Social comparisons in job search*

Jingcheng Fu[†]
Martin Sefton[‡]
Richard Upward[§]

Accepted by *Journal of Economic Behavior and Organization*
October 2019

Abstract

Using a laboratory experiment we examine how social comparisons affect behavior in a sequential search task. In a control treatment subjects search in isolation, while in two other treatments subjects get feedback on the search decisions and outcomes of a partner subject. The average level and rate of decline of reservation wages are similar across treatments. Nevertheless, subjects who are able to make social comparisons search differently from those who search in isolation. Within a search task we observe a reference wage effect: when a partner exits, the subject chooses a new reservation wage which is increasing in partner income. We also observe a social comparison effect between search tasks: subjects whose partners in a previous task searched for longer choose a higher reservation wage in the next task. Our findings imply that the provision of social information can change job-seekers' search behavior.

Declarations of interest: none
Keywords: Job search; social comparisons; distributional preferences; laboratory experiment
JEL classification: D83, D91, C91, J64

*We have benefited from comments received from participants at ESA 2015 (Heidelberg), CBESS-CeDEx-CREED Meeting (Amsterdam) 2016, LEW 2016 (Middlesex) and seminars at the Universities of Nottingham, Erasmus, Edinburgh and Mainz. We also thank Spiros Bougheas, Ken Clark, Simon Gächter, Daniele Nosenzo, Joep Sonnemans and two anonymous referees for helpful comments. This work was supported by the ESRC Grant ES/K002201/1 (Network of Integrated Behavioural Science), and was completed while Fu was an ESRC-supported PhD student under award 1369118 at the School of Economics, University of Nottingham.

[†]Corresponding author. jingcheng.fu@gmail.com, National University of Singapore
[‡]martin.sefton@nottingham.ac.uk, University of Nottingham
[§]richard.upward@nottingham.ac.uk, University of Nottingham

1 Introduction

In this paper, we investigate experimentally whether social comparisons affect job search. This is motivated by the fact that job search is typically not undertaken in isolation. Instead, job-seekers are aware of the job search activity of friends, colleagues and acquaintances, and information about others' job search successes and failures is often available. The social nature of search introduces scope for several factors that are usually ignored in economists' models of job search. In particular, a considerable body of evidence from social psychology shows that social comparisons have a powerful influence on the way humans feel and behave (Festinger 1954). People care about their status and evaluate this by comparing themselves with others, and people who are uncertain about what decision to make are influenced by the observed behavior of others (Bandura & Walters 1977). Recent field and experimental studies in economics have also found social comparison effects in various contexts, such as labor effort provision (e.g. Abeler et al. 2010, Angelova et al. 2012, Clark et al. 2010, Cohn et al. 2014, Gächter et al. 2012, 2013), job satisfaction (e.g. Card et al. 2012, Clark & Oswald 1996) and choice under risk (e.g. Friedl et al. 2014, Gamba et al. 2017, Haisley et al. 2008, Lahno & Serra-Garcia 2015, Linde & Sonnemans 2012, Rohde & Rohde 2011, Schwerter 2019).

There is indirect evidence that social comparisons affect job search. Using survey data, Clark (2003) finds that the unemployment rate in the social reference group is strongly positively correlated with the self-reported well-being of the unemployed. Thus, apparently, the unemployed feel better when they see that others around them are also unemployed. If this reduces the motivation to exit unemployment in areas with high unemployment it may contribute to regional variation in unemployment rates. More generally, social comparison effects may contribute to findings, such as those of Marmaros & Sacerdote (2002), that search outcomes are correlated within groups of individuals who interact.

Social comparison effects on job search can arise for a variety of reasons. One reason is that job-seekers may have *distributional preferences*, i.e. they care about both their own income and their income relative to others. There is extensive evidence that interpersonal comparisons can influence individuals' sense of well-being (Frank 1985, Loewenstein et al. 1989). Indeed, a large literature has emerged studying the impact of relative wage concerns on labor market outcomes (Akerlof & Yellen 1990, Frank 1984). As a consequence, economists have developed formal models incorporating distributional concerns into individual preferences. Using a prominent example of such a model (Fehr & Schmidt 1999) and applying it to a job-search context, we show that the reservation wages of job-seekers with distributional preferences will be influenced by the wages other job-seekers have accepted.

A second reason that social comparisons may affect job search stems from *learning*. There is a large literature empirically supporting the idea that people learn from each other in complex environments (see Manski 2000). Even if offer processes are independent, and even if job-seekers care only about their own narrowly-defined self-interest, in an environment where weighing up the attractiveness of a wage offer requires comparing it with a dynamic lottery, a job-seeker may be uncertain about whether it is worth accepting the wage offer. Whether others have accepted or rejected similar offers may then influence their decision. For example, if job-seekers observe others rejecting offers, this may lead them to reject similar offers.[1]

While field data offers ample evidence that social context matters because agents' behavior within groups is correlated, it is difficult to attribute correlated outcomes in field data to social comparison effects because there are other ways that interaction within a group can affect search outcomes. As noted in several theoretical and empirical studies of labor markets, social interaction can also affect job search by influencing a job-seeker's

[1]Distributional preferences and learning do not exhaust the possible mechanisms by which social comparisons affect behavior. A large literature in psychology on "social facilitation" (Zajonc 1965) emphasize the importance of being observed for performance on a variety of tasks.

available options and information about those options. For instance, graduates from the same institution may share job news and make use of alumni contacts which help them find a job. These types of *network effects*, which work through changes to the offer process (or information about the offer process) and which therefore have direct effects on the expected costs and benefits of taking an offer or waiting, can be viewed through the lens of standard economic theory in which job-seekers weigh up the expected costs and benefits of search.[2]

To investigate whether correlated group outcomes can arise from social comparisons, we use an experimental approach. Specifically, we create an experimental environment where network effects are absent, allowing us to identify pure social comparison effects on job search. In our view, observing pure social comparison effects in this setting is highly suggestive that social comparison effects will matter more generally. Of course, this does not provide information about the relative strength of network effects and social comparison effects in any given field setting.

Our framework for examining whether social comparisons affect job search is a sequential search decision experiment. This is based on McCall's (1970) "Basic Search Paradigm" in which searchers receive a wage-offer from a distribution and decide whether to accept it, receiving this wage in each remaining period, or reject it and wait for another offer. A long-established experimental literature has studied the sequential search task and its variants. Early studies compared individual behavior with optimal search rules and alternative heuristics (Schotter & Braunstein 1981, Braunstein & Schotter 1982, Hey

[2]There is a large empirical literature about how individuals use networks to collect information in order to find jobs and how search outcomes are influenced by social connections. According to an overview by Ioannides & Loury (2004), the literature finds that the use of networks in job search is widespread and generally productive, as it increases the job-finding rate and improves the quality of the match. These findings are reinforced by a number of more recent studies (e.g. Cingano & Rosolia 2012, Dustmann et al. 2016, Cappellari & Tatsiramos 2015, Brown et al. 2016). A number of studies have developed theoretical frameworks for analyzing the effects of social networks on job search (e.g. Calvó-Armengol & Jackson 2004, Galenianos 2013, Horváth 2014). The current study suggests that social comparison effects offer an additional (and overlooked) explanation for the empirical phenomenon that search outcomes are correlated within social networks.

1982, 1987, Oaxaca & Cox 1989, 1992) and more recent work has focused on behavioral explanations for deviations from theoretical predictions (Kogut 1990, Sonnemans 1998, Schunk 2009, Schunk & Winter 2009, Brown et al. 2011). A feature of all of this literature is that the search decisions are made in isolation. We extend this literature by adding a social context to the search task.

We do this by varying across treatments the feedback given to subjects about the search decisions and outcomes of others. In a control *Isolated treatment* subjects conduct the search task in isolation. They receive no information about others' search decisions or outcomes. In two other treatments, subjects are randomly and anonymously paired and receive feedback on their partner's search decisions and outcomes as they conduct the search task. In the *Partner treatment* subjects observe their partner's income, whereas in the *Partner-Offer treatment*, they are also informed of the offers rejected by their partner. In all treatments the subjects are given full information about the stochastic offer process, and the earnings of a subject depend only on how they react to their own offers, and are independent of the decisions and earnings of other subjects. Thus, for a fully rational self-interested subject, feedback about a partner's search decisions and outcomes is irrelevant.

In our Isolated treatment, in line with the long-established literature, average reservation wages are lower than the expected-earnings maximizing benchmark, and reservation wages decline within the search task. In the Partner and Partner-Offer treatments we observe similar average levels and rates of decline of reservation wages, but we also observe that subjects systematically respond to feedback about their partner's search outcomes.

In the Partner and Partner-Offer treatments, when a subject observes their partner accepting a low wage they reduce their reservation wage more than they otherwise would, while if they observe their partner accepting a high wage they reduce their reservation wage less than they otherwise would.

We show that these results are qualitatively consistent with predictions from a model based on Fehr & Schmidt's (1999) model of inequity aversion, in which job-seekers care about earnings differentials as well as their own earnings. To this extent, our results complement findings of "behindness aversion" from studies of social comparison effects on risk-taking (Lahno & Serra-Garcia 2015, Schmidt et al. 2015, Schwerter 2019).

The Partner-Offer treatment also allows us to see whether offers rejected by the partner affect reservation wages. These offers do not directly affect partner income, but do afford information about the partner's reservation wage. We find that rejected offers do not influence the reservation wage, which suggests the within-task social comparison effects do not arise because job-seekers simply attempt to copy their partner's reservation wage. This contrasts with evidence of imitative behavior from risk-taking studies (e.g. Lahno & Serra-Garcia 2015). We do, however see some evidence of social learning across repeated search tasks. In particular, subjects who observe their previous partner searching for longer increase their reservation wage (hence their own expected search duration) in the next task.

In summary, our experiment reproduces the qualitative findings of previous studies that have focused on individual decision-making in isolated search environments. In addition, in a first experiment studying this task in a social context, we find systematic social comparison effects. Some of these effects are consistent with those predicted by a simple model of distributional preferences, and in addition we find social feedback influences learning across repeated tasks.

The remainder of the paper is organized as follows. In Section 2 we review the (separate) relevant literatures on search experiments and social comparisons. In Section 3 we describe our experimental design and procedure, and in Section 4 we present our results. Section 5 offers a discussion and some concluding comments in which we discuss the relevance of these findings to policy and to other search environments.

2 Literature

2.1 Search Experiments

The sequential search model (McCall 1970) provides a tractable framework to analyze search decisions. The model assumes that a job-seeker receives one wage offer per period of search, where offers are independently drawn from a stationary and non-degenerate distribution, with a fixed outside option (typically a per-period unemployment benefit minus search cost). If the job-seeker accepts an offer they exit search and receive this wage for all future periods. If they reject the offer they wait for another offer in the next period. It is assumed that the time horizon is infinite and the job-seeker maximizes expected discounted utility. The optimal decision is to accept any offer above or equal to a critical value (the reservation wage), which is constant over periods, and to continue with search otherwise.

Early experimental studies by Schotter & Braunstein (1981, 1982) and Hey (1987), examine how subjects search in a lab environment based on this basic search paradigm. Other studies have investigated variants of the model, such as using a fixed time horizon (Oaxaca & Cox 1989, 1992), imperfect information about the offer distribution (Hey 1981, 1982, Moon & Martin 1990, 1992, 1996, Cox & Oaxaca 2000), heterogeneous search ability (Falk et al. 2009*b*,*a*) and variable search costs (Harrison & Morgan 1990, McGee & McGee 2016).

Previous experiments consistently show that subjects tend to (i) search too little and accept lower offers compared to the risk-neutral prediction, and (ii) reduce their reservation wages over the course of a sequential search task. Some behavioral theories have been proposed as explanations for these findings and have been tested in the laboratory. Studies suggest that the sunk-cost fallacy (Kogut 1990, Sonnemans 1998, 2000), loss and risk aversion preferences combined with bounded rationality (Schunk 2009, Schunk & Winter

2009, Soetevent & Bruzikas 2016) and subjective costs of uncertain waiting time (Brown et al. 2011) all contribute to these regular departures from the theoretical predictions.

Some experiments repeat the search task, allowing investigation of how behavior changes with experience, yet findings about learning effects are mixed. Kogut (1990) is the first experiment which repeats the search task within subject to allow for experience effects. With a small sample of subjects who conduct a series of repeated search tasks, he identifies a significant learning effect (i.e. search duration increases when subjects become more experienced) for half of the subjects. Sonnemans (1998) offers a more systematic examination of learning, showing that among subjects who repeat the search task many times, experimentation with different search strategies decreases with experience and search efficiency increases with experience. With fewer repetitions, Brown et al. (2011) find no significant learning effects.

All of these studies focus on individual decision-making, and exclude any social interaction. As far as we are aware the only experimental study embedding the basic search task in a social context is Ibanez et al. (2009), although their focus is rather different from ours. They examine whether search outcomes are improved if decisions are made by a group instead of an individual, finding that decisions made by groups are indistinguishable from individual decisions. Our focus instead is on whether an individual's search decision is influenced by social comparisons.

2.2 Social Comparison Effects

We define social comparison effects to be the causal effect on an individual's decision from observing other individuals' independent decisions and outcomes. In the context of job search, independence means that the job-seeker's offer process and earnings are not directly affected by other job-seekers' decisions and outcomes. We are particularly interested in two mechanisms for social comparisons which are relevant in the context of job search: distributional preferences and learning.

Distributional preferences have been frequently proposed as an explanation for social comparison effects. In the literature on individual risk-taking, several recent experimental studies have shown that individual choices are sensitive to the choices/earnings of other subjects even when subjects have no direct effect on each others' earnings (e.g. Schmidt et al. 2015, Linde & Sonnemans 2012, Lahno & Serra-Garcia 2015, Schwerter 2019). For example, Schwerter (2019) finds that subjects are willing to take more risks when a subject's peer has higher income, and shows this to be consistent with a model of loss aversion about a reference point provided by the income of the subject peer. His model of loss aversion about a reference point provided by peer income is analogous to Fehr & Schmidt's (1999) model of inequity aversion where disadvantageous inequality generates more disutility than advantageous inequality. As we show later, applying Fehr & Schmidt's model of distributional preferences to our laboratory setting leads to specific reference wage effects.

Another potential source of social comparison effects is social learning. Subjects may be uncertain about the quality of their decisions, and therefore "may seek to draw lessons from observation of the actions chosen and outcomes experienced by others" (Manski 2000). This might manifest itself in imitation: evidence for this is provided in a number of lab and field experiments (Lahno & Serra-Garcia 2015, Bursztyn et al. 2014, Cai et al. 2009, Conley & Udry 2010, Cai et al. 2015). For example, in a lab experiment Lahno and Serra-Garcia vary whether subjects can condition a simple lottery choice on the lottery choice or the lottery allocation of a peer. In both cases they observe imitative behavior, but to a far greater extent when peer choices are observed. They conclude that choices of the peer matter, above and beyond their direct impact on payoffs, and peer effects in risk-taking cannot only be explained by concerns about relative payoffs. In a field experiment, Bursztyn et al. study how investors' choices depend on information about how their friends invested. They find significant peer effects, and emphasize that both social utility and social learning mechanisms make a significant contribution to these.

Our paper is the first to investigate the relevance of distributional preferences and learning in a search task in which social comparisons are possible. In comparison with the literature on social comparisons in risk-taking, our task is more complex and involves a sequence of decisions which may be affected by the sequence of decisions and outcomes of a partner. We describe this task in the next section.

3 Experimental Design

3.1 The basic search task

We start by describing the search task which is common to all three treatments. To mimic the stationary, infinite horizon structure of the basic search paradigm, we use a random termination method, whereby the search task terminates after each period with a constant probability. The continuation probability plays the same role as the discount factor in the infinite horizon model: a subject maximizing expected earnings should use the same (constant) reservation wage as an infinitely-lived risk-neutral agent with a discount factor equal to the continuation probability.[3]

The search task consists of a sequence of periods. In the first period, subjects choose a reservation wage. This is an integer between 0 and 1000. Subjects then receive an offer, randomly drawn from a discrete uniform distribution over 1 to 1000. The offer is automatically accepted if it is greater than or equal to the reservation wage, and

[3]Random termination has been used extensively to mimic infinite-horizon games. See, for example, Dal Bó & Fréchette (2017). The only other search experiment of which we are aware which uses random termination is Brown et al. (2011). Brown et al. show that the use of random termination produces very similar results to a methodology which explicitly discounts the value of wage offers by the elapsed search time at which they are received.

rejected otherwise.[4] If the offer is accepted, subjects receive that wage as (points) income in all subsequent periods, receive no further offers and make no further decisions for the remainder of that sequence. If the offer is rejected, their income from the current period is zero. At the end of the period, subjects receive feedback which varies across the treatments, as we will discuss in the next subsection. The sequence continues with probability 0.95. If the sequence continues, subjects who have not accepted an offer are required to enter another reservation wage and the process repeats. At the end of the sequence, subjects are informed of their points earnings from the sequence, which is simply the sum of points earnings from all periods in the sequence. The information received at the end of each sequence does not vary across treatments.

Subjects complete 10 sequences, each sequence having the same structure described above. All offers are independent draws across subjects, periods and sequences. At the end of the tenth sequence one sequence is randomly chosen for each session and points earnings from this sequence are used to determine subjects' monetary earnings. Subjects receive a show-up fee of 5.00 GBP plus additional earnings from the chosen sequence, converted at a rate of 3,000 points = 1.00 GBP. All random draws (wage offers, sequence termination, sequence used for payment) were conducted during the session using the Z-tree random number generator.

All the information above is described carefully to subjects in the instructions which were handed out to subjects and read out at the beginning of the experiment. After reading out the instructions, control questions were given to ensure that subjects under-

[4]Subjects are required to enter the reservation wage within 15 seconds. If they do not enter a reservation wage within this time, this period's offer is rejected. The time-out rule does not seem to be binding in the great majority of cases. The average decision time of a search period is 3.8 seconds (standard deviation 1.9 seconds). The median decision time is 3 seconds, and 95% of the decisions are entered in under 8 seconds. The time restriction forces subjects to complete a period at roughly the same time, facilitating the synchronization of feedback. In the experimental instructions we refer to the reservation wage as a "minimum acceptable offer"; see instructions in Appendix A for details. We have subjects enter reservation wages rather than make binary accept/reject decisions in order to obtain more information about their search strategy. This technique was first applied in search tasks by Oaxaca & Cox (1992).

stood essential information about the search environment. The session did not start until all subjects had correctly answered all control questions. See Appendix A for a copy of instructions and control questions.

3.2 Treatments

To allow us to study the effect of social comparison, we manipulate across treatments the feedback received by subjects at the end of each period. In the simplest Isolated treatment, the feedback received by subjects in each period consists only of information on their own offer, whether it is accepted, the resulting per-period income and their accumulated earnings across all periods in that sequence. In the Partner and Partner-Offer treatments subjects are randomly re-paired at the beginning of each sequence. The random termination of sequences, which is implemented independently for each subject in the Isolated treatment, is implemented independently at the pair level in the Partner and Partner-Offer treatments. In all three treatments, offers are independent across subjects and an additional control question is added in the Partner and Partner-Offer treatments to ensure that subjects understand that own and partner offers are drawn independently.

In the Partner treatment, subjects are additionally shown their partner's income at the end of each period. They are therefore aware whether their partner has accepted an offer or not, and the amount of that offer if it was accepted. The Partner treatment allows for relatively little social feedback because subjects only learn their partner's income each period. Subjects whose partner has an income of zero cannot tell whether this is because their partner has a high reservation wage, or because their partner received low offers. This also means that there is relatively little within-sequence variation in the feedback received. The partner's per-period income is either zero (meaning the partner has not yet accepted an offer) or it is constant across periods.

In the Partner-Offer treatment, subjects again receive information about partner income at the end of a period. In addition, subjects are also shown the offer made to their partner, and whether or not their partner accepted or rejected that offer. Thus the Partner-Offer treatment allows for more feedback and more within-sequence variation. It also allows us to test whether feedback on rejected offers, as well as income, affects search behavior.[5]

The main features of the design are summarized in Table 1. Each session consisted of 12 subjects. We conducted six sessions for each treatment. No subject participated in more than one session.

Table 1: Treatment Design

	Isolated	Partner	Partner-Offer
Sessions	6	6	6
Subjects per session	12	12	12
Sequences per subject	10	10	10
Paired subjects	No	Yes	Yes
Mutual feedback on			
Offer	No	No	Yes
Income	No	Yes	Yes

3.3 Discussion of the design

The Isolated treatment provides a benchmark which we can compare to the existing results from experimental analyses of search, all of which study decisions made in isolation. In this setting, expected points earnings are maximized by choosing a constant reservation wage $r^* = 725$ (see Appendix B.1). This is also the optimal reservation wage in the Partner and Partner-Offer treatments if subjects care only about expected earnings.[6] If

[5]See Figure A1 in Appendix A for an illustration of the feedback screens seen by subjects in our three treatments.

[6]If a subject is risk averse, the level of the optimal reservation wage will change, but will still be constant across periods, and will still be the optimal reservation wage regardless of which treatment the subject is in.

social comparisons matter, the optimal reservation wage in the Partner and Partner-Offer treatments may differ from the Isolated treatment.

There are several ways to model how social comparisons may affect behavior. Here we consider the implications of allowing individual preferences to depend on own and other earnings. Such distributional preferences will affect the optimal reservation wage.[7] Specifically, following Fehr & Schmidt (1999), we assume that subject i dislikes income differences, as represented by the period utility function

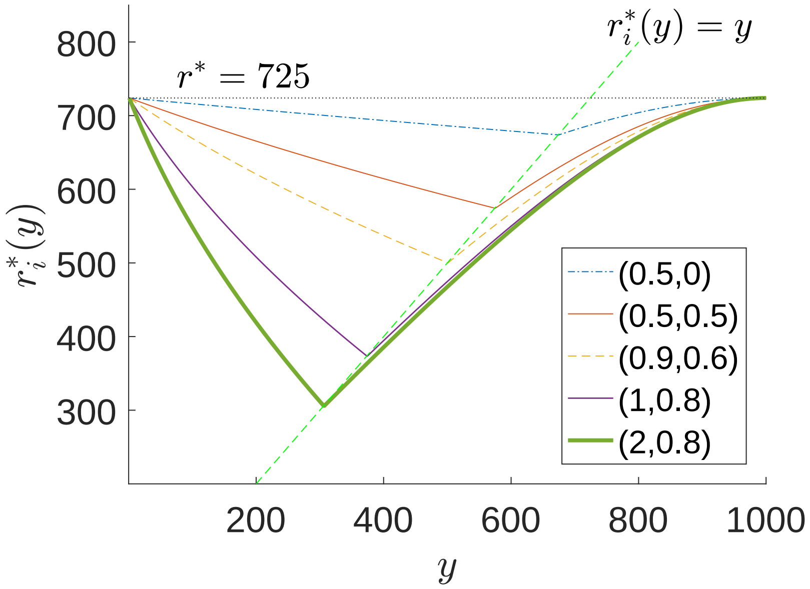
$$u_{it}(y_{it}, y_{jt}) = y_{it} - \alpha\big(\max\{y_{it}, y_{jt}\} - y_{it}\big) - \beta\big(\max\{y_{it}, y_{jt}\} - y_{jt}\big), \tag{1}$$

where y_{it} denotes i's period t income, y_{jt} denotes the period t income of i's partner, α is the marginal disutility from disadvantageous inequality and β is the marginal disutility from advantageous inequality.

If i's partner has exited with an income of $y_{jt} = y$, the optimal reservation wage $r_i^*(y)$ can be shown to be a decreasing function of the partner's income if the partner has accepted a low wage, and an increasing function if the partner has accepted a high wage (see Appendix B.2 for details). We plot the optimal reservation wage function in Figure 1 for five sets of (α, β), chosen based on the assumptions made in Fehr & Schmidt (1999) i.e. $0 \leq \beta \leq 1$ and $\alpha \geq \beta$. We observe that if the partner has accepted an offer of 0, i cannot earn less than the partner, and so period utility given by (1) is linear in i's own income y_{it} and expected utility is maximized with the expected earnings maximizing r^*. If the partner has accepted an offer of 1,000, i cannot earn more than the partner, period utility is again linear in y_{it} and expected utility is maximized with r^*. If the partner accepts an offer in between, period utility as a function of y_{it} has a kink, analogous to loss aversion with a social reference point, which depresses the optimal reservation wage.

[7] Schwerter (2019) and Linde & Sonnemans (2012) take a similar approach to analyze the effect of social reference points on risk taking in a static risk choice task.

Figure 1: Optimal reservation wage for i after j exits with y: $r_i^*(y)$



Notes: (α, β) for each curve is given by the legend; the horizontal dotted line indicates the expected earnings maximizing $r^* = 725$; the kink point of $r_i^*(y)$ lies on the 45-degree line.

To identify the relevant portion of the reservation wage function, we need to analyze the dynamic game where both i and j are still searching. Identifying the equilibrium initial reservation wage requires assumptions about both subjects' preferences and what they know about each other's preferences. For tractability, we assume subjects have identical preferences and this is common knowledge. In Appendix B.2 we show that the initial equilibrium reservation wage lies above the kink point of the $r_i^*(y)$ function. This implies that a subject never accepts a wage below the kink point and so in equilibrium only the positively-sloped part of the reservation wage function is relevant in Figure 1.

The initial reservation wage is very close to the expected earnings maximizing reservation wage r^* and remains constant until one of the subjects accepts an offer (i.e. rejected offers have no effect on reservation wages). If the partner exits, the subject adopts a new, constant reservation wage. The new reservation wage is increasing in the partner's accepted offer, but in expectation is lower than the initial reservation wage. We refer to the positive relationship between a partner's income and the subject's reservation wage as a reference wage effect.

To test for this reference wage effect in our experimental data we examine the change in the reservation wage within sequences. We examine how the reservation wage changes following the partner's exit, and whether this change is moderated by the partner's income. The change in the reservation wage is predicted to be the same in both the Partner and Partner-Offer treatments, while the Isolated treatment provides a natural placebo test.

Within-sequence social comparison effects can also arise from a desire to imitate the partner's decision. In the Partner and Partner-Offer treatments, the feedback allows the subjects to make (limited) inferences about their partner's reservation wage.[8] If they observe the partner accept an offer they would have rejected, they can infer that their partner had a lower reservation wage. If the subject only desires to imitate their partner, they should reduce their reservation wage, and their new reservation wage will be no greater than the partner's accepted offer. In the Partner-Offer treatment there is an additional scenario that would induce an imitative subject to change their reservation wage. If the subject observes the partner reject an offer they would have accepted, they can infer that their partner had a higher reservation wage. Thus they should increase their reservation wage, and their new reservation wage will be no smaller than the rejected offer.

The reference wage effect and the imitation effect are largely observationally equivalent, and consequently difficult to disentangle. However, with our design, the Partner-Offer treatment offers a testable difference: the rejected offers of partners have no impact according to the reference wage effect, whereas they have a positive impact according to the imitation effect.

[8] We do not directly reveal subjects' reservation wages to their partners. This reflects the fact that, in real job search environments, job-seekers do not typically observe their peers' reservation wages, but they often do observe their peers' search outcomes (as in the Partner treatment) or the offers their peers receive (as in the Partner-Offer treatment).

The fact that we have repeated sequences also allows us to test for an effect of social feedback across sequences. In the Isolated treatment, subjects can learn about how their own strategies affect sequence earnings and, based on prior literature (see Section 2.1), experience from earlier sequences may improve search efficiency in later sequences. In the social feedback treatments, the additional information about the partner's strategies and income might be expected to enhance this process. We therefore examine whether previous partner income and search duration affect the subject's choice of reservation wage in the next sequence.

4 Results

216 subjects who were recruited through ORSEE (Greiner 2015) participated in the 18 sessions. None of the subjects had participated in a search experiment before. All sessions lasted for approximately 50 minutes, and earnings averaged 10.11GBP per subject inclusive of the show-up fee.[9] The experiment was computerized using the z-Tree program (Fischbacher 2007) and conducted in the CeDEx laboratory at the University of Nottingham in March and April 2015 and March 2016. Subjects were randomly assigned into Isolated, Partner and Partner-Offer treatments. Observable characteristics are similar across the treatments, as shown in Table 2. The differences in proportions are insignificant according to pooled chi-square tests (p-values are reported in the last column).

We observe 2,160 sequences of search (3 treatments $\times$ 6 sessions $\times$ 12 subjects $\times$ 10 sequences), which contain 4,749 decision-making periods.[10] 5.79% of sequences were

[9] The use of random termination can lead to very long sequences. However, sessions are booked for 90 minutes and no session lasted for more than 60 minutes. The realized average sequence length is 20.88 periods with the standard deviation of 20.61, and the longest sequence lasted for 179 periods.

[10] In 7 periods, the subject passed the 15-second time limit and consequently the offer was rejected. This happened to 6 different subjects, 1 in the Isolated, 1 in the Partner and 4 in the Partner-Offer treatment.

censored, which means the subject was still searching when the sequence was terminated by the computer and no offer was accepted in that sequence.

Table 2: Observable characteristics of subjects across treatments

Treatment	Isolated		Partner		Partner-Offer		p-value from χ^2 test
Female	45	(62.5%)	44	(61.11%)	49	(68.06%)	0.656
Postgraduate	6	(8.33%)	6	(8.33%)	6	(8.33%)	1.000
Field of study							
Science	34	(47.22%)	37	(51.39%)	39	(54.17%)	0.703
Social Science	30	(41.67%)	22	(30.56%)	22	(30.56%)	0.268
Arts	8	(11.11%)	13	(18.06%)	11	(15.28%)	0.498

Notes: Sciences include Engineering and Medicine and Health Sciences. Double majors are classified as Sciences if a science major is combined with another major, or as Social Sciences if a social science major is combined with arts major.

Table 3 summarizes levels and changes in reservation wages for each treatment. It shows that the two regularly-observed departures from the theoretical predictions discussed in Section 2.1 are replicated in all treatment conditions of our experiment. First, in all treatments average reservation wages are below the reservation wage of an expected earnings maximizer ($r^* = 725$). Panel (a) of Table 3 reports average initial (period 1) reservation wages of 539, 555 and 512 in the Isolated, Partner and Partner-Offer treatments respectively, with 95% confidence intervals easily excluding r^*.[11] As a result of these low reservation wages, search spells tend to be short, on average 2.19 periods (the distribution of search spell lengths is shown in Figure C1 in Appendix C). Subjects often exit with an offer below r^* (37.45% of the sequences in our sample). Average reservation wages in all periods summarized in Panel (b) are slightly higher than average initial reservation wages, mainly due to a selection effect (subjects who set lower reservation wages tend to exit search earlier and so are under-represented in the sample), but still significantly below r^*. This finding is consistent with the existing experimental literature. For example, our average reservation wage is just below 80% of r^*, and a similar ratio can be calculated from the average reported in Brown et al. (2011).

[11] Throughout the paper, standard errors are clustered at the session level.

Table 3: Overview of reservation wages across treatments, pooled across sequences

	Obs.	Mean	Std. Err.	95% C.I.	
(a) Initial period					
Isolated	719	538.59	23.08	(479.26	597.91)
Partner	719	555.07	40.36	(451.33	658.81)
Partner-Offer	717	512.00	10.61	(484.71	539.28)
(b) Overall average					
Isolated	1,575	586.85	14.51	(549.56	624.15)
Partner	1,587	585.42	29.03	(510.80	660.04)
Partner-Offer	1,580	543.10	15.01	(504.53	581.68)
(c) Within sequence per-period change					
Isolated	855	−26.50	4.53	(−38.15	−14.85)
Partner	867	−29.81	3.65	(−39.18	−20.43)
Partner-Offer	859	−32.47	3.24	(−40.81	−24.13)

Notes: 7 periods in which the subject fails to enter a decision before the time limit (0.1% of active periods) are excluded from the summary.

Second, in all treatments we consistently observe a declining reservation wage within sequences. Panel (c) of Table 3 shows that the average per-period change in the reservation wage is −27, −30 and −32 in the Isolated, Partner and Partner-Offer treatments. The 95% confidence intervals all exclude zero. Figure 2 shows this graphically by plotting the average reservation wage across periods, conditional on search length.[12] For all subsamples, the reservation wage is declining in all treatments. This finding is also consistent with the existing experimental literature, see for example Braunstein & Schotter (1982) and Brown et al. (2011).[13]

At an aggregate level, we find strong similarities across treatments. Neither initial period reservation wages, average reservation wages across all periods, or the within sequence per-period changes in reservation wages are significantly different across treat-

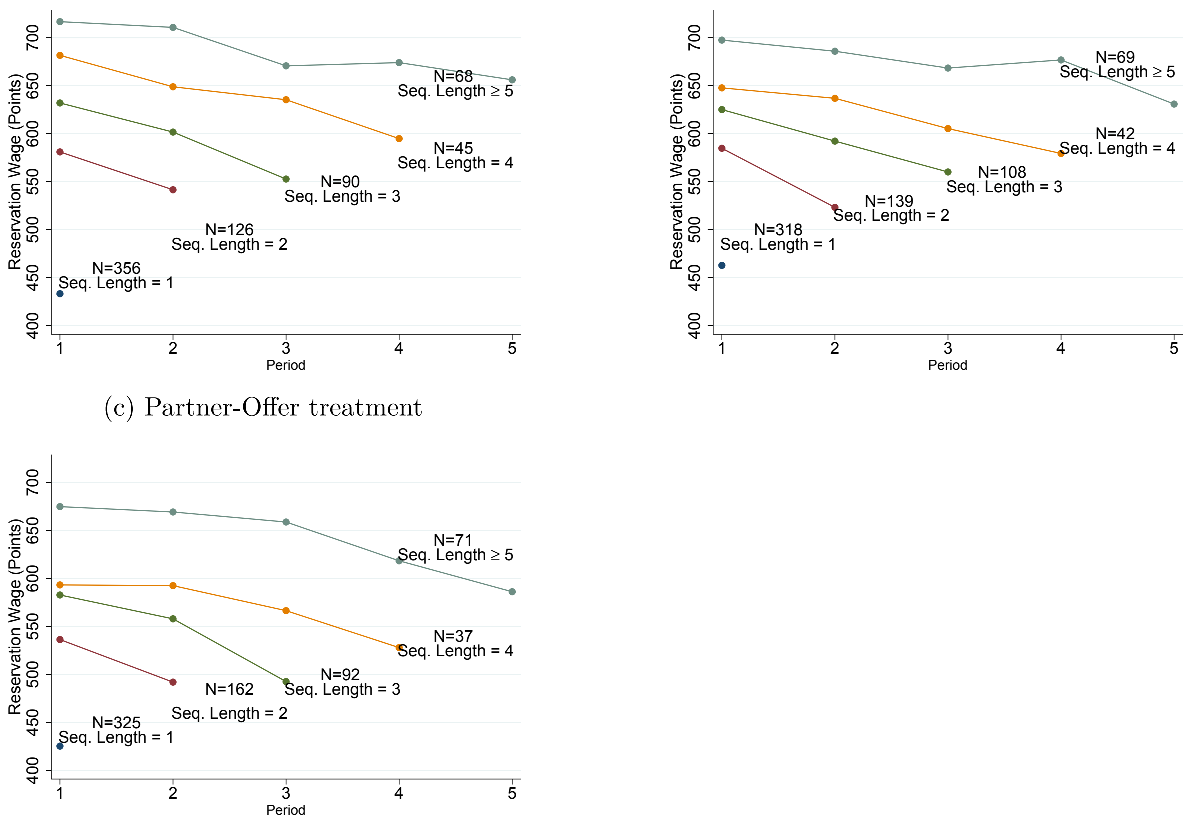
[12]We condition on search length because subjects with higher reservation wages will tend to have longer search durations. If we did not condition on search length, the average reservation wage would increase across periods as subjects with lower reservation wages exit faster, even if subjects have constant reservation wages.

[13]Indirect evidence of the declining reservation wage also comes from studies which do not elicit a reservation wage but allow subjects to recall offers. In these studies, many subjects exercise the option to recall offers that have been previously rejected (Hey 1987, Kogut 1990).

Figure 2: Reservation wage by search length

(a) Isolated treatment (b) Partner treatment

(c) Partner-Offer treatment



Notes: N in each figure refers to the number of sequences with that search length. To ensure that sequences are not selected into the subsamples by random termination, 106 sequences (4.91%) which are censored due to termination before the fifth period are not included in the summary. The sequences in which the subject passed the time limit for the initial period are also excluded.

ments.[14]

However, in the next two sections we analyze the data at a more disaggregated level and show that subjects systematically respond to social feedback in our Partner and Partner-Offer treatments, both within and between sequences.

[14]Treating each subject as an independent observation ($N = 3 \times 6 \times 12$), a Kruskal-Wallis rank test gives $p = 0.177$ for equality of initial period reservation wages, $p = 0.139$ for equality of overall average reservation wages and $p = 0.764$ for equality of per-period change in reservation wages.

4.1 The impact of social feedback within sequences

To test whether subjects are responding to their partner's search feedback, we compare the decisions made before and after feedback is received. To do this, we focus on the difference between the reservation wage in period t and the reservation wage in the previous period $t-1$. Search feedback from the partner is characterized by two key variables. First, an indicator of whether subject i's partner j accepted an offer in the previous period. Second, the value of the offer received by j in the previous period. These variables are only defined in periods in which subject i is still searching at t, and their partner j was still searching at $t-1$, and therefore the sample used in the analysis in this section consists of those observations from periods t in which the subject's partner was still searching at $t-1$. In the Isolated treatment subjects do not have a partner, we therefore create artificial partners by pairing subjects randomly. The artificial partners do not provide any feedback, and hence are used for placebo tests.

There are 1,671 periods in the sample, which is 35% of all the decision-making periods. The initial periods of a sequence (2,160, 45%) are excluded because no feedback from the partner is possible. Similarly, periods in which the partner exited before $t-1$ (910, 19%) are excluded because, again, no feedback is possible at t. In addition, 8 periods (0.2%) in which the subject timed out in the current or the previous period are also excluded. Almost all subjects (96.76%) are included in the sample for one or more sequences (the average is 5.28 sequences per subject), and observations are fairly evenly distributed across sequences. The sample selection by treatment is given in Table C1 in Appendix C.

In Figure 3 we plot the correlation between the change in subject i's reservation wage and the value of the offer received by their partner j. We do this separately, by treatment, for those cases where j rejected the offer (top row) and those cases where j accepted the offer (bottom row). We separate in this way because accepted offers lead to a change in partner's income, whereas rejected offers do not. The sample size, the Pearson correlation coefficient and p-value are given at the top of each panel, and in addition we display a scatter plot of the average change in the reservation wage and average value of the partner's offer for each decile of the partner's offer (10 data points) and a linear fit. As noted earlier, Panels (a) and (d) are placebo conditions from the Isolated treatment. Panel (b) is also a placebo condition because there is no feedback on rejected offers in the Partner treatment. As expected, correlations in all the placebo conditions are insignificant. The correlation between the change in reservation wage and partner's rejected offer is also small and insignificant in the Partner-Offer treatment (Panel (c)). The patterns are quite distinct in those cases in the Partner and Partner-Offer treatments where the partner accepts the offer (Panels (e) and (f)). The change in reservation wage is significantly and positively correlated with partner's offer, with a correlation coefficient of 0.144 ($p = 0.025$) and 0.179 ($p = 0.006$).

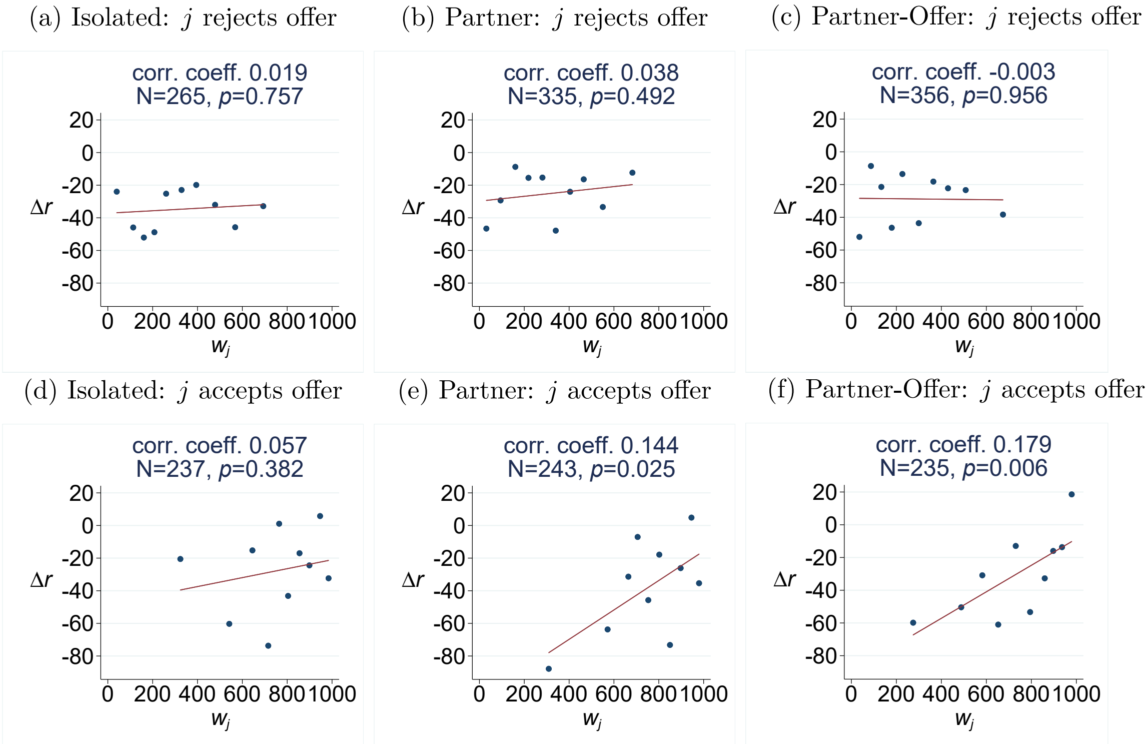
To quantitatively estimate how subjects are influenced by social comparisons, we estimate the following model for each treatment:

$$r_{is,t} - r_{is,t-1} = \beta_0 + \beta_1 a^j_{is,t-1} + \beta_2 (a^j_{is,t-1} \cdot w^j_{is,t-1}) + \beta_3 w^j_{is,t-1} + \lambda_t + \theta_s + \eta_i + \epsilon_{is,t} \quad (2)$$

The subscripts i, s and t are identifiers for, respectively, subject, sequence and period. The dependent variable is the change in the reservation wage relative to the previous period. The explanatory variables are an indicator for whether the partner accepted their offer in period $t-1$ ($a^j = 1$ for acceptance and 0 for rejection), the value of the partner's offer in period $t-1$ (w^j) and the interaction of the acceptance indicator and the offer, which is the period $t-1$ income of the partner. We control for period (λ_t) and sequence (θ_s) fixed effects, and allow for subject random effects (η_i).

In the Isolated treatment a^j and w^j are not observed, so changes in the reservation wage must be independent of these variables by design, and so β_1, β_2 and β_3 should be zero. In the Partner treatment a^j is observed but w^j is only observed if $a^j = 1$, so β_3

Figure 3: Correlation between changes in reservation wage and partner's offer



(a) Isolated: j rejects offer (b) Partner: j rejects offer (c) Partner-Offer: j rejects offer

(d) Isolated: j accepts offer (e) Partner: j accepts offer (f) Partner-Offer: j accepts offer

Notes: We separate the observations in each treatment by the decision of the partner (or placebo partner for the Isolated treatment). The Pearson correlation between partner's offer and changes in reservation wage, and unadjusted, significance level are given at the top of each panel. The panels display the binned scatter plots: for each decile of partner's offers in the subsample, we plot the average change in the reservation wage and the average of partner's offer. Also shown is the fitted linear relationship among the 10 dots. Details of this method can be found in Stepner (2013).

should be zero. In the Partner-Offer treatment, both a^j and w^j are observed in every period and so β_1, β_2 and β_3 might all be different from zero if subjects respond to this information.

Estimates of equation (2) are reported in Table 4.[15] On average across all periods and sequences in the sample, subjects reduced their reservation wage by about 30 points per period. This is very similar to the per-period decline reported in Table 3 and Figure 2. In the Isolated treatment, as expected, $\hat{\beta}_1$, $\hat{\beta}_2$ and $\hat{\beta}_3$ are all small and insignificantly different from zero. In the Partner and Partner-Offer treatments, however, $\hat{\beta}_1$ is significant and negative, indicating that a subject whose partner exits in the previous period lowers

[15]The results are robust to alternative specifications, such as dropping the period and sequence dummies, or the addition of controls for observable characteristics, see Table D1 in Appendix D.

their reservation wage by 60–70 points more than they otherwise would. $\hat{\beta}_2$ is significant and positive in the Partner and Partner-Offer treatments, showing that the value of the accepted offer is also relevant. A subject's reservation wage is increasing with the offer accepted by the partner; the slope is approximately 0.1 ($\hat{\beta}_2 + \hat{\beta}_3$). By comparison, in our analysis of job-seekers with Fehr-Schmidt preferences the slope varies between 0.13 and 0.34 depending on preference parameters (see Table B1 in Appendix B.2 for details).

The exit and income effects together imply that a subject whose partner exits may increase or reduce their reservation wage in the next period, depending on the value of the partner's exit wage. Given that the average exit wage of the partner is 746 (Partner treatment) and 718 (Partner-offer treatment), our estimates of equation (2) predict a very small negative effect of partner exit on reservation wage. This leads to a small predicted increase in the probability of exit in the period after a partner exits of less than 1%. This small effect helps to explain why the average change in the reservation wage is similar in the Isolated and social feedback treatments. By comparison, in our theoretical analysis the remaining job-seeker's average exit probability increases by 1% to 3% depending on preference parameters (also see Table B1).

In contrast to the estimated effect of accepted offers ($\hat{\beta}_2 + \hat{\beta}_3$), offers which are rejected appear to have no impact on subjects' reservation wages: $\hat{\beta}_3$ is small and insignificantly different from zero in all treatments.

These regression results reinforce the findings reported in Figure 3. First, all estimated coefficients in the Isolated Treatment are insignificant. Second, the results reproduce the positive correlation between the partner's income and the change in the subject's reservation wage in the Partner and Partner-Offer treatments. Third, the results reproduce the insignificant effect of rejected offers on reservation wages in the Partner-Offer treatment. The latter two findings are qualitatively consistent with the Fehr-Schmidt model

Table 4: Change in reservation wage between periods as a function of partner's offer and acceptance decision

	(1) Isolated	(2) Partner	(3) Partner-Offer
Mean Δr	-31.538	-30.702	-29.844
β_1 j accepted offer (a^j)	-7.984	-71.205^{**}	-63.645^{***}
	(22.810)	(33.313)	(18.923)
β_2 j's income ($a^j w^j$)	0.009	0.092^{**}	0.088^{***}
	(0.034)	(0.046)	(0.027)
β_3 value of j's offer (w^j)	0.0004	-0.006	-0.002
	(0.019)	(0.022)	(0.013)
Subject random effects	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes
Sequence fixed effects	Yes	Yes	Yes
Number of obs.	510	578	591
Number of sessions	6	6	6

Notes: In the Isolated treatments, results are based on 10,000 replications each with a random pairing of partners. The pairing affects the number of observations because whether or not feedback is available in a particular period depends on whether the partner has already exited. We report the average number of observations across all replications. Standard errors clustered at the session level are in parentheses.

analyzed in Appendix B.2.[16]

The second finding is also consistent with subjects inferring their partner's reservation wage and attempt to imitate it. As noted earlier, in our setting, subjects can make inferences about their partner's reservation wages when they observe their partner's income. When a subject observes their partner accept an offer below their own reservation wage they can infer that the partner's reservation wage is lower than their own. If the subject wishes to imitate they will lower their own reservation wage. For a fixed initial

[16]To test whether the estimated coefficients are different between the Isolated and social feedback treatments, we compare the point estimates of β_1, β_2, and β_3 from Partner and Partner-Offer treatments to the distribution of those coefficients generated by the 10,000 replications of random pairings of subjects in the Isolated treatment; the two-sided p-values are constructed as the probability of getting more extreme coefficients from the replications. β_1 is significantly smaller (more negative) in the social treatments ($p = 0.003$ and 0.006 for Partner and Partner-Offer, respectively), while β_2 is significantly larger (more positive) in the social treatments ($p = 0.005$ and 0.007). There is no difference in the estimates of β_3 between the Isolated and social treatments ($p = 0.757$ and 0.932).

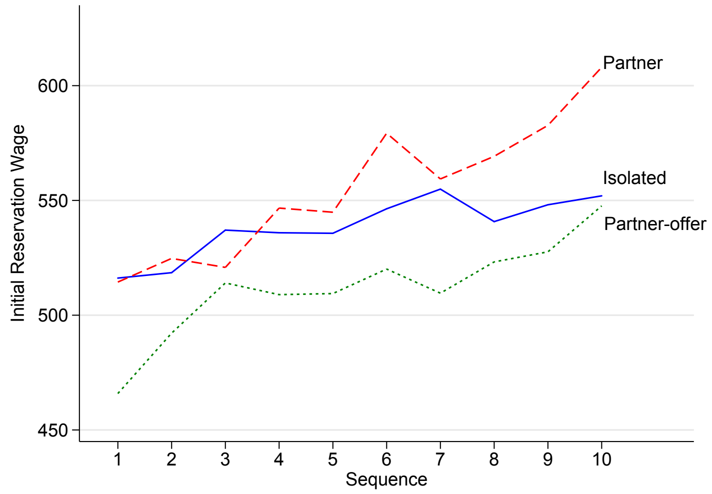
own reservation wage the reduction will be larger the lower the accepted offer. However, reservation wage imitation would also imply an analogous rejected offer effect when the subject observes their partner reject an offer above their own reservation wage. We do not observe such an effect in either Figure 3c or Table 4.

4.2 Impact of social feedback between sequences

We now consider whether feedback also changes reservation wages between sequences. Recall that subjects and their partners are randomly re-paired at the end of each sequence, and only one randomly-drawn sequence is used to determine earnings. A sequence is therefore very much like previous sequences, and from this perspective we might expect behavior to be similar across sequences. However, behavior might change across sequences if subjects learn from their own outcomes and the outcomes of their partners in earlier sequences.

In Figure 4 we plot the average period 1 reservation wage in each sequence across all 10 sequences. We use period 1 reservation wages for this comparison because reservation wages in period 1 are not yet influenced by the within-sequence feedback documented in the previous section. Figure 4 shows that initial reservation wages tend to increase across sequences in all three treatments. A linear trend estimated separately for each treatment yields a slope of 9 points (standard error 2 points, $p = 0.005$) per sequence in the Partner treatment, 6 (3, $p = 0.093$) in the Partner-Offer treatment, and 3 (3, $p = 0.227$) in the Isolated treatment. This implies that, in the Partner treatment, subjects choose a reservation wage 16.63% higher in the final sequence compared to the first sequence. In the Partner-Offer treatment, this increase is estimated to be 11.71%. In the Isolated treatment, this increase is estimated to be 6.43%. However, the difference in sequence trend between Partner and Isolated treatments is only marginally significant ($p = 0.067$), and the difference between Partner-Offer and Isolated treatments is insignificant ($p =$

Figure 4: Period 1 reservation wages by treatment and sequence



0.499). The difference between Partner and Partner-Offer treatments is also insignificant ($p = 0.351$).[17] Therefore, at the aggregate level, we do not find strong evidence of treatment differences.

During each sequence, subjects in the Partner and Partner-Offer treatments observe their partner's income and search duration. We therefore examine whether the change in reservation wage between sequences is affected by social feedback in the form of previous partner income and search duration from the previous sequence. We measure the between-sequence change in period 1 reservation wages, $\Delta r_{is1} = r_{is1} - r_{i(s-1)1}$. Feedback from the previous sequence is measured as own sequence earnings and duration of search, and previous partner j's sequence earnings and duration of search. An alternative measure of income is the exit offer. Since subjects receive direct feedback on partner's per-period income (i.e. the exit offer), and so the exit offers might be regarded as the more salient measure of relative "success".

There are 1,756 subject-sequences for which we have information on Δr_{is1}, income and search duration for the subject and their partner in the previous sequence, which

[17]The findings are robust to the inclusion of subject characteristics, reported in Table D2. Table D2 also examines sequence trends for exit period reservation wages. These are positive and significantly different from zero in all treatments, but are higher in the social feedback treatments (and again the difference is only significant between Partner and Isolated treatments).

is 81.30% of all subject-sequences. We cannot calculate Δr_{is1} in the first sequence for each subject (216, 10%). We also lose a very small number of observations (4, 0.19%) because of subjects who were timed out before they entered a value for r. Because we require information on both i and j's income and search duration in the previous sequence, we also drop the pair of subject-sequences if either i or j were censored in the previous sequence (184, 8.52%). The sample selection by treatment is given in Table C2 in Appendix C.

The model we estimate is

$$\Delta r_{is1} = \delta_0 + \delta_1 Y_{i(s-1)} + \delta_2 T_{i(s-1)} + \delta_3 Y_{j(s-1)} + \delta_4 T_{j(s-1)} + \epsilon_{is1} \tag{3}$$

where $Y_{j(s-1)}$ is previous partner j income and $T_{j(s-1)}$ is previous partner search duration, both measured in sequence $s-1$.

Results from estimating (3) are reported in Table 5. In the top panel, we report estimates which use total sequence earnings as a measure of partner income, and in the bottom panel we report estimates which use exit offer as a measure of partner income.

The estimated constant is always positive because, on average, subjects increase their period 1 reservation wage across sequences, as shown in Figure 4. Note also that the constant is larger in the Partner and Partner-Offer treatment. There is some evidence that a subject's income in the previous sequence causes them to choose a lower reservation wage in the current sequence. This effect, measured by δ_1 is always negative, but the significance of the effect varies across treatments and according to the measure of income used. We find more consistent evidence that longer search duration in the previous sequence causes subjects to choose a lower reservation wage in the current sequence. This effect, measured by δ_2, is significant in all treatments and for both measures of income.

Table 5: Change in reservation wage between sequences as a function of previous partner's income and search length

	(1) Isolated	(2) Partner	(3) Partner-Offer
(a) Income = total sequence earnings (000s points)			
δ_0 Constant	25.447***	33.672**	44.428***
	(6.846)	(15.353)	(15.077)
δ_1 $Y_{i(s-1)}$	−0.050	−2.122	−1.016*
	(0.061)	(1.555)	(0.573)
δ_2 $T_{i(s-1)}$	−6.786***	−14.618***	−17.985***
	(0.503)	(2.225)	(2.711)
δ_3 $Y_{j(s-1)}$	−0.021	1.948	0.869*
	(0.285)	(1.369)	(0.528)
δ_4 $T_{j(s-1)}$	−0.717	7.976*	5.118**
	(2.673)	(4.078)	(2.011)
(b) Income = exit offer (points)			
δ_0 Constant	21.923	66.483	85.815**
	(17.851)	(59.067)	(37.363)
δ_1 $Y_{i(s-1)}$	−0.070***	−0.102	−0.059***
	(0.016)	(0.068)	(0.022)
δ_2 $T_{i(s-1)}$	−5.215***	−12.603***	−16.928***
	(0.505)	(1.460)	(2.433)
δ_3 $Y_{j(s-1)}$	0.002	0.052**	−0.005
	(0.023)	(0.026)	(0.033)
δ_4 $T_{j(s-1)}$	−1.065	6.718*	4.830**
	(2.665)	(3.953)	(2.205)
Subject random effects	Yes	Yes	Yes
Number of obs.	583	580	595
Number of sessions	6	6	6

Notes: the dependent variable is the change in period 1 reservation wage between sequence s and sequence $s-1$. The social feedback variables are standardized to have zero mean and unit standard deviation. Standard errors in parentheses are clustered at the session level. In the Isolated treatments, results are based on 10,000 replications each with a random pairing of partners. The pairing affects the number of observations because whether or not feedback is available in a particular period depends on whether the partner has already exited. We report the average number of observations across all replications.

Of more interest are the effects of previous partner income and search duration given by the parameters δ_3 and δ_4. As expected, δ_3 and δ_4 are insignificantly different from zero in the Isolated treatment where no information on partners is available. We find only mixed evidence that previous partner income matters for the choice of reservation wage in the current sequence. When income is measured as total sequence earnings in panel (a), the effect is positive, small and not consistently significantly different from zero. When income is measured as exit offer in panel (b), the effect is significant and positive in only the Partner Treatment. The estimated effect of previous partner search duration is more clear-cut. In both the Partner and Partner-Offer treatments δ_4 is estimated to be positive and significant in both panels. In other words, a subject who learns that their partner in the last sequence searched for longer increases their reservation wage by more in the next sequence.

5 Discussion and Conclusions

It is widely recognized that social interaction matters for outcomes in labor markets. However, the existing literature has tended to focus on network effects, whereby social networks provide additional options and information about those options. In this paper we use an experimental setting to investigate an alternative channel for social interactions to influence search: social comparison effects.

Our experiment employs a search task based on McCall's (1970) canonical model of labor market search, which offers predictions for optimal search decisions in a known search environment. Similar search tasks have been examined in the earlier experimental literature. To implement the stationary infinite-horizon decision-making problem, we use a random termination method whereby subjects search with an uncertain time horizon. In this setting, the continuation probability plays a similar role to the discount factor, and the strategy that maximizes expected earnings is the same constant reservation wage that maximizes the expected discounted utility in the infinite-horizon model.

In the treatment where subjects search in isolation, the findings from the previous literature are replicated in our search task with random termination. Subjects tend to set lower reservation wages than is consistent with maximizing expected earnings, and they tend to decrease reservation wages over the course of a search task.

We then consider how social comparisons affect search behavior. We conduct two treatments in which subjects are paired and complete independent search tasks. During the task they receive information about their partner's search, but importantly their decisions have no direct effect on their partner's offer process. Thus our experiment excludes possible network effects.

In one treatment we allow subjects to observe a partner's income at the end of each period. This information is available both during search and after offer acceptance; these two states are analogous to the search and employment spells in a job-seeker's labor market experience. In another treatment we also allow subjects to observe offers received by a partner. This in addition to the partner's acceptance/rejection decision gives additional information about the partner's reservation wage. Such social comparisons may affect search due to distributional preferences or learning effects.

In these novel treatments we show that social comparisons have a real impact on search decisions. In particular, we find that when a subject observes their partner exit with a low wage they tend to decrease their reservation wage by more than they otherwise would, and if they observe their partner exit with a high wage they tend to decrease their reservation wage by less than they otherwise would.

This pattern is qualitatively consistent with a reference wage effect driven by distributional preferences. In particular, applying the Fehr-Schmidt model of inequality aversion to our setting, an active searcher's reservation wage increases with the income of a partner over a particular range of parameter values. This finding complements the findings from studies on social comparison effects in risk-taking, where risk-taking is influenced by the earnings of others in a manner consistent with aversion to earning less than others. We also find evidence of social comparison effects across repeated search sequences, in that subjects tend to choose higher reservation wages when they observe their previous partner searching longer.

The social comparison effects we observe imply that job-seekers who search in a social environment will have an increased job-finding rate after their partner has found a job, because their reservation wage declines. Thus, the difference in search duration between partners will be smaller than in the case of two isolated job-seekers. The effects also imply that job-seekers who search in a social environment will have positively correlated wages even when the offer distributions they face are uncorrelated, because the partner's exit wage influences the remaining job-seeker's reservation wage.

These findings suggest that observing others' search outcomes affects search behavior, even absent any network effects. The fact that job-seekers are influenced by social comparisons and the search outcome of others may allow policy makers to use effective low-cost interventions. For example, a community may have chronically high unemployment rates, because job-seekers in that community generally have high reservation wages so that they are seldom exposed to the job search success of others. Presenting them with search outcome of other job-seekers outside the community who have lower reservation wages may induce them to lower reservation wages and increase their job-finding rates. In such a way social comparison effects may be harnessed to alleviate community-level unemployment.[18]

[18] Altmann et al. (2018) find, in a large-scale field experiment, that a low-cost intervention in the form of a brochure containing information about job search and unemployment can have positive effects on job-seekers' employment and wages.

Two obvious questions that arise from this study are: first, whether the social comparison effects we observe are relevant to real job-search settings, and second, whether the effects we observe have implications for other search settings. We note that the social interaction in our experiment is very limited; the subjects in our various treatments experience a very similar environment, the only difference being limited to information about an anonymous other. In real job search environments we expect social interaction to be much richer. We see no reason to suppose that social comparison effects would be weaker in the richer environment. This question could be further explored in future research which varies the form of social interaction. We hypothesize that with richer interaction, for example allowing communication between subjects, we would observe stronger social comparison effects. On the second question, we note that the sequential search model may also apply to other settings such as consumer product search, flat-hunting and even searching for partners. In all these settings, the decisions of peers may allow for social comparisons which affect the decision to accept or reject a particular offer. We leave such issues to further research.

References

Abeler, J., Altmann, S., Kube, S. & Wibral, M. (2010), 'Gift exchange and workers' fairness concerns: When equality is unfair', *Journal of the European Economic Association* **8**(6), 1299–1324.

Akerlof, G. A. & Yellen, J. L. (1990), 'The fair wage-effort hypothesis and unemployment', *Quarterly Journal of Economics* **105**(2), 255–283.

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

Angelova, V., Güth, W. & Kocher, M. G. (2012), 'Co-employment of permanently and temporarily employed agents', *Labour Economics* **19**(1), 48–58.

Bandura, A. & Walters, R. H. (1977), *Social learning theory*, Prentice-Hall Englewood Cliffs, NJ.

Braunstein, Y. M. & Schotter, A. (1982), 'Labour market search: an experimental study', *Economic Inquiry* **20**(1), 133–144.

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

Brown, M., Setren, E. & Topa, G. (2016), 'Do informal referrals lead to better matches? evidence from a firm's employee referral system', *Journal of Labor Economics* **34**(1), 161–209.

Bursztyn, L., Ederer, F., Ferman, B. & Yuchtman, N. (2014), 'Understanding mechanisms underlying peer effects: Evidence from a field experiment on financial decision', *Econometrica* **82**, 1273–1301.

Cai, H., Chen, Y. & Fang, H. (2009), 'Observational learning: Evidence from a randomized natural field experiment', *American Economic Review* **99**(3), 864–82.

Cai, J., De Janvry, A. & Sadoulet, E. (2015), 'Social networks and the decision to insure', *American Economic Journal: Applied Economics* **7**(2), 81–108.

Calvó-Armengol, A. & Jackson, M. O. (2004), 'The effects of social networks on employment and inequality', *American Economic Review* **94**(3), 426–454.

Cappellari, L. & Tatsiramos, K. (2015), 'With a little help from my friends? Quality of social networks, job finding and job match quality', *European Economic Review* **78**, 55–75.

Card, D., Mas, A., Moretti, E. & Saez, E. (2012), 'Inequality at work: The effect of peer salaries on job satisfaction', *American Economic Review* **102**(6), 2981–3003.

Cingano, F. & Rosolia, A. (2012), 'People I know: Job search and social networks', *Journal of Labor Economics* **30**(2), 291–332.

Clark, A. E. (2003), 'Unemployment as a social norm: Psychological evidence from panel data', *Journal of Labor Economics* **21**(2), 323–351.

Clark, A. E., Masclet, D. & Villeval, M. C. (2010), 'Effort and comparison income: Experimental and survey evidence', *ILR Review* **63**(3), 407–426.

Clark, A. E. & Oswald, A. J. (1996), 'Satisfaction and comparison income', *Journal of Public Economics* **61**(3), 359 – 381.

Cohn, A., Fehr, E., Herrmann, B. & Schneider, F. (2014), 'Social comparison and effort provision: Evidence from a field experiment', *Journal of the European Economic Association* **12**(4), 877–898.

Conley, T. G. & Udry, C. R. (2010), 'Learning about a new technology: Pineapple in Ghana', *American Economic Review* **100**(1), 35–69.

Cox, J. & Oaxaca, R. (2000), 'Good news and bad news: Search from unknown wage offer distributions', *Experimental Economics* **2**, 197–225.

Dal Bó, P. & Fréchette, G. R. (2017), 'On the determinants of cooperation in infinitely repeated games: A survey', *Journal of Economic Literature* **forthcoming**.

Dustmann, C., Glitz, A. & Schönberg, U. (2016), 'Referral-based job search networks', *Review of Economic Studies* **83**(2), 514–546.

Falk, A., Huffman, D. & Sunde, U. (2009*a*), Do I have what it takes? Equilibrium search with type uncertainty and non-participation. IZA Discussion Paper No. 2531.

Falk, A., Huffman, D. & Sunde, U. (2009*b*), Self-confidence and search. IZA Discussion Paper No. 2525.

Fehr, E. & Schmidt, K. M. (1999), 'A theory of fairness, competition, and cooperation', *Quarterly Journal of Economics* **114**, 817–868.

Festinger, L. (1954), 'A theory of social comparison processes', *Human Relations* **7**(2), 117–140.

Fischbacher, U. (2007), 'z-Tree: Zurich toolbox for ready-made economic experiments', *Experimental Economics* **10**(2), 171–178.

Frank, R. H. (1984), 'Are workers paid their marginal products?', *American Economic Review* **74**(4), 549–571.

Frank, R. H. (1985), *Choosing the right pond: Human behavior and the quest for status.*, New York: Oxford University Press.

Friedl, A., De Miranda, K. L. & Schmidt, U. (2014), 'Insurance demand and social comparison: An experimental analysis', *Journal of Risk and Uncertainty* **48**(2), 97–109.

Gächter, S., Nosenzo, D. & Sefton, M. (2012), 'The impact of social comparisons on reciprocity', *Scandinavian Journal of Economics* **114**(4), 1346–1367.

Gächter, S., Nosenzo, D. & Sefton, M. (2013), 'Peer effects in pro-social behavior: Social norms or social preferences?', *Journal of the European Economic Association* **11**(3), 548–573.

Galenianos, M. (2013), 'Learning about match quality and the use of referrals', *Review of Economic Dynamics* **16**(4), 668–690.

Gamba, A., Manzoni, E. & Stanca, L. (2017), 'Social comparison and risk taking behavior', *Theory and Decision* **82**(2), 221–248.

Greiner, B. (2015), 'Subject pool recruitment procedures: organizing experiments with ORSEE', *Journal of the Economic Science Association* **1**(1), 114–125.

Haisley, E., Mostafa, R. & Loewenstein, G. (2008), 'Subjective relative income and lottery ticket purchases', *Journal of Behavioral Decision Making* **21**(3), 283–295.

Harrison, G. W. & Morgan, P. (1990), 'Search intensity in experiments', *Economic Journal* **100**, 478–486.

Hey, J. D. (1981), 'Are optimal search rules reasonable? And vice versa?(And does it matter anyway?)', *Journal of Economic Behavior & Organization* **2**(1), 47–70.

Hey, J. D. (1982), 'Search for rules for search', *Journal of Economic Behavior & Organization* **3**(1), 65–81.

Hey, J. D. (1987), 'Still searching', *Journal of Economic Behavior & Organization* **8**(1), 137–144.

Horváth, G. (2014), 'Occupational mismatch and social networks', *Journal of Economic Behavior & Organization* **106**, 442–468.

Ibanez, M., Czermak, S. & Sutter, M. (2009), 'Searching for a better deal–on the influence of group decision making, time pressure and gender on search behavior', *Journal of Economic Psychology* **30**(1), 1–10.

Ioannides, Y. M. & Loury, L. D. (2004), 'Job information networks, neighborhood effects, and inequality', *Journal of Economic Literature* **42**(4), 1056–1093.

Kogut, C. A. (1990), 'Consumer search behavior and sunk costs', *Journal of Economic Behavior & Organization* **14**(3), 381–392.

Lahno, A. M. & Serra-Garcia, M. (2015), 'Peer effects in risk taking: Envy or conformity?', *Journal of Risk and Uncertainty* **50**(1), 73–95.

Linde, J. & Sonnemans, J. (2012), 'Social comparison and risky choices', *Journal of Risk and Uncertainty* **44**, 45–72.

Loewenstein, G. F., Thompson, L. & Bazerman, M. H. (1989), 'Social utility and decision making in interpersonal contexts', *Journal of Personality and Social psychology* **57**(3), 426.

Manski, C. F. (2000), 'Economic analysis of social interactions', *Journal of Economic Perspectives* **14**, 115–136.

Marmaros, D. & Sacerdote, B. (2002), 'Peer and social networks in job search', *European Economic Review* **46**, 870–879.

McCall, J. J. (1970), 'Economics of information and job search', *Quarterly Journal of Economics* **84**(1), 113–126.

McGee, A. & McGee, P. (2016), 'Search, effort, and locus of control', *Journal of Economic Behavior & Organization* **126**, 89 – 101.

Moon, P. & Martin, A. (1990), 'Better heuristics for economic search: Experimental and simulation evidence', *Journal of Behavioral Decision Making* **3**(3), 175–193.

Moon, P. & Martin, A. (1992), 'Purchasing decisions, partial knowledge, and economic search experimental and simulation evidence', *Journal of Behavioral Decision Making* **5**(4), 253–266.

Moon, P. & Martin, A. (1996), 'The search for consistency in economic search', *Journal of Economic Behavior & Organization* **29**, 311–321.

Oaxaca, R. L. & Cox, J. C. (1989), 'Laboratory experiments with a finite horizon job search model', *Journal of Risk and Uncertainty* **2**, 301–329.

Oaxaca, R. L. & Cox, J. C. (1992), 'Direct test of the reservation wage property', *Economic Journal* **102**, 1423–1432.

Rohde, I. M. T. & Rohde, K. I. M. (2011), 'Risk attitudes in a social context', *Journal of Risk and Uncertainty* **43**(3), 205–225.

Schmidt, U., Friedl, A. & Lima de Miranda, K. (2015), Social comparison and gender differences in risk taking, Technical report, Kiel Working Paper.

Schotter, A. & Braunstein, Y. M. (1981), 'Economic search: an experimental study', *Economic Inquiry* **19**, 1–25.

Schunk, D. (2009), 'Behavioral heterogeneity in dynamic search situations: Theory and experimental evidence', *Journal of Economic Dynamics & Control* **33**, 1719–1738.

Schunk, D. & Winter, J. (2009), 'The relationship between risk attitudes and heuristics in search tasks: A laboratory experiment', *Journal of Economic Behavior & Organization* **71**(2), 347–360.

Schwerter, F. (2019), Social reference points and risk taking, Technical report, Mimeo, University of Cologne.

Soetevent, A. R. & Bruzikas, T. (2016), Risk and loss aversion, price uncertainty and the implications for consumer search. Tinbergen Institute Discussion Paper 16-049/VII.

Sonnemans, J. (1998), 'Strategies of search', *Journal of Economic Behavior & Organization* **35**, 309–332.

Sonnemans, J. (2000), 'Decisions and strategies in a sequential search experiment', *Journal of Economic Psychology* **21**(1), 91–102.

Stepner, M. (2013), 'BINSCATTER: Stata module to generate binned scatterplots', Statistical Software Components, Boston College Department of Economics.

Zajonc, R. B. (1965), 'Social facilitation', *Science* **149**(3681), 269–274.

Appendix A Experimental Instructions

[*] *indicates differences across treatments.*

Welcome! You are about to take part in a decision-making experiment. For participating in this experiment you will earn a show-up fee of £5. You can earn additional money depending on your decisions, so it is important that you read these instructions with care.

Please switch off your mobile phones. Communication with other participants is prohibited during the experiment. If you violate this rule, you will be dismissed from the experiment and you will forfeit all payments. If you have any questions, please raise your hand. A member of the experiment team will come and answer them in private.

The experiment will consist of 10 sequences. In each sequence you can earn points. After the completion of the experiment, the computer will randomly draw one sequence and all participants will receive additional earnings determined by the points they earned in that sequence. The points earned in that sequence will be converted to Pounds at the following rate:

$$3{,}000 \text{ points} = 1 \text{ Pound}$$

At the end of the experiment this amount plus your show-up fee of £5 will be paid to you privately in cash.

Description of a sequence

Each of the 10 sequences of the experiment has the same structure. Each sequence consists of multiple periods. Your total earnings from a sequence will be the sum of points you earn over the periods.

You will not know exactly how many periods there will be in a sequence until the sequence ends. The number of periods is determined by the computer following a simple rule: after each period, there is a 5% chance that the sequence ends and a 95% chance the sequence continues.

In period one, the computer will randomly draw an offer from 1 to 1000 points. Every offer is equally likely. Before you see the offer, you will be asked to choose a minimum offer that you would be willing to accept. The computer will then accept the offer for you if, and only if, it is above or equal to the minimum value you chose.

- If the offer is accepted, you earn that amount in period one and in every future period of the sequence.
- If the offer is rejected, your earnings from period one will be 0 and, if the sequence continues, you will receive a new offer in period two.

In period two, if you accepted the offer in period one you do not have to make a decision. Your earnings for the period will be equal to the offer you accepted in period one. If you rejected the offer in period one, you will again be asked for the minimum offer you would accept, and you will receive a new offer, which again will be accepted if and only if it exceeds your minimum acceptable offer.

- If the offer is accepted you earn that amount in period two and in every future period of the sequence.
- If the offer is rejected, your earnings from period two will be 0 and, if the sequence continues, you will receive a new offer in period three.

This process will continue until the sequence ends. Remember there is a 95% chance that the sequence continues, and a 5% chance that it ends, at the end of any period. At the end of the sequence your earnings for the sequence will be the sum of your earnings from each period in the sequence.

As long as you have not accepted an offer in the sequence, if the sequence continues to the next period you will get a new offer. The new offer will be drawn independently from the previous offers. This means in every period the offers from 1 to 1000 are equally likely regardless of what offers were made previously.

In each period where you have to enter a minimum acceptable offer you are required to enter it within 15 seconds. If you do not enter a minimum acceptable offer before the time limit is reached, the computer will automatically reject the offer in that period. The following examples illustrate how earnings are calculated.

Example 1:
The sequence lasts for 14 periods. A participant's choice and the offers they draw are listed as follows:

Period	1	2	···	14
Minimum acceptable offer	243	-	···	-
Offer	675	-	···	-
Period Income	675	675	···	675

In Period 1, the offer of 675 is drawn and exceeds the chosen minimum acceptable offer, 243. Because this sequence lasts for 14 periods, the participant's earning from the sequence is 675*14 = 9,450 points.

Example 2:
The sequence lasts for 31 periods. A participant's choices and the offers they draw are listed as follows:

Period	1	2	3	4	5	6	7	···	31
Minimum acceptable offer	455	770	61	531	916	380	-	···	-
Offer	279	208	15	66	106	564	-	···	-
Period Income	0	0	0	0	0	564	564	···	564

In the first five periods none of the offers are accepted because they are less than the minimum acceptable offer. In Period 6, the offer of 564 is drawn and is accepted because it exceeds the minimum acceptable offer of 380. Because there are still 31-5=26 periods remaining in the sequence, the participant's earning from the sequence is 564*(31-5) = 14,664 points.

Example 3:
The sequence lasts for 8 periods. A participant's choices and the offers they draw are listed as follows:

Period	1	2	3	4	5	6	7	8
Minimum acceptable offer	655	818	138	927	700	322	877	427
Offer	525	51	67	823	659	285	525	225
Period Income	0	0	0	0	0	0	0	0

None of the offers are accepted because they are all less than the minimum acceptable offer. Therefore the participant's earning from the sequence is 0 points.

The numbers in these examples have been chosen just for illustrative purposes. In the actual experiment, the computer will determine the number of periods in a sequence (using the rule that at the end of a period there is a 5% chance the sequence stops and a 95% chance the sequence continues). As long as you have not accepted an offer in the sequence, the computer will also draw the offer in each period (each offer from 1 to 1000 is equally likely).

[**Partner and Partner-Offer treatments only:** In each sequence, you will be randomly and anonymously paired with another participant. After the completion of each sequence, participants in the room will be randomly re-paired so the people you are paired with will change from sequence to sequence. In each sequence the person you are paired with is also choosing whether to accept offers. The offers you and that participant receive will be independent. This means you are equally likely to receive any offer from 1 to 1000, regardless of the other person's offer. When the computer determines whether to continue the sequence at the end of the period, it does this for both of you. That is, if the sequence continues, it continues for both of you and if it stops it stops for both of you.]

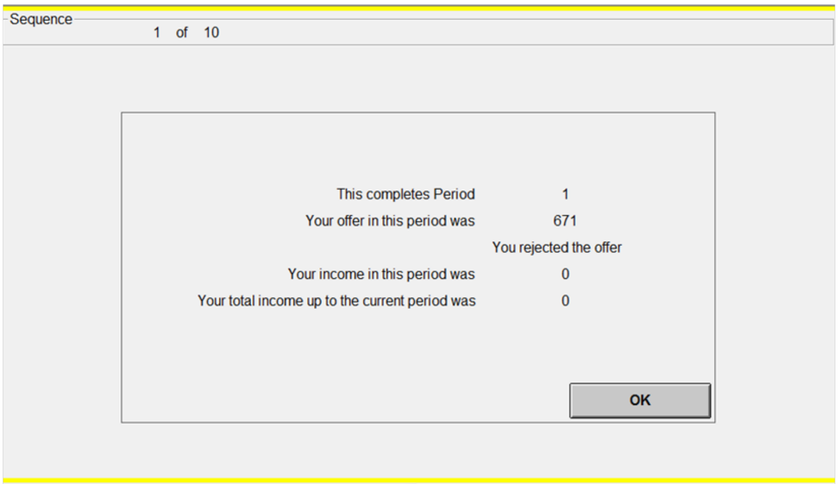
The minimum acceptable offer will be chosen by you in each period before an offer is accepted. [**Isolated treatment only:** At the end of each period, you will observe your offer and whether it is accepted (unless you already accepted an earlier offer). At the end of every period, you will receive feedback on period income. You will also observe your total income up to the period.] [**Partner treatment only:** At the end of every period, both you and the other participant will receive feedback on period income of both you and that participant. Both of you will also be informed of any offers made in that period and whether they were accepted. You will also observe your total income from the sequence up to the period.] [**Partner-Offer treatment only:** At the end of every period, both you and the other participant will receive feedback on period income of both you and that participant. Both of you will also be informed of any offers made in that period and whether they were accepted. You will also observe your total income from the sequence up to the period.] The information will be displayed on the screen in the following layout:

[**Screenshots: Figure A1**]

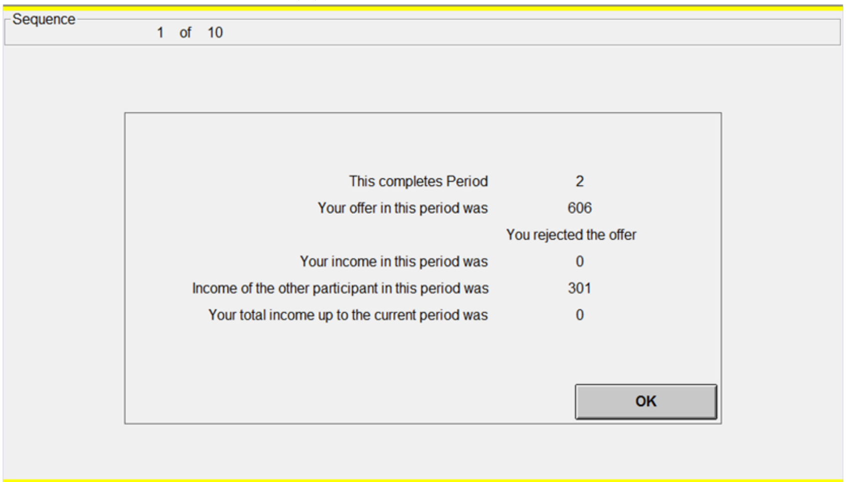
After the completion of each sequence, you will see the total number of periods, and your total income from all the periods in the sequence.

At the end of experiment, one of the 10 sequences will be drawn and the total points you earned in that sequence will be converted to cash as part of your payment.

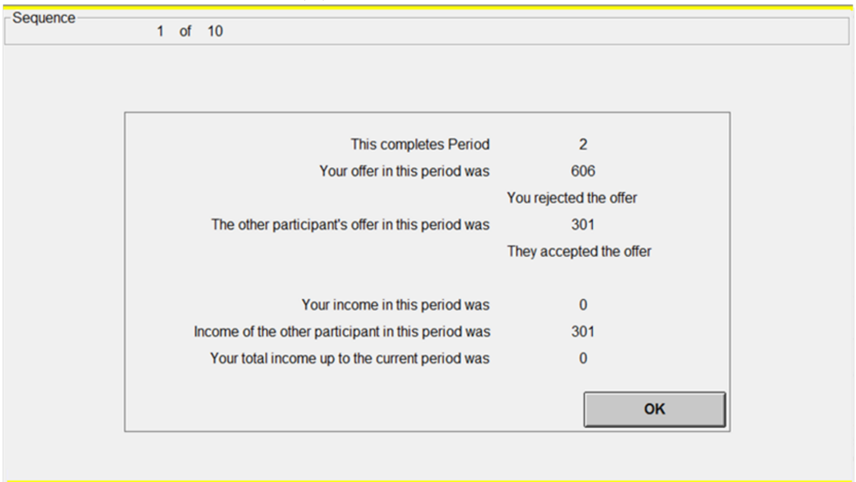
Before we begin the decision-making part of the experiment we will ask some questions to make sure you understand these instructions. Please look at you screen and follow the instructions. If at any time you have a question raise your hand and someone will come to your desk to answer it.



(a) Isolated Treatment



(b) Partner Treatment



(c) Partner-Offer Treatment

Figure A1: Screenshots for period feedback

Control questions

[*] *indicates differences across treatments.*

1. What is the probability that the computer will offer you more than 500 in Period 1 of a round?
- Lower than 50%
- Exactly 50%
- Higher than 50%
(Message if a wrong answer is selected: "The answer you picked is incorrect; the offer is drawn from 1, 2 to 1000 with equal probability, so in each period the probability of getting an offer larger than 500 is exactly 50%.")

2. Suppose in the Period 1 you rejected an offer of 233. What is the probability that the computer will offer you more than 500 in Period 2?
- Lower than 50%
- Exactly 50%
- Higher than 50%
(Message if a wrong answer is selected: "The answer you picked is incorrect; the offers you get are independently drawn, so the probability of getting an offer larger than 500 in Period 2 is exactly 50%.")

3. Suppose you accepted an offer of 879 in Period 1, what is the probability that the round will continue to Period 2?
- Lower than 95%
- Exactly 95%
- Higher than 95%
(Message if a wrong answer is selected: "The answer you picked is incorrect; the probability the round will continue to Period 2 is exactly 95%. At the end of every period there is a 5% chance the round stops and a 95% chance the round continues.")

4. Suppose you rejected an offer of 435 in Period 10, what is the probability that the round will continue to Period 11?
- Lower than 95%
- Exactly 95%
- Higher than 95%
(Message if a wrong answer is selected: "The answer you picked is incorrect; after Period 10, the probability that the round will continue to Period 11 is exactly 95%. At the end of every period there is a 5% chance the round stops and a 95% chance the round continues.")

5. In periods 1-3, the minimum acceptable offer and received offers of a participant are listed in the following table:

Period	1	2	3	⋯
Minimum acceptable offer	891	850	620	⋯
Offer	383	777	643	⋯

The participant has accepted the offer in which period?
- Period 1
- Period 2
- Period 3
(Message if a wrong answer is selected: "The answer you picked is incorrect; the participant has accepted the offer in Period 3, which is the first period that the offer received exceeds the minimum acceptable offer.")

[**Peer and Peer-Offer treatments only:**
6. Suppose in Period 1 your offer is 698, what is the probability that your paired participant's offer is larger than 500 in the same period?
- Lower than 50%
- Exactly 50%
- Higher than 50%
(Message if a wrong answer is selected: "The answer you picked is incorrect; the offers you and your paired participant get are independent, so for them the probability of getting a number larger than 500 is exactly 50%.")]

7. The experiment will consist of 10 sequences. After the completion of the experiment, how many sequence(s) will be drawn to determine the payoffs of the participants?
- Only one
- More than one
- The number of sequences is randomly determined by the computer
(Message if a wrong answer is selected: "The answer you picked is incorrect; only ONE out of 10 sequences will be drawn to determine the payoff of the participants. ")

Appendix B Theoretical Analysis

B.1 Basic setup

We describe a simple job-search model based on McCall (1970). An infinitely-lived job-seeker makes decisions and receives income over a sequence of discrete time periods.

While searching the job-seeker draws one wage offer w per period from a bounded wage distribution $F(w)$. Offers are independently drawn across periods. If the job-seeker rejects the offer their income is zero in that period and they continue search. If the job-seeker accepts the offer, they exit search and receive w for the current and all future periods. In the baseline case we assume that the period utility is

$$u_t = y_t, \text{ where } y_t = \begin{cases} w \text{ if offer } w \text{ has been drawn and accepted,} \\ 0 \text{ otherwise.} \end{cases}$$

We assume the job-seeker maximizes the expected discounted sum of period utilities:

$$\sum_{t=0}^{\infty} \delta^t E[u_t], \quad 0 < \delta < 1$$

Since the environment is stationary, the optimal strategy can be expressed as a constant reservation wage r, i.e. in any period, the job-seeker accepts if offered at least the reservation wage, and continues to search otherwise. The job-seeker's objective function can be written as a function of the reservation wage, $V(r)$. With probability $F(r)$ the job-seeker rejects the offer, earns zero in the current period and faces an identical decision problem in the next period (and hence gets $\delta V(r)$). With probability $1 - F(r)$ the job-seeker accepts the offer and earns the offered amount in all future periods. Thus,

$$V(r) = F(r)(0 + \delta V(r)) + (1 - F(r))\frac{1}{1-\delta}E[w|w \geq r],$$

which can be re-arranged as

$$V(r) = \frac{1}{(1 - \delta F(r))} \int_{w>r} \frac{w}{1-\delta}\, dF(w). \tag{1}$$

For a given offer distribution this can be used to solve the optimization problem directly. For ease of exposition, in the remainder of the discussion we assume that the wage offer follows a uniform distribution over $[0, 1]$, $F(w) = w$. Then (1) is maximized with a unique r over $[0, 1]$,

$$r^* = \frac{1 - \sqrt{1 - \delta^2}}{\delta}. \tag{2}$$

For a discount factor $\delta = 0.95$, this implies an optimal reservation wage of $r^* = 0.724$. For our experiment with wage offers following a discrete uniform distribution on $\{1, \cdots, 1000\}$, the optimal reservation wage is 725.

B.2 Distributional preferences

Now suppose that there are two job-seekers, indexed by $i \in \{1, 2\}$, who are "partners" in the sense that they perform the same sequential search task simultaneously, with wage offers being independent draws across job-seekers and periods. The job-seekers are informed of each other's income at the end of each period. We assume that the job-seekers make interpersonal comparisons in each period of search. Specifically, following Fehr & Schmidt (1999), we assume that i dislikes income differences, as represented by the period utility function (where y_{it} denotes i's period t income and y_{jt} denotes the period t income of i's partner)

$$u_{it}(y_{it}, y_{jt}) = y_{it} - \alpha\big(\max\{y_{it}, y_{jt}\} - y_{it}\big) - \beta\big(\max\{y_{it}, y_{jt}\} - y_{jt}\big) \tag{3}$$

The assumptions made in Fehr & Schmidt (1999) are that $0 \leq \beta \leq 1$ and $\alpha \geq \beta$. We retain the assumption that utility is evaluated each period, including the distaste for inequality, and i seeks to maximize

$$\sum_{t=0}^{\infty} \delta^t E[u_{it}], \quad 0 < \delta < 1 \tag{4}$$

This extends the individual choice problem of Section B.1 to a game.

Our solution concept is that of Markov Perfect Equilibrium. A Markov strategy for job-seeker i is $(r_{i0}, r_i(y))$, an initial reservation wage r_{i0} that governs i's acceptance decision when both partners are active job-seekers, and a reservation wage function $r_i(y)$ that governs i's acceptance decision when she is actively searching but the partner has already accepted an offer and obtains period income y. A Markov Perfect Equilibrium is a pair of Markov strategies $(r^*_{i0}, r^*_i(y))$, $i = 1, 2$, such that,

1. For each i, $r^*_i(y)$ maximizes (4) given partner j is inactive with period income y.
2. For each i, r^*_{i0} maximizes (4) given partner j is active using strategy $(r^*_{j0}, r^*_j(y))$, and i will revert to $r^*_i(y)$ if j exits with period income y.

We start by calculating $r^*_i(y)$ and then find r^*_{i0} using backward induction.

B.2.1 Optimal reservation wage for active job-seeker after partner exits

Consider the case that i is still searching and j has exited with a wage (and hence a per-period income) of $y_{jt} = y$. Again the dynamic problem facing i is stationary, and so the optimal reservation wage is constant.

Using a constant reservation wage r_i, i's expected discounted utility beginning from a period in which j is inactive with period income y is denoted by $V_A(r_i; y)$. With probability r_i the offer is rejected $(w < r_i)$, which gives the current period utility of $-\alpha y$ and a discounted future value of $\delta V_A(r_i; y)$, and with probability $1 - r_i$, the offer is accepted $(w \geq r_i)$, which yields a constant period utility in all future periods. Thus,

$$V_A(r_i; y) = r_i(u_i(0, y) + \delta V_A(r_i; y)) + (1 - r_i)\frac{1}{1 - \delta}E[u_i(w, y)|w \geq r_i],$$

and rearranging gives

$$V_A(r_i; y) = \frac{1}{1 - \delta r_i}\Bigg(r_i u_i(0, y) + \frac{1}{1 - \delta}\int_{r_i}^{1} u_i(w, y)\ dw\Bigg).$$

With u_i given by (1),

$$V_A(r_i; y) = \frac{1}{1 - \delta r_i}\Bigg(- r_i\alpha y + \frac{1}{1 - \delta}\bigg(\int_{r_i}^{k}(w - \alpha(y - w))\ dw + \int_{k}^{1}(w - \beta(w - y))\ dw\bigg)\Bigg),$$

where $k \equiv \max\{r_i, y\}$.

This is maximized by $r_i^*(y)$,

$$r_i^*(y) = \max\left\{\frac{1 - \sqrt{1 - \delta^2 + 2\delta(1 - \delta)\ \frac{\alpha+\beta}{1-\beta}y}}{\delta}, \frac{1 - \sqrt{1 - \delta^2 + \delta^2\ \frac{\alpha+\beta}{1+\alpha}(1 - y)^2}}{\delta}\right\}. \tag{5}$$

Equation (5) describes the optimal reservation wage for a job-seeker (with given α, β and δ) after their partner has exited search with period income y. When $\alpha = \beta = 0$ this reduces to (2) and the reservation wage is independent of the partner's income. For other values of α, β, $0 < r_i^*(y) \leq r^*$. For $y = 0$ or 1 the optimal reservation wage is r^*. For $y < \hat{y}$, $r_i^*(y)$ is decreasing in y. For $y > \hat{y}$, $r_i^*(y)$ is increasing in y.

We use $V_A(y)$ to denote the value of the game to i beginning from a period in which she is active but j has previously accepted an offer and receives period income y. That is,

$$V_A(y) = V_A(r_i^*(y); y). \tag{6}$$

B.2.2 Optimal initial reservation wage when both job-seekers are active

Now we move on to find the equilibrium initial reservation wage. Job-seeker i's expected discounted utility beginning from a period in which both job-seekers are active is a function of their initial reservation wages $V_0(r_{i0}, r_{j0})$. The period has four possible outcomes: neither of the job-seekers accept their offers, i accepts and j rejects, j accepts and i rejects, or both of the job-seekers accept. Thus $V_0(r_{i0}, r_{j0})$ can be written as the sum of four components, each regarding the payoff from one of the outcomes:

$$
\begin{aligned}
V_0(r_{i0}, r_{j0}) = &\int_0^{r_{j0}} \int_0^{r_{i0}} \big(0 + \delta V_0(r_{i0}, r_{j0})\big)\ dy_j dy_i \\
&+ \int_0^{r_{j0}} \int_{r_{i0}}^1 \big(u_i(y_i, 0) + \delta V_I(y_i)\big)\ dy_i dy_j \\
&+ \int_0^{r_{i0}} \int_{r_{j0}}^1 \big(u_i(0, y_j) + \delta V_A(y_j)\big)\ dy_j dy_i \\
&+ \frac{1}{1-\delta} \int_{r_{i0}}^1 \int_{r_{j0}}^1 u_i(y_i, y_j)\ dy_j dy_i
\end{aligned}
$$

When neither accepts, i receives current period utility of zero and in the next period begins a subgame which is identical to the original game. When i accepts and j rejects, i's current period utility is $u_i(y_i, 0)$, and we denote by $V_I(y_i)$ (subscript I for "inactive") the value of the game to i beginning from a period in which she is inactive with period income y_i and the partner is active. When i rejects and j accepts and earns period income y_j, i's current period utility is $u_i(0, y_j)$, and the value of the game to i from the beginning of the next period is $V_A(y_j)$. Finally, when both i and j accept their offers in the current period i gets utility $u_i(y_i, y_j)$ in this and every future period.

Rearranging and using (1) gives

$$
\begin{aligned}
V_0(r_{i0}; r_{j0}) = &\frac{1}{1-\delta r_{i0} r_{j0}} \Bigg(r_{j0} \int_{r_{i0}}^1 \big((1-\beta) y_i + \delta V_I(y_i)\big)\ dy_i + r_{i0} \int_{r_{j0}}^1 \big(-\alpha y_j + \delta V_A(y_j)\big) dy_j \\
&+ \frac{1}{1-\delta} \int_{r_{i0}}^1 \int_{r_{j0}}^1 \big(y_i - \alpha\big(\max\{y_i, y_j\} - y_i\big) - \beta\big(\max\{y_i, y_j\} - y_j\big)\big)\ dy_j dy_i \Bigg),
\end{aligned}
\tag{7}
$$

Note that $V_I(y_i)$ depends on both job-seeker's preferences (and i's belief about j's preferences), so identifying the equilibrium initial reservation wage requires assumptions about both job-seekers' preferences and what job-seekers know about each other's preferences. For tractability, we assume job-seekers have identical preferences and this is common knowledge.

If i is inactive with period income y_i we assume j uses the optimal reservation wage $r_j^*(y_i)$. Then,

$$
V_I(y_i) = r_j^*(y_i)\big(u_i(y_i, 0) + \delta V_I(y_i)\big) + (1 - r_j^*(y_i)) \frac{1}{1-\delta} E[u_i(y_i, w) | w \geq r_j^*(y_i)],
$$

which is rearranged as

$$
V_I(y_i) = \frac{1}{1 - \delta r_j^*(y_i)} \Bigg(r_j^*(y_i) u_i(y_i, 0) + \frac{1}{1-\delta} \int_{r_j^*(y_i)} u_i(y_i, w)\ dw \Bigg).
$$

Given (1), it is convenient to denote $l \equiv \max\{r_i^*(y_i), y_i\}$, so we have

$$V_I(y_i) = \frac{1}{1-\delta r_i^*(y_i)}\Bigg(r_i^*(y_i)(1-\beta)y_i \\ + \frac{1}{1-\delta}\big((1-r_i^*(y_i))y_i - \alpha\int_l^1 (w-y_i)dw - \beta\int_{r_i^*(y_i)}^l (y_i - w)dw\big)\Bigg). \tag{8}$$

Combining (5), (6), (7) and (8), we complete the initial objective function V_0 for i. We conduct a calibration to find the optimal response function for i given j's choice, $r_{i0}^*(r_{j0})$. We calibrate r_{i0}^* for all (α, β), $\alpha \in [0, 2)$, $\beta \in [0, 1)$, and $\alpha > \beta$ with grid equal to 0.01. The results are accurate to the third decimal place.

The simulation shows that the equilibrium r_{i0}^* is close to the expected own-earning-maximizing r^*. For the majority of (α, β), r_0^* is slightly below r^*, although for small α and small β it is slightly above r^* (Figure B1a).

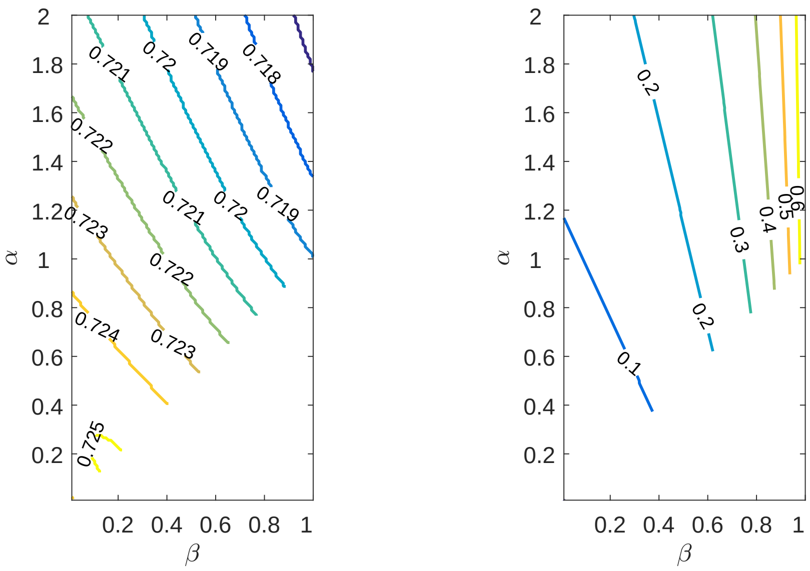
The initial equilibrium reservation wage r_{i0}^* lies above the kink point of the $r_i^*(y)$ function, $\hat{y}$. This implies that a job-seeker never accepts a wage below $\hat{y}$ (Figure B1b) and so in equilibrium only the positively-sloped part of $r_i^*(\cdot)$ is relevant in Figure 1.

B.2.3 Summary and implications

After a partner exits, the model predicts that the remaining job-seeker selects a constant reservation wage that depends on the partner's income (see Figure 1). Since the initial reservation wage lies above the kink point, only the upward sloping portion is relevant in equilibrium. Therefore the model predicts a positive relationship between the wage accepted by the partner and the reservation wage of the remaining job-seeker. In most cases, the change in reservation wage in response to partner's exit is negative, but the size of the change is moderated by the exit offer of the partner. Note that although we only report the calibration with standard Fehr-Schmidt assumptions, this qualitative prediction can be extended to negative βs, as long as $-\alpha < \beta$.

These results have three practical implications for job search outcomes. First, the probability that a job-seeker exits in period t depends positively on whether their partner exited in period $t-1$. In our setting, the probability of exiting is $1 - F(r) = 1 - r$, which changes from $(1 - r_0^*)$ before the partner exits to $(1 - r^*(y))$ after the partner exits with y. The expected change is $r_0^* - E[r^*(y)|y \geq r_0^*]$. Before the partner exits, the probability of exiting is about 28%, and the expected increase in the probability of exiting after partner exits varies from about 3% for high values of α and β to 1% for low values. The expected changes for five sets of (α, β) are reported in column (1) of Table B1.

Second, there is a "spillover" from the partner's exit wage to the remaining job-seeker's reservation wage. A job-seeker whose partner has exited with a high wage has a higher reservation wage than a job-seeker whose partner has exited with a low wage. Given



(a) Initial equilibrium reservation wage r_0^*

(b) Comparison of r_0^* with kink-point: $r_0^* - \hat{y}$

Figure B1: Equilibrium initial reservation wage for different Fehr-Schmidt parameters

Notes: $\beta \in [0, 1)$, $\alpha \in [0, 2)$, $\alpha \geq \beta$; $r^* = 0.724$. The equilibrium r_{i0}^* lies between 0.717 and 0.726 for all (α, β). In the calibrated range of (α, β), r_{i0}^* takes its minimum at $(1.99, 0.91)$ and maximum at $(0.24, 0.06)$. $(r_{i0}^* - \hat{y})$ is always positive, and takes its minimum of 0.003 at $(0.01, 0.01)$ and maximum of 0.687 at $(1.99, 0.99)$.

an initial reservation wage of r_0^*, the exit wage varies uniformly between r_0^* and 1. The reservation wage of the active job-seeker then varies from $r^*(r_0^*)$ to $r^*(1)$. The difference between $r^*(1)$ and $r^*(r_0^*)$ varies from 0.10 for high α and β to 0.04 for low α and β. Column (2) of Table B1 reports the slope of the linearized relationship between the partner's exit wage and the remaining job-seeker's reservation wage, which shows that a unit increase in y leads to a linearized increase in r^* of 0.35 for high and 0.13 for low α and β.

Third, this implies a correlation of wages across partners. By definition, independent job-seekers' wages are uncorrelated. In contrast, inequity averse job-seekers who are partners will have positively correlated wages, if they exit in sequence. The correlation is 0.13 for high α and β and 0.05 for low α and β (column(3) of Table B1).

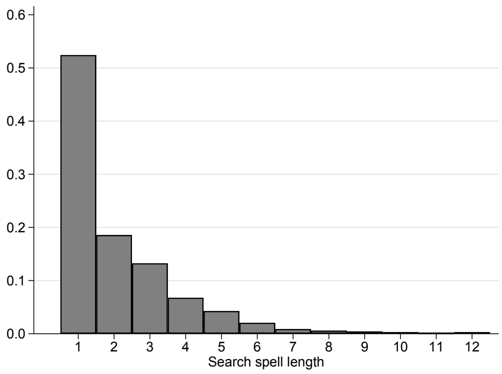
Table B1: Quantifying the implications for search outcomes

$(\alpha, \ \beta)$	(1) Expected change in prob. of exiting when j exits: $r_0^* - E[r(y) \mid y \geq r_0^*]$	(2) Linearized relationship between. $r^*(y)$ and y, $y \geq r_0^*$: $\frac{r^*(1)-r^*(r_0^*)}{1-r_0^*}$	(3) Correlation between exit wages: $\rho(y_1, y_2)^{a}$
(0, 0)	0.000	0.000	0.000
(0.5, 0.0)	0.013	0.132	0.046
(0.5, 0.5)	0.024	0.253	0.100
(0.9, 0.6)	0.027	0.297	0.110
(1.0, 0.8)	0.029	0.336	0.129
(2.0, 0.8)	0.028	0.349	0.130

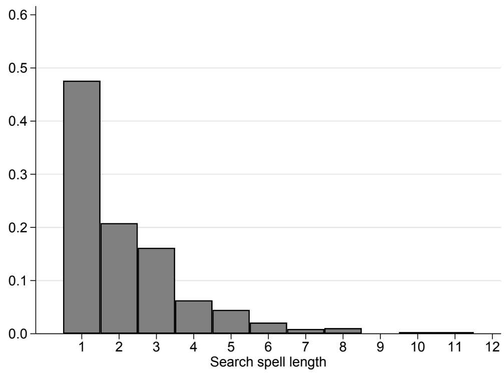
[a] Based on calibration of 100,000 pairs.

Appendix C Additional descriptives

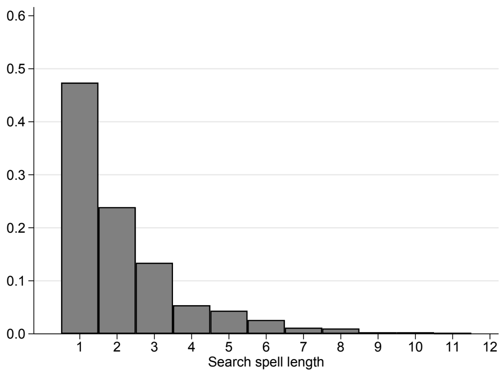
Figure C1: Distribution of search spell length by treatment



(a) Isolated treatment



(b) Partner treatment



(c) Partner-Offer treatment

Notes: 125 search spells which are censored by the end of the sequence are excluded.

Table C1: Sample for analysing within-sequence reference wage effects

	(1) Isolated	(2) Partner	(3) Partner-Offer	Total
Total	1,576	1,588	1,585	4,749
Excluded observations:				
First period	720	720	720	2,160
Timed out	1	1	6	8
Partner exited	353	289	268	910
Sample	502	578	591	1,671

Table C2: Sample for analysing between-sequence learning effects

	(1) Isolated	(2) Partner	(3) Partner-Offer	Total
Total	720	720	720	2,160
Excluded observations:				
First sequence	72	72	72	216
Timed out	1	0	3	4
Censored (i or j)	66	68	50	184
Sample	581	580	595	1,756

Appendix D Additional results

Table D1: Alternative specifications for Table 4

	(1) Isolated			(2) Partner			(3) Partner-Offer		
β_1 j accepted offer (a_j)	−5.577	−6.759	−8.913	−71.029**	−69.019*	−71.662**	−58.712***	−61.161***	−59.495***
	(22.585)	(22.234)	(22.803)	(33.712)	(38.390)	(35.521)	(17.223)	(16.297)	(19.022)
β_2 j's income ($a_j o_j$)	0.007	0.009	0.010	0.094**	0.088*	0.092*	0.083***	0.082***	0.083***
	(0.034)	(0.033)	(0.034)	(0.047)	(0.052)	(0.049)	(0.025)	(0.022)	(0.029)
β_3 value of j's offer (o_j)	0.002	−0.001	0.000	−0.008	−0.001	−0.006	−0.002	−0.001	−0.002
	(0.020)	(0.019)	(0.019)	(0.022)	(0.022)	(0.023)	(0.012)	(0.011)	(0.012)
Subject random effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period fixed effects	No	No	Yes	No	No	Yes	No	No	Yes
Sequence fixed effects	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Subject characteristics	No	No	Yes	No	No	Yes	No	No	Yes
Number of obs.	510	510	510	578	578	578	591	591	591
Number of sessions	6	6	6	6	6	6	6	6	6

Table D2: Estimates of the sequence trends

	(1) Period 1 (r_{is1})	(2)	(3) Exit period (r_{isL})	(4)
Isolated	3.733	3.732	4.078***	4.083***
	(2.555)	(2.559)	(1.293)	(1.294)
Partner	9.538***	9.540***	12.926***	12.904***
	(1.883)	(1.886)	(2.235)	(2.239)
Partner-Offer	6.333**	6.328**	7.663***	7.659***
	(2.875)	(2.877)	(2.463)	(2.466)
p-values for				
Partner vs. Isolated	0.067	0.068	0.001	0.001
Partner-Offer vs. Isolated	0.499	0.500	0.197	0.199
Partner vs. Partner-Offer	0.351	0.350	0.113	0.115
Subject random effects	Yes	Yes	Yes	Yes
Subject characteristics	No	Yes	No	Yes
Number of obs.	2,155	2,155	2,035	2,035

Notes: The sequence trends for each treatment are estimated using the following model:
$r_{ist} = \beta_0 + \beta_1 \text{Partner}_i + \beta_2 \text{Partner-Offer}_i + \beta_3 s + \beta_4 s \times \text{Partner}_i + \beta_5 s \times \text{Partner-Offer}_i + \eta_i + \epsilon_{ist}$,
where t is period 1 or the last period. The sample for column (1) and (2) excludes 5 sequences in which the subject timed out for the initial period. The sample for column (3) and (4) excludes 135 censored sequences.