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WORKING PAPER

Income Inequality Data in Growth Empirics: From Cross-Sections to Time Series

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Abstract

As in any other field of applied macro-economic or econometric research, researchers who study income inequality have to look for suitable data. Although most researchers just draw on some ready-made dataset, finding reliable data is not that straightforward and can even be very troublesome. This paper highlights some of the pitfalls in the use of inequality data. We deal with sampling problems, the choice of equivalence scale, one dimensional inequality measures, etc. We also introduce and describe a new secondary dataset on inequality data for a number of OECD countries. The main innovation of the dataset is that it creates the possibility to perform a time series analysis on inequality data. The data bring to the fore an additional problem: non-stationary behaviour of inequality series.

JEL Classification: C82, O15

Introduction

Different branches of macro-economic research are one way or the other involved with income inequality. Some studies *describe* the level and evolution of income inequality in an individual country (e.g. Piketty (2001) for France, Goodman (2001) for Great Britain), a group of countries (e.g. Brandolini (1998), Gottschalk and Smeeding (2000), Jenkins and Van Kerm (2003)) or even the entire world (e.g. Sala-i-Martin (2001), Milanovic (2002), Deaton (2003)). Some authors go a step further and try to *explain* the observed trends and changes in income inequality (e.g. Gustafsson and Johansson (1997), Förster (2000), Berry and Serieux (2003)). Thirdly, income inequality in itself appears as an explanatory variable in a lot of empirical work. One specific domain of research focuses on the effects of income inequality on economic growth (see Aghion *et al.* (1999) or Gobbin and Rayp (2004) for a brief overview).
Although the above papers do not share a common goal, they face a common difficulty: the need to define and measure income inequality. At first sight this seems fairly straightforward and in fact a lot of authors just turn to some ready-made dataset. No further questions are asked. However, as we shall argue in this paper, a bit more thought is definitely justified.

The main purpose of this paper is descriptive in nature. We clarify various practical difficulties with the use of income inequality data in macro estimates and provide guidelines for applied research on a macro-level. To illustrate our arguments we use examples from the economic literature with respect to the effects of income inequality on economic growth. Most arguments can easily be extrapolated to other areas of macro-economic and macro-econometric research.

In the first part of the paper we analyse the road from income measurement to the calculation of income inequality measures. Firstly, we look into the measurement of income flows. We define 'income' and look at two registration methods: tax records and household surveys. Secondly, we compare incomes that are measured at a different recipient level: how can the incomes received by the constituent members of a household be transferred in a single household income, and vice versa? As one can not include an entire income distribution in a regression analysis, one needs to capture the distribution in a one-dimensional income inequality measure. We map some important problems related to these one-dimensional inequality measures. To better grasp the relevance of the problems in empirical work, we also present some illustrative simulation exercises. Based on these we argue that consistency of cross-country inequality data is hard to ensure. On that account we gather inequality data for all OECD countries and sort out the countries for which a long and consistent annual time series exists. We present the resulting dataset in the second part of the paper and look for trends in income inequality over the last decades in the included countries. We make some inferences about the time series characteristics of inequality data and point out the non-stationary behaviour of these series. The dataset can be helpful to explore the possibilities of a time-series approach to the econometrics of inequality and growth.
We end by summarizing the most important insights of the paper.

Measuring incomes … part one: what income?

Before one can compute the amount of income inequality in a society, one needs to measure income itself. A lot of practical and conceptual difficulties can hinder the *registration* of income flows (cf. infra) and already the first step in the process turns out to be quite treacherous: how does one define 'income'? Which sources of income does one want to include (or exclude)?
Figure 1 gives a stylised overview of the different components that potentially matter for the measurement of income. An obvious starting point are the wages, salaries, … and other

forms of *labour income*. While this component is the main source of income on the macro-level, it will not be the main factor in the top incomes. Corporate profits, interest income and property rents must necessarily be added to complete the picture. The government will interfere in economic life by taxing the different income flows and by transferring income (e.g. unemployment benefits) to the destitute in society. Transfers might also stem from other agents in the private sector. Clearly this kind of redistribution will affect a household's disposable income. Also non-monetary income can be taken into account: e.g. if education is provided at a strongly reduced cost, the disposable income of households with children increases. In a similar way the composition of taxes might matter: the replacement of direct taxes by indirect taxes will affect households according to their respective consumption patterns.

<insert figure 1 around here>

In practice, it may not be straightforward to obtain sound data for all income flows. Two income concepts are directly measured: the tax administration registers the taxable income, household surveys aim at disposable income. For other income concepts one needs to merge different data sources. And even tax and survey data are not trouble-free.

The tax administration has (should have) information with respect to all *taxable* income. However, that is only an approximation of total income. The quality of the approximation will depend on the specifics of the tax system (e.g. tax exemptions), the amount of tax evasion, … Capital income will typically be underestimated if tax data are used. Especially the data for the lowest and highest incomes will be less reliable. Because the importance of underreporting differs with income levels, it constitutes a sizeable threat to the quality of the inequality measures. Moreover tax data contain only very limited information with respect to the amount of income received outside the production sphere. Income after taxes is not likely to be a good proxy for disposable income if there exists an extended welfare system.

A source of information more suited to approximate disposable income are the regular household income surveys that are organised by most countries[1]. But can survey data provide a more reliable picture than tax data? Although much will depend on the way these surveys are conceived, the results will probably match the true disposable income better than those obtained by using tax data. But surveys also suffer from considerable shortages. Szekely and Hilgert (1999) show that high incomes are measured with significant error in surveys. Rich people will 'moderate' their income and poor (phone-less) people will be underrepresented. Not all sources of 'non-monetary' income will show up in the survey results: e.g. direct income support by the government will be measured, but direct consumption subsidies will not. Moreover, as the results of a survey are 'projected' on the entire population, the quality of the data will also depend on the quality and the proximity of the latest population census (next to the quality of the survey itself of course). As surveys do not capture the entire population there is a risk that the sample properties do not perfectly match the properties of the entire population. Breunig (2001, 2003) shows (theoretically and empirically) that for some inequality measures (e.g. the coefficient of variation) there exists a small sample bias in case of positively skewed income distributions. Cowell (2000) mentions that for most inequality measures sampling error is only of secondary importance. As we are interested in the sampling properties of inequality measures that are most commonplace in empirical literature, we perform an explorative simulation exercise. First, we randomly draw

[1] On the other hand surveys might not outperform tax data in e.g. the measurement of income before taxes. Hence one can not make the general statement that one data source is superior the another in the measurement of 'income'.

5000 incomes from a lognormal distribution $L(\mu,\sigma^2)$ with μ = 1 and σ = 0.7[2]. The choice of parameters implies that 97.5% of the population earns less than 7.5 times the mean income. The mean itself is irrelevant, as we consider two mean-invariant inequality measures: the gini coefficient and the ratio of the income share of the tenth and first deciles[3]. The choice of distribution will influence the results of the simulation (e.g. the weight in the tails of the distribution will clearly matter for the value of the deciles ratio). We choose the lognormal distribution[4] with the specified parameters because it fits reasonably well the kernel estimates of the household income distribution for a number of European countries of Papatheodorou *et al.* (2003).
Next we randomly select 50, 100, 250, 500 and 1000 incomes out of the 5000 simulated incomes. We repeat this selection 100 times (without changing the 5000 initial incomes). The results are presented in table 1.

<insert table 1 around here>

Though we will not overstate the importance of the results given the limited set up of the simulation, we can nevertheless conclude that even in the absence of measurement error and systematic misrepresentation of income classes the risk of miscalculation remains. Even if we cover 20% of total population the minimum and maximum values of the gini and the deciles ratio differ substantially from their mean.

In the above simulation the samples were randomly drawn. In reality (cf. supra) the sampling errors might be systematic rather than random. Samples will not be reliable as rich households underreport their incomes and poor households are hard to include. We again draw 5000 incomes from a lognormal distribution with μ = 1 and σ = 0.7. Next we reduce the sample deleting all incomes below 0.86[5] (low incomes are not included). All incomes above 8.63 are cut off at that value (high incomes are underestimated). We repeat the procedure 100 times. The gini coefficient for the unadjusted (correct) total sample has a mean of 0.379 with a standard deviation of 0.004. The gini coefficient of the adjusted data (i.e. the reduced sample) has a mean of 0.349 with a standard deviation of 0.003. The difference is quite large and significant. The deciles ratio has a respective mean of 6.032 and 6.033 with a respective standard deviation of 0.143 and 0.150. So misrepresentation seems to have a differential effect depending on the inequality measure.
Both small sample properties and sampling errors could potentially affect the validity of inequality measures based on survey data.

Up to here we did not question the fact that we measure income. For many research questions the wealth distribution or asset distribution might matter more (e.g. to look into the relevance of credit constraints). But the practical difficulties linked to the measurement of assets are even larger. Deininger and Squire (1998) argue that land distribution outperforms income distribution as a proxy for asset distribution. In their sample the correlation between the gini coefficient for the initial distribution of land and that of incomes is only 39%.

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[2] Consider an essentially positive variate X $(0 < X < \infty)$ such that Y = Log(X) is normally distributed with mean μ and variance σ^2. We then say that X is lognormally distributed and write X is $L(\mu,\sigma^2)$ and Y is $N(\mu,\sigma^2)$.

[3] Whether inequality measures should be mean-invariant is also an object of discussion. If two distributions only differ in their mean, a lot of people will consider the one with the higher mean to be less unequal as everybody is richer in absolute terms (see Sen (1973)).

[4] We can also motivate our choice of a lognormal distribution by referring to Gibrat's law. Gibrat (1931) showed that if one excludes the top 1% of earners, the remaining 99% of incomes follow a lognormal distribution. This result is generalized into Gibrat's law or the 'law of proportionate effects'. It basically states that if a variable undergoes random independent proportionate changes, the distribution of the logarithms of this variable will eventually be approximately normal. However, Gibrat's law is not undisputed (see e.g. Kalecki (1945)).
It has been shown that the top 1% of the income distribution can be represented by a pareto distribution.

[5] With a lognormal distribution with parameters 1 and 0.7, there is a 5% probability that an income is below 0.86 and a 5% probability that an income is above 8.63. In our simulation about 500 out of 5000 incomes are affected.

Deininger and Olinto (2000) also show that the correlation between income inequality and asset inequality is weak. Smith (2001) demonstrates for the US that the evolution and the level of wealth within narrow income groups is very different.
Another alternative for income inequality is human capital inequality (Castelló and Doménech (2002)). Human capital can serve as an indicator for potential future income which is again a highly relevant factor to assess the relevance of credit constraints. Gyimah-Brempong and Wilson (2004) look at health human capital.

Given the choice of different income (wealth) concepts, can we determine which one is optimal? The ideal definition in empirical work will depend on the theoretical framework. To illustrate this statement we look at three theories that account for a negative relation between income inequality and economic growth: the complete market model (CMM), the imperfect market model (IMM) and the socio-political instability model (SPIM) (see Perotti (1996) for an overview). In the CMM, growth is reduced because more income inequality induces a higher demand for redistribution. More redistribution means that average taxes have to increase, which in turn will disturb savings and investment decisions. To test the relevance of this model, one needs a measure of pre-tax and pre-redistribution inequality, as well as a measure of redistribution efforts. The IMM links more inequality to lower growth through the education channel. If people are poorer (i.e. lack funding), they will under-invest in human capital, which will over time slow down growth. Now disposable income (or ideally disposable assets) will matter. Finally the SPIM links more inequality to more social unrest which negatively affects the growth performance. Again disposable income will be the more relevant income variable.
In applied work one is often restricted by the available data. Therefore, many authors do not actually test what they are claiming to test. Still their results can be informative (and in any case hard to improve upon) as long as the reader is aware of the shortcomings. The potential problems might be less relevant in a time series framework if the correlation between different income series is high enough (cf. infra). Unfortunately this might be 'wishful thinking': Ervik (1998) shows that the trend in the gini coefficients based on market incomes[6], gross incomes and disposable income for 8 countries (period 1980-1995) can differ substantially. Brandolini (1998) concludes that the behaviour of inequality of market incomes is much more homogeneous across a sample of OECD countries than the behaviour of inequality of disposable incomes. Atkinson and Brandolini (1999) show that net and gross income distributions may behave differently over time within a single country. Rehme (2002) illustrates how mixing measures of gross and net income inequality can blur the estimation results if redistribution negatively affects growth. Knowles (2001) shows that many empirical results disappear once one corrects for the fact that different income concepts have been mixed together.

Table 2 compares some properties of income inequality statistics derived from tax records and of those derived from household income surveys. In view of the use of the data in growth regressions, comparability (in case of *cross-section analysis*) and frequency (in case of *time series analysis*) deserve special attention.

<insert table 2 around here>

Measuring incomes … part two: whose income?

Income flows can be measured at different recipient levels: one can look at individuals, households or families. Surveys will normally aim at household income, as it is hard for

[6] Market income is defined as "earned income of wages and salaries and self employment, cash property income and other private cash income transfers". Gross income is a somewhat broader concept and also includes social insurance cash benefits, universal cash transfers and social assistance.

individuals to discriminate between income flows of the constituent household members. Tax data are more mixed: depending on e.g. the wedded state sometimes household income and sometimes individual income is registered. Again it will be useful to have some insights in the specificities of the tax system.

Even if income flows are consistently reported at a certain level, this might not be the level preferred by the researcher. It will suffice to add the individual incomes of the different household members to compute total household income. But it is less straightforward to approximate individual disposable income on the basis of household income data or to compare households that differ in size and composition. Dividing total household income by the number of household members might not be the best approach as it will probably result in an underestimation of disposable income. Normally there will exist scale and scope effects in consumption on the household level (e.g. one television can suffice for the entire household). Thus the disposable (expenditure) income of both constituent parts of a couple will probably exceed half of total household income. Moreover, needs can differ across household members (children will normally have less and less expensive needs). One needs to convert non-comparable incomes received by households, to comparable welfare imputed to individuals. *Equivalence scales* seek to answer the question “how much money does a household need to spend to be as well off as a single person living alone?”. Initially this question was answered in terms of equal consumption levels. Nowadays the focus is on equality of utility. Estimating equivalence scales is far from straightforward as one needs to specify individual and household utility functions[7], demand functions, the extent of joint consumption within a household, … (see Browning *et al.* (2004) for a complete methodology).
A popular method, given the complexity of the matter, is to assigns different weights to the different household members to derive the ‘equivalent disposable income’. A frequently used procedure (e.g. by the Luxembourg Income Study (LIS)) is to assign a value of 1 to a single adult person, a value of 1.7 to a couple and add 0.5 to these figures for each child. As estimated equivalence scales differ substantially, these figures can be seen as a ‘feasible compromise’. However, other (equally sensible) choices can made. For instance, one could take into account that the needs of households with only 1 child will differ from those of households with more children. Next to household size and composition also additional influential family characteristics such as region or location can be accounted for. It is important to be aware of this if one uses ‘ready-made’ data. A different choice of equivalence scale could result in a different income distribution and introduce a bias in cross-country estimates. For a more comprehensive discussion of equivalence scales we refer to Cowell and Mercader-Prats (1999), Flückiger (1999), Atkinson et *al.* (1995) and Buhmann et *al.* (1988).

To assess the impact of different equivalence scales in inequality measurement, we resort to a simulation exercise. First, we randomly draw 500 incomes from a lognormal distribution with μ = 1 and σ = 0.7 (cf. supra). In a second step we determine the marital status of each person. We randomly draw a number from the set $\{1,2\}$ for each person. A person is single if we draw the number 1, otherwise he is married. If somebody is married, we draw an additional income out of the same lognormal distribution and add it to his income. Next we determine the number of children in a household. We only consider ‘traditional’ households: married people can have 0, 1, 2 or 3 children, singles have no children. Again we randomly draw a number from the set $\{0,1,2,3\}$ for each household. The number we draw equals the number of children in a household. Children do not earn an income. Finally we consider four types of equivalence scale to adjust household income.

[7] Strictly speaking not households but the individuals that compose it have utility. However, preferences of single individuals might change once they belong to a household (e.g. the utility of ‘eating at a restaurant’).

(0) No correction: the sum of the incomes of both spouses equals household income, i.e. we ignore household ties and scale effects on the household level;
(1) The 0.5 equivalence scale: total household income is divided by the square root of the number of household members;
(2) The per capita scale: total household income is divided by the number of household members;
(3) The LIS scale: total household income is corrected using the LIS scale (single adult:1, couple: 1.7 augmented by 0.5 per child).

Note that all corrections depend on household size but differ in the weights that are accorded to each household member. Again, the included equivalence scales are the ones that are commonplace in standard empirical work[8].

Next we compute gini coefficient and the ratio of the 10^{th} and 1^{st} deciles of the income distribution based on these (corrected) data. We repeat the procedure 500 times, which enables us to evaluate the differences in the inequality measures caused by the use of different equivalence scales. We have summarized the main conclusions in table 3. Not correcting for household size logically results in an overestimation of inequality. However, it does not seem to matter much which correction is being used: for both the deciles ratio and the gini coefficient the differences in the mean are small and statistically insignificant.

<insert table 3 around here>

If we think of the 500 runs as 500 different countries, we can compute a correlation between the inequality measures and order the countries based on the degree of inequality. If we neglect the uncorrected measure, the correlations are very high for the gini coefficient and somewhat lower for the deciles ratio. In panel C we list the average differences in the ranking of the distributions if a different equivalence scale is used. If we compare e.g. the ranking of the 500 distributions based on the uncorrected data with the ranking based on the data corrected with the 0.5 equivalence scale we obtain an average difference of 50 positions (with a standard deviation of 43)). The differences in ranking seem substantial. However, one should bear in mind that all incomes were drawn from the same lognormal distribution. Therefore, income distributions are very similar and rankings will consequently be very prone to small changes. If we introduce larger differences in income distributions, the rankings become more stable[9].

Our conclusion mimics the one by Atkinson et *al.* (1995) based on LIS data. Inequality rankings at a point in time are fairly robust to the choice of equivalence scales. Buchmann et *al.* conclude that *"equivalence scales have in general no great effect on the rank order of measured inequality across countries as long as average family size is not extremely large"*. Hence the choice of equivalence scale in a cross-country or panel approach seems a less decisive factor as long as an identical scale is used for all countries. However, mixing data based on different scales is much less sensible.

What does one number say?

It is convenient to capture an entire income distribution in a single scalar in order to use it in macro estimates. On the downside one single statistic will never reveal all relevant aspects of the entire distribution. Moreover, there are different possibilities to represent a distribution. All these summary statistics can be used to rank countries, but these rankings can tell a different story (Caminada and Goudswaard (2000)).

[8] We do not insinuate that these equivalence scales are to be preferred. As they only take household size into account (and ignore location, composition, etc.), they can certainly be approved upon.

[9] If we allow for a lognormal distribution that can vary in σ between 0.4 and 0.9 the mean differences in ranking drop by about 80% (for both gini and deciles ratio). So especially with very similar income distributions, the choice of equivalence scales is important.

Does one measure systematically outperform the others? Or should one make the choice dependent on the research question? We limit the discussion in this section to some selected issues and surely do not aim to provide a comprehensive overview of income inequality measurement. For further reading we refer to different chapters in the 'handbook of income distribution' (2000) and the 'handbook on income inequality measurement' (1999) next to seminal contributions by Kolm (1969), Atkinson (1970) and Sen (1973).

The existence of 'competing' measures of inequality stems from the dual nature of the conception of inequality. Next to an objective element in this notion (a 50-50 division of the cake is objectively more equal than giving all to one and none to the other) there is also a distinctive normative feature. In more complex problems, comparing alternative income distributions among a large number of people, the measurement of the inequality measure could be intractable without bringing in some ethical concepts (Senn (1973)). Thus inequality measures become dependent on one's ethical values. Generally one needs to make a specific choice of criteria of comparisons, properties of measures, parameters, etc. based on one's motives to make these comparisons or compute these measures. Atkinson (1970) argues that the first step in measuring inequality is specifying the social welfare function (SWF). He notes that '*while there is undoubtedly a wide range of disagreement about the form the SWF should take, this direct approach allows us to reject at once those [inequality measures] that attract no supporters and also serves to emphasise that any measure of inequality involves judgments about social welfare'* (Atkinson (1970), p.257).

Income distributions are frequently represented through the Lorenz curve. This curve plots the cumulative percentage of income recipients arranged in rising order of income versus the cumulative percentage of income they earn. If everybody has the same income the Lorenz curve will be a straight (45°) line. If there exists some inequality, the line will be dented. One might be tempted to conclude that the bigger the dent is, the larger the amount of inequality will be. However, the Lorenz curve does not allow for a unique ranking of distributions based on the degree of inequality as the possibility of intersecting Lorenz curves can not be excluded[10]: no generalised statement about overall inequality can be made with a one dimensional 'Lorenz curve'-based inequality measure.
Different inequality measures are based on the Lorenz curve, some giving more weight to the bottom incomes (e.g. entropy measure with a low parameter value), some focusing more on the middle class (gini coefficient) and some valuing the top incomes more (e.g. entropy measure with a high parameter value). The gini coefficient measures the surface between the 45° line and the Lorenz curve. Figure 2 illustrates how different distributions can result in an identical gini coefficient. Changes in aggregate inequality can hide specific movements in the middle and the extremes of the income distribution. The gini coefficient will hardly react to the transfer of income from the poorest to the richest, because the middle part of the distribution is unaffected. The non-uniqueness of the ranking may not only mask changes in inequality as recorded by one specific measure, it may also lead to contradictory conclusions if different measures are used to compare distributions. Returning to our earlier example: while the gini coefficient will hardly change, the entropy measures will since they are much more sensitive to changes in the bottom and top part of the distribution.

<insert figure 2 around here>

Cowell (1995) shows that the generalized entropy class of indices (including the Theil and Atkinson indices) are theoretically superior for the measurement of inequality. This superiority follows from the fact that these measures are based on an axiomatic approach to measurement of inequality. They incorporate the notion of 'inequality preferences' (how

[10] To illustrate that the possibility of intersecting Lorenz curves is not negligible: Atkinson (1970) finds that in the data on five developing countries and seven advanced countries collected by Kuznets (1963), out of 66 pairwise comparisons of the Lorenz curves, only 16 do not intersect.

strongly does one dislike inequality) and are decomposable (inequality of the whole population can be decomposed in inequality between and within different subgroups). However, in empirical work, these measures are not commonly used as they are not readily available.
In table 4 we look into some influential contributions in the inequality-growth literature, and report which inequality measure they use. Next we point out some of the advantages and problems of the measures that are commonplace in empirical applications.

<insert table 4 around here>

Note that the Deininger and Squire dataset (DS) (cf. infra) has somewhat become 'the standard' in the empirical research since its conception in 1996. Also note that most studies use either the gini coefficient or some kind of deciles/quintiles ratio to measure income inequality. So they seem to disregard the theoretically preferable measures. The reason for this is fairly obvious: the availability of the gini coefficient (and to a lesser degree the ratio measures) is far greater than that of any other inequality index. An additional motive is a bit perverse: as the use of the gini coefficient is commonplace in empirical applications using it is necessary if one wants to compare with previous studies. As already noted the gini coefficient captures the entire income distribution, but fails to deliver an unique ranking. Especially the share of the middle class will matter. Obviously the ratio measures will also produce a ranking that is not unique. Another common measure in empirical work is the 'poverty line'. This measure indicates the percentage of the population that earns less that a certain amount of money a day. It is especially useful to map the degree of poverty in a society. However, it is hard to measure in surveys as the poorest are usually the hardest to sample.

That the choice of inequality measure matters is illustrated by Partridge (1997). For a sample of US states he finds that a larger gini coefficient (normally interpreted as more inequality) results in more growth. However, he also notes that a larger share of income for the middle quintile (normally interpreted as less inequality) increases growth. Although these results are not necessarily irreconcilable, they provide a warning not to base conclusions on a single indicator. A similar conclusion is reported by Szekely and Hilgert (1999). In their estimates there is no significant relation between the gini coefficient and economic growth in a group of Latin American countries. However, if a bottom sensitive entropy index is used the effect of inequality on growth is negative and marginally significant. If instead a top sensitive entropy index is used, both variables are significantly positively related. So in their group of countries equally acceptable summary measures lead to significantly different conclusions about the effects of inequality on growth. One should be wary of hasty conclusions and preferably look at several inequality measures.

Again the choice of the inequality measure might be inspired by the theoretical framework. Credit constraints are at the heart of the IMM. Thus one should pay special attention at the bottom decile of the income distribution to explore that particular model. In the CMM the median voter will play an important part as the policy maker will determine the appropriate tax rate based on his relative position. An income measure that assigns more weight to the middle class (the gini coefficient or the income share of the middle quintiles) could now be preferred.
Alternatively the choice can be based on welfare considerations. If one believes that poverty is the worst of evils, one will look at a poverty line or poverty gap indicator. If one is more interested in the position of the middle class the gini coefficient will be a better alternative.

To get a first impression of the potential bias in estimation results caused by differences in the inequality measure used, we perform a simulation exercise. For 50 countries we randomly draw 1000 household incomes from a lognormal distribution with μ fixed at 1 and σ randomly chosen between 0.6 and 0.8. This means that in the most equal distribution

95% of the population has an income that is below 9 times the mean income. In the most unequal distribution 95% of the population has an income below 13.5 times the mean. We also allow the mean to differ: the highest mean is 5 times the lowest mean. This will only make a difference for the value of the head count index, as all other included measures are relative to the mean income. The more the distributions resemble each other, the more important small income changes will be to discriminate between them. In particular the probability of intersecting Lorenz curves will be much higher if distributions are very similar (cf. supra) and hence a ranking of distributions based on e.g. the gini coefficient will be less robust. We only allow for limited difference in income distributions to see how sensitive the ranking of similar countries is to the use of different measures. Alternatively our 'simulated' (ranking) correlations could be considered as lower limits for a more diverse group of countries.

We repeat the exercise with samples of only 50 households to get an idea of the impact of outliers (unrepresentative samples) on the inequality measures. Cowell and Flachaire (2002) show that the Atkinson index with an inequality aversion parameter above 1 are very sensitive to small incomes. The gini index is less sensitive to extremely high incomes. Therefore, the differences between indices will be influenced by the size (representativeness) of the sample.

We compute 5 inequality measures from the simulated data:

- The gini coefficient;
- The Atkinson index with inequality aversion parameter 0.5 (low importance of inequality);
- The Atkinson index with inequality aversion parameter 5 (high importance of inequality);
- The ratio of the share of the tenth and first deciles in the income distribution;
- A head count index (the percentage of households earning less than 65. This number is equal to 60% of the mean of the income distribution with lowest possible mean income).

We repeat the procedure 100 times.

<insert table 5 around here>

A glance at table 5 shows that the differences in ranking caused by using different inequality measures can be important. The correlation between the different inequality indices and the head count index is very low. As the head count index is a *poverty* measure it does not come as a complete surprise that is only weakly correlated with *inequality* measures. Inequality measures should circumscribe the entire distribution, poverty measures only consider the bottom part. While the correlations and rank correlations between the (pure) inequality measures are on average fairly high, they sometimes differ substantially across simulation runs. Some measures are more influenced by certain particularities in the income distribution than others. In small samples the correlations are substantially lower.

Figini (2000) also finds high rank correlations between different inequality measures based on 'real life' LIS data, which indicates that our results can not be fully 'blamed' on the use of 'ad hoc' distributions.

From a point of view of empirical work, it is also interesting to look at the differences in the amount of variation in the different measures. First we rescale the measures to make them comparable. The 'standardized' variances of the gini coefficient and the Atkinson index with low inequality aversion are very similar and only about 1/4th of the variances of the deciles ratio and the Atkinson index with high inequality aversion. Overall the measures seem very stable. Of course the variance will increase if we consider more diverse income distributions. However, our results might be informative for the group of quite similar OECD countries.

Half a pound of data!

Do there exist reliable sources for ready-made income distribution data? A look at table 2 indicates that the answer might be 'yes since 1996'. Since then most researchers have applied the 'Deininger and Squire'-dataset (DS, Deininger and Squire (1996)). The DS dataset has substantially increased the data availability and comparability, both over countries and over time. Before 1996, data had to be gathered form multiple sources and data comparability was sometimes unacceptably low. Deininger and Squire collected most of the known income distribution data, submitted them to a thorough investigation and finally divided them into different categories according to some quality standards. The end result is a subset of the data that the authors have labelled 'high quality data'. The (updated) DS dataset covers a few observations of 112 developed and developing countries (in total nearly 700 'acceptable' observations) and thus seems mainly suited for cross-section or panel estimates.
The 'high quality'-label assigned to the data is not entirely undisputed. Atkinson and Brandolini (1999) show that there remain important problems, even with the so-called 'high quality' data in the DS data. They question most 'corrections' to the raw data proposed by Deininger and Squire as they doubt that these adjustments really solve the comparability problems. In short they are "*not convinced that at present it is possible to use secondary data-sets safely without some knowledge of the underlying sources; and* "*caution strongly against mechanical use of such data-sets"* (Atkinson and Brandolini (1999), p.35). Put differently, it is best not to mix data from different sources. Notwithstanding this clear warning, most authors have done just that.
Besides the DS dataset, two other influential secondary datasets exist. The World Institute for Development Economics Research (WIDER) of the United Nations University and UNDP maintain an extended version of the DS dataset[11]. Finally the Luxembourg Income Study (LIS) collects income survey data of its 29 members. Currently it provides data for a period covering 1970-2000 for the 'best' countries. This corresponds to 7 or 8 data points. But for most countries one has to settle with a shorter time period and less data points. The main achievement of the LIS is that it 'enforces' a strictly defined methodological framework on its members to end up with comparable data[12].

Another noteworthy initiative is the University of Texas Inequality Project (UTIP, Conceição and Galbraith (1998)). They have constructed long and dense time series of the Theil inequality index for over 150 countries over the period 1963-1998 based on the UNIDO Industrial Statistics. The advantages of this dataset are a very large coverage, consistency and accuracy. However, it is doubtful that the UTIP measure is a good proxy for income inequality. It is solely based on wages[13] and earnings in the industrial sector. Not only is e.g. capital income neglected, but it also assumed that earnings inequality in the industrial sector is similar to earnings inequality in other sectors of the economy. This assumption seems untenable as earnings dispersion in the services sector is normally much wider. Especially for OECD countries, with economies dominated by the services sector, the UTIP data might be an inferior proxy for total inequality.

<insert graph 1 and graph 2 around here>

To illustrate the differences in the datasets we present some graphs for Belgium and the United States. For Belgium we look at the LIS data (2 measures), the DS data, national tax data (income after taxes, 2 measures) and the UTIP data. For the USA we include the LIS

[11] For more details and access to the data: http://www.wider.unu.edu/wiid/wiid.htm
[12] For more details and access to the data: http://www.lisproject.org/keyfigures/methods.htm
[13] According to the Belgian Household Budget Survey (NIS (2003)) wages accounted for only 39,7% of the average disposable income of Belgian households in 2001. Non-wage economic activities accounted for an additional 18,1%. Social security payments and capital income accounted for respectively 29,7% and 14,7%. So while wages make up the largest component, the importance of other components can not be minimized.

data (2 measures), the US Bureau of the Census data (identical to the DS data, 3 measures) and the UTIP data.
Graph 1 compares inequality data for Belgium. On the right axis (full marks) one can read the value for the different gini coefficients. On the left axis (hollow marks), the values for the Atkinson (with a parameter value of 0,5) and Theil index can be found. First note that the gini coefficients and Atkinson indices tell roughly the same story. Secondly, although we have very few data points, the evolution in inequality seems similar in all data series, except for the UTIP data. The correlation between the tax data and the UTIP data is –0,68 for the gini coefficient and –0,75 for the Atkinson index (but there are only 20 overlapping years)
Graph 2 plots the data for the United States. The value for the full marks are again on the right axis. Again only the UTIP data seem to change the global picture (although one would also conclude that inequality is rising). The correlation of the UTIP data and the USBC data is 0,75 for the gini coefficient, 0,71 for the Atkinson and 0,67 for the Theil index.
Although we can not deduce much from the figures, we nevertheless can conclude that the choice of database can gravely affect the results of the analysis.

From cross-section to time series: a new secondary dataset on income inequality in OECD countries

As dense time series of inequality measures over longer time periods were unavailable, researchers have used cross-section estimates to determine the relationship between income inequality and economic growth. Recently researchers also started exploiting the time dimension with the advent of panel data techniques. In these studies researchers combine inequality data from different primary data sources. However, this practice entails certain risks.
In the first part of this paper we have shown that inequality measures are sensitive to changes in equivalence scales and income concept. As long as the same income definition is used for all countries, the impact on the estimates of changing that definition might still be limited. However, mixing income concepts over countries will surely blur the results. Knowles (2001) provides additional support for this result. In cross-country growth regressions most authors have used the DS data on income inequality (see table 4). Although the DS dataset has the huge merit of having increased data availability, it does not solve the comparability problem. Atkinson and Brandolini state that *"in the dataset, the user is faced with a variety of different types of estimate"*. They sum up seven sets of differences concerning income definitions, next to differences in data sources and processing. The LIS has strongly improved data comparability for the developed countries. But several problems remain as the underlying 'rough' data were collected for different purposes in different countries (see Gottschalk and Smeeding (2000)). This is reflected in differences in the survey set-up, which are bound to influence the survey results. Szekely and Hilgert (1999) show that previous estimation results based on survey data turn out to be quite sensitive to minor plausible corrections to the data. Tax systems differ even more across countries.
Filer *et al.* (2004) conclude that "*the adjustment made to create cross-sectional comparability are complex and can seriously distort within country patterns over time*" (p. 14). Although they only investigate cross-country growth rates, their warning is universally aimed at all data that have been adjusted to increase cross-country comparability.

To control for differences in income definitions and data sources across countries, researchers typically introduce dummy variables in the regressions. But is this an effective solution? Atkinson and Brandolini (1999) do not believe that heterogeneity in inequality statistics can be eliminated by a simple additional or multiplicative adjustment. The differences between income concepts are likely to be country specific as a result of differences in government fiscal policies and tax incidence. Other studies have shown that the trend in the series of different income concepts within one country can differ substantially (Ervik (1998), Brandolini, (1998) Atkinson and Brandolini (1999)) (cf. supra). A quick glance at the third panel of figure 3 shows that inequality of before and after tax income in Belgium has evolved differently over time. A dummy variable only takes care of the level differences. Thirdly, there is a possibility that a dummy variable captures an effect unrelated to the difference in income concept. This possibility is especially relevant in a context of growth regressions in which a lot of effects are unaccounted for (the omitted variable bias is a well-known problem in growth regressions).

Given the comparability problems in a cross-section framework, it might be interesting to explore the possibilities of a time series approach. Although inequality measures within one country over time will also suffer from changes in the methodology, the transparency of the data series will be higher as one will probably know when and how the methodology has changed (although adjusting the data might still be difficult). Next to comparability, there are two other important advantages. Firstly, the cross-section approach bases its conclusions on a single observation. This observation might be recorded during a transitional phase or might be distorted due to some temporary influences. The importance of a single observation is lower in a time series framework and it is more likely that outliers will be spotted. Secondly, it is interesting in its own to get some insights into the time series properties of inequality series (e.g. deterministic or stochastic trends in the data).
But while cross-section estimates are not trouble-free, a time series approach seems even more troublesome (Parker (2000), Gobbin and Rayp (2004)). The datasets we described in the previous section (with the exception of the UTIP dataset) are only suited for cross-section or panel estimates, as very few data points are available (5 or 6 data points over a period of more than 30 years). Therefore one has to look for other data sources, most notably the statistical agencies, tax administrations or 'national inequality experts' of individual countries. This is a highly time consuming occupation with no a priori guarantee at success.

We have tried to accomplish the 'nearly-impossible' and have taken on the painstaking task of gathering a (limited) dataset for income inequality containing consistent annual time series. The data are not new (although some series were not publicly available). We invested a great deal of time in collecting as much data as possible on income inequality for all OECD countries. In the dataset we only include the countries for which we find at least 25 consecutive and consistent observations.
As we want an annual inequality measure over the longest possible time period, the gini coefficient turns out to be the only viable alternative. The one exception is France for which we include the income share of the 5% richest as a proxy for income inequality.
In our sample, the gini coefficients for the different countries are not all based on the same income concept, nor were they all collected in the same manner. Some gini coefficients were derived from census data, others were calculated out of income tax data. Some inequality measures concern disposable income, some after tax income, etc. However, this might not be a huge problem, as long as the user is aware of it and does not use the data in a panel regression context. This contrasts with past practices: users of the DS/LIS dataset assume that it is fully consistent although several authors have already illustrated that it is not (cf. supra). An advantage of our dataset is the consistency of the measures over time within the individual countries.
In the remaining part of this section we give a description of the dataset. Secondly we look for some trends in income inequality in the OECD countries and finally we check the time series characteristics of the data.

Description of the dataset

Nine OECD countries are included in the dataset. The choice of countries was forced upon us due to the limited data availability (we started with all OECD countries). Fortunately the sample seems to be instructive for the entire OECD. Firstly, with Canada, France, Italy, the UK and the USA, 5 members of the G-7 are present as well as 4 smaller countries: Belgium, Finland, the Netherlands and Sweden. Secondly, the sample contains countries with an

extensive social security system (Belgium, Finland and Sweden) and countries with a limited one (the UK and the US). Thirdly, also non-EU countries are present. However, it is certainly regrettable that we were not able to include Germany.
The series for Canada, Italy and Sweden are composite series. We have combined observations from different sources to obtain longer data series. For Canada and Italy we mixed the different series. For Sweden we used the growth rates of 2 other series to extrapolate (back– and forwards) a 'basic' series. Combining series might not be the most elegant nor the most prudent approach and potentially erodes the value added of this dataset compared to the 'conventional' ones. Remember that our main motivation to construct the dataset was the greater consistency of the data over time than across countries. Given the good correspondence of the overlapping values we believe that the series for Canada and Sweden[14] remain reliable. The Italian series, however, should be looked at with a bit more prudence as we use 3 different series and the number of overlapping values is limited. Some missing values (Belgium, Italy, the Netherlands) were generated by linear interpolation.
Table 6 gives an overview of the data sources. We have also included some information on the countries that were not included as we did not find sufficient data.

<insert table 6 around here>

For Belgium, the gini coefficients of income before and after taxes are strongly correlated (.96). For the USA there exists a slightly shorter series for household incomes. The correlation between inequality of household and inequality of family income amounts to 0.97. Therefore we only look at the latter (i.e. the longest) series. Remember that we had to settle with a different inequality measure for France (income share of the richest 5% of the population).

Trends in income inequality

We have plotted the evolution of the inequality measures in our dataset to get an impression of the evolution of income inequality in the OECD countries over the last 3 decades. Note that it is risky to look at the absolute value of the inequality measures, as we use different definitions for different countries.
The graphs indicate that Canadian and French inequality has remained fairly stable (fluctuations within a 2.5%-points window).
In Finland inequality has steadily gone down between 1970 and 1992. In contrast, Swedish inequality has steadily increased.
Belgian inequality dropped substantially in the 1970s, but started to rise again in the early 1980s. The large decrease in 1982 is somewhat strange and can probably be explained by a methodological change. Also note that the Belgian tax system has a redistributive impact: the gini of after-tax- income is substantially lower than that of before-tax-income (up to 8%-point).
The large fluctuations in the first years of Dutch data seem to indicate that these are somewhat less reliable (although the fluctuations remain within a 4%-points window). From 1986 onwards inequality remains stable in the Netherlands.
Italian inequality dropped in the 1970s, and seems to fluctuate around a lower mean since the 1980s.
Inequality has strongly increased since the late 1970s / early 1980s in the UK and the USA. In the 1960s and early 1970s inequality fluctuated around a constant mean. The trend shift roughly coincides with the start of the 'Thatcher era' in the UK (1979) and the 'Reagan reign' (1981) in the USA.

[14] For Sweden the differences between the level of inequality in the overlapping years of the composite series (each time 3 years) remains fairly constant. For Canada the difference remains fully constant (0.02). Hence we just added this difference to the second series.

<insert graph 3 around here>

In general we do not discern a global trend in income inequality in our sample of rich countries. On the contrary: the individual country experiences are quite diverse. This conclusion is similar to the one of other recent comparative studies (e.g. Brandolini (1998), Gottschalk and Smeeding (2000), Atkinson (2003)). This should not come as a surprise given the fact that the degree of income inequality in a country is determined by many factors including social and political forces as well as economic ones.

Limitations of the dataset

We have to admit that the dataset can not fully live up to our initial expectations: next to the fact that we needed to combine data from different data sources to obtain long enough time series, there now seems to be a methodological break in de Belgian data. Although we made sure that the consistency of the series was acceptable we can only subscribe the warning of Atkinson and Brandolini (1999) against the mechanical use of secondary datasets. Notwithstanding these critical remarks, the dataset is a useful extension to existing data sources as it enables the use of time series estimation techniques.

Time series characteristics of the data

In this section we check the statistic properties of the income inequality time series in our dataset. We present the results of 2 unit root tests: the Augmented Dickey Fuller test (ADF, null hypothesis: unit root) and the Kwiatkowski Phillips Schmidt Shin test (KPSS, null hypothesis: (trend) stationarity).
To test for the presence of a unit root it is important to use a regression that mimics the actual data-generating process. If not all deterministic regressors (constant, trend) are included the power of the ADF-test will drop substantially. The power will also be reduced if a regressor is inappropriately added (Enders, 1995). As we do not know the actual data-generating process we need to proceed with caution. We apply the testing procedure proposed by Enders (1995, p. 256-257). In the first step we use the least restrictive model formulation, with a constant and trend included. If we can reject the null hypothesis, there is no need to proceed (recall that misspecification will always *lower* the power of the test). Otherwise we test for the presence of a trend under the null of a unit root (using an F-test). If the trend is significant, we check for a unit root using the standard normal critical values[15]. Otherwise, we estimate the regression without a trend and repeat the above procedure with respect to the inclusion of a constant term. The optimal lag length for the regression is determined by the Schwarz Bayesian Information Criterion (BIC).
In table 7 we only present the ADF-value for the relevant regression specification. Here we report the results for Belgian after tax inequality at length to illustrate the procedure.

Step 1: We do not reject a unit root if we allow for a constant and a trend in the specification (test statistic: -1.06, 5%-critical value: -3.50).
Step 2: Under the null of a unit root, the trend is not significant (test statistic: –0.18, 5%-critical value: -2.79). An F-test shows that we cannot reject the hypothesis that a unit root is present and the trend is insignificant (test statistic: 0.99, 5%-critical value: 6.73)).
Step 3: We re-estimate the model without a trend. Again a unit root can not be rejected (test statistic: -1.42, 5%-critical value: -2.93).
Step 4: Under the null of a unit root, the constant is not significant (test statistic: -1.56, 5% critical value: -2.54). An F-test shows that we cannot reject the hypothesis that a unit root is present and the constant is insignificant (test statistic: 1.34, 5%-critical value: 4.86).

[15] As our sample is quite limited, we also looked at the Dickey-Fuller values (the procedure proposed by Enders is asymptotically valid). The conclusions were very similar.

Step 5: We use a regression specification without trend and constant. The results indicate that a unit root can not be rejected (test statistic: 0.48, 5%-critical value: -1.95). In summary table 7 we only report this final test statistic and the end conclusion of the test procedure.

As ADF-tests are known to have low power for highly persistent series (roots near unity), we check the results of the ADF-tests by means of the KPSS-test (Kwiatkowski et *al.*, 1992). If level stationarity is not rejected at the 10%-level, we do not report the results for trend stationarity. If the conclusions of the ADF- and KPSS-test are mutually consistent, the trustworthiness of the conclusions increases.
As testing for unit roots only makes sense if the range of a variable is not bounded, we first had to logit-transform[16] the inequality data (the value of the gini coefficient is by construction between 0 and 1).

<insert table 7 around here >

The ADF-tests only reject a unit root in the levels for Finland. The KPSS-test always rejects level stationarity, except for Canada. For 3 countries trend stationarity is also rejected. The non-stationary evolution of inequality measures is also reported by Parker (2000).
If we look at the first differences (results not shown), the ADF-tests lead to a rejection of a unit root at the 1%-level for all countries but Italy (10%-level) and the Netherlands (5%-level). The KPSS-tests do not reject stationarity of the first differences, even if we look at the 10%-level, except for Finland (5%-level) and USA ‘families’ (5%-level).
So while the two stationarity test do not always lead to exactly the same conclusion, level stationarity of inequality is clearly rejected. Almost all series seem to be characterised by either a stochastic or a deterministic trend.

In some of the income inequality series (the US, 1993), there occurs a break in the data due to a *known* change in the data gathering methodology. For these series we adjust the series themselves, as a correction is quite straightforward. However, for other series there might be a break caused by a change in the methodology unknown to us, or caused by ‘other factors’. For instance, the election of Thatcher seems to have had a clear impact on the trend in income inequality in the UK. Based on a quick glance at the figures, we can further suspect a possible level break combined with a changing trend in the Belgian inequality series. Contrary to the breaks due to a changing methodology, breaks caused by ‘other factors’ should not be eliminated. However, as illustrated by Perron (1989), we should take these breaks into explicit account when we test for unit roots. For instance, unit root tests could wrongly identify a level stationary process with a level shift as a unit root process. Therefore, we performed some additional tests, based on the Perron methodology (Enders (1995), p.249; results not shown). The results indicate that stationarity is not restored once level and trend shifts are taken into account.

These findings shed a new light on existing work. Early work mainly focused on cross-section data. The time series characteristics of the data were never closely examined. However, as a panel data approach gains way, the time dimension becomes more important. It has been shown that one does not need to restrict the dynamic behaviour of the data in a panel set-up in case one has a very large cross-section dimension (N) and a small and fixed time dimension (T). On the other hand, if T is large relative to N, the ‘normal’ asymptotics may be misleading (Wooldridge (2002), p.175). This is indeed the case in existing empirical work: e.g. Forbes (2000) looks at 45 countries over a period of 25 years. So it seems that non-stationarity has been dismissed somewhat too easily as a potential problem in the panel data approach.

[16] We transformed the gini coefficients into log[1/(1-inequality measure)]. Since the gini coefficients is by definition between 0 and 1, the constructed variable will vary between zero and plus infinity.

Conclusions

As in any other field of applied macro-economic or econometric research, researchers who study income inequality have to look for suitable data. Although most researchers just draw on some ready-made dataset, finding reliable data is not that straightforward and can even be very troublesome. Firstly, one has to define which income flows matter. Secondly, one has to decide on the best one-dimensional representation of an entire income distribution (which inequality measure is preferable?). Economic theory can often provide guidance. But next follows the confrontation with a very grim reality: the desired data are almost never available. One has to settle with what is available and unfortunately that is not a lot in the case of inequality data.

Unfortunately, the choices really matter. Mixing inequality data based on different equivalence scales, different income concepts, etc. will blur the outcome of any econometric analysis. As a consequence of the data scarcity, researchers have predominantly resorted to a cross-section estimation techniques. However data comparability across countries is sometimes low. As we a priori believed that the comparability of inequality data within one country might be better, we gathered a new dataset with an emphasis on the time dimension for income inequality for a group of OECD countries. Although we could not obtain 100% consistency, we nonetheless believe that the new dataset can be a useful tool for future empirical work. A visual inspection of the time series shows that inequality has evolved quite differently in the included countries over the past decades. More robust econometric testing shows that level stationarity does not match the behaviour of the inequality series in our dataset. While the result that all series are either characterised by either a stochastic or a deterministic trend is noteworthy in itself, it is also of importance for econometric research.

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Figure 1: Incomes and income measurement

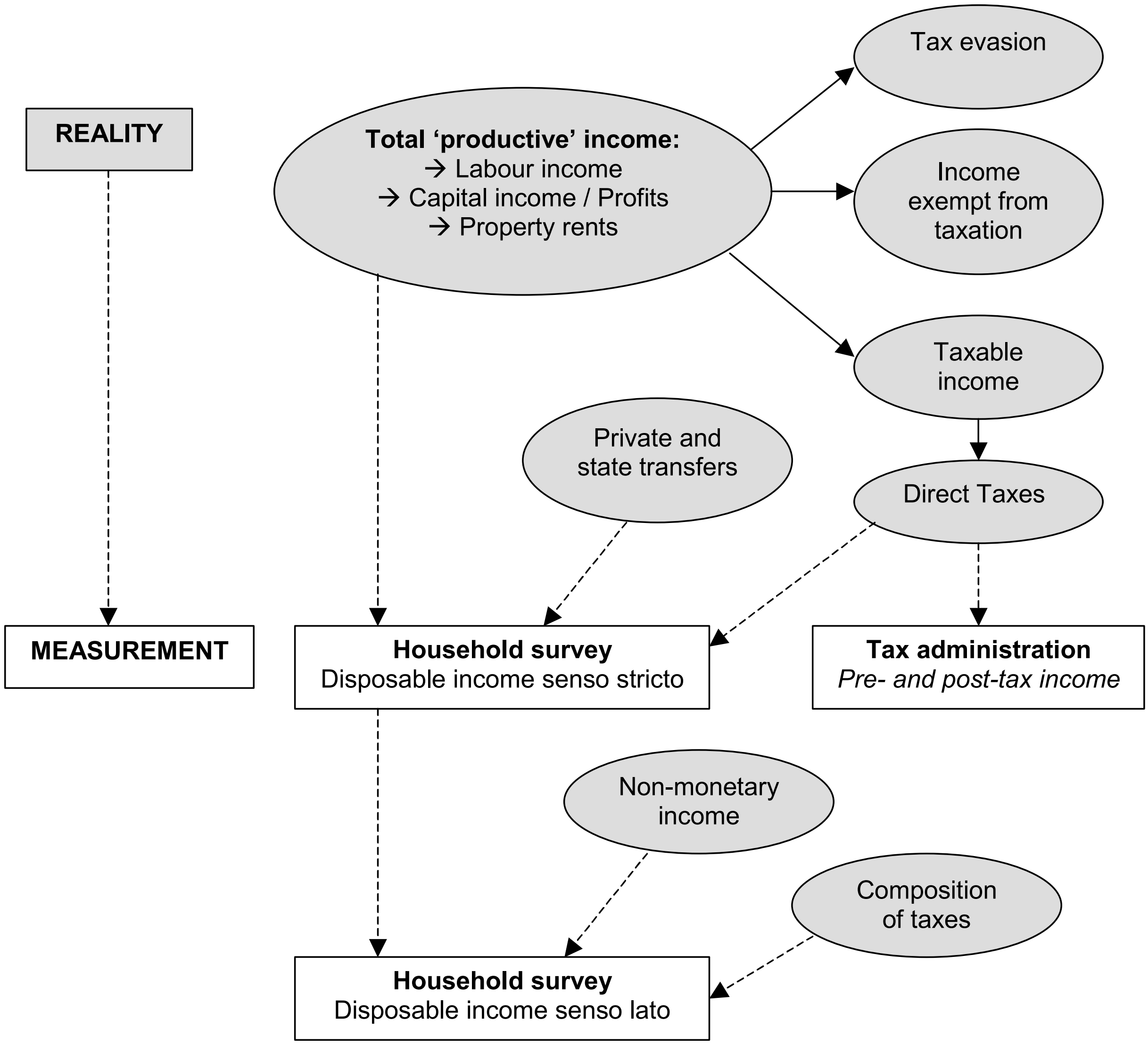


Table 1: The small sample bias in inequality measurement – a simulation exercise

PANEL A: Gini coefficient – Population Gini: 0.378				
	Mean	*St. Dev.*	*Minimum*	*Maximum*
Sample Size (%)				
50 (1%)	0.367	0.041	0.292	0.500
100 (2%)	0.367	0.025	0.282	0.438
250 (5%)	0.376	0.018	0.334	0.421
500 (10%)	0.375	0.012	0.351	0.422
1000 (20%)	0.377	0.008	0.359	0.398

PANEL B: Deciles ratio – Population Ratio: 5.933				
	Mean	*St. Dev.*	*Minimum*	*Maximum*
Sample Size (%)				
50 (1%)	5.975	1.279	3.459	9.256
100 (2%)	5.881	0.825	4.007	8.524
250 (5%)	6.038	0.668	4.472	8.175
500 (10%)	5.886	0.385	5.074	7.377
1000 (20%)	5.948	0.296	5.330	6.849

Table 2: Tax records versus household surveys

	Tax Records	***Household Income surveys***
Income concept	Depending on tax system Weak proxy for disposable income	Depending on questions Mainly labour income, but potentially a better proxy for disposable income
Reliability	Tax evasion (especially highest incomes) Low incomes are under-represented	Highest incomes are seldom correctly measured Dependent on time of year
Representativeness	Population with taxable income	Small sample of population Proximity of population census determines quality of extrapolations
Comparability	Countries: Low; very different tax systems across countries Time: Changes in tax systems over time can be considerable	Countries: Low; (very) different methodology across countries Time: Acceptable
Frequency	Annual. Long time series can be constructed	Mostly irregular, not annual. Only a few observations per country

Table 3: The impact of different equivalence scales on inequality measurement – a simulation exercise

PANEL A: Mean and standard deviation

	No correction	*Eq. Scale 1*	*Eq. Scale 2*	*Eq. Scale 3*
Gini				
Mean	0.372	0.345	0.351	0.342
Standard deviation	0.012	0.012	0.013	0.012
10th Decile/1st Decile				
Mean	6.691	5.303	5.127	5.026
Standard deviation	0.481	0.361	0.330	0.325

PANEL B: Correlations

	No correction	*Eq. Scale 1*	*Eq. Scale 2*	*Eq. Scale 3*
Gini				
Eq. Scale 2	0.904			
Eq. Scale 3	0.670	0.918		
Eq. Scale 4	0.791	0.974	0.974	
10th Decile/1st Decile				
Eq. Scale 2	0.824			
Eq. Scale 3	0.441	0.640		
Eq. Scale 4	0.636	0.819	0.833	

PANEL C: Differences in rank

	No correction	*Eq. Scale 1*	*Eq. Scale 2*	*Eq. Scale 3*
Gini	(Mean/St. Dev.)			
Eq. Scale 2	50 (43)			
Eq. Scale 3	92 (75)	45 (38)		
Eq. Scale 4	73 (62)	25 (22)	21 (18)	
10th Decile/1st Decile				
Eq. Scale 2	67 (58)			
Eq. Scale 3	125 (93)	98 (79)		
Eq. Scale 4	99 (79)	69 (59)	64 (56)	

Figure 2: Intersecting Lorenz curves

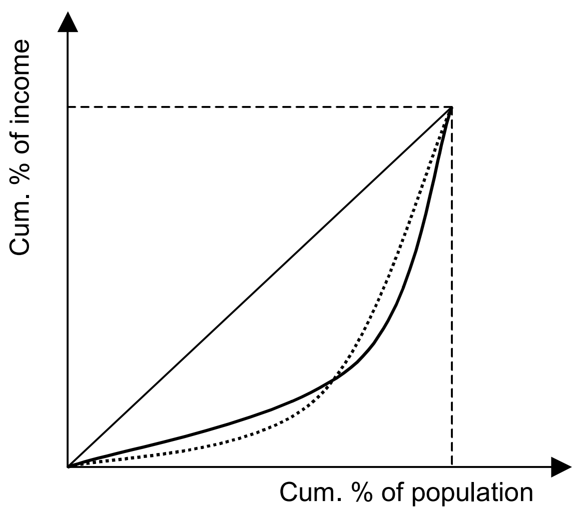


Table 4: Inequality measures and data sources in the inequality-growth literature

	Inequality measure	***Data source***
Alesina and Rodrik (1994)	1/ Gini for income distribution 2/ Gini for land distribution	Taylor and Hudson (1972); Jain (1975); Fields (1989)
Arjona *et al.* (2001)	1/ Gini for income distribution 2/ Mean log deviation of income 3/ Squared coefficient of variation 4/ Ratio of 10^{th} and 1^{st} decile	Förster (2000)
Banerjee and Duflo (2000)	Gini for income distribution	Deininger and Squire (1996)
Barro (1999)	Gini for income distribution	Deininger and Squire (1996)
Deininger and Squire (1998)	1/ Gini for income distribution 2/ Gini for land distribution	Deininger and Squire (1996)
Forbes (2000)	1/ Gini for income distribution 2/ Ratio of income share 5^{th} and sum of 1^{st} and 2^{nd} quintile 3/ Ratio of income share 5^{th} and 1^{st} quintile 4/ Income share 3^{rd} and 4^{th} quintile	Deininger and Squire (1996)
Perotti (1996)	1/ Income share of 3^{rd} and 4^{th} quintile 2/ Income share of 3^{rd} quintile	Perotti (1994)
Persson and Tabellini	1/ Income share of 3^{rd} quintile 2/ Income share of 5^{th} quintile	Lindert and Williamson (1985); Hartog and Veenbergen (1978); Jain (1975); USDC (1975); Flora *et al.* (1987); Paukert (1973)
Weede (1997)	1/ Gini for land ownership 2/ Income share of 5^{th} quintile 3/ Income share of 3^{rd} quintile 4/ Income share of 1^{st} and 2^{nd} quintile	Persson and Tabellini (1994); Alesina and Rodrik (1994)

Table 5: The impact of different inequality measures- a simulation exercise

PANEL A: Correlations

	Mean		St. deviation		Minimum		Maximum	
Sample	*Large*	*Small*	*Large*	*Small*	*Large*	*Small*	*Large*	*Small*
Gini – Atk(0.5)	0.998	0.989	0.001	0.004	0.996	0.975	0.999	0.995
Gini – Atk(5)	0.859	0.625	0.039	0.083	0.740	0.325	0.941	0.798
Gini – Deciles	0.959	0.726	0.011	0.065	0.923	0.555	0.978	0.925
Gini – HCI	0.134	0.138	0.142	0.153	-0.162	-0.289	0.427	0.484
Atk(0.5) – Atk(5)	0.858	0.637	0.038	0.080	0.737	0.371	0.939	0.806
Atk(0.5) – Deciles	0.954	0.706	0.013	0.077	0.912	0.480	0.974	0.939
Atk(0.5) – HCI	0.134	0.143	0.142	0.155	-0.163	-0.263	0.435	0.540
Atk(5) – Deciles	0.844	0.651	0.041	0.075	0.726	0.482	0.939	0.827
Atk(5) – HCI	0.123	0.215	0.141	0.132	-0.229	-0.069	0.403	0.538
Deciles – HCI	0.142	0.175	0.139	0.158	-0.158	-0.169	0.444	0.548

PANEL B : Spearman Rank Correlations

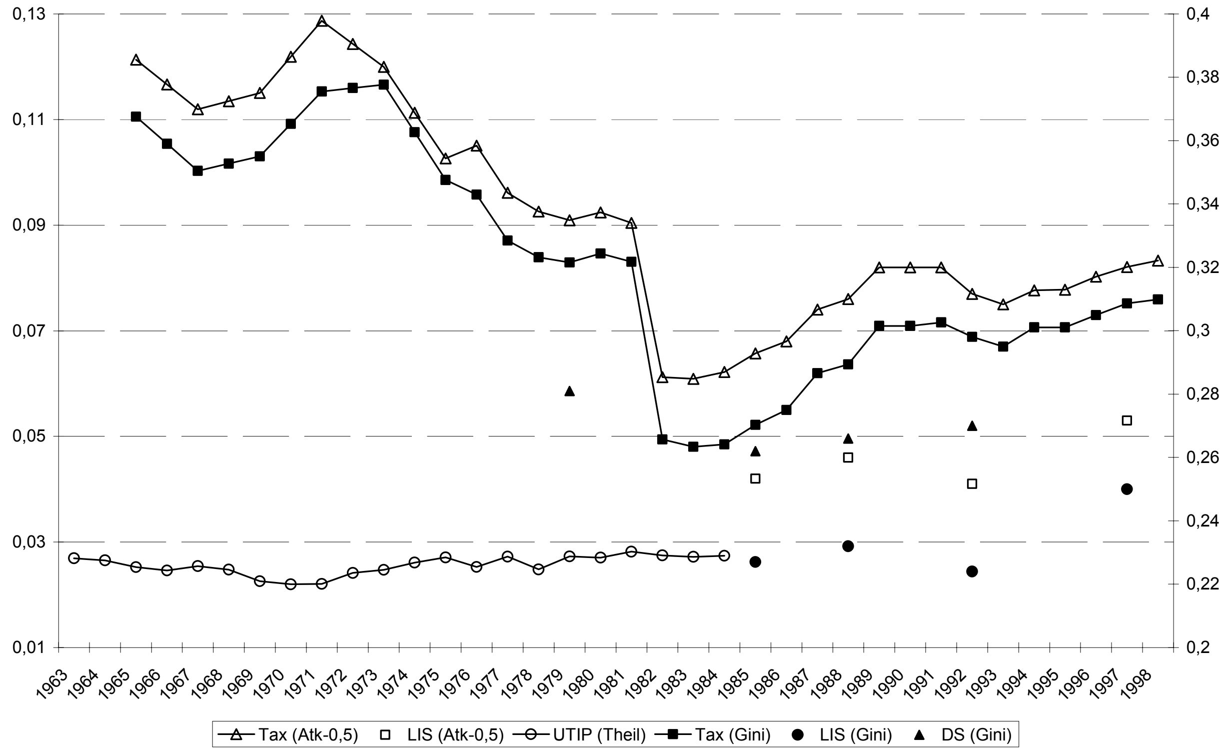
	Mean		St. deviation		Minimum		Maximum	
Sample	*Large*	*Small*	*Large*	*Small*	*Large*	*Small*	*Large*	*Small*
Gini – Atk(0.5)	0.997	0.992	0.001	0.003	0.990	0.982	0.999	0.997
Gini – Atk(5)	0.860	0.612	0.041	0.091	0.732	0.352	0.946	0.813
Gini – Deciles	0.958	0.736	0.014	0.065	0.909	0.555	0.978	0.899
Gini – HCI	0.283	0.275	0.139	0.095	-0.110	-0.289	0.572	0.451
Atk(0.5) – Atk(5)	0.863	0.639	0.041	0.087	0.736	0.371	0.947	0.834
Atk(0.5) – Deciles	0.952	0.724	0.016	0.066	0.900	0.480	0.977	0.900
Atk(0.5) – HCI	0.284	0.283	0.139	0.095	-0.128	-0.263	0.578	0.481
Atk(5) – Deciles	0.858	0.657	0.041	0.082	0.702	0.482	0.943	0.871
Atk(5) – HCI	0.304	0.379	0.133	0.079	-0.094	-0.069	0.620	0.525
Deciles – HCI	0.285	0.297	0.136	0.098	-0.111	-0.169	0.590	0.539

PANEL C : Descriptive Statistics

	Mean		Variance		Minimum		Maximum	
Sample	*Large*	*Small*	*Large*	*Small*	*Large*	*Small*	*Large*	*Small*
Gini	0.380	0.369	0.0009	0.0023	0.311	0.231	0.454	0.557
Atk(0.5)	0.116	0.112	0.0004	0.0009	0.077	0.042	0.169	0.260
Atk(5)	0.691	0.637	0.0042	0.0088	0.516	0.329	0.934	0.923
Deciles	6.112	6.189	0.9394	3.0885	4.217	2.615	8.762	18.446
HCI	0.036	0.036	0.0363	0.0034	0	0	0.297	0.360
			Standardized var.					
Gini			0.007	0.017				
Atk(0.5)			0.027	0.072				
Atk(5)			0.009	0.022				
Deciles			0.025	0.081				
HCI			2.202	2.670				

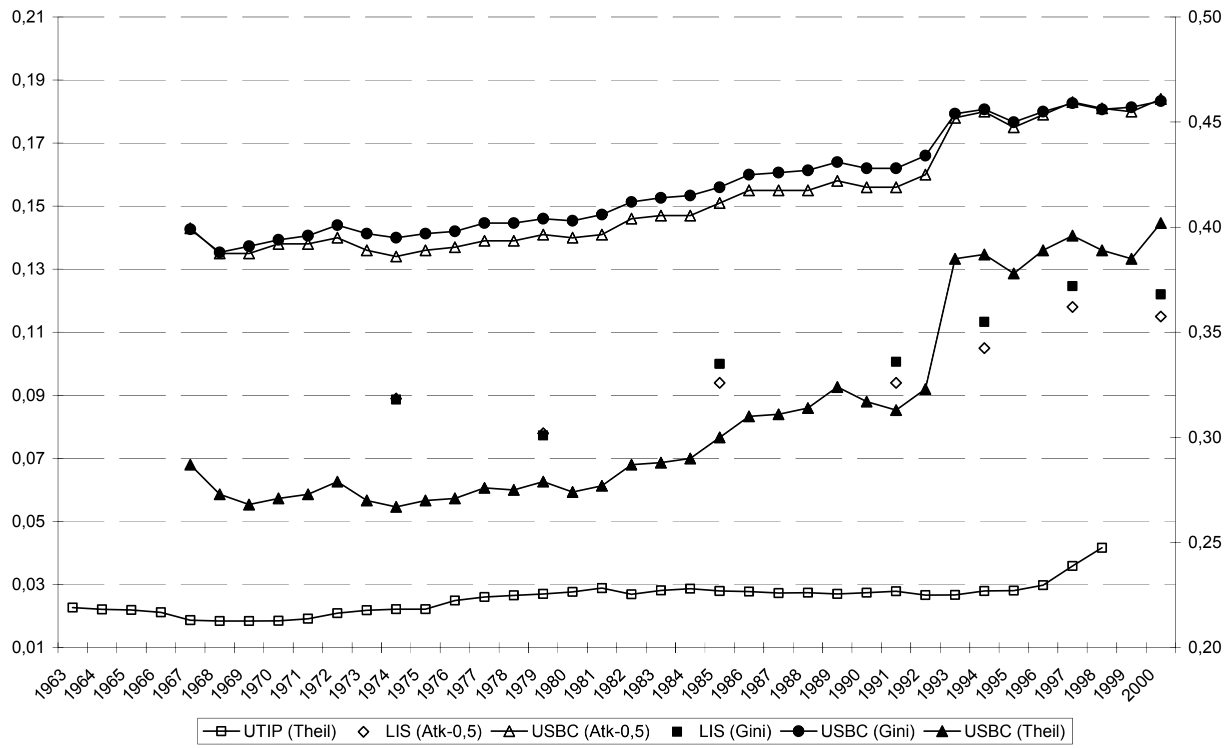
Note: *We rescale all measures by dividing them by their respective means (so the mean of the new series equals one). The standardized variance is the variance of these rescaled measures.*

Graph 1: Income inequality in Belgium – a look a different data sources



Note: Gini coefficients are measured on the right axis (full marks), all other measures on the left axis (hollow marks).

Graph 2: Income inequality in the USA – a look at different data sources



Note: Gini coefficients and the USBC Theil index are measured on the right axis (full marks), all other measures on the left axis (hollow marks).

Table 6: Data description

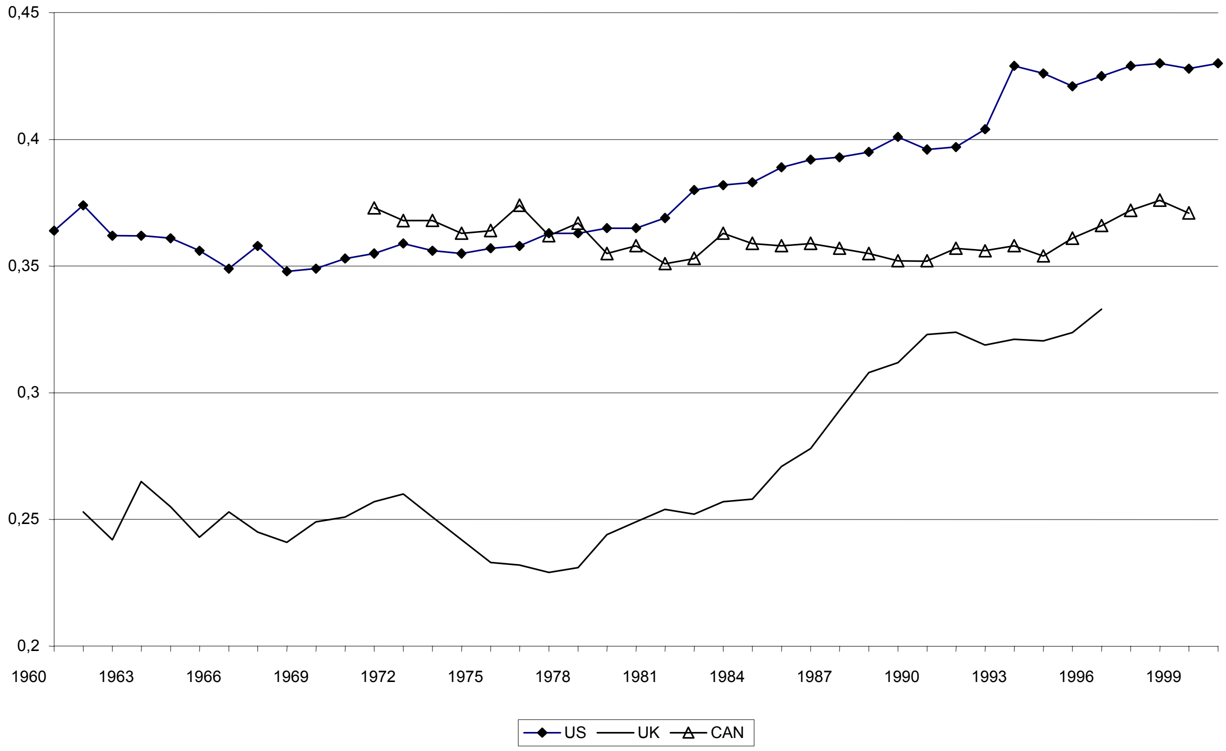
Country	Income concept	Source	Period
Included in dataset			
Belgium	Income before taxes Income after taxes	Valenduc – Tax data Valenduc – Tax data	1965 – 1998 4 years missing 1965 – 1998 4 years missing
Canada	Disposable Household Income	Composite series: Checci and Statistics Canada – Census	1971 – 1999
Finland	Disposable Income	Erikson & Jäntti – Tax data	1960 – 1992
France	Income before taxes	Thomas Piketty (2001) – Tax data	1919 – 1998
Italy	Disposable Household Income	Composite series: Brandolini, Banca d'Italia and SHIW – Census	1967 – 1995 4 years missing
The Netherlands	Disposable Household Income	CBS – Census	1975 – 1999 4 years missing
Sweden	Net Household Income	Atkinson et al, Spant and Statistics Sweden – Census	1960 – 1999
The UK	Net Household Income	Goodman & Webb – Tax data	1961 – 1996
The USA	Disposable Family Income Disposable Household Income	US Census Bureau – Census US Census Bureau – Census	1960 – 2000 1967 – 2000
Not included in dataset (time series too short, less than 25 years)			
Austria	Gross Earnings	Gusenleitner *et al.* (1996) – Social security records	1972 - 1991
Denmark	Personal disposable income	Pedersen – 2 overlapping series	1975 – 1995
Germany	Net Household Income	Grabka / GSOEP – Census	1984 – 1991 (BRD) 1992 – 1999
Japan	Gross Household Income	Mizoguchi, Takayama	1962 – 1982 1 year missing
Not included in dataset (No time series found)			
Australia, Greece, Ireland, New-Zealand, Norway, Portugal, Spain, Switzerland			

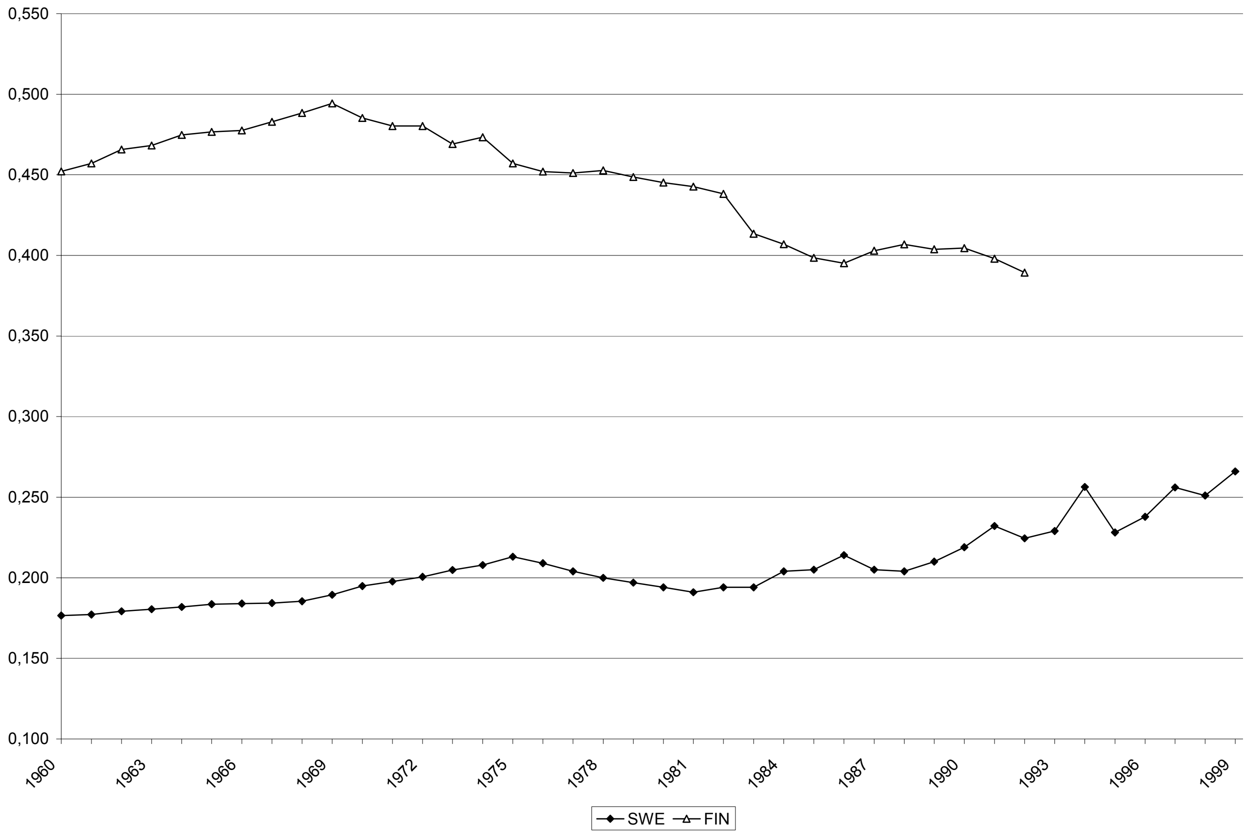
Table 7: Unit root test for income inequality (levels)

Country	ADF	ADF (constant, trend)	KPSS (no trend)	KPSS (trend)
Belgium *(after taxes)*	0.477		0.527**	0.140*
	Unit root is not rejected		*Trend stationarity only rejected at 10%*	
Belgium *(before taxes)*	0.131		0.496**	0.109
	Unit root is not rejected		*Trend stationarity not rejected*	
Canada	-0.048		0.180	
	Unit root is not rejected		*Level stationarity not rejected*	
Finland		-2.67**	0.629**	0.164**
	Reject unit root		*Reject (trend) stationarity*	
France	1.037		0.618**	0.195**
	Unit root is not rejected		*Reject (trend) stationarity*	
Italy	-0.013		0.458*	0.163**
	Unit root is not rejected		*Level stationarity only rejected at 10%*	
Netherlands	-0.924		0.485**	0.072
	Unit root is not rejected		*Trend stationarity not rejected*	
Sweden	-1.740*		0.782***	0.139*
	Unit root is only rejected at 10%		*Trend stationarity only rejected at 10%*	
UK	-1.632*		0.590**	0.139*
	Unit root is only rejected at 10%		*Trend stationarity only rejected at 10%*	
USA	-1.567		0.803***	0.214**
	Unit root is not rejected		*Reject (trend) stationarity*	

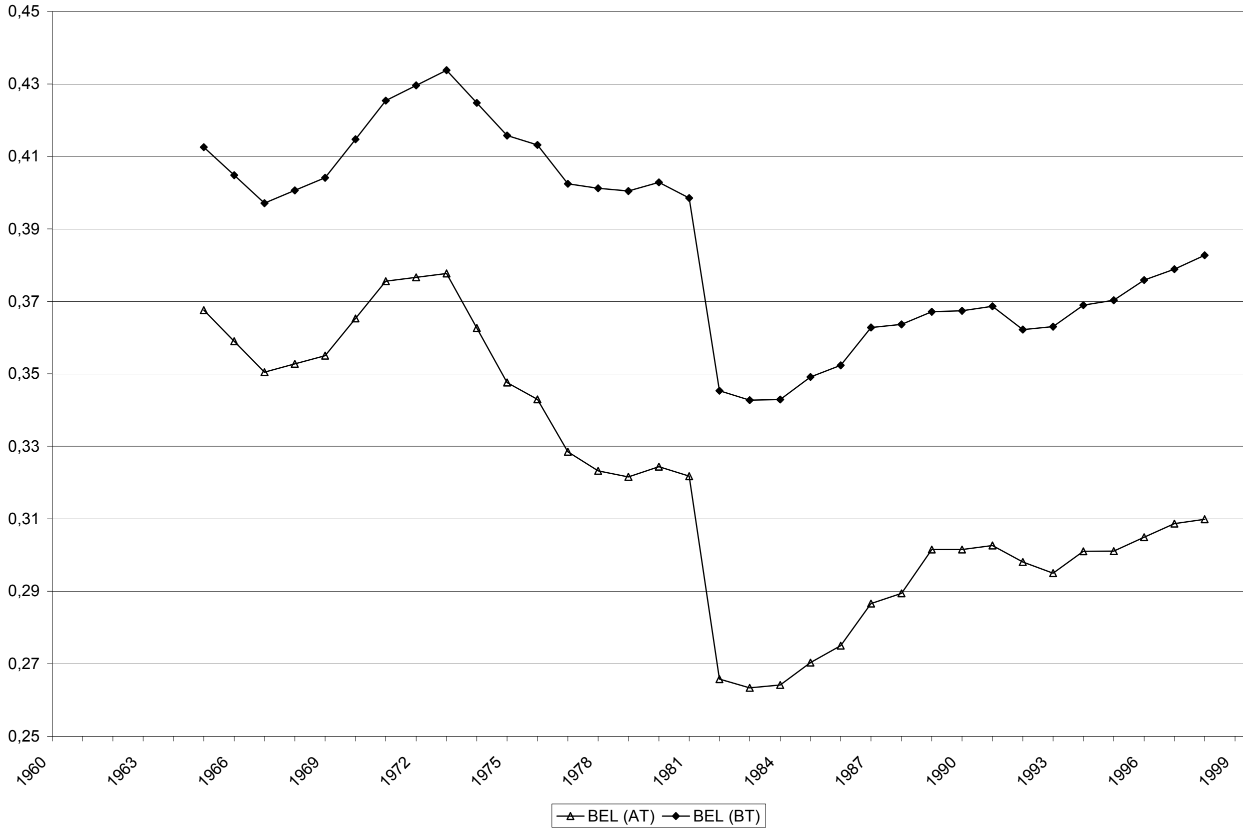
Note *Augmented Dickey Fuller test (ADF): null hypothesis is unit root*
Kwiatkowski Phillips Schmidt Sin Test (KPSS): null hypothesis is (trend) stationarity
** / ** / *** denotes rejection of null hypothesis at 10% / 5% / 1% level*

Graph 3: Income inequality in a number of OECD countries





Graph 3: Income inequality in a number of OECD countries (continued)



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