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## Job history, work attitude, and employability

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# Job History, Work Attitude, and Employability

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April 5, 2016

## Abstract

We study whether employment history can provide information about a worker’s non-cognitive skills—in particular, about “work attitude,” or the ability to work well and cooperatively with others. We conjecture that, holding all else equal, a worker’s frequent job changes can indicate poorer work attitude, and that this information is transmitted in labor markets through employment histories. We provide support for this hypothesis across three studies that employ complementary lab, field, and survey experiments. First, a laboratory labor market, in which the only valuable characteristic of workers is their reliability in cooperating with an employer’s effort requests, demonstrates that prior employment information allows employers to screen for such reliability and allows high-reliability workers to obtain better employment outcomes. Second, we conduct a field experiment that varies the frequency of job changes in fictitious job applicants’ resumes. Those applicants with fewer job changes receive substantially more callbacks from prospective employers. Finally, a survey experiment with human resource professionals confirms that the resume manipulations in the field study create different perceptions of work attitude and that these account for the callback differences. Our work highlights the potential importance of job history as a signal of worker characteristics, and points to a cost for workers of frequent job changes.

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# 1. Introduction

While traditional accounts of human capital mainly emphasize the importance of cognitive or physical skills (e.g., Becker 1964), more recent research highlights the relevance of alternative, non-cognitive, social and behavioral skills for labor market success (Bowles, Gintis, and Osborne 2001; Heckman, Stixrud, and Urzua 2006) and argues that the labor market increasingly rewards such skills (Deming 2015). These skills involve, for example, a worker’s reliability, trustworthiness, self-control, loyalty, and ability to work well with others (e.g., Heckman and Rubinstein 2001; Dohmen et al. 2009; Lindqvist and Vestman 2011). For simplicity, we refer to this broad set of characteristics as “work attitude.” A central idea of this literature is that workers who exhibit a positive work attitude are more desirable to employers and obtain better labor market outcomes. Indeed, many employers rate workers’ “attitude” as an important determinant of hiring decisions and note “poor attitude, motivation or personality” as a reason why they forgo hiring applicants for open positions (Green, Machin, and Wilkinson 1998; Bowles, Gintis, and Osborne 2001). For example, Herb Kelleher, founder and former CEO of Southwest Airlines, described his company’s hiring philosophy as: “We look for attitudes. We’ll train you on whatever you need to do, but the one thing we can’t do is change inherent attitudes in people” (Lee 1994).

An important open question, however, is how information regarding work attitude is conveyed in labor markets. At the recruitment stage, direct information on work attitude is rarely available and prospective employers have to rely on less direct signals contained in the typical employment application.<sup>1</sup> One piece of observable and typically verifiable information in most employment applications is work history—what positions an applicant has previously held, at which firms, and for how long.

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<sup>1</sup>Referrals by existing employees (Rees 1966; Pallais 2014; Burks et al. 2015) and social networks (Granovetter 1974; Gërxhani, Brandts, and Schram 2013) may provide mechanisms through which employers can obtain information about prospective workers’ abilities, including work attitude. In ongoing employment relations information on workers’ work attitude may be obtained through direct observation of their workplace behavior (Bartling, Fehr, and Schmidt 2012).

Might such information provide a signal of a prospective employee’s work attitude?<sup>2</sup>

In this paper we propose that employers will often view frequent job changes as potentially reflective of poor work attitude. In turn, employers will, *ceteris paribus*, find workers who change jobs frequently less desirable in contexts where work attitude is important.<sup>3</sup> Our conjecture thus ascribes a potentially powerful role to employment histories—a widely available type of information in labor markets—as a signal of desirable labor market qualities.

Why should applicants’ job histories convey information about their non-cognitive skills? Most employment relationships require a worker to follow directions from supervisors, cooperate and get along well with others, show loyalty and reliability, and exhibit self-control in pursuing long-term goals at the expense of short-term inclinations. Hence, employees who do these things are often more valuable to an employer and less likely to quit jobs due to personal conflicts. On the other hand, workers who fail to exhibit positive work attitude are more likely to experience workplace conflicts and either leave or be terminated.

Analysis of data from the National Longitudinal Survey of Youth 1997 (NLSY97) reveals significant relationships between measures of work attitude and the number of jobs an individual has held in her career. The NLSY97 is a large, nationally representative panel of young Americans, covering a wide range of jobs and industries in the US labor market. Controlling for various individual covariates, we find that people who are more likely to break rules, have been arrested by the police, and drink at work switch jobs significantly more often. Moreover, the personality trait

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<sup>2</sup>Publicly observable histories also form the basis of an extensive literature on screening and signaling in labor markets (Spence 1973; Arrow 1973; Stiglitz 1975; Waldman 1984). This literature has typically focused on productive skills or human capital—e.g., educational attainment as a signal of cognitive abilities that may facilitate learning and performing work-related tasks (Tyler, Murnane, and Willett 2000; Bedard 2001).

<sup>3</sup>The popular business press often recognizes that frequent job changes can be associated with perceptions of “disloyalty, fickleness and unreliability” (Trikha 2012; Suster 2010). Others have noted that workers are heterogeneous in their propensity to remain with specific employers, and that this corresponds to stable individual characteristics (Ghiselli 1974; Blumen, Kogan, and McCarthy 1955).

conscientiousness is negatively associated with the number of job changes.<sup>4</sup>

The NLSY97 data further suggest that changing jobs more frequently is associated with an increased likelihood of being unemployed. Figure 1 shows coefficient estimates from regressions of current unemployment status on the number of previous jobs for different sub-populations.<sup>5</sup> Fixing the other regressors at their means, a change from holding three to eight jobs since the age of 20 (25th vs. 75th percentile) more than doubles the probability of being currently unemployed (2.2 vs. 4.6 percent). This relationship is robust when considering, separately, different sub-populations—such as women or men, younger or older people, individuals with low or high GPA, and people from urban or rural areas. The NLSY97 data also reveal a negative relationship between the number of previous jobs and current income from wages and salaries (see Appendix A). Hence, the perils of frequent job switching for employment outcomes seem to be a broad phenomenon that applies across many demographic groups, occupations, and industries.

While this correlational analysis is suggestive of our hypothesized relationship, potentially unobservable variables do not allow for a causal interpretation of the links between work attitude, job changes, and employability. Moreover, in the field, there are many possible reasons for either a positive or negative relationship between job mobility and employability, making it challenging to isolate the particular effect that is our focus.<sup>6</sup> Therefore, in what follows, we provide direct experimental tests of our hypothesized relationship between job changes, work attitude and

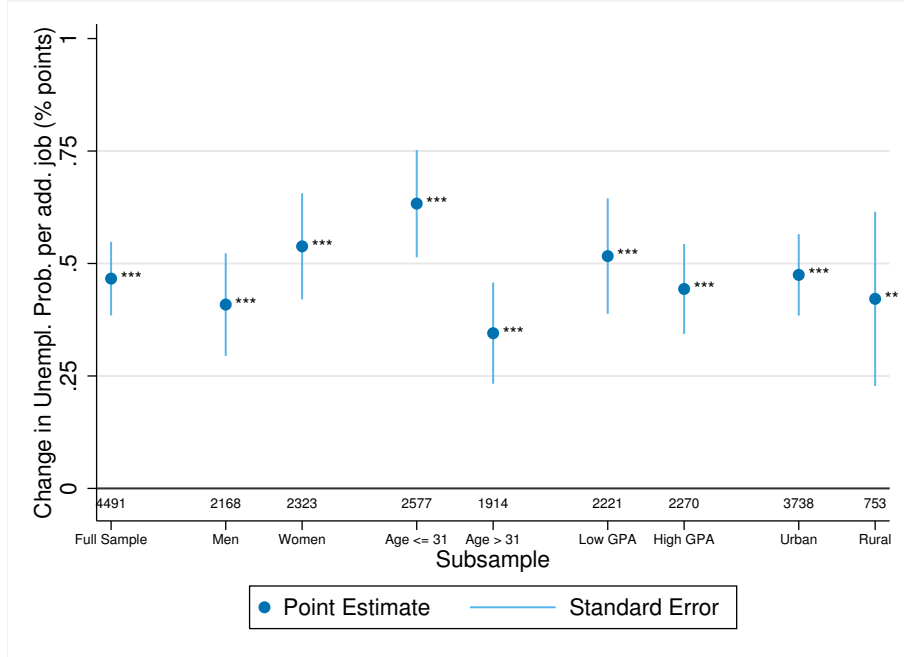
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<sup>4</sup>Details of the regression analysis are provided in Appendix A. We controlled for a number of demographic, geographical, and educational covariates as well as previous unemployment and adjusted significance tests for multiple hypothesis testing. We found no significant relationships between the number of previous jobs and emotional stability (neuroticism) as well as being a hard-working person. By contrast, the number of jobs is positively related to extraversion, agreeableness, and being open to new experiences.

<sup>5</sup>For further details of this analysis see Appendix A.

<sup>6</sup>For example, workers who switch employers more often may accumulate a larger stock of general human capital—that is, skills and knowledge that are useful across jobs, firms and industries (Mincer 1958; Becker 1962). If firms use workers’ job histories as an indicator of these general skills and knowledge, this could lead to a positive relationship between job changes and employability. Moreover, the reasons behind job changes are undoubtedly important for subsequent labor market outcomes (Jovanovic 1979; Topel and Ward 1992), and job mobility may have differential impacts at different points in a worker’s career (Bartel 1980; Mincer and Jovanovic 1982; Farber 1999).

**Figure 1:** Relationship between number of previous jobs and current unemployment status



Coefficients from OLS regressions of current unemployment status (as of the last interview wave in 2013/14) on the number of past jobs (since the age of 20), controlling for past unemployment, highest academic degree, high-school GPA, age, gender, ethnicity, geographical region, urban/rural area, and month in which the interview was conducted. Each dot represents a separate regression, corresponding to the sub-population indicated on the horizontal axis. The numbers at the bottom indicate the size of the respective subsample. The stars next to the dots indicate significance with \*\*\*  $p < 0.01$  and \*\*  $p < 0.05$ .

employability. Importantly, we do not claim to provide a comprehensive interpretation of tenure-wage-employment relationships. Rather, we propose one particular mechanism through which employment history can impact subsequent labor market outcomes, and present empirical evidence that controls, as much as possible, for alternative mechanisms and explanations for such a relationship.

Specifically, we provide evidence for employment history as a signal of work attitude using three empirical tests that employ complementary laboratory, field, and survey experiments. Table 1 provides an overview of the different approaches and how they complement each other. We test our first hypothesis—that frequent job changes provide a signal of poor work attitude—in both the laboratory, where we study whether there is a negative relationship between job changes and work at-

titude, and using a survey experiment in the field, where we test whether human resource professionals perceive candidates who change jobs more frequently as having poorer work attitude. To test our second hypothesis—that employers prefer workers with fewer employment changes—we study whether workers with fewer job changes receive more job offers in the laboratory setting and more interview requests in the field experiment. We find support for both hypotheses, in the lab as well as in the field. While the field experiment in real labor markets provides the most compelling evidence of the economic significance of our findings, the lab and survey studies deliver the clearest insights into the precise mechanisms driving the relationship between job changes and employment outcomes.<sup>7</sup>

**Table 1:** Research design

	Laboratory	Field
H1: Frequent job changes are negatively correlated with work attitude	<b>Study 1:</b> Lab Experiment	<b>Study 3:</b> Survey Experiment
H2: Where work attitude matters, employers find fewer job changes desirable		<b>Study 2:</b> Field Experiment

The laboratory environment allows us to isolate work attitude from other possible channels through which a relationship between past and future employment might occur. Our experiment eliminates heterogeneity in workers’ skills or experience as factors affecting employability. Firms value workers to the extent they exhibit positive work attitude. Other potentially confounding characteristics of workers and jobs—such as firm-specific capital, training and recruitment costs—are absent from our laboratory setting. Effort cost is equal for all workers and therefore independent of any idiosyncratic ability, and it does not vary with experience. However, workers with a greater tendency to provide voluntary effort are more valuable to firms, meaning that firms can benefit by using informative signals regarding work attitude, and should favor contracting with more reliable and cooperative workers.

<sup>7</sup>Frequent job changes can also signal other undesirable worker characteristics, such as lack of task specific skills (Gibbons and Katz 1991). We abstract from this dimension in our laboratory experiment, and address it directly in our survey experiment.



Our laboratory results show that, first, workers who switch employers less frequently tend to be those who exert higher effort. Second, following an exogenous unemployment shock that requires all workers to find new employers, job histories facilitate the signaling of positive work attitude—workers with fewer job changes receive more job offers and earn greater income than those who have switched jobs more often. Finally, by turning off the ability of firms to observe work histories we show that this information is crucial in firms’ attempts to identify reliable workers. Hence, our results clearly demonstrate that frequent job changes can serve as a signal of negative work attitude and influence employability.

We then test whether the phenomenon we identify in the laboratory is also relevant for real labor markets. We report a field experiment that studies whether frequent job changes make prospective employees less desirable to firms. Specifically, we sent resumes to several open positions for administrative and clerical work. The resumes varied, by random assignment, the applicants’ job history.<sup>8</sup> For every open position in our study, we sent two applications: one with four shorter periods of tenure at different firms, and one with a single similar combined period of tenure at one firm. We counterbalanced other aspects of the resumes. In two waves of data collection, we observe significantly higher callback rates for the applicant with fewer job changes. That is, workers who change jobs more frequently are less desirable in the field study, just as they are in our laboratory experiment. Our result is robust to variations in economic environment, industry, and job characteristics. Moreover, the size of the effect we observe in the field experiment is substantial—the differences in callback rates for the applicants with one versus four prior employers is similar in magnitude to differences for applicants with one versus eight months of unemployment (Kroft, Lange, and Notowidigdo 2013) and to differences between white and black applicants (Bertrand and Mullainathan 2004).

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<sup>8</sup>Many studies used a similar methodology to test for other aspects of job-market discrimination (Riach and Rich 2002; Bertrand and Mullainathan 2004; Carlsson and Rooth 2007; Oberholzer-Gee 2008; Kroft, Lange, and Notowidigdo 2013; Eriksson and Rooth 2014; Deming et al., forthcoming; Bartos et al., forthcoming).

Finally, we conducted a third study to obtain more precise information on the inference that prospective employers make when receiving the resumes in the field study. Specifically, we surveyed professionals with experience in human resources (HR) management to obtain their impressions of the resumes used in the field study. The results show that HR professionals attribute a less positive work attitude to a resume with more frequent job changes—specifically, worse evaluations for the characteristics “reliable”, “team oriented”, and “patient.” Moreover, perceived work attitude largely explains the HR professionals’ greater stated willingness to invite applicants with fewer job changes for an interview. Thus, the survey experiment provides evidence confirming that the resumes in the field study create different perceptions of applicants’ work attitudes, and that these perceptions are important drivers of callbacks.

Our evidence that employers discriminate against frequent job changes may have implications that go beyond the value of work history as a signal of work attitude. For instance, workers may be unwilling to undertake job changes out of fear of the negative impact on future prospective employers’ perception of work attitude. Indeed, the popular business press regularly warns against the perils of job hopping and provides suggestions for how to manage the associated negative perceptions.<sup>9</sup> This inertia or friction in job mobility may create inefficient matching between employees and employers. Labor market frictions are a key feature of modern search theory in macroeconomics because they provide potential explanations for the existence of unemployment and wage inequality (e.g., Petrongolo and Pissarides 2001; Rogerson, Shimer, and Wright 2005). Previous work has focused primarily on structural factors for why workers may refuse a job offer and wait for a more attractive one, such as how quickly they can sell their houses (Head and Lloyd-Ellis 2012). Our paper adds to this literature by proposing a mechanism for labor market frictions that arises endogenously, through employers’ preference for workers with a positive work

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<sup>9</sup>See, for example, Green (2013) and Levinson (2009)

attitude and the limited information available to employers on this characteristic.

Our study is further related to a large empirical literature studying the relationship between job mobility and wage growth. While some of these studies find that mobility and wage growth are positively related (Topel and Ward 1992; Becker and Hills 1983; Bartel 1980), others find a negative relationship (Light and McGarry 1998; Mincer and Jovanovic 1982; Borjas 1981). Our paper contributes to this literature, by studying the impacts of exogenous variations in job mobility. We provide one possible mechanism through which prior job mobility may affect future outcomes, though our focus is on employability rather than wages.<sup>10</sup>

The rest of this paper is structured as follows. The next section presents the design and results of our laboratory experiment. Sections 3 and 4 present, respectively, the field study and the connected survey study of HR professionals. Finally, in Section 5 we provide a broad interpretation of the combined results and conclude.

## 2. Laboratory Experiment

Our laboratory experiment uses a setting in which a worker’s productivity for a firm is determined by her work attitude (i.e., reliability and cooperativeness) and where skills are irrelevant. Specifically, we employ a widely used experimental labor market paradigm in which incomplete contracts create incentives for inefficient shirking by workers.<sup>11</sup> Workers are valuable to firms if they act cooperatively and reliably by voluntarily providing high effort in response to high wages. To study whether employers use employment histories as a signal of this behavioral quality, we exogenously manipulate whether employers have access to workers’ job histories. We additionally induce an unemployment shock, following which all workers must

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<sup>10</sup>A separate strand of literature explores how job tenure with a particular firm relates to wage profiles (Dustmann and Meghir 2005; Altonji, Smith, and Vidangos 2013; Bagger et al. 2014). This is distinct from our study because we focus on job tenure solely for its signaling purposes when changing jobs between firms.

<sup>11</sup>Our laboratory experiment builds upon Brown, Falk, and Fehr (2004), modifying their design to address our research questions.

search for new employers, in order to identify which types of employees firms find most desirable.

## 2.1. Experimental Design

Each experimental labor market consists of 17 participants, of which seven are randomly assigned the role of a firm; the remaining ten participants are assigned the role of a worker. Each participant is identifiable through a permanent ID number. The experiment lasts 30 periods. In any given period, each firm can hire at most one worker and each worker can work for at most one firm. Because labor supply exceeds labor demand, in each period some workers are unemployed.

Every period is divided into two stages: a hiring stage and a work stage. In the hiring stage, firms can post two kinds of offers: *i*) public wage offers, which any worker can accept, and *ii*) private wage offers, which are targeted to specific workers. Each offer contains a binding wage,  $w \in \{1, 2, \dots, 100\}$ , and a desired effort level,  $\hat{e}$ . A worker can accept any public offer or any private offer directed to her. A private offer is thus a clear indication that a firm has a preference for one particular worker. At the end of the hiring stage, up to seven firms and workers are matched in an employment relationship.

The second stage is the work stage, in which employed workers decide on the actual effort level they provide. To eliminate heterogeneity in skills, subjects do not perform a work task but instead simply choose a numerical effort level,  $e \in \{1, 2, \dots, 10\}$ , which implies monetary costs according to a standard effort cost schedule,  $c(e)$  (see Table 2).<sup>12</sup> The employer earns 10 ECU per unit of worker effort  $e$  but also has to pay the wage  $w$ :  $\pi_{\text{firm}} = 10e - w$ .<sup>13</sup> The worker's payoff from employment is equal to the wage minus the effort costs:  $\pi_{\text{worker}} = w - c(e)$ . Unemployed workers receive  $\pi_{\text{unempl}} = 5$ ; firms without a worker receive a payoff of zero in that period.

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<sup>12</sup>This is a standard approach in experimental labor markets. Brüggemann and Strobel (2007) show that such numerical effort choices produce similar behavior as real effort decisions.

<sup>13</sup>All payoffs are denoted in "Experimental Currency Units" (ECU) that were converted into

**Table 2:** Workers’ effort cost

$e$	1	2	3	4	5	6	7	8	9	10
$c(e)$	0	1	2	4	6	8	10	12	15	18

While aggregate payoffs are maximized if workers provide maximum effort, the worker’s monetary incentive—in the absence of repeated-game incentives—is to shirk and provide minimal effort. Effort in this context is thus a one-dimensional proxy for the voluntary provision of costly, but productive effort at work—i.e., a measure of an employee’s cooperativeness, reliability, and diligence.

To study the role of work histories as a signal of work attitude, we experimentally vary whether workers’ employment histories are available to firms. In the “History” condition, each firm sees a table on the computer screen listing all ten workers in the labor market, sorted by their ID number. The table indicates, for all previous periods, either the ID of the firm that hired the worker in that period or whether that worker was unemployed.<sup>14</sup> However, the table does not show workers’ effort or wages, only the firm for which they worked (see Appendix B). This provides prospective employers with a simple version of the employment histories typically contained in job applications, including job changes and spells of unemployment. By contrast, the job history table is absent in the “No History” condition.

Our two hypotheses are that work histories provide a signal of work attitude and that firms use this signal when deciding which workers to employ. Specifically, we expect that workers who provide higher voluntary effort will tend to be those who remain longer with the same employer. In addition, when employment histories are available, we expect that firms will use this information to make private offers preferentially to workers with fewer prior job changes. By contrast, if the number of previous employers is not diagnostic of future effort choices or firms do not appreciate

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Swiss Francs at a rate of 20 ECU = 1 CHF ( $\approx$  1.06 USD) at the end of the experiment.

<sup>14</sup>If the worker was unemployed in a particular period, the cell is filled with a dash. Workers could see a similar table that listed the firms by their ID number and listed which workers worked for a particular firm across periods.

the signaling value of previous job changes, then we should not observe that the availability of employment histories affects labor market outcomes.

To investigate whether firms use employment histories as a means to screen for high-effort workers, we implement an exogenous layoff shock that forces all firms to seek a new worker. From period 17 onwards, we remove the option for firms to make private offers to the worker they had hired in period 16, and we remove the option for workers to see or accept public offers from the firm they had worked for in period 16. This change is permanent, meaning that no market participant is allowed to interact with their partner from period 16 in any of the remaining periods. This shock introduces an exogenous layoff, which requires all workers to search for new employment opportunities.<sup>15</sup> This design feature allows us to investigate which workers firms find desirable in a context where all workers are simultaneously—and for exogenous reasons—searching for new employment. Yet, firms are able to evaluate prospective workers based on their employment histories only in the History condition.<sup>16</sup> The No History condition thus serves as a placebo test, in which we do not expect a relationship between a worker’s number of prior employers and employability.

## Procedures

We conducted the experiment between December 2012 and May 2013, and additional sessions in June 2015, at the Laboratory for Behavioral and Experimental Economics at the University of Zurich. Each session was randomly assigned to one of the two treatment conditions. All interactions between participants took place via the z-Tree computer interface (Fischbacher 2007). Computer stations were separated by partition walls, ensuring anonymity of the participants. The participants received

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<sup>15</sup>Participants did not know that this shock would happen in period 17. They were informed that this restriction would come into effect at some point “between period 10 and period 20.” We did this to rule out that firms would strategically separate from long-term employees in period 16 just to be able to re-hire them in period 17.

<sup>16</sup>Note, however, that in both conditions firms have private information about the workers they had previously employed.

detailed written instructions and then had to complete a comprehension check to make sure that they understood the rules of the experiment (See Appendix B). We read instructions aloud to establish common knowledge.

We recruited a total of 561 participants using the software, h-root (Bock, Baetge, and Nicklisch 2014). Of these, 272 (16 markets) were in the No History condition and 289 (17 markets) were in the History condition. Sessions lasted slightly under two hours, and participants earned an average of 51 Swiss Francs (about 54 US dollars).

## 2.2. Results

### Are work histories an informative signal of work attitude?

Figure 2 depicts the relationship between workers' effort and their employment history during the first 16 periods of the experiment. In the History condition, workers who had a single employer throughout periods 1 to 16 provided an average effort of 9.2, close to the maximum of 10. Average effort decreases with the number of pre-shock employers to a level of 4.7 for workers with six different pre-shock employers ( $p = 0.040$ ; Mann-Whitney-U test [MWU]).<sup>17</sup> Similarly, workers in the No History condition with one employer also exerted higher effort on average than those who changed jobs more frequently (9.2 for one employer vs. 5.9 for six employers;  $p < 0.001$ , MWU). Hence, regardless of whether work histories are available, workers who act more cooperatively and reliably are also those with fewer job changes.

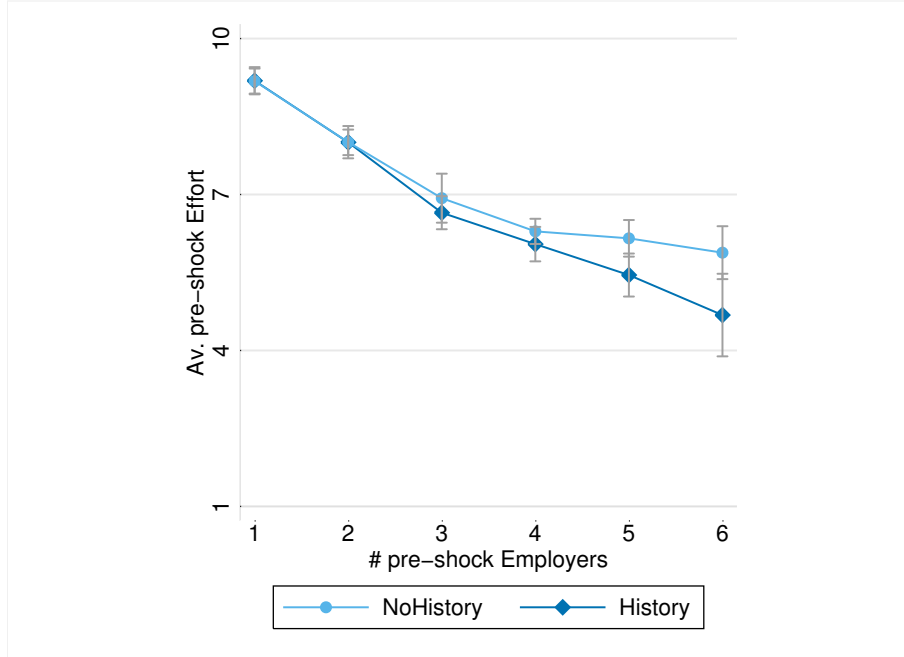
We further examined the relationship between voluntary work effort and the frequency of job changes using Ordinary Least Squares (OLS) regressions. Our analysis is based on the following linear regression model:

$$N_i = \alpha + \beta(e_i - 1) + \varepsilon_{im}. \quad (1)$$

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<sup>17</sup>Since observations are not independent within markets we use a cluster-robust version of the MWU test (see Datta and Satten 2005).

**Figure 2:** Voluntary Effort and Number of Employers



Average effort a worker exerted in periods 1 to 16 in relation to the number of different employers that the worker had during that phase. Unit of observation: worker. Error bars calculated using 1000 bootstrap pseudo-samples, accounting for clustered standard errors on the market level. There is a negative relationship between the effort a worker exerted in periods 1 to 16 and the number of employers she had in that phase.

The dependent variable,  $N_i$ , is the number of employers a worker  $i$  had in the 16 periods before the turnover shock and  $e_i$  is the worker's effort level in periods 1 to 16. We use  $(e_i - 1)$  in the regression model so that the constant,  $\alpha$ , can be interpreted as the number of employers of a worker who provided the minimum effort of 1 before the shock.<sup>18</sup> We allow the error terms,  $\varepsilon_{im}$ , to be correlated within each labor market.

Column 1 in Table 3 reports the regression results for the History treatment. The constant of about 5 indicates that a worker who provided the minimum effort before the shock had, on average, five different employers (out of a maximum of 7) during

<sup>18</sup>Every participant had at least one employer before the shock. We obtain similar results if we use, instead, first-period effort as an explanatory variable or if we control for the number of periods unemployed prior to the shock.



**Table 3:** Regression analysis of number of employers

Condition	(1) History	(2) No History	(3) Pooled
Avg. Effort Periods 1-16	-0.357*** (0.036)	-0.342*** (0.043)	-0.342*** (0.042)
History			-0.276 (0.405)
History X Avg. Effort 1-16			-0.015 (0.055)
Constant	5.051*** (0.295)	5.327*** (0.286)	5.327*** (0.281)
adj. R <sup>2</sup>	0.337	0.243	0.303
N	170	160	330

OLS regressions, standard errors in parentheses, adjusted for clustering at the session level using White sandwich estimators. Unit of observation: workers.

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Dependent variable:* number of different employers before the shock (periods 1 to 16).

*Independent variables:* Constant: average number of pre-shock employers for a worker in the No History condition (History condition for column 1) who provided minimum effort; “Effort Periods 1-16:” effort provided by the worker in periods 1 to 16 (subtracting 1 ( $e_i - 1$ ) to facilitate interpretation of the constant); “History:” dummy for History treatment condition; “History  $\times$  Avg. Effort 1-16:” interaction between History dummy and pre-shock effort.

that time. Increasing first-period effort by one unit is associated with a reduction of the number of pre-shock employers by about 0.36 ( $p < 0.001$ , t-test). We observe a similar pattern in the No History condition, as shown in column 2. Providing minimum effort results in 5.3 pre-shock employers, and increasing effort by one unit reduces the number of pre-shock employers by about 0.34 ( $p < 0.001$ , t-test). In column 3, we pool the data from both treatments and additionally include a dummy for the History treatment as well as its interaction with the number of employers. This allows us to test whether the relationship between effort and number of pre-shock employers is stronger in the History condition. Yet, both the coefficient of the History dummy and the interaction term are insignificant ( $p = 0.501$  and  $0.787$ , t-tests), confirming that the relationship between work attitude and job history is similar in both conditions. Together, these findings support our prediction that workers who change jobs frequently are less reliable and cooperative.<sup>19</sup>

<sup>19</sup>Note that this relationship alone does not tell us the reasons behind job changes—that is, whether a worker left the employer for a better offer elsewhere or whether the current employer did not make another offer to the worker. Our data indicate that job changes tend to be driven by employers. Specifically, in 86% of the cases in which workers changed jobs, they did not receive a private offer from their old employer. On the other hand, 91% of private offers from a worker’s

**Result 1 (Employment history and effort)**

*Frequent job changes are indicative of lower effort provision. This relationship holds for workers in the History condition as well as in the No History condition.*

**Do firms prefer workers with stable employment?**

As we show above, job histories provide valuable information about workers' reliability in providing voluntary effort. Do firms take this information into account when making job offers?

Figure 3 suggests that firms indeed use workers' employment history to screen for high-effort workers. In period 17, workers with one pre-shock employer receive 84% more private offers in the History compared to the No History condition ( $p = 0.007$ , MWU). And while the number of offers drops sharply in the History condition with the number of pre-shock employers, we observe no such trend in the No History condition.

The regression analysis in Table 4 estimates the relationship between the frequency of job changes and employability, while controlling for prior unemployment spells. Specifically, we estimate a regression model using the following equation:

$$y_i = \alpha + \beta_1(N_i - 1) + \beta_2 U_i + \varepsilon_{im}. \quad (2)$$

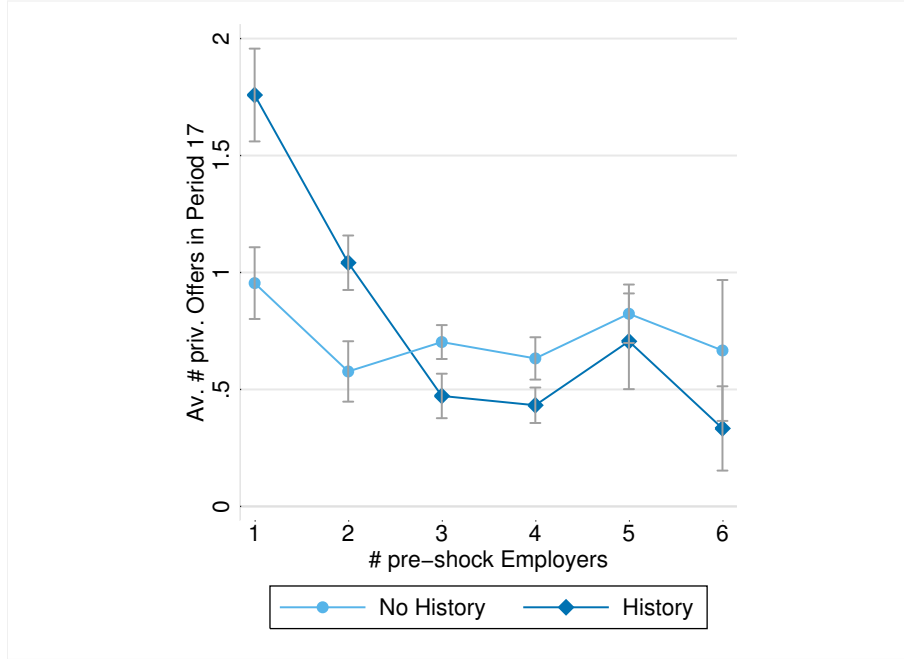
The number of private offers received by an employee in period 17,  $y_i$ , is regressed on the number of pre-shock employers minus 1, i.e.,  $N_i - 1$ . Thus, the constant reflects the number of private offers obtained by a worker with exactly one pre-shock employer.<sup>20</sup> We additionally control for the number of periods unemployed prior to the shock,  $U_i$ , which is also observable for prospective employers in the History condition. Column 1 shows that, controlling for unemployment spells, each additional employer before the shock significantly reduces the number of private

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previous employer are accepted.

<sup>20</sup>No worker was unemployed for the entire 16 pre-shock periods.

**Figure 3:** Private Offers in Period 17



Number of private employment offers that a worker receives from firms at the beginning of period 17 (directly after the shock), as a function of the number of different employers that the worker had before the shock (periods 1 to 16). Unit of observation: worker. Error bars calculated using 1000 bootstrap pseudo-samples, accounting for clustered standard errors on the market level. In the No History condition, where firms have to rely solely on own information from their previous employment relations with workers, there is almost no effect of number of employers. In the History condition, where firms can observe all workers' employment history before the shock, there is a pronounced negative effect of number of previous employers.

offers in period 17 by 0.219 in the History condition ( $p = 0.005$ , t-test). By contrast, column 2 shows that in the No History condition—where information about job changes is private information—the coefficient of the number of additional employers is close to zero and statistically insignificant ( $p = 0.936$ , t-test). In column 3, we pool the observations from both treatments and include a dummy for treatment History as well as the corresponding interaction terms. The results confirm that the coefficient for the number of additional employers differs significantly between the History and the No History conditions ( $p = 0.019$ , t-test).<sup>21</sup>

<sup>21</sup>We also find that every additional period of unemployment reduces the number of private offers by 0.074 ( $p = 0.009$ , t-test) in the History and 0.050 ( $p = 0.023$ , t-test) in the No History condition

**Table 4:** Regression analysis of private offers in Period 17

Condition	(1) History	(2) No History	(3) Pooled
# Employers	-0.219*** (0.068)	-0.005 (0.056)	-0.005 (0.055)
# Periods Unemployed	-0.074*** (0.025)	-0.050** (0.020)	-0.050** (0.020)
History			0.673*** (0.239)
History $\times$ # Employers			-0.215** (0.087)
History $\times$ # Periods Unemployed			-0.024 (0.031)
Constant	1.639*** (0.193)	0.965*** (0.147)	0.965*** (0.145)
$R^2$	0.254	0.047	0.185
N	170	160	330
Clusters	17	16	33

OLS regressions, standard errors in parentheses, adjusted for clustering at the session level, using White sandwich estimators. Unit of observation: worker.

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Dependent variable:* Number of private offers to worker after the shock (period 17).

*Independent variables:* Constant: the baseline is a worker in the (No) History condition who was continuously employed by the same firm for all 16 periods before the shock. “History:” dummy for History treatment condition; “# Employers:” number of additional pre-shock employers; “# Periods Unempl.:\times # Employers:” interaction between History dummy and additional employers; “History  $\times$  # Periods Unemployed:” interaction between History dummy and periods unemployed.

Our experimental design allows us to follow workers for the remaining 14 periods after the unemployment shock (i.e., periods 17 to 30). The “life-time” loss in earnings for workers with unsteady pre-shock job histories in the History condition is quite sizable. Workers with five or six pre-shock employers earned, on average, 261 ECU. By contrast, workers with one or two pre-shock employers earned, on average, 428 ECU after the layoff shock, or about 64% more ( $p < 0.001$ , MWU). In the No History condition, the difference in earnings between these two groups of employees is much smaller (312 ECU vs. 360 ECU,  $p < 0.001$ , MWU).

## Result 2 (Employment history and job outcomes)

*When employment histories are available, workers who were employed by fewer firms*

(see columns 1 and 2). One interpretation for the negative relationship between unemployment and employability in the no History condition is that firms do not make private offers in period 17 to workers they “fired” before the shock. Although firms cannot make private offers to workers they had employed in Period 16, they can re-hire workers they had employed in earlier periods.

*receive more private job offers and achieve higher profits. These relationships are much weaker when employment histories are not available.*

We have seen that, when firms can screen for workers with stable job histories, workers with frequent employment changes incur significant losses; but this screening also has broader labor market implications. For instance, as Figure 4 shows, the availability of employment histories influences the length of employment and unemployment spells. On average, employment relations last longer when job histories are available (2.4 vs. 3.2 periods,  $p = 0.002$ , MWU, see also column 4 in Table 3), and workers also remain unemployed longer (1.9 vs. 2.3 periods,  $p = 0.001$ , MWU). Further analysis reveals that this is driven by both the demand and the supply sides of the labor market. Firms are more likely to make job offers to their current workers in the History than in the No History condition ( $p < 0.001$ , t-test), and workers are more likely to accept job offers from their current employers ( $p = 0.028$ , t-test).<sup>22</sup> Consistent with this decreased mobility, providing employers with employment histories reduces workers' average number of pre-shock employers. If we estimate a version of the models in Table 3 with only the History dummy and a constant we observe that workers have, on average, 0.4 or 12% fewer employers in periods 1 through 16 ( $p = 0.09$ , t-test).<sup>23</sup>

### **Result 3 (Labor market frictions)**

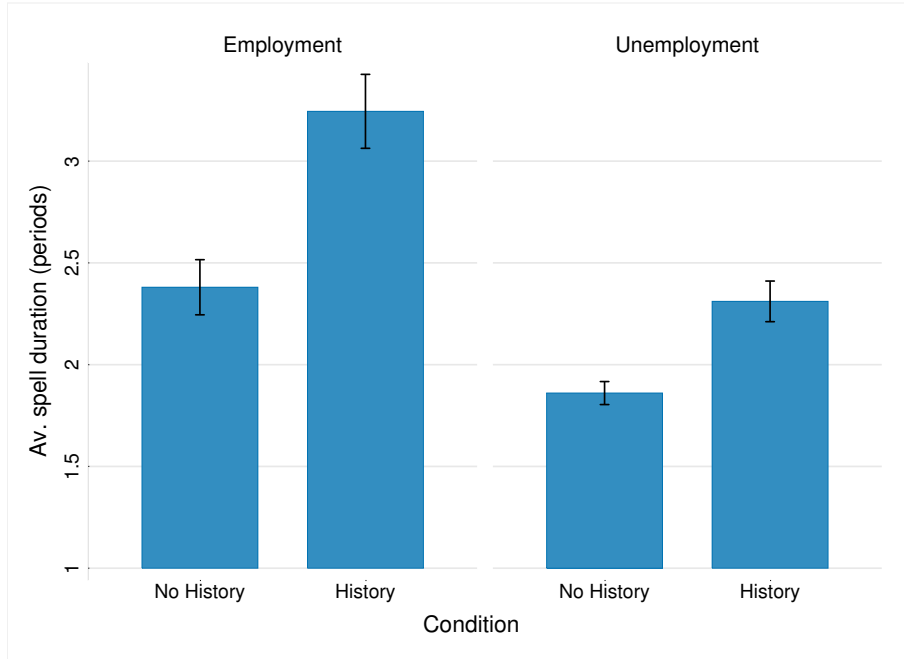
*Employment relationships and unemployment spells last longer and number of employers decreases when job histories are publicly observable.*

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<sup>22</sup>The p-values are based on OLS regressions of the respective variables on a treatment dummy with cluster-robust standard errors at the labor market level.

<sup>23</sup> We also find that observable job histories make private reputation portable: Firms in the History condition are more willing to make offers to workers they had not employed previously. In period 17, only 38% of employed workers had previously worked for that firm in the History condition, while in the No History condition firms hired workers that they had employed before in 68% of cases. OLS regressions confirm that this difference is statistically significant ( $p = 0.006$ , t-test).

**Figure 4: Hysteresis with Job Market History**



Average duration of employment and unemployment spells, in number of periods, across treatment conditions. Unit of observation: market. Left Panel: The average employment spell is about 36 percent longer in History than in No History. Right Panel: The availability of employment histories increases unemployment spells by 24 percent.

### 3. Field Experiment

The results of our laboratory experiment indicate that frequent job changes can provide a negative signal of prospective employees' work attitude. The experiment also shows that laboratory employers use this information, if it is available, to determine which workers to seek out for employment. However, this study leaves open the question of whether similar behavioral patterns exist outside of the laboratory. Therefore, we additionally conducted a field experiment to examine whether workers with fewer job changes in their resume are similarly more desirable to employers in real labor markets.

### **3.1. Experimental design**

Our field experiment employs the well-established correspondence method typically used to study discrimination in the hiring process (e.g., Riach and Rich 2002; Bertrand and Mullainathan 2004; Carlsson and Rooth 2007; Pager, Western, and Bonikowski 2009; Oreopoulos 2011). The method consists of applying to job openings with carefully designed, fictitious applications and then measuring whether the prospective employers call back the applicants for a job interview. In our study, for every open position we sent two applications that were closely matched in terms of educational background, observable skills, and total length of tenure, but which varied in the applicants' job history.

We conducted the field experiment in two waves. The first wave took place between May and June 2012, and the second wave one year later, from April to June 2013. During these periods we sent out a total of 1680 email applications in response to job ads in the German-speaking part of Switzerland (680 in the first wave and 1000 in the second wave). To ensure a sufficient number of job vacancies with similar skill requirements, we follow the majority of correspondence studies and focus on commercial jobs (i.e., administrative and clerical work). Employees in such jobs constitute about 11 percent of Switzerland's total workforce (Swiss Federal Statistical Office 2008).

#### **Creating the applications**

For each wave, we created two male and two female identities for the fictitious job applicants. Each identity was assigned a unique name and portrait photo. To avoid ethnic discrimination, we created names based on a list of the most common Swiss first and family names. The photos were borrowed from students who gave us their permission to use them for the study. To track firms' responses, we assigned each identity a home address, an email address, and a cell phone that automatically

redirected calls to a voice mail box.<sup>24</sup> We took great care to ensure that the resumes were realistic and appealing. To achieve this, we used templates from the Swiss professional association for commercial employees and related websites, and also consulted Human Resources professionals.

Because we always sent two applications per open position, we created two resumes describing virtually identical applicants in all observable characteristics, except for the frequency of job changes. Both applicants were 26 years old and well-qualified, as they had a diploma in commercial studies with high grades. They both had eight years of work experience in exactly the same job functions. To differentiate the two resumes, we described the job functions using different terms (e.g., human resources vs. personnel management) and also changed the order in which the functions appeared on the resumes. Both applicants were currently employed when we sent out the applications. We further gave both applicants a set of complementary qualities that employers typically desire for commercial workers, such as relevant computer skills, as well as good knowledge of the Swiss national languages and English. In order not to raise any suspicions from the employers, we used a different formatting and layout for the two resumes. We counterbalanced the two formatting schemes with treatment assignment.

## **Treatments**

Each resume had a male and a female identity. For each identity we implemented a version with continuous employment at a single firm (“One Employer”) and a version with comparable experience but multiple employers (“Four Employers”). The Four Employers resume signals that the job applicant had moved rather frequently from one employer to the next. After a degree in commercial education, the job applicant

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<sup>24</sup>We used real postal addresses and tagged the letter boxes with the corresponding names in order to collect responses by postal mail. We used different phone lines and different email providers for the two candidates.



made horizontal moves between four firms every twenty to twenty-four months.<sup>25</sup> In contrast, the job applicant with the One Employer resume had spent his or her entire post-education career at the same company. Both resumes exhibited a total of eight years of work experience in exactly the same job functions: administration, accounting, human resources, communication with customers, and purchasing.

Although short breaks between jobs are not unusual, they could affect callback rates because employers may consider them as unemployment spells that could signal low productivity (Oberholzer-Gee 2008; Kroft, Lange, and Notowidigdo 2013; Eriksson and Rooth 2014). We removed the gaps from the Four Employers work history in the second wave. Thus, for a given job opening, the only relevant difference between the two applicants was the number of previous employers.

### **Responding to job ads and measuring callbacks**

Over the two waves of data collection, we surveyed all administrative and clerical job ads posted on four large job search websites. To obtain reasonably high callback rates, we restricted ourselves to job postings that were no older than ten days and that offered a job in the broader area of Zurich or adjacent cantons (i.e., reasonably close to the applicants' home addresses). Our sample of job postings covers a broad spectrum of commercial jobs, including jobs in customer services, sales support, or management assistance.

For each job ad, we sent out two applications—one with a Four Employers resume and the other with a One Employer resume. We randomized which of the two applicants was assigned the Four and One Employer resume, respectively, and then submitted both resumes, in randomized order, a couple of hours apart. Both applicants always had the same gender, which was determined at random, unless an employer explicitly asked for candidates of a specific gender.

We recorded all incoming responses within seven weeks after the submission of

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<sup>25</sup>The companies were chosen from a list of employers that offer commercial positions from a vocational counseling website.

the applications, although in practice the majority of the employers contacted the applicants in the first two weeks. Because we are interested in whether the employers exhibit a preference for an applicant, we define a callback as an explicit request for an interview or a message stating that one of the applicants is shortlisted for interview.<sup>26</sup> Two research assistants who were blind to the experimental conditions coded the responses according to these pre-defined rules. To minimize the inconvenience caused to the employers, we declined interview invitations within 24 hours.<sup>27</sup>

### 3.2. Results

In total, we sent 1,680 applications to 840 job vacancies in a broad range of industries (see Table 7 in the Appendix). Most ads were for jobs in private limited liability companies (87.7%), followed by state owned firms or NGOs (8.8%), and organizations of other legal forms (3.5%, e.g., single proprietors or cooperatives); 75.4 percent of the job ads were for full-time jobs (i.e., at least four days per week).<sup>28</sup> Because we always assigned both treatment conditions within each vacancy, our sample of firms is, by construction, balanced across treatments.

We received callbacks for 17.1 percent of the applications; 57.9 percent of the applications were immediately rejected, 14.6 percent got no answer at all, and 10.4 percent were informed that more documents would be needed (without receiving an interview request or being short-listed).<sup>29</sup> The average response time was 8.3 days. The majority of the responses came in by email (85%), followed by phone call (13%), and postal mail (2%).

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<sup>26</sup>Like other correspondence studies (e.g., Bertrand and Mullainathan 2004; Eriksson and Rooth 2014; Kroft, Lange, and Notowidigdo 2013), we do not observe whether an applicant ends up getting the job, but simply whether a prospective employer contacts the applicant for a job interview. It seems reasonable that an invitation for a job interview reflects an employer’s hiring preference. We would thus expect that differences in interview rates also translate into differences in hiring rates.

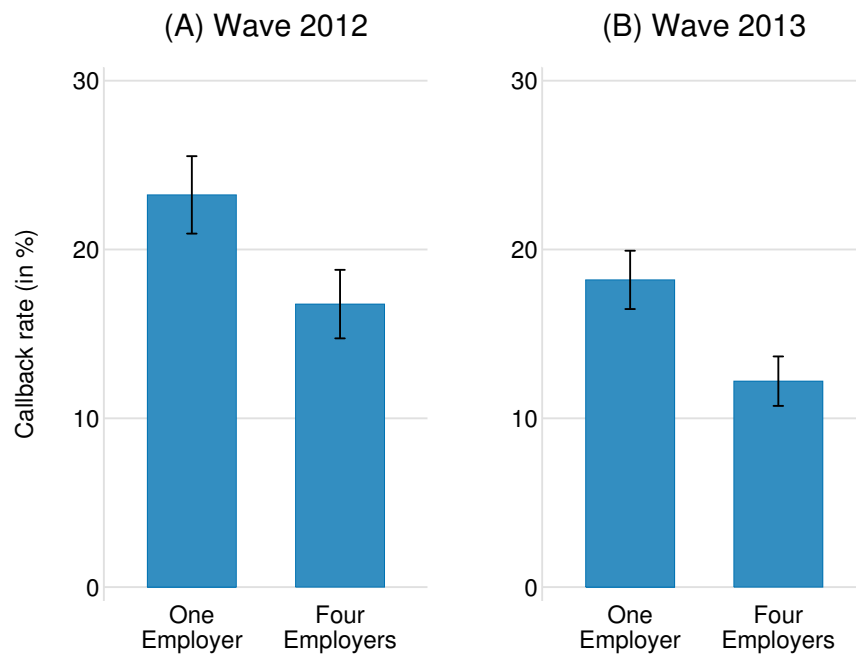
<sup>27</sup>The experiments were approved by the Human Subjects Committee of the Faculty of Economics, Business Administration, and Information Technology of the University of Zurich.

<sup>28</sup>The sample includes job openings placed by employment agencies (16.2%); the results do not change if we exclude these observations from the analysis.

<sup>29</sup>Our results are qualitatively the same if we treat requests for additional documents as callbacks (see Table 8 in the Appendix).

The results from the first wave show that the Four Employers profile led to a substantially lower callback rate (see Panel A in Figure 5). While the probability of a callback was 23.2 percent for the One Employer condition, the Four Employers resume resulted in a lower callback rate of 16.8%. Hence, the callback rate for the applicant with a single previous employer is almost 40 percent higher than for the applicant with four previous employers. The treatment effect is statistically significant according to a non-parametric McNemar test for paired observations (see Siegel and Castellan 1988) that compares how often one profile is preferred over the other ( $p = 0.003$ ).

**Figure 5:** Treatment effects



Error bars indicate standard error of the mean. Panel A displays average callback rates by treatment condition for the 2012 wave. Panel B shows the results for wave 2013, where the Four Employers resume did not contain employment gaps between the job changes.

As noted above, we adapted the resumes in the 2013 wave and removed all gaps between job changes. Panel B in Figure 5 shows that the results replicate when the Four Employers resume has no gaps between jobs. The effect in the 2013 wave is

similar in magnitude: we observe an almost 50 percent higher callback rate, from 12.2% in the Four Employers treatment to 18.2% in the One Employer treatment ( $p = 0.001$ , McNemar test).<sup>30</sup>

The observed treatment effect is sizable compared to other correspondence studies. For example, our treatment effect is comparable to the difference in callback rates that Kroft, Lange, and Notowidigdo (2013) found between applicants with one and eight months of unemployment. Our effect is also similar to the callback difference between white- and black-sounding names reported in Bertrand and Mullainathan (2004).<sup>31</sup>

Additional regression analysis corroborates the preceding non-parametric results. Specifically, we estimate the following linear probability model:

$$y_{ij} = \alpha + \beta_1 * N_{ij} + \beta_2 * \mathbf{X}_{ij} + \beta_3 * \mathbf{Z}_j + \epsilon_{ij}. \quad (3)$$

The dependent variable  $y_{ij}$ , indicating whether applicant  $i$  received a callback for job vacancy  $j$ , is regressed on a dummy variable,  $N_{ij}$ , indicating the Four Employers treatment. We control for month, gender of the applicant, gender of the HR contact person, and gender match between the two. Furthermore, we include dummies for employment agencies and part-time jobs, as well as the firms' industry and

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<sup>30</sup>Overall, the callback rate in both treatments was lower in 2013 than in 2012 ( $p < 0.001$ , MWU). One possible reason is that the applicants faced tougher labor market conditions in 2013. Monthly regional labor market statistics (SECO 2013) show that the average number of applicants per job increased from 8.8 to 10.4, and that the local unemployment rate rose from 2.7 to 2.8 between the first and the second wave. An occupation-specific but less direct indicator of labor market conditions is the average response time in our field experiment, which we can use as a proxy for the number of applications the HR recruiters had to assess at that time. In line with the aggregate labor market data we find a significant increase in average response time from 7.7 work days in 2012 to 8.7 work days in 2013 ( $p = 0.025$ , MWU). As shown in the regression analysis, the effect of multiple previous employers is neither more nor less important when workers have to compete more fiercely for jobs.

<sup>31</sup>Kroft, Lange, and Notowidigdo (2013) found that callback rates dropped from roughly 7% to 4%. With a standard deviation of 0.212 this implies a standardized mean effect (i.e., Cohen's  $d$ ) of 0.142. Bertrand and Mullainathan (2004) found callback rates of 9.7% and 6.4% for white and black-sounding names, respectively. The standard deviation in callbacks was 0.272, implying a Cohen's  $d$  of 0.121. In our study, pooling both waves, we found a 20.2% callback for One Employer and a 14.1% callback for Four Employer. The standard deviation in callbacks was 0.377, resulting in a Cohen's  $d$  of 0.164

legal form. Finally, we also consider the (log) driving distance to the work place and monthly local labor market conditions (i.e., the number of applicants per open position and the employment rate on a cantonal level). The control variables that vary within vacancies are represented by the vector  $X_{ij}$ , and those measured at the vacancy level are included in the vector  $Z_j$ . We allow for idiosyncratic variation with the error term,  $\epsilon_{ij}$ . We estimated our regression model using OLS and corrected standard errors for clustering at the vacancy level. The results remain the same if we use a Probit model instead.

**Table 5:** Regression analysis

Dependent variable	Callback = 1						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Four Employers	-0.062*** (0.014)	-0.060*** (0.018)	-0.060*** (0.018)	-0.061*** (0.018)	-0.061*** (0.018)	-0.060*** (0.018)	-0.074*** (0.019)
Four Emp. X wave 2012		-0.005 (0.028)	-0.005 (0.028)	-0.005 (0.028)	-0.005 (0.028)	-0.005 (0.028)	0.002 (0.029)
Wave 2012		0.050 * (0.029)	0.038 (0.030)	0.042 (0.029)	0.032 (0.030)	0.039 (0.031)	0.043 (0.031)
Industry experience							0.063 ** (0.031)
Constant	0.202 *** (0.014)	0.182 *** (0.017)	0.192 *** (0.032)	0.487 *** (0.156)	0.268 (0.192)	0.154 (0.246)	0.142 (0.246)
Additional controls?							
Month			Yes		Yes	Yes	Yes
Gender/gender match			Yes			Yes	Yes
Firm/job characteristics				Yes		Yes	Yes
Ln(driving distance)				Yes	Yes	Yes	Yes
Labor market cond.					Yes	Yes	Yes
Observations	1680	1680	1680	1680	1680	1680	1680
$F$	20.328	8.271	6.200	6.913	5.488	5.110	4.642
Prob> $F$	0.000	0.000	0.000	0.000	0.000	0.000	0.000

OLS regressions, cluster-robust standard errors at the job ad level.

*Dependent variable:* dummy indicating a callback.

*Independent variables:* “Four Employers:” dummy for Four Employers profile; “Wave 2012:” dummy for first wave of study (in 2012); “Industry experience:” dummy whether applicant has had previous work experience in the corresponding industry; “Month:” dummies for month when application was sent; “Gender / gender match:” dummies for gender of applicant and HR person, and corresponding interaction term; “Firm / job characteristics:” industry dummies, legal form dummies, employment agency dummy, and part-time job dummy; “ln(driving distance):” log of distance in meters by car (using Google Maps); “Labor market conditions:” monthly local unemployment rate and number of applicants per open position (statistics from State Secretariat for Economic Affairs (SECO)).

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Column 1 in Table 5 shows the regression results without control variables. We find a significant 6.2 percentage point reduction in the average callback rate in

the Four Employers treatment ( $p < 0.001$ , t-test). In Column 2 we test whether the treatment effect is significantly different between the two waves by including a dummy for the 2012 wave and its interaction with the treatment. The interaction effect is small and statistically insignificant ( $p = 0.867$ , t-test), suggesting that the employment gaps in the 2012 resumes cannot explain the treatment effect. Columns 3 through 6 illustrate that the Four Employers effect is robust in magnitude and significance if we control for a variety of background variables.<sup>32</sup>

#### **Result 4 (Multiple employers and employability in the field)**

*Applicants with more employment changes are significantly less likely to receive callbacks. This effect is similar in magnitude and significance irrespective of whether resumes include short employment gaps between jobs.*

At this point we want to emphasize that our results do not imply that more frequent job changes will always reduce employability. A higher frequency of job changes can, in principle, also signal desirable qualities, such as that a worker gained more transferable human capital due to more diverse work experiences (Mincer 1958; Becker 1962). For example, the probability that our applicants had work experience in the industry of the prospective employer was naturally higher for the worker with more diverse experience (Four Employers) than for the worker with One Employer (50 vs. 32.6 percent,  $p < 0.001$ ,  $\chi^2$ -test). To explore the extent to which more diverse industry experience had a compensating positive effect on employability we additionally included a dummy variable, “Industry experience,” in our regression model. This variable takes a value of one if the applicant has ever worked in the industry of the prospective employer and zero otherwise. Column 7 of Table 5 illustrates that industry experience significantly increases the probability of a callback by 6.3

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<sup>32</sup>We additionally examined possible sources of heterogeneity in the treatment effect, including the applicants’ gender, job vacancies placed by employment agencies, full-time job openings, driving distances to the work place, as well as monthly regional labor market conditions. However, none of the interactions reaches statistical significance at conventional levels. We also find that the point estimates of the treatment effect are negative for all but one of the eight applicant identities. All of these additional tests are available from the authors upon request.

percent ( $p = 0.040$ ). Hence, separately from the negative effect of switching jobs on employability, job changes can increase the likelihood of relevant work experience, and this work experience makes a candidate more employable. At the same time, however, the coefficient for the Four Employers treatment is approximately 23 percent more negative in Column 7 than in Column 6, where we do not control for industry experience. Hence, if the One and the Four Employers candidates have similar levels of industry experience, employers will discriminate even more strongly against the Four Employers candidate than our earlier estimates suggest.

## 4. Survey Experiment

Returning to Table 1, we have thus far found evidence supporting Hypothesis 2 in both a laboratory environment and in a non-laboratory labor market. The remaining open question is whether workers in natural labor markets who change jobs more frequently also tend to have, or are perceived to have, lower work attitude. Answering this question can help address if the effect of employment history that we find in the field experiment is at least partly due to employers' perceptions of work attitude. To obtain a measure of such perceptions, we conducted an additional survey experiment with Human Resources professionals.

### 4.1. Survey Experiment: Environment and Design

We used a job fair for university graduates in Zurich to recruit a large sample of HR professionals. At this fair, about 130 mostly large companies from a variety of industries (e.g., engineering, electronics, telecommunications and consulting) present themselves to job seekers.<sup>33</sup> Each company had its own booth, at which company representatives, including recruiters, were available for questions about what kind of employees the firm is looking for or how the application process works. We ap-

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<sup>33</sup>See Table 9 in the Appendix for descriptive statistics of our survey sample.

proached each company and asked whether the most experienced HR representative would be willing to participate in a short survey. The surveys were administered by four research assistants in April 2014. A total of 83 HR professionals completed the survey.

Given the smaller sample size than in our field experiment, we selected two male candidates from those used in the field experiment. Each survey participant was shown a “Four Employers” and a “One Employer” resume, side by side. We randomized which of the two candidates would be the one with the greater number of job changes and counterbalanced the order (left or right) in which the candidates were presented.

In the survey, participants rated both candidates on ten characteristics using 7-point Likert scales, ranging from 1 “does not apply at all” to 7 “applies fully.”<sup>34</sup> The characteristics can be broadly divided into skills or experience (captured by the items “skilled,” “experienced in commerce,” and “multi-talented”) and work attitude (i.e., “able to work in teams,” “willing to adapt,” “patient,” “honest,” “reliable,” “self-directed,” and “goal-oriented”). We further asked participants how likely they would be to call back a candidate for a job interview, on a scale from 1 “very unlikely” to 7 “very likely,” had the applicant applied for a job at their firm.

The survey responses allow us to examine which qualities HR professionals associate more strongly with the different resumes from the field experiment, and which of these qualities are likely responsible for the difference in callback rates in the field experiment between candidates with more frequent job changes relative to those with fewer.

## 4.2. Results

To distinguish between skills/experience and work attitude, we created an index for each dimension by averaging the ratings for a respondent’s perceptions of the

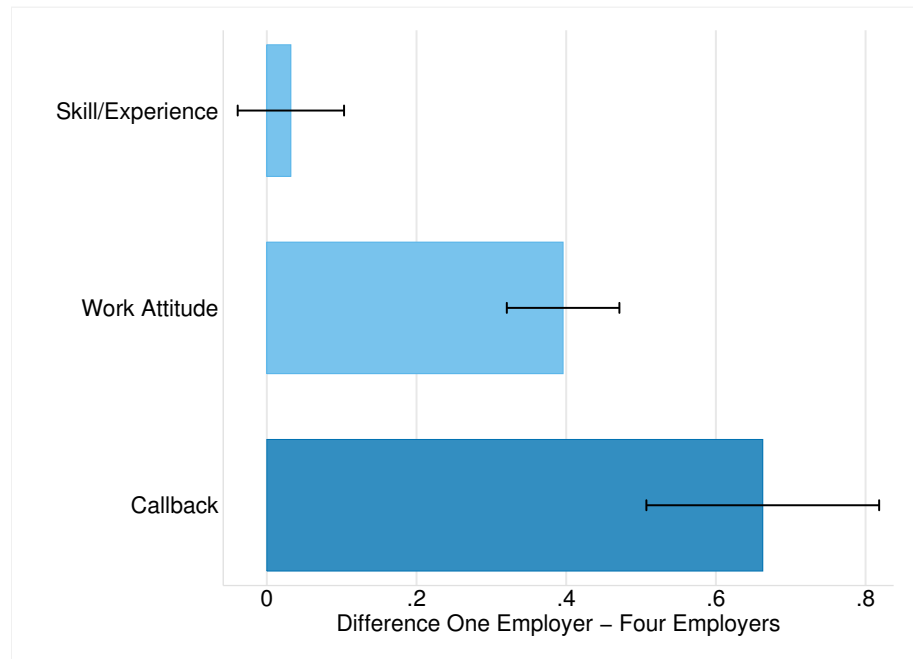
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<sup>34</sup>The survey is provided in the Appendix.



individual qualities in that dimension. Figure 6 reveals that, relative to the candidates with fewer previous employers, the Four Employers candidates score 0.40 points lower on Work Attitude ( $p < 0.001$ , Wilcoxon signed rank test (WSR)). By contrast, the difference in the Skills/Experience score between the two candidate profiles is smaller (0.03 points) and statistically insignificant ( $p = 0.651$ , WSR).<sup>35</sup>

**Figure 6:** Rating Differences between One Employer and Four Employers Resumes



Average difference in ratings, on a 7-point Likert scale, One Employer minus Four Employers. Error bars indicate standard error of the mean.

Moreover, the HR professionals indicate that they would be more likely to call back the One Employer than the Four Employers candidate for a job interview ( $p < 0.001$ , WSR). We thus replicate that employers are more likely to invite those candidates for a job interview who change jobs less frequently, confirming a key result from our laboratory and field experiments in this separate sample of HR professionals.

<sup>35</sup>Table 11 (see Appendix) lists the treatment difference separately for each of the ten items. We observe the strongest treatment effects on three dimensions of work attitude—“patient,” “reliable,” and “teamwork”—and the weakest on two of the three dimensions of Skills/Experience—“multi-talented” and “experienced.”

### Result 5 (Different Ratings of Resumes with One and Four Employers)

We replicate our previous result that employers prefer candidates with fewer job changes. Moreover, recruiters perceive candidates with more frequent job changes to have a lower work attitude than candidates with fewer job changes. We observe no such difference with respect to skills and experience.

**Table 6:** Regression analysis survey responses: Skill/Experience and Work Attitude

	(1)	(2)	(3)	(4)
Four Employers	-0.663*** (0.156)	-0.282** (0.123)	-0.643*** (0.142)	-0.292** (0.124)
Work Attitude		0.962*** (0.185)		0.932*** (0.205)
Skill/Experience			0.617*** (0.188)	0.043 (0.219)
Constant	5.518*** (0.129)	0.626 (0.978)	2.237** (1.032)	0.547 (1.160)
adj. $R^2$	0.053	0.275	0.152	0.271
N	166	166	166	166
% explained	—	57.5	3.0	55.9

OLS regressions, cluster-robust standard errors in parentheses, clustered on recruiter level.

Unit of observation: recruiter-resume (2 resumes per recruiter).

Significance levels: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

*Dependent variable:* Callback rating for a resume (7-point Likert scale).

*Independent variables:* “One Employer:” dummy variable for resume with only one employer; “Skill/Experience:” unweighted average of ratings on “skilled,” “experienced in commerce,” and “multi-talented;” “Work Attitude:” unweighted average of ratings on “able to work in teams,” “willing to adapt,” “patient,” “honest,” “reliable,” “self-directed,” and “goal-oriented.”

*% explained:* Result of Oaxaca-Blinder decomposition of Four Employers effect: how much of the 0.663 point treatment difference in invitation ratings is explained by the difference in the respective regressors?

To assess the extent to which the perceived quality differences can account for differences in callbacks, we estimate the following model:

$$y_{ij} = \alpha + \beta_1 * N_{ij} + \beta_2 * A_{ij} + \beta_3 * S_{ij} + \epsilon_{ij}. \quad (4)$$

The dependent variable  $y_{ij}$  is the likelihood of callback a recruiter  $j$  assigns to a candidate  $i$ .  $N_{ij}$  is a dummy variable indicating the Four Employers treatment. We additionally include the applicants’ score for work attitude ( $A_{ij}$ ) and skills/experience ( $S_{ij}$ ). We estimate the model using OLS and correct standard errors to account

for dependence in the error term  $\epsilon_{ij}$  at the recruiter level. Column 1 in Table 6 reports the unconditional effect of the Four Employers treatment: Callback likelihood ratings are, on average, 0.66 points lower in the Four Employers than in the One Employer treatment ( $p < 0.001$ , t-test). In column 2, we add the work attitude score and find that the coefficient is close to one and highly significant ( $p < 0.001$ , t-test). That is, an increase in perceived work attitude by one point increases the callback rating by roughly one point. Crucially, the Four Employers treatment effect shrinks from  $-0.663$  to  $-0.282$ , a reduction by 57.5%. This suggests that over half of the treatment effect can be explained by the fact that recruiters rate “Four Employers” candidates lower on work attitude than those with One Employer.<sup>36</sup>

By contrast, although the skill/experience score is positively associated with the likelihood of callbacks, it does not explain much of the treatment effect (see column 3). While an increase in skills/experience by one point increases the callback rating by about 0.6 points ( $p = 0.002$ , t-test), the Four Employers coefficient decreases by only 3%. Hence, perceptions of skills and experience are predictive of callback likelihood, but they do not help explain why Four Employers candidates achieve, on average, lower callback rates than One Employer applicants.

Finally, column 4 includes both scores simultaneously as regressors. The coefficients of the Four Employers dummy and work attitude remain practically unchanged compared to column 2, whereas the skills/experience coefficient is close to zero and statistically insignificant. Hence, perceived work attitude is more strongly related to callback ratings than perceived skills/experience.

### **Result 6 (Work Attitude Explains why Recruiters Prefer One Employer)**

*Recruiters report they are less likely to call back candidates with more prior employers in large part because they perceive them to have a poorer work attitude than those with fewer prior employers. Perceptions of skills and experience do not*

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<sup>36</sup>This result is equivalent to a pooled Blinder-Oaxaca decomposition (Blinder 1973; Oaxaca 1973).

*explain the treatment effect.*

## 5. Discussion and Conclusion

This paper puts forth a novel interpretation of the relationship between job changes and employability. We argue that job changes can provide a signal of a worker’s non-cognitive skills, such as cooperativeness, reliability, and ability to work well with others—which we describe broadly as “work attitude.” Our motivating hypothesis is that workers who are less cooperative, reliable, team-oriented, and generally more difficult to get along with will often be, holding all else equal, the ones who change jobs more frequently. As a consequence, we expect that prospective employers will use employment history as a signal of work attitude and discriminate against employees who change jobs more frequently. An inspection of the National Labor Survey of Youth 1997 provides some correlational support for these predictions.

Building on these suggestive relationships, we combine lab, field, and survey experiments to test our hypotheses more directly. In the laboratory experiment we find that employment history provides a signal of work attitude. Workers who switch employers less frequently are more likely to fulfill employers’ effort requests. Firms recognize this and exhibit a preference for hiring workers with fewer job changes when this information is available. In the field experiment we sent out pairs of resumes for several open job listings—one resume in which the applicant changed jobs frequently and another in which the applicant remained with a single employer. As in the laboratory experiment, we find that employers exhibit a preference for job applicants with fewer job changes: Frequent job changes result in substantially lower callback rates. To verify that the differential demand for the candidates from the field experiment is due to employers’ perceptions of the candidates’ work attitude, we conducted a survey with HR professionals. The results confirm that a primary inference that arises from the resumes used in the field experiment is that recruiters

perceive workers who change jobs more frequently as lower on dimensions related to work attitude—particularly reliability, patience and ability to work in teams. Moreover, this perception accounts for a large part of recruiters’ stated preferences for the applicant with fewer prior job changes. This provides corroborative evidence that at least one important mechanism driving the results in our field experiment is similar to that in our laboratory experiment.

Hence, from all of our studies, in combination, two central results emerge. First, in the contexts we study, firms have a preference for workers who change jobs less frequently. We observe a strong effect in both the lab and field. Second, changing jobs less frequently is correlated—or at least perceived to be correlated—with measures of greater reliability and more positive work attitude.

Several further interesting observations arise from our studies. In the laboratory experiment, when firms can observe work histories, those workers with fewer job changes earn considerably more following a shock in which everyone has to search for new employment. We also observe greater history dependence in labor market outcomes when job histories are available: Workers tend to stay either employed or unemployed for longer periods. In combination, these findings suggest that concerns about appearing to have poor work attitude may, in some cases, create labor market inefficiencies. Perhaps most importantly, they suggest a possible friction in labor market mobility—workers may fear changing jobs due to the impact on their perceived work attitude.

Finally, we want to stress that we do not believe that workers who change jobs less frequently will always be more attractive to employers. There may be many contexts in which frequent job changes convey desirable qualities, such as varied experience, larger professional networks, and greater ambition. Any of these things may mitigate or entirely counteract the effects we observe in our studies. Indeed, in our field experiment we find that industry experience—which is more likely for an applicant with more frequent job changes—increases the likelihood of a favorable

response from a prospective employer. Thus, even in our data there are ways in which employment changes can be beneficial. It is also important to note that our analysis focuses primarily on horizontal job changes; however, a large number of vertical moves, leading to more challenging and better paid positions, may be much less likely to yield the kind of negative signal we observe here. Hence, we acknowledge that there are settings in which our motivating conjecture may not apply. Our point, however, is that where work attitude and reliability are important relative to concerns like those above, the market may interpret frequent job changes as a negative signal of this quality, and workers' job market prospects may be harmed by frequently changing jobs. We leave for future work to identify the important boundaries on our finding.

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# Appendices

## A. Analysis of NLSY97 Data

The National Longitudinal Survey of Youth 1997 (NLSY97) is a nationally representative sample of 9,000 Americans who were between the ages of 12 and 16 at the onset of the survey study; the latest wave was conducted between 2013 and 2014 and included more than 7,000 participants.<sup>37</sup>

**Outcome measures.** For the relationship between frequency of job changes and measures of work attitude, we use the total number of jobs held since the age of 20 (“Jobs”). For the relationship between number of previous jobs and current employment status, we use an indicator variable for being unemployed in October 2013, the last month for which data on all participants in the 2013 wave are available (“Unempl”). As an additional outcome, we examine earnings from wages and salaries in 2012 (“Wages”).

**Work attitude and other personality measures.** We examine those variables we believe to be related to work attitude, and for which enough observations were available in the data set. We constructed the variables “Break Rules” and “Work Hard” based on two sets of four questions each.<sup>38</sup> We also analyzed the variables “Drink at Work” (= 1 if a participant reported to have ever drunk alcohol at work) and Ever Arrested (= 1 if a participant reported to have ever been arrested by the

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<sup>37</sup>The dataset is publicly available at <https://www.nlsinfo.org/content/cohorts/NLSY97>.

<sup>38</sup>For Break Rules, we computed the average responses to the following questions: “I do not intend to follow every little rule that others make up;” “When I was in school, I used to break rules quite regularly;” “I support long-established rules and traditions;” “Even if I knew how to get around the rules without breaking them, I would not do it” (coding inverted for questions 3 and 4). For Work Hard, we took the average responses to the following questions: “I do not work as hard as the majority of people around me;” “I do what is required, but rarely anything more;” “I have high standards and work toward them;” “I make every effort to do more than what is expected of me” (coding inverted for questions 1 and 2). All responses use a 7-point Likert scale from “Disagree strongly” (= 1) to “Agree strongly” (= 7).

police) as measures of work attitude.<sup>39</sup> The 2007 wave additionally elicited the Big Five personality traits using the Ten Item Personality Inventory (TIPI). The corresponding variables are “Extraverted,” “Agreeable,” “Conscientious,” “Emotionally Stable,” and “Open to Experience.”

**Covariates.** We include the following basic demographic and geographical variables: age (“Age”, in years in 2013), gender (“Gender”), dummies for ethnicity (“Ethn”), region dummies (“Region”, i.e., Northeast, North, South, West), and a dummy for urban versus rural area (“Urban”). As measures of educational attainment we include dummies for highest academic degree achieved (“HDeg”) and the (standardized) grade point average before leaving secondary school (“GPA”).<sup>40</sup> We also include the total number of weeks a participant was employed since the age of 20 (“Empl”). Finally, we control for the month of the most recent interview (“IntMonth”, dummies) because cumulative variables, such as the number of previous jobs, may be systematically higher for those who were interviewed at a later date.

### A.1. Number of previous jobs, work attitude, and personality

We ran separate regressions for each of the work attitude and personality measures, respectively, and adjusted the standard errors for multiple hypothesis testing using the Holm-Bonferroni method. To compare effect sizes, we standardized each measure to mean zero and standard deviation of 1. The dependent variable in each regression is Jobs. In addition to the work attitude and personality measures, we include the controls Empl, HDeg, GPA, Age, Gender, Ethn, Region, Urban, and IntMonth.

Figure 7 presents the results from this analysis. Individuals who are more likely to break rules, have been arrested by the police, and drink at work switch jobs

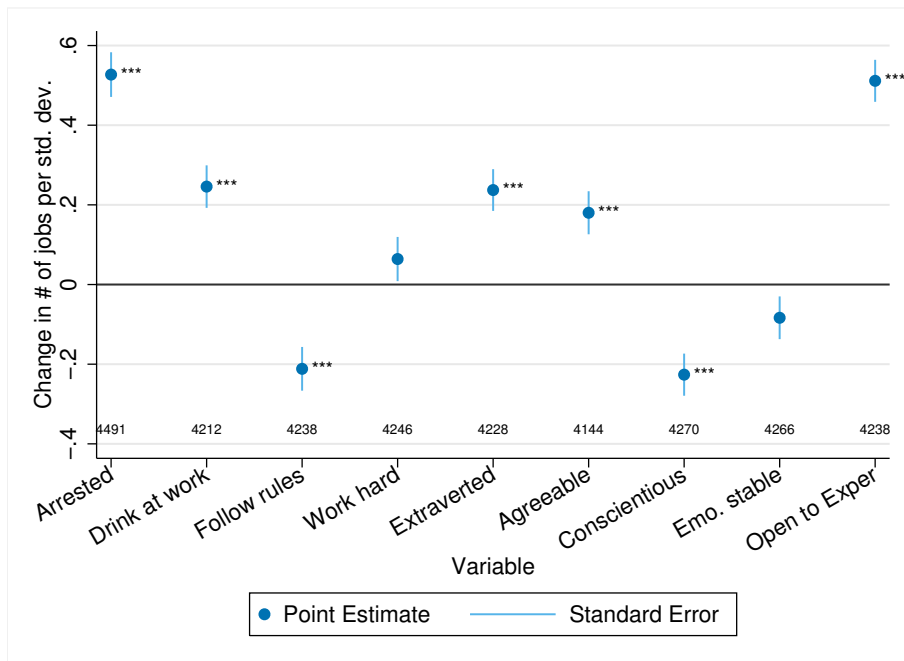
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<sup>39</sup>Drink at Work featured in different waves for different participants. Here, we pool these waves and code everybody who answered positively at least once as 1, the others as 0.

<sup>40</sup>An alternative measure of academic skill is the Armed Services Vocational Aptitude Battery, a set of quantitative and verbal reasoning tests administered during the 1999 wave; all results are robust to using this measure instead of GPA.

significantly more often ( $p < 0.01$  in each case).<sup>41</sup> Less conscientious labor market participants also switch jobs more often ( $p < 0.01$ ). By contrast, hardworking individuals do not change jobs more frequently relative to others ( $p = 0.25$ ). While emotional stability is unrelated to job switching ( $p = 0.24$ ), being extraverted, agreeable, and open to new experiences is associated with a higher frequency of job changes ( $p < 0.01$  in each case).

**Figure 7:** Number of previous jobs, work attitude, and personality



OLS regressions of the number of job changes (Jobs) on measures of work attitude and personality traits controlling for Empl, HDeg, GPA, Age, Gender, Ethn, Region, Urban, and IntMonth. Each dot represents a separate regression for each of the work attitude and personality measures. The number of observations (at the bottom) varies across regressions because of missing survey responses. The stars next to the dots indicate significance adjusted for multiple hypothesis testing using the Holm-Bonferroni correction. Significance levels: \*\*\*  $p < 0.01$ .

<sup>41</sup>All p-values reported in this section are based on t-tests and the standard errors are corrected for multiple hypothesis testing.

## A.2. Current unemployment status, labor earnings, and number of previous jobs

In this section we examine whether the number of job changes predicts current unemployment status as well as income from employment. We regressed current unemployment (Unempl) on prior number of jobs (Jobs), while including the covariates Empl, HDeg, GPA, Age, Gender, Ethn, Region, Urban, and IntMonth. Figure 1 shows that a higher number of jobs in the past is associated with a higher probability of being currently unemployed. Specifically, every additional job in a worker’s employment history increases the probability of unemployment by 0.47 percentage points ( $p < 0.01$ ). This relationship is present across a variety of subgroups. We also found that a higher number of previous jobs is negatively related to wage and salary income in the year before the 2013 wave (see Figure 8). An additional job in the past is associated with \$1,504 lower annual income, on average ( $p < 0.01$ ).<sup>42</sup> This relationship is also robust across different subgroups, although the income disadvantage due to frequent job changes is larger for men than for women ( $p < 0.01$ ).

## B. Laboratory Experiment: Subject Instructions

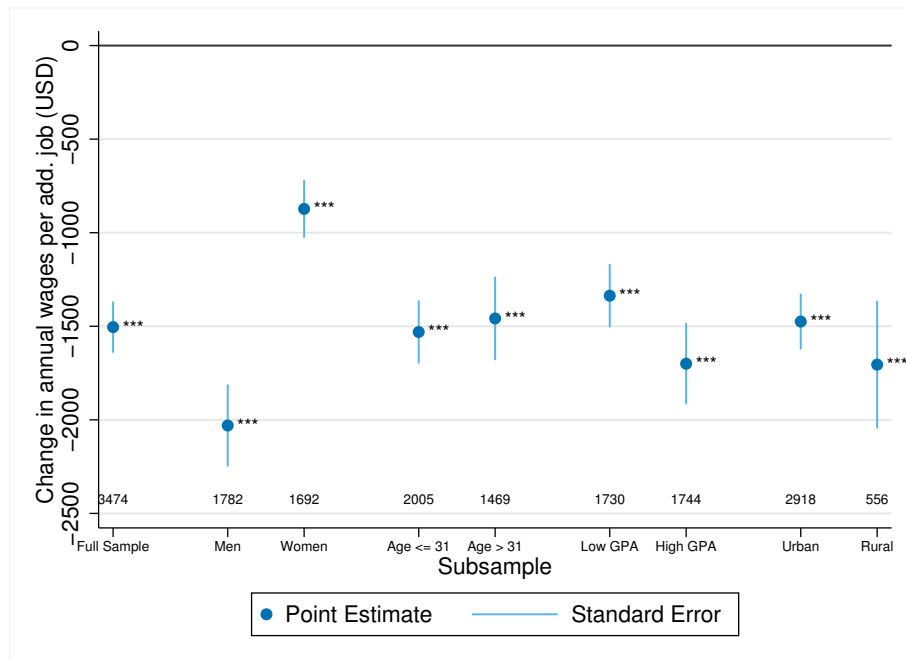
We present the complete instructions for the History condition. The highlighted section (“History Table”) was removed for the No History condition. Instructions follow the wording in Brown, Falk, and Fehr (2004). Comprehension questions, exit questionnaire, and ztree files are available upon request from the authors.

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<sup>42</sup>Our results are consistent with Light and McGarry (1998) who used the older NLSY79 panel to show that more frequent job changes is associated with lower earnings.



**Figure 8:** Current labor earnings and number of previous jobs



OLS regressions of the number of previous job changes (Jobs) on labor earnings controlling for Empl, HDeg, GPA, Age, Gender, Ethn, Region, Urban, and IntMonth. Each dot represents a separate regression for each of the subgroups. The numbers at the bottom indicate the size of the respective subsample. The stars next to the dots indicate significance. Significance levels: \*\*\*  $p < 0.01$ .

## Initial Instructions

Thank you for participating in today's experiment.

I will read through a script to explain to you the nature of today's experiment as well as how to navigate the computer interface with which you will be working. I will use this script to make sure that the information given in all sessions of this experiment is the same. Please follow the instructions carefully.

In addition to a 10 CHF payment that you receive for your participation, you will be paid an amount of money that you accumulate from the decision task that will be described to you in a moment. The exact amount you receive will be determined during the experiment and will depend on your decisions and the decisions of others. You will be paid privately, in cash, at the conclusion of the experiment.

All monetary amounts you will see in this experiment will be denominated in ECUs or Experimental Currency Units. We will convert ECUs into CHF at the rate of

**1 ECU = 0.05 CHF.**

*If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you.*

*Please do not talk, exclaim, or try to communicate with other participants during the experiment.*

*Do not use the computer in a way not specified by these instructions or by the experimenters.*

Participants intentionally violating the rules may be asked to leave the

## Basic Information

### Number of Periods:

The experiment is divided into periods. In each period you have to make decisions, which you will enter in a computer. There are **30 periods** in total.

### Buyers and Sellers:

In this room there are 17 participants. These 17 participants have been randomly divided into 2 groups: buyers and sellers. These roles are fixed, that means each buyer will remain a buyer, and each seller will remain a seller for the entire experiment. Whether you are a buyer or a seller is displayed on the computer screen. Please raise your hand if you do not see where the screen tells you whether you are a buyer or a seller. **There are 7 buyers and 10 sellers.**

### Identification Number:

All participants have received an **identification number (ID)**, which they will keep for the entire experiment. Your identification number is displayed on the computer screen.

## An Overview of the Experiment Procedures

In each period of the experiment every buyer can trade a product with one seller. The seller earns a profit through the trade when he sells the product at a price that exceeds his production costs. The buyer earns a profit through the trade when the price he pays for the product is less than what it is worth to him. How high the production costs are for the traded product, and how much the product is worth to the buyer both depend on the quality of the product. We will describe below how the quality of a product is determined.

Each of the 30 periods is structured as follows:

### 1. Trading Phase

Each period commences with a **trading phase**, which lasts 2 minutes. During this phase buyers can submit trade offers that can be accepted by sellers.

When submitting an offer a buyer has to specify **three things**:

- Which **price** he offers to pay
- Which product **quality** he desires
- To which **seller** he wants to submit the offer.

Buyers can submit two types of offers: private offers and public offers.

- **Private offers** are submitted to one seller only and can only be accepted by that seller.
- **Public offers** are submitted to all sellers and can be accepted by any seller.

A buyer can submit as many offers as he likes in each period. Sellers can accept submitted offers at any point. **Each buyer and each seller can only enter one trade agreement in each period.** As there are 7 buyers and 10 sellers, in each period there will be some sellers who will not trade.

### 2. Quality Choice

Following the trading phase each seller who has entered a trade agreement then determines which quality of product he will supply to his buyer. **The seller is not obligated to supply the product quality desired by his buyer.** Once every seller has chosen which product quality to supply, the ECUs gained by each participant in that period have been determined. After this the next period begins.

The ECUs gained in all 30 periods are summed up at the end of the experiment, exchanged into CHF and paid together with the initial 10 CHF in cash.

## The Experiment Procedures in Detail

There are 7 buyers and 10 sellers in the experiment. Your role is fixed throughout the experiment. During the experiment you will enter your decisions on a computer screen. In the following we describe in detail how you can make your decisions in each period.

### The Trading Phase

Each period commences with a trading phase. During the trading phase each buyer can enter into a trading agreement with one seller. In order to do this **each buyer can submit as many trade offers as he wishes.**

#### Buyer's Screen

In each trading phase, buyers will see the following screen:

Period 1 of 1			Remaining Time (sec): 101		
<b>History Table</b>					
Public Offers			Your private offers		
Buyer	Price	Req. quality	Price	Req. quality	to Seller
<b>Public Offers Table</b>			<b>Private Offers Table</b>		
Your Identification Number: -1					
Make your offers here					
<input type="radio"/> public					
<input type="radio"/> private					
If private, to which Seller? <input type="text"/>					
Your price <input type="text"/>					
Requested quality <input type="text"/>					
<input type="button" value="OK"/>					
<div><input type="checkbox"/> 1 <input type="checkbox"/> 2 <input type="checkbox"/> 3 <input type="checkbox"/> 4 <input type="checkbox"/> 5 <input type="checkbox"/> 6 <input type="checkbox"/> 7 <input type="checkbox"/> 8 <input type="checkbox"/> 9 <input type="checkbox"/> 10</div>					
Your Seller		Your price		Your req. quality	

In the top left corner of the screen is the current period of the experiment. In the top right corner of the screen is the time remaining in this trading phase, displayed in seconds. **The trading phase in each period lasts 2 minutes** (= 120 seconds). When this time is up the trading phase is over. Subsequently, no further offers can be submitted or accepted for the period.

#### Buyer's Screen: Making an Offer

Once the buyers see the above screen displayed the trading phase commences. Each buyer now has the opportunity to submit trade offers to the sellers. In order to do so they have to enter three things on the right hand side of the

screen:

1. Offer Type
2. Price
3. Desired Quality

#### 1. Offer Type

First the buyer has to specify whether he wants to submit a public or private offer:

- **Public trade offers** will be communicated to all participants in the market. All sellers see all public offers on their screens. A public offer can therefore be accepted by **any seller**. Each buyer will also see all public offers submitted by other buyers. To submit a public offer, a buyer clicks on the field „public“ when making an offer, **and enters “0” in the field “to which Seller”**.
- **Private trade offers** are submitted to **one seller only**. Only this seller will be informed of this offer and only this seller can accept that trade offer. No other seller or buyer will be informed about that offer. To submit a private offer, a buyer clicks in the field „private” when making an offer and then specifies **to which seller** he wants to submit the offer in the field below. Each of the 10 sellers has an identification number (seller 1, seller 2, ... , seller 10). Each seller keeps his identification number for the entire course of the experiment. To submit an offer to a specific seller, the buyer enters the number of that seller (e.g. „5“ for seller 5).

#### 2. Price

Once the buyer has specified to whom he wants to submit an offer, he must determine **which price to offer**. He enters this in the field „your price“. The price must be an integer and cannot be below 0 or above 100:

$$0 \leq \text{price offered} \leq 100$$

#### 3. Desired Quality

Finally, a buyer has to specify which product quality he desires. He enters this in the field „desired quality“. The **desired quality** must be an integer and cannot be lower than 1 or higher than 10.

$$1 \leq \text{desired quality} \leq 10$$

After a buyer has completely specified a trade offer, he must click on the „ok“ button to submit it. As long as he has not clicked „ok“, he can change the trade offer. After he has clicked „ok“, the offer will be displayed to all sellers to whom the buyer has submitted the offer.

### **Buyer's Screen: Open Offers**

On the left side of the buyer's screen are the „**public offers**“. All public offers in the current trading phase are displayed here. Every buyer can see which buyer submitted the offer, which price he offered and which quality he desired. All buyers also have an identification number, which they keep for the whole course of the experiment.

In the middle of the buyer's screen, under „**your private offers**“, each buyer will see all his private offers he has submitted in the current trading phase. He can see to which seller he submitted an offer, which price he offered and which quality he desired.

**Each buyer can submit as many private and public offers as he wishes in each period.** Each offer that he submits can be accepted at any time during the trading phase.

**Each buyer can enter only one trade agreement in each period.** Once one of his offers has been accepted he will be notified which seller accepted which of his offers. In the bottom right corner of the screen the identification number of the seller will be displayed as well as the buyer's offered price and desired quality. Because each buyer can enter only one trade agreement in each period, all his other offers will be automatically cancelled. Also, he will not be able to submit any further offers.

**No seller can enter more than one trade agreement in each period.** Buyers will be constantly informed which sellers have not yet accepted a trade offer. In the bottom right corner, they will see 10 fields. Once a seller has accepted an offer, an „x“ will appear in the field next to his identification number. Buyers cannot submit private offers to a seller who has already entered a trade agreement.

Once all buyers have entered a trade agreement or after the 2 minutes are up, the trading phase is closed by the computer.

No buyer is obligated to submit trade offers, and no seller is obligated to accept a trade offer.

## Seller's Screen

During the Trading Phase, sellers will see the following screen:

Period	1 of 20	Remaining Time [sec] 114
--------	---------	--------------------------

## History Table

Your Identification Number 1		
------------------------------	--	--

Private offers to you		
from Buyer	Price	Req. quality
<h3 style="margin: 0;">Private Offers Table</h3>		
<input type="button" value="accept"/>		

Public Offers		
From Buyer	Price	Req. quality
<h3 style="margin: 0;">Public Offers Table</h3>		
<input type="button" value="accept"/>		

Your Buyer	Your price	Req. quality

This screen is similar to the buyer's screen and contains information about the current period, remaining time for trading, and currently open public offers from all buyers. The screen also shows all private offers that are made to this particular seller. A seller cannot see private offers that are made to other sellers. Every offer that is shown on the screen contains the buyer's ID, the offered price, and the desired quality.

Each seller can accept at most one offer. To accept a private offer, the seller clicks the row of the offer he wants to accept and confirms by clicking the **“accept”** button under the list with the private offers. To accept a public offer, the seller clicks the row of the offer he wants to accept and confirms by clicking **“accept”** under the list with the public offers.

As long as the seller does not click “accept”, he can change his decision by clicking on a different offer. As soon as the seller has pressed the „accept“ button he will see which offer he has accepted in the bottom row of the screen.

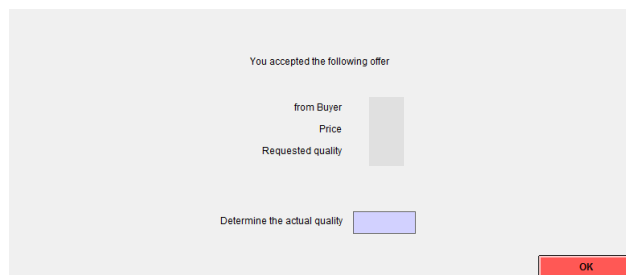
**Each seller can enter only one trade agreement in each period.** Once a seller has accepted one offer he cannot accept any further offers.

## Choice of Product Quality

Following the trading phase, all sellers who have entered a trade agreement then determine which product quality they will supply to their respective buyers. **The product quality that the buyer desired in his trade offer is not binding for his seller.** His seller can choose the exact quality the buyer desired, but he can also choose a higher or lower product quality.

### Seller's Screen

The seller's screen looks like this:



The screenshot shows a light gray rectangular window. At the top, it says "You accepted the following offer". Below this, there are two columns of text. The left column contains "from Buyer", "Price", and "Requested quality". The right column contains a gray rectangular input field. Below these columns, there is a label "Determine the actual quality" followed by a blue rectangular input field. In the bottom right corner of the window, there is a red button with the text "OK".

The seller enters the quality and clicks "ok". The product quality the seller chooses has to be an integer between 1 and 10.

$$1 \leq \text{product quality} \leq 10$$

### Buyer's Screen

While the seller determines the actual product quality, we ask the buyer to specify which quality he expects the seller to supply on a separate screen. In addition we ask him to state how sure he is of this expectation.

## How are the incomes calculated?

The incomes of all buyers are determined in the same way and the incomes of all sellers are also determined in the same way. **Each buyer can therefore calculate the income of his seller and each seller can calculate the income of his buyer.** Further, each buyer and seller is informed of the identification number of his trading partner in each period.

Please note that buyers and sellers can incur losses in each period. Any loss you incur has to be paid from your initial sum of money or from earnings in other periods.



**Buyer Income:**

If a buyer does not enter a trade agreement during a trading phase he gains an income of 0 ECUs for that period.

If one of a buyer's trade offers is accepted, his income depends on which price he offered and which product quality his seller supplied to him. His income will be determined as follows:

$\text{Buyer's Income} = 10 * \text{Product Quality} - \text{Price}$
--

As can be seen from the above formula the buyer's income is higher, the higher the product quality actually supplied by his seller. At the same time his income is higher, the lower the price he paid for the product.

**Seller Income:**

If a seller has not entered a trade agreement during a trading phase he gains an income of 5 ECUs for that period.

If a seller has accepted a trade offer, his income will be equal to the price he receives minus the production costs he incurs for the product quality supplied. The income of the seller is determined as follows:

$\text{Seller's Income} = \text{Price} - \text{Production Costs}$
---

The production costs of a seller are higher, the higher the quality of the product he chooses. The production costs for each product quality are displayed in the table below:

Product Quality	1	2	3	4	5	6	7	8	9	10
Production Costs	0	1	2	4	6	8	10	12	15	18

As can be seen from the above information the seller's income is higher, the higher the price that he accepted. Further, his income is higher, the lower the product quality he supplies to the buyer.

**Income Screen:**

You will be informed of your income and the income of your respective buyer/seller on an „**income screen**“. On this screen the following information will be displayed:

- Which buyer/seller you traded with
- Which price you offered/accepted
- The desired quality by the buyer
- The product quality supplied by the seller
- The income of the buyer and the seller in this period

After the income screen has been displayed, the respective period is concluded, and the trading phase of the following period begins. Once you have finished studying the income screen please click on the „next“ button.

[Authors' note: The following paragraph appears only in the NoHistory condition (up to “[...] represent the buyers.”)]

## History Table

Period	Seller 1	Seller 2	Seller 3	Seller 4	Seller 5	Seller 6	Seller 7	Seller 8	Seller 9	Seller 10
1	2	-	1	4	-	5	7	3	-	6
2	3	7	-	-	2	5	6	-	1	4
3	-	7	-	6	2	-	1	3	4	5

At any time during the experiment, you will be able to see a history table. This table lists the trade partners for every trade that has occurred in the past. You can see the first few rows of a buyer's table above. Each row of this table corresponds to a period of the experiment. The number of the period can be seen in the leftmost column. Each column of the table represents a seller. The IDs of the sellers are shown in the top row. The cells of table for a particular seller show the buyer with whom that particular seller traded in the respective period. For example, in the sample table, seller 5 traded with buyer 2 in period 3. Remember that there are more sellers than buyers, so that in each period, some sellers will not trade. In the history table, this is indicated by a dash (“-”).

The seller's history table looks identical, but the columns here represent the buyers.

## Trade Restriction

At a randomly determined period, which will be between period 10 and period 20, a “**trade restriction**” will come into action. This restriction prevents any buyer from making private offers to the seller with whom he traded in the period before the restriction came into action. Likewise, any seller will be prevented from accepting public offers from the buyer with whom he traded in the previous period. For example, if buyer X traded with seller Y in period 14, and the trade restriction starts in period 15, then buyer X and seller Y will not be able to trade any longer after this period. The following rules apply:

- The period when the trade restriction comes into action is **not known in advance**
- The trade restriction applies only to the buyer/seller with whom you traded in the **period immediately before** the trade restriction came into action; all other buyers/sellers will still be available
- Once the trade restriction comes into effect, you will not be able to trade with this buyer/seller for **all remaining periods** of the experiment
- A buyer cannot select his “restricted” seller for a private offer
- A seller cannot see or accept any public offers from his “restricted”

buyer

The experiment will not commence until all participants are completely familiar with all procedures. In order to make sure that this is the case we ask you to answer a couple of questions that will be displayed on the computer screen. Following these questions we will begin the experiment, which will last for 30 periods.

Do you have any questions?

## C. Field Experiment: Appendix

**Table 7:** Descriptive statistics

	Mean	Sd
May	0.673	0.470
June	0.199	0.399
Industry: cars	0.026	0.160
Industry: bank	0.019	0.137
Industry: chemical	0.023	0.149
Industry: service and admin	0.235	0.424
Industry: trade	0.115	0.320
Industry: tourism	0.007	0.084
Industry: construction/housing	0.086	0.280
Industry: logistics	0.031	0.173
Industry: communication	0.036	0.186
Industry: electro/metal industry	0.151	0.358
Industry: food industry	0.014	0.119
Industry: legal	0.036	0.186
Industry: public administration	0.031	0.173
Industry: insurance	0.012	0.109
Industry: travel agency	0.005	0.069
Industry: health service	0.023	0.149
Industry: hospital	0.031	0.173
Industry: transport	0.007	0.084
Industry: fiduciary	0.096	0.295
Industry: other	0.017	0.128
Legal: public or ngo	0.088	0.284
Legal: LLC	0.877	0.328
Legal: other	0.035	0.183
Employment agency	0.170	0.376
Part-time job	0.175	0.380
Avg. ln(driving distance)	9.704	1.322
Male HR person	0.321	0.467
Male applicant	0.487	0.500
Applicants per vacancy	9.709	5.013
Local unemployment rate	2.781	0.406

**Table 8:** Regression analysis: alternative callback definition

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Four Employers	-0.033** (0.014)	-0.032* (0.017)	-0.032* (0.017)	-0.032* (0.017)	-0.033* (0.017)	-0.032* (0.017)	-0.055*** (0.019)
Four Employers X wave 2012		-0.003 (0.028)	-0.003 (0.029)	-0.004 (0.029)	-0.005 (0.029)	-0.004 (0.029)	0.007 (0.029)
Wave 2012		0.068** (0.032)	0.051 (0.034)	0.059* (0.033)	0.031 (0.036)	0.046 (0.035)	0.052 (0.035)
Industry experience							0.106*** (0.037)
Constant	0.292*** (0.016)	0.264*** (0.020)	0.280*** (0.040)	0.723*** (0.180)	0.660*** (0.237)	0.469* (0.284)	0.458 (0.283)
Additional controls?							
Month FE			Yes		Yes	Yes	Yes
Gender match FE			Yes			Yes	Yes
Firm / job charact. FE				Yes		Yes	Yes
ln(driving distance)				Yes	Yes	Yes	Yes
Labor market					Yes	Yes	Yes
Observations	1680	1680	1680	1680	1680	1680	1680
$F$	5.881	3.829	5.165	9.672	4.570	6.923	5.979
Prob> $F$	0.016	0.010	0.000	0.000	0.000	0.000	0.000

This table shows OLS coefficient estimates (standard errors adjusted for clustering at the job advertisement level are reported in parentheses). The dependent variable is a dummy indicating a callback (alternative definition, including requests for additional documents). “Four Employers” is a dummy for treatment Four Employers. “Wave 2012” is a dummy for the first wave of the experiment in 2012. “Industry experience” is a dummy indicating whether the applicant has had some previous work experience in the corresponding industry. “Month FE” contains dummies for the month when the application was sent. “Gender match FE” includes dummies for gender of the applicant and the HR person and the corresponding interaction term. “Firm/job charact. FE” includes industry dummies, legal form dummies, employment agency dummy and part-time job dummy. “ln(driving distance)” is the log of the distance in meter by car, calculated with Google Maps. “Labor market” contains the monthly local unemployment rate and number of applicants per open position, based on the statistics from the State Secretariat for Economic Affairs (SECO). Significance levels are denoted as follows: \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## D. Survey Experiment: Appendix

**Table 9:** Descriptive statistics for the participants who stated they were actively involved in the assessment of job candidates.

Variable	Mean	Median	Industry	#
Firm size (employees)	24'892	1'300	Plant Engineering/-Construction	14
Staff at booth	3.5	3	Electrical Ind./Electronics	12
# resumes/month	54.5	30	IT / Telecom	10
Years HR experience	6.7	5	Consulting	12
% female	59	—	Mechanical Engineering	8
Age (10-year bracket)	—	25–35	Chemical Ind./Pharma	5
Sample size	83		Medical Technology	3
			Financial Services/Banking	3
			Optomechanics	2
			Consumer Goods	2
			Other	12
			Total	83

**Table 11:** Difference in ratings of the 10 different characteristics (One Employer Rating rating minus Four Employers rating), mean and p-value of paired t-test.  $N = 83$ .

Characteristic	Mean Diff.	p-value (corr.)
patient	1.24	<0.001***
reliable	0.77	<0.001***
teamwork	0.40	<0.001***
honest	0.27	0.199
skilled	0.19	0.229
willing to adapt	0.34	0.299
goal-oriented	-0.17	0.989
self-directed	-0.07	1.000
multi-talented	-0.05	1.000
experienced	-0.05	1.000

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ , Holm-Bonferroni correction.