

NATIONAL RESEARCH INSTITUTE OF LEGAL POLICY
RESEARCH REPORT NO. 263

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SOCIOECONOMIC DIFFERENCES IN CRIME AND VICTIMIZATION
A Register-Based Study

Helsinki 2013

ISBN 978-951-704-414-1
ISSN 1797-562X
HAKAPAINO OY, HELSINKI 2013

Department of Social Research
University of Helsinki
Helsinki

Socioeconomic Differences in Crime and Victimization
A Register-Based Study

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ACADEMIC DISSERTATION

To be presented, with the permission of the Faculty of Social Sciences of the University of Helsinki, for public examination in the Auditorium of Arppeanum (Snellmaninkatu 3, 2nd floor) on 27 April 2013, at 10 am.

Helsinki 2013

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ACKNOWLEDGEMENTS

My decision to embark on doctoral studies was far from clear-cut. After much hesitation, I started working on this dissertation in late 2008 with the promising Risk Factors of Crime in Finland (RFCF) -dataset that had just been collected by the National Research Institute of Legal Policy. This turned out to be one of the best decisions I have ever made. From the start, this project has been a joint effort, and I have received considerable help and support during these four and half years from many colleagues, friends and family. I hope to include you all in the following.

While officially graduating in sociology, I feel my dissertation is better described as a mixture of criminology and demography. This mixture reflects the academic background of my two excellent supervisors. Amassing a wide knowledge of criminological theory and research, the contribution of research director *Janne Kivivuori* was instrumental in shaping the main research questions of this thesis. He has greatly influenced my thinking about causes of crime and the need for multidisciplinary research to best understand criminal behavior. His example has taught me to always aim for objective empirical research, no matter how difficult it may at times be. I was very fortunate to have professor *Pekka Martikainen* as my other supervisor. I met him for the first time after sending my research plan to him in late 2008, and he took a gamble with me in becoming my supervisor without knowing much about me. Despite being a demographer rather than a criminologist by trade, he has given insightful feedback on every manuscript along the way, and always found time to read and comment on my texts and analyses in the middle of his busy schedule.

Two researchers who have made an invaluable contribution to my career as a researcher and to this particular research project, respectively, deserve to be mentioned. *Jukka Savolainen*, currently an associate professor of criminology at University of Nebraska, is the reason why I ended up studying crime. We met when he was teaching a practical research course during my master's studies, and during this course I become interested in criminology. Researcher *Ville Hinkkanen* of National Research Institute of Legal Policy, on the other hand, needs to be praised for his contribution not only to this project, but also for making register-based criminological research possible in our institute. The RFCF-project was originally his initiative, and without Ville's persistence and legal expertise we would not have the access to the data we have now. Ville has also provided me expert advice in all things concerning statistical methodology.

Professor *Catrien Bijleveld* and assistant professor *Torkild Hovde Lyngstad*, two researchers whose work I admire, reviewed the thesis. The feedback received from them was very helpful and encouraging. I would also like to thank all the anonymous reviewers in the four substudies. I was delighted to hear that professor *Paul Nieuwbeerta* accepted the invitation to act as the opponent in the defense of my thesis. It is an honour to have one of the leading researchers in life-course criminology as my opponent.

I have worked at the National Research Institute of Legal Policy since 2006. It has been a great environment to do research on crime, as one is surrounded by researchers working both in law and social science. I wish to thank all the colleagues who I have worked with over the years. I am grateful to director *Tapio Lappi-Seppälä* for providing me the opportunity to work on my thesis side-by-side with my other duties as planning officer. My boss *Kaijus Ervasti* has ensured that I always had time for my research. Senior researchers *Venla Salmi* and *Reino Sirén*, who both co-authored one substudy in the thesis, deserve special thanks. The countless discussions I have had with both of them on criminological theory and statistical methodology have shaped this thesis in many ways. My good friends *Sirpa Turunen* and *Petri Danielsson* have helped me enormously with my administrative duties. Also, I would like to thank *Eira Mykkänen* for making the layout of this thesis suitable for publication.

The other important collective which I was fortunate to belong to was the Monday seminar of the Population, Health and Living Conditions (VTE) doctoral program at the University of Helsinki. The steering group of docent *Ossi Rahkonen*, professor *Eero Lahelma*, professor *Pekka Martikainen* and docent *Ari Haukkala*, as well as current and prior coordinators *Netti Mäki*, *Hanna Konttinen* and *Elina Mäenpää* are responsible for maintaining the high standards in these meetings. I want to thank all the participants of these seminars for providing very helpful feedback on my manuscripts. Especially the post-seminar sessions at Oluthuone Kaisla saved many bleak Mondays!

An important part of this research was conducted during the six months I spent as a Fulbright visiting scholar at Population Studies Center of University of Pennsylvania. Only after arriving to Philadelphia it occurred to me that the now classic cohort study (“Delinquency in a Birth Cohort”) by Wolfgang, Figlio and Sellin was conducted at UPenn. During our stay in Philadelphia, I was able to solely focus on my own research in one of the top universities in the US. I owe gratitude to professors *Irma Elo* and *John MacDonald* for making the visit possible (and once again to *Pekka* for making the initial contact). I would like to thank the Finnish Fulbright Center for granting me the scholarship and helping us with several practical

matters during the six months. Also, the meetings with other Nordic criminologists working with register-based data have been important occasions to discuss the possibilities of these data and compare ideas. Special thanks go to *Torbjørn Skardhamar*, *Felipe Estrada* and *Anders Nilsson*, and to everybody else who attended these meetings.

To name just a few, *Timo*, *Tomi*, *Teemu*, *Olli-Pekka*, *Elina*, *Heta*, *Lasse*, *Lotta* and *Rolle* have remained great friends to me despite my endless ramblings about this thesis. While I have firmly sat in the ivory tower, *Ville* and *Jussi* have been my “eyes and ears on the street”. Discussions with both of you on the reality of crime (and some football) have been invaluable. *Antti* and *Birgitta*, my parents-in-law, deserve thanks for providing us a place to live in during the renovation of our own apartment. Without your help, our excursion to Philadelphia would not have been possible.

Finally, I want to thank my family. My parents *Mailis* and *Keijo* have always believed in me and supported me through thick and thin. The same applies to my sister *Katri* and my brother *Olli-Pekka*. While completing this thesis is a major landmark for me, much more important things have happened since I started this project. Our daughters *Emma* and *Sara* were born, and especially the impending birth of the younger one acted as a needed incentive to get this thesis done. Whatever I write here about the support my wife *Heidi* has given me during these years feels like an understatement. I would not be here without you.

Helsinki 8.3.2013

Mikko Aaltonen

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ABSTRACT

The association between socioeconomic status (SES) and crime is one of the central themes in criminology. While empirical studies on this issue have given mixed results, strong belief in an inverse association between SES and crime underlies Finnish criminal policy, where social policy is seen as an integral part of crime prevention. However, no proper population-based analysis of socioeconomic differences in crime in Finland is available. Using a register-based sample of the general population, the current study focused on socioeconomic differences in crime and violent victimization among young adults aged 19–30.

The main result of Substudy I was that the bivariate associations between the four measures of SES – education, income, occupation-based social class, and unemployment history – and violent crime, property crime and driving while intoxicated (DWI) are strong, education being the strongest predictor. Using a more advanced within-individual longitudinal design to account for selection processes, Substudy II focused on the temporal association between unemployment and crime. In this design, violent crime and DWI were not longer associated with unemployment, meaning that the crime rates of the same individuals did not vary by current unemployment status. Property crime rate, however, was higher during periods of unemployment.

Substudies III and IV focused on violent offending and victimization. Examining police-reported male violence, Substudy III showed that low SES and prior criminality were stronger predictors of violence in private places, against both men and women, than violence in public places. Men with low SES were also more likely to be suspected of intimate partner violence. Substudy IV compared socioeconomic differences in violent victimization in register-based data and survey data, finding that socioeconomic differences in both datasets were highly sensitive to the seriousness of the violence measured: the more serious the measured violence, the greater the differences were.

In sum, the results show that crime committed by young adults in Finland is heavily concentrated in the lower social strata, with victims of serious violence often in similarly poor positions. While the existence of an inverse SES-crime association is not surprising, the scale of these differences is: the most obvious methodological reason to this is the high-quality register data that actually captures people from all strata. However, it is yet unclear how much different selection mechanisms contribute to these associations. In order to find ways to reduce social exclusion and crime, earlier

processes related to both individual traits and difficult environments that push individuals to paths of cumulative disadvantage need to be better understood. The effects of different social policy reforms on crime should also be analyzed.

Keywords: crime, victimization, socioeconomic status, register-based research

TIIVISTELMÄ

Sosioekonomisen aseman ja rikollisuuden välinen yhteys on kriminologian historian tutkituimpia teemoja. Osittain ristiriitaisesta tutkimusnäytöstä huolimatta oletus käänteisestä yhteydestä on suomalaisen, sosiaalipolitiikan tärkeyttä korostavan kriminaalipolitiikan kantavia ajatuksia. Uutta kottimaista tutkimusnäyttöä näistä teemoista ei kuitenkaan ole ollut saatavilla. Tämän tutkimuksen tarkoituksena oli tutkia sosioekonomisia eroja rikollisuudessa ja väkivallan uhriksi joutumisessa erityisesti 19–30-vuotiaiden nuorten aikuisten parissa laajan rekisteriaineiston avulla.

Ensimmäisen osatyön tulokset osoittivat, että kaikki neljä sosioekonomisen aseman mittaria – koulutus, tulot, ammattiin perustuva sosiaaliluokka ja työttömyyshistoria – ovat yksilötasolla vahvassa yhteydessä väkivalta- ja omaisuusrikoksiin että rattijuopumukseen. Koulutusryhmittäiset erot olivat erityisen suuria. Ainoastaan työttömyyden ja rikollisuuden yhteyttä pitkittäisasetelmassa tarkastelleen toisen osatyön tulokset puolestaan viittaavat siihen, että työttömyyden vaihtelulla on samaa yksilöä eri elämäntilanteissa tarkasteltaessa yhteys ainoastaan omaisuusrikollisuuteen, ei väkivaltaan tai rattijuopumukseen.

Osatyöt III ja IV tarkastelivat väkivallan tekijöitä ja uhreja. Rikoksesta epäillyn miehen matala sosioekonominen asema ja aiempi rikostausta olivat voimakkaammassa yhteydessä yksityisellä kuin julkisella paikalla tapahtuneeseen, poliisin tietoon tulleeseen väkivaltaan, riippumatta uhrin sukupuolesta. Matala sosioekonominen asema oli yhteydessä myös perheväkivaltaan. Viimeisessä osatyössä tarkasteltiin väkivallan uhriksi joutumisen todennäköisyyttä sosioekonomisen aseman mukaan sekä kysely- että rekisteriaineistossa. Molemmat aineistot osoittavat, että havaitut sosioekonomiset erot ovat voimakkaasti kytköksissä siihen, miten vakavaa väkivaltaa mitataan. Mitä vakavampi väkivalta on kyseessä, sitä huono-osaisempia uhrit keskimäärin ovat.

Tutkimuksen yhteenvetona voi todeta, että nuoren aikuisen matala sosioekonominen asema on voimakkaassa yhteydessä rikoskäyttäytymiseen. Vakavan väkivallan uhrit kuuluvat usein tähän samaan ryhmään. Vaikka sosioekonomisen aseman ja rikoskäyttäytymisen yhteys on oletusten mukainen, suhteelliset erot ryhmien välillä ovat lopulta yllättävän suuret. Yhtenä syynä tähän lienee se, että rekisteriaineistot tavoittavat hyvin kaikki väestöryhmät, ja myös vakavimmin syrjäytyneet henkilöt ovat mukana. Sosioekonomisten erojen olemassaolo ei kuitenkaan kerro sitä, kuinka paljon erilaiset valikoitumisprosessit vaikuttavat niiden taustalla. Jotta syrjäytymistä ja rikollisuutta voitaisiin parhaiten ehkäistä, tällaista kasaantunutta

huono-osaisuutta aiheuttavat yksilö- ja ympäristötekijät, sekä niiden yhdysvaikutukset, tulisi tuntea nykyistä paremmin. Sosiaalipoliittisten uudistusten rikosvaikutusten arviointi olisi myös hyödyllistä.

ABBREVIATIONS

| | |
|-------|--|
| AL | Adolescence-limited offender |
| ALMP | Active labour market policy |
| BCS | British Crime Survey |
| CI | Confidence interval |
| DWI | Driving while intoxicated |
| FE | Fixed-effects (regression model) |
| FNVS | Finnish National Victimization Survey (<i>Kansallinen uhritutkimus</i>) |
| HR | Hazard ratio |
| ICVS | International Crime Victimization Survey |
| IPV | Intimate partner violence |
| KELA | Social Insurance Institution of Finland (<i>Kansaneläkelaitos</i>) |
| LCP | Life-course persistent offender |
| NCVS | National Crime Victimization Survey |
| NRILP | National Research Institute of Legal Policy (<i>Oikeuspoliittinen tutkimuslaitos</i>) |
| OR | Odds ratio |
| RFCF | Risk Factors of Crime in Finland – dataset (<i>Suomalaisen rikoskäyttyymisen riskitekijät – aineisto</i>) |
| SE | Standard error |
| SES | Socioeconomic status |

LIST OF ORIGINAL PUBLICATIONS

This thesis is based on the following publications:

- I Aaltonen, Mikko & Kivivuori, Janne & Martikainen, Pekka. 2011. Social Determinants of Crime in a Welfare State: Do They Still Matter? *Acta Sociologica* 54(2): 119–138
- II Aaltonen, Mikko & MacDonald, John & Martikainen, Pekka & Kivivuori, Janne. 2013. Examining the Generality of the Unemployment-Crime Association. Forthcoming in *Criminology*
- III Aaltonen, Mikko & Kivivuori, Janne & Martikainen, Pekka & Salmi, Venla. 2012. Socioeconomic status and criminality as predictors of male violence: does victim's gender or place of occurrence matter? *British Journal of Criminology* 52(6): 1192–1211
- IV Aaltonen, Mikko & Kivivuori, Janne & Martikainen, Pekka & Sirén, Reino. 2012. Socioeconomic differences in violent victimization: Exploring the impact of data source and the inclusivity of the violence concept. *European Journal of Criminology* 9(6): 567–583

1 INTRODUCTION

The individual-level association between varying measures of socioeconomic status (SES) and criminal behavior has been a central theme in sociological criminology since the advent of the discipline (Vold et al. 1998). Similarly, the belief that social exclusion causes crime has been a central tenet of Finnish criminal policy, and the old quote that “good social policy is the best criminal policy” (see Anttila 1952) still describes the Nordic model of crime prevention, especially when contrasted with the Anglo-American world (Lappi-Seppälä & Tonry 2011). Despite the abundance of empirical research on the SES-crime link (Ellis & McDonald 2001), there are at least three factors that call for another such study in Finland.

First, since the heavy economic recession of the 1990s, Finland has witnessed both growing income differences (OECD 2011) and permanent long-term unemployment (Ministry of Employment and the Economy 2012; Myrskylä 2011). Social exclusion among youth, in particular, is increasingly perceived as a key societal concern; the latest Internal Security Programme lists social exclusion as one of the major threats to general security in Finland (Ministry of the Interior 2012). In the National Programme for Reducing Violence (National Council for Crime Prevention 2005), preventing social exclusion is highlighted as a critical component of violence reduction. However, there is a need for further research on the effects of social exclusion on crime, as studies that would systematically assess the distribution of crime in Finland using several indicators of SES surprisingly do not exist.

Second, a closer look at recent Finnish empirical research on crime reveals the small size of criminology as an academic discipline in Finland. Within quantitative criminology, the best-covered topics are the determinants of juvenile delinquency (Kivivuori & Bernburg 2011) and homicide (Kivivuori & Lehti 2012). Victimization surveys are the only nationally representative surveys that capture people of different ages. While some cohort studies¹ have also examined determinants of crime, the primary focus of those multidisciplinary longitudinal studies lies elsewhere. Thus, we seem to be lacking a proper description of the distribution of crime within the population in terms of socioeconomic and demographic factors, as well

¹ Notable prospective cohort studies with an interest in crime are the Jyväskylä Longitudinal Study of Personality and Social Development (1959) and the Northern Finland Birth Cohort (1966 & 1985–86) studies, as well as the *From a Boy to a Man* (1981) study (Elonheimo 2010). A promising new data source is the entirely register-based 1987 Finnish Birth Cohort (Paananen & Gissler 2011).

as longitudinal studies on criminal careers in general. Given the dominance of U.S.-based research in the field, the best evidence on determinants of crime comes from a society markedly different from Nordic welfare states. Whether all this evidence is useful for extrapolation to Finnish context is a matter of debate. It could be argued that the study of social determinants of crime should be particularly sensitive to macro-level conditions.

Third, the data sources successfully used in Finnish demographic research on socioeconomic differences in mortality and other health outcomes (see Laaksonen & Silventoinen 2011) have not been properly employed in criminological research. Nordic administrative registers provide a good, and so far largely untapped, means to study crime (Lyngstad & Skardhamar 2011). These data are exceptionally good for studying socioeconomic differences, as they include reliable measurements on many aspects of socioeconomic attainment (education, income, occupation, unemployment, and so forth). Perhaps more importantly, samples drawn from the Population Information System are not biased by selective nonresponse like most surveys; thus, these data include the most marginalized individuals. When such a dataset incorporates information about criminal behavior from both police and court records, we can build relatively inexpensive longitudinal datasets with large samples required for a population-based study of crime. These data can contribute to our understanding of the causes and consequences of criminal behavior.

2 THEORETICAL FRAMEWORK

When compared to those who study social mobility (Wright 2005) or health inequality (Mackenbach 2012), criminologists have arguably been less keen to develop the concept of SES systematically (Farnworth et al. 1994). The current study uses socioeconomic status as the general term under which concrete ways to measure it are organized. As mentioned, the measures used here are education, occupational social class, income, and unemployment duration. Simply put, the typical way to think about the interdependencies among these measures is to say that the effect of education on income is mediated by occupation (or a lack of one). However, education and occupational social class do not only operate via income, as they can also have direct effects on different outcomes (Lahelma et al. 2004). The current analysis is exploratory in the sense that no *a priori* choices regarding the superiority of one measure over the other were made beforehand.

To sufficiently cover the history of SES as a component of criminological theory would require a separate dissertation. Most classical theories of crime incorporate SES into their frameworks, albeit to varying extent (Tittle 1983), and discussions of poverty causing crime reach way beyond the advent of criminology as an independent academic discipline (Vold et al. 1998). Some contributions must be left out because of the abundance of material, so theory is discussed under three headings that describe the works most relevant to this study. First, the most influential classical theories that include SES as a critical determinant of crime are reviewed. Second, SES is located within the framework of life-course criminology, arguably the leading research paradigm in 21st-century approaches. Third, the proposed mechanisms through which low SES increases the risk of violent victimization are discussed.

2.1 SES in Classical Theories of Crime

According to Vold et al. (1998, 108), probably the oldest of the theories “with a non-individual orientation are those that explain criminal behavior in terms of economic differences or influence”. Explaining why those individuals in lower social classes committed a disproportionate share of crime and why crime was more prevalent in poor areas occupied a major place in criminological theory for most of the 20th century.

The influential formulation of strain theory by Robert K. Merton (1938; 1968) sees inequality of opportunity as the driving force behind crime.

Merton argues that in Western societies, cultural goals of affluence and wealth are universal, but the opportunities to reach such successes are unequal. When faced with a situation in which cultural goals cannot be met through institutionalized means because of structural inequality, people adapt in different ways. Although most individuals across social strata accept legal means as the way to achieve success, some individuals, Merton's "innovators," resort to illegal methods to get money (Merton 1968). This imbalance between goals and means (Messner & Rosenfeld 2006) creates the inverse association between SES and crime (Vold et al. 1998).

Focusing on neighborhood-level variation in urban areas, the social disorganization theory by Shaw and McKay (1942) offers a different explanation to why the poorest areas have the highest crime rates. Building on the work by the Chicago School of Human Ecology (Vold et al. 1998), Shaw and McKay argued that social disorganization, understood as weak social bonds and decreased social control, found particularly in poor transitional urban areas, causes crime. Importantly, higher crime in poor areas cannot be traced back to characteristics of individuals residing in the area, but the characteristics of the areas themselves affect crime. In addition, high-crime neighborhoods provide more opportunities to commit crime and also to learn ways of committing crime from experienced offenders (Vold et al. 1998).

While strain and social disorganization were originally two separate theoretical developments (Cloward 1959), attempts have been made to combine them. Cloward (1959) argued that in addition to worse access to legitimate means of achieving success, low SES groups had greater access to illegitimate options in their neighborhoods. What is more, although such areas suffered from social disorganization as defined by Shaw and McKay, lower-class areas were at the same time "organized in terms. . . of criminal values" (Cloward 1959, 170). In line with cultural deviance theories, Shaw and McKay argued that the greater exposure to adult criminal structures provides people with the means of learning both values conducive to crime and concrete skills for committing crime. Thus, Cloward combined differential opportunity structures, comprising both "appropriate learning environments" and "opportunities to discharge to role once learned" (Cloward 1959, 168), to Merton's notion of strain.

Despite borrowing elements from social disorganization and learning theories, subsequent subcultural theories by Cohen (1955) and Cloward and Ohlin (1960) are usually seen as extensions of strain theory. If Merton's original contribution focused on monetary success, Cohen argued that pursuit of social status among peers could also motivate crime among lower-class individuals. Furthermore, he saw gang delinquency as rebellion

toward middle-class values, and the delinquent subculture as “a way of dealing with the problems of adjustment” (Cohen 1955, 121). Thus, the inability of lower-class youth to reach middle-class positions results in the creation of a new value system, the delinquent subculture. As a further development, Cloward and Ohlin (1960) argued that those youth that do not seek middle-class status as a way of life, but only the monetary success associated with it (“crass materialism,” Cloward & Ohlin 1960, 97) are the most serious delinquents (Vold et al. 1988).

The classic strain theories by Merton, Cohen, and Cloward and Ohlin dominated criminology in the 1950s and 1960s (Agnew & Brezina 2010), but this prominence did not last long. In the U.S., Large-scale anti-poverty programs informed by these theories ultimately ended after political opposition in the 1970s (Vold et al. 1988, 169); the theory itself also came under scrutiny (Kornhauser 1978; see also Vold et al. 1988). Social control theory was one of the competing theories that gained ground (Bernard 1984). Unlike social disorganization theory, which accepted an empirical SES-crime link but sought to explain it with social bonds, Travis Hirschi’s social control theory (1969) was explicitly critical of class-based strain theory as “inadequate and misleading” (Hirschi 1969, 10), especially in its failure to explain middle-class crime. Instead, Hirschi states that a “delinquent act results when an individual’s bond to society is weak or broken” (1969, 16). Social control theory does not question the motivation to commit crimes, but claims that variation in social control (comprising attachment to meaningful others, rational commitment to society, involvement in conventional activities, and belief that rules of society should be obeyed) explains variation in crime (Hirschi 1969, 16–26; Paternoster & Bachman 2010).

Despite the fact that Hirschi was skeptical of strain theory, lack of social control can yet explain why employment and crime might be connected (Savolainen 2010). Partly, this argument resembles the one made in routine activity theory by Cohen and Felson (1979) about variations in criminal opportunity. According to routine activity theory, crime results when motivated offenders meet suitable targets in the absence of capable guardians. Employment structures temporal patterns in daily life and should generally reduce the time that potential offenders have available for “unstructured socializing with peers” (Osgood et al. 1996; van der Geest et al. 2011) and illegal activities. In addition to modifying routine activities, commitment to work can render crime a less attractive option, as those individuals with jobs have more to lose, “a stake in conformity” in Jackson Toby’s (1957) terms, if they take part in crime (Uggen & Wakefield 2008; Paternoster & Bachman 2010). Furthermore, increased social interaction with others at work increases informal social control (Sampson & Laub

1993). Thus, the theorized crime-reducing effect of work is attributable to both increased social control over potential offenders and the creation of pro-social bonds (Uggen & Wakefield 2008).

Similarly, economic choice theory – traced back to economists Becker (1968) and Ehrlich (1973) – that views employment and crime as strongly connected also considers the effect of allocation of time on crime. The theory builds on rational choice (Albertson & Fox 2012; Cornish & Clarke 1986) and the trade-off between legal and illegal activities. According to Ehrlich (1973), potential offenders can engage in both work and crime, and the time devoted to each is decided on the basis of expected return (income). The individual decides to “engage in illegal activities if the marginal gain from illegality exceeds the marginal gain from legal activities” (Albertson & Fox 2012, 49). If net return (return after costs, i.e., risk of punishment) from work decreases, or the net return from illegal activities increases, an individual is expected to dedicate more time to criminal activities (Albertson & Fox 2012). According to the theory, an increase in the unemployment rate should lower the opportunity cost associated with crime, which should in turn increase crime. The theory also anticipates that the deterrent effect of punishment is stronger for those who are working, as getting arrested for a crime might result in a loss of job (Uggen & Wakefield 2008).

Strain theory lost its standing as the dominant explanation for crime during the 1960s, and competing explanations, especially for the more specific employment-crime link, have since appeared. However, after the theoretical (Kornhauser 1978) and methodological (Tittle et al. 1978) critiques, strain theory has subsequently seen a transformation (Cullen 1984; Vold et al. 1998) into micro- and macro-level variants that have been developed separately by different authors. General strain theory by Robert Agnew (1992) is an individual-level theory, written at a “social-psychological level” (Agnew 1992, 48) and incorporating insights from psychological research and frustration-aggression theory into strain tradition. Agnew argued that the theories by Merton (1938), Cohen (1955), and Cloward and Ohlin (1960) had a too-narrow focus on the type of strain resulting when lower-class individuals are prevented from reaching monetary success or middle class status (Agnew 1992, 50). Focusing on the interaction between individual and social environment, Agnew’s version outlines three major sources of strain: prevention from achieving positively valued goals, threatened removal of positively valued stimuli, or threatened presentation with noxious or negatively valued stimuli (Agnew 1992, 50).

Institutional-anomie theory (Rosenfeld & Messner 1994), the macro-level extension of strain theory, retains Merton’s argument that goals-

means imbalance in the culture – the “American dream” in this case – is key to understanding the relationship between inequality and crime. Taking Merton’s thesis further, they argue that American culture places an emphasis on materialism and individualism, and people are judged on the basis of their achievements rather than by who they are and “Everyone is encouraged to succeed” (Messner & Rosenfeld 2006, 129). This success is measured in economic terms, and the open competition for riches generates cultural pressure (Vold et al. 1998). At the same time, the American dream does not prohibit – at least not strongly enough – people from using illegal means to achieve those goals. However, they depart from Merton in the sense that they do not emphasize social structure, but rather the dominance of the economy over other social institutions, where the “mentality of the marketplace penetrates into the non-economic realms of social life” (Messner & Rosenfeld 2006, 129). Finally, building on the concept of relative deprivation rather than strain, Young (1999) has written about the tension between cultural inclusion and structural exclusion, and that even in the absence of absolute deprivation and poverty, perceptions of injustice and disadvantage can cause crime (Young 2006). With discontent resulting from group comparisons, rather than universal goals-means imbalance, as the mechanism, Young argues that people in a social group can resort to crime if they subjectively feel unfairly treated and that they deserve rewards similar to others (Young 2006; Grover 2008).

2.2 SES, Crime, and Life Course

Explaining the causes of continuity and change in criminal behavior over an individual’s life course has become one of the major challenges for criminological research and theory (DeLisi & Piquero 2011). Although what is now called the *criminal career paradigm* – often credited to the seminal Philadelphia cohort study by Wolfgang et al. (1972) and the National Academy of Sciences report by Blumstein et al. (1986) – was initially met with some opposition (Gottfredson & Hirschi 1986; 1987), it has since become a dominant framework in quantitative criminology (Piquero et al. 2003). This development has obviously been paralleled with, and probably partly caused by, the increase in the number of high-quality longitudinal data and improvements in the statistical methods to analyze these data. The key dimensions of crime in this paradigm are “participation, frequency, specialization, escalation, career length, and desistance” (DeLisi & Piquero 2011, 289). One central point of contention has been the etiology of the so-called age-crime curve (Figure 1). Three theoretical contributions

from 1990–1993 were instrumental in shaping this paradigm further: self-control theory by Michael Gottfredson and Travis Hirschi (1990), developmental taxonomy by Terrie Moffitt (1993), and the age-graded theory of informal social control by Robert Sampson and John Laub (1993; 2003).

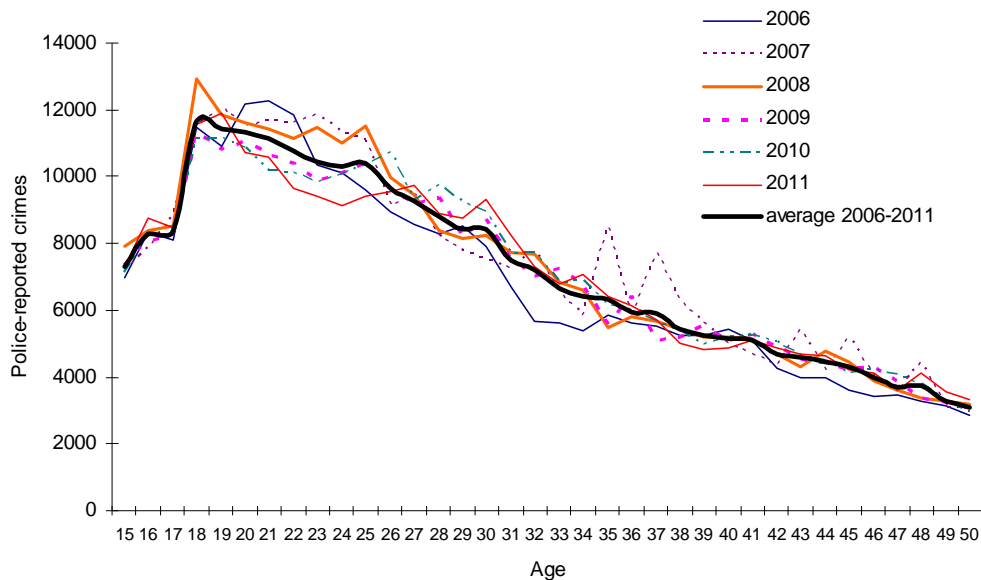


Figure 1 Age-crime curve in Finland. Number of suspects by age in police-reported crime (all solved cases), 2006–2011 (Source: Statistics Finland)

Before investigating what role SES plays in each of these three theories, two concepts need to be introduced to highlight the differences among them. Inspired by economist James Heckman (1981), Nagin and Paternoster (1991; 2000) introduced the concepts of population heterogeneity and state dependence to criminologists, originally as two possible explanations of why past criminal behavior tends to be the best predictor of future criminality. Importantly, they claim that criminological theories can be grouped under these two concepts (Nagin and Paternoster 1991).

Simply put, the population heterogeneity perspective suggests that behavioral continuity is caused by time-stable individual traits. In criminological literature, the terms criminal propensity or criminality are commonly used to refer to stable, between-individual differences. While this propensity is in practice always partly unmeasured, the theories favoring population heterogeneity explanations usually emphasize “enduring personal characteristics” (Wilson & Herrnstein 1985), and argue that differences in psychological characteristics persist over time (Nagin & Paternoster 1991). The alternative view, state dependence, asserts that past acts exert a “genuine behavioral influence” (Nagin & Paternoster 1991, 167) on the probability of future acts. While this formulation of state dependence

explains continuity in offending actions, it can accommodate change as well: “Noncriminal behavior such as acquiring satisfying employment. . . can have the consequence of decreasing the probability of offending” (Nagin & Paternoster 2000, 118). According to Laub and Sampson (2003), these arguments are better understood as theories of *kinds of people* or *kinds of contexts*. Of the three theories introduced above, self-control theory emphasizes kinds of people, whereas the other two incorporate both processes. Moffitt’s theory can be seen as leaning toward the population heterogeneity argument, Sampson and Laub’s toward state dependence, in that it views social context as important during all stages of life.

Interestingly, the theory from the most vocal opponents (Gottfredson & Hirschi 1987) of the criminal career paradigm has ended up playing an essential role in shaping life-course research on crime. The general theory of crime by Gottfredson and Hirschi (1990), which views low self-control² as the key determinant of criminal behavior over the life-course, has persistent population heterogeneity at the heart of its argument. According to this theory, low self-control is a stable individual trait that is established early in life, mainly as a result of poor parental supervision and inconsistent upbringing. Those with low self-control are impulsive, shortsighted risk-takers who favor acts of “instant gratification.” They argue that low self-control manifests itself in different forms of “analogous behaviors” such as crime, smoking, drug use, and inability to hold a job during the individual’s life course. Following this logic, low SES and crime in adulthood are actually caused by same stable trait, and there is no causal association between the two. Furthermore, they are critical of the sociological tradition of narrowly focusing on the SES-crime link, and write that social class is so complex a construct that an empirical relationship between SES and crime could at best “only serve as a starting point for theoretical speculation” (Gottfredson & Hirschi 1990, 80). On a more general level, one could say that self-control theory presents an antithesis to theories that seek to explain crime by proximate life events and life circumstances in adulthood.³

Given that explanations stressing the importance of social structure had been the bread and butter of classic sociological criminology, self-control

² Although within criminological literature the concept of self-control is typically traced to Gottfredson and Hirschi (1990), similar constructs have been studied under several other disciplines: Moffitt et al. (2011: 1) state that “interest in self-control unites all the social and behavioral sciences”.

³ While the original formulation by Gottfredson and Hirschi (1990) views low self-control as a stable trait caused by early environmental factors as opposed to genetic factors, recent studies suggest that self-control is both malleable (Moffitt et al. 2011) to some extent and influenced by genetic factors (Boisvert et al. 2012).

theory posed a serious challenge in claiming that a selection process explains the association between adult SES and crime. Within the life-course framework, the work by Sampson and Laub (1993; 2003) has probably been the most convincing in defending the role of social context. While they do not deny the role of stable individual traits in explaining variations in crime, they reject “the idea of determinism and lawful predictability from childhood factors” (Laub & Sampson 2003, 34), as they do not sufficiently explain long-term life-course outcomes. While they agree that criminal propensity is related to differences in stable individual traits, they argue that criminal propensity is neither time-stable nor only related to biological or psychological factors, but inevitably “a black box” that results from a variety of individual, situational, and community factors (Laub & Sampson 2003, 23).

Unlike Gottfredson and Hirschi, Sampson and Laub see employment, a key component of adulthood SES, as a critical variable in crime causation, especially in explaining desistance from crime. However, for them the causal effects are not attributable to economic situation or material resources (they explicitly criticize theories that claim social class or poverty are the only factors needed to explain crime) but rather to differences in informal social control, social bonds, and routine activities (Laub & Sampson 2003). In a nutshell, the key argument in their age-graded theory of informal social control is that social bonds in both adolescence (family, school, and peers) and adulthood (e.g., work and marriage) affect crime regardless of prior differences in criminal propensity, and that the effect of early individual-level risk factors on crime is mediated by failures in attachment to key institutions of informal and formal social control, particularly during the transition to adulthood (Sampson & Laub 1997, 10). Their concept of cumulative disadvantage (Sampson & Laub 1997) describes this mediation process. Adapting elements from labeling theory (Lemert 1967; Becker 1963), they theorize a state-dependent explanation for why crime in youth explains crime in adulthood. Delinquent behavior in adolescence, especially the official sanctions that follow, causes failure in school and weakens chances to find work. These events weaken adult-life social bonds, which in turn increases the probability of future crime (Sampson & Laub 1997, 144). Instead of persistent heterogeneity, cumulative disadvantage describes a dynamic “snowball effect,” where adolescent delinquency and its consequences “increasingly ‘mortgage’ one’s future” (Sampson & Laub, 147; Nilsson & Estrada 2011). Furthermore, Sampson and Laub (1997) expect the labeling effect to be greatest among children from lower classes.

Despite the fact that Sampson and Laub's theory contrasts with the population heterogeneity perspective, they have also criticized the concept of state dependence for focusing too much on explaining continuity in offending. According to them, a simplistic debate between kinds of people and kinds of contexts does not acknowledge the idea of behavioral change (Laub & Sampson 2003, 277). According to Sampson and Laub, changes in criminal behavior cannot be fully understood by looking at either stable individual traits or past events. They stress that some major turning points in individuals' life courses result from exogenous random events and macro-level shocks, "beyond the pale of individual choice" (Laub & Sampson 2003, 34).

If Gottfredson and Hirschi's view of the antecedents of crime is rather deterministic in contrast to Sampson and Laub's, who emphasize the possibility of change due to exogenous life events and social bonds, Moffitt's developmental taxonomy of antisocial behavior (1993) could be seen as taking a middle ground between the two. Instead of assuming continuously distributed criminal propensity, the theory proposes two distinct types of offenders called adolescence-limited (AL) and life-course persistent (LCP) offenders. Thus, in contrast to "general" theories that describe a uniform causal process for all offenders (Skardhamar 2009), where criminal propensity varies in degree as opposed to kind, Moffitt's theory offers two separate causal processes to explain variation in crime over an individual's life course. Whereas AL offenders have a short criminal career limited to the teenage years, LCP offenders start their criminal careers early and persist at least until young adulthood (Moffitt 1993; Farrington 2010).

According to Moffitt (1993), a peak in offending during adolescence is the hallmark of the AL group. Antisocial behavior is here explained by a "maturity gap" and peer influence, and has thus largely social origins. The maturity gap refers to the difference between biological and social maturation: the dissatisfaction with being restricted to the social role of child and denied the freedom and rewards of adult life causes youth to rebel against parents and other adults, and Moffitt claims that it is almost normative for a youth to find delinquent behaviour appealing during this stage. Adolescents mimic the delinquent lifestyles of their LCP peers to demonstrate independence from their parents and to gain social acceptance. However, most AL offenders desist from crime once they reach their legitimate adult roles. Even though most AL offenders age out of crime as a consequence of maturation, desistance may be delayed for those adolescents who face "snares," such as criminal records, uncompleted education, and substance abuse, which "narrow the options for conventional behaviour" (Moffitt 1993, 691; Moffitt 2006).

Continuity in offending over time, on the other hand, is mostly due to the LCP group. The origins of the LCP type appear early in life, “when the difficult behaviour of a high-risk young child is exacerbated by a high-risk social environment” (Moffitt 2006, 278). Both inherited and acquired neuropsychological variation, “initially manifested as subtle cognitive deficits, difficult temperament or hyperactivity,” interacts with problematic parenting, weak family bonds, and poverty (Moffitt 2006, 278). Maternal substance abuse, poor prenatal nutrition, and child abuse can affect neuropsychological development (Moffitt 1993, 680). Furthermore, given that many characteristics (temperament, personality, cognitive skills) of parents and their children tend to be correlated, the implication of the theory is that high-risk children are disproportionately born into families that have the fewest psychological resources to deal with such children (Moffitt 1993, 681; Skardhamar 2009). Later, a child with such traits runs into problems in interpersonal relationships, and these negative transactions during childhood and adolescence gradually “construct a disordered personality,” manifested as aggressive and persistently antisocial behavior. In addition to these stable differences, or “contemporary continuity” in antisocial behavior attributable to persisting individual traits, the LCP type also suffers from snares, or “cumulative continuity,” which make desistance ever less likely (Moffitt 1993, 683–684). If the offenses by the AL group usually consist of non-violent crimes and status offenses, the crimes committed by the LCP group are generally more serious and, importantly, more often violent offenses (Moffitt 2006). The LCP offender is thus the type of individual that criminal career research seeks to identify, and the type of criminal career that society would most like to prevent.⁴

In Moffitt’s theory, SES and crime are correlated through at least two processes. First, poverty and low SES in the family of origin are more prevalent in the LCP group, and these are listed by Moffitt as important childhood risk factors for persistent criminal behavior. Second, offending by both LCP and AL groups can lead to snares, such as dropping out of school, and this in turn explains why educational level and crime are correlated among young adults. Thus, the effect of SES in the family of origin is theorized to be causal, whereas the correlation between young adults’ own SES and crime is caused by selection because of snares. Here, Moffitt’s ar-

⁴ The typological approach proposed by Moffitt has also had its critics, and the question of the LCP group being fundamentally different from others remains a subject of debate (Skardhamar 2009). Furthermore, the validity of evidence based on so-called group-based trajectory modeling (Nagin 2005), often presented as supporting taxonomic theories, has been brought into question, since continuously distributed (as opposed to categorical) heterogeneity in criminal careers can produce similar results (Skardhamar 2010).

gument closely parallels Sampson and Laub's concept of cumulative disadvantage.

While all three theories share an element in recognizing the role of individual traits established early in life, several assumptions are markedly different. First, Moffitt's theory differs from the others by presenting a taxonomy instead of a general theory, which expects a dose-response association between the continuously distributed risk factors (such as self-control) and crime. Second, Gottfredson and Hirschi see the age-crime curve largely as a consequence of maturation, which "is just that" (Gottfredson and Hirschi 1990, 136), whereas both Moffitt and Sampson and Laub stress that social processes during the life course alter the age-crime curve. Third, regarding the role of life events, it could be argued that Moffitt's emphasis, like that of Gottfredson and Hirschi, is in childhood and early adolescence, while Sampson and Laub stress that life events and human agency in adulthood play a major role in explaining crime (DiPrete & Eirich 2006, 291).

The three aforementioned theories have shaped empirical research on crime over life course during the past two decades. Within these theories, SES can be understood as a family-level risk factor, as a time-varying source for informal social control in the form of employment, as a consequence of early-life antisocial behavior that leads into further difficulties, or as a spurious correlate, brought about by low self-control. Despite these hypothesized pathways, it is evident that none of these theories influential in 21st-century criminology put a major emphasis on SES, and low SES now seems to be considered a risk factor among others rather than as a variable of fundamental importance in explaining crime. Informed by these life-course theories of crime, the current study takes as its starting point the notion that several selection processes earlier in life are likely to have a major impact on the detected SES-crime associations in young adulthood.

2.3 SES and Violent Victimization

While the association between SES and criminal behavior has been debated and theorized rigorously, discussions explicitly focusing on SES and the risk of violent victimization are less varied. On the other hand, as victimology appears to be a less cohesive (Lauritsen & Archakova 2008) field than criminology,⁵ it is also challenging to get a complete picture of the state of victimological theory.

⁵ Often, victimology is seen as a sub-field of criminology rather than as a parallel field (Lauritsen 2010).

The most prominent explanation for socioeconomic differences in violent victimization has probably been a combination of two approaches: lifestyle theory by Hindelang et al. (1978) and routine activity theory by Cohen and Felson (1979). Although the two approaches, which closely resemble each other, were originally developed to explain different phenomena (Meier & Miethe 1993), they are commonly presented as one framework for understanding differences in the risk of victimization. In this framework, the factors that influence the risk of victimization are usually formulated as exposure to crime, proximity to crime, target attractiveness, and capable guardianship (Cohen et al. 1981; Meier & Miethe 1993). Lifestyle theory was originally developed to explain differences in violent victimization between social groups (Meier & Miethe 1993), and for this reason, the following review focuses on the origins of lifestyle theory and its critique.

According to Hindelang et al. (1978) the likelihood of personal victimization depends on a concept of they call lifestyle (Hindelang et al. 1978, 241), which refers to daily routine behaviors, including vocational and leisure time activities. Individual lifestyle is affected by role expectations and structural constraints imposed on individuals, which in turn vary not only by demographic factors such as age and gender, but also by income, education, and occupation. While role expectations are related to cultural norms that define preferred and anticipated behaviors for people of different statuses, structural constraints refer to “limitations on behavioural options” (Hindelang et al. 1978, 242). For instance, it is evident that economic resources can affect educational opportunities and possible areas of residence. People adapt to these expectations and constraints on both individual and group levels, and the group-level adaptations result in shared adaptations within subgroups in a society. What results is a lifestyle; in other words, “regularities in behavioural patterns” (Hindelang et al. 1978) in daily routine activities. This lifestyle modifies exposure to high victimization risk situations directly, and differences in exposure to risk can explain socioeconomic differences in victimization (Hindelang et al. 1978).

In addition to the “direct effect” that operates through lifestyle-based variation in daily routine activities, such as spending time in public places, using public transportation, or living in a certain area, that are thought to explain variations in victimization, lifestyle can also affect victimization indirectly, through associations with others (Hindelang et al. 1978). Hindelang et al. (1978) propose that social interactions occur disproportionately with similar others, and that an individual’s probability of victimization depends on the degree to which he or she shares demographic features with potential offenders. In other words, if we believe that offending is inversely

related to SES, “demographically homogenous social interaction” (Hindelang et al. 1978, 256) leads to a situation where individuals who are similar to offenders are their most likely victims.

Since the first contribution of Hindelang et al. (1978), the lifestyle theory has been critiqued (Finkelhor & Asdigian 1996) and further developed (Cohen et al. 1981; Wilcox et al. 2003). The opportunity theory of victimization by Cohen et al. (1981) places less weight on lifestyle and puts forth new propositions about victimization in different types of crime, but retains the assumption that income and violent victimization should be inversely associated due to greater exposure and proximity, and less protection from capable guardians in low-income groups. Finkelhor and Asdigian (1996), on the other hand, claim that lifestyle theory offers a limited explanation of youth victimization, in particular. According to them, lifestyle theory focuses too much on “street crime” committed by strangers and victimization that happens due to risky behavior such as binge drinking and delinquency, but falls short in explaining violence unrelated to such activities, for instance violence committed by family members or acquaintances. They argue that personal characteristics can affect victimization risk irrespective of routine activities (Finkelhor & Asdigian 1996).

Although SES is seen as a somewhat distal predictor in the lifestyle theory of victimization, socioeconomic differences in violent victimization have been a controversial topic in research and policy discussing violence against women. It is often argued that SES has a limited role in explaining intimate partner violence (IPV), and that IPV “does not respect class” (Dobash et al. 2004). In an influential study, Mooney (2000, 185) reports that “domestic violence can occur and at equal frequency throughout the class structure.” Emphasizing the SES independence of IPV was particularly common among the pioneers of feminist research on violence against women, and the assertion that men abuse women in all social classes was an effective way to raise awareness about IPV as a serious social problem (Renzetti 2009).

Felson and Lane (2010) argue that the “gender perspective,” as opposed to the alternative “violence perspective” that views causes of violence as uniform regardless of the type of violence, perceives IPV mainly as an effort to dominate and control the partner.⁶ With gender inequality as the root cause, according to a gendered view, “IPV is primarily a problem of men's

⁶ “Family violence perspective” and “violence against women perspective” are alternative concepts to divide the competing branches of IPV research under two broad terms (Felson & Cares 2005). These terms are also related to the debate about gender symmetry in IPV perpetration (Dobash & Dobash 2004).

violence against women caused by wider societal rules and patriarchal beliefs that encourage male dominance and female subordination” (Dixon & Graham-Kevan 2011, 1146). While feminist scholars disagree on which is a more fundamental type of inequality, that related to gender or that related to SES (DeKeseredy 2011), it is evident that the two types of structural inequalities have often appeared at odds with each other as explanations of IPV. However, there is some evidence that this contrast might be fading, as researchers have attempted to integrate both types of inequalities into a framework for understanding IPV perpetration (DeKeseredy & Schwartz 2010).

3 EMPIRICAL EVIDENCE

Studies on the relationship between SES and crime can be divided into those that use aggregated (macro) data and those that use individual-level (micro) data. In general, studies on the macro level (e.g., municipality or country) give stronger support to the SES-crime link than studies using individual-level data (Jarjoura 2002). In a meta-analysis of macro-level studies, Pratt and Cullen (2005) found the strongest evidence for social disorganization and economic deprivation as determinants of aggregate crime rate. In an earlier meta-analysis, Hsieh and Pugh (1993) discovered that poverty and economic inequality were strong macro-level predictors of crime. As the current study uses individual-level data, the following review of existing research concentrates on studies that have used micro-level data. Furthermore, emphasis is placed on longitudinal studies.

The current study focuses on crime and the victimization of young adults, and the independent variables measure personal SES, not that of the family of origin. While research shows a person's own SES and life situation is generally a better predictor than that of the individual's parents (Ellis & McDonald 2001), it should also be noted that measurement of a person's own socioeconomic attainment is endogenous with relation to criminal behavior. As we know that those who end up as chronic offenders tend to have an early onset of criminal behavior (Moffitt 1993), and levels of criminal behavior peak before SES is established, the direction of causality is unclear. While low SES is often seen as an antecedent of crime, it is equally plausible that crime in youth causes low SES in adulthood (Caspi et al. 1998; Nilsson & Estrada 2011). For this reason, parental SES is less problematic in terms of causal ordering, although it is arguably a rather distal predictor of crime. Given the intergenerational transmission of education (Kivinen et al. 2007), individual-level measures of SES inevitably also correlate with parental SES.

Because of the difficulty of measuring SES in youth, a transitional phase of the life course (Halleröd & Westberg 2006), several variables tapping into individual SES are used here. The following review is categorized according to the predictors used in the current study. While for example education and unemployment are not interchangeable in terms of substance, they also differ in terms of measurement. Educational level is a rather static measurement that only moves "upwards": when a person completes a degree, it is (almost) never taken away. All the other measures – occupation-based social class, taxable income, and unemployment – are

more dynamic measures that can be highly variable during young adulthood.

Finally, reflecting the areas where quantitative research on crime is prominent, most research reviewed here has been conducted either in the USA, in the UK, in the Netherlands or in the Nordic countries. The general social and economic context in these countries is rather different, and it has been proposed that the Nordic welfare states represent a “social democratic welfare regime” that is characterized by “comprehensive risk coverage, generous benefit levels, and egalitarianism” (Esping-Andersen 1999, 78). It is possible, and even likely, that the individual-level effect of SES on crime is conditional on the macro context. While there is no reason to expect that the macro context in any of these countries would reverse the association between SES and crime, it is still certain that income distributions (Wilkinson & Pickett 2009) and poverty rates (Esping-Andersen 1999), for instance, vary markedly between these countries. If these are the causal factors that explain the association between SES and crime, one would expect a weaker association between SES and crime in the Nordic countries. The implications of the welfare state context for the interpretation of the results of this study are discussed more thoroughly in section 7.2.

3.1 The Debate on Social Class and Crime

Sociological research on social class and social mobility typically employs an occupation-based (current, previous, or family) measure of SES (Breen 2005; Erola 2010), whereas income is also frequently used to measure SES, for instance by classifying families or individuals into income quintiles. Given the amount of research on juvenile delinquency (Sampson & Lauritsen 1994) with self-reported data typically collected in classrooms, these measures in empirical research on crime and delinquency usually tap into parental occupation and income, since the status of the youth is not yet established.

Before the advent of the self-report method, studies based on official crime statistics from courts or police typically indicated a clear inverse association between social class and crime. The first self-report surveys (Porterfield 1943; Nye & Short 1957; Jaakkola 1966, in Finland) challenged these results and showed that delinquency in adolescence was actually much more prevalent than previously thought, and, additionally, evenly distributed across social classes (on the history of self-report delinquency surveys, see Kivivuori 2011). It appeared that there were delinquent children in all kinds of families, and this led to the conclusion – sup-

ported by insights from labeling theory in particular (Kivivuori 2011) – that the association between social class and officially documented crime is an artifact caused by biased control, not by differences in criminal propensity or behavior. In empirical terms, this branch of the SES-crime debate reached its pinnacle in 1978, when Tittle, Villemaz, and Smith published a meta-analysis of 363 studies and concluded that there is only a weak association between social class and crime; especially the newer studies reviewed found no association. Thus, the general belief among many criminologists that social class and crime are correlated was deemed a “myth” without sound empirical footing (Tittle et al. 1978).

Not surprisingly, the claim that the SES-crime association was a myth was met with criticism. Both Hindelang et al. (1979) and Braithwaite (1981) pointed out that many self-report studies focus on the less serious end of the crime-delinquency continuum, and consequently a naïve comparison between estimates from datasets using self-reported and official data blurs the distinction between trivial and serious offenses (Hindelang et al. 1979). Additionally, the failure to reach children from the lowest social classes in school-based surveys can artificially weaken the association (Braithwaite 1981). Regarding class bias in official reports, Braithwaite (1981) concluded that there was no credible evidence to suggest that there would be a major class bias in reporting serious crimes. A study by Farnworth et al. (1994) seems to have settled this debate: They found that once true underclass status and serious and repeated offending behaviour are measured, an inverse association between SES and offending is robust in self-reports of crime as well (see also Kivivuori 2011). Essentially the same finding was confirmed by Bjerck (2007), who discovered a strong association between household economic resources and serious crime after accounting for measurement error in economic resources, using instrumental variable methods. Thus, evidence suggests that the measurement of both SES and crime affect the strength of the observed association between the two. In their review of studies on the SES-crime association, Ellis and McDonald (2001) come to following conclusions: A majority studies on the SES-crime link find an inverse association, and individual SES is more closely associated with crime than parental SES. The evidence for the latter, especially that based on self-reports, is mixed however (see Bjerck 2007). Finally, they found that the association is stronger in studies using official data, which is partly a function of the severity of crime measured.

At least in the American context, one way to settle the debate of social class and crime was to create a distinction between the persistently poor underclass and others. Accounting for the non-linearity (small differences between high and middle classes, and a big difference between middle and

low classes) of the SES-crime link seems to better capture the SES-crime association in an American setting (Farnworth et al. 1994). However, not all evidence indicates such a threshold effect: Galloway and Skardhamar (2010) analyzed the effect of parental income – measured as a 11-year annual average to avoid errors related to short-term measurement of income (Bjerk 2007) – on the onset of children’s offending activity in Norway and found evidence of gradual differences between income deciles. The effect, particularly robust in serious theft, was not attributable to the most disadvantaged groups only (Galloway & Skardhamar 2010). The issue of linearity has also been examined by Wright et al. (1999), with strikingly different results. Using data from the Dunedin Multidisciplinary Health and Development Study, they found that both low and high SESs of parents – measured as occupational statuses, with additional robustness checks using measures of education and income – increase the probability of adolescent delinquency, but because of different and indirect mechanisms. According to their results, low parental SES causes delinquency due to financial strain and decreased educational and occupational aspirations, but high parental SES is connected to crime due to greater risk-taking and social power, in turn related to a lesser likelihood of detection, and lesser commitment to conventional values (Wright et al. 1999). It is, however, unclear whether the findings of Wright et al. (1999) are replicated across samples.

Even though the current consensus favors an inverse association between social class and crime, it is as yet unclear what the mediating mechanisms between the two are. Regarding the relationship between a low SES family and offending, researchers have suggested that family socialization practices might play an important mediating role (for a review, see Farrington & Welsh 2007). Fergusson et al. (2004) examined the association between childhood economic disadvantage and crime in adolescence in a New Zealand birth cohort and found that the initial strong association between the two could be almost completely explained by the mediating factors related to parenting practices, conduct problems, school failure, and delinquent peers.

3.2 Education

While the association between parental SES and juvenile delinquency has been debated at length, the bivariate inverse association between education/academic performance and crime/delinquency is generally a very robust finding in empirical research. Felson and Staff state that “academic performance is one of the strongest and most consistent correlates of delin-

quency” (2006, 299). A meta-analysis by Maguin and Loeber (1996), which reviewed studies on the effect of academic performance on delinquency, reports consistent negative association between the two in both cross-sectional and longitudinal designs. Using a cut-off point in the middle of the grading scale (A–C vs. D–F in the American grading system), they found that the overall bivariate effect (OR) of low versus high academic performance on delinquency was 2.07–2.11. They also found evidence suggesting that school performance measured later predicts delinquency better than that measured earlier, that the association between the two is rather linear, and that the gradient is steeper for males than females. In the reviewed cross-sectional studies, outcome measure (self-report vs. official) did not matter, but longitudinal studies show a greater education effect when tested with official data (Maguin & Loeber 1996).

Prior studies conducted in Finland indicate that both low parental education and an individual’s own low education and poor academic performance are important determinants of crime. Using nationally representative data on a sample of boys born in 1981, with police-reported crime as the outcome, Elonheimo (2010) found that those boys belonging to a high-level offending group (4 % of the cohort, more than five crimes during the four-year follow-up period) often had parents with low education, and also themselves fared poorly in school in terms of academic performance. In a 31-year follow-up study of the entire cohort that had been born in northern Finland in 1966, Riala et al. (2003) found that after controlling for parental occupation-based social class and psychiatric morbidity, both low academic performance and completing only basic education predicted drunken driving (driving while intoxicated, DWI). When compared to all other educational levels, those with only basic education had a 3.0 OR for one DWI conviction, whereas effects were especially pronounced (OR 8.6) for recidivist DWI offenders. Grade point averages (scale from 4 to 10) at age 16 were 7.3 for those with no DWI convictions, 6.6 for those with 1–2 DWI convictions and 6.3 for those with three or more convictions (Riala et al. 2003). In Sweden, Ring and Svensson (2007) found that educational achievement largely mediated the effect of parental occupation-based social class on crime in both survey and register data.

If the debate on social class and crime focused mostly on measurement issues on both sides of the regression equation, it appears that selection effects have been more on the agenda when the effect of education on crime has been studied. While education is known to be a major determinant of occupation-based social class in adulthood, it is hard to justify claims that education would only mirror one’s social position. Despite the fact that parents’ educational levels (Kivinen et al. 2007) predict children’s educa-

tional attainments, this variable is also correlated with individual characteristics such as cognitive skills, low self-control, conduct disorders, and attention problems (Maguin & Loeber 1996; Heckman et al. 2006; Moffitt et al. 2011). Given important role of these individual characteristics as a predictors of both academic performance and crime (Felson & Staff 2006; Heckman et al. 2006; Savolainen et al. 2012), teasing apart the possible causal effect of dropping out of school on crime is difficult.

In their meta-analysis of studies on the education-crime link, Maguin and Loeber (1996) examined the role of some third factors (parental SES, conduct problems, attention problems, and intelligence) that might explain the association between the two by selection processes. On the basis of the review, they concluded that intelligence and attention problems are important common causes for both academic performance and delinquency, whereas the role of parental SES and conduct problems is limited (Maguin & Loeber 1996). Giving support to self-control theory, Felson and Staff (2006) found that low self-control is a major common cause between low academic performance and high delinquency, and concluded that the relationship between the two is spurious, i.e., attributable to prior differences between those who do well in school and those who do not. A recent Finnish study (Savolainen et al. 2012) using the northern Finland birth cohort data examined the effects of antisocial propensity and academic performance on late-adolescent (17–19 years old) delinquency using structural equation models. In contrast to Felson and Staff, they found that the effect of low academic performance at age 15 remains significant after controls for antisocial personality comprising conduct problems and hyperactivity at age 8. They speculated that this might be due to their measuring more serious crimes, whereas some of the earlier studies have looked at less serious and more prevalent delinquency. However, Savolainen et al. (2012) also confirmed that for the most part, low academic performance and low school attachment mediate the effects of earlier individual-level differences (antisocial personality and learning difficulties) and low parental education. In a prior analysis with of same data, Savolainen et al. (2010) found that the interaction effect of attention deficit hyperactivity disorder (ADHD) and low verbal ability on crime was direct rather than mediated by education, but educational marginalization and alcohol use had an additive effect after controls for early childhood measures of criminal propensity. The combined effect of high scores on the ADHD measure and low verbal ability was particularly strong (Savolainen et al. 2010).

As noted by Savolainen et al. (2012), it is plausible to assume that the effects of delinquency and academic performance during adolescence are reciprocal, making causal inference difficult. Perhaps the most convincing

evidence on the causal effects of education on crime comes from the field of economics. Two studies, one in the US (Lochner & Moretti 2004) and one in the UK (Machin et al. 2010), have used changes in compulsory schooling laws to estimate the effect of added education on crime rates. Both of these reforms raised the minimum age that one is allowed to leave school, thus prolonging the time spent in education for those individuals who would have otherwise left school at the earlier possible date. This identification strategy using natural experiments, either the state-level temporal variation in implementation of schooling leaving age laws (Lochner & Moretti 2004) or two different reforms conducted in 1947 and 1973 (Machin et al. 2010), allows estimating the effect of an exogenous shift (the raised minimum level of education due to legal reform) in education on crime. Lochner and Moretti (2004), using instrumental variable methods to estimate the causal effect, find a decrease in incarceration rates, arrest rates, and self-reported crime. The findings from the U. K. by Machin et al. (2012, 2) are in line with the findings from the U. S., and they conclude, “improving educational attainment of the marginal individuals can act as a key policy tool in the drive to reduce crime”. Two recent studies analyzing compulsory school reform in Sweden confirm the same crime-reducing effect of education for both the affected cohorts (Hjalmarsson et al. 2011) and their offspring (Meghir et al. 2012).

3.3 Unemployment

If (occupation-based) social class is a somewhat abstract and complex (Gottfredson & Hirschi 1990) concept with several possible classification schemes and sometimes contested boundaries between classes (Breen 2005), and educational level is a problematic variable for causal analysis in that it only changes upwards, then unemployment represents a more concrete shock (or life event) that many theories assume to increase crime (Bushway & Reuter 2002). What is more, the time-varying nature of employment makes it an interesting independent variable for researchers working with longitudinal designs. If one accepts the potential outcomes framework as the standard for causal inference (Morgan & Winship 2007), it is definitely easier to imagine a random assignment of employment or unemployment than that of something as fundamental and stable as social class. Although measuring unemployment does not equal measuring social class, it certainly provides the researchers better opportunities to study the link between economic hardship and crime in a more dynamic manner.

Despite the fact that unemployment and occupation-based social class are inevitably correlated measures, a separate vein of research focusing particularly on work and crime can be identified. The domain of work and crime has also interested labor economists, and some major contributions to this issue stem outside of traditional criminology. The majority of research has focused on the macro-level unemployment-crime link that seeks to clarify whether changes in unemployment rate affect crime rates. Once again, due to the individual-level nature of the current study, these studies are mentioned only briefly. Cantor and Land (1985) argue that an increase in the unemployment rate has both crime-reducing (less opportunities when people spend more time at home) and crime-increasing (motivation) effects, but these can be disentangled as the motivation effect is proposed to lag (as financial problems mount), whereas the protective effect is imminent. In his review of aggregate-level studies, Chiricos (1987) concludes that the unemployment rate is connected to the property crime rate, but not to the violent crime rate. In a more recent contribution from labor economics, Raphael and Winter-Ebmer (2001) use instrumental variable methods (state military contacts and exposure to oil shocks as instruments for unemployment rate), and obtain results largely in line with Chiricos' review. Thus, at least the aggregate-level property crime rate seems to increase when unemployment increases. However, in another review, Freeman (1999) concludes that the effect of unemployment on crime is far from overwhelming (Freeman 1999, 3543).

The leading studies on unemployment and crime with individual-level data have used either within-individual models with longitudinal panel data or experimental settings in ex-prisoner/high-risk populations. It could be argued that the unemployment-crime link has often become a question of desistance from crime, not least because of Sampson and Laub's (1993; 2003) theory that stresses the importance of work in providing possibilities for desistance. Another reason for this is likely to be data availability, as those longitudinal data with sufficiently short measurement intervals are usually high-risk samples. Thus, most studies with a general population frame use aggregate-level data. One exception to this is the pioneering study by Farrington et al. (1986), who studied within-individual change (used each boy as his own control) in unemployment and crime during adolescence in a sample of 411 London boys. They found that property crime was more prevalent (0.83 property offences per year when unemployed versus 0.39 when not) during periods of unemployment than while employed, while other types of crime were not. Furthermore, they discovered an interaction effect showing that unemployment was linked to more crime only among those with a high criminal propensity, determined based on

childhood risk factors. Thus, the unemployment effect might be conditional on stable individual factors. Later, however, the link between employment and crime in adolescence was studied further, as several studies indicated that working during adolescence was actually linked to more, rather than less, crime (Kouvonen 2002). The latest studies have discovered that this crime-increasing effect of work during adolescence is likely to be a selection artifact (Apel et al. 2008; Paternoster et al. 2003; Staff et al. 2010), i.e. caused by prior differences between those who work and those who do not.

Studies on employment and crime using high-risk samples often emphasize the selected nature of their data: especially prison inmates that tend to have limited work histories and low qualifications (Raphael 2011; Kivivuori & Linderborg 2009), suffer from poor physical and mental health, and often have substance abuse problems (Joukamaa et al. 2010). It is thus evident that (compared to the general population) their employment prospects are limited, and separating the effect of unemployment from these selection processes is difficult. Longitudinal studies conducted in offender samples during adulthood have used different strategies to control for selection into unemployment. Typically, these studies examine the same individuals during different periods, and compare their level of crime of when unemployed versus when employed. To estimate the within-individual effect of unemployment on crime, both two-level hierarchical regression models and fixed-effects (FE) regression models have been used to obtain an “estimate that controls for all stable characteristics of the offender” (Allison 2009, 1).

The landmark study by Horney et al. (1995) investigated within-individual variation in several life circumstances, including employment and crime in a sample of convicted felons. Using 36-month retrospective life calendar data, they found that work was related weakly to crime, and surprisingly, they reported that property crime was more prevalent during the months when the men were working. However, in a more recent study with similar data focusing explicitly on stress from different sources, Felson et al. (2011) found that financial stress was linked most closely to property crime, but not so much to violent crime. In the model controlling for financial stress, employment additionally reduced property crime, but the effect was not statistically significant. Studying a population of female inmates, Slocum et al. (2005) found very similar results: nonviolent crime and drug use were less likely during periods of employment, while violent crime was not.

While studies based on American samples dominate the literature on unemployment and crime, important contributions from the Netherlands and Nordic countries have been published recently. Two Dutch studies

(van der Geest et al. 2011; Verbruggen et al. 2012), using high-risk samples of individuals 18 to 32 years of age from juvenile justice institutions, have conducted longitudinal analysis on trajectories of employment and crime. Van der Geest et al. (2011) used semi-parametric group-based models, or latent class growth analysis, to study the development of crime and its relation to employment status with yearly data among the boys in the sample. They found an inverse association between days of employment and crime. However, job stability was associated with crime only in the group with a lower overall level of crime, whereas they could only find an association between temporary jobs and crime in the group with a higher overall level of offending. They speculate that those with the highest risk of offending, a small group of individuals in the sample, seldom reach stable jobs, and therefore an effect cannot be identified (van der Geest et al. 2011). Extending the analysis to females from similar institutions, Verbruggen et al. (2012) found that employment reduced offending for both high-risk men and women. Prolonged unemployment, however, additionally increased the probability of crime only among females (Verbruggen et al. 2012).

Even if the aforementioned articles are desistance studies in the sense that they focus on employment and crime among a pre-defined high-risk group of individuals, recent Nordic studies have focused explicitly on the question of employment and criminal desistance among ex-prisoners. Using Finnish register data, Savolainen (2009) examined the effect of employment on recidivism in a sample of men convicted to a prison sentence in 1996. He found that after adjusting for several covariates, those who became employed after release had a 40 % lower rate of re-offending. Using similar data from Norway, Skardhamar and Telle (2012) found that having a post-release job reduced the risk of recidivism, but they also concluded that a major part of this association could be attributed to systematic selection effects due to observable background characteristics. For instance, those with a greater attachment to the labor market prior to a prison sentence have a lower risk of re-offending, whereas those convicted of property crimes have a greater risk of recidivism (Skardhamar & Telle 2012).

Despite these high-quality observational studies on employment and crime, attributing a causal effect to employment remains a challenge. Like Skardhamar and Telle (2012) note, in the absence of random allocation to employment after release, controlling for unobservable differences between those who get employed and others, such as motivation and readiness for change (Skardhamar & Telle 2012), cannot be accomplished with register data. The same limitation applies to within-individual analysis – e.g. FE model (Bjerk 2009) – that only controls for stable differences between in-

dividuals: if motivation is something that varies both between and within individuals, the most reliable way to ensure that selection into employment does not confound the estimate is to somehow randomize individuals into treatment (employment) and control (unemployment) groups. As discussed in the context of education and crime, results from longitudinal studies suggest that early criminogenic factors such as “explosive” behavioral style (Caspi et al. 1987), low parental resources, poor cognitive skills (Caspi et al. 1998), and low self-control (Moffitt et al. 2011) predict labor market failure in adulthood. Many would argue that without such controls selection bias is likely to affect the results from observational data. Furthermore, the evidence is mounting that prior delinquency (Caspi et al. 1998), criminal record (Pager 2003), and incarceration (Waldfogel 1994; Western 2002) worsens an individual’s chances of finding work, as predicted by life course theories (Laub & Sampson 2003; Moffitt 1993).

While selection effects (omitted variable bias or reverse causality) are a potential threat to all causal analysis based on observational data, the argument is made stronger here by the fact that in the case of employment-recidivism association results from observational studies are in contrast with experimental evidence (Bushway & Apel 2012). A review by Visser et al. (2005; see Raphael 2011 for a slightly more optimistic conclusion) suggests that almost all ex-offender employment programs, aimed at providing employment to ex-prisoners in the hope of reducing re-offending, have failed to show clear results. The notable exception is the study by Uggen (2000), who found an effect conditional on age in his analysis of the National Supported Work Demonstration Program. In a randomized setting, he discovered that individuals over 27 years old showed lower rates of recidivism when provided with employment opportunities, but no such effect could be detected for younger individuals. This led him to conclude that work may serve as a turning point for older individuals, but not necessarily for those who are younger (Uggen 2000). Regarding the role of work as a turning point (Sampson & Laub 1993), it is possible that employment has the potential to deter crime among ex-convicts, but these exogenous employment “shocks” are either: a) not good enough in the context of re-entry programs, or b) occur frequently enough in naturalistic settings. It might be that the “relevant ‘social goods’ are in limited supply among the offender populations” (Savolainen 2009, 301). A very interesting recent study analyzing the timing of job entry and desistance among crime-prone men in Norway suggests that reductions in offending happen prior to job entry rather than the other way round: this evidence casts doubt over the hypothesis that employment would be an exogenous shock that causes desistance from crime (Skardhamar & Savolainen 2013).

Finally, there are two promising studies (Andersen 2012; Fallesen et al. 2011) from Denmark that show active labor market policies (ALMP) might help reduce crime, indicating that the unemployment-crime link is not solely a question of desistance in high-risk groups. Using experimental data, where the unemployed individuals were assigned randomly to an intensified ALMP (treatment group) or standard ALMP, Andersen (2012) found that levels of offending are lower during participation in the intensified ALMP, consisting of job search programs and regular meetings with caseworkers. Similarly, relying on natural experiments, Fallesen et al. (2011) discovered that workfare policies – increased activation of “unemployed uninsured” youth by making work a requirement for receiving unemployment benefits – resulted in reductions in crime. Similar results were obtained both from a single radical policy reform (that was relaxed later due to complaints and law suits) in the municipality of Farum and using the municipality-level timing of other activation reforms in Denmark to construct a difference-in-differences model to assess the effect of policy reforms on crime (Fallesen et al. 2011).

3.4 SES and Victimization

Studies on socioeconomic differences in violent victimization typically rely on cross-sectional population-based surveys. The most well-known surveys are the National Crime Victimization Survey (NCVS) in the US and the British Crime Survey (BCS) in the UK. The Finnish variant is the National Victimization Survey (FNVS), conducted for the first time in 1980. The International Crime Victim Survey (ICVS) is a comparative study conducted in several countries. One of the main aims of these repeated cross-sectional surveys is to provide an alternative means of analyzing crime rate trends (Lynch & Addington 2007). While longitudinal studies on victimization are rare, nationally representative victimization surveys have the advantage over school-based self-report surveys in that they include people of all ages. The following review focuses mostly on survey-based research, as register-based studies on victimization are rare outside homicide research.

Regarding descriptive results from the large-scale surveys in the US and UK, the NCVS (Bureau of Justice Statistics 2010) shows large differences in violent victimization by household income, whereas victimization risk appears more evenly spread in the BCS. However, the unemployed have a higher risk of victimization in the BCS as well (Flatley et al. 2010). Analysis combining the ICVS data from 17 countries, on the other hand, indicates that high income and education increased the risk of victimization

(van Kesteren et al. 2000). In their review of correlates of violent victimization, Sampson and Lauritsen (1994) report consistent differences by SES in victimization risk. Of the measures used, low family income and unemployment are stronger predictors than level of education. Studies with both Finnish (Kivivuori 2003) and Swedish (Nilsson and Estrada 2006) data show clear socioeconomic differences in violent victimization, and additionally indicate that such differences might be increasing. Thus, most victimization surveys support the notion that the risk of violent victimization varies by socioeconomic status.

Researchers have also been interested in area-level measures of disadvantage and violent victimization risk. Sampson et al. (1997) found that concentrated disadvantage in a neighborhood predicted violent victimization rates. Using Dutch neighborhood-level data, Nieuwbeerta et al. (2008) found that low social cohesion and socioeconomic disadvantage were associated with higher homicide rates in a neighborhood. However, the effect of neighborhood disadvantage might be somewhat different in a Nordic setting. For instance, Nilsson and Estrada (2007) examined area-level variation in affluence and victimization risk, but came to the conclusion that violence occurring within the neighborhood (as opposed to private residences and other areas) constitutes such a small proportion of all violence that neighborhood disadvantage can at best explain only a relatively small amount of all violence. Furthermore, they found that the initial differences by area-level resource deficiency could be accounted for by controlling for individual-level factors related to housing, age, family type, and lack of financial resources (Nilsson & Estrada 2007).

One of the most consistent predictors of violent victimization is a person's own criminal behavior (Lauritsen & Laub 2007; Nofziger 2009). If low SES increases the probability of offending, it might be that criminal behavior or lifestyle indirectly links low SES to an increased risk of offending. Furthermore, the implications of the so-called *victim-offender overlap* have been discussed in the context of population heterogeneity and state dependence (Lauritsen & Laub 2007): is it so that offending and victimization share common risk factors, or is there a dynamic association between the two, where offending increases the risk of subsequent victimization (or vice versa)? Both processes are likely to matter (Lauritsen & Laub 2007). Zhang et al. (2001) found that a deviant lifestyle measured at time 1 increased the risk of violent victimization at time 2. At least among adolescents, Nordic research supports the notion that criminal behavior partly mediates the effect of low SES on increased risk of violent victimization (Bjarnason et al. 1999; Savolainen et al. 2009). It is also important to consider the effect of victimization on later socioeconomic attainment, espe-

cially serious violence, which can damage a person's long-term education and employment prospects through psychological trauma and physical injury (MacMillan 2001).

If the association between measures of SES and general violent victimization seems well established, the question of the effect of low SES on intimate partner violence (IPV) is more controversial⁷, bearing in mind the claim that "domestic violence does not respect class" (Dobash et al. 2004, 578). A strict "equal risk" hypothesis assumes that there is no bivariate association between measures of SES and IPV perpetration. While SES can also be measured on a family level, meta-analyses have separately assessed the effects of both male (perpetrator) and female (victim) SES on the risk of IPV. Schumacher et al. (2001) found that most measures of SES were inversely associated with IPV perpetration, with low income having the strongest effect, whereas the victim's low education and unemployment increased the risk of IPV victimization. In a later review, Stith et al. (2004) reported that male unemployment, low income, and low education have a significant but weak association with IPV perpetration. While in the same direction, similar measurements for female victims yielded only small effects (Stith et al. 2004). Jewkes (2002), on the other hand, concluded that poverty is the only consistent socioeconomic or demographic predictor of IPV.

Summing up the evidence on SES and IPV, a strict "equal risk" hypothesis is not generally supported (Renzetti 2009), especially if this hypothesis refers to bivariate differences in IPV perpetration between men from different SES groups. The studies by Mooney (2000) and Lupri et al. (1994) are some notable exceptions to this pattern. On the other hand, SES appears to be a rather distal and relatively weak determinant of IPV (Stith et al. 2004). The "relative conventionality" (Dobash et al. 2004) of the IPV offender, referring to a comparison between perpetrators of male-to-female and male-to-male violence, seems to be a more sound argument. Thus, IPV offenders might have a higher SES on average than other violent men. Recent studies on homicide have compared men who kill their intimate partners with those who kill unrelated men, and found that men who kill their partners indeed seem less disadvantaged (Dobash et al. 2004; Kivivuori & Lehti 2012). However, the later study with Finnish data shows that they

⁷ One point of contention has been the choice of survey methodology, as different types of surveys give strikingly different results on the prevalence of IPV. Surveys specially designed to capture IPV, for instance "violence against women" surveys (Johnson 1996), typically produce much higher prevalence estimates of IPV than crime victimization surveys (Heiskanen 2000).

still are a highly disadvantaged group when compared to the general population. Furthermore, they examined a larger variety of homicides, and showed that intimate partner homicides cluster together with other homicides in close relations in terms of unemployment and substance abuse problems of the offender (Kivivuori & Lehti 2012).

3.5 Nordic Register-Based Research

Official data on registered crimes from police and courts has always had a major role in empirical research on crime, and in that sense, register-based research is nothing new. However, the possibility of linking information across several registers into samples drawn from population registers and creating extensive datasets comprising longitudinal measurements on both life circumstances and crime has not yet been utilized fully in Finland. Given the difficulty of reaching the most marginalized individuals in terms of community surveys, and declining response rates in surveys in general, register-based data can provide a more complete picture of distribution of crime across all social strata, as it does not suffer from non-response. In Finland, there is a strong tradition of register-based research in demography and health-related research, but until recent years, applications within criminology have been less common. The potential for register-based data seems to be underused in other Nordic countries as well (Lyngstad & Skardhamar 2011). Dutch criminologists, on the other hand, have been faster to take advantage of data somewhat similar to register data. A random sample of people convicted in 1977 (*Criminal Careers and Life-Course Study*) has been used in several studies to investigate longitudinal patterns in criminal careers (see e.g. Blokland et al. 2005; Bushway et al. 2009; Nieuwbeerta et al. 2011). A sample spanning five generations, similarly from the Netherlands, has been used in the study of intergenerational transmission of offending (Bijleveld & Wijkman 2009). While these are not purely register-based studies, they also use official documents and archival records to study crime.

It could be argued that register-based research, especially that taking full advantage of register-linkages across administrative data sources, has only begun to gather momentum in Nordic criminology during the last decade, the last couple of years in particular. In the following, some important contributions using Nordic register-based data are mentioned. Kyvsgaard (2003) did a benchmark longitudinal study on criminal careers in Denmark, using register-based data. With data on a total birth cohort born in 1966 in Denmark, Christoffersen (together with British criminolo-

gists Soothill and Francis) examined the consequences of parental alcohol abuse (2003), determinants of rape (2005), violent crime and suicides (2007), and risk factors for DWI (2008). In addition to studies mentioned in the previous sections, Skardhamar (2009) examined the effect of family dissolution on longitudinal patterns of children's crime in Norway. In Sweden, Nilsson & Estrada (2009, 2011) and Bäckman & Nilsson (2010) have studied the causes (including crime) of adult-life social exclusion using Stockholm Birth Cohort Data that combines both survey and register-based data. Both Nilsson (2003) and Skardhamar (2004) examined social backgrounds of prison inmates in Sweden and Norway, respectively. It appears that the "social" causes and consequences of crime, a salient theme in Nordic criminology (Kivivuori & Bernburg 2011), have interested researchers working with these longitudinal register-based data. The goal of the current study is to analyze similar themes with Finnish data.

4 THE AIMS OF THE STUDY

The main aim of the study is to examine socioeconomic differences in various types of criminal offending, with a particular focus on both violent offenders and their victims. Given the difficulty of measuring SES in young adulthood, multiple measures of SES are compared, and the feasibility of each measure is considered. As pure register-based research on crime and victimization is still rare in Finland, the results from the victimization study are contrasted with similar analysis using survey data to assess the validity of register-based analysis, bearing in mind the possible bias caused by hidden crime. Due to the same lack of research and basic information about distribution of crime by socioeconomic measures in the Finnish population, descriptive results are given space as well.

Most research on determinants of crime in Finland has used adolescent samples with self-reported data on offending, whereas crime in young adulthood has been studied less. Criminal career research is, in general, still in its infancy in Finland. Sampson and Laub (2003, 22) have argued that the focus of future research should be on “(desistance from) persistent and serious delinquency” rather than on high-prevalence low-rate offending during teen years. A similar argument underlies Moffitt’s (1993) dual taxonomy: if offending is only studied in adolescence, we easily confound adolescence-limited (AL) and life-course persistent (LCP) groups. Furthermore, it is impossible to study personal SES and its association with crime before any measure of SES is established. Thus, this study excludes offending in adolescence by definition, and focuses mostly on crime among young adults aged 19–30 at the baseline. Results on violent offending and victimization are also presented for 31–50-year-olds.

The specific aims in the substudies were:

- 1) To examine socioeconomic differences in violent crime, property crime, and driving while intoxicated (DWI), and elaborate on the possible effect of low SES on crime by controlling for third variables (Substudy I)
- 2) To assess the more specific unemployment-crime association using within-individual panel models to better account for selection bias regarding the SES-crime link (Substudy II)
- 3) To compare relative differences by SES in male-perpetrated violence, when the violence outcome is disaggregated by gender of the victim and place of occurrence (Substudy III)

- 4) To compare the association between SES and violent victimization by seriousness of violence measured in register-based and survey data (Substudy IV)

All substudies share a comparative element in that several outcome variables are used. While this is not a study of specialization in criminal careers (see e.g. Nieuwbeerta et al. 2011), the ability of SES to explain variation in different types of crime and different types of violence is analyzed. Substudies I and II seek to tease out the *effect* of low SES on crime by employing varying strategies to control for alternative explanations, whereas substudies III and IV focus mostly on bivariate associations between measures of SES, crime and victimization, and relative socioeconomic differences detected with different outcome variables. Additionally, Substudy IV has a methodological orientation related to measurement of victimization.

There is a clear tension between social selection and social causation perspectives in criminology (Kivivuori & Bernburg 2011), and intensified theoretical critique (Gottfredson & Hirschi 1990) and methodological development (Morgan & Winship 2007) have meant that a researcher wishing to prove empirically that low SES *causes* crime has to take selection arguments into account. Substudy I employs a “traditional” elaboration model, where both confounding (prior crime) and mediating (low income and unemployment following lack of education) variables are controlled to obtain a more realistic estimate of the adjusted effect of each SES variable on crime. However, this modeling strategy is problematic with register data due to the lack of measurement for variables measuring major competing hypotheses. Even with a longitudinal set-up, the possible early onset of criminal behavior, especially among chronic offenders, makes direction of causality difficult to establish. As an attempt to overcome some difficulties related to selection effects, Substudy II uses a more advanced within-individual design, where only within-individual variation can contribute to the estimated unemployment effect on crime. Using each individual as his or her own control in a panel data setting provides a way of dealing with unobserved stable between-individual heterogeneity (Allison 2009). Unemployment was chosen as the independent variable due to both theoretical interest and methodological (daily information available) reasons.

Substudies III and IV approach the question of socioeconomic differences in violent offending and victimization from a comparative angle – these studies do not attempt to answer whether low SES causes crime, but instead assess whether socioeconomic differences vary by type of violent offense (Substudy III) or by seriousness of violence (Substudy IV). Thus, the focus is shifted from the independent variables to the measurement of

outcome, and its implications on conclusions derived from prior studies. Substudy III examines different types of male-perpetrated violence, and the outcome is altered by gender of the victim and place of occurrence. Cases tagged as male-to-female domestic violence by the police are analyzed separately. The aim of this analysis is to see whether socioeconomic differences in violent offending are different by type of violence committed, with special focus on differences in male-to-male and male-to-female violence. Finally, Substudy IV contrasts the results on socioeconomic differences in violent victimization derived from the register-based data to those from the crime victimization survey, to see what the implications of hidden crime might be for register-based study of victimization. Here, the comparison is done by altering the severity of violence measured, as we know that at least the SES-crime gradient is sensitive to seriousness of crime measured (Farnworth et al. 1994). In addition, it is likely that register-based data reaches the most serious cases of violence better. This means that a blunt comparison between the two would confound the possibly different “domains of behaviour” (Hindelang et al. 1979) tapped by each methodology. However, if no common ground between the two data is found, the validity of register-based data to study victimization is brought into question.

5 DATA AND METHODS

5.1 Description of Risk Factors of Crime in Finland -dataset

The project Risk Factors of Crime in Finland (RFCF) was initiated in 2008 by the National Research Institute of Legal Policy with funding from the Finnish Ministry of Justice. Given the lack of such projects before, it was started as a feasibility study to investigate the possibilities of register-based research on determinants of crime in Finland.

The primary data, a random sample of 150,010 Finnish residents, was obtained from the Population Information System administered by the Population Register Centre. The criterion for inclusion was that the person was alive on 31.12.2003. The sample was stratified by gender and age so that young men aged 15–29 at the top of the age-crime curve were over-sampled in order to assure reliable estimates for the most criminally active group. In the analyses, the differential sampling ratios were weighted accordingly to ensure that the results would represent the actual target population. The personal identification number, that all Finnish citizens and foreigners who have lived in Finland for at least a year have, was used to combine data from different registers to the primary data.

The final sample used in the analysis contains information from registers of Statistics Finland, the Finnish Tax Administration, and the Social Insurance Institution of Finland. Statistics Finland provided information about occupation and education at the end of years 2000 and 2004 for every individual. Information about taxable income during years 2004–2006, from both work and assets, was obtained from the Tax Administration. The Social Insurance Institution of Finland provided information about unemployment benefits (basic unemployment allowance and labor market subsidy) and disability retirement during the years 1999–2006. In addition to this data, the primary data from the Population Information System included basic demographic information about gender, age, marital status, number of children, and municipality they currently live in.

The data on crime was gathered from two registers maintained by the National Research Institute of Legal Policy. Originally, the data on convictions and fines comes from the Legal Register Centre, and data on police-reported violence comes from the Police. The data on convictions was available from 1999 onwards, data on fines from 2001 onwards, and data on police-reported violent crime from 2005 onwards.

Different substudies used the variables describing socioeconomic and demographic characteristics of the individuals in slightly varying ways. However, as a rule, in each of the substudies, independent variables were

measured in or before the year 2004, to ensure the correct temporal order between determinants and outcomes. Thus, information about crime used as the outcome was from 2005 onwards. The exception is Substudy II, where the data on crime and unemployment was set up as a 3-month interval panel data, and the contemporaneous association between unemployment and crime was analyzed. Substudies analyzing violent crime only (III & IV) used police-reported violent crime as the outcome. Substudies that analyzed several types of crime had convictions and fines as outcomes, because police-reported crime was not available for all penal codes. Stata 10.1. (StataCorp 2007) was used to conduct the statistical analyses.

Table 1 List of register sources and information used in the substudies

| Source | Years | Variables |
|---|-------------------------------------|--|
| Population Register Centre | | Gender, date of birth, date of death, country of origin, date of moving abroad, number of children, marital status, mother's age |
| Statistics Finland | 2000; 2004 | Education Occupation-based social class |
| Tax Administration | 2004–2006 | Taxable income from work and assets |
| Social Insurance Institution | 1999–2006 | Unemployment (Basic Unemployment Allowance, Labour Market Subsidy) Disability retirement |
| National Research Institute of Legal Policy (original sources Legal Register Centre and Police) | 1999–2008 2001–2008 2005–2007 | Convictions Fines Police-reported violence |

5.2 Substudy I

The aim of the first substudy was to examine socioeconomic differences in three types of crime using all the available measures for SES. The sample was restricted to 28,485 males and females who were 19–30 years old at the baseline (31.12.2004). Those born abroad were excluded as the reliability of register-based measures, for example educational degrees from abroad, could not be confirmed, and the comparability with Finnish degrees is thus questionable. Those who died or migrated abroad before baseline were also excluded.

Substudy I examined socioeconomic differences in three types of crime: violent crime, property crime, and driving while intoxicated (DWI). The first two are based on broad classifications of crimes by Statistics Finland (Statistics Finland 2005), where the most common offenses are assault

and petty assault for violence (89 % of all police-reported crimes against life and health), and theft and petty theft (57 % of all police-reported crimes against property in 2011) for property (Statistics Finland 2012; see Appendix for a total list of offenses included). DWI convictions combine penal codes “driving while intoxicated” (47 % in 2011) and “driving while seriously intoxicated” (53 % in 2011). Convictions and fines for these offenses were followed from 1.1.2005 to 31.12.2008.

This study used all the available measures for SES. The education variable measures the highest educational qualification, and is coded in three groups: basic education or no education, upper secondary vocational school, and general upper secondary school or higher. Current occupation-based social class is based on the International Standard Classification of Occupations (ISCO-88), recoded into four categories: upper white collar, lower white collar, manual (including persons permanently employed in the armed forces), and no occupation. Annual income comprises earned taxable income (salary) recoded into quintiles. Given that some of the subjects reached the age of 18 during 1999–2004, only then becoming eligible for unemployment allowance, the measure for days unemployed (including both basic unemployment allowance and labor market subsidy) was adjusted for exposure to unemployment by dividing the number of days unemployed by the possible number of days available for work during the time period. This variable was recoded into quartiles, the group with no unemployment history being the reference group.

In addition to SES, there were controls included for other, possibly confounding, demographic factors that are having been born to a teenage mother (under 18 when the child was born), marital status, and having children. Having children was disaggregated by age when the first child was born (over or under 20 years of age). There were also controls for crime during 1999–2004, i.e. prior to baseline. To investigate the possible continuity in offending, the same categories of crime (violence, property, and DWI) were used as dummy variables to indicate a prior conviction in such crime.

Cox proportional hazards regression (Kleinbaum & Klein 2005) was used as the statistical model to take timing of offenses into account. For each crime outcome, the subjects were followed until their first offense. The model allows for censoring of cases due to deaths and migration and allows these individuals to be included in the follow-up before censoring, ultimately at the end of the four-year follow-up or when committing a crime. 284 people migrated abroad during the 4-year follow-up period and 102 people died. As all individuals could commit all three types of offenses (in theory), a competing risks model was not used, but a person remained

uncensored in models for two other types of crime if he/she committed one type of crime. The results were reported as hazard ratios with their 95 % confidence intervals.

5.3 Substudy II

The aim of the second substudy was to examine within-individual variation in unemployment status and three types of crime. For Substudy II, the RFCF data was rearranged as a repeated-measures panel data with three-month measurement intervals spanning years 2001–2006. The number of days unemployed and the crime count was obtained for each of the 24 periods. Foreign-born individuals were excluded from the analysis, and the focus was only on male unemployment and crime. If a person died or migrated abroad, the subsequent periods were dropped from the analysis. Similarly, periods of incarceration and disability retirement (outside the labor force) were excluded. The main subsample used in the analysis consisted of 15,658 men who were 20–30 years old in 2001. After taking censoring into account, the total number of observations ($N \times T$) was 367,645.

Similar to Substudy I, the outcomes were based on convictions and fines. In addition to violent crime, property crime, and DWI, this study included a variable measuring count of all crimes during a three-month period. The crimes were classified according to the date of the offense, not that of the conviction. Whereas substudy I combined all unemployment benefits received into one measure of unemployment length, Substudy II took a more detailed look at different forms of unemployment. The data from the Social Insurance Institution of Finland (KELA) includes all periods of basic unemployment allowance and labour market subsidy from 2001–2006. While the size of benefits received is only slightly different during these two, labour market subsidy is means-tested (e.g. spouse's income can affect the money received) and is meant for both those without sufficient work history and the long-term unemployed. If a person receives basic unemployment allowance for 500 days without getting a new job, he or she will start receiving labour market subsidy instead. Unfortunately, the data did not include information about earnings-related unemployment allowance, which is the type of support one receives instead of basic unemployment allowance in the case that he or she has been paying unemployment insurance while working. However, most unemployed youth receive their money from KELA, and more importantly, the measure used should capture those unemployed experiencing greater financial strain, as earnings-related unemployment allowance is typically much larger than basic unem-

ployment allowance (KELA 2005). Given that unemployment was measured instead of employment, several robustness checks were presented to assess the possibility that the heterogeneity of the reference category (not unemployed) biases the results. Additionally, the effect of length of unemployment was analyzed.

In the second part of the analysis, the focus was only on periods of labour market subsidy. As the data includes both active and passive periods of labor market subsidy, we could assess whether active labor market policies (ALMP) affect crime. Importantly, the money unemployed youth receive is roughly the same during both periods, but during active periods, the youth are required to take part in labor market training, subsidized work, and other activation measures. If the unemployment effect is related to social control and routine activities, instead of just on economic situation, we should expect to see lower levels of crime during activation periods (Andersen 2012; Fallesen et al. 2011). This analysis focused only on 18–24-year-olds, as those youth are subjected most intensely to ALMP's.

All analyses in Substudy II were based on fixed-effects (FE) regression models (Allison 2009). This strategy was adopted to account for possible selection effects related to the unemployment-crime link. If the unemployment-crime link is brought about solely by stable between-individual differences (Gottfredson & Hirschi 1990), where selection into unemployment accounts for the correlation between unemployment and crime, we should not see levels of crime changing by the *current* unemployment status. If self-control is a stable characteristic, we cannot use it to explain why the *same person* would commit more crime while unemployed, without resorting to alternative explanations, such as opportunity structure, low social control, strain, or economic problems. In effect, FE model conditions explain away *all stable observed and unobserved* between-individual variation, and only models within-individual change in both unemployment and crime (Allison 2009). Given that the outcome was a count variable, we used FE Poisson regression with robust standard errors (to adjust the standard errors for over-dispersion, see Wooldridge 1999) to obtain the within-individual estimates. Additionally, FE logistic models with binary outcome variables were fitted as robustness checks.

5.4 Substudy III

The third substudy examined socioeconomic differences in violence committed by males, disaggregating the outcome variable by gender of the victim and place of occurrence. The RFCF sample for the third substudy com-

prised 48,063 males who were 19–50 years old at the baseline. Like substudies I and II, foreign-born individuals were excluded from the analysis.

The outcome variables were based on all incidents of police-reported crime during 2005–2007. The penal codes included petty assault (Finnish penal code 21:7 §), assault and attempted assault (21:5 §), aggravated assault and attempted aggravated assault (21:6 §), and attempted homicide (21:1–2 §). The outcome variables were disaggregated by both gender of the victim and place of occurrence, and additionally, those cases that the police coded as domestic violence were treated separately. In the analysis, five dichotomous outcome variables were used: male-to-male violence in private places, male-to-male violence in public places, male-to-female violence in private places, male-to-female violence in public places, and male-to-female violence, coded as domestic violence by the police. Although the variables were not mutually exclusive by definition, (i.e. if a violent incident in a public place had both male and female victims, it contributed to both male-to-male and male-to-female outcomes), 92 % of the cases had only one victim, and for this reason outcomes were overlapping due to the same case being used twice, though only to a small extent.

The education measure used in the analysis was the same as in Substudy I. Due to the inclusion of 31–50-year-olds, the measure for both unemployment and income were altered slightly. The unemployment measure (between years 1999–2004) was now divided into the categories *no unemployment*, *under one year*, *over one year*, and *disability retirement*. Income quintiles were created separately for each cohort to reflect income relative to age. Marital status was also included as a covariate. Prior crime and convictions were measured with two dummy variables: the first indicating an unconditional prison sentence during 1999–2004, the second indicating a prior conviction for violent crime during the same period prior to baseline.

Like Substudy I, this study took advantage of the exact dates of both crimes and censoring due to deaths or emigration, and used Cox regression as the primary statistical model. Age was modeled with linear and quadratic terms, to allow a possible curvilinear association with age and violent crime. SES-age interactions were also tested, to assess the possible age-graded effect of SES on violence. The significance of interactions was tested with likelihood ratio tests. The results from these models were reported as hazard ratios and their 95 % confidence intervals. Additionally, logistic regression models (Long & Freese 2006) were used to assess the joint effect of all SES variables on violence, and predicted probabilities from these models were used to assess differences between opposite ends of SES distribution on both absolute and relative scales.

5.5 Substudy IV

The last substudy, a comparison of socioeconomic differences in violent victimization using register-based data and survey data, used the Finnish National Victimization Survey from years 2003 and 2006 (N=8,562) to complement the register-based analysis. The two sweeps of FNVS were combined to ensure sufficient statistical power. In this substudy, we included all males and females 19–50 years old at baseline (N=65,010) from RFCF data in the analysis. Unlike other substudies, foreign-born individuals were also included, as it was impossible to exclude them reliably from the FNVS data.

In FNVS, respondents are asked screening questions about violent victimization during the past 12 months, and those individuals with such experiences are asked additional detailed questions about a maximum of their three latest incidents. This incident-level information was used to create the outcome variables for violent victimization. In the RFCF data, the outcome variable was based on police reports of violence (same penal codes as in Substudy III), where we could identify the victim in each case. A three-year follow-up was used to ensure a sufficient amount of individuals registered as victims of violent incidents. In the model comparing only police-reported violence, the register-based model used one-year follow-up to match that in the survey data.

While the FNVS data includes several self-reported SES measurements, older sweeps of the data also include register-based measures of education, main economic activity, and occupational social class. To ensure better comparability between the two data, these register-based measures were used to contrast the survey-based results to those obtained from the RFCF data. Given that the FNVS data does not include a register-based measure of income, a self-reported measure of household income was used instead. Register-based measures included education, occupation-based social class, income quintile, and unemployment length. Of all measures, educational level was best comparable between the two data, as it is based on the same primary register source (Statistics Finland). The univariate distributions of education were highly similar in both data. While the other measures were not directly comparable, as they could not be categorized in the exact same way, they still tap into the same dimensions in both data. However, this difference between the measures in the two data needs to be taken into account when interpreting the results.

Logistic regression models, with controls for age and gender, were used to examine the association between each independent variable and victimization separately. Fully adjusted models were not used, as the two data do not contain the exact same variables. Within both data, the outcome vari-

ables were changed in each step from more inclusive to less inclusive to capture violence that is more serious. By altering the relative inclusivity of the violence outcome in each data, we could assess the sensitivity of detected socioeconomic differences to the definition of violence. The categories were not mutually exclusive – the first outcome included all victimizations, and those who experienced only less serious violence were excluded from the next outcomes. In other words, the more serious outcomes were nested within the outcomes that also included less serious violent incidents. Finally, we did a comparison of those individuals who answered that they reported their violent victimization to police (FNVS) with those individuals who we know were registered as victims by the police (RFCF).

Table 2 Summary of datasets used in each substudy

| Substudy | Outcome | Follow-up period | Age at baseline | Gender | N | Foreign-born included |
|----------|-----------------------|------------------|-----------------|--------|-------------------------|-----------------------|
| I | Convictions and fines | 2005–2008 | 19–30 | M/F | 28,485 | No |
| II | Convictions and fines | 2001–2006 | 18–30 | M | 15,658 | No |
| III | Police | 2005–2007 | 19–50 | M | 48,063 | No |
| IV | Police Self-report | 2005–2007 | 19–50 | M/F | 65,010 (R) 8,562 (S) | Yes |

5.6 Ethical Considerations

Data protection guidelines and ethical regulations approved by the data protection authorities and the National Research Institute of Legal Policy in the collection, use, and reporting of the data were followed. The study was conducted according to a research plan, and the guidelines of the Finnish Advisory Board on Research Integrity (Tutkimuseettinen neuvottelukunta 2009). The Population Register Centre, Statistics Finland, Social Insurance Institution of Finland, Tax Administration, Legal Register Centre, Police, and ultimately the data protection authorities provided the permissions to use the register-based data.

6 RESULTS

In the following, the main results from the four substudies are summarized under separate headings for each substudy. More detailed, descriptive statistics and the complete statistical models are available in the original articles.

6.1 Four Measures of SES and Different Types of Crime (Substudy I)

The first substudy examined socioeconomic differences in convictions for violent crime, property crime, and driving while intoxicated (DWI) among 19–30-year-old males and females. Of the entire sample, 4.8 % were convicted of at least one of the three during the four-year follow-up: 77 % of those committed only one type of offence during the follow-up, and 6 % were convicted of all three during 2005–2008.

Bivariate analysis showed that the SES variables, education, income, unemployment length, and occupation-based social class indicated a clear SES gradient in participation in all three types of crime (for gender-specific crime rates, see Table 3). Generally, the differences were more pronounced when the number of separate convictions was analyzed instead of prevalence. Looking at hazard ratios (HR) from crude Cox regression models, controlling only for gender, age, and each SES variable separately, the differences in all types of crime were largest by educational level. Those with only basic education had a HR of 49.3 (property), 14.2 (violence), and 11.2 (DWI) when compared to the reference group of those with upper secondary education or higher. The respective HRs for youth with vocational education were 7.3, 3, and 3.8. Particularly in property crime, there were also major differences by opposite ends of income (16.5), unemployment (7.4) and occupation-based social class (21.7). Although smaller, the crude HRs were of considerable magnitude in violence and DWI as well. Additionally, the crude models showed the continuity in offending: those individuals that had prior convictions of any of the three types of crime during 1999–2004 committed new crimes with a much higher probability than those who did not have prior convictions.

Table 3 Gender-specific crime rates (total number of convictions and fines in each crime type divided by number of person-years) by measures of SES during 2005–2008

| | | Males | | | Females | | |
|---------------------------|----------------------|--------------------------|----------|------|--------------------------|----------|-----|
| | | Crimes/1000 person-years | | | Crimes/1000 person-years | | |
| | | Violence | Property | DWI | Violence | Property | DWI |
| Education | Upper sec. or higher | 2,1 | 0,4 | 2,6 | 0,1 | 0,1 | 0,3 |
| | Vocational | 8,3 | 10,3 | 13,0 | 1,0 | 0,4 | 1,3 |
| | Basic education | 35,8 | 57,3 | 48,3 | 8,1 | 10,4 | 5,4 |
| Income 2004 | 1st quintile | 3,9 | 1,5 | 6,5 | 0,0 | 0,2 | 0,2 |
| | 2nd quintile | 8,4 | 5,9 | 12,7 | 0,5 | 0,3 | 0,9 |
| | 3rd quintile | 11,6 | 8,7 | 13,3 | 0,9 | 0,7 | 0,8 |
| | 4th quintile | 13,9 | 18,4 | 19,6 | 3,0 | 3,0 | 1,5 |
| | 5th quintile | 22,2 | 49,9 | 31,9 | 2,9 | 3,9 | 3,4 |
| Unemployment 1999-2004 | No | 6,1 | 8,7 | 9,1 | 0,5 | 0,7 | 0,9 |
| | 1st quartile | 13,7 | 13,7 | 15,8 | 0,5 | 0,4 | 0,5 |
| | 2nd quartile | 13,3 | 28,9 | 16,7 | 1,5 | 2,2 | 0,2 |
| | 3rd quartile | 16,7 | 32,0 | 31,6 | 3,0 | 1,4 | 2,6 |
| | 4th quartile | 32,0 | 30,4 | 39,9 | 5,8 | 7,2 | 3,9 |
| Occupation 2004 | Upper white collar | 2,1 | 0,2 | 3,8 | 0,2 | 0,5 | 0,4 |
| | Lower white collar | 4,1 | 5,4 | 5,0 | 0,5 | 0,2 | 0,7 |
| | Manual | 8,4 | 6,0 | 12,7 | 1,4 | 0,2 | 1,3 |
| | No occupation | 20,0 | 34,7 | 27,3 | 2,8 | 3,5 | 2,1 |

Although the four SES variables are correlated, when they were mutually adjusted in model 1, all the variables retained their statistically significant associations with all three types of crime. The effect of having a basic education remained the strongest by far, as adjustment for income, occupation-based social class, and unemployment history only partly explained the education effect. This implies that the education effect is explained only partly by later labour market failure and low income. When other demographic variables, marital status, having children, and having been born to a teenage mother were controlled in model 2, the effects of SES variables were altered only slightly. However, when the controls for prior convictions were included in model 3, the effect of SES variables was reduced greatly. Although this is a rather crude test for selection effects, the fact that the effects of low income and having no occupation disappear indicates that prior crime has a major effect on these measures. In other words, we cannot rule out the possibility of reverse causation, where prior crime causes low income and lack of occupation instead of the other way round. The effects of education and unemployment, on the other hand, remained

significant. The fully adjusted models showed that continuity in offending was somewhat crime-specific, as a prior conviction in one type of crime generally predicted a similar crime in the future.

In the last analysis, Substudy I examined the possible gender-specific interaction effects of having low SES on multiple measures. The simplified Cox models indicated that the relative effect of having only basic education was stronger for women, meaning that females with criminal convictions were clustered more strongly in the low education group than similar men. However, the interaction effects for combinations of SES variables were mostly insignificant, and when significant, the models did not indicate that having low SES on several measures would have an added (over the main effects) effect on crime for males. While the interactions were not significant for females, models show some indication that having low SES on multiple measures would increase levels of female crime more. Altogether, the combined effects of two SES variables were generally larger for violent crime and property crime by women.

6.2 Within-Individual Variation in Unemployment and Crime (Substudy II)

The second substudy focused solely on the temporal association between unemployment and crime using FE regression models to obtain a within-individual estimate on the effect of unemployment on different types of crime. During the six-year follow-up, 41.5 % of men in the sample committed at least one crime resulting in conviction or fine. This large share is explained by minor traffic offences. 5 % were convicted of property crime, 4 % of violent crime, and 6 % of DWI. Regarding unemployment during follow-up, 27 % were registered as unemployed for at least one day, with a mean of 119 days during six years. In the restricted sample focusing on ALMPs and crime, the mean number of days on the active labor market subsidy was 30 (prevalence 17 %), and 65 days on the passive subsidy (29 % prevalence). Thus, both crime and unemployment were relatively common during the follow-up.

Like substudy I showed, there is a relatively strong bivariate association between unemployment length and crime, and the only exception to the otherwise linear association is the group with the longest unemployment, which had a lower rate of crime than expected. A pooled model, where all three-month periods were combined into one sample that ignores the clustered nature of the data, showed the same thing: if between-individual differences are included, there was a strong association between all types of

crime and unemployment status. However, when a FE Poisson model was used instead, the results looked very different (Table 4). In the within-individual model, the same individual no longer committed more violent crime ($\beta=0.03$, $SE=.012$) or DWI ($\beta=0.13$, $SE=0.09$) while unemployed than while not. Property crime, on the other hand, was still associated with unemployment status at the time: property crime rate is 43 % ($IRR=e^{0.36}$) higher ($\beta=0.36$, $SE=0.12$) during periods when the person was unemployed for at least one day. All crime outcome was also related to unemployment with $\beta=0.18$ ($SE=0.05$). Specifying unemployment during a period differently (for at least one month or with linear terms describing the number of days unemployed) did not change the substantive interpretation of the results. Running the same analysis with a logistic FE model or with a lagged (t-1) unemployment specification produced similar results. Longer unemployment length increased the risk of property crime, but still did not exhibit a dose-response association with violent crime or DWI.

Table 4 Pooled and fixed-effects estimates of temporal variation in unemployment and crime (regression coefficient, standard error in parenthesis). In ALMP model, the within-individual estimate contrasts ALMP period with a period of “passive” labor market subsidy.

| | All crime | Property | Violence | DWI |
|-------------|----------------|----------------|----------------|----------------|
| Pooled | 1.22*** (0.07) | 1.98*** (0.12) | 1.67*** (0.10) | 1.58*** (0.10) |
| FE Poisson | 0.18** (0.05) | 0.36** (0.12) | 0.03 (0.12) | 0.13 (0.09) |
| FE Logistic | 0.01 (0.04) | 0.25*** (0.07) | 0.07 (0.11) | 0.11 (0.09) |
| <i>ALMP</i> | | | | |
| FE Poisson | -0.26 (0.13) | -0.50* (0.22) | -0.02 (0.41) | -0.36 (0.29) |

The second part of the analysis examined levels of crime during participation in ALMPs versus “passive” unemployment. Only periods of labor market subsidy were included in the FE analysis. The results indicated that property crime rates ($\beta=-0.50$, $SE=0.22$) were lower during participation on ALMPs, despite the fact that the benefit size does not change considerably (the mean incomes during active and passive labor market subsidy were very similar). The coefficient for DWI was also rather large ($\beta=-0.36$, $SE=0.29$), but not statistically significant due to small sample size. Thus, the findings of Substudy II show that the association between unemployment and property crime cannot be accounted for by stable between-individual heterogeneity. On the other hand, we could not confirm that unemployment would increase rates of violence and DWIs in this sample. While the fact that we only find an association between unemployment and property crime would suggest that lack of economic resources might explain the unemployment effect, the more restricted ALMP analysis hints

that social control and routine activities are likely to play a role as well, as financial situation should be largely similar during active and passive periods of labor market subsidy. All in all, it appears that selection into unemployment, based on stable unobserved factors, plays a major role in creating the unemployment-crime association in young adulthood.

6.3 Male Violence by Victim's Gender and Place (Substudy III)

The third substudy focused on violent crime, examining the relative importance of SES and prior crime as explanations of different types of police-reported violence committed by 19–50-year-old males. 1,557 (3.2 percent) were suspected of 2,506 violent offences during the three-year follow-up. Male-to-male violence in public places was the most common offense type (1.3 percent suspected), followed by male-to-female violence in private places, with one percent of males suspected. Descriptive analysis showed that the prevalence of male-to-male violence in public places decreased clearly with age, but other types of violence did not manifest similarly clear age-crime curves and instead remained more stable with age. Due to these possible non-linearities, age was modeled with both linear and quadratic terms to allow a curvilinear association between age and type of violence.

Crude models, where all the independent variables were entered separately controlling only for age and age squared, showed that all five types of violence were more common in the lowest SES groups. Even though the upper age limit of the sample was increased from 30 to 50 years of age, having only basic education (vs. upper secondary or higher) remained the SES measure with the strongest effect (HRs from 5.6 to 19.7). Likewise, those with either a prior unconditional prison sentence (HRs from 10.0 to 31.3) or a prior conviction for violent crime (HRs from 10.2 to 20.9) had a vastly higher probability of committing any of the five types of violence. However, the effect sizes of both SES and prior crime variables differed by type of violence. In general, violence in private places appeared to be more determined by low SES of the offender, whereas violence in public places appeared more random, while still way more common in low SES groups. These differences by place of occurrence were more marked in male-to-male violence, whereas the difference in predictors of violence with female victims was lesser between violence in public and private places.

Table 5 sums up the main findings from the analysis. Instead of comparing every coefficient between the models, predicted probabilities from logistic regression models were used to assess the contribution of SES and

prior crime on both relative and absolute scales. Here, those who had low SES on every measurement (basic education, long unemployment and lowest income quintile, $n=1,064$) were compared to those who had high SES on every measure (upper secondary or higher education, no unemployment and highest income quintile) to see how the probability of violence changes when we move from lowest to highest SES group. The same was done for variables indicating prior crime (prison or violence conviction). The results showed that the probability of violence changed the most in male-to-male violence in private places between opposite ends of composite SES scale in relative terms. This was followed by male-to-female violence in private places. If an absolute scale was used, these were reversed. The results on absolute scale highlight the fact that being reported to police because of violence is very uncommon, even in the group scoring the lowest on the composite SES scale. Regarding violence with a female victim, low SES contributed the least to violence in public places, slightly more to cases coded as domestic violence by the police, and the most to violence in private places. Prior criminality, on the other hand, contributed the most to violence in public places in violence with female victims, whereas violence with male victims in private places was associated with prior criminality the most: one third of those men with lowest composite SES, prior prison sentence, and prior conviction for violent crime, committed such violence during the three-year follow-up. Overall, it seems that all types of examined police-reported violence are much more common among men in low SES groups.

Table 5 *The absolute and relative differences in predicted probabilities in different types of violent between a) composite low and high SES groups b) those with prior prison sentences/violent convictions and those without*

| Victim gender | Type | SES* | | Prison/Violence** | |
|---------------|----------|----------|----------|-------------------|----------|
| | | Absolute | Relative | Absolute | Relative |
| Male | Private | 2,3 | 28 | 7,7 | 25 |
| Male | Public | 2,0 | 8 | 9,6 | 17 |
| Female | Private | 3,3 | 18 | 3,8 | 8 |
| Female | Public | 0,8 | 9 | 2,4 | 13 |
| Female | Domestic | 1,3 | 11 | 1,6 | 7 |

* Low SES (low education + low income + long unemployment vs. high SES (high income + upper secondary or higher education + high income)

** Both prior prison + prior violent conviction vs. no prior prison or violent convictions

In the last models, the possibly age-graded effects of SES on types of violence were tested. In these models, the only distinction was made between violence against males and females to gain statistical power and reduce the number of models to estimate. The interactions were tested separately for all the three SES variables. Likelihood ratio tests showed that model fit

improved in three of the six models when the effect of SES was allowed to vary by age. The results from these models show that the effect of education was the most robust with age. However, in male-to-male violence, allowing the interactions altered the results so income and unemployment had no effect among the youngest individuals. Thus, there is some evidence that male-to-male violence between males that often happens in public places might be more random with relation to SES. However, this finding might also reflect measurement issues, as measurements for both income and unemployment do not necessarily properly tap into actual low SES among the younger individuals. Despite finding some indication of age-interactions, the effects of low SES remained surprisingly stable with age.

6.4 SES and Violent Victimization in Two Data Sources (Substudy IV)

The last substudy examined measures of SES as predictors of violent victimization in survey (FNVS) and register (RFCF) data among 19–50-year-old Finnish men and women. While substudy III altered the outcome variable by victim gender and place of occurrence, here the interest was in the seriousness of the violence. This was done for multiple reasons. First, a crude comparison between a survey-based measure of victimization and a measure based on police reports of violence would conceal the seriousness of violence. Second, seriousness of violence is related to likelihood of police reporting, implying that more serious violence can be more reliably measured with official data. The analysis was conducted separately for 19–30-year-olds and 31–50-year-olds.

In the survey data, 14 percent of respondents reported some type of violent victimization during last 12 months. Eight percent reported physical violence, and only three percent violence resulting in a physical injury. When the most inclusive category was used as the outcome in (controlling for age and gender) crude logistic regression models, low education and low income emerged as predictors of violent victimization in both age groups, whereas occupation-based SES and being on disability pension were significant only in the older group. When those individuals who only experienced threats during the last year were removed from the outcome variable, the results remained for most part similar. However, when the outcome variable only included individuals who experienced violence resulting in physical injury, the most serious violence in the survey context, the differences between SES groups became more marked, particularly in

the older group. In addition to increased effects of low education, low income, and lower white collar/manual worker position, the unemployed had a greater risk of physical violence resulting in injury.

If the survey analysis showed increasing socioeconomic differences with increasing seriousness of violence, in the register-based analysis SES variables had a major effect in the more inclusive outcome variable. Low education (OR 9.3) and long unemployment (OR 3.9) had the strongest effect in the younger group; in the older group, being in the lowest income quintile was also a major risk factor (OR 4.8). Once those who experienced only minor assault or assault are excluded, the effects grew even stronger. The odds ratios for having only basic education (35.6; 20.0) were inflated due to low number of cases of most serious forms of violence in the university-educated group of individuals in both age groups. In the older group, having low income (OR 15.5) or long unemployment (OR 13.9) were also strong predictors. The results on the effect of education with all five outcomes (three survey, two register) are summarized in figure 2.

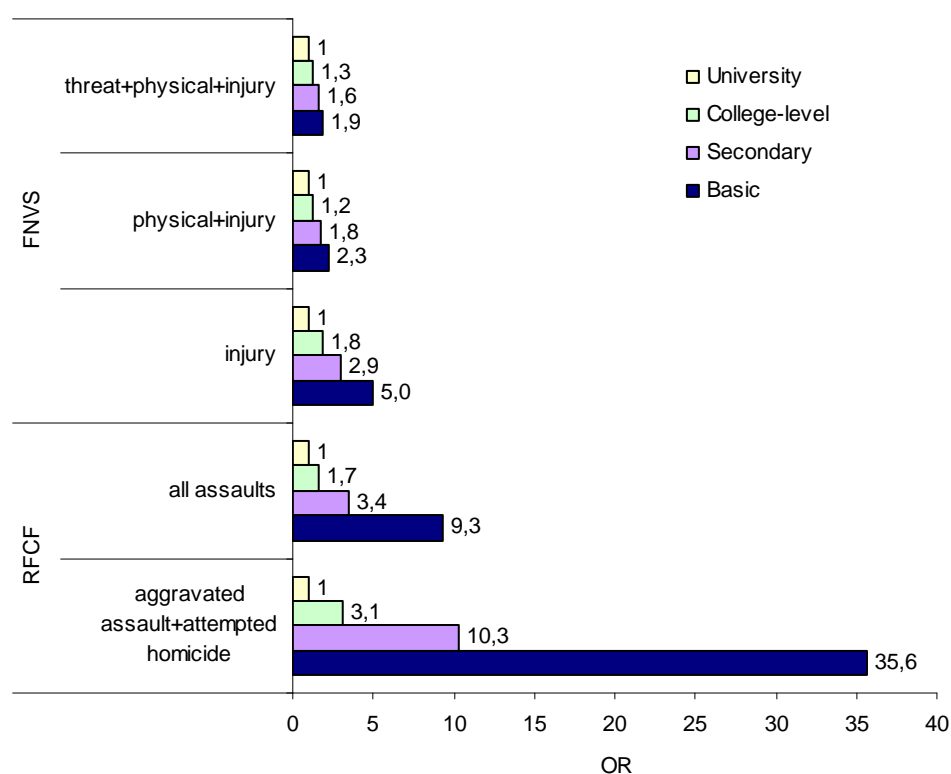


Figure 2 The odds of violent victimization by education with outcome variables of different severity in survey (FNVS) and register (RFCF) data (odds ratios from crude logistic regression models controlling for age and gender)

In the final part of the analysis, a more direct comparison between the two datasets was attempted. In the survey data, the outcome was restricted to only those cases that were, according to the respondent, reported to the police. Education, the only comparable SES measure, was used as the sole determinant to assess socioeconomic differences in this modified survey outcome and actual police-reported violence during one year in register-based data. Interestingly, the prevalence of victimization after this modification is identical (0.9 percent) in the two data. However, this analysis indicates that survey data is likely to underestimate socioeconomic differences in police-reported violence, as the odds ratio associated with basic education was 3.96 in survey data and 6.37 in register-based data. To sum up the results from the analysis, it appears that the two data give roughly similar pictures on the relationship measures of SES and seriousness of violence: the more serious violence we measure, the more selected the victims are in terms of SES. A key difference between the datasets was the share of female victims that was much larger in survey data.

7 DISCUSSION

7.1 Socioeconomic Differences in Crime and Victimization

The main finding of this study is that young adults who perpetrate crime and are victims of violence are concentrated in low SES groups. This is true for all examined crime types, but particularly so for property crime and violent victimization in more serious incidents.⁸ Of the SES measures used, education has the strongest association with both crime and victimization. The bivariate associations between other measures of SES – income, occupation-based social class and unemployment length – and crime are also of considerable magnitude and in the expected direction. This is despite the fact that we know that most measures of SES in young adulthood are “noisy,” and do not necessarily tap into actual disadvantage or economic hardship (Halleröd & Westberg 2006). Some measurement error among young adults is implied by substudy IV that shows greater effects for income, occupation-based social class, and unemployment length among 31–50-year-olds in both survey and register-based data on violent victimization.

The effect of low education on crime seems particularly robust on the basis of the current analysis, as it persists after controlling for prior crime and possible mediators of low income, low occupation-based social class, and unemployment in Substudy I. However, it remains unclear if the comparatively strong effect of education after controlling for other SES measures partly reflects noisy measurement of other SES variables. On the other hand, it is also evident that selection effects play a substantial role in creating the strong association between SES in young adulthood and crime. When within-individual variation in unemployment status and crime is analyzed, only property crime is more frequent during spells of unemployment. This implies that the initial association between unemployment and violent crime/DWI is a product of either a) unobserved between-individual differences or b) time-varying variables not measured.

A closer look at socioeconomic differences in violent crime shows that the SES-violence association, regarding both offending and victimization, is dependent on type of violence examined. When male violence was disaggregated by gender of the victim and place of occurrence, socioeconomic differences were greatest in male-to-male violence in private places, followed by male-to-female violence in similar settings. While also exhibiting

⁸ Another study using RFCF data found out that the effect of low SES on violent offending is similarly dependent on the seriousness of violence measured (Aaltonen 2010)

major differences by perpetrator SES, especially male-to-male violence in public places appears slightly more random in terms of offender SES and prior criminality. Low SES and prior criminality appear as risk factors for police-reported male-to-female domestic violence as well. Regarding the association between SES and violent victimization, the analysis shows that the detected relationship is highly sensitive to inclusivity of the violence concept used in both survey and register-based data. If all forms of violence, from verbal threats to aggravated physical violence, are included in the outcome, survey data shows only small differences in victimization risk. However, once the outcome only includes victims of serious violence, in terms of injuries, socioeconomic differences in victimization risk appears much larger. This is especially so in the register-based data, which offers better population coverage in the absence of nonresponse. In line with findings from Finnish homicide research (Kivivuori et al. 2007), victims of aggravated assaults and attempted homicides are a highly selected group in terms of low SES.

While the SES-crime debate (Tittle et al. 1978) focused mostly on family-level measures of SES and adolescent delinquency, the claim that SES-crime-link is a myth is clearly refuted in the current analysis. However, the functional form of the association between measures of SES and crime remains somewhat unclear. Some have suggested that we need to measure “sustained underclass status” (Farnworth et al. 1994) or reach the “truly disadvantaged” (Wilson 1987) to see the SES-crime link. If this is the case, arguing for social exclusion as the cause of crime would be warranted. Such argument would imply minimal differences in crime between the higher income quintiles, and a sudden increase after a certain threshold (Hipp & Yates 2011). On the other hand, later research has shown that at least on the area-level, the association between poverty and crime in USA is best characterized as a diminishing positive effect rather than as an exponential effect or as a threshold effect (Hipp & Yates 2011).

The results from this study are somewhere between these two, yet leaning towards threshold or exponential association. An exponential association is favored by results from substudy I, indicating that the poorest income quintile, especially among men, has a much higher risk of crime than the four other quintiles, and results from substudy III, which show an exponentially higher violence risk poorest quintile in all types of violence. However, there are also traces of gradient-like association; for instance, in substudy III, where there are gradual differences in hazard ratios in all types of violence between income quintiles 1 and 4. Despite this, the overall conclusion is that the functional form of SES-crime association is exponential rather than linear. In many ways, these results resemble those from

Norway analyzing the relationship between family income and children's offending (Galloway & Skardhamar 2010). While these results indicate that the "truly disadvantaged" (Wilson 1987) stand out from the rest, gradual differences across different SES scales warrant continued attention as well (Galloway & Skardhamar 2010).

Although the current study is not strictly a study of social exclusion, and socioeconomic differences were assessed across the entire distributions rather than just focusing on the most disadvantaged end, the joint contribution of several measurements of low SES was assessed in Substudies I and III. Using such combined measure should better reach those in the risk of marginalization. First of all, as the adjusted model in substudy I showed, the effects of all SES variables persist after mutually adjusting for them. This implies that these tap into partly different dimensions, but also that having low SES on several measures results in a higher probability of crime. However, no actual interactive effect, where having for instance both low education and long unemployment history, would have caused crime to increase more than the main effects models predicted. It seems evident that cumulative disadvantage matters in the sense that faring poorly on several measurements increases crime in a dose-response fashion, but the current analysis fails to confirm if such accumulation of problems increases the risk of crime in an interactive way.⁹

Most theories of crime referenced earlier are general theories in the sense that they seek to several types of antisocial or criminal behavior. Even though the current study does not endorse abandoning such a general view of crime, it still shows that disaggregating the outcome variable can sometimes produce rather different results regarding risk factors for different types of crime. For instance, if only one outcome variable measuring all crime was used, substudy II would have not been able to identify the different associations unemployment has with property crime and other types of crime. The same applies to victimization research: disaggregating the outcome by rather simple criteria of seriousness of violence produces strikingly different results regarding socioeconomic differences in violent victimization. While it is important to measure a wide variety of victimization experiences, the conflation of different types of violence to one outcome can make a researcher blind to the shades of grey in the data (Dobash & Dobash 2004). The register-based study of victims of police-reported vio-

⁹ One needs to bear in mind that the interpretation of interactions is different in additive (e.g. linear regression) and multiplicative (e.g. logistic regression) models (Kendler & Gardner 2010; see also Allison 1999).

lence shows that serious violence is highly concentrated in the lowest social strata (see also Aaltonen 2010).

7.2 Three Explanations to Strong SES-Crime Link in Finland

In the following section, tentative interpretations – based on theory and prior evidence – of the causes of empirical results seen here are presented. In general, the argument for causal effects relies on strain theories (Merton 1968), whereas those in favor of self-control theory (Gottfredson & Hirschi 1990) are likely to argue that these are brought about by selection mechanisms. Life-course theories by Sampson & Laub (1993) and Moffitt (1993) would suggest integrating these two. However, the following explanations are not mutually exclusive; it is possible that all are operational.

Causal Effect – Recession and Growing Inequalities

The sheer magnitude of socioeconomic differences, especially by educational level, in crime committed by Finnish young adults makes it hard to ignore the role of SES in explaining crime. With most SES measures and crime outcomes, there is a dose-response association between the two, where low SES and crime are related inversely throughout the SES scale, with the lowest category typically standing out. One explanation for large observed differences by SES is the fact that register-based data offers exceptionally good population coverage. Furthermore, we analyzed relatively serious crime among young adults, not minor delinquency among adolescents. Following Farnworth et al. (1994), we both a) reach those in most marginalized positions and b) measure serious enough crime, and this enables us to detect a substantial association between measures of SES and crime. Additionally, the association between low education and crime persists after controls for other SES measures and prior crime. We also discovered that unemployment is associated with higher levels of property crime when analyzed in a within-individual setting that provides a much more stringent test for selection, due to stable traits rather than traditional cross-sectional models which control for a limited number of third variables (Allison 2009). Of course, despite these strong associations, the models used here are still not able to identify causal effects (Morgan & Winship 2007). The rather obvious mediating effects of alcohol and drug abuse should be investigated in the future, as it is plausible that substance abuse

indirectly connects low SES, especially serious social exclusion, to increased crime, but also to increased risk of violent victimization.

If the estimates from this study are interpreted in a strain (or relative deprivation) framework, the developments in the last two decades could offer insight into why SES and crime have such a strong association. After a period economic boom with low levels of unemployment, the heavy depression in the early 1990s left Finland with a group of persistently unemployed people. Income inequality has grown markedly since the recession (OECD 2011). Tarkiainen et al. (2012) have shown that the life expectancy of the lowest income quintile has increased much less than that of other income quintiles during 1988–2007 in Finland. Furthermore, recession also meant significant cutbacks in some welfare services, such as maternity and child health clinics, healthcare in schools, and childcare assistance provided by municipalities (Salmi et al. 2012). The proportion of children living in families below the poverty line has also increased steadily since the recession (Karvonen et al. 2009). Many individuals examined here are those who grew up during the recession, and it is likely that the aforementioned cutbacks in welfare services had the greatest effect on families with low resources. It is also evident that those young adults who have no secondary education fare very poorly in the labor market, and the relative differences by educational level in unemployment levels are particularly marked among men (Sipilä et al. 2011). Furthermore, intergenerational continuities are certain to play a role in creating the strong association between education and crime; we know that educational qualifications are correlated across generations (Kivinen et al. 2007), and it is likely that the education effect is partly related to the socioeconomic composition of the family of origin (Fergusson et al. 2004; Ring & Svensson 2007).

On the other hand, Finland still ranks high of economic equality in comparative terms (Karvonen et al. 2009). While poverty (Karvonen et al. 2009) is more prevalent now than previously, it would still seem implausible to argue that socioeconomic differences in crime are large in Finland because of high prevalence of *absolute* poverty. Thus, a causal argument for SES-crime link is more firmly rooted if placed in a relative deprivation context: even in the absence of absolute poverty, relative change for the worse can still matter. What seems certain is that at least the demand for low-skill labor has decreased (Myrskylä 2012), and securing employment is becoming increasingly dependent on educational qualifications (Sipilä et al. 2011). If those individuals who were previously employed in low-skill manual labor jobs, and had a greater criminal propensity even then, are now left unemployed and relying on welfare benefits, then increased levels of crime would seem a plausible outcome.

However, the main problem with the argument of the eroding welfare state is that we do not have any clear evidence that crime would have increased as a consequence of developments since the 1990's recession. The number of homicides has decreased during the last two decades (Lehti 2012); victimization surveys (1980–2009, Sirén et al. 2010) and self-report delinquency surveys (1995–2008, Salmi 2009) indicate a rather steady level of violence, and self-report surveys (Salmi 2009) and official crime statistics (Sirén & Salmi 2011) indicate declining levels of property crime. Thus, even though several mechanisms indirectly suggest that the effect of SES on crime could be on the increase, we do not see the overall level of crime actually rising after the deep recession during early 1990s.¹⁰ Furthermore, bearing in mind the several studies that showed no SES-crime association with American data (Tittle et al. 1978; see also Sampson & Lauritsen 1994; Dunaway et al. 2000), the comparatively substantial SES-crime association found in a welfare state like Finland is puzzling (for similar results on Norway, see Galloway & Skardhamar 2010). The fact that the functional form of the association between measures of SES and crime is exponential implies that relative differences, as opposed to a clear distinction between marginalized and others, matter as well. It is possible that the effect of disadvantage is greater when the individual is surrounded with successful others (Galloway & Skardhamar 2010). While relative deprivation appears to affect crime in a Nordic context (Bernburg et al. 2009), it is yet unclear if such effect should be bigger or smaller here than elsewhere (see also Savolainen et al. 2013). To answer these questions, we need both a) research on changes in SES-crime-relationship in Finland and b) comparative studies assessing socioeconomic differences in crime in different countries.

Social Control Deficits – Unintended Consequences of the Welfare State

Instead of relying on strain theory, Kivivuori and Lehti (2006) put forward an alternative explanation for the strong correlation between low SES and lethal violence in Finland. Analyzing long-term homicide trends and socie-

¹⁰ Some Nordic criminologists have argued that an aggregate crime trend might mask a polarized development in youth crime: an increasing amount of youth do not commit any crimes whatsoever, while at the same time crime at the other extreme grows more serious (Kivivuori & Bernburg 2011). Others have suggested that improvements in security technology, and expansion of the security sector in general, explain decreases in property crime (Farrell et al. 2011).

tal changes in Finland, they argue that the decrease in demand of unskilled (male) labor and the welfare state response to the permanent unemployment that followed had “unintended consequences.” Seeking to solve the paradox of why the rise of the welfare state was not coupled with reductions in the rate of homicide, they look at the change in the labor market situation of middle-aged men in the lowest social stratum, who are known to be responsible for the relatively high level of homicide in Finland. Savolainen et al. (2008, 83) argue that “there is nothing exceptional about the Finnish homicide rate outside this narrow sociodemographic context.” During the 1960s and 1970s, these men became increasingly redundant in forestry and construction sectors, as companies could substitute labor force with technology. Instead of offering relief work, the state created income transfers, subsidized housing and unemployment benefits to ensure that these men would not be excluded from the society. These policies helped reduce both absolute poverty and relative inequality. However, the problem with this substitution of work with benefits for the males in the lowest social stratum was that it was not accompanied with new sources of social control. Although work as a source of income was replaced with social security, work as a source of social control was not. (Kivivuori & Lehti 2006).

In addition to the decreased informal social control from work, the introduction of subsidized housing also contributed to the decay of informal social control. Interestingly, as the amount of homeless male alcoholics decreased, homicide similarly “moved indoors” beyond informal social control. This also meant that applying situational crime prevention to these settings became more difficult (Kivivuori & Lehti 2006). The typical Finnish homicide is now characterized as a situation where a group of middle-aged marginalized men drink excessive amounts of alcohol in a private apartment, and the violent disputes that often happen in such settings sometimes have deadly consequences. They argue that relative deprivation is a poor explanation of violence that happens among equally deprived individuals (Kivivuori & Lehti 2006). In a later contribution, Savolainen et al. (2008, 84) develop the theory further and draw parallels with institutional anomie theory, that posits that social policy is expected to reduce violence only when they also “strengthen the capacity of institutions to exercise social control.” While they conclude that the Finnish welfare state has failed to provide alternative forms of social control for these troubled males, they also point out that their theory should not be confused with the “conservative rhetoric that perceives generous welfare benefits as a source of moral decay” (Savolainen et al. 2008, 84).

The two studies by Kivivuori and Lehti (2006) and Savolainen et al. (2008) focus solely on homicide, but we have yet to find out whether this hypothesized “control deficit” created inadvertently by welfare state policies could affect other types of crime as well. On the one hand, it could be argued that the marginalized middle-aged homicide offender does not have much in common with the younger individuals studied here, and results based on homicide studies cannot be extrapolated to the larger population of offenders studied here. Furthermore, it appears that the level of homicide among young adults is not particularly high in Finland. On the other hand, the adverse effect of permanent exclusion from the labor market is consistent with Sampson and Laub’s (1993) theory of informal social control, and it is hard to see why such a mechanism would be limited only to homicide offenders. The results from RFCF data indicate that even younger victims of serious forms of non-lethal violence are also highly selected in terms of education and unemployment history. This applies to offenders in these cases as well (Aaltonen 2010).

Savolainen et al. (2008) propose that the key problem in social policy regarding middle-aged men in lowest social stratum is that society no longer looks at the labour market to improve their situation, and instead prevents them from entering absolute poverty by means of income transfers. This leaves them with “few incentives to pursue a middle-class way of life” (Savolainen et al. 2008). However, like the authors mention, the same cannot be said of state’s response to youth unemployment. At least on a formal level, considerable efforts are made to prevent youth from permanent labor market exclusion. For instance, under 25-year-old unemployed youth without vocational education have to participate in active labor market programs (AMLMP) and apply to schools to stay eligible for certain unemployment benefits. While the overall effectiveness of ALMP’s as labor market measures can be questioned (Calmfors et al. 2002), the results from this study and from recent Danish studies (Andersen 2012; Fallesen et al. 2011) as well, seem to support the control deficit -hypothesis among young adults: crime is lower during the participation in activation programs than while on passive unemployment. Of course, one could argue that the “threat effect” presented by ALMPs is harmful as it is likely to make some individuals not register as unemployed at all. Bearing these reservations in mind, the control deficit hypothesis should clearly be examined further among younger offenders as well. Furthermore, different kinds of labor market reforms could provide interesting natural experiments for researchers interested in the effect of employment on crime. The effects of such reforms on crime could be analyzed in Finland as well (see e.g. Andersen 2012).

Selection Effect – Downside of Meritocratic Society?

Despite the strong observed associations between measures of SES and crime, and compelling explanations favoring a causal interpretation for these associations, it is still possible that the SES-crime link we are witnessing is brought about by selection mechanisms. Given the common finding that most chronic offenders start their criminal careers early in life, before the establishment of any of the measures for SES used here, it is likely that such an early onset of problem behaviors has an effect on subsequent educational failure and employment prospects that follow. If this is the case, causation would be reversed. A slightly different argument suggested by criminological theories with a population heterogeneity argument, such as self-control theory (Gottfredson & Hirschi 1990) or Moffitt's (1993) life-course persistent offender, is that it is not reverse causation that matters, but the omitted variables describing stable personal characteristics that explain both low SES and crime. Both of these mechanisms, reverse causality and selection based on stable individual traits, could produce substantial associations between measures of SES in young adulthood and crime without there being any causal pathway from low SES to crime.

Furthermore, the substance of the SES measures can also be questioned. Social scientists are likely to argue that education primarily mirrors an individual's social status, and that intergenerational continuity in educational attainment represents structural inequality of opportunity (Kivinen et al. 2007). However, one could also say that education and academic performance instead measure individual traits, such as cognitive ability or personality, rather than social structure; a substantial part of the correlation between parents' and children's education is likely to be due to inheritable genetic factors (Keltikangas-Järvinen 2009). Furthermore, the effect of rapid educational expansion in Finland, particularly the diminishing size of the group with only basic education, on crime can also be debated: is it so that the strong effect of low education mirrors the selected nature of the increasingly marginal group with no qualifications, or is their increasingly difficult position in the labor market (Sipilä et al. 2011) causing more strain and incentives to commit crime?

American sociologist Susan Meyer (1997) has argued that the poor in rich Western countries are selected in many ways, and individuals found in the lowest social stratum are likely to suffer from "multiple liabilities." This is because societies implement policies that reduce poverty due to random catastrophes, such as job loss, death of a spouse, and serious illness (Mayer 1997, 149). On a comparative scale, such events should have a lesser impact in welfare states like Finland due to the societal safety net.

The other side of the issue is that after these more evenly distributed causes of poverty are removed, poverty becomes less random, and “those who remain poor become less like everyone else” (Meyer 1997, 149). In fact, if one believes that Nordic countries *approach* the ideal of a meritocratic society (Breen & Jonsson 2005; Björklund et al. 2002) where equality of opportunity ensures that individual motivation and ability determine life success, it logically follows that a measure of educational level should be more indicative of individual traits in an egalitarian setting than in a country where social mobility is suppressed by a class society with structural barriers preventing upward social mobility (see also Adkins & Guo 2008). Thus, the question is: if social policy suppresses variation in socioeconomic conditions and lowers the boundaries between different social classes by providing, for example, free education for all, do individual factors such as cognitive skills and personality become more important in determining who is successful and who is not (Mackenbach 2012)? If this did happen, high social mobility could lead to greater rather than smaller socioeconomic differences in crime when measured by personal SES. Regarding SES and crime in Finland, we do not yet know if such compositional change has happened, or if socioeconomic differences have increased. Savolainen et al. (2013) propose that the somewhat gradual relationship between measures of SES and crime in Finland could be explained by mechanisms described above. Similar discussions related to causes of persistent and widening health inequalities in Western welfare states are currently ongoing (Batty et al. 2006; Chapman et al. 2009; Mackenbach 2012).

Results from Substudy II indicate that selection effects play a major role in creating the association between unemployment and crime. The probability of violence or DWI changes only slightly while unemployed, when the men in the sample are compared with themselves during other states. On the other hand, property crime has a temporal association with unemployment even after strong controls for persistent heterogeneity. However, with this approach selection effects are inevitably a black box; we can only say that unemployment status does not affect violent crime when all the factors that remained stable between measurement points are taken into account. Thus, we cannot separate the effects of stable individual traits from the environment. We can only say that the stable factors matter, and individual traits are likely to play a substantial role if prior research is to be believed, but criminal propensity is still a black box (Laub & Sampson 2003).

The selection argument is compelling in many ways, and it clearly should be investigated further to better understand the etiology of margin-

alization and crime (Kivivuori & Bernburg 2011). However, even if we discover that educational failure and chronic unemployment are highly selected phenomena, it does not mean that such events would not have an added effect on later life outcomes. Quoting Sampson and Laub (1997, 23): “To assume that individual differences influence the choices one makes in life (which they certainly do), does not mean that social mechanisms emerging from those choices can then have no causal significance. Choices generate constraints and opportunities that themselves have effects not solely attributable to individuals.” Even if those who embark on criminal careers are likely to differ from rest of the population in multiple “stable” ways, it would seem unlikely that failures to complete education and persistent unemployment would not contribute to the downhill snowball that was set in motion earlier. However, to obtain reliable estimates for the contribution of such factors, selection processes resulting from both individual traits and environmental influences need to be taken seriously. It is evident that hereditary factors play a role (Jokela 2006; Baker et al. 2010). Another interesting avenue for further research would be to study the conditional effects of life events, such as unemployment, in interaction with individual traits – it could well be that the response to such events varies by individual characteristics (Farrington et al. 1986).

7.3 Methodological Issues

Any study that uses official data – be it police-reported crime, convictions or hospital discharges – to measure crime, has to take into account the possibility that the factors that predict offending might also influence the likelihood of getting caught. For instance, if police focuses control on poor neighborhoods, an observed SES-crime link might actually tell us more about differences in control rather than differences in criminal propensity by SES. Ever since the first self-report studies that reported a much smaller SES-crime link that was previously discovered using official data as the outcome brought control bias (or “extralegal bias”) to the agenda (Kivivuori 2011), and it has remained an important question since then.

We can, without a doubt, say that police-reported crime underestimates the quantity of crime, and especially less serious forms of violence and shoplifting are way more common than police or court data would suggest. However, the evidence on whether there is systematic bias in control or reporting by individual characteristics is less consistent. The body of research on determinants of police-reporting (Akers & Kaukinen 2009; Felson & Paré 2005; Gottfredson & Hindelang 1979; Skogan 1984; for slight-

ly different results, see Baumer 2002; Hart & Rennison 2003), using mostly victimization surveys, does not support the claim that only the crimes by low SES individuals would be controlled, or only low SES victims of violence would report crimes. A recent study with survey data showed that the frequency of criminal behavior and alcohol use were the strongest determinants of police control among Finnish 9th graders (Saarikkomäki 2010). On the other hand, there was also evidence of greater control on boys, those doing poorly at school, children living with single fathers, and those living in cities. Although control and reporting biases are likely to exist, and have an effect on the results of the substudies, the sheer magnitude of most of the socioeconomic differences discovered here would require the biases to be much larger than any existing evidence suggests (for summary of Nordic studies on this matter, see Kivivuori & Bernburg 2011, 427–428).

If measuring crime is difficult, measuring SES and other life circumstances is not unproblematic either. Even though the RFCF sample is better representative of all social strata than most surveys, the available measures do not enable us to completely identify those most seriously marginalized. Added register-based measures on debt defaulting, social assistance and other benefits would improve the identification of the most disadvantaged groups. For instance, it is likely that the association between unemployment and crime would be stronger if we could identify those “off the grid” who never sign up as unemployed (see also Myrskylä 2012), and also those on earnings-related unemployment benefits. However, even with these improvements, the actual life circumstances of young adults going through a transitional phase in their lives are always going to be difficult to measure without error. The same applies to those most seriously marginalized. The reality of their lives, for instance their actual place of residence or source of income, is not likely to be properly captured by any administrative register.

The standard form of quantitative inquiry in social sciences has been to use regression analysis with cross-sectional data, and try to tease out causal estimates for variables of interest by controlling for confounding third variables. While it is questionable if causal effects can ever be discovered with such methods and data (Berk 2004; Morgan & Winship 2007), this form of quantitative inquiry is particularly problematic with register-based data that lacks the measurement of key theoretical constructs. Even if the criminologist using register-based data is solely interested in the effects of variables describing one’s position in the social structure, the argument would inevitably be made stronger if the researcher could account for alternative explanations. Given that the most likely opponents of sociological theories of crime are those theories that focus on individual traits (Gottfredson &

Hirschi 1990; Moffitt 1993), the discussions on omitted variable bias inevitably concern the lack of variables measuring individual-level risk factors such as low self-control. Especially in the context of violence, one major omitted variable is substance abuse. Furthermore, while the focus of this study was on the effect of personal SES on crime and victimization, similar measures on their parents would have made the analysis stronger.

This study used two strategies to overcome the issues related to selection effects. The first was to focus on comparisons between crime types rather than elaboration by controlling for third variables. While we cannot rule out the possibility that the effect of SES is not causal in any of the comparative models, we can still say that some types of crime are more strongly connected to low SES than others. The second strategy was to use fixed-effects regression models to obtain within-individual estimates. The FE model guards against selection due to stable traits, but it is still possible that other time-varying factors lie behind the detected unemployment effect on property crime. If FE models are used, more measures on time-varying variables should be included in future studies, and the possibility of using such models with short measurement intervals should be explored further. Overall, researchers working with register-based data in social sciences would probably benefit from a better understanding of the so-called “potential outcomes framework,” which provides a means for estimating causal effects with observational data in certain settings (for an introduction to the counterfactual model of causality, see Morgan & Winship 2007). Finally, despite the large data size in the current study, reliable population-based analysis of rare forms of police-reported crime (e.g. white-collar crime) would require even larger random samples, or alternatively case-control designs where offender populations are oversampled.

7.4 Advancing Register-Based Research on Crime and Victimization

While Finnish register-based data has been used to study aspects of crime before (see e.g. Kinnunen 2002, Savolainen et al. 2007, Savolainen 2009), the research project Risk Factors of Crime in Finland – and this study as an integral part of the project – has been a pioneering study in the sense that it built a large-scale register-based dataset dedicated to the study of crime. While starting off cautiously as a feasibility study, it has subsequently proved its worth as an important resource to analyze socioeconomic and demographic distribution of Finnish officially registered crime. Descriptive

reporting of results from such data is a valuable addition to knowledge about crime in Finland.

Despite the appeal of register-based data, the limited nature of theoretically relevant information available means that register-based studies cannot replace survey research and prospective longitudinal studies. Of course, the best scenario is one where register-based follow-ups are combined with survey instruments of varying kinds. This is because register-based measures of independent variables can only tap into constructs that are being registered by the authorities responsible for the primary operational registers, and only examining such factors that are conveniently available is not an optimal way to test and develop the theories of crime. However, when register-based data can answer the research question one has in mind, it is hard to see why other types of data would be used. For those interested in serious crime and chronic criminal careers, relatively uncommon phenomena on population level, the large sample sizes and long follow-ups made possible by such data are a pressing argument for register-based research. It is safe to say that there are several questions left to be answered with these data (Lyngstad & Skardhamar 2011).

What kinds of research questions are best answered with register-based data? First of all, the longitudinal nature and timing of events are a key strength of the data: this enables the researcher to better study what came first (see e.g. Monsbakken et al. 2012). When the exact dates of events are available, one can construct panel data with monthly or weekly measurement intervals. Second, panel data methods, from fixed-effects regression models to more advanced counterfactual methods (e.g. regression discontinuity and instrumental variable models, see Morgan & Winship 2007) enable researchers to construct quasi-experimental designs, and take advantage of natural experiments that exogenously manipulated an independent variable of interest to estimate causal effects (e.g. the effect of education on crime by Lochner & Moretti 2004). Along a similar vein, sibling models could be used to compare children from same families. As mentioned, the effects of labour market and social policy reforms – whose primary purpose is elsewhere – on crime could be assessed with these data as well. Third, one additional interesting possibility is register-based study of victimization. In international comparison, it is very rare to have data that can identify victims of police-reported crime. Longitudinal studies of victimization are in general much rarer than such on offending. While such study would have to be limited to serious violence, it could still shed light on the dynamics of social exclusion and violent victimization.

8 CONCLUSION

On the basis of the current study we know that crime among young adults in Finland is far from a random phenomenon with relation to SES. The magnitude of these differences makes it hard to believe that we would only be witnessing statistical artifacts brought about by biased control. While the existence of socioeconomic differences in crime does not mean that Finnish social policy has failed to reduce crime, the finding of substantial socioeconomic differences in crime in a welfare state context raises questions. What are the implications of relative equality of opportunity in Finland for the association between SES and crime? Are we witnessing an increasing selection into educational failure and subsequent change in the composition of the lowest social strata (Mackenbach 2012), or does the effect of having no education become stronger through mechanisms of relative deprivation and declines in the demand for unskilled labor? It is apparent that the optimal policy to combat crime related to social exclusion depends on the answer to this question (see also Mackenbach 2012 for discussion on health inequalities). Furthermore, it is possible that both processes operate simultaneously.

While not an easy task, future research needs to examine the possibility of changing SES-crime association by comparing different cohorts to see what has happened to the gradient during the last decades. To draw inferences about social mobility, such research should assess the effects of both family and personal SES on crime. We clearly need more research about the extreme end, those “truly disadvantaged” (Wilson 1987) or socially excluded. However, while social exclusion might be a handy concept for public debates about social problems as it captures the multi-faceted nature of the “liabilities” (Meyer 1997) that the most marginalized group suffers from, a concept comprising all possible adverse outcomes is very difficult to work with in empirical research. Even if social problems tend to cluster, we still need to find out which of those problems are the true causes that we need to tackle, and which are only markers or correlates. Overall, the discussion about causes and consequences of social exclusion would benefit from a better understanding of selection processes. Intergenerational continuities in both social exclusion and crime in Finland are not yet well described, let alone explained. Acquiring this understanding requires better co-operation across disciplines. If we believe that the results we see here are brought about by a process of cumulative disadvantage (Sampson & Laub 1997), where individual traits and difficult environments operate jointly to push the snowball to a downhill trajectory, we need to identify

the critical life course stages where interventions have the best possibility to succeed.

The argument for early intervention is convincing (Heckman 2006), but solid scientific evidence on different kinds of programs is only beginning to accumulate, and robust evaluations of such programs are scarce outside the USA (Farrington & Welsh 2007). The problem is that many crime-reducing interventions with sound footing (Farrington & Walsh 2007) are often already implemented in Nordic welfare states as universal policies (Kivivuori & Aaltonen 2009). Bearing in mind the possibility that crime prevention programs with good intentions can, at worst, cause harm (McCord 2003; Petrosino et al. 2002), the large-scale implementation of future programs should be based on proper evaluations. A great example of this is the Finnish anti-bullying program KiVa (Kärnä 2012). On basis of results from this study, the transition from compulsory school to secondary education is a critical phase, and increasing the completion rate of secondary schooling, the vocational track in particular, is a major challenge. Even though this study could not establish a causal effect of education on crime, the evidence from natural experiments (Lochner & Moretti 2004; Machin et al. 2010; Hjalmarsson et al. 2011; Meghir et al. 2012) suggests that crime could be reduced further if we can find ways to increase completion rates in secondary schools (in other words increase levels of education in the group with minimum qualifications). Alternative ways of learning, once again within the vocational track, are probably needed to accomplish this.

On the other hand, the more selected and small the group without secondary education becomes, the less likely it is that educational policy alone will suffice. As research on criminal recidivism shows (Giordano et al. 2002; see also Skardhamar & Telle 2012, Skardhamar & Savolainen 2013), opportunity alone is not enough if individual motivation is not there. In the end, people are active agents operating between structural constraints and individual predispositions (Laub & Sampson 2003; Nilsson & Estrada 2009), not simply objects reacting to forces beyond them. Due to these reasons, the “marginal” individual – the person who would respond positively to a possible universal educational reform – is not necessarily the one who would have embarked on a criminal career anyway. In a society that already provides free education and values education highly, it seems likely that individually tailored approaches and early intervention are needed to reach those most likely to become “life-course persistent offenders” (Moffitt 1993). While such selective policies (Farrington & Welsh 2007) carry the risk of labeling, ignoring the evidence on early risk factors (Farrington & Welsh 2007), and the fact that the square ones children are born

into are far from equal even in Finland, is a similarly risky choice made by the society.

This study has shown that inverse association between SES and crime is strong in Finland. Given the supposed decline of SES as the “master variable” in criminological research, do these results mean that SES should be brought back to the forefront of criminological inquiry? At least in Finland, the interest in social class appears to have revived in social sciences (Erola 2010). Resorting to a sociologist’s standard answer, my response is both no and yes. Given that this study has updated the picture of socioeconomic differences in crime, the true challenge is to understand the pathways to serious social exclusion and crime. Multidisciplinary inquiry – what criminal career research on crime at its best represents (see DeLisi & Piquero 2011) – on etiology and the consequences of these phenomena is needed. Such studies are essential in finding ways to prevent criminal careers. Furthermore, register-based data could and should be used to evaluate the possible effects that different social policy reforms have on crime. Regarding the role of SES in criminological research, for me the most important thing is to continue developing ways to collect data that reach those in the most marginalized positions. Without such data, researcher claiming that SES is unrelated to either crime or victimization is standing on thin ice. In this sense SES remains very important: the main contribution of this study has been to uncover the strong association between SES, crime, and violent victimization once all segments of the population are actually reached.

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APPENDIX

Penal codes included in the outcome variables in Substudies I and II (Statistics Finland 2005):

http://www.stat.fi/meta/luokitukset/rikokset/001-2005/index_en.html

Property Crime:

theft; aggravated theft; petty theft; receiving (stolen property) offense; negligent receiving (stolen property) offense; receiving (stolen property) violation; aggravated receiving (stolen property) offense; professional receiving (stolen property) offense; unauthorized use; petty unauthorized use; aggravated unauthorized use; theft of use of a motor vehicle; petty theft of use of a motor vehicle; aggravated theft of use of a motor vehicle; robbery; aggravated robbery; extortion; aggravated extortion; damage to property; petty damage to property; aggravated damage to property; embezzlement; petty embezzlement; aggravated embezzlement; fraud; petty fraud; aggravated fraud; means of payment fraud; petty means of payment fraud; preparation of means of payment fraud; aggravated means of payment fraud; tax fraud; petty tax fraud; aggravated tax fraud; tax infraction; accounting offense; negligent accounting offense; other offenses in trade; forgery; petty forgery; possession of forgery materials; aggravated forgery; dishonesty by a debtor; aggravated dishonesty by a debtor; fraud by a debtor; aggravated fraud by a debtor; deceitfulness by a debtor; violation by a debtor; favoring of a creditor; smuggling; petty smuggling; other offenses against property

Violent Crime:

manslaughter; murder; homicide; attempted manslaughter; murder or homicide; infanticide; assault; aggravated assault; petty assault; negligent homicide; grossly negligent homicide; negligent injury; grossly negligent injury; other crimes against life and health

Driving While Intoxicated:

drunken driving; aggravated drunken driving