How Does Potential Unemployment Insurance Benefit Duration Affect Re-employment Timing, Sorting and Wages?*

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Abstract

How potential benefit duration (PBD) of unemployment insurance (UI) affects wages and matching is crucial to evaluate PBD extensions as a policy tool and to understand the causal relation between unemployment, job search and wages. Discontinuities in PBD at age cut offs have allowed recent research to identify the effect of PBD extensions, but even questions as elementary as whether PBD increases or reduces re-employment wages are highly debated and the causal mechanisms through which UI and PBD affect labor market outcomes are unclear. The overall effect of PBD on wages confounds effects that operate through longer unemployment with effects of changes in job search. PBD affects the timing of re-employment, so that only aggregate effects of longer unemployment are identified and the effects of job search are difficult to separate from changes in the timing of re-employment. This dynamic selection crucially affects estimated effects of PBD and their interpretation, but is not well understood. To further clarify the nature of PBD effects and to separate unemployment duration, search and selection effects, we decompose wages into individual, firm and match specific wage components. We use the discontinuity in PBD from 12 to 18 month from Schmieder, von Wachter and Bender [2016] to identify effects of PBD on these components and their dynamics over the unemployment spell. Adding to evidence on the importance of firms, we find that the entire wage loss operates through the firm fixed effect. We then decompose the re-employment wage path into the same components to study re-employment dynamics. The decomposition allows us to better distinguish dynamic selection and sorting from wage effects. Even though PBD does not affect the average individual and match fixed effect, it does affect how these re-employment wage components vary with unemployment duration. Changes in the firm fixed effect are partly offset by changes in the dynamics of the other components, which may reconcile the diverging findings regarding effects on search and the direction of wage effects. Our preliminary results indicate that PBD effects are likely a combination of unemployment duration, search effects and dynamic selection. The results imply two effects in addition to the previously documented unemployment duration effect. High-wage individuals with prior jobs at high-wage firms that exit to low-wage firms tend to leave unemployment around benefit exhaustion. This sorting implies that the estimated effects of PBD depend on who

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enters unemployment and sample restrictions. Rather than being driven by this postponement effect, the effect of PBD on wages appears to be mainly due to a downward shift in the firm-specific re-employment wage component at shorter unemployment durations. Overall, our preliminary results show that dynamic selection plays an important role in the effects of PBD as well as in the steep decline of re-employment wages with unemployment duration. We will use the detailed firm- and individual-specific information in our data to shed more light on the nature and underlying causes of both sorting and wage loss in the final paper.

JEL classification: J31, J64, J65
Keywords: unemployment insurance, benefit duration, dynamic selection, job search

1 Introduction

The opponents of longer potential benefit duration (PBD) stress its effect of prolonging unemployment, with all its negative consequences. The advocates, in contrast, focus on UI's supporting function in providing a social safety net as well as improving job search and thereby match quality and the allocation of labor. Understanding the effects of UI is crucial to assess its value as a policy tool. The effect of UI and its potential benefit duration are also of interest to academics, because they can shed light on important labor market questions, such as the effects of unemployment duration and job search.

The existing empirical literature provides solid evidence that an increase in PBD unambiguously leads to longer unemployment durations [Card, Chetty and Weber 2007; Van Ours and Vodopivec 2008; Tatsiramos 2009; Caliendo, Tatsiramos and Uhlendorff 2013]. However, it is less clear how UI and PBD affect re-employment wages. Several papers document that re-employment wages drop with unemployment duration [e.g. Lalive 2007; Schmieder, von Wachter and Bender 2016], but whether this effect is causal is less clear. In addition, there may be offsetting positive effects of PBD on wages beyond reducing wages through longer unemployment duration. If UI improves job search and matching, workers with longer PBD should achieve higher post-unemployment wages conditional on the actual unemployment duration.

Two recent studies using discontinuities in PBD by age to identify the effect of PBD arrive at quite opposite conclusions. Schmieder, von Wachter and Bender [2016] investigate the effect of extending PBD from 12 to 18 months in Germany. They show that in the absence of changes of the re-employment wage path due to PBD extensions, the effect of PBD only operates through unemployment duration rather than reservation wage effects. If so, PBD extensions identify the effect of unemployment duration on wages. They test for changes of re-employment wages conditional on unemployment duration and conclude that there are no reservation wage effects. They document that there are no relevant search effects, so that PBD and thus unemployment duration decreases re-employment wages. In contrast, Nekoei and Weber [2017] find an increase in re-employment wages due to extending PBD from 30 to 39 weeks in Austria. They show that workers with longer PBD also have higher re-employment wages conditional on unemployment duration, particularly in the weeks approaching the exhaustion point. Thereby, they provide evidence that there are effects of PBD extensions on wages beyond unemploy-

ment duration that can improve re-employment wages. They argue that these effects arise from changes in search behavior and conclude that job search during unemployment improves match quality. However, while they provide evidence that the effects are not entirely due to sorting, they do not attempt to disentangle changes in search from changes in sorting. These contradictory findings show that to understand whether UI has positive effects in addition to the negative effects of prolonged unemployment and thus whether it identifies the effect of unemployment on wages, the channels through which UI affects post-unemployment wages are decisive.

We employ the same institutional setting and identification strategy as Schmieder, von Wachter and Bende [2016]. We use linked-employer-employee-data (LEED) to decompose workers' wages into an individual, a firm and a match fixed effect. We then analyze these estimated wage components to learn more about the channels through which UI affects wages. The individual fixed effect is informative about changes in dynamic selection, i.e. at which duration what type of individual exit to employment. The firm fixed effects – and the change in the firm fixed effect between pre- and post-unemployment jobs – is informative on how workers sort into firms and climb the job ladder. Finally, we interpret the match fixed effect as a direct measure of match quality, implying that a comparison of the match effects between workers with different PBD shows to which extent better matches are formed, potentially due to synergies between workers' skills and firms' production technologies. We first decompose the overall effect of PBD on re-employment wages into effects on these wage components. We then analyze how these wage components evolve with unemployment duration and how these dynamic processes are affected by changes in PBD. Our data contain information on all workers and detailed information on workers and firms that allow us to examine the nature and potential causes of these dynamics more closely.

Our preliminary results clearly emphasize that it is crucial to understand sorting over the unemployment spell and how unemployment insurance affects it. Dynamic selection biases estimates of the effects of unemployment duration on wages even in the absence of search effects. Instrumental variable strategies such as the one employed by Schmieder, von Wachter and Bender [2016] are questionable in the presence of sorting, but a better understanding of dynamic selection could clarify strategies that may work. Our results imply that individuals moving

from high- to low-wage firms play an important role in the dynamics over the unemployment spell. This implies that the results of studies of the effects of PBD and unemployment depend on the sample of workers entering unemployment, specifically on the firm fixed effect. This is concerning for the many studies of the effect of unemployment that rely on mass-layoffs, because their results will depend on the firm-fixed effects of the involved firm. Neither Schmieder, von Wachter and Bender [2016] nor Nekoei and Weber [2017] attempt to disentangle search and sorting, but understanding dynamic selection is also important to study job search. A better understanding of sorting can allow us to disentangle the effects of sorting from the effects of job search. In addition, the nature of sorting itself may provide important insights into how individuals search for jobs and transition between them. We will use our detailed data that includes information on those who exit the labor force as well as employment biographies of all workers at all firms and detailed firm information to examine these issues further.

The next Section reviews the institutional background and discusses our data. Section 3 introduces our empirical strategy. Section 4 analyzes the validity of our RDD strategy and shows that our approach and data reproduce prior results. Section 5 decomposes the effect of PBD on re-employment wages into the effects on wage components and analyzes how PBD affects the re-employment path of these wage components. Section 6 analyzes how the dynamics of these wage components change with PBD duration in order to analyze the role of search and sorting. The final Section concludes.

2 Institution and Data

2.1 Institutional Framework

This study is based on the unemployment insurance regime that was in place in Germany between July 1987 and March 1997 with an additional two year phasing-out period and therefore corresponds the longest period without substantial changes to UI in Germany to date.[1] PBD ranges from six to 32 months and is determined by a claimant's age and labor market experience within the last seven years before unemployment.[2] Individuals who have worked at

[1] Bundesgesetzblatt, Volume 1987, Part I, p. 1542.
[2] For a detailed overview of the regime see Schmieder, von Wachter and Bender [2012*a*].

least six months during the qualifying period are eligible for UI. Until 1993 the monthly disbursement for claimants with children was 68 percent of the previous gross salary and 63 for claimants without children. Later the benefits were lowered to 67 and 60, respectively.[3] Unemployed workers who voluntarily quit their job or who apply late for unemployment benefits face cut-off weeks. The phasing-out period between March 1997 and 1999 concerned only individuals requesting unemployment benefits during this period and being employed more than one year within the last three years before March 1997. The lowest PBD is six months and it rises in two months steps reaching the maximum at 32 months. The reference groups span over age windows of two to five years and labor market experience brackets of two to four months.

Our investigation focuses on middle aged workers, since young and old employees are more idiosyncratic concerning job-to-job transitions and reemployment chances. For older individuals reemployment is entangled with retirement decisions and the group finding a new job after unemployment at that age is positively selected. In contrast, for younger individuals moving from a job to another job via unemployment is prevalent and periods spent in unemployment can be an intentional choice. Therefore, we analyze claimants at the threshold age of 42 years. We employ a minimum of three years of working experience within the last seven years before unemployment which results in a sample of labor market attached claimants. Thus, our main identification strategy follows Schmieder, von Wachter and Bender [2016] and is based on using variation in PBD from 12 to 18 months.

2.2 Data

We analyze the impact of PBD extensions on re-unemployment wages by decomposing wages into person, firm and match components. Decomposing wages requires linked employer-employee data with long employment biographies and realized mobility between firms (i.e. connectedness). In particular, the estimation of a constant component between employer and employee, the match effect, demands that at minimum two employees work at the same firm and that those individuals are employed at least at one other firm in the sample. To shed light on the underlying mechanisms, we also need detailed information on both individuals and firms. The LIAB Mover Model meets these criteria. The administrative linked employer-

[3] Bundesgesetzblatt, Volume 1993, No. 72, p. 2357.

employee data is provided by the Research Data Center of the German Federal Employment Agency. It covers more than 4.5 million individuals (with about 700 000 movers) employed at around 2 million firms. The LIAB Mover Model spans from 1993 to 2008 allowing us to investigate the UI system for individuals which were unemployed between 1993 and 1999. Moreover, we can track these individuals at least 9 nine years after the beginning of their unemployment spell.

The LIAB Mover Model contains day-to-day information on employees covered by social security as well as unemployment benefit recipients. Demographic characteristics, such as age, educational background and gross daily wages are comprised. On the firm side the data includes total employment, industry affiliation and salaries. For about 25 000 firms participating in a survey additional characteristics are available, such as detailed annual reports about the employee structure, workers hired, vacancies and revenues.

Unemployment is defined in the data as receiving unemployment insurance benefits. Therefore, unemployment duration is measured as the period an individual claiming benefits. Moreover, we consider the length between two employment spell as nonemployment duration. Our empirical focus is on individuals who experience a period of unemployment between two full-time employment spells. Other forms of employment are not taken into account since the data does not include hourly wages, instead it provides daily wages.

To identify individuals eligible for PBD we have to derive the age at the beginning of the unemployment spell and the labour market experience within the qualifying period of seven years. We can infer the age of the claimant from the start of unemployment which is reported at daily precision and the claimant's date of birth date which is provided in months. Concerning the labour market experience, we either calculate the number of days in employment directly for workers who have not changed jobs within the qualifying period or we construct a lower bound measure of days in employment for the other claimants by assuming every day of unemployment recorded before 1993 as taking place during the qualifying period. Because our empirical research design relies on individuals with the maximal required days in employment in their age bracket, we can be sure to identify the actual PBD treatment correctly in the data.

3 Empirical Strategy

3.1 Identifying Effects of Potential UI Benefit Duration

Maximum UI benefit duration depends on the length of prior labor force participation, which makes it related to later labor market outcomes. To identify the effects of potential UI benefit duration in the presence of this endogeneity problem, we use variation in maximum UI benefit duration at the age thresholds discussed in Section 2. Following Schmieder, von Wachter and Bender [2016], we argue that close to these thresholds, those eligible for longer UI benefits only differ from those with shorter UI benefits in their age. Thereby, this discontinuity locally identifies the effects of potential benefit duration, which we first document graphically. We formally study the effects using the following standard regression discontinuity model:

$$y_i = \alpha + \gamma D_{a_i \geq a^*} + \beta_0 (1 - D_{a_i \geq a^*}) a_i + \beta_1 D_{a_i \geq a^*} a_i + \varepsilon_i \tag{1}$$

where y_i is an outcome, such as wages or wage components as discussed below. a_i is the age of individual i and $D_{a_i \geq a^*}$ is an indicator for the individual being older than the relevant age threshold a^* at the time of separation. In our main specification, $D_{a_i \geq a^*}$ indicates that i was 42 or older at the time of separation, and thus eligible for 18 instead of 12 months of UI benefits. We estimate equation (1) locally around these cutoff(s) a^*, specifying how outcomes change with age as a linear function with different slopes on each side of the cutoff. We argue that γ is the effect of extending potential UI benefits from 12 to 18 months, because $D_{a_i \geq a^*}$ is quasi-randomly assigned based on age at separation for those close to the threshold. The regression discontinuity analysis is confined to males working in West Germany to ensure homogeneity in the effects of PBD extensions and comparability over time. [4]

Schmieder, von Wachter and Bender [2012*a*,*b*, 2016] use the same identification strategy and present ample evidence for the validity of the crucial assumption that longer potential benefit duration is indeed quasi-randomly assigned close to the age cutoffs. The results in Section 4 add to this evidence by showing that the number of separations as well as average pre-employment wages and other worker characteristics are continuous at the age threshold in our sample. For comparability with the results of Schmieder, von Wachter and Bender

[4] We will investigate how the inclusion of women affects our results in sensitivity tests.

[2016], we use a bandwidth of two-years on each side of the threshold. Optimal bandwidth calculations [Imbens and Kalyanaraman 2012; Calonico, Cattaneo and Titiunik 2014] suggest a smaller bandwidth, so we apply the bias correction of Calonico, Cattaneo and Titiunik [2014] throughout. Note that this correction makes our point estimates differ from the effect sizes suggested by the graphs. We will assess the robustness of our results to these choices in future analyses. All standard errors are regression discontinuity robust [Calonico, Cattaneo and Titiunik 2014].

3.2 Outcomes

We first use the strategy described above to estimate the effects of potential UI benefit duration for male workers on the main outcomes of Schmieder, von Wachter and Bender [2016]: the duration of UI receipt and non-employment as well as post-unemployment wages. Studying non-employment duration and wages requires the individual to have an employment spell after unemployment. Consequently, our estimates are the effects of potential benefit duration conditional on finding a job again and do not capture effects of potential benefit duration on permanent exit from employment.

To analyze how UI benefit extensions affect wages, we next estimate wage components using the following match effects model:

$$w_{ijt} = x_{ijt}\beta + \theta_i + \psi_j + \lambda_{ij} + \tau_t + \eta_{ijt} \tag{2}$$

where w_{ijt} is the log daily wage of individual i working at firm j in year t. x_{ijt} is a vector of time-varying control variables that includes work experience in days, following Mincer linearly and quadratic. Furthermore the model comprises year fixed effect denoted as τ_t to account for the business-cycle. θ_i and ψ_j are worker and firm fixed effects as in the two-way fixed effect models commonly used decompose wages from linked employer-employee data [see Abowd, Kramarz and Woodcock 2008, for an overview]. The match effect model generalizes these two-way fixed effect models by additionally including the interaction between firm and individual fixed effect, λ_{ij}, which is referred to as match fixed effect. See Woodcock [2015]; Jackson [2013] and Mittag [2016] for further discussion of the match effects model. Identification of worker and firm fixed effects requires all firms in the sample to be connected by realized

mobility. Even within such connected groups, a normalization is necessary, so we normalize the average firm fixed effect to be zero. Match fixed effects nest both firm and individual fixed effects, so the mean of the match effects within each individual and firm is not identified. We normalize both to zero, so that estimated match effects sum to zero for each individual and firm. For further discussion of identification, see Abowd et al. [2002] for the two-way fixed effects model and Woodcock [2015] for the match effects model.

We use the methods of Mittag [2016] to estimate the model including the fixed effects. In order to capture the fixed effects as precisely as we can, we estimate the wage decomposition using the entire data (including workers of all ages and females). Otherwise the firm fixed effect may depend on the sample used in the outcome model. We then use the estimated fixed effects as dependent variables in equation (1). The dependent variable in equation (2) is log wages, so the units of the fixed effects are also log wages. Thus, treatment effects on these wage components estimated from equation (1) can be interpreted as changes in percent of the daily wage of the individual.

The estimated individual fixed effect of individual i, $\hat{\theta}_i$ captures the wage component due to all observed and unobserved worker characteristics that are permanent. Including a match effect in equation (2) allows wages to differ systematically between jobs and thus allows pre- and post unemployment wages to differ systematically. Thereby, its inclusion makes it more credible that the estimated individual fixed effect indeed isolates a permanent worker-specific wage component and is not biased by any treatment effects. In line with this, we show that there is no effect of potential benefit duration on worker fixed effects. The estimated firm fixed effect of firm j, $\hat{\psi}_j$ indicates how much the daily wage of a worker at at firm j differs from what the average firm pays a worker with the same permanent wage component θ and time-varying observables x. The estimated match fixed effect $\hat{\lambda}_{ij}$ captures the job-specific wage component. Such job-specific wage components likely arise if the productivity of workers differs across firms, for example due to specialization or firm specific human capital. They may also arise from differences in bargaining power, differences in compensation over the life-cycle or any other wage determinants that are constant within job, but not within firm or individual. The normalization of match effects attributes average match quality to the respective individual or firm fixed effect, so that match effects should be interpreted as deviations from the respective

individual (or firm) average.

4 RDD Validity and Prior Results

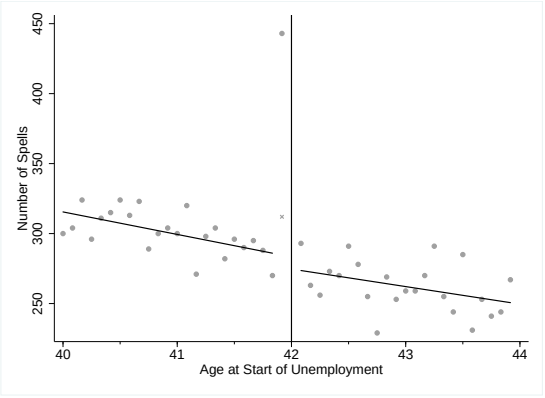
We first replicate the analyses of the validity of the regression discontinuity design and the basic results of Schmieder, von Wachter and Bender [2016] using our data. Despite the differences in the sample composition and our smaller sample, the results are remarkably similar.

Specifically, Figure 1 panel (a) provides a frequency plot of age at the start of the unemployment spell around the age cutoff. There is no evidence that individuals postpone the start of unemployment to increase their PBD. [5] Figures 1 panel (b) and (c) provide further evidence of the validity of the regression discontinuity design by showing that it passes two falsification tests. Panel (b) confirms that as in Schmieder, von Wachter and Bender [2016], there is no effect on pre-unemployment wages. Panel (c) extends this test by showing that there is no change in the worker fixed effect around the cutoff. This shows that there is no selection into treatment based on time-invariant wage determinants, which adds further credibility to the RDD assumptions. In both graphs the visual expression seems to suggest a difference at the threshold, but it is small in substantive terms and not confirmed by the RDD estimate. The RDD coefficients are equal to 0.0062 and -0.00511, respectively, with p-values above 0.7. Figure 1 panel (c) also provides evidence that treatment does not substantially bias the estimated individual fixed effects. As pointed out above, the effects of unemployment and hence PBD extensions can affect the estimated individual fixed effects in a standard two-way fixed effects model. Workers to the right of the threshold experience longer unemployment durations, so this could create spurious treatment effects on the individual fixed effect. Our model includes a match effect, so that only changes in mean match quality can affect the estimated individual fixed effects. The graph shows that this does not seem to influence our estimates. This is due to the fact that there is at most a negligible effect on match effects, as we show below.

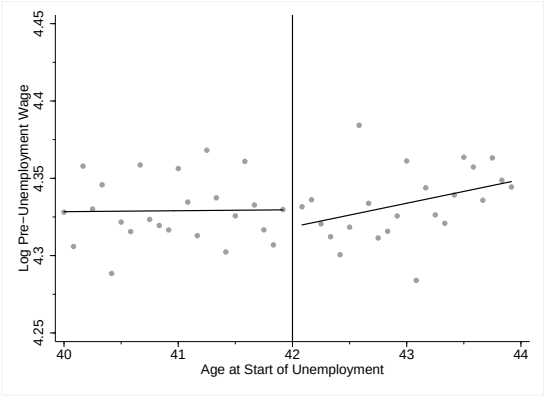
[5] For those, who start unemployment at age 41 years and 11 months two values are displayed. One showing the number of observations with an age adjustment where we include workers born in the calendar month in which unemployment starts, given that the unemployment spell starts on the 1st or the 2nd of the month. Here we assume that these workers are born after the 2nd. This will include a small number of treated individuals, roughly 1/15th of those who start UI in the month of their birthday, in the control group and slightly attenuate our results. The other excludes these workers and makes the frequency plot continuous. In fact, the frequency of new unemployment spells slightly drops after the cutoff for the unadjusted case. Thus, if there is any threshold manipulation, it is into shorter PBD. We will assess the sensitivity of our results to this convention in future analyses.

FIGURE 1
RDD VALIDITY AROUND THE AGE THRESHOLD

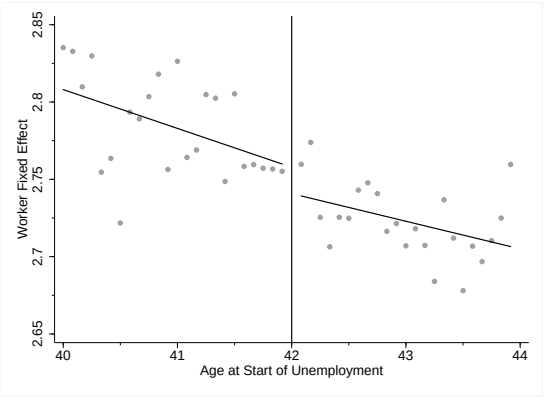
(a) Frequency of Observations



(b) Log Pre-Unemployment Wages



(c) Worker Fixed Effect



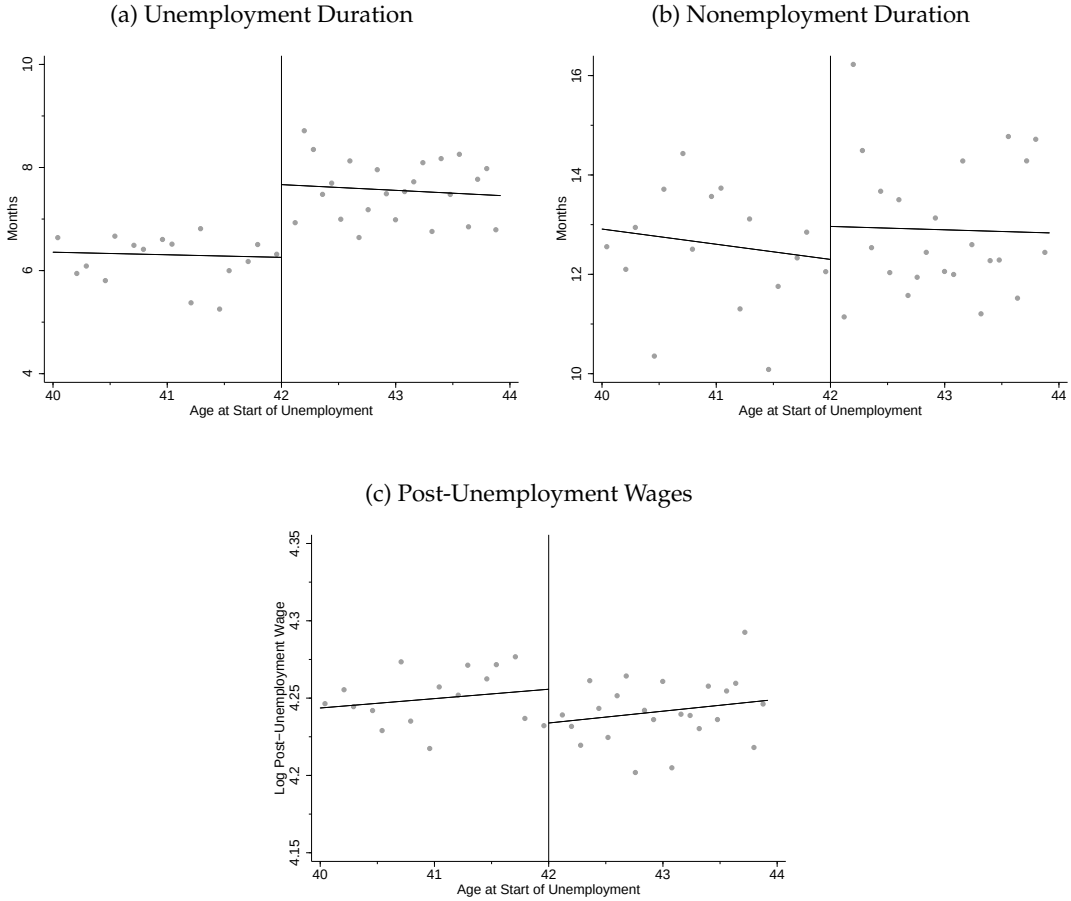
Notes: Panel (a) displays the density of spells by age at the start of the UI spell. Pabel (b) displays log pre-Unemployment wages for the last observation before unemployment. Panel (c) displays worker fixed effects obtained from the wage decomposition. Monthly bins. See Footnote 5 for further information. Vertical line marks UI age threshold at age 42.

Next, we confirm that estimates of the effects of PBD from our data are in line with the results of Schmieder, von Wachter and Bender [2016] who use the universe of UI claimants. Figure 2 visualizes the change in unemployment duration, non-employment duration and post-unemployment wages at the discontinuity where PBD jumps from 12 to 18 months. Table 1 provides the corresponding RDD estimates. We follow Calonico, Cattaneo and Titiunik [2014] in documenting bias-corrected estimates with robust standard errors. The results are remarkably similar to those of Schmieder, von Wachter and Bender [2016]. Longer PBD increases time spent in unemployment and non-employment as one would expect:[6] The six months PBD ex-

[6]The differentiation between unemployment, defined as the duration of benefit receipt, and nonemployment, defined as the time gap between two full-time employment spells without any intermediate dependent employment is not trivial in this context and the differences between these two measures of "not working" require further analyses. We focus on unemployment in this preliminary draft, but will analyze and discuss non-employment

tension leads to an increase of 1.51 months of unemployment duration, which is close to the effect of 1.77 month reported by Schmieder, von Wachter and Bender [2016]. We find the PBD extension to lead to an additional 2.17 months of non-employment, which is larger than the 0.95 months in Schmieder, von Wachter and Bender [2016]. However, before the bias correction that they do not apply, our estimate is close to theirs at slightly less than one additional month of nonemployment.

FIGURE 2
THE EFFECT OF PBD EXTENSIONS I



Notes: Results from RDD regression using a bandwidth of 2 years and a linear approximation. Vertical line marks UI age threshold at age 42.

While the positive effect of a PBD extension on the duration of un- and nonemployment is well established, its effect on post-unemployment wages is debated more. Figure 2 panel (c) shows that post-unemployment wages decrease by 0.05 log points (5 percent) at the age cutoff where PBD increases from 12 to 18 months. The bias-corrected estimate in Table 1 is smaller

further in future revisions.

at -0.01 log points and not statistically significant at the 10 percent level. The conventional RDD point estimate is -0.02 log points. Both estimates are similar to the effect of -0.0078 log points in Schmieder, von Wachter and Bender [2016], but not significantly different from 0. Their estimate is significant at the 5 percent level using the much larger entire population of unemployed workers. Thus, the true effect is likely negative in both samples and our estimate only suffers from a lack of precision.

TABLE 1
EFFECT OF PBD ON UNEMPLOYMENT AND NONEMPLOYMENT DURATION AND WAGES

	Unemployment Duration (1)	Nonemployment Duration (2)	Log Wage (3)
RD estimate	1.51***	2.1741**	-0.01016
	[0.42837]	[0.98779]	[0.02087]
Mean	6.87	12.75	4.25
Effect relative to mean	0.24	0.17	
Observations	13392	13392	13392

Robust standard errors according to Calonico, Cattaneo and Titiunik (2014) in brackets. */ **/ *** refers to $\alpha = 0.1/0.05/0.01$.
Source: LIAB Mover Model, own calculations.

5 Decomposing the Effect of PBD on Wages and the Re-employment Wage Path

As described in section 3.2 we run the match effects model (2) on the entire sample of full-time employees in West Germany to decompose wages into person, firm and match fixed factors. The results are displayed in Table 3.

The estimates for experience and experience squared as well as the year dummies are highly significant and conform with previous findings. Furthermore, the F-test for the relevance of the fixed effects indicates that they play a important role in explaining wages.

5.1 Decomposing Aggregate Effects

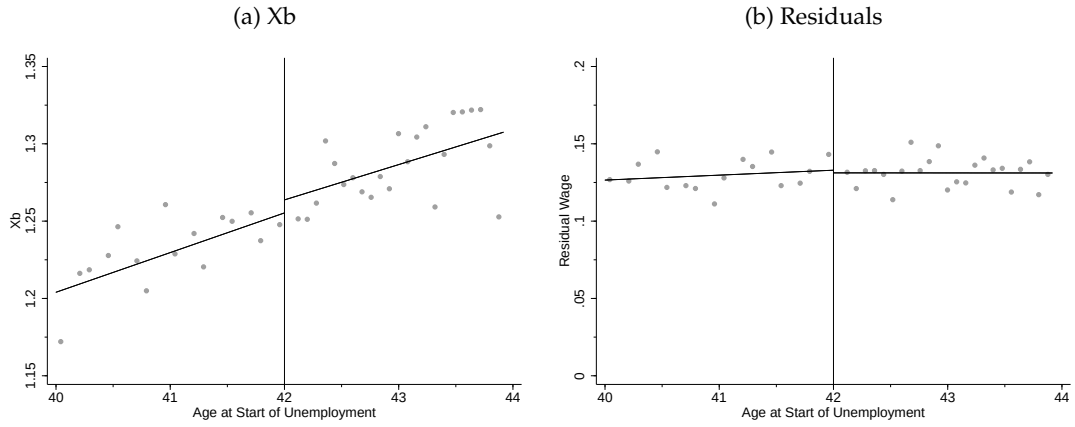
Looking individually at these wage components, we gain insights on how post-unemployment wages are affected by PBD extensions. First Figure 3 panel (a) and (b) reveal that neither the observed time-varying component Xb nor the residuals from the regression exhibit a jump at the age threshold. Therefore, they are not responsible for changes in re-employment wages.

TABLE 2
WAGE DECOMPOSTITION

	Coefficients	SE
Experience	0.0075549***	0.0000692
Experience Sq	-0.00000321***	0.0000000455
1993	0.3290116***	0.005341
1994	0.2833377***	0.0045276
1995	0.2521413***	0.0038334
1996	0.1980015***	0.0030971
1997	0.1435327***	0.00242
1998	0.0912137***	0.0016614
1999	0.0504677***	0.0009083
2000	Reference Year	
2001	-0.0437623***	0.0009027
2002	-0.0966004***	0.0015459
2003	-0.1313081***	0.002236
2004	-0.1898366***	0.0030394
2005	-0.2472489***	0.0038093
2006	-0.3014595***	0.0046257
2007	-0.3477242***	0.0053302
2008	-0.3920538***	0.0060598
N	31990529	
Adjusted R-squared	0.8186	
F-Test Coefficients	3046.83	
F-Test Fixed Effects	20.27	

Estimated with the model provided by Mittag [2016].
Source: LIAB Mover Model, own calculations.

FIGURE 3
THE EFFECT OF PBD EXTENSIONS II

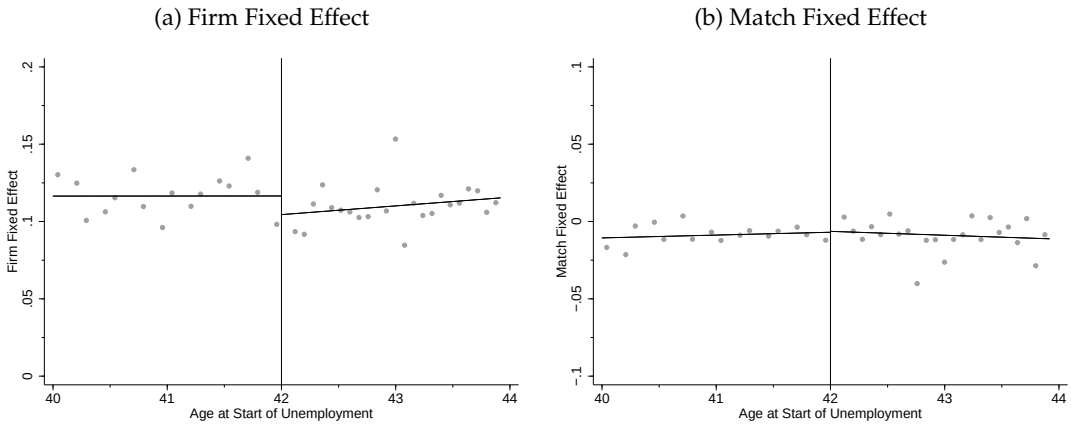


Notes: Results from RDD regression using a bandwidth of 2 years and a linear approximation. Vertical line marks UI age threshold at age 42.

This is supported by the corresponding bias-corrected RDD-estimates of the effect of PBD on each wage component presented in Table 3. Only the effect on the firm fixed effect is significantly different from zero. Six months of additional PBD lead to a decrease of 0.02 log points

in the post-unemployment firm fixed effect. The magnitude is sizable; it implies that workers with longer PBD accept jobs at firms that pay 2 percent less to all their workers compared to the group with the shorter PBD. The effect on the firm fixed effect is of the same order of magnitude as the effect of wages overall and potentially even larger. This is in line with prior results that find the effect to be driven by moving to firms that are worse on some dimension [Nekoei and Weber 2017; Schmieder, von Wachter and Heining 2018]. We do not find effects on other wage components, so our results suggest that the entire wage loss from longer PBD is due to working for lower-paying firms. We will further scrutinize this finding. The RDD results for the firm and match fixed effect are presented in Figure 4 panel (a) and (b).

FIGURE 4
THE EFFECT OF PBD EXTENSIONS III



Notes: Results from RDD regression using a bandwidth of 2 years and a linear approximation. Vertical line marks UI age threshold at age 42.

Interestingly, there is no effect on the match effect, i.e. those with longer PBD sort into firms that pay low wages to all employees rather than accepting low wage jobs. Since individuals in the age range we examine do not switch jobs frequently and only slowly climb up the job ladder [Haltiwanger et al. 2017], this suggests that the wage losses are not transitory. Indeed, we neither find match fixed effects nor on the job wage growth to indicate any re-bound of post-unemployment wages. We will further examine whether and how wages re-bound after unemployment using our long employment biographies. The fact that average match quality remains unaffected also explains why we find no bias in the estimated individual fixed effects above.

TABLE 3
EFFECT OF PBD ON WAGE COMPONENTS

	Xb	Residuals	Firm Fixed Effect	Match Fixed Effect
	(1)	(2)	(3)	(4)
RD estimate	0.0096	-0.01267	-0.02171*	0.00951
	[0.02455]	[0.00899]	[0.01237]	[0.00806]
Observations	13392	13392	13392	13392

Robust standard errors according to Calonico, Cattaneo and Titiunik (2014) in brackets. */ **/ *** refers to $\alpha = 0.1/0.05/0.01$.
Source: LIAB Mover Model, own calculations.

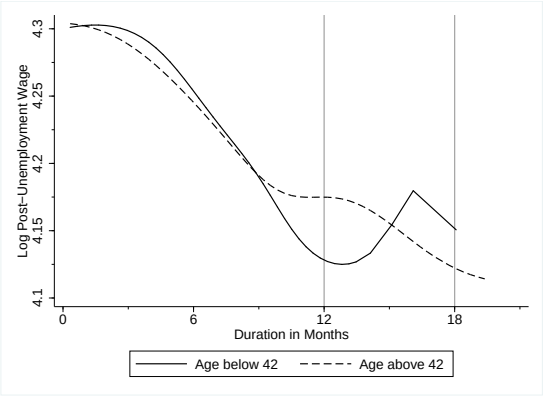
5.2 Decomposing Dynamic Differences

We next examine whether the re-employment path of these wage components shifts with PBD. As Schmieder, von Wachter and Bender [2016] show, if the distribution of re-employment wages does not change, then the effect of PBD only operates through unemployment duration. This rules out reservation wage effects and provides evidence against effect of job search. However, the test Schmieder, von Wachter and Bender [2016] use needs to rule out positive dynamic selection, which could offset reservation wage effects. We first confirm in Figure 5 panel (a) that the re-employment wage path does not change significantly in our data either. The figure is similar to that in Schmieder, von Wachter and Bender [2016], who argue that is provides evidence against reservation wage effects. Workers with the longer PBD achieve *lower* re-employment wages when finding a new job within the first 8 months of unemployment. This is contrary to theoretical expectations, which imply *higher* re-employment wages for the group with the lower unemployment exit hazard. After month eight, the picture is reversed. Accepting a job close to the first benefit exhaustion is associated with a drop in re-employment wages for the group with 12 months of benefit duration. The decrease in re-employment wages goes hand in hand with a steep rise in the unemployment hazard (Figure 5 panel (b)). A similar picture emerges for the group with longer PBD at 18 months, their benefit exhaustion. Generally, the comparison of re-employment wages between the two groups after month 12 of unemployment is difficult, because there are hardly any unemployed left in the group with the shorter PBD.

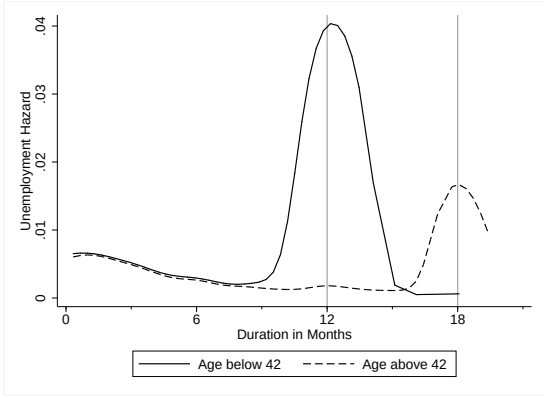
Note that while treatment status is randomized, treatment assignment is not randomized at each duration, because treatment affects duration. Thus, the difference between average treatment and control group outcomes at any (non-zero) value of unemployment duration is

FIGURE 5
RE-EMPLOYMENT PATHS I

(a) Log Post-Unemployment Wages



(b) Unemployment Hazard

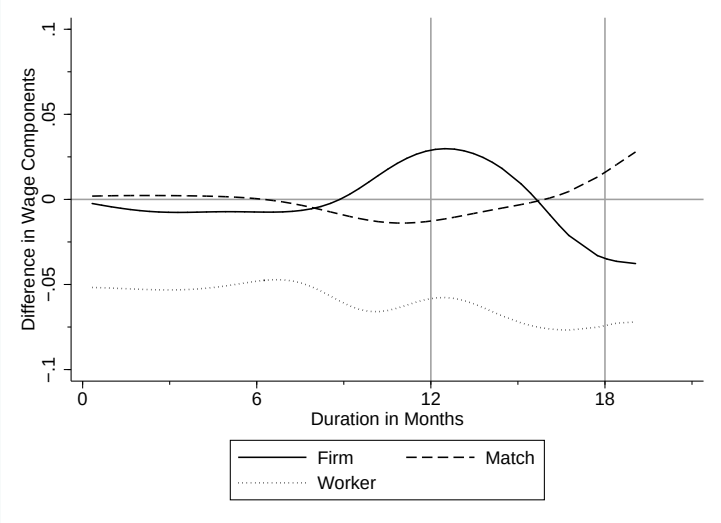


Notes: Panel (a) displays daily log post-unemployment wages of unemployment exit. Panel (b) displays daily unemployment hazards. For both groups calculated with a local polynomial estimation applying a gaussian kernel. Vertical lines mark UI exhaustion at 12 and 18 months.

not necessarily the dynamic treatment effect [Ham and LaLonde 1996]. If there is dynamic selection, the difference between treatment and control group suffers from the interpretation problem Bedoya et al. [2017] describe for quantile treatment effects: it combines the dynamic treatment effect with a "mobility effect", because treatment potentially re-shuffles individuals along the time axis. Ideally, we would isolate the dynamic treatment effect from dynamic selection. This would allow us to separate changes in the re-employment wage path into sorting or dynamic selection effects and search effects.

To gain first insights into this question, we first analyze how individual, firm and match fixed effects change with realized unemployment duration in the two PBD regimes. Figure 6 shows that longer PBD duration clearly affects the path of the re-employment wage components. These effects of PBD beyond the effect of longer unemployment duration make it questionable whether PBD extensions yield valid (and interesting) instrumental variable estimates of the effect of unemployment duration on wages, even when the re-employment wage path does not shift as Schmieder, von Wachter and Bender [2016] argue. These changes in the composition of re-employment wages could either arise from sorting, i.e. changes in the composition of the individuals leaving at time t, or from dynamic treatment effects, i.e. changes in the re-employment wages of those leaving at a given time t. The latter would likely indicate effects of search on wages.

FIGURE 6
DIFFERENCES IN THE RE-EMPLOYMENT PATH OF WAGE COMPONENTS



Notes: Difference between PBD groups in individual, firm and match fixed effect by month of unemployment exit. For both groups calculated with a local polynomial estimation applying a gaussian kernel. Vertical lines mark UI exhaustion at 12 and 18 months.

We first examine the effect on the re-employment path of the individual fixed effect. Individual fixed effects are constant over time and should thus not be affected by treatment. As we show above, this is the case in our sample. Consequently, changes in the re-employment path of the individual fixed effect are purely due to dynamic selection. The results in Figure 6 show that longer PBD affects dynamic selection: Around the two thresholds, those who leave unemployment from the group for which the respective threshold is relevant have higher individual fixed effect than those who leave at the same time from the group for which the threshold is irrelevant. This suggests that some individuals with (relatively) higher wages seem to postpone re-employment to the end of the UI spell, but could also have other causes. We will further characterize the nature of this dynamic selection, specifically whether it is driven by individuals that postpone re-employment or by a change in sample selection, because we restrict the sample to those who have at least one post-unemployment job.

Descriptively, Figure 6 suggests that there may be positive dynamic selection, which could offset changes in the firm fixed effect along the re-employment wage path. This may explain why Schmieder, von Wachter and Bender [2016] do not find evidence of reservation wage effects. Nekoei and Weber [2017] document changes in the re-employment wage path and show that the effect of PBD depends on PBD duration. However, they attribute these changes to

search effects, while our results for the individual fixed effect clearly show that sorting also has an important effect on estimates. Specifically, the non-monotonic pattern of the dynamic differences in 6 suggest that one would obtain spurious variation in the size of the treatment effect by varying the time in unemployment considered. However, the differences drop to zero quickly after the second exhaustion point, so this is unlikely to drive the results of Nekoei and Weber [2017].

The dynamic differences in the firm and match fixed effect in Figure 6 are more difficult to interpret, because they potentially combine search and sorting effects. If one assumes that the individual fixed effect completely controls for dynamic selection, then the dynamic difference between firm and match fixed effects at t is the conditional treatment effect of PBD on firm/match fixed effects. However, there may also be dynamic selection on firm fixed effects. The fact that the treatment effects around benefit exhaustion are symmetric and of opposite sign suggests that sorting also plays a role. The analysis of pre- and post-unemployment firm fixed effects of those exiting around these exhaustion dates in the next section indeed suggests that some high-wage individuals with prior employment at high wage firms who accept jobs at low-wage firms tend to postpone their exit from unemployment to the end of the unemployment spell. In addition to suggesting that sorting is important, this shows that estimated effects of PBD will depend on the distribution of firm fixed effects among those entering unemployment. Those with large wage losses tend to experience longer unemployment, which would affect re-employment wages even in the absence of search effects. This appears particularly relevant for studies of mass layoffs, where the effect will depend on the firm fixed effects of the affected firms. We find a qualitatively similar pattern for the match effect, but the changes are small and noisy.

6 Dynamics Over the Unemployment Spell

Overall, our preliminary results from the previous section show that not only unemployment duration and search, but also sorting or dynamic selection plays an important role in studies of PBD. In order to analyze search effects, we need to be able to isolate them from sorting effects. This requires a better understanding of the nature and causes of dynamic selection. The presence of dynamic sorting also raises the question whether the step drop in re-employment

wages with unemployment duration is indeed an effect of unemployment duration or partly explained by the sorting patterns we find. More generally, a better understanding may also shed light on theories of job search and how UI affects outcomes. To shed light on these questions, we will analyze dynamic patterns in wage components and how they change with PBD in more detail. The preliminary results in this section provide a first impression of what mechanically drives the effects we find above. Future revisions of this paper will use additional data to examine potential causes.

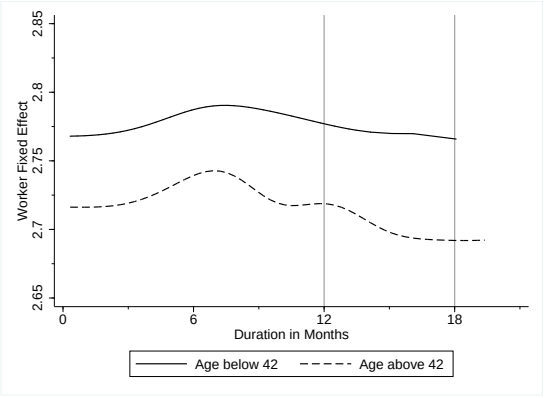
Figure 7 plots the average worker fixed effect by unemployment duration under the two PBD regimes, thereby disaggregating the dynamic differences in the individual fixed effect in Figure 6. As we point out above, there is no treatment effect on individual fixed effects, so the changes in the re-employment path of individual fixed effects can only be due to dynamic selection. The individual fixed effect captures all time-invariant worker characteristics, so Figure 7 panel (a) shows how dynamic selection on permanent worker characteristics changes with PBD. Over most of the unemployment spell, the average worker fixed effect declines slightly, i.e. there is negative dynamic selection, with only small differences between the two PBD groups. However, around four months before benefit exhaustion, the two PBD groups start to differ, leading to the off-setting effects we document above. Dynamic selection seems to be positive after the UI-cutoff date.

These results require further investigation. We will investigate whether the patterns indeed indicate positive dynamic selection or whether they are due to restricting the sample to those who eventually find a job again. Our data include those who permanently exit the labor force, so we will be able to investigate this issue. We will use the entire sample to examine how these patterns of dynamic selection differ by PBD for other age groups. We will examine the implications of this sorting for overall treatment effects. We plan to use the individual specific characteristics in our data to analyze to what extent dynamic selection is driven by time-invariant observables and unobservables to assess our ability to detect and address the problem of dynamic selection. Thereby, we hope to be able to provide evidence on the underlying causes of dynamic selection, e.g. whether they are driven by sample selection, more patience or differences in the ability and willingness to post-pone re-employment.

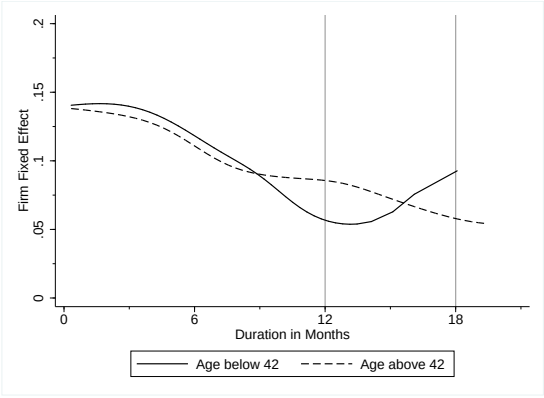
Figure 7 panel (b) plots the average firm fixed effect as well as the difference between the

FIGURE 7
RE-EMPLOYMENT PATHS II

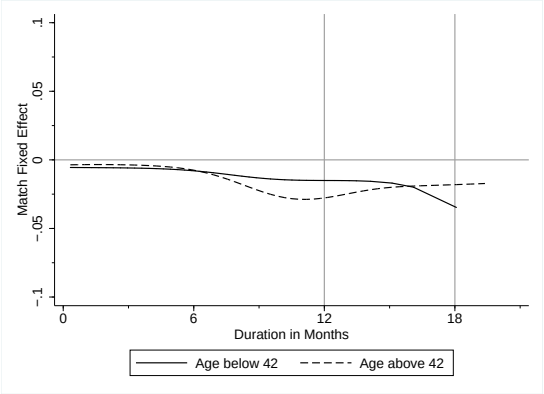
(a) Worker Fixed Effect



(b) Firm Fixed Effect



(c) Match Fixed Effect



Notes: Panel (a) displays daily worker fixed effects of worker at unemployment exit. Panel (b) displays daily firm fixed effects of worker at unemployment exit. Panel (c) displays daily match fixed effects of worker at unemployment exit.For both groups calculated with a local polynomial estimation applying a gaussian kernel. Vertical lines mark UI exhaustion at 12 and 18 months.

pre- and post-unemployment firm fixed effect by unemployment exit months. Contrary to the worker fixed effect in Figure 7 panel (a), we find clear evidence of a treatment effect on firm fixed effects. If dynamic selection is only due to time-invariant worker characteristics, the results in figure 7 panel (b) would isolate dynamic treatment effects and would show us when the wage losses due to longer PBD occur. However, our preliminary results suggest that sorting on the firm fixed effect is also likely to play a key role, so that analyses of what drives the overall effect need to disentangle search and sorting. Specifically, we see a substantial drop in the average firm fixed effect around the respective UI exhaustion dates. This decline persists when looking at the difference between pre- and post-unemployment firm fixed effects, i.e. those ex-

iting around UI exhaustion indeed incur wage losses in terms of their firm fixed effect. Examining pre-employment firm fixed effects further shows that the pattern is driven by individuals who come from high-wage firms and accept post-unemployment jobs at low wage firms. One possible explanation may be that those who face steep wage losses postpone re-employment to the end of UI benefit eligibility. This is well aligned with the positive dynamic selection we find above in that more productive individuals may be more likely or able to do so. We will examine whether the same workers are driving these two effects, i.e. whether those incurring losses on the firm fixed effect indeed have above average individual fixed effects. However, there are many other possible explanations besides the simple idea that forward-looking agents facing wage losses may be better of claiming UI longer than returning to employment. The patterns we currently document could also be driven by reference-dependent job search or arise from other changes in job search behavior. They could in principle be due to simple explanations such as recall or cyclical employment. The detailed information on individuals, firms and their workforce will allow us to distinguish these potential causes.

Interestingly, these changes at most partly drive the overall effect on wages. Those leaving unemployment around exhaustion experience longer unemployment with longer PBD and therefore indeed have lower re-employment wages. We will estimate the overall effect of PBD on the firm fixed effect with and without this group of people to analyze what share of the overall effect is driven by this pattern. We will examine whether the wage loss these individuals incur is entirely explained by longer unemployment. Our preliminary results suggest that the deviations from the re-employment path are roughly symmetric, so that they only have a small impact on the overall effect. Rather, we find evidence that individuals exiting early in the unemployment spell accept jobs at lower paying firms if their PBD is longer and thereby drive the effect of PBD on re-employment wages. We will examine what drives these effects further and use our detailed data to attempt to disentangle search and sorting.

Figure 7 panel (c) provides the corresponding graph for the match fixed effect. In line with the fact that we do not find an overall treatment effect, the results are noisier and inconclusive. There is a similar pattern of individuals accepting unusually low match effects towards the respective benefit exhaustion dates. However, the results are noisy and contrary to firm fixed effects, we do not find an aggregate effect.

7 Conclusion

We examine how an increase in the potential benefit duration affects job quality after unemployment. We apply the same unemployment insurance regime and identification strategy as Schmieder, von Wachter and Bender [2016]. Hence, we estimate in a RDD framework the impact of a change in the potential benefit duration from 12 to 18 months at the threshold age of 42 for workers with a high labour market attachment on post-unemployment outcomes. We confirm Schmieder, von Wachter and Bender [2016] results that an extension of the duration by 6 months increases unemployment duration and decreases post-unemployment wages. In contrast to Schmieder, von Wachter and Bender [2016] who have access to the universe of claimants, we use a smaller linked employer-employee data set, the LIAB Mover Model. This data allows us to decompose wages into person, firm and match fixed effects. Therefore, we can investigate the channels through which the detrimental effect on post-unemployment wages work.

Using the fixed effects from a wage decomposition, we show that the impact almost entirely operates through the firm fixed effect. The magnitude is sizable; it implies that those workers with the longer PBD sort into firms that pay 2 percent less to all their workers compared to the group with the shorter PBD. This aligns well with recent findings on the importance of firms for wage determination [Card, Heining and Kline 2013]. The wage-loss associated with a longer PBD does not appear to be transitory. This suggests that the wage loss is probably caused by more permanent reasons, which we will examine further. We find pronounced changes in the re-employment path of wage components. Thus, our results indicate wage effects beyond the effect of unemployment duration, which stand in contrast to Schmieder, von Wachter and Bender [2016].

The main contribution of this paper is to analyze the role of search and sorting and how they help to explain the contradictory results of the recent literature. Our preliminary analyses of dynamic differences and the dynamics of wage components suggest that dynamic selection over the unemployment spell plays an important role in the analyses of UI and PBD effects. Interestingly, we can show that those workers with the longer PBD achieve *lower* re-employment wages when finding a new job within the first 8 months of unemployment. This contradicts theoretical expectations as the value of their outside option is higher. Specifically, we find

evidence that (relatively) good workers who face substantial wage losses due to moving to a low-wage firm exit toward the end of their UI benefit eligibility period.

Understanding the nature of this sorting is important to our understanding of how individuals react to longer PBD and how this affects outcomes. It is also crucial to understand sorting in order to isolate search effects from sorting. Thus, we will use our detailed data on individuals to further examine what kind of workers leave UI later in response to longer PBD and how this affects their outcomes. We will use firm attributes from the firm survey and the detailed information on workforce characteristics to analyze what kind of firms drive these effects. We hope that these analyses will shed further light on the underlying causes of the patterns we find and can help to explain both why PBD affects re-employment wages and why re-employment wages drop steeply with unemployment duration.

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