gation—in this case, inorganic reaction mechanisms in solution—matures, the need to keep this general view while sharpening the focus increases. It is encouraging that in the volume under consideration several contributors have attempted to meet this need.

Cattalini's discussion of substitution in square planar complexes and Chaffee and Edwards's chapter on the importance of the role played by intermediates containing both oxidant and reductant in some nonmetallic systems are examples: In Cattalini's chapter, the data for many Pt(II) complexes are compared with more limited data on Rh(I), Pd(II), and Au(III) complexes, and from these comparisons the author makes arguments about the nature of the transition states and expanded coordination number intermediates as a function of metal ion identity. The article by Kustin and Swinehart is exceptional in the incisiveness of both the questions examined and the route taken to the answers. This chapter stresses the importance of the structure and rate of solvent exchange in describing metal-ion complexation processes. The rates at which the various lanthanide ions react with ligands, and especially the sudden decrease in reactivity in the second 7 of the 14 tripositive ions, well illustrate the authors' premise. It is somewhat disappointing to me, however, that there is no extended speculation on what structural and electronic properties of aquo metal ions lead to a choice between associative and dissociative mechanisms.

The chapter on peroxide reactions stresses the Fe(III)-catalyzed decomposition of hydrogen peroxide. The authors argue that the classic, Haber-Weiss, free radical mechanism of the ferric-hexaquo-ion-catalyzed reaction is not satisfactory, and stress the role of species such as $FeOOH^{2+}$ and FeO^{3+} in this reaction. They generalize this inorganic model to the catalase- and hemin-catalyzed decompositions. This chapter provides many interesting (admittedly biased) interpretations. Other reviews include a well-referenced chapter on binuclear cobalt complexes and a discussion of non-bridging ligand effects in oxidation-reduction reactions of metal complexes.

This collection offers several high-quality reviews and enough information of general interest to chemists and biologists dealing with related problems to warrant attention. The index is well done and the errors detected by this reviewer were few. Although not a complete survey of inorganic mechanisms, the book will serve a useful purpose.

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Concepts of Force

Force in Newton's Physics. The Science of Dynamics in the Seventeenth Century. RICHARD S. WESTFALL. Macdonald, London, and Elsevier, New York, 1971. xii, 580 pp., illus. $23.95. History of Science Library.

The literature on 17th-century dynamics is varied and plentiful, yet Westfall has succeeded in filling an empty slot. His book is the first account of the whole field since Dugas's *La Mécanique au XVII^e Siècle* of 1954, and is in fact the first extensive account in English. This alone makes its publication an event of special interest to historians of science and historically minded physicists.

Westfall's principal objective is to present a critical study of the growth of Newton's concept of force, both as a conceptual tool in his dynamics and in its relations to his philosophy of nature. Since Newton's dynamics cannot be properly understood without an appreciation of the 17th-century legacy he inherited, a secondary objective is to trace the development of dynamical ideas from Galileo to Leibniz. This is a sizable program, and difficult to carry out satisfactorily. To my mind, Westfall has achieved his purpose only to a moderate degree: there is a striking imbalance in structure and content, and considerable unevenness in interpretation.

Take the structural imbalance. The main title notwithstanding, less than half the book (the final two chapters) is devoted to Newton, though admittedly this asymmetry is mitigated by the much greater amount of detail that distinguishes the chapters on Newton. Moreover, Westfall does not always show clearly how Newton's ideas relate to the very wide range of dynamical thinking that preceded him (obvious exceptions being that of Galileo, Descartes, and Huygens); a fair proportion of the pre-Newton material, though wholly welcome in itself, does not do very much to illuminate Newton's own thought.

There are some unexplained omissions. I note with pleasure the great number of thinkers Westfall discusses: Galileo, Descartes, Gassendi, Hobbes, Baliani, Mersenne, Marcus Marci, Torricelli, Huygens, Pardies, de Chales, Wren, Wallis, Hooke, Borelli, Mariotte, Roberval, and Leibniz. But with a list already as long as this, why omit discussion of Isaac Beeckman, whose dynamical ideas strongly influenced Descartes and are of greater interest than those of (say) Hobbes? Or the Cartesian Malebranche, who wrote copiously on the collision problem, yet who is mentioned only once, in a footnote? Or Honoré Fabri, who is not mentioned at all?

The interest and value of the book are increased by Westfall's decision not to confine his analyses to force as a conceptual entity, but to examine also dynamical and physical problems in which the concept figured importantly. This being so, it is disappointing to find no mention of the problem of center of percussion as treated by Descartes, Roberval, or the aforementioned Fabri. Had Westfall examined Descartes's treatment of this problem he would surely have revised his claim that "the most important step he [Descartes] took in the direction of a mathematical mechanics was his analysis of impact" (p. 89). Given that this same analysis of impact is crucial to an understanding of Descartes's and indirectly Newton's concepts of force, it is depressing to find no reference to the Scholastic notion of modal contrariety between motion and rest, without which Descartes's rules of impact are unintelligible, and no reference to the principle of least change of incompatible modes, which Descartes himself showed was an essential component in the demonstration of his rules and which is an embryonic form of the principle of least action.

On the positive side, there are interesting chapters on Galileo and Leibniz and a fine chapter on Huygens. It is particularly pleasing to have this short study on Huygens, who for some reason has never attracted the same attention as Galileo, Descartes, or Newton but whose attempt to reduce mechanics to kinematics, a feature of his thought underlined by Westfall, was an important turn in 17th-century dynamics. The many sections on the lesser-known figures mentioned above contain much material not easy to obtain elsewhere; from these very useful surveys I might single out an analysis of Torricelli's tussle with the force of percussion.

On the critical and interpretative level, there are some comments I think ought to be made. The chapter on Descartes does not lack interest, and is often stimulating, but as a guide to the fundamental theses of Cartesian mechanics it is on the whole superficial and at times simply erroneous. It adds little to (and in places even subtracts from) the work of Koyré, Costabel, Gueroult, and others. One major error is the assertion that the Cartesian distinction between a body's "force of motion" and "the determination of its motion" corresponds to the distinction between the force of motion and the direction of motion. A careful study of the texts in which the concept of determination plays a part shows that by "determination" Descartes understood something like "the directional mode of force." This is not a quibble, for Westfall's error leads to a general misconception concerning Descartes's dynamics, typified by his mystifying remark that "a simple reflection like that of [a] ball [rebounding from a hard surface] entails no dynamic action whatever" (p. 67). This misreading not only vitiates Westfall's assessment of Descartes, it has unfortunate ramifications in the chapter on Leibniz, where he writes that Leibniz rejected the Cartesian distinction between force and determination and then goes on to give an analysis of Leibniz's use of the latter concept which indicates that it was precisely the Cartesian idea of determination that Leibniz had in mind.

Westfall also misreads Descartes's analysis of the problem of circular motion in claiming that "he employed a conceptual scheme which treated the tangential path as the resultant of a circular motion and a radial tendency away from the center" (p. 81). The converse was the case: the circular motion and the centrifugal tendency were both, in different ways, resultants of the revolving body's innate and primary endeavor to pursue uniform rectilinear motion along the tangent.

The chapters on Newton are well documented and contain shrewd insights into his dynamical thought. Nonetheless, they are not free from confusion and misinterpretation. The principal thesis that emerges from the book is that the mechanical philosophy, according to which natural phenomena were to be explained in terms of material particles in motion and interacting with each other only via discrete impulses transmitted by contact, was in the long run inimical to the development of dynamics. It was Newton who made possible the creation of a completely quantifiable dynamics by introducing the notion of continuously acting attractions and repulsions into his philosophy of nature. This is an important thesis, and certainly worth presenting and defending. I do not find Westfall's defense of it convincing, however. In his study of Newton he seems to me (i) to confuse "force" as a mechanism proposed to account for phenomena (for example, the models of impact and attraction) with "force" as a dynamical concept designed to codify the phenomena and their explanatory mechanisms in terms of mathematical laws, and (ii) to blur the distinction between the physical transition from a model of discrete impact to one of continuous attraction or repulsion and the mathematical transition from an analysis of the discrete model in terms of force quantified as Δmv to an analysis of the continuous model in terms of force quantified as Δmv in a given time, that is, effectively as ma.

Westfall is correct in spotlighting the fact that prior to the *Principia* Newton entertained side by side two kinds of force: that which maintained a body in "inertial" motion, and that which caused changes in its inertial motion. Yet he spoils the whole case by claiming (pp. 490–491) that with the *Principia*,

> after a fruitless flirtation with an absolutistic dynamics, in which the force of a body would express its absolute motion, he abandoned completely the conception of force as mv, the force of a body's motion. Phrases about bodies moving by their inherent force alone, artifacts left behind in the historical development of his dynamics, should not be allowed to mislead us on that score.

It is precisely Westfall's refusal to take these phrases in the *Principia* seriously (assuming of course that Newton meant what he wrote and wrote what he meant—a moderately reasonable assumption) that has misled him. This misunderstanding of Newton post-1687 derives ultimately from the belief—pervading the whole book, though seen at its most damaging in the chapters on Newton—that the 17th-century "principle of inertia" can be interpreted in the modern sense of a principle specifying an uncaused physical state of body, in which no force whatever is associated with the body in inertial motion. This belief is not supported by the historical facts. In particular, it is not true with respect to Newton, who held, right up to the third edition of the *Principia*, that a body is maintained in free uniform rectilinear motion by its "innate force" (the *vis insita*), measurable by the quantity of motion (mv), and also that this "inertial" motion can be changed by an "impressed force" (the *vis impressa*), measured by the *change* in quantity of motion.

Westfall's mistaken belief consequently leads him into all sorts of trouble. For example, he writes (p. 363):

> . . . two major ambiguities associated with the concept of force remained. Is force the measure of motion or the measure of change of motion? If it is the latter, is its paradigm case impact or free fall, is it measured by Δmv or by ma? These questions remained to plague the composition of the *Principia*.

Comparison of Newton's texts with Westfall's discussion of them invites the speculation that Newton was less plagued by these questions than Westfall is. The second major ambiguity becomes less problematic when it is noted that in the *Principia* Newton explicitly distinguishes between "impressed force" (quantified as above) and the "accelerative quantity of [centripetal] force," quantified as being "proportional to the velocity which it generates in a given time"—a distinction to which Westfall devotes inadequate attention.

To be paradoxical in conclusion, Westfall's study is both stimulating and confusing, both defective and valuable. Specialists will find a lot to criticize, yet I hope they will agree that it is a book that needed to be written. I certainly welcome its appearance.

There are five informative appendices on the usages of the term "force" by Galileo, Descartes, Gassendi, Huygens, and Borelli, two on Newton's usages of "impressed force" and "action," and an extensive (though not at all complete) bibliography of literature on mechanics in the 17th century. The index is well prepared.

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