

This paper contains useful remarks and clarifications on several indices proposed in the literature on interdistributional comparisons. The aim of the paper and its results are clearly explained.

I list below the main weak points that in my view discourage publication of the present version.

1) The paper rests on the axiom GDF (Group-specific Disadvantage Focus). However, the utility of this axiom is not explained. An example is provided considering two distributions with different concentration of rich and poor people, but this is a very peculiar case, while a solid axiom should be meaningful over the domain of all admissible couples of distributions. Suppose for instance two CDF which cross each other at each centile. In this case it is less evident why an index should satisfy the GDF Axiom. The link with the Roemer theory is quickly sketched, while the author could exert more ...effort in this direction. In fact, the GDF axiom contains two different recommendations, which (in my view) should be captured by two separated axioms:

- a) Accounting only for advantages of a group against another one
- b) Assessing these advantages at each quantile or percentile.

If a) can be motivated in several ways, b) requires additional (and independent) motivation (e.g. based on the Roemer ex-post EOp theory). Some measures (as those of the Dagum type) compare the outcome of each individual of a group with those of all individuals of another group. This procedure is consistent with the first idea, but not with the second one, so checking if these measures satisfy or not GDF is not a meaningful exercise.

2) The author should be more rigorous introducing his notation:

- He should properly define direct and inverse distribution functions.
- I quote from Wikipedia: "Cumulative density function is a self-contradictory phrase resulting from confusion between: probability density function, and cumulative distribution function. The two words cumulative and density contradict each other"
- More substantially, can the author provide a rigorous reference (or proof) for expression (1) in a general setting (including the discrete case)? Is it necessary to assume density functions in his analysis?

3) References: Two basic textbooks and two new articles concerning the main issue of the paper are omitted:

Chackravarty S. (1990), Ethical Social Index Numbers, Springer Verlag, Berlin.

Handcock, M, S. and M. Morris (1999), Relative Distribution Methods in the Social Sciences, New York, Springer.

Magdalou, B. and Nock, R. (2011), Income distributions and decomposable divergence measures. Journal of Economic Theory 146, p.2440-2454.

Le Breton et al. (2011), "A stochastic dominance approach to the measurement of discrimination". Forthcoming, Journal of Economic Theory.