What Works for the Unemployed?
Evidence from Quasi-Random Caseworker Assignments

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Abstract

Active labor market policies are integral to modern welfare states. In particular, classroom training has received public attention as a way to mitigate skill mismatches caused by globalization or automation. We analyze whether these programs help unemployed job seekers find jobs using a novel random caseworker instrumental variable (IV) design. Leveraging administrative data from Denmark, our identification strategy exploits that (i) job seekers are quasi-randomly assigned to caseworkers, and (ii) caseworkers differ in their tendencies to assign similar job seekers to different programs. Using our IV strategy, we find assignment to classroom training increases employment rates by 25 percent two years after initial job loss. This finding contrasts with the conclusion reached by ordinary least squares (OLS), which suffers from a negative bias due to selection on unobservables. The employment effects are driven by job seekers who complete the programs (post-program effects) rather than job seekers who exit unemployment upon assignment (threat effects), and the programs help job seekers change occupations. We show that job seekers exposed to offshoring – who tend to experience larger and more persistent employment losses – also have higher employment gains from classroom training. By estimation of marginal treatment effects, we conclude total employment may be increased by targeting training toward job seekers exposed to offshoring.

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1 Introduction

Active labor market policies (ALMPs) are integral to modern welfare states, but the use of ALMPs varies widely between countries. For example, Denmark spends as much as two percent of its gross domestic product (GDP) on ALMPs, whereas US labor market policies rely more on passive measures such as unemployment and disability insurance (Bown and Freund, 2019; Kreiner and Svarer, 2022). ALMPs offer a diverse set of programs including classroom, private, and public job training, but classroom training is increasingly receiving attention as a way to mitigate skill mismatches caused by structural changes in the labor market (World Economic Forum, 2020). Job seekers displaced by, for example, trade, technology, or automation may have accumulated specific human capital that is no longer in demand in the labor market, and as a result, these job seekers in particular may need retraining to find employment (Hummels et al., 2018; Costa et al., 2019; Humlum and Munch, 2019).

Do these classroom-training programs help unemployed job seekers find jobs? The literature suggests private job training is the more effective program, whereas the evidence for classroom training is at best mixed. McCall et al. (2016) review the evidence for six countries and conclude for Denmark that despite the large expenditures, "classroom training is largely ineffective in raising their participants' employment rates." They continue as follows: "Moreover, also in line with the evidence for other countries, programs that involve on-the-job training and are thus more similar to regular jobs, on average perform considerably better than vocational classroom training."[1]

Yet, prior evaluations of ALMPs faced three empirical challenges. The first challenge is that job seekers self-select into programs based on their preexisting job opportunities (Ashenfelter, 1978). For example, a job seeker who already has a pending job offer may not enroll in a long-term training course. Such information is typically unobserved to the researcher but likely plays a crucial role in the selection into classroom training. Second, job seekers may reap heterogeneous returns from training programs. For example, retraining may particularly benefit job seekers whose skills have become obsolete due to globalization or automation. The literature has mainly examined effect heterogeneity across age, gender, and unemployment-

[1] McCall et al. (2016) also conclude that "More broadly, the US literature suggests that zero is sometimes, but not always, a good summary of the impact of training programs.". The meta study by Card et al. (2018) finds that "Human capital programs have small (or in some cases even negative) short term impacts, coupled with larger impacts in the medium or longer run."

duration groups, whereas exposure to globalization has rarely been considered (Hyman, 2018). Finally, comparing studies of different ALMPs is difficult, because research designs differ in their study populations and time horizons (Card et al., 2018), and any single treatment often pulls job seekers in from a multitude of alternative programs (Heckman et al., 2000).

In this paper, we develop theory and use data from the Danish labor market to construct caseworker-tendency instruments that overcome the three challenges. Our identification strategy exploits the facts that, in Denmark, (i) job seekers are quasi-randomly assigned to caseworkers according to their day of birth (1-31), and (ii) caseworkers differ in their tendencies to assign job seekers to different training programs. These instrumental variables (IVs) allow us to evaluate the heterogeneous causal effects of different ALMPs within a coherent framework. IV methods are uncommon in the vast literature evaluating ALMPs, due to data limitations and lack of exogenous variation determining selection into training.[2]

We structure our analysis within a generalized Roy model of how caseworkers assign job seekers to training programs. Our setting is theoretically challenging because caseworkers choose from a menu of training options, and the benefits to job seekers are heterogenous across these programs. We show caseworker tendencies, defined as the leave-out means of program assignment rates by caseworkers, emerge as theory-consistent instruments that satisfy the conditions for non-parametric identification of potential outcomes along all treatment margins.

We establish four headline findings. First, using the caseworker-tendency instruments, we estimate that classroom-training programs have persistent positive effects on employment. Attending a classroom-training program is associated with 25 percent more hours of employment two years after the initial job loss. By contrast, we do not find that job training programs, such as wage subsidies, are associated with robust employment gains.

Second, we compare the IV estimates to ordinary least squares (OLS) estimates that assume "selection on observables only" – a conventional assumption in the literature. Strikingly, we find the IV and OLS estimates yield opposite conclusions regarding the benefits of ALMPs. In particular, using OLS implies all ALMPs have negative employment effects and that classroom training is particularly detrimental to employment. The stark differences between the IV and

[2]McCall et al. (2016) identify only one published study using an IV approach: Frölich and Lechner (2010). Their instrument exploits variation in training propensities across regions in Switzerland. Hyman (2018) is a more recent example of a study using an IV approach (see below for more details). Cederlöf et al. (2021) develop a similar caseworker instrument for the Swedish labor market to examine the characteristics of effective caseworkers. They do not evaluate the impact of ALMP programs on the unemployed.

OLS estimates (even with a rich set of controls) highlight the importance of accounting for selection based on unobserved characteristics of participants when evaluating ALMPs.

Third, we study the dynamics of how classroom training relates to employment at different time horizons. We decompose the classroom-training effects into threat effects, lock-in effects, and post-program effects, and make two important findings. First, we find OLS and IV identify similar-sized and negative lock-in effects of classroom training. Hence, OLS and IV do not differ due to different lock-in effects; they differ because job seekers with worse job prospects are the people who opt into training.[3] Second, we find the long-run IV is primarily driven by post-program effects rather than threat effects. This finding suggests classroom training increases employment by *reskilling* job seekers. The dynamic perspective of our approach bears resemblance to the timing-of-events evaluation method (Abbring and van den Berg, 2003) that has been used to evaluate threat and lock-in program effects using duration models.[4] Interestingly, van den Berg and Vikström (2022) derive identification results in a dynamic treatment setting, where the control group consists of not-yet-treated individuals, and show in an application that classroom training in Sweden has positive long-run earnings effects. They also emphasize the importance of accounting for the fact that non-treated job seekers tend to have short unemployment durations and more favorable personal characteristics.

Finally, we examine heterogeneity in the causal effects of training. We find job seekers initially employed in offshoring-exposed occupations – who tend to experience larger and more persistent employment losses – also have much higher gains from classroom training and that they drive the positive effects of these programs. Skill mismatches caused by globalization is an often-cited motivation for training programs, and our results suggest globalization-exposed job seekers have higher employment gains and that total employment may be increased by targeting training toward these job seekers.[5] We also explore whether our local average treatment effects

[3]The bias is a prospective version of Ashenfelter's dip (Ashenfelter, 1978). In theory appendix C, we show this selection bias arises from optimal training assignments if caseworkers take into account the opportunity costs of time spent on training (in terms of foregone earnings) and have latent information about these job options.

[4]Crépon and van den Berg (2016) provides a review of the the literature with a focus on a dynamic perspective of program effects.

[5]For example, the Trade Adjustment Assistance (TAA) program, one of the few classroom-training programs in the US, targets job seekers who have lost their jobs due to increased imports (U.S. Department of Labor, 2022). In Denmark, a stated goal of the classroom-training program is to "solve labor market restructuring and adaptation problems in accordance with the needs on the labor market in a short and a long term perspective." (Danish Ministry of Education, 2021), but classroom training is not targeted toward specific job seeker types. To the best of our knowledge, our paper provides the first direct evidence about whether training programs are particularly beneficial to job seekers who have lost their jobs due to international trade.

(LATE) for instrument compliers are informative for a broader set of job seekers. By estimation of marginal treatment effects, we find the positive effects of classroom training are similar for job seekers with different underlying resistances to training. These findings suggest that our LATE estimate for classroom training is representative of the broader population of job seekers.

We make three contributions to measurement that are critical for our empirical analysis. First, we collect two new administrative data sets: caseworker meeting registrations and individual job plans. The meeting registrations allow us to link job seekers to caseworkers. The job plans are law-mandated logs of caseworker inventions and provide detailed information about all ALMP program assignments in Denmark. Second, we document that many job centers allocate job seekers to caseworkers based on their day of birth (1-31), establishing our source of quasi-random variation. Third, we link our data to registers at Statistics Denmark, allowing us to study how treatment effects vary along a wealth of job-seeker characteristics, such as exposure to offshoring.

We also make a methodological contribution by extending judge IV designs to settings with multiple treatments and heterogeneous treatment effects. We conduct a host of specification checks that allow us to interpret our IV estimates as local average treatment effects (LATE). First, we test the exogeneity assumption by validating that caseworker assignments do not correlate with observable characteristics of job seekers. Second, we verify that caseworker tendencies are highly relevant for training assignments and that the effects likely are monotone across job seekers.[6] Finally, our identifying assumption is that caseworkers' tendencies for assigning training programs do not systematically correlate with other drivers of their value added. We provide evidence that supports this exclusion restriction, showing that the training tendencies are uncorrelated with factors potentially driving their value added such as the experiences, meeting frequencies, and caseloads of caseworkers.

The empirical ALMP program-evaluation literature is extensive. The paper closest to ours is Hyman (2018), who evaluates the Trade Adjustment Assistance (TAA) program that targets workers displaced by import competition in the US. He also constructs a caseworker-tendency instrument and finds significantly positive earnings effects for TAA-approved workers. We provide broader evidence for the impact of ALMP programs as we compare classroom training with job-training programs, we decompose the effects into threat, lock-in, and post-program

[6]Important for our setting with multiple treatments, we show the program-specific instruments do not have "cross-effects" on different training programs; see Behaghel et al. (2013); Bhuller and Sigstad (2022).

effects, and we examine heterogeneity across subpopulations including globalization-exposed job seekers. In addition, our instrument relies directly on randomness generated by the day-of-birth rules in the allocation of unemployed job seekers to caseworkers. A growing strand of the literature uses randomized controlled trials (RCTs) that also have the potential to address the empirical challenges in program evaluations outlined above. Our natural-experiment strategy complements this literature in several ways. The natural setting of our study mitigates concerns about external validity. Also, RCTs are often limited in scale, due to resource requirements, whereas we exploit the extent of our data to estimate effects by subpopulations. Further, we decompose the dynamics of our IV estimates and estimate marginal treatment effects to extrapolate to broader populations.[7]

The remainder of the paper is structured as follows. Sections 2 and 3 first describes our institutional setting and data. Section 4 presents a conceptual framework and derives our identification strategy based on caseworker tendencies. Section 5 performs instrument diagnostics. Section 6 presents our main empirical results, estimating how classroom training affects labor market outcomes. Section 7 decomposes the impacts into ex-ante, lock-in, and post-program effects. Section 8 studies heterogeneity in the treatment effects across training programs and worker types. Section 9 concludes.

2 Institutional Setting

In this section, we describe key features of the unemployment insurance system in Denmark. We mainly focus on caseworkers and the assignment of training programs, and we document how job seekers are quasi-randomly allocated to caseworkers, due to a day-of-birth rule.

2.1 Unemployment Insurance in Denmark

In Denmark, unemployed job seekers may receive unemployment insurance (UI) benefits for up to two years. The UI benefits are financed by membership contributions to UI funds, which the government subsidizes. The economic incentive to claim UI benefits is high: the UI benefits cover 90 percent of prior monthly earnings up to 3,075 USD.

[7] In addition, RCTs sometimes include many initiatives that make isolating the effects of particular programs difficult. For example, the Danish labor market authorities have adopted a systematic use of RCTs, and in some experiments, the treatment consists of a mix of job-search assistance programs, caseworker meetings, job training, and classroom training, see, for example Graversen and van Ours (2008), Vikström et al. (2013), and Gautier et al. (2018). McCall et al. (2016) provides a discussion of challenges faced by RCTs.

The public employment services in Denmark build on a *right and duty* principle (Kreiner and Svarer, 2022). That is, unemployed job seekers have the *right* to receive UI benefits but also a *duty* to live up to specific requirements. The requirements involve regular meetings with a caseworker at the local job center, active job search, and participation in training programs assigned by the caseworker. If the job seeker does not comply with the caseworker assignments, she will lose her right to receive UI benefits.

Table 1 shows the average job seeker meets with her caseworker for the first time in week five of the UI spell. The meetings typically last 30-45 minutes, and the average job seeker has 4.2 meetings during the entire UI spell.

2.2 Training Programs

The caseworker must prepare a *job plan* for the job seeker, specifying their assignments to training programs.

Caseworkers have two classes of training programs at their disposal: *job training* and *classroom training*. Job training includes internships and wage subsidies for employment at a private or public firm for a pre-specified period. Classroom training includes ordinary education, vocational training, and job-search-assistance courses. *Ordinary education* includes basic coursework, for example, language training or primary school courses, and more advanced coursework, for example, a university class. *Vocational training* courses target specific occupations or industries, such as a forklift certificate or a pc course.[8] Finally, *job-search-assistance courses* include dedicated courses on writing a CV or job application.[9]

Table 2 reports the share of job seekers in our analysis sample who are assigned to training programs within the first 12 months of their UI spell. The table shows that 40 percent of all job seekers are assigned to classroom training and 24 percent are assigned to job training. Classroom and job training are not mutually exclusive: 12 percent of job seekers are assigned to both programs. Finally, the table shows 48 percent of job seekers are in passive UI; they are neither assigned to classroom training nor to job training. Across all cells of Table 2, i.e. irrespective of the training status, job seekers must meet continuously with a caseworker over the UI

[8] A stated goal of the vocational training program in Denmark is to "solve labor market restructuring and adaptation problems in accordance with the needs on the labor market in a short and a long term perspective." (Danish Ministry of Education, 2021).

[9] We distinguish between job search assistance provided through caseworker meetings (monitoring and general guidance of job search, sanctioning etc.), and job search assistance provided in dedicated classroom courses.

spell. It is during these meetings that job-search monitoring, counseling, and sanctioning takes place. Hence, *all* job seekers are provided job-search assistance facilitated through caseworker meetings.

Table 1 compares the timing and duration of caseworker meetings and training programs. The average trainee spends 35 and 61 days on classroom and job training, respectively. In comparison, these job seekers spend the equivalent of almost a full day with their caseworker. Put differently, an essential way caseworkers affect job seeker activities is by assigning training programs.

Table 1: Timing and Duration of Training Programs

	(1) Assignment rate (%)	(2) Timing (week no.)	(3) Duration (days)
Caseworker meetings	100	5	1
Classroom Training	40	21	35
Job Training	24	24	61

Notes: Column (1) reports the share of job seekers who have at least one caseworker meeting (row 1), and the share of job seekers, who are assigned to a training program (row 2-3) within the first 12 months after job loss. Column (2) reports the average timing of the activities: how many weeks after initial job loss the average job seeker has her first meeting with a caseworker (row 1), and after how many weeks the assigned training program starts (row 2-3). Column (3) reports the duration of the activities: how many days the average job seeker is supposed to spend at training facilities, conditional on assignment (row 2-3). For comparison, row 1 reports our best estimate of how many days the average job seeker spends with a caseworker during the first 12 months after job loss. It is based on (i) the fact that the average job seeker has 4.2 caseworker meetings in total, (ii) which are 30-45 minutes long, and (iii) presuming job seekers spend at least five hours per day at training facilities.

Table 2: Assignment to Classroom and Job Training

	Share of job seekers (pct)	
	Classroom Training = 0	Classroom Training = 1
Job Training = 0	48	28
Job Training = 1	12	12

Note: Share of job seekers in the analysis sample who are assigned to a given training program within the first 12 months after the UI-spell start.

2.3 Assignment of Job Seekers to Caseworkers

The public employment services in Denmark are organized by local job centers, which allocate job seekers to caseworkers. Our identification strategy exploits that many job centers assign job seekers to caseworkers based on their monthly day-of-birth (1-31).

Figure 1 illustrates this allocation for a representative job center, plotting the day-of-birth distributions of job seekers allocated to different caseworkers in the job center. The figure reveals two features. First, job centers use *blocks* of days to allocate job seekers: caseworker 1 primarily handles job seekers born on the $1^{st} - 7^{th}$ of the month, caseworker 2 handles the $8^{th} - 15^{th}$, and so forth. The block structures enable us to easily detect whether a job center uses a day-of-birth allocation rule. Second, job centers occasionally deviate from the day-of-birth allocation rule. For example, caseworker 1 is also allocated a few job seekers outside the $1^{st} - 7^{th}$ interval. Deviations could happen for exogenous reasons, for example, caseworker illness, or endogenous reasons, for example, match effects. To circumvent these endogenous deviations, we base our identification strategy on the *day-of-birth-predicted* caseworkers, thus exclusively using the quasi-random variation arising from job seeker birthdays.

Figure 1: Allocation of Job seekers to Caseworkers over Day of Birth

Notes: This figure illustrates the day-of-birth distributions of job seekers allocated to different caseworkers in a representative job center. Due to data confidentially, we simulate a job center with a compliance rate of 60 percent, the median compliance in our analysis sample.

3 Data

Our empirical analysis relies on two new registers from the Danish Agency for Labor Market and Recruitment (STAR). The registers record meetings between caseworkers and job seekers and the assignments to training programs. We link these registers to several administrative registers at Statistics Denmark using unique person identifiers, providing detailed information on the characteristics and labor market outcomes of job seekers.

3.1 Data Sources

Caseworker Meetings

Our data on caseworker meetings records the date, time, and type of all meetings in the Danish job centers from 2011 to 2019. The dataset also includes identifiers for the job seeker and caseworker participating in each meeting. We use this dataset to link unemployed job seekers to caseworkers. First, we link job seekers to the caseworker who participated in the job seekers' first face-to-face meeting. We call this caseworker her *realized caseworker*.[10]

Second, we document the use of day-of-birth allocation rules in all 94 job centers in Denmark over time. Online Appendix OA2.4 details our procedure, which proceeds in two steps. First, we identify the job-center units in which job seekers are allocated to caseworkers.[11] Second, we link each caseworker to the birthday of their job seekers. For each job-center unit and year, we assign a *predicted caseworker* to each day in the month (1-31), defined as the caseworker with the most job seekers born on that day. Appendix Table B.1 shows we are able to link 90 percent of all job seekers (84 percent of all UI spells) who had at least one caseworker meeting to a *realized* and a *day-of-birth-predicted* caseworker.

Training Assignments

We measure the assignment of job seekers to classroom- and job-training programs, using a register on the individual *job plans* prepared by caseworkers. As noted in Section 2, the job plans are law-mandated registrations of all training assignments throughout the UI spells of job seekers. The job plans have two advantages over existing registers.

First, the job plans include *all* training assignments, regardless of whether the job seeker ends up participating in the programs. In the Danish context, the standard dataset for research on ALMPs is the *Danish Register for Evaluation of Marginalization* (DREAM). However, DREAM only records actual participation in programs. Furthermore, the data source for

[10]See Online Appendices OA2.2 and OA2.3 for details on how we identify the first face-to-face meeting in the UI spell and implement a crosswalk between caseworker identifiers over time.

[11]Some job centers are organized into smaller units (e.g., according to job seeker age), wherein caseworkers are allocated to job seekers. See further details in Online Appendix OA2.4.

DREAM is, in fact, the job-plan registrations. As discussed in McCall et al. (2016), measuring *assignment*, as opposed to only participation, may be more relevant for policy, because "it corresponds to what policies, in most contexts, can actually do."

Second, the job plans record the exact *timing* of the training assignments, including the start and end dates of the programs. By linking the timing of training to data on when job seekers enter and exit unemployment, we can decompose the dynamic employment effects of training into threat, lock-in, and post-program effects; see Section 7.1.

Online Appendix OA2.8 provides a validation of the job plans and our two interventions of interest: assignment to classroom and job training. Two insights merit note. First, we find that a substantial part of the *classroom-training* assignments likely represents actual education (ordinary education or vocational training). We find that 44 percent of classroom training assignees are *registered* as enrolled in an education within the first 12 months of unemployment. This corresponds to 51% of total assigned classroom training days. Given that some assignments never lead to an enrollment, this is likely a lower bound of the education content of classroom training. Second, the *job-training* assignments include both public and private internships and wage subsidies. For example, one third of the internships are public sector programs. This observation is important because the existing literature has found very different employment effects of public and private programs (Card et al., 2010).

Labor Market Outcomes

Our data on employment outcomes come from the *Register for Employees* (BFL), recording the work hours, earnings, and occupational codes of all job spells in Denmark from 2008 to 2021. A notable feature of the BFL register is that it contains a high-quality and continuous measure of employment hours, which stems directly from third-party reports to the Tax Authorities. We combine the register with DREAM to measure non-supported employment; see details in Online Appendix OA2.5.

Job Seeker Characteristics

Our data on *demographics* come from the *Population Register* (BEF), recording the gender, age, and country of origin for all Danish residents. We obtain information on job seekers' *education* from the *Education Register* (UDDA). Finally, we measure the *labor market histories* of job seekers using DREAM, BFL, and the *Employment Classification Module* (AKM). The DREAM register records public transfers, including UI benefits and education subsidies. The BFL and AKM registers provide information on past employment of the job seekers, including earnings, hours, industry, and occupations.

3.2 Analysis Sample

We base our analysis sample on all job seekers who became unemployed between 2012 and 2018 and had at least one meeting with a caseworker from the local job center.[12] Using our linked job-seeker–caseworker data, we obtain information about the job seeker's *realized* and *day-of-birth predicted* caseworker.

We apply four additional sample restrictions to support our identification strategy. First, based on a visual inspection of the day-of-birth allocation rules, we drop job-center-unit-years that do not use a clear (block) structure for the allocation of birthdays, or where the compliance to the block structure is very low. See Online Appendix OA2.4. Appendix Table B.1 shows the number of unique job centers drops from 94 to 53, while the day-of-birth compliance rate increases from 42 percent to 51 percent when we impose this restriction. Second, we exclude from the analysis sample all job seekers with non-western origin, because an institutional feature of the Danish immigration system makes non-western job seekers over-represented among job seekers born on the 1st of the month.[13] Third, to implement our identification strategy based on random caseworker allocations, we require that all job-center-unit-year cells contain at least two predicted caseworkers. Finally, each caseworker in the sample must have at least 50 assigned job seekers over the sample period. Appendix Table B.1 shows our final analysis sample captures 53 of all job centers and about 20% of all job seekers who initiated a UI-spell from 2012-2018 and had at least one caseworker meeting. Appendix Figure A.1 shows these job centers are spread out across Denmark.

[12] Because job plans are available from 2012 onward, we base our analysis sample on UI-entries from 2012 to 2018.

[13] An immigrant who arrives in Denmark without a birth certificate are automatically assigned January 1st as their birthday. As a result, non-western make up 10 percent of job seekers born on the 1st of the month, while only constituting 3.5 percent of the job seeker sample.

4 Identification Strategy

The goal of this paper is to estimate the effects of assignment to training programs, as opposed to passive UI, on the employment of job seekers. Identifying these effects is challenging because job seekers may opt into training programs based on their job opportunities. This selection bias could come through self-selection of the job seekers or from the caseworker assignments. For example, a job seeker with a pending job offer may be more resistant to start training than a job seeker without immediate job prospects. Similarly, caseworkers may be hesitant to assign a long-term training course to job seekers who already have good job options. Furthermore, whether controlling for observables of the job seekers eliminates this selection bias is unclear, because job seekers with similar work *histories* could face different job *prospects* that are not recorded in our administrative data.

To overcome this identification challenge, we follow the judge IV literature and develop caseworker-tendency instruments for assignment to training programs. Our setting departs from the canonical judge IV setup in two aspects. First, caseworkers may assign job seekers to *multiple* training programs. Second, job seekers are allocated to caseworkers based on an *observed rule* (day of birth).

In Appendix C, we develop a generalized Roy model that addresses these complications. First, we show our setting collapses to the canonical single-treatment case once we compare caseworkers with similar tendencies for the other training programs. Second, we state the identifying assumptions explicitly in terms of the day-of-birth-predicted caseworkers.

In particular, we instrument the assignment of job seeker i to training program k with the training program assignment rate among other job seekers with the same day-of-birth predicted caseworker:

$$Z_{ki} = \mathbb{E}\left[D_{kj} | c(b_j) = c(b_i), j \neq i\right] \tag{1}$$

where D_{ki} equals one if job seeker i is assigned to program k within the first 12 months after job loss, and $c(b)$ allocates job seekers with day of birth $b \in \{1, 31\}$ to caseworkers c. We denote caseworkers with a low Z_k as *k-restrained* and caseworkers with a high Z_k as *k-inclined*.

The theoretical extension in Appendix C motivates instrument diagnostics in Section 5 and facilitates the estimation of marginal treatment effects in Section 8.3. In our main empirical analysis (Section 6), the theory highlights the importance of controlling non-parametrically for the other training-program instruments (Blandhol et al., 2022). In practice, we first estimate a standard two-stage least squares (TSLS) specification to facilitate comparisons with how prior papers have handled multiple treatments in judge IV setups (Maestas et al., 2015; Bhuller et al., 2020). In a second step, we show robustness to estimating the specification around an evaluation point for the other training-program instruments (Mountjoy, 2022).

Let Y_{it} denote the employment of job seeker i in period t. The TSLS specification reads

$$D_{ki} = \delta_{j(i)} + \delta_{k1} Z_{1i} + \delta_{k2} Z_{2i} + \varepsilon_{1i} \tag{2}$$

$$Y_{it} = \beta_{j(i)t} + \beta_{1t} D_{1i} + \beta_{2t} D_{2i} + \varepsilon_{2i}, \tag{3}$$

where j are job-center-unit-year combinations, the units wherein our randomization takes place. Hence, we compare job seekers from the same job-center unit and year who, due to their day-of-birth, receive different treatments. We cluster standard errors on job seeker and predicted caseworker level.

Importantly, our identification strategy does *not* preclude that caseworkers differ in their "value added" to the outcomes of job seekers, as documented by, e.g., Behncke et al. (2010) and Cederlöf et al. (2021). Instead, our identifying assumption is that caseworkers' tendencies for assigning training programs do not systematically correlate with their value added (entering ε_{2i} in Equation (3)). In Section 5.4, we provide evidence that supports this exclusion restriction, showing that the training tendencies are uncorrelated with factors potentially driving their value added such as the experiences, meeting frequencies, and caseloads of caseworkers.

5 Instrument Diagnostics

In this section, we assess our caseworker-tendency instruments. We provide evidence that the instruments satisfy the relevance, exogeneity, exclusion, and monotonicity conditions for interpreting our IV estimates as local average treatment effects (LATE).

Our identification strategy makes two departures from earlier judge-IV designs. First, we directly exploit the *source* of quasi-random assignment coming from the day-of-birth allocation of job seekers to caseworkers.[14] We find exploiting the source of randomization is crucial for instrument exogeneity in our context. Second, we have a setting with multiple treatments, which

[14]Existing studies either rely on the law mandating some random allocation or anecdotal evidence on why the realized allocation may be quasi-random.

motivates new instrument diagnostics. In particular, we show the program-specific tendencies do *not* alter the assignment of other programs (Behaghel et al., 2013; Bhuller and Sigstad, 2022).

5.1 Relevance

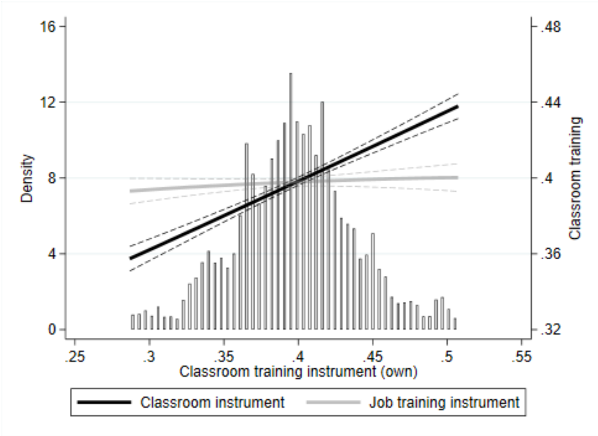
Figure 2 shows how our caseworker-tendency instruments affect the assignment of job seekers to training programs after initial job loss. The figure delivers two takeaways.

First, the caseworkers' training program tendencies strongly affect the job seekers' assignment to training programs. Figures 2a-2b corroborate this conclusion by combining the first-stage relationships with the distribution of caseworker training tendencies. For example, shifting from the most classroom-training *restrained* to the most classroom-training *inclined* caseworker within a job-center-unit-year corresponds to an 8-percentage-point increase in the probability of being assigned to classroom training within the first 12 months. This increase is a 20 percent increase relative to the mean classroom-training assignment rate.[15] As Figures 2c-2d show, most of these assignments occur in the first quarter after the initial job loss. In Sections 7.1 and 6.1, we study how the assignments affect the dynamics of training and employment of job seekers.

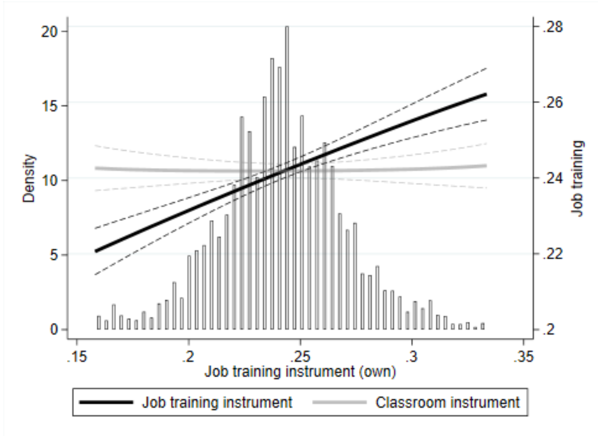
Second, the program-specific instruments do *not* alter the assignment to other programs. For example, Figure 2a shows the assignment to classroom training only is affected by the caseworkers' classroom-training tendencies, not by their job-training tendencies. As discussed in Section 4, this absence of "cross-effects" is crucial for identification in our setting with multiple treatments. In particular, it allows us to collapse our setting to the canonical single-treatment case by comparing caseworkers with similar tendencies for the other training programs. Importantly, this collapse requires us to find otherwise similar caseworkers who differ in their tendency for each training program. Fortunately, Appendix Figure D.1 shows caseworker tendencies are indeed uncorrelated across programs in our setting.

[15] Appendix Table D.1 shows the program-specific instruments have power for each training program.

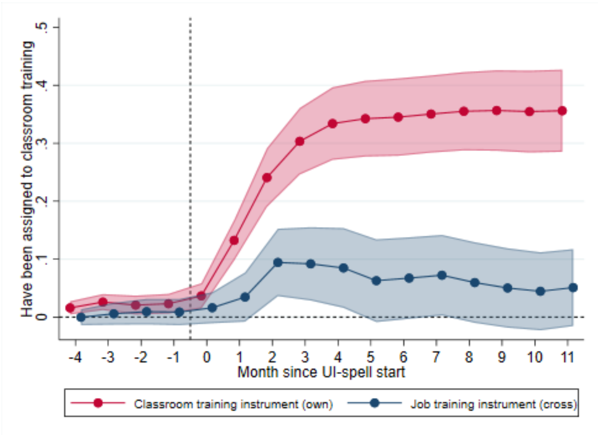
Figure 2: Assignment to Training Programs



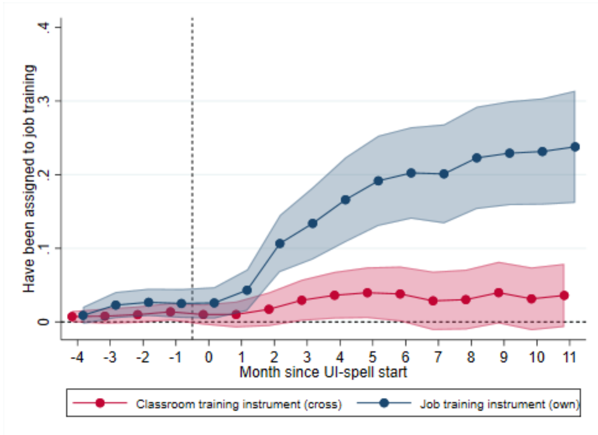
(a) Classroom Training



(b) Job Training



(c) Classroom Training



(d) Job Training

Note: Panel A (B) represents the first stage of assignment to classroom training (job training) within the first 12 months on the two caseworker-tendency instruments. The bars represent the distribution of the own-instrument demeaned by job-center-unit-year fixed effects and the cross-instrument (excluding top and bottom 1%). The black line represents the coefficients from a local linear regression of the training program on the own-instrument, both demeaned by job-center-unit-year fixed effects and the cross-instrument. The gray line represents the coefficients from a local linear regression of the training program on the cross-instrument, both demeaned by job-center-unit-year fixed effects and the own-instrument. The local linear regressions are based on an Epanechnikov kernel (with bandwidth 0.1). Panels C and D represent a dynamic version of the first stage. They represent the first stage of having been assigned to classroom training (job training) in or prior to a given month on the two instruments. Shaded areas represent 95% confidence intervals.

5.2 Exogeneity

In Table 3, we test the exogeneity of the caseworker-tendency instruments. The table is based on the following logic: if job seekers are allocated to caseworkers in a quasi-random fashion, we should not be able to predict caseworkers' training tendencies based on the characteristics of job seekers measured before job loss.

The exogeneity test yields three takeaways. First, the assignment of job seekers to training programs is highly endogenous (Columns (1) and (2)), replicating the common finding that job seekers select into training. Second, instrumenting with the training tendencies of job seekers' *realized* caseworker does not solve the endogeneity issue (Columns (3) and (4)), because job centers deviate from the day-of-birth allocation rule in a non-random fashion. Finally, using the *day-of-birth-predicted* caseworkers, caseworker training tendencies and job seekers' characteristics are uncorrelated (Columns (5) and (6)). The evidence suggests our day-of-birth-predicted training tendencies are indeed exogenous shifters to the assignment of job seekers to training programs. These findings highlight the importance of using the explicit quasi-random variation arising from the day-of-birth allocation rules.

5.3 Monotonicity

The monotonicity assumption states that a job seeker assigned to training by a training-restrained caseworker also should be assigned by a training-inclined caseworker. Furthermore, in our setting with multiple treatments, monotonicity requires that the program-specific caseworker training tendencies do not alter the assignment to other programs. Behaghel et al. (2013) call this assumption *extended monotonicity*; see Section 4 for discussions. In other words, we require that the "own-instruments" are significant and the "cross-instruments" are insignificant among all subsamples of job seekers.

To test the monotonicity assumption, we adapt two specification tests from Bhuller et al. (2020) to our setting with multiple treatments. First, in Appendix Tables D.4 and D.5, we split the job seekers into quartiles based on their propensities for assignment to training. We then show the "own-instruments" are significant and the "cross-instruments" are insignificant in all quartiles.

Second, we assess whether caseworkers who are more training-inclined toward one job seeker are also more training-inclined toward other job seekers. To test this possibility, we rely on the same quartiles but now measure caseworker training tendencies using job seekers from *other* subgroups. Appendix Tables D.6 and D.7 show that, in all subgroups, the coefficient on the "reversed" own-instrument is positive, whereas the "reversed" cross-instrument is insignificant. Taken together, these tests support the monotonicity assumption of our identification strategy.

Table 3: Testing for Random Assignment of Job Seekers to Caseworkers

	Actual assignment		Realized Caseworker		Predicted Caseworker		Covariates	
	(1) Classroom training	(2) Job training	(3) Classroom training	(4) Job training	(5) Classroom training	(6) Job training	(7) mean	(8) sd
Demographics								
Age	0.002***	0.000	-0.002***	-0.002***	0.000	-0.000	41.636	12.121
Male	0.014**	-0.082***	0.001	0.005	-0.001	0.001	0.480	0.500
Immigrant	0.132***	0.022*	0.018**	0.016*	-0.003	0.001	0.051	0.220
Descendant	0.154***	-0.004	0.043	0.007	-0.001	-0.004	0.002	0.042
Married	-0.063***	-0.022***	-0.009**	-0.007*	-0.003*	-0.001	0.418	0.493
Number of children	-0.013***	-0.017***	-0.005***	-0.005***	-0.001	-0.002	0.754	1.007
Education								
Primary	0.017	0.117	0.019	0.006	0.014	-0.025	0.001	0.037
Lower secondary	-0.034	-0.005	-0.008	-0.022	0.003	-0.000	0.198	0.398
Upper secondary	-0.050**	-0.010	0.002	-0.015	0.001	-0.003	0.525	0.499
Short cycle tertiary	-0.001	-0.019	0.008	-0.009	0.006	0.002	0.052	0.223
Bachelor	-0.111***	-0.023	-0.018	-0.050**	0.005	0.003	0.154	0.361
Master	-0.064**	0.045	-0.008	-0.037*	-0.006	-0.002	0.056	0.229
Doctoral	-0.165***	0.017	-0.029	0.011	0.017	0.002	0.002	0.050
Labor market history								
UI-benefits in year t-1	-0.062***	-0.081***	-0.003	0.021***	-0.000	0.003	0.442	0.497
UI-benefits in year t-2	-0.072***	-0.058***	-0.006	0.010**	0.001	-0.003	0.449	0.497
Cash benefits previous 5 years	0.102***	0.050***	0.013**	0.020***	0.000	-0.001	0.078	0.268
Parental leave in year t-1	-0.012	0.016	-0.019***	-0.025***	0.000	0.002	0.075	0.264
Education subsidy in year t-1	-0.111***	-0.026***	-0.002	-0.022***	-0.002	-0.004	0.102	0.302
Public transfers in year t-1	0.095***	0.107***	0.008*	0.034***	0.001	0.002	0.359	0.480
Employed in year t-1	-0.092***	-0.141***	-0.002	-0.005	0.001	0.002	0.885	0.319
Employed in year t-2	-0.054***	-0.025**	-0.024***	-0.036***	0.001	-0.004	0.880	0.326
Hours in year t-1	-0.001***	-0.001***	-0.000***	-0.001***	-0.000	0.000	93.008	58.026
Hours in year t-2	0.001***	0.001***	0.000*	0.000**	0.000	-0.000	94.685	59.971
Earnings in year t-1 (1,000 DKK)	-0.001*	-0.003***	0.000	-0.000	0.000	0.000	17.445	14.049
Earnings in year t-2 (1,000 DKK)	-0.003***	-0.005***	-0.001	-0.001**	-0.000	-0.000	16.860	13.491
Previous industry								
Manufacturing	0.082***	0.010	-0.005	-0.035***	0.001	-0.001	0.117	0.321
Construction	-0.177***	-0.140***	-0.007	-0.031***	-0.002	-0.005	0.099	0.299
Wholesale and repair	0.021**	0.085***	0.011**	-0.001	0.002	-0.003	0.134	0.341
Transportation and storage	0.048***	-0.029**	-0.003	-0.008	0.002	-0.005	0.040	0.195
Accomodation and food service	-0.056***	-0.006	-0.013	-0.035***	0.002	0.001	0.038	0.190
Administrative and support service	0.027**	-0.029***	0.010	-0.019***	-0.002	-0.005	0.084	0.278
Education	-0.034***	-0.010	0.003	0.023***	-0.004	-0.002	0.054	0.227
Human health and social work	-0.063***	-0.028***	0.004	0.011*	-0.002	-0.002	0.181	0.385
Previous occupation								
Professionals	-0.042**	-0.056***	-0.005	-0.009	0.006	-0.004	0.138	0.345
Technicians and associate professionals	-0.028	-0.047***	0.001	0.003	0.008	-0.010	0.087	0.282
Clerical support workers	0.008	0.000	0.002	0.005	0.004	-0.002	0.097	0.296
Service and sales workers	-0.067***	-0.034**	-0.011	-0.014	0.009	-0.005	0.239	0.426
Skilled agricultural, forestry and fishery	-0.176***	-0.175***	-0.012	-0.068***	0.015*	-0.022**	0.022	0.147
Craft and related trade workers	-0.102***	-0.098***	-0.009	-0.033***	0.006	-0.006	0.157	0.364
Plant and machine operators, assemblers	-0.012	-0.061***	-0.005	-0.022*	0.011*	-0.004	0.088	0.283
Elementary occupations	-0.056***	-0.043**	-0.021*	-0.031**	0.006	-0.007	0.131	0.338
Missing	0.108***	0.038	-0.019	-0.051***	0.018**	-0.020**	0.018	0.132
UI-fund								
Academics Association	0.046***	-0.033**	0.061***	0.047**	0.008	0.001	0.068	0.251
Danish Trade Union Association	0.018***	-0.002	-0.003	-0.031***	0.001	0.002	0.648	0.478
Obs	196029	196029	195358	195358	196029	196029	196322	
Number of FE's	223	223	223	223	223	223		
F-stat	83.857	98.966	2.734	8.507	0.858	0.921		
P-value	0.000	0.000	0.000	0.000	0.731	0.619		

Notes: Columns (1) and (2) regress the assignment to training programs on job seekers' covariates. Columns (3) and (4) regress the training tendency of job seekers' *realized* caseworker on the job seekers' covariates. Columns (5) and (6) regress the training tendency of job seekers' *day-of-birth-predicted* caseworker (defined in Equation (1)) on the job seekers' covariates. The training tendencies are leave-out means. To ease comparison across columns, the dependent variable has been standardized to have mean zero and standard deviation one. Education levels are defined according to the 9 sections of the ISCED classification. The omitted category is "Unknown education" and "Early childhood education". Industries are defined as the 21 sections of the NACE classification. Only dummies for the eight largest industries are included in the regression. Previous occupations are defined as the 10 sections of the ISCO08 classification. The omitted category is "Managers". All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job seeker level, but only significance levels are reported: *p<0.10 ** p<0.05 *** p<0.01.

5.4 Exclusion

To interpret our IV estimates as treatment effects of training, we require the *exclusion restriction* that caseworker training tendencies only affect job-seeker outcomes through the assignment to training. An obvious threat to the exclusion restriction is that caseworkers serve multiple purposes. Aside from assigning training programs, caseworkers meet with job seekers and give advice on job search. For example, caseworkers who are very training-inclined could also meet more frequently with their job seekers, or provide them with better job-search advice, which could violate the exclusion. We conduct two tests of the exclusion restriction.

First, we test whether caseworkers' training tendencies correlate with proxies for the quality or quantity of their *job-search advice*. For example, the quality of advice could depend on the caseworker's experience, and the quantity of advice could depend on her caseload. Following this idea, Appendix Table D.8 shows caseworker training tendencies are uncorrelated with their experience and caseload.

Second, we assess whether more training-inclined caseworkers *meet* earlier or more frequently with the job seekers. To address this concern, in Appendix E.3, we reestimate our main IV specification while controlling for the frequency and timing of meetings between the job seeker and caseworker. Our results are robust to these controls and suggest meeting frequency and timing do not explain our estimates for the effects of training.

In summary, these specification tests support the exclusion restriction that caseworker tendencies only affect job seeker outcomes through the assignment of training programs.

6 Effects of Assignment to Classroom Training

In the following sections, we use our caseworker-tendency instruments to estimate the effects of assignment to training on the employment of job seekers. We benchmark our IV estimates to OLS estimates that assume "selection on observables only." In Sections 6 and 7, we first focus on classroom training. In Section 8, we then compare the effects of classroom training to the effects of job training.

6.1 Employment Effects

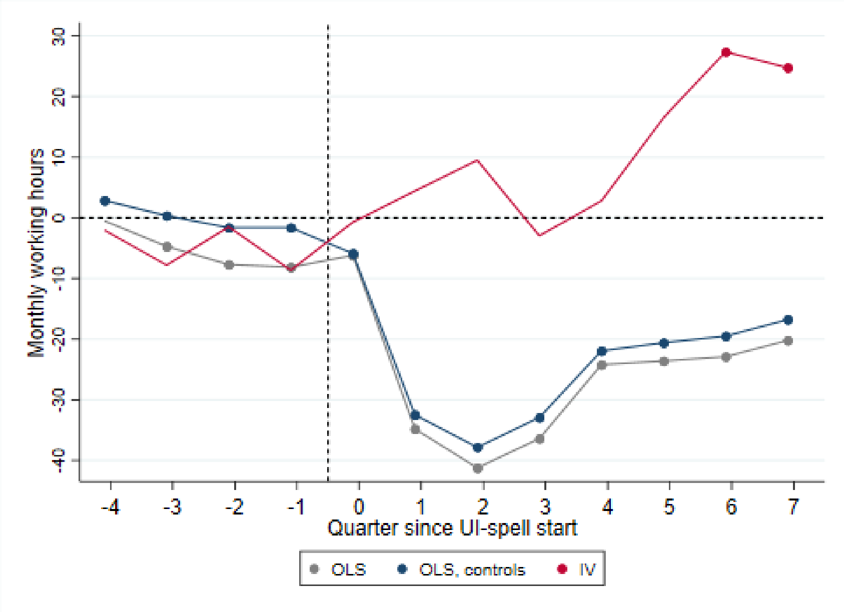
Figure 3 shows the effect of assignment to classroom training on the average monthly work hours in a given quarter relative to job loss ("UI-spell start"). The figure includes our IV estimates, instrumenting assignment to training with caseworker training tendencies, and conventional OLS estimates, with and without controls. We use the pre-determined covariates from Table 3 as control variables.[16] The figure reveals three insights.

First, the IV estimates show persistent positive effects of assignment to classroom training on employment. The employment gains grow steadily over time, stabilizing at about 25 hours per month two years after the initial job loss, equivalent to a 25 percent increase relative to the employment before job loss. Second, OLS reaches the *opposite* conclusion regarding the benefits of assignment to classroom training. In particular, the OLS estimates suggest training is associated with reduced employment, especially when job seekers are most engaged in training. The OLS conclusions align with the prior literature summarized by McCall et al. (2016).[17] Third, controlling for pre-determined characteristics of the job seekers ("OLS, controls") eliminates the pre-period bias but does not change the strong lock-in effects in the post-period. The spurious lock-in effects of OLS highlight that job seekers with worse job *prospects* are the individuals who opt into training, revealing a *prospective* version of the Ashenfelter dip (Ashenfelter, 1978).

[16] Biewen et al. (2014) and van den Berg and Vikström (2022) propose a dynamic control strategy, combining the "selection on observables" assumption with the timing of program assignments. We adopt a simple control strategy based on pre-determined characteristics to facilitate comparison to the IV estimates, which do not use information about the timing of program assignment. That said, all jobseekers in our analysis sample have at least one meeting with their caseworkers and are thus exposed to their caseworkers' tendencies to assign training programs.

[17] See quote in Footnote 1.

Figure 3: Effect of Assignment to Classroom Training



Note: Effect of assignment to classroom training on average monthly working hours in a given quarter relative to job loss. The gray and blue lines represent the effect obtained with a simple OLS regression and OLS that controls for job seeker predetermined characteristics. These controls include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). The red line represents the effect obtained by IV-estimation. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

Appendix Figures A.2a and A.2b show the benefits are consistent across alternative labor market outcomes. Assignment to classroom training increases the extensive margin of employment by about 20 percentage points two years after initial job loss, and the monthly earnings increase by about 600 USD (5,000 DKK), equivalent to 25 percent of job seekers' earnings before job loss.

Appendix E presents various robustness tests of our IV estimates. First, our baseline TSLS specification controls linearly for assignment to job training, yet Blandhol et al. (2022) highlight the importance of allowing for *flexible* controls to interpret TSLS estimates as LATEs. Our theoretical framework in Appendix C also formalizes this point. In Appendix E.1, we show our results are robust to estimating our TSLS specification around an evaluation point z_2' for the job-training instrument (Mountjoy, 2022). Second, as explained in Appendix C, our caseworker-tendency instrument shifts job seekers into classroom training from two margins: From "passive UI" to "classroom training only", and from "job training only" into "both job training and classroom training." Appendix E.2 shows most job seekers are shifted along the passive margin, and our results are robust to focusing exclusively on these compliers.

6.2 Differences between OLS and IV

In further analysis, we investigate the differences between the IV and OLS estimates. We conclude the differences are driven by negative *selection bias* (job seekers with worse job prospects opting into training) and not *effect heterogeneity* (instrument compliers gaining more from training than the average trainee).

First, investigating effect heterogeneity on *observables*, Appendix Figure E.4 shows the OLS estimates are robust to reweighing job seekers to match the characteristics of compliers. Second, investigating the role of *unobserved* heterogeneity, Section 8.3 shows the marginal treatment effects are similar for job seekers with different latent resistance to training. These findings indicate the differences between OLS and IV are not driven by effect heterogeneity on unobservables. Finally, Appendix Figure A.3 highlights the negative selection into training by unpacking the OLS and IV estimates into their implied employment outcomes with and without training. The figure shows the job seekers who opt out of training have better employment prospects than job seekers whose training decisions depend on the training tendency of their caseworker. In Section 7.1, we further investigate the difference between OLS and IV by decomposing the effects into the underlying threat, lock-in, and post-program effects of assignment to classroom training.

7 Mechanisms

This section investigates the mechanisms through which assignment to classroom training affects employment. In Section 7.1, we first decompose the effects into threat, lock-in, and post-program effects of assignment to classroom training. In Section 7.2, we investigate the role of occupational mobility in generating the employment gains from classroom training.

7.1 Threat, Lock-in, and Post-program Effects

The OLS estimates in Figure 3 align with the existing literature (McCall et al., 2016; Jespersen et al., 2008; Munch and Skipper, 2008). In the literature, the short-run drop in employment has been ascribed to negative *lock-in* effects, whereas the long-run losses have led to the conclusion that the *post-program* effects of training are at best zero. The IV estimates, however, are difficult to rationalize with the same underlying dynamics. To investigate the difference between OLS and IV, we now decompose the effect of assignment to classroom training into the underlying threat, lock-in, and post-program effects.

Our decomposition relies on splitting all job seekers who get assigned to classroom training (assignees, hereafter) into four mutually exclusive training states in a given period, $s \in \{a, b, c, d\}$. The state of an assignee depends on whether she (*a*) has not yet been assigned to training, (*b*) has not yet started her training assignment, (*c*) is undergoing training, or (*d*) is done with training. We can thus decompose the effects of assignment to classroom training on employment in a given period (β_{1t} in Equation (3)) into contributions from the assignees in each of the states:

$$\beta_{1t} = \sum_{s \in \{a,b,c,d\}} \gamma^s_{1t} \times (\beta^{1s}_{1t} - \beta^{0s}_{1t}), \tag{4}$$

where γ^s_{1t} denotes the share of assignees present in state s in period t, and $(\beta^{0s}_{1t}, \beta^{1s}_{1t})$ denotes their potential employment outcomes with and without the training assignment. Appendix F.1 describes how we assign job seekers to training states in our data.

Importantly, each of the components in Equation (4) corresponds to an effect discussed by the existing literature: job seekers who (*b*) have not yet started training are subject to the so-called *threat* effect of training. Job seekers who (*c*) are undergoing training are subject to the *lock-in* effect. Job seekers who (*d*) are done with training are subject to the *post-program* effect. Finally, job seekers who (*a*) have not yet been assigned to training are subject to a *placebo* effect.

Our estimation of Equation (4) proceeds in three steps. First, we estimate the training state probabilities γ^s_{1t} by regressing indicators D^s_{it} for whether a job seeker is in training state s on our treatment variables:

$$D^s_{it} = \gamma^s_{1t} D_{1i} + \gamma^s_{2t} D_{2i} + \Omega X_{it} + \varepsilon_{it}. \tag{5}$$

Second, we estimate the employment outcomes in each training state β^s_{1t}. To do so, we follow Abadie (2002) and interact the outcome variable Y with the state indicators D^s_t and then regress these interactions on our treatment-status indicators:

$$Y_{it} \times D^s_{it} = \beta^s_{1t} D^s_{it} + \beta^s_{2t} D_{2i} + \Omega X_{it} + \varepsilon_{it}. \tag{6}$$

Our final step is to estimate the state-specific counterfactual outcomes β^{0s}_{1t}. For simplicity, we here assume the average counterfactual outcomes in a given period are homogeneous across states:

$$\beta_{1t}^{0s} = \beta_{1t}^{0} \tag{7}$$

Then, again following Abadie (2002), we can estimate the counterfactual employment by running the regression:

$$Y_{it} \times (1 - D_{1i}) = \beta_{1t}^{0}(1 - D_{1i}) + \beta_{2t}^{0} D_{2i} + \Omega X_{it} + \varepsilon_{it}, \tag{8}$$

Appendix Figure A.3 plots the counterfactual employment outcomes according to OLS and IV.

We use Equations (4)-(8) to decompose the OLS and IV estimates in Equation (3). For the IV estimates, we instrument the treatment variables (D_{1i}, D_{2i}) with our instruments (Z_{1i}, Z_{2i}), using the first-stage Equation (2).

7.1.1 Training-State Probabilities

Figure 4 plots the training-state probabilities γ_{1t}^{s}, estimated by OLS (Panel (a)) and IV (Panel (b)). These probabilities represent the probability of being in a given state in a given month, conditional on being assigned to classroom training within the first 12 months of job loss, for the full population and the subpopulation of compliers, respectively.

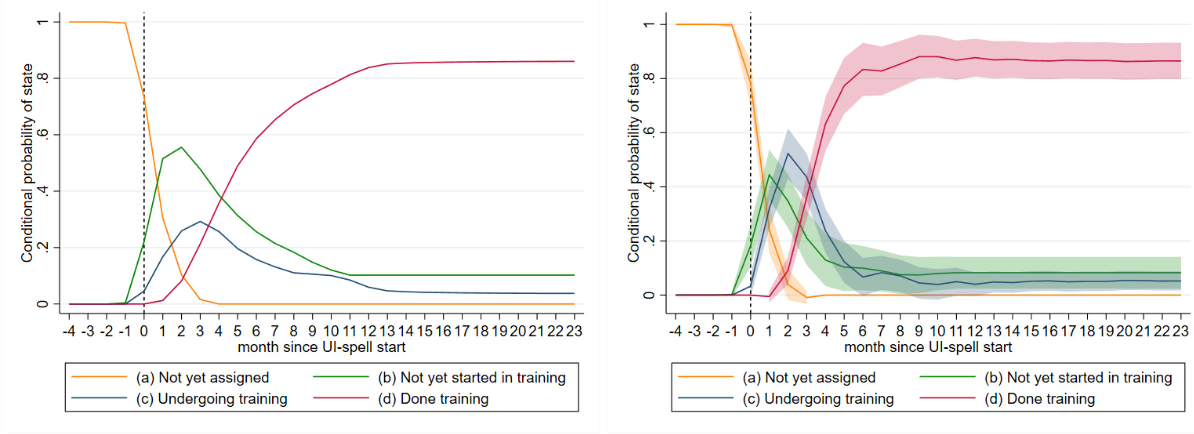
Comparing Panels (a) and (b), the training dynamics are similar in the full population and the subpopulation of compliers. In both populations, most job seekers are waiting for their training program to begin (green) or undergoing the training program (blue) in the first couple of months after job loss. After the first year, about 90 percent of job seekers are done with their assigned training program (red).[18] This finding suggests the differences between the OLS and IV are *not* driven by heterogeneous training dynamics. For example, the OLS and IV are not different because the probability of being "locked in" to training is larger for the full population than for the complier population. If anything, compliers are more likely to be locked into training in the first couple of months.

Because the two populations experience similar training dynamics, the difference between OLS and IV must be due to a difference in potential outcomes for assignees. Appendix Figure A.4 supports this hypothesis: across training states, assignees in the full population have

[18]Note the finding that 90 percent of job seekers progress into state (d) is *not* mechanical; if a job seeker exits unemployment before completing the training program (d), she will remain in her latest state (b or c) in all future periods. See Online Appendix F.1 for details.

lower potential outcomes than assignees in the subpopulation of compliers. To further support this claim, Online Appendix E.5 shows the baseline OLS and the OLS re-weighted by the IV training dynamics are very similar.

Figure 4: Training-State Probabilities



(a) OLS

(b) IV

Note: The figures plot the state probabilities conditional on assignment to classroom training (γ^s_{1t}). They are obtained by regressing an indicator for being in state s in period t on assignment to classroom, job training, and a set of controls. For the OLS, the set of controls include job-center-unit-year fixed effects as well as socio-demographics and labor market history from Table 3, education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). For the IV, we only include job-center-unit-year fixed effects, and we instrument assignment to classroom and job training by the predicted caseworker tendencies. Shaded areas represent 95% confidence intervals.

7.1.2 Decomposition of Employment Effects

Figure 5 decomposes the employment effect of assignment to classroom training into a placebo, threat, lock-in, and post-program effect. Panels (a) and (b) represent a decomposition of the OLS and IV estimate, respectively.

Comparing the panels yields three findings. First, OLS and IV identify a similar-sized negative lock-in effect of classroom training. Hence, lock-in effects are not causing the difference between OLS and IV.

Second, OLS and IV are different in the short run because they identify very different threat effects. In particular, the IV estimates reveal a positive threat effect of assignment to classroom training, confirming prior experimental estimates (Black et al., 2003).[19] By contrast, the OLS suggests a strong negative threat effect. The difference between OLS and IV in estimated threat

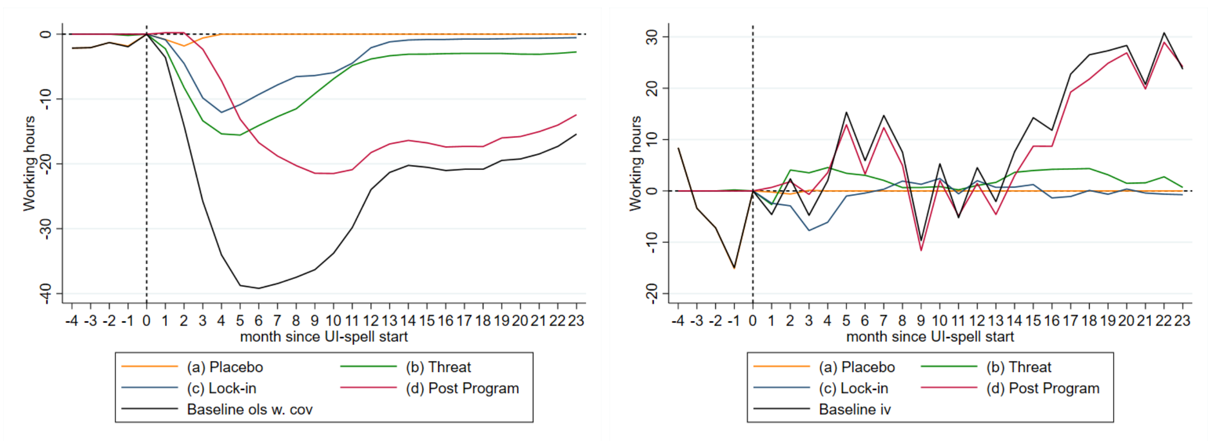
[19]Black et al. (2003) shows the threat of training makes job seekers exit unemployment; we show job seekers exit unemployment for employment.

effects reflects that job seekers have depressed job options while waiting for their assigned training program to begin.[20]

Third, OLS and IV are different in the long run because they identify very different post-program effects. The IV estimates show completion of classroom training helps job seekers find jobs, whereas the OLS estimates suggest classroom training hurts their long-run employment potential. The differences between OLS and IV show job seekers who opt into training have persistently lower employment potentials.[21]

Finally, the IV estimates show the positive long-run employment effects of classroom training are driven by post-program effects rather than threat effects.[22] This finding suggests classroom training increases employment by *reskilling* job seekers and that classroom training could help mitigate structural challenges in the labor market. In Section 8.2, we investigate whether job seekers exposed to offshoring have higher gains from classroom training.

Figure 5: Decomposition of the Effect on Working Hours



(a) OLS (b) IV

Note: Decomposition of the baseline OLS and IV estimate of the effect of assignment to classroom training on working hours in a given month relative to job loss. The baseline estimate is decomposed into contributions from each of the four training-states, $\gamma^{s}_{1t} \times (\beta^{1s}_{1t} - \beta^{0s}_{1t})$, estimated using Equations (4)-(8). For the IV estimates, we instrument the treatment variables D_{ki} using the first-stage Equation (2). This figure shows no indications of statistical significance.

[20] Appendix Figure A.4 shows average employment in state (c) is lower for the full population than in the subpopulation of compliers.

[21] Appendix Figure A.4 shows average employment in state (*d*) is lower for the full population than in the subpopulation of compliers.

[22] To ease comparison with the baseline IV estimate, Appendix Figure A.5 aggregates the monthly contributions to the quarterly level.

7.2 Occupational Mobility

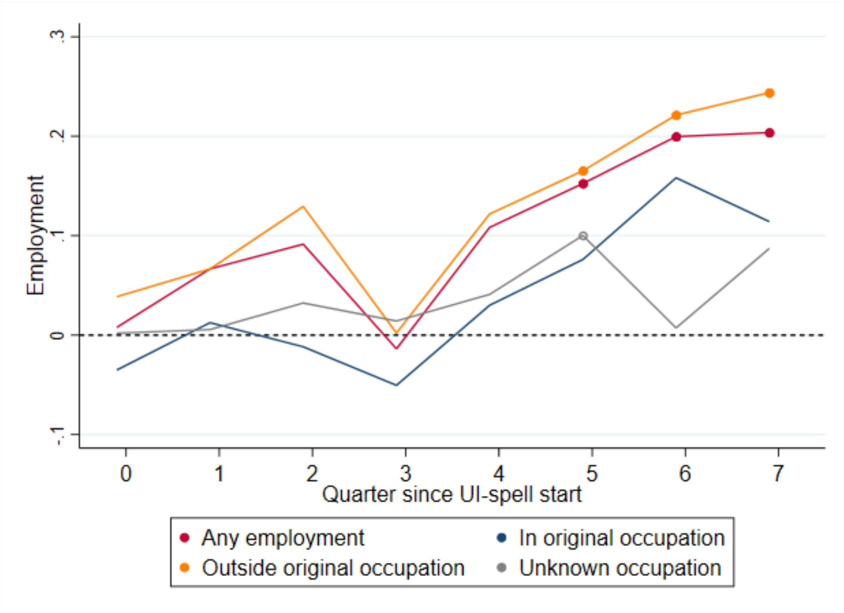
A core motivation of reskilling programs is to enhance the occupational mobility of job seekers.[23] Figure 6 evaluates this goal by decomposing the total employment effects of classroom training into employment inside and outside job seekers' previous occupations. In particular, for each job seeker, we identify the occupation that the job seeker most frequently held prior to job loss, which we denote as her *original occupation*. We then decompose total employment in a given period into three mutually exclusive categories: employment in the original occupation, outside the original occupation, and in an unknown occupation.[24] The figure is based on three-digit ISCO08 occupational codes, but Appendix Figure A.6 shows our results are qualitatively robust to using more or less disaggregated occupational codes.

Figure 6 shows the gains in employment from classroom training are primarily driven by employment *outside* job seekers' original occupations. This observation suggests the *post-program* effects of classroom training, which are dominant in the long run, help job seekers by enhancing their occupational mobility.

[23] A stated goal of the vocational training program in Denmark is to "solve labor market restructuring and adaptation problems in accordance with the needs on the labor market in a short and a long term perspective." (Danish Ministry of Education, 2021).

[24] We categorize the individual as employed in an unknown occupation if she has positive working hours but a missing occupation code.

Figure 6: Occupational Mobility



Note: Effect of assignment to classroom training on any employment (red), employment in the original occupation (blue), outside the original occupation (orange), or in an unknown occupation (gray). Occupations are based on 3-digit isco08 codes. The effects are obtained by IV-estimation of regressions that include job-center-unit-year fixed effects. Standard errors are two-way clustered on the predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level. Note employment in the three occupation subcategories (in original, outside original or unknown) are mutually exclusive and sum to employment in any occupation. Due to fixed effects (singletons), the effects of classroom training on employment in each occupation subcategory do not necessarily sum to the effect on employment in any occupation.

8 Heterogeneity

This section studies heterogeneity in the effects of training. In Section 8.1, we first investigate the effect by *program* types, distinguishing between job training and classroom training. In Section 8.2, we explore whether classroom training is more beneficial to job seekers *exposed to offshoring*. In Section 8.3, we explore heterogeneity in treatment effects across job seekers' unobserved resistance to training by estimation of *marginal treatment effects* (MTE). Finally, we evaluate whether classroom training can close the employment gap between job seekers with different exposures to offshoring.

8.1 Classroom Training vs. Job Training

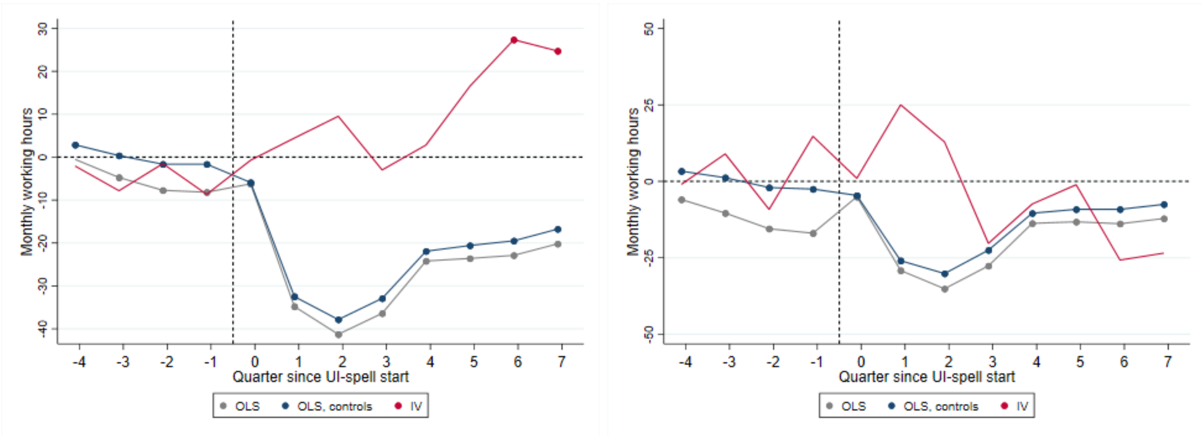
Figure 7 plots the effect of assignment to classroom training and job training on average monthly working hours in a given quarter relative to job loss.

Panel (a) repeats our main Figure 3, showing classroom training increases employment by

25 hours per month two years after initial job loss. Panel (b) shows job training causes a large yet short-lived increase in employment in quarters one and two after the initial job loss. Although not statistically significant, these short-lived effects are economically significant. However, the employment boost from job training quickly dies out and becomes negative two years after job loss. Appendix Figure A.7 shows the conclusions remain when we consider extensive-margin employment instead of working hours.

Figure 7: Employment Effects of Assignment to Training

(a) Classroom Training (b) Job Training



Notes: Effect of assignment to classroom training (Panel A) and job training (Panel B) on average monthly working hours in a given quarter relative to job loss. The gray and blue lines represent the effect obtained with a simple OLS regression and OLS that controls for predetermined job seeker characteristics. These include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). The red line represents the effect obtained by IV-estimation. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

Overall, our IV estimates indicate assignment to classroom training is *more* effective in raising employment rates than job training. This finding challenges the conventional wisdom that job training is more effective, see McCall et al. (2016). In Section 8.3, we investigate the heterogeneity between job seekers in their benefits of job and classroom training.

Finally, a natural question is whether complementarities exist between classroom training and job training. For example, classroom training could be even more effective if the job seeker also gains workplace experience from job training. Unfortunately, we do not have the statistical power to estimate such interaction effects between assignment to classroom and job training.[25,26]

[25]Table 2 shows the number of job seekers assigned to both training programs.

[26]To estimate interaction effects, we would need an additional first-stage equation with an interaction of the

8.2 Exposure to Offshoring

Training programs are often motivated by skill mismatches that arise from globalization (Hyman, 2018). For example, offshoring of production could make some job seeker skills obsolete in the local labor market. We now investigate whether job seekers with higher exposure to offshoring benefit more from classroom training.

We adopt the offshorability index from Autor and Dorn (2013) and characterize all job seekers according to the offshorability of their most frequently held occupation before job loss. The offshorability index is based on the task content of occupations and, as stated by Autor and Dorn (2013), it "captures the degree to which an occupation requires either direct interpersonal interaction or proximity to a specific work location." The index is defined such that occupations requiring *less* interpersonal interaction and proximity to the work place location are *more* exposed to offshoring.[27]

For simplicity, we distinguish between occupations with “high” and “low” risk of offshorability, as measured by the 75th percentile in the job seeker distribution. Namely, we define all job seekers above the 75th percentile to be high-risk. Using this definition, most high-risk job seekers in our sample were employed as *cleaners* and *office clerks* prior to job loss. For comparison, most low-risk job seekers were employed as *shop sales assistants* or *child-care workers*. See Online Appendix Table OA2 for additional examples.

Appendix Figure A.8 plots the employment of job seekers by their exposure to offshoring. The figure shows job seekers at high risk of offshoring have depressed employment rates after job loss compared with job seekers at low risk. Strikingly, this employment gap persists four years after the initial job loss.

Figure 8 plots the employment effects of assignment to classroom training (Panel (a)) and job training (Panel (b)) by job seeker exposure to offshoring. Panel (a) shows job seekers at high risk of offshoring have much larger gains from classroom training.[28] Two years after job loss, high-risk job seekers gain 50 to 75 hours of employment per month from assignment to

indicators (leniencies) for classroom and job training on the LHS (RHS):

$$D_{1i}D_{2i} = \delta_{j(i)} + \delta_1 Z_{1i} + \delta_2 Z_{2i} + \delta_3 Z_{1i} Z_{2i}.$$

When estimating this equation, the coefficient on $Z_1 Z_2$ is insignificant (F stat of 0.406).

[27] Appendix OA2.6 describes how we use the O*NET database to construct an occupation-based measure of exposure to offshorability.

[28] Appendix Figure A.9 shows the conclusions are similar when considering extensive-margin employment.

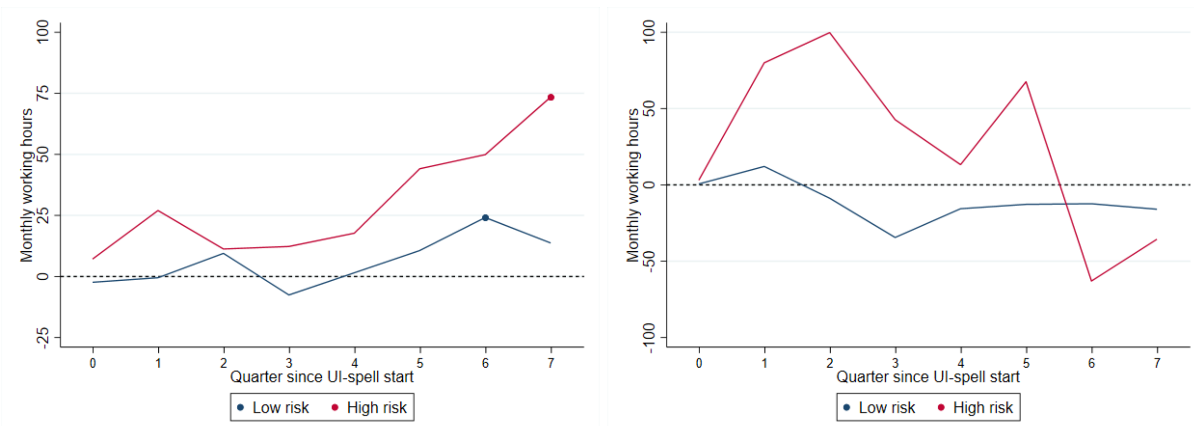
classroom training.[29]

Why do job seekers at high risk of offshoring gain more from classroom training? In Section 7.1, we showed that the long-run gains from classroom training are driven by the post-program effects as job seekers complete the programs. Furthermore, in Section 7.2, we showed classroom training enhances occupational mobility. These pieces of evidence suggest classroom training helps job seekers reskill toward new occupations. Occupational mobility may be particularly helpful for job seekers exposed to offshoring, who may face obstacles reattaching to the labor market with their current skill set and may be hit by future offshoring shocks if they stay in their original occupations (Humlum and Munch, 2019).

Figure 8: Employment Effects of Classroom Training by Exposure to Offshoring

(a) Classroom Training

(b) Job Training



Note: IV estimates of the effect of assignment to classroom training (Panel (a)) and job training (Panel (b)) on average monthly working hours in a given quarter relative to job loss, for job seekers at high and low risk of offshoring. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

Panel (b) shows the effect of assignment to job training by job seeker exposure to offshoring. Although statistically insignificant, the estimates suggest high-risk job seekers have less robust long-term gains from job training compared with classroom training. At best, job training has a null employment effect for high-risk job seekers two years after initial job loss. At worst, job training *decreases* employment by 33 hours per month for these job seekers. The negative effects for high-risk job seekers could arise from the job training reinforcing the ties to their original occupations where offshoring is likely to occur again (Hummels et al., 2012).

[29] Appendix Tables B.3 and B.4 summarize the effects by year.

8.3 Marginal Treatment Effects

In this section, we investigate the selection patterns into classroom and job training by estimating marginal treatment effects (MTEs). Building on the theoretical framework in Section 4 (Appendix C), MTEs correspond to the average treatment effect (ATE) among job seekers with a particular resistance to training U. To estimate the ATE in the broader population, we follow Mogstad et al. (2018) and impose shape restrictions on the MTE functions. In particular, we assume an additively separable and linear specification in the quantiles of resistances to training. Appendix OA1.3 details our estimation approach.

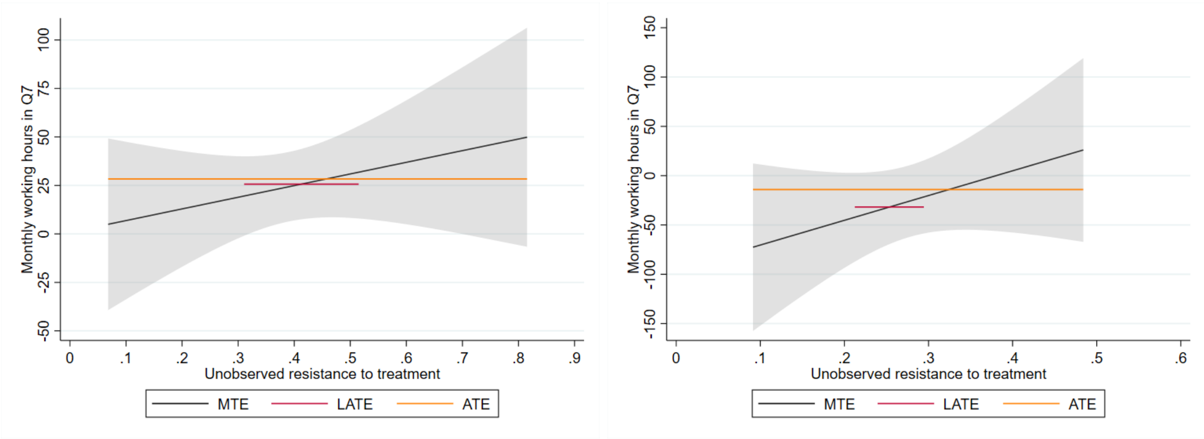
8.3.1 MTE, LATE, and ATE

Figure 9 plots the MTE functions for classroom and job training. Although imprecisely estimated, the MTE functions reveal important insights.

First, the MTE of classroom training (Panel (a)) is positive for all job seekers, varying between 5 and 50 additional working hours per month for job seekers with the lowest and highest resistance to training. Because compliers represent the center of the resistance distribution, our LATE estimate for instrument compliers are informative about the effect of classroom training in the broader population (ATE).

Second, the MTE of job training (Panel (b)) increases steeply with job seekers' resistance to treatment, varying from an employment *decrease* of 73 working hours per month to an *increase* of 26 working hours per month. Put differently, job seekers with the *lowest* gains from job training are the ones who have a *lower* resistance to treatment. For example, finding a supported job for a job seeker who already has good job opportunities may be easier. The stark effect heterogeneity implies our LATE estimates are more negative than the average effects of job training in the broader population (ATE). These differences could help reconcile our findings with estimates in the literature, showing more positive effects for job-training interventions (McCall et al., 2016).

Figure 9: Marginal Treatment Effects



(a) Classroom Training

(b) Job Training

Note: The panel plots the MTE, LATE and ATE on monthly working hours in quarter 7 relative to job loss. Panel A plots the effect of assignment to classroom training and panel B, the effect of assignment to job training. These estimates are based on job seekers within the common support of the relevant propensity score (see panels A and B in Appendix Figure A.10). Note the x-axis in panel A and B of this figure differ slightly from that in Appendix Figure A.10, because the propensity scores are re-estimated based on the common support sample. The shaded area represents 90% confidence intervals on the MTE function. Note these do not take account of generated regressors: the propensity score is estimated and used as a regressor in the second-stage equation.

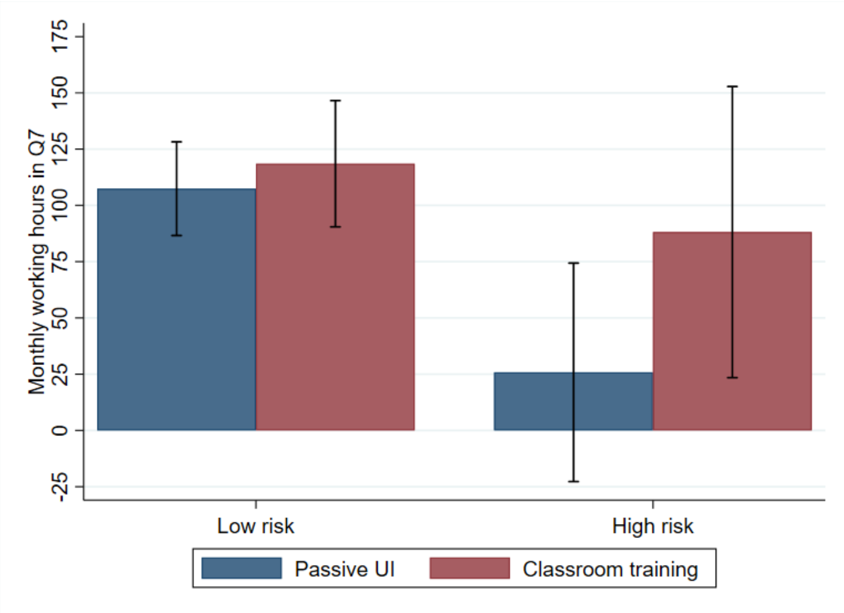
8.3.2 Policy Experiment

In Section 8.2, we found a persistent employment gap following job loss between job seekers at high and low risk of offshoring. In this section, we evaluate how much of the employment gap can be closed by targeting classroom training to the high-risk job seekers. To evaluate this counterfactual, we leverage the MTE functions (estimated separately by risk groups) to evaluate employment gaps under different training scenarios.[30]

Figure 10 plots employment (two years after initial job loss) by risk group with and without classroom training. The plot delivers three takeaways. First, high-risk job seekers suffer depressed employment rates relative to low-risk job seekers in the scenario without training (blue bars). Second, providing all job seekers with classroom training (red bars) closes the employment gap between high- and low-risk job seekers. Third, because the employment prospects of low-risk job seekers are unaffected by classroom training, a cost-effective to close the employment gap is to redistribute classroom training from low-risk to high-risk job seekers.

[30]Appendix Figure A.11 shows that, across risk groups, the MTE, LATE, and ATE estimates of the effect of classroom training are very similar.

Figure 10: Policy Counterfactuals



Note: Counterfactual employment rate in quarter 7 relative to initial job loss, estimated separately for low- and high-risk job seekers. Black lines represents 90% confidence intervals. Standard errors do not take into account the fact that the propensity score is estimated and then used as a regressor.

9 Conclusion

This paper investigates the employment effects of assigning unemployed job seekers to training programs. Using caseworker-tendency instruments, we find large employment effects of assignment to classroom training: two years after job loss, the job seekers assigned to a classroom training program have 25 percent higher employment rates. By contrast, we do not find robust employment effects of job-training programs.

Interestingly, we find a stark difference between our IV estimates and OLS estimates that assume "selection on observables" only. The latter approach is widely used in the literature and suggests classroom training is associated with detrimental employment effects. The differences between OLS and IV highlight the importance of controlling for unobserved job-seeker characteristics.

Studying the dynamics of training programs, we show the large negative lock-in effects suggested by OLS, to a large extent, are driven by job seekers with *worse job prospects* opting into training. Further, we show the long-run employment effects detected by our IV are driven by post-program effects rather than threat effects. That is, the increase in employment is driven by job seekers who actually complete the training programs. We further show classroom training enhances employment *outside* the job seekers' original occupation, which suggests that participation in classroom training helps job seekers reskill.

Finally, we study heterogeneity in the causal effects of training programs across job seeker exposure to offshoring. We show job seekers at high risk of offshoring, who face depressed employment prospects after job loss, have larger gains from classroom training.

We estimate MTEs and use them to recover the ATE of training and evaluate counterfactual policies. We show our LATE estimate for classroom training can be extrapolated to the full population of job seekers and that classroom training can close the employment gap of job seekers exposed to offshoring.

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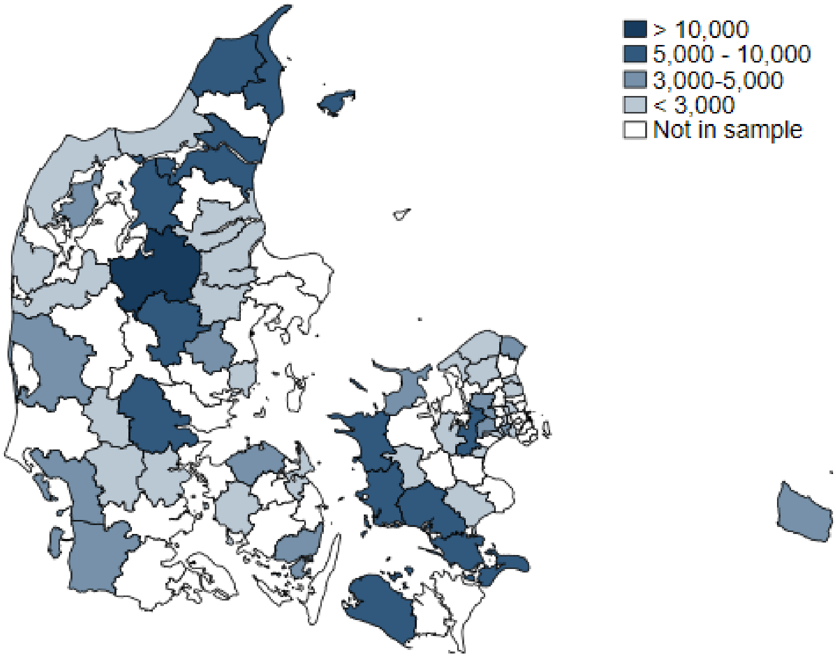
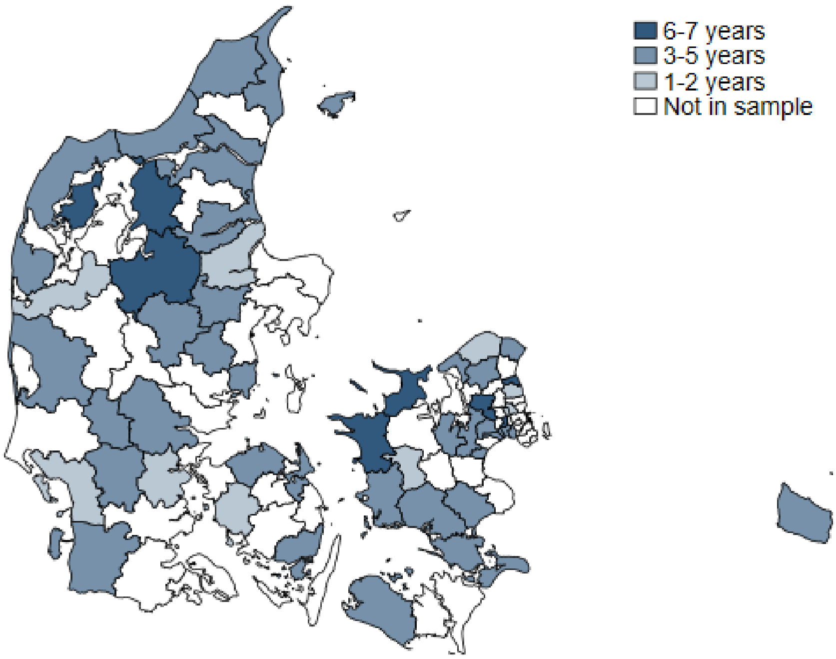
A Appendix Figures

Figure A.1: Analysis Sample Geographical Coverage



(a) Number of job seekers

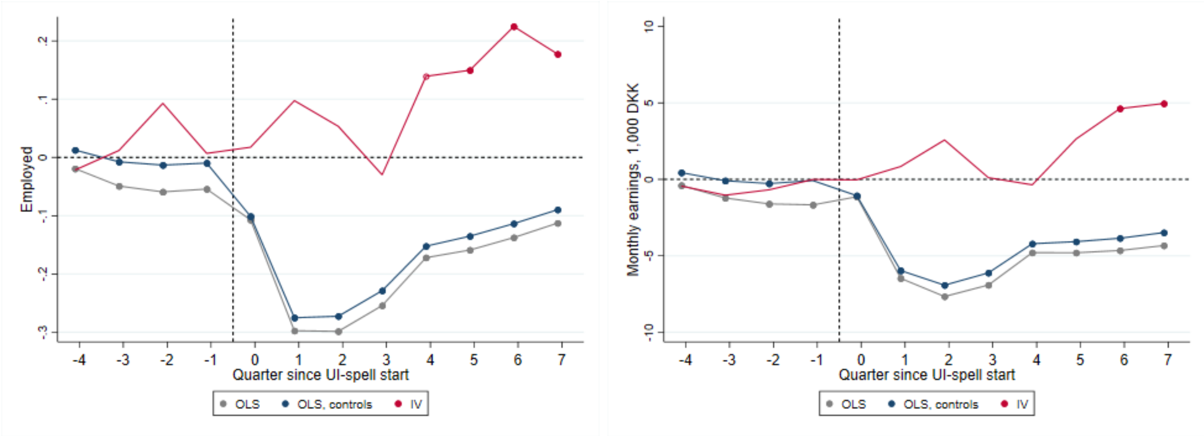


(b) Number of years

Note: The figure breaks the analysis sample by job centers in Denmark. Panel A shows the number of job seekers from a given job center. Panel B shows the total number of years that a given job center is part of our analysis sample. With four exceptions, a job center corresponds to a municipality (Esbjerg-Fanø, Ishøj-Vallensbæk, Læsø-Frederikshavn and Dragør-Tårnby).

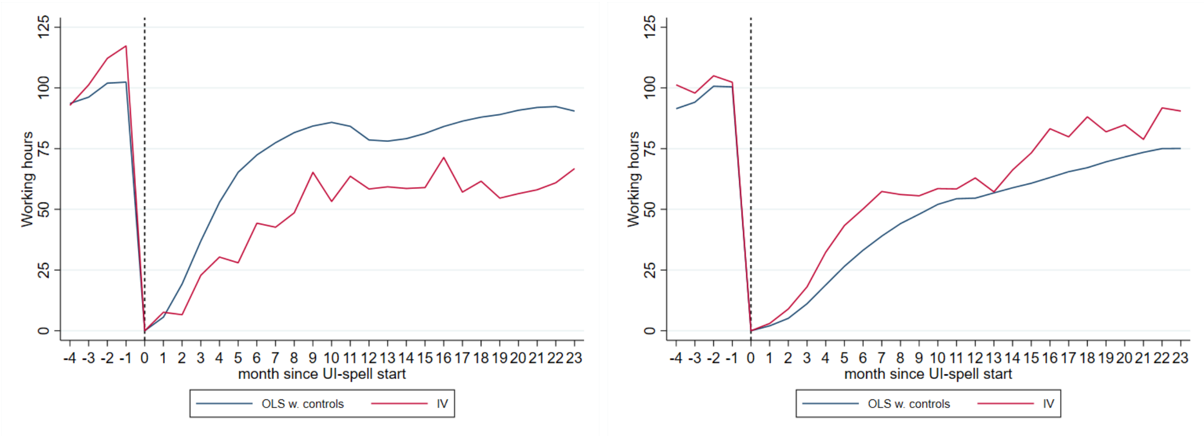
Figure A.2: Effect of Assignment to Classroom Training

(a) Extensive-Margin Employment (b) Earnings (1,000 DKK)



Note: Effect of assignment to classroom training on extensive margin employment (Panel (a)) and monthly earnings (Panel (b)) in a given quarter relative to job loss. The gray and blue lines represent the effect obtained with a simple OLS regression and OLS that controls for job seeker predetermined characteristics. These controls include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). The red line represents the effect obtained by IV-estimation. All regressions include fully interacted job-center unit and year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

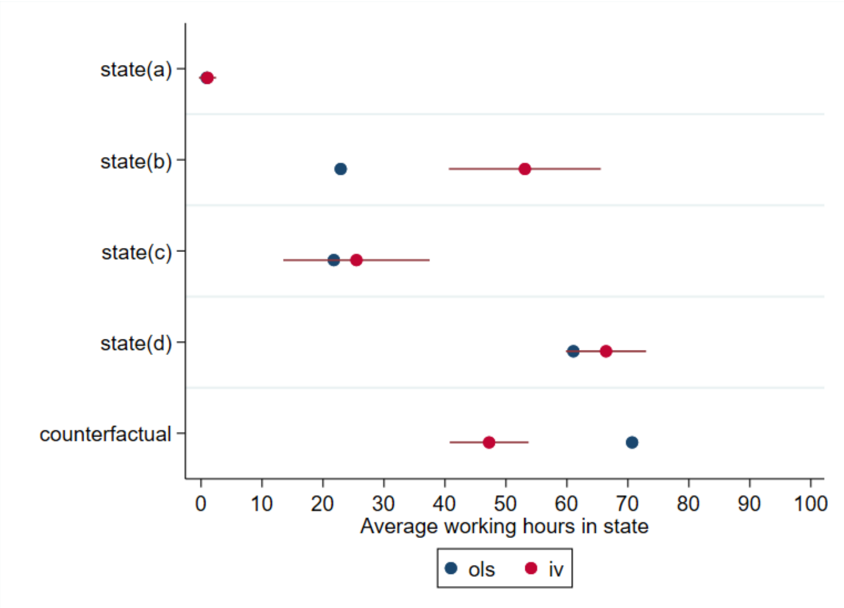
Figure A.3: Average Working Hours



(a) Not assigned to training (b) Assigned to training

Note: The panels plot average monthly working hours by training assignment status for the individuals in the OLS and IV population (blue vs. red). All regressions include job-center-unit-year fixed effects. The OLS regressions also include a rich set of predetermined job seeker characteristics: socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08).

Figure A.4: Average Employment by Training State

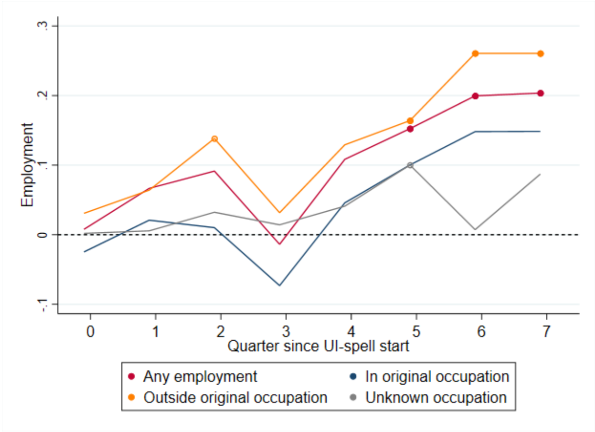


Notes: Average monthly working hours in months 0-23 following job loss and 95% confidence intervals for a given training state. Standard errors are constructed based on 100 bootstrap repetitions (see Appendix OA3.1).

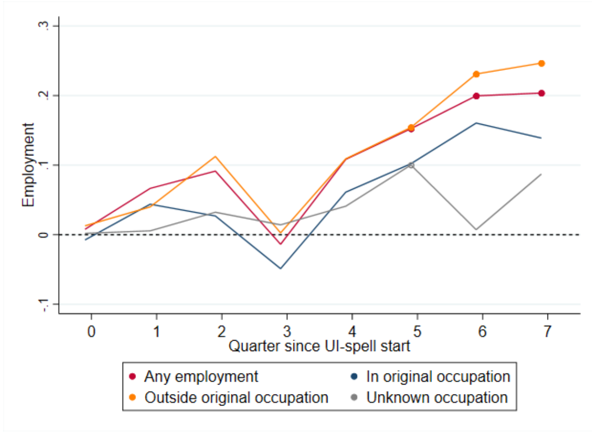
Figure A.5: Threat, Lock-in, and Post-program Effects by Quarter after Job Loss

Note: The IV placebo, threat, lock-in, and post-program effects of assignment to classroom training on working hours in a given quarter relative to job loss. The contributions by quarter correspond to the sum of the monthly contributions plotted in Figure 5.

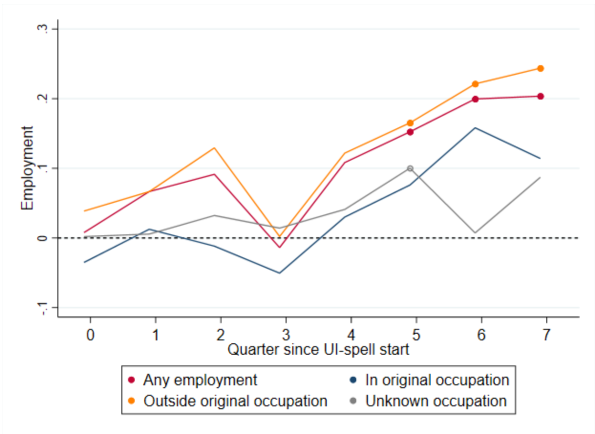
Figure A.6: Occupational Mobility



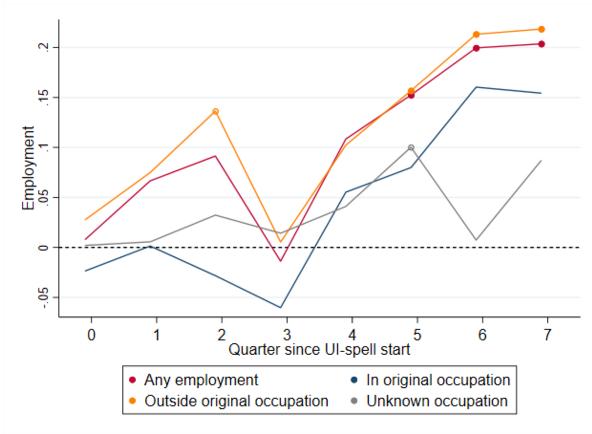
(a) 1-digit isco08



(b) 2-digit isco08



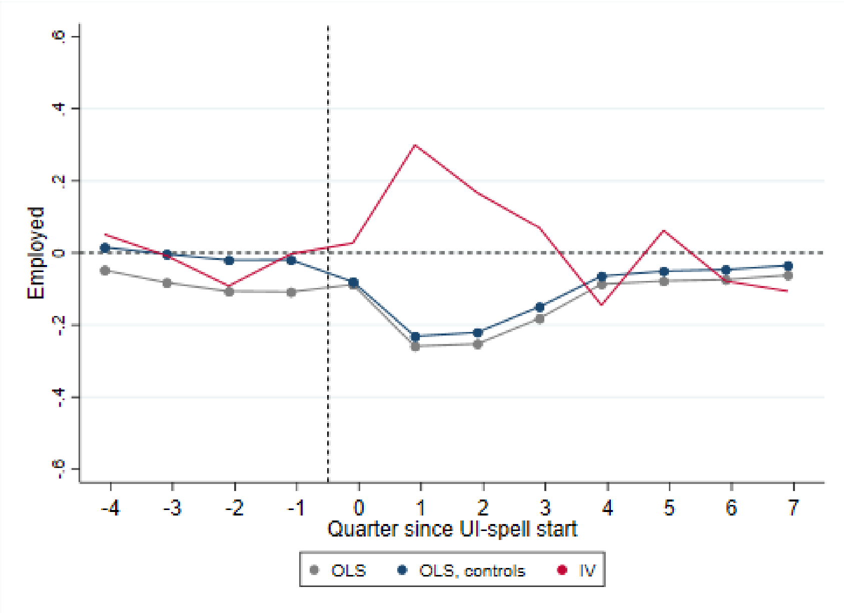
(c) 3-digit isco08



(d) 4-digit isco08

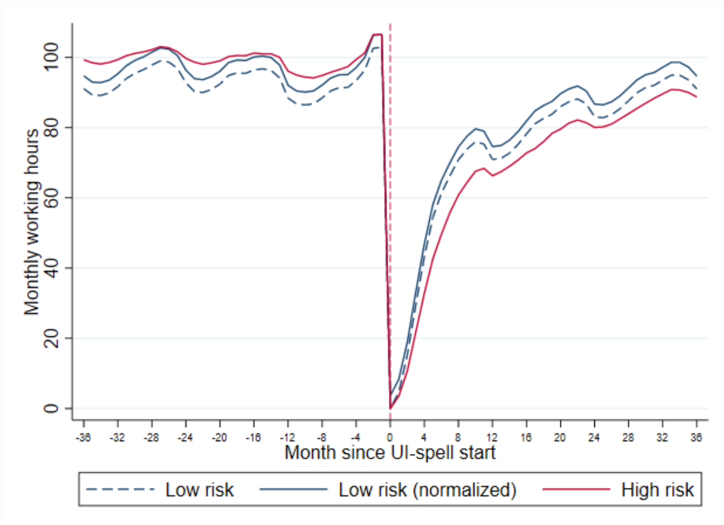
Note: Effect of assignment to classroom training on any employment (red), employment in the original occupation (blue), outside the original occupation (orange) or in an unknown occupation (gray). The effects are obtained by IV-estimation of regressions that include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

Figure A.7: Effect of Job Training on Extensive-Margin Employment



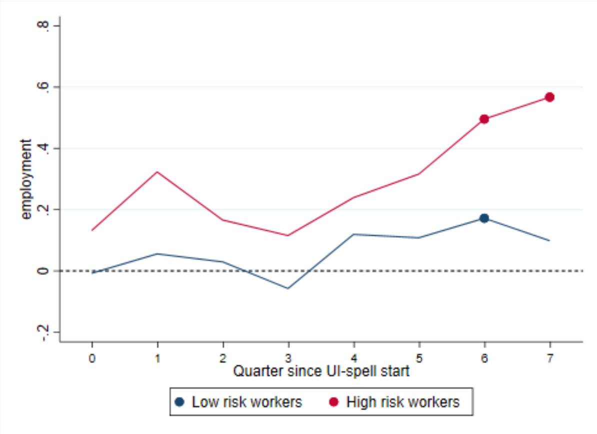
Note: Effect of assignment to job training on extensive-margin employment in a given quarter relative to job loss. The gray and blue lines represent the effect obtained with a simple OLS regression and OLS that controls for job-seeker predetermined characteristics. These controls include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). The red line represents the effect obtained by IV-estimation. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

Figure A.8: Employment Prospects by Exposure to Offshoring

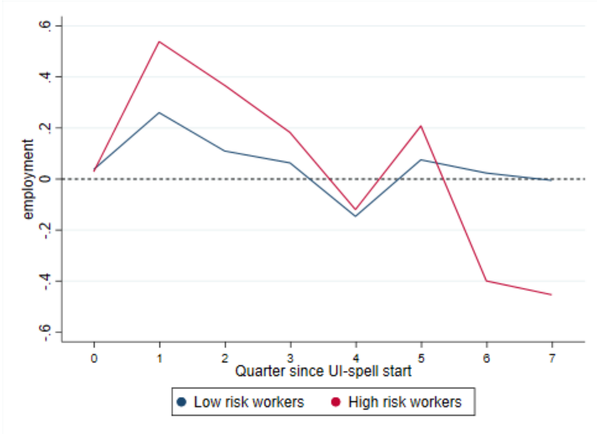


Note: Employment by job seeker exposure to offshoring. The offshorability index is based on Autor and Dorn (2013), and we use the 75th percentile in the job-seeker distribution to distinguish between job seekers at high and low risk. To facilitate comparison, we normalize the employment level for the low-risk job seekers to match the employment of high-risk job seekers in the month before job loss.

Figure A.9: Effects of Training by Exposure to Offshoring
Extensive-Margin Employment



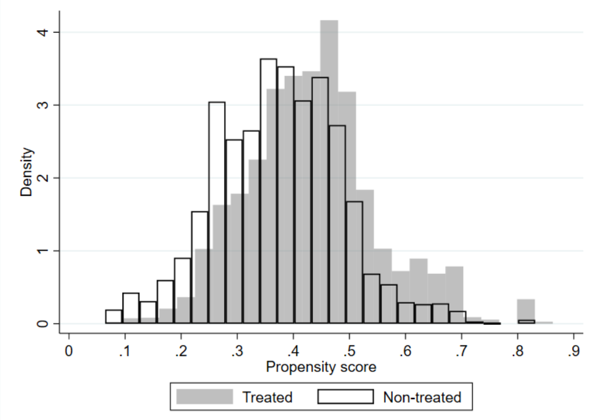
(a) Classroom Training



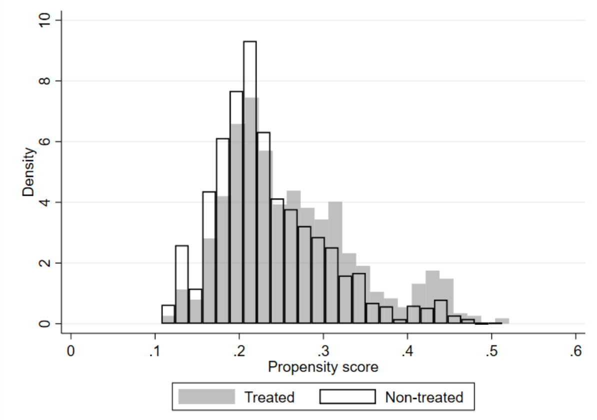
(b) Job Training

Note: IV estimates of the effect of assignment to classroom training (Panel (a)) and job training (Panel (b)) on extensive-margin employment in a given quarter relative to job loss, for job seekers at high and low risk of offshoring. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

Figure A.10: Common Support



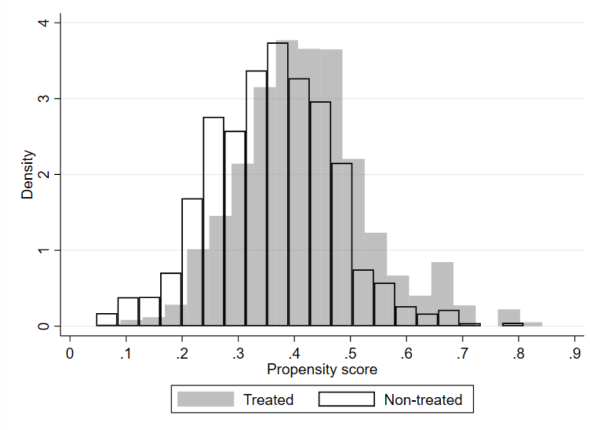
(a) Classroom Training



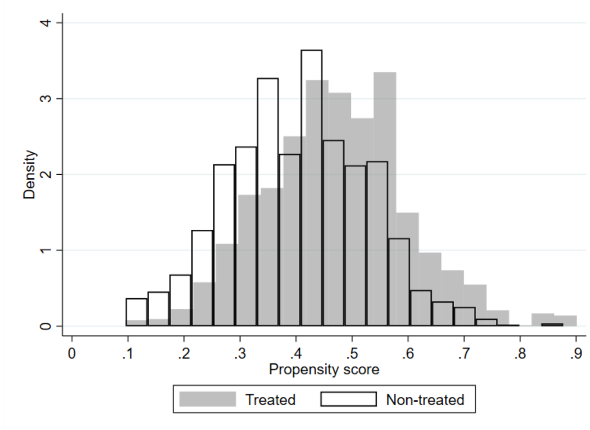
(b) Job Training

Note: The panels plot the common support of the propensity score for treated and non-treated job seekers. Panel (a) represents the propensity score for classroom training and Panel (b) represents the propensity score for job training. The propensity scores are obtained by estimating and saving the predicted values from the first-stage equations (2).

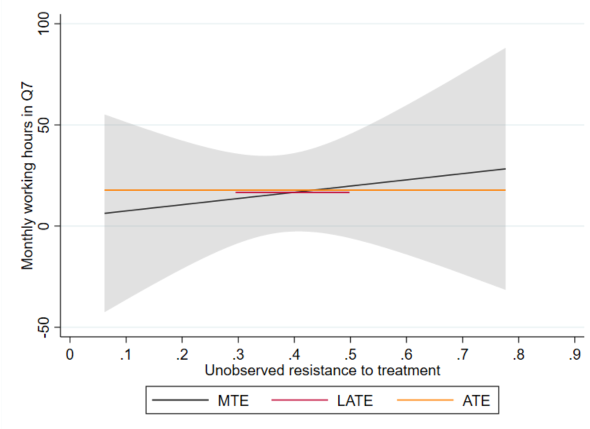
Figure A.11: Common Support and Marginal Treatment Effect of Classroom Training by Offshorability Risk



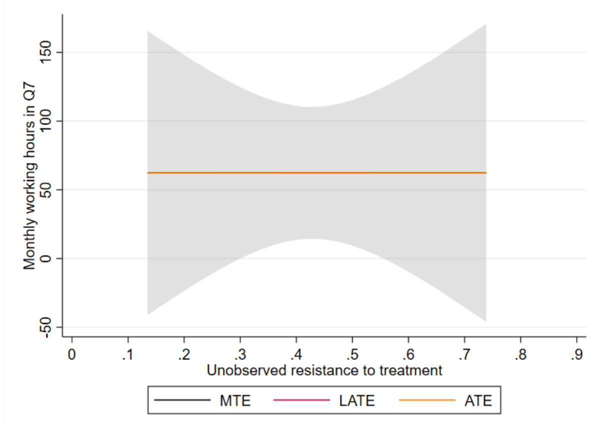
(a) Common support, low risk



(b) Common support, high risk



(c) MTE, low risk



(d) MTE, high risk

Note: MTE estimations conducted separately for job seekers at low and high risk of offshoring. Panels (a) - (b) plot the common support of the propensity score for treated and non-treated job seekers. Panel (a) represents the propensity score for classroom training and Panel (b) represents the propensity score for job training. The propensity scores are obtained by estimating and saving the predicted values from the first-stage equations (2). Panels (c) - (d) plot the MTE, LATE, and ATE on monthly working hours in quarter 7 relative to job loss. These estimates are based on the job seekers within the common support of the relevant propensity score. The shaded area represents 90% confidence intervals on the MTE function. Note these do not take into account generated regressors: the propensity score is (re-estimated based on the common-support sample and) used as a regressor in the second-stage equation.

B Appendix Tables

Table B.1: Analysis Sample Restrictions

	#UI-spells	#Job seekers	#Job centers	#Caseworkers realized	#Caseworkers predicted	UI-spell (weeks)	Compliance (pct)
UI-spells from 2012-2018	**2,200,778**	**940,965**	**94**	.	.	**20**	.
... with at least one meeting	**1,111,334**	**700,530**	**94**	.	.	**59**	.
- Merge caseworker links onto	934,197	627,542	94	7,912	1,982	35	42
- Only j-u-y's using birthday allocation	213,679	160,160	53	2,467	733	33	51
- Drop non-western job seekers	206,267	154,408	53	2,420	733	33	51
- Min. two caseworkers per j-u-y	198,981	148,959	53	2,391	721	33	51
- Caseload size $\geq$ 50	196,322	147,155	53	2,382	622	33	51
Analysis Sample	**196,322**	**147,155**	**53**	**2,382**	**622**	**33**	**51**

Note: Number of units (UI-spells, job seekers, job centers, realized and predicted caseworkers) retained after each sample-selection step along with sample descriptives (average UI-spell length and the compliance between realized and predicted caseworker). Row (1) reports the statistics for all UI spells initiated from 2012-2018 in Denmark. Row (2) restricts to UI spells that had at least one caseworker meeting. Row (3) restricts to UI-spells that appears in the linked job-seeker-caseworker data, i.e. the UI-spells that have information about realized and predicted caseworker. Row (4) restricts to job seekers with a non-western origin. Row (5) restricts to job-center-unit-years with minimum two (predicted) caseworkers. Row (6) restricts to (predicted) caseworkers that were assigned at least 50 job seekers. Row (7) is identical to row (6) and summarizes the final analysis sample.

Table B.2: Summary of Caseworker-Tendency Instruments

	count	mean	sd	Pseudo percentiles 1	10	25	50	75	90	99
Classroom-Training Instrument										
Z_c	196,322	0.40	0.12	0.16	0.25	0.32	0.39	0.46	0.54	0.72
Z_c, residualized by κ_{juy}	196,322	0.40	0.04	0.29	0.34	0.37	0.40	0.42	0.45	0.51
Z_c, residualized by κ_{juy}, Z_j	196,322	0.40	0.04	0.29	0.34	0.37	0.40	0.42	0.45	0.51
Job-Training Instrument										
Z_j	196,322	0.24	0.07	0.12	0.17	0.19	0.23	0.28	0.34	0.46
Z_j, residualized by κ_{juy}	196,322	0.24	0.03	0.16	0.21	0.22	0.24	0.26	0.28	0.33
Z_j, residualized by κ_{juy}, Z_c	196,322	0.24	0.03	0.16	0.21	0.22	0.24	0.26	0.28	0.33

Note: Due to data confidentiality, the table reports pseudo-percentiles (each percentile is an average of the 5 job seekers closest to the actual percentile). κ_{juy} is shorthand for fully interacted job-center-unit-year fixed effects. Z_c and Z_j are shorthand for the classroom- and job-training instruments.

Table B.3: Effect of ALMP by Exposure to Offshoring
Monthly Working Hours

	Monthly Working hours		
	(1)	(2)	(3)
	year 1	year 2	year 3
Low risk			
Classroom Training	10.869	1.378	6.277
	(9.827)	(10.568)	(12.300)
Job Training	-12.308	-16.751	21.148
	(20.003)	(22.318)	(24.014)
High risk			
Classroom Training	34.315	91.002**	59.315
	(26.458)	(45.335)	(71.598)
Job Training	-9.883	-95.025	-218.272
	(45.000)	(87.203)	(142.803)
Obs low risk	148,078	137,064	120,928
Obs high risk	44,752	41,148	35,709

Note: IV estimates of the effect of assignment to classroom training and job training on average monthly working hours in a given quarter relative to job loss. Note only job seekers who have employment information in all four quarters of the year are included in the estimations. Estimations are run separately for job seekers at high and low risk of offshoring. All regressions include fully interacted job-center unit and year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level.

Table B.4: Effect of ALMP by Exposure to Offshoring
Extensive-Margin Employment

	Extensive Margin Employment		
	(1)	(2)	(3)
	year 1	year 2	year 3
Low risk			
Classroom Training	0.042	0.000	0.085
	(0.063)	(0.052)	(0.069)
Job Training	0.046	-0.115	0.075
	(0.131)	(0.116)	(0.160)
High risk			
Classroom Training	0.406**	0.248	0.194
	(0.204)	(0.159)	(0.324)
Job Training	-0.279	-0.277	-1.048
	(0.340)	(0.273)	(0.680)
Obs low risk	148,078	137,064	120,928
Obs high risk	44,752	41,148	35,709

Note: IV estimates of the effect of assignment to classroom training and job training on extensive-margin employment in a given quarter relative to job loss. Note only job seekers who have employment information in all four quarters of the year are included in the estimations. Estimations are run separately for job seekers at high and low risk of offshoring. All regressions include fully interacted job-center unit and year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level.

C Identification Strategy

In this section, we present a generalized Roy model of the assignment of job seekers to training programs. We use the model to discuss the bias in estimators that assume "selection on observables only" and derive our IV strategy based on caseworker tendencies. Our model deviates from the canonical judge IV setup in two aspects. First, caseworkers may assign job seekers to *multiple* training programs. Second, job seekers are allocated to caseworkers based on an *observed rule* (day of birth).

C.1 Setup

A job seeker i may be assigned to classroom training $D_1 \in \{0, 1\}$ and job training $D_2 \in \{0, 1\}$. Her potential employment outcomes are

$$Y_i(D_i) = \beta_{0i} + \beta_{1i}D_{1i} + \beta_{2i}D_{2i}. \tag{9}$$

Job seekers are allocated to caseworkers who assign training programs. Let $c(b)$ denote the default rule that allocates job seekers with day-of-birth $b \in \{1, 31\}$ to a caseworker c.

Caseworkers differ in their preferences for the programs (V_1, V_2) but rank individual job seekers similarly in their resistance to participate in each program (U_1, U_2). For example, a job seeker with a pending job offer may be more resistant to start training than a job seeker with no immediate job prospects.

Let c_i denote the caseworker assigned to job seeker i. The job seeker is assigned to training program $k \in \{1, 2\}$ if

$$D_{ki} = \mathbf{1}[V_{kc_i} \geq U_{ki}], \tag{10}$$

where we normalize the marginal distributions of the resistances to be uniform, $U_{ki} \sim U[0, 1]$.

We assume the preferences of a job seeker's *day-of-birth-predicted* caseworker are independent of the job-seeker's training resistances and potential outcomes:

$$V_{c(b_i)} \perp\!\!\!\perp (U_i, \beta_i) \tag{11}$$

A sufficient condition for Equation (11) is that job seekers' training and employment potential are unrelated to their day of birth. Note Equation (11) allows for both differences in general ability of caseworkers ("value added") and match effects between job seekers and caseworkers, as long as these are orthogonal to caseworker preferences for training.[31]

C.2 Selection into Training

The selection patterns into training programs are governed by how the job seekers' resistance to training U_i correlate with their employment potential β_i in Equation (9). For example, job seekers with worse job opportunities (low β_{i0}) may be less resistant to training (low U_{ik}). Furthermore, controlling for observables of the job seekers may not necessarily eliminate this selection bias. For example, two job seekers with identical work histories might face different job *prospects* that are not recorded in our administrative data. For example, one of the job seekers could have a pending job offer. If caseworkers learn about these *latent* job prospects during the meetings at the job center, controlling for observables will not alleviate the selection bias. These concerns motivate developing our identification strategy based on caseworker tendencies that control for unobservables of the job seekers.

C.3 Caseworker-Tendency Instruments

We instrument the assignment of job seeker i to training program k with the tendency of her *day-of-birth-predicted* caseworker to assign program k. For job seeker i, we measure the program tendencies using the assignment rates among other job seekers with the same day-of-birth predicted caseworker:

$$Z_{ki} = \mathbb{E}\left[D_{kj}|c(b_j) = c(b_i), j \neq i\right]. \tag{12}$$

Because job seekers are quasi-randomly allocated to caseworkers (Equation (11)), caseworkers' preferences are revealed by her observed k-tendency as she handles a large number of cases:

$$Z_{ki} \to \mathbb{P}(V_{kc(b_i)} \geq U_{kj}) = V_{kc(b_i)} \quad \text{as} \quad N_{c(b_i)} \to \infty. \tag{13}$$

We now show the caseworker-tendency instruments satisfy the *exogeneity*, *exclusion*, and *monotonicity* conditions for the identification of local average treatment effects (LATE) of training programs (Imbens and Angrist, 1994). To ease the exposition, we assume job centers perfectly comply with the day-of-birth rule to allocate job seekers to caseworkers, $c_i = c(b_i)$. In

[31] For example, if caseworkers differ in their value added β_c, we would redefine $\beta_{0i} = \beta_{0i} + \beta_{c_i}$.

Appendix OA1.2, we extend our identification results to allow for non-compliance with the allocation rule.

Exogeneity and Exclusion

Because caseworker training tendencies recover preferences (V_1, V_2), it follows from Equation (11) that they satisfy the independence and exclusion restrictions:

$$Z_i \perp\!\!\!\perp \beta_i. \tag{14}$$

We purposely exclude the job seeker from her own training-tendency instruments to ensure this exogeneity restriction holds.

Extended Monotonicity

In the canonical binary-treatment case, monotonicity requires that the instrument shift all job seekers toward or away from the treatment in consideration. However, because job seekers face *multiple* training options, identification in our setting requires an *extended* monotonicity assumption about how instruments affect multiple training programs.

Using Equation (13), we can restate Equation (10) in terms of caseworker tendencies:

$$D_{ki} = \mathbf{1}[Z_{ki} \geq U_{ki}]. \tag{15}$$

Equation (15) implies the assignment to classroom training and job training are two separate decisions. In particular, the assignment of job seeker i to training k depends solely on the k-tendency of her caseworker. Hence, comparing two otherwise similar caseworkers, a more k-inclined caseworker will shift all job seekers toward training program k but not alter the participation in other programs l:

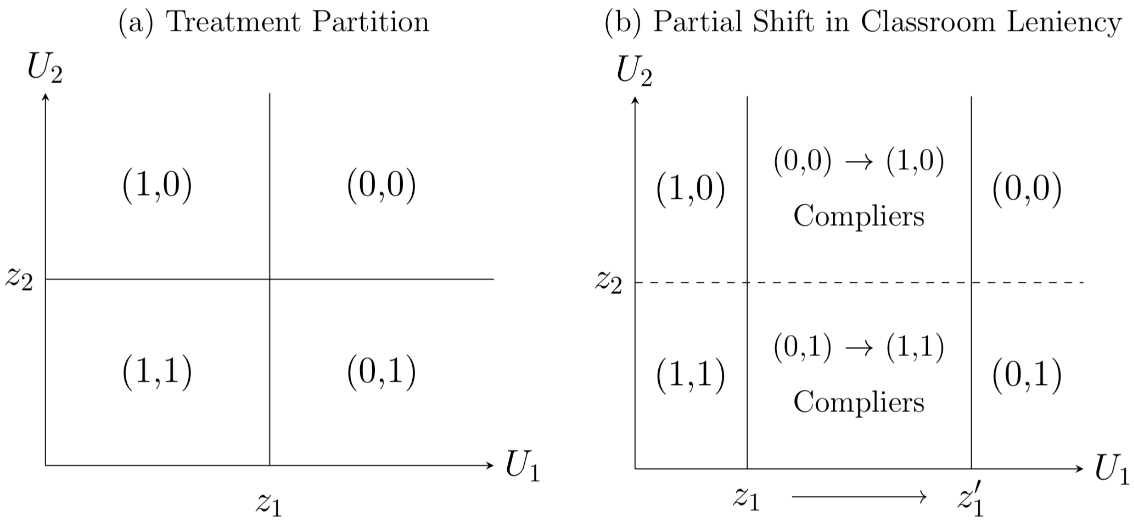
$$z'_k > z_k \implies D_{ki}(z'_k, z_l) \geq D_{ki}(z_k, z_l) \tag{16}$$

$$D_{li}(z'_k, z_l) = D_{li}(z_k, z_l), \quad k \neq l, \forall i. \tag{17}$$

The property in Equations (16)-(17) is labeled *extended monotonicity* in the literature and plays a key role in IV analysis with multiple treatments (Behaghel et al., 2013; Lee and Salanié, 2018, 2020; Bhuller and Sigstad, 2022). In particular, when evaluating treatment k, extended monotonicity allows us to collapse the analysis to the canonical single-treatment case by comparing caseworkers with similar tendencies for the other training programs l.[32]

Figure C.1 illustrates the concept of extended monotonicity. Panel (a) first illustrates how the assignment to training programs depends on job seeker resistance U and caseworker preference thresholds Z. The horizontal and vertical axes represent job seekers' resistances to classroom and job training, respectively. The two lines represent the caseworker's preference for each training program. A job seeker is assigned to training k if her resistance is below the caseworker threshold, $U_{ki} \leq Z_{ki}$. The caseworker preference thresholds thus partition job seekers into four training combinations: $(D_1, D_2) \in \{(0,0), (1,0), (0,1), (1,1)\}$.

Figure C.1: Assignment to Training Program



Notes: The horizontal and vertical axes measures job seekers' resistance to classroom and job training, respectively. Panel (a) illustrates how job seekers are assigned to program k if their resistance U_k is lower than the caseworker preference thresholds z_k. Panel (b) shows how a partial increase in classroom-training tendency $z_1 \to z_1'$, holding job-training tendency fixed at z_2, shifts some job seekers toward classroom training but does *not* alter the participation in job training.

Panel (b) illustrates how a partial increase in classroom-training tendency $z_1 \to z_1'$, holding job-training tendency fixed at z_2, shifts some job seekers toward classroom training but does *not* alter the participation in job training. The area between z_1 and z_1' represents job seekers who are shifted (horizontally) into classroom training. The new trainees come from two margins: passive UI $(0,0) \to (1,0)$ and job training only $(0,1) \to (1,1)$. Importantly, the shift in classroom-training tendency does *not* induce any (vertical or diagonal) shifts into or away from

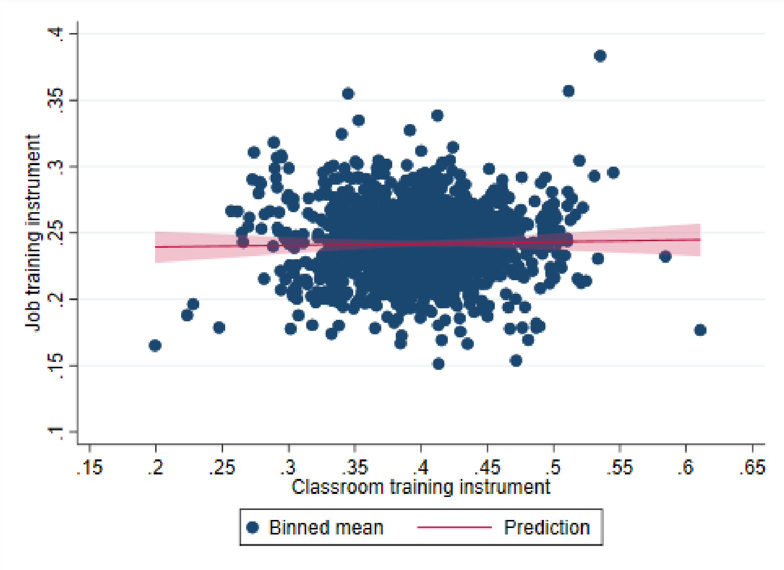
[32] In many settings with multiple treatments, we have economic theory and empirical evidence that IVs should not satisfy extended monotonicity. Mountjoy (2022) shows shorter commuting distances to community college divert some students from enrolling in four-year college. Kirkeboen et al. (2016) show crossing the admission threshold between a preferred and next-best major changes the likelihood that students enroll in alternative majors to the next-best option.

job training.

In Online Appendix OA1.1, we show the extended monotonicity property is sufficient to identify LATE for instrument compliers. In a nutshell, extended monotonicity implies the standard single-treatment LATE analysis applies for each own-instrument once we condition on an evaluation point for the cross-instruments.

D Instrument Diagnostics

Figure D.1: Correlation between Caseworker Tendencies



Notes: Each bin represents the caseworker tendency for 100 job seekers in the sample demeaned by job-center-unit-year fixed effects. The red line represents the linear prediction obtained by OLS regression of the demeaned job-training instrument on the demeaned classroom-training instrument. The shaded areas represents 95% confidence intervals based on standard errors clustered at the level of the predicted caseworker. The slope of this line is estimated to 0.013 (with a t-stat of 0.45).

D.1 Relevance

Table D.1: First-Stage Estimates

	(1) Classroom Training	(2) Job Training
Classroom-Training Instrument	0.353***	0.022
	(0.036)	(0.021)
Job-Training Instrument	0.057*	0.232***
	(0.033)	(0.038)
Obs	196,322	196,322
F-stat	48	19
Complier share	0.08	0.04

Note: All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. The complier share with respect to treatment k is obtained in two steps. First, approximate a k-restrained and k-inclined caseworker by percentile 1 and 99 of the own-instrument demeaned by job-center-unit-year fixed effects (and the cross-instrument). Second, the first-stage coefficient (on the own-instrument) is multiplied by the difference between the two percentiles. See additional details in Online Appendix OA3.2. *p<0.10 ** p<0.05 *** p<0.01.

Table D.2: First-Stage Estimates of Classroom-Training Assignment

	Assignment to Classroom Training						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Classroom-Training Instrument	1.000***	1.000***	0.945***	0.735***	0.710***	0.353***	0.353***
	(0.008)	(0.009)	(0.009)	(0.020)	(0.021)	(0.024)	(0.036)
Job-Training Instrument	-0.000	-0.000	-0.029**	-0.065**	-0.016	0.057*	0.057*
	(0.012)	(0.015)	(0.015)	(0.030)	(0.031)	(0.034)	(0.033)
Obs	196,322	196,322	196,322	196,322	196,322	196,322	196,322
F-stat	8882	5921	5287	657	591	106	48
Caseworker$^{(a)}$	Realized	Predicted	Predicted	Predicted	Predicted	Predicted	Predicted
Leave-out$^{(b)}$	-	-	spell	spell	spell	spell	spell
FE's$^{(c)}$	-	-	-	j × u	j × u, y	j × u × y	j × u × y
Clustering$^{(d)}$	-	-	-	-	-	-	yes

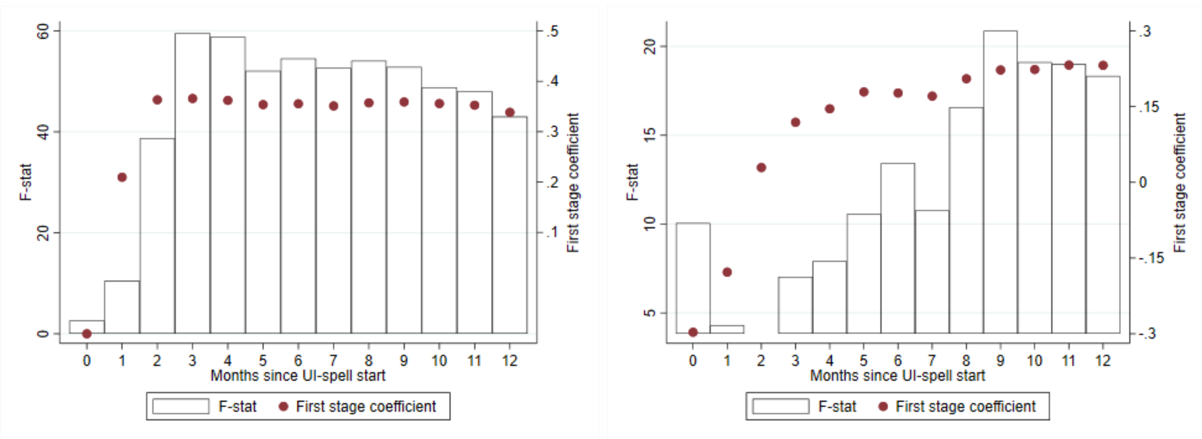
Note: $^{(a)}$ Caseworker refers to whether the instruments are based on the *realized* or the *day-of-birth-predicted* caseworker. $^{(b)}$ Leave-out refers to whether the instrument is defined as the leave-out spell mean (as opposed to the mean). $^{(c)}$ FE's refers to whether the regression includes job-center-unit FE's (j×u), job-center-unit and year FE's (j×u, y), or fully interacted job-center-unit-year FE's (j×u×y). $^{(d)}$ Clustering refers to whether standard errors are two-way clustered (on predicted caseworker and job-seeker level).

Table D.3: First-Stage Estimates of Job Training Assignment

	Assignment to Job Training						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Classroom-Training Instrument	-0.000	0.000	-0.017**	-0.027	-0.020	0.022	0.022
	(0.007)	(0.008)	(0.008)	(0.018)	(0.018)	(0.022)	(0.021)
Job-Training Instrument	1.000***	1.000***	0.883***	0.531**	0.470***	0.232****	0.232***
	(0.010)	(0.013)	(0.014)	(0.027)	(0.027)	(0.030)	(0.038)
Obs	196,322	196,322	196,322	196,322	196,322	196,322	196,322
F-stat	4668	2821	2213	196	149	31	19
Caseworker$^{(a)}$	Realized	Predicted	Predicted	Predicted	Predicted	Predicted	Predicted
Leave-out$^{(b)}$	-	-	spell	spell	spell	spell	spell
FE's$^{(c)}$	-	-	-	j × u	j × u, y	j × u × y	j × u × y
Clustering$^{(d)}$	-	-	-	-	-	-	yes

Note: $^{(a)}$ Caseworker refers to whether the instruments are based on the *realized* or the *day-of-birth-predicted* caseworker. $^{(b)}$ Leave-out refers to whether the instrument is defined as the leave-out spell mean (as opposed to the mean). $^{(c)}$ FE's refers to whether the regression includes job-center-unit FE's (j×u), job-center-unit and year FE's (j×u, y), or fully interacted job-center-unit-year FE's (j×u×y). $^{(d)}$ Clustering refers to whether standard errors are two-way clustered (on predicted caseworker and job-seeker level).

Figure D.2: First-Stage Robustness to Time Horizon



(a) Assignment to classroom training (b) Assignment to job training

Note: First-stage regressions of assignment to training within a given time horizon on the own-instrument and the other instrument (defined with the same time horizon). Time horizon refers to the month in which the assigned training program is set to start. The red dots represents the first-stage coefficient (on the own-instrument), whereas the bars represents the F-stat on both instruments. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level.

D.2 Monotonicity

Table D.4: Testing for Monotonicity with the Baseline Instrument

	Assignment to Classroom Training			
	q1	q2	q3	q4
Classroom-Training Instrument	0.182***	0.433***	0.393***	0.404***
	(0.053)	(0.057)	(0.068)	(0.060)
Job-Training Instrument	0.040	0.026	0.033	0.045
	(0.057)	(0.073)	(0.070)	(0.073)
Obs	49,000	49,001	49,001	49,290
Dep var Mean	0.223	0.343	0.442	0.581
Dep var sd	0.417	0.475	0.497	0.493
F-stat (instruments)	6	29	18	27
P-value (F-stat)	0.002	0.000	0.000	0.000

Note: The sample is partitioned into quartiles based on predicted assignment to classroom training, resulting from an OLS regression of assignment to classroom training on predetermined job-seeker characteristics. These include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). Each column represents the coefficients from a quartile-specific first-stage regression based on the baseline instruments and including job-center-unit-year fixed effects. Standard errors (in parenthesis) are two-way clustered on predicted caseworker and job-seeker level. *p<0.10 ** p<0.05 *** p<0.01.

Table D.5: Testing for Monotonicity with the Baseline Instrument

	Assignment to Job Training			
	q1	q2	q3	q4
Classroom-Training Instrument	-0.008	0.037	0.073	0.007
	(0.030)	(0.041)	(0.045)	(0.051)
Job-Training Instrument	0.214***	0.164***	0.183***	0.314***
	(0.068)	(0.061)	(0.069)	(0.063)
Obs	49,006	49,005	49,007	49,300
Dep var Mean	0.118	0.194	0.268	0.387
Dep var sd	0.323	0.396	0.443	0.487
F-stat (instruments)	5	5	4	13
P-value (F-stat)	0.007	0.007	0.014	0.000

Note: The sample is partitioned into quartiles based on predicted assignment to job training, resulting from an OLS regression of assignment to job training on predetermined job-seeker characteristics. These include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). Each column represents the coefficients from a quartile-specific first-stage regression based on the baseline instruments and including job-center-unit-year fixed effects. Standard errors (in parenthesis) are two-way clustered on predicted caseworker and job-seeker level. *p<0.10 ** p<0.05 *** p<0.01.

Table D.6: Testing for Monotonicity with the Reverse Instrument

	Assignment to Classroom Training			
	q1	q2	q3	q4
(Reversed) Classroom-Training Instrument	0.157***	0.371***	0.343***	0.124***
	(0.032)	(0.055)	(0.057)	(0.037)
(Reversed) Job-Training Instrument	0.0011	-0.032	-0.006	-0.075
	(0.020)	(0.069)	(0.074)	(0.054)
Obs	48,598	49,001	49,001	45,710
Dep var Mean	0.224	0.343	0.442	0.57
Dep var sd	0.417	0.475	0.497	0.495
F-stat (instruments)	12	23	19	6
P-value (F-stat)	0.000	0.000	0.000	0.003

Note: The sample is partitioned into quartiles based on predicted assignment to classroom training, resulting from an OLS regression of assignment to classroom training on job-seeker predetermined characteristics. These include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). For each quartile a "reversed" instrument is constructed: using the average training assignment probability for job seekers with the same predicted caseworker but belonging to one of the other three quartiles. Each column represents the coefficients from a quartile-specific first-stage regression based on these 'reversed' instruments and including job-center-unit-year fixed effects. Standard errors (in parentheses) are two-way clustered on predicted caseworker and job-seeker level. *p<0.10 ** p<0.05 *** p<0.01.

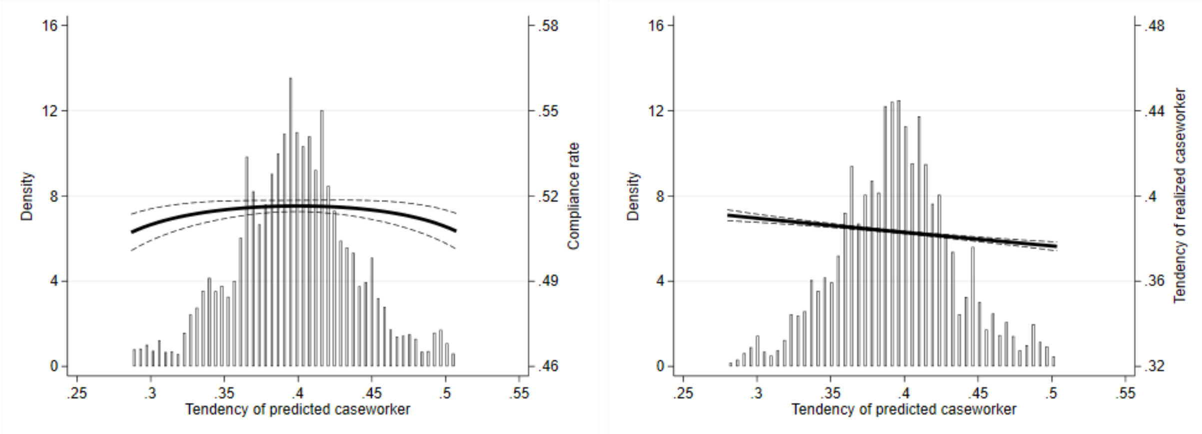
Table D.7: Testing for Monotonicity with the Reverse Instrument

	Assignment to Job Training			
	q1	q2	q3	q4
(Reversed) Classroom-Training Instrument	-0.025	0.050	0.060	-0.011
	(0.027)	(0.040)	(0.045)	(0.039)
(Reversed) Job-Training Instrument	0.148***	0.204***	0.204***	0.086**
	(0.055)	(0.049)	(0.052)	(0.041)
Obs	49,006	49,005	49,007	49,249
Dep var Mean	0.118	0.194	0.268	0.383
Dep var sd	0.323	0.396	0.443	0.487
F-stat (instruments)	4	12	8	2
P-value (F-stat)	0.020	0.000	0.000	0.093

Note: The sample is partitioned into quartiles based on predicted assignment to job training, resulting from an OLS regression of assignment to job training on all predetermined job seeker characteristics. These include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). For each quartile a "reversed" instrument is constructed: using the average training assignment probability for job seekers with the same predicted caseworker but belonging to one of the other three quartiles. Each column represents the coefficients from a quartile-specific first-stage regression based on these reversed' instruments and including job-center-unit-year fixed effects. Standard errors (in parentheses) are two-way clustered on predicted caseworker and job-seeker level. *p<0.10 ** p<0.05 *** p<0.01.

D.2.1 Monotonic Compliance to Caseworker Allocation Rule

Figure D.3: Monotonic Compliance to Classroom Instrument



(a) Compliance rate across caseworker tendency (b) Caseworker tendency if non-compliance

Notes: All plotted values in the figure have been demeaned by job-center-unit-year fixed effects and the job-training tendency for the predicted caseworker. In Panel (a), the black line represents the coefficients from a local linear regression of demeaned compliance rate on demeaned classroom-training tendency of the *predicted* caseworker. In Panel (b), the black line represents the coefficients from a local linear regression of the demeaned classroom-training tendency of the *realized* caseworker on demeaned classroom-training tendency of the *predicted* caseworker. The compliance rate indicates whether the realized caseworker is the day-of-birth-predicted caseworker. The local linear regressions are based on an Epanechnikov kernel and bandwidth 0.1. In both panels, the bars represent the distribution of the classroom-training tendency for the *predicted* caseworker (excluding top and bottom 1%).

D.3 Exclusion

Table D.8: Experience, Caseload Size, and Classroom-Training Tendency

	Summary			Training stringency	
	caseworkers	mean	sd	coef	se
Caseload size					
Meetings/year	622	425.6	244.2	0.0000	(0.000)
Assigntments/year	622	117.2	64.5	0.0000	(0.000)
Experience					
Years	622	2.8	1.34	0.0002	(0.002)
Meetings	622	426.2	244.67	0.0000	(0.000)
Assignments	622	117.4	64.79	0.0000	(0.000)
Obs	622	1307		1307	

Note: Columns (1)-(3) report the number of unique caseworkers along with the mean and standard deviation of experience and caseload size across caseworkers and years. Columns (4)-(5) report the coefficient and standard errors from a regression of caseworker experience or caseload size in a given year on the caseworker's classroom-training tendency in the same year, while controlling for job-center-unit-year fixed effects as well as the caseworker's job-training tendency. Standard errors (in parentheses) are two-way clustered on predicted caseworker level. *p<0.10 ** p<0.05 *** p<0.01.

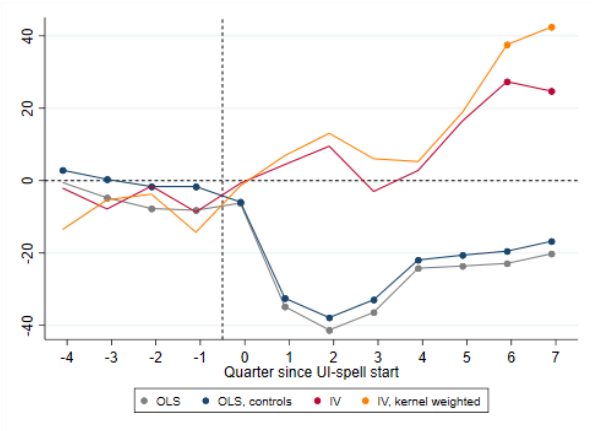
E Robustness

E.1 Local IV

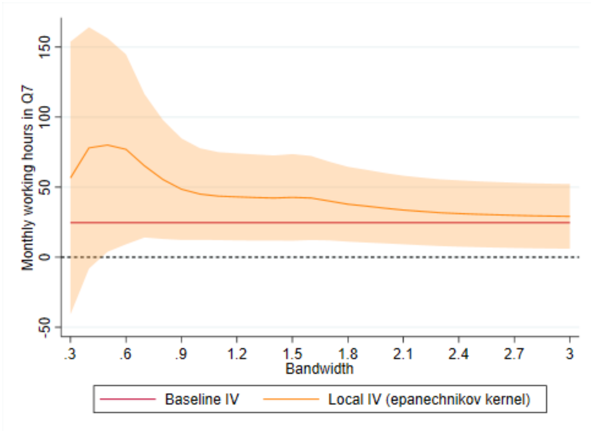
Our baseline TSLS specification controls linearly for assignment to job training, yet Blandhol et al. (2022) discuss the importance of allowing for *flexible* controls in order to interpret TSLS estimates as LATEs. We now show our results are robust to estimating our TSLS specification around an evaluation point z_2' for the job-training instrument (Mountjoy, 2022).

In particular, we estimate our TSLS specification using an Epanechnikov kernel to weight all observations around the mean of the job-training instrument (within bandwidth 1.5 SDs). Figure E.1a plots this local IV estimate along with our baseline IV estimate. The figure shows our results are robust to local estimation of Equations (2)-(3). Figure E.1b suggests this result holds across a wide range of kernel bandwidths.

Figure E.1: Local IV



(a) Local IV



(b) Effect in Q7 against bandwidth choice

Note: Panels (a)-(b) plot the effect of assignment to classroom training on average monthly working hours in a given quarter relative to job loss. In all panels, the red line represents the baseline IV estimate and the blue line represents the OLS estimate (with controls). In Panel (a), the orange line represent the local IV estimate obtained by using an Epanechnikov kernel (with bandwidth 1.5) to weight all observations around the mean of the job-training instrument. Full (hollow) dots indicate significance at the (10%) 5% level. In Panel (b), the orange line shows the local IV estimate obtained for a given choice of bandwidth on the Epanechnikov kernel. The light-orange area shows the 95% confidence intervals on the local IV estimate. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level.

E.2 Separate Treatment Margins

Given extended monotonicity, our caseworker-classroom-tendency instrument shifts compliers into classroom training from two margins. Namely, the instrument shifts some compliers from passive UI into classroom training, $(0, 0) \rightarrow (1, 0)$, and it shifts some compliers from job training into both job training and classroom training, $(0, 1) \to (1, 1)$.[33]

Table E.1 decomposes the first stage by treatment margin. Column (1) shows the baseline first stage for assignment to classroom training. Column (2) shows the first stage for compliers shifted from passive UI and into classroom training, $(0, 0) \to (1, 0)$. These estimates are obtained by estimating the first stage with an interaction between assignment to classroom training and non-assignment to job training on the LHS; see Equation (18). Column (3) shows the first stage for compliers shifted from job training into both job training and classroom training, $(0, 1) \to (1, 1)$. These estimates are obtained by estimating the first stage with an interaction between assignment to classroom training and assignment to job training on the LHS; see Equation (19). Importantly, the baseline first-stage coefficient (Column (1)) equals the sum of the first-stage coefficients by treatment margin (Column (2)+(3)).

$$D_{1i}(1 - D_{2i}) = \delta_{j(i)} + \delta_{10} Z_{1i} + \delta_2 Z_{2i} \tag{18}$$

$$D_{1i} D_{2i} = \delta_{j(i)} + \delta_{11} Z_{1i} + \delta_2 Z_{2i}. \tag{19}$$

We highlight two insights from Table E.1. First, the coefficient on the classroom tendency instrument is about three times larger for the passive margin (Column (2)) than for the job-training margin (Column (3)). Likewise, the F-stat on the instruments is two times larger for the passive margin than for the job-training margin. Second, the table shows the majority of compliers with the classroom-training instrument come from the passive margin (6%) rather than the job-training margin (2%).

Figure E.2 shows our results are robust to focusing exclusively on compliers shifted from the passive margin and into classroom training. The red line represents our baseline IV estimate, which captures the average employment effect across margins. The solid orange line represents the employment effect for compliers shifted from the passive margin and into classroom training. Online Appendix OA1.1.3 explains how we estimate this margin-specific effect of classroom training. Clearly, the baseline IV estimate is very similar to the effect for compliers shifted from the passive margin into classroom training. If anything, the effect of classroom training is more positive for compliers shifted from the passive margin.

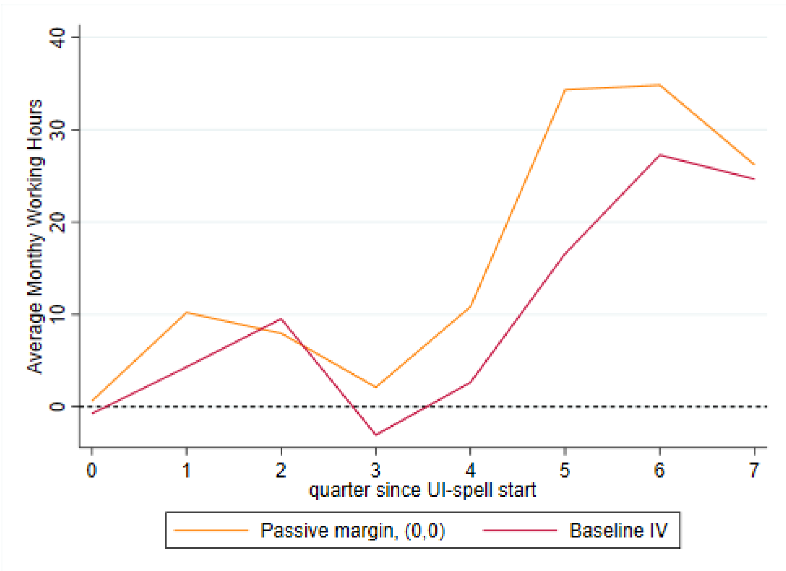
[33] Given extended monotonicity, our classroom-training instrument *never* shifts anyone from "job training only" into "classroom training only", $(0, 1) \to (1, 0)$

Table E.1: First-Stage Estimates by Margin

	Classroom Training by Margin		
	Any margin $(0, x) \to (1, x)$	Passive margin $(0, 0) \to (1, 0)$	Job-training margin $(0, 1) \to (1, 1)$
Classroom-Training Instrument	0.353***	0.263***	0.090***
	(0.036)	(0.031)	(0.017)
Job-Training Instrument	0.057*	-0.037	0.094***
	(0.033)	(0.035)	(0.025)
Obs	196,322	196,322	196,322
Obs by cell	78,034	54,590	23,454
F-stat	48	36	18
compliers	0.08	0.06	0.02

Note: First-stage estimates for classroom training by job-training margin. Column (1) uses assignment to classroom training as the dependent variable; that is, it corresponds to our baseline first stage estimate (Column (1) of Table D.1). Columns (2) and (3) uses assignment to classroom training interacted with indicators for non-assignment and assignment to job training as the dependent variable. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. The complier share in the bottom of the table represents the share of compliers in the population who are shifted into classroom training by our classroom-training instrument. To calculate this share, we re-scale the coefficient on the classroom instrument by the difference in classroom-tendency for a classroom-restrained and a classroom-inclined caseworker (approximated by percentile 1 and 99 on the classroom-training instrument). *p<0.10 ** p<0.05 *** p<0.01.

Figure E.2: Employment Effect by Treatment Margin

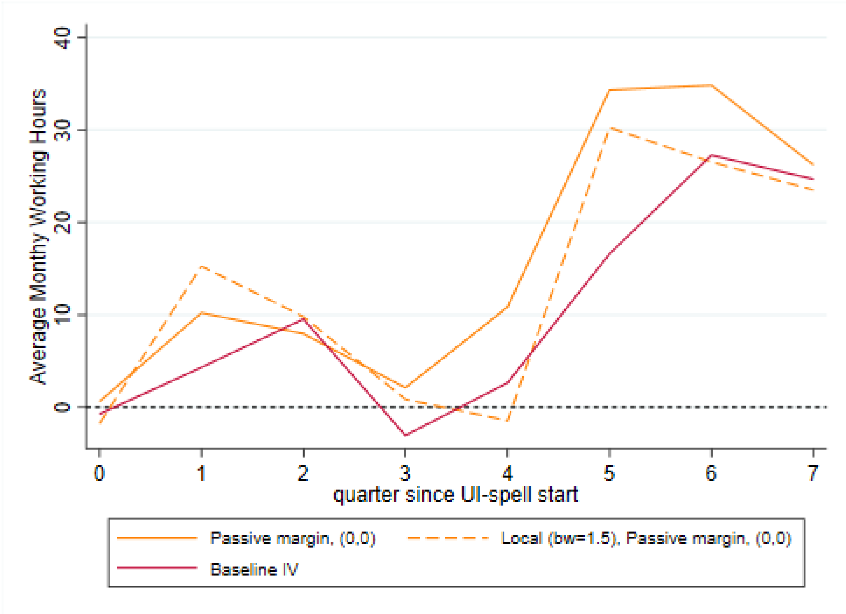


Note: The effect of assignment to classroom training on average monthly working hours in a given quarter relative to job loss. The red line represents the baseline IV estimate. The orange line represents the employment effect for compliers shifted from passive UI and into classroom training, $(0, 0) \to (1, 0)$. Online Appendix OA1.1.3 explains how we estimate this margin-specific effect of classroom training. To facilitate comparison with the baseline estimate, we control linearly for the job-training instrument. All regressions include job-center-unit-year fixed effects. This figure shows no indications of statistical significance.

The orange line in Figure E.2 relies on *controlling linearly* for assignment to job training.

Figure E.3 shows the results are robust to a local estimation around an evaluation point z_2' for the job training instrument (Mountjoy, 2022).

Figure E.3: Employment Effect by Treatment Margin
Local Estimation



Note: The effect of assignment to classroom training on average monthly working hours in a given quarter relative to job loss. The red line represents the baseline IV estimate. The solid orange line represents the employment effect for compliers shifted from passive UI and into classroom training, $(0, 0) \rightarrow (1, 0)$. The dashed orange line represents a *local* version of solid orange line: the estimates are obtained using an Epanechnikov kernel (with bandwidth 1.5) to weight all observations around the mean of the job-training instrument. All regressions include job-center-unit-year fixed effects. This figure shows no indications of statistical significance.

E.3 Control for Meeting Timing and Frequency

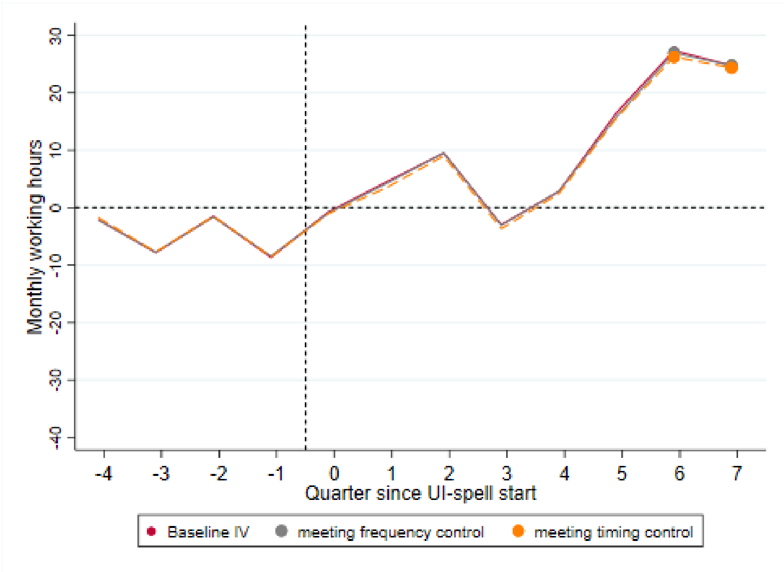
As discussed in section 5.4, one violation of the exclusion restriction is if caseworker-classroom-training tendencies affect employment rates through more frequent or earlier meetings. We test this possibility by re-estimating our main regressions while controlling for the frequency with which the job seeker meets with her caseworker and the timing of the first meeting.[34][35] Because the meeting frequency (and timing) is endogenous, we instrument the meeting frequency (timing) of job seeker i with the caseworker's general meeting frequency (timing), measured as a leave-out mean.

[34]We define meeting frequency for job seeker i as the number of caseworker meetings per week of unemployment. If the job seeker's UI-spell is longer than 26 weeks, we only consider the first 26 weeks, because meeting-frequency requirements change after 26 weeks. This measure includes all caseworker meetings held, regardless of the participating caseworker.

[35]Timing of the meeting is measured as the number of weeks between the UI-spell start and the first meeting

Figure E.4 presents the baseline IV estimate of the effect of assignment to classroom training along with the IV estimate obtained while controlling for meeting frequency and timing. Evidently, our IV estimates are very robust to the inclusion of these controls. We take this finding as evidence in support of the exclusion restriction.

Figure E.4: Controlling for Meeting Frequency & Timing

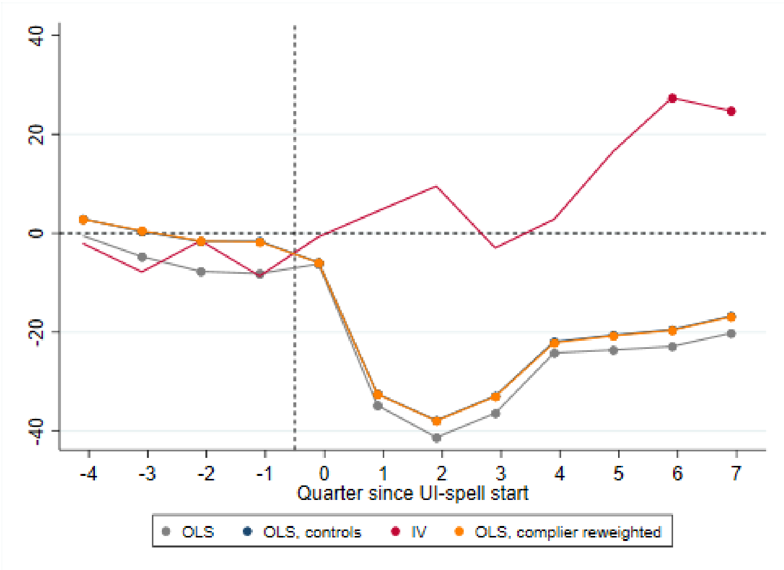


Note: The effect of assignment to classroom training on average monthly working hours in a given quarter relative to job loss. The red line represents the baseline IV estimate. The orange and black lines represent the IV estimate obtained by further controlling for caseworker meeting frequency and timing of the first meeting. Both of these controls are instrumented by corresponding leave-out means. All regressions include fully interacted job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

E.4 Complier-Characteristic Reweighted OLS

To test for effect heterogeneity across compliers and non-compliers, we assign all job seekers in the sample a weight according to their similarity with compliers in terms of observable characteristics. The weights are obtained by first partitioning the sample into eight subgroups based on the job seekers' (i) unemployment status in year t-1 and her (ii) predicted probability of assignment to classroom training based on her predetermined characteristics. These characteristics include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). Second, we compute the share of compliers in each subgroup and we then assign each job seeker a weight that reflects her similarity with compliers. We use these weights to estimate a complier re-weighted OLS. Figure E.5 shows the complier re-weighted OLS is very similar to the standard OLS, suggesting effect heterogeneity based on observables *not* is driving the difference between IV and OLS.

Figure E.5: Complier-Characteristic Reweighted OLS



Note: The effect of assignment to classroom training on average monthly working hours in a given quarter relative to job loss. The red line represents the baseline IV estimate and the blue line represents the OLS estimate (with controls). These controls include socio-demographics and labor market history from Table 3 as well as education levels (9 sections defined as ISCED), previous industry (21 sections defined as NACE), and the typical occupation over the career (3-digit ISCO08). The orange line represents a complier re-weighted OLS. To obtain the complier weights, the population is partitioned into eight subgroups according to (i) the job seeker's unemployment status in year t-1 and (ii) her predicted probability of assignment to classroom training (based on the aforementioned set of controls). All job seekers are then assigned a weight according to the share of compliers in the subgroup. To calculate the share of compliers, the subgroup-specific first stage was re-scaled by p1 and p99 on the own-instrument for classroom training. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job-seeker level. Full (hollow) dots indicate significance at the (10%) 5% level.

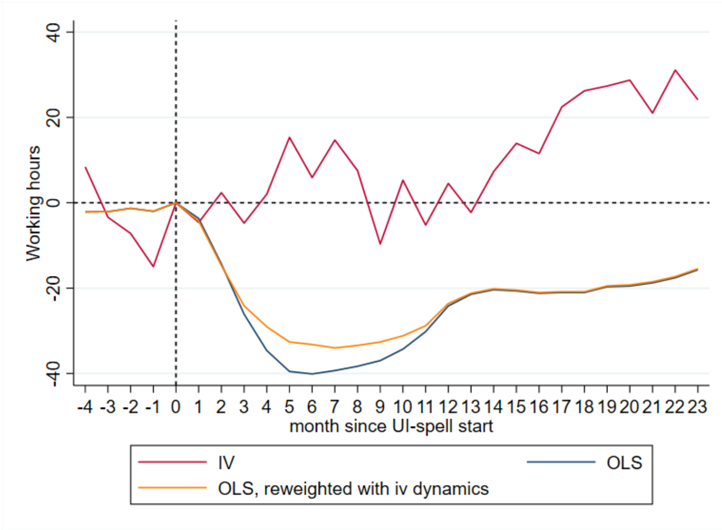
E.5 OLS Reweighted by IV Training Dynamics

Figure E.6 plots three series based on our OLS and IV estimates for training-state probabilities (γ^s_{1t}), potential outcomes in a given state (β^{1s}_{1t}), and counterfactual outcomes (β_{0t}). We use these inputs to compute the average effect of assignment to classroom training.[36] Using the IV estimates for all three inputs, we obtain the baseline IV estimate. Correspondingly, using the OLS estimates for all three inputs, we obtain the OLS estimate. These are plotted in red and blue in Figure E.6. We also compute a hybrid estimate that uses the OLS estimate for potential outcomes (β^{1s}_{1t} and β_{0t}) and the IV estimate for training-state probabilities (γ^s_{1t}). That is, the hybrid corresponds to reweighting our baseline OLS estimate by the IV training dynamics. It is depicted in orange in the figure. Evidently, the baseline OLS and OLS reweighted by IV

[36]We plug the estimates for γ^s_{1t}, β^{1s}_{1t}, and β_{0t} into Equation (4)

training dynamics are very similar.

Figure E.6: OLS Reweighted by IV Dynamics



Note: The figure plots the effect of assignment to classroom training on average monthly working hours in a given quarter relative to job loss. The effects are computed based on our OLS and IV estimates for γ_{1t}^{s}, β_{1t}^{1s}, and β_{0t}, according to equation (4). The red line represents the effects computed based on IV estimates only, the blue line represents the effects based on OLS estimates only. The orange line represents a hybrid: it is computed based on IV estimates for γ_{1t}^{s} and OLS estimates for β_{1t}^{1s} and β_{0t}. This figure shows no indication of statistical significance.

F Threat, Lock-in, and Post-program Effects

F.1 Training States

We split job seekers assigned to classroom training (assignees) into four mutually exclusive states. First, we identify the following dates for all assignees:

t^1: Assignment to training ($\sim$ First caseworker meeting)

t^2: Start of training

t^3: End of training

t^e: End of UI-spell

Second, define four mutually exclusive states, $s \in \{a, b, c, d\}$, for assignees in a given period t:

(a) Yet to be assigned:

$t < t^1$

(b) Yet to start training (or exit UI before training start):

$t^1 \leq t < t^2$

$t^e < t^2$ & $t \geq t^e$

(c) Undergoing training (or exit UI during training):

$t^2 \leq t \leq t^3$

$t^2 \leq t^e < t^3$ & $t \geq t^e$

(d) Done with training (or exit UI after/by end of training):

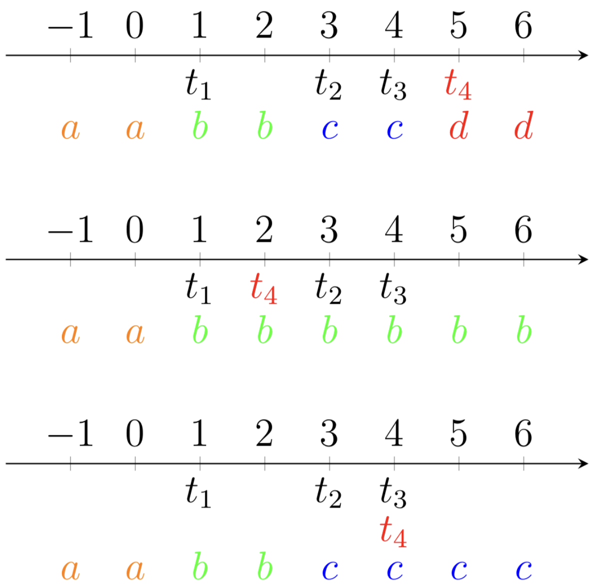
$t > t^3$

$t^e \geq t^3$ & $t > t^e$

All job seekers who ever are assigned to training will belong to one of the states $s \in \{a, b, c, d\}$ in a given period t. If the job seeker exits unemployment after the end of the activity, she will transition through all four states. However, if she exits unemployment earlier, she will remain in her latest state in all later periods. Figure F.1 illustrates this point with three scenarios. In all three scenarios, a job seeker meets with her caseworker in month 1 relative to job loss (t1), is assigned to training that starts in month 3 (t2) and ends in month 4 (t3). What differs across scenarios is when the job seeker exits unemployment, and hence how many states she progresses through.

In the first scenario, the job seeker exits unemployment in month 5. Therefore, she progresses through all four states: She is (a) yet to be assigned in months -1 to 0, (b) yet to start training in months 1-2, (c) undergoing training in months 3-4, and (d) done with training from month 5 and onward. In the second scenario, the job seeker exits unemployment even before she has started training. Therefore, she only progresses to state (b) and remains there in all periods onward. In the third scenario, the job seeker exits unemployment while undergoing training. Therefore, she only progresses to state (c) and remains there in all periods onward.

Figure F.1: A Job Seeker's Transition through Training States



Online Appendix

What Works for the Unemployed? Evidence from Quasi-Random Caseworker Assignments

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Mette Rasmussen — *U of Copenhagen*

OA1 Identification Strategy

OA1.1 Local Average Treatment Effects

In this section, we show the caseworker-tendency instruments developed in Section C.3 identify local average treatment effects (LATE) for instrument compliers (Imbens and Angrist, 1994). Proposition 1 states the identification result. Section OA1.1.1 illustrates the result in the simple case of two caseworkers. Section OA1.1.2 expands the range of instrument values. Section OA1.1.3 implements the identification approach with a series of local two-stage least-squares regressions.

Proposition 1 (Mean Potential Outcomes of Instrument Compliers)

1. *Denote job seekers who shift from treatment d to d' if assigned a caseworker with tendencies z' instead of z by*

$$\{d \to d'\}_{(z \to z')} = \{u \in [0,1]^2 \quad \textit{s/t} \quad d' = \mathbf{1}[z' \geq u],\, d = \mathbf{1}[z \geq u]\}. \tag{20}$$

2. *Define potential outcomes Y and training assignments D as in Section C.1.*

3. *The caseworker-tendency instruments developed in Section C.3 identify the mean potential outcomes for instrument compliers along all training margins as*

$$\mathbb{E}\left[Y(d'')|\{d \to d'\}_{(z_k \to z'_k, z_l)}\right] = \frac{\mathbb{E}\left[Y\mathbf{1}_{[D=d'']}|z'_k, z_l\right] - \mathbb{E}\left[Y\mathbf{1}_{[D=d'']}|z_k, z_l\right]}{\mathbb{E}\left[\mathbf{1}_{[D=d'']}|z'_k, z_l\right] - \mathbb{E}\left[\mathbf{1}_{[D=d'']}|z_k, z_l\right]}. \tag{21}$$

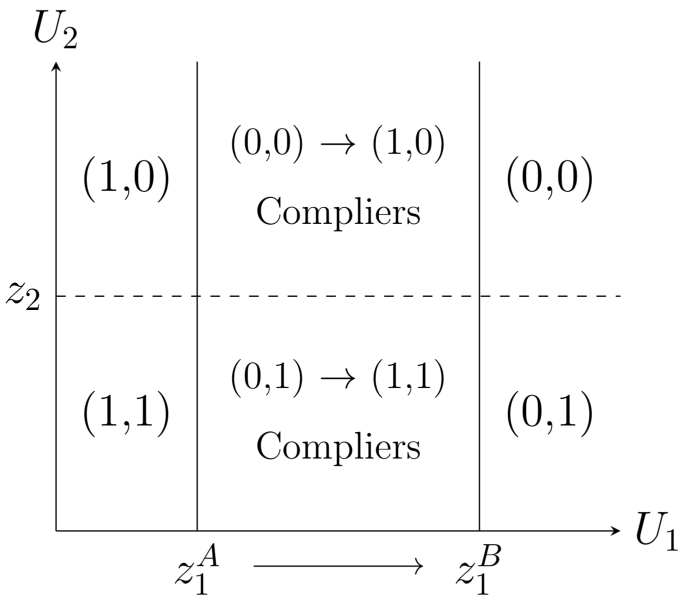
Having recovered the mean potential outcomes, we can calculate the treatment effects of compliers around each separate treatment margin. For example, the LATE for job seekers who are shifted from passive unemployment to classroom training is

$$\mathbb{E}\left[\beta_1 | \{(0,0) \to (1,0)\}_{(z_k \to z_k', z_l)}\right] = \mathbb{E}\left[Y(1,0) - Y(0,0) | \{(0,0) \to (1,0)\}_{(z_k \to z_k', z_l)}\right]. \quad (22)$$

OA1.1.1 Illustration with Two Caseworkers

Figure OA1 illustrates how extended monotonicity facilitates the identification of potential outcomes of instrument compliers. In the figure, we compare caseworkers A and B, who have similar tendencies to assign job training but differ in their tendency with respect to classroom training.[37]

Figure OA1: Compliers wit a Shift in the Classroom-Training Instrument



Comparing the share of job seekers who are assigned to classroom training by caseworkers A and B, we can estimate the share of compliers with the $(z_1^A \to z_1^B)$ shift at z_2. By splitting the counts by job-training status, we can calculate the share of compliers along each separate margin:

$$\mathbb{E}\left[D_1(1-D_2)|z_1^B, z_2\right] - \mathbb{E}\left[D_1(1-D_2)|z_1^A, z_2\right] = \mathbb{P}\left[\{(0,0) \to (1,0)\}_{(z_1^A \to z_1^B, z_2)}\right] \quad (23)$$

$$\mathbb{E}\left[D_1 D_2|z_1^B, z_2\right] - \mathbb{E}\left[D_1 D_2|z_1^A, z_2\right] = \mathbb{P}\left[\{(0,1) \to (1,1)\}_{(z_1^A \to z_1^B, z_2)}\right]. \quad (24)$$

[37]The example focuses on shifts in the classroom-training instrument, keeping job-training instruments fixed. All arguments apply symmetrically to shifts in the job-training instrument, keeping classroom-training instruments fixed.

Second, by studying how total employment shifts across treatment cells, we can estimate the total potential outcomes of the instrument compliers:

$$\begin{aligned}&\mathbb{E}\left[YD_1(1-D_2)|z_1^B,z_2\right]-\mathbb{E}\left[YD_1(1-D_2)|z_1^A,z_2\right]\\&=\mathbb{P}\left[\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]\times\mathbb{E}\left[Y(1,0)|\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]\end{aligned}\tag{25}$$

$$\begin{aligned}&\mathbb{E}\left[Y(1-D_1)(1-D_2)|z_1^B,z_2\right]-\mathbb{E}\left[YD_1(1-D_2)|z_1^A,z_2\right]\\&=-\mathbb{P}\left[\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]\times\mathbb{E}\left[Y(0,0)|\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]\end{aligned}\tag{26}$$

$$\begin{aligned}&\mathbb{E}\left[Y(1-D_1)(1-D_2)|z_1^B,z_2\right]-\mathbb{E}\left[Y(1-D_1)(1-D_2)|z_1^A,z_2\right]\\&=\mathbb{P}\left[\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]\times\mathbb{E}\left[Y(1,1)|\{(0,1)\to(1,1)\}_{(z_1^A\to z_1^B,z_2)}\right]\end{aligned}\tag{27}$$

$$\begin{aligned}&\mathbb{E}\left[Y(1-D_1)D_2|z_1^B,z_2\right]-\mathbb{E}\left[YD_1(1-D_2)|z_1^A,z_2\right]\\&=-\mathbb{P}\left[\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]\times\mathbb{E}\left[Y(0,1)|\{(0,1)\to(1,1)\}_{(z_1^A\to z_1^B,z_2)}\right].\end{aligned}\tag{28}$$

Finally, relating the shifts in total employment (Equations (25)-(26) and (27)-(28), respectively) to the shifts in count shares (Equations (23) and (24), respectively), we can isolate the mean potential outcomes of compliers who are shifted into classroom training if assigned to caseworker B instead of caseworker A:

$$\mathbb{E}\left[Y(1,0)|\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]=\frac{\mathbb{E}\left[YD_1(1-D_2)|z_1^B,z_2\right]-\mathbb{E}\left[D_1(1-D_2)|z_1^A,z_2\right]}{\mathbb{E}\left[D_1(1-D_2)|z_1^B,z_2\right]-\mathbb{E}\left[D_1(1-D_2)|z_1^A,z_2\right]}\tag{29}$$

$$\mathbb{E}\left[Y(0,0)|\{(0,0)\to(1,0)\}_{(z_1^A\to z_1^B,z_2)}\right]=\frac{\mathbb{E}\left[Y(1-D_1)(1-D_2)|z_1^B,z_2\right]-\mathbb{E}\left[Y(1-D_1)(1-D_2)|z_1^A,z_2\right]}{\mathbb{E}\left[(1-D_1)(1-D_2)|z_1^B,z_2\right]-\mathbb{E}\left[(1-D_1)(1-D_2)|z_1^A,z_2\right]}\tag{30}$$

$$\mathbb{E}\left[Y(1,1)|\{(0,1)\to(1,1)\}_{(z_1^A\to z_1^B,z_2)}\right]=\frac{\mathbb{E}\left[YD_1D_2|z_1^B,z_2\right]-\mathbb{E}\left[YD_1D_2|z_1^A,z_2\right]}{\mathbb{E}\left[D_1D_2|z_1^B,z_2\right]-\mathbb{E}\left[D_1D_2|z_1^A,z_2\right]}\tag{31}$$

$$\mathbb{E}\left[Y(0,1)|\{(0,1)\to(1,1)\}_{(z_1^A\to z_1^B,z_2)}\right]=\frac{\mathbb{E}\left[Y(1-D_1)D_2|z_1^B,z_2\right]-\mathbb{E}\left[Y(1-D_1)D_2|z_1^A,z_2\right]}{\mathbb{E}\left[(1-D_1)D_2|z_1^B,z_2\right]-\mathbb{E}\left[(1-D_1)D_2|z_1^A,z_2\right]}.\tag{32}$$

OA1.1.2 Expanding the Range of Instrument Values

The identification approach in Section OA1.1.1 relies on partial shifts in one training-tendency instrument, keeping the other instrument fixed. For this purpose, we define compliers with one instrument around values of the other instrument.

Denote the most inclined and most restrained caseworkers with respect to classroom training around a value of job-training tendency z_2' as

$$z^{\text{min}}(z_2') = \arg\min_{z \in \mathcal{Z}} z_1 \quad \text{s/t} \quad z_2 \in [z_2' - \epsilon_2, z_2' + \epsilon_2] \tag{33}$$

$$z^{\text{max}}(z_2') = \arg\max_{z \in \mathcal{Z}} z_1 \quad \text{s/t} \quad z_2 \in [z_2' - \epsilon_2, z_2' + \epsilon_2], \tag{34}$$

where ϵ is a bandwidth parameter.

The set of compliers with the classroom instrument around z_2' is thus defined as

$$U_1^c(z_2') = \{i \quad \text{s/t} \quad u_{i1} \in [z_1^{\text{min}}(z_2'), z_1^{\text{max}}(z_2')]\}. \tag{35}$$

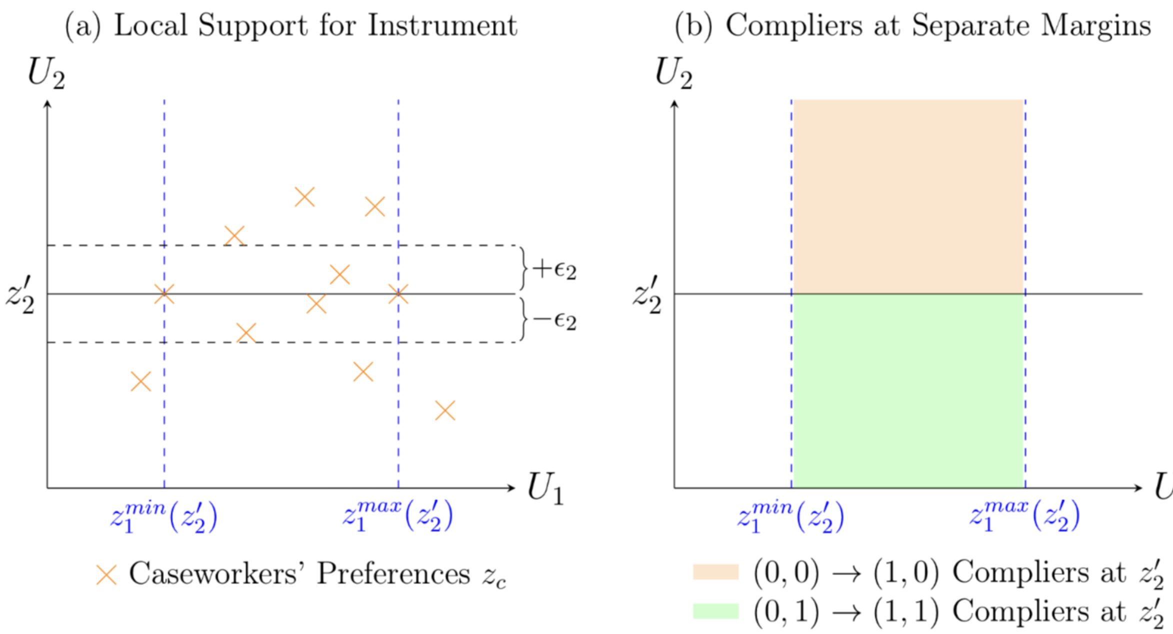
We can split the compliers according to their job-training statuses:

$$U_1^{(c,0)}(z_2') = \{i \quad \text{s/t} \quad u_{1i} \in [z_1^{\text{min}}(z_2'), z_1^{\text{max}}(z_2')], u_{2i} > \mu_2(z_2')\} \tag{36}$$

$$U_1^{(c,1)}(z_2') = \{i \quad \text{s/t} \quad u_{1i} \in [z_1^{\text{min}}(z_2'), z_1^{\text{max}}(z_2')], u_{2i} \leq \mu_2(z_2')\}. \tag{37}$$

Figure OA2 illustrates the identification of point-specific compliers.

Figure OA2: Compliers with Classroom-Training Instrument



The complete set of compliers with the classroom instrument is the union of the point-specific compliers:

$$U_1^c = \bigcup_{z_2' \in [0,1]} U_1^c(z_2'). \tag{38}$$

OA1.1.3 Econometric Implementation

Proposition 1 suggests that, to identify the causal effects of classroom training, we regress the outcome variables,

$$T = \{D_1 D_2, D_1(1 - D_2), (1 - D_1)D_2, (1 - D_1)(1 - D_2), .. \tag{39}$$

$$Y, YD_1D_2, YD_1(1 - D_2), Y(1 - D_1)D_2, Y(1 - D_1)(1 - D_2)\} \tag{40}$$

on the classroom-training instrument Z_1, holding the job-training instrument fixed at some evaluation point z_2':

$$T_i = \beta_0^T + \beta_1^T Z_{i1} \quad \text{for} \quad z_{i2} \in [z_2' - \epsilon_2, z_2' + \epsilon_2]. \tag{41}$$

Having estimated Equation (41), we can recover mean potential outcomes for classroom training $d \in \{0, 1\}$ along each margin of job training using the Wald ratios:

$$\mathbb{E}\left[Y(d, 0)|U_1^{(c,0)}(z_2')\right] = \frac{\hat{\beta}_1^{Y(D_1=d)(1-D_2)}}{\hat{\beta}_1^{(D_1=d)(1-D_2)}} \tag{42}$$

$$\mathbb{E}\left[Y(d, 1)|U_1^{(c,1)}(z_2')\right] = \frac{\hat{\beta}_1^{Y(D_1=d)D_2}}{\hat{\beta}_1^{(D_1=d)D_2}} \tag{43}$$

To increase power, we can stack the point-specific evaluations in Equation (41) into a single regression, controlling flexibly for the job-training instruments (e.g., using bins of bandwidth ϵ_2),

$$T_i = \beta_0^T + \beta_1^T Z_{i1} + g_{\epsilon_2}(Z_{i2}; \beta_2^T), \tag{44}$$

and plug in

$$\mathbb{E}\left[Y(d, 0)|U_1^{(c,0)}\right] = \frac{\hat{\beta}_1^{Y(D_1=d)(1-D_2)}}{\hat{\beta}_1^{(D_1=d)(1-D_2)}} \tag{45}$$

$$\mathbb{E}\left[Y(d, 1)|U_1^{(c,1)}\right] = \frac{\hat{\beta}_1^{Y(D_1=d)D_2}}{\hat{\beta}_1^{(D_1=d)D_2}}. \tag{46}$$

Note Equations (44) and (45)-(46) simplify to the standard TSLS specification if the control function $g(\cdot)$ is linear in Z_{i2}, i.e. $g(Z_{i2}; \beta_2^T) = \beta_2^T Z_{i2}$. Blandhol et al. (2022) discuss the importance of allowing for flexible controls in order to interpret TSLS estimates as LATEs. In Section OA1.3, we show the linear TSLS specification is valid if the marginal treatment effects of classroom training β_{1i} are uncorrelated with the job seekers' resistance to job training U_{2i}.

In the case of homogeneous treatment effects, we only need to control for Z_{i2} to the extent that the instruments are correlated. In Appendix C, we show Z_1 and Z_2 are largely orthogonal in our data, alleviating this concern for identification.

In practice, we first estimate the standard TSLS specification to facilitate comparisons to how prior papers have handled multiple treatments in judge IV setups (Bhuller et al., 2020; Maestas et al., 2015). In a second step, we follow Mountjoy (2022) and show robustness to estimating the specification around an evaluation point z_2' for the job-training instrument.

OA1.2 Non-compliance with Caseworker Allocation Rule

In this section, we allow job centers to deviate from their predicted quasi-random rule for allocating job seekers to caseworkers, $c_i \neq c(b_i)$. We clarify the conditions under which our birthday-predicted caseworker-tendency instruments satisfy the relevance, exogeneity, exclusion, and monotonicity conditions for identification of LATEs.

Exogeneity and Exclusion

To assess exogeneity and exclusion, we note the variation in the caseworker-tendency instrument comes solely from the birthdays of job seekers,

$$Z_{ki} = \mathbb{E}\left[D_{ki} | c(b_j) = c(b_i), j \neq i\right]. \tag{47}$$

Hence, the instruments satisfy the exogeneity and exclusion criteria if job seekers' training and employment potentials are unrelated to their birthday in the month:

$$b_i \perp\!\!\!\perp (U_i, \beta_i). \tag{48}$$

Relevance and Monotonicity

Monotonicity requires that workers with a more training-inclined *predicted* caseworker also end with a (weakly) more training-inclined *realized* caseworker:

$$V_{kc(b_i)} > V_{kc(b_j)} \implies V_{kc_i} \geq V_{kc_j},\; V_{lc_i} = V_{lc_j}, \quad k \neq l. \tag{49}$$

The "monotonic compliance" condition in Equation (49) implies the tendency instruments are also relevant. In Section 5, we provide empirical support for monotonic compliance. First, we show no correlation exists between the training tendency of a job seeker's predicted caseworker and the rate of compliance with the default allocation rule. Second, we show that if deviating from the allocation rule, job seekers with more training-inclined predicted caseworkers are *not* reassigned to less training-inclined realized caseworkers.

OA1.3 Marginal Treatment Effects

The patterns of selection into training depend on the correlation between job seekers' resistance to training U_i and their potential employment outcomes β_i. To make inferences about these selection patterns, we follow Mogstad et al. (2018) and impose shape restrictions on the marginal treatment response (MTR) and marginal treatment effect (MTE) functions. In practice, we assume an additively separable and linear specification in the quantile of the distribution of suitability for training:

$$\mathbb{E}[\beta_{i0}|U_i] = \alpha_{00} + \alpha_{01}U_1 + \alpha_{02}U_2 \tag{50}$$

$$\mathbb{E}[\beta_{i1}|U_i] = \alpha_{10} + \alpha_{11}U_{1i} \tag{51}$$

$$\mathbb{E}[\beta_{i2}|U_i] = \alpha_{20} + \alpha_{21}U_{2i}. \tag{52}$$

A positive value of the α_{0k} parameter captures if job seekers with worse job opportunities select into training (negative "selection on levels"). A negative value of the α_{1k} parameter captures if caseworkers prioritize training for job seekers who have the most to gain from the programs (positive "selection on gains").[38]

OA1.3.1 Estimation

Given the shape restrictions specified in (50)-(52), we can write the employment outcome for job seekers assigned to a caseworker with leniency Z, as a function of second-order polynomials in the propensity score with respect to classroom and job training. Namely, plugging (50)-(52)

[38]That is, we allow potential employment outcomes to depend linearly on unobserved resistance to training. We have good reasons to believe job seekers opt into training based on their potential outcomes. For instance, a job seeker with high potential employment, for example, due to a pending job offer, may be resistant to start in classroom training, regardless of the training tendency of her caseworker. This outcome would be consistent with Figure A.3.(a), showing that never-takers of training have higher employment rates than compliers not assigned to training.

into (9), we get

$$\begin{aligned}\mathbb{E}[Y_i|Z_i] &= \int_0^1 \int_0^1 (\alpha_{00} + \alpha_{01}U_1 + \alpha_{02}U_2) dU_1 dU_2 \\ &+ \int_0^{\hat{D}_{i1}} (\alpha_{10} + \alpha_{11}U_1) dU_1 + \int_0^{\hat{D}_{i2}} (\alpha_{20} + \alpha_{21}U_2) dU_2 \\ &= \beta_0 + \beta_{11}\hat{D}_{i1} + \beta_{21}\hat{D}_{i2} + \beta_{12}\hat{D}_{i1}^2 + \beta_{22}\hat{D}_{i2}^2 \end{aligned} \tag{53}$$

with

$$\beta_0 = \alpha_{00} + \frac{\alpha_{01} + \alpha_{02}}{2}, \quad \beta_{11} = \alpha_{10}, \quad \beta_{12} = \frac{\alpha_{11}}{2}, \quad \beta_{21} = \alpha_{20}, \quad \beta_{22} = \frac{\alpha_{21}}{2}, \tag{54}$$

where we have replaced caseworker preferences Z by the propensity score by normalizing the marginal distributions of the resistances to be uniform, $U_{ki} \sim U[0,1]$. We estimate the MTE functions based on the common support of the propensity scores for treated and non-treated job seekers. Note Equation (54) simplifies to the standard TSLS estimator in Equations (2)-(3) if MTEs are constant ($\alpha_{11} = \alpha_{21} = 0$).

OA1.3.2 Recovering Target Parameters

Following Mogstad et al. (2018), we can convert the estimated MTR functions into a host of parameters of interest, for example, the average treatment effect (ATE) for classroom training,

$$\text{ATE}_1 = \int_0^1 (\hat{\alpha}_{10} + \hat{\alpha}_{11}U_1) dU_1 = \hat{\alpha}_{10} + \frac{\hat{\alpha}_{11}}{2}. \tag{55}$$

OA2 Data

OA2.1 UI spells for 2012-2018

We identify all UI spells initiated in 2012-2018 based on the DREAM register. This register covers the entire Danish population and contains information about *weekly* receipts of any type of public transfers including UI-benefits. We define a UI spell as the weeks in which the job seeker receives UI benefits or participates in activation programs (UI benefits hereafter), using the following DREAM codes: 111, 115, 121, 213, 214, 215, 216, 217, 218, 219, 231, 299. We identify the *start* of the UI spell as the first week with UI benefits, followed by at least eight consecutive weeks with no UI-benefits. We define the *end* of the UI spell as the last week with UI benefits, followed by at least eight consecutive weeks with no UI benefits. Note this means that we allow for shorter breaks from receipt of UI-benefits.

OA2.2 Linked Job-Seeker–Caseworker Data

A new data set from the Danish Agency for Labor Market and Recruitment (STAR) records the meetings between caseworkers and job seekers in Danish job centers from 2011-2019. This dataset contains information on the date, time, and type of all meetings. The data also records identifiers for the job seeker and caseworker attending each meeting, allowing us to link job seekers to their caseworkers. The job seeker identifiers can furthermore be linked to the registers in Statistics Denmark. The caseworker identifiers are administrative IDs and cannot be linked to the other registers.

This section describes the steps we take to clean and prepare the raw caseworker data. The goal is to end up with a data set that links unemployed job seekers (i) to the caseworker who participated in the job seeker's first face-to-face meeting, denoted her *realized caseworker*, and (ii) to her day-of-birth *predicted caseworker*.

Match UI Spells to Caseworker Meetings

First, we expand the sample of UI spells initiated from 2011-2018 with information about all caseworker meetings held during the UI-spell.[39] Table OA1 shows we are able to match around 50 percent of all UI spells (75 percent of all job seekers) to at least one caseworker meeting. This reflects that many job seekers exit unemployment before having a caseworker meeting: the table shows the average UI-spell length increases substantially, from 20 to 59 weeks, when we restrict to job seekers who had at least one caseworker meeting.

Timing, Type, and Contact of First Meeting

To ensure we do observe the *first* meeting for all job seekers, we make a three restrictions of the data. In particular, we only keep UI spells if the first registered meeting (i) takes place within the first 16 weeks of the UI spell (the 95th percentile), (ii) is a "regular meeting" or an "information meeting",[40], and if it (iii) takes place "in person" (i.e., not held via telephone or e-mail). Table OA1 shows these restrictions do not affect the average UI-spell length.

[39] Because UI spells are constructed based on weekly data, we do not know the exact start date of the UI spell. Therefore. we also include meetings held in the week prior to the start of the UI spell and meetings held in the week after UI spell ends.

[40] To be precise, we require the meeting type to be coded as "Jobsamtal", "Jobsamtale med deltagelse af A-kassen", "Informationsmøde", "Informationsmøde uden mødepligt"

Crosswalk of Caseworker Identifiers

To account for potential discontinuations of the caseworker identifiers, we also implement a crosswalk around months in which the vast majority (>95 percent) of caseworkers in a job center change identifiers. We provide more details below in Online Appendix OA2.3. Table OA1 shows the crosswalk reduces the number of unique caseworkers by around 300 (2 percent).

Identify the Realized Caseworker

Finally, we collapse the data to the first face-to-face meeting in the UI spell and denote the caseworker participating in this meeting the *realized caseworker*. For the vast majority (99 percent) of job seekers, the first face-to-face meeting simply corresponds to first meeting in the UI spell. For a minority of the job seekers. the first face-to-face meeting corresponds to the second meeting in the UI spell. The reason is that some job centers first invite job seekers to an information meeting, in which one caseworker meet with a group of job seekers, and only thereafter invites the job seeker to a face-to-face meeting with the day-of-birth allocated caseworker. Hence, if the first meeting is an information meeting, we use the second meeting in the UI spell (provided it was a face-to-face meeting). If neither the first nor the second meeting was a face-to-face meeting, we drop the job seeker from the sample. Table OA1 shows when we collapse the data to the first face-to-face meeting, we keep almost all job seekers (99 percent) but lose about 50 percent of the caseworkers. This finding may reflect that some job centers have caseworkers who only meet job seekers if they become long-term unemployed.

Identify the Day-Of-Birth-Predicted Caseworker

We now have a dataset that links job seekers to their *realized* caseworker. We link this dataset to the birth records for all job seekers and document the use of day-of-birth allocation rules in all job-center-unit-years. In other words, we now identify the *day-of-birth-predicted* caseworker for all job seekers. See details below in Online Appendix OA2.4. Table OA1 shows about 25% of the realized caseworkers end up as predicted caseworkers, and across all job-center-unit and years, the compliance rate between predicted and actual caseworkers is 43%.

Table OA1: Linked Job-seeker–Caseworker Data Restrictions

	#UI-spells	#Job seekers	#Job centers	#Caseworkers realized	#Caseworkers predicted	UI-spell (weeks)	Compliance (pct)
UI-spells from 2011-2018	**2,553,018**	**1,031,865**	**94**	**.**	**.**	**20**	**.**
- Who had at least one meeting	1,274,371	772,589	94	21,772	.	59	.
- Timing, type & contact of meeting	1,084,398	700,857	94	17,665	.	59	.
- Crosswalk caseworker identifiers	1,084,398	700,857	94	17,392	.	59	.
- Identify realized caseworker	1,082,080	699,646	94	8,637	.	35	.
- Identify predicted caseworker	1,072,535	694,381	94	8,637	2,149	35	43
Linked job-seeker-caseworker data	**1,072,535**	**694,381**	**94**	**8,637**	**2,149**	**35**	**43**

Note: Number of units (UI spells, job seekers, job centers, realized and predicted caseworkers) retained after each data restriction along with descriptives (average UI-spell length and the compliance between realized and predicted caseworker). Row (1) reports the statistics for all UI spells initiated from 2011-2018 in Denmark. Row (2) restricts to UI spells that had at least one caseworker meeting. Row (3) restricts to UI spells for whom the first meeting (i) took place within the first 16 weeks, (ii) was labelled as a "regular meeting" or an "information meeting", and (iii) was held "in person". Row (4) implements a crosswalk of caseworker identifiers. Row (5) restricts to UI spells for whom we can identify their realized caseworker. Row (6) restricts to UI spells for whom we can identify their day-of-birth predicted caseworker. Row (7) is identical to row (6) and summarizes the final linked job-seeker–caseworker data.

OA2.3 Crosswalk of Caseworker IDs

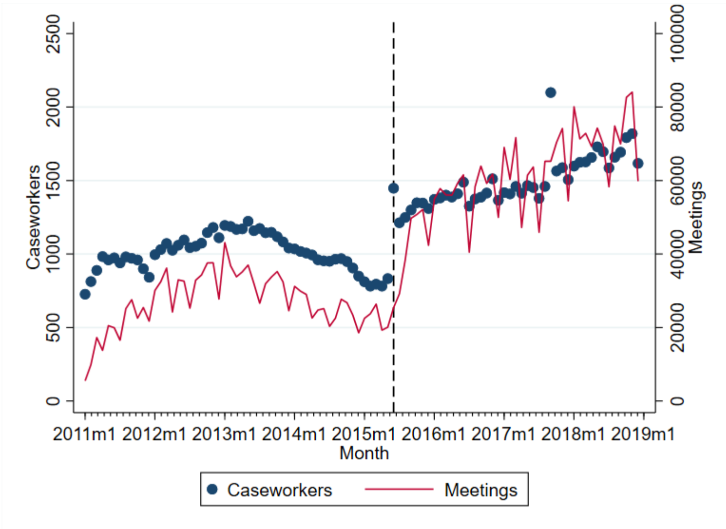
The caseworker identifiers in our data was generated by the IT system in the local job center,[41] and will be subject to discontinuations if a job center changes IT system. That is, a caseworker will have her identifier changed even though she remains employed in the same job center. We implement a crosswalk of the caseworker identifiers to account for such discontinuations.

Evidence of Discontinued Caseworker IDs

Figure OA1 shows the number of registered meetings in a given month (red lines) as well as the number of active caseworker IDs (blue dots). The black dotted line indicates the implementation of the 2015 employment reform, which increased the frequency of caseworker meetings for all job seekers. Besides a sharp increase in the number of meetings around the reform, the figure reveals a significant jump in the number of active caseworker IDs in June 2015 and September 2017. This jump could likely reflect the adoption of a new IT system.

[41] For this reason, the caseworker identifiers cannot be linked to the Danish registers.

Figure OA1: Caseworker IDs and Meetings



Let $caseworkers_{jt}$ represent all caseworker IDs that were active in job center j in month t. Further, let $exits_{jt}$ represent all caseworker IDs that were active in month t but not in month t+1, and let $entries_{jt}$ represent all caseworker IDs that were active in month t but not in t-1. The rate of caseworker churning in a given job center j and month t can then be measured as

$$churning_j = \frac{exits_{jt} + entries_{jt}}{caseworkers_{jt}}. \tag{56}$$

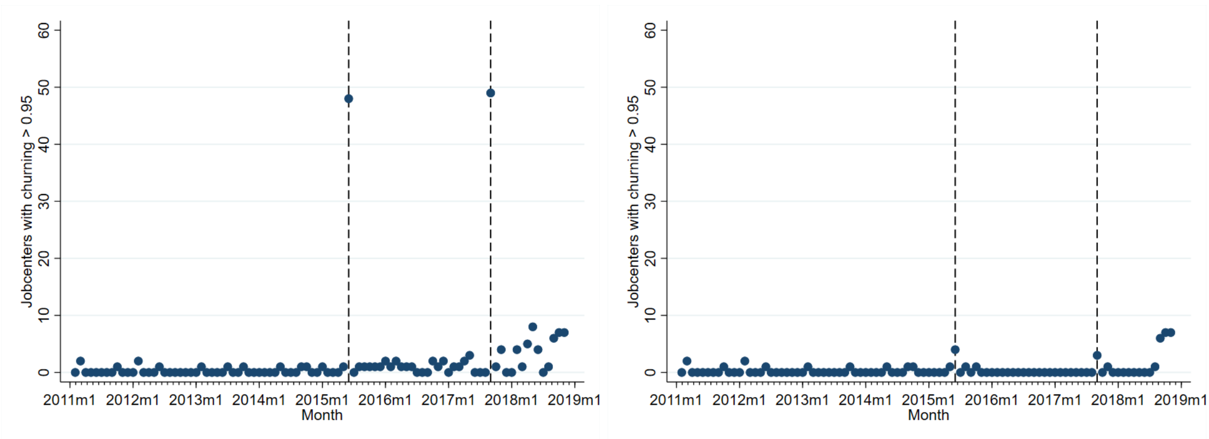
Note that, in principle, the churning rate could be > 1, if some caseworker IDs enter and exit in the same month (these IDs would count twice in the nominator but once in the denominator). We will ensure that the churning rate is bounded between 0 and 1 (by max letting an ID count once in the nominator). Lastly, when calculating the churning rate for a given job center j in a given month t, we require that

ii. the job center had at least two active caseworkers in period $t-1$, t and $t+1$

iii. the job center did not have churning $= 1$ in the previous period.
If churning = 1 in two consecutive months, the first churning event is kept.

Figure OA2a plots the number of job centers with a churning rate ≥ 0.95 in a given month. Consistent with Figure OA1, many job centers experience a churning rate ≥ 0.95 in June 2015 and September 2017. This finding could reflect that many job centers changed their IT system and therefore had to discontinue the old caseworker identifiers in these two months. Yet, the figure also shows a few job centers also experience a high churning rate in other months. Hence, some job centers might also have changed IT systems in other months.

Figure OA2: Job Centers with Churning ≥ 0.95



(a) Raw data

(b) Raw data + Crosswalk

We implement a crosswalk in all job-center $\times$ months in which the churning rate ≥ 0.95. The crosswalk algorithm is explained in detail below. Figure OA2b plots the number of job centers with a churning rate ≥ 0.95 in a given month *after* we apply this crosswalk. Clearly, the crosswalk reduces churning rates in job centers significantly in June 2015 and September 2017.

Crosswalk Algorithm for a Given Job Center and Break Month t

1. Restrict to meetings registered within +/- 6 months from t (and drop month t):

i. Require that job center have registered meetings in all months within window

ii. Keep job seekers who had at least one meeting before *and* after break month t.

2. Set up a transition matrix showing how job seekers who met with a given caseworker before the data break are distributed across caseworkers after the data break:

i. Rows represent caseworkers who were active in the month before the break, $t-1$. Columns represent caseworkers who were active in the month after the break, $t+1$.

ii. Find all job seekers who met with a given caseworker j in the *six months* before the break.

ii. Count the number meetings these job seekers have with any of the caseworkers in the *six months* after the data break.

3. Do mapping from caseworker before the break to caseworkers after the break:

i. Find the row-max in the transition matrix. That is, for a given caseworker before the break, what caseworker did most of her job seekers meet with after the data break.

ii. If multiple before-caseworkers map into the same after-caseworker, use the mapping based on the highest crosswalk.

iii. Mapping must be based on a crosswalk of at least x job seeker meetings. The threshold x is obtained in the following way:

- For a given job center, construct a transition matrix around some *placebo* break month (July 2016, because no job centers had churning=1 in this month).
- Keep only caseworkers who were active before *and* after this break.
- Off-diagonal elements in the placebo transition matrix inform us about the magnitude of crosswalk in a month *without* a break. Use the 95th percentile as x.

OA2.4 Day-Of-Birth Allocation Rules

We impute the day-of-birth allocation rules that have potentially been applied in all 94 job centers in Denmark over time. The imputation involves two steps. Hereafter, we do a visual inspection of the imputed day-of-birth rules to see what job-center-unit-years likely used this type of allocation rule.

Step 1: Identify Job-center Units

We identify job-center units over time. For each caseworker, we calculate the share of job seekers

- above age 30
- with an academic education (bachelor's, master's, or PhD)

We then check the distribution of the caseworker means in a given year: a bimodal distribution indicates the job center organizes the caseworker in units. Hereafter, we can then assign job seekers to a unit based on their individual characteristics (age or education). We assign caseworkers to a unit based on the characteristic of the majority of their assigned job seekers.

Step 2: Day-Of-Birth Distributions

For job centers without units

i. For each caseworker in a given job-center-year, count the total number of assigned job seekers with birthday on a given day of the month (1-31).

ii. Across caseworkers in a given job-center-year, the caseworker with the highest number of job seekers born on a specific day of the month (1-31) becomes the *predicted caseworker* for that day.

For job centers with units

i. For each caseworker in a given job-center-unit-year, count the number of assigned job seekers, who were born on a specific day of the month and belong to the same unit as the caseworker.

ii. Across caseworkers in a given job-center-unit-year, the caseworker with most job seekers born on a given day of the month (1-31) becomes the predicted caseworker for that day; that is, we use the mode.

Finally, we (visually) inspect the day-of-birth protocols and correct for differences in caseload size across caseworkers. For example. some job centers clearly use a day-of-birth rule but have one caseworker who deviates from the rule. If this caseworker also has a very large caseload, she will become the predicted caseworker for all job seekers (all day-of-birth). In this case, we place the "problematic" caseworker in her own unit, and recompute the day-of-birth protocol for the entire job center.

Step 3: Visual Inspection

We now have an imputed day-of-birth allocation rule for all job-center-unit-years. Hereafter, we do a visual inspection of the imputed day-of-birth allocation rules. In particular, we check whether the imputed day-of-birth protocol appears to follow some *system*, which is clearly and easily readable by the unaided eye. For example, can we see one caseworker is responsible for day 1-7, another caseworker is responsible for day 8-15, and so on.[42] We also consider whether the job-center-unit-year as a whole tend to *comply* with the imputed rule. We drop job-center-unit-years, when there no clear system in the imputed day-of-birth allocation rule exists or when compliance with the imputed rule is very low.

OA2.5 Outcomes

We base employment outcomes on the BFL register. This register covers the entire Danish population for 2008-2021 and is based on third-party reports to the Danish Tax Authorities. We use the variables ajo_loentimer and ajo_smalt_loenbeloeb to measure *any* working hours and earnings in a given month, and we define extensive-margin employment as months with non-zero working hours.

We use the DREAM register to separate non-supported employment from supported employment. In particular, we define non-supported employment as employment with no contemporaneous receipt of UI benefits and participation in activation programs (DREAM code cannot read 111, 115, 121,213, 214, 215, 216, 217, 218, 219, 231, 299) .

For robustness, we consider an alternative outcome definition: employment with no contemporaneous receipt of any types of public transfers (blank DREAM code).

OA2.6 Offshorability Index

We use the O*net database (version 26.1, November 2021) to construct an occupation-based measure of exposure to offshorability.[43] The O*net database contains a wide range of occupational descriptors, for example, "Task ID 4.A.3.a.3: Controlling machines and processes", which can be used to characterize an occupation. Each descriptor is associated with an importance and level scale. Following O*net guidelines, we construct one measure for each descriptor that incorporates both the level and the importance scale. Because the occupations in O*net are measured at the detailed onet-soc2019 level, and occupations in the Danish registers are measured at the coarser isco08 level, we apply a crosswalk and collapse the occupational characteristics from O*net to the isco08 level. Finally, we do a standardization such that the

[42]More sophisticated, yet still clear, systems could involve caseworkers dividing even and odd days between them. However, “block” systems are easier to detect.

[43]O*net data are downloaded from https://www.onetcenter.org/db_releases.html. We use four O*net datasets: Work activities, Work context, Abilities, and Skills.

occupational descriptors have mean 0 and standard deviation 1. Hereafter, we use the occupational descriptors to construct an index for the offshorability of a given isco08 code. Here, we follow the definition of offshorability from Autor and Dorn (2013). In particular, we use the following task items:

- Interpersonal interactions
  - 4.C.1.a.2.1 Face-to-face discussions
  - 4.A.4.a.4 Establishing and maintaining interpersonal relationships
  - 4.A.4.a.5 Assisting and caring for others
  - 4.A.4.a.8 Performing for or working directly with the public
  - 4.A.4.b.5 Coaching and developing others
- Proximity to specific work place location
  - 4.A.1.b.2 Inspecting equipment, structures, or material
  - 4.A.3.a.2 Handling moving objects
  - 4.A.3.a.3 Controlling machines and processes
  - 4.A.3.a.4 Operating vehicles, mechanized devices, or equipment
  - 4.A.3.b.4 Repairing and maintaining mechanical equipment (*0.5)
  - 4.A.3.b.5 Repairing and maintaining electronic equipment (*0.5)

Table OA2: Five Largest Low- and High-Risk Occupations

Low-risk occupations	High-risk occupations
1. Shop sales assistant	1. Cleaners and helpers in offices, hotels and other establishments
2. Child-care workers	2. General office clerks
3. Home-based personal care workers	3. Clerical support workers
4. Pedagogical work	4. Commercial sales representatives
5. Carpenters and joiners	5. Mail carriers and sorting clerks

Note: Job seekers in the analysis sample are divided into low- and high risk according to the offshorability of their previous occupation (using the 75th percentile in the job seeker distribution). The table lists the five largest low- and high-risk occupations in the sample, that is, the occupations held by most low- and high-risk job seekers.

OA2.7 Imputation of Occupations

We use AKM data to identify the previous occupation of all job seekers in our sample. AKM data allow us to see the yearly occupation of all employed individuals in Denmark from 1995-2017.[44] We use 4-digit isco codes to measure occupations. To account for the revision of isco codes in 2010, we apply an m:1 conversion key from isco88 to isco08 codes, such that we work with isco08 codes in all years. Following Hummels et al. (2014), we impute missing isco08-codes whenever possible. In particular, we merge workplace identifiers[45] onto teh sample and use them to construct employment spells for the individual. If the individual has a missing isco08 code within an employment spell, we extrapolate from years in which the isco08 was not missing.[46] Finally, we identify the previous occupation for the job seekers in our sample. Here, we use the *typical* occupation over the job seeker's career, that is, the occupation in which she had most experience over the career (prior to the start of the UI spell).[47]

OA2.8 Validation of Job Plans

For information on training-program *assignments*, we rely on the individual job plans that case-workers prepare for job seekers. The job-plan registrations are collected by the Danish Agency for Labor Market and Recruitment (STAR) and gathered in the so-called *Plan* data register (PLAN). Since 2015, this register has been the source of two well-known sources of information about ALMPs in the Danish labor market:

- Macro-level data: *jobindsats.dk*
- Micro-level data: *Danish Register for Evaluation of Marginalization (DREAM)*.

Importantly, whereas PLAN shows ALMP *assignments*, jobindsats.dk and DREAM show *participation* in ALMPs. Hence, PLAN data also include all the programs that were assigned but that the job seeker never participated in.

To the best of our knowledge, we are the first to use PLAN data for research on ALMPs, and this section serves as a validation of the dataset. First, we introduce the different types of ALMP

[44] To ensure we focus on employed individuals, we drop individuals in years when they have a missing or unknown industry code

[45] We use ARBNR ("arbejdsstednummer") from RAS, which is available from 1995 onwards.

[46] We require non-missing isco08-codes to be similar within an employer.

[47] In case of a tie, we use the lowest isco08 code.

assignments registered in PLAN data. Second, we benchmark PLAN data up against ALMP participation in the DREAM register. Third, to understand the content of a classroom-training assignment in PLAN data, we benchmark PLAN data against two Danish registers, UDDA and VEUV, which contain information about enrollment in ordinary education (degree courses) and adult and continuing training (non-degree courses). The UDDA and VEUV mainly cover publicly provided education and thus provide a lower bound on the actual educational activity of job seekers.

OA2.8.1 PLAN Data

PLAN data contain information about all caseworker-*assigned* training programs for unemployed job seekers, including the start and end date of the programs and the status of the programs (e.g., cancelled or completed).

We use PLAN data to distinguish between two broad classes of ALMPs: classroom training and job training.[48] Each class represents a composite of the activation categories used on *jobindsats.dk* and in *DREAM*. Table OA3 shows classroom training primarily includes "Ordinær uddannelse" (31%) and "Øvrig opkvalificering og vejledning" (66%), whereas job training primarily covers "Virksomhedspraktik" (73%) and "Løntilskud" (21%).[49] In section OA2.8.2, we show how the activation categories in PLAN data match the activation categories in DREAM.

Table OA3: Training Classes and Activation Categories in PLAN Data

	Total assignments	Share (pct)
A: Classroom training	119,826	100
Opkvalificering og vejledning	79,017	66
Ordinær uddannelse	36,930	31
Andet	3,879	3
B: Job training	67,411	100
Virksomhedspraktik	48,954	73
Løntilskud	14,337	21
Andet	4,120	6

Note: The analysis sample of job seekers is enriched with all classroom- and job-training assignments in PLAN data during the first 12 months after job loss. Because a job seeker may be assigned to multiple programs, the same job seeker may appear in multiple rows. Panel A decomposes total classroom- (job-) training assignments into three activation categories. Column(1) reports total assignments, and Column (2) reports the share of assignments in a given activation category relative to total classroom- (job-) training assignments.

[48]This distinction relies on two variables in PLAN data, course_id and job_id.
[49]This distinction relies on the variable *aktivtyp* in Plandata

PLAN data also contain information on more disaggregated types of training programs.[50] Table OA4 and OA5 report the different types of classroom- and job-training programs registered in PLAN data for job seekers in our sample. There are two key take aways from this table. First, job-training programs include both public and private programs. For example, about one third of all "Virksomhedspraktik" is public. This observation is important for the interpretation of our results, because the existing literature has found different employment effects of public and private programs.

Second, the vast majority of classroom-training programs are registered as "Øvrige forløb" (65%) or "AMU" (25%). The latter type of program, "AMU," represent vocational training courses, which typically take place at dedicated facilities and target specific skills. The former type, "Øvrige forløb," is a rather generic category, and the extent to which it represents actual education or job-search-assistance (JSA) courses is not clear from the title alone. In sections OA2.8.3 and OA2.8.4, we shed more light on the actual education content of classroom training.

Table OA4: Job-Training Types

	Total Assignments	Share (pct)	Duration (days)	Timing (weeks)
Virksomhedspraktik, privat	32,378	48	30	23
Virksomhedspraktik, offentlig	16,572	25	29	23
Løntilskud, offentlig	8,030	12	104	25
Løntilskud, privat	6,234	9	94	27
Rotationsvikar	2,762	4	154	28
Residual, fx nytteindsats	730	1	332	27
Voksenlærlig og elev	705	1	691	29

Note: The table is based on all job seekers in the analysis sample who were assigned to job training within the first 12 months of unemployment. The table lists the specific types of job-training programs that these job seekers are assigned to.Note a job seeker may be assigned to multiple program types (i.e., she may appear in multiple rows) and even to multiple activities of the same type (i.e., she may appear multiple times in the same row). Column (1) reports the number of assignments to a given program type. Column (2) relates the number of program-specific assignments to total job-training assignments. Column (3) reports the average duration of the program, measured in days. Column (4) reports the average timing of the program, measured in weeks relative to the UI-spell start.

[50] The two variables *course_id* and *job_id* show different types of classroom and job training programs, respectively. We enrich these variables with formats available at STARWIKI. *job_id*: https://starwiki.atlassian.net/wiki/spaces/FYS/pages/48890079/JobOrderTypeIdentifier *course_id*: https://starwiki.atlassian.net/wiki/spaces/FYS/pages/48890020/CourseTypeIdentifier

Table OA5: Classroom Training Types

	Total Assignments	Share (pct)	Duration (days)	Timing (weeks)
Øvrige forløb	77,674	65	34	21
AMU	29,445	25	19	21
Erhvervsuddanenlser	2,731	2	103	24
Andet tilbud[a]	2,161	2	87	21
AVU	2,154	2	68	22
KVU, MVU, LVU	1,388	1	80	23
Privat udbudte kurser	1,112	<1	32	23
Danskundervisning	995	<1	159	19
Realkompetencevurdering	890	<1	6	19
Mentor	471	<1	99	29
Læse-, skrive- og regnekursus	381	<1	57	19
SOSU uddannelser	263	<1	134	24
Residual, fx mestring af sygdom	161	<1	44	32

Note: The table is based on all job seekers in the analysis sample who were assigned to classroom training within the first 12 months of unemployment. The table lists the specific types of classroom-training programs that these job seekers are assigned to. Note that a job seeker may be assigned to multiple program types (i.e., she may appear in multiple rows) and even to multiple activities of the same type (i.e., she may appear multiple times in the same row). Column (1) reports the number of assignments to a given program type. Column (2) relates the number of program-specific assignments to total classroom-training assignments. Column (3) reports the average duration of the program, measured in days. Column (4) reports the average timing of the program, measured in weeks relative to the UI-spell start.

OA2.8.2 PLAN vs. DREAM

Whereas PLAN data contain information on *assignments* to training, DREAM contains information about *participation* in training during unemployment. With one exception, the activation measures in DREAM have been based on PLAN data since 2015.[51] For this reason, we would expect a high correspondence between activation assignments in PLAN data and activation participation in DREAM.

Table OA6 shows the correspondence between training assignments in PLAN data and participation in DREAM. A given cell in this table reports the share of job seekers, who were assigned to some activation category in PLAN data (row), and end up participating in some activation category in DREAM (column). Both assignment and participation must be within the first 12 months after job loss. Because job seekers may be assigned to multiple types of activation, the columns in a given row do not sum to 100.

The table shows a clear mass concentration in the diagonals of the first four columns. For the vast majority of job seekers, assignment to some activation category in PLAN data is accom-

[51] Importantly, "6-ugers selvvalgt" in DREAM is based on a different data source (TASS-systemet), and when determining what an individual did in a given week, "6-ugers selvvalgt" is prioritized above other activation measures. Hence, the individual can be in "ordinær uddannelse" according to PLAN data but in "6-ugers selvvalgt" according to DREAM.

panied by participation in a similar activation category in DREAM. For example, 87 percent of the job seekers, who were assigned to "1. Opkval. og vejl." end up participating in the same category in DREAM. Importantly, the fact that the diagonals do not sum to 100% does not *only* reflect that some assignments never lead to participation. It also reflects that DREAM operates with the additional activation category "6-ugers selvvalgt," which dominates the other categories. That is, some of the job seekers who are assigned to "1. Opkval. og vejl." may be classified in DREAM as participating in "6-ugers selvvalgt" (see footnote 51).

Table OA6: Activation in PLAN and DREAM

	Share participating in DREAM (pct)				
Assignment in Plan	1. Opkval. og vejl.	2. Ordinær udd.	3. Virk. praktik	4. Løntilskud	5. 6-ugers selvvalgt
1. Opkval. og vejl.	**87**	10	25	7	15
2. Ordinær uddannelse	32	**75**	24	7	**31**
3. Virk. praktik	36	11	**94**	18	14
4. Løntilskud	35	9	60	**93**	13
5. Andet	31	12	37	9	11

Note: Each row represents all job seekers in the analysis sample, who are assigned to a given activation type within the first 12 months after job loss. The columns report the share of these job seekers who end up participating in a given activation category in DREAM (participation within the first 12 months after job loss). Because job seekers may be assigned to multiple programs, the same job seeker can appear in multiple columns. That is, the columns in a row do not sum to 100. The activation categories in the columns are based on the following DREAM codes: "1. Vejledning og opkvalificering" (213), "2. Ordinær uddannelse" (214), "3. Virksomhedspraktik" (217, 218), "4. Løntilskud" (215, 216), "5. 6-ugers selvvalgt uddannelse" (231). The activation categories in the rows are based on the variable *aktivtyp* in PLAN data, constructed for the macro data on Jobindsats.dk.

OA2.8.3 PLAN vs. UDDA

The *Education register* (UDDA) contain information about enrollment in ordinary education, which we denote *Degree Courses*. Every year on September 30, Statistics Denmark takes *stock*: they register if an individual currently is *enrolled* in an education and register the day the individual enrolled in that education.[52]

To get an indication of the share of degree courses in classroom training, we compare classroom-training assignees in PLAN data with *enrollments* in UDDA. However, note this comparison likely represents a *lower bound* of the share of degree courses in PLAN data. First of all, because some individuals will be assigned to but never enroll in a degree course.For example, they may find a job upon assignment and therefore never enroll in education. Second of all, due to the nature of the UDDA register (the stock as of September 30), we cannot be sure

[52]Statistics Denmark also takes stock of whether the individual has *completed* an education and register the day the individual obtained her degree. That is, they do not record the day the individual enrolled, only the completion date.

to see all enrollments in degree courses. In particular, we lack information about some enrollments in courses with a short duration. For example, if the individual starts *after* September 30 of year t and the course ends *before* September 30 in the following year $t + 1$, she will never appear as "enrolled" in the UDDA register (i.e., the enrollment date is unobservable to us).

Table OA7 reports the share of assignees to a specific classroom-training program who also end up enrolling in an ordinary education in UDDA. Column (3) shows 4 percent of job seekers assigned to any classroom training enroll in a degree course within the first 12 months from the UI spell start. Column (4) shows this share increases to 5 percent if we consider enrollments within the first 15 months from the UI spell start. In column (5)-(6), the enrollment rates are weighted by the length of the assigned training programs. Column (5) suggests degree courses can account for 9 percent of the days spent in classroom training.

Table OA7: Classroom-Training Assignees Enrolled in UDDA

	#Program	>1 program	Enrollment (pct)		Length-weighted enrollment (pct)	
	assignees	(pct)	month 0-11	month 0-14	month 0-11	month 0-14
Classroom training	62,163	13	4	5	9	10
Øvrige forløb	48,764	15	4	5	4	5
AMU	13,841	39	3	4	4	5
Erhvervsuddannelser	1768	48	28	31	52	56
Andet tilbud	1669	54	3	4	3	4
AVU	1588	53	6	7	9	11
KVU, MVU, LVU	947	45	11	12	37	38
Danskundervisning	574	48	2	2	2	3
Realkompetencevurdering	477	61	16	19	17	20
Privat udbudte kurser	417	51	2	2	1	1
Mentor	341	65	2	1	1	1
Læse-, skrive- og regnekursus	255	59	7	9	9	11
SOSU uddannelser	211	42	42	44	71	72
Residual	97	60	1	2	2	3

Note: The table is based on all job seekers in the analysis sample (restricted to UI- spells initiated from 2012-2016), who were assigned to classroom training within the first 12 months of unemployment. The table lists the specific *types* of classroom-training programs these job seekers are assigned to. Because a job seeker may be assigned to multiple types of classroom training programs, the same job seeker may appear in multiple rows. Column (1) reports total job seekers assigned to a given type of classroom-training program during her UI spell. Column (2) reports the share of job seekers who were assigned to multiple program types, for example, both assigned to "Øvrige forløb" and "AMU". Columns (3)-(4) report the share of all classroom assignees who were enrolled in UDDA. Columns (5)-(6) report the share of all classroom assignees who were enrolled in UDDA, weighted by total length of the assigned training programs. For columns (3) and (5), the UDDA course must start within month 12, whereas for column (4) and (6) the UDDA course must start within month 15 of the UI-spell start.

OA2.8.4 PLAN vs. VEUV

The *Course participant register* (VEUV) contains information about enrollments in publicly co-funded adult and continuing training courses that lead to formal qualifications. We denote these courses *Non-degree courses*. We use this register to check whether classroom-training assignees in PLAN data (job seekers assigned to classroom training within the first 12 months of unemployment) become enrolled in a non-degree course within the first 12 (15) months from the UI-spell start.

Because we only have VEUV data until September 2018, we can only observe course enrollments within all 12 (15) months after job loss for job seekers who initiated their UI-spell no later than September (April) 2017. For simplicity, we restrict our sample to UI-spells initiated from 2012-2016.

Table OA8 summarizes the correspondence between classroom-training assignments in PLAN data and non-degree course enrollments in VEUV. Column (3) shows 41% of all classroom training assignees enroll in a non-degree course during the first 12 months after the UI-spell start. Column (4) shows this share increases to 43% if we consider enrollment in courses that start within the first 15 months after the UI-spell start. This finding suggests non-degree courses constitute a relatively large share of classroom training. In Column (5)-(6), the enrollment rates are weighted by the length of the assigned training programs. Column (5) suggests non-degree courses can account for 45% of the total days spent in classroom training.

Table OA8 also reports the course enrollment rate for assignees to specific classroom-training programs. We highlight two insights. First, the course enrollment rate is as high as 87% for assignees to "AMU" (row 3, column3). This finding is reassuring because "AMU" - unlike "Øvrige forløb" - is a category known to us. "AMU" refers to adult vocational training courses, and any enrollment in these courses is registered in VEUV. Hence, when we see that (100-87=) 13% of "AMU" assignees *never* enroll in VEUV, we have an indication of the extent to which enrollments follow assignments. In other words, we may take 13% as a proxy for the share of classroom assignees who never enroll in a program.

Second, about one third of assignees to "Øvrige forløb" enroll in a VEUV course within the first 12 months of unemployment (row 2, column 3). Importantly, this share can*not* (only) be explained by job seekers being assigned to multiple types of programs.[53] This finding suggests a significant share of "Øvrige forløb" is non-degree courses as registered in VEUV.

[53] A concern would be that assignment to "Øvrige forløb" is always accompanied by assignment to "AMU," which has a high course-enrollment rate. However, the vast majority of assignees to "Øvrige forløb" are *not* assigned to other types of programs. Hence, the high course-enrollment rate for assignees to "Øvrige forløb" cannot (only) be driven by assignments to other programs.

Table OA8: Classroom-Training Assignees Enrolled in VEUV

	#Program assignees	>1 program (pct)	Enrollment (pct)		Length-weighted enrollment (pct)	
			month 0-11	month 0-14	month 0-11	month 0-14
Classroom training	62,163	13	41	43	45	47
Øvrige forløb	48,764	15	32	35	32	34
AMU	13,841	39	87	88	88	89
Erhvervsuddannelser	1768	48	54	56	42	44
Andet tilbud	1669	54	28	30	30	32
AVU	1588	53	77	79	82	83
KVU, MVU, LVU	947	45	64	65	57	57
Danskundervisning	574	48	85	86	88	89
Realkompetencevurdering	477	61	58	61	77	78
Privat udbudte kurser	417	51	34	36	38	40
Mentor	341	65	23	27	20	25
Læse-, skrive- og regnekursus	255	59	80	83	93	94
SOSU uddannelser	211	42	55	56	35	36
Residual	97	60	19	21	19	21

Note: The table is based on all job seekers in the analysis sample (restricted to UI spells initiated from 2012-2016) who were assigned to classroom training within the first 12 months of unemployment. The table lists the specific *types* of classroom-training programs these job seekers are assigned to. Because a job seeker may be assigned to multiple types of classroom-training programs, the same job seeker may appear in multiple rows. Column (1) reports total job seekers assigned to a given type of classroom-training program during her UI spell. Column (2) reports the share of job seekers who were assigned to multiple program types, for example, both assigned to "Øvrige forløb" and "AMU". Columns (3)-(4) report the share of all classroom assignees who were enrolled in VEUV. Columns (5)-(6) report the share of all classroom assignees who were enrolled in VEUV, weighted by total length of the assigned training programs. For Columns (3) and (5), the VEUV course must start within month 12, whereas for Columns (4) and (6), the VEUV course must start within month 15 of the UI-spell start.

Table OA9 lists the 10 most popular adult vocational training courses that classroom training assignees enrolls in. As is evident, these popular courses target specific skills, for example, accounting (6, 7, 10), operating a computer (1, 4, 5, 8, 9) or a machine (2). [54]

Table OA9: Popular Courses

Titel	Enrollments
1. Anvendelse af regneark til enkle beregninger	2,258
2. Gaffeltruck certifikatkursus B, 7 dage	1,881
3. Vurdering af basale færdigheder	1,726
4. Indskrivning og formatering af mindre tekster	1,536
5. Anvendelse af præsentationsprogrammer	1,525
6. Daglig registrering i et økonomistyringsprogram	1,339
7. Placering af resultat og balancekonti	1,338
8. Brug af pc på arbejdspladsen	1,307
9. Informationssøgning på internettet til jobbrug	1,257
10. Bilagsbehandling med efterfølgende kasserapport	1,219

Note: The table reports the 10 vocational training courses that classroom-training assignees most frequently enroll in. Classroom-training assignees are based on our analysis sample (restricted to UI-spells initiated from 2012 to 2016); assignment to classroom training must be within the first 12 months of unemployment. Course enrollments are based on VEUV; the course must start within the first 12 months of unemployment.

[54] With one exception, the same courses are in the top 10 if we focus on assignees to "Øvrige forløb" or "AMU". For assignees to "Øvrige forløb," "Design og Automatisering af regneark" is in the top 10 instead of "Informationssøgning til jobbrug." For assignees to "AMU," "Vejen som arbejdsplads" is in the top 10 instead of "Bilagsbehandling med efterfølgende kasserapport."

OA2.8.5 Back-of-the-Envelope Calculations for the Content of Classroom Training

What share of classroom training is made up of job-search-assistance (JSA) courses? We now present some simple calculations that put an *upper bound* on this share.

The calculations build on the idea that (i) classroom training includes three types of courses (JSA courses, degree courses and non-degree courses) and that (ii) some assignments never lead to enrollment. Building on the previous sections, we have crude proxies for the enrollment rate into degree courses (UDDA) and non-degree courses (VEUV). Table OA10 reports these numbers as well as the share of classroom assignees who enroll in minimum one of these two courses, denoted *Any Education Course*. We also have a crude proxy for the non-enrollment rate of classroom-training assignments, if we are willing to assume the non-enrollment rate for "AMU" can be extrapolated to all classroom-training programs. This proxy is also reported in Table OA10.

Based on these proxies, we calculate the residual share of classroom-training assignees: job seekers assigned to classroom training but who (i) do not have a registered education enrollment and (ii) may not have enrolled in any classroom-training program. Because we likely do not capture all education enrollments, we may interpret the residual as an *upper bound* of the share of classroom assignees who enroll in JSA courses.[55] Table OA10 reports the proxies and the computed residual, which suggests *maximum* 43% of classroom assignees enroll in JSA courses. The table also suggests JSA courses account for *no more* than 38% of total classroom-training days.

Table OA10: Content of Classroom Training

	(1)	(2)	(3)	(4)	(5)
		Registered enrolment			
	Non-enrollment	Degree Course	Non-degree Course	Any Education Course	Residual
Share of classroom assignees	13	4	41	44	43
Share of classroom training days	12	9	45	51	38

Note: Column (1) represents the Non-enrollment Rate. These are based on the share of "AMU" assignees who do not enroll in VEUV (Columns (3) and (5) of Table OA8). Column (2) represents the enrollment rates in Degree Courses. These are based on the share of classroom assignees who enroll in UDDA (Columns (3) and (5) of Table OA7). Column (3) represents the enrollment rate in Non-Degree Courses. These are based on the share of classroom assignees who enroll in VEUV (Columns (3) and (5) of Table OA8). Column (4) represents the enrollment rate in Any Registered Education. This is the share of classroom assignees who enroll in at least one of the two categories in Columns (2) and (3). Column (5) is the residual category, which is calculated as 100 - (Column (1) + Column (4)).

[55] For example, because UDDA reports the stock of individuals enrolled as of September 30 in a given year, we will not capture all enrollments in degree courses, for example, if they are initiated after September 30 of year t.

OA3 Estimation

OA3.1 Bootstrapping standard errors

We bootstrap standard errors on a given statistic, b, using a "Bayesian Bootstrap procedure":

1. Assign each observation in the sample a random and strictly positive weight. Obtain these weights by (i) drawing numbers from a uniform distribution and (ii) normalizing by the sum, $w_i = \frac{u_i}{\sum_i u_i}$.

2. Recompute the statistic with these weights.

3. Do N iterations of 1-2, such that we obtain an entire distribution of the relevant statistic, b. Compute standard errors based on this distribution:

$$se(b) = \sqrt{\frac{1}{N} \times \sum_{n}^{N} (b_n - \bar{b})^2}. \tag{57}$$

OA3.2 Complier Calculus

Given monotonicity, the population of job seekers can be split into three types: compliers, always-, and never-takers. *Compliers* are job seekers who would have received a different classroom-training decision had they been assigned to a different caseworker, namely, job seekers who (i) start in training k if assigned to a k-inclined caseworker and (ii) do not start in training k if assigned to a k-restrained caseworker. *Never-takers* are job seekers who do not start in training k irrespective of the encouragement of the caseworker. That is, even if assigned to the most k-inclined caseworker, never-takers do not start in training k. *Always-takers* are job seekers who do start in training k irrespective of the encouragement of their caseworker. That is, even if assigned to the most k-restrained caseworker, always-takers do start in training k.

Share of Compliers, Always-, and Never-Takers

In the judge-leniency literature, the share of compliers, always-, and never-takers are often calculated in a setting with one treatment and a continuous instrument (Bhuller et al., 2020; Dahl et al., 2014). The idea is to approximate a “strict” and a “lenient” judge with some percentile of the residualized instrument,[56] and then re-scale the first-stage coefficient by the proxies for lenient and strict caseworkers.

Given extended monotonicity - and non-correlation of the instruments conditional on job-center unit and year fixed effects[57] - the approach from the single-treatment setting can easily be extended to our *multiple-treatment* setting. In particular, we take the following approach to identify the share of compliers, always-, and never-takers for treatment k:

1. Residualize training intervention D_{ki} and the own-instrument Z_{ki} on job-center-unit-year fixed effects, $\delta_{j(i)}$, and the cross-instrument Z_{-ki}. Add the unconditional mean and denote the residualized intervention and own-instrument $D^r_{k,i}$ and $Z^r_{k,i}$, respectively.

2. Identify a k-restrained and k-inclined caseworker as the 1st and 99th percentile of $Z^r_{k,i}$. Denote these percentiles $Z_k^{r,min}$ and $Z_k^{r,max}$, respectively.

3. Estimate the first-stage regression

$$D^r_{k,i} = \pi_{10} + \pi_{11} Z^r_{k,i} + u_{k,i}. \tag{58}$$

4. Predict treatment k for job seekers assigned to a k-restrained and -inclined caseworker

$$\hat{D}_k^{r,min} = \hat{\pi}_{10} + \hat{\pi}_{11} Z_k^{r,min} \tag{59}$$

$$\hat{D}_k^{r,max} = \hat{\pi}_{10} + \hat{\pi}_{11} Z_k^{r,max} \tag{60}$$

5. Calculate the share of compliers, always-, and never-takers as

$$P_c = \hat{D}_k^{r,max} - \hat{D}_k^{r,min} = \hat{\pi}_{11} \times (Z_k^{r,max} - Z_k^{r,min}) \tag{61}$$

$$P_a = \hat{D}_k^{r,min} = \hat{\pi}_{10} + \hat{\pi}_{11} Z_k^{r,min} \tag{62}$$

$$P_n = 1 - P_c - P_a. \tag{63}$$

Note the above method is equivalent to a method in which we only residualize based on job-center-unit-year fixed effects in step 1:

[56] The instrument is residualized by fixed effects representing the level at which randomization occurs

[57] Because the two instruments are uncorrelated, the distribution of the own-instrument, Z_{ki}, conditional on job-center unit and year fixed effects, $\delta_{j(i)}$, is largely invariant to controlling for the cross-instrument, Z_{-ki}, that is, $Z_{ki}|\delta_{j(i)} \sim Z_{ki}|\delta_{j(i)}, Z_{-ki}$.

1. Residualize training intervention D_{ki} and the instruments, Z_{ki} and Z_{-ki}, on job-center-unit-year fixed effects, $\delta_{j(i)}$. Add the unconditional mean and denote the residualized variables $D^r_{k,i}$, $Z^r_{k,i}$, and $Z^r_{-k,i}$,

2. Identify a k-restrained and k-inclined caseworker as the 1st and 99th percentile of the own-instrument $Z^r_{k,i}$. Denote these $Z_k^{r,min}$ and $Z_k^{r,max}$, respectively. A restrained and inclined caseworker, $Z_k^{r,min}$ and $Z_k^{r,max}$, will have approximately the same value of the cross-instrument, if the two instruments are uncorrelated. That is, $Z_{-k}^{r,max} - Z_{-k}^{r,max} \sim 0$.

3. Estimate the first-stage regression. Due to extended monotonicity, the coefficient on the cross-instrument, π_{12}, is close to zero:

$$D^r_{k,i} = \pi_{10} + \pi_{11} Z^r_{k,i} + \pi_{12} Z^r_{-k,i}. \tag{64}$$

4. Predict treatment k for job seekers assigned to a k-restrained and -inclined caseworker:

$$\hat{D}_k^{r,max} = \hat{\pi}_{10} + \hat{\pi}_{11} Z_k^{r,min} + \underbrace{\hat{\pi}_{12}}_{\sim 0} Z_{-k}^{r,min} \tag{65}$$

$$\hat{D}_k^{r,min} = \hat{\pi}_{10} + \hat{\pi}_{11} Z_k^{r,max} + \underbrace{\hat{\pi}_{12}}_{\sim 0} Z_{-k}^{r,max} \tag{66}$$

5. Calculate the share of compliers, always-, and never-takers

$$P_c = \hat{\pi}_{11} \times (Z_k^{r,max} - Z_k^{r,min}) + \underbrace{\hat{\pi}_{12}}_{\sim 0} \times \underbrace{(Z_{-k}^{r,max} - Z_{-k}^{r,min})}_{\sim 0} \tag{67}$$

$$P_a = \hat{\pi}_{10} + \hat{\pi}_{11} Z_k^{r,min} + \underbrace{\hat{\pi}_{12}}_{\sim 0} Z_{-k}^{r,min} \tag{68}$$

$$P_n = 1 - P_c - P_a. \tag{69}$$

Potential outcomes

Building on the population shares obtained above, we can estimate the potential outcomes for compliers, always-, and never-takers with respect to a specific treatment k:

1. Residualize the outcome, Y_{it}, and instruments, Z_{1i} and Z_{1i}, on job-center-unit-year fixed effects. Add the unconditional mean and call the residualized variables, $Y^r_{i,t}$, $Z^r_{k,i}$, and $Z^r_{-k,i}$.

2. Identify a k-restrained and k-inclined caseworker as the 1st and 99th percentiles of the own-instrument $Z^r_{k,i}$. Denote these $Z^{r,min}_k$ and $Z^{r,max}_k$, respectively. A restrained and an inclined caseworker, $Z^{r,min}_k$ and $Z^{r,max}_k$, will have approximately the same value of the cross-instrument, if the two instruments are uncorrelated. That is, $Z^{r,max}_{-k} - Z^{r,max}_{-k} \sim 0$

3. Estimate the reduced form *conditional on treatment* and predict the outcome for job seekers assigned to a k-restrained and k-inclined caseworker:

$$Y^r_{i,t} = \alpha_0 + \alpha_k Z^r_{k,i} + \alpha_{-k} Z^r_{-k,i}, \quad \text{if} \quad D_{k,i} = 1 \tag{70}$$

$$\hat{Y}_t(D_k = 1, Z^{r,min}_{k,i}, Z^{r,mean}_{-k,i}) = \hat{\alpha}_0 + \hat{\alpha}_k Z^{r,min}_{k,i} + \hat{\alpha}_{-k} Z^{r,min}_{-k,i} = \hat{Y}^{always}_t \tag{71}$$

$$\hat{Y}_t(D_k = 1, Z^{r,max}_{k,i}, Z^{r,max}_{-k,i}) = \hat{\alpha}_0 + \hat{\alpha}_k Z^{r,max}_{k,i} + \hat{\alpha}_{-k} Z^{r,max}_{-k,i}. \tag{72}$$

4. Estimate the reduced form *conditional on non-treatment* and predict the outcome for job seekers assigned to a k-restrained and k-inclined caseworker:

$$Y^r_{i,t} = \alpha_0 + \alpha_k Z^r_{k,i} + \alpha_{-k} Z^r_{-k,i}, \quad \text{if} \quad D_{k,i} = 0 \tag{73}$$

$$\hat{Y}_t(D_k = 0, Z^{r,min}_{k,i}, Z^{r,min}_{-k,i}) = \hat{\alpha}_0 + \hat{\alpha}_k Z^{r,min}_{k,i} + \hat{\alpha}_{-k} Z^{r,min}_{-k,i} \tag{74}$$

$$\hat{Y}_t(D_k = 0, Z^{r,max}_{k,i}, Z^{r,max}_{k,i}) = \hat{\alpha}_0 + \hat{\alpha}_k Z^{r,max}_{k,i} + \hat{\alpha}_{-k} Z^{r,max}_{-k,i} = \hat{Y}^{never}_t. \tag{75}$$

5. Because always-takers start in training even if they are assigned to the most k-restrained caseworker, (71) identifies the outcome for always-takers.

6. Because never-takers do *not* start in training even if they are assigned to the most k-inclined caseworker, (75) identifies the outcome for never-takers.

7. To get the outcome for compliers if they do not start in training, exploit that job seekers who do not start in training are a mix of never-takers and non-encouraged compliers:

$$\hat{Y}^{com}_{0,t} = \hat{Y}_t(D_k = 0, Z^{max}_k > Z^{min}_k) \tag{76}$$

$$= \frac{P_c + P_n}{P_c} \times \hat{Y}_t(D_k = 0, Z^{r,min}_{k,i}, Z^{r,min}_{-k,i}) - \frac{P_n}{P_c} \times \hat{Y}_t(D = 0, Z^{r,max}_{k,i}, Z^{r,max}_{-k,i}). \tag{77}$$

8. To get the outcome for compliers if they do start in training, exploit that job seekers who

start in training are a mix of always-takers and encouraged compliers:

$$\hat{Y}^{com}_{1,t} = \hat{Y}_t(D_k = 1, Z^{max}_k > Z^{min}_k) \tag{78}$$

$$= \frac{P_c + P_a}{P_c} \times \hat{Y}_t(D_k = 1, Z^{r,max}_{k,i}, Z^{r,max}_{k,i}) - \frac{P_a}{P_c} \times \hat{Y}_t(D_k = 1, Z^{r,min}_{k,i}, Z^{r,min}_{-k,i}).. \tag{79}$$