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by

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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.

Keywords: guarantee, program evaluation, regression-discontinuity design.

JEL-codes: C14, J64, J68.

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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 unemployed, i.e. not participating in any labor market program, 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. A municipal program was first introduced for the youngest unemployed, and after two years, a similar program even comprised the older youth, i.e. up to 24 years. 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.

This paper focuses on the program directed at the unemployed aged 20-24, referred to as Utvecklingsgarantin (UVG).¹ It was introduced in January 1, 1998, and is still in practice. In essence, the UVG-program is a blend of the conventional features in many other programs as it consists of vocational rehabilitation (training), work schemes, and (to a lesser extent) on-the-job training. What is novel is the fact that the youth are guaranteed an assignment to the UVG-program no later than 100 days after becoming unemployed, given that they are still openly unemployed.

Our goal in this paper is thus to determine the *effect of a guarantee for program participation* on the subsequent labor market attachment. 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 among the youth, if considered more as a threat than a guarantee. Black *et al.* (2002) provide evidence for such a pattern as they evaluate the WPRS system

¹ Henceforth we refer to this program as the UVG-program or the UVG-guarantee.

in the US.² 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 the UVG-guarantee, we make use of three conditions: first, it covers individuals aged 24 but not 25, implying that we might be able to apply a regression-discontinuity design for the study.³ Second, the municipalities volunteered for being responsible for the UVG-program, 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 settings and the UVG-program. Section 3 presents a search-theoretic framework for our empirical analysis, and Section 4 discusses the identification strategy. In Section 5, we show the empirical results, and the final section concludes.

2 The design of the UVG-program

The UVG-program differs from traditional youth labor market programs in at least two significant ways. First, it implies a guarantee for some kind of activity within 100 days of (open) unemployment. Second, it is run by the municipalities instead of the National Labor Market Board.

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 aged 20-24. The local employment offices were overcrowded by job seekers, and the caseworkers had no time to help their

² 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.

³ For a discussion and applications of the regression discontinuity approach, see *e.g.* Angrist & Krueger (1999) and Hahn *et al.* (2001).

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 the UVG-program for the unemployed aged 20-24.⁴ Except for minor modifications and a change of name, the program is still in practice in 2002. The municipalities have the opportunity to either continue or stop providing the program at the beginning of each calendar year.

This agreement implies that the local employment offices are responsible for the youth during the first 90 days of unemployment. If the individual is still unemployed after 90 days, he or she is sent to the municipal UVG-office which, in turn, has 10 days to assign the unemployed to some (appropriate) activity.

The content of the activity varies among participants. The possibility to combine different measures in order to adjust the program to the individual is novel to UVG. According to studies on the implementation of UVG, during the first years, approximately 60 percent of the assignments were into work-place practice; roughly 15 percent into training; in the rest of the cases, the program consisted of a combination of both training and practice (SK, 1999, and US, 2000). This approximately reflects the distribution of the traditional youth labor market programs provided by the National Labor Market Board.

The local employment office assigning the individual to the program pays the municipality a constant compensation of SEK 150 (\$15.5) per participant and working day which is meant to cover the cost of administration and the actual program. Implementation studies indicate that the *actual* cost per participant has varied considerably among municipalities, possibly implying a variation in the quality of the program (SK, 1999).

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 to be assigned to UVG. In practice, the interpretation of the age limit has varied among municipalities, which we discuss in more detail in Section 4.

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 during UVG. This is also the case for those qualified for (means tested) social assistance. If the individual rejects an offer to participate in UVG without any acceptable reason, she can lose the benefits.⁵ UVG-participants without any previous compensation for unemployment receive a moderate compensation of SEK 1,967 (\$203) per month. All three groups thus have financial incentives to accept an UVG offer.⁶

By 1999, approximately four municipalities out of five had agreed to provide the UVG-program.⁷ In most cases, the reason for *not* providing UVG – 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 UVG-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 the Appendix.

Considering the design of the program, we would expect the mean pre-program unemployment period to be reduced in municipalities providing the program. 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 percent of the unemployed individuals aged 22-24 were assigned to a program within the promised period. After the introduction of UVG, in 1998, the corresponding share was 30 percent. Thus, the pre-program unemployment was indeed reduced but not to the expected extent.

⁵ The rules on this issue were clear: rejection will lead to loss of benefits. But in practice, the rule was not always strictly applied. According to an implementation study (US, 1999), one third of the participants felt that they were forced into the program.

⁶ Unlike other labor market programs, participation in UVG could not be used to qualify for renewed entitlement to UI benefits.

⁷ This figure is based on a survey of the Swedish Municipalities' Organization, SK (1999). Information on which municipalities provided the program in 1998 is difficult to obtain, as described in the Data Appendix.

Why the reduction was so moderate is not clear, but the local employment offices seem to have been reluctant to assign unemployed individuals to the UVG-program, either due to a distrust towards the municipal authorities or the relatively high cost of an assignment to the UVG-program for the employment office.⁸

Thus, what treatment do we evaluate? In general, the treatment is “being covered by the UVG-guarantee”. The UVG-program reduced the pre-program unemployment periods for all participants, independent of the program. It was not a large-scale program, however: in 1998, a majority of all program participants aged 22-24 years were still assigned to other programs than the UVG. Only approximately 12 percent of all participants were assigned to the UVG. The treatment thus mainly consisted of a faster assignment to some of the traditional programs but, to some extent, also of participation in a new (and possibly better) program.

3 Theoretical framework

In this section, we outline a simple model of an unemployed worker’s job search to illustrate the expected impact of a guarantee such as the UVG-program. Let us begin by examining the situation without the UVG-guarantee, our comparison 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 UVG-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 re-

⁸ Compared with other labor market programs, the cost of SEK 150 (\$15.5) per participant and working day is high. Recall that the compensation to the participant is not included in this amount.

cently, 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 the UVG-guarantee in this framework. Recall that time-limited UI benefits and the possibility to participate in all other programs except the UVG characterize our comparison state. First, if the UVG-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 UVG-program can allocate to job search compared to participants in other programs.

Second, the relative effectiveness of the UVG-program may also affect the job finding rates before participation, if unemployed workers are aware of UVG being better than other programs.⁹ If so, we would expect the hazard to increase less prior to participation in the presence of the UVG. These effects should, however, be moderate, considering that only 12 percent of the participants were assigned to UVG; 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 UVG as much as all other programs. Recall that rejecting an offer to participate in UVG 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 UVG-program is equal to unity. Consequently, the benefits expire after 100 days

⁹ 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.

unless the individual accepts to participate in UVG. 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 UVG 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 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 UVG-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.

4 Identification of the treatment effect

4.1 What is the comparison state?

The question in most evaluation studies is *what was the effect of the treatment compared to 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 identify (Sianesi, 2002. For a discussion in Swedish, see Carling and Larsson, 2000).

The reason, in short, is that it is virtually impossible to avoid participating in a program given that unemployment lasts sufficiently long. The probability of being assigned to a program sooner or later is close to unity. The relevant comparison state in the Swedish set-up is thus not *no treatment at all* but *no treatment now but perhaps later*. Consequently, in a strict sense, as long as no group is excluded from the treatment, the evaluation studies are only able to identify the effect of program timing.

The design of the UVG-program provides an exception, however. The age limit at the 25th birthday, and the fact that not all municipalities provide the program imply that a no-treatment state exists. 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*. Naturally, all the other programs exist in both worlds.

4.2 Identification

Having access to repeated cross sections before and after the introduction of the UVG-program on January 1, 1998, we can use three dimensions to identify the effect of UVG: time, age and municipality. This is illustrated in Figure 1.

Figure 1 Dimensions for identifying the treatment effect

	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

Note: ‘Not UVG’ refers to a municipality that did not provide the UVG-program during 1998, whereas ‘UVG’ refers to a municipality that did so.

Group A¹, which consists of individuals younger than 25 who entered the unemployment registers during 1998 in a municipality providing UVG, is the only group directly affected by UVG. Depending on the assumptions of the indirect effects of UVG or other changes in the environment, the treatment effect can be identified by some of the following equations:

- (1) $\alpha^1 = (A^1 - A^0)$
- (2) $\alpha^2 = (A^1 - A^0) - (B^1 - B^0)$
- (3) $\alpha^3 = (A^1 - A^0) - (C^1 - C^0)$
- (4) $\alpha^4 = \{(A^1 - A^0) - (B^1 - B^0)\} - \{(C^1 - C^0) - (D^1 - D^0)\},$

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 UVG 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 UVG-program between 1997 and 1998.¹⁰

The estimators α^2, α^3 , and α^4 identify the treatment effect through comparison groups. We may obtain an unbiased estimate of the treatment impact by any of these as long as the UVG-program 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 the UVG-guarantee. 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.

However, UVG 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.

¹⁰ 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.

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 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 to 30 per cent in group A.¹¹ Figure 2 shows the program assignment rates before and after the introduction of UVG for all four groups. The assignment rate is calculated as the number of individuals assigned within 120 days, divided by the total number of unemployed individuals excluding those exiting unemployment within 120 days for other reasons than program participation.¹²

Figure 2 The estimated probability of being assigned to any program within 120 days (%). Standard errors in parentheses.

		Flow 1997		Flow 1998	
		Not UVG	UVG	Not UVG	UVG
Age	≥ 25	23.5 (0.52)	19.6 (0.17)	23.8 (0.50)	20.4 (0.17)
	< 25	31.8 (0.52)	25.4 (0.17)	33.9 (0.53)	30.0 (0.19)

Note: The young age group consists of individuals aged 22-24, the old age group includes individuals aged 25-27.

¹¹ 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.

¹² This is a sufficient measure, since we found the program assignment hazard rates to be roughly constant in the first year.

As expected, the program assignment rate increased most among individuals directly affected by the introduction of UVG.¹³ However, the pre-program period of the age group below 25 was also shortened in non-providing municipalities. Figure 2 also shows that these municipalities were relatively efficient 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 UVG 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 UVG-guarantee was far from random.

In theory, we may use the age limit of 25 to estimate the treatment effect by a sharp regression-discontinuity design. 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.

5 Empirical results

5.1 The dimensions of identification in practice

The identification strategy is based on information on whether and when the individual's municipality began providing the UVG-guarantee, 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: individu-

¹³ The 4.6 percentage point increase corresponds to approximately 18 percent. Most of the increase seems to be due to the introduction of UVG; in 1998, around 12 percent of all program participants in our sample were assigned to the UVG-program.

als 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 (UVG-providing municipality, age < 25) if

- (i) the municipality where she lives has started providing the UVG-program 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.

Thus, if her municipality did not start providing the UVG-program 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 the UVG-program some time in 1998 is included in group A or C, depending on her age.¹⁴

5.2 The outcome measures

We can follow the individuals in the Employment Service records until 22 June 2000. The effect of UVG is defined using various outcome measures. Since the goal of UVG – similar to all 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 A⁰, B⁰, C⁰, D⁰).

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,

¹⁴ Individuals in the late inflow in 1997 may have been covered by the UVG-program if they knew that the program 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 the UVG-program if their municipality did not start providing the program until the fall. Section 5.7 discusses these issues.

employment, and regular education during that period. The variable thus provides a measure of future employment stability.¹⁵

5.3 The net treatment effect

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

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

¹⁵ 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.

Figure 3 Mean of *share of ES days* (%). Standard errors in parentheses.

		Flow 1997		Flow 1998	
		Not UVG	UVG	Not UVG	UVG
Age ≥ 25		49.3 (0.37) n=6 583	47.9 (0.13) n=55 438	48.1 (0.36) n=7 131	46.4 (0.12) n=59 075
Age < 25		48.4 (0.33) n=8 158	45.9 (0.12) n=63 545	46.0 (0.34) n=7 877	43.8 (0.12) n=60 884
		$\alpha^1 = -2.1 (0.17)$ $\alpha^2 = 0.3 (0.50)$ $\alpha^3 = -0.6 (0.25)$ $\alpha^4 = 0.6 (0.71)$			

5.4 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.¹⁶

Figure 4 shows 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 UVG-guarantee *on*

¹⁶ For the definition of employment and unemployment, see the Data Appendix. Alternative definitions (including e.g. temporary employment and part-time unemployment into employment) do not significantly alter the results.

¹⁷ In the calculations, it is postulated that the hazard is constant after 1 110 days.

average had any significant impact on the length of the first unemployment spell.

Figure 4 Expected duration of unemployment. Standard errors in parentheses.

		Flow 1997		Flow 1998	
		Not UVG	UVG	Not UVG	UVG
Age \geq 25		197 (4.7) n=6 583	205 (1.6) n=55 438	161 (3.4) n=7 131	172 (1.2) n=59 075
Age < 25		169 (3.6) n=8 158	177 (1.4) n=63 545	132 (2.7) n=7 877	142 (1.0) n=60 884
		$\alpha^1 = -35 (1.72)$ $\alpha^2 = 2 (4.82)$ $\alpha^3 = -2 (2.64)$ $\alpha^4 = -1 (7.80)$			

However, recall that the theory suggested that the UVG-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 UVG. 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.

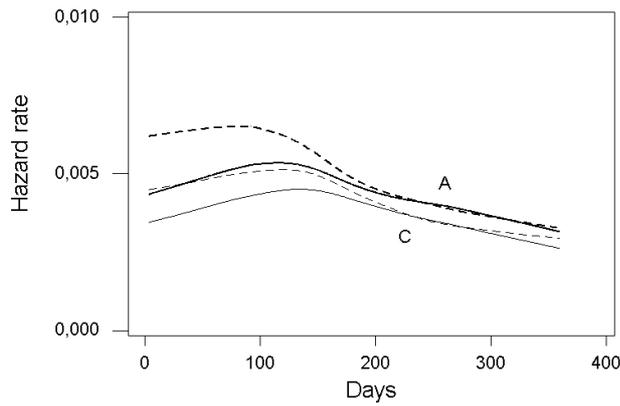
We use the same empirical hazard estimations as presented above to estimate the probability of finding a job within 120 days of unemployment. The results are reported in Figure 5. Once more, we consider α^3 to be the most valid estimator, and thus, Figure 6 shows the evolution of the hazard for groups A and C.

Figure 5 The estimated probability of finding a job within 120 days of unemployment. Standard errors in parentheses.

	Flow 1997		Flow 1998	
	Not UVG	UVG	Not UVG	UVG
Age ≥ 25	66.5 (0.65)	63.8 (0.23)	72.2 (0.59)	68.8 (0.21)
Age < 25	68.5 (0.57)	67.5 (0.21)	76.3 (0.53)	74.7 (0.20)

$\alpha^1 = 7.2$ (0.29)
$\alpha^2 = -0.3$ (0.83)
$\alpha^3 = 2.2$ (0.43)
$\alpha^4 = 0.1$ (1.25)

Figure 6 The empirical hazard rates for the treatment (A) and the comparison (C) group, 1997 (continuous line) and 1998 (dashed line).



The UVG-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 percent, then decreasing to approximately zero for 120 days.¹⁸ However, Figures 4, 5, and 6 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 Figure 2 that the introduction of the UVG-guarantee seems to have increased the total volume of program participation. Given that 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.

5.5 What is the relation between dose and response?

Sometimes the reason for the impact of a treatment being small is that the change in the economic environment from the treatment is small. Using the terminology of Imbens (2000), among others, the *response* to the treatment is weak due to a low treatment *dose*. As long as there is variation among units, we can explore the causal relation between them by regressing the response on the dose.

In our case, the treatment dose of a unit is the increase in the program participation rate in each municipality and age group. The response is the decrease in the *share of ES days* variable. Figure 7 shows the fitted line between the mean response and the mean dose of the four groups A-D: An increase in the program assignment within 120 days by one percentage point results in a 0.17 percentage point decrease in the net impact of the treatment, indicating a weak response to the treatment dose.

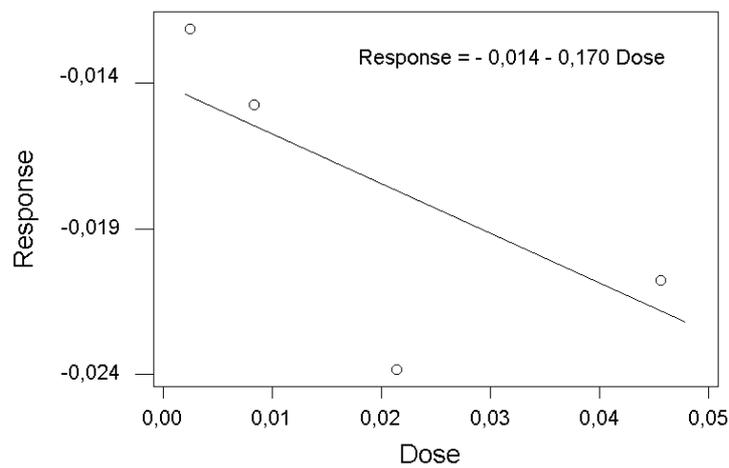
The standard errors in Figure 2 suggest, however, that we have a relatively large variation in the treatment dose among the municipalities. The same applies to the response measure, as reported in Figure 3. Presupposing that the municipality specific dose is exogenous, we can use this variation on the mu-

¹⁸ It should be noted that this impact is expressed in percent, whereas $\alpha^1 - \alpha^4$ in Figure 5 are expressed in percentage points. Furthermore, the distribution of spells ending on various days is not uniform, and thus, summing the impact in Figure 6 over the 120-day period produces the 2.2 percentage point impact estimated by α^3 .

municipality level to estimate a similar regression. Figure 8 shows a relation between the dose and the response comparable to Figure 7.

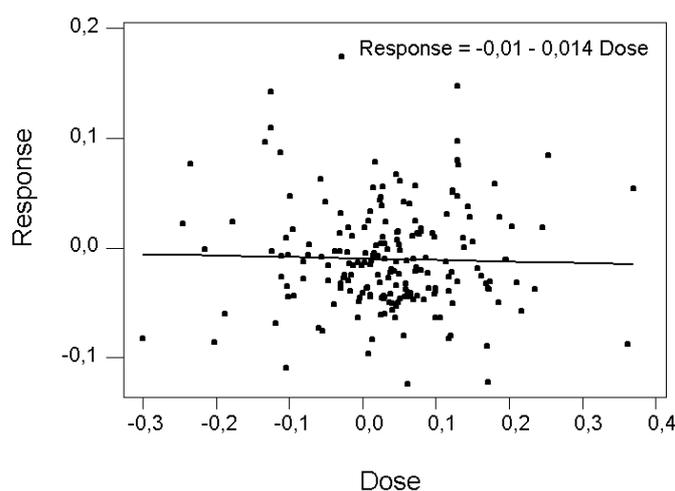
In sum, the results suggest that the small impact of treatment cannot be explained by a low treatment dose. Even in municipalities where we observe quite large increases in the program assignment rate, the response is still weak. Thus, shortening open unemployment does not seem to play any important role for the success on the labor market during the following 18 months.

Figure 7 Dose-response regression, four municipality groups



Note: The dose is defined as the change from 1997 to 1998 in the program assignment rate within 120 days; the response is defined as the change from 1997 to 1998 in the outcome variable *share of ES days*.

Figure 8 Dose-response regression, all UVG-providing municipalities



Note: See Figure 7.

5.6 Is the treatment effect common to all?

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, 2000).

The goal of the UVG-program – like the goal of most 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

¹⁹ A detailed description on how the variable is defined is found in the Data Appendix.

program, the weaker is her position on the labor market. Figure 9 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.²⁰

Figure 9 Mean of *share of ES days* (%) for the strongest (S) and the weakest (W) quartiles. Standard errors in parentheses.

		Flow 1997		Flow 1998	
		Not UVG	UVG	Not UVG	UVG
Age ≥ 25	S:	43.8 (0.77)	43.1 (0.25)	40.3 (0.80)	40.3 (0.25)
	W:	55.8 (0.65)	55.9 (0.25)	55.3 (0.63)	55.1 (0.23)
Age < 25	S:	41.4 (0.65)	40.2 (0.22)	39.0 (0.63)	37.6 (0.22)
	W:	57.4 (0.67)	55.1 (0.25)	55.8 (0.73)	54.4 (0.27)
		Strongest:		Weakest:	
		$\alpha^1 = -2.6$ (0.31)		$\alpha^1 = -0.7$ (0.37)	
		$\alpha^2 = -0.2$ (0.96)		$\alpha^2 = 0.9$ (1.06)	
		$\alpha^3 = 0.2$ (0.47)		$\alpha^3 = 0.1$ (0.50)	
		$\alpha^4 = -0.9$ (1.51)		$\alpha^4 = 1.2$ (1.28)	

²⁰ One may wonder whether previous unemployment adequately reflects the individual heterogeneity by which the UVG-effect varies. And more specifically, whether the absence of evidence of a heterogeneous effect is a consequence of this choice. We have therefore made a thorough investigation of this matter. We consider five measures of "strength on the labor market": previous income, unemployment duration, the time registered at the unemployment office, the caseworker's assessment of the need for job search assistance as well as the need for additional labor market training. These five variables are put into a measure model and a factor analysis is performed for the 1997-sample. The analysis suggests the presence of two factors that we label "actual strength" (driven by the first three variables) and "assessed strength" (driven by the last two variables). 13 additional variables are then used to predict the factor score for the 1997-sample through a regression model. The predictive variables relate to education, school-grades, family status, work experience as well as previous unemployment history. The regression model is thereafter used for predicting the individual factor-score for both the 1997 and 1998 samples, and to classify the individual's labor market strength. However, we find no evidence of a heterogeneous UVG-effect, and therefore, we decided to present the simpler analysis above.

5.7 Additional checks of the results

Information on whether and when a municipality provided the UVG-guarantee 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 UVG-guarantee some time during 1998 are included in group A. In some cases, however, the individual registered as unemployed before the municipality started providing the program, and thus, 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. Neither do the results change when we exclude all 49 municipalities in the County of Västra Götaland, since we consider the records for that County to be unreliable (See the Appendix).

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

Finally, we have tested different age restrictions, as well as excluded individuals non-eligible for unemployment insurance benefits. The results remain the same in both cases.

6 Conclusions

This paper is an evaluation of a youth measure called the UVG-program 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 program 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 the UVG-guarantee 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 UVG-guarantee.

Third, the UVG-program was everything but a guarantee: it implied an increase from around 25 to 30 percent in the probability of being assigned to some program within the promised 100-day period. However, although we would expect more from a guarantee, the increase is still significant, varying among municipalities. Exploiting this variation in the program assignment rate between the municipalities, we estimate dose-response functions, and find no significant correlation between the program assignment – the dose – and the outcome variable – the response. Thus, the negligible impact is not explained by a small dose.

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

Returning to the question raised 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, the UVG-program 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 UVG-program and other labor market programs. The UVG-program 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.

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 percent 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.

Finally, we believe that the identification strategy assures the internal validity of our results. The external validity of the results is a quite different question. For example, we cannot be sure that the impact would have been the same for significantly higher doses of the treatment, *i.e.* if the UVG-guarantee had been an *actual* guarantee of activity within 100 days, or for persons entering unemployment after 1998.

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Data Appendix

Data sources

Data for the empirical analysis is obtained from sources included in the IFAU database. The population of the IFAU database consists of the entire Swedish population from 1990 to 1998. The most important source for this study is *HÄNDEL* which originates from the public employment offices in Sweden and contains information on spells of unemployment, participation in labor market programs and some individual characteristics, including the municipality code.

For the identification strategy of this study, it is important to know which of the individuals were covered by the UVG-guarantee. Thus, two pieces of information are crucial: whether and when the individual's municipality started providing the UVG-guarantee, and the individual's exact date of birth. To protect individual anonymity, the IFAU database only contains information on the year of birth. We have given a special order to Statistics Sweden for the month of birth for the individuals in our sample.

Exact information on which municipalities have concluded an agreement on the UVG-program and when the first agreement was concluded are not collected into any document. The agreement, if there was any, was made between the municipal labor market authority and the local employment office. Our procedure was thus to gather information from the local level.

In 1998, there were 288 municipalities in Sweden. Our first step was to contact the 21 county labor boards governing the local employment offices by e-mail. 13 of these were able to provide more or less exact information for a total of 162 municipalities. As a second step, we then contacted either the municipal labor market authority or the local employment office (or both) in the remaining 126 municipalities. Lena Ståhl at the Ministry of Industry helped us by gathering the information for municipalities in Stockholm County. Our attempt to obtain information from the archives at the National Labor Market Board was unsuccessful.

The first e-mails were sent on November 11, 2001, and by February 15, 2002 we had received information for 256 municipalities. The remaining 32 municipalities are excluded from our study. These municipalities are:

Flen	Forshaga	Gagnef	Gnesta
Grums	Helsingborg	Höganäs	Kil
Kristinehamn	Lidingö	Ludvika	Lycksele

Malå	Mjölby	Norrtälje	Nyköping
Oxelösund	Tyresö	Täby	Sigtuna
Skurup	Sundbyberg	Svalöv	Svedala
Säffle	Tomelilla	Trelleborg	Vallentuna
Vaxholm	Vingåker	Östersund	Österåker

Moreover, we considered the records for Ödeshög, Ydre and Boxholm to be unreliable, and thus, these were excluded. Finally, information for the 49 municipalities in Västra Götaland County seemed uncertain (all municipalities in this area were claimed to have started on Jan 1, 1998) and thus we checked the robustness of the results when Västra Götaland is excluded. According to our records, 198 municipalities started providing the UVG-guarantee some time during 1998. Table A1 shows the distribution of months:

Table A1 Distribution of the starting months for the UVG-program during 1998

Month	No. of municipalities
January	118
February	15
March	25
April	23
May	6
June	7
July	2
August	1
September	1
October	0
November	1
December	0

Sample construction

From the *HÄNDEL* database, we collect the entire inflow during 1997 and 1998 of individuals born in 1967-78. As inflow in 1997, we define all individuals who enter the Employment Service register during 1997; the same applies for 1998. Thus, the samples for 1997 and 1998 overlap to some extent. We observe the entire *HÄNDEL* history for these individuals, and we can follow them until 22 June 2000.

Table A2 summarizes the sampling procedure. From the original sample of 586 653 individuals, we exclude observations with incorrect or missing information. *INSPER* and *SOKATPER* are tables in the *HÄNDEL* database. *INSPER*

contains information on the date of entry into and exit from the Employment Service register, whereas *SOKATPER* includes detailed information on the activities, or job search categories, in each registration spell. Examples of search categories (variable *SKAT*, *sökandekategori*) are open unemployment and participation in a program. Dates for the start and end of each *SKAT* are reported in *SOKATPER*.

Table A2 Sample construction

	No. of excluded obs.	sample size
All observations		586 653
incorrect year of birth ¹⁾	384	586 269
municipality code missing	25	586 224
	Inflow 1997	
		375 564
overlapping INSPER spells ²⁾	56 210	319 354
too old INSPER registration data ³⁾	166	319 188
overlapping SOKATPER spells ⁴⁾	26 013	293 175
information on whether the municipality provides UVG missing	26 325	266 850
22-27 years at registration with ES	133 126	133 724
	Inflow 1998	
		345 781
overlapping INSPER periods ²⁾	43 934	301 847
too old INSPER registration data ³⁾	162	301 685
overlapping SOKATPER periods ⁴⁾	22 907	278 778
information on whether the municipality provides UVG missing	24 243	254 535
22-27 years at registration with ES	119 568	134 967

¹⁾ Year of birth may be incorrect either within *INSPER*, such that an individual has a different year of birth for different registration periods, or between *INSPER* and *SYS9698*.

²⁾ Individuals with fully or partly overlapping periods, periods of one day only, an incorrect order of serial numbers, double serial numbers, a negative period length, or a registration date after June, 22, 2000 (censoring date) in *INSPER* are excluded. However, observations with overlapping periods before the year of inflow and the same starting date for both periods are included, collecting only the latest of the double periods.

³⁾ Age at the first registration must be at least 16 years, otherwise we assume the observation to be incorrect.

⁴⁾ Individuals with the following incorrect information are excluded: registration date in *INSPER* different from the registration into first search category in *SOKATPER*, de-registration date in *INSPER* different from the de-registration from the last search category.

Definition of some important variables

Unemployment and employment

All search categories (*SKAT*, *sökandekategori*) are included in our definition of an unemployment spell. The end of the unemployment spell is determined by the date and reason for de-registration (*AVDM*, *avaktualiseringsdatum*, and *AVORS*, *avaktualiseringsorsak*). If *AVORS* = (1, 2, 3), the spell is defined to end in employment.

Program participation

An individual is defined to participate in a program if her unemployment spell contains a search category *SKAT* = 42-83. *SKAT* = 66 stands for participation in the UVG-program.

Share of ES days

The longest possible period for which we can follow an individual who registers with the ES records on December 31, 1998 is until June 22, 2000, i.e. 539 days. Thus, the numerator of the outcome variable *share of ES days* is 539. The denominator is the sum of days registered with ES from the date of (first) registration.

History in the Employment Service register

The variable history in the ES register defines the number of days registered with the ES records since the first registration until the actual registration in 1997 or 1998. For comparability, this is expressed in relative terms as a share. In other words, the numerator is the sum of all unemployment spells (see the definition of an unemployment spell above) from the first registration until the actual registration. For individuals in the inflow 1997 (1998), the denominator is the sum of all calendar days from the first registration until December 31, 1997 (1998). Thus, the denominator is an approximation.

Table A3 Mean unemployment history in the four groups, 1997 and 1998.

Group	Share of unemployment days of all calendar days since the first unemployment spell, %. Standard deviation in parentheses.
1997	
A ⁰ : municipality provides UVG, Age < 25	35.7 (23.3)
B ⁰ : municipality does not provide UVG, Age < 25	37.9 (23.4)
C ⁰ : municipality provides UVG, Age ≥ 25	37.8 (24.7)
D ⁰ : municipality does not provide UVG, Age ≥ 25	40.6 (25.0)
1998	
A ¹ : municipality provides UVG, Age < 25	34.6 (22.8)
B ¹ : municipality does not provide UVG, Age < 25	36.2 (23.0)
C ¹ : municipality provides UVG, Age ≥ 25	39.9 (24.2)
D ¹ : municipality does not provide UVG, Age ≥ 25	42.7 (24.3)

Eligibility to unemployment benefits

Individuals are defined as eligible for UI benefits if their *KASNR* = 02-69. Thus, *KASNR* = 00, 98, 99 or missing indicates non-eligibility: 51 796 non-eligible individuals in the inflow 1997, and 42 841 non-eligible individuals in the inflow 1998.

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