

DISCUSSION

ANDREW LUGG*

WHAT GENERATIVISM IS NOT: A REPLY TO BRIAN BAIGRIE

IN A RECENT article on Kepler's justification of the ellipse law Brian Baigrie criticizes accounts of 'justification as a dynamical process whereby hypotheses are generated from an entire spectrum of resources — the theories, principles, laws, and associated conjectures — that are available to the scientist'.¹ While allowing that this "generativist" conception is superior to the traditional Baconian one, he complains that it 'furnish[es] an incomplete and misleading picture of the emergence of cognitive authority in science' (p. 633). In particular Baigrie rejects my contention that Kepler 'justified the ellipse hypothesis by showing how it can be derived (or generated) from generally accepted evidence and theory' (p. 643).² In his view 'the cognitive resources available to Kepler could no more guarantee an elliptical orbit than could Brahe's data' (p. 645).

Baigrie does not challenge the generativist account of the "rationality" of Kepler's adoption of the ellipse law. 'Lugg is right,' he says, 'to maintain that the discovery of the ellipse hypothesis was a rational affair because Kepler "never replaced, revised or supplemented any of his views arbitrarily but . . . always had good reasons for modifying them in the ways that he did"' (p. 660). Nonetheless Baigrie remains dissatisfied. It is, he insists, far from clear that 'Kepler's deliberations *in fact* promoted his ends' (p. 661), it being 'ultimately Newton who "saved" the Copernican theory' (p. 663). To appreciate the emergence of cognitive authority in science, we must look beyond 'the rationality of a hypothesis' and acknowledge that 'while Newton may have been remiss in characterizing Kepler's ellipse as a mere guess . . . he was right to have appropriated credit for establishing the elliptical orbit' (p. 664).

*Department of Philosophy, Faculty of Arts, University of Ottawa, 65 University, Ottawa, Ontario, Canada, K1N 6N5.

Received 3 September 1991.

¹Brian S. Baigrie, 'The Justification of Kepler's Ellipse', *Studies in History and Philosophy of Science* 21 (1990), 633–664. The quotation is from p. 643. All references in the text are to this paper.

²See my 'The Process of Discovery', *Philosophy of Science* 52 (1985), 207–220, especially p. 216. I should perhaps also note that 'generativism' is Baigrie's word, not mine.

But what exactly is it that generativism fails to provide? According to Baigrie it falls short because it treats Kepler's and Newton's arguments the same way and ignores the all-important fact that Newton 'outlin[ed] the dynamical relations between the laws of Kepler, and the forces that produce Kepler motion in the planets, in a way which ruled out alternative accounts of celestial phenomena' (p. 663). However, this argument is far from conclusive. Baigrie cannot mean that Newton provided an exact outline and excluded all alternative accounts, it being possible to develop better explanations of the motions of the planets (as the subsequent history of physics itself confirms). But if all that Baigrie means is that Newton was justified in thinking that he had outlined the correct relations and forces and ruled out alternative accounts, the alleged difference between Newton and Kepler evaporates, it being Baigrie's view as well as mine that Kepler's discovery was a 'rational affair'. In other words taking 'establish' in the strong sense of 'conclusively demonstrate' neither Kepler nor Newton can be said to have established the ellipse hypothesis, while taking it in the weak sense of 'demonstrate on the basis of theories, principles, laws and associated conjectures believed to be true', both can be said to have done so.³

The point becomes clearer still when we consider the 'ends' that Kepler's and Newton's deliberations were meant to promote. For contrary to Baigrie neither scientist claimed to derive his conclusion from physical assumptions (and thereby 'save' Copernicanism), their common object having been to delineate the mathematical character of the relevant forces and to establish the physical intelligibility of their conclusions. The plain fact is that both scientists could — and did — profess to have determined the laws of planetary motion while continuing to explore the nature of the forces that cause the planets to move according to these laws.⁴ Newton may have been more successful than Kepler in formulating a satisfactory physical theory of planetary motion but he too could claim only to have shown that his conclusions were physically 'probable' and 'natural' (compare p. 663).

Moreover I do not see that Baigrie advances the discussion by arguing that the cognitive authority of science derives from the 'ossification' of arguments, a

³Along the same lines I would take issue with Baigrie's argument that 'the notion that Kepler "proved" the ellipse hypothesis, and therefore that he should be credited with establishing the elliptical orbit, does not make much sense in the light of Newtonian dynamics' (p. 645). On the one hand I would argue that Kepler may properly be characterized as having proved the hypothesis regardless of the illumination provided by Newtonian dynamics; on the other hand I would argue that were Baigrie right, the suggestion that Newton proved the hypothesis would likewise have to be deemed senseless in the light of Einsteinian dynamics.

⁴Significantly Kepler obtained the ellipse hypothesis prior to convincing himself that it made good physical sense, while Newton continued to worry about the nature of attraction and repulsion for many years after the publication of the *Principia*. In this regard it is important to notice that the *Nova Astronomia* — which outlines 'the entire chain of reasoning that led Kepler to the ellipse' (p. 647) — is much more revealing than the *Epitome*, on which Baigrie focuses his attention.

process ‘not unlike the hardening of muscular tissue into bony substance’ (pp. 633–634). If in arguing this Baigrie means to suggest only that the quality and quantity of arguments for theories can become so overwhelming that they come to be — and deserve to be — regarded as facts, it is hard to see why he rejects generativism. Indeed there would seem to be no alternative to saddling him with the indefensible view that the older a theory the better, save regarding him as holding the generativist’s view that scientists establish theories by a process of deliberation and debate (and correlatively as treating cognitive authority as resulting from scientists collectively reasoning their way to mutually acceptable conclusions).⁵

Finally a word about the nature of “generativism” itself. To my way of thinking this should not be taken to constitute a general account of scientific justification comparable to ‘Baconian justification’ (or Baigrie’s own theory of ‘foundational justification’). What is being urged is only that we can retrieve the picture of scientists reasoning to laws by focusing on the manner in which they marshal evidence for them, trace out the consequences of accepting them, examine alternative hypotheses and the like. The point that I was attempting to make in my paper — and which seems to me still worth making — was not that generativism is a better theory of scientific justification than “Baconian justificationism” but that we can appreciate and account for the “cognitive authority” of scientific theories by the simple expedient of reviewing how they were generated (this last being understood to encompass both their discovery and their consolidation).⁶

⁵The reason that Baigrie fails to see this may be that he wrongly assumes that ‘the rationale for Lugg’s advancing a distinction between “Kepler’s justification of the ellipse hypothesis” and “the justification of the hypothesis as such” is to be found in his confidence that the former species of justification leads invariably to the latter’ (p. 662). What I actually hold is the much more modest view that further deliberation and debate may be required to establish a theory to the satisfaction of the scientific community as a whole; I do not claim that the first kind of justification always leads to the second, still less that it must.

⁶In this connection I might mention that Baigrie couples my view with N. R. Hanson’s (see p. 643) whereas I believe (and argued at length in ‘The Process of Discovery’, *op. cit.*, note 2) that one may think of Kepler as reasoning logically to his conclusion without committing oneself to the idea of a “logic of discovery”.