

Why should changing the bathwater have to harm the baby?

Peter A. Ubel · Dylan M. Smith

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There is a great deal of agreement among us and the three people who have written comments in response to our paper. For instance, we agree, to quote Sprangers and Schwartz, that the term “response shift” does not semantically “capture the phenomena it purports to describe” [1]. We also agree that “conceptual confusion surrounds response shift research that needs to be resolved,” and that, as Reeve states, the term is “misused” and “misunderstood” [2].

But we do not agree about whether keeping the term “response shift” does more harm than good. Both commentaries emphasize that the term is widely used now, thereby suggesting the significant cost of abandoning the term. No doubt, it is easier to jettison misleading terminology early in its history than later. But we contend that the ubiquity of this term is not an argument *against* removing the term, but instead strengthens our case that it is time to move beyond this language, in favor of clearer language and more discrete concepts. If no one was using

this term, it would not matter very much whether we abandoned it or not. The fact that it is in such widespread use, and is so confusing, is the reason we felt the need to write our original article. Words matter. Strong connotations (for example, that response shift is the same thing as scale recalibration) are not easily overcome.

Take for example Sprangers and Schwartz’s suggestion that the field “would be helped by explicitly distinguishing between recalibration response shift, reprioritization response shift and reconceptualization response shift.” We wholeheartedly agree about the need to distinguish among these three phenomena. But we cannot see why anyone would want to call all of them response shift. The term does not add any value to these concepts. Instead, it suggests that these concepts are more closely related than they really are.

Perhaps the ultimate reason we are splitters and our critics are lumpers is that our critics still believe that these three concepts, while distinguishable, nevertheless have one thing in common—they all represent threats to measurement validity. Sprangers and Schwartz state, for example, that “only if these three types of change have been accounted for or ruled out does a change in the common factor means reflect true change.” We disagree with this view. We believe that quality of life—and other broad measures like well-being, happiness and life satisfaction—are not necessarily invalidated when people change their life priorities in response to circumstance. Such reprioritization, in fact, is recognized in the psychology literature as a mechanism by which people adapt to circumstances.

The goal of quality of life research should be to develop measures that best reflect the quality of people’s lives, such that people reporting high quality of life should, in general, be better off than those reporting low quality of life. If

P. A. Ubel
Center for Behavioral and Decision Sciences in Medicine,
University of Michigan, Ann Arbor, MI, USA

P. A. Ubel (✉)
Department of Psychology, University of Michigan,
Ann Arbor, MI, USA
e-mail: paubel@umich.edu

P. A. Ubel
VA Health Services Research and Development Center of
Excellence, VA Ann Arbor Healthcare System,
Ann Arbor, MI, USA

D. M. Smith
Center for Medical Humanities, Compassionate Care, and
Bioethics, Department of Preventive Medicine, Stony Brook
University Medical Center, Stony Brook, NY, USA

people thrive in the face of adversity by changing their goals in life, this should not be seen as a threat to the validity of quality of life measurement, nor as “untrue” change. Instead, it should be understood as a mechanism by which people experience high quality of life in the face of adversity.

The lumping of three distinct phenomena under the rubric of response shift has impeded our ability to distinguish between true change and measurement error.

References

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