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Abstract

In the German unemployment insurance system, Integration Agreements (IA) are mandatory contracts between the employment agency and the unemployed, jointly signed by the latter and the caseworker. IAs stipulate rights and obligations but are generally perceived as instruments to control search behavior. We designed and implemented a Randomized Controlled Trial involving thousands of newly unemployed workers, where we randomize the timing of the IA as well as the extent to which this timing is announced prior to the meeting. Randomization is at the individual level. We use administrative registers to observe outcomes. A theoretical analysis of anticipation of prior announcements provides suggestions to empirically detect this. The results show that IAs early in the spell have on average a small positive effect on entering employment within a year. When classifying individuals using an employability indicator, we find that this result is driven by individuals with adverse prospects. Among them, being assigned to an early IA increases the probability of re-employment within a year from 45% to 53%.

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1 Introduction

During the past decades, a view has emerged that Active Labor Market Programs (ALMP) are on average not very effective in bringing unemployed individuals back to work. Specifically, average reemployment effects of participation in training and workfare are often rather low, while the effectiveness of job search assistance and monitoring varies with the setting at hand and is typically low for groups with relatively bleak labor market prospects. Card et al. (2018) and Crépon and van den Berg (2016) provide recent overviews, but discouraging findings were already documented and summarized as early as Heckman et al. (1999). The evidence is of concern even in labor markets with favorable conditions, as unemployment may drive individuals out of the regular labor market and, indeed, may lead them to drift away from mainstream society. This has led to a search for novel ALMP policy instruments.

In this paper, we evaluate one such novel policy, called Mandatory Integration Agreements (IA). An IA is a written contract that stipulates rights and obligations of an unemployment insurance (UI) recipient. The signing of this contract takes place upon entry into UI, at the end of the first meeting of the UI recipient and his/her caseworker in the employment agency.[1] Both the UI recipient and the caseworker should sign the IA. Its contents is based on a template of textual building blocks that may slightly vary across occupation and family status but in practice the template is rather uniformly specified. The template reflects existing rules and laws (see also Schütz et al., 2011, and Boockmann et al., 2013, for descriptions of the IA; see also below).

Since the IA does not impose constraints that tighten the existing rules, one could argue that it is not being perceived as a monitoring device, at least as long as the unemployed individual is aware of the existing rules. Instead, the design and phrasing of the IA suggest a "nudge" character of the policy. Signing the IA, with its apparently symmetric design with rights and obligations and with its space for two signatures, may be viewed as a sort of ritual that may increase the commitment on both sides and foster a cooperative bond between the unemployed and the caseworker, effectively reducing the disutility of search as perceived by the unemployed. However, as we shall see, the content of the IA consists mostly of a list of obligations on the job search activities on the part of the unemployed worker, such as a minimum number of job applications per time unit. Even some of the stated rights of the unemployed can be seen as veiled threats to comply. Moreover, the caseworker may impose the IA unilaterally if the unemployed refuses to sign, and the resulting contract is legally binding. The punishment for non-compliance

[1]For ease of exposition we refer to "being a UI recipient" as "being unemployed". In Section 2 we discuss subtle differences.

with aspects of IA is one-sided and involves UI benefits reductions. With all this in mind, and given the self-reported assessments of the IA by surveyed workers and caseworkers (see Section 2), it is more accurate to view the IA as a refresher on obligations and as a reminder of monitoring and potential punishments. As such, the IA may have effects similar to monitoring, but the effects may be stronger because of the nudging that may make the IA a more comprehensive experience than the alternative of a simple confrontation with a list of obligations.

We employ a Randomized Controlled Trial (RCT) with randomization at the individual level to evaluate the IA. Specifically, we randomize two aspects of the policy: the timing of the IA and the advance notification of the IA. Randomization takes place upon entry into unemployment. The timing of the IA is randomized over possible elapsed times from entry into unemployment until the IA. One treatment arm involves the IA in the first month, one involves the IA at 3 months, and one at 6 months. In addition, we randomize whether those assigned to receive the IA at 3 months also receive an advance notification of the timing of the future IA at 3 months, to be received upon entry into unemployment. In total these constitute four possible treatment statuses, each with a 25% assignment probability. The RCT was carried out in 5 local labor market regions in Germany. These were chosen for reasons of representativeness but also because they are large, because no ALMP pilots or other evaluations were held there, and no reorganizations took place in the local employment agencies at the time. It was promised to the agencies that their performance ratings would not be affected by the RCT.

We use a number of data sources. First, we observe the output of the randomization tool. Secondly, population register data on UI recipients provide daily observations on outcomes, ALMP participation (including IA), meetings, covariates, employment spells, and past labor market outcomes. Third, we held a survey of caseworkers working in the agencies that participate in the RCT, one month before the RCT began. Fourth, we carried out a survey of UI recipients around two months after entry into UI. The non-response in the UI recipients' survey was sizeable. Also, most of the caseworkers did not allow merging of their own responses to records of their clients. For these reasons the survey data are of limited use. We merely use them to informally gauge workers' and caseworkers' perceptions of the IA.

Our RCT is the first evaluation of the IA policy. Also, it is the first large-scale RCT of ALMP in Germany with randomization at the individual level. Note that the combination of monitoring with nudging makes our evaluation potentially relevant for other policies that combine these components, such as devices to avoid tax avoidance.[2] In addition, some

[2]From a sociological-institutional perspective, IAs can be seen as an example of new public manage-

other OECD countries have recently implemented what could be called weak versions of the IA policy (Knotz, 2018, and Immervoll and Knotz, 2018), usually without the formal contract-signing ceremony and without threats of enforcement. In our view, an evaluation of the full-blown IA in Germany, with its strong legalistic tradition and adherence to the law, provides an interesting benchmark.

The comparison of those who are notified about a future IA at three months to those who are not is an innovative feature of our study design. This feature connects our paper to the literature on anticipation of future treatments (see e.g. Black et al., 2003, and van den Berg et al., 2009).[3] Using a search-theoretical framework, we show that the two treatment arms lead to an observationally distinct difference in the re-employment rate around the three-month threshold. At first sight it may seem that inference on the latter is hampered by the challenge that randomization is lost when conditioning on survival until close to 3 months. However, in the paper we develop a novel method to detect qualitative features of the re-employment rate that are informative on the presence of anticipation of the treatment at 3 months and that are preserved if randomization is lost. Clearly, this has wider relevance for the evaluation of anticipatory effects of future events.

To investigate heterogeneity of effects we divide the population of unemployed into two groups based on their predicted median unemployment duration until re-employment. Predictions are based on an inflow sample into unemployment from the year before our experiment, conditioning on individual labor market histories and characteristics. We show that local labor market conditions in these years are stable. We split the sample into individuals with a high (above 6 months) and a low (below 6 months) predicted median duration and perform sensitivity analyses with respect to this threshold value. We do not use in-sample observations to quantify the prediction model in order to avoid overfitting and the related risk of biased treatment effects (Abadie et al. 2018).

Interestingly, our findings already led to a policy change in the use of IAs by the German Federal Employment Agency. Specifically, by now, individuals who are regarded

ment strategies or new public contratualism with reconstructed citizens – in our case job seekers – as customers (O'Flynn, 2007). In this view, contracts that define requirements, monitoring, and incentives constitute the legitimate relationship between the state as the principal and the job seeker as the agent.

[3]Effects of advance announcements and notifications of future treatments are hard to identify because they are often not observed and they may obliterate the very treatment they announce, if they cause an exit from the state that is the eligibility state for the treatment. Non-experimental studies have relied on policy discontinuities (Blundell et al., 2004, De Giorgi, 2005, van den Berg et al., 2020) or on self-reported assessments of the likelihood of a treatment in the near future under unconfoundedness (van den Berg et al., 2009) or on register data with observed advance announcements in a timing-of-events model setting (Lalive et al., 2005, Crépon et al, 2018). RCTs are uniquely equipped to study anticipation effects because advance announcements are predetermined by the study design. Büttner (2008) applies this in an RCT to estimate effects of the announcement of participation in a future job search assistance program. However, his sample sizes are in the low 100s and he resorts to propensity score methods to deal with implementation issues.

as having favorable labor market prospects are not obliged anymore to undergo an IA during the first 3 months of unemployment.

The outline of the paper is as follows. Section 2 describes the German UI benefit system and IAs. Sections 3 and 4 discuss the setup of the experiment and the data, respectively. Methodological considerations and novel methodological contributions are in Section 5. The empirical results are presented in Section 6. Section 7 concludes.

2 Institutional background

2.1 Unemployment insurance benefits

The German unemployment compensation system has two pillars. The first is unemployment insurance (UI). As a norm, upon inflow into unemployment, UI eligibility requires that individuals have been working and paying social security contributions for at least 12 months within the period of 24 months immediately prior to unemployment (30 months since 2020). UI benefit recipients have to be registered as unemployed at the Federal Employment Agency (FEA). The UI entitlement duration depends on the duration of the prior employment period and the age of the recipient. The highest possible entitlement duration for individuals below 50 years is 12 months. This increases for older individuals, up to 24 months for those aged above 58 if they were employed for at least 48 months in the 5 years prior to unemployment. The replacement ratio is about 67% for individuals with dependent children and about 60% for those without, with a benefits level cap that is binding for only a small percentage of newly unemployed.

After expiration of UI, unemployment compensation is reduced to unemployment assistance or “welfare”. This is the second pillar of the system. Welfare is tax-financed and means-tested, and the level depends on household composition but not on former earnings. In 2012 it equaled around 345 Euro per month with supplementary accommodation costs as well as support in case of specific needs. Recipients have to register and receive placement services in job centers that are partly administered by the FEA and partly by municipalities. In the paper we restrict attention to UI benefit recipients.

2.2 Integration agreements

IAs were introduced as a policy in Germany in 2002.[4] Appendix 2 provides an actual example of an IA for an unemployed physiotherapist, with a slightly abridged English translation. Most of its contents is uniform across all IAs but a few features may vary

[4]The law covering the policy is written in the Social Code II §15 and the Social Code III §37.

across occupations. The latter applies in particular to the geographical range of the job search (here: nationwide), affecting the minimum number of applications per month, the maximum time allowed for submitting a list of qualifications to succesfully exert one's occupation (typically one week), and the time until the next meeting (here: two months). As already discussed in Section 1, most of the IA text is about UI recipients' obligations, and even some of the text on the recipients' rights can be interpreted as a reminder of obligations or as a veiled threat in case of noncompliance (e.g. that the agency promises make a phone call if it identifies an appropriate vacancy and in some cases may immediately send an actual job offer).

As noted in Section 1, the IA is signed at the end of the first meeting of the UI recipient and his/her caseworker. Before that, the meeting covers formalities such as entering of information about the client into the computer system of the FEA, and a discussion of plans for job search and future participation in ALMP programs. This includes the information on occupation, qualifications and household status that may affect the few open details of the IA to be signed. According to our survey among caseworkers (see Subsection 2.3), the first meeting usually takes about 50 minutes, and of these, about 15 minutes are used for the IA. Regarding the timing of the first meeting we should point out that individuals are required to register as a job seeker three months before unemployment entry or – if they do not know about this three months in advance – as soon as they receive a dismissal note. As a result, the first meeting with a caseworker can take place before the actual unemployment entry as well. (In our RCT, however, all caseworkers were instructed to conclude the first IA only after the actual unemployment entry; see Section 3.)

The assignment of caseworkers to clients is quasi-random and is typically governed by the first letter of the last name of the client, by first or last digits of various codes that the individual bears, and by who is the first available caseworker when the client enters the agency for the first meeting. As a rule, the client keeps the same caseworker throughout his/her UI spell. There is no space for discretionary behavior by the caseworker regarding the contents of the IA. However, it is possible that the individual impact of the IA depends on the caseworker's behavior. We return to this in the results section. After the first meeting, the caseworker only updates the IA if strictly necessary, e.g. if the unemployed hands in a disability note. Apart from this, the IA is typically updated after at least 6 months (for those aged 25+), to take changing circumstances and completed ALMP participation into account.[5]

[5]Before the IA is signed, caseworkers may profile their clients according to their assessment of the support they need. We do not observe this in our data but our extended analysis allows effects to differ

If the UI recipient is found not to comply with the obligations and guidelines on search behavior and ALMP participation, whether they are mentioned in detail in the IA or not, then (s)he may receive a punishment in the form of a benefits reduction (i.e., a sanction). These are relatively severe, typically involving a full benefits withdrawal for at least one week, where the length of the period depends on the type of violation. A second detected violation may lead to a complete UI benefits withdrawal for more weeks.

2.3 Self-reported perceptions of IA among caseworkers

To gauge caseworkers' perceptions and assessments of the IA for UI recipients, we held a short survey among caseworkers in the agencies that participate in the RCT in June 2012, that is, one month before the RCT began.[6] Unit non-response was 28% and there was also substantial item non-response, resulting in a total of 159 respondents who answered each question used in this subsection.

The survey was set up as a list of statements for each of which the caseworker could indicate his or her agreement. We observe that 16% of the respondents agrees mostly or fully with the statement that IAs are supportive for the job seekers in their search for work. Next, 19% agree mostly or fully with the statement that IAs helps the job seekers to claim their rights. Conversely, 74% state that they use the IA at least to some extent to control the effort by the job seeker (i.e. to monitor the job seeker). These numbers confirm the descriptions of the nature of the IA in Section 1 and Subsection 2.2. Regarding the contents of the IA agreement, the caseworkers' responses support our above descriptions as well.

The survey also reveals that caseworkers envisage IA effect heterogeneity. On average, they believe that IAs do not increase the re-employment probability of individuals who have a good connection to the labor market and who can be expected to find work on their own within half a year. They tend to view IAs as more useful for individuals who in their view need to be activated and/or receive job search assistance or training. Of course it is not clear whether the views on the usefulness of such support precede the views on the usefulness of an IA. But it appears that the usefulness of an IA is regarded to be higher if the individual does not have excellent prospects.

We also conducted a short survey among a sample of participants in the experiment. Again, the unit and item non-response was sizeable. More importantly, unit non-response was not balanced across treatment groups,[7] which is why we mostly do not use the survey

by an index of individual characteristics and labor market history.

[6]Preliminary findings from this survey were reported in German in van den Berg et al. (2014); descriptives available upon request.

[7]This follows because replication of the estimation of treatment effects with register data but using

responses. The survey was again set up as a list of statements for each of which the respondent could indicate his or her agreement. The survey was held around 1.5 months after unemployment entry and the questions relating to the IA were only put forward to respondents who (and were assigned to have) received the IA in the first month. In the resulting small subsample of 127 individuals, less than half (44%) agrees mostly or fully that IAs are supportive in their search for work. However, a much larger fraction (80%) feels that the IA serves as a reminder of their obligations during their search for work. And 78% agrees with the statement that the IA is a tool with which the caseworker can control the individual (i.e. to monitor the job seeker). Here it should be kept in mind that the respondents are informed that the survey is carried out by the IAB (which is the main research and data institute of the FEA) among employment agency clients. Although they are also informed that responses are strictly confidential, some may have given answers that they deem to be desired by the FEA, so that the actual assessment of IAs may be even more tilted towards monitoring and away from counseling.

3 Experimental design

3.1 Treatment arms

We randomize two aspects of the policy: the timing of the IA and the advance notification of the IA. Randomization takes place at the individual level upon entry into unemployment. We allow for four treatment arms. In treatment arm A, the IA is supposed to be signed in the first month of unemployment. In treatment arms B and C, this is supposed to occur three months after entry (if the individual is still unemployed), and in treatment arm D the signing is supposed to take place for the first time six months after entry (again conditional on unemployment). Treatment arms C and D do not include an advance notification of the future IA. In contrast, treatment arm B involves the receipt of a written announcement during their first meeting with the caseworker, informing the individual about the requirement to sign an IA in the third month of unemployment. This includes a detailed description of the typical content of IAs. In addition to that, it states that non-compliance with the content of the IA may lead to a sanction in form of benefits cuts (see Appendix 3 for the exact wording of the announcement).

Table 1 summarizes the treatment arms. Each of the four possible treatment statuses in the RCT is given a 25% assignment probability. The Social Code legal framework does not allow for an RCT with a treatment arm in which the individual is never confronted

only the sample respondents leads to results that differ significantly from those in Section 6 below.

with an IA. Similarly, it was not possible to randomize parts of the contents of the IA, so we could not introduce random variation e.g. in the number applications per week or in the highest commuting time deemed acceptable for offers provided to the individual. Note, however, that this would have increased the number of treatment arms considerably, which would be impractical and would lead to underpowered inference at given sample sizes.

3.2 Implementation of the RCT

We set up the experiment in five regional employment agencies out of a total of around 180 nation-wide.[8] The agencies were selected on the following criteria. Firstly, during the time of the experiment (2012-2013), they hosted no other pilot projects, for example for the evaluation of other active labor market policies. Secondly, during this time, they did not face any other organizational changes, restructurings or mergers. Thirdly, the regions they served should not be too small in terms of population, to safeguard the sample size. In June 2012, around 2.8% of all unemployed individuals in Germany were registered at one of the five agencies. Fourthly, they had to be dispersed across East and West Germany and across rural and urban regions, jointly creating some representativeness. The unemployment rate averaged across the five agencies does not differ from the national average (6.7% versus 6.8%; both measured in June 2012). However, unemployment rates range from 2.5% in a Bavarian agency to 12.0% in an East German agency in the RCT. The agencies were informed by the FEA that they were selected to participate in the RCT. To prevent that the agencies' performance ratings would be affected by the work for the RCT or by the outcomes of clients involved in the RCT, it was communicated that RCT participation would not affect their performance goals.

At each of the five agencies, two representatives of the FEA and of the research team presented the RCT to the agency head. FEA experts conducted instruction lessons with team leaders of caseworker teams in participating agencies before the project started (teams usually consist of 5 up to 15 caseworkers). The caseworker team leaders, in turn, instructed single caseworkers. The research team designed instruction material consisting of a presentation, a FAQ list and a two-sided plastic slide summarizing the experimental design which was meant to be placed on each caseworker's desk throughout the experiment. The presentation highlighted the importance of the research question and why it could only be answered by means of an RCT. The material included verbal and graphical

[8]The RCT design was approved after an internal review by the IAB Project Approval board and after a critical review by the legal department of the FEA, without the imposition of any additional constraints on the proposed design.

descriptions of the treatment arms. The target population was described and it was emphasized that other elements of the placement process were not supposed to differ across treatment groups, and in particular that all groups should have the same degree of access to ALMP instruments. Follow-up information was made available by email and telephone.

The target population of the experiment is the full set of new entries into unemployment in one of the five employment agencies between July 2012 and January 2013. Individuals who were eligible for UI were supposed to participate in the trial, where those aged below 25 or registered as unemployed at some instance in the quarter prior to the current unemployment spell were excluded. This is because those categories faced different institutional environments and/or placement processes. We also exclude females because parental leave is not observable in the data and cannot be identified as distinct from unemployment. Parental leave spells can take up to three years and are usually taken up by the mother of the child instead of the father.[9]

In the first meeting between the caseworker[10] and a newly unemployed individual in the target population, the latter was randomly assigned with equal 25% probabilities to one of the four treatment arms.[11] The randomization is triggered by the caseworker during the meeting. The caseworker had to open an app and enter the client's identification number, name and date of birth into a computer system. Both the app and the system were developed by the FEA for evaluation studies. The system generates a random number (not based on above characteristics) which then determines the assigned treatment status. In the RCT, the assigned status was immediately displayed in the caseworker app and the caseworker had to acknowledge it by entering it into the usual placement software program. This stores the time and the randomization outcome as well as anonymized identifiers of the client and the caseworker. Caseworkers were not able to manipulate the randomization, for example by re-running the randomization. Importantly, the unemployed individuals were *not* informed about the RCT.[12]

It is also important to note that the protocol specified that the content of the IAs can

[9]The sample size for women would be substantially smaller than the size of our sample of men, causing any analysis of the former to be under-powered.

[10]Recall that in general this might take place before entering unemployment. In our experiment, however, all caseworkers were instructed to deal with the IA and IA-related issues only after the actual unemployment entry.

[11]In an additional fifth group, the unemployed were assigned to be treated "as usual" with respect to the IA. This typically corresponds to an early IA during the first meeting with the caseworker. There were no clear instructions for the caseworkers for this group, so it is hard to interpret findings for this group, and, indeed, the outcomes for this group may be affected by the ongoing RCT. Therefore we exclude this group from the analysis.

[12]The default would be to obtain informed consent. This can be disposed of if it would plausibly induce selection and if there is no convincing prior evidence that a participant will be worse off because of participation. If consent is not required, informing participants about the experiment can be disposed of if the latter would plausibly induce changes in behavior and would thus invalidate the RCT.

not be influenced by the randomized treatment. However, it is possible that the content of the IAs that were signed in later months (typically 6 months or later after inflow) systematically differed from the content of earlier IAs. We regard such potential differences as part of the treatment. It is also possible that the treatment assignment influenced the frequency of subsequent meetings between the unemployed and the caseworker or that it influenced ALMP access. We will address this aspect below in more detail using data on this.

4 Data

4.1 Registers

The empirical analysis uses administrative data of the Institute for Employment Research (IAB) of the FEA.[13] These consist of individual records for the full labor force, notably from the so-called integrated employment history register (IEB). The IEB contains socio-demographic individual characteristics and detailed employment and unemployment histories including daily earnings, transfer payments and participation in ALMP programs, sanctions and meetings with caseworkers. The IEB does not contain information about working hours and self-employment but we observe self-employment subsidies and whether a job is full-time or part-time. The data also include a variable capturing the day at which an IA is signed, which is important to validate whether the caseworker follows the experimental protocol for treatment groups B, C and D.

The IEB records are merged at the individual level with the variables that are recorded by the computer system used for the randomization. In our analysis, we use the assigned treatments recorded by the latter system. Recall that this also provides anonymized identifiers that enable the linkage of unemployed sharing a caseworker.

The main outcome variable is the duration from the start of the unemployment spell to the beginning of the first subsequent employment spell. The start of the unemployment spell corresponds to the first day of UI receipt (or the first day of being registered as a job seeker without some parallel employment, if that day occurs before the randomization).[14]

[13]We use registers named IEB version V12.01.00 and ASU-EEI version V06.09.00-201604. These are social data with administrative origin which are processed and kept by IAB according to Social Code III. The data contain sensitive information and therefore are subject to the confidentiality regulations of the German Social Code (Book I, Section 35, Paragraph 1).

[14]For individuals who are not registered as unemployed or as job seeking on the day of randomization, we define the start of the unemployment spell to equal the day of randomization. Individuals who are still employed on the day of the randomization are excluded from the sample, as the entry into unemployment after randomization may be endogenous among them. We also exclude individuals who were unemployed for more than 6 weeks on the day of randomization because such a pattern is hard to reconcile with the guidelines on the timing of the first meeting and/or with the experimental protocol.

The duration outcome as defined above might include intermittent periods in which an individual is not registered as unemployed and does not receive any benefits from the FEA. For expositional convenience we nevertheless refer to this as part of the unemployment duration. We exclude one individual from the sample because randomization occurred on a day outside the experimental time window. We exclude 7 individuals who could not be unambiguously matched to administrative records. This leaves us with an estimation sample of 4,163 entrants into unemployment, with groups A, B, C and D containing 1061, 1013, 1068 and 1021 individuals, respectively. Descriptive statistics are in the next subsection.

Across the five regional employment agencies, 213 caseworkers participated in the experiment. Some of these may have worked part-time. On average, each caseworker dealt with 20 RCT participants, where the number per caseworker ranged from one to 76.[15] See Figure 6 in the Appendix for the distribution of RCT participants across caseworkers.

We finish this subsection by listing data sources that we do not have access to but that might have been useful to study. Firstly, we do not observe the content of the IAs at the individual level. Secondly, we do not observe whether the IA is unilaterally signed. Thirdly, we do not observe caseworker characteristics beyond an anonymous identifier. Fourthly, we do not observe this caseworker identifier for clients who do not participate in the RCT. These limitations are motivated by costs of digitization as well as by requirements to protect confidential information and privacy. Fifthly, caseworkers virtually never gave consent to merge their survey data records with administrative records of their clients. The caseworker survey data could not be merged with other data sources either.

4.2 Balancing tests and timing of the IA

Since caseworkers could not manipulate the randomization tool, we do not expect significant differences between the four treatment groups in their pre-randomization characteristics. To proceed, we perform a range of separate regressions in which individual pre-randomization characteristics are regressed on three binary indicators of the treatment statuses A, B and C (leaving out D as the reference status). Judged on the basis of joint F-tests for the three coefficients, almost each characteristic is well-balanced across the four experimental groups. Table 2 presents the distribution of selected characteris-

[15] This caseload refers to the participants in the experiment to the extent that they are used in the analysis. It is possible that the caseworkers concurrently dealt with job seekers who did not participate in our experiment, for example because they entered unemployment before or after the period of randomization or because they had a previous unemployment experience shortly before the current entry into unemployment or because they fell outside of the sampling criteria e.g. because of their gender. We cannot match such unemployed individuals to the 213 caseworkers in our experiment.

tics across the four groups and the corresponding p-values for the balancing tests. Table A.1 in the Appendix provides results for additional characteristics including labor market history indicators. Those results confirm that the randomization worked well. As a more encompassing way to examine the same issue, we estimate a multinomial logit model for the four treatment statuses as functions of the individual characteristics. This gives a p-value of 0.42 for the ensuing likelihood ratio test statistic of the null hypothesis of all coefficients of the characteristics being equal to zero, confirming randomized assigment.

In the RCT, the exact timing of the IA was not under our perfect control. In practice, the date at which the IA is signed depends on when meetings between caseworker and client are held, and the latter is subject to variation e.g. due to sickness absence and holidays. To assess this empirically, one may consider the estimation of Kaplan-Meier survival functions for the duration until the IA by the different treatment groups. Unfortunately, the interpretation of the estimates is problematic, as the durations until the IA are right-censored by exit to employment. One could assume independent right-censoring (conditionally on observed covariates) as an identifying assumption for the effect of the treatment status on the duration until IA, meaning that there are no unobserved confounders driving both the duration until IA and the duration until employment. However, this assumption is untenably strong, because if it were believed to be true then one could study the effect of the timing of the IA with non-experimental methods, defying the point of this study.

With this in mind, we merely provide some indicative statistics. In 25% of the cases where the IA takes place, the difference between intended and actual date exceeds 1 month. Figure 1 plots the Kaplan-Meier estimates. Clearly, they differ strongly across the treatment groups, which is of course to be expected. For example, after two months, less than 10% of group A is estimated to not have signed an IA yet. Further, the estimated survival functions for the duration until IA are virtually identical across the groups C and D in the first two months of unemployment. If this were not the case then that would cast serious doubt on the implementation of the experimental design. The estimated functions for B and C are virtually identical throughout, which may be tentatively interpreted as confirming that those who anticipate the IA at 3 months do not use this knowledge to manipulate the timing of the IA.

Perhaps more surprisingly, the estimated functions show some variation within treatment groups, reflecting IA scheduling deviations. By exploiting the caseworker identifier variable, we find that such deviations are more common for some caseworkers than for others. Since the timing of meetings is primarily determined by the caseworker, this suggests that scheduling deviations primarily originate from the caseworker's views or attitude. As a sensitivity analysis one may therefore drop all the clients of caseworkers with relatively many extreme deviations from the sample. Alternatively, one may estimate models allowing for interactions between the treatment status and an indicator of the caseworker's propensity to have scheduling deviations.

5 Methodological considerations

5.1 Outcomes

The empirical analysis of the RCT faces a number of challenges that are common in the case of survival outcomes. First, note that ideally one would like to know effects on conditional re-employment rates at various elapsed durations t, as such rates are more closely related to behavior at t than for instance survival probabilities at t. However, with treatments affecting re-employment before any $t > 0$, randomization is lost if we condition on survival at some $t > 0$, as the composition in terms of unobserved characteristics will systematically differ across treatment arms (see e.g. Abbring and van den Berg, 2005). Therefore the comparison of re-employment rates in different treatment groups at some elapsed duration $t > 0$ does not allow for meaningful causal inference if the treatments may affect re-employment differentially before t.

This has a number of implications in our setting. It is conceivable that in groups A and B the treatments lead to group-specific behavior from the onset, so that the hazard rates in groups A or B cannot be meaningfully compared to the hazard rates in any other group at any $t > 0$. In contrast, individual behavior should on average be identical across groups C and D until 3 months. Following insights from van den Berg et al. (2020), non-parametric causal inference on the difference between the re-employment rates in C and D is then possible for t exactly equal to 3 months. After 3 months, the treatment regimes differ between C and D, so that causal inference on re-employment rates is not possible anymore. Also, following van den Berg et al. (2020), the discontinuities in re-employment rates within group C at 3 months and within group D at 6 months enable identification of a causal effect of the IA on the re-employment rate at exactly those points in time, under the assumption that no other events take place at those points in time that lead to a discontinuity in the individual hazard rates. This approach does not allow for causal inference on re-employment rates at any other value of t. In practice, even these limited opportunities for causal inference on re-employment rates are not feasible, as they would require IA meetings in C and D to take place at exactly 3 months and 6 months sharp, respectively. The empirical variation around those dates precludes such inference.

Because of this, our primary outcomes of interest are the unconditional probabilities of leaving unemployment within certain durations t.

A second common challenge, by analogy to Ham and LaLonde (1996), is that inference on post-unemployment outcomes is hampered for the reason that those are only observed if exit to work occurs before the end of the observation window. Whether this condition is satisfied depends on the treatment status and on unobservables, so, again, randomization is lost. Because of this, we do not examine accepted wages as outcome variables. We do examine the total earnings obtained in t periods after inflow into unemployment. These earnings add UI benefits received to labor earnings in employment and are observed for every individual.

5.2 Anticipation of future IA date

The comparison between treatment arms B and C enables us to evaluate whether advance notification of the timing of an IA at 3 months affects outcomes. To understand the results we study a job search model of unemployed workers who are exposed to an event (IA) at a duration τ (3 months). As a starting point we assume that the event is unattractive from the point of view of the worker in the sense that it imposes constraints on his behavior, from τ onwards.

In the spirit of Mortensen (1986), consider an unemployed individual who searches sequentially for a job. Given a particular search effort s, job offers arrive according to the rate $\lambda \cdot s$. Offers are random drawings from a wage offer distribution $F(w)$. Every time an offer arrives the decision has to be made whether to accept it or to reject it and search further. Once a job is accepted, it will be held forever at the same wage. During unemployment, a flow of benefits b is received and a flow of search costs $c(s)$ has to be paid. The individual maximizes the expected present value of income over an infinite horizon. For convenience we take the model to be stationary apart from the event at τ. That is, $b, c(.), \lambda$ and F are assumed to be constant over time. Also, the model determinants are taken to satisfy the usual regularity assumptions.

Behavior at durations $t < \tau$ depends on how much is known about the IA. If the individual does not know about the treatment then his behavior up to τ can be captured by a reservation wage ϕ_0 and an optimal search effort s_0 that are constant over time. If the individual anticipates the event at τ then the model is genuinely nonstationary (van den Berg, 1990) and behavior up to τ can be captured by differential equations for the reservation wage $\phi(t)$ and optimal effort $s(t)$, derived from the following asset flow equation for the expected present value of income $R(t)$,[16]

$$\phi(t) = \rho R(t) = \max_{s(t)} \left[\frac{\phi'(t)}{\rho} + b - c(s(t)) + \frac{\lambda s(t)}{\rho} \int_{\phi(t)}^{\infty} (1 - F(w))dw \right] \tag{1}$$

where $R(t)$ decreases until $t = \tau$ and thus $\phi(t)$ decreases as well while $s(t)$ increases until τ. Compared to the setting with no knowledge about the future event, $\phi(t) < \phi_0$ and $s(t) > s_0$. In a nutshell, individuals who anticipate the event aim to avoid the reduced attractiveness of the search environment after τ by being less selective with respect to job offers and by searching harder, before τ.

In obvious notation, the re-employment (or hazard) rates up to τ in cases C and B can be expressed as,

$$\theta_0(t) = \lambda s_0(1 - F(\phi_0)), \qquad \theta(t) = \lambda s(t)(1 - F(\phi(t)))$$

respectively. Clearly, $\theta(t)$ increases until τ. This implies that the re-employment rate on the interval $(0, \tau)$ is larger in B than in C and that the difference increases as t increases. This is the first main finding of this subsection. The ranking of B and C extends to the unconditional re-employment probability for any interval $(0, t)$ with $t < \tau$.

Regarding treatment B, it is not difficult to show that the above equations imply that

$$\frac{\phi''(t)}{\phi'(t)} = \rho + \theta(t) \tag{2}$$

so $\phi'(t)$ and $\phi''(t)$ are both negative, implying that $\phi(t)$ decreases at an increasing pace until $t = \tau$. Likewise, $s(t)$ increases at an increasing pace until $t = \tau$. By integrating (2) over the interval (t, τ) we obtain that $\phi'(t)$ can be written as

$$\phi'(t) \;=\; [\phi'(\tau)] \;\cdot\; e^{-\rho(\tau - t)} \;\cdot\; \Pr(T > \tau | T > t) \tag{3}$$

where $\phi'(\tau)$ is the left-hand side derivative at τ. This equation provides insight into the determinants of the extent of anticipation of the event at τ at a fixed value of t. After all, if $\phi'(t)$ is much below zero then this means that the individual is strongly modifying his optimal strategy in response to the future event. Now consider the three terms on the right-hand side. The first term $\phi'(\tau)$ captures how severe the change in the search environment at τ is, so it is a measure of the relevance of the event.[17] For our

[16]This follows from van den Berg (1990), incorporating an optimally chosen search effort along the lines of van den Berg and van der Klaauw (2006).

[17]This can be seen most easily in the special case where search effort is fixed at say $s \equiv 1$ and the event at τ is an increase of the job offer arrival rate from say λ_L to λ_R whereas nothing else changes after that. Then, from equation (1), we have, coming from $t \uparrow \tau$, that $\rho R(\tau) = \phi'(\tau)/\rho + b - c(1) + \lambda_L \int_{\phi(\tau)}^{\infty} (1 - F(w))dw/\rho$ and, coming from $t \downarrow \tau$, that $\rho R(\tau) = b - c(1) + \lambda_R \int_{\phi(\tau)}^{\infty} (1 - F(w))dw/\rho$. This

purposes, the second and third term are more relevant as they capture anticipation of a given severity of the event at τ. The second term captures that the future event is more important at t if the discount rate is low. The third term captures that the future event is more important at t if the individual is unlikely to escape unemployment before τ. This term equals $\exp(-\int_t^\tau \theta(u)du)$ so it only depends on the re-employment rate. Of course this in itself depends on the path of ϕ. After all, equation (2) is not a recursive expression. As a first-order approximation, the second and third terms can be represented by $\exp(-(\rho + \theta(t))(\tau - t))$.

This suggests that, for a given adverse event at τ, we can expect a large difference in outcomes between treatment arms B and C if ρ is small and if re-employment rates $\theta(t)$ are small. In practice, re-employment rates are an order of magnitude larger than commonly assumed values of the discount rate (e.g., average re-employment rates are around 2 per year whereas a typical value of ρ is 0.05 per year). This means that the individual employability (or, similarly, the probability to become long-term unemployed) is the key candidate for the study of heterogeneous treatment effects when comparing B and C on $[0, \tau)$. This is the second main finding of this subsection and it is based on a novel approach to interpret nonstationary search models.

So far we have not modelled behavior after τ. At the individual level, behavior is equal for B and C (and can be represented by ϕ_1 and s_1 that are constant over time). Therefore, the magnitude of the change in behavior *at* τ does differ between B and C. With treatment arm B, the present value $R(t)$ is a continuous function at τ so the reservation wage does not change as time proceeds from just before τ (say, at $t = \tau^-$) to τ, so $\phi(\tau^-) = \phi_1$. With arm C, the event is unanticipated, so the perceived present value jumps downward at τ, and therefore the reservation wage jumps downward as well, from ϕ_0 to ϕ_1. The latter leads to an upward jump (i.e., a discontinuity) in the re-employment rate at τ.

To use this for a test we first need to address the fact that the re-employment rates are also affected by search effort. This in turn requires a more explicit discussion of the nature of the event at τ. In particular, the IA may be seen as imposing a minimum required search effort s^* which exceeds the value chosen in absence of the IA. In that case, the effort at τ will jump upward both in B and in C. However, we have seen that $s(t)$ exceeds s_0 at any $t < \tau$, so the upward jump in effort is smaller in B than in C. Taking this together with the results on the reservation wage at τ, this means that the upward jump in the re-employment rate for treatment arm C is larger than for treatment arm B. This is the third main finding of this subsection, and it leads to a test comparing

gives $\phi'(\tau) = (\lambda_L - \lambda_R) \cdot \int_{\phi(\tau)}^{\infty} (1 - F(w))dw$ which is the change in the arrival rate λ times a measure of its relevance in the expected present value.

the sizes of a discontinuity in the hazard rate at τ between groups B and C.

The first challenge for the implementation of this test idea is the issue discussed in Subsection 5.1. Dynamic selection due to unobserved heterogeneity may proceed at different speeds in groups B and C, precluding a clean comparison (quantitative causal inference) of hazard rates around τ. In an RCT, systematic unobserved characteristics at baseline are independent of the treatment status. It is not difficult to show that in that case, a discontinuity of the individual hazard rate at an elapsed duration τ is preserved under aggregation over unobserved heterogeneity. However, the ranking of the discontinuity sizes between groups B and C is not necessarily preserved as it depends on interactions between the treatment status and the unobserved characteristics in the individual hazard rates up to τ.

A second challenge is that, as discussed earlier, the timing of the IA is not homogeneous within treatment arms, so τ is dispersed within groups B and C. This complicates the inference based on hazard rates around τ. In particular, we do not observe the individual-specific τ if the individual leaves unemployment before τ. We therefore do not aim to identify discontinuities but rather examine the steepness of the slopes of the empirical hazard rates around 3 months, and we consider findings based on the shape of the hazard rates around τ as tentative evidence only.

We finish this subsection with some more general remarks. Firstly, as mentioned above, the IA event may include nudging elements leading to an increase of the job offer arrival rate and thereby an improvement of re-employment opportunities after τ. If individuals can acknowledge this benefit of nudging in advance then, before τ, this would make the future event less unattractive in the eyes of individuals in group B. This could mitigate the size of the differences between the effects of B and C before τ. Secondly, individuals in C may expect the IA event to occur at some rate η, in which case the IA has a so-called ex-ante effect. This also tends to mitigate the size of the differences between B and C. Thirdly, note that up to τ, the groups C and D behave identically on average, so for the above purposes D may be added to C on that time interval.

6 Results

6.1 Average effects

Figure 2 shows Kaplan-Meier estimates of the survival functions until exit to employment, that is, estimates of the probability of having found a job as a function of the time t since the start of the unemployment spell. Note that we do not censor observations if they leave registered unemployment without entering employment directly. Therefore, the estimated survival rate at a duration t simply equals the ratio of the number of individuals at risk (i.e. who have not found a job yet) divided by the size of the corresponding treatment group. We discuss standard errors of estimated effects in binary-outcome analyses below, so the discussion of the estimated functions is brief. The estimated functions for the four groups are virtually indistinguishable in first 120 days after the unemployment entry. This suggests that signing an IA very early has on average no short-term impact on the probability of getting a job. At higher durations (around the median of about 200 days) individuals assigned to group D have a lower probability of having entered employment. There seem to be no systematic differences between groups A, B and C.

Next, we estimate linear probability models. In what follows we take treatment arm D (not-previously announced IA at 6 months) to be the reference category. The outcome y_{it} is a binary indicator which is one iff an individual i moved to work before t, and $A_i = 1$ iff i is assigned to group A, etc.

$$y_{it} = \beta_0 + A_i\delta_A + B_i\delta_B + C_i\delta_C + \varepsilon_{it} \tag{4}$$

We also estimate versions including a vector x_i containing individual characteristics like age, nationality, education, last observed daily earnings and other labor market history indicators. Table 3 reports the latter results, for t equal to 90, 180, 270 and 365 days. Not surprisingly, the results without x_i are virtually identical to those in the table.

The coefficients for A, B and C are close to zero and insignificant at 90 days after entry into unemployment. At $t = 180$ and $t = 270$ the differences are not statistically significant either. The point estimates for effects at 270 days are around 2 to 3 percentage points for A, B and C as compared to D. At one year, the effect estimates range from 3 to 5 percentage points; these are statistically significant at the 5% level for A and C and at the 10% level for B. Thus, on average, being assigned to a late IA reduces the probability of re-employment within a year by about 4 percentage points, from 69% to 65%, and it commensurately increases the probability of long-term unemployment. On average it does not matter at any t whether the IA is signed immediately or after 3 months.

None of these results suggests that it matters much whether the IA at 3 months is announced in advance or not. To scrutinize this in more detail we use information in the data on the exit rate to work around t =90 for groups B and C. In line with the approach proposed in Subsection 5.2, we examine the steepness of the slopes of the empirical hazard rates around 3 months. Figure 3 displays kernel hazard estimates for B and C for durations up to 6 months (the bandwidth is 14 days).

This indicates that the hazard rate for B increases less steeply than for C, around 90 days, although the difference is not overwhelming. The result fits the theoretical prediction and thus provides evidence for anticipatory behavior. Individuals who are not informed in advance about the IA adjust their behavior more abruptly upon the signing of the IA, leading to a larger increase of the exit rate to work than among those who are informed in advance. However, this is not a quantitatively important phenomenon, as we do not find evidence of a larger re-employment probability for B at 90 days (or beyond) in Table 3.

6.2 Heterogeneous effects

Employability. For policy reasons it is interesting to know if there are certain identifiable types of individuals whose re-employment benefits strongly or does not benefit at all from the timing and/or prior announcement of IAs. A key result from the theoretical analysis in Subsection 5.2 is that an individual's employability is the prime candidate for the study of heterogeneous treatment effects, in particular when comparing treatment arms B and C.[18] The caseworker survey (see Subsection 2.2) suggests that caseworkers often do not regard IAs as useful for the re-employment chances of individuals who are thought to find work on their own within half a year. In contrast, they see more potential for IAs in the case of individuals thought to need some help to bring them back to work. This also points at effect variation by employability.

We do not directly observe individual employability or caseworkers' expectations on employability in our sample. However, we may obtain an indicator of individual employability by predicting individual unemployment durations in terms of individual characteristics and labor market history. Rather than considering many possible employability types, we consider a binary classification. For this, we estimate a duration model on a different but similar sample. The estimated duration model is then used to classify individuals

[18] More generally, the behavior of individuals with low employability may be more restricted by the controlling aspects of IA, but they may also become more averse to these aspects if they expect to be exposed to them for a long period.

according to whether the predicted median duration exceeds 6 months or not.[19,20]

Specifically, we estimate a descriptive Weibull Proportional Hazard model for the duration until employment given individual characteristics and labor market history x, so in obvious notation, $\theta(t|x) = \alpha t^{\alpha-1} \exp(x'\beta)$. The median $m(T|x)$ of T given x is then equal to

$$m(T|x) = (\log 2)^{\frac{1}{\alpha}} \exp(-\frac{x'\beta}{\alpha})$$

It is not difficult to show that in this model, $\mathbb{E}(T|x) = m(T|x) \cdot \gamma$ for some $\gamma > 0$ that does not depend on x or β. Thus, a low median is equivalent to a low expected duration. Note that the individual predicted median duration $m(T|x)$ is a monotonic function of the single index $x'\beta$, so the binary outcome $\mathrm{I}(m \gtrless 6)$ should give an employability classification that is relatively robust to misspecifications of the prediction model and to changes of the threshold value.

We estimate the prediction model with data we obtained covering all inflows into unemployment in the same regions in the year 2011, that is, from before the RCT. This is motivated by the fact that 2011 and 2012 are comparable years in terms of labor market conditions and in terms of stocks and flows into and out of UI among men aged 25-64 in the five regions (see Statistics of the FEA, 2019). Conditions in these two years 2011 and 2012 were slightly more favorable than in the surrounding years. Indeed, along the above dimensions, 2011 and 2012 are more similar to each other than to any of the surrounding years since 2009. The year 2011 was slightly more favorable than 2012, but the relevant flows differ only up to about 5% between the two years. This also applies to differences if examined by region and across 10-year age groups.

The 2011 sample consists of 55,545 men aged 25-64. This is substantially larger than our RCT sample, because it covers a larger inflow window but also because the 2011

[19]This approach can be seen as a profiling exercise. Indeed, before the IA is signed, caseworkers may profile their clients into categories, to shape thoughts about appropriate pathways towards re-employment. Such profiling is soft in the sense that it does not rely on algorithm but on the caseworker's observation of the client's characteristics and history, the caseworker's subjective impressions, and the caseworker's assessment of the support that the client may need most. Here, it also plays a role whether the caseworker expects the unemployed individual to return to employment on his own within 6 months. Our data do not contain reliable information on profiling outcomes. (At the macro level, about half of the inflow of unemployed is classified as being able to return on his own within 6 months.) To the extent that profiling is carried out before the IA, the profile should be orthogonal to the treatment arm in the RCT. However, the profiling may be updated at a later point in time. If this is in response to the assigned treatment then this must be seen as part of the assigned treatment.

[20]A standard approach in the literature is to use the control group and to regress the outcome variable on a set of baseline characteristics and then to use this model to predict the potential outcomes for the full experimental sample. Based on that one can stratify the sample into groups with different levels of expected outcomes. Abadie et al. (2018) point out that this endogenous stratification can lead to substantial biases. Moreover, our sample sizes are modest, and, in fact, in our regions there is no natural control group during the RCT.

sampling design does not exclude some types of individuals or spells that would not be eligible for inclusion in the RCT, such as spells of individuals who had been unemployed at some point in the 3 months prior to the onset of the spell, or spells with meeting timing sequences deemed inadmissible for the RCT, or spells where individuals moved to work before the IA.[21] Table A.2 in the Appendix gives the estimation results for the prediction model.[22] Using the estimated prediction model, around 40% of our RCT sample are predicted to have a median duration less than 6 months. (The next subsection contains sensitivity analyses regarding the 6-month threshold value.) Table A.3 in the Appendix describes mean differences between covariates in the ensuing low- and high-employability groups in the RCT sample. The lower-employability group with predicted medians above 6 months does not primarily consist of young unskilled workers but actually contains many older workers with higher previous wages and long previous employment spells, presumably with obsolete skills and coming from sectors in decline. The actual predicted median is not associated with the wage in the previous job.

Some further comments are in order regarding the usage of the prediction model. Firstly, spells that start in 2011 may be ongoing at the onset of the RCT in July 2012, meaning that they may be affected by the execution of the RCT, even though the RCT is designed to avoid such externalities. More generally, it is undesirable if the predicted medians are affected by outliers in the spell lengths. We investigate these issues empirically by artificially right-censoring spells at various points in time when estimating the prediction model. It turns out that the results (available upon request) are robust with respect to this.

Secondly, the predicted employability should relate to the views of the caseworkers in the RCT regarding employability because it reflects the experiences that the caseworkers accumulated before the RCT. The spells starting in 2011 were subject to the standard IA regime, meaning that the IA usually occurs during the first meeting. In a different regime (e.g. where everybody only receives an IA after 6 months), individual employability may change and caseworkers may respond to this. Whether such an equilibrium policy effect is quantitatively important depends on whether the regime affects the ranking of individuals in terms of their employability index.

[21] Also, recall that the RCT sample excluded 20% of the inflow as they were randomized to not be in one of the four treatment arms.

[22] In the 2011 sample, we predict for 87% of the individuals who experienced a duration of more than 6 months that their predicted median is above 6 months. For 47% of those with a completed duration less than 6 months it is predicted that the median is below 6 months. The latter may be due to the restrictiveness of the Weibull model, which may lead to a slight underestimation of the prevalence of short durations.

Results by employability. Figure 4 shows Kaplan-Meier estimates of the survival functions until exit to work. Among those with high employability, the estimated functions for the four treatment groups are very close. This suggests that, among them, signing an IA very early has on average no impact on their probability of getting a job. In contrast, among those with low employability, the survival function for group D is markedly different from the functions for groups A, B and C, where the latter three are virtually equal. In particular beyond 150 days group D displays a lower probability of having entered employment.

Table 4 presents estimation results for the regressions by employability. Among those with high employability, we do not find any significant difference between treatment groups, regardless of the elapsed duration. The coefficients in the table have a negative sign, meaning that the probability of returning to work within a certain amount of time is highest for group D. Thus, early IAs are obviously not an effective tool to speed up re-employment for individuals with good labor market prospects. The same applies to the early notification of IAs.

This is different for those with lower employability. Here, early IAs in the first or third month of unemployment have significant positive and quantitatively relevant effects on re-employment within 9 months, as compared to having a later IA. For treatment groups A and C the difference with D is even significant at an elapsed duration as low as 6 months. One year after entry into unemployment, the differences between A, B and C on the one hand and D on the other hand range from 6 to 9 percentage points; these differences are all highly significant. Thus, on average, among those with low employability, being assigned to a late IA reduces the probability of re-employment within a year by about 8 percentage points, from 53% to 45%. This is a substantial effect. For this it does not matter whether an IA is signed immediately or after 3 months.

The results also indicate that it is not quantitatively relevant whether the IA is announced in advance. To examine this in more depth, Figure 5 presents the equivalent of Figure 3 for each of the two employability groups. Each panel in the figure displays kernel hazard estimates for B and C for durations up to 6 months (bandwidths equal 14 days). Note that the vertical axis of the left panel (high employability) is more compressed than the vertical axis of the right panel.[23] Among high-employability individuals we find a marked difference in the increase of the hazard rate around 90 days, with C displaying a much stronger increase than B (this is robust w.r.t. the bandwidth choice). According to the theoretical analysis, this means that individuals with treatment status B anticipate

[23] Also note that the horizontal axis only covers the early stages of the spells. After 180 days the hazards decrease substantially.

the IA at 3 months and modify their behavior before the IA in response to that. However, as in the full sample, this does not lead to a difference between B and C in the unconditional average re-employment probabilities at 90 days, so the anticipation is quantitatively unimportant. Among low-employability individuals the hazard rates around 90 days are remarkably similar for B and C, and this does not provide much evidence of anticipatory behavior.

Caseworker identifier. As an additional heterogeneity analysis we interact the treatment effects with the caseworker identifier. Such an investigation can only have a limited scope due to (i) the large number of caseworkers and (ii) the fact that, although caseworker assignment is arguably quasi-random, we were not able to randomize it within our RCT. However, interaction effects may be informative on the presence of heterogeneity in the extent to which a caseworker is able to put the IA to good use. If such heterogeneity is indeed present then this provides an incentive for the employment agency to let less effective caseworkers learn from more effective caseworkers.

In the data used for the above results there are 13 caseworkers with each over 50 clients in the RCT. We estimate regression models in which the treatment effects are interacted with 13 corresponding binary caseworker indicators and where these indicators are also included as additive regressors, using the same data. Clients of caseworkers who had 50 or less clients in the RCT are the baseline category in these regression models. Among disadvantaged clients, we find strong evidence of effect heterogeneity according to an F test (p-value is 0.041).[24] The results are robust with respect to small changes in the threshold value of 50 clients per caseworker. For the reasons mentioned above we do not zoom in further on sources of effect heterogeneity by caseworker. However, the results motivate further research to identify whether caseworkers can increase the re-employment effect of early IAs among disadvantaged clients by adopting the work practice used by the caseworkers whose clients display the largest effects.

6.3 Additional outcome measures and sensitivity analyses

Wage-related outcomes. Recall from Subsection 5.1 that inference of average treatment effects on initial wages in accepted jobs is not possible due to right-censoring of unemployment spells at the end of the observation window. With this in mind, Figure A.1 in the Appendix compares kernel density estimates for the initial wage (per day) in accepted jobs, measured at various duration endpoints. This does not suggest any large

[24] In the subsample of clients with high employability we do not find evidence of effect heterogeneity (p-value 0.65). Note that in this subsample we do not find an effect in a homogeneous specification either.

or systematic differences across the treatment groups.[25,26] Stratification by employability is not informative, as the degree of right-censoring differs starkly between the two subsamples. Moreover, as discussed in Subsection 6.2, there is no simple relation between the previous wage and employability, which further complicates interpretations of any differences in post-unemployment wage effects.

Usage of active labor market programs. According to the experimental protocol, caseworkers should not allow the assigned IA treatment to affect the frequency of meetings with the unemployed or their access to ALMP programs. To verify this we examine whether these are associated with each other, using the detailed information on meetings and ALMP participation in the data. Such analyses are descriptive as the observation of meetings and ALMP participation is restricted by the realized duration outcome. We regress the number of days spent in ALMP and the number of invitations divided by the days spent in unemployment on indicators for being assigned to treatment A, B or C, controlling for observed background characteristics x. Analogously, we investigate whether the treatment groups differ in the probability of receiving vacancy referrals from the employment agency. Table 5 contains results by employability. None of the coefficients is significantly different from zero. This suggests that our main findings are not driven by differences in the access to ALMP, the receipt of vacancy referrals or the number of meetings with the caseworkers.

Next, we examine whether the effects are driven by differential access to wage subsidies across treatment groups. We re-estimate effects by only considering transitions into unsubsidized jobs as transitions to employment, while defining jobs with wage subsidies as non-employment. The results are in Table 6. There are some slightly different coefficients and significance levels which may reflect a slightly earlier flow into subsidized work among those who receive an early IA. However, the overriding pattern of results is not strongly affected by this.

The data do not record spells of self-employment, but they do record take-up of self-employment subsidies. In Germany, unemployed who start their own business can receive

[25] The survey that was held among a subsample of RCT participants about 1.5 months after entry (Subsection 2.3) includes a question about the lowest acceptable wage (i.e., the reservation wage) for those still unemployed. It is difficult to use this information. As discussed in Subsection 2.3, the sample of respondents is non-balanced. Moreover, the reservation wage is a determinant of being unemployed at 1.5 months. With these caveats in mind, we find no evidence that the observed reservation wages are systematically different across treatment groups (at the 10% level; results available upon request). This is consistent with the absence of differences in accepted wages.

[26] As an alternative income-related measure one may consider the sum of UI benefits and earnings from employment, e.g. until 12 months after entry, as this is observable for every individual, and it can be interpreted as a proxy for the present value of income, at least over the first year after entry. However, the data only provide net UI benefits levels and do not allow for backward calculation of gross levels.

financial support from the employment agency (*Gründungszuschuss*).[27] Table 7 presents results if spells of subsidized self-employment are counted as regular employment. Again, the overall pattern of results shows robustness to this.

We also investigate whether the probability of a recall is affected by the treatment. The results (available upon request) suggest that among higher-employability workers there are only small differences across treatment groups in terms of their recall probability during the first 90 days. There are no recall effects after 90 days of unemployment and no recall effects at any time among lower-employability individuals. These results suggest that for unemployed workers who expect a recall it is not important whether or not they sign an IA.

What the results in this subsection suggest is that IAs do not work by way of participation in other ALMPs. Also, the usage of other ALMPs does not seem to depend on the timing or advance notification of the IA. IAs thus appear to operate independently of other policy measures. IA effects can therefore be seen as policy effects that are separate from any effects of other ALMPs.

Alternative (sub)sampling criteria. Next, we choose an alternative threshold for splitting the sample into two groups. Instead of using a predicted median duration of 6 months, we take 7 months as the threshold. This leads to more equal sample sizes (2027 below this threshold and 2136 above it). The results are robust with respect to this (Table 8).

Although the inflow into UI tends to be dominated by well-connected workers with reasonably good re-employment perspectives, it cannot be ruled out that some newly unemployed workers have multiple complex personal and/or professional problems such that it is not realistic to expect a return to work. We examine whether the results change when omitting individuals with a predicted median duration of more than 3 years until employment. This results in a reduction of the size of the lower-employability subsample from 2475 to 1758. While this leads to a loss of precision for some of the estimated effects, most point estimates are close to those for our main specification (see Table A.4 in the Appendix).

Finally, recall from Subsection 4.2 that IA scheduling deviations are more common for some caseworkers than for others. As a sensitivity analysis we drop all the clients of caseworkers with relatively many extreme deviations from the sample.[28] The results are

[27]These subsidies lasts for up to 15 months. In the first 6 months, the subsidy equals the UI benefits level. In the subsequent 9 months, the individual receives 300 euro. At the moment of application for the subsidy, the unemployed has to be eligible for at least 150 days of UI benefits receipt.

[28]For this purpose we define that a caseworker deviates from the schedule if (i) an unemployed of

qualitatively the same (see Table A.5 in the Appendix).

7 Conclusions

Signing an Integration Agreement in the first or third month of unemployment (as opposed to later, in the sixth month) has on average a small positive effect on entering employment within a year. Put differently, a late IA reduces the probability of re-employment within a year, by about 4 percentage points, from 69% to 65%, and it commensurately increases the probability of long-term unemployment. For this, it does not matter whether the IA is signed immediately or after 3 months.

A theoretical analysis based on job search models suggests that an individual's employability is the prime candidate for the study of effect heterogeneneity, and this is corroborated by caseworker survey responses. It turns out that among those with high employability, the timing of the IA does not affect the probability of returning to work within any amount of time. If only, early IAs have negative effects on exits to work. Thus, early IAs are not an effective tool to speed up re-employment for individuals with good labor market prospects. This is different for those with lower employability. Here, early IAs in the first or third month of unemployment have significantly positive and quantitatively relevant effects on re-employment within 9 months and within 12 months, as compared to having a later IA. The differences are sometimes even significant at an elapsed duration as low as 6 months. On average, among those with low employability, being assigned to an early IA increases the probability of re-employment within a year by about 8 percentage points, from 45% to 53% (so the relative increase is 18%). This is a substantial effect. For this it does not matter whether an IA is signed immediately or after 3 months. Note that the positive over-all effects of early IAs are exclusively driven by the lower-employability group.

We conclude from this that the IA is a valuable policy tool, especially for newly unemployed individuals with adverse labor market prospects. It strongly reduces their probability of long-term unemployment. Conversely, for individuals with favorable prospects, the IA does not bring advantages on average. As such, the IA is an interesting new addition to the toolkit of active labor market policies for the group that is usually targeted by more traditional policies. It seems that the nudging approach taken in the IA, in which monitoring is presented in a constructive fashion, delivers desirable outcomes.

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treatment group A is unemployed for 60 days or more and does not sign an IA before 60 days of unemployment, (ii) an unemployed of treatment group B or C signs an IA before day 60 or after 135 days of unemployment and (iii) an unemployed of treatment group D signs an IA before day 135 or after day 245. In the sensitivity analysis we exclude caseworkers who deviate in more than 40% of their cases.

Our paper contains a detailed theoretical analysis of anticipatory effects of advance announcements of the IA, and we develop an innovative econometric approach to detect such announcement effects. The corresponding empirical findings suggest anticipatory behavior in response to the advance announcement of an IA at 3 months. Individuals who are not informed in advance adjust their behavior more abruptly upon the signing of the IA, as reflected in a larger increase of the exit rate to work than what is observed among those who are informed in advance. However, this is not a quantitatively important phenomenon, as we do not find evidence of announcement effects on unconditional re-employment probabilities at any elapsed duration.

We also examine interaction effects with caseworker identifiers, keeping in mind some methodological limitations in this respect. Among disadvantaged clients, we find strong statistical evidence of effect heterogeneity. In our view, this motivates further research to identify whether early IAs for disadvantaged clients can be put to better usage by adopting work practices used by the most effective caseworkers.

According to the experimental protocol, caseworkers should not allow the assigned IA treatment to affect the frequency of meetings with the unemployed or their access to ALMP programs. We verified that this was indeed the case (and this also applies to the frequency of vacancy referrals), so that effects cannot be attributed to differential usage of other policy instruments. Also, results are robust with respect to the usage of wage subsidies or self-employment subsidies. All in all, IA effects appear to operate independently of other policy measures. We do not find effects on recalls or on accepted wages, where it should be kept in mind that the latter are only observed for uncensored unemployment spells.

Recently, first findings from our study were presented to the governing board of the German FEA. This led the FEA to implement a major modification of the usage of IAs in the UI system. Specifically, job seekers who are considered to be able to find work by themselves within six months are not subjected anymore to an obligatory IA in the first three months of unemployment. In the absence of aggregate data on the fraction of newly unemployed UI recipients with this perceived reemployment characteristic, we cannot quantify the number of individuals who directly benefit from this policy change. A crude indication could be based on the annual inflow into UI (2.55 million in 2012) and the fraction with high employability in our data (41%).[29]

[29] A possibly wide upper bound can be constructed from the number of 0.9 million individuals who, in a period of 5 months in 2016, registered themselves as job searchers because they expected to lose their current job within 100 days and who were deemed to be able to find work by themselves within six months of unemployment.

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Figures and tables

Table 1: Experimental Design

Group	IA in month	IA announced
A	1	No
B	3	Yes
C	3	No
D	6	No

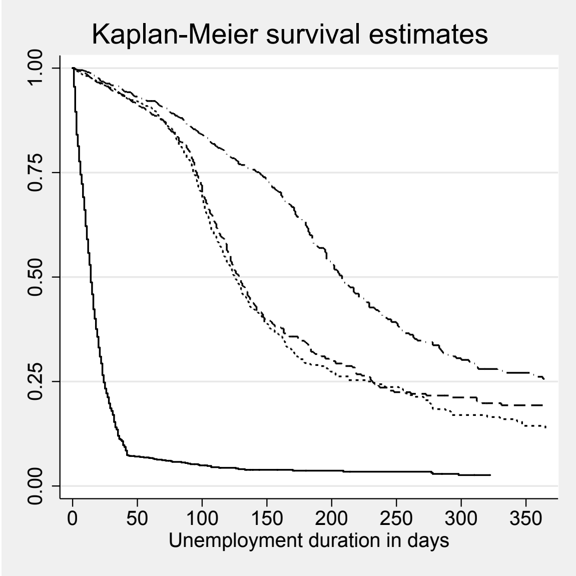
Notes: IA: integration agreement. IA announced: written announcement on IA handed out in the first month of unemployment.

Table 2: Balancing - distribution of selected observed characteristics across experimental groups

	Treatment group (N=4,163)				
	A	B	C	D	p-value
Age	41.7	42.1	41.6	41.3	0.48
Vocational training	0.693	0.715	0.700	0.699	0.738
University degree	0.090	0.081	0.089	0.102	0.431
Abitur (High school degree)	0.147	0.145	0.154	0.177	0.163
German	0.892	0.885	0.880	0.899	0.536
Turkish	0.032	0.034	0.035	0.035	0.979
Previous wage	66.278	66.592	65.776	66.065	0.966
Duration of previous employment spell	639.531	629.747	637.778	662.849	0.632
Duration of previous non-employment spell	119.012	119.934	103.491	113.112	0.217
Share in unemployment last 5 years	0.139	0.141	0.135	0.133	0.573
Subsidized selfemployment in the last 5 years	0.036	0.046	0.045	0.033	0.331
Subsidized employment in the last 5 years	0.123	0.116	0.127	0.125	0.887
ALMP in the last 5 years	0.416	0.396	0.404	0.390	0.656
Average wage in the last 5 years	63.887	64.424	64.325	63.023	0.802

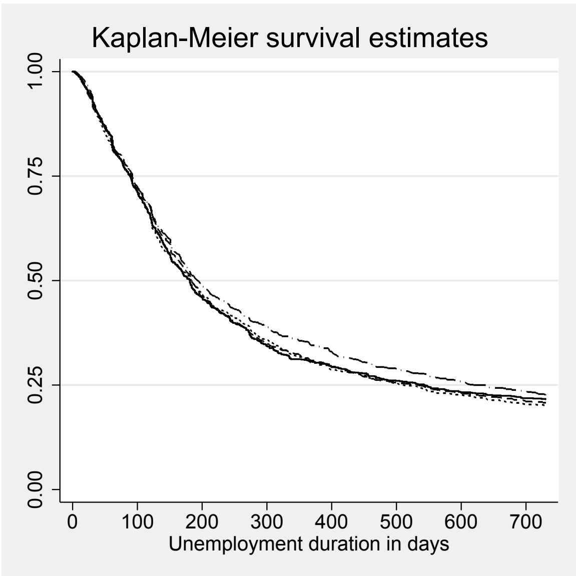
Notes: Treatment A/C/D: integration agreement in month 1/3/6. Treatment B: integration contract in month 3 with written announcement in month 1. X variables measured at the day of randomization. F-stat and p-value: F-statistic and its p-value from regression of variable on three treatment group dummies with constant. Sample size is 4163, with 1061, 1013, 1068 and 1021 in groups A, B, C and D, respectively.

Figure 1: Kaplan-Meier estimates of the survival function until signing an IA.



Notes: Solid: IA in month 1 (Group A), long dash: IA in month 3 with announcement (Group B), dot: IA in month 3 without announcement (Group C), dash dot: IA in month 6 (Group D). Number of observations: 4,163.

Figure 2: Kaplan-Meier estimates of the survival function until exit to work.



Notes: Solid: IA in month 1 (Group A), long dash: IA in month 3 with announcement (Group B), dot: IA in month 3 without announcement (Group C), dash dot: IA in month 6 (Group D). Number of observations: 4,163.

Figure 3: Kernel hazard estimates for treatment groups B and C.

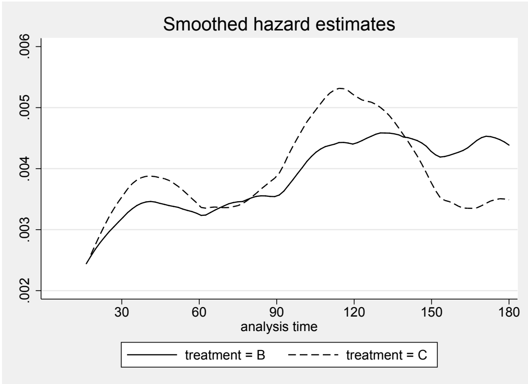
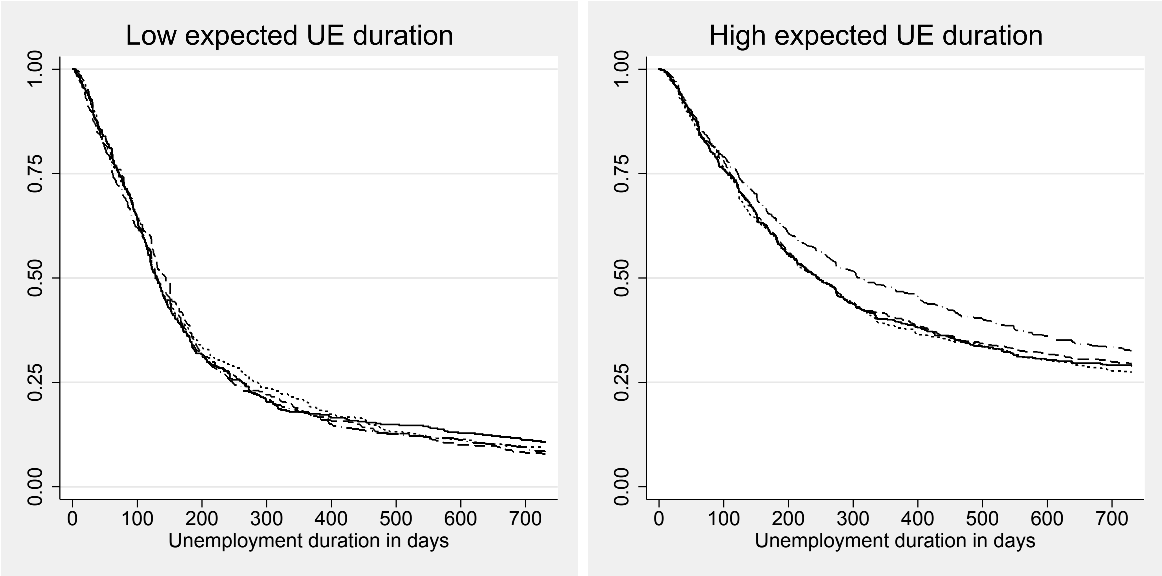


Table 3: Exit to work within 90, 180, 270 and 365 days after unemployment entry

Until day:	*90*		*180*		*270*		*365*	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
A	0.007	(0.019)	0.022	(0.021)	0.027	(0.020)	0.041**	(0.019)
B	-0.010	(0.019)	0.001	(0.021)	0.021	(0.020)	0.033*	(0.020)
C	-0.003	(0.019)	0.022	(0.021)	0.024	(0.020)	0.047**	(0.019)
Mean D	0.254		0.474		0.589		0.650	

Notes: Linear probability models. Dependent variable is one if an individual has found a job within 90/180/270/365 days after unemployment entry. Number of observations: 4,163. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Figure 4: Kaplan-Meier estimates of the survival function until exit to work - depending on predicted unemployment-to-employment duration.



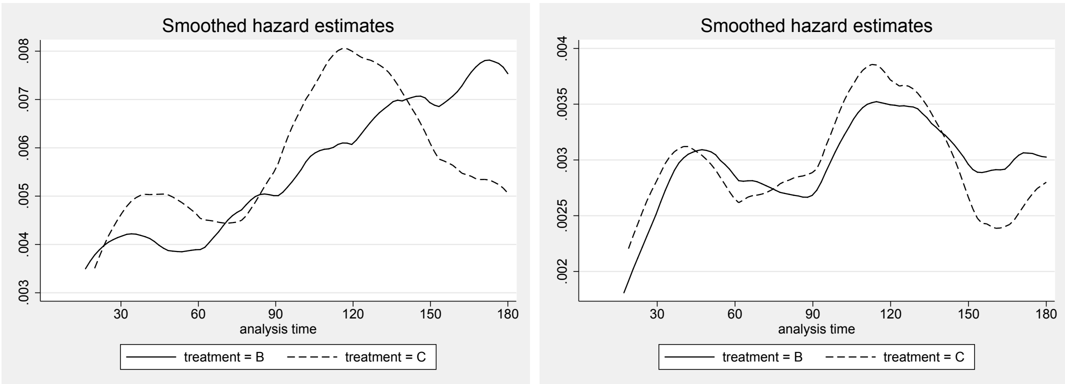
Notes: Individuals with predicted median unemployment duration $\leqslant$ 6 months on the left (n=1,688) and individuals with predicted median unemployment duration > 6 months on the right (n=2,475). Solid: IA in month 1 (Group A), long dash: IA in month 3 with announcement (Group B), dot: IA in month 3 without announcement (Group C), dash dot: IA in month 6 (Group D).

Table 4: Exit to work within 90, 180, 270 and 365 days after unemployment entry - Heterogeneous effects depending on predicted unemployment duration

Until day:	*90*		*180*		*270*		*365*	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
	Predicted median unemployment duration $\leqslant$ 6 months[a]							
A	-0.024	(0.032)	-0.011	(0.032)	-0.021	(0.029)	-0.006	(0.026)
B	-0.027	(0.033)	-0.045	(0.033)	-0.025	(0.029)	-0.002	(0.026)
C	-0.032	(0.032)	-0.021	(0.033)	-0.040	(0.029)	-0.020	(0.027)
Mean D	0.344		0.642		0.771		0.823	
	Predicted median unemployment duration > 6 months[b]							
A	0.030	(0.023)	0.046*	(0.027)	0.061**	(0.028)	0.075***	(0.027)
B	0.006	(0.023)	0.036	(0.027)	0.054*	(0.028)	0.060**	(0.027)
C	0.019	(0.022)	0.049*	(0.027)	0.064**	(0.027)	0.091***	(0.027)
Mean D	0.191		0.357		0.462		0.530	

Notes: Linear probability models. Dependent variable is one if an individual has found a job within 90/180/270/365 days after unemployment entry. Predicted median unemployment duration is based on the coefficients of a hazard rate model estimated on an inflow sample into unemployment in the year before the experiment. Number of observations: [a]N: 1,688, [b]N: 2,475. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Figure 5: Kernel hazard estimates for treatment groups B and C by degree of employability.



Notes: Left panel: sample of individuals with predicted median unemployment duration $\leqslant$ 6 months (N=1,688). Right panel: individuals with predicted median unemployment duration $>$ 6 months (N=2,475).

Table 5: Active labor market policy participation, invitations to meetings and vacancy referrals

	Predicted median duration $\leqslant$ 6 months[a]						Predicted median duration $>$ 6 months[b]					
Until day	*90*		*180*		*365*		*90*		*180*		*365*	
Participation in ALMP												
A	-0.000	(0.011)	-0.009	(0.012)	-0.008	(0.012)	0.004	(0.011)	0.007	(0.011)	0.003	(0.010)
B	-0.005	(0.011)	-0.010	(0.012)	-0.009	(0.012)	0.001	(0.011)	0.002	(0.011)	0.002	(0.010)
C	0.018	(0.013)	0.009	(0.013)	0.007	(0.013)	-0.003	(0.010)	-0.003	(0.010)	0.000	(0.010)
Mean D	0.052		0.071		0.080		0.056		0.063		0.066	
Invitations to meeting												
A	0.000	(0.001)	0.000	(0.001)	-0.000	(0.001)	0.000	(0.001)	-0.000	(0.001)	-0.000	(0.001)
B	-0.000	(0.001)	-0.000	(0.001)	-0.000	(0.001)	0.000	(0.001)	0.000	(0.001)	0.000	(0.001)
C	0.001	(0.002)	0.001	(0.002)	0.001	(0.002)	0.001	(0.001)	0.000	(0.001)	0.000	(0.001)
Mean D	0.013		0.013		0.013		0.011		0.011		0.011	
Vacancy referrals												
A	0.003	(0.005)	0.003	(0.005)	0.003	(0.005)	-0.003	(0.003)	-0.003	(0.003)	-0.003	(0.003)
B	0.007	(0.005)	0.006	(0.005)	0.006	(0.005)	-0.002	(0.004)	-0.001	(0.004)	-0.001	(0.004)
C	-0.004	(0.005)	-0.004	(0.004)	-0.005	(0.004)	-0.002	(0.003)	-0.001	(0.003)	-0.000	(0.003)
Mean D	0.054		0.050		0.050		0.036		0.032		0.031	

Notes: OLS regressions. Outcome variables: sum of days in ALMP participation / sum of invitations / sum of vacancy referrals, each divided by the number of days spent in unemployment, by employability indicator. In the latter, the predicted median unemployment duration is based on the coefficients of a hazard rate model estimated on an inflow sample into unemployment in the year before the experiment. Number of observations: [a]N: 1,688, [b]N: 2,475. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Table 6: Exit to unsubsidized work within 90, 180, 270, 365 days after unemployment entry

Until day:	*90*		*180*		*270*		*365*	
			Predicted median duration $\leqslant$ *6 months*[a]					
A	-0.016	(0.032)	-0.009	(0.033)	-0.033	(0.029)	-0.017	(0.026)
B	-0.017	(0.032)	-0.046	(0.033)	-0.034	(0.029)	-0.019	(0.027)
C	-0.023	(0.032)	-0.025	(0.033)	-0.065**	(0.030)	-0.031	(0.027)
Mean D	0.322		0.628		0.768		0.823	
			Predicted median duration > *6 months*[b]					
A	0.023	(0.022)	0.031	(0.027)	0.059**	(0.027)	0.074***	(0.027)
B	0.002	(0.022)	0.029	(0.027)	0.054*	(0.028)	0.069**	(0.027)
C	0.015	(0.022)	0.041	(0.027)	0.063**	(0.027)	0.094***	(0.027)
Mean D	0.183		0.349		0.445		0.512	

Notes: Linear probability models. Dependent variable is one if an individual has started an unsubsidized job within 90/180/270/365 days after unemployment entry. Predicted median unemployment duration is based on the coefficients of a hazard rate model estimated on an inflow sample into unemployment in the year before the experiment. Number of observations: [a]N: 1,688, [b]N: 2,475. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Table 7: Exit to work or self-employment subsidies within 90, 180, 270 and 365 days after unemployment entry

Until day:	*90*		*180*		*270*		*365*	
			Predicted median duration $\leqslant$ *6 months*[a]					
A	-0.026	(0.032)	-0.006	(0.032)	-0.014	(0.028)	0.001	(0.025)
B	-0.030	(0.033)	-0.046	(0.033)	-0.023	(0.029)	-0.000	(0.026)
C	-0.032	(0.032)	-0.021	(0.032)	-0.040	(0.029)	-0.020	(0.026)
Mean D	0.348		0.647		0.776		0.828	
			Predicted median duration > *6 months*[b]					
A	0.029	(0.023)	0.040	(0.027)	0.057**	(0.028)	0.071***	(0.027)
B	0.005	(0.023)	0.028	(0.027)	0.041	(0.028)	0.047*	(0.027)
C	0.025	(0.023)	0.047*	(0.027)	0.063**	(0.027)	0.090***	(0.027)
Mean D	0.193		0.369		0.480		0.548	

Notes: Linear probability models. Dependent variable is one if an individual has started a regular job or started receiving selfemployment subsidies within 90/180/270/365 days after unemployment entry. Predicted median unemployment duration is based on the coefficients of a hazard rate model estimated on an inflow sample into unemployment in the year before the experiment. Number of observations: [a]N: 1,688, [b]N: 2,475. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Table 8: Exit to work within 90, 180, 270 and 365 days after unemployment entry - Alternative threshold for high predicted unemployment-to-employment duration (7 months)

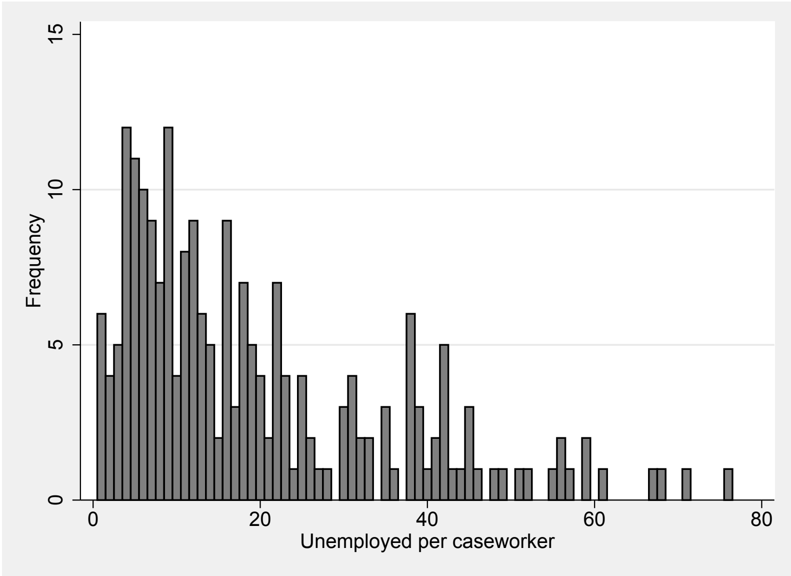
Until day:	*90*		*180*		*270*		*365*	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
	Predicted median unemployment duration $\leqslant$ 7 months[a]							
A	-0.016	(0.029)	0.004	(0.030)	-0.001	(0.027)	0.017	(0.024)
B	-0.020	(0.029)	-0.030	(0.030)	-0.006	(0.027)	0.016	(0.025)
C	-0.034	(0.029)	-0.013	(0.030)	-0.016	(0.027)	-0.001	(0.025)
Mean D	0.331		0.614		0.741		0.799	
	Predicted median unemployment duration > 7 months[b]							
A	0.028	(0.024)	0.039	(0.029)	0.055*	(0.030)	0.065**	(0.030)
B	0.001	(0.024)	0.031	(0.029)	0.048	(0.030)	0.051*	(0.030)
C	0.026	(0.024)	0.051*	(0.029)	0.056*	(0.029)	0.088***	(0.029)
Mean D	0.179		0.339		0.441		0.507	

Notes: Linear probability models. Dependent variable is one if an individual has found a job within 90/180/270/365 days after unemployment entry. Predicted median unemployment duration is based on the coefficients of a hazard rate model estimated on an inflow sample into unemployment in the year before the experiment. Number of observations: [a]N: 2,027, [b]N: 2,136. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Appendix (For Online Publication)

Appendix 1. Supplementary tables and figures

Figure 6: Caseload: unemployed per caseworker.



Notes: Caseload defined as unemployed experiment participants per caseworker.

Table A.1: Balancing - distribution of observed characteristics across experimental groups

	A	B	C	D	p-val.
Age	41.686	42.099	41.636	41.315	0.477
Vocational training	0.693	0.715	0.700	0.699	0.738
University degree	0.09	0.081	0.089	0.102	0.431
Abitur (High school degree)	0.147	0.145	0.154	0.177	0.163
School degree: no information	0.015	0.022	0.016	0.024	0.402
German	0.892	0.885	0.880	0.899	0.536
Turkish	0.032	0.034	0.035	0.035	0.979
Having a handicap	0.004	0.008	0.005	0.011	0.190
Previous wage	66.278	66.592	65.776	66.065	0.0966
Duration of previous employment spell	639.531	629.747	637.778	662.849	0.632
Duration of previous non-employment spell	119.012	119.934	103.491	113.112	0.217
Share in unemployment last 5 years	0.139	0.141	0.135	0.133	0.573
Share in employment last 5 years	0.139	0.141	0.135	0.133	0.573
Subsidized selfemployment in the last 5 years	0.036	0.046	0.045	0.033	0.331
Subsidized employment in the last 5 years	0.123	0.116	0.127	0.125	0.887
ALMP in the last 5 years	0.416	0.396	0.404	0.390	.0.656
Average wage in the last 5 years	63.887	64.424	64.325	63.023	0.802
At least 1 emp. spell within the last 5 years	0.971	0.973	0.967	0.967	0.796
At least 2 emp. spell swithin the last 5 years	0.707	0.713	0.705	0.702	0.962
Last job: vocational training	0.029	0.016	0.02	0.024	0.192
Last job: regular job	0.943	0.958	0.948	0.943	0.396
Recall in the past	0.229	0.223	0.194	0.189	0.051
Employment agency 1	0.122	0.118 0	0.106	0.116	0.693
Employment agency 2	0.210	0.241	0.200	0.206	0.110
Employment agency 3	0.260	0.249	0.275	0.247	0.422
Employment agency 4	0.223	0.217	0.225	0.211	0.858
Employment agency 5	0.185	0.175	0.194	0.221	0.048
Sector of the previous job:					
Agriculture	0.010	0.015	0.008	0.007	0.304
Manufacturing	0.167	0.178	0.164	0.156	0.611
Water supply	0.004	0.010	0.006	0.009	0.310
Construction	0.191	0.163	0.162	0.149	0.064
Trade	0.122	0.115	0.126	0.123	0.894
Traffic	0.067	0.094	0.081	0.087	0.140
Gastronomy	0.040	0.045	0.037	0.054	0.262
Information and Communication	0.025	0.017	0.022	0.022	0.595
Scientific and technical services	0.036	0.032	0.032	0.037	0.860
Other business services	0.196	0.203	0.234	0.184	0.032
Public Administration, Social Insurances	0.025	0.016	0.013	0.033	0.006
Education	0.019	0.024	0.017	0.028	0.272
Health and Welfare	0.014	0.021	0.027	0.028	0.104
Other Services	0.025	0.015	0.010	0.022	0.057

Notes: N=4,163. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Group D: IA in month 6 without announcement. F-stat and p-value: F-statistic and its p-value from regression of variable on three treatment group dummies with constant.

Table A.2: Weibull model for duration until employment based on pre-experimental sample

	Coefficient	Standard error
Employment agency 2	-8.85e-06	.018879
Employment agency 3	-.1414771	.017397
Employment agency 4	-.0167922	.0181735
Employment agency 5	.0750153	.0183099
Entry quarter 2	-.1006481	.0142735
Entry quarter 3	-.1603311	.0150353
Entry quarter 4	-.1726862	.0139331
Age	1.300917	.1584928
Age^2	-.0538913	.0058426
Age^3	.0009594	.0000933
Age^4	-6.26e-06	5.45e-07
German	-.0436228	.0193867
Turkish	-.0382289	.0325708
Vocational training	.0814223	.0129426
University degree	-.0663318	.0338407
Abitur (High school degree)	.120339	.0239843
Handicap	-.6095661	.0302381
Sector of the previous job:		
Agriculture	.3805257	.0494353
Manufacturing	.2052959	.0343096
Water supply	.2932843	.0639192
Construction	.3256049	.0335695
Trade	.1790048	.0349138
Traffic	.4331452	.035983
Gastronomy	.2286416	.039277
Information and Communication	.0810237	.0510702
Scientific and technical services	.0534806	.0441261
Other business services	.3222727	.0330364
Public Administration, Social Insurances	.0667482	.0512931
Education	.0830213	.0471203
Health and Welfare	-.010769	.0430726
Other Services	-.0905017	.0502339
At least 1 emp. spell within the last 5 years	.8038531	.0415418
At least 2 emp. spell swithin the last 5 years	.2320936	.0203656
Duration of previous employment spell	-.0001852	.0000176
Days from employment to unemployment	-.0006947	.0000198
Duration of previous non-employment spell	-.0002526	.0000276
Average wage in the last 5 years	-.0020294	.0003255
Share in employment last 5 years	.3911503	.0349109
Share in unemployment last 5 years	-.6575559	.0344058
Subsidized selfemployment in the last 5 years	.0741586	.0315838
Subsidized employment in the last 5 years	.1402286	.0172089
ALMP in the last 5 years y	.0719991	.012963
Wage	.0015413	.000327
Wage^2	-1.18e-06	5.16e-07
Wage^3	1.45e-10	7.12e-11
Recall in the past 5 years	.1655345	.0183285
More than one recall in the past 5 years	.4107274	.0223874
Constant	-16.16147	1.569772
$\log(\alpha)$	-.4217094	.0043116

Notes: Estimates are based on in inflow sample into unemployment in 2011 in the five labor agencies participating in the experiment. "Wage" is last daily wage in euros in previous employment. Number of observations: 55,545. Log-Likelihood: -91,214.384

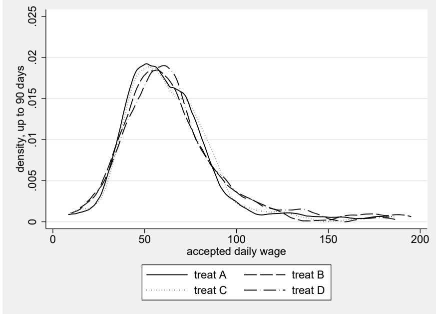
Table A.3: Descriptive statistics in RCT sample by employability as predicted from 2011 sample

	Predicted median duration $\leqslant$ *6 months*	*Predicted median duration* $>$ *6 months*
Age	36.9 (9.2)	44.9 (11.4)
Vocational training	0.790	0.642
University degree	0.028	0.133
Abitur	0.089	0.201
German	0.885	0.891
Turkish	0.031	0.036
Previous wage	61.7 (24.7)	69.2 (43.5)
Duration of previous employment spell	395.4 (386.4)	810.9 (658.9)
Duration of previous non-employment spell	97.8 (119.3)	124.7 (241.7)
Share of unempl. in previous 5 years (%)	0.148	0.129
Subsidized self-employment in the last 5 years	0.022	0.053
Subsidized employment in the previous 5 years	0.159	0.099
ALMP in previous 5 years	0.490	0.341
Average wage in previous 5 years	56.3 (22.0)	69.1 (41.4)
Sector of the previous job:		
Agriculture	0.013	0.008
Manufacturing	0.136	0.186
Water supply	0.003	0.010
Construction	0.258	0.104
Trade	0.010	0.137
Traffic	0.112	0.061
Gastronomy	0.047	0.042
Information and Communication	0.008	0.031
Financial services / Insurances	0	0.015
Real estate	0.002	0.009
Scientific and technical services	0.012	0.049
Other business services	0.280	0.154
Public Administration, Social Insurances	0.009	0.030
Education	0.005	0.034
Health and Welfare	0.005	0.035
Other Services	0.005	0.026

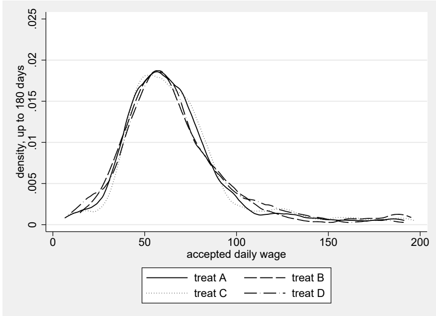
Notes: Characteristics are measured at the moment of randomization. Standard deviations in parentheses.

Figure A.1: Kernel density estimates for initial daily wage after unemployment.

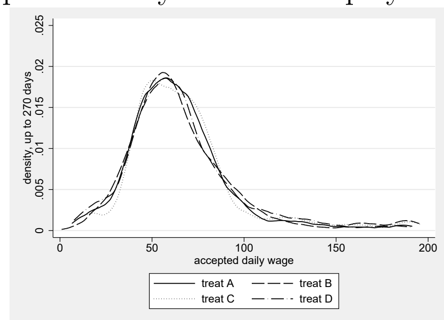
up to 90 days after unemployment



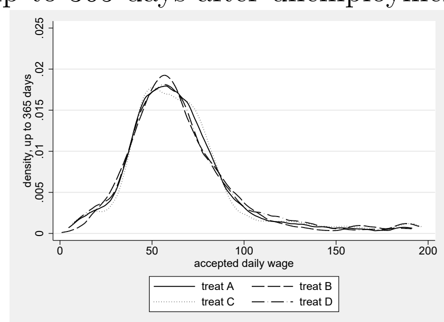
up to 180 days after unemployment



up to 270 days after unemployment



up to 365 days after unemployment



Notes: Daily gross first wage after unemployment. Optimized kernel bandwidth $\approx$ 6 euro.

Table A.4: Exit to work within 90, 180, 270 and 365 days after unemployment entry - Leaving out individuals with predicted median unemployment-to-employment duration > 3 years

Until day:	*90*		*180*		*270*		*365*	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
	Predicted median unemployment duration $\leqslant$ *6 months*[a]							
A	-0.024	(0.032)	-0.011	(0.032)	-0.021	(0.029)	-0.006	(0.026)
B	-0.027	(0.033)	-0.045	(0.033)	-0.025	(0.029)	-0.002	(0.026)
C	-0.032	(0.032)	-0.021	(0.033)	-0.040	(0.029)	-0.020	(0.027)
Mean D	0.344		0.642		0.771		0.823	
	Predicted median unemployment duration > 6 months[b]							
A	0.048*	(0.028)	0.046	(0.033)	0.071**	(0.034)	0.078**	(0.033)
B	0.008	(0.027)	0.042	(0.034)	0.054	(0.034)	0.059*	(0.034)
C	0.041	(0.027)	0.052	(0.033)	0.061*	(0.033)	0.091***	(0.033)
Mean D	0.191		0.379		0.489		0.566	

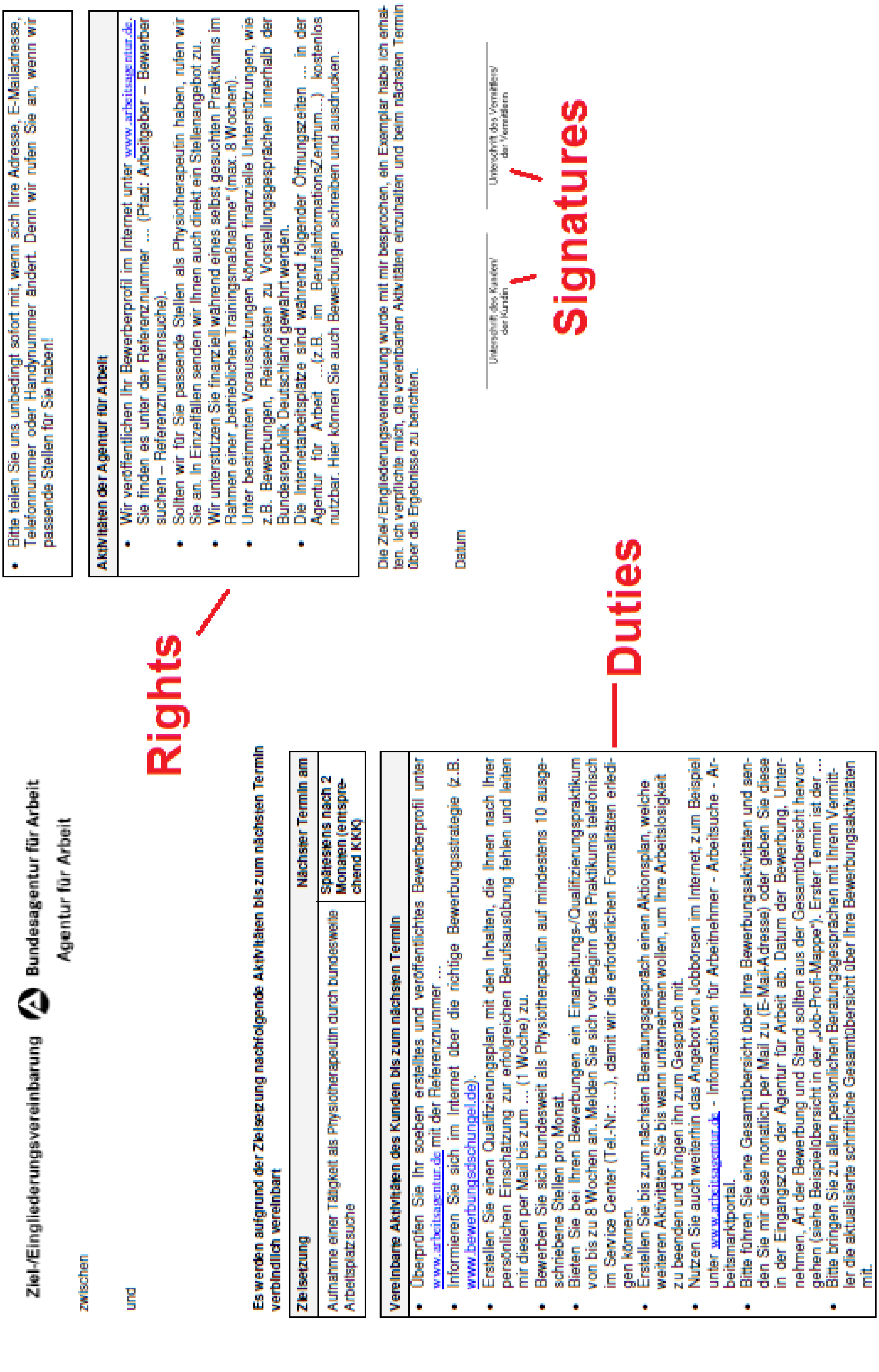
Notes: Linear probability models. Dependent variable is one if an individual has found a job within 90/180/270/365 days after unemployment entry. Predicted median unemployment duration is based on the coefficients of a hazard rate model estimated on an inflow sample into unemployment in the year before the experiment. Number of observations: [a]N: 1,688, [b]N: 1,758. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Table A.5: Exit to unsubsidized work within 90, 180, 270, 365 days after unemployment entry. leaving out caseworkers with a high schedule deviation (> 0.4)

Until day:	*90*		*180*		*270*		*365*	
	Predicted median unemployment duration $\leqslant$ *6 months*[a]							
A	-0.032	(0.034)	-0.009	(0.034)	-0.028	(0.030)	-0.005	(0.027)
B	-0.036	(0.034)	-0.055	(0.034)	-0.036	(0.030)	-0.005	(0.027)
C	-0.034	(0.034)	-0.022	(0.035)	-0.049	(0.031)	-0.028	(0.028)
Mean D	0.351		0.652		0.787		0.832	
	Predicted median unemployment duration $>$ *6 months*[b]							
A	0.029	(0.025)	0.034	(0.029)	0.054*	(0.029)	0.071**	(0.029)
B	-0.000	(0.025)	0.031	(0.029)	0.050*	(0.030)	0.051*	(0.029)
C	0.020	(0.025)	0.049*	(0.029)	0.061**	(0.029)	0.072**	(0.029)
Mean D	0.201		0.372		0.474		0.543	

Notes: Linear probability models. Dependent variable is one if an individual has started an unsubsidized job within 90/180/270/365 days after unemployment entry. Predicted median unemployment duration is based on the coefficients of a hazard rate model estimated on an inflow sample into unemployment in the year before the experiment. Number of observations: [a]N: 1,504, [b]N: 2,160. Group A: IA in month 1. Group B: IA in month 3 with announcement at first meeting. Group C: IA in month 3 without announcement. Reference group: IA in month 6. Significance levels: *: 10-percent, **: 5-percent, ***: 1-percent. Individual controls included but not shown: age, nationality, education, previous wage, handicap, previous employment history.

Appendix 2. Example of an IA



Ziel-/Eingliederungsvereinbarung — Bundesagentur für Arbeit

Agentur für Arbeit

zwischen

und

Es werden aufgrund der Zielsetzung nachfolgende Aktivitäten bis zum nächsten Termin verbindlich vereinbart

Zielsetzung	Nächster Termin am
Aufnahme einer Tätigkeit als Physiotherapeutin durch bundesweite Arbeitsplatzsuche	Spätestens nach 2 Monaten (entsprechend KKK)

Vereinbarte Aktivitäten des Kunden bis zum nächsten Termin

- Überprüfen Sie Ihr soeben erstelltes und veröffentlichtes Bewerberprofil unter www.arbeitsagentur.de mit der Referenznummer …
- Informieren Sie sich im Internet über die richtige Bewerbungsstrategie (z.B. www.bewerbungsdschungel.de).
- Erstellen Sie einen Qualifizierungsplan mit den Inhalten, die Ihnen nach Ihrer persönlichen Einschätzung zur erfolgreichen Berufsausübung fehlen und leiten mir diesen per Mail bis zum … (1 Woche) zu.
- Bewerben Sie sich bundesweit als Physiotherapeutin auf mindestens 10 ausgeschriebene Stellen pro Monat.
- Bieten Sie bei Ihren Bewerbungen ein Einarbeitungs-/Qualifizierungspraktikum von bis zu 8 Wochen an. Melden Sie sich vor Beginn des Praktikums telefonisch im Service Center (Tel.-Nr.: …), damit wir die erforderlichen Formalitäten erledigen können.
- Erstellen Sie bis zum nächsten Beratungsgespräch einen Aktionsplan, welche weiteren Aktivitäten Sie bis wann unternehmen wollen, um Ihre Arbeitslosigkeit zu beenden und bringen ihn zum Gespräch mit.
- Nutzen Sie auch weiterhin das Angebot von Jobbörsen im Internet, zum Beispiel unter www.arbeitsagentur.de - Informationen für Arbeitnehmer - Arbeitsuche - Arbeitsmarktportal.
- Bitte führen Sie eine Gesamtübersicht über Ihre Bewerbungsaktivitäten und senden Sie mir diese monatlich per Mail zu (E-Mail-Adresse) oder geben Sie diese in der Eingangszone der Agentur für Arbeit ab. Datum der Bewerbung, Unternehmen, Art der Bewerbung und Stand sollten aus der Gesamtübersicht hervorgehen (siehe Beispielübersicht in der „Job-Profi-Mappe"). Erster Termin ist der …
- Bitte bringen Sie zu allen persönlichen Beratungsgesprächen mit Ihrem Vermittler die aktualisierte schriftliche Gesamtübersicht über Ihre Bewerbungsaktivitäten mit.

- Bitte teilen Sie uns unbedingt sofort mit, wenn sich Ihre Adresse, E-Mailadresse, Telefonnummer oder Handynummer ändert. Denn wir rufen Sie an, wenn wir passende Stellen für Sie haben!

Aktivitäten der Agentur für Arbeit

- Wir veröffentlichen Ihr Bewerberprofil im Internet unter www.arbeitsagentur.de. Sie finden es unter der Referenznummer … (Pfad: Arbeitgeber – Bewerber suchen – Referenznummernsuche).
- Sollten wir für Sie passende Stellen als Physiotherapeutin haben, rufen wir Sie an. In Einzelfällen senden wir Ihnen auch direkt ein Stellenangebot zu.
- Wir unterstützen Sie finanziell während eines selbst gesuchten Praktikums im Rahmen einer „betrieblichen Trainingsmaßnahme" (max. 8 Wochen).
- Unter bestimmten Voraussetzungen können finanzielle Unterstützungen, wie z.B. Bewerbungen, Reisekosten zu Vorstellungsgesprächen innerhalb der Bundesrepublik Deutschland gewährt werden.
- Die Internetarbeitsplätze sind während folgender Öffnungszeiten … in der Agentur für Arbeit …(z.B. im BerufsInformationsZentrum…) kostenlos nutzbar. Hier können Sie auch Bewerbungen schreiben und ausdrucken.

Die Ziel-/Eingliederungsvereinbarung wurde mit mir besprochen, ein Exemplar habe ich erhalten. Ich verpflichte mich, die vereinbarten Aktivitäten einzuhalten und beim nächsten Termin über die Ergebnisse zu berichten.

Datum

Unterschrift des Kunden/ der Kundin

Unterschrift des Vermittlers/ der Vermittlerin

Slightly abridged translation of the example of an IA:

Objective: Taking up employment as a physiotherapist through nationwide job search;
Next appointment: The latest after 2 months.
Bindingly agreed activities of the client until the next appointment:

- Check your recently created and published profile at www.arbeitsagentur.de with the reference number,
- Inform yourself about application strategies on the internet (e.g. www.bewerbungsdschungel.de),
- Create a qualification plan with the items that are in your opinion missing for a successful integration and send it to me by mail until (...),
- Apply nationwide as a physiotherapist by at least 10 vacancies per month.
- In your applications, offer to work as a training-/qualification intern for up to 8 weeks. Before starting the internship, contact the service center by phone (...), so we can complete all required formalities.
- Until the next consultation create an action plan which includes how and until when you want to undertake other activities to leave unemployment, and bring this to the consultation.
- Continue using internet job search engines, for example at www.arbeitsagentur.de.
- Please conduct an overview on your application activities and send it to me by e-mail every month or leave it in the entrance zone of the labor market agency. The overview should contain the date of application, the organization, the kind of application and the state of the application (you find an example at ...). The first date for this is (...).
- To all personal consultations, please bring with you the actual complete overview of your application activities.
- If your address, e-Mail, phone number or mobile phone number changes, please let us know as soon as possible. After all, we will call you when we have found an appropriate vacancy!

Activities of the labor market agency:

- We publish your applicant profile on the internet at www.arbeitsagentur.de. You will find it under the reference number (...)
- Should we find an appropriate vacancy for a physiotherapist for you then we will call you. In applicable cases we directly send you a job offer.
- We support you financially during your internship in a company-based training program (max. 8 weeks).
- Under certain conditions, financial support can be granted, e.g. for applications, travel expenses for personal interviews within Germany.
- Computers can be used free of charge in the labor market agency during the following opening hours (...). Here you can also write and print your applications.

The integration agreement was discussed with me and I received a copy. I oblige myself to comply with the agreed activities and to report the results at the next appointment.

Appendix 3. Announcement of future IA (treatment arm B)

Bundesagentur für Arbeit

Informationsblatt zur Eingliederungsvereinbarung

Wenn Sie innerhalb von drei Monaten seit Beginn ihrer Arbeitslosigkeit keine Beschäftigung aufnehmen, wird Ihre Arbeitsvermittlerin/ Ihr Arbeitsvermittler nach diesen drei Monaten mit Ihnen eine Eingliederungsvereinbarung abschließen.

In der Eingliederungsvereinbarung legt Ihre Arbeitsvermittlerin/ Ihr Arbeitsvermittler mit Ihnen folgendes fest:

- Ihr Eingliederungsziel,
- die Vermittlungsbemühungen der Agentur für Arbeit,
- welche Eigenbemühungen zur beruflichen Eingliederung Sie in welcher Häufigkeit mindestens unternehmen müssen und wie Sie diese nachweisen,
- die vorgesehenen Leistungen der aktiven Arbeitsförderung (vgl. § 37 Abs. 2 SGB III).

Pflichten aus der Eingliederungsvereinbarung

Ein Anspruch auf Arbeitslosengeld setzt generell voraus, dass Sie alle Möglichkeiten zur beruflichen Eingliederung nutzen. Hierzu gehört auch, dass Sie die Verpflichtungen der Eingliederungsvereinbarung erfüllen. Mit der Verpflichtung, sich aktiv um eine Beschäftigung zu bemühen, hat der Gesetzgeber betont, dass in erster Linie Sie gefordert sind, Ihre Beschäftigungslosigkeit zu beenden. Ihre Arbeitsvermittlerin/Ihr Arbeitsvermittler wird Sie dabei beraten und unterstützen.

Aktivitäten im Rahmen der Eingliederungsvereinbarung können z.B. schriftliche Bewerbungen, die Auswertung von Stellenanzeigen in Zeitungen, Fachzeitschriften und anderen Medien, Vorsprachen bei Betrieben, die Arbeitsplatzsuche per Inserat, die Nutzung der JOB-BÖRSE unter www.arbeitsagentur.de, der Besuch von Arbeitsmarktbörsen und ähnliches sein.

Welche konkreten Aktivitäten Sie im Rahmen der Arbeitsuche unternehmen bzw. wie Sie Ihre Eigenbemühungen nachweisen müssen, entnehmen Sie Ihrer Eingliederungsvereinbarung bzw. der schriftlichen Festsetzung Ihrer Eigenbemühungen. Erbringen Sie die Pflichten im Zusammenhang mit den Eigenbemühungen nicht, nicht rechtzeitig oder nicht vollständig, tritt eine Sperrzeit ein. Die Dauer einer Sperrzeit bei unzureichenden Eigenbemühungen beträgt zwei Wochen.

Wollen Sie die Pflichten aus der Eingliederungsvereinbarung nicht erfüllen bzw. keine Eigenbemühungen unternehmen, haben Sie keinen Leistungsanspruch bzw. kann Ihr Leistungsanspruch – gegebenenfalls rückwirkend – entfallen.

Translation of the Announcement:

If you do not take up employment within three months since unemployment start, after these three months your caseworker will conclude an integration agreement with you. In the integration agreement, your caseworker will determine with you

- your integration goal,
- supporting activities of the labor market agency,
- the efforts you must undertake for occupational integration, also their frequency and verification,
- your planned participation in active labor market programs.

Obligations from the integration agreement
An entitlement to unemployment benefits generally requires that you utilize all opportunities for your occupational integration. This includes fulfilling the obligations from the integration agreement. With the obligation to actively seek employment, the legislation emphasizes that it is mainly yourself who is responsible for your unemployment exit. Your caseworker will advise and support you.

Activities in the framework of the integration agreement may be e.g. written applications, searching for job advertisements in newspapers, journals and other media, auditions at companies, job search by ad, using the job search engine (Job-Boerse) at www.arbeitsagentur.de and other job search engines, and so on. Which particular activities you have to undertake during job search and how you have to verify your efforts will be documented in the integration agreement. If you do not perform your duties on necessary search efforts (not in time or not completely), a cutoff-period of benefits will take place. The duration of a cut-off period due to inadequate search efforts amounts to two weeks.

If you do not fulfill the obligations arising from the integration agreement or do not undertake search efforts, you are not entitled to benefit receipt and your benefits entitlement can - eventually backdated - be omitted.