Response to Pickett and Wilkinson (2015)

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Pickett and Wilkinson (2015b) advance two main critiques of my article (Rambotti, 2015): that I ignore much literature, and that I mistakenly believe that the measures of poverty and inequality are different. Before addressing these points, I emphasize that my criticism was not ill-intentioned. Indeed, I share P&W's concern for excessive income inequality, I commend their effort to attract general attention to this issue, and I pay homage to them with my title choice. The spirit (or bubble) level is an instrument: calibrating an instrument maintains (or improves) its accuracy, and is predicated upon the calibrated instrument having validity and usefulness. I thus welcome their attempt to recalibrate me.

Their first criticism can be safely rejected. I cited several reviews, including some referenced by P&W in their response (Kondo et al., 2009; Pickett & Wilkinson, 2015a). Indeed, there are more, but their reading still does not grant the hegemonic narrative of the field that P&W advance. In fact, scholars have found that the “relationship between income inequality and health is unclear” (Macinko, Shi, Starfield, & Wulu, 2003, p. 407), that “the published evidence so far is by no means conclusive” (Subramanian & Kawachi, 2004, p. 78), and that, net of partially supportive results, 30% of the remainder are wholly unsupportive (Wilkinson & Pickett, 2006), described subsequently as “a small proportion of negative findings” (Pickett & Wilkinson, 2015a, p. 318). Scholars provided important mechanism tests, but also highlighted that the findings are still not conclusive, and that the mechanisms themselves are not mutually exclusive (Elgar, 2010; Elgar & Aitken, 2011; Layte, 2012).

As for the second criticism, I refer the reader to my discussion of the relationship between income inequality and poverty (section 3), where I acknowledge that the two are “clearly dependent” (Rambotti, 2015, p. 125), while also distinguishing between headcount and gap measures that catch respectively breadth and depth of the income distribution. In section 4.2 I refer to the analysis of Kawachi and Kennedy (1997) on the choice of income inequality indicators. I invite the reader to compare Table 2 in their article (Kawachi & Kennedy, 1997, p. 1124) with Table 1 in mine (Rambotti, 2015, p.

126) to appreciate the difference in the magnitude of the correlation coefficients. P&W also suggest that previous multilevel analyses controlled the effect of individual incomes, thus representing a better alternative to my analysis. P&W here fail to recognize the collective nature of poverty as a social fact. This is highlighted, for instance, by the analyses that followed the experiment *Moving to Opportunity*, which show the positive effect of (low-poverty) neighborhood environment on health even *without* an increase of income and earnings (Chetty, Hendren, & Katz, 2015; Kling, Liebman, & Katz, 2007; Ludwig et al., 2011, 2012).

To conclude, I emphasize that none of the points raised here and in the original article are incompatible with the fact that excessive income inequality has adverse social effects, with the existence of social gradients, or with psychological explanations. However, as we look at the bigger picture and analyze it in its complexity, we should strive to avoid reductionism. There is an “enormous and well-developed body of social science literature” (Beckfield & Krieger, 2009, p. 153) that can help shed light on the structural side of this story, which until now has been too readily dismissed.

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