

Response to comments on “Country Inequality Rankings and Conversion Schemes”

Timm Bönke and Carsten Schröder (March 14, 2012)

First of all, we would like to thank an anonymous referee (“*Anonymous*”) for constructive comments, the readers for their interest in our work (until today, the paper was downloaded 136 times), and the editorial office for efficient handling of our submission. We think that the suggestions will help us to improve the paper, and we are confident that we can address them accurately.

First, we would like to comment on the *Critical Assessment of Anonymous*, followed by our comments on *Anonymous’ Short Report*.

Comments on *Critical Assessment of Anonymous*.

Summary of points 1-3. As emphasized by *Anonymous*, nine combinations of standard-of living variable¹ and population weights² are conceivable when measuring income inequality, and the aim of the current work is to “illustrate the consequences which arise from differences in the divisors of household incomes on one hand and in the weighting factors on the other.” Particularly, the work empirically investigates how switching two different population weights – household size vs. household needs (equivalence scales) – impact cross-country inequality rankings. In this respect, *Anonymous* misses references to research advocating a weighting of households by needs (i.e., by equivalence scales).

Response to points 1-3. The standard approach in inequality analyses is a two-step procedure. In a first step, equivalent incomes are derived by deflating the household incomes using equivalence scales. In a second step, a population weight, defined as the number of household members divided by overall population size, is assigned to each equivalent income. Accordingly, equivalent incomes are weighted by the number of persons in the households (size weighting).

Weighting equivalent incomes by equivalence scales (or a proportional factor) is not the standard method in applied research, but it may be viewed as superior from an axiomatic viewpoint. Respective references are provided in the introduction.³ With our work we seek to complement the debate on the adequacy of particular weighting procedures⁴ and axiomatic works on the properties of needs and size weighted equivalent income distributions with empirical evidence. To the best of our knowledge, such a systematic empirical comparison has not been provided in previous empirical research.

¹ Three candidates have been recommended: household income, per capita income, or equivalent income.

² Weighting by households, by individuals or by needs.

³ Axiomatic works include Ebert (1999, 2004) Ebert and Moyes (2003) and Shorrocks (2004). See the paper for exact references. The issue of “adding up” is a key aspect in the axiomatic work of Ebert and Moyes (2003).

⁴ See footnote 5 in the paper for references.

In a revised version of this manuscript we would highlight the originality of our empirical contribution and better motivate it with the axiomatic works by relating it to our empirical findings.

Summary of points 4 and 5. The Buhman et al. (1988) equivalence scale (ES) does not consider differences in needs of different age groups. Moreover, the selected equivalence scale parameters ($\theta = 0.5, \theta = 0.25$) are “out of the value range of wide-spread equivalence scales.”

Response to points 4 and 5. Numerous ESs have been recommended in previous literatures, and it is lively disputed which one should be used. With this in mind, we have chosen an equivalence scale which is well-known, frequently used, and flexible enough to systematically scrutinize the sensitivity of our findings to the supposed level household-size economies. Particularly, the whole range from absence to perfect household size economies is captured by variation of a single parameter, (θ). While considering needs differences across age groups is, of course, interesting and technically doable, its introduction would hamper a concise presentation of results.

We will better motivate our choice of the Buhman et al. (1988) equivalence scale in a revised version of the paper.

However, we disagree with the statement that the chosen scale parameters “are out of range”. First, results are presented for the whole range of household-size economies ($0 \leq \theta \leq 1$). Only for expositional reasons, the presentation focuses on two particular levels ($\theta = 0.5, \theta = 0.25$). Second, when $\theta = 0.5$ we have the well-known square-root scale which is recommended by the Luxembourg Income Study:

<http://www.lisdatacenter.org/data-access/key-figures/inequality-and-poverty/> , and can also be found at the OECD web page on equivalence scales:

http://www.oecd.org/LongAbstract/0,3425,en_2649_33933_35411112_119669_1_1_1,00.html

The square root scale is rather close to the OECD modified scale. The latter “assigns a value of 1 to the household head, of 0.5 to each additional adult member and of 0.3 to each child” (see OECD web page).

	Household type							
	A1C0	A1C1	A1C2	A1C3	A2C0	A2C1	A2C2	A2C3
OECD modified	1.0	1.3	1.6	1.9	1.5	1.8	2.1	2.4
Square root	1.0	1.4	1.7	2.0	1.4	1.7	2.0	2.2

Note. A denotes an adult and C a child. So, A2C1 denotes a household consisting of two adults and one child.

Comments on *Short Report of Anonymous.*

Summary of report. First, it is recommended to discuss the pros and cons of the two weighting schemes and also to provide the reader with our opinion in the conclusions. Second, notation should be optimized and some results should be discussed more deeply. Last, *Anonymous* misses a methodological contribution.

Response. We are happy to consider these recommendations in a revised version. Particularly, we believe that the paper has a methodological contribution: It provides a concise framework to study the research questions at hand, which builds on well-known methods in the field of inequality measurement and statistical inference.