ONLINE APPENDIX: SUPPLEMENTARY ANALYSES AND ADDITIONAL ESTIMATES FOR

Does Reducing Unemployment Benefits During a Recession Reduce Youth Unemployment? Evidence from a 50 Percent Cut in Unemployment Assistance

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Appendix A

Table A1

Summary of Literature on Effects of Benefit Changes on Unemployment Duration

Author(s)	Country/Data	Period	Age	Econometric Approach	Benefit Elasticity
Hunt (1995)	Germany: GSOEP.	1983-1988	<57	Hazard Function: Identification exploits cut to benefit for unemployed without children.	Effect unclear, sign changes with specification.
Carling, Holmlund and Vejsiu(2001)	Sweden: LINDA database.	1994-1996	<55	Hazard Function: Identification exploits cut to replacement rate for a subset of the unemployed.	Duration elasticity 1.6. Effect is stronger for younger workers (age < 25).
Røed and Zhang (2003)	Norway: Unemployment Register.	Became unemployed in the 1990s	<60	Hazard Function: Identification exploits exogenous variation in replacement rate.	Duration elasticity 0.95 for men and 0.35 for women. Disincentive effects at work throughout the business cycle.

Fortin, Lacroix and Drolet (2004)	Canada: Monthly administrative files of the social assistance program in the Province of Quebec.	1983-1993	18-29	Hazard Function Difference-in-Difference: Identification uses a change in the reform that removed an age threshold for benefit	Duration elasticity 0.25 and 0.28, for aged 22-29 single men and women respectively. Insignificant results for 18-21 year olds.
Lalive, van Ours and Zweimüller (2006)	Austria: Austrian Social Security Database.	1987-1991	35–54	Hazard Function Difference-in-Difference: Identification exploits variation in benefit changes by previous earnings levels.	Duration elasticity of 0.15.
Meyer and Mok (2007)	United States: Administrative data on UI claimants in New York State.	1988-1989		Hazard Function Difference-in-Difference: Identification exploits variation in benefit increases by previous earnings levels.	Duration elasticity for males ranges from 0.07 to 0.22, while for females they are larger, ranging from 0.36 to 0.47. Elasticities for those younger than 40, are close to zero, while those for individuals 40 and older range from 0.30 to 0.46.
Lemieux and Milligan (2008)	Canada: Census and LFS.	1986-1991	Low educated childless males age 25-39	Regression Discontinuity: Identification exploits an entitlement threshold at age 30.	Entitlement to benefit reduces probability of employment by 3 to 5 percentage points.
Bargain and Doorley (2011)	France: Sample from French Census.	1982-1999	Low educated single childless males	Regression Discontinuity: Identification exploits entitlement changes at age 25.	Labor market participation elasticity of between -0.06 to -0.04.
Michelacci and Ruffo (2015)	United States: Survey of Income and Program	1985-2004	18-65	Hazard Function:	Duration Elasticity -0.23 (and insignificant) for those aged 20-24 and

	Participation (SIPP), Current Population Survey (CPS) and Panel Study if Income Dynamics (PSID).			Identification exploits cross state variation in unemployment levels.	-0.86 (and significant) for those aged 41-60.[19]
Card et al. (2015b)	Austria: Austrian Social Security Database.	Claimants from 2001 to 2012	<50	Regression Kink Design: Identification exploits a kink in the unemployment insurance rule.	Duration elasticity 0.1-2.7 (1.4-1.9 for large bandwidths) but many of the elasticities estimated are statistically insignificant.
Card et al. (2015a)	United States: Data on UI claimants in Missouri.	Initiated a claim from 2003-2013.		Regression Kink Design: Identification exploits a kink in the unemployment insurance rule	Duration elasticity 0.35 pre-recession and 0.65-0.9 during recession.
Landais (2015)	United States: Continuous Wage and Benefit History (CWBH) for 5 U.S. states.	From mid to late 1970s (depending on state) to 1984		Regression Kink Design: Identification exploits a kink in the unemployment insurance rule.	Duration elasticity 0.25-0.38, with the lower bound occurring when the unemployment rate is at 11.8 percent and the upper bound when the state unemployment rate is at 4.5 percent.
Kroft and Notowidigdo (2016)	United States: Survey of Income and Program Participation (SIPP).	1985-2000	Prime age males	Hazard Function: Identification exploits cross state variation in unemployment levels. Macro elasticity	Duration elasticity 0.28-0.99 as unemployment varies from one standard deviation above the mean to one standard deviation below the mean.
Rebollo-Sanz and Rodriguez-Planas (2018)	Spain: Social Security longitudinal data from the Continuous Sample of Working	2012 and 2013 waves	20-50	Hazard Function: Identification exploits a reduction in the replacement rate after 180 days	Duration elasticity 0.86.

	Histories (CSWH).				
Kyyra and Pesola (2017)	Finland: Register on job seekers.	Initiated a claim from 2003-2007	<54	Regression Kink Design: Identification exploits a kink in the unemployment insurance rule	Duration Elasticity 1.5-2.0.

Table A2

Fuzzy Regression Discontinuity Results for the Impact of the Benefit Cut using Alternative Bandwidths. Standard Errors in Parentheses

	Age 18	Age 19
First Stage:	0.70***	0.40***
Effect on Proportion Treated	(0.04)	(0.04)
Effect of Treatment on	**-61.30*** **	**-37.70**
Unemployment Duration	**(15.95)**	**(24.27)**
N	9914	7465
Effective N	2291	2123
Optimal Bandwidth (days)	47.77	52.29
Alternative Bandwidths		
Twice Optimal Bandwidth	-62.65***	-36.60**
	(10.72)	(14.35)
Half Optimal Bandwidth	-57.25**	-91.10**
	(24.74)	(43.66)
One Month	-49.50*	-71.529
	(26.11)	(45.424)
Two Months	-62.30***	-52.86**
	(17.16)	(25.15)
Three Months	-59.18***	-38.02**
	(13.54)	(19.44)
Four Months	-60.72***	-40.15**
	(11.43)	(15.91)
Five Months	-63.69***	-36.10**
	(10.21)	(14.16)
Six Months	-66.95***	-34.93***
	(9.27)	(13.11)

Notes: *** Denotes significant at the 1 percent level. ** Denotes significant at the 5 percent level. * Denotes significant at the 10 percent level.

Table A3

Fuzzy Regression Discontinuity Results for the Impact of the Benefit Cut, Controlling for Covariates Including Education. Standard Errors in Parentheses

	Age 18		Age 19	
	Without Covariates	With Covariates§	Without Covariates	With Covariates§
First Stage:	0.70***	0.70***	0.40***	0.39***
Effect on Proportion Treated	(0.04)	(0.04)	(0.04)	(0.05)
Effect of Treatment on	**-55.89*****	**-54.09*****	**-45.12**	**-42.26**
Unemployment Duration	**(16.92)**	**(16.39)**	**(27.54)**	**(28.01)**
N	8909	8909	6582	6582
Effective N	1997	1997	1931	1931
Optimal Bandwidth (days)	46.99	46.99	54.01	54.01

Notes: § The included covariates are education, gender, nationality, and previous employment. *** Denotes significant at the 1 percent level.

APPENDIX B

Hazard Analysis

While the RD approach provides information on the average duration effect, a hazard function approach provides information on the timing of the exits out of unemployment. We follow previous work (Meyer 1990) and specify a continuous time reduced form proportional hazards model with a flexible baseline hazard:

(B1) $$h_{\mathrm{i}}(t) = h_0(t)\exp[\boldsymbol{X}_{\mathrm{i}}(t)'\boldsymbol{\beta}]$$

where $h_0(t)$ is the baseline hazard at time t, $\boldsymbol{X}_{\mathrm{i}}(t)$ is a vector of possibly time-varying covariates for individual i at time t and $\boldsymbol{\beta}$ is a vector of unknown parameters.

The key to our empirical approach is the specification of $X_{\mathrm{i}}(t)'\boldsymbol{\beta}$. As with the RD approach, we compare individuals entering before and after the legislated cut April 29, 2009. When estimating the hazard functions we regard those who commenced a spell in the month following April 29 as the treatment group and those who commenced a spell in the month prior to April 29 as the control group. To account for any time of year effects that may cause durations for those entering prior to April 29 to differ from those entering after this date, we adopt a Difference-in-Difference (DiD) specification, which also includes spells from the same months in 2008. Using maximum likelihood, we estimate the following separately for 18 and 19 year olds:

(B2) $$\boldsymbol{X}_{\mathrm{i}}(t)'\boldsymbol{\beta} = \boldsymbol{Z}_{\mathrm{i}}(t)'\boldsymbol{\theta} + \alpha_1 T_{\mathrm{i}} + \delta D_{2009,\mathrm{i}} + \phi T_{\mathrm{i}} D_{2009,\mathrm{i}}$$

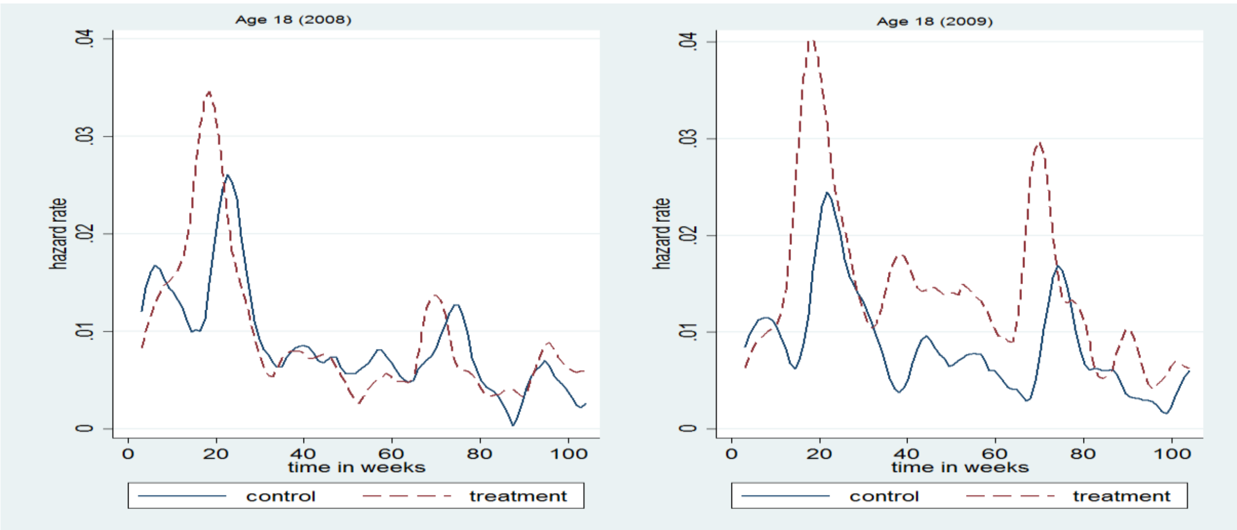
$\boldsymbol{Z}_{\mathrm{i}}(t)$ is a vector of covariates including gender, nationality, education, and previous employment; T_i is a dummy variable indicating entry after April 29; and $D_{2009,\mathrm{i}}$ is a dummy variable indicating entry into unemployment in 2009. The parameter of interest is ϕ, which measures the change in the hazard resulting from the cut in benefit payments.

We begin by presenting Kaplan-Meier nonparametric hazard functions for the control and treatment groups, for both the pre-intervention and the intervention years. For this analysis the treatment groups consist of those commencing a spell in the month after April 29 and the control groups are those entering one month earlier. The treatment year is 2009. The estimated hazards are shown in Figure B1. In all four graphs, there are pronounced seasonal peaks that coincide with the September start of the academic year. However, it is the difference between the treatment and control groups on each graph that are of interest in considering the effect of the benefit cut. Looking first at Figure B1(a) we see very little difference in the hazard functions for 18 year olds entering in 2008, when there was no treatment. However, this changes markedly in 2009 when the hazard for those entering after April 29, the date of the legislated benefit cut, is consistently higher. 18 year olds subject to the benefit cut were more likely to leave unemployment in almost every week following the commencement of their spell. Figure B1(b) indicates a similar but weaker pattern for 19 year olds.

To examine these changes more formally, we estimate the hazard DiD model given by Equations B1 and B2, and present the results in Table B1. The results shown are for the proportional hazard model, specifying a quadratic in duration to capture a nonlinear baseline hazard. Looking at the control variables, it appears that neither gender nor nationality had an impact on the likelihood of exit. However, lower educated workers and those with no previous job were less likely to exit. The key parameter is the coefficient on the interaction term between year and month of entry. We see a significant effect of the legislation for 18 year olds, while the effect is positive but not significant for 19 year olds. The results from the estimated hazard imply that 18 years olds entering after the legislation were 26 percent more likely to exit their JA spell than those in receipt of the higher benefit.

Figure B1: Kaplan-Meier Unemployment Exit Hazard Functions, Entrants to Unemployment One Month Before and After April 29, 2008 (left panel) and 2009 (right panel)

(a) Age 18



(b) Age 19

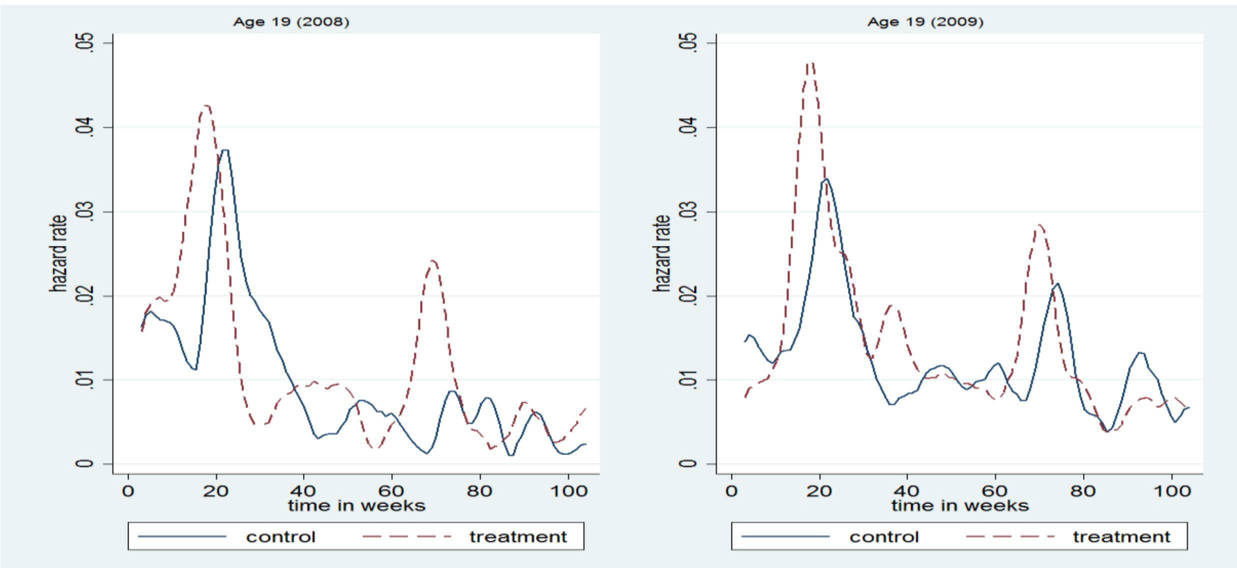


Table B1

Difference-in-Difference Hazard Function Results. Standard Errors in Parentheses

	Age 18	Age 19
Treatment Months	-0.13**	-0.06
	(0.08)	(0.08)
Treatment Year	0.10	0.09
	(0.07)	(0.08)
Treatment Months x Treatment Year	**0.23*** **	**0.09**
	(0.09)	**(0.10)**
Nationality Irish	-0.12	-0.02
	(0.09)	(0.10)
Low Education	-0.47***	-0.26***
	(0.05)	(0.06)
No Previous Employment Spell	-0.33***	-0.28***
	(0.04)	(0.06)
Male	-0.01	-0.03
	(0.05)	(0.05)
t	-0.01***	-0.01***
	(0.001)	(0.001)
$t^2/100$	0.003***	0.003***
	(0.0002)	(0.0002)
Constant	-3.73***	-3.77***
	(0.11)	(0.12)
N	2188	1689

Notes: Reference year for Difference-in-Difference estimation is one year earlier in each case. *** Denotes significant at the 1 percent level. ** Denotes significant at the 5 percent level.

APPENDIX C

Wage Analysis

Given the importance of exits to work, we examine these exits in more detail in this appendix. In a simple job search model, faster exits to work following a benefit cut arise as a result of increased search intensity and/or lower reservation wages. While we have no data on search intensity, we do have information on annual earnings and weeks worked for every year in which the individual worked. This allows us to calculate weekly earnings in the year the claimant exited unemployment. However, because the earnings data refer to the entire calendar year, they may not refer to the earnings actually received on exiting unemployment for those who have multiple employment spells in their exit year. Moreover, for people who exit in 2008 or 2009, the available annual earnings may also include income from an employment spell earlier in the exit year. Although we cannot directly identify the affected observations, we have experimented with excluding the groups most likely to be problematic. Our results are not sensitive to these exclusions and so we present the results based on all exits to work.

When we focus on those who exit to work, the sample sizes are relatively small if we use only those individuals entering unemployment one month before and after the treatment. For this reason, we use those entering unemployment six months before (control) and six months after (treatment) the legislation when considering exit wages. Figure C1 plots the density of accepted wages for 18 and 19 year olds. For context, we also include lines at €270 and €304, which correspond to youth subminimum weekly wages for 18 and 19 year olds respectively, based on a 39 hour working week. We see that the average wages accepted by these workers typically correspond to low paid minimum wage level jobs, as might be expected given their characteristics. The wage densities for the treatment and control groups are quite similar and suggest only a limited role for lower reservation wages in explaining the faster exits to work in response to the benefit cut.

While the wage densities provide a useful summary of accepted wages, they are not sufficient to determine the impact of a benefit cut on wages. As noted by Schmieder, von Wachter and Bender (2016), changes to the benefit system change post-unemployment wages through two channels. Firstly, a benefit cut may shift the post-unemployment wage path down; the accepted wage at a given duration falls. Secondly, the benefit cut may change the distribution of claimants along the post-unemployment wage path; those subject to the cut may have shorter durations. The densities given in Figure C1 combine both effects, which may offset each other in aggregate. To identify the shift in the path of post-unemployment wages, we follow Schmieder, von Wachter and Bender (2016) and estimate post-unemployment wages conditional on the duration of the unemployment spell. To allow for possible time of year effects, we estimate a DiD model using 2008 as the control year. Formally, we estimate

$$\text{(C1)} \qquad W_{\mathrm{i}} = \boldsymbol{Z}_{\mathrm{i}}'\boldsymbol{\theta} + \alpha_1 T_{\mathrm{i}} + \delta D_{2009,\mathrm{i}} + \phi \mathrm{T}_{\mathrm{i}} D_{2009,\mathrm{i}} + \beta_1 Dur_{\mathrm{i}} + \beta_2 Dur_{\mathrm{i}}^2 + \varepsilon_{\mathrm{i}}$$

W_{i} is the weekly post-unemployment wage. As was the case in the DiD hazard model, $\boldsymbol{Z}_i$ is a vector of covariates including dummies for nationality, gender, education, and previous employment; T_i is a dummy variable indicating entry to unemployment in the six months following April 29; $D_{2009,\mathrm{i}}$ is a dummy variable indicating entry into unemployment in the treatment year. Dur_{i} measures the duration (in months) of the relevant unemployment spell, and we include a quadratic in duration to allow for a nonlinear post-unemployment wage path. If individuals reduce reservation wages in response to longer spells of unemployment, we would expect the duration effect to be negative. The key parameter of interest is ϕ, the interaction term that measures the shift in the post-unemployment wage path resulting from the cut in benefit payments.

The results of this model are given in Table C1. The coefficients on unemployment duration indicate that longer spells of unemployment reduce post-unemployment wages. However, in keeping with the recent literature (Krueger and Mueller 2016), the effect sizes are relatively modest: an additional year of unemployment duration reduces post-unemployment exit wages by approximately 3.6 percent and 6.9 percent for 18 and 19 year olds respectively. The coefficient on the interaction term indicates no significant impact of the treatment on wages for either age group. Since there is no evidence that exit wages fell in response to the benefit cut, this leads us to infer that increased job search intensity rather than lower reservation wages explains the faster exits to work. This is plausible since the accepted wages of this group are already close to the minimum wage rate, so there is limited scope for reducing reservation wages.

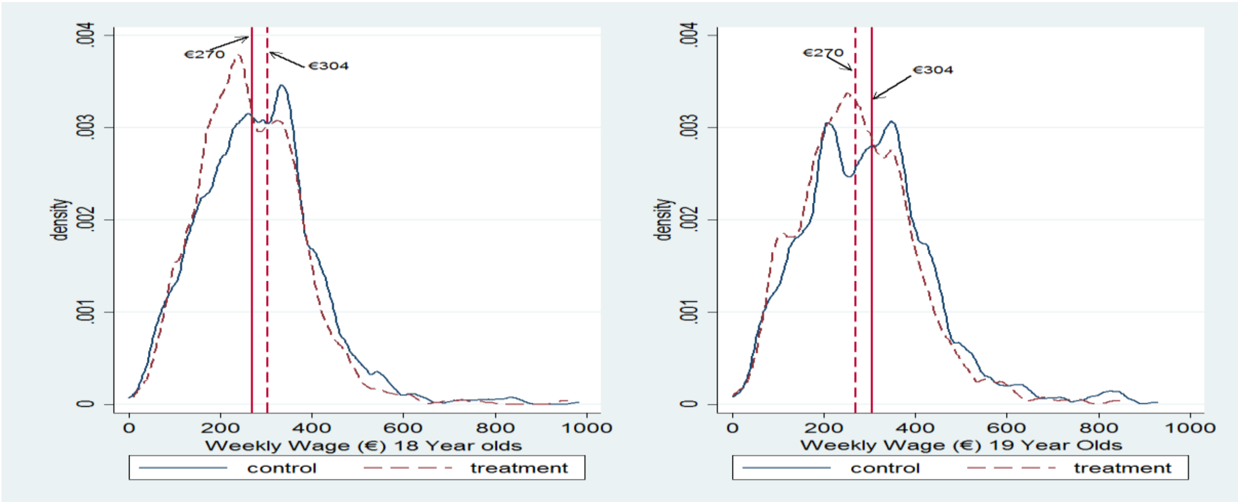
Table C1

Difference-in-Difference Model of Weekly Wages in Year of Exit from Unemployment. Standard Errors in Parentheses

	Age 18	Age 19
Treatment Month	-24.53*** (6.74	-16.95 (11.23)
Treatment Year	-13.70 (6.58)	0.77 (11.21)
Treatment Month x Treatment Year	**13.35** **(9.10)**	**-15.81** **(15.11)**
Nationality Irish	15.58 (11.12)	24.13 (17.04)
Low Education	4.77 (4.94)	16.37* (9.18)
No Previous Employment Spell	-10.44* (5.34)	-6.14 (11.61)
Male	58.06*** (5.00)	59.55*** (8.06)
Dur	-0.83** (0.34)	-1.70*** (0.64)
Dur^2	0.007 (0.005)	0.018* (0.01)
Constant	251.57*** (12.51)	257.67*** (19.21)
N	3432	3187

Notes: Reference year for Difference-in-Difference estimation is one year earlier in each case. *** Denotes significant at the 1 percent level. ** Denotes significant at the 5 percent level. * Denotes significant at the 10 percent level.

Figure C1: Kernel Densities of Weekly Wages in Year of Exit from Unemployment by Treatment and Control Groups, 18 Year Olds (left Panel) and 19 Year Olds (right Panel)



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