DISCUSSION

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OVERDETERMINED PROBLEMS AND ANOMALIES*

I

THIS is a comment on a recent paper of Andrew Lugg[1] on overdetermined problems in science. I think that his paper is a valuable contribution to our understanding of scientific change, and my comment is to be viewed as an addition. My point is that the picture sketched by Lugg is too simple for some (or, perhaps, most) cases, and that it can be profitably extended. I will show this by examining the role played by anomalies in the dismantling of theories and by inquiring into the relations between anomalies and overdetermined problems. The arguments presented here will have to be substantiated by detailed case studies in the course of future research.

II

In Lugg's account, overdetermination arises in situations where no accepted theory allegedly or in fact provides the answer to the problem to be solved. I will be concerned in this subsection with a problem which arised in connection with an accepted theory, namely the perihelion precession of Mercury.[2] Problems of this kind are called *anomalies,* but this classification hides important structural characteristics. In the present case, Leverrier (1859) proposed to solve the problem by assuming the existence of an undiscovered planet between Mercury and the Sun; he had successfully predicted the existence of an undiscovered planet (Neptune) before and therefore was inclined to give full credit to Newton's celestial mechanics. Thirty-six years later Newcomb proposed a modification of Newton's theory.

It is clear that some distinction has to be made here: the solutions proposed

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[1]A. Lugg, 'Overdetermined Problems in Science', *Stud. Hist. Phil. Sci.* 9 (1978), 1 – 18.

[2]I have taken this example from an unpublished manuscript by Willem Roos (State University of Utrecht). See also: R. Adler, M. Bazin and M. Schiffer, *Introduction to General Relativity* (New York: McGraw-Hill, 2nd edition, 1975), sections 6.3 and 6.4; and A. Musgrave, 'Method or Madness?', in *Essays in Memory of Imre Lakatos,* R. S. Cohen, P. K. Feyerabend and M. W. Wartofsky (eds.) (Dordrecht: Reidel, 1976), pp. 457 – 467.

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by Leverrier and Newcomb are very different in character. Before anyone had looked into the matter, the existence of an undiscovered planet was a real possibility; modifying a well-established theory is quite another matter. If we mean by 'anomaly' a phenomenon which does not conform to the relevant theory, Leverrier was trying to solve an anomaly — in the usual way of 'saving the phenomena'. It is somewhat superficial, however, to say that Newcomb did the same thing. For him, it had been established that the theory as it stood was not relevant for (or did not apply to) the phenomenon to be explained.

Newcomb was facing a problem that for him[3] was overdetermined by the conjunction of classical celestial mechanics and the available background knowledge, including the fact that the hypothetical planet could not be found this time. The overdetermination here is closely analogous to the type of overdetermination described by Lugg.

III

The foregoing example shows how a 'mere' anomaly can turn into a so-called 'persistent' anomaly. This raises the question: exactly when does an anomaly turn into a persistent anomaly? There are some cases where a date can be given.

Take, for example, the tellurium – iodine inversion. When Mendeleev formulated his periodic law for the first time (1869), he faced the problem of explaining why the order indicated by the atomic weights of tellurium and iodine is the reverse of that indicated by their chemical properties.[4] We may call this an anomaly, because in 1869 a good case could be, and in fact was, made for the assumption that the atomic weight of tellurium had been determined incorrectly. The anomaly turned into a persistent anomaly, however, when in 1895 Staudenmaier[5] proved (1) that the anomalous value for the atomic weight of tellurium is correct, and (2) that all other available proposals which aimed to explain the anomaly were ill-founded. The deep impression made by Staudenmaier's results can be judged by the reactions of Mendeleev and Ostwald: Mendeleev refused to believe that there was something wrong with the periodic law and started to doubt the determinations of the atomic weight of iodine as well, even despite the fact that the atomic weight of iodine had been determined very accurately over and over again;[6] Ostwald concluded that the periodic

[3]The words 'for him' are inserted here, because Dicke's proposal (1964) shows that even within the framework of classical celestial mechanics the possibilities were not exhausted at the time Newcomb thought they were. (See Adler, Bazin and Schiffer, *op. cit.* note 2.)

[4]See J. R. Smith, *Persistence and Periodicity* (Ph.D. Thesis, Chelsea College, 1976), pp. 343 – 347.

[5]L. Staudenmaier, 'Untersuchungen über das Tellur', *Z. anorg. Chem.* **10** (1895), 189 – 221.

[6]See *op. cit.* note 4, p. 344. (Smith ascribes Mendeleev's change of mind on the accuracy of the atomic weight of iodine to Brauner's results. I think my interpretation is the more probable, because Brauner's results were published six years before Staudenmaier's.)

law actually was not a proper law but only an imperfect empirical generalization.[7]

IV

Both examples serve to illustrate the thesis that persistent anomalies are overdetermined problems. There is another connection between anomalies and overdetermined problems. In the situations described in Lugg's paper there is no theory which rightly or wrongly provides the answers. These situations frequently occur because the theory that could have provided the answer has been gradually dismantled by anomalies, the discovery of inconsistencies, and so forth. In such a context, scientists will try to formulate a new theory, and in doing so they confront at least one kind of overdetermined problem and usually two kinds.

In the first place, the new theory has to explain not only the anomalies but also the otherwise successful career of the old theory; thus, the new theory cannot but clash with the background knowledge which has been collected in the course of using the old theory *and* examining the anomalies. Overdetermined problems of this sort always arise in the course of theory succession.

In the second place, theory succession is almost invariably connected with the incorporation of phenomena which had not found a place in some theory before. Research into those phenomena will have resulted in the development of specific background knowledge in the form of methodological rules, laboratory methods, and the like. (The early researches into radioactivity will serve as an example here.) This time, an overdetermined problem arises as a result of the effort to reconcile bodies of background knowledge stemming from wholly different quarters.

The foregoing suggestions provide an indication of how overdetermined problems may be found in the very complex cases of theory succession in well-established sciences; they are an addition to Lugg's Ice-Age example. As has been said before, careful case studies are needed before anything more definite can be said.

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[7]W. Ostwald (abstract of Staudenmaier's paper), *Z. phys. Chem.* **19** (1896), 168.