IFAU – INSTITUTE FOR LABOUR MARKET POLICY EVALUATION

Indirect effects of unemployment and low earnings: crime and children's school performance

Anna Nilsson

DISSERTATION SERIES 2005:1

Presented at the Department of Economics, Stockholm University

The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Industry, Employment and Communications, situated in Uppsala. IFAU's objective is to promote, support and carry out: evaluations of the effects of labour market policies, studies of the functioning of the labour market and evaluations of the labour market effects of measures within the educational system. Besides research, IFAU also works on: spreading knowledge about the activities of the institute through publications, seminars, courses, workshops and conferences; creating a library of Swedish evaluational studies; influencing the collection of data and making data easily available to researchers all over the country.

IFAU also provides funding for research projects within its areas of interest. There are two fixed dates for applications every year: April 1 and November 1. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The authority has a traditional board, consisting of a chairman, the Director-General and eight other members. The tasks of the board are, among other things, to make decisions about external grants and give its views on the activities at IFAU. A reference group including representatives for employers and employees as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P.O. Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

This doctoral dissertation was defended for the degree of Doctor in Philosophy at the Department of Economics, Stockholm University, May 23, 2005. The first two essays are revised versions of research previously published by IFAU as Working Paper 2003:14 and Working Paper 2004:6.

ISSN 1651-4149

Contents

Acknowledgements

First of all I would like to express my deepest gratitude to my supervisor, Jonas Agell, for tremendous support, interest and valuable advice. This thesis has also benefited greatly from discussions on empirical issues with Peter Skogman Thoursie and Per Pettersson Lidbom. Matz Dahlberg and Peter Fredriksson have also contributed by giving excellent advice. Further, I would like to thank IFAU, and especially Susanne Ackum, for giving me the opportunity of being part of an inspiring research environment. Financial support from IFAU is gratefully acknowledged as well as English improvements by Christina Lönnblad.

My friends at the department have made the days at the University enjoyable, special thanks to Maria, Carlos and Martina. I am also grateful to all my friends outside academia for often helping me keeping my mind off research. A special thanks to Hanna, for always understanding, giving advice and just being there.

Finally, the endless love and support from my family help me in everything that I do. I thank my parents, Birgitta and Per, my brother Erik and sister Karin with families, my grandmother Gerda and most importantly Michael.

Stockholm, April 15, 2005

Anna Nilsson

Introduction

Introduction

This thesis consists of three self-contained essays that consider indirect effects of unemployment and low earnings on crime and children's school performance. The first essay, *Crime, unemployment and labor market programs in turbulent times* (joint with Jonas Agell), investigates the effect of unemployment and participation in labor market programs, in general and among youth, on Swedish crime rates using a new panel data set for Swedish municipalities for the period 1996-2000. The exceptional variation in Swedish unemployment in the 1990s provides a remarkable (quasi-) experiment. Between 1996 and 2000 the overall unemployment rate (including those enrolled in labor market programs) decreased from 11.9 to 6.8 percent, and for those most likely to commit crimes, people under the age of 25, unemployment decreased from 21.2 to 8.7 percent. But the decrease in unemployment was far from uniform across the country, and our identification strategy is to use the exceptional variation in the improvement in labor market conditions across municipalities to isolate the relationship between unemployment and crime. We also consider whether placement in labor market programs reduce crime. Such an effect could arise for many reasons. Program participation may imply: (i) that there is less time for other activities, including crime; (ii) social interactions that prevent the participant from adopting the wrong kind of social norms; (iii) a greater ability to earn legal income in the labor market. Unlike most previous studies we identify a statistically and economically significant effect of general unemployment on the incidence of burglary, auto-theft and drug possession. Contrary to much popular wisdom, however, we could not establish a clear association between youth unemployment and the incidence of youthful crimes and there is no evidence that labor market programs – general ones and those targeted to the young – help to reduce crime.

The second essay, *Earnings and crime: The case of Sweden*, analyzes whether low earnings has an effect on Swedish crime rates, considering the overall crime rate and specific property crime categories, using a panel of county-level data for the period 1975–2000. Various measures of the income distribution are considered, based on annual labor earnings as well as annual disposable income. The results indicate that the effect of low earnings on crime in Sweden is at best weak. We estimate a significant effect of low earnings on the number of auto thefts, but the effect is small. Low earnings seem to have no effect on the overall crime rate, the number of burglaries or the robbery rate. The results give, however, further support for an unambiguous link between unemployment and the overall crime rate as well as specific property crime categories. These findings are in contrast with results from, for example, the United States where wages are found to have a stronger impact on crime than unemployment. The differing results could, at least partly, be explained by the fact that during the period investigated, Swedish unemployment has been of a more permanent nature than U.S. unemployment, and that transitory earnings fluctuations appear to dominate the Swedish earnings distribution for young men, a part of the population committing a disproportionate share of many crimes.

Finally, the third essay, *Parental unemployment and children's school performance,* considers another possible indirect effect of unemployment, namely the school performance of the children of the unemployed. I use Swedish data on individual GPA from the completion of primary school at age 16 and final grades from upper secondary school for a majority of all children completing primary school in 1990 directly moving on to three years of upper secondary school, which they complete in 1993. The empirical method builds on the idea that primary school GPA can be used to control for family and individual heterogeneity. The huge variation in Swedish unemployment during the beginning of the 1990s, which can be traced to macroeconomic events, provides an ideal setting for testing the hypothesis that parental unemployment affects children's school performance. The main results can be summarized as follows. If a mother is subjected to an unemployment spell during the period when one of her children attends upper secondary school, the school performance of the child marginally improves. This implies that, for women, the positive effect of having extra time on your hands exceeds the negative effects of the disadvantages caused by unemployment. This positive effect of having an unemployed mother seems to increase with the length of the unemployment spell. On the opposite, having a short-term unemployed father has a negative effect on a child's school performance while the effect is insignificant for long-term paternal unemployment. The fact that a long-term unemployment spell of the father has a less clear effect could be interpreted as the shock of unemployment wearing out. One explanation for the differing results across genders could be that women in general cope better with being unemployed and hence are able to use their new extra time doing something productive, such as spending quality time with their children.

Essay I

Crime, unemployment and labor market programs in turbulent times[#]

1 Introduction

Many commentators seem to take for granted that unemployment is an important determinant of crime, and that policies that are helpful in combating unemployment have a positive side-effect on criminal activity. However, the available empirical evidence suggests that the issues are less clear-cut.[1] In summarizing the literature, Freeman (1999, p. 3543) writes "…unemployment is related to crime, but if your prior was that the relation was overwhelming, you were wrong. Joblessness is not the overwhelming determinant of crime that many analysts and the public *a priori* expected it to be." Moreover, in discussing explanations behind the sharp increase in crime in the United States during the 1960s and 1970s, and the 1990s drop in crime, Freeman stresses

[#] **Co-authored with Jonas Agell.** We thank Matz Dahlberg, Peter Fredriksson, Oskar Nordström Skans, Henry Ohlsson, Per Pettersson Lidbom, Peter Skogman Thoursie, two anonymous referees and the Managing Editor Alan Krueger for helpful comments. We have also benefited from presentations at several departments and conferences, including the annual meetings of EALE and EEA, the International Microeconometrics conference in Dublin, and the CESifo Area workshop on Employment and Social Protection. This research was funded by a grant from the Institute for Labour Market Policy Evaluation (IFAU).

[1] The basic economic theory of crime suggests that the unemployed, and individuals with low wages, face strong incentives to commit (property) crimes. Following Becker (1968) and Ehrlich (1973), the economics of crime considers an individual, who bases his choice of whether to become a criminal on a comparison of the returns to legal and illegal activities. Since involuntary unemployment can be expected to reduce the return to working in the legal sector, there will be a substitution effect that induces people to commit more crime. There are extended economic models of crime where the link between unemployment and criminal activity is less clear-cut. When people can commit crime while working, unemployment may have a zero impact, see e.g. Grogger (1998).

variables like the earnings of less skilled workers and the sanctions imposed by the justice system, and there is no mentioning of unemployment.[2]

This paper uses a new panel data set for Swedish municipalities for the period 1996-2000 to explore how unemployment, in general and among youth, impacts on crime. We believe that this is a useful exercise for the following reasons. First, the exceptional variation in Swedish unemployment in the 1990s provides a remarkable (quasi-) experiment. Between 1996 and 2000 the overall unemployment rate (including those enrolled in labor market programs) decreased from 11.9 to 6.8 percent, and for those most likely to commit crimes, people under the age of 25, unemployment decreased from 21.2 to 8.7 percent. But the decrease in unemployment was far from uniform across the country, and our identification strategy is to use the exceptional variation in the improvement in labor market conditions across municipalities to isolate the relationship between unemployment and crime. Most previous studies have used data for countries and periods in which unemployment is fairly stable, or changes steadily over time. With such data it is not easy to separate the effect of unemployment from the effect of fixed effects and general time trends, and to avoid that omitted variables bias the result. In our data, the variation in unemployment is much larger than the variation in other covariates, which mitigate these problems.[3] Moreover, since the variation in unemployment can most probably be traced to macroeconomic events, which are exogenous to the municipality, bias due to reverse causation in the crime-unemployment dimension might be a lesser problem.[4]

Second, a large literature explores how labor market programs affect subsequent earnings; see e.g. Calmfors, Forslund and Hemström (2004). We focus on a different effect: does placement in labor market programs reduce crime? Such an effect could arise for many reasons. Program participation may

[2] Two recent panel studies conclude that unemployment may in fact have played an important role. Using U.S. state-level data Raphael and Winter-Ebmer (2001) report results indicating that a substantial portion of the decline in U.S. property crime rates during the 1990s is attributable to the decline in the unemployment rate. Using U.S. county-level data Gould, Weinberg and Mustard (2002) estimate that a one-percentage point increase in the state unemployment rate of non-college educated men increases reported burglary crime with 3.1 percent.

[3] In our regressions we reduce the risk of omitted variables bias even further by including a number of explanatory variables that might be correlated with unemployment, like e.g. age, immigrant status, income and education.

[4] Throughout the post-war period, until 1990, Swedish unemployment never exceeded 4 percent. Between 1991 and 1993, however, GDP fell by more than five percent, and there was a sudden quadrupling of unemployment. Including those enrolled in labor market programs unemployment increased from less than 4 percent to almost 13 percent of the work force. During the late 1990s there was a strong recovery, and it is crime data from this latter period that we use in the following (there is no municipality-level crime data available before 1996). For discussions of Swedish macroeconomic events of the 1990s, see Lindbeck (1997) and Thakur et al. (2003).

imply: (i) that there is less time for other activities, including crime; (ii) social interactions that prevent the participant from adopting the wrong kind of social norms; (iii) a greater ability to earn legal income in the labor market. To the best of our knowledge no other study has explored this issue.

Third, in view of the social and economic issues at stake, it is surprising that there is so little evidence on these issues for countries other than the United States. We believe that the Swedish experience is interesting in its own right, and that it is of interest to analyze whether the relationship between unemployment and crime is of a different nature in a welfare state, with a strong system of social transfers.[5]

We find that there is a statistically and economically significant correlation between unemployment and the incidence of burglary, auto-theft and drug possession. A calculation suggests that the sharp drop in unemployment during the late 1990s may have reduced burglary and auto-theft with 15 and 20 percent, respectively. These effects appear to be so large as to warrant the attention of policy-makers. But we find no evidence that labor market programs reduce crime, and there is no evidence that youth unemployment, and youth labor market programs, have an impact on crime.

The next section describes our data, and presents our empirical methodology. Section 3 reports our basic fixed effect regressions on how unemployment and labor market programs affect main crime categories. We also present results illustrating the difficulty of identifying the unemployment effect in an environment with normal business cycle fluctuations. Section 4 addresses some specification issues, and section 5 turns to the impact of youth unemployment and youth labor market programs.

[5] We are aware of three previous Swedish studies that analyze the link between unemployment and crime: le Grand (1986), Schuller (1986) and Edmark (2002). Le Grand uses aggregate time series data and finds a negative partial correlation between burglary and the vacancy rate. Schuller uses cross-sectional data for Swedish municipalities, and finds no significant correlations between crime and unemployment. Edmark (2003) finds that county unemployment is significantly correlated with property crime.

2 Data and empirical specification

Our panel data set includes 288 of Sweden's 289 municipalities (we exclude Nykvarn, which was formed only in 1999), and annual data for the 1996-2000 period. Beginning in 1996, the official crime statistics collected by *The National Council for Crime Prevention* contain a municipality-level breakdown of the total number of crimes reported to the police, as well as a detailed breakdown among different crime categories. Though we emphasize property crimes like auto-theft and burglary (i.e. crimes for which economic incentives may play a greater role) we also report results for violent crimes, like assault and robbery. Table 1 shows the descriptive statistics for our crime variables (Appendix 1 contains the exact definitions). For all crimes we express the annual incidence per 100,000 residents.

Table 1 Descriptive statistics, crime variables

Variables	Min	Max	Weighted mean	Number of zeros	Standard deviation	St. dev. net of fixed effects
All crimes	2115	24856	11965.6	0	3106.9	1059.8
Burglary	238	4008	1506.4	0	500.2	281.7
Theft	635	8108	3987.6	0	1185.4	397.6
Auto theft	0	1955	691.6	3	298.9	125.7
Assault	35	1594	607.2	0	193.8	83.1
Assault on unfam. man	0	599	221.0	19	87.4	40.4
Damage crime	168	5068	1228.2	0	396.7	211.4
Robbery	0	327	75.6	178	38.8	16.0
Possession of drugs	0	1202	275.9	58	146.7	93.4

Note: All crime categories are expressed as the annual incidence per 100,000 residents. Our complete panel consists of 1437 observations for 288 municipalities during the period 1996-2000. We have dropped one municipality, Nykvarn, which was formed in 1999. We have also dropped one outlier observation for "All crimes" for the municipality of Årjäng in 1996. The means are computed after weighing all observations by the area and time specific size of population. Number of zeros are the number of observations for which the crime category has zero reported crimes per 100,000 residents. Standard deviations net of fixed effects show the standard deviations that remain after eliminating all variation due to fixed municipality effects and common time effects.

The crimes that we focus on in the next section are the five broad categories shown in the upper part of the table. Clearly, property crimes like theft and auto-theft are far more common than violent crime in the form of assault. There is also a huge variation in the incidence of crime across municipalities: the overall incidence of crime in *Upplands Bro* in 1996 (24856 crimes per 100,000 residents) is almost twelve times larger than that in *Ydre* (2115 crimes per 100,000 residents). The lower part shows four crime categories, for which young offenders are known to be heavily over-represented, assault against unfamiliar male, damage crime, robbery and possession of drugs. The final column shows the standard deviation that remains after netting out all variation due to fixed municipality and time effects. Below, we will analyze whether this residual variation can be linked to the residual variation of local unemployment.

Poor data quality is an important problem for students of crime. The crimes that are recorded by the police can be expected to underestimate true criminal activity by a relatively large margin. If this under-coverage varies systematically over time there is cause for concern. For example, there is evidence that under-coverage has decreased for certain crime categories during the second half of the 1990s.[6] Since unemployment decreased substantially during the same period there is a risk that there will be a downward bias in the crime-unemployment effect computed from the official crime statistics. Still, our empirical approach mitigates this problem to a great extent. First, for auto theft and burglary (i.e. two of the crimes that we focus on in the next section) the extent of underreporting is most probably small and stable over time.[7] Second, our fixed effect specification eliminates the influence of measurement errors that (a) vary across municipalities but remain constant over time, and (b) changes in the same manner over time in all municipalities. Hence, our results will not be biased by changes in under-reporting that are common to all municipalities. Trends in under-coverage that are specific to the municipality may still bias our crime-unemployment effects, but only in so far as they are correlated with municipality-level trends in unemployment.

The starting point for our investigation is the following model:

$$Crime_{it} = \alpha_i + \lambda_t + \beta X_{it} + \theta(u_{it} + p_{it}) + \gamma \frac{p_{it}}{u_{it} + p_{it}} + \varepsilon_{it} \tag{1}$$

[6] This evidence largely relies on comparisons between the official crime statistics and victimization data from household crime surveys. *National Council for Crime Prevention* (2001) includes detailed discussions of the development of under-coverage for main crime categories. Domestic violence against children and sexual harassment are examples of crime where under-coverage appears to have decreased. A crime category for which under-coverage increased during the second half of the 1990s is drunk driving. During this period the police shifted to less systematic monitoring practices.

[7] See e.g. *National Council for Crime Prevention* (2001). The victims from auto theft and burglary have to report the crime to the police if they are to receive compensation from insurance companies.

Here, i and t are indices for municipality and time, $Crime_{it}$ is the log of the number of crimes of a particular category per 100,000 residents, α_i is a municipality fixed effect, λ_t is a year fixed effect, X_{it} is a vector of demographic and economic controls, and u_{it} and p_{it} are the shares of those openly unemployed and in labor market programs in the relevant demographic groups. The fixed effect terms eliminate variation in crime rates caused by factors varying across municipalities but constant over time, and vice versa. Since the time dummies remove all national trends, we identify the impact of unemployment and program participation on crime via the within-municipality deviations from aggregate trends. Our standard errors are robust to heteroscedasticity and consistent with respect to serial correlation within the municipality.[8]

The term $u_{it} + p_{it}$ is a measure of total unemployment, which groups together the idle unemployed as well as those who participate in programs. This specification, which assumes that participation in labor market programs is equivalent to being unemployed, rests on the observations that participants in Swedish labor market programs are drawn from the pool of unemployed people, and that a prime motive for allocating people to programs in the 1990s was to help them to secure unemployment benefits in the future; see Calmfors, Forslund and Hemström (2004) for further discussion. To check whether labor market programs have a separate effect from that of idle unemployment, we then add a variable describing how overall unemployment is divided between idle unemployment and program participation, the program take-up rate $p_{it}/(u_{it} + p_{it})$.[9]

Table 2 presents the descriptive statistics for our explanatory variables. For each municipality *The National Labor Market Board* provided us with (annual) information about the number of openly unemployed and the number of individuals enrolled in labor market programs, and *Statistics Sweden* provided

[8] We estimate (1) treating each municipality as an independent cluster. The Monte Carlo analysis of Kézdi (2002) shows that the finite-sample bias of the cluster estimator is smaller than the bias of the estimators that assume no serial correlation at any sample size. These simulations also reveal that the cluster estimator is unbiased in samples of usual size, and slightly biased downward if the cross-sectional sample is very small.

[9] As pointed out by one of the referees, the share of individuals in a given demographic group that participates in a labor market program, p_{it}, is probably a better measure of the pure incapacitation effect of programs than the program take-up rate, $p_{it}/(u_{it}+p_{it})$. We have re-estimated (1) using p_{it} instead of $p_{it}/(u_{it}+p_{it})$, and this does not change any substantive conclusions concerning the magnitudes of the key coefficients θ and γ in eq. (1). But they are estimated with much less precision in these alternative regressions. It appears that multi-collinearity is an issue. Thus, while the correlation between $p_{it}/(u_{it}+p_{it})$ and $u_{it}+p_{it}$ is 0.29, the correlation between p_{it} and $u_{it}+p_{it}$ is as high as 0.94.

us with complete municipality-level age distributions.[10] There is clearly considerable variation across municipalities in unemployment and program take-up rates, in particular for the younger cohorts. Average unemployment (computed after weighing by populations size) for those aged 18-24 is 13 percent, but the standard deviation is huge, and the min- and max values vary between 1 and 44.7 percent. On average 38.8 percent of unemployed aged 18-24 are placed in a labor market program, and the min- and max values vary between 8.8 and 70.5 percent.

Our remaining regressors include a range of economic and socioeconomic indicators. Some were included because they have been identified as significant determinants of crime, others because we judged it important to reduce the risk of omitted-variables bias by including as much information as possible about time-varying municipality-level heterogeneity. We include the age distribution of each municipality to account for the overrepresentation of the young in all crime statistics. For the same reason we also include the proportion of males and the proportion of residents not born in Sweden. Some studies indicate that low wages/low education have an effect on crime that operates in addition to unemployment, and for this reason we include municipality-level measures of schooling composition. The preceding literature has suggested several reasons[11] why per capita income might matter for the incidence of crime, and since average income is correlated with unemployment in the same location, we include average income among our regressors. This implies that the coefficient on the unemployment variable will be estimated net of the fact that income during unemployment is generally lower than during employment.

---

[10] Since there is no municipality-level data on labor force participation, we computed our unemployment rates by dividing the total number of unemployed by the size of the relevant demographic group.

[11] In areas with high incomes there can be expected to be a greater supply of theft-worthy goods, which should induce more property crime. Alternatively, more prosperous areas can be expected to devote larger resources to crime preventing activities, which should reduce property crime. Also, since the income elasticity of alcohol consumption can be expected to be positive, and since alcohol consumption has been shown to induce (violent) crime, including a measure of per capita income is a way of controlling for unobservable alcohol consumption. See Raphael and Winter-Ebmer (2001), and Gould, Weinberg and Mustard (2002) for further discussion.

Table 2 Descriptive statistics, control variables

Variables	Min	Max	Weighted mean	Standard deviation	St. dev. net of fixed effects
Proportion unemployed:					
aged 18-64	0.014	0.248	0.089	0.036	0.007
aged 18-24	0.010	0.447	0.130	0.072	0.017
aged 25-64	0.015	0.225	0.082	0.031	0.006
Program take-up rate:					
aged 18-64	0.139	0.693	0.336	0.066	0.032
aged 18-24	0.088	0.705	0.388	0.091	0.047
aged 25-64	0.136	0.693	0.322	0.064	0.034
Proportion not born in Sweden	0.018	0.376	0.109	0.046	0.003
Income per capita (in kronor)	71452	210474	106149	13942	1404
Age distribution:					
proportion aged 0-15	0.140	0.259	0.197	0.018	0.002
proportion aged 16-19	0.029	0.086	0.046	0.004	0.002
proportion aged 20-24	0.033	0.120	0.061	0.010	0.002
proportion aged 25-54	0.336	0.515	0.424	0.022	0.004
Proportion of men	0.476	0.527	0.494	0.008	0.001
Prop. with no high-school degree	0.105	0.431	0.267	0.052	0.004
Prop. with high-school degree	0.255	0.501	0.395	0.030	0.005

Note: For all control variables we have 1437 observations, covering 288 municipalities during the period 1996-2000. For further description of data and data sources, see text. The means are computed after weighing all observations by the area and time specific size of population. Standard deviations net of fixed effects show the standard deviations that remain after eliminating all variation due to fixed municipality effects and common time effects.

We do not include measures of detection risk and punishments among our regressors. Though this omission may bias our estimates of the crime-unemployment effect,[12] we believe that the bias is bound to be small. First, since it is likely that criminals' perceptions of detection risks and penalties change only gradually over time, and since our panel spans only five years, our fixed municipality effects should pick up most of the action from omitted deterrence variables. Second, our yearly time dummies eliminate the contaminating influence from changes in deterrence variables that are common to all municipalities. Finally, in section 4 we use an instrumental variables

[12] See Levitt (1997).

approach that (among other things) deals with the potential bias from omitted variables.

A comparison of the two final columns of Table 2 shows that most of our regressors have little independent variation, once we eliminate all variation due to general time trends and municipality fixed effects. For our age, gender and schooling variables the residual standard deviations fall in the interval .001-.004. For our variables of primary interest, overall unemployment and the program take-up rate for different age groups, the residual standard deviations are typically between 2 to 5 (overall unemployment) and 10 to 20 (programs) times as large. Compared to previous panel studies of the relationship between crime and unemployment we have unusually large independent variation in our labor market variables. Raphael and Winter-Ebmer (2001, table 1) report that the residual variation of their unemployment variable is of the same magnitude as the residual variation of other main regressors (black, poor and age structure). Since the standard error of the coefficient of a given independent variable decreases with the total sample variation this suggests that we can obtain comparatively precise estimates of the coefficients on our unemployment and program variables.

Figure 1 plots the change over the five-year period 1996-2000 in burglary per 100,000 residents against the reduction in overall unemployment across 285 municipalities. Two patterns stand out. First, there is indeed a huge variation across municipalities in the decrease in unemployment. Second, the plot is quite disperse, and it is not easy visually to detect a clear association between unemployment and the burglary rate. However, in a simple OLS-regression, where we weigh all observations by the size of population, the slope coefficient is significant at the five-percent level.

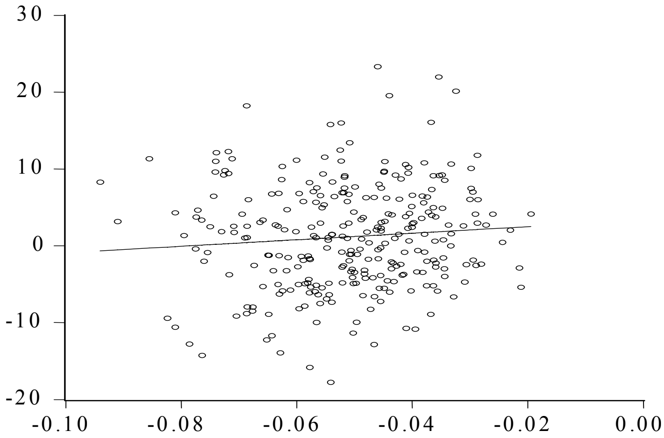

Figure 1 Annualized change in burglary (in %) on the vertical axis and percentage point change in total unemployment on the horizontal axis across 285 municipalities, 1996-2000. The burglary rate is measured as number of reported crimes per 100,000 residents. Our raw data includes 286 municipalities for which we have information about the change in crime and unemployment between 1996-2000. In constructing the figure we dropped one outlier, the municipality of Bengtsfors (for this municipality unemployment actually increased substantially between 1996-2000). The regression line comes from an OLS-regression, where the change in crime is regressed on a constant and the change in unemployment.

Our next task is to examine whether these associations survive more careful analysis, where we exploit the year-by-year variation in our data and bring in our full set of explanatory variables.

3 Our baseline specification

Table 3 presents our basic OLS estimates of the coefficients on overall unemployment and the program take-up rate in specification (1) for the five crime categories listed in the upper part of Table 1. All observations are weighted by the area and time specific size of population, and the estimated coefficients have the interpretation of semi-elasticities; they show the increase in percent of a given crime created by a one-percentage point increase in the rate of unemployment or the program take-up rate.

Table 3 The baseline specification

		Property crime			Violent crime
	All crime	Burglary	Theft	Auto-theft	Assault
Proportion unemployed	1.221*	2.838**	1.251	3.904**	1.270
aged 18-64	(0.680)	(1.261)	(0.831)	(1.909)	(1.061)
Program take-up rate	0.090	0.172	0.110	0.248	-0.033
aged 18-64	(0.151)	(0.257)	(0.195)	(0.319)	(0.220)
Observations	1436	1437	1437	1434	1437
Adjusted R-squared	0.945	0.811	0.943	0.89	0.894

Note: Standard errors are shown in parenthesis. They are robust to heteroscedasticity and consistent with respect to serial correlation within the municipality. In all regressions the dependent variable is the log of the crime rate per 100,000 residents. We loose one observation in column 1 (because of an apparent error in the coding of the raw data), and three observations in column 4 (auto theft) because of the censoring at zero. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects, and the time-varying variables shown in Table 2. ** and * denote significance at the five and ten percent level, respectively. All observations are weighted by the area and time specific size of population.

The coefficients have the expected signs, though not all of them are statistically significant. The coefficient on the unemployment variable is significant at the five-percent level in the equations for auto-theft and burglary. But the association between our overall crime index and unemployment is weak and only statistically significant at the ten percent level, and like some previous studies we find that unemployment has a statistically insignificant effect on the main category of violent crime, assault.

The coefficients in the burglary and auto-theft equations matter economically. A one-percentage point drop in unemployment causes (everything else held constant) reductions of 2.8 percent in the burglary rate, and 3.9 percent in the auto-theft rate. Since the mean unemployment rate decreased with 5.1 percentage points (from 11.9 to 6.8 percent) between 1996-2000, our coefficients predict a decrease of 14.5 percent for burglary and 19.9 percent for auto-theft.[13] These results are on par with, or even stronger, than those reported in two recent panel studies for the United States. Gould, Weinberg and Mustard (2002), who use county-level data, estimate that a one-

[13] In a number of auxiliary regressions we have checked whether unemployment has a different impact across different municipalities. More specifically, we added interaction effects between overall unemployment and the share of population with only elementary education, and between unemployment and the share of population not born in Sweden. We find no evidence that the unemployment effect differs between areas with different educational achievements, but there is some evidence that unemployment plays a greater role in areas with a high immigrant share. In the equation for theft, the interaction term between unemployment and immigrant share is positive, and significant at the five-percent level. Evaluated at an immigrant share of ten percent, this estimate returns a semi-elasticity of unemployment of .96. This interaction effect, however, was not estimated with a statistically significant coefficient in any of the other equations.

percentage point drop in unemployment reduces burglary and auto-theft 3.1 and .85 percent, respectively, while Raphael and Winter-Ebmer (2001), who use state-level data, estimate reductions of 2.1 and 1.0 percent for the same crimes.

In all columns the coefficient on the program take-up rate is close to zero, and statistically insignificant. But the measure of labor market programs that we try out in Table 3 captures program participation among all individuals of working age and it is possible that programs that are targeted towards youth have a more pronounced impact. Also, if there is reverse causation from crime to spending on programs there might be an upward bias in OLS estimates of the coefficient on the program variable. We return to these issues below.

It is the fact that our panel covers a period with extraordinary unemployment shocks that makes it suited for identifying an unemployment effect on crime. By the same argument we would also expect to obtain less precise results if we re-estimate our model over a period with less extreme fluctuations in unemployment. Since a municipality-level breakdown of crime statistics in Sweden is not available before 1996 we cannot directly evaluate this proposition. However, we can obtain some indications from an alternative data source, county-level crime statistics. Though this data set has the disadvantage of having a much smaller cross-sectional dimension (there are 21 counties in Sweden), it covers a period (1973-2000) containing both stable and unstable conditions in the labor market.

Table 4 presents our county-level results for our benchmark model for all crimes, burglary and auto-theft. Here, we have divided the county panel in two sub-samples, each covering a period of 14 years, 1973-86 and 1987-2000. In the earlier period Swedish unemployment fluctuated within the normal postwar band, and in our data the mean and (unadjusted) standard deviation of county unemployment are 0.029 and 0.012, respectively. During the latter time period, which includes the macroeconomic turbulence of the 1990s, the mean and standard deviation are 0.054 and 0.029, i.e. the standard deviation is almost 2.5 times higher in the unstable period. As can be seen in Table 4 our robust standard errors, calculated for samples containing less than one fifth of the observations underlying our municipality level regressions, are so large so as to render all the estimated coefficients statistically insignificant. However, except for the equations for all crimes the standard errors are much smaller in the unstable period, and the estimated semi-elasticities in the unstable period are close to those reported in the lower part of Table 3. In the stable period, by contrast, the semi-elasticities are close to zero, and estimated with a negative sign in the equations for all crimes and burglary.[14]

Table 4. County-level semi-elasticities estimated in two alternative macroeconomic environments

	All crime		Burglary		Auto theft	
	Stable period	Unstable period	Stable period	Unstable period	Stable period	Unstable period
Prop. unemployed	-0.304	1.424	-0.462	1.716	1.998	2.953
aged 18-64	(0.778)	(1.089)	(1.807)	(1.138)	(3.804)	(2.253)
Observations	294	294	294	294	294	294

Note: Standard errors are shown in parenthesis. They are robust to heteroscedasticity and consistent with respect to serial correlation within the county. In all regressions the dependent variable is the log of the crime rate per 100,000 residents. All regressions include a complete set of county and year effects, measures of demography, and a measure of mean earnings in the county. All observations are weighted by the area and time specific size of population. The stable period refers to 1973-86, the unstable period to 1987-2000.

4 Alternative specifications: crime spillovers and instrumental variables

Do the economically significant crime-unemployment relations remain as we estimate alternative models? A first issue concerns crime-spillovers. We have so far ignored all spatial interactions between municipalities. It appears likely, however, that criminal activities are correlated across adjacent municipalities – a criminal may choose to live in one community while committing crime in a neighboring community. For example, in their study of crime against foreigners in Germany, Krueger and Pischke (1997) find strong evidence of spatial correlation in anti-foreigner crime rates. A structurally oriented way of dealing with spatial spillover effects is to add covariates from neighboring municipalities to the estimating equation. Rather than allowing for spatial interactions via a transformation of the error term along the lines of e.g. Anselin (1988) – a procedure that has less obvious behavioral interpretations – we thus add new regressors to the estimating equation.

For each municipality we have constructed measures of unemployment and program take-up rates in surrounding municipalities, and then included these as

[14] Alternatively, a less generous social safety net may also explain why the unemployment effect appears to be stronger in the latter, more volatile period. But in fact, Swedish unemployment benefits were on average slightly more generous during 1987-2000 than during 1973-86. See OECD (1999), Figure 3.3.

additional regressors.[15] The results from these extended regressions are shown in Table 5, which should be compared to our benchmark results of Table 3. In the equation for burglary, the semi-elasticity for unemployment increases from 2.8 to 3.3, and it remains precisely estimated. The unemployment spillover coefficient is estimated with a non-intuitive negative sign, but its *t*-value is only .69. In the equation for auto-theft the semi-elasticity for unemployment drops by 30 percent, from 3.9 to 2.7, and the standard error increases marginally, which implies that the *t*-value falls from 2.05 to 1.31. At the same time the unemployment spillover coefficient is large (3.1), though imprecisely measured. An *F*-test shows that the two unemployment variables in the equation for auto-theft are jointly statistically significant (*p*-value = .070).

Table 5. Model with spillover effects from neighboring municipalities

	All crime	Burglary	Auto theft
Proportion unemployed aged 18-64	0.872	3.345**	2.735
	(0.923)	(1.389)	(2.088)
Program take-up rate aged 18-64	0.075	0.173	-0.063
	(0.168)	(0.266)	(0.356)
Proportion unemployed aged 18-64, neighbors	1.147	-1.892	3.128
	(1.735)	(2.732)	(2.777)
Program take-up rate aged 18-64, neighbors	0.011	0.107	1.233*
	(0.273)	(0 .486)	(0.671)
Observations	1431	1432	1429
Adjusted R-squared	0.945	0.810	0.891

Note: Standard errors are shown in parenthesis. They are robust to heteroscedasticity and consistent with respect to serial correlation within the municipality. In all regressions the dependent variable is the log of the crime rate per 100,000 residents. We loose one observation in column 1 (because of an apparent error in the coding of the raw data), and three observations in column 3 (auto theft) because of the censoring at zero. Moreover, we loose five observations in all columns because the island of Gotland has no neighboring municipalities. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects, and the time-varying variables shown in Table 2. ** and * denote significance at the five and ten percent level, respectively. All observations are weighted by the area and time specific size of population.

Another important specification issue follows from our assumption that unemployment is exogenous in the regressions reported in the previous section. Though our data from a particularly volatile period in the labor market appears

[15] Technically, we started off by constructing a 288×288 contiguity matrix Π, where the *ij* element is equal to 1 if municipalities *i* and *j* have a common border, and equal to zero otherwise. For each year, we then define two 288×1 vectors U and P containing the unemployment and program take-up rates in all municipalities. The products ΠU and ΠP then returns vectors where the *nth* element are the average unemployment and program take-up rates in municipalities sharing a common border with municipality *n*.

well suited to mitigate problems of endogeneity, we cannot rule out that omitted variables, simultaneity in the crime-unemployment relationship, and measurement errors bias our OLS estimates of how unemployment impacts on crime.[16] Depending on the relative importance of these potential confounding influences, the overall bias may go either way.[17] Similar arguments apply to our estimates of the impact of labor market programs; for example, to the extent that a local crime shock generates increased spending on programs the OLS results reported in the previous section will suffer from an upward bias.

We adopt an instrumental variables approach to address these issues. Our instrument set includes three instruments. Following Bartik (1991), Blanchard and Katz (1992) and Gould, Weinberg and Mustard (2002), the first two instruments interacts the initial sectoral composition of employment in each municipality with the national composition trends in employment (see Appendix 3 for the details). Our third instrument exploits the differential sensitivity of different municipalities to international trade and exchange rate shocks. Specifically, we interact the pre-treatment (1994) share of manufacturing employment in each municipality with a trade-weighted measure of the Swedish exchange rate against 21 other currencies. The bottom panel of Table 6, column (2), shows the strength of the first stage regressions. Our instruments do a good job in predicting unemployment, and the F-value for the joint statistical significance of the instruments is 26.59, with p-value=0.0000.[18] They do a worse job in predicting the program take-up rate; we return to this below.

---

[16] The treatment literature suggests an indirect way of probing the plausibility of the assumption of exogeneity. Consider the situation where a treatment variable (e.g. unemployment) has been found to have an effect on an outcome variable (e.g. burglary rate and auto-theft). As discussed by e.g. Angrist and Krueger (1999), we may then want to check whether the treatment variable has an impact in samples where the effect is known to be zero. A finding that the effect is in fact not zero would undermine our assumption that the treatment variable is exogenous. In this spirit, we have run regressions where we estimate the impact of unemployment on (twice) lagged crime rates. In the equations for all crimes, burglary and auto-theft (i.e. specifications where we identified a statistically significant treatment effect in Table 3) we obtain t-values for the unemployment variable of 0.56, 0.96 and 1.21, respectively.

[17] For more detailed discussions of endogeneity problems in the relationship between crime and unemployment, see Raphael and Winter-Ebmer (2001) and Gould, Weinberg and Mustard (2002).

[18] See also Table A4 in the appendix. Whether our instruments are truly exogenous determinants of unemployment – in the sense that they are uncorrelated with the error term in (1) – is a more difficult question, which cannot be tested directly. On *a priori* grounds, however, one may note that both the national employment trends and the trade-weighted exchange rate should be unaffected by the unemployment rate in any municipality, and it appears unlikely that unemployment has an impact on lagged industrial composition. Moreover, we have subjected our instruments to tests of refutability, along the lines discussed in footnote 16. We have thus estimated reduced form regressions, where we regress (twice) lagged crime rates against our

Columns (2), (4) and (6) in Table 6 present our TSLS estimates, along with the OLS estimates from Table 3. Like the OLS results, the TSLS regressions suggest that unemployment has a positive and statistically significant impact on all crimes and auto-theft. Furthermore, in all equations (including the one for burglary) the TSLS coefficients are larger, or much larger, than their OLS counterparts. But since the TSLS standard errors are between 60 and 100 percent larger than the OLS standard errors, the 95 percent confidence intervals become huge, and it is only in the equation for auto-theft where the OLS estimate falls outside the two-sided 95 percent confidence interval of the TSLS estimate. We view this as evidence that our OLS estimates of the previous section do not exaggerate the impact of unemployment on crime. In fact, our OLS estimates appear to constitute a *lower* bound on the effect of unemployment on all crimes, burglary and auto-theft.[19]

---

three instruments (and all our other explanatory variables, except our unemployment and program take-up variables). Simple F-tests for the joint statistical significance of our instruments produce p-values of 0.286, 0.308 and 0.710 in the equations for all crimes, burglary and auto-theft. In assessing the credibility of our TSLS results it is also important to test our over-identifying restrictions (we have more instruments than endogenous variables). We have regressed the TSLS residuals on our exogenous variables, and tested for the joint statistical significance of our instrument set. In these regressions, we failed to reject the null that our instruments are uncorrelated with the residuals.

[19] One may conjecture that errors in measuring unemployment – which would tend to create a downward bias in the OLS estimates – can go some way in explaining why the TSLS results exceed the OLS results. It should be recalled that the lack of labor force data at the municipality level created problems when we constructed our unemployment variable. For comparison, Raphael and Winter-Ebmer (2001) also report that their TSLS results generally exceed their OLS results.

Table 6. Instrumenting unemployment and program participation

	All crimes		Burglary		Auto theft	
	OLS	TSLS	OLS	TSLS	OLS	TSLS
	(1)	(2)	(3)	(4)	(5)	(6)
Prop. unemployed	1.221*	5.739**	2.838**	5.821	3.904**	18.76**
aged 18-64	(0.680)	(2.866)	(1.261)	(8.827)	(1.909)	(7.313)
Program take-up rate,	0.090	-0.163	0.172	-1.447	0.248	1.510
aged 18-64	(0.151)	(0.953)	(0.257)	(3.311)	(0.319)	(2.497)
Observations	1436	1411	1437	1412	1434	1409
F-value first stage regr.						
Prop. unemployed	--	26.59	--	See	--	See
		(0.000)		column		column
Program take-up rate	--	3.79	--	2	--	2
		(0.010)				

Note: Standard errors are shown in parenthesis. They are robust to heteroscedasticity and consistent with respect to serial correlation within the municipality. In all regressions the dependent variable is the log of the crime rate per 100,000 residents. All regressions include a complete set of municipality and year effects, and the time-varying variables shown in Table 2. ** and * denote significance at the five and ten percent level, respectively. All observations are weighted by the area and time specific size of population. I. The results of the OLS columns are those reported in Table 3.

The TSLS coefficient on the program take-up variable takes on the expected negative sign in columns (2) and (4). But since the *F*-value for joint statistical significance of our instruments in the first-stage program regression is only 3.79, we view these results with much suspicion. As discussed by e.g. Staiger and Stock (1997), TSLS estimates and standard errors are highly unreliable in situations when the first stage *F* statistic is less than ten. Some studies of wage setting in Sweden have instrumented labor market programs using various lags of program placement and idle unemployment.[20] While there are obvious drawbacks to using lags of potentially endogenous variables as instruments, we have nevertheless experimented with such instruments as well. As should be expected adding lagged unemployment and program take-up rates to our benchmark instrument set strengthens the first stage program regression. In general, these alternative instrument sets produced TSLS coefficients on the unemployment variable that were closer to – but still larger – than the OLS coefficients. The TSLS coefficients on the program take-up rate remained

[20] For a recent example, see e.g. Dahlberg and Forslund (1999). Dahlberg and Forslund argue that lagged unemployment and lagged program placement approximate the administrative decision rule of Swedish labor market authorities when they allocate resources across regions.

imprecisely estimated, with point estimates close to zero, and as often positive as negative.

The alternative specifications tried in this section suggest that our finding that unemployment is a potentially important determinant of main property crimes is robust. They also confirm the difficulty of finding reliable evidence that labor market programs reduce crime.

5 Youthful crimes and youth unemployment

Young people commit a disproportionate share of many crimes. According to the statistics from *The National Council for Crime Prevention* on suspected criminals in the year 2000, individuals aged 18-24 were over-represented as suspects for the following crime categories: assault against unfamiliar man 42 percent, robbery 37 percent, auto-theft and drug possession 32 percent, burglary 31 percent and damage crime 29 percent. If we broaden the age category to 15-24 (i.e. we also include the youngest culprits), the percentages increase to 69 percent (robbery), 60 percent (assault against unfamiliar man), 57 percent (auto-theft), 51 percent (damage crime), 49 percent (burglary) and 37 percent (drug possession).

This section analyzes whether unemployment among young people, and programs targeted towards the same group, have an effect on crime. A first look at the issues is provided by Figure 2 that plots the change in the robbery rate against the reduction in the overall unemployment rate for those aged 18-24. The scatter plot is again quite disperse. The OLS slope coefficient is positive, although only marginally significant (p-value = 0.07). Figure 2 also shows the exceptionally diverse development of youth unemployment during the late 1990s. Across all municipalities youth unemployment decreased with 12.5 percentage points between 1996-2000, but the decrease varies from 23.5 percentage points in the municipality of Överkalix to 1.8 percentage points in the municipality of Bengtsfors.

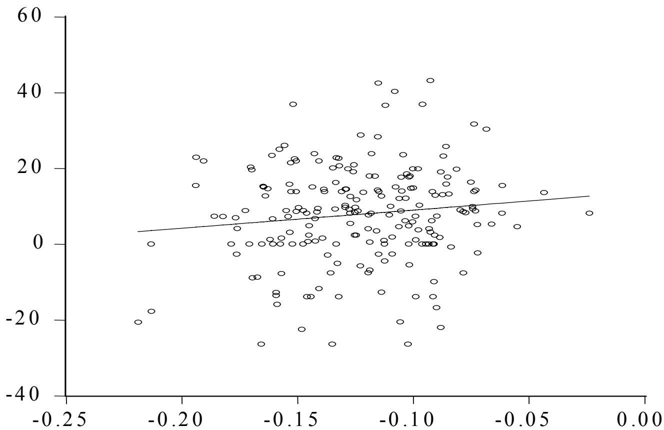

Figure 2 Annualized change in robbery (in %) on the vertical axis and percentage point change in youth unemployment on the horizontal axis across 204 municipalities, 1996-2000. The robbery rate is measured as number of reported crimes per 100,000 residents. Because of the logarithmic transformation of the robbery rate we dropped 82 municipalities with a zero robbery rate in constructing the figure. The regression line comes from an OLS-regression, where the change in crime is regressed on a constant and the change in unemployment, age 18-24.

Table 7 presents our basic fixed effect regressions for the six crime categories where young are the most over-represented in the official crime statistics. As before, our left-hand side variable is the log of the crime rate per 100,000 residents, we include our full set of time-varying explanatory variables and fixed time and municipality effects, and we weigh all observations by the area and time specific size of population. Also, we use four variables to characterize labor market outcomes, unemployment among those aged 18-24 and 25-64, respectively, and program placement in the same groups.[21]

[21] In constructing these variables we weighted labor market variables for the different age groups by their shares of the overall population aged 18-64. Thus the coefficients in Table 6 are not directly comparable to the semi-elasticities of previous tables. To achieve comparability the coefficients must be multiplied by the (average) population shares, which are 0.13 (age group 18-24) and 0.87 (age group 25-64).

Table 7. The baseline specification: youth crime

	Assault unfam. man	Robbery	Auto theft	Drug poss.	Burglary	Damage crime
Prop. unemployed	-9.497*	16.100	-6.492	-2.956	-0.729	-4.051
aged 18-24	(5.300)	(10.370)	(6.155)	(10.795)	(4.329)	(3.812)
Program take-up rate	-2.314	-3.060	-0.888	-2.214	-0.942	1.535
aged 18-24	(1.615)	(2.788)	(1.701)	(3.085)	(1.123)	(1.305)
Prop. unemployed	3.400	-0.372	6.76***	9.703**	3.790**	1.834
aged 25-64	(2.112)	(3.855)	(2.523)	(4.076)	(1.623)	(1.686)
Program take-up rate	-0.411	-0.010	0.456	0.175	0.351	-0.253
aged 25-64	(0.391)	(0.814)	(0.408)	(0.766)	(0.313)	(0.309)
Observations	1418	1159	1434	1379	1437	1437
Adjusted R-squared	0.855	0.880	0.891	0.777	0.812	0.833

Note: Standard errors are shown in parenthesis. They are robust to heteroscedasticity and consistent with respect to serial correlation within the municipality. In all regressions the dependent variable is the log of the crime rate per 100,000 residents. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects, and the time-varying variables shown in Table 2. ***, ** and * denote significance at the one, five and ten percent level, respectively. All observations are weighted by the area and time specific size of population.

To our surprise, we find no clear evidence that unemployment and program take-up among the young have an impact on crime. The coefficients on unemployment for those aged 18-24 are, with one exception, estimated with low precision. In the equation for assault on unfamiliar male (this violent crime category includes various forms of street violence, where young men are heavily over-represented both among victims and perpetrators) we estimate a *negative*[22], and marginally significant, coefficient on unemployment for those aged 18-24. The coefficient on the program take-up rate for those aged 18-24 is in most cases estimated with the predicted negative sign. But the point estimates are numerically small, with *t*-ratios at, or below, unity. At the same time, unemployment among the population at large is significantly correlated with main categories of youthful crimes. Unemployment for those aged 25-64 appears with positive and statistically significant coefficients in the equations for auto-theft and burglary. We also estimate a significant positive coefficient on this variable in the equation for drug possession. Multiplying the coefficient

[22] Similarly, Raphael and Winter-Ebmer (2001) and Gould, Weinberg and Mustard (2002) find that state- and county-level unemployment have a negative impact on some categories of violent crime. Raphael and Winter-Ebmer report evidence that this is due to a lower frequency of interactions between victims and perpetrators when unemployment is high.

of 9.7 with a population share of 0.87 produces (see footnote 21) a semi-elasticity of 8.4, which certainly is of a magnitude that matters economically.

We have estimated alternative models that instrumented our labor market variables along the lines discussed in the previous section. These regressions confirm the impression from the OLS results of Table 7 that unemployment among those aged 25-64 plays a role for burglary, auto-theft and drug possession. The results for unemployment and program take-up among those aged 18-24 were less informative – the relevant coefficients were very imprecisely estimated, and we could not find instruments that were at the same time strong and plausibly exogenous.[23]

A final unresolved issue derives from the fact that some youth crimes have an incidence of zero in many municipalities. Because of our logarithmic transformation these observations become missing values in Table 7. This implies that we lose close to 20 percent of the observations in the equation for robbery, and 4 percent of the observations for drug possession. To see whether this censoring matters for our results we estimate two alternative models. First, since the incidence of crimes per 100,000 residents is measured on a scale that only takes on non-negative integer values, our left-hand side variable is a count variable. Because of this we estimate a Poisson regression model, using our full sample. Second, we simply re-code all zeros to ones, before introducing the logarithmic transformation of our left-hand side variable, and then estimating our baseline fixed effect model. In either case, we are left with a full sample of 1437 observations. The results are shown in Table 8.[24] It does not appear that censoring is an important issue. Comparing with the results for robbery and drug possession in Table 7, the order of magnitude of the coefficients remains the same. Also, in both tables it is only in the equation for drug possession that we identify a statistically significant coefficient, the one on unemployment for those aged 25-64.

[23] Interestingly, while our labor demand shifters of the previous section (relating to aggregate trends in employment composition and exposure to trade and exchange rate volatility) do a good job in predicting first-stage unemployment among those aged 25-64, they are only weakly related to unemployment among those aged 18-24.

[24] Because of the logarithmic transformation used in the baseline model, the estimated coefficients in the Poisson model are comparable to those presented in Table 6.We do not report the standard errors in our Poisson regressions. These standard errors are defined by the conditional mean of the dependent variable, which is a poor assumption.

Table 8. Robbery and drug possession: dealing with censoring

	Robbery		Drug possession	
	Poisson regression model	Fixed effects regression on recoded data	Poisson regression model	Fixed effects regression on recoded data
Proportion unemployed aged 18-24	13.927	19.329 (12.859)	-0.344	-1.085 (11.220)
Program take-up rate aged 18-24	-5.008	-2.648 (3.899)	-7.035	-2.262 (3.262)
Proportion unemployed aged 25-64	3.322	2.078 (4.478)	5.605	10.149** (4.297)
Program take-up rate aged 25-64	-0.247	0.137 (0.915)	-0.450	0.142 (0.808)
Observations	1437	1437	1437	1437

Note: Standard errors are shown in parenthesis. They are robust to heteroscedasticity and consistent with respect to serial correlation within the municipality. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects, and the time-varying variables shown in Table 2. ** and * denote significance at the five and ten percent level, respectively. All observations are weighted by the area and time specific size of population.

Summing up, we find no strong evidence that youth unemployment, and labor market programs targeted to the young, have an impact on those crimes where young offenders are known to constitute a large share of the total. Thus, our study gives no support to those policymakers who argue that youth unemployment is an important factor in creating criminal environments. In view of our robust evidence that general unemployment has an impact on some broad crime categories we find these results puzzling. There are, however, possible explanations. The weak association between youth unemployment and youthful crimes could reflect that many of those involved in criminal activity in the youngest age cohorts still attend school. Among young men in the age-group 15-18, a group that is heavily over-represented when it comes to crimes like robbery, assault, and damage crimes, about 95 percent attend secondary or upper-secondary school (see Björklund, Edin, Fredriksson and Krueger (2004)). The absence of a clear negative impact of youth programs on youth crime could reflect that youth involved in criminal activity manage to opt out of the programs; i.e. there is a selection of non-criminal youth into programs.

6 Conclusions

Today, most economists would probably agree that microeconomic data sets containing information about the criminal record and other background variables of individuals offer a better way of identifying behavioral responses than aggregate data. Yet, because of the paucity of such microeconomic data most students of crime have resorted to the use of aggregate data, where identification is achieved from observing the regional correlation between crime and unemployment. This, however, is not an easy task. Though our use of panel data from a period with extraordinary shocks to unemployment can be expected to mitigate important problems due to e.g. omitted variables and reverse causation, we acknowledge that our results should be interpreted with caution.

Our findings can be summarized as follows. First, even in a welfare state where social insurance cushions a substantial part of the income loss from job displacement, a shock to general unemployment has a statistically and economically significant impact on main categories of property crime. This finding appears robust to alternative modeling assumptions. Second, and contrary to much popular wisdom, we could not establish a clear association between youth unemployment and the incidence of youthful crimes. Third, we found no evidence that labor market programs reduce crime.

A final observation is that prime-aged unemployment, measured by unemployment for those aged 25-64, *is* robustly correlated with main categories of youthful crimes, including drug possession. This finding is consistent with the idea, expressed in the sociological mobility literature, that unstable life conditions of parents can be expected to have adverse spillover effects on the life-choices of their children. Studying the link between the labor market opportunities of parents and the criminal record of their children seems like an important topic for the future.

References

Angrist, J D and A B Krueger (1999), Empirical strategies in labor economics in *Handbook of Labor Economics* vol. 3A (eds. Ashenfelter, O. C. and Card, D.), Elsevier, Amsterdam.

Anselin, L (1988), *Spatial econometrics: Methods and Models*, Kluwer Academic, Dordrecht.

Bartik, T J (1991), *Who benefits from state and local economic development policies?*, Kalamazoo: Upjohn Institute for Employment Research.

Becker, G (1968), Crime and punishment: an economic approach, *Journal of Political Economy* 76, 169-217.

Blanchard, O J and L F Katz (1992), Regional evolutions, *Brookings Papers on Economic Activity* (issue 1), 1-61.

Björklund, A, P-A Edin, P Fredriksson and A Krueger (2004), Education, equality, and efficiency – An analysis of Swedish school reforms during the 1990s, IFAU Report 2004:1.

Calmfors, L, A Forslund and M Hemström (2004), Does active labour market policy work? Lessons from the Swedish Experience, in *Labor Markets and Public Regulation* (eds. J Agell, M Keen and A Weichenrieder), MIT Press.

Dahlberg, M and A Forslund (1999), Direct displacement effects of labour market programmes: the case of Sweden, IFAU Working Paper 1999:7.

Edmark, K (2003), The Effects of Unemployment on Property Crime: Evidence from a Period of Unusually Large Swings in the Business Cycle, Working Paper 2003:14, Department of Economics, Uppsala University.

Ehrlich, I (1973), Participation in illegitimate activities: a theoretical and empirical investigation, *Journal of Political Economy* 81, 521-565.

Freeman, R (1996), Why do so many young American men commit crimes and what might we do about it?, *Journal of Economic Perspectives* 10, 25-42.

Freeman, R (1999), The economics of crime, in *Handbook of Labor Economics*, vol. 3C (eds. Ashenfelter, O C and Card, D), Elsevier, Amsterdam.

Gould, E, B Weinberg and D Mustard (2002), Crime rates and local labor market opportunities in the United States: 1979-1997, *Review of Economics and Statistics* 84, 45-61.

le Grand, C (1986), Kriminalitet och arbetsmarknad; inklusive en tidsserieanalys av inbrottsfrekvensen 1950-1977, Institutet för social forskning, Stockholms universitet.

Grogger, J (1998), Market wages and youth crime, *Journal of Labor Economics* 16*,* 756-791.

Kézdi, G (2002), Robust standard error estimation in fixed-effects panel models, mimeo, University of Michigan.

Krueger, A and J-S Pischke (1997), A statistical analysis of crime against foreigners in unified Germany, *Journal of Human Resources* 34, 182-209.

Levitt, S D (1997), Using electoral cycles in police hiring to estimate the effect of police on crime, *American Economic Review* 87, 270-290.

Lindbeck, A (1997), The Swedish experiment, *Journal of Economic Literature* 35, 1273-1319.

National Council for Crime Prevention (2001), *Brottsutvecklingen i Sverige 1998-2000,* BRÅ-rapport 2001:10, Stockholm.

OECD (1999), *Benefit systems and work incentives*, OECD, Paris.

Raphael, S and R Winter-Ebmer (2001), Identifying the effect of unemployment on crime, *Journal of Law and Economics* 41, 259-283.

Schuller, B-J (1986), Ekonomi och kriminalitet – en empirisk undersökning av brottsligheten i Sverige, Ph.D.-dissertation, Department of Economics, University of Gothenburg.

Staiger, D and J H Stock (1997), Instrumental variables regression with weak instruments, *Econometrica* 65, 557-586.

Thakur, S, M Keen, B Horváth and V Cerra (2003), *Sweden's welfare state. Can the bumblebee keep flying?* International Monetary Fund, Washington D.C.

Appendix

A.1 Definitions of variables

Table A1. Definitions of crime variables

Variables	Definitions
All crimes	All crimes reported in the municipality during the year.
Burglary	All burglary, not including firearms.
Theft	All thefts from vehicles, in public places, restaurants, shops, schools etc. Also including shoplifting and pickpocketing.
Auto theft	All car thefts, both attempted and completed.
Assault	All assaults, not with fatal ending, against children, women and men.
Assault against man, unfamiliar with the victim	Assault against male where the perpetrator is unfamiliar with the victim, both outdoors and indoors.
Damage crime	All damage crime, including graffiti.
Robbery	All robbery against the person.
Possession of drugs	Including possession of drugs and own usage.

Note: All variables are number of crimes reported to the police per 100,000 inhabitants.

Table A2. Definitions of control variables

Variables	Definitions
Proportion unemployed aged 18-64, 18-24 and 25-64.	Number of unemployed individuals out of total population in relevant age-group.
Proportion unemployed in labor market programs, aged 18-64, 18-24 and 25-64.	Number of individuals in labor market programs out of total number of unemployed individuals in relevant age-group.
Proportion not born in Sweden	Number of individuals not born in Sweden out of total population.
Income per capita (in kronor)	Taxable income per capita.
Age distribution	Proportion of individuals in different age-groups out of total population.
Proportion of men	Number of men out of total population.
Proportion with no high-school degree	Proportion of the population with at most nine years of schooling.
Proportion with high school degree	Proportion with between 10 and 12 years of schooling.

A.2 Results for the baseline specification

Table A3. Results for the control variables corresponding to the results in Table 3.

	All crime	Burglary	Theft	Auto-theft	Assault
Income per capita	-6.2e-06**	-3.4e-06	-6.7e-06**	-1.7e-05**	-8.4e-06***
(in kronor)	(3.0e-06)	(5.4e-06)	(3.4e-06)	(7.6e-06)	(3.3e-06)
Proportion not born	0.369	-4.067	-1.766	-4.763	3.742
in Sweden	(2.123)	(3.400)	(2.910)	(4.424)	(2.516)
Age distribution:					
prop. aged 0-15	-1.546	-2.975	1.692	6.714	-4.481
	(2.855)	(4.880)	(3.578)	(6.825)	(3.941)
prop. aged 16-19	-3.475	-7.119	-1.710	10.823	6.558
	(4.561)	(6.310)	(5.655)	(8.430)	(5.633)
prop. aged 20-24	-7.589***	-11.739**	-9.176***	-2.671	-5.335
	(2.947)	(5.564)	(3.646)	(7.503)	(3.964)
prop. aged 25-54	1.929	1.423	2.711	3.838	-2.009
	(1.362)	(2.196)	(1.736)	(2.930)	(2.075)
Prop. with no	1.859	-0.789	2.464	3.602	-2.555
high-school degree	(2.143)	(3.777)	(2.615)	(5.199)	(2.961)
Prop. with	4.548***	5.467**	6.926***	7.282*	-0.453
high-school degree	(1.299)	(2.718)	(1.655)	(4.152)	(1.765)
Proportion of men	-5.256	-9.791	-2.848	-2.330	-3.193
	(4.808)	(9.192)	(6.108)	(11.864)	(6.963)

Note: Robust standard errors are shown in parenthesis. In all regressions the dependent variable is the log of the crime rate per 100,000 residents. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects. ***, ** and * denote significance at the one, five and ten percent level, respectively. The weighted fixed effects model weighs all observations by the area and time specific size of population.

A.3 Descriptive statistics, alternative earnings measures

This section explains how we constructed the three instruments that constitute our benchmark instrument set. Our first two instruments were constructed by interacting initial employment composition at the municipality-level with the national trend in industrial growth to construct measures of the change in labor demand in different municipalities. Specifically, let the aggregate growth rate in industry *j* between time *t* and time *t-1* be

$$g_j = \frac{L_{j,t}}{L_{j,t-1}} - 1 \tag{A1}$$

where $L_{j,t}$ is number of employed workers in industry *j* at time *t* in the country.

Our first instrument for unemployment in municipality *i* will then be these national growth rates interacted with the municipality-specific composition of industrial employment, lagged one period:

$$Instrument_{1i} = \sum_j \left[\left(L_{i,j,t-1} \times g_j \right) + L_{i,j,t-1} \right] \tag{A2}$$

Our second instrument will be the corresponding interaction but with industrial composition of employment lagged 2 periods:

$$Instrument_{2i} = \sum_j \left[\left(L_{i,j,t-2} \times g_j \right) + L_{i,j,t-2} \right]. \tag{A3}$$

Our raw data is taken from the RAMS database of *Statistics Sweden*. This register-based data base includes information about all individuals who have their residence in Sweden, their work places, and the sectoral affiliation of the work place. In our application we construct our instruments for 288 municipalities, and we differentiate between industries at the two-digit level.

Our third instrument interacts a measure of a municipality's exposure to international trade with a trade-weighted exchange rate. Specifically, we use the RAMS database to compute the share of employed workers in a given municipality in year 1994 who were employed in industries producing manufacturing goods. Our third instrument then becomes:

$$Instrument_{3i} = Manufacturing_{i,1994} \times TCW_{t-1}, \qquad \text{(A4)}$$

where $Manufacturing_{i,1994}$ is the 1994 share of manufacturing employment in municipality *i*, and TCW_{t-1} is the lag of the *Total Competitiveness Weight Index* of the Swedish Riksbank. The *TCW*-index measures the Swedish exchange rate against 21 different countries, where the weights for the currencies are based on imports and exports of produced goods.

A.4 The strength of our instruments

Table A4. First stage regressions of municipality unemployment and program take-up rates on instruments

	Unemployment	Program take-up rate
National employment composition trend no 1	-2.70e-07***	5.33e-07***
	(3.98e-08)	(2.05e-07)
National employment composition trend no 2	1.05e-07***	-4.59e-07
	(2.57e-08)	(2.83e-07)
Trade exposure	1.33e-08*	1.42e-08
	(7.19e-09)	(3.40e-08)
F-statistics	26.59	3.79
(Prob > F)	(0.0000)	(0.0102)

Note: Robust standard errors are shown in parenthesis. In addition to the variables shown in the table, all regressions include the time-varying control variables of Table 2, as well as a complete set of municipality and year effects. ***, ** and * denote significance at the one, five and ten percent level, respectively. The table also reports the *F*-statistic, and the associated *p*-value, when testing the hypothesis that the coefficients on the three instruments are all zero in the first stage regression. All observations are weighted by the area and time specific size of population.

Essay II

Earnings and crime: The case of Sweden[#]

1 Introduction

Earlier research has shown that the unemployment rate affects some major crime categories, especially property crime, in Sweden.[25] Higher unemployment leads to e.g. more auto theft and burglary. There is also a large international literature indicating that low wages and income inequality affect crime rates. For reviews of this literature, see e.g. Bourguignon (2001), Eide (1999) and Freeman (1999). There is, however, no study of these relationships using Swedish data; hence, the aim of this paper is to investigate whether there is a separate effect of the income of low-skilled workers and income inequality on crime rates in Sweden.

Economic theories of crime explain variations in crime rates through the varying incentives and deterrents faced by individuals. Following Becker (1968), it is assumed that the individual chooses whether to engage in criminal activities by comparing the returns from those activities to the returns from working legally. Presumably, the expected returns to illegal activities to a great extent depend on the opportunities provided by potential victims and could therefore be seen as proportional to the average income in society. However, the cost of committing crime increases with the potential legal income of the criminal, through the opportunity cost of time. Hence, the cost of committing crime is low for workers with a low income and in the presence of considerable

I have benefited from helpful comments by Jonas Agell, Matz Dahlberg, Per-Anders Edin, Peter Fredriksson, Anna Larsson, Matthew Lindquist, Oskar Nordström Skans, Peter Skogman Thoursie and Per Pettersson Lidbom as well as from seminar participants at the Economic Council of Sweden, IFAU and Stockholm University. I would also like to thank Helge Bennmarker and Leif Petersson for providing the data. This research was funded by a grant from the Institute for Labour Market Policy Evaluation (IFAU).

[25] See Edmark (2003) and Nilsson and Agell (2005).

income inequality, the gap between the average income and the potential legal income of low-skilled workers will be large and give incentives for people at the bottom part of the income distribution to commit crime.

In one of the first empirical papers on the economics of crime, Ehrlich (1973), analyzed the variations in crime rates across U.S. states and found a strong positive correlation between income inequality, measured by the percentage of the population with an income below one half of the median income, and property crime. More recently, Freeman (1994, 1996) emphasizes the role of falling real earnings of the less educated in the high crime rate in the United States, Gould et al (2002) use U.S. county-level data and show that the decline in wages of unskilled men can explain more than 50 percent of the increase in both violent and property crime during the 1980s and 1990s while Machin and Meghir (2004) find an effect of wages of less skilled workers in the U.K. on property crime, using data on police force areas for the period 1975–1996.[26] In a cross-sectional study on U.S. counties, Kelly (2000) finds property crime to be significantly influenced by poverty[27] and Bourguignon et al (2003) argue that criminals in Colombia are to be found among people living in households where the income per capita is below 80 percent of the mean. The share of the population in that group and their mean income relative to the overall population appear to be main determinants of variations in the property crime rate. The literature thus indicates a link between the income of low-skilled workers, income inequality and crime. This paper studies whether there exists such a relationship in Sweden.

This paper uses a new panel dataset covering all Swedish counties during the period 1975–2000. I have access to individual-level income information, which allows me to construct various measures of the income distribution, considering income measures based on annual labor earnings as well as annual disposable income. I construct measures of the changes in the distribution of earnings and disposable income at the county-level from the individual-level register-based longitudinal dataset LINDA, which is a representative sample

[26] Moreover, Grogger (1998) uses individual level data and concludes that falling real wages played an important role in the increase of youth crime during the 1970s and 1980s in the U.S., and Imrohoroglu et al (2000) conclude that increased inequality has prevented a larger decline in crime in the U.S. Other papers studying the effect of income inequality on violent crime are Demombynes and Özler (2002), Entorf and Spengler (2000) and Fajnzylber et al (1998, 2002).

[27] Kelly (2000) measures poverty as the proportion of the population below the poverty rate, subtracting single mothers and people below 18 and above 65. Kelly (2000) argues that these groups should be excluded because of their limited means to resort to crime in response to their situation.

covering 3.4 percent of the population annually.[28] Figure 1 illustrates the evolution of median earnings and the 25th percentile of the earnings distribution (in 1980 SEK). Both measures evolved in a similar manner until the beginning of the 1990s, when the earnings at the 25th percentile decreased in comparison to median earnings. Since 1995, the development is once more parallel, although the distance between median earnings and the earnings at the 25th percentile is larger than before 1990. This study analyzes whether this relative deterioration in the earnings of less skilled workers has affected the overall crime rate, as well as specific property crime rates. I also consider several measures of earnings inequality, taking both the top and the bottom of the distribution into account. Measures of disposable income will be considered as a sensitivity analysis. In addition to the income measures, I control for various demographic and economic characteristics of the county and include county and time-fixed effects as well as county-specific time trends.

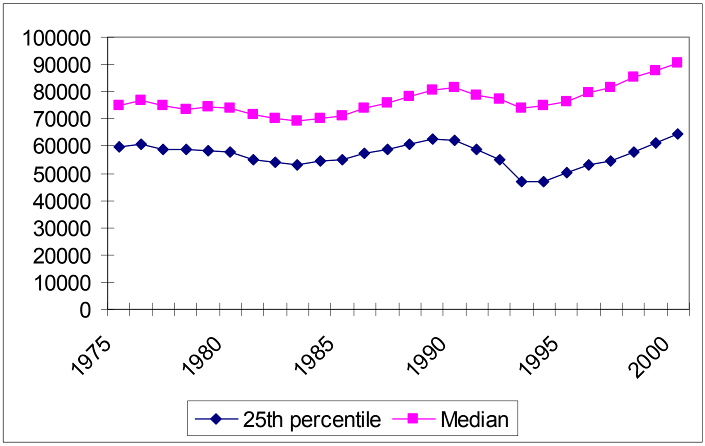

Figure 1 The percentiles of the earnings distribution are the national means of the corresponding percentiles in all counties each year. The measures are calculated using the individual earnings measure in LINDA for all men aged 25–64.

My results indicate that the effect of low earnings on crime in Sweden is, at best, weak. The low earnings measures have no effect on the overall crime rate, the number of burglaries or the robbery rate. I do, however, estimate a small, but significant, effect of low earnings on the number of auto thefts. The results imply a 0.1-percent decrease in the number of auto thefts caused by a ten-

[28] The data set includes approximately 300,000 individuals each year. For details about LINDA, see Edin and Fredriksson (2000).

percent increase in the earnings at the 25th percentile. Further, there is an unambiguous link between unemployment and property crime. According to my results, a one-percentage point drop in the unemployment rate would decrease the overall crime rate, the number of auto thefts and the robbery rate by approximately 1.2, 6.3 and 4.2 percent, respectively. These findings are in contrast with the results from, for example, the United States where wages are found to have a stronger impact on crime than unemployment. The differing results could, at least partly, be explained by the fact that during the period investigated, Swedish unemployment has been, of a more permanent nature than U.S. unemployment, and that transitory earnings fluctuations appear to dominate the Swedish earnings distribution for young men, a part of the population committing a disproportionate share of many crimes.

The next section presents the empirical specifications that will be estimated in the paper, some of which are inspired by the existing literature, while others are more modified to the Swedish environment. Section 3 describes the data and section 4 presents the results and addresses specification issues. Section 5 discusses possible explanations for the weak effect of low earnings on Swedish crime rates, and the final section concludes.

2 Empirical specifications

In the economic theory of crime, an individual chooses between legal and illegal activities by comparing the returns to these activities within the framework of the economic theory of choice under uncertainty. It is not necessarily a choice between two mutually exclusive activities, but a choice of determining the optimal allocation of time between competing legal and illegal activities.[29]

The expected returns to illegal activities depend on the opportunities provided by potential victims of crime. If criminals were unable to choose their targets, the expected gain from crime would be proportional to the average income in society. However, the cost of devoting time to illegal activities depends on the opportunity cost of time. The opportunity cost is the returns to legal activities, which is a function of the individual's ability, education and other legitimate training. In a society with considerable income inequality, the gap between average income and the potential legal income of low-skilled workers will be large and hence, give incentives for people at the bottom part of the income distribution to commit crime.

[29] See e.g. Grogger (1998) for a theoretical model allowing an individual to work in the legal labor market and commit crime during the same period.

Many studies have investigated the effect of income inequality on crime, the majority of these using data from the U.S., but there are also studies on the effect of income inequality on crime in, for example, the U.K. and Colombia. As a starting point, I consider the empirical specification of Machin and Meghir (2004) and then extend the analysis to investigate specifications more modified to the Swedish environment.

Machin and Meghir (2004) study the effect of changes in wages at the bottom end of the wage distribution on different property crime rates, using data on the police force areas of England and Wales between 1975 and 1996. In their benchmark fixed-effects specification, Machin and Meghir (2004) let the dependent variable be the log odds ratio of a certain crime category, a result from their structural model, which aims at modeling the aggregate probability of engaging in criminal activities.[30] The log of the wage at the 25th percentile of the wage distribution in the specific area is included as an explanatory variable, together with the share of the population aged 15-24 and the log of the conviction rate. The share of the population aged 15-24 is included because of the over-representation of young people in crime statistics and because this share could be negatively correlated with the wage measure. As an extension, they also include a measure of the net return to crime, measured from a victimization study, where crime victims had been asked to report the value of the stolen property.

The first specification to be estimated will hence be inspired by the specification of Machin and Meghir (2004). However, in contrast to their study, I will estimate a semi-logarithmic specification, for ease of comparison with previous studies of determinants of Swedish crime rates, where the coefficients have the interpretation of semi-elasticities (i.e. they show the percentage change in the crime rate due to a unit change in any of the explanatory variables). The starting point for the analysis is thus the following model:

$$\ln(Crime_{it}) = \alpha_i + \lambda_t + \beta_1 25th_{it} + \beta_2 YouthM_{it} + \beta_3 Cl_{it} \qquad (1)$$

where $Crime_{it}$ is the number of crimes reported to the police per 100,000 residents in the county of the crime category investigated, while α_i and λ_t are

[30] The model considers value-functions of four combinations of working and engaging in crime. Assuming the unobservables to be distributed as extreme values, the probability of each option is logistic. Taking the log odds ratio, which is approximated by a linear function, helps aggregating at the regional level.

county- and year-fixed effects.[31] $25th_{it}$ is the earnings at the 25th percentile, while $YouthM_{it}$ is the proportion of men aged 15-24[32] in the county and Cl_{it} is the clear-up rate for the specific crime category. Further, it should be noted that throughout the paper, all variables are weighted by the county- and time specific population and the standard errors are robust to heteroscedasticity.[33]

I lack data on conviction rates, which was the deterrence variable in the specification of Machin and Meghir (2004); hence, I instead include the clear-up rate of each crime category.[34] Examining the effect of clear-up rates on crime should be done with caution, since there is a potential problem of reverse causation. For example, if the number of police officers is kept constant in a county where crime is rising, the increase in crime is likely to cause a reduction in the clear-up rate. This is to say that there will be a causal and negative effect of crime on the clear-up rate, causing a downward bias in the coefficient on the clear-up rate.

To more extensively control for county-level demographic and economic factors, I will, as a first extension of this benchmark specification, include the proportion of foreign citizens, controlling for their over-representation in the crime statistics.[35] Moreover, from previous research it is known that the unemployment rate has an effect on property crime in Sweden.[36] The unemployment rate is not included in the benchmark model of Machin and Meghir (2004), with the explanation that wages explain both the return to work

[31] Besides controlling for time-invariant heterogeneity, the fixed effects to some extent help in addressing potential problems of measurement error. For example, the number of recorded crimes most likely underestimates true criminal activity. If this measurement error varies systematically across counties and time, my results could be biased. The empirical specification with fixed effects helps reduce this problem by eliminating the influence of measurement errors that (a) remains constant over time and (b) varies in accordance with a general time-trend. See Nilsson and Agell (2005) for a more detailed discussion on measurement errors in Swedish crime data.

[32] Machin and Meghir (2004) include a measure of the proportion of both men and women aged 15-24, but it is actually the men in that age group who are over-represented in the crime statistics.

[33] I have chosen not to use the cluster-estimator in the benchmark specification, since it is known to have good properties only when the number of groups is large relative to the number of clusters. For a discussion of inference problems in the presence of group effects when the number of groups is small, see e.g. Wooldridge (2002, 2003).

[34] The clear-up rate is measured as the percentage of all reported crimes (in one specific category in one county) that are solved in the same year that they are reported. The conviction rate used by Machin and Meghir (2004) is defined as the number of convictions divided by the total number of reported crimes against the property.

[35] Preferably, I would include the proportion of individuals not born in Sweden, but I do not have any data on this before 1984. However, the two variables are highly correlated.

[36] See Nilsson and Agell (2005) for an extensive analysis of the effect of unemployment on Swedish crime rates.

and the employment decision. However, this is only true if there are no factors, such as minimum or efficiency wages, creating involuntary unemployment. In Sweden, there are no statutory minimum wages, but the labor market is characterized by strong unions and labor market regulations, making it unreasonable to assume no involuntary unemployment. The effect of unemployment on crime is probably both a "lack-of-activity" effect and an income effect. Unfortunately, I cannot separate the two effects, potentially causing the unemployment coefficient to capture part of the earnings effect. The inclusion of the unemployment rate in the analysis could therefore lead to under-estimated coefficients on the earnings measure.

It also seems reasonable to take the return to crime into consideration. This is done by Machin and Meghir (2004) by including a measure derived from a victimization study where crime victims had been asked to report the value of the stolen property. Unfortunately, I have no access to such data. However, theory predicts that the returns to illegal activities could be seen as proportional to average income in society.[37] An increase in overall earnings should imply more theft-worthy goods, and most likely a higher return to property crime.[38] Hence, in addition to the earnings at the 25th percentile, I will include median earnings in the specification. I choose to include median earnings instead of mean earnings since it is less correlated with earnings at the 25th percentile, thus diminishing a potential problem of multi-collinearity.

Finally, in the most far-reaching specification, I will more extensively take advantage of the panel structure of the data and allow for county-specific time trends. Assuming the unobserved covariates to be constant within a county for 26 years, or that they are changing over time in accordance with a national time trend, is restrictive. Including county-specific trends relaxes the restriction and allows the trend to vary across counties, eliminating the within-county variation caused by factors that are county specific over time. For a more detailed discussion on the issue, see Friedberg (1998) and Raphael and Winter-Ebmer (2001).

The most complete model estimated will thus be:

[37] The argument concerning the returns from illegal activities follows Ehrlich (1973).

[38] In addition to reflecting the returns from illegal activities, the literature also indicates other reasons for why average income might be of importance for the incidence of property crime. For example, more prosperous areas might devote larger resources to crime preventing activities, potentially decreasing property crime. Average income has also been suggested to capture an effect of alcohol consumption on crime, given that the income elasticity of alcohol consumption is positive. For a more detailed discussion, see e.g. Raphael and Winter-Ebmer (2001).

$$\ln(Crime_{it}) = \alpha_i + \lambda_t + \gamma_i time_t +$$
$$\beta_1 25th_{it} + \beta_2 Median_{it} + \beta_3 U_{it} + \beta_4 Cl_{it} + \beta_4 YouthM_{it} + \beta_5 FC_{it} + \varepsilon_{it} \quad (2)$$

where γ_i are the county-specific coefficients on a linear time trend, $Median_{it}$ is county median earnings, U_{it} is the county unemployment rate, and FC_{it} is the proportion of foreign citizens.

An important specification issue is the possibility of a correlation between the residual in (2) and my earnings variables. Such endogeneity problems can arise for different reasons. First, if my earnings variables are measured with error, there will be a bias in the fixed-effect regression. Second, if there are omitted variables correlated with my measures of earnings, there will be a bias in my estimates of the earnings coefficient. Third, if earnings are, to some extent, jointly determined with my crime variables, my estimates will be contaminated by simultaneity bias.

Considering potential measurement errors in my earnings variables, the earnings measure is based on filed tax reports, which can be considered to be of good quality. Concerning omitted variable bias, I believe my fixed-effects specification with county-specific trends to do a good job in reducing the potential influence of omitted variables. Below, I will also report results from an indirect approach of assessing the influence of omitted variables. The risk of endogeneity bias stemming from a reverse causality between the earnings distribution and crime deserves some consideration, however. There is a possibility that an increasing number of crimes in an area induce an outflow of firms and high-income individuals. This would imply a causal link from crime to county earnings leading to biased estimates. However, this simultaneity bias is most likely a problem at the municipality rather than at the county level, and there is no evidence of crime-induced migration between counties in Sweden. Widerstedt (1998) studies the determinants of moving to a new county in Sweden. Although she does not directly test for whether higher crime rates lead to an outflow of high-income individuals, her evidence suggests that the higher the income, the lower is the probability of moving to a new county. Hence, I deem there to be reasons to believe that reverse causation is – for my purpose – a second-order issue.[39]

[39] Another specification issue that must be considered when studying the determinants of crime is related to "crime-spillovers", see Nilsson and Agell (2005). The issue of crime-spillovers must, however, also be considered as more significant at the municipality rather than at the county level, since counties constitute a much larger geographical area. While there are 289 municipalities in Sweden, there are only 21 counties.

3 The data

The panel data set includes annual data from 21 Swedish counties for the period 1975–2000.[40] The crime data were provided by *The National Council for Crime Prevention*. I focus on the effect of the earnings distribution on the overall crime rate and three major property crime-categories: burglary, auto theft and robbery (although robbery must be considered as a combination of property and violent crime). Table 1 shows the descriptive statistics for the crime variables. The county of *Stockholm* accounts for all the maximum-values of the crime variables, whereas the counties of *Gotland*, *Blekinge* and *Jämtland* share the minimum-values. The final column shows the standard deviation that remains after netting out all variation due to fixed county and time effects.

Table 1 Descriptive statistics, crime variables

Variables	Min	Max	Mean	Standard deviation	Standard deviation net of FE
All crimes	5400.2	21600.8	10693.9	2787.0	819.3
Burglary	687.1	3383.4	1398.0	418.3	191.6
Auto theft	119.9	1926.3	442.0	241.5	104.4
Robbery	1.8	210.7	31.5	30.8	10.4

Note: All crime categories are expressed as the annual incidence per 100,000 residents. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. The variables are more carefully explained in Appendix 2.

Figures 2–3 show the evolution of the different crime categories since 1975. All crime rates have increased, except the burglary rate, which has been volatile from year-to-year, but shows no discernible trend.

[40] During this period, there have been changes in the county-structure, in 1997 *Kristianstad* and *Malmöhus* county jointly became *Skåne* county and in 1998, *Göteborg- & Bohus-county, Älvsborg* and *Skaraborg* were merged into *Västra Götaland* county. I use the latter classification throughout the whole period, leaving me with 21 counties.

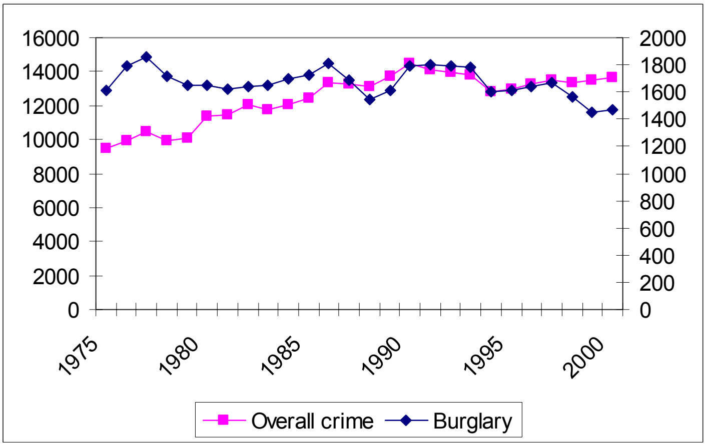

Figure 2 The total number of crimes per 100,000 residents reported to the police is on the left axis, and the number of burglaries per 100,000 residents on the right.

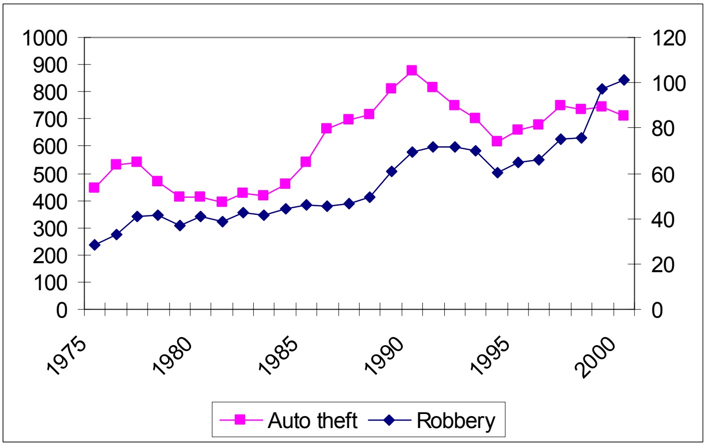

Figure 3 The number of auto thefts per 100,000 residents reported to the police is on the left axis, and the number of robberies per 100,000 residents on the right.

The data used to construct various measures of the earnings distribution are taken from the register-based longitudinal data set LINDA; see Appendix 1 for more information. The data set builds on a representative sample of the population starting in 1960 covering 3.4 percent of the population annually, which implies approximately 300,000 individuals. I focus on reported earnings of the male population of working age 25–64, giving a sample of about 75,000 individuals per year. The reason for only using the earnings of males is that during the relevant period, there was a large shift in female labor market participation. Including the earnings of females would thus give an inconsistent measure of the evolution of earnings over time.

Table 2 Descriptive statistics, control variables

Variables	Min	Max	Mean	Standard deviation	Standard deviation net of FE
25th earnings percentile	32915.3	72218.6	56786.7	6764.0	2020.0
Median earnings	69381.6	90363.1	76784.3	5203.1	1177.3
Unemployment	0.008	0.128	0.043	0.026	0.006
Proportion of men aged 15–24	0.055	0.080	0.068	0.005	0.002
Proportion of foreign citizens	0.006	0.100	0.039	0.020	0.004
Clear-up rates:					
All crimes	0.11	0.35	0.210	0.038	0.023
Burglary	0.02	0.7	0.104	0.063	0.048
Auto theft	0.04	0.49	0.194	0.090	0.035
Robbery	0.06	0.82	0.287	0.111	0.089

Note: All earnings variables are computed from the individual annual earnings measure included in LINDA and measured as annual earnings in 1980 SEK. The clear-up rates are measured as the percentage of all reported crimes (in one specific category) solved in the same year that they are reported. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. However, there are three missing observations for the county of Gotland. The variables are more carefully explained in Appendix 2.

I construct measures of the county-specific earnings distribution using a measure of earnings including sickness benefits, but not pensions and unemployment insurance. Table 2 presents the descriptive statistics for the earnings at the 25th percentile and median earnings. Since unemployment benefits are not included in my earnings measure, all unemployed individuals are registered as having zero earnings during their unemployment spell. When constructing the earnings measures, all individuals with zero earnings have been excluded in an attempt at only measuring the earnings of the working population.[41]

Table 2 also presents descriptive statistics for other economic and demographic factors. *The National Labour Market Board* provided me with data on the unemployment rate as a percentage of the county labor force. *Statistics Sweden* has provided me with data on the proportion of men aged 15–24, and the proportion of foreign citizens, which will account for the over-

[41] The restriction to positive earnings is standard in the earnings mobility literature; see e.g. Gottschalk and Moffitt (1994).

representation of those groups in the crime statistics. From *The National Council for Crime Prevention*, I retrieved a measure of the clear-up rate for each crime category, measured as the percentage of all reported crimes (in one specific category in one county) that are solved in the same year that they are reported. The final column in Table 2 reports the standard deviation that remains after netting out all variation, due to fixed county and time effects. This residual variation is marginally larger for the unemployment rate than for the variables capturing the age distribution and the proportion of foreign citizens. The clear-up rates, however, display the largest residual variations by far. Comparing the residual variation of the unemployment rate and the low earnings measure, the unemployment rate loses almost 80 percent of the standard deviation when taking fixed effects into consideration, while the corresponding figure for the low earnings measure is 70 percent. All variables are more carefully explained in Appendix 2.

4 Results

4.1 Benchmark model

Table 3 presents the results for the benchmark specification, inspired by Machin and Meghir (2004). The coefficient on the 25^{th} earnings percentile is negative and statistically significant at the ten-percent level for auto theft. The magnitude of the coefficient is, however, very small; a ten-percent increase in the earnings at the 25^{th} percentile would imply a 0.05 percent decrease in the number of auto thefts. The marginal effects are reported in the last row of Table 3.[42] The coefficient on the low earnings measure is positive but insignificant for the other crime categories. The small effects estimated here should be compared to those estimated by Machin and Meghir (2004), who find that a ten-percent increase in the wage at the 25^{th} percentile implies a 0.8 percentage point lower property crime rate. For vehicle crime,[43] they report a corresponding effect of 0.5 percentage points.

---

[42] The coefficient in front of the 25^{th} percentile, β_1, represents the change in crime in percent due to an increase of 1 SEK in the earnings at the 25^{th} percentile. The overall mean value of the earnings at the 25^{th} percentile for my panel is 56,787 SEK. The effect of a ten-percent increase in the earnings at the 25^{th} percentile would thus imply a $0.1 \times 56787 \times \beta_1$ percent change in the crime rate.

[43] Vehicle crime includes both theft of and from a vehicle, as well as criminal damage to a vehicle. My auto theft measure only includes theft of a vehicle.

Table 3 Benchmark model

	Overall crime	Burglary	Auto theft	Robbery
25th earnings percentile	5.3e-07	1.7e-06	-9.4e-06*	8.3e-07
	(1.9e-06)	(3.9e-06)	(5.1e-06)	(5.0e-06)
Clear-up rate	-0.555***	-0.529*	0.117	-0.149**
	(0.195)	(0.285)	(0.294)	(0.075)
Men aged 15–24	6.336***	17.313***	15.647***	14.191***
	(1.993)	(4.220)	(5.437)	(4.564)
Observations	546	546	546	543
Adjusted R-square	0.954	0.836	0.910	0.963
Marginal effect, 25th percentile	0.003	0.01	-0.05	0.005

Note: The marginal effect is the change in the crime rate in percent caused by a ten-percent increase in earnings at the 25th percentile. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. For robbery, there are three missing observations on the clear-up rate for the county of Gotland. ***, ** and * denote significance at the one, five and ten percent level, respectively.

The coefficients on the clear-up rate are negative and statistically significant for all the specific crime categories except auto theft. However, since I was concerned with a potential downward bias of this coefficient, I cannot say a great deal about the relationship between the clear-up rate and the number of crimes.[44] The proportion of young men in the county has a clear positive effect on all crime rates. The coefficient is significant at the one-percent level in all specifications. The coefficient for overall crime suggests an increase in the overall crime rate of 6.3 percent, following a one-percentage point increase in the proportion of men aged 15-24. My next task is to examine whether these results survive a more careful analysis.

4.2 Extended model

To more extensively control for county-level demographic and economic factors, I will, as a first extension of this benchmark specification, include the proportion of foreign citizens, controlling for their over-representation in the crime statistics. A measure of median earnings is also included to control for the return to crime. Further, from previous research, the unemployment rate is known to have an effect on property crime in Sweden, which is accounted for by adding the overall county unemployment rate to the specification. The effect of unemployment on crime is, however, partly an income effect. The inclusion

[44] In the extended model below, I report results from an indirect approach to test the assumption of exogeneity concerning clear-up rates.

of the unemployment rate in the analysis could therefore lead to under-estimated coefficients on the low earnings measure. To investigate the extent of this potential under-estimation, the unemployment rate will not be included in the first specification.

Table 4 presents results from three specifications where the total number of crimes per 100,000 residents is the dependent variable. Specification (1) includes all control variables mentioned above except unemployment, specification (2) extends the analysis by including the unemployment rate, and specification (3) is an expansion of specification (2), taking county-specific trends into consideration.

As in Table 3, the coefficient on the 25th percentile of the earnings distribution displays a positive sign in the first two specifications, although the coefficient is statistically insignificant. In specification (3), however, after including county-specific time trends, the coefficient is still insignificant but negative. From specification (2), it is evident that the inclusion of the unemployment rate does not dampen the effect of low earnings. Median earnings, included to reflect the potential return to crime, exhibit the expected positive coefficient, significant at the five-percent level in the most extensive specification. Evaluated at the overall mean value of median earnings, a ten-percent increase in median earnings would induce a 0.06 percent increase in the overall crime rate.[45] The marginal effects of median earnings are reported in the last row of Table 4.

[45] As for the earnings at the 25th percentile, the coefficient in front of median earnings, β_2, represents the change in crime in percent due to an increase of 1 SEK in median earnings. The overall mean value of median earnings for my panel is 76,784 SEK. The effect of a ten-percent increase in median earnings would thus imply a $0.1 \times 76784 \times \beta_2$ percent change in the crime rate.

Table 4 Extended model, overall crime

	(1)	(2)	(3)
25^{th} earnings percentile	6.5e-07	2.7e-06	-1.1e-06
	(1.9e-06)	(2.0e-06)	(2.2e-06)
Median earnings	0.00002***	0.00002***	7.7e-06**
	(2.3e-06)	(2.8e-06)	(3.7e-06)
Unemployment	-	2.053***	1.095*
	-	(0.660)	(0.626)
Clear-up rate	-0.537***	-0.419**	-0.187
	(0.197)	(0.189)	(0.212)
Men aged 15–24	5.256***	5.193***	7.128***
	(2.015)	(1.963)	(2.585)
Foreign citizens	1.353	1.344	4.099***
	(0.876)	(0.921)	(1.431)
Observations	546	546	546
Adjusted R-square	0.954	0.955	0.965
County-specific trends	No	No	Yes
Marginal effect, 25^{th} percentile	0.004	0.02	-0.006
Marginal effect, median earnings	0.15	0.15	0.06

Note: The marginal effects are the change in the crime rate in percent caused by a ten-percent increase in earnings at the 25^{th} percentile and median earnings, respectively. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. ***, ** and * denote significance at the one, five and ten percent level, respectively.

The unemployment rate is estimated with a positive coefficient, significant at the ten-percent level in specification (3). A one-percentage point drop in the unemployment rate induces a reduction in the overall crime rate of 1.1 percent, relying on the most extensive specification. The coefficient on the clear-up rate is negative in all specifications, but it turns insignificant when county-specific trends are included. The coefficient on the proportion of men aged 15-24 is statistically significantly estimated at the one-percent level in all specifications. Relying on the most extensive specification, the coefficient indicates a 7.1-percent increase in the overall crime rate, following a one-percentage point increase in the proportion of young men. The proportion of foreign citizens first conveys insignificant coefficients in the first two specifications. In specification (3), on the other hand, the coefficient is quite large and significant at the one-percent level, indicating this measure to be negatively correlated with the county-specific trends.

Although I have not yet found any evidence of a link between low earnings and crime in Sweden, it cannot be expected that we have seen the full picture when only having considered the overall crime rate. The measure of overall crime includes many different crime categories and one would not expect economic factors to have the same effect on them all. I will continue by studying the effect of earnings on more specific crime categories. Tables 5–7 show results for the same three specifications as above, with the dependent variables being the three specific crime categories burglary, auto theft and robbery.

The effect of low earnings varies between the different crime categories. As in Table 3, I estimate a positive effect of the 25^{th} percentile on the number of burglaries in the first two specifications, significant at the ten-percent level in specification (2). However, including county-specific trends induces the coefficient to change signs. The pattern is similar for robbery, except that all coefficients are insignificant. For auto theft, the coefficient is negative in all specifications and significant at the one-percent level in specification (3). The coefficient is still very small and implies a 0.1-percent decrease in the number of auto thefts caused by a ten-percent increase in the earnings at the 25^{th} percentile. Thus, the effect of low earnings can, at best, be considered as weak, since is seems to have no separate effect on the overall crime rate, the number of burglaries or the robbery rate.[46] Further, it does not seem that the effect of low earnings "suffers", to any large extent, from the inclusion of the unemployment rate in the specification. For burglary and robbery, the coefficient on the low earnings measure marginally increases between specifications (1) and (2), while it marginally decreases for auto theft.

Although I believe the problem of endogeneity to be small because of the use of county-level data as well as a restrictive specification, I here report results from two indirect approaches to evaluate the impact of endogeneity. First, to appreciate the influence of omitted variables bias, I stepwise extend the specification while observing the estimates on the variable of primary interest. If the estimates are robust to the inclusion of essential control variables (earlier proved to be determinants of crime), they can be presumed not to be highly sensitive to any omitted variables. Applying this approach to the low earnings

[46] Although excluding individuals with zero earnings eliminates all individuals having been unemployed during the whole year, short-term unemployed will still be included in the earnings measure. Excluding all individuals with annual earnings below 100,000 in 1995 (a somewhat arbitrary amount but pre-tax earnings below this threshold must be considered as very low and cannot represent a full-time worker) and using earnings at the 25^{th} percentile for the remaining individuals does not yield more significant results on the low earnings measure, and the coefficients are very similar to when only excluding individuals with zero earnings.

measure indicates that the coefficients on the earnings at the 25th percentile remain small, as I gradually extend the number of control variables and thus, I believe the problem of omitted variable bias to be a less serious issue. Second, to test the assumption of exogeneity of my low earnings measure, I perform a test of refutability, suggested by Angrist and Krueger (1999). The idea is to evaluate the impact of the specific control variable in samples where the effect is known to be zero. Results indicating a non-zero impact can be interpreted as an indication of a lingering endogeneity problem. When estimating the effect of low earnings on the twice lagged auto theft rate, the only crime-category for which I have found a significant impact of low earnings, I obtain a t-value of 0.08. Hence, my significant effect of low earnings on the number of auto thefts does not seem to be driven by endogeneity.[47]

Considering median income, it mainly produces positive coefficients. The exception is specification (2) for burglary, where I estimate a negative coefficient, significant at the five-percent level. However, when including county-specific trends, the coefficient becomes negative and insignificant. For auto theft and robbery, the coefficients are all positive and significant at the one-percent level. According to the most extensive specification, a ten-percent increase in median earnings would increase the number of auto thefts and robberies by 0.5 and 0.4 percent, respectively.

[47] Although the interpretation of the test is somewhat unclear when starting with an insignificant estimate I have performed analogous tests for the other crime categories and obtain t-values of 0.64 and 1.0 for overall crime and burglary, respectively. For robbery, on the other hand, the t-value is 2.11, indicating a potential endogeneity problem.

Table 5 Extended model, burglary

	(1)	(2)	(3)
25^{th} earnings percentile	1.9e-06	8.5e-06*	-2.7e-06
	(3.9e-06)	(4.4e-06)	(3.7e-06)
Median earnings	6.8e-08	-0.00002**	7.9e-06
	(6.8e-06)	(7.5e-06)	(6.5e-06)
Unemployment	-	7.059***	1.485
	-	(1.599)	(0.973)
Clear-up rate	-0.525*	-0.414	0.008
	(0.287)	(0.253)	(0.078)
Men aged 15–24	15.478***	14.824***	7.450*
	(4.212)	(3.892)	(3.934)
Foreign citizens	2.221	1.966	3.348
	(2.631)	(2.269)	(2.359)
Observations	546	546	546
Adjusted R-square	0.836	0.848	0.921
County-specific trends	No	No	Yes
Marginal effect, 25^{th} percentile	0.01	0.05	-0.02
Marginal effect, median earnings	0.0005	-0.15	0.06

Note: The marginal effects are the change in the crime rate in percent, caused by a ten-percent increase in earnings at the 25^{th} percentile and median earnings, respectively. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. ***, ** and * denote significance at the one, five and ten percent level, respectively.

The unemployment rate seems to be a robust determinant of property crime. For auto theft and robbery, all coefficients are quite large, positive and significant at the one-percent level. A one-percentage point increase in the unemployment rate would, according to my estimates, produce an increase in the number of auto thefts and robberies by 6.3 and 4.2 percent, respectively. The results for auto theft are in accordance with the findings of Nilsson and Agell (2005), where a one-percentage point increase in the unemployment rate was estimated to increase the number of auto thefts by 3.9 percent. For burglary, on the other hand, the coefficient in specification (3) is insignificantly estimated in comparison to the findings of Nilsson and Agell (2005), where unemployment was found to have a significant impact on burglary.

Table 6 Extended model, auto theft

	(1)	(2)	(3)
25th earnings percentile	-9.1e-06*	-3.4e-06	-0.00002***
	(5.0e-06)	(5.2e-06)	(6.4e-06)
Median earnings	0.00004***	0.00003***	0.00006***
	(8.6e-06)	(9.8e-06)	(0.00001)
Unemployment	-	6.193***	6.318***
	-	(1.870)	(2.117)
Clear-up rate	0.122	0.237	-0.382
	(0.291)	(0.286)	(0.280)
Men aged 15–24	13.212**	12.867**	12.694*
	(5.520)	(5.419)	(6.740)
Foreign citizens	2.949	2.716	8.586**
	(2.873)	(3.013)	(3.990)
Observations	546	546	546
Adjusted R-square	0.910	0.913	0.929
County-specific trends	No	No	Yes
Marginal effect, 25th percentile	-0.05	-0.02	-0.11
Marginal effect, median earnings	0.31	0.23	0.46

Note: The marginal effects are the change in percent in the crime rate caused by a ten-percent increase in earnings at the 25th percentile and median earnings, respectively. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. ***, ** and * denote significance at the one, five and ten percent level, respectively.

The coefficients on the clear-up rates display both negative and positive signs. None of the coefficients are significant in the most extensive specification.[48] Briefly considering the coefficients on the remaining control variables, it is evident that the relative size of the group of men aged 15-24 is a strong determinant of property crime. It is significant at the ten-percent level or more, in all specifications. The proportion of foreign citizens exhibits coefficients with both negative and positive signs. The only coefficient that is significant is the one for auto theft in specification (3).

[48] As mentioned above, I would expect the coefficient on the clear-up rate to be subject to a downward bias. Using the same indirect approach to test the assumption of exogeneity as for the low earnings measure, I estimate the effect of clear-up rates on twice lagged crime rates and obtain t-values of 0.04, 1.39 and 0.13 for overall crime, burglary and robbery, respectively. For auto theft, the t-value is 2.59, indicating this crime category to have the most obvious problem with endogeneity of the clear-up rate. Excluding the clear-up rate from the specification does not, however, alter the results for the coefficient that is my main concern on the low earnings variable.

Table 7 Extended model, robbery

	(1)	(2)	(3)
25th earnings percentile	7.1e-07	6.4e-06	-4.4e-06
	(4.9e-06)	(4.9e-06)	(6.2e-06)
Median earnings	0.00008***	0.00007***	0.00005***
	(5.4e-06)	(6.8e-06)	(0.00001)
Unemployment	-	6.068***	4.214***
	-	(1.524)	(1.678)
Clear-up rate	-0.157**	-0.106	-0.061
	(0.077)	(0.078)	(0.077)
Men aged 15–24	15.272***	14.470***	12.269*
	(5.104)	(5.045)	(7.412)
Foreign citizens	-1.347	-1.377	4.127
	(2.627)	(2.737)	(3.332)
Observations	543	543	543
Adjusted R-square	0.963	0.964	0.969
County-specific trends	No	No	Yes
Marginal effect, 25th percentile	0.004	0.04	-0.02
Marginal effect, median earnings	0.61	0.54	0.38

Note: The marginal effects are the change in the crime rate in percent caused by a ten-percent increase in earnings at the 25th percentile and median earnings, respectively. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. However, for robbery, there are three missing observations for the county of Gotland, leaving me with 543 observations. ***, ** and * denote significance at the one, give and ten percent level, respectively.

To sum up, there seems to be no strong separate effect of low earnings on Swedish crime rates. I have only been able to estimate a significant effect, in specifications where county-specific time trends are included, on auto theft. However, the unemployment rate seems to have a strong effect both on overall crime and specific property crime categories, thereby supporting previous Swedish research. Below, I will discuss possible explanations for these weak effects of low earnings on Swedish crime rates but first, I will investigate whether the results differ when using alternative measures of the earnings distribution.

4.3 Alternative earnings measures[49]

An alternative way of considering the lower end of the earnings distribution, following Ehrlich (1973), would be to include a measure of *the proportion of*

[49] Descriptive statistics for all variables used in this section are reported in Appendix 3.

the population with earnings below one half of the median income. Estimating a specification identical to eq. (2) above and replacing earnings at the 25th percentile with a measure of the proportion of the population with earnings below one half of the median earnings (here called the relatively poor), I obtain insignificant coefficients on the new control variable for all crime categories except auto theft. The results indicate that a one-percentage point increase in the proportion of relatively poor would induce a 2.3 percent increase in the number of auto thefts.[50] Thus, it seems like low earnings only have a weak effect on Swedish crime rates. The low earnings measures have no effect on the overall crime rate, the number of burglaries or the robbery rate. The number of auto thefts, however, seems to be influenced by the lower part of the earnings distribution, represented by the earnings at the 25th percentile and the proportion with earnings below one half of the median earnings.[51]

Although I have found low earnings to have a weak effect on crime in Sweden, earnings inequality might still affect Swedish crime rates. When thinking about measures of earnings inequality, the first that comes to mind are perhaps the Gini-coefficient and some percentile quotient like the 90th/10th, 90th/50th or 50th/10th. However, after estimating a variety of such specifications, I find these variables to be uncorrelated with the crime rate. Testing similar specifications as in section 4.2, including fixed effects, county-specific trends and control variables but letting the Gini-coefficient or percentile quotients control for the earnings distribution, the earnings variables give insignificant results. Hence, it does not seem that measures of overall earnings inequality do a better job in capturing an effect of earnings on Swedish crime rates than measures of low earnings.

5 Why weak effects of low earnings?

Why do pre-tax earnings have such a weak effect on crime in Sweden? Even though I found an effect of low earnings on the number of auto thefts, I would have expected to find a link between low earnings and property crime in general. There are, however, a number of possible explanations for these weak results, which will be discussed here.

First, a lack of variation in the independent variable might be an explanation for the lack of significant coefficients on the low earnings measure.

[50] The estimation results are reported in Appendix 4.

[51] I have also used measures of the earnings at other percentiles at the lower end of the earnings distribution, such as the 10th earnings percentile, instead of the earnings at the 25th percentile, but the results are similar to those reported above.

Considering Figure 1, it is evident that most of the variation in the low earnings measure is concentrated to the 1990s. With the disadvantage of losing degrees of freedom, I have re-estimated eq. (2) splitting the panel into two periods, the stable period 1975-1987 and the turbulent period 1988-2000. However, regarding the low earnings measure, the only significantly estimated coefficient obtained is for burglary during the stable period. Consequently, it does not seem that a lack of variation is causing the weak results.[52]

Second, including both fixed effects and county-specific trends in the specification means posing high demands for obtaining significant estimates, since the fixed effects joint with county-specific trends eliminate a great deal of the variation. Excluding the time-fixed effects as well as the county-specific trends makes all the coefficients on the earnings at the 25th percentile and median earnings significant at least at the ten-percent level (the coefficients do not, however, suggest larger effects than in the above specifications). For overall crime, auto theft and robbery, the coefficients on the earnings at the 25th percentile are negative and those on median earnings positive, as expected. For burglary, the signs are reversed, indicating that the low earnings measure and median earnings capture different effects for burglary than for the other crime rates. Regardless of the puzzling signs for burglary, it seems that one reason for my insignificant results of low earnings is the restrictive specification used. However, since the aim is to estimate the causal effect of low earnings on crime, the fixed effects and county-specific trends do serve a purpose.

Third, the weak effect of low earnings on crime using Swedish data should be compared with results from, for example the United States, where Gould et al (2002) find an effect on crime of both wages of unskilled men and the unemployment rate, with the wage effect being the stronger one. Their conclusion is that wages are a better measure of the labor market prospects of potential criminals, since unemployment is often short-lived and highly cyclical. Considering the far-reaching consequences of incarceration, crime rates should be more responsive to long-term changes in labor market conditions than to short-term fluctuations. Might it then be the case that unemployment to a larger extent is of a persistent nature in Sweden than in the United States and that this could be a reason for the strong effect of unemployment on Swedish crime rates? The incidence of long-term unemployment could be considered to reflect the persistence of unemployment.

[52] Also for unemployment has the main variation occurred during the 1990s. Although including unemployment in the specification has not influenced the results for low earnings using the whole panel, it might still be the reason for insignificant results during the turbulent period. However, excluding the unemployment rate when re-estimating eq. (2) on the two periods does not alter the results for the turbulent period.

Comparing Swedish and U.S. unemployment rates, it is apparent that the incidence of long-term unemployment has been more widespread in Sweden than in the United States during the period investigated in this paper.[53] OECD (1992, 2003) reports figures suggesting that the proportion of the Swedish unemployment rate characterized as long-term (unemployed six months or more) was 24.9 percent in 1983 and 35.2 percent in 1995. The corresponding figures for the U.S. were 23.9 and 17.3, respectively. The more permanent nature of Swedish unemployment could thus explain the strong effects of unemployment on Swedish crime rates. The weak effects of low earnings on crime could potentially be a result of the high earnings mobility in Sweden. If earnings have a large transitory component, this can be expected to decrease the importance of earnings for crime. Permanent components of earnings should, on the other hand, have a larger effect on crime. Gustavsson (2002) report persistent earnings inequality to have increased dramatically in Sweden during the beginning of the 1990s and to have remained at a high level throughout the decade. However, transitory earnings fluctuations appear to dominate the earnings dispersion of young males, a part of the population committing a disproportionate share of many crimes, which might potentially explain the weak effects of low earnings on Swedish crime rates. Unfortunately, I am not aware of any similar studies for the United States.

Fourth, another aspect distinguishing Sweden from the United States is the social insurance system. In Sweden, unemployment benefits are generous, as is social assistance, and low earners are able to receive housing benefits, for example. Hence, earnings, excluding all transfers, do not fully reflect the true economic situation of low-skilled workers and could therefore be uncorrelated with crime. Considering disposable income might give another picture. In LINDA, a somewhat consistent measure of disposable income is available since 1978.54 Constructing similar measures as for the earnings distribution, I have estimated the effect on crime of the disposable income at the 25th percentile and median disposable income in a specification otherwise identical to eq. (2), and these results are reported in Table 8.55 The significant effect of the 25th percentile for auto theft now disappears. On the other hand, disposable income at the 25th percentile seems to have a significant impact on the robbery rate. Although the results differ between using earnings and disposable income,

[53] For a comparison of unemployment rates, see OECD (1991) for the period 1974-1989, and OECD (2003) for the period 1990-2000. For the incidence of long-term unemployment, see OECD (1992) for the period 1983-1989 and OECD (2003) for the period 1990-2000.

[54] Unemployment insurance, social assistance and housing benefits are all included in the measure of disposable income, as well as many other transfers.

[55] Descriptive statistics for the disposable income measures are reported in Appendix 3.

the effects must still be considered as weak and the Swedish social insurance system cannot be considered to be the reason for insignificant effects of low earnings on crime.

Table 8 Disposable income

	Overall crime	Burglary	Auto theft	Robbery
25^{th} percentile, disposable income	-7.4e-06	-0.00002	-0.00004	-0.00005***
	(7.3e-06)	(0.00001)	(0.00003)	(0.00002)
Median disposable income	-7.8e-06	-5.5e-06	5.1e-06	0.00003*
	(6.3e-06)	(0.00001)	(0.00003)	(0.00002)
Unemployment	1.271**	1.665	7.454***	3.838**
	(0.609)	(1.055)	(2.282)	1.724
Clear-up rate	-0.145	0.009	-0.634*	-0.067
	(0.226)	(0.076)	(0.329)	(0.078)
Men aged 15–24	8.560***	5.227	8.457	-3.811
	(3.256)	(4.617)	(8.758)	(7.682)
Foreign citizens	5.069***	8.495**	3.338	-4.738
	(1.638)	(3.402)	(6.302)	(4.370)
Observations	483	483	483	480
Adjusted R-square	0.964	0.924	0.933	0.970
County-specific trends	Yes	Yes	Yes	Yes
Marginal effect, 25^{th} percentile	-0.03	-0.08	-0.17	-0.21
Marginal effect, median income	-0.04	-0.0.03	0.03	0.15

Note: The marginal effects are the change in the crime rate in percent caused by a ten-percent increase in disposable income at the 25^{th} percentile and median disposable income, respectively. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity and all regressions include a complete set of municipality and year effects. The complete panel consists of 483 observations for 21 counties during the period 1978–2000. However, for robbery, there are three missing observations for the county of Gotland, leaving me with 480 observations. ***, ** and * denote significance at the one, give and ten percent level, respectively.

6 Conclusions

This paper uses a new panel dataset covering all Swedish counties during the period 1975–2000. I have access to individual-level income information, which allows me to construct various measures of the earnings of low-skilled workers and earnings inequality. This paper is the first to study an effect of earnings on crime using Swedish data.

My main results can be summarized as follows. First, the effect of low earnings on crime in Sweden can be considered as weak, at best, since it seems to have no separate effect on the overall crime rate, the number of burglaries or the robbery rate. I do, however, estimate a significant effect of low earnings on the number of auto thefts, but the effect is small. The coefficient implies a 0.1-percent decrease in the number of auto thefts caused by a ten-percent increase in the earnings at the 25th percentile. Second, there is an unambiguous link between unemployment and Swedish crime rates. According to my results, a one-percentage point drop in the unemployment rate would decrease the overall crime rate, the number of auto thefts and the robbery rate by approximately 1.1, 6.3 and 4.2 percent, respectively.

So the question is why pre-tax earnings have such a weak effect on crime in Sweden. Even though I found an effect of low earnings on the number of auto thefts, I would have expected to find a link between low earnings and property crime in general. As discussed above, the permanent nature of Swedish unemployment and the dominating transitory earnings fluctuations for those most likely to commit crime could be a reason for the weak effect of low earnings and the strong effect of unemployment on Swedish crime rates. I cannot, however, determine whether the nature of earnings fluctuations is the explanation for the differing results between Sweden and the United States, and therefore conclude with a call for future research.

References

Angrist, J D and A B Krueger (1999), Empirical strategies in labor economics in *Handbook of Labor Economics* vol. 3A (eds. Ashenfelter, O.C. and D. Card), Elsevier, Amsterdam.

Becker, G (1968), Crime and punishment: an economic approach, *Journal of Political Economy* 76, 169–217.

Bourguignon, F (2001), Crime as a social cost of poverty and inequality: a review focusing on developing countries in *Facets of globalization: international and local dimensions of development* (eds. Yussuf, S., S. Evenett and W. Wu), World Bank, Washington.

Bourguignon, F, J Nuñez and F Sanchez (2003), What part of the income distribution does matter for explaining crime? The case of Colombia, Working paper 2003-04, DELTA.

Demombynes, G and B Özler (2002), Crime and local inequality in South Africa, Working paper 2925, World Bank, Washington.

Edin, P-A and P Fredriksson (2000), LINDA – Longitudinal individual data for Sweden, Working paper 2000:19, Department of Economics, Uppsala University.

Edmark, K (2003), The effects of unemployment on property crime: Evidence from a period with unusually large swings in the business cycle, Working paper 2003:14, Department of Economics, Uppsala University.

Ehrlich, I (1973), Participation in illegitimate activities: a theoretical and empirical investigation, *The Journal of Political Economy* 81(3), 521–565.

Eide, E (1999), Economics of criminal behavior in *Encyclopedia of Law and Economic* vol. 5 (eds. Bouckaert, B. and G. De Geest), Cheltenham.

Entorf, H and H Spengler (2000), Socioeconomic and demographic factors of crime in Germany: Evidence from panel data of the Berman states, *International Review of Law and Economics* 20, 75–106.

Fajnzylber, P, D Lederman and N Loayza (1998), Determinants of crime rates in Latin America and the world, Latin American and Caribbean Viewpoint Series Paper, World Bank, Washington.

Fajnzylber, P, D Lederman and N Loayza (2002), Inequality and violent crime, *The Journal of Law and Economics* 45, 1–39.

Freeman, R (1994), Crime and the labor market in *Crime* (eds. Wilson, J.Q. and J. Petersilia), ICS Press, San Francisco.

Freeman, R (1996), Why do so many young American men commit crimes and what might we do about it?, *The Journal of Economic Perspectives,* 10(1), 25–42.

Freeman, R (1999), The economics of crime in *Handbook of Labor Economics* vol. 3C (eds. Ashenfelter, O.C. and D. Card), Elsevier, Amsterdam.

Friedberg, L (1998), Did unilateral divorce raise divorce rates? Evidence from panel data, *The American Economic Review* 88(3), 608–627.

Gottschalk, P and R Moffitt (1994), The growth of earnings instability in the U.S. labor market, *Brookings Papers on Economic Activity*, 217-272.

Gould, E, B Weinberg and D Mustard (2002), Crime rates and local labor market opportunities in the United States: 1979–1997, *The Review of Economics and Statistics* 84(1), 45–61.

Grogger, J (1998), Market wages and youth crime, *Journal of Labor Economics* 16(4), 756–791.

Gustavsson M (2002), Earnings dynamics and inequality during macroeconomic turbulence: Sweden 1991-1999, Working paper 2002:20, Department of Economics, Uppsala University.

Imrohoroglu, A, A Merlo and P Rupert (2000), What accounts for the decline in crime?, Working paper 00-11, C.V. Starr Center for Applied Economics, New York University, New York.

Kelly, M (2000), Inequality and crime, *The Review of Economics and Statistics* 82(4), 530–539.

Machin, S and C Meghir (2004), Crime and economic incentives, *Journal of Human Resources* 39(4), 958-979.

Nilsson, A and J Agell (2005), Crime, unemployment and labor market programs in turbulent times, Essay I in this thesis.

OECD (1991), *Employment Outlook,* Paris, OECD.

OECD (1992), *Employment Outlook,* Paris, OECD.

OECD (2003), *Employment Outlook,* Paris, OECD.

Raphael, S and R Winter-Ebmer (2001), Identifying the effect of unemployment on crime, *Journal of Law and Economics* vol. XLIV, 259–283.

Widerstedt, B (1998), Determinants of long and short distance migration in Sweden in *Moving or staying? Job Mobility as a sorting process* UES 464, Umeå University.

Wooldridge, J M (2002), *Econometric analysis of cross section and panel data,* The MIT Press, Cambridge, Massachusetts.

Wooldridge, J M (2003), Cluster-sample methods in applied econometrics, *The American Economic Review* 93(2), 133–138.

Appendix

A.1 Deriving the earnings measure

All earnings measures at the county level are derived using the individual earnings measure in LINDA for all men aged 25–64. The earnings measure included in LINDA has not been entirely consistent throughout the period. Since 1975 it is, however, possible to construct a measure consistent for the remaining period with some small corrections. The earnings measure for the period 1975–1977 is constructed by adding income from employment (*A-inkomst av tjänst* + *Beskattningsbar sjöinkomst*) and income from business (*A-inkomst av jordbruk* + *A-inkomst av rörelse*) and then subtracting the sum of pensions (*Pension*), unemployment compensation (*Dagpenning vid arbetslöshet* + *KAS*), and compensation during labor market training (*Utbildningsbidrag*). For the period 1978–2000, the earnings measure is directly available in the data with some minor adjustments.

A.2 Definitions of variables

Table A1 Definitions of crime variables

Variables	Definitions
All crimes	All crimes reported in the county during one year.
Burglary	All burglary, not including firearms, in the county during one year.
Auto theft	All auto thefts, both attempted and completed, in the county during one year.
Robbery	All robberies, with and without the use of firearms, in the county during one year.

Note: The crime data were provided by The National Council for Crime Prevention (BRÅ).

Table A2 Definitions of control variables

Variables	Definitions
25^{th} earnings percentile	The county-specific 25^{th} percentile.
Median earnings	The county-specific median earnings.
Unemployment	The proportion of unemployed of the county labor force.
Proportion of men aged 15–24	Proportion of men aged 15–24 in each county population.
Proportion of foreign citizens	Proportion of the population in each county that are not Swedish citizens.
Clear-up rates	The percentage of all reported crimes (in one category) that are solved in the same year that they are reported.

Note: The earnings measures at the county-level are calculated using the individual annual earnings (in 1980SEK) measure in LINDA for all men aged 25–64.The data on unemployment were provided by *The National Labour Market Board*, *while* the proportion of men aged 15–24 and the proportion of foreign citizens were provided by *Statistics Sweden. The National Council for Crime Prevention* provided me with data on clear-up rates for different crime categories.

A.3 Descriptive statistics, alternative earnings measures

Table A3 Descriptive statistics, control variables

Variables	Min	Max	Mean	Standard deviation	Standard deviation net of FE
Proportion of relatively poor	0.074	0.252	0.145	0.036	0.009
Gini-coefficient	0.258	0.459	0.332	0.045	0.010
$90^{th}/10^{th}$ percentile quotient	2.559	10.790	4.787	1.786	0.538
$90^{th}/50^{th}$ percentile quotient	1.233	2.196	1.528	0.143	0.032
$50^{th}/10^{th}$ percentile quotient	1.628	7.603	3.128	1.110	0.341
10^{th} earnings percentile	10567	46085	27045	7547	2194
25^{th} percentile, disposable income	25700	50696	41511	4042.6	856.5
Median disposable income	37823	67825	51635	5675.1	817.5

Note: All earnings variables are computed from the individual annual earnings measure included in LINDA and measured as annual earnings in 1980 SEK. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. However, I only have data on disposable income for the period 1978-2000.

A.4 Supplementary estimation results

Table A4 Proportion of relatively poor

	Overall crime	Burglary	Auto theft	Robbery
Proportion of relatively poor	0.366	0.359	2.325*	-0.404
	(0.391)	(0.632)	(1.250)	(1.235)
Median earnings	-6.0e-07	-2.7e-06	0.00004***	0.00003***
	(2.9e-06)	(5.2e-06)	(0.00001)	(8.9e-06)
Unemployment	1.028	1.500	6.447***	4.596***
	(0.632)	(0.977)	(2.029)	(1.672)
Clear-up rate	-0.180	0.010	-0.385	-0.061
	(0.213)	(0.078)	(0.281)	(0.077)
Men aged 15–24	7.208***	7.833**	15.257**	13.273*
	(2.563)	(3.846)	(6.725)	(7.244)
Foreign citizens	4.230***	3.377	8.741**	3.647
	(1.426)	(2.374)	(4.045)	(3.441)
Observations	546	546	546	543
Adjusted R-square	0.965	0.921	0.928	0.969
County-specific trends	Yes	Yes	Yes	Yes
Marginal effect, median earnings	-0.005	-0.02	0.31	0.23

Note: The marginal effect is the change in the crime rate in percent caused by a ten-percent increase in median earnings. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a complete set of municipality and year effects as well as county-specific trends. The complete panel consists of 546 observations for 21 counties during the period 1975–2000. For robbery, there are three missing observations on the clear-up rate for the county of Gotland. ***, ** and * denote significance at the one, five and ten percent level, respectively.

Essay III

Parental unemployment and children's school performance[#]

1 Introduction

There is a large literature studying various indirect effects of unemployment, suggesting an effect of unemployment on health, both mental and physical, and crime to mention a few. Björklund and Eriksson (1998) examine research on the link between unemployment and mental health in the Nordic countries, and find that most longitudinal studies suggest unemployment to be associated with deteriorating mental health. Eliason and Storrie (2004) use Swedish individual level data and find that losing one's job shortens the life of men, while Nilsson and Agell (2005) report significant effects of unemployment on property crime. The aim of this paper is to consider another possible indirect effect of unemployment, namely the school performance of the children of the unemployed.

When a family member becomes unemployed, the whole family is likely to be affected by the new situation which, in turn, could affect the school performance of the children concerned. Households suffering from unemployment typically experience several disadvantages, such as lower incomes, smaller social networks and potentially also having to live in worse neighborhoods. On the other hand, being unemployed might mean that you can spend more time with your children. This extra time could, of course, be spent helping your children with homework or other issues to improve their achievement in school.

[#] I have benefited from helpful comments by Jonas Agell, Peter Fredriksson and Peter Skogman Thoursie as well as from seminar participants at Stockholm University. I would also like to thank Louise Kennerberg for providing me with the data. This research was funded by a grant from the Institute for Labour Market Policy Evaluation (IFAU).

As far as I am aware, this is the first paper analyzing a relationship between parental unemployment and children's school performance.[56] In addition to studies of other indirect effects of unemployment, the question could also be linked to the literature on the effect of a divorce on the educational achievement of children, since many of the disadvantages of families suffering from unemployment spells could be compared to the difficulties following a divorce.[57] For example, single-parent families on average have lower incomes and smaller social networks, characteristics mentioned above for families suffering from unemployment. There is a large literature indicating that youth living with a single mother or a stepparent have lower rates of completing high school or starting college, and higher rates of arrest and drug use as compared to youth in intact families.[58] As when evaluating the effect of divorce on school performance, the question to solve is whether the disadvantages of children whose parents experience unemployment could be predicted prior to the unemployment spell, or whether the unemployment caused the disadvantages. To determine such a causal effect of parental unemployment on children's school performance, the pre-existing disadvantages of the family or youth must be controlled for. This is done by controlling for the pre-incident skill-level of the child, in terms of primary school grade point average (GPA), and various child and family characteristics.

I use Swedish data on individual GPA from the completion of primary school (*grundskolan*) at the age of 16 and final grades from upper secondary school (*gymnasiet*) for a majority of all children completing primary school in 1990 and directly moving on to three years of upper secondary school, which they complete in 1993. Since grades are to a large extent used for entrance to higher education in Sweden, investigating the determinants of grades is of great importance. Only considering children attending a three-year upper secondary school program will induce sample-selection problems, which will be discussed in detail below. To estimate the effect of parental unemployment on children's school performance, children with at least one parent subjected to unemployment during the period they attend upper secondary school will be

[56] Micklewright et al (1990) is the only study of which I am aware that mentions a potential effect of parental unemployment on children's school performance and the focus of that study is on children leaving school.

[57] For reviews of the literature on educational disadvantages of children experiencing a divorce or growing up with a single parent, see Amato and Keith (1991) and Cherlin (1999).

[58] Manski et al (1992) use U.S. data and estimate the effect of divorce on high school graduation, letting the family structure be endogenous. Their findings indicate that living in an intact family increases the probability of a child graduating from high school. Hoffman and Johnson (1998) evaluate the effect of family structure on adolescent drug use and Coughlin and Vuchinich (1996) discuss the effect on delinquency.

compared to children whose parents have been working throughout the whole period. I will also consider whether maternal and paternal unemployment have different effects on children's school performance and whether the length of the unemployment spell is of importance. Grades are not reported until the pupil has turned 16, i.e. when the pupil has completed primary school; hence, due to data availability, I can only estimate the effect of parental unemployment on the school performance during upper secondary school.

The huge variation in Swedish unemployment at the beginning of the 1990s provides an ideal setting for my study.[59] The unemployment rate in 1990 was less than two percent of the labor force while in 1993, it was close to nine percent. Individuals in all segments of society experienced unemployment spells during this period, and becoming unemployed when unemployment rates are high could lead to more distress than otherwise, since the possibility of getting a new job is lower. On the other hand, being unemployed when unemployment rates are high is often considered to be associated with lower social and psychological costs.[60] Nonetheless, it is an advantage that the variation in Swedish unemployment can be traced to macroeconomic events, which are exogenous to the individual.

My results indicate that having an unemployed father has a negative effect on a child's school performance, while having an unemployed mother has a positive effect. The positive effect of having an unemployed mother seems to increase with the length of the unemployment spell. One explanation for the differing results across genders could be that women in general cope better with being unemployed and hence, are able to use their new extra time doing something productive, such as spending quality time with their children.

The next section describes the data, and presents the empirical strategy. Section 3 reports the results, section 4 contains some sensitivity analysis, focusing on issues of sample-selection bias, and the final section sums up.

2 Data and empirical strategy

2.1 The children and their parents

The data set contains information on individual GPA from the completion of primary school at the age of 16 and final grades from upper secondary school at the age of 19 from *Elevregistret* collected by *Statistics Sweden.* I concentrate my study on the children completing primary school in 1990 and then directly

---

[59] For a discussion of the Swedish macroeconomic crisis of the 1990s, see Lindbeck (1997).

[60] See Åberg et al (2003) for a discussion on social interactions and unemployment.

continuing with three years of upper secondary school. I have excluded children choosing to attend a two-year vocational program after primary school and children taking a sabbatical year after primary school or choosing to spend a year abroad during upper secondary school. Excluding children only attending upper secondary school for two years means that I disregard children who might have high probabilities of living in a problematic family. Although such children are of great importance when studying the determinants of children's educational achievement, I choose to concentrate on the largest relatively homogenous group of children choosing a three-year upper secondary school program. Focusing on the children graduating from three years of upper secondary school in 1993 leaves me with 35,550 individuals after excluding individuals with missing observations on key-variables.

In total, 109,392 children completed primary school in 1990. Out of these, 53,000 (48.4 %) completed upper secondary school in 1993. However, 5,750 of these children completed a program shorter than three years and for 11,700 of the remaining children, I have missing observations on key-variables.[61] Hence, in my study, I consider 35,550 children completing primary school in 1990 and completing a three-year upper secondary school program in 1993. In total, I consider 32.5 percent of all children completing primary school in 1990, the largest relatively homogenous group. 16,474 (15.1 % of the population) of the excluded children had still not completed an upper secondary school program in 1999, 31,384 (28.7 % of the population) attended a shorter upper secondary school program which they completed before 1993, and 8,534 children (7.8 % of the population) completed some kind of upper secondary school program during the period 1994 to 1999. The characteristics of the excluded children will be discussed in section 4.2.

[61] The excluded children mainly have missing observations on the family structure (whether the child lives with both his/her biological parents). The children studied can still have missing observations on paternal or maternal characteristics, but not on both parents. The group with missing observations on key variables is discussed in more detail in section 4.2.

Table 1 Characteristics of the children and their parents

	Children	Mother	Father
Female	0.497		
Year of birth:			
Min	1972		
Max	1974		
Country of birth:			
Sweden	0.963	0.900	0.758
Nordic countries	0.006	0.046	0.028
Europe	0.008	0.030	0.033
Outside Europe	0.023	0.016	0.014
Missing observation	0	0.008	0.167
Highest level of completed education:			
Primary		0.175	0.187
Secondary		0.443	0.351
University		0.348	0.273
Missing observation		0.034	0.189

Note: Primary school indicates compulsory school for ten years or less, secondary school indicates up to four years of upper secondary school and university indicates at least some studies at the university after upper secondary school. Missing observations on the characteristics of the parents stem from the fact that information on both parents is only included when children live in the same household as both parents. The complete dataset contains information on 35,550 children.

Table 1 gives some descriptive statistics on the characteristics of the children in my sample and their parents. Information on family and youth characteristics is gathered from the longitudinal database on education, income and employment (LOUISE) and the register-based labor market statistics database (RAMS) of *Statistics Sweden.* Worth noting is that there are almost as many girls as boys in the sample, and that a very large proportion of the children were born in Sweden. In my data, about 98.6 percent of the children completed primary school during the calendar year when they became 16 years old, 1.4 percent in the year they turned 17 and only 15 percent when they turned 18.

Missing observations on the characteristics of the parents stem from the fact that information on both parents is only included when children live in the same household as both parents. In LOUISE, children are registered as belonging to the same family as only one of the parents if the parents are divorced. If there is a cohabitant adult in the family, it is not the biological/adoptive parent of the child.[62]

2.2 Data on school performance

During the period of my study, Sweden had a national relative grading system where the grades were determined based on comparisons with the national average achievement. The scale ranged from 1 to 5 and the goal was that the national average should be 3, with a standard deviation of 1. Hence, the fraction of pupils to receive each grade was predetermined.[63]

The relative grading system implied that grades in primary school were determined by comparing all children with the national average achievement in the specific grade, while grades in upper secondary school were determined by comparing with the achievement of everyone attending the same upper secondary school program. Grade competition would thus depend on the chosen program, where children choosing a demanding theoretical program (e.g. the natural science program) would have to compete harder for high grades than during primary school. The increasing competition implies that grades from primary and upper secondary school are not entirely comparable. I will take this into consideration by ranking the GPA from both primary and upper secondary school of all children attending the same program and school during upper secondary school.[64] Hence, the ranking is made within program and school, comparing the GPA of all children attending the same upper secondary school and program over time. The ranking is constructed such that the higher the ranking, the better the GPA; thus a ranking of one implies that the child has the lowest grade within the attended program and school. The effect will thus be determined by whether the ranking of the children concerned has been affected by parental unemployment.

---

[62] Among the children studied here, only 17 percent seem to live without their father (a number based on the information on missing observations on the country of birth of the father). In 1990, only 70 percent of all children aged 16 lived with both their biological/adoptive parents. The group of children studied here is thus not representative of the general population in that respect. I will discuss the characteristics of the children studied in comparison to other groups in more detail below.

[63] This system was criticized for its lack of knowledge orientation and was changed into a criterion-referenced system, implemented in the school year of 1995/96. To guide teachers in their grading, there were national achievement tests in Math, Swedish and English. Since grades are determined by the teacher or the school, I would have preferred data from the standardized test scores, but such data are not available. However, since grades are to a large extent used for entrance to higher education, investigating the determinants of grades is of great importance.

[64] The specific school is taken into consideration to account for the incidence of grade inflation. See Wikström and Wikström (2004) for a discussion of grade inflation and school competition.

Table 2 shows descriptive statistics on the children's GPA from primary and upper secondary school, the GPA ranking and dummy variables capturing the program attended during upper secondary school. On average, these children got a lower GPA from upper secondary school than when completing primary school. This lower GPA from upper secondary school could certainly be an effect of higher grade-competition. Considering the GPA ranking, the average size of the groups within which the children are compared is 29, which will be considered when interpreting the coefficients on parental unemployment discussed in more detail below. The program variables indicate that almost 85 percent of the children attending a three-year upper secondary school program chose a theoretical program (natural science, social science, business, technical science, humanities).

Table 2 Descriptive statistics, schooling variables

Variables	Min	Max	Mean	Standard deviation
GPA, primary school	1	5	3.643	0.521
GPA, upper secondary school	1	5	3.352	0.631
GPA rank	1	189	29.147	28.579
Natural science program	0	1	0.132	0.338
Social science program	0	1	0.208	0.406
Business program	0	1	0.248	0.432
Technical science program	0	1	0.197	0.398
Humanities program	0	1	0.059	0.235
Non-theoretical program	0	1	0.156	0.362

Note: The program variables are dummy variables equal to one if the child attended the specific program during upper secondary school, and zero otherwise. The complete dataset contains information on 35,550 children.

2.3 The empirical specification

The empirical method builds on the idea that primary school GPA can be used to control for family and individual heterogeneity. The starting point for the econometric analysis will be the following model:

$$r_{ispt} = \alpha_j + \theta r_{isp,t-1} + \delta U_i + \beta X_{it} + \varepsilon_{it}, \qquad (1)$$

where the dependent variable is the GPA rank of individual *i*, within upper secondary school *s* and program *p* at time *t*, in this case 1993 when the child completes upper secondary school. θ is the coefficient on the corresponding GPA rank upon the completion of primary school, α_j captures municipality-specific factors, δ is the coefficient capturing the unemployment effect where U_i is a dummy variable taking the value of one if the child experienced parental unemployment during upper secondary school. X_{it} is a vector of control variables and ε_{it} is an individual-specific error term. The coefficients will have the interpretation of the change in ranking position due to a change in the control variables. Since the ranking is constructed such that the higher the ranking, the better the GPA, a positive coefficient indicates a GPA improvement and vice versa.

The *Swedish National Labour Market Administration* has provided information on the parents' employment status. I have information on all unemployment spells of the parents during 1990-1993, i.e. the period in question. I have used this information to construct a dummy variable capturing the event of an unemployment spell in the family, which will be equal to one if at least one of the parents has been unemployed at least once during the period when the child attends upper secondary school, and zero otherwise.

The control variables in eq. (1) can be considered as either characterizing a continuous change or an incident during upper secondary school. Because of less than clear-cut evidence on the effect of parental separation on children's school performance, I will include parental separation as a control variable in some specifications to avoid a potential source of omitted variable bias.[65]

[65] Using Swedish data, Björklund and Sundström (2002) find no impact of parental separation during childhood on the educational outcomes as adults when using a sibling approach to control for unobservable family characteristics. This contrasts with findings of Jonsson and Gähler (1997) who also use Swedish data and find that parental divorce has an impact on primary school grades at the age of 16. While Björklund and Sundström (2002) estimate a long-run effect of parental separation, Jonsson and Gähler (1997) estimate a short-run effect; hence the differing results could be an indication of a distinction between the temporary and permanent effects of parental separation on educational outcomes.

Changes in family structure are recorded in LOUISE. In this study, I will only consider a separation of the child's biological/adoptive parents as a divorce. However, the parents do not need to be married prior to the separation; they only need to be cohabitant.[66] The separation variable will therefore be a dummy variable equal to one if there has been a separation in the family, and zero otherwise.

Another event that could possibly influence the school performance of a child is if the family moves and the child has to change schools. I have information on which school the child attends when completing primary school and upper secondary school. However, since all children change schools when starting upper secondary school, it cannot be determined through school-codes whether the child has changed schools during upper secondary school. Hence, I use information, from LOUISE, on which municipality the family resides in each year.[67] Although the child could have changed schools without moving to a new municipality, this is the best indication of changing schools to which I have access. A more detailed geographical classification is that of parishes. However, parishes are often so small that it is very likely that the child does not have to change schools even if the family moves to a new parish. Moving to a new municipality does almost exclusively result in the child having to change schools, however. The variable capturing the incidence of a family moving will be a dummy variable equal to one if the family has moved to a new municipality during upper secondary school, and zero otherwise.

Other issues that could influence the school performance of children are the economic situation of the family, as well as how much the child must compete for parental attention. To control for such issues, I have collected information, also from LOUISE, on social assistance, the evolution of family disposable income as well as the change in the number of children living at home during the relevant three years.[68] Social assistance is characterized by a dummy variable equal to one if the family received social assistance during the three-year period, and zero otherwise. The other variables are differences occurring between 1990 and 1993.

---

[66] A separation must be considered as distressing for a child regardless of whether his/her parents were married prior to the separation.

[67] In 1990, there were 284 municipalities in Sweden. In 1992, two additional municipalities were formed but I use the former classification.

[68] The measures of disposable income and social assistance do not take family size into account. However, I indirectly control for family size through the divorce variable and the number of children living at home. The number of children living at home is counted as the number of children living in the family in addition to the child I am investigating.

It should be noted that both parental unemployment and separation could affect other control variables such as disposable income or social assistance of the family. It must also be considered that the probability of a child experiencing parental unemployment could increase if the child lives with both parents. This would induce a correlation between the incidence of unemployment and separation possibly influencing the results. I will take this into consideration by including an interaction between the unemployment and separation variables as well as by separating the effect of unemployment between mothers and fathers.

Descriptive statistics on the control variables are shown in Table 3. During the period I have chosen to investigate, there was significant turbulence in the Swedish labor market and this volatility is clearly visible in the data. Nearly twenty percent of the children experienced at least one of their parents becoming unemployed. From eq. (1), it is obvious that the observations that will identify the estimates are those where a change has taken place, i.e. those observations determining the coefficient on parental unemployment are the children having experienced an unemployment spell in their family. Since I cannot prove that this group is representative of the population, the results are not general to the whole population; it is an advantage, though, that this group is large. It is also an advantage that the variation in Swedish unemployment can be traced to macroeconomic events, which are exogenous to the individual. To consider the measured effects as causal effects, unemployment must be assumed to be randomly assigned. Although this is a strict assumption, the link to macroeconomic events probably makes it less unrealistic than during other periods. This issue will be discussed in more detail below.

Table 3 Descriptive statistics, control variables

Variables	Min	Max	Mean	Standard deviation
Unemployment	0	1	0.193	0.395
Separation	0	1	0.016	0.126
Move, new municipality	0	1	0.017	0.130
Social assistance	0	1	0.098	0.298
Disposable income	-3.118	17.587	-0.008	0.101
Children	-4	3	-0.191	0.477

Note: Unemployment, separation, the move-variable and social assistance are all dummy variables. Disposable income and the number of children are the differences in these variables occurring between 1990 and 1993. Disposable income is calculated for the family in basic amounts in 1990 SEK. Children is the number of children living at home in addition to the child I am investigating. The complete dataset contains information on 35,550 children.

Table 3 also reveals that the incidence of separation is low, and that I must therefore be careful in drawing any strong conclusions on the effect of a separation on children's school performance. It is not surprising that so few children experience a separation in my sample, since most separations occur when children are younger.[69] In the separation measure above, I have only taken separations of biological/adoptive parents into account. It could, however, be argued that a child would also suffer from a separation from a stepparent and taking such separations into account could potentially increase the group of children having experienced a separation during the period, thereby giving a more reliable estimate. It turns out, however, that considering all separations only increases the group from 1.6 to 2 percent of the full sample. The group of children experiencing a move to a new municipality is also small and calls for the same caution regarding the estimated effects as in the case of separation. Further, nearly ten percent of the children lived in families that received social assistance during the period in question, and family disposable income marginally decreased as well as the number of children living in these families.

In addition to the control variables mentioned above, all specifications will include information on child and parental nationality, as well as parental education. Although I include primary school GPA to control for individual and family heterogeneity, eq. (1) is best characterized as a cross-section. Hence, to minimize the risk of omitted variable bias, I include what I view as

[69] Suppose that the oldest child in a family has started upper secondary school, then the parents must have stayed together for at least seventeen years and the number of separations can be assumed to decrease with the length of the relationship.

important family and child characteristics in the specification.[70] The nationality of the child is captured through a dummy variable equal to one if the child was not born in Sweden, and zero otherwise. Two dummy variables capture parental nationality, one that is equal to one if at least one of the parents were born outside Sweden but within Europe. The other dummy-variable is equal to one if at least one of the parents were born outside Europe. Hence, if one of the parents was born outside Sweden but within Europe and the other was born outside Europe, both dummy variables are equal to one. The variables capturing parental education are dummy variables equal to one, if at least one of the parents has achieved the relevant level of education, secondary or university education. Secondary school indicates up to four years of upper secondary school and university indicates at least some studies at the university after upper secondary school.

The error term in eq. (1) captures, among other things, the ability and motivation of the child. If the motivation of a child is correlated with incidents occurring during upper secondary school for which I do not control, I could have a specification bias problem. Naturally, there are incidents that could occur during upper secondary school and influence the child's grades for which I am not able to control. Examples of such factors are alcoholism in the family, a parent being incarcerated or if the child starts socializing with the wrong crowd. Ultimately, I have to hope that these omitted influences are rare enough so as not to create significant problems.

Another specification issue that deserves some attention, is that of endogeneity stemming from reverse causation. If a child has difficulties in school and this influences the parents in such a way that they start neglecting their work, eventually leading to unemployment, this would lead me to estimates exaggerating the impact of unemployment on children's school performance. This situation, seems very far-fetched, however, and I believe such a problem to be a second-order issue.

Many studies use a sibling-difference approach when studying the impact of family characteristics on youth outcomes. The main advantage of these studies is that any omitted variables describing the children's family situation that are stable over time are shared by all siblings and hence, cancelled out of the equation. A disadvantage of the sibling approach is that it does not control for within-family heterogeneity, i.e. the approach assumes that all siblings are raised identically, which is often not the case.[71] In my study, the upbringing

[70] The family and child characteristics are presented in Table 1.

[71] Holmlund (2004) finds that within-family heterogeneity biases her basic sibling-approach estimates when estimating labor market consequences of teenage childbearing.

will, to a large extent, be captured by the pre-incident youth outcome in terms of primary school GPA. Further, the sibling-approach will have the same problems with time-variant omitted variables as the approach used in this paper.[72]

3 Results

3.1 Basic results

As mentioned above, the inclusion of some of the control variables could influence the coefficient of primary interest, i.e. that on parental unemployment. For example, families suffering from unemployment have, on average, lower incomes; hence, the disposable income variable might capture part of the "unemployment-effect", and vice versa. Table 4 presents the results from four specifications where I include different sets of control variables. Specification (1) disregards the separation and income variables, specification (2) includes separation, specification (3) extends the analysis by including both family disposable income and social assistance and specification (4) is an expansion of specification (3), including an interaction between parental unemployment and separation. The step-wise extension of the specification will give some guidance to the severity of the correlation issues discussed above. In addition to the control variables mentioned above, all specifications include primary school GPA ranking and information on whether the child has moved during upper secondary school, the change in the number of children living in the family, child and parental nationality and parental education.

As can be seen, the coefficient on parental unemployment is influenced by the inclusion of both the separation and the income variables. The coefficient is negative in the first two specifications, and then turns positive. It is, however, insignificant in all specifications. A possible explanation for the lack of an effect of parental unemployment on the school performance of the children concerned could be that the effect of parental unemployment depends on whether it is the mother or the father who is unemployed. I will consider this possibility below.

[72] For a discussion of the sibling-estimator, see Ermisch & Francesconi (2001).

Table 4 Basic results of unemployment impact on school ranking

Specification	(1)	(2)	(3)	(4)
GPA rank, primary school	0.817***	0.817***	0.816***	0.816***
	(0.005)	(0.005)	(0.005)	(0.003)
Unemployment	-0.012	-0.004	0.153	0.165
	(0.213)	(0.213)	(0.214)	(0.214)
Separation	-	-0.992	-0.881	-0.724
	-	(0.657)	(0.656)	(0.763)
Move	-0.829	-0.796	-0.717	-0.714
	(0.603)	(0.604)	(0.604)	(0.640)
Disposable income	-	-	0.225	0.228
	-	-	(0.346)	(0.806)
Social assistance	-	-	-2.068***	-2.067***
	-	-	(0.278)	(0.286)
Children	0.105	0.090	0.074	0.074
	(0.173)	(0.174)	(0.174)	(0.172)
Interaction	-	-	-	-0.579
	-	-	-	(1.462)
Child nationality, not Swedish	-1.838***	-1.846***	-1.651***	-1.651***
	(0.462)	(0.462)	(0.463)	(0.487)
Parental nationality:				
Europe	0.043	0.048	0.142	0.141
	(0.272)	(0.272)	(0.273)	(0.274)
Outside Europe	-0.510	-0.504	-0.115	-0.115
	(0.667)	(0.667)	(0.665)	(0.653)
Parental education:				
Secondary	0.844***	0.848***	0.725***	0.726***
	(0.265)	(0.265)	(0.265)	(0.279)
University	2.916***	2.921***	2.701***	2.701***
	(0.272)	(0.272)	(0.274)	(0.283)

Note: Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a municipality-specific effect. The complete panel consists of 35,500 observations. *** denotes significance at the one-percent level.

The coefficients on the separation and move variables are, as anticipated, negative although insignificant. Disposable income also displays an insignificant coefficient with the expected positive sign. On the other hand, the dummy variable capturing whether the family has received social assistance during the period exhibits a negative coefficient significant at the one-percent level. According to my results, a child living in a family receiving social assistance during upper secondary school loses two GPA ranking positions. It should be kept in mind that the ranking is constructed such that the higher the ranking, the better the GPA. The loss of two ranking positions should be compared to the mean rank of 29.1, implying that the effect is quite small. Further, the coefficient on the number of children is positive, which is non-intuitive but insignificant.[73]

Considering the background variables, it is evident that being born outside Sweden has a negative effect on the GPA ranking. It could be expected that such an effect would be taken into consideration through the primary school GPA. However, it is not unlikely that the disadvantages of being born in another country become more evident at a higher, and more demanding, educational level. Parental nationality does not seem to have any additional effect during upper secondary school,[74] while parental education does. The effect of having parents with a low education could certainly become more evident when attending upper secondary school through lower parental understanding and support. It remains to be seen how robust these basic results are when I consider which parent is unemployed and the length of the unemployment spell.

3.2 Disentangling the unemployment effect

So far, I have investigated whether there is an effect of at least one parent having at least one unemployment spell during the period when their child attends upper secondary school, and the results have been insignificant. It could, however, be imagined that the effect would be different, depending on whether the mother or the father was the one suffering from unemployment, and depending on the length of the unemployment spell. In all likelihood, long-term employment creates more distress than short-term unemployment. Moreover, the effect could differ between men and women, simply because they react differently to becoming unemployed. Eliason and Storrie (2004) find

[73] It could be the case that the number of children living in a family is not linearly related to the school performance of the children. However, different specifications yield similar results.

[74] Considering more narrow geographical classifications does not yield more significant results.

that losing your job shortens the life of men but not of women. One of their explanations is that women can cope better when becoming unemployed because of their more developed social networks outside the workplace. Coping better could imply being able to use your new extra time to do something productive, such as spending quality time with your children. Suffering greatly from being unemployed almost certainly makes it difficult to be a positive influence on the surroundings. Separating the effect by gender also helps reducing the problem with a potentially higher probability of experiencing parental unemployment for children living with both parents.

To investigate whether the effect of unemployment on children's school performance depends on whether it is the mother or the father who has been unemployed, I include dummy-variables capturing the separate events. If both the mother and the father have been unemployed during the period, both dummy-variables will be equal to one. The length of the unemployment spell is also captured by dummy-variables. I construct four dummy-variables capturing whether the child has had a short-term unemployed mother, a long-term unemployed mother, a short-term unemployed father or a long-term unemployed father. Hence, at most two of these dummy-variables can equal one for one specific child. Table 5 displays the descriptive statistics on the new unemployment variables.

Table 5 Descriptive statistics, unemployment variables

Variables	Min	Max	Mean	Standard deviation
Unemployment, mother	0	1	0.129	0.335
Unemployment, father	0	1	0.081	0.272
Short-term unempl. mother	0	1	0.080	0.272
Long-term unempl. mother	0	1	0.049	0.214
Short-term unempl. father	0	1	0.054	0.226
Long-term unempl. father	0	1	0.026	0.161

Note: All variables are dummy variables. As long-run unemployment, I count unemployment spells lasting more than one year. The complete dataset contains information on 35,550 children.

The first column in Table 6 repeats the coefficient from specification (3) in Table 4 and the last two columns present the results from two new specifications replicating specification (3) in Table 4, but exchanging the unemployment variable with the new dummy variables separating the unemployment effect. Interestingly, the coefficient on the mother being unemployed is positive, which could be interpreted as the positive effect of having extra time on your hands exceeding the negative effects of the disadvantages caused by unemployment. The estimates suggest that having a short-term unemployed mother during upper secondary school improves the child's GPA rank by 0.8 positions and having a long-term unemployed mother improves the GPA rank by 0.9 positions, small but precisely estimated effects. The positive effect increasing with the length of the unemployment spell most likely captures the effect of the mother having even more time to spend with her children when she is unemployed for a long period of time.

Table 6 Investigating the effect of parental unemployment

Specification	Base-specification	Mother/father	Short-term/Long-term
Unemployment	0.153	-	-
	(0.214)	-	-
Mother	-	0.820***	-
	-	(0.253)	-
Father	-	-0.950***	-
	-	(0.302)	-
Mother short-term	-	-	0.779**
	-	-	(0.319)
Mother, long-term	-	-	0.890**
	-	-	(0.374)
Father, short-term	-	-	-1.176***
	-	-	(0.361)
Father, long-term	-	-	-0.485
	-	-	(0.513)

Note: Base-specification is column (3) in Table 4. As long-run unemployment, I count unemployment spells lasting more than one year. Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a municipality-specific effect and the same control variables as the base-specification. Estimates on the control variables are shown in Appendix 1. The complete panel consists of 35,550 observations. *** and ** denote significance at the one and five percent level, respectively.

Having an unemployed father, however, seems to have a negative effect on children's school performance. The estimates suggest that having a short-term unemployed father during upper secondary school decreases the child's GPA rank by 1.2 positions. However, the effect of having a long-term unemployed father is not measured with statistical significance. These results could be an indication of the fact that women actually do cope better with being unemployed than men and are able to use their extra time in a productive manner, while the unemployment of the father is predominantly destructive.

The fact that long-term parental unemployment has a less clear effect could be interpreted as the shock of unemployment wearing out.

It can be imagined that the positive effect of having an unemployed mother might partly depend on her educational level. Quality time with a parent could have a lower value in an educational respect if the parent is low-skilled. To investigate whether paternal education has an effect in this respect, I included interactions between parental unemployment and educational level in the specification presented in column 2 above. However, I find no evidence of the positive effect of maternal unemployment depending on the educational level of the mother. Neither is the negative effect of paternal unemployment altered by such interactions. To return to the correlation issue between parental unemployment and the income variables discussed above, the results presented in the last two columns of Table 6 are robust to a step-wise expansion of the specification.[75]

4 Sensitivity analysis

4.1 The unemployment sample

As mentioned above, the observations determining the coefficient on parental unemployment are the children having experienced an unemployment spell in their family. For the results in this paper to be considered as general, I would have to prove that this sub-sample is representative. I already stated that it is an advantage that this group is large, and that the variation in unemployment during this period can be traced to macroeconomic events, but it is also of interest to consider more detailed characteristics of the sub-sample, as compared to the full sample, to determine whether the unemployment spells during the period can be considered as randomly assigned, i.e. unrelated to family characteristics and the pre-incident youth outcome.

[75] To appreciate the influence of omitted variables bias, I gradually extend the number of control variables while observing the estimate on parental unemployment. It turns out that the coefficients on maternal and paternal unemployment are robust to the inclusion of other control variables, such as social assistance and disposable income and thus, we believe the correlation problem to be a less serious issue.

Table 7 Characteristics of children with unemployed parents

	Children, full sample	Children, u-sample	Mother, full sample	Mother, u-sample	Father, full sample	Father, u-sample
Primary GPA	3.643	3.560				
Female	0.497	0.476				
Country of birth:						
Sweden	0.963	0.937	0.900	0.870	0.758	0.751
Nordic	0.006	0.010	0.046	0.056	0.028	0.036
Europe	0.008	0.016	0.030	0.034	0.033	0.039
Outside E	0.023	0.037	0.016	0.033	0.014	0.027
Missing obs.	0	0	0.008	0.007	0.167	0.147
Highest level of completed education:						
Primary			0.175	0.227	0.187	0.241
Secondary			0.443	0.522	0.351	0.395
University			0.348	0.215	0.273	0.194
Missing obs.			0.034	0.036	0.189	0.170

Note: U-sample is the children having experienced an unemployment spell in their family; the remaining columns are taken from Tables 1 and 2. GPA from primary school is mean GPA. Outside E is outside Europe. The complete dataset contains information on 35,550 children, 6,862 of whom experienced parental unemployment between 1990 and 1993.

Table 7 presents descriptive statistics on the unemployment sub-sample, along with the statistics from Table 1 and 2 on the full sample. As can be seen, the unemployment sample exhibits a primary school GPA, which is on average only marginally lower than that in the full sample. There are also relatively small differences in the number of females and the country of birth for both the children and the parents. It is, however, clear that the parents in the unemployment sub-sample are in general less educated.[76] This comes as no surprise, since unemployment rates are generally higher among low-skilled workers. This pattern would, however, most likely be even more evident with data from another time period when unemployment rates were lower. Finally, it is worth noting that the number of missing observations on the characteristics of the parents is, somewhat surprisingly, marginally smaller for the unemployment sample. These statistics imply that the unemployment sample is not entirely representative. Hence, my results cannot be considered as representative for the whole population.

[76] These differences are statistically significant.

4.2 The excluded children

As mentioned above I have, due to heterogeneity, excluded all children not choosing to attend a three-year upper secondary school program directly after primary school and actually managing to complete the program in three years. This means that I have excluded children choosing to attend a shorter upper secondary school program and children choosing to postpone their upper secondary school for some reason, or who simply drop out of school. It could be imagined that the children I have excluded might have high probabilities of living in a problematic family. For example, it is very likely that the parents of the excluded children are less educated than the parents of the children choosing a three-year program and consequently, have higher probabilities of becoming unemployed.

In total, there were 109,392 children completing primary school in 1990. Out of these, there were 53,000 (48.4 %) children completing upper secondary school in 1993, 31,384 (28.7 %) attending a shorter upper secondary school program which they completed before 1993 and 8,534 (7.8 %) children completing some kind of upper secondary school program during the period 1994 to 1999. 16,474 children (15.1 %) had still not completed an upper secondary school program in 1999, here called dropouts. For 11,700 of the 53,000 children completing upper secondary school in 1993, I have missing observations on key-variables, here called the missing observations group. The missing observations are mainly on the disposable income of the family or the family structure (whether the child lives with both his biological parents). Table 8 contains comparable descriptive statistics for the groups of children discussed above.

Table 8 Comparing children and family characteristics

	Children					Mother					Father				
	Sample	Before 1993	1994-1999	Drop-outs	Missing obs.	Sample	Before 1993	1994-1999	Drop-puts	Missing obs.	Sample	Before 1993	1994-1999	Drop-outs	Missing obs.
Female	0.497	0.441	0.472	0.479	0.564										
Country of birth:															
Sweden	0.963	0.953	0.898	0.890	0.892	0.900	0.847	0.801	0.741	0.683	0.758	0.763	0.708	0.618	0.666
Nordic countries	0.006	0.009	0.011	0.015	0.010	0.046	0.058	0.050	0.069	0.043	0.028	0.036	0.024	0.040	0.027
Europe	0.008	0.006	0.015	0.014	0.011	0.030	0.024	0.034	0.033	0.025	0.033	0.022	0.030	0.024	0.025
Outside Europe	0.023	0.026	0.060	0.060	0.035	0.016	0.015	0.041	0.047	0.020	0.014	0.014	0.036	0.041	0.019
Missing obs.	0	0.005	0.015	0.022	0.052	0.008	0.058	0.074	0.110	0.230	0.167	0.166	0.198	0.277	0.264
Education:															
Primary						0.175	0.303	0.172	0.338	0.157	0.187	0.311	0.176	0.303	0.178
Secondary						0.443	0.492	0.397	0.411	0.367	0.351	0.410	0.341	0.323	0.337
University						0.348	0.145	0.351	0.134	0.244	0.273	0.111	0.281	0.091	0.218
Missing obs.						0.034	0.060	0.079	0.118	0.232	0.189	0.168	0.202	0.282	0.267

Note: Sample refers to the full sample I use in this study, before 1993 refers to the children attending a shorter upper secondary school program which they completed before 1993, 1994-1999 refers to the children completing some kind of upper secondary school program between 1994 and 1999, drop-outs refers to the children who had still not completed an upper secondary school program in 1999 and missing observations refers to the children that completed a three-year upper secondary school program in 1993 displaying missing observations on key-variables. Primary school indicates compulsory school for 10 years or less, secondary school indicates up to four years of upper secondary school and university indicates at least some studies at the university after upper secondary school.

Worth noting is that the number of missing observations on the parents' characteristics is larger for all other groups as compared to the one I study (however, only marginally so for the group of children completing upper secondary school before 1993). As commented above, these missing observations to a large extent stem from the fact that information on both parents is only included when children live in the same household as both parents. Hence, more missing observations on the parents' characteristics most likely indicate that a larger proportion of these children have divorced parents when completing primary school as compared to the children I study. The proportion of missing observations on the characteristics of the father is the largest among the dropouts and for mothers, there are most missing observations in the group I have excluded because of many missing observations on key-variables.

Further, there seems to be a smaller proportion of children born abroad among the children choosing to go through with an upper secondary school program directly after primary school (i.e. those completing upper secondary school before 1994). The same pattern seems to hold for parental nationality. Considering parental education, the groups completing a shorter upper secondary school before 1993 and the dropouts have the least educated parents. The remaining groups have similarly educated parents.

It seems that the children in the group I study live in families with relatively highly educated parents, and that the proportion of children living in divorced families is probably smaller than in the other groups. Therefore, it can only be assumed that my estimated effects of parental unemployment on children's school performance hold for children living in relatively stable families. To make any suggestions regarding these effects for children in more problematic families, this group must be investigated separately. However, it can be imagined that children living in more problematic families would, to an even larger extent, be affected by additional strains on the family.

5 Conclusions

As far as I am aware, this is the first paper investigating the effect of parental unemployment on children's school performance. The empirical method builds on the idea that primary school GPA can be used to control for family and individual heterogeneity. I use data on children completing primary school in 1990 and thereafter directly continuing with three years of upper secondary school.

My main results can be summarized as follows. If a mother is subjected to an unemployment spell during the period when one of her children attends upper secondary school, the school performance of the child marginally improves. This implies that the positive effect of having extra time on your hands exceeds the negative effects of the disadvantages caused by unemployment. The positive effect of having an unemployed mother seems to increase with the length of the unemployment spell. However, having a short-term unemployed father has a negative effect on a child's school performance, while the effect is insignificant for long-term paternal unemployment. One explanation for the differing results across genders could be that women in general cope better with being unemployed and hence, are able to use their new extra time doing something productive, such as spending quality time with their children. The fact that a long-term unemployment spell of the father has a less clear effect could be interpreted as the shock of unemployment wearing out.

Given that this is the first paper analyzing the relationship between parental unemployment and children's school performance, it seems appropriate to mention issues that have not been addressed in this paper as suggestions for future research. To generalize the effect of parental unemployment on children's school performance, one would need to consider the relationship for children choosing to never attend upper secondary school, dropping out of upper secondary school or attending a shorter upper secondary school program. It would also be interesting to consider the relationship between parental unemployment and children's school performance during a period with lower unemployment rates. As discussed earlier, it could be the case that being unemployed when unemployment rates are low is associated with higher social and psychological costs of being unemployed, making it more difficult to use your new extra time to help your children.

References

Amato, P R and B Keith (1991), Parental divorce and adult well-being: A meta-analysis, *Journal of Marriage and the Family* 55, 23-41.

Björklund, A and T Eriksson (1998), Unemployment and mental health: evidence from research in the Nordic countries, *Scandinavian Journal of Social Welfare* 7, 219-235.

Björklund, A and M Sundström (2002), Parental separation and children's educational attainment: A siblings approach, Discussion paper 643, IZA, Bonn.

Cherlin, A J (1999), Going to the extremes: Family structure, children's well-being and social science, *Demography* 36(4), 421-428.

Coughlin C and S Vuchinich (1996), Family experience in preadolescence and the development of male delinquency, *Journal of Marriage and the Family* 58(2), 491-501.

Eliason, M and D Storrie (2004), The echo of job displacement, Working paper 135, Department of Economics, School of Economics and Commercial Law, Göteborg University.

Ermisch, J F and M Francesconi (2001), Family structure and children's achievements, *Journal of Population Economics* 14(2), 249-270.

Hoffman J P and R A Johnson (1998), A national portrait of family structure and adolescent drug use, *Journal of Marriage and the Family* 60(3), 633-645.

Holmlund, H (2004), Estimating long-term consequences of teenage childbearing – A test of the sibling approach, *The Journal of Human Resources*, forthcoming.

Jonsson, O and M Gähler (1997), Family dissolution, family reconstitution, and children's educational careers: Recent evidence from Sweden, *Demography* 34(2), 277-293.

Lindbeck, A (1997), The Swedish experiment, *Journal of Economic Literature* 35, 1273-1319.

Manski, C F, G D Sandefur, S McLanahan and D Powers (1992), Alternative estimates of the effect of family structure during adolescence on high school graduation, *Journal of American Statistical Association* vol. 87 No. 417, 25-37.

Micklewright, J, M Pearson and S Smith (1990), Unemployment and early school leaving, *The Economic Journal* vol. 100 No 400, 163-169.

Nilsson A and J Agell (2005), Crime, unemployment and labor market programs in turbulent times, Essay I in this thesis.

Wikström, C and M Wikström (2004), Grade inflation and school competition: An empirical analysis based on Swedish upper secondary schools, *Economics of Education Review* 24(3), 309-322.

Åberg, Y, P Hedström and A-S Kolm (2003), Social interactions and unemployment, Working paper 2003:18, Department of Economics, Uppsala University.

Appendix

A.1 Results for control variables in Table 6

Table A1 Results for control variables corresponding to the results in Table 6.

Specification	Mother/father	Short-term/Long-term
GPA rank, primary school	0.816***	0.816***
	(0.005)	(0.005)
Separation	-0.919	-0.916
	(0.656)	(0.656)
Move	-0.804	-0.800
	(0.603)	(0.603)
Disposable income	0.200	0.198
	(0.333)	(0.332)
Social assistance	-2.091***	-2.099***
	(0.279)	(0.279)
Children	0.077	0.069
	(0.174)	(0.174)
Child nationality, not Swedish	-1.669***	-1.677***
	(0.463)	(0.462)
Parental nationality:		
Europe	0.164	0.162
	(0.273)	(0.273)
Outside Europe	-0.094	-0.122
	(0.666)	(0.665)
Parental education:		
Secondary	0.738***	0.740***
	(0.265)	(0.265)
University	2.724***	2.727***
	(0.274)	(0.273)

Note: Standard errors are in parenthesis. All standard errors are robust to heteroscedasticity. In addition to the variables shown in the table, all regressions include a municipality-specific effect. The complete panel consists of 35,500 observations. ***, ** and * denote significance at the one, five and ten percent level, respectively.

Publication series published by the Institute for Labour Market Policy Evaluation (IFAU) – latest issues

Rapporter/Reports

2005:1 Ahlin Åsa & Eva Mörk ”Vad hände med resurserna när den svenska skolan decentraliserades?”

2005:2 Söderström Martin & Roope Uusitalo ”Vad innebar införandet av fritt skolval i Stockholm för segregeringen i skolan?”

2005:3 Fredriksson Peter & Olof Åslund ”Påverkas socialbidragsberoende av omgivningen?”

2005:4 Ulander-Wänman Carin ”Varslad, uppsagd, återanställd. Företrädesrätt till återanställning enligt 25 § LAS i praktisk tillämpning”

2005:5 Isacsson Gunnar ”Finns det en skillnad mellan samhällets och individens avkastning på utbildning?”

2005:6 Andersson Christian & Iida Häkkinen “En utvärdering av personalförstärkningar i grundskolan”

2005:7 Hesselius Patrik, Per Johansson & Laura Larsson ”Hur påverkar kravet på läkarintyg sjukfrånvaron? Erfarenheter från ett socialt experiment”

2005:8 van den Berg J & Bas van der Klaauw “Job search monitoring and sanctions – a brief survey of some recent results”

2005:9 Sibbmark Kristina & Anders Forslund ”Kommunala arbetsmarknadsinsatser riktade till ungdomar mellan 18 och 24 år”

2005:10 Lindqvist Linus, Laura Larsson & Oskar Nordström Skans ”Friårets arbetsmarknadseffekter”

2005:11 Hjertner Thorén Katarina ”Kommunal aktiveringspolitik: en fallstudie av det praktiska arbetet med arbetslösa socialbidragstagare”

2005:12 Gartell Marie & Håkan Regnér ”Sambandet mellan val av högskola och inkomster efter examen för kvinnor och män”

2005:13 Kennerberg Louise & Kristina Sibbmark ”Vilka deltar i svenska för invandrare?”

2005:14 Sibbmark Kristina & Caroline Runeson ”Arbetsmarknadspolitisk översikt 2004”

Working Papers

2005:1 Ericson Thomas “Personnel training: a theoretical and empirical review”

2005:2 Lundin Martin “Does cooperation improve implementation? Central-local government relations in active labour market policy in Sweden”

2005:3 Carneiro Pedro, James J Heckman & Dimitriy V Masterov “Labor market discrimination and racial differences in premarket factors”

2005:4 de Luna Xavier & Ingeborg Waernbaum “Covariate selection for non-parametric estimation of treatment effects”

2005:5 Ahlin Åsa & Eva Mörk “Effects of decentralization on school resources”

2005:6 Cunha Flavio, James J Heckman & Salvador Navarro “Separating uncertainty from heterogeneity in life cycle earnings”

2005:7 Söderström Martin & Roope Uusitalo “School choice and segregation: evidence from an admission reform”

2005:8 Åslund Olof & Peter Fredriksson “Ethnic enclaves and welfare cultures – quasiexperimental evidence”

2005:9 van der Klaauw Bas, Aico van Vuuren & Peter Berkhout “Labor market prospects, search intensity and the transition from college to work”

2005:10 Isacsson Gunnar “External effects of education on earnings: Swedish evidence using matched employee-establishment data”

2005:11 Abbring Jaap H & Gerard J van den Berg “Social experiments and instrumental variables with duration outcomes”

2005:12 Åslund Olof & Oskar Nordström Skans “Measuring conditional segregation: methods and empirical examples”

2005:13 Fredriksson Peter & Bertil Holmlund “Optimal unemployment insurance design: time limits, monitoring, or workfare?”

2005:14 Johansson Per & Per Skedinger “Are objective measures of disability reliable?”

2005:15 Hesselius Patrik, Per Johansson & Laura Larsson “Monitoring sickness insurance claimants: evidence from a social experiment”

2005:16 Zetterberg Johnny “Swedish evidence on the impact of cognitive and non-cognitive ability on earnings – an extended pre-market factor approach”

2005:17 Nordström Skans Oskar & Linus Lindqvist “Causal effects of subsidized career breaks”

2005:18 Larsson Laura, Linus Lindqvist & Oskar Nordström Skans “Stepping-stones or dead-ends? An analysis of Swedish replacement contracts”

2005:19 Dahlberg Matz & Magnus Gustavsson “Inequality and crime: separating the effects of permanent and transitory income”

2005:20 Hjertner Thorén Katarina "Municipal activation policy: A case study of the practical work with unemployed social assistance recipients"

2005:21 Edin Per-Anders & Magnus Gustavsson "Time out of work and skill depreciation"

Dissertation Series

2005:1 Nilsson Anna "Indirect effects of unemployment and low earnings: crime and children's school performance"

2003:1 Andersson Fredrik "Causes and labor market consequences of producer heterogeneity"

2003:2 Ekström Erika "Essays on inequality and education"