intermediary of a duckbilled platypus. Lamarck, for all the genius of his thought, presented no documentation for his ideas and confined his examples to speculation about giraffes' necks and drunkards' intestines. Darwin floated seeds, spoke to pigeon fanciers, and watched earthworms.

We arrive then at the key point: Darwin triumphed by his documentation and convinced the world that evolution had occurred. Yet he did it with his abridgement, the *Origin*—without footnotes and without citation of sources. Since he could not have been more successful in the impact of his documentation, the longer version was clearly not necessary to achieve his result.

But could the longer version, with its more copious documentation, have carried the day for his theory of natural selection? The answer again is clearly no; for the difficulties of natural selection in 1859 placed its vindication far beyond the power of any data then available. First of all, the genetic key was missing and not to be supplied for another 40 years. Natural selection requires a particulate theory of inheritance to ensure the preservation of favorable variants in populations. Second, and perhaps more important, natural selection was philosophically far too radical for Victorian minds; for it explodes any concept of inherent progress, denies to life an ontological status separate from inanimate matter, and attributes the properties of mind to the highly complex workings of a material brain. The 19th century was not ready for this brand of materialism. Today, all scientists accept materialism (at least in their workplace), and the philosophically astute realize that it poses no threat to our love for music, subjective insight, and love itself. Yet, when I read the tracts of the Creation Research Society and watch Arthur Koestler groping for inherent meaning, I wonder if we are ready for Darwin yet.

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Newton at a Major Juncture

The Mathematical Papers of Isaac Newton. DEREK T. WHITESIDE, Ed. With the assistance in publication of M. A. Hoskin and A. Prag. Cambridge University Press, New York. Vol. 5, 1683–1684. xxiv, 628 pp., illus. $65. 1972. Vol. 6, 1684–1691. xxxvi, 614 pp. $72.50. 1975.

In the spring of 1684, for reasons that are not entirely clear, Isaac Newton, Lucasian Professor of Mathematics at Cambridge University, tardily complied with university statute by depositing in the library fair copies of his lectures on algebra delivered during the previous decade. From all evidence, the text of the 97 lectures that make up the deposited manuscript was composed as a whole within a few months, and the absence of any draft versions makes it difficult to gauge how much of the content had actually passed over the lectern. The manuscript represents to all intents and purposes Newton's final word on matters algebraic. By early 1684 mechanics, especially the problem of planetary orbits determined by central forces, was taking increasing hold of his attention; with the visit of Edmond Halley in late July and early August of that year, Newton's career took its fateful turn leading to the *Principia.*

In the latest two volumes of his already classic edition of Newton's mathematical papers, Derek T. Whiteside provides material enabling us to catch Newton at this major juncture of his career. Volume 5, consisting primarily of the deposited lectures on algebra (pp. 54–517), is in essence a companion piece to volume 4 and completes the record of Newton's activities at Cambridge in the quiet and fruitful decade between the optics controversy of 1672/73 and Halley's visit. Volume 6 makes it possible to follow Newton through several reworkings of the treatise on motion begun in 1684 on the basis of his insight into the generality of Kepler's area law for a body moving under any centrally directed force. That treatise ultimately became the core of books I and II of the *Principia*, and so volume 6 is also a companion piece, not to previous volumes in this edition but rather to John Herivel's *Background to Newton's 'Principia'* (Oxford University Press, 1966) and especially to Alexandre Koyré and I. Bernard Cohen's variorum edition of the *Principia* itself (Harvard University Press, 1971).

A comparison of the material in the two volumes reveals contrast and even irony. Newton seems to have been ambivalent in his attitude toward algebra right from the start. For all its heuristic powers, it seemed to him a dodge that lacked the elegance and force of geometrical demonstration. Except for his method of approximating roots, he made no essentially original contributions to the subject. Drawing his concepts and methods from others, most notably Gerard Kinckhuysen and René Descartes, he failed to work on them that special transformation he effected on predecessors' results in other fields. The lectures he deposited in 1684 had mathematical and stylistic faults which he left unrevised (though we have from roughly the same period a "First Book of Universal Arithmetic" [volume 5, pp. 538–621] which begins the process of polishing).

Newton might never have turned back to the work had not his successor in the Lucasian chair, William Whiston, come across the manuscript in 1705/6 and decided to publish it. Newton could do little to stop Whiston, managing only to get the title changed from *Arithmetica Universalis sive Algebrae Elementa* to *Arithmetica Universalis sive De Compositione et Resolutione Arithmetica Liber* and to keep his name out of the book. Whiston printed the text (London, 1707) as he had found it, errors and all. Only after a popular English translation by Joseph Raphson appeared in 1720 did Newton undertake minor revisions for a second Latin edition in 1722, again hiding his authorship. Neglected when given originally, ignored when deposited in the library, never publicly acknowledged by their author, the published lectures nonetheless became after Newton's death perhaps his most popular and widely read work.

By contrast, as both volume 6 of the *Papers* and the variorum edition of the *Principia* show, on the subject of mechanics Newton wrote and rewrote, derived and rederived, calculated and recalculated in a never-ending effort to be more precise, more exact, more elegant (for the story of this effort after 1687, see I. B. Cohen's *Introduction to Newton's 'Principia'*, Harvard University Press, 1971). This was the subject he created, where every previous result took on new form and meaning at his hands. He wrote for publication, he meant to be read, and he had the reader in mind. Yet it appears that few people actually read the *Principia* with the care it deserved, and Newton earned the reputation of having written a deliberately obscure treatise. In fact, he did not earn it; as Whiteside remarks (volume 6, p. 25),

> Why the *Principia* so quickly gained its ill-deserved popular reputation of being impossibly difficult is not easy to understand: certainly, though his natural terseness of style and crabbed mode of presentation was no help to its comprehension and assimilation, there is no evidence that Newton sought deliberately to be any more esoteric therein than he needed be. While the undiluted richness of their intricate mix no doubt

played its part in creating the myth of the work's impenetrability, all too few of the methods there employed will individually—in divorce from the often highly ingenious manner of their dynamical application—seem novel to the student of our earlier volumes.

The final irony, of course, is that the *Principia* had its greatest influence only after being "translated" into algebraic terms by the rational mechanicians of the 18th century. The lectures on algebra and the development of the treatise on motion show Newton moving in the opposite direction. For all his references to algebra as a general or universal arithmetic, indeed as the science of "abstract relations of quantities" (volume 5, p. 132), he ultimately ranked geometry higher and sought to maintain its autonomy. Thus one finds in the lectures a full appreciation of the goals and techniques of algebra as they had emerged and been articulated by algebraists since François Viète, and yet, at the same time, a reaffirmation of the canons of Greek geometry. Toward the end, for example, Newton rejects Descartes's and others' classification of curves by the degree of their algebraic expressions in favor of the ancient criterion of simplicity of geometrical construction, even to the point of again separating the straight line and circle from the conic sections (though the revise in volume 5, pp. 538–621, does restore the algebraic ordering). Nonetheless, he goes on to show in great detail how various conic sections can be used to solve cubic problems also solvable by the geometrically simpler conchoid.

Indeed, the conic sections loom large in Newton's lectures on algebra. They are used to solve algebraic equations, and algebraic analysis in turn is used to construct the curves and determine their structural elements. Moreover, they play this major role in a treatise unusual for the number and variety of examples drawn from astronomy, optics, hydrostatics, statics, kinematics, and dynamics. That is, Newton's physical researches find their way into his algebraic lectures, thus linking the academic exercise with the magnum opus. But the link works against algebra. From the original treatise on motion to the last revisions of the *Principia*, some of the boldest mathematical innovations (and most signal failures; see Whiteside's remarks in volume 6, pp. 26–27) occur in the determination of planetary orbits, and it is fascinating to follow Newton's abandonment of the methods of Cartesian algebraic geometry in favor of what are now recognized as projective methods (see especially volume 6, p. 229ff). Whatever help Newton received from algebra and the method of fluxions and series, he saw the universe as a geometrical entity, and he preferred to treat it that way. How nicely the development of his thought in volume 6 tends to support Whiteside's observation in volume 5 (p. xii) that

> In later years, certainly, he grew increasingly soured with the often cumbersome computations and techniques of Cartesian algebra—at one point, indeed (if we may believe David Gregory), he qualified it as 'the Analysis of Bunglers in Mathematicks'—and we may be certain that his reluctance during 1705–6 to have Whiston edit the deposited text of his algebraic lectures was not merely the manifestation of a growing personal antagonism to his successor in the Lucasian chair.

Surely that reluctance had something to do with Newton's sense of the inadequacy of algebra in dealing with celestial mechanics.

All this is, of course, only a taste of the wealth to be found in the latest volumes of Newton's mathematical papers. On a more specific level, for example, one can follow Newton's attempts to construct a "Gravitational Theory of Optical Refraction" in which light corpuscles are constrained by a centripetal force to move on an orbit through a medium (volume 6, pp. 422–434), or his effort to establish a general theory of quadrature for algebraic curves (volume 6, pp. 450–455), or his painstaking but unsuccessful try at computing the rate of motion of the moon's apogee and mean secular advance (volume 6, pp. 508–537). Most important, one has a chance to tour Newton's mathematical mind accompanied by its surest modern guide. For lest the by now expected be overlooked, let us confirm Whiteside's continued mastery of historical editing and his encyclopedic knowledge of the work of Newton and his contemporaries. Is it niggling, however, to suggest that installments of the tour have become rather expensive for the private person?

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Laws That Govern Behavior

Psychophysics. Introduction to Its Perceptual, Neural, and Social Prospects. S. S. STEVENS. Geraldine Stevens, Ed. Wiley-Interscience, New York, 1975. vi, 330 pp., illus. $19.95.

In 1860, Gustav T. Fechner—physicist, physiologist, philosopher—published *Elemente der Psychophysik. Elemente* put forth a new science concerned with quantifying the relation between sensation and stimulus, and played a mighty role in the birth of experimental psychology. In 1975 appears *Psychophysics* by S. Smith Stevens—experimental psychologist and first professor of psychophysics (at Harvard). The new book sums up a scientific lifetime devoted largely to the problem posed by Fechner. Between these two books stands no work of comparable stature. Just as Fechner's volumes defined the field for the ensuing hundred years, so Stevens's publications, culminating in this book published two years after his death in January 1973, have since the late 1950's provided its leading paradigm.

Unlike *Elemente*, which followed ten years of intense experimentation but few publications, *Psychophysics* follows 40 years of both intensive research and numerous publications. *Psychophysics* weaves together many threads to provide a cohesive picture of the current and potential state of the art as Stevens saw it in 1972 when he finished the manuscript. His wife and editorial collaborator, Geraldine Stevens, put the finishing touches to the book.

Elemente, being all new, overflowed with theoretical and methodological details. *Psychophysics*, as a summation, could address major issues and leave most details to cited references. Whereas *Elemente* was a beginning *Psychophysics* is a culmination, but it is hardly a termination, for it boldly points the way to intriguing "prospects" in physiology and social psychology, prospects that have already begun to be realized. The major difference between *Elemente* and *Psychophysics* (aside from length and style) lies in the law that ties sensation magnitude, ψ, to stimulus magnitude, ϕ, and the way the law was formulated and justified.

Fechner wanted to measure sensation magnitude—how strong a stimulus appears—but he believed along with most of his contemporaries that measurement required units to add together. And how could a sensation be divided into pieces to be added together? As William James put it in his oft-quoted claim, "Our feeling of pink is surely not a portion of our feeling of scarlet; nor does the light of an electric arc seem to contain that of a tallow candle in itself." Fechner's solution was to assume that the smallest physical difference (jnd) that an observer can just notice between two magnitudes must evoke a constant subjective difference. Thus, in just barely