CRIMINOLOGY

EXAMINING THE GENERALITY OF THE UNEMPLOYMENT–CRIME ASSOCIATION*

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This article examines whether the relationship between unemployment and criminal offending depends on the type of crime analyzed. We rely on fixed-effects regression models to assess the association between changes in unemployment status and changes in violent crime, property crime, and driving under the influence (DUI) over a 6-year period. We also examine whether the type of unemployment benefit received moderates the link to criminal behavior. We find significantly positive effects of unemployment on property crime but not on other types of crime. Our estimates also suggest that unemployed young males commit less crime while participating in active labor market programs when

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compared with periods during which they receive standard unemployment benefits.

The association between unemployment and crime remains a matter of debate, as both theory and empirical evidence suggest that selection mechanisms explain why individuals who are unemployed also commit a disproportionate share of crime in the population (Caspi et al., 1998; Gottfredson and Hirschi, 1990; Heckman, Stixrud, and Urzua, 2006). In other words, it remains unclear whether unemployment causes crime or whether both are a reflection of underlying traits in the people most likely to become unemployed and criminally active. Selection into unemployment is then a fundamental issue when addressing the unemployment–crime link. A further complication related to the unemployment–crime link is that theories that view unemployment as a causal determinant of crime give varying hypotheses on whether unemployment should increase all types of crime or whether we should only expect levels of income-generating crime to increase as a result of financial problems that joblessness creates. Supporting the latter view, macrolevel evidence indicates that the unemployment rate is a better predictor of property crime than of violent crime (Cantor and Land, 1985; Chiricos, 1987; Raphael and Winter-Ebmer, 2001).

Macrolevel evidence aside, a substantial portion of individual-level research on employment and crime focuses on the topic of criminal desistance (Laub and Sampson, 2001; Uggen and Wakefield, 2008). Most experimental studies focus only on high-risk samples of people who are on probation or parole, thereby addressing only the question of how employment provided to offenders influences crime (Berk, Lenihan, and Rossi, 1980; Uggen, 2000)—not whether periods of unemployment that happen through the natural life cycle influence criminal behavior. Evidence from experimental program evaluations on the effects of employment and job placement on crime is inconclusive (Bushway and Apel, 2012; Raphael, 2011; Visher, Winterfield, and Goggeshall, 2005). Quasi-experimental evidence in the United States that uses minimum age restrictions on work in different states as an instrument for different age–work propensities in states shows that increased work is associated with reduced youth crime (Apel et al., 2008). However, few individual-level, quasi-experimental studies have addressed the unemployment–crime link directly, and none of which we are aware of examine this issue in a convincing fashion among adults in a general population sample.

Given that most research on unemployment and crime has been conducted in the United States, Nordic countries represent a different context in which to examine the association between unemployment and crime. In addition to universal access to free higher education, the tax policies in these countries redistribute income and provide the entire population with

a host of social welfare benefits not available in the United States. If the effect of unemployment on crime is related to financial hardship, then this effect should be smaller in countries that soften the financial shock that results from job loss. Despite the comparatively generous social policies and more even income distribution, recent research has shown that substantial socioeconomic differences in crime persist in the Nordic countries (Aaltonen, Kivivuori, and Martikainen, 2011; Galloway and Skardhamar, 2010; Nilsson and Estrada, 2009). However, it remains unclear to what extent selection processes contribute to socioeconomic differences in crime in Nordic countries.

The current study examines whether periods of unemployment are associated with changes in crime, holding constant stable individual differences in criminal propensity. We focus specifically on three questions. First, we examine whether periods of unemployment are associated with increased levels of crime overall once stable individual differences are taken into account. Second, we examine whether periods of unemployment and the length of unemployment are associated with within-individual changes in different types of crime. Third, we examine whether crime rates vary depending on the type of unemployment benefits that individuals receive. We use a quasi-experimental design to examine the effects of unemployment over relatively short time intervals as a method for identifying the effects that shifts in unemployment have on crime (see Felson et al., 2011; Horney, Osgood, and Marshall, 1995; Laub and Sampson, 2003; Osgood et al., 1996). Longitudinal panel data with short time intervals can shed more light on these questions with the help of fixed-effects models (Allison, 2009) that control for time-stable individual differences and allow one to identify the unemployment effect from only within-individual change. As an addition to prior studies, we use Finnish register-based data on both unemployment and criminal convictions to construct a longitudinal panel of timing of unemployment, type of unemployment, and different categories of criminal convictions. We use a nationally representative sample of Finnish men 20–30 years of age ($N = 15{,}658$) whom we follow for 6 years over 3-month intervals, thus allowing us to shed light on the effect of unemployment on crime and types of crime in an unselected sample of the entire population who commit a crime at least once during the study period.

THEORETICAL FRAMEWORK

Effects of life events on crime have been discussed under the concepts of *population heterogeneity* and *state dependence*, ideas first introduced to criminology as explanations of the relationship between past and future offending (Nagin and Paternoster, 1991; Piquero, Farrington, and Blumstein, 2003). Gottfredson and Hirschi's (1990) self-control theory is typically

presented as an example of the population heterogeneity perspective. According to the theory, low self-control is a stable individual trait that explains both crime and unemployment along with several other analogous behaviors, such as substance abuse and driving accidents. Thus, the correlation between employment and crime in adulthood is said to be spurious, resulting from an underlying common cause for both outcomes. Recent research has supported the notion that childhood low self-control predicts a variety of adverse outcomes in adulthood, even after controlling for socioeconomic status and intelligence (Moffitt et al., 2011). The key feature of this line of reasoning is that these stable propensities are established early on and that they manifest in different forms of antisocial behavior over the course of life (Gottfredson and Hirschi, 1990; see also Moffitt, 1993). If crime is solely a function of stable traits such as low self-control, then an extreme version of the population heterogeneity argument would suggest that within-individual variation in unemployment should be unrelated to any type of crime.

In contrast, the concept of state dependence asserts that dynamic processes matter and that events can change the trajectories of individual lives. Whereas state-dependent processes can explain continuity in criminal behavior (Nagin and Paternoster, 1991), salient life events also can trigger positive (or negative) change. The emphasis on the effects of life events on crime was articulated most clearly in the work of Sampson and Laub (1993), who argued that social bonds and informal social control in adulthood have an effect on crime even after taking into account stable criminal propensity and early risk factors (see Laub and Sampson, 2003; Sampson and Laub, 1993). Marriage and work are key institutions that provide both bonds and control. The initial link between unemployment and crime may be a result of stable propensities in the population shaped early in life, but the process of working itself may create a "knifing-off" point from the past (Laub and Sampson, 2003). Conversely, unemployment is connected with crime because unemployed persons lack the social bonds that provide them with a stake in conforming law-abiding behaviors. Thus, it is not employment as such (Uggen, 1999) but the social bonds and control associated with it that reduce offending. Furthermore, Sampson and Laub (1993) stressed that job quality is of primary importance for desistance from crime given that only higher quality jobs will provide the individual with the necessary prosocial bonds (Uggen, 1999).

Whereas several state-dependent explanations propose a causal effect of unemployment on crime, these explanations differ with respect to the types of crime they seek to explain. Hypotheses derived from control theories, such as those proposed by Sampson and Laub (1993), provide a general theory that can apply to all crime types (Bushway and Reuter, 2002). Strain theories (Agnew, 1992; Merton, 1968) also are applicable to explaining the

link between unemployment and several types of crime. Merton's (1968) original formulation was interpreted as a focus on utilitarian crimes as an innovative response to the strain produced in American society from the disjuncture between the emphasis on economic success and culturally prescribed means. Farnworth and Lieber (1989) noted in their test of strain theory that their finding of a stronger association between the "gap between economic goals and educational expectations" and the prevalence of serious utilitarian versus nonutilitarian delinquency was evidence that was "consistent with general expectations concerning the theory's particular value for different types of offending" (p. 271). In contrast, Agnew's (1992, 2006) general strain theory seemed to be more focused on aspects by which strain also may produce general offending patterns that include nonutilitarian crimes as a response to one's negative affective state when coping mechanisms fail (Felson et al., 2011).

In contrast with criminological theories that expect general associations between unemployment and crime, economic choice theory (Becker, 1968; Ehrlich, 1973) is concerned primarily with the association between employment and crimes that have economic returns. Instead of reacting to strain or lack of informal social control stemming from unemployment, here, the individual makes a choice between legal and illegal work based on the relative attractiveness of each option and its potential payoff (Bushway and Reuter, 2002). According to Felson et al. (2011), responding to financial stress with income-generating crime is in accordance with a rational choice perspective that views a specific type of crime as an instrumental response to a specific type of stress. Although employment and crime are not mutually exclusive choices, allocation of time to work reduces the amount of time available for other activities (Bushway and Reuter, 2002). In a related fashion, routine activity theory (Cohen and Felson, 1979) points to the ways in which unemployment changes daily routine activities: Increased periods of idleness expose people to greater criminal opportunities (Osgood et al., 1996). However, hypotheses derived from routine activities theory are not limited to property crime, as increased idle time can for instance facilitate greater alcohol consumption, which might, in turn, lead to more violent disputes.

In sum, two key contrasts divide criminological theories in their stance on unemployment and crime. The first is the division between theories that favor selection processes, which argue that the association between unemployment and crime is spurious, and those perspectives that hypothesize a causal effect of unemployment on crime. Here, Gottfredson and Hirschi (1990) are in one corner, while the rest of the aforementioned theories remain in the other corner. The second more subtle distinction is whether unemployment should be related to different types of crimes. Economic choice theory favors a crime-specific link, whereby unemployment should

be correlated with increases in property crimes that have financial payoff. More general criminological theories do not explicitly rule out an association between unemployment and several types of crime. The current study derives its research questions from these theoretical distinctions. In the next section, we provide a review of selected empirical studies on employment and crime. The focus is on studies that have analyzed within-individual change in life circumstances and crime, as those provide the closest comparison for current study.

EMPIRICAL STUDIES ON EMPLOYMENT AND CRIME

As mentioned, experimental evidence on the crime-reducing effect of employment programs is mixed (Visher, Winterfield, and Goggeshall, 2005). However, one should bear in mind that the programs evaluated are a special case of employment being conditionally offered to high-risk groups and that failure rates of appearance and completion are high (Bushway and Reuter, 2002). One positive exception is the study by Uggen (2000), who discovered that the effect of work on crime in the National Supported Work Demonstration Program experiment was moderated by age, such that a crime-reducing effect was evident for the treatment group who received work support and were older than 26 years of age. Uggen's (2000) findings suggested that some of the modest-to-null effects discovered in other employment and crime experiments may be driven by the fact that the programs were delivered to individuals before they were at an appropriate age at which work would produce a "knifing-off" point in their life courses. Regarding evidence from Nordic societies, recent studies using register data (Savolainen, 2009; Skardhamar and Telle, 2012) indicated that becoming employed after prison is associated with reduced recidivism. Thus, work seems important in a welfare state context as well.

Most individual-level studies with observational data have used samples designed to capture sufficient numbers of offenders to generate reasonable statistical power to detect correlations between periods of employment and crime (Uggen and Wakefield, 2008). Longitudinal data on a general population or specific cohorts are usually more readily available for adolescents. Farrington et al. (1986) examined changes in employment status and crime among a sample of at-risk adolescents and found that the property crime rate was higher during periods of unemployment but that the rates of other types of crime did not vary by employment status. They also found that the effect of unemployment on crime was restricted to those with high criminal propensity, indicating an interaction effect between preexisting risk factors and unemployment (Farrington et al., 1986). The common finding that employed school-aged youth commit more crime than unemployed youth of the same age may seem to contradict such findings. However, several

studies based on longitudinal data have examined the effect of adolescent work on delinquency, and they have concluded that the high level of crime among working adolescents is likely to stem from preexisting differences between those who work and those who do not during school-aged years (Apel et al., 2007; Paternoster et al., 2003; Staff et al., 2010).

A body of research exists that uses retrospective life event calendars (Roberts and Horney, 2010) that ask individuals to indicate variations in life circumstances and crime either month-to-month or during specific time points on a calendar. These studies analyzed different types of crime. In a pioneering article, Horney, Osgood, and Marshall (1995) found that time-varying life circumstances were associated with offending among adults on probation. Drug use, in particular, was strongly correlated with offending. Contrary to expectations, working seemed to increase property crime and had no significant effect on other types of crime. Later research with similar data, however, suggested that unemployment is linked to different types of crime. Felson et al. (2011) examined the variation in different types of unemployment-related stress and three types of crime (violence, property, and drug dealing) during a 36-month follow-up. They discovered that financial strain was most strongly related to property crimes and drug dealing, whereas family stress was strongly associated with violent crimes. In addition, employment had a significant crime-suppressing effect on drug dealing. Unstructured socializing increased the risk of all three types of crime. Analyzing female inmates, Slocum, Simpson, and Smith (2005) discovered that nonviolent crime and drug use was less likely during spells of employment, but similar effects were not detected for violent crime.

A recent study by van der Geest, Bijleveld, and Blokland (2011) examined the association between employment and serious offending between 18 and 32 years of age for a Dutch sample of individuals who were institutionalized as juveniles. Their results showed that crime was inversely associated with the number of days employed during a year but that this association was most pronounced among the lower offending part of their sample. Regular employment had no effect on the crime rates of high-risk offenders in the sample. The lack of an employment stability effect in this study may be the result of the fact that only a few men in the high-risk offending group succeeded in remaining in the same job for a long time, and thus, the variation in employment stability provided inadequate statistical power to detect an effect. Comparing males and females in the same high-risk sample as that of the aforementioned study, Verbruggen, Blokland, and van der Geest (2012) found that employment was associated with decreased serious offending for both men and women but that the effects of both employment and unemployment durations were different between sexes: Unemployment duration was associated with increased offending for females and with decreased offending for males. Employment duration, on

the other hand, was associated only with decreases in male offending (Verbruggen, Blokland, and van der Geest, 2012).

Although the effect of job quality on crime has been scrutinized (Laub and Sampson, 2003; Uggen, 1999), it is equally plausible that the type of unemployment benefit matters as well. Two recent register-based Danish studies have used experiments (Andersen, 2012) and natural experiments (Fallesen et al., 2012) to assess the effect of active labor market programs on crime, both finding that such programs reduce crime. Andersen (2012) found that unemployed individuals randomly assigned to an intensified active labor market program that included an obligation to participate in a 2-week job search program and regular meetings with a caseworker had a lower level of crime in the follow-up than did the control group, who received a standard program. The findings of Fallesen et al. (2012) are in line with these and show that in Denmark, those unemployed persons living in a municipality that was subjected to a unique policy experiment that imposed strict work or training requirements on welfare recipients in specific municipalities had a substantially lower crime rate during the years of the experiment compared with subjects in municipalities that did not implement these work-to-welfare requirements. These results implied that more structured time use during these unemployment periods reduces crime in comparison with idle unemployment, supporting theories that attribute the effect of unemployment on crime on informal social control and routine activities.

Overall, results from several longitudinal studies of employment and crime suggest that employment status and levels of crime are linked. Yet, many complications remain. Although existing studies do not support an extreme population heterogeneity argument so that stable characteristics would completely explain the within-individual association between unemployment status and crime, it is still unclear whether such an association is found across crime types. Further investigation of crime-specific effects is warranted to complement results from other European studies that have used omnibus or serious offending measures (Savolainen, 2009; van der Geest, Bijleveld, and Blokland, 2011; Verbruggen, Blokland, and van der Geest, 2012), and this is an equally important issue for theory development. Regarding experimental evidence, we know a lot about the failures of job programs to reduce crime (Bushway and Apel, 2012), but we know less about how the period of unemployment in the general population is causally connected with crime. Good longitudinal studies that do exist are typically focused on adolescents or high-risk offender samples. It is unclear as to whether estimates from such data can be extrapolated to the rest of the population likely to experience periods of unemployment. Furthermore, estimates from studies that rely on retrospective data from offender self-reports of both employment and crime may be biased upward, as

individuals who have a greater likelihood of reporting employment also might be those who are less likely to report criminal offenses (Farrington et al., 1986). Although official administrative data are by no means perfect measures for assessing the crime–employment link, they at least speak to the connection between official work and crime, and they rule out the possibility that responding behavior could confound the association. When such data are available nationwide, the analysis does not need to be limited to criminally active individuals, as all individuals who have at least one offense during the period of observation can contribute to within-individual estimates.

CURRENT RESEARCH

We build on prior research and exploit our unique access to Finnish register data in an effort to examine the association between unemployment and crime (see Lyngstad and Skardhamar, 2011, for a review of Nordic register data). A key strength of the data is that the information on unemployment and crime comes from different sources and, although retrospective, is not reliant on people remembering the exact timing of these events. Unlike most prior within-individual studies, the current data do not represent a high-risk sample but a nationally representative general-population sample from which we can identify all individuals with at least one crime during the 6-year follow-up window. The fixed-effects model effectively estimates the parameter of unemployment for only those individuals who have variation in crime and unemployment during the follow-up (Allison, 2009). Nevertheless, this estimate from the sample is meaningful because it provides a method for identifying the effect of unemployment on crime among those in the general population who have committed at least one crime during the follow-up—not just for those who are defined by a high-risk sampling frame. In this study, we specifically answer two research questions and provide a series of robustness checks for our findings.

> *First, we ask whether the same individual commits more crimes while unemployed than while not unemployed and whether such an association depends on the type of crime. Furthermore, does unemployment length matter?*

In the first part of the analysis, we examine temporal patterns of unemployment and crime overall as well as its classification by types of crime. We use four outcome variables: all crime, property crime, violent crime, and driving under the influence (DUI). These outcomes, comprising relatively common crimes among Finnish young adults, are differentially related to the economic hardship that unemployment causes. Property crime should

be connected with unemployment according to all "state-dependent" explanations reviewed. For violent crime, the link is less obvious: Unemployment might increase strain or stress, which could induce violence. Such an association also could materialize because of lack of social control or changed routine activities. In any case, the unemployment–violent crime association should be more indirect than one by which unemployment increases one's likelihood of committing crimes that have economic payoff. DUI should, in principle, be least connected with economic problems because alcohol costs money. On the other hand, drinking alcohol could increase during idle time when someone is unemployed, thus increasing one's risk of being charged with DUI.

If no temporal association exists between periods of unemployment and any type of crime, then theories that suggest that stable population heterogeneity is the cause of the unemployment–crime link are supported. On the other hand, if we find associations with all types of crime, then theories that predict a general unemployment–crime association are supported, and pure selection hypothesis is refuted. Finally, if we find only an association between unemployment and property crime, then economic choice theory is supported, and the generality of the unemployment–crime association is brought into question. Several different unemployment specifications are used to assess the robustness of our findings. Furthermore, as a longer unemployment duration is expected to cause greater financial difficulties, we test whether a dose–response association is found.

Second, we ask whether the unemployment crime occurs through a lack of financial resources or through a lack of social control and changes in routine activities.

Although our data do not include direct time-varying measures of social control and routine activities, the structure of the Finnish unemployment benefit system allows us to assess indirectly these possible mechanisms that link unemployment to criminal behavior. In particular, youth younger than 25 years of age without secondary education or prior work experience are targeted by active labor market programs (typically labor market training or practical training among youth), which seek to enhance the future employment prospects of unemployed youth. The other aim of these obligations is to make "passive" unemployment more difficult, as a refusal to take part in the programs may result in losing the eligibility for the unemployment benefit. Unemployed youth without sufficient work history in Finland receive a *labor market subsidy* in either *active* or *passive* form. The active form of the subsidy is paid during participation in active labor market programs (Hämäläinen and Ollikainen, 2004). The passive labor market subsidy simply pays a monetary benefit while a youth is unemployed. This

provides us with additional quasi-experimental variation from which to examine the unemployment–crime link. We assume that informal social controls and daily routine activities are more structured during active periods, as one is required to participate in practical training, education, or subsidized work to receive his or her monetary payment. The size of the benefit, however, is practically equal in both active and passive labor market subsidies in this age group. If the unemployment effect is at least partly attributable to changes in informal social control, social bonds, and routine activities—and not only to lack of income—then we should expect levels of crime to be lower during participation in the active programs.

DATA AND METHODS

Our data come from the register-based Risk Factors of Crime in Finland (RFCF) database, which contains longitudinal individual-level information about a stratified random sample of 150,010 Finnish residents. In this analysis, we use a subsample of 15,658 male individuals who were 20–30 years of age in 2001. They were followed for 6 years, from the beginning of 2001 to the end of 2006, until they were 26–36 years of age. Although the unemployment–crime association among females should be investigated as well, the current study focuses on men because the original sampling frame oversampled men, and this, coupled with a much lower baseline level of crime among women, means that joint estimates would still mainly describe the association between unemployment and crime among men.[1] Furthermore, the size of the data is too small to analyze sex interactions reliably or to run separate models by sex. The data were created by combining information from several administrative registers into a sample drawn from the Finnish Population Information System. The key variable to linking the data from different sources is the personal identification number, a unique and highly reliable identifier that all Finnish citizens and those who have resided in the country for more than 1 year have. Using this identifier, we were able to find both unemployment and criminal records for all subjects in the data.

The data are set up as 3-month-interval panel data, thus comprising a maximum of 24 quarters of information regarding number of crimes and days unemployed during each time period. If an individual has died or moved to another country during the follow-up, then the time periods after those events are censored from the analysis. Although the data do not include the exact dates of entries and exits from prison, we used information about convictions (that included a prison sentence) in each period to

1. When the fixed-effects models in table 2 were fitted again in the total sample including both men and women, the results were almost identical.

approximate the times during which the individuals were absent from the labor force because of incarceration. These periods are dropped from the analysis. Exact information on pretrial custodies (remand prison) is available and is similarly used in censoring those periods from the analysis. Periods of disability retirement (outside labor force) also are excluded.

MEASURES FOR UNEMPLOYMENT

Unemployment Benefit Types and Duration

The data on unemployment come from the administrative registers of the Social Insurance Institution of Finland (SIIF), the authority responsible for basic social security. The data include the start and end date of each unemployment spell during 2001–2006. Three types of unemployment benefits exist in Finland: *earnings-related unemployment allowance*, *basic unemployment allowance*, and *labor market subsidy*. The first is a benefit aimed at those who have sufficient employment histories and have paid unemployment insurance while working (not paid by SIIF and, thus, not included in the data), the second for those with similar employment histories but no insurance, and the third for those with neither. Unemployed youth most often receive the third option. In the first part of the analysis, we classify as unemployed those who received either basic unemployment allowance or a labor market subsidy (basic benefits), who receive roughly the same monthly monetary amount (SIIF, 2005). The basic unemployment allowance requires a work history of at least 34 weeks during the previous 28 months prior to unemployment, which is similar to unemployment insurance benefits paid in the United States for someone who loses his or her job by no fault of his or her own. If a person is not employed after 500 days of receiving the basic unemployment allowance, then he or she will then begin to receive a labor market subsidy. The labor market subsidy can be thought of as a basic level of public assistance that all Finnish adult residents who either lack work history or are undergoing long periods of unemployment receive; this type of support is not available in the United States.

We measure unemployment during each 3-month period using dummy variables indicating 1) at least 1 day of unemployment and 2) at least 1 month of unemployment, and with 3) a continuous variable for the number of days of unemployment. To assess the effect of unemployment length, we use two different specifications. The first is a variable that indicates the total cumulative number of unemployed periods (the total number of 3-month periods unemployed) since the start of the follow-up, whereas the second is a variable that indicates the length of the current unemployment spell (the number of successive 3-month periods unemployed). In effect, the reference category remains the same, but the focal status of the variable now records the length of unemployment.

We focus on young men because older unemployed people increasingly receive an earnings-related unemployment allowance that we cannot capture as these benefits are not paid by SIIF. However, they receive considerably more money monthly than those who receive the basic benefits do (roughly twice as much among men 20–29 years of age), and thus, they differ in terms of financial strain (SIIF, 2005). More importantly for this study, we note that in 2004, 76 percent of unemployed men 20–29 years of age received basic benefits (SIIF, 2005). Thus, *our measure should capture the majority of those young men who are registered as unemployed and, more importantly, those who are experiencing worse financial strain related to unemployment*. Our validity checks (see the figure in the online supplement)[2] indicate that unemployment length in 2004 is inversely related to yearly income (more than half of those who were unemployed for more than 6 months belonged to the poorest quintile), and when checked against a register of occupations maintained by Statistics Finland, the absence of registered occupation becomes more and more likely as unemployment length grows (87 percent of those who were unemployed for at least 1 day in the 4th quartile of 2004 were without occupations at the end of 2004).

Although these validity checks indicate that the unemployment measure taps well into financial hardship, a possible heterogeneity in the reference category "not unemployed" might influence the results. In other words, we compare the same unemployed individual with himself during all other possible states. Although most young men who are not unemployed are either working or studying, "off-the-grid" individuals outside of the education and labor markets are potentially those who are most seriously marginalized and criminally active, and including them in the reference category might bias the unemployment effect downward. Why would somebody not sign up as unemployed and receive the benefits associated with it? First, receiving unemployment benefits requires regular reports of unemployment status to employment services, and the individual must actively seek employment and accept offered work or training (SIIF, 2012). Second, additional restrictions exist on individuals 18–24 years of age who lack vocational training, as they are required to apply to schools and to participate in the active labor market programs to remain eligible for their unemployment benefits. To assess whether these features of the system influence the results, we conduct several robustness checks. Given the age restriction for receiving unemployment benefits for those without education and work

2. Additional supporting information can be found in the listing for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2011.51.issue-3/issuetoc.

history, we fit separate models for those without basic education once they turn 25 years of age. After this age, our unemployment measure becomes a cleaner assessment of true unemployment (and the reference category subsequently less ambiguous) for this subgroup of individuals with low education, known to have a comparatively high level of crime in Finland (Aaltonen, Kivivuori, and Martikainen, 2011). Finally, as prior research (Uggen, 2000) has indicated that the effect of unemployment might be age graded, we examine whether the effect of unemployment on crime increases with age.

Active versus Passive Unemployment

In the second part of our analysis, we examine only time periods during which the individual received a labor market subsidy, and we include only such persons who received both active and passive benefits at least once. An individual receives an active benefit while participating in active labor market programs, whereas the passive unemployment benefit is paid to the person not participating in such programs. This distinction is useful because the monetary amount is practically the same, but one form of benefit requires that individuals increase their human capital skills. Given that our hypothesis is that crime should be lower during active labor market programs, monetary benefits seeming to be marginally higher during passive unemployment periods do not jeopardize the analysis (in the data, the average incomes of those who received active benefits for more than 8 months during 2004 were 5,490 euros yearly, whereas those who received passive benefits for the same duration had yearly average incomes of 5,740 euros). As active programs target mostly individuals younger than 25 years of age, we reduce the analysis to those who are 18–24 years of age. In this analysis, we use a static age window, meaning that younger individuals enter and older individuals exit the sample with time. The data set (9,460 time periods, 1,476 men) is thus slightly different than that of previous models.

MEASURES FOR CRIME

The crime data that include both court sentences and fines come from a register of convictions that the National Research Institute of Legal Policy maintains. We classify the crimes temporally according to the date of the offense, not the date of the conviction. As the data are not large enough to obtain reliable estimates for rare crimes, we focus on types of crime that are common on population level. We use four count outcome variables: all crime, violent crime, property crime, and DUI. The "all crime" category is dominated by minor traffic offenses, thus resulting in a high prevalence measure during the follow-up. In any case, such an outcome is useful when

it records the incidence of events. The category of property crimes consists mainly of thefts and larcenies, whereas assaults and minor assaults are the most typical violent crimes. Previous research has shown substantial (between-individual) differences in all three specific types of crime by level of education and unemployment among young adults (Aaltonen, Kivivuori, and Martikainen, 2011).

STATISTICAL MODEL

We use a fixed-effects Poisson regression model (Allison, 2009) to estimate the time-varying effect of unemployment on crime rate. We use a fixed-effects model to remove the confounding effects of stable unobserved differences between subjects that are likely to affect crime. In effect, we focus only on within-individual change (or "use each individual as his own control") in both unemployment and crime during 24 three-month periods. As overdispersion (conditional variance exceeds the conditional mean) is often a problem with crime count data, we calculate robust standard errors (Wooldridge, 1999) for the estimates. The model is estimated according to the following form:

$$\log \lambda_{it} = \alpha_i + x\beta_{it} + \mu_t \tag{1}$$

In the Poisson equation, λ represents the estimated number of crimes for each individual i at time t, controlling for time (μ) and individual (α) parameters. In other words, we estimate a two-way fixed-effects model that includes separate intercepts for every individual and for every time period.[3] Our primary parameter is the effect of unemployment β for an individual in a given time period who is *ever unemployed*. Thus, this model effectively compares the change in crime before and after a period of unemployment for individuals who are ever unemployed relative to anyone else in the sample that also has a criminal conviction.[4] The person level, fixed

3. The use of dummy variables for time periods instead of age could be problematic if the expected pattern of change varied by baseline age. However, as we are examining a group of individuals with a monotonically declining age trend in crime, this is not an issue. Other specifications using calendar age produced similar results for the key unemployment parameters of interest.
4. To observe that it is the case that the fixed-effects estimator is essentially a difference-in-differences model, see Bertrand, Duflo, and Mullainathan (2004). Because we obtain variation on relative effect of unemployment after including dummy variables for each person (fixed effects) and time period for only individuals being unemployed, the relative effect for unemployment on crime is for those who are ever unemployed (treatment) relative to those who are not (control) and have been convicted of a crime.

Table 1. Descriptive Statistics (N = 15,658)

Variables	Mean	SD	Minimum	Maximum	Prevalence
Age in 2001	24.49	3.17	20	30	—
Crimes 2001–2006					
All	1.56	6.11	0	191	41.5%
Property crime	.32	2.93	0	95	5.1%
Violent crime	.07	.47	0	16	3.9%
DUI	.10	.62	0	25	5.7%
Prison 2001–2006					
Months convicted	.28	3.83	0	144	1.2%
Months in remand prison	.02	.45	0	21	.9%
Unemployment 2001–2006					
Days unemployed	119	320	0	2,161	26.8%
"Active vs. passive" data[a]					
Days on active benefit	30	94	0	1,294	17.0%
Days on passive benefit	65	175	0	2,070	28.8%

ABBREVIATIONS: ALMP = active labor market program; DUI = driving under the influence; SD = standard deviation.
[a]Subsample of male individuals 18–24 years of age. Fixed age window, 15,476 respondents.

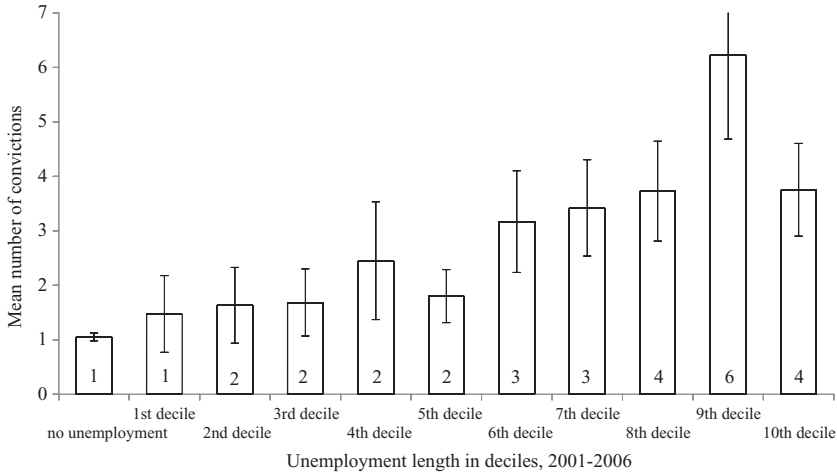
effects control for the average between-person differences in the levels of offending, so we are effectively measuring a series of person-level interrupted time series during which individuals who experience unemployment are the treatment group and everyone else who has a criminal conviction serves as the comparison group. Thus, whereas those without variation in unemployment measure do not directly contribute to the estimated unemployment parameter, they still contribute to the expected pattern of change in crime *with time* when there is no change in unemployment status. As a robustness check, we fit the models again with lagged ($t - 1$) unemployment variables and as fixed-effects logistic models. We used Stata 12.1 (StataCorp, College Station, TX) to estimate the statistical models.

RESULTS

DESCRIPTIVE STATISTICS

After censoring deaths (n = 49) and moves abroad (n = 90), a total of 374,922 valid time periods remain in the final sample of 15,658 Finnish male individuals 20–30 years of age at the baseline. When periods of incarceration and disability are removed, the number of valid periods drops by 2 percent (n = 367,503). Approximately 41.5 percent of men in the sample were convicted of at least one offense during 2001–2006: 5 percent of property crime, 4 percent of violent crime, and 6 percent of DUI (table 1). As mentioned, the high prevalence in the all crime category is explained by minor traffic offenses. Only 1.2 percent were sentenced to

Figure 1. Mean Number of Crimes during 2001–2006 by Total Number of Days Unemployed during 2001–2006



NOTE: Vertical bands indicate 95-percent confidence intervals.

unconditional prison terms, and .9 percent spent time in pretrial custody (remand prison). Furthermore, 27 percent of the sample had at least one unemployment spell during 2001–2006.[5] Within each 3-month period, roughly half of those who had at least 1 day of unemployment were unemployed for the entire 3-month period. Relatively strong continuity exists in unemployment: Of those who were unemployed for at least 1 day during a 3-month period, 79 percent were unemployed during the following period as well, and only 1.6 percent changed from being not unemployed to being unemployed.

Figure 1 shows the bivariate association between unemployment length and the total number of convictions and fines. When the sample is divided into the no-unemployment group (73 percent) and ten equal-sized bins by the total number of days unemployed, a clear association exists between the total number of unemployed days and crime during the entire follow-up. The only exception to the otherwise linear pattern is the highest risk in the ninth decile (6.2 offenses per 6 years), which is much lower in the tenth decile (3.8 offenses).

5. The unemployment rate remained stable in Finland during 2001–2006: The rate decreased from 9.1 percent in 2001 to 7.7 percent in 2006 (Ministry of Employment and the Economy, 2012).

Table 2. Unemployment Status and Different Types of Crime

Variables	All Crime		Property Crime		Violent Crime		DUI	
	β	(SE)[b]	β	(SE)	β	(SE)	β	(SE)
Pooled Model								
Unemployed (>1 day)	1.224***	(.070)	1.983***	(.115)	1.674***	(.010)	1.579***	(.101)
FE Models[a]								
(M1) Unemployed (>1 day)	.176**	(.054)	.359**	(.115)	.025	(.123)	.133	(.092)
(M2) Unemployed (>30 days)	.150**	(.051)	.246**	(.092)	.048	(.110)	.132	(.093)
(M3) Unemployed (linear, *n* of days)	.002**	(.001)	.004**	(.001)	.001	(.001)	.001	(.001)
N (FE)	6,446		760		579		870	
$N \times T$ (FE)	153,245		17,471		13,423		20,268	

NOTES: Crime-type specific estimates from pooled and fixed-effects Poisson models; men and women, baseline 20–30 years of age.
ABBREVIATIONS: DUI = driving under the influence; FE = fixed-effects model; SE = standard error.
[a]Time fixed effects (23 dummy variables for time periods) omitted from the table.
[b]Pooled models use cluster-robust standard errors, and FE models use robust standard errors.
$^{**}p < .01$; $^{***}p < .0001$.

FIXED-EFFECTS POISSON MODELS

To put the estimates from the within-individual models into context, first we present results from a pooled model that does not distinguish between within- and between-individual variations. In a pooled setting, using only cluster-robust standard errors to take dependence between observations into account, a Poisson model indicates a strong association between unemployment ($\beta = 1.22$, incidence rate ratio = 3.40) and total crime rate. The association is strongest with property crime, but the associations with violent crime and drunk driving are also of considerable magnitude. These results, however, show only the overall average correlation and do not control for time-stable differences in crime between people who go through periods of unemployment and those who do not.

Table 2 presents the estimates from Poisson regression models that control for between-individual differences in the average crime rate using different unemployment specifications (M1–M3). As in pooled models, an association between unemployment and crime exists when crime is measured with an omnibus measure ($\beta = .18$, incidence rate ratio = 1.19). Although the association is positive for all crime types, it is significant in property crime offenses only: The rate of property crime is on average 43 percent higher during unemployed periods for the same person compared with

Table 3. Cumulative Unemployment Length, Current Unemployment Spell Length, and Crime

Variables	All Crime		Property Crime		Violent Crime		DUI	
	β	(SE)	β	(SE)	β	(SE)	β	(SE)
(M1) Cumulative Length								
<1 year	.153*	(.068)	.307*	(.154)	.007	(.147)	.122	(.111)
1–2 years	.159*	(.070)	.377**	(.127)	.070	(.173)	.179	(.128)
2–3 years	.199	(.113)	.415*	(.180)	.082	(.196)	−.015	(.162)
3+ years	.244	(.127)	.550*	(.256)	.392	(.239)	.052	(.179)
(M2) Length of Current Spell								
<1 year	.176**	(.068)	.345*	(.146)	−.053	(.149)	.129	(.110)
1–2 years	.191*	(.088)	.454**	(.161)	.269	(.210)	.196	(.155)
2–3 years	.163	(.146)	.530*	(.249)	−.128	(.279)	.150	(.210)
3+ years	.007	(.148)	.058	(.304)	.200	(.367)	−.027	(.285)
N (M1)	6,440		752		577		863	
$N \times$ T (M1)	152,296		16,899		13,140		19,806	
N (M2)	6,390		726		548		833	
$N \times T$ (M2)	148,191		15,182		11,802		18,176	

NOTES: Fixed-effects Poisson regression with robust standard errors. Men, baseline 20–30 years of age.
Time fixed effects (23 dummy variables for time periods) omitted from the table.
ABBREVIATION: DUI = driving under the influence; SE = standard error.
$^{*}p < .05$; $^{**}p < .01$.

nonunemployed periods. When compared with the pooled models, the associations between unemployment and crime are greatly reduced: For property crime, the within-individual estimate is only 18 percent of the pooled estimate (fixed-effects β / pooled β), whereas for violent crime, the estimate from the fixed-effects model is only 2 percent of the pooled estimate. These results indicate that unobserved between-individual differences play a major role in creating the initial association between unemployment and crime.

Whereas all unemployment specifications used in table 2 give substantially similar estimates, it seems that longer unemployment (greater than 1 month) has a weaker association with crime than does the variable that distinguishes between those who were unemployed at least 1 day during 3 months and others. A closer inspection (model M3 from table 2 fitted again with a quadratic term for unemployment length) of this association revealed that within the 3-month period, the association between the number of days unemployed and crime was curvilinear rather than linear, and the probability of offending was greatest when the person was unemployed roughly 60 days. In essence, we have an example of diminishing marginal returns to scale of unemployment on crime rates. Next, we examine in more detail whether the length of unemployment matters. The first model in table 3 shows the relationship between total unemployment length and

crime. For all crime and property crime outcomes, it seems that crime rates become higher when unemployment histories become more chronic: The larger the cumulative number of unemployed periods, the greater is the within-individual difference in crime during periods of unemployment and periods of nonunemployment. However, even with this specification, violent crime and DUI do not have a statistically significant association with unemployment status.

Model 2 in table 3 refines this picture by looking at the number of consecutive periods of unemployment. Similarly, as in the first model, it seems that an initial dose-response effect exists, but it disappears for the longest durations. This is in line with the bivariate results presented in figure 1 and with the prior finding (van der Geest, Bijleveld, and Blokland, 2011) that some of the more frequent offenders tend to have erratic unemployment spells instead of persistent unemployment. Supporting this view, our data reveal that frequent offenders have more interrupted (unemployed for only part of the 3-month period) than uninterrupted (unemployed for the entire 3-month period) time periods of unemployment. Overall, the hypothesis that unemployment is more strongly associated with property crime is further supported by these findings.

EFFECTS OF UNEMPLOYMENT TYPE

The overall conclusion from the series of models estimated is that a temporal association exists between unemployment and property crime and that the association tends to grow stronger as unemployment becomes more chronic. This finding implies that the unemployment effect is related to financial problems. However, these results do not rule out the possibility that other life circumstances could matter as well. To assess the effect of other mechanisms, which may fall under the rubric of routine activities or social control, we restrict the analysis to a more selected subgroup of individuals who receive either active or passive unemployment benefits. The comparison now represents only two different states of unemployment in which the individual receives roughly the same amount of money. If the unemployment effect on crime is related to changes in lifestyle routines or to the social control that attending active labor market programs causes, we should expect to observe a difference between the two groups. Although the assignment to these programs cannot be assumed to be random (Hämäläinen and Ollikainen, 2004), by using within-individual models and focusing on individuals who received both types of unemployment benefits during the follow-up, we can at least control for stable individual differences in the rates of crime between the two groups.

Table 4 shows a detectable difference in levels of crime between active and passive unemployment states. Among those young men who received

Table 4. Crime Types during Participation on Active Labor Market Programs versus "Passive" Unemployment

Variables	All Crime		Property Crime		Violent Crime		DUI	
	β	(SE)	β	(SE)	β	(SE)	β	(SE)
Unemployment Type								
Active (ref. passive)	−.259	(.134)	−.502**	(.224)	−.021	(.413)	−.363	(.289)
N	355		101		49		71	
$N \times T$	2,837		841		393		620	

NOTES: Fixed-effects Poisson regression with robust standard errors. Men, fixed age window 18–24 years of age.
Time fixed effects (23 dummy variables for time periods) omitted from the table.
ABBREVIATIONS: DUI = driving under the influence; SE = standard error.
$^{*}p < .05$.

both types of benefits at least once, the level of property crime is substantially lower ($\beta = -.50$) during active labor market programs. The effect again only appears for property crime even though the subsidy is nearly the same. Although not significant, the reduction of drunk driving offenses during active labor market programs is also of considerable magnitude ($\beta = -.36$). These results suggest that the unemployment–crime link we are observing in terms of property crime in Finland for young adults is not solely the result of a lack of income.

ROBUSTNESS CHECKS

As mentioned, the ambiguity of the reference category in the models presented in tables 2 and 3 might bias the results, as all of those who are not receiving unemployment benefits are not necessarily better off.[6] Next, we present some additional models to assess the robustness of our findings to specific subgroup differences. First, we examine those who lack secondary education after they turn 25 years of age, as we know that the restrictions for receiving unemployment benefits are relaxed at that age and that the measure of unemployment benefits becomes cleaner for less educated individuals who are older than 25 years of age. However, when models are reestimated for this subgroup of individuals older than 25 years of age who lack a secondary education, the effect sizes are similar ($\beta = .40$ for property crime in the new model compared with $\beta = .36$ in table 2) to those of the original model. The positive association between unemployment and property crime is significant, but the effects on other types of crime

6. Also, some unemployed individuals receive an earnings-related unemployment allowance that we cannot identify, and the size of that group grows with age.

remain insignificant, and the relationship between unemployment and violent crime is reversed (table 5). When we estimated the same model for individuals 20–24 years of age without a secondary education, the unemployment effects were slightly stronger, although we can be certain that the unemployment measure is less precise in measuring actual joblessness for the younger subgroup. Thus, the finding of increasing unemployment effect with age (Uggen, 2000) is not replicated. Although natural maturation could explain why the association between unemployment and crime seems to decline marginally with age, not even the use of a cleaner unemployment measure makes the associations stronger in our data.

Next, we replaced the Poisson models with fixed-effects logistic regression models to remove the possibility that a small number of high crime counts are driving the results. The major difference between these models is that we are now only estimating a change in the prevalence of crime in the sample and not in its frequency. The association between unemployment and overall crime becomes null in logistic regression models (table 5). This is probably because the most common offenses in the data are minor traffic offenses that many young men commit at least once. The association between property crime and unemployment, on the other hand, remains statistically significant (odds ratio [OR] $e^{.25} = 1.28$). Finally, following the reasoning of Cantor and Land (1985), who suggested that the effect (increased motivation) of unemployment on crime might not be instantaneous, we used lagged values of independent variables instead of contemporaneous measures. By using a lagged ($t - 1$) measure of unemployment, we could detect a significant association between unemployment at t_{-1} and property crime at t_0. The effect, however, was weaker than the contemporaneous one.

DISCUSSION

All our models indicate that the within-individual association between unemployment and crime is consistent for property crime, whereas violent crime and drunk driving are not associated with unemployment in a meaningful way. Over longer follow-up, those people who commit violent crimes and drive while drunk are certainly unemployed for much longer times than most, but they do not seem to be more likely to commit these crimes while they are registered as unemployed. These findings were confirmed with models that take the length of unemployment into account and use binary outcome variables and lagged unemployment measures. Thus, the conclusion is that within-individual variation in unemployment status is not associated with violence and DUI convictions, but it is associated with property crime convictions.

Table 5. Robustness Checks with Fixed-Effects[a] Poisson and Logistic Models

Variables	All Crime		Property Crime		Violent Crime		DUI	
	β	(SE)	β	(SE)	β	(SE)	β	(SE)
(M1) Basic Education, >25 Years of Age, Poisson	.199	(.113)	.406*	(.185)	−.080	(.251)	−.081	(.179)
(M2) Binary Outcome, Logistic	.009	(.037)	.248***	(.069)	.070	(.107)	.108	(.089)
(M3) Lagged Unemployment, Poisson	.086	(.049)	.214*	(.082)	.043	(.123)	.053	(.095)
N (M1)	610		157		123		170	
$N \times T$ (M1)	14,150		3,459		2,769		3,864	
N (M2)	6,446		760		579		870	
$N \times T$ (M2)	153,245		17,471		13,423		20,268	
N (M3)	6,310		737		567		838	
$N \times T$ (M3)	143,727		16,192		12,573		18,686	

ABBREVIATION: DUI = driving under the influence; SE = standard error.
[a]Time fixed effects (23 dummy variables for time periods) omitted from the table.
$^{*}p < .05$; $^{***}p < .0001$. Unemployment specified as at least one day of unemployment during the three-month period.

Our results partially support theories that view unemployment as a causal determinant of crime. However, our results indicate that the effects of unemployment are specific to property crime and relatively small compared with stable differences in offending between people. In discovering a crime type-specific association, our results give support to economic choice theory and those variants of strain theory that do not imply a general strain–crime association (see also Felson et al., 2011). However, as we do not actually measure individual choice or strain, these conclusions are made with caution. In any case, our results are in line with Farrington et al. (1986), who found that unemployment only increased property crime, and with Felson et al. (2011), who discovered that financial stress was mostly related to property crime. On the other hand, the associations with other types of crime also were mostly positive, which indicates that the lack of significant associations may be the result of low statistical power. On the one hand, the fact that we only find an effect for property crime suggests that the effect is related to lack of income. On the other hand, the more restricted effect we found when comparing the same unemployed individuals while on active versus passive unemployment benefits implies that the effects of social control and routine activities cannot be dismissed, as this effect cannot be attributed to differences in income.

According to Laub and Sampson (2003), a *stable* job is the kind of turning point that should reduce recidivism (see also Skardhamar and Telle, 2012). This study focused on the temporal association between spells of unemployment and crime in a general-population sample of convicted individuals and not on the longer term effects over the life course. Although we detect much variation in unemployment during the 6-year window, it might still be that these shifts do not represent major life events and that individual life circumstances actually remain stable. Despite these reservations, given that the fixed-effects estimates are much smaller than the ones from models that do not control for between-individual differences, our results are consistent with those theories (Gottfredson and Hirschi, 1990) that emphasize that stable heterogeneity in the population drives an important share of the variation in offending. Although theory and evidence suggest that individual traits such as low self-control certainly affect both unemployment and crime (Moffitt et al., 2011), in this study, these selection processes remain a black box.

LIMITATIONS

Although the current administrative data are of high quality, as with all data, notable limitations exist. For one, the composition of the reference category is relatively heterogeneous. If we could reliably identify each person who does not receive an unemployment allowance but is still in a

marginalized position, then we might witness stronger unemployment effects. These sources of missing information mean that our estimates are a lower bound, as those individuals who are convicted of crimes and who are not seeking work or unemployment benefits are being compared with those who are convicted and are seeking work. More nuanced measurement of work and unemployment would make the analysis stronger. In general, studying socioeconomic status and employment among youth will always be complicated because of the transitional nature of life circumstances during young adulthood. However, some additional measures of financial hardship, such as social assistance receipt or debt defaulting, would be useful for identifying those individuals who are in the worst financial positions. In the end, despite the good population coverage of Finnish register data, it will always be difficult to research and identify those individuals who are off of society's radar. Navigating in Finnish administrative bureaucracy might require skills and patience that some of the most troubled individuals do not possess.

However, when the unemployment measure is cross-checked with other measures, it is evident that unemployed individuals are much worse off financially than those in the reference group are. All robustness checks indicate that at least a type 1 error in property crime models is unlikely, as the models generally indicate a statistically significant positive association between the two. To improve the data even more, we should incorporate more time-varying measures of life circumstances into the model. Regarding the measurement of crime, self-reports or police-reported crime would be good alternative sources to use as outcomes, but it is hard to tell whether this would systematically alter the results.

Another methodological problem concerns the ways of modeling population heterogeneity and state dependence (Ousey et al., 2008). The fixed-effects approach does a good job of taking out the time-stable individual effects (Allison, 2009), but the possibility of dynamic selection bias (Bjerk, 2009) remains. It is likely that prior crimes and criminal sanctions, in particular, have an effect on subsequent employment chances (Pager, 2003; Western, 2002), which means that the issues of reverse causation emerge. One solution could be to use dynamic panel data models (Halaby, 2004) with lags of the dependent variable as instruments so as to take the state-dependent effect of prior crime into account (for an application in criminology, see Ousey et al., 2008). However, the approach of including prior crime lags does not solve the simultaneity problem in identifying the unemployment-to-crime link, and the potential for omitted dynamic variables that explain both processes remains. In the end, these limitations mean that the causal effects of unemployment on crime are better left to be identified with settings that provide exogenous variation in unemployment or with experimental designs where feasible (Berk, 2004). In sum, our estimates may be

biased downward by the failure to identify the actually unemployed, off-the-grid individuals and may be biased upward because of the simultaneous determination (Mesters, 2011) of both unemployment and crime.

CONCLUSION

The Nordic countries clearly differ from the United States in many ways, and the presence of a strong safety net makes joblessness less of a financial concern in Finland. Although it would be too much to claim that no structural inequalities exist in Finland, several indicators reveal that such inequalities are less pronounced in Finland than almost anywhere else. Although this safety net should protect against severe poverty related to job loss, unemployment and crime are still associated on the individual level in our sample. Even though economic choice and strain theory (if the latter is considered in its more limited form) seem supported, as we only find a positive association with property crime, differences in the level of criminal involvement by type of unemployment indicate that social control and routine activities are likely to be a part of the mechanism by which unemployment causes crime. However, the missing association between unemployment and nonproperty crime suggests that these other mechanisms may not apply across all crime types either. Selection processes (or unobserved time-varying factors) largely explain the association between unemployment and nonproperty crime, as within-individual change in unemployment explains only a small fraction of variation in nonproperty crime when stable heterogeneity is taken into account.

The protective effect of active labor market programs discovered in this study indicates that programs that actively incorporate young adults into the labor market are promising in reducing crime, suggesting that attaching control and activation to unemployment benefits might be better than simply giving out money with no conditions attached. Future studies should examine the effects of different kinds of active labor market programs on crime. Of course, it might be that the requirement of participation in such programs is one reason why several people choose not to apply for benefits, which might, in turn, create incentives for income-generating crime. Despite this possibility, existing empirical evidence backed up with insights from social control and routine activity theories indicates that active labor market programs could be one way to reduce unemployment-related youth crime (Andersen, 2012; Fallesen et al., 2012). It has been suggested that in advanced welfare state regimes, "pure" income transfers lose their marginal utility in reducing crime at some point by creating a control deficit (Kivivuori and Lehti, 2006). Although the current research cannot prove the "control deficit" hypothesis, the findings are consistent with the idea

that to reduce crime, decommodification of life chances is best served with a modicum of social control.

Regarding theoretical work hypothesizing a causal effect of employment on crime, the generality of the association should be considered. What is more, existing individual-level evidence suggests the effect of unemployment on crime is likely to be conditional on preexisting characteristics (Farrington et al., 1986), thus implying an interaction effect between criminal propensity and unemployment. Future research should investigate whether the aggregate-level association between unemployment and crime stems only from marginal changes in demand for labor among high-risk individuals or whether a more general effect exists that would apply across the whole distribution of criminal propensity. The desistance perspective has dominated theoretical work on employment and crime in life-course criminology: The current discussion concerns primarily employment and its effect on desistance among high-risk individuals, whereas considerations in the general population have taken a back seat. Instead of understanding unemployment as a fundamental causal factor that pushes individuals to criminal careers, the current view is that work is something that potentially saves a career criminal. To understand whether both processes exist, it would be interesting to determine 1) whether unemployment affects only the crime of individuals who have criminal backgrounds or high criminal propensities (Ousey and Wilcox, 2007), or 2) can (sometimes truly exogenous) adverse life events also affect individuals without such predispositions? Existing individual-level research that has used high-risk samples generally supports the former view (Uggen, 2000; van der Geest, Bijleveld, and Blokland, 2011); the importance of the latter process remains an open question.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Figure S1. Income Quintile in 2004 by Number of Months Unemployed in 2004