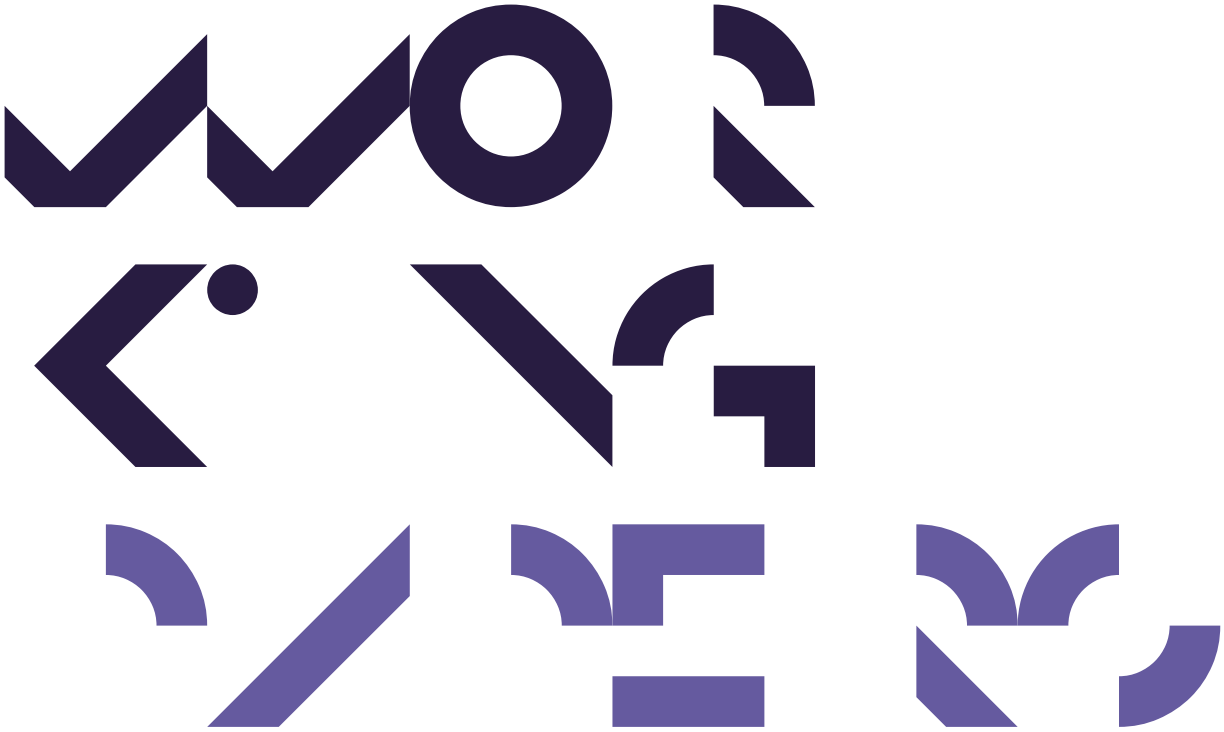

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Active Labor Market Programs and Reservation Wages: Its a Hazard

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Abstract

Using a randomized controlled trial, this paper shows that positive earnings effects of labor market programs might be driven by an employment and/or a wage effect. The findings of this paper suggest that treated individuals in a high-intense scheme are more prone to have lowered short-term reservation wages compared to non-treated and thus accepts lower wages. In a less intense scheme with use of private providers, treated individuals are more likely to have gained formal human capital accumulation, and thereby raised reservation wages, which again might give rise to long-lasting effects.

Keywords: Active labor market programs, randomized controlled trial, hourly wages, mixed proportional hazard models.
JEL codes: C41, J31, J64

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1 Introduction

Often when evaluating the success or failure of an Active Labor Market Program (ALMP) we investigate labor market earnings. This is an obvious objective of output for various reasons, however it also leaves other interesting questions unanswered. E.g. if a program is found to have a positive effect on earnings, then the effect might originate on the intensive or extensive margin. I.e. the program might have shortened unemployment duration enabling the before unemployed worker to spend a couple of weeks more accumulating earnings, and thus a positive earnings effect shows up. Or the wage that the worker receives after participating in a program might have been increased due to him having been given better qualities, thus also summing up to higher earnings. For society as such, earnings might be the up-front main objective for evaluating the program, but if the effects are short lived only, then the question of whether the program was a success or not - and whether it was cost-efficient - hangs upon whether the effects are primarily driven by the intensive or extensive margin.

This paper extends the analysis of Sørensen (2015) in which I show that advancing the timing of entry into, as well as intensifying the use off, ALMP deliver heterogenous effects on earnings. I make use of a randomized controlled trial which was conducted in two Danish counties during the winter of 2005/2006 and lasted for the first 30 weeks of unemployment for each participant. There were two main institutional differences in the RCT setting between the two counties; the intensity of the ALMP was slightly higher in one than in the other, and in the less intense of the two, the public employment services contracted out part of the program to private providers.[1] I show that the earnings effects are short lived for men in the high-intense county and persistent for men in the less intense county. This finding is then attributed to theories of taxing leisure time and accumulating human capital, respectively. Lastly, I found that persistent earnings effects are driven by workers in the high end of the pre-unemployment earnings distribution, while short lived earnings effects refer to workers in the low to mid end of the pre-unemployment earnings distribution. Now, an interesting question arises; why do we observe differences in the persistence of effects between counties participating in roughly the same experiment? In Sørensen (2015) I argue that effects in the high-intense county are driven by a theory of taxing away leisure time while unemployed, i.e. the unemployed workers are adequately annoyed with the program as a whole so they self-select them into the first job they can find, and thus lower the duration of unemployment without gaining longer-term knowledges of how to secure better worker-firm matches afterwards. In the less intense county, I argue that there is actual human capital accumulation going on securing better worker-firm matches thus delivering long-term effects. A way to test this hypothesis is to look at the hourly wages for workers during the first year after entering the experi-

[1] In the high-intense county, unemployed workers had to meet with a caseworker every week for a seven week duration, while in the less intense, the meeting rate were every other week. It was the sequence of meetings with a caseworker, that the less intense county contracted out to private providers.

ment. This paper uses a competing risks framework (Abbring and van den Berg (2003)) in which I simultaneously model the transition out of unemployment into employment and the selection into an hourly wage level allowing for correlation in the transition patterns. Support towards taxing leisure time would be that treatment lowered unemployment duration (positive program and locking-in effects) and lowered reservation wages utilized in lower hourly wages (positive effect on wage hazard). On the contrary, if I find treatment to have less positive out-of-unemployment effects but have increased reservation wages - i.e. higher hourly wages - then the empirics speak in favor of human capital accumulation.

2 Data and Institutional Settings of the RCT

In this section, I give a brief overview of the data and institutional settings; for a thorough description see Sørensen (2015).

The RCT used in this paper was carried out in Southern Jutland and Storstroem county in Denmark with take-up between the first week of November 2005 and the last week of February 2006. All workers entering unemployment during this window without any unemployment spells within the last six months were randomly split between a treatment and a control group based on birthdays. If a worker was born between the 1st and 15th of any given month he was allocated to the treatment group and to the control group otherwise. Employment is defined as entering a paid un-subsidized job. Hourly wages are earnings normalized by the number of hours worked, where hours worked is estimated from payments to a mandatory pension scheme that is contingent on how many hours the worker has been working.

In contrast to the non-treated - who were assigned to the contemporary system - treated individuals were exposed to: (i) a two-week job search assistance program, (ii) frequent meetings with a case worker, and (iii) earlier entry into labor market training. Graversen and van Ours (2008a,b) show that overall, the experiment increased the hazard into self support (the absence of public transfers) and lowered average unemployment duration by approximately two weeks.

3 Methodology

I use a competing risks framework to capture the effects of intensified ALMP on hourly wages while explicitly controlling for the selection from unemployment into employment. For simplicity, I right-sensor transitions out of unemployment into other benefits, self support without labor market earnings, or out of the labor force. I.e. I model the transition from unemployment into employment simultaneously with the *transition* into an hourly wage level. I have chosen to model hourly wages in a hazard framework for two main reasons; (a) effects on the wage hazard can be interpreted as effects on reservation wages, and (b) in a perfect data world, estimating wages as a hazard ensures estimating the complete wage distribution. Regarding (a), think of a situation in which a worker faces

an employer negotiating for wages to form a worker-firm match. Now let the employer present the worker with an offer. If the offer is high enough, then the worker accepts, if not then either the employer quits the negotiations or gives the worker a new higher offer. The wage hazard offers the econometrician the probability that a worker will accept a wage offer given that he has not accepted any offers below the current one. I.e. if the econometrician estimates a negative effect on the wage hazard, it becomes more likely that the worker will refuse more offers before accepting a wage, and thus that the reservation wage of the worker has increased. Estimating wages as a hazard goes back to Donald, Green, and Paarsch (2000). Later, Arni, Lalive, and Ours (2013) used wage hazards to show how sanctions affected post-unemployment wages.

Clearly, since treatment was randomly allocated, the effects of treatment are trivially identified. I thus need only to rely on two assumptions for identification of my model framework (see Abbring and van den Berg (2003)); (i) that there is variation with my observed regressors, and (ii) a finite tail in the distribution of unobserved heterogeneity. I satisfy the first assumption by including individual information of labor market experience, former wages, prior occupational level, educational level, nationality, age, and marriage into the conditioning set. Moreover, I include duration and wage dependence in terms of baseline hazards. The second assumption is standard in the literature using single-spells within a mixed proportional hazard model, as is the case for my version of the competing risks model.

Let t denote time until leaving unemployment and let w be the wage level that is observed. The instantaneous hazards from unemployment to employment and into a wage level are then given by

$$\theta_e(t \mid x_e, d_1, d_2, \nu_e) = \lambda_e(t) \exp(x_e'\beta_e + d_{e,1}\delta_{e,1} + d_{e,2}\delta_{e,2} + \nu_e) \tag{1}$$

$$\theta_w(\omega \mid x_w, d, \nu_w) = \lambda_w(\omega) \exp(x_w'\beta_w + d_w\delta_w + \nu_w), \tag{2}$$

where $\delta_{e,1}$ and $\delta_{e,2}$ capture treatment effects within and after the first 30 weeks of unemployment, respectively. This distinction is included to estimate pseudo locking-in and program effects in the selection from unemployment to employment. In the wage hazard, treatment effects are captured by treatment or no treatment. Following the literature on duration analysis, the duration dependence parameter, $\lambda_e(t)$, and the likewise baseline wage hazards are modelled as step functions

$$\lambda_h(\gamma) = \exp\left[\sum_{k_h} \lambda_{h,k_h} \mathbb{1}(\gamma \in k_h)\right], \quad h \in \{e, w\}, \tag{3}$$

with k_e and k_w specifying the number of baseline hazards in the transition to employment and hourly wages, respectively. The likelihood function thus becomes (suppressing

parameters for simplicity)

$$\mathscr{L} = \prod_{i=1}^{I} \int_{\nu} \theta_e(t)^{c_e} S_e(t) \left[\theta_w(\omega) S_w(\omega)\right]^{c_e} dG(\nu), \tag{4}$$

where $c_e = 1$ if the individual enters employment and zero otherwise. $S_e(t)$ is the time-to-transition specific survivor function for employment, i.e. it measures the fraction present at time t who could potentially transit to employment. Likewise, $S_w(\omega)$ is the fraction of individuals that has transited to employment, but have not accepted a wage level below ω. $G(\nu)$ is the cumulative joint distribution of the unobserved heterogeneity which will be modeled by two mass-points in each transition.

4 Results

In this section, I present results from the competing risks model.

Table 1 shows the estimated effects of treatment on the hazards of transiting to employment and into wage levels.[2] Overall, I find that treatment increased the probability to transit to employment during the trial and after, which is aligned with the findings of Graversen and van Ours (2008a,b) as well as with those of Blasco and Rosholm (2011), who also use this particular experiment to investigate the re-entry rate into unemployment.

It is interesting, however, that I find treated workers to increase their hazard out of unemployment by 25 percent in Storstroem and only 10 percent in Southern Jutland during the trial (the psudo locking-in effect) while the program effect came to 12 percent in Storstroem and 38 percent in Southern Jutland. This points towards support for my hypothesis, that treatment lowered unemployment overall, but in Southern Jutland, treated workers were more prone to go through the entire treatment compared to treated workers in Storstroem. This corresponds with the conjectures from above and from Sørensen (2015), that the program had a fundamentally different impact in Southern Jutland than it had in Storstroem. The results presented in Table 1 indicate that treated individuals in Storstroem systematically selected themselves out of unemployment during the program while the case is that some treated individuals in Southern Jutland left unemployment during the trial but compared to Storstroem treated workers, were more likely to stay unemployed while treated and then afterwards select themselves into employment. Given this structure, it is more likely that treated individuals in Southern Jutland experienced a formal human capital accumulation than treated individuals from Storstroem, simply because one should think that in order to gain the full human capital acquisition you need to stay to complete the program.

Turning to treatment effects on the wage hazard, Table 1 shows that treatment had a statistically significant negative impact on the wage hazard in Southern Jutland while it had a significant positive impact on the corresponding hazard in Storstroem. Remember

[2] Table 2 and 3 in the appendix present all estimation results.

Table 1: Parameters of interest from the estimation of treatment effects on the hazard into employment and into a specific wage level.

	Southern Jutland	Storstroem
Unemployment to employment		
Pseudo locking-in effects ($\delta_{e,1}$)	0.100***	0.229***
	(0.001)	(0.002)
Pseudo program effects ($\delta_{e,2}$)	0.324***	0.119***
	(0.008)	(0.002)
Treatment effects on the wage hazard (δ_w)	-0.032***	0.019***
	(0.002)	(0.001)
Observed heterogeneity	Yes	Yes
Unobserved heterogeneity	Yes	Yes
Log Likelihood	-14,227,150	-20,387,862
Individuals	1,337	1,596

Notes: *** indicates statistical significance at the 1% level. Standard errors are displayed in parentheses. The full set of parameter estimates can be found in Table 2 and 3 in the Appendix.

the definition of a hazard - the probability of *exiting* a state, given that you have not exited before. I.e. a negative effect on a wage hazard implies that you are more likely to achieve a higher wage level. We thus see from Table 1 that treated individuals in Southern Jutland gained higher wages than non-treated, and that treated individuals in Storstroem ended up accepting lower wages than their non-treated counterparts. This finding supports the conjecture that the high-intense scheme lowered reservation wages while the less intense scheme with meetings being handled by private providers raised reservation wages.

Since the model estimates effects into employment simultaneously with effects into a wage level, we can compose a combined effect. The estimates support that, in Southern Jutland, treatment is likely to have been completed, and thus more likely to have had a true human capital accumulation effect. Moreover, since treated individuals in Southern Jutland simultaneously gained higher hourly wages for the jobs that they found compared to non-treated, then we should not expect that the earnings effects found by Sørensen (2015) are due to an employment time effect, but more to a better worker-firm match effect - an effect that Sørensen (2015) found to be persistent. On the other hand, the results on Storstroem effects tell a different story. The treated are more likely to exit during the experiment window, but combined with the effects on the wage hazard, they exit for worse worker-firm matches than the non-treated in Storstroem. The finding of a short-term earnings effect in Storstroem by Sørensen (2015) is consistent with the current finding of faster transfer to employment but at a lower hourly wage level for treated workers than non-treated. Thus, the findings are pointing towards that treated workers in Storstroem lowered their reservation wages possibly due to the high-intense scheme which they ended up being less likely to complete, while Southern Jutland workers increased their reservation wages after completing the ALMP.

5 Conclusions

In some cases, active labor market programs might help unemployed workers in the long-term by removing frictions or building upon human capital or they might solely work in the short-term by taxing away the leisure time of the unemployed worker, giving him an incentive to exit unemployment as fast as possible at a possibly lower wage. The results of this paper show how both seem to be the driving force behind an otherwise comparable experiment that was conducted in two Danish counties. In Storstroem - the county with a relative high-intense scheme - treated individuals left unemployment faster, but at a lower wage level than otherwise, and treated individuals from Southern Jutland - the county with a relative less intense scheme and private providers - benefitted by receiving higher wages after leaving unemployment than non-treated individuals. Together with the findings of Sørensen (2015), the results imply that treated individuals from both counties experienced short-term earnings effects, and that it indeed is likely that treatment in Southern Jutland worked by accumulating human capital and thus increased reservation wages, while it worked by taxing away leisure time in Storstroem with lower reservation wages in the end. The findings of Sørensen (2015) and this paper thus reveal that both taxing leisure time and adding human capital might deliver positive short-term earnings effects.

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A Tables

Table 2: Estimation results, Southern Jutland

Unemployment to employment			Hourly wages		
	Est	S.d.		Est	S.d.
Experience	-0.112	(0.000)	Experience	0.040	(0.000)
Experience squared/100	0.381	(0.001)	Experience squared/100	-0.095	(0.001)
Treatment (dur <= 30 weeks)	0.100	(0.001)	Treatment (dur <= 30 weeks)	-	-
Treatment (dur > 30 weeks)	0.324	(0.008)	Treatment (dur > 30 weeks)	-	-
Treatment	-	-	Treatment	-0.032	(0.002)
Married	-0.090	(0.002)	Married	-0.066	(0.002)
Occupation, top 2005	-0.041	(0.008)	Occupation, top 2005	-0.005	(0.008)
Occupation, middle 2005	0.052	(0.001)	Occupation, middle 2005	-0.014	(0.002)
Occupation, base 2005	-0.107	(0.002)	Occupation, base 2005	0.062	(0.001)
Education, vocational 2006	-0.111	(0.001)	Education, vocational 2006	-0.181	(0.002)
Education, bachelor 2006	-0.550	(0.004)	Education, bachelor 2006	-0.031	(0.004)
Education, master 2006	-0.770	(0.016)	Education, master 2006	-0.488	(0.016)
Western immigrant	-0.055	(0.001)	Western immigrant	0.017	(0.004)
Non-western immigrant	-0.792	(0.004)	Non-western immigrant	0.413	(0.004)
Age 25-29	-0.003	(0.002)	Age 25-29	-0.107	(0.002)
Age 30-39	0.228	(0.002)	Age 30-39	-0.502	(0.004)
Age 40-49	0.280	(0.004)	Age 40-49	-0.176	(0.002)
Age 50+	-0.100	(0.002)	Age 50+	-0.180	(0.002)
Hourly Wage 2005	-	-	Hourly Wage 2005	-0.934	(0.001)
Baseline week hazard 1 - 1	-1.232	(0.002)	Baseline wage hazard 50 - 120	-5.844	(0.004)
Baseline week hazard 2 - 7	-1.940	(0.002)	Baseline wage hazard 121 - 130	-3.982	(0.004)
Baseline week hazard 8 - 11	-1.968	(0.002)	Baseline wage hazard 131 - 150	-2.817	(0.002)
Baseline week hazard 12 - 20	-1.888	(0.002)	Baseline wage hazard 151 - 170	-2.158	(0.004)
Baseline week hazard 21 - 30	-2.139	(0.004)	Baseline wage hazard 171 - 200	-1.696	(0.002)
Baseline week hazard 31 - 52	-2.966	(0.004)	Baseline wage hazard 201 - 250	-1.193	(0.004)
Baseline week hazard 53 +	-3.145	(0.004)	Baseline wage hazard 251 +	-0.729	(0.004)
ν(E1)	-5.377	(0.016)	ν(W1)	-2.609	(0.004)
ν(E2)	0.000	(0.000)	ν(W2)	0.000	(0.000)
Type E1_W1	-7.986	(0.016)			
Type E1_W2	-5.377	(0.016)			
Type E2_W1	-2.609	(0.004)			
Type E2_W2	0.000	(0.000)			
Pr(E1_W1)	0.000				
Pr(E1_W2)	0.004				
Pr(E2_W1)	0.068				
Pr(E2_W2)	0.927				
Log likehood	-14,227,150				
Observations	1,337				

Table 3: Estimation results, Storstroem

Unemployment to employment			Hourly wages		
	Est	S.d.		Est	S.d.
Experience	-0.193	(0.000)	Experience	0.041	(0.000)
Experience squared/100	0.583	(0.000)	Experience squared/100	-0.112	(0.000)
Treatment (dur <= 30 weeks)	0.229	(0.002)	Treatment (dur <= 30 weeks)	-	-
Treatment (dur > 30 weeks)	0.119	(0.002)	Treatment (dur > 30 weeks)	-	-
Treatment	-	-	Treatment	0.019	(0.001)
Married	-0.017	(0.000)	Married	-0.157	(0.001)
Occupation, top 2005	-0.397	(0.008)	Occupation, top 2005	-0.252	(0.002)
Occupation, middle 2005	0.322	(0.002)	Occupation, middle 2005	-0.102	(0.001)
Occupation, base 2005	0.127	(0.002)	Occupation, base 2005	-0.021	(0.002)
Education, vocational 2006	-0.038	(0.001)	Education, vocational 2006	-0.101	(0.001)
Education, bachelor 2006	-0.613	(0.000)	Education, bachelor 2006	-0.432	(0.002)
Education, master 2006	-0.416	(0.008)	Education, master 2006	-0.534	(0.004)
Western immigrant	0.083	(0.001)	Western immigrant	0.193	(0.008)
Non-western immigrant	-1.302	(0.000)	Non-western immigrant	0.304	(0.000)
Age 25-29	0.165	(0.004)	Age 25-29	0.291	(0.002)
Age 30-39	0.417	(0.002)	Age 30-39	0.219	(0.000)
Age 40-49	0.412	(0.001)	Age 40-49	0.287	(0.002)
Age 50+	0.199	(0.001)	Age 50+	0.333	(0.002)
Hourly Wage 2005	-	-	Hourly Wage 2005	-1.302	(0.001)
Baseline week hazard 1 - 3	-1.630	(0.002)	Baseline wage hazard 50 - 120	-5.720	(0.008)
Baseline week hazard 4 - 7	-1.710	(0.002)	Baseline wage hazard 121 - 130	-3.791	(0.001)
Baseline week hazard 8 - 11	-1.723	(0.001)	Baseline wage hazard 131 - 150	-2.839	(0.001)
Baseline week hazard 12 - 20	-1.404	(0.000)	Baseline wage hazard 151 - 170	-1.932	(0.001)
Baseline week hazard 21 - 30	-1.569	(0.002)	Baseline wage hazard 171 - 200	-1.394	(0.002)
Baseline week hazard 31 - 52	-1.268	(0.004)	Baseline wage hazard 201 - 250	-1.091	(0.002)
Baseline week hazard 53 +	-1.612	(0.002)	Baseline wage hazard 251 +	-0.137	(0.004)
ν(E1)	-2.263	(0.004)	ν(W1)	-2.433	(0.004)
ν(E2)	0.000	(0.000)	ν(W2)	0.000	(0.000)
Type E1_W1	-4.696	(0.006)			
Type E1_W2	-2.263	(0.004)			
Type E2_W1	-2.433	(0.004)			
Type E2_W2	0.000	(0.000)			
Pr(E1_W1)	0.008				
Pr(E1_W2)	0.087				
Pr(E2_W1)	0.073				
Pr(E2_W2)	0.833				
Log likehood	-20,387,862				
Observations	1,596				

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