Unemployment Insurance Receipt, Take-Up and Labor Market Conditions*

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Preliminary. Comments Welcome!

Abstract

This paper examines new facts about the Incidence of Unemployment Insurance (UI) receipt after job seperations. Using German administrative data over 30 years, I show that among likely eligible job exiters, about 27% do not receive any UI in their first year after job-loss and this share varies substantially with current labor market conditions: The UI recipiency rate increases by 4-5 percentage points for a one percentage point increase in the unemployment rate relative to the previous year. Turning to mechanisms, I investigate the role of UI take-up — rather than ineligiblity — in driving the observed pattern. Motivated by a measurement error framework and drawing on additional survey data, I argue that UI take-up behavior plays a key role in explaining the pattern of UI receipt, while characteristics of the unemployed together with potential ineligibility explain only about 1/3 of the variation with labor market conditions. Comparing the documented parameters of UI receipt with counterfactual scenarios of take-up behavior highlights the importance of take-up in influencing the de-facto generosity of UI.

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1 Introduction

The most common tool designed to mitigate short-run earnings losses of laid off workers is unemployment insurance (UI). Especially in recessions, when unemployment durations are long, receiving UI benefits helps affected worker to smooth their consumption (Schmieder et al. (2012); Kroft and Notowidigdo (2016)). Yet, not all individuals experiencing a job-loss receive UI either because they are not eligible or because they fail to claim (aka take-up) benefits.

Active claiming as well as meeting of certain eligibility criteria are universal features of UI systems, yet our understanding of how they limit the benefit transfer to the unemployed is incomplete. While a reduction in the incidence of UI receipt reduces the average benefits payed per laid of worker, we know little about it's extent, how it evolves over the job-loss spell and how it affects UIs capacity in buffering income losses over the business cycle. These features matter for the targeting properties of UI, the capacity of UI in insuring worker against the costs of job-loss and UI's role as an automatic stabilizer — a role emphasized in recent literature studying social insurance over the business cycle (McKay and Reis (2016)).

In this paper, I make three main contributions: First, using detailed administrative data from Germany and a novel and transparent sample of job seperators, I quantify the incidence of UI receipt and how it evolves in the first year after job loss. The rate of workers receiving UI (recipiency rate) increases steadily in the first months after separation, while a significant share of about 27 percent never receives any UI in the first year after job loss. Second, I examine how UI receipt varies with the labor market conditions individuals' face at time of layoff. A priori, it is unclear, how UI receipt correlates with labor market conditions. On the one hand, a higher expected benefit of UI receipt could increase claiming UI in recessions (Anderson and Meyer (1997), Kroft (2008)), thereby amplifying the stabilizing role of UI. On the other hand, a shift in the pool of unemployed towards individuals less in need — well educated, high-wage workers (Mueller (2017)) — as well as an increased burden of claiming due to capacity constraints at local UI agencies could reduce claiming and lower the stabilizing capacity of UI. I document that the propensity to do so correlates negatively with labor market conditions these workers face at job-loss. The magnitude of this variation is meaningful and only partly driven by differences in UI receipt. Third, I investigate whether the documented patterns of UI receipt are driven by incomplete take-up or ineligibility. Using insights from a measurement error framework in combination with additional survey data, I argue that a most of the cyclicality of UI receipt and to a lesser extent it's level reflects take-up behavior instead of differences in eligibility.

Using detailed German administrative data for about 5 million employment exiters over 30 years, this paper starts with constructing a sample of of likely eligible, nonemployed workers. The daily structure of the data allows to paint a detailed picture of UI receipt in the first year after job-loss.

To investigate the variation of UI receipt with labor market conditions I rely on the change in the unemployment rate at job separation. The magnitude of this variation is meaningful: A one percentage point (p.p.) increase in the change of the national unemployment rate, i.e. the difference between current and last years unemployment rate, is associated with a 4-5 p.p. increase in UI receipt. In monetary terms, the average worker leaves about 600 Euro more benefits unclaimed in the first year after job loss when entering unemployment in a boom rather than a recession despite beeing shorter unemployed. This variation is not just a mechanical consequence of longer nonemployment durations in recessions, with take-up increasing instantly after job loss if labor market conditions are worse, and robust to alternative sample restrictions and take-up measures.

Compositional changes of worker exits only explain part of the observed differences. A large set of individual, firm and regional characteristics explains only 20% of the variation in UI receipt over the business cycle while revealing notable differences in the selection into UI receipt. When adding more detailed controls including individual- or establishment fixed effects the association over the business cycle changes only slightly. Applications of Oster's method (Oster (2017)) indicate that this association is not likely to be driven by unobserved variables alone. A negative association of take-up and labor market conditions also extends to the regional level. While effect sizes are smaller when holding aggregate conditions constant, the associations are robust to the inclusion of detailed controls — including regional fixed effects — with a slightly larger drop in coefficient size.

Does the observed incidence of UI receipt reflect take-up behavior or could it also stem from ineligibility or other forms of measurement error? I argue that the high accuracy of UI information in the administrative data alleviates measurement concerns in benefit receipt — a widespread phenomenon in survey data (Meyer et al. (2018),Bruckmeier et al. (2019)). Yet, "false positives" in the eligibility status, i.e. wrongly classifying individuals as eligible due to temporary ineligibility as well as unobserved labor market states are possible. To clarify the conditions under which such measurement error would affect my estimates, I follow Meyer and Mittag (2017) and set up a simple measurement error framework. Drawing on additional survey data from the Germa Socio Economic Panel (SOEP) that allows to examine states not observed in the administrative data and using additional sample restrictions in the administrative data, I examine the role of confounders empirically. While results indicate some role of unobserved states and temporary ineligibility, their influence seems limited overall. In my preferred specification, observed characteristics and measurement error in take-up explain jointly about 1/3 of the raw cyclicality.

Lastly, I compare the generosity of the current UI system for the average separator to scenarios with different average take-up and cyclicality. While the cyclicality of UI helps to buffer the higher costs of job loss in recessions, incomplete take-up signifcantly limits the de-facto generosity of UI compared to a scenario with full take-up. These results underscore the importance of considering parameters of UI take-up and eligibility when studying the generosity and design of UI schemes.

My work relates to and complements the small but growing body of work on UI take-up. Seminal papers include Blank and Card (1991), showing that the decline in uninsured unemployment in the US in the 70ies is due to a decrease in UI take-up, and Anderson and Meyer (1997), arguing that this decrease is a result of a reduction in UI generosity during that period. More recent papers are Kroft (2008) studying the optimality of UI in presence of imperfect UI take-up and Auray and Fuller (2020) and Auray et al. (2019), who model UI take-up in an equilibrium search model targeted to match characteristics of the US system with experience rating. Closer to my setting, where contribution is entirely based on workers' gross wage, are Blasco and Fontaine (2021), Fontaine and Kettemann (2017) and Kettemann (2017). Most of these papers study mechanisms of UI take-up with less focus on measurement an —perhaps as a consequence— take-up rates vary greatly between studies.[1] My paper provides new evidence on the incidence and magnitude of UI take-up paying particular attention to sample construction and the role of measurement error. Blasco and Fontaine (2018) model barriers to take-up as claiming frictions. Consistent with this idea, I document an increase in take-up over time since job loss from 50% directly after job loss to more than 70% one year later. My work is closely related to and complements the work by Kettemann (2017), who studies counter-cyclicality in UI take-up mostly from a theoretical perspective, focusing on implications for aggregate unemployment dynamics. By providing a comprehensive empirical analysis of how UI take-up relates to labor market conditions, my work empirically validates the importance of counter-cyclical UI take-up. While imperfect take-up has been well documented for other programs in Germany (Riphahn (2001); Bruckmeier and Wiemers (2012)), mine is — to the best of my knowledge — the first study that examines UI take-up in the German context.

This paper also relates to work on UI and more specifically UI's role during recessions. Kroft and Notowidigdo (2016); Schmieder et al. (2012) study the optimality of UI over the business cycle from an insurance perspective, whereas McKay and Reis (2016) focuses on its role as automatic stabilizer. All these papers argue for more generous UI during downturns than during booms. My paper documents that through a countercyclical take-up rate UI is more generous per laid off worker during recessions than during booms while it highlights and quantifies the limiting capacity of buffering income losses after employment exits, complementing work on projecting replacement rates during recessions (Ganong et al. (2020)). For different papers that do not study UI take-up directly, the properties of UI take-up matter as an ingredient or for the interpretation of results: Chodorow-Reich and Karabarbounis (2016) are interested in the cyclicality of UI take-

[1] Blasco and Fontaine (2021) report a range of take-up rates between 30% and 70% for the literature on UI take-up.

up, Kroft and Notowidigdo (2016) discuss differences in take-up along the business cycle causing a form of selection bias, and in Jäger et al. (2021) take-up matters for the interpretation of results.

More broadly, this paper relates to recent work that examines the effect of different interventions on program take-up, such as Bhargava and Manoli (2015) and Finkelstein and Notowidigdo (2019). A common finding of these papers is that information provision and complexity reduction can have significantly positive effects on program take-up. My work demonstrates that take-up is imperfect even for experienced workers highly attached to the labor force where general eligibility requirements are simple and transparent.

2 Institutions and Data

In this section, I describe the legal requirements for receiving UI, how the process of claiming benefits works in practice and which requirements unemployed have to fulfill while on UI. I then describe briefly the main data sources and how I select the samples used in the following analyses.

2.1 Institutional Background

Eligibility Requirements. In Germany —like in most Western countries— UI is organized as a mandatory insurance where employed workers (except for minor employment and civil servants) have to contribute a certain fraction of their gross wages up to a yearly adjusted contribution limit. Eligibility for UI in turn depends on past employment histories. Workers are eligible for UI if they contributed (i.e. worked) at least one year during the last two years. They are eligible to receive UI up to a maximum duration of 12 months if they contributed at least two years within the last five years.[2] In addition, the reason for becoming unemployed matters. To prevent welfare abuse, voluntary quits or quits due

[2]There have been further increases in eligibility duration for older workers over the last decades. For instance, since 2008, workers above the age of 58 can receive up to 24 months of UI if additional contribution requirements are met - see for example Gudgeon et al. (2019) and references therein. In addition, the time windows over which individuals could collect claims were more generous in some periods, the described two and five years reflect minimum durations.

to misbehavior of the unemployed are sanctioned with benefit cuts at the beginning of UI receipt. The maximum duration of these benefit cuts increased over time: It was set to four weeks at UI start until the early 80ies, increased to eight weeks in 1982 and is now at 12 weeks. If workers assist their employer in the course of their own separation, usually by accepting severance payments in exchange for agreeing to an earlier separation date than labor law requires for involuntary quits, this can result in a delayed starting date of UI. The length of this delaying period depends on the size of the severance payment and on the duration by which a separation shortens an employment period through enabling an earlier separation.[3] Importantly however, the type of employment exit only affects initial benefit cuts, without changing overall UI eligibility.

Application Process. Individuals have to apply for UI benefits in order to receive them. A key requirement is to officially register as unemployed. This has to be done in person at the local UI agency and at latest at the first day after job loss.[4] Individuals are asked basic information about their current employment status and are provided with documents for applying for UI benefits.

A second key requirement is applying for UI benefits. This requires to file several detailed forms. For instance, unemployed have to gather a list of exact employment histories and wages over the past -up to 5- years relevant for UI calculation. This has to be certified by the employers they worked at during that time. The filled-out forms can be handed in individually or sent via post or online to the UI agency, which then calculates the eligibility duration and benefit size. In case of missing or incomplete forms, the agency asks the unemployed to hand in the missing information.

Since 2004, individuals also have to register as 'job searcher'. In order to maintain full eligibility, the registration has to happen before getting laid off[5], and can preliminarily be

[3]Sanction or delaying periods are not equally likely for all separations. In case of a plant closure, for example, separation is involuntary from the employee's side and separation contracts are uncommon.

[4]As of 2018, there were about 600 local UI agencies in Germany (`https://www.arbeitsagentur.de/ueber-uns`), which are assigned based on place of residence. Registering as unemployed after the first day of job loss delays the period over which individuals can claim benefits accordingly.

[5]Individuals have to register for job search at least three months in advance of a (likely) layoff. In case they are informed about the layoff later, they have to register within the next days after that. Delayed registering is punished with a one-week benefit cut at the beginning of UI receipt.

done online or via telephone. The formal registration as 'job searcher' usually happens when registering as unemployed. This registration requires to report job-search relevant information such as past employment history and preferred occupation.

UI Receipt. When on UI, benefits are transferred to the bank account of the unemployed at the end of each month. For most of the time period under study, the replacement rate has been .67 (.60) of the pre-unemployment net wage for individuals with (without) dependent children, up to a —seldom binding— contribution limit. Individuals have to obey basic job search requirements during their time on UI. These requirements include regular meetings (~ every 6-8 weeks) with the assigned caseworker at the local UI agency to discuss progress with job search and job search strategies (Schütz et al. (2011)). In addition, caseworkers can send job referrals to the unemployed, to which applying is mandatory. They can also require that the unemployed applies and documents own applications. Disobeying these job search requirements can lead to sanctions (benefit cuts), though this type of sanctions is uncommon for UI benefits.[6]

2.2 Data and Sample Selection

Main Data Source: Social Security Data. As main data source I use the Integrated Employment Biographies (IEB) from the Institute for Employment Research (IAB). This data comprises information on around 80% of employment records, with the most notable exceptions being civil servants and self-employed.[7] In addition, all periods of UI receipt and registered job search are covered.[8] The data spans the period 1975-2013 and contains a rich set of personal characteristics such as age, gender, wage, industry and occupation. Most notably, periods of employment and UI receipt are covered on a daily basis and can be considered as highly reliable.[9] The administrative data reflects actual periods of

[6]Schmieder and Trenkle (2020) report, for example, -apart from inital off times- low sanction rates over the spell of UI receipt.

[7]This data has been used, for example, in Card et al. (2013), Schmieder et al. (2016) and Dustmann et al. (2015)

[8]Additional periods covered by the IEB are participation in social assistance and active labor market programs.

[9]Since the reason for the collection of employment data is to document social security contribution, the employment duration has to be recorded correctly. Similar reasons apply for the periods of UI receipt.

employment and UI receipt. For UI the period starts at the first day a worker satisfies all requirements to receive benefits, which is –conditional on applying successfully– immune to delays in claiming or granting benefits. In contrast, sanction periods or delays in registering as unemployed lead to a delayed UI start.

Sample Selection. I construct an flow sample out of social security reliable employment for the years 1980 to 2010. I apply two broad selection principles:

1. Generate a sample where UI receipt is quantitatively important.

2. Focus on likely eligible and nonemployed worker.

The main findings of the paper –in particular the cyclicality– are quite robust to alternating sample restrictions. Yet, I focus on these baseline restrictions as they make the interpretation of results easier.[10] The set of restrictions that follow the first principle selects individuals that, if eligible, could claim a non-negligible benefit amount. I restrict to pre-nonemployment real gross wages (in 2010 values) above 1,400 Euro or approximately a minimum value of 850 Euro of monthly benefits.[11] In addition, individuals are required to be nonemployed, i.e. without any social security reliable employment, for at least one month. Thus, eligible individuals would in case of claiming receive at least about 850 Euro.

The set of restrictions that follow the second selection principle select individuals that are likely eligible based on their working history and nonemployed. I select individuals leaving social security employment that are -based on their recent working history- eligible for at least 12 months of UI benefits.[12] In addition, I apply restrictions that re-

[10]Table A.1 in the appendix describes the consequence of each of the different restrictions and of combinations thereof on the numbers of observations and on recipiency rates as introduced in the next subsection. Figure A.1 shows how recipiency rates vary when relaxing individual restrictions of the baseline-sample, highlighting the importance of the exit notification restriction "end-of-employment" and of excluding short nonemployment interruptions.

[11]For most of the period of interest, the net replacement rate has been 67% (60%) for individuals with (without) dependent children. Usually, this is based on the mean net wage in the year before job loss, but can in special cases be calculated as the average within the last two years. For simplicity and the unavailability of net wages in the IEB, the sample selection is based on gross wage at the last job before employment exit.

[12]Specifically, I select individuals with at least one year of working experience during the last two years and

duces the influence of potentially confounding states. These are states that are not covered in the social security data, but that may conflict with UI receipt. Examples include self-employment or maternity leave. A more detailed discussion of such states and an empirical investigation of how well the final sample performs in excluding them follows in subsection 5. I restrict the reason for employment exit to the notification "end-of-employment", which is used for "regular" employment exits. Other exit reasons are designed for exits into maternity leave or sick leave during which individuals could not receive UI benefits. This restriction thereby helps to exclude confounding states like maternity leave or disability insurance which have separate exit reasons. I also restrict to individuals aged between 25 and 55 to avoid conflicting states like military service or retirement. Finally, I restrict to individuals returning to work within the first three years after job loss. This last restriction has the clear advantage of excluding individuals who permanently exit the labor force or switch permanently into unobserved states (such as civil servants or permanent migration). At the same time, it is a relatively strong restriction as it reduces the sample by about 50%. I, therefore, replicate the main results without this restriction and show that they are similar to the baseline. The baseline sample consists of about 5 million nonemployment inflows, comprising about 50% of all UI payments among job exiters with full working history eligibility and in the relevant age- and time range.

The baseline sample differs from the typical samples used in the job-loss literature where involuntary job-exits are identified via mass-layoff or plant closures (Jacobson et al. (1993); Sullivan and von Wachter (2009)). My approach includes mass layoffs as well as individual separations, among them potentially voluntary exits that are, apart from initial sanctions or off times, also eligible for UI. This has the advantage of covering a broader sample of job exits, while the additional restrictions on selection principles result in a sample that is also more tailored towards eligible workers with high potential benefits. The main results replicate when restricting to only those individuals in the baseline sample getting laid off during a plant closure.

at least two years of working experience during the last five years without any UI-receipt during this time. In addition, I restrict to individuals that were at least six months employed at their current employer.

Complementary Survey Data. I complement my analysis with survey data that contains additional information on retirement, self-employment and other states not observed in the administrative data. I use the German Socioeconomic Panel (SOEP) with detailed information on a representative sample of German residents for the years 1984-2015 and construct a sample that is comparable to the one used with the administrative data.[13] Details on its construction are discussed in appendix A.

3 Quantifying UI Receipt

I now turn to a description of the incidence of UI receipt in the data, how it evolves over time since job loss, and the extent to that benefits are left on the table. [14]

3.1 The first Year after Job Loss

After layoff, individuals can either find new employment, claim and receive UI or be nonemployed without receiving UI benefits, which I call non-receipt. The composition of these states can change over time: All individuals start as nonemployed and increasingly find employment over time. Similarly, some individuals might not receive UI right away but decide to claim later.

How do these three states evolve over time? Figure 1 (a) plots the shares of employment, UI receipt and non-receipt over the first year after job-loss for the baseline sample.[15] To simplify interpretation it defines employment and UI benefits as absorbing states which shuts down transitions between UI and employment. Figure 1a makes the following points: First, about half of all job-looser start receiving UI immediately, where the other half remains in non-receipt. The share of UI recipients increases over time to above 70% of all job looser after one year. Especially in the first months after job loss a significant

[13]For a description of this data source, see for example Göbel et al. (2018).

[14]While UI recipiency describes benefit receipt without any statement on eligibility, UI take-up describes benefit receipt among the eligible unemployed, thus implying a behavioral (non)response. The two measures are identical for a sample of only eligible individuals. To what UI receit reflect take-up behavior is more discussed in section 5.

[15]The focus on the first year is motivated by the fact that after one year of absence from work, individuals forgo their eligibility, except for the case when they register as unemployed before. In practice, this restriction is not very consequential as few individuals enter UI later than one year after job loss. Apppendix Figure A.2 shows the dynamics over the 3 years after job loss.

share of individuals switches from non-receipt to UI receipt. A discontinuous increases in the share of UI recipients at 12 weeks coincides with common sanction durations, suggest that a small share of this delay reflects temporary ineligibility. Second, individuals also transition from non-receipt to employment and make up ultimately about 20% of all job-looser. The transition to employment is less stark in the first month and more spread out over the whole first year after job loss than for UI receipt. Third, and as a result of the previous observations, a substantial share of individuals are not receiving UI despite being nonemployed. Directly after job loss 50% of all individuals are in non-receipt. This share decreases to about 10% after one year with transitions to employment and UI receipt contributing about equally to this reduction.

The substantial share of non-receipt suggests that a significant amount of money is left on the table. I provide a simple approximation to the amount of money left unclaimed by calculating the approximate daily benefit amount for each individual multiplied with her realized duration in non-receipt.[16] To calculate the approximate net benefit level, I first estimate a net replacement rate -i.e. the share of gross earnings individuals get replaced on UI after paying taxes and social security contributions- at the median, assuming that half of the individuals is single with no children while the other half is married with dependent children. Using the median income of 2,051 Euro and applying the tax and transfer system for the year 2020, I obtain a net replacement rate of about .43. Multiplying this replacement rate with the individual pre-layoff gross earnings delivers in a second step the approximate benefit amount. Figure 1 (b) shows the weekly benefit amount left on the table among all job looser. In the first week, this amounts to about 150 Euro per individual and drops over time, to about 30 Euro after one year as more and more individuals leave non-receipt. Cumulating over the first year, the average job-looser leaves about 2,300 Euro on the table. In comparison, the average benefits received over that period amount to 7,530. To put it differently: Incorprating non-receipt reduces the actual average replacement rate among all job looser by 30%, a substantial drop.

[16] More formaly, let t be the time since layoff in weeks, $I(t \leq ntu_i)$ an indicator of whether individual i is in non-receipt at time t, w_i the daily gross wage an individual received at its pre-unemployment job in 2010 earnings and $\hat{b}$ the estimated average net replacement rate. The average benefits left unclaimed per job-looser in week t, $\hat{B}_t$ can be estimated as: $\hat{B}_t = 7 \cdot \hat{b} \cdot \sum_i w_i I(t \leq ntu_i)$.

3.2 Measurement Concepts

As the last subsection has shown, the incidence of UI receipt is a dynamic event. This subsection introduces three different definitions of UI receipt that capture different aspects of these dynamics and that are used in the rest of the paper. The subsection ends with a summary of these measures in the baseline sample and survey data. For individual i with nonemployment duration in days $nonempdur_i$ and time untill UI receipt in days $uidur_i$ we can define different measures that vary depending on when individuals start receiving UI in their nonemployment spell. Furthermore, $\hat{elig_i}$ denotes the proxied UI eligibility, which — as a direct consequence of restricting to likely eligible individuals — equals one in our sample. Based on this notation, we can define *any UI receipt* according to equation 1:

$$UIreceipt_i^A = I(uidur_i < nonempdur_i \& uidur_i < 365 | \hat{elig_i} = 1) \tag{1}$$

Any UI receipt is 1 if individuals have at least one day of UI receipt before their next employment spell and 0 otherwise. As a more restrictive measure, we can define *immediate UI receipt* according to equation 2.

$$UIreceipt_i^I = I(uidur_i < nonempdur_i \& uidur_i < 10 | \hat{elig_i} = 1) \tag{2}$$

Immediate UI receipt is only 1 if individuals start to receive benefits right away (within the first 10 days after job loss). It follows directly from 1 and 2 that $takeup_i^I \leq takeup_i^A$ for all i. Those two measures differ only when individuals start to receive UI with a delay. Equation 3 defines a third measure that incorporates this delayed component directly:

$$fracUI_i = 1 - \frac{min(nonempdur_i, uidur_i, 365 | \hat{elig_i} = 1)}{min(nonempdur_i, 365 | \hat{elig_i} = 1)} \tag{3}$$

This *fraction of days in UI* is calculated as the fraction of days in nonemployment in the first year after job loss that is covered by UI receipt, assuming individuals stay on UI the remaining first year after job loss.

All measures capture different parts of the UI receipt dynamic and differ only because some individuals receive benefits delayed. Appendix Table A.2 illustrates conceptually how these measures vary for different scenarios of $nonempdur$ and $uidur$. Empirically, immediate UI receipt and any UI receipt correspond to different points in time since job-loss in Figure 1 (a). The measure for fraction of days on UI and captures how much individuals receive UI during the first year of their spell.

Bringing those measures to the data, Table 1 shows that for "any UI receipt" the mean is .73 for the baseline sample in the administrative data. With .77, the corresponding mean in the SOEP is somewhat higher. Immediate receipt is with a mean of .52 lower, where the fraction of days on UI is with a mean of .64 somewhere in the middle. Since in 70% of cases benefit receipt happens immediately or never (Figure 1 (a)), there is a high overlap between the different measures.[17]

Appendix Figure A.1 and Appendix Table A.1 show that receipt drops considerably when relaxing the restrictions in the baseline sample. In particular, individuals with low wages or very short nonemployment durations are unlikely to receive UI after job loss. In sum, there is a sizable fraction of individuals that do not receive UI after job loss in my baseline sample and of those who do, a significant share starts receiving UI with delay.

4 UI-Receipt and Labor Market Conditions

I now turn to examining the relationship between UI receipt and labor market conditions. In subsection 4.1 I present first evidence on the relationship between UI receipt and the yearly labor market conditions on the national level (the business cycle). Subsection 4.2 then explores to what extent differences in (un)observed characteristics can explain the raw correlation between UI receipt and business cycle indicators and subsection 4.3 explores the association between UI-receipt and labor market conditions on a regionally disaggregated level.

[17] The correlation between $UIreceipt^A$ and $fracUI$ is .87 and the correlation between $UIreceipt^A$ and $UIreceipt^I$ .61.

4.1 The Business Cycle

As main measure for national labor market conditions I use the change in the unemployment rate between year $t-1$ and t, denoted as ΔUR_t. One advantage of this measure is that it captures the labor market conditions for new employment exiters more directly and does not —in contrast to the unemployment rate— hinge on the stock of long-term unemployed (Schmieder et al. (2012)). I replicate the main results using the unemployment rate UR_t and the GDP growth rate g_t as alternative measures in the appendix.

Figure 2 re-examines the dynamics of UI-receipt up after job-loss, but split up by whether individuals lost their job at times when labor markets where good or bad, proxied by a declining vs. increasing unemployment rate. Figure (a) shows the situation for a declining unemployment rate ($\Delta UR_t \leq 0$) and constrasts it with the UI-receipt dynamics when labor market conditions worsen ($\Delta UR_t > 0$). The share of UI recipients is lower at all points in time in the first year after job loss when the unemployment rate is declining, compared to a scenario of increasing unemployment.[18] Measures for immediate as well as any receipt are both about 8 p.p. higher in the latter scenario. Thus, the difference in receipt between times of growing vs. times of shrinking unemployment rates reflects mostly an instantaneous increase in UI receipt after job-loss. It is also noteworthy that the sudden increase in UI receipt after 12 weeks of job exit is similarly sized in booms and recessions, suggesting that differences in UI-receipt are not a result of differences in benefit sanctions.

Job looser leave more money on the table in good times. Figure 2 (b) shows the evolution of the difference in unclaimed benefits between good and bad labor market conditions over the first year after job loss, counting only individuals as long they have not started a new job yet. At all points in time since layoff, individuals that entered in good labor market conditions leave on average more money on the table than individuals entering when labor markets are bad.[19] This culminates to a difference of 600 Euro after one

[18] Appendix Figure A.1 shows the timing of UI receipt for good and bad labor market conditions over the full three years after job loss.

[19] This is despite the fact that individuals stay on average shorter in nonemployment when labor market conditions are worse.

year per laid off individual or to about 50 Euro per month *despite* shorter unemployment durations of individuals laid off during booms rather than recessions

To examine the variation of UI-receipt over time and its variation with the business cycle in more detail, I collapse the baseline sample to the yearly level. Figure A.4 (a) plots the resulting time series of all three recipiency measures over time and compares them with the time series for ΔUR_t. Recipiency measures differ mostly in their level, but exhibit a very similar pattern over time, as can be seen most clearly in the detrended time-serien in Figure A.4 (b). The time series show a clear negative association between mean receipt and labor market conditions.

To quantify the association between receipt and labor market conditions, I regress the variables for UI-receipt collapsed to the yearly level ($UIreceipt_t$) on variables for business cycle conditions (BC_t), controlling for a time trend $f(t)$.

$$UIreceipt_t = \beta BC_t + f(t) + \epsilon_t \tag{4}$$

All specifications use robust and bias-corrected (HC3) standard errors.

Estimates of β from various different specifications are reported in Table 2. Panel A shows results for the collapsed baseline sample for the different UI-receipt measures and labor market conditions, using an hp-filtered and in separate specifications a linear trend control. For $UIreceipt_t^A$ (the yearly mean of any take-up $UIreceipt_i^A$) as dependent and ΔUR_t as independent variable, both detrended using an hp-filter (Column (5)), a 1 p.p. increase in ΔUR_t is associated with a statistically significant 4.62 p.p. increase in $UIreceipt_t^A$. This result is robust to using a linear trend control (Column (6)) and using the other UI-receipt measures as dependent variables (Column (1) to (4)).

Panel B replicates the results from panel A, but abolishes the right-censoring restriction set in the baseline sample. Results are similar to those in panel A. The specification from Column (5) with ΔUR_t as independent variable is with 5.34 instead of 4.62 somewhat larger. In some other cases, the results are slightly smaller but are all in a similar ballpark, while the mean recipiency rates is significantly lower than in panel A.[20] Thus, the right-censoring restriction seems not to be consequential for the measured association. Similarly, Appendix Figure A.3 shows that relaxing other baseline restrictions reduces visibly the observed recipiency rate, but not the observed cyclicality. Appendix Table A.3 shows that results also replicate when using the unemployment rate or the GDP growth rate as measures for the business cycle, albeit with somewhat smaller magnitudes. Overall, a countercyclical variation of UI receipt over the business cycle is a robust feature of the data.

4.2 The Role of Observed Characteristics

The documented countercyclical variation is so far based on raw UI-receipt measures. What role do compositional changes over the business cycle play in explaining this relationship? I use two complementary approaches to address this question.

First I use a semi-parametric specification that creates a yearly recipiency measure, $UIreceipt_t^C$ that holds observed characteristics constant at its 1980 values. This variable can easily be plotted over time, without pre-specifying any time-trend in UI-receipt and links naturally to the year-level specification in the last section. This specification first uses the following regression specification at the individual level:

$$UIreceipt_{it} = \alpha + \sum_{t=1981}^{2010} \gamma_t I(year = t) + X_{it}^T \theta + u_{it} \tag{5}$$

X_{it} is a vector of control variables and γ_t a dummy-variable for year t. The yearly recipiency measure that holds observed characteristics constant can then be constructed as $takeup_t^C = takeup_{1980} + \hat{\gamma}_t$.

Second, I run individual-level regressions that include a measure for business cycle variation directly. This specification requires to specify the time-trend of UI-receipt directly, but allows for a more systematic investigation of the sensitivity of the business cycle variation with regard to different set of controls (Oster (2017)). Equation 6 shows this specification.

[20] The mean of $takeup_t^A$ drops from .686 in panel A to .487. in panel B

$$UIreceipt_{it} = \tilde{\alpha} + \tilde{\beta} BC_t + X_{it}^T \tilde{\theta} + \zeta year_t + \tilde{u}_{it} \tag{6}$$

Compared to equation 5 it replaces the year-dummies with the variable BC_t for business cycle variation with coefficient $\tilde{\beta}$, and adds a linear trend component. The vector X_{it} includes different sets of controls.

To investigate the sensitivity to controls I start with including detailed, flexible controls of individual, firm and regional characteristics. The set of individual level controls consists of dummies for gender, age in years, education groups, 2-digit occupation groups, non-German nationality, past nonemployment experience and variables for last wage and last wage-squared in Euro as well as experience and experience squared. The set of firm controls consists of 5-digit industry dummies, 20 firm-size dummies, the layoff size relative to firm size and a dummy for plant closure. Regional controls are dummies on the county (Kreis) level.

Figure 3 visualizes the influence of the different set of controls using the semi-parametric approach. Figure 3 (a) shows the development of yearly recipiency rates with and without controls, while Figure 3 (b) shows detrended time-series. While there is a slight change in the trend visible for the version with controls, the variation over the business cycle seems only slightly affected by observed characteristics, as can be seen more clearly in the hp-filtered version in Figure 3 (b). Table 3 shows parametric results from the individual level regression. Column (1) shows a version without any controls except for a linear time trend (ζ). The estimated effect size is with .0494 somewhat larger but comparable to the aggregate effect sizes of Table 2. Column (2) adds flexible individual level characteristics to the regression which drops the coefficient slightly by about 10% (from .0494 to .0443) compared to a version with no controls, while the R-squared rises notably from 2% to about 11%. Column (3) adds flexible firm and regional controls into the regression. This reduces the effect size slightly further which is about 20% lower than in a version without controls and increases the R-squared to about .15. Thus, while individual characteristics are predictive for UI-receipt they seem only to play a limited role in explaining the variation of UI-receipt over the business cycle. To measure the degree of coefficient stability more formally, I follow the method proposed by Oster (2017) and her assumptions on the maximum R-squared R^2_{max} (of 1.3 times the actual R-squared) that would be achieved in a hypothetical regression of UI-receipt that includes all relevant controls. The resulting measure for coefficient stability δ for individual controls (compared to no controls) is above 16 in the specification with individual controls only an around 9 when including all controls jointly, implying that a very high degree of selection in terms of unobserved variables -16 or 9 times the degree of selection that results from the observed controls-would be needed to overturn the association completely. In sum, about 80% of the variation over the business cycle can not be explained by detailed observed characteristics, despite having good properties in predicting UI-receipt (now R^2 rises from .02 to .15).

The controls are of interest in their own right. Appendix Table A.5 reports results from a version with controls that shows a lower recipiency rate for females, non-Germans and high-wage earners and a higher recipiency rate for highly educated and older individuals as well as those getting laid off together with other coworkers.

To investigate the coefficient stability with regard to constant unobserved characteristics, I make use of repeated observations for firms and individuals to perform individual and firm-specific fixed effects respectively. The individual fixed effect specification compares, for example, the receipt behavior of the same individual that loses her job during a boom with the decision when losing her job again in a recession (or vice versa). Table 3 Column (4) - Column (7) shows these estimates. Column (4) (Column (6)) represent OLS regressions without controls for a sample of repeated observations of the same individual (the same firm) and Column (5) (Column (7)) contains coefficients for the respective individual (firm) fixed effect specifications plus the full set of individual and regional controls, as used in Column (3).[21] The fixed effect estimates are about 23% smaller and the one with fixed effects plus full controls about 26% smaller than the version without controls. At the same time, these estimates are accompanied by a large rise in R^2. For

[21] The individual sample of column (1) - (3) selects all individuals from the baseline sample that appear with at least two distinct job loss events. The firm sample of column (4) - (6) selects all firms with at least two job loss events.

the individual fixed effect specification, the R^2 rises to above .60. Relatedly, an Oster's δ of above 9 emphasizes that a very high degree of selection for the unobserved variables would be needed to be able to overturn the observed association with labor market conditions completely.[22] The corresponding time series for the semi parametric approach are plotted as additional lines in Figure 3. While they seem to explain some part of the trend, the change in the cyclicality is visibly modest.

Taken together, observed characteristics reduce the effect size of labor market conditions by 20%-30% compared to a version without controls. At the same time, 70%-80% of the effect size can not be explained by observed characteristics and only a very high selection in terms of unobserved characteristics could eliminate the observed association completely.

4.3 Regional Labor Market Conditions

This subsection extends the analyses to the regional level. There are two reasons for doing so. First, from a measurement and identification side including a more granular level allows for a more robust inference as the number of units increases and to absorb unobserved constant confounders stemming from region and time jointly. Second, the variation of UI-receipt with regional labor market conditions is interesting in its own right and relates to studies examining the capacity of UI to mitigate regional disparities and shocks (Di Maggio and Kermani (2016)) and place-based redistribution more broadly (Gaubert et al. (2021)). I study labor market conditions at two different regional units: The county (Kreis) level comprising about 400 units and the more granular municipality (Gemeindeverband) level with about 4,400 units. Both of these administrative units are commonly used to identify effects at the regional level (Dauth et al. (2012); Fuest et al. (2018)). I use yearly information on the change in the regional unemployment rate between $t-1$ and t for the years 1998 onwards, for which information is available on both regional levels.[23] The appendix contains additional results for the unemployment rate.

[22] Due to the large R^2 in the individual sample, Oster's δ is even above 1 when setting R^2_{max} to 1 (results not shown).

[23] The information stems from the INKAR data base of the BBSR (BBSR (2018)), retrieved at https://www.inkar.de/. Information on GDP growth is not available at this level of disaggregation.

Appendix Figure A.5 displays mean recipiency rates on the county level . It reveals a big heterogeneity in recipiency rates between regions, where counties in the lowest UI-receipt decile have a recipiency rate that is about 30 p.p. lower than counties in the highest decile.

To examine the association between regional labor market conditions and take-up, I perform variants of the following regression:

$$UIreceipt_{irt} = \alpha + \beta LMC_{rt} + X_{irt}^T \gamma + e_{irt} \tag{7}$$

LMC_{rt} reflects a measure for labor market conditions in region r and year t, X_{irt} represents a vector of additional controls and $UIreceipt_{irt}$ is a take-up measure for individual i residing in location r at time of layoff t.

Table 4 shows the results of this regression for different sets of controls. Column (1) controls for a linear time trend. All other specifications incorporate a set of year fixed effects to shut down variation on the national level. As a result, estimates are based entirely on variation stemming from the regional level. In the baseline specification (Column 2 of panel A), a 1 p.p. increase in the labor market conditions in the own region is associated with a 2.55 p.p. increase in take-up, which is about one half of the association on the national level. This lower association is perhaps not surprising as it reflects variation on a very local level. In contrast to variation on the national level, individuals might for example have the opportunity to work in neighboring regions with better labor market conditions. Consistent with this interpretation, the association in Column (1) -with linear time trends only- is comparable to the variation on the national level and the more disaggregate municipality level in panel B produces somewhat smaller results than the ones on the county level. Column (3)-(6) present versions with different details of controls, consisting of the same individual, regional and firm controls as in Table 3. The influence of these controls is slightly more consequential in the regional variation. The parameter estimate from a version with all controls (Column (6)) drops by about 30% compared to the specification with year controls only. The biggest part of this drop stems from individual characteristics. The specification with individual controls only shows, in fact, the lowest association (Column 2). Oster's δ remains well above one in all specifications. In addition, the relative drop in effect sizes is similar in the version on the county level and the municipality level despite using more granular municipality-level fixed effects in the latter. In addition, Appendix Table A.6 replicates the specification using the regional unemployment rate as measure for local labor market conditions with similar results. These findings suggest that unobserved characteristics are again unlikely to overturn the negative association of UI receipt with labor market conditions.

Figure 4 plots non-parametrically the association with regional labor market conditions corresponding to Column (2) and (6) of Table 4 as binned scatter plots.

5 Mechanisms: The Role of UI Take-Up vs. Eligibility

The patterns on incidence of UI receit documented so far can either reflect UI take-up — i.e. the claiming behavior of UI eligible individuals — or variation in elegibility. In this section, I investigate the role of take-up in explaining the observed pattern. In doing so, it is helpful to frame the incidence of UI as an imperfect measure of take-up, where imperfect conditioning on eligible individuals can lead to measurement error. While the literature on take-up mostly focuses on mechanisms,[24] recent literature shows that measurement error in take-up can bias results in theory and practice (Meyer and Mittag (2017); Mittag (2019)) and might partly explain the large differences in take-up rates observed between studies. The next subsection 5.1 discusses when ineligibility can occur. In subsection 5.2, I introduce a simple measurement error framework to analyze under what conditions impfect conditioning on eligible individuals leads to measurement error and can lead to biased estimates. Subsection 5.3 provides a way to combine information from the administrative and survey data on the yearly level and uses the combined information to gauge the influence of common sources of measurement error on year-level take-up rates and aggregate cyclicality.

[24]For example, the review by Currie (2004) focuses on the role of different barriers to take-up without discussing the measurement error aspect.

5.1 How Ineligibility Can Occur

Periods of benefit receipt and dependent employment are observed with high accuracy in the administrative data. Thus, the cyclicality in observed take-up rates and their variation with regional labor market conditions reflect actual differences in benefit receipt. This does not answer the question, however, whether those differences reflect differences in take-up behavior, or might in part be due to measurement error in the eligibility status.

Mismeasurement of eligibility is less emphasized in the literature on measurement error in take-up but potentially more relevant in my context. As I don't observe eligibility directly in our data, I have to apply the institutional rules that decide on eligibility to the data. Table A.8 gives an overview of potential sources of ineligibility and how they can be addressed in the administrative as well as in the the SOEP data. The sources of ineligibility can be grouped into (a) mismeasurement in the contribution duration, (b) temporary ineligibility due to sanction periods and (c) imperfect conditioning on nonemployment. How likely are these cases in practice?

A core eligibility requirement is a minimum contribution duration through social security reliable employment in the years before job exit, something that is precisely measured in the administrative data. This information is recorded by the employer and misreporting punishable by law, reinforcing the reliability of this data. Contribution durations are, thus, measured with high accuracy and their contribution to potential measurement error should be negligible.[25]

Sanctions and related off-times are possible and not directly observable. It is however worth re-iterating that in my context voluntary quits only generate a temporary ineligibility and corresponding sanction durations are typically not longer than 12 weeks. In addition, the dynamics of benefit receipt as documented in the previous section only exhibit a modest increase of benefit receipt at common sanction durations, suggesting that this type of ineligibility only plays a modest role.

Mismeasuring eligibility due to imperfectly conditioning on nonemployment is a possibility as well: In order to qualify for UI individuals have to register as unemployed

[25]The restriction to full eligibility cases ensures that even in the presence of some small mismeasurement, individuals would still receive some benefits.

at the local UI agency which in turn requires to be currently nonemployed while also searching for a job.[26] While some of the requirements implied by this unemployment definition, such as "actively searching", can potentially be fulfilled when claiming benefits, other employment states makes claiming essentially impossible. While social security-reliable employment and nonemployment is recorded with high accuracy, states like civil servants, maternity leave, longer sickness absences or self-employment are not reported in the admin data directly. The baseline sample is designed to minimize the influence of these unobserved states. Restricting to individuals returning to employment excludes individuals that switch permanently to civil servants or other unobserved states.[27] In addition, the restriction to the employment notification "end of employment" helps to exclude individuals that are in maternity leave or enter disability insurance, while the age restriction should make interruptions due to military service unlikely. How well these unobserved states are, however, excluded in practice remains an empirical question that is addressed in subsection 5.3. The next subsection conceptionalises how this type of measurement error could affect the interpretation of results.

5.2 Unobserved Ineligibility as Measurement Error - A Framework

The literature on measurement error in take-up usually concentrates on mismeasurement in benefit receipt modelled as a measurement error in the (binary) dependent variable. (Meyer et al. (2018); Bruckmeier et al. (2019)). In contrast, in my context the error stems from an imperfect conditioning on eligibility status. I set up a simple measurement error framework that illustrates how and when mismeasurement due to imperfect conditioning can bias my estimates. [28]

Let us define $elig_i^T$ as the true but unobserved eligibility status and denote with N^E the number of eligible individuals in the population. N represents the total population of likely eligible individuals i.e. $(eli\hat{gible}_i = 1\,\forall i)$. Furthermore, let us denote with $UIreceipt_i$ the correctly observed receipt of UI benefits, and with $Pr[UIreceipt_i = 1|e\hat{lig} =$

[26]See https://www.sozialgesetzbuch-sgb.de/sgbiii/138.html for the relevant law text.

[27]This restriction has a big influence on UI take-up. Figure A.2 shows that take-up decreases considerably when including permanent exiters into the analysis.

[28]Appendix B provides a more detailed and self contained measurement error framework.

$1] = Pr[UIreceipt_i = 1] = \frac{\sum UIreceipt_i}{N}$ the population mean of our take-up variable. We are however interested in statements about take-up that require condition on the subset where eligibility is correctly measured, that is $Pr[UIreceipt_i = 1|e\hat{lig} = 1, elig = 1]$. In particular, we would like now to run the following (infeasible) linear probability model:

$$UIreceipt_i = a + \beta LMC_i + \epsilon_i \, \forall i \in N^E$$

Were LMC_i is a recentered variable for labor market conditions and $E[UIreceipt_i|LMC_i, elig_i^T = 1] = a + \beta LMC_i$, with the error term indepent by assumption. The feasible regression in our case writes:

$$UIreceipt_i = \widetilde{a} + \tilde{\beta} LMC_i + \widetilde{\epsilon}_i \, \forall i \in N$$

Following the language of Meyer and Mittag (2017) in defining $pr(elig_i^T = 0|e\hat{lig} = 1, LMC_i) = \alpha_i$ as the conditional probability of missclassification and assuming additionally that $\alpha_i = \alpha_I(LMC_i)$is a differentiable function of LMC_i where LMC_i is the only source of between individual heterogeneity in α_i, we can write the relationship between the true and the feasible regression as follows:

$$E[UIreceipt_i|LMC_i] = (1 - \alpha_I(LMC_i))(a + \beta LMC_i) \tag{8}$$

We can use now equation 8 to examine the bias on take-up for different cases of measurement error.

Special Case: Constant Measurement Error. If the probability of a false negative does not depend on labor market conditions (i.e. $\alpha_i = \alpha_I$), estimates based on equation 8 are biased in the following way:

$$\begin{aligned} &E[\widetilde{a}] = (1 - \alpha_I)E[UIreceipt_i|elig^T = 1] \\ &\text{and} \\ &E[\widetilde{\beta}] = (1 - \alpha_I)\beta \end{aligned} \tag{9}$$

This equation implies that wrongly classifying someone as eligible leads to an attenuation bias that drives the mean (i.e. the estimated take-up rate) as well as the variation with labor market conditions towards zero.

How large is α_I? Using the estimated take-up rate $E[\tilde{a}]$ and the fact that take-up is at most one, we can estimate the maximum share of false positives as $\tilde{\alpha}_I^{upper} = 1 - E[\tilde{a}] \approx 1 - .73 = .27$ This number implies that, in case of constant error, we *underestimate* the cyclicality of take-up by a factor of at most about $.27$.

General Case: Measurement Error Correlated with Labor Market Conditions. For the general case of $\alpha_i = \alpha_I(LMC_i)$ we can obtain the following expression:

$$E[\widetilde{\beta}] = (1 - \alpha_I)\beta - E[\frac{\partial \alpha_I}{\partial LMC}]E[UIreceipt_i | elig^T = 1] \tag{10}$$

Equation 10 shows that on top of the attenuation bias in the constant error case, we have a term that depends on the direction and size of the relationship between error and labor market conditions. It shows that, for our results to be completely spurious and recalling that in our case $E[\widetilde{\beta}] \approx -5$, would require an $E[\frac{\partial \alpha_I}{\partial LMC}] \geq 5$, that is a one p.p. increase in the national unemployment rate would need to reduce the share of wrongly classified as eligible by at least 5 p.p.

5.3 Adjusting for Unobserved States

Adjustment Method. Using the SOEP data and a sample that follows closely the selection process in the administrative data (see appendix A for details on the sample construction), I generate a yearly take-up rate for any UI take-up, called $UIreceipt_t^{SOEP}$. Appendix Figure A.6 shows that the raw take-up measure in the SOEP and the admin sample track each other quite close, fostering confidence in that admin and survey data sample correspond to the same or similar population. In addition, I generate an adjusted measure in the SOEP $takeup_t^{Adjust,SOEP}$, that excludes individuals that enter states that would conflict with UI receipt and are not observed in the administrative data, but are instead observed in the SOEP. The states considered in the baseline adjustment are maternity leave, self-employment, civil servants, retirement and disability receipt. The difference between those states -$\Delta UIreceipt_t^{SOEP}$- shows how consequential the omission of these states is for measured take-up in the SOEP data. To relate this difference back to the administrative data I construct the following adjusted take-up measure:

$$takeup_t^{adj} = UIreceipt_t + \Delta UIreceipt_t^{SOEP} \tag{11}$$

One advantage of this method is that it is only relying on survey information to gauge the influence of unobserved states while it allows for a direct comparison to the year-level take-up of the administrative sample.[29] As a second advantage, this procedure can be used flexibly to incorporate other adjustments of the take-up measure, including changes in the administrative sample like restricting to longer nonemployment durations or to layoffs due to plant-closure, adjustments that holds observed characteristics constant or to implement combinations thereof.

Adjustment Results. Table 5 shows how different adjustments affect mean take-up and the coefficients of the association between take-up and labor market conditions. As the SOEP sample starts in the year 1985, the table contains results for two periods: A full period that covers the years 1980 to 2010 in panel A, containing adjustment results that are possible with the administrative data only, and in panel B a SOEP period that ranges from 1985 to 2010 and contains also adjustments that combine administrative and SOEP data.

I start with performing different adjustments that are only based on the administrative data, and can therefore be performed in both data sets. Column (1) replicates the year-level association with raw take-up for the full period as well as the SOEP period. The effect size in the SOEP period is with .055 somewhat larger than the full period, at the same time standard errors are larger, potentially reflecting the small number of observations for this time period. The relative drop in the effect size from holding observed characteristics constant (using the flexible controls specification as in Table 3 Column (3)) is -again-

[29] Relatedly, survey-related differences to the administrative measures such as systematic misreporting in the survey data (Bruckmeier et al. (2019)) are likely mitigated on the yearly aggregate level.

about 20% as shown in Column (2). Increasing the minimum nonemployment duration in the administrative sample to at least 4 months in Column (3) instead changes barely the coefficient compared to the raw association, suggesting that short ineligibility periods at nonemployment entry contribute little to the observed cyclicality. To investigate the issue of short ineligibilities further, I restrict to layoffs due to large plant closures in Column (4). This restriction has the advantage that it ensures that separation was involuntary and that individuals did not agree to an earlier separation, which are both prerequisites for temporary ineligibility at nonemployment start. In this case, the mean take-up rate increases by about 12 percentage points to 84%, while the coefficient decreases slightly to .051 in the SOEP period and somewhat more to .037 over the full period. There are, however, reasons aside from a potential cyclicality of temporary ineligibility that could lead to a dampening of the association in this case.[30]

In a next step, I perform adjustments that incorporate the additional information from the SOEP data, concentrating on the SOEP period. The specification in Column (5) adjusts the raw take-up measure from the admin data for unobserved states that are observed in the SOEP data. In particular, it excludes individuals in parental leave, self-employment, civil servants, retirement and disability receipt. In this specification, the mean take-up rate increases by 7 percentage points relative to the raw take-up measure indicating a notable role of measurement error from imperfectly controlling for these states in the baseline sample. Importantly however, the accompanied drop in the coefficient of ΔUR_t to .050 is modest at most, suggesting that the observed cyclicality in take-up is largely immune to this type of measurement error.[31]

In a final step, Column (6) and (7) combine the different adjustments. In Column (6) all but the plant-closure adjustment are implemented simultaneously, leading to a drop of about 27% or almost 1/3 relative to the raw version and the coefficient being only significant on the 5% level. Column (7) shows effect sizes for a version that combines

[30] A plant closure might reflect bad job opportunities for affected workers even in booms. Similarly, potential spillover effects from other laid off workers as mechanism for the counter cyclicality should be less relevant for plant closures, where several –often similarly skilled– workers get laid off at the same time.

[31] We can make a bounding argument in the spirit of Oster (2017): Since take-up is at most 1 and given that in our case a 7 percentage point increase in the take-up rate goes hand in hand with a drop in the take-up coefficient by 0.05 we can argue that the cyclicality of additional unobserved states has to be about at least three times the association of the considered unobserved states.

all adjustments including the plant closure adjustment jointly. In this case, the relative decrease is 20% with a coefficient significant on the 1% level.

Appendix Table A.7 repeats the adjustment process with the unemployment rate and GDP growth as independent variables. The relative drop in these cases appears somewhat larger. Column (6), for example, shows that already without the plant closure restriction the coefficient drops by about 35% when using GDP growth as independent variable and by 65% when using the unemployment rate. These coefficients exhibit, however, large standard errors and are –as indicated by results of previous sections– less robust in general, limiting the interpretability of those coefficients somewhat.

In sum, adjusting for different sources of measurement error and holding observed characteristics constant appears to dampen the cyclicality somewhat, while at least for ΔUR_t as measure of labor market conditions there is a sizable and significant countercyclical pattern when adjusting for all issues jointly.

The main adjustments of $takeup_t^{adj}$ are also plotted in Figure 5 (a) and compared with the raw recipiency rate $UIreceipt_t$ for the baseline admin data. The adjusted take-up rate that excludes confounding states in the soep data follows the unadjusted admin data closely. The adjusted measure slightly lies above the unadjusted in most years, indicating the attenuation of mean take-up in the admin sample due to failure of excluding those states. The cyclicality appears as well similar for the raw and the adjusted measure. Figure 5 (b) compares the cyclicality of adjusted and unadjusted measures more directly by plotting the hp-filtered take-up rates vs. the filtered unemployment rate.

Taken together, the documented cyclicality in take-up appears robust to adjusting for a variety of potentially confounding states as well as to the inclusion of observed characteristics.

6 The Buffering Role of UI

[Under construction]

In this section, I investigate the role of incomplete take-up and UI receipt for the buffering role of UI both on average and over the business cycle and compare it with counterfactual take-up regimes. This allows for a comparison of how the average generosity of UI would change when take-up parameters, i.e. take-up rate as well its cyclicality over the business cycle change. As measure for the generosity of UI, I focus on the the average cumulative payments of UI receipt per individual unemployed.

I report results for bot the actual parameters of the UI-system, and two counterfactual scenarios: One where the take-up rate is as in the actual data but not allowed to vary with labor market conditions at the time of job-loss, and another one where take-up is complete. The average cumulative payments of UI receipt for the actual scenario are calculated from the data. The counterfactual scanarios are based on a number of simplyfing assumptions and should therefore be interpreted with a grain of salt. In particular, I abstracts from changes in nonemployment durations which ignores for example from behavioral responses to UI receipt and assume that marginal UI recipients have the same characteristics as current UI recipients. Furthermore, I assume that changes in take-up are proportional over the first year after job loss, which is ilustrated in figure (2).

The results are presented in table 6. The actual scenario is depicted in Panel A Column (1) - (3). Here the average UI recipient receives 7,532 in regular times but 7,945 in a scenario where the unemployment rate increases by 1 percentage point. Column (4) - (6) of the same panel holds the take-up rate constant over the business cycle. Panel B repeats the exercise with a scenrio of complete take-up, of 9,840.

Overall, these scenarios underscore that countercyclical take-up contributes to a buffering role of UI for the average job separator compared to a scenario where take-up where held constant, while at the same time less generous than a scenario where take-up is held constant.

7 Conclusion

Understanding how unemployment insurance affects laid off workers requires knowledge on how individuals select into UI. This paper provides new evidence on the incidence of unemployment insurance receipt and take-up and how it varies with labor market conditions. It documents a sizable and negative association of UI take-up with labor market conditions that is only in small parts explained by observed differences or measurement error in UI take-up. It provides evidence consistent with a self selection into UI due to higher benefits in recessions, important for models of the labor market that rely on this type of selection (Chodorow-Reich and Karabarbounis (2016) and Kettemann (2017)) and relates to studies concerned that self selection into UI changes the pool of unemployment along the business cycle (Kroft and Notowidigdo (2016)).

While my findings are consistent with a cost-benefit trade-off in determining UI take-up, they remain silent on which costs or barriers prevent job losers from taking up UI in practice and on the optimality of such barriers from a welfare perspective. In light of large heterogeneity in observed take-up between individuals, regions and firms it could be of interest to examining those aspects in future research.

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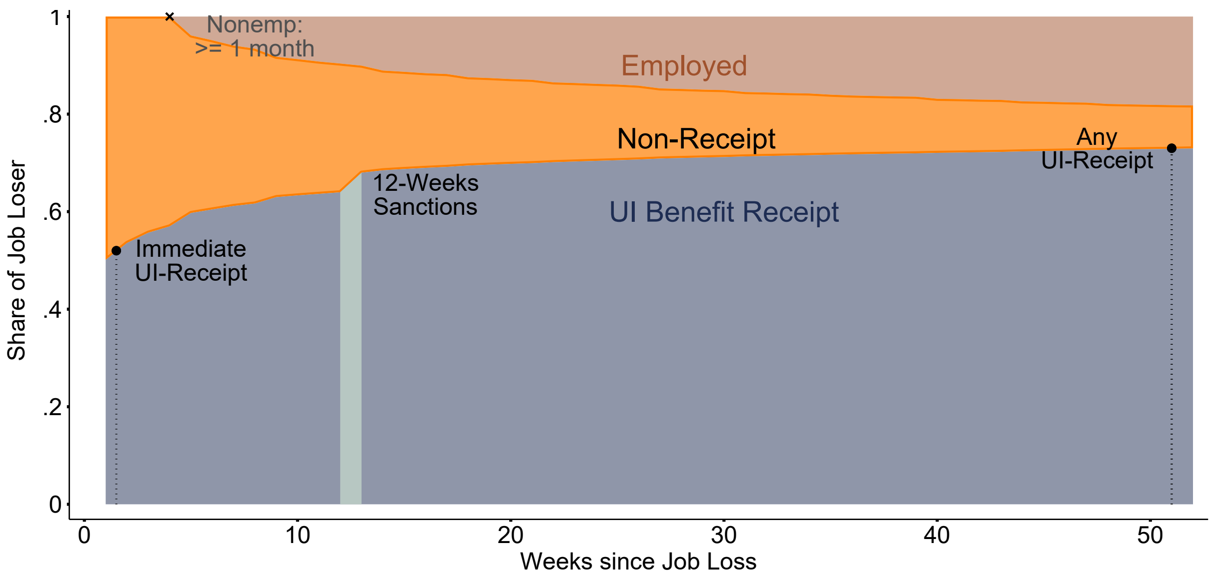
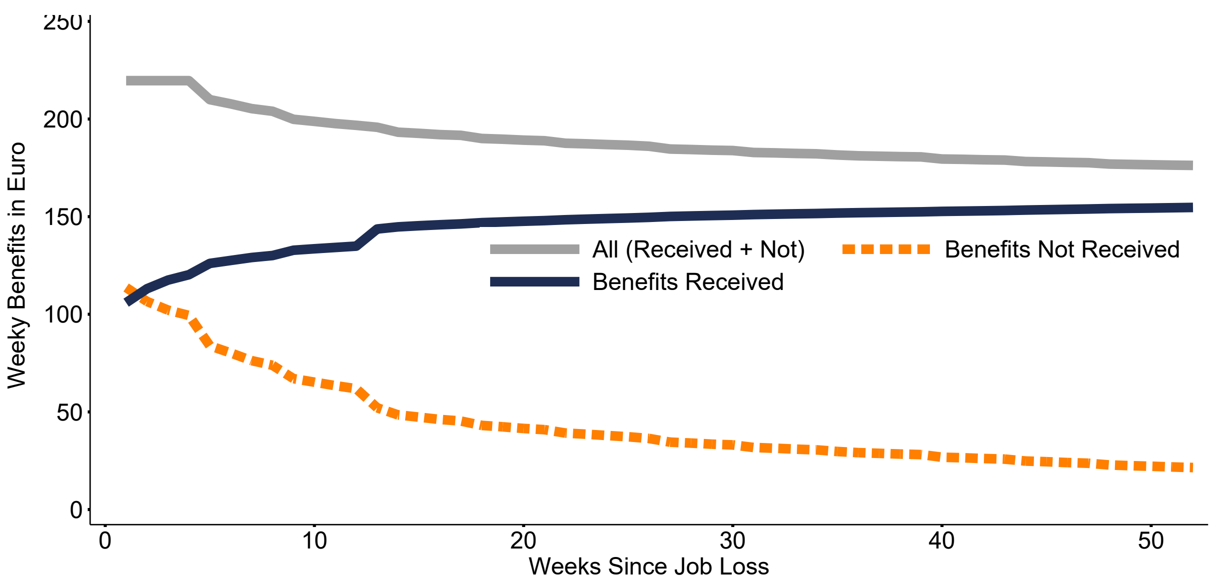
Figure 1: The first Year after Job Loss



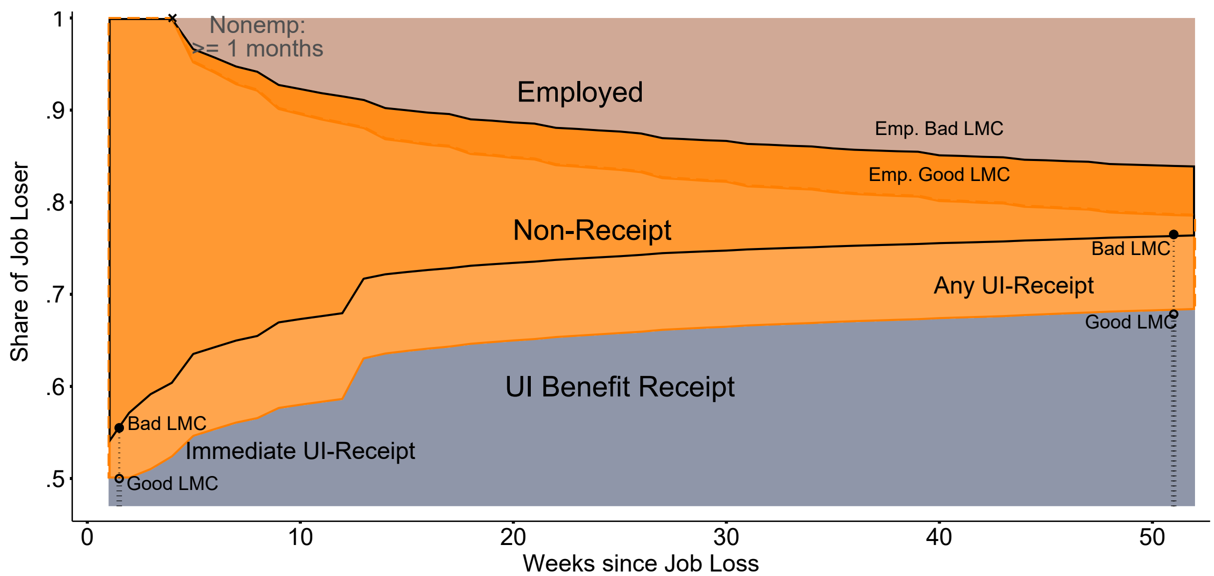
(a) States after Job Loss: UI-Receipt, Non-Receipt and Employment



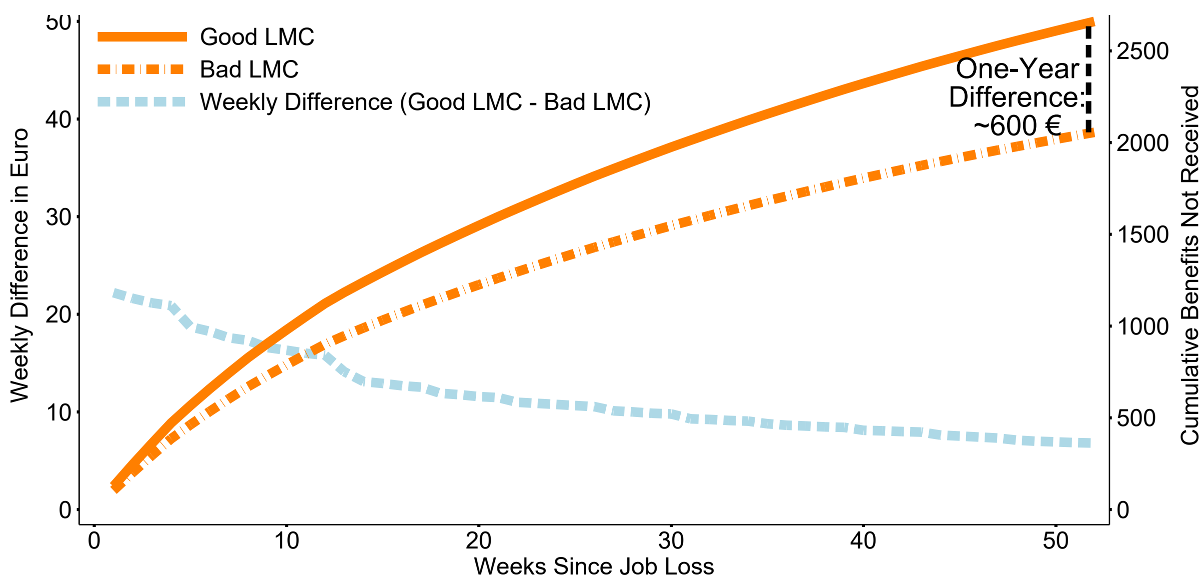
(b) Benefits (Not) Received

Notes: This figure shows (a) the evolution of different labor market states over time since job loss and (b) the amount of weekly benefits received and weekly benefits left on the table (in 2010 values) for the baseline sample. Figure (a) tracks the labor market states of all individuals in their first year after job loss on a weekly level, where employment and UI receipt are defined as absorbing states. Figure (b) shows the corresponding evolution of the weekly claimed and unclaimed benefits. Benefits are imputed as .43 x the gross pre-earnings in 2010 values.

Figure 2: The first Year after Job Loss by Labor Market Conditions



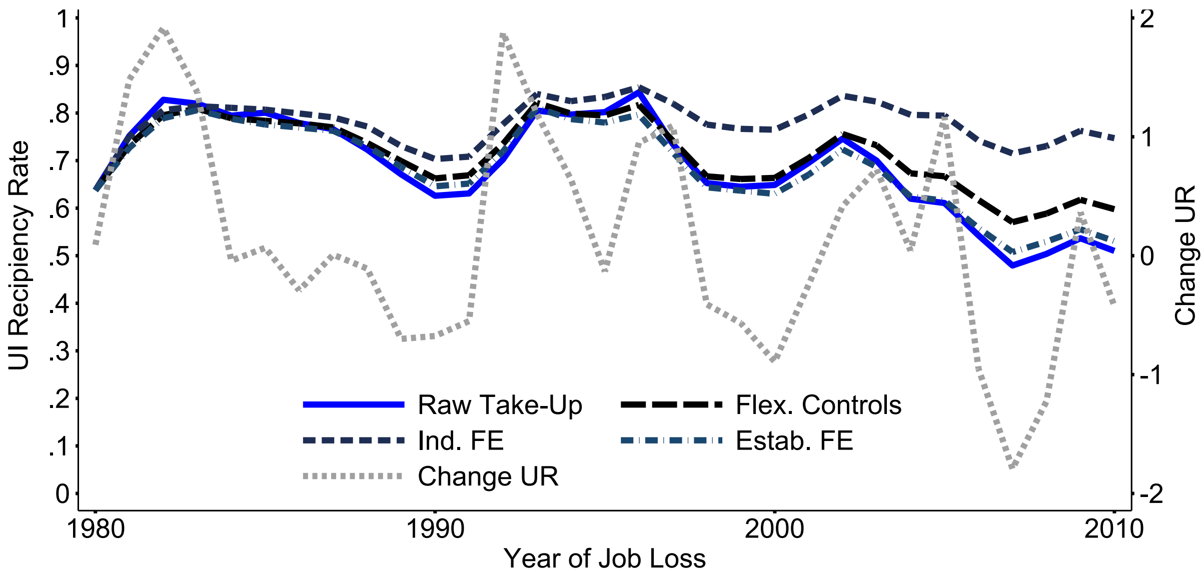
(a) States after Job Loss: Good vs. Bad LMC



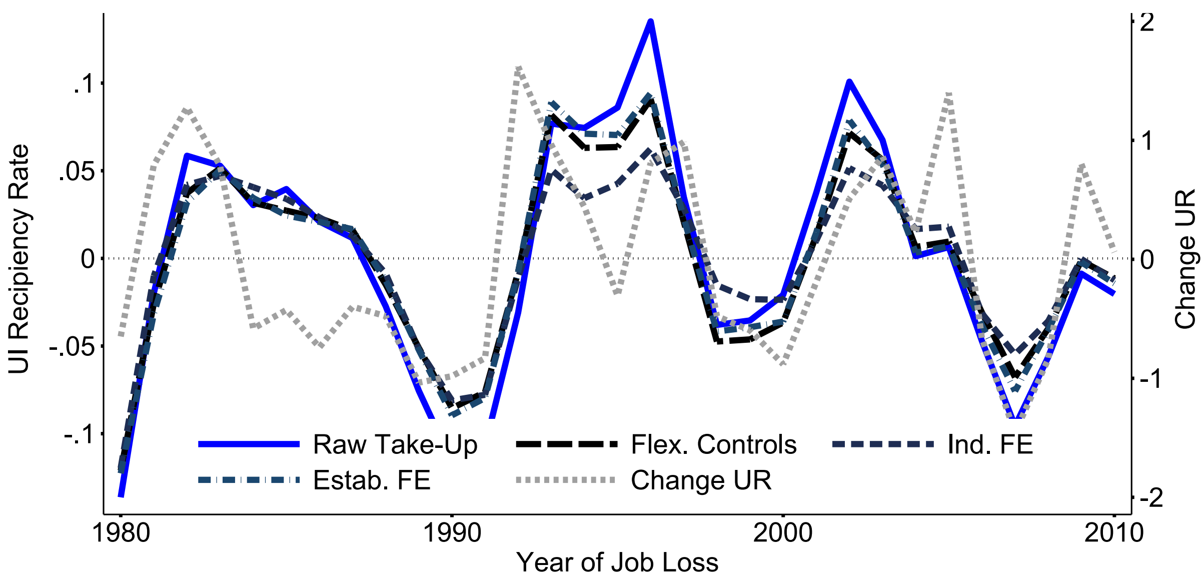
(b) Benefits (Not) Received by Labor Market Conditions

Notes: This figure shows the evolution different labor market states over time since job loss for times when labor market conditions are good vs. when they are bad (a) at time of layoff, measured by a decreasing vs. increasing national unemployment rate. Figure (b) plots the mean of the cumulative imputed benefits received per person (.43 x the mean gross pre-earnings in 2010 values) for good and bad labor market conditions respectively.

Figure 3: Labor Market Conditions and the Role of Individual and Firm Characteristics



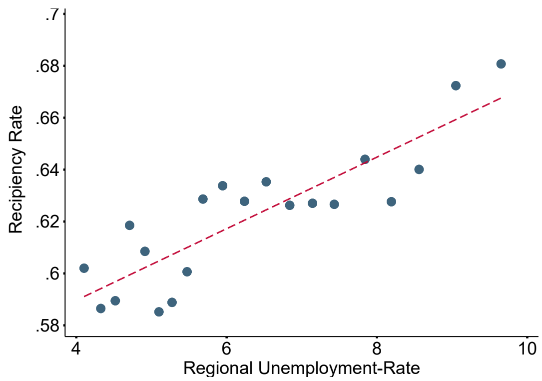
(a) Raw vs. All Controls



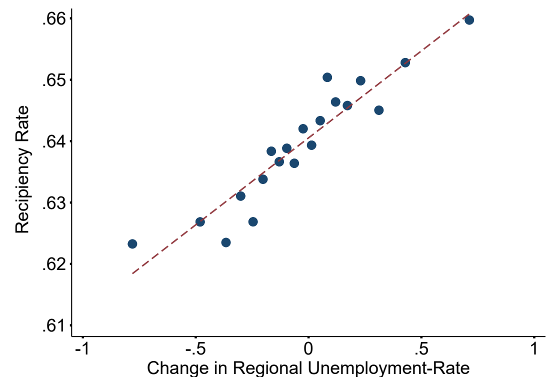
(b) Raw vs. All Controls, HP-Filtered

Notes: This figure shows UI receipt and a measure for labor market conditions over time for the baseline sample, using different sets of controls. Figure (a) shows raw means on the yearly level, figure (b), (d)) show the corresponding hp-filtered time series using smoothing parameter 1600 (the default in Stata). Flex. Controls, refers to the specification in table 3 col (3) that controls flexibly for individual, establishment and regional controls. Ind. FE refers to col. (5) in table 3, that controls for individual fixed effects, and estab-FE are etablishment fixed effects as in col (7) of table 3.

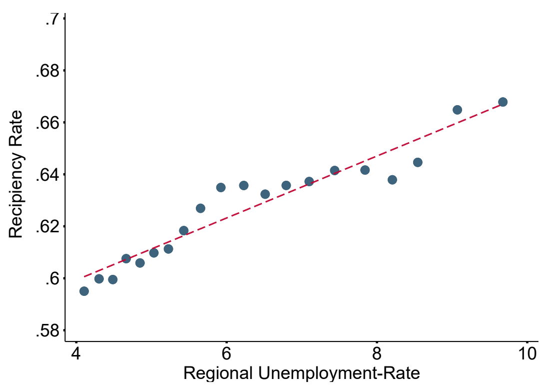
Figure 4: UI Receipt and Local Labor Market Conditions



(a) UI receipt and Regional UR: No Controls



(b) UI receipt and Regional ΔUR: No Controls

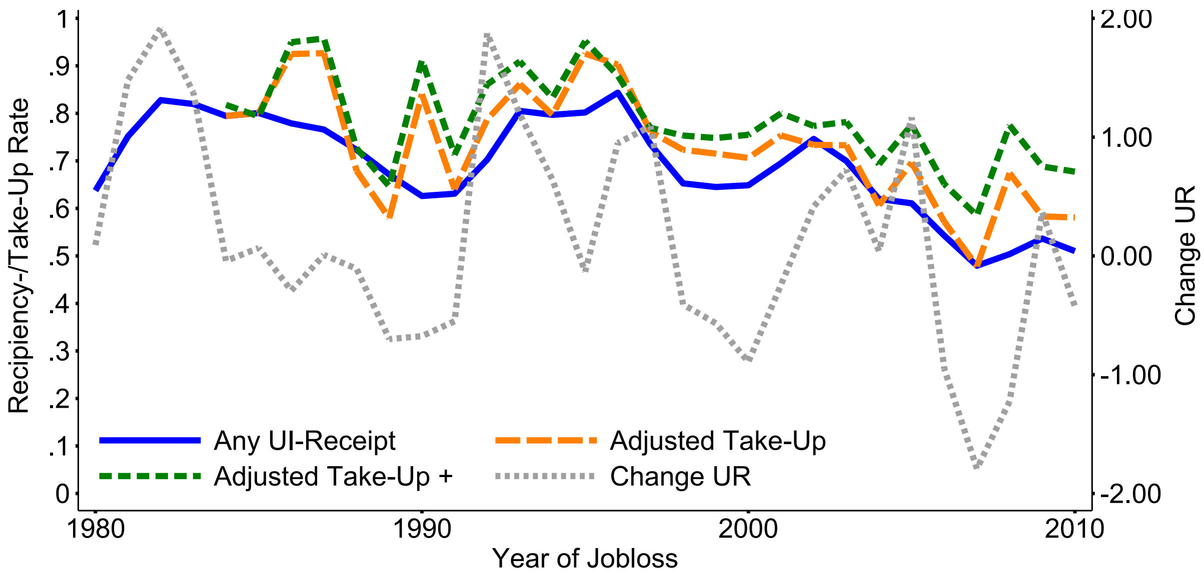


(c) UI receipt and Regional UR: All Controls

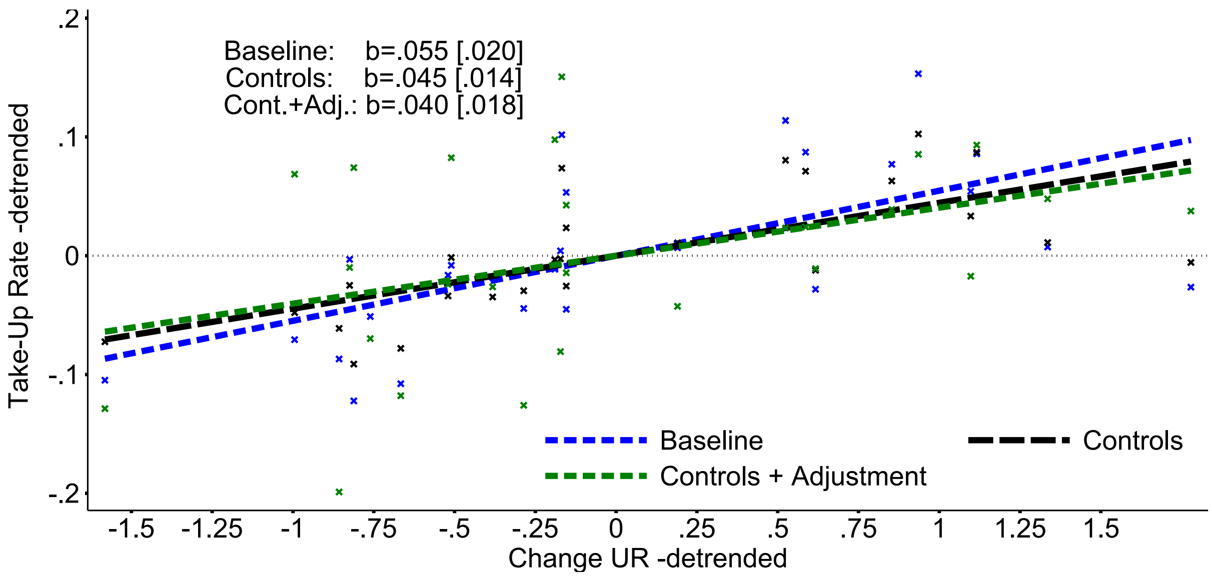
(d) UI receipt and Regional ΔUR: All Controls

Notes: This figure shows the association between any UI receipt and (the change in) the local unemployment rate defined on the county level. Values below the 1st and above the 99th percentile are capped. The graphs with no controls includes year fixed effects only. The graphs with all controls includes -beside the year fixed effects- the same set of variables as in column (6) of table 4.

Figure 5: UI Receipt and Take-Up vs. Eligibility



(a) Adjusting for Ineligible States



(b) Adjusting for Ineligible States - HP-Filtered

Notes: This figure shows UI receipt and take-up rates over time (filtered and unfiltered) for any UI receipt in the administrative data, adjusting for states observed in the SOEP-data but not in the admin data. Adjusted states exclude cases where individuals are either in self-employment, maternity leave, pensions, civil servants, military occupations or similar states based on information in the SOEP. The 'adjusted+' specification further controls for observed differences (as in table 3 col (3)) and restricts to nonemployment durations longer than 3 months to exclude delayed take-up due to sanctions. Figure (a) shows the time-series while figure (b) shows the scatter-plot and corresponding estimates of the effect sizes (compare table 5 panel B col (1), (2) and (6)). Figures are based on the SOEP-period (1985-2010).

Table 1: Summary Table of Baseline Samples: Administrative and Survey Data

		Full Period: 1980 - 2010	SOEP Period: 1985 - 2010	
		Administrative Sample		SOEP Sample
Name	Description	(1)	(2)	(3)
Incidence of UI receipt				
$UIreceipt^A$	Indicator variable, $= 1$ if at least some UI benefit receipt	0.73	0.72	0.77
	before end of nonemployment spell	[0.44]	[0.45]	[0.42]
$UIreceipt^I$	Immediate UI benefits (within 10 days)	0.52	0.51	
		[0.50]	[0.50]	
$fracUI$	fraction of days in nonemployment during which individuals	0.65	0.64	
	receive UI within first year of nonemployment	[0.44]	[0.44]	
Individual Characteristics				
age	Age in years at start of nonemployment	36.7	36.7	39.0
		[8.38]	[8.38]	[8.37]
female	Indicator variable, $= 1$ if individual female	0.29	0.30	0.24
		[0.45]	[0.45]	[0.43]
wage	Daily gross wage in Euro at last job before nonemployment	64.59	68.31	80.1
	wages are top coded at SSC limit in admin data	[25.76]	[26.36]	[28.8]
parttime	Indicator variable, $= 1$ if last job before nonemployment	0.06	0.06	0.017
	was reported as part-time (excluding minor employment)	[0.23]	[0.24]	[0.13]
nongerman	Indicator variable, $= 1$ if reported nationality is not German	0.11	0.10	0.10
		[0.31]	[0.30]	[0.36]
N		4,718,394	3,851,180	775

Notes: This table shows mean and standard deviation (in brackets) for selected variables in the baseline samples of the administrative and the SOEP survey data. The full period covers years between 1980 and 2010 and the SOEP period years between 1985 and 2010. Column (1) shows descriptives for the baseline administrative sample covering the full period, column (2) shows descriptives for the same sample but restricting to the SOEP period and column (3) shows descriptives using the SOEP sample and the SOEP period. Means in the SOEP sample are calculated using cross-sectional survey weights.

Table 2: Incidence of UI receipt and the Business Cycle: Year-Level Regression

	Immediate UI receipt $takeup^I$		Fraction of Insured Nonemp. $fracUI$		Any UI receipt $takeup^A$	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline Sample						
ΔUR_t	0.0447*	0.0455*	0.0491**	0.0504**	0.0462**	0.0476**
	[0.0164]	[0.0177]	[0.0155]	[0.0164]	[0.0140]	[0.0151]
R^2	0.226	0.516	0.316	0.597	0.340	0.624
Panel B: No Right-Censoring of Nonemp. Duration						
ΔUR_t	0.0416**	0.0421*	0.0516**	0.0527**	0.0534**	0.0546**
	[0.0147]	[0.0161]	[0.0149]	[0.0163]	[0.0146]	[0.0164]
R^2	0.235	0.603	0.339	0.684	0.363	0.696
N obs.	31	31	31	31	31	31
Mean Depvar Panel A	0.455	0.455	0.592	0.592	0.686	0.686
Mean Depvar Panel B	0.320	0.320	0.443	0.443	0.487	0.487
Trend-Control: HP-Filter	x		x		x	
Trend-Control: Linear		x		x		x

Notes: This table shows year-level regressions of the association between labor market conditions and different take-up measures. Panel A shows results for the baseline sample (collapsed to the yearly level) and Panel B for a sample that removes the right-censoring restriction of the nonemployment duration that is used in the baseline sample. Robust and bias-corrected (HC3) standard errors are in brackets. +, *, ** and *** denote significant levels on the 10 %, 5%, 1% and 0.1% significance level respectively.

Table 3: Take-Up and the Business Cycle: Controls

	Baseline Sample			Individual FE Sample		Establishment FE Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ΔUR_t	0.0494***	0.0443**	0.0393**	0.0397**	0.0303***	0.0519**	0.0397***
	[0.0146]	[0.0138]	[0.0128]	[0.0123]	[0.0079]	[0.0161]	[0.0107]
R^2	0.019	0.108	0.146	0.009	0.616	0.019	0.322
Mean Indep. Var	0.345	0.345	0.345	0.319	0.319	0.365	0.365
Oster's δ		16.945	9.067		9.430		12.708
N obs.	4718394	4718394	4718394	812266	812266	4080377	4080347
Mean Dep. Var	0.730	0.730	0.730	0.772	0.772	0.732	0.732
Trend Controls	x	x	x	x	x	x	x
Individual Controls		x	x		x		x
Firm & Regional Controls			x		x		x
Firm Fixed Effects							x
Individual Fixed Effects					x		

Notes: This table shows individual level regressions of the association between labor market conditions and any UI receipt for different sets of controls. Standard errors are bootstrapped with clusters on the yearly level and 100 replications. The independent variables are the yearly change in the national unemployment rate in panel A, the yearly unemployment rate in panel B and the yearly growth rate of GDP in panel C. Osters' delta is calculated relative to column (2) and assuming a maximum R^2 of 1.3 times the actual R^2. Individual controls are dummies for gender, age in years, education, 2-digit occupation groups, non-German nationality, past nonemployment experience and variables for last wage and last wage-squared in Euro as well as experience and experience squared. Regional controls are dummies on the county (Kreis) level. Firm-level controls consist of 5-digit industry dummies, 20 firm-size dummies, the layoff size (relative to firm size) and a dummy for plant closure. +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1 % significance level respectively.

Table 4: Any UI receipt and Local Labor Market Conditions

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Local Labor Market Conditions at County Level						
$\Delta UR_{t,r}$	0.0639***	0.0255***	0.0157***	0.0170***	0.0242***	0.0183***
	[0.0024]	[0.0056]	[0.0037]	[0.0041]	[0.0038]	[0.0025]
R^2	0.022	0.028	0.166	0.134	0.079	0.194
Oster's δ			4.350	5.097	38.093	6.847
Panel B: Local Labor Market Conditions at Municipality Level						
$\Delta UR_{t,r}$	0.0529***	0.0178***	0.0110***	0.0121***	0.0163***	0.0126***
	[0.0017]	[0.0025]	[0.0020]	[0.0022]	[0.0023]	[0.0016]
R^2	0.021	0.028	0.166	0.134	0.092	0.202
Oster's δ			4.486	5.417	25.201	6.834
N obs.	1107735	1107735	1107735	1107735	1107735	1107735
N counties	402	402	402	402	402	402
N municipalities	4475	4475	4475	4475	4475	4475
Year-FE		x	x	x	x	x
Individual Controls			x			x
Firm Controls				x		x
County/Municipality -FE					x	x

Notes: This table shows individual level regressions of the association between regional labor market conditions and any UI take-up. The regional level r is defined on the county (i.e. Kreis) level for panel A and on the municipality (i.e. Gemeindeverband) level for panel B, and t refers to the yearly level. Standard errors are clustered on the county (Kreis) level in panel A and on the municipality (Gemeindeverband) in panel B. Individual controls are dummies for gender, age in years, education, 2-digit occupation and variables for last wage and last wage-squared in Euro. Regional Controls are county fixed effects. Firm-level controls consist of 5-digit industry controls and 20 firm-size dummies. +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1 % significance level respectively.

Table 5: Any UI receipt: Take-Up vs. Eligibility and the Business Cycle: Controls and Adjustments

	Raw	Control	Long Nonemp.	Plant Closure	Adjusted States	Combined (1)-(3), (5)	All Combined (1)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full Period (1980-2010)							
ΔUR_t	0.048**	0.038**	0.047**	0.037**			
	[0.015]	[0.011]	[0.014]	[0.013]			
R^2	0.624	0.561	0.642	0.695			
Mean Dep. Var	0.690	0.713	0.716	0.842			
Panel B: SOEP Period (1985-2010)							
ΔUR_t	0.055*	0.045**	0.054**	0.051***	0.050*	0.040*	0.045***
	[0.020]	[0.014]	[0.018]	[0.011]	[0.023]	[0.018]	[0.011]
R^2	0.703	0.721	0.722	0.772	0.502	0.423	0.622
Mean Dep. Var	0.676	0.705	0.700	0.824	0.730	0.783	0.921
Trend Control (linear)	x	x	x	x	x	x	x

Notes: This table shows regressions on the yearly level of the association between any UI receipt and labor market conditions for controls and different adjustments to measurement error. Column (1) shows raw UI receipt and column (2) UI receipt holding observed characteristics constant. Column (3) restricts to nonemployment durations of at least 4 months and column (4) restricts to large plant closures in the admin data, both of which are intended to address sanctions or related temporary ineligibility periods. Column (6) adjusts for unobserved states using information from the SOEP, column (7) and (8) provide combinations of these adjustments. ΔUR is the percentage change in the national unemployment rate,g the GDP growth rate and UR the yearly unemployment rate. Full period refers to the baseline period between 1980 and 2010 and the SOEP period between 1985 and 2010, the period for which SOEP as well as administrative information is available. Robust, bias-corrected standard errors (HC3) are reported in brackets. +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1 % significance level respectively.

Table 6: The Buffering Role of UI for Different Scenarios of Take-Up [in Progess]

Actual Cyclicality $\partial anyUI/\partial \Delta UR = .04$			Constant Cyclicality $\partial anyUI/\partial \Delta UR = 0$		
$\Delta UR = 0$	$\Delta UR = 1$	$(\Delta UR = 1) - (\Delta UR = 0)$	$\Delta UR = 0$	$\Delta UR = 1$	$(\Delta UR = 1) - (\Delta UR = 0)$
(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mean UI-Receit ($= .73$**)**					
7532	7945	412.73	7532	7532	0
[85.60]	[90.29]	[4.69]	[85.60]	[85.60]	-
Panel C: Full UI Receit ($= 1$**)**					
-	-	-	9840	9840	0
-	-	-	[88.76]	[88.76]	-

Notes: This table shows the buffering role of UI after job separation as the average UI-receipt per seperated worker in the first year after job loss for different take-up scenarios. The actual scenario is depicted in Panel A Column (1) - (3). Column (4) - (6) of the same panel holds the take-up rate constant over the business cycle. Panel B repeats the exercise with a scenrio of complete take-up. Standard errors are in brackets.

A Data Construction Steps in the SOEP

The SOEP sample is based on the Socio Economic Panel (SOEP) version 32.1 covering the years 1984-2015. The following data sets of the SOEP are used:

- PKAL: Contains monthly information on basic labor market states. This information is gathered retroactively for the last year before the yearly interview takes place. For all years labor market states consist of full and part-time employment, vocational training, registered unemployment and "other" states. For later years, information on maternity leave, household work, mini-jobs, retraining and short-time work is included.

- PGEN: Contains person related generated variables. This includes personal characteristics such as sex, age, nationality and marital status, information on education and vocational training as well as detailed information on the current labor-market status, including information on occupation, whether individuals are self-employed, the current wage, and - in case of an employment exit- the circumstances of the exit. This data is available at the yearly level and captures the state at the time the interview takes place -in most cases between February and May.

- In addition, I use selected variables from the yearly person and household data sets.

Based on the above data sets I construct a sample that follows the restrictions in the administrative data as close as possible. The data construction proceeds in the following steps:

1. Using the PKAL data, I construct a monthly panel of all surveyed individuals in the SOEP from 1984 to 2015. All labor market states are converted into a binary employment state, employment and nonemployment. Individuals are employed if they worked either part- or full-time in the respective month, and nonemployed else. A new state starts if individuals change from employment to nonemployment (or reverse).

2. To this monthly panel I merge the additional information from the yearly PGEN and the additional selected variables from the yearly questionnaire. The information is merged on the person month level. To achieve this, I construct a month-id (corresponding to the month-id in the monthly panel) from the year and month variables at which the interview takes place in the yearly questionnaire. If the month of the interview is missing, I replace it with the month May (the modal month at which the interview took place). I impute missing values for non-survey months, by replacing each value with the last non-missing value of previous months within the same employment state and, in a second step with the first non-missing values of the next months.

3. In a next step, I construct labor-market states from the yearly variables. These variables allow for an employment/nonemployment definition that follows closer the one in the IAB data. In particular, it allows for a distinction between dependent

employment and other employment-episodes such as self-employment or civil servants which are included in the employment definition from the monthly panel. From the variable stib, the values 210-340 and 510-550 are selected as these contain jobs that are usually social security reliable dependent employment. With this information, I update the state definition to also capture transitions from dependent employment to other (employment) states and reverse. These additional changes occur by construction at the month at which the interview was held. For each resulting state I calculate the beginning, end and duration in months. In addition, I calculate variables relevant for UI eligibility: The duration individuals were in dependent employment in the last 2 and in the last 5 years (without nonemployment interruptions) at the end of each employment state.

4. I construct further variables that are needed for mimicking the sample-restriction in the admin-data. Age in years stems from the personal-related data. Information from gross monthly earnings stem from the yearly questions in the last survey before entering nonemployment and are inflation adjusted (with 2000 as base year). The reason for employment exit stems from the personal-related information as well. While this variable is different than the one from the admin-data it allows for excluding cases, which are likely excluded by the admin-data as well, such as cases related to sickness, retirement and pregnancy or cases that indicate prior self-employment. This leaves the variable job end with the following reasons (corresponding values in brackets): employer initiated layoff (1), fixed -term contract (2), voluntary quit (4), both sided quit (5), plant closure (11).

B Measurement-Error a Framework

Take-up can be measured with error. Errors could occur if:

1. Individuals are in an unobserved employment state or other states that would invalidate UI receipt. For example, entering self employment, migration, maternity leave, sickness.
2. Individuals could be temporary ineligible due to a sanction period at UI entry.

Both cases could lead to falsely assigning individuals as not taking up UI. The remainder of this appendix section shows this error and its implication more formally. It draws heavily on Meyer and Mittag (2017).

B.1 Measurement Error on the Individual-Level

Notation:

- $takeup_i^T$: True, but unobserved take-up dummy
- $takeup_i$: Observed, but potentially contaminated take-up dummy
- N: are the numbers of all observations

- N^T: are the number of cases where there is no error in measured UI take-up (i.e. $takeup_i^T = takeup_i$)
- N^F: are the number of cases where there is measurement error.
- In our setting, we are merely concerned with "false negatives", defined as[32]:

$$Pr(y_i = 0 | y_i^T = 1) = \alpha_{1i} \tag{A.1}$$

Following the language of Meyer and Mittag (2017), this is the conditional probability of misclassification.

We would like to run the following (infeasible) linear probability model:

$$takeup_i^T = \beta LMC_i + \epsilon_i \tag{A.2}$$

Were LMC_i is a variable for labor market conditions and $E[takeup_i^T | LMC_i] = \beta LMC_i$. The error is here independent by assumption:

$$E[\epsilon_i | LMC_i] = 0 \tag{A.3}$$

Thus, running the infeasible regression of equation A.2 would deliver a consistent estimate $\hat{\beta}$: $E[\hat{\beta}] = \beta$.

The feasible regression writes:

$$takeup_i = \widetilde{\beta} LMC_i + u_i + \widetilde{\epsilon}_i \tag{A.4}$$

Where $\widetilde{\epsilon}_i$ is again an error term independent of labor market conditions $E[\widetilde{\epsilon}_i | LMC_i] = 0$ and u_i is the measurement error that takes on the following form:

$$u_i = takeup_i - takeup_i^T = \begin{cases} -1 & \text{if } i \text{ is a false negative} \\ 0 & \text{if } i \text{ is reported correctly} \end{cases} \tag{A.5}$$

The OLS-coefficient (if it were feasible) of regressing u_i on labor market conditions LMC_i is:

$$\hat{\delta} = \frac{1}{\sum LMC_i^2} \cdot \sum LMC_i \cdot u_i \tag{A.6}$$

this simplifies in our case to:

$$\hat{\delta} = \frac{1}{\sum LMC_i^2} \left\{ \sum_{i\, s.t.\, y_i = y_i^T} LMC_i \cdot 0 + \sum_{i\, s.t.\, y_i = 0 \& y_i^T = 1} LMC_i \cdot (-1) \right\} = -\frac{N}{\sum LMC_i^2} \left(\frac{N_{FN}}{N} \bar{LMC} \right) \tag{A.7}$$

This is generally nonzero. The bias writes as:

$$E[\hat{\delta}] = -\frac{N}{\sum LMC_i^2} Pr(y_i = 0, y_i^T = 1) E[LMC_i | y_i = 0, y_i^T = 1] \tag{A.8}$$

[32] "False positives" would write as $Pr(y_i = 1 | y_i^T = 0) = \alpha_{0i}$. Here I assume $\alpha_{0i} = 0$.

Special Case: Constant Error

Let's now further assume that the probability of misclassification is constant, i.e.:

$$Pr(y_i = 0|y_i^T = 1) = \alpha_{1i} = \alpha_1 \tag{A.9}$$

This assumption rules out a systematic relationship between labor market conditions and misclassification. In this case, the probability of misclassification writes as:[33]

$$Pr(u_i|LMC_i) = \begin{cases} 1 - \alpha_1 \beta LMC_i & \text{if } u_i = 0 \\ Pr(y_i = 0|y_i^T = 1) \cdot Pr(y_i = 0|LMC_i) = \alpha_1 \beta LMC_i & \text{if } u_i = -1 \end{cases} \tag{A.10}$$

The conditional expectation of the measurement error in this case is:

$$E[u_i|LMC_i] = -\alpha_1 \beta LMC_i \tag{A.11}$$

and assuming LMC_i to be non-stochastic, the bias can be simplified further to:

$$\begin{aligned} E[\hat{\delta}] &= E[\frac{1}{\sum LMC_i^2} \cdot \sum LMC_i \cdot u_i] && \text{(A.12)} \\ &= \frac{1}{\sum LMC_i^2} \cdot E[\sum LMC_i \cdot u_i] && \text{(A.13)} \\ &= \frac{1}{\sum LMC_i^2} \cdot \sum LMC_i \cdot (-\alpha_1 \beta LMC_i) && \text{(A.14)} \\ &= -\frac{1}{\sum LMC_i^2} \cdot \alpha_1 \beta \cdot \sum LMC_i^2 && \text{(A.15)} \\ &= -\alpha_1 \beta && \text{(A.16)} \end{aligned}$$

The expectation of the biased coefficient is thus:

$$E[\hat{\hat{\beta}}] = (1 - \alpha_1)\beta \tag{A.17}$$

When UI Take-Up is problematic for Cyclicality

Let's recall the general definition of bias in equation:

$$E[\hat{\delta}] = -\frac{N}{\sum LMC_i^2} Pr(y_i = 0, y_i^T = 1) E[LMC_i|y_i = 0, y_i^T = 1] \tag{A.18}$$

This would lead to an overestimation, if and only if $E[\hat{\delta}] > 0$. This is the case if $E[LMC_i|y_i = 0, y_i^T = 1] < 0$

B.2 Translating the Measurement Error to the Aggregate (Yearly) Level

Let's introduce the following notation:

[33] By assumption of the linear probability model, $Pr(y_i^T = 1|LMC_i) = \beta LMC_i$. The case for $u_i = 0$ follows immediately from the fact that the probabilities have to sum up to 1.

- The observed take-up rate in year t:
  - $\bar{takeup}_t := \frac{1}{N_t}\sum_{i=1}^{N} takeup_i$
  - where N_t is the number of individuals entering nonemployment in year t
- The number of individuals, for which take-up is measured correctly or with error in year t
  - N_t^T : Are the number of cases in year t where there is error in measured UI take-up (i.e. $takeup_i^T = takeup_i$)
  - N_t^F : Number of cases in year t, where take-up is measured incorrectly.
  - with: $N_t = N_t^T + N_t^F$
  - $\alpha_{1t} = \frac{N_t^F}{N_t}$ the share of false negatives in year t
- The true, but unobserved take-up rate in year t:
  - $\bar{takeup}_t^T := \frac{1}{N_t^T}\sum takeup_i^T$

Using the individual-level measurement error and the notation above we can write:

$$
\begin{aligned}
\bar{takeup}_t &= \frac{1}{N_t}\left(\sum_i takeup_i^T + u_i\right) && \text{(A.19)}\\
&= \frac{1}{N_t}\left(\sum_i takeup_i^T + \sum_{i\,s.t.\,y_i=0\&y_i^T=1}(-1)\right) && \text{(A.20)}\\
&= \frac{1}{N_t}\left(N_t\cdot takeup_t^{\bar{T}} - N_t^F\right) && \text{(A.21)}\\
&= \bar{takeup}_t^T - \frac{N_t^F}{N_t} && \text{(A.22)}\\
&= (1-\alpha_{1t})\bar{takeup}_t^T && \text{(A.23)}
\end{aligned}
$$

Thus, measurement error leads to an underestimation of the true take-up rate. The size of this error increases with the share of false negatives, α_{1t} and with the true take-up rate. We would like to run the following (infeasible) regression:

$$\bar{takeup}_t^T = \beta^A LMC_t + \epsilon_t \tag{A.24}$$

Assuming $E[\epsilon_t|LMC_t] = 0$, this allows for consistent estimates of β^A. However, we can only run the following regression:

$$\bar{takeup}_t = \widetilde{\beta^A} LMC_t + u_t + \tilde{\epsilon}_t \tag{A.25}$$

With $E[\epsilon_t|LMC_t] = 0$, and:

$$u_t = \bar{takeup}_t - \bar{takeup}_t^T = -\alpha_{1t}\bar{takeup}_t^T \tag{A.26}$$

Special Case: Constant Error the 2nd

A constant error implies $\alpha_{1t} = \alpha_1$. Analogous to the individual regression, this results in bias of the following form:

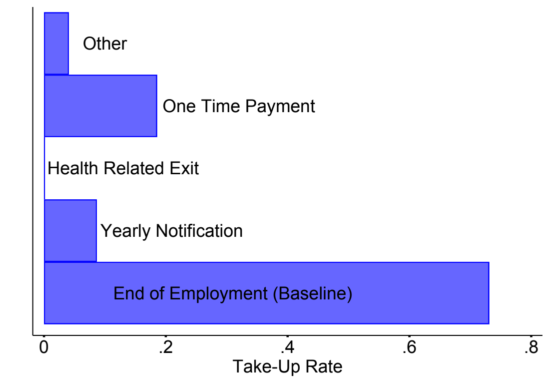
$E[\widehat{\hat{\beta^A}}] = (1 - \alpha_1)\beta^A$

When UI Take-Up is problematic for Cyclicality the 2nd

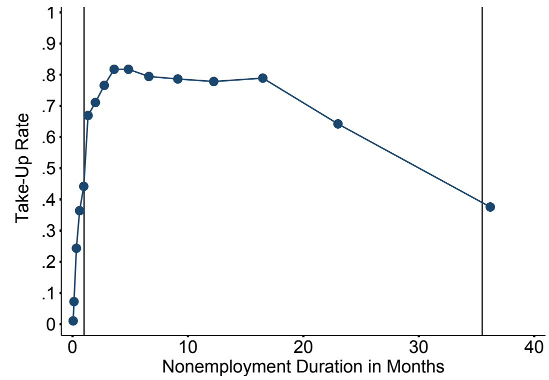
Measurement error would lead to an overestimation of the (negative) cyclicality, iff $Cor(LMC_t, u_t) > 0$

C Appendix Figures and Tables

Figure A.1: Take-Up Rates over Values of Variables used for Sample Restriction

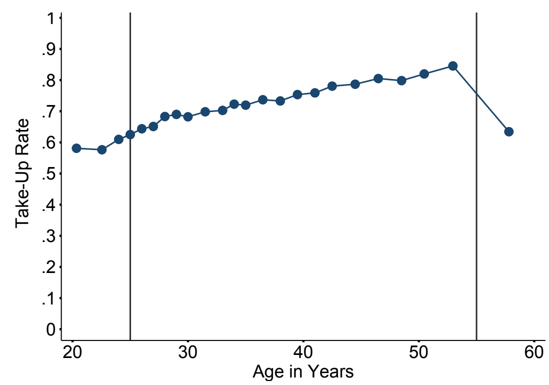


(a) Exit-Notification



(b) Nonemployment Duration

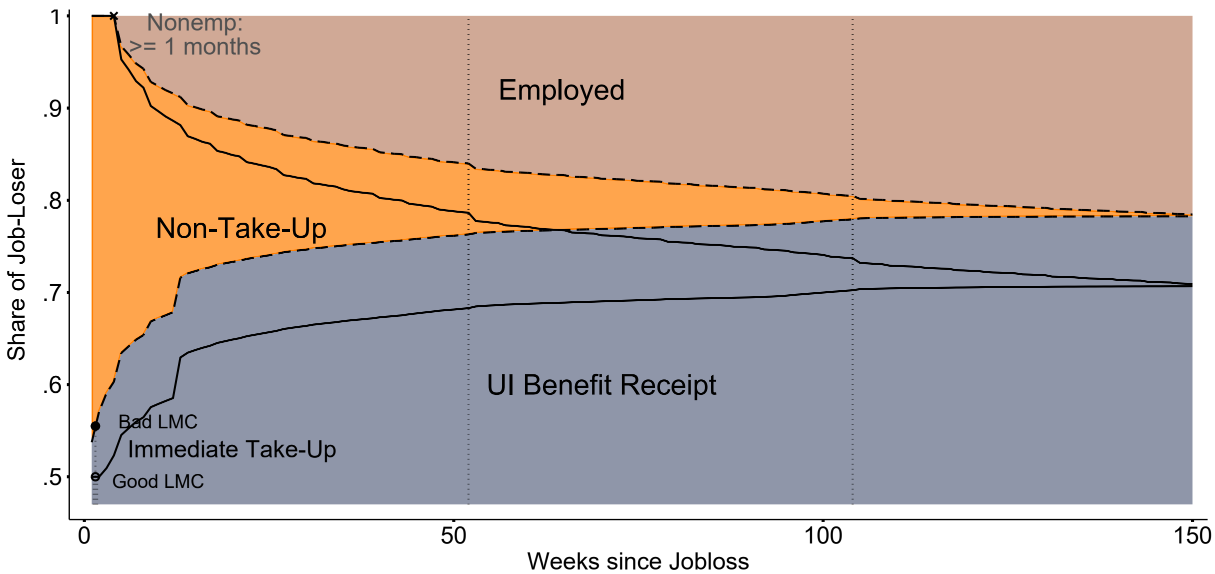
(c) Monthly Gross-Wage



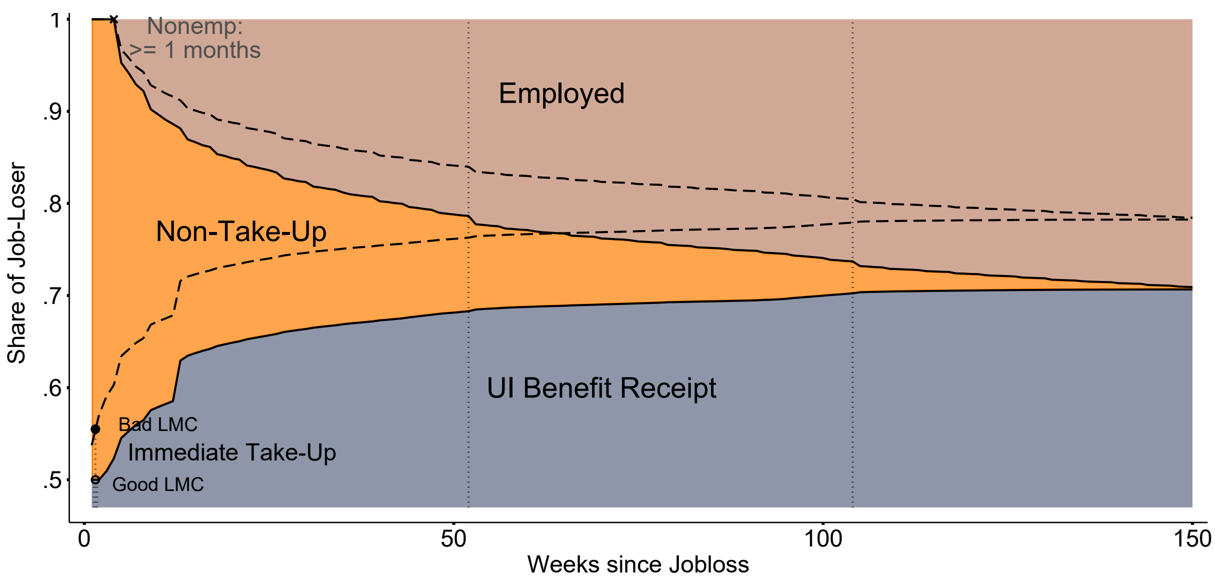
(d) Age

Notes: This figure shows take-up rates by variables that are used for constructing the baseline sample. For each variable, the restriction of this variable in the baseline sample is withdrawn while all other restrictions are maintained. Graphs thus show how (mean) take-up varies for the full spectrum of values of the respective variable. Vertical lines indicate cutoff values used to construct the baseline. Depending on the variable, values above or below the cutoff are excluded in the baseline. The construction is based on a 2% random sample.

Figure A.2: States over first three Years after Job Loss: Good vs. Bad Labor Market Conditions



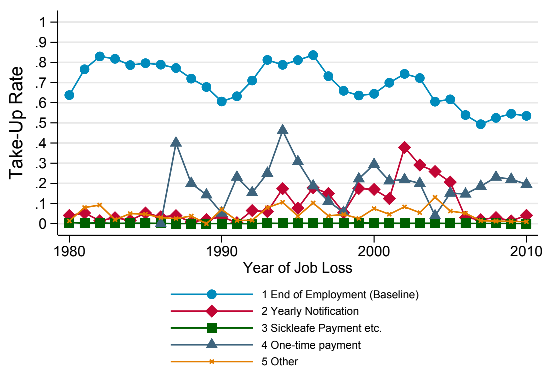
(a) Bad Labor Market Conditions ($\Delta UR \geq 0$)



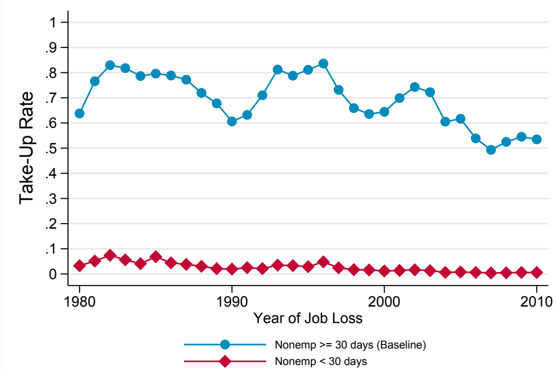
(b) Good Labor Market Conditions ($\Delta UR < 0$)

Notes: This figure shows the share of different states over time since job loss for the baseline sample, by good and bad labor market states, where good labor market states are all observations where the unemployment rate decreases (($\Delta UR < 0$)) and bad labor market states where the unemployment rate increases ($\Delta UR \geq 0$). Dashed vertical lines mark the values for good labor market states and solid vertical lines the values for bad labor market states. Shares are calculated on a weekly basis. Employment and UI receipt are defined as absorbing states and Individuals are required to be in employment within 3 years after job-loss.

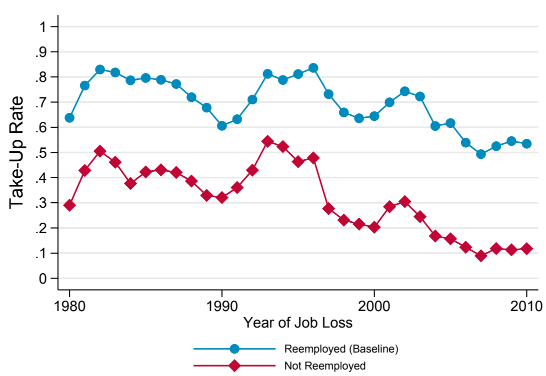
Figure A.3: Take-Up Rates over Time for alternating Sample Restrictions



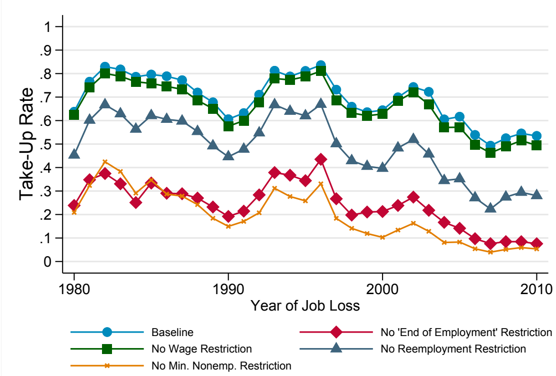
(a) Exit-Notification



(b) Nonemp. Duration by $\leq$ ($>$) 30 days



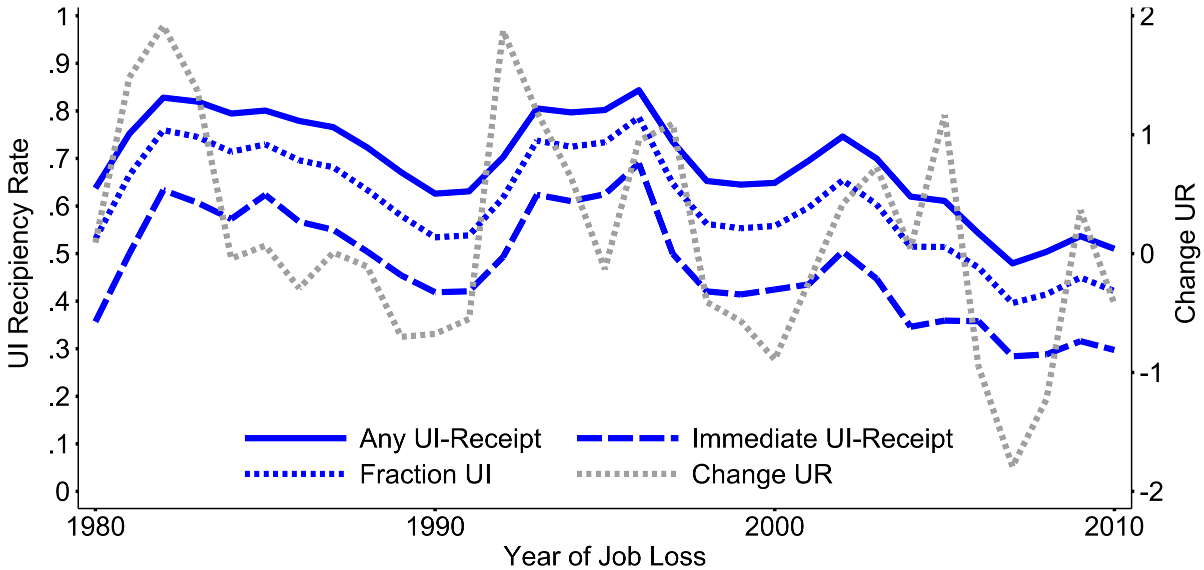
(c) Nonemp. Cens. after 3 years (yes/no)



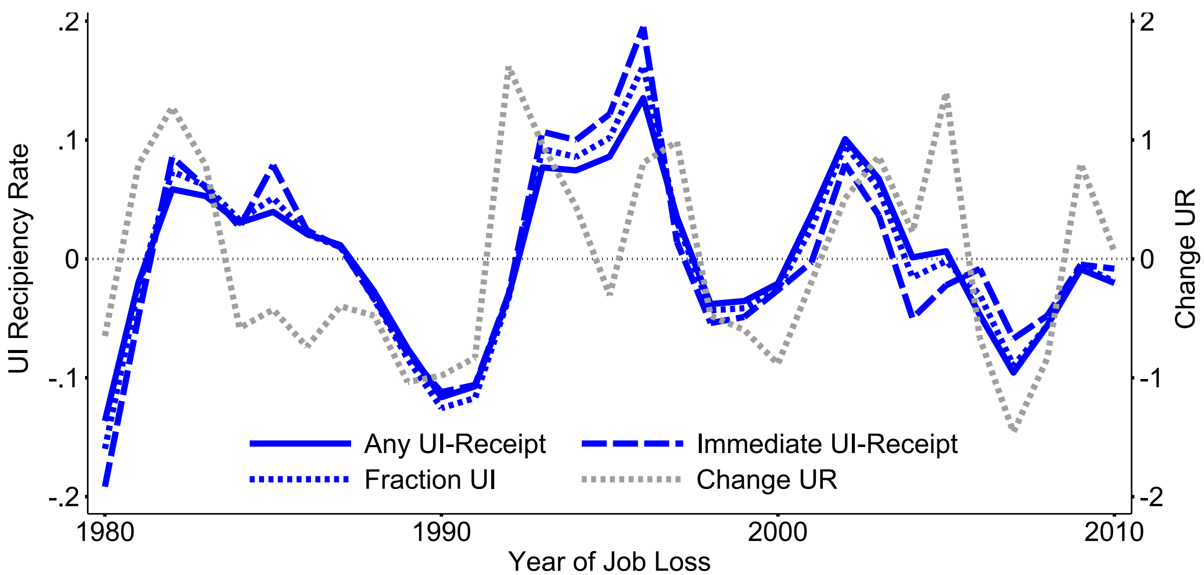
(d) Relaxing Different Restrictions

Notes: This figure shows time series of any UI take-up for alternating sample restrictions. Figure (a) - (c) compare time series for the baseline sample to time series that contains values outside that of the baseline sample for one variable (while holding other restrictions constant). Figure (d) shows time series for samples that relax several restrictions of the baseline sample. The construction is based on a 2% random sample.

Figure A.4: UI Receipt and Labor Market Conditions: Raw vs. HP-Filtered Time Series



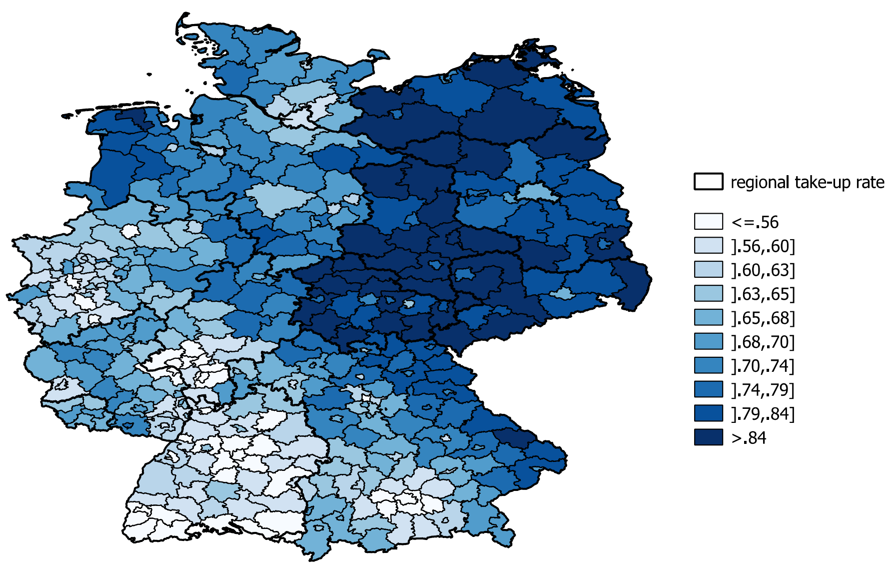
(a) UI Receipt & UR Change, Raw



(b) UI Receipt & UR Change, HP-Filtered

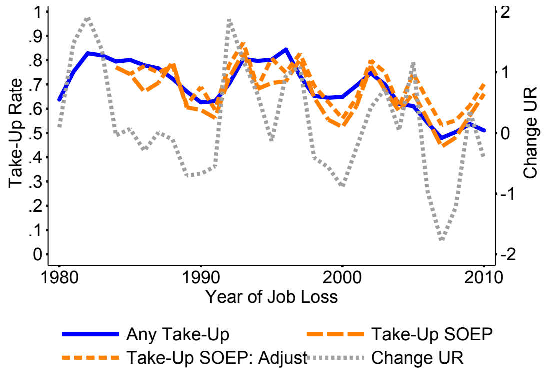
Notes: This figure shows measures for UI receipt and a measure for labor market conditions over time for the baseline sample. Figure (a) shows raw means on the yearly level, figure (b), (d)) show the corresponding hp-filtered time series using smoothing parameter 1600 (the default in Stata).

Figure A.5: Take-Up on the County Level (Place of Residence)

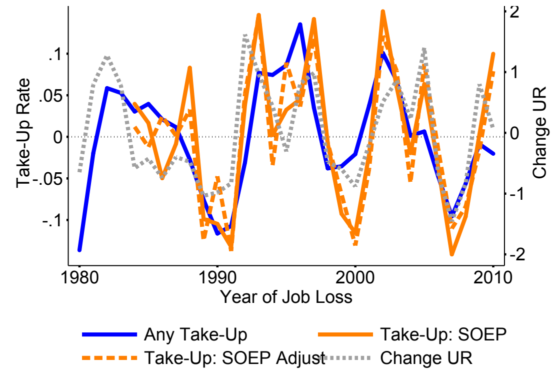


Notes: This figure shows mean take-up rates by place of residence on the county level (Kreis) in Germany for the baseline sample in the admin data. The sample restricts to job losses for the years >=1999, since place of residence is not available earlier. Regional take-up rates are grouped into percentiles. Darker values indicate higher take-up rates.

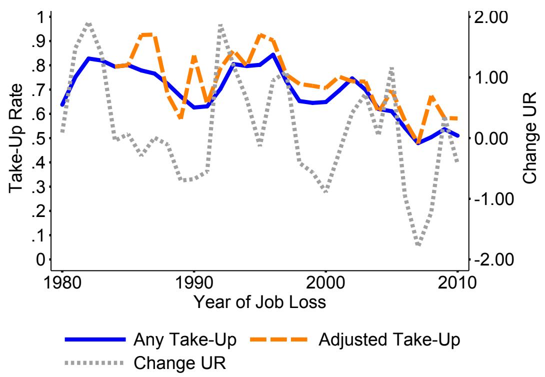
Figure A.6: Adjusting for Unobserved States



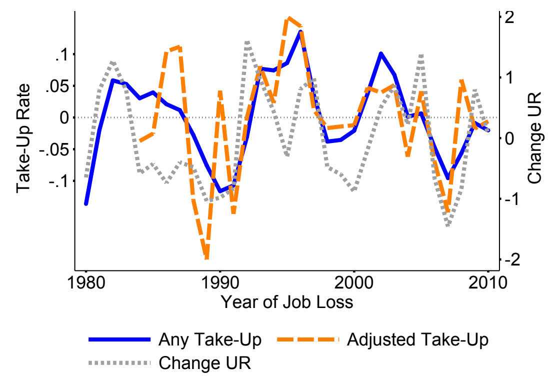
(a) UI Take-Up in Admin Data and SOEP



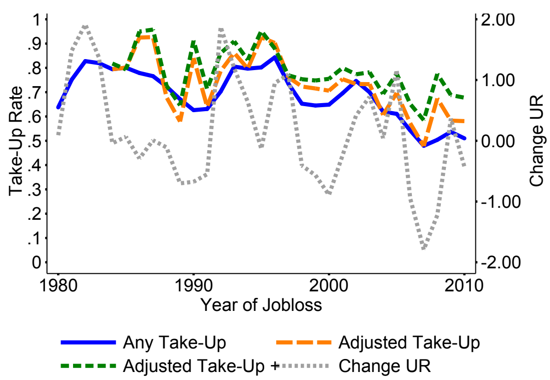
(b) UI Take-Up in Admin Data and SOEP - HP-Filtered



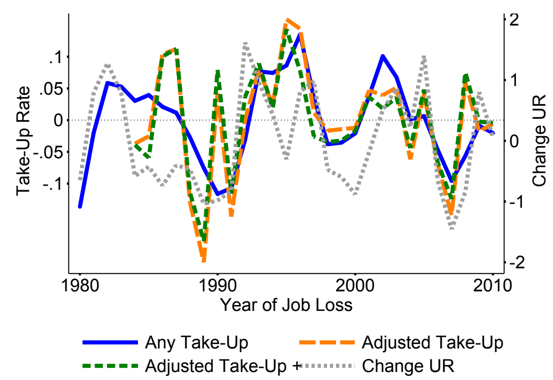
(c) UI Take-Up: Raw vs. Adjusted



(d) UI Take-Up: Raw vs. Adjusted - HP-Filtered



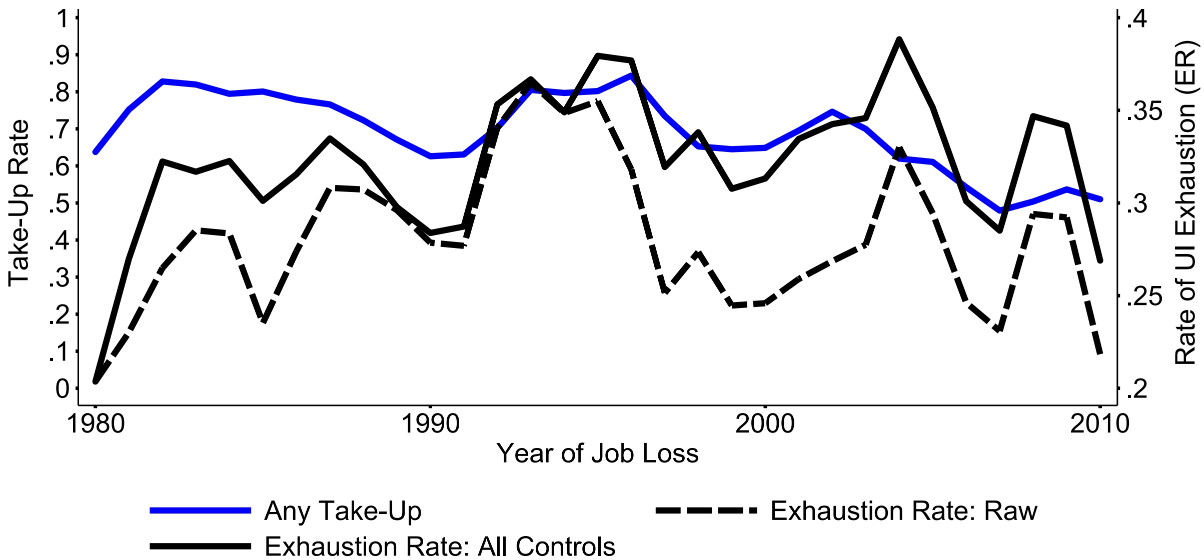
(e) Raw vs. Adjusted plus



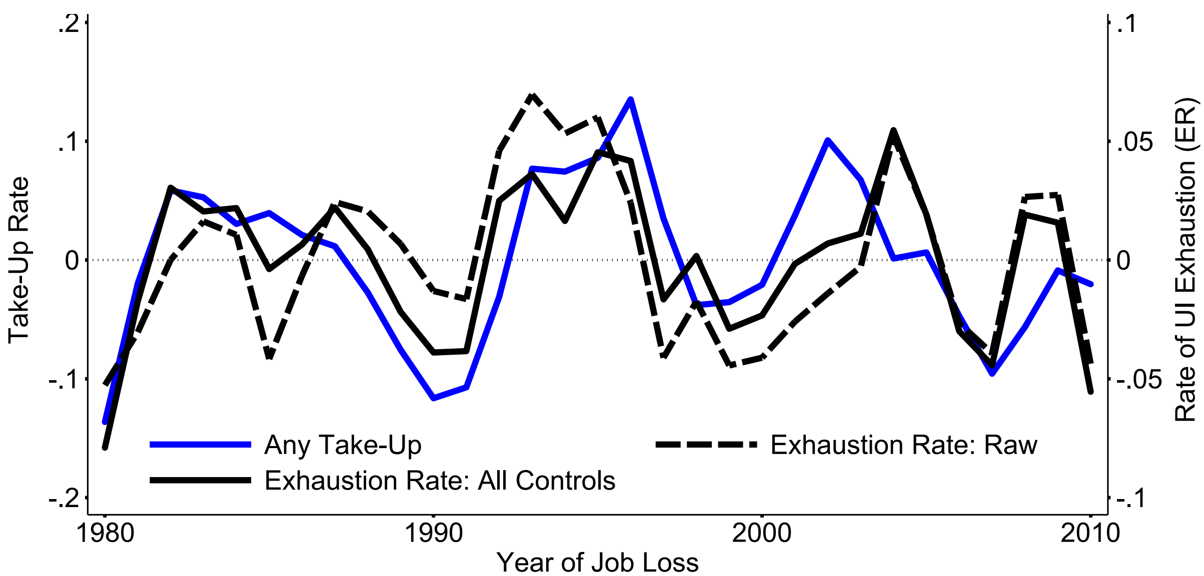
(f) Raw vs. Adjusted plus - HP-Filtered

Notes: This figure shows take-up rates over time (filtered and unfiltered) for any UI take-up in the administrative data and in the SOEP data. Adjusted states exclude cases where individuals are either in self-employment, maternity leave, pensions, civil servants, military occupations or similar states based on information in the SOEP. Figures on the left ((a), (c) and (e)) show raw means on the yearly level, figures on the right ((b), (d) and (f)) show the corresponding hp-filtered time series using smoothing parameter 1600 (the default in Stata). Controls consist of the full set of controls as described in the tablenotes of table 3.

Figure A.7: Take-Up and the Rate of UI Exhaustion



(a) Take-Up and Exhaustion Rate -Raw



(b) Take-Up and Exhaustion Rate -HP-Filtered

Notes: This figure shows cyclicality of take-up, ΔUR and the exhaustion rate. Figure (a) shows raw pattern over time and figure (b) shows the corresponding hp-filtered time series using smoothing parameter 1600 (the default in Stata). Raw exhaustion rates are calculated as the yearly fraction of individuals from the baseline sample that are nonemployed for at least 12 months (the most common duration of UI entitlement). The exhaustion rate with controls is calculated on the same set of individuals holding a full set of controls constant (see tablenotes of table 3).

Table A.1: Different Restrictions and Take-Up Rates: Admin Data

		(1) Full Sample	(2) Restriction: End of Employment	(3) Restriction: Wage	(4) Restriction: nonemp $\geq$ 1 month	(5) Restriction: nonemp $\leq$ 36 month
Full Sample	takeup	0.18	0.28	0.17	0.27	0.16
	N	53,142	31,529	43,998	29,212	41,429
End of Emp	takeup	0.28	0.28	0.26	0.52	0.25
	N	31,529	31,529	25,649	13,739	24,071
Wage	takeup	0.17	0.26	0.17	0.26	0.15
	N	43,998	25,649	43,998	23,041	35,030
nonemp $\geq$ 1 month	takeup	0.27	0.52	0.26	0.27	0.29
	N	29,212	13,739	23,041	29,212	17,499
nonemp $\leq$36 month	takeup	0.16	0.25	0.15	0.29	0.16
	N	41,429	24,071	35,030	17,499	41,429
End of Emp & Wage	takeup	0.26	0.26	0.26	0.54	0.24
	N	25,649	25,649	25,649	10,183	20,183
End of Emp & nonemp $\geq$ 1 month	takeup	0.52	0.52	0.54	0.52	0.71
	N	13,739	13,739	10,183	13,739	6,280
End of Emp & nonemp $\leq$ 36 month	takeup	0.25	0.25	0.24	0.71	0.25
	N	24,071	24,071	20,183	6,280	24,071
End of Emp& Wage & nonemp $\geq$ 1 month	takeup	0.54	0.54	0.54	0.54	**0.73**
	N	10,183	10,183	10,183	10,183	**4,718**

Notes: This table shows rounded numbers of observations in thousand and corresponding take-up rates for different restrictions. 'Takeup' is defined as having any UI receipt in the first year of nonemployment before returning to work. 'Full Sample' refers to all employment exits with full UI eligibility in the age range between 25 and 55 years. 'Wage' refers to monthy gross earnings at last job of at least 1200 Euro per month (in 2010 values). 'End of Emp' refers to the restriction that the reason for employment exit is recorded as end of employment. Values for the resulting final sample in bolt.

Table A.2: Implied Take-Up Measures for Different Scenarios

	Immediate Take-Up		*Delayed Take-Up*		*No Take-Up*	
	short nonemp.	long nonemp.	short nonemp.	long nonemp.	short nonemp.	long nonemp.
	(1)	(2)	(3)	(4)	(5)	(6)
Example Parameters for Nonemployment and UI Duration						
$nonempdur$	100	200	100	200	100	200
$uidur$	0	0	90	90	>100	>200
Implied Take-Up Values						
$takeup^A$	1	1	1	1	0	0
$takeup^I$	1	1	0	0	0	0
$fracUI$	1	1	0.10	0.55	0	0

Notes: This table illustrates the implied values for different take-up measures using different scenarios of nonemployment duration and time till UI take-up. $nonempdur$ denotes the number of days between layoff and start of employment, whereas $uidur$ denotes the number of days between layoff and start of UI. $takeup^A$ is the dummy-variable for any UI take-up, $takeup^I$ the dummy-variable for immediate take-up (within 10 days after job loss). $fracUI$ is a continuous variable bounded between 0 and 1 that reports the fraction of nonemployment duration that is covered by UI receipt within the first year after job loss.

Table A.3: Take-Up and the Business Cycle: Year-Level Regression

	Immediate Take-Up $takeup^I$		Fraction of Insured Nonemp. $fracUI$		Any Take-Up $takeup^A$	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline Sample						
UR_t	0.0342**	0.0380**	0.0348***	0.0375***	0.0319***	0.0350***
	[0.0106]	[0.0110]	[0.0087]	[0.0085]	[0.0074]	[0.0071]
R^2	0.308	0.657	0.370	0.716	0.378	0.738
g_t	-1.9046*	-1.6611+	-2.0953*	-1.9149+	-1.9754*	-1.8170+
	[0.7978]	[0.9715]	[0.8258]	[1.0213]	[0.7829]	[0.9882]
R^2	0.246	0.496	0.345	0.581	0.373	0.609
Panel B: No Right-Censoring of Nonemp. Duration						
UR_t	0.0247*	0.0297**	0.0267**	0.0324**	0.0262**	0.0330**
	[0.0102]	[0.0107]	[0.0097]	[0.0097]	[0.0094]	[0.0093]
R^2	0.193	0.661	0.210	0.712	0.204	0.719
g_t	-1.5909*	-1.3468+	-1.8770*	-1.6531+	-1.9205*	-1.6933+
	[0.6569]	[0.7856]	[0.7434]	[0.9226]	[0.7574]	[0.9619]
R^2	0.206	0.569	0.268	0.636	0.281	0.645
N obs.	31	31	31	31	31	31
Mean Depvar Panel A	0.455	0.455	0.592	0.592	0.686	0.686
Mean Depvar Panel B	0.320	0.320	0.443	0.443	0.487	0.487
Trend-Control: HP-Filter	x		x		x	
Trend-Control: Linear		x		x		x

Notes: This table shows year-level regressions of the association between labor market conditions and different take-up measures. Panel A shows results for the baseline sample (collapsed to the yearly level) and Panel B for a sample that removes the right-censoring restriction of the nonemployment duration that is used in the baseline sample. Robust and bias-corrected (HC3) standard errors are in brackets. +, *, ** and *** denote significant levels on the 10 %, 5%, 1% and 0.1% significance level respectively.

Table A.4: Take-Up and the Business Cycle: Controls

	Baseline Sample			Individual FE Sample		Establishment FE Sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
UR_t	0.0410***	0.0374***	0.0294***	0.0336***	0.0266***	0.0427***	0.0331***
	[0.0074]	[0.0074]	[0.0067]	[0.0065]	[0.0039]	[0.0086]	[0.0068]
R^2	0.026	0.114	0.148	0.013	0.618	0.026	0.325
Mean Indep. Var	8.585	8.585	8.585	8.547	8.547	8.554	8.554
Oster's δ		17.466	4.623		10.591		8.387
g_t	-2.2549**	-1.7399*	-1.5411**	-2.1625***	-1.5969***	-2.3570***	-1.6462***
	[0.7634]	[0.7088]	[0.5307]	[0.4995]	[0.3293]	[0.6476]	[0.4817]
R^2	0.019	0.106	0.145	0.012	0.614	0.019	0.296
Mean Indep. Var	0.039	0.039	0.039	0.040	0.040	0.040	0.040
Oster's δ		7.524	5.477		9.048		6.994
N obs.	4718394	4718394	4718394	812266	812266	4080377	4080347
Trend Controls	x	x	x	x	x	x	x
Individual Controls		x	x		x		x
Firm & Regional Controls			x		x		x
Firm Fixed Effects							x
Individual Fixed Effects					x		

Notes: This table shows individual level regressions of the association between labor market conditions and any UI take-up for different sets of controls. Standard errors are bootstrapped with clusters on the yearly level and 100 replications. The independent variables are the yearly change in the national unemployment rate in panel A, the yearly unemployment rate in panel B and the yearly growth rate of GDP in panel C. Osters' delta is calculated relative to column (2) and assuming a maximum R^2 of 1.3 times the actual R^2. Individual controls are dummies for gender, age in years, education, 2-digit occupation groups, non-German nationality, past nonemployment experience and variables for last wage and last wage-squared in Euro as well as experience and experience squared. Regional controls are dummies on the county (Kreis) level. Firm-level controls consist of 5-digit industry dummies, 20 firm-size dummies, the layoff size (relative to firm size) and a dummy for plant closure. +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1 % significance level respectively.

Table A.5: Take-Up and the Business Cycle: Parametric Controls

	(1)	(2)	(3)	(4)	(5)
ΔUR_t	0.0613***	0.0494***	0.0529**	0.0427***	0.0455**
	[0.0165]	[0.0133]	[0.0163]	[0.0121]	[0.0164]
Female == 1			-0.0367***		-0.0294***
			[0.0005]		[0.0005]
Non-German Nationality == 1			-0.0482***		-0.0388***
			[0.0007]		[0.0007]
Age in Years			0.0078***		0.0068***
			[0.0000]		[0.0000]
Highly Educated == 1			0.0166***		0.0272***
			[0.0005]		[0.0005]
Monthly Gross-Wage x 1,000			-0.1148***		-0.1037***
			[0.0005]		[0.0005]
Actual Exper. in Years			0.0046***		0.0036***
			[0.0001]		[0.0001]
Actual Exper. in Years^2			-0.0002***		-0.0002***
			[0.0000]		[0.0000]
Past Nonemp. Exper. == 1			0.0449***		0.0426***
			[0.0007]		[0.0007]
ln(No. of Employees at Firm)				-0.0121***	-0.0047***
				[0.0001]	[0.0001]
Relative Layoff-Size				0.1647***	0.1478***
				[0.0008]	[0.0007]
Dummy: Large Plantclosure == 1				0.0828***	0.0791***
				[0.0012]	[0.0012]
ln(Regional Population-Density)				-0.0462***	-0.0343***
				[0.0003]	[0.0003]
Year of Layoff (Time Trend)		-0.0040***	-0.0003***	-0.0015***	0.0023***
		[0.0000]	[0.0000]	[0.0000]	[0.0000]
Constant	0.7086***	8.7626***	1.2263***	3.8559***	-3.7117***
	[0.0002]	[0.0581]	[0.0702]	[0.0777]	[0.0903]
N obs.	4718394	4718394	4718394	4718394	4718394
R^2	0.014	0.019	0.071	0.051	0.090
Mean Dep. Variable	0.730	0.730	0.730	0.730	0.730
Mean Indep. Variable	0.345	0.345	0.345	0.345	0.345
Oster's δ			-31.966	8.245	14.673
Linear Time Trend		x	x	x	x
Individual Controls			x		x
Firm & Regional Controls				x	x

Notes: This table shows individual-level regressions of the association between ΔUR_t -the yearly change in the national unemployment rate between year of layoff t and the previous year $t-1$- and any UI take-up for different sets of controls. Standard errors for ΔUR_t are bootstrapped with clusters on the yearly level and 100 replications. For the other variables, robust standard errors are reported. Missing values in the control variables are dummied out (dummies not reported). +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1% significance level respectively.

Table A.6: Take-Up and Local Labor Market Conditions

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Local Labor Market Conditions at County Level						
$UR_{t,r}$	0.0217***	0.0217***	0.0124***	0.0157***	0.0125***	0.0090***
	[0.0019]	[0.0019]	[0.0007]	[0.0007]	[0.0017]	[0.0013]
R^2	0.038	0.050	0.172	0.144	0.079	0.194
Oster's δ			2.114	4.116	1.908	1.324
Panel B: Local Labor Market Conditions at Municipality Level						
$UR_{t,r}$	0.0202***	0.0201***	0.0113***	0.0147***	0.0101***	0.0076***
	[0.0014]	[0.0015]	[0.0005]	[0.0005]	[0.0012]	[0.0009]
R^2	0.038	0.049	0.172	0.144	0.092	0.202
Oster's δ			2.129	4.313	1.793	1.210
N obs.	1107735	1107735	1107735	1107735	1107735	1107735
N counties	402	402	402	402	402	402
N municipalities	4475	4475	4475	4475	4475	4475
Year-FE		x	x	x	x	x
Individual Controls			x			x
Firm Controls				x		x
County/Municipality -FE					x	x

Notes: This table shows individual level regressions of the association between regional labor market conditions and any UI take-up. The regional level r is defined on the county (i.e. Kreis) level for panel A and on the municipality (i.e. Gemeindeverband) level for panel B, and t refers to the yearly level. Standard errors are clustered on the county (Kreis) level in panel A and on the municipality (Gemeindeverband) in panel B. Individual controls are dummies for gender, age in years, education, 2-digit occupation and variables for last wage and last wage-squared in Euro. Regional Controls are county fixed effects. Firm-level controls consist of 5-digit industry controls and 20 firm-size dummies. +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1 % significance level respectively.

Table A.7: Take-Up and the Business Cycle: Controls and Adjustments

	Raw	Control	Long Nonemp.	Plant Closure	Adjusted States	Combined (1)-(3), (5)	All Combined (1)-(5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full Period (1980-2010)							
UR_t	0.035***	0.026***	0.033***	0.026*			
	[0.007]	[0.007]	[0.007]	[0.010]			
R^2	0.738	0.656	0.725	0.732			
Mean Dep. Var	0.690	0.713	0.716	0.842			
Panel B: SOEP Period (1985-2010)							
UR_t	0.032***	0.024**	0.029**	0.023+	0.025+	0.011	0.006
	[0.008]	[0.007]	[0.008]	[0.013]	[0.013]	[0.012]	[0.010]
R^2	0.703	0.605	0.690	0.726	0.467	0.334	0.371
Mean Dep. Var	0.676	0.705	0.700	0.824	0.730	0.783	0.921
Trend Control (linear)	x	x	x	x	x	x	x

Notes: This table shows regressions on the yearly level of the association between any UI take-up and labor market conditions for controls and different adjustments to measurement error. Column (1) shows raw take-up and column (2) take-up holding observed characteristics constant. Column (3) restricts to nonemployment durations of at least 4 months and column (4) restricts to large plant closures in the admin data, both of which are intended to address sanctions or related temporary ineligibility periods. Column (6) adjusts for unobserved states using information from the SOEP, column (7) and (8) provide combinations of these adjustments. ΔUR is the percentage change in the national unemployment rate,g the GDP growth rate and UR the yearly unemployment rate. Full period refers to the baseline period between 1980 and 2010 and the SOEP period between 1985 and 2010, the period for which SOEP as well as administrative information is available. Robust, bias-corrected standard errors (HC3) are reported in brackets. +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1 % significance level respectively.

Table A.8: Potential Sources of Measurement Error and whether they are Observed in the Data

	eligibility related			*Potentially Confounding States*			
not sufficiently contributed	sanction at nonemp. entry	waiting period at nonemp. entry	health/disability related exit	military / alternative service	civil servants	self-employment	migration
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information contained in Admin Data (IEB)							
observed	partly/indirectly	partly/indirectly	partly/indirectly	not observed	partly/indirectly	partly/indirectly	partly/indirectly
Contrib. duration for permanent workers observed with high accuracy. Correct reporting required by law & misreporting punishable.	Not observed for all nonemployed, but only for those who take up UI. Before 2005, only as delay in data& since than as zero benefits. Max. duration: 12 weeks.	No information in data. Can occur if individuals receive severance payments because ind. agree to early quit.	Not directly observed, but can partly be inferred from exit notification.	Not directly observed, but restricting to age ≥ 25 excludes most likely population.	Not directly observed, but permanent switches can be excluded.	Not directly observed, but permanent switches can be excluded.	Not directly observed, but permanent switches can be excluded. No information on temporary migration.
Information contained in Survey Data (SOEP)							
partly/indirectly	partly/indirectly	partly/indirectly	observed	observed	observed	observed	partly/indirectly
working history is observed, but not always clear what counts as contribution period and what does not.	Contains info on whether separation was initiated by employer, in which case sanctions are unlikely.	Contains info on if workers received severance payments and the size of payments.	Contains info on disability, parental leave & sick benefits.	Contains info on military and alternative service.	Contains info on whether current employment situation is as civil servant.	Contains info on whether current employment situation is as civil servant.	
Implied Error Type (if not observed)							
false positive	false positive	false positive	false positive	false positive	false positive	false positive	false positive

Notes: This table provides an overview over different causes of measurement error in take-up variables and whether these causes can be addressed with the administrative data or the survey data that has been used in the analysis.

Table A.9: UI Take-Up, Labor Market Conditions and the UI Exhaustion Rate

	(1)	(2)	(3)	(4)	(5)
Panel A: No Controls					
ΔUR_t	0.0494**	0.0360*	0.0167	0.0161	0.0155
	[0.0167]	[0.0151]	[0.0131]	[0.0153]	[0.0109]
Exhaustion Rate: Raw		0.8033**			
		[0.3028]			
Exhaustion Rate: Controls			1.6744***		
			[0.2444]		
Exhaustion Rate: Estab.-FE				1.7462***	
				[0.3746]	
Exhaustion Rate: Ind.-FE					1.6936***
					[0.2643]
N obs.	4718394	4718394	4718394	4718394	4718394
R^2	0.019	0.024	0.031	0.029	0.032
Panel B: Full Controls					
ΔUR_t	0.0393***	0.0272*	0.0137	0.0132	0.0142
	[0.0108]	[0.0121]	[0.0073]	[0.0109]	[0.0076]
Exhaustion Rate: Raw		0.7394***			
		[0.1908]			
Exhaustion Rate: Controls			1.3256***		
			[0.1698]		
Exhaustion Rate: Estab.-FE				1.3716***	
				[0.2342]	
Exhaustion Rate: Ind.-FE					1.2638***
					[0.1992]
N obs.	4718394	4718394	4718394	4718394	4718394
R^2	0.146	0.150	0.153	0.152	0.153
Raw Exhaustion Rate		x			
Exhaustion Rate -Controls			x		
Exhaustion Rate Estab-FE				x	
Exhaustion Rate Individual-FE					x

Notes: This table shows individual level regressions of the association between ΔUR_t and any UI take-up, including different specifications of the UI exhaustion rate as additional variable. Panel A controls only for a linear time trend, whereas Panel B includes, in addition, the full set of controls as in table 3 column 4. The raw exhaustion rate (column 2) is the mean exhaustion rate of the baseline sample per year. Column (3) contains the yearly exhaustion rate that is constructed holding the full set of observed characteristics constant. Column (4) and column(5) add an exhaustion rate that controls establishment and individual fixed effects, respectively. Exhaustion rates with controls (column (3)-(5)) are obtained from an individual level regression of UI exhaustion on yearly dummies and additional controls. Standard errors are bootstrapped with clusters on the yearly level and 100 replications. +, *, ** and *** denote significant levels on the 10%, 5%, 1% and 0.1 % significance level respectively.