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Long term unemployment and violent crimes
- using post-2000 data to reinvestigate the relationship between unemployment and crime

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Abstract

This study reinvestigates the relationship between unemployment and crime. By being the first study to use long-term unemployment, it contributes unique findings. Moreover, with a Swedish panel consisting of 288 municipalities and annual data from 1997 to 2009, the relationship is investigated for the first time with aggregate post-2000 data. The results show that long-term unemployment exhibits a strong association with violent crimes in addition to property crimes, highlighting a potential gap in the conventional theories of economics of crime. The point-estimate of long-term unemployment for violent crimes is between 1.5 and 4, and for property crimes it is between 1.3 and 2.3. Thus, long-term unemployment identifies a marginal group for committing crimes, particularly violent crimes, better than total unemployment does. Long-term unemployment plausibly creates a feeling of alienation that fosters violent and other non-rational behaviors.

JEL classification: J2, K14, K42

Key words: crime, unemployment, long-term unemployment

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1. Introduction

Recent research has repeatedly shown that there exists a positive association between unemployment and property crimes (Mustard, 2010). The consensus is that a one-percentage-point increase in unemployment increases property crimes by one to two percent (Lin, 2008, Mustard, 2010). Empirical research has not been able to establish a similar relationship between unemployment and violent crimes. These results are in line with economic theory, which assumes that labour market opportunities affect the choice between legal and illegal activities (Ehrlich, 1973). The fact that violent crimes (opposed to property crimes) are rarely economically motivated (Levitt, 2004) explains the weak relationship between unemployment and violent crimes.

Another perspective offered by theories that focus on the anger and strain[1] of unemployment (Agnew, 1992) is that the burden of unemployment explains (primarily) violent crimes. Moreover, all types of crimes, but particularly alcohol and narcotic crimes, may also well be caused by idleness (Felson, 1998). Since these mechanisms are plausible, and in line with common intuition, it is strange that previous research using aggregated data has not found any links, in particular as individual register data shows that there exists a relationship between unemployment and violent crime (Grönqvist, 2011; Rege et al, 2009). A reason for the weak relationship between unemployment and violent crimes in aggregate data may be that the frustration and alienation of unemployment are poorly measured by total unemployment, and to capture these aspects one needs a better measure of the unemployment-related side-effects than a business-cycle measure.

The purpose of this paper is therefore to reinvestigate, using a long-term unemployment measure, the unemployment effect on crime in Sweden. Being the first to explicitly analyze long-term unemployment,[2] our study contributes unique results regarding the relationship between unemployment and crime. Thus, we find that long-term unemployment rate is a stronger predictor of violent crimes than the total unemployment rate is for property crimes.

To capture the unemployment effects on crime, one needs to use the measure that best identifies those who are likely to commit crimes. In the case of property crimes the need is to identify the group that is at the margin of substitution between the legal and the illegal sector (Mustard, 2010). As economic theory states that a higher risk of unemployment decreases the

[1] G*eneral strain theory* particularly stresses the fact that deviant behaviour is caused by the strain on an individual (Agnews, 1992).

[2] Fougère et al. (2009) estimate the long-term unemployment effect simultaneously with the youth and adult unemployment effects, and find no effect of long-term unemployment.

opportunity cost of crime, most studies use the total unemployment rate as the labour market opportunity variable. Yet, although the total unemployment level seems to identify the marginal group fairly well, the youth unemployment rate might be a better identifier (Fougère et al., 2009). Research using the variation in the wage of unskilled workers tends to identify another important group (e.g. Gould et al. 2002; Machin & Meghir, 2004).

Since the total unemployment rate fails to explain violent crimes, it might be that the potential marginal group for committing violent crimes has not yet been identified. We argue that long-term unemployment not only identifies this group much better than the total unemployment rate does, but also identifies a more selective group of individuals. The variation in the total unemployment rate identifies a group that has a relatively strong attachment to the labour market, and for those who are less likely to return to the labour market in the near future, i.e. the long-term unemployed, discount rates may be particularly high. Mustard (2010) also acknowledges that crime may be more responsive to "long-term effects" rather than "short-term fluctuations". Moreover, a Swedish study using longitudinal register data finds that youths' probability of committing crimes (both violent and property) increases with the unemployment spell (Grönqvist, 2011).

This paper uses a panel data set of 288 municipalities and annual data from 1997 to 2009. The recent ups and downs in the unemployment rate, the crises in the beginning of the decade and the current financial crisis have not been used previously to identify the unemployment effect on crimes.[3] Particularly, in comparison to earlier Swedish studies and the best international studies (Fougère et al., 2009, Gould, et al., 2002, Lin, 2008, Machin & Meghir, 2004; Raphael & Winter-Ember, 2001) on this topic, the data in this paper is extremely good. Besides the introduction of a new unemployment measure and the introduction of a large panel sample, 3,744 observations,[4] a broad and innovative battery of independent variables is included. Thus, a rich set of independent variables and regional time trends is used in order to avoid an omitted variable bias.

Reversed causation, i.e. that criminal activity reduces the employability of offenders, or that economic growth is harmed by a high crime rate in the region, may also bias the unemployment effect on crime. Recent research using instrumental variable techniques shows that the relationship between the market position and crime is underestimated due to endogeneity between unemployment and crime (Fougère et al., 2009, Gould et al., 2002, Lin,

[3] As far as we know no one has used aggregated post-2000 data and only Grönqvist (2011) and Rege et al. (2009) have used post-2000 individual register data.
[4] The largest sample is found in Gould et al., with 705 counties and a time period of 16 to 19 years.

2008, Raphael & Winter-Ember, 2001, Öster & Agell, 2007). We address the endogeneity problem by using corporate bankruptcies as an instrument. Corporate bankruptcies identify a certain flow into unemployment, i.e. an unemployment variation that is assumed to be unaffected by reversed causation, but with specific effects on crime rates.

The structure of this paper is as follows: Section 2 gives an overview of the literature. Section 3 presents the data and section 4 the econometric specifications that are used. Section 5 discusses the findings, and section 6 concludes the paper.

2. Earlier research

Early work on the relationship between unemployment and crime shows a great discrepancy between empirics and theory. This gap between empirical work and theory has characterized the literature up until the 1990s (Mustard, 2010). In a comprehensive survey of early literature Chiricos (1987) reviewed 63 studies and found that about one third of the estimates showed a significant positive relationship.

Later research, using data at local levels like cities or counties, is more likely to document relationships between labour markets and crime than research that uses larger areas of aggregation Because crime varies in important ways across even relatively small geographic areas, national or state-level data might disguise a large part of the important variation that is needed to identify causation (Levitt 2001, Mustard 2010).

Recent studies generally have in common the fact that they use a time and area fixed effect specification and a much wider array of control variables than earlier studies. Thus, the problem of omitted variable bias has in later years been acknowledged and better dealt with. With these techniques, recent research consistently concludes that labour markets affect crime rates (Mustard, 2010). A typical estimate is that a one percentage-point increase in the unemployment rate increases property crimes by one to two percent (Lin, 2008, Mustard, 2010). Because the theory is most applicable to property crimes, the literature focuses primarily on these crimes. Sometimes, violent crimes are not even analysed.

Raphael & Winter-Ebmer (2001) use US state-level panel data for the period 1971-1997 and find evidence of an unemployment effect on property crimes. Evidence of a positive effect on violent crimes is much weaker (for some violent crime-categories the estimated relationships are even strongly negative). Lin (2008) also uses US state-level data, for the period 1974-2000, and reveals that unemployment has a significant effect on crime rates, especially property crime rates, but that there is no evidence of an effect on violent crimes. Papps & Winkelman (2002) analyse panel data on sixteen New Zealand regions for the period

1984 and 1996 and find that unemployment has a positive significant effect on total committed crimes and several other sub-categories, but not on violent crimes. For Sweden, Edmark (2005) and Öster & Agell (2007) show evidence of an unemployment-effect on property crimes but no support for an effect on violent crimes. Edmark (2005) uses panel data on 21 counties in Sweden for the period 1988-1999 and Öster & Agell (2007) use Swedish municipality level data for the period 1996-2000.

Mustard (2010) argues that it is important to identify groups at the margin between a legal and illegal career in order to get better precision. In doing so, evidence of a relationship between labour-market variables and crime rates is likely to be found. Gould et al. (2002) use US county-level (705 counties, generating far more observations than any of the other studies mentioned) data on unskilled men for the period 1979-1997 and analyze the effect of wages and unemployment on crime rates. They conclude that wages seem to play a more important role in determining crime rates than unemployment rates, and the effect is stronger on property crimes than on violent crimes. Machin & Meghir (2004) use panel data on 42 areas in England and Wales for the period 1975-1996 to investigate how bottom-end wages affect crime rates. By solely focusing on property crimes they discover strong evidence of bottom-end wages affecting property crimes. In addition, they conclude that, compared to unemployment, bottom-end wages are better associated with property crime rates. Ahmed et al. (1999) maintain that higher wages in the low-skilled sectors of wholesale and retail trade reduce crime. The wage-effect is larger than the unemployment effect and the effect on property crimes is stronger than on violent crimes. Thus, studies including both wages and unemployment generally find that wages are more important than unemployment in explaining variation in crime rates (Mustard, 2010).

Fougère et al. explore different labour market variables using panel data on 95 regions in France during the period 1990-2000, with similar results to the studies above, but they also identify an unemployment effect for youth (Fougère et al., 2009). A Swedish study on the relationship between youth employment rate and crime, however, does not find such a relationship (Öster & Agell, 2007), and neither does Lin (2008).

Moreover, a certain aspect of Fougère et al. (2009) is of special interest for the purpose of this paper, They are the only ones who address the effects of long-term unemployment on crime rates using aggregated data, but do not find a significant effect on crime rates. Nevertheless, this is not the main purpose of the paper and thus not thoroughly investigated. For example, the long-term unemployment variable is never regressed on the crime-categories without simultaneously using other labour-market variables as regressors. Using Swedish individual-data for 1992-2005, Grönqvist (2011) documents a relationship between the length of the unemployment spell and the probability of committing crimes.

In sum, there seems to be a consensus in recent studies that violent crimes almost never exhibit dependence on unemployment or any other labour-market variable. But, both Raphael & Winter-Ebmer (2001) and Lin (2008) doubt the correctness of these estimates and provide a discussion on the issue. First, both point out that if failing to control for crime-related commodity variables that are pro-cyclical, such as alcohol, guns, and drugs, there is a risk of underestimating the true effects of unemployment on various crimes. Levitt (2004) argues that since most crime-related commodities, such as alcohol and cocaine, are normal goods, economic improvements can have negative impacts on crime. Lin (2008) sums up the discussion by saying that the negative unemployment effect on violent crimes most likely is a consequence of an omitted variable bias. In addition, Raphael & Winter-Ember (2001) suggest another plausible explanation for the intuitively odd relationship between unemployment and violent crime. The exposure to offenders might be greater in good times, thus masking the negative effect of unemployment on violent crimes.

Another possible explanation for a negative relationship is proposed by Poutvaara & Priks (2007), who suggest that the willingness to pay for gang membership is reduced when the unemployment rate decreases. As a response, it may be optimal for gang leaders to establish a higher level of gang membership requirement that only the most dedicated gang members can meet. A decrease in the unemployment rate therefore results in smaller but more violent gangs. Hence, criminals substitute property crimes for violent crimes. Following the argumentation of Raphael & Winter-Ebmer (2001) and Levitt (2004), Lin (2008) introduces a “state crack cocaine index” calculated by Fryer et al. (2005) as a control variable in all regressions. The unemployment effect on violent crimes becomes positive but insignificant. Hence, this shows that when controlling for crime-related commodities the estimated effects on violent crimes go in a positive direction.

Moreover, Levitt has highlighted, several times, the importance of including deterrence variables in order to explain criminal activity in the US. Levitt (1996) finds that prison population size affects crime rates negatively through the channels of deterrence and incapacitation. Levitt (1997) also shows that changes in the police force affect crime rates negatively. The effect on violent crimes is shown to be larger than on property crimes.

Most studies use aggregated data, but there are good reasons for conducting studies with micro-data, which is appropriate since the theories are built upon individual behaviour (Eide et al., 2006). Grogger (1998) uses the dataset NLSY, which is US panel data containing a representative sample of youth who were first interviewed in 1979. He finds evidence of a relationship between the wages of youths and property crimes, but does not touch on violent crimes. Gould et al. (2002) use individual data alongside aggregated data and conclude that there is strong support for the aggregated data results. Rege et al. (2009) use Norwegian longitudinal data for the period 1992-2005 to investigate the effects of plant closure on criminal activity. Surprisingly, they find no effect on property crimes, but there is an association with violent crimes. As previously mentioned, Grönqvist (2011) also uses individual data and reveals an association between unemployment and violent crime.

Finally, some comments on the use of instrumental variables and its effect on the estimates. Controlling for endogeneity has not been used to study labour-market effects on crime until recently, although it has a long history in other fields of economics (Mustard, 2010). Several papers discuss the possibilities of reverse causality. Cullen & Levitt (1999) suggest that high-income individuals or employers leave areas with higher or increasing crime rates. Gould et al. (2002) claim that crime might make businesses relocate to areas with lower crime rates. Willis (1997) notes that low-wage employers in the service sector are more likely to relocate due to increasing crime rates. Mustard (2010) concludes that ”Some, but not all, studies report substantially larger estimated effects with 2SLS than with OLS”. As an exampel, Lin (2008) reports estimates on property crimes that are approximately three times as high when using 2SLS methods as when using OLS. Thus, OLS might severely underestimate the true effects of unemployment on crime.

3. Data and Descriptive Statistics

The panel data set consists of annual data for 288[5] municipalities over the period 1997 to 2009. None of the (aggregate) studies mentioned above have used post-2000 data. In addition, the relatively long time period and the number of municipalities give a larger number of observations in comparison. Studies with US data typically use state-level data and thus have a lot fewer observations. Gould et al. (2002) use county-level US data and is the only one (of the above mentioned) with more observations than this paper.

Unemployment data at municipality level has been collected from The National Labour Market Board in Sweden. The unemployment variable used in this paper is added idle unemployed and unemployed participating in labour market programs. This sum is often referred to as total unemployment. Generally, the vast majority across the country are idle

[5] The municipalities of Nykvarn and Knivsta were created in 1999 and 2003, respectively, and are therefore excluded. These new municipalities are very small.

unemployed. Statistics Sweden provides us with the long-term unemployment variable. An individual in the age group 20-24 is considered long-term unemployed if he has been registered at the National Labour Market Board as unemployed for more than 100 days. For the age group 25-64 the period is 6 months or longer. Like earlier Swedish studies, all our unemployment variables have total population in the relevant age group in the denominator and hence not the labour force. As argued by Fougère et al. (2009), this is a more effective measurement since variations in the labour force might otherwise create noise in the unemployment rate.

As shown in Figure 1 the national unemployment rates vary a lot during the period 1997 to 2009. Because long-term unemployment does not seem to vary much in comparison to total unemployment, one might incorrectly draw the conclusion that there is not enough variation in the long-term unemployment variable to convincingly identify a link to the dependant variable. However, if we examine the standardized national unemployment measures, another picture emerges. The standardized measures (subtracting the mean and dividing by the standard deviation)[6] in Figure 2 show that the relative variation is rather similar at the aggregate level, though. Actually, on the individual municipality level the variation in the long-term unemployment measure is even larger than for total unemployment. Thus, by computing the relative change:

$$(\text{Unempl}_{i,t+1} - \text{Unempl}_{it})/\text{mean}(\text{Unempl}_{i}) \quad \text{where } i \text{ is municipality and } t \text{ is time}$$

in the municipality (long-term) unemployment levels, and taking the standard deviation of these measures, we get an estimate that can be used for comparing the variation in the unemployment measures. This exercise gives 0.22 for the total unemployment variable and 0.37 for the long-term unemployment variable. Thus, in relative terms, long-term unemployment exhibits an about 1.65 times larger variation than total unemployment.

Figure 1 and Figure 2about here

We further investigate the behaviour of long-term unemployment and total unemployment in comparison to each other. Do they follow the same pattern? If not, the variables certainly capture different phenomena and hence will not show the same correlation to the dependant variable. By comparing the number of times the unemployment measures move in the same direction (simultaneously up or down) and in opposite directions (one goes up while the other goes down or exhibits no change, and vice versa), it is clear that the two

[6] Because the standardized measures are computed from unemployment at the municipality levels, we also weight with municipality population size.

unemployment measures do not always follow the same pattern and thus cannot have the same effect on crime rates. In almost 30% of the cases the rates go in opposite directions (or one is unchanged), and in about 19% of the cases it is the long-term unemployment rate that increases (or are unchanged) and the total unemployment rate that decreases. In 11% of the cases it is the long-term unemployment rate that decreases (or is unchanged) and the total unemployment rate that increases.

Data on crime rates, reported as crimes per 100,000 inhabitants, is collected from The National Council for Crime Prevention (NCCP). Property crimes in this paper include Burglary, Thefts and Pilfering, Thefts from Vehicles and Handling Stolen Property. In comparison to the categorization by NCCP, we exclude Vehicle Thefts.[7] Due to technological advances, which make vehicles difficult to steal, there has been a very large drop in vehicle thefts (NCCP, 2008); almost 60 percent since the beginning of the 90s. A large and significantly negative relationship between vehicle thefts and unemployment is probably due to this circumstance. For violent crimes the categorization follows that categorization done by NCCP, with one exception - robbery is excluded from violent crimes. The reason for this is that the motive for robbery is mostly monetary. Figure 3 illustrates the share of crimes for the two categories Property crimes and Violent crimes.

Figure 3 about here

The overall crime rate has increased by 10 to 20 percent since 1985, as shown in Figure 4. The figure also illustrates the change in the property crime rate and the violent crime rate, separately. For property crimes we find a steady decrease in crime rates during the last century; they have decreased by about 30 percent since 2000. An entirely different trend is found for violent crimes, which have gradually increased for a long time, and by more than 40 percent from 1997 to 2009 (our time period).

Figure 4 about here

In the long run, the actual crime rate most likely follows the curve of reported crimes. But the propensity to report a crime has probably increased or decreased during different periods of time, which makes it hard to evaluate, at a given time, how well the curve represents actual committed crimes (NCCP, 2006).

Since crime has to be reported in order to benefit from insurance, property crimes in general have a relatively high reporting rate. The property crime category includes a wide

[7] Weapon Thefts (and Other Thefts) are excluded because weapons may be used when committing a violent crime. These thefts are very few, thus excluding them does not change the results. Robbery are by NCCP categorized as both a violent- and a property crime.

variety of crimes that affects individuals, households or businesses, all with different propensities to report crimes. This makes it difficult to assess whether there is an actual reduction of overall property crimes. In the case of property crimes that affect individuals and households, the declining trend is not supported by the results of recent surveys (NCCP, 2008).

The propensity to report violent crimes is definitely lower than for property crimes. Mainly two things determine the propensity to report a violent crime. First, the more severe a violent crime is, the more likely it is to be reported. Second, if the victim and the offender know each other the crime is less likely to be reported. Hence, severe violence between unknowns (e.g. street violence) are more likely to be reported than violence within a family (NCCP, 2008). In contrast to property crimes, there seems to be a widely supported explanation for the changes in violent crime rates. Recent surveys show that a higher propensity to report a violent crime is the explanation for the increase in the number of reported violent crimes. This is true for every crime included in the violent crime category in this paper.

If the propensity to report a crime changes over time in the same way across all municipalities or varies across municipalities but remains constant over time, then the fixed effect estimate is unbiased due to measurement errors. As Eide et al. (2006) point out, this seems to be an implicit assumption in most studies. Nevertheless, the propensity to report a crime does vary across the country, and there is some evidence that the difference in reporting rates between different areas of Sweden has varied over time. NCCP reports that this difference seemed to decrease during the 90s. In contrast, recent studies do find that this gap might have increased again in the beginning of the 2000s. The evidence of a variation of the gap is far from clear-cut (NCCP, 2008), but by including linear and quadratic municipality-specific time trends, such a variation can be dealt with.

4. Econometric Specification

The model used in this paper is in line with that in recent papers on the topic. That is, by means of a fixed effect model with a full set of time and area-dummies, we get the within-municipality variation in crime rates and unemployment level to identify the unemployment effect on crime.

$$Log(Crime_{it}) = \alpha_i + \delta_t + \beta Unempl_{it} + \rho X_{it} + Y_i + Y^2_i + \varepsilon_{it} \qquad (1)$$

In this model, i and t are indices for municipality and time, respectively. $Crime_{it}$ is the number of crimes per 100,000 inhabitants. $Unempl_{it}$ is a variable representing the particular type of unemployment. α_i is the municipality fixed effects and δ_t is the time fixed effects. X_{it} is a vector of control variables, and Y_i and Y^2_i are municipality-specific (linear and quadratic) time trends.

One of the advantages in this paper is that we use a very broad set of control variables. A complete list of control variables with descriptive statistics and sources is shown in Table A1.

According to economic theory a high income level in a region could increase or decrease crime rates. Thus, whereas a high income or wage level in a region makes crime less attractive, the returns to crime, on the other hand, get become higher (i.e. there are more valuable goods to steal). Due to this, we add both the *Logarithm of income* and the *Logarithm of the first-difference in income*[8] to the model. With these variables we might capture the diverse effects of income on crime. The mean per capita income (gross-income for individuals aged 20 or older) in the municipality is collected from Statistics Sweden and is deflated with the Consumer Price Index (also collected from Statistics Sweden).

Men and youth are highly over represented among criminals,[9] and therefore the *Share of men* and the demographic age structure in the municipality are included in the specification. The *Share with higher education* **(**three years of undergraduate education or more**)** and the *Share with a foreign background* (including both first and second generation immigrants) are also added. Some previous empirical papers have separated foreigners into different groups (see for example Fougère et. al, 2009), but the data does not allow such a classification in this paper.

It is a well-known fact that crime rates are higher in larger cities (NCCP, 2008). To catch this phenomenon *Population density* (inhabitants per km^2) and *Logarithmic population size* are added.

Some previous papers include alcohol consumption (Edmark, 2005; Lin, 2008; Raphael & Winter-Ebmer, 2001) as a control variable, particularly as it is assumed to induce violent crimes. But since alcohol consumption is procyclical, it is uncertain whether the omission of alcohol consumption biases the unemployment effect on crime positively or negatively (Raphael & Winter-Ebmer, 2001). In this paper we add, instead of alcohol consumption (litres of alcohol purchased at the state run liquor shop, “Systembolaget”), the number of alcohol

[8] Another reason to add the first-difference is that municipality income levels are non-stationary.
[9] Different age groups are also victims of different crimes (NCCP, 2008).

permits (per 1,000 population 15 years or older) in the municipality. We prefer this variable, as it is closely correlated to alcohol consumption, and contains less measurement problems. There are several missing values in the alcohol consumption measure, and since the smuggling of alcohol (into and out of Sweden) increased largely during the time period,[10] alcohol consumption is a problematic variable. Few violent crimes seem to be committed under the influence of narcotics in comparison with alcohol (NCCP, 2008) and it is therefore not necessary to control for narcotic use.

The variables *Conviction rate* and *Police force* are collected at the county level. The *Police force* is the number of policemen (and the number of civilians employed by the police) per thousand population. *Conviction rate* is the number of convictions divided by the total number of crimes.

The National Council for Crime Prevention (NCCP, 2002) finds *Proportion divorced, Election participation* and *In and outflow* of individuals to be important explanatory variables of crime rates in Sweden. The propensity to commit crime might be higher among those growing up with divorced parents, hence there is reasonable justification for including *Proportion divorced* as a control variable. The variable is defined as the number of divorced divided by the sum of divorced and married people. This variable and *Election participation* also represent the social capital in a municipality. The elections considered are those to the Swedish National Parliament. In the time period studied, elections were held three times, 1998, 2002 and 2006. To increase the variation in election participation and to capture the change between elections, we have constructed a weighted election participation rate. That is, let EP_t and EP_{t+4} be the election participating rates of two subsequent elections. Hence:

$$EP_{t+r}=EP_t \times (5-r)/6 + EP_{t+4} \times (1+r)/6 \qquad \text{where } r=1, 2 \text{ or } 3$$

is the formula for computing the three fictive election participation rates between the real election years t and t+4. The in and outflow of individuals to and from a municipality (divided by the total population of the municipality) captures migration patterns. Thus, by including a rich variety of crime explanatories (many not included in economics papers before) the risk of spurious correlation is small.

5. Results

The baseline results of the paper are shown in Table 1 and the results from regressions with

[10] In the municipality of Strömstad for example, border trade makes the consumption of alcohol 70 litre (pure alcohol) per person a year.

added municipality-specific time trends are shown in Table 2. In contrast to other studies on this topic, all regressions are run both weighted and non-weighted by population size and these results are also shown. It turns out that this distinction itself gives some interesting findings.

The results from the weighted specification in Table 1 show that total unemployment has a significant effect on property crimes but not on violent crimes. This is in line with both economic theory and earlier research. Assuming that the point-estimate is correct, a one percentage-point increase in total unemployment generates a 0.85 percent increase in property crimes. Hence, the effect is relatively small and it is more or less a standard result. However, Table 1 also reveals a much more interesting result. In the weighted model, long-term unemployment shows a significant effect on both property and violent crimes with point estimates of 2.139 and 1.563, respectively. Long-term unemployment does not just seem to affect violent crimes in addition to property crimes, but the estimated effect also seems to be larger on both crime categories.

The non-weighted results also show another interesting aspect. All estimates in these specifications are larger, and the greatest increase in the size of the coefficient when going from a weighted to a non-weighted specification is given by the long-term unemployment variable when regressed on violent crimes. The implication of this result must be that the unemployment effect, especially the long-term unemployment effect, on violent crimes is stronger in smaller municipalities. This assertion is confirmed when we run regressions (not reported) on municipalities with fewer and more than 15,000 inhabitants separately (which almost splits the sample into two equally sized groups). In these regressions the long-term unemployment effect on violent crimes is about three times as large for the smaller municipalities as for the large ones.

Table 2 shows alternative regressions with added linear and quadratic municipality-specific time trends. The results become somewhat more mixed. Whereas the effect on property crimes decreases the effect on violent crimes actually increases (a similar result is reported in Raphael & Winter-Ebmer, 2001). Moreover, both total unemployment and long-term unemployment lose their significant effects on property crimes, and total unemployment shows a significant effect on violent crimes instead. But the non-weighted specifications still give larger coefficients, although not in every single case.

Nevertheless, the long-term unemployment variable, which is in focus in this paper, still exhibits a strong significant effect on violent crimes in every alternative specification. In addition, the size of its coefficient becomes even larger when adding the municipality-specific time trends. In sum, two important results from the baseline model are consolidated by the alternative specification. First, long-term unemployment exhibits a greater effect on crime rates, and especially violent crime rates, than total unemployment. Second, the assertion that the long-term unemployment effect on violent crimes is larger in smaller municipalities is confirmed. Moreover, probably because of the large increase in reported (as opposed to actual) violent crime rates during the time period, the unemployment effect on violent crimes may be underestimated without municipality-specific time trends.

Finally, some comments on the coefficients of the control variables. In the baseline weighted specification, as can be seen in Table 1, most of the control variables show a significant effect and with the expected signs. *In and outflow*, *Election participation* and *Conviction rate* are always significant with the expected sign when running the two unemployment measures on both crime categories. The *Share of Men*, *Share of Immigrants*, *Share divorced*, and *Alcohol permits* go in the expected direction, but are not always significant. Of course, the direction of causation can be debated, but there is indeed evidence of associations. Some changes do occur in the non-weighted model, but no clear pattern is seen. As expected, when we add municipality-specific time trends (not reported), some control variables lose their significance. The deterrence variables seem to be the most robust variables towards changes in specifications.

The rest of the control variables show both expected and unexpected signs depending on the specification and type of crime. The *Share with higher education* for example is significantly negative for property crimes and positive for violent crimes. Further, *Police force* is significantly positive for violent crimes, which may be due to responses to a high violent crime rate. However, since one could always come up with plausible explanations (particularly in a fixed effect framework) for the unexpected results, we prefer not to speculate further. We conclude that the control variables behave reasonably well and that the fit of the model (the R^2-value) is good, particularly in the weighted specification with time trends.

5.1 Adding social allowance recipients

Social allowance recipients, in addition to the long-term unemployed, are another group at the margin of committing crimes. To analyze whether crime rates vary with the share of social allowance recipients, we add such a variable into the model. The results without time trends (column 1 to 4) in Table 3 show that an increase in the share of social allowance recipients increases both property and violent crimes by about 2 percent. In the model with time trends (columns 5 to 8) social allowance recipients only affect violent crimes.

Because many of the long-term unemployed are also social allowance recipients, the long-term unemployment effect could be partly an effect of being a social allowance recipient. Although the unemployment effects decrease by 0.3 on average when including the share receiving social allowance, the results still show that unemployment predicts crime rates. Particularly, the long-term unemployment effects remain large and often significant, thus indicating that long-tem unemployment and social allowance are both indicators of alienation, each with a distinct effect on crime rates.

5.2 Linear unemployment effects?

Moreover, when the total unemployment rate and the long-term unemployment rate increase, the marginal individual is assumed to be less selective, and therefore the effect on crime rates may be decreasing. Thus, by including squared unemployment rates into the model we ascertain whether the unemployment effects are linear or not.

The squared unemployment estimates in Table 4 are mostly negative, but it is only the squared long-term unemployment effect on violent crimes that is significantly negative. Thus, in line with our expectation, the marginal individual seems to be decreasingly prone to committing crime. Because a significant squared estimate is found only for violent crime, the theories that focus on the burdens of unemployment propose another explanation. By assuming that the stigma of long-term unemployment is larger when the long-term unemployment rate is low, a rise in the long-term unemployment rates might decrease the strain of unemployment, and so also the negative externality on violent crime rates.

5.3 The effect of lagged and lead unemployment

To test whether the unemployment effects are truly causal, one can estimate the relationship between crime rates and lagged (t-1) and lead (t+1) unemployment rates. If these effects are significant, or relatively large, causality may be questioned. Still, weak relationships are consistent with dependence on unemployment and crime rates. For example, if criminal behavior is “contagious” or there are hysteresis effects, i.e. that a temporary high crime rate leads to a permanent increase in the criminal human capital (Naci et al., 2005), future crime rates may be positively associated with the current unemployment rate.

Table 5 shows the lagged (t-1) and lead (t+1) unemployment effects. As before, we present the weighted results with and without time trends. The large and significant relationship (found in Table 1) between the long-term unemployment rate and violent crimes is insignificant here for both the lagged and lead specifications. For property crimes the lagged and lead effects are, in general, significant and larger than the total (current) unemployment effects. In the model without time trends, the lagged and lead total unemployment effects on property crimes are both positive, but in the model with time trends the lagged effect is negative whereas the lead effect is positive. Thus, the conclusion from this exercise is that the long-term unemployment effects on violent crimes pass the test, but the unemployment (both the total and long-term) effects on property crimes may be spurious. The positive relationship between the current property crime rate and tomorrow's total unemployment rate may be caused by a reversed causation from property crime rates to the local labour market.

5.5 Using corporate bankruptcies as an instrument

With reversed causation the unemployment effect on crime may be biased. That is, if criminal activity reduces the employability of offenders or economic growth is harmed by a high crime rate, the unemployment effect is assumed to be overstated. However, as is often the case when using instruments, the IV-estimates turn up larger than the OLS-estimates (Fougère et al., 2009, Gould, et al., 2002, Lin, 2008, Raphael & Winter-Ember, 2001, Öster & Agell, 2007). A high IV-estimate indicates that the instrument identifies a change in the unemployment rate that affects crimes more than the average change, i.e. a *local average partial effect* is identified.[11] Thus, these findings point out that we need to learn more about the causes of the unemployment effect on crime, and what the key variation in unemployment is. This is further analyzed in an instrumental variable framework.

Besides recruitments and layoffs in existing firms (and the public sector) and the start-up and closing of firms, corporate bankruptcies may explain changes in the unemployment rate. The special feature of bankruptcies is that they are unlikely to be caused by crime rate in the region. Hence, as a response to increasing crime rates, firms may want to close down, decrease their activity or move to another location. On the contrary, bankruptcies are involuntary and therefore, plausibly, less sensitive to changes in crime rates. On the other hand, bankruptcies are likely to affect crime rates through their direct effect on unemployment, but they are not assumed to have a direct effect on crime above their influence on unemployment. At least, any such effect is very much related to what we

[11] High IV-estimates could also be caused by a weak instrument (Murray, 2006), but this does not seem to be the case in, for example, Lin (2008) and Raphael & Winter-Ember (2001).

consider as part of the unemployment effect. For example, if corporate bankruptcies influence social trust or make people feel dejected, this is one (among others) channel going from unemployment to crime. Thus, here the instrument constitutes one flow into unemployment, and in that respect it is not a conventional instrument. The contribution is rather; i) reversed causation should not plague corporate bankruptcies, and ii) a specific inflow into unemployment is analyzed.

As we will see, the number of *Bankruptcies in firms without employees* has a more differential effect on crime rates than the number of *Bankruptcies in firms with employees*, and therefore these two instruments are used separately. We begin with the IV-result, before discussing the pitfalls of instruments, for example whether the instruments are weak.

Table 7 reports large and significant unemployment effects on violent crimes when using the instrument *Bankruptcies in firms with employees* (current and lagged). The unemployment effect on property crimes is, however, insignificantly negative when using the same instrument. The IV-estimate is particularly large for the relationship between long-term unemployment effects and violent crimes. In contrast, when using the instrument *Bankruptcies in firms without employees* it is the other way around; here, the unemployment effects on property crimes are large and significant. Hence, if these IV-estimates are even remotely liable, it shows that the OLS-estimates are (largely) underestimated. Considering that this might be local effects, a somewhat weaker but also important conclusion is that bankruptcies have large implications for crime rates.

Whether the instruments are valid, i.e. exogenous, is to some extent an open question. The second problem is whether the instruments are weak. Weak instruments give biased estimates and underestimated standard errors (Murray, 2006; Stock, Wright & Yogo, 2002). In some of the first-stage regressions it seems as if the instruments are somewhat weak. With only one endogenous variable the test statistic is the same F-statistic as when testing if the instruments all have zero coefficients (Stock & Yogo, 2005). The same test-statistics apply to both tests, i.e. whether the estimate is biased (the Stock-Yogo test for reduced bias) and whether the standard error is underestimated. A rule of thumb is that the test-statistic should be above 10.[12] For the instrument *Bankruptcies in firms with employees* on long-term unemployment the F-statistic is only 4.71, and for *Bankruptcies in firms without employees* on total unemployment the F-statistic is 5.1. In these cases the instruments seem to be somewhat weak, indicating biased coefficients and underestimated standard errors. Also, the sizes of the

[12] Actually, the Stock-Yogo test for reduced bias cannot be conducted with fewer than three instruments.

estimates indicate misspecification, so with this in mind it is actually more surprising to find that the other two specifications demonstrate strong instruments. In the model with time trends the instruments turn up weak, i.e. in this specification the time trends (together with the fixed effects) remove too much of the variation in the unemployment variables.

6. Conclusion

This paper finds that long-term unemployment exhibits a strong association with violent crimes in addition to property crimes. The point-estimate of long-term unemployment is; for violent crimes between 1.5 and 4, and for property crimes between 1.3 and 2.3. The total unemployment effect on property crimes is significant but relatively small, 0.8, which indicates that the long-term unemployment measure captures a specific mechanism in comparison to what is captured with the total unemployment measure. Hence, since the long-term unemployment effect in many of our specification is larger for violent crimes than for property crimes, there is not only a quantitative difference in the results, but also a qualitative difference. That is, if long-term unemployment primarily identified the marginal group for committing crimes better than total unemployment, the effect would be larger, as in most other studies, for property crimes than for violent crimes. This highlights a potential gap in the conventional theories of economics of crime.

Although the results seem to be somewhat sensitive to the specification, the long-term unemployment effects on violent crimes are significant throughout. All other associations (total unemployment on property crimes and violent crimes, and long-term unemployment on property crimes) show up significant in some specifications and non-significant in others. Moreover, since long-term unemployment-violent crimes is the only relationship that shows the expected result in a lag and lead framework, it indicates a causal relationship between the variables.

Why does long-term unemployment, and not total unemployment, increase violent crime? This cannot be explained with conventional theory on the economics of crime. Because crime rates (especially violent crime rates) also vary positively with the share receiving social allowance, being far from the labour market seems to matter. Perhaps long-term unemployment (and receiving social allowance) creates a feeling of alienation that fosters violent and other non-rational behaviors.

The finding that the long-term unemployment effect is decreasing with the long-term unemployment level may explain why total unemployment is a poor identifier of the marginal group for committing violent crimes. Hence, with a decreasing effect, the normal variation in the total unemployment level (5 to 10 percent) probably identifies a group that is less selective, and therefore less prone to violent crimes. The non-weighted specifications, which give higher estimates, indicate that the marginal group is larger in smaller municipalities. The stigma of long-term unemployment might also be larger in small municipalities.

Furthermore, the IV-results also illustrate the importance of identifying the important pathways from unemployment to crimes. Thus, besides indicating that the OLS-estimates are biased downward rather than upward, the large IV-estimates show that unemployment related to bankruptcies has large impacts on crime.

With the above results in mind it is clear that the costs of unemployment are not only direct costs in the form of unemployment benefits etc, but also indirect costs in the form of costs of crime. Previous research on the social costs of crime generally concludes that while property crimes incur great costs to society, violent crimes are generally many times more costly to society. The link between long- term unemployment and violent crime can therefore be of substantial economic importance at the aggregated level. For a discussion on the social costs of crime and different methods of estimation see; Cohen et. al. (2004), Cohen (1998), Miller et. al. (1996) and Boardman et. al. (2011).

Even though the effect of long-term unemployment on violent crimes is plausible and likely to be causal, the choice of time period might play a role in the identifying of such an effect. There is therefore a need for studies using post-2000 data, to confirm that there has not been an overall change in the unemployment-crime patterns.

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Tables and figures

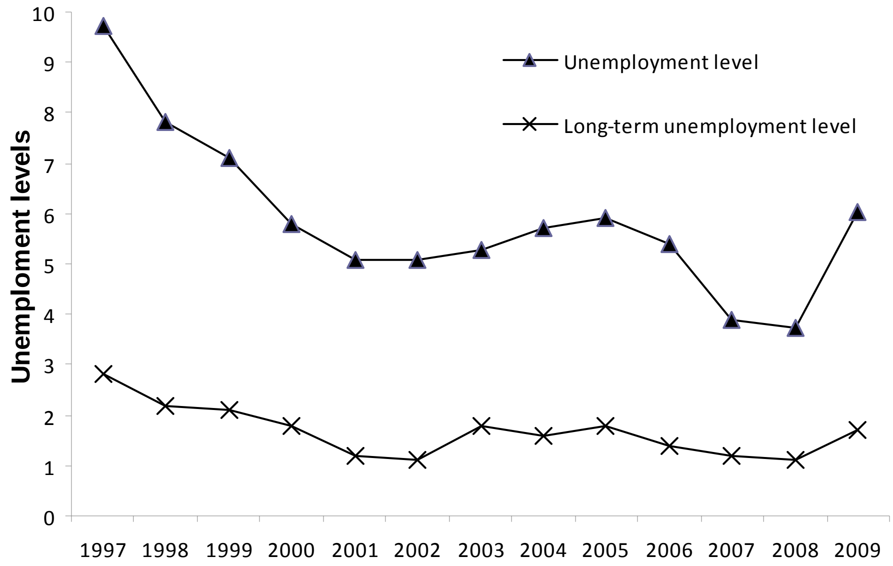

Figure 1. Graph of Unemployment Rates 1997-2009.

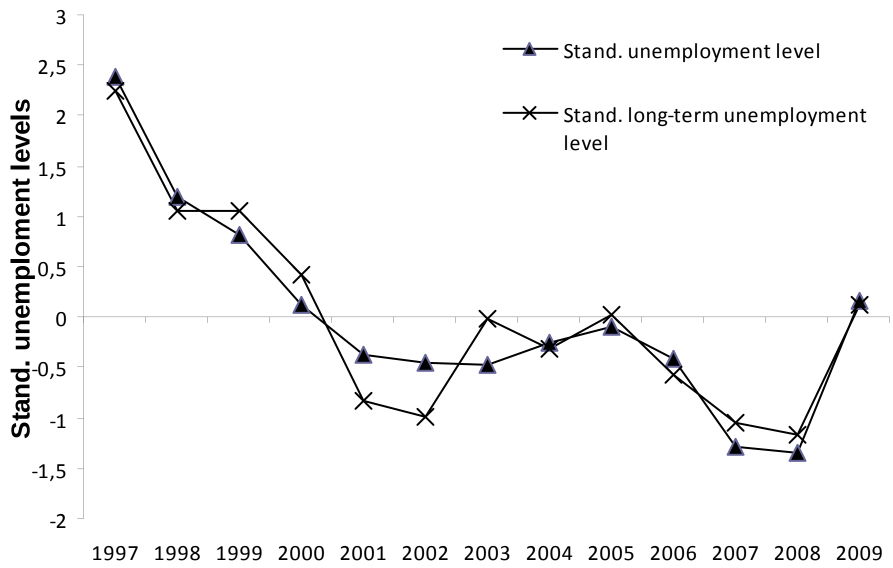

Figure 2. Standardized unemployment levels, 1997-2009.

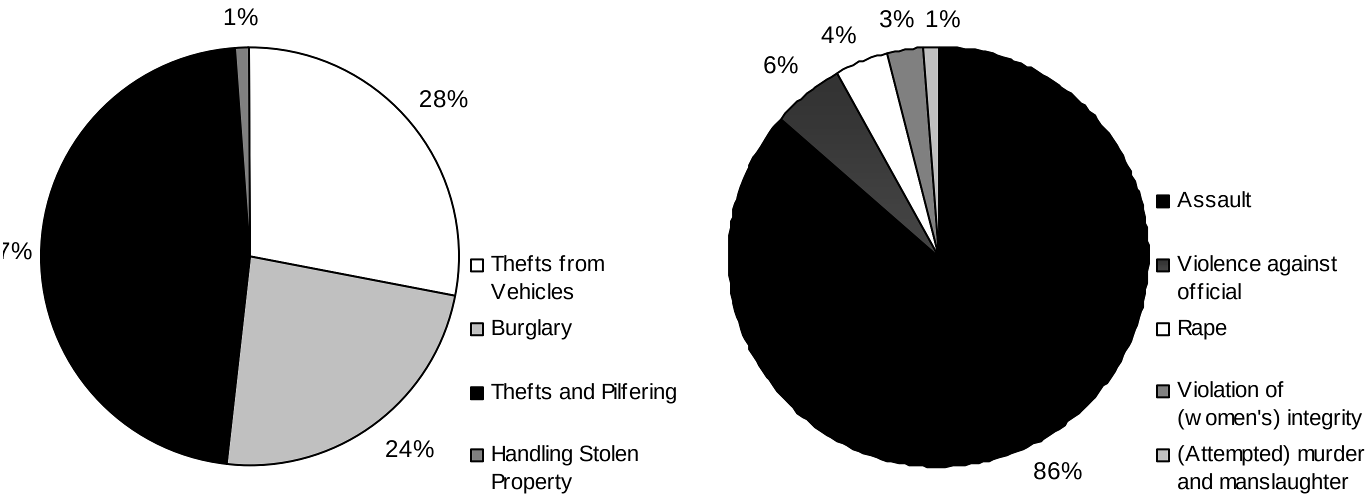

Figure 3. Illustrating the share of crimes in each crime category.

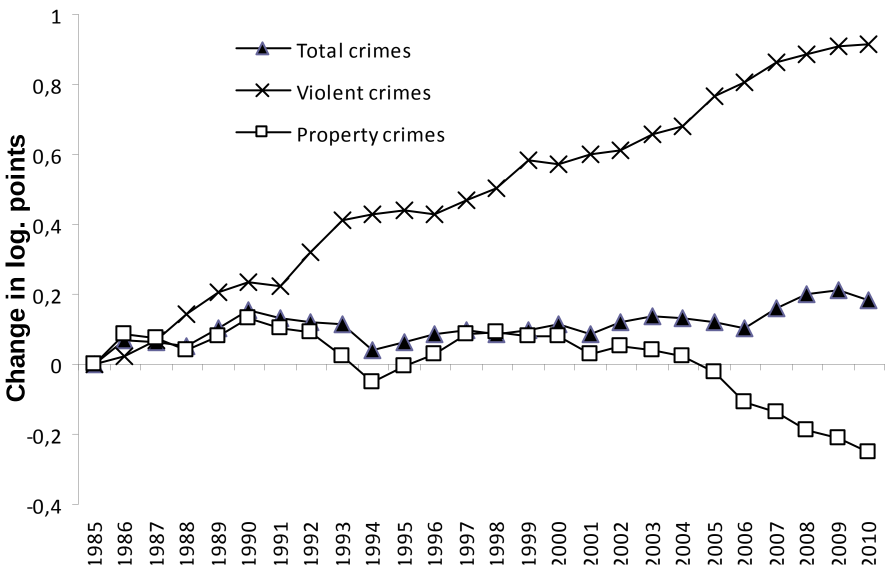

Figure 4. Illustrating the change (log.points) in crimes, 1985-2010.

Table 1. Estimating the effect of unemployment and long-term unemployment on property and violent crimes.

	Weighted				Non-weighted			
	Property	*Violent*	*Property*	*Violent*	*Property*	*Violent*	*Property*	*Violent*
Unemployment	0.850	0.520			1.374	0.572		
	(0.358)**	(0.413)			(0.374)***	(0.521)		
Long-term unemployment			2.139	1.563			2.313	2.295
			(0.669)***	(0.774)**			(0.739)***	(1.009)**
Ln first-diff. in income	0.003	0.006	0.004	0.006	0.003	0.003	0.002	0.004
	(0.002)*	(0.002)***	(0.002)**	(0.002)***	(0.002)	(0.003)	(0.002)	(0.002)*
Ln income	0.255	-0.588	0.262	-0.596	0.708	-0.687	0.633	-0.897
	(0.247)	(0.258)**	(0.246)	(0.256)**	(0.254)***	(0.369)*	(0.251)**	(0.321)***
Share: 0-15	-0.406	-0.300	-0.311	-0.237	-1.275	2.262	-1.388	1.777
	(0.948)	(0.993)	(0.950)	(0.986)	(0.912)	(1.493)	(0.908)	(1.298)
Share: 16-20	-4.614	5.184	-4.422	5.382	-1.750	8.104	-2.049	7.541
	(1.375)***	(1.614)***	(1.388)***	(1.603)***	(1.344)	(2.477)***	(1.343)	(1.971)***
Share: 21-25	-3.694	-0.982	-3.482	-0.947	-2.730	2.349	-2.968	1.411
	(1.336)***	(1.368)	(1.341)***	(1.372)	(1.352)**	(2.100)	(1.344)**	(1.885)
Share:26-35	-1.497	-2.726	-1.421	-2.595	1.595	0.416	1.418	0.171
	(0.886)*	(0.993)***	(0.877)	(0.990)***	(0.924)*	(1.380)	(0.919)	(1.274)
Share: 35-45	-1.386	-0.913	-1.258	-1.018	1.911	-0.204	1.976	-1.357
	(0.988)	(1.050)	(0.986)	(1.041)	(1.012)*	(1.669)	(1.000)**	(1.409)
Share of men	3.755	0.397	3.981	0.262	1.797	0.623	2.249	0.167
	(1.968)*	(2.138)	(1.977)**	(2.122)	(1.871)	(3.027)	(1.881)	(2.696)
Share with for. background	1.336	0.273	1.373	0.299	2.803	-0.082	2.810	-0.118
	(0.541)**	(0.537)	(0.540)**	(0.538)	(0.514)***	(0.710)	(0.514)***	(0.697)
Share with higher educ.	-1.321	1.368	-1.182	1.428	-2.720	-0.927	-2.559	-0.848
	(0.707)*	(0.736)*	(0.697)*	(0.732)*	(0.741)***	(0.947)	(0.730)***	(0.930)
Population density	0.000	-0.000	-0.000	-0.000	-0.000	-0.001	-0.000	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)***	(0.000)	(0.000)***
Ln population size	-0.232	-0.682	-0.219	-0.667	-0.353	-0.422	-0.279	-0.315
	(0.170)	(0.173)***	(0.170)	(0.172)***	(0.175)**	(0.248)*	(0.175)	(0.230)
Proportion divorced	1.131	4.063	1.057	3.930	1.414	2.882	1.137	2.231
	(0.747)	(0.904)***	(0.752)	(0.894)***	(0.791)*	(1.300)**	(0.788)	(1.079)**
In- and outflow	1.700	1.175	1.627	1.049	1.487	3.389	1.396	2.942
	(0.467)***	(0.506)**	(0.466)***	(0.502)**	(0.474)***	(0.684)***	(0.473)***	(0.602)***
Election participation	-2.013	-1.585	-2.052	-1.613	-1.488	-1.336	-1.404	-1.246
	(0.599)***	(0.653)**	(0.597)***	(0.649)**	(0.586)**	(0.835)	(0.589)**	(0.829)
Alcohol permits	0.003	0.001	0.002	0.001	0.001	0.001	0.001	0.001
	(0.001)*	(0.002)	(0.001)*	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Conviction rate	-2.135	-0.737	-2.216	-0.855	-2.087	-0.590	-2.281	-1.200
	(0.432)***	(0.386)*	(0.434)***	(0.383)**	(0.415)***	(0.693)	(0.409)***	(0.524)**
Police force	-0.072	0.165	-0.084	0.156	-0.095	0.120	-0.101	0.114
	(0.033)**	(0.035)***	(0.033)**	(0.035)***	(0.039)**	(0.049)**	(0.039)***	(0.049)**
Observations	3,744	3,744	3,727	3,727	3,744	3,744	3,727	3,727
R-squared	0.92	0.90	0.92	0.91	0.82	0.78	0.82	0.81

Notes: The dependent variables are the logarithmic numbers of crime per 100,000 inhabitants. Unemployment is the unemployment rate at the municipality level. Year and municipality fixed effects are added in every specification. The weighted models are weighted with population size. For seventeen observations the long-term unemployment rate is missing. Robust standard errors in parentheses.

Table 2. Estimating the effect of unemployment and long-term unemployment on property and violent crimes when including municipality specific (linear and quadratic) time trends.

	Weighted				Non-weighted			
	Property	*Violent*	*Property*	*Violent*	*Property*	*Violent*	*Property*	*Violent*
Unemployment	0.078	1.621			0.077	1.829		
	(0.503)	(0.607)***			(0.572)	(0.899)**		
Long-term unemployment			1.341	2.376			1.525	4.132
			(0.856)	(1.112)**			(0.975)	(1.508)***
Observations	3,744	3,744	3,727	3,727	3,744	3,744	3,727	3,727
R-squared	0.96	0.94	0.96	0.94	0.90	0.84	0.90	0.87

Notes: The dependent variables are the logarithmic numbers of crime per 100.000 inhabitants. Unemployment is the unemployment rate at the municipality level. Municipality specific (linear and quadratic) time trends, and year and municipality fixed effects are added in every specification. The weighted models are weighted with population size. For seventeen observations the long-term unemployment rate is missing. Robust standard errors in parentheses.

Table 3. Estimating the effect of unemployment on the property and violent crimes when also considering the share of social allowance recipients.

	Weighted (without time trends)				Weighted (with time trends)			
	Property	*Violent*	*Property*	*Violent*	*Property*	*Violent*	*Property*	*Violent*
Unemployment	0.562	0.017			-0.078	1.256		
	(0.366)	(0.410)			(0.517)	(0.619)**		
Long-term unemployment			1.829	1.001			1.304	2.075
			(0.676)***	(0.748)			(0.856)	(1.119)*
Social allowance recipients	1.729	2.521	1.721	2.481	0.654	2.102	0.532	2.371
	(0.424)***	(0.432)***	(0.417)***	(0.428)***	(0.675)	(0.759)***	(0.664)	(0.741)***
Observations	3,734	3,734	3,717	3,717	3,734	3,734	3,717	3,717
R-squared	0.92	0.91	0.92	0.91	0.96	0.94	0.96	0.94

Notes: The dependent variables are the logarithmic numbers of crime per 100,000 inhabitants. Unemployment is the unemployment rate at the municipality level. Year and municipality fixed effects are added in every specification. The weighted models are weighted with population size. For seventeen observations the long-term unemployment rates are missing and for ten observations the shares of social allowance recipients are missing. Robust standard errors in parentheses.

Table 4. Estimating the effect of unemployment on the property and violent crimes with quadratic effects.

	Weighted (without time trends)				Weighted (with time trends)			
	Property	*Violent*	*Property*	*Violent*	*Property*	*Violent*	*Property*	*Violent*
Unemployment	2.011	1.534			0.034	3.170		
	(0.853)**	(1.108)			(1.057)	(1.565)**		
Unemployment^2	-6.716	-5.865			0.290	-10.068		
	(4.760)	(5.809)			(6.182)	(8.783)		
Long-term unemployment			2.843	5.813			3.299	8.880
			(1.362)**	(2.202)***			(1.776)*	(3.350)***
Long-term unemployment^2			-15.019	-90.578			-48.837	-162.192
			(26.277)	(41.914)**			(40.062)	(82.391)**
Observations	3,744	3,744	3,727	3,727	3,744	3,744	3,727	3,727
R-squared	0.92	0.90	0.92	0.91	0.96	0.94	0.96	0.94

Notes: The dependent variables are the logarithmic numbers of crime per 100,000 inhabitants. Unemployment is the unemployment rate at the municipality level. Year and municipality fixed effects are added in every specification. The weighted models are weighted with population size. For seventeen observations the long-term unemployment rates are missing. Robust standard errors in parentheses.

Table 5. Estimating the effect of lagged and future unemployment on crime.

	Weighted (without timetrends)							
	Property	*Property*	*Violent*	*Violent*	*Property*	*Property*	*Violent*	*Violent*
Unemployment$_{t-1}$	0.538		0.196					
	(0.369)		(0.414)					
Unemployment$_{t+1}$		1.070		0.126				
		(0.372)***		(0.380)				
Long-term unemployment$_{t-1}$					1.736		0.535	
					(0.700)**		(0.801)	
Long-term unemployment$_{t+1}$						1.461		1.030
						(0.702)**		(0.822)
	3,744	3,744	3,744	3,744	3,439	3,439	3,439	3,439
	0.92	0.92	0.90	0.90	0.92	0.92	0.91	0.91
	Weighted (with timetrends)							
	Property	*Property*	*Violent*	*Violent*	*Property*	*Property*	*Violent*	*Violent*
Unemployment$_{t-1}$	-1.910		-0.773					
	(0.589)***		(0.739)					
Unemployment$_{t+1}$		1.600		0.422				
		(0.518)***		(0.599)				
Long-term unemployment$_{t-1}$					-1.596		-0.811	
					(0.912)*		(1.097)	
Long-term unemployment$_{t+1}$						2.563		0.637
						(0.908)***		(1.371)
	3,744	3,744	3,744	3,744	3,439	3,439	3,439	3,439
	0.96	0.96	0.94	0.94	0.96	0.96	0.94	0.94

Notes: The dependent variables are the logarithmic numbers of crime per 100,000 inhabitants. Unemployment is the unemployment rate at the municipality level. Year and municipality fixed effects are added in every specification. The models are weighted with population size. For 305 observations the long-term unemployment rates are missing. Robust standard errors in parentheses.

Table 6. Estimating the unemployment effect when using corporate bankruptcies as an instrument.

	Bankruptcies in firms with employees				Bankruptcies in firms without employees			
	Property	*Violent*	*Property*	*Violent*	*Property*	*Violent*	*Property*	*Violent*
Unemployment	-4.510	9.370			18.901	2.179		
	(3.672)	(4.251)**			(7.625)**	(6.049)		
Long-term unemployment			-14.021	24.780			19.838	1.733
			(11.395)	(12.930)*			(7.085)***	(7.465)
Observations	3,744	3,744	3,727	3,727	3,744	3,744	3,727	3,727
R-squared	0.91	0.89	0.90	0.88	0.84	0.90	0.90	0.91
F-statistics for weak IV test	12.03	12.03	4.71	4.71	5.10	5.10	10.77	10.77

Notes: The dependent variables are the logarithmic numbers of crime per 100,000 inhabitants. Unemployment is the unemployment rate at the municipality level. Year and municipality fixed effects are added in every specification. The models are weighted with population size. For seventeen observations the long-term unemployment rates are missing. Robust standard errors in parentheses.

Table A1. Descriptive Statistics

	Mean	*Standard error*	*Source*
Ln first-diff. in income	4.12	0.04	Statistics Sweden
Ln income	5.24	0.00	Statistics Sweden
Share: 0-15 (percent)	17.68	0.03	Statistics Sweden
Share: 16-20 (percent)	6.26	0.01	Statistics Sweden
Share: 21-25 (percent)	5.98	0.02	Statistics Sweden
Share: 26-35 (percent)	13.16	0.05	Statistics Sweden
Share: 35-45 (percent)	13.85	0.02	Statistics Sweden
Share of Men (percent)	49.56	0.01	Statistics Sweden
Share with for. background (percent)	15.77	0.14	Statistics Sweden
Share with higher educ. (percent)	10.19	0.08	Statistics Sweden
Population Density	604.98	19.40	Statistics Sweden
Ln population size	11.02	0.02	Statistics Sweden
Proportion divorced (percent)	20.84	0.07	Statistics Sweden
In- and outflow (per 1,000 population)	101.20	0.45	Statistics Sweden
Election participation (percent)	81.70	0.05	Statistics Sweden
Alcohol permits (per 1,000 population)	1.34	0.01	The Swedish National Institute of Public Health
Conviction rate (percent)	9.62	0.02	The National Council for Crime Prevention
Police Force (per 1,000 population)	2.46	0.01	The Swedish Police
Social allowance recipients (percent)	5.50	0.04	Statistics Sweden
Bankruptcies in firms with employees (per 1,000 population)	0.34	0.00	Statistics Sweden
Bankruptcies in firms without employees (per 1,000 population)	0.53	0.01	Statistics Sweden