Disincentive Effects of Unemployment Benefits and the Role of Caseworkers*

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Abstract

A large literature has documented that the unemployment duration of unemployment insurance (UI) recipient increases with the generosity of the UI system. This has been interpreted as the disincentive effect of UI benefits, however, unemployed workers typically also have caseworkers assigned who are monitoring and assisting the job search efforts. These caseworkers may respond to differences in UI eligibility by shifting resources (financial or time) between unemployed individuals in order to counteract the moral hazard effect of UI benefits or in order to focus resources to where they have the largest effect. This suggests that the typical estimates of the disincentive effects of UI may be biased in studies that compare workers within the same UI agency. We estimate whether caseworkers respond to the generosity of UI using a regression discontinuity (RD) design in Germany, where potential UI durations vary with age. We show that across a wide variety of measures, such as meetings, sanctions, and training programs UI caseworkers do not treat unemployed with different eligibility differently. At best we find a very small effect that workers with shorter eligibility close to the exhaustion point are more likely to be assigned to training programs that prolong their UI eligibility. The typical RD estimates of the UI disincentive effects thus seem to be valid estimates.

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1 Introduction

A large literature has documented that unemployed workers who face more generous unemployment insurance (UI) benefit levels and eligibility durations take longer to return to employment. This has typically been interpreted as the moral hazard effect of UI benefits[1], where individuals respond to the reduction in the marginal benefit of finding a job relative to remaining unemployed by lowering their search effort. Unemployed workers, however, are also influenced by caseworkers of the UI agencies, who are typically tasked with monitoring job search effort as well as with providing various types of support to the unemployed, such as job referrals, various active labor market programs or advice and motivation. Since caseworkers have limited resources in terms of time, financial means for active labor market programs and vacancies that they can refer workers to, they may respond to differences in UI eligibility across workers by targeting their efforts. If these efforts then have a direct effect on unemployment durations, this could lead to substantial biases in estimates of the disincentive effects of UI benefits. Some of the most convincing empirical estimates of the effect of UI eligibility are based on regression discontinuity designs (Lalive et al., 2006; Card et al., 2007a; Lalive, 2008; Schmieder et al., 2012a) and regression kink designs (Landais, 2015; Card et al., 2015a,b). However, these are precisely the settings where the potential bias from caseworker responses to UI eligibility may be particularly severe, since these designs effectively compare workers in the same labor market and plausibly within the same caseworker.

In this paper we provide the first estimates whether UI caseworkers respond to differences in UI eligibility across unemployed workers. We use the fact that in Germany potential UI benefit durations are a function of the exact age of claiming UI benefits where durations increase discontinuously from 12 to 15 months at age 50. As has been shown in other studies (Schmieder et al., 2012a, 2016) for earlier time periods in Germany, this discontinuity gives rise to a substantial decrease in job finding hazards and a corresponding increase in unemployment durations. Using the universe of social security data in Germany merged to a unique new dataset on active labor market program participation, caseworker activities (such as personal meetings with the unemployed, the signing of integration contract, and vacancy referrals), and sanctions, we analyze whether caseworkers respond to the increase

[1] Or a combination of a moral hazard and income effect (Chetty 2008).

in UI eligibility at the age threshold. An important aspect of the empirical analysis is that while UI eligibility is determined at the start of the UI spell, caseworkers interact with the unemployed throughout the entire unemployment spell and may assign them into different programs at various points in time. In the empirical part we therefore carefully deal with this dynamic aspect of the caseworker actions and we present both cross-sectional results - showing the effect of UI eligibility on total program participation and caseworker interactions - as well as dynamic results, showing the effect by month since the start of the UI spell.

A priori, UI eligibility can affect how caseworkers allocate their resources in various ways, depending on the objective function of the caseworker as well as the effectiveness of the resources the caseworker has at her disposition. For example, caseworkers might be trying to minimize the average time until their assigned cases get back to work. If assignment into active labor market programs increases the job finding rate among the unemployed, then the caseworker would focus these resources on workers who are most responsive to these programs, which might be individuals with shorter durations who have more to gain from participating in these programs, or it might be individuals with longer UI durations, perhaps because the threat of a training program might be particularly effective for them to overcome the larger disincentive effect from UI. Similarly, other caseworker instruments like sanctions, monitoring or wage subsidies might be complements or substitutes with UI benefits which could lead to positive or negative correlations between caseworker activities and UI generosity. Furthermore, caseworkers might also have other objective functions, which can also have ambiguous effects on how they allocate their resources. For example caseworkers might be maximizing something like a Rawlsian social welfare function and thus attempt to focus their resources on the person with lower UI benefits in order to compensate them for the disadvantage of lower benefits. Furthermore caseworkers could be focused on assigning workers to active labor market programs (ALMP) who are most likely to find a job afterwards (with or without the training program), thus leading to the appearance that the program was successful.[2] In short, depending on the type of caseworker instrument, the objective function of the caseworker, and the complementarity between UI benefits and caseworker actions, the effect of UI generosity on caseworker actions is ambiguous. In the next section, we will lay out these various ways caseworkers might be motivated to allocate

[2]See Heckman et al. (1997) and Bell and Orr (2002) for evidence and discussion of such a "creaming" effect in training programs.

their resources and derive some empirical tests from them.

The paper is related to the large literature on the effectiveness of active labor market programs and other dimensions of caseworker actions. Card et al. (2010) and Card et al. (2015c) provide an excellent overview of the literature. While results are heterogeneous across studies and some programs, such as public employment programs, may not be particularly effective, others, such as job search assistance and some training programs, seem to succeed in getting workers back into jobs over the short or medium run. Other papers (e.g. Berg et al., 2004; Abbring et al., 2005; Lalive et al., 2005; Svarer, 2007; Arni and Schiprowski, 2016), have evaluated the use of sanctions and found that they are quite successful in increasing transition rates back into employment. In a recent paper, Schiprowski (2018), shows that simply meeting with a caseworker has a positive effect on job finding rates in the Swiss UI system. A growing body of research has also documented that often the threat of being sent to a training program may be enough to induce individuals to return to work, thus seemingly serving as a monitoring device (e.g. Black et al., 2003; Giulietti et al., 2011). Overall, there is substantial evidence on the effectiveness of different caseworker actions, but less is known about the motivations and goals of caseworkers. Boockmann et al. (2013) provide survey evidence on caseworkers' motivations and strategies, but do not analyze quantitatively what factors influence caseworker actions. In contrast we investigate whether caseworkers respond to specific institutional features of the UI system, using a well defined natural experiment.

This paper complements the research estimating the disincentive effects of UI benefits. While this is a long line of research (with early seminal papers such as Moffitt, 1985 or Meyer, 1990), it has received a lot of new interest with the advent of large administrative datasets and the rise of econometric methods like regression discontinuity (e.g. Lalive, 2008; Lalive et al., 2006; Card et al., 2007a; Schmieder et al., 2012a,b; Johnston and Mas, 2015) and - more recently - regression kink designs (Landais, 2015; Card et al., 2015a,b).[3] The advantage of these designs is that they exploit policy features to isolate variation in UI generosity which is plausibly orthogonal to individual characteristics (observed and unobserved) and uncorrelated with other policy parameters, in many ways almost mimicking a randomized controlled trial. However these papers do not observe to what extent caseworkers and employment agencies themselves are responding to the policy variation by shifting various resources between different groups of workers. The nature of these designs suggests that the

[3]See Schmieder and Von Wachter (2016) for a review of this recent literature.

workers in the implicit treatment and control groups may well be assigned to the same caseworker, which implies that the problem of bias from caseworker actions may be particularly severe. Without information on caseworker activities it is impossible to gauge the possible magnitude from this bias, however the possibility that actions by intermediaries (with good intentions) may severely complicate the interpretation of differences between individuals with different 'treatments' has been recognized since the early days of statistical research such as in Student's (1931) famous critique of the Lanarkshire milk experiment. The main contribution of this paper is therefore to analyze the validity of the implicit assumption in these studies that caseworker actions are not confounding the estimates of the disincentive effects of UI, at least in the specific context that we are studying here.

The next section will discuss a simple framework for understanding the role of caseworkers and how they may bias estimates of the disincentive effects of UI. Section 3 provides institutional background on the role of UI caseworkers in Germany and describes our data. In section 4 we show based on what characteristics the unemployed are assigned to caseworker teams and provides evidence that such teams are resource constraint. In section 5 we explain our empirical method based on an age discontinuity in Germany, provide evidence that this gives rise to a valid regression discontinuity design and present the main empirical results. Section 6 concludes.

2 Theoretical Framework

We describe a simple stylized framework here to illustrate how the actions of caseworkers may bias estimates of the disincentive effects of UI and what would determine the direction of the bias. Consider a world where individuals i are unemployed at the beginning of the (single) period. The unemployed search for a job and choose search effort e_i for which they face a cost of $\psi(e_i)$. If they do find a job, they immediately start working and receive a wage w, otherwise they are unemployed for the period and receive UI benefits b_i, which may vary across individuals. The utility function is given as $u(.)$. Workers are subject to 'treatment' by a caseworker measured as ω_i, which encompasses any kind of actions at the disposal of the caseworker, such as job search monitoring, vacancy referrals or ALMP. Furthermore the probability s_i of finding a job is a function of effort and caseworker resources: $s_i = s(e_i, \omega_i)$.

Workers are maximizing expected utility:

$$V_i = \max_{e_i} \ s(e_i, \omega_i)\, u(w) + (1 - s(e_i, \omega_i))u(b_i) - \psi(e_i)$$

The first order condition for the worker is

$$\frac{\partial s(e_i, \omega_i)}{\partial e_i} \left(u(w) - u(b_i)\right) - \psi'(e_i) = 0$$

This function implicitly defines an optimal search effort function $\xi(b_i, \omega_i)$, so that workers are finding a job with probability:

$$s_i = s\left(\xi\left(b_i, \omega_i\right), \omega_i\right) \equiv \tilde{s}(b_i, \omega_i) \tag{1}$$

Consider the following linear approximation of equation (1):

$$s_i = \eta\, b_i + \pi\, \omega_i + X_i \beta + \varepsilon_i \tag{2}$$

The empirical literature focusing on the disincentive effect of UI typically estimates a version of equation (2) that omits the ω_i-term in order to identify a parameter such as η, the effect of UI benefits on the job finding hazard.[4] Typical estimation strategies such as RDD or RKD are focused on isolating variation (by selecting appropriate samples and controls X_i) such that $Cov(b_i, \varepsilon_i | X_i) = 0$. However, if caseworkers allocate resources across unemployed workers taking b_i into account this would lead to $Cov(\omega_i, b_i | X_i) \neq 0$. In this case, as highlighted by equation (2), not controlling for ω_i will lead to an omitted variable bias when estimating η.

Suppose the allocation of resources among the unemployed can be described using the following linear model:

$$\omega_i = \delta\, b_i + X_i \gamma + \epsilon_i \tag{3}$$

If $\delta \neq 0$, then estimation of equation (2) without controlling for ω_i will lead to a biased estimate of η. The sign of δ (and π) will determine the sign and the magnitude of this bias.

To see what kind of values δ might plausibly take, consider a situation where each caseworker is assigned two unemployed individuals $i = 1, 2$. Furthermore the overall amount of

[4] There are papers analyzing the effect of ALMP on unemployment durations that include UI eligibility as a control variable but the empirical design in these papers typically does not allow for interpreting this parameter causally.

resources (time, money, vacancies, ...) of the caseworker are limited to $R = \omega_1 + \omega_2$. The caseworker chooses an allocation of his resources to maximize an objective function subject to this budget constraint:

$$\max_{\omega_1,\omega_2} = W(\omega_1, \omega_2)\, s.t.\, R = \omega_1 + \omega_2$$

How caseworkers allocate their resources will depend crucially on the objective function W, as well as on how resources actually affect the unemployed. We will consider several stylized cases and highlight in each of these cases how resource allocation would be correlated with benefit levels.

First, consider the **unemployment-minimizer**, who wants to reduce average unemployment durations as much as possible. Such a caseworker can be described as maximizing the objective function $W(\omega_1, \omega_2) = s_1 + s_2 = \tilde{s}(b_1, \omega_1) + \tilde{s}(b_2, \omega_2)$. If b_i and ω_i are substitutes in $\tilde{s}(.)$, that is if $\frac{\partial^2 \tilde{s}}{\partial b_i \partial \omega_i} < 0$, then at the optimum ω_i will be negatively related to UI benefits. On the other hand if b_i and ω_i are complements, they will be positively related. The assumption of substitutes may be plausible in the case of job search monitoring or sanctions: a worker with low UI benefits may already be searching relatively hard for jobs, with little additional increase in the job finding probability if he is monitored or sanctioned more, while a worker with high UI benefit levels may be more responsive. On the other hand b_i and ω_i may plausibly be complements in the case of training programs or wage subsidies. A worker who is very motivated (due to low UI payments) may be more willing to learn new skills in a training program or to look for jobs where he might benefit from a wage subsidy. Therefore if caseworkers are trying to minimize overall unemployment durations, this would lead to a correlation between benefit generosity and caseworker resources, where the sign will depend on the search effort function of the worker.

Next, consider the caseworker who wants to maximize **perceived resource efficiency**, where we define perceived resource efficiency as the unconditional correlation between the job finding probability and caseworker resources. The idea is that a naive principal who is overlooking the caseworker might use this correlation as a simple measure for whether caseworkers are using their resources efficiently. The German UI agency for example regularly publishes statistics (on the level of local UI agencies) of the fraction of people who are participating in ALMP who then find a job within a year. Such an objective function can be expressed as: $W(\omega_1, \omega_2) = s_1\omega_1 + s_2\omega_2$. With such an objective function the caseworker would - at least to a first order approximation - simply focus resources to the person where $\tilde{s}(b_i, \omega_i)$ is the highest, which may (though not necessarily) be the opposite of what the unemployment-minimizer would do. Since search effort is decreasing in b_i, this would tend to generate a negative association with caseworker resources.

Third, consider the **welfarist** caseworker, who is maximizing a social welfare function defined over individual utilities of the unemployed. This could include a Rawlsian social welfare function such as: $W = min\{V_1(\omega_1), V_2(\omega_2)\}$ or a Utilitarian social welfare function such as: $W = V_1(\omega_1) + V_2(\omega_2)$. In the Rawlsian case, the caseworker would tend to focus resources on the disadvantaged unemployed with low UI benefits, leading again to a negative relationship between b_i and ω_i, while in the Utilitarian case the sign is ambiguous and, depending on whether b_i and ω_i are complements or substitutes, may go in the opposite direction.

Finally, consider the **bureaucratic** caseworker, who simply follows guidelines (explicit or implicit) where resources are allocated according to observable worker characteristics X_i. For example all workers are treated equally, or workers are supported / monitored purely based on observable characteristics such as age, education, qualification etc. In this case we have that: $Cov(\omega_i, b_i|X_i) = 0$, and the typical estimates of η would not be biased.

The main goal of this paper is to provide consistent estimates of δ in equation (3), which will then allow us to estimate the bias from estimating equation (2) without controlling for ω_i. Furthermore, as can be seen from the discussion, the sign of δ is informative about the objective function of the caseworkers and may provide insights into the likely complementarity of ω_i and b_i in the search effort function. It should be noted that in practice caseworker resources are a vector with many components and that it is certainly possible that caseworkers have differing objectives for different resources and that the complementarity between ω_i and b_i varies across different types of caseworker actions. We will get back to this in the interpretation of our empirical results.

3 Institutional Background and Data

3.1 Unemployment Insurance in Germany

Workers in Germany are eligible to receive UI benefits if they are unemployed and have been employed for at least 12 months in the previous two years. The replacement rate is 60 percent of the pre-unemployment wage (67 percent if the person has children) up to a wage of 5300 Euro per month in 2008.[5] Eligibility duration depends on the number of months worked in the previous 5 years before claiming UI benefits, as well as the age at the time of claiming. From 2008 onwards, the maximum potential benefit duration (PBD) of UI was 12 months for individuals below age 50 at the time of claiming UI benefits, 15 months for individuals age 50 to 54, 18 months for individuals age 55 to 57 and 22 months for individuals age 58 and older.[6] We will focus on workers who are close to the age 50 threshold when they claim UI benefits and who have worked for at least 30 months in the previous 5 years so that they are eligible to the maximum benefit duration.[7]

After individuals exhaust UI benefits the unemployed can apply for a means tested second tier benefit level called UI benefits II (UIB II).[8] The monthly benefits in this second tier program are around 370 Euro per person in addition to rent and health insurance. Individuals who are working while receiving UIB II face implicit marginal tax rates of around 90 percent. Depending on pre-unemployment income, asset levels and possibly spousal income, benefit levels in UIB II may be anywhere between substantially lower than regular UI benefits to quite similar.

Workers who quit their jobs, lose them at their own fault, or fail to register for job search prior to the UI claim may be subject to sanctions of varying durations, that is periods where

[5] The cap is lower in East Germany and increases over time, roughly at the inflation rate. The maximum wage that counts for UI benefit calculations is located approximately at the 85th percentile of the wage distribution, but rarely binding for the unemployed who typically come from lower paid jobs.

[6] The maximum PBD is the maximum duration someone can be eligible for at the beginning of the spell. This maximum can implicitly increase throughout the spell through participation in active labor market programs, since while in such a program the unemployed continue to receive UI benefits, but use up only 1 day of PBD per 2 days of program participation. We will get back to this in the empirical section.

[7] Below age 50, working for 24 months in the previous 5 years would be enough to qualify for the maximum of 12 months of benefits, but in order to have a comparable sample we impose the 30 months requirement on both sides of the cutoff. If individuals had UI spells in the previous 5 years the clock for calculating UI benefits would be reset and we therefore require that they have worked for at least 30 months since the end of that UI spell.

[8] UIB II was introduced during the Hartz reforms in 2003/2004 which merged the older systems of unemployment benefits and unemployment assistance. See Eichhorst and Marx (2011) for a detailed description of the reforms.

they do not receive UI benefits. We restrict our sample to individuals who receive UI benefits within less than 3 months after job separation, which excludes individuals with sanctions due to voluntary quits.

3.2 Caseworkers

Unemployed individuals are required to register for job search when they get notified of a lay-off and have to appear at their local employment agency (Arbeitsagentur) in order to apply for UI benefits before they can claim them.[9] At the first appointment they are assigned a caseworker whose task it is to process the benefit application, to advise and support the unemployed on job search, and to monitor job search efforts. Support can come in the form of simply discussing potential job options and application strategies, as well as by offering participation in active labor market programs (such as training programs, public employment programs or wage subsidies for potential jobs). Employment agencies also offer a platform for vacancy postings and they are used directly by employers to look for job candidates. Caseworkers have access to vacancies offered by employers and may refer them to specific unemployed workers. Apart from these supportive measures caseworkers monitor job search and can sanction individuals with benefit cuts if they fail to comply with search requirements. The duration of these cuts varies from one week (for example for delayed job search registration) to up to twelve weeks (for voluntary job quits). Sanctions are also used to punish unemployed that do not use the offered support by the caseworker. Refusing to participate in an active labor market program, canceling a started program or rejecting a vacancy referral can be punished with a benefits cut of three weeks. Lack of individual initiative in looking for a job can be punished with a cut of two weeks. The duration of benefit cuts increases with the number of sanctions. For example, the first two times a worker refuses a vacancy referral benefits are cut for three weeks, but this goes up to twelve weeks after the third refusal. Benefit cuts from different sanctions are additive up to a total duration of benefit cuts of 21 weeks, when benefits are cut completely (see Hofmann, 2012).

During the first meeting the caseworker assesses how easy it will be for the unemployed to find back into employment and assigns them to one of various profiles. These profiles may then be used to guide integration strategies by the local UI agency, though there is a lot of

[9]There are 160 employment agency districts, each comprising around 2-3 counties. Typically there is one central agency in a district as well as several smaller local branches.

freedom remaining how caseworkers may use them. Caseworkers and unemployed typically meet in regular intervals to discuss job search progress, devise new application strategies and, potentially, to monitor search efforts by the unemployed. The frequency of these meetings is up to the caseworker.[10]

Since the Hartz Reforms, caseworkers and the unemployed are required to sign a so-called integration contract (see Schütz et al., 2011), an agreement that specifies the expectations of what the unemployed worker is supposed to do each week in order to find a job as well as the support she gets from the caseworker. On the worker side, it can for example specify the number of applications she agrees to send in a given time. On the caseworker side, it can specify the funding of a targeted training program or to cover costs related to the application process. The contract usually specifies the target occupation and task, as well as the regional area in which the worker is looking for jobs. These contracts are legally binding for both sides and are supposed to be updated every six months.

Caseworkers have a variety of active labor market programs at their disposal. We analyzed a wide range of programs (all with similar results) but focus on the two largest types of programs during our time period: training programs and job placement services. Training programs encompass a range of programs designed to teach workers how to find a job or to help them acquire new job related skills. This ranges from short job coaching seminars focused on job applications and what types of jobs a worker may be qualified for all the way to longer vocational training programs (up to 10 months), similar to apprenticeships in that they combine education in a vocational school with practical experience in workplaces. Job placement services are external providers (that is firms that contract with the Federal Employment Agency) of job search assistance that provide a range of services to help workers find jobs through individual coaching and mentoring.

Local UI agencies organize caseworkers into teams and UI recipients are first assigned to both a team and a specific caseworker within a team. The role of the teams is to coordinate work between the caseworkers, to fill in for absences of caseworkers, and they may have joint resource constraints. We will discuss the role of these teams and how workers are assigned to them in more detail in the next section.

[10]This is consistent with a wide range of meeting frequencies that we find between unemployed individuals in our data, ranging from 0.44 meetings per month (at the 75th percentile) to 0.2 meetings per month (at the 25th percentile).

3.3 Data

We use data from the Integrated Employment Biographies (IEB) of the German Social Security system, which covers all social security liable employment relationships in Germany as well as information on unemployment benefits and registered job search status. We extract a sample of all individuals who enter UI between April 2008 and June 2010 who are between age 45 and 55 at the age of claiming UI and who are eligible to the maximum benefit duration of their age group.[11] For the main regression analysis we furthermore restrict the sample to a 2 year window around the age cutoff. For each UI spell we know the duration of receiving UI benefits, the duration of registered job search and nonemployment duration, that is time until the next employment spell that is covered in the dataset.[12] Furthermore we have information on participation in active labor market programs, and can infer sanctions from UI benefit spells where no benefits are paid.

We supplement the IEB with 3 additional data sources. First we obtained information on direct interactions with caseworkers along three dimensions: invitations for person appointments at the local UI agency, vacancy referrals and integration contracts.[13] Second, we also obtained data on the integration profile assignments by the caseworkers. Finally we obtained identifiers for the teams that UI recipients were assigned to.[14]

Table 1 reports summary statistics for the main characteristics of our sample. Column 1 shows the full sample of all UI claims of individuals aged 48 to 51 at the time of claiming, including individuals who have shorter eligibility durations which we do not use for our RD sample. Column 2 shows the sample restricted to individuals in our RD sample, that is individuals who have worked for at least 30 months in the preceding 5 years. We lose around 10 percent of the observations, but most characteristics remain very similar. The restricted sample is somewhat more positively selected with higher pre- and post-unemployment wages. Splitting the analysis sample into individuals above (column 3)and below (column 4) the age cutoff reveals differences between the two age groups that, as we will show later, are driven by

[11] Our data reaches until the end of 2012. We start in April of 2008 to avoid some manipulation that occurred right around the January 2008 reform where people seemed to have delayed UI claiming until the reform. Using inflows until June 2010 allows us to observe all individuals for at least 18 months within our time window and thus for the entire covered UI spell for the workers above the threshold.

[12] In particular self-employment as well as some government jobs are not subject to social security contributions and not included in the data.

[13] More information on these variables is available in Hofmann and Köhler (2014).

[14] Unfortunately, we were not able to obtain identifiers for the individual caseworkers nor the number of caseworkers that are part of a team.

age gradients, highlighting the importance of isolating the variation at the age discontinuity.

4 Caseworker Team Assignments and Team Resource Constraints

4.1 The Assignment of Unemployed Workers to Caseworker Teams

Based on conversations with caseworkers, it appears that the formation of teams varies substantially across UI agencies and can take different forms. For example in some UI agencies teams may specialize on different geographic regions (say different cities within the UI agency district), in others teams may be formed to specialize on different education levels of the UI recipients (high vs. low skill) or different industries (manufacturing vs. service). Finally, there may be UI agencies where teams are not particularly specialized and simply serve as a way of organizing caseworkers into smaller administrative units. We are not aware of systematic studies or data sources about how teams are formed at the local level. To investigate this we created a dataset consisting of all UI inflows from April 2008 to June 2010 and calculated average Team-by-Quarter characteristics of the new UI claimants. There is quite a bit of heterogeneity in team sizes with most teams having around 100 to 500 new UI claimants each quarter. The average unemployed in our sample is in a team with a total of around 360 UI inflows per quarter, around 73 of which are in the age range of 45 to 55. (See Table 1)

Using this team level dataset, we calculated a number of statistics, such as Dissimilarity Indices and Intra Class Coefficients to gauge whether the assignment of workers within UI agencies to individual teams appears to be random or systematic. We provide details of these tests in the Online Appendix but the key take-away is that the assignment is clearly not random and there is substantial sorting among observable characteristics such as education, pre-unemployment wages and age.

Given the structure of, often, specialized teams, it is not surprising that UI recipients are not randomly assigned to caseworkers or teams. Regarding our research question, a related important question is therefore whether workers are systematically assigned to different caseworkers or different teams above and below the age cutoff and whether this could potentially explain different job finding rates around the threshold. Given the nonrandom assignment to teams, this seems ex-ante quite possible. In order to investigate this, we calculated for each worker the average characteristics of the other individuals in their team excluding the worker herself, i.e. the leave-out-mean of these variables.

Figure 1 shows RD figures around the age threshold using our analysis sample and showing leave-out-means of Team-by-Quarter UI inflow characteristics as dependent variables. Suppose for example that workers on the right of the age threshold were systematically assigned to different teams, for example if some teams consisted only of workers above age 50, then we would expect the average team age to jump discontinuously at the age threshold. Figure 1 a) suggests that this is not the case as workers just above or below age 50 are in teams with the same average age. Similarly the other Panels in Figure 1 show that the team composition of workers above and below the age threshold are virtually identical with no discernible jump in demographics or job search outcomes at the threshold.

This shows that even though team assignment (and thus likely caseworker assignment) is nonrandom, teams are not formed systematically by age of the UI claimants in a way that would lead UI claimants above and below the age threshold to be in different teams. This is also in line with the fact that around 97% of workers are in teams that have UI claimants above and below age 50 (Table 1).

4.2 Are Caseworkers Resource Constrained?

Another key question is whether caseworkers do in fact face resource constraints. If caseworkers had unlimited resources in terms of funding for ALMP, vacancies that they can refer workers to and time for meetings, monitoring etc. then we would not expect UI eligibility to affect the amount of resources a worker receives from their caseworker. Since we do not observe caseworker identifiers, we analyze the question of resource constraints at the team level. If teams are facing resource constraints, then we would expect that the average resources workers receive from their caseworkers is negatively related to the total caseload of the team. We test this by estimating the following regression on the team level:

$$\ln(y_{it}) = \ln(caseload_{it}) + \theta_i + \gamma_t + X_{it} + \varepsilon_{it}$$

where y_{it} is an outcome variable such as the average number of vacancy referrals a worker received per month in team i at time (in quarters) t. $caseload_{it}$ is the number of UI inflows in team i at time t. We control for team fixed effects θ_i and time fixed effects γ_t. Since caseload may be correlated with the type of workers who become unemployed we also show results where we control for the composition of the UI claimants using a vector of demographics X_{it}, such as the share of women among new UI claimants in team i at time t, average education and similar variables.

Table 2 shows results from these regressions. Panel A shows that there is a clear negative relationship: as the number of UI claimants rise in a team, the number of referrals decreases. The estimate is a bit smaller when we control for observable worker characteristics but the point estimate still negative and highly statistically significant. Similarly, Panel B and C show that a larger caseload also reduces the number of invitations workers receive as well as the incidence of an integration contract. The last two panels focus on the biggest active labor market programs. Both the number of days in education programs and the number of days a worker spends in a placement program is significantly reduced as UI inflows increase.

These regressions suggest that caseworker teams that face larger caseloads spend less time and resources on individual workers and presumably have to make decisions regarding how to allocate their scarce resources.[15]

There are two key takeaways from this section: First: workers above and below the age threshold are on average in the same teams and most teams do in fact have workers on both sides of the cutoff. Furthermore teams do appear to be resource constrained, and thus forced to make decisions about how to allocate scarce resources. This seems to make it ex ante plausible that caseworkers would base these allocations in part on the potential benefit duration of the unemployed, which we will investigate in the next section.

5 The Effect of UI Extensions on Worker and Caseworker Behavior

5.1 The Regression Discontinuity Design

The main contribution of this paper is to provide estimates of whether caseworkers respond to differences in UI generosity across workers by allocating their resources differently. To do so we estimate variants of equation (3), with various measures of caseworker actions as outcome variables. To obtain credible identification we exploit the sharp age cutoff at age 50 in order to estimate the effect of UI generosity using a standard regression discontinuity

[15]Note, that while we think these regressions suggest that there are resource constraints, the caseload is obviously correlated with local labor market conditions and therefore, as tempting as it is, we do not believe this would be a good instrument to estimate the causal effect of these resources on job search outcomes.

design:

$$y_i = \beta + \delta \mathbf{1}(a_i \geq 50) + f(a_i) + \theta_{jt} + \varepsilon_i \tag{4}$$

where y_i is an outcome for individual i, a_i is the age of i at the time of claiming UI benefits measured in days and $f(.)$ is a function controlling for the effect of age. We estimate this equation locally around the cutoff using a linear spline specification for $f(.)$ with different slopes on each side of the cutoff. The coefficient δ captures the extent to which caseworkers act differently toward the high-UI eligible unemployed. θ_{jt} represents a vector of team j by quarter t fixed effects. We include these in order to focus on the comparison between workers with high and low UI eligibility within the same caseworker team, where resource constraints may be most relevant. In practice this makes virtually no difference and we show results without controlling for team-quarter FE, as well as when including additional controls in the online appendix.

Since we are particularly interested in whether caseworker actions may lead to bias in estimating the disincentives effects of UI (as in equation 2), we also show that there is in fact a significant effect of UI generosity on unemployment durations by estimating equation (4) with measures of job finding rates and unemployment durations as dependent variables.

Validity of the RD Design. There are two main reasons why the exclusion restriction may be violated in our situation. First, workers might systematically delay claiming UI benefits in order to be eligible for the longer potential UI durations. For example a worker who is laid off 1 week before their 50th birthday could wait 1 week in order to be eligible for an additional 3 months of UI benefits.[16] Second, there may be systematically higher inflows (and possibly of different types) at higher potential UI durations, e.g. because firms are systematically more likely to lay off workers with higher UI eligibility. We test for these possible violations using the standard RD diagnostic figures that are included in the online appendix. The density of UI inflows exhibits a small dip in the density in a 2 week window to the left of the age threshold and a little bit of extra mass just to the right. This is consistent with a small amount of claim delaying around the cutoff but only of about 200 people out of a sample of around 100,000 observations. It is consistent with the incentives to delay UI benefits, that this should mainly occur very close to the cutoff. To make sure that these

[16] The incentives for delayed claiming were discussed in detail in Schmieder, von Wachter and Bender (2012), both in the main text and the online appendix.

delaying workers do not affect our results, we exclude one month at each side of the cutoff in our baseline specification as a cautionary measure.

There also appears to be a small (1-2%) increase in the level of inflows to the right of the cutoff when we exclude observations in a 1 month window around the cutoff, which could be due to slightly higher layoff rates for older workers. To see whether this could be a sign for different selection of workers around the cutoff, we also tested for smoothness of predetermined worker characteristics at the cutoff. Despite a large N and precise estimates, these are all very smooth and do not suggest any selection around the threshold. Furthermore, we also estimated all of the specifications reported below controlling for a wide set of observable characteristics (available in the appendix), which had virtually no effect on the results.

5.2 The Disincentive Effect of UI

Figure 2 a) shows the average UI benefit duration by age of claiming UI. There is a clear discontinuous jump of about 1 month at the age 50 threshold, when UI eligibility increases from 12 to 15 months. This increase is partly mechanical - due to the increased coverage - and partly due to behavioral responses - since unemployed individuals exit unemployment slower. Figure 2 b) shows that nonemployment duration, which isolates the behavioral response, also shows a clear jump by about 0.2 months.[17] These results are confirmed in Table 3: overall the increase in potential UI durations at the age 50 threshold has a clear statistically significant effect on unemployment durations. The magnitude of the coefficient estimate for UI benefit duration is very similar to the results that Schmieder et al. (2012a) found for the earlier age cutoffs in the 1980s to early 2000s. The point estimates for unemployment duration are not quite comparable due to the shorter top-coding but in a similar ballpark.

Figure 2 c) shows how the hazard function of leaving unemployment changes at the age threshold. The figure is based on estimating equation (5) pointwise with an indicator for exiting in a given period t conditional on still being unemployed at period t. There is a clear spike in the hazard rate in the month when UI benefits expire. The difference is large and clearly significant in the months of benefit expiration (that is in the first month after benefits

[17]Since our data only covers observations until the end of 2011, we topcode all nonemployment duration spells at 18 months. Based on our work on other age cutoffs, topcoding at such a relatively low value will underestimate the effect on nonemployment durations, which is why we also show results on the probability of finding a job within 18 months. Furthermore we report below how the hazard and survivor functions change at the age cutoff, which are both unaffected by the censoring.

are cut). The hazard rate for individuals with 12 months of eligibility is also somewhat larger in the months leading up to month 12, but except for months 8 and 11 the difference is not statistically significant.[18] Figure 2 d) shows the corresponding survivor function. As suggested by the hazard rate, the survivor functions start to diverge a few months before benefit exhaustion for the 12 month group and remain different for the remaining period (though they converge again after month 15).

The results show that the UI extensions are important in practice: individuals above the threshold benefit significantly from the expanded UI duration and they remain unemployed longer. This suggests that if caseworkers take UI eligibility into account through any of the dimensions discussed in section 2, then we should observe differences in caseworker actions around the age threshold.

5.3 The Effect of UI on Caseworker Interactions

Unemployed workers above the age cutoff remain longer on UI benefits, partly due to the mechanical effect of increased UI eligibility and partly due to the behavioral effect of lower exit rates from unemployment when UI benefits increase. Since caseworkers only interact with workers while (and before) they are receiving UI benefits, there is a mechanical effect leading to more caseworker interactions with the unemployed who claim benefits above the age cut-off and thus remain longer on UI.

The fact that the survivor functions shown above are different even before month 12 highlights that comparing caseworker actions throughout the unemployment spell could be potentially affected by differences in who is unemployed at various unemployment durations in the two groups. Given that the survivor functions are virtually identical early on in the spell, we focus on two approaches: First, we calculate caseworker interactions for the period of 3 months before to 3 months after claiming UI benefits (or until the exit from UI if that occurs earlier).[19] Second, we show dynamic estimates of caseworker interactions at each point in the unemployment spell. While the latter does not solve the selection problem per se, it allows us to easily see whether there are differences in caseworker interactions in the

[18] Analysis of the earlier age cutoffs in Schmieder et al. (2012a) suggest that the fact that the difference in hazard rates is not statistically significant until month 11 is likely due to the smaller sample size here and that hazard rates are indeed higher for the shorter eligibility group for most of the UI spell.

[19] We start three months before UI entry, since workers who know that their job will end are required to register for job search three months before job loss.

months when the survivor functions are identical and thus selection bias likely minimal and is helpful to paint a more complete dynamic picture.

Table 4 presents estimates on how caseworkers interactions at the beginning of the UI spell vary at the age discontinuity. The top panel focuses on the direct interactions between caseworkers and unemployed, namely how often the number of times the caseworker invites the unemployed to a meeting, how often an integration contract is signed and the number of job referrals the unemployed receives from the caseworker. All three coefficients are very precisely estimated and very close to zero. For example, on average a UI claimant is invited 1.8 times by a caseworker over the initial UI phase (i.e. until the third month of UI), while our point estimate implies that at the age discontinuity this increases by around 0.3% at the cutoff and the 95% confidence intervals are tight enough so that we can rule out increases of more than 2.3%. The coefficients are similarly precise and small for the number of signed contracts and the number of job referrals workers receive. The corresponding RD-plots in Figures 3 a)-c) similarly show no jump at the relevant age cutoff, underscoring that caseworkers do not respond their assistance with regard to potential UI duration. This finding is robust with respect to several robustness checks performed in the next section. We can also observe whether workers are looking for part- or fulltime jobs. Given that this is recorded by the caseworker as a result of discussing the job search prospects with the worker, this could also be potentially affected by the caseworker's perception of the unemployed and her urgency to find a job due to UI eligibility. Again we find no difference in this outcome variable at the cutoff.

As an indirect way of testing whether caseworkers may interact differently with the unemployed who are older than age 50 we can explore the assignment into different labor market profiles that occurs at the beginning of a UI spell. We begin by collapsing the different labor market profiles to a single index corresponding to the ordering of the profile (the profile that corresponds to workers with the highest expected job finding probability receives a value of 1 and the profile with the lowest a value of 4). Figure 3 d) shows how the profile assignment varies by age and that older workers are systematically assigned to profiles that correspond to lower expected job finding probabilities. Furthermore there is a small jump around age 50, where the average profile index increases from around 2.2 to 2.3. It is noteworthy that profile assignment may occur before or after an individual actually claims UI benefits (it can happen when the worker first registers for job search for example which may be several months prior to the start of UI). As Figure 3 d) shows, the profile index does not jump exactly at age 50, but rather shows a rapid increase from a few months before to a few months after the age 50 cutoff. This suggests, that profile assignment may not actually be related to an increase in UI benefits but rather a form of age discrimination where age 50 is a salient number and caseworkers may believe it harder for workers above that age cutoff to find jobs. Of course this could also reflect age discrimination by potential employers who may be biased against hiring workers older than age 50. Furthermore, it is important to note that even in the presence of age discrimination, the validity of the RD design should not be affected, which is only based on the age at claiming UI benefits. Since we are comparing workers who claim benefits shortly before or after their 50th birthday, both groups will be age 50 for most of their UI spell and in particular when they get closer to the exhaustion point of UI benefits, thus even if employers discriminate against workers older than age 50 it would not have a differential effect on the treatment and control group in this design.[20] The differences in profiles could however affect the actions of the caseworker throughout the spell if the initial assessment affects the integration strategy of the caseworkers. Given that we do not observe any significant differences this seems unlikely however.

Another important role for caseworkers is to monitor search efforts and sanction workers who do not comply with job search requirements. While almost 20% of workers receive some sanction at the beginning of the UI spell (translating to on average sanction duration of about 5 days), this again does not change at the UI threshold.

Next we turn to assignments to active labor market programs. Figure 3 f) shows for the most common program category – education programs – , how the average duration of program participation (within the three months before and after start of UI receipt) varies with the age of claiming UI benefits. The figure shows that there are no discontinuities at the age cutoffs in any of the measures. For example, an unemployed person spends around 1.4 days per month of UI receipt in a training program, but this does not vary at the age cutoff. This is also confirmed in Table 4 which shows no economically or statistically significant differences at the age cutoff. Thus it does not appear that caseworkers compensate for the differences in UI eligibility by targeting ALMP resources differently to workers with different UI eligibility.

[20] Consistent with this, in section 5.5 we provide a placebo test, where we find no increase in nonemployment duration at age 50 in a time period where there was no UI discontinuity at that age threshold.

5.4 Dynamic Effects of UI on Caseworker-Unemployed Interactions

Above we focused on caseworker interactions at the beginning of the UI spell in order to isolate the caseworker response to UI eligibility from the mechanical effect that the duration of contact between the caseworker and the UI recipient is changing. However, looking at how initial average caseworker variables are changing at the age discontinuity may hide important dynamic patterns. For example, one way how caseworkers might respond to potential UI durations is by concentrating extra resources on workers close to the exhaustion point to help these workers find a job before they run out of UI benefits. Similarly, caseworkers can help the unemployed close to the exhaustion point by prolonging their duration on UI benefits through active labor market programs, during which individuals may receive benefits even after regular UI benefits are exhausted. We estimate how caseworker responses are changing throughout the UI spell using the following regression model:

$$y_{it} = \beta_t + \delta_t \mathbf{1}(a_i \geq 50) + f_t(a_i) + \theta_{jt} + \varepsilon_{it} \,|\, t_i \geq t \tag{5}$$

t_i is the month individual i is exiting UI and we estimate this equation for each month t when individuals are on UI ($t = 1, ..., 12$). Thus the sample in each regression are all individuals who are still in UI in month t. The outcome y_{it} are caseworker interactions of individual i in month t of the UI spell. θ_{jt} are team-by-quarter fixed effects separately estimated for each duration t. Estimating equation (5) for each t provides a vector of β_t which represents the average caseworker interactions of individuals in month t just to the left of the age discontinuity, while δ_t represents the discontinuous shift in the number of caseworker interactions in that month. Plotting δ_t and $\beta_t + \delta_t$ provides the level of interactions to the left and the right of the cutoff and the estimated standard errors on δ_t allow for a straightforward test of whether the levels of interactions change at the age discontinuity in each month.

One concern about these dynamic estimates is that while the RD design implies that workers are comparable on both sides of the cutoff at the beginning of the UI spell, there could be differential selection of who returns to work in the two groups. In that case the composition of workers would change throughout the UI spell and we would not necessarily be comparing similar workers in later months of UI. We address this in two ways: first we show in Appendix Figure 10, that along a wide range of observables worker characteristics change very little throughout the UI spell and do not appear to change differentially between the two groups. We also reestimate all specifications below controlling for a vector of worker characteristics in equation (5) and found virtually identical results (Appendix Figure 7 and 8).

Figure 4 a)-c) shows how caseworker contacts in the form of invitations to the UI agency, vacancy referrals, and integration contracts change throughout the unemployment spell as well as at the age discontinuity. Invitations (a) occur as early as 3 months before the start of the UI spell, while 40 percent of the unemployed receive an invitation just prior to starting on UI benefits and an additional 50 percent during the first month of UI benefits. Afterwards invitations continue at a pace of about one in every three months. A similar patter can be observed for vacancy referrals (c) with a spike early on in the UI spell and then about one referral every three months. Most integration contracts (b) are signed right at the beginning of the UI spell, with some occurring prior to that (presumably at the time when individuals register for job search) or shortly afterwards. There are new integration contracts (or likely rather updates to the existing contracts) later during the UI spell at a frequency of about 1 every 4 months.

Regarding differences at the age discontinuity, we observe relatively small and mostly insignificant differences between the two groups later in the UI spell. There is a small uptick in invitations in the 12 month group prior to benefit exhaustion which may be due to workers being invited for a final meeting to discuss the upcoming benefit expiration. The point estimates also suggest that workers below the age group are slightly less likely to sign new integration contracts and receive vacancy referrals later on in the spell, but the difference is very small (e.g. about 0.02 vacancies referred per month in month 6). It seems hard to imagine that integration contracts or vacancy referrals would lead to a reduction in the job finding hazard, so if anything these caseworker actions might actually counteract the disincentive effect. This would suggest that the true disincentive effect revealed by the higher reemployment hazard for workers with 12 months of UI (which is visible in Figure 2) is perhaps biased downwards, but given the small difference in these resources (and that we do not know that they are at all effective) this bias is likely very small.

In Figure 4 d) we show the average number of days UI recipients are sanctioned throughout the UI spell. Sanctions are most likely right at the beginning, due to voluntary quits or failure to register for job search in time. Afterwards they are less common and decline over the UI spell, with around 0.5 days of sanctioned UI benefit days per month and no meaningful differences between the two groups. Thus it does not seem that caseworkers attempt to counteract the disincentive effects of UI benefits through increased sanctioning of the workers with higher eligibility.

Figure 4 e) shows the number of days spent in education or vocational training programs. Participation in such programs increases initially, with a maximum around 4 months after the start of receiving UI benefits. Interestingly there is a gap in time spent in education programs in the 4 months prior to the exhaustion point with individuals with 12 months of PBD participating about an extra 0.4 days per month. Participation in a training program effectively allows for an extension of UI benefits, since an individual continues to receive UI benefits while in a program (even if regular benefits are already exhausted) and if benefits are not yet exhausted, each day in a training program only uses up half a day of UI eligibility. Furthermore if an individual finishes a training program she can always receive UI benefits for the remainder of the calendar month. This may create an incentive for the unemployed to participate in training programs close to the UI exhaustion point or for the caseworker to allocate slots in training programs to workers who otherwise might lose UI benefit soon. While we cannot disentangle whether this is due to the caseworker shifting resources or due to the unemployed seeking out training programs, overall the effect is quite small. Furthermore it seems likely that participation in training programs decreases job finding probabilities at least in the short run, since workers are likely not looking for jobs while in the program and have fewer incentives to find a job right away. Thus this would again go in the opposite direction of the disincentive effect of UI benefits and thus lead to a downward bias in the estimated disincentive effect. Figure 4 f) shows similarly that there are no differences for participation in private job placement services.

While the Figure 4 revealed a few small differences in integration contracts, vacancy referrals and training programs for workers close to the exhaustion point, the differences are small and, as discussed above, at most seem to suggest a slight downward bias in typical moral hazard estimates of UI. Furthermore given that the results suggest that workers with lower UI eligibility receive fewer contracts and vacancy referrals, while being more likely to go into training programs, this does not point clearly towards the caseworkers either trying to provide extra or less resources to this group. Given the small magnitudes and not very clear pattern, our overall takeaway is that caseworkers do not or only to a very small degree respond to UI eligibility and that this is unlikely to lead to a significant bias in measured disincentive effects.

5.5 Summary of Robustness and Heterogeneity Analysis

We conducted a wide range of robustness checks that we include in the online appendix and only briefly summarize here. Overall our main finding that caseworker interactions are not affected by PBD is remarkably robust and not affected by our choice of specification.

To check for potential (functional) misspecification in age, we varied the size of the age window around the age threshold and included quadratic age polynomials separately on each side of the cutoff. We also estimated all specifications using a rich set of control variables, such as pre-unemployment wage, dummies for nationality, gender, children, marital status, East Germany and six educational groups, the duration until UI take-up, actual experience, occupational-, industry- and establishment tenure as well as seasonal (monthly) controls. We also estimated all main specifications with and without team-quarter fixed effects. Further checks use bias adjusted estimates with robust standard errors as proposed by Calonico et al. (2014) and assess the sensitivity of not excluding values within one month at each side of the cutoff.

For all specifications during the period with real treatment, the effect on duration of UI benefits receipt – and for all but one specification for the effect on on nonemployment– are positive, similar in size and mostly statistically significant. The estimates on caseworker actions are in contrast insignificant and close to zero for almost all specifications with the exception of small effects in some training program specifications and the assigned labor market profile at UI entry. For the main caseworker actions (invitation to meetings as well as provided vacancy referrals) the precision of the estimates rules out caseworker reactions of economically meaningful size.

We also conducted a placebo test at the age threshold 50 for the years 2006 and 2007 (when PBD did now vary at this age threshold). There is no increase in UI benefit or nonemployment durations at the age threshold in these placebo years suggesting that there are no other confounding factors at the age threshold that would bias either the nonemployment estimates or the caseworker estimates.

Finally we investigated whether there are heterogeneous treatment effects that might cancel each other out in the aggregate. Imagine for example that caseworkers have a form of gender bias that might let them behave like a “unemployment minimizer” toward one gender while as a "welfarist" towards the other gender, by pooling men and women it might appear that caseworkers' resource allocation is unaffected by UI benefits. To investigate this we split our samples by a wide range of observable characteristics such as gender, pre-unemployment wage, team size, and others we did not find noteworthy differences in these treatment effects across the groups.

6 Conclusion

This paper investigates one aspect of caseworker actions, namely whether they respond to differences in UI eligibility across the unemployed. Our results suggest that caseworkers do not seem to significantly change their behavior at the age discontinuity determining UI eligibility, which may be surprising given that there are many possible motivations that would lead to caseworkers adjusting their behavior as discussed in Section 2. In particular it does not appear to be the case that caseworkers systematically shift resources to help the disadvantaged unemployed with shorter UI durations, or to counteract the disincentive effects of UI. The differences we documented in Figure 4 towards the end of the UI spell are very small and do not point in a systematic direction. Our findings are thus consistent with the stylized model of the bureaucratic caseworker laid out in the framework section, where caseworkers follow either explicit or implicit guidelines regarding how to interact with the unemployed in a way that is not responsive to the difference in UI generosity or the different search behavior induced by it. Alternatively the findings would also be consistent with a lack of awareness or salience of the differences in UI eligibility.

At least in this context it does not seem that caseworkers are using their resources to minimize unemployment durations or to maximize welfare of the unemployed in a utilitarian sense. A positive implication of this finding from a research perspective is that the typical estimates of the disincentive effects of UI are not confounded by endogenous caseworker actions.

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Table 1: Summary Statistics for UI Spells Age 48 to 52

	(1) All Spells	(2) Eligigible for max PBD	(3) Age 48/49 and max PBD	(4) Age 50/51 and max PBD
Individual Characteristics				
Female	0.48	0.47	0.46	0.47
Non-German	0.091	0.076	0.077	0.074
Age in Years	49.9	50.0	48.9	51.0
Education in Years	11.1	11.2	11.2	11.2
Unemployment and UI Duration				
Nonemployment Duration in Months (cap 36 Months)	19.3	18.4	17.6	19.2
	[15.1]	[15.0]	[14.9]	[15.0]
Nonemployment duration capped at 18 months	11.5	11.2	10.8	11.5
	[6.95]	[6.96]	[6.96]	[6.94]
Duration of UI Receipt (net)	7.00	7.08	6.44	7.77
	[5.35]	[5.47]	[4.95]	[5.90]
Pre- and Post UI Characteristics				
Next Daily Earnings after Unemployment	52.5	54.7	55.1	54.3
	[31.1]	[31.8]	[32.2]	[31.4]
Last Daily Earnings prior to Unemployment	57.0	63.1	63.7	62.6
	[42.0]	[41.5]	[41.5]	[41.6]
Maximum UI Duration (imputed)	12.6	13.4	12	15
	[2.97]	[1.50]	[0]	[0]
Probability of Leaving Unemp. within first 18 Months	0.54	0.57	0.59	0.54
	[0.50]	[0.50]	[0.49]	[0.50]
Invitations amd Referrals				
Number of Invitations during UI Receipt	3.18	3.25	3.17	3.32
Number of Contracts during UI Receipt	2.20	2.33	2.26	2.40
Number of Referrals during UI Receipt	2.17	2.42	2.44	2.39
Profile Assignment				
Market Profile at Beginning of UI Spell	0.14	0.16	0.18	0.14
Activation Profile at Beginning of UI Spell	0.095	0.11	0.12	0.097
Support Profile at Beginning of UI Spell	0.22	0.24	0.21	0.27
Development Profile at Beginning of UI Spell	0.026	0.022	0.019	0.026
Active Labor Market Programs				
Number of Days in Training Programms when on UI	13.7	15.1	15.6	14.5
Number of Days in Placement Services when on UI	4.55	4.74	4.00	5.54
Team-Related Characteristics				
Mean N per Team	347.5	359.1	362.2	355.8
Mean N between age 45 and 55 per Team	70.2	72.6	73.1	72.1
At least one worker on both sides of cutoff	0.97	0.97	0.97	0.98
Number of observations	138272	98405	50880	47525

Notes: This Table summarizes the data for all UI entries from April 2008 to June 2010 where the worker age at the time of claiming UI was >= 48 and < 52 years. Column (1) shows all individuals with age between 48 and 52. Column (2) restricts this to workers who have worked at least 3 years during the last 5 years and took up UI benefits within 3 months after job loss, which assures that they are eligible to the maximum potential benefit duration (PBD) on each side of the cutoff. Standard deviations for selected variables are shown in brackets.

Table 2: The Relationship Between Team Caseload and Caseworker Assistance

	(1)	(2)
Panel A: ln(Number of Referrals)		
ln(caseload)	-0.0611**	-0.0484**
	[0.0139]	[0.0138]
R^2	0.238	0.256
Panel B: ln(Number of Invitations)		
ln(caseload)	-0.0809**	-0.0570**
	[0.00921]	[0.00949]
R^2	0.279	0.324
Panel C: ln(Number of Contracts)		
ln(caseload)	-0.0928**	-0.0641**
	[0.00851]	[0.00874]
R^2	0.212	0.278
Panel D: Days per month in Education Programs		
ln(caseload)	-0.144**	-0.135**
	[0.0465]	[0.0435]
R^2	0.171	0.174
Panel E: Days per month in Placement Programs		
ln(caseload)	-0.111**	-0.0963**
	[0.0267]	[0.0226]
R^2	0.310	0.314
Number of Team-Quarters	2637763	2637763
Team FE	x	x
Quarter FE	x	x
Controls		x

Notes: Standard errors clustered on the team-level († P<.1, * P<.05, ** P<.01)).
This table shows regressions of intensity of caseworker assistance on the number of UI inflows (caseload) per team-quarter. Regressions are weighted by team size. Controls are mean values for female, non-german nationality, pre-unemployment wage, experience and education.

Table 3: The Effect of Potential UI Durations on UI and Nonemployment Duration

	(1) Unemp Ins. Benefit Duration	(2) Duration Nonemp to emp	(3) Non-Emp Duration topcoded at 18 Months	(4) Exit Prob 15 Mon	(5) Exit Prob 18 Mon
Increase in Potential UI Dur. from 12 to 15 Months					
D(Age above Cutoff)	1.00	0.42	0.22	-0.031	-0.014
	[0.080]**	[0.12]**	[0.10]*	[0.0073]**	[0.0073]†
$\frac{dy}{dP}$	0.33	0.14	0.075	-0.010	-0.0047
	[0.027]**	[0.041]**	[0.034]*	[0.0024]**	[0.0024]†
Effect relative to mean	0.14	0.058	0.020	-0.058	-0.025
R^2	.2057	.2816	.1855	.1903	.1936
Adjusted R^2	.0575	.0993	.0335	.0393	.0431
Observations	98405	59965	98405	98405	98405
Mean of Dep. Var.	7.08	7.12	11.2	0.53	0.57

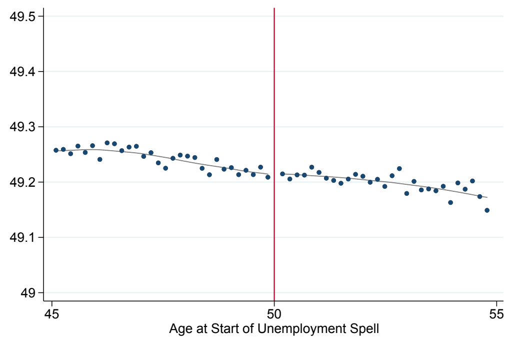
Notes: Standard errors clustered on day level († P<.1, * P<.05, ** P<.01)).
Coefficients from RD regressions with a bandwidth of 2 years but excluding observations within 1 month on each side. Local linear regressions with different slopes on each side of cutoff controlling for caseworker-team by quarter fixed effects.

Table 4: Potential UI Duration on Assistance in Job-Search

Caseworker Assistance and Search Selectivity				
	(1)	(2)	(3)	(4)
	Number of Invitations	Number of Signed Contracts	Number of Job referrals	Looking for part and fulltime jobs
D(Age above Cutoff)	0.0062	0.011	-0.026	-0.00088
	[0.018]	[0.013]	[0.033]	[0.0032]
Effect relative to mean	0.0034	0.0078	-0.020	-0.013
Observations	98405	98405	98405	98405
Mean of Dep. Var.	1.83	1.35	1.27	0.066
Profile Assignments, Sanctions, and Active Labor Market Programs				
	(1)	(2)	(3)	(4)
	Labor Market Profile Index	Fraction of Sanctions at UI entry	Days in Placement Services	Days in Training Programs
D(Age above Cutoff)	0.11	-0.0046	-0.0025	0.031
	[0.019]**	[0.0061]	[0.13]	[0.26]
Effect relative to mean	0.051	-0.022	-0.0021	0.0061
Observations	52098	98405	98405	98405
Mean of Dep. Var.	2.22	0.21	1.20	5.07

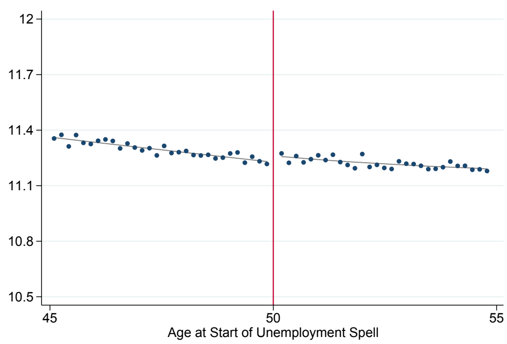
Notes: Standard errors clustered on day level († P<.1, * P<.05, ** P<.01)).
Local linear regressions with different slopes on each side of cutoff controlling for caseworker-team by quarter fixed effects.

Figure 1: Are workers assigned to different types of teams around the age threshold?

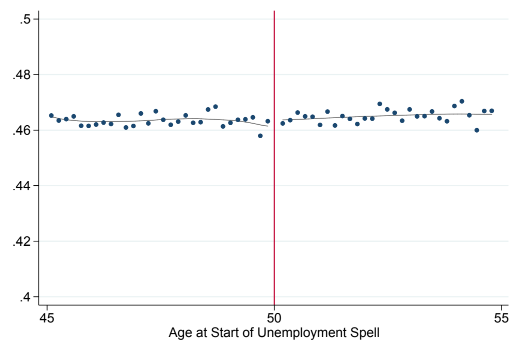


(a) Fraction of Individuals above 50

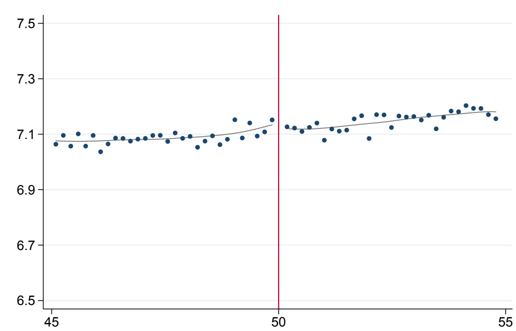
(b) Non-German



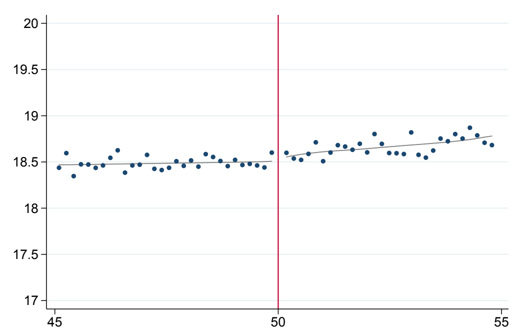
(c) Education in Years



(d) Female



(e) UI Duration

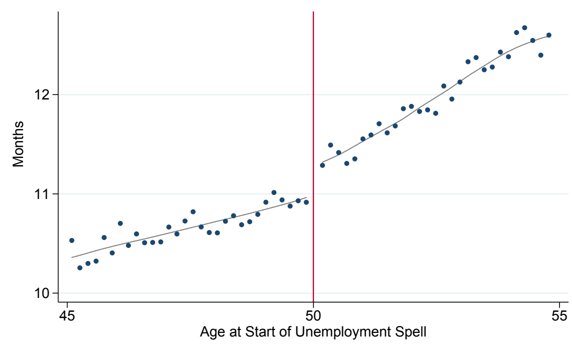


(f) Nonemp Duration

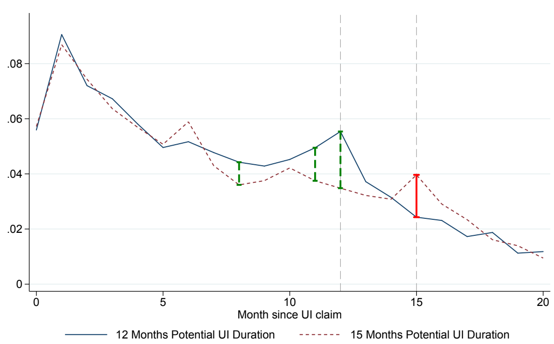
Notes: This figure shows team level leave-out means of different characteristics of UI recipients by age on the team-quarter level. Binsize is 60 days.

Figure 2: The Effect of Potential Benefit Durations on Job Finding

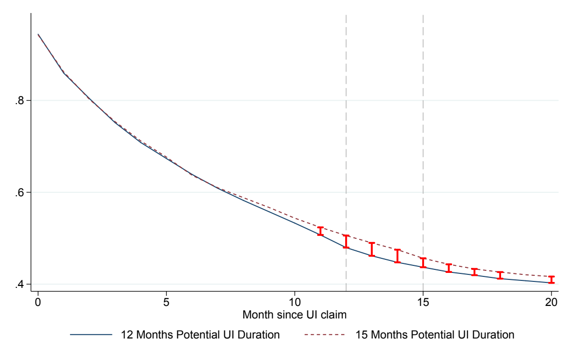
(a) UI Benefit Duration



(b) Nonemployment Duration



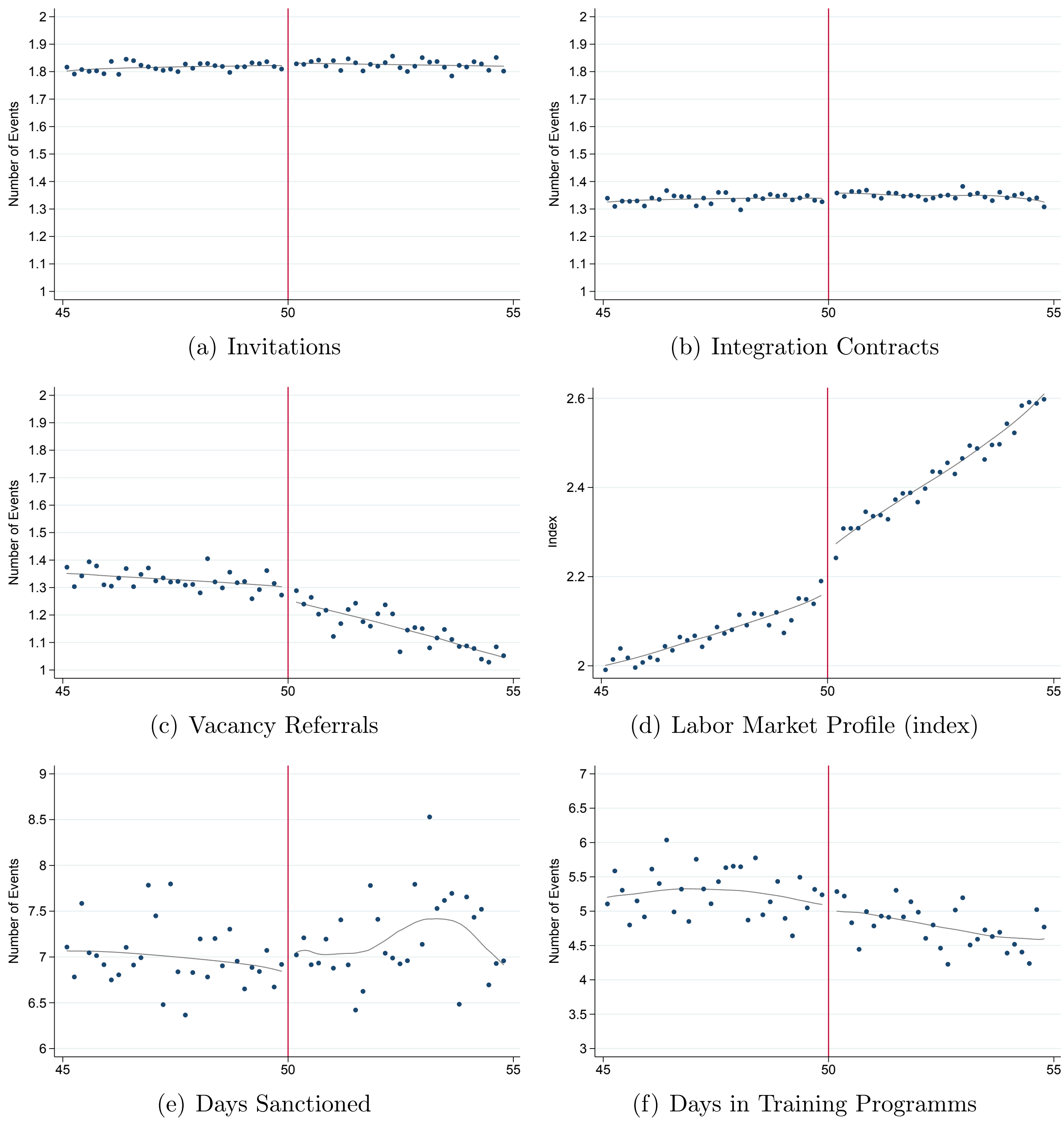
(c) Exit Hazard from Unemployment



(d) Survivor Function

Notes: Panels (a) and (b) show cross-sectional RD plots for the number of days in UI benefit receipt (a) and the number of days in nonemployment capped at 18 months (b) controlling for team x quarter fixed effects. The binsize is set to 60 days. Panel (c) shows the monthly hazard function for for the two eligibility durations estimated at the cutoff (via pointwise RD regressions). Panel (d) shows the corresponding survival functions. Where the hazard and survival function are statistically significantly different from each other the figures shows vertical bars between the two lines.

Figure 3: Caseworkers Resources / Actions around UI Cutoff



(a) Invitations

(b) Integration Contracts

(c) Vacancy Referrals

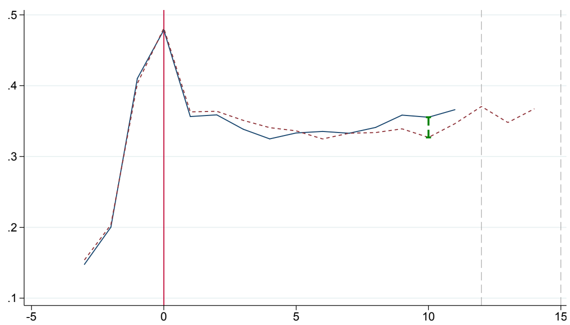
(d) Labor Market Profile (index)

(e) Days Sanctioned

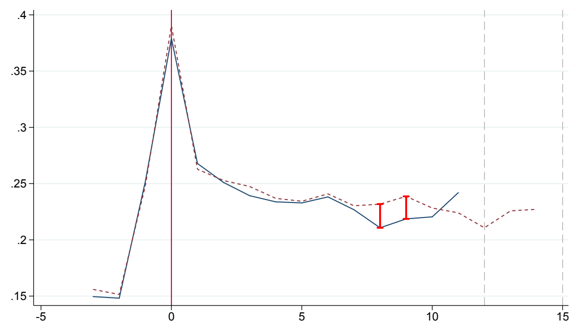
(f) Days in Training Programms

Notes: This figure shows cross-sectional RD plots for caseworker contacts around the age cutoff 50. The outcome variables are measured as number of contacts per month from 3 months prior to UI entry up to four months after UI entry for Figure a) - c). and the assigned index value of the four relevant labor market profiles (where one means good prospects and four bad prospects) at the beginning UI entry for figure d). The binsize is set to 60 days.

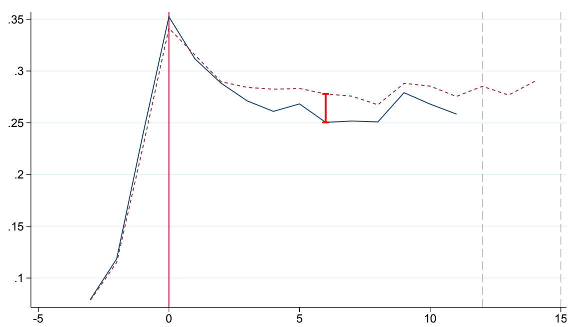
Figure 4: The Effect of Potential UI Durations on Caseworker Interactions Throughout the Unemployment Spell



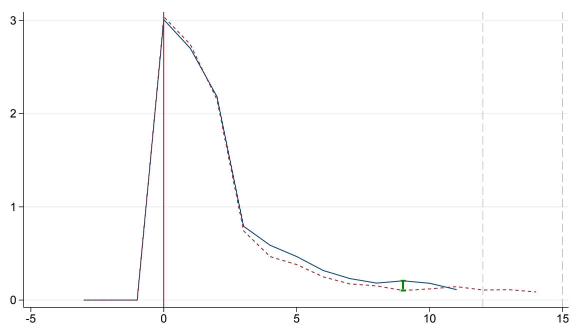
(a) Invitations



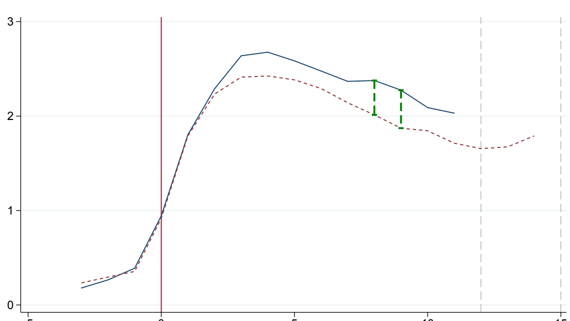
(b) Contracts



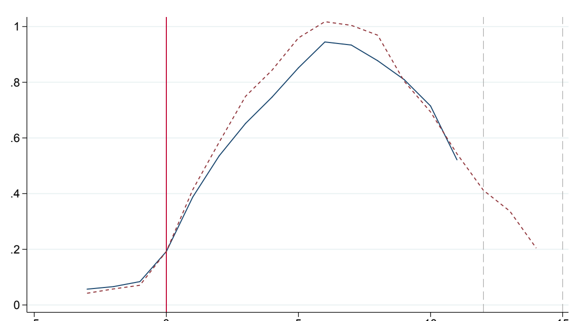
(c) Vacancy Referrals



(d) Sanctions



(e) Training Programms



(f) Placement Services

Notes: This figure shows estimated counseling/monitoring intensities over the spell of UI benefit receipt (conditioned on receiving still UI benefits) for both eligibility durations. The blue solid line indicate estimates for 12-, the red dashed line estimates for the 15 months eligibility duration. Vertical bars indicate significant differences on the 5% significance level for the respective months. Figure a) - c) shows the number of interactions per months on UI, figure d) - f) number of days in the respective month. The regressions control for fixed effects on the team x quarter-level.