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ABSTRACT

How Does Potential Unemployment Insurance Benefit Duration Affect Reemployment Timing and Wages?*

Recent papers identify the effects of unemployment insurance and potential benefit duration (PBD) on unemployment duration and reemployment wages using quasi-experiments. To make known problems of heterogeneity in quasi-experiments tractable, they often use models of job search, but we argue that letting the data speak without restrictions remains surprisingly informative. We focus on two broad questions: How informative are the local average effects quasi-experiments identify and what can we learn about causes and mechanisms from quasi-experiments in the presence of heterogeneous treatment effects? We first line out a framework for treatment effect heterogeneity with two interdependent outcomes, such as duration and wages, and then re-examine the effects of longer PBD in Schmieder, von Wachter and Bender (2016). Local average effects become more informative when amended with other parameters identified by (quasi-)randomization: Duration effects of PBD almost exclusively prolong few long spells, which helps to explain differences between studies. Dynamic selection into reemployment timing is non-monotonic, but does not change with PBD at short durations so dynamic treatment effects are identified at short durations. For wage effects of PBD, we find neither evidence of positive effects nor meaningful heterogeneity. Even though key structural parameters are not identified because LATE confounds average effects with the covariance of first and second stage effects, the data remain informative about causes and mechanisms. A wage decomposition shows that wage loss operates through the firm fixed effect, which speaks against individual-based causes such as skill depreciation or bargaining. Using dynamic treatment effects and mediation analyses, we find PBD to affect wages even for workers who do not change unemployment duration, i.e. directly. The negative direct effect we find casts doubt on key assumptions of common models of job search.

JEL Classification: J31, J64, J65

Keywords: unemployment, unemployment insurance, benefit duration, heterogeneous treatment effects

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1 Introduction

Understanding the effects of unemployment insurance (UI), such as whether and how it increases or decreases wages, is crucial for policy and for labor market questions, including how search, selectivity and unemployment duration affect labor market outcomes. The recent literature (surveyed in Schmieder and Von Wachter 2016) has made substantial progress by using (quasi-)random variation in potential benefit duration (PBD) at age cutoffs to identify local average effects. Yet in the presence of treatment effect heterogeneity these local average effects, such as RDD or IV estimates, can be difficult to interpret and contain limited information about causes and mechanisms (Heckman, Urzua and Vytlacil 2006; Deaton 2009; Imbens 2010). Recent papers take heterogeneity into account by interpreting local average effects through the lens of theoretical job search models that allow for heterogeneity. Despite this, heterogeneous treatment effects can still create statistical challenges, especially with two interdependent outcomes such as unemployment duration and reemployment wages.

In this paper, we examine what can be learned from (quasi-)experiments without models that restrict treatment effect heterogeneity, i.e. what the data alone can tell us. A data-driven approach enables us to identify which conclusions require assumptions beyond validity of the quasi-experiment and to evaluate these assumptions. We argue that the data remains informative despite heterogeneous treatment effects, even without strong restrictions. We focus on two broad questions: First, how informative are the local average effects quasi-experiments identify, i.e. what can we learn from local effects of PBD on unemployment duration and reemployment wages? We show that documenting the extent and nature of treatment effect heterogeneity can help us to learn more from quasi-experiments about the effects of PBD and hence the consequences of PBD extensions, which are of key policy relevance. Second, what can we learn about mechanisms and causes behind wage effects from quasi-experiments without imposing the restrictions of a model? We argue that even though key structural parameters depend on the relationship between treatment effects at the individual level and are hence not identified by (quasi-)experiments, we can still learn about potential causes and mechanisms using wage decompositions and tools from mediation analysis.

To establish these points, we first propose a framework for (quasi-)experiments with two interdependent outcomes to understand what and how we can learn from the observed data.

For the effects of PBD, it is well known that the informativeness of local effects crucially depends on whether all individual treatment effects have the same sign and that the interpretation of estimated effects depends on who responds to the PBD extensions (e.g. Heckman, Urzua and Vytlacil 2006). Therefore, it is important to test sign restrictions and learn about the nature of heterogeneous treatment effects using parameters such as conditional average treatment effects (CATE) and quantile treatment effects (QTE), which (quasi-)experiments identify. We argue that it is simpler to analyze the local effects of PBD on unemployment duration than the effects on wages. For duration effects, it is plausible to assume only positive treatment effects, which makes QTEs more informative about the distribution of treatment effects.

Wage effects are also more difficult to interpret than duration effects, because the mechanisms and causal paths behind wage effects are more complex. Duration effects may arise from an income effect due to higher total available benefits or relaxed search constraints due to a decrease in the probability of running our of benefits (Meyer 1990; Katz and Meyer 1990; Card, Chetty and Weber 2007*a*; Chetty 2008). For wage effects, the literature has put forward a plethora of potential mechanisms including bargaining power, signaling (e.g. Jarosch and Pilossoph 2019; Zuchuat et al. 2023), skill depreciation (e.g. Ljungqvist and Sargent 1998; Cohen, Johnston and Lindner 2023) and changes in job search (e.g. Marinescu and Skandalis 2021; DellaVigna et al. 2022), job offers (e.g. Zuchuat et al. 2023), selectivity (e.g. Le Barbanchon, Rathelot and Roulet 2017) and matching (e.g. Raposo, Portugal and Carneiro 2019; Schmieder, Von Wachter and Heining 2023). For our second question, we thus focus on wage effects to examine what and how we can learn about the causes and mechanisms through which PBD extensions affect wages. We first argue that wage components from a fixed effects decomposition can provide suggestive evidence on potential causes, such as the importance of firms, skill depreciation, bargaining or matching.

In the absence of measures of skills or bargaining power studies typically examine what part of the effect of PBD on wages operates through longer unemployment duration, i.e. indirectly, and whether PBD affects wages if unemployment durations remain fixed, i.e. directly. Even though they do not pin down specific causes, these direct and indirect effects are informative about broader mechanisms behind wage effects. For example, Nekoei and Weber (2017) decompose wage effects into a (direct) selectivity and an (indirect) duration component.[1] The

[1]Conceptualizing effects caused by selectivity or reservation wages as direct effects assumes that these effects

indirect effect is usually thought to reduces wages for reasons such as skill depreciation, bargaining or signaling. On the contrary, direct effects are assumed to be non-negative because the option of receiving longer benefits should not harm workers who do not change behavior. But the direct effect may increase wages, for example because workers become more selective or increase their reservation wages. Direct effects are thereby central to understanding job search as well as whether and why longer PBD can improve post-unemployment outcomes, which in turn is crucial to reconcile the diverging estimates in the recent literature. The discussion about the sign and causes of the effect of PBD on wages, estimates of which range from negative (e.g. Schmieder, von Wachter and Bender 2016) to positive (Nekoei and Weber 2017), can be seen to revolve around whether there is a direct effect and if so, whether it can offset the wage loss from longer unemployment duration. More generally, separating direct and indirect effects is of broad relevance whenever a treatment affects two interdependent outcomes, as in mediation analyses.

We first discuss problems of identifying the parameters behind direct and indirect effects in the presence of treatment effect heterogeneity. Quasi-experiments do not identify the direct effect of PBD on wages. Even ruling out direct effects does not identify the effect of duration on wages. This stems from the fact that using PBD as an instrument for duration (Schmieder, von Wachter and Bender 2016) confounds the average effect of duration on wages and the covariance of the effects of PBD on duration and of duration on wages. More generally, we show that the IV estimator weights individual treatment effects by the first stage coefficient of the individual. Thereby, the IV weights depend on another parameter of interest, so that LATEs are weighted averages of two treatment effects. In consequence, LATEs do not isolate the effect on the outcome of interest, but estimate the sum of the average treatment effect and the covariance of first and second stage coefficients divided by the first stage effect. Thus, quasi-experiments do not allow us to separate direct from indirect effects. We argue that tools from mediation analysis (Glynn 2012) still allow us to analyze direct and indirect effects without restricting heterogeneity by analyzing whether wage effects are present among workers who do not change duration, i.e. in subsamples without duration effects. We show that wage de-

do not operate through longer unemployment duration. While models such as the ones in Schmieder, von Wachter and Bender (2016) and Nekoei and Weber (2017) do not rule out that selectivity increases wages via longer unemployment duration, both papers treat selectivity or reservation wage effects as movements off rather than along the duration-wage path, which makes them direct effects.

compositions are useful to study dynamic selection and to isolate dynamic treatment effects on wages, which can provide further insights into direct and indirect effects. Since the dynamic treatment effect estimates the average wage effect at every unemployment duration, it generalizes the strategy of learning about direct and indirect effects by analyzing how effects on duration and wages co-vary across subsamples.

The second, empirical part of this paper uses the insights from our framework to revisit the analysis of Schmieder, von Wachter and Bender (2016). They use an age-based discontinuity in PBD in Germany to estimate the effects of PBD on unemployment duration and wages, as well as the effect of unemployment duration on wages. We start by examining duration effects to understand who extends unemployment durations and then analyze wage effects to show that the tools we provide enable us to learn about heterogeneity in wage effects as well as the mechanisms behind these effects without the constraints of a theoretical model. For duration effects of PBD, we use QTEs to show that effects are concentrated among few workers and exclusively prolong long un- and nonemployment spells. These results suggest that the PBD extension did not relax search constraints for most individuals. Our findings thus add empirical support to the hypothesis of Nekoei and Weber (2017) that differences between prior results could arise from differences in the relevance of search constraints. Heterogeneous duration effects imply changes in dynamic selection, which complicate dynamic analyses. We use predetermined wage components from the wage decomposition described below to show that, even in the absence of effects of unemployment, reemployment wages vary substantially and non-monotonically over the unemployment spell. However, we find neither duration effects nor effects of PBD on dynamic selection before benefit exhaustion. Thereby, we provide evidence that the dynamic treatment effect is identified before shorter PBD ends.

For wage effects, we use linked-employer employee data (LEED) to decompose wages into time-varying observables and unobservables, as well as individual, firm and job fixed effects. Estimating effects on these wage components provides evidence that the effect of PBD on wages is mainly due to working for lower-paying firms, which speaks against individual-based causes of job loss such as skill depreciation, signaling or bargaining. Despite extensive testing, we find no evidence of positive effects of PBD on wages and little evidence of treatment effect heterogeneity. Thus, local effects are informative about wage effects of PBD extensions, which is important for policy analyses. Our finding that treatment effects do not vary with demographics also suggests that differences in estimated wage effects between studies stem from differences in the economic environment or UI policies rather than differences between the populations under study.

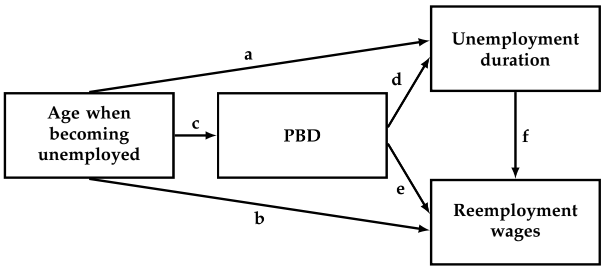
Finally, we examine what the data can tell us about the presence of a direct effect of PBD on wages. We use our result that there are no duration effects at short durations to analyze wage effects in samples without duration effects and dynamic treatment effects. These analyses provide evidence of a direct effect of PBD on wages. Therefore, we cannot estimate the effect of duration on wages. However, we find no empirical support for meaningful indirect effects. The negative direct effect we find shows that the common assumption that direct effects are positive, which partitions PBD effects into a positive direct selectivity channel and a negative indirect duration channel in models such as those of Schmieder, von Wachter and Bender (2016) and Nekoei and Weber (2017), is at odds with the empirical facts. A negative direct effect is also difficult to reconcile with the common assumption that workers cannot incur wage losses without changing duration, since longer PBD alone cannot make them worse off, suggesting that non-wage benefits may be important. Thus, letting the data speak without a model helps us identify assumptions that are at odds with the empirical facts and thereby aids the development of models that are consistent with the data.

Overall, we find the data to be very informative by itself, even when allowing for unrestricted treatment effect heterogeneity. The next section introduces a model-free theoretical framework of heterogeneity and the empirical tools we use. Section 3 reviews the institutional background, introduces our data, and replicates prior results. Section 4 examines duration effects and dynamic selection. Section 5 investigates wage effects. The final section concludes.

2 Learning in the Presence of Treatment Effect Heterogeneity

The effects of PBD are difficult to study because of endogenous selection into unemployment, PBD and unemployment duration. Consequently, many of the key findings in the literature come from quasi-experiments using discontinuities of PBD at age cutoffs in an RDD (e.g. Lalive 2007; Card, Chetty and Weber 2007*a*; Van Ours and Vodopivec 2008; Tatsiramos 2009; Centeno and Novo 2009; Degen and Lalive 2013; Caliendo, Tatsiramos and Uhlendorff 2013; Schmieder, von Wachter and Bender 2016; Nekoei and Weber 2017). This identification strategy constitutes a quasi-experiment in which one variable (age) generates random variation in an endogenous determinant (PBD) of two outcomes (unemployment duration and wages). Since one outcome, duration, may affect the other outcome, wages, this first outcome can also be seen as a mediator.[2] Figure 1 illustrates how this situation yields two "chained" IV or mediation analyses.

FIGURE 1
IV WITH TWO INTERDEPENDENT OUTCOMES



By ruling out that the IV directly affects the outcomes (arrows a and b), quasi-experiments are equivalent to mediation analyses without direct effects, i.e. that assume a "treatment" (the IV) to affect outcomes only through a mediator (the endogenous variable). This assumption identifies local effects of PBD on unemployment duration and reemployment wages. Schmieder, von Wachter and Bender (2016) argue that the effect of unemployment duration on wages (arrow f) is also identified if there is no direct effect of PBD on wages (arrow e). This assumption justifies using PBD as an IV for unemployment duration, because arrows c,d and f form two "chained" IV diagrams. Equivalently, we can see duration as a second mediator for which this assumption rules out direct effects. Formally, letting PBD_i, d_i, y_i denote realized PBD, unemployment duration and reemployment wage of individual i, we can write

$$d_i = \alpha_{di} + \beta_{di} PBD_i + \varepsilon_{di} \tag{1}$$

$$y_i = \alpha_{yi} + \beta_{yi} PBD_i + \gamma_{yi} d_i + \varepsilon_{yi} \tag{2}$$

where β_{di} is the (individual-level) effect of PBD on unemployment duration, β_{yi} is the direct effect of PBD on reemployment wages and γ_{yi} is the effect of unemployment duration on reemployment wages. Combining these two equations yields

$$y_i = \tilde{\alpha}_{yi} + (\beta_{yi} + \beta_{di}\gamma_{yi}) PBD_i + \tilde{\varepsilon}_{yi} \tag{3}$$

Thus, the effect of PBD on wages is the sum of the direct effect, β_{yi} and the indirect effect $\beta_{di}\gamma_{yi}$.

[2] We follow the literature in assuming that reemployment wages do not affect unemployment duration.

2.1 Effects of PBD: Learning About and From Heterogeneity

We are interested in the effect of changing PBD from 12 to 18 months on unemployment duration (β_{di}) and on reemployment wages ($\beta_{yi} + \beta_{di}\gamma_{yi}$). Under the assumptions above local average effects, i.e. (weighted)[3] expectations of the individual effects, are identified. However, positive and negative effects can cancel in these local average effects quasi-experiments identify, which can take any value between the maximum and the minimum of the individual treatment effects in the sample (e.g. Heckman, Urzua and Vytlacil 2006). To understand whether we can learn from (quasi-)experiments about the effects of PBD that determine the costs and benefits of PBD extensions as a policy tool, we thus need to assess whether the effects have the same sign for all individuals. The presence of heterogeneous treatment effects also makes many questions beyond mean impacts relevant, such as who benefits or responds most. Thus, to better understand the consequences of PBD extensions, it is important to learn about treatment effect heterogeneity. Parameters quasi-randomization identifies, such as CATEs, QTEs and other effects on features of the marginal distributions (Firpo and Pinto 2016), allow us to make progress with both questions.

For the effect of PBD on unemployment duration, sign restrictions seem plausible by ruling out negative effects: It would be odd for individuals to stay unemployed for a shorter period when they are eligible for unemployment benefits longer. Still, it is undisputed that duration effects are heterogeneous. Nekoei and Weber (2017) argue that differences in the probability of exhausting benefits, i.e. how binding a constraint benefit exhaustion is for the worker, are a key determinant of heterogeneous effects. How binding search constraints are affects both how much more time workers take until reemployment, i.e. duration effects, and how much more selective they become, which may affect wages. Therefore, it is important to understand whether and to what extent PBD extensions extend long spells, which would point toward relaxing relevant search constraints. Assuming no negative effects makes QTEs more informative about the nature of heterogeneity in duration effects, because individuals can only move in one direction along the x-axis. As our empirical analyses show this restriction enables us to infer that long spells indeed became longer from QTEs. QTEs also identify subsamples without

[3] For RDD estimates, we abstract from the problems of IV weights (Angrist and Imbens 1995; Heckman, Urzua and Vytlacil 2006; Heckman and Urzua 2010), since in the RDD analyses we examine, the compliers are a well defined group (individuals at the threshold) and the weights are well behaved (they are asymptotically uniform, but decrease in the distance to the threshold in finite samples for bias-corrected RDD estimates).

duration effects, which are crucial to examine the presence of direct effects.

For the effect of PBD on wages, sign restrictions are not plausible, since some individuals may benefit from longer durations. Restricting the sign of individual wage responses thereby implicitly presupposes the answer to many questions of interest, such as whether job search is productive. Therefore, we examine the question whether positive treatment effects are present empirically using QTEs and CATEs, which can establish, but not rule out the presence of positive effects (Bedoya et al. 2017). We also use CATEs and QTEs to examine the nature of heterogeneity in order to show that local effects are informative and to understand who gains or loses wages. Documenting the nature of treatment effect heterogeneity is also crucial to make sense of differences between studies and whether they arise from differences in demographics or differences in the UI system or other economic conditions.

2.2 The Problem of Identifying Structural Parameters

As Deaton (2009) points out, the treatment effects identified by (quasi-)randomization of PBD we discuss above often leave mechanisms in a black box. For example, whether PBD can increase wages by making workers more selective or how unemployment duration affects wages are crucial questions for our understanding of job search and unemployment. So randomization does not identify all parameters of interest. This problem is particularly acute for parameters that depend on the relation between two outcomes affected by one treatment. The effect of PBD on wages illustrates the problem well. As equation (3) shows, this effect combines the direct effect of PBD on wages, β_{yi} and an indirect effect of PBD that affects wages via unemployment duration, $\beta_{di}\gamma_{yi}$. The indirect effect in turn depends on two parameters, the effect of PBD on duration, β_{di} and the effect of duration on wages, γ_{yi}.

Equation (3) shows that estimating the effect of PBD on wages yields $\mathbb{E}[\beta_{yi} + \beta_{di}\gamma_{yi}] = \mathbb{E}[\beta_{yi}] + \mathbb{E}[\beta_{di}]\mathbb{E}[\gamma_{yi}] + \mathrm{Cov}(\beta_{di}, \gamma_{yi})$ (Glynn 2012).[4] Thereby, the estimated effect combines 3 channels: First, the average direct effect, $\mathbb{E}[\beta_{yi}]$. Second, the product of the average effect of PBD on duration, $\mathbb{E}[\beta_{di}]$, and the average effect of duration on wages, $\mathbb{E}[\gamma_{yi}]$. And third, the covariance of the individual effects of PBD on duration and duration on wages.[5] Therefore, the

[4]This result requires PBD to be independent of treatment effects (β_{di}, β_{yi} and γ_{yi}). We maintain this assumption throughout, as it seems plausible in the setup we examine, because those to the left and right of the age threshold should not differ in the distribution of their treatment effects.

[5]Note that this covariance is part of and hence distinct from the covariance between the effects of PBD on

estimated effect of PBD on wages leaves open the question whether PBD affects wages directly or indirectly. Neither does it answer the question whether indirect effects are driven by the duration response, the response of wages to duration or whether those with strong duration effects also have strong responses of wages to duration. While randomizing PBD (at best) identifies $\mathbb{E}[\beta_{di}]$, it does not identify the other three parameters that make up the effect.

It is tempting to learn about the direct effect (or even $\mathbb{E}[\gamma_{yi}]$) by controlling for realized unemployment duration when estimating the effect of PBD on wages. However, duration is endogenous so the coefficient on duration is biased, i.e. $\mathbb{E}[\hat{\gamma}_{yi}] \neq \mathbb{E}[\gamma_{yi}]$. Since duration is correlated with PBD by virtue of PBD affecting duration, this bias spills over to the estimated average direct effect, i.e. $\mathbb{E}[\hat{\beta}_{yi}] \neq \mathbb{E}[\beta_{yi}]$. As Yamamoto (2013) and Dippel et al. (2019) show, separating these structural parameters with one IV requires strong assumptions, namely that PBD is exogenous once we condition on duration. Even if these assumptions were to hold, these strategies (at best) estimate $\mathbb{E}[\beta_{yi}]$ and $\mathbb{E}[\gamma_{yi}]$. However, such estimates contain limited information about direct and indirect effects (Glynn 2012). If the estimate of $\mathbb{E}[\beta_{yi}]$ differs from zero, there is a direct effect for some individuals. However, even if the estimate is zero, there may be important direct effects at the individual level that cancel in the estimated average. The same logic applies to $\mathbb{E}[\gamma_{yi}]$ and the presence of an effect of duration on wages. As we argue above, γ_{yi} likely is positive for some and negative for other individuals, leaving it unclear what we can learn from a (weighted) average.

In light of these complications, the result of Schmieder, von Wachter and Bender (2016) that the ratio of the effect of PBD on wages and the effect of PBD on duration identifies the effect of unemployment duration on wages if PBD does not affect wages directly seems important. Unfortunately, when both the effect of PBD on duration, β_{di} and the effect of duration on wages, γ_{yi} vary between individuals, the average effect of duration on wages, $\mathbb{E}[\gamma_{yi}]$, is not identified even if we are willing to rule out direct effects.[6] To see the problem, consider the

duration and wages that Nekoei and Weber (2017) find to be negative.

[6]Note that even in the absence of heterogeneity this assumption cannot be tested by examining whether the reemployment wage path changes as Schmieder, von Wachter and Bender (2016) propose. When duration is endogenous, the effect of duration on wages differs from the slope of the conditional expectation, i.e. $\mathbb{E}[\hat{\gamma}] \neq \gamma$ in the linear case without heterogeneity. That is, the true causal effect of duration on wages moves individuals off rather than along the observed reemployment wage path, implying that the reemployment wage path changes in the absence of any direct effects. It only remains constant when duration is exogenous, i.e. when neither a quasi-experiment nor a test is necessary. In the presence of treatment effect heterogeneity, the test is uninformative, because there could be direct effects for some individuals that cancel in the average. However, as this discussion shows, other tests for IV validity could be used to test for the presence of direct effects (e.g. Kitagawa 2015).

ratio of individual effects of PBD defined by equations (3) and (1). The ratio of the (indirect) effect of PBD on wages, $\beta_{di}\gamma_{yi}$, and the effect of PBD on duration, β_{di}, for individual i is the effect of unemployment duration on wages, γ_{yi}, for individual i. Hence, the expectation of the ratio of these effects is indeed the average effect of duration on wages: $\mathbb{E}\left[\frac{\beta_{di}\gamma_{yi}}{\beta_{di}}\right] = \mathbb{E}[\gamma_{yi}]$. However, standard IV estimates, including those Schmieder, von Wachter and Bender (2016) propose, computes the ratio of expectations rather than the expectation of the ratio. Using the same argument (and assumption) as above when discussing the estimated effect of PBD on wages, this ratio estimates

$$\frac{\mathbb{E}[\beta_{di}\gamma_{yi}]}{\mathbb{E}[\beta_{di}]} = \mathbb{E}[\gamma_{yi}] + \frac{Cov(\beta_{di}, \gamma_{yi})}{\mathbb{E}[\beta_{di}]} \tag{4}$$

Therefore, this IV strategy only yields the average effect of duration on wages, $\mathbb{E}[\gamma_{yi}]$, if the effect of PBD on duration and the effect of duration on wages are uncorrelated. This condition trivially holds when either of the two effects is constant. With heterogeneous effects, it seems unlikely to hold since unemployment duration is likely affected by its effect on wages.

The observations above clarify the well-known problem of IV estimators in the presence of treatment effect heterogeneity (Heckman and Urzua 2010; Imbens 2010). The IV estimator replaces the expectation of the ratio by the ratio of the expectations. The left hand side of equation (4) shows that the IV estimator is indeed a weighted average of individual effects. It provides a simple expression for the IV weights, $\frac{\beta_{di}}{\mathbb{E}[\beta_{di}]}$: IV weights individual treatment effects by the first stage coefficient of the individual (normalized to make the weights sum to 1).[7] This result implies that the weighted averages IV identifies are not just a slight loss of information, but induce a fundamental interpretation problem, because they confound two parameters of interest: Any LATE is both the average effect of the endogenous variable on the outcome (weighted by $\frac{\beta_{di}}{\mathbb{E}[\beta_{di}]}$) and the average effect of the IV on the endogenous variable (weighted by $\frac{\gamma_{yi}}{\mathbb{E}[\beta_{di}]}$).

The right hand side of equation (4) shows how LATEs combine two treatment effects: under the assumption of IV validity and that the IV neither predicts the size of the first stage effects nor the second stage effects, the IV estimator estimates the average effect of interest

[7]This result just slightly generalizes the IV weights in Theorem 1 and 2 of Angrist and Imbens (1995) (and hence Proposition 1 of Schmieder, von Wachter and Bender (2016)). It is more intuitive and allows researchers to assess potential differences between LATE and ATE by examining treatment effect heterogeneity in their first stage estimates. It also yields the simple expression for the differences between LATE and ATE. See Masten and Torgovitsky (2016) for a similar formula that applies to the more general case of 2SLS.

plus the covariance of first and second stage effects divided by the average first stage effect. This discussion shows that the problems of IV estimators are amplified in a setting with multiple interdependent outcomes. With two outcomes, questions of interest often concern features of the joint distribution of two treatment effects. For example, if we want to separate direct and indirect effects or even analyze the effect of duration on wages, we need to learn about the covariance of two treatment effects. Neither randomization nor methods to estimate heterogeneous treatment effects (e.g. Wu and Perloff 2006; Heckman and Vytlacil 2007; Arellano and Bonhomme 2012) identify such features of the joint distribution of two treatment effects.

2.3 Learning About Mechanisms

The difficulties of identifying structural parameters discussed above raise the question what and how we can still learn about mechanisms behind wage effects. We first estimate effects on wage components to better isolate treatment effect heterogeneity from heterogeneity that arises even in the absence of treatment (Arellano and Bonhomme 2012) and to assess the plausibility of specific causes of wage effects such as skill depreciation, bargaining and matching of workers to firms. Specifically, we use LEED to decompose reemployment wages based on the following match effect model:

$$y_{ijt} = x_{ijt}\beta + \theta_i + \psi_j + \lambda_{ij} + \eta_{ijt} \tag{5}$$

where, in a slight extension of the notation in equations (1)-(3), y_{ijt} is the log daily wage of individual i working at firm j in year t. x_{ijt} is a vector of time-varying observables. In our application x_{ijt} only contains predetermined variables: work experience (linear and quadratic), and year fixed effects. θ_i and ψ_j are worker and firm fixed effects (e.g. Abowd, Kramarz and Woodcock 2008). The match effect model (e.g. Woodcock 2015) generalizes this model by adding the interaction between firm and worker fixed effects, λ_{ij}, which is a job fixed effect. See Appendix A for our results and discussion of potential problems such as limited mobility or unemployment inducing bias in our estimated worker fixed effects.

The worker fixed effect, θ_i, captures the wage component due to all permanent worker characteristics. Thus, it cannot capture treatment effects, since treatment changes at the time of separation by definition. Thereby, the worker fixed effect enables us to better study dynamic selection. The pre-determined wage components ($x\beta$ and θ) also allow us to purge some heterogeneity that does not arise from treatment which helps us isolate dynamic treatment effects.

The remaining, malleable, wage components provide suggestive evidence on potential causes by showing how PBD affects wages. The firm fixed effect, ψ_j, indicates how much the daily wage of workers at firm j differs from what the average firm pays workers with the same θ and x. This wage differential is common to all workers at the firm, so that it captures wage effects that apply to all workers at a given firm. Treatment effects on firm fixed effects thus show whether longer PBD causes individuals to work for firms that pay higher or lower wages to all of their workers. The job fixed effect, λ_{ij}, captures the job-specific wage component arising from idiosyncrasies of specific worker-firm matches. They reflect complementarities in productivity, heterogeneity in bargaining power, or any other wage determinants that are constant within job, but not within firm or worker.[8] Treatment effects on job fixed effects are informative about the extent to which higher- or lower-paying worker-firm matches are formed after unemployment.

In addition to learning about specific causes from wage components we examine broader mechanisms by analyzing the presence of direct and indirect effects. Both Schmieder, von Wachter and Bender (2016) and Nekoei and Weber (2017) conceptualize the effect of PBD on wages as the combination of a positive direct effect due to selectivity or reservation wages and a negative indirect effect due to longer unemployment durations. Nekoei and Weber (2017) argue that differences in wage effects between studies arise from direct effects being more and indirect effects less pronounced whenever the PBD extension relaxes more relevant search constraints. Understanding direct and indirect effects is thus not only crucial to reconcile the existing evidence, but also important for questions such as whether unemployment duration reduces wages or whether job search increases wages.

To learn about direct and indirect effects, we adapt the approach of mediation analysis. As Glynn (2012) discusses, a key tool is to examine how wage effects differ between samples with more or less pronounced duration effects. Finding effects on the outcome in samples without effects on the mediator establishes the presence of direct effects. How effects on the outcome vary with the size of effects on the mediator is informative about the presence and size of indirect effects. Understanding the nature of treatment effect heterogeneity well, as we argue

[8] The mean of the job fixed effects within each individual and firm is not identified and normalized to zero (Woodcock 2015), so they should be interpreted as deviations from the respective individual and firm average.

above, is crucial for this approach as it allows us to understand where duration effects are more pronounced and to identify subsamples without duration effects. We can use the same tools to examine whether wage effects are aligned with these duration effects. We generalize this strategy by looking at the average effect of PBD on reemployment wages conditional on unemployment duration with long PBD, which is also called the dynamic treatment effect.[9] In combination with understanding at which durations workers extend unemployment duration with longer PBD, this dynamic treatment effect can be seen as a very fine-grained version of the subsample analyses Glynn (2012) suggests, since dynamic treatment effects estimate the wage effect for subsamples defined by every realized duration. If there are wage effects at durations where there are no duration effects, direct effects must be present. Dynamic treatment effects also show whether wage effects are larger or smaller at durations where duration effects are present and can thereby provide suggestive evidence about the presence of indirect effects.

While the dynamic treatment effect is a CATE, it is more difficult to estimate than the CATEs that we use to document heterogeneity. The dynamic treatment effect conditions on duration in the treated state, which depends on PBD and is only observed for the treated group. To formalize these problems and describe our empirical strategy, we use counterfactual outcomes (Rubin 2005; Heckman 2008).[10] Let PBD_i denote the realized or counterfactual PBD duration of individual i, i.e. which UI regime (defined by its PBD) individual i is exposed to. Then d_{PBDi} denotes individual i's unemployment duration when eligible for PBD months of UI benefits and $Y_{PBD_i}(d)$ is the log reemployment wage of individual i in an UI system with PBD_i months of PBD as a function of unemployment duration d. We analyze two PBD durations, 12 and 18 months, so $Y_{12i}(d_{12i})$ and $Y_{18i}(d_{18i})$ are the outcomes in the two possible PBD regimes, one of which is observed. $Y_{18i}(d_{12i})$ and $Y_{12i}(d_{18i})$ are the (fundamentally unobservable) outcomes if individual i had chosen the unemployment duration of one regime, but received the wage response of the other PBD regime. This additional notation allows us to define the dynamic treatment effect as:

$$\Delta^{PBD \to Y}(d) = \mathbb{E}[Y_{18i}(d_{18i})|d_{18i} = d] - \mathbb{E}[Y_{12i}(d_{12i})|d_{18i} = d] \tag{6}$$

Unfortunately, dynamic treatment effects are not identified even with perfect randomiza-

[9] Unless another outcome is explicitly stated, we refer to the dynamic treatment effect on reemployment wages.

[10] See Heckman, Schmierer and Urzua (2010) for a discussion of the relation between counterfactual outcomes and the random coefficient model we use above. In our set up with only one unemployment spell per person, the two formalizations are equivalent without loss of generality.

tion (Ham and LaLonde 1996), because the second term of Equation (6) conditions on unemployment duration in the treated state, which is never observed along with wages in the untreated state. We only observe the difference between average treatment and control group outcomes conditional on unemployment duration, which we call the dynamic difference:

$$DD(d) = \mathbb{E}[Y_{18i}(d_{18i})|d_{18i} = d] - \mathbb{E}[Y_{12i}(d_{12i})|d_{12i} = d] \tag{7}$$

Equations 6 and 7 show that the dynamic difference is the dynamic treatment effect $\Delta^{PBD\to Y}(d)$ plus a dynamic selection effect, $\Delta^{PBD\to DS}(d)$:

$$\begin{aligned} DD(d) =& \mathbb{E}[Y_{18i}(d_{18i})|d_{18i} = d] - \mathbb{E}[Y_{12i}(d_{12i})|d_{18i} = d] + \\ & \mathbb{E}[Y_{12i}(d_{12i})|d_{18i} = d] - \mathbb{E}[Y_{12i}(d_{12i})|d_{12i} = d] \\ =& \Delta^{PBD\to Y}(d) + \underbrace{\mathbb{E}[Y_{12i}(d_{12i})|d_{18i} = d] - \mathbb{E}[Y_{12i}(d_{12i})|d_{12i} = d]}_{\text{Dynamic Selection Effect } \Delta^{PBD\to DS}(d)} \end{aligned} \tag{8}$$

Thereby, the dynamic difference $DD(d)$ at any given duration d may reflect a treatment effect ($\Delta^{PBD\to y}(d)$) or changes in the outcome (conditional on d) arising from individuals changing the timing of reemployment, i.e. effects of changes in dynamic selection ($\Delta^{PBD\to DS}(d)$). Thus, even perfectly randomizing treatment status only identifies dynamic treatment effects if either treatment does not affect duration or if duration does not affect the outcome. In the remainder of this section, we argue that the fixed effects decomposition allows us to estimate dynamic selection on observables and time-invariant characteristics as well as how dynamic selection changes with PBD and helps isolate dynamic treatment effects.

First, consider dynamic selection, which is frequently studied (e.g. Lancaster 1979; Van den Berg and Van Ours 1996; Alvarez, Borovičková and Shimer 2016; Ahn and Hamilton 2020; Mueller and Spinnewijn 2023; Zuchuat et al. 2023) to understand whether the steep drop of reemployment wages with duration is due to sorting (dynamic selection) or an effect of unemployment duration (duration dependence). Formally, individuals exiting at a specific unemployment duration d may differ from the overall population in terms of some outcome such as reemployment wages even in the absence of treatment:

$$DS(d) = \mathbb{E}[Y_{12i}(d_{12i})|d_{12i} = d] - \mathbb{E}[Y_{12i}(d)] \tag{9}$$

If the individuals who actually exit at some unemployment duration d differ from the population of interest even if we were able to assign the same unemployment duration d to the entire population, $DS(d)$ differs from zero, so that there is dynamic selection on Y_{12}. We only observe the first term, the average wage among those who actually exit at duration d, but not the second term, the average wage if everyone exited at duration d. Most analyses of dynamic selection therefore analyze pre-determined predictors of the outcome for which the second term is constant in d and can be estimated from the entire population. However, the more relevant question is whether workers differ in their (counterfactual) wages. Therefore, we study dynamic selection by examining pre-determined (and lagged) wage components. Plugging equation (5) (with counterfactual wage components defined in analogy to $Y_{PBD}(d)$) into equation (9) yields:

$$
\begin{aligned}
&\mathbb{E}[Y_{12i}(d_{12i})|d] - \mathbb{E}[Y_{12i}(d)] = \qquad (10)\\
&\mathbb{E}[x_{ijt}\beta + \theta_i + \psi_{12j} + \lambda_{12ij} + \eta_{12ijt}|d] - \mathbb{E}[x_{ijt}\beta + \theta_i + \psi_{12j}(d) + \lambda_{12ij}(d) + \eta_{12ijt}(d)] = \\
&\underbrace{\mathbb{E}[x_{ijt}\beta + \theta_i|d] - \mathbb{E}[x_{ijt}\beta + \theta_i]}_{\text{Pre-determined}} + \underbrace{\mathbb{E}[\psi_{18j} + \lambda_{18ij} + \eta_{18ijt}|d] - \mathbb{E}[\psi_{12j}(d) + \lambda_{12ij}(d) + \eta_{12ijt}(d)]}_{\text{Potentially affected by PBD or d}}
\end{aligned}
$$

The wage components in the first two terms of the last line are predetermined in our application, because the time-varying observables only include experience and year dummies and the worker fixed effect is defined as time-invariant. $\mathbb{E}[x_{ijt}\beta + \theta_i|d]$ and their grand mean, $\mathbb{E}[x_{ijt}\beta + \theta_i]$, can be estimated from the data, which allows us to study dynamic selection on these components, i.e. on observables and time-invariant wage determinants. These estimates allow us to test whether better workers sort into longer or shorter unemployment durations, which is of substantive interest and important to assess the bias in dynamic analyses.

We are mainly interested in changes in dynamic selection, because they prevent identification of the dynamic treatment effect as Equation (8) shows. A first useful insight is to see the dynamic selection effect as arising from heterogeneous duration effects. Changes in dynamic selection arise from changes in the order in which individuals leave unemployment, which requires heterogeneous duration effects. Thus, we can use tests for the presence of heterogeneous treatment effects to examine a necessary condition for changes in dynamic selection. In the absence of heterogeneous duration effects, the dynamic treatment effect is identified.

Decomposing wages even allows us to estimate parts of the dynamic selection effect:

$$\Delta^{PBD \to DS}(d) = \underbrace{\mathbb{E}[x_{ijt}\beta + \theta_i | d_{18i} = d] - \mathbb{E}[x_{ijt}\beta + \theta_i | d_{12i} = d]}_{\text{Identified: dynamic selection effect on observable and time-invariant characteristics}} \tag{11}$$
$$+ \underbrace{\mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt} | d_{18i} = d] - \mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt} | d_{12i} = d]}_{\text{Not Identified: dynamic selection effect on time-varying unobservables}}$$

Changes of the pre-determined wage components in the first line have to arise from changes in dynamic selection. By virtue of being pre-determined, we can estimate $\mathbb{E}[x_{ijt}\beta + \theta_i | d_{12i} = d]$ from the control group and $\mathbb{E}[x_{ijt}\beta + \theta_i | d_{18i} = d]$ from the treatment group. Thereby, we can estimate the part of the dynamic selection effect due to observable and time-invariant characteristics, which is informative about which kind of workers change unemployment duration in response to PBD. It also allows us to test whether the dynamic treatment effect is identified. If $\Delta^{PBD \to DS}(d)$ is zero for every d, we may be comparing different individuals, but the dynamic treatment effect is still identified, because they do not differ in their average outcomes.

Combining Equations (7) and (11) shows how our wage decomposition helps to isolate the dynamic treatment effect:

$$DD(d) = \underbrace{\mathbb{E}[\psi_{18j} + \lambda_{18ij} + \eta_{18ijt} | d_{12i} = d] - \mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt} | d_{12i} = d]}_{\text{Not Identified:} \Delta^{PBD \to y}(d)}$$
$$+ \underbrace{\mathbb{E}[x_{ijt}\beta + \theta_i | d_{18i} = d] - \mathbb{E}[x_{ijt}\beta + \theta_i | d_{12i} = d]}_{\text{Identified: dynamic selection effect from observable and time-invariant characteristics}} \tag{12}$$
$$+ \underbrace{\mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt} | d_{18i} = d] - \mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt} | d_{12i} = d]}_{\text{Not Identified: dynamic selection effect from time-varying unobservables}}$$

We can estimate the dynamic selection effect in the second line as Equation (11) shows, which enables us to study reemployment wages net of dynamic selection effects from observable and time-invariant characteristics. However, we cannot separate the dynamic treatment effect from the third line of Equation (12), because the wage components in this term are potentially affected by treatment, so that $\mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt} | d_{18i} = d]$ is unobservable. Thus, our approach isolates the dynamic treatment effect if covariates and individual fixed effects completely control for changes in dynamic selection. That is, it works if those who exit at duration d from the treatment group do not differ from the control group in terms of the sum of the firm fixed effect, job fixed effect and time-varying unobservables they would have obtained with short

PBD, i.e. if $\mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt}|d_{18i} = d] - \mathbb{E}[\psi_{12j} + \lambda_{12ij} + \eta_{12ijt}|d_{12i} = d] = 0$.[11] Under this assumption, the dynamic difference between these three wage components (i.e. firm fixed effects, job fixed effects and time-varying unobservables) is the dynamic treatment effect on the respective component and their sum is the dynamic treatment effect on wages, $\Delta^{PBD \to y}(d)$.

3 Data, Identification Strategy and Prior Results

We use the same identification strategy as Schmieder, von Wachter and Bender (2016) applied to smaller, but more detailed data. Specifically, we use the LIAB Mover Model (Institute for Employment Reseach 2012), an administrative linked employer-employee data set that covers more than 4.5 million individuals (with about 700,000 movers) employed at around 2 million firms from 1993 to 2008. We can track these individuals at least nine years after the beginning of their unemployment spell. The data contain demographic characteristics, such as age and educational background, as well as daily wages of employees covered by social security and benefits of UI recipients. However, the data does not include hourly wages. We therefore restrict our sample to workers coming from and moving to full-time employment to minimize differences in working hours. Like prior studies, we measure unemployment duration by benefit receipt and nonemployment by the time between two employment spells. The data include basic firm information such as industry and location, as well as measures constructed from the records of all employees, such as measures of aggregate employment, salary or tenure. Appendix B provides summary statistics for all samples we use.

Next to its richness and high precision, the data has the advantage of high worker mobility between firms (i.e. connectedness), which is crucial for wage decompositions (Andrews et al. 2008). The detail of our data come at the expense of a much smaller sample, which is less than 3 percent of the sample size of Schmieder, von Wachter and Bender (2016). Due to this smaller sample size, our estimates are often imprecise or require additional assumptions to increase power. Therefore, our substantive conclusions should be treated with caution. It would be useful to scrutinize them further using larger samples. Our methods and analyses

[11] By attempting to make people comparable in their wages, our approach is a control function approach. The main alternative is matching, which would attempt to match treated individuals to individuals with the same (unobserved) untreated unemployment duration instead of adjusting wages (Heckman and Vytlacil 2007). Matching seems useful if a good model of unemployment durations and covariates that predict them are available.

below provide the tools and a blue print to do so.

Closely following Schmieder, von Wachter and Bender (2016), identification is based on an extension of PBD from 12 to 18 months at the age threshold of 42 years. As Schmieder, von Wachter and Bender (2012) discuss, this discontinuity was in place in Germany between July 1987 and March 1997 with an additional two year phasing-out period (BGBl. 1987, p.1542). PBD ranges from six to 32 months and is determined by age and employment in the seven years before unemployment. To ensure eligibility for maximum PBD on both sides of the age cutoff, we examine individuals who had a minimum of three years of work experience in the seven years before unemployment. We restrict the sample to males working in West Germany immediately before and after unemployment to focus on the most homogeneous sample.

The validity of this identification strategy requires that individuals do not manipulate the running variable. This assumption was found to hold by Schmieder, von Wachter and Bender (2016). Results from examining discontinuities in the density of the running variable, using frequency plots as well as the formal test of Cattaneo, Jansson and Ma (2020) are reported in Appendix C. In addition, we estimate placebo effects on a wide range of predetermined variables. The results in Appendix Table C.1 show no evidence of threshold manipulation.[12] We find a that longer PBD decreases the probability of being in our sample by three percentage points and correspondingly increases the probability of job-to-job transitions by the same amount. Schmieder, von Wachter and Bender (2016) also find a small negative effect on the probability of reemployment, which they argue does not affect their results. We use our sample of all employment trajectories to further scrutinize this conjecture by examining RDD effects on the probabilities of taking each employment trajectory as well as tests whether the composition of workers in each trajectory changes with PBD. Appendix C summarizes these results and discusses why this source of sample selection is unlikely to affect our results.

We first replicate key analyses of Schmieder, von Wachter and Bender (2016). See Appendix C for more results and discussion. To ensure comparability, we use their RDD specification: A bandwidth of two years on each side of the threshold and linear trends in age at the beginning of unemployment.[13] Table 1 confirms that longer PBD increases time spent in un-

[12] As a robustness check, we include the variables that were close to significance or had sizable coefficients (number of prior jobs and days of benefit receipt as well as industry categories) in our RDD analysis. Controlling for these variables has no meaningful effect on our estimates, as Appendix Table C.4 shows.

[13] In addition to comparability to prior results, conventional RDD estimates also make our estimated dynamic

Table 1
Effect of PBD on Unemployment and Nonemployment Duration and Reemployment Wages

	Unemployment Duration (1)	Nonemployment Duration (2)	Log Wage (3)
RD estimate	0.5631	0.0682	-0.0283
p-value	[.0003]	[.9025]	[.0335]

Unemployment duration corresponds to benefit duration, whereas nonemploymet duration measures the time between two jobs. RDD estimates using a bandwidth of 2 years and linear functions of age on either side of the cutoff. Appendix C reports results from alternative specifications that condition on covariates or apply bias corrections. p-values in brackets. N: 13,567.

employment (by 0.56 months) and nonemployment (by 0.07 months). Longer PBD decreases reemployment wages by 0.028 log points. Contrary to the effects on wages and unemployment duration, the effect on nonemployment duration is not significant and becomes substantially larger in specifications that condition on covariates (0.25 months) or apply the bias correction of Calonico, Cattaneo and Titiunik (2014) (1.1 months). Despite the differences in sample composition and size, the results are similar to those of Schmieder, von Wachter and Bender (2016). They report larger effects on unemployment (1.77 months) and non-employment duration (0.95 months), but smaller wage effects (0.008 log points). We cannot reject that our effects are equal to their point estimates for any of our results. Finding slightly smaller duration responses and larger wage responses is not surprising for the sample with higher labor force attachment we study.

4 Duration Effects

4.1 Understanding Heterogeneity in the Effect of PBD on Duration

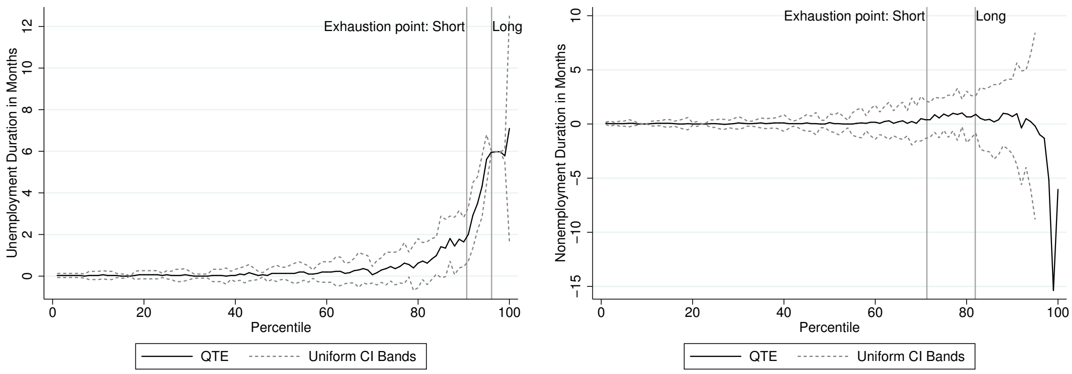
Recent studies have consistently found small duration effects, ranging from being indistinguishable from zero (Lalive 2007) to almost a month (Schmieder, von Wachter and Bender 2016). See Nekoei and Weber (2017, online Appendix B.2) for an overview. As we argue above,

and quantile treatment effects "add up" to our point estimates. In studies with less of a methodological focus, one may prefer specifications that condition on the pre-determined covariates for which we find differences at the threshold or apply a bias correction (Calonico, Cattaneo and Titiunik 2014). We report these results in Appendix Table C.4. However, we focus on conventional, unconditional results to simplify exposition, since these additional results are very similar to the ones we report. As one would expect, bias corrections decrease the precision of our estimates, while conditioning on covariates increases precision, but neither results in meaningful changes in the point estimates unless pointed out otherwise.

making the heterogeneity underlying local effects visible is important to understand whose unemployment durations change, whether PBD relaxes search constraints and whether and how dynamic selection changes. We first extend analyses of the effect of PBD on average durations to its effect on the distribution of unemployment duration by estimating QTEs (Firpo 2007).[14] Figure 2a plots the QTE on unemployment duration, i.e. the horizontal distance between the cumulative distributions of unemployment duration in the treatment and control group. The quantile treatment effect is flat and not significantly different from zero until it slowly starts to rise between the 70th and 80th percentile. At the 87th percentile, it becomes significantly different from zero and quickly rises to a difference of 5-6 months. This result is similar to Qu, Yoon and Perron (2024), who find PBD in Austria to mainly affect the distribution of unemployment durations above its 90th percentile. The 87th percentile corresponds to an unemployment duration of about 10.5 months for those with short PBD, so this result indicates that treatment does not change the frequency of unemployment spells that are meaningfully shorter than PBD. In other words, duration effects are driven by changes shortly before and after the first exhaustion point, before which the CDFs do not change.

FIGURE 2
QUANTILE TREATMENT EFFECTS ON DURATION

(A) UNEMPLOYMENT DURATION (B) NONEMPLOYMENT DURATION



The QTE is the horizontal distance between the cumulative distributions of the outcome in the treatment and control group. Vertical lines mark benefit exhaustion at 12 and 18 months for the control and treatment group, respectively. The confidence interval level is 90 percent.

[14] Throughout this paper, we estimate QTEs using the entire sample in the RDD analysis window. Estimating the entire quantile function at the discontinuity (Qu, Yoon and Perron 2024) would be preferable, but requires a much larger sample. Our approach requires the stronger assumption that treatment is not only exogenous at the discontinuity, but in the entire bandwidth after purging the running variable. We only purge the running variable for wage outcomes, because we find that doing so makes no difference or only adds noise for non-wage outcomes. Results of the respective other approach are either reported in the supplement or available upon request. Any relevant differences are reported in the text.

Figure 2b repeats the same analysis for nonemployment duration. The distribution of nonemployment durations remains unaffected until the 70th percentile, which corresponds to a duration of slightly more than 11 months. The difference reaches its maximum of about one months around the 80th percentile, i.e. around almost 16 months of nonemployment for those with 12 months of PBD. Above the 80th percentile, the distributions seem more similar again. Since both quantiles and effects are more dispersed for nonemployment, the confidence bands are too wide for us to even reject the null hypothesis of no effect on nonemployment duration. Despite the low power, a key result that remains is that before the exhaustion points, we do not see any changes and confidence bands remain tight.

For both un- and nonemployment, our results imply that spells of 12 or more months became more frequent. Concluding that long spells became longer requires further assumptions (Bedoya et al. 2017). Assuming that there are no negative individual treatment effects implies that the QTE is globally positive, which is consistent with Figure 2a. Under this assumption, we can rule out any duration effects until the QTE starts to increase from zero, somewhere between the 60th and 80th percentile.[15] Consequently, we cannot reject the null hypothesis of no duration effects on either un- or nonemployment until the exhaustion points or shortly before.

The situation changes dramatically shortly before the first exhaustion point at 12 months, beyond which the distribution of durations shifts to the right slightly for nonemployment and substantially for unemployment. The QTE is roughly constant at around 6 months for unemployment and one month for nonemployment. For unemployment, this means that for each spell lasting 12+x months in the control group, we observe a spell in the treatment group that lasts 18+x months. Thus, roughly 10-15 percent of workers in our sample exit close to the exhaustion point regardless of PBD duration. For nonemployment, we see a similar pattern, but the distribution of durations only shifts to the right by one month between 12 and 18 months. Our much less pronounced results for nonemployment duration confirm that exhaustion effects are driven by program exit rather than reemployment (Fitzenberger 2004; Card, Chetty and Weber 2007*b*). Following their arguments, we focus on nonemployment below.

[15] To see this, consider the person with the shortest duration in the untreated state, say d'_{12}, who changes duration. This person moves to a higher duration, which c.p. makes the QTE slope upward. This upward slope could be offset if someone else took their place and exited after a duration of d'_{12} in the treated state. Yet such a person cannot exist: Workers with $d_{12} < d'_{12}$ are ruled out by the fact that d'_{12} is the shortest duration at which there is a treatment effect. And workers with $d_{12} > d'_{12}$ are ruled out by ruling out negative treatment effects.

Our results are consistent with a sixth of those extending unemployment staying without employment for an additional 6 months, while the remaining five sixth claim six additional months of benefits without extending their nonemployment duration. Contrary to the effect on unmeployment duration, the nonemployment effect is smaller than the distance between the exhaustion points and there is still mass beyond 18 months, leaving room for multiple explanations, such as all nonemployment spells between 12 or 18 months being extended by a month. Yet the peaks in the hazard rates just move with the exhaustion point, so that the QTE drops back to zero at the second exhaustion point. This pattern suggests that the probability of exit only changed at the exhaustion point, i.e. that the effect is mainly or entirely due to a small fraction of workers adding 6 months of nonemployment. Such an effect is also better aligned with the fact that workers seem to take 6 more months of benefits, but we cannot rule out smaller duration effects.

That there is no meaningful effect on exit rates between the exhaustion points, so that effects mainly seem to consist of taking the full additional 6 months of benefits, suggests that the PBD extension did not have an effect on search effort for most workers. This finding is well aligned with the recent literature on job search, which finds that search effort moves with benefit reduction points (DellaVigna et al. 2017) and that effort peaks arround exhaustion points, but is flat before exhaustion and drops after it (Marinescu and Skandalis 2021; DellaVigna et al. 2022). Overall, those who exhaust benefits are better off due to longer UI payments, but their search constraints were not relaxed and behavioral responses to the PBD extension appear to be limited to a very small fraction of workers.

Our evidence that search constraints did not change much helps to understand why RDD estimates of duration effects vary across studies. In the extreme case we find, where duration effects are only driven by those who actually exhaust benefits, the effect of PBD on duration estimates the product of the probability of exhausting benefits and the average duration effect among those who exhaust benefits. Thus, the estimated effect will be larger (c.p.) if more people exhaust benefits, which seems more likely when PBD for the control group is shorter. It will also be larger (c.p.) if those who exhaust benefits take more additional time until reemployment, which likely makes duration effects larger for longer PBD extensions. The estimated effect will therefore depend on the length of PBD as well as by how much it is extended. That we find longer PBD to relax only few search constraints thus provides further empirical evidence in favor of the conjecture by Nekoei and Weber (2017) that the relevance of search constraints is an important determinant of the differences in duration effects between studies.

Importantly, that QTEs for both un- and nonemployment duration are precisely estimated to be zero before the first exhaustion point implies that there are no effects of PBD on duration at short durations. It is unclear whether this finding generalizes, since search constraints may be more relevant with shorter PBD, which may lead to duration responses before exhaustion and hence more dispersed duration effects. However, that we find clear evidence of no duration effects before benefit exhaustion in our setting is of crucial importance, since it rules out dynamic selection effects and indirect effects before the exhaustion effects start.

4.2 Understanding Heterogeneity in Reemployment Timing: Dynamic Selection

To corroborate our finding that there are no duration effects before the exhaustion points (which identifies part of the dynamic treatment effect), we examine dynamic selection effects. Lagged wages and our wage decomposition allow us to go beyond prior analyses of pre-determined observables by studying dynamic selection on wages directly. The worker fixed effect enables us to include unobserved time-invariant characteristics in these analyses. Before turning to dynamic selection effects, we first examine whether dynamic selection is related to wages, i.e. whether and how worker quality varies with unemployment duration.

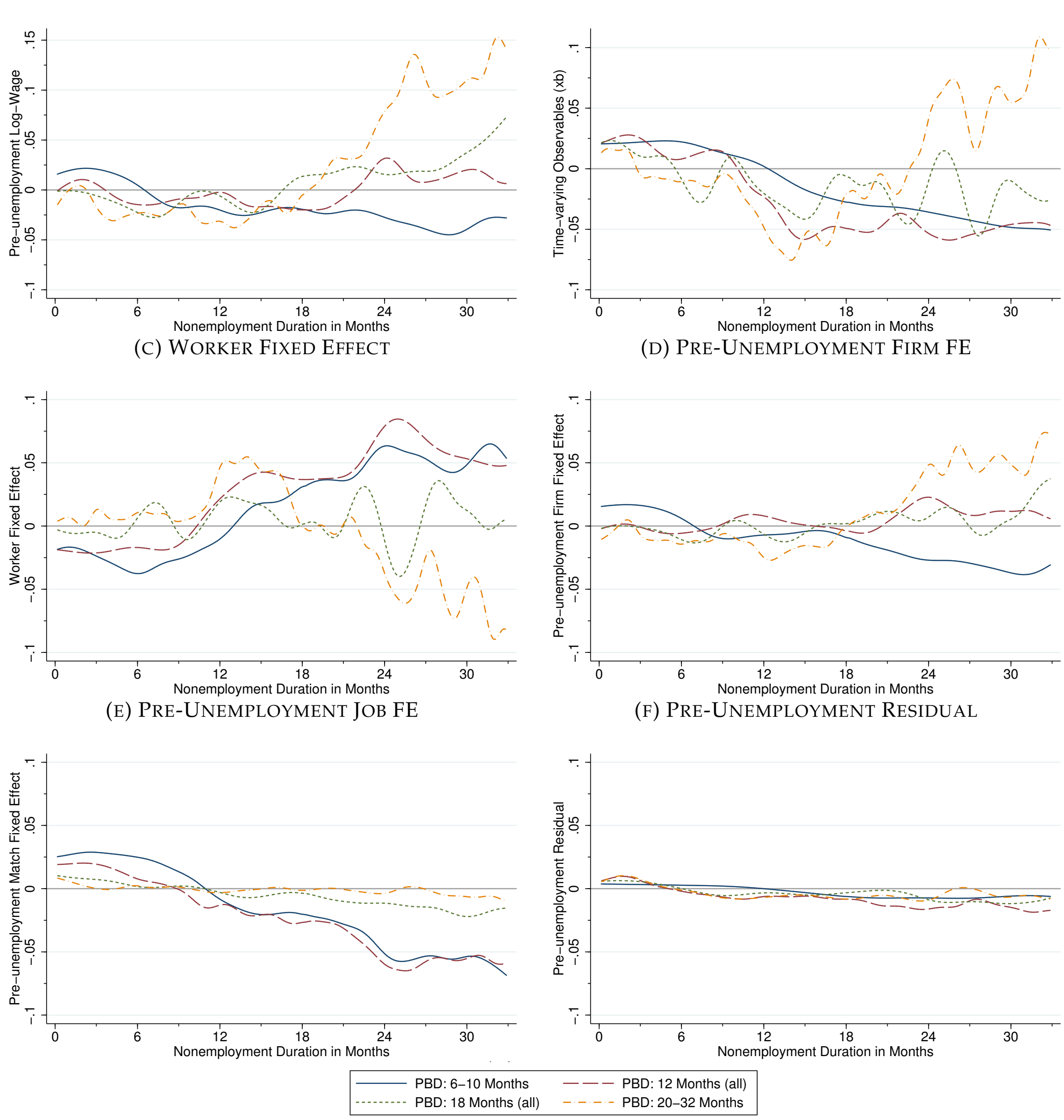
These analyses do not require random variation in PBD. Therefore, we examine a large sample of four groups of workers who encompass all PBD categories of the UI system instead of our RDD sample, which makes our results more precise and more informative about the overall population.[16] Specifically, our samples include everyone in our data who can be cleanly determined to be eligible for 12 and 18 months of PBD. Our treatment and control group are subsamples of these populations. In addition, we examine workers with shorter (6-10 months) and longer (20-32 months) PBD. The main difference between these groups is their age, as maximum PBD was primarily determined by age. See Appendix Table B.1 for summary statistics. Figure 3 plots the expectation of wages and wage components conditional on nonemployment duration. Following Equation (9), we plot deviations of the conditional

[16]Since we are not interested in RDD effects, we do not adjust wages and components for age in these analyses.

expectation from the unconditional expectation, but this normalization only shifts the levels of the lines while we are interested in their slopes.[17]

FIGURE 3
DYNAMIC SELECTION IN LARGE PBD GROUPS

(A) PRE-UNEMPLOYMENT WAGES (B) TIME-VARYING OBSERVABLES: $X\hat{\beta}$
(C) WORKER FIXED EFFECT (D) PRE-UNEMPLOYMENT FIRM FE
(E) PRE-UNEMPLOYMENT JOB FE (F) PRE-UNEMPLOYMENT RESIDUAL



The line for each group plots the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Supplement Figures S.2-S.5 provide confidence bands.

Panel 3a shows pre-unemployment wages as an aggregated measure of dynamic selection. With the exception of the group with short PBD, those staying in unemployment longer are

[17] To test hypotheses about the direction of dynamic selection Supplement Figures S.2-S.5 plot each function separately with uniform confidence bands. When interpreting these graphs, one should keep in mind that dynamic selection is defined relative to all workers who return to employment rather than relative to all nonemployed workers or the pool or nonemployed workers at a given time.

positively selected in terms of their previous wages. The wage differences over the unemployment spell are large, up to five percent before exhaustion and even larger for the two groups with longest PBD at longer durations. These differences are larger than or similar to estimates of wage loss from unemployment typically found in the literature. Substantial variation stems from the predetermined wage components of the post-unemployment job in Panel 3b ($x\hat{\beta}$) and 3c (worker fixed effect), which are both measures of worker quality. They make post-unemployment wages vary with duration in the absence of any effects of unemployment. Therefore, dynamic selection can introduce biases larger than common effect sizes in duration models and other dynamic analyses. The wage components of the pre-unemployment spell in Panels 3d to 3f are less volatile, but still contribute meaningfully to dynamic selection on previous wages. This importance of lagged wage components may be taken as evidence of backward-looking behavior in job search, for example due to reference dependence (DellaVigna et al. 2017) or job ladders (Haltiwanger, Hyatt and McEntarfer 2018).

Our measures increase with duration in several instances, i.e. the quality of reemployed workers often increases with nonemployment duration. This increase is significant in several cases. The increasing pattern is most noteworthy for the worker fixed effect. Pre-unemployment wages also increase at long durations for 3 of our 4 groups, partly due to increases in the pre-unemployment firm fixed effect and $X\beta$. Thus, both positive and negative dynamic selection is present, which shows that the common question in the literature whether dynamic selection is positive or negative is an oversimplifying dichotomy. Such non-monotonic dynamic selection can create spurious effects in either direction, so many common analyses rely on the assumption of negative or monotonic dynamic selection, and should use analyses similar to ours to test these assumptions in their sample.

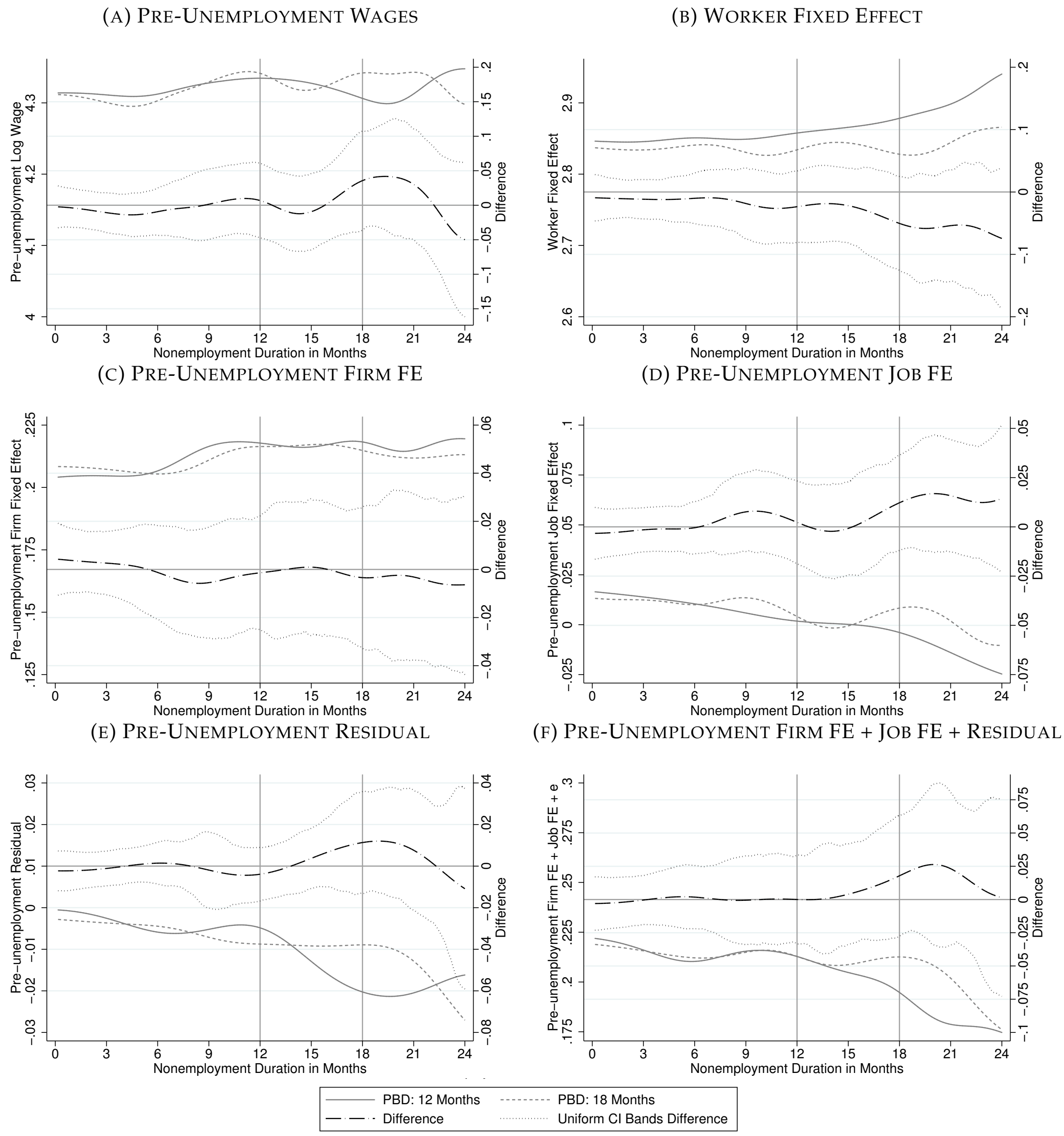
The pronounced and non-monotonic patterns of dynamic selection on wage components raise the question whether predetermined observable characteristics can help to detect or even explain them. Appendix Figure D.1 plots the analog of Figure 3 for years of education, highest degree obtained, firm size, tenure, days of benefit receipt before unemployment and previous industry categories. Supplement Figures S.6-S.9 plot the lines with uniform CIs for hypotheses testing. These observables clearly detect the problem of dynamic selection and several variables (tenure, years, firm size) indicate the presence of positive dynamic selection. However, none of these observables are able to detect the non-monotonic patterns we see for wage-based measures above. Therefore, observables are unlikely to explain the variation in wages we document, so that analyzing wage components is necessary to detect the full extent of the problem and provides additional information about dynamic selection.

Analyzing the causes of this non-monotonic pattern of dynamic selection is beyond the scope of this paper. Yet our results suggest that non-monotonic dynamics are the result of a combination of multiple mechanisms with different directions. For example, the U-shaped patterns we find may arise from the combination of better workers selecting into shorter unemployment durations (causing a decline as in e.g. Zuchuat et al. 2023) and increasingly positive selection into returning to employment at all (causing an increase). Heterogeneity also seems to play a role in that pooling groups with heterogeneous dynamics contributes to these patterns. Dynamic selection clearly differs between those with 12 and 18 months of PBD, but we show below that these differences do not stem from PBD itself. Hence they have to arise from other differences, such as demographics or industry composition. The last 4 panels of Appendix Figure D.1 show that industry composition changes substantially with duration. Workers from different industries likely not only differ in their nonemployment durations, but also in their wages and wage components, which makes our graphs an average of heterogeneous duration profiles. Analyses of dynamic selection and its causes therefore likely need to zoom in on more homogeneous samples to isolate specific mechanisms.

For analyses of dynamic treatment effects, it is more important to know whether dynamic selection changes with PBD. Above, we show that individuals only change duration after the first exhaustion point, which implies that dynamic selection does not change before the first exhaustion point, because changes in dynamic selection stem from heterogeneous duration effects. To corroborate the hypothesis that ΔDS does not bias the dynamic treatment effect before the first exhaustion point, Figure 4 plots the same conditional expectations as Figure 3 for our treatment and control group, as well as the dynamic selection effect defined by equation (11) with confidence bands.

Before the exhaustion points, we cannot reject the hypothesis that dynamic selection does not change for any of our wage measures and the differences in the conditional expectations are very close to zero. We observe small and insignificant changes in the worker fixed effect, which

FIGURE 4
DYNAMIC SELECTION EFFECTS IN RDD SAMPLE



Conditional means are estimated nonparametrically by a local linear regression. Differences are the point-wise differences between the lines, which are plotted with uniform 90 percent confidence bands. All estimates condition on age. Vertical lines mark PBD exhaustion at 12 and 18 months for the control and treatment group, respectively.

we purge from our analyses below. The malleable components stay close to zero and small deviations cancel in the aggregate, so that their sum (Figure 4f) is remarkably flat and close to 0. We provide further evidence that there are no duration effects before benefit exhaustion by examining how PBD affects the duration profile of pre-determined characteristics. Supplement figure S.10 plots graphs analogous to Figure 4 for years of education, degree obtained, non-citizen, as well as firm size, tenure and days of benefit receipt before unemployment.[18] We do

[18]Such analyses can also show how the sample composition changes at durations where duration effects exist.

not find any meaningful effects on any other measures of dynamic selection in the rich LIAB data. Overall, we are only able to reject the null hypothesis of no change in dynamic selection for previous tenure. Given the large number of variables we test, this result is likely spurious. Thus, these analyses add further evidence to the hypothesis that there is no dynamic selection effect, i.e. that $\Delta DS(d) = 0$, before individuals approach the exhaustion points. Thereby, they imply that before the first exhaustion point, the dynamic difference in equation (8) estimates the dynamic treatment effect defined in equation (6).

5 Wage Effects

Estimates of the effect of PBD on wages vary widely, including in sign. Understanding whose wages change by how much in response to PBD is important to understand the consequences of PBD extensions. In addition, learning about the channels that drive wage effects is not only crucial to reconcile diverging findings, but also key to understanding why and how PBD and unemployment affect wages. We first examine what effects on wage components can tell us about potential causes in section 5.1 and then document the nature of treatment effect heterogeneity in section 5.2. In Section 5.3 we analyze direct and indirect wage effects to understand and reconcile estimated wage effects.

5.1 Effects of PBD on Wage Components

Table 2 presents RDD-estimates of the effect of PBD on the (estimated) firm fixed effect, job fixed effect and the residual.[19] Figure D.2 provides RDD plots. Results in levels and changes are very similar, which adds to their credibility by showing that there is no selection on lagged wage components. In line with the small, but insignificant effect on the worker fixed effect we document above, the effect on the sum of all malleable wage components in column 4 is slightly smaller, but not significantly different from the effect on wages in Table 1 (0.0283). The effects on the job fixed effect and time-varying unobservables are very small. Thus, neither factors remaining constant over this employment spell nor time-varying unobservables seem

Thereby, they can also provide evidence on who receives benefits longer and who stays without employment longer. Unfortunately, our samples, especially the control group become very small at longer durations, so that none of these results are significant.

[19]Note that contrary to mean differences, RDD effects are not additively separable. Thus, estimating effects on wage components is similar to, but not equivalent to decomposing the RDD effect on wages.

to be the source of changes in reemployment wages. The overall effect mainly stems from the firm fixed effect. These results suggest that the wage loss from the PBD extension stems from workers with longer PBD working for firms that pay lower wages to all employees.

TABLE 2
EFFECT OF PBD ON REEMPLOYMENT WAGE COMPONENTS

Outcome:	Firm FE (1)	Job FE (2)	Residual (3)	Sum (4)
Levels	-0.0148	-0.0050	-0.0025	-0.0223
p-value	[.0589]	[.3658]	[.5629]	[.0214]
Changes	-0.0158	-0.0072	-0.0014	-0.0244
p-value	[.0689]	[.3767]	[.8350]	[.0574]

The first two lines use wage components of the post-unemployment job from Equation (5) as outcomes in the RDD. The third and fourth line subtracts the pre-unemployment component from the respective wage component. Column 4 uses the sum of the firm FE, job FE and the residual as the outcome. N: 13,567

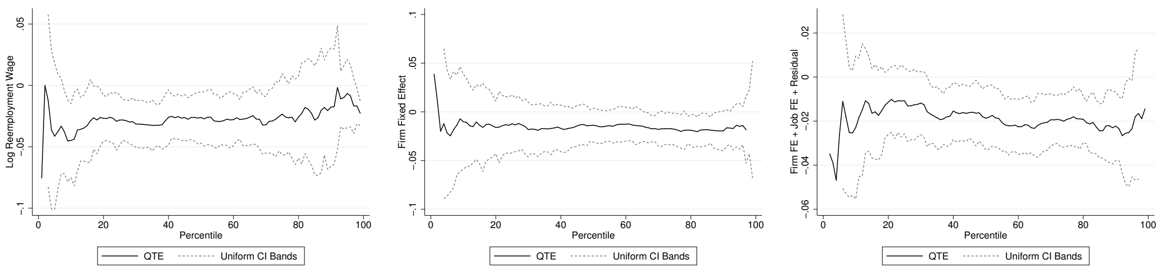
Substantively, this finding extends recent evidence on the importance of firms in wage dynamics (Haltiwanger, Hyatt and McEntarfer 2018) and dispersion (Card, Heining and Kline 2013) by showing that firms also play a key role for wage loss from PBD extensions. It is in line with prior studies that find lower wages after unemployment to be driven by moving to firms that are worse on some dimension (Nekoei and Weber 2017; Schmieder, Von Wachter and Heining 2023). It also adds to prior evidence that job loss causes long-lasting wage losses. As Nekoei and Weber (2017) argue, an effect on firm rather than job quality favors explanations based on worker sorting and speaks against bargaining as a cause of wage loss. An effect that operates only through the wage component common to all workers at the firm also makes it difficult to explain wage loss from unemployment with individual characteristics such as skill depreciation or signalling.

5.2 Heterogeneity in the Effect of PBD on Wages

We next examine the presence of positive wage effects and heterogeneity in order to understand what quasi-experiments can tell us about average effects, who is particularly affected and whether differences between studies stem from differences in demographics. Figure 5 shows QTEs on wages, the firm fixed effect and the sum of all malleable wage components. QTEs for the job effect and the residual are uninformative and hence only reported as Supplement Figure S.11. We can reject the hypothesis that the distribution does not change in all three cases. The QTEs do not provide evidence of positive wage effects, since they are all globally negative. However, we cannot reject the hypothesis that QTEs are positive at some point and even a globally negative QTE does not rule out some positive effects. Yet the QTEs are also remarkably flat as one would expect with constant treatment effects. Overall, these unconditional tests do not yield any evidence of either heterogeneous or positive wage effects.

FIGURE 5
QUANTILE TREATMENT EFFECTS ON WAGES AND COMPONENTS

(A) LOG REEMPLOYMENT WAGE (B) FIRM FIXED EFFECT (C) FIRM FE + JOB FE + RESIDUAL



The QTE is the horizontal distance between the cumulative distributions of the outcome in the treatment and control group. All outcomes are age adjusted to purge the effect of the running variable. The confidence interval level is 90 percent.

To further probe for the presence of heterogeneity and positive treatment effects, we estimate CATEs. To do so, we split our sample according to 25 binary variables and separately apply our RDD estimator to each subsample. Appendix Tables D.1-D.3 report treatment effects for subsamples defined by education categories, citizenship, marital status, occupation, industry and blue collar jobs. We also examine whether treatment effects vary with continuous variables (pre-unemployment wages, firm and individual fixed effect, residual, experience, tenure, firm size, number of jobs, cumulative days of benefit receipt, number of spells of benefit receipt, total time spent in nonemployment) by estimating treatment effects for samples split at the median of these variables.[20]

For each outcome, the first and third columns report the point estimates for the two subsamples defined by the variable in the first column, columns 2 and 4 report the corresponding p-values of a test whether the coefficient differs from 0. Rejecting the hypothesis that the effect is smaller than 0 for some subsample would establish the presence of positive wage effects (but only negative CATEs do not rule out positive effects within subsamples, since there may

[20]It would be desirable to test whether treatment effects vary with the continuous variables, but contrary to an RCT, doing so is not straightforward in an RDD. While splitting the sample at the median may not be ideal to examine positive treatment effects, it maximizes our power to detect heterogeneity in the analyses below. In a similar vein, it would be useful to conduct joint tests of significance, but it is not clear how to do that in an RDD.

be heterogeneity around the CATE). Yet despite testing this hypothesis in 50 subsamples for 5 outcomes each, we cannot reject the hypothesis that the effect is not positive for a single subsample. 21 of the 250 point estimates are positive, but none of them is significant even without correcting for multiple hypothesis testing. In addition, 19 of the 21 positive point estimates are from wage components on which we do not find any effect, i.e. where positive effects are more likely by chance. Thus, we find no evidence of positive treatment effects at all in our sample.

CATEs can also help to establish the presence of treatment effect heterogeneity: Rejecting that average effects are the same for the two subsamples implies that treatment effects are heterogeneous. The last column for each outcome reports p-values of a permutation test whether the coefficients in the two subsamples are the same. We can only reject that the CATEs are identical for 8 out of 125 coefficient pairs at the 10 percent level. We find significantly higher wage loss for workers with less than median experience for three outcomes (job fixed effect, residual and the sum of malleable wage components) and for blue collar workers for two outcomes (job fixed effects and residual). This finding may indicate the presence of some heterogeneity and suggests that PBD extensions may harm those who face higher hurdles finding or maintaining jobs more. However, we find fewer significant differences than one would expect to find by chance and most differences are remarkably close to zero.

Overall, we do not find any evidence of either positive or heterogeneous wage effects despite extensive testing. These findings give ground for cautious optimism that these two key obstacles to the interpretation of local effects are unlikely to affect estimates of the effects of PBD on wages. Therefore, local effects on wages and wage components provide useful information about the consequences of PBD extensions and can thus be used to guide policy. Whether this finding extends to other studies hinges on the presence of positive treatment effects and heterogeneity, which can be analyzed using the same tools. Our finding that treatment effects do not meaningfully vary with demographics suggests that differences in average effects between studies do not stem from differences in the populations these studies analyze. Unless substantial heterogeneity arises from demographics we do not observe, estimated wage effects differ between studies due to differences in how workers respond to changes in PBD, i.e. due to differences in the effect of PBD on duration or how PBD affects wages. That is, our results point to diverging estimates arising from differences in the economic environment or

UI policies, rather than differences between the populations studied.

5.3 What Does the Data Say About Mechanisms?

As discussed in section 2, a key question of the recent literature is whether PBD affects wages directly. Schmieder, von Wachter and Bender (2016) argue that there is no direct effect, so the effect of PBD on wages is entirely due to longer nonemployment durations. Our results above cast doubt on this conclusion: If wage effects were solely due to duration effects, the heterogeneous duration effects we find should translate into heterogeneous wage effects, which does not appear to be the case. In addition, wage losses that operate through the firm fixed effect are not straightforward to reconcile with the wage loss stemming from individual-specific factors such as unemployment duration unless there is strong sorting of workers to firms. Hence, we next revisit the question whether there is a direct effect of PBD on wages and examine what we can learn about the effect of duration on wages in the presence of heterogeneity.

To do so, we analyze subsamples in which the effect on the mediator, duration, is particularly pronounced or absent. This strategy appears particularly promising in our case, since we provide clear evidence that there are no duration effects at short unemployment durations. Therefore, we split the sample at median unemployment duration[21] and separately estimate effects on wages and wage components for these two subsamples.

TABLE 3
ANALYZING DIRECT EFFECTS OF PBD ON WAGES BY SPLITTING THE SAMPLE

Outcome: Sample:	Log Wages (1)	Firm FE (2)	Job FE (3)	Residual (4)	Sum (5)
$\leq$ median	-0.0219	-0.0138	-0.0125	-0.0008	-0.0271
unemp. duration	[.2142]	[.1658]	[.0661]	[.8875]	[.0286]
$>$ median	-0.0337	-0.0154	0.0024	-0.0040	-0.0170
unemp. duration	[.0864]	[.1995]	[.7823]	[.5183]	[.2472]

Estimated using wage components from Equation (5) as outcomes in the RDD separately for the sample of observations with unemployment duration below and above median unemployment duration. p-values in brackets. N: 6,808 (below median), 6,759 (above median).

Table 3 reports estimated RDD effects on wages and wage components for the workers

[21]Such analyses are only valid if the variable based on which the sample is split is exogenous. Even though unemployment duration is affected by PBD, our results above show that median unemployment duration is not affected by PBD, so that the samples remain comparable across treatment status. In other cases, one would need to rely on exogenous variables that predict the size of effects on the mediator as Glynn (2012) discusses. See Nekoei and Weber (2017) p. 549 for further discussion and a more ambitious version of our argument. We use unemployment rather than nonemployment duration to define the samples, because the result that there are no effects below the median is stronger for unemployment duration.

with below and above median unemployment duration. We find clear evidence of wage effects below median unemployment duration, which establishes the presence of direct effects. In the presence of direct effects, identification of the effect of duration on wages breaks down even in the absence of heterogeneity. We find wage effects of similar magnitude for the sample of workers with duration effects, so we cannot rule out the presence of an effect of unemployment duration on wages. Yet the fact that effect sizes are similar in the two samples suggests that effects of unemployment duration on wages are small and do not contribute substantially to the wage loss from unemployment in our case.

As we argue in section 2.3 dynamic treatment effects generalize this strategy of subsample analyses and can yield further insights into direct and indirect effects. For each malleable wage component (as well as their sum) Figure 6 shows its expectation conditional on nonemployment duration separately for those with short and long PBD. The dynamics of these job components is of interest in itself, but studied in detail and with more precision in Schmieder, Von Wachter and Heining (2023). In line with their results, the steep wage drop over the UI spell remains net of dynamic selection. It is driven by the firm fixed effect. The job fixed effect and the residual also decline with duration, but contribute much less to the overall decline.

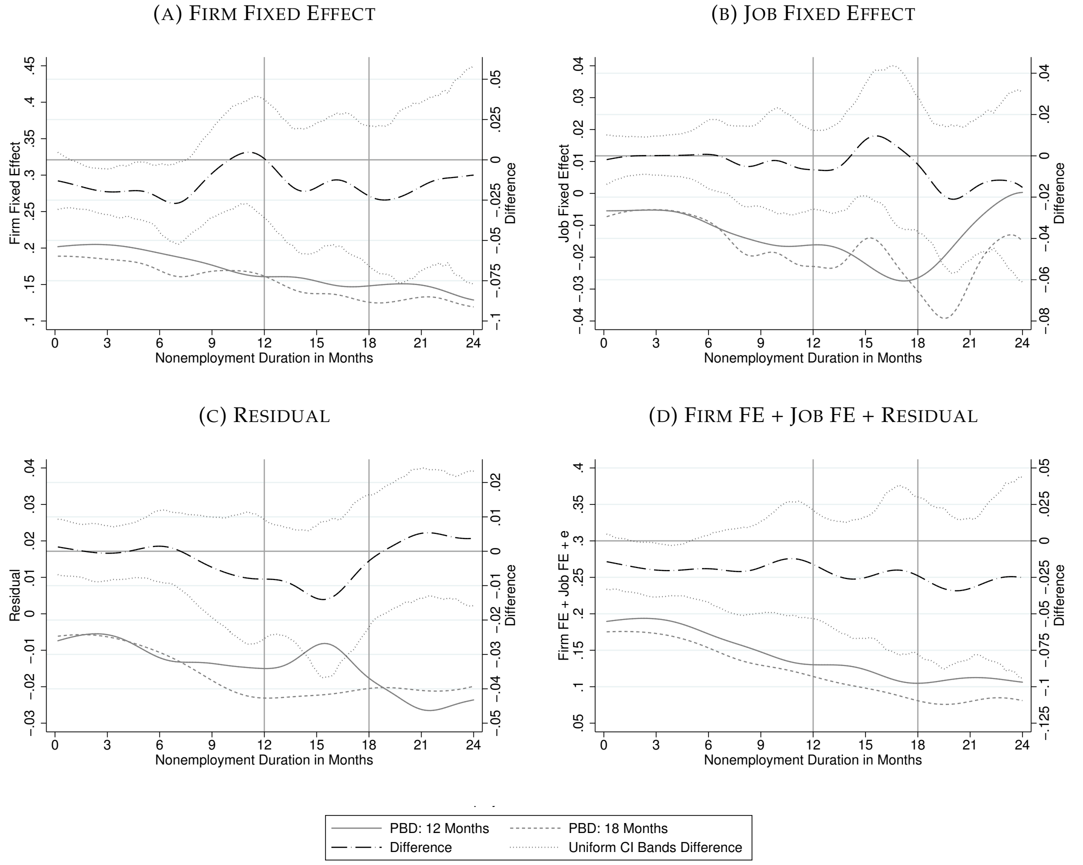
We are mainly interested in the difference between the paths of those with long and short PBD, which is plotted with uniform CIs. As we argue above, this dynamic difference estimates the dynamic treatment effect under the assumption that there is no dynamic selection on the respective wage component itself.[22] Before the first exhaustion point, we find no duration effects, which implies the validity of this assumption. We see some variation at larger durations, but overall the dynamic treatment effects are remarkably flat. We cannot reject a constant dynamic treatment effect for any wage component, which is in line with the evidence of constant treatment effects above.

The dynamic treatment effects provide a more nuanced picture of who drives wage effects. Thereby, they provide clearer evidence of a direct effect and are more informative about indirect effects than the sample split above. We can reject that the dynamic treatment effect is positive or zero for the firm fixed effect and the sum of the three malleable wage components, which establishes that PBD affects wages directly. Negative effects exist at durations long be-

[22] Appendix Figure D.3 repeats the analysis using differenced wage components showing that even the weaker assumption of not sorting into nonemployment duration on the difference in wage components is sufficient.

FIGURE 6

DYNAMICS OF WAGE COMPONENTS OVER NONEMPLOYMENT SPELL



Conditional means are estimated nonparametrically by a local linear regression. Differences are the point-wise differences between the lines, which are plotted with uniform 90 percent confidence bands. All estimates are conditional on age. Results without the age adjustment are available upon request. Vertical lines mark PBD exhaustion at 12 and 18 months for the control and treatment group, respectively. Appendix Figure D.3 repeats the analysis using differenced wage components. Results for ALG are in Supplement Figure S.12.

fore the first exhaustion point, showing that our results above do not stem from the way we split the sample. The evidence on whether there is also an effect of duration on wages is less clear. The dynamic treatment effect can provide evidence on indirect effects by examining whether it changes at durations with more pronounced duration effects. In our application, duration effects are very concentrated, so this advantage of the dynamic treatment effect is less useful. We do not see duration effects at durations as short as the ones Nekoei and Weber (2017) study, so we clearly have nothing to say about indirect effects at short durations. At longer durations, the dynamic treatment effects stay roughly constant or increase slightly, which speaks for no or slightly positive effects of duration on wages. That we find no meaningful treatment effect heterogeneity and no substantial increase in effect size for long durations speaks against a meaningful effect of duration on wages. However, at these longer durations, our results are noisy and may be affected by changes in dynamic selection, so they are far from conclusive.

Overall, our results point toward a roughly constant and direct effect of PBD on wages. A negative direct effect is puzzling, as economic theory suggests that direct effects of PBD on wages should be positive due to higher reservation wages or more selectivity. A simple explanation may be that longer PBD allows workers to find jobs with non-wage attributes that more than offset the wage loss. To provide some evidence on the plausibility of this potential explanation, Appendix Table D.4 reports results from using 7 non-wage outcomes (tenure and firm size of the post-unemployment job as well as changes in firm size, firm, industry category, occupation category and location (state) of the employer) as outcomes in our RDD. In line with Nekoei and Weber (2017), we do not find any significant or meaningful effects. However, this overall effect combines direct effects with indirect effects that may arise from duration effects at longer nonemployment durations. In the aggregate, a negative effect of nonemployment duration on non-wage attributes may well offset potential positive direct effects at shorter durations that we aim to investigate here.

Since we show that there are no duration effects at short nonemployment durations (and hence neither indirect effects nor changes in sample composition), the dynamic differences of our non-wage outcomes in Appendix Figure D.4 isolate direct effects at short durations. We find a significant increase in tenure in the first job after unemployment for those exiting nonemployment early, which suggests that they indeed find better jobs. We also see less changes of industry and suggestive evidence of more returns to the same firm, which are ambiguous measures of match quality, but could point to beneficial non-wage effects of longer PBD. We do not see any systematic or significant differences in firm size, changes of occupation,[23] employer location and firm size. These results add some plausibility to the hypothesis that better non-wage attributes may explain the puzzling negative direct effect of the PBD extension. However, the evidence for gains in non-wage attributes is weak and the negative direct effect could also arise from other explanations or an invalid assumption.[24]

[23]Switching occupation becomes significantly less likely after the first exhaustion point, but this could also be due to changes in dynamic selection.

[24]However, threshold manipulation is unlikely to explain our findings. The negative dynamic treatment effect before benefit exhaustion would require most workers who manipulated the threshold to exit long before the first exhaustion point, but it makes little sense for these workers to manipulate the threshold.

6 Conclusion

We examine how and what we can learn from (quasi-)experiments about the relation between PBD, unemployment duration and wages from the data alone, i.e. without a model that restricts treatment effect heterogeneity. To do so, we first provide a framework for (quasi-)experiments with two interdependent outcomes. This framework shows how and what we need to learn about treatment effect heterogeneity to better understand the effects of PBD. It establishes that without strong assumptions, (quasi-)experiments do not identify key structural parameters and cannot separate direct and indirect effects. Using insights from the literatures on IV and mediation analysis as well as a wage decomposition we examine what can be learned from the data alone nevertheless and provide tools to make progress in the presence of treatment effect heterogeneity.

We then use these tools to revisit the analyses of Schmieder, von Wachter and Bender (2016). Using a smaller but more detailed data set, we find that duration effects are indeed heterogeneous and concentrated at long durations. They appear to be due to a small fraction of individuals with unemployment duration close to their PBD staying in unemployment about as much longer as the PBD extension. Our results for nonemployment suggest that only a few of them were induced to stay without employment longer. A likely explanation for this finding that the PBD extension had only very small effects on behavior is that the extension at already long PBD did not relax binding search constraints for most workers. If so, one would not expect the PBD extension to yield large benefits from job search, which may help to reconcile the negative effect of this PBD extension with the positive effect of an extension from a shorter base PBD that Nekoei and Weber (2017) document in Austria. Our empirical evidence thereby supports their conjecture that extensions at shorter UI durations relax more binding search constraints and thereby amplify the positive selectivity effects of longer UI receipt.

Heterogeneity in unemployment duration leads to dynamic selection, which we analyze using wage components. We document sizable and non-monotonic variation in wages over the unemployment spell due to dynamic selection. Such variation leads to bias in studies of the effects of unemployment duration on wages. The non-monotonic pattern makes these biases difficult to predict in a given application, but our analyses can help researchers gauge the direction and extent of the problem and mitigate its consequences. The non-monotonic dynamics also imply a surprising extent of positive dynamic selection especially at longer durations, showing that the common question whether dynamic selection is positive or negative is an oversimplifying dichotomy. Rather, it appears that there is a lot of heterogeneity in employment dynamics. Pooling these heterogeneous profiles creates non-monotonic aggregates, suggesting that researchers likely need to zoom in on more homogeneous samples to isolate specific patterns and mechanisms. However, we do not find any evidence that dynamic selection changes with PBD before the first exhaustion point. Together with our finding of no duration effects before this point, this result allows us to estimate the dynamic treatment effect up until the first exhaustion point. So overall, the plausibility of sign restrictions, and the fact that duration effects do not depend on underlying structural parameters allow us to infer a detailed characterization of how PBD affects unemployment duration from the data only.

We find that the wage loss from longer PBD mostly or entirely stems from moving to lower-paying firms. This finding adds to evidence on the importance of firms. It suggests a persistent wage loss that is difficult to explain by skill depreciation, signaling, bargaining or other individual-specific explanations. We find no evidence of positive effects of PBD on wages and at most limited treatment effect heterogeneity, suggesting that local average effects of the effect of PBD on wages are informative to assess the consequences of policies such as PBD extensions. The methods we use here can be applied to examine whether this finding extends to other studies. Such analyses could provide further evidence that differences between studies stem from policy differences rather than differences in demographics. This conjecture is also supported by several of our other findings, such as that we find duration effects to vary with policy features like exhaustion points rather than with demographics. Overall, our analyses of the effects of PBD on duration and wages shows that when amended with other parameters (quasi-)experiments identify, local average effects yield insights that are important to understand the consequences of PBD extensions, to interpret local effects and to compare them across studies even without a model or strong restrictions of heterogeneity.

Even though key structural parameters are not identified without strong assumptions, using mediation analysis and a wage decomposition enables us to shed light on mechanisms and the presence of direct and indirect effects. Contrary to prior work, we find evidence of a direct effect of PBD on wages, implying that PBD is not a valid IV for duration and hence that the effect of duration on wages is not identified. We find little evidence that duration affects wages. Rather, our results are consistent with the entire wage loss due to PBD stemming from a (roughly) constant and negative direct effect of PBD on wages. Thereby, our approach of letting the data speak by itself sheds light on the validity of common assumptions. Most importantly, the data appears to contradict the assumption that direct effects of PBD are non-negative, showing that either some workers are harmed by PBD extensions even though they do not stay unemployed longer or that non-wage attributes play an important role. Yet we find only small effects on non-wage outcomes. In consequence, understanding how PBD and unemployment duration affect wages requires either more detailed data on non-wage attributes or less restrictive models that allow for negative direct effects.

Overall, we show that it is crucial to allow for unrestricted treatment effect heterogeneity when studying effects of PBD and more generally in (quasi-)experiments with two interdependent outcomes. We provide a model-free framework and tools to conceptualize, detect and mitigate problems of treatment effect heterogeneity when an IV affects two interdependent outcomes. We document the presence and relevance of these issues by analyzing the effects of a PBD extension, showing that the data remain surprisingly informative even without restrictions. The tools we provide can thus help researchers take treatment effect heterogeneity into account and make progress when studying the relation between PBD, duration and wages and more generally when analyzing mediators or IV with two interdependent outcomes.

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Appendix

A Wage Decomposition

A.1 Estimating Wage Components

Throughout the paper, we use the match effects model in Equation (5) including three fixed effects (worker, firm and job), but also estimate specification omitting various fixed effects to examine their importance. We use the entire sample of full-time employees in Germany (including workers from eastern and western Germany of all ages and females) for the decomposition. Otherwise the firm fixed effect may depend on the sample used in the outcome model. Table A.1 reports our estimates of β, which are as expected and significant at any conventional level.

TABLE A.1
WAGE DECOMPOSTITION

	X (1)	WFE (2)	TWFE (3)	JFE (4)
Experience	0.003801	0.006025	0.004154	0.006968
Experience Squared	-0.000006	-0.000005	-0.000004	-0.000003
Year Fixed Effects	Yes	Yes	Yes	Yes
Worker Fixed Effects	No	Yes	Yes	Yes
Firm Fixed Effects	No	No	Yes	Yes
Match Fixed Effects	No	No	No	Yes

X refers to a model including only X-variables and year fixed effects, WFE refers to a model additionally including worker fixed effects compared to the previous model, TWFE refers to a model additionally including firm fixed effects compared to the previous model and JFE refers to a model additionally including job fixed effects compared to the previous model. The job effect model uses the algorithm described in Mittag (2016). All displayed coefficients are significant at the 0.001 level. N: 42,113,038.

We then use the estimated fixed effects from the job fixed effects model in column (4) as dependent variables in the RDD. The dependent variable in Equation (5) is log wages, so the units of the fixed effects are also log wages. Thus, treatment effects on these wage components can be interpreted as changes in percent of the daily wage of the individual. Table A.2 provides the mean and standard deviation for each set of fixed effects, for the entire population of workers and firms in our data and the RDD sample. For the population of workers and firms in our data, the job fixed effects have mean zero, which must be the case by definition as the job fixed effects are standardized such that they sum up to zero for each individual. Note that the first columns present statistics for the population of annual employment spells. Thus, the fixed effects are weighted by the number of observations they correspond to, so that the average firm effect does not have to be zero despite being normalized to be zero in the population of firms. The statistics for our RDD sample refer to the post-unemployment spell, so none of the normalizations have to hold within this sample.

The average of all three sets of fixed effects is lower in the RDD sample compared to the population overall. This is because our RDD sample focuses on workers experiencing an unemployment spell between two full-time employment spells. These workers are negatively selected, work at lower-wage firms and move to lower-paying jobs compared to workers staying with their current employer or making direct job-to-job transitions.

TABLE A.2
SUMMARY STATISTICS OF FIXED EFFECTS BY SAMPLE

	Population		RDD	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Worker Fixed Effect	3.234	0.534	2.856	0.468
Firm Fixed Effect	0.214	0.207	0.171	0.222
Job Fixed Effect	0	0.098	-0.010	0.158
Observations	42,113,038		13,693	

Column 1 and 2 use all observations in full-time employment in in the LIAB Mover-Model. Column 3 and 4 report statistics for our RDD sample. Note that the first columns present statistics for the population of annual employment spells. Thus, the fixed effects are weighted by the number of observations they correspond to, so that the average firm effect does not have to be zero despite being normalized to be zero in the population of firms.

A.2 Bias in Estimated Worker Fixed Effects

A common concern regarding estimated effects is that estimation errors of different wage components are negatively correlated at the individual level (Andrews et al. 2008). That is, the estimation error of two estimated fixed effects is correlated at the individual level. For example, Andrews et al. (2008) show that in the TWFE, $Cov(\hat{\theta} - \theta, \hat{\psi} - \psi) < 0$ in samples with limited mobility. Thus, estimates that depend on this covariance such as estimates of worker sorting (as measured by $Cov(\theta, \psi)$) are biased in finite samples.

However, this bias does not affect the analyses in this paper. Rather than analyzing the relation between different wage components, we only analyze one wage component at a time. With few exceptions, this wage component is the dependent variable in our analyses and we never include other wage components among the explanatory variables. Thus, our estimates do not depend on the covariance of the estimated effects. Therefore, the covariance of the estimation error, which is the source of this bias, does not matter for our estimates. They depend only on the expectations of these wage components and their covariance with other covariates, which contrary to the covariance with other estimated wage components, are not affected by this mobility bias. In addition, we are mainly interested in differences between treatment and control groups, so such biases would only be relevant to us if they change with treatment.

Another concern is that treatment may affect the estimated worker fixed effects. The true worker fixed effect is the wage component determined by all (observable and unobservable) time-invariant characteristics. Treatment and unemployment duration are time-varying characteristics by definition, so they cannot affect the true worker fixed effect. Hence, the worker fixed effect can only vary with treatment and unemployment duration due to (dynamic) selection. However, this argument does not trivially extend to the estimated worker fixed effect we study. One may be concerned that the estimated worker fixed effect, $\hat{\theta}_i$, is biased in the sample we analyze, because we only analyze the subpopulation for which we observe an unemployment spell. If this unemployment spell has a meaningful effect on subsequent employment spell(s), then we are implicitly selecting a sample with at least one unusually low-wage employment spell in the sampled window. This low-wage spell will drive down the average wage we observe for this worker and thus negatively affect estimation error in the worker fixed effect. Thus, estimated worker fixed effects for those with unemployment spells may suffer from a downward bias in finite samples where the number of time periods and matches does not go to infinity for each worker. However, we compare two groups of workers with an unemployment spell, so any bias from unemployment affects both the treatment and the control group. Thus, this bias is only of concern to us if treatment increases or reduces the bias.

There are good conceptual and empirical reasons to believe this problem is negligible in our analysis. An important difference to the standard two-way fixed effects model that mitigates the potential bias is that we include a job fixed effect in Equation (5). This job fixed effect allows wages to differ systematically between jobs. Thereby, it should capture the effect of unemployment by allowing matches after unemployment to differ from those before unemployment. The worker fixed effect may still be affected if the post-unemployment jobs reduce the mean job fixed effect which is attributed to the worker fixed effect by normalizing job fixed effects to sum to zero within each individual. However, such bias mechanically has to have a (sign-reversed) effect on the job fixed effect. Yet, we find no effect on average job quality, implying at most small bias in the worker fixed effects. If the bias is small in the first place, it seems unlikely to change at the discontinuity sufficiently to have a meaningful impact on our results.

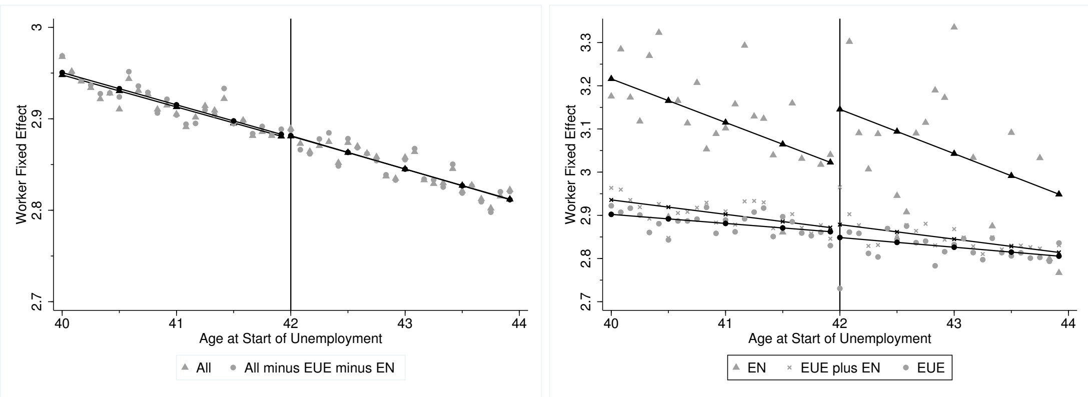
Indeed, we do not find any evidence of bias. Figure A.1 shows that the average worker fixed effects drop at the age discontinuity, raising concerns of bias. However, the effect is not statistically significant and does not appear to arise from unemployment. Figure A.1a provides evidence that the drop in worker fixed effects in Figure A.1b is not caused by sampling workers with unemployment spells by showing that there is no systematic difference between the treatment and control group when comparing the worker fixed effects of all workers experiencing an unemployment spell independently of the labor market status following unemployment. Rather, as Figure A.1b shows, the difference in our sample is offset by the average worker fixed effect of workers without any spells after unemployment. That is, those who become reemployed with long PBD are slightly worse in their time-invariant wage component, while those who do not return to employment subject to social security are slightly better in their time-invariant wage components. We find no meaningful effect of treatment on the probability of this trajectory or the attributes of workers taking it in Section C below. Thus, we believe that these small differences reflect sampling variation rather than meaningful bias.[25]

[25] Note that even with bias, one would expect the gap to close, but not entirely, unless one has reasons to believe that longer PBD biases the worker fixed effects of those not entering reemployment upwards (which seems unlikely, since their worker fixed effect is determined by employment prior to becoming unemployed).

FIGURE A.1
RDD PLOT OF WORKER FIXED EFFECT FOR DIFFERENT TRANSITION PATHS

(A) "ALL" TRANSITIONS FROM EMPLOYMENT

(B) CLOSING THE GAP IN OUR SAMPLE



The graphs combine standard RDD plots (Linear fit, bandwidth of 2 age years and monthly bins) where the estimated worker fixed effect from Equation 5 is the dependent variable for different samples. All observations meet the UI eligibility criteria and are assigned to samples based on their post-unemployment employment status. The first sample in **Panel A** includes all individuals exiting employment regardless of the subsequent status. The second sample removes our analysis sample and those transitioning directly to permanent non-employment from the first sample. **Panel B** provides RDD plots for the two samples that were removed in the second sample of Panel A, i.e. for those transiting directly to nonemployment (EN), our RDD sample of those transiting to employment via unemploymnet (EUE) and the union of these two samples.

B Summary Statistics

TABLE B.1
SUMMARY STATISTICS FOR OTHER EMPLOYMENT TRAJECTORIES

Transition from Job to ...	Job		Direct Nonemp.		Unemp. then Nonemp.		Unemp. to Non-FT Job		Unclear	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (8)	Mean (9)	SD (10)
Industry Category...										
Farming, etc.	0.079	0.269	0.047	0.211	0.076	0.266			0.079	0.270
Retail, Services	0.244	0.429	0.221	0.415	0.131	0.338			0.277	0.448
Gov. Dominated	0.134	0.340	0.256	0.437	0.139	0.346	0.263	0.442	0.121	0.326
Manufac., etc.	0.540	0.498	0.474	0.500	0.640	0.481	0.394	0.491	0.514	0.500
Highest Degree...										
High School	0.114	0.318	0.106	0.307	0.174	0.379			0.250	0.433
Voc. Training	0.673	0.469	0.498	0.500	0.552	0.498	0.557	0.499	0.602	0.490
University	0.213	0.409	0.396	0.489	0.275	0.447	0.258	0.440	0.148	0.356
Occupation Category...										
Simple	0.321	0.467	0.244	0.429	0.384	0.487	0.414	0.495	0.506	0.500
Skilled	0.380	0.485	0.332	0.471	0.281	0.450	0.232	0.424	0.277	0.448
High Skilled	0.299	0.458	0.424	0.494	0.335	0.473	0.354	0.480	0.217	0.412
Non-Citizen	0.061	0.240	0.138	0.345	0.184	0.388			0.185	0.389
Experience	191	66	175	67	177	66	164	70	174	71
Firm Size	1170	2912	951	1790	909	1334	800	1714	695	1295
Days of Benefit Rec.	127	309	103	263	193	470	290	427	360	605
# of Jobs	1.631	1.029	1.676	0.981	1.548	0.948	1.576	0.959	2.108	1.385
Age at First Employment	24.7	5.7	25.3	5.6	25.2	5.7	25.8	5.9	25.2	6.1
Blue Collar Job	0.490	0.500	0.327	0.469	0.501	0.501	0.465	0.501	0.631	0.483
Log Wage	4.545	0.328	4.638	0.433	4.513	0.377	4.368	0.376	4.348	0.434
Worker FE	2.884	0.451	3.092	0.504	2.956	0.511	2.985	0.560	2.842	0.506
$X\beta$	1.379	0.394	1.266	0.397	1.294	0.396	1.218	0.424	1.258	0.430
Firm FE	0.276	0.147	0.286	0.127	0.281	0.118	0.197	0.228	0.260	0.177
Job FE	0.009	0.111	-0.002	0.064	0.000	0.046	-0.011	0.075	-0.006	0.108
Residual	-0.002	0.100	-0.004	0.234	-0.017	0.186	-0.021	0.115	-0.007	0.185
Firm+Job FE+e	0.282	0.203	0.280	0.276	0.263	0.229	0.166	0.273	0.248	0.286
Observations	68,132		1,289		367		99		834	

This table provides summary statistics for the samples of employment trajectories that are not included in our analysis sample, which we use to analyze sample selection and RDD validity. All variables refer to information from the employment spell before separation. Missing values were not disclosed due to small sample sizes.

TABLE B.2
SUMMARY STATISTICS RDD SAMPLE: VARIABLES FROM EMPLOYMENT SPELL BEFORE UNEMPLOYMENT

	All			Treatment			Control		
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	Mean (7)	SD (8)	N (9)
Industry Category...									
Farming, etc.	0.197	0.398	13,693	0.202	0.402	6,076	0.193	0.395	7,617
Retail, Services	0.162	0.369	13,693	0.159	0.366	6,076	0.165	0.371	7,617
Gov. Dominated	0.092	0.289	13,693	0.089	0.285	6,076	0.095	0.293	7,617
Manufac., etc.	0.541	0.498	13,693	0.542	0.498	6,076	0.541	0.498	7,617
Cyclical	0.223	0.416	13,693	0.226	0.418	6,076	0.221	0.415	7,617
Highest Degree...									
High School	0.165	0.371	13,591	0.163	0.369	6,022	0.166	0.372	7,569
Voc. Training	0.723	0.448	13,591	0.728	0.445	6,022	0.718	0.450	7,569
University	0.113	0.316	13,591	0.110	0.312	6,022	0.115	0.319	7,569
Occupation Category...									
Simple	0.458	0.498	13,692	0.454	0.498	6,075	0.461	0.499	7,617
Skilled	0.359	0.480	13,692	0.366	0.482	6,075	0.353	0.478	7,617
High Skilled	0.183	0.387	13,692	0.180	0.385	6,075	0.186	0.389	7,617
Non-Citizen	0.095	0.293	13,668	0.094	0.292	6,065	0.095	0.293	7,603
Married	0.723	0.447	3,393	0.738	0.440	1,533	0.711	0.454	1,860
Tenure	54.7	64.9	13,693	57.0	67.5	6,076	52.9	62.7	7,617
Experience	170	70	13,693	175	71	6,076	166	69	7,617
Firm Size	426	1923	13,693	413	1850	6,076	436	1980	7,617
Days of Benefit Rec.	384	602	13,693	384	617	6,076	384	589	7,617
Days of Nonemp.	906	1165	13,693	832	1124	6,076	965	1195	7,617
# of Jobs	2.126	1.382	13,693	2.122	1.384	6,076	2.130	1.381	7,617
Age at First Employment	25.213	6.262	13,693	26.150	6.188	6,076	24.465	6.220	7,617
Blue Collar Job	0.679	0.467	13,693	0.683	0.465	6,076	0.676	0.468	7,617
Log Wage	4.323	0.379	13,693	4.325	0.381	6,076	4.321	0.378	7,617
...Age Adj.	4.318	0.379	13,693	4.316	0.381	6,076	4.320	0.378	7,617
Firm FE	0.213	0.209	13,693	0.216	0.192	6,076	0.210	0.221	7,617
...Age Adj.	0.209	0.209	13,693	0.210	0.192	6,076	0.208	0.221	7,617
Job FE	0.006	0.158	13,693	0.007	0.159	6,076	0.004	0.157	7,617
...Age Adj.	0.004	0.158	13,693	0.005	0.159	6,076	0.003	0.157	7,617
Residual	-0.009	0.130	13,693	-0.009	0.142	6,076	-0.008	0.120	7,617
...Age Adj.	-0.008	0.130	13,693	-0.008	0.142	6,076	-0.008	0.120	7,617
Firm+Job FE+e	0.210	0.294	13,693	0.214	0.293	6,076	0.206	0.294	7,617
...Age Adj.	0.205	0.294	13,693	0.206	0.293	6,076	0.204	0.294	7,617

This table provides summary statistics of the variables measured during the employment spell before unemployment for our main analysis sample.

TABLE B.3
SUMMARY STATISTICS RDD SAMPLE: VARIABLES FROM EMPLOYMENT SPELL AFTER UNEMPLOYMENT

	All			Treatment			Control		
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	Mean (7)	SD (8)	N (9)
Industry Category...									
Farming, etc.	0.176	0.381	13,693	0.182	0.386	6,076	0.171	0.377	7,617
Retail, Services	0.222	0.416	13,693	0.223	0.417	6,076	0.221	0.415	7,617
Gov. Dominated	0.131	0.338	13,693	0.134	0.341	6,076	0.128	0.335	7,617
Manufac., etc.	0.470	0.499	13,693	0.458	0.498	6,076	0.479	0.500	7,617
Cyclical	0.204	0.403	13,693	0.211	0.408	6,076	0.198	0.399	7,617
Changed ...									
Employer Location (State)	0.170	0.376	13,693	0.170	0.376	6,076	0.171	0.376	7,617
Industry Category	0.471	0.499	13,693	0.462	0.499	6,076	0.478	0.500	7,617
Occupation Category	0.411	0.492	13,693	0.406	0.491	6,076	0.415	0.493	7,617
Firm	0.936	0.245	13,693	0.932	0.252	6,076	0.939	0.240	7,617
Non-Citizen	0.078	0.268	13,682	0.078	0.268	6,074	0.078	0.268	7,608
Years of Education	12.8	2.2	13,693	12.7	2.2	6,076	12.8	2.2	7,617
Tenure	10.2	24.4	13,693	10.8	27.2	6,076	9.6	21.9	7,617
Firm Size	350	1471	13,693	331	1253	6,076	365	1624	7,617
Change in Firm Size	-75	2270	13,693	-81	2121	6,076	-71	2381	7,617
# of Jobs	5.798	3.238	13,693	5.788	3.238	6,076	5.807	3.238	7,617
# of Unemp. Spells	3.768	3.840	13,693	3.748	3.901	6,076	3.784	3.791	7,617
Log Wage	4.250	0.370	13,693	4.246	0.381	6,076	4.254	0.361	7,617
...Age Adj.	4.254	0.370	13,693	4.238	0.381	6,076	4.267	0.361	7,617
Worker FE	2.856	0.468	13,693	2.825	0.465	6,076	2.881	0.468	7,617
...Age Adj.	2.857	0.467	13,693	2.847	0.465	6,076	2.865	0.468	7,617
$X\beta$	1.246	0.426	13,693	1.275	0.431	6,076	1.223	0.421	7,617
...Age Adj.	1.246	0.425	13,693	1.251	0.431	6,076	1.242	0.421	7,617
Firm FE	0.171	0.222	13,693	0.170	0.219	6,076	0.171	0.224	7,617
...Age Adj.	0.173	0.222	13,693	0.165	0.219	6,076	0.179	0.224	7,617
Job FE	-0.010	0.158	13,693	-0.011	0.168	6,076	-0.009	0.149	7,617
...Age Adj.	-0.010	0.158	13,693	-0.013	0.168	6,076	-0.008	0.149	7,617
Residual	-0.013	0.115	13,693	-0.013	0.117	6,076	-0.013	0.114	7,617
...Age Adj.	-0.011	0.115	13,693	-0.013	0.117	6,076	-0.011	0.114	7,617
Firm+Job FE+e	0.148	0.270	13,693	0.146	0.279	6,076	0.150	0.261	7,617
...Age Adj.	0.151	0.270	13,693	0.140	0.279	6,076	0.161	0.261	7,617
Differenced ...									
Log Wage	-0.073	0.369	13,693	-0.079	0.382	6,076	-0.067	0.359	7,617
...Age Adj.	-0.064	0.369	13,693	-0.078	0.382	6,076	-0.053	0.358	7,617
Firm FE	-0.042	0.256	13,693	-0.046	0.242	6,076	-0.039	0.267	7,617
...Age Adj.	-0.036	0.256	13,693	-0.044	0.242	6,076	-0.029	0.266	7,617
Job FE	-0.015	0.231	13,693	-0.018	0.238	6,076	-0.013	0.225	7,617
...Age Adj.	-0.014	0.231	13,693	-0.018	0.238	6,076	-0.011	0.225	7,617
Residual	-0.004	0.174	13,693	-0.004	0.185	6,076	-0.004	0.165	7,617
...Age Adj.	-0.003	0.174	13,693	-0.004	0.185	6,076	-0.002	0.165	7,617
Firm+Job FE+e	-0.062	0.365	13,693	-0.068	0.378	6,076	-0.057	0.355	7,617
...Age Adj.	-0.054	0.365	13,693	-0.067	0.378	6,076	-0.043	0.354	7,617
Nonemployment duration	10.89	15.77	13,693	10.86	15.17	6,076	10.91	16.23	7,617
...Age Adj.	10.66	15.77	13,693	10.73	15.17	6,076	10.61	16.23	7,617
Unemployment Duration	4.80	4.30	13,693	5.17	4.92	6,076	4.50	3.71	7,617
...Age Adj.	4.80	4.30	13,693	5.10	4.92	6,076	4.56	3.71	7,617

This table provides summary statistics of the variables measured during the employment spell after unemployment for our main analysis sample.

TABLE B.4
SUMMARY STATISTICS FOR LARGE PBD GROUPS

	6-10 Months of PBD		12 Months of PBD		18 Months of PBD		20-32 Months of PBD	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (8)
Age at Separation	32.1	9.6	31.2	6.5	44.7	3.7	50.2	4.2
Experience	36.0	39.1	76.8	54.0	110.9	77.0	135.2	84.7
Years of Education	12.8	2.1	12.6	1.8	12.8	2.0	12.7	2.0
Log Wage	3.899	0.392	4.073	0.384	4.104	0.404	4.157	0.412
Worker FE	3.482	0.311	3.343	0.357	3.163	0.445	3.067	0.489
$X\beta$	0.441	0.255	0.649	0.344	0.850	0.465	0.976	0.514
Firm FE	0.038	0.271	0.105	0.244	0.078	0.230	0.095	0.228
Job FE	-0.059	0.217	-0.021	0.175	0.009	0.143	0.013	0.134
Residual	-0.005	0.112	-0.004	0.137	-0.001	0.131	-0.001	0.140
Nonemp. duration	4.096	3.631	3.829	3.355	5.248	4.987	6.378	6.403
Unemp. Duration	11.258	14.611	8.950	12.949	9.721	12.949	10.011	12.403
Observations	41,206		220,773		15,639		54,669	

This table provides summary statistics for the samples of employment spells by PBD duration, which we use to analyze dynamic selection in larger population groups in section 4.2. All variables refer to information from the employment spell before separation.

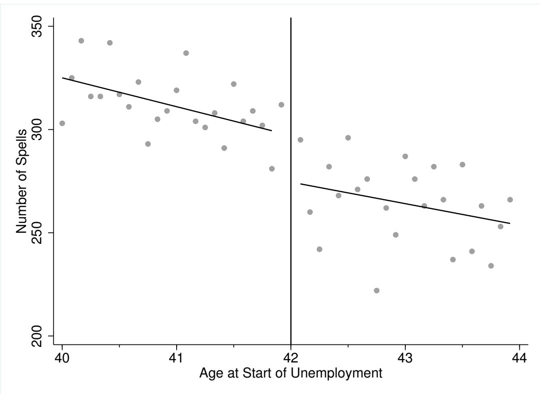
C RDD Validity and Prior Results

C.1 RDD Validity

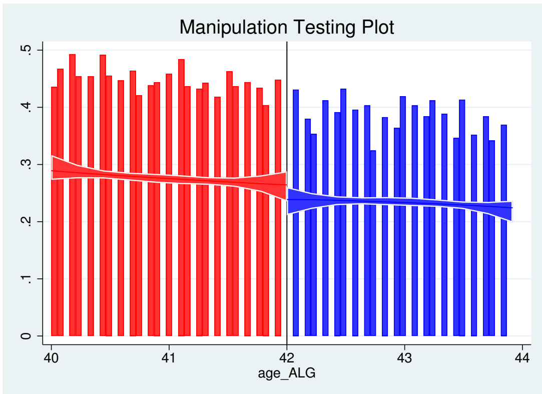
This section summarizes RDD validity checks and our replication of the basic results of Schmieder, von Wachter and Bender (2016). First, we find no evidence for bunching around the threshold or for any structural gaps in pre-unemployment characteristics around the threshold that would suggest strategic sorting of workers, thereby rendering the RDD invalid. Specifically, Figure C.1a 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. Figure C.1b provides results from the formal test for a discontinuity in the density of the running variable by Cattaneo, Jansson and Ma (2020). We cannot reject the hypothesis that the density is smooth at the cutoff.

FIGURE C.1
RDD VALIDITY AROUND THE AGE THRESHOLD

(A) FREQUENCY OF OBSERVATIONS



(B) CJM TEST OF THRESHOLD MANIPULATION



Panel A contains a standard RDD plot of the number of observations around the threshold at age 42 (marked by the vertical line) with linear fit, bandwidth of 2 age years and monthly bins. Panel B provides results from the test of RDD validity by Cattaneo, Jansson and Ma (2020).

We provide additional evidence ot the validity of the RDD by showing that there are no effects on other pre-determined variables. Table C.1 conducts this placebo test for education, occupation, citizenship, blue collar job before unemployment, firm size, experience, tenure, number of prior jobs, cumulative days of benefit receipt, age at first job and industry categories.

Identification of duration and wage effects also requires that treatment does not affect the probability of becoming re-employed (full-time) after unemployment (Ham and LaLonde 1996). Violations of this assumption have consequences similar to threshold manipulation in

TABLE C.1
RDD VALIDITY: EFFECTS ON PREDETERMINED VARIABLES

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Highest Degree:			Occupation Category:		
	High School	Voc Training	University	Simple	Skilled	High Skilled
RD Estimate	-0.005	0.004	0.001	-0.007	0.024	-0.016
p-value	[.711]	[.792]	[.953]	[.680]	[.169]	[.234]
Observations	13,466	13,466	13,466	13,566	13,566	13,566
Outcome	Non-citizen	Blue Collar Job	Firm Size	Experience	Tenure	Number of Prior Jobs
RD Estimate	0.009	0.011	-9.410	1.982	-0.031	0.080
p-value	[.391]	[.511]	[.895]	[.433]	[.989]	[.108]
Observations	13,542	13,567	13,567	13,567	13,567	13,567
Outcome	Cumul. Days of benefit receipt	Age at first job	Ind: Farming, etc.	Ind: Retail, Services	Ind: Gov. dominated	Ind: Manufac-turing, etc.
RD Estimate	25.684	-0.169	0.011	-0.002	-0.016	0.011
p-value	[.230]	[.450]	[.425]	[.875]	[.116]	[.534]
Observations	13,567	13,567	13,567	13,567	13,567	13,567
Outcome	Wages and Components of Previous Job					
	Log Wage	Worker FE	$X\beta$	Firm FE	Job FE	Residual
RD Estimate	-0.003	-0.017	0.012	0.001	0.002	-0.001
p-value	[.841]	[.304]	[.420]	[.887]	[.687]	[.820]
Observations	13567	13567	13567	13567	13567	13567

This table presents results from using pre-determined variables as outcomes in the RDD. All variables except for the worker fixed effect are measured before unemployment.

TABLE C.2
RDD VALIDITY: EFFECTS ON FREQUENCIES OF DIFFERENT EMPLOYMENT TRAJECTORIES

Transition: Job to...	Job (1)	Nonemp. (2)	Unemp. to FT Job (3)	Un- then Nonemp. (4)	Unemp. to Non-FT Job (5)	Unclear (6)
RD Estimate	0.032	0.003	-0.035	0.000	0.001	-0.001
p-value	[.000]	[.055]	[.000]	[.658]	[.176]	[.330]

This table uses all workers who fulfill the conditions for being included in our RDD sample, except for the condition that we observe a full time job for them after unemployment. Each column reports the effect of longer PBD on the probability of taking the respective employment trajectory, i.e. the estimated treatment effect from using a dummy for whether the worker took this trajectory as the dependent variable in our RDD. N: 84,276

that it results in some individuals missing from one side of the cutoff (but contrary to threshold manipulation, they disappear rather than showing up on the other side of the cutoff). Therefore, our tests above provide some evidence that our approach is valid, but this assumption can also be tested directly. We do so by estimating RDD effects on the probability of each of 6 employment trajectories we observe in our data: Our sample, job-to-job transitions (JTJ), permanent exit, permanent exit after unemployment, non-full-time employment after unemployment as well as a small residual category of other trajectories. The results in Table C.2 show that longer PBD decreases the probability of being in our sample by three percentage

TABLE C.3
RDD VALIDITY: EFFECTS ON PRE-DETERMINED COVARIATES FOR OTHER EMPLOYMENT TRAJECTORIES

Transition from Job to ...	Job		Nonemp.		Unemp. then Nonemp.		Unemp. to Non-FT Job		Unclear	
Outcome	Effect (1)	p-value (2)	Effect (3)	p-value (4)	Effect (5)	p-value (6)	Effect (7)	p-value (8)	Effect (9)	p-value (10)
Highest Degree ...										
High School	-0.010	[.046]	-0.024	[.512]	0.082	[.264]	0.203	[.103]	-0.102	[.120]
Voc. Training	0.007	[.332]	-0.093	[.105]	-0.038	[.718]	-0.146	[.479]	0.068	[.335]
University	0.003	[.658]	0.117	[.038]	-0.043	[.650]	-0.057	[.752]	0.034	[.443]
Experience	0.362	[.722]	-20.723	[.004]	-9.984	[.479]	-12.679	[.618]	-7.475	[.455]
# of Jobs	0.005	[.776]	0.151	[.156]	-0.055	[.788]	-1.005	[.044]	0.072	[.714]
Non Citizen	-0.005	[.163]	0.021	[.586]	0.095	[.258]	0.076	[.372]	-0.034	[.542]
Blue Collar Job	-0.009	[.232]	-0.092	[.083]	0.133	[.207]	-0.213	[.278]	-0.068	[.299]
Firm Size	-32.459	[.463]	2.854	[.985]	89.641	[.677]	-865.677	[.403]	293.622	[.137]
Days of Benefit Rec.	-0.354	[.940]	-7.515	[.803]	62.936	[.490]	65.032	[.687]	-155.777	[.093]
Age at First Job	0.170	[.048]	1.616	[.007]	1.406	[.233]	2.726	[.201]	1.133	[.188]
Occupation Cat. ...										
Simple	-0.001	[.878]	-0.052	[.283]	0.253	[.014]	0.009	[.960]	-0.045	[.521]
Skilled	0.006	[.450]	0.004	[.941]	-0.238	[.009]	0.072	[.686]	-0.023	[.713]
High Skilled	-0.005	[.522]	0.048	[.393]	-0.015	[.878]	-0.081	[.686]	0.069	[.217]
Industry Cat. ...										
Farming, etc.	0.001	[.776]	0.009	[.721]	0.013	[.828]	-0.120	[.399]	0.005	[.888]
Retail, Services	0.014	[.031]	0.031	[.494]	0.039	[.584]	0.105	[.447]	-0.040	[.522]
Gov. Dominated	-0.005	[.351]	0.022	[.653]	-0.186	[.015]	-0.087	[.637]	-0.034	[.435]
Manufac., etc.	-0.012	[.127]	-0.057	[.313]	0.104	[.325]	0.073	[.710]	0.085	[.229]
Previous...										
Log Wage	0.004	[.373]	0.029	[.598]	-0.084	[.317]	0.260	[.092]	0.025	[.667]
$x\beta$	0.003	[.568]	-0.123	[.004]	-0.054	[.523]	-0.079	[.613]	-0.037	[.547]
Firm FE	0.001	[.681]	-0.004	[.790]	0.028	[.269]	0.113	[.283]	0.034	[.176]
Job FE	0.000	[.797]	-0.004	[.597]	-0.002	[.869]	0.019	[.368]	0.011	[.416]
Residual	-0.001	[.473]	0.029	[.377]	-0.018	[.688]	0.054	[.416]	-0.014	[.480]
Firm FE+Job FE+e	0.000	[.959]	0.021	[.579]	0.009	[.870]	0.186	[.135]	0.031	[.379]
Observations	68,121		1,289		367		98		834	

This table presents results from estimating RDD effects for workers who fulfill all conditions for being included in our RDD sample, except for the condition that we observe a full time job for them after unemployment. The subsequent employment outcome defines the 6 samples in the first row (results for our analysis sample are reported in table C.1). We apply the RDD to each of these samples using the pre-determined variables in column (1) as outcomes. All outcomes are measured before unemployment. Sample sizes for each trajectory are reported in Table C.2.

points and correspondingly increases the probability of JTJ by the same amount. We also find a significant effect on the probability of permanent exit, but it is very small in magnitude.

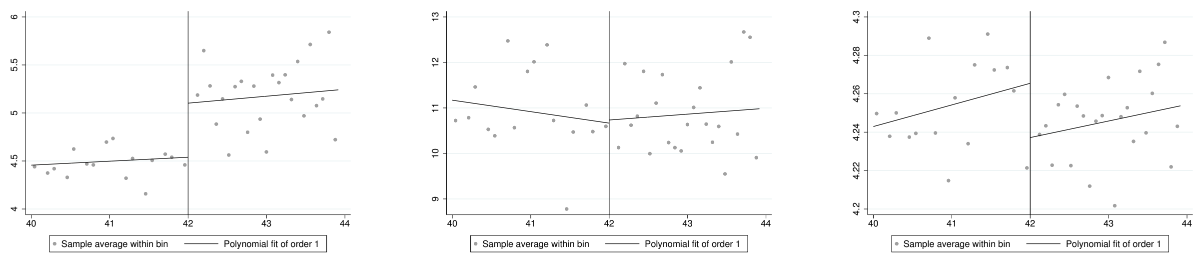
These results raise the question whether failure of this assumption causes bias in our results. Our tests of threshold manipulation suggest that while treatment reduces the probability of being in our sample, it does not have any meaningful effects on sample composition. We provide further evidence of this "missing at random" condition by estimating RDD effects on pre-determined variables for all other employment trajectories. The results in Table C.2 show that only few coefficients are significant and the differences we find do not suggest that the additional job-to-job transitions in our treatment group remove workers from our treatment group who are systematically positively or negatively selected. We do find some systematic difference among those who transit from employment to permanent non-employment. We only find a minuscule effect on the probability of taking this trajectory, so sample selection would have to be extreme to explain these effects. In conclusion, these results raise some concerns about sample selection, but overall confirm the conclusion of Schmieder, von Wachter and Bender (2016) that the assumptions of the RDD hold.

C.2 Replicating Prior Results

Section 3 shows that estimates of the effects of PBD from our data are in line with the results of Schmieder, von Wachter and Bender (2016). Here, we provide additional results. Figure C.2 shows RDD plots for the effects on unemployment duration, non-employment duration and reemployment wages in Table 1. These graphs add evidence to the validity of the RDD and the absence of non-linearities around the cutoff.

FIGURE C.2
RDD PLOTS FOR THE OUTCOMES FROM SCHMIEDER, VON WACHTER AND BENDER (2016) USING OUR SAMPLE

(A) UNEMPLOYMENT DURATION (B) NONEMPLOYMENT DURATION (C) LOG REEMPLOYMENT WAGES



RDD plots (Linear fit, bandwidth of 2 age years and monthly bins) for the outcomes in Schmieder, von Wachter and Bender (2016) using our RDD sample. Vertical line marks age threshold at age 42.

Table C.4 provides evidence that these results are robust to alternative specification choices. While we find no evidence that any variables change at the cutoff in Table C.1, one may be concerned that covariates play a role. The upper part of Table C.4 provides evidence that covariates do not affect our results by conditioning on pre-determined variables: Highest degree obtained dummies, non-citizen status, tenure in the previous job and whether the previous job was a blue-collar job. Including other pre-determined variables does not affect these results in a meaningful way. If the RDD is valid, including the covariates as further control variables should not make any difference. Indeed, the point estimates hardly changes and the statistical significance remains unchanged.

As we argue in Section 3, we report conventional RDD estimates throughout to improve comparability and ease interpretation. The lower panel of Table C.4 reports bias-corrected estimates and p-values using bias-correction robust SEs to show that this convenient choice does not affect our substantive conclusions. The bias correction slightly increases the estimated duration effects and has no meaningful effect on their significance. If anything, precision slightly increases. The point estimates of the wage effects are virtually unchanged. The only exception are the results for differenced wage components, where the effect is reduced for the firm fixed effect and increases for the job fixed effect. Most coefficients remain significant with the bias correction. Robust SEs are slightly larger, but the overall pattern we find remains unchanged.

TABLE C.4
ALTERNATIVE RDD SPECIFICATIONS

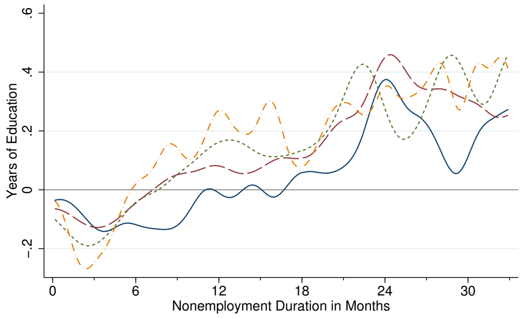
	Duration: Unemp. (1)	Duration: Nonemp. (2)	Log Wage (3)	Firm FE (4)	Job FE (5)	Residual (6)	Sum (7)
Conditoning on Predetermined Variables							
Levels	0.611	0.250	-0.025	-0.015	-0.005	-0.003	-0.023
p-value	[.000]	[.648]	[.058]	[.054]	[.344]	[.511]	[.017]
Changes			-0.027	-0.016	-0.008	-0.001	-0.026
p-value			[.035]	[.057]	[.343]	[.831]	[.045]
Bias-corrected Estimates and Robust SEs							
Levels	0.753	1.101	-0.022	-0.014	-0.004	-0.008	-0.025
Conv. p-value	[.000]	[.048]	[.100]	[.080]	[.473]	[.077]	[.009]
Robust p-value	[.002]	[.200]	[.299]	[.280]	[.620]	[.277]	[.099]
Changes			-0.034	-0.005	-0.015	-0.009	-0.029
Conv. p-value			[.010]	[.527]	[.072]	[.182]	[.024]
Robust p-value			[.102]	[.687]	[.251]	[.405]	[.154]

This table repeats the analyses in Tables 1 and 2 with different RDD specifications. The effects in the upper panel conditions on the following pre-determined covariates: Highest degree obtained dummies, non-citizen status, tenure in the previous job and whether the previous job was a blue-collar job. Including other pre-determined variables does not affect these results in a meaningful way. The first two lines use wage components of the post-unemployment job from Equation (5) as outcomes in the RDD. The third and fourth line subtracts the pre-unemployment component from the respective wage component. The lower panel applies the bias correction of Calonico, Cattaneo and Titiunik (2014) to our estimates in Tables 1 and 2. The first p-value uses conventional SEs, the second p-value uses bias-correction robust SEs. Column 7 uses the sum of the firm FE, job FE and the residual as the outcome. N: 13,567

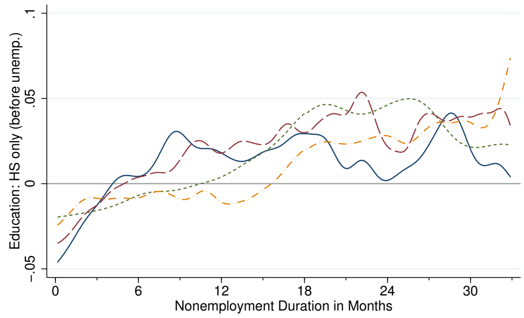
D Additional Analyses

FIGURE D.1
DYNAMIC SELECTION ON PRE-DETERMINED COVARIATES FOR LARGE PBD GROUPS

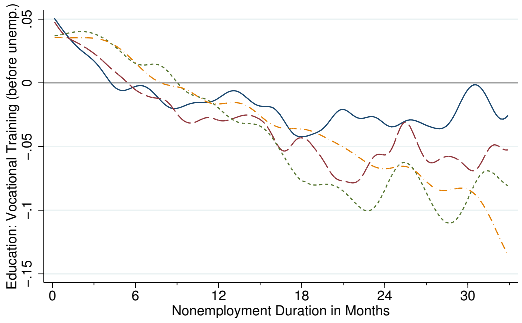
(A) YEARS OF EDUCATION



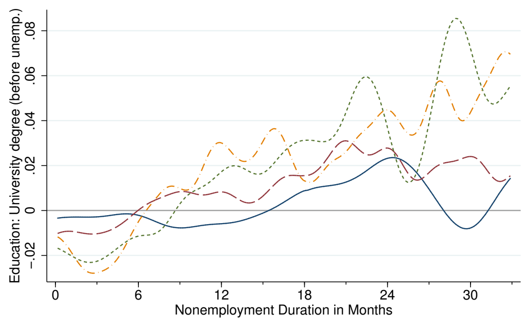
(B) HIGHEST DEGREE: HIGH SCHOOL ONLY



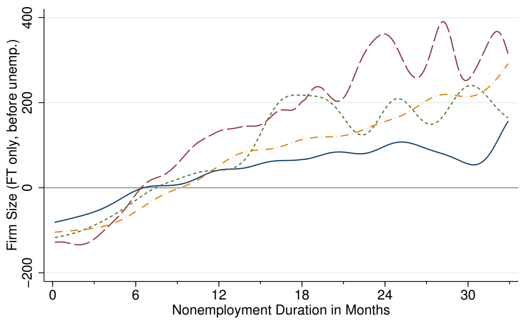
(C) HIGHEST DEGREE: VOCATIONAL TRAINING



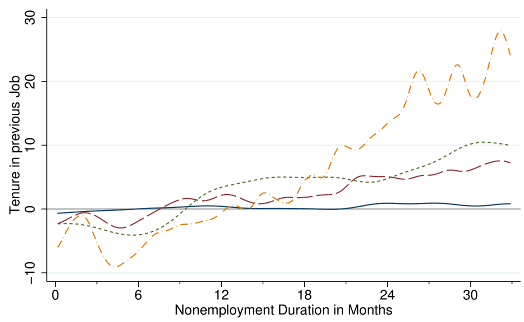
(D) HIGHEST DEGREE: SOME UNIVERSITY



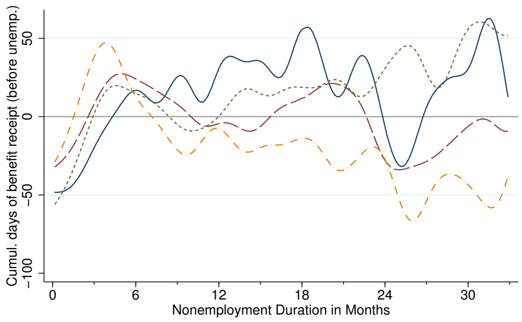
(E) PREVIOUS FIRM SIZE



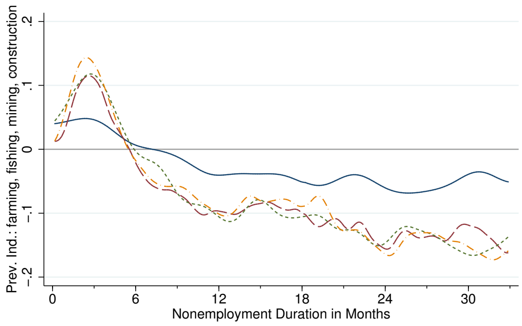
(F) TENURE IN PREVIOUS JOB



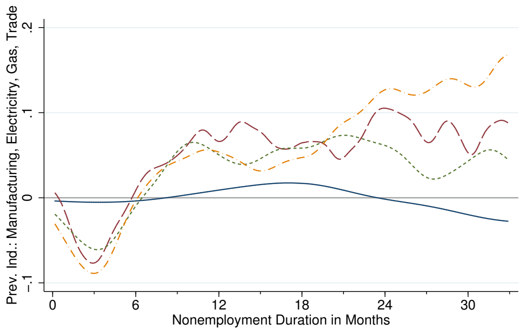
(G) Days of Benefit Receipt Before Unemployment



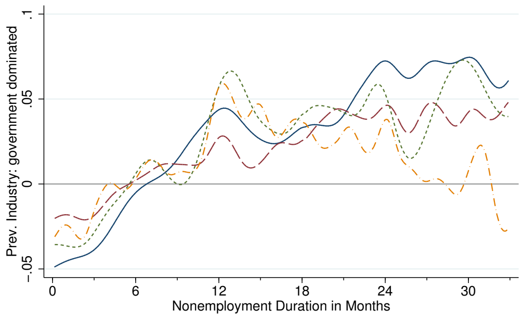
(H) Industry: Farming, Fishing, Mining, Construction



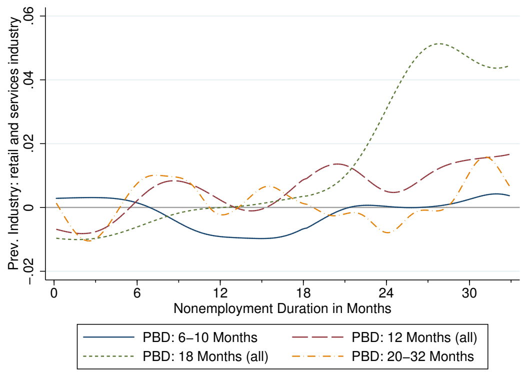
(I) Industry: Retail and Services



(J) Industry: Government Dominated



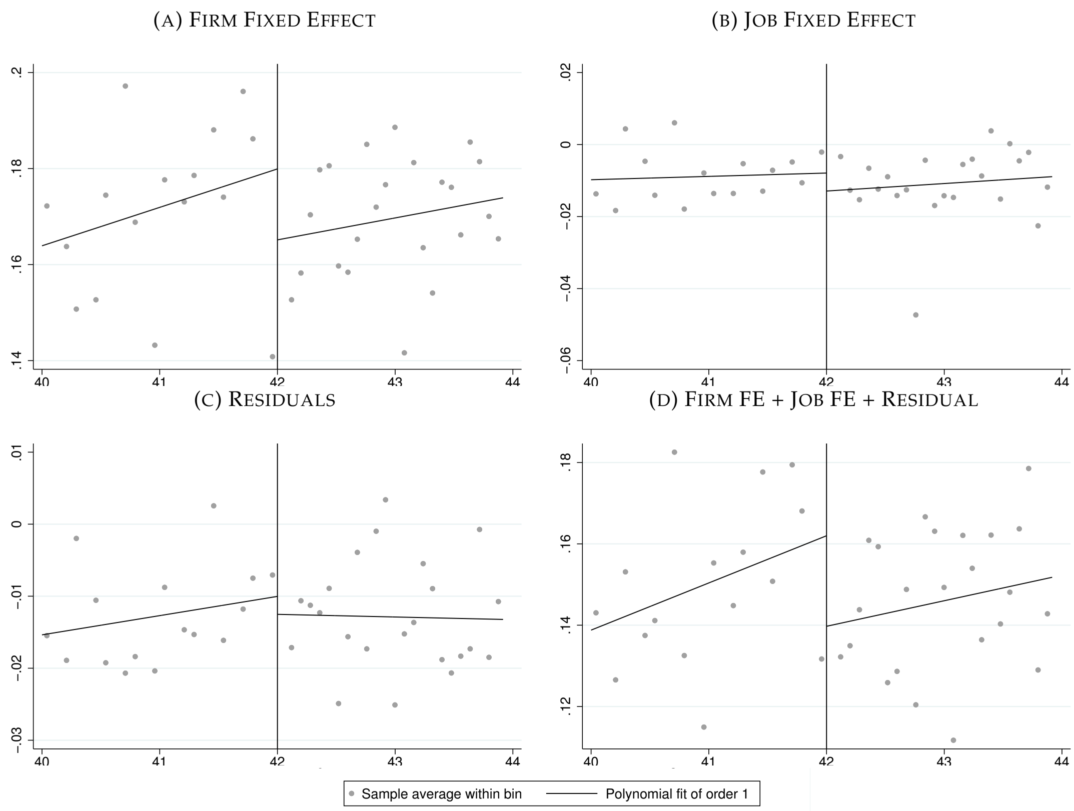
(K) Industry: Manufacturing, Electricitry, Gas, Trade



The line for each group plots the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Supplement Figures S.6-S.9 provide confidence bands.

FIGURE D.2

THE EFFECT OF PBD EXTENSIONS ON WAGE COMPONENTS: GRAPHICAL RDD EVIDENCE



RDD plots (Linear fit, bandwidth of 2 age years and monthly bins) for the estimated wage components from Equation (5). Vertical line marks age threshold at age 42.

TABLE D.1
CONDITIONAL AVERAGE TREATMENT EFFECTS: WAGES AND FIRM FIXED EFFECT

Outcome	Log Wages					Firm Fixed Effect				
	Sample 1		Sample 2		p-value	Sample 1		Sample 2		p-value
Sample split by...	Effect (1)	p-value (2)	Effect (3)	p-value (4)	(1)=(4) (5)	Effect (6)	p-value (7)	Effect (8)	p-value (9)	(6)=(8) (10)
Highest Degree...										
High School	-0.025	[.086]	-0.054	[.052]	[.409]	-0.017	[.050]	-0.009	[.676]	[.721]
Voc. Training	-0.036	[.249]	-0.025	[.076]	[.689]	-0.014	[.339]	-0.015	[.097]	[.937]
University	-0.030	[.022]	-0.021	[.592]	[.829]	-0.014	[.095]	-0.024	[.208]	[.712]
Non-Citizen	-0.027	[.054]	-0.027	[.533]	[.996]	-0.015	[.062]	-0.009	[.745]	[.820]
Married	0.009	[.851]	-0.032	[.297]	[.248]	-0.021	[.482]	-0.014	[.412]	[.754]
Occupation Cat....										
Simple	-0.032	[.078]	-0.028	[.124]	[.858]	-0.022	[.033]	-0.007	[.564]	[.336]
Skilled	-0.035	[.047]	-0.016	[.417]	[.500]	-0.013	[.198]	-0.018	[.144]	[.768]
High Skilled	-0.020	[.132]	-0.032	[.304]	[.710]	-0.011	[.184]	-0.026	[.155]	[.499]
Industry Cat....										
Farming, etc.	-0.024	[.125]	-0.044	[.063]	[.516]	-0.012	[.169]	-0.025	[.097]	[.513]
Retail, Services	-0.025	[.076]	-0.045	[.224]	[.559]	-0.012	[.148]	-0.032	[.166]	[.361]
Gov. Dominated	-0.029	[.035]	-0.022	[.668]	[.879]	-0.015	[.060]	-0.017	[.534]	[.952]
Manufac., etc.	-0.042	[.034]	-0.017	[.340]	[.353]	-0.026	[.033]	-0.006	[.536]	[.214]
Cyclical	-0.019	[.233]	-0.061	[.015]	[.191]	-0.011	[.226]	-0.028	[.075]	[.371]
Blue Collar Job	-0.016	[.513]	-0.028	[.034]	[.672]	-0.026	[.055]	-0.009	[.358]	[.311]
Previous...										
Log Wage	-0.025	[.098]	-0.032	[.078]	[.800]	-0.016	[.168]	-0.014	[.173]	[.911]
Firm FE	-0.025	[.154]	-0.033	[.088]	[.791]	-0.021	[.052]	-0.010	[.355]	[.462]
Residual	-0.019	[.292]	-0.040	[.043]	[.438]	-0.013	[.255]	-0.017	[.114]	[.774]
Worker FE	-0.031	[.041]	-0.026	[.214]	[.857]	-0.020	[.055]	-0.010	[.406]	[.534]
Experience	-0.047	[.024]	-0.010	[.563]	[.163]	-0.019	[.100]	-0.010	[.347]	[.546]
Tenure	-0.008	[.696]	-0.048	[.007]	[.127]	-0.010	[.345]	-0.019	[.089]	[.589]
Firm Size	-0.023	[.205]	-0.033	[.092]	[.713]	-0.016	[.151]	-0.012	[.256]	[.817]
# of Jobs	-0.036	[.038]	-0.015	[.452]	[.429]	-0.009	[.385]	-0.020	[.084]	[.440]
# of Unemp. Spells	-0.039	[.041]	-0.014	[.436]	[.345]	-0.013	[.248]	-0.016	[.138]	[.841]
Days of Nonemp.	-0.023	[.203]	-0.034	[.084]	[.680]	-0.020	[.065]	-0.011	[.357]	[.543]
Days of Benefit Rec.	-0.038	[.054]	-0.019	[.284]	[.473]	-0.012	[.268]	-0.017	[.119]	[.761]

This table reports results from splitting the sample according to the variable in the first column and estimating our RDD effects on the outcome in the first row separately for each of the two samples. All variables refer to the pre-unemployment job or state. For the continuous variables in the last 11 rows of the table, the samples are split at the median of the respective variable. For each sample, the first column reports the estimated treatment effect and the second column the p-value from a test whether it is zero. The final column for each outcome reports the p-value from testing whether the treatment effect is the same in the two samples conducted by randomly permuting the variable in the first column 1,500 times. Sample sizes are provided in Table D.3.

TABLE D.2
CONDITIONAL AVERAGE TREATMENT EFFECTS: JOB FIXED EFFECT AND RESIDUAL

Outcome	Job Fixed Effect					Residual				
	Sample 1		Sample 2		p-value	Sample 1		Sample 2		p-value
	Effect	p-value	Effect	p-value	(1)=(4)	Effect	p-value	Effect	p-value	(6)=(8)
Sample split by...	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Highest Degree...										
High School	-0.002	[.751]	-0.020	[.130]	[.241]	-0.003	[.529]	0.000	[.979]	[.819]
Voc. Training	-0.013	[.275]	-0.002	[.800]	[.345]	-0.002	[.732]	-0.003	[.612]	[.975]
University	-0.005	[.384]	-0.004	[.866]	[.957]	-0.002	[.638]	-0.006	[.524]	[.809]
Non-Citizen	-0.005	[.400]	-0.005	[.763]	[.981]	-0.003	[.508]	0.001	[.923]	[.752]
Married	-0.011	[.460]	-0.001	[.947]	[.488]	0.019	[.144]	-0.013	[.205]	[.005]
Occupation Category...										
Simple	-0.002	[.844]	-0.009	[.220]	[.495]	-0.004	[.443]	0.000	[.980]	[.632]
Skilled	-0.002	[.810]	-0.011	[.219]	[.429]	-0.002	[.677]	-0.003	[.683]	[.957]
High Skilled	-0.010	[.083]	0.017	[.300]	[.055]	-0.001	[.766]	-0.008	[.390]	[.538]
Industry Category...										
Farming, etc.	-0.005	[.466]	-0.006	[.544]	[.899]	-0.006	[.234]	0.008	[.456]	[.221]
Retail, Services	-0.004	[.508]	-0.010	[.445]	[.702]	-0.002	[.727]	-0.007	[.517]	[.633]
Gov. Dominated	-0.006	[.303]	0.004	[.826]	[.578]	-0.001	[.799]	-0.016	[.294]	[.339]
Manufac., etc.	-0.004	[.568]	-0.005	[.497]	[.909]	-0.001	[.840]	-0.003	[.590]	[.843]
Cyclical	-0.003	[.663]	-0.012	[.247]	[.497]	-0.003	[.462]	0.001	[.892]	[.642]
Blue Collar Job	0.015	[.183]	-0.015	[.016]	[.011]	-0.014	[.066]	0.002	[.654]	[.083]
Previous...										
Log Wage	-0.006	[.445]	-0.004	[.591]	[.912]	-0.001	[.866]	-0.004	[.515]	[.759]
Firm FE	-0.007	[.304]	-0.003	[.744]	[.675]	0.009	[.143]	-0.014	[.032]	[.007]
Residual	-0.003	[.658]	-0.007	[.410]	[.727]	0.001	[.896]	-0.006	[.347]	[.437]
Worker FE	-0.001	[.936]	-0.009	[.257]	[.411]	0.000	[.999]	-0.005	[.324]	[.534]
Experience	-0.015	[.076]	0.005	[.485]	[.050]	-0.011	[.057]	0.005	[.380]	[.053]
Tenure	-0.007	[.385]	-0.003	[.709]	[.653]	-0.004	[.499]	-0.001	[.896]	[.678]
Firm Size	-0.005	[.562]	-0.006	[.438]	[.939]	0.003	[.690]	-0.008	[.165]	[.230]
# of Jobs	-0.007	[.294]	-0.004	[.721]	[.788]	-0.009	[.119]	0.005	[.384]	[.114]
# of Unemp. Spells	-0.011	[.131]	0.001	[.908]	[.319]	-0.005	[.373]	0.000	[.968]	[.509]
Days of Nonemp.	0.003	[.645]	-0.014	[.085]	[.109]	-0.009	[.162]	0.004	[.503]	[.126]
Days of Benefit Rec.	-0.003	[.726]	-0.007	[.321]	[.713]	0.000	[.983]	-0.005	[.448]	[.591]

This table reports results from splitting the sample according to the variable in the first column and estimating our RDD effects on the outcome in the first row separately for each of the two samples. All variables refer to the pre-unemployment job or state. For the continuous variables in the last 11 rows of the table, the samples are split at the median of the respective variable. For each sample, the first column reports the estimated treatment effect and the second column the p-value from a test whether it is zero. The final column for each outcome reports the p-value from testing whether the treatment effect is the same in the two samples conducted by randomly permuting the variable in the first column 1,500 times. Sample sizes are provided in Table D.3.

TABLE D.3
CONDITIONAL AVERAGE TREATMENT EFFECTS: FIRM FE + JOB FE+ RESIDUAL

	Sample 1			Sample 2			p-value
	Effect	p-value	N	Effect	p-value	N	(1)=(4)
Sample split by...	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Highest Degree...							
High School	-0.022	[.041]	11244	-0.029	[.246]	2222	[.787]
Voc. Training	-0.030	[.127]	3735	-0.020	[.079]	9731	[.629]
University	-0.021	[.037]	11953	-0.033	[.284]	1513	[.694]
Non-Citizen	-0.023	[.023]	12258	-0.013	[.697]	1284	[.763]
Married	-0.012	[.698]	931	-0.028	[.259]	2439	[.524]
Occupation Category...							
Simple	-0.028	[.030]	7345	-0.016	[.273]	6221	[.553]
Skilled	-0.017	[.178]	8703	-0.031	[.035]	4863	[.469]
High Skilled	-0.023	[.031]	11084	-0.017	[.487]	2482	[.813]
Industry Category...							
Farming, etc.	-0.023	[.040]	10885	-0.024	[.222]	2682	[.942]
Retail, Services	-0.017	[.088]	11367	-0.048	[.074]	2200	[.231]
Gov. Dominated	-0.022	[.026]	12320	-0.029	[.399]	1247	[.857]
Manufac., etc.	-0.031	[.035]	6230	-0.015	[.247]	7337	[.387]
Cyclical	-0.017	[.118]	10536	-0.039	[.058]	3031	[.356]
Blue Collar Job	-0.025	[.168]	4342	-0.021	[.069]	9225	[.856]
Previous...							
Log Wage	-0.023	[.102]	6785	-0.022	[.099]	6782	[.987]
Firm FE	-0.020	[.125]	6784	-0.026	[.059]	6783	[.749]
Residual	-0.015	[.254]	6784	-0.030	[.032]	6783	[.430]
Worker FE	-0.021	[.114]	6786	-0.024	[.090]	6781	[.849]
Experience	-0.046	[.002]	6784	0.001	[.957]	6783	[.013]
Tenure	-0.022	[.119]	6784	-0.022	[.092]	6783	[.999]
Firm Size	-0.018	[.190]	6812	-0.026	[.051]	6755	[.701]
# of Jobs	-0.025	[.040]	7689	-0.019	[.241]	5878	[.737]
# of Unemp. Spells	-0.029	[.032]	7062	-0.015	[.286]	6505	[.479]
Days of Nonemp.	-0.025	[.061]	6819	-0.020	[.146]	6748	[.791]
Days of Benefit Rec.	-0.015	[.265]	6793	-0.029	[.032]	6774	[.477]

This table reports results from splitting the sample according to the variable in the first column and estimating our RDD effects on the sum of the firm FE, job FE and the residual. All variables refer to the pre-unemployment job or state. For the continuous variables in the last 11 rows of the table, the samples are split at the median of the respective variable. For each sample, the first column reports the estimated treatment effect and the second column the p-value from a test whether it is zero. The third column reports the sample sizes, which also apply to the results in Tables D.1 and D.2. The final column reports the p-value from testing whether the treatment effect is the same in the two samples conducted by randomly permuting the variable in the first column 1,500 times.

TABLE D.4
RDD EFFECTS ON NON-WAGE OUTCOMES

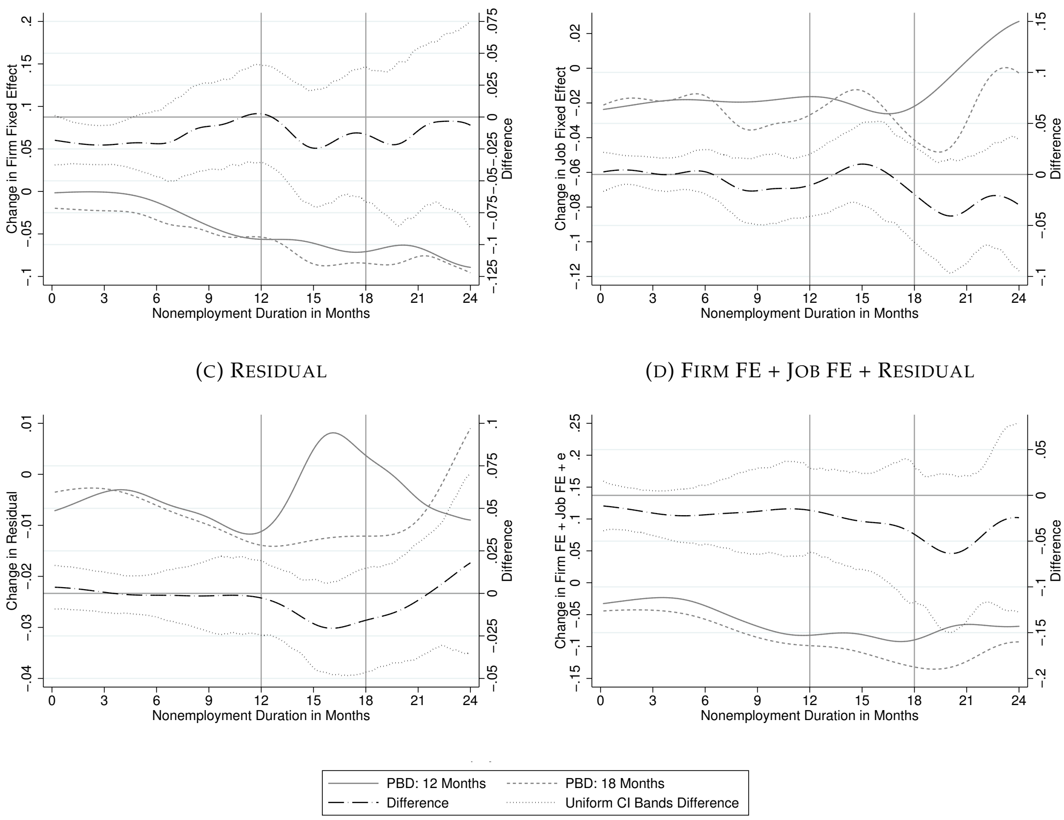
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Firm Size		Change of...			
	Level	Change	Location (State)	Industry Cat.	Occupation Cat.	Firm
RD Estimate	-51.726	-42.316	0.017	-0.017	0.024	0.007
p-value	[.253]	[.596]	[.209]	[.331]	[.177]	[.453]
Outcome	Tenure	Ind: Farming, etc.	Ind: Retail, Services	Ind: Gov. dominated	Ind: Manufacturing, etc.	
RD Estimate	1.296	0.011	0.018	-0.014	-0.016	
p-value	[.154]	[.439]	[.215]	[.243]	[.383]	

This table presents results from using measures of non-wage attributes as outcomes in the RDD. All variables refer to the post-unemployment job. N: 13,567

FIGURE D.3
DYNAMICS OF CHANGES IN WAGE COMPONENTS OVER NONEMPLOYMENT SPELL

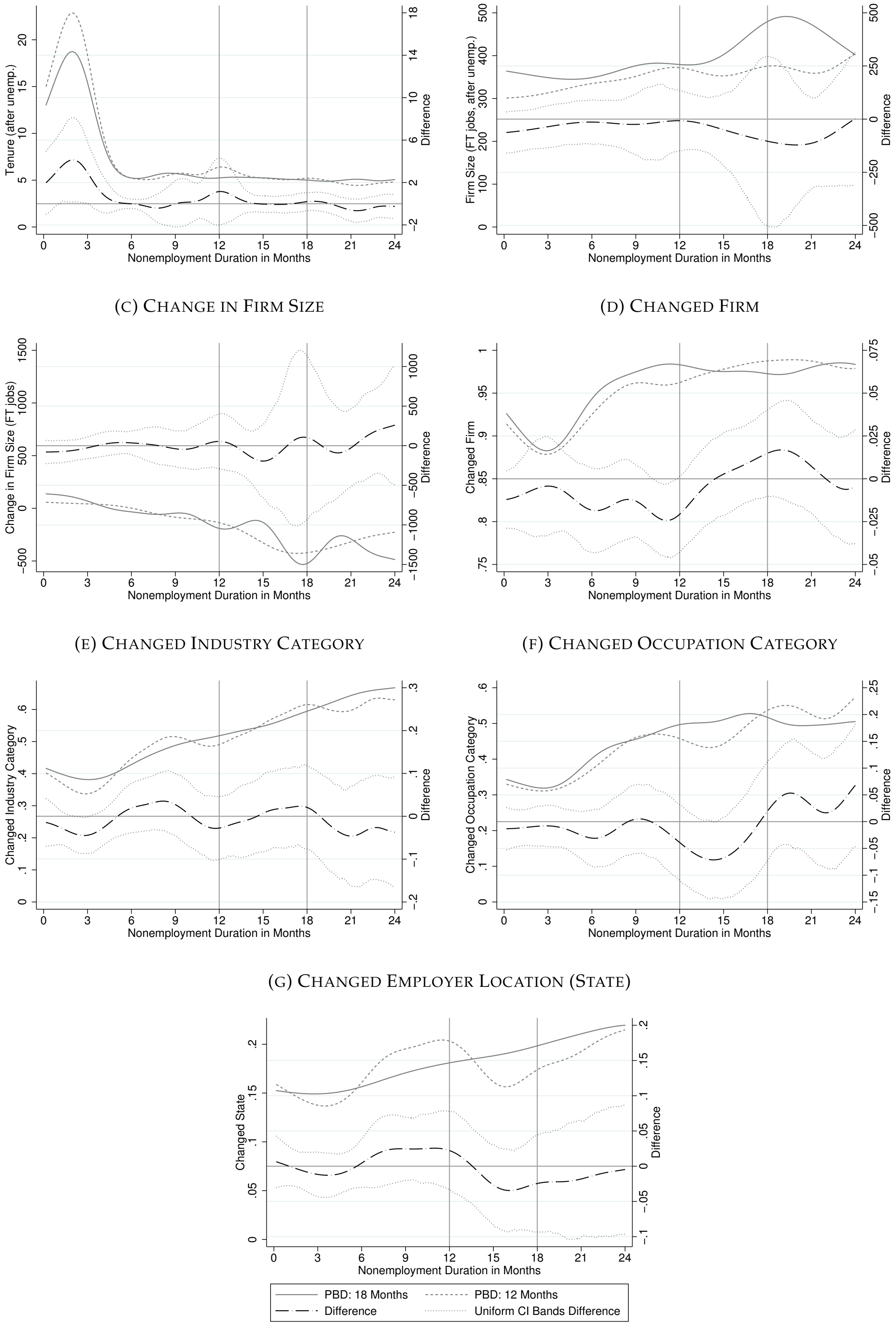
(A) FIRM FIXED EFFECT (B) JOB FIXED EFFECT

(C) RESIDUAL (D) FIRM FE + JOB FE + RESIDUAL



This figure replicates Figure 6 using differenced wage components as outcome variables. Conditional means are estimated non-parametrically by a local linear regression. Differences are the point-wise differences between the lines, which are plotted with uniform 90 percent confidence bands. All estimates condition on age. Vertical lines mark PBD exhaustion at 12 and 18 months for the control and treatment group, respectively.

FIGURE D.4
DYNAMICS OF NON-WAGE OUTCOMES OVER NONEMPLOYMENT SPELL



(A) TENURE IN POST-UNEMPLOYMENT JOB

(B) POST-UNEMPLOYMENT FIRM SIZE

(C) CHANGE IN FIRM SIZE

(D) CHANGED FIRM

(E) CHANGED INDUSTRY CATEGORY

(F) CHANGED OCCUPATION CATEGORY

(G) CHANGED EMPLOYER LOCATION (STATE)

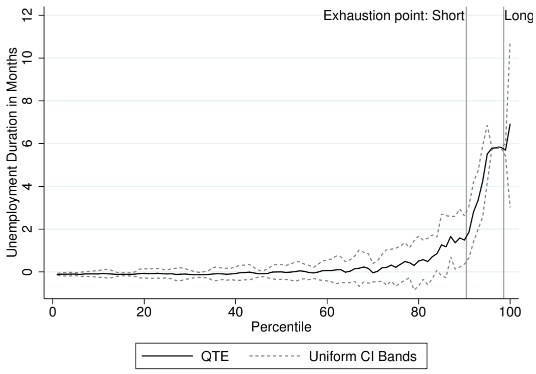
Conditional means are estimated nonparametrically by a local linear regression. Differences are the point-wise differences between the lines, which are plotted with uniform 90 percent confidence bands. Vertical lines mark PBD exhaustion at 12 and 18 months for the control and treatment group, respectively.

Supplement

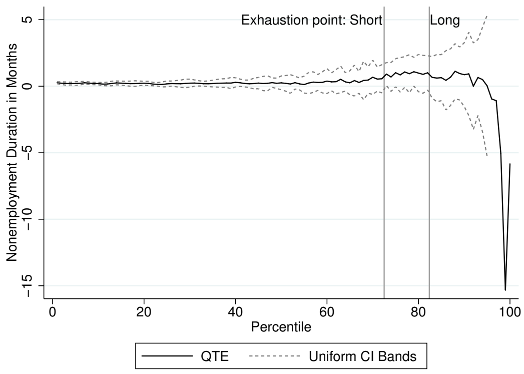
S.1 Section 5:

FIGURE S.1
QUANTILE TREATMENT EFFECTS ON DURATION AFTER PURGING THE RUNNING VARIABLE (AGE)

(A) UNEMPLOYMENT DURATION

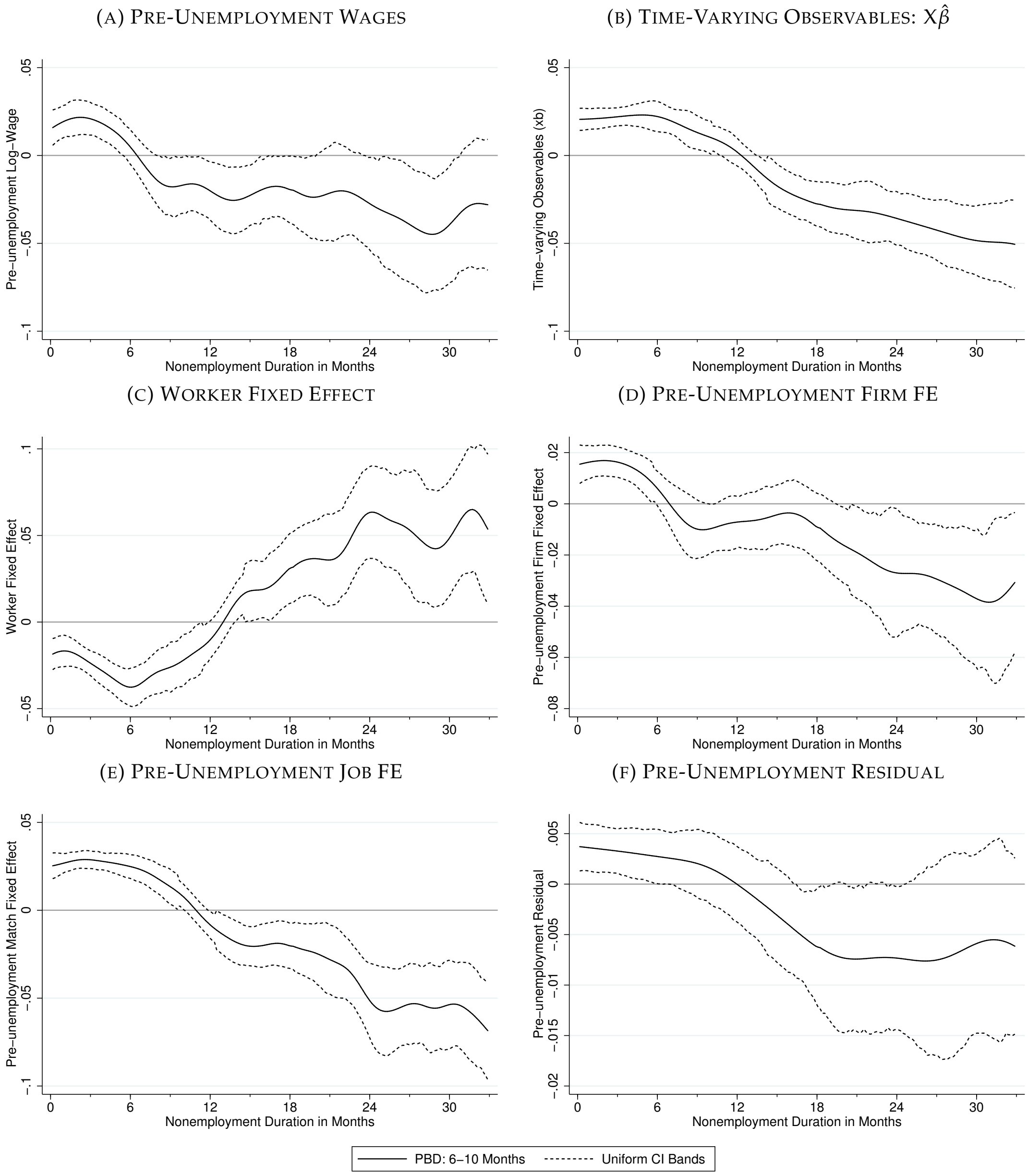


(B) NONEMPLOYMENT DURATION



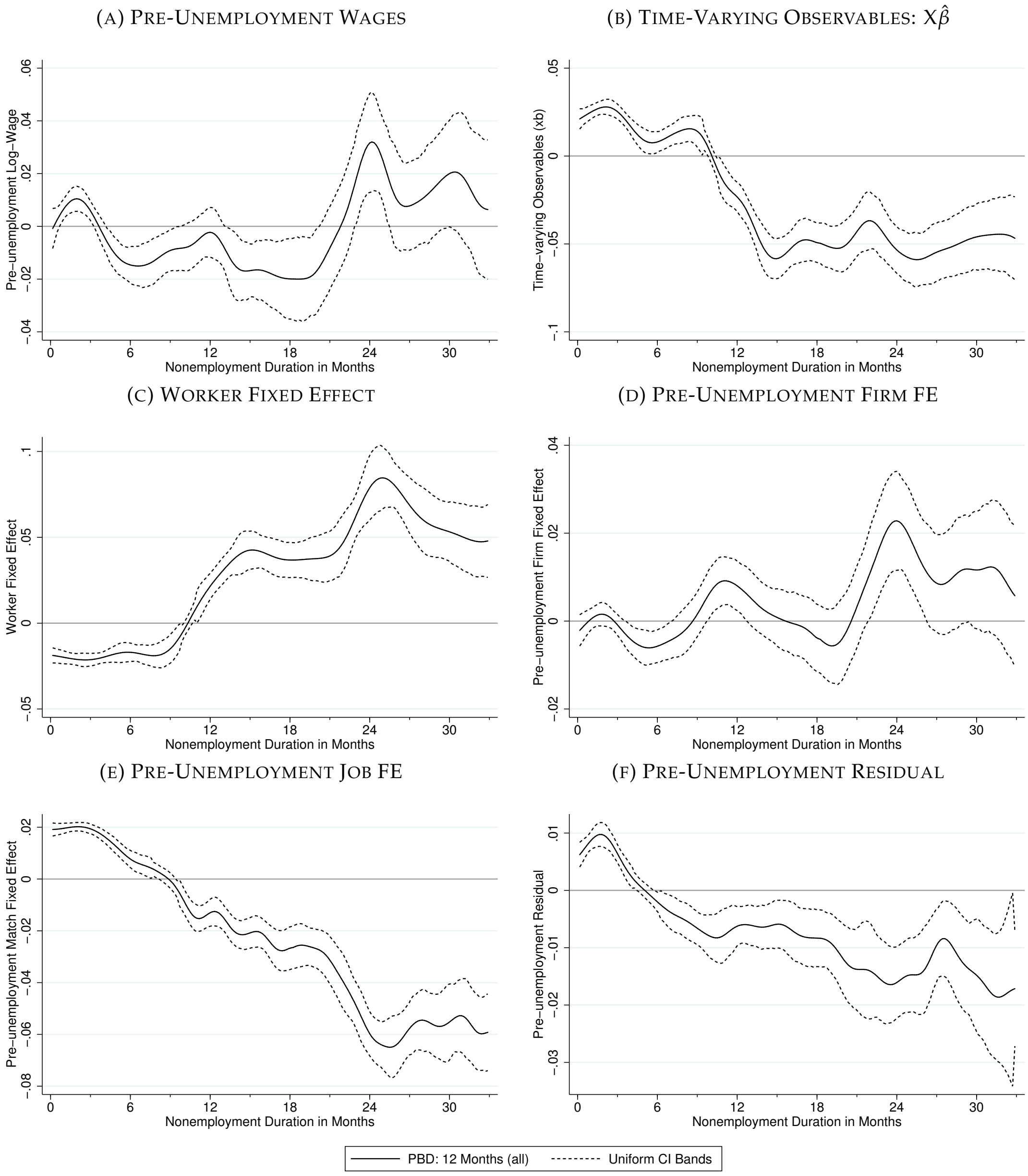
The quantile treatment effect is the horizontal distance between the cumulative distributions of the outcome in the treatment and control group. Vertical lines mark benefit exhaustion at 12 and 18 months for the control and treatment group, respectively. The confidence interval level is 90 percent.

FIGURE S.2
DYNAMIC SELECTION: 10-12 MONTHS OF PBD



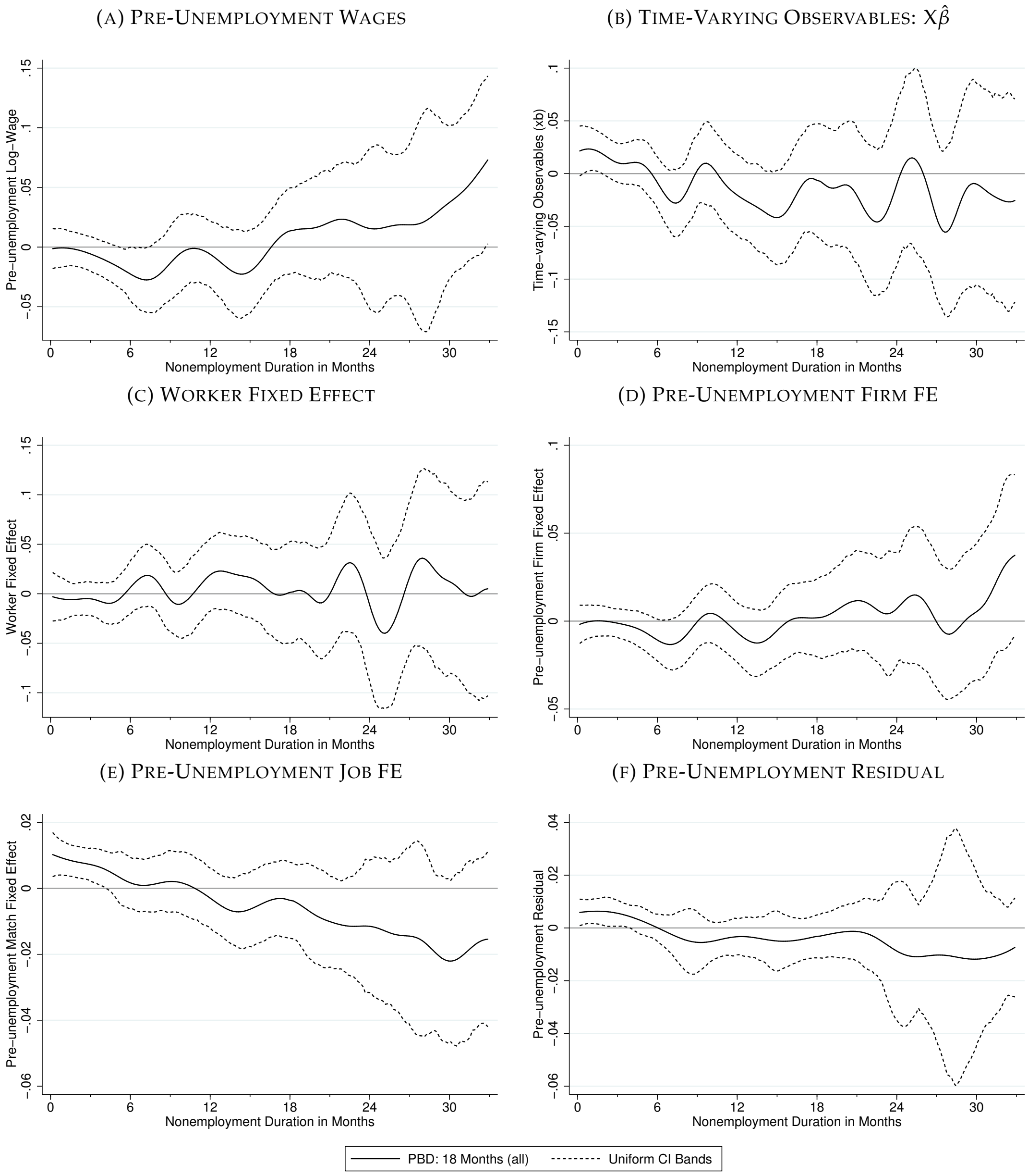
Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

FIGURE S.3
DYNAMIC SELECTION: 12 MONTHS OF PBD (ALL)



Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

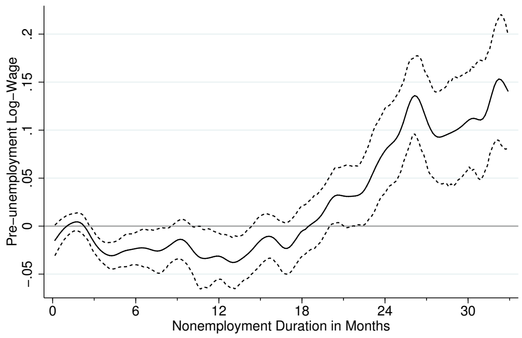
FIGURE S.4
DYNAMIC SELECTION: 18 MONTHS OF PBD (ALL)



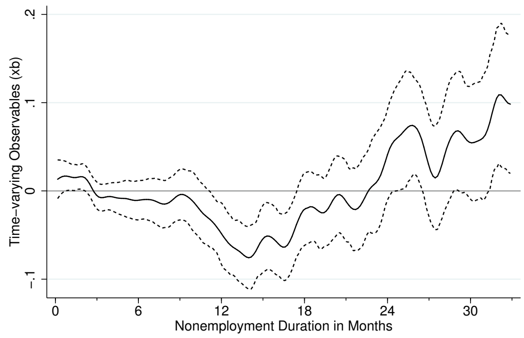
Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

FIGURE S.5
DYNAMIC SELECTION: 20-32 MONTHS OF PBD

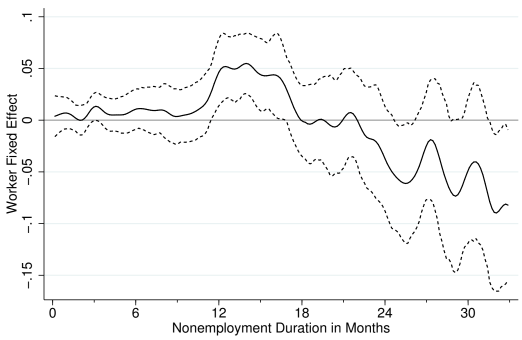
(A) PRE-UNEMPLOYMENT WAGES



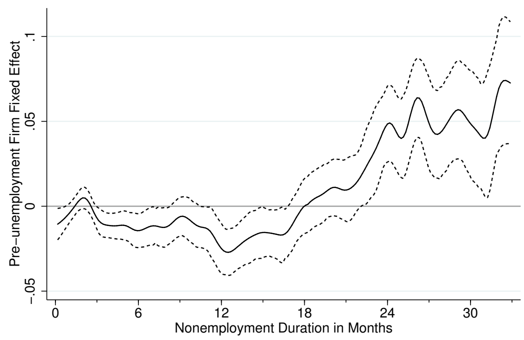
(B) TIME-VARYING OBSERVABLES: $X\hat{\beta}$



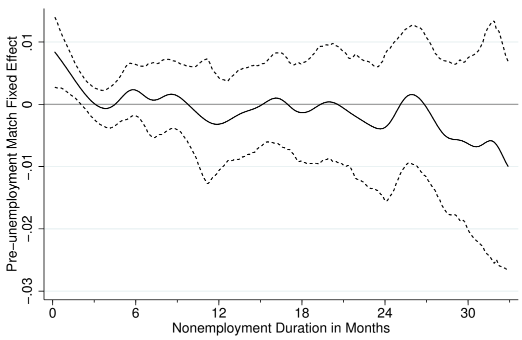
(C) WORKER FIXED EFFECT



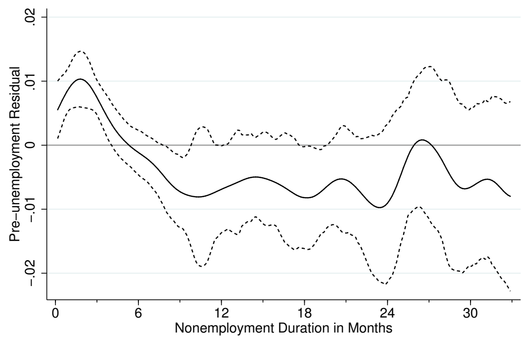
(D) PRE-UNEMPLOYMENT FIRM FE



(E) PRE-UNEMPLOYMENT JOB FE



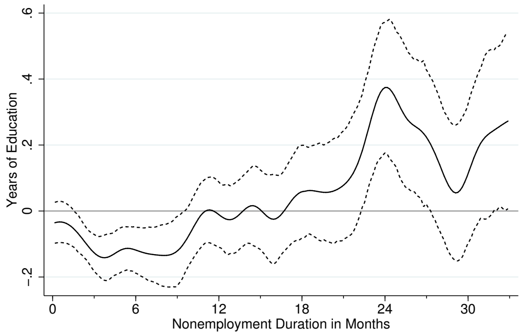
(F) PRE-UNEMPLOYMENT RESIDUAL



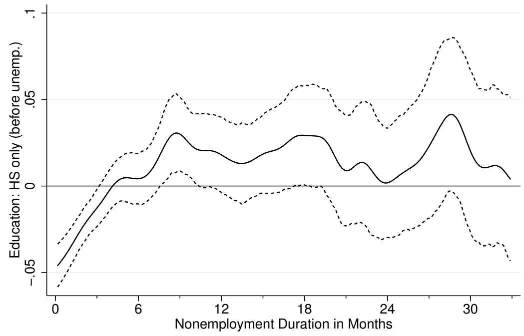
Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

FIGURE S.6
DYNAMIC SELECTION ON COVARIATES: 10-12 MONTHS OF PBD

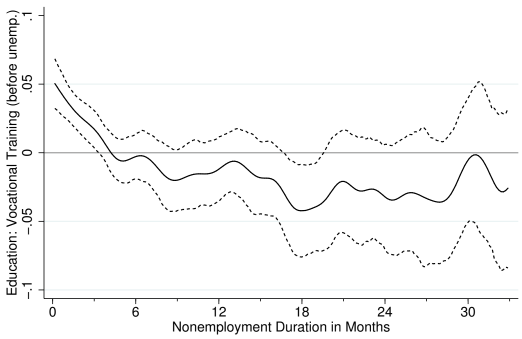
(A) YEARS OF EDUCATION



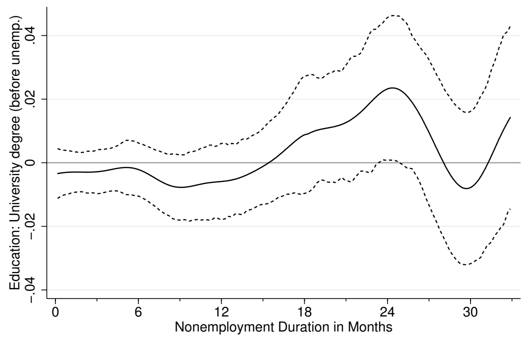
(B) HIGHEST DEGREE: HIGH SCHOOL ONLY



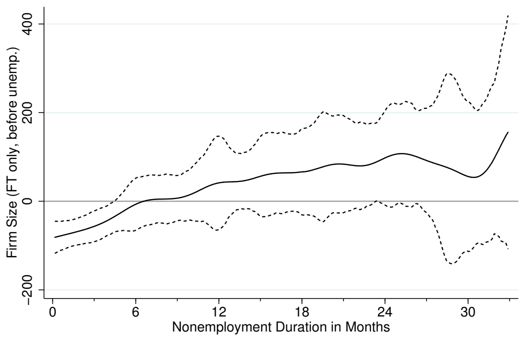
(C) HIGHEST DEGREE: VOCATIONAL TRAINING



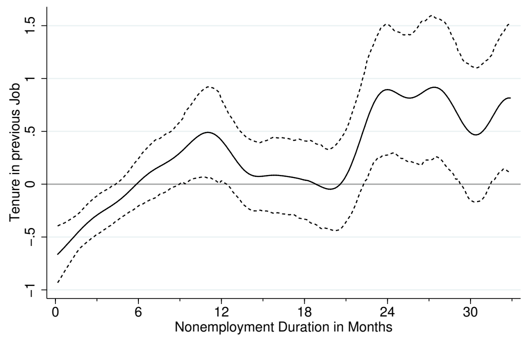
(D) HIGHEST DEGREE: SOME UNIVERSITY



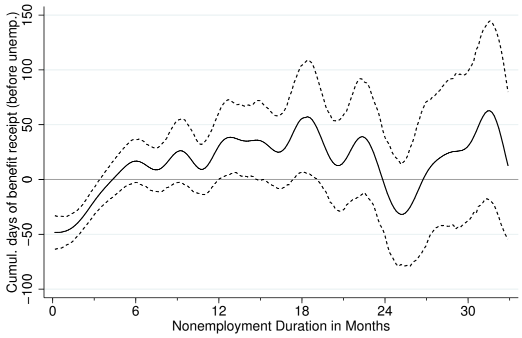
(E) PREVIOUS FIRM SIZE



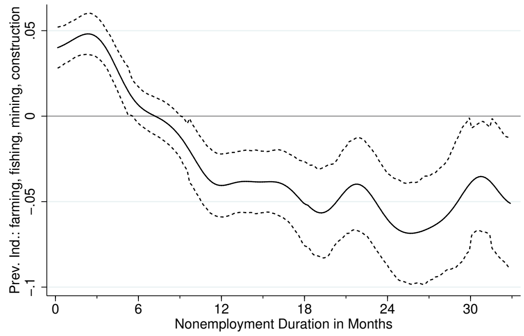
(F) TENURE IN PREVIOUS JOB



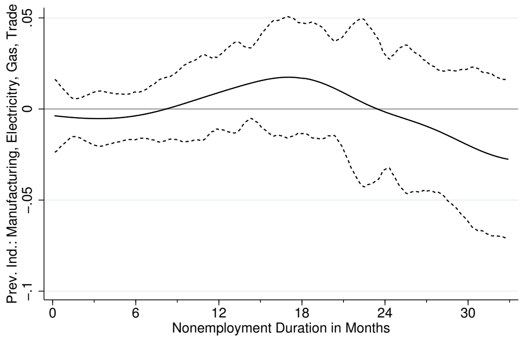
(G) DAYS OF BENEFIT RECEIPT BEFORE UNEMPLOYMENT



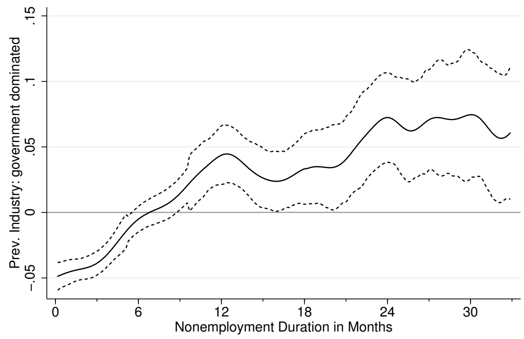
(H) INDUSTRY: FARMING, FISHING, MINING, CONSTRUCTION



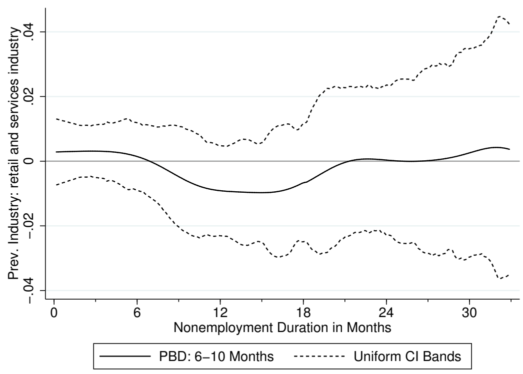
(I) INDUSTRY: RETAIL AND SERVICES



(J) INDUSTRY: GOVERNMENT DOMINATED



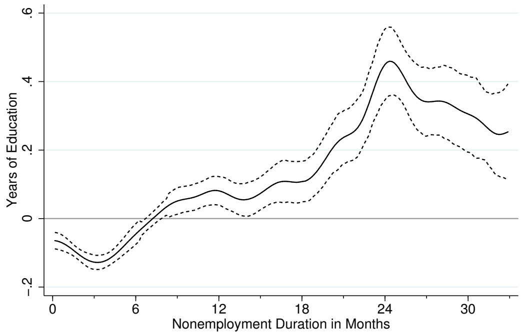
(K) INDUSTRY: MANUFACTURING, ELECTRICITRY, GAS, TRADE



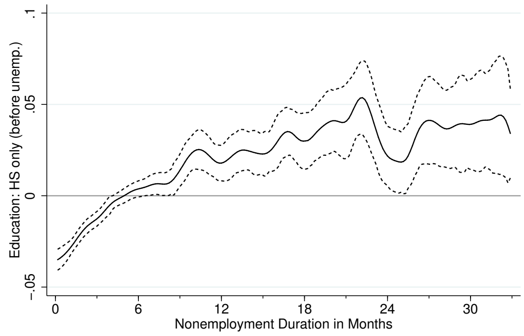
Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

FIGURE S.7
DYNAMIC SELECTION ON COVARIATES: 12 MONTHS OF PBD (ALL)

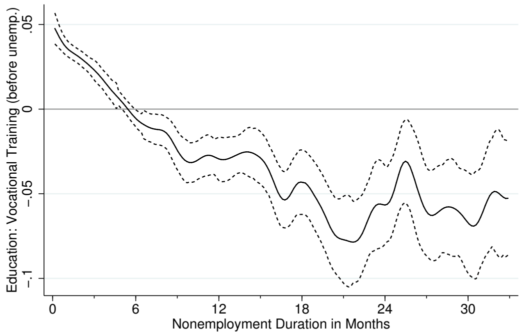
(A) YEARS OF EDUCATION



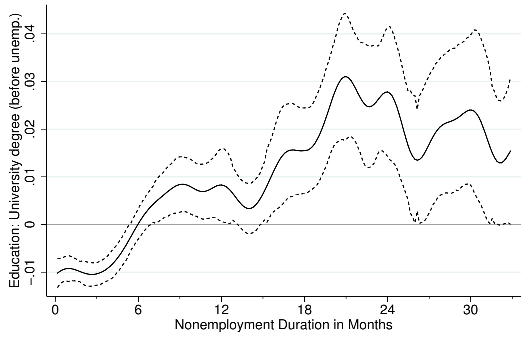
(B) HIGHEST DEGREE: HIGH SCHOOL ONLY



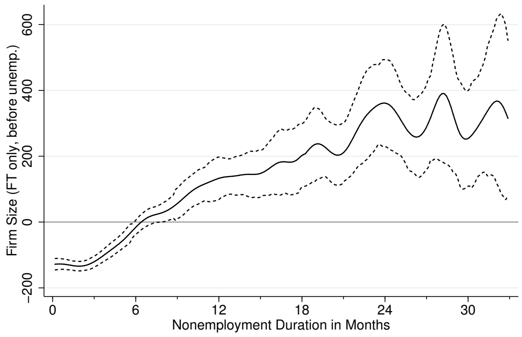
(C) HIGHEST DEGREE: VOCATIONAL TRAINING



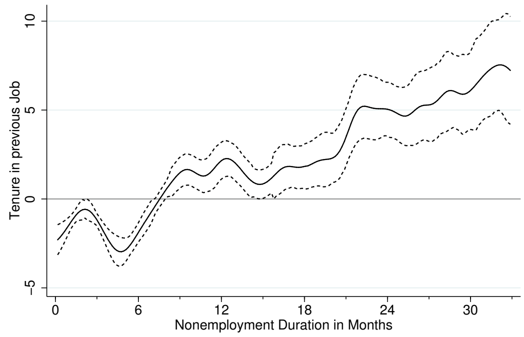
(D) HIGHEST DEGREE: SOME UNIVERSITY



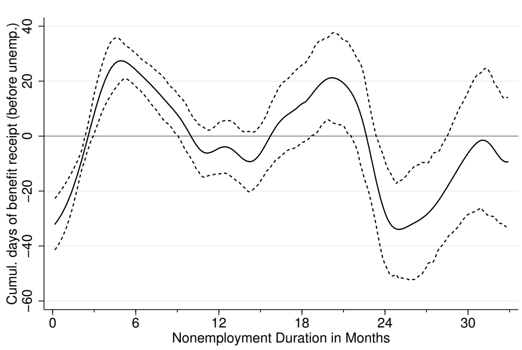
(E) PREVIOUS FIRM SIZE



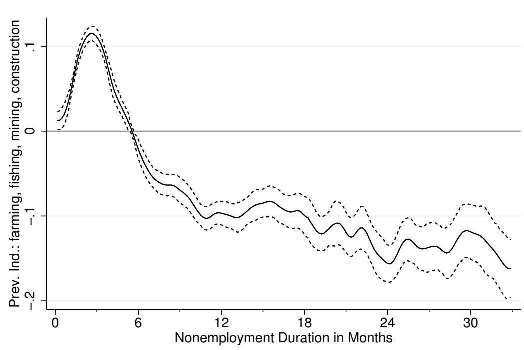
(F) TENURE IN PREVIOUS JOB



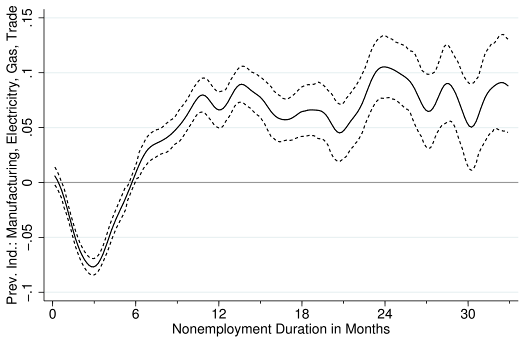
(G) DAYS OF BENEFIT RECEIPT BEFORE UNEMPLOYMENT



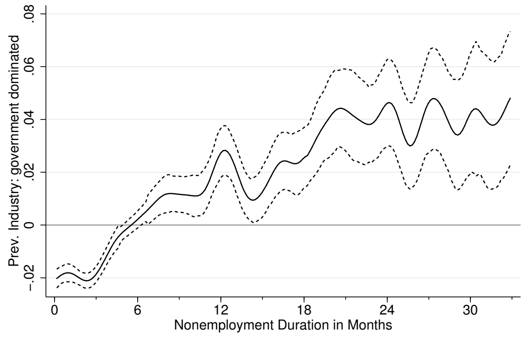
(H) INDUSTRY: FARMING, FISHING, MINING, CONSTRUCTION



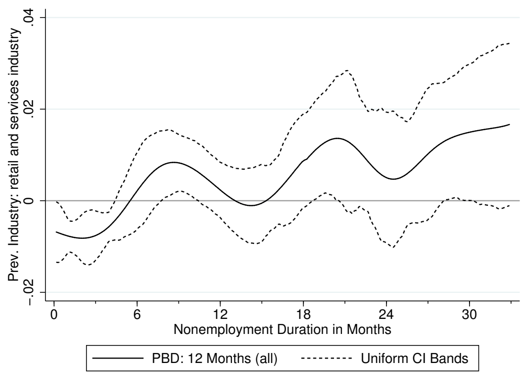
(I) INDUSTRY: RETAIL AND SERVICES



(J) INDUSTRY: GOVERNMENT DOMINATED



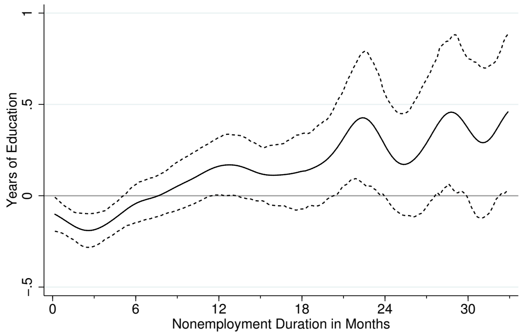
(K) INDUSTRY: MANUFACTURING, ELECTRICITRY, GAS, TRADE



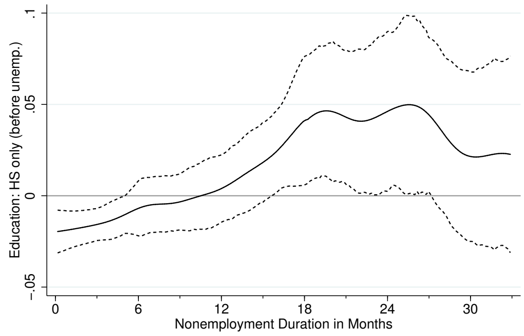
Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

FIGURE S.8
DYNAMIC SELECTION ON COVARIATES: 18 MONTHS OF PBD (ALL)

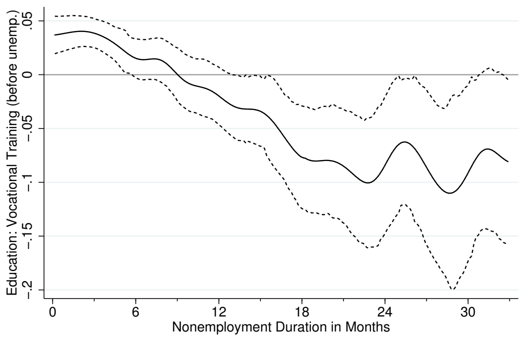
(A) YEARS OF EDUCATION



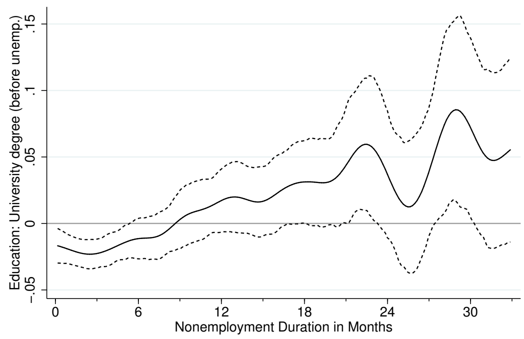
(B) HIGHEST DEGREE: HIGH SCHOOL ONLY



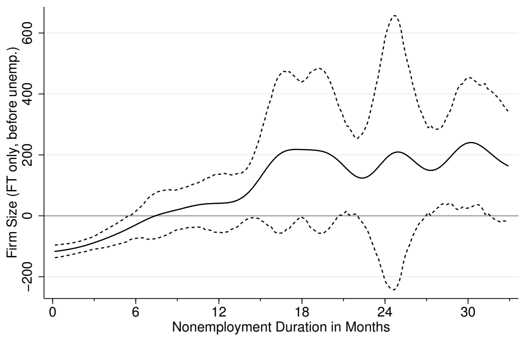
(C) HIGHEST DEGREE: VOCATIONAL TRAINING



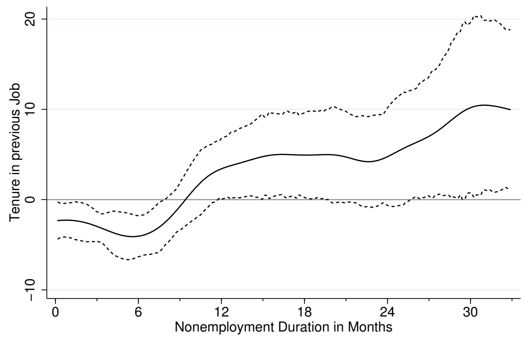
(D) HIGHEST DEGREE: SOME UNIVERSITY



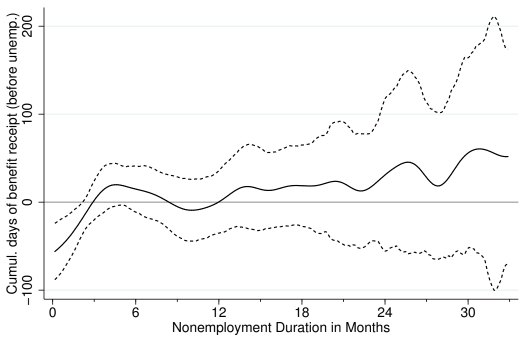
(E) PREVIOUS FIRM SIZE



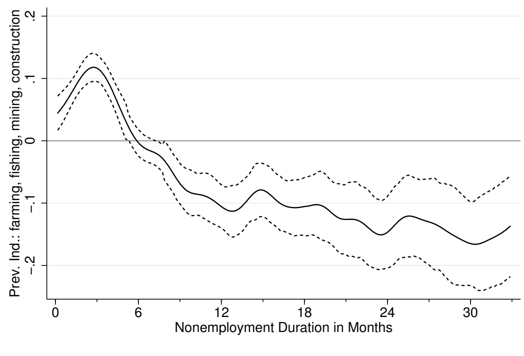
(F) TENURE IN PREVIOUS JOB



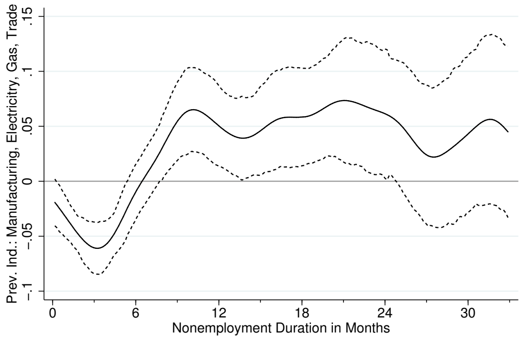
(G) DAYS OF BENEFIT RECEIPT BEFORE UNEMPLOYMENT



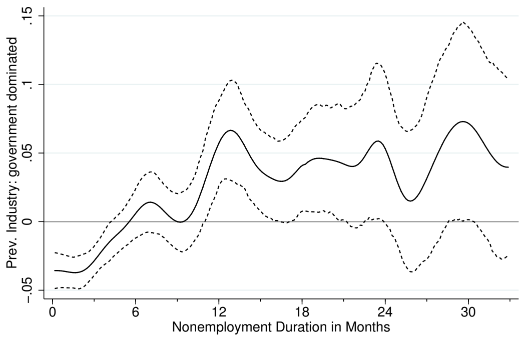
(H) INDUSTRY: FARMING, FISHING, MINING, CONSTRUCTION



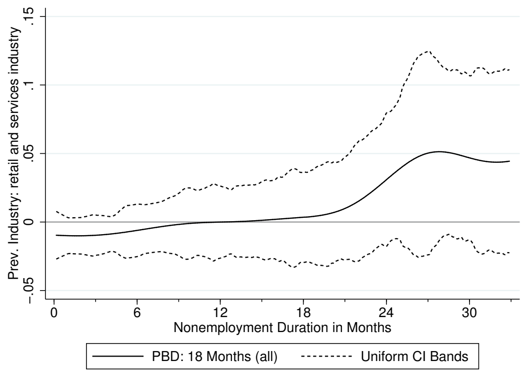
(I) INDUSTRY: RETAIL AND SERVICES



(J) INDUSTRY: GOVERNMENT DOMINATED



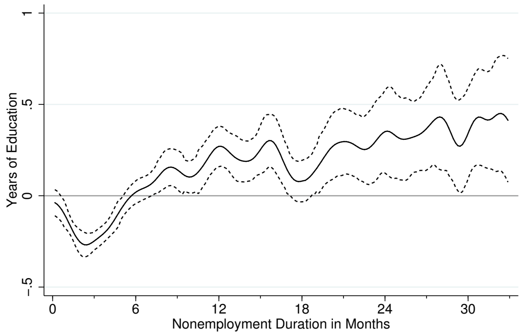
(K) INDUSTRY: MANUFACTURING, ELECTRICITRY, GAS, TRADE



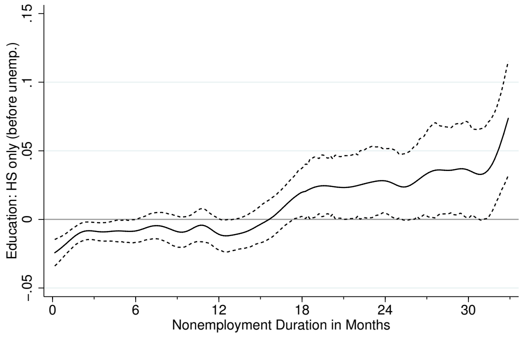
Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

FIGURE S.9
DYNAMIC SELECTION ON COVARIATES: 20-32 MONTHS OF PBD

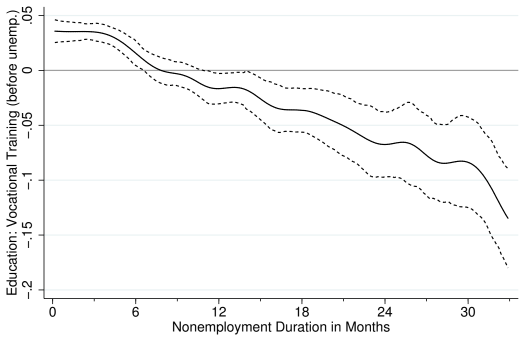
(A) YEARS OF EDUCATION



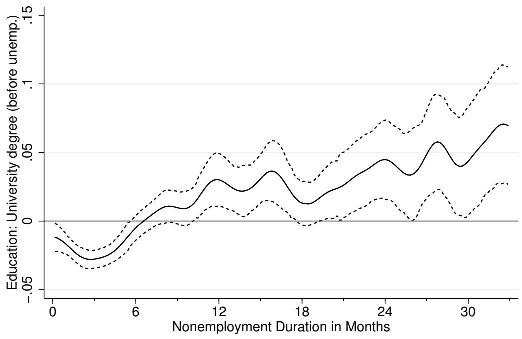
(B) HIGHEST DEGREE: HIGH SCHOOL ONLY



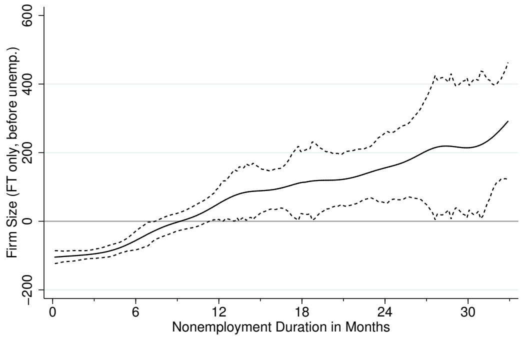
(C) HIGHEST DEGREE: VOCATIONAL TRAINING



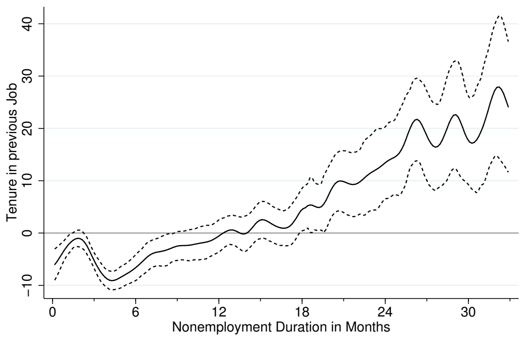
(D) HIGHEST DEGREE: SOME UNIVERSITY



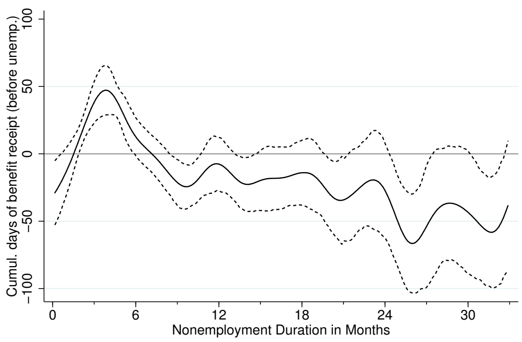
(E) PREVIOUS FIRM SIZE



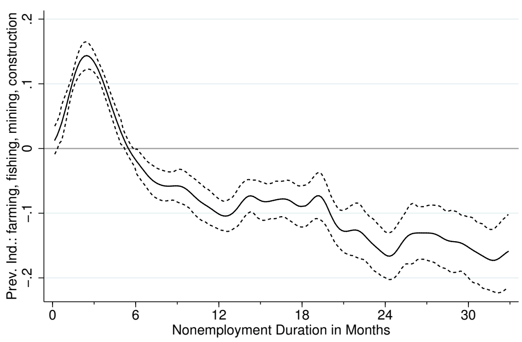
(F) TENURE IN PREVIOUS JOB



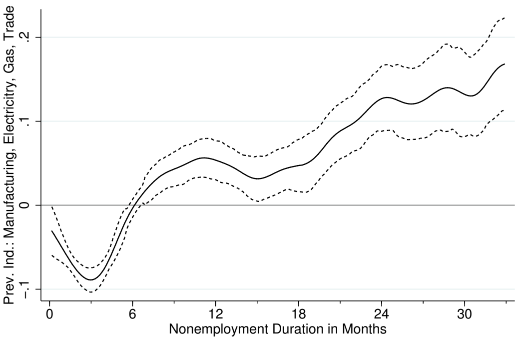
(G) DAYS OF BENEFIT RECEIPT BEFORE UNEMPLOYMENT



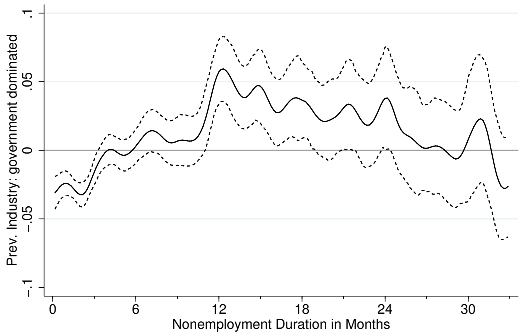
(H) INDUSTRY: FARMING, FISHING, MINING, CONSTRUCTION



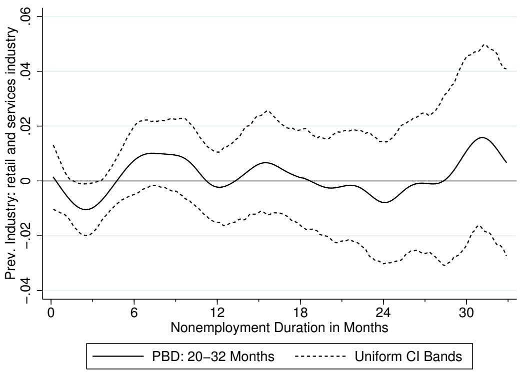
(I) INDUSTRY: RETAIL AND SERVICES



(J) INDUSTRY: GOVERNMENT DOMINATED

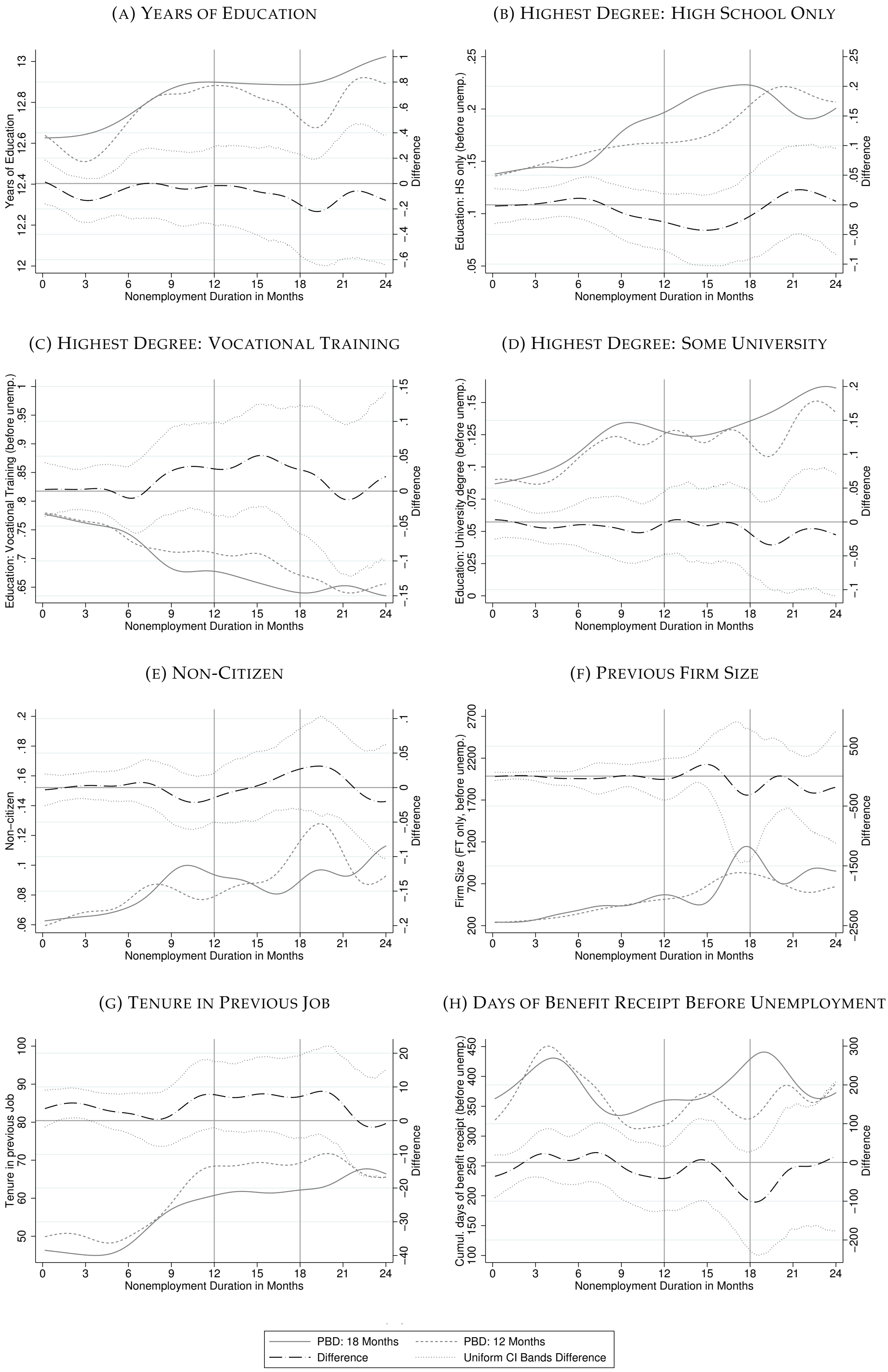


(K) INDUSTRY: MANUFACTURING, ELECTRICITRY, GAS, TRADE



Solid lines plot the deviation of the conditional mean of the respective outcome from the overall mean as defined by Equation (9). Conditional means are estimated nonparametrically by a local linear regression. Dashed lines provide 90 percent confidence bands.

FIGURE S.10
DYNAMICS OF PRE-DETERMINED COVARIATES OVER THE NONEMPLOYMENT SPELL

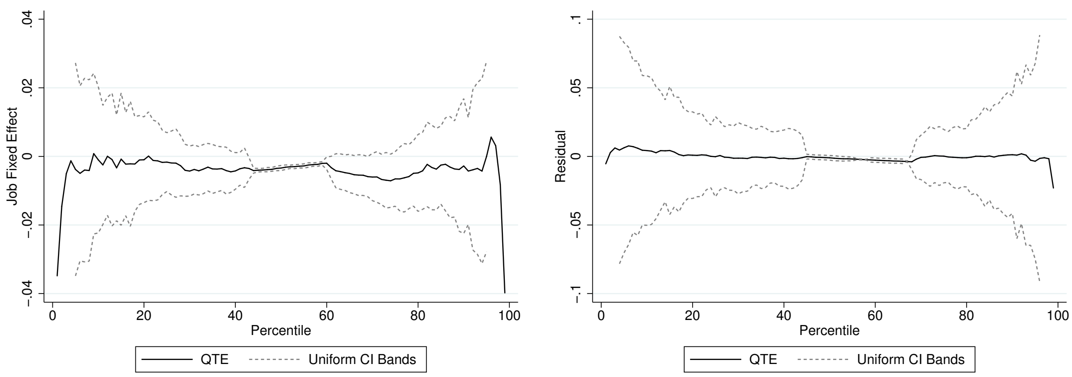


Conditional means are estimated nonparametrically by a local linear regression. Differences are the point-wise differences between the lines, which are plotted with uniform 90 percent confidence bands. Vertical lines mark PBD exhaustion at 12 and 18 months for the control and treatment group, respectively.

S.2 Section 6:

FIGURE S.11
QUANTILE TREATMENT EFFECTS ON JOB FIXED EFFECT AND RESIDUAL

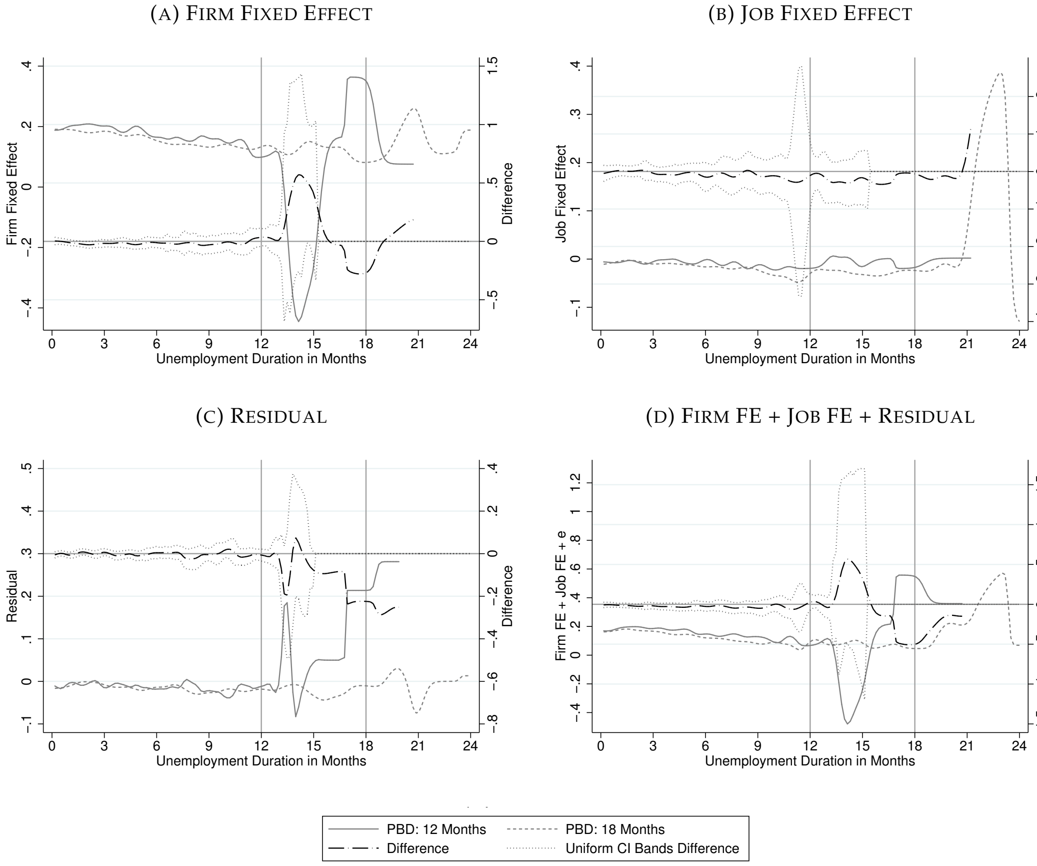
(A) JOB FIXED EFFECT (B) RESIDUAL



The quantile treatment effect is the horizontal distance between the cumulative distributions of the outcome in the treatment and control group. All outcomes are age adjusted to purge the effect of the running variable. The confidence interval level is 90 percent. Note that the distributions of both outcomes have point mass at 0, resulting in a flat and precisely estimated QTE (that is tilted slightly due to the age adjustment).

FIGURE S.12

DYNAMICS OF WAGE COMPONENTS OVER UNEMPLOYMENT SPELL



This figure replicates Figure 6 using unemployment instead of nonemployment duration. Conditional means are estimated nonparametrically by a local linear regression. Differences are the point-wise differences between the lines, which are plotted with uniform 90 percent confidence bands. All estimates are conditional on age. Vertical lines mark PBD exhaustion at 12 and 18 months for the control and treatment group, respectively.