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YOUTH UNEMPLOYMENT AND CRIME:
NEW LESSONS EXPLORING LONGITUDINAL REGISTER DATA

by

Hans Grönqvist

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Hans Grönqvist

SOFI, Stockholm University

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ABSTRACT

This paper investigates the link between youth unemployment and crime using a unique combination of labor market and conviction data spanning the entire Swedish working-age population over an extended period. The empirical analysis reveals large and statistically significant effects of unemployment on several types of crime. The magnitude of the effect is similar across different subgroups of the population. In contrast to most previous studies, the results suggest that joblessness explain a meaningful portion of why male youths are overrepresented among criminal offenders. I discuss reasons for the discrepancy in the results and show that that the use of aggregated measures of labor market opportunities in past studies is likely to capture offsetting general equilibrium effects. Contrary to predictions by economic theory the effect of unemployment on crime is not mediated by income. Instead, an analysis of crimes committed during weekdays versus weekends provides suggestive evidence that unemployment increases the time that individuals have to engage in crime.

Keywords: Unemployment; Delinquency; Age-crime profile
JEL: K42; J62

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1. INTRODUCTION

The age pattern of crime is close to universal. In virtually all countries, criminal activity rises with age, peak in the late teens, then fall (e.g. Hirschi and Gottfredson 1983). For example, while the conviction rate among Swedish men aged 19 to 24 in the year 2005 amounted to 4.2 percent the corresponding figure for men aged 29 to 34 was nearly half as large. A popular explanation for the age distribution of crime is that youths are more exposed to unemployment (e.g. Freeman 1996; Grogger 1998). Economists have argued that the income loss generated by unemployment lowers the opportunity cost of engaging in crime (cf. Becker 1968; Ehrlich 1973). Others have hypothesized that joblessness triggers frustration and anger, which in turn may lead to violent behavior (e.g. Agnew 1992). It has also been suggested that unemployment provides individuals with more time and opportunities to commit crime (Felson 1998). Understanding the link between youth unemployment and crime is not only important to help explain the age distribution of crime but is also a key issue for public policy since any relationship would indicate that the social benefits of investments in labor market programs may exceed those usually claimed.

The aim of this paper is to empirically investigate the effect of youth unemployment on crime. Although a large body of research has documented the relationship between overall labor market opportunities and crime there are surprisingly few studies on youths.[1] Freeman (1999) reviews the literature and concludes that there is a sizable negative correlation. There

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[1] Past investigations of the relationship between overall labor market opportunities and crime include: Entorf and Spengler (2000), Gould et al. (2002), Machin and Megihr (2004), Lin (2008), Papps and Winkelmann (2002), Raphael and Winter-Ebmer (2001), Doyle et al. (1999), Cornwell and Trumbull (1994). All of these studies use regional level panel data. In general, the results in these studies suggest that a one-percentage-point increase in the unemployment rate increases property crime by about 1–2 percent. The effect on violent crime is much smaller. Edmark (2005) find similar results using Swedish data. One of the few studies employing individual level data is Rege et al. (2009). The paper shows convincing evidence that involuntary job loss in Norway induced by plant closures is associated with a 14 percent higher probability of being charged of a crime. Unfortunately, since the research design requires the subjects to have stable employment histories it is less suitable for studying youths who often have little or no previous job experience.

are however several reasons to be concerned about the results in past studies. First, the observed relationship between unemployment and crime is (at least partially) likely to be spuriously driven by factors researchers have been unable to control for. Impatience represents one such factor if individuals with high discount rates are more prone to criminal activity and more exposed to unemployment (cf. DellaVigna and Paserman 2005; Grönqvist and Lindahl 2011). Joblessness could also be linked to consumption of various criminogenic goods; e.g. alcohol and narcotics.[2] Second, the literature has not been able to establish direction of causality. One problem is that potential employers may react adversely towards individuals with a criminal record (Grogger 1995; Kling 2006). A related issue is that firms might choose to relocate as a consequence of rising crime rates (Cullen and Levitt 2005). If crime raises the risk of unemployment instead of the opposite this will bias the results in past studies upwards. Third, due to data limitations most investigations have relied on aggregated measures of unemployment and crime. In these models any effect of unemployment on criminal behavior is likely to be confounded by general equilibrium effects. For instance, in areas with high unemployment rates there may be fewer resources available to steal and fewer potential victims on the streets (Mustard 2010). Although some studies have tried to account for these problems, no single study has been able to properly address all issues.

The most convincing research to date use regional level panel data to control for persistent unobserved regional factors. Fougère, et al. (2009) study a panel of French regions and find a statistically and economically significant effect of youth unemployment on property crime. Consistent with economic theory there is little or no significant effect on violent crimes. Öster and Agell (2007) examine a panel of Swedish municipalities observed during the 1990s and find no significant effect of youth unemployment on crime. Only a few studies use individual level data. In general, these studies find a stronger relationship

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[2] See e.g. Grönqvist and Niknami (2011) for evidence on the link between alcohol and crime.

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(Freeman 1999). Grogger (1998) and Witte and Tauchen (1994) show that employment and wages are significantly negatively correlated with self-reported illegal behavior in the US. A key limitation in these studies is however that the data used does not allow the researchers to account for unobserved characteristics of the individuals.

In contrast to past studies, this paper uses individual level data from administrative registers. The dataset covers the entire Swedish working-age population and includes rich information on labor market, educational and demographic characteristics of the individuals. The data have been merged to information on the universe of convictions in Swedish courts from 1985 to 2007. Among other things, there is information on the type of crime as well as the exact date of the offense. The longitudinal nature of the data makes it possible to disentangle the effect of unemployment from many aspects of the individual using several identification strategies. My main approach is to relate changes in unemployment to changes in criminal behavior. The advantage of the strategy is that it eliminates all fixed unobserved characteristics of the individual. One drawback is that the model excludes the possibility of controlling for the influence of past criminal behavior (at least without imposing strong assumptions). I therefore supplement the analysis with regressions that relate levels of crime to levels of unemployment in which I condition on lagged crime. The benefit of both models is that they are likely to come closer to having a causal interpretation of the estimates than what has been feasible in the previous literature. Comparing the results from two models that rests on different assumptions also provides a way to corroborate the findings.

This paper contributes to the literature by using data not previously available to researchers.[3] Coupled with an identification strategy that rules out many potential confounding factors, I am able to explore several new aspects of the question. First, no

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[3] Only a few studies have ever used population conviction data to study crime. One exception is Hjalmarsson and Lindquist (2010) who use similar Swedish crime data to investigate the intergenerational association in crime.

previous study using individual level data has explicitly tried to account for unobserved characteristics of the individual. Second, the data makes it possible to investigate subgroups of the population with elevated risks of criminal involvement. The data identify individuals with a criminal history, low cognitive skills, immigrant status, and poor parental socioeconomic background. It is well known that these groups account for a disproportionate share of total crimes committed (e.g. Levitt and Lochner 2001; Hällsten, Sarnecki and Szulkin 2011). Learning about the extent to which worse labor market prospects help explain criminal behavior in these groups could provide valuable knowledge on how to optimally target labor market and educational programs. No past investigation has ever separately studied population subgroups using a compelling identification strategy. Third, the data allows me to disentangle some of the mechanisms through which youth unemployment may affect crime. By using information on disposable income it is possible to investigate whether the potential effect is mediated via income, as suggested by economic theory. Moreover, if employment matters because it incapacitates individuals so that they have less time to commit crime then the effect of joblessness should be more pronounced for crimes committed during weekdays when most individuals normally work. I examine this by exploiting information on the exact date of the offense.[4] Last, the use of individual data makes it possible to isolate the effect of unemployment on the supply of criminals. This is important since it provides a cleaner test of the economic theory of crime than what can be achieved using aggregated data.

Unemployment is found to have a small and statistically significant effect on violent crime and a sizable effect on financially motivated crimes: being unemployed for more than six months increases the likelihood of committing a violent crime by about 2 percent and raises the probability of committing theft by about 33 percent. I also find large and significant effects on drug offenses as well as drunken driving (DUI).The impact of joblessness on

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[4] This strategy was first adopted by Rege et al. (2009).

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overall crime constitutes almost one quarter of the crime gap between 19 to 24 year olds and individuals aged 29 to 34. This result suggests that unemployment may explain a meaningful portion of why male youths account for a disproportionate share of total crime. There is also a clear dose-response relationship in the sense that the risk of crime is increasing in the time spent in unemployment. The estimates are similar across different subgroups of the population. In contrast to the predictions by economic theory the effect is not mediated via income. Instead, an analysis of crimes committed during weekdays versus weekends provides tentative evidence that unemployment may increase the time and opportunities that individuals have to engage in crime. The analysis further shows little evidence that the adverse effects of unemployment persist over time.

My findings partly contradict those in the previous literature using aggregated data that in general finds smaller effects. One possible explanation is that the use of aggregated data has prevented past investigations to identify pure behavioral responses to unemployment. This view is supported by an auxiliary analysis where I collapse the individual level data by county and year.

The paper is outlined as follows. Section 2 provides a brief background to the Swedish criminal justice system and gives some facts about youth crime in Sweden. Section 3 discusses the data and research design. Section 4 presents results from the analysis and Section 5 concludes.

2. JUVENILE CRIME AND THE SWEDISH JUSTICE SYSTEM [5]

The Swedish crime rate is high compared to many other countries. In the year 2006 the total number of assaults per 100,000 inhabitants reported to the police was 845. The same year official crime statistics show that 787 assaults per 100,000 inhabitants were recorded by the US police and the corresponding number for Canada was 738 (Harrendorf et al. 2010). Even

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[5] This section primarily draws on Axelsson (2010).

though these figures partly reflect differences in the propensity to report crime the rates are comparable across many types of crimes where underreporting is likely to be similar. For instance, in 2006 the burglary rate per 100,000 persons in Sweden was 1,094. In the US the burglary rate was 714 and in Canada the rate amounted to 680 recorded cases per 100,000 inhabitants.

As previously mentioned, youths represents the most criminally active age group in Sweden. Figure 1 shows the share of convicted persons in 2005 by age relative to national conviction rates. A number above (below) one indicates that the share of convicted persons for that age group is higher (lower) than the average for all age groups. As can be seen the relative overall conviction rate peaks already before age 20, then falls. Note that the share of convicted persons has dropped to the average for all ages as early as age 29. Figure 1 also shows that the age distribution of theft is even more heavily skewed to the right. However, there is no clear age-crime profile for violent crimes. It is worth mentioning that these findings hold irrespective of the type of crime data used (cf. Swedish National Council for Crime Prevention 2007).

In Sweden, the general courts deal with both criminal and civil cases. The general courts are organised in a three-tier system: district courts, courts of appeal and the Supreme Court. The district court is the court of first instance. Criminal cases are normally instituted when a public prosecutor initiates prosecution proceedings against a suspect by submitting an application to a district court. The court rules on cases after a main hearing attended by both parties, who state their claims and other circumstances relating to the case. Criminal cases are normally tried by one judge and three lay judges. Those who lack the economic means to take advantage of their rights are entitled to public legal aid.

The age of criminal liability is 15. All individuals above this age are treated in the same juridical system. Some special rules does however apply for juveniles. For instance, cases involving youths are to be dealt with promptly.

3. DATA AND EMPIRICAL STRATEGY

3.1 Data and sample selections

This study uses data from various administrative registers collected and maintained by Statistics Sweden. The data span the entire Swedish population aged 16 and above each year 1985 to 2007 and include information on a wide range of labor market, educational and demographic characteristics. The dataset was augmented with information on all convictions in Swedish district courts during the period. Among other things there is information on the type of offense and the sentence ruled by the court. Date of offense is known in about 70 percent of the cases. There is however some measurement error in this variable since the exact date of the crime is not always known (e.g. a burglary that is not discovered before the owner returns home from her holidays). In these cases the court makes an educated guess about the crime date. One conviction may include several crimes and I observe all crimes within a single conviction. Speeding tickets, and other minor offenses are not included in the data.

The crime categories of interest in this paper are: (i) Any crime; (ii) Violent crimes; (iii) Theft; (iv) Drugs; (v) Drunken driving (DUI). All of these categories represent common types of crimes in Sweden. Table A.1 describes the exact way in which these variables have been constructed.

My main sample includes male youths aged 19 to 25 with at least one recorded unemployment spell.[6] The period of observation is from 1992 to 2005. The sample contains 723,392 individuals. The reason for not including younger individuals is that most youths

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[6] To be registered as unemployed an individual needs to report to the state employment office. Unemployment benefits are contingent on having registered.

below this age are enrolled in upper secondary school.[7] Information on employment status is further only available from 1992. To allow for a lag between the date of the crime and the conviction I choose to end the observation period in 2005.

In the analysis I separate between how many days an individual has been unemployed during the year. More specifically, I create dummies for if the individual has been unemployed 1-90 days, 91-180 days, and more than 180 days. It is plausible to think that the risk of crime should increase with the time spent in unemployment. Long-term unemployed will have had more time to engage in crime and have experienced greater reductions in legal income compared to short-term unemployed. No previous study has quantified the relationship between unemployment duration and crime.

I include a set of standard individual background characteristics in the analysis: high school graduation, family size, marital status, age, and immigrant status. Information on disposable income and annual earnings is used to test the prediction by economic theory that the main reason for why unemployment matters is because it generates a loss of income (e.g. Freeman 1999). The data further contain an exact link between children and their biological parents. It is therefore possible to add information each parent's highest completed level of education. Parental education is observed the year the child turns 16. At this age most parents have completed their education. In the data there is also information on compulsory school grades for children who finished school during the period 1988 to 2007. I include the grade point average (GPA) in the analysis as a combined measure of cognitive skills and ambition. To account for changes in the grading system over time as well as potential grade inflation I compute the percentile ranked GPA by year of graduation.

Table 1 presents descriptive statistics for selected variables. To see how well the results are likely to generalize to the whole population of male youths, summary statistics is shown

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[7] In Sweden, more than 95 percent of all individuals continue immediately to upper secondary school. Most of the students who graduate do so at age 19.

both for my main sample and for all males aged 19 to 25. Comparing the numbers in column (1) to those in column (2) it is clear that my sample is slightly disadvantaged compared to the entire population of male youths: the share convicted persons is higher (5.2 versus 4.2 percent), percentile ranked compulsory school GPA lower (39 versus 43), and parental education poorer; although the latter difference is small. Not surprisingly, the average number of days spent in unemployment is higher and mean disposable income lower.

Data on criminal behavior is in this paper inferred from register information on convictions. The main advantage of administrative data compared to self-report data is that the latter is known to be plagued by underreporting and measurement error (McDonald 2002). The large samples available in administrative registers also increase statistical precision. However, conviction data are not flawless. Criminal behavior is only observed for individuals who have been convicted in court. One concern is that people with worse labor market opportunities may be more likely to get convicted conditional on actually having committed a crime. This is the case if, for instance, jobless individuals are more likely to get caught or have fewer resources available for defense at a criminal trial. This is a caveat important to bear in mind when interpreting the results. Note however that this is only a problem if this kind of selection is not picked up by the control variables.[8] Recall that my empirical strategy accounts for all permanent unobserved heterogeneity in addition to time-varying factors. Moreover, the analysis focuses on a sample of individuals where everyone becomes unemployed at least once. This means that the sample is homogenous in the sense that the only difference between individuals is the timing of the onset of the unemployment spell. If the criminal justice system treats individuals with elevated unemployment risks similar then the problem is less severe in this population.

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[8] In their study of the effect of education on crime as measured by arrests Lochner and Moretti (2005) raises a similar concern. Using data on self-reported crime they conclude that for this to be a problem education must substantially alter the probability of being arrested conditional on criminal behaviour.

3.2 Research design

As discussed earlier, any investigation of the link between unemployment and crime needs to consider potential omitted variables and reverse causation. Studies using aggregated data are plagued by additional problems associated with general equilibrium effects. Although individual level data account for the last problem it is important to also deal with the first issues.

The longitudinal nature of the data allows me to take one important step in the direction towards identifying the causal effect of joblessness on crime. My main analysis is based on the following regression model

$$(1) \quad \text{Crime}_{it} = \alpha + \beta_1 \text{Un}_{it}^{1-90} + \beta_2 \text{Un}_{it}^{91-180} + \beta_3 \text{Un}_{it}^{>180} + X_{it}'\gamma + \delta_i + \theta_t + \varepsilon_{it}$$

where *i* and *t* denotes individual and year, respectively. Crime_{it} is an indicator set to unity if the individual has committed crime during the year and zero otherwise. Unemployment is measured with three dummies for if the individual has been unemployed for a given number of days during the year. X_{it} is a vector controlling for time-varying variables; δ_i is a set of individual fixed effects; θ_t is a set of year fixed effects; and ε_{it} is an error term. The main parameters of interest in this model are β_1, β_2, β_3. Each parameter provides the effect on crime of being unemployed for *d* number of days relative to individuals that have not yet started (or just ended) their unemployment spell. Since it is reasonable to assume that the probability of committing crime is increasing in the time spent in unemployment I expect that $\beta_1 < \beta_2 < \beta_3$.

The individual fixed effects absorb all individual characteristics that are persistent over time regardless whether these are observed or not. Some persons may, for instance, be less patient by nature, which could make them less likely to invest in work and at

the same time more likely to engage in crime (e.g. Lochner and Moretti 2005). Another

potential omitted factor is ability (e.g. Levitt and Lochner 2001). The year fixed effects

controls for time-varying factors that affect all individuals in the same way. This could be cuts

in governmental spending or macroeconomic events.

The model assumes that the error term is mean zero conditional on past, current

and future values of the regressors, i.e. that $E(\varepsilon_{it}|\text{Un}_{it}, \text{X}_{it}, \delta_i, \lambda_t) = 0$ for all t. This

assumption is violated if unobserved individual shocks correlates with an individual's

employment status and risk of crime. Controlling for individual characteristics that change

over time alleviates this problem but does not remove it. The assumption is also corrupted if

past factors linked to crime affect unemployment today and are not captured by the control

variables. One issue that in has been raised in the literature is the possibility that employers

discriminate between individuals with a criminal record (e.g. Grogger 1995, Kling 2006).

Fortunately, I am able to address this matter by estimating regressions that controls for lagged

crime. The model is specified as follows

$$(2)\ \text{Crime}_{it} = \mu + \theta_1 \text{Un}_{it}^{1-90} + \theta_2 \text{Un}_{it}^{91-180} + \theta_3 \text{Un}_{it}^{>180} + \omega \text{Crime}_{i,t-1} + \text{X}'_{it}\rho + \tau_t + v_{it}$$

Under the assumption that controlling for past criminal behaviour (and other covariates)

captures all unobserved factors that are correlated with unemployment and crime, i.e.

$E\left(v_{it}|\text{Un}_{it}, \text{Crime}_{i,t-1}, \text{X}_{it}, \tau_t\right) = E\left(v_{it}|\text{Crime}_{i,t-1}, \text{X}_{it}, \tau_t\right)$, the ordinary least squares

(OLS) estimator will consistently estimate $\theta_1, \theta_2, \theta_3$. Also this supposition could be

questioned. Nevertheless, the benefit of the model is that it provides a way to corroborate the

findings.[9] Although no single model will identify the causal impact of youth unemployment

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[9] It is also possible to estimate fixed effects models that include lagged outcomes. However,
in this model the residual will be mechanically correlated with the lagged dependent variable
causing the OLS estimator to be biased (Nickell 1981). Although there are methods to

on crime both models are likely to produce estimates that come closer to having a causal interpretation than what has been possible in the previous literature.

4. RESULTS

4.1 Main results

This section presents the results from my empirical analysis of the effect of youth unemployment on crime. My baseline specification, given by equation (1), relates changes in an individual's probability of committing a given type of crime during the year (for which he later was convicted for) to changes in the number of days spent in unemployment. I also present results from regressions controlling for the lag of the dependent variable. To conserve space I suppress the coefficients of the control variables (available on request). In general, these show a significant increased risk of crime for individuals that have not completed high school, immigrants, individuals with lower compulsory school GPA, past offenders, and individuals with worse parental socioeconomic background. The standard errors are clustered at the individual level to account for arbitrary serial correlation and heteroscedasticity.

Before proceeding to the main results it is useful to illustrate the raw correlation between unemployment and crime. For this purpose Table 2 presents pooled OLS estimates. Panel A contains estimates from regressions that only control for year and age. Panel B adds further covariates: dummies for high school graduation, county of residence, foreign-born, crime in the past two years, married, divorced, each parent's highest completed level of education, number of children (linearly), and compulsory school GPA (linearly).

In column (1) of Panel A it is clear that there is a sizable correlation between unemployment and criminal behavior. Being unemployed between 1 and 90 days is associated with a 1.93 percentage point increased risk of committing crime. In relation to the mean of the

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estimate such models (e.g. Arellano and Bond 1991) they rely on rather strong assumptions of the data generating process.

dependent variable this translates into a 37 percent (.0193/.0527) higher probability of engaging in crime. The magnitude of the effect is increasing in the number of days spent in unemployment. Individuals who have been unemployed for more than 180 day are 6.19 percentage points more likely to commit crime. Due to the large sample size the statistical precision is high and all estimates are significant well below the one percent level. The same is true for the F-test of joint significance of the unemployment coefficients. A similar pattern is visible in columns (2) to (5). For all types of crime there is a strong and statistically significant relationship between joblessness and the likelihood of committing crime. There is also a clear dose-response relationship.

As can be seen in Panel B, adding a rich set of covariates to the regressions renders the size of most of the coefficients to cut in half. The finding highlights the importance of accounting for omitted individual characteristics. In all cases except for drug offenses the dose-response relationship prevails.

Even though the regressions control for a large number of covariates it is still likely that unobserved individual characteristics biases the OLS estimator. To the extent that these characteristics are constant over time the individual fixed effects estimator will control for this. Table 3 presents the regression output based on equation (1). Comparing these estimates with the pooled OLS estimates in Panel B of Table 2 we can see that the coefficients are further reduced. In general, the estimates in Table 3 are about one third as large. Being unemployed between 1 and 90 days is found to increase the probability of committing any type of crime by about .03 percentage point. The results also show that being unemployed for more than 180 days quadruples this risk. Relative to the sample mean long-term unemployed individuals (>180 days) are about 22 percent (.0120/.0527) more likely to engage in crime. In column (2) we can see that the magnitude of the effect is smaller for violent crimes. Long-term unemployment is found to raise the risk of violent crimes by .05 percentage points, or

stated differently, by just below 2 percent (.0005/.0260). As can be seen in column (3), the effect is substantially stronger for theft. Long-term unemployment increases the likelihood of committing theft by .46 percentage points, or by 33 percent (.0046/.0138).

The difference between the estimates in columns (2) and (3) is striking and the result is consistent with economic theory which suggests that the effect should be stronger for financially motivated crimes. Columns (4) and (5) also show large effects of unemployment on drug offenses and DUI. However, since the means of the dependent variables are quite low the results should be interpreted with caution. For sake of comparison Panel B also displays estimates from regressions using a continuous measure of days in unemployment. Evaluated at 100 days of unemployment the results reveal a .05 percentage point higher probability of committing crime.

To better understand the magnitude of the coefficients it is useful to compare the estimates to other known determinants of criminal behavior. Ability represents one such factor (e.g. Freeman 1996; Levitt and Lochner 2001). In my sample, the crime gap between individuals who scored above versus below the median in the compulsory school GPA distribution is 5.63 percentage points (.0767-.0204) (cf. Table 5). The effect of long-term unemployment on crime constitutes roughly 20 percent of this gap (.0120/.0563). Repeating this exercise for violent crimes and theft I find that the effect of long-term unemployment constitutes about 1.6 (.0005/.0563) and 27 (.0046/.0563) percent of the crime gap between these groups, respectively.

Another way to grasp the size of the effect is to relate it to the age-crime profile. In 2005 the conviction rate for male youths aged 19 to 24 was 4.2 percent. The corresponding number for males aged 29 to 34 was 2.2 percent. The same year the share of individuals aged 19 to 24 who experienced at least 180 jobless days was 9.3 percent and analogous figure for 29 to 34 year olds was 4.1 percent. The results in this paper suggests that long-term

unemployment account for almost one quarter (.0120/(.093-.041)) of the crime gap between these groups. This is by all measures a substantial effect.

One concern with the individual fixed effects model is that it does not account for the potential influence of past criminal behavior on current employment status. To investigate this I estimated regressions controlling for lagged crime; cf. equation (2). The results are presented in Table 4. The size of the coefficients is higher for all types of crimes compared to the estimates in Table 3. For instance, long-term unemployment is found to raise the probability of committing any type of crime by 2.89 percentage points. When placed in relation to the sample mean the estimate suggest a 55 percent (.0289/.0527) increase in the risk of crime. Aside from being larger the overall pattern of the estimates is similar to that in Table 3: clear indications of a dose-response relationship; and substantially smaller effects for violent crime. It is also worth mentioning the huge predictive power of lagged crime, especially for violent crimes.

4.2 Is the effect different for high risk individuals?

Having demonstrated that the effect of joblessness triumph different identification strategies I next ask whether the relationship is stronger in subgroups of the population at higher risk of criminal involvement. I examine this by estimating separate regressions for each subgroup. The results are shown in Table 5. To conserve space I only present estimates for the linear measure of unemployment duration; however, the results are similar when relaxing functional form by using dummies. All reported coefficients and standard errors are scaled up by a factor 100. Numbers in italics show sample means. For comparison purposes, the first row shows the baseline estimates presented in Table 3.

I start by examining individuals that differ in terms of criminal history. Criminal background is defined as having committed any type of crime in the past two years.

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Comparing the estimates for past offenders to those of persons with no criminal background we can see that the estimates are larger in the former group. Evaluated at 100 days of unemployment, the coefficient suggests a 2.14 percentage point increase in the probability of committing any type of crime for past offenders and a .45 percentage point increase for subjects with no criminal background. However, when compared to the mean of the dependent variable the magnitude of the effect is quite similar: a 9 and a 12.2 percent increase in the risk of crime, respectively. The pattern holds also for the other types of crime.

I proceed by comparing the effect size between foreign-born individuals and native Swedes. It is well-known that immigrants are overrepresented among criminal offenders (Hällsten, Sarnecki and Szulkin 2011). This is confirmed by looking at the sample means. The results reveal that the magnitude of the unemployment effect is bigger in absolute terms for foreign-born compared to natives. 100 days of unemployment is found to raise the risk of crime among immigrants by about .94 percentage points. For natives this results show a .45 percentage point increase; although, in relative terms the results are again similar across groups.

Table 5 also hosts results from separate analyses by parental education. Low educated parents is defined as no parent having attained education beyond the compulsory school level. High educated parents is defined as at least one parent having graduated from high school or more. For most types of crime there is a positive and statistically significant effect of time spent in unemployment on the probability of engaging in crime. In nearly all cases the estimates are however not statististically distinguishable across the groups.

Last, I investigate possible heterogenous effects with respect to compulsory school GPA. It turns out that the magnitude of the impact is greater for individuals with compulsory school GPA below the median. Still, the relative effect sizes are quite similar across the groups.

In summary, the results in this subsection suggest little evidence that the effect of joblessness is stronger in particular subgroups of the population.

4.3 Extensions

So far, the results presented in this paper suggest that unemployment is adversely related to crime. This subsection attempts to disentangle some of the mechanisms that have been proposed in the literature as explanations of the unemployment-crime link. I also provide evidence on the potential dynamic impact of joblessness on crime.

4.3.1 Mechanisms

Standard economic models of crime suggest that an individual chooses whether or not to engage in crime based on the expected returns to legal and illegal activities (e.g. Becker 1968, Ehrlich 1973). The model implies that joblessness may induce people to commit crime because unemployment lowers an individual's expected legal income prospects. The model also implicitly suggests that any effect on crime is likely to be more pronounced for crimes that are associated with financial gains. The results presented in this paper of much stronger effects of unemployment on theft compared to violent crimes indeed support the economic theory. This finding has also been documented in many previous studies using aggregated data.

Since my data include detailed information on income it is possible to probe deeper into this issue. I started with quantifying the effect of unemployment on disposable income by re-estimating equation (1) with the only exception that income was used as dependent variable (available on request). It turns out that long-term unemployment is associated with 17 percent lower disposable income and 73 percent lower annual earnings. The difference in the estimates is due to various social transfers following joblessness.

Having documented the "first-stage" relationship between joblessness and income I then estimated equation (1) controlling for disposable income. If the income loss is the main mechanism behind the unemployment-crime relationship one would expect to find the estimates in Table 3 to fall when including income as a regressor. Note however that the results from this exercise should be interpreted with caution since income cannot be considered as a predetermined variable. In Table 6 we can see that the coefficients are virtually unchanged when controlling for disposable income. This result suggests that the income mechanism might not be as important as emphasized in the literature.

It has also been suggested that employment mechanically incapacitates individuals thereby preventing them to commit crime (Felson 1998). If true, then unemployment will increase the time and opportunities that individuals have to engage in illegal behavior. Rege et al. (2009) tests this mechanism by separately investigating crimes committed during weekdays versus weekends. If there is an incapacitation effect of employment then one would imagine that the impact of unemployment on crime is stronger during weekdays when most individuals normally work. The results in Rege et al. show that unemployment caused by involuntary plant closures in Norway indeed leads to more crime during weekdays compared to weekends.

I investigated this mechanism by extracting information on the date of the offense which allowed me to identify whether the crime was committed on a weekday or weekend. Since the mean of the dependent variable already is low for some types of crime the analysis was only possible for any type of crime. The results are shown in Table 7. The point estimate in column (1) show that 100 days of unemployment leads to a .46 percentage point increase in the probability of committing crime on a weekday. The coefficient in column (2) suggests smaller effect for weekend crimes: being unemployed for 100 days is associated with a .17 percentage point increase in the likelihood of engaging in crime on the weekend. Also

when compared to the sample mean the effect is stronger for weekday crimes. The probability of committing weekday crimes increases by about 12.5 percent (.0046/.0388) whereas the corresponding number for weekend crimes is 8 percent (.0017/.0204). The results provide some support for the idea that unemployment increase the time and opportunities that individuals have to commit crime. Still, the difference in the estimates is not especially big.

4.3.2 Dynamics

It is possible that unemployment has persistent effects on illegal behavior. A large literature has claimed that unemployment has long-lasting effects on labor market performance, especially among youths (e.g. Ellwood 1982; Nordström Skans 2011). It has for instance been shown that joblessness depreciates human capital (e.g. Edin and Gustavsson 2008). If true, then past unemployment spells may affect an individual's current criminal behavior even conditional on current employment status.

To identify dynamic effects I included the number of days spent in unemployment in the previous year as a regressor. The results are shown in Table 8. We can see that the lagged number of days in unemployment in most cases enters insignificant. The exceptions are theft and drug offenses. However, the magnitude of the estimates is substantially smaller compared to the contemporary effect. These results suggest that joblessness has no meaningful dynamic effects on criminal behavior.

4.4 Reconciling the evidence with past studies

In contrast the previous literature this paper finds what seems to be quite large effects of unemployment on crime, especially for theft. Why are the results different? One likely candidate for explanation is that the use of aggregated data in past studies mixes effects of unemployment throughout the entire market for crime (e.g. Freeman 1999; Mustard

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2010). Unemployment may for instance reduce criminal opportunities by decreasing the resources available to steal. An increase the supply of criminals could also crowd out criminal opportunities. Another channel works through cross-regional spillovers. If unemployment in one region is associated with higher levels of unemployment in neighboring regions the effect of joblessness on crime will be attenuated. The fact that Öster and Agell (2007) find no significant effect of youth unemployment on crime in a panel of Swedish municipalities overlapping the period of analysis used in the present paper indicates the existence of such general equilibrium effects. Despite its potential importance no previous work has investigated the role of general equilibrium effects in this context. This is of course because large samples of individual level data are needed.

To shed some light on this issue I collapsed the data by county and year and estimated models where I regressed the (log) conviction rate on the average number of days of unemployment together with a set of county and year fixed effects. This specification is the standard model applied in many previous studies on the relationship between overall labor market opportunities and crime using regional level panel data (e.g. Edmark 2005; Raphael and Winter-Ebmer 2001; Gould et al. 2002). Since clustering at the county level risks understating the standard deviation of the estimator in regressions with few cross-sectional units (Bertrand, et al. 2004) panel corrected standard errors are calculated using a Prais-Winsten regression where a county specific AR(1) process is assumed.[10]

The results in Table 9 suggests that a one standard deviation increase in the average number of days in unemployment increases the share of convicted persons by about .3 percent (16.25*.0002). This effect is indeed smaller than the estimate in Table 3 which suggests that a one standard deviation increase in the number of days spent in

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[10] The results are similar when estimating the model by OLS and accounting for serial correlation by clustering at the county level.

unemployment increases the probability of committing crime by 7.1 percent

((74.18*.000051)/.0527). Admittedly, the statistical precision is poor. Still, the upper

limit of the 95 percent confidence interval (.0041) rules out large effects. The confidence

interval suggests that a one standard deviation increase in the average number of days in

unemployment increases the conviction rate by no more than 1.3 percent

((16.25*.000041)/.0527). Note also the negative sign for theft which is consistent with

the idea that high unemployment rates reduce criminal opportunities. Similar findings

have been reported for single estimates in other studies using aggregated data (e.g.

Raphael and Winter-Ebmer 2001; Gould et al. 2002; Öster and Agell 2007).

One objection towards interpreting the discrepancy in the results as

resulting from general equilibrium effects is that I am focusing on a sample of

individuals on the margin to commit crime; i.e. male youths. Targeting individuals at

higher risk of engaging in crime could make it easier to detect any effect of

unemployment on crime (cf. Mustard 2010). However, this argument is not consistent

with the results in Table 9 which are based on the same sample as in my main analysis.

CONCLUDING REMARKS

This paper concerns the effect of youth unemployment on crime. Using unique

individual labor market and conviction data, the empirical analysis reveals large and

statistically significant effects of unemployment on several types of crimes. The effect is

particular large for theft. The results indicate that youth unemployment is one important

determinant of the age distribution of crime. From a policy perspective, the results

suggest that the social benefits of investments in labor market programs may extend

beyond those usually claimed.

My results both support and contradict standard economic theory. On the one hand, finding larger effects for acquisitive crimes speaks in favor of the theory. On the other hand, I find that the impact of unemployment is not mediated via income. Instead, a separate analysis of crimes committed during weekdays versus weekends supports the idea that unemployment increases the time that individuals have to engage in crime.

The estimates are substantially larger than the ones typically found in the literature using aggregated data. My analysis shows that one reason could be that aggregated data entail offsetting general equilibrium effects which masks any effect of unemployment on the supply of crime.

The fact that unemployment is found to have large effects on crime in a country like Sweden with an extensive welfare state and a strong focus on active labor market policy suggests that the effect may be even stronger in other countries. Nevertheless, in order to identify such effects the results in this paper stresses the need for longitudinal individual data.

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Figure 1. Share of convicted persons for crimes committed in 2005 by age relative to national conviction rates
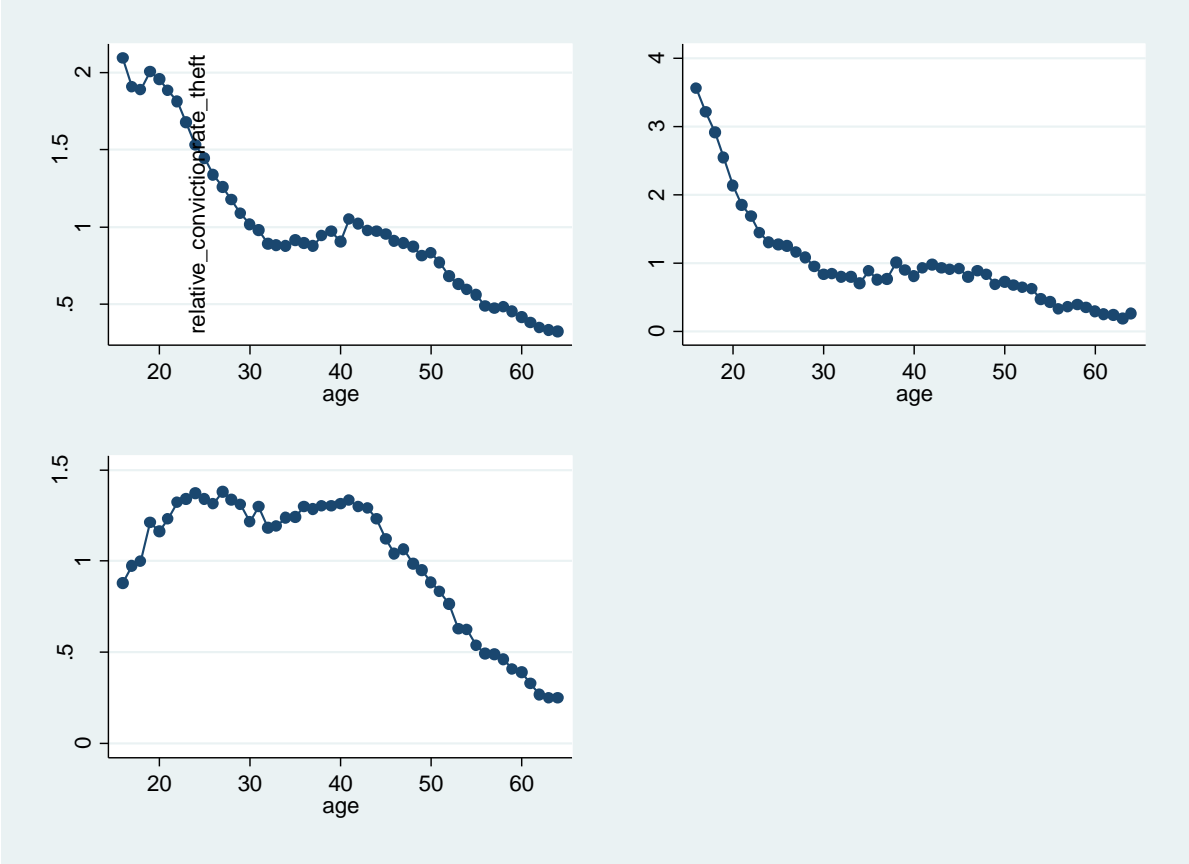
Notes: Top left figure plots the overall conviction rate. Top right figure plots the conviction rate for thefts. Bottom left figure plots the conviction rate for violent crimes. The sample includes all men aged 16 to 65.

Table A.1. Definitions of crime categories

Crime type	Explanation	Legal text
Any crime	Any recorded conviction regardless of the type of crime	
Violent crime	The full spectrum of assaults from pushing and shoving that result in no physical harm to murder.	BRB Chapter 3 paragraph 4; BRB Chapter 17 paragraphs 1 and 4
Theft	The full spectrum of thefts from shop-lifting to burglary. Robbery is also included.	BRB Chapter 8
Drug offenses	Dealing and possession of illicit drugs	SFS 1968:64
Drunken driving (DUI)	Driving vehicle under the influence of alcohol.	SFS 1951:649

Table 1 Mean (std.) of selected variables

	Sample of analysis N=723,392 (1)	Entire population N=5,888,123 (2)
Any crime (0/1)	.053	.042
	(.224)	(.203)
Days in unemployment	50.91	35.03
	(74.18)	(65.65)
Disposable income	86,706	90,497
	(72,370)	(90,446)
Percentile rank of compulsory school GPA	39.00	43.00
	(26.51)	(27.90)
Mother completed at least high school (0/1)	.691	.700
	(.462)	(.458)
Father completed at least high school (0/1)	.572	.583
	(.495)	(.583)

Notes: Summary statistics is conditional on no missing values. The sample in column (1) represents the main sample of analysis and consists of male youths aged 19 to 25 with at least one unemployment spell during the observation period 1992 to 2005. The sample in column (2) consists of all individuals in this age span and observation period regardless of unemployment experience.

Table 2 Pooled OLS estimates of the effect of unemployment on the probability of committing a given type of crime

	Any crime (1)	Violent crime (2)	Theft (3)	Drugs (4)	DUI (5)
A. No controls except year and age					
Unemployed 1-90 days	.0193** (.0003)	.0095** (.0002)	.0068** (.0002)	.0054** (.0001)	.0019** (.0001)
Unemployed 91-180 days	.0342** (.0004)	.0166** (.0003)	.0113** (.0002)	.0074** (.0002)	.0033** (.0001)
Unemployed >180 days	.0619** (.0006)	.0297** (.0005)	.0196** (.0003)	.0105** (.0002)	.0060** (.0002)
P-value joint F-statistic	.0000	.0000	.0000	.0000	.0000
B. Controlling for year, age, high school graduation, county of residence, immigrant status, number of children compulsory school GPA, married, divorced, and parental education					
Unemployed 1-90 days	.0118** (.0003)	.0049** (.0001)	.0043** (.0001)	.0036** (.0001)	.0011** (.0001)
Unemployed 91-190 days	.0186** (.0004)	.0071** (.0003)	.0061** (.0002)	.0036** (.0002)	.0018** (.0001)
Unemployed >180 days	.0328** (.0006)	.0120** (.0005)	.0095** (.0003)	.0033** (.0002)	.0032** (.0002)
P-value joint F-statistic	.0000	.0000	.0000	.0000	.0000
Mean of dependent variable	.0527	.0260	.0138	.0083	.0031

Notes: The table displays coefficients on dummies for if the individual experienced at least d number of days as unemployed during the year. The dependent variable is set to unity if the individual has committed a given type of crime during year and zero otherwise. Each column and panel represents a separate regression. The unit of observation is a person-by-year cell (3,816,376 observations). The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. All regressions control for possible missing values in the regressors. Robust standard errors in parentheses account for serial correlation and heteroscedasticity. */** denote significance at the 5/1 percent level.

Table 3 The effect of unemployment on the probability of committing a given type of crime

	Any crime (1)	Violent crime (2)	Theft (3)	Drugs (4)	DUI (5)
A. Dummies for number of days unemployed during the year (ref.= zero days)					
Unemployed 1-90 days	.0030**	.0001**	.0008**	.0014**	.0004**
	(.0003)	(.0001)	(.0001)	(.0001)	(.0001)
Unemployed 91-180 days	.0062**	.0007**	.0023**	.0022**	.0005**
	(.0003)	(.0001)	(.0002)	(.0002)	(.0001)
Unemployed >180 days	.0120**	.0005**	.0046**	.0031**	.0010**
	(.0006)	(.0002)	(.0003)	(.0002)	(.0002)
P-value joint F-statistic	.0000	.0001	.0000	.0000	.0000
B. Days of unemployment×100	.0051**	.0004**	.0020**	.0013**	.0004**
	(.0021)	(.0001)	(.0001)	(.0001)	(.0001)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.0527	.0260	.0138	.0083	.0031
Number of individuals	723,392	723,392	723,392	723,392	723,392

Notes: The dependent variable is set to unity if the individual has committed a given crime during the year and zero otherwise. Each column and panel represents a separate regression. All regressions control for age, high school completion, number of children, married and divorced. The unit of observation is a person-by-year cell (3,816,376 observations). The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. All regressions control for possible missing values in the regressors. Robust standard errors in parentheses account for arbitrary serial correlation and heteroscedasticity. */** denotes significance at the 5/1 percent level.

Table 4 The effect of unemployment on the probability of committing a given type of crime controlling for the lag of the dependent variable

	Any crime (1)	Violent crime (2)	Theft (3)	Drugs (4)	DUI (5)
A. Dummies for number of days unemployed during the year (ref.= zero days)					
Unemployed 1-90 days	.0107**	.0015**	.0040**	.0035**	.0010**
	(.0003)	(.0001)	(.0002)	(.0001)	(.0001)
Unemployed 91-180 days	.0163**	.0022**	.0054**	.0041**	.0015**
	(.0004)	(.0002)	(.0002)	(.0002)	(.0001)
Unemployed >180 days	.0289**	.0033**	.0090**	.0051**	.0027**
	(.0005)	(.0002)	(.0003)	(.0002)	(.0002)
Lag of dep. variable (t-1 year)	.2310**	.7821**	.2108**	.2892**	.0376**
	(.0014)	(.0020)	(.0024)	(.0034)	(.0023)
P-value joint F-statistic	<.0000	<.0000	<.0000	<.0000	<.0000
B. Days of unemployment×100	.0126**	.0016**	.0038**	.0022**	.0011**
	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.0527	.0260	.0138	.0083	.0031
Number of individuals	723,392	723,392	723,392	723,392	723,392

Notes: The dependent variable is set to unity if the individual has committed a given crime during the year and zero otherwise. Each column and panel represents a separate regression. All regressions control for age, high school completion, number of children, married, divorced, foreign-born, parental education and compulsory school GPA. The unit of observation is a person-by-year cell (3,816,376 observations). The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. All regressions control for possible missing values in the regressors. Robust standard errors in parentheses account for arbitrary serial correlation and heteroscedasticity. */** denotes significance at the 5/1 percent level

Table 5 The effect of number of days in unemployment on the probability of committing a given type of crime by subgroup

	Any crime	Violent crime	Theft	Drugs	DUI
	(1)	(2)	(3)	(4)	(5)
Baseline	.0051**	.0004**	.0020**	.0013**	.0004**
	(.0021)	(.0001)	(.0001)	(.0001)	(.0001)
	.0527	*.0260*	*.0138*	*.0083*	*.0031*
Criminal history (past two years)	.0214**	.0010	.0090**	.0081**	.0018**
	(.0014)	(.0006)	(.0010)	(.0008)	(.0005)
	.2500	*.1556*	*.0872*	*.0682*	*.0151*
No criminal history	.0045**	.0004**	.0014**	.0007**	.0003**
	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)
	.0366	*.0154*	*.0078*	*.0034*	*.0022*
Foreign-born	.0094**	.0012**	.0034**	.0022**	.0006**
	(.0006)	(.0002)	(.0004)	(.0003)	(.0002)
	.0922	*.0494*	*.0260*	*.0159*	*.0035*
Swedish born	.0043**	.0002**	.0018**	.0012**	.0004**
	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)
	.0466	*.0223*	*.0119*	*.0071*	*.0031*
Low educated parents	.0046**	.0002	.0016**	.0012**	.0003
	(.0007)	(.0003)	(.0004)	(.0003)	(.0002)
	.0638	*.0324*	*.0165*	*.0095*	*.0037*
High educated parents	.0052**	.0004**	.0020**	.0014**	.0004**
	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)
	.0511	*.0251*	*.0134*	*.0081*	*.0031*
Below median GPA	.0070**	.0004**	.0027**	.0022**	.0005**
	(.0004)	(.0001)	(.0002)	(.0002)	(.0001)
	.0767	*.0390*	*.0207*	*.0142*	*.0048*
At least median GPA	.0026**	.0002**	.0010**	.0006**	.0002**
	(.0003)	(.0001)	(.0001)	(.0001)	(.0001)
	.0204	*.0075*	*.0035*	*.0020*	*.0011*
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.0527	.0260	.0138	.0083	.0031
Number of individuals	723,392	723,392	723,392	723,392	723,392

Notes: The dependent variable is set to unity if the individual has committed a given crime during the year and zero otherwise. All estimates and standard errors are multiplied by 100. Numbers in italics show the mean of the dependent variable. Each cell represents a separate regression. All regressions control for age, high school completion, number of children, married and divorced. The unit of observation is a person-by-year cell (3,816,376 observations). The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. All regressions control for possible missing values in the regressors. Low educated parents is defined as both parents having completed no more than compulsory school. High educated parents is defined as at least one parent having completed more than compulsory school. Robust standard errors in parentheses account for arbitrary serial correlation and heteroscedasticity. */** denotes significance at the 5/1 percent level

Table 6 The effect of number of days in unemployment on crime controlling for disposable income

	Any crime	Violent crime	Theft	Drugs	DUI
	(1)	(2)	(3)	(4)	(5)
Baseline	.0051**	.0004**	.0020**	.0013**	.0004**
	(.0021)	(.0001)	(.0001)	(.0001)	(.0001)
Controlling for disposable income	.0052**	.0004**	.0020**	.0013**	.0004**
	(.0021)	(.0001)	(.0001)	(.0001)	(.0001)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.0527	.0260	.0138	.0083	.0031
Number of individuals	723,392	723,392	723,392	723,392	723,392

Notes: The dependent variable is set to unity if the individual has committed a given crime during the year and zero otherwise. All estimates and standard errors are multiplied by 100. Each cell represents a separate regression. All regressions control for age, high school completion, number of children, married and divorced. The unit of observation is a person-by-year cell (3,816,376 observations). The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. All regressions control for possible missing values in the regressors. Robust standard errors in parentheses account for arbitrary serial correlation and heteroscedasticity. */** denotes significance at the 5/1 percent level

Table 7 The effect of number of days in unemployment on the probability of committing crime on weekends versus weekdays

	Weekday crime (1)	Weekend crime (2)
Number of days unemployed×100	.0046** (.0002)	.0017** (.0001)
Individual fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
County fixed effects	Yes	Yes
Mean of dependent variable	.0388	.0204
Number of individuals	723,392	723,392

Notes: The dependent variables are indicators set to unity if the individual committed any type of crime on weekdays/weekends and zero otherwise. Each cell represents a separate regression. All regressions control for age, high school completion, number of children, married and divorced. The unit of observation is a person-by-year cell (3,816,376 observations). The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. All regressions control for possible missing values in the regressors. Robust standard errors in parentheses account for arbitrary serial correlation and heteroscedasticity. */** denotes significance at the 5/1 percent level

Table 8 The effect of number of days in unemployment and its lag on the probability of committing a given type of crime

	Any crime (1)	Violent crime (2)	Theft (3)	Drugs (4)	DUI (5)
Number of days unemployed current year×100	.0051** (.0024)	.0004** (.0001)	.0020** (.0001)	.0015** (.0001)	.0004** (.0001)
Number of days unemployed last year×100	.0002 (.0002)	.0000 (.0001)	.0003* (.0001)	.0008** (.0001)	.0001 (.0001)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	.0527	.0260	.0138	.0083	.0031
Number of individuals	723,392	723,392	723,392	723,392	723,392

Notes: The dependent variables are indicators set to unity if the individual has committed a given crime during the year and zero otherwise. Each column represents a separate regression. All regressions control for age, high school completion, number of children, married and divorced. The unit of observation is a person-by-year cell (3,816,376 observations). The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. All regressions control for possible missing values in the regressors. Robust standard errors in parentheses account for arbitrary serial correlation and heteroscedasticity. */** denotes significance at the 5/1 percent level

Table 9 The effect of unemployment on crime using aggregated data

	Any crime (1)	Violent crime (2)	Theft (3)	Drugs (4)	DUI (5)
Average number of days in unemployment ×100	.0002 (.0019)	.0045 (.0030)	−.0035 (.0036)	.0141 (.0099)	.0030 (.0039)
County fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations (N×T)	308	308	308	308	308
Number of counties	24	24	24	24	24

Notes: The period of observation is 1992 to 2005. The dependent variable is the (log) share convicted persons for crimes committed in a given year in a county-by-year cell. Each cell represents a separate regression. Panel corrected standard errors are calculated using a Prais-Winsten regression where a county specific AR(1) process is assumed. The sample consists of males aged 19 to 25 observed during the period 1992 to 2005. */** denotes significance at the 5/1 percent level