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Abstract

This paper analyzes the effects of unemployment insurance (UI) benefits on unemployment exits and subsequent labor market outcomes. We exploit a piecewise linear relationship between the previous wage and UI benefits in Finland to identify the causal effects of the benefit level by using a regression kink design. According to our findings, higher benefits lengthen nonemployment spells and decrease time spent in part-time unemployment, and thus result in more full-time unemployment. Also the re-employment probability and post-unemployment wage are negatively affected. The results for the duration of the first post-unemployment job are not conclusive, but in total both employment and earnings in the two years following the beginning of the unemployment spell decrease with higher benefits.

Key words: Unemployment duration, job match quality, unemployment insurance, regression kink design

JEL classes: J64, J65

1 Introduction

There is a vast empirical literature showing that more generous unemployment insurance (UI) benefits prolong unemployment (see Tatsiramos and van Ours, 2014 for a survey). However, more generous UI benefits may also have favorable effects by, for example, improving subsequent job matches. Job seekers with more generous benefits can search longer for a job that matches their skills and may, therefore, find more stable and better paid jobs (Ehrenberg and Oaxaca, 1976; Marimon and Zilibotti, 1999; Acemoglu and Shimer, 2000). On the other hand, if human capital depreciates during unemployment or if employers discriminate against applicants based on their unemployment history, the effect of generous UI benefits on match quality can also be negative. Empirical evidence to date is mixed and it is unclear which effect dominates, i.e. do more generous benefits improve or impair match quality. This is an important topic because longer unemployment spells caused by higher benefits are more (less) acceptable when they lead to better (worse) matches between job seekers and vacant jobs.

In this study, we find that higher UI benefits prolong nonemployment duration and *decrease* the post-unemployment wage rate. As such, the effect of the benefit level on labor market prospects over a longer time period is unambiguously negative. We reach this conclusion using a regression kink design and rich register-based data covering the entire population of unemployed workers in Finland. Our research design exploits the relationship between the previous wage and UI benefits. The piecewise linear benefit rule allows us to identify the causal effect of the benefit level on various outcomes (see Card et al., 2015, and references therein).

Our findings indicate that higher UI benefits prolong nonemployment duration with an elasticity around 1.5 to 2. We also examine the effect of the UI benefit level on the duration of UI benefit receipt, but the results are not conclusive. We find that higher UI benefits lead to a decrease in the share of days spent on partial unemployment benefits, i.e. in subsidized part-time or temporary jobs. The elasticity of the share of partial unemployment days in the UI spell with respect to the benefit level is quite large in absolute value, approximately -5 in most cases, but the average share of partial unemployment days is low to begin with, implying a modest absolute effect. According to our results, the probability that the UI spell ends in employment decreases with a higher benefit level, with an elasticity around -0.5. Higher benefits also reduce the wage in the first job after unemployment with an elasticity of around -0.5 to -1. On the other hand, the estimated elasticity of the duration of the next job with respect to the benefit level is in general positive, which is somewhat surprising considering our results for the wage rate. The estimates for job duration are, however, very imprecise and hence essentially uninformative.

To assess the overall effect of UI benefits we consider cumulative working days and earnings in the two years following the beginning of the unemployment spell. We find that earnings decrease with higher UI benefits with an elasticity of -1 to -2. This earnings effect is influenced by decreasing working days as we find that the elasticity of the number of working days in the following two years with respect to the UI benefit level is -0.5 to -1. The finding that higher UI benefits decrease subsequent working days is obviously at least in part driven by potentially longer nonemployment spells and is consistent with our observation that higher benefits lead to less part-time and temporary employment. All in all, the overall effect of UI benefits on labor market outcomes over the period of two years is negative.

As in previous regression kink design studies, our results are quite sensitive to the choices of bandwidth and polynomial order. Since no single optimal procedure to make such choices exists, we report a range of nonparametric estimates based on local linear and quadratic specifications using various bandwidth selectors. In addition, we use a more parametric approach with additional covariates and larger samples to increase efficiency. The negative effect of the UI benefit level on the share of days spent on partial unemployment benefits is robust to changes in the specification and bandwidth, as are the effects on post-unemployment earnings. The results for the other outcomes are more sensitive to changes in the estimation method.

Our paper contributes to the literature on the effects of UI generosity on unemployment and post-unemployment outcomes. Our estimates for the effects of the UI benefit level on nonemployment duration are quite imprecise and large compared to the majority of previous elasticity estimates, but are in line with results from Sweden (Carling et al., 2001). Estimates for the elasticity of unemployment duration with respect to the benefit level from previous studies using a regression kink design have also been high in comparison to those usually found in the literature (Card et al., 2015).

Previous empirical evidence on the effects of the benefit level on subsequent labor market outcomes is scarce and the results are mixed.[1] Addison and Blackburn (2000) find that higher UI benefits have hardly any effect on subsequent wages in the US labor market, but Centeno (2004) shows that higher benefits increase the duration of the subsequent employment spell. Ek (2013) finds evidence that higher UI benefits decrease annual earnings and monthly wages in Sweden, while the probability of re-employment and em-

[1]The studies that consider the effects of UI on match quality have mostly analyzed the impacts of potential benefit *duration*. The results of these studies are also mixed, with some studies finding a positive association between benefit duration and post-unemployment job quality in terms of either higher wages or job stability (e.g. Tatsiramos, 2009; Centeno and Novo, 2009; Gaure et al., 2008; Nekoei and Weber, 2015) and others showing negative or no effects of longer benefit durations on match quality (e.g. Degen and Lalive, 2013; Lalive, 2007; Caliendo et al., 2013, Card et al., 2007, van Ours and Vodopivec, 2008; Le Barbanchon, 2016, Schmieder et al. 2016).

ployment durations do not appear to be affected. Using Spanish data, Rebollo-Sanz and Rodriguez-Planas (2016) find no effect on post-unemployment wages and no decrease in other measures of match quality.

Our results are in line with the Swedish evidence on post-unemployment earnings and contrary to previous research, indicate that also the re-employment probability and working days in the next two years are affected negatively by a higher UI benefit level. Previous studies have not examined the effect of the benefit level on time spent in partial unemployment. Our finding that higher UI benefits decrease the share of days in subsidized part-time and temporary employment during the UI spell provides new evidence on a potential mechanism through which the generosity of UI benefits can affect subsequent labor market outcomes.

The rest of the paper proceeds as follows. The next section describes the Finnish UI system during the period under investigation. This is followed by a section discussing our identification strategy and estimation procedures. Section 4 introduces our data and section 5 contains graphical evidence. Section 6 discusses our estimation results. The final section concludes.

2 Institutional framework

In Finland, earnings-related UI benefits are paid by unemployment funds, most of which are organized along the industry or occupation lines, and administrated by labor unions. Membership is voluntary, but as many as 85% of all workers are enrolled in unemployment funds (Uusitalo and Verho, 2010). A worker who registers as an unemployed job seeker at the public employment agency is entitled to 500 days of UI benefits provided that he or she has been a member of an unemployment fund for at least 10 months (membership condition) and has worked for at least 34 weeks during the past 28 months (employment condition). The benefits are paid for 5 days a week, so the maximum benefit duration is 100 calendar weeks. If the UI recipient leaves unemployment without exhausting his or her benefits, and then returns to unemployment before satisfying the employment condition again, he or she will be entitled to unused UI benefits from the previous spell (given that he or she did not leave the labor market for a period longer than 6 months without an acceptable reason). Those who exhaust their UI benefits can claim a means-tested, flat-rate labor market subsidy, which is paid by the Social Security Institution for an indefinite period.[2]

[2]Those unemployed who do not belong to an unemployment fund but satisfy the employment condition are eligible for a flat-rate basic allowance which is the same amount as the labor market subsidy but is not means-tested and is paid for a period of 500 days. In practice, this benefit type is of minor importance and their recipients are not covered in our analysis.

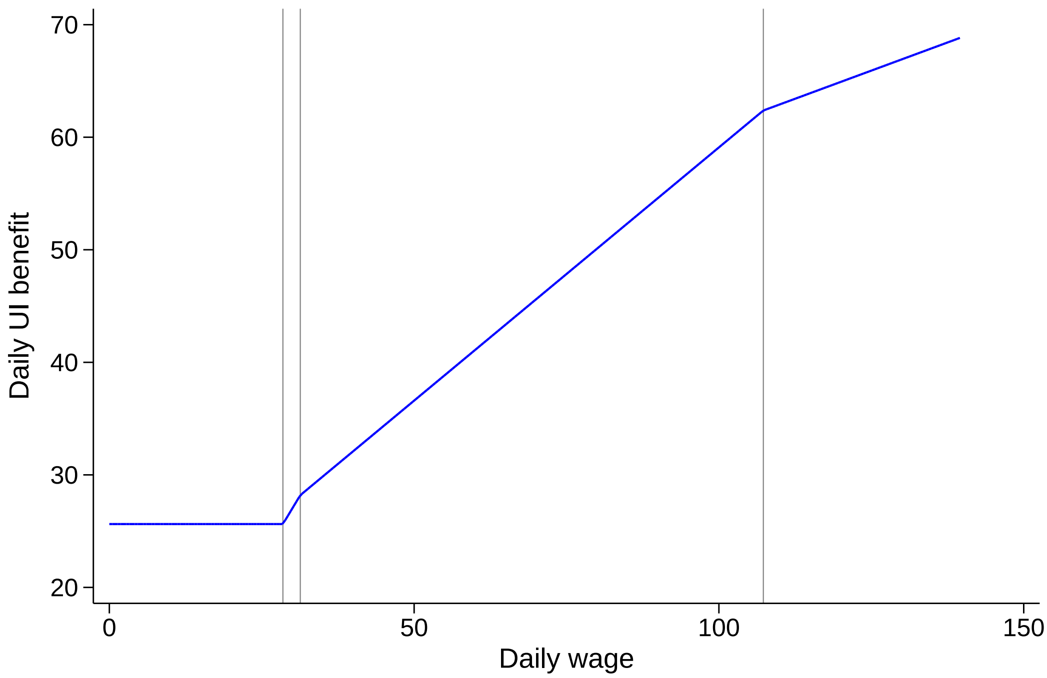

Figure 1: Daily wage and UI benefit level (EUR)

Individuals who participate in labor market training programs receive a labor market training subsidy. Because this subsidy equals the unemployment benefit the worker would have otherwise received plus a daily allowance for maintenance and possibly for accommodation, we make no distinction between earnings-related labor market training subsidies and UI benefits in our analysis. Furthermore, an unemployed worker who takes up a part-time job (or a very short full-time job) does not necessarily lose his or her benefits entirely but may be entitled to a reduced amount of benefits. In exchange for these partial benefits, the worker is expected to continue his or her search for full-time employment. The entitlement period for a worker on partial UI benefits elapses at a reduced rate proportional to the ratio of the partial benefit to full-time benefit. Due to part-time unemployment and labor market training, UI recipients can collect earnings-related benefits longer than 500 days.

The UI benefit consists of a basic component equal to the full amount of the labor market subsidy and an earnings-related component. The latter is 45% of the difference between the previous daily wage (the monthly earnings divided by 21.5) and the basic daily component up to a previous daily wage of 107 EUR (in 2009). There is no cap on the benefit level but daily wages exceeding 107 EUR increase the benefit by only 20% of the exceeding amount. The daily benefit cannot exceed 90% of the underlying daily wage which restricts the benefit amount at low levels of earnings. Figure 1 illustrates the relationship between UI benefits and previous daily wage in 2009. The first vertical line corresponds to the basic component, and between the first and second vertical lines the afore mentioned rule of max 90% replacement ratio is in effect. The third vertical line corresponds to the daily wage of 107 EUR with wages exceeding this level increasing benefits by only 20% of the exceeding amount.[3]

There are a few exceptions in the benefit rules described above. First, workers with at least 20 years of employment history who have been a member of an unemployment fund for at least five years and who were dismissed without cause can receive a higher benefit for up to 185 days. Second, starting in 2005 workers with at least three years of employment history who were dismissed without cause or who worked for the same employer under fixed-term contracts for at least 36 months within the past 42 months have had an option to enroll in an employment program. Participants of this program are entitled to higher UI benefits for 20 days and a higher labor market training subsidy for the duration of training programs that are specified in an individual-specific action plan. Finally, workers aged 59 or more (57 or more for those born before 1950) on the day when regular UI benefits expire are entitled to extended UI benefits until retirement. We do not consider these groups of workers with differing benefit schedules in our analysis.

3 Statistical methods

3.1 Identification

To identify the effect of UI benefits we take advantage of the kink in the benefit rule that determines the benefit level as a function of past daily wage (i.e the change in the slope at 107 EUR in figure 1). The basic idea is that a kink in the relationship between the outcome variable (e.g. unemployment duration) and the past wage at the kink point of the benefit rule is indicative of the causal effect of benefits under the identifying assumption that the direct effect of past wage on the outcome is smooth at that point. This approach is known as "regression kink design" (RKD) due to Nielsen et. al (2010), and it is a close cousin of the regression discontinuity design. While the regression discontinuity design identifies the causal effect from a *jump* in the average outcome associated with a jump in the policy variable, the regression kink design identifies the causal effect from a *kink* in the average outcome associated with a kink in the policy variable.

[3] There is a fixed supplement to the daily benefit corresponding to the number of dependent children. The benefit increases stepwise for one, two and three or more children, without affecting the size of the kink at 107 EUR.

To fix ideas, consider the following stylized model

$$Y = \alpha + \tau B + \varepsilon, \tag{1}$$

where Y is an outcome (e.g. unemployment duration or post-unemployment earnings), $B = b(W)$ is the daily UI benefit, which is a deterministic function of the previous daily wage W with a kink at $W = w^*$, and ε is an error term. The parameter of interest is τ, the causal effect of the UI benefit on the outcome Y. Because both Y and W are labor market outcomes and presumably affected by the same unobserved characteristics, the unemployed who received different wages on their previous jobs are likely to have different expected Y due to unobserved factors, and therefore $E(\varepsilon|W) \neq 0$. Since B is a function of W, the OLS estimate of τ from (1) would be biased due to the endogeneity of B. To deal with this problem, we can augment the model by adding a "control function" defined as $g(W) \equiv E(\varepsilon|W)$:

$$Y = \alpha + \tau B + g(W) + \upsilon, \tag{2}$$

where B and W are mean-independent of the new error term υ by construction. However, the effect of B cannot be distinguished from the direct effect of W without further assumptions. Nielsen et al. (2010) show that if $g(\cdot)$ is continuously differentiable without having a kink at $W = w^*$, then

$$\tau = \frac{\lim_{w \downarrow w^*} dE(Y|W = w)/dw - \lim_{w \uparrow w^*} dE(Y|W = w)/dw}{\lim_{w \downarrow w^*} b'(w) - \lim_{w \uparrow w^*} b'(w)}. \tag{3}$$

The RKD estimand, the right-hand side of (3), equals the ratio of the change in the slope of the conditional expectation of the outcome variable to the change in the slope of the deterministic benefit rule at the cutoff w^*. Thus, despite the endogeneity of the UI benefit, its causal effect is identified without any assumptions about $g(\cdot)$ except the smoothness.

Given the result in (3) we could estimate τ by regressing Y on B while controlling for the direct effect of W using some flexible but smooth function. Alternatively, we can invoke the relationship in (3) directly. This latter approach is more general as it does not hinge on the assumption that the regression function is additively separable. Namely, Card et al. (2015) show that the RKD estimand can be interpreted as the average treatment effect in a more general, nonseparable model of the form

$$Y = y(B, W, \varepsilon), \tag{4}$$

which allows for unrestricted heterogeneity in the effect of B. They show that for this model the RKD estimand identifies

$$E\left(\left.\frac{\partial y(b^*, w^*, \varepsilon)}{\partial b}\right| B = b^*, W = w^*\right), \tag{5}$$

where $b^* = b(w^*)$ and the expectation is taken with respect to the conditional distribution of ε given $B = b^*$ and $W = w^*$. This parameter is known as "the treatment on the treated" (Florens et al. 2008) or "local average response" (Altonji and Matzkin 2005), and it equals the average effect of a marginal increase in b at the point (b^*, w^*) holding fixed the conditional distribution of unobservable characteristics.

3.2 Estimation

Card et al. (2015, 2016) discuss nonparametric inference using local polynomial regressions. Since the denominator of the RKD estimand is known in our case, we only need an estimate of the numerator. The nonparametric estimation of the conditional expectation of the outcome variable amounts to solving $\left(\alpha^-, \beta_p^-\right)$ and $\left(\alpha^+, \beta_p^+\right)$, $p = 1, 2, \ldots P$, by minimizing the objective functions

$$\sum_{i \in \Omega^-} \left(Y_i - \alpha^- - \sum_{p=1}^{P} \beta_p^- (w_i - w^*)^p\right)^2 K\left(\frac{w_i - w^*}{h}\right)$$

and

$$\sum_{i \in \Omega^+} \left(Y_i - \alpha^+ - \sum_{p=1}^{P} \beta_p^+ (w_i - w^*)^p\right)^2 K\left(\frac{w_i - w^*}{h}\right)$$

where P is the order of the polynomial function, $K(\cdot)$ is a kernel function, h is a bandwidth, Ω^- and Ω^+ are the set of observations below and above the wage cutoff w^* respectively. An estimate for the average local treatment effect is obtained by dividing the estimate of $\beta_1^+ - \beta_1^-$, the numerator of the RKD estimand, with the change in the slope of the benefit rule at w^*.

If the uniform kernel is used, which is the leading choice in the applied work, the estimation problem reduces to OLS estimation of the model

$$E(Y|W = w) \;=\; \alpha + \delta_0 D + \sum_{p=1}^{P} \left[\beta_p (w - w^*)^p + \delta_p D (w - w^*)^p\right], \tag{6}$$

where $D = 1\{w > w^*\}$ is an indicator for observations with the previous wage above the cutoff, using a subsample of observations in a neighborhood of the cutoff that satisfy the condition $|w - w^*| \leq h$. Because δ_1 is the change in the slope of the conditional expectation of Y at w^*, we can obtain an estimate of τ by dividing the OLS estimate of δ_1 with the change in the slope of the benefit rule at w^*.

In addition to the kernel function, we also need to choose the bandwidth h and the polynomial order P. The bandwidth is a trade-off between the precision of the estimates and accuracy of the polynomial approximation to the unknown underlying expectation function. Several competing bandwidth selector methods have been proposed. Calonico et al. (2014) argue that the commonly used bandwidth selectors tend to yield bandwidths that are too large to ensure the validity of the underlying distributional approximations. As a result, the RKD estimates may be subject to a non-negligible bias and the resulting confidence intervals can be severely biased. They propose an alternative method where the RKD point estimate is corrected by an estimated bias term, and the standard error estimates are adjusted for additional variability that results from the estimation of the bias correction term. This procedure yields bias-corrected point estimates and confidence intervals that are more robust to the bandwidth choice than the conventional methods. Calonico et al. (2014) also introduce a new method to choose the bandwidth such that the point estimator is mean square error (MSE) optimal. More recently Calonico et al. (2016a) develop further bandwidth selection procedures, including bandwidth selectors that minimize the coverage error rate (CER) of the robust bias-corrected confidence interval, which may be preferred for inference purposes.

Card et al. (2015, 2016) compare conventional nonparametric RKD estimates to their bias-corrected alternatives obtained using different polynomial orders and bandwidth selectors and using both real-world data and simulated data. They argue that in some cases – including their analysis of the effects of UI benefits on unemployment duration using Austrian data – the uncorrected linear RKD model can produce more useful estimates than the bias-correction procedure of Calonico et al. (2014), which may come at the cost of a substantial loss in precision with possibly only a small reduction in bias. This is because when the bias term is imprecisely estimated, the overall variance of the bias-corrected estimator can be much higher. Moreover, Card et al. (2015) claim that the MSE-optimal bandwidth selector discussed in Calonico et al. (2014) yields bandwidths that are "too small" in their empirical setting, and therefore they advocate the use of the same bandwidth selector but without the regularization term which reflects the variance in the bias estimation and guards against large bandwidths.

When it comes to the choice of the polynomial order, linear ($P = 1$) and quadratic ($P = 2$) models have been typically used in nonparametric analysis. Calonico et al. (2014) state that the local quadratic estimator is preferable to the local linear estimator in the RKD setting due to boundary bias considerations, whereas Card et al. (2014, 2016) argue that the best choice of polynomial order in MSE sense depends on the sample size and the (unknown) derivative of the conditional expectation function $E(Y|W = w)$ (and $E(B|W = w)$ in the fuzzy RKD settings) in the particular data set. In empirical applications, the polynomial models have often been compared using some information criteria.

In general, RKD estimates have been found to be rather sensitive with respect to polynomial order and bandwidth choices (but not to the choice of the kernel function). This is unfortunate as there is no consensus on how these choices should be made. Calonico et al. (2014) advocate the use of the bias-corrected estimates from the quadratic model using their selector for the optimal bandwidth. Card et al. (2015, 2016) seem to favor uncorrected estimates from the linear model based on the rule-of-thumb bandwidth of Fan and Gijbels (1996) or the MSE-optimal bandwidth selector of Calonico et al. (2014) without the regularization term. Aldo (2016) points out that local linear estimates can be biased due to confounding nonlinearities and recommends a more parametric approach where control variables are added to eliminate or mitigate the bias.

In our analysis, we present a range of conventional and bias-corrected nonparametric local linear and quadratic estimates using alternative bandwidth selectors to provide a clear picture of the sensitivity of our estimates to these choices. We also conduct more parametric analysis by estimating models from larger subsets of data (i.e. including also observations far away from the wage cutoff) while controlling for observed individual characteristics and choosing the polynomial order on the basis of the Akaike information criteria.

4 Data and descriptive statistics

Our data are drawn from various administrative registers. The primary data source is the register on job seekers, maintained by the Ministry of Employment and the Economy. The register covers all registered applicants at the public employment agency. Without registration as an unemployed job seeker one cannot qualify for unemployment benefits, so all UI recipients – and many unemployed non-recipients and employed job seekers – should be included. The register contains information on unemployment spells, labor market training courses and job placement programs, as well as demographic characteristics, such as age, gender, education, occupation and living region. However, there is no information on receipt of unemployment benefits, nor on job spells or earnings.

While UI benefits are paid by individual unemployment funds, each fund must report the benefits it paid out to the the Insurance Supervisory Authority. From its registers we obtain information on received UI benefits and earnings-related labor market training subsidies. In addition, we merge employment and earnings records from the registers of the Finnish Centre for Pensions, which is a statutory co-operation body of all providers of earnings-related pensions in Finland. It keeps comprehensive records on job spells and earnings for the entire Finnish population, which will be used to determine pension benefits.

We focus on workers who became unemployed between 2003 and 2007 and who qualified for 500 days of UI benefits. The beginning of the period is restricted by the fact that there were changes in the benefit schedule before this. We do not consider unemployment spells that began after 2007 in order to have a long enough follow-up period for post-unemployment outcomes. Our current data ends in December 2009. We exclude workers older than 54 (to drop those eligible for extended UI benefits after regular UI benefits) and those who were eligible for the higher benefit based on long employment history or due to participating in labor market training based on the action plans. We also exclude individuals whose UI benefits have been reduced due to other benefits,[4] those who began to collect UI benefits more than 80 days after the date of job separation,[5] and those who have been laid off temporarily (the temporary layoff status is directly observed in the UI records). We express daily wages in 2009 EUR using the deflator applied to the unemployment benefits, and pool the observations from different years by centering around the wage cutoff. The daily wage is determined during the employment condition weeks and is the actual wage used as the basis of the benefit payments. In order to eliminate the kinks at the lower end of the wage distribution, we drop individuals whose daily wage deviates from the wage cutoff by more than 55 EUR. Finally, we drop 286 observations that are outside the true benefit schedule. These constitute only 0.14% of our estimation sample and dropping them enables us to use a sharp regression kink design. After these restrictions, our estimation sample consists of almost 200,000 unemployment spells.

We consider several unemployment outcomes. One measure is the time to the next job (or nonemployment duration), which is defined as the number of days between two consecutive job spells. We define UI duration as the sum of days on UI benefits and earnings-related labor market subsidies. We consider a spell as ending in employment if the person becomes employed for a period of at least four weeks. Shorter breaks are considered part of the same nonemployment spell and ignored in the measure of unemployment duration. Our results are robust to variations in this condition. All job placement programs are observed in the data and transitions into these programs are not regarded as transitions into employment when calculating the time to the next job or defining the re-employment status. Periods on partial UI benefits and labor market training are

[4]Benefits such as home care allowance when taking care of children as well as partial disability pension can lower the UI benefit an unemployed worker is entitled to. We exclude 2,539 individuals due to such reductions.

[5]Our results are robust to varying this restriction between 30 and 90 days.

included in the unemployment spells and we examine how the benefit level affects the fraction of days on partial benefits during the compensated spell of unemployment. The nonemployment spells as well as the UI benefit spells are censored at two years.

In table 1 we report descriptive statistics for the whole estimation sample described above as well as the sample around the kink point. Panel A describes our outcome variables and panel B shows descriptive statistics for individual characteristics. As discussed above we consider unemployment outcomes including the UI duration, total nonemployment duration and the share of UI benefit days that is spent on partial benefits, i.e. in subsidized part-time or temporary employment. We also examine the share of UI benefit spells ending in employment and in order to analyze the quality of the post-unemployment jobs we consider the wage and duration of the next job.[6] To get a more comprehensive picture of post-unemployment outcomes, we also consider working days and earnings within the first two years following the beginning of the unemployment spell.

In all our outcome measures, the differences between the full sample and the sample around the kink point are in line with the fact that the kink point is situated in the upper part of the wage distribution. The average previous daily wage in the full sample is 87 EUR which is 19 percent lower than the kink point of 107 EUR. Workers around the kink point find a new job somewhat faster than an average UI recipient (218 versus 231 days), and their new jobs are higher paid and last longer on average. The main differences in individual characteristics between the full sample and the sample around the kink point also stem from the location of the kink point slightly higher than the mean in the wage distribution. The sample around the kink point has a slightly lower share of women and is somewhat higher educated. Our sample does not include workers who have voluntarily quit their jobs and who are therefore subject to a 90-day waiting period, and therefore the rather low share of dismissed workers reflects the large share of workers who have been employed with fixed-term contracts prior to unemployment.

5 Graphical evidence

The key identifying assumption in our RKD analysis is that conditional on ε, the density of the past wage is smooth at the wage cutoff w^*. This smooth density condition rules out (perfect) manipulation of the assignment variable at the kink point. Figure 2 shows the number of unemployment spells by bins of 1 EUR relative to the cutoff. The graph shows no signs of discontinuity in the number of spells close to the cutoff. A formal McCrary test

[6] The wage and duration of the next job are set to 0 for those who are not re-employed. The measure for pre-unemployment wage is the actual wage used in calculating the UI benefit and subject to a proportional deduction due to pension insurance payments. Therefore it is not directly comparable to the post-unemployment wage which is registered without the deduction.

Table 1: Descriptive statistics for full sample and sample around the kink point

	Full sample		Around kink	
	Mean	SD	Mean	SD
A. Outcomes				
UI duration (days)	141	137	136	135
Time to next job (days, censored 2 years)	231	253	218	244
Fraction of partial unemployment in UI days	0.05	0.16	0.04	0.14
Re-employment probability	0.83	0.37	0.85	0.36
Duration of next job (days)	336	531	380	572
Daily wage of next job (euros)	86.7	43.8	98.9	47.4
Working days within next 2 years	305	195	314	187
Earnings within next 2 years	22,504	20,236	26,525	20,597
B. Covariates				
Daily wage used to determine UI benefit	86.70	22.60	106.00	5.73
Daily UI benefit	55.40	9.17	64.30	3.98
Dismissed	0.06	0.23	0.08	0.27
Age	38.10	9.43	38.30	9.04
Female	0.61	0.49	0.45	0.50
Helsinki metropolitan area	0.14	0.34	0.17	0.37
Occupation:				
Scientific, technical, arts	0.17		0.28	
Healthcare, social workers	0.21		0.12	
Administrative, clerical, IT	0.10		0.08	
Commercial	0.06		0.04	
Agriculture, forestry, fishing	0.04		0.02	
Transportation	0.03		0.04	
Construction and mining	0.10		0.15	
Manufacturing I	0.12		0.14	
Manufacturing II	0.04		0.05	
Service workers	0.11		0.06	
Other	0.03		0.02	
Education:				
Compulsory or missing	0.22		0.19	
Secondary	0.62		0.55	
Tertiary	0.17		0.27	
Observations	199,011		31,359	

Notes: The around-the-kink sample includes those unemployed whose previous daily wage deviates from the cutoff value by 10 EUR or less. The group Manufacturing I includes painters, textile, metal, machinery, electrical and wood workers and the group Manufacturing II includes handicraft, printing, food processing, chemical processing, paper production and machine operators in energy production and water supply and treatment.

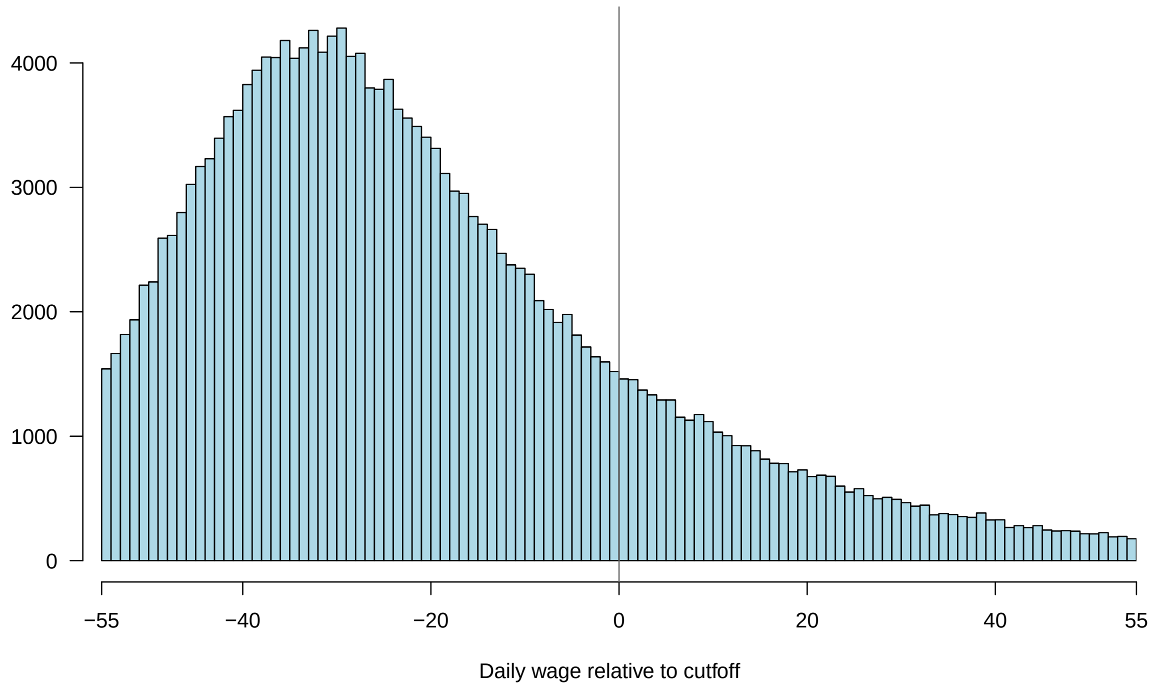

Figure 2: Number of unemployment spells (bin size = 1 EUR)

as usually conducted in the regression discontinuity design literature also shows no lack of continuity at the kink.[7] Card et al. (2015) also extend the idea of the McCrary test to the RKD by testing the assumption of the continuity of the derivative of the density function. The number of observations in each bin is regressed on polynomials of previous earnings (centered at the cutoff) and the interaction term. When we do a similar exercise, the coefficient of the interaction term for the first order polynomial is insignificant, indicating that the smoothness assumption is not violated.

The regression kink design also requires that the relationship between the covariates and the outcome variable is smooth around the cutoff point. In order to examine whether this holds in our set up, we plot mean values of selected covariates in each bin of the assignment variable. As seen in figure 3, there are nonlinearities in the relationship between some covariates and daily wage. We also observe clear kinks, for example, around -30 EUR in the share of health care and social work employees, and around -10 EUR in the share of spells beginning in June or July. Nonetheless, the covariates evolve rather smoothly around the cutoff point and bias-corrected estimates using MSE-optimal bandwidths for each covariate indicate no significant kinks in the covariates.[8]

[7] Point estimate of log difference in height is 0.0069 with standard error 0.021.

[8] We also estimated kinks for the covariates using the MSE-optimal bandwidth for our UI duration outcome and none of the estimates were significant.

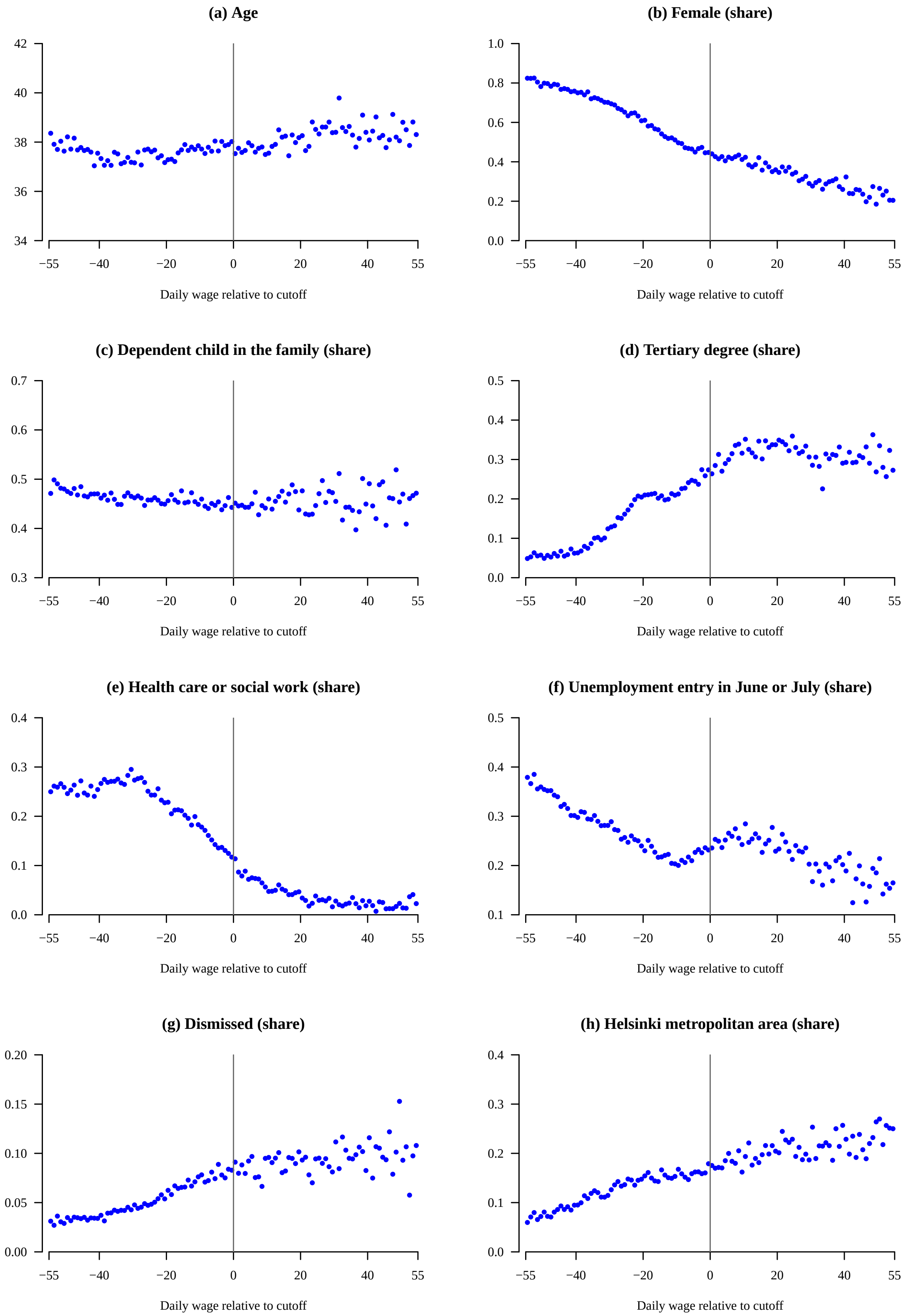

Figure 3: Local averages of selected covariates (bin size = 1 EUR)

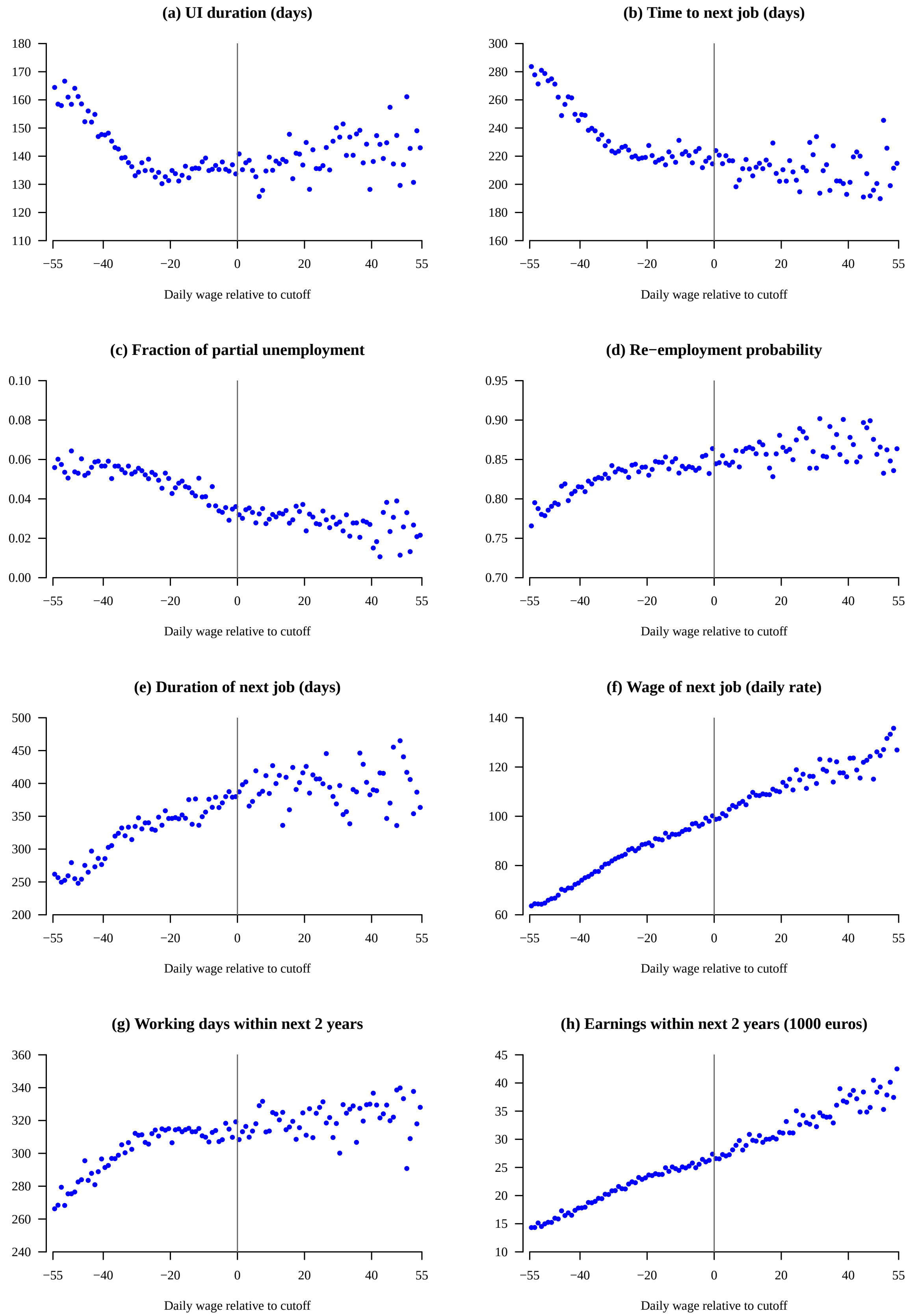

Figure 4: Local averages of outcome variables (bin size = 1 EUR)

Figure 4 displays the relationship between previous wage and various outcomes. We observe some nonlinearities in the relationship between previous wage and the outcomes, which are likely to be associated with compositional changes in the underlying population as we also see nonlinearities in the covariates. Due to these nonlinearities, the local linear model can fit the data well only for relatively short bandwidths, i.e. bandwiths of 30 EUR at a maximum. Wider bandwiths call for higher order polynomials and/or controls for observed characteristics. Focusing on the cutoff, there appears to be some evidence of kinks at the wage cutoff, most notably for the fraction of partial unemployment.

6 Regression kink estimates

6.1 Conventional local linear models

The graphical evidence in figure 4 suggests that the local linear model could fit the data well near the wage cutoff but is likely to be too restrictive for wider bandwidths. As such we restrict our local linear regression analysis to bandwidths between 10 and 30 EUR. We do not report results for smaller bandwidths which are very noisy and essentially uninformative. Figure 5 shows estimated elasticities of the outcomes with respect to the UI benefit level as well as 95% confidence intervals from linear specifications without control variables for a range of bandwidths. The bandwidths are measured as euros of daily wage and the elasticities are calculated at the mean UI benefit and mean of the outcome for each separate bandwidth. Bias-corrected estimates with robust confidence intervals for various optimal bandwidth selection methods are reported and discussed in the next section.

Considering the absence of clearly visible kinks in figure 4, it is unsurprising that many of the elasticity estimates are not statistically significant. The estimated effect of the UI benefit level on UI duration is positive but insignificant at small bandwidths and hovers around zero as the bandwidth widens. The point estimates for the elasticity of nonemployment duration are positive across the whole range of bandwidths but very imprecise especially when using narrow bandwidths. The estimated effect on the fraction of partial unemployment is, on the other hand, negative and significant for all but the smallest bandwidths. It therefore appears that decreasing the UI benefit level would induce unemployed workers to take up more part-time or temporary employment. The elasticity estimates are quite large in absolute value, but should be considered in the context of the rather low average share of partial unemployment. The estimates indicate that a 1% decrease in the UI benefit level would increase the share of partial unemployment days in the UI spell by approximately 5%, i.e. from an average of 4% to 4.2%. It should be noted that this is a combination of more unemployed workers taking partial benefits and those on partial benefits receiving partial benefits for a larger share of their total time on UI benefits. On average 10% of UI spells include time on partial benefits, and conditional on receipt of partial benefits, the share of partial unemployment days is approximately 40%.

Looking next at the effect of the UI benefit level on the re-employment probability, the elasticity estimates in figure 5 are negative, but only barely significant at a few bandwidths. A negative estimate would imply that higher benefits lower the re-employment probability, but even though the effect is more precisely estimated at wider bandwidths, we lack statistical power to be able to say anything conclusive. The estimated elasticity of the duration of the first job after re-employment is positive and around 1, but again statistically insignificant with very wide confidence intervals at smaller bandwidths. The wage in the first job after unemployment appears to be affected negatively by the UI benefit level, with the elasticity estimates in figure 5 mostly around -0.5. This would imply that potentially longer nonemployment durations related to higher UI benefits (though such an effect is not statistically signifcant in the top-right graph) could lead to a relatively lower wage due to e.g. discrimination by employers or human capital depreciation.

The estimates for the effect of the UI benefit level on the number of working days within two years of the beginning of the unemployment spell are slightly negative but again only significant at a few bandwidths. This potentially negative effect would of course bemechanically influenced by any increase in unemployment or nonemployment duration stemming from a higher UI benefit level, but there is little evidence of such effects. All in all, any positive effect that a higher UI benefit level may have on the duration of the first post-unemployment job appears to not compensate for prolonged unemployment or the adverse employment effects of not taking up part-time or temporary work. The estimates for earnings in the first two years after the beginning of the unemployment spell indicate that a higher UI benefit level decreases earnings within the next two years with an elasticity of roughly -1. This result obviously combines any actual wage effect implied by a lower post-unemployment wage and the potential effect of prolonged unemployment and subsequently less time employed.

To sum up, the elasticity estimates in figure 5 are relatively insensitive with respect to the bandwidth choice but rather imprecise. We find statistically significant negative effects on the fraction of part-time unemployment and earnings within the next two years. The effects on the duration and wage of the next job are only marginally significant. Other effects have expected sign but are too imprecisely estimated for any conclusions.

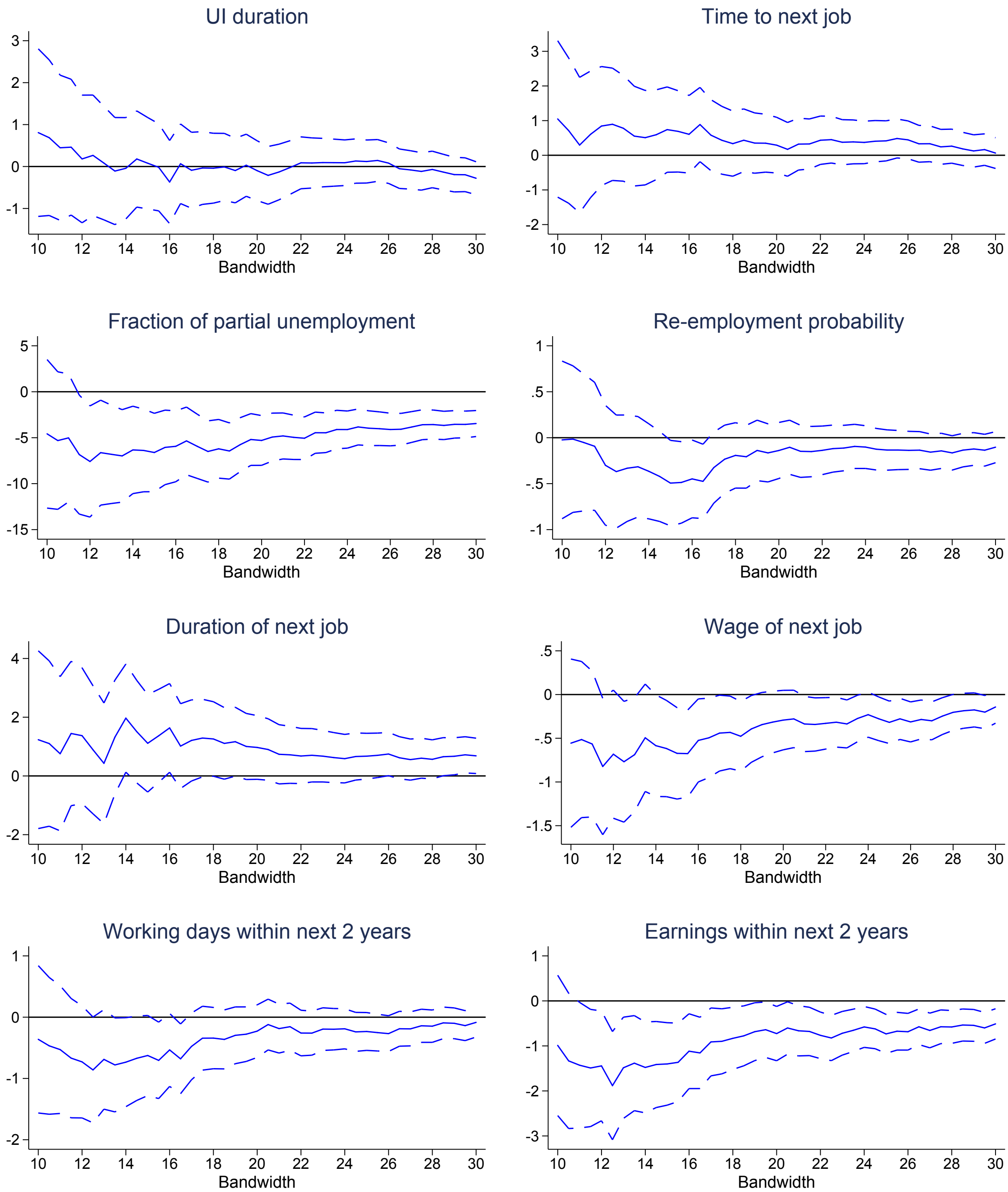

Figure 5: Conventional elasticity estimates from local linear models at varying bandwidths along with 95% confidence intervals

6.2 Bias-corrected estimates

To study the robustness of the results depicted in the figures above, we next present both conventional and bias-corrected estimates from linear and quadratic specifications using different bandwidth selection methods.[9] Tables 2 and 3 show results for unemployment and post-unemployment outcomes respectively. Columns 1 to 3 in table 2 display results for linear specifications and columns 4 to 6 show results for quadratic specifications. The conventional elasticity estimates from the linear specifications correspond to the estimates in figure 5. For both the linear and quadratic specifications three alternative bandwidth selection methods are used: the MSE-optimal bandwidth, the MSE-optimal bandwidth without the regularization term and the CER-optimal bandwidth. Generally the CER-optimal bandwidths are very narrow, about half the MSE-optimal bandwidth, leading to very large standard errors.

Looking first at the UI duration, the elasticity estimates vary somewhat depending on the estimation method used, with the bias-corrected estimates slightly higher in general. The bias-corrected estimates range from 0.9 to 3.8 and are quite noisy, with especially the narrow CER-optimal bandwidths leading to very large standard errors. Using the MSE-optimal bandwidth for the linear specification, the elasticity estimates of 3.0 and 3.8 are statistically significant, albeit quite high compared to the other point estimates from linear models at wider bandwidths. They are more in line with the elasticity estimates from quadratic specifications, which also are rather large but mainly statistically insignificant.

Turning to the elasticity estimates for the time to the next job, i.e. nonemployment duration, the bias-corrected estimates are again larger than the conventional estimates. Using the narrow CER-optimal bandwidth the estimates are higher than at the MSE-optimal bandwidths, but the standard errors are also large leading to essentially uninformative results. The wider MSE-optimal bandwidths without regularization yield bias-corrected elasticities of 1.5 and 1.6 for the linear and quadratic specifications respectively, with the quadratic estimate statistically significant. The elasticity of 1.6 would imply a 3.5 day increase in the nonemployment duration if the UI benefit level increased by 1%. There is only one prior estimate obtained from Finnish data for the elasticity of unemployment duration w.r.t. the UI benefit level. Uusitalo and Verho (2010) find an elasticity of 0.8, but this is for a specific group of unemployed entitled to increased UI benefits for the first 150 days of unemployment and thereby not necessarily generalizable. For the time to next job Carling et al. (2001) find an elasticity of 1.6 w.r.t. to the benefit level in Sweden, which is in line with our bias-corrected estimates at the MSE-optimal bandwidths without regularization.

The elasticity estimates in figure 5 implied that the fraction of time spent on partial

[9] We use the rdrobust package (Calonico et al. 2016b) for these estimations

Table 2: Conventional and bias-corrected elasticity estimates using competing optimal bandwidth choices for unemployment outcomes

	Linear models			Quadratic models		
	(1)	(2)	(3)	(4)	(5)	(6)
UI duration						
Bandwidth	8.59	12.45	4.67	17.73	36.86	8.83
Conventional elasticity	3.02**	0.29	-0.19	2.10	1.57***	2.41
Conventional std error	[1.29]	[0.74]	[3.25]	[1.72]	[0.56]	[4.94]
Bias-corrected elasticity	3.84**	1.14	0.93	2.31	1.70	3.24
CCT robust std error	[1.90]	[1.64]	[5.19]	[2.37]	[1.09]	[6.65]
Time to next job						
Bandwidth	12.88	15.06	7.00	19.55	58.47	9.74
Conventional elasticity	0.87	0.73	2.60	2.04	1.53***	4.67
Conventional std error	[0.79]	[0.62]	[1.99]	[1.67]	[0.34]	[4.83]
Bias-corrected elasticity	0.96	1.48	3.58	1.59	1.62**	6.37
CCT robust std error	[1.24]	[1.04]	[3.41]	[2.28]	[0.82]	[6.45]
Fraction of partial unemployment						
Bandwidth	11.85	16.40	6.44	14.76	54.66	7.35
Conventional elasticity	-7.25**	-5.43***	-1.25	-5.19	-4.86***	11.81
Conventional std error	[3.11]	[1.82]	[8.08]	[8.53]	[0.95]	[26.90]
Bias-corrected elasticity	-9.04**	-7.56**	1.86	-1.72	-4.48	6.68
CCT robust std error	[4.52]	[3.11]	[12.82]	[11.13]	[7.80]	[34.14]
Re-employment probability						
Bandwidth	10.39	22.30	5.65	20.95	38.78	10.43
Conventional elasticity	-0.01	-0.10	0.61	-0.49	-0.42*	1.34
Conventional std error	[0.41]	[0.13]	[1.03]	[0.56]	[0.22]	[1.64]
Bias-corrected elasticity	-0.28	-0.24	1.54	-0.44	-0.18	2.53
CCT robust std error	[0.60]	[0.43]	[1.64]	[0.77]	[0.48]	[2.21]
Bandwidth selection	MSE	MSE no reg	CER	MSE	MSE no reg	CER
Polynomial order for point estimate	1	1	1	2	2	2
Polynomial order for bias correction	2	2	2	3	3	3

unemployment benefits would increase if the UI benefit level decreased. This also shows up in the bias-corrected estimates in table 2, where the elasticity of partial unemployment w.r.t. the UI benefit level is negative except at the very narrow CER-optimal bandwidths. The standard errors for the bias-corrected estimates using the CER-optimal bandwidths are again very large leading to uninformative point estimates. The bias-corrected estimates from the linear specification at the MSE-optimal bandwidth with and without the regularization term indicate elasticities of -9 and -7.5 respectively. As discussed above related to figure 5, the average share of partial unemployment days in an UI spell is quite low and conceals a high share of partial unemployment conditional on taking up any partial unemployment benefits. The elasticity of -9 implies that a 1% decrease in the UI benefit level would lead to a 0.4 percentage point increase in the fraction of time spent on partial unemployment benefits. Although this is a small increase, it does indicate that lower benefits induce the unemployed to take up part-time or temporary jobs. The bias-corrected estimates for elasticity of the re-employment probability w.r.t the UI benefit level in table 2 are also negative except for the narrow CER-optimal bandwidths, but all the estimates are statistically insignificant. Considering the noisy conventional elasticity estimates in figure 5, this is not surprising.

Table 3 shows the elasticity estimates for post-unemployment outcomes. As with the unemployment outcomes, estimates from both linear and quadratic specifications for various optimal bandwidth selection methods are shown. The bias-corrected elasticity estimate of the duration of the next job w.r.t the UI benefit level is not robust to different polynomial orders and bandwidths. The point estimates are mostly positive, but very imprecise. For the first post-unemployment wage the bias-corrected elasticity estimates are negative except when using the narrow CER-optimal bandwidth in the quadratic specification. The narrow bandwidths lead, once again, to very large standard errors. The bias-corrected estimates using the MSE-optimal bandwidths with and without regularization range from -0.25 to -1.4 but are not statistically significant. In line with figure 5, the conventional elasticity estimate at the MSE-optimal bandwidth without regularization is -0.79 and statistically significant.

Working days within the two years following the beginning of the unemployment spell appear to be slightly negatively affected by a higher level of UI benefits. The bias-corrected elasticity estimates are negative across the board, but again the narrow CER-optimal bandwidths are associated with very large standard errors. The point estimates with larger absolute values (-2.2 and -2.6 in linear and quadratic models) are marginally significant implying that a 1% increase in the UI benefit level would lead to a 7 to 8 day decrease in the number of working days in the following two years. As discussed above, such an effect is consistent with a longer initial unemployment duration and less time spent in part-time and temporary employment. Bias-corrected elasticity estimates for earnings in the two years after the beginning of the unemployment spell are also negative except at the narrow CER-optimal bandwidths. The linear specification with the wider MSE-optimal bandwidth without the regularization term yields a statistically significant elasticity estimate of -1. Such a decrease in earnings due to higher UI benefits is in line with our findings of lower post-unemployment wages and less working days in subsequent years.

6.3 Higher order polynomials and larger bandwidths

Most of the nonparametric estimates above are quite noisy. To increase statistical power of the analysis we also conduct a more parametric analysis using larger subsets of the data. Because the relationships between the outcome variables and daily wage become clearly nonlinear when we move away from the wage cutoff (see figure 4), it is quite obvious that the linear model does not fit to the data well when large bandwidths are used and hence

Table 3: Conventional and bias-corrected elasticity estimates using competing optimal bandwidth choices for post-unemployment outcomes

	Linear models			Quadratic models		
	(1)	(2)	(3)	(4)	(5)	(6)
Duration of next job						
Bandwidth	9.38	13.66	5.10	12.98	32.56	6.46
Conventional elasticity	0.36	1.36	6.20	3.83	0.01	22.12*
Conventional std error	[1.72]	[0.98]	[4.27]	[4.26]	[1.07]	[12.05]
Bias-corrected elasticity	0.35	2.56	11.10	3.41	-0.81	25.56
CCT robust std error	[2.70]	[2.06]	[7.41]	[5.92]	[8.11]	[16.34]
Wage of next job						
Bandwidth	7.71	11.61	4.19	13.68	24.98	6.81
Conventional elasticity	-0.73	-0.79**	-0.46	-1.14	-0.73	0.58
Conventional std error	[0.74]	[0.40]	[1.86]	[1.25]	[0.52]	[3.59]
Bias corrected elasticity	-0.70	-1.27	-0.74	-1.40	-0.25	0.59
CCT robust std error	[1.16]	[0.91]	[3.21]	[1.71]	[0.98]	[4.78]
Working days within next 2 years						
Bandwidth	7.60	9.56	4.13	13.71	19.19	6.83
Conventional elasticity	-1.43	-0.60	-0.85	-0.79	-1.99**	0.66
Conventional std error	[0.93]	[0.66]	[2.33]	[1.51]	[0.91]	[4.36]
Bias-corrected elasticity	-2.23*	-1.39	-1.58	-1.49	-2.64*	-0.09
CCT robust std error	[1.27]	[1.06]	[3.40]	[1.99]	[1.42]	[5.59]
Earnings within next 2 years						
Bandwidth	10.77	19.25	5.85	13.21	26.65	6.58
Conventional elasticity	-1.35	-0.61*	1.00	-0.87	-1.80**	3.87
Conventional std error	[0.83]	[0.33]	[1.74]	[2.14]	[0.85]	[6.25]
Bias-corrected elasticity	-1.58	-0.98**	2.60	-0.61	-2.05	4.16
CCT robust std error	[1.15]	[0.41]	[2.63]	[2.91]	[2.05]	[8.42]
Bandwidth selection	MSE	MSE no reg	CER	MSE	MSE no reg	CER
Polynomial order for point estimate	1	1	1	2	2	2
Polynomial order for bias correction	2	2	2	3	3	3

higher order polynomial models are called for. We consider polynomial models of orders 1 to 3, with and without control variables. In tables 4 and 5 we report elasticity estimates for bandwidths ranging from 10 to 55 EUR from the specification with the lowest value of the Akaike information criterion.[10] The estimates in panel A are from the specification outlined in (6), whereas the estimates in panel B are from an augmented specification that include controls for the year and month of unemployment entry, gender, the number of children, interactions between the number of children and gender, education, occupation, age, capital region and a dummy for dismissed workers.

In the local analysis, the control variables do not contribute to identification but their inclusion may reduce sample noise and hence lead to more precise elasticity estimates. Their inclusion also provides a useful robustness check, as the point estimates should not

[10]For most outcomes the estimates from the linear models are sensitive with respect to the bandwidth, whereas the estimates from quadratic and cubic models remain quite stable after a certain value of the bandwidth (typically around 30 EUR).

Table 4: Elasticity estimates for unemployment outcomes at varying bandwidths based on a polynomial model with the lowest Akaike information criterion

		UI duration			Time to next job			Fraction of partial unemployment			Re-employment probability		
BW	N	Pol.	Elasticity	(SE)	Pol.	Elasticity	(SE)	Pol.	Elasticity	(SE)	Pol.	Elasticity	(SE)
Panel A. No covariates													
10	31,359	2	8.08*	(4.10)	1	1.05	(1.16)	1	-4.58	(4.08)	1	-0.02	(0.43)
15	48,689	2	2.90	(2.24)	2	1.77	(2.53)	1	-7.13***	(2.31)	2	1.07	(0.95)
20	67,621	3	5.23	(3.65)	1	0.30	(0.42)	3	0.97	(14.53)	3	1.94	(1.56)
25	88,756	3	3.99	(2.63)	1	0.44	(0.31)	1	-4.89***	(1.11)	1	-0.14	(0.12)
30	111,352	3	0.47	(2.03)	1	0.07	(0.24)	1	-4.56***	(0.87)	1	-0.11	(0.09)
35	134,169	3	0.91	(1.63)	3	1.82	(1.83)	2	-9.06***	(2.70)	1	-0.06	(0.07)
40	155,990	3	1.39	(1.35)	3	1.69	(1.52)	2	-8.49***	(2.25)	3	-0.17	(0.57)
45	174,392	3	1.43	(1.16)	3	1.42	(1.3)	2	-8.38***	(1.94)	2	-0.54**	(0.20)
50	188,836	3	1.89*	(1.01)	3	2.23*	(1.13)	2	-7.69***	(1.70)	2	-0.54***	(0.18)
55	199,011	2	1.03**	(0.38)	2	1.89***	(0.43)	2	-7.71***	(1.51)	2	-0.66***	(0.16)
Panel B. With covariates													
10	31,359	2	8.27**	(3.92)	1	1.31	(1.12)	1	-3.12	(4.03)	1	-0.12	(0.43)
15	48,689	2	2.66	(2.14)	2	1.79	(2.44)	1	-5.16**	(2.28)	1	-0.54**	(0.24)
20	67,621	2	1.67	(1.42)	1	0.40	(0.41)	1	-4.23**	(1.52)	1	-0.16	(0.16)
25	88,756	3	4.97*	(2.52)	1	0.46	(0.30)	1	-3.03**	(1.10)	1	-0.15	(0.11)
30	111,352	2	0.90	(0.79)	1	0.08	(0.24)	1	-2.64***	(0.86)	1	-0.10	(0.09)
35	134,169	2	0.63	(0.65)	3	2.17	(1.77)	2	-7.28**	(2.67)	1	-0.06	(0.07)
40	155,990	3	1.01	(1.30)	3	1.65	(1.47)	2	-6.22**	(2.23)	2	-0.50**	(0.23)
45	174,392	3	1.01	(1.11)	3	1.35	(1.26)	2	-5.56***	(1.92)	2	-0.51**	(0.20)
50	188,836	2	0.61	(0.41)	3	1.96*	(1.10)	2	-4.47**	(1.68)	2	-0.52***	(0.18)
55	199,011	2	0.40	(0.37)	2	1.73***	(0.41)	2	-4.61***	(1.50)	2	-0.64***	(0.16)

Notes: BW = bandwidth. N = Number of observations. Pol. = Order of the polynomial function chosen on the basis of the Akaike information criterion. Elasticities in panel B are from models that include controls for the year and month of unemployment entry, gender, the number of children, interactions between the number of children and gender, education, occupation, age, capital region and a dummy for dismissed workers. The standard errors in parenthesis. Significance levels: *** 1%, ** 5% and * 10%.

change notably. A comparison of the models for larger bandwidths is less straightforward. The kinks in the relationships between the background characteristics and daily wage in figure 3 raise some doubts about the smoothness assumption of the wage effect in the unconditional models when large bandwidths are used. The inclusion of control variables can mitigate confounding nonlinearities due to nonsmooth changes in the (observed) composition of the workers across the wage distribution (Aldo 2016). In the case of large bandwidths the smoothness assumption may therefore be more likely to be valid and the RKD estimates more reliable when we condition on the covariates. A counter argument is that the kinks in the distributions of observed characteristics make also kinks in the distribution of unobserved characteristics more likely, and thereby the RKD estimates should be treated with caution.

The results in tables 4 and 5 show that the point estimates from our parametric analysis are in general relatively stable across the range of bandwidths and, given the same polynomial degree, the estimates are not sensitive to the inclusion of control variables. Somewhat larger differences emerge for wider bandwiths but this is to be expected. The elasticity of the UI duration w.r.t the UI benefit level is around 1 but the estimates are rather imprecise and not robust to the inclusion of control variables. The elasticity of the time to next job is slightly higher at just below 2. This estimate is robust to the inclusion of covariates when the bandwidth is at least 35 EUR. For bandwidths between 15 to 30 EUR the elasticity from the quadratic model is also around 2, with an AIC only marginally higher than for the linear model reported in the table. These elasticity estimates are around the same magnitude as our bias-corrected nonparametric estimates for nonemployment duration and since they increase in precision with the increase in bandwidth and addition of covariates, this robustness check is reassuring in terms of tackling the lack of sufficient data in the vicinity of the cutoff for this outcome.

As in our previous results, the elasticity of partial unemployment is large in absolute value. The estimate appears sensitive to the inclusion of covariates and bandwidth, but is consistently negative across the range of bandwidths. The elasticity of the re-employment probability is robust around -0.5 at larger bandwidths and up to a bandwidth of 25 EUR the quadratic and cubic estimates are quite similar and only marginally dominated by the linear model reported in the table. These estimates are slightly higher in absolute value than our bias-corrected nonparametric estimates and more precise.

The results for the post-unemployment outcomes in table 5 indicate that the elasticity of the duration of the next job is around 1 but, as in our previous results for this outcome, this estimate is not very robust. The elasticity of the wage in the first job after unemployment is negative and statistically significant at most bandwidths, varying around -0.5 and -1.5 , which is about the same magnitude as our other results for this outcome. These two indicators of post-unemployment job quality are in contrast with each other. It should be noted that the results for the duration of the next job are not very robust, but the opposing effects could indicate that higher benefits enable workers to wait for more stable job offers but this comes at the cost of relatively lower wages. Looking at employment in the longer term, the elasticity of working days in the next two years is around -1 but imprecisely estimated except for the largest bandwidths. Our bias-corrected nonparametric estimates varied somewhat depending on the bandwidth selection method and were about the same or slightly higher in absolute value. It appears that if higher UI benefits have a positive effect on the duration of the first job after unemployment, this is not sufficient to compensate for the longer nonemployment duration induced by higher benefits. The elasticity of earnings in the next two years ranges from -1.5 to -2, which is in line with our previous results for post-unemployment earnings, but the results here are more precise. This indicates that the combination of a lower post-unemployment wage and less working days in subsequent years quite clearly leads to a substantial negative effect of the UI benefit level on earnings.

Table 5: Elasticity estimates for post-unemployment outcomes at varying bandwidths based on a polynomial model with the lowest Akaike information criterion

		Duration of next job			Wage of next job			Working days within next 2 years			Earnings within next 2 years		
BW	N	Pol.	Elasticity	(SE)	Pol.	Elasticity	(SE)	Pol.	Elasticity	(SE)	Pol.	Elasticity	(SE)
Panel A. No covariates													
10	31,359	1	1.23	(1.56)	1	-0.56	(0.50)	1	-0.36	(0.62)	1	-0.99	(0.83)
15	48,689	1	1.11	(0.86)	1	-0.68**	(0.28)	2	-1.31	(1.34)	2	-1.08	(1.81)
20	67,621	1	0.97*	(0.57)	2	-1.41*	(0.72)	3	-0.90	(2.19)	2	-2.37*	(1.27)
25	88,756	1	0.68	(0.42)	1	-0.32**	(0.14)	3	-2.73*	(1.58)	3	-3.51	(2.45)
30	111,352	1	0.70**	(0.33)	2	-1.05**	(0.41)	1	-0.09	(0.13)	3	-2.21	(1.71)
35	134,169	2	-0.19	(1.00)	3	-1.05	(0.81)	3	-1.63	(0.98)	2	-1.88***	(0.56)
40	155,990	3	2.09	(2.05)	3	-0.58	(0.68)	3	-1.28	(0.82)	3	-2.60**	(1.20)
45	174,392	3	2.05	(1.74)	2	-1.30***	(0.25)	3	-1.27*	(0.7)	2	-1.23***	(0.43)
50	188,836	3	0.19	(1.53)	2	-1.12***	(0.22)	3	-1.57**	(0.61)	3	-2.37**	(0.90)
55	199,011	2	-0.08	(0.57)	3	-2.23***	(0.46)	2	-0.90***	(0.23)	3	-2.29***	(0.81)
Panel B. With covariates													
10	31,359	1	1.37	(1.49)	1	-0.53	(0.49)	1	-0.48	(0.60)	1	-1.16	(0.81)
15	48,689	1	1.34	(0.82)	1	-0.64**	(0.27)	2	-1.36	(1.30)	2	-1.19	(1.76)
20	67,621	1	1.51**	(0.55)	2	-1.57**	(0.70)	3	-0.81	(2.12)	2	-2.63**	(1.24)
25	88,756	1	1.09**	(0.40)	1	-0.22	(0.14)	1	-0.19	(0.16)	1	-0.73***	(0.26)
30	111,352	1	0.93***	(0.31)	2	-0.77*	(0.40)	1	-0.03	(0.13)	2	-1.43**	(0.70)
35	134,169	1	1.14***	(0.25)	3	-1.27	(0.79)	3	-1.85*	(0.95)	2	-1.53**	(0.55)
40	155,990	1	1.07***	(0.22)	2	-0.95***	(0.28)	3	-1.19	(0.79)	3	-2.47**	(1.18)
45	174,392	3	3.12*	(1.66)	2	-0.94***	(0.24)	3	-1.12	(0.67)	2	-1.01**	(0.43)
50	188,836	3	1.71	(1.46)	2	-0.86***	(0.21)	2	-0.60**	(0.25)	2	-0.96**	(0.38)
55	199,011	2	0.29	(0.54)	3	-1.64***	(0.45)	2	-0.75***	(0.22)	3	-1.81**	(0.80)

Notes: BW = bandwidth. N = Number of observations. Pol. = Order of the polynomial function chosen on the basis of the Akaike information criterion. Elasticities in panel B are from models that include controls for the year and month of unemployment entry, gender, the number of children, interactions between the number of children and gender, education, occupation, age, capital region and a dummy for dismissed workers. The standard errors in parenthesis. Significance levels: *** 1%, ** 5% and * 10%.

6.4 Robustness checks

As a comparison, we also estimate bias-corrected nonparametric elasticities of our various outcomes using linear and quadratic specifications for a range of bandwidths.[11] The estimates are generally in line with those in tables 4 and 5. The bias-corrected estimates for the elasticity of the next job duration are not robust across the bandwidth range, as was the case in our other analyses. For the other outcomes, the bias-corrected estimates are relatively stable across the range of bandwidths. As a further robustness check, we also consider covariate adjusted bias-corrected elasticity estimates introduced in Calonico et al. (2016a). We estimate linear and quadratic specifications such as in tables 2 and 3 but with covariate-adjusted point estimates and covariate-adjusted robust bias-corrected confidence intervals. The results are in general similar to those in tables 2 and 3 and no notable increase in precision is achieved.[12]

[11] Results not shown, available on request. The pilot bandwidth used for estimating the bias was set to be equal to the main bandwidth. See Calonico et al. (2016a) for discussion.

[12] Results not shown, available on request.

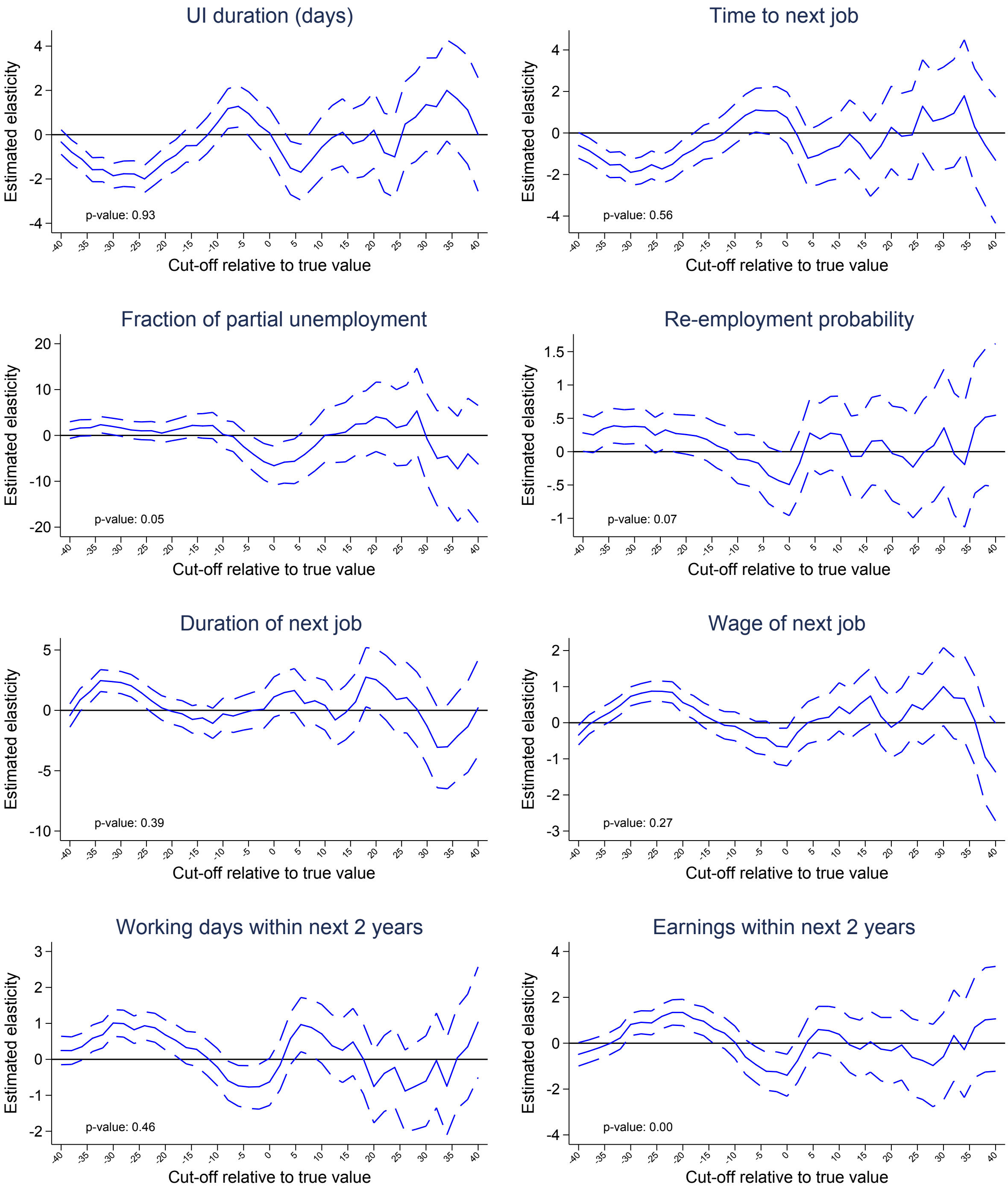

Figure 6: Conventional local linear elasticity estimates for placebo cutoffs along with 95% confidence intervals (bandwidth = 15 EUR around each placebo cutoff)

In addition to varying bandwidths and alternative estimation methods we also consider the robustness of our results by examining the effect of the UI benefit level on outcomes at different cutoff points. In figure 6 we provide elasticity estimates from local linear regressions similar to those in figure 5 but for placebo cutoff points. The true value of the cutoff is at 0 in each figure and the p-value indicates the fraction of estimates that are larger in absolute value than the estimate at the true cutoff. The outcomes for which the results have been consistent in our other robustness checks are also clearest here, i.e. the share of partial unemployment in the UI spell and earnings within two years of the beginning of the unemployment spell. For the other outcomes it is harder to distinguish the estimates at the true cutoff from the placebo estimates. Given that the elasticity estimates for e.g. unemployment duration were small and imprecise, it is unsurprising that a large fraction of the placebo estimates are larger than the actual estimates. Moreover, for several outcomes there are clearly distinguishable significant placebo estimates that coincide with the kinks in the share of health care and social workers and the month of unemployment entry, that is, when the placebo cutoff is smaller than the true one (see figure 3). Therefore, it appears that the changes in workforce composition across the wage distribution are influencing these estimates. As discussed in the previous section, this should be taken into account when using observations further away from the cutoff by applying quadratic or even higher order polynomial models and/or by including control variables in the analysis.

7 Conclusions

Research on the effects of the UI benefit level on labor market outcomes other than unemployment duration is scarce and the results are mixed. In this study we have provided further evidence on the effects of the UI benefit level on unemployment and subsequent labor market outcomes. To identify the causal effect of the UI benefit level, we exploited a kink in the relationship between the previous wage and UI benefits in Finland. We used a large register based data set with accurate information on the UI benefit level and previous wage which allowed us to apply a sharp regression kink design. We compared different nonparametric estimation methods proposed in the literature on regression kink design and similar to previous studies our results were quite sensitive to choices regarding polynomial order and bandwidth. Despite the large data and accurate benefit and wage information, our nonparametric estimates were rather imprecise regardless of the polynomial order and bandwidth selection method. Results from specifications with added covariates estimated using larger samples were more precise and generally of the same magnitude as nonparametric estimates from our other specifications.

We found robust evidence that the UI benefit level has a large negative effect on the share of days spent on partial unemployment benefits during the UI spell, i.e. the time spent in subsidized part-time or temporary employment. Also the findings for post-unemployment earnings were robust to varying estimation methods: Our results showed that the wage in the first job after unemployment and also subsequent earnings in the two years after the beginning of the unemployment spell decrease with an increase in the UI benefit level. Results for other outcomes were more sensitive to the choice of specification, but our findings indicate that higher UI benefits also increase the nonemployment duration and decrease the re-employment probability and number of working days in the next two years. We also examined the duration of UI benefit receipt and the duration of the first post-unemployment job, but the results for these outcomes were inconclusive.

In summary, we found no evidence of positive effects on match quality for the UI benefits, and thereby the overall effect of higher UI benefits on labor market outcomes over the two-year period is unambiguously negative.

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