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LETTER

Reply to Johnson and Wright

Johnson and Wright (hereafter J&W) claim, in their brief communication entitled "Reply to Philipona and O'Regan" that there are various difficulties in the reasoning in Philipona and O'Regan's (2006) paper entitled "Color naming, unique hues and hue cancellation predicted from singularities in reflection properties," which was published in this journal in 2006. These difficulties supposedly prevent our paper's conclusions from being accepted.

In fact, we find that J&W's arguments are based on a grave misunderstanding of the purpose of our paper, of the results that we present, and of some elementary mathematical concepts. They in no way undermine our conclusions.

Call u the triplet of cone quantum catch for the light that is incident on a surface, and v the triplet of cone quantum catch for the light that is reflected off that surface. Philipona and O'Regan (2006) present results from numerical calculations showing that:

1. each surface can be associated with one 3×3 matrix $\mathbf{A}$ such that the relation $v = \mathbf{A}\ u$ holds to a very high degree of accuracy for any natural illuminant,
2. the vast majority of such matrices associated with Munsell chips have three real eigenvalues,
3. Munsell chips that are most often given a name in the World Color Survey are chips whose associated matrices have a singular configuration of eigenvalues, as measured by a "singularity index."

The conclusion of the paper is that this striking coincidence lends credence to the idea that data about color naming derive from facts about natural lights, surface reflexion properties, and human photo pigments, rather than from facts about neural pathways or cortical representations.

In reply, J&W put forward three main arguments: they find unclear the empirical significance of our mathematical analysis, they question the precise definition of our "singularity index," and they claim that our work is lacking in quantitative analysis when it comes to the World Color Survey.

All these arguments show a profound misunderstanding of the purpose of the paper, since they are simply not the kind of arguments suited to undermine our purpose. Our purpose was to show that a quantity stemming from anthropological studies coincides with a physically defined quantity that we call the "singularity index." This is a surprising and significant fact that holds true whether or not the singularity index is somehow empirically intuitive (cf. paragraph 4 and paragraph 6 of J&W's "Mathematical analysis" section), and whether or not its precise definition might be questioned ("Singularity index" section). As J&W must surely have noticed, we ostensibly did not provide a mathematical theory to link the singularity index with the number of speakers in the WCS having a word to designate the color of a chip. The conclusion of the paper was merely to *lend credence* to the idea that such a theory exists, it cannot be criticized because it did not provide it. Furthermore, although we do not have such a theory, the idea surely seems intuitive that naming behavior should be linked to some kind of measure of the dimensionality of the cone catch triplets generated when surfaces are illuminated by different lights.

Not only have J&W misunderstood our purpose, but they have also misunderstood exactly what our results are. Most importantly, they seem to believe that we are appealing to singularities in the space of cone quantum catch triplets (see abstract, paragraph 1 and paragraph 6 of "Mathematical analysis" section), while this is precisely *not* what we do. As made clear in the summary above, our work points out singularities in a set of *reflexion properties* (i.e., in the set of matrices associated with Munsell chips). As a consequence, J&W's arguments simply miss the point. For instance, they claim that (paragraph 2 of "Mathematical analysis" section) "it might appear that [...] P&O are employing some familiar technique such as principal component analysis" on cone catch triplets, and then they go on to explain that we do not do it the right way. But obviously we are *not* doing a PCA, J&W are criticizing their own, peculiar, interpretation of our goal and work.

Going a step further in this error, J&W seem to have read our paper from the viewpoint that it is about coordinates and coordinate changes: they seem to think we are showing that singularities in the space of cone catches appear when expressed in the appropriate coordinate system—this error pervades their paper, and is to be found e.g., in the "Introduction," in Philipona and O'Regan paragraph 1, paragraph 6, paragraph 7, paragraph 8, of "Mathematical analysis," in the "Singularity index" section, and in paragraph 1 of the "Data from the WCS" section. But this is very much not the case, as should be clear from our rough summary above. In fact, one could even state that it is *precisely the opposite*, in the sense that a major motivation for our work was to discover properties in the system constituted by a set of lights, surfaces, and photo pigments, that are *not* dependant on a particular choice of coordinates in the space of cone catch triplets.

In paragraph 8 of their "Mathematical analysis" section, it seems that J&W do not appreciate that our results are empirical. That is, our approach predicts that four categories of colored surfaces should be distinguished because there turn out to be, as visible in our Figure 3, only four chips having a singular configuration of eigenvalues within the set of Munsell chips. This has nothing to do with the number of ways a triplet can have exactly one or two zero values. Had the reflexion properties of Munsell

chips been different, our approach might have predicted a different number of focal chips in the WCS.

In the same paragraph 8, J&W raise a question concerning rounding error. They seem not to have remembered that what we refer to as a singular Munsell chip is one for which the ratio of singularity values is strong *relative to other chips*. This is not rounding. This point is also related to J&W's valid questioning of the use of ratios rather than absolute values in the definition of our singularity index ("Singularity index" section). The fact is that it is the singularity index as we have defined it that correlates very precisely with WCS data. As noted earlier, we make no pretense of explaining this very troubling coincidence. It is this that a theory linking naming to reflection properties would have to explain.

Finally, with due respect, J&W appear to have misunderstood some elementary mathematical notions. For instance, it is nonsensical to speak of the dimension of a vector, as the authors seem to discover through their cake metaphor (paragraph 4 and paragraph 5 of the "Mathematical analysis" section). One can only speak of the dimension of a vector *set*. It is also nonsensical to suggest that dimensionality depends on the coordinate system used to represent vectors (paragraph 1 and paragraph 6). In our paper, when speaking of the dimensionality associated with a colored chip, we are speaking of the dimensionality of the set of cone catch triplets for the light reflected by the chip under various natural illuminants. This is *independent* of any choice of coordinates.

In the same vein, what is the relevance of J&W's noting that symmetric matrices have real eigenvalues (paragraph 2 and paragraph 3 of the "Mathematical analysis" section), since this is well known to be only a *sufficient* condition, not a *necessary* one? The fact is: the matrices associated with Munsell chips are *not* symmetric—as rightly noticed by J&W, they have no reason to be so—yet almost all have real eigenvalues.

To summarize, it is our position that J&W have brought no valid argument against our conclusion.

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