**Work in progress – please do not quote**

# The Effect of Relative Income on Crime: Evidence from Micro-data*

Susan Niknami

SOFI, Stockholm University

**Abstract**

This study examines whether relative income affect criminal behavior using high-quality individual data from various Swedish administrative records. My empirical approach relates individual level changes in relative income to changes in criminal behavior and exploits the fact that an individual cannot fully decide his own place in the income distribution. For this reason variation in relative income is likely to come close to being exogenous, holding constant own income as well as permanent unobserved individual and regional characteristics. The empirical analysis reveals a statistically significant effect on property crime. A one standard deviation increase in relative income differences increases the propensity to commit property crime by about 9 percent. The effect is mainly driven by past offenders, low educated individuals and individuals below age 40. The results suggest that inequality in earnings is more strongly related to criminal behavior than inequality in disposable income.

JEL classification: K42, D31, D39.

Key Words: Crime, Income Inequality, Relative Income Differences

*I am grateful to Matz Dahlberg, Hans Grönqvist, Markus Jäntti, Matthew Lindquist, Anders Stenberg, Mårten Palme, Erik Plug, Eskil Wad ensjö, and seminar participants at SOFI for valuable comments. I would also like to thank Anna Öster (Nilsson) for kindly sharing her data with me.

## 1. INTRODUCTION

Criminal activity imposes enormous costs on society and crime reduction is therefore high on the public agenda. In the U.S., for instance, crime is estimated to cost the society between $300 billion and over $1 trillion each year (Anderson, 1999). Needless to say, in order to combat criminal behavior it is vital to understand its underlying sources. Relative income ranks high among the potential determinants of crime and remains a recurrent theme in the public debate.[1] The empirical relationship between relative income and crime is however still not well understood.

In this paper I analyze the effect of relative income on criminal behavior using high-quality longitudinal data from Swedish administrative registers. The data covers the entire working age population and encompass detailed information on labor market, educational and demographic characteristics during 1990 to 2007. The data have been merged to the Swedish conviction register that include genuine information on all individual convictions in Swedish courts during 1985 to 2007. Among other things there is information on the type and the date of offence.

Theoretically there are two main reasons why relative income may affect an individual's propensity to commit crime. According to economic theory, an individual's expected returns to crime increase when being next to rich individuals who have belongings worth steeling (cf. Becker, 1968). The sociological literature (cf. Merton, 1938) instead emphasizes that a lower relative income causes feelings of relative deprivation which in turn generates frustration and anger among the poorest individuals since they become relatively poorer. Low relative incomes may therefore in particular provoke acts of violent crime.

[1] A large literature considers the importance of other likely determinants of crime focusing both on social and individual characteristics as well as features of the criminal justice system. Some examples of such studies are Adda, McConnel and Rasul (2011), Bayer, Hjalmarson and Pozen (2009), Card and Dahl (2011), Dahl and DellaVigna (2009), Deming (2011), Donohue and Levitt (2001), Doyle (2008), Draca, Machin and Witt (2011), Duggan (2001), Dustmann and Piil Damm (2009), Grönqvist and Niknami (2011), Hjalmarson and Lindquist (2011), Jacob and Lefgren (2003), Kling, Ludwig and Katz (2005), Lee and McCrary (2009), Lochner and Moretti (2004), Meghir, Palme and Schnabel (2011), and Weiner, Lutz and Ludwig (2009).

A large empirical literature has investigated the relationship between relative income and crime at the aggregate level. Freeman (1999) reviews the literature.[2] The empirical evidence is however inconclusive and not straightforward to interpret. Credible identification of the parameter of interest hinges on the ability to isolate the effect of relative income from that of other factors. Data limitations have however forced researchers to use cross-sectional or national time-series data.[3] All past studies further rely on aggregate data. There are several reasons for why aggregate data may be inappropriate when analyzing the relationship between relative income and crime. To start with, it is not possible to properly test the underlying theories using aggregate data since they are formulated at the individual level.

Moreover, with aggregate data one cannot separate the effect of relative income from that of absolute income. From a policy perspective it is essential to distinguish the effect of being poor from that of having a low relative income. If aggregated measures of relative income are only related to the crime through own income, then increased inequality driven solely by richer becoming richer may have no effect on crime.

Aggregate measures of relative income could also cancel out the effect of interest if low relative incomes increases the risk of crime while high relative incomes decreases the risk for crime, generating an average effect equal to zero. Another limitation with this this type of data is that it may mask the effect if the relationship between relative income and crime only exists at the lower end of the income distribution.

Despite the analytical limitations related to the use of aggregate data, this type of data may also produce a spurious relationship between relative income and crime at the level of a population. This will occur if crime is a nonlinear function of income at the individual level. Although this "aggregation problem" has been raised in other contexts (see Gravelle 1998) it

[2] More recent studies are: Brush (2007), Choe (2008), Dahlberg and Gustavsson (2008), Demombynes and Özler (2005), Fajnzylber, Lederman and Loayza (2002a, 2002b), Hipp (2007), Kawachi, Kennedy and Wilkinson (1999), Kelly (2000), Machin and Meghir (2004) and Nilsson (2004).
[3] A few number of studies however relies on regional panel data to control for permanent unobserved area characteristics (e.g. Machin and Meghir, 2004).

has not previously been discussed for this topic. In the paper I describe how the problem is likely to affect the link between aggregated measures of relative income and crime.

The main innovation of this paper is to identify the effect of relative income on crime at the individual level. Another important contribution is that the empirical approach used eliminates many of the potential confounders that could bias the results. My empirical analysis relates individual level changes in relative income differences to changes in criminal behavior and exploits the fact that an individual's relative income not only depends on own effort, but also on everyone else's effort. An individual therefore cannot fully decide his own place in the income distribution. For this reason variation in relative income is likely to come close to being exogenous, holding constant own income as well as permanent unobserved individual and regional characteristics. This is further the first paper to examine to examine whether the effect differs across subgroups of the population that may be at higher risk of committing crime. My paper also relates to the literature on relative income and well-being that in general finds that a low relative income has a negative effect on job satisfaction, happiness as well as health (e.g. Card et. al 2010; Daly, Wilson and Johnson 2013; Dynan and Ravina 2007; Luttmer 2005).

Consistent with economic theory I find that a drop in relative income has a positive and statistically significant effect on the probability to commit property crime. A one standard deviation increase in relative income differences increases an individual's probability of committing property crime by about 9 percent. The estimate constitutes about 7 percent of the (unadjusted) crime gap between individuals with compulsory versus university education. The increase is mainly driven by past offenders, persons with low education, and persons below age 40. I find little evidence that a lower relative income increases violent crime. There is however a significant positive effect of relative income differences on the probability of being sentenced to prison and on committing drug related crimes.

The analysis separates between different income sources. Disposable income is closely related to the economic model of crime since it reflects the true amount of money that people receive in their “pocket”. However, I find that inequality in gross labor earnings is more strongly related to criminal behavior than inequality in disposable income. One explanation for this finding could be that earnings are more closely associated with social status than disposable income. Earnings may also be easier to observe than disposable income and therefore better signal one’s position in the income distribution. In any case, this result indicates that redistributive policy may not be an efficient tool to decrease crime caused by inequality.

The paper unfolds as follows. Section 2 gives the conceptual framework. Sections 3 and 4 discuss the data and the research design. Section 5 presents the results and Section 6 concludes.

## 2. CONCEPTUAL FRAMEWORK

### 2.1 THEORY

Following the seminal work of Becker (1968) and extensions by Ehrlich (1973), Chiu and Madden (1998), Bourguignon (2001) and others, the decision to participate in crime can be formulated as a function of legal income $Y$, the probability of getting caught $p$, the severity of punishment $S$, a fixed cost of committing crime $K$, and the monetary payoff to crime $R$:

(1) $$Crime = f(Y, p, S, K, R)$$

where criminal participation depends negatively on $Y$, $p$, $S$ and $K$ and positively on $R$. The expected net value of committing crime can be seen as a probability weighted average of the inputs in the crime production function and an individual engages in crime if this value is positive.

Relative income is related to the expected returns to illegal activities as a low relative income decreases the opportunity cost of committing crime. To illustrate these ideas, consider Figure 1 which shows the income distribution of Society A, B and C. In this very simplified example, Society A and C have identical income distributions, which are wider compared to Society B. Assume now that the probability of getting caught, the severity of punishment and the fixed cost of crime are the same in all societies. Also suppose that individual $i$ has the same legal income $y_i$ in all societies, and perfectly knows the shape of the distribution as well as his own position in the distribution. The crosshatched area in the figures represents the incomes of the $j$ individuals with a higher income than individual $i$, i.e. $\sum_j (y_j - y_i | y_j > y_i)$. This area is largest in Society A.

Economic theory stipulates that the incentives to commit crime depend on the expected net returns to crime. Since we have assumed that $Y, p, S$ and $K$ are the same in all three societies, any differences in the expected net returns will solely be due to $R$. Let us add the assumption that people only consider individuals with higher relative incomes as potential victims and that $R$ increases with the amount of resources that these individuals hold (c.f. Chiu and Madden 1998). Individual $i$ then has the strongest incentives to commit crime in Society A because the amount of resources among the potential victims is biggest in Society A.

The economic framework only considers financially motivated types of crimes such as property crime. The sociological strain theory provides a more plausible description of the relation between income inequality and non-acquisitive types of crimes, such as violent crime (Merton, 1938). In this framework individuals are assumed to compare themselves to people that are more advantaged. Being relatively more disadvantaged is believed to raise frustration and anger which in turn may trigger crime. Consequently, an individual's probability of committing crime increases as the economic gap between the more affluent and the individual widens. Consider again Figure 1. The strain theory predicts that individual $i$ will be most frustrated and therefore most likely to commit crime in Society A.

The stylized reasoning above suggests that an empirical analysis would benefit from using an individual measure of relative income as aggregated inequality measures cannot capture both the income dispersion and the position in the income distribution.

## 2.2 THE AGGREGATION PROBLEM

An additional reason for the need of using individual level data to study the link between relative income and crime is that aggregated measures relative income such as regional income inequality may be spuriously correlated with aggregate measures of crime if individual crime is a nonlinear function of income. This issue has to the best of my knowledge not been discussed in the previous literature but has received increased attention in the literature on income inequality and health (see e.g. Gravelle, 1998, Miller, 2001).

The idea is that a mechanical relationship may arise because the aggregated measure is derived from individual income. Depending on the functional form, this association can be either positive or negative. Figure 2 illustrates one version of the aggregation problem where the relation between individual income and crime is assumed to be negative and convex. As income increases, the probability of engaging in crime decreases, but at a declining rate. Suppose that an individual's probability of committing crime depends *only* on his income level and not on income inequality. Now compare the two societies, A and B, where the average income, $\bar{y}$, is the same but the income distributions are different. Assume for simplicity that half of the population has low income ($A_{low}$, $B_{low}$) and that the other half has high income ($A_{high}$, $B_{high}$). Poor people in Society A have $\Delta$y lower income than poor people in Society B. This increases population A's crime rate by $C_{1A}$-$C_{1B}$. On the other hand, rich people in Society A have $\Delta y$ higher income than rich people in Society B. This however only reduces the risk of crime by $C_{2A}$-$C_{2B}$ in Society A. The total crime rate (the average of $A_{low}$ and $A_{high}$ versus $B_{low}$ and $B_{high}$) is therefore higher in Society A compared to B. This stems entirely from the fact that crime is a convex function of individual income. In other words, when using aggregate data, we might wrongly interpret the relationship as income inequality having a direct effect on crime. Note that the aggregation problem does not arise when crime is a linear function of income. Figure 3 depicts this case. Society A and B have different distributions but identical crime rates.

The micro-relationship between income and crime may also generate a negative correlation between inequality and crime at the aggregate level. This will occur if the relationship between individual income and crime is negative and concave. Figure 4 illustrates a slightly different and perhaps more realistic case, were the curve is *concave* for low income levels and *convex* for high income levels. It means that the risk of crime diminishes at an increasing rate at low income levels and at a decreasing rate at high income levels. In my data, the relationship between individual income and crime takes this exactly shape, see Figure 5. The artificial correlation can in this case be either negative or positive depending on the income levels of the individuals. Figure 4 demonstrates the former case. The distribution of income is again larger in Society A than in B, but this time the crime rate is also lower implying a negative correlation between inequality and crime at the aggregate level. In this case, the aggregation problem will reduce any possible (positive) causal effect of income inequality on crime.

Note that in all of the above-mentioned examples, the relation between income and crime is negative and the income distributions are identical. Nevertheless, we can see that the way income inequality affects crime at the aggregate level differs across the examples. Thus, without information on the shape of the relationship between income and crime it is impossible to know how the aggregation problem affects the link between inequality and aggregated crime rates. By using individual data on both income and crime, as in the present study, it is however possible to avoid this problem.

## 3. DATA

The micro data used in this study come from several longitudinal administrative registers maintained by Statistics Sweden (SCB). It provides information on the entire Swedish population aged 16−65 each year from 1990 to 2007. The registers include information on a wide range of labor market, educational and demographic characteristics, as well as geographic identifiers. These data have been linked to the Swedish conviction register kept by the National Council for Crime Prevention (BRÅ). It contains complete information on all convictions in Swedish district courts from 1985 to 2007. All crimes within the same conviction, if several, are included in the data. Less severe crimes are handled by the district attorney but are still in the data. The data contain information on type of offence as well as the sanction ruled by the court. There is further information on both conviction and offence date.[4]

The main analysis is based on a panel consisting of a 10 percent random sample of males aged 25 to 65 observed at least once between 1990 and 1999 (289,833 individuals).[5] Since people do not need to be convicted the same year as they commit the crime I increase the number of crimes in my sample by ending the observation period at least eight years before the last observed conviction. The reason for only studying males is that men account for a disproportionate large share of crimes committed. The lower age restriction ensures that most individuals have completed their education and moved from their parents which otherwise would complicate the analysis. Of course, this restriction implies that many potential criminals are excluded from the sample. Still, almost 60 percent of all convicted individuals are aged between 25–65 at the time of offence (see Table A1).

The main crime categories used in this study are: (i) any crime, (ii) violent crime, and (iii) property crime. Violent crime is closely linked to sociological theory while economic theory primarily concerns property crime. I also study drug offences and drunk driving since these are common types of crimes in Sweden. Table A.1 describes how the crime categories have been constructed.

I mainly use disposable income (measured in 1990 year's prices) to compute relative income differences, i.e. the net income from work and capital combined with net social benefits and transfers. Disposable income is arguably the income concept most strongly

[4] The exact date of crime is known for about 70 percent of all offences. The court makes an educated guess about the date of offence when the date of crime is unknown (for instance in cases when a house break-in is not immediately detected). This obviously generates some measurement error. However, since I analyze annual data this type of measurement error is most likely small.

[5] The reason for not analyzing the entire population is that the regressions then becomes too computational demanding.

linked to the underlying theory as it directly reflects the amount of money that individuals receive in their "pocket". In one part of the analysis I also use gross earnings.

Relative income is measured at the municipal level. In doing so, I implicitly assume that people compare themselves with individuals living in the same municipality. There are 290 municipalities in Sweden and the average municipality in my sample hosts about 36,000 individuals. In the analysis, I also consider smaller (parishes) and larger (counties) geographic units.

Relative income is further measured by the Yitzhaki index that compares own income with the income of the more affluent (see e.g. Eibner and Evans, 2005). This index was originally developed to mathematically formulate relative deprivation in income (Yitzhaki, 1979).[6] The Yitzhaki index can be written as:

$$(2) \qquad Yitzhaki\ Index_i = \frac{1}{N} \sum_j (y_j - y_i)\ \forall\, y_j > y_i$$

where $y_i$ is the income of individual $i$, $y_j$ is the income of those earning more than $i$ and $N$ is the total population size. Thus, a given index value reflects the sum of the income differences between $i$ and the $j$ individuals with higher incomes than $i$. The sum is divided by the number of people in the society to make the measure invariant to population size. A high value implies that the individual's relative income is low and therefore that the relative income differences are large. Put differently, the index accounts for an individual's relative position in the income distribution of a given reference group expressed as a measure of the dispersion of

[6] The concept relative deprivation was formulated by Runciman (1966) as ''the extent of the difference between the desired situation and that of the person desiring it''.

incomes of those earning more. The Yitzhaki index is closely connected to the theoretical framework presented in Section 2.1 (see also Figure 1).[7]

Tables A3 and A4 display descriptive statistics for selected variables. We can see that about 2 percent of all individuals in my sample are convicted for some type of crime each year. The corresponding numbers for violent crime, property crime, drug crime and drunk driving are .3, .5, .2 and .4 percent. Note also that there is a great deal of variation in inequality both within and between individuals.[8] Table A5 shows descriptive statistics for selected individual characteristics. These are presented for the entire sample and by criminal status. Criminal status refers to whether individuals committed crime in all, some or none of the years that they are in the data. As expected, the characteristics differ substantially depending on the criminal status. Most people do not commit crime and those who do are in general younger, less educated, and have a criminal past.

One advantage of using individual level conviction data is that one can analyze the potential effect of inequality on individual criminal behavior and avoid the aggregation problem. This has not been possible in previous studies which have all relied on aggregated police report data. While self-reported crime data would also circumvent these problems, such data are typically based on only a few observations and do not objectively measure crime. Still, conviction data also have its flaws. One concern is that crime status is only recorded for individuals that have been convicted. In the next section, I discuss the implications of this in more detail and explain how my empirical approach handles the potential problems.

[7] As alternative measures of inequality I also study the percentile ranking and Gini coefficient. The Gini coefficient is closely related to the Yitzhaki index as it can be shown that the average Yitzhaki index in a society is equal to some constant *k* multiplied by the Gini coefficient (Yitzhaki, 1979).

[8] Since the data is unbalanced the between and the within variation do not sum up to the overall variation.

## 4. EMPIRICAL DESIGN

In the absence of a controlled experiment in which relative income are randomly assigned across individuals, it is not possible to rule out that any observed relationship between relative income and crime is driven by omitted variables or reverse causality. In this section, I describe how the individual level panel data allows me to adopt an empirical strategy that alleviates many of these concerns. The baseline model can be written as follows:

$$(3) \qquad Crime_{ict} = \alpha + \beta_1 (YI)_{ict} + f(Income) + g(age) + \theta_i + \theta_c + \theta_t + \varepsilon_{ict}$$

where $Crime_{ict}$ is a dichotomous variable, which takes the value one if individual $i$ in municipality $c$ committed crime in year $t$ and zero otherwise. $(YI)_{ict}$ is the Yitzhaki index. $f(Income)$ is a function of disposable income and $g(age)$ is a function of age. $\theta_i$ represents a set of individual fixed effects. $\theta_c$ and $\theta_t$ are municipality and year fixed effects. The year fixed effects control for national trends, such as the business cycle. The municipality fixed effects eliminate all variation in crime caused by factors varying across locations that are constant over time. For instance, $\theta_c$ captures industry structure and permanent characteristics of the local justice system.

By including individual fixed effects the empirical model relates changes in relative income differences to changes in criminal behavior. The individual fixed effects absorb all permanent individual-specific characteristics that may affect both the likelihood of committing crime and an individual's relative income. An individual's aversion towards risky behavior is for instance likely to affect both variables. Individuals with high discount rates may also be more prone to commit crime and to take up low-paid work. Other potential confounders this specification accounts for are ability, family background and/or, to the extent it is a permanent trait, Attention Deficit Hyperactivity Disorder (ADHD).

The coefficient of interest is $\beta_1$, which provides the effect of relative income differences on the probability of committing a crime. The regressions control for own income. $\beta_1$ is therefore identified by changes in relative income differences caused either by changes in the incomes of other people in the municipality or by changes following a move to another municipality.[9] The coefficient captures changes in relative income due to both permanent and transitory shifts in others' incomes. Theory does not tell whether it is inequality in permanent and/or transitory income that matters and it is beyond the scope of this paper to separate between these two since that would require strong assumptions.

The model rests on the identifying assumption that relative income differences are uncorrelated with the error term in the past, present and future. This assumption is violated if unobserved individual characteristics that are systematically correlated with relative income differences vary over time. However, relative income differences depend not only on own effort, but also on everyone else's effort. Hence, an individual cannot fully decide his own place in the distribution by changing his behavior. It is also important to note that the model controls for individual disposable income. This implies that any variation in relative income differences caused by changes in own income are explicitly controlled for in the regressions. The model then presumably accounts for the most important confounding factor. Income further provides a good proxy for potential omitted factors that vary over time. Still, time-varying chocks is probably less of a concern when studying older people since vast majority of individuals fall into the criminal path early in life and the individual fixed effects absorb any preexisting "pushes" into criminal activity. Remaining potential confounders would then be unobserved factors at the municipal level that are not absorbed by the regional fixed effects. In the robustness checks I include municipality×year fixed effects and show that such factors are not likely to drive the results.

[9] I estimate equation (3) with a linear probability model since I am interested in the entire population of men aged 25–65. A logit model only would identify the effect for those who change their criminal status during the period of observation. Table A5 reveals that this group differs from the entire population of interest.

One potential problem with the individual level conviction data is that crime status only is recorded for individuals that have been convicted. The concern is that individuals with low relative incomes may more often get convicted conditional on actually having committed a crime which then generates a spurious relationship between inequality and crime. Note however that this type of selection is only a problem if it is not picked up by any of the controls in the empirical model. Since my regressions accounts for permanent differences in unobserved individual and municipality characteristics as well as individual income the potential problem is arguably not severe.

## 5. RESULTS

This section presents the results from my empirical analysis. Throughout, estimates are reported for any crime, violent crime and property crime. In Section 5.1, I show the main results and compare the findings of pooled OLS with individual fixed effect estimates. Section 5.2 provides results from robustness checks and 5.3 explores potential heterogeneous effects across subgroups. Additional results are given in Section 5.4

### 5.1 MAIN RESULTS

Table 1 reports my main results of the effect of relative income differences on crime. Each coefficient represents an estimate from a separate regression. The standard errors are clustered at the individual level to account for serial correlation and heteroscedasticity.

Panel A starts by showing the pooled OLS results. The OLS regressions control for cubics in age and income as well as dummies for educational level (five levels), missing values in education, foreign-born, year and municipality fixed effects. For all types of crime definitions, the coefficients are strongly significant and large in magnitude.

As discussed in the previous section, it is likely that unobserved individual characteristics such as ability, family background and risk aversion bias the OLS estimator upwards. An individual fixed effects model is therefore presumably appropriate. But it is worth noting that individuals who only are in the sample one year do not contribute to identifying $\beta_1$ in an individual fixed effect model. In my sample 13,241 out of 289,833 individuals belong to this category. Panel B shows the pooled OLS results from regressions which excludes this category. The point estimates are very similar to those in Panel A suggesting that results from individual fixed effects estimations more easily can be extrapolated to the entire population of interest. Having said this, we continue to Panel C that shows the results from estimating equation (3) where all permanent individual characteristics are accounted for. The estimates are reduced by between 80 and 95 percent compared with the OLS coefficients. A one standard deviation increase in relative income differences is associated with an increased risk of committing crime by 2.4 percent.[10] The estimate is statistically significant at the 1 percent level and implies an increase in the number of people committing crime by 57 from a baseline of 2300 per 100,000 individuals (.038×.015×100,000). The corresponding effect on violent crime is 1.6 percent (or 6 people) and not statistically significant. However, the point estimate in Column (3) suggests that relative income differences have a highly statistically significant effect on property crime. The coefficient of .029 translates into about a 9 percent higher risk of property crime for a one standard deviation increase in relative income differences. This is equivalent to an increase in the number of convicted persons by about 44 per 100,000 individuals.

Two things are worth mentioning about the results. First, the huge drop in the magnitude of the point estimates when controlling for individual fixed effects clearly highlights the importance of accounting for persistent individual unobservables. Second, consistent with economic theory, the size of the estimates is substantially larger for property crime.

[10] This is the point estimate multiplied with the within individual standard deviation divided by outcome mean. Henceforth a one standard deviation increase in inequality refers to the within variation in the data (cf. Tables A.3 and A.4). Percentage effects and significant levels in the tables are calculated based on non-rounded values.

Although the point estimates in Panel C are smaller than in Panel A they still indicate that relative income differences affect crime. To get a better sense of whether this effect is large it is convenient to relate the estimates to other factors that have been shown to be important in explaining criminal behavior. It is well known that individuals with low socioeconomic status are more likely to engage in crime (see e.g. Lochner and Moretti 2004). In my sample, 700 out of 100,000 individuals with 9 year compulsory schooling as their highest education are convicted for property crime each year (see outcome mean in Table 3). For individuals with university education this number is only 100. The effect of a one standard deviation increase in relative income differences on property crime thus constitute only about 1/13 (44/600) of the educational gap in crime. The magnitude of the effect of relative income differences on property crime therefore seems to be fairly modest.

## 5.2 ROBUSTNESS CHECKS

Table 2 presents results from robustness checks. The baseline results are shown in Panel A. In this specification the standard errors allow for serial correlation at the individual level. It is reasonable to think that serial correlation matters most at the individual level and failure to account for this would understate the standard errors. However, since relative income differences also vary over time at the municipality level (for individuals who stay), the standard errors in Panel B instead allow for serial correlation at the municipal level. As shown, the standard errors are virtually identical to those in Panel A and therefore do not change the interpretation of my results.

In order for the individual fixed effects estimator to be consistent, the Yitzhaki index must be uncorrelated with the error term in equation (4). One concern is that changes in municipality characteristics may be related to both inequality and crime. To examine whether this is a problem, Panel C includes municipality-by-year fixed effects in the regressions. This approach relaxes the baseline specification by allowing for municipality-specific trends. The model therefore controls for all changes in municipality properties, such as police resources, that may be correlated with both inequality and crime. The strategy also accounts for serial correlation at both the individual (standard errors clustered at the individual level) and the municipal level (municipality-by-year fixed effects). This approach is computational burdensome as it amounts to estimating thousands of fixed effects. Note also that as the municipality-by-year fixed effects absorb much of the variation in data it is comforting to find that the estimates are more or less the same.

Although my research design exploits variation in an individual's relative income differences stemming from the income of other individuals, I cannot fully exclude the possibility of reverse causality. In the following exercise I therefore examine the effect of inequality on the probability of committing crime in the last quarter of the year. As crime committed in the end of the year cannot affect an individual's relative income earlier the same year, this approach provides a test of reverse causality. It is however important to bear in mind that the precision becomes poorer since the mean of the outcome gets smaller. Still the results given in Panel D show that the effect of a one standard deviation increase in Yitzhaki on the probability of committing property crime is similar to that in the baseline regression.

Another factor that may bias the results is time-varying individual unobservables. The baseline model controls for all permanent individual characteristics. The key identifying assumption is that important unobserved individual characteristics do not change over time. To test this assumption, Panel E excludes individual (disposable) income from the regressions. Individual income is presumably the most important time-varying factor to control for since it directly governs self-selection into inequality. It is therefore reassuring that the point estimates are not significantly different from the baseline regressions. This result suggests that it is unlikely that other (less important) time-varying individual factors drive the results. [11]

Although I restrict the sample to individuals aged 25 years and older, some individuals may still be enrolled in higher education. Individuals who invest in education will temporarily have low relative incomes but since education increases expected future incomes, a passing low relative income need not affect the probability of committing crime. I therefore excluded individuals below age 30 from the sample. As can be seen in Panel F, the results do not change much.

### 5.3 SUBGROUP ANALYSIS

Table 3 shows results for different subgroups of the population. Again, Panel A presents the baseline estimates for the full sample. I start by dividing the sample by age to analyze if younger people are driving the results. Youths account for a disproportionate share of crimes committed and are also more likely to be exposed to high levels of income inequality (see e.g. Freeman, 1996). The first row of Panel B shows the results for individuals below 40 and the second row those for individuals 40 years or older. For both groups there is a statistically significant effect of relative income differences on property crime. The effect is however larger for the younger group. A one standard deviation increase in relative income differences increases the likelihood of committing property crime by about 11 percent for individuals under 40. The corresponding number is about 6 percent for individuals who are 40 or older. Interestingly, the effect of violent crime is negligible and statistically insignificant also for the young “high risk” group.

I also stratify the sample by educational attainment: compulsory schooling, upper secondary school and university. It is well-established that individuals with fewer years of

---

[11] A recent paper by Cobb-Clarke and Shurer (2011) shows that unobserved individual characteristics are stable over shorter time periods.

schooling are at higher risk of criminal involvement (see e.g. Lochner and Moretti, 2004). The relationship between inequality and crime may therefore be stronger for individuals with low education if they are less able to cope with exposure to low relative incomes. The first row of Panel C shows the results for individuals with at most compulsory education. The coefficients of .097 and .062 for any crime and property crime are strongly significant. These parameters imply that a one standard deviation increase in the Yitzhaki index increases the probability of committing any crime by 4.3 percent and the likelihood of committing property crime by 11.8 percent.

The next row of Panel C gives the results for individuals with upper secondary education. Also for this group there is a significant effect on property crime, and relative to sample mean, the magnitude of the effect is about the same as for individuals with compulsory schooling. The last row of Panel C shows the results for individuals with university education. If we look across this row we can see the point estimates are relatively low and not statistically significant. These findings confirm that less educated individuals are more sensitive to their relative income position.

Panel D compares the working and the non-working populations. Since the non-working population has more time to commit crime one might expect stronger effects for this group.[12] Grönqvist (2011) and Rege et al. (2009) show that the effect of unemployment on crime partly can be explained by this mechanism. While the point estimate for property crime is larger among non-employed than employed; .031 compared to .010, the impact is lower for this group when related to the sample mean. In addition, the point estimate for violent crime is weakly significant for the employed population. The findings do not support the hypothesis that employment incapacitates individuals and thereby prevent them from committing crime. One potential explanation for the strong effect in the working population may be that this

[12] In the data there is only information on whether individuals work or not. I do not know if individuals are unemployed, long-term sick or out of labor force if they are registered as not working.

group have a more clear reference group (their colleagues) and therefore respond stronger to changes in relative income differences.

The last panel compares individuals with a criminal record and no criminal past. Criminal background is defined as having been convicted for a crime committed in the past 5 years. Contrasting the point estimates of the two groups suggest that the effect of relative income differences on property crime is solely driven by past offenders. The estimate of .268 suggests a 11.3 percent increase in the risk of property crime. The effect on violent crime is further close to 5 percent and statistically significant for this group. For individuals with no criminal background, the point estimates of property and violent crime are small and imprecisely estimated but the effect on any crime is still statistically significant. This may be due to increases in other crime categories. In the next section I analyze alternative crime outcomes.

## 5.4 ADDITIONAL RESULTS

### 5.4.1 Other crime outcomes

So far, the analysis has focused on property and violent crimes. In this section I consider drug related crimes and drunk driving as these are common types of crimes in Sweden. To investigate more serious crimes I also examine offences that have resulted in prison sentences. Table 4 shows the results. The coefficient .016 in Column (1) implies that a one standard deviation increase in relative income differences raises the probability of committing drug related crimes by almost 12 percent. The point estimate of drunk driving is not significant and close to zero. There is however a significant effect on the probability of being sentenced to prison. A one standard deviation increase in the Yitzhaki index increases the likelihood of prison by about 9 percent. The results thus suggest that inequality also matters for other crime outcomes than those traditionally analyzed.

### 5.4.2 Other reference groups

It is not clear from theory what geographic level inequality operates on. It is for instance possible that large geographic units do not perfectly reflect an individual's true comparison group or the pool of group of potential victims. On the other hand, most individuals cross the boundaries of smaller geographic units on a daily basis and a higher level might therefore better proxy an individual's true expected returns to crime. Note also that if the reference group is small, people more likely know the neighbors which may prevent them from committing crime (c.f. Glaeser and Sacerdote 1999). Nevertheless, neighborhood inequality may still provoke people to commit crime outside their neighborhood, at least if people commit crime to attain a certain material level.

To test if the choice of geographic unit is important, I regressed income inequality on crime for three different geographic units: parish, municipal, and county level. In my sample these units host on average about 4,000, 36,000 respective 200,000 individuals.[13] Table 5 shows that this exercise results in very similar point estimates. There is a significant effect of inequality on both any crime and property crime for all geographic units and the magnitude of the estimates is about the same. Interestingly, the results suggest that the choice between parish, municipality and county unit is not important for my analysis. These findings are consistent with a story that there might be mechanisms operating at different geographic levels. For instance, while the strain theory may explain the observed relationship between inequality and crime at the parish level economic incentives could instead underlie the relationship at the municipal and county levels.

### 5.4.3 Alternative specifications

[13] There are 2,512 parishes, 290 municipalities and 21 counties in Sweden.

This section explores alternative specifications. I start by using numbers of crimes as an outcome instead of a dichotomous variable. The obvious advantage of analyzing crime at the intensive margin is that it fully explores the information in the data. Starting with Column (1) of Panel B in Table 6, the highly significant coefficient of .188 suggests that a one standard deviation increase in relative income differences increases the number of crimes by about 7 percent. The effect is almost three times as large as in the main specification (see Panel A). We can see that the increase partly seems to be driven by property crime which increases by about 15 percent (9 percent in the main specification). This corresponds to an increase in the number of property crimes by 101 from a baseline of 700 per 100,000 individuals. The results imply that relative income differences not only affect the probability of engaging in property crime but also increase the number of property crimes committed by individuals already participating in crime. As in the main specification, the point estimate for violent crime is relatively small and not significant.

Disposable income is closely related to the economic model of crime as it reflects the net amount of money that an individual receives. Still, it is possible that people care about prestige and social status and therefore value earned income more than transferred welfare. In Panel D of Table 6 relative income differences as well as individual income are instead measured by gross labor earnings.[14] It is interesting to note that the point estimates are larger and more precisely estimated compared to the baseline results. A one standard deviation increase in relative income differences now raises the probability of committing property crime by 19.6 percent (9.1 percent in the baseline). The corresponding number for violent crime is 7.7 percent (1.6 percent in the baseline) and statistically significant. The results suggest that individuals care more about inequality in earnings than in disposable incomes. To get a sense of the magnitude of the effect of earnings inequality on crime I again compare the

[14] The within variation of the Yitzhaki earnings index is .024.

estimates with the educational crime gap between individuals with compulsory and university education. The estimates of earnings inequality imply an increase in the number of people committing crime by about 57 (property) and 18 (violent) per 100,000 individuals. These correspond to about 9.5 (57/600) and 3 (18/600) percent of the educational crime gap (compare with baseline estimates: 7.3 and 1 percent).

In the last panel of Table 6 I estimate the effect of an individual's percentile ranking in the (disposable) income distribution on the probability of committing crime.[15] In contrast to the Yitzhaki index this measure only accounts for an individual's relative position ignoring distances in incomes between individuals. The findings indicate that differences in income rank matters less than differences in income levels. A one standard deviation decrease in the percentile ranking position increases the probability of committing property crime by 4.7 percent (9.1 percent in baseline).

## 6. CONCLUDING REMARKS

Social scientists have for long been interested in knowing whether relative income affects criminal behavior as both economic and sociologic theories predict. The empirical relationship between relative income and crime is however still not well understood.

In this paper I analyze the effect of relative income on criminal behavior. I use rich individual level conviction data from various Swedish registers to examine this question. To the best of my knowledge all previous studies have used aggregate data and have therefore not been able to property address this issue. With individual data one avoids also the potential problem of a spurious correlation between relative income and crime that may arise at the aggregate level (described in Section 2.2).

[15] The within variation of the percentile ranking variable is .013.

I find evidence that relative income differences positively affect the probability of committing property crime. The effect is small and mainly driven by younger individuals, persons with low education, and past offenders. There is little support that a low relative income increases violent crime as has been hypothesized in sociologic theory. Neither do I find evidence that the geographic unit of analysis is important. There is however an effect of relative income on both the probability of getting sentenced to prison and of committing drug related crimes. The results thus suggest that inequality also may be important for other types of crimes than those traditionally analyzed.

The findings indicate that gross labor earnings inequality is more strongly related to criminal behavior than inequality in disposable income. One potential explanation for this pattern could be that earnings are more closely related to social status and prestige. It is also possible that labor earnings are easier to observe and therefore provide a better signal about the relative position in the income distribution than disposable income.

Overall, the results suggest that relative income affect criminal behavior. A set of robustness checks are performed on the data to assess the validity of the findings and the results appear not to be driven by reverse causality or omitted variable bias. However, the fact that the effect is small suggests that policies aiming at decreasing relative income differences would do little in reducing crime.

It is important to keep in mind that Sweden is considered as a country with a relatively low level of income inequality, and the results may therefore not be extended to other countries. In such a case, the results could still provide a lower boundary of the effect of relative income differences on crime.

Although it is beyond the scope of this study it would certainly be interesting to investigate the link between inequality and the probability of starting a criminal career since my results suggest that the effect is mainly driven by past offenders. Future studies should also analyze how long term exposure to inequality affects criminal behavior.

**REFERENCES**


Adda, J., McConnell, B. and I. Rasul (2011), "Crime and the Decriminalization of Cannabis: Evidence from a Localized Policing Experiment", Unpublished Manuscript, UCL.

Anderson, D.A. (1999), "The Aggregate Burden of Crime", *Journal of Law and Economics,* 42(2): 611–642.

Bayer, P., Hjalmarsson, R. and D. Pozen (2009), "Building Criminal Capital Behind Bars: Peer Effects in Juvenile Corrections", *Quarterly Journal of Economics,* 124(1): 105–147.

Becker, G. (1968), "Crime and Punishment: An Economic Approach", *Journal of Political Economy,* 76(2): 169–217.

Blau, J. and P. Blau (1982), "The Cost of Inequality: Metropolitan Structure and Violent Crime", *American Sociological Review,* 47(1): 114–129.

Bourguignon, F. (2001), "Crime as a Social Cost of Poverty and Inequality: A Review Focusing on Developing Countries", Yussuf, S., S. Evenett and W. Wu (eds), *Facets of Globalization: International and Local Dimensions of Development,.),* Washington: World Bank.

Brush, J. (2007), "Does Income Inequality Lead to More Crime? A Comparison of Cross-Sectional and Time-Series Analyses of United States Counties", *Economics Letters,* 96(2): 264–268.

Card, D. and G. Dahl (2011), "Family Violence and Football: The Effect of Unexpected Emotional Cues on Violent Behavior", *Quarterly Journal of Economics,* 126(1): 103–143.

Card, D., Mas, A., Moretti, M. and E. Saez (2010), "Inequality at work: the Effect of Peer Salaries on Job Satisfaction", NBER WP No. 16396.

Chiu, W. and P. Madden (1998), "Burglary and Income Inequality", *Journal of Public Economics,* 69(1): 123–141.

Choe, J. (2008), "Income Inequality and Crime in the United States", *Economic Letters,* 101(1): 31–33.

Cobb-Clarke, D. and S. Schurer (2011), "The Stability of Big-Five Personality Traits", IZA Discussion Paper No. 5943.

Dahl, G. and S. DellaVigna (2009), "Does Movie Violence Increase Violent Crime?", *Quarterly Journal of Economics,*124(2): 677–734.

Dahlberg, M. and M. Gustavsson (2008), "Inequality and Crime: Separating the Effects of Permanent and Transitory Income", *Oxford Bulletin of Economics and Statistics,* 70(2): 129–53.

Daly, M., Wilson, D. and N. Johnson (2013), " Relative Status and Well-Being: Evidence from U.S. Suicide Deaths", forthcoming in *Review of Economics and Statistics.*

Deming, D. (2011), “Better Schools, Less Crime?”, *Quarterly Journal of Economics*, 126(4): 2063–2015.

Demombynes, G. and B. Özler (2005),”Crime and Local Inequality in South Africa”, *Journal of Development Economics*, 76(2): 265–292.

Donohue, J. and S. Levitt (2001), “The Impact of Legalized Abortion on Crime”, *Quarterly Journal of Economics*, 116(2): 379–420.

Doyle, J. (2008), “Child Protection and Adult Crime: Using Investigator Assignment to Estimate Causal Effects of Foster Care”, *Journal of Political Economy*, 116(4): 746–770.

Draca, M., Machin, S. and R. Witt (2011), “Panic on the Streets of London: Police, Crime and the July 2005 Terror Attacks”, *American Economic Review*, 101(5): 2157–81.

Duggan, M. (2001), “More Guns, More Crime”, *Journal of Political Economy*, 109(5): 1086–1114.

Dustmann, C. and A. Piil Damm (2009), “The Effect of Growing Up in a High Crime Area on Criminal Behaviour: Evidence from a Random Allocation Experiment”, Unpublished Manuscript, UCL.

Dynan K. and E. Ravina (2007), “ Increasing Income Inequality, External Habits, and Self-Reported Happiness”, *American Economic Review*, 9(2): 226–231.

Ehrlich, I. (1973), “Participation in Illegitimate Activities: a Theoretical and Empirical Investigation”, *Journal of Political Economy*, 81(3): 521–565.

Eibner, C. and W. Evans (2005), “Relative Deprivation, Poor Health Habits, and Mortality”, *Journal of Human Resources*, 40(3): 591–620.

Fajnzylber, P., Lederman, D. and N. Loayza (2002a), “What Causes Violent Crime?”, *European Economic Review*, 46(7): 1323−1357.

Fajnzylber, P., Lederman, D. and N. Loayza (2002b), “Inequality and Violent Crime”, *Journal of Law and Economics*, 45(1): 1−39.

Fowles, R. and M. Merva (1996), “Wage Inequality and Criminal Activity: An Extreme Bounds Analysis for the United States”, *Criminology*, 34(2):163–182.

Freeman, R. (1996), “Why Do So Many Young American Men Commit Crimes and What Might We Do About It?”, *Journal of Economic Perspectives*, 10(1): 25–42.

Freeman, R. (1999), “The Economics of Crime”, O. Ashenfelter and D. Card (eds.), *Handbook of Labor Economics*, 3c, Amsterdam: Elsevier Science.

Glaeser E. and B. Sacerdote (1999), “Why is There More Crime in Cities?”, *Journal of Political Economy*, 107(6): 225–258.

Gravelle, H. (1998), "How Much of the Relation between Population Mortality and Unequal Distribution of Income is a Statistical Artifact?", *British Medical Journal*, 316(7128): 382–85.

Grönqvist, H. (2011), "Youth Unemployment and Crime: New Lessons Exploring Longitudinal Register Data", SOFI Working Paper No. 7/2011.

Grönqvist, H. and S. Niknami (2011), "Alcohol Availability and Crime: Lessons from Liberalized Weekend Sales Restrictions", SOFI Working Paper No. 9/2011.

Hipp, J. (2007), "Income Inequality, Race, and Place: Does the Distribution of Race and Class within Neighborhoods Affect Crime Rates?", *Criminology*, 45(3): 665–697.

Hjalmarson, R. and M. Lindquist (2011), "The Origins of Intergenerational Associations in Crime: Lessons from Swedish Adoption Data", SOFI Working Paper No. 11/2011.

Hsieh, C. and M.D. Pugh (1993), "Poverty, Inequality, and Violent Crime: A Meta-Analysis of Recent Aggregate Data Studies", *Criminal Justice Review*, 18(2): 182–202.

Jacob, B. and L. Lefgren (2003), "Are Idle Hands the Devil's Workshop? Incapacitation, Concentration and Juvenile Crime", *American Economic Review*, 93(5): 1560–1577.

Kawachi, I., Kennedy B. and R. Wilkinson (1999), "Crime: Social Disorganization and Relative Deprivation", *Social Science and Medicine*, 48(6): 719–731.

Kelly, M. (2000), "Inequality and Crime", *Review of Economics and Statistics*, 82(4): 530−539.

Kling, J., Ludwig, J. and L. Katz (2005), "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment", *Quarterly Journal of Economics*, 120(1): 87–130.

Lee, D. and J. McCrary (2009), "The Deterrence Effect of Prison: Dynamic Theory and Evidence", Unpublished Manuscript, Princeton University.

Lochner, L. and E. Moretti (2004), "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports", *American Economic Review*, 94(1):155–159.

Luttmer, E. (2005) "Neighbors as Negatives: Relative Earnings and Well-being", *Quarterly Journal of Economics*, 120(3): 963–1002.

Machin, S. and C. Meghir (2004), "Crime and Economic Incentives", *Journal of Human Resources*, 39(4): 958–979.

Meghir, C., Palme, M. and M. Schnabel (2011), "The Effect of Education Policy on Crime: An Intergenerational Perspective", IZA Discussion Paper No. 6142.

Merton, R. (1938), "Social Structure and Anomie", *American Sociological Review*, 3(5): 672–682.

Miller, D. (2001), "Income Inequality and Mortality in the US: Aggregated Data and Micro Relationships", Unpublished Manuscript, UC Berkeley.

Nilsson, A. (2004), "Income Inequality and Crime: the Case of Sweden", IFAU Working Paper No. 2004–6.

Rege, M., Skardhamar, T., Telle, K. and M. Votruba (2009), "The Effect of Plant Closures on Crime", Statistics Norway Discussion Paper No. 593.

Runciman, W. (1966), *Relative Deprivation and Social Justice*, London: Routledge and Kegan Paul.

Yitzhaki, S. (1979), "Relative Deprivation and the Gini Coefficient", *Quarterly Journal of Economics*, 93(2): 321–24.

Weiner, D., Lutz B. and J. Ludwig (2009), "The effects of school desegregation on crime", NBER Working Paper No. 15380.

**Table A.1** Share of people convicted for committing crime in 1990 by age

| | Aged 16–24 | Aged 25–65 |
|---|---|---|
| Share of convicted people (109,962) | 36.3% | 57.7% |
| Share of those getting convicted that are sentenced to prison | 8.9% | 17.8% |

*Notes*: 109,962 individuals in the conviction administrative register have committed a crime in 1990. The first column of row 1 shows the share of criminals in age-group 16–24 and the second column shows the shows the share of criminals in age-group 25–65. Row 2 gives the share of convicted individuals in each age-group who are sentenced to prison.

**Table A.2.** Definitions of crime categories

| Crime type | Explanation | Legal text |
|---|---|---|
| Any crime | Any recorded conviction in a criminal trial regardless of type of crime. | |
| Violent crime | The full spectrum of assaults from pushing and shoving that result in no physical harm to murder. | BRB Chapter 3; BRB Chapter 4; BRB Chapter 17 paragraphs 1,2,4,5,10 |
| Property crime | The full spectrum of property crimes from shop-lifting to burglary. Robbery is also included. | BRB Chapter 8 |
| Drug related crime | Dealing and possession of illicit drugs. | SFS 1968:64 |
| | | SFS 1951:649 |
| Drunk driving | Driving vehicle under the influence of alcohol. | |
| Prison | Sentenced to prison in criminal trial for any type of crime. | |

*Notes:* BRB=Brottsbalk (Criminal Code). SFS=Svensk författningssamling (Swedish Statute Book).

**Table A.3** Summary statistics for selected crime and inequality variables

| Variable | Mean | Overall std. dev. | Within std.dev. | Between std.dev. |
|---|---|---|---|---|
| *Crime characteristics* | | | | |
| Any crime | .023 | .150 | | |
| Violent crime | .004 | .062 | | |
| Property crime | .005 | .068 | | |
| Drugs | .002 | .045 | | |
| Drunk driving | .004 | .062 | | |
| *Inequality characteristics* | | | | |
| Yitzhaki /$10^6$ | .033 | .029 | .015 | .027 |
| Gini-coefficient | .229 | .043 | .022 | .035 |

*Notes*: The sample consists of men aged 25 to 65 observed from 1990 to 1999 (2,328,650 observations). For the Yitzhaki index the within std. dev. refers to the deviation from the individual average and for the Gini coefficient it refers to the deviation from the municipality average. The between std. dev. refers to the variation in average Yitzhaki index between individuals and to the variation in average Gini coefficient between municipalities. All numbers are rounded to three decimals.

**Table A.4** Descriptive statistics of the Yitzhaki index by subgroup

| *Yitzhaki index/$10^6$* | Mean | Overall std. dev. | Between std.dev. | Within std.dev. |
|---|---|---|---|---|
| *Age* | | | | |
| Less than 40 | .037 | .030 | .029 | .015 |
| 40 or older | .030 | .028 | .026 | .013 |
| *Education* | | | | |
| Compulsory school | .036 | .027 | .025 | .013 |
| Upper secondary school | .032 | .027 | .025 | .014 |
| University | .028 | .030 | .030 | .016 |
| *Employment* | | | | |
| Non-employed | .056 | .036 | .034 | .016 |
| Employed | .026 | .023 | .022 | .012 |
| *Criminal background (within 5 years)* | | | | |
| Criminal past | .047 | .033 | .030 | .016 |
| No criminal past | .032 | .028 | .027 | .014 |

*Notes*: The sample consists of men aged 25 to 65 observed from 1990 to 1999 (2,328,650 observations). The within std. dev. refers to the deviation from the individual average. The between std. dev. refers to the variation in average Yitzhaki index between individuals. All numbers are rounded to three decimals.

**Table A.5** Summary statistics for selected variables

| | Entire sample (1) | Crime equal to 0 in every period (2) | Crime equal to 1 in every period (3) | Crime equal to both 0 and 1 (3) |
|---|---|---|---|---|
| Age | 43.474<br>(11.335) | 43.871<br>(11.439) | 34.615<br>(8.909) | 40.832<br>(10.193) |
| Missing information on education | .017<br>(.128) | .015<br>(.121) | .077<br>(.267) | .029<br>(.167) |
| Compulsory schooling | .290<br>(.454) | .284<br>(.451) | .505<br>(.500) | .334<br>(.472) |
| Upper secondary school | .450<br>(.497) | .445<br>(.497) | .389<br>(.488) | .484<br>(.500) |
| University | .243<br>(.429) | .256<br>(.437) | .029<br>(.168) | .153<br>(.360) |
| Non-employed | .220<br>(.414) | .198<br>(.399) | .867<br>(.339) | .363<br>(.481) |
| Criminal past (up to 5 years) | .088<br>(.284) | .024<br>(.153) | .926<br>(.261) | .521<br>(.500) |
| Disposable income | 127,106<br>(197,080) | 129,699<br>(189,157) | 51,948<br>(36,370) | 110,002<br>(244,222) |
| Number of observations | 2,328,650 | 2,029,090 | 2,346 | 297,214 |

*Notes:* The sample in column (1) consists of males aged 25 to 65 observed at least once in 1990 to 1999. Column (2) gives the descriptive statistics for those who never commit a crime during the period. Column (3) shows the sample statistics for those who commit a crime in each period that they are in the sample. The last column gives the descriptive statistics for those who at least once commit a crime *and* at least once do not commit a crime. Standard errors are given in parentheses. All numbers are rounded to three decimals.

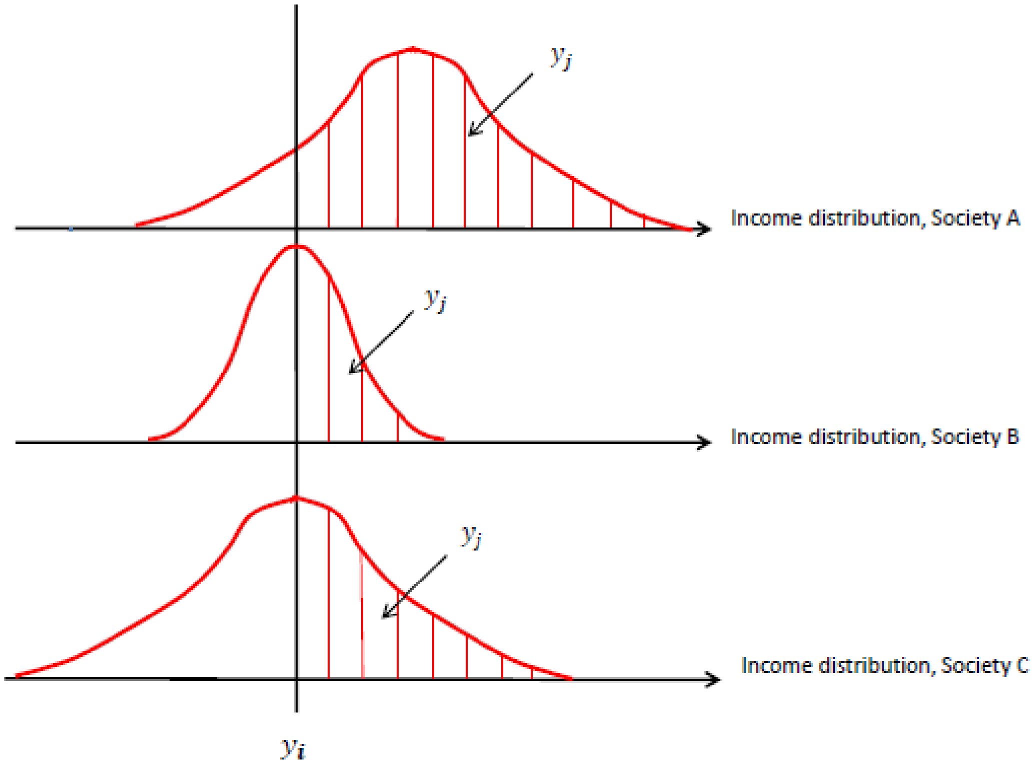


**Figure 1.** The income distribution of society A, B and C. The vertical line marks individual i´s absolute income.

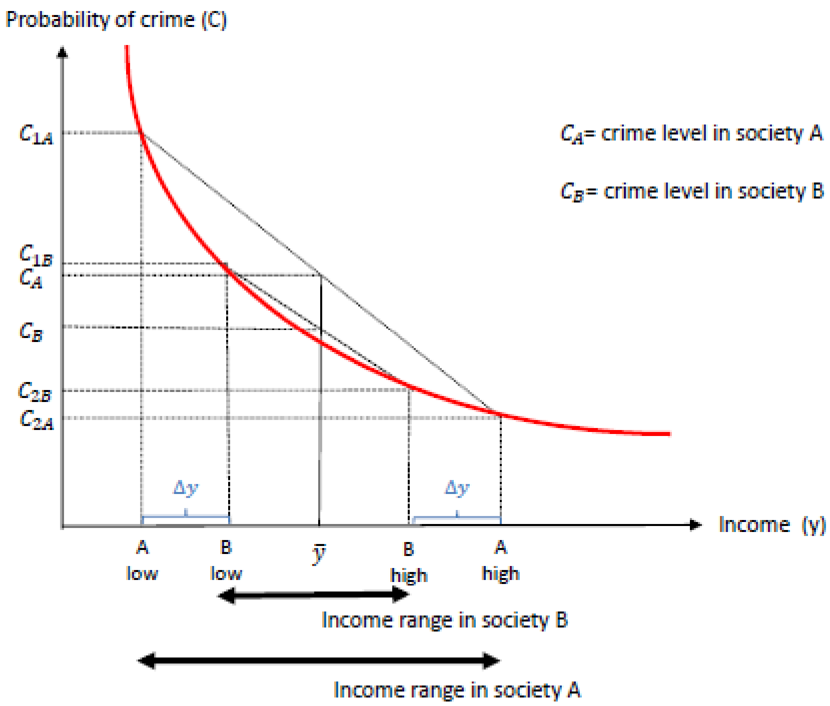


**Figure 2.** The probability of crime as a convex function of individual income.

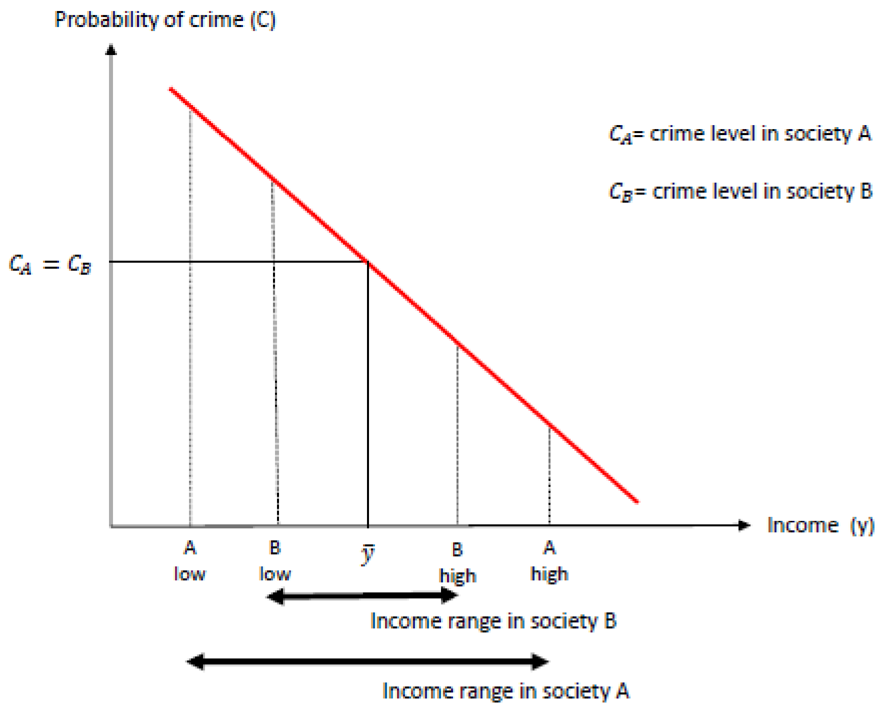


**Figure 3.** The probability of crime as a linear function of individual income.

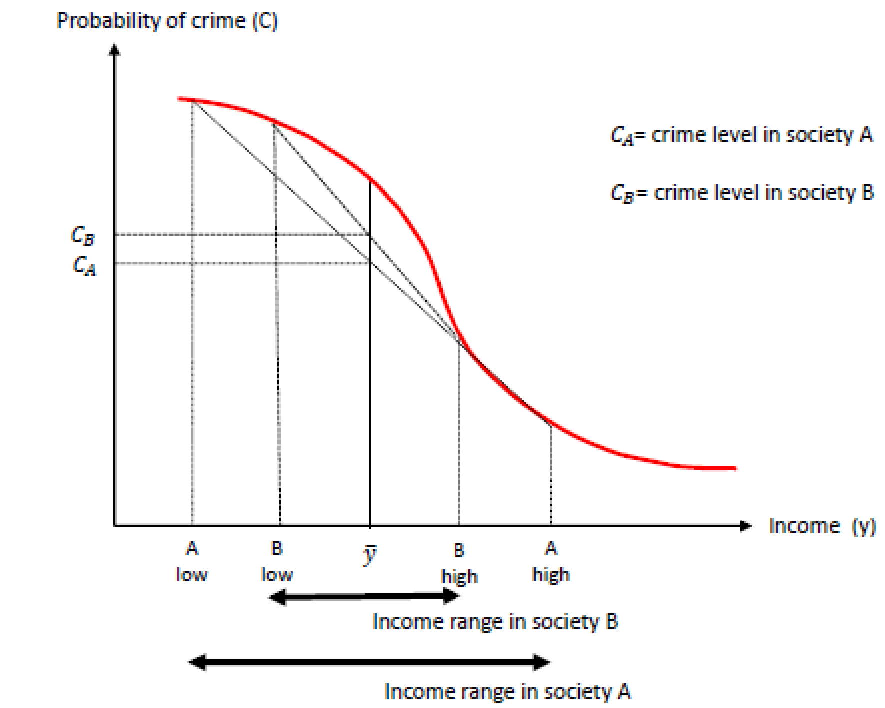


**Figure 4.** The probability of crime as both a concave and convex function of individual income.

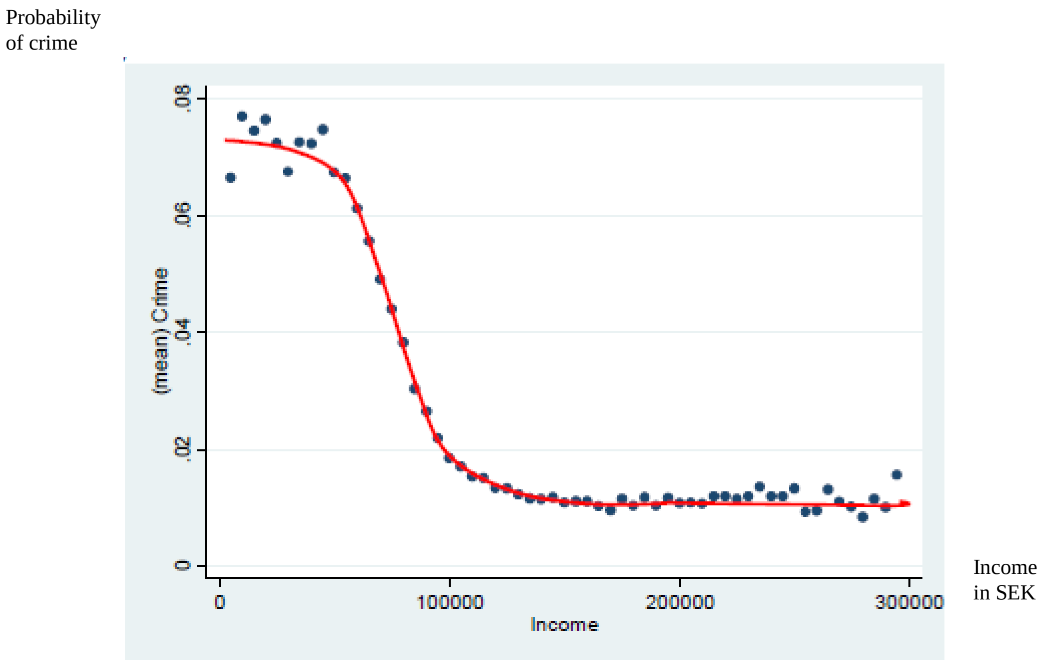


**Figure 5.** The probability of crime as a function of individual income. The sample consists of men aged 25 to 65 observed from 1990 to 1999 (2,328,650 observations).

**Table 1.** The effect of relative income differences on the probability of committing a given type of crime

| | Dependent variable | | |
|---|---|---|---|
| | Any crime<br><br>(1) | Violent crime<br>(2) | Property crime<br>(3) |
| A. Pooled OLS | .530***<br>(.010)<br>[33.1%] | .110***<br>(.003)<br>[42.0%] | .186***<br>(.006)<br>[58.2%] |
| B. Pooled OLS (restricted sample) | .534***<br>(.010)<br>[33.4%] | .111***<br>(.003)<br>[42.4%] | .187***<br>(.006)<br>[58.6%] |
| C. Individual FE | .038***<br>(.010)<br>[2.4%] | .004<br>(.004)<br>[1.6%] | .029***<br>(.004)<br>[9.1%] |
| Outcome mean | .023 | .004 | .005 |
| Municipality FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* The dependent variable is set to one if the individual has committed a given type of crime and zero otherwise. Each cell presents a separate regression. Relative income differences are measured by the Yitzhaki index. Each coefficient and its corresponding standard error is scaled by $10^6$. The sample consists of men aged 25 to 65 observed during the period 1990 to 1999(2,328,650 observations). Men who are only in the sample one period or who do not experience any change in Yitzhaki are excluded from the regressions in Panel B (2,315,409 observations). All regressions control for a cubic in both age and income. The regressions in Panels A and B also control for education dummies (five levels), missing values and foreign-born. The standard errors in parentheses are robust to serial correlation and heteroscedasticity at the individual level. All numbers are rounded to three decimals. Percent and significance level have been calculated based on non-rounded values. *** = significant at 1 %, ** = significant at 5 %, * = significant at 10 %.

**Table 2.** Robustness checks

| | Dependent variable | | |
|---|---|---|---|
| | Any crime<br>(1) | Violent crime<br>(2) | Property crime<br>(3) |
| A. Baseline | .038***<br>(.010)<br>[2.4%] | .004<br>(.004)<br>[1.6%] | .029***<br>(.004)<br>[9.1%] |
| B. Std. errors clustered at the municipality level | .038***<br>(.012)<br>[2.5%] | .004<br>(.004)<br>[1.5%] | .029***<br>(.006)<br>[8.7%] |
| C. Municipality × year FE | .044***<br>(.011)<br>[2.7%] | .004<br>(.004)<br>[1.4%] | .035***<br>(.005)<br>[11.0%] |
| D. Estimating the effect of crime committed in quarter 4 | .003<br>(.005)<br>[0.6%] | .000<br>(.002)<br>[0.5%] | .008***<br>(.002)<br>[8.0%] |
| E. Removing control for own income | .033***<br>(.009)<br>[2.1%] | .003<br>(.004)<br>[1.3%] | .028***<br>(.004)<br>[8.8%] |
| F. At least 30 years old | .035***<br>(.010)<br>[2.3%] | .004<br>(.004)<br>[1.5%] | .024***<br>(.004)<br>[8.0%] |
| *Outcome mean:* | | | |
| Baseline | .023 | .004 | .005 |
| Quarter 4 | .007 | .001 | .001 |
| At least 30 years old | .022 | .003 | .004 |
| Individual FE | Yes | Yes | Yes |
| Municipality FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* The dependent variable is set to one if the individual has committed a given type of crime and zero otherwise. Each cell presents a separate regression Relative income differences are measured by the Yitzhaki index. Each coefficient and its corresponding standard error is scaled by $10^6$. The sample consists of men aged 25 to 65 observed during the period 1990 to 1999 (2,328,650 observations) except in panel E where individuals below 30 are excluded (2,011,116 observations). All regressions control for a cubic in both age and income. Standard errors in parentheses are robust to serial correlation and heteroscedasticity at the individual level (except in panel B.). All numbers are rounded to three decimals. Percent and significance level have been calculated based on non-rounded values. *** = significant at 1 %, ** = significant at 5 %, * = significant at 10 %.

**Table 3.** Effect of relative income differences on crime by subgroups

| | Dependent variable | | |
|---|---|---|---|
| | Any crime | Violent crime | Property crime |
| | (1) | (2) | (3) |
| A. Total sample | | | |
| Baseline | .038*** | .004 | .029*** |
| *(N: 571,904)* | (.010) | (.004) | (.004) |
| | [2.4%] | [1.6%] | [9.1%] |
| *Outcome mean:* | .023 | .004 | .005 |
| B. Age | | | |
| Less than 40 | .057*** | .009 | .049*** |
| *(N: 936,174)* | (.016) | (.007) | (.008) |
| | [2.7%] | [2.4%] | [10.6%] |
| *Outcome mean:* | .033 | .006 | .007 |
| 40 or older | .024** | .002 | .013*** |
| *(N: 1,392,476)* | (.012) | (.004) | (.004) |
| | [1.9%] | [1.2%] | [5.9%] |
| *Outcome mean:* | .017 | .002 | .003 |
| C. Education | | | |
| Compulsory school | .097*** | .004 | .062*** |
| *(N:675,906)* | (.023) | (.010) | (.011) |
| | [4.3%] | [1.1%] | [11.8%] |
| *Outcome mean:* | .029 | .005 | .007 |
| Upper secondary school | .064*** | .010 | .037*** |
| *(N:1,047,910)* | (.016) | (.006) | (.007) |
| | [3.6%] | [3.4%] | [11.1%] |
| *Outcome mean:* | .025 | .004 | .005 |
| University | .000 | .001 | .003 |
| *(N:565,756)* | (.014) | (.004) | (.003) |
| | [0.0%] | [1.2%] | [4.0%] |
| *Outcome mean:* | .011 | .001 | .001 |
| Individual FE | Yes | Yes | Yes |
| Municipality FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* The dependent variable is set to one if the individual has committed a given type of crime and zero otherwise. Each cell presents a separate regression. Relative income differences are measured by the Yitzhaki index. Each coefficient and its corresponding standard error is scaled by $10^6$. The sample consists of men aged 25 to 65 observed during the period 1990 to 1999. All regressions control for a cubic in both age and income. Standard errors in parentheses are robust to serial correlation and heteroscedasticity at the individual level. All numbers are rounded to three decimals. Percent and significance level have been calculated based on non-rounded values. *** = significant at 1 %, ** = significant at 5 %, * = significant at 10 %.

**Table 3.** *(Continued).*

| | Dependent variable | | |
|---|---|---|---|
| | Any crime<br><br>(1) | Violent crime<br>(2) | Property crime<br>(3) |
| D. Employment | | | |
| Non-employed | -.001 | -.004 | .031** |
| *(N:511,890)* | (.023) | (.011) | (.013) |
| | [0.0%] | [-.6%] | [3.1%] |
| *Outcome mean:* | .052 | .011 | .016 |
| | | | |
| (h) Employed | .044*** | .006* | .010*** |
| *(N:1,816,760)* | (.011) | (.004) | (.003) |
| | [3.3%] | [4.1%] | [7.4%] |
| *Outcome mean:* | .015 | .002 | .002 |
| | | | |
| E. Criminal background | | | |
| Have criminal past | .238*** | .085*** | .268*** |
| *(N:205,442)* | (.059) | (.030) | (.034) |
| | [2.9%] | [4.9%] | [11.3%] |
| *Outcome mean:* | .131 | .028 | .038 |
| | | | |
| (j) No criminal past | .026*** | -.001 | -.001 |
| *(N:2,123,208)* | (.008) | (.002) | (.002) |
| | [2.8%] | [-0.8%] | [-0.9%] |
| *Outcome mean:* | .013 | .002 | .001 |
| Individual FE | Yes | Yes | Yes |
| Municipality FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* The dependent variable is set to one if the individual has committed a given type of crime and zero otherwise. Each cell presents a separate regression. Relative income differences are measured by the Yitzhaki index. Each coefficient and its corresponding standard error is scaled by $10^6$. The sample consists of men aged 25 to 65 observed during the period 1990 to 1999 (2,328,650 observations). All regressions control for a cubic in both age and income. Standard errors in parentheses are robust to serial correlation and heteroscedasticity at the individual level. All numbers are rounded to three decimals. Percent and significance level have been calculated based on non-rounded values. *** = significant at 1 %, ** = significant at 5 %, * = significant at 10 %.

**Table 4**. Other crime outcomes

| | Dependent variable | | |
|---|---|---|---|
| | Drugs | Drunk driving | Prison |
| | (1) | (2) | (3) |
| Yitzhaki index | .016*** | .005 | .035*** |
| | (.003) | (.004) | (.005) |
| | [11.6%] | [1.6%] | [9.1%] |
| *Outcome mean:* | .002 | .004 | .005 |
| Individual FE | Yes | Yes | Yes |
| Municipality FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* The dependent variable is set to one if the individual has committed a given type of crime and zero otherwise. Each cell presents a separate regression. Relative income differences are measured by the Yitzhaki index. Each coefficient and its corresponding standard error is scaled by $10^6$. The sample consists of men aged 25 to 65 observed during the period 1990 to 1999 (2,328,650 observations). All regressions control for a cubic in both age and income. Standard errors in parentheses are robust to serial correlation and heteroscedasticity at the individual level. All numbers are rounded to three decimals. Percent and significance level have been calculated based on non-rounded values. *** = significant at 1 %, ** = significant at 5 %, * = significant at 10 %.

**Table 5.** Estimates of the effect of relative income differences on the probability of committing a given type of crime using different reference groups

| | Dependent variable | | |
|---|---|---|---|
| | Any crime (1) | Violent crime (2) | Property crime (3) |
| *Reference group:* | | | |
| A. Parish | .039*** | .005 | .026*** |
| | (.009) | (.003) | (.004) |
| | [2.7%] | [1.9%] | [8.9%] |
| B. Municipality | .038*** | .004 | .029*** |
| | (.010) | (.004) | (.004) |
| | [2.4%] | [1.6%] | [9.1%] |
| C. County | .042*** | .004 | .034*** |
| | (.011) | (.004) | (.005) |
| | [2.4%] | [1.3%] | [9.8%] |
| Outcome mean | .023 | .004 | .005 |
| Individual FE | Yes | Yes | Yes |
| Reference group FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* The dependent variable is set to one if the individual has committed a given type of crime and zero otherwise. Each cell presents a separate regression. Relative income differences are measured by the Yitzhaki index. Each coefficient and its corresponding standard error is scaled by $10^6$. The sample consists of men aged 25 to 65 observed during the period 1990 to 1999 (2,328,650 observations). All regressions control for a cubic in both age and income. Standard errors in parentheses are robust to serial correlation and heteroscedasticity at the individual level. All numbers are rounded to three decimals. Percent and significance level have been calculated based on non-rounded values. *** = significant at 1 %, ** = significant at 5 %, * = significant at 10 %.

**Table 6.** Alternative specifications

| | Dependent variable | | |
|---|---|---|---|
| | Any crime<br><br>(1) | Violent crime<br>(2) | Property crime<br>(3) |
| A. Baseline | .038***<br>(.010)<br>[2.4%] | .004<br>(.004)<br>[1.6%] | .029***<br>(.004)<br>[9.1%] |
| B. Intensive margin | .188***<br>(.025)<br>[6.6%] | .011<br>(.007)<br>[2.9%] | .067***<br>(.008)<br>[14.9%] |
| C. Relative labor earnings differences | .085***<br>(.008)<br>[8.8%] | .012***<br>(.003)<br>[7.7%] | .038***<br>(.003)<br>[19.6%] |
| D. Percentile rank | -.029***<br>(.008)<br>[-1.6%] | -.001<br>(.003)<br>[-0.4%] | -.017***<br>(.003)<br>[-4.7%] |
| *Outcome mean:* | | | |
| Baseline | .023 | .004 | .005 |
| Intensive margin | .041 | .005 | .007 |
| Individual FE | Yes | Yes | Yes |
| Municipality FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |

*Notes:* The dependent variable is set to one if the individual has committed a given type of crime and zero otherwise. Each cell presents a separate regression. Relative income differences are measured by the Yitzhaki index. Each coefficient and its corresponding standard error is scaled by $10^6$ in Panels A to C and by $10^3$ in Panel D. The sample consists of men aged 25 to 65 observed during the period 1990 to 1999 (2,328,650 observations). All regressions control for a cubic in both age and income (earnings in Panel C). Standard errors in parentheses are robust to serial correlation and heteroscedasticity at the individual level. All numbers are rounded to three decimals. Percent and significance level have been calculated based on non-rounded values. *** = significant at 1 %, ** = significant at 5 %, * = significant at 10 %.