

Does early intervention help the unemployed youth?

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Abstract

This paper evaluates a measure targeted at unemployed individuals aged 20–24. The main purpose of the measure is to prevent long-term unemployment by guaranteeing an assignment to some labor market program within 100 days of unemployment. Municipalities voluntarily agree to offer the guarantee. To identify the effect of the measure, we use three conditions: The guarantee covers individuals aged 24 but not 25, one fifth of the municipalities does not provide the guarantee, and the guarantee existed in 1998 but not in 1997. We find no evidence that the measure did significantly improve the future labor market situation of the youth, which suggests that early intervention in the unemployment spell is not important.

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1. Introduction

Acting on a pre-election promise, the new Swedish government declared after the election in 1994 that no youth should stay openly (and passively) unemployed, i.e. not participating in any labor market program while being unemployed, for more than 100 days. At the time of the declaration, unemployment, including youth unemployment, reached its post-war highest level.

The declaration swiftly came into operation by the government convincing the municipalities to offer labor market programs to the youth. And a municipal program

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was set up to enforce the promise, for the unemployed youth of age 20 up to 24 years.¹ This was an untraditional choice as such programs are usually run by the State (through the National Labor Market Board, AMS). But, at the time, the employment offices were under considerable pressure due to the exceptionally high unemployment rate, and putting some of the responsibility for the youth on the municipalities served as a means of diminishing the pressure on the offices.

Our goal in this paper is to determine the *effect of a guarantee for early program participation* on the subsequent labor market attachment. In the following we will use interchangeably UVG (Utvecklingsgarantin, its Swedish name), early intervention, and guarantee to denote the guarantee for early program participation². By guaranteeing the assignment to a program within 100 days, long-term open unemployment is avoided. It has been argued elsewhere that long-term open unemployment might be devastating for future labor market prospects. On the other hand, such a guarantee might provide an attractive alternative to regular employment, and thereby extend the time the youth stay detached from working life.

Furthermore, the guarantee may also induce an increased job-finding rate, if considered more as a threat than a blessing. Black et al. (2002) provide evidence for such a pattern as they evaluate the WPRS system in the US.³ The program implies a ‘guarantee’ for mandatory employment and training services to individuals with long expected unemployment spells, and they find a sharp increase in the exit from unemployment prior to the start of services.

To identify the causal effect of early intervention, we make use of three conditions: first, it covers individuals aged 24 but not 25. Second, the municipalities volunteered for offering the guarantee, and not all of them chose to do so. Thus, an alternative identification strategy is to compare the volunteering with the non-volunteering municipalities. Third, the data are repeated cross sections, so that we can also compare the behavior of the age group before and after the introduction of the guarantee, that is in 1997 and 1998.

The remainder of this paper is organized as follows: in Section 2, we describe the institutional setting and Section 3 discusses the treatment and a theoretic framework for our empirical analysis. Section 4 discusses the identification strategy. In Section 5, we show the main empirical results, Section 6 presents some alternative ways of estimating the treatment effect, and the final section concludes.

2. The institutional setting and data

In 1994, the Government had promised to prevent the youth from being unemployed for more than 100 days. By the end of 1997, the promise had still not been realized for the youth

¹ Two years earlier, a similar design had been set up for the youngest unemployed (18–19 years).

² The UVG-program will denote the municipal program that was introduced to enforce the guarantee.

³ The initials WPRS stand for “Worker Profiling and Reemployment Services”. The length of the unemployment spell of an Unemployment Insurance (UI) claimant is predicted. In order to continue receiving benefits, individuals with long predicted spells or high predicted probabilities of UI benefit exhaustion must accept to receive employment and training services early in their spell.

aged 20–24. The local employment offices were overcrowded by job seekers, and the caseworkers had no time to help their clients as effectively as desired. Thus, the idea to let the municipalities take over the responsibility for the unemployed youth seemed attractive for at least two reasons. The local employment offices would be able to allocate more resources to taking care of the adult unemployed while the municipalities took care of the youth. Furthermore, many argued that a decentralization of labor market policy to the municipal level—closer to the local labor market—would improve the quality of the programs.

From January 1, 1998, the municipalities could voluntarily agree with the National Labor Market Board to provide early intervention for the unemployed aged 20–24.⁴ Except for minor modifications and a change of name, it was still in practice in 2002. The municipalities have the opportunity to either continue or stop providing the guarantee at the beginning of each calendar year. The agreement is that the local employment offices are responsible for the youth during the first 90 days of unemployment. If the individual is still openly unemployed after 90 days, he or she is sent to the municipal program office which, in turn, has 10 days to assign the unemployed to some (appropriate) activity, referred to as the early intervention program.

The content of the activity varies among participants. According to studies on the implementation of early intervention program, during the first years, approximately 60% of the assignments were into work-place practice; roughly 15% into training; in the rest of the cases, the program consisted of a combination of both training and practice (SK, 1999; US, 2000). This approximately reflects the distribution of the traditional youth labor market programs provided by the National Labor Market Board. So, in essence, the youth is offered the same bundle of measures as they would have been, had the guarantee not existed.⁵

The local employment office sending the individual to the municipality pays a constant compensation of SEK 150 (€ 17) per participant and working day which is meant to cover the cost of administration and the early intervention program. Implementation studies indicate that the *actual* cost per participant has varied considerably among municipalities, possibly implying a variation in the quality of the activity (SK, 1999).

Compensation to participants is not included in the above amount. The size of the compensation depends on what the individual received as openly unemployed. Individuals qualified for unemployment insurance (UI) benefits receive an amount equal to the UI benefits while in a program, including the early intervention program. This is also the case for those qualified for (means tested) social assistance. If the individual rejects an offer to participate in a program without any acceptable reason, she can lose the benefits.⁶ Participants without any previous compensation for unemployment receive a moderate

⁴ The upper age limit is set to the 25th birthday: if the individual is registered as unemployed more than 100 days before her 25th birthday, she is covered by the guarantee. In practice, the interpretation of the age limit has varied among municipalities, which we discuss in more detail in Section 4.

⁵ Moreover, there is no evidence that the introduction of the early intervention altered the composition of traditional programs. We have checked this by comparing, in municipalities that provide the early intervention program, the distribution in traditional programs before and after the early intervention program.

⁶ The rules on this issue were clear: rejection will lead to loss of benefits. But in practice, the rule is not always strictly applied. For instance, according to an implementation study (US, 1999), one third of the participants in the UVG-program felt that they were forced into it.

compensation of SEK 1967 (\$203) per month. All three groups thus have financial incentives to enter the programs upon an offer.⁷

By 1999, approximately four municipalities out of five had agreed to provide the guarantee.⁸ In most cases, the reason for *not* providing it—according to the municipalities themselves (SK, 1999)—was that the number of long-term unemployed aged 20–24 was low. We may thus expect the economic environment to differ systematically between the municipalities that do and do not provide the guarantee. We will return to how this selection of municipalities affects the identification of the guarantee effect.

We have access to the Employment Service database (HÄNDEL) which contains all individuals registered as job seekers from 1991 and onwards. HÄNDEL includes information on the length of spells on unemployment, as well as data on some individual characteristics, including information on the municipality. For a detailed description of the data, see Carling and Larsson (2002).

Considering the nature of the guarantee, we would expect the mean pre-program unemployment period to be reduced in municipalities providing it. In fact, no one aged below 25 should be observed to be openly unemployed for more than 100 days. The data reveals that this was not the case, however. In 1997, roughly 25% of the unemployed individuals aged 22–24 were assigned to a program within the promised period. After the introduction of early intervention, in 1998, the corresponding share was 30%. Thus, the pre-program unemployment was indeed reduced but not to the expected extent.

Why the reduction was so moderate is not clear, but the local employment offices seem to have been reluctant to send unemployed individuals to the early intervention program, either due to distrust towards the municipal authorities or the relatively high cost of an assignment to the early intervention program for the employment office.⁹

3. What is the treatment?

The question in most evaluation studies is: *what was the effect of the treatment compared with what would have happened had the individual not received the treatment.* The identification of such an effect requires the existence of a no-treatment state. In the previous literature, it has been argued that the design of Swedish labor market policy during the 1990s implies that such a state is difficult to come by (Sianesi, 2002; for a discussion in Swedish, see Carling and Larsson, 2000).

The reason, in short, is that the participation in a program could always be postponed and it is observed that, given eligibility (i.e. being openly unemployed), the probability of program participation approaches almost unity after some one and a half year of unemployment. The relevant comparison state in the Swedish set-up is thus not *no*

⁷ Unlike other labor market programs, participation in early intervention program could not be used to qualify for renewed entitlement to UI benefits.

⁸ This figure is based on a survey of the Swedish Municipalities' Organization, SK (1999).

⁹ Compared with other labor market programs, the cost of SEK 150 (€ 17) per participant and working day is high. Recall that the compensation to the participant is not included in this amount.

treatment at all but no treatment now but potentially later. Consequently, in a strict sense, unless a non-eligible group is constructed, the evaluation studies would only be able to identify the effect of program timing.

The guarantee implies such an exceptional case of a constructed non-eligible group. The age limit at the 25th birthday, and the fact that not all municipalities offered the guarantee is the reason. The comparison in this study is thus between *a world with a guarantee* of program participation within 100 days of unemployment and *a world without such a guarantee*. In both the factual and the counterfactual states the traditional labor market programs are run as usual. However, early intervention reduces the expected pre-program unemployment periods for all those eligible, irrespective of them participating in the traditional programs or in the early intervention program. Actually, the early intervention program was not a large-scale program: in 1998, a majority of all program participants aged 22–24 years were still assigned to traditional programs. Only about 12% of all program participants were assigned to the early intervention program. Thus, the treatment is a faster assignment to the traditional programs and also, to some extent, assignment to a new (and possibly better) program.

Hence, we will interpret the treatment as an intervention that reduces young workers' pre-program unemployment spells by putting pressure on the employment offices and by the threat of lost unemployment compensation for them. We will disentangle the two parts of the treatment empirically. For the time being, it will be assumed that the intervention is successful in the sense that it represents a government's ability of implementing, on a decentralized level, an intended policy. This assumption is relaxed in Section 6.

One may wonder what can be said theoretically about the aforementioned treatment. We will here discuss the expected impact of early intervention in a job search theoretic framework. Let us begin by examining the situation without the guarantee, our counterfactual state. Two issues then affect the value of unemployment: the time limit of 300 days of the unemployment insurance (UI) benefits, and the possibility to participate in all labor market programs except the early intervention program.

From previous studies, both theoretical and empirical, we know that the job finding rate increases as the benefit exhaustion is approached (for example, see Mortensen, 1977). This is due to a decrease in the value of unemployment over time which, in turn, implies a decline in the worker's reservation wage. After the exhaustion date, the hazard is constant, given the stationarity of the wage offer distribution.

In the presence of labor market programs, however, the pattern may be different if the programs can be used to avoid UI benefit exhaustion. Until recently, this has been the case in Sweden. The evolution of the job finding rate now depends on how the unemployed worker values the program: the more attractive is the program, the smaller is the increase in the hazard rate. Theoretically, even decreasing exit rates from unemployment could be observed. Empirical evidence from Sweden suggests a slightly increasing job finding rate as the benefit exhaustion approaches, however (see Carling et al., 1996).

Labor market programs may, of course, have an impact even after the assignment to the program. If programs are effective, they may lead to more job offers, implying higher job finding rates and better jobs after participation. During participation, however, the search

activity is often observed to diminish, implying lower job finding rates. Better jobs after participation may also imply a lower risk of re-unemployment.

We can think of at least four potential effects of the introduction of early intervention in this setting. Recall that time-limited UI benefits and the possibility to participate in all other programs except the early intervention program characterize our comparison state. First, if the early intervention program is of better quality than the other available programs—as argued by the municipalities—we should find an increase in the job finding rate and a decrease in the re-unemployment rate during and after participation. During participation, the effect also depends on how much time participants in the early intervention program can allocate to job search compared to participants in other programs.

Second, the relative effectiveness of the early intervention program may also affect the job finding rates before participation, if unemployed workers are aware of it being better than other programs.¹⁰ If so, we would expect the hazard to increase less prior to participation in the presence of the early intervention. These effects should, however, be moderate, considering that only 12% of the participants were assigned to the early intervention program; the majority still participated in other programs.

Third, the introduction of the time limit of 100 days per se may alter the form of the hazard during the first 100 days of unemployment, even if the unemployed workers value early intervention program as much as all other programs. Recall that rejecting an offer to participate in early intervention program disqualifies the unemployed from UI benefits and social assistance. Moreover, supposedly, the guarantee implies that after 100 days of unemployment, the probability of being offered the early intervention program is equal to unity. Consequently, the benefits expire after 100 days unless the individual accepts to participate in the early intervention program. Thus, given that all programs are equally attractive to the unemployed workers, we would expect the job finding rate to increase more quickly in the presence of early intervention since the UI benefits are now exhausted earlier.

Fourth, the guarantee implies a quicker assignment to programs and thus, a reduction in the pre-program unemployment spells. If long-term unemployment makes an individual less attractive for the employers or reduces her search activity, shorter pre-program unemployment spells should imply increased job-finding rates during and after program participation. Such an effect could be interpreted as a positive impact, for those eligible, of *early* as compared to *late* participation.

In sum, the net treatment effect depends on the signs and magnitudes of these four effects. Due to the low assignment rate to the early intervention program, the third and fourth effect should dominate. Thus, we would expect to find an increase in the job finding rate, at least during the first 100 days of unemployment. Furthermore, if preventing long-term unemployment is indeed important, we should find an increase in the employment rate and a decrease in the re-unemployment rate even after the first 100 days.

¹⁰ Naturally, workers may care about other aspects than program effectiveness—for example the content of the program and the compensation level—when deciding on participation.

4. Identification of the treatment effect

Having access to the unemployment inflow before and after January 1, 1998, we can use three dimensions to identify the effect of early intervention (UVG): time, age and municipality. This is illustrated in Fig. 1.

Group A^1 , which consists of individuals younger than 25 who entered the unemployment registers during 1998 in a municipality providing early intervention, is the only group directly affected by early intervention. Depending on the assumptions of the indirect effects of early intervention or other changes in the environment, the treatment effect can be identified by some of the following equations:

$$\alpha^1 = (A^1 - A^0) \quad (1)$$

$$\alpha^2 = (A^1 - A^0) - (B^1 - B^0) \quad (2)$$

$$\alpha^3 = (A^1 - A^0) - (C^1 - C^0) \quad (3)$$

$$\alpha^4 = \{(A^1 - A^0) - (B^1 - B^0)\} - \{(C^1 - C^0) - (D^1 - D^0)\}, \quad (4)$$

where A^i , B^i , C^i , D^i ($i=0, 1$) now denote the labor market outcome for each group. α^1 compares the outcome of the treated group with the outcome of the corresponding age group that flowed into unemployment in the same municipalities the year before early intervention was introduced. This “before–after” estimator is only valid if there were no changes in the overall state of the youth labor market other than the introduction of the

	Flow 1997			Flow 1998	
	Not UVG	UVG		Not UVG	UVG
Age ≥ 25	D^0	C^0		D^1	C^1
Age < 25	B^0	A^0		B^1	A^1

Fig. 1. Dimensions for identifying the treatment effect. Note: ‘Not UVG’ refers to a municipality that did not provide the early intervention program during 1998, whereas ‘UVG’ refers to a municipality that did so.

guarantee between 1997 and 1998.¹¹ As we learn later on, the labor market did improve between these periods. But for this estimator to be valid, there also need to be no cohort effects, in particular *compositional changes* in unemployed youth between the two periods must be ruled out. Such changes may arise even from a violation of the above assumption. Given the different macro conditions it is possible that young people becoming unemployed in the good year 1998 were of the lower average labor market ‘quality’ than those becoming unemployed in the bad year 1997.

The estimators α^2 , α^3 , and α^4 identify the treatment effect by a comparison of other groups than the treated. We may obtain an unbiased estimate of the treatment impact by any of these as long as the guarantee did not indirectly affect the labor market of groups B and C, and all municipalities and age groups experienced a similar business cycle improvement.

Disregarding the indirect effects so far, let us consider the implications of the changes in the economic environment on the choice of the estimator. α^2 is valid as long as the business cycle improved to the same extent for an age group in municipalities with and without early intervention. If, on the other hand, changes in the economic environment differed between municipalities but were identical for the youth below and above the age of 25, α^3 is a valid estimator. Finally, α^4 will take care of both the municipality-specific and the age-group specific business cycle change, and thus appears to be an attractive estimator. Of course, it is required that any change in municipality-specific business cycle must be the same for the age-groups, and any change in age-specific business cycle must be the same in the two groups of municipalities.

However, early intervention may have had indirect or “spill-over” effects on groups B or C. For example, the municipalities choosing not to provide it may have put an additional effort into taking care of that age group to legitimate their choice. In that case, group B will be affected, and α^2 will produce a downward biased estimate of the true impact of treatment, even if the change in the business cycle is the same in the different municipalities.

Furthermore, the fact that the municipal offices took over the responsibility for the young unemployed below 25 may also have allowed the employment offices to take better care of the older youth. If so, α^3 will produce a downward biased estimate. In the presence of either of these indirect effects, α^4 will also be (downward) biased.

The evolution of pre-program unemployment rates from 1997 to 1998 provides a measure of the indirect effects. As already noted, the program assignment rate within 120 days rose from around 25–30% in group A.¹² Table 1a shows the program assignment rates before and after the introduction of early intervention for all four groups. The assignment rate is calculated as the number of individuals assigned within 120 days,

¹¹ The before–after estimator usually refers to a strategy for comparing an individual with herself, and thus requires longitudinal data. Heckman and Robb (1985) show that repeated cross-sectional data are sufficient to construct a before–after estimator as long as the expected no-program outcome after the introduction of the program equals the no-program outcome before the introduction. Another way of stating this assumption is to claim that the approximation error averages out.

¹² By setting the limit to 120 days instead of 100, we make sure that our results do not depend on a short delay in registering the assignment.

Table 1
Collection of the empirical results

(a) Implementation: *Probability of being assigned to any program within 120 days (%)*

Group	1998			1997		
	Obs.	Estimate	SE	Obs.	Estimate	SE
A	60,884	30.0	0.19	63,545	25.4	0.17
B	7877	33.9	0.53	8158	31.8	0.52
C	59,075	20.4	0.17	55,438	19.6	0.17
D	7131	23.8	0.50	6583	23.5	0.52

(b) Outcome measure: *Share of ES days (%)*

Group	1998			1997			Parameter	Estimate	SE
	Obs.	Mean	SE	Obs.	Mean	SE			
A	60,884	43.8	0.12	63,545	45.9	0.12	α^1	−2.1	0.17
B	7877	46.0	0.34	8158	48.4	0.33	α^2	0.3	0.50
C	59,075	46.4	0.12	55,438	47.9	0.13	α^3	−0.6	0.25
D	7131	48.1	0.36	6583	49.3	0.37	α^4	0.6	0.71

(c) Outcome measure: *Expected duration of unemployment (days)*

Group	1998			1997			Parameter	Estimate	SE
	Obs.	Estimate	SE	Obs.	Estimate	SE			
A	60,884	142	1.0	63,545	177	1.4	α^1	−35	1.72
B	7877	132	2.7	8,158	169	3.6	α^2	2	4.82
C	59,075	172	1.2	55,438	205	1.6	α^3	−2	2.64
D	7131	161	3.4	6,583	197	4.7	α^4	−1	7.80

(d) Outcome measure: *Probability (%) of finding a job within 120 days of unemployment*

Group	1998			1997			Parameter	Estimate	SE
	Obs.	Estimate	SE	Obs.	Estimate	SE			
A	60,884	74.7	0.20	63,545	67.5	0.21	α^1	7.2	0.29
B	7877	76.3	0.53	8158	68.5	0.57	α^2	−0.3	0.83
C	59,075	68.8	0.21	55,438	63.8	0.23	α^3	2.2	0.43
D	7131	72.2	0.59	6583	66.5	0.65	α^4	0.1	1.25

(e) Outcome measure and heterogeneity: *Share of ES days (%) for strongest (S) and weakest (W) quartiles*

Group	Q.	1998		1997		Parameter	Q.	Estimate	SE
		Mean	SE	Mean	SE				
A	S	37.6	0.22	40.2	0.22	α^1	S	−2.6	0.31
	W	54.4	0.27	55.1	0.25		W	−0.7	0.37
B	S	39.0	0.63	41.4	0.65	α^2	S	−0.2	0.96
	W	55.8	0.73	57.4	0.67		W	0.9	1.06
C	S	40.3	0.25	43.1	0.25	α^3	S	0.2	0.47
	W	55.1	0.23	55.9	0.25		W	0.1	0.50
D	S	40.3	0.80	43.8	0.77	α^4	S	−0.9	1.51
	W	55.3	0.63	55.8	0.65		W	1.2	1.28

divided by the total number of unemployed individuals excluding those exiting unemployment within 120 days for other reasons than program participation.¹³

As expected, the program assignment rate increased most among individuals directly affected by the introduction of early intervention.¹⁴ However, the pre-program period of the age group below 25 was also shortened in non-providing municipalities. Table 1a also shows that these municipalities were relatively effective in assigning individuals to programs already in 1997, which provides an explanation for why they did not conclude an agreement on the program. The increase nevertheless suggests that early intervention may have had an indirect effect on group B, implying that α^2 produces a downward biased estimate of the guarantee effect. Consequently, α^4 may also be biased.

The program assignment rate among the older youth does not seem to have changed significantly from 1997 to 1998, however, suggesting that we should use α^3 to estimate the treatment effect. A further argument for using α^3 is that groups B and D are relatively small, implying a low estimate precision. However, the main reason why we prefer α^3 to α^2 and α^4 is related to selection: an individual's date of birth may be regarded as random, whereas the decision made by the local authorities to provide the guarantee was far from random.

The assumption underlying α^3 is that the labor market trends (within early intervention areas) are the same for those above and below the age-limit. In theory, we may use the age limit of 25 to estimate the treatment effect by comparing those arbitrarily close to the limit. However, there are two practical problems. First, the standard errors increase as we approach the age limit and second, the interpretation of the age limit varied between municipalities and individuals, implying that in practice, the limit was not sharp. Some municipalities assigned individuals close to their 25th birthday to the program whereas other municipalities were very strict about the age limit.

In practice, the identification strategy is based on information on whether and when the individual's municipality began providing the early intervention, and the individual's age when registering with the Employment Service (ES). Furthermore, the time dimension is based on the date of entry into the ES records: individuals entering during 1997 (1998) are included in the inflow 1997 (1998). The following example illustrates the construction of the different groups.

An individual registering with ES in February 1998 is included in group A (early intervention providing municipality, age <25) if

- (i) the municipality where she lives has started offering early intervention some time during 1998, and if
- (ii) she was at least 22 years in February 1998, and did not celebrate her 25th birthday before March 1998.

¹³ This is a sufficient measure, since we found the program assignment hazard rates to be roughly constant in the first year.

¹⁴ The 4.6 percentage point increase corresponds to approximately 18%. Most of the increase seems to be due to the introduction of early intervention program; in 1998, around 12% of all program participants in our sample were assigned to the early intervention program.

Thus, if her municipality did not start offering guarantee in 1998, she is included in either of the non-providing municipality groups B or D, depending on her age. Furthermore, if she was at least 25 but not yet 28 in February 1998, she is included either in group C or D.

We apply an identical age definition to the inflow in 1997. The municipality dimension is now based on the 1998 information: an individual living in a municipality that started providing early intervention some time in 1998 is included in group A or C, depending on her age in 1997.¹⁵

5. Empirical results

5.1. The outcome measures

We can follow the individuals in the Employment Service records until 22 June 2000. The effect of early intervention is defined using various outcome measures. Since the goal of early intervention—similar to all Swedish active labor market programs—is to shorten the unemployment period and increase the chances of getting a job, we examine the job finding rate during the first unemployment period in 1998 (1997 for the comparison groups A^0 , B^0 , C^0 , D^0).

We reckon, however, that the best measure of the effect of the guarantee is obtained when the first *and* (potential) subsequent unemployment spells are examined simultaneously. The share of days an individual is registered with the Employment Service (ES) as a job seeker within a period of 1.5 years after the start of the initial unemployment period captures all spells of unemployment, employment, and regular education during that period. The variable thus provides a measure of future employment stability.¹⁶

5.2. The net treatment effect

Table 1b shows the share of days registered in the ES records as a job seeker during the 18 months period after the start of the unemployment, thus reflecting the net effect of the guarantee on unemployment. The overall decrease in the share variable reflects the improvement in the state of the labor market from 1997 to 1998. Consequently, the before–after estimator produces the most favorable estimate of the treatment effect.

The sign of the estimated effect depends on which of the estimators α^1 , α^2 , α^3 , or α^4 is chosen. In our opinion, the best comparison group consists of individuals above 25 in

¹⁵ Individuals in the late inflow in 1997 may have been covered by the guarantee if they knew it was to be introduced in their municipality at the beginning of 1998. Furthermore, some of the early inflow in 1998 in group A may not have been covered by early intervention if their municipality did not offer it until the fall. Section 5.4 discusses these issues.

¹⁶ The reason for choosing 1.5 years, or 539 days to be precise, is that we can follow the sample until 22 June 2000. Thus, the maximum period we can observe for an individual whose unemployment starts on December 31, 1998 is 539 days. Naturally, it would be preferable to follow the individuals for a longer period of time to be able to say something about the long-term effects.

early intervention providing municipalities. According to α^3 , early intervention moderately decreased future unemployment by 0.6 percentage points. In relative terms, this corresponds to 1.3%. Comparing the treatment group to the corresponding age group in non-providing municipalities, α^2 yields a slightly negative but statistically insignificant estimate. Table 1a suggests that this result is downward biased, however.

In sum, we find no evidence for a strong net effect of early intervention in either direction. If anything, the results suggest that it slightly decreased the number of days registered with ES, thus indicating a very small positive treatment effect.

5.3. Dynamics of the treatment effect

To explore the composition of the net effect in more detail, we have examined the duration of the first unemployment spell. We are interested in the probability of employment.

Table 1c gives the change in mean length of the first unemployment spell for the four groups. The mean is calculated using results from empirical hazard estimations.¹⁷ As for the net impact, the before–after estimator again produces the most favorable estimate for the treatment impact. As soon as the development of group A is related to any comparison group, the estimated effect turns to zero. In other words, the results do not suggest that the guarantee *on average* had any significant impact on the length of the first unemployment spell.

However, recall that the theory suggested that the guarantee might already have an impact during the first 100 days of unemployment, as the UI benefits expire unless the individual accepts to participate in the early intervention program. The results in Black et al. (2002) show that such an impact may exist even without the threat of UI benefit expiration, if the individuals consider the program to be worse than open unemployment.

We use the same empirical hazard estimations as presented above to estimate the probability of finding a job within 120 days of unemployment. The results are reported in Table 1d. Once more, we consider α^3 to be the most valid estimator, and thus, Fig. 2 shows the evolution of the (smoothed) hazard for groups A and C.

The guarantee does indeed seem to have a positive impact on the probability of employment during the first 120 days. At the beginning of the unemployment spell, the impact is estimated to be roughly 10%, then decreasing to approximately 0 for 120 days.¹⁸ However, the results in Table 1c–d and Fig. 2 together suggest that the positive impact on employment during the first 120 days is neutralized by a decreased probability of employment during and after participation, possibly due to decreased job search, or a “lock-in” effect, among participants. Recall from Table 1a that the introduction of the guarantee seems to have increased the total volume of program participation. Given that

¹⁷ In the calculations, it is postulated that the hazard is constant after 1110 days. Eberwein et al. (2002) propose to use the median instead, which in our case gives qualitatively the same result.

¹⁸ It should be noted that this impact is expressed in percent, whereas α^1 – α^4 in Table 1d are expressed in percentage points. Furthermore, the distribution of spells ending on various days is not uniform, and thus, summing the impact in Fig. 2 over the 120-day period produces the 2.2 percentage point impact estimated by α^3 .

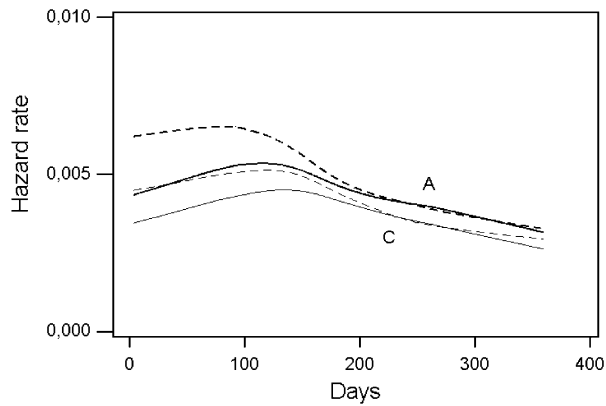


Fig. 2. The empirical hazard rates for the treatment (A) and the comparison (C) group, 1997 (continuous line) and 1998 (dashed line).

the unemployed individuals search less while participating compared to when in open unemployment, we would expect to find an increased “lock-in” in group A.

5.4. Additional checks of the results

Variation in the impact of treatment across individuals is an important aspect in evaluating labor market programs. Individual characteristics, like gender or educational background, may be sources of such variation (for an example of Swedish youth programs, see Larsson, 2003). The goal of early intervention and most other Swedish labor market programs is to help those who need help most, i.e. individuals with a weak position on the labor market. We thus want to evaluate this goal by examining the variation in the impact of treatment across individuals with a *strong* versus a *weak initial position*. Initial refers to the state at the time when the individual registers as unemployed.

As an indicator of the individual’s strength on the labor market, we use her history in the Employment Service register prior to the actual unemployment spell.¹⁹ The more the individual has been registered with the ES, basically implying either open unemployment or participation in some labor market program, the weaker is her position on the labor market. Table 1e reports the results for the strongest and the weakest quartile in each group A–D. In short, there is no considerable heterogeneity in the treatment effect between the strong and the weak; α^3 produces almost identical estimates for the quartiles.

Information on whether and when a municipality provided early intervention is crucial for the identification of the treatment effect. Thus, we have checked the result with respect to a number of modifications in the municipality variable. In the analysis presented so far, all municipalities that started providing the guarantee some time during 1998 are included in group A. This implies that group A may contain individuals who registered as

¹⁹ A detailed description on how the variable is defined is found Carling and Larsson (2002) as well as an extended analysis of heterogeneous effects.

unemployed before the municipalities introduced the guarantee. In other words, group A may be contaminated. Nevertheless, the results are the same when all municipalities with a starting date later than January 1, 1998 are excluded.

Another issue is whether the late inflow in 1997 was in fact covered by early intervention in municipalities that started offering it in early 1998. The guarantee may have affected the behavior already in 1997 if the individuals knew that it was about to be enforced. To check how this inflicted the results, we have excluded the inflow after September both in 1997 and 1998: the results do not change notably.

Moreover, we have tested different age restrictions, as well as excluded those individuals who were not entitled for unemployment insurance benefits. The results remain the same in both cases.

Finally, we have had the possibility of controlling for a rich set of covariates. This has been done either, depending on the outcome measure, by least-squares regression or semi-parametric hazard-regression. The caveat of the regression approach is that something must be assumed about the relationship between the outcome variable and the covariates. We have applied a simple linear model and obtained the same estimate of the treatment-effect with a minor reduction in the standard error.

6. Relaxing the assumption of an effective implementation of early intervention

In Section 3, we introduced the assumption that early intervention was implemented as intended. The results presented in Table 1a suggest this not to be the case. The increase in the program assignment rate was 5 percentage points for the treated group, that is A. Potentially, the introduction of the guarantee could have brought the rate to 100%, i.e. an increase in the program assignment rate of 75 percentage points. Would the assessment of the guarantee differ had the implementation been more successful? Using the terminology of Imbens (2000), among others, would the *response* to the treatment have been stronger if the treatment *dose* had been higher? This question is of interest for two reasons. First, there is some evidence that the guarantee gradually came into force, and if so, an evaluation in the first year of early intervention might underestimate the guarantee-effect in the years to come. Second, an implementation of a similar policy in a different setting might induce a much stronger dose, and if so, the present findings might not be particularly useful for predicting the outcome of that policy.

Let the treatment dose be the increase in the program assignment rate (within 120 days) in 1998 compared with 1997. The response is the change 1998 in the *share of ES days* variable compared with 1997. Fig. 3 shows the fitted line between the mean response and the mean dose of the four groups A–D: It is thus estimated that an increase in the dose by 1 percentage point results in a 0.17 percentage point decrease in the response.

In what follows, we assume that it is possible to implement the policy such that the dose is 75 percentage points, i.e. the guarantee, in accordance with the political promise, implies that every youth gets early training. We also make the assumption that the effect is indeed homogenous as reported in Section 5.4 and that the response is linear in dose. Then, the expected decrease in *ES days* due to a fully implemented guarantee can be extrapolated by using the slope-coefficient in Fig. 3 to be about 12 percentage points, implying a

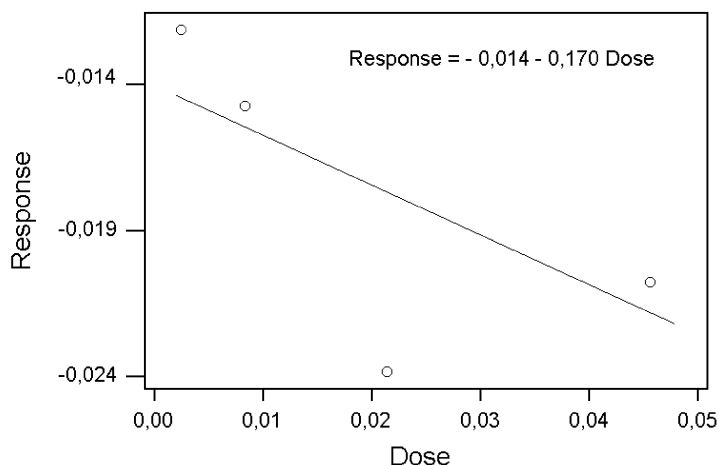


Fig. 3. Share of ES days (Response) and program assignment rate (Dose).

substantial improvement. However, the standard error is 5.1 percentage points, and therefore nothing precise could be said about the magnitude of the effect of such guarantee. Hence, we proceed by checking the credibility of the effect-estimate of 12 percentage points with two alternative approaches.

The first approach exploits the between-municipalities variation in dose. Presupposing that the municipality-specific dose is exogenous, we can use this variation on the municipality level to estimate a dose–response function. Fig. 4 shows a relation between the dose and the response comparable to Fig. 3.²⁰ The slope-coefficient is only 0.014, which implies that the expected decrease in *ES days* due to such guarantee is only slightly greater than 1 percentage points.

The second approach exploits the time-variation in the dose. Fig. 5 shows the evolution of the probability of being assigned to any program within 120 days. The x-axis is set to zero at the introduction of early intervention, the vertical dashed line highlights the fact that the unemployed in the inflow were affected by the guarantee some 100 days prior to its introduction. The solid line shows that group C had a time-constant probability during 1997 and 1998. Group A, on the other hand, experienced a steady increase in the probability starting about the time of the introduction of early intervention. Evidently, the guarantee came into effect gradually, rather than immediately on January 1, 1998. The implication is that the dose increased gradually during 1998, being about 2 percentage points in early 1998 and about 8 points in late 1998.²¹

²⁰ The large fraction of dose taking on negative values might be surprising. This is however partly due to the many municipalities of small size. We have excluded all those with less than 100 individuals and redone the analysis. We get the same estimated value for the slope parameter and note that 13 out of 69 big municipalities had a negative dose.

²¹ Note the increase prior to the guarantee. It follows as a consequence of the possibility for workers flowing into unemployment in late 1997 to be affected by the guarantee as long as 100 days in unemployment had not passed at the turn of the year.

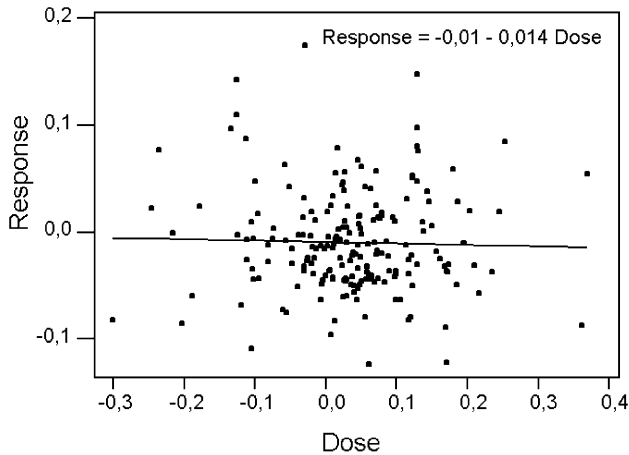


Fig. 4. Dose–response regression, all early intervention providing municipalities.

If the dose increased during 1998, we should also expect to see an increase in the early intervention effect during the year. In fact, using the estimate of the slope-coefficient in Fig. 3 and the six percentage points increase in dose in 1998, we should expect about 0.72 increase in the early intervention effect.

We have calculated an estimate of α^3 for each month in 1998, and even the last three months in 1997. Fig. 6 shows the evolution of the early intervention effect. The effect increases during the year as expected, but by only about 0.1 percentage points, which is much smaller than the expected 0.72.

In sum, it is difficult to be specific on the magnitude of the early intervention effect, had the guarantee actually ensured the unemployed youth training within 100 days. The extrapolation in Fig. 3 indicates quite strong effects of such a guarantee. But there are

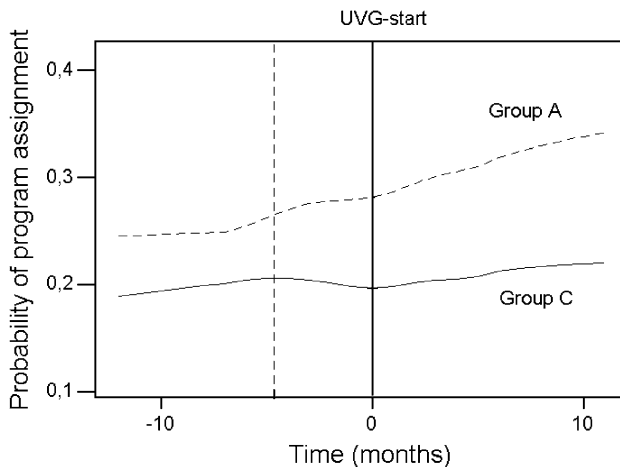


Fig. 5. The assignment probability over time (months). The functions are smoothed.

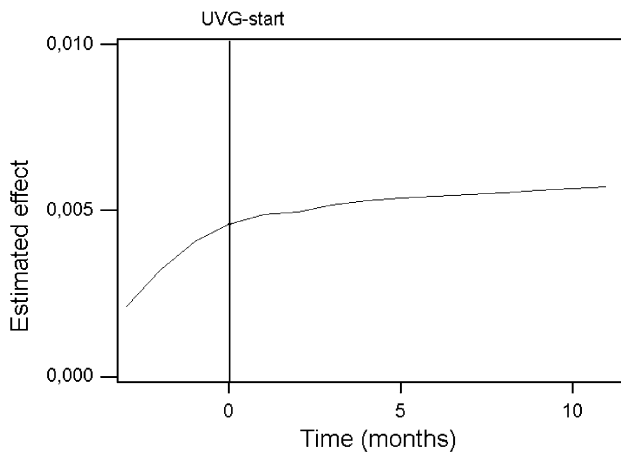


Fig. 6. The early intervention effect estimated by month. The function is smoothed.

reasons to be skeptical to this form of mechanical extrapolation of the observed dose–response relationship. The large standard errors is one reason, the other is the sensitivity to the choice of estimator. In Section 5, estimators α^2 , α^3 , or α^4 were insignificantly different, yet consistently pointed at no effect of early intervention. Extrapolation based on these three estimators would however yield very different estimates of the guarantee-effect. Additionally problematic is that the strong effect is not confirmed when we are using two different approaches to determine the size of the early intervention effect. Both approaches suggest that a completely successful implementation of the guarantee would have lowered *ES days* by only one or two percentage points.

7. Conclusions

This paper is an evaluation of a youth measure called Utvecklingsgarantin (UVG), or early intervention, with the goal of preventing open unemployment spells longer than 100 days; open unemployment here referring to a state where the individual does not participate in a labor market program. The set-up of the guarantee implies three possible dimensions for identification of the treatment effect: age, municipality, and time. We claim that this design allows us to compare *a world with a guarantee* with *a world without such a guarantee*.

We have four major findings. First, using the Employment Service (ES) records, we evaluate the overall impact of the guarantee on the subsequent labor market attachment. We estimate a modest decrease in the number of days the individual is registered with the ES during the 18-month period after the start of the unemployment. This decrease, however, is too small to indicate an appreciably more stable transition out of unemployment.

Second, we find a slightly increased probability of employment during the first 120 days of unemployment, similar to the results shown in Black et al. (2002), suggesting that

early intervention works more as a threat than a promise. This small positive impact is neutralized by a negative impact after the first 120 days, however. Thus, on average, the first unemployment spell is not significantly shorter in the group covered by the guarantee.

Third, we find no evidence that the treatment effect would depend on individual characteristics reflecting the individual's initial attachment to the labor market. We consider this attachment to be stronger the shorter is the individual's unemployment history.

Fourth, early intervention was everything but a guarantee: it implied an increase from 25–30% in the probability of being assigned to some program within the promised 100-day period. In an attempt to determine the treatment effect had early intervention actually been a guarantee, i.e. a 100% probability of being assigned to some program within the promised 100-day period, we obtain a large and positive treatment effect, although very imprecisely estimated. This caused us to consider alternative methods of determining the effect. They confirmed the positive effect but of a much smaller magnitude. Our prior belief based on this analysis is that the effect would be small were the guarantee to be more successfully implemented.

The result that only less than a third of the target group was assigned to a program within the promised 100 days is noteworthy per se. To call for a 100% assignment is probably not desirable, since some of the individuals may have had definite job or study plans in the close future. But claiming this to be the case for seven out of ten is unrealistic. The reluctance to put the guarantee into effect at the local employment offices may have been due to the offices mistrusting the municipal authorities or economic disincentives. In any case, exploring the underlying reasons for this result for a future design of similar guarantees is crucial.

Returning to the question posed in the title of this paper: do our results suggest that early intervention helps the unemployed youth? Naturally, the answer depends on the desired impact. In the very short run, early intervention indeed seems to have succeeded in slightly increasing employment. This small positive impact disappears in course of time, however, probably due to a low search activity during participation in the early intervention program and other labor market programs. Early intervention increased the total volume of program participation, and thus, more individuals were "locked in" into a passive job search. The impact of a shorter unemployment history on employment stability during the following 18 months also seems to be negligible. Thus, our conclusion is that, at least in this specific case, shortening the unemployment spell does not seem to have played any significant role for the individual's labor market prospects within the subsequent 18 months.

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