Online Appendix

A Dynamic matching estimator

In Subsection 5.1 we provide results from using the dynamic matching estimator proposed by Vikström (2017)[1]. Here we provide a formal definition of the estimator. The notation is as defined ias, T_i is duration until employment, S_i is duration until program participation, $Y_{t,i}$ is an indicator equal to 1 if $T < t$ and 0 for $T \geq t$. The treatment effect on treated survivors is estimated by the difference in survivor functions between treated and non-treated, where non-treated observations are reweighted to resemble the composition of the treated in terms of the observed characteristics. The weight are re-estimated for every time period to take the changing compositions (due to dynamic exits) into account. The estimator is

$$\begin{aligned}\widehat{\text{ATTS}}(t,s) &= \prod_{k=s}^{t}\left[1 - \frac{\sum_i Y_{k,i}\mathbb{1}_{(T_i>k-1)}\mathbb{1}_{(S_i=s)}}{\sum_i \mathbb{1}_{(T_i>k-1)}\mathbb{1}_{(S_i=s)}}\right] \\ &\quad - \prod_{k=s}^{t}\left[1 - \frac{\sum_i \hat{w}_i(s,k)Y_{k,i}\mathbb{1}_{(T_i>k-1)}\mathbb{1}_{(S_i>k)}}{\sum_i \hat{w}_i(s,k)\mathbb{1}_{(T_i>k-1)}\mathbb{1}_{(S_i>k)}}\right] \qquad (1)\end{aligned}$$

Where the weights are given by:

$$\hat{w}_i(s,k) = \frac{\hat{p}_s(X_{i,s})}{1-\hat{p}_s(X_{i,s})}\frac{1}{\prod_{m=s+1}^{k} 1-\hat{p}_m(X_{i,m})} \qquad (2)$$

The weights contain $\hat{p}_s$, which is the propensity score $(\Pr(S = s|X_{i,s}; S \geq s; T > t-1)$, estimated using a logit regression. The estimator can straightforwardly be extended to take selective right-censoring into account. For the details we refer to Vikström (2017).

B Timing-of-events model

The timing-of-events model contains two hazard rates, which have a mixed proportional specification. The job finding rate is given by:

$$\theta_e(t|x,\tau_0,s,v_e) = \phi_e(t)\psi_e(\tau_0+t)\exp\Big[x\beta_e + \delta_{t-s}I(t>s)\Big]v_e \qquad (3)$$

and the entry rate into the program by:

$$\theta_p(s|x,\tau_0,v_p) = \phi_p(s)\psi_p(\tau_0+s)\exp(x\beta_p)v_p \qquad (4)$$

We specify the duration dependence patterns $\phi_e(t)$ and $\phi_p(s)$ as piecewise constant, so $\phi_j(t) = \exp\Big(\sum_{m=1}^{M}\pi_{jm}I_m(t)\Big)$ for $j = e,p$), where $I_m(t)$ describes duration

[1] Vikström, J. (2017). Dynamic treatment assignment and evaluation of active labor market policies. *Labour Economics*, 49:42-54

intervals with thresholds after $3, 6, 9, 12, 18, 24$ months. The calendar time indicators $\psi_e(\tau_0 + t)$ and $\psi_p(\tau_0 + t)$ contain dummy variables for each quarter (e.g. 2008Q2, 2008Q3, ..., 2011Q3, 2011Q4).

The unobserved heterogeneity (v_e, v_p) is modeled using a discrete mass-point specification, so

$$p_k = \Pr(v_e = v_{ek}, v_p = v_{pk})$$

with unrestrictive mass points $v_{e1}, \dots v_{eK}$ and $v_{p1}, \dots, v_{pK}$ under the restriction $p_1 + \dots + p_K = 1$. We try different values of K. In practice, for higher values of K, the locations of some mass points converge.

The loglikelihood function takes exogenous right censoring of durations into account. The loglikelihood function is specified as

$$\begin{aligned}\log \mathcal{L} \;=\; \textstyle\sum_i \log \; & \Big[\sum_{k=1}^{K} p_k \cdot \theta_e(t_i|x_i, \tau_{0i}, s_i, v_{ek})^{c_{ei}} \cdot \exp\Big(- \int_0^{t_i} \theta_e(u|x_i, \tau_{0i}, s_i, v_{ek}) du \Big) \cdot \\ & \theta_p(s_i|x_i, \tau_{0i}, v_{pk})^{c_{pi}} \cdot \exp\Big(- \int_0^{s_i} h_a(u|x_i, \tau_{0i}, v_{pk}) du \Big) \Big]\end{aligned}$$

where t_i is the observed unemployment duration, s_i the observed duration before entering the program, c_{ei} denotes if the individuals finds work and c_{pi} describes if entry in the program is observed.

C Common trend assumption

Exploiting the policy discontinuity requires a common trend assumption. Below we discuss the justification of this assumption.

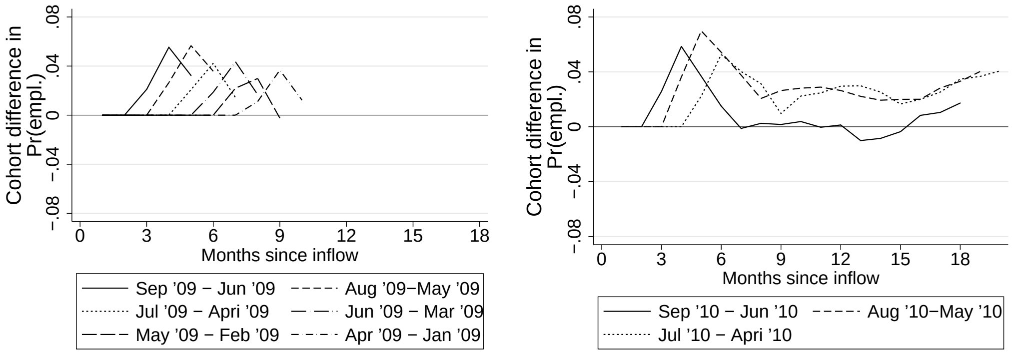
The assumption that the remaining terms in equations (9) and (10) are negligible has some similarities with the common trend assumption in a difference-in-differences estimator. The simple difference estimator require that in the absence of the policy discontinuity the employment rate of the January 2010 cohort would have been the same as the employment rate of the October 2009 cohort (and similar for the other comparisons). Similarly, the double difference estimator requires that in the absence of the policy discontinuity, the *difference* in employment rate between the January and October cohort would be the same in 2009/2010 as a year earlier in 2008/2009. This is by definition not testable.

We explore two ways of assessing the *plausibility* of the assumptions. First, we consider placebo treatment effects. In our setting, this implies imposing a *placebo policy discontinuity* at a point in time different from the real discontinuity. Guided by the constraints imposed by our observation time period, we impose a placebo treatment in November 2009 and estimate treatment effects as we do in the actual analysis. This implies comparing survivor functions for September 2009 - July 2009, for August 2009 - June 2009 and so forth. These simple differences are presented in panel (a) of Figure C1. Note that the lines are cut off whenever a cohort reaches the *actual* policy discontinuity in March 2010. The lines show that the cohort differences in employment are small and positive at early durations and reverse towards zero at later durations. We can also impose the placebo discontinuity *after* the real

Figure C1: Placebo treatment effects (quasi-experimental ITT)

(a) Simple differences: placebo discontinuity in November 2009

(b) Simple differences: placebo discontinuity in November 2010



discontinuity, which we do in panel (b). This has the advantage that we can include the differences at longer durations, and the disadvantage that we can only use three cohort comparisons (again to avoid interference of the real discontinuity). The results show that also in this case there are small positive employment differences at early durations, but they diminish at longer durations. Comparing these patterns with our real discontinuity estimates (panel (a) of Figure 11) is reassuring: the placebo and real estimates differ strongly and the placebo estimates are much closer to zero. If anything, the placebo estimates suggest that a small *positive* bias might apply at early durations, in which case the real estimates would constitute lower bounds of the impact of the programs. Note that estimating double differences for the placebo discontinuity is problematic because the cohorts from the prior year would be affected by the real discontinuity in panel (b) and by the turbulent labor market developments in 2008 for panel (a).

Our second approach to consider the plausibility of the common trend assumption is to investigate the survivor functions used in the main analysis, but over the first couple of months of each cohort, that is, *before* either cohort is affected by the policy discontinuity. Since all estimators condition on survival up to t_2, we can use information on job finding before t_2 to get some indication about the similarity of the cohorts. To have a sufficient number of pre-discontinuity months in the latest cohort, we focus on the comparisons of December 2009, November 2009 and October 2009.

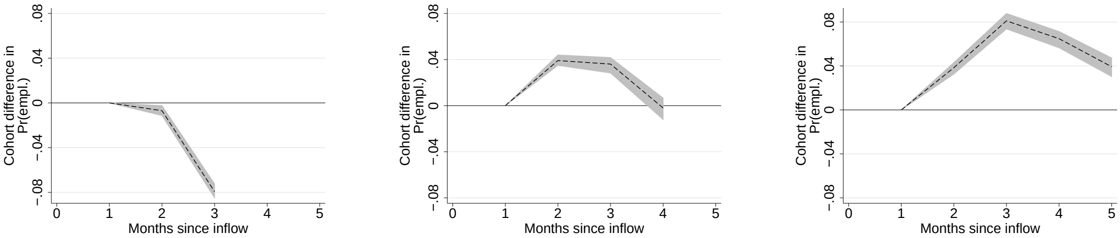
Estimates are presented in Figure C2 for single differences (panels (a), (b) and (c)), including 95% confidence intervals computed using bootstrapping. We find that for the single difference estimator, the first comparison (a) shows a negative and significant difference. On the other hand, the second and third comparisons (b) and (c) show a significant positive difference that diminishes over time. Although these suggest that the common trends assumption is violated and thus our estimates might be biased, the lack of a consistent pattern across the three comparisons makes us confident that the main conclusion drawn from the estimates in Figure 11 still

Figure C2: Common trend tests

(a) Dec. '09 - Sep. '09, simple dif.

(b) Nov. '09 - Aug. '09, simple dif.

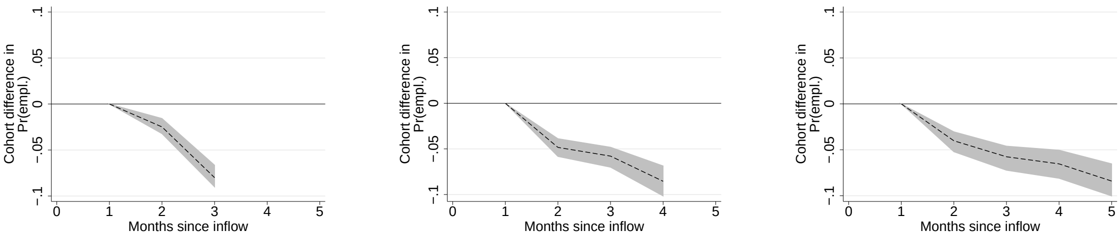
(c) Oct. '09 - Jul. '09, simple dif.



(d) Dec. '09 - Sep. '09, double dif.

(e) Nov. '09 - Aug. '09, double dif.

(f) Oct. '09 - Jul. '09, double dif.



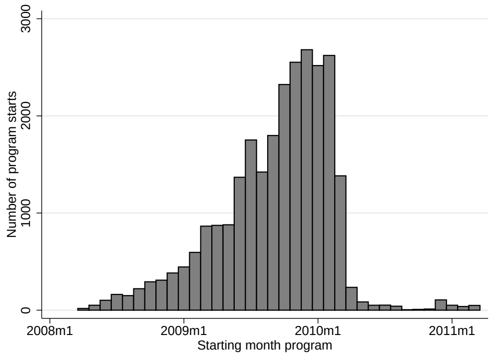
holds.[2] We perform a similar check for the double differences estimator in panels (d), (e) and (f) of Figure C2. Here the patterns are more similar across the comparisons, showing a significant negative difference. As a result, our estimates will be biased downwards somewhat, and thus provide lower bounds of the effect. This might explain that the magnitude of the negative impact seems particularly large in some comparisons. Overall, these results are not too surprising, as our approach can clearly not fully control for all business cycle variation in job finding rates.

[2] In particular, the three panels suggest that estimates from the December - September comparison are biased downwards, estimates from the November - August comparison are nearly unbiased and estimates from the October - July comparison are biased upwards. Given that all three comparisons yield similar results (see Figure 11), we argue that the bias is unlikely to alter the main conclusions.

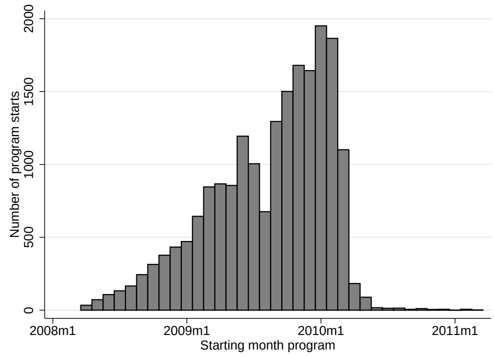
D Additional empirical material

Figure D1: Starting dates of externally provided programs

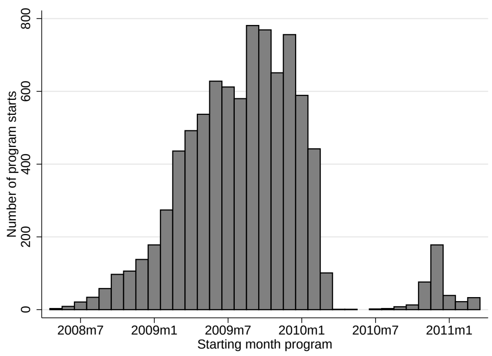
(a) IRO (Individual reintegration agreement)



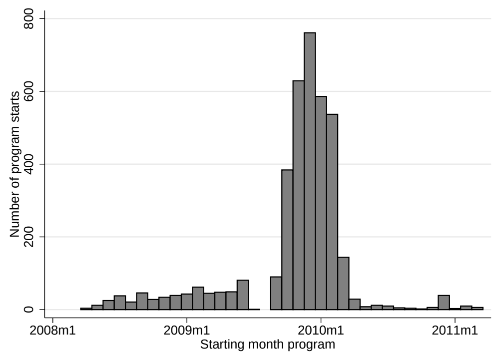
(b) Short training



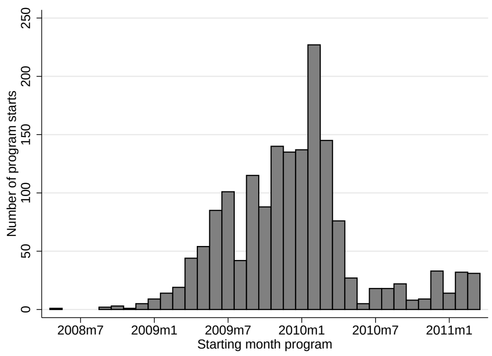
(c) Jobhunting



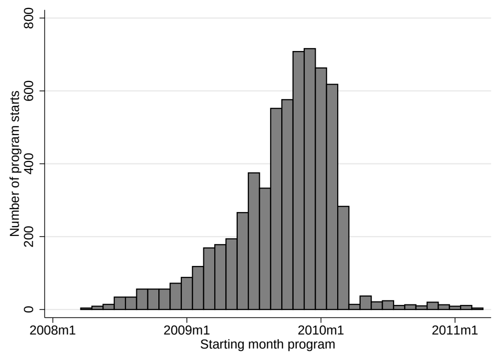
(d) Standard programs



(e) Traineeship

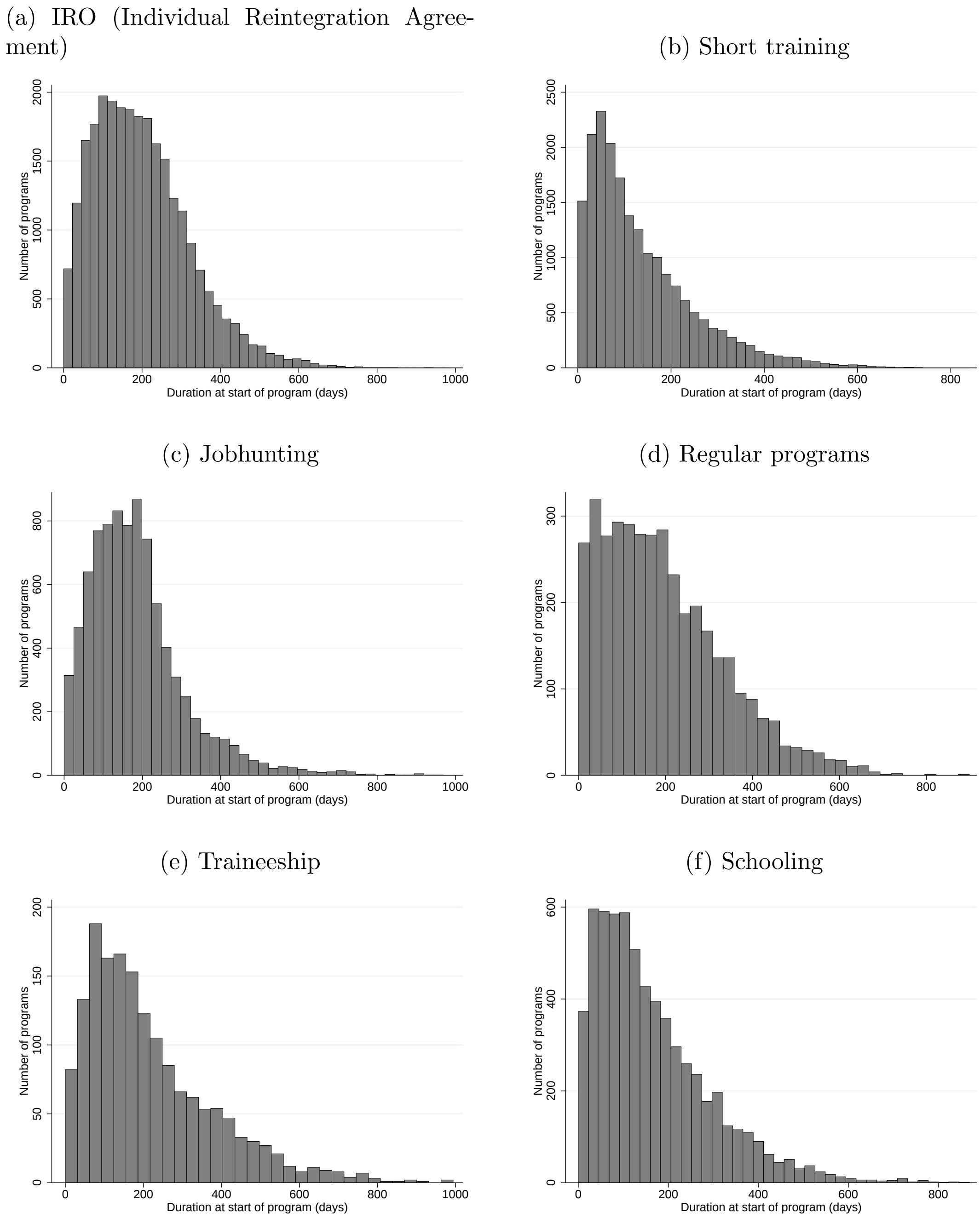


(f) Schooling



These figures present the density of starting (calendar) dates of the programs from external providers. Each bar represents one month, and the programs have been categorized into 6 types. The sudden discontinuation in March 2010 is clearly visible for each program type.

Figure D2: Timing of externally provided programs



(a) IRO (Individual Reintegration Agreement)

(b) Short training

(c) Jobhunting

(d) Regular programs

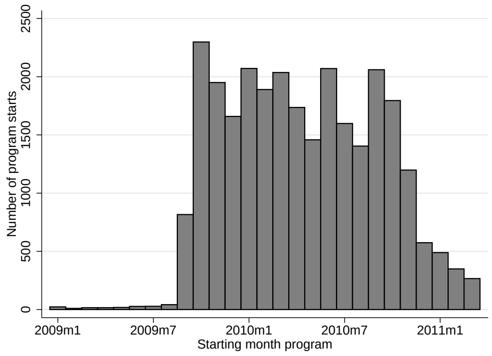
(e) Traineeship

(f) Schooling

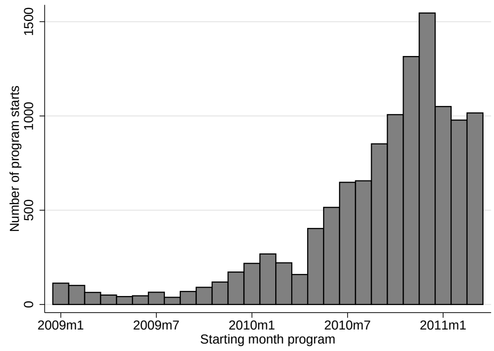
These figures present the density of elapsed unemployment duration at the start of an individual's first external program. The programs have been categorized into 6 types. Most programs start around 3 to 6 months after inflow in unemployment, although a considerable probability of starting a program at longer durations remains..

Figure D3: Starting dates of internally provided programs

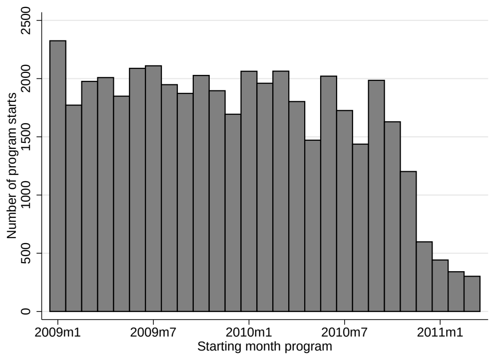
(a) CTC



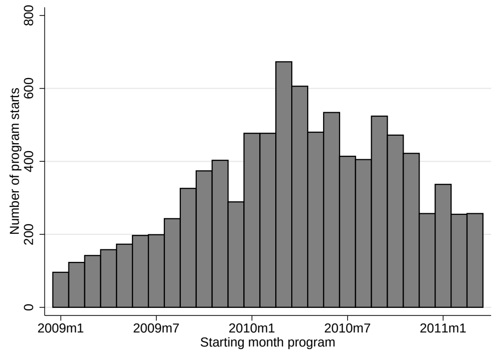
(b) Vacancy referral



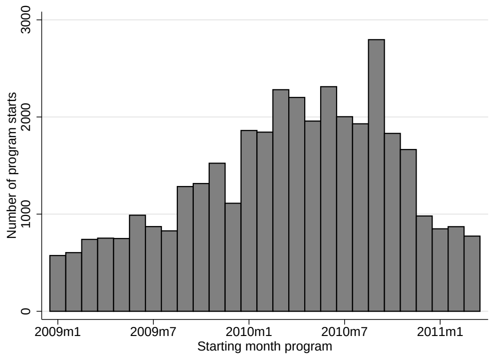
(c) Tests (different types)



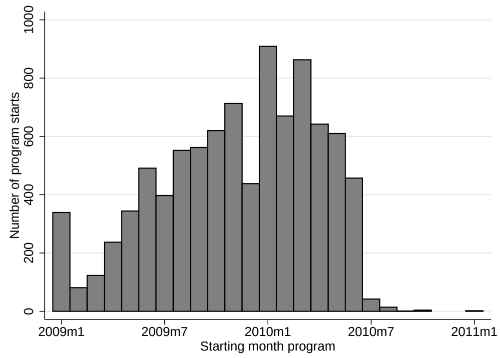
(d) Employment on trial basis



(e) Workshop

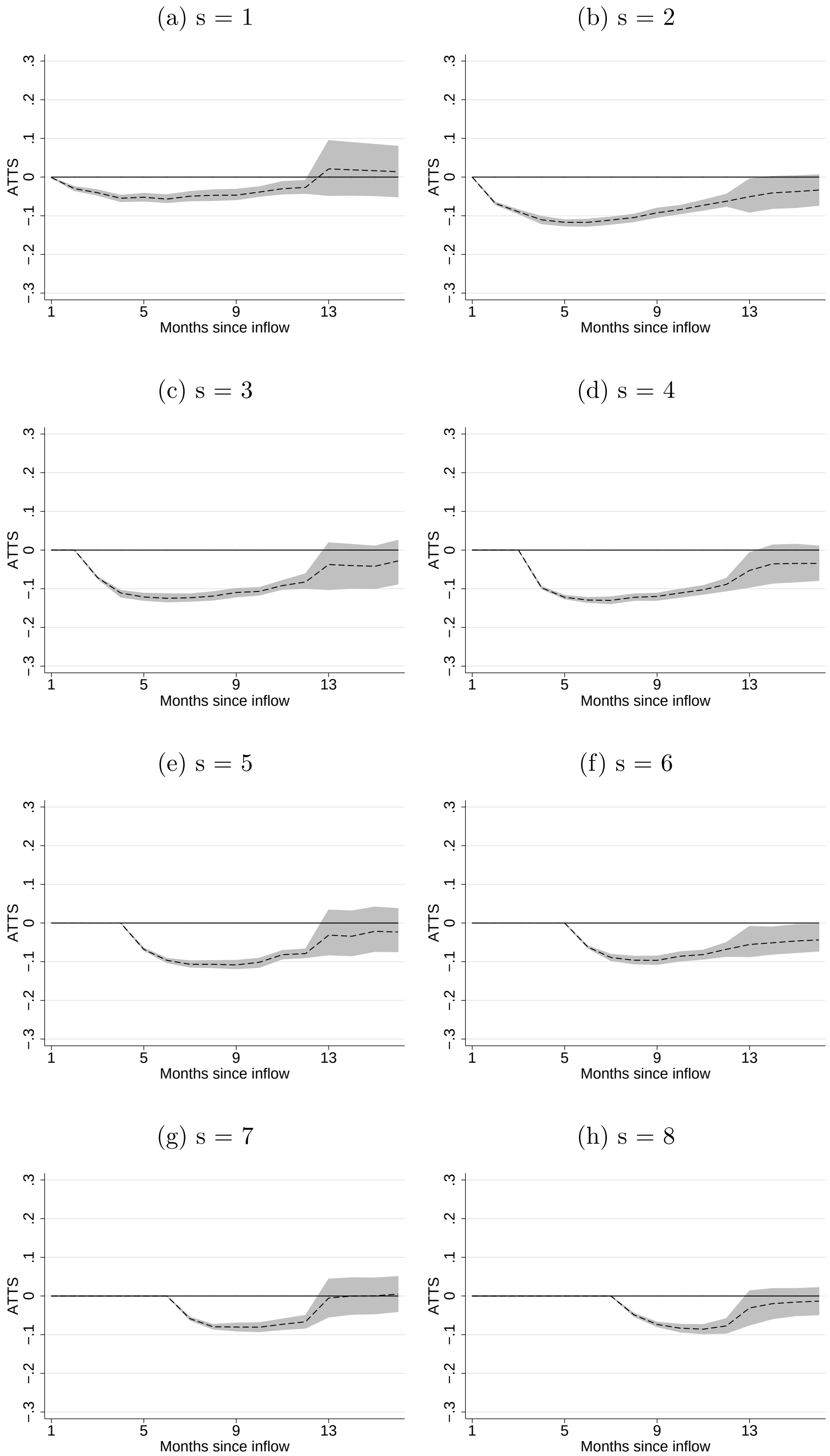


(f) Entrepeneurship support program



These figures present the density of starting (calendar) dates of the programs that are internally provided. Each bar represents one month, and the programs have been categorized into 6 types. While the incidence of the different programs varies across years, there is no indication of any response to the discontinuity of *externally* provided program in March 2010.

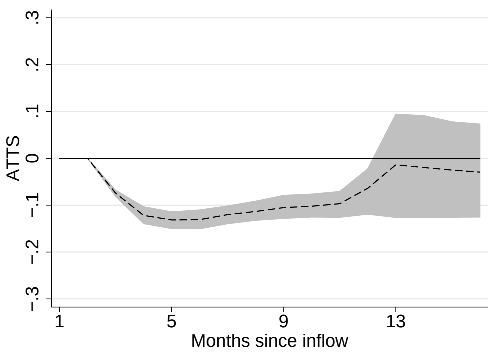
Figure D4: Dynamic matching estimator with 95% confidence intervals (full sample)



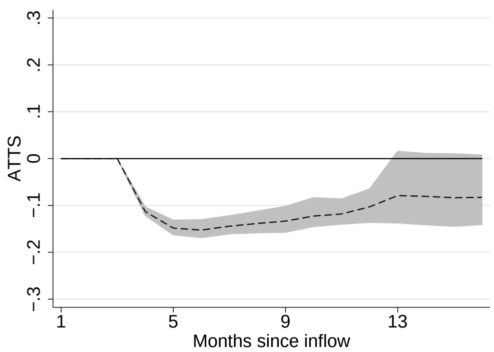
Average program impact on the employment probability for treated survivors (ATTS), estimated using a dynamic matching estimator on the full sample of UI recipients. The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D5: Dynamic matching estimator with 95% confidence intervals (discontinuity sample)

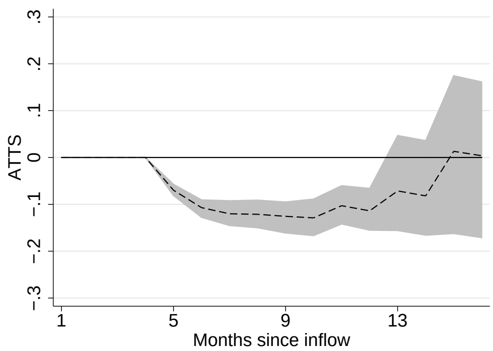
(a) s = 3



(b) s = 4

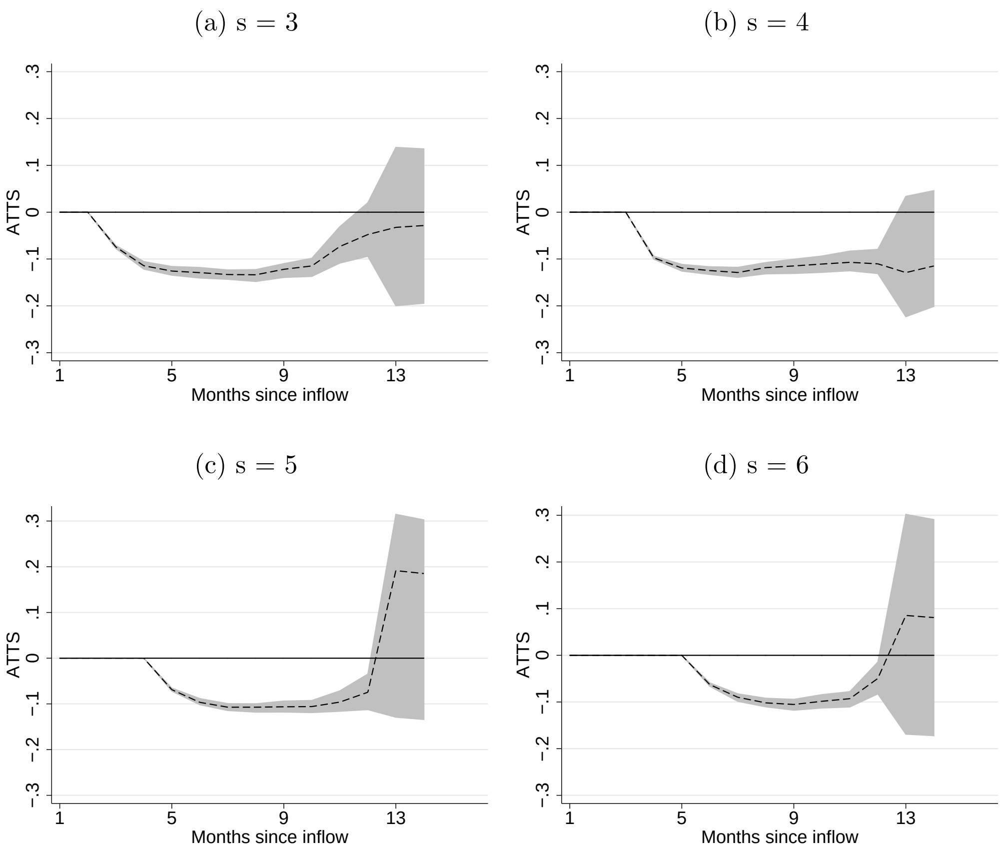


(c) s = 5



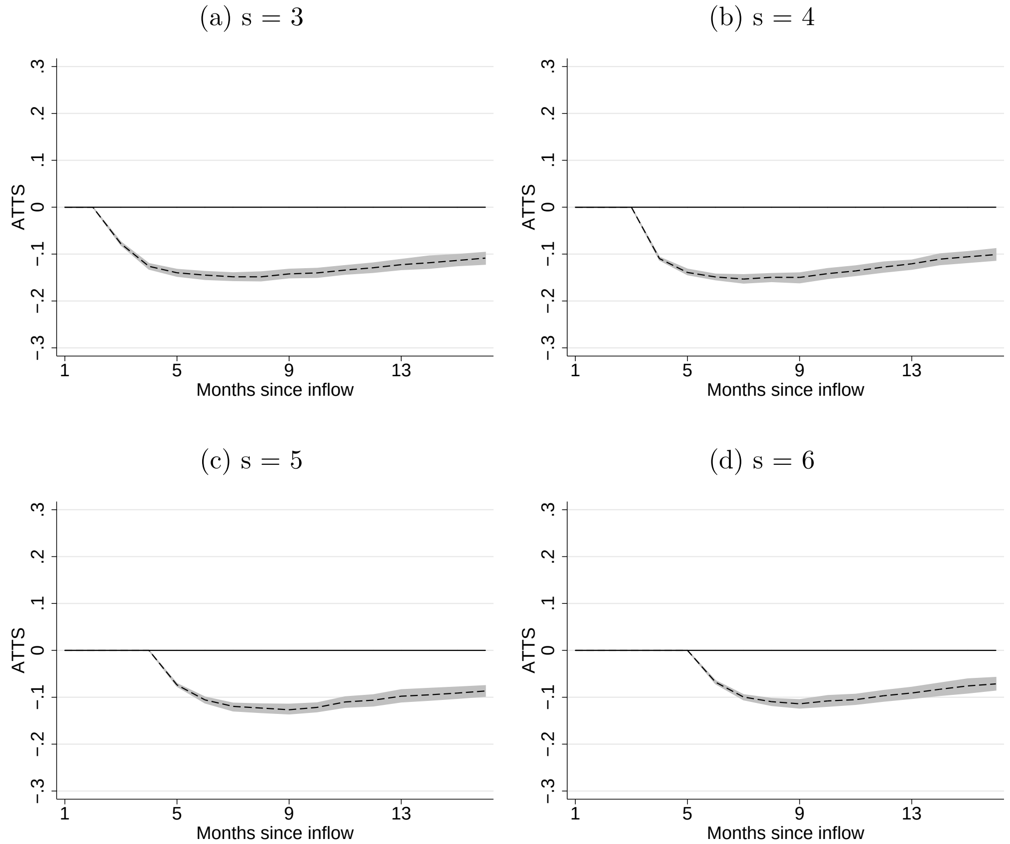
Average program impact on the employment probability for treated survivors (ATTS), estimated using a dynamic matching estimator on the "discontinuity sample" of UI recipients. The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D6: Dynamic matching estimator with 95% confidence intervals (censored sample)



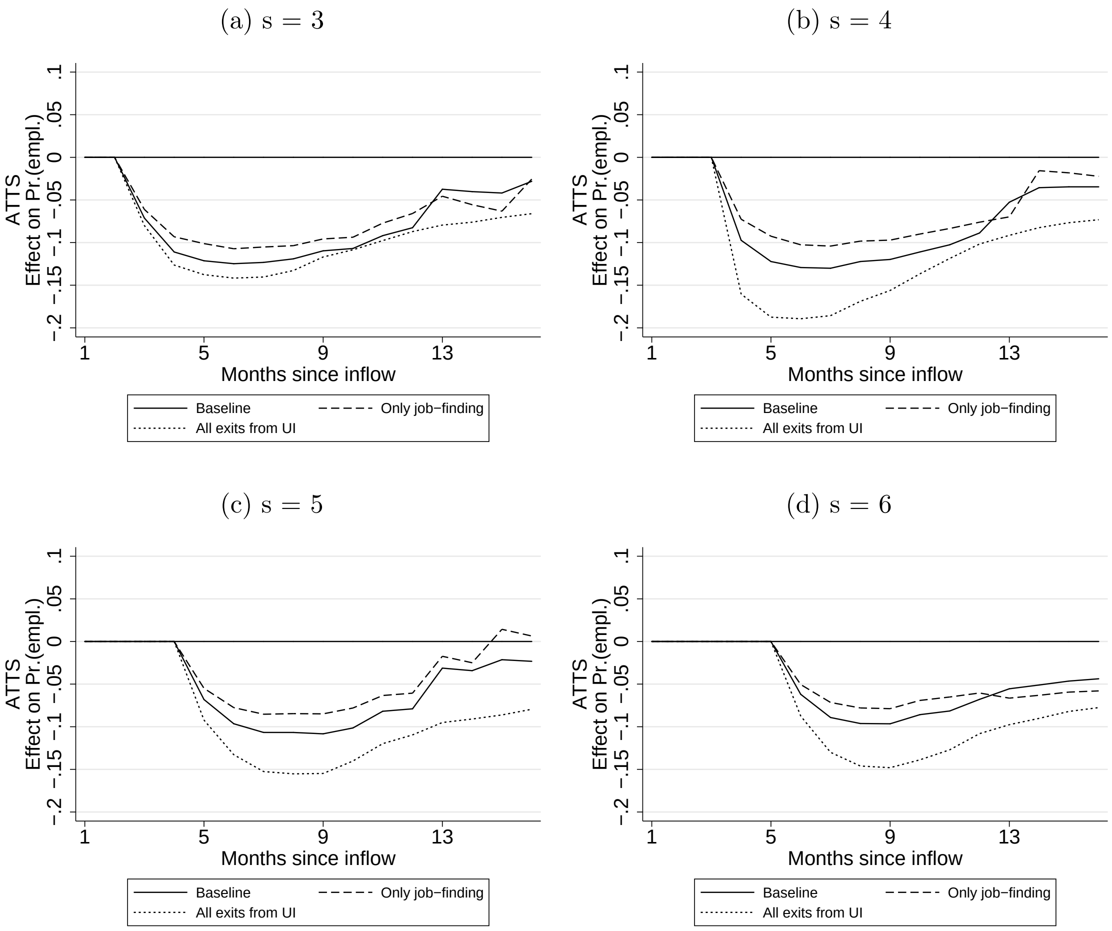
Average program impact on the employment probability for treated survivors (ATTS), estimated using a dynamic matching estimator on the sample of UI recipients that entered unemployment before the discontinuity ("pre-discontinuity sample"). The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D7: Dynamic matching estimator with 95% confidence intervals (only age dummies)



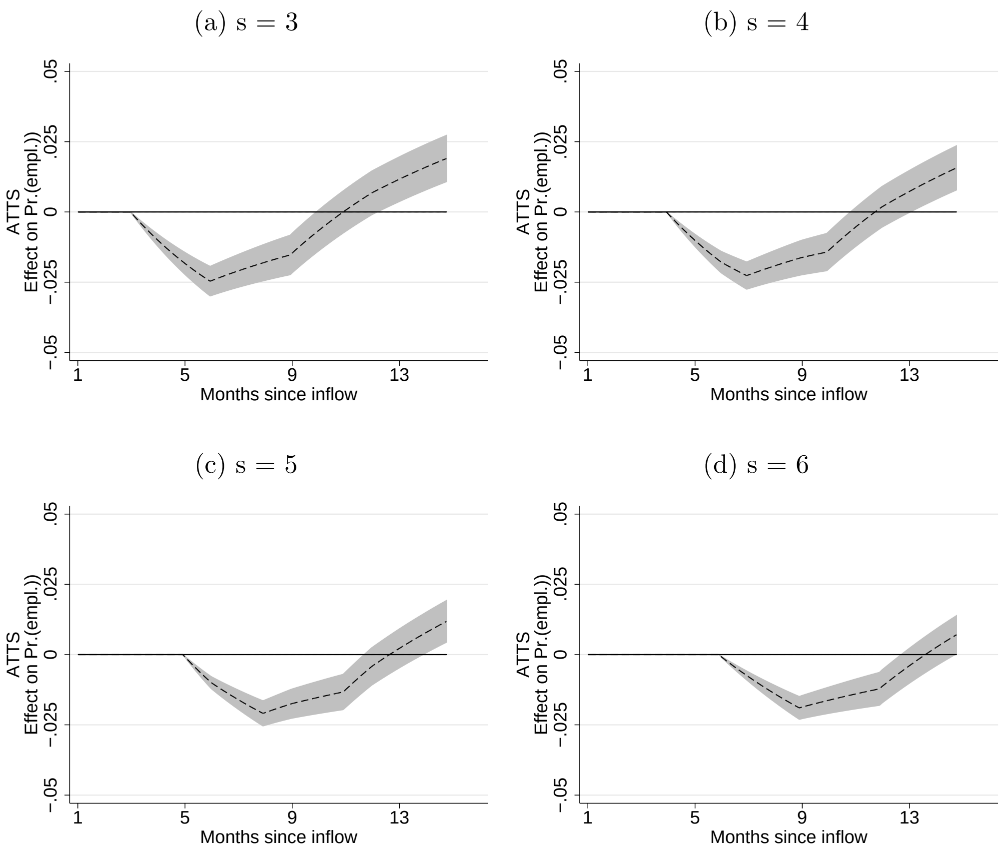
Average program impact on the employment probability for treated survivors (ATTS), estimated using a dynamic matching estimator on the full sample of UI recipients, *including only a set of age dummies to match on.* The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D8: Dynamic matching ATTS estimates: Comparison of different outcome measure definitions



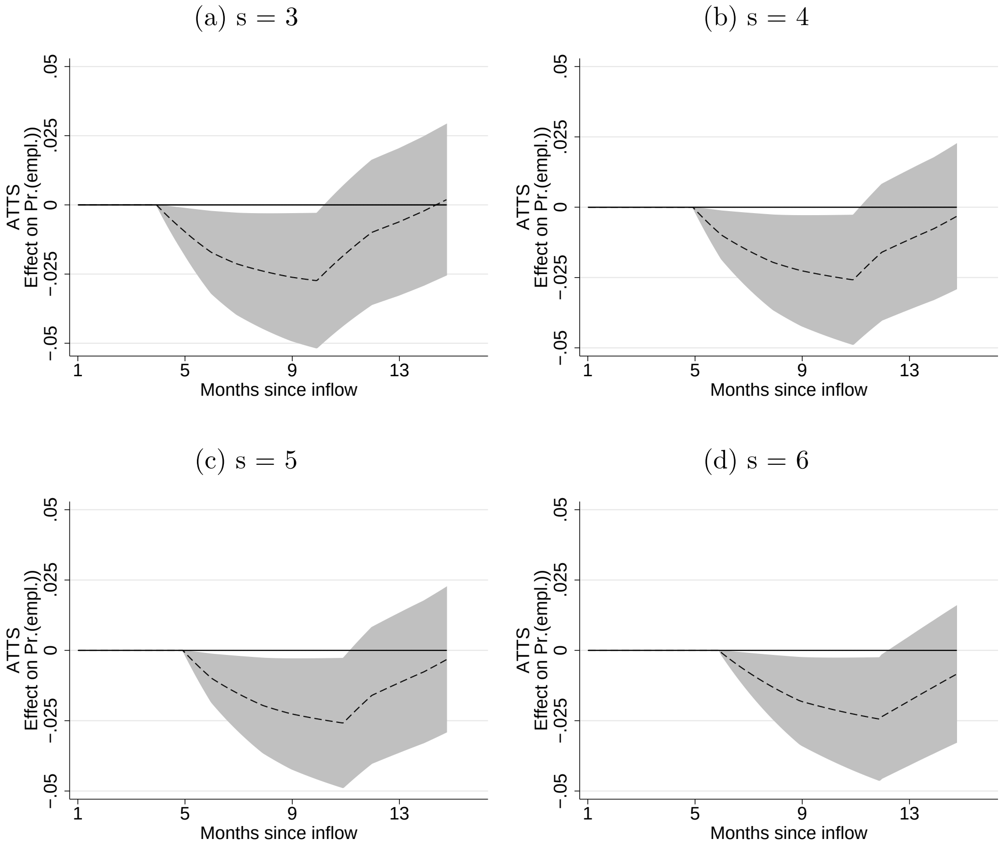
Average program impact on the exit probability for treated survivors (ATTS), estimated using a dynamic matching estimator on the full sample of UI recipients. The three line denote estimates based on different definitions of outcome measure (exits from UI). The baseline includes exits to employment and exits due to unknown reasons. The 'Only job-finding' estimates only consider exits to employment and the 'All exits from UI' considers exits from UI due to any reason.

Figure D9: Timing-of-events estimates with 95% confidence intervals (full sample)



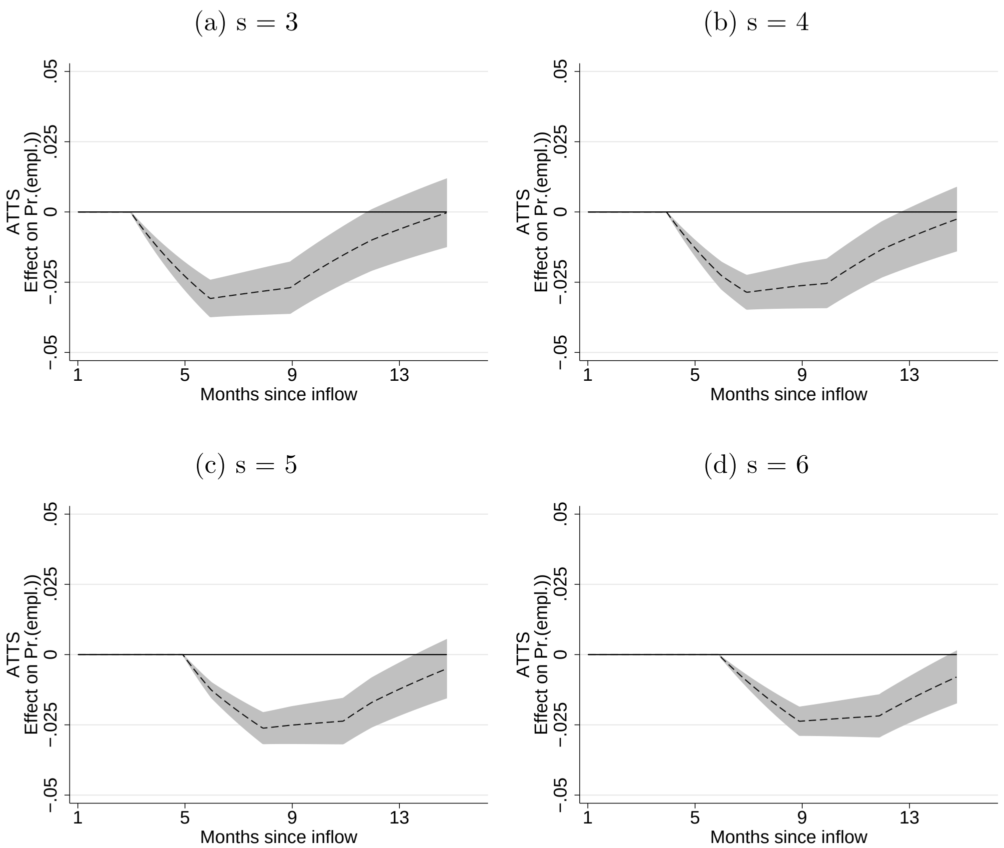
Average program impact on the employment probability for treated survivors (ATTS), estimated using the Timing-of-Events model on the full sample of UI recipients. The confidence intervals have been constructed using the delta method.

Figure D10: Dynamic matching estimator with 95% confidence intervals (discontinuity sample)



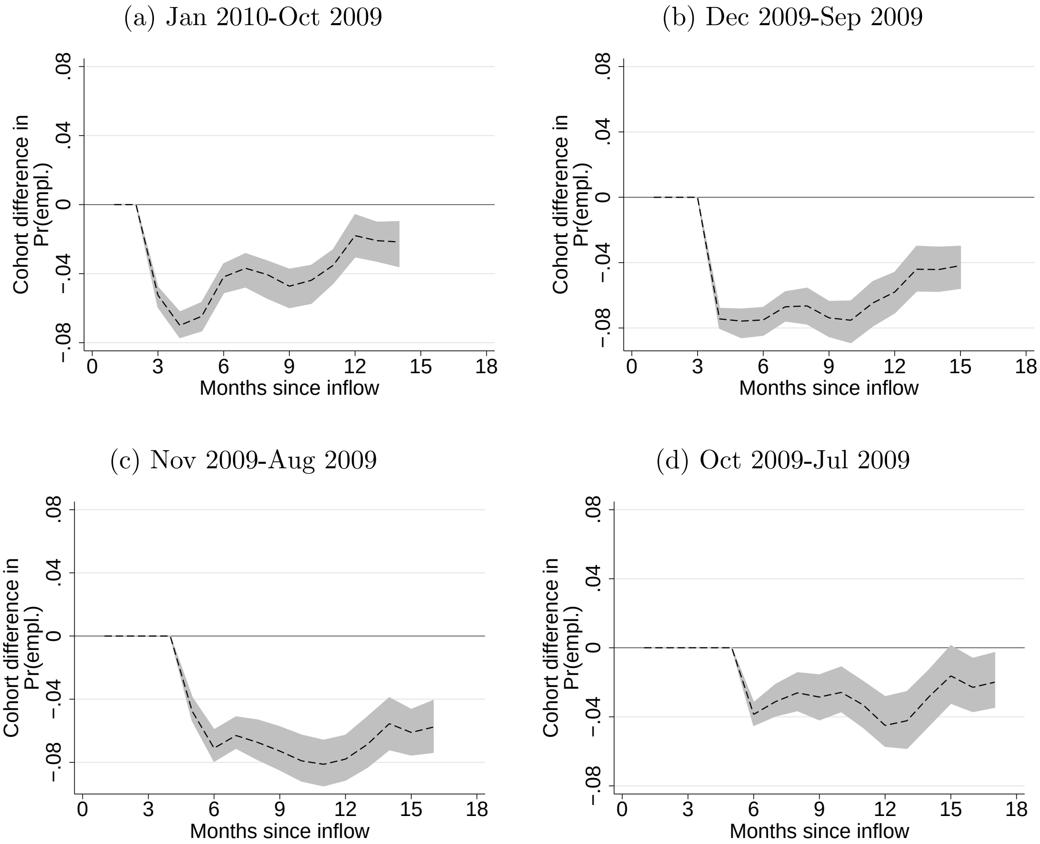
Average program impact on the employment probability for treated survivors (ATTS), estimated using the Timing-of-Events model on the "discontinuity sample" of UI recipients. The confidence intervals have been constructed using the delta method.

Figure D11: Timing-of-events estimates with 95% confidence intervals (pre-discontinuity sample)



Average program impact on the employment probability for treated survivors (ATTS), estimated using the Timing-of-Events model on the "pre-discontinuity sample" of UI recipients. The confidence intervals have been constructed using the delta method.

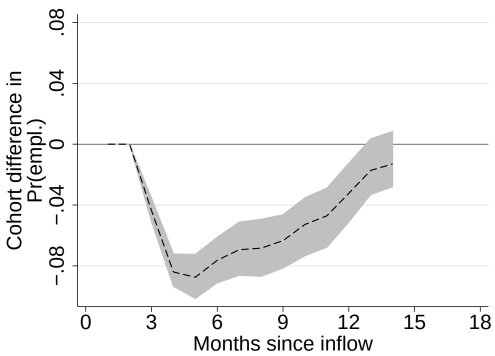
Figure D12: Experimental estimates (single differences) with 95% confidence intervals



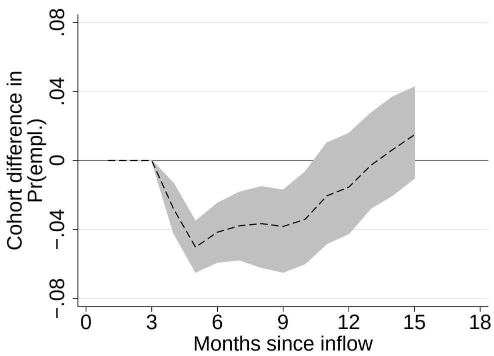
Average program impact on the employment probability (intention-to-treat), estimated using the program discontinuity as a natural experiment. Presented estimates are from the simple difference estimator (equation 9). The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D13: Experimental estimates (double differences) with 95% confidence intervals

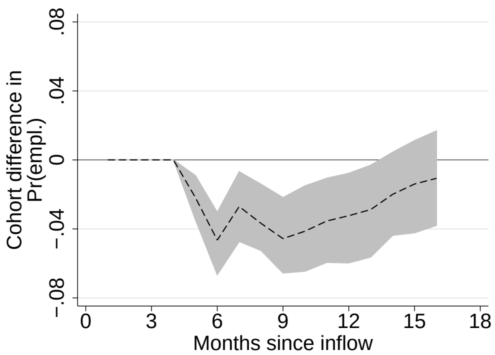
(a) Jan 2010-Oct 2009



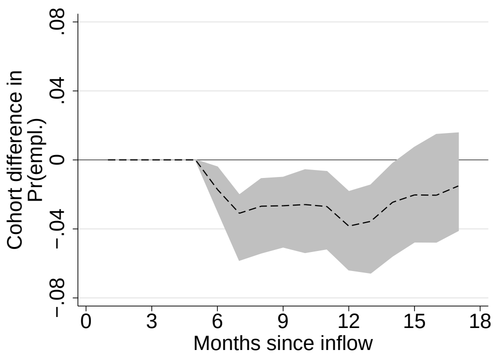
(b) Dec 2009-Sep 2009



(c) Nov 2009-Aug 2009

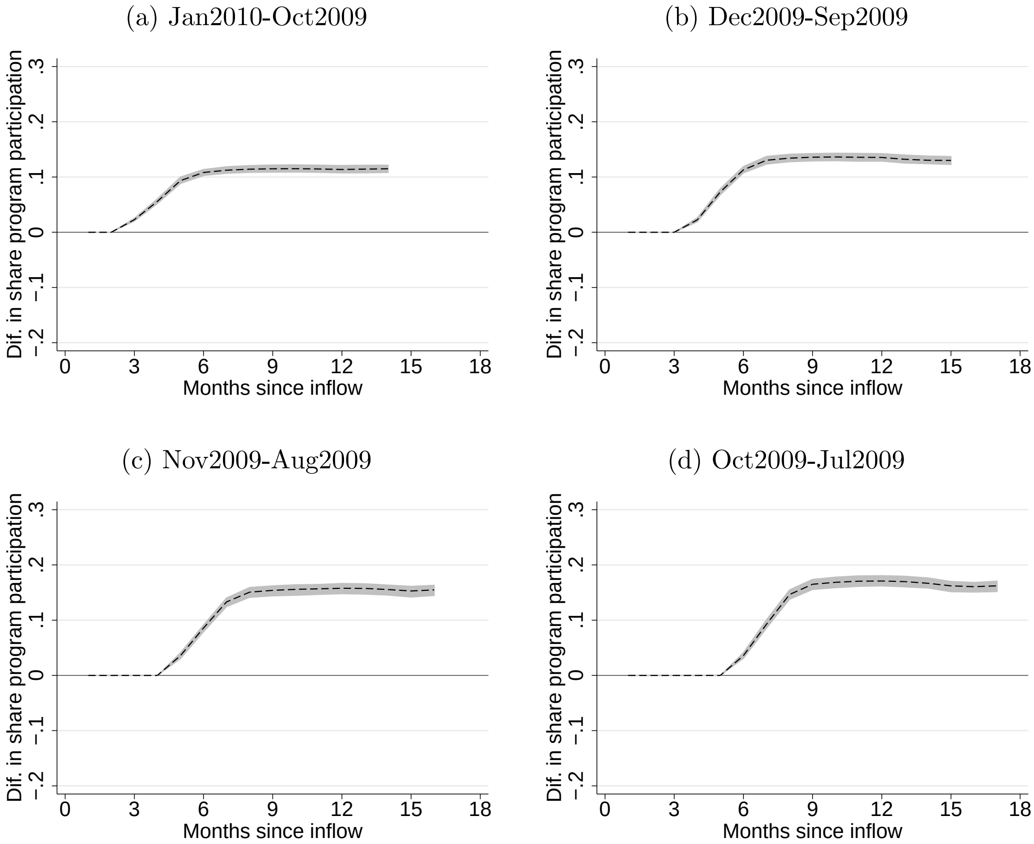


(d) Oct 2009-Jul 2009



Average program impact on the employment probability (intention-to-treat), estimated using the program discontinuity as a natural experiment. Presented estimates are from the double difference estimator (equation 10). The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D14: Single difference in treatment share with 95% confidence intervals



(a) Jan2010-Oct2009

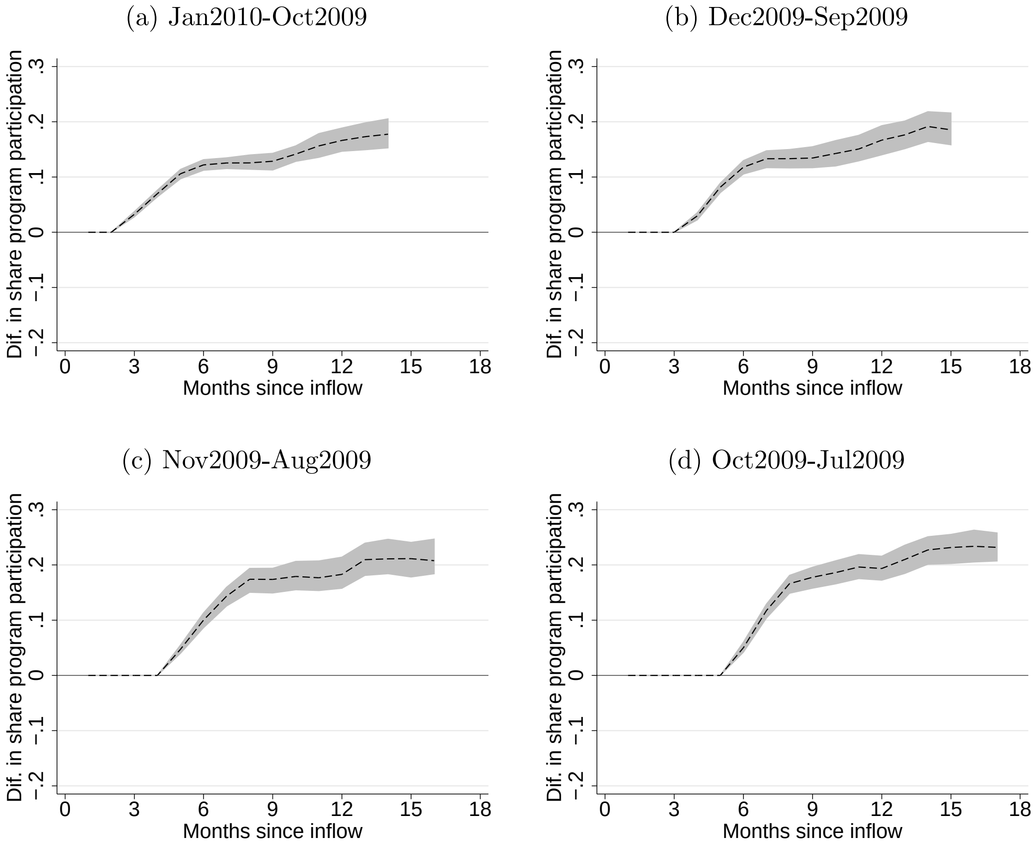
(b) Dec2009-Sep2009

(c) Nov2009-Aug2009

(d) Oct2009-Jul2009

Estimated (simple) difference in external program participation (see equation 11). The confidence intervals have been constructed using bootstrapping (100 repetitions).

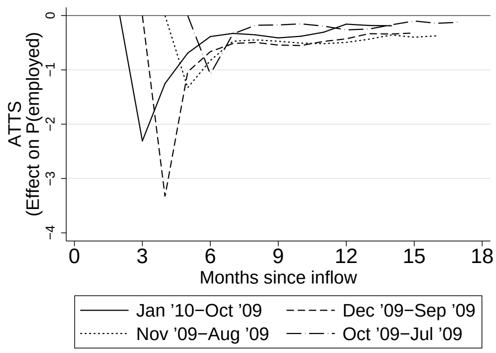
Figure D15: Double difference in treatment share with 95% confidence intervals



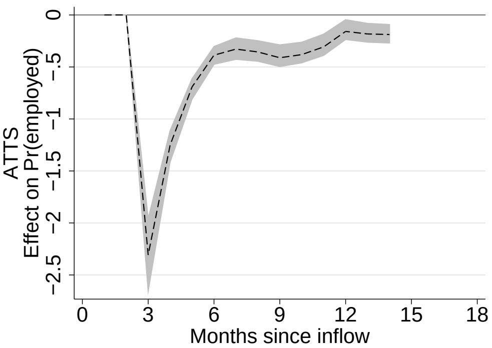
Estimated (double) difference in external program participation (see equation 11). The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D16: Average treatment effects (single differences), with 95% confidence intervals and including extreme values at early durations

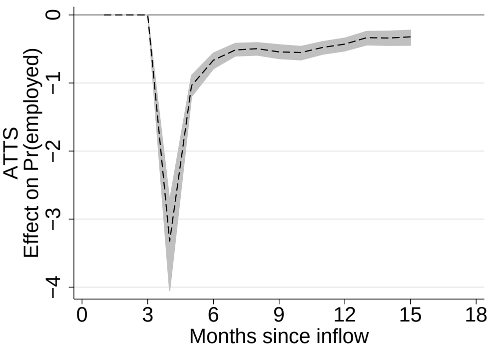
(a) All 4 comparisons



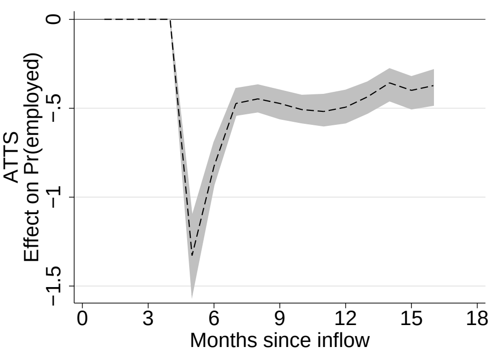
(b) Jan2010-Oct2009



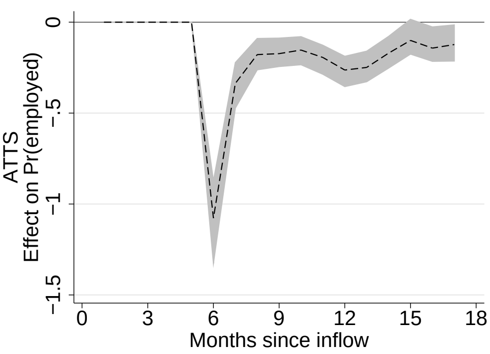
(c) Dec2009-Sep2009



(d) Nov2009-Aug2009



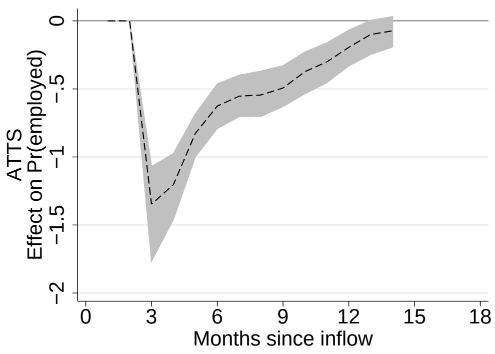
(e) Oct2009-Jul2009



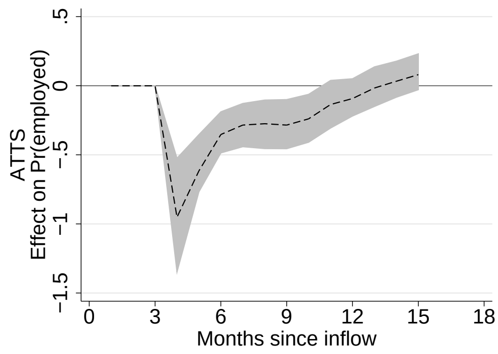
Average treatment effects (ATET) of program participation on the employment probability, estimated using the program discontinuity as a natural experiment. Presented estimates are from the simple difference estimator. The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D17: Average treatment effects (double differences) with 95% confidence intervals

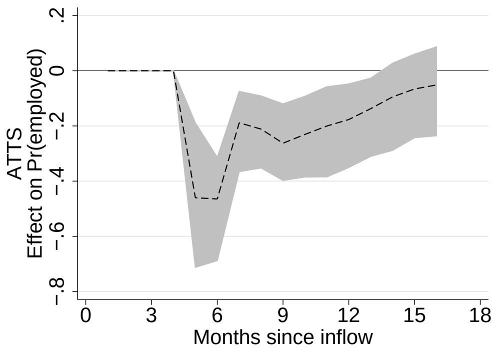
(a) Jan2010-Oct2009



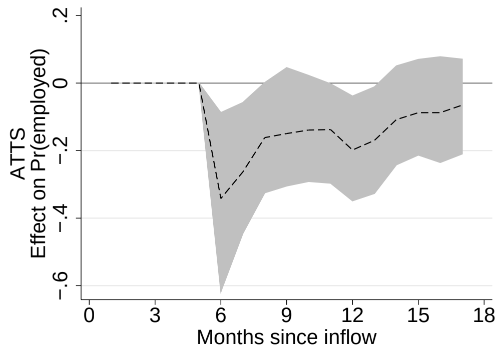
(b) Dec2009-Sep2009



(c) Nov2009-Aug2009



(d) Oct2009-Jul2009



Average treatment effects (ATET) of program participation on the employment probability, estimated using the program discontinuity as a natural experiment. Presented estimates are from the double difference estimator. The confidence intervals have been constructed using bootstrapping (100 repetitions).

Figure D18: ATET(t), based on matching estimates

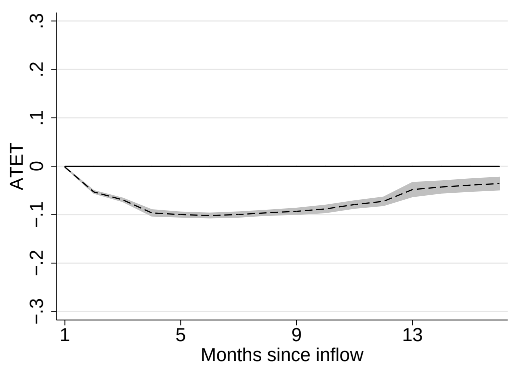


Table D1: Estimates Timing-of-Events model

	Full sample				Discontinuity sample				Pre-disc. sample			
	UI exit rate		Program rate		UI exit rate		Program rate		UI exit rate		Program rate	
	Coef.	st.er.	Coef.	st.er.	Coef.	st.er.	Coef.	st.er.	Coef.	st.er.	Coef.	st.er.
Program effect:												
Months 0-3	0.874	0.014			0.899	0.045			0.834	0.019		
Months 4-6	1.036	0.016			0.914	0.048			0.987	0.023		
Months 7-	1.247	0.015			1.223	0.047			1.181	0.028		
Individual characteristics:												
Female	0.973	0.004	1.045	0.011	0.940	0.010	1.062	0.025	1.049	0.006	1.045	0.011
Age 25-25	0.759	0.007	1.398	0.022	0.773	0.016	1.202	0.042	0.763	0.010	1.437	0.024
Age 35-45	0.645	0.009	1.625	0.024	0.655	0.019	1.329	0.047	0.665	0.012	1.672	0.026
Age 45-55	0.552	0.010	1.837	0.026	0.579	0.023	1.443	0.052	0.607	0.014	1.915	0.028
Age 55-60	0.298	0.012	1.548	0.029	0.330	0.028	1.257	0.060	0.382	0.018	1.613	0.031
Middle educated	1.134	0.005	1.197	0.011	1.103	0.010	1.238	0.024	1.152	0.007	1.193	0.011
High educated	1.230	0.006	1.064	0.014	1.171	0.014	1.069	0.034	1.286	0.009	1.040	0.015
Income (cat. 2)	1.092	0.007	1.173	0.016	1.079	0.015	1.228	0.036	1.087	0.009	1.175	0.016
Income (cat. 3)	1.204	0.007	1.205	0.016	1.190	0.015	1.225	0.036	1.198	0.009	1.206	0.016
Income (cat. 4)	1.292	0.007	1.134	0.017	1.264	0.016	1.144	0.038	1.279	0.010	1.151	0.017
Income (cat. 5)	1.205	0.008	0.918	0.018	1.177	0.017	0.872	0.043	1.199	0.011	0.945	0.019
Married/Cohabiting	1.201	0.004	0.987	0.009	1.227	0.009	1.023	0.022	1.195	0.006	0.989	0.010
Immigrant	0.651	0.008	0.991	0.017	0.628	0.019	0.979	0.042	0.652	0.012	0.993	0.018
UI history (cat. 2)	1.160	0.005	0.978	0.013	1.186	0.012	0.893	0.030	1.105	0.008	0.983	0.014
UI history (cat. 3)	1.075	0.006	0.943	0.014	1.124	0.013	0.812	0.035	0.999	0.009	0.946	0.015
UI history (cat. 4)	1.032	0.007	0.830	0.016	1.178	0.017	0.684	0.043	0.958	0.010	0.821	0.016
Sickness/disability history	0.558	0.007	1.216	0.016	0.552	0.018	1.258	0.035	0.527	0.011	1.166	0.017
UI eligibility $\geq$ 1 year	0.806	0.006	1.342	0.014	0.826	0.013	1.313	0.032	0.808	0.008	1.330	0.015
UI eligibility $\geq$ 2 year	0.643	0.008	1.465	0.017	0.629	0.018	1.474	0.040	0.656	0.011	1.436	0.018
Unemployed hours	1.248	0.010	1.283	0.024	1.392	0.024	1.513	0.058	1.218	0.015	1.276	0.025
Region 2	0.971	0.007	0.758	0.015	0.941	0.014	0.842	0.032	0.987	0.010	0.769	0.016
Region 3	1.026	0.007	0.617	0.016	0.970	0.014	0.577	0.035	1.012	0.010	0.642	0.016
Region 4	0.976	0.007	0.746	0.017	0.904	0.016	0.561	0.039	1.016	0.011	0.816	0.018
Region 5	1.012	0.007	0.504	0.017	0.944	0.015	0.406	0.039	1.074	0.010	0.529	0.017
Region 6	0.932	0.007	0.517	0.018	0.881	0.015	0.469	0.039	0.962	0.011	0.538	0.018
Duration dependence:												
Months 1-3	1		1		1		1		1		1	
Months 4-6	1.528	0.005	1.460	0.010	1.218	0.013	1.142	0.032	1.663	0.006	1.445	0.010
Months 7-9	1.141	0.006	1.997	0.015	0.842	0.022	0.850	0.146	1.243	0.010	1.899	0.014
Months 10-12	0.902	0.008	2.474	0.025	0.701	0.034			0.941	0.015	2.223	0.022
Months 13-18	0.730	0.009	2.704	0.043	0.359	0.051			0.760	0.021	2.030	0.041
Months 19-24	0.609	0.014	2.646	0.089	0.285	0.072			0.633	0.062		
Months 25-	0.519	0.020			0.212	0.105						
Unobserved heterogeneity												
v_1	0.0031	0.024	0.0000	0.878	0.0007	0.046	0.0001	2.571	0.0030	0.030	0.0000	3.255
v_2	0.0024	0.025	0.0006	0.199	0.0006	0.080	0.0014	0.261	0.0022	0.031	0.0004	0.056
p_1	0.534				0.390				0.616			
p_2	0.466				0.610				0.384			
Observations	582,580				112,678				428,160			

Both equations also contain 8 industry dummies and quarter-of-year fixed effects, which are omitted from this table.