

principle, or even in some respects true (as Kosslyn thinks), they are not a good basis of explanation for imagery effects.

46. A couple of other effects which they consider - 'selective interference' and evidence for 'parallel processing' in imagery - are concluded to be about equally elegantly explicable in 'propositional or in image terms. Even here, however, only imagery theory would have been likely to have predicted the effects.

47. Kosslyn & Pomerantz, 1977 pp.68-9. C.f. Wilton, 1978 pp.566-7.

48. Kosslyn & Pomerantz, 1977 pp.70-71.

49. Kosslyn & Pomerantz, 1977 pp.73-4.

50. 1978 p.298.

51. 1978.

52. E.g. 1981.

53. E.g. Shepard, 1981, 1984a.

54. This is no accident - see Palmer [1978 p.298-9].

55. See R82-3.

40. See Finke, 1980 p.130, 1986 p.82.

41. He gives his reasons for taking this view in Finke [1980]. He generally prefers to speak of "levels" rather than "stages" of processing, but in this context (as opposed, for instance, to that of

56. E.g. Pylyshyn, 1973, 1978, 1979a,b,c, 1981, 1984.

Notes to §II.C.4.

1. 1910.

2. Reviewed in Segal [1971b].

3. See §I.C.1 above.

4. Segal & Fusella, 1970, 1971.

5. Brooks, 1967, 1978; Atwood, 1971; Janssen, 1976a,b; Baddeley, Grant, Wright & Thompson, 1975; Baddeley & Lieberman, 1981 - see §I.C.2 above for discussion.

6. 1977 p.67.

7. In 1980 Finke reviewed his own work to that date (not all of which is considered here) and other work which bears on the issue of the involvement of specifically visual mechanisms in imagery. He argues that imagery can involve mechanisms at several different "levels" or stages within the visual processing system [Finke, 1980]. As I do not accept the view taken there of perception as an essentially linear, data driven sequence of information processing stages (see figure II.C.1_1, and compare the diagram recently given by Finke [1986 p.82]), I cannot fully go along with the argument which he [Finke, 1980] puts forward, but he makes a powerful case for the involvement of perceptual mechanisms in imagery. For a more recent review of relevant work see Finke [1985], and for a popularization see Finke [1986].

8. Finke, 1979. This paper is based on his doctoral thesis of the same year.

9. Finke, 1979.

10. Marks, 1973.

11. E.g. 1984 p.238-41.

12. It should perhaps be pointed out that "tacit knowledge" as understood by Pylyshyn has little in common with the "tacit knowledge of Polanyi's epistemology [e.g. Polanyi, 1958].

13. E.g. Pylyshyn, 1978.

14. McCollough, 1965.

15. For a demonstration of the effect, and a non-technical discussion, see Walker [1986].

16. 1977.

17. Finke & Schmidt, 1977 p.602.

18. 1977 pp.602-3; Finke, 1980 pp.119-120.

19. 1973.

20. Finke, 1980 p.120. As intimated in a previous note, I have reservations about the rhetoric of "levels" used here.

21. 1978.

22. Marks, 1973.

23. 1978. See §§II.B.4 and 5 above.

24. According to Woodworth & Schlosberg [1954 pp.422-3] such "geometrical illusions" can be somewhat

diminished by prolonged scrutiny of the figures. But the illusion soon returns in full strength if, for instance, the figure is turned around, or even if the observer simply briefly looks away from it before looking back. Just knowing that these are illusions does little or nothing to diminish them.

25. See Pylyshyn, 1978.

26. Pressey & Wilson [1974] state that their demonstration of the Poggendorff illusion in imagery shows that the cause of the illusion cannot be peripheral. This is correct inasmuch as they seem to have shown that it cannot result from any of the physiological processes in the retina (or other very peripheral parts of the visual system) which have sometimes been put forward as the causes of such illusions. It is unlikely that imagery affects the retina itself. However, I don't think that Pressey & Wilson would wish to claim to have shown that the illusion is entirely the product of computational processes carried out upon 'propositional' descriptions encoded in the sort of homogenous, quasi-linguistic representational system which Pylyshyn proposes. The 'cognitive impenetrability' of the illusion survives their work.

27. 1974.

28. Pressey & Wilson, 1974 p.447.

29. Berbaum & Chung, 1981 p.126.

30. Berbaum & Chung, 1981 p.126.

31. Berbaum & Chung, 1981 p.126. Note, however, that the subjects used here were the same ones who had taken part in the first version of the experiment, so they could no longer be considered to be unaware of the illusion.

32. 1984.

33. Marks, 1973.

34. Wallace, 1984 pp.159-60.

35. Wallace, 1984 p.159-60.

36. What I have described here are experiments 2 and 3 of Wallace [1984]. His experiment 1 also deals with the Ponzo illusion; however, in this case the subjects were shown figures which consisted of the inducing oblique lines without the horizontals, or perhaps with just one of them. They were then asked to imagine that the missing horizontal line or lines were present, with these horizontals being "of the same physical length" [Wallace, 1984 p.157]. Those who claimed to have succeeded in this task of imagination (all of the 'high' and three of the 'low' imagers, apparently) were then questioned as whether or not the

lines appeared to differ in length, and by how much. Wallace claims that here too the illusion appeared for all the subjects who managed the requested visualization (i.e. the lower line seemed shorter). However, I find it hard to make sense of a procedure which requests that the lines be imagined as equal, and then asks if they ~~seem~~ to be equal. It seems to me that the fact that this ill-conceived experiment 'worked' actually detracts from the credibility of Wallace's other results. In this instance it looks to me very much as if experimental 'demand characteristics' or 'experimenter effects' would furnish the most plausible explanation. And if here, why not in the other experiments?

37. 1980.

38. Wallace, 1980 p.390.

39. See Finke, 1986 pp.82-3.

40. See Finke, 1980 p.130, 1986 p.82.

41. He gives his reasons for taking this view in Finke [1980]. He generally prefers to speak of "levels" rather than "stages" of processing, but in this context (as opposed, for instance, to that of the Craik & Lockhart [1972] "levels of processing" theory of memory) these terms seem to be more or less interchangeable.

42. Finke, 1986 p.82.

43. Finke, 1985 pp.254-6.

44. 1985 p.254.

45. Finke, 1979 p.241.

46. 1983.

47. 1981 p.126.

48. 1980, 1984.

49. The 'positive' results of Wallace's poorly designed experiment 1 [Wallace, 1984] make me particularly suspicious on this score. [See note 36 above.]

50. 1974.

51. Finke, 1985 p.254.

52. 1982.

53. 1978.

54. Broerse & Crassini, 1980, 1984.

55. Finke [1981]. See also Kunen & May [1981], and the reply given by Broerse & Crassini [1981]. Broerse &

Crassini's later study [1984] is designed to meet Finke's criticisms, and still finds no imagery induced McCollough effect.

56. 1980; Kaufman, May & Kunen, 1981.

57. Marks, 1983a pp.103-4.

58. Marks, 1983a p.104.

Notes to §II.C.5.

1. C.f. Dilman, 1967 p.36.

2. See §II.A.3 above.

3. See §I.A.I.

4. See §II.A.3 above.

5. Kant, 1781 (trans. p.183).

6. A mental picture, as Berkeley [1734 introduction §12] pointed out, could be assigned to stand for a universal (a "general idea"). However, this would require that we already have a grasp of the universal.

7. In his recent popularization of his views Kosslyn directly acknowledges the force of Berkeley's argument [Kosslyn, 1983 p.6], although he later makes a halfhearted and unpersuasive suggestion as to how it might be overcome [Kosslyn, 1983 p.207]. But Kosslyn is not really concerned with epistemology, and in his more serious work he can, and does, sidestep the relevant questions [Kosslyn, 1980 p.116].

8. Fodor, 1975 p.191.

9. Olson & Bialystok, 1983 p.26.

10. Olson & Bialystok, 1983 p.x.

11. Fodor, 1980.

12. See Fodor, 1978a p.521.

13. Fodor, 1978b pp.246-7.

14. E.g. Fodor, 1981a p.31; 1985 p.99.

15. See, e.g., P.M. Churchland, 1979; P.S. Churchland, 1980.

16. Fodor, 1980.