Comment on Per Skedinger:
Employment Policies and Displacement
in the Youth Labour Market

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This is a balanced and thoughtful paper that deals with important issues and provides new empirical results. The paper falls into two distinct parts, one dealing with the effects of employment protection legislation and the other with displacement effects of labour market programmes. I discuss these two issues in turn.

1. Employment protection

It is not an easy task to evaluate how employment protection influences unemployment. Time-series investigations on data for a single country face all the usual problems with few degrees of freedom and little variation in the data. Sweden is a case in point here. We have some clear changes in legislation, in 1974 and 1982 (and for a brief period in the early 1990s). These data do not give us more than two dummy variables, a rather weak basis for empirics in a field where many other events have also occurred. In 1974 there was a major reform of the unemployment insurance system, with a doubling of the maximum benefit period as well as the introduction of a new form of "cash assistance" (*kontant arbets-marknadsstöd*), especially relevant for youth in the labour market. In addition, there has been a gradual increase in replacement ratios as well as increased coverage of unemployment insurance. Clearly, it is not easy to disentangle the separate effects of changes in employment protection legislation under such circumstances. The case for a *comparative* approach is

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strengthened in this situation; by looking at the experiences of several countries, we might hope to find more robust evidence. This is exactly what Skedinger does.

I have little disagreement with Skedinger's discussion of the implications of employment protection regulations for youth unemployment The cross-country comparisons are striking, although they ignore, of course, all other country-specific variables that may influence relative youth unemployment. The case made by the author is certainly strengthened by the fact that the country-specific time-series investigations confirm the conclusions from the figures. It would have been interesting to have the figures disaggregated by teenagers and young adults, in the same way as is done in the regressions. Are the cross-country regularities driven only by differences in teenage relative unemployment?

How sound, then, is the basic hypothesis in the analysis, namely that employment protection increases youth unemployment (or at least increases youth unemployment relative to adult unemployment)? The argument seems plausible, although one should bear in mind that the distinction between inflow and duration is relevant for youth as well as for adults. More liberal rules for fixed-term contracts are likely to make employers more inclined to hire young people, but there will also be more inflow into unemployment when the fixed-term contracts expire. Indeed, according to the labour force surveys, the expiration of fixed-term contracts accounts for a very large fraction (30–50 percent) of the unemployment inflow in Sweden. It seems plausible that the adverse duration effect of employment protection legislation will dominate the inflow component in the case of youth, but the argument does not seem waterproof.

I have briefly looked at some data on exit rates from unemployment in Sweden to examine whether the 1982 reforms had any discernible effects for youth relative to adult unemployment duration. (As discussed by Skedinger, the new legislation involved more liberal rules for temporary employment.) Weekly exit rates were calculated for "short-term" unemployed in the labour force surveys, i.e., individuals with less than six months' unemployment.[1] The data pertain to unemployment outflow, irrespective of whether the destination is employment or non-participation.

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[1] The Swedish labour force surveys are designed so that almost 90 percent of those interviewed in one survey are interviewed again three months (13 weeks) later. I used information on the number of unemployed in a particular quarter t in the duration class 1–13 weeks, $U_t^{1,13}$, as well as the number of unemployed in quarter $t+1$ (i.e., 13 weeks later) in

Figure 1. Exit rates from unemployment 1976–1993
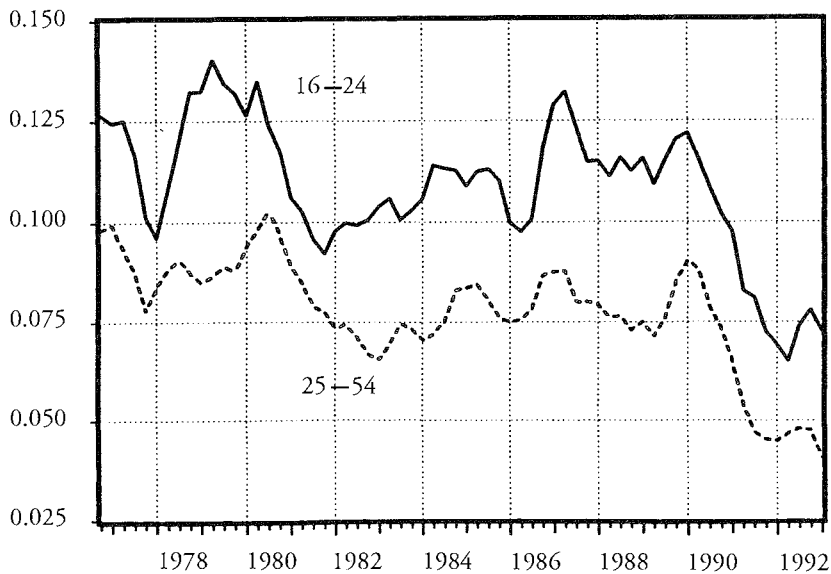
Note: The figure shows weekly exit rates by age, calculated from the labour force surveys as described in footnote 1, seasonally adjusted and smoothed by means of a three-period moving average.

The time series are displayed in Figure 1. Exit rates among youth (aged 16–24) are clearly higher than among adults (aged 25–54), which simply mirrors the well-known fact that young people have shorter spells of unemployment than adults. It is somewhat puzzling (and worrying) that the strong business upturn during the second half of the 1980s had only rather weak effects on the exit rates among adults. I ran a few regressions to look at these exit rates in somewhat more detail. I took the log of the exit rate (*ln e*) as dependent variable and the log of the aggregate vacancy rate (*ln v*) as an indicator for job availability. A trend and a 1982 dummy were also included. The results are displayed in Table 1.

the duration class 14–26 weeks, $U_{t+1}^{14,26}$. Weekly exit rates (*e*) were calculated from the equation

$$U_{t+1}^{14,26} = U_t^{1,13} (1-e)^{13}.$$

Björklund and Holmlund (1981, 1989) have used these kinds of data to examine the evolution of exit rates among short-term as well as long-term unemployed in Sweden.

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Table 1. Exit rates from unemployment in Sweden.
Dependent variable: *ln e*

	Age 16–24			Age 25–54			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constant	–2.29	–2.14	–2.04	–2.71	–2.38	–2.32	–2.32
	(74.83)	(29.04)	(22.79)	(79.61)	(33.24)	(26.23)	(21.14)
ln v	0.31	0.28	0.28	0.39	0.31	0.31	0.31
	(11.21)	(8.95)	(9.01)	(12.44)	(10.24)	(10.18)	(8.16)
Trend/100		–0.18	–0.39		–0.42	–0.54	–0.54
		(2.14)	(2.82)		(5.15)	(3.99)	(3.21)
D1982			0.10			0.06	0.06
			(1.87)			(1.07)	(0.99)
AR(1)	No	No	No	No	No	No	Yes
DW	1.68	1.76	1.85	1.08	1.46	1.48	1.85
$\bar{R}^2$	0.67	0.69	0.70	0.73	0.81	0.81	0.81

Notes: The sample period is 1976.2–1993.2 (1976.3–1993.2 in column (7)). *D1982* = 1 from the second quarter of 1982, zero elsewhere. *e* is the weekly exit rate from unemployment. The vacancy rate (*v*) is the total number of unfilled vacancies as a percentage of the labour force. Seasonal dummies are included. Absolute *t*-values in parentheses.

There is a trend decline in exit rates for both youth and adults, somewhat stronger for adults.[2] The period from 1982 and onwards is associated with a relatively more favourable development for youth; the dummy takes a significant positive value for youth but is insignificant for adults. These exercises certainly do not provide overwhelming evidence that the 1982 reforms reduced youth unemployment duration, but they are at least consistent with what one would expect and with Skedinger's basic arguments.

I also have a small remark on the analysis of employment protection. Skedinger suspects simultaneous equations bias, arguing that high unemployment may increase the demand for employment protection and hence lead to a positive bias in his coefficients. The opposite relationship, however, seems at least as plausible in light of the changes that took place in the 1980s. Fear of high unemployment, and high youth unemployment in particular, seems to have increased the support for more liberal rules. The bias would then be negative, in which case Skedinger's results

[2] If the trend is replaced by a measure of replacement ratios for workers covered by unemployment insurance, we find a negative and significant coefficient for the benefit variable for adults.

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would *understate* the rise in youth unemployment caused by employment protection legislation.

2. Job-creation programmes

The analysis of how job-creation measures affect youth employment and unemployment is complicated by the fact that such measures have exhibited a very marked counter-cyclical pattern. We observe a marked positive correlation between youth unemployment and labour market policy measures targeted at youth (and a negative correlation between youth regular employment and such measures). Does this negative correlation mean that programmes crowd out employment? Or does it capture a policy-response function according to which programmes quickly adjust to changes in the prevailing labour market situation? This is a tricky "chicken-or-egg" question, which has no obvious and simple solution. The fundamental identification problem is that it is difficult to find variables that are correlated with policies without being part of the employment equation.

Skedinger adopts a VAR approach, which may well be the best choice in a case like this. This approach does not eliminate the need for identifying assumptions, however. The impulse response functions do not make sense without assumptions concerning the residuals. Skedinger assumes that innovations to job-creation measures affect employment within the current quarter, whereas there is no within-period effect in the opposite direction. In other words, the labour market authorities do not react to changes in youth employment within the same quarter. This seems to be a reasonable assumption, at least more plausible than the alternative.

The results of the impulse response functions are striking. A positive innovation to job creation produces a reduction in regular employment of roughly the same order of magnitude during the first quarter. There is thus complete and immediate crowding out. The negative employment effects persist for more than one year. I find these very quick and strong crowding out effects somewhat surprising. To the extent that crowding out in part takes place through wage adjustments, one would expect longer lags. Are the results an artefact of some peculiar specification of the model?

Skedinger's result is driven by the negative correlation between the error terms in the unrestricted VAR equations ($R = -0.44$). This means that an innovation to job creation will have an immediate negative im-

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Table 2. Residual correlations with alternative specifications

	Unemployment rate			Unemployment rate excluded, layoff notific-ations exogenous
	Exogenous (Skedinger)	Endogenous	Excluded	
Correlation coefficient	–0.443	–0.166	–0.386	–0.304
Regression coefficient	–1.149 (4.45)	–0.418 (1.52)	–1.128 (3.77)	–0.738 (2.62)

Notes: The regression coefficients were obtained from regressions with the employment and job creation residuals from det VAR equations as dependent and independent variables (*t*-values in parentheses). The sample period is 1975.1–1991.4 in the last column, otherwise 1971.3–1991.4 (as in Skedinger).

pact on regular employment under the maintained identifying assumption. By regressing the residual from the employment equation in the VAR on the residual from the corresponding job-creation equation, we obtain a direct measure of crowding out during the first quarter (since the residuals are measured in the same units). Skedinger's specification implies a regression coefficient of –1.15, which is his measure of crowding out during the first quarter. I carried out a few experiments with Skedinger's (and other) data to check how robust his results are. The results are displayed in Table 2, with Skedinger's results in the first column.

One issue is the use of the aggregate unemployment rate as an exogenous variable in this case. Youth unemployment has accounted for a substantial fraction of total unemployment (30–40 percent), so it does not seem unproblematic to treat aggregate unemployment as exogenous. Table 2 gives information about the contemporaneous correlations and regression coefficients for three sets of alternative specifications. In the first case I excluded aggregate unemployment, in the second I included unemployment in the VAR as an endogenous variable, and in the third case I instead used an alternative business cycle indicator, advance notifications of layoffs. The latter indicator can probably be taken as exogenous with respect to youth employment. The sample period becomes shorter with this variable, however. The table reveals that the negative correlation between the error terms survives alternative specifications. In general, the correlation and regression coefficients are somewhat smaller compared to Skedinger's specification. The first-period crowding-out effect with layoff notifications as a cyclical variable is –0.74, still a substantial effect.

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All in all, the analysis indicates that job-creation programmes targeted at youth may produce substantial crowding out of regular youth employment. It remains to be seen whether these results survive more detailed investigations. Given the serious identification problems in this field, the most promising avenue for future research may well be to undertake down-to-earth case studies in order to learn about the mechanisms through which crowding out works in practice.

References

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