

Synesthesia and Method

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COMMENTARY ON: Cytowic, R. E. (1995) [Synesthesia: Phenomenology and Neuropsychology](#) *PSYCHE*, 2(10).

ABSTRACT: Richard Cytowic has done considerable service to the scientific study of synesthesia, conducting important research and publishing two recent books on the subject. The study of synesthesia raises interesting questions about scientific method, both because of the negative reception it received initially--often being viewed as tainted by a reliance upon introspective reports--and because of the connections Cytowic has found between synesthetic perception and the limbic system, thereby possibly undermining some of the claims to objectivity in perception and scientific method. I dispute some of the more extreme methodological conclusions Cytowic draws from his work and reinforce others by reference to different arguments current within the philosophy of science.

1. Introduction

1.1 Richard Cytowic's two recent books on synesthesia, his textbook *Synesthesia: A Union of the Senses* (1989) and his popular *The Man Who Tasted Shapes* (1993), have done much to raise the level of awareness of synesthesia. Cytowic's treatment can be seen as a challenge to the academic community to acknowledge the existence of synesthesia and to deal with its implications for medical and scientific practice and for our understanding of the nature of sensation. I agree with him that the nature of synesthesia can help to focus concerns about these matters and that the issues raised need careful treatment by scientists and philosophers.

1.2 Cytowic goes on to find that synesthesia has great import for all kinds of issues, including many matters methodological, and especially that it undermines the worship of objectivity widely practised within our culture. Here I shall focus on these claimed implications for methodology and epistemology. The topics that I comment upon below have been selected in the first instance by Cytowic; in particular, he claims, either explicitly or by implication, that an understanding of synesthesia: argues for a greater role of emotionality (section 3), tacit versus explicit knowledge (section 4) and subjectivity (section 5) in our intellectual or scientific lives; better explains language learning (section 7); and demonstrates that impossibility of artificial intelligence (section 6). I do not attempt to unpack Cytowic's arguments on these themes in detail, as they are spread over two books and various articles. I do attempt to indicate the main lines of argument. Whereas I am sympathetic to many of Cytowic's *conclusions* (excepting those about artificial intelligence (AI) and language acquisition), I do not find that he has generally made good cases for them; instead, the best arguments for a methodological role for subjective experience are quite different from those suggested by Cytowic's reflections upon synesthesia. In particular, recent discussions within philosophy of science have reached many of the same conclusions by way of clearer and more compelling arguments, as I shall indicate below.

2. The Reality of Synesthesia

2.1 The first published case of synesthesia appears to have been John Locke's (1690) report of a blind man who "bragged one day that he now understood what *scarlet* signified.... It was like the sound of a trumpet". Locke's case may not have been what I will be referring to as synesthesia, but it does bring out one important feature: the association of sensations across sensory modalities. As Cytowic has well argued, though, synesthesia proper should be understood as an *involuntary* cross-modal association, triggered by some range of sensory events in one modality. Furthermore, such involuntary associations appear not to be acquired (cf. Cytowic, this issue, section 2.1) and may have a genetic basis (1989, pp. 56-60 and pp. 232-235; <1> Baron-Cohen et al., 1993, supports all of these points as well).

2.2 In contrast with this understanding of synesthesia, the term has often been used to refer to nothing more than a manifestation of people's general ability to find inspiration in one mode of sensation for understanding experience in another. Such cross-modal associations are quite common; indeed, they are constantly revealed in the metaphorical content of ordinary language--for example, when we call some music 'warm,' or a spicy drink 'sharp.' Synesthesia, as it has recently been revealed experimentally, goes well beyond such associations, for they are relatively ephemeral links dependent upon time and place, whereas synesthetic links from, say, a taste to a shape are enduring and invariant across a wide range of contexts. The synesthete's cross-modal relations are involuntary--they cannot be turned off--unlike our ordinary abilities to cross-associate music and color or taste and touch. The abnormal character of synesthesia is sufficiently demonstrated by the ability of synesthetes to report nearly identical associations after the lapse of one year (achieving a 92% accuracy rate), versus a matched control group's

inability to obtain a similar result after only one week (achieving a 38% accuracy rate, for a difference corresponding to a significance level of $p < 0.001$; Baron-Cohen, et al., 1993)--or, in the case of a single individual, identical associations were reported after ten years (Devereaux, 1966). A variety of other experiments point to the same result: Cytowic has recorded the cerebral blood flow of a synesthete during his experiencing of intense synesthesia, revealing an abnormal pattern of brain activity (Cytowic, 1989, pp. 120-127; happily, studies of blood flow among synesthetes are being continued by Baron-Cohen's research group using PET scans, see Paulesu, et al., submitted); Cytowic has uncovered characteristic semantic differences in the way synesthetes associate cross-modally (1989, chapter 6); and, in general, synesthetes report a significantly different phenomenology from the ordinary, and their reports are consistent with each other even though they have been generated independently (this statement is consistent with recent reports on synesthesia covering dozens of synesthetes; Cytowic, 1989, reports on 42 cases). Thus, for example, although the precise linkages between cross-modal sensations that synesthetes have may vary greatly (whether they are from this sound to that color or from this tactile sensation to another color), they uniformly report having always (to their knowledge) had the cross-modal links that they currently have (e.g., Baron-Cohen et al., 1993, p. 423) and that these particular links have not changed over time.

2.3 The idea that such first-person reports can be ignored as inherently unreliable, and so the experience of synesthesia can be denied, is one that Cytowic has had to contend with throughout his work on synesthesia (1993, chapters 5-10). Such scepticism may arise from behaviorist methodological doubts about the merit of first-person reports generally or, alternatively, from the possibility of interpreting the reports as caused by something other than synesthesia. I shall describe a non-behaviorist methodology that takes first-person accounts seriously below (section 5). As for the second concern, it seems unlikely that an alternative cause of the reports will be forthcoming, in light of the evidence sketched above. In the absence of synesthesia, the probability seems small indeed that any significant number of people would not merely independently report invariant cross-modal associations but be able to reproduce the details of the reports at will from one to ten years later. This would apparently involve either some quite devious cheating or remarkable feats of memory. To be sure, synesthesia is associated with abnormally good memories (1993, pp. 129ff), but one would also have to posit abnormal motivations, since there is no apparent gain in store for synesthetes. In short, then, however strange it may seem to non-synesthetes, synesthesia is real, and Cytowic's arguments about its methodological bearing deserve to be taken seriously.

3. Emotion versus Reason

3.1 As Cytowic notes, Plato and Socrates viewed emotion and reason as in a kind of struggle, one in which it was vitally important for reason to win out. Aristotle took a more moderate view, that both emotion and reason are integral parts of a complex human soul--a theory proposed by Aristotle in explicit opposition to Platonism (*De Anima* 414a 19ff). Cytowic appears to endorse the Platonic line, with the notable difference that he would apparently rather have emotion win out.

3.2 Cytowic adduces considerable evidence that the limbic system is implicated in synesthesia, including a shift in brain metabolism during synesthetic experience away from the cortex and toward the limbic system (see Cytowic this issue, section 6). This connection between synesthesia and the limbic suggests to Cytowic that the traditional emphasis on objectivity and rationality in Western culture--presumably manifestations of cortical function--is misplaced or overdone (perhaps because Cytowic sees synesthesia as underlying such crucial and general phenomena as language acquisition--see section 7 below). The main line of reasoning Cytowic is apparently relying upon runs: the limbic system is heavily interconnected with the rest of the brain, including the cortex; the limbic is involved in human emotions; therefore, it is reasonable to conclude that the limbic and emotion are involved in all human reasoning.<2> More purely theoretical considerations surely will have made a similar conclusion unavoidable already. Emotions are clearly connected to our system of drives, which affects everything we undertake, including our thought processes. Emotions also quite clearly serve to highlight some features of the environment while suppressing others. Therefore, what we come to notice, choose to think about, involve in our planning and become habituated to is all conditioned by our emotional state (see de Sousa, 1987, chapter 7). This suggests that emotion and thought, so far from being in *opposition* to one another--as they are traditionally portrayed--are constantly sustaining one another, the one's turn feeding the other's twist. This, at any rate, is my thought, and so too that Cytowic's belabored and overblown support for the primacy of emotion at the *expense* of thought or reason is as much foolishness as the more traditional glorification of Pure Reason.<3>

3.3 Another road to the same conclusion can be found by considering that no one has ever managed to produce any all-encompassing characterization of reason *per se* that has satisfied more than a handful of people. The best available explanation for that failure is that there is no such thing as 'absolute' or 'pure' reason. The most that we appear capable of doing is to identify rationality *relative* to some particular set of goals--i.e., we can produce an incremental conception of rationality that changes as goals are added or dropped (cf. Giere, 1988, chapter 1). If rationality thus consists of finding effective means to achieve a set of goals, then such rationality is not to be had without due regard for the emotional structure that plays such a large role in setting up and caring for our goals. A rationality without emotions, and so without goals, is not so much inhuman (as in the Spock of *Star Trek*) as it is impossible. And so a rationality defined by *contrast* with emotion is empty. Aristotle surely holds the more defensible ground. (For some more recent efforts aimed at integrating emotion and reason see, in addition to de Sousa: Johnson, 1987; Mandler, 1984; Oatley and Johnson-Laird, 1987; Oatley, 1992.)

3.4 I am not objecting to Cytowic's eulogizing of emotion merely out of a joy of criticism. In granting to his opponents that there is some great gulf between reason/thought/language on the one hand and emotion on the other Cytowic is granting far too much. The fact is that Cytowic's enemies here are real, they are numerous, and they are powerful. They are also radically confused. But much of the support they may have won over the years depends upon just this exaggeration of the divide between reason and emotion, leading for example to the thought that language and reason can be

construed as formal processes that are axiomatizable and that achieving that axiomatization would be to achieve an artificial intelligence in which emotion plays no part. Similarly, some philosophers have thought that the development of philosophical logics will resolve the outstanding problems of philosophy, that epistemic logic will resolve our concerns about epistemology, or that a logic of confirmation will answer all of our questions about theory evaluation. What such studies are more likely to produce, however, are codifications of previously and independently fixed opinions about epistemology and scientific method. This is not to say that formal studies are not important: I believe that they have a central role in both AI and philosophy--they are, however, only vital as *tools*, not vital as theories.

4. Tacit Knowledge

4.1 Cytowic argues that much of what we know is and must remain tacit, that is to say unavailable to our processes of articulation. This is of a piece with his view that language is not the be all and end all of cognition. Cytowic takes a first step towards this conclusion by noting that in split-brain experiments there is evidence that the right hemisphere can know facts which it is unable to express verbally, while being fully capable of expressing the knowledge by other means, for example by hand movements (1993, p. 17).[4](#) The conclusion that we can have a practical inability to express some of our knowledge is inevitable. More than that, this suggests (without demonstrating) that right-hemispherical cognition, in the form of problem solving, can be quite sophisticated without the use of ordinary language, since linguistic processing in most people is very strongly dominated by the left hemisphere. Such a conclusion would be consistent with granting that (non-human) animal problem solving, etc. is reasonably considered cognition.

4.2 Cytowic goes on to make a very much stronger claim: that much of our knowledge is ineffable. That is, that we know things which are not just impossible to express given some physical handicap or limitation, but which are just impossible to express. Cytowic takes synesthetic experience to be an example of that. Thus, he points out the difficulties that synesthetes report in their attempts to explain their conditions: "No matter whether they sent five-page letters, sketches, or paintings that tried to capture their sensations, they uniformly apologized that the materials could not even begin to convey what their experiences were 'really like.'" (1993, p. 118) This notion that synesthetic experience is ineffable, just as the notion that the qualia (sensory constituents of experience) which we all have are likewise ineffable, while tempting, is wrong. First, it needs to be noted that there is no interesting difference in the expressibility of ordinary qualia and synesthetic qualia--or, at any rate, none has yet been claimed. That is, what we (non-synesthetes) do not have is a hard-wired (involuntary) connection between qualia that synesthetes do; but what the connection connects--two qualia of different modalities--these we do have, independently of each other. Since these two modal features of experience are simultaneous in the synesthete, perhaps they jointly make up a single quale which non-synesthetes will never have; we nevertheless experience *components* of such experience, and there is no difficulty in our imagination to putting these components together. In

other words, we share all that we need to share in order to understand the phenomenologies of synesthetes: we have the same vocabulary, if you like, of primary sensations.<5> The difficulty some have found with accepting synesthesia as a real condition lies not with an inability to understand the proposed phenomenology, but perhaps does lie in a refusal to take any subjective reports seriously, much as some doctors disastrously refuse to believe patients when they report pain.

4.3 This does not so far imply that the qualia of synesthesia are after all expressible: independently of any consideration about synesthesia, many have claimed that qualia of any kind are inherently ineffable--i.e., whereas we have predicates such as 'appears red' or 'is hungry', sentences employing them could never catch the precise qualia that I have when I have a red sensation or when I am hungry. Such ineffability of experience, if real, would go some way toward showing that our experiential knowledge of the world is not formalizable or representable in any (known) language. Qualia, however, are not ineffable: what is implicitly being demanded of language here is more than is sensible.

4.4 Consider the assertion that an object is red. To normal speakers it conveys something about their potential sensations should they be exposed to the object under various environmental conditions. To color-blind speakers insensitive to red it conveys less about such sensations, since it says nothing about brightness; it does convey information about how most people may react to the object under various conditions. To blind people the assertion carries even less information; they will be in a worse position for predicting future events than others, but better than had they heard nothing about the color of the object. Asserting not that the object is red, but instead that one has a sensation of red conveys something different but related; under fairly ordinary circumstances the hearer will be able to infer that there is a red object at hand inducing the red sensation and thus be in a position to make the kinds of inferences indicated above about the red object. So certainly *something* about the qualia is communicable to each of these kinds of interlocutors; just what is communicated depends upon the degree to which phenomenologies are shared. It is not at all unreasonable to say that the qualia have been expressed to all three interlocutors, although varying amounts of information have been communicated. The idea that it may be or ought to be possible to capture *precisely* the feel or sensation X in some linguistic expression--or else we should declare that X is ineffable--is apparently just the thought that what is expressible is that which *both* the originator and the recipient *experience*. But in that case nothing is expressible, for language is powerless to force the experiences of two people to be identical. Language can no more carry my qualia to you than it can carry around buckets of water. Demanding *experiential identity* as a precondition to granting that the experience be expressible at all is demanding something non-sensical. Regardless of such spurious demands, anyone who experiences hunger or red sensations knows "perfectly well" what I'm talking about when I refer to such of my own sensations. So long as two agents are sharing a language they must be sharing their phenomenologies to some degree or other--in an informational sense of sharing.<6> What may finally be ineffable is not worth talking about.

4.5 I will agree with Cytowic, and many others, that there is indeed such a thing as tacit knowledge which is not (in humans at the very least) directly available to conscious linguistic processing, and presumably is not recorded in a declarative form.<7> Such an observation does not depend upon any fact about synesthesia. It has been forcefully made by many philosophers previously, notably in Michael Polanyi's epistemology: "We can know more than we can tell and we can tell nothing without relying on our awareness of things we may not be able to tell" (Polanyi, 1964, p. x). Other notable defenders of tacit knowledge are Hubert and Stuart Dreyfus who have particularly emphasized the role of non-linguistic skills in their critique of artificial intelligence (Dreyfus and Dreyfus, 1986). Their arguments support the idea that consciously available linguistic representations cannot by themselves capture human phenomenology. Unfortunately the argument does not really carry very much further in the current debates of cognitive science. Although it *suggests* that artificial systems will not be able to produce qualia via linguistic processing alone, it does not formally imply such a result: *our* linguistic processing and qualia being distinct does not imply that *everyone's* linguistic processing and qualia are distinct. Furthermore, this conclusion does not even rid us of Fodor's language of thought hypothesis (Fodor, 1987)--that all cognition occurs using some form of low-level, non-conscious language--for, while granting that qualia enter into cognition, the Fodorian language employed in non-conscious reasoning must be distinct from any ordinary, consciously available language and so may yet be sufficient, in the right neural context, for the existence of qualia. I do not believe in such stories, but they do not appear to be ruled out by the existence of knowledge which remains tacit vis-a-vis ordinary language.

5. Objective Methodology

5.1 The question whether scientific method is or can be objective has been one of the main battlegrounds of the philosophy of science throughout its existence. It would be impossible even to sketch the history of those battles here, but I will toward the end give a hint or two about why the pure objectivists have mostly abandoned the battlefield. In this case again Cytowic appears to be on the winning side, in opposition to objectivism (which is not to say on the side of relativists, constructivists or other obscurantists--there are more than two sides here), but he has found his way to that side by quite unordinary routes, and apparently with no awareness of the more travelled roads. The unordinary routes Cytowic has followed lead through the terrain we have just been viewing: the ineffability of subjective (synesthetic) experience, the crucial role of emotionality in behavior and cognition, and the implausibility of escape from these issues by denying the reality and importance (at least for synesthetes) of synesthesia (or, for the rest of us, of subjective experience generally).

5.2 The denial of the relevance of subjective reports to scientific results has played a major role in twentieth-century science. Cytowic appears to attribute it to the rise of psychophysics (1993, p. 58), but such an attribution would be a mistake.<8> Psychophysics was originated by Gustav Fechner (1860) as a method for the study of

sensation: in particular, Fechner used the method of "just [subjectively!] noticeable differences" for developing quantitative laws of sensation. So far from ignoring subjective reports of sensation, Fechner's methods *crucially depended* upon such reports. It would be more accurate to lay the blame for this particular distortion of scientific method upon behaviorism, admittedly a school of thought that evolved out of psychophysics (but that is a trait shared by most of modern psychology), or else upon logical positivism, which provided intellectual aid and comfort to behaviorism. The way in which positivism supported the behaviorist denial of the value of subjective reports is through its (early) verificationist criterion of meaningfulness. According to that criterion, anything meaningful has to be verifiable, at least in principle, by the examination of direct experience. By that criterion the subjective reports of *others* cannot be taken at face-value, for they cannot be verified by any operation available to us (or better: available to *me*; who cares about you?). Therefore, the reports of, for example, the introspective psychologists active in the early century (e.g., Edward Titchener) are best thought of as the meaningless mumblings of the deeply confused. What psychology, or any science, should be doing is gathering up reports of publicly observable phenomena and generating and testing theories to explain those phenomena. Psychology, in particular, is properly constrained to theories of behavior *alone* and must eschew metaphysical speculations about some wild fantasy world called consciousness.

5.3 Such dogma has been surprisingly influential. No doubt much of the reason for its influence is that its leading proponents in the early and mid-century, John Watson and B.F. Skinner, were remarkably effective propagandists. Regardless of their successes, hard-core behaviorism is not coherent with scientific method in general. The mental world that subjective reports are apparently reports about can be simply ignored only if we are prepared to deny that verbal behavior is a kind of behavior. For if we accept verbal behavior as a kind of behavior, quite as legitimately an object of study as the pecking behavior of pigeons, then we must be prepared also to tolerate, indeed support, hypotheses about why one form of verbal behavior is seen in humans and not others. That is, the subjective reports of humans about their mental life--including beliefs, desires, forgetfulness, qualia and all the rest--must be explained in terms of some theoretical model that can predict the behaviors in question. Skinner had the idea that purely empirical laws could do this trick (Skinner, 1957), laws which introduce no theoretical terms (whether mental entities or genetic factors).[9](#) Skinner was not notably successful in that venture (see, for example, Chomsky, 1959). But, aside from Skinnerian distaste for the mental, there could be no reason to refuse to employ theoretical terms in psychological theories that would not apply equally well to other scientific theories[10](#)--as would, for example, the sceptical stance of extreme empiricism towards theoretical entities *generally*. Mental entities, then, and the theories in which they participate should be treated as on a par with, say, subatomic particles and the theories in which they participate. The latter are introduced because they help enormously with our making sense of experimental physics. But, similarly, postulated mental states such as beliefs and desires help us, if not enormously, then substantially more than nothing in making sense of human behavior, including the experimental results of psychology and especially recent cognitive psychology, as any survey text in the field reveals, such as Glass and Holyoak (1986). The point then is that quite ordinary views of scientific method demand

that we take into account the data supplied by reports of subjective experience--what Dennett (1991) calls our heterophenomenologies, meaning our reports of our phenomenologies to others.<11> Their dismissal would leave as some kind of cosmic coincidence--entirely inexplicable--the fact that the vast majority of humans will report having similar beliefs under appropriately similar circumstances, etc.<12> The ignoring of subjective reports is not some kind of victory for objective science, it is only the backwash of an intolerantly narrow vision of science, now rapidly draining away.

5.4 A completely different concern about subjective reports than simply that they are about things that we cannot (as third persons) observe directly is the consideration that introspective reports show some tendency to be systematically misleading. Thus, if one were to bias first-year psychology undergraduates by using all powers of authority to persuade an experimental group that, say, the concept of rhinoceroses is sexually arousing, then introspective reports about their thinking of the subject of rhinoceroses would likely differ from those of a control group. More remarkable perhaps are our powers to confabulate, to simply make up out of whole cloth the most ridiculous stories to cover over a lack of information--as in Korsakoff's syndrome when memory-impaired patients will answer questions about events they cannot remember apparently because they believe they can remember (cf. Adams and Victor, 1989, p. 824). Although the more bizarre confabulations involve serious brain damage, it is not unreasonable to suppose that they draw upon inventive or rationalizing processes that exist in all of us. So, such stories rightfully give rise to scepticism about the content of subjective reports. And, indeed, there is considerable evidence that subjective reports about mental states are systematically misleading in the more mundane settings (cf. Nisbett and Wilson, 1977). None of that, however, implies that subjective reports are not reports, that data are not data: the facts that people can confabulate, and do so under memory impairment, are facts that need explaining every bit as much as the fact that people do not so confabulate when their memories are unimpaired. What we must conclude is that the heterophenomenological data are rich, intricate and dangerous.

5.5 Cytowic is surely right that the subjective reports of synesthetes must be taken seriously. They need hardly be taken as the truth *uncritically*: as I suggested in section 2, the fact that synesthetes report such commonality in the nature of their synesthetic experiences as they do can plausibly be explained only if their reports are reports of a common synesthetic condition. This is, or ought to be, uncontroversial. But Cytowic goes on to draw grandiose conclusions about the nature of science, society, and spirituality, the main theme of which is that objectivity is something of a charade and subjectivity ineluctable (views arrived at largely by way of the synesthetic connection with emotion via the limbic system outlined in section 3.3 above; see especially Cytowic 1993, part II). Cytowic goes too far in denouncing objectivity, I believe. Furthermore, whereas the fact that the limbic system is implicated in synesthesia is relevant to this debate over objectivity, it is at best a very small part of the argument. For the notion that overmuch has been made of objectivity appears to be undeniable on other grounds. The out-moded notion that there is nothing subjective about scientific method is out-moded precisely because there already is a whole range of compelling arguments against it. For some examples: Russell Hanson (1958) points out that how we identify and individuate objects,

and thus form observations, is dependent upon our background knowledge and subjective belief about what theories of the phenomena are true; Dudley Shapere (1982) and Ian Hacking (1983) point out that the border between what we call observational and what we call theoretical is not just hazy but also in motion, so that our theories can hardly be founded upon some indubitable bedrock of reports couched in a pure observation language in any case; Thomas Kuhn (1962) and Imre Lakatos (1970) point out that scientific theories rise and fall not purely as a matter of comparing them with data, but largely due to subjective appraisals of how productive an entire research tradition has been; Paul Feyerabend (1975) has emphasized many of the difficulties of comparing theories across research traditions and the advantages of science simultaneously supporting competing traditions; Richard Rudner (1956) and Michael Scriven (1974) make clear that value judgements are an integral part of theory evaluation in science; <13> Peter Galison (1987) and Allan Franklin (1986) make clear that value judgements are an integral part of experimental procedure. And these are just a few among dozens of other recent and influential arguments made within the philosophy of science leading to the conclusion that a purely objective conception of science is nothing more than a pipe-dream.

5.6 It is undeniable that subjective concerns are interwoven into scientific practice. The subjectivity of human, scientific practice is the very motive for the many scientific practices that are designed specifically to minimize them--such as double-blind experimental techniques, the use of statistics, recording measurements, etc. And there is nothing wrong with that minimization either: allowing experimentalists to report whatever suited their fancy would be no advance in scientific method. But the glorifying of objectivity to the point of no longer being able to recognize the influence of subjectivity is a radical mistake.

6. The Status of Artificial Intelligence

6.1 The glorification of objectivity to the point of being unable to recognize any role for subjectivity is surely just the kind of mistake *some* researchers in artificial intelligence have made--and Cytowic understandably responds to this by attacking AI as one of the many heads of the dragon-beast of objectivity. In his discussion of artificial intelligence (1993, pp. 197-201) Cytowic makes the mistake--shared by many of those same AI researchers--of thinking that artificial intelligence lives or dies with the notion that all of human cognition and consciousness can be "objectified," meaning by this that it can all be captured by providing a computer system with a theorem prover and then pouring in some huge amount of propositional "knowledge" and letting it crank away (this appears, in fact, to be much of the rationale behind Doug Lenat's CYC project--an AI project aiming at the production of an enCYClopedic AI system; Lenat, et al., 1990). Thus, Cytowic avers (1993, p. 204): 'AI argues that we really never "understand" something until we can break it down into formal logic statements.' But logicism, this view within AI that that is all that is needed for an artificial intelligence, while highly influential, has never achieved that exclusive grip on the field that its believers no doubt would have liked; indeed, far from being identifiable with artificial intelligence, logicism is distinctly

on the wane, beset on all sides by research programs that do not share its mystical vision of logic predominant--including connectionist neural networks (Rumelhart and McClelland, 1986, and McClelland and Rumelhart, 1986), evolutionary algorithms (Holland, 1992), and Bayesian artificial intelligence (Pearl, 1988). (For a critique of logicism, see Korb, 1995b.)

6.2 Cytowic's arguments against the possibility of artificial intelligence *per se*--especially that AI fails to take emotion or qualia into account--are therefore misdirected, as they do not cut against many of the most important currents of AI research today. Emotionality, specifically, is an aspect of cognition that has in fact been explored by some computationalists as a necessary ingredient for cognition (see Oatley and Johnson-Laird, 1987; in my own work I am investigating the role of salience, an ingredient of emotionality, in the formation of concepts in Korb and Thompson, 1994).<14> The defence of artificial intelligence is not at all along the lines that Cytowic suggests, namely that emotions are not necessary and that qualia are also 'nice but unnecessary' (1993, p. 197). The more plausible defence is that emotions and qualia, if in fact necessary for an artificial intelligence, can be represented, and if not linguistically, then non-linguistically. The only limit to AI representations that need be acknowledged is that those representations, and operations performed upon them, must be computable. And that is not obviously any kind of severe restriction. It is not, for example, tantamount to a restriction to a logical language (nor to von Neumann machines, inasmuch as neural networks are computationally equivalent; cf. Franklin and Garzon, 1991). Real-numbered variables can be used, subject to round-off errors, and indeed are crucial to all three of the competitors to logicism I named above. Heuristic and indeterministic programming are freely available to AI (indeed, chaotically generated pseudo-random numbers may be used rather than ordinary pseudo-random numbers, if that is desirable; Wallace, 1990). If, finally, the restriction to what is computable turns out to be too severe to achieve an artificial intelligence, then of course AI must fail. But no one has made that case; for example, no one has (even!) demonstrated, as would be necessary, that human cognition in fact relies upon any non-computable process. (Note that it would not be enough here to show that the human brain encompasses non-computable processes but also that human cognition is *dependent* upon those processes. For an *attempt* in that direction see Penrose 1989, 1994; the latter has attracted [extensive commentary in PSYCHE.](#))

6.3 It is certainly true, as Cytowic remarks, that 'winning at poker involves more than knowing the rules' (1993, p. 204); perhaps Cytowic (and Dreyfus, whose misguided complaint Cytowic is here echoing; see Korb, 1995a) will be surprised to learn that I *have* developed a Bayesian AI program that wins at poker. Of course, it does 'know' a good deal more than the rules of poker.

7. The Origins of Language

7.1 I offer a final example of the tendency of Cytowic to find implications that apparently are not there to be found, one that concerns an important issue, and so is worth raising

doubts about. This is Cytowic's claim that language depends upon cross-modal associations (1993, p. 122):

Language would probably never have evolved without humans first being able to form the kinds of cross-modal associations present in synesthesia. This assertion goes back to the discussion we had of cross-modal associations in monkeys, who are unable to associate two non-limbic senses. Humans can do this, and it is this capacity that underlies the ability to assign names to objects and proceed to higher and higher levels of mental abstraction.

7.2 Every statement here is disputable. The idea that monkeys cannot associate a visual image of an experimenter with a correlated sharp (startling) sound when any rat can do as much is nonsense. I also have doubts that cross-modal associations of any kind are strictly necessary for language. In his 1989 text Cytowic raises this issue only to counter those who have thought that the cross-modal associations of synesthesia might be dependent upon language; but to deny the dependency in one direction it is not necessary to claim it in the other. Nevertheless, Cytowic quotes Geschwind as establishing this dependency of language on cross-modal association:

The ability to acquire speech has as a prerequisite the ability to form cross modal associations. In sub-human forms, the only readily established sensory-sensory associations are those between a non-limbic (i.e. visual, tactile or auditory) stimulus and a limbic stimulus. It is only in man that associations between two non-limbic stimuli are readily formed and it is this ability which underlies the learning of names of objects. (Geschwind, 1964, p. 155)

7.3 No doubt it is true that in humans cross-modal associations precede the acquisition of language; indeed, the ability to draw such associations undoubtedly aids in conceptualization directly. However, the case for such early cross-modal associations being synesthetic has not been made (see Baron-Cohen, this issue, for a discussion). Were that case to be made, it would still not follow that synesthetic associations are *necessary* for language acquisition in humans. And even were they shown to be necessary for humans, it would not follow that they are necessary in general, which finally is what Cytowic is claiming. This last inference from the single case of Homo Sapiens to the universal generalization across all possible species of linguistic agent is a mind-boggling induction, one that ought at least to be approached cautiously. The artificial intelligence programs I know of that aim to do concept formation--a first wobbly step towards automated language--do not deal in more than one sensory modality. This may in fact be a serious weakness; perhaps they would do far better if they accommodated multiple modalities. Nonetheless, in their limited fashion, they do in fact succeed in generating useful concepts (see, for example, chapter 3 of Shavlik and Diettrich, 1990).

8. Conclusion

8.1 Synesthesia is certainly a fascinating condition. It appears very likely that much about perceptual processes generally can be learned from it, and perhaps an understanding of it can shed light on such questions as the nature of qualia and consciousness, although no one has yet suggested how. But Cytowic has not demonstrated that any substantial methodological or epistemological discoveries are in store; he has demonstrated instead an over-eagerness to draw conclusions without first adducing relevant evidence. I readily

acknowledge, however, as Karl Popper always emphasized, science is not just a critical enterprise where ideas get shot down; a readiness to conjecture bold and unexpected ideas is a necessary ingredient for the progress of science, and Cytowic's speculative remarks have clearly stimulated a number of useful lines of inquiry.

8.2 Of greater concern than speculative excess is Cytowic's assertion of the primacy of emotion and subjectivity over reason and objectivity. Although our culture has a history of overindulging a love of Reason, it is not at all clear that an equal and opposite overindulgence of Unreason is the proper remedy. Indeed, many have launched head-long into just such an over-reaction, especially in the social sciences and humanities, not merely glorifying the role of subjectivity in science, but denying any possibility of objective underpinnings for science (especially the Edinburgh "strong programme" in the sociology of science). It is arguable that the greater danger now to the future of science comes from the Irrationalists rather than the Rationalists. But I view both parties as equally appalling sources of authoritarianism and intolerance, and I hope that Aristotelian moderation may ultimately win out.

Notes

<1> References unadorned by author will be to Cytowic throughout.

<2> Cytowic goes so far as to say "we are irrational creatures by design..." (this issue, section 8.2). No doubt a sprinkling of hyperbolic rhetoric is unobjectionable, but one has to wonder what concept of rationality is in play here. Taken literally, Cytowic's claims would imply that all mammals are irrational, since they share in those brain features that he has drawn upon to reach this conclusion. But ordinary English, while endorsing the description of, say, a fox as irrational when rabid, also denies irrationality when the fox cleverly escapes its irrational hunters. If Cytowic intends his 'irrational' to be a technical term with a special meaning, he has an unfulfilled obligation to explain that meaning.

<3> Thus Cytowic's "In humans, the relationship between cortex and subcortical brain is not one of dominance and hierarchy, therefore, but of multiplex reciprocity and interdependence" (this issue, section 8.3) is closer to the truth than his assertion immediately thereafter that "it is an emotional evaluation, not a reasoned one, that ultimately informs our behavior" (section 8.4).

<4> Cytowic's further argument that split-brain research demonstrates that 'we [i.e., all humans] possess two minds that differ in content, mode of organization, and even in goals' (1993, p. 214; my emphasis) is as gratuitous as it is wrong: even were we to grant that split-brains encompass multiple minds (which rubs against Cytowic's other observation that the two hemispheres are heavily interconnected via the limbic in addition to the corpus callosum), this would hardly show that ordinary brains likewise encompass

multiple minds--unless we were also to adopt the strange notion that slicing the corpus callosum is a fairly minor operation after all.

<5> It is clear from Cytowic's discussion of form constants (1993, chapter 14) that the secondary percepts of synesthesia are not ordinary--being too simple--yet this does not prevent our understanding them, as that very discussion demonstrates.

<6> For an interesting discussion of the precision with which ordinary language may communicate information about subjective experience, see Marcel, 1988, pp. 133-135.

<7> Of course, this presumption might be incorrect for tacit knowledge, as there is no guarantee that all declarative representations can be raised to conscious awareness, and so some may be tacit. "Implicit" learning (and knowledge), by the way, is related to, but distinct from, tacit knowledge: implicit learning refers to learning that occurs without conscious involvement (or at least recall), such as learning while anesthetized, learning by amnesiacs (which may lead to skill acquisition) or visual learning by blindsight patients (Weiskrantz, 1986). What is learned in this fashion would typically be unavailable to consciousness subsequently and so tacit. But tacit knowledge is a more general phenomenon. For example, after learning how to ride a bicycle we can articulate only the vaguest generalities when trying to teach others; yet the learning process itself was "fully" conscious and is "fully" available to conscious recall.

<8> Cytowic has privately reported that such an attribution was also not his intention.

<9> Although he must be right about that in some tenuous sense: strictly as a matter of logic, it is always possible to replace a set of sentences that employ 'theoretical' terms with a set of sentences that do not and which are equivalent to the first set in its observational implications. In particular, we can use either Ramsey's technique or Craig's axiomatization techniques (Ramsey, 1931; Craig, 1956). However, the same can be said of *any* empirical science, and the point is vacuous in any case: Ramsification of theories eliminates theoretical terms only by quantifying over them and thus retaining an ontological commitment to theoretical entities; Craig's axiomatization technique produces a countably *infinite* collection of *axioms*, and so is not humanly usable or interpretable.

<10> Some apologists for behaviorism, such as Amsel (1989), have pointed out, of course correctly, that behaviorists disagreed with one another and are best viewed as making up different camps. In particular, Amsel would like us to see *neo-behaviorism*, as exemplified in the moderate writings of such behaviorists as Watson (1919) and Skinner (1938), as a more rational and acceptable viewpoint than radical behaviorism, as exemplified in the more extreme writings of such other behaviorists as Watson (1930) and Skinner (1971). The neo-behaviorists acknowledged that verbal behavior is a

legitimate object of study, and Skinner (1950) for example even employed such concepts as motivation and emotion! What this slides over is the fact that Skinner's entire program was aimed at producing laws that either relate stimuli and responses directly or relate these with 'internal' factors in precise ways *so that the latter might be eliminated*; indeed, Skinner's 1950 paper just is an argument that a theory of learning is unnecessary, because internal states are unnecessary.

<11> It is perhaps worth pointing out that an endorsement of the use of heterophenomenological data is entirely distinct from an endorsement of introspection as a methodology, as practised, for example, by Titchner in the early century. Introspective method aims to take as the data of psychology one's own phenomenology, on the assumption that one's observations of one's own mental life are reliable. *Heterophenomenology*, on the other hand, takes the *reports* as data--not whatever subjective phenomenology may (or may not) have given rise to those reports; the reports themselves are as public as any other phenomena recorded in scientific data and may be recorded as reliably. Heterophenomenology allows for Titchner's methodology to be deeply confused even while taking his subjective reports seriously, that is as standing in need of scientific explanation.

<12> I should note that Paul Churchland's dismissal (1981) of such psychological concepts as belief and desire is of a different variety: he dismisses them in his imagination on the ground that a new, better science than current psychology will one day provide a complete set of replacements for psychological predicates using neurophysiological predicates. Churchland's eliminativism, unlike behaviorism, takes quite seriously the task of explaining all varieties of behavior using theories that incorporate theoretical terms, even if it goes on to dismiss one large class of theories based upon little more than a personal desire.

<13> Scriven's article I especially recommend for those who believe that an absolute demarcation between fact and value is possible and that scientific method requires the (impossible) abandonment of values. It will prove equally therapeutic for those who believe that there is no objective basis for value judgements.

<14> Cytowic's claim that neural networks employing backpropagation *ipso facto* take advantage of emotionality (1993, pp. 199f) might be thought to argue for the same result. However, the idea that backpropagation incorporates emotional processing because one of its inventors, Werbos (1974), was inspired by Freud's concept of the backflow of psychic energy is, to be generous, unsuccessful. Inspiration may flow through Werbos, but that alone doesn't show that it flows through neural networks.

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