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ABSTRACT

This paper examines if active labor market 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 by 29% two years after initial job loss of compliers. 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 estimating marginal treatment effects, we conclude that 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 2% 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 training and private and public on-the-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 on-the-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-analysis 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. The caseworker-tendency instruments generate natural experiments to evaluate the heterogeneous causal effects of different ALMPs within a coherent framework. Natural experiments 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 heterogeneous 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. In particular, we provide evidence that the instruments satisfy an *extended monotonicity* condition by which tendencies for classroom training do not affect the assignment to on-the-job training and vice versa. The extended monotonicity allows us to identify heterogeneous treatment effects in our setting with multiple unordered treatments.

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% more hours of employment two years after the initial job loss of instrument compliers. By contrast, we do not have the statistical power to document effects of on-the-job training.

[2]McCall et al. (2016) identify only one published study using natural experiments: 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 that uses a caseworker-tendency 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.

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.[3] We find that the stark differences between the IV and OLS estimates (even with a rich set of controls) reflect a 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).

Third, we study the dynamics of how assignment to classroom training relates to employment at different time horizons. We decompose the effects of classroom training on employment into "threat effects" (that occur before job seekers participate in training), "lock-in effects" (during training), and "post-program effects" (after participation).

We find that OLS and IV differ in the short run because they yield different estimated threat effects. In particular, while the IV estimates reveal a positive threat effect of assignment to classroom training, confirming prior experimental estimates (Black et al., 2003), the OLS estimates suggest large negative threat effects. The difference between OLS and IV in estimated threat effects reflects that job seekers have worse job options while waiting for their assigned training program to begin.[4] At longer horizons, the positive IV estimates are primarily driven by post-program effects rather than threat effects. This finding suggests that classroom training increases employment by providing job seekers with new skills valued in the labor market.[5] Using course titles to disaggregate classroom training, we find that "skills & wrap-around" courses help workers switch occupations, while "job search" courses are more effective in helping workers find jobs in their original occupations, especially at the beginning of their UI spells.

Finally, we examine heterogeneity in the causal effects of classroom training. We find job seekers initially employed in offshoring-exposed occupations – who tend to experience larger

[3]Matching estimators that also rely on conditional independence show the same negative selection bias as OLS. Dynamic treatment approaches, as in Biewen et al. (2014), remove only a small fraction of the negative bias.

[4]The bias is a prospective version of Ashenfelter's dip (Ashenfelter, 1978).

[5]This is consistent with van den Berg and Vikström (2022), who derive identification results in a dynamic setting 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. The dynamic perspective of our approach bears a 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. Crépon and van den Berg (2016) provide a review of this literature with a focus on a dynamic perspective of program effects.

and more persistent employment losses – also have higher gains from classroom training and that they drive the positive effects of these programs. Skill mismatches caused by globalization or technological change are an often-cited motivation for training programs (e.g., Braxton and Taska (2023)), and our results suggest globalization-exposed job seekers have higher employment gains and that total employment may be increased by targeting classroom training toward these job seekers.[6] We also explore whether our local average treatment effects (LATE) for instrument compliers are informative for a broader set of job seekers. By the estimation of marginal treatment effects, we find positive effects of classroom training for job seekers with different underlying resistance to training. These findings suggest that our LATE estimate for classroom training is representative of the broader population of job seekers.

We make four 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 interventions 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, based on detailed course titles recorded in the job plans, we define economically meaningful training categories, a substantive improvement over existing ALMP research. Finally, 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 independence 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 (our first-stage relationships) and that the effects likely are monotone across job seekers.[7] Finally, our identifying assumption is that

[6] 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, we are the first to show that training programs particularly benefit globalization-exposed job seekers. Furthermore, we show that classroom training helps workers switch to new occupations.

[7] Important for our setting with multiple treatments, we show the program-specific instruments do not have

caseworkers' tendencies for assignment to training programs are uncorrelated with other dimensions of their activities that also affect worker outcomes. We provide evidence that supports this exclusion restriction, showing that the training tendencies are uncorrelated with other factors potentially driving their value added, such as the experiences, meeting timing, and caseloads of caseworkers. We also provide falsification tests that show that caseworker tendencies do not affect the outcomes of job seekers whose training decisions are unaffected by the instruments (so-called zero-first-stage tests). Further, our explorations of the mechanisms behind the employment effects indirectly support the exclusion restriction by suggesting *reskilling* is the key mechanism. Indeed, job seeker reskilling is difficult to rationalize with other caseworker behaviors than training assignments.

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 positive earnings effects for TAA-approved workers. We provide broader evidence for the impact of ALMP programs as we compare classroom training with on-the-job training programs, we decompose the effects into threat, lock-in, and post-program 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 allocating unemployed job seekers to caseworkers. A growing strand of the literature uses randomized controlled trials (RCTs) that also have the potential to address the identification challenges in program evaluations outlined above. Our natural-experiment strategy complements this literature in several ways. The natural setting of our study helps mitigate some concerns that arise in intervention studies, including experimenter-demand effects, Hawthorne effects, and limits to scalability (Levitt and List, 2007; List, 2022). Also, RCTs are often limited in scale due to resource requirements, whereas we exploit the extent of our data to estimate effects by subpopulations.[8] Further, we decompose the dynamics of our IV estimates and estimate marginal treatment effects to extrapolate to broader populations.

The remainder of the paper is structured as follows. Sections 2 and 3 first describe our

"cross-effects" on different training programs; see Behaghel et al. (2013); Bhuller and Sigstad (2024).

[8]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, on-the-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) provide a discussion of challenges faced by RCTs.

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 these effects into the threat, lock-in, and post-program effects of training. Sections 8 and 9 study heterogeneity in the treatment effects across training programs and worker types. Section 10 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% of prior monthly earnings up to 3,075 USD.

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 that, among job seekers who meet with a caseworker, the first meeting typically occurs in week five of the UI spell.[9] The meetings last 30-45 minutes, and the average job seeker has four meetings over her unemployment spell.[10] It is during these caseworker meetings that job-search monitoring, counseling, and sanctioning take place. On top of this job search

[9]Table L.1 shows that about 25% of job seekers (50% of UI spells) exit unemployment before meeting with a caseworker. The Danish activation laws mandate a minimum meeting frequency, but proactive caseworkers may encourage jobseekers to meet more frequently.

[10]Among job seekers who complete their training, 79% have a subsequent meeting with their caseworker. This means our training treatment effects occur in an environment where job seekers engage with the activation system and actively search for jobs.

assistance, some job seekers are also assigned to training programs.

Table 1: Timing and Duration of Activities Assigned within First 12 Months

	(1) Assignment rate (%)	(2) Timing (week no.)	(3) Duration (days)
Caseworker meeting	100	5	1
Classroom training	39	18	52
On-the-job training	25	21	88

Notes: This table shows the timing and duration of activities assigned within the first 12 months after job loss. The table focuses on job seekers who meet with a caseworker. Column (1) reports the share of job seekers who have at least one caseworker meeting (row 1), and who are assigned to a training program within the first 12 months after job loss (rows 2-3). Column (2) reports the average timing of the *first* assigned activity measured relative to job loss. Column (3) reports the total duration of *all* assigned activities. Rows 2-3 report the total number of days job seekers are supposed to spend on classroom and on-the-job training. For comparison, row 1 reports our best estimate of how many days the average job seeker spends with the caseworker over the unemployment spell. It is based on (i) the observation that job seekers on average have 4 caseworker meetings over the unemployment spell, (ii) each of which lasts about 30-45 minutes, and (iii) presuming job seekers spend at least five hours per day at training facilities.

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: *classroom training* and *on-the-job training*.

Classroom training includes skills training (e.g., ordinary education or vocational training), wrap-around services (e.g., job counseling), and job search courses (e.g., CV and job application).[11,12] Most skills training are vocational training courses that target specific occupations or industries, such as a forklift certificate or a personal computer course. A stated goal of classroom training is to facilitate occupational switching.

On-the-job training includes internships and wage subsidies for employment at a private or public firm for a pre-specified time period. An objective of these programs is to provide the job seeker with some hands-on workplace experience. Internships offer short-term placements where participants continue to receive their unemployment benefits without salary from the host company, whereas wage subsidies cover part of workers' salary on a fixed-term contract.[13] Employers are motivated to participate in these programs due to the financial benefits and to fulfill corporate social responsibilities.

[11] Table L.4 breaks down classroom training by the types of courses.

[12] We distinguish between the job search assistance provided through caseworker meetings, which all job seekers receive, and job search assistance provided in dedicated classroom courses.

[13] Table L.3 breaks down on-the-job training by the types of programs.

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 39% of all job seekers are assigned to classroom training, and 25% are assigned to on-the-job training. The two training programs are not mutually exclusive: 13% of job seekers are assigned to both programs. Finally, the table shows 48% of job seekers are in "passive UI"; they are neither assigned to classroom training nor to on-the-job training. Hence, while all job seekers in the sample receive job-search assistance facilitated through caseworker meetings, only about half the job seekers are also assigned to some training program.[14]

Table 2: Assignment to Classroom Training and On-The-Job Training

	Percent of Job Seekers	
	On-the-job training = 0	On-the-job training = 1
Classroom training = 0	48	13
Classroom training = 1	27	12

Notes: This table shows the share of job seekers in the analysis sample who are assigned to a given type of training program within the first 12 months after the UI-spell start.

As we will show in Figures 2 and C.1, the assignments to classroom and on-the-job training are empirically unrelated. Indeed, since the programs have different objectives ("skills" vs. "experience"), there are good reasons why they are not close substitutes. For example, newly graduated job seekers may have up-to-date skills, but lack labor market experience, and so would benefit from on-the-job training. Other more experienced job seekers may have outdated skills and so may benefit from classroom training. This indicates that assignment to classroom training and on-the-job training may be two separate decisions for the caseworker.

Table 1 compares the timing and duration of caseworker meetings and training programs. The average trainee spends 52 and 88 days in total on classroom and on-the-job training, respectively. In comparison, these job seekers spend the equivalent of almost a full day with their caseworker over their unemployment spell. Put differently, an essential way caseworkers affect job seeker activities is by assigning training programs.[15]

[14]The Danish activation system mandates that job seekers must be assigned a training program after 6 months of UI recipience. In practice, however, caseworkers have leeway in deciding who and when to assign to programs. Our administrative records show that, after 12 months of unemployment, 14% of job seekers have not been assigned to a training program.

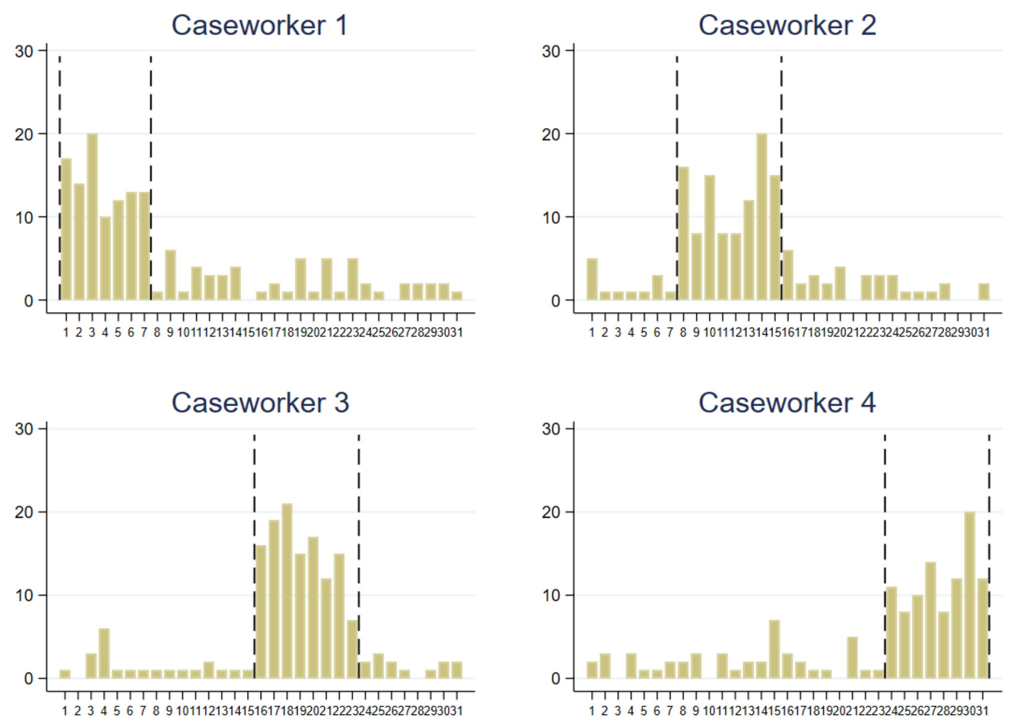
[15]Caseworkers generally do not interact with the on-the-job or classroom training programs, which are administered by different entities at separate locations. Instead, the primary task of the caseworkers is to steer job seekers toward these programs.

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 that arises from job seeker birthdays. As we show in Figure D.12, it is crucial to use the *predicted* caseworkers (as opposed to the *realized* ones) to uncover the causal effects of training programs.

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 58%, 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 with detailed course titles. 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 record the date, time, and type of all meetings in the Danish job centers from 2012 to 2019. The dataset also includes identifiers for the job seeker and caseworker participating in each meeting. We use this dataset to create a linked job seeker–caseworker data set.

First, we identify job seekers who became unemployed from 2012 to 2018. We link each of the job seekers to the caseworker who participated in the job seekers' first face-to-face meeting. We call this caseworker her *realized caseworker*.[16]

Second, we document the use of day-of-birth allocation rules in all 94 job centers in Denmark over time. Online Appendix L.4 details our procedure, which proceeds in two steps. First, we identify the job-center units in which job seekers are allocated to caseworkers.[17] 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. This procedure is similar to Cederlöf et al. (2021), who identify the day-of-birth predicted caseworker for Swedish job seekers. Appendix Table A.1 shows we are able to link 90% of all job seekers (84% 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 training and on-the-job training programs using a new 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 three 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 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." Furthermore, as highlighted by Black et al. (2003), data on assignment are critical to capture any "threat effects" of training that may occur before job seekers participate in the programs.

Second, the job plans record the exact *timing* of the training assignments, including the start

[16] See Online Appendices L.2 and L.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.

[17] 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 L.4.

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.

Third, the job plans contain detailed course titles of the training assignments, allowing us to define the programs along economically meaningful lines, such as job-search courses, skills training, and wrap-around courses. Appendix L.5.2 describes the data and our keywords for categorizing courses.

Online Appendix L.5 provides a validation of the job plans and our two interventions of interest: assignment to classroom training and on-the-job training. Two insights merit note. First, a substantial part of the *classroom training* assignments likely represents actual education (ordinary education or vocational training). We find that 44% of classroom training assignees are *registered* as enrolled in education within the first 12 months of unemployment. This corresponds to 50% of the total assigned classroom training days. Given that some assignments never lead to enrollment, this is likely a lower bound of the education content of classroom training. Second, the *on-the-job-training* assignments include both public and private internships and wage subsidies. For example, one-third of the internships are public sector programs.

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 2019.[18] 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 L.6.

Job Seeker Characteristics. Our data on *demographics* come from the *Population Register* (BEF), recording the gender, age, and country of origin of 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

[18] We cap the outcome data at this horizon to focus on labor market outcomes before the onset of COVID-19 and related lockdowns in 2020

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 (i) who became unemployed between 2012 and 2018 and (ii) who had at least one meeting with a caseworker from the local job center.[19]

Using our linked job seeker–caseworker data, we obtain information about the job seekers' *realized* and *day-of-birth predicted* caseworker. Using the job plan data, we obtain information about the job seekers' assignments to classroom and on-the-job training programs.

We apply five 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 L.4). Appendix Table A.1 shows the number of unique job centers drops from 94 to 51, while the day-of-birth compliance rate increases from 42% to 52% when we impose this restriction. Second, we exclude from the analysis sample all job seekers of 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.[20] Third, we exclude job seekers for whom we cannot identify their previous occupation. We make this restriction because we will distinguish job seekers by the offshorability of their previous occupation in Section 9.1. Fourth, 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. Fifth, to estimate training tendencies reliably, we require that each caseworker in the sample must have at least 50 assigned job seekers over the sample period. Figure A.1 provides a tree chart of our main sample of job seekers.

Appendix Table A.1 shows our analysis sample captures 51 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

[19] We focus our main sample on job seekers who meet a caseworker because any caseworker effects, including training tendencies, are only relevant to the population of job seekers who meet their caseworkers. Indeed, Section 5.4.2 implements a placebo test, showing that caseworker tendencies do not affect the outcomes of job seekers who don't meet the caseworkers.

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

meeting (the linked job seeker–caseworker data). Appendix Figure A.2 shows these job centers are spread out across Denmark. We develop our caseworker-tendency instruments based on this *full* sample of job seekers. In order to study the dynamic effects of assignment to training programs, we base all estimations on a *balanced* sample of job seekers for whom we observe labor market outcomes in the first two years after job loss.[21] In Section D.1.6, we show that we obtain similar results if we used the full (but unbalanced) sample of job seekers.

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. This type of information may be revealed to the caseworker during meetings, but it is unobserved to the econometrician. 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 identification strategy is that job seekers are assigned to caseworkers according to their day of birth (1-31) and that caseworkers differ in their tendencies to assign different types of training. These institutional features imply that job seekers' likelihood of being assigned to training programs is effectively determined by their birthdays in the month. Furthermore, since job seekers' birthdays are otherwise unrelated to their labor market outcomes, they constitute valid instruments for identifying the causal impacts of training programs. We rely here on the "birthday block" rules for caseworker assignments documented in Section 3.1 and provide evidence in Section 5 that the birthday allocations constitute valid and relevant instruments for training assignments.

[21] Since we observe outcomes up until 2019, we essentially restrict the sample to job seekers who became unemployed from 2012 to 2017.

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 B, we develop a generalized Roy model that addresses these methodological novelties. In the model, caseworkers differ in their preferences for the training programs but rank individual job seekers similarly in their resistance to participate in each program.[22,23]

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. Let $c(b)$ denote the rule that allocates job seekers with day of birth $b \in \{1, 31\}$ to caseworkers c, and let the indicator D_{ki} equal one if job seeker i is assigned to program k within the first 12 months after job loss. Our training tendency instruments are defined as follows:

$$Z_{ki} = \frac{1}{(J(i)-1)} \sum_{j \neq i} \mathbf{1}[c(b_j) = c(b_i)] \times D_{kj}, \quad J(i) = \sum_{j} \mathbf{1}[c(b_j) = c(b_i)], \tag{1}$$

where $J(i)$ is the number of job seekers with the same day-of-birth predicted caseworker as job seeker i. We denote caseworkers with a low Z_k as k-*restrained* and caseworkers with a high Z_k as k-*inclined*.

The instruments must satisfy the usual assumptions of relevance, independence, exclusion, and 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. In particular, the assignment of job seeker i to training k must solely depend 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. This *extended monotonicity* assumption plays a key role in IV analysis with

[22] 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.

[23] The role of caseworkers' preferences is motivated by our qualitative interviews at the job centers, during which a caseworker, for example, ascribed differences in training tendencies to reflect differences in "values" ("*værdisæt*" in Danish) of caseworkers.

multiple treatments (Behaghel et al., 2013; Lee and Salanié, 2018, 2020; Bhuller and Sigstad, 2024).[24] See theory and further discussion in Appendix B.

The theoretical extension in Appendix B motivates instrument diagnostics in Section 5 and facilitates the estimation of marginal treatment effects in Section 9.2. 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 comparison with how prior studies have handled multiple treatments in judge IV setups (Autor 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).

We estimate the dynamic effects of assignment to classroom training on the employment of job seekers. Let Y_{it} denote the employment of job seeker i in period t relative to the start of the unemployment spell. The TSLS specification reads

$$D_{ki} = \delta_{q(i)k} + \delta_{k1} Z_{1i} + \delta_{k2} Z_{2i} + \delta_{k3} X_i + \varepsilon_{1i} \tag{2}$$

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

where X_i is a vector of pre-determined characteristics of job seekers (see Appendix L.7), and $q(i)$ are job-center-unit-start-year combinations, the units wherein our randomization takes place. Hence, we compare similar 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 the job seeker and predicted caseworker levels.

We abstract from interaction terms between classroom and on-the-job training in Equations (2)-(3) since our focus is on the causal effects of the separate training interventions. Furthermore, we do not have the statistical power to identify the interaction terms.[25] Leaving out

[24] The plausibility of extended monotonicity depends on the institutional setting. In our context, Section 2 provides reasons why the assignment to classroom and on-the-job training programs may be independent decisions for caseworkers. By contrast, Humphries et al. (2023) considers a setting where treatments are mutually exclusive such that judge leniency instruments by construction do not satisfy the extended monotonicity assumption. Humphries et al. (2023) develop alternative assumptions and methods for handling multi-treatment judge IVs in such settings.

[25] To estimate interaction effects, we would need an additional first-stage equation with an interaction of the indicators (tendencies) for classroom and on-the-job training on the LHS (RHS):

$$D_{1i} D_{2i} = \delta_{q(i)} + \delta_1 Z_{1i} + \delta_2 Z_{2i} + \delta_3 Z_{1i} Z_{2i} + \delta_4 X_i$$

When estimating this equation, the coefficient on $Z_1 Z_2$ is barely significant (p-value of 0.104).

interaction effects implies the β_{1t} coefficient represents the effect of shifting compliers into classroom training from either passive UI or on-the-job training only. Appendix D.1.4 shows that our results are robust to focusing exclusively on compliers shifted from the passive margin and into classroom training.

Because our instruments rely on the *cross-sectional* variation in the training tendencies across caseworkers, our main IV analysis does not consider the *timing* of training assignments. However, in Section 7, we combine the caseworker instruments with assumptions about the transitions of job seekers across training states to identify how training relates to employment at different time horizons.

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 only affect job seeker outcomes, including any caseworker value added, through training assignments. In Section 5.4, we provide evidence that supports this exclusion restriction, showing that the training tendencies are uncorrelated with other factors potentially driving their value added, such as the experiences, meeting timing, and caseloads of caseworkers. We also provide placebo tests that the caseworker tendencies do not affect the outcomes of job seekers whose training decisions are unaffected by the instruments.

5 Instrument Diagnostics

In this section, we assess our caseworker-tendency instruments. We provide evidence that the instruments satisfy the relevance, independence, 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.[26] We find exploiting the source of randomization is crucial for instrument independence in our context. Second, we have a setting with multiple treatments, which 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

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

Sigstad, 2024).

Finally, we note that our exclusion restriction is milder than the *strict* version proposed by Frandsen et al. (2023) because we allow caseworkers to differ in value-added unrelated to training tendencies; see Appendix B.1 for theoretical discussions. Indeed, our instrument diagnostics follow the Frandsen et al. (2023) guidelines for testing average exclusion and average monotonicity.

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.

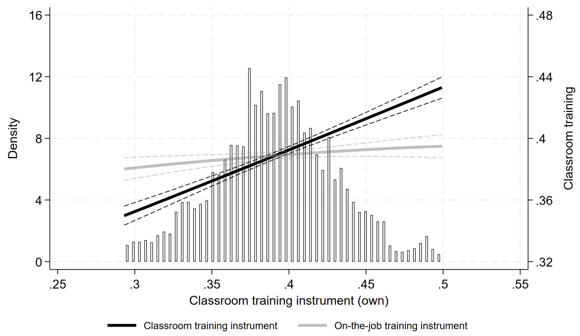
First, the caseworkers' training program tendencies strongly affect the job seekers' assignment to training programs. Figures 2a-2b corroborate this conclusion by combining the first-stage relationships with the distribution of caseworker training tendencies. For example, shifting from the most classroom 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% increase relative to the mean classroom training assignment rate.[27] As Figures 2c-2d show, most of these assignments occur in the first quarter after the initial job loss. The typical job seeker in our sample is unemployed for about 22 weeks. In Section 7, 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 is only affected by the caseworkers' classroom training tendencies, not by their on-the-job-training tendencies. This absence of "cross effects" is not surprising: Theoretically, the two programs are unlikely to be close substitutes (see discussion in Section 2), and empirically, caseworkers' training tendencies are uncorrelated across programs (see Appendix Figure C.1). As discussed in Section 4, the 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 requires us to find otherwise similar caseworkers who differ in their tendency for each training

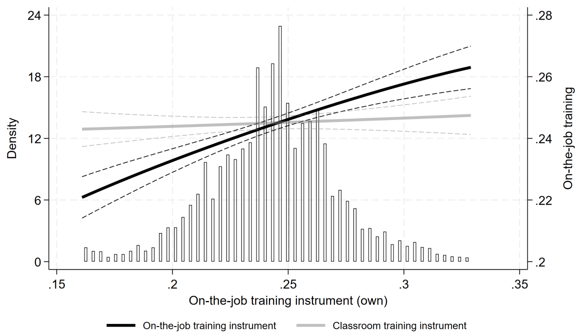
[27] Appendix Table C.2 shows the program-specific instruments have power for each training program.

program. Fortunately, as already mentioned, caseworker tendencies are indeed uncorrelated across programs in our setting, see Appendix Figure C.1.

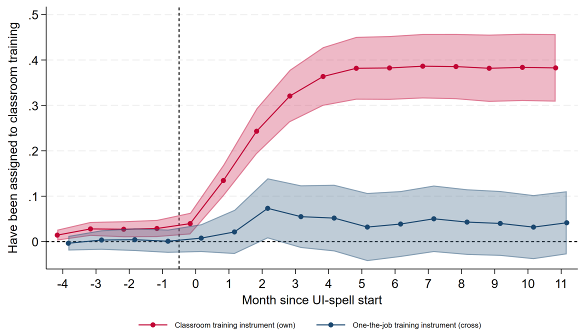
Figure 2: Assignment to Training Programs



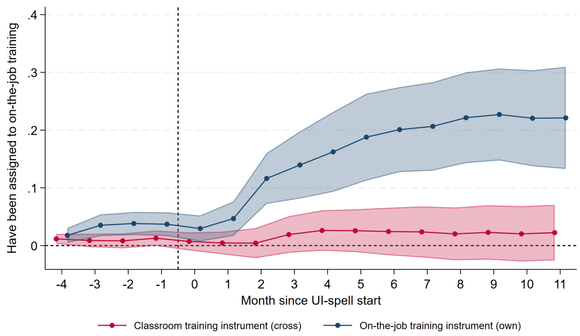
(a) Classroom Training



(b) On-The-Job Training



(c) Classroom Training



(d) On-The-Job Training

Notes: This figure plots the first stage estimates for assignment to training programs. Panels (a) and (b) represent the first stage of assignment to classroom training and on-the-job training within the first 12 months on the two caseworker-tendency instruments. The sample includes workers who leave unemployment within the 12-month window. The bars represent the distribution of the own-instrument demeaned by job-center-unit-year fixed effects, predetermined job seeker characteristics (see Appendix L.7) 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, predetermined job seeker characteristics 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, predetermined job seeker characteristics and the own-instrument. The local linear regressions are based on an Epanechnikov kernel (with bandwidth 0.1). The results are robust to using the same specification as in Bhuller et al. (2020): top and bottom 2% excluded, triangular kernel, and bandwidth = 0.15. Panels (c) and (d) represent a dynamic version of the first stage. They represent the first stage of having been assigned to classroom training and on-the-job training in or prior to a given month on the two instruments. Shaded areas represent 95% confidence intervals.

As described in Section 4, our instruments rely on an initial step of identifying the birthday blocks that job centers use to allocate job seekers to caseworkers. These blocks are administrative rules that show up as clear discontinuities in our data and constitute an improvement over many existing judge IV studies that rely on anecdotal evidence for why the realized allocation may be quasi-random. However, because the birthday blocks are identified by us (see Appendix L.4), we now discuss how measuring error in this initial step affects our main estimates and inference. First, the practice of inferring administrative rules from data discontinuities is common in the RD literature (see, e.g., Mountjoy (2024); Altmejd et al. (2021); Hoekstra (2009)), where Porter and Yu (2015) show that the asymptotic distribution of the second-stage estimator is unaffected by the fact that the cutoffs are estimated from the data rather than known ex-ante. Second, in finite samples, estimation error in the initial step will tend to weaken the first stage and may bias 2SLS toward the OLS estimates (Bound et al., 1995). Fortunately, Figure 2 shows that caseworker tendencies are strong instruments, especially for classroom training. However, any weak-instrument bias will work against the stark differences between OLS and 2SLS we document in Section 6.1.

5.2 Independence

In Table 3, we test the independence 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. We use the predetermined job seeker characteristics suggested by Lechner and Wunsch (2013); see Appendix L.7.

The independence test yields three takeaways. First, the assignment of job seekers to training programs is highly endogenous (Columns (1) and (2)), confirming 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.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Actual assignment		Realized Caseworker		Predicted Caseworker		Covariates	
	Classroom training	On-the-job training	Classroom training	On-the-job training	Classroom training	On-the-job training	mean	sd
Demographics								
Age	0.002***	0.001*	0.000*	−0.001***	0.000*	0.000	41.915	11.980
Male	0.011*	−0.071***	0.002	0.005	−0.001	−0.001	0.480	0.500
Immigrant	0.126***	0.013	0.005	−0.002	−0.001	−0.002	0.043	0.202
Descendant	0.080	−0.088	0.027	−0.052	0.004	0.002	0.002	0.041
Number of children	−0.020***	−0.020***	−0.003*	−0.005**	−0.001	−0.001	0.875	1.037
Married	−0.063***	−0.020***	−0.007*	−0.008*	−0.005***	−0.001	0.423	0.494
Academics Association	0.083***	−0.041***	0.031***	0.014	0.003	0.000	0.095	0.293
Danish Trade Union Association	0.034***	0.018***	−0.005	−0.022***	0.004	−0.003	0.713	0.453
Education level								
Primary	0.015	0.121	0.063	0.039	0.043	0.001	0.001	0.037
Lower secondary	−0.055**	−0.024	0.013	0.006	0.011	0.004	0.197	0.398
Upper secondary	−0.074***	−0.033	0.022	0.009	0.010	0.000	0.525	0.499
Short cycle tertiary	−0.054*	−0.055**	0.024	0.002	0.010	0.009	0.051	0.221
Bachelor	−0.146***	−0.061**	0.009	−0.017	0.014	0.007	0.154	0.361
Master	−0.129***	−0.016	0.021	−0.035	0.005	−0.003	0.058	0.235
Doctoral	−0.205***	−0.006	0.037	0.026	0.027	0.022	0.002	0.048
Pre-treatment outcomes (four years before unemployment)								
Employed in month -48	−0.051***	−0.004	−0.001	−0.002	−0.002	−0.002	0.670	0.470
Working hours in month -48	0.000*	0.000	0.000	0.000	0.000	0.000	93.806	75.822
Earnings (1,000 DKK) in month -48	0.000	0.000	0.000	0.000***	0.000	0.000	16.313	17.965
Unemployed in month -48	−0.069***	−0.044***	−0.018***	−0.015	0.001	−0.004	0.158	0.365
Total months employed since month -48	−0.003***	−0.004***	0.000	−0.001	0.000	0.000	32.004	13.611
Total accumulated working hours since month -48	0.000**	0.000**	0.000	0.000	0.000	0.000	4538.179	2295.402
Total accumulated earnings (1,000 DKK) since month -48	0.000**	0.000**	0.000	0.000	0.000	0.000	809.022	569.208
Total months unemployed since month -48	0.001**	−0.001*	0.001*	−0.001	0.000	0.000	8.665	9.852
Labor market history (two years before unemployment)								
Employed in month -6	0.016	0.013	−0.011	−0.018	0.004	0.005	0.647	0.478
Employed in month -24	0.011	0.009	0.011	0.004	−0.003	0.011**	0.658	0.474
Total months employed since month -6	−0.015***	−0.036***	0.001	0.000	0.001	0.003	4.130	2.313
Total months employed since month -24	0.002	0.007***	0.001	0.001	−0.001	−0.001	15.827	7.778
Any employment in previous 24 months	−0.022	−0.023	−0.027***	−0.019	−0.003	0.002	0.943	0.232
Number of employers last 24 months	−0.012***	−0.019***	0.000	0.003	−0.001	−0.001	1.762	1.087
Unemployed in month -6	0.064***	0.076***	−0.007	0.004	−0.001	0.011*	0.191	0.393
Unemployed in month -24	−0.048***	−0.014	−0.010	−0.006	0.001	0.009*	0.212	0.409
Total months unemployed since month -6	−0.012***	−0.029***	0.003	0.000	0.000	0.001	0.689	1.307
Total months unemployed since month -24	0.002	0.005***	0.001	0.005***	0.000	−0.001**	4.568	5.631
Any unemployment in previous 24 months	−0.040***	−0.045***	−0.010	0.005	−0.001	0.007**	0.565	0.496
Number of UI-spells last 24 months	−0.043***	−0.018***	−0.017***	−0.017***	0.003	0.001	0.361	0.610
Public benefits in month -6	−0.018	−0.035***	−0.004	−0.009	−0.005	−0.010**	0.450	0.497
Public benefits in month -24	0.000	−0.010	0.008	0.009	−0.001	0.003	0.423	0.494
Total months of public benefits since month -6	0.027***	0.036***	0.004**	0.006***	0.001	0.002*	2.376	2.472
Total months of public benefits since month -24	0.001	0.002*	0.001	0.003***	0.000	0.000	10.226	8.260
Any public benefits in previous 24 months	−0.066***	−0.060***	−0.012**	−0.025***	−0.001	−0.003	0.815	0.389
Earnings (1,000 DKK) in month -6	0.000*	0.000	0.000	0.000	0.000	0.000	16.812	18.468
Earnings (1,000 DKK) in month -24	0.000	0.000	0.000	0.000*	0.000	0.000	16.457	16.615
Total accumulated earnings (1,000 DKK) since month -6	0.000***	0.000	0.000	0.000	0.000	0.000	112.704	93.056
Total accumulated earnings (1,000 DKK) since month -24	0.000**	0.000***	0.000	0.000	0.000	0.000	410.489	306.669
Working hours in month -6	0.000	0.000	0.000	0.000	0.000	0.000	92.216	76.063
Working hours in month -24	0.000	0.000*	0.000	0.000	0.000	0.000	93.786	76.054
Total accumulated working hours since month -6	0.000***	0.000	0.000	0.000	0.000	0.000	583.340	372.798
Total accumulated working hours since month -24	0.000	0.000	0.000	0.000	0.000	0.000	2253.039	1276.781
Previous industry								
C Manufacturing	0.071***	0.015	−0.007	−0.024***	0.002	0.006	0.118	0.322
F Construction	−0.129***	−0.106***	0.001	−0.008	0.000	0.001	0.107	0.309
G Wholesale and repair	0.010	0.074***	0.001	0.000	0.002	0.004	0.130	0.337
H Transportation and storage	0.037**	−0.043***	−0.011	−0.007	−0.003	0.005	0.041	0.197
I Accomodation and food service	−0.077***	0.006	−0.020*	−0.038***	0.005	0.007	0.039	0.194
N Administrative and support service	0.010	−0.053***	0.006	−0.005	0.003	0.005	0.098	0.297
P Education	−0.054***	−0.030**	−0.014*	0.012	−0.008**	0.011**	0.058	0.234
Q Human health and social work	−0.081***	−0.033***	−0.001	0.002	−0.003	0.008**	0.189	0.391
Typical occupation								
High risk of offshoring	0.063***	0.063***	0.007	0.010*	−0.004	0.007**	0.250	0.433
Professionals	−0.035*	−0.037**	0.012	0.010	0.011*	−0.008	0.142	0.349
Technicians and associate professionals	−0.046**	−0.059***	0.011	0.012	0.008	−0.013*	0.088	0.284
Clerical support workers	−0.039*	−0.038**	0.008	0.010	0.011	−0.005	0.099	0.299
Service and sales workers	−0.055***	−0.015	0.008	0.006	0.009	−0.005	0.240	0.427
Skilled agricultural, forestry and fishery workers	−0.141***	−0.137***	0.002	−0.027*	0.019**	−0.016	0.022	0.148
Craft and related trade workers	−0.100***	−0.077***	0.002	−0.019	0.008	−0.001	0.163	0.369
Plant and machine operators, and assemblers	0.000	−0.047**	0.009	−0.005	0.010*	−0.006	0.090	0.286
Elementary occupations	−0.061***	−0.053***	−0.009	−0.013	0.006	−0.010	0.133	0.339
Obs	167,222	167,222	166,666	166,666	167,222	167,222	167,222	
Number of FE's	189	189	189	189	189	189		
F-stat	54.094	64.714	3.001	5.599	1.179	1.126		
P-value	0.000	0.000	0.000	0.000	0.155	0.231		

Notes: This table implements a randomization test of the caseworker-tendency instruments. Columns (1) and (2) regress the assignment to training programs on predetermined job seeker covariates (see Appendix L.7). 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 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 1-digit ISCED classification. The omitted category is "N.e.c. or missing". Previous industries (within the previous 24 months) are defined as the 21 sections of the NACE level-1 classification. The randomization test and all regressions with controls include dummies for the eight largest industries. Typical occupations are defined as the 10 sections of the 1-digit ISCO08 classification. The omitted category is "Managers". "High risk of offshoring" is a dummy indicating the offshoring risk of the job seeker's previous occupation. Although not reported in the table, the regressions also include dummies for the month of entry to unemployment. 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: *$p<0.10$ ** $p<0.05$ *** $p<0.01$.

5.3 Monotonicity

The monotonicity assumption states that a job seeker assigned to training by a training-restrained caseworker should also 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 Appendix B for discussions. In other words, we require that the "own-instrument effects" are substantively and statistically different from zero, while the "cross-instruments" have precise zero effects.

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 C.5 and C.6, we split the job seekers into quartiles based on their propensities for assignment to training. We then show that 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, we rely on the same quartiles but now measure caseworker training tendencies using job seekers from *other* subgroups. Appendix Tables C.7 and C.8 show that, in all subgroups, the coefficient on the "reverse-sample" own-instrument is positive, whereas the "reverse-sample" cross-instrument is insignificant, substantively and statistically (except in one subgroup). Taken together, these results support the monotonicity assumption of our identification strategy.

5.4 Exclusion

To interpret our IV estimates as treatment effects of training, we require the *exclusion restriction* that caseworkers' training tendencies are uncorrelated with other dimensions of their activities that affect worker outcomes. This section examines the validity of this exclusion restriction. First, in Section 5.4.1, we assess whether caseworkers' training tendencies are correlated with proxies for the quality or quantity of their job-search advice. Second, in Section 5.4.2, we conduct "zero first-stage" placebo tests that the caseworker instruments should not affect the outcomes of job seekers whose training decisions are unaffected by the tendencies of their caseworkers. Finally, we discuss how our subsequent explorations of mechanisms help inform the exclusion restriction.

5.4.1 Caseworkers' Experience, Caseloads, and Meeting Behavior

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 their job search. Therefore, one concern may be that caseworkers who are very training-inclined also meet earlier with their job seekers or provide them with better job-search advice. We conduct two tests to address these concerns.

First, we test whether caseworkers' classroom 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 logic, Table 4 shows that caseworkers' training tendencies are largely uncorrelated with their caseloads (columns (1)-(2)) and experiences (columns (3)-(5)). Out of the ten displayed coefficients, only one is borderline significant (at the 10% level). The point estimate implies that, within the same job-center-unit-year, a one standard deviation increase in the caseworker's classroom training tendency is associated with 8 more meetings per year. This corresponds to a 2% increase relative to the mean.

Table 4: Experience, Caseload Size, Meeting Timing and Training Tendencies of Caseworkers

	Caseload size		Experience			Meeting Timing
	Meetings/year (1)	Assigned job seekers/year (2)	Years (3)	Meetings (4)	Assigned job seekers (5)	Job seeker's 1st meeting (6)
Classroom training	159.82*	34.74	0.03	219.57	74.14	-0.09
	(93.40)	(25.07)	(0.54)	(375.76)	(120.53)	(0.51)
On-the-job training	135.51	-27.25	0.80	629.16	85.18	-0.99
	(118.99)	(29.49)	(0.64)	(438.69)	(124.96)	(0.64)
Obs	1,221	1,221	1,221	1,221	1,221	1,221
Dep var mean	430.56	117.66	1.17	868.95	255.01	5.04
Dep var SD	235.64	64.12	1.21	783.83	237.21	1.38
SD of res. CT	0.05	0.05	0.05	0.05	0.05	0.05
SD of res. OTJ	0.04	0.04	0.04	0.04	0.04	0.04

Notes: This table shows the coefficients and standard errors from a regression of caseworker experience, caseload size or meeting timing in a given year on the caseworker's classroom training tendency and on-the-job training tendency in the same year, while controlling for job-center-unit-year fixed effects and mean job seeker predetermined characteristics (see Appendix L.7). The caseworker's training tendencies are based on her predicted job seekers. The caseworker's caseloads, experiences, and meeting timing are based on meetings (with all job seekers) in which the caseworker participated. "Meetings/year" and "Assigned job seekers/year" are the total number of meetings and assignment meetings the caseworker participated in. " Years", "Meetings", and "Assigned job seekers" represent accumulated years, meetings, and assignment meetings by a given year. "Job seeker's 1st meeting" is the number of weeks into the UI spell that the job seeker's first meeting takes place (conditional on the caseworker participating in this meeting). Standard errors (in parentheses) are clustered on the caseworker level. The bottom of the table shows the standard deviation of caseworkers' classroom ("CT") and on-the-job ("OTJ") training tendencies (residualized by job-center-unit-year fixed effects and mean predetermined job seeker characteristics). *p<0.10 ** p<0.05 *** p<0.01.

Second, we assess whether more classroom training-inclined caseworkers *meet* earlier with the job seekers. Column (6) of Table 4 shows the caseworker's training tendencies are unable to predict the timing of the job seeker's first meeting in her UI spell. That is, job seekers with more training-inclined caseworkers do not meet sooner with their caseworkers. To further support the exclusion restriction, we re-estimate our main IV specification while controlling for the frequency and timing of meetings between the job seeker and caseworker; see Appendix D.1.5. Our results are robust to these controls and suggest meeting frequency and timing do not explain our estimates for the effects of classroom training.

5.4.2 Zero-First-Stage Tests

To further examine the exclusion restriction, we conduct two "zero-first-stage" (ZFS) tests, following Bound and Jaeger (2000). The idea of the ZFS test is to find subgroups whose training decisions are unaffected by the caseworker tendency instruments. Then, if the exclusion restriction holds, the instrument should not affect the labor market outcomes of this group either.

We conduct ZFS tests among two subgroups. First, we examine a group of "never-takers" with the lowest propensities of training. Second, we study job seekers who do not meet their caseworkers.[28]

"Never-Takers" Placebo Test. Panel A of Table 5 zooms in on a group of "never-takers" with the very lowest predicted rates of training. We identify these job seekers by estimating a linear probability model for assignment to any training (classroom or on-the-job training) based on predetermined characteristics and then focusing on job seekers with the lowest propensity score.[29]

Columns (1)-(2) first confirm that the first stages are zero among this "never-taker" group. Columns (3)-(4) then conduct the ZFS test, showing that the instruments also do not affect their employment outcomes.

"No Meeting" Placebo Test. Our main sample includes job seekers who had at least one meeting with their caseworker during their UI spell. We now perform a placebo test of our caseworker tendency instruments based on job seekers who did *not* have a meeting with a caseworker during their UI spell (or any other UI spell initiated within 12 months). The logic behind

[28](Acemoglu et al., 2022, Figure 5) conducts two similar ZFS tests (although in a very different context: the impact of the Socialist Party on the emergence of fascism in Italy after World War I). First, they identify never-taker municipalities as the bottom tier of the first-stage propensity score distribution (lowest predicted socialist share). Second, they limit the analysis to municipalities where the treatment channel is deactivated (no socialist candidate fielded).

[29]Table 5, Panel A focuses on job seekers below the 3rd percentile of training probabilities, but Tables C.10-C.11 show our results are robust to setting the cutoff at other percentiles.

the test is that caseworkers' training tendencies should not matter for job seekers they do not meet with.

Table C.9 shows that 3% and 2% of job seekers without caseworker meetings are assigned to classroom and on-the-job training, respectively. These assignment rates are very low compared to our main sample, in which 39% and 25% of job seekers are assigned to training programs (Table 1).

Panel B of Table 5 shows the first stage and reduced form estimates on this "no meeting" placebo sample. Columns (1)-(2) show that the caseworker training tendencies do not affect the training decisions of these job seekers.[30] Column (3) shows the ZFS test that the instruments also do not affect their employment outcomes.

Table 5: Zero-First-Stage Tests

	First Stage		Reduced Form	
	Classroom Training	On-the-job training	Employed in Q7	Monthly Working hours in Q7
	(1)	(2)	(3)	(4)
Panel A: Never-taker job seekers				
Classroom training tendency	0.07	0.03	-0.03	16.80
	(0.11)	(0.08)	(0.11)	(17.71)
On-the-job training tendency	0.02	-0.15	-0.22	-33.38
	(0.15)	(0.13)	(0.17)	(29.26)
Obs	5,012	5,012	5,012	5,012
Pscore	0.24	0.24	0.24	0.24
Dep. var mean	0.13	0.07	0.85	111.34
F-stat (all Z's)	0.19	0.65		
Panel B: No meeting job seekers				
Classroom training tendency	0.00	0.00	-0.01	-1.36
	(0.01)	(0.01)	(0.02)	(3.07)
On-the-job training tendency	0.01	-0.02*	-0.02	-4.31
	(0.01)	(0.01)	(0.03)	(4.60)
Obs	166,688	166,688	166,688	166,688
Dep. var mean	0.03	0.02	0.87	122.09
F-stat (all Z's)	0.16	1.41		

Notes: This table reports the first stage estimates of assignment to training programs (columns 1-2) and the reduced form estimate on extensive margin employment and working hours in quarter seven after job loss (columns 3-4) for a subpopulation of job seekers whose training decisions are likely unaffected by caseworkers' training tendencies. In panel A, this subpopulation of job seekers is identified by estimating an LPM for assignment to *any training* (classroom or on-the-job training) based on predetermined characteristics (see Appendix L.7) and sub-setting to job seekers with the lowest propensity score (< percentile 3). In panel B, the subpopulation of job seekers are those who never meet with a caseworker (see Table C.9). All regressions include job-center-unit-year fixed effects and controls for predetermined job seeker characteristics. Standard errors are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

[30]Column (2) shows one marginally statistically significant estimate (at the 10% level) from the on-the-job training instrument. However, the point estimate is an order of magnitude smaller than our baseline estimates in Table C.2 (and with the opposite sign) and economically close to zero.

5.4.3 Mechanisms

In addition to the direct tests above, our investigation of mechanisms in the sections below lends further support to the exclusion restriction. First, in Section 7, we show that the effect of assignment to classroom training is driven by job seekers who leave unemployment after completing their training programs (post-program effects). This timing is difficult to rationalize with other caseworker behaviors than training assignments. Second, in Section 8.2, we show that assignment to skills training helps workers switch occupations, whereas job-search courses tend to help workers find jobs in their original occupations, especially at the beginning of their unemployment spells. These mechanisms suggest that our effects are driven by the contents of the classroom courses, as opposed to other caseworker behaviors.[31]

6 Effects of Assignment to Classroom Training

Section 5 shows that our caseworker instruments are relevant, especially for classroom training, and we find no violations of the independence, (extended) monotonicity, or exclusion assumptions required to interpret the IV estimates as LATE for compliers.

In this section, we use our caseworker 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".

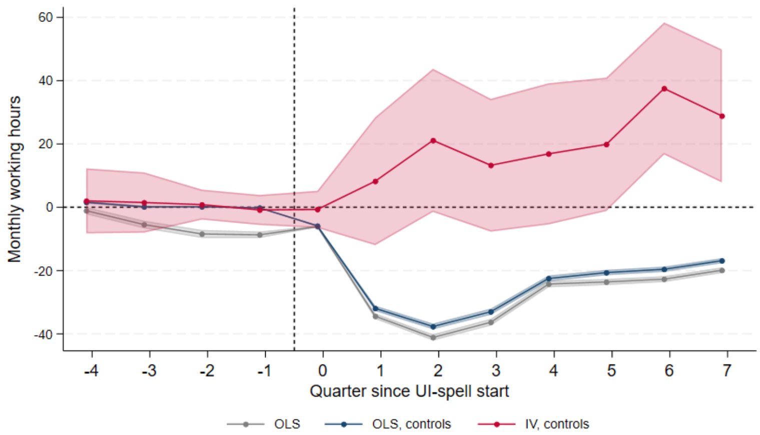
Figure 3 shows estimates of the effects of assignment to classroom training on the average monthly hours of employment ("monthly working hours") in a given quarter relative to job loss ("UI-spell start"). The figure includes our IV estimates (with controls), instrumenting assignment to training with caseworker training tendencies, and conventional OLS estimates, with and without controls. These controls include predetermined characteristics of job seekers; see Table 3 and Appendix L.7. [32] The figure reveals three insights.

[31]As a final note, following Kitagawa (2015), our setup with binary treatments and outcomes (e.g., extensive-margin employment and occupational mobility) yields a simple test of instrument validity that the ITT should not be larger than the first stage. Indeed, the fact that our IV estimates for employment (Figure D.3.(a)) and occupational mobility (Figure 8) are well below 1 implies that our main specifications pass this test too.

[32]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. These strategies build on the "dynamic treatment assignment" (DTA) approach introduced by Sianesi (2004). Our main analysis adopts 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. All job seekers in our analysis sample have at least one meeting with their caseworkers and are thus exposed to their caseworkers' tendencies to assign training programs. However, in Section 6.1, we compare our main IV estimates to DTA estimators.

First, the IV estimates show persistent positive effects of assignment to classroom training on hours of employment. The employment gains grow steadily over time, stabilizing at about 29 hours per month two years after the initial job loss, equivalent to a 29% increase relative to hours worked before job loss.[33] The effects in the last two quarters are statistically significant at the 1% level. Furthermore, Figure D.2 shows these significant effects persist into the third year, although with a slight attenuation in the point estimates.[34]

Figure 3: Effects of Assignment to Classroom Training on Hours of Employment



Notes: This figure shows the effect of assignment to classroom training on average monthly hours of employment ("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 pre-determined characteristics (see Appendix L.7). The red line represents the effect obtained by IV estimation, including controls for job seeker pre-determined characteristics. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals (see Table D.1 for standard errors).

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 hours of employment, especially when job seekers are most engaged in training.[35] The OLS conclusions align with the prior literature summarized by McCall et al. (2016).[36]

Third, controlling for pre-determined characteristics of the job seekers ("OLS, controls") does not change the strong lock-in effects in the post-period. The spurious lock-in effects of

[33] The brief dip in the IV estimates in quarter three after job loss is likely statistical noise. Appendix Figure D.1 unpacks the IV estimates by plotting the employment outcomes with and without classroom training for compliers in a given quarter relative to job loss. This figure shows that the employment of compliers assigned to training flattens somewhat from quarter two (solid line), whereas the employment for compliers *not* assigned to training increases up until quarter four and only thereafter flattens (dashed line).

[34] We use a two-year post window in our main specification because it allows us to study a balanced sample of UI spells from 2012-2017 that do not coincide with COVID-19. By contrast, Figure D.2 relies on an unbalanced sample after quarter 7.

[35] In Section 6.1, we further examine the differences between the OLS and IV estimates, concluding the differences reflect negative selection bias in OLS, as opposed to differences in causal estimands.

[36] See quote in Footnote 1.

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).

In Figure 3, we use hours of employment as the outcome to capture employment effects at the extensive and intensive margins. Appendix Figure D.3a and D.3b show the effects are similar for alternative labor market outcomes, including extensive-margin employment and earnings.

Table H.1 summarizes the effects two years after initial job loss. At this point, assignment to classroom training increases the extensive margin of employment by 21 percentage points, corresponding to 26% of the pre-job loss employment level, and the monthly earnings by about 28% of the job seekers' earnings before job loss. Hence, the effects on extensive margin employment and earnings are very similar to the effects on working hours.

The similarity of the effects across outcomes indicates *how* assignment to classroom training affects job seekers. First, the similar effects on earnings and working hours suggest assignment to classroom training primarily increases job seekers' earnings by boosting hours and not individual productivity (hourly wages). Second, the fact that the effects on extensive-margin employment and working hours are close (26% and 29%) suggests assignment to classroom training primarily increases the job seekers' ability to find jobs (extensive-margin employment) as opposed to the number of working hours conditional on employment (intensive-margin employment).

To be clear, a 21pp increase in employment rates from a 52-day training program is a substantial treatment effect. Where do these large employment effects come from? In Appendix F, we decompose the LATE of classroom training into the potential outcomes of compliers with and without training. The decomposition shows that the large employment effects reflect that compliers have depressed employment without training. This partly reflects that our population, at the outset, is unemployed, leaving room for any intervention to have substantial effects. In Section 9.1, we investigate whether workers who face structural challenges in the labor market from the offshoring of domestic jobs have larger gains from occupational switching through skills training.

Appendix D.1 presents various robustness tests of our IV estimates. First, in Appendix D.1.1, we show that our first and second-stage results are similar if we use a 6-, 9-, 12-, or 15-month window for defining the treatment (D_{ki} in Equation (1)). Second, our baseline TSLS specification controls linearly for assignment to on-the-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 B also formalizes this point, which is particularly relevant if the instruments are associated. In Appendix D.1.3, we show our results are robust to estimating our TSLS specification around an evaluation point z_2' for the on-the-job training instrument (Mountjoy, 2022). The similarity of the baseline and local TSLS partly reflects that the instruments are empirically unrelated (Figures 2 and C.1). Third, as explained in Appendix B, our caseworker-tendency instrument shifts job seekers into classroom training from two margins: From "passive UI" to "classroom training only", and from "on-the-job training only" into "both on-the-job training and classroom training." Appendix D.1.4 shows most job seekers are shifted along the passive margin, and our results are robust to focusing exclusively on these compliers. Finally, in Appendix D.1.2, we show our estimates are robust to re-weighting our sample of UI spells such that job-center-units are given weight according to their sizes (regardless of their years of sample coverage).

6.1 Differences between OLS and IV

In this section, we discuss 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).

Effect Heterogeneity. To examine effect heterogeneity on *observables*, Appendix E.1 first shows our OLS estimates are robust to reweighing job seekers to match the characteristics of compliers. Further supporting that observable heterogeneity is not driving our results, Appendix E.3 shows our estimates are similar if we use a matching estimator instead of OLS to control for selection on observables.

The negative average effect of OLS could mask that some subpopulations have positive estimated effects of training under conditional independence. To investigate this, Appendix E.5 follows Knaus et al. (2022) and implements a LASSO criterion to select the observables that predict treatment heterogeneity under conditional independence. The LASSO estimator implies that 99.65% of UI spells have negative effects of classroom training under conditional independence (with an average effect similar to the OLS estimate).

To investigate the role of *unobserved* heterogeneity, Section 9.2 shows the marginal treatment effects are positive across job seekers with different latent resistances to training. These findings suggest the differences in the signs of OLS and IV are not driven by effect heterogeneity on unobservables.[37]

Selection Bias. To investigate selection on unobservables, Appendix Figure D.4 unpacks the OLS and IV estimates into their implied employment outcomes with and without training. The figure shows that 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. Black et al. (2022) use similar insights to develop tests for selection into treatment based on unobservables.

Some of the negative selection bias of OLS could reflect the dynamic treatment assignments (DTA) concerns emphasized in the training literature: Job seekers are only assigned to a training course if they have not already found a job (Sianesi, 2004). To investigate this source of dynamic selection bias, Appendix E.4 implements the dynamic treatment assignment (DTA) estimator of Biewen et al. (2014). The analysis shows that accounting for dynamic treatment assignments helps remove 8% of the negative bias of OLS. Still, the DTA estimator estimates substantial negative effects of classroom training (-13 working hours per month two years after job loss).

7 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 while job seekers participate in the programs, 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 train-

[37]Heckman and Vytlacil (2005) show that many treatment effects of interest can be expressed as weighted averages of the MTE schedule. Hence, the differences in signs of IV and OLS do not reflect that the LATE is a different causal estimand than, e.g., the Average Treatment Effects (ATE), the Average Treatment Effect on the Treated (ATET), or other more policy-relevant parameters.

ing (assignees, hereafter) into five mutually exclusive training states in a given period, $s \in \{a, b, c, d1, d2\}$. 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, (*d1*) has dropped out before completing the training, or (*d2*) has completed the 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:

$$\beta_{1t} = \sum_{s\in\{a,b,c,d1,d2\}} \gamma_{1t}^{s} \times (\beta_{1t}^{1s} - \beta_{1t}^{0s}), \tag{4}$$

where γ_{1t}^{s} denotes the share of assignees present in state s in period t, and $(\beta_{1t}^{0s}, \beta_{1t}^{1s})$ denotes their potential employment outcomes with and without the training assignment. Appendix G.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, especially if they actually completed the training (*d2*). Finally, job seekers who (*a*) have not yet been assigned to training are subject to a *placebo* effect. [38]

Estimating the separate contributions of these effects requires identifying the counterfactual outcomes of assignees in each state. For example, for lock-in effects, how many assignees would have been employed if they were not currently undergoing training? Importantly, without additional assumptions, we can only identify the *average* counterfactual outcome of assignees, which in turn is a mix of the required state-specific counterfactuals. We pursue two approaches to separate the state-specific counterfactual outcomes.

First, we assume that the transitions of assignees across training states are uncorrelated with their counterfactual outcomes without assignment to training. This "exogenous training state transitions" assumption would be violated if, for example, assignees participate in training when their counterfactual employment outcomes are the worst. Second, to relax this assumption, we adopt an alternative strategy that relies on the distinct timing of the different effects. For example, if assignees train when they face adverse job opportunities, we should observe that

[38] Our definition of threat effects follows Black et al. (2003) who observe that job seekers assigned to training programs exit unemployment before the programs start. By contrast, Rosholm and Svarer (2008) identify "systematic" threat effects that arise in stricter activation regimes.

the mean counterfactual outcome of assignees is comparably low in the months when they train most intensively.[39] Because the two approaches turn out to deliver similar results, we focus the main text on the simple "exogenous training state transitions" setup and relegate the second, more technically involved, approach to Appendix G.3.

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

$$D_{it}^s = \gamma_{1t}^s D_{1i} + \gamma_{2t}^s D_{2i} + \Omega X_{it} + \varepsilon_{it}. \tag{5}$$

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

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

Our third and final step is to estimate the state-specific counterfactual outcomes β_{1t}^{0s}. The "exogenous training state transition" approach assumes that 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 outcome, β_{1t}^0, 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 D.4 plots the estimated counterfactual employment outcomes, β_{1t}^0, 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). The vector of controls, X_{it}, includes job-center-unit-year fixed effects and controls for job seeker predetermined characteristics.

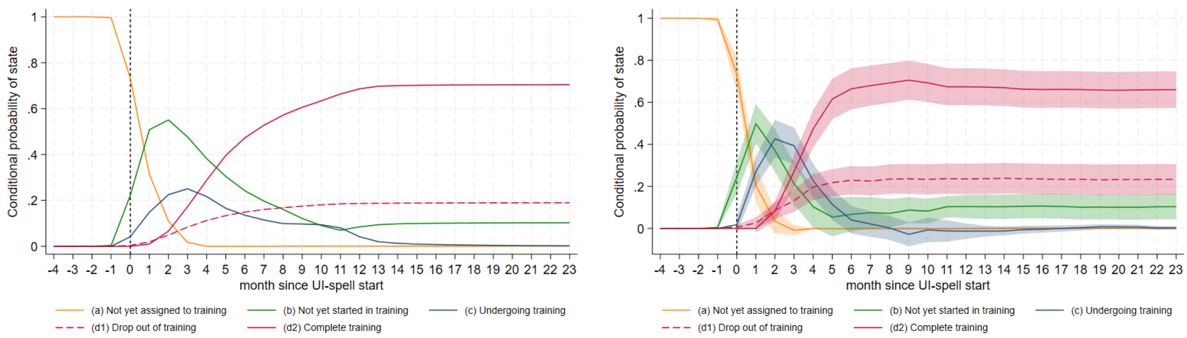
[39] This method is similar to Kline and Walters (2016); Hull (2018) who interact a single instrument with a stratifying covariate (in our case, time) to identify sub-treatments.

7.1 Training-State Probabilities

Figure 4 plots the training-state probabilities γ^s_{1t}, 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 89% of job seekers are done with their assigned training program (red lines), among whom 66% fully completed their courses (solid red line).[40] 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.

Figure 4: Training-State Probabilities



(a) OLS

(b) IV

Notes: This figure shows estimated state probabilities conditional on assignment to classroom training (γ^s_{1t}). The estimates are obtained by estimation of Equation (5). Panel (a) shows OLS estimates obtained by OLS regression. Panel (b) shows IV estimates obtained by instrumenting assignments to classroom training and on-the-job training by the predicted caseworker tendencies (according to the first-stage Equation (2)). All regressions include fully interacted job-center-unit-year fixed effects and controls for predetermined job seeker characteristics (see Appendix L.7). Colored bands represent 95% confidence intervals.

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

[40]Note the finding that 89% of job seekers progress into states (d1) and (d2) is *not* mechanical; if a job seeker exits unemployment before starting their training program, she will remain in her latest state ((b) or (c)) in all future periods. See Online Appendix G.1 for details.

G.2 supports this hypothesis by showing that the employment outcomes of assignees (OLS) are lower than the corresponding counterfactual outcomes for compliers (IV). To further support this claim, Appendix E.2 shows the baseline OLS and the OLS re-weighted by the IV training dynamics are very similar.

7.2 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 estimates, respectively.

Comparing the panels yields four findings. First, 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).[41] By contrast, the OLS suggests a strong negative threat effect. The difference between OLS and IV in estimated threat effects reflects that job seekers have worse job options while waiting for their assigned training program to begin.[42]

Second, OLS and IV are different in the long run because they identify very different post-program completion 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.[43] These findings suggest classroom training increases employment by *reskilling* job seekers and that classroom training could help mitigate structural challenges in the labor market. In Section 9.1, we investigate whether job seekers exposed to offshoring have higher gains from classroom training.

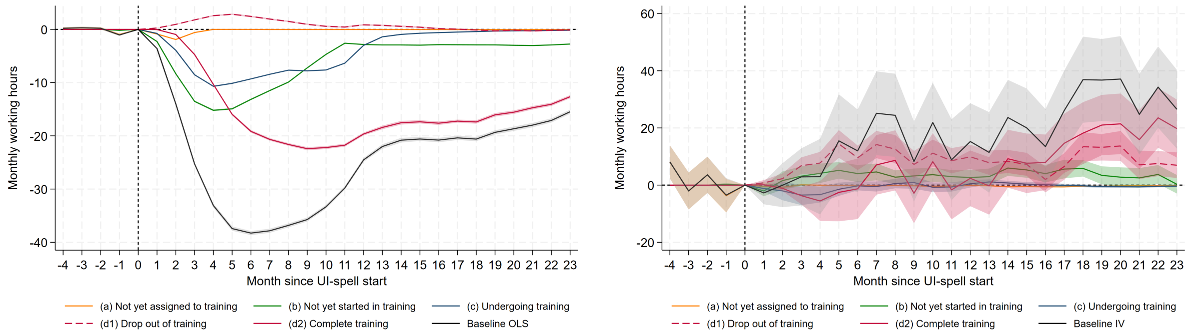
Finally, OLS and IV both estimate that participating in training (state (*c*)) lowers employment while dropping out (state (*d1*)) is associated with improved employment outcomes. Hence, the signs of OLS and IV do *not* differ due to their estimated "lock-in" or "drop out" effects.

[41] Black et al. (2003) shows the threat of training makes job seekers exit unemployment; we show job seekers exit unemployment for employment. Maibom (2022) provide similar results from an experiment in Denmark. In contrast to these studies, our long-run employment gains of training are driven by positive post-program effects.

[42] Appendix Figure G.2 shows average employment in state (b) is lower for the full population than in the subpopulation of compliers.

[43] Appendix Figure G.2 shows average employment in state (*d2*) is lower for the full population than in the subpopulation of compliers.

Figure 5: Decomposition of the Effect on Working Hours



(a) OLS

(b) IV

Notes: This figure shows a decomposition of the baseline estimate of the effect of assignment to classroom training on working hours in a given month relative to job loss. Panel (a) represents a decomposition of the baseline OLS estimate; panel (b) represents a decomposition of the baseline IV estimate. The baseline estimate is decomposed into contributions from each of the five training states, $\gamma_{1t}^{s} \times (\beta_{1t}^{1s} - \beta_{1t}^{0s})$, estimated using Equations (4)-(8). For the IV estimates, we instrument assignments to classroom training and on-the-job training by the predicted caseworker tendencies (according to the first-stage Equation (2)). The colored bands represent 95% confidence intervals. Standard errors are constructed based on 100 bootstrap repetitions (see Appendix J.1).

As previously mentioned, Appendix G.3 adopts an alternative strategy to estimate the state-specific causal effects in Figure 5. Figure 4 provides a simple reason why the different strategies yield similar results at longer horizons (after month 6): Because training assignees mostly complete their training ($\gamma_{1t}^{(d2)}$ approaches 65%-70% six months after UI spell start), the total employment effects β_{1t} will primarily be driven by the post-program effects $\beta_{1t}^{(d2)}$ after this horizon.

8 Heterogeneity across Training Programs

The preceding sections focused on classroom training, the largest category of programs, where our caseworker instruments have the most power. In this section, we study effect heterogeneity across different types of training programs.

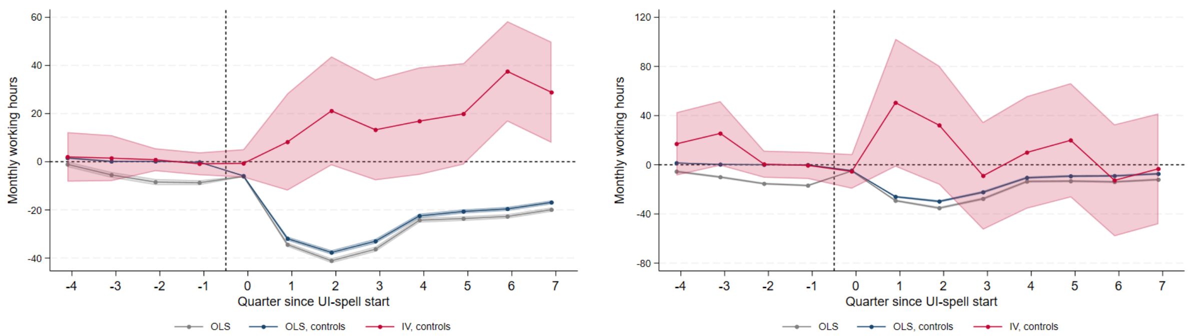
In Section 8.1, we first distinguish between classroom and on-the-job training. In Section 8.2, we next split classroom training into types of courses.

8.1 Classroom vs. On-The-Job Training

How does the effect of assignment to classroom training compare to the effect of assignment to on-the-job training?

Figure 6 plots the effect of assignment to classroom training and on-the-job training on average monthly hours of employment in a given quarter relative to job loss. Panel (a) repeats our main Figure 3, showing classroom training increases employment by 29 hours per month two years after initial job loss. Panel (b) shows the effects of assignment to on-the-job training. These are more imprecisely estimated than the effects of classroom training, not least because of very low statistical power in the first stage (see Table C.2), which prevents us from making firm conclusions. However, taking the estimates at face value suggests on-the-job training causes a large yet short-lived increase in hours of employment in quarters one and two after the initial job loss. These short-lived effects quickly die out.[44] Table H.1 summarizes the effects of assignment to classroom training and on-the-job training on labor market outcomes measured two years after initial job loss (in percent of pre-job loss levels). As already shown in Section 6, we find statistically significant and roughly similar-sized effects of assignment to classroom training on working hours, earnings, and extensive margin employment in quarter seven relative to job loss.[45] In contrast, and albeit with large confidence intervals, the figure shows there are no robust effects of assignment to on-the-job training on any of these outcomes.

Figure 6: Effects of Classroom and On-the-Job Training on Hours of Employment



(a) Classroom Training

(b) On-The-Job Training

Notes: This figure shows the effect of assignment to classroom training (Panel (a)) and on-the-job training (Panel (b)) on average monthly hours of employment ("montly 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 (see Appendix L.7). The red line represents the effect obtained by IV estimation, including the same set of controls. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals.

[44] We do not have the statistical power to split on-the-job training into internships and wage subsidies or between private and public sector jobs.

[45] Appendix Table D.5 of the updated paper reports the impacts of classroom and OTJ training on wages (conditional on employment) at different horizons. The table shows that OTJ has no statistically significant impact on wages (although the wide confident bands warrant soft conclusions). Classroom training has a positive effect on wages in quarter two after a job loss, which then rapidly dies out from quarter 3 and onward. The short-lived positive effect on wages could suggest duration dependence plays a role in explaining why classroom training does not increase earnings beyond the direct employment effect.

8.2 Disaggregation of Classroom Training

Are the positive employment effects of classroom training driven by specific types of courses? For example, courses that help workers apply for jobs could work differently than those that provide vocational skills. To investigate these issues, Appendix L.5.2 uses a new data set on *course titles* to split classroom training into "job-search" and "skills & wrap-around" courses.[46,47]

Using this classification, we extend our TSLS specification with an additional training state, splitting classroom training into "job search" and "skills and wrap-around" courses, which we instrument with the corresponding caseworker tendencies:

$$D_{ki} = \delta_{q(i)k} + \delta_{k1a}Z_{1ai} + \delta_{k1b}Z_{1bi} + \delta_{k2}Z_{2i} + \delta_{k3}X_i + \varepsilon_{1i} \tag{9}$$

$$Y_{it} = \beta_{q(i)t} + \beta_{1at}D_{1ai} + \beta_{1bt}D_{1bi} + \beta_{2t}D_{2i} + \beta_{3t}X_i + \varepsilon_{2it}, \tag{10}$$

Table 6 reports the first stage coefficients from the estimation of Equation (9). The table shows the caseworker instruments strongly predict assignment to "job search" courses (F-stat of 21.0) and "skills & wrap-around" courses (F-stat of 38.1), but that we have less power for the assignment to on-the-job training (F-stat of 8.5). The diagonal elements of Table 6 show that our caseworker instruments are clear predictors of their respective training programs. The table also reveals two borderline significant off-diagonals (at the 10% significance level). In particular, Row (1) shows that higher "job search" tendencies tend to pull workers away from "skills & wrap-around" classroom courses. This suggests some degree of substitutability between "job search" and "skills & wrap-around" courses; job seekers tend to be assigned to one or the other. It is important to note that the off-diagonal coefficient occurs *within* classroom training, and therefore does not invalidate our main analysis, where we distinguish between on-the-job training and classroom training.

[46] Table L.4 breaks down classroom training into narrower services. As the table shows, few job seekers are assigned ordinary education.

[47] As Appendix L.5.2 discusses, we combine "skills" and "wrap-around" courses into one category because their dividing lines are often less clear, and combining the two helps support the statistical power of our IV analysis.

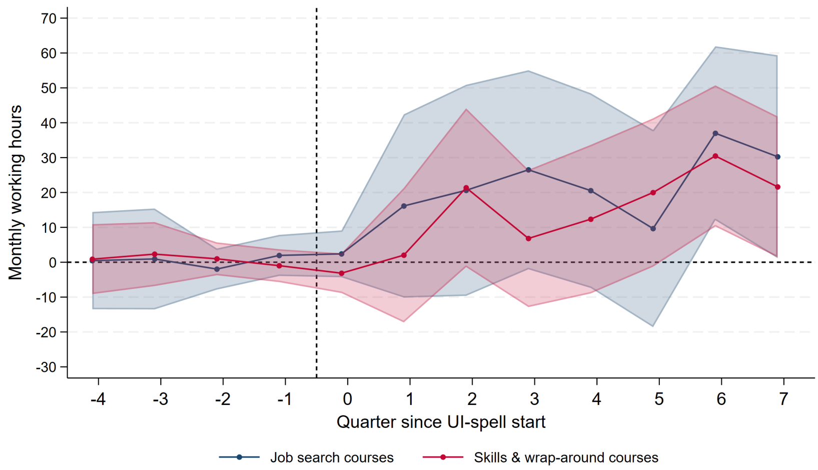
Table 6: First-Stage Estimates for Classroom Training Disaggregation

	(1) D(Job search courses)	(2) D(Skills & wrap-around courses)	(3) D(On-the-job training)
Z(Job search courses)	0.43***	−0.09*	0.05
	(0.06)	(0.05)	(0.04)
Z(Skills & wrap-around courses)	0.00	0.40***	−0.01
	(0.02)	(0.04)	(0.03)
Z(On-the-job training)	0.03*	0.01	0.22***
	(0.02)	(0.03)	(0.05)
Obs	167,222	167,222	167,222
F-stat (all Z's)	21.0	38.1	8.5
F-stat (own-Z)	60.1	111.0	22.8
Cov	yes	yes	yes

Notes: This table reports the first stage coefficients from estimations of Equation (9). All regressions include job-center-unit-year fixed effects as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

Figure 7 shows the IV estimates of the effect of assignment to "job search" courses and "skills & wrap-around" courses (within the first 12 months of unemployment) on average monthly working hours in a given quarter relative to initial job loss. The course types are similarly effective in increasing employment hours. In particular, "job search" and "skills & wrap-around" courses both increase employment by 20-30 hours per month two years after initial job loss. The difference in employment gains is not statistically significant (Appendix Table H.2). Table H.3 summarizes the effects of assignment to "job search" and "skills & wrap-around" courses on alternative labor market outcomes measured two years after initial job loss.

Figure 7: Working Hours in Quarter Relative to Job Loss

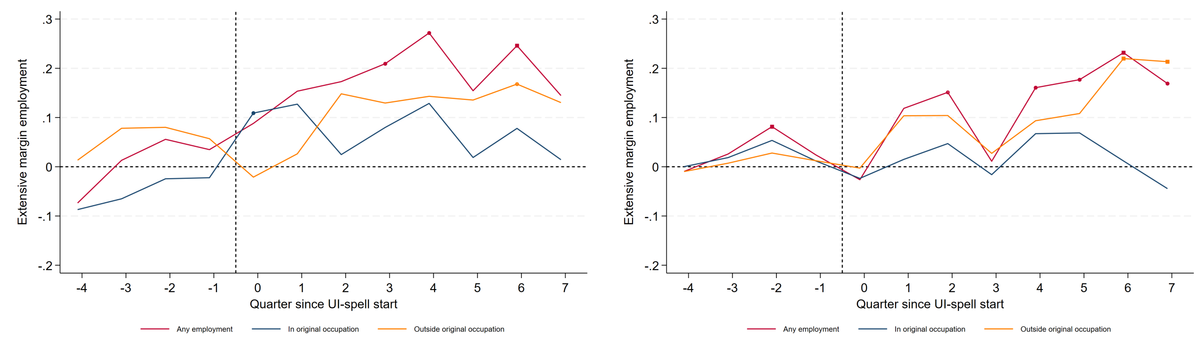


Note: This figure shows IV-estimates of the effect of assignment to "job search" courses and "skills & wrap-around" courses on average monthly hours of employment ("monthly working hours") in a given quarter relative to job loss. The effects arise from IV estimations of Equations (9)-(10). All regressions include fully interacted job-center-unit-year fixed effects, as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered on predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals.

8.2.1 Occupational Mobility

A core motivation for (vocational) training programs is to enhance the occupational mobility of job seekers. Figure 8 evaluates the goal by decomposing the employment effects of "job search" and "skills & wrap-around" courses into the probability of employment inside and outside the job seekers' previous occupation (the occupation held most frequently prior to job loss).[48] Interestingly, "job search" and "skills & wrap-around" courses appear to have different effects on the occupational mobility of job seekers. Panel (a) suggests that the short-run employment gains from "job search" courses are driven by employment *inside* workers' original occupations. By contrast, Panel (b) shows that the gains from "skills & wrap-around" courses are driven entirely by employment *outside* the job seekers' original occupations. The findings suggest "skills and wrap-around" courses work by helping workers switch to new occupations. By comparison, "job search" courses also push some job seekers to more quickly find jobs in their previous occupations.

Figure 8: Employment In and Outside Original Occupation



(a) "Job Search" Courses

(b) "Skills & Wrap-around" Courses

Notes: This figure shows the effect of assignment to "job search" courses (panel a) and "skills & wrap-around" courses (panel b) on any employment as well as employment probabilities inside or outside the job seeker's "original occupation". The IV estimates plotted are based on the main sample restricted to UI spells of job seekers for whom we could identify their "original occupation": the occupation held most frequently prior to job loss (99.1% of the main sample). The comparison of the "original occupation" and occupations held at any other point in time is based on 3-digit ISCO08 codes. All regressions include fully interacted job-center-unit-year fixed effects and controls for job seeker predetermined characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. Full (hollow) dots indicate significance at the 5% (10%) level, and squares indicate significance at the 1% level. Appendix Table H.4 reports standard errors for all coefficients plotted in the figure.

[48] The figure is based on three-digit occupational codes, but Appendix Figures H.1 -H.2 show that the results are quantitatively robust to using more or less disaggregated occupational codes.

9 Heterogeneity across Workers

This section studies heterogeneity in the effects of classroom training across different workers. In Section 9.1, we first explore whether classroom training is more beneficial to job seekers *exposed to offshoring*. In Section 9.2, we explore heterogeneity in treatment effects across job seekers' unobserved resistance to training by the estimation of *marginal treatment effects* (MTE). Finally, in Section 9.3, we use these estimates to evaluate counterfactual policies that target training programs to the job seekers with the largest estimated returns.

9.1 Exposure to Offshoring

Classroom training programs are often motivated by structural shifts in the labor market – e.g., caused by jobs moving abroad – rendering some skills obsolete in the domestic labor market. In this section, we investigate whether job seekers exposed to offshoring have larger gains from assignment to classroom training.

To investigate this, we adopt the offshorability index from Autor and Dorn (2013) and characterize all job seekers according to the offshorability of their typical occupation prior to job loss. For simplicity, we distinguish between occupations with "high" and "low" offshorability risk, split at the 75th percentile of the distribution of offshorability. Using this cutoff, many high-risk job seekers in our sample worked as office clerks prior to job loss, and many low-risk job seekers were employed as child-care workers. Online Appendix L.8 provides additional details and examples of the offshorability index.

Table 7 shows IV estimates of the effect of assignment to classroom training on monthly hours of employment, extensive-margin employment, and earnings in quarter 7 after job loss. The confidence bands for the offshoring splits are wide, and the results should thus be interpreted with caution. However, taking the point estimates at face value, job seekers at high risk of offshoring have larger gains from assignment to classroom training. In particular, Panel B shows that the assignment of high-risk job seekers to classroom training increases their employment by 54.9 hours and earnings by 8,900 DKK per month in quarter 7 after job loss. These substantial effects on employment and earnings reflect that compliers exposed to offshoring have poor job prospects if not receiving classroom training.

Table 7: Effects of Training by Exposure to Offshoring

	Labor market outcomes in quarter seven after job loss		
	Monthly working hours	Extensive margin employment	Monthly earnings
	(1)	(2)	(3)
Panel A: Low risk job seekers			
Classroom training	18.57	0.16*	4.53*
	(12.22)	(0.08)	(2.39)
On-the-job training	−5.76	−0.01	−4.14
	(24.90)	(0.17)	(5.04)
Panel B: High risk job seekers			
Classroom training	54.89**	0.33**	8.88*
	(21.62)	(0.15)	(4.77)
On-the-job training	11.78	−0.21	17.25
	(84.83)	(0.55)	(19.89)
Obs low risk	125,413	125,413	125,413
Obs high risk	41,809	41,809	41,809
Classroom training Z-stat	1.46	1.04	0.82
Classroom training P-value	0.14	0.30	0.41
On-the-job training Z-stat	0.20	-0.35	1.04
On-the-job training P-value	0.84	0.72	0.30

Note: This table shows IV estimates of the effect of assignment to classroom training and on-the-job training on labor market outcomes in quarter seven relative to job loss, for job seekers at high and low risk of offshoring. The estimates are obtained by separately estimating Equations (2)-(3) for job seekers at high- and low risk of offshoring. All regressions include job-center-unit-year fixed effects, as well as controls for job seeker pre-determined characteristics (see Appendix L.7). Standard errors are two-way clustered at the predicted caseworker and job seeker levels. The bottom of the table reports a test (Z-stat= $(\beta_h - \beta_l)/\sqrt{se_h^2 + se_l^2}$ and p-value) of whether the difference in coefficients in the two samples (e.g., the coefficient on classroom training for low vs. high risk) is statistically significant. *p<0.10 ** p<0.05 *** p<0.01.

Why may job seekers exposed to offshoring gain more from classroom training? In Section 7, 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 8.2.1, we showed classroom training (in particular “skills & wrap-around” courses) enhances occupational mobility by helping job seekers into new occupations. 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; Autor et al., 2014).

9.2 Marginal Treatment Effects

In this section, we investigate the selection patterns into classroom training by estimating marginal treatment effects (MTEs). In Section 9.3, we use the MTEs to evaluate policies that target training to the job seekers with the largest gains from the programs.

Building on the theoretical framework in Section 4, the MTEs correspond to the average treatment effect (ATE) among job seekers with a particular resistance to training. The resistances to training are in turn based on job seekers’ propensities to be assigned to the training programs. 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 B.4 details our estimation approach.

MTE and Policy Parameters. Figure 9 plots the MTE function for assignment to classroom training. Although imprecisely estimated, the MTE function reveals important insights. First, the MTE of assignment to classroom training is positive for all job seekers, varying between 17 and 45 additional hours of employment per month for job seekers with the lowest and highest resistance to training, respectively.

Second, the substantial LATE of classroom training estimated in Section 6 do not reflect that job seekers with the highest gains from training are more likely to opt into the programs. By contrast, the MTE curve slopes upwards, meaning that workers who are more resistant to the program have higher gains from classroom training.

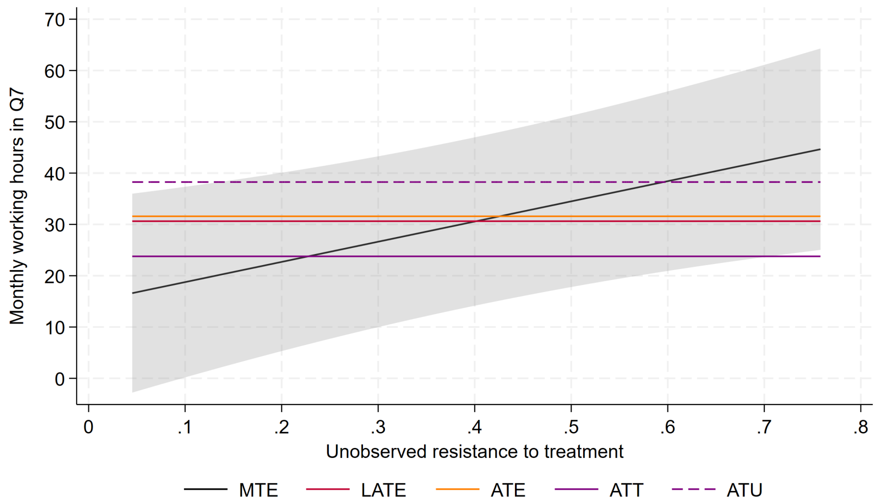
Third, as detailed in Appendix B.4.2, we can convert the estimated MTE functions into a host of parameters of interest. For example, the average treatment effect (ATE), the average treatment effects on the treated (ATT) and on the untreated (ATU), as well as the local average treatment effect (LATE). These policy parameters are also plotted in Figure 9.[49] The figure shows that our LATE estimate for instrument compliers is similar to the average effect of classroom training in the broader population of job seekers (ATE).

Finally, even after taking into account the negative selection on gains (as revealed by the positively sloped MTE), there are substantial gains of assignment to classroom training for the average treated job seeker. That is, the ATT suggests the average treated job seeker works 24 additional hours per month two years after initial job loss.[50]

[49]Reassuringly, the LATE of classroom training obtained through MTE integration (31 working hours per month in Q7) is comparable to the 2SLS estimate plotted in Figure 3 (29 working hours in Q7).

[50]Job seekers' resistance to training could be influenced by the generosity of UI benefits. From this perspective, the ATT might be lower in labor markets with lower UI replacement rates.

Figure 9: MTE, LATE, ATE, ATT and ATU of Classroom Training



Notes: This figure plots the MTE, LATE, ATE, ATT and ATU of assignment to classroom training on average monthly hours of employment ("monthly working hours") in quarter 7 after initial job loss. As detailed in Appendix B.4, the *MTE* estimates are obtained from a specification with second-order polynomials in the propensity scores for classroom training. The estimations are based on job seekers within the common support of the relevant propensity score (linear probability model based on predetermined job seeker characteristics, and trimmed by percentile 1 and 99).Note the x-axis in this figure differs slightly from that in Figure I.2, because the propensity scores are re-estimated based on the trimmed common support sample). The shaded area represents 90% confidence intervals for the MTE function (these do not account for generated regressors). The *LATE* estimate is obtained by averaging over individual LATE estimates. These are obtained by integrating the MTE function from the propensity score if the job seeker was assigned the least vs. most training-inclined caseworker (as approximated by percentile 1 and 99 on the relevant caseworker tendency instrument). The *ATE* estimate is obtained by integrating the MTE function over the common support of the propensity score. The *ATT* estimate is obtained by integrating the MTE function, while giving more weight to low-U_1 job seekers. The *ATU* estimate is obtained by integrating the MTE function, while giving more weight to high-U_1 job seekers.

The positive slope of the MTE curve reflects that job seekers with higher resistance to classroom training have larger gains from the programs. Could this heterogeneity reflect that high MTE job seekers receive different types of training courses? To investigate this, Appendix Figure I.5 correlates job seekers' propensity scores for classroom training with the duration of their first assigned training courses. The idea behind the test is that trainees with a higher observed propensity score have, on average, a higher unobserved resistance to training and, thus, MTEs. The figure shows that the correlation is small and insignificant, both economically and statistically. For example, going from a propensity score of 0.4 to 0.5 lowers the predicted training days from 33 to 31 days. Similarly, Figure I.6 correlates *caseworkers'* training tendencies with the duration of their first training assignments. The idea is that high-tendency caseworkers are more likely to treat job seekers with higher MTEs of training. Again, the effects are small, economically and statistically. For example, going from a training tendency of 0.25 to 0.55 lowers the predicted training days from 36 to 34 days. These results indicate that high-MTE job seekers are not assigned to longer (and thus more costly) courses such that reallocating training to these job seekers would not increase program costs. In the next section, we evaluate the effects of such targeted policy experiments.

9.3 Policy Experiments

We now use our MTE functions to evaluate policies that target classroom training to the job seekers with the highest estimated returns to the programs.

In Column (1) of Table 8, we first use the ATT estimates to evaluate the effects of classroom training in the current policy regime. The current policy increases the earnings of trainees by about 5,200 DKK ($750) per month in quarter 7 after job loss. Figure D.2 shows that the effects are stable and statistically significant in quarters 6 to 9. In Appendix K, we calculate that these earnings effects of classroom training are 2.7 times higher than the average program costs. Furthermore, discounting the flows of costs and benefits, the net present value of classroom training is positive for any annual interest rate below 75%. Notably, this is a very high internal rate of return (IRR) on investment. In comparison, the "returns to college" literature often finds IRRs around 12% (Kane and Rouse, 1995; Heckman et al., 2003).

Despite these substantial benefits in the status quo, Sections 9.1 and 9.2 indicate that we could do even better by targeting training to job seekers exposed to offshoring or with the MTEs. The next subsections evaluate such targeted policies.

9.3.1 Target Job Seekers Exposed to Offshoring

To evaluate the effects of assigning classroom training to job seekers at high risk of offshoring relative to the status quo assignment regime, we first estimate the MTE functions separately by risk groups. In Column (2) of Table 8, we then report the ATE when (the 25%) job seekers at high risk of offshoring are assigned to classroom training.[51] Comparing the estimates to the status quo in Column (1) shows that targeting classroom training toward job seekers at high risk of offshoring could generate 28.2 additional hours of employment and 4,900 DKK ($710) higher earnings per trainee per month, almost a doubling of the current rate of return.

9.3.2 Target High-MTE Job Seekers

Column (3) of Table 8 we evaluate the effects of targeting classroom training to the job seekers with the largest MTEs, keeping the fraction of job seekers assigned to classroom training fixed at 39%. Comparing Columns (3) and (1) shows that targeting classroom training toward job

[51] Appendix Figure I.7 shows the MTE, LATE, ATE, ATT, and ATU of classroom training for job seekers at high and low risk of offshoring. The estimates are similar for each risk group, which partly reflects that their MTE curves are relatively flat.

Table 8: Policy Experiments – Targeting Classroom Training (Returns Per Assignee)

	Status Quo	Target high offshorability	Target high MTE
	(1)	(2)	(3)
Monthly working hours	23.7	51.9	41.0
	(10.4)	(20.4)	(11.1)
Monthly earnings (1,000 DKK)	5.2	10.1	8.5
	(2.1)	(4.1)	(2.2)

Notes: This table reports the returns per assignee from classroom training on average monthly hours of employment ("working hours") and average monthly earnings (1,000 DKK) in quarter seven relative to job loss, under different policy scenarios. Column (1) reports the returns per assignee in the status quo regime, where 39% of all job seekers are assigned (ATT). Column (2) reports the returns per assignee when classroom training is targeted to the 25% job seekers at high risk of offshoring. This policy counterfactual is obtained by estimating and integrating the MTE function (for high risk job seekers only) over the common support of the propensity score (ATE). Column (3) reports the returns per assignee when classroom training is targeted to the 39% of job seekers with the highest MTE. This policy relevant treatment parameter (PRTE) is obtained by integrating the MTE function (estimated for the entire population of job seekers) over the top 39% of the U_1 distribution, see Equation (52) in Appendix B.4.2. Standard errors (reported in parentheses) do not account for generated regressors.

seekers with the highest MTEs could generate 17.3 additional hours of employment and 3,300 DKK ($482) higher earnings per trainee per month, a 60-70% increase over the status quo. As discussed in Section 9.2, Appendix Figures I.5 and I.6 suggest that high-MTE job seekers are not assigned to more longer training courses. This suggests that targeting training courses to high-MTE job seekers could boost earnings benefits without increasing program costs.

9.3.3 Summary

The policy experiments above reveal large employment gains from better targeting of classroom training to the job seekers with the largest return to the programs.[52] In particular, employment could be increased by around 15-30 hours per month per trainee by targeting the program toward job seekers with the highest risk of offshoring or MTEs.[53] Our cost-benefit calculations in Appendix K show that the earnings effects of classroom training are several times higher than the program costs.[54] As Sections 6 and Appendix F show, these large employment gains can arise because the targeted job seekers have poor employment prospects without the training programs.

[52]Our results are consistent with the result of Lechner and Smith (2007) who show caseworkers do not target training to the job seekers with the largest gains (e.g., highest MTEs), and that statistical treatment rules (based on worker observables, such as exposure to offshoring) would do better than caseworker assignments.

[53]While targeting training based on our observable index of offshorability is straightforward, it is more difficult to target job seekers with the highest MTEs. In particular, because job seekers with the highest MTEs have the highest resistance to training in our case, policymakers cannot rely on self-selection to implement the targeted policies.

[54]The structural estimates of Maibom (2022) warrant caution about using the pecuniary costs and benefits to infer the welfare effects of mandatory reemployment programs, because the high-MTE job seekers may have large non-pecuniary costs of attending these programs (as revealed by their resistance to participate in these programs.)

10 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 29% higher hours of employment. By contrast, we do not have the statistical power to detect the effects of on-the-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 effects on employment. 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 primarily are driven by job seekers with *worse job prospects* opting into training. Further, we show the long-run employment effects detected by our IV estimates are driven by post-program effects rather than threat effects. That is, the increase in employment is mainly generated by job seekers who actually complete the training programs. We further split classroom training into "job search" and "skills and wrap-around" courses, and show that the latter especially helps workers switch occupations. This 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 there are large employment gains from better targeting of classroom training to the job seekers with the largest returns to the programs, for example job seekers exposed to offshoring.

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

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A Analysis Sample

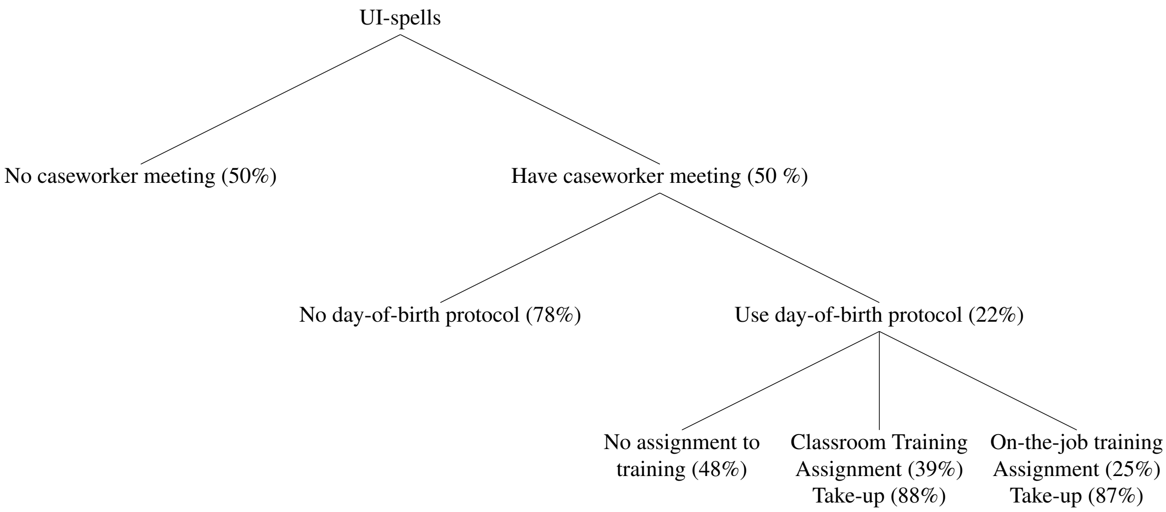
This section describes our analysis sample. Table A.1 shows how different restrictions shrink our analysis sample, and Figure A.1 shows a tree chart of our analysis sample of trainees. Figure A.2 shows the geographical coverage of our analysis sample.

Table A.1: Analysis Sample Restrictions

				Caseworkers		Average	
	UI-spells	Jobseekers	Jobcenters	Realized	Predicted	Weeks of UI	Compliance
Linked jobseeker-caseworker data	**934,922**	**628,352**	**94**	**7,910**	**1,949**	**33.35**	**0.42**
- Only j-u-y's using day-of-birth	203,101	152,057	51	2,330	680	32	0.52
- No non-western immigrants	196,522	146,964	51	2,289	680	32	0.52
- Previous occupation observed	192,813	144,046	51	2,270	680	32	0.52
- Min two caseworkers per j-u-y	186,604	139,342	51	2,245	670	32	0.52
- Caseload size $\geq$ 50	183,778	137,477	51	2,225	577	32	0.52
Full sample (2012-2018)	**183,778**	**137,477**	**51**	**2,225**	**577**	**31.88**	**0.52**
Balanced sample (2012-2017)	**167,222**	**127,713**	**51**	**2,060**	**536**	**31.92**	**0.52**

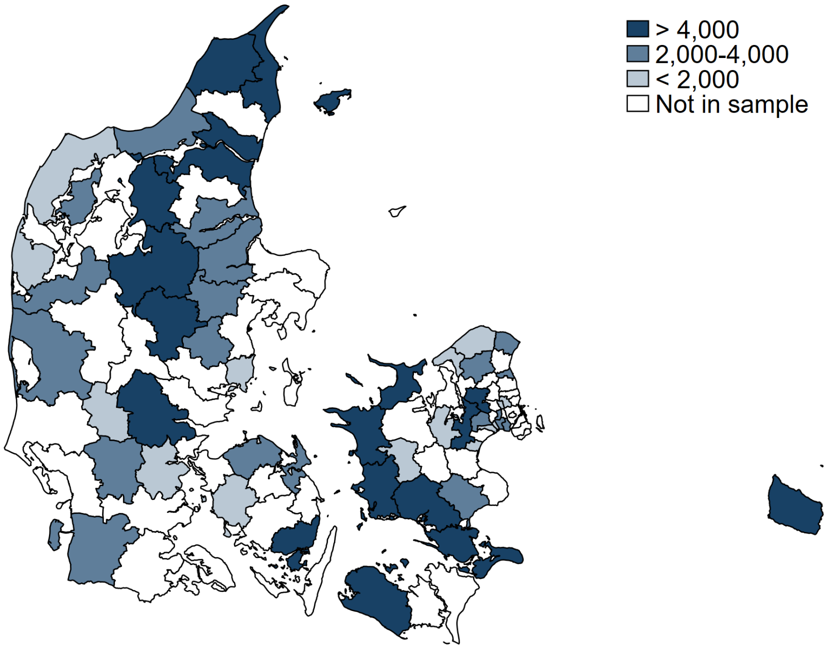
Notes: This table shows the number of units (UI-spells, job seekers, job centers, realized and predicted caseworkers) retained after each sample-selection step along with sample descriptives (average UI-spell length and the compliance between realized and predicted caseworker). Row (1) reports the statistics for all job seekers, who i) initiated a UI spell from 2012-2018 and ii) can be linked to a caseworker (i.e. all job seekers in the linked job seeker–caseworker data set; see Online Appendix Table L.1). Row (2) restricts the sample to job-center-unit-years that use a clear (block) structure for the allocation of birthdays. Row (3) restricts the sample to job seekers with no non-Western immigrants. Row (4) restricts the sample to job seekers for whom we observe their previous occupation. Row (5) restricts the sample to job-center-unit-years with minimum two (predicted) caseworkers. Row (6) restricts the sample to (predicted) caseworkers who were assigned at least 50 job seekers. Row (7) is identical to row (6) and summarizes the final (full) analysis sample. Row (8) reports statistics for a balanced analysis sample; job seekers for whom we observe their labor market outcomes throughout the first two years after job loss. These are job seekers who initiated a UI spell from 2012 to 2017.

Figure A.1: Tree Chart of Analysis Sample

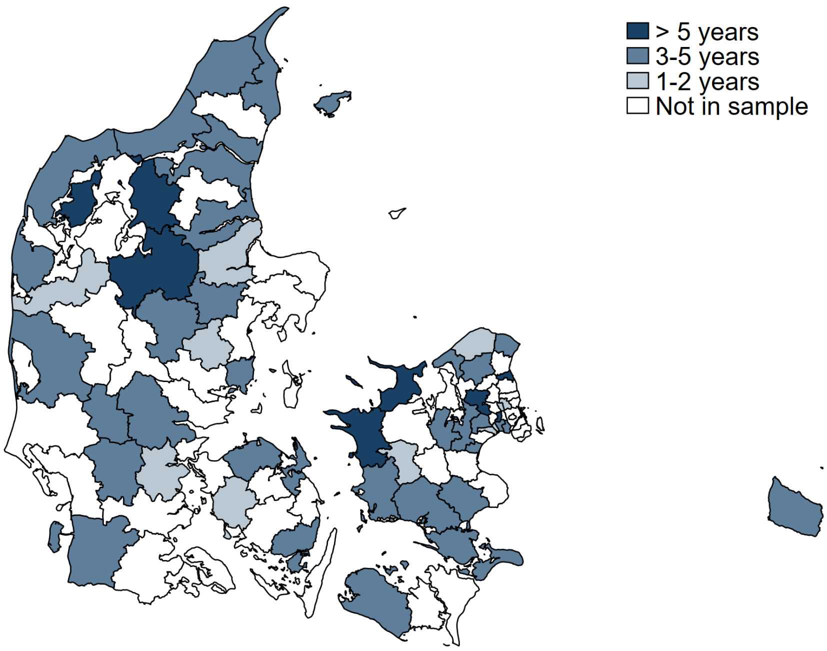


Notes: This figure provides a tree chart of our main analysis sample of UI-spells (periods in which the individual receives UI-benefits). For 50% of all UI-spells initiated from 2012-2018, the job seeker had at least one caseworker meeting (cf. Table L.1). 22% of UI-spells were initiated in a job-center-unit-year that uses a day-of-birth protocol (cf. Table A.1). Among UI-spells in the final analysis sample, 48% of UI-spells are not assigned to any training, 39% are assigned to classroom training and 25% to on-the-job training (12% are assigned to both types of training) within the first 12 months of job loss (Cf. Table 2). Using job seekers who are still unemployed when the assigned program starts as a proxy for take-up rates, the take-up of classroom training and on-the-job training is 88% and 87%, respectively.

Figure A.2: Geographical Coverage of Analysis Sample



(a) Number of job seekers



(b) Number of years

Notes: This figure breaks down the full analysis sample by job centers in Denmark (i.e. UI spells from 2012 to 2018). 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).

B 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).

B.1 Setup

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

$$Y_i(D_i) = \beta_{0i} + \beta_{1i}D_{1i} + \beta_{2i}D_{2i}. \tag{11}$$

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.[55]

The role of caseworkers' preferences is motivated by our qualitative interviews at the job centers, during which a caseworker, for example, ascribed differences in training tendencies to reflect differences in the "values" ("*værdisæt*" in Danish) of caseworkers.

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

$$D_{ki} = \mathbf{1}[V_{kc_i} \geq U_{ki}], \tag{12}$$

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:

$$V_{c(b_i)} \perp\!\!\!\perp (U_i, \beta_i) \tag{13}$$

[55]Maibom (2022) provides a structural interpretation of job seekers "resistances to training" as their non-pecuniary costs of attending these programs.

A sufficient condition for Equation (13) is that job seekers' training and employment potential are unrelated to their day of birth. Note Equation (13) allows for both differences in the 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.[56]

B.1.1 Selection into Training

The selection patterns into training programs are governed by how the job seekers' resistance to training U_i correlates with their employment potential β_i in Equation (11). 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.

B.2 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:

$$Z_{ki} = \frac{1}{(J(i)-1)} \sum_{j \neq i} \mathbf{1}[c(b_j) = c(b_i)] \times D_{kj}, \quad J(i) = \sum_{j} \mathbf{1}[c(b_j) = c(b_i)]. \tag{14}$$

Because job seekers are quasi-randomly allocated to caseworkers (Equation (13)), a caseworker's preferences are revealed by her observed k-tendency as she handles a large number of cases:

$$Z_{ki} \xrightarrow{p} \mathbb{P}(V_{kc(b_i)} \geq U_{kj}) = V_{kc(b_i)} \quad \text{as} \quad N_{c(b_i)} \to \infty. \tag{15}$$

We now show the caseworker-tendency instruments satisfy the *independence*, *exclusion*, and *monotonicity* conditions for the identification of local average treatment effects (LATE)

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

of training programs (Imbens and Angrist, 1994). To ease the exposition, we first assume job centers perfectly comply with the day-of-birth rule to allocate job seekers to caseworkers, $c_i = c(b_i)$. In Section B.5, we extend our identification results to allow for non-compliance with the allocation rule.

Independence and Exclusion. Because caseworker training tendencies recover preferences (V_1, V_2), it follows from Equation (13) that they satisfy the independence and exclusion restrictions:

$$Z_i \perp\!\!\!\perp \beta_i. \tag{16}$$

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

Extended Monotonicity. In the canonical binary-treatment case, monotonicity requires that the instrument shifts 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 (15), we can restate Equation (12) in terms of caseworker tendencies:

$$D_{ki} = \mathbf{1}[Z_{ki} \geq U_{ki}]. \tag{17}$$

Let $D_i(Z)$ denote the potential training assignments of job seeker i depending on the training tendencies Z of his caseworker. Equation (17) implies the assignment to classroom training and on-the-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:

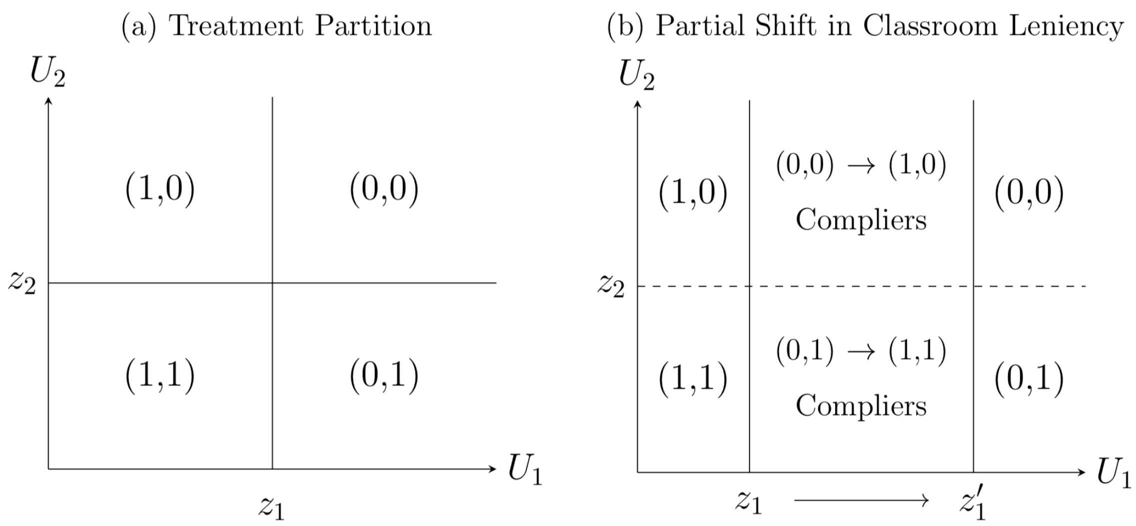
$$z'_k > z_k \implies D_{ki}(z'_k, z_l) \geq D_{ki}(z_k, z_l) \tag{18}$$

$$D_{li}(z'_k, z_l) = D_{li}(z_k, z_l), \quad k \neq l, \forall i. \tag{19}$$

The property in Equations (18)-(19) 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, 2024). 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.[57]

Figure B.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' resistance to classroom training and on-the-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 B.1: Assignment to Training Program



Notes: This figure illustrates the concept of extended monotonicity. The horizontal and vertical axes measure job seekers' resistance to classroom training and on-the-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 $z_1 \rightarrow z_1'$, holding on-the-job training tendency fixed at z_2, shifts some job seekers toward classroom training but does *not* alter the participation in on-the-job training.

Panel (b) illustrates how a partial increase in classroom training tendency $z_1 \rightarrow z_1'$, holding on-the-job training tendency fixed at z_2, shifts some job seekers toward classroom training but does *not* alter the participation in on-the-job training. The area between z_1 and z_1' represents job

[57]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 colleges divert some students from enrolling in four-year colleges. Kirkeboen et al. (2016) show crossing the admission threshold between a preferred and a next-best major changes the likelihood that students enroll in alternative majors to the next-best option.

seekers who are shifted (horizontally) into classroom training. The new trainees come from two margins: passive UI $(0, 0) \to (1, 0)$ and on-the-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 on-the-job training.

B.3 Local Average Treatment Effects

Proposition 1 formalizes how the extended monotonicity property is sufficient to identify LATEs of training program for job seekers whose training assignments depend on the preferences of their caseworker. 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.

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*

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

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

3. *The caseworker-tendency instruments developed in Section B.2 identify the mean potential outcome for each treatment d'' of 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

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

Proof. Consider caseworkers A and B, who have the same tendency to assign on-the-job training but differ in their tendency for classroom training.[58] Comparing the share of job seekers

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

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 on-the-job-training status, we can calculate the share of compliers along each separate margin:

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

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

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:

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

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

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

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

□

B.3.1 Econometric Implementation

Following Proposition 1, we may identify the causal effects of classroom training by regressing the outcome variables

$$T = \{D_1 D_2, D_1(1-D_2), (1-D_1)D_2, (1-D_1)(1-D_2), .. \tag{33}$$

$$Y D_1 D_2, Y D_1(1-D_2), Y(1-D_1)D_2, Y(1-D_1)(1-D_2)\} \tag{34}$$

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

$$T_i = \beta_0^T + \beta_1^T Z_{i1} \quad \text{for} \quad z_{i2} \in [z_2' - \epsilon_2, z_2' + \epsilon_2]. \tag{35}$$

Having estimated Equation (35), we may recover mean potential outcomes for classroom training $d \in \{0,1\}$ along each margin of on-the-job training using the Wald ratios:

$$\mathbb{E}\left[Y(d,0)|U_1^{(c,0)}(z_2')\right] = \frac{\hat{\beta}_1^{Y(D_1=d)(1-D_2)}}{\hat{\beta}_1^{(D_1=d)(1-D_2)}} \tag{36}$$

$$\mathbb{E}\left[Y(d,1)|U_1^{(c,1)}(z_2')\right] = \frac{\hat{\beta}_1^{Y(D_1=d)D_2}}{\hat{\beta}_1^{(D_1=d)D_2}}. \tag{37}$$

To increase power, we can stack the point-specific evaluations in Equation (35) into a single regression, controlling flexibly for the on-the-job training instrument (e.g., using bins of bandwidth ϵ_2),

$$T_i = \beta_0^T + \beta_1^T Z_{i1} + g_{\epsilon_2}(Z_{i2}; \beta_2^T), \tag{38}$$

Note Equations (36)-(38) 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. Section B.4 presents a marginal treatment effects framework that shows that 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 this case of no "essential heterogeneity" (Heckman et al., 2006), we only need to control for Z_{i2} to the extent that the instruments are correlated.[59] Appendix Figure C.1 shows that Z_1 and Z_2 are largely orthogonal in our data, alleviating this concern for identification.

[59] Heckman et al. (2006) define "no essential heterogeneity" as $\text{Cov}(\beta_i, D_i) = 0$. Because selection into treatment D_i is governed by job seekers' resistances U_i, this condition is equivalent to $\text{Cov}(\beta_i, U_i) = 0$.

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; Autor 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 on-the-job-training instrument.

B.4 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:

$$\mathbb{E}[\beta_{i0}|U_i] = \alpha_{00} + \alpha_{01}U_1 + \alpha_{02}U_2 \tag{39}$$

$$\mathbb{E}[\beta_{i1}|U_i] = \alpha_{10} + \alpha_{11}U_{1i} \tag{40}$$

$$\mathbb{E}[\beta_{i2}|U_i] = \alpha_{20} + \alpha_{21}U_{2i}. \tag{41}$$

These shape restrictions are the minimal extension that allows for heterogeneous MTEs and allow us to state conditions under which the conventional 2SLS specification identifies well-defined treatment effects.

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").[60]

B.4.1 Estimation

Given the shape restrictions specified in (39)-(41), we can write the employment outcome for job seekers assigned to a caseworker with leniency Z, as a function of second-order polynomials

[60]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 D.4.(a), showing that never-takers of training have higher employment rates than compliers not assigned to training.

in the propensity score with respect to classroom and on-the-job training. Namely, plugging (39)-(41) into (11), we get

$$\begin{aligned}
\mathbb{E}[Y_i|Z_i] &= \int_0^1 \int_0^1 (\alpha_{00} + \alpha_{01}U_1 + \alpha_{02}U_2) dU_1 dU_2 \\
&\quad + \int_0^{\hat{D}_{i1}} (\alpha_{10} + \alpha_{11}U_1) dU_1 + \int_0^{\hat{D}_{i2}} (\alpha_{20} + \alpha_{21}U_2) dU_2 \\
&= \beta_0 + \beta_{11}\hat{D}_{i1} + \beta_{21}\hat{D}_{i2} + \beta_{12}\hat{D}_{i1}^2 + \beta_{22}\hat{D}_{i2}^2 \qquad (42)
\end{aligned}$$

with

$$\beta_0 = \alpha_{00} + \frac{\alpha_{01} + \alpha_{02}}{2}, \quad \beta_{11} = \alpha_{10}, \quad \beta_{12} = \frac{\alpha_{11}}{2}, \quad \beta_{21} = \alpha_{20}, \quad \beta_{22} = \frac{\alpha_{21}}{2}, \qquad (43)$$

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 (43) simplifies to the standard TSLS estimator in Equations (2)-(3) if MTEs are constant ($\alpha_{11} = \alpha_{21} = 0$).

Our main estimates are based on a linear probability model (LPM) to estimate the propensity scores. Appendix Figures I.3 and I.4 show our MTE estimates are robust to using a logit model instead.

B.4.2 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). In the following, we provide examples of different parameters related to classroom training.

We can recover the local average treatment effect (LATE) among instrument compliers by averaging over individual LATEs. These are obtained by integrating the MTE function between the propensity score if the job seeker was assigned to the least vs. most training-inclined caseworker (approximated by percentile 1 and 99 on the caseworker tendency instrument),

$$\begin{aligned}
\text{LATE}_1 i &= \mathbb{E}\left[\frac{1}{\hat{D}_{1i}^{strict} - \hat{D}_{1i}^{lenient}} \int_{\hat{D}_{1i}^{lenient}}^{\hat{D}_{1i}^{strict}} (\hat{\alpha}_{10} + \hat{\alpha}_{11}U_1) dU_1\right] \qquad (44) \\
&= \hat{\alpha}_{10} + \frac{\hat{\alpha}_{11}}{2} \times \mathbb{E}\left[\hat{D}_{1i}^{strict} + \hat{D}_{1i}^{lenient}\right] \qquad (45)
\end{aligned}$$

We can recover the average treatment effect (ATE) in the population by integrating the MTE function over the common support of the propensity scores, [61]

$$\text{ATE}_1 = \mathbb{E}\left[\frac{1}{\hat{D}_1^{max} - \hat{D}_1^{min}} \int_{\hat{D}_1^{min}}^{\hat{D}_1^{max}} (\hat{\alpha}_{10} + \hat{\alpha}_{11}U_1)dU_1\right] \tag{46}$$

$$= \hat{\alpha}_{10} + \frac{\hat{\alpha}_{11}}{2} \times \mathbb{E}\left[\hat{D}_1^{max} + \hat{D}_1^{min}\right] \tag{47}$$

We may also recover the average treatment effect on the treated (*ATT*), which is a weighted average of the MTE function that gives more weight to individuals with *low*-U_1

$$\text{ATT}_1 = \mathbb{E}\left[\frac{1}{\hat{D}_1 - \hat{D}_1^{min}} \times \int_{\hat{D}_1^{min}}^{\hat{D}_1} (\hat{\alpha}_{10} + \hat{\alpha}_{11}U_1)dU_1|D_1 = 1\right] \tag{48}$$

$$= \hat{\alpha}_{10} + \frac{\hat{\alpha}_{11}}{2} \times \mathbb{E}\left[\hat{D}_1 + \hat{D}_1^{min}|D_1 = 1\right] \tag{49}$$

The counterpart is the average treatment effect on the *untreated* (ATU), which corresponds to a weighted average of the MTE function that gives more weight to individuals with *high*-U_1

$$\text{ATU}_1 = \mathbb{E}\left[\frac{1}{\hat{D}_1^{max} - \hat{D}_1} \times \int_{\hat{D}_1}^{\hat{D}_1^{max}} (\hat{\alpha}_{10} + \hat{\alpha}_{11}U_1)dU_1|D_1 = 0\right] \tag{50}$$

$$= \hat{\alpha}_{10} + \frac{\hat{\alpha}_{11}}{2} \times \mathbb{E}\left[\hat{D}_1^{max} + \hat{D}_1|D_1 = 0\right] \tag{51}$$

We may also evaluate alternative policies, for example, the effect of assigning x% of job seekers with the largest MTE's to classroom training. To evaluate this policy relevant treatment parameter (PRTE), we integrate the MTE function over the top x% of the U_1 distribution,

$$\text{PRTE} = \mathbb{E}\left[\frac{1}{\hat{D}_1^{max} - P} \times \int_{P}^{\hat{D}_1^{max}} (\hat{\alpha}_{10} + \hat{\alpha}_{11}U_1)dU_1\right] \tag{52}$$

$$= \hat{\alpha}_{10} + \hat{\alpha}_{11}(\hat{D}_1^{max} + P) \tag{53}$$

where P corresponds to percentile 1-x% in a uniform distribution from $(\hat{D}_1^{min}, \hat{D}_1^{max})$.

B.5 Non-Compliance with Caseworker Allocation Rule

In this extension, 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, independence, exclusion, and monotonicity conditions for the identification of LATEs.

[61] The (pseudo) min and max of propensity scores, $(\hat{D}_1^{min}, \hat{D}_1^{max})$.

Independence and Exclusion

To assess independence and exclusion, we note the variation in the caseworker-tendency instrument (Equation (14)) comes solely from the birthdays of job seekers. Hence, the instruments satisfy the independence and exclusion criteria if job seekers' training and employment potentials are unrelated to their birthday in the month:

$$b_i \perp\!\!\!\perp (U_i, \beta_i). \tag{54}$$

Relevance and Monotonicity

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

$$V_{kc(b_i)} > V_{kc(b_j)} \implies V_{kc_i} \geq V_{kc_j},\ V_{lc_i} = V_{lc_j}, \quad k \neq l. \tag{55}$$

The "monotonic compliance" condition in Equation (55) 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 caseworkers with training tendencies below the average.

C Instrument Diagnostics

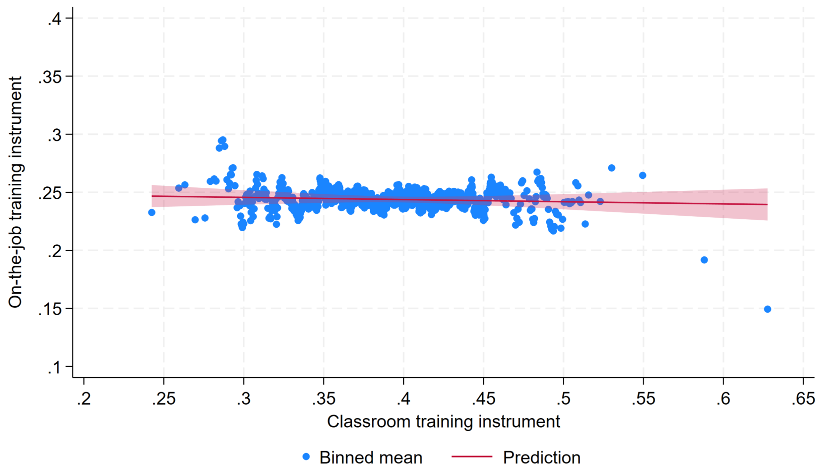
This section presents diagnostic statistics on our caseworker-tendency instruments. We first present summary statistics on the instruments before examining the relevance, monotonicity, and exclusion criteria for interpreting the IV estimates as LATE for compliers.

Table C.1: Summary of Caseworker-Tendency Instruments

	(1) count	(2) mean	(3) sd	(4) p1	(5) p10	(6) p25	(7) p50	(8) p75	(9) p90	(19) p99
Classroom training instrument										
Z_1	167,222	0.39	0.12	0.15	0.24	0.32	0.38	0.45	0.55	0.73
Z_1, residualized by $q(i)$	167,222	0.39	0.04	0.29	0.34	0.37	0.39	0.42	0.44	0.50
Z_1, residualized by $q(i)$, Z_2	167,222	0.39	0.04	0.29	0.34	0.37	0.39	0.42	0.44	0.50
Z_1, residualized by $q(i)$, Z_2, cov	167,222	0.39	0.04	0.29	0.34	0.37	0.39	0.42	0.44	0.50
On-the-job training instrument										
Z_2	167,222	0.24	0.07	0.12	0.17	0.19	0.23	0.28	0.35	0.46
Z_2, residualized by $q(i)$	167,222	0.24	0.03	0.16	0.21	0.23	0.24	0.26	0.28	0.33
Z_2, residualized by $q(i)$, Z_1	167,222	0.24	0.03	0.16	0.21	0.23	0.24	0.26	0.28	0.33
Z_2, residualized by $q(i)$, Z_1, cov	167,222	0.24	0.03	0.16	0.21	0.23	0.24	0.26	0.28	0.33

Notes: This table reports the mean, standard deviation an pseudo-percentiles for the caseworker-tendency instruments. Due to data confidentiality, the data has been collapsed such that a pseudo-percentile is based on the five job seekers closest to the actual percentile. Z_{1i} and Z_{2i} represent the classroom- and on-the-job-training instruments, $q(i)$ represent fully interacted job-center-unit-year fixed effects, and cov represents predetermined job seeker characteristics (see Appendix L.7).

Figure C.1: Correlation between Caseworker Tendencies



Notes: This figure shows the correlation of caseworker training tendencies. Each bin represents the caseworker tendency for 100 job seekers in the sample demeaned by job-center-unit-year fixed effects and predetermined job seeker characteristics (see Appendix L.7). The red line represents the linear prediction obtained by OLS regression of the demeaned on-the-job-training instrument on the demeaned classroom-training instrument. The shaded areas represent 95% confidence intervals based on standard errors clustered at the level of the predicted caseworker. The regression line has a slope of -0.018 (with a t-stat of -0.62).

C.1 Relevance

Table C.2: First-Stage Estimates of Classroom and On-the-Job-Training Assignment

	(1) D(Classroom training)	(2) D(On-the-job training)
Z(Classroom training)	0.38***	0.01
	(0.04)	(0.02)
Z(On-the-job training)	0.05	0.21***
	(0.03)	(0.05)
Obs	167,222	167,222
F-stat (all Z's)	53.0	10.7
F-stat (own-Z)	105.5	21.5
Own-instrument pseudo-pct 1	0.29	0.16
Own-instrument pseudo-pct 99	0.50	0.33
Complier share	0.08	0.04
Cov	yes	yes

Notes: This table reports the first stage coefficients from estimations of Equation (2). All regressions include job-center-unit-year fixed effects as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered at the predicted caseworker and job seeker levels. The complier share with respect to treatment k is obtained in two steps. First, we 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, job seeker controls 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 J.2. *p<0.10 ** p<0.05 *** p<0.01.

Table C.3: First-Stage Estimates of Classroom-Training Assignment

	D(Classroom training)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Z(Classroom training)	1.00***	1.00***	0.95***	0.67***	0.65***	0.38***	0.38***	0.38***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)
Z(On-the-job training)	0.00	0.00	−0.02	−0.09***	−0.06*	0.05	0.05	0.05
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
Obs	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222
F-stat (all Z's)	7694.8	5316.7	4727.4	402.1	371.9	97.5	54.1	53.0
Caseworker[(a)]	Realized	Predicted	Predicted	Predicted	Predicted	Predicted	Predicted	Predicted
Leave-out[(b)]	-	-	spell	spell	spell	spell	spell	spell
FE's[(c)]	-	-	-	jxu	jxu + y	jxuxy	jxuxy	jxuxy
Clustering[(d)]	-	-	-	-	-	-	yes	yes
Covariates[(e)]	no	no	no	no	no	no	no	yes

Notes: This table reports the first stage coefficients from estimations of Equation (2). [(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 at the predicted caseworker and job seeker levels. [(e)] "Covariates" refers to whether the estimation includes controls for predetermined job seeker characteristics (see Appendix L.7). *p<0.10 ** p<0.05 *** p<0.01.

Table C.4: First-Stage Estimates of On-The-Job Training Assignment

	D(On-the-job training)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Z(Classroom training)	0.00	0.00	−0.01	−0.04*	−0.04**	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Z(On-the-job training)	1.00***	1.00***	0.88***	0.49***	0.45***	0.21***	0.21***	0.21***
	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)
Obs	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222
F-stat (all Z's)	4055.8	2445.8	1918.7	132.1	108.0	19.2	10.1	10.7
Caseworker[(a)]	Realized	Predicted	Predicted	Predicted	Predicted	Predicted	Predicted	Predicted
Leave-out[(b)]	-	-	spell	spell	spell	spell	spell	spell
FE's[(c)]	-	-	-	jxu	jxu + y	jxuxy	jxuxy	jxuxy
Clustering[(d)]	-	-	-	-	-	-	yes	yes
Covariates[(e)]	no	no	no	no	no	no	no	yes

Notes: This table reports the first stage coefficients from estimations of Equation (2). [(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 at the predicted caseworker and job seeker levels. [(e)] "Covariates" refers to whether the estimation includes controls for predetermined job seeker characteristics (see Appendix L.7). *p<0.10 ** p<0.05 *** p<0.01.

C.2 Monotonicity

Table C.5: Testing for Monotonicity with the Baseline Instrument: Classroom Training

	D(Classroom Training)			
	q1	q2	q3	q4
Z(Classroom training)	0.23***	0.32***	0.49***	0.48***
	(0.05)	(0.07)	(0.07)	(0.05)
Z(On-the-job training)	0.05	0.00	0.11	−0.02
	(0.05)	(0.08)	(0.09)	(0.07)
Obs	41,800	41,801	41,805	41,794
Dep var Mean	0.21	0.34	0.44	0.58
Dep var sd	0.40	0.47	0.50	0.49
F-stat (instruments)	9.68	9.78	28.03	41.82
P-value (F-stat)	0.000	0.000	0.000	0.000

Notes: This table implements a monotonicity test for assignment to classroom training based on the baseline instruments. 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 (see Appendix L.7). 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 and controls for predetermined job seeker characteristics. Standard errors (in parentheses) are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

Table C.6: Testing for Monotonicity with the Baseline Instrument: On-the-Job Training

	D(On-the-job Training)			
	q1	q2	q3	q4
Z(Classroom training)	−0.04	0.04	0.05	−0.01
	(0.04)	(0.04)	(0.05)	(0.05)
Z(On-the-job training)	−0.01	0.11	0.18**	0.40***
	(0.07)	(0.08)	(0.07)	(0.07)
Obs	41,804	41,804	41,806	41,800
Dep var Mean	0.11	0.20	0.27	0.40
Dep var sd	0.32	0.40	0.45	0.49
F-stat (instruments)	0.53	1.90	3.29	16.34
P-value (F-stat)	0.587	0.151	0.038	0.000

Notes: This table implements a monotonicity test for assignment to on-the-job training based on the baseline instruments. The sample is partitioned into quartiles based on predicted assignment to on-the-job training, resulting from an OLS regression of assignment to on-the-job training on predetermined job seeker characteristics (see Appendix L.7). 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 and controls for predetermined job seeker characteristics. Standard errors (in parentheses) are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

Table C.7: Testing for Monotonicity with the Reverse-Sample Instrument: Classroom Training

	D(Classroom Training)			
	q1	q2	q3	q4
Reverse-sample Z(Classroom training)	0.15***	0.29***	0.39***	0.11***
	(0.04)	(0.06)	(0.07)	(0.03)
Reverse-sample Z(On-the-job training)	−0.01	−0.12	0.14*	−0.06
	(0.03)	(0.07)	(0.08)	(0.06)
Obs	41,489	41,801	41,805	39,945
Dep var Mean	0.21	0.34	0.44	0.57
Dep var sd	0.40	0.47	0.50	0.49
F-stat (instruments)	8.68	12.59	19.25	6.28
P-value (F-stat)	0.000	0.000	0.000	0.002

Notes: This table implements a monotonicity test for assignment to classroom training based on “reverse-sample” instruments. 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 (see Appendix L.7). For each quartile, a “reverse-sample” 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 ’reverse-sample’ instruments and including job-center-unit-year fixed effects and controls for predetermined job seeker characteristics. Standard errors (in parentheses) are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

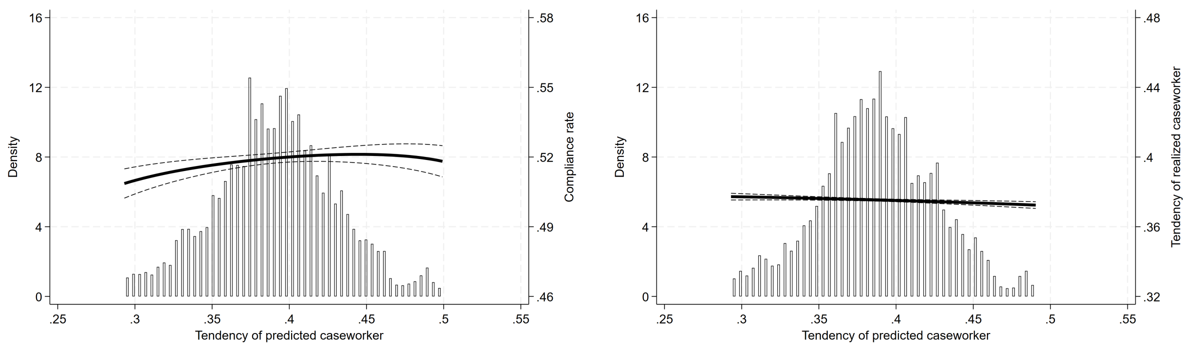
Table C.8: Testing for Monotonicity with the Reverse-Sample Instrument: On-the-Job Training

	D(On-the-job Training)			
	q1	q2	q3	q4
Reverse-sample Z(Classroom training)	–0.02	0.04	0.04	–0.02
	(0.03)	(0.04)	(0.05)	(0.04)
Reverse-sample Z(On-the-job training)	0.07	0.11**	0.27***	0.14
	(0.05)	(0.06)	(0.06)	(0.08)
Obs	41,804	41,804	41,806	41,800
Dep var Mean	0.11	0.20	0.27	0.40
Dep var sd	0.32	0.40	0.45	0.49
F-stat (instruments)	1.28	2.86	11.28	1.35
P-value (F-stat)	0.279	0.058	0.000	0.260

Notes: This table implements a monotonicity test for assignment to on-the-job training based on "reverse-sample" instruments. The sample is partitioned into quartiles based on predicted assignment to on-the-job training, resulting from an OLS regression of assignment to on-the-job training on job seeker predetermined characteristics (see Appendix L.7). For each quartile, a "reverse-sample" 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 reverse-sample instruments and including job-center-unit-year fixed effects and controls for predetermined job seeker characteristics. Standard errors (in parentheses) are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

C.2.1 Monotonic Compliance to Caseworker Allocation Rule

Figure C.2: Monotonic Compliance to Classroom Instrument



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

Notes: Panel (a) of this figure shows the correlation between the day-of-birth compliance rate and the classroom-training tendency of the predicted caseworker for all job seekers in the sample. Panel (b) shows the correlation between the classroom-training tendencies of the realized and predicted caseworker for job seekers that do not comply with the day-of-birth allocation rule.. All plotted values in the figure have been demeaned by job-center-unit-year fixed effects, predetermined job seeker characteristics (see Appendix L.7), and the on-the-job-training tendency for the predicted caseworker. In both panels, the bars represent the distribution of the classroom-training tendency for the *predicted* caseworker (excluding top and bottom 1%). The black line in panel (a) represents the coefficients from a local linear regression of the day-of-birth compliance rate on the classroom-training tendency of the *predicted* caseworker. The black line in panel (b) represents the coefficients from a local linear regression of the classroom-training tendency of the *realized* caseworker on the demeaned classroom-training tendency of the *predicted* caseworker. The local linear regressions are based on an Epanechnikov kernel with bandwidth 0.1. The results are robust to using the same specification as in Bhuller et al. (2020): top and bottom 2% excluded, triangular kernel, and bandwidth = 0.15. The day-of-birth compliance rate indicates whether the actual caseworker corresponds to the day-of-birth predicted caseworker. Dotted lines represent 95% confidence intervals.

C.3 Exclusion

Table C.9: Placebo Sample Restrictions

	UI-spells	jobseeker	jobcenters	Weeks of UI	Assignment to Classroom training	Assignment to On-the-job training
UI-spells (2012-2018) with no meeting	**1,091,415**	**583,315**	**94**	**5**	**6.5**	**2.9**
- identify job-center-unit-year	1,088,962	582,022	94	5	6.5	2.9
- predict caseworker	1,080,169	578,457	94	5	6.5	3.0
- keep only j-u-y's using day-of-birth	232,945	144,983	51	4	5.3	2.9
- No non-western immigrants	228,521	142,080	51	4	5.3	2.8
- Balanced sample (2012-2017)	211,351	134,001	51	4	5.1	2.9
- Caseworker stringency observed	205,054	130,097	51	4	5.1	2.8
- No other UI-spells with meeting	166,688	111,410	51	4	3.1	1.9
Placebo sample	**166,688**	**111,410**	**51**	**4**	**3.1**	**1.9**

Notes: This table shows the number observations lost after each restriction towards the placebo sample. First, we identify all job seekers who did not have a caseworker meeting during their UI-spell, and place these job seekers in a *job-center-unit-year* cell, based on the year they became unemployed and recordings of their municipality of residence, age, and education. Then, we use our imputed birthday protocols to *predict the caseworker* the job seeker should have been assigned to, given her job-center-unit-year and day-of-birth, and we restrict the sample to job-center-unit-years that are using *day-of-birth block structures* for assignments. Hereafter, we make a few restrictions to ease comparison with our baseline sample. In particular, we restrict the sample to job seekers i) of *western origin*, ii) for whom labor market outcomes are not affected by Covid-19 throughout the first two years (i.e. job seekers from *2012-2017*). and iii) for whom we observe their caseworkers' *training stringency* (i.e. job seekers assigned to caseworkers who are in our main sample). Finally, we restrict the sample to job seekers who did not have *another UI-spell* (in which they meet with a caseworker and potentially are assigned to training) starting in the 12 months prior to/following the current UI-spell.

Table C.10: ZFS Test - First Stage Robustness to Percentiles

	Pct 1		Pct 2		Pct 3		Pct 4		Pct 5	
	Classroom training (1)	On-the-job training (2)	Classroom training (3)	On-the-job training (4)	Classroom training (5)	On-the-job training (6)	Classroom training (7)	On-the-job training (8)	Classroom training (9)	On-the-job training (10)
Z(Classroom training)	0.11	0.19	0.01	0.04	0.07	0.03	0.04	0.04	0.12	0.06
	(0.20)	(0.14)	(0.13)	(0.10)	(0.11)	(0.08)	(0.10)	(0.07)	(0.09)	(0.07)
Z(On-the-job training)	0.28	0.08	−0.03	−0.13	0.02	−0.15	0.02	−0.23*	−0.09	−0.09
	(0.27)	(0.23)	(0.17)	(0.16)	(0.15)	(0.13)	(0.13)	(0.12)	(0.11)	(0.10)
Obs	1,648	1,648	3,320	3,320	5,012	5,012	6,684	6,684	8,361	8,361
Cov	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Pscore	0.18	0.18	0.22	0.22	0.24	0.24	0.26	0.26	0.28	0.28
Dep. var mean	0.10	0.05	0.11	0.05	0.13	0.07	0.14	0.07	0.15	0.08
F-stat (all Z's)	0.58	1.24	0.02	0.35	0.19	0.65	0.07	1.85	1.41	0.66

Notes: This table reports the first stage estimates of assignment to training programs for a subpopulation of job seekers whose training decisions are likely unaffected by caseworkers' training tendencies. These job seekers are identified by estimating a linear probability model for assignment to *any training* (classroom or on-the-job training) based on predetermined characteristics (see Appendix L.7), and sub-setting to job seekers with the lowest propensity score. Each column represents estimates for a given percentile of the propensity score distribution used as cut-off. All regressions include job-center-unit-year fixed effects and controls for predetermined characteristics. Standard errors are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

Table C.11: ZFS Test - Reduced Form Robustness to Percentiles

	Employed in Q7				
	Pct 1	Pct 2	Pct 3	Pct 4	Pct 5
	(1)	(2)	(3)	(4)	(5)
Z(Classroom training)	−0.24	−0.14	−0.03	−0.04	−0.01
	(0.19)	(0.14)	(0.11)	(0.10)	(0.09)
Z(On-the-job training)	−0.36	−0.24	−0.22	0.00	−0.02
	(0.29)	(0.23)	(0.17)	(0.15)	(0.13)
Obs	1,648	3,320	5,012	6,684	8,361
Cov	yes	yes	yes	yes	yes
Pscore	0.18	0.22	0.24	0.26	0.28
Dep. var mean	0.88	0.86	0.85	0.84	0.84

(a) Employment

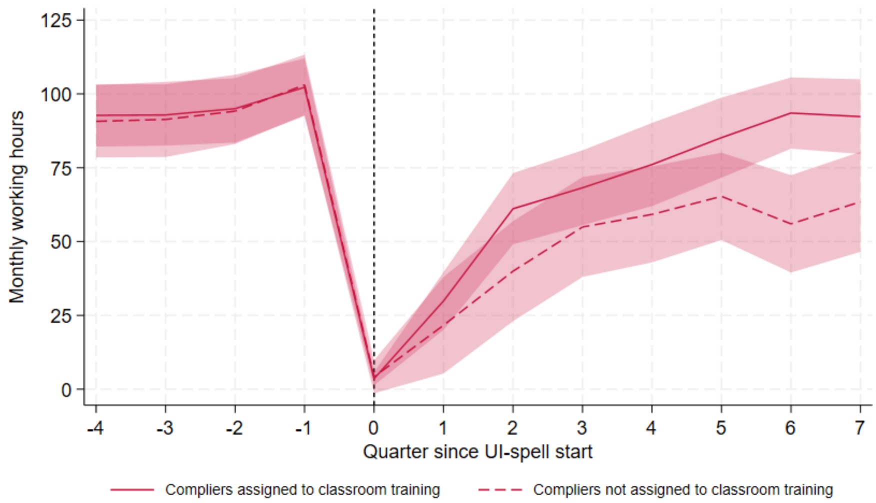
	Working hours in Q7				
	Pct 1	Pct 2	Pct 3	Pct 4	Pct 5
	(1)	(2)	(3)	(4)	(5)
Z(Classroom training)	−0.44	28.40	16.80	16.16	15.47
	(31.03)	(21.84)	(17.71)	(15.50)	(13.57)
Z(On-the-job training)	−87.23*	−46.99	−33.38	−6.47	−11.94
	(47.04)	(39.66)	(29.26)	(23.82)	(20.48)
Obs	1,648	3,320	5,012	6,684	8,361
Cov	yes	yes	yes	yes	yes
Pscore	0.18	0.22	0.24	0.26	0.28
Dep. var mean	116.97	113.34	111.34	111.02	110.15

(b) Working hours

Notes: This table reports the reduced form estimate on extensive margin employment and working hours in Q7 (panels a and b) for a subpopulation of job seekers whose training decisions are likely unaffected by caseworkers' training tendencies. These job seekers are identified by estimating a linear probability model for assignment to any training (classroom or on-the-job training) based on predetermined characteristics (see Appendix L.7), and subsetting to job seekers with the lowest propensity score. Each column represents estimates for a given percentile of the propensity score distribution used as cut-off. All regressions include job-center-unit-year fixed effects and controls for predetermined characteristics. Standard errors are two-way clustered at the predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

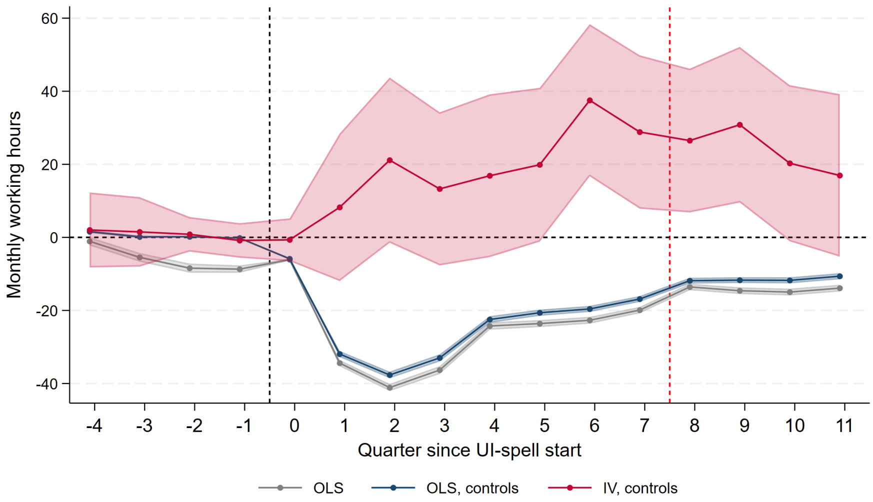
D Effects of Assignment to Classroom Training

Figure D.1: Hours of Employment by Complier Status



Notes: This figure plots average monthly hours of employment ("working hours") by training assignment status for individuals in the IV population (compliers). The outcome for compliers (not) assigned to classroom training is obtained by running a regression with the outcome interacted by an indicator for (non-) assignment to classroom training on the left-hand side, and indicators for (non) assignment to classroom training and assignment to on-the-job training on the right-hand side. The training indicators on the right-hand side are instrumented by the caseworker training tendencies. All regressions include job-center-unit-year fixed effects and controls for predetermined job seeker characteristics (see Appendix L.7). Colored bands represent 95% confidence bands.

Figure D.2: Hours of Employment Effect of Assignment to Classroom Training (Extended Period)



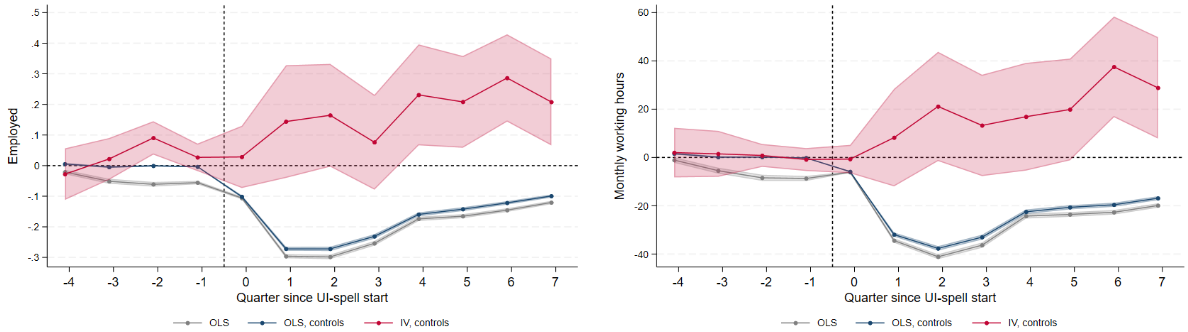
Notes: This figure shows the effect of assignment to classroom training on average monthly hours of employment ("working hours‘") in a given quarter relative to job loss. This figure is based on our main sample of job seekers who initiated a UI-spell from 2012-2017. To avoid Covid-19 effects, we use labor market outcomes up until the end of 2019. Hence, up until event quarter 7 the estimates are based on *all* job seekers in the sample (and is identical to Figure 3). After event quarter 7 (indicated by the red dashed line), estimates are no longer based on all job seekers from the main sample. The gray line represents the effect obtained with a simple OLS regression and OLS that controls for job seeker pre-determined characteristics (see Appendix L.7). The red line represents the effect obtained by IV estimation, including controls job seeker pre-determined characteristics. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered on predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals (see Table D.1 for standard errors).

Table D.1: Hours of Employment Effect of Assignment to Training Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Average monthly working hours in quarter relative to job loss															
	Q = -4	Q = -3	Q = -2	Q = -1	Q = 0	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7	Q = 8	Q = 9	Q = 10	Q = 11
Classroom Training	2.03	1.50	0.84	−0.84	−0.66	8.23	21.14*	13.28	16.88	19.88*	37.53***	28.85***	26.50***	30.83***	20.29*	16.96
	(5.24)	(4.85)	(2.42)	(2.42)	(2.99)	(10.30)	(11.53)	(10.70)	(11.37)	(10.74)	(10.63)	(10.71)	(10.04)	(10.86)	(10.91)	(11.35)
On-the-job training	17.04	25.52*	0.47	−0.47	−5.40	50.51*	32.15	−8.95	10.07	19.94	−12.63	−3.29	−17.44	−35.52	−13.17	−27.91
	(13.12)	(13.37)	(5.61)	(5.61)	(7.17)	(26.54)	(24.69)	(22.35)	(23.35)	(23.70)	(23.22)	(22.96)	(22.20)	(26.39)	(22.92)	(23.31)
Obs	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	164,451	159,594	154,061	150,311
Dep. var mean (pre-job loss)	101	101	101	101	101	101	101	101	101	101	101	101	101	101	101	101
P-value (CT vs. OTJ)	0.31	0.10	0.95	0.95	0.56	0.16	0.70	0.40	0.80	1.00	0.06	0.23	0.08	0.03	0.22	0.12

Note: This table shows IV estimates of the effect of assignment to classroom and on-the-job training programs on average monthly hours of employment ("working hours‘") in a given quarter relative to job loss. The estimates are based on our main sample of job seekers who initiated a UI-spell from 2012-2017. To avoid Covid-19 effects, we use labor market outcomes up until the end of 2019. Hence, up until event quarter 7 the estimates are based on *all* job seekers in the sample. After event quarter 7, estimates are no longer based on all job seekers from the main sample. All regressions include job-center-unit-year fixed effects and controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered on predicted caseworker and job seeker levels. The bottom of the table reports the p-values for the difference in coefficients on classroom training ("CT") and on-the-job training ("OTJ"). *p<0.10 ** p<0.05 *** p<0.01.

Figure D.3: Employment and Earnings Effect of Assignment to Classroom Training



(a) Extensive-Margin Employment

(b) Earnings (percent of pre-job loss level)

Notes: This figure plots the effect of assignment to classroom training on extensive margin employment (Panel (a)) and monthly earnings in percent of the pre-job loss level (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 (see Appendix L.7). The red line represents the effect obtained by IV-estimation, including controls for predetermined job seeker characteristics. All regressions include fully interacted job-center unit and year fixed effects. Standard errors are two-way clustered on predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals (see Table D.2 and D.3 for standard errors).

Table D.2: Extensive-Margin Employment Effect of Assignment to Training Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Extensive margin employment in quarter relative to job loss											
	Q = -4	Q = -3	Q = -2	Q = -1	Q = 0	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7
Classroom training	−0.03	0.02	0.09***	0.03	0.03	0.14	0.16*	0.08	0.23***	0.21***	0.29***	0.21***
	(0.04)	(0.03)	(0.03)	(0.02)	(0.05)	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)	(0.07)	(0.07)
On-the-job training	0.20*	0.11	−0.08	−0.01	−0.07	0.56**	0.23	0.12	−0.15	0.14	−0.04	−0.05
	(0.11)	(0.09)	(0.07)	(0.06)	(0.11)	(0.24)	(0.19)	(0.17)	(0.17)	(0.17)	(0.17)	(0.16)
Obs	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222
Dep. var mean (pre-job loss)	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8
P-value (CT vs. OTJ)	0.06	0.42	0.04	0.53	0.43	0.12	0.78	0.83	0.06	0.70	0.08	0.16

Notes: This table shows IV-estimates of the effect of assigning job seekers to classroom and on-the-job training within the first 12 months of unemployment on extensive margin employment. All regressions include fully interacted job-center-unit-year fixed effects, and controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. The bottom of the table reports the p-values for the difference in coefficients on classroom training ("CT") and on-the-job training ("OTJ"). *p<0.10 ** p<0.05 *** p<0.01.

Table D.3: Earnings Effect of Assignment to Training programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Monthly earnings (in percent of pre-job loss level) in quarter relative to job loss											
	Q = -4	Q = -3	Q = -2	Q = -1	Q = 0	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7
Classroom training	−3.73	1.51	−2.07	2.07	−0.37	8.45	20.83**	12.08	8.32	14.04	31.09***	28.70***
	(5.62)	(7.30)	(3.20)	(3.20)	(2.75)	(9.22)	(10.04)	(9.30)	(9.65)	(9.28)	(9.89)	(10.11)
On-the-job training	16.55	21.04	−1.09	1.09	−5.51	35.53	14.96	−18.34	0.20	7.88	−26.64	−5.47
	(12.76)	(15.47)	(7.38)	(7.38)	(6.16)	(22.54)	(20.84)	(20.82)	(20.54)	(21.17)	(22.70)	(22.77)
Obs	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222
P-value (CT vs. OTJ)	0.17	0.21	0.91	0.91	0.46	0.29	0.81	0.21	0.73	0.80	0.03	0.19

Notes: This table shows IV-estimates of the effect of assigning job seekers to classroom and on-the-job training within the first 12 months of unemployment on average monthly earnings (in percent of pre-job loss levels) in a given quarter relative to job loss. All regressions include fully interacted job-center-unit-year fixed effects, and controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. The bottom of the table reports the p-values for the difference in coefficients on classroom training ("CT") and on-the-job training ("OTJ"). *p<0.10 ** p<0.05 *** p<0.01.

Table D.4: Reduced-Form Effects of Training Programs on Extensive-Margin Employment

	Employment in a given quarter Q relative to job loss						
	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Z(Classroom training)	0.06**	0.06**	0.03	0.09***	0.08***	0.11***	0.08***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Z(On-the-job training)	0.13***	0.06	0.03	−0.02	0.04	0.01	0.00
	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Obs	167,222	167,222	167,222	167,222	167,222	167,222	167,222
Dep. var mean (pre-job loss)	0.8	0.8	0.8	0.8	0.8	0.8	0.8
Cov	yes	yes	yes	yes	yes	yes	yes

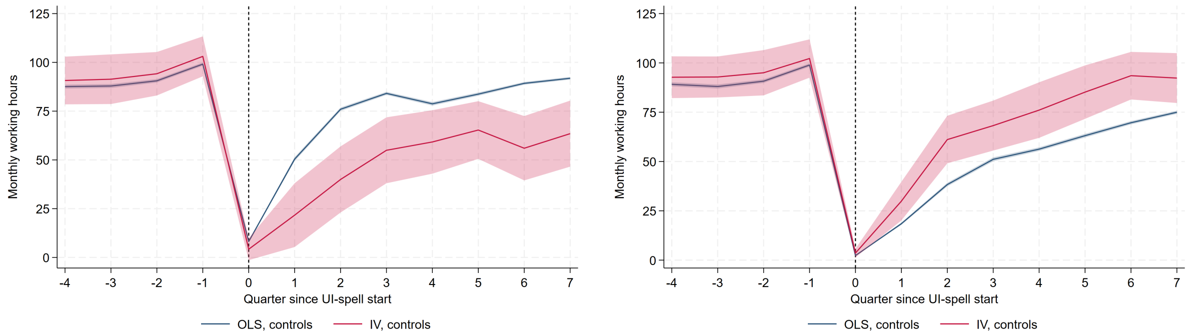
Note: This table shows reduced-form estimates of the effect of assignment to training programs on extensive-margin employment in a given quarter relative to job loss. All regressions include job-center-unit-year fixed effects and controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered on predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

Table D.5: Reduced-Form Effects of Training Programs on Hourly Wages (Conditional on Employment)

	Hourly wages in a given quarter Q relative to job loss						
	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Z(Classroom training)	11.94	24.55**	−4.17	−7.03	2.54	−5.84	−5.27
	(15.75)	(11.56)	(12.33)	(8.20)	(9.23)	(8.31)	(8.51)
Z(On-the-job training)	−2.34	−14.98	−6.29	26.18*	−6.36	−7.69	−0.10
	(19.44)	(14.19)	(14.66)	(14.57)	(11.59)	(10.72)	(10.66)
Obs	67,171	91,234	101,196	100,070	106,911	112,766	115,576
Dep. var mean (pre-job loss)	208.0	208.0	208.0	208.0	208.0	208.0	208.0
Cov	yes	yes	yes	yes	yes	yes	yes

Note: This table shows reduced form estimates of the effect of assignment to training programs on hourly wages (conditional on employment) in a given quarter relative to job loss. All regressions include job-center-unit-year fixed effects and controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered on predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

Figure D.4: Hours of Employment by Assignment Status and Population



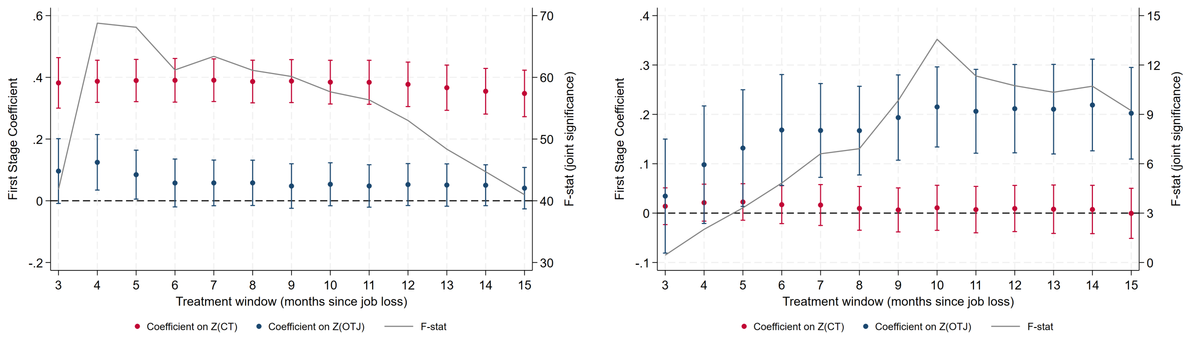
(a) Not assigned to training (b) Assigned to training

Notes: This figure plots average monthly hours of employment ("working hours") by training assignment status for individuals in the OLS (blue) and IV (red) population. Panel (a) plots the outcomes for job seekers *not* assigned to classroom training, and Panel (b) plots the outcomes for job seekers *assigned* to classroom training. The outcome for the OLS-population of job seekers (not) assigned to classroom training is obtained by running a regression with the outcome interacted by an indicator for (non-) assignment to classroom training on the left-hand side, and indicators for (non) assignment to classroom training and assignment to on-the-job training on the right-hand side. The outcome for the IV-population of job seekers is obtained by running the same regressions while instrumenting (non) assignments by the caseworker training tendencies. All regressions include job-center-unit-year fixed effects and controls for predetermined job seeker characteristics (see Appendix L.7). Shaded areas represent 95% confidence bands.

D.1 Robustness Analyses

D.1.1 Window for Treatment

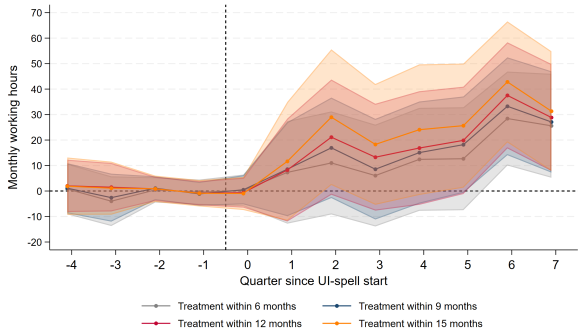
Figure D.5: First Stage Estimates by Treatment Windows



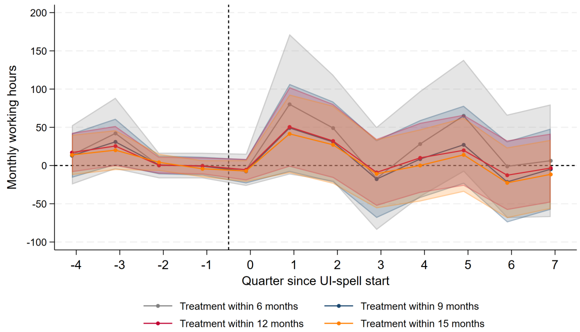
(a) Classroom Training (b) On-the-job Training

Note: This figure plots first stage estimates for assignment to classroom training (panel a) and on-the-job training (panel b), at various treatment windows (number of months since unemployment start). All regressions include fully interacted job-center-unit-year fixed effects, as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. Colored lines represents 95% confidence intervals.

Figure D.6: Hours of Employment Effects of Assignment to Classroom Training by Treatment Window



(a) Classroom Training



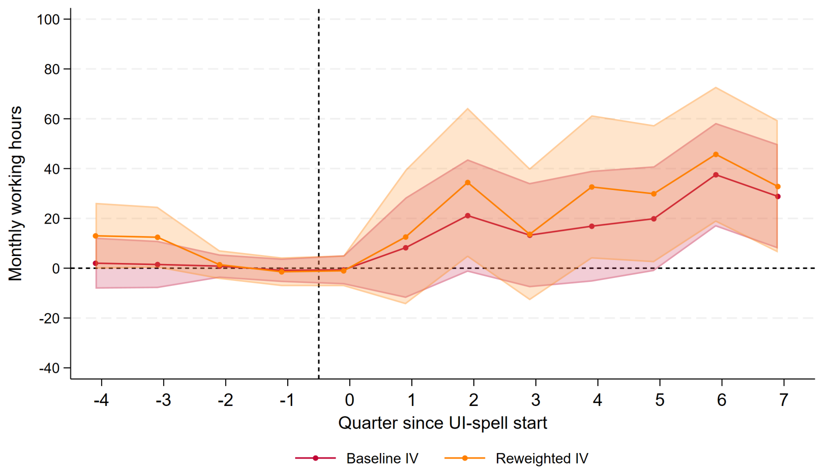
(b) On-the-job Training

Note: This figure shows IV-estimates of the effect of assigning job seekers to classroom training (panel a) and on-the-job training (panel b) within a given number of months since job loss on average monthly hours of employment ("working hours") in a given quarter relative to job loss. All regressions include fully interacted job-center-unit-year fixed effects, as well as controls for predetermined job seeker characteristics. Standard errors are clustered on predicted caseworker and job seeker level. Colored bands represent 95% confidence intervals.

D.1.2 Job Center Re-Weighted IV

Figure D.7 re-weighs our sample of UI spells such that job-center-units are given weight according to their sizes (regardless of how many years they are part of the sample). In particular, we weight UI spell i from job-center-unit j by the inverse of the number of years t, j is in the sample, $w_{i(j)} = \frac{1}{t}$. Note that the weights are rescaled; $w_{i(j)}$ is divided by the sum of weights in the sample of UI-spells, $\sum_i w_{i(j)}$, such that they sum to one.

Figure D.7: Effect of Assignment to Classroom Training



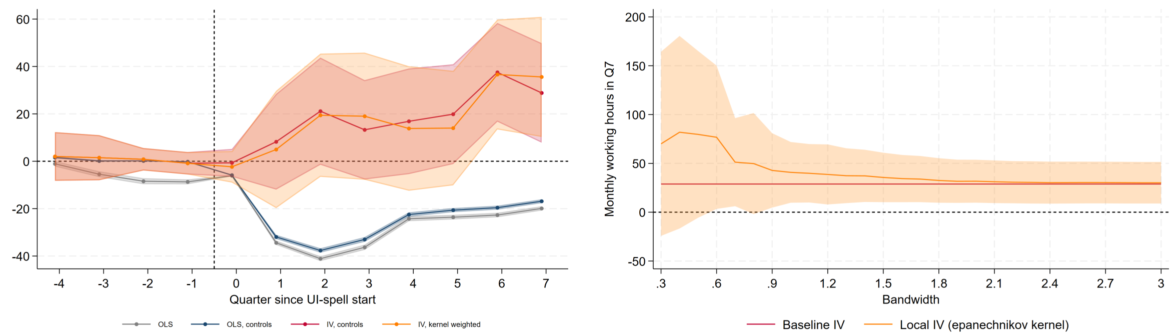
Notes: This figure shows the effect of assignment to classroom training on average monthly hours of employment ("working hours") in a given quarter relative to job loss. The red line represents our baseline IV estimate. The orange line represents the IV-estimate when observations (UI-spells, i) are weighted by the inverse of the number of years t that their job-center-unit is a part of the sample, $w_{i(j)} = \frac{1}{t}$. Note that the weights are rescaled; $w_{i(j)}$ is divided by the sum of weights in the sample of UI-spells, $\sum_i w_{i(j)}$, such that they sum to one. All regressions include job-center-unit-year fixed effects as well as controls for job seeker predetermined characteristics (see Appendix L.7). Standard errors are two-way clustered on predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals.

D.1.3 Local IV

Our baseline TSLS specification controls linearly for the assignment to on-the-job training. Yet, our theory in Section B.3.1 and Blandhol et al. (2022) highlight the role of *flexible* controls for interpreting TSLS estimates as LATEs. In this section, we show our results are robust to estimating our TSLS specification around an evaluation point z_2' for the on-the-job-training instrument following (Mountjoy, 2022).

In particular, we estimate our TSLS specification using an Epanechnikov kernel to give positive weight to all observations around the mean of the on-the-job training instrument (within bandwidth 1.5 SDs). Figure D.8a plots this local IV estimate along with our baseline IV estimate. The figure shows our results are robust to a local estimation of Equations (2)-(3). Figure D.8b suggests this result holds across a wide range of kernel bandwidths.

Figure D.8: Local IV Estimate of the Effect of Classroom Training on Hours of Employment



(a) Local IV

(b) Effect in Q7 against bandwidth choice

Notes: This figure plots the effect of assignment to classroom training on average monthly hours of employment ("monthly working hours") in a given quarter (panel a) and in quarter seven (panel b) relative to job loss. In Panel (a), the orange line represents the local IV estimate obtained by using an Epanechnikov kernel (with bandwidth 1.5) to give positive weight to all observations around the mean of the on-the-job-training instrument. The gray and blue lines represent the estimates obtained by simple OLS regression and OLS including controls for predetermined job seeker characteristics (see Appendix L.7), and the red line represents the baseline IV estimate (also including controls). In Panel (b), the orange line shows the local IV estimate obtained for a given choice of bandwidth on the Epanechnikov kernel. The red line again represents the baseline IV estimate. All regressions include job-center-unit-year fixed effects. Standard errors are two-way clustered at the predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals.

D.1.4 Separate Treatment Margins

Given extended monotonicity, our classroom tendency instrument shifts compliers into classroom training from two margins: from passive UI, $(0, 0) \rightarrow (1, 0)$, and from on-the-job training only, $(0, 1) \rightarrow (1, 1)$.

Following Section B.3.1, we can estimate the share of compliers (and their LATEs) along each of the treatment margins by regressing the treatment status indicators (and their interactions with outcomes) on the training tendency instruments:

$$T \in \{D_1(1-D_2), (1-D_1)(1-D_2), D_1D_2, (1-D_1)D_2\}$$

$$T_i = \beta^T_{q(i)} + \beta^T_1 Z_{1i} + \beta^T_2 Z_{2i} + \beta^T_3 X_i \tag{56}$$

$$Y_iT_i = \beta^{YT}_{q(i)} + \beta^{YT}_1 Z_{1i} + \beta^{YT}_2 Z_{2i} + \beta^{YT}_3 X_i, \tag{57}$$

where $\beta_{q(i)}$ denote job center-unit-year fixed effects, and X_i denotes covariates.

Along the passive UI margin, for example, the share of compliers and their LATE are

$$\mathbb{P}\left[(0,0) \rightarrow (1,0)\right] = \hat{\beta}_1^{D_1(1-D_2)} \tag{58}$$

$$\mathbb{E}\left[Y(1,0) - Y(0,0) | (0,0) \rightarrow (1,0)\right] = \frac{\hat{\beta}_1^{YD_1(1-D_2)}}{\hat{\beta}_1^{D_1(1-D_2)}} - \frac{\hat{\beta}_1^{Y(1-D_1)(1-D_2)}}{\hat{\beta}_1^{(1-D_1)(1-D_2)}}. \tag{59}$$

Table D.6 decomposes the baseline first stage (Column (1)) by the treatment margins (Columns (2) and (3)). The decomposition delivers two insights. First, the majority of compliers with the classroom-training instrument come from the passive margin (6%) rather than the on-the-job training margin (2%). Second, the instruments have twice the statistical power (F-stat) at the passive margin relative to the on-the-job training margin.

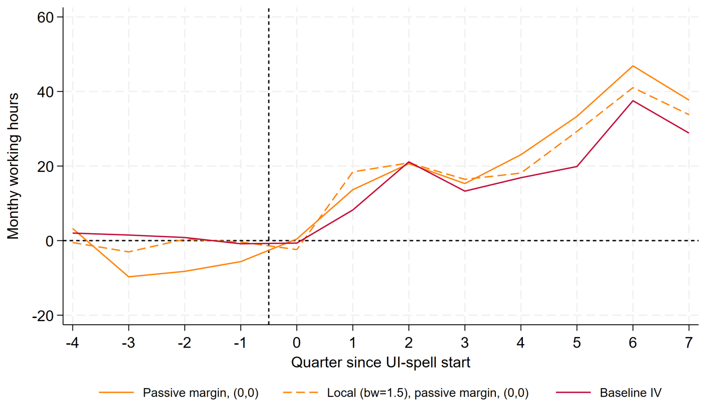
Table D.6: First-Stage Estimates by Margin

	Classroom Training by Margin		
	(1) Any Margin $(0,x) \rightarrow (1,x)$	(2) Passive Margin $(0,0) \rightarrow (1,0)$	(3) On-The-Job-Training Margin $(0,1) \rightarrow (1,1)$
Z(Classroom training)	0.38***	0.27***	0.11***
	(0.04)	(0.03)	(0.02)
Z(On-the-job training)	0.05	–0.02	0.07***
	(0.03)	(0.04)	(0.03)
Obs	167,222	167,222	167,222
Obs by cell	65,084	45,341	19,743
Cov	yes	yes	yes
F-stat	53.01	41.06	16.44
Compliers	0.08	0.06	0.02

Notes: First-stage estimates for classroom training by on-the-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 C.2). Columns (2) and (3) use assignment to classroom training interacted with indicators for non-assignment and assignment to on-the-job training as the dependent variable. All regressions include job-center-unit-year fixed effects as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered at the predicted caseworker and job seeker levels. The complier share at 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 D.9 investigates whether our findings for the employment effects of classroom training are robust to focusing on the main treatment margin from passive UI, $(0, 0) \rightarrow (1, 0)$. The red line represents our baseline IV estimate, which captures the average employment effect across margins. The solid orange line shows the LATE along the passive UI margin, and the dashed orange line represents a local estimation around the mean value of the on-the-job training instrument. The figure shows that our conclusions about the positive hours of employment effects of classroom training are robust to focusing on the compliers shifted from the passive margin into classroom training.

Figure D.9: Employment Effect by Treatment Margin



Notes: This figure shows the effect of assignment to classroom training on average monthly hours of employment ("working hours") in a given quarter relative to job loss, by treatment margins. 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, $(0, 0) \rightarrow (1, 0)$. To facilitate comparison with the baseline estimate, these estimates are obtained while controlling linearly for the on-the-job-training instrument. The dashed orange line represents a *local* version of the solid orange line: the estimates are obtained using an Epanechnikov kernel (with bandwidth 1.5) to give positive weight to all observations around the mean of the on-the-job-training instrument. All regressions include job-center-unit-year fixed effects as well as controls for predetermined job seeker characteristics (see Appendix L.7). This figure omits indications of statistical significance.

D.1.5 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.[62,63] Because

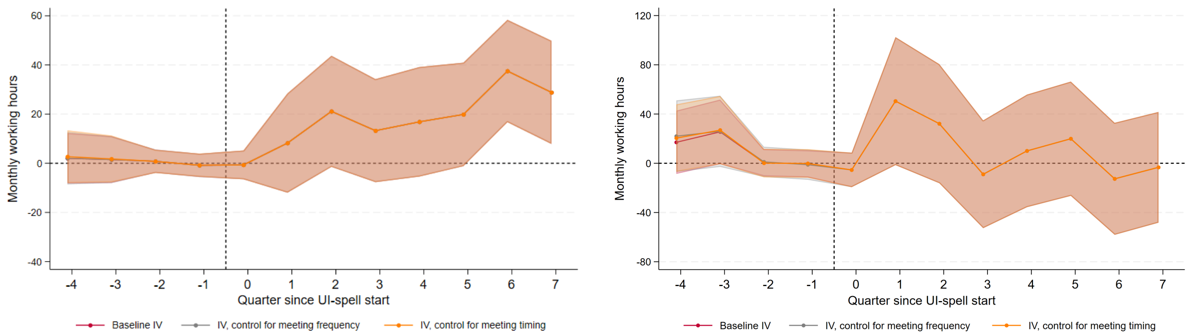
[62]We define meeting frequency for job seeker i as the number of caseworker meetings per week of unemployment. If the job seeker's UI spell is longer than 26 weeks, we only consider the first 26 weeks because meeting-frequency requirements change after 26 weeks. This measure includes all caseworker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

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

the meeting frequency (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.

Figure D.10 presents the baseline IV estimate of the effect of assignment to training along with the IV estimate obtained while controlling for meeting frequency and timing. Evidently, our IV estimates for classroom and on-the-job training are robust to the inclusion of these controls. We take this finding as evidence in support of the exclusion restriction.

Figure D.10: Effects of Classroom Training, while controlling for Meeting Frequency & Timing



(a) Classroom Training

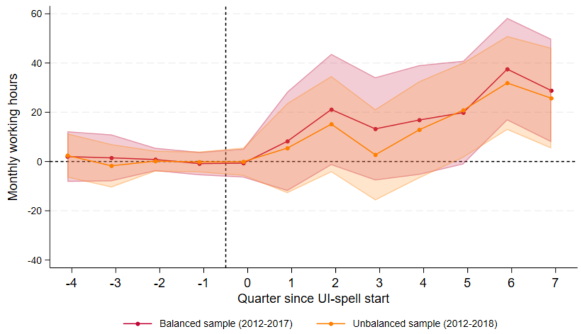
(b) On-The-Job Training

Notes: This figure shows the IV-estimate of the effect of assignment to classroom training (panel a) and on-the-job training (panel b) on average monthly hours of employment ("working hours") in a given quarter relative to job loss. The red line represents the baseline IV estimate. The orange and gray 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 as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered at the predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals.

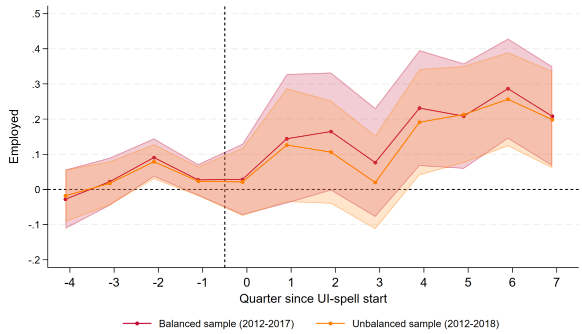
D.1.6 Balanced vs. Unbalanced Sample

Figure D.11 shows our results are robust to extending our current *balanced* sample of job seekers, who became unemployed from 2012 to 2017, with an additional year. Adding job seekers who became unemployed in 2018 implies that the sample becomes unbalanced; we do not observe outcomes in the second year after job loss for job seekers from 2018. That said, the figure shows that our results are robust to extending the sample to job seekers who also became unemployed in 2018.

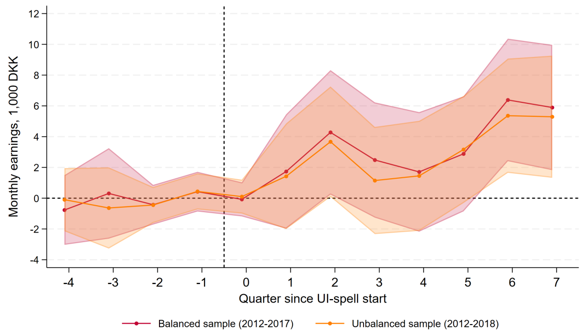
Figure D.11: Effect of Classroom Training Based on a Balanced vs. Unbalanced Sample



(a) Hours of Employment



(b) Extensive-Margin Employment

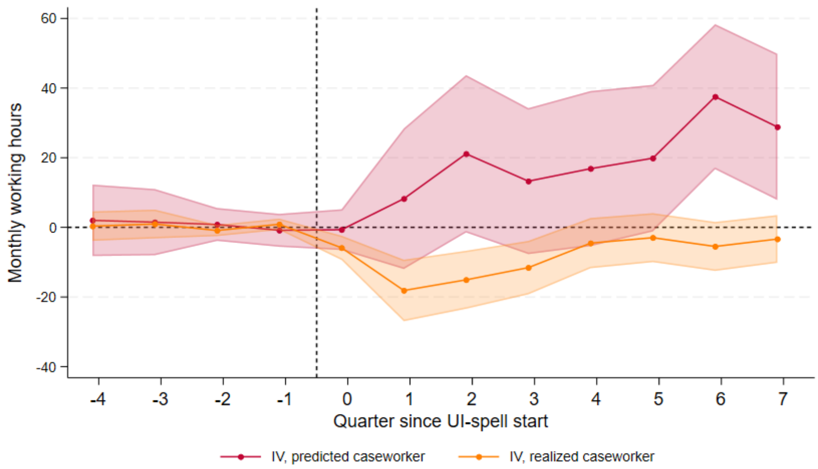


(c) Earnings

Notes: This figure plots the IV estimates of the effect of assignment to classroom-training programs on average monthly hours of employment ("working hours"), extensive margin employment, and monthly earnings (in 1,000 DKK), in a given quarter relative to job loss. The red line represents our baseline IV estimates. These are based on a *balanced* sample of job seekers for whom we observe labor market outcomes over the full first two years after job loss, i.e., job seekers who became unemployed from 2012 to 2017. The orange line represents estimates based on the full, *unbalanced* sample of job seekers who became unemployed from 2012 to 2018. We do not observe the labor market outcomes in the full first two years after job loss for job seekers who became unemployed in 2018. All regressions include job-center-unit-year fixed effects and controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered at the predicted caseworker and job seeker levels. Colored bands present 95% confidence intervals.

D.2 Realized Caseworker Instrument

Figure D.12: Effect of Assignment to Classroom Training: Realized Caseworker Instrument



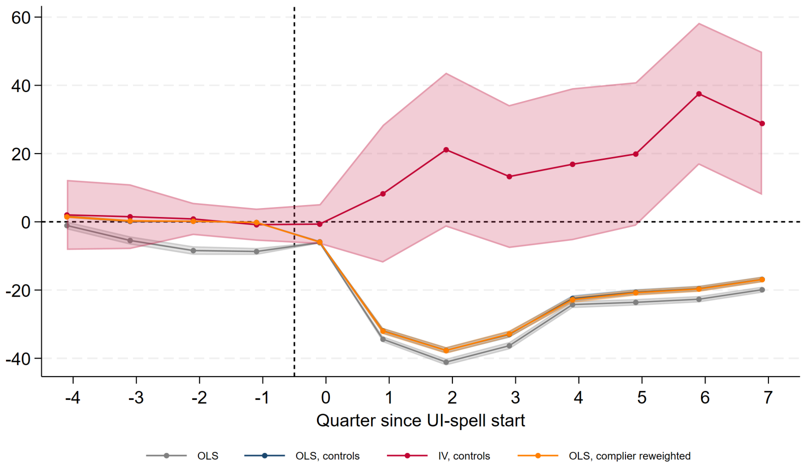
Notes: This figure shows the IV estimate of the effect of assignment to classroom training on average monthly hours of employment ("working hours") in a given quarter relative to job loss. The red line represents the baseline IV estimate obtained when instrumenting training assignment by the training tendencies of the job seeker's *predicted* caseworker. The orange line line represents the IV estimate obtained when instrumenting training assignment by the training tendencies of the job seeker's *realized* caseworker. All regressions include job-center-unit-year fixed effects and controls for job seeker predetermined characteristics (see Appendix L.7). Standard errors are two-way clustered on job seeker and caseworker levels (predicted caseworker for the red line; realized caseworker for the orange line). Colored bands represent 95% confidence intervals.

E Differences between OLS and IV

E.1 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 (ii) predicted probability of assignment to classroom training based on her predetermined characteristics (see Appendix L.7). Second, we compute the share of compliers in each subgroup and 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.1 shows the complier re-weighted OLS is highly similar to the standard OLS, suggesting effect heterogeneity based on observables is *not* driving the difference between IV and OLS.

Figure E.1: Complier-Characteristic Reweighted OLS



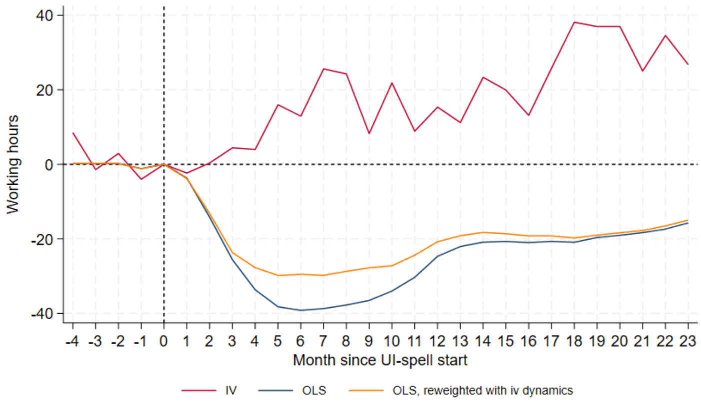
Notes: This figure shows the effect of assignment to classroom training on average monthly hours of employment ("working hours") in a given quarter relative to job loss. The gray and blue lines represent the effect obtained with a simple OLS regression and OLS that controls for job seeker predetermined characteristics (see Appendix L.7). The red line represents the effect obtained by IV-estimation, including controls for predetermined job seeker characteristics. 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 at the predicted caseworker and job seeker levels. Colored bands represent 95% confidence intervals.

E.2 OLS Reweighted by IV Training Dynamics

Figure E.2 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.[64] 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.2. 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 training dynamics are similar.

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

Figure E.2: OLS Reweighted by IV Dynamics



Notes: This figure plots the effect of assignment to classroom training on average monthly hours of employment ("working hours") in a given quarter relative to job loss. The effects are computed based on our OLS and IV estimates for γ^s_{1t}, β^{1s}_{1t}, 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^s_{1t} and OLS estimates for β^{1s}_{1t} and β_{0t}. This figure omits indications of statistical significance.

E.3 Matching Estimator

We construct a matched sample of job seekers by implementing the following procedure for each treatment type (classroom and on-the-job training):

1. Estimate a propensity score for assignment to classroom (on-the-job) training, based on a probit model and all covariates in our "OLS, controls" specification.

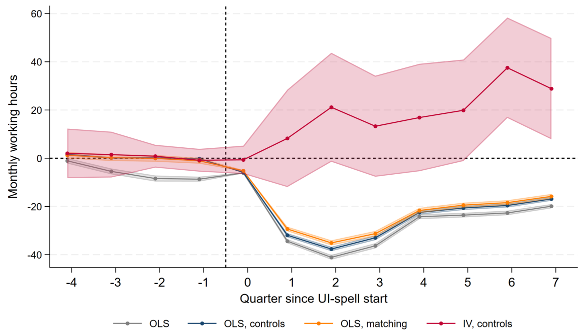
2. Use one-to-one nearest-neighbor matching (with replacement, in common support of propensity scores) to match all classroom (on-the-job) treated to a control job seeker.

3. Subset to all classroom (on-the-job) treated and matched control job seekers. If a job seeker is a matched control for multiple treated, we duplicate the observation such that the job seeker appears as many times as she is a matched control. I.e. in the treatment-specific subpopulation, there are N classroom (on-the-job) treated job seekers and N matched control job seekers.

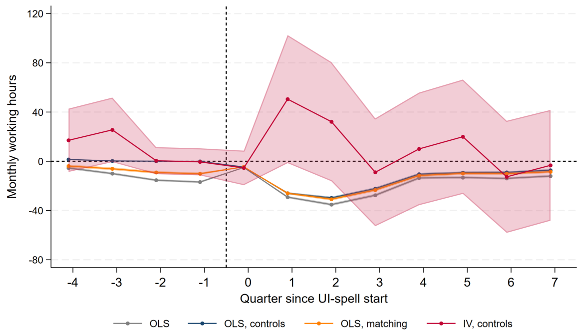
4. Append both treatment-specific sub-populations and estimate our simple OLS specification based on this "matched sample of job seekers".

Figure E.3 shows the matching estimator deliver similar results as our "OLS, controls" specification.

Figure E.3: Hours of Employment Effects of Training: Matching Estimator



(a) Classroom Training



(b) On-the-job Training

Note: The figure shows the effect of assigning job seekers to classroom training (panel a) or on-the-job training (panel b) on their monthly working hours. All regressions include fully interacted job-center-unit-year fixed effects. "OLS" includes no further controls. "OLS, controls" and "IV, controls" includes predetermined job seeker characteristics (see Appendix L.7). "OLS, matching" represents OLS estimates for a matched sample of job seekers based on predetermined job seeker characteristics. In particular, job seekers assigned to classroom or on-the-job training are matched to job seekers who are observationally similar. The procedure is based on 1:1 nearest neighbor matching on either a propensity score for assignment to classroom or on-the-job training, and propensity scores arise from a probit model with similar controls as in in the "OLS, controls" specification. Standard errors are clustered on predicted caseworker and job seeker level. Colored bands represents 95% confidence intervals.

E.4 Dynamic Treatment Assignments

We follow Biewen et al. (2014) and implement the following *dynamic treatment assignment* (DTA) estimator:

1. Define four treatment strata: months 0-2 (q0), months 3-4 (q1), months 6-8 (q2) and months 9-11 (q3) relative to the start of the unemployment spell.

2. For each strata, identify the population of job seekers who either i) started their first classroom and/or on-the-job training during the strata ("treatment", hereafter) or ii) were unemployed at the beginning of the strata. Based on this strata-population, we do the following for each treatment type (classroom and on-the-job training):

   (a) Estimate a propensity score for assignment to classroom (on-the-job) training, based on a probit model and predetermined job seeker characteristics (see Appendix L.7).

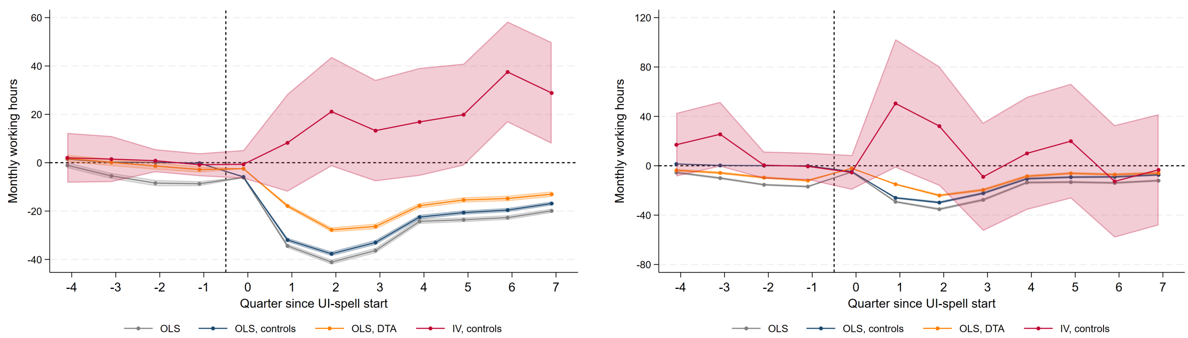
   (b) Use one-to-one nearest-neighbor matching (with replacement, in the common support of propensity scores) to match all classroom (on-the-job) treated to a control job seeker. Note that the control job seekers may be unemployed throughout the entire strata (and they may potentially be assigned to the other treatment in the strata), but they may also exit unemployment during the strata.

(c) Subset to all classroom (on-the-job) treated and matched control job seekers in the strata. If a job seeker is a matched control for multiple treated, we duplicate the observation such that the job seeker appears as many times as she is a matched control. I.e. in the strata-treatment specific subpopulation, there are N classroom (on-the-job) treated job seekers and N matched control job seekers.

3. Append all strata-treatment specific sub-populations, and estimate our simple OLS specification based on this "matched sample".

Figure E.4 plots the OLS, IV and DTA effects. The figure shows that the DTA estimator helps remove 8% of the gap between the OLS and IV-estimate in quarter seven relative to job loss. However, the DTA estimator still estimates substantial negative effects of classroom training (-13.2 working hours per month in quarter seven).

Figure E.4: Hours of Employment Effects of Training: Dynamic Treatment Assignment



(a) Classroom Training

(b) On-the-job Training

Note: The figure shows the effect of assigning job seekers to classroom training (panel a) or on-the-job training (panel b) on their average monthly hours of employment ("working hours"). All regressions include fully interacted job-center-unit-year fixed effects. "OLS" includes no further controls. "OLS, controls" and "IV, controls" includes predetermined jobseeker characteristics (see Appendix L.7). "OLS, DTA" represents OLS estimates for a matched sample of job seekers bed on dynamic treatment assignments. In particular, job seekers assigned to classroom or on-the-job training are matched to job seekers who were still unemployed in the beginning of the quarter in which the training program starts. The procedure is based on 1:1 nearest neighbor matching on either a propensity score for assignment to classroom or on-the-job training, and propensity scores arise from a probit model based on predetermined job seeker characteristics. Standard errors are clustered on predicted caseworker and job seeker level. Colored bands represents 95% confidence intervals.

E.5 LASSO

We follow Knaus et al. (2022) and implement a LASSO estimator for identifying the relevant control variables and interaction terms under conditional independence.

First, we use the LASSO criterion to select the relevant control variables. Table E.1 compares the effects of classroom and on-the-job training on average monthly working hours in quarter seven relative to job loss, obtained with OLS and LASSO estimation of a linear regression model. The model includes 75 covariates (see Appendix L.7) as well as job-center-unit-year fixed effects (189 levels), i.e. 264 controls in total. We allow LASSO to select among the covariates but require that the fixed effects and treatment dummies are always selected. The table shows that we obtain very similar effects with OLS and LASSO, reflecting that most covariates are selected by LASSO.

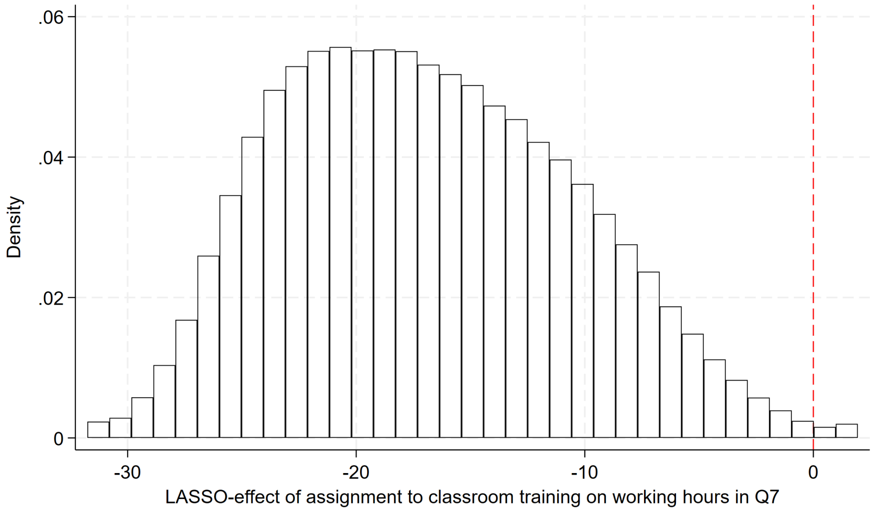
Table E.1: OLS and LASSO Estimates

	Working hours in Q7	
	(1)	(2)
	OLS	LASSO
Classroom training	−16.86***	−16.97***
	(0.35)	(0.35)
On-the-job training	−7.32***	−7.43***
	(0.40)	(0.40)
Obs	167,222	167,222
Selected Controls	264	244

Notes: OLS and LASSO estimates of a linear regression model for average monthly hours of employment ("working hours") in quarter seven relative to job loss ("Q7"). The model includes two treatment dummies (for assignment to classroom and on-the-job training), (75) covariates as well as job-center-unit-year fixed effects (189 levels), i.e. 264 controls in total. LASSO is allowed to select among the covariates, yet it is required to always select fixed effects and treatment dummies. The table reports robust standard errors. *p<0.10 ** p<0.05 *** p<0.01.

Second, we now allow for interaction effects with our covariates and use the LASSO criterion to select the relevant dimensions of effect heterogeneity. In particular, we include interactions between treatment dummies (assignment to classroom and on-the-job training) and covariates in the linear regression model and use LASSO to estimate this extended model. Thereafter, we predict the outcome (hours of employment in quarter seven relative to job loss) in two scenarios: i) when job seekers are assigned to classroom training, $\hat{Y}_{D=1}$, and ii) when they are not assigned, $\hat{Y}_{D=0}$. Figure E.5 plots the distribution of predicted differences, $\hat{Y}_{D=1} - \hat{Y}_{D=0}$, across U-spells in our sample. The figure shows that LASSO estimates that 99.65% of UI-spells have negative effects of assignment to classroom training (with an average effect of -16.87, similar to our OLS estimate in column (1) of Table E.1).

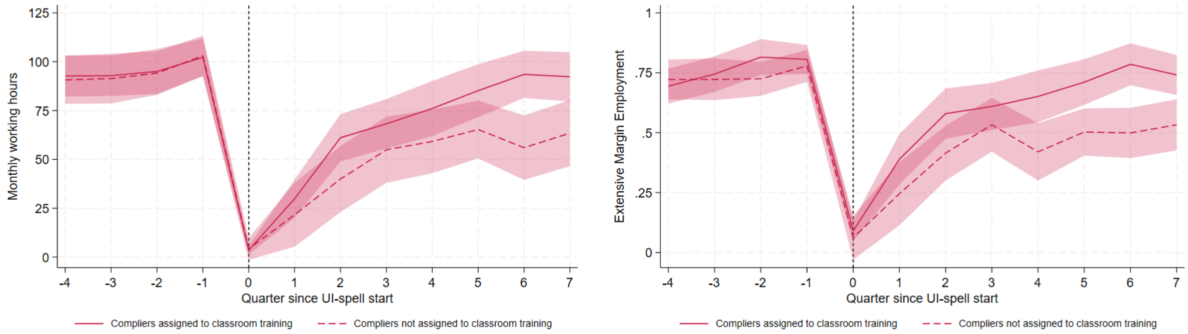
Figure E.5: LASSO-Estimates of the Hours of Employment Effects of Classroom Training



Note: The figure shows the distribution of LASSO-estimated effects of assigning job seekers to classroom training on their monthly hours of employment ("working hours") in quarter seven relative to job loss. The estimates are obtained by LASSO-estimation of a linear regression model including treatment dummies (for assignment to classroom and on-the-job training), 75 covariates, interactions between treatment dummies and covariates, and job-center-unit-year fixed effects (189 levels). LASSO is allowed to select among the covariates and interaction terms, yet it is required to always select fixed effects and treatment dummies. Based on (penalized) LASSO estimates, hours of employment in quarter seven is predicted for all job seekers in two scenarios: when assigned to classroom training and when not assigned to classroom training. The figure plots the distribution of differences (after censoring at percentile 0.1 and 99.1).

F Potential Outcomes

Figure F.1: Potential Outcomes of Compliers

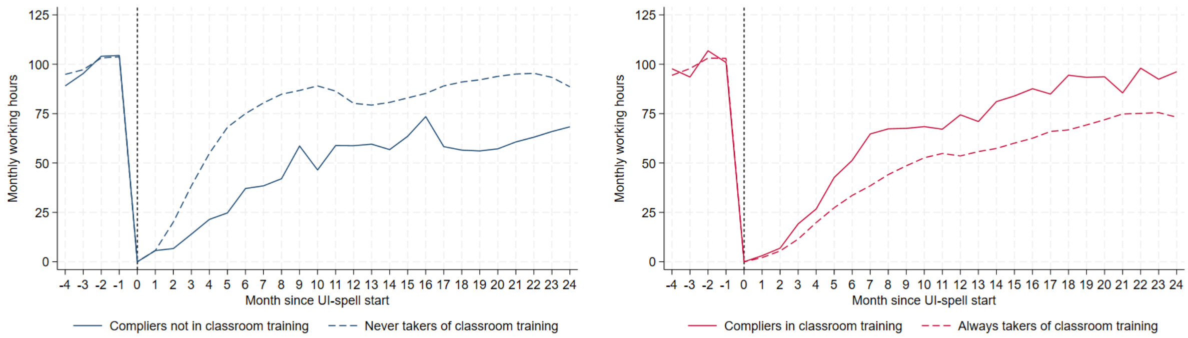


(a) Monthly working hours

(b) Extensive margin employment

Note: This figure decomposes LATE of classroom training on average monthly hours of employment ("working hours", panel a) and extensive-margin employment (panel b) into the potential outcomes of compliers with and without assignment to classroom training. All regressions include fully interacted job-center-unit-year fixed effects, as well as controls for predetermined job seeker characteristics (see Appendix L.7). Shaded areas represent 95% confidence bands.

Figure F.2: Potential Outcomes of Never and Always-Takers



(a) Never-takers and compliers not in training

(b) Always-takers and compliers in training

Note: This figure shows average monthly hours of employment ("working hours") in a given quarter relative to job loss for never-takers and compliers not in classroom training (panel a) as well as always-takers and compliers in classroom training (panel b). All regressions include fully interacted job-center-unit-year fixed effects, as well as controls for predetermined job seeker characteristics (see Appendix L.7). All estimates rely on using percentiles 1 and 99 of the classroom-training instrument as proxies for training-restrained and training-inclined caseworkers. See further details in Online Appendix J.2. This figure omits indications of statistical significance.

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

G.1 Training States

We split job seekers assigned to classroom training (assignees) into five mutually exclusive training states. First, we identify the following dates (months) 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, we define five mutually exclusive states, $s \in \{a, b, c, d1, d2\}$, for training assignees in a given month t:

(a) Yet to be assigned (“placebo”):
$(t < t_1)$ or $(t \geq t_e \text{ \& } t_e < t_1)$

(b) Yet to start training (“threat”):
$(t_1 \leq t < t_2)$ or $(t \geq t_e \text{ \& } t_1 \leq t_e < t_2)$

(c) Undergoing training (“lock-in”):
$(t_2 \leq t \leq t_3)$

(d) Done with training:

- (d1) Drop out of training (“drop out”):
$(t \geq t_e \text{ \& } t_2 \leq t_e \leq t_3)$

- (d2) Complete training (“ complete”):
$(t > t_3 \text{ \& } t_e > t_3)$

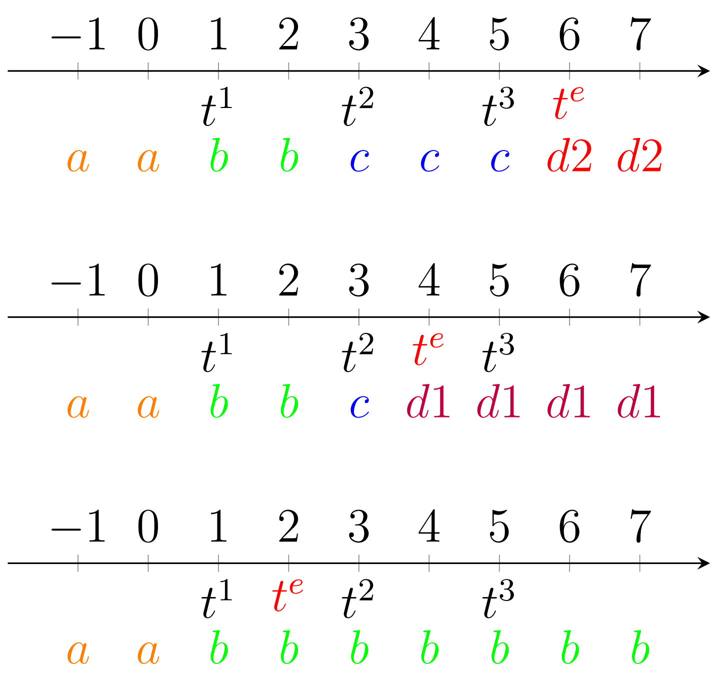
All job seekers assigned to training will belong to one of the states $s \in \{a, b, c, d1, d2\}$ in a given period t. If the job seeker exits unemployment after the end of the training program, she will transition through all states. However, if she exits unemployment earlier, she will remain in that state in all later periods. Figure G.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 (t_1) and is assigned to training that starts in month 3 (t_2) and ends in month 5 (t_3). 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 6. Therefore, she progresses through four states: She (a) is yet to be assigned in months -1 to 0, (b) is yet to start training in months 1-2, (c) is undergoing training in months 3-5, and (d2) has completed training from month 6 and onward.

In the second scenario, the job seeker exits unemployment while undergoing training, i.e., she drops out of training. Therefore, she only progresses to state (d1) and remains there in all periods onward.

In the third 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.

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



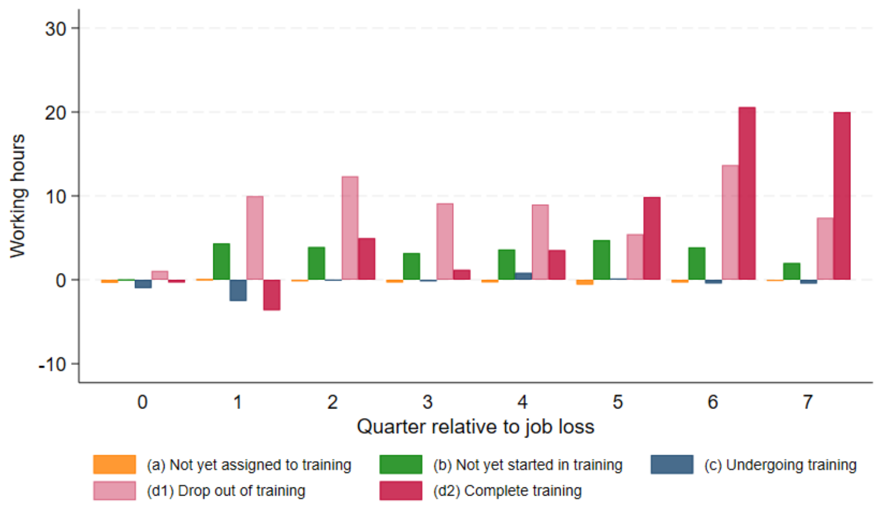
Notes: This figure illustrates how job seekers may transition through training states.

G.2 Results

Figure G.2: Average Hours of Employment by Training State

Notes: This figure shows average monthly hours of employment ("working hours") in months 0-23 following job loss and 95% confidence intervals. First, by estimation of Equation (6), we obtain estimates for the monthly working hours in a given training state s and month t. Second, using the state probabilities as weights (see Figure 4), we compute a weighted average of the monthly working in hours in months 0-23 for a given state. Blue dots represent estimates obtained by OLS regressions, and red dots represent estimates obtained by IV regressions. Standard errors are constructed based on 100 bootstrap repetitions (see Appendix J.1).

Figure G.3: Quarterly Decomposition of the IV Estimate for Hours of Employment



Notes: This figure shows the IV placebo, threat, lock-in, and post-program effects of assignment to classroom training on average monthly hours of employment ("working hours") in a given quarter relative to job loss. The quarterly effects are simple averages of the monthly effects plotted in Figure 5. This figure omits indications of statistical significance.

G.3 Decomposition with Heterogeneous Counterfactuals

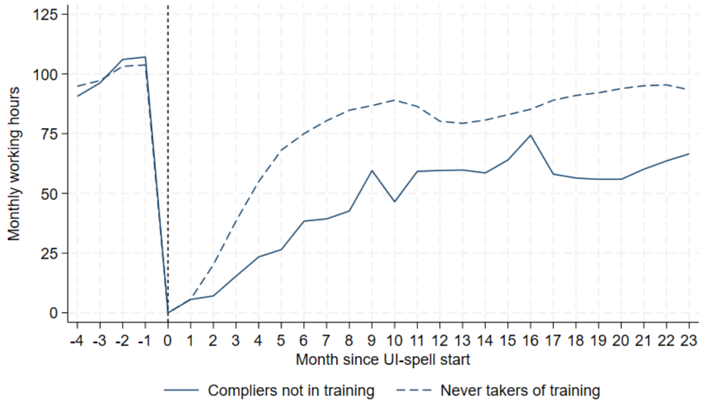
We relax the assumption of homogeneous counterfactuals by estimating state-specific counterfactuals for compliers assigned to training, β_{1t}^{0s}. To this end, we exploit that the average counterfactual for compliers can be written as a weighted average of the state-specific counterfactuals, using the state probabilities as weights:

$$\beta_{1t}^{0} = \sum_{s \in \{a,b,c,d1,d2\}} \gamma_{1t}^{s} \times \beta_{1t}^{0s} \tag{60}$$

We already estimated and plotted the *average* counterfactual outcome of compliers over time, β_{1t}^{0}, in figure D.4. Our challenge is that this average represents a mix of job seekers in the four training states, and splitting it into state-specific counterfactuals requires additional assumptions. Mathematically, Equation (60) constitutes T equations (one average outcome per period) in $5 \times T$ unknowns (five state-specific outcomes per period) and is not identified without restrictions on how outcomes vary across states or time. We will adopt three restrictions on how counterfactuals vary over time. We validate that the data support each restriction.

First, we allow for a general evolution in employment prospects since the time of job loss. For example, as time passes, more job seekers may find jobs for other reasons than changing training states. We measure this general evolution in job prospects based on the outcomes for never-takers, Y_t^{never}. These are job seekers who do not train even if assigned to the most training-inclined caseworker and hence are not shifted across training states by our caseworker instrument. We follow the approach of Bhuller et al. (2020) when identifying the outcome of never-takers; see Online Appendix J.2 for further details. Figure G.4 shows the average employment rate for never-takers and compliers that do not train. Evidently, the two groups share a similar profile where job prospects improve rapidly in the first nine months following job loss and then flatten.

Figure G.4: Never-Takers and Compliers Not in Training



Notes: This figure shows the average monthly hours of employment ("working hours") in a given quarter relative to job loss, for never-takers and compliers not assigned to classroom training. All estimates rely on using percentiles 1 and 99 of the classroom-training instrument as proxies for training-restrained and training-inclined caseworkers. See further details in Online Appendix J.2. This figure omits indications of statistical significance.

Second, we allow the consequences of job loss to differ for compliers and never-takers. For example, if caseworkers target training to job seekers who struggle to find jobs otherwise, we would expect job loss to be more detrimental to the employment prospects of compliers. Indeed, Figure G.4 shows compliers have (increasingly) lower employment after job loss compared to never-takers. We model this gap by introducing a selection premium, δ_0, common to all compliers from the first month after job loss.[65]

Third, we allow for state-specific selection premiums, δ_s. These premiums capture whether or not job seekers switch into training based on their counterfactual in the period. To identify these state-specific premiums, we leverage that the training states kick in at different time horizons, as shown by the state probabilities in Figure 4. For example, if job seekers train when they face adverse job opportunities, we should observe that the mean counterfactual outcome of compliers is comparably low in months 2-6 when they train most intensively. Econometrically, we identify the state-specific selection premiums from periods with high state probabilities, γ_{it}^s.

Putting the pieces together, we model the state-specific counterfactual employment outcome of compliers as follows,

$$\beta_{1t}^{0s} = Y_t^{never} + \delta_0 \times \mathbf{1}(t \geq 1) + \delta_s \times \gamma_{1t}^s \tag{61}$$

The next section explains how we estimate the job loss penalty, δ_0, and state-specific selection

[65] Mechanically, both never-takers and compliers have null (non-supported) working hours in the period of job loss (month 0). For this reason, we identify the job loss penalty for compliers from month 1 onward.

premiums, δ_s. After that, we compute the state-specific counterfactuals and present a decomposition of the baseline IV estimate based on these counterfactuals.

Selection Premiums

In a first step towards estimation of the selection premiums, we insert (61) and estimated parameters in equation (60):

$$\hat{\beta}^0_{1t} = \sum_{s\in\{a,b,c,d1,d2\}} \hat{\gamma}^s_{1t} \times \underbrace{\left(\hat{Y}^{never}_t + \delta_0 \times \mathbf{1}(t \geq 1) + \delta_s \times \hat{\gamma}^s_{1t}\right)}_{\beta^{0s}_{1t}} \tag{62}$$

In this equation, blue font indicates parameters we already have estimated, and red font indicates unknown parameters. Hence, we have T equations in five unknowns. Note that Equation (62) point identifies the parameters if we have five data periods t with varying assignment rates. However, we have more than five periods and will estimate the unknown parameters by minimizing the sum of squared deviations as specified below.[66]

$$\min_{\delta_0,\delta_s} \sum_{t\in\mathcal{T}} \left[\hat{\beta}^0_{1t} \quad - \sum_{s\in\{a,b,c,d1,d2\}} \hat{\gamma}^s_{1t} \times \left(\hat{Y}^{never}_t + \delta_0 \times \mathbf{1}(t \geq 1) + \delta_s \times \hat{\gamma}^s_{1t}\right)\right]$$

$$\text{s.t.} \quad \hat{Y}^{never}_t + \delta_0 \times \mathbf{1}(t \geq 1) + \delta_s \times \hat{\gamma}^s_{1t} \geq 0 \quad \forall (t, s)$$

We solve the minimization problem subject to the constraint that counterfactual employment should always be non-negative. Note that we use all event months (-4: +23), except the month of and prior to job loss (0 and -1), when solving the constrained minimization problem.[67]

Panel (a) of figure G.5 plots the estimated selection premiums. The fact that $\delta_a, \delta_b, \delta_c, \delta_{d1}$, and δ_0 are economically and statistically close to zero suggests that i) never-takers and compliers have similar employment rates prior to job loss, and ii) after a job loss, compliers have similar employment outcomes as never-takers as long as they have not completed their training. Hence, we do not find evidence suggesting that job seekers are assigned to or participate in training when they have worse job prospects. The fact that δ_{d1} is negative and statistically significant suggests that counterfactual employment rates are lower when compliers finish their training. The estimate suggests compliers would have been employed 57 hours *less* than never-takers had

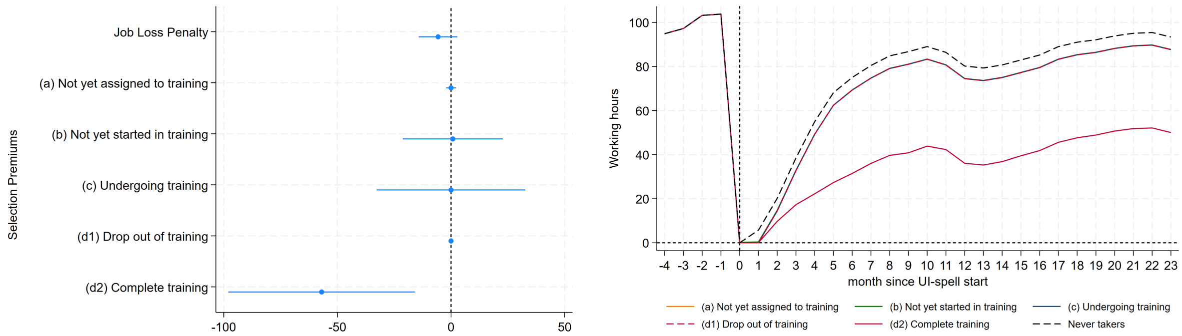
[66] This is inspired by Mogstad et al. (2018) who formulate the conversion of LATE estimates to target parameters as a minimization problem.

[67] We set the starting values to zero and test whether the identified minimum is invariant to this choice of starting values.

they not finished their training. The substantial negative selection premium for job seekers in state (d2) is consistent with trainees being structurally challenged in the labor market.

Panel (b) of figure G.5 plots the counterfactual employment rate in a given state, $\hat{\beta}_t^{0s}$, that results from plugging the estimated never-taker outcome and selection premiums into (61). The figure shows that, following job loss, compliers in state (d2) have much worse job prospects than never-takers.

Figure G.5: Heterogenous counterfactuals



(a) Selection premiums

(b) Heterogeneous counterfactuals

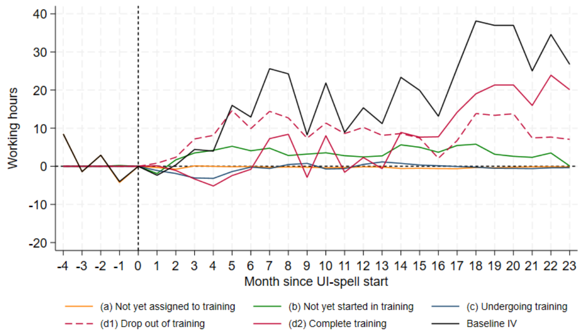
Notes: This figure shows the counterfactual employment rates for compliers in a given training state. Panel (a) plots selection premiums into the training states and 95% confidence intervals (except for state d1, where the standard error of 194.6 would mask the variation in other state premiums). Standard errors in this panel are constructed based on 100 bootstrap repetitions (see description of the bootstrap procedure in appendix J.1). Panel (b) plots the resulting state-specific counterfactual employment rates. There is no indication of statistical significance in Panel (b).

Decomposition with Heterogeneous Counterfactuals

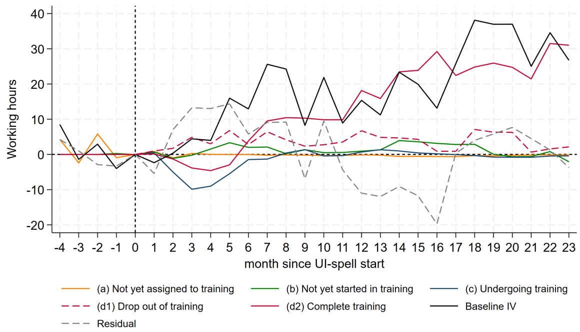
We now have all inputs to do a decomposition of our IV estimate that allows for heterogeneous counterfactuals. Because our model of the counterfactuals has fewer parameters than we have data points, the decomposition will not match the data exactly. For this reason, we introduce a residual term to the decomposition.

Figure G.6 compares our decomposition of the IV estimate with homogeneous and heterogeneous counterfactual employment rates. This figure clearly shows our conclusions are robust to the assumption of homogeneous counterfactuals across states: by large, the threat, lock-in, and post-program effects do not depend on whether we assume homogeneous counterfactuals.

Figure G.6: Decomposition of Effect on Working Hours



(a) Homogeneous counterfactuals



(b) Heterogeneous counterfactuals

Notes: This figure shows decompositions of the baseline IV estimate of the effect of assignment to classroom training on working hours in a given month relative to job loss. Panel (a) represents a decomposition with homogeneous counterfactuals, according to Equation (4), and (7). Panel (b) represents a decomposition with heterogeneous counterfactuals, according to Equation (4), and (61). This figure omits indications of statistical significance.

H Heterogeneity across Training Programs

H.1 Classroom vs. On-The-Job Training

Table H.1: Effects of Training on Labor Market Outcomes in Q7

	Outcomes in quarter seven relative to job loss		
	Working hours	Employment	Earnings (1,000 DKK)
	(1)	(2)	(3)
Classroom training	28.85***	0.21***	5.90***
	(10.71)	(0.07)	(2.08)
On-the-job training	−3.29	−0.05	−1.12
	(22.96)	(0.16)	(4.68)
Obs	167,222	167,222	167,222
Dep. var (pre-job loss)	101.3	0.8	20.5
Cov	yes	yes	yes

Notes: This table shows IV-estimates of the effect of assignment to classroom and on-the-job training on labor market outcomes in quarter seven relative to initial job loss. They include (non-supported) hours of employment ("working hours"), extensive margin employment, and monthly earnings. All regressions include fully interacted job-center-unit-year fixed effects, as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered on predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

H.2 Disaggregation of Classroom Training

Table H.2: Hours of Employment Effect of Assignment to Different Courses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Monthly working hours in quarter relative to job loss											
	Q = -4	Q = -3	Q = -2	Q = -1	Q = 0	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7
Job search courses	0.47	0.94	−1.96	1.96	2.41	16.14	20.62	26.51*	20.55	9.67	36.99***	30.25**
	(7.12)	(7.38)	(3.01)	(3.01)	(3.43)	(13.41)	(15.44)	(14.56)	(14.25)	(14.44)	(12.73)	(14.85)
Skills & wrap-around courses	0.89	2.33	0.99	−0.99	−3.14	2.04	21.37*	6.82	12.35	19.98*	30.48***	21.62**
	(5.12)	(4.68)	(2.41)	(2.41)	(2.91)	(9.84)	(11.61)	(10.04)	(10.87)	(10.86)	(10.32)	(10.30)
On-the-job training	17.16	25.55*	0.62	−0.62	−5.01	52.17**	33.80	−6.64	12.07	21.06	−8.03	0.72
	(12.95)	(13.23)	(5.55)	(5.55)	(7.21)	(26.51)	(24.79)	(21.65)	(23.10)	(23.72)	(22.60)	(22.32)
Obs	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222	167,222
Dep. var mean (pre-job loss)	101	101	101	101	101	101	101	101	101	101	101	101
P-value (JS vs. SW)	0.96	0.86	0.39	0.39	0.13	0.34	0.97	0.19	0.57	0.51	0.64	0.58

Notes: This table shows IV-estimates of the effect of assigning job seekers to a given type of course within the first 12 months of unemployment on average monthly hours of employment ("working hours"). All regressions include fully interacted job-center-unit-year fixed effects, and controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. The bottom of the table reports the p-values for the difference in coefficients on job search courses ("JS") and Skills and wrap-around courses ("SW"). *p<0.10 ** p<0.05 *** p<0.01.

Table H.3: Effects of Different Courses on Labor Market Outcomes in Q7

	Outcomes in quarter seven relative to job loss		
	Working hours	Employment	Earnings (1,000 DKK)
	(1)	(2)	(3)
Job search courses	30.25**	0.15	5.31*
	(14.85)	(0.10)	(2.75)
Skills & wrap-around courses	21.62**	0.16**	4.95**
	(10.30)	(0.07)	(2.05)
On-the-job training	0.72	−0.03	−0.37
	(22.32)	(0.16)	(4.57)
Obs	167,222	167,222	167,222
Dep. var (pre-job loss)	101.3	0.8	20.5
Cov	yes	yes	yes

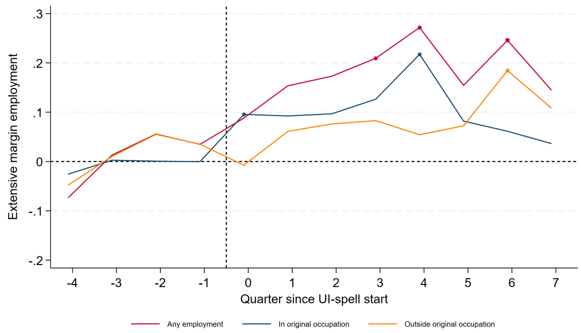
Notes: This table shows IV-estimates of the effect of assignment to "job search" courses, "skills & wrap-around" courses and on-the-job training on labor market outcomes in quarter seven relative to initial job loss. They include (non-supported) monthly hours of employment ("working hours"), extensive-margin employment, and monthly earnings (1,000 DKK). All regressions include fully interacted job-center-unit-year fixed effects, as well as controls for predetermined job seeker characteristics (see Appendix L.7). Standard errors are two-way clustered on predicted caseworker and job seeker levels. *p<0.10 ** p<0.05 *** p<0.01.

Table H.4: Employment In and Outside Original Occupation (3-digits)

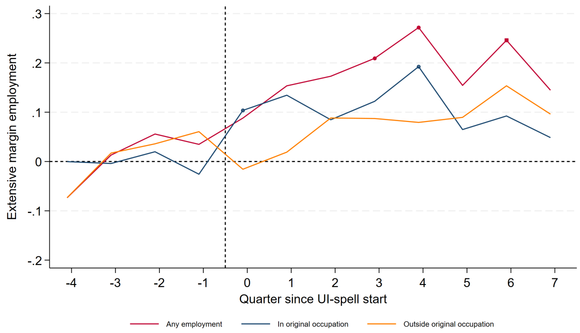
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Extensive margin employment in quarter relative to job loss											
	Q = -4	Q = -3	Q = -2	Q = -1	Q = 0	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7
Panel A: Employment in any occupation												
Job search courses	−0.07	0.01	0.06	0.03	0.09	0.15	0.17	0.21**	0.27**	0.15	0.25***	0.14
	(0.06)	(0.05)	(0.04)	(0.03)	(0.06)	(0.13)	(0.11)	(0.11)	(0.11)	(0.10)	(0.09)	(0.09)
Skills & wrap-around courses	−0.01	0.03	0.08***	0.02	−0.03	0.12	0.15*	0.01	0.16**	0.18**	0.23***	0.17**
	(0.04)	(0.03)	(0.03)	(0.02)	(0.05)	(0.09)	(0.08)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)
On-the-job training	0.19*	0.11	−0.07	0.00	−0.06	0.57**	0.25	0.12	−0.12	0.17	0.01	−0.02
	(0.10)	(0.09)	(0.07)	(0.05)	(0.11)	(0.24)	(0.19)	(0.16)	(0.16)	(0.16)	(0.16)	(0.15)
Panel B: Employment inside the original occupation												
Job search courses	−0.09	−0.07	−0.02	−0.02	0.11**	0.13	0.02	0.08	0.13	0.02	0.08	0.01
	(0.08)	(0.08)	(0.09)	(0.09)	(0.05)	(0.09)	(0.08)	(0.09)	(0.10)	(0.09)	(0.09)	(0.09)
Skills & wrap-around courses	0.00	0.02	0.05	0.01	−0.02	0.01	0.05	−0.02	0.07	0.07	0.01	−0.04
	(0.06)	(0.05)	(0.05)	(0.06)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
On-the-job training	0.21	0.15	0.02	−0.06	0.01	0.16	0.02	−0.14	−0.11	0.04	−0.17	−0.11
	(0.15)	(0.14)	(0.13)	(0.14)	(0.09)	(0.16)	(0.16)	(0.15)	(0.14)	(0.15)	(0.15)	(0.15)
Panel C: Employment outside the original occupation												
Job search courses	0.01	0.08	0.08	0.06	−0.02	0.03	0.15	0.13	0.14	0.14	0.17	0.13
	(0.07)	(0.08)	(0.09)	(0.08)	(0.04)	(0.11)	(0.10)	(0.10)	(0.10)	(0.11)	(0.10)	(0.09)
Skills & wrap-around courses	−0.01	0.01	0.03	0.01	0.00	0.10	0.10	0.03	0.09	0.11	0.22***	0.21***
	(0.05)	(0.05)	(0.05)	(0.06)	(0.04)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)
On-the-job training	−0.02	−0.05	−0.09	0.06	−0.07	0.41**	0.23	0.26	−0.01	0.14	0.17	0.09
	(0.12)	(0.13)	(0.14)	(0.15)	(0.09)	(0.18)	(0.16)	(0.19)	(0.16)	(0.17)	(0.16)	(0.17)
Obs	165,753	165,753	165,753	165,753	165,753	165,753	165,753	165,753	165,753	165,753	165,753	165,753
Cov	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: This table shows IV-estimates of the effect of assignment to training programs on any employment (panel a), employment outside (panel b) and inside (panel c) the job seeker's original occupation, in a given quarter relative to job loss. The IV estimates are based on the main sample restricted to UI-spells of job seekers for whom we could identify their "original occupation": the occupation held most frequently prior to job loss (99.1% of the main sample). The comparison of the "original occupation" and occupations held at any other point in time is based on 3-digit ISCO08 codes. All regressions include fully interacted job-center-unit-year fixed effects and controls for job seeker predetermined characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. *p<0.10 ** p<0.05 *** p<0.01.

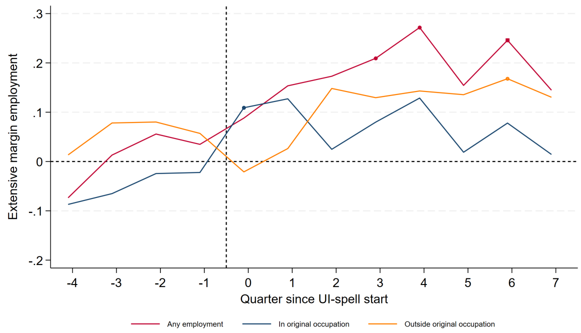
Figure H.1: Robustness of Occupational Mobility Results: Job Search Courses



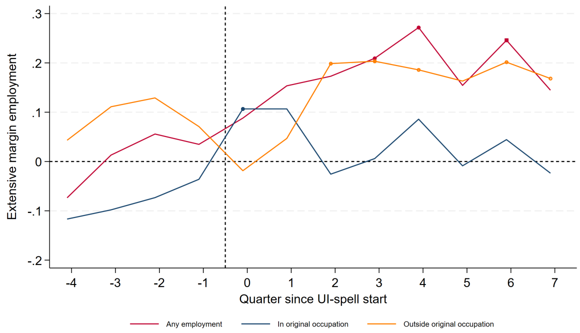
(a) 1-digit ISCO08



(b) 2-digit ISCO08



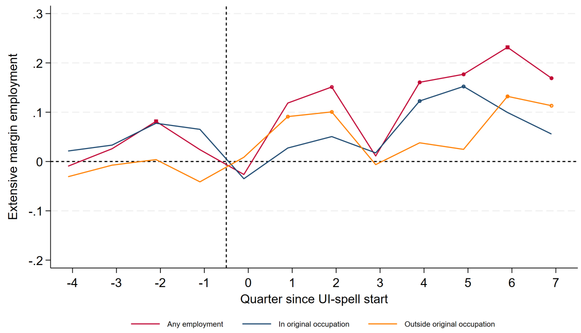
(c) 3-digit ISCO08



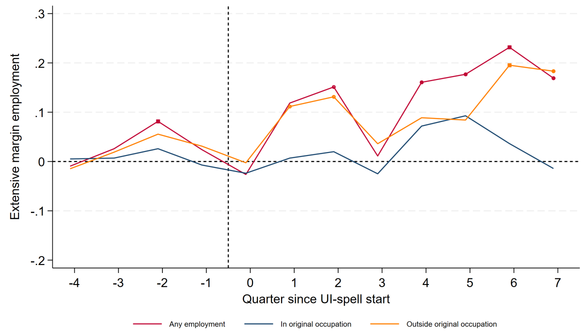
(d) 4-digit ISCO08

Notes: This figure shows the IV-estimates of the effect of assignment to *"job search" courses* on any employment as well as employment in- or outside the job seeker's "original occupation" in a given quarter relative to job loss. The estimates are based on the main sample restricted to UI-spells of job seekers for whom we could identify their "original occupation": the occupation held most frequently prior to job loss (99.1% of the main sample). In panels (a)-(d), the comparison of the original occupation and occupations held at any other point in time is based on 1-4 digit ISCO08 codes. All regressions include fully interacted job-center-unit-year fixed effects and controls for job seeker predetermined characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. Full (hollow) dots indicate significance at the 5% (10%) level, and squares indicate significance at the 1% level.

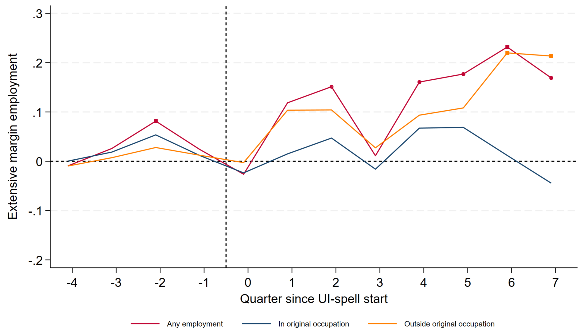
Figure H.2: Robustness of Occupational Mobility Results: Skills & Wrap-Around Courses



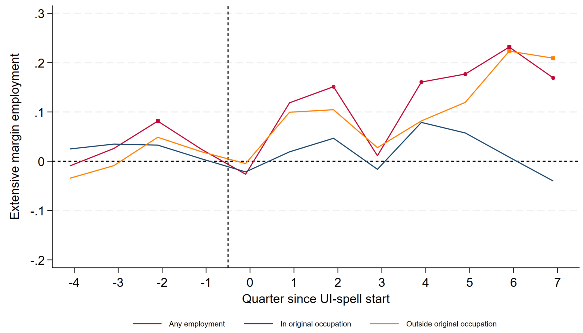
(a) 1-digit ISCO08



(b) 2-digit ISCO08



(c) 3-digit ISCO08



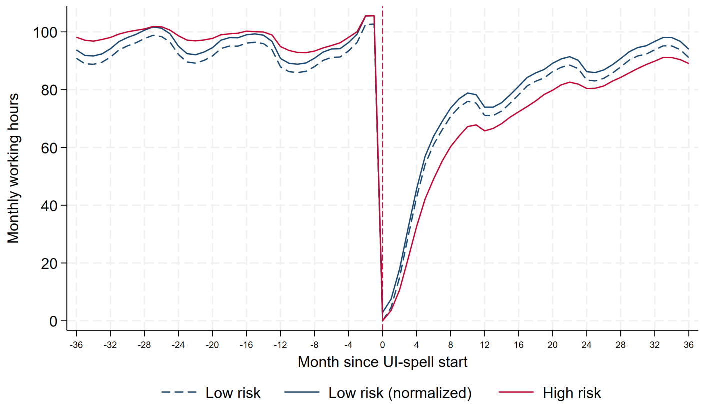
(d) 4-digit ISCO08

Notes: This figure shows the IV-estimates of the effect of assignment to *"skills & wrap-around" courses* on any employment as well as employment in- or outside the job seeker's "original occupation" in a given quarter relative to job loss. The estimates are based on the main sample restricted to UI-spells of job seekers for whom we could identify their "original occupation": the occupation held most frequently prior to job loss (99.1% of the main sample). In panels (a)-(d), the comparison of the original occupation and occupations held at any other point in time is based on 1-4 digit ISCO08 codes. All regressions include fully interacted job-center-unit-year fixed effects and controls for job seeker predetermined characteristics (see Appendix L.7). Standard errors are clustered on predicted caseworker and job seeker level. Full (hollow) dots indicate significance at the 5% (10%) level, and squares indicate significance at the 1% level.

I Heterogeneity across Workers

I.1 Exposure to Offshoring

Figure I.1: Hours of Employment Prospects by Exposure to Offshoring



Notes: This figure shows 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. No indication of statistical significance in this figure.

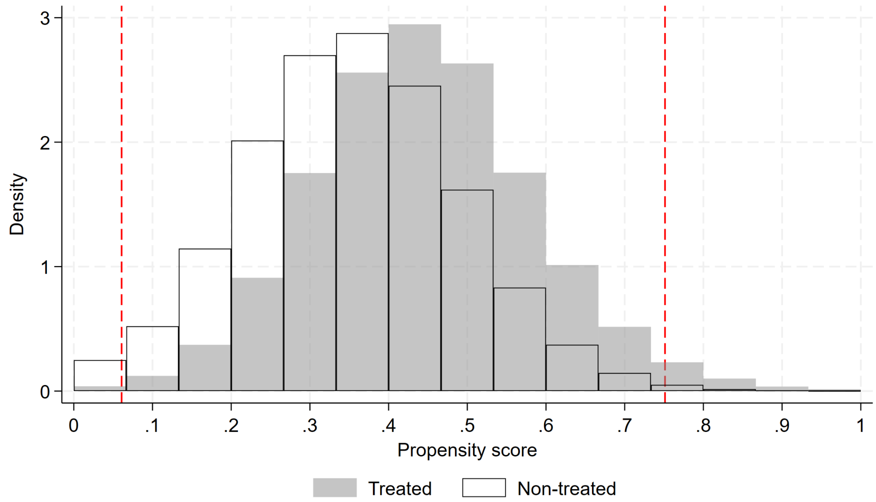
Table I.1: Hours of Employment Effects of Training by Exposure to Offshoring

	Monthly working hours in quarter q relative to job loss											
	Q = -4	Q = -3	Q = -2	Q = -1	Q = 0	Q = 1	Q = 2	Q = 3	Q = 4	Q = 5	Q = 6	Q = 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Low risk job seekers												
Classroom training	0.44	5.21	2.45	−2.45	−3.65	3.97	13.93	7.34	15.86	13.72	29.91**	18.57
	(5.81)	(5.84)	(2.78)	(2.78)	(3.11)	(10.11)	(12.02)	(11.05)	(11.91)	(11.08)	(11.92)	(12.22)
On-the-job training	8.33	18.83	−0.27	0.27	−7.28	35.39	17.32	−30.01	−7.06	1.95	−10.80	−5.76
	(14.27)	(14.01)	(5.79)	(5.79)	(7.18)	(24.62)	(24.17)	(22.47)	(24.53)	(24.00)	(24.26)	(24.90)
Panel B: High risk job seekers												
Classroom training	6.53	−10.65	−4.00	4.00	8.32	19.72	38.11	26.08	14.92	32.51	54.36**	54.89**
	(12.25)	(12.59)	(4.86)	(4.86)	(5.86)	(28.67)	(28.84)	(27.74)	(26.93)	(29.42)	(22.56)	(21.62)
On-the-job training	57.73	66.02	9.47	−9.47	8.04	138.99	118.89	118.74	110.49	132.51	−13.45	11.78
	(50.02)	(49.02)	(18.39)	(18.39)	(20.17)	(112.64)	(120.54)	(115.93)	(105.21)	(117.04)	(82.31)	(84.83)
Obs low risk	125,413	125,413	125,413	125,413	125,413	125,413	125,413	125,413	125,413	125,413	125,413	125,413
Obs high risk	41,809	41,809	41,809	41,809	41,809	41,809	41,809	41,809	41,809	41,809	41,809	41,809
Classroom training Z-stat	0.45	-1.14	-1.15	1.15	1.80	0.52	0.77	0.63	-0.03	0.60	0.96	1.46
Classroom training P-value	0.65	0.25	0.25	0.25	0.07	0.60	0.44	0.53	0.97	0.55	0.34	0.14
On-the-job training Z-stat	0.95	0.93	0.50	-0.50	0.72	0.90	0.83	1.26	1.09	1.09	-0.03	0.20
On-the-job training P-value	0.34	0.35	0.61	0.61	0.47	0.37	0.41	0.21	0.28	0.27	0.98	0.84

Note: This table shows IV estimates of the effect of assignment to classroom training and on-the-job training on monthly hours of employment ("working hours") in a given quarter relative to job loss, for job seekers at high and low risk of offshoring. The estimates are obtained by separately estimating Equations (2)-(3) for job seekers at high- and low risk of offshoring. All regressions include job-center-unit-year fixed effects, as well as controls for job seeker pre-determined characteristics (see Appendix L.7). Standard errors are two-way clustered at the predicted caseworker and job seeker levels. The bottom of the table reports a test (Z-stat= $(\beta_h - \beta_l)/\sqrt{se_h^2 + se_l^2}$ and p-value) of whether the difference in coefficients in the two samples (e.g. the coefficient on classroom training for low vs. high risk) is statistically significant. *p<0.10 ** p<0.05 *** p<0.01.

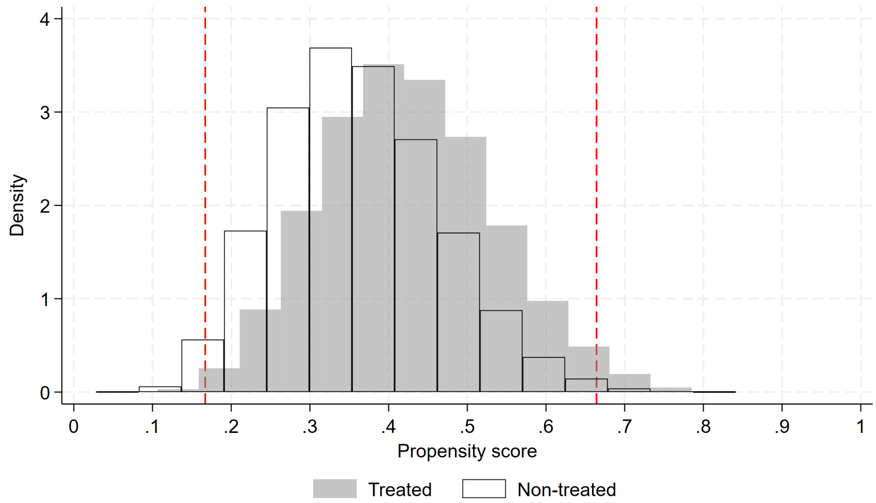
I.2 Marginal Treatment Effects

Figure I.2: Common Support for Classroom Training (Linear Probability Model)



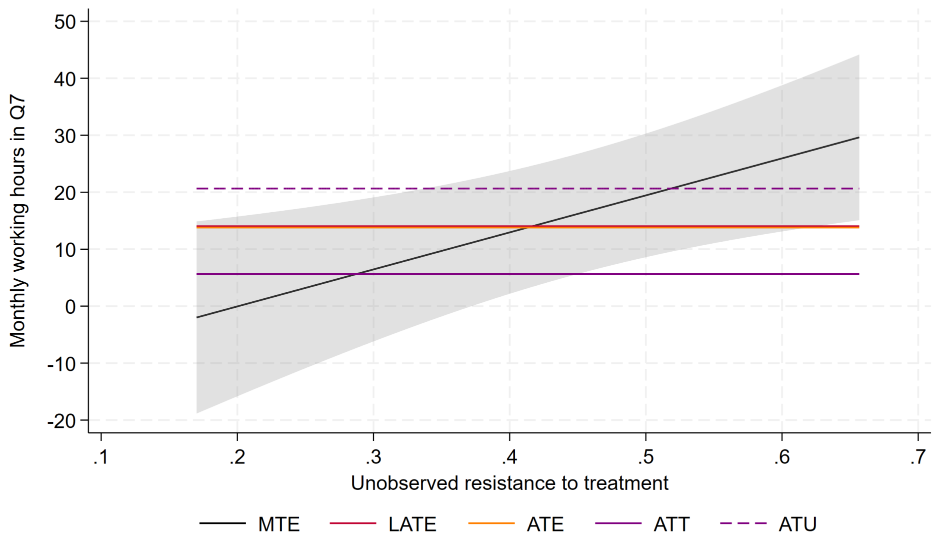
Notes: This figure shows the common support of the propensity score for assignment to classroom training. Gray bars represent the propensity score distribution for assigned ("treated") job seekers, and white bars represent the distribution for non-assigned ("non-treated") job seekers. The propensity scores are obtained by estimating and saving the predicted values from the first-stage equations (2) including job-center-unit-year fixed effects and controls for predetermined job seeker characteristics. The dashed red lines indicates pseudo-percentiles 1 and 99 of the common support propensity scores.

Figure I.3: Common Support for Classroom Training (Logit Model)



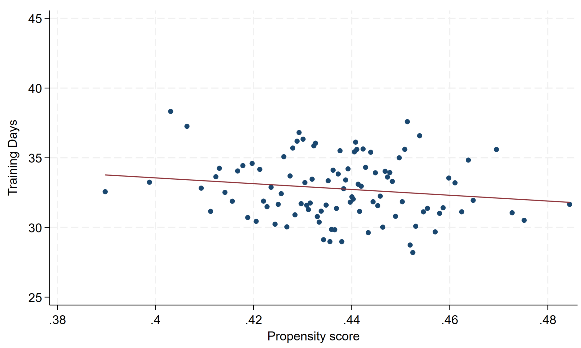
Notes: his figure shows the common support of the propensity score for assignment to classroom training. Gray bars represent the propensity score distribution for assigned ("treated") job seekers, and white bars represent the distribution for non-assigned ("non-treated") job seekers. The propensity scores are obtained by estimating and saving the predicted values from a logit model for assignment to training based on job-center-unit-year fixed effects and controls for predetermined job seeker characteristics. The dashed red lines indicates pseudo-percentiles 1 and 99 of the common support propensity scores.

Figure I.4: MTE, LATE, ATE, ATT and ATU of Classroom Training (Logit Model)

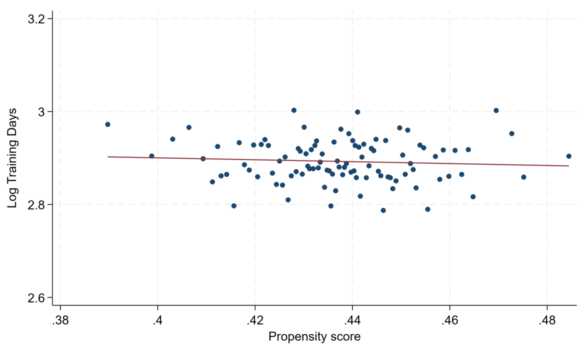


Notes: This figure plots the MTE, LATE, ATE, ATT and ATU of assignment to classroom training on average monthly hours of employment ("monthly working hours") in quarter 7 after initial job loss. As detailed in Appendix B.4, the *MTE* estimates are obtained from a specification with second-order polynomials in the propensity scores for classroom training. The estimations are based on job seekers within the common support of the propensity score, which is obtained by estimation of a logit model for assignment to classroom training (trimmed by percentile 1 and 99). Note the x-axis in this figure differs slightly from that in Figure I.3, because the propensity scores are re-estimated based on the common support sample. The shaded area represents 90% confidence intervals for the MTE function (these do not account for generated regressors). The *LATE* estimate is obtained by averaging over individual LATE estimates. These are obtained by integrating the MTE function from the propensity score if the job seeker was assigned the least vs. most training-inclined caseworker (as approximated by percentile 1 and 99 on the relevant caseworker tendency instrument). The *ATE* estimate is obtained by integrating the MTE function over the common support of the propensity score. The *ATT* estimate is obtained by integrating the MTE function, while giving more weight to low-U_1 job seekers. The *ATU* estimate is obtained by integrating the MTE function, while giving more weight to high-U_1 job seekers.

Figure I.5: Job Seeker Training Propensity Scores and Training Duration



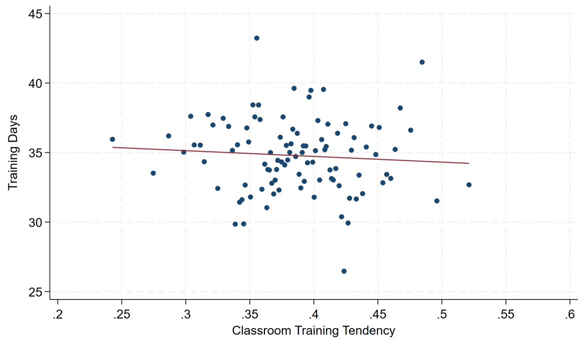
(a) Training days



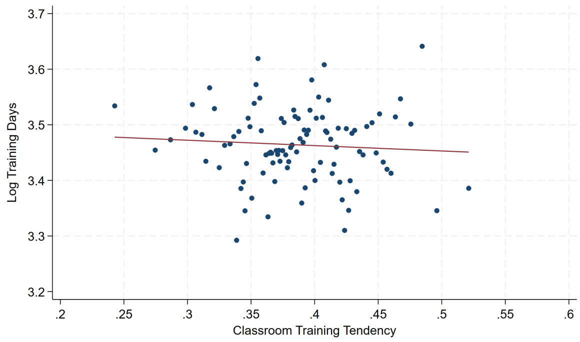
(b) Log Training Days

Notes: This figure represents a binned scatter plot of the propensity score for trainees (which sets the upper limit on the resistance to training for this group) and the training duration of the first training program (controlling for job-center-unit-year fixed effects and predetermined job seeker characteristics). Panel (a) plots mean training days (removing two outlier programs) and panel (b) plots mean log training days. The slope of the fitted line is -21 (p-val=0.13) in panel (a) and -0.2 (p-val=0.44) in panel (b).

Figure I.6: Caseworker Training Tendencies and Training Durations



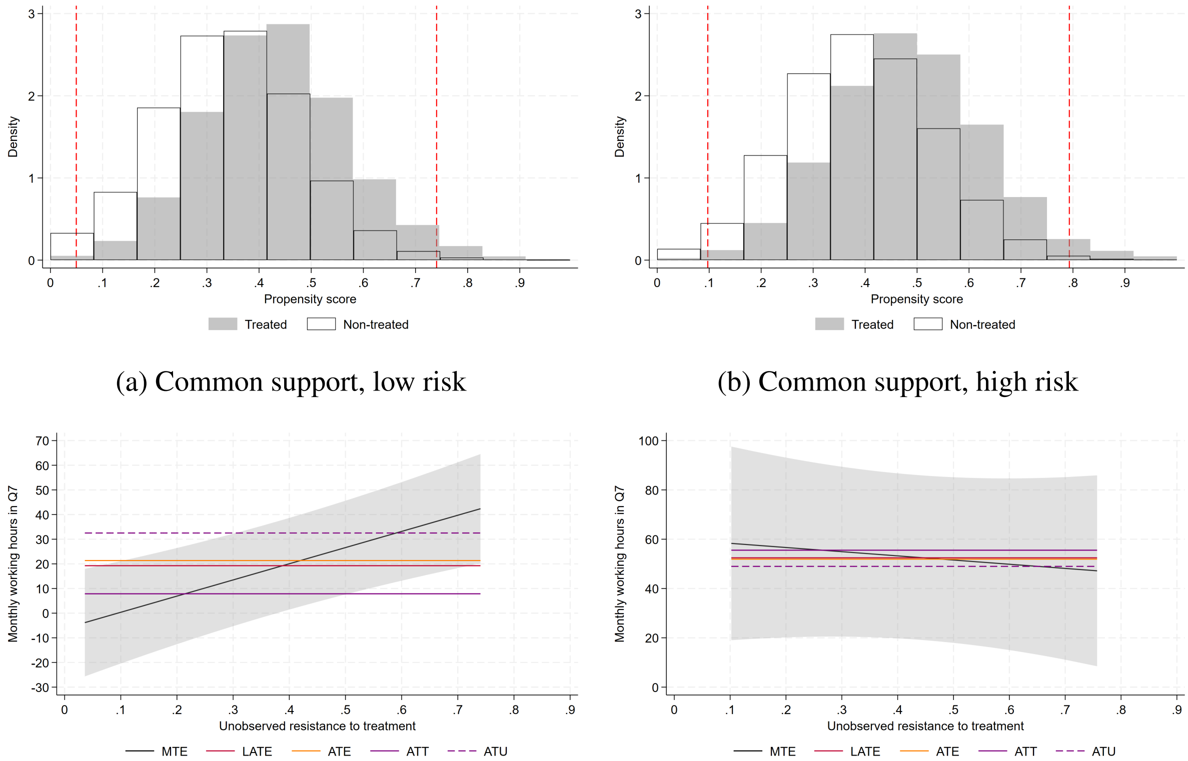
(a) Training days



(b) Log Training Days

Notes: This figure represents a binned scatter plot of caseworker classroom training tendencies and the average training duration of the first training program (controlling for job-center-unit-year fixed effects and the caseworker's training tendency for on-the-job training). Panel (a) plots mean training days (removing two outlier programs) and panel (b) plots mean log training days. The slope of the fitted line is -4.1 (p-val=0.55) in panel (a) and -0.10 (p-val=0.58) in panel (b).

Figure I.7: Common Support and MTE of Classroom Training by Offshorability Risk



(a) Common support, low risk

(b) Common support, high risk

(c) MTE, low risk

(d) MTE, high risk

Notes: This figure shows MTE estimations of the effect of assignment to classroom training on average monthly hours of employment ("working hours") in quarter 7 relative to job loss, 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. The propensity scores are obtained by estimating and saving the predicted values from the first-stage equations (2) including job-center-unit-year fixed effects and predetermined job seeker characteristics. Panels (c) - (d) plot the MTE, LATE, ATE, ATT and ATU of assignment to classroom training on monthly working hours in quarter 7 relative to job loss. As detailed in Appendix B.4, the *MTE* estimates are obtained from a specification with second-order polynomials in the propensity scores for classroom and on-the-job training. The estimations are based on job seekers within the common support of the relevant propensity score. The shaded area represents 90% confidence intervals for the MTE function (these do not account for generated regressors).

J Estimation Procedures

J.1 Bootstrap Standard Errors

We bootstrap standard errors on a given statistic, b, using a "Bayesian Bootstrap procedure" Shao and Tu (2012):

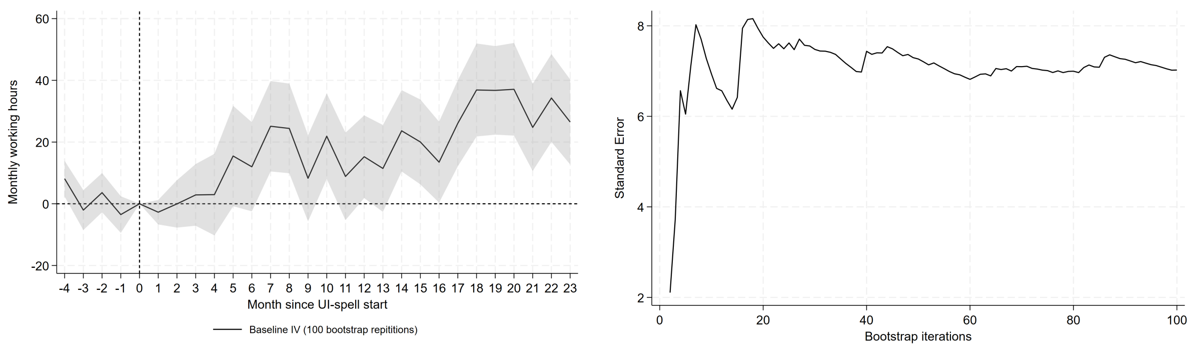
1. Assign each observation in the sample a random and strictly positive weight. Obtain these weights by (i) drawing numbers from a uniform distribution and (ii) normalizing by the sum, $w_i = \frac{u_i}{\sum_i u_i}$.
2. Recompute the statistic with these weights.

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:

$$se(b) = \sqrt{\frac{1}{N} \times \sum_{n}^{N} (b_n - \bar{b})^2}. \tag{63}$$

Figure J.1 illustrates the convergence of standard errors for a given number N of bootstrap repititions. Panel (a) plots the IV-estimate of assignment to classroom training on average working hours in a given month relative to job loss (also plotted in Figure 5). Panel (b) plots the convergence of bootstrapped standard errors (on the IV estimate in month 23) as we increase the number of bootstrap iterations. The procedure of Andrews and Buchinsky (2000) (which still is state-of-the-art; see, e.g., applications in Serrato and Wingender (2016) and Head and Mayer (2019)) builds on this convergence plot by formalizing how the rate of convergence informs the appropriate number of bootstrap replications. The convergence plot shows that the Bootstrap estimates converge beyond 50 iterations (after some initial fluctuations due to the limited number of resamples). Hence, our choice of 100 replications is well beyond standard criteria for the minimum number of bootstrap replications. Furthermore, since the bootstrap standard errors converge from above, our estimates are conservative of the population parameters. For example, increasing the number of replications from 50 to 100 only decreases our standard error estimates by 3.4%.

Figure J.1: Convergence of Bootstrap Standard Errors



(a) Baseline IV Estimate

(b) Standard Error of IV Estimate in Month 23

Notes: Panel (a) plots the baseline IV estimate from Figure 5b showing the effect of assignment to classroom training on hours of employment ("working hours") in a given month relative to job loss. The colored band represents 95% confidence intervals. Standard errors are constructed based on a "Bayesian Bootstrap Procedure" (as detailed in Appendix J.1), using 100 bootstrap repetitions (see Appendix J.1). Panel (b) plots the bootstrapped standard errors for a given number of iterations.

J.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 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,[68] 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[69] - 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, $\delta_{q(i)}$ and the cross-instrument Z_{-ki} (and covariates, X_i). 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 $Z^{r,min}_k$ and $Z^{r,max}_k$, respectively.

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

[69] 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, $Z_{ki}|\delta_{j(i)} \sim Z_{ki}|\delta_{j(i)}, Z_{-ki}$.

3. Estimate the first-stage regression

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

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

$$\hat{D}^{r,min}_k = \hat{\pi}_{10} + \hat{\pi}_{11} Z^{r,min}_k \tag{65}$$

$$\hat{D}^{r,max}_k = \hat{\pi}_{10} + \hat{\pi}_{11} Z^{r,max}_k \tag{66}$$

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

$$P_c = \hat{D}^{r,max}_k - \hat{D}^{r,min}_k = \hat{\pi}_{11} \times (Z^{r,max}_k - Z^{r,min}_k) \tag{67}$$

$$P_a = \hat{D}^{r,min}_k = \hat{\pi}_{10} + \hat{\pi}_{11} Z^{r,min}_k \tag{68}$$

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

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

1. Residualize training intervention D_{ki} and the instruments, Z_{ki} and Z_{-ki}, on job-center-unit-year fixed effects, $\delta_{q(i)}$ (and covariates, X_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 $Z^{r,min}_k$ and $Z^{r,max}_k$, respectively. A restrained and 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 first-stage regression. Due to extended monotonicity, the coefficient on the cross-instrument, π_{12}, is close to zero:

$$D^r_{k,i} = \pi_{10} + \pi_{11} Z^r_{k,i} + \pi_{12} Z^r_{-k,i} + u_{k,i}. \tag{70}$$

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

$$\hat{D}^{r,max}_k = \hat{\pi}_{10} + \hat{\pi}_{11} Z^{r,min}_k + \underbrace{\hat{\pi}_{12}}_{\sim 0} Z^{r,min}_{-k} \tag{71}$$

$$\hat{D}^{r,min}_k = \hat{\pi}_{10} + \hat{\pi}_{11} Z^{r,max}_k + \underbrace{\hat{\pi}_{12}}_{\sim 0} Z^{r,max}_{-k} \tag{72}$$

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

$$P_c = \hat{\pi}_{11} \times (Z_k^{r,max} - Z_k^{r,min}) + \underbrace{\hat{\pi}_{12}}_{\sim 0} \times \underbrace{(Z_{-k}^{r,max} - Z_{-k}^{r,min})}_{\sim 0} \tag{73}$$

$$P_a = \hat{\pi}_{10} + \hat{\pi}_{11} Z_k^{r,min} + \underbrace{\hat{\pi}_{12}}_{\sim 0} Z_{-k}^{r,min} \tag{74}$$

$$P_n = 1 - P_c - P_a. \tag{75}$$

Potential outcomes

Building on the population shares obtained above, we can estimate the potential outcomes for compliers, always-takers, 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, $\delta_{q(i)}$ (and covariates, X_i). Add the unconditional mean and call the residualized variables, $Y_{i,t}^r$, $Z_{k,i}^r$, and $Z_{-k,i}^r$.

2. Identify a k-restrained and k-inclined caseworker as the 1st and 99th percentiles of the own-instrument $Z_{k,i}^r$. Denote these $Z_k^{r,min}$ and $Z_k^{r,max}$, respectively. A restrained and an 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 reduced form *conditional on treatment* and predict the outcome for job seekers assigned to a k-restrained and k-inclined caseworker:

$$Y_{i,t}^r = \alpha_0 + \alpha_k Z_{k,i}^r + \alpha_{-k} Z_{-k,i}^r + u_i, \quad \text{if} \quad D_{k,i} = 1 \tag{76}$$

$$\hat{Y}_t(D_k = 1, Z_{k,i}^{r,min}, Z_{-k,i}^{r,mean}) = \hat{\alpha}_0 + \hat{\alpha}_k Z_{k,i}^{r,min} + \hat{\alpha}_{-k} Z_{-k,i}^{r,min} = \hat{Y}_t^{always} \tag{77}$$

$$\hat{Y}_t(D_k = 1, Z_{k,i}^{r,max}, Z_{-k,i}^{r,max}) = \hat{\alpha}_0 + \hat{\alpha}_k Z_{k,i}^{r,max} + \hat{\alpha}_{-k} Z_{-k,i}^{r,max}. \tag{78}$$

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:

$$Y_{i,t}^r = \alpha_0 + \alpha_k Z_{k,i}^r + \alpha_{-k} Z_{-k,i}^r + \epsilon_i, \quad \text{if} \quad D_{k,i} = 0 \tag{79}$$

$$\hat{Y}_t(D_k = 0, Z_{k,i}^{r,min}, Z_{-k,i}^{r,min}) = \hat{\alpha}_0 + \hat{\alpha}_k Z_{k,i}^{r,min} + \hat{\alpha}_{-k} Z_{-k,i}^{r,min} \tag{80}$$

$$\hat{Y}_t(D_k = 0, Z_{k,i}^{r,max}, Z_{k,i}^{r,max}) = \hat{\alpha}_0 + \hat{\alpha}_k Z_{k,i}^{r,max} + \hat{\alpha}_{-k} Z_{-k,i}^{r,max} = \hat{Y}_t^{never}. \tag{81}$$

5. Because always-takers start in training even if they are assigned to the most k-restrained caseworker, (77) 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, (81) identifies the outcome for never-takers.

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

$$\hat{Y}_{0,t}^{com} = \hat{Y}_t(D_k = 0, Z_k^{max} > Z_k^{min}) \tag{82}$$

$$= \frac{P_c + P_n}{P_c} \times \hat{Y}_t(D_k = 0, Z_{k,i}^{r,min}, Z_{-k,i}^{r,min}) - \frac{P_n}{P_c} \times \hat{Y}_t(D = 0, Z_{k,i}^{r,max}, Z_{-k,i}^{r,max}). \tag{83}$$

8. To get the outcome for compliers if they do start in training, note that job seekers who start in training are a mix of always-takers and encouraged compliers:

$$\hat{Y}_{1,t}^{com} = \hat{Y}_t(D_k = 1, Z_k^{max} > Z_k^{min}) \tag{84}$$

$$= \frac{P_c + P_a}{P_c} \times \hat{Y}_t(D_k = 1, Z_{k,i}^{r,max}, Z_{k,i}^{r,max}) - \frac{P_a}{P_c} \times \hat{Y}_t(D_k = 1, Z_{k,i}^{r,min}, Z_{-k,i}^{r,min}).. \tag{85}$$

K Cost-Benefit Analysis

In this section, we examine the direct costs and benefits of classroom training. We use the estimates conduct simple calculations of the cost-benefit ratio and internal rate of return of classroom training. To be clear, these calculations are meant as "back-of-the-evelope" illustrations of our estimated magnitudes and omit several important factors, including general equilibrium effects in labor markets (Heckman et al., 1998) and non-pecuniary costs of training (Maibom, 2022).

K.1 Program Costs

We measure the costs of classroom courses using the "rate catalogs" published by the Danish Ministry of Education (2024). We use the program costs of vocational non-degree courses ("AMU kurser") as this is the largest group of classroom courses (Table L.4) and generally more expensive than "job search" or "wrap-around" courses, making our cost-benefit ratios conservative.

We use the rate catalogs from our sample period 2011-2019 and the formula for the program costs used by the Ministry of Education.[70] We scale the weekly costs by 7.4 ($= 52/7$), reflecting the average 52-day duration of classroom training in our sample (Table 1).

Using these inputs, we calculate that classroom training costs 23,319 DKK ($3,743) per trainee when using the average course price in the rate catalogs. There is a distribution of costs across vocational non-degree courses. In particular, classroom training costs 22,197 DKK when using the median price, 14,764 DKK when using the 1st percentile of course prices, and 41,851 DKK when using the 99th percentile of course prices.

K.2 Program Benefits

We measure the benefits of classroom training as the effects on earnings. Table 8 shows that the status-quo policy increases the earnings of trainees by 5,200 DKK per month in quarter 7

[70]The formula for program costs is given by:

$$PricePerWeek = \frac{(Cost + Overhead + Building)}{40} + \frac{(OverheadTransport + BuildingTransport)}{CourseLength},$$

where the rates are measured in full-time years (200 course days), and the course length is left-censored at one week. The Ministry of Education used this formula when imposing a price cutoff on courses in 2011 (Ramboll, 2011).

after job loss. Figure D.2 shows that we have stable and positive point estimates in quarters 2-11. However, to be conservative, we only use the stable and statistically significant effects in quarters 6-9. Assuming that the 5,200 DKK monthly earnings effects are valid in all four quarters 6-9, this gives a total nominal earnings effect of 62,400 DKK ($= 5200 \times 3 \times 4$) per trainee.

K.3 Rate of Return

Combining the estimates from above implies that the average program benefits of classroom training are 2.7 ($= 62400/23319$) times higher than the average program costs. Discounting the flows of costs and benefits, the net present value of classroom training is positive for any annual interest rate below 75%.[71] Notably, this is a very high internal rate of return (IRR). In comparison, the "returns to college" literature often finds IRRs around 12% per year (Kane and Rouse, 1995; Heckman et al., 2003). Even with the most extreme program costs (99th percentile), the nominal benefits of classroom training are 1.5 ($= 62400/41851$) times higher than the costs, and the IRR is 25% per year.

L Data Construction

L.1 UI Spells

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 that follows 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.

[71] We assume that the costs fall in week 18 (the average start date of training, cf. Table 1) and the benefits fall equally in quarters 6-9 (the significant effects in Figure D.2).

L.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 2012-2019. This dataset contains information on the date, time, and type of all meetings. The data also record 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 our steps 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 2012-2018 with information about all caseworker meetings held during the UI spell.[72] Table L.1 shows we are able to match around 50% of all UI spells (75% 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 19 to 34 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 impose three restrictions on the sample 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",[73] and if it (iii) takes place "in person" (i.e., not held via telephone or e-mail). Table L.1 shows these restrictions do not affect the average UI spell length.

[72] 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 the UI spell ends.

[73] To be precise, we require the meeting type to be coded as "Jobsamtale", "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%) of caseworkers in a job center change identifiers. We provide more details below in Online Appendix L.3. Table L.1 shows the crosswalk reduces the number of unique caseworkers by 446 (3%).

Identifying 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 as the *realized caseworker*. For the vast majority (99%) of job seekers, the first face-to-face meeting simply corresponds to the 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 meets 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 is a face-to-face meeting). If neither the first nor the second meeting was a face-to-face meeting, we drop the UI spell from the sample. Table L.1 shows when we collapse the data to the first face-to-face meeting, we keep almost all job seekers (99%) but lose about 45% of the caseworkers. This finding may reflect that some job centers have caseworkers who only meet job seekers if they become long-term unemployed.

Identifying 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 of 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 L.4. Table L.1 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 42%.

Table L.1: Linked job seeker–Caseworker Data Restrictions

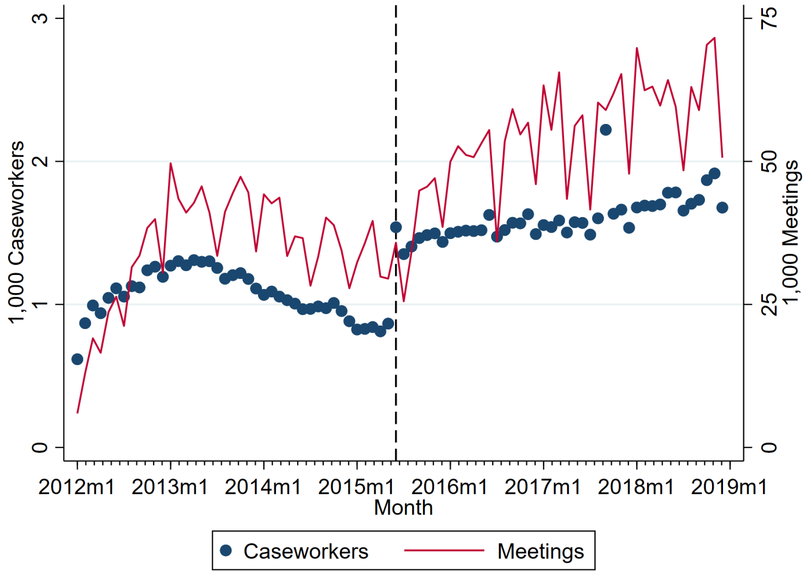
				Caseworkers		Average	
	UI-spells	Jobseekers	Jobcenters	Realized	Predicted	Weeks of UI	Compliance
UI-spells from 2012-2018	**2,200,785**	**940,971**	**94**	.	.	**19**	.
- who had at least one caseworker meeting	1,109,370	700,058	94	18,681	.	34	.
- timing, type and contact	944,462	633,445	94	14,372	.	33	.
- crosswalk caseworker id's	944,462	633,445	94	13,926	.	33	.
- identify realized caseworker	942,141	632,176	94	7,910	.	33	.
- identify day-of-birth protocol	934,922	628,352	94	7,910	1,949	33	0.42
Linked jobseeker-caseworker data	**934,922**	**628,352**	**94**	**7,910**	**1,949**	**33**	**0.42**

Notes: This table shows the 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 2012-2018 in Denmark. Row (2) restricts the data to UI spells that had at least one caseworker meeting. Row (3) restricts the data to UI spells for whom the first meeting (i) took place within the first 16 weeks of job loss, (ii) was labeled 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 the data to UI spells for whom we can identify their realized caseworker. Row (6) restricts the data 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.

L.3 Crosswalk of Caseworker IDs

Figure L.1 shows the number of registered meetings (red line) as well as the number of active caseworker IDs (blue dots) in a given month. The black dashed line indicates the implementation of the 2015-employment reform in Denmark, 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 shows the number of caseworkers generally follows the trend in meetings (unemployment). However, the figure also shows two significant jumps in the number of active caseworker IDs in June 2015 and September 2017. The sudden jumps in the number of active caseworker IDs (relative to the number of meetings) suggest a data break that likely could be caused by the adoption of new IT systems. Namely, the caseworker IDs are generated in the local job centers and may be discontinued if the job center adopts a new IT system.

Figure L.1: Caseworker IDs and meetings



Notes: This figure shows the registered number of caseworker IDs and meetings held in the Danish job centers from 2012 to 2019. The blue dots represent the number of unique caseworker IDs that were active in a given month (i.e., registered at least one meeting). The red line represents the total number of meetings held in the job centers in a given month. The black dotted line indicates a reform that increased the number of meetings during the first six months of unemployment.

We now define a job-center-specific churning rate that is informative about whether caseworker IDs turn over in a given month. Let $caseworkers_{qt}$ represent all caseworker IDs that were active in job center q in month t. Further, let $exits_{qt}$ represent all caseworker IDs that were active in month t but not in month t+1, and $entries_{qt}$ 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 q and month t can then be measured as

$$churning_{qt} = \frac{exits_{qt} + entries_{qt}}{caseworkers_{qt}} \tag{86}$$

We bound this churning rate to be between 0 and 1.[74] When calculating the churning rate for a given job center q in a given month t, we will require

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

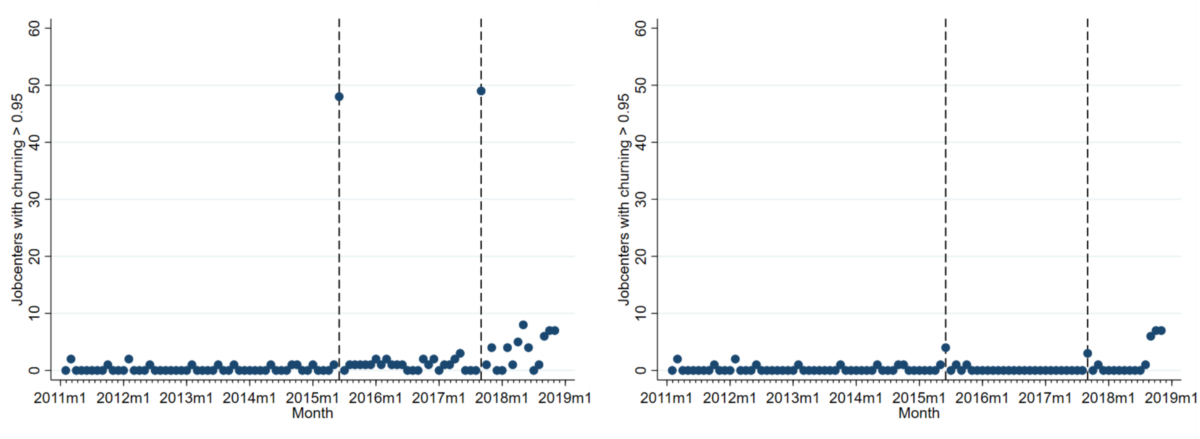
iii. the job center did not have churning $= 1$ in the previous period.[75]

Figure L.2a uses the raw data to plot the number of job centers in a given month with a churning rate $\geq$ 0.95 (denoted "high churning" hereafter). Note that close to half of all job centers

[74]In principle, the churning rate could be $>$ 1, if some caseworker IDs enter and exit in the same month.
[75]If churning = 1 in two consecutive months, the first churning event is kept.

experience a churning rate ≥ 0.95 in June 2015 and September 2017. This could reflect that many job centers changed IT system in these two months, such that caseworkers are assigned new IDs. To account for these breaks, we develop the crosswalk algorithm described below. Figure L.2b plots the number of job centers with a high churning rate after this crosswalk has been implemented. Clearly, the crosswalk reduces the number of job centers with a high churning rate substantially.

Figure L.2: job centers with churning ≥ 0.95



(a) Raw data (b) Crosswalk

Notes: This figure is based on job centers with at least 4 active caseworker IDs in a given month. Each dot represents the number of these job centers that had a churning rate of at least 0.95. Panel (a) is based on the raw data; Panel (b) is based on the crosswalked data.

Crosswalk algorithm for a given job center and break month t:

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

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

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

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

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

   ii. Find all job seekers that met with caseworker j in the *6 months* before the data break.

ii. Count the number of meetings these job seekers have with any of the caseworkers in the *6 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. I.e., for a given caseworker before the data 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 largest crosswalk.

4. Accept mapping if certain criteria are met:

i. The mapping must be based on a crosswalk of at least x job seeker meetings. The threshold x is data-driven and job center-specific and obtained as follows. For a given job center: Do Step 1-2 for a *placebo* break month (we use July 2016 since no job centers had churning=1 in this month). The off-diagonal elements in the placebo transition matrix inform us about the magnitude of crosswalks in a month *without* a break. Use the 95^{th} percentile of these crosswalks as threshold x if at least 3 active caseworkers. If less than three active caseworkers, use the average threshold for other job centers.

ii. The mapping must satisfy within-period consistency: Around a given data break month, the before-caseworker in one row cannot equal the after-caseworker in a different row. If this is the case, we disregard both mappings.

iii Implement crosswalk in a given job center and month if the churning rate in the job center in that month is > 0.95. If the churning rate is > 0.95 in consecutive months, we only implement the first churning event.

L.4 Day-Of-Birth Allocation Rules

Based on a survey of job centers and interviews with caseworkers, we know that job centers in Denmark often allocate job seekers to caseworkers based on the job seekers' day of birth. 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 visually inspect 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

For each caseworker in a given job-center-year, we calculate the share of job seekers

- above age 30
- with an academic education[76]

We then check the distribution of the caseworker means in a given job-center-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 whose birthdays fall 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.

[76]We define academic as having (at least) a bachelor's degree. Using Danish classifications, we include "60 Bacheloruddannelser", "70 Lange videregående uddannelser" and "PhD og forskeruddannelser".

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 (or a few) caseworkers who deviate from the rule. If this caseworker also has a very large caseload, she will become the predicted caseworker for all job seekers (every 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-unit-year.

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 days 1-7, another caseworker is responsible for days 8-15, and so on.[77] 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 where there is no clear system in the imputed day-of-birth allocation rule exists or where the compliance with the imputed rule is very low.

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

L.5 Job Plans

We measure training program assignments using the individual job plans that caseworkers prepare for job seekers. These job plan registrations are collected by the Danish Agency for Labor Market and Recruitment (STAR) in the so-called *PLAN* register. Since 2015, this register has been the source of two well-known sources of information about ALMPs in the Danish labor market: *jobindsats.dk* (aggregate statistics on ALMPs) and *Danish Register for Evaluation of Marginalization (DREAM)* (individual-level register on public programs).

We are the first to use PLAN for research. The register offers two advantages relative to existing data sources on ALMPs. First, PLAN records *assignments* to ALMPs, whereas DREAM and jobindsats.dk only show *participations* in ALMPs. In particular, PLAN includes the start dates, end dates, and activity status (e.g., canceled or completed) of all assigned programs, including those that job seekers do not participate in. Second, PLAN includes detailed *course titles*, which allow us to categorize programs along economically meaningful lines. In particular, DREAM and jobindsats.dk include a large residual category of classroom courses, "other activation (ØVO)", which we classify into "job search", "skills", and "wrap-around" based on their course titles.

Section L.5.1 provides an overview of the types of ALMPs we measure in PLAN. Section L.5.2 explains how we use the course titles to classify training programs. Finally, Section L.5.3 validates PLAN against existing registers on participation in ALMP and education programs.

L.5.1 Training Programs

We use PLAN to distinguish between two broad classes of ALMPs: classroom training and on-the-job training.[78] Table L.2 provides summary statistics on each of these programs. Classroom training consists of about 11% job search courses (CV and job application courses), 48% skills training (vocational training and ordinary education), and 41% wrap-around services (e.g., job counseling and matching).[79] Section L.5.2 further describes the contents of classroom training.

[78] This distinction relies on two variables in PLAN data, *course_id* and *job_id*, each of which indicates the specific types of classroom and on-the-job training programs. We use the variable formats from STARWIKI, *job_id*: https://starwiki.atlassian.net/wiki/spaces/FYS/pages/48890079/JobOrderTypeIdentifier and *course_id*: https://starwiki.atlassian.net/wiki/spaces/FYS/pages/48890020/CourseTypeIdentifier.

[79] The Workforce Investment Act (WIA) in the US uses the following related terminology: "core" (job search courses), "intensive" (caseworker advice and wrap-around services), and "full" (skills training) to delineate program types (Decker and Berk, 2011).

On-the-job training consists of 71% internships and 22% wage subsidies, but wage subsidies are typically longer programs (119 vs. 37 days). Table L.3 show that about 66% of internships and 41% of wage subsidies occur at private firms.

Table L.2: Active Labor Market Programs

	(1) Assignments (count)	(2) Share (%)	(3) Timing (week no.)	(4) Duration (days)
Classroom Training	**103,271**	**100**	**18**	**55**
- Job search courses	11,562	11	18	19
- Skills courses	49,390	48	19	54
- Wrap-around courses	42,319	41	20	51
On-the-job training	**59,960**	**100**	**21**	**88**
- Internships	42,854	71	21	37
- Wage subsidies	13,236	22	24	119
- Other	3,870	6	28	295

Notes: This table shows all assignments to classroom and on-the-job training, as well as the underlying categories, during the first 12 months of unemployment. The statistics for classroom training deviates slightly from those reported in Table 1 for two reasons. First, the panel is based on a combination of original activation categories recorded in the Job Plan data as well as data on course titles (Table 1 was only based on original activation categories). Second, we do not collapse assignments that overlap in time (Table 1 collapsed assignments that overlapped in time to one assignment). Because a job seeker may be assigned to multiple training programs, the same job seeker can appear in multiple rows. Column (1) reports total assignments to a given category. Column (2) reports the share of assignments to a given activation category in percent of the total classroom (on-the-job) training assignments. Column (3) reports the average timing (week number in the UI-spell) of the first assigned activity. Column (4) reports the total duration (days) of all assigned activities.

Table L.3: On-The-Job Training Programs: Detailed Breakdown

	(1) Assignments (count)	(2) Share (%)	(3) Timing (week no.)	(4) Duration (days)
On-the-job training	**59,960**	**100**	**21**	**88**
- Wage subsidies	13,236	22	24	119
- Private	5,552	9	26	110
- Public	7,684	13	23	123
- Internships	42,854	71	21	37
- Private	28,050	47	22	38
- Public	14,804	25	22	33
- Other	3,870	6	28	295
- Rotation substitute	2,651	4	27	170
- Adult apprentice and student	618	1	29	693
- Workfare	533	1	28	67
- Flexjob, early retirement etc.	68	<1	26	2897

Notes: This table shows a breakdown of on-the-job training assigned within the first 12 months of unemployment. The table is based on activation categories recorded in the Job Plan data. Note that the statistics for on-the-job training deviates slightly from those reported in Table 1, since activities that overlap in time are not collapsed to one activity. Because a job seeker may be assigned to multiple training programs, the same job seeker can appear in multiple rows. Column (1) reports total assignments to a given category. Column (2) reports the share of assignments to a given activation category in percent of the total on-the-job training assignments. Column (3) reports the average timing (week number in the UI-spell) of the first assigned activity. Column (4) reports the total duration (days) of all assigned activities.

L.5.2 Course Titles

This section describes how we use a new data set on the *course titles* of training programs to classify classroom training courses along economically meaningful lines.[80]

The course titles constitute a major leap forward for ALMP research in Denmark. In particular, it allows us to classify the contents of "other activation (ØVO)", a large residual that has been a black box for researchers and policymakers in Denmark.[81,82]

Panel A of Table L.4 shows the breakdown of classroom training by "activation categories", the available data on ALMPs prior to this study. "Other activation (ØVO)" constitutes about two-thirds of all classroom training according to this categorization. The remaining one-third

[80] Our data on course titles was collected in the Fall of 2023 based on a special request from the "Expert Group for the Employment Efforts of the Future" to enhance our study (Danish Ministry of Employment, 2024). We thank the chairman of the expert group, Professor Claus Thustrup Kreiner, for facilitating this data request.

[81] The Danish Agency for Labor Market and Recruitment (STAR) describes "Other Activation (ØVO)" as a *"residual category for various different programs. The category includes education and courses, including language training, and shorter counseling programs not covered by the definition of ordinary education."* (STAR, 2022, p. 48).

[82] The former Danish Minister of Employment, Peter Hummelgaard, has problematized that *"[we] spend around 4.5 billion DKK a year on a course group titled "Other Activation (ØVO)", which includes a very wide range of expenditures. For most of the contents, we have no evidence that it actually brings workers closer to jobs or education."* (Danish Broadcasting Corporation, 2022).

are categorized skills courses, such as vocational training and ordinary education. As the table shows, vocational non-degree courses constitute around 79% of all categorized skills courses. These are shorter courses (averaging 32 days) that target specific jobs (e.g., a truck driver's license or a personal computer certificate) and take place at dedicated facilities. Ordinary education (e.g., primary education and post-secondary education) and degree programs constitute a small fraction of classroom training, but have relatively long durations.

In Panel B of Table L.4, we use the course titles to classify "other activation (ØVO)" into three subcategories of classroom training: "job search" courses, "skills" courses, and "wrap-around" services. We observe more than 9,000 unique course titles for job seekers and are able to merge a course title to 97% of all "Other Activation" courses assigned to job seekers in our sample.

Table L.5 shows the common keywords of each classroom category. First, we classify "job search courses" as all courses within "other activation (ØVO)" that include the (Danish) word for "search" ("søg") or "cv" ("cv") in the title.[83] Second, we define "skills courses (uncategorized)" as all courses within "other activation (ØVO)" that either has a skill-related word in the title[84], e.g. "education" ("udd"), or an entire title indicating skill content[85], e.g. "trailer license". Finally, we classify all remaining courses within "other activation (ØVO)" – i.e. courses that are neither "job search" nor "skills" courses – as "wrap-around courses". Wrap-around courses may involve direct matching of unemployed job seekers and firms, individual career counseling and coaching, e.g. on how to switch industries, or even "softer measures" like rehabilitation activities. Table L.5 shows that wrap-around courses frequently have words like "coach" (19.1%), "individual" (8.9%), "counseling" (6.4%), or "job counselor" (4.5%) in the title.

[83] We do not distinguish between lower and upper case letters. Further, we make sure not to include courses if "cv" is part of the word "cvr" (the identifier for firms), or where "søg" is part of other words, e.g. "forsøg" (attempt), "besøg" (visit), or "søgemaskineoptimering" (search engine optimization).

[84] See all competence-related words in Panel B of Appendix Table L.5).

[85] This is based on manual classification of titles, done by the Danish Agency for Labor Market and Recruitment.

Table L.4: Data Sources on Classroom Training

	(1) Assignments (count)	(2) Share (%)	(3) Timing (week no.)	(4) Duration (days)
Panel A: Data on Activation Categories				
Classroom training	**103,124**	**100**	**18**	**55**
Other Activation (ØVO)	71,056	69	18	47
Skills courses (categorized)	32,068	31	19	55
- Vocational training	28,181	88	19	45
- Non-Degree courses	25,483	79	19	33
- Degree courses	2,698	8	22	130
- Ordinary education	3,887	12	20	102
- Primary education	1,810	6	21	77
- Post secondary education	1,153	4	22	99
- Language courses etc.	924	3	17	149
Panel B: Data on Course Titles				
Other activation (ØVO)	**71,203**	**100**	**19**	**47**
- Job search courses	11,562	16	18	19
- Skills courses	17,322	24	20	42
- Wrap-around courses	42,319	59	20	51

Notes: This table shows a breakdown of classroom training assigned within the first 12 months of unemployment, based on two data sources. Panel A is based on activation categories recorded in the Job Plan data. Note that the statistics for classroom training deviates slightly from those reported in Table 1, since activities that overlap in time are not collapsed to one activity. Panel B is based on data on course titles. Due to the different data sources, the statistics for "Other activation" differs slightly in Panel A and B. Because a job seeker may be assigned to multiple training programs, the same job seeker can appear in multiple rows. Column (1) reports total assignments to a given category. Column (2) reports the share of assignments to a given activation category in percent of the total classroom training assignments. Column (3) reports the average timing (week number in the UI-spell) of the first assigned activity. Column (4) reports the total duration (days) of all assigned activities.

While "job search" courses are easy to distinguish from other classroom courses, the dividing lines between "skills" and "wrap-around" courses are less clear. For that reason, we combine "skills" and "wrap-around" courses into one category in our IV analysis, which also helps support the statistical power of our analysis.

Table L.5: "Other Activation (ØVO)" Courses Classified by Keywords in Course Titles

English translation of...	Danish word in course title	"Other activation" courses	Disaggregation of "Other activation (ØVO)": Job search courses	Skills courses	Wrap-around courses
(1)	(2)	(3)	(4)	(5)	(6)
Panel A: "Job search" words					
search	soeg	15.7	94.6	.	.
cv	cv	1.6	9.4	.	.
Panel B: "Skills" words					
educ(ation)	udd(annelse)	7.1	3.2	26.9	.
future	fremtid	2.2	0.1	9.1	.
industry	branche	1.0	3.3	1.9	.
upskill	opkval	1.5	0.2	6.1	.
competence	kompetence	1.6	0.1	6.7	.
_[a]	rkv[a]	0.9	.	3.8	.
dyslexic	ordblind	0.2	.	0.8	.
language[b]	sprog[b]	1.1	.	4.7	.
read, write, mathematics	laese, regne, matematik	0.1	.	0.6	.
it	it	0.6	.	2.3	.
Panel C: Other words					
clarification	afklar(ing)	5.5	2.7	11.0	4.1
counsel(ling), advis(ing), spar(ring)	vejled(ning), raadgiv(ning), spar(ring)	7.3	4.6	11.3	6.4
individual	individuel	5.7	1.2	0.9	8.9
coach	coach	11.7	0.1	1.7	19.1
job counselor	jobformidler	2.7	.	.	4.5
job service	jobservice	1.3	.	.	2.2
job club	jobklub	4.4	2.9	.	6.6
job cafe	jobcafe	0.9	.	1.4	0.9
job hunt	jobjagt	0.6	.	.	1.0
workshop	workshop	1.6	1.1	.	2.4
six weeks[c]	seks uger[c]	1.3	0.2	0.6	1.9
outplacement	outplacement	1.0	2.0	.	1.1
firm, internship, wage subsidy	virk(somhed), praktik, løntilskud	2.8	0.5	0.8	4.2
Panel D: Rehabilitation-related words					
ill[d], handicap	syg, handikap [d]	1.9	.	1.4	2.6
recovery[e]	traening[e]	1.6	.	0.9	2.3
Panel A words		16.6	100.0	.	.
Panel B words		14.9	3.6	59.1	.
Panel C words		38.1	14.9	19.4	52.4
Panel D words		3.3	.	2.3	4.6
Panels A+B+C+D words		64.8	100.0	62.0	56.0

Note: The table shows the share of "Other activation (ØVO)" courses with specific words in the title. The first two columns show the English translation (column (1)) of the Danish word used in the title (column (2)). Note that brackets indicate what the abbreviation is short for. Column (3) reports the share of all "Other Activation" courses with the word in the title. All "Other activation" courses are disaggregated into three categories: job search courses, skills courses, and wrap-around courses. Columns (4)-(6) report the share of courses in a category that have a specific word in the title. Due to confidentiality rules, "-" indicates that less than five job seekers were assigned to the courses (in the specific category). [a] "rkv" is an abbreviation for "realkompetencevurdering", which translates to "real competence assessment". [b] "sprog" (language) includes specific languages, e.g."dansk" (Danish), or "engelsk" (English). [c] "seks uger" (six weeks) is the short name of a six weeks long public training program ("seks ugers selvvalgt/jobrettet uddannelse"). [d] "syg" (ill) includes specific illnessess, e.g. stress or depression, as well as public benefits that ill job seekers may receive. [e] "traening" (recovery) includes different forms, e.g. "motion" (work out) or mindfulness.

L.5.3 Validation of PLAN Data

In this section, we validate the PLAN register against existing register data. In summary, we find that PLAN aligns well with existing registers on ALMP and education programs.

PLAN vs. DREAM The DREAM register records individual *participation* in training programs during unemployment. Because the activation measures in DREAM have been based on PLAN data since 2015, we would expect a high correspondence between activation assignments in PLAN data and activation participation in DREAM.[86]

Table L.6 shows the correspondence between training assignments in PLAN data and training 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 an activation category in PLAN data is accompanied by participation in a similar activation category in DREAM. For example, 87% of the job seekers who were assigned to "(1) Other activation" end up participating in the same category in DREAM.[87]

Table L.6: Activation in PLAN and DREAM

	Share participating in DREAM (percent)				
Assignment in PLAN	Other activation	Skills courses (categorized)	Internships	Wage subsidies	6 weeks
	(1)	(2)	(3)	(4)	(5)
(1) Other activation	**87**	10	25	7	15
(2) Skills courses (categorized)	31	**75**	24	7	**30**
(3) Internships	35	10	**93**	18	14
(4) Wage subsidies	34	9	60	**93**	13
(5) Other	30	12	40	9	10

Notes: This table shows how assignments to training in the PLAN data correspond with participation in training in the DREAM register. 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 according to PLAN. 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).

[86]As an exception, "6 weeks of own choice" in DREAM is based on a different data source (TASS), and when determining what an individual did in a given week, "6 weeks of own choice" is prioritized above other activation measures. Hence, the individual can be in "skills courses (categorized)" according to PLAN data but in "6 weeks of own choice" according to DREAM.

[87]Notably, 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 (5) "6 weeks of own choice," which dominates the other categories. That is, some of the job seekers who are assigned to "(1) Other activation" may be classified in DREAM as participating in "(5) 6 weeks of own choice" (see footnote 86).

PLAN vs. UDDA The *Education Register* (UDDA) contains information about enrollment in *degree courses*. Every year on September 30, Statistics Denmark takes stock of the current enrollees. 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. Notably, enrollments in UDDA likely represent a *lower bound* of the share of degree courses in PLAN data: First, some job seekers 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, due to the nature of the UDDA register (the stock as of September 30), we cannot be sure 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 L.7 reports the share of assignees to a specific classroom training program who also end up enrolling in an ordinary education in UDDA. Column (4) shows 4% of job seekers assigned to any classroom training program enroll in a degree course within the first 12 months from the UI spell start. Column (5) shows this share increases to 5% if we consider enrollments within the first 15 months from the UI spell start. In Columns (6)-(7), the enrollment rates are weighted by the length of the assigned classroom training programs. Column (6) suggests degree courses can account for 9% of the days spent in classroom training. Reassuringly, the enrollment rates in UDDA are substantially higher for job seekers assigned to vocational degrees or post-secondary education programs in PLAN.

Table L.7: Assignment to Classroom Training and Enrollment in UDDA

Assignment in PLAN	(1) Total assignees	(2) Share (%)	(3) >1 program (%)	(4) Enrollment in UDDA (%) month 0-11	(5) Enrollment in UDDA (%) month 0-14	(6) Length-weighted enrollment (%) month 0-11	(7) Length-weighted enrollment (%) month 0-14
Classroom training	**56,834**	**100**	**13**	**4**	**5**	**9**	**10**
Other activation (ØVO)	47077	39.9	17.4	3	5	4	5
Skills courses (categorized)	35458	30.1	40.8	7	8	11	13
– Vocational training	14871	12.6	39.2	6	8	10	12
– Non-degree courses	13019	11	38	3	4	4	5
– Degrees	1852	1.6	47.4	30	33	54	58
– Ordinary education	2858	2.4	49	7	9	17	19
– Primary education	1391	1.2	51	6	8	11	13
– Post-secondary education	857	0.7	44	11	12	36	37
– Language courses, etc.	610	0.5	51.5	4	6	5	7

Notes: The table reports the share of assignees to a specific classroom training program who enroll in an ordinary education in the UDDA register. 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 the 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.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 the 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.

PLAN vs. VEUV The *Course Participant Register* (VEUV) contains information about enrollment in *non-degree courses*. These are mostly publicly co-funded adult and continuing training courses that lead to formal qualifications. 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) enroll in a non-degree course within the first 12 (15) months from the start of their UI spell.[88] Again, because VEUV shows *participation*, these enrollment rates likely constitute lower bounds on the share of non-degree courses in PLAN. Furthermore, VEUV does not cover all non-degree courses in Denmark.

Table L.8 summarizes the correspondence between classroom training assignments in PLAN data and enrollments in non-degree courses in VEUV. Column (4) shows 41% of all classroom training assignees enroll in a non-degree course during the first 12 months after the UI spell starts. Column (5) shows this share increases to 43% if we consider enrollment in courses that start within the first 15 months after the UI spell starts. This finding suggests that non-degree courses constitute a relatively large share of classroom training. In Columns (6)-(7), the enrollment rates are weighted by the length of the assigned training programs. Reassuringly,

[88]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.

the enrollment rates in VEUV are substantially higher for job seekers assigned to non-degree vocational training as well as language courses.

Table L.8: Assignment to Classroom Training and Enrollment in VEUV

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total	Share	>1 program	Enrollment in VEUV (%)		Length-weighted enrollment (%)	
Assignment in PLAN	assignees	(%)	(%)	month 0-11	month 0-14	month 0-11	month 0-14
Classroom training	**56,834**	**100**	**13**	**41**	**43**	**44**	**46**
Other activation (ØVO)	47077	39.9	17.4	31	34	31	34
Skills courses (categorized)	35458	30.1	40.8	81	82	81	82
– Vocational training	14871	12.6	39.2	82	83	82	83
– Non-degree courses	13019	11	38	86	87	88	89
– Degrees	1852	1.6	47.4	54	56	41	43
– Ordinary education	2858	2.4	49	74	75	76	77
– Primary education	1391	1.2	51	76	78	80	82
– Post-secondary education	857	0.7	44	65	66	59	59
– Language courses, etc.	610	0.5	51.5	81	82	89	89

Notes: The table reports the share of assignees to a specific classroom training program who enroll in a training course in the VEUV register. 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.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 the 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 L.9 lists the ten most popular non-degree courses that classroom training assignees enroll 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).[89]

Table L.9: Popular Courses

Title	Enrollments
1. Use of spreadsheets for simple calculations	2,141
2. Forklift certificate course B, 7 days	1,707
3. Assessment of basic skills	1,503
4. Use of presentation programs	1,454
5. Entry and formatting of short texts	1,442
6. Placement of income statement and balance sheet accounts	1,265
7. Daily recording in a financial management program	1,240
8. Use of PC at the workplace	1,225
9. Internet information searching for job use	1,207
10. Voucher processing with subsequent cash report	1,155

Notes: This 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.

[89] With one exception, the same courses are in the top 10 if we focus on assignees to "Other Activation (ØVO)" or "Vocational non-degree courses".

L.6 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) .

L.7 Predetermined job seeker characteristics

We collect a set of predetermined job seeker characteristics, which we use in our independence test (Table 3) and as controls in various regressions (e.g., Equations (2)-(3)). This set of controls is based on the "sufficient" set of controls suggested by Lechner and Wunsch (2013)[90]. In particular, our set of predetermined job seeker characteristics include:

1. Baseline Characteristics

- Age
- Gender
- Education level dummies (1-digit ISCED; 9 sections). We include seven dummies.
- Origin dummies ("immigrant" and "descendant")
- Marital status dummy
- Number of children
- UI-fund association dummies ("Danish Trade Union" and "Academics Association").

[90]Lechner and Wunsch (2013) investigates which variables are required as control variables for classical evaluation studies of ALMPs that rely on selection on observables, and conclude that *"basic socio-demographic information together with certain information on the unemployment spell, region and pre-treatment outcomes as well as detailed short-run labor market histories appear to be sufficient to remove most of the biases."*

2. *Timing of entry into unemployment/program*
   - Month of entry to unemployment dummies.
3. *Region dummies*
   - Job-center-unit-year (which essentially corresponds to municipality-year) fixed effects
4. *Short-term labor market history (up to 2 years before unemployment)*
   - Employment
     - Dummy for any employment in the previous 24 months
     - Dummy for being employed in month 6/24 prior to job loss
     - Months of employment accumulated over the previous 6/24 months
     - Number of unique employers over the previous 24 months
   - Unemployment
     - Dummy for any UI-benefits in the previous 24 months
     - Dummy for receiving UI in month 6/24 prior to job loss
     - Months receiving UI-benefits accumulated over the previous 6/24 months,
     - Number of UI-spells over the previous 24 months.
   - Other public benefits.[91]
     - Dummy for receiving any public benefits in the previous 24 months
     - Dummy for receiving "other public benefits" in month 6/24 prior to job loss
     - Months reeciving "other public benefits" accumulated over the last 6/24 months,
   - Earnings and working hours
     - Earnings in month 6/24 prior to job loss
     - Total earnings accumulated over the previous 6/24 months

[91] "Other public benefits" include any type of public benefits other than UI-benefits, for example, cash benefits, parental leave benefits, education subsidies etc.

- Working hours in month 6/24 prior to job loss
- Total working hours accumulated over the last 6/24 months

5. *Pre-treatment outcomes (up to four years before unemployment)*

- Employment, earnings, working hours, and UI-benefits 4 years before job loss
- Accumulated months of employment, earnings, working hours, UI-benefits over the previous 4 years before job loss.

6. *Characteristics of last employment*

- Dummies for typical occupation (1-digit ISCO08; 10 levels) prior to job loss. We include nine dummies.
- Dummies for last industry (NACE; 21 levels) in the previous 24 months. We include eight dummies (representing the largest industries in our sample).
- Dummy for being at high risk of offshoring (see Appendix L.8). This dummy is included in the independence test, but not generally as a control in regressions.

L.8 Offshorability Index

We use the O*NET database (version 26.1, November 2021) to construct an occupation-based measure of exposure to offshorability.[92] 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.[93] 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 occupational descriptors have a mean 0 and a standard deviation 1. Hereafter, we use

[92] 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.

[93] Because importance is measured on a scale from 1-5 and level is measured on a scale from 1-7, we first do a normalization and hereafter aggregate the two.

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)

The offshorability index is obtained by i) summing over the task items listed above and ii) reversing the sign, such that the index is *decreasing* in interpersonal interactions and in proximity to work place location. Finally, iii) the index is standardized to be between 0 and 1. Importantly, it is the *sum* of interpersonal interactions and proximity to work place that determines how exposed an occupation is to offshoring.

Table L.10 lists the ten occupations (in the sample) with lowest and highest risk of offshoring according to the offshorability index. As expected, “contact centre salespersons”, “coding, proof-reading and related clerks” and “statisticians” are in the top of the offshorability index, while “fire fighters”, “dentists” and “nurses” are in the bottom of the index. This is in accordance with Autor and Dorn (2013), who note that “the five least offshorable occupations are respiratory therapists, dentists, fire fighters, elevator installers, and podiatrists, while the most offshorable occupations are clothing pressing machine operators, weighers, statisticians, operations researchers, and financial records processing clerks."

Table L.10: Occupations in Lowest and Highest Risk of Offshoring

Lowest risk			Highest risk		
Occupation	Offshoring index	No. of Job seekers	Occupation	Offshoring index	No. of Job seekers
Fire fighters	0.0	60	Financial analysts	0.77	7
Mining and quarrying labourers	0.0	7	Draughtspersons	0.77	321
Dentists	0.13	21	Handicraft workers in textile, leather etc.	0.79	21
Domestic housekeepers	0.14	63	Shoemakers and related workers	0.79	12
Cleaning and housekeeping supervisors in offices, hotels etc.	0.14	16	Mathematicians, actuaries and statisticians	0.81	16
Nursing professionals	0.17	1043	Economists	0.88	40
Printers	0.17	151	Contact centre salespersons	0.88	138
Nurses and midwifes	0.18	6	Advertising and marketing professionals	0.89	63
Forestry labourers	0.18	44	Legal professionals not elsewhere classified	0.91	120
Air conditioning and refrigeration mechanics	0.19	12	Coding, proof-reading and related clerks	1.0	7

Notes: This table shows the ten previous occupations of job seekers in the sample with the lowest and highest risk of offshoring.

For simplicity, we distinguish between job seekers from occupations with "high" and "low" risk of offshorability by defining all job seekers above the 75th percentile in the job seeker distribution to be high-risk. In our sample, this corresponds to defining all occupations with an offshorability index of 0.53 or above as being high risk.

Table L.11 lists the five *largest* high- and low-risk occupations in our sample, i.e. the occupations in which most low-risk and high-risk job seekers in our sample were employed. The table shows that most of the high-risk job seekers previously were employed as "cleaners and helpers" or "office clerks". The offshorability index for these two occupations are 0.64 and 0.68, respectively. These values are – by definition – well above the threshold of 0.53 for being high-risk, yet far from the maximum of 1.00.

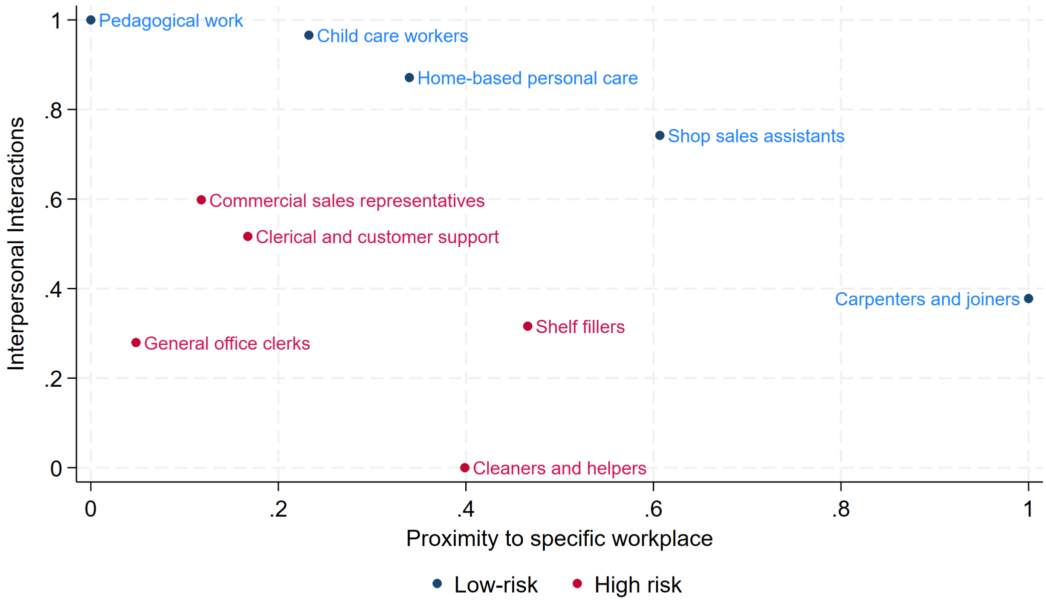
Table L.11: Largest Low- and High-offshorability Risk Occupations

Largest low risk			Largest high risk		
Occupation	Offshoring index	No. of Job seekers	Occupation	Offshoring index	No. of Job seekers
Shop sales assistants	0.36	10,063	Cleaners and helpers in offices, hotels etc.	0.64	5,517
Child care workers	0.42	6,646	General office clerks	0.68	3,985
Home-based personal care workers	0.42	5,020	Clarical and customer support workers	0.57	3,691
Pedagogical work	0.49	4,453	Shelf fillers	0.53	2,459
Carpenters and joiners	0.33	3,684	Commercial sales representatives	0.57	2,000

Notes: This table shows the five largest occupations among job seekers at low and high risk of offshoring (i.e. the occupations most frequently held by job seekers in each group). Job seekers in the analysis sample are divided into low-risk and high-risk according to the offshorability of their previous occupation (using the 75th percentile in the job seeker distribution).

Figure L.3 illustrates why "cleaners and helpers" is a high-risk occupation. Namely, although the occupation require some work place proximity, it requires very little interpersonal interactions compared to other occupations. The low degree of interpersonal interactions becomes the dominant factor that yields a high offshorability index value.

Figure L.3: Occupation Rankings in Terms of Offshorability Index Inputs



Notes: This figure illustrates how the five largest low and high-risk occupations rank in terms of the two inputs to the offshorability index: "interpersonal interactions" and "proximity to work place location". The offshorability index is obtained by i) summing over the two inputs (each of which represents the sum of underlying task items), ii) reversing the sign of the sum, and iii) standardizing such that the index ranges from 0 to 1. To ease exposition, the two inputs plotted on the x- and y-axis of this graph have been scaled such that they range from 0 to 1.

L.9 Imputation of Missing Occupations

We obtain information about occupations from two data sources: BFL and AKM.

L.9.1 Occupations from BFL

The BFL register covers the entire Danish population for 2008-2021 and is based on third-party reports to the Danish Tax Authorities. This register includes information about occupations at the *monthly* level.

We collapse BFL to UI-spell $\times$ month, keeping the "dominating occupation" in the month; the ISCO08-code where the individual had most working hours. Then, we impute missing occupational codes in a given *month*. First, following Hummels et al. (2014), we impute missing ISCO08-codes whenever possible. In particular, we use the workplace identifier (AJO_ARBNR_SENR) to identify employment spells of the individual. If the individual has a missing ISCO08 code within an employment spell, we extrapolate from months in which the ISCO08 was not missing.[94] Second, we use the ISCO08-codes of job seekers with similar (4-digit) education and (6-digit) industry to impute the remaining missing ISCO08-codes of job seekers. This procedure follows Statistics Denmark (2024).

Hereafter, we collapse the data to UI-spell $\times$ quarter, keeping the "dominating occupation" in the quarter; the ISCO08-code where the individual had most working hours (prioritizing non-missing ISCO08, and lower digit ISCO08-codes in case of a tie). We sum over total working hours and earnings at the dominating occupation, and we then define the individual as being employed in the quarter, if she had working hours > 0.

Finally, we use the quarterly occupations from the BFL is register to investigate the effects of assignment to training programs on occupational mobility, see section 8.2.1.

L.9.2 Occupations from AKM

The AKM register contains the *yearly* occupation of all employed individuals in Denmark from 1995-2017.[95] 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.

We impute missing occupational codes in a given year following Hummels et al. (2014). In particular, we merge workplace identifiers[96] onto the 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.[97]

Finally, we use the 4-digit isco codes to identify the *typical occupation* of job seekers over their career prior to job loss. That is, the occupation in which she had most experience over the career (prior to the start of the UI spell).[98] We then characterize job seekers as being in "high" or "low" risk of offshoring based on their typical occupation prior to job loss, see section L.8.

[94] We require non-missing ISCO08-codes to be similar within an employment spell.

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

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

[97] We require non-missing ISCO08-codes to be similar within an employer.

[98] In case of a tie, we use the lowest ISCO08 code.