



Author response

Defining synaesthesia: A response to two excellent commentaries

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This paper is an author response to two commentaries on 'Defining Synaesthesia' (Simner, 2012) by David M. Eagleman ('Synesthesia in its protean guises', 2012), and Roi Cohen Kadosh and Devin B. Terhune ('Redefining synaesthesia?', 2012). Together with these authors, I seek to more closely examine existing criteria on which definitions of synaesthesia have been based. In particular, I focus on the fact (a) that existing definitions paint synaesthesia as an overly homogenous condition, (b) synaesthesia may have multiple neurological causes, and (c) synaesthetes and non-synaesthetes may lie on a continuum.

In their two fascinating reviews, David M. Eagleman, Roi Cohen Kadosh, and Devin B. Terhune shine a light on the very delicate process of establishing a definition of synaesthesia, following a similar attempt of my own in an earlier paper ('Defining Synaesthesia', Simner, 2012). In their commentaries, these authors supplement, and sometimes challenge, the ideas I proposed, but overall provide a series of important further considerations while being kind enough to broadly support my goals. These commentaries remind us, in a skillful way, that difficulties arise in defining synaesthesia not only because it has a very large range of manifestations, but also because it may have more than one neurological cause, as well as a large degree of genetic heterogeneity. Moreover, the condition of synaesthesia may represent a continuum rather than a bounded category, along which synaesthetes gradually merge into non-synaesthetes. I very much welcome these reviews, which share my overall aims, and which highlight the slippery series of stepping stones that must be navigated when attempting to establish a definition of this fascinating condition.

The opinions from my own paper and these two commentaries converge on the view that a number of accepted definitional criteria for synaesthesia should be re-evaluated. We are in agreement that certain features found in subsets of synaesthetes should not be erroneously attributed to *all* synaesthetes as defining features of the condition. Hence, we agree that synaesthesia may resist a definition based on consistency (that synaesthetic

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pairings must necessarily be consistent over time) or spatial extension (that synaesthetic colours and other sensations must have a specific location in space). We also agree that a definition of synaesthesia as ‘a merging of the senses’ is mis-informative, since there are both sensory and non-sensory variants of the condition – these latter being those involving higher level constructs, such as words, thoughts, personality traits, emotional states, and so on. Cohen Kadosh and Terhune also suggest that synaesthesias triggered by ‘cultural tools’ (p. 20) such as words and letters might arise at different stages in development, and our own work (Simner, Harrold, Creed, Monro, & Foulkes, 2009) reflects this by showing that synaesthetically coloured letters develop on a trajectory that parallels literacy acquisition in young children, for example. Cohen Kadosh and Terhune also make the important point that a better understanding of synaesthesia may come from a consideration of precisely those variants that were omitted from my opinion piece – acquired or temporary forms – and I agree with them in their estimation that these forms have great scientific value. On a related point all three commentators suggest that different types of synaesthetic experience may result from ‘different mechanisms such as disinhibition . . . excessive connectivity . . . or lack of cortical specialization’ (Cohen Kadosh and Terhune, 2012, p. 20). In this way, they argue against a homogenous phenomenon occurring from a single mechanism, and I am mindful to address this in more detail below.

I previously suggested that synaesthesia might be classified by its neurological cause or mechanism, and my commentators rightly point out that we should be speaking in the plural – that the condition is likely to be underpinned by not one, but several neurological processes – and these may be working in parallel within any given synaesthete, or individually across synaesthetes. These possible mechanisms include ‘several fundamentally different neural processes (e.g., neuronal overgrowth, underpruning, imbalanced inhibition and excitation) all of which happen to converge on the similar result of unusual perceptual or cognitive pairings’ (Eagleman, 2012; p.18). In my own paper, I attempted to be neutral in this regard by using the term ‘hyper-associative mechanism’ to represent *one of any number of* possible neurological processes that might give rise to the ‘open channel’ between different brain regions, which allows sound to be interpreted as colour, taste as touch, touch as smell, and so on. In fact, this neutral term should cover not *one* of several possible mechanisms, but rather, *one or more* of these possibilities, and I am grateful to my commentators for pointing this out. Whether a functional connection is established by hyper-connectivity, by disinhibited pathways, by other means, or indeed, a combination of these, the outcome is the opening of a communication between regions that would otherwise not directly interact to produce a conscious experience in the average person. If these mechanisms can be classified together in this way, under this functional rubric, then we may indeed seek to use a neurological basis to explore the outer limits of the condition. Ultimately, however, we may be talking ourselves into a corner. If neurological classifications guide our definition of synaesthesia, and if these mechanisms are numerous (or greater than one in any case), then we may only be able to unite them by reference to their functional consequence, in which case, we may be flirting, once again, with behavioural classifications. Perhaps our aim here is not to draw a clear boundary around the term ‘synaesthete’, but rather, to explore the very limits of what we *do* understand. Since we know, for example, that at least some synaesthetes are characterized by hyper-connectivity in white matter pathways (Rouw & Scholte, 2007), we might then explore the consequences of finding this type of hyper-connectivity in less familiar regions. (In my original piece, I raise the possibility of a hypothetical ‘verbal synaesthete’ with

hyper-connectivity in, say, fronto-temporal language regions where semantics, lexical forms, and syntax are mediated. He or she might manifest this 'synaesthesia' as unusually strengthened connections in language processing, being able to make automatic and extraordinary connections between words; see also Sinha, 2010 for other examples.) In taking this approach, we cannot guarantee that we will include all synaesthetes, given the possibility of multiple mechanisms, but we would certainly discover new variants that might otherwise go unnoticed. What is clear, ultimately, is that any attempt to define the condition in neurological terms must acknowledge the possibility of multiple mechanisms, and might come to full fruition only at a point where we have a better understanding of these.

This leads to a second, related point, made by Eagleman, which I am glad to acknowledge. My exploration of synaesthesia in neurological terms tacitly assumed that the communication between regions is all or nothing – that synaesthetes have this type of atypical 'cross-talk' while non-synaesthetes do not. David Eagleman rightly points out that the correct way to view this may in fact be on a continuum. He presents the stark possibility that any 'goal of achieving a clear definition for synesthesia may never be realized. This is because hyper-connectivity is not all-or-none; it's spectral' (p. 17). However, before we throw out the entire supposition, we should check the bathwater for the baby. A useful definition of synaesthesia in neurological terms could recognize this spectral quality by imposing a threshold cut-off, as we do in studies that behaviourally identify synaesthetes. For example, in behavioural tests we often do not find a clear bi-modal distribution that separates synaesthetes from controls. These tests are still nonetheless performed, and a conservative threshold is imposed to separate the most highly typical synaesthetes from the remainder. There is a key drawback in this approach, which I have acknowledged previously, and which my commentators echo, which is that we may be placing an inappropriately tight restriction on whom we do include, and this is the risk we run. Alternatively, in more positive terms, we might yet explore ways to unite both neurological and behavioural measures, and this could give clues as to how best to establish a cut-off point for synaesthesia in neurological terms. I elaborate on this below.

Synaesthetes and non-synaesthetes appear to lie on a continuum, both with respect to their neurological characteristics and in certain aspects of their cross-modal behaviour. Specifically, Eagleman has pointed out that neurological mechanisms such as hyper-connectivity are spectral, and we know elsewhere that both synaesthetes and non-synaesthetes make similar systematic cross-modal pairings in behaviour tasks. For example, both synaesthetes and non-synaesthetes match colour and sound systematically, in that higher pitch sound pairs with lighter colours, either intuitively for non-synaesthetes, or explicitly in the conscious photisms of synaesthetes (Ward, Huckstep, & Tsakanikos, 2006). However, there is one behavioural feature of synaesthesia which is apparently absolute. This draws a clear line in the sand between synaesthetes and non-synaesthetes, and is based on the criterion of conscious awareness, as mentioned above. We tap this in our studies by asking 'Have you ever (i.e., before this testing date) believed that letters had colours (or that words had tastes etc.)' Only synaesthetes will answer yes, since this is one of the few, very clear defining features of the condition. In other words, while non-synaesthetes are able to *generate* letter-colour combinations on demand (and may even tend to share their preferences from one person to the next; Simner *et al.*, 2005) they have not previously been aware of these associations and do not entertain them in daily life at a conscious level. In contrast, synaesthetes have, and

they do. Given this, I would argue that despite the behavioural continuum of cross-modality which merges synaesthetes with non-synaesthetes (and which may or may not be reflected in degrees of connectivity), this continuum has a definite central point based around conscious experience, with synaesthetes on one side and non-synaesthetes on the other. Furthermore, we might use this feature to reflect back on the accompanying neurological patterns, to ask whether this behavioural characteristic might inform us about the usefulness of a neurological criterion. Put more clearly, if neurological cross-talk is spectral, and if it is in any sense functionally correlated, future advances might allow us explore the point at which that hyper-association comes to give rise to *conscious experiences* of an associated synaesthesia.

I end by pointing out that, even if we do not achieve a goal of using neurological characteristics to draw a firm, highly inclusive, appropriately exclusive, boundary around all synaesthetes and *only* around synaesthetes, the spirit of my suggestion is rather one of exploration. The notion to convey is that a criterion based on synaesthetic neurological mechanism(s) might allow us to discover cases of synaesthesia we might otherwise not have considered. Where we find strong and statistically atypical connectivity (for example) in regions not usually associated with synaesthesia (e.g., centres of high-order reasoning) we might come to entertain the resultant behaviour as a possible synaesthetic variant, and one that might otherwise have escaped our attention. This approach might show that synaesthetic sensations derive not only from sounds, touch, tastes, words, and so on, but also from more unexpected sources, such as the act of decision making, or very fine-grain motor movements, or navigating social interactions, and so on – all of which have been suggested to me by synaesthetic individuals at various points in my research over the last 10 years. In exploring the limits of this unusual condition, and in correcting previous attempts to define the condition, I am happy that my commentators have been kind enough to broadly support my goals, and that they have raised important clarifications where needed. I am happy, also, that the debate on these issues has been opened.

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