IFAU – INSTITUTE FOR LABOUR MARKET POLICY EVALUATION

Inequality and crime:
separating the effects of permanent and transitory income

Matz Dahlberg
Magnus Gustavsson

WORKING PAPER 2005:19

The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Industry, Employment and Communications, situated in Uppsala. IFAU's objective is to promote, support and carry out: evaluations of the effects of labour market policies, studies of the functioning of the labour market and evaluations of the labour market effects of measures within the educational system. Besides research, IFAU also works on: spreading knowledge about the activities of the institute through publications, seminars, courses, workshops and conferences; creating a library of Swedish evaluational studies; influencing the collection of data and making data easily available to researchers all over the country.

IFAU also provides funding for research projects within its areas of interest. There are two fixed dates for applications every year: April 1 and November 1. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The authority has a traditional board, consisting of a chairman, the Director-General and eight other members. The tasks of the board are, among other things, to make decisions about external grants and give its views on the activities at IFAU. A reference group including representatives for employers and employees as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P.O. Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

ISSN 1651-1166

Inequality and crime: separating the effects of permanent and transitory income[♣]

by

Matz Dahlberg[♦] and Magnus Gustavsson[♥]

June 27, 2005

Abstract

Earlier studies on income inequality and crime have typically used total income or total earnings. However, it is quite likely that it is changes in permanent rather than in transitory income that affects crime rates. The purpose of this paper is therefore to disentangle the two effects by, first, estimating region-specific inequality in permanent and transitory income and, second, estimating crime equations with the two separate income components as explanatory variables. The results indicate that it is important to separate the two effects; while an increase in the inequality in permanent income yields a positive and significant effect on total crimes and three different property crimes, an increase in the inequality in transitory income has no significant effect on any type of crime. Using a traditional, aggregate, measure of income yields mainly insignificant effects on crime.

[♣] We have received useful comments and suggestions from Patrik Hesselius, Matthew Lindquist, Eva Mörk, and from seminar participants at FIEF, SOFI and Uppsala University. Magnus Gustavsson gratefully acknowledge financial support from the Jan Wallander and Tom Hedelius Foundation.

[♦] Department of Economics, Uppsala University and IFAU; e-mail: Matz.Dahlberg@nek.uu.se

[♥] Department of Economics, Uppsala University; e-mail: Magnus.Gustavsson@nek.uu.se

Table of contents

1 Introduction

Earlier studies on income inequality and crime have typically used inequality in total income or total earnings as explanatory variables. However, income can be considered as consisting of two parts, one permanent and one transitory, and it is quite likely that it is changes in the permanent part rather than in the transitory part that affects crime rates, as the two have different ramifications for the duration of inequality. An increase in the dispersion of permanent income leads to greater income inequality in both the short- and the long-term. An increase in the dispersion of transitory income, on the other hand, only creates short-term inequality.

The purpose of this paper is to disentangle the effects from inequality in permanent income from the effects from inequality in transitory income on crime. This is done in two steps. In the first step, we estimate region-specific inequality in permanent and transitory income using a very rich dataset on Swedish individuals. While several previous studies have decomposed year-to-year changes in inequality into its permanent and transitory components, this is the first time that these estimations have been carried out on a regional level.[1] In the second step, we estimate crime equations with the two estimated income components as explanatory variables. This is, as far as we know, the first time that this separation of income has been used in the literature estimating the effects of income inequality on crime.

The results indicate that it is important to separate the two effects; while an increase in the inequality in permanent income yields a positive and significant effect on property crime, an increase in the inequality in transitory income has no significant effect on any type of crime. When using a traditional, composite, measure of income, we get mainly insignificant effects on crime.

The rest of the paper is organized as follows. Section 2 presents some theoretical considerations on the relationship between income inequality and crime, and section 3 presents the income data and estimates permanent and transitory income for each county in Sweden. Section 4 discusses the econometric specification of the crime equations and the data to be used when estimating them. Section 5 presents the results, and, finally, section 6 concludes.

---

[1] The previous studies include Moffitt & Gottschalk (1995, 2002) and Haider (2001) for the US, Dickens (2000) for the UK, Cappellari (2004) for Italy, and Gustavsson (2004a, b) for Sweden.

2 Income inequality and crime: theoretical considerations

Why should there be an association between crime rates and income inequality? It turns out that both economic and sociological theory has linked income inequality to criminal activity. While economists have suggested that inequality may capture differential returns to criminal activity and, thereby, have an association with crime rates, sociologists have hypothesized that inequality and social welfare in general may work through other channels; inequality may be associated with lack of social capital, lack of upward mobility, or social disorganization, all of which may cause higher levels of crime.[2]

Ever since Becker's seminal work in the late 1960s (Becker, 1968), economists have suggested that economic incentives for crimes are higher in areas with greater income inequality (see eg Ehrlich, 1973, Chiu & Madden, 1998, Bourguignon, 2001, and Chisholm & Choe, 2005). In his work, Becker (1968) proposes an occupational choice model in which the incentives for individuals to commit crime are determined by the differential returns from legitimate and illegitimate pursuits.[3]

However, based on economic theory, the sign of the effect of income inequality on crime is ambiguous. This is because income inequality may also be associated with the level of protection from crime. Private crime protection measures may include guard dogs, bars on windows, electric fences, and alarm systems with armed security response. Chiu & Madden (1998) provide a model that allows for richer neighborhoods to have lower crime rates, partly because they may employ effective defense strategies against crime.

Turning to sociological theories, it has been put forward that the prevalence of crime may be linked to a lack of upward mobility in society. Coser (1968, cited by Blau & Blau, 1982, p. 119) argues that people who perceive their poverty as permanent may be driven by hostile impulses rather than rational pursuit of their interests. Wilson & Daly (1997) hypothesize that sensitivity to

[2] The discussion of these mechanisms follows Demombynes & Özler (2005).

[3] Hence, income inequality is supposed to be associated with crime levels via a relationship with the returns from crime and non-crime activities.

inequality, especially by those at the bottom, leads to higher risk tactics, such as crime, when the expected payoffs from low-risk tactics are poor. If income inequality is correlated with social mobility, then these theories imply a higher prevalence of criminal behavior in more unequal areas.

Closely related to theories involving social mobility are those related to social disorganization and crime. In an influential paper, Merton (1938) proposes that the lack of upward mobility in a society, combined with a high premium on economic affluence results in anomie, a breakdown of standards and values. According to Merton, poverty or even “poverty in the midst of plenty” alone is not sufficient to induce high levels of crime. Only when their interaction with other interdependent social and cultural variables is considered, one can explain the association between crime and poverty.

The above theories, connecting crime to inequality, have spawned a large number of empirical studies. Most of these have estimated whether crime rates are affected by different measures of income inequality, using such measures as the Gini coefficient, the variance of log income, and different percentile quotients, like the 90/10-quotient. These different measures do however yield quite different conclusions in different studies. For example, while certain studies using US data find a significant and positive relation between the Gini coefficient and crime rates (see Freeman, 1999, for an overview), Nilsson (2004) find no significant effects from the Gini coefficient on crime rates using Swedish data.

We believe that one explanation for the diverging results in the literature may be due to the use of an aggregate measure of income. It is a relatively old thought in economics, dating back at least to Friedman & Kuznets (1954), that an individual’s income in a given period can be divided into a permanent and a transitory component. Since changes in permanent and transitory income have different ramification for the duration of inequality, it is quite likely that they will have different impacts on the crime rates. From the sociological theories related to above, it is clear that it is an individual’s permanent position in society that is the main factor affecting one’s decision to commit crime or not, not the individual’s transitory deviation from the permanent position. From the economic theories, it is however not clear whether it is inequality in permanent or in transitory income that matters. Using an aggregate measure of income, as the earlier studies have done, will however restrict inequality in permanent and transitory income to have the same impact.

To make the argument clear, consider the following model:

(1) $$y_{it} = u_i + \varepsilon_{it},$$

where y_{it} is the log of total income in period t for individual i, $u_i \sim (\bar{u}, \sigma_u^2)$ is permanent income for individual i which is assumed to be constant over the life-cycle and have a constant variance, and $\varepsilon_{it} \sim iid(0, \sigma_\varepsilon^2)$ capture transitory stochastic deviations from permanent earnings. With this model, the cross-sectional variance of income in year t is:

(2) $$Var(y_{it}) = \sigma_u^2 + \sigma_\varepsilon^2,$$

that is, the variance of total income is the sum of the variance of permanent income and the variance of transitory income.

Equation (2) illustrates the potential pitfall of using total income. For example, suppose that changes in the variance of total income is used to study whether income inequality can explain differences in crime rates across different regions. If only permanent inequality affect crime rates, equation (2) shows that such a study has rather limited prospects of obtaining clear or systematic evidence. A region with both low inequality in permanent income and low crime rates may have a large dispersion in transitory earnings, and hence a large cross-sectional variance in income. The results may hence show that inequality has no, or even a negative, effect on crime.

In the end, it is an empirical question whether inequality in permanent income has another impact on crime rates than inequality in transitory income. Therefore, the aim with this paper is to allow the two income components in equation (2) to have separate effects on the crime rates.

3 Permanent and transitory income

3.1 An econometric model of income dynamics

Even though the permanent earnings model presented in the preceding section is intuitive, an empirical model of income dynamics must have several additional properties. To begin with, the variance of income must be allowed to change over time. The following enhanced model allows for this:[4]

$$(3) \qquad y_{it} = p_t u_i + \varepsilon_{it} .$$

In equation (3), the variable u_i and its year-specific factor loading, p_t, capture permanent, or persistent, income. As an approximation, the variable u_i can be thought of as capturing all individual characteristics that matter for permanent relative income and p_t as reflecting the time-varying price of these characteristics, but in practice, this specification is simply a means to allow the variance of permanent income to change over time. As before, the variable ε_{it} capture stochastic transitory deviations from permanent income but its variance is now year specific, denoted $\sigma^2_{\varepsilon_t}$. With this model, the variance of log income is

$$(4) \qquad Var(y_{it}) = p_t^2 \sigma_u^2 + \sigma^2_{\varepsilon_t} ,$$

and the auto-covariance between year *t* and *t-s* is

$$(5) \qquad Cov(y_{it}, y_{i,t-s}) = p_t p_{t-s} \sigma_u^2 .$$

Equation (4) demonstrates that an increase in the dispersion of either permanent or transitory income both generates increased cross-sectional income dispersion. The character of the change depends crucially, however, on which of these two components that changes. A persistent rise in the permanent component increases long-run inequality as the relative labor market advantage of workers with chronically high income is enhanced. An increase in the transitory component, without any change in the dispersion of permanent income, generates increased cross-sectional income dispersion by raising year-to-year income instability but with no change in long-term inequality.

Changes in the permanent and transitory components are closely related to changes in measures of income mobility, ie changes in the rate at which individuals shift positions in the income distribution (transition across quantiles

[4] The description of this model draws heavily on Baker & Solon (2003).

of the income distribution). Increases in the permanent component will cause the auto-covariances to grow in greater proportion than the variances, so auto-correlations increase. In contrast, increases in the transitory component alone will only increase the variances, so auto-correlations decrease. Equal proportional increases in the two components will leave auto-correlations unchanged, even though individual income instability will be increased. Changes in auto-correlations thus identify changes in the ratio of persistent to transitory income inequality.[5]

Previous studies have shown that more additional features must be added to a model of income dynamics if one should correctly estimate changes in permanent and transitory inequality (see the discussion in Baker & Solon, 2003, and the references therein). In particular, the permanent and transitory income components should be allowed to vary with age and transitory shocks should be allowed to last for several periods.

For each county separately, the following generalization of equation (3) is used:[6]

$$y_{iat} = p_t u_{ia} + q_a \varepsilon_{it}, \tag{6}$$

where

$$u_{ia} = u_{i,a-1} + r_{ia}, \tag{7}$$

and

$$\varepsilon_{it} = \rho\varepsilon_{i,t-1} + \delta v_{i,t-1} + v_{it}, \tag{8}$$

---

[5] A more detailed discussion of the connection to income mobility can be found in Moffitt & Gottschalk (1995).

[6] Some of the models that we have experimented with produce negative estimates of some of the variances or show other clear signs of being over-specified – these models are naturally discarded. The model presented here is the most advanced possible without over-specifying the model. We have not applied Newey's (1985) specification test as the previous literature on income dynamics show that this test always (at least in the studies that we are aware of) rejects the hypothesis that the right model is specified. The drawbacks with this test for assessing the goodness of fit of models of income dynamics are further discussed in Baker (1997) and Baker & Solon (2003). A general critic of tests such as that proposed in Newey (1985) is also found in Leamer (1983).

where a indicates age. Beginning with the permanent component, equations (6) and (7) model permanent income as a random walk in age where the innovation at each age is $r_{i,a} \sim \text{iid}(0, \sigma^2_{r,a})$.[7] The innovation variance at each age, $\sigma^2_{r,a}$, is allowed to take on one value up until age 34 and another one thereafter. We also estimate the variance of an initial permanent shock that capture the accumulation of individuals' permanent shocks up to the start of the sample period, denoted σ^2_u. We found that not allowing for age varying permanent shocks produced noticeable different results as well as substantially larger standard errors for the estimated parameters. By allowing for age variation we recognize that younger individuals are more likely to be engaged in job-shopping and have no job-securing tenure and are therefore more likely to experience larger permanent shocks to income.

For the transitory component, q_a in equation (6) is an age-specific parameter that permits the magnitude of the transitory component to vary with age.[8] It is allowed to take on three different values, one for individuals aged no more than 34 years, one for individuals aged 35 to 44, and one for individuals aged 45 and above. Like for the permanent component we found age variation to be important for the precision of the estimates. Equation (8) models deviations from permanent income as a first order autoregressive moving average process with a year-specific innovation ν_{ibt}, which in turn has a year-specific variance, denoted $\sigma^2_{\nu_t}$.

Although the model in equations (6)-(8) is more complex than that in equation (3), the intuition from the simpler model still holds. An increase in the permanent variance preserves the order of individuals in the income distribution but spreads them out further, and this greater spread remains year after year. An increase in the transitory variance leads to more scrambling of workers' order in the annual income distribution, and the scrambling gets redone every year.

[7] A random walk specification is also used, among others, in Dickens (2000), Moffitt & Gottschalk (2002), and Gustavsson (2004a, b). The use of a "random growht model" for permanent earnigs results in negative estimates of some of its variances; the same results for Sweden is also found, and discussed, in Gustavsson (2004a, b).

[8] In this model, the terms "permanent" and "transitory" are questionable. To be consistent with previous studies, however, we use the term permanent for the non-mean reverting component and transitory for the mean reverting component.

3.2 Income data and estimation methodology

To calculate permanent and transitory income inequality from 1974 to 2000 we use the register-based longitudinal database LINDA, constructed to be cross-sectionally representative of the Swedish population each year.[9] The dataset is large; it contains 3.35 percent of the Swedish population each year corresponding to over 300,000 individuals. An attractive feature of the database is that attrition from the sample can be due only to death or migration. Information about individuals' incomes comes from tax reports, so the income variable is free of the measurement errors that are common in survey data such as recall errors, rounding errors and top-coding.

The definition of an individual's income used in the analysis is the log of total earnings from all jobs during a year (including sickness benefits); like Nilsson (2004) we use this income definition since it is the most consistent over time available in the LINDA database.[10] Consequently, our inequality measures will to a large extent reflect inequality in labor market outcomes. However, labor earnings are the primary source of income for a majority of people and therefore most likely highly correlated with alternative measures of income.[11]

To estimate the parameters of equations (6)-(8) we employ the minimum distance estimator described in Chamberlain (1984) and Abowd & Card (1989). This means that variances and auto-covariances of income constitute the dependent variable in the estimations. To calculate these we first, for each county and year 1974 to 2000, select all males 20 to 59 years old with positive income; thus the resulting panels are unbalanced as individuals may have missing values for some years.[12] This design permits us to use the largest possible sample in the construction of each element of the auto-covariance matrices. The restriction to males is because the large changes in female labor

[9] The registers are maintained by Statistics Sweden; see Edin & Fredriksson (2000) for details.

[10] See Edin & Fredriksson (2000) for a detailed description of how the earnings measure in LINDA is constructed.

[11] Of course, other measures of income, preferable disposable family income, could yield different estimates in how the permanent and transitory variances have evolved over time. However, since such a measure is not available in LINDA, we focus on labor earnings solely since this measure is the most straightforward to interpret.

[12] Because data on income stem from tax-reports we must recognize that there is an annual income threshold for being forced to fill a tax report. The highest threshold is for 1974; 21,213 real SEK in 2000. Our definition of positive income is thus real income above the 1974 threshold.

force participation during the sample period would confound an analysis of female income (this restriction is standard in the literature on permanent and transitory inequality).[13] The resulting total sample sizes for each county range from 2,463 (Jämtland) to 31,585 (Stockholm) individuals.[14] It is worth pointing out that the sample sizes for the counties in many cases actually match the total sample size in the PSID used in the US studies by Haider (2000) and Moffitt & Gottschalk (1995, 2002), and in several cases are larger.

In constructing the auto-covariance matrices for different cohorts we employ the methodology used by Moffitt & Gottschalk (1995). For each year, individuals are categorized into four 10-year age cohorts: 20-29, 30-39, 40-49, and 50-59. This level of disaggregation assures a minimum of 160 individuals in the calculations of the auto-covariances, though a majority of the calculations invoke a substantially larger sample. In year t of the data, we divide the individuals into these four age groups and follow them through to year $t+1$, year $t+2$, etc until either the end of our data is reached (the year 2000) or until the age interval in question reaches beyond age 59.[15] For example, the 40-49 cohort in 1974 can be followed through to 1984 when they are 50-59, but no further. For each of the four cohorts, the variance for the initial year, t, and the auto-covariances between year t and each subsequent year are calculated. A fresh set of cohorts is begun in each year, so in year $t+1$ individuals are again divided into the four age groups 20-29, 30-39, etc and the variance for $t+1$ and the auto-covariances between year $t+1$ and each subsequent year are calculated. The end result of this procedure is 1004 unique elements in each county's covariance matrix. This way of constructing the covariance matrix ensures that every individual's element is included uniquely into one cell of the matrix, at the same time allowing us to maximize the number of elements since we can use a fully unbalanced panel with constant age intervals in each year.

Each county's resulting auto-covariance matrix is used in the minimum distance estimation of equations (6)-(8). Basically, the implied variances and auto-covariances of the model are fitted to the corresponding empirical

---

[13] The results would be confounded because we estimate a model of income, not of entry and exit; appending a model of entry and exit is beyond the scope of this paper.

[14] The county of Gotland is excluded due to the very small sample size and because most of the crimes are commited by tourists.

[15] The individuals in the age group 50-59 can not be followed at all. However their incomes are used to construct diagonal elements of the covaraince matrix, ie to calculate the variances.

moments in the data by non-linear least squares. In these estimations the age of the cohort aged 20-29 is defined to be 25, the age of the cohort aged 30-39 is 35, and so forth. Appendix A contains a description of the estimation procedure.

3.3 Results

Here we will briefly describe the estimation results for three selected counties. Table B1 in Appendix B contains the parameter estimates and associated standard errors for the county of Stockholm $(n = 31,585)$ together with results for the most northern and southern counties - Norrbotten $(n = 4,841)$ and Skåne $(n = 17,724)$ respectively.[16] Based on the parameter estimates we can obtain age-specific estimates of permanent and transitory inequality.

Figure 1a-c contains the predicted evolution of permanent and transitory inequality from 1974 to 2000 for individuals defined to be 45 years old (ie in the age interval 40-49) together with the predicted variance from the full model (the variance of permanent income plus the variance of transitory income) and the actual variance for 45-year olds. The permanent and transitory variance both make up around half of the total variance in all three counties. One would perhaps expect the transitory variance to be smaller, but the use of an unbalanced panel design tends to boost the transitory variance; see Gustavsson (2004b) for a comparison of results from unbalanced and balanced panel designs.[17] Unlike previous studies we also include immigrants, who are likely to have more transitory income due to higher unemployment rates.

[16] For corresponding estimates for the whole of Sweden during this period, see Gustavsson (2004a, b).

[17] The higher transitory variance will not affect our final crime estimates as long as the changes over time are unaffected. Note also that there is no a priori reason to prefer a balanced panel since this design induces potentially severe sample selection effects as only individuals with positive earning during all sample years are included.

Figure 1a: Permanent and transtiory inequality in the county of Stockholm, 1974-2000

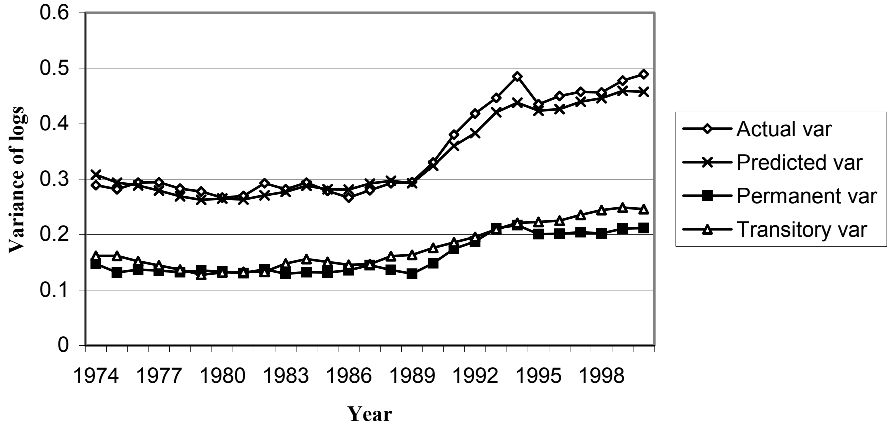


Figure 1b: Permanent and transtiory inequality in the county of Norrbotten, 1974-2000

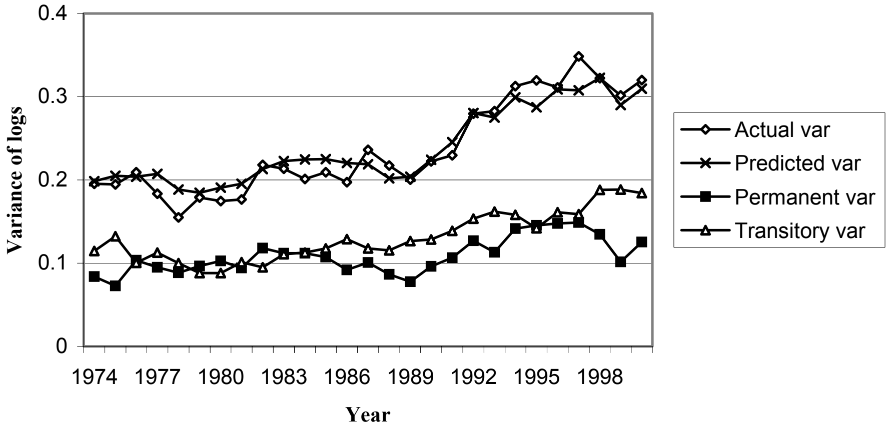
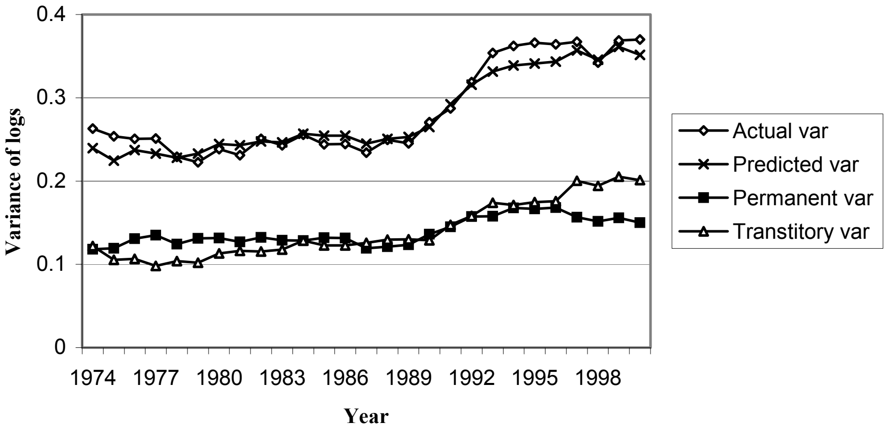


Figure 1c: Permanent and transtiory inequality in the county of Skåne, 1974-2000



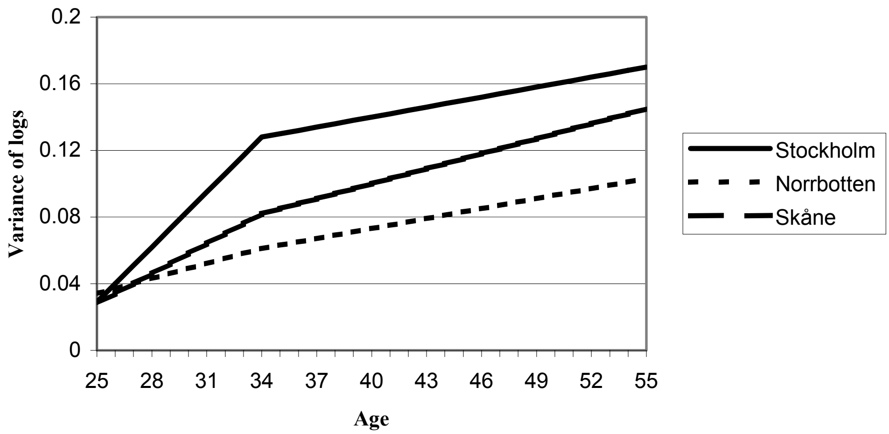
Cross-sectional inequality among 45-year olds is largest in Stockholm and smallest in Norrbotten. The estimates of the permanent and transitory component are more irregular for Norrbotten, likely reflecting the smaller sample for this county (due to fewer people living there). There are year-to-year differences in the evolution of permanent and transitory variances across the counties up until the mid 1990s. The major movements are similar though, with only small changes up to the late 1980s and large increases during the deep Swedish recession in the first half of the 1990s. There are, however, larger differences from the mid 1990s as permanent inequality levels of in Stockholm but decreases in Skåne and Norrbotten. Transitory inequality also displays a stronger increase in Skåne and Norrbotten than in Stockholm during this period. Overall, there are some important cross-county differences in the evolution of inequality – especially during the 1990s.

As our econometric model of income dynamics results in age-specific estimates of permanent and transitory inequality, it is natural to ask which age we should use when we investigate the connection to crime rates.[18] A first suggestion might be to use the estimates for the youngest age, ie for those in

[18] Since, by construction, the proportional changes over time within a county are the same for all ages, we cannot include several different ages since these would be perfectly collinear.

the age interval 20-29 (defined to be 25 years old in the estimations), since young individuals commit most crimes. However, our estimates show that the magnitudes and life-cycle patterns of permanent and transitory inequality differ across counties (different estimates of the random walk variances and of the q_a parameters). To illustrate the meaning of this, Figure 2 graphs the life-cycle pattern of permanent inequality holding time constant, for the county of Stockholm, Norrbotten, and Skåne. The figure shows that inequality among 25-year olds actually is largest in Norrbotten, but also that this is a quickly passing state as the age-inequality pattern is much steeper in Stockholm and Skåne. Hence, even if inequality is low in Stockholm among individuals aged 25, those with low expected permanent income over the life-cycle will have much lower expected *relative* income in the future than the corresponding 25-year old individuals in Norrbotten. Since, according to economic theories along the lines of Becker (1968), expected future relative income is closely connected to the decision to commit crime, it is important to take account of the life-cycle differences across counties when estimating the connection between permanent inequality and crime rates.

Figure 2: Permanent inequality over the life-cycle in the county of Stockholm, Norrbotten, and Skåne



To get measures of inequality that take account of the cross-county differences in life-cycle patterns, we compute the average permanent and transitory inequality over the life cycle for each county. That is, for a given year and county we calculate the permanent variance for each age (25, 26, and so forth up to 55), and then compute the average of these variances. The corresponding is done for transitory inequality. These averages of permanent and transitory inequality are the measures that we use in our estimated crime equations.

4 Data and econometric specification of crime equation

When estimating the crime equations, we will use panel data from Swedish counties. In the panel, we observe the 21 counties over the time period 1974-2000. We will investigate the effects of income inequality on four different types of crime: total crime, shoplifting, auto theft, and burglary. Time-series graphs of these crime categories are available in Appendix C.[19]

We assume that there is an underlying process that connects per capita crime rates, C_{it}, and the two measures of income inequality, $PERM_{it}$ and $TRANS_{it}$, in county $i = 1, \ldots, N$ at time $t = 1, \ldots, T$ such that

$$C_{it} = e^{\alpha PERM_{it} + \beta TRANS_{it}}$$

where $PERM_{it}$ is the estimated inequality in permanent income and $TRANS_{it}$ is the estimated inequality in transitory income and where we are interested in estimating the parameters α and β. Since crime rates are non-negative, the exponential form is suitable. Furthermore, for the exponential form, any changes are proportional to the crime rate, which seems more plausible than for example constant changes produced by a linear relation.

In order to avoid misspecifications due to omitted variables, we control for observable as well as unobservable variables that might explain the crime rate and that might be correlated with the two inequality measures. The observable variables, x_{it}, that we use are crime-specific clear-up rates (proxy for the

[19] The crime data is defined as the reported crime per 100,000 inhabitants and year, and it is collected from The Swedish National Council for Crime Prevention (BRÅ). With total crime we mean *all* reported crimes (not just the total of the crime categories that we use in this paper).

probability of getting caught), unemployment rate, share of men in the age interval 15-24, share of foreign citizens, and share of the population that is divorced.[20] The unobservable variables are county-specific fixed effects, f_i, to control for unobserved variables that affects the crime rate and that stay constant over time for each county, time-specific fixed effects, λ_t, to control for unobserved macro-economic shocks that affect the crime rate in each county in the same way in a given year, and county-specific time trends, $trend_i$. The fixed county- and time-effects might be correlated with the observable variables. Furthermore, the crime rate can also be affected by disturbances, ε_{it}. Thus, we have the following relationship to be estimated for the crime rate

$$(9) \qquad C_{it} = e^{\alpha PERM_{it} + \beta TRANS_{it} + \delta' x_{it} + f_i + \lambda_t + trend_i + \varepsilon_{it}}$$

To estimate equation (9), we take the logarithm of it and use OLS to estimate the following familiar log-linear fixed effect model[21]:

$$(10) \qquad \ln(C_{it}) = \alpha PERM_{it} + \beta TRANS_{it} + \delta' x_{it} + f_i + \lambda_t + trend_i + \varepsilon_{it}$$

There are three things that we have to deal with when estimating equation (10). First, it is quite likely that the disturbances are heteroscedastic. Crimes are discrete events and the number of crimes committed is an integer. While this is not a problem for larger populations, for smaller populations it is, since the discrete nature of the crimes then will transfer to the crime rate, which is our dependent variable. For a population of 5,000, one additional crime corresponds to 20 crimes per 100,000 inhabitants. Since the precision of crime rate estimates as a consequence will depend on the population size, we cannot expect the variance of the regression errors to be homoscedastic, if we estimate equation (10) with common methods. The smaller the population is, the larger is the variance.[22] We take care of this problem by estimating robust standard errors. Second, if there is a serial correlation in the error process (that is, if the crime rates are serially correlated), the resulting standard errors are inconsis-

[20] These are control variables that are typically used when estimating crime equations; see, eg, Raphael & Winter-Ebmer (2001), Levitt & Donohue (2001) and Edmark (2005).
[21] The log-linear fixed effect model has been frequently used in papers estimating crime equations; see, eg, Raphael & Winter-Ebmer (2001) and the references cited therein.
[22] See Osgood (2000) for a discussion in a cross-sectional setting.

tently estimated and may lead to severely biased estimates in small samples (see, eg Kezdi, 2002, and Bertrand et al., 2004). Therefore, we allow the errors to be correlated over time within each county.[23] Third, since $PERM_{it}$ and $TRANS_{it}$ are estimated variables, their estimated standard errors might be biased. To correct for this, we present bootstrapped confidence intervals for these two variables.[24]

5 Results

For comparison reasons we start by using a traditional, aggregate, measure of income when calculating income inequality. Three measures are used: the predicted variance (which is the sum of the estimated variances in permanent and transitory income; cf equation (2)), the Gini coefficient, and the variance. The results of including these measures in equation (10) are presented in Table 1.

If it is the case that permanent income has an effect on crime while transitory income does not, then we might end up with the false result of insignificant effects when using an aggregate income measure. This is also what we mainly get; none of the measures we use for income inequality enters significantly for the categories total crimes, auto theft and burglary.[25] For shoplifting, however, all three measures enter significantly at the five percent significance level, indicating that the larger the income inequality is, the more shoplifting we observe.

Next we turn to the, for the purpose of this paper, more interesting question of whether the results in Table 1 change when we separate aggregate income into a permanent and a transitory part. The results are presented in Table 2. For each crime category, we present two different sets of estimates; in column (1) we present the results when we don't control for the crime-specific clear-up rate and in column (2) we present the results when we control for the clear-up rate.[26] The reason for this division is the potential problems the endogeneity of the clear-up rate might cause in interpreting the coefficients for the permanent

[23] Technically, this was done in STATA by clustering on county.
[24] We use 1000 bootstrap replications to estimate the confidence intervals.
[25] These results are in line with Nilsson (2004).
[26] In Appendix D we also present the corresponding when weighted least squares is used.

and transitory income variables.[27] In parenthesis, we present the traditionally estimated standard errors. However, for the two income variables these estimates are biased since the variables are estimated. Therefore, we rely on bootstrap confidence interval when making inference for the income variables.[28] 98 percent bootstrap confidence intervals are presented within brackets.

From the results in Table 2, it is clear that it is important to decompose income; while the inequality in permanent income enters statistically significant in all estimations (the four 98 percent bootstrap confidence intervals never cover the zero; as a matter of fact, *all* 1000 bootstrap estimates are positive for all crime categories), the inequality in transitory income never enters significantly. It hence seems like it is inequality in permanent income – and not in transitory income - that is important in determining property crime (it can be noted that, compared to the results in Table 1, the result for shoplifting are strengthened when permanent income is used instead of aggregate income).

The results for the other variables in Table 1 and 2 are in line with those in previous studies - with the noticeable exception of the unemployment rate which enters insignificantly in all specifications but one. This result for the unemployment rate is perhaps not surprising as compared to the bulk of the earlier studies aiming at estimating the effect of unemployment on crime (for a discussion on this issue, see Raphael and Winter-Ebmer, 2001). However, it is rather surprising given the results in more recent work, such as Raphael and Winter-Ebmer (2001), Nilsson and Agell (2003), and Edmark (2005). It is especially surprising given that we are using the same type of data as in Edmark (county-level data for Sweden). One explanation for the different

[27] As can be seen from Table 2, the estimates for the two income variables are very similar in columns (1) and (2), indicating that the clear-up rate does not cause any problems in interpreting the coefficients for the income variables. Due to space constraints in Table 1, we did not report the results when the clear-up rates where included in those regressions. However, when the clear-up rates where included, we obtained very similar results for the income variables as those reported in Table 1.

[28] In each bootstrap iteration, we first draw individuals with replacement from the income data base (ie, from the LINDA data base). Then we use the procedure presented in section 3 to estimate the county-specific inequalities in permanent and transitory income. Finally, we use the estimated county-specific inequalities in permanent and transitory income and estimate the crime equations. This procedure is repeated 1000 times, providing us with 1000 estimates on the coefficients for the two income variables.

results could be the disaggregation of the income variable into its two components, as done in this paper. However, from the results in Table 1 where total income is used, this does not seem to be the explanation. The main differences compared to the study by Edmark, besides the income variable, are that we have a longer time period (although covering the time period used by her) and a different functional form (she is using a log-log specification). This might indicate that the results for the unemployment variable can be sensitive to the choice of time period or choice of functional form.

Table 1. Estimates when using inequality in aggregate measure of income.

	Total crime			Shoplifting			Auto theft			Burglary		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Predict. Var.	0.023 (0.200)			1.191 (0.443)**			0.783 (0.576)			-0.241 (0.467)		
Gini		-0.111 (0.618)			5.132 (2.321)**			0.759 (2.640)			-1.460 (1.271)	
Variance			-0.002 (0.194)			1.382 (0.624)**			0.547 (0.611)			-0.321 (0.485)
Unemployed	1.514 (0.986)	1.540 (0.974)	1.523 (0.969)	-1.054 (2.114)	-1.404 (2.145)	-1.108 (2.158)	2.850 (2.134)	3.008 (2.269)	2.942 (2.155)	0.458 (1.239)	0.604 (1.255)	0.482 (1.244)
Men 15-24	5.251 (3.589)	5.331 (3.595)	5.289 (3.614)	-11.442 (14.305)	-11.677 (13.452)	-11.591 (14.019)	12.224 (9.847)	13.073 (9.776)	12.599 (9.924)	2.285 (5.844)	2.531 (5.675)	2.389 (5.848)
Foreign	4.845 (2.188)**	4.741 (2.146)**	4.813 (2.222)**	1.144 (6.504)	3.426 (5.401)	1.883 (6.001)	4.398 (6.747)	3.964 (7.388)	4.226 (6.919)	2.771 (3.859)	2.073 (3.873)	2.608 (3.783)
Divorced	11.642 (6.640)*	11.476 (6.527)*	11.545 (6.545)*	-1.589 (17.229)	-1.453 (17.561)	-2.770 (16.627)	48.172 (17.016)**	45.599 (16.190)**	46.264 (16.653)**	27.934 (9.504)***	27.912 (9.045)***	28.197 (9.142)***
Observations	540	540	540	520	520	520	540	540	540	540	540	540
R-squared	0.94	0.94	0.94	0.97	0.97	0.97	0.92	0.92	0.92	0.91	0.91	0.91

Notes. Robust standard errors are presented in parentheses. Time dummies, county-specific fixed effects and county-specific time trends are included in all specifications. Clustering is made on counties (allowing for autocorrelation in the residuals).
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2. Estimates when using inequality in permanent and transitory income.

	Total crime		**Shoplifting**		**Auto theft**		**Burglary**	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Permanent	1.247	1.409	2.789	2.562	1.839	1.838	0.534	0.715
	(0.444)**	(0.467)***	(0.726)***	(0.702)***	(0.943)*	(0.917)*	(0.842)	(0.842)
[98% bootstrap CI]	[1.15 1.50]	[1.31 1.68]	[2.09 3.12]	[1.93 2.97]	[1.30 2.14]	[1.30 2.18]	[0.38 0.96]	[0.57 1.15]
Transitory	-0.303	-0.338	0.811	0.463	0.501	0.534	-0.448	-0.445
	(0.261)	(0.259)	(0.543)	(0.521)	(0.618)	(0.608)	(0.510)	(0.520)
[98% bootstrap CI]	[-0.47 0.16]	[-0.48 0.14]	[-0.41 1.25]	[-0.58 1.06]	[-0.46 1.02]	[-0.40 1.01]	[-0.64 -.31]	[-0.64 0.30]
Clear-up rate		0.033		2.411		-0.619		0.012
		(0.239)		(0.626)***		(0.286)**		(0.078)
Unemployed	1.249	1.352	-1.368	0.017	2.620	2.588	0.290	0.170
	(0.881)	(0.759)*	(2.091)	(2.105)	(2.138)	(2.188)	(1.311)	(1.327)
Men 15-24	6.307	6.477	-10.224	-6.553	13.137	10.418	2.955	2.982
	(3.065)*	(3.092)**	(13.690)	(11.560)	(9.847)	(9.770)	(5.690)	(5.908)
Foreign	6.514	5.936	3.239	3.176	5.840	3.714	3.829	2.793
	(2.143)***	(2.312)**	(6.402)	(5.358)	(6.692)	(6.633)	(4.401)	(4.847)
Divorced	10.206	5.352	-4.537	2.532	46.932	49.526	27.025	25.673
	(6.275)	(6.129)	(17.475)	(17.435)	(17.055)**	(17.897)**	(9.398)***	(9.655)**
Observations	540	520	520	520	540	520	540	520
R-squared	0.95	0.95	0.97	0.97	0.92	0.92	0.91	0.91

Notes. See notes to Table 1.

6 Conclusions

Earlier studies on income inequality and crime have used inequality in total income or total earnings as explanatory variable. However, from sociological theories on inequality and crime, it is rather an individual's permanent position in society that is the main factor affecting one's decision to commit crime or not, not the individual's transitory deviation from the permanent position, implying that it is the inequality in permanent income, and not in transitory income, that is the important determinant for crime. Although less clear, this can also be the prediction from economic theories. Using an aggregate measure of income, as the earlier studies have done, will however restrict inequality in permanent and transitory income to have the same impact, making it difficult to obtain systematic evidence if only one of the components matter (or if the two components matter to different degrees).

We have in this paper investigated whether the effects from inequality in permanent income on crime differ from the effects from inequality in transitory income on crime. To that end, we started out by estimating, using very rich income data from Sweden, region-specific inequality in permanent and transitory income. Then we used the two estimated income components as explanatory variables in four different crime equations; total crime, shoplifting, auto theft, and burglary.

The results indicate that it is crucially important to separate the two effects; while an increase in the inequality in permanent income yields a positive and significant effect on total crimes and the three different property crimes, an increase in the inequality in transitory income has no significant effect on any type of crime. Using a traditional, aggregate, measure of income yields mainly insignificant effects on crime.

References

Abowd, J & D Card (1989) "On the Covariance Structure of Earnings and Hours Changes" *Econometrica*, Vol. 57, 411-445.

Baker, M (1997) "Growth Rate Heterogeneity and the Covariance Structure of Life-Cycle Earnings" *Journal of Labor Economics*, Vol. 15, 537-579.

Baker, M & G Solon (2003) "Earnings Dynamics and Inequality among Canadian Men, 1976-1992: Evidence from Longitudinal Tax Records" *Journal of Labor Economics*, Vol. 21, 289-321.

Becker, G S (1968) "Crime and Punishment: An Economic Approach" *The Journal of Political Economy*, Vol. 66, 169-217.

Bertrand, M, E Duflo & S Mullainathan (2004) "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, Vol. 119, 249-275.

Blau, J R & P M Blau (1982) "The Cost of Inequality: Metropolitan Structure and Violent Crime" *American Sociological Review*, Vol. 47, 114-129.

Bourguignon, F (2001) "Crime as a Social Cost of Poverty and Inequality: A Review Focusing on Developing Countries" In Yusuf, S, S Evenett, & W Wu (eds) *Facets of Globalization*.

Cappellari, L (2004) "The Dynamics and Inequality of Italian Men's Earnings: Long-term Changes or Transitory Fluctuations?" *Journal of Human Resources*, Vol. 39, 475-499.

Chamberlain, G (1984) "Panel Data" in Z Griliches and M Intriligator (eds), *Handbook of Econometrics*, Vol. 2, Elsevier Science B.V.

Chisholm, J & C Choe (2005) "Income Variables and the Measures of Gains from Crime" *Oxford Economic Papers*, Vol. 57, 112-119

Chiu, W H & P Madden (1998) “Burglary and Income Inequality” *Journal of Public Economics*, Vol. 69, 123-141.

Demombynes, G & B Özler (2005) “Crime and Local Inequality in South Africa” Forthcoming in *Journal of Development Economics*.

Dickens, R (2000) “The Evolution of Individual Male Earnings in Great Britain: 1975-95” *Economic Journal*, Vol. 110, 27-49.

Donohue, J & S Levitt (2001) “The Impact of Legalized Abortion on Crime” *Quarterly Journal of Economics*, Vol. 116, 379-420.

Edin, P-A & P Fredriksson (2000) “LINDA – Longitudinal Individual Data for Sweden” Working Paper 2000:19, Department of Economics, Uppsala University.

Edmark, K (2005) “The Effects of Unemployment on Property Crime: Evidence from a Period with Unusually Large Swings in the Business Cycle” forthcoming in *Scandinavian Journal of Economics*.

Ehrlich, I (1973) “Participation in Illegitimate Activities: A Theoretical and Empirical Investigation” *The Journal of Political Economy*, Vol. 81, 521-565.

Freeman, R (1999) “The Economics of Crime” in Ashenfelter, O C & D Card (eds), *Handbook of Labor Economics*, Vol. 3C, Elsevier, Amsterdam.

Friedman, M & S Kuznets (1954), *Income from Independent Professional Practise*, NBER.

Gustavsson, M (2004a) “Changes in Permanent and Transitory Earnings Inequality in Sweden: 1991-1999” in M Gustavsson, *Empirical Essays on Earnings Inequality*, Ph.D. dissertation, Uppsala University.

Gustavsson, M (2004b) “Trends in the Transitory Variance of Earnings: Evidence from Sweden 1960-1990 and a comparison with the United

States" in M Gustavsson, *Empirical Essays on Earnings Inequality*, Ph.D. dissertation, Uppsala University.

Haider, J (2001) "Earnings Instability and Earnings Inequality of Males in the United States: 1967-1991" *Journal of Labor Economics*, Vol. 19, 799-836.

Kezdi, G (2002) "Robust Standard Error Estimation in Fixed-Effects Panel Models" Mimeo, University of Michigan.

Leamer, E (1983) "Let's Take the Con Out of Econometrics" *American Economic Review*, Vol. 75, 31-43.

Merton, R K (1938) "Social Structure and Anomie" *American Sociological Review,* Vol. 3, 672-682.

Moffitt, R & P Gottschalk (1995) "Trends in the Covariance Structure of Earnings in the U.S.: 1969-1987" mimeo, Johns Hopkins University.

Moffitt, R & P Gottschalk (2002) "Trends in the Transitory Variance of Earnings in the United States" *Economic Journal*, Vol. 112, 68-73.

Newey, W (1985) "Generalized Method of Moments Specification Testing" *Journal of Econometrics*, Vol. 29, 229-256.

Nilsson, A (2004) "Income Inequality and Crime: The Case of Sweden" IFAU Working Paper 2004:6.

Nilsson, A & J Agell (2003) "Crime, Unemployment and Labor Market Programs in Turbulent Times" IFAU Working Paper 2003:14.

Raphael, S & R Winter-Ebmer (2001) "Identifying the Effect of Unemployment on Crime" *Journal of Law and Economics*, Vol. 44, 259-283.

Wilson, M & M Daly (1997) “Life Expectancy, Economic Inequality, Homicide and Reproductive Timing in Chicago Neighborhoods” *British Medical Journal*, Vol. 314, 1271-1274

Appendix A: The minimum distance estimation

The parameters of the model in equations (6)-(8) are estimated by applying the minimum distance estimator of Chamberlain (1984). Specifically, let $\mathbf{C}_b$ contain the distinct elements of the population auto-covariance matrix of y_{ibt} for cohort b and let $\mathbf{C}$ be an aggregate vector stacked with the $\mathbf{C}_b$ vectors. Let the vector $\boldsymbol{\theta}$ contain all the parameters of our model and let $\mathbf{C} = f(\boldsymbol{\theta})$ express the model's moment restrictions. Our model then implies that the general variance element in $\mathbf{C}$ is

$$\text{(A1)}\quad Var(y_{iat}) = p_t^2(\sigma_u^2 + \sum\nolimits_a \sigma_{ra}^2) + q_a^2[\rho_t^2 Var(\varepsilon_{i,t-1}) + (2\rho\delta + \delta^2)\sigma_{v_{t-1}}^2 + \sigma_{v_t}^2],$$

and that the general auto-covariance element for years t and $t-s$ is

$$\text{(A2)}\quad Cov(y_{iat}, y_{i,a-s,t-s}) = p_t p_{t-s}(\sigma_u^2 + \sum\nolimits_{a-s} \sigma_{ra}^2) + q_a q_{a-s}[\rho^s Var(\varepsilon_{i,t-s}) + \delta\rho^{s-1}\sigma_{v_{t-s}}^2].$$

The vector $\mathbf{C}$ is estimated by the sample counterpart $\hat{\mathbf{C}}$, and $\hat{\boldsymbol{\theta}}$ is chosen to minimize a distance function

$$\text{(A3)}\qquad \mathbf{D} = (\hat{\mathbf{C}} - f(\hat{\boldsymbol{\theta}}))'\mathbf{W}(\hat{\mathbf{C}} - f(\hat{\boldsymbol{\theta}})),$$

where $\mathbf{W}$ is a positive definite weighting matrix.

The asymptotically optimal choice of $\mathbf{W}$ is the inverse of a matrix that consistently estimates the covariance matrix of $\mathbf{C}$. However, Altonji and Segall (1996) and Clark (1996) provide Monte Carlo evidence of potentially serious finite sample bias in the estimate of $\boldsymbol{\theta}$ using this approach.[29] We therefore follow the practice of the most recent literature and use the identity matrix as the weighting matrix. This "equally weighted minimum distance estimation" amounts to using non-linear least squares to fit $f(\hat{\boldsymbol{\theta}})$ to $\hat{\mathbf{C}}$.

[29] This bias arises because of correlated sampling errors in the second and fourth moments of income.

As outlined in Chamberlain (1984), auto-correlation and heteroskedasticity robust standard errors for $\hat{\boldsymbol{\theta}}$ are obtained from the formula

(A4) $$(\mathbf{G}'\mathbf{G})^{-1}\mathbf{G}'\mathbf{V}\mathbf{G}(\mathbf{G}'\mathbf{G})^{-1},$$

where $\mathbf{G}$ is the gradient matrix $\partial f(\boldsymbol{\theta})/\partial\boldsymbol{\theta}$ evaluated at $\hat{\boldsymbol{\theta}}$ and $\mathbf{V}$ is a block diagonal matrix where the diagonal contains the estimated covariance matrices of each $\hat{\mathbf{C}}_b$ vectors.

Appendix B: Estimates of income dynamics

Table B1. Estimates of income dynamics for three Swedish counties.

	Stockholm		Norrbotten		Skåne	
Permanent component						
σ_u^2	0.029	(0.003)	0.034	(0.007)	0.028	(0.003)
$\sigma_{r,26-34}^2$	0.011	(0.001)	0.003	(0.001)	0.006	(0.001)
$\sigma_{r,35-55}^2$	0.002	(0.0002)	0.002	(0.0004)	0.003	(0.0004)
p_{74}	1.000		1.000		1.000	
p_{75}	0.947	(0.045)	0.929	(0.123)	1.005	(0.064)
p_{76}	0.964	(0.045)	1.109	(0.137)	1.053	(0.065)
p_{77}	0.958	(0.043)	1.063	(0.131)	1.070	(0.065)
p_{78}	0.948	(0.042)	1.026	(0.122)	1.026	(0.062)
p_{79}	0.958	(0.041)	1.071	(0.123)	1.054	(0.062)
p_{80}	0.953	(0.041)	1.105	(0.125)	1.056	(0.062)
p_{81}	0.941	(0.039)	1.060	(0.118)	1.037	(0.060)
p_{82}	0.967	(0.040)	1.185	(0.130)	1.059	(0.061)
p_{83}	0.938	(0.039)	1.154	(0.125)	1.046	(0.060)
p_{84}	0.948	(0.039)	1.154	(0.122)	1.043	(0.059)
p_{85}	0.945	(0.038)	1.131	(0.119)	1.058	(0.059)
p_{86}	0.960	(0.039)	1.045	(0.112)	1.057	(0.059)
p_{87}	0.993	(0.040)	1.096	(0.116)	1.005	(0.056)
p_{88}	0.963	(0.038)	1.016	(0.107)	1.014	(0.056)
p_{89}	0.937	(0.038)	0.961	(0.102)	1.023	(0.057)
p_{90}	1.004	(0.040)	1.069	(0.112)	1.075	(0.059)
p_{91}	1.088	(0.043)	1.126	(0.117)	1.109	(0.061)
p_{92}	1.129	(0.044)	1.229	(0.126)	1.156	(0.063)
p_{93}	1.199	(0.046)	1.161	(0.119)	1.156	(0.063)
p_{94}	1.215	(0.047)	1.298	(0.130)	1.193	(0.064)
p_{95}	1.169	(0.044)	1.315	(0.130)	1.188	(0.063)
p_{96}	1.171	(0.043)	1.326	(0.131)	1.193	(0.063)
p_{97}	1.179	(0.043)	1.330	(0.130)	1.152	(0.061)
p_{98}	1.171	(0.043)	1.266	(0.124)	1.134	(0.060)

p_{99}	1.196	(0.043)	1.099	(0.107)	1.149	(0.060)
p_{00}	1.200	(0.043)	1.222	(0.118)	1.128	(0.059)
Transitory component						
q_{25-34}	1.000		1.000		1.000	
q_{35-44}	0.723	(0.006)	0.689	(0.014)	0.690	(0.007)
q_{45-55}	0.684	(0.006)	0.621	(0.013)	0.626	(0.007)
ρ	0.822	(0.004)	0.759	(0.011)	0.795	(0.005)
δ	-0.406	(0.007)	-0.368	(0.017)	-0.395	(0.009)
Year-specific innovation variances $\sigma^2_{v_t}$						
1974	0.345	(0.012)	0.297	(0.025)	0.311	(0.016)
1975	0.286	(0.012)	0.298	(0.028)	0.219	(0.013)
1976	0.235	(0.011)	0.189	(0.025)	0.206	(0.014)
1977	0.207	(0.011)	0.222	(0.026)	0.175	(0.013)
1978	0.189	(0.010)	0.185	(0.023)	0.190	(0.013)
1979	0.169	(0.010)	0.157	(0.021)	0.182	(0.013)
1980	0.183	(0.010)	0.164	(0.021)	0.209	(0.013)
1981	0.186	(0.010)	0.199	(0.022)	0.213	(0.014)
1982	0.186	(0.010)	0.180	(0.023)	0.208	(0.013)
1983	0.217	(0.010)	0.221	(0.024)	0.212	(0.014)
1984	0.230	(0.011)	0.221	(0.024)	0.238	(0.014)
1985	0.212	(0.011)	0.230	(0.024)	0.218	(0.014)
1986	0.200	(0.011)	0.256	(0.027)	0.218	(0.015)
1987	0.204	(0.011)	0.221	(0.023)	0.226	(0.014)
1988	0.235	(0.011)	0.217	(0.025)	0.234	(0.014)
1989	0.235	(0.011)	0.247	(0.027)	0.233	(0.014)
1990	0.259	(0.012)	0.249	(0.028)	0.229	(0.015)
1991	0.274	(0.013)	0.274	(0.030)	0.276	(0.016)
1992	0.287	(0.014)	0.306	(0.033)	0.296	(0.017)
1993	0.309	(0.015)	0.320	(0.033)	0.329	(0.018)
1994	0.325	(0.016)	0.303	(0.035)	0.311	(0.019)
1995	0.321	(0.015)	0.261	(0.033)	0.317	(0.018)
1996	0.321	(0.015)	0.316	(0.036)	0.316	(0.019)

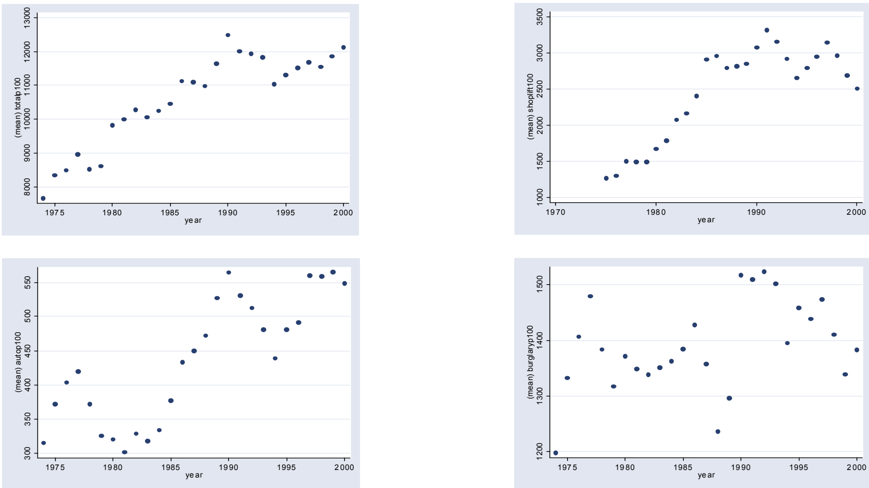
1997	0.340	(0.015)	0.305	(0.037)	0.377	(0.020)
1998	0.353	(0.015)	0.379	(0.041)	0.351	(0.020)
1999	0.357	(0.015)	0.369	(0.039)	0.376	(0.019)
2000	0.346	(0.014)	0.352	(0.035)	0.360	(0.018)

Note: The estimated model is outlined in equations (6)-(8). The estimates for each county are based on 1004 variances and auto-covariances. Heteroskedasticity and auto-correlation robust standard errors are in parentheses.

Appendix C: Time graphs of crime

In this appendix we present time series graphs of total crime, shoplifting, auto theft, and burglary, for the period 1974-2000.

Figure C1: Average reported crime rates (per 100' inhabitants) for the four crime categories, 1974-2000



Appendix D: Weighted least squares

In this appendix we present the results for permanent and transitory income when using weighted least squares.

Table D1. Estimates when using inequality in permanent and transitory income. Weighted least squares.

	Total crime		**Shoplifting**		**Auto theft**		**Burglary**	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Perman.	0.814	0.947	1.815	1.624	0.814	0.947	-0.149	0.080
	(0.427)*	(0.438)**	(1.029)*	(0.997)	(0.427)*	(0.438)**	(0.762)	(0.732)
Transitory	-0.347	-0.414	0.856	0.535	-0.347	-0.414	-0.609	-0.712
	(0.252)	(0.259)	(0.638)	(0.668)	(0.252)	(0.259)	(0.480)	(0.496)
Clear-up		-0.054		1.806		-0.054		-0.033
		(0.231)		(0.495)***		(0.231)		(0.098)
Unempl.	1.485	1.442	-1.564	0.277	1.485	1.442	2.265	2.057
	(1.009)	(0.802)*	(2.290)	(2.016)	(1.009)	(0.802)*	(1.818)	(1.789)
Men15-24	4.140	3.818	-8.093	-7.823	4.140	3.818	-0.124	-1.091
	(3.391)	(3.537)	(13.698)	(11.778)	(3.391)	(3.537)	(7.408)	(7.607)
Foreign	3.036	2.211	5.322	4.796	3.036	2.211	-2.630	-3.944
	(2.430)	(2.624)	(5.335)	(5.264)	(2.430)	(2.624)	(5.002)	(5.029)
Divorced	16.712	12.315	-8.461	-1.835	16.712	12.315	33.906	31.141
	(6.313)**	(5.955)*	(12.322)	(13.096)	(6.313)**	(5.955)*	(9.361)***	(9.007)***
Obs.	540	520	520	520	540	520	540	520
R-squared	0.97	0.97	0.99	0.99	0.97	0.97	0.94	0.94

Notes. Robust standard errors are presented in parentheses. Time dummies, county-specific fixed effects and county-specific time trends are included in all specifications. Clusatering is made on counties (allowing for autocorrelation in the residuals).
* significant at 10%; ** significant at 5%; *** significant at 1%

Publication series published by the Institute for Labour Market Policy Evaluation (IFAU) – latest issues

Rapporter/Reports

2005:1 Ahlin Åsa & Eva Mörk ”Vad hände med resurserna när den svenska skolan decentraliserades?”

2005:2 Söderström Martin & Roope Uusitalo ”Vad innebar införandet av fritt skolval i Stockholm för segregeringen i skolan?”

2005:3 Fredriksson Peter & Olof Åslund ”Påverkas socialbidragsberoende av omgivningen?”

2005:4 Ulander-Wänman Carin ”Varslad, uppsagd, återanställd. Företrädesrätt till återanställning enligt 25 § LAS i praktisk tillämpning”

2005:5 Isacsson Gunnar ”Finns det en skillnad mellan samhällets och individens avkastning på utbildning?”

2005:6 Andersson Christian & Iida Häkkinen “En utvärdering av personalförstärkningar i grundskolan”

2005:7 Hesselius Patrik, Per Johansson & Laura Larsson ”Hur påverkar kravet på läkarintyg sjukfrånvaron? Erfarenheter från ett socialt experiment”

2005:8 van den Berg J & Bas van der Klaauw “Job search monitoring and sanctions – a brief survey of some recent results”

2005:9 Sibbmark Kristina & Anders Forslund ”Kommunala arbetsmarknadsinsatser riktade till ungdomar mellan 18 och 24 år”

2005:10 Lindqvist Linus, Laura Larsson & Oskar Nordström Skans ”Friårets arbetsmarknadseffekter”

Working Papers

2005:1 Ericson Thomas “Personnel training: a theoretical and empirical review”

2005:2 Lundin Martin “Does cooperation improve implementation? Central-local government relations in active labour market policy in Sweden”

2005:3 Carneiro Pedro, James J Heckman & Dimitriy V Masterov “Labor market discrimination and racial differences in premarket factors”

2005:4 de Luna Xavier & Ingeborg Waernbaum “Covariate selection for non-parametric estimation of treatment effects”

2005:5 Ahlin Åsa & Eva Mörk “Effects of decentralization on school resources”

2005:6 Cunha Flavio, James J Heckman & Salvador Navarro “Separating uncertainty from heterogeneity in life cycle earnings”

2005:7 Söderström Martin & Roope Uusitalo "School choice and segregation: evidence from an admission reform"

2005:8 Åslund Olof & Peter Fredriksson "Ethnic enclaves and welfare cultures – quasiexperimental evidence"

2005:9 van der Klaauw Bas, Aico van Vuuren & Peter Berkhout "Labor market prospects, search intensity and the transition from college to work"

2005:10 Isacsson Gunnar "External effects of education on earnings: Swedish evidence using matched employee-establishment data"

2005:11 Abbring Jaap H & Gerard J van den Berg "Social experiments and instrumental variables with duration outcomes"

2005:12 Åslund Olof & Oskar Nordström Skans "Measuring conditional segregation: methods and empirical examples"

2005:13 Fredriksson Peter & Bertil Holmlund "Optimal unemployment insurance design: time limits, monitoring, or workfare?"

2005:14 Johansson Per & Per Skedinger "Are objective measures of disability reliable?"

2005:15 Hesselius Patrik, Per Johansson & Laura Larsson "Monitoring sickness insurance claimants: evidence from a social experiment"

2005:16 Zetterberg Johnny "Swedish evidence on the impact of cognitive and non-cognitive ability on earnings – an extended pre-market factor approach"

2005:17 Nordström Skans Oskar & Linus Lindqvist "Causal effects of subsidized career breaks"

2005:18 Larsson Laura, Linus Lindqvist & Oskar Nordström Skans "Stepping-stones or dead-ends? An analysis of Swedish replacement contracts"

2005:19 Dahlberg Matz & Magnus Gustavsson "Inequality and crime: separating the effects of permanent and transitory income"

Dissertation Series

2003:1 Andersson Fredrik "Causes and labor market consequences of producer heterogeneity"

2003:2 Ekström Erika "Essays on inequality and education"