographically narrow	0.10	-0.00	0.12	0.01	0.03
	(0.25)	(0.04)	(0.24)	(0.14)	(0.13)
Treatment					
X occ. broad and geo. broad	-0.65**	-0.10	0.08	-0.17	-0.08
	(0.30)	(0.08)	(0.33)	(0.16)	(0.17)
X occ. broad and geo. narrow	-0.17	0.03	-0.16	0.05	-0.03
	(0.28)	(0.06)	(0.23)	(0.18)	(0.16)
X occ. narrow and geo. broad	0.41	-0.11**	0.05	0.01	0.06
	(0.36)	(0.05)	(0.27)	(0.16)	(0.16)
X occ. narrow and geo. narrow	0.65*	-0.04	0.71	-0.05	0.12
	(0.36)	(0.04)	(0.58)	(0.20)	(0.22)
Model	Linear	Linear	Neg. Bin.	Neg. Bin.	Neg. Bin.
Observation weeks	1-11	1-11	1-11	1-11	1-11
N	305	363	541	490	487

Each column represents three separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (3)-(5) are negative binomial model regressions where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: Effect of intervention on interviews - extensions (split by geographical breadth)

	Number of interviews		
	(1)	(2)	(3)
	Lab	Survey	Total
Treatment	0.61	0.40*	0.44*
	(0.79)	(0.27)	(0.28)
Treatment			
X geographically broad	1.90**	0.65**	0.85***
	(1.47)	(0.40)	(0.40)
X geographically narrow	0.19	0.14	0.12
	(0.81)	(0.33)	(0.36)
Treatment			
X occ. broad and geo. broad	0.99	0.41	0.42
	(2.00)	(0.56)	(0.50)
X occ. broad and geo. narrow	-0.75*	-0.27	-0.37
	(0.20)	(0.24)	(0.20)
X occ. narrow and geo. broad	1.99*	0.83**	1.14***
	(1.67)	(0.53)	(0.58)
X occ. narrow and geo. narrow	0.65	0.85	0.87
	(1.36)	(0.85)	(0.93)
Model	Poisson	Poisson	Poisson
Observation weeks	1-10	1-10	1-10
N	540	466	464

Each column represents three separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (1)-(3) are Poisson regression models where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 22: Effect of intervention - all coefficients

	(1) Number of listed	(2) Total number of applications	(3) Total number of interviews
Treatment	-34.99	0.01	0.44*
	(52.09)	(0.09)	(0.28)
Age	4.71	0.08*	-0.01
	(14.04)	(0.05)	(0.04)
Age^2	-12.52	-0.10*	-0.01
	(18.49)	(0.05)	(0.06)
Gender	72.41	-0.12	0.29
	(47.31)	(0.12)	(0.22)
Weeks unemployed	-0.71	0.00	-0.01*
	(0.66)	(0.00)	(0.00)
Weeks unemployed^2	0.01	0.00	0.00
	(0.09)	(0.00)	(0.00)
Financial problem	101.74*	0.00	0.26
	(52.81)	(0.14)	(0.19)
Couple	-73.41	-0.09	0.38
	(48.40)	(0.15)	(0.31)
Children	-84.87	0.11	0.05
	(56.63)	(0.19)	(0.19)
High educated	-24.84	-0.01	0.23
	(59.23)	(0.15)	(0.23)
White	54.64	-0.10	-0.03
	(69.10)	(0.16)	(0.18)
Constant	584.41**	4.43*	-0.13
	(276.25)	(3.82)	(0.72)
Model	Linear	Neg. Bin.	Poisson
Observation weeks	1-12	1-11	1-10
N	541	487	464

Each column represents one regression. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup) and individual random effects. Columns (2) and (3) are Poisson regression models where we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8.4 Detailed discussion of robustness checks

Below we discuss in more detail the robustness checks that have been performed with regard to the empirical model and of the definition of variables. We consider (1) individual fixed effects instead of random effects, (2) weekly observations instead of aggregated data, (3) linear models instead of count data models, (4) excluding the last one or two observations per individual, (5) an alternative breadth measure and (6) IV regressions with the use of the alternative interface as the treatment intensity.

As we discuss in section 5.1, we used individual random effects models in all empirical analysis up to this point to increase precision. A Hausman test does not reject validity of the random effects model. In Table 23 we show our baseline regressions using individual fixed effects instead of random effects. We include regressions with outcome variables: breadth of listed vacancies, breadth of applications, number of applications (in the lab, outside the lab, and total) and the number of interviews (in the lab, outside the lab, and total). For each outcome we show the overall effect and the effect by initial occupational breadth. We find very similar overall pattern but reduced precision and significance. Occupational breadth of listed vacancies increases significantly for narrow searchers, and decreases (slightly) for broad searchers. For breadth of applications the estimates suggest a similar pattern, but none of these are statistically significant. Similar to our baseline estimates we find no effect on the number of applications. For interviews we find large positive coefficients for narrow searchers, but due slightly reduced precision these are not statistically significant.

While we have (at most) 12 weekly observations per individual, we use data in all estimations that has been aggregated into two observations per individual (before and after the intervention). We do so to minimize problems related to serial correlation (as suggested by Bertrand et al. (2004)). We can estimate the same regressions including all observations. The specification is identical except that we now include 12 time fixed effects instead of 2. We present the key results in Table 24. We find that patterns are very similar: breadth of listed vacancies increases (strongly for narrow searchers), the same happens with breadth of applications (but not significantly) with no significant effect on the number of applications. The point estimate for the number of interviews remains economically large but slightly lower, and retains significance only for narrow searchers. For them it is significant both inside and outside the lab.

As a third robustness check we consider the model specification for the number of applications and interviews. Since these are count variables and contain many zeros, we use Poisson regressions or negative binomial regressions in the main analysis. One might wonder whether the use of these models drives our results. In Table 25 we present linear regressions for the main specifications in which we used non-linear models. These are the number of applications (in the lab, outside the lab, and total) and the number of interviews (in the lab, outside the lab, and total). We find similar patterns when using simple linear regression: there is no clear impact on applications, but the point estimate for interviews is economically large, and significant for narrow searchers.

The fourth robustness check considers the way we obtain our data on applications and interviews in the lab. As discussed, participants can save a vacancy if they are interested, and will be asked whether they applied in subsequent weeks. Once they have applied, they will be asked whether they

Table 23: Robustness: intervention effect using individual fixed effects

	Listed		Applications			Interviews		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Breadth	Breadth	In lab	Outside lab	Both	In lab	Outside lab	Both
Treatment	0.13**	0.062	0.066	-0.031	0.011	0.57	0.25	0.29
	(0.062)	(0.21)	(0.16)	(0.095)	(0.094)	(0.73)	(0.25)	(0.26)
X occupationally broad	-0.060**	-0.098	-0.070	-0.019	-0.026	-0.38	0.25	0.050
	(0.029)	(0.24)	(0.20)	(0.15)	(0.14)	(0.41)	(0.43)	(0.32)
X occupationally narrow	0.32***	0.23	0.19	-0.056	0.029	1.08	0.22	0.47
	(0.11)	(0.35)	(0.26)	(0.12)	(0.12)	(1.21)	(0.30)	(0.41)
Model	Linear (Ind. FE)	Linear (Ind. FE)	Neg. bin. (Ind. FE)	Neg. bin. (Ind. FE)	Neg. bin. (Ind. FE)	Poisson (Ind. FE)	Poisson (Ind. FE)	Poisson (Ind. FE)
Observation weeks	1-12	1-11	1-11	1-11	1-11	1-10	1-10	1-10
N	540	305	410	428	424	134	306	314

Each column represents two separate regressions. All regressions include individual fixed effects and period fixed effects (separately for each subgroup). Column (1) concerns listed vacancies, columns (2)-(5) concern applications and columns (6)-(8) concern interviews. Columns (1)-(2) are linear regressions, columns (3)-(5) are negative binomial regressions, and columns (6)-(8) are Poisson regression models. In columns (3)-(8) we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses (except for the neg. bin. model). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 24: Robustness: intervention effect using weekly data

	Listed		Applications				Interviews	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Breadth	Breadth	In lab	Outside lab	Both	In lab	Outside lab	Both
Treatment	0.11*	0.0038	0.077	-0.056	-0.028	0.56	0.24	0.29
	(0.058)	(0.15)	(0.11)	(0.064)	(0.058)	(0.66)	(0.23)	(0.25)
Treatment								
X occupationally broad	-0.055	-0.22	-0.065	-0.12	-0.12	-0.44	-0.11	-0.18
	(0.034)	(0.17)	(0.12)	(0.085)	(0.075)	(0.32)	(0.26)	(0.23)
X occupationally narrow	0.27***	0.26	0.21	-0.0020	0.051	1.32*	0.61*	0.73*
	(0.086)	(0.22)	(0.16)	(0.089)	(0.083)	(1.18)	(0.42)	(0.48)
Model	Linear	Linear	Neg. Bin.	Neg. Bin.	Neg. Bin.	Poisson	Poisson	Poisson
Observation weeks	1-12	1-12	1-11	1-11	1-11	1-10	1-10	1-10
N	2392	934	2251	2016	1984	2098	1776	1744

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Column (1) concerns listed vacancies, columns (2)-(5) concern applications and columns (6)-(8) concern interviews. Columns (1)-(2) are linear regressions, columns (3)-(5) are negative binomial regressions, and columns (6)-(8) are Poisson regression models. In columns (3)-(8) we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses (except for the neg. bin. model). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 25: Robustness: intervention effect using linear models

	Applications			Interviews		
	(1)	(2)	(3)	(4)	(5)	(6)
	In lab	Outside lab	Both	In lab	Outside lab	Both
Treatment	0.17	-0.21	0.024	0.039	0.12	0.17
	(0.46)	(0.87)	(1.22)	(0.043)	(0.084)	(0.11)
Treatment						
X occupationally broad	0.093	-0.028	0.045	0.00070	0.057	0.023
	(0.58)	(1.33)	(1.86)	(0.041)	(0.12)	(0.13)
X occupationally narrow	0.25	-0.38	-0.020	0.074	0.18*	0.30**
	(0.63)	(1.03)	(1.44)	(0.068)	(0.10)	(0.15)
Model	Linear	Linear	Linear	Linear	Linear	Linear
Observation weeks	1-11	1-11	1-11	1-10	1-10	1-10
N	541	490	487	540	466	464

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (1)-(3) concern applications and columns (4)-(6) concern interviews. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 26: Robustness: intervention effect excluding each individual's last one or two weeks

	Applications				Interviews		
	(1) Breadth	(2) In lab	(3) Outside lab	(4) Both	(5) In lab	(6) Outside lab	(7) Both
Treatment	0.0037	0.099	-0.021	0.029	0.38	0.37*	0.42*
	(0.20)	(0.15)	(0.091)	(0.091)	(0.66)	(0.26)	(0.28)
Treatment							
X occupationally broad	-0.47**	-0.053	-0.028	-0.023	-0.57	-0.035	-0.098
	(0.22)	(0.18)	(0.13)	(0.12)	(0.29)	(0.28)	(0.24)
X occupationally narrow	0.47	0.26	-0.014	0.082	1.18	0.80**	0.96**
	(0.29)	(0.25)	(0.13)	(0.13)	(1.23)	(0.44)	(0.52)
Model	Linear	Neg. bin.	Neg. bin.	Neg. bin.	Poisson	Poisson	Poisson
Observation weeks	Varying	Varying	Varying	Varying	Varying	Varying	Varying
N	302	499	487	484	473	464	462

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Columns (1)-(4) concern applications and columns (5)-(7) concern interviews. Column (1) is a linear regression, columns (2)-(4) are negative binomial regressions, and columns (5)-(7) are Poisson regression models. In columns (2)-(7) we report [exp(coefficient) − 1], which is the percentage effect. Standard errors clustered by individual in parentheses (except for the neg. bin. model). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 27: Robustness: intervention effect using a different breadth measure based on transitions observed in the BHPS

	Listed		Applications			Interviews		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Breadth	Breadth	In lab	Outside lab	Both	In lab	Outside lab	Both
Treatment	0.0053**	0.0028						
	(0.0027)	(0.0097)						
Treatment								
X occupationally broad	-0.0012	-0.0098	-0.042	-0.049	-0.070	-0.84	0.16	0.031
	(0.0024)	(0.0094)	(0.21)	(0.13)	(0.13)	(0.74)	(0.28)	(0.30)
X occupationally narrow	0.0076	0.015	0.20	-0.018	0.091	1.07**	0.50**	0.64**
	(0.0053)	(0.015)	(0.20)	(0.13)	(0.12)	(0.50)	(0.25)	(0.25)
St.Dev. dep. var.	.029	.047						
Model	Linear	Linear	Neg. bin.	Neg. bin.	Neg. bin.	Poisson	Poisson	Poisson
Observation weeks	1-12	1-11	1-11	1-11	1-11	1-10	1-10	1-10
N	540	305	541	490	487	540	466	464

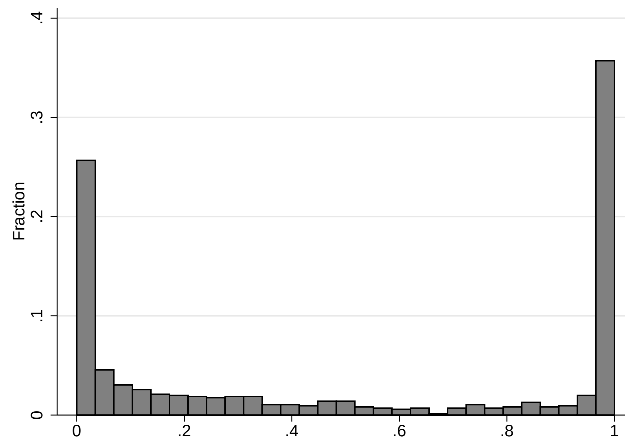
Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Column (1) concerns listed vacancies, columns (2)-(5) concern applications and columns (6)-(8) concern interviews. Columns (1)-(2) are linear regressions, columns (3)-(5) are negative binomial regressions, and columns (6)-(8) are Poisson regression models. In columns (3)-(8) we report [exp(coefficient) $-$ 1], which is the percentage effect. Standard errors clustered by individual in parentheses (except for the neg. bin. model). The outcome measure in column (1) is breadth of listed vacancies based on the BHPS transitions. The outcome measure in column (2) is breadth of applications based on the BHPS transitions. The groups used in this table ("occupationally broad" and "occupationally narrow") are defined using the breadth measure based on BHPS transitions. Note that the blank spaces in this table are specifications that are unaffected by the breadth measure and would thus produce the same result as our baseline specification. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 28: Robustness: intervention effect using alternative interface usage instrumented with treatment assignment

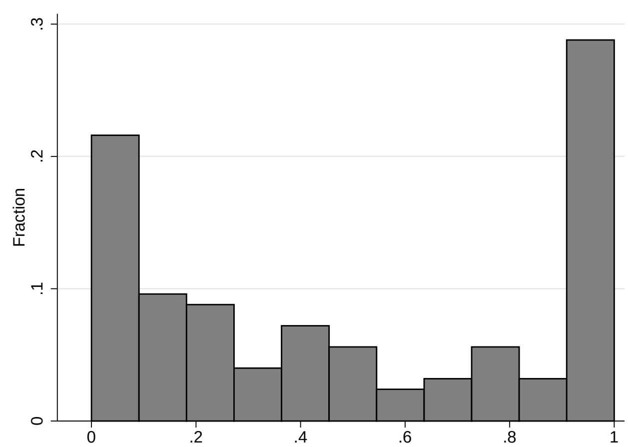
	Listed		Applications				Interviews	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Breadth	Breadth	In lab	Outside lab	Both	In lab	Outside lab	Both
Alt. interface use	0.24**	0.036	0.32	-0.42	0.019	0.072	0.24	0.32
	(0.12)	(0.38)	(0.85)	(1.64)	(2.29)	(0.080)	(0.16)	(0.20)
Alt. interface use								
X occupationally broad	-0.18**	-1.10**	0.18	-0.21	-0.025	0.0014	0.13	0.053
	(0.090)	(0.55)	(1.38)	(3.12)	(4.36)	(0.099)	(0.29)	(0.32)
X occupationally narrow	0.54***	0.84*	0.41	-0.58	-0.016	0.12	0.30*	0.49**
	(0.16)	(0.46)	(0.99)	(1.66)	(2.31)	(0.11)	(0.16)	(0.24)
Model	Linear IV	Linear IV	Linear IV	Linear IV	Linear IV	Linear IV	Linear IV	Linear IV
Observation weeks	1-12	1-11	1-11	1-11	1-11	1-10	1-10	1-10
N	540	305	541	490	487	540	466	464

Each column represents two separate regressions. All regressions include time-slot fixed effects, period fixed effects (separately for each subgroup), individual random effects and individual characteristics. Column (1) concerns listed vacancies, columns (2)-(5) concern applications and columns (6)-(8) concern interviews. All columns are linear IV regressions in which the use of the alternative interface is instrumented for by treatment assignment. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

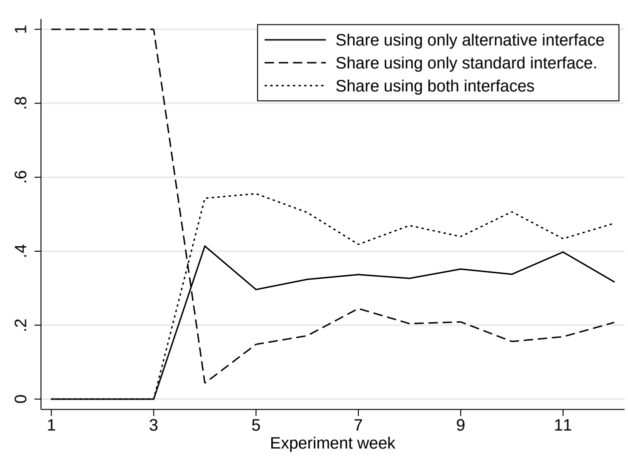
Figure 13: Usage of the alternative interface (contains only the treatment group participants in weeks 4-12)



(a) Share of listed vacancies from alternative interface (user by week observations)



(b) Share of listed vacancies from alternative interface (one observation per user)



(c) Alternative interface usage over time

Table 29: Correlation between different broadness measures for listed vacancies

	M listed	G4 listed	G3 listed	G2 listed	G1 listed
M listed	1				
G4 listed	.97	1			
G3 listed	.99	.97	1		
G2 listed	.98	.94	.97	1	
G1 listed	.96	.91	.94	.98	1

M is the broadness measure used in the empirical analysis, Gx is the Gini-Simpson measure applied to the x-digit SOC code. Correlation are computed based on individual observations, collapsed into two periods as is done in the empirical analysis.

Table 30: Correlation between different broadness measures for applications

	M applied	G4 applied	G3 applied	G2 applied	G1 applied
M applied	1				
G4 applied	.73	1			
G3 applied	.80	.93	1		
G2 applied	.83	.87	.95	1	
G1 applied	.79	.79	.87	.91	1

M is the broadness measure used in the empirical analysis, Gx is the Gini-Simpson measure applied to the x-digit SOC code. Correlation are computed based on individual observations, collapsed into two periods as is done in the empirical analysis.

received an interview. Most applications are sent in the first week after saving the vacancy (86%), while most interviews are obtained in the first two weeks (83%). As a result, we must observe an individual one week after saving a vacancy to obtain information about applying and two weeks after saving a vacancy to obtain information about an interview. This is the reason that we exclude week 12 in regressions for applications and weeks 11 and 12 in regressions for interviews. Alternatively, we can exclude for each individual his/her last one or two observations. The results from the main specification when using this approach are shown in Table 26. The results are very similar, with again no impact on applications overall though broad individuals apply significantly less broad and narrow individuals apply broader (but insignificantly). Also the significantly positive effects on interviews overall and for narrow individuals in particular remain.

Fifth, we consider our method for defining occupational breadth of job search. In our approach the distance between two occupations is based on the number of common digits of the two occupational codes (see section 3.7 for the detailed description). Alternatively, one can focus on a particular digit of the occupational code and call occupations identical if they share the same code up to that digit. Broadness can then be defined by the well-known Gini-Simpson index. The several different measures of breadth are highly correlated; for example, for listed vacancies our measure has a correlation above 0.95 with 4 different Gini-Simpson measures (see Tables 29 and 30). Not surprisingly we find very similar results when we adopt this alternative measure. A more elaborate alternative is to use empirically observed transitions between occupations in labor market surveys to measure the "closeness" of the two occupations.[62] We apply this approach to measure the breadth of listed vacancies and the breadth

[62] We thank an anonymous referee for this suggestion.

Table 31: Characteristics of the treatment and control group (based on the first week initial survey), for different groups of survivors

	Control group survivors in:			Treatment group survivors in:			T-test (p-value) for equality in:		
	week 1	week 4	week 12	week 1	week 4	week 12	week 1	week 4	week 12
Demographics:									
female (%)	42	43	34	43	41	41	0.83	0.87	0.43
age	36	36	37	36	37	40	0.85	0.32	0.15
high educ [a] (%)	44	43	48	41	41	43	0.63	0.77	0.55
survey qualification level	4.2	4.3	4.4	4.4	4.4	4.6	0.36	0.44	0.48
white (%)	80	81	81	80	81	77	0.97	0.97	0.59
number of children	0.66	0.68	0.77	0.38	0.39	0.46	0.01	0.03	0.08
couple (%)	25	24	26	21	20	19	0.41	0.35	0.3
any children (%)	31	30	33	24	24	29	0.17	0.33	0.62
Job search history:									
vacancies applied for	75	69	92	53	43	34	0.18	0.09	0.01
interviews attended	2.4	2.5	1.9	2.2	1.9	1.6	0.68	0.41	0.46
jobs offered	0.37	0.42	0.44	0.48	0.43	0.47	0.43	0.96	0.90
at least one offer (%)	20	21	23	20	18	19	0.91	0.50	0.52
days unempl. (mean)	290	291	305	228	182	190	0.39	0.13	0.14
days unempl. (median)	81	84	87	77	78	80			
less than 183 days	0.75	0.76	0.73	0.78	0.79	0.76	0.60	0.54	0.64
less than 366 days	0.84	0.85	0.82	0.87	0.89	0.86	0.54	0.41	0.51
jobseekers allow. (£)	49	49	46	56	59	65	0.46	0.38	0.27
housing benefits (£)	65	67	81	62	62	74	0.90	0.82	0.81
other benefits (£)	9.7	11	1.6	18	19	26	0.41	0.5	0.21
Weekly search weeks 1-3:									
listed	493	513	477	493	464	415	1	0.32	0.33
viewed	25	25	26	26	25	24	0.57	0.81	0.36
saved	10	10	12	11	10	9.7	0.54	0.86	0.32
applied	3.3	3.8	4.6	2.5	2.7	2.6	0.14	0.13	0.035
interview	0.098	0.11	0.11	0.083	0.096	0.08	0.66	0.65	0.55
applications other	9.3	9.2	11	7.4	7.5	6.7	0.13	0.17	0.027
interviews other	0.54	0.51	0.32	0.47	0.47	0.52	0.48	0.69	0.11
broadness listed[b]	3.2	3.2	3.2	3.3	3.2	3.1	0.50	0.57	0.39
broadness applied[b]	3	3	3	3.2	3.2	3.1	0.34	0.40	0.45
hours spend[c]	11	11	11	12	12	12	0.15	0.34	0.61
concern health (1-10)	1.5	1.3	1.8	1.7	1.8	2.1	0.48	0.12	0.47
conc. financial (1-10)	7.2	7.3	7.1	7	6.9	7.1	0.47	0.29	0.93
conc. competition (1-10)	7.4	7.5	7.3	7.2	7.2	7.3	0.43	0.37	0.97
met caseworker (%)	32	32	30	28	28	27	0.48	0.45	0.58
Observations	152	127	73	143	123	79			

of applications.[63] In addition, we use the breadth of listed vacancies in the first three weeks (as measured by this method) to define the groups "narrow" and "broad" searchers (as we do in all main analysis). The results of the main regressions are presented in Table 27. We find that the effect on breadth of listed is similar: breadth increases significantly for the full sample and the effect is larger for narrow searchers (though not significant due to slightly lower precision). Note that the new breadth measure has a different scale and to interpret the magnitude of the effect we include the standard deviation of the dependent variable in the table. We find that the effect of the intervention is about 1/6 of a standard deviation, which is very similar to the effect of 1/5 of a standard deviation in our baseline. For applications we find that results are very similar to our baseline results. The coefficients suggest an increase in breadth for narrow searchers and a decrease for broad searchers, though neither is statistically significant. We find no effect on the number of applications, and a significant increase in interviews (both in the lab and outside the lab) for narrow searchers.

Finally we consider an interpretation that all our results are intention-to-treat effects. Since using the alternative interface was voluntary for all individuals in the treatment group, some changed back to the normal interface quickly while other used it continuously for 9 weeks (we show the extent to which users use the alternative interface in Figure 2). One might argue that not all job seekers in the treatment group were treated (with the same intensity). We are hesitant to emphasize this interpretation too much, because suggestions about alternative occupations can affect job seekers even after a user switches back to the standard interface. They might simply search for the suggested occupations on the standard interface. The suggestions might even affect job search through other channels. However, for the sake of comparison, we can consider treatment assignment as an instrument for actual usage when estimating our empirical models. We define actual usage as the share of listings that is performed using the alternative interface in a particular week. This share is around 50% for the treatment group and differs substantially depending on the different groups (as is shown in figure 2). The results of estimating the effect of alternative interface usage, using treatment assignment as an instrument, are presented in Table 28. As expected, the estimates are larger in magnitude. We find that breadth of listed vacancies increases (with a coefficient of 0.24** compared to 0.13** in the baseline results). Additionally, we also find that breadth of applications increases significantly for narrow searchers. The number of applications is unaffected, and interviews increase significantly for narrow searchers.[64]

[63] We use occupational transitions in the BHPS (that we also apply to generate suggestions in the search interface). The advantage of this approach is that theoretically this creates a continuous measure of closeness between two occupations and that this measure is based on real-world transitions. In praxis there is a downside due to sample size: the transitions identify a limited number of occupations to which transitions are somewhat common (often no more than 5) and assign a zero to the rest. The reason is the limited size of the BHPS relative to the large number of possible transitions (353 occupations lead to $353^2 = 124,609$ possible transitions).

[64] Note that we use linear models for all instrumental variable specifications.

8.5 Experimental instructions and supplemental documents

8.5.1 Consent form

Consent Form for Participants: "How Do Unemployed Search for Jobs?"

Thank you for your willingness to consider taking part in this study. Please read the information below carefully. By signing the consent form below, you indicate that you have understood the purpose of the study, you have been made aware of your rights and you have agreed with the terms and conditions of the study.

Purpose of the study

The study is undertaken to understand better how people search for jobs. The study aims to observe how people search for real jobs. The goal is to document parts of the job search process.

How will this work?

The study will be conducted over a period of 12 weeks and you are asked to take part to one weekly session of 2 hours taking place at a pre-agreed time slot. You will be asked to come to our computer facilities, located at the School of Economics, 31 Buccleuch Place, EH8 9JT Edinburgh. There will be a maximum of 30 participants present at the same time in the facilities. The research team aims to provide an environment that is conducive to the job search of participants and hopes that participants will attend for the duration of the study or up to the point you find a job.

You will be able to spend most time each week to search for job vacancies. These job vacancies are obtained from two sources:

- Our main data source is the vacancy database of Universal Jobmatch and coincides with those used at Jobcentre Plus.
- Additionally, our database includes a small number of vacancies (no more than 2 per 100 vacancies) that is added for research purposes. These "research vacancies" are included to understand better which types of vacancies people are interested in even if these are not currently offered. If you express interest in such a vacancy, you will be immediately informed that this is a research vacancy before you start any application.

We will track the pages you consult, what vacancies you are looking at and consider applying to. This information will never be linked to any of your personal information such as your name and address, which will be stored separately. Your personal information will never be given out to anyone and will be accessible only to selected members of the research team.

You will also be asked some survey questions about your job search in the past week and your wellbeing. In the initial week, we will also ask a number of questions about your background and unemployment history. Six month after the end of your participation we will send you a survey about your labour market experience and your well-being.

Note that we ask all participants to stay for the full 2 hours in the laboratory. But if you do not want to search for jobs anymore, we provide some alternative ways in which you can use the computer and internet facilities.

If you are unable to participate to a session, please inform us as soon as possible (under jobsearch@ed.ac.uk or 0131 6508324). The research team will attempt to provide additional slots in case a participant misses his time slots for justified reasons (e.g., job interviews, illness).

Important notes

- Participation to this study is entirely voluntary. You should by no means feel complied to participate. You can also withdraw from the study at any time if you wish to do so.

- Since the study is to gain understanding in how people search for jobs, the research team holds no particular view on how individuals should search for jobs. Thus, you should search for jobs in the same way as you would normally do.

- The study is conducted by the research team, and no personalized information is shared with any other organization. Therefore, no information will be shared with Job Centre Plus or the Department of Work and Pensions. If you would like to obtain a record of your search activities, e.g. to use for discussion with your case worker, you can obtain a printed record to take along at the end of each session.

- You should be aware that **participation in this study does not provide any additional benefits**, and in particular it does not provide particular help in job search. In particular, you **<u>should follow your usual job search strategy</u>**, such as for example looking at other job vacancies beyond those provided in our database, searching from home via the internet, and contacting friends and acquaintances. You should not take the time within the study as an indication of the appropriate time to spend on searching for a job.

- All the data collected during your time in our computer facility is anonymous. Your search activities will not be matched to your identity in any way. You will be attributed a randomly generated number at the first session and all data records will be matched to that number.

- We will ask you for a telephone number that we can use to contact you. We will only contact you to remind you of the time slot you have been allocated to and to inform you of any changes in schedule. Of course the telephone number will not be matched to the data we collect in the laboratory.

- You have the right to withdraw entirely from the study (i.e. ask us to delete all the data records associated with you) at any point during the study.

- The impersonal data collected will be used for research purposes (and ONLY for research purposes). Personal data will never be given out, and will be eliminated after the study is completed. The results of the study will be published in peer-reviewed scientific journals.

Compensation

You will be compensated for your efforts of coming to and participating in each session in our computer facility with a compensation of £12.50 per visit (2 hours) to the laboratory. Additionally, if you participated in all four sessions in the first four weeks you are entitled to a £50 clothing voucher for job market attire as compensation for arranging the visit every week. The same holds for weeks 5 to 8 and for weeks 9 to 12.

Eligibility

Participants have to be at least 18 years of age, permanent residents of the UK and living in Edinburgh (or within a distance of 5 miles from Edinburgh). You should be seeking for a job for a period of 4 weeks or less at the start date of the study.

Signature

If any of the material above is unclear to you, or if you have any doubts and would like clarification, please consult a member of the research team before proceeding.

If you are willing to take part in this study, please sign the consent form below:

I certify that I voluntarily participate in this research study. I certify that I read and understood the information above, and am eligible for taking part in this study.

(please print your name)

(please sign)

(place and time of signature)

8.5.2 Lab instructions

UNIVERSITY JOB SEARCH STUDY: INSTRUCTIONS

Please do not start using the computer before we indicate you to do so.

We will read these instructions aloud at the start of the first session.

INTRODUCTION

Welcome and thank you for coming here today. Before we explain how each session will work, we would like to raise your attention to the following:

- **Health and Safety**: There will always be one person from the research team in the computer room. There is one toilet on this floor that you are free to use. In case of fire, please do follow the signs for fire exit. The main exit is through the staircase you have used to come up here.
- **No smoking**: Smoking is not allowed in this building.
- **Silence**: Since there are many of you in the room, we would appreciate if you would keep silent, so that everyone can concentrate on their computer activity.
- **Mobile phones**: Mobile phones must either be switched off or be on "silent" during each session. We would appreciate if you leave it on only if you are expecting an important phone call. And if you do receive a phone call, please leave the room and take the call outside (in the staircase).
- **Food and drinks** are not allowed in this room.
- **Questions**: Please do not hesitate to call us if you have a question.

WHAT IS THE STUDY ABOUT?

The goal of the study is to understand how people search for jobs. Importantly, we hold no preconceptions regarding how people *should* search for jobs. We designed this study to find out what people usually do and what strategies are most successful. At the moment, we do not know what these are. We are interested in finding out common patterns in search strategies, and kindly ask you to search exactly in the same way as you normally would.

WHAT WILL HAPPEN IN EACH SESSION

When you come in, you will be assigned to a computer station. We may provide specific instructions at the beginning of the session, so please do wait for us to indicate the start of the session. We will now describe how each session will proceed.

1. **LOGIN**

You have received a unique login number and password that you can use to login on the website here and also from home. You will be able to access your records using this login information.

2. SURVEY

Each weekly session will start with a **short survey**, asking questions about your past week and job search. After filling the survey, you will be re-directed towards the job search engine's main page.

For the first session, we will ask you to fill in a longer survey asking you questions about your background, qualifications and job search experience so far. You will only need to answer this initial survey once, in this session. It should take 20 minutes to fill in this initial survey.

3. THE JOB SEARCH ENGINE

We have designed our own job search engine. It allows you to search through all UK vacancies that are also recorded in Universal Jobmatch.

We ask you to search for jobs using this search engine only for a minimum of 30 minutes.

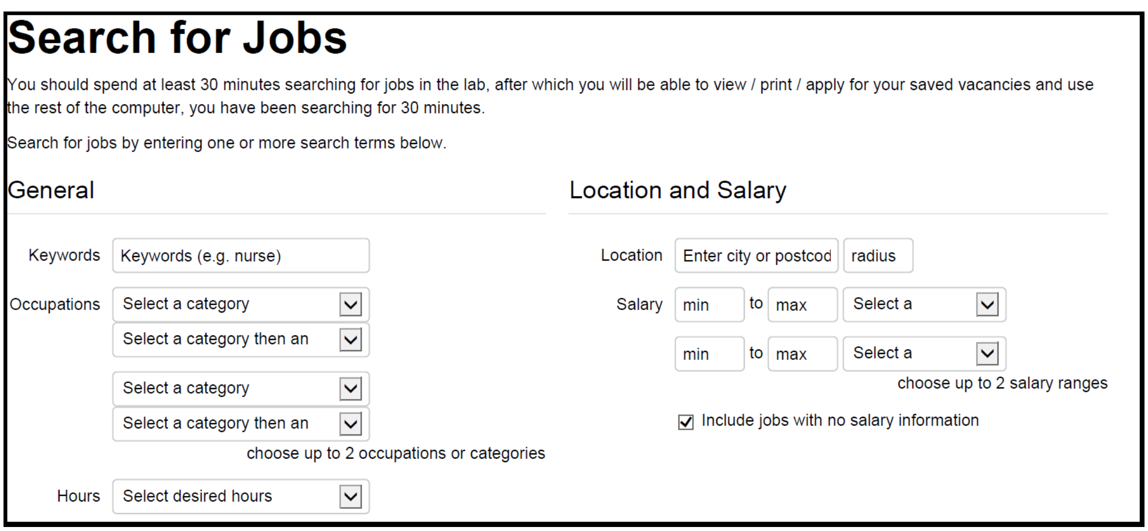
You can search using various criteria (keywords, occupations, location, salary, preferred hours). Importantly, you do not have to specify all of these. You just need to fill at least one of them.

If you specify more than one criterion, it is important to note that the computer will search for vacancies that satisfy all the criteria at the same time. For example, if you enter a keyword and you also select an occupation, it will search for vacancies that match both at the same time. Vacancies that match the keyword but not the occupation will not be shown.

Within some categories you can fill in more than one field. For example, within "occupations" you can specify up to two of them. If you do fill in two occupations, the computer that match either the first OR the second occupation. Vacancies that match one occupation but not the other will still be shown. You can also specify more than one pay range. This allows you to specify, for example, the hourly wages and the yearly wages that you are willing to accept. If you only specify hourly wages, it will not show vacancies that only specify yearly wages.

If you fill in your preferred hours, for example full time work, it will only list vacancies where the employer ticked a box that it is full-time work. Vacancies where the employer did not explicitly state that it is full-time work will not be shown.

If you leave a field empty, the computer will not use that criterion to restrict your search.



Once you have defined your search criteria, you can press the search button at the bottom of the screen and a list of vacancies fitting your criteria will appear. You can click on each individual vacancy to get more information about it. You can then either

- **Save the job (if you are interested in applying)**
- **Do not save the job (if you are not interested)**

If you save the job, the computer will keep a record of the vacancy. You will be able to see all records of all saved vacancies at the end of the session.

If you do not want to save the job and want to go back to the search results, we will first ask you a few questions about why you are not interested in the job. Your answers are very important to us.

You can modify your search criteria at any point and launch a new search.

Note that we have also created a small number of vacancies ourselves (about 2% of the database), which are there for research purposes only. This is to learn whether you would find these vacancies attractive and would consider applying to them if they were available. We kept them to a minimum not to disturb your search. These vacancies will appear as all the other vacancies and may appear in your search results. But we will inform you at the end of the 30 minutes of any vacancy that may not be real. You will be able to see the list of your saved vacancies immediately after the 30 minutes are over, and we will indicate if any of them was an artificial one.

We may try alternative interfaces for the job search engine in the coming weeks. We will inform you if we do so and will explain the changes at that point in time.

4. FREE USE OF THE FACILITIES (after 30 minutes)

We will let you know when the first 30 minutes are over. You will then be free to use the computer for other purposes. You can of course keep searching using our job search engine, or you can do other things, such as write your CV, write a letter, or even send e-mails. You can use the facilities for up to 2 hours.

If you do not wish to continue searching or use the computer for other purposes, you are free to leave.

END OF THE SESSION

We can print a record of your job search for the day (just call us once you have finished), but only if that is your wish. You are free to show these records to your adviser at the Job Centre. They informed us that this would count as a proof of search activity.

Compensation: In general, you will receive a total of £11 as a compensation for your travel and meal expenses. This time, as you will soon discover in the initial survey, we do offer you the possibility of investing part of this compensation in this initial session. This is not compulsory. But if you do choose an investment option, your earnings will then be a function of what investment you have chosen.

Please collect your compensation from the registration room. You will get an envelope and be asked to sign a receipt. Note that the Job Centre has agreed that these £11 are a compensation for expenses and are not an income.

IMPORTANT NOTES

LOG IN FROM HOME OR FROM ANOTHER COMPUTER

You will be able to use our search engine from home or from another computer as well. You just need to log in on the website and use your login information. You will be able to see all the vacancies you saved and will be able to retrieve all the relevant information about them.

Note that as indicated in the consent form, all records saved are anonymous. These will not be matched to your names at any point.

YOUR COMMITMENT

Note that it is very important for us that you come back every week and search in our facilities, unless of course you have found a job. If for one reason or the other you do have to cancel your session in a given week, please let us know as soon as possible. We will either try to reallocate you to another slot or ask you to search from home in that particular week. If you have found a job, please do let us know. This is of course of key importance for our study.

Also, importantly, you will receive a £50 clothing voucher for <u>each four consecutive weeks</u> you come. The first voucher will be distributed in the fourth week, that is, three weeks from now. The second voucher will be distributed in the eighth week and the third voucher in the twelfth week.

Thank you very much for your attention. If you have any questions, please raise your hand and we will come to you.

8.5.3 Lab instructions alternative interface

PLEASE READ

NEW JOB SEARCH INTERFACE

IMPORTANT CHANGES

We have designed a new search interface that should give you a better idea of jobs that might be relevant to you. This new interface suggests additional types of jobs (occupations) that are related to your preferred occupation.

You will be asked to specify your preferred occupation and the interface will return suggestions of other occupations that may be of interest to you. They may not all be relevant, but hopefully some will be relevant and will allow you to broaden your search horizon.

We use two methodologies to do this:

The first is using information from national labour market statistics, which follows workers over time and record in what occupation they are employed. The data records transitions between occupations and we can identify the most common occupations people switch to from a given occupation. We will ask you to indicate your preferred occupation using a keyword search and selecting the relevant title in a drop-down menu. The second is using information on transferable skills across occupations from an American website (called O*net). For each occupation, we will suggest up to 10 related occupations that require similar skills.

Since the databases are different for each of the two routes, we will ask you to specify your preferred occupation twice and select it in the menu of possible occupations. So we will ask you again to indicate your preferred occupation using a keyword search and selecting the relevant title in a drop-down menu.

Once you have specified your preferred occupation for each of the two methodologies, you can then click “Save and Start Searching” and you will be taken to a new screen that will suggest these new occupations to you.

The occupations will be listed in two columns:

The left column suggests occupation based on the first methodology (based on the UK labour market transitions). The right column suggests occupations based on the second methodology (O*net related occupations).

You can select or unselect the occupations you find relevant and would like to include in your search.

We also have information about how competitive the labour market is for a given set of occupations. We have constructed “heat maps” that use recent labour market statistics for Scotland and show you where jobs may be easier to get (because there are many jobs relative to the number of interested job seekers). These maps are based on broad categories of jobs, not on each very specific occupation. You can click on the button “heat map” to see the relevant map. We would like you to try this new interface from now on.

It is nevertheless possible to switch back to the old interface that you have used in the previous weeks. You will see a button on the screen indicating "use old interface". If you click it, you will be taken to the old search engine interface. From there you can also return the new interface.

Thank you very much for your attention.

8.5.4 Baseline survey questionnaire

INITIAL SURVEY

We will start by asking a few questions about your background and personality. Please fill in the answers as appropriate.

Gender: [drop down menu]

- ☐ Male
- ☐ Female

Country of birth: [drop down menu with all countries in alphabetical order]

Ethnicity: [drop down menu]

- ☐ Caucasian white
- ☐ East Asian
- ☐ Black African
- ☐ Black Caribbean
- ☐ Indian
- ☐ Pakistani
- ☐ Bangladeshi
- ☐ Other

Age: ____ [number]

What are the first 3 letters of the postcode of your residence? [EH1 until EH17 as dropdown menu]

Qualifications (tick the appropriate box): [drop down menu]

- ○ Ph.D.
- ○ Postgraduate Masters degree
- ○ Undergraduate Degree
- ○ Other higher education
- ○ A level / Higher or equivalent (secondary education)
- ○ GCSE
- ○ Other qualification
- ○ No qualification

Date you became unemployed: ___ / ___ / ___ [numbers]

Date of registration with Job Seeker Allowance: ___ / ___ / ___ [numbers]

Job experience

From (date) to (date)	Employer	Job title	Reason for departure
[numeric fields] ___ (month) ___ (year)	**[open field]**	**[open field]**	[drop down menu] Temporary contract Redundancy Voluntary quit

How long do you think you will need to find a job? [drop down menu]

- ☐ Less than 4 weeks
- ☐ Less than 8 weeks
- ☐ Less than 12 weeks
- ☐ Less than 6 months
- ☐ Less than a year
- ☐ it will take me more than a year

In what occupation would you prefer finding a job?

[drop down menu with the detailed list of occupations available in universal job match]

Preferred location (and radius)

City: _______________ Postcode: ______________ Radius: ______ (miles)

In what range of salaries are you looking for a job?

£ _______ [number] to £ ________ [number] ______ [drop down menu: per hour, per week, per month]

What type of contract are you looking for? (you can select more than one answer if appropriate)

- ☐ Full Time
- ☐ Contract
- ☐ Part Time
- ☐ Placement Student
- ☐ Temp
- ☐ Other

How many vacancies did you apply since you have become unemployed? ____ [Number]

How many job interviews did you get so far? ____ [Number]

How many job offers did you get so far? ____ [Number]

What are your most important concerns at the moment (rate on scale from 0 (not a concern at all) to 10 (very strong concern)).

My financial situation is deteriorating	___ [number]
Personal difficulties prevent me from focusing on job search	___ [number]
Health-related problems hinder my job search activities	___ [number]

Risk preferences question

We now offer you the possibility to do a gamble with some of the compensation you will receive for today's session. You do not have to participate. If you participate, we will reduce your compensation by £2.80, but you will earn an amount of money depending on the gamble you choose and the outcome of the gamble.

We propose you 5 gambles. You can only choose one of them. Indicate your choice at the bottom of the page.

Each gamble corresponds to a flip of a coin and has two possible outcomes (Heads or Tail). We indicate below what you would win in each case. We will flip a coin at the end of the session, when you leave the room. Note that you do not have to play and you can simply choose to keep £2.80.

Gamble 1

TAIL: £2.40 **HEADS: £3.60**

Gamble 2

TAIL: £2.00 **HEADS: £4.40**

Gamble 3

TAIL: £1.60 **HEADS: £5.20**

Gamble 4

TAIL: £1.20 **HEADS: £6.00**

Gamble 5

TAIL: £0.20 **HEADS: £7.00**

Your choice [drop down menu]

- ☐ **I keep £2.80**
- ☐ **I play Gamble 1**
- ☐ **I play Gamble 2**
- ☐ **I play Gamble 3**
- ☐ **I play Gamble 4**
- ☐ **I play Gamble 5**

Time preferences questions

At the end of the session, one participant in the room will be selected at random and will receive lottery tickets (in addition to the compensation promised). Each ticket gives the chance to win up to £250,000. Note that the lottery tickets will be sent at the date indicated to the person's home address, so you will not need to collect them here.

Could you please indicate for each of the 15 choices below which option you would prefer. If you are selected, we will select one of the 15 choices at random and send you the relevant number of tickets at the date chosen.

Choice 1:	☐ 5 lottery tickets today	☐ 6 lottery tickets in a week
Choice 2:	☐ 5 lottery tickets today	☐ 7 lottery tickets in a week
Choice 3:	☐ 5 lottery tickets today	☐ 8 lottery tickets in a week
Choice 4:	☐ 5 lottery tickets today	☐ 9 lottery tickets in a week
Choice 5:	☐ 5 lottery tickets today	☐ 10 lottery tickets in a week
Choice 6:	☐ 5 lottery tickets today	☐ 6 lottery tickets in 4 weeks
Choice 7:	☐ 5 lottery tickets today	☐ 7 lottery tickets in 4 weeks
Choice 8:	☐ 5 lottery tickets today	☐ 8 lottery tickets in 4 weeks
Choice 9:	☐ 5 lottery tickets today	☐ 9 lottery tickets in 4 weeks
Choice 10:	☐ 5 lottery tickets today	☐ 10 lottery tickets in 4 weeks
Choice 11:	☐ 5 lottery tickets in 8 weeks	☐ 6 lottery tickets in 12 weeks
Choice 12:	☐ 5 lottery tickets in 8 weeks	☐ 7 lottery tickets in 12 weeks
Choice 13:	☐ 5 lottery tickets in 8 weeks	☐ 8 lottery tickets in 12 weeks
Choice 14:	☐ 5 lottery tickets in 8 weeks	☐ 9 lottery tickets in 12 weeks
Choice 15:	☐ 5 lottery tickets in 8 weeks	☐ 10 lottery tickets in 12 weeks

8.5.5 Weekly survey questionnaire

Weekly job survey

We will now ask a few questions about your other search activities over the past week.

How many hours did you spend searching for jobs? *

For the following questions please exclude any searching done during the previous session here at the university or applications made as a result.

Did you search for jobs using any of the following (you can select more than one answer if appropriate)

- [ ] DirectGov / Universal Jobmatch
- [ ] Other internet websites
- [ ] Newspapers
- [ ] Through friends / family / acquaintances
- [ ] Through the jobcentre
- [ ] Through a private employment agency
- [ ] Approached employers directly (handing in CVs etc.)

Please specify any other ways you looked for a job

How many other vacancies did you apply to? *

Please tell us the title, employer and salary information for any jobs you applied for (if known)

How many interviews did you go to? *

How many job offers did you get? *

Did you accept a job offer? *

○ Yes ○ No

If you have worked in a temporary or part-time job in the past week please tell us about it (title, employer, hours, part/full-time, salary information)

If you took part in any training since last weeks session please tell us what this was

Did you meet a case worker at the jobcenter? *

Yes No

Are jobs that you encounter in your other search activities broadly similar to those that you encounter when searching here at the university? *

Very similar Similar Different Very different

Finally we will ask a few general questions.

What are your most important concerns at the moment (rate on scale from 0 (not a concern at all) to 10 (very strong concern))

My financial situation is deteriorating *

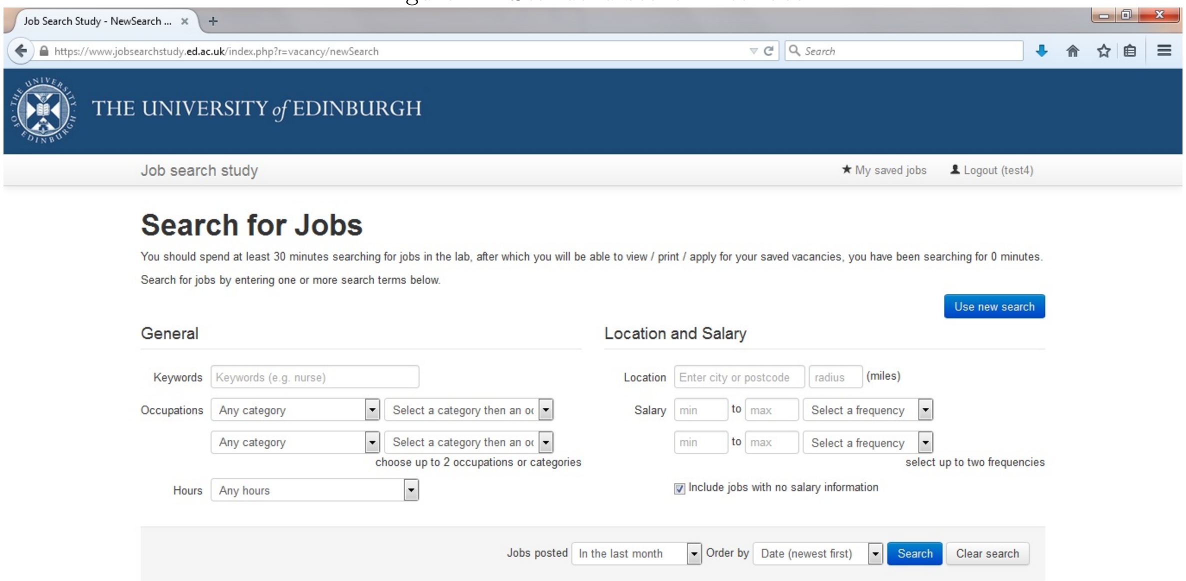
Personal difficulties prevent me from focusing on job search *

There is strong competition for jobs *

Health-related problems hinder my job search activities*

Do you have any feedback for us on our search engine and computer interface?

Figure 14: Standard search interface



8.5.6 Screenshots

Figure 15: Screenshot of the tool (for preferred occupation 'cleaner')

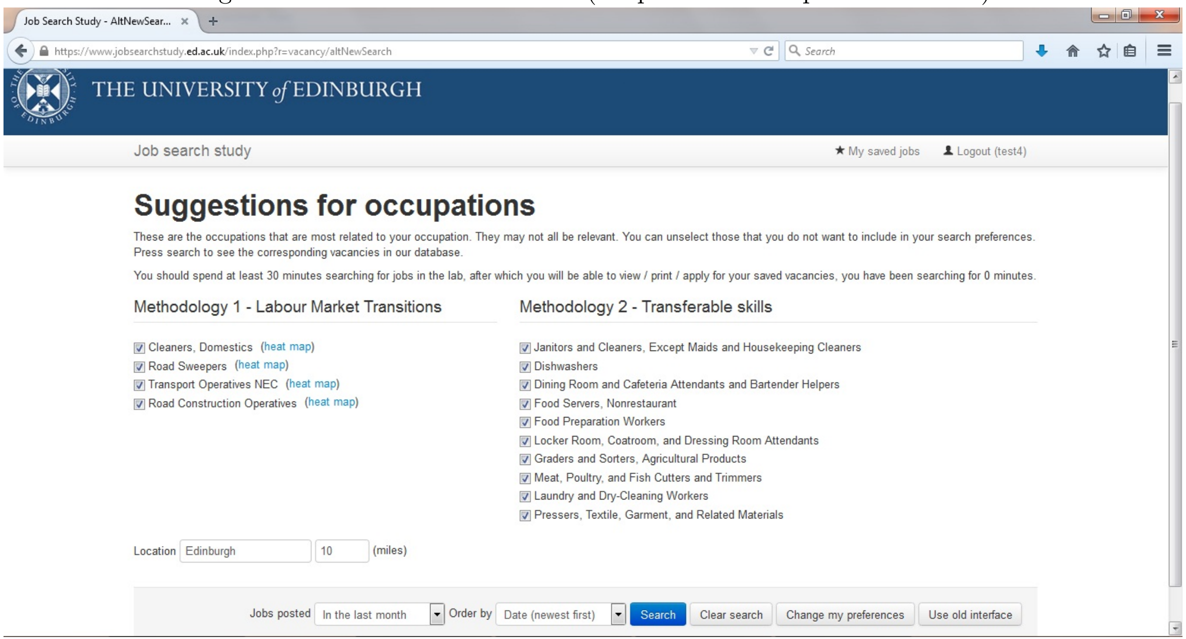
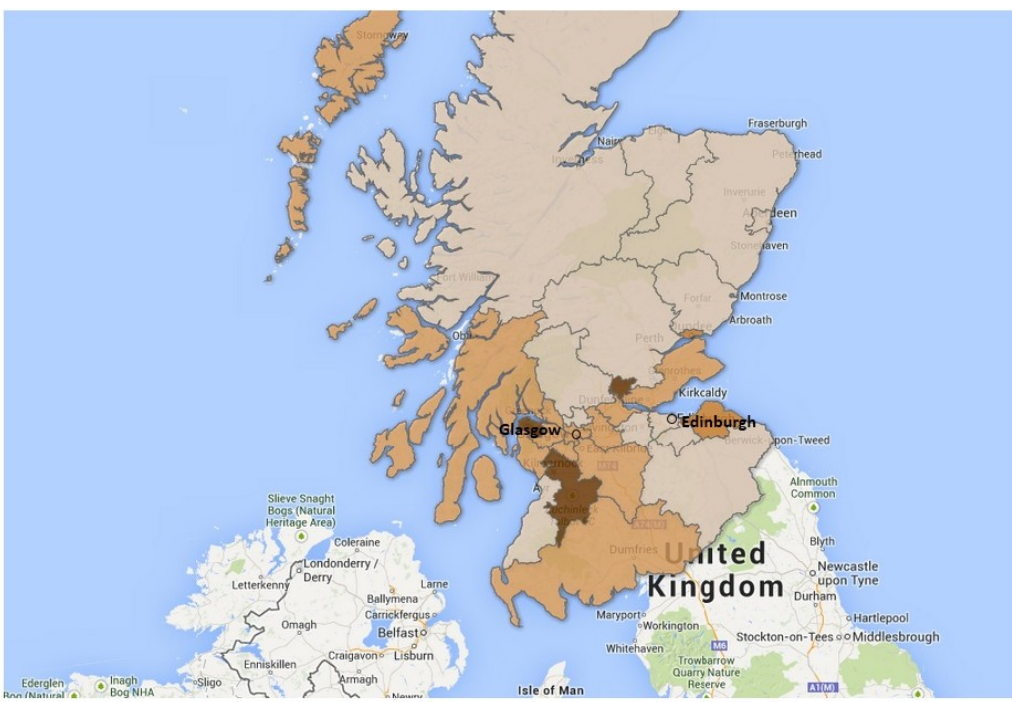


Figure 16: Example of a heatmap



The darker the color, the higher the number of job seekers per vacancy in the particular occupation.