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ABSTRACT

Providing Advice to Job Seekers at Low Cost: An Experimental Study on Online Advice*

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. This significantly broadens the set of jobs they consider and significantly increases their job interviews. Effects are strongest for participants who otherwise search narrowly and have been unemployed for a few months.

JEL Classification: D83, J62, C93

Keywords: online job search, occupational broadness, search design

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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 little is said about how they should obtain such jobs and how one might advise them in the process. Also the large literature on active labor market policies is predominantly silent about the effective provision of job search advice. Most studies confound advice with monitoring and sanctions, that is, they confound information with incentives. 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.

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. Of course, 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. We designed a web-based search tool that makes suggestions of relevant occupations that job seekers could consider, using an algorithm based on representative labor market statistics. In a nutshell, the algorithm identifies occupations that should be relevant to job seekers given their profile (and preferred occupation in particular) by using information on labor market transitions of other job seekers with comparable profile and by considering skill transferability.

Our second contribution is to evaluate how the tool affects job search behavior, i.e., to see if job seekers adjust their job search strategies in response to the suggestions they receive. Search strategies should react to advice if seekers lack relevant information, while little reaction is expected if incentives are the main problem. 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. 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. These are key advantages

[1] See Venn (2012) for an overview of requirements across OECD countries.
[2] See the clarification in Card et al. (2009), p. 6.

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 applications, interviews and job offers, and 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. However, because of the logistics required for this 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 survey data. 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 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) document the wide use of the internet. 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. One replicates "standard" platforms where job seekers themselves decide which keywords and occupations to search for, similar to interfaces used on Universal Jobmatch (the official job search platform provided by the UK Department of Work and Pensions) and other commercial job search sites. The second "alternative" platform provides targeted occupational advice. It asks participants which occupation they are looking for - which can coincide 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 with similar occupations make and the second contains occupations for which skill requirements are similar. 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 where market tightness is more favorable, indicating that jobs might be easier to find. Both web interfaces access the database of live vacancies of Universal Jobmatch, which features a vacancy count at over 80% of the official UK vacancies. The benefits of such intervention are 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.

A priori, the effectiveness of the alternative interface might not be obvious. Broader search could delude search effort. Moreover, the additional information on the alternative interface is taken from readily available sources and, therefore, might already be known to the participants or to their advisers at the job centre. On the other hand, job search occurs precisely because people lack relevant information that is costly and time-consuming to acquire. It has long been argued that information about occupational fit is a key piece of information that individuals need to acquire, and therefore our intervention focusses on this dimension.[3] The main benefit of the internet is precisely the ability to disseminate information at low cost, and our implementation makes wider occupational exploration easy.

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 these initial three weeks, half of the participants continue with this interface throughout the study, while the other half was offered to try the alternative interface. We report the overall impact on the treatment group relative to the control group. We also compare treatment and control in particular subgroups of obvious interest: our study has more scope to affect people who search narrowly prior to our intervention, and differential effects by duration of unemployment seem to be a major policy concern as mentioned in the introductory paragraph. Overall, we find that our intervention exposes job seekers to jobs from a broader set of occupations, increasing our measure of broadness by 0.2 standard deviations which corresponds to the broadening that would occur naturally after an additional three months of unemployment. The number of job interviews increases by 44%. This is driven predominantly by job seekers who initially search narrowly. They apply closer to home and experience a 75% increase in job interviews (compared to similarly narrow searchers in the control group). Among these, 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 and are associated with a significant increase in the number of job applications to vacancies on our site. We take these as a promising indication that targeted low-cost advice can increase individuals' breath of search and their job interviews.

Since we collect information both on search success when searching in our computer facilities as well as through other search channels, we can study possible spill-overs beyond our targeted channel. In contrast to studies that monitor and sanction search activities (Van den Berg and Van der Klaauw (2006)), our advice-based intervention does not induce crowding out between these channels. To the contrary, interviews obtained through other search channels increase significantly. This indicates that the information that we provide on our interface also positively affects search through other channels.

In a later section we lay out a simple learning theory that exposes why narrow individuals with slightly longer unemployment duration might be particularly helped by our intervention. In essence, after loosing 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 broadness declines, but effects on job

[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.

interviews are theoretically ambiguous because search effort is better targeted. In the data it is indeed the case that initially broad individuals in the treatment group become occupationally narrower than comparable peers in the control group, but effects on interviews are insignificant for this group.

Our findings suggest concrete policy recommendations: targeted web-based advice might be helpful to job seekers. This is particularly interesting because interventions such as the one we evaluate have essentially zero marginal costs, and could be rolled out on large scale without much burden on the unemployment assistance system.[4]

Clearly these results need to be viewed with caution. 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. Further research would also be required to investigate whether improved search by some workers affects the job prospects of others. Such analysis is desirable, but requires a larger sample size and longer follow-up.

The subsequent section reviews related literature. Section 3 outlines the institutional environment. Section 4 describes the new tool we propose. The experimental design is discussed in Section 5 and section 6 presents our empirical analysis and findings. Section 7 uses a simple model to illustrate the forces that might underlie our findings, and the final section concludes.

2 Related Literature

As mentioned in the introductory paragraph, most studies on job search assistance evaluate a combination of advice and monitoring/sanctions. An example in the context of the UK, where our study is based, is the work by Blundell et al. (2004) that evaluates the Gateway phase of the New Deal for the Young Unemployed, which instituted bi-weekly meetings between long-term unemployed youth and a personal advisor to "encourage/enforce job search". The authors establish significant impact of the program through a number of non-experimental techniques, but cannot distinguish whether "assistance or the "stick" of the tougher monitoring of job search 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.[5] Nevertheless, their study indicates that there might be room for effects of additional information provision as advice within the official UK system is limited since "many claimants' first contact with the job centre focuses mainly on claiming benefits, and not on finding work" (Gallagher et al. (2015)).

Despite the fact that a lack of information is arguably one of the key frictions in labor markets and

[4] 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.

[5] 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.

an important reason for job search, we are only aware of a few studies that exclusively focus on the effectiveness of information interventions in the labor market.[6] Prior to our study the focus has been on the provision of counseling services by 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 counselling services are effective and outperform private sector conselling services. The latter appear even less promising when general equilibrium effects are taken into account (Crepon 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 is actually delivered and 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. (2015) 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 no significant effect overall, but for those at risk of long-term unemployment they find a positive effect between 8 months and a year after sending the brochure. 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, is easily replicable, treated a large sample, and has administrative data to assess success. 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 rather than tailored to the occupations they are looking for.

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 that job search is stratified by educational attainment but that job seekers lower their aspirations over time. Using the same data source, Faberman and Kudlyak (2014) investigate whether the declining hazard rate of finding a job is driven by declining search effort. They find little evidence for this. The data lacks some basic information such as employment/unemployment status and reason for leaving the site, but they document some patterns related to our study: Occupational job search is highly concentrated and absent any exogenous intervention it broadens only slowly over time, with 60% of applications going to the modal occupation in week 2 and still 55% going to the modal occupation after six months.

[6] There are some indirect attempts to distinguish between advice and monitoring/sanction. Ashenfelter et al. (2005) apply indirect inference to ascertain the effectiveness of job search advice. They start by citing experimental studies from the US by Meyer (1995) which have been successful but entailed monitoring/sanctions as well as advice. Ashenfelter et al. (2005) then provide evidence from other interventions that monitoring/sanctions are ineffective in isolation. Indirect inference then attributes the effectiveness of the first set of interventions 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.

Marinescu and Rathelot (2014) investigate the role of differences in market tightness as a driver of aggregate unemployment. They discipline the geographic broadness of search by using U.S search 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 the majority of applications aimed at jobs within 25km distance and 82% of applications staying in the same city (Core-Based Statistical Area), even if some 10% go to distances beyond 100km.[7] Using the same data source, Marinescu (2014) investigates equilibrium effects of unemployment insurance by exploiting state-level variation of unemployment benefits. The level of benefits affects the number of applications, but effects on the number of vacancies and overall unemployment are limited. Marinescu and Wolthoff (2014) document that job titles are an important explanatory variable for attracting applications in Careerbuilder.com, that they are informative above and beyond wage and occupational information, and that controlling for job titles is important to understand the remaining role of wages in the job matching process. 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.

To our knowledge, our study is the first that undertakes job-search platform design and evaluates it. The randomized setup allows for clear inference. 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. (2014), who study a change at the online consumer platform Ebay which changed the presentation of its search results to order it more by price relative to other characteristics. This lead to a decline in prices, which is assessed in a consumer search framework. While similar in broad spirit of search design, the study obviously differs substantially in focus.

3 Institutional Setting

We describe briefly the institutional settings relevant for job seekers in the UK during the study. Once unemployed, a job seeker can apply for benefits (Job Seekers Allowance, JSA), by visiting their local job centre. If they have contributed sufficiently through previous employment, they are eligible for contribution-based JSA, which is £56.25 per week for those aged up to age 24, and £72 per week for those aged 25 and older. These benefits last for a maximum of 6 months. Afterwards - or in the absence of sufficient contributions - income-based JSA applies, with identical weekly benefits but with extra requirements. The amount is reduced if they have other sources of income, if they have savings or if their partner has income. Once receiving JSA, the recipient is not eligible for income assistance, however they may receive other benefits such as housing benefits.

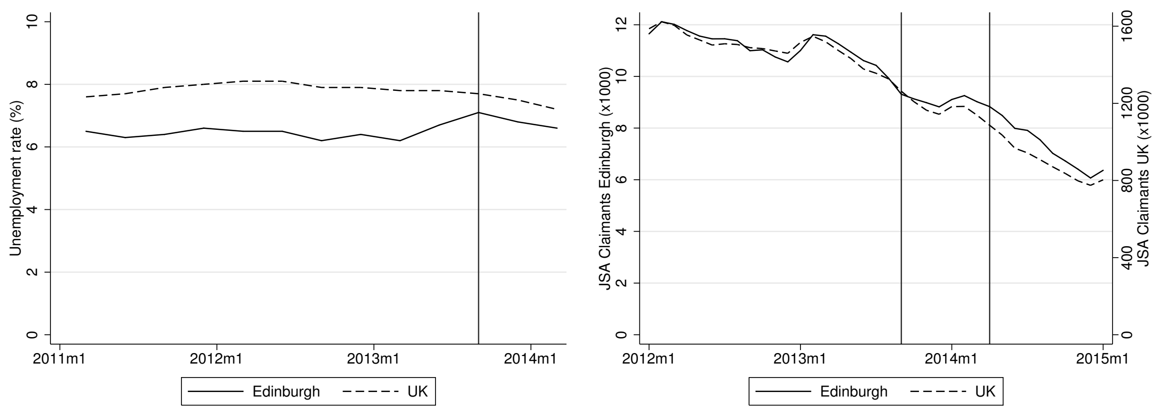
JSA recipients should be available and actively looking for a job. In practice, this implies committing to agreements made with a work coach at the job centre, such as meeting the coach regularly, applying to suggested vacancies, participating in suggested training. They are not entitled to reject

[7] These numbers are based on Figure 5 in the 2013 working paper. Neither paper provides numbers on the breath of occupational search. The "distaste" for geographical distance backed out in this work for the US is lower than that backed out by Manning and Petrongolo (2011) from more conventional labor market data in the UK, suggesting that labor markets in the UK are even more local.

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.

In Figure 1 we present aggregate labor market statistics. Figure (a) shows the unemployment rate in the UK and Edinburgh since 2011. The vertical line indicates the start of our study. The unemployment rate in Edinburgh is slightly lower than the UK average, and is rather stable between 2011 and 2014. These statistics are based on the Labour Force Survey and not the entire population. Therefore we present the number of JSA claimants in the Edinburgh and the UK in panel (b), which is an administrative figure and should be strongly correlated with unemployment. We find that the number of JSA claimants is decreasing monotonically between 2012 and 2015, and that the Edinburgh and UK figures follow a very similar path. The number of JSA claimants in Edinburgh during our study is approximately 9,000, such that the 150 participants per wave in our study are about 2% of the stock. The monthly flow of new JSA claimants in Edinburgh during the study is around 1,800 (not shown in the graph).

Figure 1: Aggregate labor market statistics



(a) Unemployment rate

(b) JSA claimants

4 Description of New Tool

The first contribution of this paper is a new tool aimed at providing relevant information to job seekers at a low cost. We propose to embed this tool in an on-line job search interface, so that job seekers can directly use the information provided in their search.

We designed an on-line job search interface in collaboration with professional programmers from the 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 (and can change at any time). We use two methodologies to compile a list of 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. It is based on the standard idea in the experimentation literature that others have already borne the cost of experimentation and found suitable outcomes, and this knowledge would be useful to reduce the experimentation costs of a given job seeker.

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).[8] 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. For each occupation we created a list of the 3 to 5 most common transitions; at least 3 if available and at most 5 if more than 5 were available. These consist of occupations that are in both datasets in the top-10 common transitions. If there are less than 3 of these, we added the most common transitions from each of the datasets.

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.

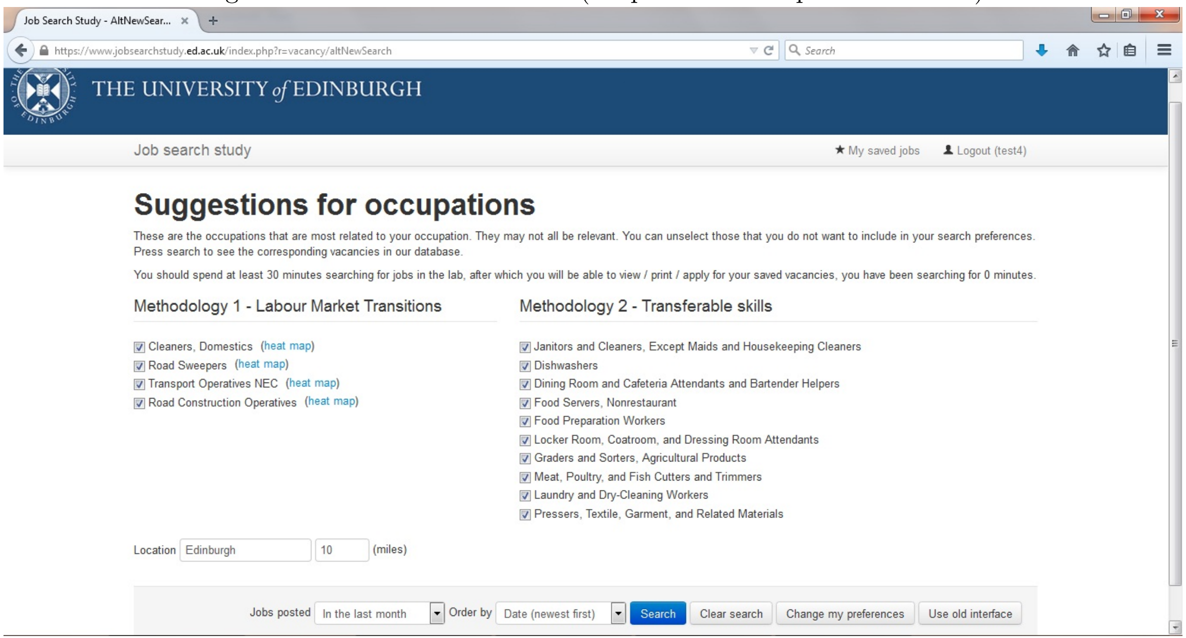
The tool is directly embedded in the job search interface. That means that once participants have specified their preferred occupation, they could then click "Save and Start Searching" and were taken to a new screen where a list of suggested occupations was displayed. The occupations were listed in two columns: The left column suggests occupations based on the first methodology (based on labor market transitions). The right column suggests occupations based on the second methodology (O*net related occupations). Figure 2 shows a screenshot of the tool, with suggestions based on the preferred occupation 'cleaner'. Participants were fully 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. If they then click the "search" button, the program searches through the same underlying vacancy data as in the control group but selects all vacancies that fit any of the selected occupations in their desired geographic area.[9]

In addition to these suggestions, the interface also provides visual information on the tightness of the labor market for broad occupational categories in regions in Scotland. The goal here is to provide information about how competitive the labor market is for a given set of occupations. We constructed

[8]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.

[9]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 vacancy data. We therefore asked participants twice for their preferred occupation, once in O*net form and BHPS form. The query on the underlying database relies on keyword search, taking the selected occupations as keywords, to circumvent problems of differential coding.

Figure 2: Screenshot of the tool (for preferred occupation 'cleaner')



"heat maps" that use recent 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).[10] 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. So they could check them for each broad category before actually performing a search, not for each particular vacancy.

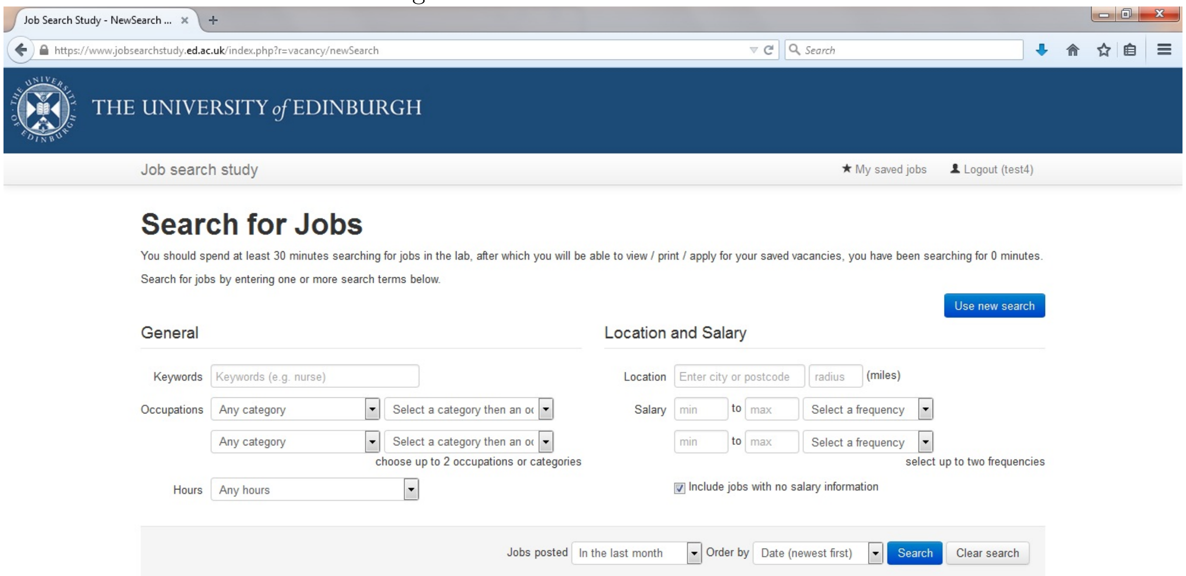
In principle this tool can be used with any database of vacancies that includes occupational codes; for our experimental approach we combine it with one of the largest such datasets in the UK.

5 Experimental Evaluation of the Tool

To evaluate experimentally the new tool, we conducted a randomized controlled experiment and invited job seekers in the area of Edinburgh to our computer facilities for a period of 12 weeks during two waves, one in the fall of 2013 and one in the spring of 2014. We used a "standard" interface for comparison, which relies on a keyword search as in most existing job search platforms. All participants started with the standard search platform. Half of the sample was exposed to the new tool after 3 weeks. We now describe the experimental design in more detail.

[10]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/). We created the heat maps at the two-digit level because data was only available on this level. Clearly, this implies that the same map is offered for many different 4-digit occupations, and job seekers might see the same map several times. Obviously a commercial job search site could give much richer information on the number of vacancies posted in a geographic area and the number of people looking for particular occupations in particular areas. An example of a heat map is presented in the Online Appendix 9.2.6.

Figure 3: Standard search interface



5.1 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), again in collaboration with the computer applications team at the University of Edinburgh. 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.

Figure 3 shows a screenshot of the main page of the standard search interface. 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 "do 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. We recorded all search activity taking place outside the lab. This is however 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

Figure 4: Number of vacancies

(a) Posted vacancies in our study

(b) Active vacancies in our study and in UK

search criteria. This is shared by commercial sites like Universal Jobmatch or Monster.com at the time of our study, which also provide no further guidance to job seekers on things such as related occupations.

5.2 Vacancies

In order to provide a realistic job search environment, both the new tool and the standard search interface 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. This is a crucial aspect in the setup of the study, 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. Panel (a) of Figure 4 shows the number of posted vacancies available through our search engine in Edinburgh and in the UK for each week of the study (the vertical line indicates the start of wave 2). Each week there are between 800 and 1600 new vacancies posted in the Edinburgh. Furthermore, there is a strong correlation between vacancy posting in Edinburgh and the UK. In panel (b) the total number of active vacancies in the UK is shown over the second half of 2013 and 2014.[11] As a comparison the total number of active vacancies in the database used in the study in both waves is shown. It suggests that the database contains over 80% of all UK vacancies, which is a very extensive coverage compared to other online platforms.[12] It is well-known that not all vacancies on online job search platforms are genuine, so the actual number might be somewhat lower.[13] We

[11]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

[12]For comparison, the largest US jobsearch platform has 35% of the official vacancies; see Marinescu (2014), Marinescu and Wolthoff (2014) and Marinescu and Rathelot (2014). The size difference might be due to the fact that the UK platform is run by the UK government.

[13] For Universal Jobmatch evidence has been reported on fake vacancies covering 2% of the stock posted by a single account (Channel 4 (2014)) and speculations of higher total numbers of fake jobs circulate (Computer Business Review

introduced ourselves a small number of additional posts (below 2% of the database) for a separate research question (addressed in a separate paper). 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".[14] This small number is unlikely to affect job search, and there is no indication of differential effects by treatment group.[15]

5.3 Recruitment Procedure and Experimental Sample

We recruited job seekers in the area of Edinburgh. 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. We imposed no further restrictions in terms of nationality, gender, age or ethnicity.

We obtained the collaboration of several local public unemployment agencies (Job Centres) to recruit job seekers on their premises. Their counsellors were informed of our study and were asked to advertise the study. We also placed posters and advertisements at various public places in Edinburgh (including libraries and cafes) and posted a classified ad on a popular on-line platform (not limited to job advertisements) called Gumtree. Table 1 presents the sign up and show up rates. Of all participants, 86% were recruited in the Jobcentres. Most of the other participants were recruited through our ad on Gumtree. We approached all visitors at the Jobcentres during two weeks, which was chosen to reach a large share of those on Job Seeker Allowance who tend to meet their advisers in bi-weekly intervals. Out of those 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. 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, which means most of them signed up at the Jobcentres.[16] The sign up rate at the Jobcentres was slightly higher for the online survey (58%), however out of those that signed up, only 21% completed the first

(2014)). Fishing for CV's and potential scams are common on many sites, including Carreerbuilder.com (The New York Times (2009a)) and Craigslist, whose chief executive, Jim Buckmaster, is reported to say that "it is virtually impossible to keep every scam from traversing an Internet site that 50 million people are using each month" (The New York Times (2009b)).

[14] Participants were asked for consent to this small percentage of research vacancies. They were informed about the true nature of such vacancies if they expressed interest in the vacancy before any actual application costs were incurred, so any impact was minimized.

[15] 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 in any case to online job search sites (see footnote 13) and that this is mentioned to job seekers in many search guidelines (see e.g. Joyce (2015)).

[16] 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.

survey. This was partly caused by the fact that about one-fourth of the email addresses that were provided was not active.[17]

In section 6.3.1 we discuss in more detail the representativeness of the sample, by comparing the online and the lab participants with population statistics.

5.4 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. The study consisted of two waves: wave 1 started in September 2013 and wave 2 started in January 2014. 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 (described later in this section) for a minimum of 30 minutes.[18] After this period they could continue to search or use the computers for other purposes such as writing emails, updating their CV, or applying for jobs. They could stay in our facility for up to two hours. We emphasized that no additional job search support or coaching would be offered.

All participants received a compensation of £11 per session attended (corresponding to the government authorized compensation for meal and travel expenses) and we provided an additional £50 clothing voucher for job market attire for participating in 4 sessions in a row.[19]

Participants were asked to register in a dedicated office at the beginning of each session. At the first session, they received a unique username and password and were told to sit at one of the computer desks in the computer laboratory. The computer laboratory was the experimental laboratory located at the School of Economics at the University of Edinburgh with panels separating desks to minimize interactions between job seekers. They received a document describing the study as well as a consent form that we collected before the start of the initial session (the form can be found in the Online Appendix 9.2.1). We handed out instructions on how to use the interface, which we also read aloud (the instructions can be found in the Online Appendix 9.2.2). We had assistance in the laboratory to

[17] 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 in Table 1.

[18] 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. Given that participants spent around 12 hours a week on job search, a minimum of half an hour per week is unlikely to be a binding constraint on weekly job search, while it was a sufficient duration for us to collect data. Furthermore, similar to our lab participants, the participants in the online survey (who did not come to the lab and had no restrictions on how much to search) also indicate that they search 12 hours per week on average. Among this group, only in 5% of the cases the reported weekly search time is smaller than 30 minutes. In the study, the median time spent in the laboratory was 46 minutes. We made sure that participants understood that this is not an expectation of their weekly search time, and that they should feel free to search more and on different channels.

[19] All forms of compensation effectively consisted of subsidies, i.e. they had no effect on the allowances the job seekers were entitled to. The nature and level of the compensation were discussed with the local job centres to be in accordance with the UK regulations for job seeker allowances.

Table 1: Recruitment and show-up of participants

	Full sample	Wave 1	Wave2
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] Based on only one day of recruitment (see footnote 17).

answer questions. We clarified that we were unable to provide any specific help for their job search, and explicitly asked them to search as they normally would.

Once they logged in, they were automatically directed to our own website.[20] 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 9.2.4 and 9.2.5.

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 and were offered the option of printing these saved vacancies. This list of printed vacancies could be used as evidence of required job search activity at the Jobcentre. It was, however, up to the job seekers to decide whether they wanted to provide that evidence or not. We also received no additional information about the search activities or search outcomes from the Jobcentres. We only received information from the job seekers themselves. 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.[21] Those who stayed could either keep searching with our job search engine or use the computer for other purposes (such as updating their CV, applying on-line or using other job search engines). We did not keep track of these other activities. Once participants left the facility, they could still access our

[20] www.jobsearchstudy.ed.ac.uk

[21] Participants were of course allowed to leave at any point in time but they were only eligible to receive the weekly compensation if they had spent 30 minutes searching for jobs using our search engine.

Table 2: Randomization scheme

	Wave 1	Wave 2
Monday 10 am	Control	Treatment
Monday 1 pm	Treatment	Control
Monday 3:30 pm	Control	Treatment
Tuesday 10 am	Treatment	Control
Tuesday 1 pm	Control	Treatment
Tuesday 3:30 pm	Treatment	Control

website from home, for example in order to apply for the jobs they had found.

5.5 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 new tool (as shown in Figure 2), 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 each time slot into treatment and control over the two waves, to avoid any correlation between treatment status and a particular time slot. Table 2 illustrates the randomization. Note that the change 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 9.2.3), including how the recommendations were constructed, in the fourth week of the study before starting their search. For them, the new interface became the default option when logging on. It should be noted, though, that it was made clear to participants that using the new interface was not mandatory. Rather, they could switch back to the previous interface by clicking a button on the screen indicating “use old interface”. If they switched back to the old interface, they could carry on searching as in the previous weeks. They could switch back and forth between interfaces. This ensures that we are not restricting choice, but are rather offering advice.

5.6 Acceptance of Search Interface

In order for the study to provide a valid environment to study search behavior, it is important that participants themselves take it seriously and do not view our service inferior to search environments in the overall marketplace. In an exit survey we asked participants to evaluate the interface and found that participants evaluated it 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 for both interfaces.

6 Empirical Analysis

We now turn to the empirical analysis. We first discuss the outcome variables of interest and the econometric specification. We then provide background information on our sample (and its represen-

Table 3: Outcome variables

	Search activity in the lab	Search activity outside the lab
Listed vacancies		
Occupational Broadness	√	
Geographical Broadness	√	
Number	√	
Applications		
Occupational Broadness	√	
Geographical Broadness	√	
Number	√	√
Interviews		
Number	√	√
Core and non-core occupations	√	

tativeness) and the results of the analysis.

6.1 Outcome variables

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. Of course, ultimately one would also like to evaluate the effects on job finding and the characteristics of 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. We will nevertheless briefly discuss the evidence we have in the last subsection.

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. Therefore we also document the weekly applications and interviews through other channels as outcome variables.

We summarize in Table 3 the outcome variables of interest. All measures are defined on the set of vacancies retrieved in a given week, independent of whether they arose due to many independent search queries or few comprehensive queries. The main outcome variables relate to (1) listed vacancies, (2) applications and (3) interviews.[22]

The most immediate measure of search relates to listed vacancies, i.e., the listing of vacancies that appears on the participants' screen as a return to their search query. By default the list is ordered by date of vacancy posting (most recent first), but participants can choose to sort them according to other criteria such as job title, location and salary. Note that we limit ourselves to the list of vacancies the participants actually saw on their screen. A page on the screen is limited to at most 25 listed vacancies, and participants have to actively move from one screen to the next to see additional vacancies. Thus,

[22]We also constructed measures based on the viewed and saved vacancies. The results were qualitatively similar to the those obtained for the listed and applied vacancies. They are available upon request.

we exclude the vacancies on pages that were not consulted by the participant. As mentioned earlier, all analysis are at the weekly level and, thus, we group all listings in a week together.[23]

The second measure of search behavior relates to applications. Here we have information about applications based on search activity conducted inside the laboratory as well as outside the laboratory which we collected through the weekly surveys. For the applications based on search in the laboratory, we asked participants to indicate for each vacancy saved in the previous week whether they actually applied to it or not.[24] 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 the 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 have to drop observations based 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 broadness (occupational and geographical). For occupational broadness we focus on the UK Standard Occupational Classification code (SOC code) of a particular vacancy, which consists of four digits.[25] 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 only share the first 3 digits, 2 if they only share the first 2 digits, 3 if they share only the first digit and 4 if they share no digits. This distance, averaged over all possible pairs within a set, is the measure that we use in the empirical analysis.[26] Note that it is increasing in broadness (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 broadness we use a simple measure. Since a large share of searches restricts the location to Edinburgh, we use the weekly share of a participants searches that goes beyond Edinburgh as the measure of geographical broadness.[27]

Our third outcome measure is interviews - which is the measure most closely related to job prospects.

[23]The alternative interface tends to necessitate less search queries than the standard interface to generate the same number of vacancies because on the alternative interface one query is intended to also return vacancies for other related occupations. For that reason the weekly analysis seems more interesting compared to results at the level of an individual query, for which results arise rather mechanically. This also means that in a given week each vacancy vacancy is counted at most once, even if it is returned as a result to multiple queries.

[24]If they have not applied, they are asked whether they intend to apply and if they answered affirmatively they were asked again next week whether they did apply or not. A similar procedure is followed for interviews.

[25]The first digit of the code defines the "major group" , the second digit defines the "sub-major group", the third digit defines the "minor group" and the fourth digit defines the "unit group" which provides a very specific definition of the occupation. Some examples are "Social science researchers" (2322), "Housekeepers and related occupations" (6231) and "Call centre agents/operators" (7211).

[26]Our results are robust to using the Gini-Simpson index as an alternative broadness measure. In tables 23 and 24 in the Online Appendix we show that the several different measures of broadness are highly correlated. For example, for listed vacancies our measure has a correlation above 0.95 with 4 different Gini-Simpson measures.

[27]Note that the direct surroundings of Edinburgh contain only smaller towns. The nearest large city is Glasgow, which takes about 1-1.5 hours of commuting time.

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 exclude weeks 11 and 12 of the experiment, because for job search done in these weeks we do not observe interviews. We have information on the number interviews, but the number is too small on average to compute informative broadness 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 can estimate separate treatment effects for interviews in core and non-core occupations. For the number of applications and interviews we also look at activity outside the lab.

6.2 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. 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. We have estimated a fixed effects model and performed a Hausman test for each of the main specifications. 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. 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).

Specifically, we compare a variable measuring an outcome (Y) in the control and treatment group before and after the week of intervention, controlling for period fixed effects (α_p, 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:

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

where i relates to the individual, p to the period and $\eta_i + \epsilon_{ip}$ is an error term consisting of an individual specific component (η_i) and a white noise error term (ϵ_{ip}). Individual characteristics X_i include gender, age and age squared, unemployment duration and unemployment duration squared[28] 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 6.3.3 we document attrition across treatment and control groups and find no evidence of asymmetric attrition in terms of observable characteristics. Second, our panel structure

[28]Unemployment duration is defined as the reported duration at the start of the study.

allows us to control for time-invariant heterogeneity and use within-individual variation. When we estimate a random and fixed effects model, the Hausman test fails to reject the latter. Since the treatment itself is assigned at the group-level it is unlikely to be correlated with unobserved individual characteristics. However, differential attrition could create correlation between the treatment and unobservable individual characteristics and would therefore lead to rejection of the random-effects model. The fact that we can never reject this model is thus an indication against differential attrition between treatment and control groups.

Another important aspect relevant for the econometric specification is the potential heterogeneity of effects across individuals. Given the nature of the intervention, 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 broadness (splitting the sample at the median level of broadness over the first three weeks).

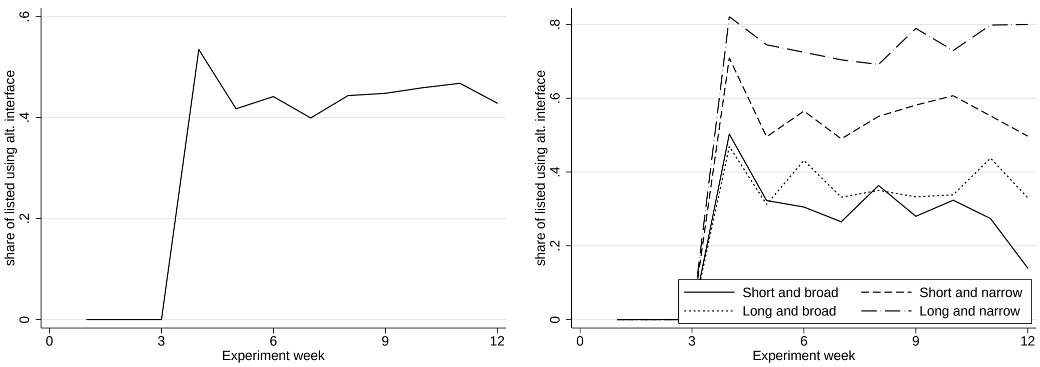
Second, the willingness to pursue new options depends on the incentives for job search, which change with unemployment duration for a variety of reasons. Longer-term unemployed might be those for whom the search for their preferred jobs turned out to be unsuccessful and who need to pursue new avenues, while they are also exposed to institutional incentives to broaden their search (the Jobcentres require job seekers to become broader after three months). Note again that we are always comparing otherwise identical individuals in the treatment and control groups, so the incentives to broaden their search by themselves would not be different, but the information we provide to achieve this differs. We therefore also interact the treatment effect with unemployment duration. In the subsequent section we provide a simple theoretical model formalizing the channels that may explain differential effects.

Note that since we did not force job seekers to use the alternative interface, our intervention is an intention-to-treat. Panel (a) of Figure 5 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. This does not mean that only 50% of the treatment group is treated, though, because 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 revert back to searching with the standard interface.[29]

For the sake of brevity, 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. Before turning to the estimation results, we now provide background information on our experimental sample.

[29]The variation in usage results from both between and within users. In the treatment group, around 65% of the week-participant observations contain listed vacancies from both the standard and the alternative interface. See Figures 10 and 11 in the Online Appendix for the distribution of these shares.

Figure 5: 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 broadness)

6.3 Descriptive statistics on our sample

6.3.1 Representativeness of the sample

Since the participants were not randomly selected from the population of job seekers in Edinburgh, one may worry that the sample consists of a selective group that differs from the general population.[30] To provide some indication of the degree of selection, we compare characteristics of the participants to the online survey participants for whom barriers to participation are lower, and to aggregate statistics of job seekers available from The Office of National Statistics (NOMIS) where we truncate unemployment duration to obtain a sample with similar median. These descriptive statistics are presented in Table 4. The first four columns show the mean, standard deviation, minimum and maximum for the lab participants, while the next four columns show the same statistics for the online survey participants. In column 9, the p-value of a two-sided t-test for equal means is shown. Finally, in column 10 aggregate statistics of job seekers in Edinburgh are shown, for the variables for which these are available.[31]

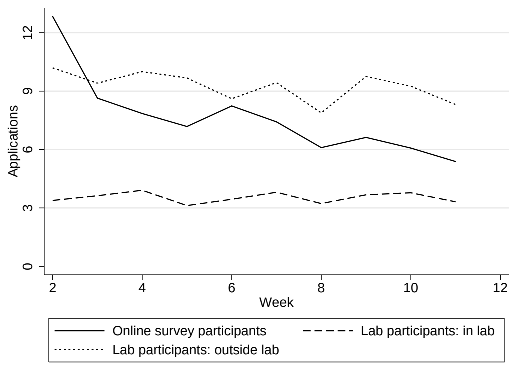
Demographic variables, based on the first week baseline survey, show that 43% of the lab participants are female, the average age is 36 and 43% have some university degree. 80% classify themselves as 'white' and 27% have children. The online survey participants differ somewhat in composition: they are more likely to be female, they are slightly younger and they have less children. When comparing these statistics to aggregate statistics of Edinburgh job seekers, we find that we oversample women and non-whites, while the average age is very similar.

The lower part of Table 4 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, which lead to 0.48 interviews

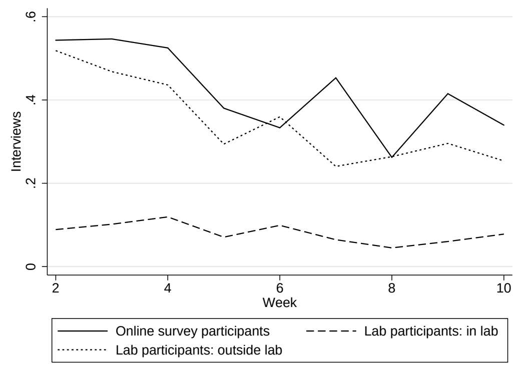
[30] We do drop the observations on one participant from our sample because this participant had been unemployed for over 30 years and was therefore an extraordinary outlier in our sample. We only include participants who search at least once, which excludes two participants who showed up once without searching and never returned. Including them the analysis has no effects on the qualitative findings.

[31] 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.

Figure 6: Jobsearch behavior online and lab participants



(a) Applications



(b) Interviews

and 0.42 job offers.[32] 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 most dimensions, except that they attended more job interviews.

We can also compare job search behavior of participants in our study with the online survey participants. The online survey includes a question asking for the weekly number of applications sent and the weekly number of job interviews. We compare the control group lab participants to the online survey participants to assess whether participation in the study affects the participants. When using data on applications and interviews in our study, we assign both of these to the week in which search activity was performed that lead to either to these. On average this implies that applications are assigned to search activity one week before the application was send, while interviews are assigned to search activity two weeks before the interview is reported. The average number of applications are shown in panel (a) of Figure 6 and the average number of interviews in panel (b) of Figure 6. For lab participants we observe both the number of applications from job search in the lab, and the number of applications reported through other job search activities. The number of applications outside the lab is quite similar to the number reported by the online participants, while the sum of the two types of applications for lab participants is somewhat higher than for the online participants. In panel (b) we find that the sum of interviews in- and outside the lab is very similar to the number reported by the online participants. The average number of weekly interviews is 0.47 for lab participants and 0.42 for online participants and these numbers are not statistically different (p-value 0.23).

6.3.2 Treatment and Control Groups

In order to evaluate the effect of the alternative interface on job search behavior and outcomes we compare treated and non-treated individuals. Both of these groups used the same interface in the

[32]We censor the response to the survey question on the number of previous job offers at 10.

Table 4: 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	sd	min	max	mean	sd	min	max	pval	
Demographics:										
gender (%)	43	50	0	1	52	50	0	1	.09	33
age	36	12	18	64	34	12	18	64	.08	35
high educ (%)	43	50	0	1	43	50	0	1	1.00	
white (%)	80	40	0	1	77	42	0	1	.43	89
number of children	.53	1	0	5	.28	.57	0	2	.02	
couple (%)	23	42	0	1	23	42	0	1	.96	
any children (%)	27	45	0	1	23	42	0	1	.41	
Job search history:										
vacancies applied for	64	140	0	1000	75	187	0	1354	.53	
interviews attended	.48	0.84	0	6	2.7	4	0	20	.00	
jobs offered	.42	1.1	0	8	.51	1.6	0	10	.52	
at least one offer (%)	20	40	0	1	24	34	0	1	.36	
days unempl. (mean)	260	620	1	5141	167	302	8	2929	.15	111
days unempl. (median)	80				118					81
less than 183 days (%)	76	43	0	1	75	44	0	1	.76	
less than 366 days (%)	85	35	0	1	91	28	0	1	.13	
job seekers allowance (£)	52	75	0	1005	58	42	0	280	.49	
housing benefits (£)	64	129	0	660	48	95	0	400	.36	
other benefits (£)	14	65	0	700	12	56	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.

first three weeks, and the alternative interface was only provided to the treatment group from week 4 onwards. This means that we can use the information from the first three weeks to correct for fixed differences between treated and control group individuals. In principal this should not be necessary though, since the treatment was assigned randomly. Still, the group fixed effects will increase precision. In Table 5 we compare characteristics of the treatment and control group to ensure that the composition of the groups is balanced.[33]

We compare the treated and control group on the same set of demographic and job search history variables as in Table 4, and additionally we compare job search behavior in our study over weeks 1-3. For demographic and job search history variables, only one out of 32 t-tests suggests a significant difference, which is the average number of children. In terms of job search behavior in our study over the first three weeks, we find that the control group lists on average 498 vacancies, of which 25 are viewed, and 10 are saved. Out of these, participants report to have applied to 3 and eventually get an

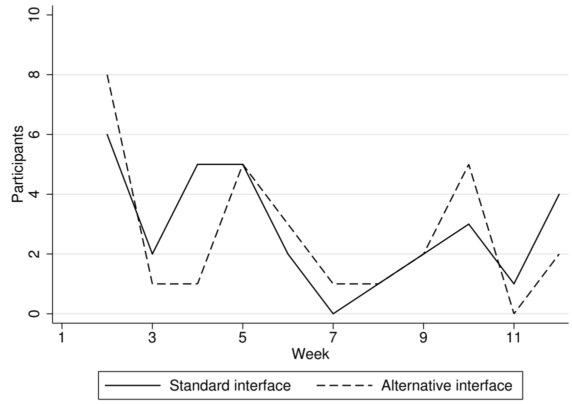
[33] For example, it could be the case that by expressing a strong preference for a particular time slot, participants self-select into groups. Since we switch around the treatment assignment of groups in the second wave (see Table 2), this is unlikely to be problematic though.

Table 5: Characteristics of the treatment and control group

	Control group				Treatment group				T-test
	mean	sd	min	max	mean	sd	min	max	pval
Demographics:									
female (%)	44	50	0	1	41	49	0	1	.67
age	36	11	18	62	37	12	18	64	.53
high educ [a] (%)	43	50	0	1	42	50	0	1	.85
survey qualification level	4.2	1.9	1	8	4.5	1.9	2	8	.27
white (%)	80	40	0	1	81	40	0	1	.88
number of children	.64	1.1	0	5	.40	.83	0	5	.04
couple (%)	25	44	0	1	21	41	0	1	.37
any children (%)	30	46	0	1	25	43	0	1	.37
Job search history:									
expect job within 12 weeks (%)	59	49	0	1	57	50	0	1	.70
vacancies applied for	74	154	0	1000	53	122	0	1000	.21
interviews attended	.41	0.63	0	3	.56	1	0	6	.13
jobs offered	.37	.96	0	5	.49	1.2	0	8	.35
at least one offer (%)	19	40	0	1	21	41	0	1	.77
days unemployed (mean)	286	668	1	5028	231	563	1	5141	.45
days unemployed (median)	80				78				
less than 183 days	.75	.44	0	1	.78	.42	0	1	.54
less than 366 days	.85	.36	0	1	.86	.34	0	1	.64
job seekers allowance (£)	48	41	0	225	56	101	0	1005	.43
housing benefits (£)	64	123	0	600	63	136	0	660	.96
other benefits (£)	9.6	39	0	280	18	84	0	700	.39
Weekly search activities in weeks 1-3:									
listed	498	396	4.3	3049	488	377	1	1966	.82
viewed	25	14	3	86	26	18	0	119	.55
saved	10	10	0	65	11	12	0	79	.56
applied	3.3	5.8	0	45	2.5	4.3	0	33	.18
interview	.094	.34	0	3.3	.087	.24	0	1.5	.84
applications other	9.2	11	0	68	7.5	8.3	0	37	.15
interviews other	.53	.70	0	4	.49	.79	0	5	.69
broadness listed[b]	3.2	.61	0	3.7	3.2	.57	1	3.7	.73
broadness applied[b]	3	.95	0	4	3.2	.90	0	4	.36
hours spend[c]	11	8.2	.50	43	12	10	1	43	.12
concern health (scale 1-10)	1.5	2.5	0	10	1.7	2.7	0	10	.46
concern financial (scale 1-10)	7.3	2.6	0	10	6.9	3.1	0	10	.30
concern competition (scale 1-10)	7.4	2.3	0	10	7.2	2.2	.50	10	.52
met caseworker (%)	31	37	0	1	29	39	0	1	.61
Observations	155				140				

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 6.1. [c] The number of hours spend on job search per week, as filled out in the weekly survey, averaged over week 2 and 3.

Figure 7: Attrition of participants in the standard and alternative interface groups (excluding job finding)



interview in 0.09 cases. Furthermore, they report about 8 weekly applications through channels outside our study, leading to 0.03 interviews on average. For the sets of listed vacancies and applications we compute a measure of occupational broadness (as described in subsection 6.1), of which the average values are also shown in the table. 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 extend 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. The values for job search behavior of the treatment group are very similar, and never differ significantly.

6.3.3 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 of Figure 7. An exit from the study is defined to occur in the week after the last session in which the individual attended a lab session. Here we present the number of participants leaving the study per week due to reasons other than finding employment. In most weeks, we lose between 2 and 4 participants, and these numbers are very similar in control and treatment groups.

As discussed in subsection 6.2, the fact that the random effects model cannot be rejected provides reassurance about the degree of differential attrition. Here we document attrition in more detail. In particular, we investigate whether the composition of the control treatment group changes over time due to attrition, by looking at observable characteristics of those that remain in the study. We compute mean values of the same set of variables as in Table 5, for individuals remaining in the study in week 1, 6 and 12. For each of these groups of survivors, we test whether the treatment and control group

Table 6: Effect of intervention on listed vacancies

	Broadness 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	-0.15***	0.03	-58.89
	(0.05)	(0.03)	(78.30)
X occupationally narrow	0.42***	-0.05*	-7.45
	(0.08)	(0.03)	(50.79)
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 group fixed effects, period fixed effects, individual random effects and individual characteristics. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 25 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.

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 statistically 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 section (6.4.4). We now turn to the analysis of the effects of the intervention on the different outcome variables of interest.

6.4 Results

6.4.1 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 6.1), the other in terms of geography (fraction of vacancies outside Edinburgh metropolitan area). We also measure the number of vacancies that were listed.

We estimate a linear model with individual random effects (equation (1)). The results are presented in Table 6. The first row presents a significant positive overall effect on broadness of search in terms of occupation. The broadness 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 broadness of listings over time for those who remain in our study and are not treated (see Figures 12 and 13 in the Online Appendix), which implies that the treatment effect is equivalent to the broadening that on average happens over 13 weeks. We find no evidence of an overall effect on geographical broadness 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 seekers searched in the first three weeks.[34] 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. We also find evidence of a substitution effect in terms of geographical broadness. Those who expand their search in terms of occupation appear to also become more narrow in the geographical area they look at, possibly because they now find more jobs within close proximity and have a lower need to search further away. The opposite is true for those who narrow their search in terms of occupation.[35,36]

The different effects 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 7.

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). The effect is estimated for four groups: interactions of occupational broadness and unemployment duration. We find that results do not change much, though 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.

6.4.2 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 which we

[34] There are, of course, several other dimensions along which we could estimate heterogeneous effects. Since we have only a limited sample size, we decided to estimate only heterogeneous effects by initial broadness. For this factor we have a clear hypothesis for the effect of the intervention, while estimation of heterogeneous effect along many other dimension might be considered data mining. Note however that initial broadness is correlated with other factors and may therefore pick up the difference in effect along other dimensions. In particular, being an initially broad searcher is correlated with age (correlation coefficient is -0.36), gender (0.07), being in a couple (-0.06), having children (-0.19) and being higher educated (-0.11).

[35] In the Online Appendix we also report estimates where we split the sample according to broadness 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 broadness). This could still be driven by initial occupational broadness, since this is negatively correlated with initial geographical broadness (coefficient -0.36) and is not controlled for. Indeed, when we split both by occupational and geographical broadness the effects are driven by the occupational dimension, which we will henceforth focus on.

[36] 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) broadness measure if the number of listed is two (one) or larger, which excludes different numbers of observations depending on the variable of interest.

Table 7: Effect of intervention on listed vacancies - interactions

	Broadness of listings		Number of listings
	(1)	(2)	(3)
	Occupational	Geographical	Lab
Treatment			
X long unempl. and occ. broad	-0.17***	0.06	49.43
	(0.06)	(0.04)	(128.13)
X short unempl. and occ. broad	-0.14**	0.00	-167.84**
	(0.06)	(0.05)	(77.43)
X long unempl. and occ. narrow	0.45***	-0.04	31.23
	(0.11)	(0.04)	(51.02)
X short unempl. and occ. narrow	0.39***	-0.06*	-46.04
	(0.08)	(0.03)	(69.94)
Model	Linear	Linear	Linear
Observation weeks	1-12	1-12	1-12
N	540	541	541

Each column represents one regression. All regressions include group fixed effects, period fixed effects, individual random effects and individual characteristics. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

collected through the weekly surveys. Since the distribution of applications contains a large share of zeros, we estimate a negative binomial model, with individual random effects.[37] 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 broadness (approximately one-fifth of a standard deviation). When we split the sample according to initial occupational broadness, we find the same pattern as for listings. Those who searched more narrowly in terms of occupation become occupationally broader, while those that searched broadly become more narrow. Due to reduced precision both effects are not significantly different from zero (but neither are they significantly different from the large effects observed for listings in Table 6 and point estimates are of similar order of magnitude). We find no effects on the number of applications for either group (columns (3) - (5)), however the large positive but insignificant coefficient for the occupationally narrow group (0.28) suggests that this group may have increased their applications. There is also a negative effect on geographical broadness for the occupationally narrow job seekers (column (2)).[38]

[37] 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)). Therefore we also report results from Poisson regressions in the Online Appendix (Table 17), using either random effects or fixed effects. While findings are broadly consistent when using a Poisson model, the most notable exception are applications where magnitude and precision are noticibly lower.

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

Table 8: Effect of intervention on applications

	Broadness 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	-0.26	-0.02	-0.08	-0.05	-0.04
	(0.21)	(0.04)	(0.18)	(0.11)	(0.11)
X occupationally narrow	0.28	-0.09***	0.28	-0.02	0.06
	(0.26)	(0.03)	(0.23)	(0.11)	(0.11)
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 group fixed effects, period fixed effects, 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)). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Again, we split these effects by the duration of unemployment and report results in Table 9. In column (1), we find that occupational broadness goes down for long term unemployed broad searchers, while it goes up for short term unemployed narrow searchers (though both are not significant as a result of larger standard errors). The number of applications through search activity in the lab increases with 58% for the long term unemployed narrow searchers, significant at the 10% level.[39] We find no effect for the other groups.

We find a similar pattern when analyzing the saved vacancies, which is the intermediate step between listing a vacancy and applying. The number of saved vacancies increases for long term unemployed narrow searchers, while the coefficient for broadness for this group is positive but insignificant. For the sake of brevity we do not report the estimates here, but they can be found in the Online Appendix, Table 15.

6.4.3 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 calculate broadness measures. So we only look at a measure of the number of interviews obtained as a result of search conducted inside the laboratory

[39] This effect is insignificant in the Poisson specification in the Online Appendix.

Table 9: Effect of intervention on applications - interactions

	Broadness of applications		Number of applications		
	(1) Occupational	(2) Geographical	(3) Lab	(4) Outside lab	(5) Total
Treatment					
X long unempl. and occ. broad	-0.46*	-0.04	-0.17	-0.18	-0.14
	(0.25)	(0.04)	(0.23)	(0.13)	(0.13)
X short unempl. and occ. broad	-0.04	-0.01	-0.09	0.10	0.07
	(0.27)	(0.07)	(0.23)	(0.17)	(0.16)
X long unempl. and occ. narrow	0.19	-0.11***	0.58*	-0.02	0.09
	(0.27)	(0.03)	(0.38)	(0.15)	(0.16)
X short unempl. and occ. narrow	0.37	-0.07	0.08	-0.03	0.04
	(0.34)	(0.04)	(0.24)	(0.14)	(0.14)
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 group fixed effects, period fixed effects, 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)). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and outside the laboratory.[40] 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, though only the increase in out-of-lab interviews is statistically significant. When interpreting the magnitude of these estimates, it should be noted that the average number of weekly interviews is low. 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. This also explains why the effect on interviews in the lab, while being larger in magnitude, is often not statistically significant.

When splitting the sample according to broadness 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.[41]

[40] 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.

[41] We find little evidence of heterogeneity in treatment effects when we split the sample according to initial geographical

Table 10: Effect of intervention on interviews

	Number of interviews		
	(1)	(2)	(3)
	Lab	Survey	Total
Treatment	0.61	0.40*	0.44*
	(0.79)	(0.27)	(0.28)
Treatment			
X occupationally broad	-0.42	0.17	0.12
	(0.39)	(0.31)	(0.28)
X occupationally narrow	1.23	0.64**	0.75**
	(1.16)	(0.34)	(0.39)
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 group fixed effects, period fixed effects, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 done inside the lab and outside the lab.[42] These findings highlight that our intervention is particularly beneficial to people who otherwise search narrowly and who have been unemployed for some months. Overall, it does not seem detrimental to those that became more narrow in their search.

The set of weekly interviews is too small to compute broadness 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 broadness of listed vacancies, and separate treatment effects on interviews in core vs non-core occupations allow some assessment of whether this lead to more "broadness" in job interviews. Results are presented in Table 12. We indeed find that the increase in the number of

broadness. We find a significant treatment effect for those who searched broadly geographically, but all the coefficients are positive across the board and not significantly different across sub-groups. Results are presented in the Online Appendix in Table 21.

[42] The extremely large value of the increase in lab interviews for the long term narrow searchers is due a low level of interviews of 0.08 per week in the pre-treatment period, which grew to 0.20 after the treatment.

Table 11: Effect of intervention on interviews - interactions

	Number of interviews		
	(1)	(2)	(3)
	Lab	Survey	Total
Treatment			
X long unempl. and occ. broad	-0.14	-0.16	-0.11
	(0.83)	(0.26)	(0.28)
X short unempl. and occ. broad	-0.59	0.48	0.31
	(0.30)	(0.47)	(0.39)
X long unempl. and occ. narrow	5.81***	1.06***	1.56***
	(3.22)	(0.57)	(0.68)
X short unempl. and occ. narrow	-0.08	0.41	0.34
	(0.54)	(0.34)	(0.35)
Model	Poisson	Poisson	Poisson
Observation weeks	1-10	1-10	1-10
N	540	466	464

Each column represents one regression. All regressions include group fixed effects, period fixed effects, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

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

Each column represents three separate regressions. All regressions include group fixed effects, period fixed effects, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 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.[43]

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 5 shows the evolution of the fraction of treated participants using the interface, splitting the sample by occupational broadness 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. The fractions remain quite stable over the 8 weeks. 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. 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.[44]

6.4.4 Effects on Job finding

We now briefly turn 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 of the same order of magnitude as the confirmed job finding rate.[45]

We classify job seekers in three categories depending on the information recorded in week 3 (before the intervention) and week 12 (last week of the intervention): Job seekers are either (1) present in the study and having no job ("no job"), (2) not present in the study and unclear outcome ("out of study"), (3) not present in the study and having found a job ("job").

Table 13 presents the distribution of job seekers across categories, as well as the average length

[43]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.

[44]We only asked this question once, in the first week. Asking it on a weekly basis might have affected people's behavior by structurally emphasizing that they had not yet succeeded in finding employment.

[45]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 perfectly whether the person found a job or just quit the study.

Table 13: Summary statistics on job finding and drop out for weeks 3 and 12

	In Study - No Job	Found a Job	Out of Study	Job finding week^{+}	
				mean	(std)
Week 3					
Standard interface	130	12	9	2.2	(0.6)
Alternative interface	128	10	6	2.1	(0.7)
Week 12^{++}					
Standard interface	72	36	19	7.6	(2.2)
Alternative interface	79	27	18	8.1	(2.6)

$^{+}$ Job finding week conditional on finding a job by the respective week. $^{++}$ Outcome by week 12 for individuals that were still present in week 4.

(in weeks) job finders had to wait to find a job. Note that we record the week they accepted a job offer, not the week the job actually started. For week 12, we report the distribution for those who were still in the study in week 4 and have therefore been exposed to the new interface if they were in the treatment group. There is indication that the job finding rate is slightly higher in the standard interface than in the alternative interface already in week 3, however this appears more pronounced in week 12, but since we have around 15% of our sample who dropped out and we do not know if they found a job or not, 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.[46] 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 14. We fail to find significant

[46] 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 5). 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 5. 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 14: Treatment effects on job finding rate

	(1)	(2)
Treatment	-0.14	-0.18
	(0.25)	(0.31)
Treatment x Occupationally narrow		0.09
		(0.56)
N	253	253

Proportional Cox Hazard model, with session group dummies, and controls for gender, age, age squared, ethnicity (white), cohabiting, university degree, number of children and financial concerns. We exclude observations censored at 3 weeks or less. Reported values are coefficients. * $p < 0.10$.

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. We return to advocating larger studies in the conclusion.

7 An Illustrative Model

In the empirical section we saw that our information intervention increases occupational broadness: listings are broader and more job interviews are obtained, possibly driven by jobs outside the core occupations. Job interviews are increased particularly for long-term but narrow searchers, and there is an indication that they they apply more. Searchers who already search broadly without our intervention decrease their broadness. While it is obvious why narrow individuals are affected differently from broad ones, it might be less obvious why it is the longer-term unemployed that seem to react stronger to our information intervention. Here we briefly sketch a very stylized occupational job search model that is capable of rationalizing these findings and of organizing our thoughts about the driving forces. The goal is not to provide the richest framework, but to provide a simple setup in which the previous findings can be captured with intuitive arguments in a coherent framework. Among other simplifications, we only model "broadness" in a crude way (neither distinguishing listings vs applications as these are qualitatively similar, nor incorporating geography).

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 $f(e_i)$.[47] 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 increasing and convex function $c(\sum e_i)$.[48] 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. If her job prospects are good she obtains a job in occupation i with arrival probability $a_H f(e_i)$, otherwise she obtains a job with probability $a_L f(e_i)$, where $a_H > a_L = 0$, where the equality is assumed only for simplicity. The uncertainty can be about whether the skills of the job seeker (still) meet the requirements of the occupation. The individual does not know whether she is a high or low type, but assigns probability p_i to being a high type. This probability is all that is relevant for the decision of the individual in this binary environment. But when we introduce 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 this probability, 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 this average prior and her effort, her expected chances of getting a job offer in occupation i are

$$h(p_i, e_i) = f(e_i)(p_i a_H + (1 - p_i)a_L).$$

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

$$H(p, e) = 1 - \prod_i (1 - h(p_i, e_i))$$

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, if she finds a job this is permanent and pays wage w per period, and if she remains unemployed she obtains benefits b for that period. Obviously searching in an occupation changes the beliefs about it. An individual who has a prior p_i^t at the beginning of period t and spends effort e_i^t during the period but

[47] The decreasing returns capture that the number of job opportunities within an occupation may be limited. We are focusing on the individual worker's search here, and do not additionally model the aggregate matching function that might depend on the total number of vacancies and the number of other job seekers who explore the same occupation. All of this is suppressed as the individual takes it as given. For simplicity we also abstract from heterogeneity in occupations which might make the return to search occupation-specific. As mentioned, we also abstract from geography, though effects on broadness consistent with our empirical findings could easily be obtained by assuming that more search effort in a given occupation means applying to jobs that are further away geographically and that the benefit of a job equals the wage minus geographical distance.

[48] In models with only one occupation it is immaterial whether c is convex or f concave or both. With multiple occupations, we chose a setup where the costs are based on total effort, which links the various occupations, while the return to search is occupation specific. In this setting, if returns were linear all search would be concentrated in only one market. If costs were linear, then changes in one market would not affect how much individuals search in other markets. So both play a separate role here.

does not get a job will update her beliefs about the chance of being a high type in occupation i by Bayes rule. Let $B(p_i^t, e_i^t)$ denote this new belief. For interior beliefs we have[49]

$$p_i^{t+1} = B(p_i^t, e_i^t) = \begin{cases} = p_i^t & if \quad e_i^t = 0 \\ < p_i^t & if \quad e_i^t > 0, \end{cases} \tag{2}$$

since there is no learning without effort, and the individual becomes more pessimistic if she does put effort but does not get a job. Let $B(p, e) = (B(p_1, e_1), ..., B(p_I, e_I))$ denote the vector of updates.

The state variable for an individual is the time period t because of her finite life-time, and her belief vector at the beginning of this period p $(= p^t)$. Given this, she chooses her search effort vector e $(= e^t)$ to maximize her return. She obtains for sure her outside option of doing nothing in the current period: her current unemployment benefit payment and the discounted value of future search. Additionally, if she finds a job, she gets the lifetime value of wages (W_t) to the extent that they exceed her outside option. Finally, she has to pay the search effort costs. So the return to search is given by

$$R_t(p) = \max_e \left(b + \delta R_{t+1}(B(p,e)) + H(p,e)\Big(W_t - (b + \delta R_{t+1}(B(p,e)))\Big) - c(\sum_i e_i) \right) \tag{3}$$

The model implies that an individual may search in multiple occupations due to decreasing returns in each one. The distribution of her effort across occupations depends on the set of priors p_i, $i \in 1, .., I$. For our purposes a two-period model suffices (for which $R_3 = 0$, $W_2 = w$ and $W_1 = w(1 + \delta)$).[50] The first period captures the newly unemployed, and the second period the longer-term unemployed.

The unanticipated introduction of the alternative interface provides an additional source of information on occupations. It displays a list of occupations suitable for someone like her. In general, this implies that for these occupations the individual may update her beliefs positively, while for those not on the list she may update her beliefs downwards. To formalize this mechanism, 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.[51] The alternative interface induces an update in belief p_i^t when it is introduced, but given this update problem (3) continues to characterize optimal behavior.

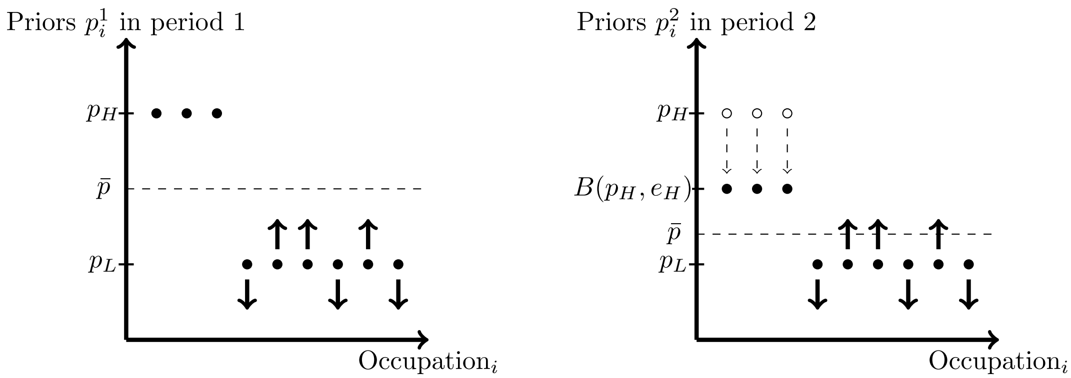
In order to gain some insights in how this affects the occupational broadness of search, consider for illustration two types of occupations. Occupations $i \in 1, ..., I_1$ are the "core" ones where the job seeker is more confident and holds first period prior $Q_i = Q_H$ leading to average belief $p_i = p_h$, while

[49] 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.

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

[51] If the individual has already exerted search effort the updating is more complicated but obviously being on the list continues to be a positive signal. Consider a period t with prior p_i^t. The information that occupation i is on the list in the alternative interface can be viewed as changing the very first prior p_i^1, and this translates into the updated prior in period t by successively applying the updating formula (2), using the efforts that have been exerted in the interim.

Figure 8: 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.

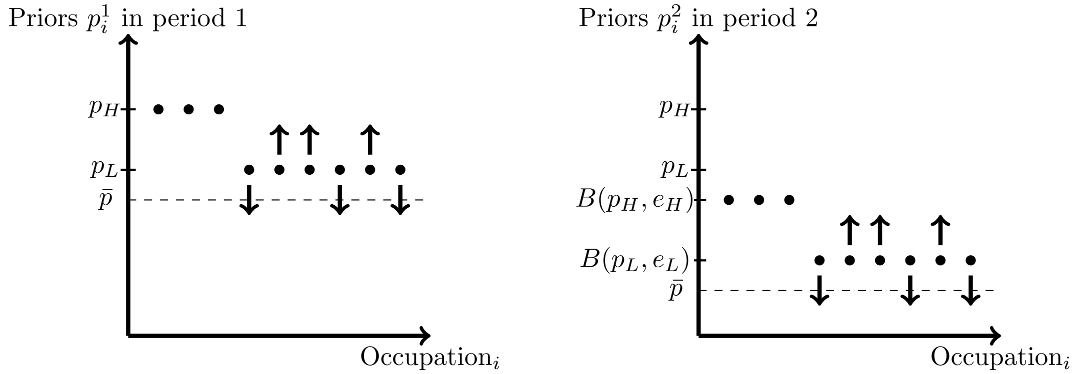
(b) Narrow search, longer-term unemployed: Update in first three occupations leads to lower belief in these (dashed arrows for p_H in first three occupations). This brings threshold $\bar{p}$ closer to the beliefs in other occupations (p_L), so that some additional information moves some occupations above the threshold and broadens search (arrows for occupations 4-10).

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 (3): 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. The level of $\bar{p}$ is increasing in the average belief about the core occupations (if core occupations are more attractive search is expanded there, which drives up the marginal cost of any further search in non-core occupations) and in the number of core occupations (again core occupations as a whole attract more search effort).

We depict our notion of an individual who is recently unemployed and narrow in Figure 8 (a). The person 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 therefore she does not want to explore the less likely occupations. In fact, the distance in employment prospects is so large that small changes in the prior p_L induced by the alternative interface - indicated by the thick arrows in the

Figure 9: Model Illustration: broad search



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

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

figure - do not move them above the threshold $\bar{p}$.[52] So there would be no difference in search behavior with or without the alternative interface.

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 the beliefs are now closer together, and since they are the only source of heterogeneity the utility of applying to either of them are also closer. (If one were to additionally model penalties for failing to search broader over time, this would reinforce the effect since it would also reduce the perceived distance in utility between these occupations.) In a model with multiple 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 Figures 12 and 13 in the Online Appendix). Panel (b) depicts a shorter time frame where beliefs did not fall that much and p_L remains below the new $\bar{p}$ so that the individual remains narrow. But since the distance is closer those with access to the alternative interface obtain information that moves some of their beliefs about non-core occupations above the threshold $\bar{p}$, which makes it attractive to search there and they become broader than their peers without such information. These increased opportunities materialize in a higher shadow-cost of remaining at the current level of search effort. Therefore, search effort weakly increases relative to the control group without alternative interface, and strictly so if the cost function is smooth. In turn it must lead to better job prospects (as the higher search effort

[52] Whether the information of the alternative interface leads to small changes in the prior or large ones depends on the informativeness of the alternative interface. We consider here the case where the informativeness is low enough (e.g., $\bar{q}_L - \underline{q}_L < \epsilon$ for sufficiently small ϵ so that the support of initial beliefs is not very dispersed, which bounds the possible change in priors due to additional information). We do not explore informativeness that yields to large changes in the prior here, as it would have the counterfactual implication that recently unemployed individuals already become broad due to the alternative interface.

needs to be compensated to make it individually optimal). So this rationalizes why longer-unemployed individuals in the treatment group become broader and their number of interviews increases, relative to the control group. 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 9 (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 because an individual has rather equal priors already early in unemployment, 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 8 (a)). In both cases, the person already searches in all occupations, but additional negative information (i.e., occupations that are not included in the list that is recommended in the alternative interface) might move the prior of those occupations so low that the person stops searching there and becomes narrow. Effects on search effort (in case it is flexible) and job prospects are ambiguous: search effort can now be concentrated more effectively on promising occupations which raises effort and job prospects; alternatively the negative information on some occupations can translate simply into reduced search effort which is privately beneficial but reduces job prospects. Depending on parameters, either can dominate.[53] This can rationalize why otherwise broad searchers become narrower in our treatment group, without significant effects on job prospects.

Thus, the model is able to replicate differential effects by broadness and unemployment duration. In this model, as in all models of classical decision theory, more information can only improve the expected utility for the individual. This is true even if some groups like those that already search broadly would cut back on search effort in a way that reduces job prospect, as they save on private costs. Obviously socially, when taking into account unemployment benefit payments, this could lead to costs if some of the broad searchers have parameters that lead them to cut back on search effort in non-core occupations in a way such that their job prospects decline. We find fairly limited evidence on reduced effort and no significant negative effects on job interviews for any subgroup, but more studies might be necessary to confirm both the empirical findings and our rationalization here.

8 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 sug-

[53] Which effect dominates depends importantly on the curvature of the total cost function $c(\sum e_i)$ and of the returns to occupational effort $f(e_i)$. Consider the extreme case of extremely convex effort costs: costs are zero up to some threshold $\sum e_i = \bar{e}$ and infinite thereafter. In this case clearly workers expend exactly $\bar{e}$ units of total effort. Better information does not alter this but targets this effort better, so job prospects increase. Consider alternatively an economy with strictly curved $f(.)$ and linear $c(.)$ and let e_L denote the search effort in a non-core occupation for a broad individual. Now replace the returns function $f(e_i)$ with a function $\tilde{f}(e_i)$ that is identical for $e_i < e_L$ but beyond that level marginal returns are zero ($\tilde{f}(e_i)$=$f(e_i^*)$ for $e_i > e_L$). Clearly the new function is more concave than the old one. Under this extreme return function, the effects of additional information are clear. For occupations with negative news the individual cuts his effort, without expanding it in occupations with positive news as the additional benefits are zero. Clearly search effort and job prospects fall as a consequence of additional information. While these extreme cases help to build intuition, less extreme cases display similar effects and we do not have a full characterization of when job prospects will increase.

gestions for occupations based on where other people find jobs and which occupations require similar skills. It provides this information in an easily accessible way by showing two lists and links to maps with market tightnesses, and provides all associated vacancies at the click of a button. 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. There is currently strong interest in interventions of this kind.[54] 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 broadness of job search. In particular, it makes initially narrow searchers consider a broader set of options, but decreases occupational broadness for initially broad searchers, even though overall the former effect dominates. Overall we find a positive effect on job interviews. This effect is driven by participants with longer-then-median unemployment duration in our study. This can be rationalized in a 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.

Our findings indicate that targeted job search assistance can 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 all their millions of job seekers, it is obvious that the cost per participant is at the order of a few pence.[55] So any meaningful positive employment effects would swamp the costs. Yet it should be obvious that additional larger-scale roll-out of such assistance would be required to document the full employment effects. The sample size in this study is restrictive, so is the absence of access to administrative data to follow individuals longer-term. The study also does not allow the assessment of equilibrium effects that arise if everyone obtained information.

Nevertheless, the paper documents the positive effects that can be obtained by targeted interventions on information. 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.

[54]We thank the Behavioral Insights team of the UK cabinet office, the Department of Work and Pensions, and the researchers at the Welsh government for their interest in our work.

[55]The study also devoted substantial resources (£80,000/$120,000) to attracting participants, compensating participants, and for research assistants to carry out these activities (see also Footnote 4). A commercial website would not need to incur such costs as they already have job seekers who search on their site. These numbers do not include the salaries of the authors. Even if the latter were included, the cost per participant at a large website would still only be some pence.

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9 Appendix - For Online Publication

9.1 Extended results

In Tables 16, 17 and 18 we present results using individual fixed effects rather than random effects. The coefficients are very similar to those obtained from the random effects model, though due to reduced precision, statistical significance is reduced somewhat. Table 17 also reports estimates using a Poisson regression rather than a negative binomial model.

In Table 19 we present the effect of the intervention on listed vacancies, separated by initial geographical broadness. An individual is defined to be geographically broad if his share of searches that is outside the Edinburgh area is above the median in the first 3 weeks of the study. The overall effect is presented in the first row (the same as in Table 6), which shows that the intervention increased occupational broadness. When splitting the effect by initial geographical broadness (rows (2) and (3)), we find that the positive effect is only prevalent among those that are geographically broad. However, when we estimate four effects for the combinations of initial occupational and geographical broadness (rows (4)-(7)), we find that occupational broadness is the main determinant of the effect. Irrespective of geographical broadness, those that were occupationally narrow become broader, while those that were occupationally broad become narrower. In column (2) we find a similar pattern for the effect on geographical broadness.

In Table 20 we present the effect of the intervention on applications, splitting the effect by initial geographical broadness. Column (1) and (2) show that this provides no new insights: the effect is not different for geographically narrow and broad participants. The same holds for the effect on the number of applications (columns (3)-(5)), which does not appear to depend on initial geographical broadness.

In Table 21 we present the effect of the intervention on interviews, again splitting the effect by initial geographical broadness. Rows (2) and (3) show that the positive effect on interviews is most pronounced among those that were initially geographically broad, though the coefficient is positive for both groups. In rows (4)-(7) we find that with the exception of those that were occupationally broad and geographically narrow, all groups have positive effects, though due to larger standard errors not all are significant.

For the three main specifications, we report all coefficients in Table 22. Tables 23 and 24 present correlations between different measures of occupational broadness. Table 5 shows descriptive statistics for the treatment and control group, conditional on survival up to week 1, 6 or 12 (with t-tests for equality).

Table 15: Effect of intervention on saved vacancies

	Occupational broadness of saved (1)	Number of saved vacancies (2)	Number of saved vacancies (3)
Treatment	-0.10	1.03	1.02
	(0.09)	(0.08)	(0.09)
Treatment			
X occupationally broad	-0.25***	-0.04	-0.05
	(0.09)	(0.08)	(0.10)
X occupationally narrow	0.09	0.13	0.11
	(0.13)	(0.11)	(0.12)
Treatment			
X long unempl. and occ. broad	-0.26**	0.01	0.01
	(0.12)	(0.11)	(0.12)
X short unempl. and occ. broad	-0.23**	-0.09	-0.10
	(0.11)	(0.10)	(0.13)
X long unempl. and occ. narrow	0.07	0.21	0.22*
	(0.19)	(0.16)	(0.14)
X short unempl. and occ. narrow	0.09	0.07	0.03
	(0.15)	(0.13)	(0.14)
Model	Linear	Negative Binomial	Poisson
Observation weeks	1-12	1-12	1-12
N	516	541	541

Each column represents three separate regressions. All regressions include period fixed effects and individual random effects. Columns (2) reports results from a Negative Binomial regression model and column (3) reports results from a Poisson regression, for both 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 16: Effect of intervention on listed vacancies - fixed effects model

	Broadness of listings		Number of listings
	(1)	(2)	(3)
	Occupational	Geographical	Lab
Treatment	0.13**	-0.02	-20.95
	(0.06)	(0.04)	(50.60)
Treatment			
X occupationally broad	-0.16***	0.02	-55.16
	(0.05)	(0.05)	(73.05)
X occupationally narrow	0.42***	-0.06	12.15
	(0.08)	(0.04)	(49.92)
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 period fixed effects and individual fixed effects. Standard errors clustered by individual in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 10: Distribution of the share of listed vacancies that results from using the alternative interface per participant-week observation (contains only the treatment group participants in weeks 4-12)

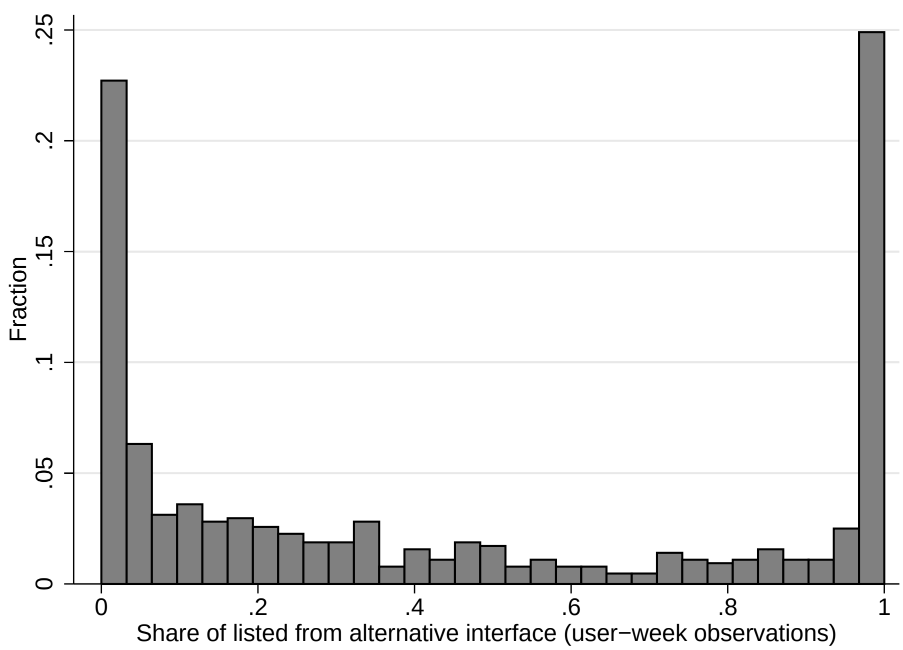


Table 17: Effect of intervention on applications - alternative specifications

	Broadness of applications		Number of applications			Number of applications		
	(1) Occupational	(2) Geographical	(3) Lab	(4) Outside lab	(5) Total	(6) Lab	(7) Outside lab	(8) Total
Treatment	0.06	-0.06	-0.01	-0.06	-0.01	-0.01	-0.07	-0.02
	(0.21)	(0.05)	(0.17)	(0.11)	(0.11)	(0.17)	(0.11)	(0.11)
Treatment								
X occupationally broad	-0.02	-0.03	-0.08	-0.04	-0.03	-0.09	-0.07	-0.05
	(0.22)	(0.07)	(0.22)	(0.17)	(0.17)	(0.21)	(0.16)	(0.17)
X occupationally narrow	0.14	-0.08*	0.05	-0.07	0.00	0.06	-0.06	-0.01
	(0.30)	(0.05)	(0.24)	(0.12)	(0.13)	(0.24)	(0.12)	(0.13)
Model	Linear FE	Linear FE	Poisson FE	Poisson FE	Poisson FE	Poisson RE	Poisson RE	Poisson RE
Observation weeks	1-11	1-11	1-11	1-11	1-11	1-11	1-11	1-11
N	305	363	410	428	424	541	490	487

Each column represents two separate regressions. All regressions include group fixed effects and period fixed effects. Columns (3)-(8) 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 18: Effect of intervention on interviews - fixed effects model

	Number of interviews		
	(1) Lab	(2) Survey	(3) Total
Treatment	0.57	0.25	0.29
	(0.73)	(0.25)	(0.26)
Treatment			
X occupationally broad	-0.49	0.22	0.10
	(0.33)	(0.38)	(0.31)
X occupationally narrow	1.34*	0.26	0.41
	(1.12)	(0.26)	(0.31)
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 period fixed effects and individual fixed effects. 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 19: Effect of intervention on listed vacancies - extensions (split by geographical broadness)

	Broadness 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	0.27***	-0.06*	4.89
	(0.08)	(0.03)	(69.44)
X geographically narrow	-0.02	0.06**	-77.47
	(0.06)	(0.03)	(57.89)
Treatment			
X occ. broad and geo. broad	-0.12**	-0.01	76.28
	(0.06)	(0.05)	(146.78)
X occ. broad and geo. narrow	-0.17***	0.07*	-156.18**
	(0.06)	(0.04)	(67.64)
X occ. narrow and geo. broad	0.53***	-0.10**	-41.13
	(0.09)	(0.04)	(53.02)
X occ. narrow and geo. narrow	0.25***	0.03	47.72
	(0.09)	(0.02)	(74.45)
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 group fixed effects, period fixed effects, 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 broadness)

	Broadness 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	-0.12	-0.07*	0.23	-0.03	0.02
	(0.23)	(0.04)	(0.23)	(0.11)	(0.11)
X geographically narrow	0.18	-0.04	-0.03	-0.03	0.00
	(0.24)	(0.04)	(0.18)	(0.11)	(0.11)
Treatment					
X occ. broad and geo. broad	-0.41	-0.05	0.12	-0.11	-0.06
	(0.29)	(0.06)	(0.29)	(0.15)	(0.14)
X occ. broad and geo. narrow	-0.08	-0.00	-0.22	0.00	-0.03
	(0.24)	(0.06)	(0.19)	(0.15)	(0.14)
X occ. narrow and geo. broad	0.07	-0.09**	0.31	0.02	0.09
	(0.27)	(0.04)	(0.30)	(0.14)	(0.14)
X occ. narrow and geo. narrow	0.60	-0.08**	0.24	-0.09	0.04
	(0.37)	(0.03)	(0.31)	(0.15)	(0.16)
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 group fixed effects, period fixed effects, 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 broadness)

	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	0.94	0.60**	0.65**
	(0.93)	(0.33)	(0.33)
X geographically narrow	0.43	0.21	0.25
	(0.90)	(0.31)	(0.35)
Treatment			
X occ. broad and geo. broad	-0.15	0.96**	0.78**
	(0.74)	(0.63)	(0.49)
X occ. broad and geo. narrow	0.73	-0.19	-0.25
	(0.22)	(0.24)	(0.23)
X occ. narrow and geo. broad	1.76**	0.55*	0.70**
	(1.33)	(0.36)	(0.40)
X occ. narrow and geo. narrow	0.87	0.71*	0.75*
	(1.23)	(0.53)	(0.58)
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 group fixed effects, period fixed effects, 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	-0.02	0.44*
	(52.09)	(0.11)	(0.28)
Age	4.71	0.04	-0.01
	(14.04)	(0.04)	(0.04)
Age^2	-12.52	-0.07	-0.01
	(18.49)	(0.05)	(0.06)
Gender	72.41	-0.16	0.29
	(47.31)	(0.11)	(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*	0.12	0.26
	(52.81)	(0.14)	(0.19)
Couple	-73.41	-0.20	0.38
	(48.40)	(0.11)	(0.31)
Children	-84.87	0.12	0.05
	(56.63)	(0.17)	(0.19)
High educated	-24.84	-0.11	0.23
	(59.23)	(0.12)	(0.23)
White	54.64	-0.20	-0.03
	(69.10)	(0.14)	(0.18)
Constant	584.41**	8.15***	-0.13
	(276.25)	(7.20)	(0.72)
Model	Linear	Poisson	Poisson
Observation weeks	1-12	1-11	1-10
N	541	487	464

Each column represents one regression. All regressions include group fixed effects, period fixed effects 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 11: Distribution of the share of listed vacancies that results from using the alternative interface per participant (contains only the treatment group participants in weeks 4-12)

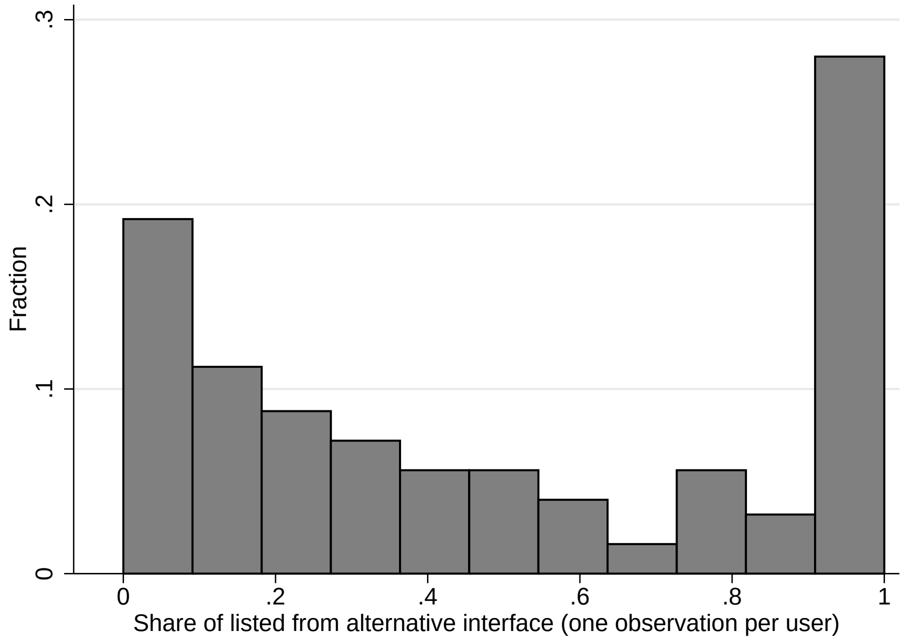


Table 23: Correlation between different broadness measures for listed vacancies

	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 24: 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.

Table 25: Mean values of characteristics of the treatment and control group for survivors in week 1, 6 and 12

	Control group survivors in:			Treatment group survivors in:			T-test (p-value) for equality in:		
	week 1	week 6	week 12	week 1	week 6	week 12	week 1	week 6	week 12
Demographics:									
female (%)	44	44	34	41	39	41	0.67	0.47	0.43
age	36	36	37	37	38	40	0.53	0.21	0.15
high educ [a] (%)	43	44	48	42	42	43	0.85	0.76	0.55
survey qualification level	4.2	4.3	4.4	4.5	4.5	4.6	0.27	0.47	0.48
white (%)	80	78	81	81	79	77	0.88	0.85	0.59
number of children	0.64	0.7	0.77	0.4	0.38	0.46	0.04	0.02	0.08
couple (%)	25	23	26	21	18	19	0.37	0.30	0.30
any children (%)	30	32	33	25	24	29	0.37	0.24	0.62
	59	57	58	57	54	51	0.70	0.71	0.40
Job search history:									
vacancies applied for	74	76	92	53	46	34	0.21	0.09	0.01
interviews attended	0.41	0.38	0.33	0.56	0.46	0.51	0.13	0.41	0.15
jobs offered	0.37	0.38	0.44	0.49	0.47	0.47	0.35	0.55	0.90
at least one offer (%)	19	21	23	21	19	19	0.77	0.71	0.52
days unempl. (mean)	286	289	305	231	200	190	0.45	0.23	0.14
days unempl. (median)	80	88	87	78	80	80			
less than 183 days	0.75	0.75	0.73	0.78	0.76	0.76	0.54	0.87	0.64
less than 366 days	0.85	0.85	0.82	0.86	0.87	0.86	0.64	0.64	0.51
jobseekers allow. (£)	48	49	46	56	61	65	0.43	0.34	0.27
housing benefits (£)	64	73	81	63	65	74	0.96	0.73	0.81
other benefits (£)	9.6	11	1.6	18	19	26	0.39	0.57	0.21
Weekly search weeks 1-3:									
listed	498	492	477	488	450	415	0.82	0.42	0.33
viewed	25	25	26	26	24	24	0.55	0.60	0.36
saved	10	10	12	11	9.8	9.7	0.56	0.69	0.32
applied	3.3	3.9	4.6	2.5	2.8	2.6	0.18	0.16	0.04
interview	0.094	0.1	0.11	0.087	0.1	0.08	0.84	0.94	0.55
applications other	9.2	9.5	11	7.5	7.4	6.7	0.15	0.14	0.03
interviews other	0.53	0.45	0.32	0.49	0.45	0.52	0.69	0.99	0.11
broadness listed[b]	3.2	3.2	3.2	3.2	3.2	3.1	0.73	0.73	0.39
broadness applied[b]	3	3	3	3.2	3.1	3.1	0.36	0.46	0.45
hours spend[c]	11	11	11	12	12	12	0.12	0.26	0.61
concern health (1-10)	1.5	1.5	1.8	1.7	1.9	2.1	0.46	0.27	0.47
conc. financial (1-10)	7.3	7.1	7.1	6.9	7	7.1	0.30	0.82	0.93
conc. competition (1-10)	7.4	7.4	7.3	7.2	7.2	7.3	0.52	0.45	0.97
met caseworker (%)	0.31	0.31	0.3	0.29	0.27	0.27	0.61	0.46	0.58
Observations	155	111	73	140	107	79			

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 6.1. [c] The number of hours spend on job search per week, as filled out in the weekly survey, averaged over week 2 and 3.

Figure 12: Broadness of listed vacancies (only control group participants that remained in the study until the end)

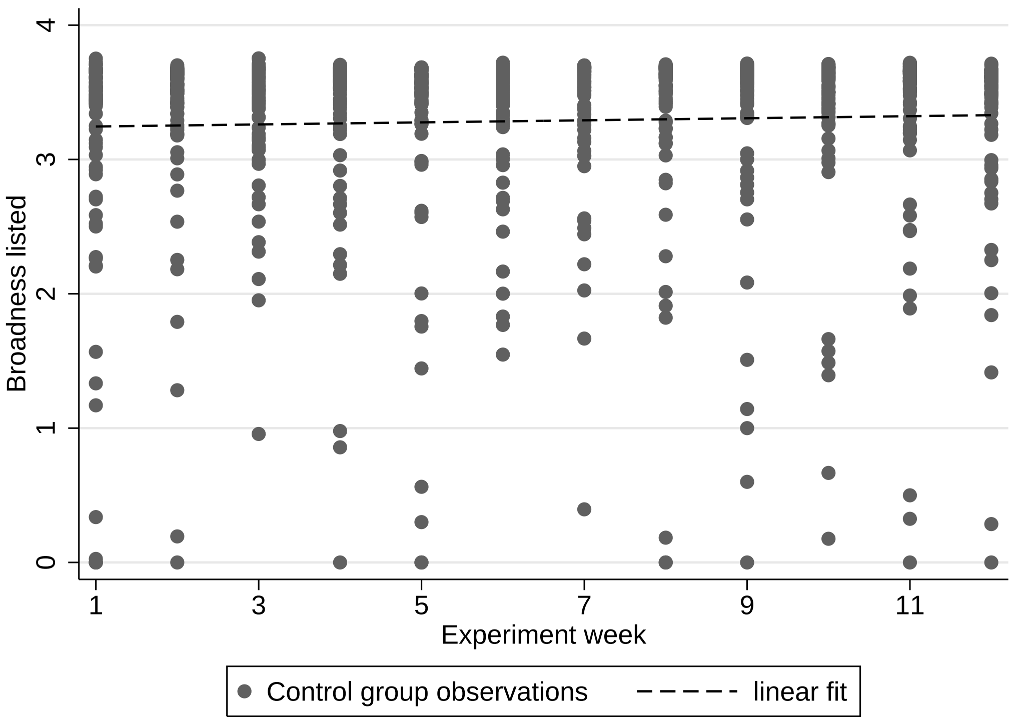
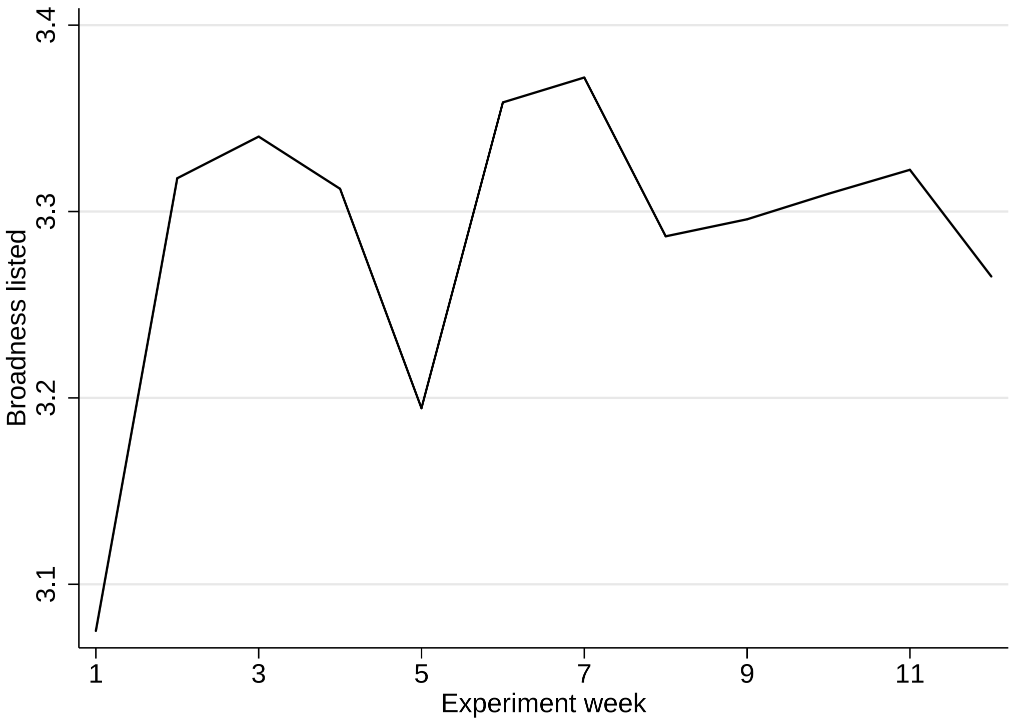


Figure 13: Average broadness of listed vacancies (only control group participants that remained in the study until the end)



9.2 Experimental instructions and supplemental documents

9.2.1 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 **should follow your usual job search strategy**, 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)

9.2.2 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. <u>SURVEY</u>

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. <u>THE JOB SEARCH ENGINE</u>

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 <u>a minimum of 30 minutes</u>.

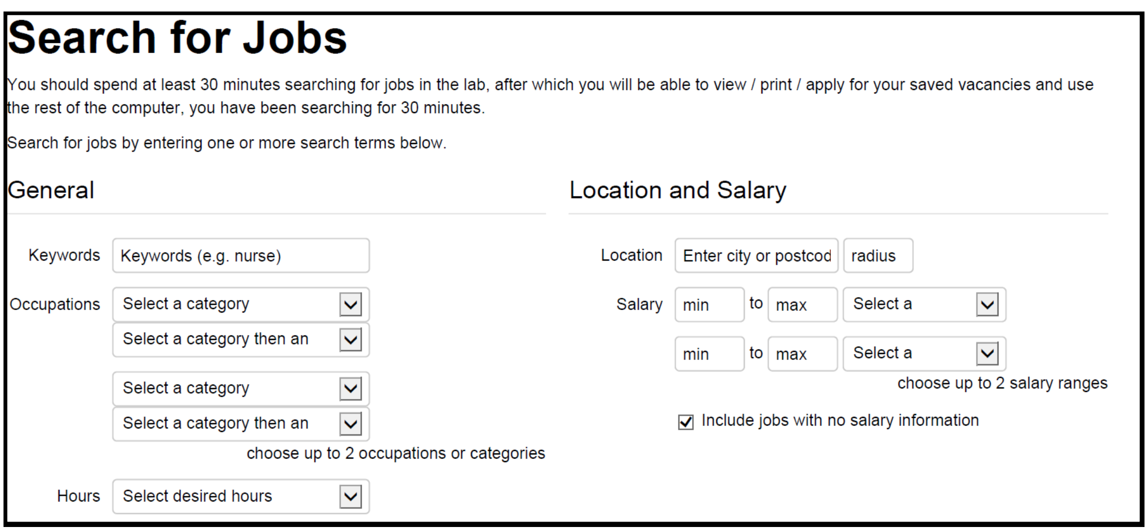
You can search using various criteria (keywords, occupations, location, salary, preferred hours). Importantly, you do not have to specify all of these. You just need to fill at least one of them.

If you specify more than one criterion, it is important to note that the computer will search for vacancies that satisfy <u>all</u> the criteria at the same time. For example, if you enter a keyword <u>and</u> you also select an occupation, it will search for vacancies that match <u>both at the same time</u>. 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 <u>the first OR the second occupation.</u> 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 each four consecutive weeks 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.

9.2.3 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.

9.2.4 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

9.2.5 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 *

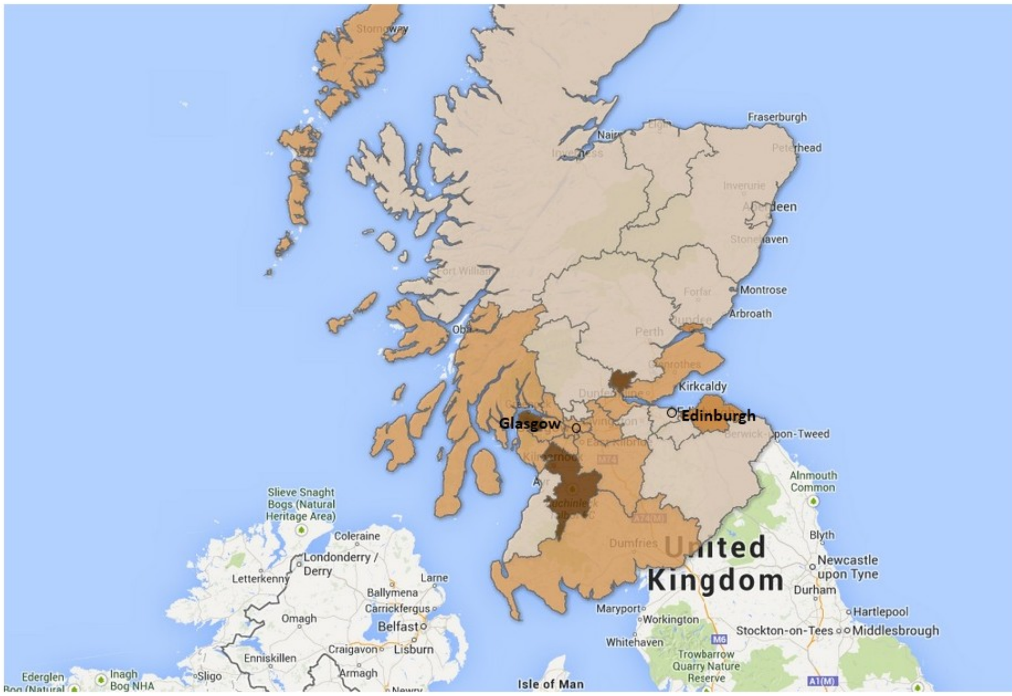
There is strong competition for jobs *

Health-related problems hinder my job search activities*

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

9.2.6 Heat maps

Figure 14: Example of a heatmap



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