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Swedish Youth Labour Market Policies Revisited*

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Summary: The paper studies the relative effectiveness of various Swedish labour market policies for young workers. Using age discontinuities which define the policy regime each individual is covered by, we present quasi-experimental evidence on the relative efficiency of different policy regimes currently in use. Results suggest that youth policies are more effective than the policies covering other unemployed. The effects mainly appear early in the unemployment spell; we find no evidence of positive long run effects. To uncover which part of the policies that accounts for the positive effect, we use matching techniques to study the relative effectiveness of youth programmes and general adult programmes which are available also for the young. The results indicate that youth programmes are significantly less effective than programmes for adults. Overall, the evidence suggest that youth policies speed up the transition from unemployment to jobs either due to pre-programme deterrence effects or because of more intense job search support from the public employment services before the programmes.

Zusammenfassung: Dieser Beitrag behandelt die Effektivität der Arbeitsmarktpolitik für junge Erwachsene in Schweden. Durch Nutzung von Altersunterschieden beim Eintritt in die verschiedenen Programme lässt sich die relative Wirksamkeit der Maßnahmen als quasinatürliches Experiment bestimmen. Die Ergebnisse zeigen, dass Maßnahmen für junge Erwachsene effektiver sind als Maßnahmen für andere Arbeitslose. Dieser Effekt tritt vor allem bei kurzer Arbeitslosigkeit ein. Um die Wirksamkeit der einzelnen Maßnahmen zu testen, verwenden wir Matching-Verfahren. Dabei werden auch die allgemeinen Arbeitsmarktprogramme einbezogen, die ebenfalls von jungen Erwachsenen wahrgenommen werden. Dabei zeigt sich, dass die Programme selbst für junge Erwachsene weniger effektiv sind als die für andere Erwachsene. Insgesamt zeigt sich, dass Programme für junge Erwachsene den Übergang aus der Arbeitslosigkeit in die Beschäftigung beschleunigen, entweder weil sie die Teilnehmer abschrecken oder weil die Jobsuche im Vorfeld der Programme besser gefördert wird.

1 Introduction

Youth unemployment and late labour market entry are of growing concern in the Swedish policy discussion. However, the large scale evaluations of Swedish youth labour market policies that exist are based on data from the 1990s and leave several issues unexplained.[1] This paper presents some additional evidence on the effectiveness of current youth labour market policies in order to fill some of this knowledge gap.

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1 The only previous large sample studies are Larsson (2003) and Carling and Larsson (2005). See Calmfors et al (2004) for a review of other evaluations of Swedish youth programs and of Swedish labour market policies in general.

Swedish labour market programmes targeted at youth have a history dating back to 1984. Youth programmes were however not used on a larger scale until the start of the economic recession in the early 1990s. Currently, young unemployed are treated differently from other unemployed in both "hard" aspects, such as in the types of available programmes and the timing of the programmes and "soft" aspects such as the priority that case workers should put into job search assistance according to various policy documents.

Whereas labour market programmes generally are administrated by the Public Employment Services (PES), unemployed in the age range 20–24 are sometimes referred to a municipality-provided programme (the youth guarantee, YG), and other times referred to normal PES-administrated programmes. On average, programme placements take place earlier for individuals below 25 than for older unemployed.

Previously estimated effects of programmes for young people vary. Several studies of programmes in the 1980s found positive effects, at least for some programmes under some circumstances and in the long run. But these results were based on small samples and cannot readily be generalised.[2] Larsson (2003) found negative employment and income effects of both labour market training for youth and youth practice in the early 1990s. In addition, she found that labour market training (but not youth practice) had a negative effect on the transition rates to regular education. Comparing youth practice and training, the former outperformed the latter, at least in the short run.[3]

The only study of the municipality provided youth guarantee (Carling and Larsson 2005) pertains to 1998, the first year of the programme. The evidence in that paper indicates that the employment prospects of the participants were not significantly affected by the introduction of the youth guarantee. Due to the shape of the hazard function the authors interpret their estimated zero net-effect as the sum of two opposing forces: a positive pre-programme effect and a negative effect from the time of programme placement.

The overall aim of this paper is to provide new evidence on the effectiveness of different Swedish youth labour market policies in promoting transitions from unemployment to work. Since the Swedish active labour market policies are very general (all unemployed are covered by some policy component) it is very difficult to identify whether the labour market policies as such do speed up the transitions to job. Instead we focus on the *relative* effectiveness of different policy components. To this end, we use data from spells starting in the period 1999–2003 and perform three types of analyses.

First, we study the effects of being covered by different policy packages depending on the age group (20–24 or 25+). The identifying strategy is to compare individuals who are close in age but are covered by different policy regimes. The results from this analysis will provide an overall judgement of the effectiveness of youth labour market policies. This analysis basically updates the results in Carling and Larsson (2005).

However, these estimates are very much "reduced form" in character since the policy package has many different components. One such component is that, as we have already

2 The studies are surveyed in Calmfors et al (2004).

3 This result is in conformity with evaluation results for Swedish programmes for adults in the 1990s.

mentioned, some youth programmes are run by the municipalities.[4] This represents a more general trend towards more of decentralisation in Swedish labour market policies. Hence, in the *second* part of the analysis we analyse to what extent any effects of the policy regimes for youth may be attributed to the programme providers: does decentralisation of labour market policies improve the results? This question has not previously been addressed in connection with Swedish ALMPs.

Young people between 20 and 24 years old are exposed to different programme types. The *third* part of our analysis deals with the effects of training programmes compared to programmes with both training and workplace experience components ("practice programmes"). This analysis brings the analysis in Larsson (2003) more up to date.[5]

In the latter two parts of the analysis we cannot use age discontinuities for identification. Instead, we use matching techniques and identify the effects of the programmes under the identifying assumption that participants in different forms of programmes with the same background characteristics have the same expected outcomes irrespective of programme they actually participate in. To this end, we use an unusually rich register data set which makes the causal interpretation more credible.

Our results, first, show that the policy regime for the 20–24 year olds is effective in shortening the unemployment spells, even though the effects are short-lived, providing a somewhat more positive picture than Carling and Larsson (2005). Second, municipality provided youth programmes are outperformed by labour market programmes provided by the PES. Finally, programmes providing practice seem to outperform training programmes for young people for some outcomes but not for others. Hence, these results are less clear-cut than the results in Larsson (2003).

Since the positive effects of the youth policies materialize before the programmes are typically set in, a tentative overall conclusion is that the positive effects from youth policies are driven by higher quality search-assistance and/or pre-programme deterrence effects from early expected programme entry rather than from positive treatment effects for those actually participating in the programmes. This is supported by the fact that the youth programmes are outperformed by the programmes available for older unemployed when studying the effects on participants.

How do these results for Swedish youth labour market policies square with evidence from other countries? White and Knight (2003) surveyed the literature on the effects of labour market programmes for young people.[6] Wage subsidy programmes generally seemed to increase the job-finding rate, whereas studies of job creation programmes in the public sector pointed to negative effects of such programmes. The results for vocational training programmes were mixed, whereas job search assistance programmes generally produced positive results. This pattern of programme effects is consistent with the general pattern of

4 A complication related to the municipality programmes is that there is a serious lack of information about programme content. The scattered available evidence indicates that programme content varies quite a lot between municipalities.

5 Unlike Larsson (2003) we do not, however, compare participation in these programmes to non-participation.

6 See also the review in Heckman, LaLonde and Smith (1999) and the discussion in Heckman and Krueger (2003).

programme effects found for adults, both in Sweden and internationally (Calmfors, Forslund and Hemström 2004, Martin and Grubb 2001). Blundell et al. (2004) studied the New Deal for Young People in the UK. This programme has many components, two of which are mandatory job search assistance and wage subsidies. The authors found that both job search assistance and wage subsidies contributed to an increase in the job-finding probability, but that the treatment effect was much larger in the short run than in the long run. In applicable parts, these findings are in line with our results.

The rest of the paper is structured as follows. In section 2 we provide a background by discussing Swedish labour market policies targeted at young people. In Section 3 we discuss our identification strategies. Section 4 gives a data description. The results are presented in Section 5, dealing with the effects of different policy regimes, and Section 6, treating the relative effects of different programmes. Section 7 concludes the paper.

2 Swedish ALMPs Targeted at Youth

Until the early 1980s there were no programmes targeted at youth – young persons could participate in the same programmes as adults, i.e., training programmes and relief work (temporary public sector jobs). The first Swedish labour market programme explicitly targeted at youth was *youth teams* introduced in 1984. In 1987 and 1989 this programme was followed by two different versions of a programme called *schooling-in slots*. During 1992 *youth practice* was introduced. This programme, in contrast to its predecessors, rapidly reached large volumes. The programme was targeted at youth below the age of 25.

In the 1994 electoral campaign, the incumbent minister of labour promised that no young person (below age 25) should be left unemployed for more than 100 days. Instead, all young unemployed should be placed in programmes after 100 days of unemployment. The general election resulted in a new government, but the objective to refer young people to labour market programmes at an early stage of the unemployment spell has been present in one way or the other since the mid 1990s.

Since the early 1990s, Swedish municipalities are responsible for offering secondary education to all youth below age 20. In addition, the municipalities have an obligation to keep themselves informed about the employment status of all youth between 16 and 20 years of age. Since October 1995, Swedish municipalities have also had the opportunity to assume responsibility for the employment situation of youth between 18 and 19 years. This municipality provided programme, called *municipality youth programme* (MYP), provides education or practice to facilitate a transition to work or to stimulate participation in regular education. Although referral to the MYP is conditional on the signing of a contract between the municipalities and the PES almost all unemployed teenagers have been covered by such a contract and the MYP "market share" among programmes for teenagers has been nearly 100 percent since.[7]

7 According to Sibbmark and Forslund (2005), 265 municipalities (out of 281) had signed an agreement with the regional labour market authority in 2004 and the ones that had not signed appear to have had a very small number of unemployed teenagers.

Although on the agenda since 1994, the target that unemployed youths (below 25) should be placed in programmes within 100 days was not met by 1998. In 1998 there was a reform opening for the possibility of municipality provided youth programmes also for unemployed in the age range 20 to 24. A condition was that the municipality and the regional labour market authority signed an agreement to this effect – something which happened in some, but not all, of the municipalities. The contract meant that the PES could refer unemployed to the municipality in a programme called the *youth guarantee*[8] (YG). The YG amounts to an obligation to offer the target group a full-time activity after 100 days of unemployment.[9] The duration of the programme is capped at 12 months. In 2004, 205 out of 281 municipalities had signed an YG-agreement with the regional labour market authority.

In contrast to the MYP for teenagers, the YG does not have a 100% market share of programmes for 20–24 year olds, during the period 1999 to 2003 the market share was around one third. This happens for two reasons. The first is that a few (mainly small) municipalities have chosen not to sign the contracts, and the second reason, which is quantitatively more important, is that the PES also refers 20–24 year olds to programmes not explicitly targeted at youths. The two major programmes in this category are labour market training and practice programmes.

From this description it should be clear that Swedish labour market policies for relatively young workers have three different "regimes", one for teenagers (18–19 year olds), one for young adults (20–24) and one general regime (25+).

It is important to note that the presence of the municipality provided youth programmes is not the only difference in treatment between the different age groups. For instance, even in the municipalities that do not provide the YG-programme for young adults, we see much earlier programme placements for 24-year olds than for 25-year olds. We attribute this to a policy target that pertain to all PES activity, namely that young adults should be placed in programmes early.

A second important note is that youth unemployment is measured and targeted separately, suggesting that the PES-offices may put specific effort into job broking activities for this group. An indicator that this may be true is that some PES-offices have specific caseworkers for young unemployed (see e. g. Lundin 2004).

Thus, in our analysis we will try to first evaluate two of the three available policy regimes against each other.[10] The second part of the analysis compares actual participants in municipality provided youth programmes to participants in general PES-provided labour market programmes. It should be noted that treatment by the municipality is somewhat of a black box from an evaluator's point of view, the municipalities are free to define the programme as they choose and there is no systematic follow up of the actual implementation.

8 Previously, the *development guarantee*.

9 The unemployment clock starts ticking from the day the unemployed youth registers as a job seeker at the public employment service.

10 We do not evaluate the policy regime for the teenagers. The main reason is that the difference in labour market performance between persons aged 19 and 20 is likely to be much larger than the difference between persons aged 24 and 25. Hence, a comparison between 19- and 20-year olds may reveal not only differences due to policy regimes but also due to "pure" age effects.

Our final part of the analysis therefore compares participants in training programmes to participants in practice programmes in order to get some insights into which form of treatment works better for young unemployed workers.

3 Identification Strategies

The two parts of our analysis use different types of identifying assumptions. In the first part we use age discontinuities to identify the effects of being part of one policy package relative to another. In the second part, which compares programme types and providers, we have to rely on comparisons between observably equivalent individuals in order to identify the effects. This analysis thus rests on stronger identifying assumptions than the previous part. However, we believe that by conditioning on participation in some form of programme (rather than comparing to further open unemployment) the results are less susceptible to alternative explanations.

3.1 Identification of Regime Effects

In general we have three age based policy regimes, all of which are defined by the age of the unemployed. The first regime covers teenagers (age 18–19), the second covers young adults (20–24), and the third covers adult unemployed (25+). In this paper we only study the two latter.

Each regime may supply a number of characteristics, some which we may measure and some which we cannot measure. To summarize ideas, we may think of a regime as being defined by:

- the amount and quality of job search assistance provided before programmes
- the timing of programmes
- the selection of *who* goes into a programme
- the length of programmes
- the content of programmes
- the economic compensation to programme participants

Note that since all of these items may be known in advance by the unemployed, it is possible that they affect the behaviour of the individual already *before* they take effect. Therefore we choose to study workers covered by the different regimes already from the start of their unemployment spells.

Which of these regimes an unemployed is subject to is defined by his or her age. We define age according to the age in years 90 days after registration. The 90 days are used since programme placements are according to age at each point in time (not according to age at registration) and various policy documents state that young workers should be placed in

programmes after around 90 to 120 days. We therefore assume that individuals and case workers from the date of registration onwards act according to the age the individual will have reached 90 days after registration.

To further strengthen the identification, we include a linear function of age in days. This is to exclude the possibility that we are capturing age effects and attributing them as effects of policies. With such a control variable, we are only attributing systematic deviations from a linear relationship between date of births and outcomes as the effect of the reform. As we show, including further covariates does not matter substantially for our analysis.

Apart from estimating OLS regressions, we also estimate Cox proportional hazard models, which assume that the log of the hazard out of unemployment is a linear function of the covariates, whereas the baseline hazard may vary freely with time under analysis.

3.2 Identification of the Relative Effects of Different Programmes for Participants

We do not use the age discontinuities when studying the relative efficiency of different programmes on the actual participants. Even though individuals over and below an age threshold are offered different mixes of programmes (if they participate at all), it may be misleading to make comparisons between the two groups if the selection processes into programmes are fundamentally different between the groups. Specifically, it is clear that the time to programmes will have very different meaning for people covered by different regimes. A further indication that this is a real problem comes from the fact that our results suggest significant pre-treatment effects for young workers.

Instead we use matching techniques and compare workers aged 20 to 24 with similar (observed) labour market histories but who participate in different forms of programmes. It is of course possible that the selection processes into the programmes differ between programmes, so that participants in some programmes are fundamentally different from participants in other programmes. To alleviate this identification problem, we adjust the comparisons with respect to a rich set of covariates. Our identifying assumption is that, conditional on covariates, there are no differences in expected outcomes between participants in the municipality provided youth programmes and the programmes which also are available for the adult unemployed (and the same for participants in training programmes and practice programmes).

In general, we implement the identification strategy by means of propensity score matching.[11]

4 Data and Description

We use data from various population-wide registers collected in the IFAU-database. Our main data source which also defines our samples is the data base HÄNDEL which comprise various sets of information on all individuals registered at the PES including the timing of registration, programme placements and reasons for leaving the registers. From this

11 The standard assumptions underlying this estimator are well known, and will not be discussed here.

data base we get data on spells of registered unemployment, programmes and various background characteristics. These data are available from 1990 to November 2005.

To complement the PES data, we add information about employment, earnings, schooling, welfare receipts and family situation from other registers which originally were collected by Statistics Sweden. These data are both used to construct control variables capturing family situation, immigration status, previous earnings, number of employers and welfare receipts and to construct similar outcome variables. Since the data coming from Statistics Sweden only are available up to 2003, the number of observations drops somewhat when studying these outcomes.

When studying the differences in regimes we follow individuals from the date of registration and look at various outcomes relative to this date.[12] When studying the effects of programmes, we construct the data similarly but measure outcomes relative to the date of programme placement. Programme effects are studied for all youths aged between 20 and 24 starting a programme between 1999 and 2003, whereas regime effects are identified from the differences between 24 and 25 years old individuals registering between 1999 and 2003.

Both the timing of programmes and the type of programme suggest that the discontinuity bites on the age (in years) the individual will have 90 days after registration as unemployed and thus we use this age when studying regimes. This is consistent both with intentions stated in various policy documents that young workers should not be unemployed without programme placement more than 100 days and with the regulations for the YG-programme stating that referral to programmes for workers below 25 should take place between the 90th and the 120th day of an unemployment spell.

In Figure 1 we show observable differences in how the unemployed are treated depending on age by showing differences between survival curves where the exit is programme start. We include all spells starting in 1999–2003 for young persons in ages between 22 and 27 years, with age being defined by the age in years 90 days after registration. In all cases we censor spells that end before the start of a programme. The similarity in survival within cohorts in ages 25–27 and 22–24, respectively, and the difference between these two groups are striking. This evidence forcefully suggests that we are justified in treating young people in those ages as covered by two different policy regimes. The survival curves further suggest that those in ages 22–24 years enter programmes significantly earlier than those between 25 and 27 years. Still after 600 days there is a marked difference between the younger group and the older, but the most striking difference occurs in the neighbourhood of 100 days after the beginning of the unemployment spell.

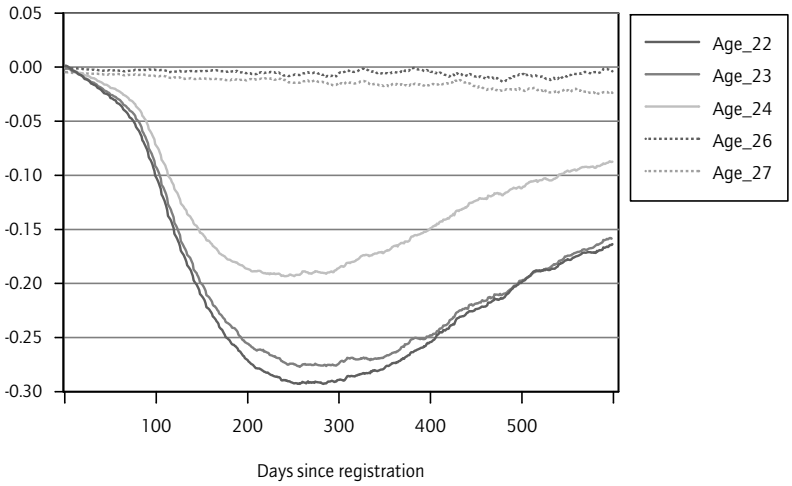
5 Results – Effects of Policy Regimes

In this section we estimate the effects of the youth regime on various labour market related outcome variables. This will give an estimate of the overall effects of the youth regime.

12 Statistics Sweden's data are annual and are measured relative to the calendar year of registration.

Figure 1

Difference to 25 Year Olds in Survival Outside of Program for Different Age Groups



Note: Exits from registration are treated as censored. Estimates are corrected for month of registration.

A first look at the programme effects can be provided by simply looking at the outflow to work from the unemployment registers, depending on age. Figure 2 shows differences in survival in the PES register where the exit is exit to work. The survival curves show that adults remain jobless to a much larger extent than the 22–24 year-olds. Furthermore, this difference occurs very early in the register spells – the main part of the difference arises within the first 100 days, i.e., before (expected) programme start.[13] The most natural interpretation of this finding is that it reflects either an effect of intensified job search assistance early in the unemployment spells of young people or a pre-programme deterrence effect.[14] This interpretation as well as the main thrust of the results presented below is well in accordance with the results in Carling and Larsson (2005).

In addition to the "suggestive" evidence in Figure 2, we present a number of different regressions for effects on a number of possible outcomes. First, we use two measures derived from the PES registers: the probability of being in the PES registers at different dates, hazards to employment and to any destination outside the PES registers.[15] These are outcomes considered in previous studies, which often have only had access to data from

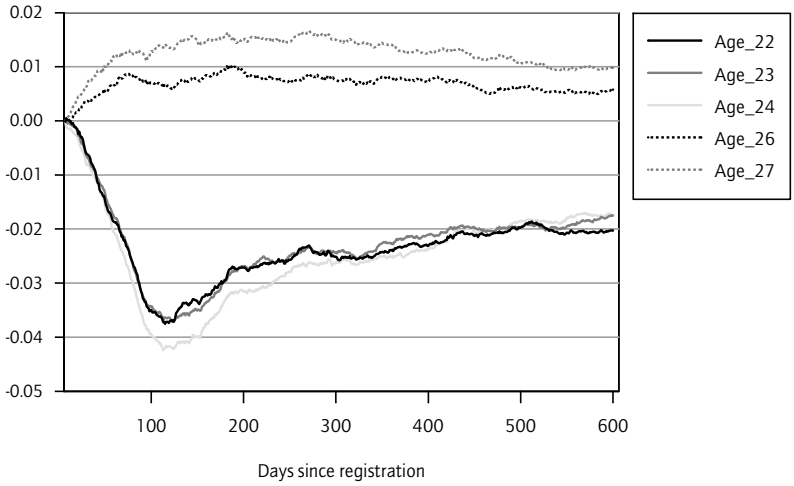
13 The institutional framework implies that programme starts should occur after around 100 days. See the description of the youth policy regime in Section 2. See also the description of programme entrance in Figure 1.

14 See Hägglund (2006) for a discussion of pre-programme effects as well as estimates of pre-programme effects based on randomised experiments.

15 The distinction between being registered at any point in time and the hazard out of the register is potentially important for young people, who may very well experience multiple short spells of employment and non-employment.

Figure 2

Difference to 25 Year Olds in Survival Outside of Job for Different Age Groups



Note: Exits to other destinations than jobs are treated as censored. Estimates are corrected for month of registration.

the National Labour Market Board. Second, we use information from registers collected by Statistics Sweden to look at future earnings, employment and social assistance take-up. These are outcomes that have rarely been considered in previous evaluations of Swedish youth labour market programmes.

Results are presented in Table 1. Note that the point of estimating the effects on various non-duration outcomes is that they study more long run effects that are measured outside of the initial unemployment spell. This is important to the extent that young persons are prone to have many short spells of unemployment. We also present Cox proportional hazard regressions which are stratified on month of inflow. All regressions are presented in two versions – one with and one without a large number of controls. Including covariates should increase efficiency but can also be viewed as a test of whether there are remaining selection issues despite the discontinuity set-up.

The regression results first confirm the message of Figure 2: the young adults leave the register faster than the adults in the beginning but not later in the spells. This leads to a reduction by just above five days in the register during the first year after the beginning of the unemployment spell. However, looking at outcomes dated one year or more after the beginning of spells, nothing is significantly different from zero. The finding that there is no significant difference between the groups in the probability of being in the register after around one year suggests that young persons experience more of repeated unemployment spells than the adults (compare to Figure 2 which displays differences in survival).

Table 1

Effects of Being 24 rather than 25 Years old 90 Days after Registration

	Registration (PES) (# days from first registration)						
	45d	90d	183d	365d	1095d	Days in one year	Inc. (t+2)
No controls	–0.019**	–0.035**	–0.013**	–0.004	0.001	–5.003**	–0.013
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.823)	(0.014)
With Controls	–0.023**	–0.042**	–0.020**	–0.006	–0.000	–6.822**	–0.009
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.725)	(0.013)
N	251,738	251,738	251,738	251,738	198,712	251,634	132,140
	Employment	**Employment**	**Social assistance**	**Hazard to job**		**All hazards**	
	(t+1)	(t+2)	(t+2)	<120d	>120d	<120d	>120d
No controls	–0.002	–0.008	–0.002	1.115**	1.008	1.117**	0.966**
	(0.004)	(0.005)	(0.003)	(0.012)	(0.013)	(0.009)	(0.009)
With Controls	–0.000	–0.007	–0.003	1.135**	1.090**	1.139**	1.053**
	(0.004)	(0.004)	(0.003)	(0.013)	(0.015)	(0.010)	(0.011)
N	201,939	156,094	156,094	334,195	334,195		

Note: Regressions are OLS, linear probability models or Cox Proportional Hazard models. All regressions controls for age in days at registration. Other covariates capture education, family situation, labour market history and demographic characteristics. The variables are the same as in the matching described in Appendix A except for time to program which is not relevant in this application. All variables are measured at the start of the registration spell. Income is log annual income. Employment is a dummy measured in November by Statistics Sweden. Social assistance is a dummy variable for positive benefits during the calendar year. Standard errors are corrected for individual clusters, accounting for repeated observations by the same individual.

** Significant at the 5% (1%) level.

Once again, we think that the evidence either suggests effects of intensified job search assistance or pre-programme deterrence effects. Given the insignificant long-run effects, a natural conjecture is that programme participation in the YG programme is less effective than participation in the PES programmes. This is the subject matter of the next section.

6 Results – The Relative Effects of Programmes for Young People

In this section we study the effectiveness of the youth programmes relative to other programmes. First, we compare the municipality provided Youth Guarantee programme to PES-provided alternatives. There are no previous direct studies of this issue although the timing of the hazards presented in Carling and Larsson (2005) as well as the results presented above suggest that the effects of the youth regime is less pronounced after the point at which programme placements typically occurs. Second, we compare training programmes to practice programmes. The results in Larsson (2003) from such a comparison indicated better effects of the practice programme, especially in the short run. However,

her results pertain to the early 1990s, and both programmes and the economic environment have changed substantially since then.

Since selection into different forms of programmes may well be non-random we use a large number of available covariates to adjust for any observed differences between different programme participants. This is done using both regression and propensity score matching. We estimate the effects on essentially the same outcomes as in the previous section, only measured from the start of the programme in this case.

Table A1 in the appendix provides a description of participants in the municipality provided YG programme and participants in the PES administrated labour market training (LMT)[16] and work practice (WP) programmes which are by far the two most common alternatives. By estimating a probit regression including these covariates as well as dummies for month of registration and county dummies we predict the individuals' propensity to participate in YG and create a matched sample drawn from the PES programme participants by matching on the "nearest neighbour". Note that this means that estimated effects from the matched comparisons are representative for the individuals actually participating in the YG. The third column shows the difference between the actual and matched samples along with standard errors.

Table 3 shows estimated programme effects based on OLS and Cox proportional hazard regressions as well as results based on propensity score matching where both regressions and matching use all the covariates described in Table A1.

The results suggest that the municipality provided youth guarantee programmes (YG) are outperformed by the combination of labour market training and work practice provided by the PES: the YG participants experience more PES registration, lower hazards to jobs and to all exits, lower future income and employment and higher take-up of social assistance. The estimates are remarkably similar between the matching approach and the parametric Cox and OLS regressions. Since the matched estimates only are representative for those participating in YG while the regression estimates assumes a common treatment parameter the results thus suggest that the effects do not vary substantially between participants YG and PES-programmes. This notion is further supported by the fact that matching on characteristics representative of the PES-programme participants instead of the YG-participants gives near identical results (available upon request).

The discrepancy between "all exits" and job exits estimates in Table 2 reflects more frequent exits to regular education among YG participants. It should be noted, however, that it is unlikely that future participation in education is driving the negative long run results – even though a higher rate of participation in regular education may reduce employment for mechanical reasons it should not increase unemployment.

The selection into YG rather than the PES programmes can take place in two steps: first the municipality signs an agreement with the PES, second the individual is referred to either YG or some PES-programme. To see whether this two-step selection affects our esti-

16 We code all programs recorded as either "labour market training" or "preparatory training" in the HÄNDEL database as being labour market training.

Table 2

Estimates of Relative Programme Effects – Municipality (YG) vs. PES

	Registration (# days from programme starts)				Hazards	
	183	365	730	1095	To job	All
Regressions						
YG	0.009**	–0.003	0.015**	0.010**	0.872**	0.975**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.008)	(0.007)
N	89,986	89,986	88,329	73,430	89,986	89,986
Matching						
YG	–0.001	–0.010*	0.009*	0.011**	0.894**	0.996
	(0.005)	(0.004)	(0.004)	(0.004)	(0.012)	(0.010)
Constant	0.431**	0.243**	0.148**	0.121**		
	(0.005)	(0.004)	(0.003)	(0.003)		
N	81,470	8,470	79,604	64,076	81,470	81,470

	Non-PES administered outcomes			
	Income (t+2)	Employment (t+1)	Employment (t+2)	Social assistance (t+2)
Regressions				
YG	–0.173**	–0.070**	–0.071**	0.032**
	(0.014)	(0.004)	(0.005)	(0.003)
N	44,026	74,996	53,716	53,716
Matching				
YG	–0.142**	–0.061**	–0.068**	0.031**
	(0.021)	(0.006)	(0.007)	(0.006)
Constant	6.528**	0.510**	0.575**	0.217**
	(0.018)	(0.005)	(0.006)	(0.005)
N	37.332	65,652	46,314	46,314

Note: Regressions are OLS, linear probability models or Cox Proportional Hazard models. Matching is by nearest neighbour, representative for YG. Various covariates capturing education. family situation, labour market history and demographic characteristics are included. See Apendix A for a description. The same variables are included in the regressions as in the matching model. Income is log annual labour income. Employment dummy is measured in November by Statistics Sweden. Social assistance is a dummy variable. Standard errors are corrected for individual clusters.

***** ,****** Significant at the 5% (1%) level.

mates we have estimated the same model including only municipalities with a signed agreement and the results are very similar: even within these municipalities YG-participants have less transitions to jobs, more frequent transitions to education and worse long run labour market performance than a matched sample of PES-programme participants.

Table 3

Estimates of Relative Programme Effects – Training vs. Practice

	Registration (# days from programme starts)				Hazards	
	183	365	730	1095	To job	All
Regressions						
Training	0.072**	0.041**	–0.002	–0.000	0.855**	0.843**
	(0.005)	(0.004)	(0.003)	(0.003)	(0.010)	(0.008)
N	49,229	49,229	48,529	41,533	49,229	49,229
Matching						
Training	0.059**	0.045**	0.004	0.003	0.881**	0.877**
	(0.007)	(0.006)	(0.005)	(0.005)	(0.016)	(0.013)
Constant	0.402**	0.214**	0.142**	0.120**		
	(0.006)	(0.005)	(0.004)	(0.005)		
N	57,286	57,286	56,545	47,549	57,286	57,286

	Non-PES administered outcomes			
	Income (t+2)	Employment (t+1)	Employment (t+2)	Social assistance (t+2)
Regressions				
Training	0.073**	–0.003	0.014*	0.010*
	(0.016)	(0.005)	(0.006)	(0.004)
N	25,770	42,285	30,669	30,669
Matching				
Training	0.057*	–0.009	0.020*	0.012
	(0.024)	(0.008)	(0.009)	(0.007)
Constant	6.646**	0.564**	0.603**	0.167**
	(0.021)	(0.007)	(0.008)	(0.006)
N	29,002	48,624	34,550	34,550

Note: Regressions are OLS, linear probability models or Cox Proportional Hazard models. Matching is by nearest neighbour, representative for the labour market training. Various covariates capturing education. family situation, labour market history and demographic characteristics are included. See Apendix A for a description. The same variables are included in the regressions as in the matching model. Income is log annual labour income. Employment dummy is measured in November by Statistics Sweden. Social assistance is a dummy variable. Standard errors are corrected for individual clusters.

***** ,****** Significant at the 5% (1%) level.

In Table 3 we show the results when we compare training programmes to practice programmes. Descriptions of the samples are found in columns four to six in Table A1 in the appendix. In terms of PES registration and hazards to jobs and all exits training does worse than practice. The effect on registration is, however, only significant over a one-year horizon. As in the previous analysis, the results do not vary with the choice of method

(matching or regression), nor do they depend on whether the matching is representative for the WP or LMT-participants.

When instead considering income, employment and social assistance take-up, a somewhat different picture emerges. Training now outperforms practice in terms of the effects on both employment and income; these effects are significantly positive the second year after programme entry. The matching estimate of the effect on social assistance take-up is insignificant. This pattern in combination with the results on PES registration and hazards would be consistent with training giving rise to human capital accumulation (both directly and indirectly through exits to regular education) giving positive treatment effects through higher productivity. This process seems to take some time, which, e.g., could be explained by the treatment (and the higher productivity associated therewith) resulting in an increased reservation wage.

7 Conclusions

The paper studies the relative effectiveness of various components of the Swedish labour market policies directed at young workers. Using age discontinuities which define which policy regime an individual is covered by we are able to present quasi-experimental evidence on the relative effectiveness of the policy regimes currently in use. Our results show that youth policies are more effective than the adult oriented policies in shortening the unemployment spells of young workers. The effects mainly appear early in the unemployment spell and are short lived; we find no evidence of positive long run effects. Further results suggest that youth *programmes* run by the municipalities are outperformed by general labour market programmes when studying the effects on the actual participants.

A tentative conclusion is that the positive effects from youth policies are driven by higher quality search-assistance and/or pre-programme deterrence effects from early expected programme entry. Regardless of which interpretation one should trust, the results highlight that intensified job search (either through assistance or incentives), and not just "active" programmes may be relevant instruments in facilitating transitions to jobs for young workers. Furthermore, the results suggest that decentralizing the implementation to the local authorities from the national Public Employment Service reduced programme effectiveness in promoting transitions to jobs.

Since we know little of what treatment the municipalities provide, we also study differences in effectiveness of different PES-administrated programmes and find, in line with previous studies, that training programmes have worse short run performance than practice programmes. However, the long run results show signs of opposite effects. This result is interesting since it departs from the general notion that training programmes are less effective than practice programmes in Sweden. The results are consistent with training programmes providing higher productivity and therefore higher reservation wages. However, given that this paper is the first to find this kind of long run effects, and that the results differ somewhat between outcome variables, the results call for further research on the long run effects of labour market training in Sweden in general before further conclusions can be drawn.

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Appendix

Description of Programme Participants

Table A1

Comparisons of Actual and Matched Programme Participants Municipality vs. PES and Labour Market Training vs. Work Practice

	Municipality vs. PES			LMT vs. WP		
	Municipality	PES	Matched	LMT	WP	Matched
Year	2001.1	2000.9	0.021	2000.9	2000.8	−0.001
			(0.014)			(0.019)
Age	22.07	22.29	0.017	22.33	22.24	−0.006
			(0.015)			(0.021)
Male	0.587	0.632	0.006	0.662	0.590	−0.001
			(0.005)			(0.007)
Immigration status						
Non-Nordic immigrant	0.171	0.131	−0.002	0.141	0.117	0.006
			(0.004)			(0.005)
Nordic Immigrant.	0.006	0.007	0.000	0.007	0.007	−0.001
			(0.001)			(0.001)
Swedish w. Non-Nordic parent	0.082	0.061	−0.004	0.068	0.051	−0.002
			(0.003)			(0.004)
Swedish with Nordic parent	0.082	0.076	−0.003	0.079	0.072	−0.002
			(0.003)			(0.004)
Education (Ref. Vocational high school)						
University (at lest 3 years)	0.011	0.012	0.001	0.010	0.014	−0.002
			(0.001)			(0.001)
Some post HS ed.	0.045	0.046	0.003	0.041	0.053	−0.003
			(0.002)			(0.003)
Academic High school	0.138	0.146	0.005	0.146	0.147	−0.005
			(0.003)			(0.005)
Not completed HS	0.404	0.352	−0.008	0.370	0.326	0.005
			(0.005)			(0.007)
Missing	0.013	0.015	−0.000	0.013	0.018	0.001
			(0.001)			(0.001)
Family (ref. no children)						
Living w parents	0.388	0.356	0.005	0.358	0.353	0.008
			(0.005)			(0.007)
Single parent	0.038	0.029	0.000	0.027	0.031	0.001
			(0.002)			(0.002)
Couple with children	0.081	0.091	−0.003	0.089	0.094	0.003
			(0.003)			(0.004)
Social assistance previous year	0.342	0.290	−0.014**	0.298	0.278	0.008
			(0.005)			(0.007)
Social assistance two years before	0.346	0.305	−0.012*	0.315	0.291	0.010
			(0.005)			(0.007)

	Municipality vs. PES			LMT vs. WP		
	Municipality	PES	Matched	LMT	WP	Matched
Labour market history						
First period	0.177	0.153	0.006	0.143	0.166	0.003
			(0.004)			(0.005)
First programme	0.504	0.458	0.008	0.459	0.458	–0.007
			(0.005)			(0.007)
Days since last period	398.1	398.0	11.006**	390.5	408.3	1.691
			(4.090)			(5.492)
Number of days in prev.	315.7	350.9	–3.955	351.8	349.5	2.904
			(3.415)			(5.046)
Income previous year	429.8	550.3	14.094*	591.0	493.1	–13.540
			(5.548)			(10.412)
Income two years before	288.5	380.1	12.374**	400.7	351.3	–10.116
			(4.418)			(8.716)
# Employers previous year	1.059	1.193	0.018*	1.222	1.153	–0.006
			(0.009)			(0.013)
# Employers two years before	0.911	1.044	0.005	1.063	1.017	–0.012
			(0.009)			(0.013)
Search in extended geographic area	0.206	0.253	–0.001	0.247	0.262	–0.009
			(0.004)			(0.006)
Disabled	0.041	0.070	–0.005*	0.066	0.076	–0.000
			(0.002)			(0.003)
Compensation (ref Unemployment Insurance)						
Basic amount	0.378	0.293	–0.013*	0.292	0.294	0.007
			(0.005)			(0.006)
None	0.256	0.232	0.003	0.225	0.243	0.008
			(0.005)			(0.006)
Days in spell before program	95.83	77.22	–0.381	78.24	75.78	1.074
			(0.630)			(0.843)
Propensity	0.540	0.381	0.000	0.612	0.541	0.000
			(0.002)			(0.002)
Periodes	40,735	49,229	81,470	28,643	20,578	57,286
Individuals	37,056	44,991	54,791	26,982	19,520	36,923

Note: Data for programme spells starting between 1999 and 2003. Individuals are aged between 20 and 24 at the time of programme entry. Only Labour Market Training and Work Practice spells are included in PES programs. Averages are for the full population. Differences are after nearest neighbour matching on probit-estimated propensity score. The probit included all listed covariates as well as 60 dummies for month of programme start and 24 county dummies. "Age" was interacted with "Year". "Previous earnings" were included with squares. "Previous employers" were included as dummies (0,1, 2, at least 3). "Days in spell before program" were included as dummies (one for each 30 day period, one for 300–400 days). Standard errors are corrected for individual clusters.

***** ,****** Significant at the 5% (1%) level.