

FORUM ON “METHODOLOGICAL CRITIQUE ON CATEGORY SPECIFICITY”

COMMENTARY ON KEITH R. LAWS: “ILLUSIONS OF NORMALITY”

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In his paper Laws (2005) raises serious methodological concerns about the published evidence underpinning apparent category specific disorders. While the call for more rigorous, empirical testing of category effects is to be welcomed, some of the points raised appear somewhat contradictory. In his review, Dr Laws raises a number of contentious arguments:

1. Laws claims that, if materials are appropriately matched and ceiling effects are avoided, controls will tend to perform better at naming living things than nonliving things.

Laws argues, based on naming responses to the Snodgrass and Vanderwart line drawings, that nonliving things are structurally more similar than living things and this slows their responses down. This novel claim is in direct contrast to the established position on the structure of living and nonliving things. Laws' evidence for this claim is based on a single measure of similarity (the number of pixels shared across exemplars within the relevant category or domain). Other studies, using a variety of different similarity measures have consistently supported the view that living things are structurally more similar than nonliving things (e.g., Devlin et al., 1998; Gaffan and Heywood, 1993; Garrard et al., 2001; Greer et al., 2001; Lamberts and Shapiro, 2002; McCrae et al., 1997; Tyler et al., 2000).

2. It follows from (1) that, if a patient shows an equivalent level of performance on living and nonliving things on matched materials, they are, in fact showing a living things deficit relative to the “normal” pattern.

If, as Laws claims, the normal pattern of performance when materials are properly matched, is to be better at naming living things than non-living things, rather than reported category specific deficits for living things being spurious, such findings are likely to represent *underestimates* of the size of the relative deficit! In fact, even patients with no apparent difference in their naming performance for living things and non-living things should also be reclassified as having a living things deficit, since the normal pattern would be to be *better* on living things. Such an interpretation does

not, however, rest easily with the author's ‘doubts about whether published category-specific cases have documented the deficits that are claimed’. As is acknowledged in the paper, the vast majority of documented category specific deficits are in the direction of a disproportionate living things deficit. If Laws is correct about the pattern of normal behaviour, perhaps the number of reports of category specific deficits is even greater than is currently reported.

3. Patients reported to have a disproportionate deficit for nonliving things may simply reflect the ‘normal’ pattern of behaviour.

This extrapolation of the Laws position seems at odds with the literature. If this is the normal pattern, one would presumably expect deficits for nonliving things to be common, which, as Laws acknowledges, they are not.

We also question the emphasis that this review places on naming scores as measures of semantic deficits. Naming is only one estimate of category-specific performance and should not be the sole criterion on which category effects are to be judged. It is well established that patients may be able to retrieve semantic information about objects in the absence of naming success (see Plaut, 2002, for a review) and that the amount and quality of this information may differ according to the category from which an object is drawn and the nature of the task (Tyler and Moss, 1997; Moss et al., 1998). We have reported, for example, patients who appear to have a greater problem with living things than artefacts, but this is confined to the distinctive properties of living things. Their knowledge of the shared properties of living things was no different from their knowledge of the shared properties of artefacts (Moss et al., 1997, 1998). Unless a study takes into account the possibility of more subtle semantic deficits no firm conclusions can or should be drawn about category-specific deficits in general. While we agree with Laws that some published studies suffer through inadequate stimulus matching and/or insufficient control data, we would also like to emphasise that reported category effects will *always* be subject to the characteristics and parameters of a particular task

(i.e., stimulus characteristics and sample size, number of trials and presentation rate). It is thus critical to measure category effects across a wide range of carefully matched and controlled tasks, a point we have stressed elsewhere (Tyler and Moss, 2001; Tyler et al., 2003).

With respect to our own studies, Laws has been rather over-dismissive in suggesting that control data were not appropriately reported. His Table I suggests that our study of JBR (Bunn et al., 1998) used only within patient X^2 analyses to show that JBR had a living things deficit. However, this was not the case. We reported naming data for our colour photo set for both a young and elderly control group. Controls were not at ceiling, and we found a small but significant disadvantage for living things for a group of 40 young controls, and no significant difference for the older group. Although we did not explicitly carry out a statistical test to show that the JBR's 45% deficit for living things was statistically different to the 3% effect for the young controls, it is quite obvious that JBR's pattern is not in the normal range. We also reported that JBR's deficit fell out side of 2 standard deviations of the effect for older controls. Although we did not report control difference scores for living and non-living things in our later study of RC (Moss et al., 1998), we do show RC's naming scores for each category alongside the means for the control group, and is clear that his living things disadvantage is much greater than that of the controls, even though no formal statistical test is reported. Finally, in a third paper, reporting patient SE (Moss, et al., 1997) we did meet all of Laws' criteria for comparison to control groups. However, he dismisses this paper on the basis that we "produced naming data that were contradicted by an earlier study of the same patient". This earlier study was in fact his own (Laws et al., 1995). It is clearly puzzling that the same patient should show different naming performance in two studies, but this does not seem to be an adequate basis to dismiss our study as not having used control data appropriately. Within the context of our study, SE did show a larger deficit for naming living relative to non-living things which was greater than the maximum discrepancy for any individual control subject.

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