

A monochrome view of colour

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Abstract: Saunders & van Brakel's criticism of Berlin & Kay's methodology misunderstands the fact that scientific hypotheses are tested by generating new, replicable data with novel explanatory power. Thus, although Berlin and Kay studied differences in colour words *between* languages, the same patterns are also present in colour word usage *within* languages, in a range of literary and other textual databases.

Saunders & van Brakel (S&vB) have written a strongly argued criticism of the influential work of Berlin and Kay (1969). They have several criticisms, but they seem largely reducible to methodological failings of one sort or another. If in Berlin and Kay they find "an appearance of sloppiness," then it can only be said that in their own criticisms there is an appearance of nitpicking, and of the setting up of artificial standards, unrealistic and arbitrary hurdles that perhaps no study could ever meet. Worse still, such standards are perhaps irrelevant. Of course it would be better to study many more languages with hundreds of bilingual speakers, not one of whom was acculturated to the West (but is that possible if they are properly bilingual?). Surely these criticisms are close to becoming irrefutable? And of course there are a thousand confounding measures that Berlin and Kay did not take into account. To be flippant, it *might* be the case that the colour of the interviewer's socks altered the respondent's behaviour; and there were of course no controls for this. But do such criticisms mean we can discount the Berlin and Kay hypothesis entirely?

Somewhere, S&vB have misunderstood the nature of science. It creates *hypotheses* that may or may not be right or useful. Its test is in findings that replicate and are useful for predicting and explaining other unanticipated phenomena. And as such the Berlin and Kay hypothesis is undoubtedly extremely useful. Of course more extensive data would be nice, and it is therefore strange to find S&vB not mentioning the World Color Survey (WCS) of Kay et al. (1991).¹

The WCS looks at 25 speakers from each of 111 languages. Whether this will be sufficient for S&vB is not clear; one has a sneaking suspicion that it will not be, but it certainly appears methodologically sound. That the original Berlin and Kay position is genuinely a working hypothesis is shown in the WCS analyses by the discovery of some anomalies that require the original schema to be modified (although they hardly invalidate the basic formulation in any serious way).

Does the Berlin and Kay hypothesis provide insight into data remote from those on which the hypothesis was created? The original study says colour names are not mere categories but can be ordered, with each colour term having a number indicating its evolutionary antiquity, and white and black being older in some sense than orange and purple. Berlin and Kay derived their ordering from comparisons *between* languages, and said nothing about differences between colour words *within* languages. Differences in the use of colour words within a single language that correlate with the Berlin and Kay order therefore provide indirect support for the meaningfulness of that ordering. In 1983 I reported three sets of data on the frequency of colour words in English poetry, in English novels, and in Chinese poetry, in which there was a highly significant association with the Berlin and Kay order, older words being used more (McManus 1983). The older words also had longer entries in the Oxford English Dictionary, were listed more often in spontaneous colour word listings, and on the semantic differential had higher evaluation and activity, but not potency (McManus 1983). Previously, Hays et al. (1972) had reported similar colour word frequency effects in English, Spanish, French, German, Russian, Romanian, and Hebrew. Recently I (McManus, in press) have extended my earlier study by using eight large and very different computerised text bases (Biological Abstracts, Dissertation Abstracts, English Poetry, English Verse drama, GeoRef, MathSci, MedLine, and PsycLit). The 11 basic colour terms were used more

than half a million times, and again there was a highly significant correlation of frequency with the Berlin and Kay ordering (Spearman's $\rho = 0.802$; $n = 11$; $p = .003$), with no significant heterogeneity between sources. Such results suggest that the Berlin and Kay hypothesis satisfies the main criterion of being scientifically testable – it can generate new and testable hypotheses. Without Berlin and Kay it is difficult to produce any coherent explanation of such consistent differences that is not merely *ad hoc*.

Ultimately, the criticisms of S&vB have a familiar ring: they are those of the Standard Social Science Model (SSSM), so eloquently described by Tooby and Cosmides (1992) with its strong emphasis on the strength of social and cultural influences on psychological processes, and the denial of biological or neurological influences. That description also overlaps with the "radical cultural relativism" that Kay et al. (1991a) suggest afflicts so much cultural anthropology. As a result, perhaps the S&vB target article misses much of the potential excitement when both cultural *and* biological factors come into a dynamic, evolutionary interplay. To use an obvious but nevertheless appropriate metaphor, their analysis is disappointingly monochrome, perhaps even jaundiced, when full colour is so much more interesting.

NOTE

1. As an aside, there is here "an appearance of sloppiness" in S&vB, because although they mention the paper, albeit only as an aside, they cite it wrongly in their final manuscript; it actually appeared in the *Journal of Linguistic Anthropology*, not *Linguistic Anthropology*, and the proper title is "Biocultural implications of systems of colour naming" [my emphasis; S&vB's omission]. It is tempting to suggest that this a secondary citation, just as the criticism of the colour preference work looks secondary, in that Davidoff (1991) is cited, but not McManus et al. (1981), which specifically addresses the question about preference for hue, saturation or chroma, discussed only rhetorically by S&vB.

Over the rainbow: The classification of unique hues

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Abstract: Saunders & van Brakel's analysis of the phenomenal categorization and subsequent experimental research in unique hues fails to include contemporary methodological improvements. Alternative strategies are offered from the author's research that rely less on language and world knowledge and provide strong evidence for the general theoretical constructs of elemental hue, nonbasic, and basic color terms.

There is no doubt that one may find occasional anomalous results from the long history of color research. However, 300 years of experimentation have yielded *some* methodological progress. Unfortunately, in section 4.1, Saunders & van Brakel (S&vB) concentrate mostly on early work that had serious flaws and ignore the more recent methodological refinements for determining unique hues. One can certainly manufacture apparent disagreement among researchers with such a technique. And by accumulating results from many studies that use many (sometimes flawed) methods, one can amass what looks like a huge spectral range for each unique hue. But even S&vB are less than enthusiastic about such artifice, and ultimately return to their own private brand of folk psychology.

S&vB note that there have been a number of attempts to analyze the spectrum into constituent hues. After several paragraphs devoted to this issue, S&vB concede that this issue is "irrelevant" to questions about unique hues. However, their point about the unreliability of casual color naming is well taken. Color phenomenology, although useful as a starting point, must be refined by careful experimentation.

Overall, S&vB (sect. 4.2) give the impression that the methods used for determining unique (elemental) hues are rather haphaz-