# Perceived Wage Differences and Early Career Job Search Choices

Asker Nygaard David  Nikolaj Arpe Harmon
Sonja Settele  Daphné Skandalis

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**Abstract**

Misperceptions about the relative wages offered in different jobs may distort job search, especially at labor market entry. We survey early career job seekers in Denmark and link responses to administrative data. Our survey i) elicits beliefs about typical wages in three relevant jobs, ii) contains a randomized information treatment revealing actual typical wages, and iii) elicits planned search behavior and beliefs about own potential wages. Comparing beliefs about typical wages to administrative data reveals large relative misperceptions. In 80% of cases, the perceived wage gap between two jobs differs from the truth by more than 50%. Using the information treatment, we show that these misperceptions causally affect search intentions and realized post-survey wages in administrative data. Interpreting estimates through a discrete choice model, we find that 9.5% of job applications are misallocated due to wage misperceptions, resulting in the same welfare loss as a 0.8% reduction in all wages.

**JEL-Codes**: J31, J62, J64, D83, D84

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**Contact**: Asker Nygaard David, University of Copenhagen, anc@econ.ku.dk. Nikolaj Harmon, University of Copenhagen, nikolaj.harmon@econ.ku.dk. Sonja Settele, University of Cologne, ECONtribute and MPI for Behavioral Economics, settele@wiso.uni-koeln.de. Daphné Skandalis, University of Copenhagen, daphne.skandalis@econ.ku.dk. **Acknowledgements**: We thank Sydnee Caldwell, Michele Belot, Teodora Boneva, Ingrid Haegele, Philipp Kircher, Roland Rathelot, Jeremias Klaeui and Michael Siegenthaler. Georgios Louvaris, Annasofie Marckstrøm Olesen, Sophie Laura Bune, Dánjal Hátún Augustinussen, Carl Benedikt Huels, and Emma Thevissen provided excellent research assistance. **Funding**: Financial support through the Rockwool Foundation (project: 3039) is gratefully acknowledged. Settele: Funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany's Excellence Strategy – EXC 2126/2-390838866. The activities of CEBI are funded by the Danish National Research Foundation (Grant DNRF134) **Ethics approval:** Ethics approval was received from the IRB of the University of Copenhagen. **Pre-registration:** The data collection was pre-registered at the AEA RCT Registry (`https://www.socialscienceregistry.org/trials/11592`). A previous version of this paper was circulated as Rockwool Foundation Study Paper 279 under the title 'Relative Wage Misperceptions and Early Career Job Search'.

# 1 Introduction

The first jobs workers take durably shape their careers.[1] With little experience to draw on, young workers must choose among a range of relevant jobs that differ in wages and other characteristics. In standard models of job search, workers perceive these wage differences correctly and use them to search optimally. If young workers in practice misperceive wage differences across jobs, this may distort their search and lead to misallocation. Some workers may apply to and get hired in suboptimally low-paying jobs. Other workers may apply for high-paying jobs even though they would be better off targeting lower-paying jobs with more attractive non-wage amenities or a greater likelihood of being hired.

In this paper, we examine the extent and costs of relative wage misperceptions among early career workers. To do this, we uniquely combine both a survey experiment, linked administrative data, and a theoretical job search framework. Concretely, we conduct a new survey of 1,902 early career job seekers in Denmark, which we link to administrative data on individual wages and job outcomes. For each respondent, we construct three personalized job types that represent typical career options given their educational background. These are defined by salient dimensions such as sector, industry, occupation, and firm size. For each job type, the survey first elicits incentivized beliefs about the average wage earned by prior cohorts from the same educational background as the respondent. Comparing answers to ground truth wage data from administrative records allows us to directly measure job seekers' misperceptions about relative wages in each job. Next, the survey contains a randomized information treatment where treated job seekers are shown the actual average wages for prior cohorts in each job type. By subsequently eliciting job seekers' expectations about the wages *they themselves* would earn in each job type along with their planned application behavior, this information treatment allows us to identify the causal effect of correcting misperceptions on job applications. Crucially, by linking to administrative data on realized job outcomes after the survey, we validate that these changes in planned behavior in the survey translate into actual changes in job outcomes. Finally, we use the

[1] The literature highlights the persistent effects of graduating in a bad labor market (e.g. von Wachter and Bender (2006), Kahn (2010), Cockx and Ghirelli (2016), von Wachter (2020)) and of getting a first job at a large firm Arellano-Bover (2024).

close link between our survey measures and a theoretical job search framework to quantify the overall costs of relative wage misperceptions.

The first part of our analysis provides novel evidence about the extent and nature of relative wage misperceptions, i.e. misperceptions about how offered wages differ across jobs. Using survey responses about average wages for prior cohorts, we compute job seekers' perceptions of the typical wage gap between each of the three jobs in the survey. We then compare these to the actual gaps in wages among prior cohorts in administrative data. Perceived and actual wage gaps are clearly related (regression slope 0.54; correlation 0.27), suggesting that job seekers are broadly aware of wage differences across jobs. At the same time, substantial relative wage misperceptions exist: In 80% of cases, the perceived wage gap between two jobs differs from the truth by more than 50%. While there is substantial heterogeneity, the typical pattern is that job seekers understate the true gap in wages so that lower-paying jobs appear relatively more attractive. Approximately 65% of wage gaps are underestimated, with 55% underestimated by more than 50% and 36% underestimated by more than 100%—meaning respondents actually perceive the lower-paying job to pay higher wages. This pattern of predominantly underestimated wage gaps holds at the individual level as well: two-thirds of job seekers in our sample are underestimators, in the sense that they underestimate the gap between the highest-paying job and the other two jobs.

We substantiate that these measured misperceptions are practically meaningful in a number of ways. Since we focus on beliefs about an objective benchmark—realized average wages for prior cohorts—the observed misperceptions cannot be explained by respondents having private information about their own situation or about labor market conditions. Yet, we verify in the data that prior cohort wages are indeed relevant for early career workers: beliefs about typical wages for prior cohorts are strongly correlated with beliefs about the potential wages job seekers themselves expect to face. Examining alternative measures and subsamples, we also find no evidence that measured misperceptions are driven by job seekers misunderstanding the three jobs asked about in the survey or not viewing these jobs as relevant for themselves. In line with theories of rational inattention, we find some evidence that job seekers are better informed when true wage gaps are larger and thus more consequential. However, misperceptions are large in these cases as well.

In the second part of our analysis, we leverage the randomized information treatment to establish the causal effects of relative wage information on job search. After job seekers have reported their beliefs about wages for prior cohorts, treated job seekers are shown the actual wages for prior cohorts, allowing them to infer their misperceptions about the different jobs. Using post-treatment survey questions, we show that this causes job seekers to update beliefs about the wage differences they themselves expect to face and also changes their planned application behavior accordingly. When job seekers are informed that the wage gap between two jobs for prior cohorts is in fact $X\%$ larger than they thought, the perceived gap in wages that the job seekers themselves expect to face increases by $0.3X\%$. In turn, the relative likelihood of applying to the two jobs changes by $1.2X\%$.

Combining the reduced form treatment effects on wage beliefs and application behavior, we estimate directly how beliefs about own potential wages affect job search. Formally, this amounts to a 2SLS estimator where the information shock from our treatment serves as the instrument for the perceived gap in potential wages between jobs. We find a large effect: a 1% change in the perceived wage gap between two jobs changes the relative likelihood of applying by 4.2%. This 2SLS estimate can be interpreted as the pure effect of relative wage misperceptions under an exclusion restriction: Our information treatment should *only* affect applications by changing beliefs about potential wages but should not affect beliefs about non-wage amenities or the likelihood of getting the job if applying. Our survey allows us to test this assumption using a range of post-treatment questions. We find no evidence that the information treatment affected any non-wage beliefs.

Using the linked administrative data, we next analyze the effect of the information treatment on post-survey job outcomes. This allows us to verify that the treatment effects on planned applications in the survey also translate into actual labor market behavior. The survey results yield clear directional predictions for post-survey job outcomes: For the majority of job seekers, who tend to underestimate true wage gaps, misperceptions make lower-paying jobs appear relatively more attractive. The information treatment should therefore shift such job seekers to apply for and get hired into higher-paying jobs, with the reverse holding for those who overestimate true gaps. We confirm these predictions in the administrative data. Among the two-thirds of job seekers who are underestimators, the information treatment significantly increases the actual wages earned in the first post-survey job, while we see evidence of a negative effect for overestimators. As expected, these wage effects are driven by job seekers still actively searching at the time of the survey, who can adjust their behavior to new information without incurring additional switching or search costs.

Finally, we provide a quantification of the overall costs of relative wage misperceptions among early career workers. To this aim, our survey and reduced form analyses were deliberately set up to identify a simple discrete choice model of job search. We use this to compute each individual's counterfactual search behavior and hiring outcomes without relative wage misperceptions. We find that current relative wage misperceptions distort 9.5% of job applications and on average lowers welfare by the same amount as an across-the-board wage decrease of 0.8% in all jobs. As expected, the nature of this welfare loss is highly heterogeneous and reflects two different forms of misallocation: For job seekers who underestimate actual wage gaps, welfare losses reflect that misperceptions push them to apply for too many lower-paying jobs. For job seekers who overestimate wage gaps, welfare losses reflect that they apply for too many high-paying jobs with low hiring likelihoods and poor non-wage amenities.

This paper expands on the literature studying job search and workers' misperceptions about employment opportunities. A main strand of this literature has examined misperceptions about the rate of job finding or the overall level of reemployment wages and found that such misperceptions may prolong unemployment spells (e.g. Krueger and Mueller (2016), Spinnewijn (2015), Mueller et al. (2021), Belot et al. (2019), Belot et al. (2025), Behaghel et al. (2024), Altmann et al. (2022), Altmann et al. (2025)).[2] Our paper differs fundamentally from this work by studying misperceptions about *relative* wages across jobs and by showing that these generate costs beyond excessive unemployment durations because they distort the allocation of workers to jobs. Another strand of the literature asks whether wage misperceptions affect mobility among *employed* workers (Jäger et al., 2024)

[2]Some papers have also investigated the impact of wage misperceptions on other outcomes such as gender inequality (Roussille, 2024) or measures of willingness-to-pay for amenities (Andresen et al., 2026).

and/or whether high switching costs restrict mobility even when workers know of outside jobs paying higher wages (Caldwell et al., 2025). We focus instead on the *initial* allocation of workers across jobs at labor market entry—a primary determinant of lifetime earnings. Here misperceptions likely play a larger role: because early career job seekers face lower switching costs, wage information bears directly on the decision in front of them.

We also relate to a literature studying how much increases in offered wages affects job seekers likelihood to apply for a job, i.e. the own wage elasticity of applications to a job. Prior studies analyzing wage variation in online job ads typically find elasticities below 1 (Holzer et al., 1991; Dal Bó et al., 2013; Banfi and Villena-Roldán, 2019; Marinescu and Wolthoff, 2020; Dube et al., 2020; Belot et al., 2022; Azar et al., 2022; Beerli et al., 2025). The results from our information treatment imply a markedly larger elasticity of around 3. One suggestive interpretation is that job seekers in online job-ad settings may interpret wage premiums as signals of reduced hiring probabilities or lower non-wage amenities. Since we see no indications that our wage information treatment had such signaling effects, the elasticity we obtain may be larger because it captures the pure effect of wage beliefs.

Finally, our paper is connected to the literature studying the determinants of college major choices (e.g., Betts (1996); Arcidiacono et al. (2012); Wiswall and Zafar (2015b,a); Conlon and Patel (2025)). We complement this literature by focusing on the choice of career path once education is completed. While expected wages in different majors seem to have limited effects on major choice (Hastings et al., 2016; Ballarino et al., 2022; Bonilla-Mejía et al., 2019; Kerr et al., 2020), our findings suggest that career choices at the job entry stage are much more responsive to job-specific wage expectations. Methodologically, the link between our information treatment and discrete choice model is also closely related to the study of major choice in Wiswall and Zafar (2015a).

The rest of the paper is organized as follows. Section 2 discusses the survey design and data. Section 3 provides novel descriptive evidence about misperceptions of wage differences across jobs. Section 4 establishes the causal effects of these misperceptions on job search. Section 5 quantifies the costs of relative wage misperceptions using a discrete choice model. Section 6 concludes.

# 2 Data and survey design

To measure relative wage misperceptions, we fielded a survey with early career job seekers in Denmark in the summer of 2023. Below, we first describe our setting and the linked administrative data that we use. We then describe the survey data in detail. At the end, we discuss the reliability of our survey measures and present a range of validity checks.

## 2.1 Empirical setting

Our analysis focuses on graduates from higher education in Denmark. We focus on graduates early in their careers since we expect relative wage misperceptions to be particularly prevalent and consequential when workers have limited labor market experience and may face persistent effects from early job choices. Our focus on Denmark allows us to leverage rich administrative registers in the design, sampling and analysis of the survey. A few other features of the Danish setting are important as well. While most jobs in Denmark are covered by collective bargaining agreements, wage setting is highly decentralized, thus generating substantial wage dispersion across jobs (Dahl et al., 2013). As in most countries, higher education degrees in Denmark typically qualify candidates for a number of distinct careers, meaning graduates face a choice between a set of different jobs and careers, often offering very different terms and wages. Whether workers correctly perceive these wage differences across jobs is an empirical question: On the one hand, workers have limited experience with wages early in their career and—as is the case in many countries—direct information about wages is rarely included in job postings. On the other hand, there are many sources of wage information that active job seekers could rely on in the absence of information costs or behavioral biases.[3] Finally, an important feature of the Danish setting is that graduates can claim unemployment benefits immediately upon graduation. As we discuss below, high-frequency claims data therefore help us survey workers right at the start of their job search.

[3]For example, many Danish workers' unions publicize information about typical wages in various categories of jobs.

## 2.2 Administrative data

We use administrative data for two main purposes in this project. First, we use it in the design of our survey to identify which types of jobs are relevant for graduates from different degrees, and to construct ground truth measures of the typical wages that these jobs have paid in the past. This is based on the transitions of higher-education graduates into their first jobs, which we observe using individual-level data on labor market outcomes (BFL) and educations (UDDA) for individuals who graduated between 2010 and 2018. Second, we link our survey sample to administrative data to obtain background characteristics and post-survey labor market outcomes. Specifically, we link the Danish registers on individual labor market outcomes (BFL), educations (UDDA) and government transfers (DREAM) up to December 2024.

## 2.3 Definition of job types

Throughout the survey, we ask each respondent about three types of jobs relevant for young workers with their educational background. Below we summarize how we define these job types. Additional details are given in Appendix C.1.

A strength of our research design is that we analyze graduates not from a single degree but from a wide range of educational backgrounds. Since different jobs and career paths are relevant for graduates from different backgrounds however, this requires us to create tailored job types. For each educational background (measured using 6-digit Danish ISCED codes), we construct these tailored job types to achieve three objectives: First, the job types must be relevant and capture a substantial share of respondents' realistic employment opportunities. Second, they should differ in the wages they offer to ensure meaningful trade-offs. Third, they must correspond directly to categories in administrative records so that we can compute objective benchmarks for typical wages using administrative data.

In practice, for each educational background, we define the three job types by grouping jobs based on some combination of occupation, industry, firm sector, and firm size. We implement a data-driven procedure to select the appropriate job groupings using administrative data on the first jobs of graduates from the same educational background between

2010 and 2018: For each educational background, we consider all possible ways of grouping jobs by firm sector and size (public/private, above or below 50 employees), occupation codes (using either 3, 4, or 6 digit Danish ISCO codes), and/or industry codes (using either 1- or 2-digit Danish NACE codes). Among all such possible groupings, we then select those that satisfy two conditions: i) the three most common job types must cover more than 40% of the transitions into first jobs for prior graduates and ii) the third most common job type must cover at least 5% of transitions. These conditions ensure that the three most common job types altogether cover a substantial share of relevant jobs and that none of them are too narrow. If multiple groupings satisfy the two conditions, we choose the grouping that best predicts entry-level wages.

Some smaller educational backgrounds cannot be associated with three meaningful job types using this procedure, either because they concern too few graduates or because their graduates all end up in virtually the same type of job. Graduates from these educational backgrounds were automatically excluded from our study.[4] In the end, our analyses focus on 88 higher-education backgrounds, covering 76% of higher-education graduates in 2010-2018 (see Appendix Table A1). We refer to these as the "selected" educational backgrounds.

After hand-checking the resulting job types and making marginal adjustments, this procedure gives us three job types for each educational background that we can describe intuitively to the survey respondents. For example, for respondents with a Master in Economics, the procedure selects a grouping by sector (public vs. private) and 1-digit industry, producing three job types: (1) private-sector jobs in financial and insurance activities, (2) public-sector jobs in public administration and defense, and (3) private-sector jobs in business services. For respondents with a Master in Physics, it instead selects a grouping by sector, firm size, and 3-digit occupation, producing: (1) research and teaching in higher education, (2) teaching at the upper-secondary level, and (3) software development in large private firms. The two examples illustrate how the procedure adapts to each educational background's labor market: industry distinctions matter more for economists, while occupational distinctions matter more for physicists. Table C1 lists the

[4] Graduates from these educational backgrounds could receive a survey invitation but their version of the survey did not ask about detailed job types and did not contain an information treatment.

exact job descriptions shown to respondents and Appendix C.1.1 provides the full list of educational backgrounds and job types. Because the procedure is designed to produce job types that are meaningful to respondents, we included several questions in the survey to assess how well respondents understood them. As discussed in Section 2.6, the responses confirm that the job types were generally well understood, and also allow us to provide robustness analyses restricted to the subsample of respondents for whom comprehension was strongest.

## 2.4 Sampling procedure

It is important for us to survey workers immediately around the time when they actively search. Otherwise, we may overstate the extent of misperceptions, if workers only seek out detailed wage information right when they need it. Using two sampling procedures, our survey targeted individuals who are either looking for their first job after graduation or are entering unemployed job search within a few years following their graduation. First, we collaborated with the Danish Agency for Labor Market and Recruitment (STAR) to contact all individuals below age 40 who initiated a claim for unemployment benefits during the summer of 2023. Since most education programs end in the summer and many graduates immediately claim benefits, sampling during these months allows us to reach a large fraction of new graduates.[5] Second, we collaborated with the University of Copenhagen (UCPH) to contact all UCPH students about to finish their Master's degree in the summer of 2023. We invited them into the survey in mid-June 2023, i.e. right before typical Master's graduation dates. This second sampling procedure allows us to also survey graduates who enter the labor market without going through unemployment.

Sampled individuals were invited to the survey in three waves via the so-called "e-Boks" e-mail system—the official channel through which government agencies in Denmark communicate with citizens.[6] The invitation contained an individualized link to the online

[5]Økonomi og Indenrigsministeriet (2018) report that about half of Danish graduates receive unemployment benefits within 6 months of graduation.

[6]The first wave in mid-June included all individuals sampled from UCPH, as well as new unemployment benefit claimants from the past three weeks. The second and third wave were conducted three and eight weeks later and each included only benefit claimants from the preceding three weeks. Due to an error by our "e-Boks" provider, some observations were lost in the second wave, as explained in Appendix D.1.

survey and provided a monetary incentive to participate.[7] Appendix Table A2 provides descriptive statistics for our sample of invitees and participants. 22,461 people from our selected educational backgrounds were invited to participate in our survey (Column (2)); 1,902 completed the survey and are included in our study sample (Column (1)). Appendix Table A2 shows that the sample of all invited individuals is quite similar to our study sample in their demographic characteristics, although individuals in the study sample do tend to have higher educational attainment (63% have completed a Master's degree) than the invited population (46% with a Master's degree)—a common phenomenon in online surveys (Haaland et al., 2023). Moreover, survey participants are slightly more likely to have graduated less than a year before the survey (66%, compared to 44% in the invited population). A similar picture emerges when we compare our study sample to the full population of young job seekers. As we show in Table 3 and Appendix Table A4, our conclusions are unchanged if we reweight the study sample to account for non-participation.

## 2.5 Survey questions

This section describes the key elements of the survey. Additional details, including the full survey instructions, are provided in Appendix C.2.

**Beliefs about prior-cohort average wages** The first part of the survey aims to measure how well informed respondents are about typical wage differences across their possible career paths. Chiefly, we elicit respondents' beliefs about the entry-level *average wage received by prior cohorts* in the three different job types. For each job type, we ask respondents to consider people who graduated in 2010-2018 from the same educational background as themselves and started working full-time in this job type. We then ask them to report the average pretax monthly wages over the first year of employment for these individuals. To help respondents understand the wage concept we use, we show them the average wage received by prior cohorts with *any* higher education diploma and in *any* job type. This anchor contains no information about wage differences across jobs but clarifies the scale

[7] Among those who finished the survey, we raffled off 20 gift cards worth DKK1,000 (approx. USD160) each.

on which we measure wages (e.g. monthly vs. hourly/yearly wages) and thereby reduces potential measurement error in beliefs (Ansolabehere et al., 2013).[8] To incentivize effort and truthful reporting, we provide a monetary incentive to get close to the true value of the average wages computed in administrative data: the three closest answers received a gift card worth DKK1,000.

In a similar way, we also elicit respondents' beliefs about two other statistics for prior cohorts: the average wage earned 5 years after starting in each job type, and the rate of successful applications sent to each job type. Along with a range of socioeconomic questions at the start of the survey, these beliefs elicited before the information treatment serve as predetermined covariates in various parts of our analysis (cf. Table 1).

**Randomized information about prior-cohort average wages** Next, the survey includes a randomly assigned information treatment. The treatment group is informed of the true average prior-cohort wages in each of the three job types. This information is illustrated by a figure listing the true average prior-cohort wages in the three job types alongside the respondent's own answers. The control group is shown a corresponding figure only containing their own answers (see Appendix Figures C1-C2).

**Planned search behavior and beliefs about own potential wages** After the information treatment, we ask respondents to think of three specific jobs that they view as representative of each of the three job types asked about in the survey. We then elicit their planned search behavior and beliefs about what they themselves expect to earn in these jobs as described below.

To measure search behavior, we ask respondents to consider a situation in which they have found job postings for each of the three jobs and are able to apply to at most one of them. We then ask them to report the likelihood that they would end up applying to each job posting or to none of them. Answers are reported as probabilities, recorded using four sliders that range from 0.01-0.97 and automatically adjust to make the probabilities sum to 1. Eliciting self-assessed probabilities instead of a single deterministic choice offers two

[8] As we show in Appendix Figure A5, there is no evidence of bunching around the anchor.

advantages here (Wiswall and Zafar, 2018). First, it increases the information content of the data because respondents' answers express the strength of their preferences for applying to different jobs rather than just their highest ranked option. Second, it implies that the respondents' answers can be mapped directly to a standard discrete choice framework with decision uncertainty. We use this to estimate the costs of relative wage misperceptions in Section 5. As an alternative measure of job attractiveness, respondents are also presented with a scenario in which they simultaneously receive mutually exclusive job offers from all three jobs and are then asked to report the likelihood of accepting each offer.

Job seekers' application decisions should depend on the potential wage they expect to face in each job type. To elicit beliefs about such *own potential wages*, we next ask respondents to report what starting monthly pretax wage they expect they would earn if hired into each of the three jobs.

Finally, we ask a range of questions about other job characteristics the job seeker expected to face in each of the three jobs. These include work hours, earnings five years in the future, the probability of receiving an offer if applying, and how well they expected to perform and get along with colleagues (both on 6-item Likert scales). As a summary measure of the perceived attractiveness of non-wage amenities in the three jobs, respondents are also presented with an alternative version of the scenario in which they simultaneously receive job offers from all three jobs. In the alternative version, all jobs involve the same wage. As above, job seekers are then asked to report the likelihood of accepting each job offer. As we expand on in Section 4.4, we use these additional questions to assess belief spillovers from the information treatment.

**Winsorization** Typed numerical survey responses are prone to generating outliers. We winsorize all such variables at the 2.5th and 97.5th percentiles prior to our analyses. This includes survey-reported wages, hours and application success probabilities.

## 2.6 Discussion of survey data reliability

**Understanding of job types** A crucial question for our survey design is whether respondents properly understood the three job types they were asked about. We validate this in two ways. First, we directly asked respondents how well they understood the job types: 75% of respondents reported that their understanding was good or very good (Appendix Figure A1). Second, we tested their understanding at the end of the survey by asking some of them to place a concrete job example into one of the three types: 76% of the respondents answered correctly (Appendix Figure A2).[9] As we show later, our results are also robust to excluding respondents showing poor understanding in these questions (Tables 3 and A4).

**Relevance of job types** Another premise for our analysis is that the three survey job types are relevant to respondents and represent a trade-off in the wages they offer. In Table 2 we provide detailed descriptive statistics on the job types based on both administrative and survey data. Columns correspond to different variables and data sources, while panels correspond to different ways of grouping the job types across respondents. In Panel A, we group job types according to the share of prior cohorts who started their first job within each type. As expected, the job types cover a substantial share of starting jobs: the most common job type on average covers 26% of starting jobs for prior cohorts, the second most common 15% and the least common one 10% (Column (1)). In Panel B, we instead group jobs according to each respondent's stated likelihood of applying to the jobs in our survey. Answers confirm that the job types capture relevant employment options considered by job seekers. The reported likelihood of applying for their most preferred job is 62% on average, suggesting that most job seekers have a clear leaning towards one of the job types (Column (2)). The likelihood of applying for their second and third most preferred job types are 22% and 7%, showing that job seekers still have substantial uncertainty about their choice across the three jobs. The generally high reported likelihoods of applying also underscore that the job types were perceived as relevant by respondents; the average reported likelihood of not applying to any of the three job types was only 8%. Finally, in Panel C we group job types according to the average wage paid to prior cohorts and see substantial differences. For the average respondent, typical wages in the highest-paying job are 14% (4,180 DKK) higher than in the lowest paying one (Column (3)).

[9] For details on the creation of the examples, see Appendix Section C.1.2.

**Prior cohort wages versus own potential wages** Our survey elicits respondents' beliefs about two distinct wage concepts for each of the three job types: average entry-level wages for prior cohorts (elicited before the information treatment) and respondents' own potential entry-level wages (elicited after the information treatment). We focus on prior cohort entry-level wages because they constitute a useful benchmark for the typical entry-level wages that respondents themselves should face which we can compute objectively from administrative data. Contrasting administrative data with elicited beliefs about prior cohorts therefore allows us to construct an individual-level measure of wage misperceptions which is not confounded by job seekers having correct private information about their own labor market opportunities.[10] When making search decisions about which jobs to target, what should matter to job seekers is the potential wages they themselves would face in the different jobs. We therefore elicit these beliefs after the information treatment. This allows us to study the causal effect of wage beliefs on job search decisions using randomized treatment variation.

**Planned applications versus real transitions** Whenever survey respondents report their planned behavior, a critical question is how likely it is to reflect their actual future behavior. With the linked administrative data, we can validate our survey measure of planned job applications by comparing it to respondents' actual job transitions after the survey. We present these validation exercises in Appendix Figure A3 and Appendix Table A8 and find a strong association. Comparing job seekers from the same educational background, a 1 percentage point increase in the reported likelihood of applying to some job type is associated with a 0.25 percentage point increase in the likelihood of being hired into this job type later. Conditional on being hired into one of the three job types covered by the survey, this association increases further to 0.66. Moreover, we show in Section 4.5 that the estimated effects of our information treatment on planned applications also translate to changes in actual hiring outcomes in administrative data.

[10]Measuring misperceptions using elicited beliefs about own potential wages raises a fundamental identification problem: the actual wage a job seeker *would* earn in a given job is only observed if the job seeker in fact ends up in this job. Trying to infer these unobserved actual wages from data on other workers will lead to erroneous measures of misperceptions if job seekers have correct private information about how their wage prospects differ from other workers.

**Randomization and timing of survey** Table 1 shows descriptive statistics for all pre-treatment covariates elicited in the survey, both for the full study sample and for the treatment and control groups separately. Comparing treatment and control groups, treatment randomization appears to have been successful; across the 30 pre-determined covariates, we find statistically significant treatment-control differences for two variables at the 10% level. As shown later, all results are also robust to the inclusion of pretreatment controls. A key objective of our survey design was to measure wage beliefs around the time of active job search. Table 1 confirms that we were successful in reaching respondents at this time. 65% of respondents report actively searching at the time of the survey and another 24% report having just secured a new job or being about to start searching. In total, 89% of respondents were thus either actively searching, had recently finished, or were about to start searching. As we show later, all results are also robust if we restrict the data to only include respondents who report actively searching at the time of the survey. Appendix Table A3 shows descriptive statistics and balance tests for this sample.

# 3 Measuring relative wage misperceptions

In this section, we provide descriptive evidence on the extent and nature of wage misperceptions. We do this by contrasting workers' beliefs about the wages for prior cohorts in different jobs with administrative data on the true wages earned by these cohorts.

## 3.1 Misperceptions about wage levels in individual jobs

While the focus of this paper is on misperceptions about relative wages across jobs, we begin by briefly considering general misperceptions about the level of wages. The analysis sample consists of 1,902 job seekers indexed by $i$, each of whom answered questions about three different jobs indexed by $j$. For each individual-by-job, $i, j$, our survey data contains the belief about the average wage earned by prior cohorts $\widetilde{V_{i,j}}$, which we compare to the true value $V_{i,j}$ computed from administrative data. Note that throughout the paper, we use tilde (e.g. $\widetilde{V_{i,j}}$) to denote beliefs and lowercase letters to denote logged variables (e.g. $v_{i,j} = \log V_{i,j}$).

For each job seeker and job, Figure 1 compares respondents' beliefs about the average wage earned by prior cohorts to the true values. Panel (a) shows a binscatter plotting the perceived log wage ($\widetilde{v_{i,j}}$) against the truth ($v_{i,j}$). There is a clear positive relationship but it is not one-to-one; the corresponding regression slope is 0.65. This is consistent with recent evidence in Caldwell et al. (2025): people's beliefs are highly but not perfectly correlated with true wages. Second, Panel (b) shows the full distribution of wage misperceptions, measured as the difference between the true and perceived wage in percent of the true wage, i.e., $\left(V_{i,j} - \widetilde{V_{i,j}}\right)/V_{i,j}$. Misperceptions about wages are substantial and heterogeneous. For some individuals and jobs, wages are overestimated ($V_{i,j} - \widetilde{V_{i,j}} < 0$), while for others they are underestimated ($V_{i,j} - \widetilde{V_{i,j}} > 0$). Wage overestimation dominates overall: wages are overestimated in 60% of cases, and the mean overestimation rate is 3.3%. This is in line with findings from the literature on misperceptions about general wage levels. Altmann et al. (2025) find that unemployed Danish job seekers overestimate re-employment wages of comparable workers by about 2% on average.

## 3.2 Relative wage misperceptions

Next, we turn to the key focus of our paper: misperceptions about wage *differences* across jobs. When job seekers decide which jobs to target in their search, it is these misperceptions about *relative wages* that should matter. Importantly, results on misperceptions about wage levels (such as those in Figure 1) say little about relative misperceptions: If a worker overestimates wages in all jobs to the same extent for example, misperceptions about wage levels can be large while there are no misperceptions about wage differences.[11] To measure these relative wage misperceptions, we focus on wage gaps between pairs of jobs. For each job seeker, we form all possible pairwise comparisons between the three jobs they were asked about, resulting in dyadic data where observations are individuals-by-job pairs, $i, j, j'$. Letting $\Delta$ denote differences in variables across jobs, our survey data allows us to compute job seekers' perceived wage gap between jobs $j$ and $j'$ for prior cohorts, either in

[11] Conversely, if a worker overestimates the wage in low-paying jobs and underestimates wages in high-paying jobs, misperceptions about wage differences can be large while misperceptions about wage levels are modest.

levels, $\Delta\widetilde{V_{i,j,j'}} = \widetilde{V_{i,j}} - \widetilde{V_{i,j'}}$, or in logs, $\Delta\widetilde{v_{i,j,j'}} = \widetilde{v_{i,j}} - \widetilde{v_{i,j'}}$. We compare these to the true gaps from administrative data, $\Delta V_{i,j,j'} = V_{i,j} - V_{i,j'}$ and $\Delta v_{i,j,j'} = v_{i,j} - v_{i,j'}$.

Figure 2 compares respondents' beliefs about prior cohort wage gaps across jobs to the true prior cohort gaps. Panel (a) shows a binscatter plotting perceived gaps in log wages ($\Delta\widetilde{v_{i,j,j'}}$) against true gaps ($\Delta v_{i,j,j'}$), based on respondent-by-job pairs $(i, j, j')$ ordered such that $\Delta\widetilde{v_{i,j,j'}}$ is positive. Again, we see that beliefs are strongly but not perfectly correlated with the truth; the corresponding regression slope is 0.54.

Panel (b) characterizes the full distribution of relative wage misperceptions. While we focus on log wage gaps both in Panel (a) and the regression analyses presented later, our preferred measure when describing the distribution of misperceptions is the *underestimation rate*, i.e., the difference between the true and perceived gap in percent of the true gap:

$$\frac{\Delta V_{i,j,j'} - \Delta\widetilde{V_{i,j,j'}}}{\Delta V_{i,j,j'}} \tag{1}$$

The advantage of this measure is that it succinctly captures several different qualitative features of misperceptions: An underestimation rate of 0 implies correct beliefs. An underestimation rate between 0 and 100% means that the size of the true gap is being understated. An underestimation rate of 100% implies that the jobs are perceived to pay exactly the same, while an underestimation rate above 100% implies getting the relative wage ranking wrong. Finally, a negative underestimation rate means that the size of the wage gap is in fact being *over*estimated. Panel (b) shows the distribution of the underestimation rate across all individuals and job pairs.[12] In green, we present as a benchmark the CDF that we would observe under perfect information. In blue, the actual CDF highlights that in about 65% of cases, the wage gap is underestimated. Further, we see that 55% of wage gaps are underestimated by more than 50%, while 36% are underestimated by strictly more than 100%—meaning that respondents actually rank the two jobs wrong. We also see that 12% of wage gaps are underestimated by exactly 100%. This corresponds to situations where respondents attributed the exact same wage to two job types even though

[12] Appendix Figure A4 instead shows the raw distribution for the underestimation of the log wage gap, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$. Motivated by theory, this is the misperception measure emphasized in the regression analysis of Section 4.

the jobs in fact paid different wages. Finally, we also see that some wage gaps are instead overestimated, sometimes by a large amount. 25% of wage gaps are overestimated by more than 50% and 20% by more than 100%. Overall, in 80% of cases, the perceived wage gap between two jobs differs from the truth by more than 50%.

In Table 3, we show additional descriptive statistics regarding relative wage misperceptions using different measures and different subsamples of our data. The first row corresponds to the full sample and Columns (1)-(4) simply summarize key statistics regarding the wage gap underestimation rate. In Columns (5) and (6), we consider a different measure: the underestimation of the log wage gap, $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$. Motivated by theory, this is the main misperception measure used in the regression analyses in Section 4.[13] Unsurprisingly, we also find large misperceptions using this measure; its mean absolute value is 0.13 log points, implying that on average job seekers' beliefs about wage gaps are off by 13%. Appendix Figure A4 shows the distribution of this misperception measure across the sample of all job pairs. In Column (7), we show that the pattern of wage gaps being mostly underestimated holds also if we look across individuals instead of job pairs. Classifying each job seeker as either an *overestimator* or *underestimator* depending on whether they overestimate or underestimate the log gap between the highest-paying job and the average of the two other jobs, we find that 67% of job seekers in our sample are underestimators.

**Heterogeneity and robustness** To substantiate that the measured misperceptions are meaningful in practice, the additional rows of Table 3 describe the distribution of relative wage misperceptions, with various sample restrictions and changes in variable definitions.

A possible explanation for the large misperceptions we observe is that some true wage differences may be relatively small; if tracking wage differences is costly for job seekers, theories of rational inattention suggest that they may optimally decide to remain uninformed in these cases.[14] To test this, we restrict our sample to job pairs where the true wage gaps for prior cohorts are above 5% or 10%, respectively. Consistent with rational inattention,

[13]As we return to in Section 5, our reduced form specifications involving log wage gaps can be derived from a simple discrete choice job search model.

[14]For jobs where the true difference in typical wages is small, we may also worry that measured misperceptions stem simply from rounding in responses.

we find evidence that relative misperceptions are indeed smaller for job pairs with larger gaps. However, we continue to see very substantial misperceptions even in these samples. Even restricting to job pairs where the true gap is above 10%, we find that in 63% of cases, the perceived wage gap between two jobs differs from the truth by more than 50%.

The remaining rows dispel a number of other concerns. Measured misperceptions are not driven by rare jobs or by jobs that job seekers do not view as relevant; we see similar levels of misperceptions if we restrict to the two job types where prior cohorts were most frequently hired, or if we restrict to job types where respondents have a high reported likelihood of applying in the control group.[15] Applying a simple procedure to correct obvious reporting errors suggests that such errors do not explain the measured misperceptions.[16] Restricting only to job seekers actively searching at the time of answering the survey, we see that misperceptions are not driven by possible inattention among respondents who have already completed their job search or among graduates who have not started searching actively. Finally, using reweighting or restricting the sample based on measured understanding of job types, we see no evidence that measured misperceptions are affected by survey non-participation or by respondents not understanding the job types in the survey.

A final concern is that relative misperceptions about prior cohort wages could be large simply because they are unrelated to the wages job seekers themselves expect to face and thus are not relevant for job seekers' decision-making. Using our survey data on own potential wages for the control group, Figure 3 plots beliefs about the gap in wages that job seekers *themselves* expect to face across two jobs, $\Delta\widetilde{w_{i,j,j'}}$, against their belief about the gap in prior cohort wages for these jobs, $\Delta\widetilde{v_{i,j,j'}}$. The plot confirms that prior cohort wages are a relevant benchmark for job seekers; there is a clear positive relationship with an estimated regression slope of 0.63. Moreover, as we show in Section 4 below, receiving information about wages for prior cohorts leads job seekers to update their beliefs about the potential wages they will face.

Summing up, we find that young job seekers have substantial misperceptions about wage differences across jobs that they consider in their job search. Most commonly, mis-

[15]Since the likelihood of applying is elicited after the information treatment, we only include the control group when imposing this sample restriction.

[16]Appendix Section D.2 explains the procedure we use to correct errors.

perceptions take the form of underestimating true wage gaps, although overestimation of wage gaps occurs about a third of the time. An implication of this is that relative wage misperceptions cause lower-paying jobs to appear overly attractive for most job seekers but that the opposite also occurs. Over the next sections, we examine the extent to which these misperceptions distort search behavior and job outcomes.

# 4 Effects of relative wage misperceptions on job search

In this section, we examine to what extent relative wage misperceptions causally affect job search behavior. We do this by leveraging exogenous variation generated by our information treatment: treated job seekers were shown actual wages for prior cohorts in the different jobs, which should influence their perceptions about what they themselves would receive in each job. We proceed in four steps. First, we show nonparametric treatment-control comparisons to qualitatively establish the existence of a causal effect. Second, we apply parametric linear regressions to quantify how much beliefs and applications change when workers are informed that they misperceived relative wages by some specific amount. Third, we use a 2SLS framework to collapse these results into an elasticity that captures the responsiveness of job applications to perceived relative wages. Throughout these three analyses, we use the same individual-by-job pair survey data as in Section 3.2. Finally, we use individual-level administrative data to validate the treatment effects on post-survey job outcomes.

## 4.1 Treatment-control comparisons: Does the information treatment affect beliefs and applications?

Our treatment provided information about actual wages for prior cohorts in the different jobs, and therefore about the actual *wage gaps* in prior cohorts between each job pair $(j, j')$. The expected effect therefore depends on job seekers' baseline misperception about these gaps. We measure the baseline misperception of the wage gap between job $j$ and $j'$ as the difference between the actual log wage gap and the perceived log wage gap for prior cohorts (elicited *before* the information treatment), $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$. If this misperception measure is positive, the information treatment reveals to the job seeker that they underestimated the wage gap between $j$ and $j'$ for prior cohorts. In response, we expect the job seeker to upward-adjust the potential wage they expect to receive in job $j$ relative to job $j'$. Assuming that beliefs about wage gaps affect job search, we also expect them to become more likely to apply for job $j$ relative to job $j'$.

Column (1) of Table 4 confirms these predictions using simple treatment-control comparisons. Reordering our job pairs so that $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}} \geq 0$ for all included observations, we regress our outcomes of interest on a treatment dummy, $T_i$.[17] Since treatment was assigned at the individual level, we use cluster-robust inference clustered on individuals throughout all analyses. In Panel A, the outcome is the gap in (log) potential wages that the respondent expects to face, $\Delta \widetilde{w_{i,j,j'}}$ (elicited *after* the treatment). We find a significant effect: informing job seekers that they underestimated prior cohort wages in job $j$ relative to job $j'$ increases the potential wage they themselves expect to face in $j$ relative to $j'$ by 4.2% on average. In Panel B, the outcome is the gap in the (log) reported likelihood of applying to the two jobs, which we denote $\Delta \pi_{i,j,j'} = \pi_{i,j} - \pi_{i,j'}$. We again find a significant effect: informing job seekers that they underestimated the prior cohort wages in job $j$ relative to job $j'$ increases their relative likelihood of applying to job $j$ by 19.5% on average. As robustness checks, Columns (2)-(4) repeat the analyses after controlling for all predetermined survey responses and/or restricting only to respondents who report actively searching at the time of the survey. Results are virtually identical to those in Column (1). In addition, Appendix Table A9 shows that we find similar positive treatment effects if we use the likelihood of accepting job offers as the outcome instead of the likelihood of applying.

**Heterogeneity by baseline misperceptions** In addition to the treatment effect depending on the *sign* of $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$, we of course also expect it to depend on its *magni-*

[17]Note that this sample definition does not exclude any unique job pairs from the analysis. For any individual, and given pair of jobs $A$ and $B$, either the wage in $A$ is underestimated relative to $B$, or the wage in $B$ is underestimated relative to $A$ (no respondents are exactly correct about wage gaps in our survey).

*tude*: we expect a bigger effect if the treatment reveals that the job seeker underestimated the wage gap for prior cohorts by a larger amount. In Figure 4 we group our sample of job pairs into three equal-width bins based on the value of $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$. Within each bin, we then estimate treatment effects using simple treatment-control comparisons, both for the relative perceived gap in own potential wages (Panel (a)) and for the relative application likelihood (Panel (b)). We see the expected patterns. In cases where the information treatment revealed larger underestimation of the prior cohort wage gap, we estimate larger upward-adjustments of the perceived gap in potential wages, as well as larger increases in the relative likelihood of applying. Conducting formal tests for pairwise equality, all estimates in Panel (a) are significantly different from each other at the 1 percent level. Given the lower precision in Panel (b), only the first and third estimates are significantly different here, and only at the 10 percent level.

## 4.2 OLS Regressions: How responsive are beliefs and applications to informing workers of their misperceptions?

The treatment-control comparisons above establish that correcting relative wage misperceptions shifts beliefs and applications, with larger effects when the treatment reveals a larger misperception. We now scale these effects by the size of the revealed misperception, yielding a per-unit coefficient that does not depend on the particular distribution of misperception sizes in our sample. Both panels of Figure 4 are consistent with treatment effects increasing linearly in $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$, which suggests the following two parsimonious regression specifications:

$$\Delta\widetilde{w_{i,j,j'}} = \gamma_1\, T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}) + \gamma_2\, (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}) + e_{i,j,j'} \tag{2}$$

$$\Delta\pi_{i,j,j'} = \theta_1\, T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}) + \theta_2\, (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}) + u_{i,j,j'} \tag{3}$$

The coefficients on the interaction terms, $\gamma_1$ and $\theta_1$, capture the (linear) effects of the information treatment and have a simple interpretation: if the information treatment reveals to a job seeker that they underestimated a particular prior cohort wage gap by $X\%$, equations

(2) and (3) imply that the job seeker's perceived gap in potential wages increases by $\gamma_1 X\%$ and that their relative likelihood of applying changes by $\theta_1 X\%$. Since we control for the underestimation of the prior cohort wage gap, $(\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$, identification of $\gamma_1$ and $\theta_1$ comes from treatment-control comparisons *within* different groups of individuals and job pairs, each with a given level of $(\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ (as in Figure 4). Random assignment therefore ensures that $\gamma_1$ and $\theta_1$ have a causal interpretation.

To treat all jobs symmetrically, we estimate (2) and (3) on the full sample of all possible pairwise job comparisons.[18] For the same reason, equations (2) and (3) do not include constant terms ($\gamma_0$, $\theta_0$) or an uninteracted treatment dummy ($T_i$) because the left- and right-hand-side variables are mean zero by construction in this sample, both for the treatment and control group.[19] We continue to use standard errors clustered on individual. Our inference thus accounts fully for the fact that a given job (and job pair) enters the sample several times for the same individual.

Column (1) of Table 5 presents OLS estimates of (2) (Panel A) and (3) (Panel B). The estimate for $\gamma_1$ is 0.3: If informed that they underestimated the wage gap by $X\%$ for prior cohorts, job seekers adjust their perceived gap in potential wages by $0.3\,X\%$. This is in line with typical estimates from the literature on belief updating for information treatments (Haaland et al., 2023). In particular, our estimated learning rate of 30% is similar to the 12–34% learning rates for beliefs about re-employment wages in Denmark found by Altmann et al. (2025). The estimate for $\theta_1$ is 1.2: if informed that they underestimated the wage gap between job $j$ and $j'$ by $X\%$ for prior cohorts, job seekers respond by increasing their relative likelihood of applying for $j$ by $1.2\,X\%$. Columns (2)-(4) demonstrate the robustness of the estimates to controlling for pre-treatment covariates interacted with the underestimation of the prior cohort wage gap, and/or restricting only to the sample of

[18]Each job seeker thus contributes six job pairs to this sample since a given unique pair of jobs $A$ and $B$ enters twice, both for $(j, j') = (A, B)$ and for $(j, j') = (B, A)$. Avoiding this duplicity would require imposing some (arbitrary) rule for determining whether to include $(A, B)$ or $(B, A)$ in the sample. Deterministic rules (e.g. requiring $\left(\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}\right) > 0$) would imply that individual jobs are not treated symmetrically in estimation. Picking the ordering at random simply introduces additional noise in estimates. As a robustness check, Appendix Table A5 shows results using various different rules that restrict to unique job pairs. Results are similar throughout.

[19]As is easily verified, introducing a constant term and/or an uninteracted treatment dummy therefore generates exact zero estimates for the constant term and the coefficient on the treatment dummy, while leaving other coefficients unchanged.

currently active job seekers.

## 4.3 2SLS framework: How does application behavior respond to beliefs about relative wages?

Results in the previous section show how our information treatment shifted workers' perceived relative wages and in turn how much this shifted job application behavior. We can use the relative magnitudes of these effects to infer the effects of relative wage beliefs on job applications. Formally, this corresponds to treating (2) and (3) as the first stage and reduced form in a 2SLS framework. The excluded instrument is the interacted treatment dummy, $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$, and the second stage equation of interest is

$$\Delta \pi_{i,j,j'} = \beta_1 \, \Delta \widetilde{w_{i,j,j'}} + \beta_2 \, (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}) + \varepsilon_{i,j,j'} \tag{4}$$

The coefficient $\beta_1$ corresponds to our key causal elasticity of interest: if the perceived wage gap between job $j$ and $j'$ increases by 1 percent, the relative likelihood of applying to $j$ increases by $\beta_1$ percent. Consistent 2SLS estimation of $\beta_1$ requires the usual instrument exogeneity assumption that the instrument, $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$, is uncorrelated with the error term, $\varepsilon_{i,j,j'}$. As above, random assignment implies that the treatment and control groups are similar at baseline, meaning that instrument exogeneity will not be violated due to differences in baseline unobservables. Notably, interpreting the estimated $\beta_1$ as capturing the pure effect of wage beliefs, requires the stronger exclusion restriction that the instrument *only* affects application behavior through its effect on perceived wages. We return to this at length in Section 4.4 below.

Panel C of Table 5 shows 2SLS estimates of (4). The estimate for $\beta_1$ in Column (1) is 4.2. Increasing the perceived wage gap between job $j$ and $j'$ by 1 percent increases the relative likelihood of applying for $j$ by 4.2%. This is a substantial effect and implies that wage misperceptions can generate important distortions in job search (as we flesh out in Section 5). As above, Columns (2)-(4) of Table 5 show robustness to the inclusion of pre-treatment covariates and/or restricting to active job seekers. Moreover, Appendix Table A10 shows that we obtain similar results when estimating a 2SLS specification with $T_i$ as the excluded instrument in the reordered sample approach of Section 4.1 Appendix Table A4 show that results are also robust to considering only job seekers with a good understanding of the three job types and reweighing to correct for survey non-participation. Finally, Column (1) of Appendix Table A11 shows that our estimates are robust to letting the prior-cohort wage gap misperception, $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$, enter the specification flexibly rather than linearly.

## 4.4 Belief spillovers and interpretation of the 2SLS estimate

The 2SLS estimates above quantify how application behavior responds when our information treatment shifts perceived relative wages by some amount. This is informative in its own right about how early career job seekers respond to wage information. For the purpose of quantifying overall costs in Section 5 however, we are interested in the stronger interpretation that the 2SLS estimate captures the *pure* effect of changes in perceived relative wages. As noted above, this requires the exclusion restriction that the information treatment (the instrument) *only* affects applications by changing wage beliefs. The key concern with this assumption is belief spillovers to other dimensions that affect application decisions: upon learning that a particular job offers a higher wage, a job seeker could also perceive that the job is harder to get or that it offers different non-wage amenities.

Ex ante, we expect belief spillovers to be less likely in our setting than in many other commonly studied settings. In response to a treatment that involves (or mimics) firms changing their actual posted wage for example, it is rational for job seekers to expect higher posted wages to attract more applicants, thus making the job harder to get. Similarly, they might also expect the wage change to go hand-in-hand with firms making systematic changes to offered non-wage amenities; a higher wage might for example be introduced to compensate for longer or more strenuous work hours. In contrast, since our information treatment only informs a small number of job seekers about realized past wages, our treatment should not induce aggregate changes in the number of applicants and is less likely to convey information about systematic changes in firms' offered terms of employment. Whether our information treatment in fact affected non-wage beliefs remains an empirical question however.[20] Accordingly, we designed our survey to directly allow us to test for the existence of belief spillovers.

**Test of belief spillovers on hiring likelihood** We first consider belief spillovers about how hard each job is to get. Recall that, besides applications, we asked respondents how likely they would be to accept each of the three jobs if they received simultaneous, mutually exclusive offers from all of them (Section 2.5). The two decisions differ in one key respect: whether to apply depends both on how attractive a job is and on how likely an application is to yield an offer, whereas whether to accept an offer already in hand depends only on how attractive the job is. If learning that a job pays more led workers to also view it as harder to get, this belief would affect applications but not offer acceptances—so the information treatment would move the two outcomes differently. Finding instead that it moves them similarly indicates no such spillover. Panel A of Table 6 reestimates the reduced form (3) using the gap in log likelihood of accepting an offer as the outcome. The estimated coefficients (1.02–1.19) in fact turn to be almost identical to the corresponding estimates for job application in Panel B of Table 5 (1.04–1.28), indicating little role for offer-probability spillovers.

In Panel B we provide a second, more direct test by using as outcome the respondents' self-reported gap in the log probability of receiving an offer if applying. Again, we see no evidence of belief spillovers. The estimate is statistically indistinguishable from zero and in fact has the opposite sign of what standard theories would predict.

**Test of belief spillovers on non-wage amenities** Second, we test whether the information treatment shifted perceived non-wage amenities. We do this using the "equal wage" follow-up to the offer-acceptance scenario from above. After reporting the likelihoods of acceptance in the original scenario, the follow-up asked respondents to report corresponding acceptance likelihoods in a counterfactual version of the scenario. The counterfactual

[20] In a Bayesian learner framework for example, it is straightforward to write down information structures in which wage signals such as our treatment do not lead to spillovers on beliefs about other characteristics, as well as information structures in which they do. This is true regardless of whether job seekers perceive that actual wages and other job characteristics are correlated. What matters for spillovers is only the correlation between *misperceptions* about wages and *misperceptions* about other job characteristics.

version was explicitly described as differing *only* in that the offered starting wages in the three jobs were now equal.[21] If the wage information treatment generated spillovers to perceived non-wage amenities, this should manifest in the information treatment also affecting acceptance decisions in this equal wage counterfactual. In Panel C of Table 6 we use the gap in log acceptance likelihoods under equal wages as the outcome in the reduced form (3). We again see no evidence of belief spillovers. The coefficients are statistically insignificant and much smaller than the corresponding coefficients in Panel A.

As additional evidence, we directly asked respondents about four specific job characteristics: the wage they would expect to earn in five years if starting today, the expected weekly hours they would work, as well as their expected performance and how well they expect to get along with colleagues (both on 6-item Likert scales). In Table 7, we use cross-job gaps in these dimensions as outcomes. We see a significant effect only for the expected wage in five years (Panel A), further corroborating that the treatment affects wage perceptions but does not spill over onto non-wage amenities.

**Interpretation** Taken together, these results support the stronger interpretation of our 2SLS coefficient as capturing the pure effect of own wage beliefs on applications. Within the precision of our data at least, we see no evidence that the information treatment generate beliefs spillovers beyond wages. Appendix Table A11 further shows that our 2SLS estimates are robust to controlling for gaps in other perceived job characteristics, including the "equal wage" acceptance likelihood which captures the net attractiveness of jobs' non-wage amenities.

The fact that we isolate the wage-belief channel may also explain why we find applications to be more sensitive to wages than much prior work. A natural benchmark for our 2SLS results is the own wage elasticity of job applications (Holzer et al., 1991; Dal Bó et al., 2013; Banfi and Villena-Roldán, 2019; Marinescu and Wolthoff, 2020; Dube et al., 2020; Belot et al., 2022; Azar et al., 2022)—the percentage increase in likelihood of applying to a given job when that job increases its offered wage by 1%. By focusing on relative applica-

[21]Specifically, the wage of each job was specified to be equal to the middle offered wage that the job seeker expected to face across the three jobs.

tion likelihoods and relative wage beliefs, our 2SLS framework estimates a slightly different parameter,[22] however we can convert our results into an own wage elasticity using the link between our framework and a standard discrete choice model (see Appendix Section B.4). Applying this conversion at the bottom of Table 5, we obtain an own wage elasticity of about 3—substantially higher than prior estimates, which typically find elasticities below 1. Our spillover analysis suggests an explanation. Most prior estimates exploit variation in posted wages on online job-ad platforms, where wages may be perceived as signals about competition for the jobs and non-wage amenities; the estimated elasticity therefore combines the pure wage response with offsetting responses through other channels. In contrast, our information treatment conveys information about realized past wages to a small number of job seekers and appears not to have been perceived as a signal about other job characteristics. Accordingly, our estimates isolate the wage-belief channel that prior estimates may capture only as part of a bundled response.

### 4.5 Post-survey wages: Do the survey treatment effects translate to actual job outcomes?

Next, we turn to analyze individually-linked administrative data on post-survey wages. Crucially, this allows us to validate that the estimated effects above translate into actual changes in job outcomes and are not just confined to reported behavior — a common concern with survey experiments.

Results above suggest that the effect of the information treatment on post-survey wages should depend critically on initial misperceptions: For job seekers who underestimate true wage gaps, misperceptions make lower-paying jobs appear relatively attractive. The information treatment should cause these job seekers to apply for and earn higher wages post-survey. The converse should be true for job seekers who tend to overestimate true wage gaps. In Table 8, we estimate treatment effects on starting wages in job seekers' first post-survey job. Note that the analysis data differ from the above sections in that they

[22] To see the difference, consider a case where job $j$ increases its offered wage while wages are unchanged in job $j'$ and all other jobs. The likelihood of applying to job $j$ increases, and the own wage elasticity measures the extent of this increase. If focusing instead on the *relative* likelihood of applying to $j$ vs. $j'$, there is an additional effect because the likelihood of applying to job $j'$ also decreases.

now contain a single observation per job seeker. Moreover, since not all job seekers find a new job within our sample frame, the number of job seekers included in the analysis drops to 1,171.[23]

As a benchmark, Column (1) ignores heterogeneity by initial misperceptions and simply regresses log monthly wages on a treatment dummy. Consistent with most job seekers being underestimators, the average treatment effect is positive but not statistically significant. Column (3) instead estimates heterogeneous effects by initial misperceptions: we classify each job seeker as either an *underestimator* or *overestimator* based on whether they underestimate or overestimate the log gap between the highest-paying job and the average of the two other jobs (as in Section 3), and interact this classification with the treatment dummy. As predicted, the information treatment increases post-survey wages for underestimators (3.1 percent on average, significant at the 10% level). The implied effect for overestimators, reported at the bottom of the table, is −1.1 percent, though not statistically significant. Columns (5) and (7) restrict attention to respondents actively searching at the time of the survey. As anticipated, all effects increase in magnitude in this subsample, and the effect for understimators becomes significant at the 5% level.

As robustness checks, Table 8 also reports estimates with pre-treatment controls included: as expected given random assignment, estimates change little, although the interaction term loses significance once controls are added. As usual, because Table 8 conditions on having found a new job, a natural concern is that the estimated wage effects could reflect composition changes. Appendix Table A14 shows that the information treatment does not affect the likelihood of finding a new job, consistent with the treatment changing beliefs about relative wages across jobs—which matter for *which* jobs to target but not for job-finding rates.[24] Finally, Appendix Table A13 shows that estimated treatment effects

[23] Our available administrative data runs until December 2024. Job seekers' new jobs are measured as the first post-survey job spell at a firm that the job seeker did not work at prior to the survey. To be counted, we require job spells to last more than one month and to be the worker's highest-paid job. To avoid measurement errors from jobs that de facto started later in a month, we exclude the first month of employment but average over all remaining months of the employment when computing monthly wages in the new jobs. As we show in Appendix Table A14, we see no effect of the treatment on the likelihood of finding a new job within our sample period.

[24] In standard models, job-finding rates respond chiefly to beliefs about the overall level of wages, since this affects the relative attractiveness of unemployment vs. employment (e.g. Altmann et al. (2025)). Given that beliefs about wage levels are much closer to correct on average (cf. Figure 1), our information treatment should have a much more limited systematic impact on these beliefs.

covary with the size of misperceptions as should be expected.

Appendix Table A15 provides a direct comparison of treatment effects in the survey and administrative data. Using survey data on job seekers' application likelihoods, perceived hiring likelihoods and typical wages for past cohorts, we compute the *expected wage* that each job seeker would earn if hired into a new job based on the stated application behavior in the survey. Reestimating the specifications from the administrative data (Table 8) using this survey-predicted wage measure as the outcome, we see the same pattern as for realized wages: a positive effect for underestimators, a negative interaction for overestimators, and sharper effects among active searchers. In terms of magnitudes, point estimates are somewhat larger in the administrative data, although the differences are never statistically significant.

# 5 The costs of relative wage misperceptions

We close our analysis by quantifying the costs that misperceptions impose on workers. With this aim in mind, our survey and reduced form analysis was designed to identify a simple discrete choice model of job search (building on Harmon et al. (2025)). Combining our reduced form estimates with this additional structure, we can compute counterfactuals that remove relative wage misperceptions for each individual job seeker. Going beyond the reduced form wage results, this allows us to assess the overall extent of misallocation and the welfare costs accounting also for the non-pecuniary aspects of jobs. In the rest of this section, we present our discrete-choice model, describe the counterfactual analysis, and present the estimates we obtain for the costs of misperceptions. Appendix Section B gives full details and derivations.

## 5.1 Model outline

We use a single-agent discrete choice model of job search. Matching the setup in our survey, we consider a job seeker $i$ who at a point in time has the option of applying for one of three different jobs indexed by $j = 1, 2, 3$. Alternatively, she can decide to apply for none of them and simply continue searching. We assume that the continuation value of being hired into job $j$ is $Y_{i,j}$ and that the continuation value of continuing to search is $U_i$. Accordingly, the surplus value of job $j$ over continued search can be defined as $S_{i,j} = Y_{i,j} - U_i$. We will assume that this surplus value depends on the wage the worker will earn, $W_{i,j}$, a set of non-wage amenities summarized by the scalar $Z_{i,j}$, and an idiosyncratic taste shock, $\xi_{i,j}$, capturing other utility-relevant characteristics: [25]

$$S_{i,j} = \Psi_i W_{i,j}^{\delta} Z_{i,j} \xi_{i,j} \tag{5}$$

Here $\Psi_i$ is an individual-specific parameter allowing for heterogeneity in the general returns to employment, while $\delta$ is a parameter governing the curvature of surplus with respect to wages.

Optimal application behavior can be characterized by considering the surplus value of an application relative to the value of continued search. We let $A_{i,j}$ denote the surplus value of an application to job $j$. This will simply equal the likelihood of getting hired times the surplus value from being hired. Letting $P_{i,j}$ denote the probability of being hired at job $j$, we have:

$$A_{i,j} = P_{i,j} S_{i,j} \tag{6}$$

Importantly, as (5) and (6) make clear, the attractiveness of applying to job $j$ depends not only on the wage it offers but also on its non-wage amenities and the likelihood of being hired. In what follows, we let $H_{i,j}$ denote the total *non-pecuniary value* of applying to job $j$, which we define as

$$H_{i,j} = P_{i,j} Z_{i,j} \xi_{i,j}$$

The surplus value of an application to job $j$ can then be written as $A_{i,j} = \Psi_i W_{i,j}^{\delta} H_{i,j}$.

To allow for misperceptions, we assume that job seekers do not necessarily base their decisions on the true surplus value, $A_{i,j}$, but on their *perceived* surplus value, $\widetilde{A_{i,j}}$. This depends on their perceptions about the potential wage they would earn, $\widetilde{W_{i,j}}$, their prob-

[25] Treating $Z_{i,j}$ as scalar is not restrictive. For example, if we were to instead assume that jobs are characterized by a $K$-dimensional vector $\left(Z_{i,j}^1, Z_{i,j}^2, ..., Z_{i,j}^K\right)$ and that surplus is given by $\Psi_i W_{i,j}^{\delta} \Pi_k \left(Z_{i,j}^k\right)^{\delta_k}$, we would still arrive at equation (5) by simply defining $Z_{i,j} = \Pi_k \left(Z_{i,j}^k\right)^{\delta_k}$.

ability of being hired $\widetilde{P_{i,j}}$, and the non-wage amenities $\widetilde{Z_{i,j}}$:

$$\widetilde{A_{i,j}} = \widetilde{P_{i,j}}\widetilde{S_{i,j}} \tag{7}$$

$$\widetilde{S_{i,j}} = \Psi_i \widetilde{W_{i,j}}^{\delta} \widetilde{Z_{i,j}} \xi_{i,j} \tag{8}$$

Finally, we assume that sending an application incurs a cost of $c_i$.

Letting $\widetilde{A_{i,0}}$ denote the surplus value of not applying anywhere, job seeker $i$'s application choice then solves:

$$j_i^* = \text{argmax}_{j\in\{0,1,2,3\}} \widetilde{A_{i,j}} - \mathbf{1}_{\{j\neq 0\}} c_i$$

where $\mathbf{1}_{\{j\neq 0\}}$ is an indicator for applying to any job (and thus incurring the application cost).

After imposing a standard extreme value distribution on the taste shocks, optimal behavior for job seekers implies that the likelihood of applying to job $j$ obeys the following equation:[26]

$$P(j_i^* = j) = \frac{\exp((\alpha_i + \widetilde{p_{i,j}} + \delta\widetilde{w_{i,j}} + \widetilde{z_{i,j}})/\sigma)}{1 + \sum_{j'=1,2,3} \exp((\alpha_i + \widetilde{p_{i,j'}} + \delta\widetilde{w_{i,j'}} + \widetilde{z_{i,j'}})/\sigma)} \tag{9}$$

Here, $\sigma$ is a scale parameter for the taste shocks and $\alpha_i$ is a constant capturing the overall perceived attractiveness of the three jobs. As in previous sections, lowercase letters refer to logged values.

Equation (9) motivates our survey design and reduced form regression analyses. Prior to actually making an application decision, the left-hand side of Equation (9) shows the expected likelihood that job seeker $i$ will choose to apply to job $j$. This is exactly what job seekers report in our main survey question on application probabilities. The right-hand side of (9) relates this likelihood to perceived characteristics of the different jobs, including particularly the potential wage they expect to face in the job, $\widetilde{w_{i,j}}$, which job seekers also report in our survey. This equation also implies a direct link between the structural

[26]Specifically, we assume that the taste shocks, $\xi_{i,j}$, are i.i.d. Frechet-distributed with shape parameter $\sigma^{-1}$.

parameters of the model and our 2SLS estimate of the effect of wage beliefs on applications from equation (4)—in the absence of spillover effects of our information treatment on other beliefs: $\beta_1 = \frac{1}{\sigma}\delta$. Below, we use this to compute a model counterfactual based on the 2SLS estimates.

## 5.2 Counterfactual analysis and assumptions

Our counterfactual analysis aims to quantify the costs of relative wage misperceptions in terms of misallocated applications and corresponding welfare losses. For each job seeker in the control group, we compare their actual situation at the time of the survey to a counterfactual situation in which they have no relative wage misperceptions and examine differences in application behavior and welfare. We compute the counterfactual situation based on the discrete choice model above, combining it with individual reported application behavior in the survey and the reduced form 2SLS estimates. We focus on the control group so that the baseline situation is not contaminated by our information treatment; thanks to randomization, the results remain representative of the full set of respondents. Depending on the specific cost measure we focus on, the counterfactual analysis requires us to impose additional assumptions which we discuss below.

**Misallocated applications** Our survey provides direct measures of misperception about average prior-cohort wages. But search behavior depends on the potential wages workers expect to face themselves, not on the average wage of prior cohorts. Computing any counterfactual therefore requires us to take a stand on workers' relative misperceptions about their own potential wages. We adopt the simple assumption that the measured relative misperceptions about wage gaps for prior cohorts coincide with the relative misperceptions about own potential wage gaps across jobs. This assumption suffices to characterize counterfactual application behavior and to measure how many applications would be reallocated across jobs in the absence of relative wage misperceptions.

**Welfare costs** Leaning further into the model structure, we also use the model to assess welfare, accounting for both wages, non-wage amenities and the likelihood of being hired. Specifically, for each job seeker $i$, we benchmark the welfare costs of relative wage misperceptions against an across-the-board wage change. This amounts to asking how much lower all wages would have to be in order for $i$ to face the same utility from a job applications in the absence of relative wage misperceptions. The welfare counterfactual requires three additional assumptions: First, we need to take a stand on misperceptions about the non-pecuniary value of applying to each job, i.e. hiring likelihoods and non-wage amenities. Since the focus of our analysis is on wage misperceptions, we perform our counterfactual under the simple benchmark that job seekers have correct beliefs about these non-pecuniary values. Second, we need to take a stand on what happens to job seekers for whom the first application decision does not result in employment. Mirroring standard job search models, we assume that these workers continue searching with some individual-specific flow utility and time discount rate, while sending additional applications at some exogenously fixed Poisson rate. Finally, we need to impose a parameter restriction on the parameter governing the curvature of utility with respect to wages, $\delta$. This reflects the usual challenge that choice behavior does not separately identify this parameter from the scale parameter of the taste shocks.[27] For our main results, we impose $\delta = 1$ corresponding to risk-neutrality. In Appendix Tables A16 and A17 we show results for the risk-averse case, $\delta < 1$. This leads to slightly higher welfare costs of misperceptions.

## 5.3 The costs of relative wage misperceptions, results

Table 9 shows the results of our counterfactual analysis measuring the costs of relative wage misperceptions. For each individual in our control group, we compare their actual, current situation to a counterfactual in which they have no relative wage misperceptions. We then average across individuals. Standard errors in parentheses were obtained by bootstrapping individuals and recomputing both the model counterfactuals and the reduced form results we use.

The first row of Panel A shows results for all job seekers. As a first simple measure of costs, Column (1) shows that 9.5% of all job applications are misallocated in the sense that

[27]Since $\beta_1 = \frac{1}{\sigma}\delta$, scaling up $\sigma$ and $\delta$ by the same amount leaves application behavior unchanged.

job seekers would have preferred to send them to a different job if they correctly perceived wage differences. Column (2) instead computes the implied welfare cost. For the average job seeker, their relative wage misperceptions lower welfare by the same amount as a 0.8% drop in all wages.

Columns (3) and (4) illustrate how these average welfare costs come about by shifting applications. We estimate that relative wage misperceptions depress the average applied-for wage by 1.2% (Column (3)), a somewhat smaller effect than suggested by the reduced form estimates.[28]. This (expected) wage loss is partially offset however since misperceptions on average also shift applications towards jobs with better non-wage amenities and/or higher hiring probabilities, raising the average non-pecuniary value of an application by 0.14% (Column (4)).

As noted in previous sections, the fact that relative wage misperceptions on average shift applications to lower-paying jobs reflects that most job seeker tend to underestimate the size of true wage gaps so that misperceptions make lower-paying jobs look relatively more attractive. In the second and third rows of Panel A, we unpack this by explicitly splitting jobseekers into *underestimators* and *overestimators*, defined by whether they under- or overestimate the wage gap between the job offering them the highest wage and the average of the two others. Columns (1) and (2) show that relative wage misperceptions create similar levels of misallocation and welfare costs for both groups. Respectively, 10.4% and 6.7% of applications are misallocated, leading to the same average welfare losses as a 1.0% and 0.5% decrease in wages for the two groups. In line with expectations however, Columns (3) and (4) show that the nature of misallocation and welfare losses differs sharply between the groups. Among underestimators, misperceptions have the same qualitative effects as for the average job seeker, only they are larger in magnitude.[29] For overestimators,

[28]Point estimates in Table 8 suggest an average effect of the information treatment on wages of around 2%. With the estimated learning rate of the information treatment being about one-third, a naive extrapolation would suggest an effect of removing all relative wage misperceptions as large as 6%. The difference may simply be due to noise; confidence intervals in Table 8 easily allow for wage effects even smaller than in the model counterfactual. Alternatively, it could reflect differences in the wage measure being used. Table 9 deals with applied-for wages, while Table 8 examines actual reemployment wages. The latter may for example also be influenced by differences in worker bargaining behavior.

[29]Comparing to the reduced form wage effects of the information treatment in Table 8, estimated effects for under- and overestimators again suggest somewhat larger effects of misperceptions than in Table 8. This may simply be due to noise or may be due to differences in the wage measure used (see footnote 28).

however, relative wage misperceptions cause welfare losses by making high-paying jobs look excessively attractive, leading them to suboptimally target *higher*-paying jobs with worse non-wage amenities and/or lower likelihoods of being hired.

Finally, Panels B and C illustrate heterogeneity by the overall extent of relative wage misperceptions. These panels focus on job seekers who are in the top and bottom deciles in terms of the magnitude of misperceptions, measured using the absolute value of misperceptions about log gaps across all job pairs. Among those with the largest relative misperceptions (Panel B), almost one-in-five applications are misallocated and welfare costs are equivalent to a decrease in all wages of more than 2.8%. Among those with the smallest relative misperceptions (Panel C), fewer than one-in-fifty applications are misallocated and welfare costs are negligible.

# 6 Conclusion

Using linked administrative and survey data in Denmark, this paper shows that young job seekers have large misperceptions about wage differences across jobs. While there is substantial heterogeneity, most of them underestimate the true wage gap between jobs such that lower-paying jobs look relatively more attractive. Providing information that corrects these misperceptions has large effects on job search decisions and hiring outcomes, on average shifting young workers towards higher-paying jobs. Crucially, these effects are not confined to stated intentions: linking respondents to administrative records, we find that correcting wage beliefs raises the wages workers actually earn in their next job. Overall, our findings show that relative wage misperceptions impose substantial costs by distorting how young workers sort into jobs. They also point to a remedy: early-career job seekers are responsive to wage information and easily reached at the point of labor market entry, making this a particularly promising moment for policy to intervene.

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# 7 Tables and Figures

Table 1: Covariate balance and descriptive statistics, study sample individuals

| | (1) All | (2) Control group | (3) Treatment group | (4) p-value (2) = (3) |
|---|---|---|---|---|
| Age | 28.07 | 28.15 | 27.98 | 0.326 |
| Female | 0.62 | 0.61 | 0.63 | 0.420 |
| Months between survey and expected/actual graduation | 16.61 | 17.58 | 15.60 | 0.195 |
| Employed long-term | 0.11 | 0.12 | 0.11 | 0.270 |
| Employed short-term, without job offer | 0.05 | 0.06 | 0.04 | 0.117 |
| Employed short-term, with job offer | 0.03 | 0.04 | 0.02 | 0.122 |
| Unemployed, with job offer | 0.25 | 0.25 | 0.25 | 0.972 |
| Unemployed, without job offer | 0.56 | 0.53 | 0.58 | 0.060* |
| Currently studying | 0.07 | 0.07 | 0.07 | 0.957 |
| Actively searching for a job | 0.65 | 0.63 | 0.67 | 0.130 |
| Currently searching or about to start new job | 0.87 | 0.86 | 0.88 | 0.112 |
| Currently searching, about to start new job or planning to search in the next 3 months | 0.89 | 0.88 | 0.89 | 0.270 |
| Good understanding of: | | | | |
| Most common job | 0.82 | 0.81 | 0.82 | 0.564 |
| Second most common job | 0.73 | 0.73 | 0.73 | 0.940 |
| Least common job | 0.72 | 0.73 | 0.71 | 0.388 |
| Close to someone: | | | | |
| In most common job | 0.59 | 0.60 | 0.58 | 0.394 |
| In second most common job | 0.49 | 0.48 | 0.49 | 0.666 |
| In least common job | 0.51 | 0.53 | 0.49 | 0.092* |
| Perceived avg. wage of prior cohorts (1000s): | | | | |
| In most common job | 32.27 | 32.13 | 32.42 | 0.259 |
| In second most common job | 31.21 | 31.16 | 31.26 | 0.717 |
| In least common job | 32.55 | 32.37 | 32.73 | 0.180 |
| Perceived avg. wage of prior cohorts after 5 years (1000s): | | | | |
| After starting at most common job | 39.51 | 39.51 | 39.52 | 0.968 |
| At second most common job | 38.15 | 38.10 | 38.21 | 0.784 |
| At least common job | 39.89 | 39.91 | 39.88 | 0.943 |
| Perceived avg. probability to receive offer, if applying: | | | | |
| In most common job | 0.17 | 0.17 | 0.16 | 0.263 |
| In second most common job | 0.15 | 0.15 | 0.14 | 0.481 |
| In least common job | 0.14 | 0.15 | 0.14 | 0.639 |
| Part of STAR sample | 0.91 | 0.91 | 0.91 | 0.971 |
| Individuals | 1,902 | 976 | 926 | |

**Notes:** The first three columns of the table show sample means for all pre-treatment survey variables, respectively for the full study sample (Column (1)), only for the control group (Columns (2)) and only for the treatment group (Column (3)). Column (4) shows p-values from a test of equal means between the treatment and control groups. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table 2: Descriptive statistics, survey job types

| | (1) Share of first jobs, prior cohorts | (2) Stated likelihood of applying | (3) Avg. wage, prior cohorts | (4) Perceived avg. wage, prior cohorts | (5) Perceived own potential wage |
|---|---|---|---|---|---|
| Data source: | Admin | Survey | Admin | Survey | Survey |
| **Panel A:** *Grouped by share of first jobs in prior cohorts* | | | | | |
| Highest | 25.64% | 37.80% | 30,883 | 32,269 | 33,312 |
| Middle | 14.81% | 24.11% | 30,735 | 31,208 | 32,385 |
| Lowest | 09.62% | 28.75% | 31,674 | 32,548 | 33,503 |
| **Panel B:** *Grouped by stated likelihood of applying* | | | | | |
| Highest | 18.62% | 61.94% | 31,310 | 33,080 | 34,322 |
| Middle | 16.01% | 21.73% | 31,164 | 32,021 | 33,061 |
| Lowest | 15.44% | 07.00% | 30,817 | 30,923 | 31,816 |
| **Panel C:** *Grouped by avg. wage of prior cohorts* | | | | | |
| Highest | 14.75% | 31.46% | 33,148 | 32,763 | 34,091 |
| Middle | 17.93% | 31.04% | 31,176 | 31,901 | 33,075 |
| Lowest | 17.39% | 28.16% | 28,968 | 31,360 | 32,033 |

**Notes:** The table shows study sample means for various characteristics of the three job types asked about in the survey. Columns correspond to different variables, either from administrative data or the survey. Panels correspond to different ways of grouping the three jobs into highest, middle and lowest. In Panel A jobs are grouped in terms of how large a share of entry-level jobs they cover in past cohorts. In Panel B, jobs are grouped in terms of the respondent's stated likelihood of applying to them. In Panel C, jobs are grouped by their average wage for prior cohorts.

Table 3: Relative wage misperceptions, summary

| | Wage gap underestimation rate between job pairs $\frac{\Delta V_{i,j,j'} - \widetilde{\Delta V_{i,j,j'}}}{\Delta V_{i,j,j'}}$ | | | | Underestimation of log wage gap between job pairs $\Delta v_{i,j,j'} - \widetilde{\Delta v_{i,j,j'}}$ | | Individuals who are underestimators | Sample size: | |
|---|---|---|---|---|---|---|---|---|---|
| | $> 100\%$ | $> 50\%$ | $> 0\%$ | $< -50\%$ | Mean abs. value | Std. Dev. | Share | Individuals | Job pairs |
| Full sample | 36.1% | 55.2% | 66.7% | 25.1% | 0.129 | 0.167 | 66.6% | 1,902 | 5,706 |
| Actual log wage gap $> 0.05$ | 30.0% | 51.1% | 68.6% | 20.2% | 0.142 | 0.164 | 68.8% | 1,583 | 2,456 |
| Actual log wage gap $> 0.1$ | 20.9% | 45.7% | 69.2% | 17.3% | 0.146 | 0.166 | 70.3% | 1,046 | 1,471 |
| Two most frequent job types | 36.2% | 55.4% | 66.5% | 25.3% | 0.123 | 0.162 | 66.6% | 1,902 | 3,804 |
| Likelihood of applying $> 15\%$ for both jobs, control group | 31.3% | 54.9% | 66.3% | 24.8% | 0.114 | 0.151 | 65.7% | 623 | 1,392 |
| Full sample, correcting reporting errors | 36.2% | 55.1% | 66.8% | 24.9% | 0.126 | 0.162 | 66.5% | 1,901 | 5,703 |
| Currently active job seekers | 35.9% | 55.3% | 65.9% | 26.5% | 0.129 | 0.166 | 66.1% | 1,237 | 3,711 |
| Reweighted for non-participation | 35.9% | 55.3% | 67.0% | 24.4% | 0.130 | 0.170 | 65.6% | 1,826 | 5,478 |
| Good understanding of all job types | 34.5% | 54.2% | 66.7% | 24.5% | 0.125 | 0.163 | 66.3% | 1,019 | 3,057 |
| Correct answer to job type validation question | 35.0% | 52.4% | 63.8% | 27.6% | 0.123 | 0.159 | 65.8% | 1,135 | 3,405 |

**Notes:** The table summarizes the distribution of relative wage misperceptions. Rows correspond to different subsamples. The first four columns show the share of individual-by-job pairs $(i, j, j')$ for which the wage gap underestimation rate exceeds 100%, 50%, 0% or falls below -50% respectively. The next two columns report the mean absolute value and standard deviation of the underestimation of the log wage gap. The third last column instead uses individual-level data $(i)$ and shows what fraction of individuals are underestimators in the sense that they underestimate the typical wage in the highest-paying job relative to the average of the two other jobs. The last two columns show the number of individuals and unique individual-by-job pairs in each subsample. The shown subsamples are as follows: *Actual log wage gap* restricts to job pairs where the actual log gap in prior cohort wages is above some cutoff. *Two most frequent job types* restricts to the two jobs for each educational background that cover the largest share of starting jobs for past cohorts. *Likelihood of applying>15% for both jobs* only considers the control group and restricts the sample based on the respondent's reported likelihood of applying for the job types. *Correcting reporting errors* uses a simple procedure to correct likely reporting errors (see Appendix D.2). *Currently active job seekers* are respondents who report being actively searching at the time of the survey. *Reweighted for non-participation* reweights the study sample to match the invited population using propensity score weighting. *Good understanding of all job types* restricts to respondents who reported having a "Good" or "Very good" understanding of the survey job types. *Correct answer to job type validation question* restricts to respondents who answered correctly the job type validation question (see Appendix C.1.2).

Table 4: Overall treatment effect of the information experiment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **A):** Perceived gap in log own potential wages $\Delta\widetilde{w_{i,j,j'}}$ | | | | |
| Treatment, $T_i$ | $0.042^{***}$ | $0.042^{***}$ | $0.043^{***}$ | $0.042^{***}$ |
| | (0.005) | (0.005) | (0.006) | (0.006) |
| **B):** Gap in log likelihood of applying $\Delta\pi_{i,j,j'}$ | | | | |
| Treatment, $T_i$ | $0.195^{***}$ | $0.196^{***}$ | $0.199^{**}$ | $0.192^{**}$ |
| | (0.073) | (0.071) | (0.089) | (0.088) |
| Pre-treatment controls | | ✓ | | ✓ |
| Currently active job seekers | | | ✓ | ✓ |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 5,706 | 5,706 | 3,711 | 3,711 |

**Notes:** The table shows OLS estimates over individual-by-job pairs $(i, j, j')$ in the study sample, ordered so that the wage gap for prior cohorts is underestimated in all cases, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}} > 0$. Columns (3) and (4) restrict the sample to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as controls all pre-treatment survey variables from Table 1. Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table 5: Effect of relative misperceptions on search, 2SLS

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **A) First stage:** Perceived gap in log own potential wages $\Delta\widetilde{w_{i,j,j'}}$ | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})$ | 0.294*** | 0.298*** | 0.302*** | 0.297*** |
| | (0.032) | (0.031) | (0.040) | (0.039) |
| **B) Reduced form:** Gap in log likelihood of applying $\Delta\pi_{i,j,j'}$ | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})$ | 1.235*** | 1.275*** | 1.151* | 1.039* |
| | (0.468) | (0.450) | (0.596) | (0.577) |
| **C) Second stage:** Gap in log likelihood of applying $\Delta\pi_{i,j,j'}$ | | | | |
| $\Delta\widetilde{w_{i,j,j'}}$ | 4.197*** | 4.276*** | 3.813** | 3.500* |
| | (1.539) | (1.452) | (1.909) | (1.862) |
| Controls: | | | | |
| $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$ | ✓ | ✓ | ✓ | ✓ |
| Pre-treatment controls | | ✓ | | ✓ |
| Currently active job seekers | | | ✓ | ✓ |
| First stage F-stat | 85.48 | 90.92 | 56.72 | 57.44 |
| Own wage elasticity | 3.04 | 3.09 | 2.76 | 2.53 |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 11,412 | 11,412 | 7,422 | 7,422 |

**Notes:** The table shows estimates over individual-by-job pairs $(i, j, j')$ in the study sample. Panels A and B show OLS estimates, while Panel C shows 2SLS estimates using $T_i \times \left(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right)$ as the excluded instrument. All specifications include the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$, as a control. Columns (3) and (4) restrict the sample to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as additional controls all pre-treatment survey variables from Table 1 interacted with the underestimation of the log wage gap for prior cohorts. The implied own wage elasticity of applications to a job is computed at the bottom as explained in Appendix Section B.4. Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table 6: Belief spillovers and the exclusion restriction

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **A):** Gap in log likelihood of accepting job offer | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 1.022** | 1.111** | 1.188** | 1.165** |
| | (0.475) | (0.457) | (0.599) | (0.576) |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 11,412 | 11,412 | 7,422 | 7,422 |
| **B):** Perceived gap in log probability of offer, if applying | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 0.361 | 0.371 | 0.389 | 0.440 |
| | (0.256) | (0.254) | (0.331) | (0.325) |
| Individuals | 1,859 | 1,859 | 1,204 | 1,204 |
| $N$ | 10,762 | 10,762 | 6,936 | 6,936 |
| **C):** Gap in log likelihood of accepting job offer, if wages were equal | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 0.355 | 0.457 | 0.758 | 0.825 |
| | (0.461) | (0.456) | (0.588) | (0.579) |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 11,412 | 11,412 | 7,422 | 7,422 |
| Controls: | | | | |
| $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$ | ✓ | ✓ | ✓ | ✓ |
| Pre-treatment controls | | ✓ | | ✓ |
| Currently active job seekers | | | ✓ | ✓ |

**Notes:** The table shows OLS estimates over individual-by-job pairs $(i, j, j')$ in the study sample. The outcome is the gap across the two jobs in: the log reported likelihood of accepting an offer (Panel A); the perceived log probability of receiving an offer conditional on applying (Panel B); and the log reported likelihood of accepting an offer when both jobs offer the same starting wage (Panel C). Changes in observation totals across panels stem from missing reports and zeros in the raw outcome variables. All specifications control for the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$. Columns (3) and (4) restrict the sample to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as additional controls all pre-treatment survey variables from Table 1 interacted with the underestimation of the log wage gap for prior cohorts. Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table 7: Belief spillovers on additional job characteristics

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **A):** Perceived gap in log wage after 5 years | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 0.234*** | 0.239*** | 0.270*** | 0.265*** |
| | (0.038) | (0.038) | (0.047) | (0.046) |
| Individuals | 1,887 | 1,887 | 1,228 | 1,228 |
| $N$ | 11,314 | 11,314 | 7,364 | 7,364 |
| **B):** Perceived gap in log weekly hours worked | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 0.017 | 0.025 | 0.029 | 0.027 |
| | (0.026) | (0.026) | (0.032) | (0.031) |
| Individuals | 1,855 | 1,855 | 1,208 | 1,208 |
| $N$ | 10,998 | 10,998 | 7,160 | 7,160 |
| **C):** Gap in how well they think they would get on with their colleagues | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 0.041 | -0.016 | 0.374 | 0.284 |
| | (0.237) | (0.238) | (0.300) | (0.295) |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 11,412 | 11,412 | 7,422 | 7,422 |
| **D):** Gap in how well they think they would perform in the job type | | | | |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 0.139 | 0.128 | 0.318 | 0.291 |
| | (0.329) | (0.322) | (0.416) | (0.403) |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 11,412 | 11,412 | 7,422 | 7,422 |
| Controls: | | | | |
| $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$ | ✓ | ✓ | ✓ | ✓ |
| Pre-treatment controls | | ✓ | | ✓ |
| Currently active job seekers | | | ✓ | ✓ |

**Notes:** The table shows OLS estimates over individual-by-job pairs $(i, j, j')$ in the study sample. In Panels A-B, outcome variables are gaps in logged values of perceived job characteristics. In Panels C-D, outcome variables are the gap in how well the job seeker thinks they would get on with colleagues in the jobs and in how well the job seeker thinks they would perform in the jobs (6-item Likert scales). Changes in observation totals across panels stem from missing reports and zeros in the raw outcome variables. Besides the reported regressor, all specifications include the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$ as a control. Columns (3) and (4) restrict the sample to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as additional controls all pre-treatment survey variables from Table 1 interacted with the underestimation of the log wage gap for prior cohorts. Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table 8: Effect of information treatment on post-survey wages

| | Full sample | | | | Currently active job seekers | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treatment, $T_i$ | 0.017 | 0.019 | 0.031* | 0.029* | 0.026 | 0.022 | 0.051** | 0.050** |
| | (0.015) | (0.014) | (0.018) | (0.016) | (0.019) | (0.018) | (0.023) | (0.021) |
| $T_i \times$ Overestimator | | | -0.043 | -0.031 | | | -0.071* | -0.080** |
| | | | (0.032) | (0.029) | | | (0.040) | (0.039) |
| Overestimator | | | 0.035 | 0.030 | | | 0.040 | 0.046* |
| | | | (0.022) | (0.020) | | | (0.028) | (0.026) |
| Treatment Effect for Overestimators | | | -0.011 | -0.002 | | | -0.020 | -0.030 |
| | | | (0.027) | (0.025) | | | (0.033) | (0.033) |
| Pre-treatment controls | | ✓ | | ✓ | | ✓ | | ✓ |
| $N$ | 1,171 | 1,171 | 1,171 | 1,171 | 802 | 802 | 802 | 802 |

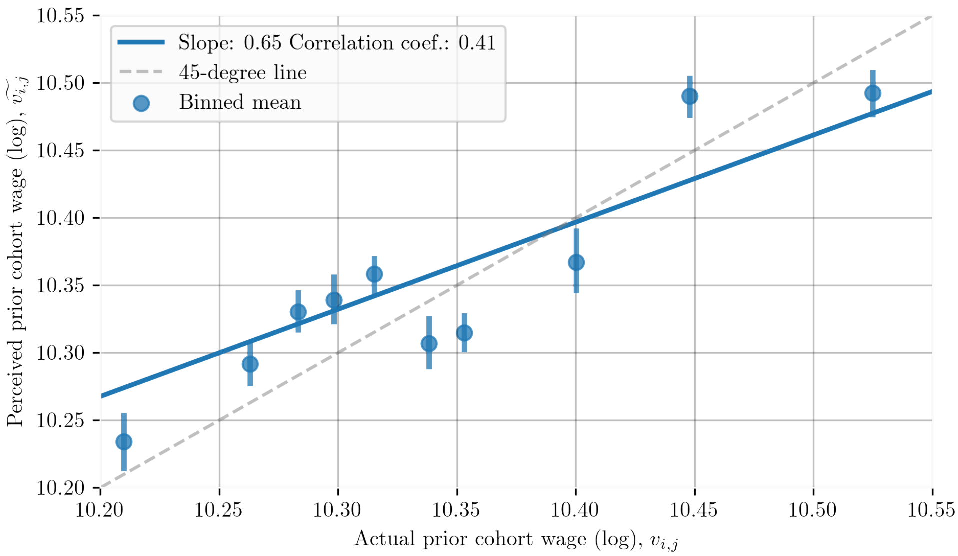
**Notes:** The table shows OLS estimates over individuals ($i$) in the study sample who are observed finding a new job post-survey. The outcome variable is the log monthly wage in the new job, measured in administrative data. Columns (1) and (5) contain the most parsimonious regression specifications, including only a constant term and treatment dummy. Other columns additionally add a dummy for the job seeker being an overestimator along with the corresponding interaction with the treatment dummy, and/or add all pre-treatment survey variables from Table 1 as controls. Columns (5)-(8) restrict the sample to individuals reporting that they are actively searching at the time of the survey. In Columns (3), (4), (7) and (8), the implied treatment effect for overestimators is reported in the row below the coefficients along with the standard error. A person is an overestimator if they overestimate the log gap in average wages between the highest-paying job and the average of the two other jobs. Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table 9: Model Counterfactual: The costs of relative wage misperceptions

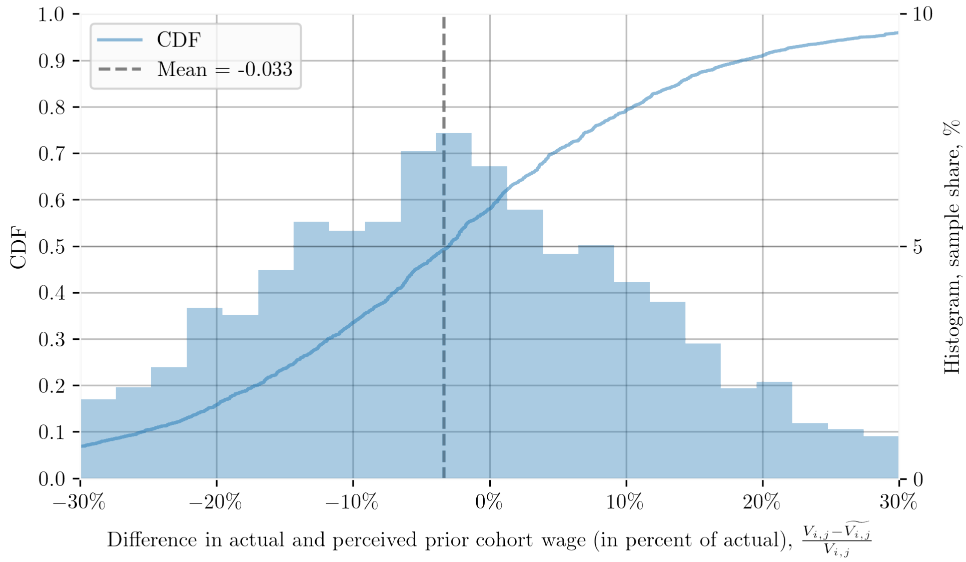
| | (1) Share of applications misallocated | (2) Welfare cost (wage growth equivalent) | (3) Effect on avg. applied-for wage | (4) Effect on avg. non-pecuniary application value |
|---|---|---|---|---|
| *Panel A: No restrictions* | | | | |
| Everyone | 9.47% | -0.83% | -1.20% | 0.14% |
| | (3.49) | (0.40) | (0.42) | (0.08) |
| Underestimators | 10.4% | -0.95% | -1.76% | 0.46% |
| | (3.80) | (0.46) | (0.62) | (0.22) |
| Overestimators | 6.74% | -0.49% | 0.38% | -0.75% |
| | (2.66) | (0.25) | (0.19) | (0.39) |
| *Panel B: Relative misperceptions in top decile* | | | | |
| Everyone | 19.2% | -2.88% | -3.74% | 0.14% |
| | (7.33) | (1.42) | (1.48) | (0.44) |
| Underestimators | 19.1% | -2.84% | -4.80% | 0.96% |
| | (7.25) | (1.41) | (1.82) | (0.53) |
| Overestimators | 19.6% | -3.22% | 3.78% | -5.65% |
| | (9.06) | (1.80) | (2.45) | (3.19) |
| *Panel C: Relative misperceptions in bottom decile* | | | | |
| Everyone | 1.92% | -0.03% | -0.08% | 0.03% |
| | (0.76) | (0.01) | (0.05) | (0.03) |
| Underestimators | 1.92% | -0.03% | -0.13% | 0.07% |
| | (0.79) | (0.02) | (0.06) | (0.04) |
| Overestimators | 1.93% | -0.03% | -0.02% | -0.01% |
| | (0.77) | (0.02) | (0.06) | (0.05) |

**Notes:** The table shows the average effects of current relative wage misperceptions computed from a discrete-choice model of job search. The base data are individuals ($i$) in the control group of the study sample. For each individual, model outcomes are computed both under current beliefs and under a counterfactual set of beliefs that removes all relative wage misperceptions. Details of the model and counterfactual calculations are given in Section 5 and Appendix B. Column (1) reports the share of an individual's applications that would go to a different job in the absence of relative wage misperceptions. Column (2) reports the welfare cost of relative wage misperceptions, expressed as the equivalent across-the-board proportional change in all wages: the amount by which all wages would have to change to leave the job seeker at their current utility level under the counterfactual. Column (3) reports the effect of misperceptions on the average applied-for wage, and Column (4) reports the effect on the average non-pecuniary application value (the value of hiring likelihoods and non-wage amenities, holding wages fixed). Both are expressed as the deviation from the counterfactual in percent. Panels restrict the sample by the size of relative misperceptions, defined as the mean of the absolute value of misperceptions about log wage gaps across all job pairs. Panel A imposes no restriction. Panel B restricts to job seekers in the top decile, and Panel C to job seekers in the bottom decile. Within each panel, the row *Everyone* averages over all individuals in the panel's sample, while *Underestimators* and *Overestimators* are individuals who, respectively, underestimate or overestimate the log gap in average wages between the highest-paying job and the average of the two other jobs. Results in this table normalize the wage-utility curvature parameter to $\delta = 1$, corresponding to risk neutrality. Standard errors in parentheses are obtained by a bootstrap procedure that resamples individuals and recomputes both the 2SLS estimate and the model counterfactuals within each bootstrap sample.

Figure 1: Misperceptions about wages in individual jobs



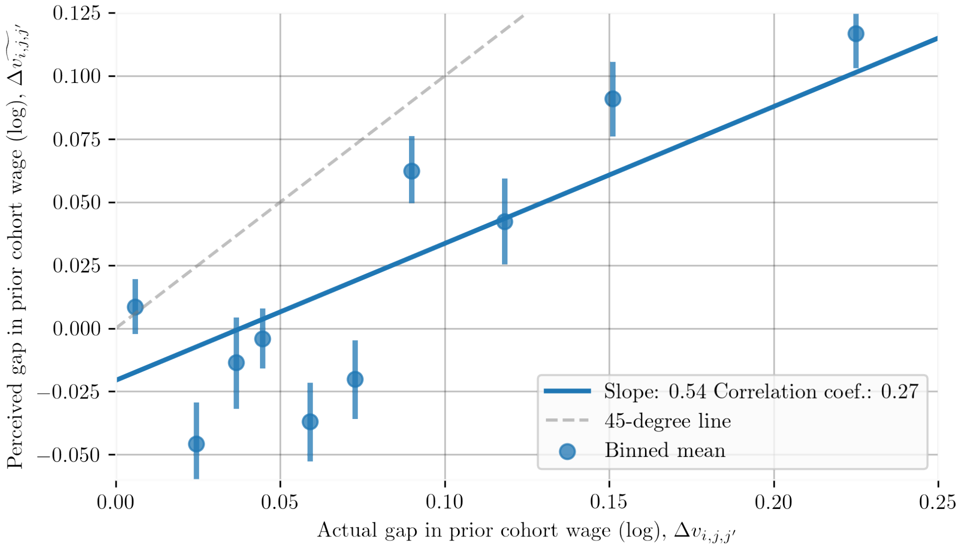

(a) Binscatter, perceived vs. actual prior-cohort wages



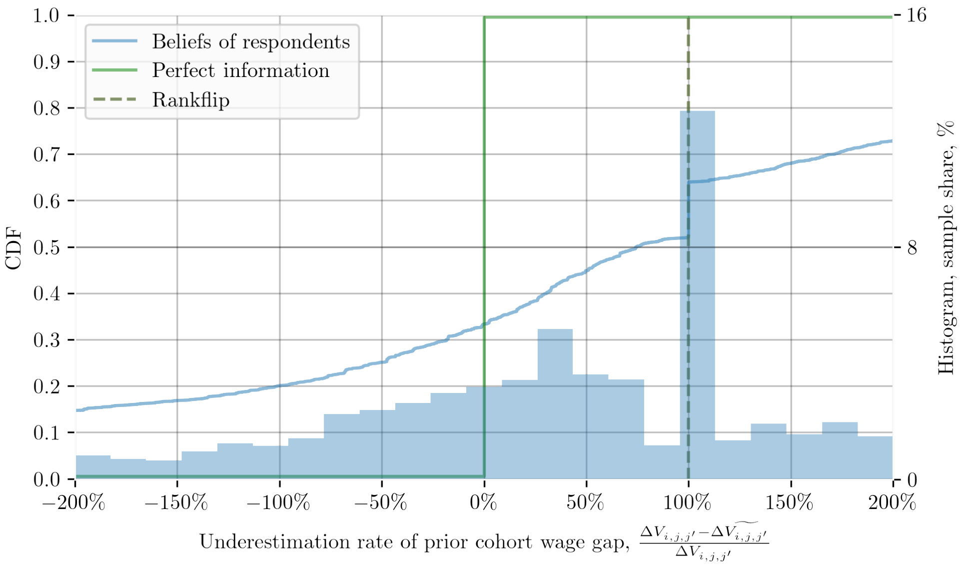

(b) Distribution of misperceptions about individual jobs

**Note:** The figures are based on all individual-by-jobs $(i, j)$ in the study sample. In Panel (a), the bin scatter splits the data in ten equal-sized bins according to the actual prior cohort wage and then plots within-bin means against each other. Error bars show 95% confidence intervals for the within-bin mean of the perceived (log) prior cohort wage, clustering on individuals. The line shows the fit of an OLS regression. The legend reports the regression line slope as well as the pairwise correlation coefficient. Panel (b) shows a histogram and empirical CDF for the difference in the actual vs. perceived prior cohort wage (in percent of the actual prior cohort wage).

Figure 2: Relative wage misperceptions across job pairs



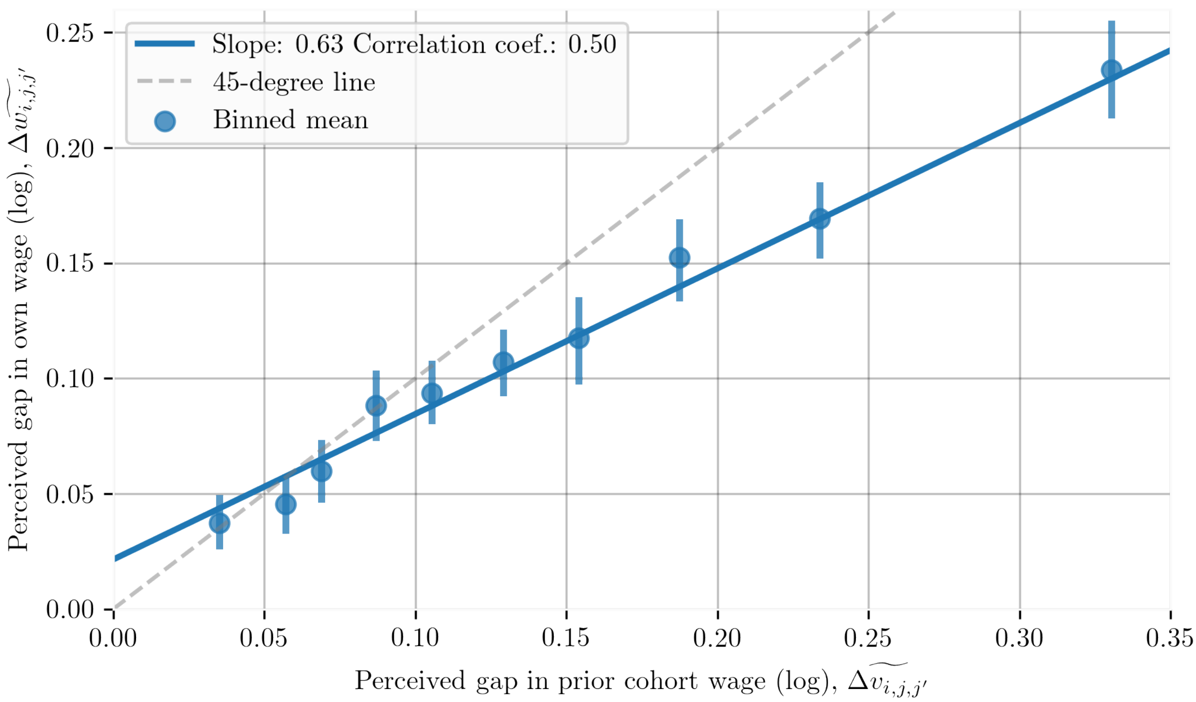

(a) Bin scatter, perceived vs actual prior-cohort wage gaps




(b) Distribution of misperceptions about wage gaps

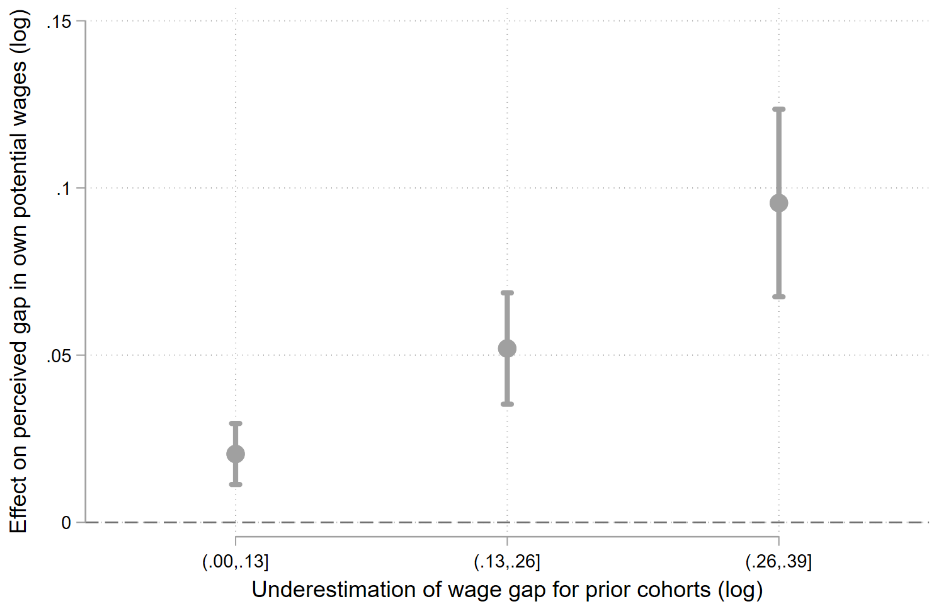
**Note:** The figure is based on individual-by-job pairs $(i, j, j')$ in the study sample, ordered so that the wage gap for prior cohorts is positive in all cases, $\Delta v_{i,j,j'} > 0$. In Panel (a), the bin scatter splits the data in ten equal-sized bins according to the actual prior cohort wage gap and then plots within-bin means against each other. Error bars show 95% confidence intervals for the within-bin mean of the perceived (log) gap in prior cohort wages, clustering on individuals. The line shows the fit of an OLS regression. The legend reports the regression line slope as well as the pairwise correlation coefficient. Panel (b) shows a histogram and empirical CDF for the underestimation rate of the prior cohort wage gap. The observed mass point at 1 reflects that job seekers report two jobs paying the exact same wage in 11.7% of cases.

Figure 3: Perceived own potential wages vs. perceived prior cohort wages

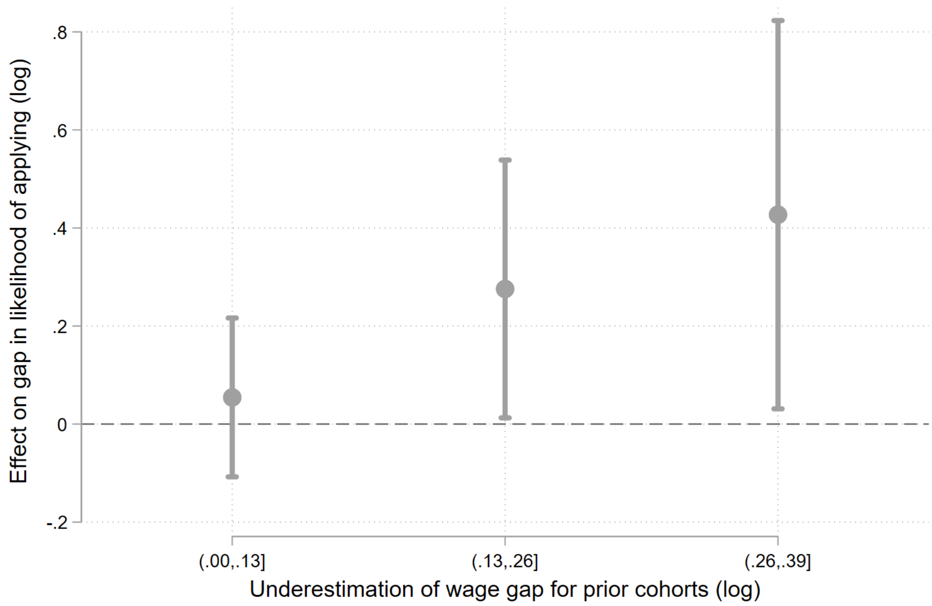



**Note:** The figure is based on individual-by-job pairs $(i, j, j')$ in the study sample, ordered so that the wage gap for prior cohorts is positive in all cases, $\Delta v_{i,j,j'} > 0$. The bin scatter splits the data in ten equal-sized bins according to the perceived prior cohort wage gap and then plots within-bin means against each other. Error bars show 95% confidence intervals for the within-bin mean of the perceived (log) gap in own potential wages, clustering on individuals. The line shows the fit of an OLS regression. The legend reports the regression line slope as well as the pairwise correlation coefficient.

Figure 4: Heterogeneous effects by size of initial misperceptions




(a) Treatment effect on $\Delta\widetilde{w_{i,j,j'}}$




(b) Treatment effect on $\Delta\pi_{i,j,j'}$

**Note:** The figure is based on all individual-by-job pairs $(i, j, j')$ in the study sample, ordered such that $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}} > 0$. The data is split into three equal-width bins according to the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$. The figures plot the effect of the information treatment within each bin, estimated via a linear regression that includes a constant and the treatment dummy. In Panel A, the outcome variable is the perceived gap in (log) own potential wages between the jobs, $\Delta\widetilde{w_{i,j,j'}}$. In Panel B, the outcome variable is the (log) relative likelihood of applying to the two jobs, $\Delta\pi_{i,j,j'}$. Error bars show 95% confidence intervals using clustering on individuals.

# Appendices

# A Additional tables and figures

Table A1: Descriptive statistics, graduates from higher education 2010-2018

| | All (%) | Selected educations (%) |
|---|---|---|
| **Education level** | | |
| Less than a bachelor's degree | 12.2 | 12.2 |
| Bachelor's degrees | 49.3 | 47.0 |
| Master's degree | 35.1 | 36.5 |
| Ph.d. and research programmes | 3.4 | 4.3 |
| **Education field** | | |
| Teaching and learning | 6.9 | 8.2 |
| Human sciences | 6.9 | 4.3 |
| Arts | 3.0 | 2.7 |
| Social sciences | 9.7 | 10.6 |
| Business economics, administration and law | 22.2 | 23.7 |
| Science | 4.4 | 2.9 |
| Information and communication technology (ICT) | 4.4 | 5.6 |
| Engineering, technology and industrial production | 7.3 | 7.7 |
| Building and civil engineering | 3.8 | 4.2 |
| Social and health | 25.1 | 24.6 |
| Services | 1.6 | 1.7 |
| **Education region** | | |
| Copenhagen | 43.4 | 41.8 |
| Zealand | 8.6 | 10.2 |
| Southern Denmark | 14.2 | 13.4 |
| Central Jutland | 23.6 | 24.3 |
| North Jutland | 10.2 | 10.4 |
| **Educations** | 287 | 88 |
| **Individuals** | 469,961 | 359,413 |

**Notes:** The table shows the composition of all higher education graduates in Denmark 2010-2018, as well as the composition only for graduates from the selected educations included in the study. The first panel shows the shares with different education levels (based on Danish ISCED groupings), the second panel shows the shares in different education fields (based on Danish ISCED groupings), the third panel shows the shares in each geographical region of Denmark.

Table A2: Descriptive statistics, invited sample, study sample and all young job seekers

| | Sample (1) completed survey | Sample (2) invited to survey | Population (3) of young job seekers |
|---|---|---|---|
| **Demographic characteristics** | | | |
| Age | 27.97 | 28.15 | 28.44 |
| Woman | 0.62 | 0.62 | 0.59 |
| Danish citizen | 0.92 | 0.91 | 0.88 |
| **Education level** | | | |
| Less than a Bachelor's degree | 0.07 | 0.11 | 0.14 |
| Bachelor's degree | 0.30 | 0.42 | 0.49 |
| Master's degree | 0.63 | 0.46 | 0.37 |
| **Education field** | | | |
| Teaching and learning | 0.09 | 0.11 | 0.07 |
| Social sciences | 0.18 | 0.13 | 0.12 |
| Business economics, administration and law | 0.14 | 0.20 | 0.25 |
| Science | 0.07 | 0.04 | 0.03 |
| Information and communication technology (ICT) | 0.09 | 0.07 | 0.07 |
| Engineering, technology and industrial production | 0.08 | 0.06 | 0.07 |
| Building and civil engineering | 0.03 | 0.04 | 0.04 |
| Social and health | 0.15 | 0.22 | 0.20 |
| **Status in 2023** | | | |
| Graduated less than 1 year ago | 0.66 | 0.44 | 0.27 |
| Graduated 1-2 years ago | 0.06 | 0.08 | 0.07 |
| Graduated 2-5 years ago | 0.11 | 0.14 | 0.14 |
| Graduated less than 1 year after survey | 0.04 | 0.22 | 0.36 |
| Work experience below 1 year | 0.70 | 0.67 | 0.62 |
| Work experience of 1-2 years | 0.10 | 0.12 | 0.13 |
| Work experience of 2-5 years | 0.14 | 0.15 | 0.17 |
| Work experience above 5 years | 0.05 | 0.06 | 0.09 |
| Received UI in 2023 | 0.90 | 0.89 | 0.47 |
| Cumulated weeks of UI in 2023 | 13.39 | 13.33 | 7.04 |
| Monthly wage in last job (1,000 DKK) | 34.35 | 34.46 | 34.47 |
| Lives in Copenhagen | 0.27 | 0.28 | 0.28 |
| **Individuals** | 1,902 | 22,461 | 89,978 |

**Notes:** Using a range of variables from administrative data, the table compares the study sample to both the invited sample and the corresponding population of all young job seekers. Column (1) shows variable means for the study sample. Column (2) shows variable means for all individuals invited to the survey. Column (3) shows variable means for all young job seekers from the selected educations, defined as individuals less than 40 years old who either graduated from their degree or were unemployed in 2023.

Table A3: Covariate balance and descriptive statistics, currently active job seekers

| | (1) All | (2) Control group | (3) Treatment group | (4) p-value (2) = (3) |
|---|---|---|---|---|
| Age | 28.41 | 28.49 | 28.32 | 0.471 |
| Female | 0.62 | 0.60 | 0.64 | 0.185 |
| Months between survey and expected/actual graduation | 19.63 | 20.61 | 18.65 | 0.334 |
| Employed long-term | 0.04 | 0.04 | 0.04 | 0.889 |
| Employed short-term, without job offer | 0.05 | 0.06 | 0.05 | 0.376 |
| Employed short-term, with job offer | 0.01 | 0.01 | 0.01 | 0.405 |
| Unemployed, with job offer | 0.09 | 0.09 | 0.09 | 0.772 |
| Unemployed, without job offer | 0.81 | 0.80 | 0.82 | 0.320 |
| Currently studying | 0.05 | 0.05 | 0.05 | 0.792 |
| Good understanding of: | | | | |
| Most common job | 0.80 | 0.80 | 0.79 | 0.766 |
| Second most common job | 0.73 | 0.73 | 0.74 | 0.713 |
| Least common job | 0.72 | 0.72 | 0.72 | 0.914 |
| Close to someone: | | | | |
| In most common job | 0.57 | 0.59 | 0.55 | 0.115 |
| In second most common job | 0.48 | 0.47 | 0.49 | 0.514 |
| In least common job | 0.50 | 0.52 | 0.48 | 0.132 |
| Perceived avg. wage of prior cohorts (1000s): | | | | |
| In most common job | 32.01 | 31.89 | 32.12 | 0.469 |
| In second most common job | 31.08 | 30.98 | 31.18 | 0.539 |
| In least common job | 32.25 | 32.02 | 32.48 | 0.170 |
| Perceived avg. wage of prior cohorts after 5 years (1000s): | | | | |
| After starting at most common job | 39.14 | 39.33 | 38.94 | 0.372 |
| At second most common job | 37.81 | 37.75 | 37.87 | 0.807 |
| At least common job | 39.44 | 39.48 | 39.39 | 0.853 |
| Perceived avg. probability to receive offer, if applying: | | | | |
| In most common job | 0.15 | 0.15 | 0.15 | 0.648 |
| In second most common job | 0.13 | 0.13 | 0.13 | 0.975 |
| In least common job | 0.13 | 0.13 | 0.13 | 0.976 |
| Part of STAR sample | 0.95 | 0.95 | 0.94 | 0.520 |
| Individuals | 1,237 | 619 | 618 | |

**Notes:** The first three columns of the table show sample means for all pre-treatment survey variables, respectively for the currently active job seekers in the study sample (Column (1)), only for the control group (Columns (2)) and only for the treatment group (Column (3)). Column (4) shows p-values from a test of equal means between the treatment and control groups among the currently active job seekers. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table A4: Effect of relative misperceptions on search, additional robustness

| | Reweighted for non-participation | | Received and passed validation test | | Good understanding of all job types | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\Delta\widetilde{w_{i,j,j'}}$ | 3.762* | 3.675** | 5.446** | 5.384** | 2.838 | 3.636** |
| | (2.025) | (1.856) | (2.368) | (2.188) | (1.972) | (1.818) |
| Controls: | | | | | | |
| $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Pre-treatment controls | | ✓ | | ✓ | | ✓ |
| First stage F-stat | 48.91 | 70.42 | 36.31 | 38.57 | 52.17 | 58.10 |
| Own wage elasticity | 2.72 | 2.66 | 3.93 | 3.88 | 2.05 | 2.62 |
| Individuals | 1,826 | 1,826 | 1,019 | 1,019 | 1,135 | 1,135 |
| $N$ | 10,956 | 10,956 | 6,114 | 6,114 | 6,810 | 6,810 |

**Notes:** The table shows additional 2SLS estimates, over individual-by-job pairs $(i, j, j')$ in the study sample. The outcome variable is the (log) relative likelihood of applying to the two jobs, $\Delta\pi_{i,j,j'}$. The excluded instrument is $T_i \times \left(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right)$. Besides the reported regressors, all specifications include the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$ as a control. Columns (1) and (2) reweight the study sample to match the invited population using propensity score reweighting. Columns (3) and (4) restrict attention to individuals who were shown the validation test regarding understanding of the survey job types and answered it correctly. Columns (5) and (6) restrict attention to individuals who reported having a good understanding of all the survey job types. Columns (2), (4) and (6) include as additional controls all pre-treatment survey variables from Table 1 interacted with the underestimation of the log wage gap for prior cohorts. The implied own wage elasticity of applications to a job is computed at the bottom via a standard discrete choice framework (see Appendix Section B.4 for details). Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table A5: Effect of relative misperceptions on search, results when restricting to unique job pairs

| | $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}} > 0$ | | $\Delta v_{i,j,j'} > 0$ | | Randomly selected ordering | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **A) First stage:** Perceived gap in log own potential wages $\Delta\widetilde{w_{i,j,j'}}$ | | | | | | |
| Constant | | 0.000 | | 0.061*** | | 0.004 |
| | | (0.004) | | (0.003) | | (0.003) |
| Treatment, $T_i$ | | 0.003 | | 0.006 | | -0.004 |
| | | (0.006) | | (0.004) | | (0.004) |
| $T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})$ | 0.294*** | 0.281*** | 0.294*** | 0.273*** | 0.294*** | 0.294*** |
| | (0.032) | (0.045) | (0.032) | (0.032) | (0.032) | (0.032) |
| **B) Reduced form:** Gap in log likelihood of applying $\Delta\pi_{i,j,j'}$ | | | | | | |
| Constant | | 0.030 | | 0.313*** | | 0.029 |
| | | (0.071) | | (0.052) | | (0.043) |
| Treatment, $T_i$ | | 0.006 | | 0.093 | | -0.081 |
| | | (0.102) | | (0.075) | | (0.061) |
| $T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})$ | 1.235*** | 1.198* | 1.235*** | 0.992** | 1.235*** | 1.229*** |
| | (0.468) | (0.680) | (0.468) | (0.491) | (0.468) | (0.468) |
| **C) Second stage:** Gap in log likelihood of applying $\Delta\pi_{i,j,j'}$ | | | | | | |
| Constant | | 0.029 | | 0.089 | | 0.013 |
| | | (0.069) | | (0.108) | | (0.041) |
| Treatment, $T_i$ | | -0.005 | | 0.072 | | -0.063 |
| | | (0.102) | | (0.076) | | (0.059) |
| $\Delta\widetilde{w_{i,j,j'}}$ | 4.197*** | 4.260* | 4.197*** | 3.639** | 4.197*** | 4.181*** |
| | (1.539) | (2.351) | (1.539) | (1.747) | (1.539) | (1.540) |
| Control for $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| First stage F-stat | 85.48 | 39.58 | 85.48 | 70.90 | 85.48 | 85.43 |
| Own wage elasticity | 3.04 | 3.08 | 3.04 | 2.63 | 3.04 | 3.03 |
| Individuals | 1,902 | 1,902 | 1,902 | 1,902 | 1,902 | 1,902 |
| $N$ | 5,706 | 5,706 | 5,706 | 5,706 | 5,706 | 5,706 |

**Notes:** The table shows estimates over individual-by-job pairs $(i, j, j')$ in the study sample. Each pair of columns uses a different restriction to impose that a unique job pair enters the sample only once per individual: Columns (1) and (2) impose that the underestimation of the log wage gap for prior cohorts is positive. Columns (3) and (4) impose that the true wage gap is positive. Finally, Columns (5) and (6) randomly pick an ordering (for a given person $i$ and unique pair of jobs $A, B$, it is chosen at random whether to include $i, A, B$ or $i, B, A$ in the data). Panel A and B show OLS estimates, while Panel C shows 2SLS estimates using $T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})$ as the excluded instrument. In addition to the reported regressors, all specifications include the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$, as a control. Columns (1), (3) and (5) use the specifications from the main text and omit constant terms and uninteracted treatment dummies. This always leads to numerically identical results. Columns (2), (4) and (6) show results including a constant term and uninteracted treatment dummy. The implied own wage elasticity of applications to a job is computed at the bottom via a standard discrete choice framework (see Appendix Section B.4 for details). Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table A6: Relationship between jobs' perceived starting wages and other perceived characteristics

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | $\widetilde{w_{i,j,t+5}}$ | Hours$\leq$ 35 | Hours $\in (35, 40)$ | Hours$\geq$ 40 | Log hours | Log probability of offer, if applying | Colleagues | Performance |
| **A)**: Full sample | | | | | | | | |
| Log perceived wage, $\widetilde{w_{i,j}}$ | 0.837*** | -0.363*** | -0.099** | 0.461*** | 0.148*** | 0.385** | 0.311*** | 0.448*** |
| | (0.020) | (0.034) | (0.039) | (0.039) | (0.012) | (0.157) | (0.115) | (0.099) |
| Constant | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Log perceived wage, $\widetilde{w_{i,j}}$ | 0.818*** | -0.333*** | -0.557*** | 0.890*** | 0.227*** | 0.101 | 0.437*** | 0.737*** |
| | (0.031) | (0.042) | (0.068) | (0.066) | (0.017) | (0.143) | (0.143) | (0.174) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Individuals | 1,887 | 1,902 | 1,902 | 1,902 | 1,855 | 1,859 | 1,902 | 1,902 |
| $N$ | 5,659 | 5,706 | 5,706 | 5,706 | 5,532 | 5,479 | 5,706 | 5,706 |
| **B)**: Control sample | | | | | | | | |
| Log perceived wage, $\widetilde{w_{i,j}}$ | 0.865*** | -0.378*** | -0.108** | 0.485*** | 0.153*** | 0.145 | 0.379** | 0.363*** |
| | (0.024) | (0.048) | (0.053) | (0.052) | (0.016) | (0.215) | (0.160) | (0.137) |
| Constant | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Log perceived wage, $\widetilde{w_{i,j}}$ | 0.837*** | -0.388*** | -0.605*** | 0.993*** | 0.250*** | 0.168 | 0.558*** | 0.761*** |
| | (0.044) | (0.058) | (0.090) | (0.089) | (0.023) | (0.197) | (0.183) | (0.233) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Individuals | 966 | 976 | 976 | 976 | 946 | 949 | 976 | 976 |
| $N$ | 2,897 | 2,928 | 2,928 | 2,928 | 2,823 | 2,797 | 2,928 | 2,928 |

**Notes:** The table shows OLS estimates from regressing various job characteristics on the log perceived starting wage. It is based on individual-by-jobs $(i, j)$ in the study sample. Panel A shows results for this full sample, while Panel B restricts attention to the control group. Within each of these panels, the first subpanel shows a specification with just the regressor and a constant term, while the second subpanel includes individual fixed effects. In Column (1) the outcome variable is the perceived log monthly wage 5 years after starting in the job. In Columns (2), (3) and (4) the outcome variables are dummies for perceived weekly hours being below 35, being 35-40 or being above 40 respectively. In Column (5) the outcome is perceived log weekly hours. In Column (6) the outcome is the perceived log probability of receiving an offer, if applying. In Column (7) the outcome is how well the job seeker expects to get along with their colleagues in the job (6-item Likert scale). In Column (8) the outcome is how well the job seeker expects to perform in the job (6-item Likert scale). Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table A7: Relationship between likelihood of applying and perceived characteristics of jobs

| | Log likelihood of applying, $\pi_{i,j}$ | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **A)**: Full sample | | | | | | | |
| Log perceived wage, $\widetilde{w_{i,j}}$ | 5.132*** | | | | | | 1.868*** |
| | (0.282) | | | | | | (0.355) |
| Log perceived wage after 5 years, $\widetilde{w_{i,j,t+5}}$ | | 4.744*** | | | | | 2.802*** |
| | | (0.215) | | | | | (0.291) |
| Hours$\in (35, 40)$ | | | 1.198*** | | | | 0.538*** |
| | | | (0.125) | | | | (0.106) |
| Hours$\geq 40$ | | | 2.039*** | | | | 0.904*** |
| | | | (0.127) | | | | (0.113) |
| Log probability of offer, if applying | | | | 0.534*** | | | 0.371*** |
| | | | | (0.033) | | | (0.029) |
| Colleagues | | | | | 0.542*** | | 0.265*** |
| | | | | | (0.032) | | (0.030) |
| Performance | | | | | | 0.611*** | 0.362*** |
| | | | | | | (0.022) | (0.025) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Individuals | 1,902 | 1,887 | 1,902 | 1,859 | 1,902 | 1,902 | 1,850 |
| $N$ | 5,706 | 5,659 | 5,706 | 5,479 | 5,706 | 5,706 | 5,453 |
| **B)**: Control sample | | | | | | | |
| Log perceived wage, $\widetilde{w_{i,j}}$ | 5.166*** | | | | | | 1.994*** |
| | (0.357) | | | | | | (0.489) |
| Log perceived wage after 5 years, $\widetilde{w_{i,j,t+5}}$ | | 4.552*** | | | | | 2.471*** |
| | | (0.287) | | | | | (0.399) |
| Hours$\in (35, 40)$ | | | 1.472*** | | | | 0.619*** |
| | | | (0.165) | | | | (0.148) |
| Hours$\geq 40$ | | | 2.328*** | | | | 0.930*** |
| | | | (0.163) | | | | (0.155) |
| Log probability of offer, if applying | | | | 0.533*** | | | 0.361*** |
| | | | | (0.044) | | | (0.038) |
| Colleagues | | | | | 0.577*** | | 0.273*** |
| | | | | | (0.042) | | (0.041) |
| Performance | | | | | | 0.616*** | 0.344*** |
| | | | | | | (0.029) | (0.032) |
| Individual FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Individuals | 976 | 966 | 976 | 949 | 976 | 976 | 943 |
| $N$ | 2,928 | 2,897 | 2,928 | 2,797 | 2,928 | 2,928 | 2,781 |

**Notes:** The table shows OLS estimates from regressing various job characteristics on the log likelihood of applying to the job with individual fixed effects. It is based on individual-by-jobs $(i, j)$ in the study sample. Panel A shows results for this full sample, while Panel B restricts attention to the control group. Row 1 is the perceived log monthly starting wage in the job. Row 2 is the perceived log monthly wage 5 years after starting in the job. Row 3 and 4 are dummies for perceived weekly hours being between 35-40 and being above 40, respectively. Row 5 is the perceived log probability of receiving an offer, if applying. Row 6 is how well the job seeker expects to get along with their colleagues in the job (6-item Likert scale). Row 7 is how well the job seeker expects to perform in the job (6-item Likert scale). Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table A8: Likelihood of transitioning into a job type post-survey vs. planned likelihood of applying for it in the survey, regressions

| | First new job is in the jobtype | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Reported likelihood of applying | 0.281*** | 0.254*** | 0.784*** | 0.669*** |
| | (0.018) | (0.022) | (0.039) | (0.050) |
| Constant | 0.030*** | | 0.097*** | |
| | (0.005) | | (0.012) | |
| Education-by-job type FE | | ✓ | | ✓ |
| First job is in one of the job types | | | ✓ | ✓ |
| Individuals | 1,902 | 1,899 | 646 | 635 |
| $N$ | 5,706 | 5,697 | 1,938 | 1,905 |

**Notes:** The table shows OLS estimates over individual-by-jobs $(i, j)$ in the study sample. The outcome variable is a dummy for whether the individual found a new job of the corresponding type post-survey, measured in administrative data. The regressor of interest is the reported likelihood of applying to the corresponding job type in the survey. Columns (2) and (4) add fixed effects defined at the level of the respondents' educational background-by-job type (j). Columns (3) and (4) drop individuals who did not find a new job in any of the three survey job types. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table A9: Overall treatment effect on reported job acceptance

| | Gap in log likelihood of accepting job offer | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment, $T_i$ | 0.222*** | 0.230*** | 0.255*** | 0.255*** |
| | (0.075) | (0.074) | (0.093) | (0.092) |
| Pre-treatment controls | | ✓ | | ✓ |
| Currently active job seekers | | | ✓ | ✓ |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 5,706 | 5,706 | 3,711 | 3,711 |

**Notes:** The table shows OLS estimates from a regression including a constant term, the treatment dummy and possibly controls. The base data are individual-by-job pairs $(i, j, j')$ in the study sample, ordered so that the wage gap for prior cohorts is underestimated in all cases, $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}} > 0$. The outcome variable is the gap in the reported log likelihood of accepting an offer from the jobs. Columns (3) and (4) restrict attention only to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as controls all pre-treatment survey variables from Table 1. Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table A10: Overall local average treatment effect

| | Gap in log likelihood of applying $\Delta\pi_{i,j,j'}$ | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $\Delta\widetilde{w_{i,j,j'}}$ | 4.634*** | 4.647*** | 4.635** | 4.596** |
| | (1.633) | (1.598) | (1.968) | (2.005) |
| Pre-treatment controls | | ✓ | | ✓ |
| Currently active job seekers | | | ✓ | ✓ |
| First stage F-stat | 82.23 | 86.06 | 56.71 | 56.69 |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 5,706 | 5,706 | 3,711 | 3,711 |

**Notes:** The table shows 2SLS estimates using $T_i$ as the excluded instrument and perceived gap in log own potential wages, $\Delta\widetilde{w_{i,j,j'}}$, as the endogenous variable. Besides the reported regressors all specifications include a constant. The base data are individual-by-job pairs $(i, j, j')$ in the study sample, ordered so that the wage gap for prior cohorts is underestimated in all cases, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}} > 0$. Columns (3) and (4) restrict attention only to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as controls all pre-treatment survey variables from Table 1. The first stage and reduced form are reported in Table 4. Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table A11: Effect of relative wage perceptions on search, non-linear controls and controls for differences in other perceived job characteristics

| | Gap in log likelihood of applying $\Delta\pi_{i,j,j'}$ | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| $\Delta\widetilde{w_{i,j,j'}}$ | 4.285*** | 4.531*** | 3.365*** | 4.052** | 4.130*** | 3.927*** | 3.445*** |
| | (1.542) | (1.584) | (1.147) | (1.597) | (1.505) | (1.500) | (1.206) |
| $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$ | -2.705*** | -2.229*** | -0.800* | -1.804*** | -2.047*** | -2.161*** | -0.710* |
| | (0.651) | (0.606) | (0.418) | (0.550) | (0.588) | (0.577) | (0.401) |
| $(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})^2$ | -0.000** | | | | | | |
| | (0.000) | | | | | | |
| $(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})^3$ | 4.783 | | | | | | |
| | (4.110) | | | | | | |
| $\Delta$ Log probability of offer, if applying | | 0.602*** | | | | | 0.243*** |
| | | (0.034) | | | | | (0.026) |
| $\Delta$ Non-pecuniary attractiveness of job | | | 0.690*** | | | | 0.591*** |
| | | | (0.016) | | | | (0.017) |
| $\Delta$ Log hours | | | | 2.492*** | | | 1.233*** |
| | | | | (0.614) | | | (0.423) |
| $\Delta$ Colleagues | | | | | 0.475*** | | 0.080*** |
| | | | | | (0.033) | | (0.024) |
| $\Delta$ Performance | | | | | | 0.570*** | 0.110*** |
| | | | | | | (0.024) | (0.019) |
| First stage F-stat | 85.15 | 75.13 | 84.80 | 76.72 | 85.32 | 84.91 | 71.30 |
| Own wage elasticity | 3.10 | 3.27 | 2.44 | 2.93 | 2.99 | 2.84 | 2.48 |
| Individuals | 1,902 | 1,859 | 1,902 | 1,855 | 1,902 | 1,902 | 1,824 |
| $N$ | 11,412 | 10,762 | 11,412 | 10,998 | 11,412 | 11,412 | 10,500 |

**Notes:** The table shows 2SLS estimates with additional controls. It is based on individual-by-job pairs $(i, j, j')$ in the study sample. The outcome variable is the (log) relative likelihood of applying to the two jobs, $\Delta\pi_{i,j,j'}$. The excluded instrument is $T_i \times (\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}})$. Column (1) controls non-linearly for the underestimation of the log wage gap for prior cohorts. Column (2) controls for the perceived gap in the (log) probability of receiving a job offer if applying to the jobs. Column (3) controls for the perceived gap in non-pecuniary attractiveness, defined as the gap in the log reported likelihood of accepting an offer from the jobs in the situation where the jobs offer the exact same starting wage. Column (4) controls for the perceived gap in log weekly hours of the jobs. Columns (5) and (6) control for the gap in how well the respondent thinks they would get along with colleagues or perform in the job (6-item Likert scales). Column (7) combines all job characteristic controls. The implied own wage elasticity of applications to a job is computed at the bottom via a standard discrete choice framework (see Appendix Section B.4 for details). Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table A12: Effect of relative misperceptions on job acceptance likelihood

| | Gap in log likelihood of accepting job offer | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $T_i \times (\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}})$ | 1.022** | 1.111** | 1.188** | 1.165** |
| | (0.475) | (0.457) | (0.599) | (0.576) |
| Pre-treatment controls | | ✓ | | ✓ |
| Currently active job seekers | | | ✓ | ✓ |
| Individuals | 1,902 | 1,902 | 1,237 | 1,237 |
| $N$ | 11,412 | 11,412 | 7,422 | 7,422 |

**Notes:** This table shows OLS estimates over individual-by-job pairs $(i, j, j')$ in the study sample. The outcome variables is the gap in the log likelihood of accepting a job offer from job $j$ given that the respondent is offered a job in all 3 job types. Besides the reported regressor, all specifications include the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta \widetilde{v_{i,j,j'}}$ as a control. Columns (3) and (4) restrict attention only to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as additional controls all pre-treatment survey variables from Table 1 interacted with the underestimation of the log wage gap for prior cohorts. Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table A13: Effect of information treatment on post-survey wages, alternative grouping

| | Full sample | | Currently active job seekers | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment, $T_i$ | 0.032 | 0.030 | 0.056** | 0.047* |
| | (0.020) | (0.018) | (0.026) | (0.025) |
| $T_i \times$Close estimator | -0.038 | -0.020 | -0.048 | -0.024 |
| | (0.036) | (0.032) | (0.045) | (0.042) |
| $T_i \times$Large overestimator | -0.023 | -0.027 | -0.074 | -0.080* |
| | (0.038) | (0.036) | (0.047) | (0.046) |
| Close estimator | 0.054** | 0.018 | 0.055* | 0.018 |
| | (0.026) | (0.023) | (0.033) | (0.030) |
| Large overestimator | 0.042 | 0.042 | 0.073** | 0.066** |
| | (0.026) | (0.025) | (0.033) | (0.033) |
| Treatment Effect for Close estimators | -0.006 | 0.010 | 0.009 | 0.023 |
| | (0.029) | (0.026) | (0.037) | (0.034) |
| Treatment Effect for Large overestimators | 0.009 | 0.003 | -0.018 | -0.033 |
| | (0.032) | (0.030) | (0.039) | (0.039) |
| Pre-treatment controls | | ✓ | | ✓ |
| $N$ | 1,171 | 1,171 | 802 | 802 |

**Notes:** The table shows OLS estimates over individuals ($i$) in the study sample who are observed finding a new job post-survey. The outcome variable is the log monthly wage in the new job, measured in administrative data. All specifications include a constant term, a treatment dummy, and dummies for being a close estimator and a large overestimator, both also interacted with the treatment dummy. The dummies are based on the difference between the perceived and the true gap in logs between the highest-paying job and the average of the two other jobs for the averages wages of prior cohorts. If the absolute value of this measure is less than 0.05, the respondent is a close estimator, and if the measure is larger than 0.05 the respondent is a large overestimator. Columns (3) and (4) restrict attention only to individuals reporting that they are actively searching at the time of the survey. Columns (2) and (4) include as controls all pre-treatment survey variables from Table 1. The implied treatment effects for close estimators and large overestimators is reported below the coefficient estimates along with the standard errors. Standard errors in parentheses are clustered at the level of the individual. * $p < 0.1$, ** $p < 0.05$,*** $p < 0.01$.

Table A14: Effect of information treatment on the likelihood of finding a new job

| | Full sample | | | | Currently active job seekers | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treatment, $T_i$ | 0.027 | 0.014 | 0.026 | 0.013 | 0.030 | 0.028 | 0.031 | 0.025 |
| | (0.022) | (0.021) | (0.027) | (0.027) | (0.027) | (0.027) | (0.034) | (0.033) |
| $T_i \times$Overestimator | | | 0.004 | 0.004 | | | -0.002 | 0.008 |
| | | | (0.047) | (0.045) | | | (0.057) | (0.056) |
| Overestimator | | | 0.025 | 0.015 | | | 0.045 | 0.046 |
| | | | (0.033) | (0.032) | | | (0.041) | (0.040) |
| Treatment Effect for Overestimators | | | 0.029 | 0.017 | | | 0.028 | 0.033 |
| | | | (0.038) | (0.036) | | | (0.046) | (0.045) |
| Pre-treatment controls | | ✓ | | ✓ | | ✓ | | ✓ |
| $N$ | 1,902 | 1,902 | 1,902 | 1,902 | 1,237 | 1,237 | 1,237 | 1,237 |

**Notes:** The table shows OLS estimates over individuals ($i$) in the study sample. The outcome variable is a dummy for whether the individual is observed finding a new job post-survey, measured in administrative data. Columns (1) and (5) contains the most parsimonious regression specifications, including only a constant term and treatment dummy. Other columns additionally add a dummy for the job seeker being an overestimator along with the corresponding interaction with the treatment dummy, and/or add all pre-treatment survey variables from Table 1 as controls. Columns (5)-(8) restrict attention only to individuals reporting that they are actively searching at the time of the survey. In Columns (3), (4), (7) and (8), the implied treatment effect for overestimators is reported in the row below the coefficients along with the standard error. A person is an overestimator if they overestimate the log gap in average wages between the the highest-paying job and the average of the two other jobs. Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table A15: Effect of information treatment on expected wage based on the survey

| | Full sample | | | | Currently active job seekers | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treatment, $T_i$ | 0.011$^{*}$ | 0.009$^{*}$ | 0.019$^{**}$ | 0.015$^{**}$ | 0.011 | 0.008 | 0.019$^{**}$ | 0.018$^{**}$ |
| | (0.006) | (0.005) | (0.008) | (0.007) | (0.007) | (0.006) | (0.009) | (0.008) |
| $T_i \times$Overestimator | | | -0.023$^{*}$ | -0.017 | | | -0.022 | -0.028$^{**}$ |
| | | | (0.013) | (0.010) | | | (0.015) | (0.013) |
| Overestimator | | | 0.020$^{**}$ | 0.015$^{**}$ | | | 0.024$^{**}$ | 0.027$^{***}$ |
| | | | (0.008) | (0.007) | | | (0.010) | (0.009) |
| Treatment Effect for Overestimators | | | -0.004 | -0.002 | | | -0.003 | -0.011 |
| | | | (0.010) | (0.008) | | | (0.012) | (0.010) |
| Pre-treatment controls | | ✓ | | ✓ | | ✓ | | ✓ |
| $N$ | 1,171 | 1,171 | 1,171 | 1,171 | 802 | 802 | 802 | 802 |

**Notes:** The table shows OLS estimates of the information treatment effect, restricted to respondents in our study sample who start a new job after the survey. The outcome is the log *expected wage*: the wage a respondent can expect to obtain given her stated application plans. For each respondent, we construct it as a weighted average of prior cohorts' wages across the three job types, weighting each job's wage by the respondent's stated likelihood of applying to it times her perceived likelihood of being hired if she applies. Columns (1) and (5) contains the most parsimonious regression specifications, including only a constant term and treatment dummy. Other columns additionally add a dummy for the job seeker being an overestimator along with the corresponding interaction with the treatment dummy, and/or add all pre-treatment survey variables from Table 1 as controls. Columns (5)-(8) restrict the sample to individuals reporting that they are actively searching at the time of the survey. In Columns (3), (4), (7) and (8), the implied treatment effect for overestimators is reported in the row below the coefficients along with the standard error. A person is an overestimator if they overestimate the log gap in average wages between the the highest-paying job and the average of the two other jobs. Standard errors in parentheses are clustered at the level of the individual. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$,$^{***}$ $p < 0.01$.

Table A16: The costs of relative wage misperceptions, $\delta = 0.5$

| | (1) Share of applications misallocated | (2) Welfare cost (wage growth equivalent) | (3) Effect on avg. applied-for wage | (4) Effect on avg. non-pecuniary application value |
|---|---|---|---|---|
| *Panel A: No restrictions* | | | | |
| Everyone | 9.47% | -0.95% | -1.20% | 0.08% |
| | (3.49) | (0.40) | (0.42) | (0.04) |
| Underestimators | 10.4% | -1.10% | -1.76% | 0.26% |
| | (3.80) | (0.46) | (0.62) | (0.10) |
| Overestimators | 6.74% | -0.55% | 0.38% | -0.44% |
| | (2.66) | (0.25) | (0.19) | (0.20) |
| *Panel B: Relative misperceptions in top decile* | | | | |
| Everyone | 19.2% | -3.30% | -3.74% | 0.07% |
| | (7.33) | (1.42) | (1.48) | (0.26) |
| Underestimators | 19.1% | -3.25% | -4.80% | 0.55% |
| | (7.25) | (1.40) | (1.82) | (0.27) |
| Overestimators | 19.6% | -3.65% | 3.78% | -3.34% |
| | (9.06) | (1.86) | (2.45) | (1.81) |
| *Panel C: Relative misperceptions in bottom decile* | | | | |
| Everyone | 1.92% | -0.03% | -0.08% | 0.02% |
| | (0.76) | (0.02) | (0.05) | (0.02) |
| Underestimators | 1.92% | -0.03% | -0.13% | 0.04% |
| | (0.79) | (0.02) | (0.06) | (0.02) |
| Overestimators | 1.93% | -0.03% | -0.02% | -0.01% |
| | (0.77) | (0.02) | (0.06) | (0.03) |

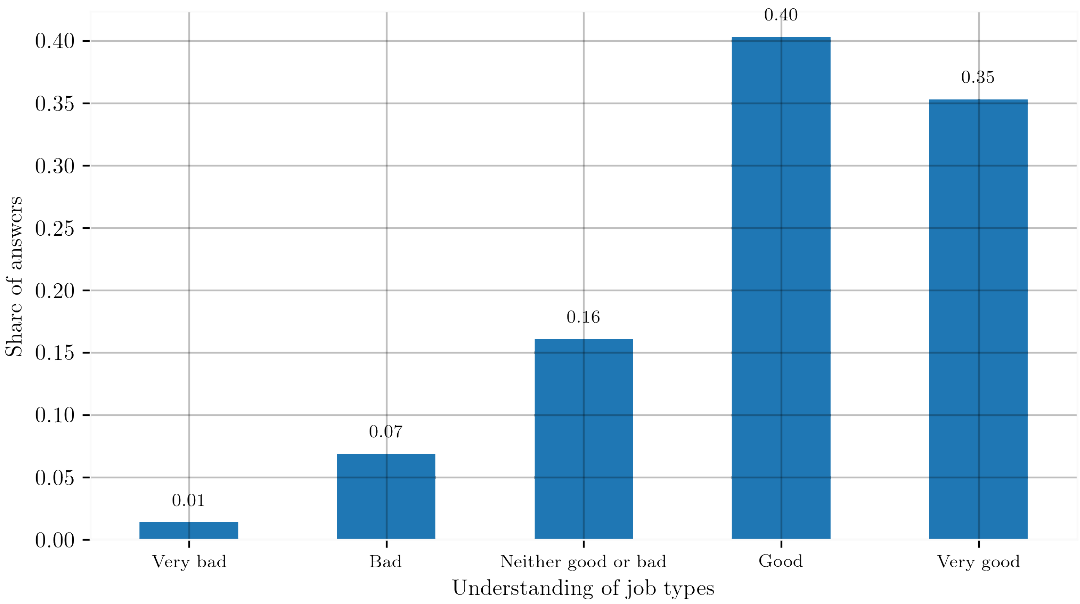
**Notes:** The table shows the average effects of current relative wage misperceptions computed from a discrete-choice model of job search. The base data are individuals ($i$) in the control group of the study sample. For each individual, model outcomes are computed both under current beliefs, $\widetilde{\boldsymbol{W}}_i^0$, and under a counterfactual set of beliefs, $\widetilde{\boldsymbol{W}}_i^1$, that removes all relative wage misperceptions. Details of the model and counterfactual calculations are given in Section 5 and Appendix B. Column (1) reports the share of an individual's applications that would go to a different job in the absence of relative wage misperceptions. Column (2) reports the welfare cost of relative wage misperceptions, expressed as the equivalent across-the-board proportional change in all wages: the amount by which all wages would have to change to leave the job seeker at their current utility level under the counterfactual. Column (3) reports the effect of misperceptions on the average applied-for wage, and Column (4) reports the effect on the average non-pecuniary application value (the value of hiring likelihoods and non-wage amenities, holding wages fixed). Both are expressed as the deviation from the counterfactual in percent. Panels restrict the sample by the size of relative misperceptions, defined as the mean of the absolute value of misperceptions about log wage gaps across all job pairs. Panel A imposes no restriction. Panel B restricts to job seekers in the top decile, and Panel C to job seekers in the bottom decile. Within each panel, the row *Everyone* averages over all individuals in the panel's sample, while *Underestimators* and *Overestimators* are individuals who, respectively, underestimate or overestimate the log gap in average wages between the highest-paying job and the average of the two other jobs. Results in this table normalize the wage-utility curvature parameter to $\delta = 0.5$. Standard errors in parentheses are obtained by a bootstrap procedure that resamples individuals and recomputes both the 2SLS estimate and the model counterfactuals within each bootstrap sample.

Table A17: The costs of relative wage misperceptions, $\delta = 0.25$

| | (1) Share of applications misallocated | (2) Welfare cost (wage growth equivalent) | (3) Effect on avg. applied-for wage | (4) Effect on avg. non-pecuniary application value |
|---|---|---|---|---|
| *Panel A: No restrictions* | | | | |
| Everyone | 9.47% | -1.02% | -1.20% | 0.04% |
| | (3.49) | (0.40) | (0.42) | (0.02) |
| Underestimators | 10.4% | -1.17% | -1.76% | 0.14% |
| | (3.80) | (0.46) | (0.62) | (0.05) |
| Overestimators | 6.74% | -0.59% | 0.38% | -0.23% |
| | (2.66) | (0.25) | (0.19) | (0.10) |
| *Panel B: Relative misperceptions in top decile* | | | | |
| Everyone | 19.2% | -3.50% | -3.74% | 0.04% |
| | (7.33) | (1.42) | (1.48) | (0.14) |
| Underestimators | 19.1% | -3.45% | -4.80% | 0.30% |
| | (7.25) | (1.40) | (1.82) | (0.14) |
| Overestimators | 19.6% | -3.86% | 3.78% | -1.81% |
| | (9.06) | (1.89) | (2.45) | (0.97) |
| *Panel C: Relative misperceptions in bottom decile* | | | | |
| Everyone | 1.92% | -0.03% | -0.08% | 0.01% |
| | (0.76) | (0.02) | (0.05) | (0.01) |
| Underestimators | 1.92% | -0.03% | -0.13% | 0.02% |
| | (0.79) | (0.02) | (0.06) | (0.01) |
| Overestimators | 1.93% | -0.04% | -0.02% | 0.00% |
| | (0.77) | (0.02) | (0.06) | (0.02) |

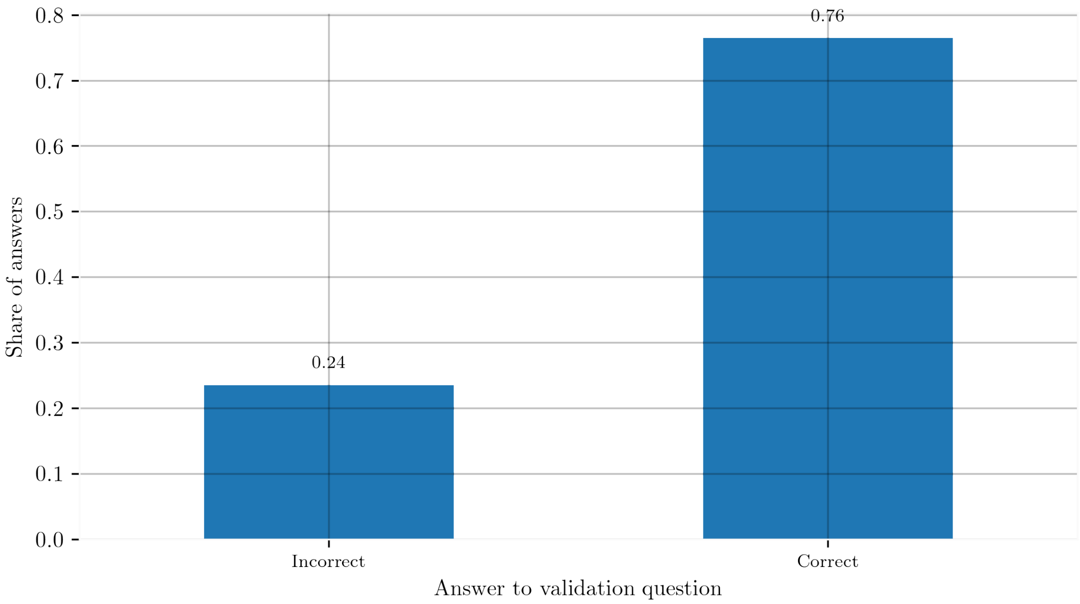
**Notes:** The table shows the average effects of current relative wage misperceptions computed from a discrete-choice model of job search. The base data are individuals ($i$) in the control group of the study sample. For each individual, model outcomes are computed both under current beliefs, $\widetilde{\boldsymbol{W}}_i^0$, and under a counterfactual set of beliefs, $\widetilde{\boldsymbol{W}}_i^1$, that removes all relative wage misperceptions. Details of the model and counterfactual calculations are given in Section 5 and Appendix B. Column (1) reports the share of an individual's applications that would go to a different job in the absence of relative wage misperceptions. Column (2) reports the welfare cost of relative wage misperceptions, expressed as the equivalent across-the-board proportional change in all wages: the amount by which all wages would have to change to leave the job seeker at their current utility level under the counterfactual. Column (3) reports the effect of misperceptions on the average applied-for wage, and Column (4) reports the effect on the average non-pecuniary application value (the value of hiring likelihoods and non-wage amenities, holding wages fixed). Both are expressed as the deviation from the counterfactual in percent. Panels restrict the sample by the size of relative misperceptions, defined as the mean of the absolute value of misperceptions about log wage gaps across all job pairs. Panel A imposes no restriction. Panel B restricts to job seekers in the top decile, and Panel C to job seekers in the bottom decile. Within each panel, the row *Everyone* averages over all individuals in the panel's sample, while *Underestimators* and *Overestimators* are individuals who, respectively, underestimate or overestimate the log gap in average wages between the highest-paying job and the average of the two other jobs. Results in this table normalize the wage-utility curvature parameter to $\delta = 0.25$. Standard errors in parentheses are obtained by a bootstrap procedure that resamples individuals and recomputes both the 2SLS estimate and the model counterfactuals within each bootstrap sample.

Figure A1: Answers to validation question; 'How good is your understanding of what each of these job types means?'



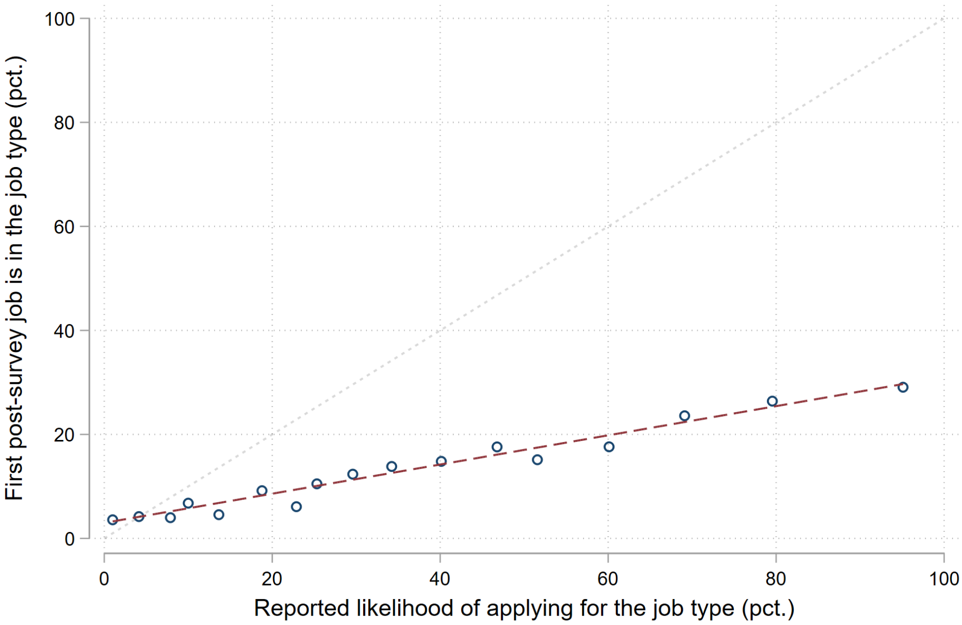

**Note:** This figure shows a histogram of the answers to the question 'How good is your understanding of what each of these job types means?'.

Figure A2: Answers to validation question; placing an example job in the right job type




**Note:** A subset of respondents were asked to place a more detailed example job into the correct survey job type (see Section C.1.2). The histogram shows the shares of respondent getting this correct vs. incorrect.

Figure A3: Likelihood of transitioning into a job type post-survey vs. planned likelihood of applying for it in the survey, binscatter

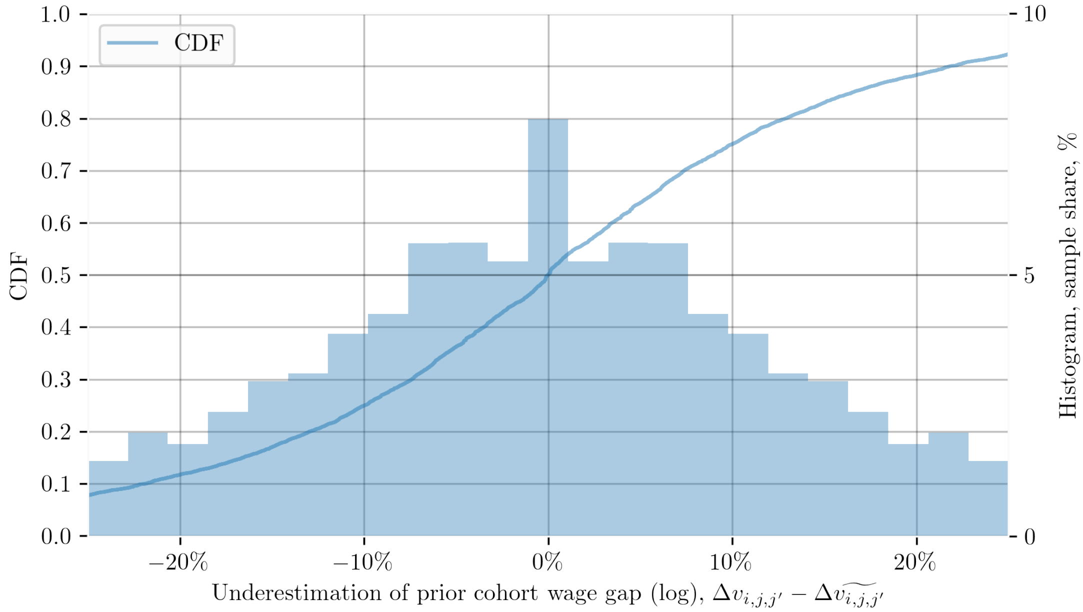



(a) All job seekers


(b) Conditional on finding a job within one of the three job types post-survey

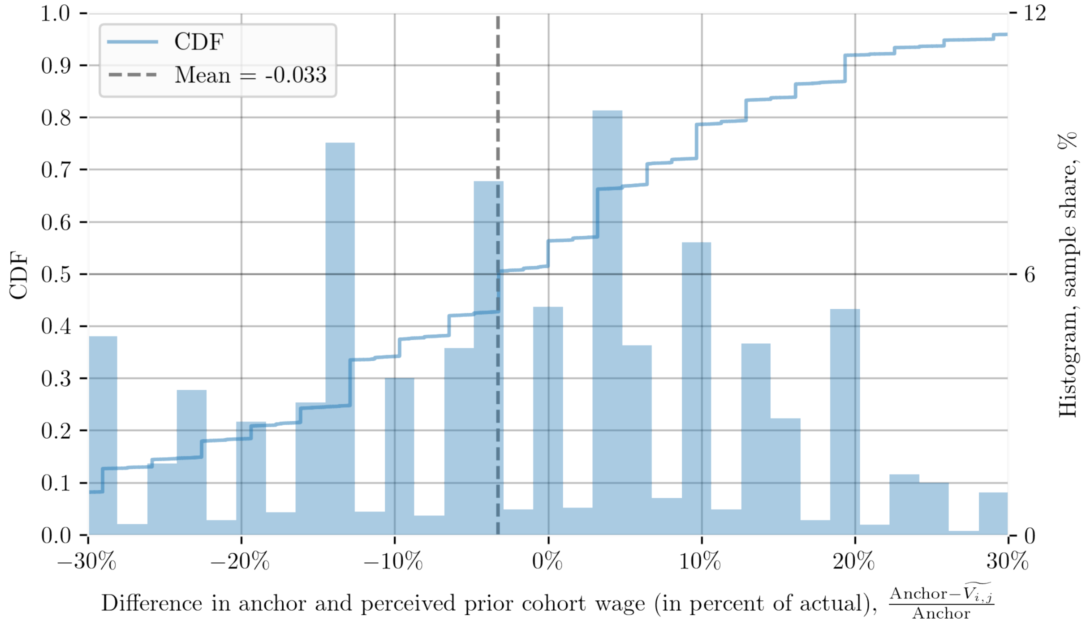
**Note:** The figures plots the likelihood of finding a new job post-survey in a given job type against the reported likelihood of applying for this job type in the survey. It is based on all individual-by-job observations $(i, j)$ in the study sample. Panel A examines this full sample, while Panel B restricts attention to individuals who are in fact observed finding a new job within one of the three survey job types. The circle plot splits the data into bins according to the reported likelihood of applying for the job type in question and then plots the within-bin share actually finding a new job within this type against the mean reported likelihood of applying for the job type. The line plot shows the fit of a corresponding OLS regression (see Table A8 for corresponding OLS estimates).

Figure A4: Distribution of misperceptions about wage gaps; log wage gaps



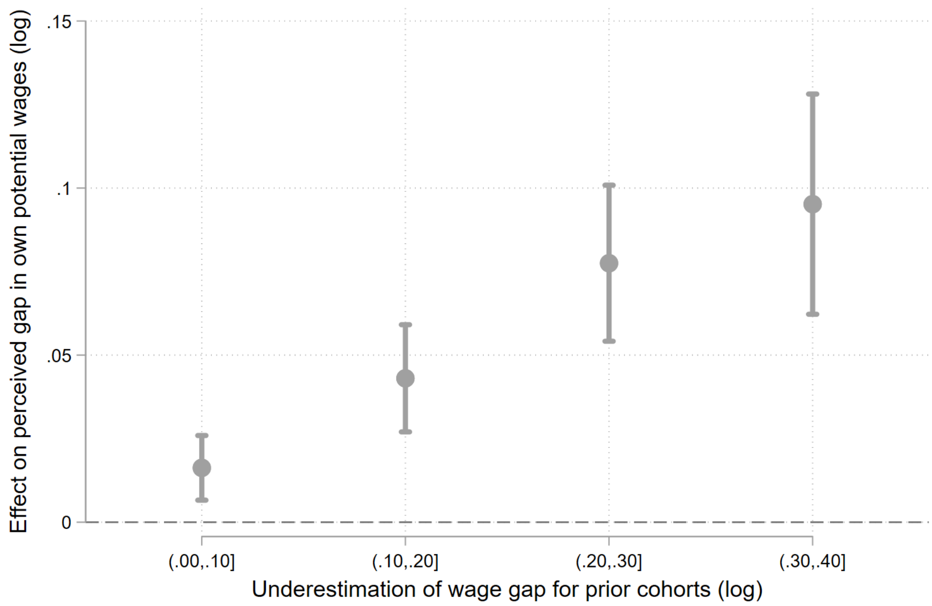

**Note:** The figure is based on all individual-by-job pairs $(i, j, j')$ in the study sample. The figure shows a histogram and empirical CDF for the underestimation of the prior cohort wage gap in logs, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$.

Figure A5: Distribution of perceived wages about individual jobs relative to anchor

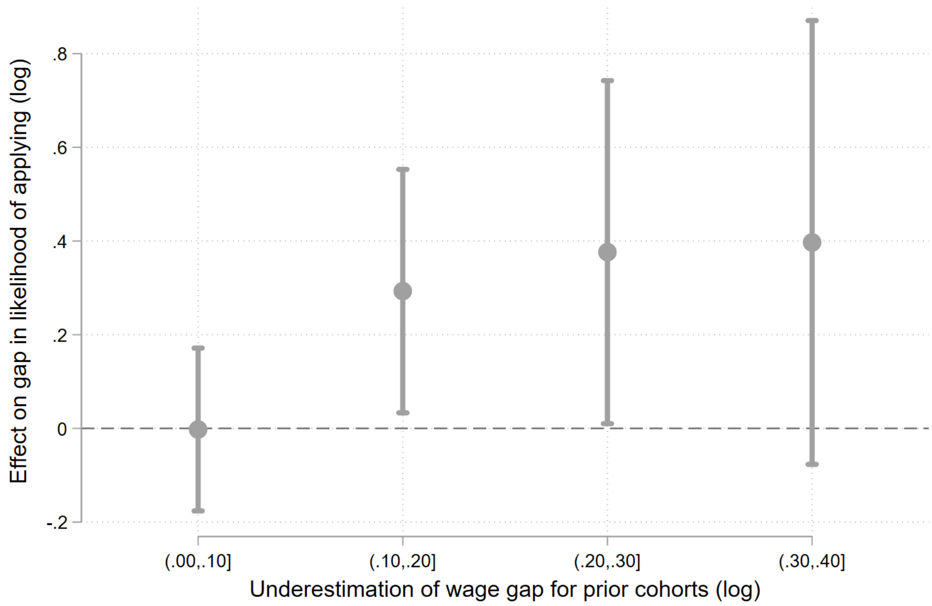



**Note:** The figure is based on all individual-by-jobs $(i, j)$ in the study sample. The figure shows the histogram and empirical CDF for the difference in the anchor vs. perceived prior cohort wage (in percent of the anchor). The same anchor of DKK31,000 was shown to all respondents.

Figure A6: Heterogeneous effects by size of initial misperceptions, alternative bins 1



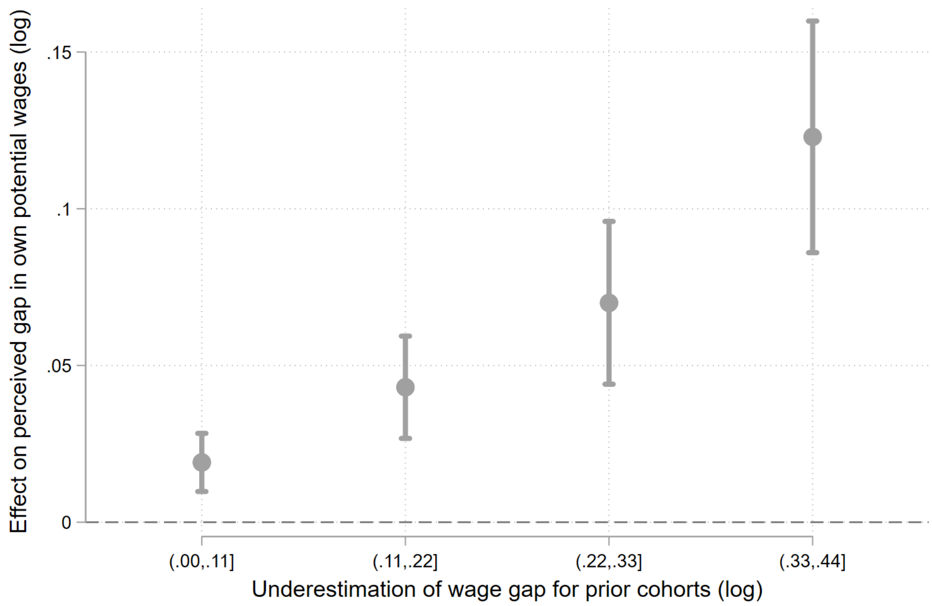

(a) Treatment effect on $\Delta\widetilde{w_{i,j,j'}}$



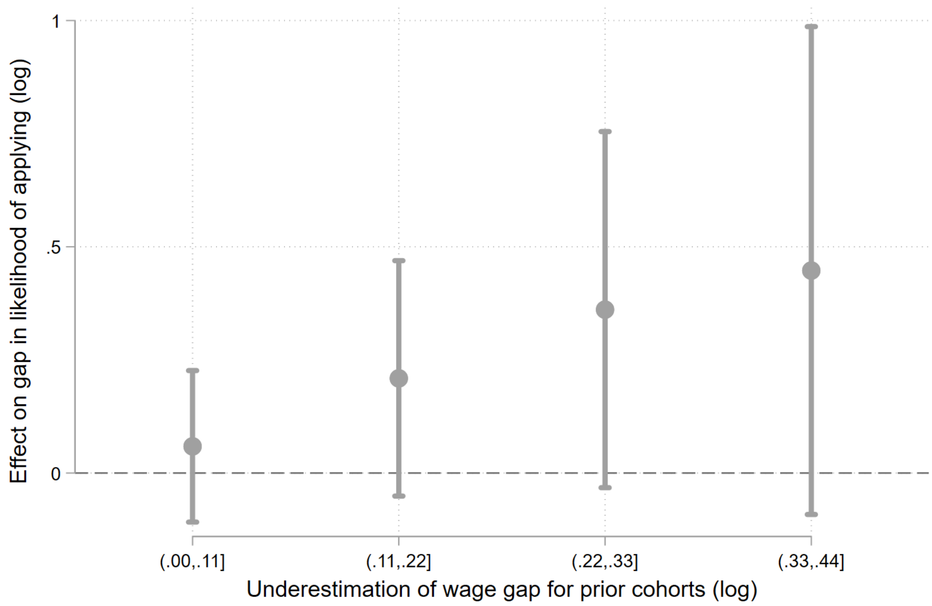

(b) Treatment effect on $\Delta\pi_{i,j,j'}$

**Note:** These figures examine the sensitivity of Figure 4 to alternative choices of bins. The figure is based on all individual-by-job pairs $(i, j, j')$ in the study sample. The data is split into four equal-width bins according to the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$, with the job pairs ordered such that $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}} > 0$. The figures plot the effect of the information treatment within each bin, estimated via a linear regression that includes a constant and the treatment dummy. In Panel A, the outcome variable is the perceived gap in (log) own potential wages between the jobs, $\Delta\widetilde{w_{i,j,j'}}$. In Panel B, the outcome variable is the (log) relative likelihood of applying to the two jobs, $\Delta\pi_{i,j,j'}$. Error bars show 95% confidence intervals using clustering on individuals.

Figure A7: Heterogeneous effects by size of initial misperceptions, alternative bins 2




(a) Treatment effect on $\Delta\widetilde{w_{i,j,j'}}$




(b) Treatment effect on $\Delta\pi_{i,j,j'}$

**Note:** These figures examine the sensitivity of Figure 4 to alternative choices of bins. The figure is based on all individual-by-job pairs $(i, j, j')$ in the study sample. The data is split into four equal-width bins according to the underestimation of the log wage gap for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$, with the job pairs ordered such that $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}} > 0$. The figures plot the effect of the information treatment within each bin, estimated via a linear regression that includes a constant and the treatment dummy. In Panel A, the outcome variable is the perceived gap in (log) own potential wages between the jobs, $\Delta\widetilde{w_{i,j,j'}}$. In Panel B, the outcome variable is the (log) relative likelihood of applying to the two jobs, $\Delta\pi_{i,j,j'}$. Error bars show 95% confidence intervals using clustering on individuals.

# B Discrete choice job search framework

This section contains additional details and derivations regarding the discrete choice job search framework used in the main text.

## B.1 Model setup, recap

The full model setup and notation is discussed in Section 5 of the main text. We briefly recap the key elements here for completeness. At a point in time, job seeker $i$ has the opportunity to apply for one of three different jobs indexed by $j = 1, 2, 3$. Alternatively, she can apply to none of them and continue searching which we will treat as option $j = 0$. Let $Y_{i,j}$ denote the continuation value of being hired into job $j$ and let $U_i$ denote the continuation value of continued search. The surplus value of being hired into job $j$ is assumed to be:

$$S_{i,j} = Y_{i,j} - U_i = \Psi_i W_{i,j}^{\delta} Z_{i,j} \xi_{i,j}. \tag{B.1}$$

Here $W_{i,j}$ is the wage worker $i$ would earn in job $j$, $Z_{i,j}$ summarizes non-wage amenities, and $\xi_{i,j}$ is an idiosyncratic taste shock capturing other utility-relevant characteristics of the job. The parameter $\Psi_i$ allows for individual heterogeneity in the general surplus from employment, while $\delta$ governs the curvature of surplus with respect to wages.

The surplus value of an application to job $j$ relative to continued search, $A_{i,j}$, will equal the probability of being hired times the surplus from employment. Letting $P_{i,j}$ denote the probability of being hired at job $j$, we have

$$A_{i,j} = P_{i,j} S_{i,j}. \tag{B.2}$$

In what follows it is useful to collect the non-wage components of the surplus value of applying in the term

$$H_{i,j} = P_{i,j} Z_{i,j} \xi_{i,j}, \tag{B.3}$$

which we refer to as the non-pecuniary value of applying to job $j$. With this notation, $A_{i,j} = \Psi_i W_{i,j}^{\delta} H_{i,j}$.

When making application decisions, job seekers need not base choices on the actual application surplus value $A_{i,j}$. Instead, they act on the perceived surplus value $\widetilde{A_{i,j}}$, determined by perceived wages $\widetilde{W_{i,j}}$, perceived hiring probabilities $\widetilde{P_{i,j}}$ and perceived non-wage amenities $\widetilde{Z_{i,j}}$:

$$\widetilde{S_{i,j}} = \Psi_i \widetilde{W_{i,j}}^{\delta} \widetilde{Z_{i,j}} \xi_{i,j}, \tag{B.4}$$

$$\widetilde{A_{i,j}} = \widetilde{P_{i,j}} \widetilde{S_{i,j}}. \tag{B.5}$$

Additionally, we assume that applying for some job imposes an application cost of $c_i$.

Finally, we adopt the convenient notation that $A_{i,0}$ and $\widetilde{A_{i,0}}$ denote the actual and perceived surplus value of choosing not to apply anywhere. Since not applying anywhere guarantees that the worker continues searching, the surpluses $A_{i,0}$ and $\widetilde{A_{i,0}}$ are both mechanically 0.

## B.2 Choice probabilities and the value of an application

Let $j_i^*$ denote the actual application choice that the worker makes, with $j_i^* = 0$ corresponding to the choice to not apply anywhere. Optimizing behavior implies that the worker chooses the option that offers the highest perceived surplus:

$$j_i^* = \text{argmax}_{j \in \{0,1,2,3\}} \widetilde{A_{i,j}} - \mathbf{1}_{\{j \neq 0\}} c_i \tag{B.6}$$

Since the optimal choice is unaffected by monotone transformations of the objective, we can transform the objective in (B.6) by adding the application cost and taking logs: Defining

$$Q_{i,j} = \log\Big(\Big(\widetilde{A_{i,j}} - \mathbf{1}_{\{j \neq 0\}} c_i\Big) + c_i\Big)$$

the problem (B.6) becomes equivalent to

$$j_i^* = \text{argmax}_{j\in\{0,1,2,3\}} \quad Q_{i,j} \tag{B.7}$$

To see why this is useful, note that for $j = 1, 2, 3$ we have (using lowercase to denote logged values as in the main text)

$$Q_{i,j} = \psi_i + \widetilde{p_{i,j}} + \delta\widetilde{w_{i,j}} + \widetilde{z_{i,j}} + \log \xi_{i,j} \tag{B.8}$$

while for $j = 0$

$$Q_{i,0} = \log c_i \tag{B.9}$$

Under the assumed Frechet distribution, $\log \xi_{i,j}$ follows a Type I extreme value distribution with scale parameter $\sigma$. Equations (B.7), (B.8) and (B.9) therefore describe a standard discrete choice logit framework, albeit with a deterministic outside option (the option of not applying anywhere). Standard derivations for discrete choice logit models therefore yield familiar expressions for the choice probabilities, which we denote by $\Pi^*_{i,j} = \Pr(j_i^* = j)$. For $j = 1, 2, 3$ we can write this as in equation (9) from the main text:

$$\Pi^*_{i,j} = \frac{\exp((\alpha_i + \widetilde{p_{i,j}} + \delta\widetilde{w_{i,j}} + \widetilde{z_{i,j}})/\sigma)}{1 + \sum\limits_{j'=1,2,3} \exp((\alpha_i + \widetilde{p_{i,j'}} + \delta\widetilde{w_{i,j'}} + \widetilde{z_{i,j'}})/\sigma)} \tag{B.10}$$

Here $\alpha_i$ is an alternative-invariant constant that indexes the overall attractiveness of applying to one of the three jobs relative to the deterministic outside option of not applying. It is defined as:

$$\alpha_i = \psi_i - \log c_i + \sigma \log \left( \frac{\exp\left[\sum_{j'=1}^{3} \exp((\psi_i - \log c_i + \widetilde{p_{i,j'}} + \delta\widetilde{w_{i,j'}} + \widetilde{z_{i,j'}})/\sigma)\right] - 1}{\sum_{j'=1}^{3} \exp((\psi_i - \log c_i + \widetilde{p_{i,j'}} + \delta\widetilde{w_{i,j'}} + \widetilde{z_{i,j'}})/\sigma)} \right).$$

For $j = 0$ we have

$$\Pi^*_{i,0} = \frac{1}{1+\sum\limits_{j'=1,2,3} \exp((\alpha_i+\widetilde{p_{i,j'}}+\delta\widetilde{w_{i,j'}}+\widetilde{z_{i,j'}})/\sigma)} \tag{B.11}$$

For much of our analysis, we will be focusing on the likelihood that a given application goes to a particular job, i.e., the likelihood of applying to a given job $j$ conditional on applying *somewhere*. We denote this by $\overline{\Pi}^*_{i,j}$:

$$\overline{\Pi}^*_{i,j} = \frac{\Pi^*_{i,j}}{1-\Pi^*_{i,0}} = \frac{\exp((\widetilde{p_{i,j}}+\delta\widetilde{w_{i,j}}+\widetilde{z_{i,j}})/\sigma)}{\sum\limits_{j'=1,2,3} \exp((\widetilde{p_{i,j'}}+\delta\widetilde{w_{i,j'}}+\widetilde{z_{i,j'}})/\sigma)} \tag{B.12}$$

Finally, for the purpose of the welfare counterfactual later, it will be convenient to let $A^E_{i,j}$ denote the expected value of an application to job $j$, conditional on choosing to apply to job $j$:

$$\begin{aligned} A^E_{i,j} =& E[A_{i,j} \mid j^*_i = j] \\ =& \Psi_i P_{i,j} W^\delta_{i,j} Z_{i,j} E[\xi_{i,j} \mid j^*_i = j]. \end{aligned}$$

Letting $\Omega_i$ be the expected value of making an application, we then have:

$$\Omega_i = \sum_{j=1,2,3} \overline{\Pi}^*_{i,j} \times \left( A^E_{i,j} - c_i + U_i \right) \tag{B.13}$$

## B.3 Link to data, 2SLS analysis and exclusion restriction

The model framework above links naturally to the survey data we use in our reduced form analysis. Elicited beliefs about the potential wage each job in the survey would offer the respondent is a direct measure of $\widetilde{W_{i,j}}$. Similarly, the reported probabilities of applying to each job in the survey, corresponds to the choice probabilities in equation (B.10) and (B.11). As convenient notation, we let $\Pi_{i,j}$ (without an asterisk) denote the reported probabilities in the data.[30]

[30] We do this to distinguish the data from the endogenous objects in the model. The distinction becomes relevant when we turn to consider counterfactuals further below.

Taking logs and differencing (B.10) across two jobs $j$ and $j'$ now yields the equation (with $\Delta$ denoting differences across pairs of jobs, as in the main text):

$$\Delta\pi_{i,j,j'} = \beta_1 \Delta\widetilde{w_{i,j,j'}} + \Delta u_{i,j,j'} \tag{B.14}$$

Here we have defined the random variable (error term) $\Delta u_{i,j,j'}$ as

$$\Delta u_{i,j,j'} = \frac{1}{\sigma}\left(\Delta\widetilde{p_{i,j,j'}} + \Delta\widetilde{z_{i,j,j'}}\right)$$

Additionally, we have defined the key reduced form parameter $\beta_1$ as

$$\beta_1 = \frac{\delta}{\sigma} \tag{B.15}$$

As emphasized in the main text, (B.14) implies that $\beta_1$ has the reduced form interpretation of being the causal effect of the wage gap between two jobs on the relative likelihood of applying to them.[31] Specifically, $\beta_1$ is the elasticity of the relative application likelihood between two jobs ($\Pi_{i,j}/\Pi_{i,j'}$) with respect to the perceived relative wage offers ($\widetilde{W}_{i,j}/\widetilde{W}_{i,j'}$). As shown in (B.15), however, $\beta_1$, also has a direct, formal mapping to key primitive parameters in the model framework. We use this further below to compute model counterfactuals from the reduced form estimates.

Equation (B.14) and the model framework can also be used to formally discuss the role played by the random information treatment and instrumental variable assumptions in our 2SLS results. The error term in (B.14), $\Delta u_{i,j,j'}$, reflects beliefs about all non-pecuniary differences that affect the attractiveness of applying to job $j$ versus $j'$, specifically the perceived difference in application success probability, $\Delta\widetilde{p_{i,j,j'}}$ and in the non-wage amenities, $\Delta\widetilde{z_{i,j,j'}}$. In general, we expect job seekers with particular beliefs about wage differences to likely also have particular beliefs about differences in these other dimensions. This leads to a classic identification problem because the regressor in (B.14) is correlated with the error term.

[31] To be precise here, under the model, $\beta_1$ is a causal effect in the sense that it measures the effect of changes in perceived wages, keeping all other non-random variables constant and the distribution of the taste shocks unchanged.

We address this identification problem by leveraging our randomized treatment assignment, $T_i$, in a 2SLS framework. As covered in Section 4 of the main text, for a given level of the wage gap underestimation for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$, our information treatment generates variation in perceived own potential wages, $\Delta\widetilde{w_{i,j,j'}}$. This means that it can be used to construct a relevant instrument. Moreover, due to random assignment, treated and untreated job seekers cannot have systematically different beliefs prior to treatment. Under the additional exclusion restriction that the information treatment also does not change beliefs about non-wage amenities or the application success probability, treatment will be independent of non-pecuniary beliefs, both overall and for a given level of the wage gap underestimation for prior cohorts, $\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}$:

$$T_i \perp \left(\Delta\widetilde{p_{i,j,j'}}, \Delta\widetilde{z_{i,j,j'}}\right) \Big| \left(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right) \tag{B.16}$$

As covered in Section 4.4, we use a range of additional questions in our survey to test this exclusion restriction directly and find it to be well-supported by the data. If we finally impose a standard 'linearity-of-controls' functional form assumption, we arrive at the second stage equation and instrument exogeneity condition used in our 2SLS analysis:[32]

$$\Delta\pi_{i,j,j'} = \beta_1 \, \Delta\widetilde{w_{i,j,j'}} + \beta_2 \left(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right) + \varepsilon_{i,j,j'} \tag{B.17}$$

$$E[\varepsilon_{i,j,j'} | T_i, \Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}] = 0 \tag{B.18}$$

[32]The usual functional form assumption imposes that in the absence of treatment, the conditional expectation of the error term is linear in the conditioning variable:

$$E\left[\Delta u_{i,j,j'} | T_i = 0, \left(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right)\right] = \beta_2 \left(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right)$$

With this, the second stage equation then arises simply by appropriately defining its error term:

$$\varepsilon_{i,j,j'} = \Delta u_{i,j,j'} - E\left[\Delta u_{i,j,j'} | \left(\Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right), T_i = 0\right]$$

The instrument exogeneity condition follows from (B.16) because $\Delta u_{i,j,j'} = \frac{1}{\sigma}\left(\Delta\widetilde{p_{i,j,j'}} + \Delta\widetilde{z_{i,j,j'}}\right)$:

$$\begin{aligned} &E\left[\varepsilon_{i,j,j'} | T_i, \Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right] = \\ &E\left[\Delta u_{i,j,j'} | T_i, \Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right] - E\left[\Delta u_{i,j,j'} | T_i = 0, \Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right] = \\ &E\left[\Delta u_{i,j,j'} | \Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right] - E\left[\Delta u_{i,j,j'} | \Delta v_{i,j,j'} - \Delta\widetilde{v_{i,j,j'}}\right] = 0 \end{aligned}$$

## B.4 Own Wage Elasticity of Applications

As discussed above, the parameter of interest in our 2SLS analysis, $\beta_1$, measures the effect of the perceived *wage gap* between two jobs on the *relative likelihood* of applying to them. Using the discrete choice framework above, however, we can convert this into an own wage elasticity of applications to a given job, i.e., a measure of how much more likely job seekers are to apply for a given job when this job unilaterally increases its wage. This is a key parameter in the literature on job search and employer market power.

Differentiating the logged choice probability in (B.10) (including the definition of $\alpha_i$) shows that for a given person $i$ and job $j$ the own wage elasticity is:[33]

$$\mathcal{E}_{i,j} = \frac{\partial \ln \Pi^*_{i,j}}{\partial \widetilde{w_{i,j}}} = \beta_1 \left[ 1 - \Pi^*_{i,j} \left( \frac{1 - \Pi^*_{i,0}\left(1 - \log \Pi^*_{i,0}\right)}{\left(1 - \Pi^*_{i,0}\right)^2} \right) \right] \tag{B.19}$$

To arrive at an aggregate elasticity over the sample, we plug in our estimate for $\beta_1$ as well as the observed choice probabilities $\Pi_{i,j}$, and then average over the $N$ job seekers in our data and the three jobs they each face:

$$\widehat{\mathcal{E}}^a = \frac{1}{N} \sum_i \left( \frac{1}{3} \sum_{j=1,2,3} \hat{\beta}_1 \left[ 1 - \Pi_{i,j} \left( \frac{1 - \Pi_{i,0}(1 - \log \Pi_{i,0})}{(1 - \Pi_{i,0})^2} \right) \right] \right) \tag{B.20}$$

## B.5 Counterfactuals without relative wage misperceptions

In Section 5 of the main text, we combine our reduced form estimates and the discrete choice framework to assess the overall costs of relative wage misperceptions. Specifically, for our control group of untreated individuals ($T_i = 0$), we compare actual current outcomes to a counterfactual in which all relative wage misperceptions are removed. The subsections below go through the key derivations and additional assumptions underlying this counterfactual.

For the purpose of describing the counterfactual, we first modify and introduce ad-

[33] Note that the expression for the elasticity here differs slightly from the standard logit elasticity formula $\mathcal{E}_{i,j} = \beta_1(1 - \Pi^*_{i,j})$. The difference reflects the deterministic outside option since the value of not applying anywhere is not subject to a random taste shock. The difference is quantitatively unimportant however because the likelihood of not applying anywhere is small in our data and $\frac{1-\Pi^*_{i,0}\left(1-\log \Pi^*_{i,0}\right)}{\left(1-\Pi^*_{i,0}\right)^2} \to 1$ as $\Pi^*_{i,0} \to 0$.

ditional notation relative to the preceding sections. Let $\boldsymbol{X} = (X_1, X_2, X_3)$ and $\widetilde{\boldsymbol{X}} = (\widetilde{X_1}, \widetilde{X_2}, \widetilde{X_3})$ be arbitrary vectors of wages for the three jobs a given worker faces. Chiefly, we are interested in examining how application choices would be different under different beliefs. We thus let $j_i^*(\widetilde{\boldsymbol{X}})$ be the workers optimal application choice if the job seeker perceives the wages to be $\widetilde{\boldsymbol{X}}$ and let $\Pi_{i,j}^*\left(\widetilde{\boldsymbol{X}}\right)$ and $\overline{\Pi}_{i,j}^*\left(\widetilde{\boldsymbol{X}}\right)$ denote the corresponding application choice probabilities. Additionally, we consider counterfactuals for the utility value of making an application. We let $\Omega_i(\boldsymbol{X}, \widetilde{\boldsymbol{X}})$ denote the utility value of making an application if the wages are actually $\boldsymbol{X}$ but the worker perceives the wages to be $\widetilde{\boldsymbol{X}}$. In what follows, we additionally use $U_i\left(\boldsymbol{X}, \widetilde{\boldsymbol{X}}\right)$ and $A_{i,j}^E\left(\boldsymbol{X}, \widetilde{\boldsymbol{X}}\right)$ for the corresponding continuation value of unemployment and conditional surplus value of applying to a given job.

In terms of the specific counterfactual comparison we are interested in, we let $\boldsymbol{W_i} = (W_{i,1}, W_{i,2}, W_{i,3})$ be the vector of actual wages in the three jobs for person $i$ and $\widetilde{\boldsymbol{W}_i^0} = (\widetilde{W_{i,1}^0}, \widetilde{W_{i,2}^0}, \widetilde{W_{i,3}^0})$ be their vector of currently perceived wages. We are then interested in computing outcomes under a counterfactual set of wage beliefs $\widetilde{\boldsymbol{W}_i^1} = (\widetilde{W_{i,1}^1}, \widetilde{W_{i,2}^1}, \widetilde{W_{i,3}^1})$ in which there is no relative wage misperception. This corresponds to $\widetilde{\boldsymbol{W}_i^1}$ satisfying:

$$\widetilde{\boldsymbol{W}_i^1} = \iota_i \boldsymbol{W}_i \tag{B.21}$$

for some $\iota_i > 0$. Note that the constant $\iota_i$ here can be interpreted as the extent of overestimation/underestimation of the general level of wages. As will become clear below, our counterfactual for application behavior calculation is invariant to $\iota_i$, reflecting that these application counterfactuals only capture the effects of removing *relative* wage misperceptions, not misperceptions about the overall level of wages.

To make sure that our welfare counterfactual also isolates the costs of relative wage misperceptions distorting applications across jobs, our welfare counterfactual will impose that the likelihood of applying to some job (i.e. the rate of making applications) is unchanged. This corresponds to choosing $\iota_i$ such that

$$\Pi_{i,0}^*\left(\widetilde{\boldsymbol{W}_i^1}\right) = \Pi_{i,0}^*\left(\widetilde{\boldsymbol{W}_i^0}\right)$$

This restriction ensures that our welfare counterfactual is not affected by misperceptions about the overall wage level that lead workers to apply to more or fewer jobs overall.

### B.5.1 Counterfactual application behavior

The first counterfactual of interest regards how application behavior would change if job seeker $i$ did not have any misperceptions about relative wages. Specifically, we want to compare the likelihood that an application currently goes to a particular job, $\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^0}\right)$ to the counterfactual likelihood without relative wage misperceptions $\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^1}\right)$. Given that we are focusing on individuals in the control group, $\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^0}\right)$ can be directly computed from the survey responses about likelihood of applying to the different jobs:

$$\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^0}\right) = \frac{\Pi_{i,j}}{1-\Pi_{i,0}}$$

Further note that *if* we knew the current perceived and actual wages, $\widetilde{\boldsymbol{W}_i^0}$ and $\boldsymbol{W_i}$, the only thing necessary to compute $\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^1}\right)$ is the reduced form parameter $\beta_1$ since (B.12) and (B.21) imply:

$$\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^1}\right) = \frac{\left(\frac{W_{i,j}}{\widetilde{W^0_{i,j}}}\right)^{\beta_1}\frac{\Pi_{i,j}}{1-\Pi_{i,0}}}{\sum\limits_{j'=1,2,3}\left(\frac{W_{i,j'}}{\widetilde{W^0_{i,j'}}}\right)^{\beta_1}\frac{\Pi_{i,j'}}{1-\Pi_{i,0}}}$$

For the control group we in fact observe $\widetilde{W^0_{i,j}}$ directly from the survey questions:

$$\widetilde{W^0_{i,j}} = \widetilde{W_{i,j}}$$

To compute the counterfactual however, we still need to take a stand on how actual potential wage differences differ from perceptions to get a handle on $\boldsymbol{W_i}$. Given the content of our survey, our benchmark will be to simply equate misperceptions about own potential (log) wage gaps to the measured misperceptions about wage gaps for prior cohorts from the survey, i.e. we assume:

$$\Delta v_{i,j,j'} - \widetilde{\Delta v_{i,j,j'}} = \Delta w_{i,j,j'} - \widetilde{\Delta w^0_{i,j,j'}} \tag{B.22}$$

Since this assumption additionally allows us to infer $\boldsymbol{W}_i$, imposing (B.22) is enough for us to compute the counterfactual application likelihood $\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^1}\right)$ using our estimate for $\beta_1$ and the equation further above.

Having computed the counterfactual application probabilities, our first measure of distortions is the share of $i$'s applications that would go to different jobs in the absence of relative wage misperceptions. We summarize this by computing the predicted total change in the shares of applications going to the different jobs if relative misperceptions were removed:[34]

$$\frac{1}{2}\sum_{j=1,2,3}\left|\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^1}\right)-\overline{\Pi}^*_{i,j}\left(\widetilde{\boldsymbol{W}_i^0}\right)\right|$$

### B.5.2 Welfare costs

Finally, leaning further into the model structure, we can also use this framework to assess the welfare costs of relative wage misperceptions.

Specifically, we consider what the total utility value of sending an application is with and without relative wage misperceptions, i.e. how much does $\Omega_i(\boldsymbol{W}_i, \widetilde{\boldsymbol{W}_i^0})$ differ from $\Omega_i(\boldsymbol{W}_i, \widetilde{\boldsymbol{W}_i^1})$. To make this comparison on a meaningful scale, we ask how much all wages would have to decrease in order to deliver the same welfare loss as the introduction of relative wage misperceptions. In the absence of relative wage misperceptions, if all wages for $i$ are scaled down by $k_i$, the value of making application will be $\Omega_i(k_i\boldsymbol{W}_i, \widetilde{\boldsymbol{W}_i^1})$. Accordingly, we compute the value $k_i$ that satisfies:

$$\Omega_i(k_i\boldsymbol{W}_i, \widetilde{\boldsymbol{W}_i^1}) = \Omega_i(\boldsymbol{W}_i, \widetilde{\boldsymbol{W}_i^0}) \tag{B.23}$$

To do this requires us to impose three additional assumptions. First, we need to take a stand on the continuation value if workers do not get a job but continue searching, $U_i$. Mirroring standard job search models, we simply assume that such job seekers earn some exogenous flow utility $b_i$ and get the opportunity to make an application again at

[34]Invoking a law of large numbers, this measure formally measures the share of applications that will have gone to a different job, after $i$ have made a large number of applications. A different summary measure would be to focus on the probability that a given single application goes to a different job, e.g. $Pr(j_i^*(\widetilde{W_{i,j}^0}) \neq j_i^*(\widetilde{W_{i,j}^1}))$. Experimenting with this type of measure gives virtually identical results.

some fixed, individual-specific Poisson arrival rate, $\lambda_i$. Assuming also that the application decision problem is stationary and a time discounting rate of $\rho_i$, this implies the following continuous time Bellman equation:

$$\rho_i U_i(\boldsymbol{X}, \widetilde{\boldsymbol{X}}) = b_i + \lambda_i \Big(1 - \Pi^*_{i,0}(\widetilde{\boldsymbol{X}})\Big)\Big(\Omega_i(\boldsymbol{X}, \widetilde{\boldsymbol{X}}) - U_i(\boldsymbol{X}, \widetilde{\boldsymbol{X}})\Big) \tag{B.24}$$

Second, to assess actual utilities, we also need to take a stand on the relationship between perceived and actual non-pecuniary values, i.e. hiring likelihoods and non-wage amenities. Here we simply adopt the benchmark assumption that workers have correct beliefs about non-pecuniary values:

$$P_{i,j} = \widetilde{P_{i,j}} \tag{B.25}$$

$$Z_{i,j} = \widetilde{Z_{i,j}} \tag{B.26}$$

Finally, we need to impose a parameter restriction on the surplus utility from jobs. This reflects the usual challenge that choice behavior does not separately identify the scale of the taste shocks.[35] For our main results we impose that surplus is linear in the wage, corresponding to risk neutrality with respect to wages

$$\delta = 1 \tag{B.27}$$

As we show in Tables A16 and A17, setting $\delta < 1$ to introduce risk aversion leads to slightly larger estimated welfare costs of misperceptions.

With the additional assumptions (B.24), (B.25), and (B.26) and a normalization like (B.27), we are able to compute the utility cost of relative wage misperceptions, $k_i$ for each individual in our control group. Subsection B.5.4 gives the details of this derivation. The key insight however is that for given $\sigma$, the conditional choice probabilities without relative

[35] From our reduced form equations, the identified parameter governing application behavior is $\beta_1 = \frac{1}{\sigma}\delta$. Scaling up $\sigma$ and $\delta$ by the same amount thus leaves application behavior unchanged.

misperceptions identify the deterministic application values up to scale since

$$\eta_i \left( \overline{\Pi}^*_{i,j} \left( \widetilde{\boldsymbol{W}^1_i} \right) \right)^\sigma = \Psi_i P_{i,j} W^\delta_{i,j} Z_{i,j} \tag{B.28}$$

with $\eta_i$ an alternative-invariant constant.

Finally, to expand on how the welfare costs arise, it is instructive to examine how distortions lead the job seekers to target jobs with systematically different wage-levels or non-pecuniary values. Under some wage beliefs $\widetilde{\boldsymbol{X}}$, we can consider the average applied-for wage, $E\left[W_{i,j^*_i(\widetilde{\boldsymbol{X}})} \mid j^*_i(\widetilde{\boldsymbol{X}}) \neq 0\right]$, as well as the average non-pecuniary value of a sent application, $E\left[H_{i,j^*_i(\widetilde{\boldsymbol{X}})} \mid j^*_i(\widetilde{\boldsymbol{X}}) \neq 0\right]$. As we detail in Subsection B.5.4, the assumptions above allow us to compute the effect of wage misperceptions on both these quantities:

$$\frac{E\left[W_{i,j^*_i(\widetilde{\boldsymbol{W}^0_i})} \mid j^*_i(\widetilde{\boldsymbol{W}^0_i}) \neq 0\right] - E\left[W_{i,j^*_i(\widetilde{\boldsymbol{W}^1_i})} \mid j^*_i(\widetilde{\boldsymbol{W}^1_i}) \neq 0\right]}{E\left[W_{i,j^*_i(\widetilde{\boldsymbol{W}^1_i})} \mid j^*_i(\widetilde{\boldsymbol{W}^1_i}) \neq 0\right]} \tag{B.29}$$

and

$$\frac{E\left[H_{i,j^*_i(\widetilde{\boldsymbol{W}^0_i})} \mid j^*_i(\widetilde{\boldsymbol{W}^0_i}) \neq 0\right] - E\left[H_{i,j^*_i(\widetilde{\boldsymbol{W}^1_i})} \mid j^*_i(\widetilde{\boldsymbol{W}^1_i}) \neq 0\right]}{E\left[H_{i,j^*_i(\widetilde{\boldsymbol{W}^1_i})} \mid j^*_i(\widetilde{\boldsymbol{W}^1_i}) \neq 0\right]} \tag{B.30}$$

### B.5.3 Implementation and inference

To generate the counterfactual results presented in the main text, we compute the different quantities of interest described above for each individual in the control group, while replacing $\beta_1$ with its estimated value $\hat{\beta}_1$ from the baseline 2SLS specification (Table 5, Panel C, Column 1). We then compute averages or percentiles over the sample as relevant. To quantify the sampling uncertainty around these estimated counterfactuals, we produce standard errors via a bootstrap procedure that resamples individuals and then recomputes both the 2SLS estimate and the counterfactual quantities of interest within each bootstrap sample. For the welfare counterfactual, we use the normalization in (B.27).

### B.5.4 Detailed derivation of welfare counterfactuals

To compute our measure of the welfare cost $k_i$, we first reorganize the assumed Bellman equation for the value of continued search, (B.24), to arrive at:

$$U_i(\boldsymbol{X},\widetilde{\boldsymbol{X}}) = \frac{b_i+\lambda_i\Big(1-\Pi^*_{i,0}(\widetilde{\boldsymbol{X}})\Big)\Omega_i(\boldsymbol{X},\widetilde{\boldsymbol{X}})}{\rho_i+\lambda_i\Big(1-\Pi^*_{i,0}(\widetilde{\boldsymbol{X}})\Big)}.$$

Plugging this into the equation for the value of an application, (B.13), and rearranging yields:

$$\Omega_i(\boldsymbol{X},\widetilde{\boldsymbol{X}}) = \frac{b_i}{\rho_i}+\frac{\rho_i+\lambda_i\Big(1-\Pi^*_{i,0}(\widetilde{\boldsymbol{X}})\Big)}{\rho_i}\left(\sum_{j=1,2,3}\overline{\Pi}^*_{i,j}(\widetilde{\boldsymbol{X}})A^E_{i,j}(\boldsymbol{X},\widetilde{\boldsymbol{X}})-c_i\right).$$

The normalization imposed above fixes $\Pi^*_{i,0}\Big(\widetilde{\boldsymbol{W}_i^1}\Big) = \Pi^*_{i,0}\Big(\widetilde{\boldsymbol{W}_i^0}\Big)$. This means that the terms involving $b_i$, $c_i$, $\lambda_i$ and $\rho_i$ all cancel when comparing the two sides of (B.23). Therefore, the equation defining $k_i$ is equivalent to:

$$\sum_{j=1,2,3}\overline{\Pi}^*_{i,j}(\widetilde{\boldsymbol{W}_i^1})A^E_{i,j}(k_i\boldsymbol{W}_i,\widetilde{\boldsymbol{W}_i^1}) = \sum_{j=1,2,3}\overline{\Pi}^*_{i,j}(\widetilde{\boldsymbol{W}_i^0})A^E_{i,j}(\boldsymbol{W}_i,\widetilde{\boldsymbol{W}_i^0}).$$

Since scaling actual wages by $k_i$ scales the surplus value of every application by $k_i^\delta$, this gives:

$$k_i = \left(\frac{\sum_{j=1,2,3}\overline{\Pi}^*_{i,j}(\widetilde{\boldsymbol{W}_i^0})A^E_{i,j}(\boldsymbol{W}_i,\widetilde{\boldsymbol{W}_i^0})}{\sum_{j=1,2,3}\overline{\Pi}^*_{i,j}(\widetilde{\boldsymbol{W}_i^1})A^E_{i,j}(\boldsymbol{W}_i,\widetilde{\boldsymbol{W}_i^1})}\right)^{1/\delta} \tag{B.31}$$

Next, using (B.28), we have for any wage beliefs $\widetilde{\boldsymbol{X}}$:

$$\begin{aligned} A^E_{i,j}(\boldsymbol{W}_i,\widetilde{\boldsymbol{X}}) &= \Psi_i W^\delta_{i,j}P_{i,j}Z_{i,j}E\Big[\xi_{i,j}\mid j^*_i(\widetilde{\boldsymbol{X}})=j\Big] \\ &= \eta_i\Big(\overline{\Pi}^*_{i,j}\Big(\widetilde{\boldsymbol{W}_i^1}\Big)\Big)^\sigma E\Big[\xi_{i,j}\mid j^*_i(\widetilde{\boldsymbol{X}})=j\Big]. \end{aligned}$$

Moreover, from the assumed Frechet distribution (assuming $\sigma < 1$) we get:

$$E\Big[\xi_{i,j} \mid j_i^*(\widetilde{\boldsymbol{X}}) = j\Big] = \Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{X}}\Big)\Big)^{-\sigma}\left[\frac{\gamma\Big(1-\sigma, -\log \Pi_{i,0}^*\Big(\widetilde{\boldsymbol{X}}\Big)\Big)}{1-\Pi_{i,0}^*\Big(\widetilde{\boldsymbol{X}}\Big)}\right],$$

where $\gamma(\cdot,\cdot)$ is the lower incomplete gamma function. Plugging this into (B.31), note that since $\Pi_{i,0}^*$ is unchanged in our counterfactual, both the fraction in square brackets and the constant $\eta_i$ cancel, implying:

$$k_i = \left(\frac{\sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^0}\Big)\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)\Big)^{\sigma}\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^0}\Big)\Big)^{-\sigma}}{\sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)\Big)^{\sigma}\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)\Big)^{-\sigma}}\right)^{1/\delta}.$$

With our assumptions above, this formula can be implemented given our data and reduced form results: Given an estimate of $\beta_1$, the actual and counterfactual application probabilities, $\overline{\Pi}_{i,j}^*(\widetilde{\boldsymbol{W}_i^0})$ and $\overline{\Pi}_{i,j}^*(\widetilde{\boldsymbol{W}_i^1})$, can be computed under assumption (B.22). Additionally, under the normalization in (B.27), the values for $\delta$ and $\sigma$ can also be inferred from the reduced form estimates because $\beta_1 = \frac{\delta}{\sigma}$.

Finally, we consider computation of the effect of wage misperceptions on average applied-for wages and non-pecuniary values. The quantity in (B.29) can be computed directly via:

$$\frac{\sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^0}\Big)W_{i,j} - \sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)W_{i,j}}{\sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)W_{i,j}}.$$

For the quantity in (B.30), we have the analogous expression:

$$\frac{\sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^0}\Big)E\Big[H_{i,j} \mid j_i^*(\widetilde{\boldsymbol{W}_i^0}) = j\Big] - \sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)E\Big[H_{i,j} \mid j_i^*(\widetilde{\boldsymbol{W}_i^1}) = j\Big]}{\sum_{j=1,2,3} \overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)E\Big[H_{i,j} \mid j_i^*(\widetilde{\boldsymbol{W}_i^1}) = j\Big]}.$$

To compute this, we combine (B.28) with the definition $H_{i,j} = P_{i,j}Z_{i,j}\xi_{i,j}$ and the conditional expectation of $\xi_{i,j}$ above. These imply:

$$E\Big[H_{i,j} \mid j_i^*(\widetilde{\boldsymbol{X}}) = j\Big] = \frac{\eta_i}{\Psi_i} W_{i,j}^{-\delta}\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)\Big)^{\sigma}\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{X}}\Big)\Big)^{-\sigma} \times \left[\frac{\gamma\Big(1-\sigma, -\log \Pi_{i,0}^*\Big(\widetilde{\boldsymbol{X}}\Big)\Big)}{1-\Pi_{i,0}^*\Big(\widetilde{\boldsymbol{X}}\Big)}\right].$$

Plugging this in, the constant $\eta_i/\Psi_i$ and the outside-option term in square brackets cancel in (B.30), and we arrive at:

$$\frac{\sum_{j=1,2,3} W_{i,j}^{-\delta}\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)\Big)^{\sigma}\Big(\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^0}\Big)\Big)^{1-\sigma} - \sum_{j=1,2,3} W_{i,j}^{-\delta}\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)}{\sum_{j=1,2,3} W_{i,j}^{-\delta}\overline{\Pi}_{i,j}^*\Big(\widetilde{\boldsymbol{W}_i^1}\Big)}.$$

# C Additional details of the survey

This section i) outlines the data-driven procedure used to create the major-specific job types in the survey and also includes a full list of job types translated to English (Section C.1), ii) presents the full survey instructions translated into English (Section C.2), and iii) shows screenshots of the interactive figures shown in the survey (Section C.3). The original Danish survey instructions can be found at `https://tinyurl.com/zjw6dc83`.

## C.1 Details on creation of job types

**Creation of job types** This section of the appendix provides additional details on the data-driven procedure used to construct job types described in Section 2.3. For each major (measured using 6-digit Danish ISCED codes), we construct these job types following three objectives: First, they must be relevant and capture a substantial share of respondents' realistic employment opportunities. Second, they should differ in wages to ensure meaningful trade-offs. Third, they must correspond directly to categories in administrative records, enabling us to build objective benchmarks for typical wages using administrative data.

In practice, for each major, we define the three job types by grouping jobs based on some combination of occupation, industry, firm sector, and firm size. We implement a data-driven procedure to select the appropriate job groupings using administrative data on the first jobs of graduates from the same major between 2010-2018, starting in each graduate's month of graduation and only including graduates who find a job within 12 months. In order to avoid issues with multiple jobs in the same month, we prioritize jobs that have at least 3 months of positive earnings in the job spell and the highest mean earnings of the job spell. Job spells are defined as person-firm connections with gaps of at most 3 months between consecutive months of employment.

For each major, we consider all possible ways of grouping jobs by firm sector and size (public/private, above or below 50 employees), occupation codes (using either 3, 4, or 6 digit Danish ISCO codes), and/or industry codes (using either 1- or 2-digit Danish NACE codes). Since the concept of firm size is not well-defined for public sector jobs, we only allow groupings on firm size when also grouping into private sector jobs. Among all such possible groupings, we then select those that satisfy two conditions: i) the 3 most common job types must cover more than 40% of the transitions into first jobs for prior graduates and ii) the 3rd most common job type must cover at least 5% of transitions. These conditions ensure that the three most common job types altogether cover a substantial share of relevant jobs and that none of them are too narrow.

If multiple groupings satisfy the two conditions, we choose the grouping that best predicts entry-level wages. For each grouping, we calculate the out-of-sample mean squared error of the prediction from a regression of log wages on dummies for the job types using 5-fold cross-validation. The grouping that has the lowest average mean squared error is then chosen. Wages are here based on the average monthly earnings for the first 12 months after the hiring month for any employment. The hiring month is dropped due to instability, e.g. if a person is hired in the middle of the month.

After hand-checking the resulting job types for each major and making marginal adjustments, this procedure gives us three job types based on basic categories that we can intuitively describe to each survey respondent. The final paragraph in this section describes the few manual changes we made to labels of occupations and industries to ensure clarity. Additionally, for two majors, Philosophy PhD (DISCED: 802520) and Information science bachelor (DISCED: 602535) graduates, we found an error in a particular 2010 occupation code that was part of one of the three most common job types in our selected grouping. For Information science bachelor we addressed this by choosing a different grouping satisfying condition i) and ii) since more were available (specifically the grouping with the second best out-of-sample mean squared error for predicting wages). For Philosophy PhD no other groupings satisfied the conditions however. Accordingly, we reran our procedure using data on only the 2011-2018 cohorts to produce the grouping for this major.

**Selection of major programs into the experiment** For very specialized major programs, where all graduates take similar jobs, the procedure above does not deliver any job types because no grouping satisfies conditions i) and ii). This reflects the fact that graduates in these programs de facto make their career-defining decisions before entering the labor market and thus are not affected by the potential wage misperceptions we study in this paper. Accordingly, we exclude these majors from our analysis. To avoid asking only about broad sectors, we also exclude majors where only coarse grouping into public vs. small/large private sector jobs satisfies conditions i) and ii). Finally, we exclude major programs where we observe very few graduates in past administrative data, since we cannot calculate precise measures of typical wages for those. We exclude majors where fewer than 50 people transitioned to a job type in 2010-2018. These restrictions lead us to focus on 88 higher-education programs, covering 76% of higher-education graduates in 2010-2018 (see Appendix Table A1). We refer to these as the "selected" majors. Individuals from non-selected majors could participate in the survey but were ineligible for the randomized information treatment and are excluded from our analysis.

Table C1: Examples of job types asked about in the survey for different majors

| **Degree** | **Job types** | **Categories** |
|---|---|---|
| Ms. Economics<br>DISCED: 703950 | - Job in the private sector, in the industry: banking, financial, or insurance activities<br>- Job in the public sector, in the industry: public administration, defense and social security<br>- Job in the private sector, in the industry: specialized services, incl. business administration | - Public/private<br>- Industry, 1-digit |
| Ms. Physics<br>DISCED: 703530 | - Job in the public sector, with the occupation: teaching and research at higher education<br>- Job in the public sector, with the occupation: teaching at upper secondary level<br>- Job in the private sector (>50 employees), with the occupation: software and application development | - Public/private/size<br>- Occupation, 3-digit |

**Notes:** Examples of the job types, that were showed to the respondents, for two different majors. They are translated from Danish.

**Manual changes made to job types** After creating the job types using the approach explained in section 2.3, all job types were examined manually to ensure consistency and interpretability. Relative to the default labels for industry and occupation, given by Statistics Denmark, we changed 18 occupation labels and 19 industry labels, most of which were smaller word changes for clarity or other typesetting changes. The remaining changes consisted of correcting instances where the default label described an industry or occupation as the residual of the other categories. For example ISCO/DISCO code 2149 is named "Other engineering work (except Electrical engineering)", referring to the other categories under the 21 code. But as our respondents would not be able to see all other categories, we created a new label, spelling out the content of this residual.

### C.1.1 Full list of job types for major that were eligible for treatment

Table C2: Educations eligible for treatment and associated job types

| Education level | Education narrow field | Jobtype 1 | Jobtype 2 | Jobtype 3 |
|---|---|---|---|---|
| Short-cycle higher education | Multimedia design | Job in the industry: wholesale and retail trade | Job in the industry: information and communication | Job in the industry: administrative services |
| Short-cycle higher education | Financial advisor etc. | Job in the private sector (>50 employees), in the industry: financial services excluding insurance | Job in the private sector (≤ 50 employees), in the industry: real estate | Job in the private sector (>50 employees), in the industry: retail trade excluding motor vehicles |
| Short-cycle higher education | Trade and marketing economist, etc. | Job in the private sector (>50 employees), in the industry: wholesale and retail trade | Job in the private sector (≤ 50 employees), in the industry: wholesale and retail trade | Job in the private sector (≤ 50 employees), in the industry: accommodation and food service |
| Short-cycle higher education | Service economist etc. | Job in the private sector, in the industry: food service activities | Job in the private sector, in the industry: accommodation | Job in the private sector, in the industry: retail trade excluding motor vehicles |
| Short-cycle higher education | Construction and construction engineering | Job in the industry: architecture and engineering, technical testing and analysis | Job in the industry: specialized construction | Job in the industry: residential construction and building projects |
| Short-cycle higher education | Electronics and IT | Job in the private sector (≤ 50 employees), in the industry: information and communication | Job in the private sector (>50 employees), in the industry: information and communication | Job in the private sector (>50 employees), in the industry: wholesale and retail trade |
| Short-cycle higher education | Installer of high voltage and plumbing technology | Job in the private sector, in the industry: construction | Job in the private sector, in the industry: specialized services, incl. business administration | Job in the private sector, in the industry: manufacturing |
| Short-cycle higher education | Technical, production and product development | Job in the private sector, in the industry: retail trade excluding motor vehicles | Job in the private sector, in the industry: wholesale trade excluding motor vehicles | Job in the private sector, in the industry: manufacture of machinery and equipment |
| Short-cycle higher education | Laboratory, food and process technology | Job in the industry: manufacturing | Job in the industry: wholesale and retail trade | Job in the industry: administrative services |
| Short-cycle higher education | Laboratory Technician | Job in the private sector, in the industry: manufacturing, with the occupation: laboratory technician work | Job in the public sector, in the industry: education, with the occupation: laboratory technician work | Job in the private sector, in the industry: specialized services, with the occupation: laboratory technician work |
| Short-cycle higher education | Agricultural technology | Job in the private sector, in the industry: agriculture, hunting, forestry and fishing | Job in the private sector, in the industry: administrative services | Job in the public sector, in the industry: education |
| Medium-cycle higher education | Pedagogue | Job in the public sector, with the occupation: childcare/child educator | Job in the public sector, with the occupation: childcare/child educator, assistant | Job in the public sector, with the occupation: special needs education |
| Medium-cycle higher education | Elementary school teacher | Job in the public sector, in the industry: education, with the occupation: teaching at elementary school level | Job in the public sector, in the industry: residential care, with the occupation: teaching at elementary school level | Job in the private sector, in the industry: education, with the occupation: teaching at elementary school level |

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Table C2: Educations eligible for treatment and associated job types

| Education level | Education narrow field | Jobtype 1 | Jobtype 2 | Jobtype 3 |
|---|---|---|---|---|
| Medium-cycle higher education | Graphical design and communication | Job in the industry: advertising and market research | Job in the industry: services in design, photography, translation, agricultural consultancy etc. | Job in the industry: wholesale trade excluding motor vehicles |
| Medium-cycle higher education | Journalist and journalistic work | Job in the private sector (>50 employees), in the industry: information and communication, with the occupation: writing, journalism and linguistics | Job in the public sector, in the industry: information and communication, with the occupation: writing, journalism and linguistics | Job in the private sector (≤ 50 employees), in the industry: information and communication, with the occupation: writing, journalism and linguistics |
| Medium-cycle higher education | Media and communication, other | Job in the industry: information and communication | Job in the industry: specialized services, incl. business administration | Job in the industry: wholesale and retail trade |
| Medium-cycle higher education | Actor | Job in the private sector (>50 employees), in the industry: creative activities, arts and entertainment, with the occupation: acting | Job in the private sector (≤ 50 employees), in the industry: creative activities, arts and entertainment, with the occupation: acting | Job in the private sector (≤ 50 employees), in the industry: creative activities, arts and entertainment, with the occupation: film, theater direction and production |
| Medium-cycle higher education | Design | Job in the private sector (>50 employees), in the industry: wholesale and retail trade | Job in the private sector (≤ 50 employees), in the industry: wholesale and retail trade | Job in the private sector (≤ 50 employees), in the industry: specialized services, incl. business administration |
| Medium-cycle higher education | Administration etc. | Job with the occupation: general office work not requiring advanced degree | Job with the occupation: retail sales work | Job with the occupation: customer information |
| Medium-cycle higher education | Finance etc. | Job in the private sector (>50 employees), in the industry: financial services excluding insurance | Job in the private sector (≤ 50 employees), in the industry: real estate | Job in the private sector (>50 employees), in the industry: insurance, reinsurance and pension funding |
| Medium-cycle higher education | Trade and marketing, etc. | Job in the industry: wholesale and retail trade | Job in the industry: administrative services | Job in the industry: accommodation and food service |
| Medium-cycle higher education | Social counseling and mediation, etc. | Job in the public sector, in the industry: education, with the occupation: social counseling | Job in the public sector, in the industry: health and social services, with the occupation: social counseling | Job in the public sector, in the industry: education, with the occupation: general office work not requiring advanced degree |
| Medium-cycle higher education | Construction and construction engineering | Job in the industry: architecture and engineering, technical testing and analysis | Job in the industry: residential construction and building projects | Job in the industry: specialized construction |
| Medium-cycle higher education | Electronics and IT, technical | Job with the occupation: software and application development | Job with the occupation: engineering work (except electrical engineering) | Job with the occupation: electrical engineering work |
| Medium-cycle higher education | Mechanical engineering | Job in the private sector (>50 employees), in the industry: manufacturing | Job in the private sector (>50 employees), in the industry: specialized services, incl. business administration | Job in the private sector (>50 employees), in the industry: transport and freight handling |
| Medium-cycle higher education | Technical, production and product development | Job in the industry: manufacturing | Job in the industry: wholesale and retail trade | Job in the industry: specialized services, incl. business administration |

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Table C2: Educations eligible for treatment and associated job types

| Education level | Education narrow field | Jobtype 1 | Jobtype 2 | Jobtype 3 |
|---|---|---|---|---|
| Medium-cycle higher education | Other technical educations | Job in the industry: manufacturing | Job in the industry: wholesale and retail trade | Job in the industry: specialized services, incl. business administration |
| Medium-cycle higher education | Nutrition and Health | Job in the industry: health and social services | Job in the industry: education | Job in the industry: wholesale and retail trade |
| Medium-cycle higher education | Occupational and physiotherapist | Job in the public sector, with the occupation: occupational therapy | Job in the public sector, with the occupation: physiotherapy | Job in the private sector, with the occupation: physiotherapy |
| Medium-cycle higher education | Nursing and health care | Job in the public sector, in the industry: health care, with the occupation: nursing, basic functions | Job in the public sector, in the industry: health care, with the occupation: social and health work at institutions and hospitals, helpers | Job in the public sector, in the industry: health care, with the occupation: social and health work at institutions and hospitals, assistants |
| Medium-cycle higher education | Officer in the defense | Job in the industry: defense, with the occupation: military work at officer level | Job in the industry: defense, with the occupation: military work, neither officer nor non-commissioned officer level | Job in the industry: defense, with the occupation: military work at non-commissioned officer level |
| Bachelor degree | Humanities without further specification | Job in the industry: health and social services | Job in the industry: wholesale and retail trade | Job in the industry: education |
| Bachelor degree | History | Job in the industry: education | Job in the industry: culture, entertainment and sports | Job in the industry: health and social services |
| Bachelor degree | Film, theater and musicology, etc. | Job in the industry: information and communication | Job in the industry: education | Job in the industry: wholesale and retail trade |
| Bachelor degree | Communication and dissemination | Job in the private sector, in the industry: wholesale and retail trade | Job in the public sector, in the industry: education | Job in the private sector, in the industry: information and communication |
| Bachelor degree | Business language bachelors | Job in the industry: wholesale and retail trade | Job in the industry: education | Job in the industry: information and communication |
| Bachelor degree | Business language, combined | Job in the industry: wholesale and retail trade | Job in the industry: specialized services, incl. business administration | Job in the industry: information and communication |
| Bachelor degree | Mathematics | Job in the industry: education | Job in the industry: banking, financial, or insurance activities | Job in the industry: information and communication |
| Bachelor degree | Natural science IT educations | Job in the private sector, with the occupation: software and application development | Job in the public sector, with the occupation: teaching and research at higher education | Job in the private sector, with the occupation: general office work not requiring advanced degree |
| Bachelor degree | Social sciences without further specification | Job in the industry: health and social services | Job in the industry: education | Job in the industry: wholesale and retail trade |
| Bachelor degree | Law | Job in the industry: specialized services, incl. business administration, with the occupation: legal work | Job in the industry: public administration, defense and social security, with the occupation: general office work not requiring advanced degree | Job in the industry: public administration, defense and social security, with the occupation: legal work |

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Table C2: Educations eligible for treatment and associated job types

| Education level | Education narrow field | Jobtype 1 | Jobtype 2 | Jobtype 3 |
|---|---|---|---|---|
| Bachelor degree | Political science | Job in the public sector, in the industry: education | Job in the public sector, in the industry: public administration, defense and social security | Job in the private sector, in the industry: organizations and associations |
| Bachelor degree | Economics | Job with the occupation: general office work not requiring advanced degree | Job with the occupation: teaching and research at long-cycle higher education | Job with the occupation: work in economics |
| Bachelor degree | Business Economics, HA | Job in the industry: specialized services, incl. business administration | Job in the industry: wholesale and retail trade | Job in the industry: banking, financial, or insurance activities |
| Bachelor degree | Technical science without further specification | Job in the industry: education | Job in the industry: specialized services, incl. business administration | Job in the industry: wholesale and retail trade |
| Bachelor degree | Electronics and IT | Job in the private sector, in the industry: information and communication | Job in the public sector, in the industry: education | Job in the private sector, in the industry: wholesale and retail trade |
| Bachelor degree | Mechanical engineering | Job in the public sector, in the industry: education | Job in the private sector (>50 employees), in the industry: manufacturing | Job in the private sector (>50 employees), in the industry: specialized services, incl. business administration |
| Bachelor degree | Pharmacy and Pharmaceutical Sciences | Job in the private sector ($\leq$ 50 employees), in the industry: wholesale and retail trade | Job in the private sector (>50 employees), in the industry: manufacturing | Job in the private sector (>50 employees), in the industry: wholesale and retail trade |
| Long-cycle higher education | Pedagogy | Job in the public sector, with the occupation: teaching at elementary school level or pedagogical work | Job in the public sector, with the occupation: special needs education and research | Job in the public sector, with the occupation: teaching and research at higher education |
| Long-cycle higher education | Film, theater and musicology, etc. | Job in the public sector, in the industry: education | Job in the private sector, in the industry: information and communication | Job in the public sector, in the industry: health and social services |
| Long-cycle higher education | Communication and dissemination | Job in the industry: education | Job in the industry: health and social services | Job in the industry: information and communication |
| Long-cycle higher education | Journalism and rhetoric | Job in the private sector (>50 employees), with the occupation: writing, journalism and linguistics | Job in the public sector, with the occupation: writing, journalism and linguistics | Job in the private sector ($\leq$ 50 employees), with the occupation: writing, journalism and linguistics |
| Long-cycle higher education | Digital design and interaction design | Job in the industry: information and communication | Job in the industry: education | Job in the industry: specialized services, incl. business administration |
| Long-cycle higher education | Experience Design | Job in the industry: health and social services | Job in the industry: education | Job in the industry: specialized services, incl. business administration |
| Long-cycle higher education | Danish-Nordic language and literature | Job with the occupation: teaching at upper secondary level | Job with the occupation: general office work not requiring advanced degree | Job with the occupation: teaching at elementary school level |
| Long-cycle higher education | West Germanic languages (English, German or Dutch) | Job with the occupation: teaching at upper secondary level | Job with the occupation: general office work not requiring advanced degree | Job with the occupation: teaching at elementary school level |

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Table C2: Educations eligible for treatment and associated job types

| Education level | Education narrow field | Jobtype 1 | Jobtype 2 | Jobtype 3 |
|---|---|---|---|---|
| Long-cycle higher education | Business Language, MA in International Business Communication | Job in the private sector, in the industry: wholesale and retail trade | Job in the private sector, in the industry: specialized services, incl. business administration | Job in the private sector, in the industry: information and communication |
| Long-cycle higher education | Humanities, other | Job in the public sector, in the industry: education | Job in the public sector, in the industry: health and social services | Job in the private sector, in the industry: education |
| Long-cycle higher education | Architecture | Job in the private sector ($\leq$ 50 employees), in the industry: specialized services, with the occupation: architecture, infrastructure and design work | Job in the private sector (>50 employees), in the industry: specialized services, with the occupation: architecture, infrastructure and design work | Job in the public sector, in the industry: health and social services, with the occupation: architecture, infrastructure and design work requiring advanced degree |
| Long-cycle higher education | Designer | Job in the industry: education | Job in the industry: wholesale trade excluding motor vehicles | Job in the industry: services in design, photography, translation, agricultural consultancy etc. |
| Long-cycle higher education | Biology | Job in the public sector, in the industry: education | Job in the public sector, in the industry: health and social services | Job in the private sector, in the industry: specialized services, incl. business administration |
| Long-cycle higher education | Physics and physical subjects | Job in the public sector, with the occupation: teaching and research at higher education | Job in the public sector, with the occupation: teaching at upper secondary level | Job in the private sector (>50 employees), with the occupation: software and application development |
| Long-cycle higher education | Sports | Job with the occupation: teaching at upper secondary level | Job with the occupation: teaching and research at higher education | Job with the occupation: teaching at elementary school level or pedagogical work for children |
| Long-cycle higher education | Mathematics | Job in the industry: education | Job in the industry: banking, financial, or insurance activities | Job in the industry: information and communication |
| Long-cycle higher education | Natural science IT educations | Job in the private sector, with the occupation: software and application development | Job in the public sector, with the occupation: teaching and research at higher education | Job in the private sector, with the occupation: general office work not requiring advanced degree |
| Long-cycle higher education | Administration, governance and management | Job in the private sector, with the occupation: finance, accounting and math work not requiring advanced degree | Job in the private sector, with the occupation: work in finance and economics | Job in the private sector, with the occupation: general office work not requiring advanced degree |
| Long-cycle higher education | Anthropology | Job in the industry: health and social services | Job in the industry: education | Job in the industry: specialized services, incl. business administration |
| Long-cycle higher education | Globalization and international social studies | Job in the industry: social work activities without accommodation | Job in the industry: education | Job in the industry: public administration, defense and social security |
| Long-cycle higher education | Law | Job in the public sector, with the occupation: other legal work besides judge and lawyer work | Job in the private sector (>50 employees), with the occupation: associate lawyer work | Job in the private sector (>50 employees), with the occupation: other legal work besides judge and lawyer work |

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Table C2: Educations eligible for treatment and associated job types

| Education level | Education narrow field | Jobtype 1 | Jobtype 2 | Jobtype 3 |
|---|---|---|---|---|
| Long-cycle higher education | Political science | Job with the occupation: work in social sciences and religion | Job with the occupation: general office work not requiring advanced degree | Job with the occupation: legal work |
| Long-cycle higher education | Psychology | Job with the occupation: work in social sciences and religion | Job with the occupation: general office work not requiring advanced degree | Job with the occupation: care work in health industry |
| Long-cycle higher education | Sociology | Job in the public sector, in the industry: health and social services | Job in the public sector, in the industry: education | Job in the public sector, in the industry: public administration, defense and social security |
| Long-cycle higher education | Economics | Job in the private sector, in the industry: banking, financial, or insurance activities | Job in the public sector, in the industry: public administration, defense and social security | Job in the private sector, in the industry: specialized services, incl. business administration |
| Long-cycle higher education | Business Economics, (Cand.merc.) | Job in the industry: specialized services, incl. business administration | Job in the industry: wholesale and retail trade | Job in the industry: banking, financial, or insurance activities |
| Long-cycle higher education | Technical science without further specification | Job with the occupation: engineering work (except electrical engineering) | Job with the occupation: software and application development | Job with the occupation: teaching and research at higher education |
| Long-cycle higher education | Biotechnology and chemical technology | Job in the public sector, with the occupation: teaching and research at higher education | Job in the private sector, with the occupation: engineering work (except electrical) | Job in the public sector, with the occupation: engineering work (except electrical) |
| Long-cycle higher education | Building and construction engineering | Job in the private sector, with the occupation: engineering work (except electrical) | Job in the public sector, with the occupation: engineering work (except electrical) | Job in the public sector, with the occupation: teaching and research at higher education |
| Long-cycle higher education | Electronics and IT | Job in the private sector, in the industry: information and communication | Job in the public sector, in the industry: education | Job in the private sector, in the industry: manufacturing |
| Long-cycle higher education | Health and welfare technology, etc. | Job in the public sector, in the industry: education | Job in the public sector, in the industry: health care | Job in the private sector (>50 employees), in the industry: manufacture of pharmaceuticals |
| Long-cycle higher education | Technology-natural sciences, combined | Job in the public sector, with the occupation: teaching and research at long-cycle higher education | Job in the private sector, with the occupation: engineering work (except electrical) | Job in the private sector, with the occupation: software and application development |
| Long-cycle higher education | Technology, production and product development | Job in the private sector (>50 employees), in the industry: manufacturing | Job in the private sector ($\leq$ 50 employees), in the industry: specialized services, incl. business administration | Job in the private sector (>50 employees), in the industry: specialized services, incl. business administration |
| Long-cycle higher education | Technical science, other | Job in the industry: manufacturing | Job in the industry: specialized services, incl. business administration | Job in the industry: information and communication |
| Long-cycle higher education | Veterinary medicine | Job with the occupation: veterinary work | Job with the occupation: teaching and research at long-cycle higher education | Job with the occupation: general office work not requiring advanced degree |
| Long-cycle higher education | Natural resources and environment | Job in the industry: education | Job in the industry: health and social services | Job in the industry: specialized services, incl. business administration |

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Table C2: Educations eligible for treatment and associated job types

| Education level | Education narrow field | Jobtype 1 | Jobtype 2 | Jobtype 3 |
|---|---|---|---|---|
| Long-cycle higher education | Pharmacy and Pharmaceutical Sciences | Job in the private sector ($\leq$ 50 employees), in the industry: wholesale and retail trade | Job in the private sector (>50 employees), in the industry: manufacturing | Job in the public sector, in the industry: education |
| Long-cycle higher education | Public health science | Job in the public sector, in the industry: education | Job in the public sector, in the industry: health care | Job in the private sector, in the industry: social work without accommodation |
| Long-cycle higher education | Odontology | Job in the private sector ($\leq$ 50 employees), in the industry: health care, with the occupation: dental work | Job in the public sector, in the industry: education, with the occupation: dental work | Job in the public sector, in the industry: residential care, with the occupation: dental work |
| Long-cycle higher education | Healthcare sciences, other | Job in the public sector, with the occupation: nursing and midwifery | Job in the public sector, with the occupation: dentist, pharmacist, physiotherapy and health work in occupational health, nutrition, audiology, optics and chiropractic | Job in the public sector, with the occupation: teaching and research at higher education |
| PhD and research education | Philosophy | Job in the public sector, with the occupation: teaching and research at higher education | Job in the private sector ($\leq$ 50 employees), with the occupation: teaching and research at higher education | Job in the public sector, with the occupation: work in social sciences and religion |
| PhD and research education | Natural sciences without further specification | Job in the industry: education | Job in the industry: specialized services, incl. business administration | Job in the industry: manufacturing |
| PhD and research education | Political science | Job in the public sector, in the industry: education | Job in the private sector, in the industry: specialized services, incl. business administration | Job in the public sector, in the industry: public administration, defense and social security |
| PhD and research education | Technical science without further specification | Job in the industry: education | Job in the industry: specialized services, incl. business administration | Job in the industry: manufacturing |
| PhD and research education | Agriculture, nature and environment without further specification | Job in the public sector, in the industry: education | Job in the private sector, in the industry: manufacturing | Job in the private sector, in the industry: specialized services, incl. business administration |
| PhD and research education | Medicine | Job in the industry: health and social services, with the occupation: general medical practice | Job in the industry: education, with the occupation: teaching and research at higher education | Job in the industry: health and social services, with the occupation: work in biochemistry, biology, botany, zoology and related fields |

### C.1.2 Creation of job type examples for explanation and validation question

To create more concrete examples used for explaining and validating understanding of the three job types in the survey, we used more granular industry and occupation codes when available. For job types shown in the survey which were defined in terms of industry, we used 4-digit NACE industries as examples of industries that fall within the given survey job type. Similarly, for job types defined in terms of occupation at the DISCO 3 or 4-digit level, we used 6-digit DISCO occupations as examples. These examples were checked manually to make sure they were interpretable and were not described using the exact same text labels as the survey job type.[36] When available, the two most common example occupations and/or industries for a given major were shown in question D1 of the survey (with 'common' again being defined in terms of the hiring outcomes of past cohorts from the same major upon entering the labor market).

We also used an analogous approach to construct a validation question at the end of the survey. For job types defined in terms of industry at the 1 or 2-digit level, or occupation at the DISCO 3 or 4-digit level, we constructed example jobs as combinations of 4-digit NACE industries and 6-digit DISCO occupation labels, using the same corrections as above. All respondents from the corresponding major were then shown one of the two most common combinations at random as an example job, and were asked to place this example within the three job types shown in the survey.

[36]This involved changing 41 labels to make them interpretable and distinct from the upper category, removing 10 labels that were not distinguishable from the upper category, and adding 11 examples where none was usable in the existing labels, but could easily be constructed.

## C.2 Full survey questionnaire (in English)

**A** Thank you very much for participating in this survey about job search and career choices - we greatly appreciate it!

As a token of our gratitude for your time, we are giving away a total of **20 gift cards to GoGift** among those who complete the questionnaire. These gift cards can be used for many things and are very easy to use in both stores and online. Each gift card is worth **1,000 DKK**.

Half of the gift cards are distributed at random to respondents who complete the entire questionnaire (it is expected that this will take approximately **15 minutes**).

The rest of the gift cards are given to respondents who complete the questionnaire and do especially well in the exercises that make up part of the survey (e.g., by guessing the answer to some factual questions correctly). Keep an eye out for questions marked with an emoji. In these questions, you can win additional gift cards!

[*Emoji*]

If you win one (or more) gift cards, you will receive a direct notification in your e-Boks [e-Boks is a Danish state-official email] no later than September 31st, 2023. During the survey, you are able to disconnect and return to complete the survey later if you need to. Your answers are saved and when you click the link again, you will automatically return to where you left off.

**B1 We are now going to ask you some questions about your education and current employment circumstances.**

**B2** Are you currently undertaking education (e.g., at a university or elsewhere)?

- Yes
- No

**B3** [**if B2==Yes**] Which of the following educations are you currently undertaking?
[**if B2==No**] Which of the following educations correspond to the highest level of education that you have completed?

[**3-layer dropdown with first layer corresponding to the following**]

- Primary or lower secondary education
- Upper secondary education (High school)
- Vocational basic course
- Vocational education
- Labor market education
- Short-cycle higher education
- Medium-cycle higher education
- Bachelor's degree
- Long-cycle higher education (e.g., Master's degree)
- PhD programme and research education

[**The education categories are based on DISCED, the Danish implementation of ISCED. The second layer corresponds to the broad field, and the third layer corresponds to the narrow field. See `https://www.dst.dk/da/Statistik/dokumentation/nomenklaturer/disced15-audd`**]

**B4** [**if B2==No**] When did you finish studying this education?
- MM/YYYY

[**If B2==Yes**] When do you expect to finish studying this education?
- MM/YYYY

**[Create custom variables that take on values depending on responses to question B3:** *custom_educ*: **Detailed education (all three layers)** *jobtype*1, *jobtype*2, *jobtype*3: **three specific types of jobs that are relevant to the respondent's education. The three job types are specific to the respondent's education and based on combinations of sector, industry, occupation and firm size. See Appendix Section C.1 for details on how the job types are generated.]**

**C1** Which of the following statements best describes your current labor market situation position?

- **I have a job** and my contract is **not expiring in the immediate future.**
- **I have a job**, but my contract **expires soon** and I have **not** accepted another job offer.
- **I have a job**. My contract **expires soon**, but I have **accepted** another job offer.
- **I do not have a job**, but I have **accepted** a job offer which is starting soon.
- **I do not have a job** and I have not accepted a job offer.

**C2** [**if C1==5**] Are you currently searching for a job?

[**if C1==1 or 2 or 3 or 4**] Are you currently searching for another job?

*Note: Searching for a job means e.g., asking employers about potential job opportunities, gathering information about vacant positions, or sending out job applications.*

- Yes
- No

**C3** [**if C2==Yes & C1==4 or 5**] When did you start searching for a job?

[**If C2==Yes and C1==1 or 2 or 3**] When did you start searching for another job?

[**If C2==No and C1==1**] When did you start searching for your current job?

[**If C2==No and C1==2**] When do you plan on starting to search for another job?

[**If C2==No and C1==3 or 4**] When did you start searching for the job you have accepted?

[**If C2==No and C1==5**] When do you plan on starting to search for a job?

*Note: Searching for a job means e.g., asking employers about potential job opportunities, gathering information about vacant positions, or sending out job applications.*

- MM/YYYY

**D1 [The Industri_example* and Funktion_example* variables are job type specific variables that give examples of the industry and occupations mentioned in the job types. These variables are empty if the job types does not include industry or occupation, or examples where deemed unnecessary.**]

We would now like for you to answer some questions about three categories of jobs people with your educational background ("$custom_educ$") could have:

- **$jobtype_1$**

  *[If Industri_example1a !="" and Industri_example1b ==""  show:]*

  An example of this industry could be:

  - *Industri_example1a*

  *[Else if Industri_example1a != "" and Industri_example1b !="" show:]*

  Examples of this industry could be:

  - *Industri_example1a* - *Industri_example1b*

  *[Do the same for Funktion instead of Industri:]*

  *[If Funktion_example1a !="" and Funktion_example1b ==""  show:]*

  An example of this type of occupation could be:

  - *Funktion_example1a*

  *[Else if Funktion_example1a != "" and Funktion_example1b !="" show:]*

  Examples of this type of occupation could be:

  - *Funktion_example1a* - *Funktion_example1b*

  *[If none of the conditions are met, don't show anything.]*

- **$jobtype_2$**

*[Same as for jobtype_1 just using Industri_example2a−b and Funktion_example2a−b instead of 1]*

- **$jobtype_3$**

  *[Same as for jobtype_1 just using Industri_example3a−b and Funktion_example3a−b instead of 1]*

**D2 Before you continue, use a moment to <u>read and think about the three categories of jobs listed above.</u>**

How well do you understand what we mean by a **$Jobtype_1$**?

• Very poorly • Poorly • Neither well nor poorly • Well • Very well

How well do you understand what we mean by a **$Jobtype_2$**?

• Very poorly • Poorly • Neither well nor poorly • Well • Very well

How well do you understand what we mean by a **$Jobtype_3$**?

• Very poorly • Poorly • Neither well nor poorly • Well • Very well

**D4** Do you know someone close to you who has had a job in this job category, such as a close friend or family member? If you are close to multiple people, think of the person who is closest to you.

**$Jobtype_1$:**

• Very close • Close • Not so close • I am not close to anybody who has had a job in this field.

**$Jobtype_2$:**

- Very close
- Close
- Not so close
- I am not close to anybody who has had a job in this field.

**$Jobtype_3$:**

- Very close
- Close
- Not so close
- I am not close to anybody who has had a job in this field.

**E1** In the following, you are asked to guess the real answers to some **factual** questions about the labor market in recent years. You will be asked a few questions about things that persons with **the same educational background as you ("$custom_educ$")** have experienced in the three job categories above.

Take your time and try to guess the correct answer!

[*Emoji*]

In the questions marked with this emoji, the 10 participants with the most correct answers will receive **a gift card of 1,000 DKK** in their e-Boks.

**E2 [Salary expectations]**

Some jobs pay better than others. This question concerns monthly **gross salary** (i.e. **before tax** and **including** contribution to pension savings) of people in full-time employment. Full-time employment refers to contracts of employment of at least 37 hours of work a week.

Consider persons with the **same** educational background as you (["**$custom_educ$**"]), who are newly graduated and completed their education in the 2010s whereafter they began working full-time.

**What do you think** were the **average monthly gross earnings during the first year of work for persons with jobs** in the following job types in the 2010s?

[*Emoji*]

As a reference, the average was approximately 31,000 DKK if **ALL** newly graduated workers of the period are considered (all positions and all educational backgrounds).

- Full-time salary of **$Jobtype_1$**: _______ DKK
- Full-time salary of **$Jobtype_2$**: _______ DKK
- Full-time salary of **$Jobtype_3$**: _______ DKK

*If you are one of 3 respondents with answers closest to the actual numbers calculated from data from the Danish labor market of the years 2010 to 2018, you will receive a gift card of 1,000 DKK directly to your e-Boks. Remember that your answer should be in Danish "Kroner" (DKK) per month (before tax and including pension savings).*

**E3 [Expectations of job application success]**

Some jobs are harder to get than others. This question concerns how often the application of unemployed people leads to an actual hiring in the firm.

Consider a person with the **same** educational background as you ([**"$custom_educ$"**]), who is newly graduated and completed their education in the 2010s whereafter they began receiving unemployment benefits and did not get a job for at least 6 weeks.

When the average person who is newly graduated sent out a job application for a position in the following job types, **what was the applicant's chance of employment at that firm**?

[*Emoji*]

As a reference, the average success rate was approximately 27 out of 1000 if considering **ALL** newly graduated (all positions and all educational backgrounds).

- Rate of success when applying for **$Jobtype_1$**: _____ out of 1000

- Rate of success when applying for **$Jobtype_2$**: _____ out of 1000

- Rate of success when applying for**$Jobtype_3$**: _____ out of 1000

*If you are one of 3 respondents with answers closest to the actual success rate calculated from data on application gathered from applications logged in joblog.dk and administrative data on hirings in the years 2016 to 2018, you will receive a gift card of 1,000 DKK directly to your e-Boks. Remember that your answer should be given as a share of 1000. If you answer 10, this means that you think 10 out of 1000 applications lead to employment for the applicant. If you answer 45, this means that you think 45 out of 1000 applications lead to employment instead.*

**E4 [Salary after 5 years]**

Some jobs offer better opportunities when it comes to wage growth than others. This question concerns the monthly **gross salary** (i.e. **before tax** and **including** pension savings) **5 years after having started a job.**

Consider again a person with the **same** educational background as you ([**"$custom_educ$"**]), who is newly graduated and completed their education in the 2010s.

[*Emoji*]

**What do you think was the monthly full-time gross salary after 5 years, for persons who started working in the following job types?**

As a reference, you are informed that the average total salary was approximately 41,000

DKK 5 years after employment if **ALL** newly graduated workers of the period are considered (all positions and all educational backgrounds).

- Full-time salary 5 years after first employment in **$Jobtype_1$**: ___________________DKK
- Full-time salary 5 years after first employment in **$Jobtype_2$**: ___________________DKK
- Full-time salary 5 years after first employment in **$Jobtype_3$**: ___________________DKK

*If you are one of 4 respondents with answers closest to the actual numbers calculated from data for the Danish labor market of the years 2010 to 2014, you will receive a gift card of 1,000 DKK directly to your e-Boks. Remember that your answer should be in Danish "Kroner" (DKK) per month (before tax and including pension savings).*

**F1.A [Treatment status T1, is determined in the following way.**
**It is 0 if not eligible, or eligible and randomly chosen for control.**
**It is 1 if eligible and chosen for treatment.**
**Eligibility is determined on an education basis:**
**Eligible educations have both a data driven specific job type classification, and the number of observed education-to-job transitions with salary information in the least common job type was at least 30.]**

**[wage treatment] [show if T1==1]**

Earlier, we asked you about the average monthly gross salary of full-time employees in different types of jobs.

We have researched the **actual** monthly gross salary of people with the same education as you ("**$custom_educ$**") who completed their education and worked full-time (at least 37 hours) in different job categories in the 2010s. The following graph depicts the **actual** **average monthly full-time gross salary in the first year of employment after graduating (the three bars) compared to your estimates (the three dots)**.

**Figure: Actual data concerning salaries vs. your estimates** (see Figure C1)

Note: When proceeding you are not able to return to this graph. You can continue from this page by clicking "next" after 15 seconds.

**F1.B** [**Reminder wages**] [**show if T1==0**]

Previously, we asked you about the **full-time salaries of newly graduated** in different types of jobs.

One of the questions was about the monthly gross salary of people with the same education as you ([**"$custom_educ$"**]) who completed their education and worked full-time (at least 37 hours) in different fields in the 2010's. The following graph depicts **your estimates** regarding the **average monthly full-time gross salary in the first year of employment after graduation.**

Figure: Your estimates regarding salaries (see Figure C2)

Note: You can continue from this page by clicking "next" after 15 seconds.

**G1** We now ask you to **consider three distinct jobs** - one from each category listed below (the same that you were asked about earlier). The jobs you consider should be relevant to a person with the same educational background as you. The positions should also be **representative** of what you think the category of jobs in question would offer **you personally** in **every aspect** (pay, probability of receiving an offer if you apply, working conditions, etc.).

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

When you have thought about each of the three jobs and what they offer, we would like for you to answer some questions about the three positions. Some of the questions can be hard to answer, but we are interested in your best guess.

Click “next” to start answering the questions.

**G2** Imagine that you are looking for a job and find postings corresponding to **the three positions we asked you to think about before** (i.e. three job postings in total). You only have the time to apply to exactly one of the positions. Alternatively, you do not apply to any of the three jobs.

It is hard to know exactly how you would act in this situation, but what do you think is the probability of each of the possibilities listed below?

*Please enter your answers as percentages. An answer of 10 percent means that you would apply for the position in 10 out of 100 instances while 50 percent means that you would apply for the job about half of the time. Please make sure that your answers add up to 100 percent.*

[**Here the respondents answer by moving indicators on four sliders from 1 to 97%, the default value is 25%. The sliders are set up, such that they always sum to 100. In practice this is done as follows: When respondents move one of the sliders, the other sliders adjust to make them sum to a 100. The adjustment is done with priority, so, if possible, only the 4th slider adjusts, then the 3rd, then 2nd then 1st.**]

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

- Probability that I will apply for **Job A**: [**slider**]
- Probability that I will apply for **Job B**: [**slider**]
- Probability that I will apply for **Job C**: [**slider**]
- Probability that I will not apply to any of the jobs: [**slider**]

[**If respondents do not move the indicator, they cannot go to the next screen; error message "You have not moved any of the sliders. You need to move at least one, to show that you have answered the question."**]

**G3** Imagine that you are offered **the three positions we asked you to think about before** (i.e. three job offers in total). You can accept exactly one offer. Alternatively, you do not accept any of the three job offers.

It is hard to know exactly how you would act in this situation, but what do you think is the probability of each of the possibilities listed below?

*Please enter your answers as percentages. An answer of 10 percent means that you would accept a job offer in 10 out of 100 instances while 50 percent means that you would accept a job offer about half of the time. Please make sure that your answers add up to 100 percent.*

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

[**Here the respondents answer by moving indicators on four sliders from 1 to 97%, the default value is 25%. The sliders are set up, such that they always sum to 100. In practice this is done as follows: When respondents move one of the sliders, the other sliders adjust to make them sum to a 100. The adjustment is done with priority, so, if possible, only the 4th slider adjusts, then the 3rd, then 2nd then 1st.**]
- Probability that I would accept **Job A**: [**slider**]
- Probability that I would accept **Job B**: [**slider**]
- Probability that I would accept **Job C**: [**slider**]
- Probability that I would not accept any of the three jobs offered: [**slider**]

[**If respondents do not move the indicator, they cannot go to the next screen;**

**error message "You have not moved any of the sliders. You need to move at least one, to show that you have answered the question."**]

**G4** Consider again the three jobs from earlier. How many hours a week would you end up working in each of these positions?

*Note: We are interested in **actual working hours**. A job that is stated to be 37 hours could require more or less than 37 hours a week in practice. The position does not need to be stated as full time.*

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

- Weekly hours in **Job A**: _______ hours
- Weekly hours in **Job B**: _______ hours
- Weekly hours in **Job C**: _______ hours

**G5** Consider once again the three jobs from earlier. What would be your starting salary in each of these positions?

*Please enter the monthly gross salary **before tax** and **including pension savings**.*

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

- Starting salary in **Job A**: _______ DKK
- Starting salary in **Job B**: _______ DKK
- Starting salary in **Job C**: _______ DKK

**G6** Consider again the three jobs from earlier. If you applied for these, how likely is it that you would receive a job offer?

*Please enter your answers as a share of 1000. If you answer 10, this means that you think you would receive an offer 10 out of 1000 times. If you answer 45, this means that you think you would receive an offer 45 out of 1000 times.*

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

- The chance that I would receive an offer if I applied for **Job A**: _______ out of 1000
- The chance that I would receive an offer if I applied for **Job B**: _______ out of 1000
- The chance that I would receive an offer if I applied for **Job C**: _______ out of 1000

**G7** Consider again the three positions we told you to imagine earlier. **In 5 years**, how many hours a week would you spend working if you started working in each of these positions tomorrow?

*Note: We are interested in **actual working hours**. A job that is stated to be 37 hours could require more or less than 37 hours a week in practice. The position does not need to be stated as full time.*

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

- Weekly hours of work in 5 years if I started working in **Job A** tomorrow: _______ hours
- Weekly hours of work in 5 years if I started working in **Job B** tomorrow: _______ hours
- Weekly hours of work in 5 years if I started working in **Job C** tomorrow: _______ hours

**G8** Consider again the three positions from earlier. What would your monthly gross salary be **in 5 years** if you started in each of these positions tomorrow?

*Please enter the monthly gross salary **before tax** and **including pension savings** after 5 years.*

| **Job A:** $Jobtype_1$ | **Job B:** $Jobtype_2$ | **Job C:** $Jobtype_3$ |
|---|---|---|

- Monthly gross salary in 5 years if I started working in **Job A** tomorrow: _______ DKK

- Monthly gross salary in 5 years if I started working in **Job B** tomorrow: _______ DKK

- Monthly gross salary in 5 years if I started working in **Job C** tomorrow: _______ DKK

**G9** Consider again the three positions from earlier. Imagine the experiences in the workplace that these three jobs would entail. How likely is it that you would get along really well with your coworkers in these positions?

| **Job A:** $Jobtype_1$ | **Job B:** $Jobtype_2$ | **Job C:** $Jobtype_3$ |
|---|---|---|

- The probability that I would get along really well with my colleagues at **Job A**:

- **Very unlikely**
- **Unlikely**
- **A little unlikely**
- **A little likely**
- **Likely**
- **Very likely**

- The probability that I would get along really well with my colleagues at **Job B**:
[**Same dropdown**]

- The probability that I would get along really well with my colleagues at **Job C**:
[**Same dropdown**]

**G10** Consider again the three jobs from earlier. Think about how challenging you would find the work in these positions. How likely is it that you would perform excellently in each of the three positions?

| **Job A:** $Jobtype_1$ | **Job B:** $Jobtype_2$ | **Job C:** $Jobtype_3$ |
|---|---|---|

- Probability that I would perform really well at **Job A**:

- **Very unlikely**
- **Unlikely**
- **A little unlikely**
- **A little likely**

- **Likely**
- **Very likely**

- Probability that I would perform really well at **Job B**:

[**Same dropdown**]

- Probability that I would perform really well at **Job C**:

[**Same dropdown**]

**G11** Consider again the hypothetical situation where you receive **job offers regarding the three positions** from earlier questions. Earlier, you gave the following probabilities when answering how likely you were to accept exactly one of the offers.

- Probability that I would accept **Job A**:

[**Previous answer from G3 is inserted here**]

- Probability that I would accept **Job B**:

[**Previous answer from G3 is inserted here**]

- Probability that I would accept **Job C**:

[**Previous answer from G3 is inserted here**]

- Probability that I would not accept any of the three jobs offered:

[**Previous answer from G3 is inserted here**]

| **Job A:** | **Job B:** | **Job C:** |
|---|---|---|
| $Jobtype_1$ | $Jobtype_2$ | $Jobtype_3$ |

Imagine that you again receive the same job offers, but that **the offers are atypical**, such that the starting salary is [**median answer from G5, rounded to 1000s, is inserted**] in all three of the jobs. Besides the starting salary, the jobs are exactly as before (including the weekly hours, relative wage growth and so on). How would this change your previous probabilities stated above?

*Please enter your answers as percentages. An answer of 10 percent means that you would accept a job offer in 10 out of 100 instances while 50 percent means that you would accept a job offer about half of the time. Please make sure that your answers add up to 100 percent.*

[**Here the respondents answer by moving indicators on four sliders from 1 to 97%, the default value is 25%. The sliders are set up, such that they always sum to 100. In practice this is done as follows: When respondents move one of the sliders, the other sliders adjust to make the sum correspond to 100. The adjustment is done with priority, so, if possible, only the 4th slider adjusts, then the 3rd, then 2nd then 1st.**]

- Probability that I would accept **Job A** when the starting salary of all three of the positions is [**median answer from G5, rounded to 1000s, is inserted**]:
[**slider**]

- Probability that I would accept **Job B** when the starting salary of all three of the positions is [**median answer from G5, rounded to 1000s, is inserted**]:
[**slider**]

- Probability that I would accept **Job C** when the starting salary of all three of the positions is [**median answer from G5, rounded to 1000s, is inserted**]:
[**slider**]

- The probability that I would turn down all three job offers is:
[**slider**]

[**If respondents do not move the indicator, they cannot go to the next screen; error message "You have not moved any of the sliders. You need to move at least one, to show that you have answered the question."**]

**G12 [$Jobtype_example_chosen$ is an example of one of the three education-specific job types. Only show this question when the job types examples are available.]**

Throughout this survey we have asked you about 3 different types of jobs. To better understand how you understand these, we pose the following question:

In which job category would you place the more concrete example:

**$Jobtype_example_chosen$**

- It is an example of **$Jobtype_1$**.
- It is an example of **$Jobtype_2$**.
- It is an example of **$jobtype_3$**.
- It does not fit into any of the three types of jobs.

**H1 Finally, we would like for you to answer some questions about yourself.**

How willing are you, in general to take a risk?

Please answer on a scale from 0 to 10[37] where 0 means that you are "completely unwilling to run any risks" and 10 means that you are "very willing to run a risk". You can also use any integer between 0 and 10 to indicate where you are situated on the scale.

[**The respondents can choose between the integers 1-10**]

Think of all the job offers you have received in the last 2 years.

How many job offers have you received in total?

[37] Note: In the survey there was a mistake, and the text stated 0 to 10, but is was only possible to choose between 1 to 10

___

How many of these did you reject?

___

How many of these did you reject because you received an offer for another job at the same time that you liked better?

___

How many job offers did you reject although you had no other jobs available?

___

How many children would you like to have throughout your entire life? (your best estimate is fine)

______ children

What do you think is the probability that you will have a child within 4 years?

______ percent (%)

**I1 [wage treatment] [T1==1]**

On a previous page of this survey, we showed you some information about the **actual** monthly gross salary of people with your educational background ([**"$custom_educ$"**]) who graduated and worked full-time (at least 37 hours) in different job categories between 2010 and 2018.

The same graph that you saw earlier is depicted below. It shows the **actual average monthly full-time gross salary in the first year of employment after graduating (the three columns) compared to your estimates (the three dots)**.

**Figure: Actual data concerning salaries vs. your estimates** (see Figure C1)

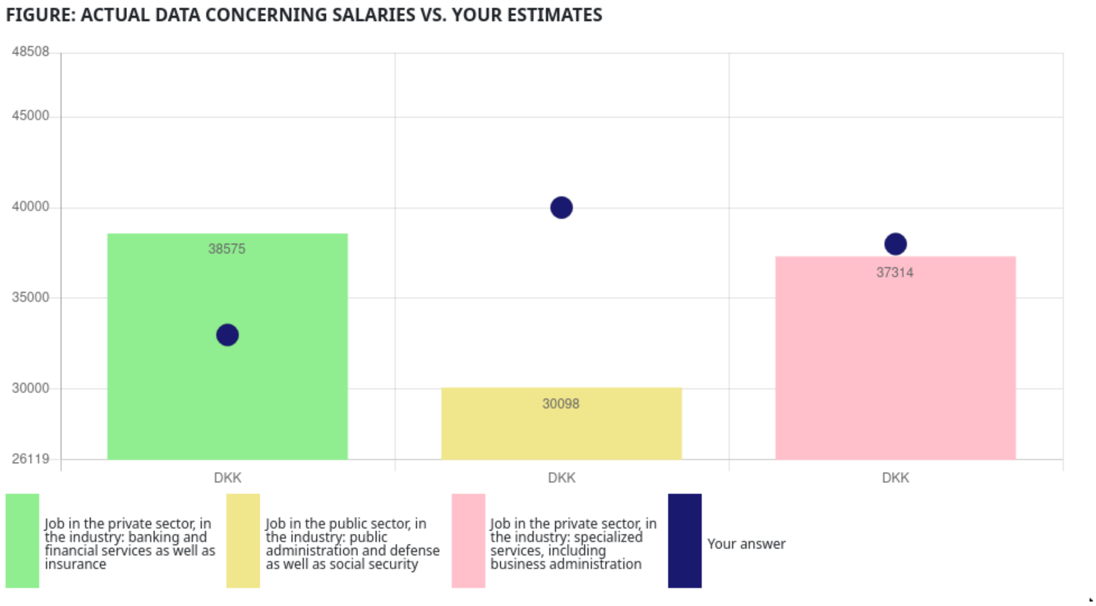
[**if T1==0 just go to next question**]

**I2** If you have any thoughts or input, you would like to share with us about job search or the survey you have just completed, please write them down below:
[**Text entry box**]

Thank you for taking part in this survey!

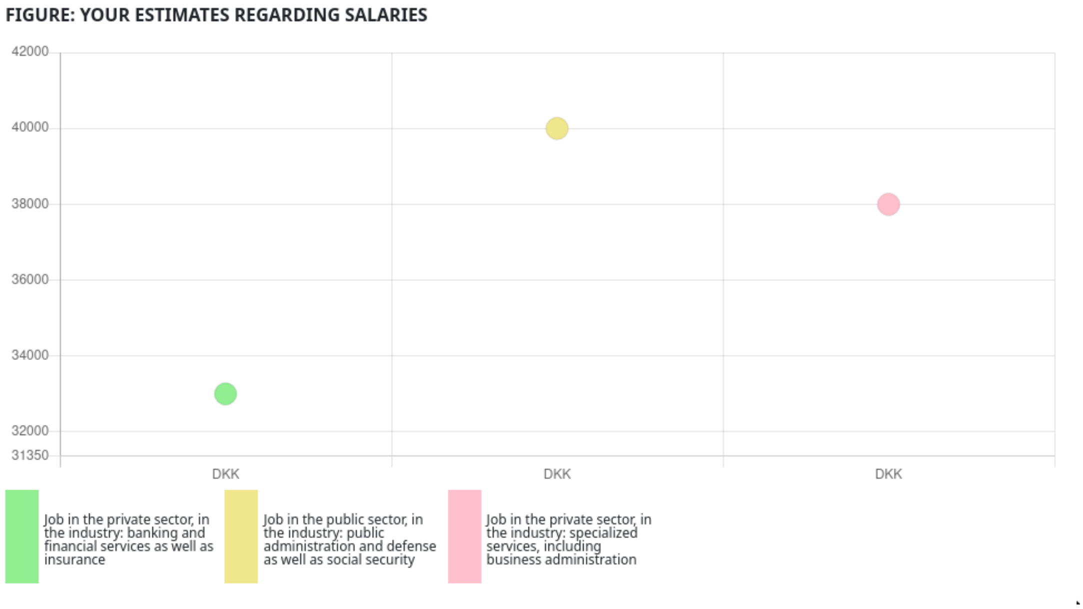
### C.3 Survey screenshots and examples

Figure C1: Example of treatment slide shown to the treatment group in the survey




**Note:** This figure shows an example of the interactive graph (translated into English) shown to the treatment group (translated into English), reminding them of their stated answers about the average wage in each job, and informing them of the actual averages in the register data.

Figure C2: Example of treatment slide shown to the control group in the survey




**Note:** This figure shows an example of the interactive graph (translated into English) shown to the control group, reminding them of their stated answers about the average wage in each job.

# D Post survey data handling

## D.1 Lost observations due to send-out error in the second wave

Due to an error introduced by our "e-Boks" e-mail provider in the second wave, the same link(s) were initially sent to multiple people, making their answers invalid and making it impossible for us to link them to register data. The mistake was found and the correct links were sent out within 4 hours. Since we can not be certain that links sent and filled out in the first 4 hours of the second wave were correct and unique, however, we have been forced to remove all answers registered in this 4 hour period (corresponding to 151 distinct online survey links, answered by multiple persons).

## D.2 Procedure to correct reporting mistakes

To assess how simple reporting errors may be contributing to our measured misperceptions, Table 3 shows how measured misperceptions change if we apply a simple procedure to correct obvious reporting errors in the raw survey data.

The specific procedure we consider works as follows. All reported monthly wages outside 7,000–120,000 DKK were flagged as being implausible. For these reports, we examined the number of digits in the raw responses. If all flagged values for the same respondent had the same number of digits, we interpret it as a systematic reporting issue. In that case, the value was rescaled according to the most plausible interpretation:

- six-digit values were treated as annual earnings and divided by 12
- two-digit values were treated as monthly earnings reported in thousands and multiplied by 1,000
- three-digit values were interpreted as hourly wages and multiplied by 160.33 (approximate full-time hours per month)

If instead the number of digits varied across flagged responses, we interpreted this as an idiosyncratic reporting error and corrected each report individually as follows:

- six-digit values were assumed to contain an extra digit and divided by 10

- four-digit values were assumed to be missing a final digit and multiplied by 10
- two-digit values were again treated as thousands and multiplied by 1,000.

Finally after applying the corrections above, values within the acceptable range of 7,000–120,000 DKK were retained, while all others were coded as missing.