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Youth Unemployment and Crime

New Evidence from the 21th century

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Abstract

In this paper we investigate the effect of youth unemployment on crime. We draw on newly found evidence showing a weak link between traditional theory of the income channel of crime and unemployment applied to young people. Stressing the importance of idle time available to commit crime, we estimate the effect of unemployment using instrumental regression and the policy changes in labor market programs following the Swedish election of 2006. Using monthly data from 21 Swedish counties we find a strong link between youth unemployment and crime during the turbulent period between 2007 and 2011. We show that the definition of unemployment matters and that there is a need to take into account different labor market programs when estimating the effect of unemployment using aggregated data.

Keywords: Unemployment, Crime, Youth Unemployment, Labor market programs

JEL Codes: E2, J2, J6, J43, H3

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1 Introduction

The event of the financial crisis in 2008 exposed the Swedish economy to several challenges. The GDP growth slowed down and Sweden experienced negative growth for the first time since the crisis in the 90´s. This contributed to an increase of the unemployment rate by about three percentage points between 2008 and 2010. The development for youth unemployment has not been so benign. Looking at the raw data we see that the number of individuals between 18 and 24 years of age either in open unemployment or in labor market programs rose from a trough of 31 866 in 2007 to a peak of 100 260 in 2010. A three folded increase.

This paper aims to investigate the effect of youth unemployment on crime, using newly gathered data from the 21th century. The purpose is to shed some light on the conflicting results from earlier studies done on data from Sweden. We will investigate the unemployment effect using changes in the policies of labor market programs. By doing this, we hope provide a better understanding of how youth unemployment affects different crime categories, but also what effects labor market programs have on crime.

The statement that unemployment brings about criminal activity is fundamentally appealing and can be modeled on individuals responding to incentives. According to Raphael and Winter-Ebmer (2001), criminal activity can be defined as a type of employment that requires time and generates income. A person who violates the justice system should compare returns on time spent on legal and illegal activities and base their decisions accordingly. This would then imply that a decrease in income and potential earnings associated with unintentional unemployment increases the relative return to criminal activities.

Research done in the area of the effects of unemployment on crime, has for several years been an interesting and hot topic. Among the questions researchers are trying to answer is what relation exist between crime and unemployment and how to prevent and offset these effects. There have been several studies looking in to how these two factors correlate. The most common crime category studied is property crimes, such as burglary and theft, which fit well into an economic framework.

During the recent years of research the most common practice has been to use panel data sets, due to better quality data collected and the increased number of observations offered. This has given the researcher a better opportunity to investigate the crime-unemployment relationship on state or county specific panel data. Among the more recent research on this we have Raphael & Winter-Ebmer (2001) and Doyle, Ahmed & Horn, (1999), who

investigate different crimes on a state-level in the United States. Research conducted by Entorf & Spengler (2000), investigated different economic factors of crime using German states. Papps & Winkelmann (2000) used regional panel data to investigate the relationship of unemployment and crime in New Zealand.

Studies have also been done on a micro level. Witte & Tauchen, (1994) studied adolescents and specifically younger men since they are usually overrepresented in crime statistics. They did this by using panel data on an individual level to study the criminality of young males. They found and concluded that employed individuals commit fewer crimes.

The outline of the thesis is arranged as follows. Following next section 2 is theory and previous research. Section 3 will provide an overview of the job guarantee program and some general definitions on crime. Section 4 the data description. Section 5, the main model used. Section 6 provides further analysis of the results and section 7 at some specific violent crimes. In Section 8 we provide our conclusions.

2 Theory and Previous Research

2.1 Theoretical models

In the early work on the theory of crime the economic models focused on the cost-benefit analysis of committing a crime. Becker (1963) models the supply of offenses as a tradeoff between the cost and the benefit of committing a crime in a rational choice environment. An increase in the risk of and individual being punished would lower the supply of crime by lowering the expected utility of committing a crime. Similarly a rise in income from legal activities would reduce the willingness to engage in illegal activities. In this framework the effect of unemployment should be substantial by lowering income and increasing the benefit of crime. Ehrlich (1973) builds on this model but expands it by including the costs and benefits from both illegal and legal activities. By treating legal and non-legal activities as competing activities he models the supply of crime as an individual's choice of optimal allocation of resources under uncertainty. For the supply-of-offenses function used by Erlich (1973) he applies an econometric analysis to look at how unemployment, labor force participation and the age composition affect the crime rate. The primary age group of focus is the share of young males between 14 and 24 years of age, a group known to often be involved in criminal activities. Worth noting is that using data from 1960, gives no effect of either unemployment, labor market participation or the share of young males gives any conclusive evidence.

Most studies reviewed also uses the wage rate as an explanatory variable to try to capture the economic incentives or motivation behind crimes against property. Doyle et al (1999) discusses the opportunity wage of legal activities and defines it as the income from employment and the income from unemployment benefits. This would then represent the expected income that Becker (1963) proposes in his rational choice model from legal activities. Higher wages should then be expected to lead to lower crime rates. A problem, discussed also by Ehrlich (1973), relates to the use of the average wage-level as an explanatory variable. The wage level might capture the benefits of illegal activities instead of the benefits of legal activities due to higher wages meaning more opportunities for crime since the value of the "loot" is expected to increase.

This problem of simultaneity between wages and crime is also found when looking at unemployment as an explanatory variable and certainly also for sanctions. High unemployment might lead to more crime but a high crime rate might also deter firms from investing and hiring. Similarly, increases in sanctions can be in response to higher crime rates. Increases in the percentage of crimes solved are of course a function of the number of

crimes committed and reported. In an overview of the economic literature of crime Witte et al (2000) points to the effort exerted to find instruments (exogenous factors) to identify the true effects of the explanatory variables on crime. This method is prevalent in most recent studies we will draw upon.

Witte et al (2000) concludes that that the rise in juvenile crime rates, and mixed results of the effect of punishment, have forced economists to expand their view from the rational choice models to incorporate variables such as education and other social factors such as peer group effects and the influence of family and community. The theoretical models are also moving away from the static models to dynamic multi-period models of crime. Lochner (1999) estimates a theoretic model including work, human capital and crime and finds that education, training and work subsidies can reduce criminal activity.

Despite this, much of the empirical research in the field has been using an empirical specification where the supply-of-offenses is based on the one used by Ehrlich (1973).

2.2 Previous empirical results

The literature on crime and unemployment typically use one of four types of data: aggregate (national) time series or cross-section data, regional panel data or individual level data. Literature of the first two types often confirms the existence of a causal relationship. Studies that use the type of aggregate data, try in a varying degree to control for other factors, but these studies are still likely to be affected by omitted variable bias. Panel data on a regional level can improve the problem of omitted variable bias. This type of panel data is less supportive of casual relationship. Individual level data is then used to observe the labor market status of a particular offender at the time of committing a crime. The advantages with this type of data are: the number of observations is large, it provides a large number of controls and possibility to focus on particular sub populations. With the individual level data the most common omitted variable problems can be solved. This would be done by comparing the crime tendency of the same individual in different states of employment. The draw back with individual level panel data is that it is rarely available, and because of that the second best choice would be to use regional or county level panel data.

Doyle, Ahmed & Horn, (1999), used annual state level data from 1982 to 1993 in their investigation of how the role of labor markets, income distribution, and demographic effected property crime. Their choice of focus on property crime was under the assumption that property crime was more likely motivated by financial gain, thus it would have been more likely to be captured by economic variables than violent crimes.

The econometric model applied in Doyle, Ahmed & Horn, (1999) is a fixed effect OLS logarithmic model. The model does solve the problem of unmeasured state specific effects. But the model is unable to solve the simultaneity bias from the chosen explanatory variables. In an attempt to solve this, they apply instrumental variable estimator in the two stage OLS model. In the model of property crime rates, they use four instruments which are violent crimes, weighted average of police officers per capita, per capita personal tax revenue and percentage voting cast for the republican candidate. To use violent crime as a suitable instrument is to assume that criminals do not substitute between property crime and violent crime. The Police officers per capita instrument is used as a geographical instrument to estimate the effect of public capital on productivity of a state. The tax revenue variable was based on the idea that tax revenue ought to be correlated with police officers, but not directly to crime. Their findings were that wages had a significant effect on property crime. Doyle et al (1999) believe that their result support previous studies using aggregate data on city, county, or state level which may underestimate the effect of legal alternatives on crime. The underestimation is due to not being able to properly capture the credible legal alternatives. They also found that males and young people have a significant and sizable effect on property crime but a different effect on violent crime.

A paper by Entorf & Spengler (2000) used data consisting of cross-section time series from 11 German states, during the period 1975 to 1996. The study aims to analyze factors like youth unemployment, demographic changes and income inequality. They approached this analysis by applying the basic specification model by Ehrlich (1973). The model is a logarithmic fixed effects OLS model.

Entorf & Spengler (2000) used several different explanatory variables when analyzing the different factors effecting crime. The data chosen on German states allowed them to exploit the different outcomes in densely populated areas, but also allowed them to look at differences between East and West Germany. The demographic and urban factors could explain why people less than 21 years of age are responsible for 28 % of total offences. By controlling for income and other economic variables, they could test the effect of criminal activity of the young and those unemployed. The factor of relative income was also used to measure the relative wealth that might explain crime based on legal income opportunities. They also looked at the split of overall unemployment and youth unemployment effects. Different types of crimes were used, but the choices of categories were limited to the availability of data. Their results suggested that a higher crime rate is more linked to urban areas and that it is associated with general unemployment. They also found that unemployed youths increased the crime level.

Another article by Papps & Winkelmann (2000), looked at in what way unemployment could relate to crime in New Zealand. They used annual panel data over the period 1984 to 1996, covering sixteen regions. Their crime data consisted of crimes that were reported to the police. The crime categories used in the research were violent crime, drug and anti-social crime, dishonesty crime, property damage, property abuse, sexual offences and administrative crime. The unemployment measure was based on people registered as unemployed at the Department of Labor.

The base model Papps & Winkelmann (2000) used was a general OLS regression based on Ehrlich (1973) log-log model. By testing the model for both random and fixed effect, they found that the use of random effects produce inconsistent estimators whereas fixed effect estimators remained consistent. Region and/or time specific fixed effects would enhanced and improved the specification of the model. They concluded that a two-way fixed effect model should be used in their investigation. The result showed that unemployment have a significant effect on dishonesty crimes committed. This category includes economic crimes like theft, fraud, car conversions and burglary.

To shortly summarize the above research, we find that results on the United States shows strong evidence for the hypothesis that a high unemployment rate has a clear correlation to increased property crimes. The result presented from Papps & Winkelmann (2000) and Entorf & Spengler (2000) shows a weaker result than the American study. These studies show that the level of unemployment only affects certain types of vandalism crime like littering and violation. The paper from Entorf & Spengler (2000) on the West German states gives weak and ambiguous results. Some estimates even show a negative correlation for some types of theft crime. For Germany as a whole the effect is much clearer and throughout positive.

2.3 Previous Research on Sweden

The empirical research of the Swedish unemployment effect on different levels of crime is a rather unexplored area, particularly regarding youth unemployment. Edmark, (2005) uses a panel data set on county level, to investigate if the level of unemployment could be a contributing factor in explaining the level of crime rate in Sweden between the years 1988 to 1999. Edmark follows the theory of economics of crime and focuses primarily on the profitability of property crime. The period of 1988 to 1999 was chosen to be able to reflect an extreme event in the modern history of Sweden. During the 1990s Sweden went in to a deep recession and experienced its worst economic crisis since the 1930s. The rate of unemployment rose sharply and public spending on unemployment benefits soared.

The model that Edmark applied is a fixed effect OLS model. This model is used to measure the relationship between unemployment and crime, with county and time specific effects. The theoretical framework of this model is based on Ehrlich (1973), who used a log-log approach. Edmark included a number of control variables in an extended fixed effect model. The reason for this approach is to decrease the risk that social and demographic factors will distort the results. The choice of such variables is either motivated by previous research or on theoretical grounds.

Edmark found that unemployment had a positive effect on certain property crimes. Significant result was obtained for aggregate crime, burglary and car theft in the model. The crime of bike theft was weakly significant in some but not all model specifications. For the other property crime variables under investigation there were no significant effects found. The estimated coefficients for aggregate property crime, burglary and car theft was robust to a varying set of covariates. Based on this variation in the estimated effect between unemployment on property crime Edmark could conclude that it is not suitable to sum specific crimes to an aggregate measure when the aim is to explain criminal behavior. For violent crimes no significant effect relating to unemployment was found. A conclusion was that higher unemployment does not solely lead to expenses directly related to unemployment, but may also have indirect effects due to increase property crime.

Grönqvist (2011) applied individual level data in his research. He found several significant effects between unemployment and different types of crime. The data covered was for the working age population in Sweden and on an individual level over the period 1985 to 2007. The crime data was aggregated data from convictions in Swedish courts including information of exact date and offence. The advantage of this individual level data is the possibility to decompose the effect of unemployment from several angles. It also has the advantage of eliminating fixed unobserved characteristics of individuals.

The study by Grönqvist (2011) focused on the overall crime rate, violent crimes, theft, drugs and drunken driving. The focus of the study was males between 19 and 25 years of age. For the analysis he created three types of dummy variables for the days of unemployment, 1-90 days, 91-180 day and more than 180 days. For the relation between unemployment and crime Grönqvist considers potential omitted variables and reverse causation. The methodology used for the analysis was a fixed effect OLS model.

The strongest findings of Grönqvist (2011) were that youth unemployment had large significant effect on theft, drugs, drunken driving and a small effect on violent crime. He also showed that age is an important determinant of crime. This study showed substantially larger coefficients than what is usually found in literature that uses aggregated data. One reason for

this according to Grönqvist (2011) could be that aggregated data entails offsetting general equilibrium effects which would hide the effect of unemployment due to the supply of criminal opportunities.

Agell & Öster (2007) looked at Sweden using data on a municipality level. The period under investigation was 1996 to 2000. The reason for the time period chosen was due to the volatile variations in unemployment in Sweden during this time. This variation helped in distinguishing the effect of unemployment on crime from other effects. Agell & Öster (2007) also estimated the effect of youth unemployment on crime during this period and compared it to the prime age unemployment during the volatile period. They also tried to see if labor market programs had an effect on crime.

Agell & Öster (2007) found a strong significance of the relation between unemployment and crime for burglary, auto theft and drug possession during a period of falling unemployment. Further, they found no clear relationship between unemployment and violent crime. When separating the youth unemployment from the unemployment of 25 to 64 year olds they found no significant results except for assault on unfamiliar males and then only a weak significance (10%). For the other youth-related crime categories they found the significant positive coefficients in the prime aged group and not in the youth group. Agell & Öster explains this by intergenerational effects where unstable life conditions of parents translates to adverse spillovers for children. When looking at the effect of labor market programs no significant result was found but the coefficients pointed in the same direction as open unemployment.

The empirical framework used by Agell & Öster (2007) consisted of panel data including 288 Swedish municipalities and annual data. The crime variables chosen was the overall crime rate, burglary, theft, auto theft, assault, assault on unfamiliar male, damage crime, robbery and possession of drugs. Among the control variables used were variables for the population's age distribution, proportion of men, income per capita, graduation level and proportion not born in Sweden. The model applied was a log-linear fixed effect OLS model. They also compared this against a 2SLS model where they used several instrumental variables. Agell & Öster (2007) showed larger coefficient using the 2SLS model than with OLS. This seems to be a common result for studies on crime and unemployment.

Compared to the studies above we use a different period consisting of data from 2002 to 2011. This period contains strong macroeconomic events as most of the other papers try to utilize in their investigation. A reason for using periods with extreme events is that it helps one in trying to distinguish the effect of unemployment from noise. In this paper we use monthly data from 21 counties to measure its effect on crime. The approach in the paper is

to apply monthly data which we believe makes us able to exploit the timing of both open unemployment and transfers to labor market programs. By using yearly data it could be hard to distinguish between the increase in unemployment and the increase in program participation since we would expect them to follow closely in such a large time frame. With the monthly data we are guaranteed more variation between counties in inflows and outflows to programs and for the relation to both unemployment and crime. This will help to get rid of some of the problems that arise from the universal nature of labor market programs across municipalities and counties.

3 Data Description

To examine the macroeconomic conditions on crime and unemployment a sample of monthly data from 21 different Swedish counties between 2002 and 2011 is used. The period contains several macroeconomic, including the collapse of Lehman and the start of the so called "Financial crisis" and currently the crisis now lingering over Europe. These large macroeconomic events have caused substantial variation in employment. This gives a good foundation to conduct research on what implications the labor market may have on criminal activities.

The choice of using county level instead of municipal level data allows us to avoid some of the risk of being biased towards the movements of criminals between different municipalities for committing crime. This can be an issue for big cities like Stockholm, where many crimes are committed by criminals that have their residences outside the Stockholm municipality. Other things to be aware of when looking at the statistics are the effects of different social and political institutions that exist in the different counties. When it comes to crime statistics, the different counties could have different focuses on how to fight crime. Some counties might have more problems with narcotics, property crime or abuse than others. The focus would then be more skewed towards one type of crime and this would then generate higher crime rates for that specific crime in the county. It could also be the case that, although some counties might have the same focus, the success of the effort might be different due to different approaches in fighting a specific crime.

Another assumption that should be considered is that individuals do not change their state of residence during the period 2002 to 2011. The potential bias from assuming no state migration is indeterminate a priori. If the individuals were to move to a county with lower unemployment rate but without changing their criminal behavior, the bias would most likely be zero. Otherwise this migration would be accompanied with increase or decrease in the crime rate, so that the bias would then be upward or downward.

The panel data for the period of study consist of monthly constructed and collected data sets, presented in Table 1. The crime specific data is collect from the National Council for Crime and Prevention[3]. The crime statistics is defined as the number of crimes reported and not the actual crimes committed. Using monthly data of course limits the breakdown of different crime categories because of small counties and months with zero crimes reported. Therefor fairly broad aggregates of common crimes are used. The unemployment data is

[3] Bråttsförebyggande Rådet – www.bra.se

collected from the Swedish Public Employment Services[4] and the population data is from Statistics Sweden[5].

Table 1 –Summary Statistics for Crime Rate and Explanatory Variables

Variables	Mean	Standard Deviation	Minimum	Maximum
Crime Variables				
Total Crime	5 186.65	7 166.57	325	37 582
Burglary	415.87	499.44	7	2 317
Drugs	301.68	419.06	5	3 094
Property Damage	209.58	620.07	1[6]	6033
Vehicle theft	483.82	597.85	10	3 613
Theft & Robbery	2 391.91	3 252.25	94	15 890
Pilfering shop	241.07	347.17	6	2 021
Assault	307.04	396.82	18	2 293
Assault stranger	144.00	201.96	5	1 220
Assault Indoors	158.35	203.53	6	1 294
Violent Crime	395.43	542.81	22	3 085
Rape	17.46	26.68	0	342
Explanatory Variables				
Population	434 296.15	476 213.03	52934	2 087 902
Men aged 15-24	30 795.97	32 700.20	3494	144 810
Foreign born men aged 15-24	3 281.70	4 487.69	141	19 958
Foreign born total	55 882.32	88 243.98	591	454 588
Age distribution:				
0 - 14	0.17	0.01	0.144	0.191
15 - 17	0.04	0.00	0.033	0.048
18 - 24	0.09	0.01	0.078	0.119
25 - 39	0.18	0.02	0.150	0.265
40 - 64	0.33	0.01	0.315	0.372
Open Unemployment	9 972.28	10 561.13	675	51 093
Open 18-24	1 867.42	1 706.88	69	11 355
Open 25-39	3 935.22	4 583.91	237	24 776
Open 40-64	4 161.83	4 391.32	362	21 237
In programs unemployment:				
18-24	1 194.67	1 350.45	10	10 573
25-64	3 861.58	3 738.51	230	23 013
Youth programs:				
Regional youth program	98.48	144.70	0	1 164
Job guarantee for youth	625.53	1 237.02	0	9 679
Youth guarantee	125.66	195.45	0	1 369
Population Density	44.39	61.83	3	317
Education				
Males	76 653.44	94 757.83	7561	455 626
Females	80 965.49	100 473.57	7962	485 626

Note. -All variable contain 2520 observations, based on the monthly data from the period 2002-2011 on a county level. The youth programs starts as of 2007, thus from 2002 until 2007 there are zero participants.

The Population data used could only be extracted on an annual basis. To convert the population data in to the same frequency as the other panel data, it was decided to interpolate the data by the cubic spline method. By applying this method to more than one data set, one should be aware that this will introduce a systematic source of serial correlation in the regressors. The interpolated data will also be systematically related to each other, by the polynomials in the cubic spline. This violates the standard assumptions in the OLS

[4] Arbetsförmedlingen – www.ams.se
[5] Statistiska Centralbyrån Sverige – www.scb.se
[6] One observation from 2003 in the county of Blekinge has been recoded from 0 to 1.

model about no autocorrelation. But since interpolation was only needed on population and no other dataset, this will not significantly affect the result.

3.1 Unemployment definitions

To estimate the effect of unemployment one must first define unemployment. This seems trivial at first but is worth reflecting upon before specifying our model. Agell & Öster (2007), which this paper makes many comparisons with, defines unemployment as being either in idle or, hereafter, open unemployment and those participating in labor market programs. This definition can be characterized as the literal interpretation of the word unemployment. In our study the open unemployment measure will be used as our unemployment variable for the main equations. Being openly unemployed is someone who is unemployed and actively looking for a job but not in any labor market program. The term broad unemployment is used when referencing the measure which includes both openly unemployed and those in labor market programs. The question is if we should expect the same effects from being in open unemployment as being in a labor market program. In an income sense they might not differ that much. For youth unemployment the difference is probably the least apparent considering that a large proportion of youths do not qualify for regular unemployment insurance in the first place since they are likely to have a short or non-existent work history.

Programs can be expected to have mechanical properties that affect behavior by lowering the amount of idle time available to commit crimes. Grönqvist (2011) finds that for 19-24 year old men crime is not necessarily mitigated through income effects but finds that unemployment has a stronger correlation with crimes committed on weekdays than weekends. This result has also been found by Rege et al (2009) when studying plant closures in Norway. These findings suggest that the amount of time available to commit crime is important along with monetary incentives. Prior to the 2006 election in Sweden and the forthcoming shut down of the two existing youth unemployment programs the Swedish National Audit Office (2006) delivered a brutal critique of the existing programs. Only 40% of responsible unemployment offices were following up on the participants' job searching activities at least once a month. There were also large discrepancies between the municipalities regarding the implementation of these programs. In 2007 a new program was launched that in many ways were supposed to become more demanding of the participants involvement and restrictions of their idle time. If we expect these changes to seriously influence unemployment's effect on crime some methodological problems will be faced when using a broad measure.

A downside of using this data is that the use of statistics containing only those registered as unemployed suffers the risk of understating the importance of unemployment if a large amount of individuals drops out from the registers[7].

4 The Job Guarantee Program for Youths (JGPY)

In 2006 voters in Sweden broke a 12 year rule of a Social Democrat lead government and voted for a coalition of right-center wing parties. As can be expected after such a political change, new job market program were to follow and one of the most important would be the job guarantee program for youths below the age of 25 (JGPY). The program was proposed in 2006 and voted on in 2007 (Regeringen, 2007). It was put to the test during the financial crisis that was soon coming to hit the world economy and Europe in particular.

At its peak the program would come to encompass close to 6%, or 54 0000, of all youths between ages 18 and 24. To put this into even greater perspective the peak rate of program participation for a single month among job seeking youths, including those already employed, was 52%. In certain counties up to 70% of all youths, either in unemployment or labor market programs, where registered for this program.

The main requirement to participate in the program is a period of unemployment of at least 90 days during the last 4 months. Following the results from Grönqvist (2011) it can expected that there is a higher probability of these individuals to commit crimes compared to persons who have recently become unemployed.

By definition an individual put in the JGPY will not be counted as being in open unemployment. In Figure 1 we plot the change in number of openly unemployed against the change in people in this program between 2008 and 2011.

[7] Svenskt Näringsliv (Fölster, F. (2012)) released a report where they estimate that 140 000 people of working age in Sweden lives "outside" the system in many ways. 24 000 of those in the age 16-24. Many in this group are however likely stay-at-home moms and children who get their income from other family members.

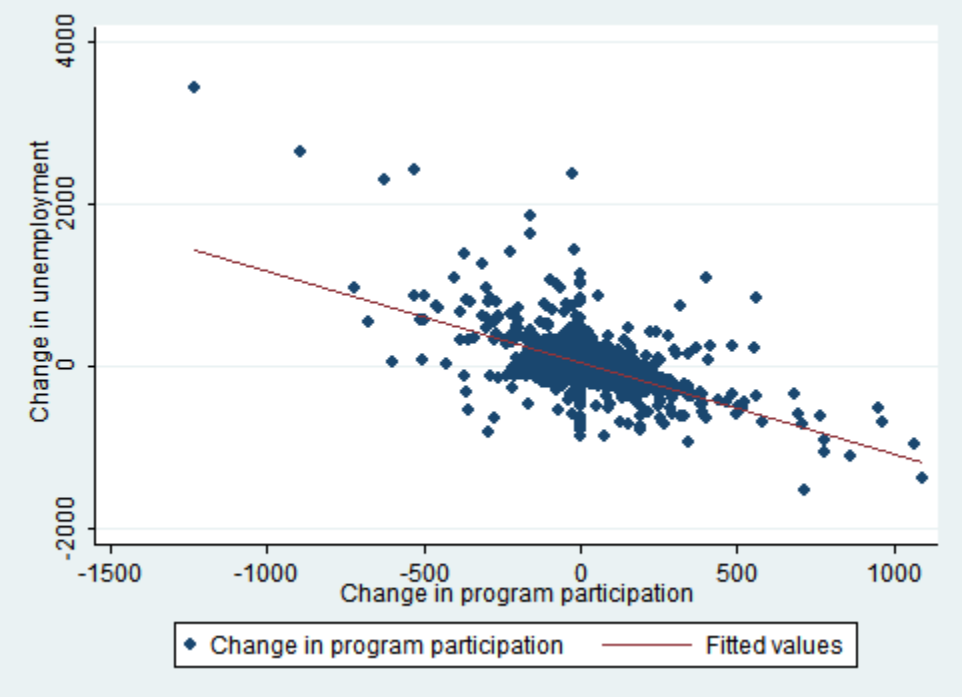

Figure 1 - Differences in Program participation and Unemployment

The fitted line shows a clear negative relationship in a simple OLS regression. Individuals in this program are expected to spend their official "work" time in the program's activities. Someone going from open unemployment into the program is therefor expected to dedicate their entire labor supply to stay in the program and to keep their benefits. After 3 months the different "soft" sides of the program can be complemented with internship, education et cetera. Together with the harsch critique the old programs suffered we expect these changes to be important. A study done by Martinson & Sibbmark (2010) found that participants where generally pleased with the programs but the assumption that the program covered the participants entire idle time was not fulfilled. However, no comparison is available with the old programs. The youths in the program spend more time in job searching activities than those in programs for older people. Further, rouhgly 38% of the responders reported to be much or somewhat more motivated to find a job compared to only 7% who responds that the program had a detrimental effect. This psychological influence can perhaps also be expected to strengthen the differentiation between being in open unemployment and in the program. The preliminary assumption that this program to some extent limit the idle time of the unemployed remains.

5 Main Results

Our main regression equation is as follows:

$$CrimeRate_{it} = \alpha_y + \delta_i + \lambda County * month + \cdots + \beta X_{it} + \gamma u_{it} + \varepsilon_{it} \qquad (1)$$

$CrimeRate_{it}$ is defined as the log of number of crimes per 100,000 residents. βX_{it} are the demographical control variables, α_y is a yearly nationwide trend variable, δ_i are the county fixed effects and $\lambda County * month$ are monthly dummies for each county. The need for county-specific monthly dummies can best be illustrated with the county Gotland, a scarcely-populated island which face a huge inflow of vacationers during the summer months making it deviate substantially from most other counties. Similarly, northern counties can be expected to have a different pattern during the winter months due to a large amount of tourists arriving to enjoy the snow[8]. Finally γu_{it} is our unemployment measure, typically the log of number of people in open unemployment.

The demographic variables follow from both the traditional theory of crime but also from earlier empirical works. Primarily the focus is on those factors that can be expected to influence youth unemployment. Control variables are included for the general age distribution in each county, and for males and immigrants to resolve any bias coming as a consequence of overrepresentation in the population. We also include the number of people with high school or higher education. Compared with many other studies we do not include the wage level, discussion and tests found in section 7.1, primarily because of lack of good data on a monthly basis. In the test we do not see a large impact of the wage level on the coefficients for the main estimations when reducing the time frame of estimation to fit available yearly data. The effect of wage levels is also expected to be correlated with the education variable as higher education and higher wages go hand in hand. Another excluded variable is the deterrence measure, typically percentage of crimes solved or number of police officers. These measures also suffer from some problems. Particularly the percentage of crimes solved as it is inherently based on number of crimes committed along with the causality questionability discussed. We believe that police activity should be captured in the fixed effects for the counties and considering the short time frame that we are focusing on this should not impact our instrumented variable of focus, unemployment. Agell & Öster have shown that including dummies for counties, the level where police resources is allocated, does not yield any significant results when looking at municipal level data. The primary focus of this paper is also focused on the results from the instrumental regressions

[8] In a simple fixed effect regression on the total crime rate we increase our R2 with about 10% using county specific monthly trends as opposed to non-county specific dummies.

which in itself should help to reduce the problems of omitted variable bias for the unemployment measure.

In the IV-regression the job guarantee program is used as an instrument and call it $jgpy_{it}$. Due to the zero-constraint when doing the logarithmic transformation the instrument is defined as the number of individuals in the program divided by the total number of individuals aged 18- 24. What does this mean in practice considering the plausibility of exogeneity? The assumption is that the main effect of the job guarantee program on unemployment comes from the idle time available to commit crimes. Our instrument is expected to impact open unemployment which in turn affects crime. In our first stage regression we will predict open unemployment with our labor market program instrument.

This consists of a group of unemployed that are more likely, on margin, according to Grönqvist (2011) to commit a crime due to the dosage effect of unemployment. This could mean that we are more likely to see an effect of unemployment than would be the case otherwise. Looking at the first stage regression, included in the appendix, we see that an increase in program participation is associated with a negative coefficient for open unemployment. This specification will cause our instrumented variable of open unemployment to reflect a higher proportion long-term unemployed.

Could the instrument be correlated with programs for older age groups? The response would typically be no, since the requirements for the equivalent programs require a longer period of unemployment before becoming legible. Remember that Agell & Öster (2007) concluded that there did not seem to be any direct effects of youth unemployment on common youth crimes using aggregated data, a result sharply contradicted by Grönqvist's (2011) study using individual data on men between 19 to 25 years of age. If it's possible to target this later group it should expected to find some effects of unemployment on crime.

Is this fair as an instrument? The program was certainly random due to the regime change in 2006. Further, an evaluation by Hall & Liljeberg (2011) found no lasting effect on the job prospects of those in the program comparing 24 year olds participants with a similar group of 25 year olds. This means that it should not be expected that the program could alter either future unemployment or other variables that could make the employment level increase the opportunities for crime.

Table 2 – Youth Unemployment and Crime 2007-2011 - 2SLS

	(1) All Crimes	(2) Burglary	(3) Pilfering shop	(4) Theft & robbery	(5) Vehicle theft	(6) Property Damage	(7) Drugs
Open unemployment age 18-24	0.246*	0.176	-0.236	0.332**	1.071****	1.696***	-0.032
	(0.129)	(0.162)	(0.213)	(0.159)	(0.311)	(0.538)	(0.394)
Education	-3.073*	-5.109*	1.880	-3.261*	-7.208*	-2.368	4.989
	(1.594)	(2.627)	(3.407)	(1.970)	(4.083)	(10.259)	(4.610)
Males 15 – 24	0.629	-1.405	-2.580	-0.836	-0.699	-5.170	-0.311
	(0.773)	(1.661)	(1.572)	(0.980)	(2.443)	(4.812)	(1.963)
Foreign born males 15-24	-0.113	-0.628**	-0.179	-0.281	-0.396	-1.279	0.718
	(0.136)	(0.249)	(0.362)	(0.180)	(0.462)	(1.196)	(0.461)
Share of foreign born citizens	-1.738	0.426	5.281	1.268	-8.671	-36.079	3.429
	(3.761)	(7.124)	(5.774)	(5.206)	(12.275)	(31.424)	(10.508)
Population density	-0.004	0.020***	0.005	0.003	-0.007	0.002	-0.006
	(0.004)	(0.007)	(0.004)	(0.004)	(0.012)	(0.031)	(0.010)
Observations	1260	1260	1260	1260	1260	1260	1260

Standard errors in parentheses

Standard errors robust to heteroscedasticity and clustered on county-level. Due to a large number of exogenous regressors compared to clusters some of these have been partial led out to fulfill the rank condition. Fixed effects, yearly and county-specific monthly dummies included in all specifications.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 3 - Youth Unemployment and Crime 2007-2011 - OLS

	(1) All Crimes	(2) Burglary	(3) Pilfering shop	(4) Theft & robbery	(5) Vehicle theft	(6) Property Damage	(7) Drugs
Open unemployment age 18-24	0.029	0.117**	-0.036	0.051**	0.155***	0.147	0.037
	(0.019)	(0.046)	(0.035)	(0.024)	(0.045)	(0.139)	(0.061)
Education	-2.578*	-4.974	1.421	-2.618	-5.114*	1.174	4.833
	(1.298)	(2.982)	(3.663)	(1.911)	(2.805)	(8.607)	(4.901)
Males 15 - 24	0.872	-1.339	-2.805	-0.521	0.327	-3.433	-0.387
	(0.640)	(1.982)	(1.657)	(0.810)	(1.221)	(4.406)	(2.248)
Foreign born males 15-24	-0.202	-0.652**	-0.096	-0.396**	-0.774**	-1.918	0.746
	(0.122)	(0.305)	(0.384)	(0.182)	(0.274)	(1.322)	(0.553)
Share of foreign born citizens	1.779	1.382	2.020	5.832	6.203	-10.915	2.320
	(2.790)	(8.758)	(6.816)	(3.465)	(5.490)	(29.010)	(12.645)
Population density	-0.001	0.021***	0.002	0.007*	0.007	0.025	-0.007
	(0.002)	(0.007)	(0.006)	(0.003)	(0.006)	(0.031)	(0.008)
Share of citizens age 25-39	5.586	-0.401	-10.106	-7.519	-13.906	-96.586*	13.659
	(5.310)	(18.708)	(11.106)	(7.736)	(12.115)	(50.278)	(25.562)
Share of citizens age 18-24	-11.222	18.150	-8.596	-11.827	-27.173*	-40.735	-31.225
	(10.086)	(21.561)	(24.005)	(7.950)	(14.778)	(86.209)	(37.513)
Share of citizens age 15-17	-11.560	-7.641	38.338*	-20.866**	-48.405**	52.532	13.147
	(8.633)	(28.368)	(21.662)	(9.705)	(17.069)	(93.372)	(33.758)
Share of citizens age 0-14	-0.889	11.609	-18.284	-6.706	-8.409	-9.809	-20.057
	(7.558)	(24.710)	(12.732)	(9.572)	(13.468)	(70.935)	(27.859)
Share of citizens age 40-64	-6.355	-26.152**	-25.100*	-20.636***	-42.389***	-56.672	9.525
	(5.433)	(10.522)	(12.049)	(7.118)	(11.836)	(64.986)	(27.502)
Observations	1260	1260	1260	1260	1260	1260	1260
R^2	0.637	0.532	0.325	0.793	0.874	0.301	0.398

Standard errors in parentheses
Robust standard errors. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

The results of the IV-regression for the five year period between 2007 and 2011 gives a significant effect of youth unemployment on most crime categories included. Two patterns clearly emerge when studying the significant coefficients of all crimes, theft & robbery, vehicles theft and property damage. The size of the increase compared to the OLS regression in Table 2 and the absolute size of the coefficient decreases when moving from less serious to more serious crimes. Property damage which mainly consists of things like vandalism shows both the highest coefficient and the largest increase compared to the OLS estimation (0.14 vs. 1.69). The link between property damage and crime is obviously hard to relate to traditional theory of income being the incentive behind crime. For theft and robbery the estimate increase from 0.05 to 0.33 and the vehicles theft category increases from 0.15 to 1.07. The coefficient for all crimes, which is not significant in the OLS estimation, goes from 0.02 to 0.246. The coefficient on burglary also increases slightly but turns insignificant in the IV-specification. This is of course contradictory since as this is one of the consistently significant results both in most of our other OLS estimations but also in the general literature. If our assumptions are right and our instrument being able to predict a marginal group of unemployed it can be seen that the effect of youth unemployment generally shows

a strong positive correlation with the crime rate. The link between the idle time available to commit crimes can be said to manifest itself in the property damage crime category.

When instrumenting with the program participation rate on the unemployment rate we could claim to be successful in identifying the change that happens to a marginal group of youths. Not only have they been unemployed at least for 90 days during the last 4 month period but they are also more likely to be males according to the general composition of those in the labor market program we use as an instrument[9]. The variation we are able to identify is likely a local average partial effect (Almén & Nordin, (2011)) and should not be interpreted as estimates valid for the entire group of youths in open unemployment. Ideally, the instrument identifies a group of unemployed where the expected duration of unemployment is longer and who stands further away from the labor market in that they are forced to enter the job guarantee program.

One of the most apparent differences with the results from our regression compared to those of Agell & Öster (2007) is that exactly the opposite relationship between crime and unemployment when looking specifically at youth unemployment is found. Where they find mostly negative, but insignificant, coefficients we find the opposite. All coefficients except for pilfering shop are positive. The result of finding larger coefficients when applying an instrument is more or less typical of the literature. Agell & Öster (2007) finds that their coefficients increase by 2 to 4 times when instrumenting the unemployment rate for the entire population. The largest increase of comparable categories is found in their auto theft category which fits our results.

Fougeré et al (2009) found substantial effects of youth unemployment and crime using IV-estimation and data from France between 1990 and 2000. Their results from an OLS model yields negative coefficients however for most crimes and the unemployment rate among 15 to 24 year olds. Not only do they get a larger coefficient but also changes in the sign of the coefficients when applying their instruments. For the group 25-39 olds they find that unemployment has a negative effect on crime even when using IV-estimates. A results that is similar to our OLS estimates for the older age groups. They speculate that criminal youths often target employed individuals and increasing unemployment among the older group leads to less opportunities for profitable crime.

To illustrate our findings of youth unemployment and the crime rate with those of Grönqvist (2011) for young males between 19 and 24. Using data on Swedish individuals he estimate that 100 days of unemployment increases the probability of being convicted for any

[9] In the descriptive statistic section of Hall & Liljeberg (2011) of we find that for the entire group in the youth guarantee program the average number of prior unemployment days was 233 in 2008 and 204 in 2009.

crime at 0.45 percentage points for natives and non-natives with 0.94 percent. Because of the difference of the dependent variable in this study, convicted versus reported, it is hard to make any easy comparisons between this group and the selection of convicted males. The relative change, compared to the mean in Grönqvists study, is 9 and 12.2 percent. This lends support to the idea of a high probability of crime for this group. Using pooled OLS estimates with less control variables he finds that being unemployed between 1 and 90 days increase the probability of being convicted for a crime with 1.93 percent. Relative to the sample mean this translates to a 37% higher probability. These are of course staggering numbers but they lend credibility to the findings of a link between youth unemployment and crime.

Agell & Öster (2007) finds an OLS based estimate of a 3.8% drop in car theft for a 1% drop in unemployment. Their IV-estimate is considerably higher at 16%. The youth share of the openly unemployed was on average 18% during our period of estimation. In a rough translation of the vehicle theft results we find that in the IV-model, where the coefficients should be interpreted as elasticity's, a 1% change in youth unemployment would lead to a 6% (1.07/0.18) change in vehicles theft. The magnitude of our results can thus be considered to be realistic and slightly above those found in the OLS estimation of Agell & Öster. Similarly they find an IV-estimated 6% drop in the overall crime rate when unemployment falls 1%. Our conversion yields a 1.4% (0.246 / 0.18) change. When comparing with the OLS estimates from the same study we note that our estimation gives larger comparable coefficients when relating to population share of the unemployed. In studies that used a log-log model Papps and Winkelmann (2000) finds an elasticity of 0.1 for total offenses compared to the one found in our estimate of 0.24. Our result is more than twice the size for youths than the general population and aggregated crime despite the relatively smaller share of unemployed youths.

Returning to Grönqvist (2011), the conviction rate of 19 – 24 year olds in 2005 was 4.2 percent compared to 2.2 percent for males aged 29 – 34. For those who have experienced joblessness for at least 180 days the conviction rate was 9.3% for 19 - 24 year olds and 4.1% for those aged 29 – 34. In general there seems to be about twice the risk of crime in the younger age cohort. This fact fits well with our estimates for all crimes and earlier studies.

Using a log-log model Edmark (2005) found a coefficient of 0.14 for burglary and 0.159 for car theft. Generally our IV-estimates are much larger, as should be expected for a group that is often overrepresented in our crime categories. What really sticks out of comparable crime categories between our estimates and those of Edmark (2005) is the vehicle theft estimation where our estimate is 6 times larger. While not directly comparable to Edmark's auto theft category, our OLS estimates for the coefficient of vehicle theft typically falls in the same

confidence band as those of Edmark despite the relatively smaller size of the youth group. The same pattern is not found for vehicle theft and the unemployment of all ages as seen in Table 5. Here the broad unemployment measure shows a negative correlation with vehicle theft of roughly the same magnitude (-0.12)

An alternate specification includes the 25-64 year olds in the IV-estimation. This will have two effects, it will likely remove at least some of the bias of our earlier estimate but it would also improve the strength of the instrument due to it being able to predict the cyclical variations of youth unemployment. However, we could no longer expect to be capturing the effect of the same marginal effect for the group who has been unemployed for at least 90 days.

Table 4 - Youth Unemployment and Crime 2007-2011 - 2SLS (alternate model)

	(1) All Crimes	(2) Burglary	(3) Pilfering shop	(4) Theft & robbery	(5) Vehicle theft	(6) Property Damage	(7) Drugs
Open unemployment age 18-24	0.098*	0.171**	-0.045	0.156**	0.530****	0.855****	0.085
	(0.053)	(0.075)	(0.092)	(0.063)	(0.112)	(0.195)	(0.197)
Broad unemployment 25-64	-0.127	-0.005	0.165	-0.151	-0.466**	-0.725***	0.101
	(0.081)	(0.089)	(0.104)	(0.107)	(0.221)	(0.281)	(0.205)
Education	-2.856**	-5.101**	1.598	-3.002*	-6.413**	-1.131	4.817
	(1.162)	(2.581)	(3.218)	(1.773)	(2.584)	(8.524)	(4.499)
Males 15 - 24	0.953*	-1.394	-3.001**	-0.451	0.487	-3.324	-0.568
	(0.515)	(1.734)	(1.528)	(0.680)	(1.199)	(3.823)	(2.014)
Foreign born males 15-24	-0.235*	-0.632**	-0.020	-0.426**	-0.843***	-1.974	0.815
	(0.125)	(0.281)	(0.345)	(0.190)	(0.294)	(1.217)	(0.502)
Share of foreign born citizens	3.733	0.619	-1.829	7.786**	11.384**	-4.864	-0.915
	(3.142)	(8.253)	(6.720)	(3.328)	(5.336)	(25.995)	(12.257)
Population density	-0.002	0.020***	0.003	0.005*	-0.000	0.012	-0.007
	(0.002)	(0.006)	(0.005)	(0.003)	(0.005)	(0.028)	(0.008)
Observations	1260	1260	1260	1260	1260	1260	1260

Standard errors in parentheses
Standard errors robust to heteroscedasticity and clustered on county-level. Due to a large number of exogenous regressors compared to clusters some of these have been partial led out to fulfill the rank condition. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Specifying our regression in this way essentially cuts our coefficients in half for our previously significant crime categories. Again, our results are more in line with the individual based study of Grönqvist (2011) in that we do find a relationship between youth unemployment and crime. The reduced size of the coefficients could be interpreted as a sign of the duration, or dosage, effect that unemployment have on the likelihood to commit a crime. It is not surprising that we find the largest estimates in those categories which are typically associated with young offenders.

6 Further Analysis and Results

The fundamental difference of the results from earlier studies from the 1990s on aggregated data and youth unemployment in Sweden certainly demands a motivation beyond that the times have changed and that our instrument is somehow superior. To do this we return to our basic OLS model and try some variations. We would like to stress that all these estimates likely suffer from equilibrium issues as have been demonstrated in almost every study that also includes IV-regressions. We still believe that it is motivated in trying to get a better picture of what has or has not changed and to add to the credibility of our IV-estimates.

We run our regressions on a longer time horizon consisting of 2002 to 2011 to see if our crisis years from 2007 to 2011 included in our main equation is somehow unique. Notable here is of course that both Edmark (2005), Agell and Öster (2007) use data mainly from the 1990s. Results from Almén & Nordin (2011) using data from 1997 to 2009 could be said to be more in line with our findings of the overall unemployment rate. They do find significance for long term unemployment which is the focus of their study. In the specifications that are comparable with ours there is no significance for the general level of unemployment on property crimes. Estimates using weighted vs. non-weighted variables show no significant difference in coefficients[10]. One explanation is that the lags for the effect of unemployment on crime are much longer for older people. Older people might have better insurance, a spouse with complementary income and larger savings. It might also be that the idle component of determining criminal behavior is larger for young people. Another explanation that undermines the ability to even be able to get correct estimates is if older people in general are dropping out from the register of the unemployment offices. This explanation unfortunately does not explain our completely opposite results when looking at youth unemployment.

[10] Specifically we are referencing Table 2.

6.1 Total unemployment and crime

To explore the differences we begin by running an OLS based regressions on open unemployment for the entire population against our crime categories.

Table 5 - Open Unemployment and Crime 2002-2011 - OLS

	(1) All Crimes	(2) Burglary	(3) Theft & robbery	(4) Vehicle theft	(5) Violent Crimes
Open unemployment	-0.004	0.049	-0.022	-0.123**	0.022
	(0.022)	(0.067)	(0.031)	(0.054)	(0.031)
Education	-0.354	1.929*	0.082	-0.727	-0.623
	(0.419)	(0.987)	(0.426)	(0.917)	(0.942)
Males 15 - 24	0.280	-1.166	-0.232	0.894	0.745
	(0.410)	(1.327)	(0.499)	(0.607)	(0.685)
Foreign born males 15-24	0.022	0.215	0.102	0.098	0.108
	(0.091)	(0.230)	(0.129)	(0.258)	(0.155)
Share of foreign born citizens	-0.761	0.799	0.511	-0.928	-0.553
	(0.608)	(1.536)	(0.779)	(1.655)	(1.003)
Population density	-0.001	0.006	0.002	0.001	-0.000
	(0.002)	(0.006)	(0.002)	(0.004)	(0.003)
Share of citizens age 25-39	3.059*	8.234	6.854**	16.261****	-0.380
	(1.639)	(5.673)	(2.750)	(3.487)	(2.758)
Share of citizens age 18-24	-4.129	17.394	0.423	-5.795	-7.417
	(4.209)	(13.750)	(6.413)	(13.015)	(10.045)
Share of citizens age 15-17	4.036	12.601	-5.318	-9.312	1.447
	(5.068)	(23.153)	(7.707)	(14.277)	(7.776)
Share of citizens age 0-14	3.578	19.510**	4.598	-8.123	-6.003
	(2.171)	(7.862)	(3.549)	(5.316)	(6.169)
Share of citizens age 40-64	0.869	-0.303	2.127	-2.654	0.112
	(2.322)	(4.733)	(2.776)	(6.074)	(3.686)
Observations	2520	2520	2520	2520	2520
R^2	0.637	0.611	0.802	0.846	0.672

Standard errors in parentheses
Robust standard errors. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

The results in Table 5 are all insignificant with exception for a negative relationship for vehicle theft.

Running the same regression against the broader measure including those in labor market programs the coefficient falls but find a significant result for violent crime. The general result from Table 6 is that unemployment exhibits a negative correlation with crime. This of course contradicts everything we expect but we note that compared to only including those in open unemployment our negative coefficients increase. Possibly we are seeing the importance of the offsetting equilibrium effects when the change in unemployment month over month leads to lower activity and a weaker supply of victims. We could also be capturing effects of

labor market programs that limit criminal behavior. The increase in the negative coefficient lends some support for this idea even for the entire group of unemployed.

Table 6 – Broad Unemployment and Crime 2002-2011 - OLS

	(1) All Crimes	(2) Burglary	(3) Theft & robbery	(4) Vehicle theft	(5) Violent Crimes
Unemployed & in programs	-0.085*	0.060	-0.045	-0.190*	-0.110***
	(0.041)	(0.068)	(0.054)	(0.105)	(0.038)
Education	-0.328	1.925*	0.089	-0.704	-0.581
	(0.428)	(0.982)	(0.439)	(0.897)	(0.901)
Males 15 - 24	0.334	-1.208	-0.201	1.026	0.814
	(0.393)	(1.354)	(0.505)	(0.603)	(0.644)
Foreign born males 15-24	0.005	0.217	0.097	0.083	0.080
	(0.091)	(0.230)	(0.127)	(0.256)	(0.152)
Share of foreign born citizens	-0.510	0.766	0.583	-0.722	-0.141
	(0.589)	(1.528)	(0.801)	(1.673)	(0.997)
Population density	-0.002	0.006	0.002	0.001	-0.001
	(0.002)	(0.006)	(0.002)	(0.004)	(0.003)
Share of citizens age 25-39	3.492**	8.145	6.993**	16.700****	0.309
	(1.642)	(5.685)	(2.779)	(3.219)	(2.573)
Share of citizens age 18-24	-4.004	17.411	0.444	-5.774	-7.192
	(4.057)	(13.688)	(6.489)	(12.944)	(9.748)
Share of citizens age 15-17	4.939	12.996	-5.282	-9.831	3.231
	(4.657)	(22.809)	(7.526)	(13.419)	(6.984)
Share of citizens age 0-14	4.651*	19.191**	4.984	-6.798	-4.351
	(2.379)	(7.879)	(3.652)	(5.109)	(6.099)
Share of citizens age 40-64	1.051	-0.438	2.228	-2.228	0.344
	(2.251)	(4.692)	(2.828)	(6.027)	(3.391)
Observations	2520	2520	2520	2520	2520
R^2	0.640	0.611	0.802	0.847	0.673

Standard errors in parentheses
Robust standard errors. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

In the third specification in Table 7 the open unemployment is divided into three age groups, 18 to 24, 25 to 39 and 40 to 64. We suspect that there are significant differences between the expected coefficients between those in the very diverse 25 to 64 group. Now our results are all positive for the youth group. Surprisingly, violent crime is significant at the 5% level and less surprisingly burglary at the 1% level, albeit with a small coefficient. No significance is found for any crime in the older age groups and the suspected difference cannot be confirmed.

Table 7 – Open Unemployment (by age group) and Crime 2002-2011 - OLS

	(1) All Crimes	(2) Burglary	(3) Theft & robbery	(4) Vehicle theft	(5) Violent Crimes
Open unemployment age 18-24	0.083*	0.168***	0.099	0.175	0.056**
	(0.042)	(0.049)	(0.064)	(0.119)	(0.026)
Open unemployment age 25-39	-0.052	-0.145	-0.018	-0.153	0.001
	(0.047)	(0.156)	(0.062)	(0.107)	(0.118)
Open unemployment age 40-64	-0.042	0.035	-0.119	-0.164	-0.045
	(0.077)	(0.159)	(0.094)	(0.155)	(0.137)
Education	-0.245	2.217**	0.153	-0.496	-0.582
	(0.459)	(0.991)	(0.470)	(0.939)	(0.991)
Males 15 - 24	0.217	-1.351	-0.262	0.768	0.725
	(0.432)	(1.243)	(0.488)	(0.617)	(0.754)
Foreign born males 15-24	0.029	0.234	0.107	0.112	0.111
	(0.093)	(0.227)	(0.132)	(0.270)	(0.159)
Share of foreign born citizens	-0.544	1.280	0.693	-0.446	-0.451
	(0.569)	(1.534)	(0.775)	(1.681)	(0.931)
Population density	-0.001	0.006	0.002	0.002	-0.000
	(0.002)	(0.006)	(0.002)	(0.005)	(0.003)
Share of citizens age 25-39	3.466*	9.440	6.979**	17.142****	-0.306
	(1.662)	(5.918)	(2.904)	(3.518)	(2.524)
Share of citizens age 18-24	-4.725	16.440	-0.414	-7.323	-7.812
	(4.523)	(13.574)	(6.792)	(13.631)	(10.150)
Share of citizens age 15-17	4.546	14.219	-5.357	-8.436	1.516
	(5.952)	(21.754)	(8.244)	(16.198)	(7.719)
Share of citizens age 0-14	2.787	18.036**	3.539	-10.038	-6.524
	(2.259)	(7.369)	(3.396)	(5.947)	(5.843)
Share of citizens age 40-64	1.303	0.154	2.909	-1.562	0.504
	(2.361)	(4.601)	(2.938)	(6.429)	(3.632)
Observations	2520	2520	2520	2520	2520
R^2	0.643	0.615	0.806	0.849	0.673

Standard errors in parentheses
Robust standard errors. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

6.2 Effect of labor market programs

Table 8 –Unemployment, Programs and Crime 2002-2011 - OLS

	(1) All Crimes	(2) Burglary	(3) Theft & robbery	(4) Vehicle theft	(5) Violent Crimes
Open unemployment age 18-24	0.061*	0.153***	0.084	0.127	0.047
	(0.035)	(0.044)	(0.054)	(0.100)	(0.028)
Unemployed in programs age 18-24	-0.026**	0.036	0.009	0.024	-0.054****
	(0.010)	(0.022)	(0.010)	(0.026)	(0.013)
Broad unemployment 25-64	-0.108	-0.148	-0.141	-0.330*	-0.081
	(0.067)	(0.102)	(0.095)	(0.181)	(0.056)
Education	-0.242	2.123**	0.211	-0.497	-0.520
	(0.453)	(0.997)	(0.484)	(0.906)	(0.873)
Males 15 - 24	0.349	-1.208	-0.199	1.011	0.840
	(0.380)	(1.331)	(0.496)	(0.598)	(0.619)
Foreign born males 15-24	0.005	0.229	0.101	0.092	0.076
	(0.089)	(0.223)	(0.127)	(0.260)	(0.148)
Share of foreign born citizens	-0.409	0.977	0.685	-0.664	-0.030
	(0.563)	(1.504)	(0.786)	(1.669)	(0.962)
Population density	-0.002	0.006	0.002	0.001	-0.001
	(0.002)	(0.005)	(0.002)	(0.004)	(0.003)
Share of citizens age 25-39	4.036**	8.101	7.095**	16.548****	1.231
	(1.763)	(5.628)	(2.842)	(2.987)	(2.644)
Share of citizens age 18-24	-3.923	14.898	-0.802	-8.448	-6.067
	(4.221)	(13.519)	(6.831)	(13.472)	(9.787)
Share of citizens age 15-17	4.583	11.265	-6.253	-11.870	3.365
	(4.853)	(22.197)	(7.861)	(13.979)	(7.145)
Share of citizens age 0-14	4.408*	17.662**	4.091	-8.865	-4.063
	(2.369)	(6.770)	(3.315)	(5.541)	(6.043)
Share of citizens age 40-64	1.387	0.667	2.795	-1.400	0.525
	(2.179)	(4.422)	(2.957)	(6.117)	(3.209)
Observations	2520	2520	2520	2520	2520
R^2	0.648	0.615	0.805	0.848	0.679

Standard errors in parentheses
Robust standard errors. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

To test our instrument we separate the labor market part from the open unemployment part and look at how the different varieties of programs affect the crime rate in Table 9. The positive relationship between open unemployment and our crime categories are consistently positive and significant at the 1% level for burglary and 10% for all crimes. For the unemployment program(s) the results are mixed but we note that where they are significant they are negative. We find that labor market programs are negatively associated with total crime level at the 10% level and negatively correlated with violent crimes at the 0.1% level. This is reassuring for our assumption that programs can work to reduce the idle time available for committing crimes. Generalizing for labor market programs is difficult since the

period 2002 to 2011 contains several different generations of labor market programs that we suspect have varying degrees of effectiveness in limiting crime.

Table 9 – Job Guarantee Programs and Crime 2007-2011 - OLS

	(1) All Crimes	(2) Burglary	(3) Theft & robbery	(4) Vehicle theft	(5) Violent Crimes
Open unemployment age 18-24	0.031	0.083	0.038	0.081	0.021
	(0.027)	(0.051)	(0.035)	(0.062)	(0.038)
Participation rate JGPY	-0.606	-0.800	-1.071	-4.076**	0.071
	(0.566)	(0.911)	(0.696)	(1.484)	(0.979)
Broad unemployment 25-64	-0.045	0.104	-0.006	0.089	-0.021
	(0.068)	(0.089)	(0.088)	(0.191)	(0.073)
Education	-2.947**	-5.221	-3.163	-7.024*	-2.497
	(1.411)	(3.084)	(2.266)	(3.399)	(2.314)
Males 15 - 24	0.896	-1.469	-0.551	0.105	0.148
	(0.592)	(1.952)	(0.777)	(1.350)	(1.039)
Foreign born males 15-24	-0.215	-0.607*	-0.392*	-0.714**	0.037
	(0.145)	(0.316)	(0.219)	(0.333)	(0.189)
Share of foreign born citizens	3.300	0.047	7.020*	8.468	0.488
	(3.564)	(9.428)	(3.723)	(5.757)	(3.434)
Population density	-0.002	0.020**	0.005*	0.001	0.003
	(0.002)	(0.007)	(0.003)	(0.006)	(0.004)
Share of citizens age 25-39	4.229	-3.058	-10.309	-25.101*	16.653**
	(6.767)	(18.859)	(8.237)	(13.677)	(6.484)
Share of citizens age 18-24	-8.665	17.403	-9.157	-19.798	-9.904
	(9.711)	(21.184)	(8.695)	(15.866)	(13.134)
Share of citizens age 15-17	-12.797	-4.000	-20.683*	-44.151**	1.453
	(9.011)	(27.639)	(10.167)	(18.315)	(10.166)
Share of citizens age 0-14	1.062	11.868	-4.296	-0.806	-9.680*
	(6.692)	(24.923)	(9.528)	(14.785)	(5.025)
Share of citizens age 40-64	-5.651	-27.456**	-20.394**	-42.976***	-1.763
	(5.542)	(10.460)	(7.797)	(13.876)	(7.997)
Observations	1260	1260	1260	1260	1260
R^2	0.641	0.532	0.795	0.878	0.495

Standard errors in parentheses
Robust standard errors. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Finally we control if our assumptions about the job guarantee program for youths fits our assumption. This is defined as a share of the total age group. We include both a variable for those in open unemployment and our instrument. For the remaining age group we add up both people in open unemployment and in programs. The new labor market program shows a negative coefficient on all non-violent crime categories. Vehicle theft was one of the crime categories that we found the highest coefficient for in our IV-estimation.

If this is a true reflection of the different effect between being in a labor market program and being in open unemployment then the method of adding these groups together, as has been done in some earlier studies, could cause problematic interference. We now suddenly

have two subgroups in our independent variable where the effect on crime is possibly pointing in different directions. Looking at the data description of Agell & Öster (2007) we can unfortunately not identify the share of youths in labor market programs. We can see that the maximum open unemployment rate is 14.2% and the rate of program participation is 10.6% for the entire age group 18-64 so this effect could of course be substantial. When differentiating between those in open unemployment and those in labor market programs Agell & Öster (2007) find no difference in sign for either category but find only significance for those in open unemployment.

Adding to this fundamental difference in the measure of unemployment as labor market policies come and go we look at the study by Edin et al (2000). In 1990 about half a percent of youths participated in some form of labor market program which rises to above 8 percent in 1994. This is similar to the peak figure of 6% in the sample used in this paper. It is therefore not obvious that one should expect the same results in different time periods when lumping these two categories of unemployed together. It might not even be possible to treat the people in labor market programs as one group due to the ever shifting policies. There seems to be large historic differences between those labor market programs that cannot really be seen as much different from regular employment, such as full time internship, and those that in practice essentially qualify as being in open unemployment. These remarks and the general results obviously relate to the definition of the unemployment rate that we discussed in section 3 and demonstrate a possible source for the some of the conflictions in our study.

In Table 10 we define a variable as the share of youths in programs out of those either in programs or in open unemployment and estimate this variable against some crime categories. The result is consistently negative coefficients. This measure could optimally be fairly independent of the labor market situation, though arguably it could be correlated with other labor market factors and follow cyclical trends. More importantly the sign is what we did expect if our analysis above is correct so we cannot simply refute it. The coefficient for violent crimes is significant at the 0.1% level and for all crimes at the 1% level.

Table 10 - Labor Program Share and Crime 2002-2011 - OLS

	(1) All Crimes	(2) Burglary	(3) Theft & robbery	(4) Vehicle theft	(5) Violent Crimes
Share of unemployed in programs	-0.214***	-0.100	-0.125	-0.226	-0.279****
	(0.065)	(0.085)	(0.111)	(0.202)	(0.062)
Education	-0.310	1.965*	0.101	-0.718	0.031
	(0.422)	(1.019)	(0.441)	(0.944)	(0.685)
Males 15 - 24	0.320	-1.151	-0.207	0.946	
	(0.391)	(1.317)	(0.502)	(0.638)	
Foreign born males 15-24	0.021	0.203	0.105	0.122	0.105
	(0.089)	(0.237)	(0.130)	(0.257)	(0.145)
Share of foreign born citizens	-0.521	1.069	0.588	-1.048	-0.281
	(0.563)	(1.635)	(0.772)	(1.669)	(1.035)
Population density	-0.002	0.005	0.002	0.001	0.001
	(0.002)	(0.005)	(0.002)	(0.004)	(0.003)
Share of citizens age 25-39	3.976**	8.898	7.297**	16.673****	0.038
	(1.629)	(5.896)	(2.653)	(3.317)	(2.865)
Share of citizens age 18-24	-2.994	18.038	1.040	-4.870	-2.812
	(3.939)	(14.026)	(6.312)	(12.681)	(9.723)
Share of citizens age 15-17	4.253	13.838	-5.637	-11.734	8.400
	(5.071)	(23.297)	(7.833)	(14.595)	(9.086)
Share of citizens age 0-14	4.187*	20.275**	4.764	-8.608	-4.455
	(2.323)	(7.698)	(3.690)	(5.373)	(6.453)
Share of citizens age 40-64	1.338	-0.092	2.404	-2.142	-0.114
	(2.189)	(4.898)	(2.822)	(5.938)	(3.589)
Observations	2520	2520	2520	2520	2520
R^2	0.647	0.611	0.803	0.847	0.677

Standard errors in parentheses
Robust standard errors. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

To take this argument to its most extreme; it is easy to see how an unemployment measure that mostly consists of individuals in labor market programs with decent unemployment benefits and 40 hour a week participation requirement could lead to the conclusion that youth unemployment actually leads to lower crime rates. Even if what we instinctively would consider to be unemployment do not. Seen in this way there could be a case made for quality labor market programs as a way to reduce crime.

Future studies using aggregate data over longer horizons would need to account for these varying implementations of labor market programs.

6.3 Variations of the IV-estimation

Table 11 - Unemployment and Crime 2002-2011 - 2SLS

	(1) All Crimes	(2) Burglary	(3) Pilfering shop	(4) Theft & robbery	(5) Vehicle theft	(6) Property Damage	(7) Drugs
Open unemployment age 18-24	0.256**	0.119	-0.193	0.249	0.727**	1.135**	0.360
	(0.116)	(0.164)	(0.140)	(0.173)	(0.284)	(0.498)	(0.391)
Education	-0.174	2.028**	-1.196	0.251	-0.250	-2.595	0.388
	(0.424)	(0.905)	(1.022)	(0.395)	(1.046)	(5.146)	(1.692)
Males 15 - 24	0.140	-1.234	-0.404	-0.365	0.508	0.757	-2.570
	(0.434)	(1.197)	(0.725)	(0.511)	(0.850)	(3.625)	(2.069)
Foreign born males 15-24	0.101	0.240	-0.171	0.182	0.345	-0.977	0.286
	(0.095)	(0.225)	(0.207)	(0.148)	(0.285)	(0.854)	(0.398)
Share of foreign born citizens	-1.129	0.787	1.713	0.096	-2.323	-18.111*	1.222
	(0.798)	(1.420)	(1.500)	(0.737)	(2.002)	(9.553)	(2.993)
Population density	0.000	0.006	0.001	0.003*	0.005	0.005	0.002
	(0.002)	(0.005)	(0.005)	(0.002)	(0.005)	(0.025)	(0.007)
Observations	2520	2520	2520	2520	2520	2520	2520

Standard errors in parentheses
Standard errors robust to heteroscedasticity and clustered on county-level. Due to a large number of exogenous regressors compared to clusters some of these have been partialled out to fulfill the rank condition. Fixed effects, yearly and county-specific monthly dummies included in all specifications. F test of excluded instruments: F(1, 20) = 19.79. Prob > F = 0.0002
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

In Table 11 and Table 12 we complement our main equation by using the different labor market programs aimed at youths for the entire period of 2002 to2011 as an instrument. We now have an instrument that we do not really know too much about due to the critique and implementation issues raised and discussed earlier. The assumption about the idle time available for crime can be said to be much weaker. There might also be other changes in rules and insurance-levels that come into play. The instrument is now defined as all youth programs[11] / population of 18 – 24 year olds. In Table 11 we find significance in the usual suspects, vehicle theft and property damage but also the aggregated crime category. There is no longer a significant result for theft & robbery. The coefficients compared to our main results in Table 3 are somewhat smaller generally. For vehicle theft we get 0.7 vs. 1.0 and for property damage 1.1 vs. 1.7. Similarly to our previous regressions the coefficient is positive for all crimes except pilfering shop. In all of the specifications pilfering shop shows a negative sign which is consistent with the result from Feguere et al (2009).

[11] As represented by AMS in Swedish: Ungdomsgarantin (-2007), kommunalt ungdomsprogram (-2007) and Jobbgarantin för ungdomar (2007-).

Table 12 - Unemployment and Crime 2002-2006 - 2SLS

	(1) All Crimes	(2) Burglary	(3) Pilfering shop	(4) Theft & robbery	(5) Vehicle theft	(6) Property Damage	(7) Drugs
Open unemployment age 18-24	0.367***	0.264	-0.082	0.468**	0.724**	1.137	-0.156
	(0.129)	(0.290)	(0.121)	(0.201)	(0.349)	(1.019)	(0.422)
Education	0.398	1.558	-1.516	0.749	0.220	6.510	-2.558
	(0.844)	(1.492)	(1.342)	(1.176)	(1.962)	(5.092)	(2.398)
Males 15 - 24	-2.109**	-3.250*	-0.225	-3.041**	-1.288	-1.522	-4.505
	(0.970)	(1.677)	(1.881)	(1.214)	(1.669)	(8.649)	(2.814)
Foreign born males 15-24	0.156	0.199	0.201	0.508*	0.990**	-1.251	-0.210
	(0.203)	(0.494)	(0.413)	(0.289)	(0.443)	(1.784)	(0.542)
Share of foreign born citizens	0.146	-0.397	-1.571	-0.945	-0.630	-1.545	7.051
	(0.674)	(1.106)	(1.776)	(0.925)	(1.356)	(4.756)	(4.423)
Population density	0.003	0.016	0.029	0.030***	0.027**	-0.036	-0.056*
	(0.008)	(0.018)	(0.021)	(0.009)	(0.014)	(0.071)	(0.033)
Observations	1260	1260	1260	1260	1260	1260	1260

Standard errors in parentheses
Standard errors robust to heteroscedasticity and clustered on county-level. Due to a large number of exogenous regressors compared to clusters some of these have been partialled out to fulfill the rank condition. Fixed effects, yearly and county-specific monthly dummies included in all specifications. F test of excluded instruments: F(1, 20) = 6.83
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Running this on only the period 2002-2006 yields an unfortunately weak instrument with an F-value of 6. Significant results are found in table 12 but we remain skeptical due to the weakness of the instrument which can lead to upward biased coefficients.

Another complicating cause that could lead to finding a different linkage between youth unemployment and crime in different periods can be of a structural nature. Looking at the percentage of unemployed made up by men, we see a massive spike following the recession. So not only did the job market for youths deteriorate, it did so in what can be assumed to be a worse way for males. Perhaps more females than men seek out higher education faced with a rough job market or this latest recession involved a larger share of the private sector, dominated by males, compared to the recession that took place in the 1990s which have been the focus of many referenced studies. If we assume that males are more likely to commit crime when unemployed, then we should see a stronger relationship between crime and unemployment during our period of interest, 2007 to 2011 than during 2002 to 2006. This could of course bias our results upwards in regards to external validity. This gender-gap observation during the current recession has also been observed in the US (Sahin, Song & Hobjin 2010). If the inherent methodological problem with the gender issue can be solved this offers an interesting research question for explaining the discrepancies between the different studies given a continued lack of longitudinal data. It would naturally also improve the estimation of the consequences of unemployment.

The fact that we find correlation between youth unemployment in both our instrumental and OLS regression is reassuring. Adding in the results from Grönqvist (2011) on individuals at the margin of committing crime should add to the confidence that our instrument works in identifying criminals on margin. The major problem in interpreting our results is of course in economic terms. Seeing our OLS results for the all crime rate around 0.1% and at around 0.25% for our IV-estimates certainly cannot match quantitatively the impact of the general unemployment level estimated in other studies. But since unemployed youths are only a fraction of total unemployed the impact indicates that these results can qualitatively compare with those for other age groups.

A case can even be made that the equilibrium effects that leads us to believe that higher unemployment lowers opportunities for crime are smaller for youths. Young people might not own things in the same extent as older people. Their wealth endowment can be said to be lower. This should hold true for houses and cars, perhaps less so for mobile phones. Although speculative, it is not unreasonable to assume that there are shorter lags between unemployment and crime for youths than for experienced workers with better insurance and larger savings. Perhaps this is why we are able to see a positive correlation between unemployment and crime even in our OLS regressions while we could see nothing for the older cohorts. It is however hard to argue for a very large effect based on the results from our OLS estimations. Evidence for an undisputable link between the general youth unemployment level and crime rates is not shown but the numbers do point in that direction. What we believe to be shown is that there is a marginal group, detectable even in aggregate data, which are likely to commit crimes due to pro-longed unemployment. The large estimates for vehicle theft and property damage, crimes that run in the hundreds of thousands a year, confirm such a view[12]. While the ability of labor market programs in helping youths to find a job is debatable they seem to be able to offer a benefit to society by affecting crime, at least temporarily. Keeping an individual from crime should also be beneficial for the long-run job prospects. We also find preliminary evidence that extensive labor market programs seem to reduce crime most in categories which cannot be explained by traditional income motives.

[12] Total number of vehicle thefts 2010: 96 819. Property damage: 167 063.

7 Summary

In this section we demonstrate a link between youth unemployment and crime both using instrumental regressions and OLS. Our estimates for typical youth crimes are generally larger than those found in other studies of the overall unemployment level using only OLS estimation. Our results also sharply contradict the negative or nonexistent effect of youth unemployment found in earlier studies for Sweden. This contradicting result opens up for the possibility that there are notable structural changes for youths in Sweden between the 1990s and the beginning of the new millennia. We find that the largest effects of unemployment on crimes that arguably can be considered less financially motivated indicating a larger component of available idle time in determining youth related criminality. It is very hard to motivate in terms of income effects why our largest coefficient is found for property damage. These results fits better in a framework where more free time available leads to an increase in crime. While studies using aggregated data generally can be considered problematic in finding clear cut effects of unemployment and crime we feel confident that our results are more in line with those found by using individual data.

The reservations and question raised in this section of course make it difficult to claim that we have found a perfect model to disentangle unemployment's effects on crime. As been stressed by most others authors before us there is an obvious need for better longitudinal datasets of individuals to combat the inherent flaws of both OLS models but also those of IV-estimations where question regarding the instrument(s) validity can be raised. We also highlight the importance of how studies measures unemployment. We find evidence for a detrimental effect on crime of labor market programs for youths but also that as policies and rules change so can this effect.

7.1 Control variables

In our estimation we have not used a control variable for income since no such data exist covering the entire period. We have not been able to get access to monthly income data. We have run most of our estimations including the log of the average income extrapolated from yearly data and found no significant effect on our main variables of interest. Running our first IV-estimation between 2007 to 2010 (last year of income data) including average income yields an estimate of vehicle theft of 0.83 ($p = 0.027$) and 1.08 ($p = 0.21$) which indicates a slight bias upwards for our estimates. For property damage the corresponding coefficient becomes 1.68 ($p = 0.021$) and 1.68 ($p = 0.057$) without income so this seems to go both ways. In the sense that the general income level differs we hope this is captured in the fixed effects estimate and the change in unemployment month over month should also capture

most of the income effects. Using data of from The Swedish Social Insurance Agency on the number of new insurance claims and average number of days insured did not yield any correlations either.

8 Violent crimes

To complement our estimation of crimes we us our first specification and take a look at violent crimes in Table 13. These crimes are rarely found to positively correlate with unemployment at any level. In the individual based study by Grönqvist (2011) there is a small correlation however that makes us want to take a closer look.

Table 13 - Unemployment and Crime 2007-2011 - 2SLS

	(1) Violent Crimes	(2) Assault	(3) Assault on stranger	(4) Assault indoors	(5) Rape
Open unemployment age 18-24	0.025	0.083	0.050	-0.099	-0.080
	(0.166)	(0.157)	(0.189)	(0.144)	(0.325)
Education	-2.526	-1.641	-0.940	1.025	-7.413
	(2.036)	(2.309)	(2.467)	(2.320)	(5.360)
Males 15 - 24	0.114	-0.882	-1.200	-0.641	-1.558
	(0.910)	(1.072)	(1.644)	(1.018)	(2.911)
Foreign born males 15-24	0.050	0.193	0.354	-0.058	-0.322
	(0.174)	(0.218)	(0.267)	(0.234)	(0.802)
Share of foreign born citizens	-0.036	-3.610	-5.826	-1.896	3.641
	(4.719)	(4.379)	(7.051)	(4.257)	(15.854)
Population density	0.003	0.003	0.006	0.004	0.013
	(0.004)	(0.005)	(0.007)	(0.004)	(0.014)
Observations	1260	1260	1260	1260	1254

Standard errors in parentheses
Standard errors robust to heteroscedasticity and clustered on county-level. Due to a large number of exogenous regressors compared to clusters some of these have been partialled out to fulfill the rank condition. Fixed effects, yearly and county-specific monthly dummies included in all specifications.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

We estimate the effect on aggregated number of violent crimes, assault, assault on stranger, assault indoors and finally rape. We find a small but positive coefficient on the first categories but the standard errors are huge. Note that we lose 6 observations on the rape category due to 0 incidents. Our main model is clearly unable to pick up on any trend here despite the results of Grönqvist (2011). The estimates are similar to the insignificant and slightly positive results of Edmark (2005).

9 Conclusions

The aim of this paper was to investigate the link between youth unemployment and crime.

In our results we found a strong link between youth unemployment and crime during the turmoil in the Swedish youth labor market following the financial crisis of 2007. We found significant results both for categories of crime that are financially motivated, such as theft, and those that are not, such as property damage. The findings add evidence to the emerging view that the idle time available to commit crime is an important component of youth unemployment's effect on the crime rate.

By using monthly, instead of yearly, data we were able to identify a marginal group of unemployed that are more likely to commit crime without the need for individual data.

We also wanted to investigate why the results from earlier studies gave conflicting results even within the same country. Comparing our results with earlier studies we show that the definition of unemployment is important for the outcome of the relationship between unemployment and crime. In times where a large part of the unemployed are enrolled in labor market program there is a chance that the behavior of those inside a program and those in open unemployment differ to an extent where it is possible to get contradictory estimates. We found evidence for a positive relationship between youths in open unemployment but a negative correlation for those in a labor market program.

Further, the correlations seem to be sensitive to the nature of the program and its implementation. These problems raise serious questions about the external validity of size-estimates from one period to another when this is not taken into account.

Prior research of aggregate youth unemployment's effect on crime in Sweden has indicated a non-existent link, but our research suggests that this might no longer be the case. Our results suggest that Sweden in the 21th century is similar to both continental Europe and the US in this regard.

Focusing on the idle time component and dosage effect of unemployment in the supply of crimes, suggests that unemployment programs and training can give additional benefits outside the realm of employment figures. The most significant effects in this study are relating to crime that would typically not be prioritized by the police force, such as vandalism and bicycle theft, when compared to more serious crimes. This also implies that the effect of punishment would remain very low. Further, considering the sheer number of these crimes it might not even be economically sensible to hire more police to solve them. If labor market

programs, and certainly other activities, are able to affect non-financially motivated crimes, this should be taken into account when evaluating their effect on society.

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11 Appendix

11.1 Index of Tables

11.2 First Stage Regression tables

Table 14 - Unemployment and Crime 2007-2011 - First Stage Regression for estimations in Table 2

	(1) Open unemployment age 18-24
Participation rate JGPY	-3.728***
	(1.058)
Education	0.308
	(4.252)
Males 15 - 24	0.943
	(2.002)
Foreign born males 15-24	-0.368
	(0.335)
Share of foreign born citizens	19.183
	(11.252)
Population density	0.008
	(0.009)
Share of citizens age 25-39	-2.643
	(16.261)
Share of citizens age 18-24	11.620
	(33.592)
Share of citizens age 15-17	-54.813
	(43.428)
Share of citizens age 0-14	36.929
	(22.665)
Share of citizens age 40-64	14.949
	(14.812)
Observations	1260
R^2	0.737

Standard errors in parentheses
F test of excluded instruments: F(1,20) = 12.43. Prob > F = 0.0021.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 15 - Unemployment and Crime 2007-2011 - First Stage regression for estimates in Table 4

	(1) Open unemployment age 18-24
Participation rate JGPY	-9.065****
	(1.218)
Broad unemployment 25-64	1.235****
	(0.078)
Education	-1.358
	(3.881)
Males 15 - 24	-0.850
	(1.101)
Foreign born males 15-24	0.288
	(0.398)
Share of foreign born citizens	-6.485
	(8.903)
Population density	0.003
	(0.004)
Share of citizens age 25-39	-25.897**
	(11.146)
Share of citizens age 18-24	-7.985
	(16.156)
Share of citizens age 15-17	7.754
	(29.174)
Share of citizens age 0-14	20.293
	(15.278)
Share of citizens age 40-64	-6.466
	(10.490)
Observations	1260
R^2	0.835
F	.

Standard errors in parentheses
F test of excluded instruments: F(1, 20) = 55.46. Prob > F = 0.0000.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$