extinction or rehabilitation and numerous important related matters.

Many other interesting points are discussed, such as the near lack of intertidal life because of ice scouring, the different bottom biotas with different substrate and depth, the great depth to which the Weddell seal dives (600 meters; for 45 or even 60 minutes). Many new data are presented on filling in the food webs, on physiology under low temperature, and on the many adaptations to this and other factors of the environment. Some of the fish have supercooled blood and do not freeze even when resting on ice platelets on the bottom. In some, blood circulation may be restricted during cold stress and metabolic heat apparently has survival value. Solar radiation is of significance to some. Seals apparently have tolerance to sustained hypoxia, and have high O_2 capacity of hemoglobin. Pregnant females make longer dives.

The section on birds is likewise of great interest, recounting the feats of the albatrosses and giant petrel (some breeding in South Georgia and feeding in off-season near Australia), and the penguins (the emperor incubating in winter on sea ice with egg between feet; the Adélie making fantastic journeys and returning to birthplace). The relations of skua and Adélie are complex and vary with season, environment, and individuals.

The freshwater ecosystems, though limited, are unusual. As with land fauna, the South Victoria Land area is rich in endemics, which suggests survival through the maximum glaciation of the Pleistocene. Some ponds thaw much less often than annually, and there are some unusual phenomena such as "ice explosions."

Ugolini and others describe the primitive nature of soils and stress the importance of moisture to biota. The microbiology of the soils and ponds presents great contrasts and many interesting problems.

The land vegetation is rich and diverse compared with the land fauna, with perhaps a thousand species, even though only two are flowering plants. Lichens are best represented, and most conspicuous. Lamb, Greene, and others elucidate the fundamental ecological roles and the great significance of climate. Rudolph discusses dissemination. Jenkin and Ashton show that productivity is high on Macquarie Island, comparable to the alpine zone in New Guinea and twice as high as coastal southern Australia.

Space allotted to terrestrial fauna is rather meager. Janetschek points out that the ice sheet supports no fauna and that arthropods are better represented in mountains (possible refugia) than at sea level in South Victoria Land. The situation is the reverse in North Victoria Land and in the Antarctic Peninsula area. Tilbrook presents excellent data for the latter region. Holdgate concludes the second volume with an excellent treatment of conservation.

As editor Holdgate has done a superb job of organizing the contributions and discussion. His brief introductions to the 14 sections present succinct background data and useful short summaries. In general, this work is a remarkable contribution to modern ecology and in no sense concerns the Antarctic alone. There is much discussion of bipolar problems, and some comparative articles or parallel ones concerning the Arctic. As is indicated, much significant research remains to be done. The production of the volumes is good, including the numerous graphs, diagrams, tables, and other useful material. Typographic errors are reasonably few.

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Optics and Methodology in the 17th Century

Theories of Light. From Descartes to Newton. A. I. SABRA. Oldbourne, London, 1970 (U.S. distributor, Elsevier, New York). 364 pp., illus. $12.75. Oldbourne History of Science Library.

Acknowledging an intellectual debt both to the late Alexandre Koyré and to Karl Popper, Sabra has undertaken in this impressive study of 17th-century optics to wed the history of that science to the history of methodology. It is his belief and, indeed, the basis of *Theories of Light* that actual scientific practice and metatheoretical convictions are dynamically related. The claim is immediately plausible and will certainly engage assent when the discussion is about an age in which the nature and scope of knowledge itself were as much a matter of debate as the nature of the natural world. Beginning with Descartes's considerations on method and his optical studies, including the derivations of the laws of reflection and refraction, and then turning to the controversy with Fermat over the proper demonstration of these laws, Sabra substantiates the view that a deeper understanding of the optical theories can be gained by a careful look at the related "methodological" teachings. After an analysis of Huygens's "Cartesianism" and his successful elaboration of a pulse theory of light, the ground has been prepared for a thorough treatment of Newtonian optics and the criticisms it provoked among the "wave theorists." Declining to write the usual survey, Sabra has not only treated "problems and controversies which have appeared to [him] to be particularly important in the development of seventeenth-century theories about the nature of light and its properties," he has in following that program engaged himself with the century's most influential methodologists.

While there are some grounds for seeing in this twofold enterprise the successful articulation of a new historiography of science, I must confess that I have found the union somewhat debilitating to both partners. In pursuing this "method" of confronting actual research in optics with the "attendant evaluations and theories of method" enunciated by the practitioners, Sabra has been unable, as I see it, effectively to focus his concern either on the development of optics in the 17th century or on the difficult and increasingly sophisticated methodological issues. The paradoxical result is that, while the discussions of method do not come fully to grips historically or philosophically with the questions they naturally raise, they occupy far more space than their "payoff" in understanding the optical theories seems to justify, and the concern with method distracts attention from the inner dynamics of the optical researches themselves. We should applaud Sabra's decision not to write a survey, but it does not relieve the book of the obligation to consider whether the period in question constitutes a meaningful or coherent "era" from the point of view of some problems or set of problems in optics *or* in scientific methodology. One should consider the interaction of researches toward a theory of light not so much with second-order speculations about the legitimacy of particular accounts as with the obviously related theorizing about the physical-chemical nature of matter and about the nature of vision and perception, as well as the "metaphysical foundations" of optical theory. The methodological issue would then take care of itself, so to speak. Sabra by no means ignores these issues, but they do not appear in support of a coherent argument (or "theory," if you wish) about the development of optics in the 17th century.

If we already had a clear notion of that development, this criticism would be inept. But we do not. Never before has a historian with Sabra's knowledge, insight, and analytic and linguistic skill tackled 17th-century optics as the subject of a book-length study. Make no mistake about the fact that this book immeasurably increases our understanding of optics in the period from Descartes to Newton. *Theories of Light* is now the touchstone and starting point of future research on the subject and must also be recognized as an invaluable source on problems of methodology in that era of philosophical upheaval. But although most historians of science would agree that those problems and controversies Sabra has illuminated so well are "important," it is not at all clear that they would understand or agree why. Finally, in the absence of a well-argued thesis, certain omissions cannot help seeming arbitrary. It is fair to ask that a book defend its choice of topics by relating them to its understanding of the subject as a whole.

The central concern of the first part of *Theories of Light* is Descartes's famous derivation of the sine law of refraction by a mechanical analogy with the deflected trajectory of a tennis ball. Because of glaring inconsistencies between the terms of that proof and Descartes's own a priori principles of physics, it has been part of common wisdom ever since the 17th century that Descartes plagiarized the law of sines, probably from Snel, and that his proof is a posteriori and ad hoc. Sabra shows rather convincingly that this view is all wrong and that we must accept Descartes's proof on its own terms. An analysis of Cartesian methodology contributes part of the argument by showing that Descartes was quite willing to accept incomplete and even hypothetical analogies at the level of explanation of particular phenomena.

The other part is contributed by the substantially historical account, which concludes that Descartes's proof must be seen as part of a tradition of mechanical analogies of reflection and refraction in which the most important previous contribution was made by the 11th-century Islamic natural philosopher Ibn al-Haytham. Since the historical argument is far more telling, it is odd that it is underdeveloped in comparison with the lengthy exposition of methodology. There is, for example, no analysis of Johann Kepler's laborious though frustrated efforts to derive a "quantitative measure of refraction," which occupy all of the bulky fourth chapter of his *Paralipomena* (1604), and no mention of his attempt in that same work to subsume refraction under the general mechanical principles of the "medieval science of weights" following the same mechanical tradition with which Stevin, Galileo, and Descartes began their careers. According to Lohne, Descartes's first attempt to derive a law of refraction was by applying the mechanics of the balance. Again, although part of the case for Descartes's originality is made to rest on his having had a hyperbolic glass made to test his reasoning, Sabra does not tell us that Kepler had already, in 1604, hypothesized on qualitative grounds that the shape of that refracting surface that would bring parallel rays to a focus would be hyperbolic. Similar questions could be raised about the possible relationship between Descartes and those in Thomas Harriot's circle. Sabra's decision to begin his study with Descartes is based on the conclusion that the Cartesian commitment to a totally mechanical view of light and its behavior marks the "legitimate starting point of physical optics," and that on the relevant issues of refraction and a physics of light Kepler rests squarely in the medieval tradition of Grosseteste, Bacon, and Witelo. Meanwhile, Descartes's own claim that Kepler was his "first teacher in Optics," cited on page 30, seems to cry out for examination.

In the consideration of Fermat's objections to that notorious proof, we are treated to an excellent example of the methodological sophistication of the age. More than willing to admit that the sine law had been experimentally vindicated, Fermat was nevertheless convinced that Descartes's proof was utterly spurious; he

. . . reasoned that the true ratio for refractions might still not be identical with that proposed by Descartes, though so near to it as to deceive the keenest observer; there was, therefore, room for a fresh attack on the problem *in spite* of the apparent verdict of experiment [p. 137].

We find here an early instance of the conviction that observational equivalence by no means entails theoretical equivalence—at the very least, as Fermat recognized, there is plenty of room for competing theories within the margin of experimental error. This conviction is most notably being advanced these days in philosophical circles by P. K. Feyerabend in his insistence that true empiricism requires at all times the existence of competing theories which are indistinguishable at the level of observational tests. Feyerabend has in mind the debates concerning the completeness of quantum mechanics (Einstein and Bohr) and the legitimacy of a "hidden variable" theory (Bohm), but the case of Fermat seems equally to vindicate the argument.

Theories of Light raises a number of issues that need further examination. First, it does not become clear in Sabra's account just what distinguishes issues that are "methodological" from those that are "scientific" or "metaphysical." Thus, for example, we do not learn whether the disagreement between the Cartesians and Fermat over the latter's use of a teleological principle of least time is about the nature of reality (is Nature purposeful?) or about "admissible" explanations. While I grant that on a realist interpretation of theories the two questions are not very distinct, to imply that such a disagreement is "methodological" seems to involve the tacit acceptance of an instrumentalist view of theories, which may not be fair to Fermat, Leibniz, Maupertuis, or other users of a principle of economy. Similarly, we can ask whether Huygens's insistence on a mechanical "criterion of intelligibility" amounts to a methodological or a metaphysical stance and what if any difference there is between that and Newton's "atomistic" presuppositions. Had a clarification been made between methodology and metaphysics it would have been easier to understand what Sabra means when he concludes that Huygens is a Cartesian, and it would have clarified his ultimately cogent argument that Newton's methodological "Baconian" tendencies (the willingness to believe that conclusions can be "deduced" from phenomena) combined with his atomism to produce those corpuscularian theories of light which Newton claimed were independent of hypotheses. That these beliefs about "truth" on the one hand and the "sources of truth" on the other may not have appeared distinct to Newton does not reduce the usefulness of the distinction.

Similarly, in talking about method should not some care be taken to distinguish what is properly epistemological from what is more authentically procedural or stylistic? It cannot be entirely sound to suppose that the "format" of Newton's *Opticks* (or the "mode of presentation" to the Royal Society of the theory of colors in 1672 as a "compelling tale of discovery") by itself allows inferences about underlying epistemological convictions. In another instance, Sabra takes great care to show that Huygens is not a Baconian despite the appearance of a plan for the Physical Assembly of the *Académie Royale* written by Huygens almost entirely out of Bacon's *Novum Organum*. In all this, the very real possibility that Huygens's tract has not an epistemological but a "political" core is never considered. Surely one part of scientific method has to do with establishing the literary and social acceptability of scientific practice. Some scientists and scientific institutions in the 17th century may have had good reasons to appear Baconian without ever giving real substance to that appearance. Indeed, one may very well ask to what extent 17th-century scientists were able to separate the procedural, stylistic, and social aspects of the Baconian ideology from the epistemological commitment to inductivism. Even if our current philosophical verities were only beginning to emerge in this period, it does not reduce their usefulness as tools of inquiry. Finally, one is left wondering whether Sabra would have come to the same conclusions about the methodology of Descartes, Fermat, Huygens, or Newton if he had considered in addition to their meta-optical speculations the ways in which they argued their support for, say, Descartes's first law of motion, the principle of inertia.

In the controversial closing section on Newtonian optics and its critics, Sabra masterfully sustains the argument that Newton's commitment to atomism permeates his optical theorizing even down to the level of the "deductions from phenomena." At that lowest level, however, atomism appears in the benign form of a principle of simplicity. Sabra's argument—borrowed from M. Gouy and supposedly hinted at by Hooke—that white light may be regarded as homogeneous if the prism is regarded as a Fourier machine seems to me too weak to support the charge of Newtonian "dogmatism." Much more could be made of the awesome unity that atomism gave Newton's natural philosophy and of the considerable experimental support it enjoyed. More attentive than any other optical theorist to the physico-chemical investigations of Boyle and Hooke, Newton maintained the essential identity of light and matter as atomic as a way of coping with the complex interactions that are the essence of physical optics. Much the same methodological motivation stands behind the proposal of Kelvin and J. J. Thomson, in the very different climate of 19th-century physics, that if light is an aether-wave then matter must also be a "mode of motion" of the aether, the smoke-ring vortex of Helmholtz. The triumph of Newton's derivation of the sine law is the defense of the hypothesis that the particles of light and matter act on each other at a distance. To support that proposal is also the essential point of the "Queries" to the *Opticks*; atomism responded well to what Newton recognized as physical, chemical, and optical specificity.

As the century closed, Newton's methodological "dogmatism" was more than matched by Huygens's metaphysical stubbornness and by Hooke's inability to turn his ideas into theories. Newton's bizarre theory of "fits of easy transmission and reflection" with which Sabra closes his excellent book is testimony enough of the crisis into which optics had fallen.

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Molecular Interaction

Organic Charge-Transfer Complexes. R. FOSTER. Academic Press, New York, 1969. xii, 472 pp., illus. $22.50. Organic Chemistry, vol. 15.

In 1952 Mulliken explained the special character of light absorption by many "molecular complexes" as being charge transfer, thereby stimulating a surge of research in this area. For a few years it seemed as if everything from the color of liver to the infectivity of viruses would be explained in terms of charge transfer, but early enthusiasm has given way to more sober and careful examination of one of the really significant aspects of intermolecular interaction. As the recent Gordon conference on electron donor-acceptor interactions demonstrated, thorough experiments on the thermodynamics, the structure, and the chemical behavior of "charge-transfer complexes" are now the rule.

Foster has written a thoughtful and comprehensive account of these complexes. Proceeding from a general and historical introduction, he describes very clearly the theory of the complexes and then their electronic, infrared, and nuclear magnetic resonance spectra. After a thorough discussion of the equilibrium constants for complex formation (methods and results), the crystal structures and electrical and magnetic properties are covered. A few pages are devoted to complexes that are of special interest. Reactions that may involve charge-transfer complexes are treated in a clear way, and a judicious and astute review of the biochemical significance of charge-transfer complexes is then presented. The book ends with a few applications of complexes in organic chemistry.

No errors were noted and no important omissions came to mind. Foster has made an effort to make the book easy to read and easy to use, and it deserves a place in every chemical and biochemical library.

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