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Visual perception as a text

SIR—In his recent review of Bruce and Green's new textbook[1] on visual perception, Sutherland[2] first criticizes the authors for their attention to the ideas of the late J.J. Gibson and then holds a number of these ideas up for ridicule. We maintain that Gibson's work belongs in textbooks and that Sutherland's attacks on particular concepts are irresponsible.

Bruce and Green stress, in Sutherland's own words, ". . . some of the exciting new developments that have taken place over the last 20 years". Gibson's work clearly meets this criterion. It challenges many long held assumptions about vision and is generating a considerable amount of research. The acclaim of a number of eminent writers[3-5] suggests the challenges are serious. Also one of the textbook's stated concerns is the ecology of vision. Since Gibson's ecological optics constitutes the only sustained attempt to bring ecological concerns into the heart of perceptual theorizing (albeit not the only attempt to connect ecology and perception), the authors would have been irresponsible to omit Gibson's work.

Sutherland's comments about particular concepts show that he does not understand Gibson's ideas. First, Gibson's claim that information in the optic array can be picked up without computational or inferential processes is said to be ". . . so silly that it is not worth taking seriously". Second, it is said that "Gibson was of course correct in believing that the starting point for what we see is the visual image, but who has ever doubted that?" The trouble here is that Gibson is one of the few who did doubt "that", so Sutherland has Gibson backwards. Moreover, Gibson's alternative to traditional image concepts, the optic array, is essential to the meaning and plausibility of his claims that perception is not inferential or computational. If one accepts "optical images", retinal images, or the like as starting points for visual perception, then the process *must* be inferential or computational. Sutherland is being consistent within his own views. He and those whose ideas he champions (Marr) accept the received views of the "givens" and concentrate their considerable cleverness on computational innovations. What Sutherland fails to recognize is that Gibson's account begins prior to those he is accustomed to. Gibson began by reexamining the nature of the givens for perception and the task of perceiving itself [6].

Finally, it is stated that one does not need to read Gibson to understand that time to collision is specified by variables in optical flow. We might add that one need not read Newton to understand that action equals reaction in the play of physical forces. Gibson brought the basis for time to collision analysis into psychology[7] and his intellectual heirs continue to study its implications[8,9].

By turning Gibson's ideas on their head Sutherland showed that he has not taken them seriously enough to find out what they are. The point of a textbook, such as Bruce and Green's, is to allow students the luxury of discovering the ideas that exist *before* evaluating them.

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X chromosomes and dosage compensation

SIR—D.A. Smith suggests that the primary role of dosage compensation is to equalize the level of transcription between X and autosomes in males rather than to equalize X-chromosome transcription between males and females (*Nature* **315**, 103; 1985). He derives this hypothesis as the logical corollary of the extremely deleterious effects of whole-chromosome aneuploidy. For mammals, X-chromosome inactivation would then be a secondary phenomenon necessary to prevent excess X-linked gene activity in females.

I would like to raise two questions. The first relates to the logic of the argument. I agree with Smith's point that, in the evolution of chromosomal sex-determination, the loss of an X chromosome in males must have forced these biological systems to deal with the problem of ensuing aneuploidy. But the solution to that problem need not have been an enhancement of X-linked activity in males. It is conceivable that the matter was resolved by gradual and individual evolution of the sex-linked genes and the system as a whole, so that lower levels of these products would be adequate. If this were the case, one would still need to inactivate one X in mammalian females to avoid functional hyperploidy but one would not find any traces of a "class of genetic elements which function to modify X-chromosome expression" in males.

The second point deals with the feasibility of testing Smith's hypothesis experimentally. I do not see how one can determine that the level of transcription of X-linked genes is equivalent to that of autosomal genes. How can comparisons be made given that we can only study individual, and therefore necessarily different, genes? For example, what is the rate of transcription for hypoxanthine-guanine phosphoribosyl transferase or factor VIII that is equivalent to that for galactose-1-phosphate-uridyl transferase or factor IX? Answers to these questions would imply that there are global rates of transcription; something that is certainly well worth investigating but that is not established.

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DNA topoisomerases in eukaryotes

SIR—In a recent *News and Views* article (*Nature* **316**, 394-395), G. North provides an interesting and informative summary of recent research on eukaryotic DNA topoisomerases. We were, of course, pleased to see our own work, which had been presented at the Cold Spring Harbor meeting on Chromosome Structure and Expression (8-12, May 1985), cited in the article. However, we feel that our results and their implications were not described with adequate accuracy. North says we have shown that "supercoiled plasmids are relaxed in yeast so long as one of the *TOP1* or *TOP2* genes is functional, which implies that topologically constrained DNA in yeast will be maintained in a relaxed state". What we have in fact shown, more accurately stated, is that the bulk of supercoiled plasmids are relaxed, with a half-life of several minutes, provided that one of the two topoisomerase genes is functional. Thus our results do not exclude the possibilities that there may be a very small population of plasmids which is maintained in a torsionally stressed state and that the bulk plasmid population may undergo transient torsional stress. Given these caveats, there may not be as great a difference between yeasts and higher organisms as North implied.

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