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NEWTON'S SCIENTIFIC AND PHILOSOPHICAL LEGACY

edited by

P.B. SCHEURER and G. DEBROCK

KLUWER ACADEMIC PUBLISHERS

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P.B. SCHEURER

Catholic University Nijmegen and University of Geneva

and

G. DEBROCK

Catholic University Nijmegen

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PREFACE

This volume contains the *Proceedings* of the International Colloquium "Newton's Scientific and Philosophical Legacy", that was held at the Catholic University of Nijmegen (The Netherlands) from June 9th to 12th 1987 to celebrate the Tercentenary of the publication of Newton's *Philosophiae Naturalis Principia Mathematica* (1667). Although 1987 was a busy year for Newton scholars, we were happy that five of most prominent among them were able to come to Nijmegen and speak on the various aspects of Newton's thought. They are the Professors I. Bernard Cohen (Harvard), Gale Christianson (Indiana State), B.J. Dobbs (Northwestern), Richard H. Popkin (UCLA) and Mordechai Feingold (Boston University). No doubt, recent scholarship has put Newton's genius in a quite different perspective from the one that had come to make up what may be called Newtonian mythology. Although his achievements in the areas of mechanics, mathematics, and optics remain indisputed, Newton's scientific efforts were apparently entirely subordinate to his religious beliefs.

This volume has been divided into four parts, preceded by a Preamble in which Prof. Christianson offers a vivid portrait of Newton as a person. The first part deals with the science of Newton as he himself understood that term. The second part considers the influence of Newton's work on later scientific developments. The third part deals primarily with the question of the methodological influence of Newton, and the last part with his more philosophical legacy. Two editorial remarks are due. The first remark concerns the use of the English language. The editors have chosen to respect the linguistic preference of the authors. Most texts use the American version of English, a few have preferred the British version. The other remark concerns the notes and references. Members of the scientific community have developed their own system which differs significantly from the system used in the Humanities. This difference too has been respected.

All in all, the colloquium lived up to its name. It was a marvelously interesting, lively, and friendly affair, and this was mostly due to the spirit of the participants.

The organization would not have been possible without the help of many people. Thanks are due in the first place to Prof. Westfall who, although unable to attend, kindly offered some golden tips. The Colloquium was made possible through a generous contribution from the Dutch Royal Academy of Science, and a donation by the Benevolentia Fund of the C&A Co. The actual organization was made possible by the tireless assistance of the members of the tiny Department of Philosophy. The major driving force was Dr.ir. Peter Kroes who was always available

and effective in kindly solving any and every problem before, during and after the Colloquium. The participants will also remember the efficient assistance of those who staffed the information desk: Marcel Albers, Lieke van der Scheer en Clementine Hendriks.

Lastly, a special note of thanks is due to Ms. van der Scheer, who not only typed some of the texts, but who saw to many details in the often frustrating process of making a text camera-ready.

The Colloquium was the sixth of a series of colloquia organized by the Department of Philosophy attached to the Faculty of Science of the same university. The aim of those colloquia is to bring together scientists and philosophers to exchange views on a variety of topics which are of common interest. The first (held in the Dutch language) was held to celebrate the Centennial of Einstein's birth. It was followed by colloquia on "New Languages in Scientific Evolution" (in English), "Values and Science" (in Dutch), "Nature, Time and History" (in English), and "The Tao of Nature" (in Dutch). The *Proceedings* of these Colloquia were published in the series of "Nijmegen Studies in the Philosophy of Nature and its Sciences" (obtainable from the Department of Philosophy, Faculty of Science, Catholic University, Nijmegen, The Netherlands).

The Editors

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PREAMBLE

NEWTON, THE MAN

GALE E. CHRISTIANSON

NEWTON, THE MAN - AGAIN

Doing easily what others find difficult is talent; doing what is impossible for talent is genius.
Henri-Frédéric Amiel

Days passed, then weeks, and finally months, during which Edmond Halley, a twenty-eight-year-old natural philosopher of the first order, must have experienced more than his share of uneasy moments. Isaac Newton, it appeared, had forgotten his pledge of August 1684, when Halley had gone up to Cambridge on what now seemed a fool's errand.

It had all started on a bitter winter's day the previous January, while Halley and two others were engaged in animated conversation over drinks before a roaring fire. The astronomer was anxious to get his friends' reaction to his new theory that the inward force of attraction between the planets and the sun must decrease in inverse proportion to the square of the distance between them. Robert Hooke, who has been accused of many things, but never of a loss for words, instantly "affirmed that upon that principle all the Laws of the celestiall motions were to be demonstrated." Sir Christopher Wren, whose forbearance befitted his lofty position as seventeenth-century England's greatest architect, let the agitated Hooke have his say before making it unanimous, stating that he, too, had reached the same conclusion independently, an assertion later affirmed by none other than Newton himself.[1] The problem, of course, lay in finding the mathematical means of proving it. Candidly admitting to his own lack of success, Sir Christopher offered Hooke and Halley a challenge; if either could bring him a convincing demonstration within two months, the victor would have two rewards - the intangible gift of honor and a book worth forty shillings. Impetuous to a fault, Hooke laid preemptive claim to the spoils, "but ... he would conceale [the solution] for some time that others triing and failing, might know how to value it, when he should make it public."[2]

March came and went, taking with it the deadline set by Wren. Spring turned to summer and still Hooke remained mute, leading Halley to observe: "I do not yet find in that particular he has been as good as his word."[3] Finally, in August, Halley, long since having lost patience with Hooke, cast an anxious eye in the direction of Cambridge and made a fateful decision: He would take the coach north to the fenlands and beard

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the lion in his den. The French mathematician Abraham de Moivre, one of a coterie of privileged young men who were given access to Newton's papers once the great man's creative powers had waned, later recounted the dramatic story of the meeting as he said Newton had related it to him:

> in 1684 Dr Halley came to visit him at Cambridge, after they had been some time together, the D^{r} asked him what he thought the curve would be that would be described by the planets supposing the force of attraction towards the sun to be reciprocal to the square of their distance from it. Sr Isaac replied immediately that it would be an Ellipsis, the Doctor struck with joy and amazement asked him how he knew it, why saith he I have calculated it, whereupon Dr Halley asked him for his calculation. Without any farther delay, Sr Isaac looked among his papers but could not find it, but he promised him to renew it, & send it.[4]

Newton, as we know so well, did not go back on his word. Instead, he passed much of the following three months laboring over a nine-page treatise whose origins may have predated Halley's surprise visit. In November 1684, some eleven months after the exchange involving Halley, Hooke, and Wren, he summoned Edward Paget, a Fellow of Trinity College, to his rooms. Placing a copy of the little work titled *De motu corporum in gyrum (On the Motion of Revolving Bodies)* in Paget's hands, he instructed him to carry the packet to Halley in London. Suffice it to say that the astronomer was little prepared for this bolt from the blue, which not only contained a solution to the original problem but far more - the mathematical seeds of a general science of dynamics.

Newton had taken Halley's measure well: tall, dark-eyed, soft of countenance and manner, an altogether pleasing presence to almost everyone, Halley was fully capable of appreciating the magnitude of what he held in his hands. Still, the final test would come when he asked Newton's permission to present the document at an upcoming gathering of the Royal Society. Halley made directly for Cambridge early in December, his thoughts likely racing far ahead of the coach that carried him toward the moment of truth. On the one hand, there was good reason to be optimistic, for why else would Trinity's Lucasian Professor of Mathematics have bothered to send him the paper? On the other, there were disturbing accounts of Newton's intractability when it came to revealing his discoveries to the world. And what of the charges of those, like Hooke, who had spoken with passion of Newton's temper and Janus

face?

Halley returned to London in time to attend the Royal Society meeting of December 10, which coincidentally marked the beginning of diarist Samuel Pepys's two-year tenure as President. A lengthy agenda faced the assembly, and by the time Halley, the last to speak, rose from his chair, a number of the Fellows had doubtless slipped into that sweet torpor which invariably attends such occasions, forcing them to improvise when asked by younger men to recall the historic moment years later. According to the minutes, "Mr. Halley gave an account that he had lately seen Mr. Newton at Cambridge, who had showed him a curious treatise, *De Motu;* which, upon Mr. Halley's desire, ... promised to be sent to the Society to be entered upon their register."[5] Newton had indeed given his answer to Halley, and Halley had dutifully communicated it to the scholarly world. For all the rumors, it appeared that the reclusive and touchy don wanted to taste immortality while still among the living. But what his contemporaries did not yet know is that the author of *De Motu,* thanks to Halley's deferential and informed inquiry, was now in the grips of something beyond even his own understanding. Alone in his shuttered chambers next to Trinity's Great Gate, Newton had entered one of those periods when his will, his likes, and his dislikes were of little consequence. By day and by night, hour in and hour out, never at rest, he was behaving like one dominated by an irresistible force. It would not release him until it had carried him on, a scant eighteen months later, to the supreme intellectual accomplishment of his life - the 550-page treatise, *Principia Mathematica.*

Almost all that is known of Newton's physical appearance as a young man is that he was of medium stature, which is to say about five feet five inches tall, barrel chested, and had turned prematurely gray by the age of thirty. John Wickins, his Trinity chamberfellow of twenty years, half teasingly told him it was "ye Effect of his deep attention of Mind," to which Newton jestingly replied that he experimented so frequently with quicksilver" as from thence he took so soon the Colour."[6] Not until 1689, when the natural philosopher had turned Whig backbencher in the historic Convention Parliament, did he seek out Godfrey Kneller, the German expatriate and principal painter of the day, to capture his likeness in oil.

The first and most appealing of the many paintings we have of Newton, the Kneller portrait was executed a year and a half after Pepys signed the imprimatur guaranteeing that the *Principia* would appear under Royal Society license. Thus, Newton was at the height of his powers and poised on the threshold of international fame. The silver, shoulder-length hair is thick and flowing, the myopic eyes somewhat protuberant but piercing, the angular chin slightly cleft but resolute, the

mouth sensuous and delicately formed. The long slender fingers of the right hand, which extend from beneath the academic gown, are suggestive of a virtuoso of quite another kind, a performer of music rather than the revolutionary choreographer of matter's deterministic dance. In sum, Kneller's midlife portrait is a foreshadowing of that famous Jovian look borne by the aged Newton, who shared with Einstein the aspect of one present at the Creation.

That such a result could have had its antecedents in the wee hours of a cold December morning nearly forty-seven years earlier borders on the surreal. It was Christmas but it was not, at least in that part of England controlled by the Puritan rebels, who had outlawed the holiday in 1642. This included the Lincolnshire village of Colsterworth, across the river Witham from which Isaac Newton was born at the time of the full moon an hour or two after midnight, "the last wonder child," in the words of economist John Maynard Keynes, "to whom the Magi could do sincere and appropriate homage."[7] The infant had arrived several weeks prematurely, and was never to forget his mother's tale of how he "was born so little they could put him into a quart pot." Indeed, so little hope was entertained for his survival that two women who had been sent to obtain some medicine "sate down on a stile by the way." Why hurry, for they were sure the child would be dead before they could get back.[8] Whether he received the medicine is not known, but the infant survived though he was too weak to hold his head upright and was fitted with a bolster as a precaution against self-injury. He was baptized in Hannah Newton's arms exactly one week later, at the beginning of the new year. The Rector of Colsterworth Church recorded the event on the very last page of the register, directly below the names of two less fortunate Newtons. Robert, the infant's grandfather, had been buried on September 20, 1641, while Isaac, the thirty-six-year-old father and namesake, had gone to his grave on October 6, 1642, never having set eyes on his only child.[9]

Hannah married well three years later, taking as her second husband the elderly but very wealthy Barnabas Smith, Rector of North Witham, a village little more than a mile to the southeast. Then she moved away, leaving her doubtless bewildered son at Woolsthorpe in the care of his maternal grandparents, the Ayscoughs. In 1662, while a Cambridge student of nineteen, Isaac underwent a period of intense religious awareness, during which he compiled a list (in shorthand) of fifty-eight sins, most of them innocuous, which he hoped to expiate through the act of self-confession. Number thirteen from the catalog is particularly revealing: "Threatening my father and mother Smith to burne them and the house over them." It was a transgression of long standing, and therefore an unusually vivid memory for one so young, because by this time Barna-

bas Smith had been dead for nine years, and Hannah had long since returned a wealthy widow to the Newton family house. The "sober, silent thinking lad," as Isaac was described by his adolescent companion and self-proclaimed sweetheart Catherine Storer, had a nasty, indeed some would say vicious, unforgiving temper.

Of all the many acts of human frailty that plagued Isaac's conscience, none was cited more frequently than a failure to revere the Creator. He had used the word "God" openly, eaten an apple "at Thy house," failed to turn "nearer to Thee for my affections," and cared "for worldly things more than God." Violations of the Sabbath were among the most common lapses of duty: "Making a feather [pen] while on Thy day; Making a mousetrap on Thy day; Squirting water on Thy day; Swimming in a kimnel [tub] on Thy day."

For one of Newton's puritanical caste, thought was tantamount to deed. The youthful zealot was as much plagued by the possibility of sin - lust, idleness, dissipation - as with specific acts of commission. "Having uncleane thoughts words and actions and dreamese," he wrote. It was with unspeakably immoral fantasies rather than intellectual doubts that the dark powers from beyond the pale would tempt him. Until the very moment of his death, Newton feared that an entire lifetime devoted to moral rectitude and Christian piety could easily go for naught. Should one give in to temptation, no matter if only temporarily and in the abstract, the fall from grace would be unmercifully swift and irreversible.

There is no reason to suppose that Isaac's formal education at Cambridge deviated from the straight and the narrow. Indeed, the fact that Benjamin Pulleyn, a Regius Professor of Greek in the making, had been named his tutor suggests that the youth was as thoroughly indoctrinated in the mysteries of peripatetic philosophy as any of his fellow students, if not more so. The most convincing evidence of this can be found in the commonplace book begun by Newton in 1661, his first year at Trinity College, and continued until his graduation in 1665. Both ends of the little work begin with notes in Greek from Aristotle's *Organon* and *Nicomachean Ethics*.

However at some point in 1663 or 1664, Newton's interest in the master waned. In a manner wholly uncharacteristic of one who so hated to waste paper that his thoughts and equations were frequently jotted on the envelopes of letters from future correspondents, he left dozens of pages blank before initiating a radically different series of entries. Isaac Newton had begun to experience the most revolutionary chapter of his intellectual development. He titled the new section, appropriately enough, *"Quaestiones quaedam Philosophicae"* or "Certain Philosophical Questions." At the top of the first page, in somewhat fainter ink, he penned a

line which summarized to perfection his unshakable commitment to the new order: *"Amicus Plato amicus Aristoteles magis amica veritas."* ("I am a friend of Plato, I am a friend of Aristotle, but truth is my greater friend.")

Newton compiled an index of the various subjects to be investigated, ranging from "Aer [Air]," "Earth," and "Matter" to "Time and Eternity," "Soule," and "Sleepe." Under a number of headings he made no entries at all, a foreshadowing of his later aversion to the formulation of insupportable hypotheses. Under others he wrote from a few sentences to several pages, borrowing much from the works of contemporary thinkers such as atomists Walter Charleton and Pierre Gassendi. Yet the *Quaestiones* themselves were clearly his own and thus provide the earliest evidence that his nascent genius was on the verge of full flower. In this index containing entries from A to Z, one observes the origins of a profound struggle to achieve a principle of unity, a singleness, a model in which the disparate pieces can be synthesized and made whole.

The philosophical notebook also contains much that was spawned from the mind of its author. Desirous of experiencing first hand the effects of highly concentrated light on the eye, he fixed his gaze on a looking glass in whose reflection he captured the image of the sun. "I looked a very little while upon ye sun in a looking-glass with my right eye & then turned my eyes into a dark corner of my chamber & winked to observe the impression made & the circles of colours wch encompassed it & how they decayed by degrees & at last vanished." Growing bolder, he repeated the procedure a second and third time. At length the brilliant, multicolored imprint of the sun overlay every object upon which Newton gazed, from an open book to a drifting cloud. The young natural philosopher was all but blind! "I ... shut myself up in my chamber made dark for three days together & used all means to direct my imagination from ye Sun. For if I thought upon him I presently saw his picture though I was in ye dark." The crisis eventually passed, but months later the spectrum returned, like a blinding chimera, whenever Newton began to meditate on what he had done, "even tho I lay in bed at midnight with my curtains drawn." If he yielded to a sense of rising panic, there is no hint of it among his notes and recollections. When he was able at last to take pen in hand, the various phases of the lingering affliction were duly set down in ten detached steps titled "Starred into the Sun."

A year or so later, while testing the theory that colors result from pressure exerted on the back of the eye, he inserted a bodkin as far back in the socket as he could endure. Then, in a test which sickens one at the thought, he pushed against the eyeball time and again until "several white, darke & coloured circles" appeared. As before, Newton dispas-

sionately related the experimental details as if they were nothing out of the ordinary, when in fact the unrecorded pain must have been excruciating.

He had also become intensely fascinated by celestial phenomena, which included the regular tracking of comets across the starry firmament. The first was observed on December 10, 1664, as he neared his twenty-second birth-day; a second appeared a week later at 4:30 in the morning. Still not satisfied, Newton continued to scan the night skies of winter while the rest of the University slept. These solitary efforts were rewarded a third time when he spied yet another comet streaking through the constellation Andromeda in early April 1665, concerning which the last datable entries in the notebook were made. How is it, he wondered, that these brilliant objects can move with such speed and seeming impunity through the massive vortices of Cartesian space? The answer, of course, is that they could not, and the day came when Newton's equations laid the French giant low.

Even Isaac Newton had his limits, and these he overstepped for the first time in 1664. "He sate up so long ... to observe a comet that appeared then," John Conduitt, Newton's nephew-in-law wrote, "that he found himself much disoriented & learned from thence to go to bed betimes." Disoriented, perhaps, but no one who has become immersed in the Newtonian canon could possibly believe that its creator ever went to bed "betimes."

We may surmise that his affliction was similar to the nervous exhaustion suffered by Newton in 1693 - the so-called Black Year. In the latter instance he wrote to accuse political philosopher John Locke of a fashioning a plot "to embroil me with women" and astounded Samuel Pepys with the assertion "that I must withdraw from your acquaintance, and see neither your nor the rest of my friends any more." Deeply concerned for Newton's sanity, both men responded to his outbursts with admirable equanimity. After he had righted himself, Newton apologized and explained the circumstances surrounding his dark passage to Locke: "When I wrote to you, I had not slept for an hour a night for a fortnight together, & for 5 nights together not a wink." Though he hardly seems to have suffered such extreme deprivation in 1664, the essential cause of his illness would appear to be the same - a protracted lack of rest combined with intense mental strain.

Newton's work habits, both in early life and in middle age, were pathological in their addictiveness. He starved himself socially, sexually, and, not least, nutritionally, courting sickness, failure, even insanity to test whether the established world would break him or whether he would triumph by displacing certain of its time-worn fundaments with new ones

of his own. John North, the eccentric but perceptive Master of Trinity from 1677 to 1683, once remarked to his nephew that "if Sir Isaac Newton had not wrought with his hands in making experiments, he had killed himself with study. A man may so engage his mind as almost to forget he hath a body which must be waited upon and served." As North well knew, it is not enough to say that Newton was an individual of unusually dogged determination. Though one balks at the word, he seems to have been nothing less than a man possessed. In this instance, however, no witch, warlock, or demon had laid claim to a human soul. Newton's possession was self-induced, the result of genius turned sorcerer.

If the philosophical notebook reveals anything, it is this: By 1664 Newton had determined what his life's method of scientific investigation would be. He had already fused the two major strands of modern science, the rational and the empirical. The logical outlook of Descartes is joined with the experimental passion of Bacon and Galileo. A substratum of yet undefined particles and mathematically determinable laws has replaced the classical-medieval world of Aristotelian and Thomistic hierarchies. But at the same time the clockwork precision with which the universe functions is for Newton anything but the result of blind chance. Behind it all he sensed the presence of intelligible planning and purposeful direction. Yet unlike a Kepler or a Leonardo, he seems not to have paused to consider the snowflake on the frost-glazed windowpane, the tulip or the spring rose, the soaring hawk, the course of the tumbling river. His was an obsession with the abstract world of symbol, equation, and geometrical line. Under the heading "Of God" he wrote, "Were men and beasts & c made by fortuitous jumblings of attomes there would be many parts useless in them, here a lumpe of flesh there a member too much. Some kinds of beasts might have but one eye some more y^n two." This amounted to far more than a passing observation; it was a reminder to himself that the underlying unity in matter is a product of the Divine Mind. Atoms there are, and mechanical laws, but when measured against the wisdom and knowledge of the Creator they are as nothing. Hence from the outset of his magnificent quest, Newton, like his intellectual hero Robert Boyle, held that no matter how coherent the world may appear to us, there is nothing below a certain depth that is truly explicable in human terms. Few things would have angered or dismayed him more than the Enlightenment assertion that his *Principia* contained the framework of a universe in which God is no longer a vital, or even necessary, part.

There followed Newton's *anni mirabiles,* the wonderful, indeed seemingly miraculous, years, during which rat-borne plague once again spread across the land and wicked London, as she was branded by the

wailing prophets of Armageddon, burned to the ground under the mark of the beast - 1666. Newton had taken the first important steps toward the calculus in the spring of the previous year by appropriating the remaining blank pages of the deceased Barnabas Smith's commonplace book for his own special purpose. He filled them with problems in the analytical geometry of Descartes and, more important, his own revolutionary mathematics which he termed the fluxions.[22] The first major paper was composed at Cambridge in May 1665, shortly after the University closed on account of the contagion. It dealt with the summation of infinitesimal arcs of curves. A second paper on fluxions and their applications to tangents and the curvature of curves was written at Woolsthorpe in November, and three others followed during 1666, culminating in the brilliant October tract in which he revised and condensed the product of two years' feverish study and analysis.[23] During a visit to the home of cleric Humphrey Babington at nearby Boothby Pagnell, Newton became one of the first to compute accurately the area under a hyperbola. So thrilled was he that he carried the calculations to fifty-two places, an operation that for most would have turned exultation into abject drudgery.[24] At twenty-three the self-taught Newton had become the most advanced mathematician in the world; yet aside from himself, no one knew.

"In August 1665," according to Conduitt, "Sir Isaac bought a prism at Sturbridge fair to try some experiments upon Descartes' book of colours."[25] The prism purchase almost certainly occurred a year earlier than Conduitt had supposed, for descriptions of some of the first optical experiments are contained in the philosophical notebook.[26] By 1665 Newton had already discovered that the color of an object is related to both the nature of its surface and the composition of the light striking it. He also conjectured that the color of the refracted ray is dependent upon its velocity and apparently believed that light is the product of corpuscular motion. A prism acts as a filter to separate slow-moving corpuscles from fast. Most important, he had discovered the varying refrangibility of light - that those rays which produce blue are more sharply bent than those which produce red.[27]

After a hiatus of some months, Newton returned to his optical studies in early 1666. As often happened when he was about to tackle a fresh problem or, as in this instance, to renew his assault on a familiar one, he began a new section in one of his notebooks. As if to emphasize the significance of what he was about, he took the additional step of beginning an entirely new volume. He titled the opening essay "Of Colours," as he had the corresponding section of the earlier *Quaestiones*. In sixty-four carefully drafted steps, he set forth the basic ideas that later

became the substance of his first Lucasian lectures and ultimately Book One of the *Opticks*.[28]

He placed a prism near the hole drilled in a shutter of his darkened chamber. "And holding another Prisme about 5 or 6 yards from ye former to refract ye rays againe I found first y^{t} ye blew rays did suffer a greater Refraction by ye second Prisme than ye Red ones. And secondly y^{t} ye purely Red rays refracted by ye second Prisme made noe other colours but Red & ye purely blew one noe other colours but blew ones."[29] If colors were nothing but modifications of white light by the medium through which it passes, as the prevailing Aristotelian wisdom dictated, the blue rays should not have been refracted more than the red by the second prism. Furthermore, the second prism should have produced other colors by turning red to orange or blue to indigo, but it did not. There was only one conclusion to be drawn: white light is composed of all the primary colors of the spectrum, each of which is immutable. When fully developed a few years later, the *experimentum crucis,* as Newton aptly termed it, so effectively refuted the arguments posed by its critics that it became the supreme symbol - a kind of scientific Holy Grail - of modern empirical analysis.

On April 15, 1726, a year and a half before his death, Newton was visited by his admirer and sometime confidant Dr. William Stukeley at his lodgings in Kensington near London. They dined together and afterward went into the garden to drink their tea under the shade of some apple trees. "Amidst other discourse," Stukeley wrote, "he told me he was just in the same situation, as when formerly, the notion of gravitation came into his mind. It was occasion'd by the fall of an apple, as he sat in the contemplative mood."[30] Voltaire, through whom Newtonian thought was popularized in eighteenth-century France, told a similar story, which he allegedly obtained while bantering with Newton's witty and beautiful niece, Catherine Barton Conduitt.[31] Young Henry Pemberton, a second generation Newton disciple, also talked of gravitation with the natural philosopher toward the end of his life. In Pemberton's account, the garden remains but not the falling apple:

> The first thoughts, which gave rise to his Principia, he had, when he retired from Cambridge in 1666 on account of the plague. As he sat alone in a garden, he fell into a speculation on the power of gravity: that as this power is not found sensibly diminished at the remotest distance from the center of the earth, to which we can rise, neither at the tops of the loftiest buildings, nor even on the summits of the highest mountains; it appeared to him reasonable to conclude, that this

> power must extend much farther than was usually thought; why not as high as the moon, said he to himself? and if so, her motion must be influenced by it; perhaps she is retained in her orbit thereby.[32]

Other than his own tersely written reflections and these less than definitive oral accounts recorded by Newton's admirers, scholars long had little additional information with which to assess the depth or the merit of the early forays into planetary dynamics. Then, in 1953, two previously overlooked holograph manuscripts were discovered in the Cambridge University Library. Newton had recorded his calculations concerning the ratios of the force of gravity to the centrifugal forces generated by the earth's motions on the reverse side of a torn parchment once used by his mother as a lease, additional evidence that this chapter of the *anni mirabiles* was indeed written at Woolsthorpe. The second manuscript built directly upon the conclusions of the first. In it is to be found the very inverse square relationship between the sun and the planets and the earth and the moon that Newton claimed to have discovered in 1666, and that he later recalculated for an excited Edmond Halley.[33]

The next logical step would have involved the calculation of gravitational attraction itself, but for this no papers survive, only the account of the mathematician-cum-theologian William Whiston, who was told by Newton that he was thrown off by the mistaken value of a degree of latitude. This "made Sir Isaac suspect that this Power was partly that of Gravity, and partly that of Cartesius's Vortices." He thus "threw aside the Paper of his Calculation, and went on to other studies."[34] Pemberton concurred: When "his computation did not answer ... expectation, he concluded that some other cause must at least join with the action of the power of gravity on the moon."[35] Reflecting on his early thoughts in a letter to Halley dated June 20, 1686, Newton made no claims beyond what is contained in his surviving papers: Gravity "was but an Hypothesis & so to be looked upon as one of my guesses which I did not rely on."[36] Ah, but what a "guess" it was!

How or when the news of London's destruction reached Woolsthrope is not known. Nor is there any hint of Newton's thoughts on learning of the disaster. The magnitude of what had occurred was beyond the comprehension of even those intimately acquainted with the great city. Still, he had survived the furious charge of the four Horsemen - the civil war of his childhood and, now, the plague and the fire of a world turned upside down. While others predicted the coming of the Last Judgment or spoke in exalted tones of the imminent appearance of the Messiah, modern science had at long last found the great lawgiver it

had been waiting for, a new Moses capable of dispelling once and for all the scholastic myths of the medieval Baals. For even as the physical world seemed to be coming apart at the seams, a secretive young natural philosopher, who described himself as "in the prime of my age of invention,"[37] was beginning to put it back together in a more harmonious fashion than anyone since Pythagoras had dared dream.

In 1936 the Portsmouths, who fell heir to Newton's papers via his niece, sold what remained of their legacy at an auction conducted in the London galleries of Sotheby and Company. It was at this sale that Lord Keynes acquired 57 of the 121 lots of alchemy, which he willed to his own King's College at his death in 1946. After sifting through these many boxes of unique treasure, Keynes composed his oft-quoted essay "Newton, the Man" in commemoration of the three hundredth anniversary of the savant's birth. What Keynes had read caused him to challenge the eighteenth-century conception of Newton as "the first and greatest of the modern age of scientists, a rationalist, one who taught us to think on the lines of cold and untinctured reason." Instead, Keynes cast him as "the last of the magicians, the last of the Babylonian and Sumerians ... who began to build our intellectual inheritance rather less than 10,000 years ago." Why?

> Because he looked on the whole universe and all that is in it *as a riddle,* as a secret which could be read by applying pure thought to certain evidence, certain mystic clues which God had hid about the world to allow a sort of philosopher's treasure hunt to the esoteric brotherhood. He believed that these clues were to be found partly in the evidence of the heavens and in the constitution of elements (and that is what gives the false suggestion of his being an experimental natural philosopher), but also partly in certain papers and traditions handed down by the brethren in an unbroken chain back to the original cryptic revelation in Babylonia. He regarded the universe as a cryptogram set by the Almighty - just as he himself wrapt the discovery of the calculus in a cryptogram when he communicated with Leibnitz. By pure thought, by concentration of mind, the riddle, he believed, would be revealed to the initiate.[38]

The twentieth-century Keynes had seemingly confirmed the eighteenth-century suspicions of Bishop Horsley and the nineteenth-century conclusions of Newton biographer Sir David Brewster. "Interesting, but not useful," was the distinguished economist's assessment of the alchemical

papers, "wholly magical and wholly devoid of scientific value."

As with so many facets of his intellectual development, the turning point in Newton's study of alchemy had come at Trinity College before his swift rise to the ranks of the professoriat. The philosophical notebook contains guidelines on how to make a crucible by coating it with a mixture of "tobaccopipe clay & salt of Tartar."[39] He further paused to contemplate the virtues of "Fier" and its byproduct "Heate," and wrote five pages on "Attomes," seven on "Motion," and three on "Matter" in addition to describing "Attraction," both magnetic and electrical, "Flux & Reflux," "Sympathy and Antipathie," "Density," and "Filtracion." Here was the true seed of the alchemy, if not the flower.

The exact point at which the collector of alchemical data made the transition to avid experimentalist remains unknown, but it could have occurred as early as 1667 and almost certainly not later than 1669. In this endeavor, as in all others, he could not rest until the tracks of his erudition were spread across an endless succession of pages, aiming, in the inspired words of his distant relative and assistant Humphrey Newton, "at something beyond ye Reach of humane Art and Industry."[40] Newton probed the vast corpus of alchemical literature - ancient, medieval, and modern - with a thoroughness never equalled before or since.[41] During the 1680s he compiled a remarkable document known to scholars as the *Index chemicus*. The guide exceeds 100 pages and contains 879 separate headings. No one has yet counted each of the individual page references to the scores of catalogued works, but a figure of 5,000 has been suggested.[42] However, to this pathological hoarder of manuscripts, the printed word was never to be taken as the last word on any subject, alchemy included. Newton had access to a considerable range of unpublished papers, some of which came into his possession either at or very near the outset of his foray into the field. Among the works purchased by Keynes, several are written in different hands, none of which match Newton's or that of his amenuensis-chamberfellow Wickins.[43] He also devised an alchemical pseudonym for himself: *"Jeova sanctus unus,"* an anagram derived from the Latinized *Isaacus Neuutonus*. Whether the anagram was significant to him for reasons other than secrecy is a fascinating question. It could also have been a private declaration that Jehovah is the one and perfect God, a scarcely veiled expression of his deep-seated antitrinitarianism.

Newton's preoccupation with the great alchemical masters from the fabled Hermes Trismegistus to the elusive Michael Maier was an outgrowth of the Renaissance belief in the *prisca sapientia,* or wisdom of the ancients. Most elaborately developed by Marsilio Ficino and Pico della Mirandola, the leading lights of the Platonic Academy at Florence in the

later fifteenth century, the doctrine proclaimed that the great truths of nature had been recorded by the most brilliant and morally upright thinkers of the past. Not surprisingly, this ancient wisdom had been preserved in a veiled and enigmatic form, the better to protect it from the corrupt hands of the vulgar. It was the researcher's sacred duty to decipher the language and rationally interpret the myths, allegories, and esoteric symbols in which the *prisca* was framed. Had not the prophet Isaiah promised enlightenment to the true believer?

> And I will give thee the treasures of darkness, and hidden riches of secret places, and thou mayest know that I, the Lord, which call thee by name, *am* the God of Israel.[44]

As an alchemist and Biblical scholar, Newton lived by this promise. He was well into his eighties when he informed Conduitt, "They who search after the Philosopher's Stone by their own rules [are] obliged to a strict & religious life."[45]

Newton's Puritan sense of mission was nothing if not awesome, and never was it more evident than when he immersed himself in the alchemist's art. But let there be no misunderstanding: he neither heard voices nor considered himself a vessel waiting to receive holy text. Neither was he a mystical lawgiver in the tradition of Pythagoras and Kepler, as some have suggested. To argue otherwise requires him to have set aside his special gifts at alchemy's door, something he would not and, indeed, could not do. "He brought with him standards of intellectual rigor of mathematics, which he applied to his own experimentation. He also brought with him the mechanical philosopher's sense that nature is quantitative."[46]

As ever, Newton appealed to an overarching principle of cosmic order. Any fundamental understanding of nature must be based on the immutability of the ultimate particles that constitute it and of the subtle alchemical spirit present in all bodies, which is the perpetrator of their activity. In one sense he was enthralled by the very questions that so enliven current scientific debate. Black holes, those Dantean pits in the firmament, are fascinating partly because their explanation promises to aid in the unification of the great and the small. The crucial theories of twentieth-century physics are relativity, which is played out across the numbing void of trackless space, and quantum mechanics, which seeks to open up invisible worlds of micromatter. The great problem is to formulate a principle that fuses the two, which has thus far eluded all aspirants, including a deeply disappointed Einstein. Newton, too, sought a grand synthesis, the wedding of the microcosm to the macrocosm to forge an

even more magnificent habitable creation - a *science universelle*.[47] As an alchemist and favorite of the Lord, he attempted nothing less than a "metascientific" synthesis, from the Greek word meaning beyond. That he was destined for supreme disappointment from the outset cannot be doubted. There is no *Principia Chemica* to match the *Principia Mathematica*. We have here another example of a heuristic principle unprovable by empirical standards. Yet we have also a further demonstration of intuitive prescience that would lead Newton to the very threshold of universal gravitation and its attendant concept of force.

"The Alchemical meditatio," wrote Ruland the Lexicographer, "is an inner dialogue with someone who is invisible, as also with God, or with oneself, or with one's good angel."[48] Alchemy has always served as a metaphor for character transformation, and with Newton something more than chemicals and metals was being tested in the crucible. In sum, it is a foolhardy student of the great man and of the seventeenth-century history of science who does not weigh most carefully the dangers of speaking in absolutes. Standing atop the peak of a great intellectual divide, Newton beheld beautiful valleys on either horizon - ancient and modern - and both called him home. Hence Keynes was perhaps less wide of the mark than recent scholarship suggests. Driven, like all first-rate scientific thinkers, Newton did tend to look on the universe as something of a cryptogram fashioned by the Almighty. And there is no denying his belief in and strong attraction to an original wisdom "laid about the world" by God for the benefit of an esoteric brotherhood. This, more than any other consideration, gives the rationalist pause. For Newton's reasoned mechanistic approach to alchemy would seem to have collided head-on with his belief in a transcendental reality.

Newton's Calvinist belief that his was a special mission may well have had its origins in his posthumous birth on Christmas Day and in the memories of his mother's account of a miraculous survival. Evidence that he thought himself among the elect abounds in his actions and in his work. He was nothing if not compulsive; when asked how his discoveries were made, he responded with a Puritan's unselfconscious candor: "By always thinking unto them." Like a prophet in the wilderness, he cut all ties with the outside world and allowed the great outflow of his mind to fulfill the need for personal love felt by those who have no such profound resource. A virtual recluse, he was at home only within the narrow but sanctified confines of his silent study, where the ideas doubled and tripled in volume, like so much mental yeast. As one historian succinctly observed, "Newton was in Cambridge but not of Cambridge."[49] He spurned all but the most superficial collaboration and could rarely force himself to acknowledge the work of another lest such recognition jeopardize the

uniqueness of his own creations. Oh, how he fumed, fretted, and raged on learning of Leibniz's independent formulation of the calculus! The cry of plagiarism went forth, later to be replaced by the ringing indictment "Second inventors count for nothing!" Lesser minds, "little smatterers" he called them, who demanded replies to their facile critiques of his papers trod on hallowed ground. By interrupting his intellectual communion with the ancient ones they disturbed Newton's search of inner unity which he fused into a search for the union of the cosmos. He rewarded them by lashing out in anger or assuming a glacial facade, refusing to acknowledge their existence. To step aside, even for a moment, would require that he expose himself to the fearsome risk of loosing his place forever. As Leon Edel wrote of Henry David Thoreau: "[H]e struggled to keep the parcel of himself from becoming unwrapped and scattered."[50]

Then, too, there were the *anni mirabiles:* at one time the calculus, the optics, and the inverse square law were known to but a single human being of twenty-three - and to God. One wonders how often Newton asked himself why he, above all men, had been blessed with the vision and insight to make such profound discoveries? Except for certain private papers, he preserved every variety of written material imaginable: random jottings, adolescent notebooks, and book-length manuscripts were al treated like Holy Writ. Indeed, the interminable recopying of his papers - five, ten, fifteen, or even twenty times over - may be taken as evidence of self-conscious awe and of a struggle for perfection. Isaac Newton knew well the words of the ancient prophet Isaiah: "Precept upon precept, precept upon precept; line upon line, line upon line; here a little, and there a little." Hidden alchemical truth appealed to him not because of any materialistic yearnings associated with its discovery but because it would serve as his calling card to posterity, providing additional proof of inner grace and favor in the sight of the Lord while guaranteeing him a place among the great minds of history, that life beyond life of which Milton so eloquently wrote.

Turning to Scripture and the fathers of the early Christian Church, Newton found additional answers and a deeper confirmation of his unique state of grace. Denouncing the doctrine of the Trinity and the condemnation of Arius by the Council of Nicaea, he declared the Church of the venerated Virgin a reincarnation of the Bible's supreme symbol of spiritual fornication - the blasphemous Whore of Babylon. Trinitarianism, he wrote, "makes a Church guilty of Apostasy from God as an Adulteress forsakes her husband. It makes her guilty of spiritual whoredom with other lovers. It makes her become the Church of the Idols, false Gods, or Daemons whom she worships, such a Church ... in Scripture is called a Synagogue of Satan."[51] Neither was Anglicanism, the official religion of

his own land, exempt for this imprecation. The scientists and philosophers of the most ancient kingdoms - Babylon, Egypt, Israel, and Greece - were also their chief priests and religious leaders. Eschewing the false gods of polytheism, they became the first true monotheists, or so Newton believed. As agents of God's eternal truth, these intellectual and spiritual giants of old recorded their discoveries and revelations in precious manuscripts to be decoded by a select few from yet unborn generations. In his generation Newton envisioned himself to be just such a chosen interpreter of God's master plan, both natural and divine. He was *homo universalis* from whom nothing discernible to the human mind could long remain obscure. In Newton's way of thinking there was no place for warfare between science and religion as there was in Voltaire's - and in our own. His attempt to seize the key to the pristine truth of ancient times was not only a wonderous scientific enterprise but a religious pilgrimage of the holiest sort, an effort to burst the bonds of his unrequited self.

The need for a unified, believable portrait of Newton has raised a fundamental problem for historians. Ever since the publication of the *Principia* exactly three centuries ago, there have been two Newtons: the man and the god. "Does [Newton] eat & drink & sleep. Is he like other men?" the Marquis de l'Hopital, a noted French mathematician, inquired of Dr. John Arbuthnot, a prominent member of the Royal Society.[52] Similar questions were asked by others during the ensuing decades, and many a true believer, the Marquis included, made the sacred trek to London to gaze upon this living shrine. An awe-stricken Halley, who served so deftly as Newton's publisher, was the first to ponder this question at length. Finally, the astronomer composed a number of verses, which he titled "Ode to Newton," and prefixed to the *Principia*. This, the first major printed assessment of the man, still seems to the Newton biographer standing before you the most cogent:

Then ye who now on heavenly nectar fare,
Come celebrate with me in song the name
Of Newton, to the Muses dear; for he
Unlocked the hidden treasuries of Truth;
So richly through his mind had Phoebus cast
The radiance of his own divinity.
Nearer to the gods no mortal may approach.

Indiana State University, U.S.A.

NOTES

1. *The Correspondence of Isaac Newton,* ed. by H.W. Turnbull, J.F. Scott, A.R. Hall, and Laura Tilling, 7 vols. (Cambridge, England, 1959-77), II, pp. 433-35, 443 (hereinafter cited as I.N. *Corres.).*
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3. *Ibid.*
4. University Library Cambridge, Additional Manuscript 4007, ff. 707^r-707^v (hereinafter cited as U.L.C. Add. MS.).
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6. Keynes Manuscript Collection, King's College Library 137 (hereinafter cited as Keynes MS.).
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8. Keynes MSS. 130(10), p. 1; 130, p. 15.
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10. Richard S. Westfall, "Short-Writing and the State of Newton's Conscience, 1662 (I)," *Notes and Records of the Royal Society of London,* 18 (June, 1963), p. 13.
11. Keynes MS. 136, p. 5.
12. Westfall, "Short-Writing," pp. 13-14.
13. U.L.C. Add. MS. 3996. Newton used several notebooks at Cambridge, each devoted to a different subject: mathematics, theology, alchemy, philosophy, etc. This work has been termed the "philosophical notebook." See Richard S. Westfall, "The Foundations of Newton's Philosophy of Nature," *British Journal for the History of Science,* I, no. 2 (1962), p. 171.
14. I.N. *Corres.,* III, p. 153.
15. U.L.C. Add. MS. 3996, ff. 109^r, 125.
16. U.L.C. Add. MS. 3975, p. 15.
17. U.L.C. Add. MS. 3996, ff. 90^r, 114-16.
18. Keynes MS. 130(10).
19. I.N. *Corres.,* III, pp. 279, 280, 284.
20. Roger North, *the Lives of the Norths,* ed. by Augustus Jessopp, 3 vols. (London, 1890), III, p. 284.
21. U.L.C. Add. MS. 3996, f. 128.
22. U.L.C. Add. MS. 4004. This volume is often referred to as the "Waste Book."
23. U.L.C. Add. MS. 3958.3, ff. 48-63. For a step-by-step analysis of these developments see *The Mathematical Papers of Isaac Newton,* ed. by D.T. Whiteside, 8 vols. (Cambridge, England, 1967-80), I.
24. U.L.C. Add. MS. 4004, f. 14^v, 20^r.

25. Keynes MS. 130(4), p. 9.
26. A.R.Hall, "Sir Isaac Newton's Notebook, 1661-1665," *Cambridge Historical Journal,* 9 (1948), p. 246. Conduitt himself seems to have discovered the fault in his calculations, for he later moved the prism purchase to 1663, which seems to be a year too early.
27. *Ibid.*, p. 249.
28. U.L.C. Add. MS. 3975, pp. 1-20.
29. *Ibid.*, p. 12.
30. William Stukeley, *Memoirs of Sir Isaac Newton's Life,* ed. by A. Hastings White (London, 1936), pp 19-20.
31. Voltaire, *Elémens de la Philosophie de Newton* (London, 1741), p. 289.
32. Henry Pemberton, *A View of Sir Isaac Newton's philosophy* (London, 1728), Preface.
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34. William Whiston, *Memoirs of the Life and Writings of Mr. William Whiston,* 2 vols. (London, 1749), I, pp. 36-37.
35. Pemberton, *A View of Sir Isaac Newton's Philosophy,* Preface.
36. I.N. *Corres.*, II, p. 440.
37. U.L.C. Add. MS. 3968.41, f. 85^r.
38. Keynes, "Newton the Man," pp. 27, 29.
39. U.L.C. Add. MS. 3996, f. 96^v.
40. Keynes MS. 135.
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42. Richard S. Westfall, "The Role of Alchemy in Newton's Career," *Reason, Experiment, and Mysticism in the Scientific Revolution,* ed. by M.L. Righini Bonelli and William R. Shea (New York, 1975), p. 203. With the exception of Index 39, which is in the Yale Medical Library, the remainder of the *Index chemicus* is in Keynes MS. 30.
43. Keynes MS. 67.
44. Isaiah 45:3.
45. Keynes MS. 130, f. 5^v.
46. Richard S. Westfall, *Never at Rest: A Biography of Isaac Newton* (Cambridge, England and New York), p. 294.
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49. Leon Edel, *Henry D. Thoreau* (Minneapolis, 1970), p. 8.
50. Keynes MS. 3, p. 14.
51. Richard S. Westfall, "Isaac Newton in Cambridge: The Restoration University and Scientific Creativity," *Culture and Politics from Puritanism to the Enlightenment,* ed. by Perez Zagorin (Berkeley and Los Angeles, 1980), p. 151.

PART I

NEWTON'S SCIENCE

I. BERNARD COHEN

NEWTON'S THIRD LAW AND UNIVERSAL GRAVITY

Newton's great *Principia* is doubly tripartite. It is composed of three "books" and has three major goals: to set forth new foundations and methods of rational mechanics; to disclose a new natural philosophy; and to develop a new system of the world based on gravitational celestial dynamics. Attention shall be focussed here on the crucial step that enabled Newton to develop and to use the concept of universal gravity and to state its quantitative law. A plausible explanation shall be offered for Newton's daring in proposing a universal force that could extend over hundreds of millions of miles of empty space, a kind of force that in its basic properties went squarely against the principles of the "received" mechanical philosophy.[1]

Early in his career, in the 1660s, when Newton was still in his twenties, he discovered that in a Keplerian system (such as the sun and planets), the quantitative law for the central force – if any – could be derived by substitution and the simplest algebra. All that was needed was a knowledge of Kepler's third or "harmonic" law and the rule for finding the force in uniform circular motion. Kepler's third law, announced in his *Harmonice Mundi* (Linz, 1619), states that the ratio of the square of the periodic time of a planet to the cube of the average distance of the planet from the sun is the same for all planets.[2] For uniform circular motion, the rule is that the force is proportional to $\frac{v^2}{r}$, where v is the speed of the body along the circle and r is the radius of the circle.[3] In order to combine this rule with Kepler's third law it must be assumed that the planetary orbits are circular,[4] in which case Kepler's third law may be stated in the form: $\frac{r^3}{T^2}$ is a constant. Then the simplest kind of algebraic manipulation yields the astonishing result that the solar planetary force is inversely proportional to the square of the distance from the sun to a planet (or is inversely proportional to r^2.)[5] But is there such a solar force acting on planets? And what sort of force can it be?

Like others who were thinking about such problems at that time Newton was confused. We may easily see why. A body that moves in a curved path "tends" to pull away from the center, as almost everyone "knows" from experience. Later on in the century this would come to be known as a "centrifugal force" or "vis centrifuga," following the publications of Christiaan Huygens.[6] Since a body moving on a curve does not

P. B. Scheurer and G. Debrock (eds.), Newton's Scientific and Philosophical Legacy, 25–53.
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fly off away from the center, it would seem that there must be a countervailing force that cancels, nullifies, or balances this outward centrifugal force – in a kind of equilibrium. We shall see that this very misleading view was ultimately directly contradicted by Newton in his *Principia*.

Once the law of centrifugal force had become generally known, after the publication in 1673 of Chistiaan Huygens's *Horologium Oscillatorium,* it occurred to a number of scientists to combine this law with Kepler's third law of planetary motion, just as Newton had done privately almost a decade earlier, to get an inverse-square planetary force.[7] But the problem that no one could solve was how to escape from the confines of circularity, so as to consider elliptical orbits in which planets move according to the law of areas.[8] Chief among those who had been thinking along these lines was Robert Hooke, appointed in 1677 to be Secretary of the Royal Society. Hooke was convinced that there must be a solar planetary force of the inverse-square variety and that a similar inverse-square terrestrial force must act on the moon. Hooke found a number of convincing arguments for such an inverse-square force, but he did not have the mathematical insight and skill to see how to deal with elliptical orbits and the law of areas.[9]

One of Hooke's first steps, on becoming Secretary of the Royal Society in 1677, was to attempt to engage Newton in a private "philosophicall" correspondence. Newton had become estranged from the Society after the controversies that followed the publication of his papers on optics in the early 1670s, subsequent to his famous paper containing his "New Theory about Light and Colors" of 1672.[10] Indeed, as Newton knew only too well, one of the chief objectors to Newton's presentation had been Hooke himself. But Hooke now apparently wanted to make amends, to let bygones be bygones. In a very friendly letter written from London in November 1679, Hooke proposed to engage Newton's attention by presenting to him a problem in dynamics: to determine the trajectory for a freely falling object dropped on a moving earth, it being assumed that the falling object could move freely, without destruction or resistance, through the body of the earth. The problem aroused Newton's interest and he responded, incidentally making a blunder that arose from too hasty an assessment of Hooke's question.[11] In subsequent letters, Hooke enlarged the topics of their discussion to a broader range of fundamental aspects of celestial mechanics.

In the opening letter of the series, on 24 Nov. 1679, Hooke "particularly" asked Newton for his "thoughts" concerning a "hypothesis or opinion of mine" of "compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards the centrall body." That is, Hooke proposed to analyze curved orbital motion into

two independent components: one, a linear inertial or uniform motion along the instantaneous tangent, and the other an accelerated motion of falling toward a center. If the magnitudes of these two separate components are properly harmonized, the rate of falling to the center will be just enough to cause the moving object to deviate from its linear inertial path so as to remain on the curved orbit.

In his reply on 28 Nov., Newton freely admitted that before the receipt of Hooke's letter he had not ever heard ("that I remember") of Hooke's "hypothesis" of "compounding the celestial motions of the Planets of a direct motion by the tangent to the curve." He added that he had not kept up with science ("Philosophy"), a subject which he had "for some years past been endeavouring to bend my self from." Nor had Newton ever encountered Hooke's hypothesis "of the laws & causes of springyness," although he was sure that both were "well known to the Philosophical world."

In subsequent letters Hooke challenged Newton to apply Hooke's analysis of curved motion to the study of planetary orbits. He announced (in a letter of 6 Jan. 1679/80) his "supposition" that "the Attraction always is in a duplicate proportion to the Distance from the Center Reciprocall." That is, that the central force is an "attraction" that varies inversely as the "square of the distance." He added that a consequence would be "that the Velocity will be in a subduplicate proportion to the Attraction and consequently as Kepler supposes Reciprocall to the Distance." In his next letter (17 Jan. 1679/80), Hooke challenged Newton to find the curve based on the Hookian analysis, supposing that the "centrall, attractive power" – drawing off the body from the tangential "equall straight motion" – is as the inverse square of the distance. Hooke said he had no doubt that Newton, with his "excellent method," would "easily find out what that Curve must be, and its proprietys, and suggest a physicall Reason of this proportion." Hooke suggested to Newton that he apply his great mathematical skills to the study of the motion of planets and the moon according to Hooke's mode of analysis, making use also of the proposed concept of an inverse-square force.

Some six years later, when Hooke wanted credit for the priority in "the rule of the decrease of Gravity, being reciprocally as the squares of the distances from the Center," Newton was rightly indignant.[12] This was only an unproved guess on Hooke's part and was a result that Newton himself had obtained more than a decade before 1679 and 1680, when he was corresponding with Hooke on these topics. But Newton did admit in private documents that Hooke had provided the stimulus for him to undertake the study of planetary orbits.[13] Newton apparently did just what Hooke had suggested, and with the insight of his extraordinary genius, he

rapidly moved ahead to solve the most pressing scientific problem of the day. He was able to find a connection between an orbital motion in an ellipse and an inverse-square force. Hooke had actually provided the key in disclosing to Newton the way to analyze curved motion, but Hooke did not have the insight to understand the significance of the law of areas, nor did he have the mathematical genius to invent a new dynamical measure of force – two factors which enabled Newton to invent celestial mechanics and to solve the problem of planetary motions.[14]

Newton, of course, did not communicate his radical solution to Hooke in a letter. Obviously, Newton knew the significance of his accomplishment – how could he not? But, being Newton, and being naturally secretive, he did not at once rush into publication; he did not communicate to the world of science the great new discovery that he had made.[15] Newton's correspondence with Hooke ended abruptly in early 1680. The next act in the drama of unfolding the Newtonian discoveries occurred in the summer of 1684. At this time, in London, there was an active discussion among Sir Christopher Wren (mathematician, astronomer, physiologist, as well as architect), Edmund Halley (geometer and astronomer), and Hooke about the law of force acting on planets moving in orbits according to Kepler's laws. All three had guessed that the force must vary inversely as the square of the distance, but they were all fully aware of the great gap between such an intuitive guess and a scientific proof. Wren and Halley finally admitted that the problem was beyond them, but Hooke insisted that he could solve it. When he failed to bring in a proof, even when stimulated by a rather generous prize offered by Wren, Halley took himself to Cambridge to discuss this problem with that extraordinary genius, Isaac Newton.[16]

On that famous visit in the summer of 1684, Newton told Halley that he had solved the problem of the force in elliptical orbits, that the force in question must be an inverse-square force. When Halley asked him how he knew this, Newton is said to have replied, "I have proved it." Either Newton could not find the papers containing his proof, or the papers were not, in his opinion, in a fit state to be shown to Halley, or perhaps there may have been some other reason; in any event, Newton agreed to write up his results and send them on to Halley. When he did so, Halley was so impressed by the enormity of the advance Newton had made that he hastened back to Cambridge for a second time in order to convince Newton to write up his results fully and to have them "registered" in the Royal Society so that the "priority of his invention" might be guaranteed. Newton complied, and the copy of the statement of his preliminary investigations, entitled *De Motu,* is still preserved in the Register for all to see.[17]

There is some confusion about the steps described thus far. Like most scholars, I assume that the sequence in which Newton develops his ideas in the tract *De Motu,* which are essentially the same as those found later in his *Principia,* corresponds more or less to the actual stages in which Newton formulated and developed his ideas during or after his correspondence with Hooke in 1679-1680, when he made his great discovery. I know of no convincing evidence that this was not the case, but some scholars – chiefly R.S. Westfall – have suggested another possibility.[18] Additionally, we do not know exactly what question Halley asked Newton on that famous visit in 1684. The primary source is a draft by John Conduitt, Newton's successor at the Mint and husband of Newton's niece, who planned to write a biography of Newton. Conduitt is not the most reliable of sources. He does not indicate whether the story he recorded was obtained directly from Newton or from Halley or through a third party, perhaps De Moivre. Furthermore, Conduitt was not very mathematically learned. He relates that Halley asked Newton what the orbit of a planet would be, supposing the force from the sun to be as the inverse square of the distance. It was to this question that Newton is said to have proudly replied that the orbit in question would be an ellipse and that he had proved it. In fact, we are on certain grounds that Newton had actually proved something quite different, namely, that if a planet moves in an elliptical orbit according to the law of areas, then, if there is a force, the force must be as the inverse square of the distance – which is the converse of the proposition supposedly embodied in Halley's question.[19]

I am not sure that Conduitt had the mathematical wit to understand the difference between these two propositions. But Newton was certainly aware that if he had proved that "A implies B" it did not mean that he had also proved that "B implies A." At least one scholar, however, has asserted that Newton did not know the difference between proving this proposition and proving its converse, and that Newton in fact never was able to prove that given an inverse-square force, the resultant closed orbit must be an ellipse.[20]

For an understanding of Newtonian science, an analysis of the stages of Newton's proof are instructive. The sequence leading up to the proof, as given in Prop. 1 of Book 1 of the *Principia*, opens with a proposition in which there are two logical parts. First Newton considers a body moving along a straight line with inertial motion, that is, a body moving uniformly. In every equal interval of time (for instance, in every second), such a body will move through the same distance along the line (Fig. 1). If we pick any point P which is not on the line, then with respect to this point the moving body will sweep out equal areas in equal times, just as

in Kepler's law of areas.

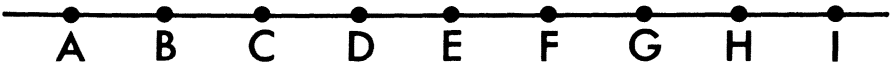

Fig. 1: In the absence of any forces or resistance, a body in motion will continue to move in a straight line uniformly, that is, with constant speed in a straight line. This is the law of inertia. Hence in a succession of equal time-intervals the body will move through the equal distances AB, BC, CD, DE, EF, ...

The reason is simple. A line from the moving body to the point P sweeps out a triangle in each successive equal time-interval. Because the body moves uniformly, or with constant speed, the bases of all of the triangles are equal. Since all the triangles have the same altitude, and since the area of a triangle is one half the altitude times the base, it follows that all of the triangles have the same area (Fig. 2).

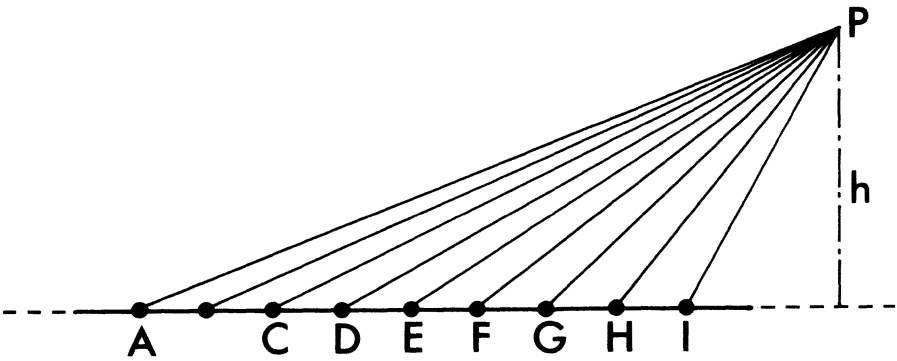

Fig. 2: It follows from the simplest geometry that in inertial motion, that is, motion in a straight line at constant speed, a line drawn from the moving body to any point P (not on the line of motion) will sweep out equal areas in equal times. Such a line is shown in the diagram. Each of the triangles ABP, BCP, CDP, DEP, EFP, ..., swept out in equal times, has the same area. The reason is that they all have the same altitude h and their bases (AB, BC, CD, DE, EF, ...) are – as we have seen – equal.

This remarkable result would have come as a tremendous surprise to readers of Newton's *Principia.*[21] No one until Newton's time had even suspected that there might be a link between Kepler's law of areas and the law or principle of inertia, announced in Descartes's *Principia* some thirty or more years before the publication of Newton's *Principia.* Before Newton, Kepler's law of areas was considered to be a geometrical rule of planetary astronomy without any known or guessed-at physical significance.

The next step in Newton's proof is to suppose that at regular intervals, the body is given a sudden blow or receives an impulsive force, directed toward the point P. The resultant motions, a result of combining the new motion with the motion the body had at the moment it received the blow, will produce a polygonal path, in which the triangles will still all have the same area, as Newton proves by ordinary geometry (Fig. 3).

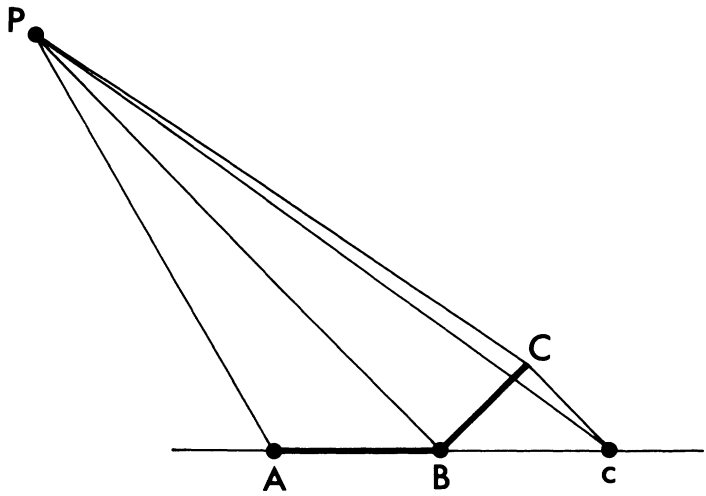

Fig. 3: A body moves, as in figs. 1 and 2, with simple intertial motion. Thus it would move through the equal distances AB, Bc, ... in equal times, sweeping out equal areas with respect to the point P. When the body arrives at B it is given a blow or an impulse (or, it is acted on by an instantaneous force) in the direction toward P. The result is that the body changes direction and now moves along the line BC. Newton proves by strictly geometric considerations that, if the body goes through the distance BC in the time in which it has traversed AB (and would have traversed Bc if it had not received the blow), the area of the new triangle BCP is equal to the area of the original triangles ABP and BcP.

It is characteristic of Newton's method that, once having established a series of geometric relations or geometric truths in a finite case, he then proceeds "to the limit," in this case by allowing the blows or impulses to succeed one another more and more rapidly.[22] The area relation continues to hold; in the limit, as the time between blows becomes vanishingly small, the force becomes continuous and the path becomes a curve (Fig. 4).

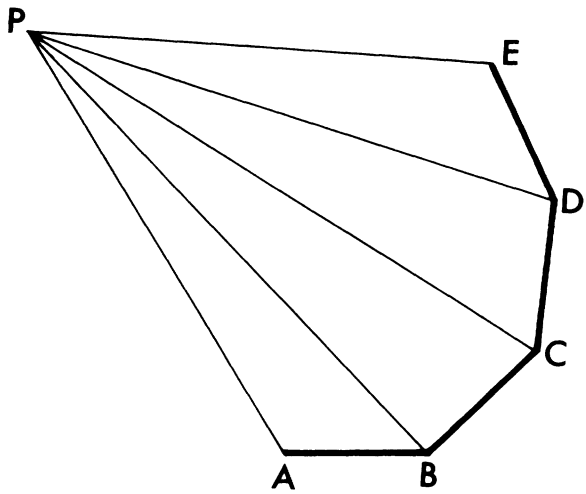

Fig. 4: When the body arrives at C it is again given an instantaneous blow directed toward P and moves from C to D in that same equal time; at D it is again given a blow directed toward P,... These equal areas are described in equal times. Now, says Newton, "let the number of triangles be increased and their width decreased indefinitely." The "ultimate perimeter" ABCDE... will be "a curved line" and "the centripetal force by which the body is constantly drawn back from the tangent will act uninterruptedly" and the areas described will continue to be proportional to the times.

This proof is adapted from Newton's Prop. 1, Theorem 1, of Book 1 of his *Principia*: "The areas which bodies in orbit describe by radii drawn to an unmoving center lie in unmoving planes and are proportional to the times." Prop. 2, Theor. 2, states and proves the converse. In Prop. 3, Theor. 3, Newton deals with Prop. 2 for a center (or central body) that is in motion, as in the case of the earth and its moon. Note that in Props. 1 and 2 the proof is perfectly general in that it does not depend on any particular curve. In the absence of any forces or resistance, a body in motion will continue to move in a straight line uniformly, that is, with constant speed in a straight line. This is the law of inertia. Hence in a succession of equal time-intervals the body will move through the equal distances AB, BC, CD, DE, EF, ...

In the following Proposition Two, Newton reverses the process. Now he shows that in a curved orbit, according to the law of areas, the force must be directed toward the point with respect to which the equal areas are reckoned. Newton, in other words, has shown that a centrally directed force, or a force directed toward a point, is both a necessary and a sufficient condition for the law of areas. In more technical terms, Newton has shown that the area law is a necessary and sufficient condition for the continual deflection of inertial motion in a central force field. We may note that Newton's proof is perfectly general; it is not limited to any specific curve, such as one of the conic sections.[23] At once Kepler's law of areas was transformed from an astronomical rule to a part of rational mechanics.[24]

A few propositions later, Newton introduces *(Principia,* Prop. 6) a new measure of force. This is a "dynamic" measure, depending on the rate with which the tangent to the curve is altered. Armed with these results, Newton (Prop. 11, Section 2) is able to deal with the problem of orbital motion along an ellipse. Prop. 11 makes use of the principles and methods of limits and insofar has a kinship with Prop. 1. That is, Newton sets up a series of geometric relations expressed as proportions or ratios. Then he introduces the limiting process, as before. The result is to prove that in the case where the orbit is an ellipse, the force directed toward a focus must be as the inverse square of the distance.[25]

At this point, a few observations are in order. First, the force in question is a new kind of force, one for which there was no generic name in the physics of Newton's day. Newton named this new force a "vis centripeta," in honor – he said – of Christiaan Huygens, the only one of Newton's contemporaries to whom he ever applied the adjective "summus." Huygens had dealt with an oppositely directed force, the "vis centrifuga."[26]

In the Newtonian analysis, there is no balance between a centripetal force and an alleged centrifugal force. Here there is no condition of equilibrium or steady "state." Rather, there is a centripetal force acting on the body to produce a centrally directed acceleration. The accelerated motion produced by this unbalanced force is a continual "falling" away from the linear inertial tangential path. This centripetal force is not balanced; it is not in equilibrium. The reason is that, according to the principles of Newtonian mechanics expressed in Axioms or Laws 1 and 2, such a force does not produce an equilibrium but a continual acceleration, a continual change in motion.

The Newtonian analysis explains why bodies seem to have a "centrifugal" force. They tend to move out along the tangent, because they have an initial component of linear inertial motion. This motion is directed out

along the tangent and for this reason appears as a tendency to get further from the center. If a ball is whirled around at the end of a string, and the string breaks, the ball does indeed start to fly off on the tangent, thus actually moving further and further away from the hand holding the end of the string. With some other topics, the results just outlined constitute the main thrust of the tract *De Motu* which Newton sent to the Royal Society to be registered, following Halley's visits. There can be no question that Newton's achievement was very great. If he had gone no further, his analysis of elliptical motion was of such a magnitude that it would have been esteemed by his contemporaries as one of the major break-throughs of the age. He had reached a level not attained by Wren or Halley or Hooke, or even Huygens. He had solved the major problem of planetary motion, truly an extraordinary feat. Furthermore, as we have seen, Newton not only had found the law of force acting in elliptical planetary motion; he had also elucidated for the first time the intimate connection between the law of inertia and the law of areas. He had also found the physical significance of all three of Kepler's laws of planetary motion. This was an extraordinary achievement for any scientist of genius, but for a Newton it was only a first step.

We may follow Newton through his subsequent advances by examining the annotations, extensions, and revisions that he entered into one of his own copies of *De Motu*. These enable us to date the next great break-through at January 1685.[27] The thrill and excitement of that great intellectual leap forward released creative forces of a cosmic quality that impelled Newton through eighteen months of creative intensity almost without parallel in the history of thought. In a period of less than two years he produced his great *Principia* and made of physics a modern subject.

In the autumn of 1684 Newton wrote a scholium that illustrates his enthusiasm on his demonstration that in elliptical orbits the force is inversely proportional to the square of the distance. "Therefore," he wrote, "the major planets revolve in ellipses having a focus in the center of the sun; and by radii drawn to the sun describe areas proportional to the times." This, he added, is "entirely [omnino] as Kepler supposed." But by January 1685, Newton had become aware that this simple statement could not square with the facts. He had recognized the complexities that might be introduced by a "displacement of the sun from the center of gravity" and the additional complication of the actions of the planets on one another ("actiones in se invicem"). The planetary universe was not as simple as Kepler had supposed. Newton now found "that the planets neither revolve exactly in ellipses nor revolve twice in the same orbit."[28] "Unless I am much mistaken," Newton concluded, it would be "beyond

the force of the whole human intellect" to produce exact laws of such interactions and to make the necessary orbital calculations. In fact, one of the primary burdens of Newton's *Principia* was to deal with precisely this question. We shall see that its introduction raised the whole subject to higher planes of exactness and understanding than would have been guessed by anyone before the *Principia* appeared.

In the various versions of *De Motu,* Newton makes no direct statement concerning the stages of thought that led him to his extraordinary conclusion. But if, as I have said earlier, we may consider the mode of presentation of the *Principia* to be in parallel with the historical development of Newton's conceptions, we can reconstitute the missing steps from the first complete version of Newton's great work. In the concluding "book" of that first version,[29] Newton explained how it had been the third Law of Motion that had led him to the startling conclusion that we have been examining. In his early analysis, as I have presented it here from *De Motu,* Newton has made use only of the first and second Laws of Motion, respectively the law of inertia and the law relating the change in motion (both in magnitude and in direction) to the external force producing it. Basically, as we shall examine more closely in a moment, Newton has been considering a body that moves about what is essentially a mathematical point, a center of force rather than another body. In nature, however, as Newton was aware, and was to state specifically in the *Principia*, forces do not originate in mathematical points in space but in physical bodies. Accordingly, Newton's analysis of the motion of a planet had been incomplete because he had not considered a second body.

Let us follow Newton's own reasoning. Consider a system of two bodies: for instance, the sun and Jupiter, or the sun and any other planet. Then the third law requires that if the central body (the sun) exerts any force on an orbiting planet, then that orbiting planet must exert an equal and opposite force on the central body. One consequence is, as Newton observed, that no longer will it be the case that one of the bodies moves around a fixed point; rather each of the bodies will move about their respective center of gravity. For most planets, the mass of the sun is so many times greater than the planetary mass that the difference between the center of the sun and the center of gravity of the pair is, to all intents and purposes, nonexistent. But in the case of the sun and Jupiter, this is no longer the case; nor is it at all the case for the system of the earth and its moon.

There is an even greater complication. If the sun pulls on the earth with a celestial force, and the earth pulls equally on the sun with the same kind of force in the opposite direction, then it follows that the earth has two separate roles with respect to this force. The earth is, first of all,

a body on which the force acts, and second, a body in which the force originates. The case is the same for any other planet, say Mars or Jupiter. That each planet is both a body on which the force acts and a body in which the force originates was an astonishing conclusion since it led to the wholly unanticipated principle that each planet must act or pull on every other planet. This phenomenon is known, ever since Newton, as planetary perturbation; its definition and the exploration of its consequences set Newton's *Principia* apart from every other work written prior to 1687. Newton's contemporaries were wrestling with the relatively simple problem of a body moving in orbit about a fixed body without any real success. But Newton had already advanced to the general problem of two bodies freely attracting each other and had even begun to consider the much more general three-body problem and the problem of many interacting bodies. Where his contemporaries could not fully and adequately deal with the grossly oversimplified problem of a fixed sun and a single orbiting planet, Newton had leapt ahead by giant's strides and was working out the mathematical consequences of the perturbations produced in a mutually interacting many-body system.

Newton concluded that one and the same force acts on each of the planets, because the planets are all of the same kind. In the *Principia* Newton made explicit two rules for natural philosophy. The first is not to introduce more causes than are necessary; the second, to assign – as far as possible – the same causes to the same effects.[30] Hence Newton could assert that the earth-sun force is the same as the force between the sun and any other planet. The next step was to identify this planetary force and the force between the earth and the moon and between Jupiter and its satellites and between Saturn and its satellites.[31] The kind of third-law argument advanced for sun and planets holds also for planets and satellites. Thus the earth pulls on the moon and the moon must equally pull on the earth. It is the same for Jupiter and Saturn and their respective moons. The same rules for natural philosophy point to the conclusion that the sun-planet and planet-moon forces are the same.

One of the great moments in Newton's *Principia* is his address to a three-body problem, essentially (but with some simplifications) corresponding to the motion of the moon around the earth, considered as a two-body problem, with the additional perturbations produced by the action of the sun. To see how complex this problem is, we have only to keep in mind that from moment to moment the earth, sun, and moon are in a different geometric relationship one to the other; at the same time the respective distances between these bodies are also changing. Newton's development of this classic three-body problem occupies Proposition 66, with its twenty-two corollaries, making up almost the whole of Section 11

of Book One.[32]

In Book 3 of the *Principia,* where Newton develops the "System of the World" according to gravitational celestial mechanics, Prop. 7 declares the law of universal gravity. "All bodies universally have gravity," he wrote, "which is proportional to the quantity of matter [i.e., mass] in each." He has already proved that gravity acts in the inverse proportion of the square of the distance and that gravity is proportional to mass.[33] Step by step, following the strictest logic and the chain of mathematical proof and deduction, while guided by the inner force of genius, Newton progressed from a solar force acting in planetary motion to the force of universal gravity.

In this progression toward universal gravity, Newton begins with the concept of terrestrial weight, something familiar to us all. Of course we do not know what causes weight, whether it arises from a "vis a tergo" or push from behind (as would be the result of a stream of gravity-producing aether-particles), or whether it comes from "spirits emitted" by bodies, whether it arises from some internal property of a body itself, or whether it is a pulling force originating in the earth. But whatever may be the cause everyone is familiar with the phenomenon of weight. Weight (Latin "gravitas") or terrestrial gravity is the given starting point. Newton then shows, by a neat calculation, that terrestrial gravity may be considered to extend as far as the moon. He does this after proving that a sphere like the earth gravitates as if all its mass were concentrated at its geometric center. He considers the acceleration of fall of a body placed at sixty earth-radii from the earth's center rather than at the earth's surface. Since the force accords with the law of the inverse square, the acceleration produced by the force at a distance of sixty earth-radii is 1/3600th of what it would be on the earth's surface. The acceleration of free fall at the earth's surface is known from direct experiment. Hence it is easy to calculate the magnitude of free fall out at a distance of 60 earth-radii from the earth's center – assuming that terrestrial gravity extends that far. Of course there actually is a body at a distance of 60 earth-radii from the earth's center: the moon. We know how the moon moves and so we can readily compute how the moon actually does fall. The theoretical calculation agrees with the observed motion of the moon to within less than half a percent.[34] Since the force of gravity extends from the earth as far out as the moon, one can conclude that it is this same force which extends from the earth to the sun and from the sun to the planets and from the planets to their satellites.[35] Gravity is a universal force, one that acts on and originates in any sample of matter – it is thus associated with anything that has mass.[36]

In example after example, the *Principia* revealed reasons for believing in universal gravity. They include the explanation of tides in the seas as a result of the combined gravitational action of the sun and moon, the phenomenon observed by Galileo that at one and the same place (save for air resistance) all bodies fall with the same acceleration regardless of their mass or weight, the way in which gravity (or the acceleration of free fall) varies on our earth with latitude, and a great variety of other phenomena. What was perhaps the most spectacular and most convincing argument of all was the explanation of the precession of the earth's axis as a result of the moon's unequal pull on the near and far bulges of an oblate earth spinning on an axis inclined to the ecliptic. Another startling result, though in and of itself a less dramatic application of the Newtonian concept of universal gravity, was the conclusion that comets are "a sort of planet," many of which will move in periodic orbits in ellipses. For many people, the grand prediction of the return of a comet in 1759 was the acid test of universal gravity.[37]

In the first draft the final book of the *Principia*, Newton explained that, "since the action of centripetal force upon the attracted body, at equal distances, is proportional to the matter in this body, it is reasonable, too, that it is also proportional to the matter in the attracting body. For the action is mutual, and causes the bodies by a mutual endeavor (by law 3) to approach each other, and accordingly it ought to be of the same kind in both bodies." He then explained that "One body can be considered as attracting and the other as attracted, but this distinction is more mathematical than natural. The attraction is really that of either of the two bodies towards the other, and thus is of the same kind in each of the bodies." (These translations come from a new English version made by I.B. Cohen and Anne M. Whitman, to be published by Harvard University Press and Cambridge University Press). And hence, Newton concluded, "the attractive force is found in both bodies":

> The sun attracts Jupiter and the other planets, Jupiter attracts its satellites and similarly the satellites act on one another and on Jupiter, and all the planets on one another. And although the actions of a pair of planets on each other can be distinguished from each other and can be considered as twin actions by which each attracts the other, yet inasmuch as they are intermediary they are not two but a simple operation between two ends. By the contraction of one intervening rope two bodies can be drawn to each other. The cause of the action is double, namely the disposition of each of the two bodies; the action is likewise double insofar as it is an action

> upon two bodies; but insofar as it is between two bodies it is simple and unique. It is not, for example, one operation by which the sun attracts Jupiter and another operation by which Jupiter attracts the sun, but one operation by which the sun and Jupiter endeavor to approach each other. By the action by which the sun attracts Jupiter, Jupiter and the sun endeavor to approach each other (by law 3), and by the action by which Jupiter attracts the sun, Jupiter and the sun also endeavor to approach each other. Moreover, the sun is not attracted by a twofold action towards Jupiter, nor is Jupiter attracted by a twofold action towards the sun, but there is one intermediary action by which both approach each other. Iron attracts a magnet just as much as a magnet attracts iron. For any iron in the vicinity of a magnet attracts other iron also. But the action between the magnet and the iron is simple and is considered by natural philosophers as simple; the operation of the iron upon the magnet is the very operation of the magnet between itself and the iron, by which both endeavor to approach each other. This is manifest from the fact that if the magnet is removed, nearly the whole force of the iron ceases. Conceive after this fashion that a simple operation, arising from the joint nature of both, is exerted between two planets, and this operation will be the same with respect to both and thus, being proportional to the matter in one of them, will be proportional to the matter in the other.

Newton could not help but be aware that the general conclusion that "all bodies must attract each other according to this law" is absurd because it is "contrary to experience with terrestrial bodies." Newton's answer was "that there is no experience at all in terrestrial bodies." He then presented the following argument:

> The attractions of homogeneous spheres near their surfaces are as their diameters. And hence a sphere, which is homogeneous to the earth and is described with a diameter of one foot, will attract a corpuscle near its own surface. And a force so tiny will produce no sensible effects. Two globes of this sort only a quarter inch distant from each other would by the force of their mutual attraction approach each other in free spaces in not less than one month. The coming together of smaller globes would be slower in the proportion of the diameters. But not even whole mountains would be sufficient for sensible

> effects. At the foot of a hemispherical mountain three miles high and six miles wide, a pendulum attracted by the force of the mountain will not deviate two minutes from the perpendicular.

In general, therefore, "It is possible to see these forces only in the huge bodies of the planets," as in the case of Saturn and Jupiter at a close approach.

From the time of his *Principia* until his death Newton was deeply troubled by the concept he had introduced: a universal gravity. He had been brought up in the "received" philosophy, sometimes known as the "mechanical philosophy," centering around the ideas of Descartes, which held that all explanations in natural philosophy must be couched in terms of what Robert Boyle called "those two grand and most catholick principles of bodies, matter and motion."[38] At first sight it would seem that to these Newton had added a third principle, force. Not only was this a departure from accepted norms, it also introduced a kind of force that was astonishing in its primary characteristics or qualities. For this force had to have the power of extending itself over many hundreds of millions of miles as a kind of grasping entity which could affect huge bodies. For instance, the gravitational force could extend far beyond the visible reaches of the solar system, that is, way out beyond the planet Saturn, to turn a comet around and cause it to return from outer space to the visible regions of the neighborhood of the sun. Newton again and again sought for some explanation of how universal gravity might act. That is, he attempted to reduce universal gravity to the action of something else: a shower of aether particles, electrical efluvia, or "spirits emitted," variations in the density of an all-pervading aether.[39] All of these attempted "explanations" or reductions of universal gravity to some accepted kind of mechanism failed. None could fulfill two major requirements: that the resultant force vary inversely as the square of the distance and that this force act mutually on every pair of bodies so as to attempt to bring them together.

It is well known that Newton publicly acknowledged the difficulty he and his fellow scientists shared in accepting the action-at-a-distance of universal gravity; in the second edition of the *Principia*, in 1713, he introduced the celebrated General Scholium with its slogan "Hypotheses non fingo." Here he said that he did not know how to explain gravity and he would not "feign hypotheses" in place of sound explanation.[40] By sound explanation he may have meant the reduction of gravitational action to some kind of interaction between some kind of matter and motion, so as to eliminate the need for a force acting nakedly at a distance through

empty space, to have the effects of universal gravity be the result of some kind of contact action. "It is enough," he wrote ("satis est") that this force really ("revera") exists and that it serves abundantly to account for the major phenomena of the heavens and our earth.[41]

Newton's aversion to accepting a force that acts at a distance without any secondary explanatory mechanism was clearly documented. As he expressed the matter in a letter to Bentley, a few years after the *Principia* was first published, no man who thinks rightly in matters of philosophy (that is, science) can possibly believe that a body can act "where it is not."[42] How, then, could Newton have initially been able to consider the properties and actions of a force of "attraction," of a type in which he did not fully believe?

I have attempted to answer this question by systematizing Newton's actual procedures and his statements about his procedures into an approach to mathematical natural philosophy which I have called the "Newtonian style."[43] All the elements of this way of doing mathematical physics have already been presented in the foregoing analysis of Newton's steps toward universal gravity. The starting point is the simplest kind of mental construct: not merely nature simplified, but an imagined construct that is derived from simplified nature. The initial construct consists of a mathematical entity, a mass point (a body which has unit mass and no physical dimensions) that moves freely in orbit about a purely mathematical point which is located in geometric rather than physical space. In this construct Newton may set up and explore any set of mathematically defined conditions that he wishes to explore and derive their mathematical consequences. Since he is dealing with an invented imaginative construct in mathematical space rather than a physical system in the world of external nature, he is free to deal with any kind of imagined conditions or forces without the limiting constraint of physics. Of course, while Newton is developing his purely mathematical principles, he does have his eye on an eventual natural philosophy. This is clear from the fact that in the diagrams accompanying this simple mathematical construct, the central point is apt to be called S and the moving mass point P (for Sol and Planeta). So, too, in a later problem, the central mass point will be labeled T (for Terra) in a construct in which there is a satellite encircling T, subject to a force of perturbation originating in S, obviously suggesting the problem of the motion of the moon.[44]

We have seen that in his first mathematical construct, Newton develops the relation between the law of inertia and the law of areas, shows that the law of areas is a necessary and sufficient condition for inertial motion in a central force field, and then goes on to prove that if

the trajectory or orbit of the moving point is an ellipse (or a parabola, or a hyperbola), the force will vary as the inverse square of the distance. If there should be any doubt that Newton is dealing with a mathematical construct and not with the world of physics, we have only to be aware that Newton knew of no hyperbolic trajectory (or possible hyperbolic trajectory) in the solar system or – for that matter – anywhere in the realm of nature. That Newton is dealing with the realm of mathematics and not physics is also seen in his analysis of elliptical motion in which the force is directed toward the center as well as toward a focus. The same is true of his studies of forces other than those of the inverse square, which obviously do not correspond to any imagined physical situations, observed or possible.

I have called this use of an imagined construct, in which Newton develops mathematically the laws of motion and force, [the] "Stage One" of the Newtonian style. There are many parallels between Newton's imagined construct and the world of external nature. We have noted that the mass point revolves in an elliptical orbit and it does so according to the law of areas. Furthermore, in a system of independent (that is, non-interacting) such revolving bodies or mass points, Kepler's third law applies. But there are many significant divergences of the construct from the world of phenomena, revealed by the senses and mathematical analysis. This confrontation of the construct and the realm of physics I call "Stage Two" of the Newtonian style. That this is, indeed, Newton's style rather than my own attempt to force his reasoning into an alien pattern is made perfectly plain at the outset of Section 11 of Book One. Newton says that thus far he has been dealing with motion resulting from "the attraction of bodies toward an immovable center," or a mathematical point, even though "there is no such thing existing in nature." In the realm of nature "attractions are made toward bodies," not toward points in geometric space. Furthermore, in the physical world, "the actions of the attracted and attracting bodies are always reciprocal and equal by Law 3."

The result of the comparison with the world of physics, in Newton's Stage Two, results in a revised Stage One. In this example just discussed, the revised Stage One is a system in which there are two bodies (still only mass points) revolving about their center of gravity, rather than a construct in which a single body moves about a center of force.

In introducing the action of each of the two bodies on the other, Newton wants the reader to understand that he is concerned "at present" (and "at present" means in Book One of the *Principia)* with the realm of mathematics, with artificial or imagined mathematical constructs and not with physics. Thus, he says, he is "laying aside all physical considera-

tions" and that his aim is to be "easily understood by a mathematical reader."[45]

With the benefit of hindsight we can see that Newton might possibly have avoided some criticism had he not used the word "attraction." But he could no longer use the term "centripetal force," since the force was no longer directed toward a single center, but had been transformed into a mutually acting force arising from and acting on each of a pair of bodies. It would have been awkward, and not especially helpful or necessary to a mathematical reader, to have rather written of "the mutually interacting force arising from and acting upon bodies" or some other such circumlocution.

At the end of Section 11, Newton returns to the distinction between mathematics and physics, between the geometric space of his mental construct and the realm of nature. He explains that he is not at all concerned with "the species or physical qualities of forces," that his only purpose is to investigate their "quantities and mathematical properties" under "any conditions imposed." Later, he says, when "we enter upon physics," we shall compare these mathematical proportions "with the phenomena of nature" and only then may we argue securely "concerning the physical species, causes, and proportions of the forces."

One of the primary results of developing the mathematical laws of a two-body system in the revised Stage One relates to Kepler's laws. He had earlier revealed the physical significance of each of Kepler's laws; now he shows that the laws are strictly or exactly true, that is, mathematically true, only in the realm of the imagination, in an imagined construct in mathematical space, one in which a single body or mass or mass point moves about a mathematical point as center of force. A considerable achievement of the *Principia* is in fact to show exactly what the deviations are, in the world of nature, from this simplified form of Kepler's laws.[46]

After developing the mathematical properties of a two-body system, in the revised Stage One, Newton advances to yet another Stage Two, another comparison and contrast of the mental construct and the world of nature. It is seen that the physical world of our solar system consists of more than two bodies, which must interact mutually. This leads to a new or further revised Stage One, in which there are three bodies. Ultimately, this contrapuntal alternation between the mathematical properties of an imagined construct in a Stage One and the phenomenological aspects of a Stage Two leads to a system more and more like that of the world we see around us. Thus, after dealing with a system of more than two bodies, Newton procedes to bodies which are not mere mass points but have physical shapes and dimensions. First he discusses uniform spherical shells and spheres, then spheres made of successive homogeneous or uniform

shells, that is, bodies with properties ever more like those observed in physical objects such as planets and moons. Eventually, Newton will introduce shapes other than the spherical. In Book Two he advances from motions in empty spaces to motions in several kinds of resisting media.

Books One and Two of the *Principia* develop, in a series of successively revised mathematical constructs, the principles of rational mechanics. In Book Three, subtitled "The System of the World," these principles are applied to physics, to natural philosophy. This is Stage Three, in which the rational mechanics Newton has developed mathematically in Books One and Two will be applied to the solar system. This application to the physical realm of the solar system is – as I have mentioned – the goal to which the whole exercise has been intended. The end-result is to delineate the Newtonian System of the World, functioning on the principles of rational mechanics, in which there is a force of universal gravity directly proportional to the product of the masses of bodies and inversely proportional to the square of the distances between them. This third stage occupies Book Three of Newton's *Principia* and is, of course, the crowning glory of the work.

Let me conclude with some observations on the extraordinary power of the Newtonian style. It freed Newton from physical considerations and restraints in developing the "mathematical principles" which were intended to serve in "natural philosophy." By beginning with the simplest system he was able to proceed securely, introducing complexities one at a time rather than all at once. This style even permitted Newton to discuss and to develop the mathematical properties and laws of a kind of force which – considered philosophically as an entity of physics – he may have found repugnant.[47] We know how strong a restraining force the abhorrence of action at a distance actually was in those days. After the Principia was published, Christiaan Huygens carefully explained that he too might have produced many, if not all, of the mathematical results of Newton's Principia. But, he added, he could not have done so because he had been inhibited by the Cartesian bias that did not permit him even to consider a force acting at a distance.[48] The example of Huygens illustrates the restraining force on creative activity of the received mechanical philosophy and the liberating quality of the Newtonian style.

The concluding General Scholium of the Principia was written essentially as a confession of failure, an expression of regret that Newton had not been able to find an explanation of gravity. He had shown that such a force exists and that it serves to account for an enormous range of phenomena, both celestial and terrestrial. It is enough (satis est), he concluded, that gravity and its laws may lead us to retrodict and to predict "all the motions of the celestial bodies and of our seas" and much else.[49]

It is a paradoxical aspect of Newton's overwhelming genius that even his failures should affect the future course of science. His public expression of his inability to find a "cause" of gravity, and his boast that what he had achieved should be "enough," became eventually the declaration of the aim of natural science. It is today a commonplace that the goal of the sciences is not to find first causes and ultimate explanations, but rather – in the Newtonian manner – to establish a set of principles that enable us accurately and fully to predict and to retrodict the phenomena of the physical world, the results of experiment and critical observation.

Harvard University, U.S.A.

NOTES

1. The mechanical philosophy, associated primarily with the name of Descartes, held that all phenomena of nature were to be accounted for in terms of matter and motion. See Marie Boas's classic memoir, "The Establishment of the Mechanical Philosophy," *Osiris*, 1952, *10*: 412-541, reprinted under the title *The Mechanical Philosophy* (New York: Arno Press, 1981). Also R.S. Westfall: *Force in Newton's Physics* (London: Macdonald; New York: American Elsevier, 1971), especially pp. 377-378.
2. For elliptical orbits Kepler's "average distance" from the sun is the semi-axis major of the ellipse. Newton learned this law on reading Thomas Streete's *Astronomia Carolina* (London, 1661; rev. ed., London, 1710), where (p. 61) it is stated that the mean distances of the planets from the sun are "in sesquialter proportion to the Periods of their Revolution in Time." We may observe that this third law was the only one of Kepler's three laws of planetary motion for which Newton gave credit to Kepler in the *Principia* (2d ed. 1713, 3d ed. 1726, Book 3, Phenomena 1, 2, and 4; in the 1st ed., 1687, these were Hypotheses 5 and 7); see my "Kepler's Century: Prelude to Newton's," *Vistas in Astronomy,* 1975, *18*: 3-40, and my "Hypotheses in Newton's Philosophy," *Physis*, 1966, *8*: 163-184.
3. On Newton's early discovery of this rule or law in the 1660s, see note 6 infra. In the post-Newtonian inertial physics, the post-Principia era, any curved motion – such as motion along a circle at a constant or uniform speed – requires a continually acting accelerating force. The reason is that there is a continual change in the tangential direction of motion.

   One must be careful not to assume that in the 1660s, when Newton discovered the law of force for uniform circular motion, he had a simple and clear idea of "centrifugal" force. (This was decades before he had invented and named the concept of "cen-

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A somewhat different version is scheduled to appear in the *Journal of the History of Ideas,* autumn issue, 1987.

tripetal" force). In the 1660s Newton didn't even have the relatively simple notion of force that was to characterize his later dynamics. At this time he used the expression "force by which a body. . . indeavours from the center," an echo of the Cartesian concept of "conatus." On the state of Newton's thinking about "force" in the 1660s and the development of his ideas, see Westfall's *Force* (cited in ref. 1 supra), ch. 7.

4. With the singular exception of Mercury, the planets have orbits which are very nearly circular. The difference between the ellipse and a circle is shown by the quantity known as the "eccentricity" (defined as the ratio of two distances: the distance between the center and a focus of the ellipse and the semi-axis major). For Mercury the eccentricity is 0.21, which is very large. For the others the eccentricity is: Venus 0.01, Earth 0.02, Mars 0.09, Jupiter 0.05, Saturn 0.06.5.

5. Since the circumference of a circle of radius r is $2\pi r$ and the time to traverse a complete radius is, by definition, the period of revolution T, it follows that the speed $v = \frac{2\pi r}{T}$; whence $v^2 = \frac{4\pi^2 r^2}{T^2}$ or, in the language of proportion, v^2 is proportional to $\frac{r^2}{T^2}$. It requires very little mathematical insight to recognize that the final term in the proportion $(\frac{r^2}{T^2})$ needs only to be slightly modified in order to become $\frac{r^3}{T^2}$, which is a constant for planets in the solar system. It is assumed that orbits are circular. Hence, since $\frac{r^3}{T^2}$ is a constant, it follows that v^2 is proportional to $(\frac{1}{r})(\frac{r^3}{T^2})$ is proportional to $\frac{1}{r}$. But if the "force" is proportional to $\frac{v^2}{r}$ and v^2 is proportional to $\frac{1}{r}$, then the "force" is proportional to $\frac{1}{r^2}$. See, further, my *Newtonian Revolution* (Cambridge: Cambridge University Press, 1980), pp. 229-230; also pp. 238-241. In his early writings on dynamics (1660s and early 1670s), Newton did not use the expression "centrifugal force" or "vis centrifuga," but rather wrote of an "endeavour" to recede from the center, an echo of a Descartes's "conatus recedendi a centro."

Another way of seeing how this law of force may be derived is to consider a planetary orbit as a circle of radius r, with an orbital speed equal to the circumference (distance traversed in one revolution) divided by the period T (the time to complete one revolution). Hence the force (which is proportional to $\frac{v^2}{r}$) will be proportional to $\frac{(\frac{2\pi r}{T})^2}{r} = \frac{4\pi^2 r^2}{T^2} r = \frac{4\pi^2(\frac{r^2}{T^2})}{r}$. Kepler's third law states that $\frac{r^3}{T^2}$ is a constant, say K. Thus if $\frac{r^3}{T^2} = K$, $\frac{r^2}{T^2} = \frac{K}{r}$. It follows that $\frac{4\pi 2(\frac{r^2}{T^2}}{r} = \frac{4\pi 2(\frac{K}{r})}{r} = \frac{4\pi^2 K}{r^2}$. Since $4\pi^2 K$ is a constant, this algebraic manipulation and substitution yields the result that the force is proportional to $\frac{1}{r^2}$ or is inversely proportional to the square of the distance. This modern algebraic formulation

captures the essence of Newton's reasoning, but it must not be supposed that Newton wrote out such equations.

6. Christiaan Huygens: *Horologium Oscillatorium* (Paris, 1673). Newton had discovered the $\frac{v^2}{r}$ law independently, but had not published his result. See John W. Herivel: "Newton's Discovery of the Law of Centrifugal Force," *Isis*, 1960, *51*: 546-553; also Herivel's *The Background to Newton's Principia* (Oxford: Clarendon Press, 1965). Huygens had apparently discovered the law of "conatus" $(\frac{v^2}{r})$ before Newton and had put together the results of his investigations in a manuscript entitled *De Vis Centrifuga* in 1659. This work was first published posthumously in 1703. In the *Horologium Oscillatorium* (1673), Huygens stated his results but did not give any proofs.
7. There is, however, an enormous conceptual gulf between Huygens's formulation and the Newtonian (or post-Newtonian) general expression $F = \frac{mv^2}{r}$. On this difference see H.J.M. Bos in *Dictionary of Scientific Biography*, vol. 6 (New York: Charles Scribner's Sons, 1972), p. 607a. On Newton's transformation of Huygens's "vis centrifuga" into the Newtonian "vis centripeta" and Newton's introduction of the latter term in honor of Huygens (the only one of his contemporaries of whom Newton used the adjective "summus"), see A. Koyre`@ and I.B. Cohen: "Newton and the Leibniz-Clarke Correspondence," *Archives Internationales d'Histoire des Sciences,* 1962, *15*: 63-126, esp. p. 122; see, further, my *Introduction to Newton's 'Principia'* (Cambridge: Harvard University Press; Cambridge: Cambridge University Press, 1971; rev. ed., 1978), p. 53n, pp. 296-297 (suppl. 1, §6).
8. It may have seemed that this step would require only skill in mathematics, but in fact it needed a reformulation of the principles of rational mechanics, which Newton would provide in his *Principia*.
9. There is no satisfactory full-length study of Hooke. On Hooke's dynamics, see Richard S. Westfall: "Hooke and the Law of Universal Gravitation," *The British Journal for the History of Science,* 1967, *3*: 254-261; also Louise Diehl Patterson: "Hooke's Gravitation Theory and its Influence on Newton," *Isis*, 1949, *40*: 327-341; 1950, *41*: 32-45.
10. Published in *Phil. Trans.* no. 80, 19 Feb. 1671/72, pp. 3075-3087. This publication and the printed documents of the ensuing controversy are reprinted in f acsimile in I.B. Cohen and Robert E. Schofield (eds.): *Isaac Newton's Papers and Letters on Natural Philosophy* (Cambridge: Harvard University Press, 1958); revised edition, 1978). See, also, H.W. Turnbull (ed.): *The Correspondence of Isaac Newton* (Cambridge: published for the Royal Society at the University Press, 1959, 1960), vols. 1-2.
11. The Newton-Hooke correspondence is available in vol. 2 of Newton's Correspondence. See Alexandre Koyre`@: "An Unpublished Letter of Robert Hooke to Isaac Newton," *Isis*, 1952, *43*: 312-327; reprinted in Koyre`@'s *Newtonian Studies* (Cambridge: Harvard University Press, 1965). On Newton's diagram and its successive misrepresentation, see J.A. Lohne: "The Increasing Corruption of Newton's Diagrams," *History of Science,* 1967, *6*: 69-89.

    A second topic introduced to elicit Newton's possible "objections" was Hooke's "hythesis of the lawes or causes of Springinesse," the famous "Ut tensio sic vis," that is, "The Power of any Spring is the same Proportion with the Tension thereof."

12. The occasion was the delivery to the Royal Society of the first book of the *Principia*. Halley reported to Newton (22 May 1686; *Correspondence*, vol. 2, p. 433) that Hooke "seems to expect you should make some mention of him in the preface." See my *Introduction to Newton's 'Principia'* (Cambridge: Harvard University Press; Cambridge: Cambridge University Press, 1971, 1978), pp. 52-54, 114-115, 132-135.
13. Many of these are printed in Suppl. 1 to my *Introduction to Newton's Principia*. It is noteworthy that in the second edition of the *Principia*, Newton shifted the corollaries to Prop. 1 of Book 1 to Prop. 2, so that Corol. 1 to Prop. 1 – the most prominent location possible – would be devoted to a proof of the true law of velocity: that "the velocity of a body attracted to an immobile center is inversely as the perpendicular dropped from the center to the straight line which is tangent to the orbit." In Hooke's letter to Newton, stating the inverse-square law, Hooke had alleged that the velocity was inversely as the distance from the center to the planetary position on the orbit" – a relation which holds only at the apsides and is otherwise inconsistent with Kepler's law of areas, a fact of which Hooke was obviously unaware. I believe that Newton's placing of this result in a position of prominence (Corol. 1 to Prop. 1 of Book 1) was a final declaration that Hooke had at best made a successful guess about the inverse-square law but did not have a genuine claim to any of the new results in the Principia.

    A first-rate account of this episode is given in R.S. Westfall: *Never at Rest: A Biography of Isaac Newton* (Cambridge, London, New York: Cambridge University Press, 1980), pp. 381-388. Westfall carefully analyzes all of Hooke's arguments and concludes (p. 387) that Hooke "compunded the confusion by combining his fallacious law of force with Kepler's fallacious law of velocities."
14. The importance of Hooke's teaching Newton how to analyze curved or orbital motions is stressed by Westfall in his *Force* (cited in n. 1 supra), pp. 426-427.
15. Augustus de Morgan once wrote – *Essays on the Life and Work of Newton* (Chicago, London: The Open Court Publishing Company, 1914), p. 39 – that a "discovery of Newton was of a two-fold character – he made it, and then others had to find out that he had made it."
16. For details see my *Introduction* (cited in n. 7 supra), pp. 47 sqq.).
17. See my *Introduction*, pp. 54 sqq. This document is to be found in the Register of the Royal Society, vol. 6, pp. 218 sqq. It has been published by Stephen Peter Rigaud: *Historical Essay on the First Publication of Sir Isaac Newton's Principia* (Oxford: at the University Press, 1838), Appendix No. 1.
18. Chiefly at issue here is whether a certain tract was written before or after the writing of the *Principia*. This tract was alleged by John W. Herivel to contain "The Originals of the Two Propositions Discovered by Newton in December 1679," *Archives Internationales d'Histoire des Sciences,* 1961, *14*: 23-33, an identification challenged by A. Rupert Hall and Marie Boas Hall: "The Date of 'On Motion in Ellipses'," Archives,1963, *16*: 23-28. Herivel's identification was revived by R.S. Westfall: "A Note on Newton's Demonstration of Motion in Ellipses," *Archives*, 1969, *22*: 51-60; see, further, Westfall's *Force* (cited in n. 1 supra), pp. 429-31. Westfall's arguments were refuted by D.T. Whiteside in a review of Westfall's article in *Zentralblatt fr Mathematik und ihre Grenzegebiete,* 1970, *194*: 2-3; see, also, Whiteside's edition of *The Mathematical Papers of Isaac Newton,* vol. 6 (Cambridge: at the University Press, 1974), pp. 14; 553-554. Westfall's summary and rebuttal appears in his *Never at Rest* (cited in no. 14 supra), pp.

387-388 (n. 145). This question of dating is not of primary significance in relation to the topic of the present paper, since – as shall be seen in what follows – Newton's great intellectual leap forward occurred in revising the tract *De Motu* in late 1684 or early 1685, not in the first composition of that work. It is therefore an independent question whether the first draft of *De Motu* represents the state of Newton's thinking in 1679/1680 or a later state, as of 1684.

19. See Newton's own statements printed in Suppl. 1 to my *Introduction (cited in n. 12 supra)*. There is no single curve that answers the question of what the path must be under the action of an inverse-square force: the result can be an ellipse, a parabola, a hyperbola, or a straight line.
20. Robert Weinstock: "Dismantling a Centuries-Old Myth: Newton's *Principia and* Inverse-Square Orbits," *American Journal of Physics,* 1982, *50*: 610-617; for a convenient summary of this dispute, see my *The Birth of New Physics* (second edition, New York: W.W. Norton, 1985), Supplement 13; also my *Newtonian Revolution* (cited in n. 5 supra), pp. 248-249.
21. This result of Newton's, to establish the relation between the law of inertia and the law of areas, is not announced blatantly by Newton in the *Principia*. The reason is, no doubt, that it is but one of the steps leading to the physical significance of the law of areas.
22. In the proof of this proposition (Prop. 1, Book 1) in the *Principia*, Newton writes: "Now let the number of triangles be increased and their width decreased indefinitely. . . ." The difference between the *Principia* and any classic work in Greek geometry is that Newton uses the geometry to establish a set of ratios or proportions, equivalent to a set of equations, and then introduces the limiting process. This character of the Principia is, in fact, declared in the opening Section 1 of Book 1, which is devoted exclusively to the theory of limits.
23. It may be noted that this was also Kepler's procedure. He first established a general law of areas and then the elliptical orbits. We, today, reverse this order and declare the elliptical orbits to be the subject of the first law, so that the law of areas becomes "Kepler's second law."
24. The term "rational mechanics" was introduced into the discourse of mathematical physics in the Preface to Newton's *Principia*.
25. In the succeeding Props. 12 and 13, Newton shows that the inverse-square law holds also for a hyperbola and for a parabola. A corollary to Prop. 13 (in the first edition, 1687) states the converse: that if there is a centrally directed inverse-square force, the orbit will be one of the conic sections. In reply to direct criticism Newton (in the second edition, 1713) sketched out the stages of his proof. See D.T. Whiteside's commentary in vol. 6 of his edition of Mathematical Papers (cited in n. 18 supra), pp. 146-149 (n. 124).
26. See n. 7 supra.
27. There are four known major versions of the tract *De Motu*. As mentioned above (n. 17) S.P. Rigaud first published a version in 1838. A second publication occurred in W.W. Rouse Ball's *An Essay on Newton's "Principia"* (London, New York: Macmillan and Co., 1893 – reprinted, with an introduction by I.B. Cohen, New York: Johnson Reprint Corporation, 1972), pp. 30-56. An edition, with an English translation and commentary, was published by A. Rupert Hall and Marie Boas Hall in their collection, *Unpublished Scientific Papers of Isaac Newton* (Cambridge: at the University Press, 1962), pp. 243-

292, with notes and commentary. The tract, in its several versions, also appears in John H. Herivel: *The Background to Newton's Principia* (Oxford: at the Clarendon Press, 1965), part 2, sect. 9. A final scholarly version and translation, with many valuable historical and analytical notes, appeared in vol. 6 of D.T. Whiteside's edition of Newton's *Mathematical Papers* (cited in n. 18 supra), pp. 30-91. On the relation of the tract *De Motu* to the *Principia*, see my *Introduction* (cited in n. 14 supra), pp. 47-81, esp. 54-62.

28. On these emendations see D.T. Whiteside's comments in *Mathematical Papers,* vol. 6, p. 78 (n. 10) and Curtis Wilson: "From Kepler's Laws, so-called, to Universal Gravitation: Empirical Factors," *Archive for History of Exact Sciences,* 1970, *6*: 89-170.
29. The first version consisted of two "books," corresponding roughly to Book 1 of the final version and a different form of what became Book 3. The text of that early Book 1 has been published by D.T. Whiteside in vol. 6 of *Mathematical Papers*. The early version of what became Book 3 of the *Principia* was published soon after Newton's death in both English and Latin versions under the title *A Treatise of the System of the World* (London, 1728; revised, 1731). See my introduction to the reprint of this work, published in London by Dawsons of Pall Mall in 1969. On these texts see my *Introduction* (cited in n. 14 supra), chapter 4 and Supplements 4, 6.

    Newton explains how he has used the third law on pp. 37-39 of the second edition (London, 1731). He is aware that "it may be objected, that according to this philosophy all bodies should mutually attract one another, contrary to the evidence of experiments in terrestrial bodies." Newton replies that "the experiments in terrestrial bodies come to no account." He shows that by considering actual magnitudes, it is easy to show that one could not observe the mutual gravitational action of one terrestrial body on another, nor the gravitational action of a terrestrial body on the earth.
30. These two rules occur in the introduction to Book 3 of the *Principia*, which is entitled "Liber Tertius: De Systemate Mundi." In the second edition (1713) there were three such rules; a fourth was added in the third edition (1726). In the first edition (1687), the "Regulae" and what later were designated as "Phaenomena" were included under an all-embracing rubric of "Hypotheses." For details, see my article on "Hypotheses in Newton's Philosophy," *Physis*, 1966, *8*: 163-184.
31. In the first edition of the *Principia* Newton made reference only to the single satellite of Saturn discovered in 1655 by Christiaan Huygens. In the later editions he introduced the additional satellites of Saturn discovered by Giovanni Domenico Cassini.
32. On this topic see my historical introduction to *Isaac Newton's 'Theory of the Moon's Motion' (1702)* (London: Dawson, 1975) and the unpublished doctoral dissertation (1975: The Johns Hopkins University) by Craig Waff on *Universal Gravitation and the Motion of the Moon's Apogee: The Establishment and Reception of Newton's Inverse-Square Law, 1687-1749.* Also D.T. Whiteside: "Newton's Lunar Theory: From High Hope to Disenchantment," *Vistas in Astronomy,* 1976, *19*: 317-328.
33. The actual stages to universal gravity from the solar-planetary force to inter-planetary forces to planetary-satellite forces and eventually to universal gravity are somewhat different in the *Principia* – the final sequence in Book 3 starting from the force between Jupiter and its satellites.

34. This proof occurs in Prop. 4, Book 3. In a Scholium Newton considers a hypothetical set of terrestrial moons.
35. In Corol. 1, Prop. 5, Book 3, Newton concludes that "there is gravity toward all planets universally" since "no one doubts that Venus, Mercury, and the rest [of the planets, primary and secondary] are bodies of the same kind as Jupiter and Saturn. And since, by the third law of motion, every attraction is mutual, Jupiter will gravitate toward all its satellites, Saturn toward its satellites, and the earth will gravitate toward the moon, and the sun toward all the primary planets." In a scholium he states: "Hitherto we have called 'centripetal' that force by which celestial bodies are kept in their orbits. It is now established that this force is gravity, and therefore we shall call it gravity from now on. For the cause of the centripetal force by which the moon is kept in its orbit ought to be extended to all the planets, by rules 1, 2, and 4."
36. The succession is as follows.

    Prop. 6: "All bodies gravitate toward each of the planets, and at any given distance from the center of any one planet the weight of any body whatever toward that planet is proportional to the quantity of matter which the body contains."

    Corol. 2 to Prop. 6: "All bodies universally that are on or near the earth are heavy [or gravitate] toward the earth, and the weights of all bodies that are equally distant from the center of the earth are as the quantities of matter in them. This is a quality of all bodies on which experiments can be performed and therefore by rule 3 is to be affirmed of all bodies universally."

    Prop. 7: "All bodies universally have gravity, which is proportional to the quantity of matter in each."
37. More than half of Book 3 of the *Principia* is devoted to the theory of comets.
38. *The Origin of Forms & Qualities,* from *The Works of the Honourable Robert Boyle* (London, 1772), vol. 3, p. 14.
39. On these various attempts to account for gravity see my "The *Principia*, Universal Gravitation, and the 'Newtonian Style,' in Relation to the Newtonian Revolution in Science," pp. 21-108 of Zev Bechler (ed.): *Contemporary Newtonian Research* (Dordrecht, Boston: D. Reidel Publishing Company, 1982) and the important studies by Henry Guerlac, collected in his *Essays and Papers in the History of Modern Science* (Baltimore: The Johns Hopkins Press, 1977). R.S. Westfall has made an extensive study of Newton's thinking on this topic and concludes – in his *Force* (cited in n. 1 supra), p. 377 – that in about 1679 Newton's "philosophy of nature" underwent a redirection of a magnitude that "can scarcely be overstated": "What he proposed was an addition to the ontology of nature. Where the orthodox mechanical philosophy of seventeenth-century science insisted that physical reality consists solely of material particles in motion, characterised by size, shape, and solidity alone, Newton now added forces of attraction and repulsion, considered as properties of such particles, to the catalogue of nature's ontology."
40. It was Alexandre Koyré who first found that "I feign" was used by Newton in English in the same context as "fingo."
41. There are many explanations that have been given of Newton's slogan, "Hypotheses non fingo," which seems to have raised more questions than it solved. The General Scholium does not express simple allegiance to the mechanical philosophy, something Newton had long since abandoned in its oversimplified form (see, e.g., Westfall's *Never*

at Rest, cited in n. 13 supra, pp. 748-749).

In an article on "Occult Qualities and the Experimental Philosophy: Active Principles in the Pre-Newtonian Matter Theory," *History of Science,* 1986, *24*: 335-381, John Henry argues that the English tradition of the mechanical philosophy differed from its Cartesian counterpart with respect to the activity of matter, so that Newton's belief in a variety of "occult qualities" of a non-scholastic kind is more in keeping with the views of his English contemporaries and immediate predecessors than has hitherto been believed.

42. Newton to Bentley, 25 Feb. 1692/93, *Correspondence,* vol. 3. "Tis inconceivable that inanimate brute matter should (without the mediation of something else which is not material) operate upon & affect other matter without mutual contact." Additionally: "That gravity should be innate inherent & essential to matter so that one body may act upon another at a distance through a vacuum without the mediation of any thing else by & through which their action or force may be conveyed from one to another is to me so great an absurdity that I believe no man who has in philosophical matters any competent faculty of thinking can ever fall into it."
43. I have developed this notion of a Newtonian style in the study referred to in note 39 supra and the book referred to in n. 5 supra. My point of view is supplementary to that held by R.S. Westfall, for which see note 39 supra.
44. In Props. 1 and 2 of Book 1, the central force is directed to S. In Prop. 11, on the force in elliptical motion, the center of force at a focus is S and the orbiting body is P. The system corresponding to the sun's perturbation of the moon is represented in Prop. 66 and its many corollaries by T, S, and P, where P stands for "secondary planet" or satellite.
45. Introduction to Sect. 11, Book 1.
46. See my "Newton's Theory vs. Kepler's Theory and Galileo's Theory: An Example of a Difference between a Philosophical and a Historical Analysis of Science," pp. 299-338 of Yehuda Elkana (ed.): *The Interaction Between Science and Philosophy* (Atlantic Highlands, N.J.: Humanities Press, 1974).
47. That is, he admitted, in a letter to Richard Bentley (25 Feb. 1692/3), he had a repugnance to a force which would act at-a-distance through empty space, "so that one body may act upon another at a distance through a vacuum without the mediation of anything else. But whether the "agent" causing gravity "be material or immaterial," Newton "left to the consideration of my readers." See *Correspondence,* vol. 3, p. 254.
48. Christiaan Huygens: *Discours de la Pesanteur* (1690), *Oeuvres*, vol. 21, pp. 472, 474. For a discussion of this topic and a translation of the relevant extracts see Alexandre Koyré: *Newtonian Studies* (Cambridge: Harvard University Press, 1965), pp. 121 sqq. and my *Newtonian Revolution* (cited in n. 5 supra), pp. 79-83.
49. Newton was evidently willing to accept the existence of small-range forces of attraction and repulsion (see n. 39 supra) and, to that extent, had indeed made a fundamental revision of the orthodox mechanical philosophy, whose full implications he abhorred, as Westfall had pointed out in both his Force (see n. 1 supra) and his biography of Newton (see n. 13 supra). But Newton did share the general abhorrence of those who adhered to the orthodox mechanical philosophy when it came to forces that could extend over vast distances, as would have to be the case for universal gravity since it affects the motion of comets way out beyond the visible solar system. Hence (see n.47 supra) he be-

lieved that gravity must act through the mediation of something else – material or immaterial. As John Henry has indicated (see n. 41 supra), Newton was hardly the first person to hold that there could be causes of natural phenomena that remain secret or unknown to us. But Newton did more; he insisted (in the General Scholium, introduced as a conclusion in the second edition of the Principia) that "it is enough" to be able to predict and to retrodict the observed phenomena, even though the active cause or force is unexplainable or is unacceptable according to the canons of a received philosophy. Since gravity "really exists," as we all know, and since Newton's gravity-like force in his final mathematical construct did produce the same effects that we observe in the world of experience, Newton held that it was more important to get on with the business of developing celestial mechanics than arguing to no end about whether a universal gravitating force does (or can) exist. His originality is discerned in his conversion of this conviction into a forcefully expressed public declaration of policy. But we must not forget that, despite Newton's great interest in electricity and in aethereal medium in relation to gravitational action, he was known to have said that universal gravity is caused directly by God.

B.J.T. DOBBS

NEWTON'S ALCHEMY AND HIS 'ACTIVE PRINCIPLE' OF GRAVITATION

Introduction

During his undergraduate years, probably about 1664, Newton accepted the Cartesian postulate of a dense aether, acting mechanically by impact, as the cause of terrestrial gravitation.[1] Manuscripts written during the next twenty years reflect his continuous commitment to some form of mechanical aether as the principle of gravitation, but in those papers Newton sometimes associated the mechanical gravitational aether with the quasi-spiritual "active principle" of alchemy that was responsible for certain non-mechanical natural processes.[2]

However, in 1684 or early in 1684/85, when he was engaged in writing the *Principia,* Newton rejected all forms of a dense mechanical aether as a cause of gravitation, apparently because he had recognized their incompatibility with celestial motions.[3] In 1684, when he derived mathematically the general area law for bodies revolving about a center of force (Theorem 1 in the various versions of *De motu* and then Proposition I, Theorem I of Book I of the *Principia),* he found that his mathematical demonstration matched Kepler's observed area law rather precisely. Within the context of the mechanical philosophy a problem immediately arose: the observed area law of Kepler should *not* fit so closely with the exact area law derived mathematically by Newton if the heavens were filled with the aethereal medium postulated by the mechanical philosophers. Unless the medium is somehow disposed to move with exactly the same variable speed that the planetary body exhibits, the planet should encounter enough resistance from the medium to cause an observed deviation from the mathematical prediction, just as projectiles in the terrestrial atmosphere are observed to deviate from mathematical prediction. In the event Newton made a dramatic break with the orthodox mechanical philosophy of his day, the philosophy that was generally understood among advanced thinkers at the time to be the most promising method of approaching the study of the natural world. He did not reject the entire system of mechanical thought, but he did reject one of its most basic assumptions: that force could be transferred only by the impact of one material body with another.

P. B. Scheurer and G. Debrock (eds.), Newton's Scientific and Philosophical Legacy, 55–80.
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The focus of this paper is on Newton's search for a different sort of causal principle for gravitation during the ensuing twenty-five years. It seems one must recognize that Newton was somewhat dismayed by the fact that a combination of mathematics and observation had forced him to abandon the mechanical causal principle he had accepted for so long. It has often been assumed that he was content to be an early version of a philosophical positivist and to let his mathematical principles stand alone, but a close scrutiny of the surviving evidence from Newton's papers will not sustain that interpretation. Quite the contrary. Newton had always accepted the Renaissance view of history as a declination from an original golden age, a time in which there had existed an original pure knowledge of things both natural and supernatural, a *prisca sapientia* subsequently lost or garbled through human sin and error and through temporal decay. By the time of the writing of the *Principia* in the mid-1680s, Newton had already been engaged for a long time in attempts to restore the original truths once known to mankind by decoding obscure alchemical texts and by searching ancient records for the original pure religion. So when the (for him) modern mechanical explanation of gravity failed, it was only natural that he should turn to ancient sources in an attempt to recapture the truer explanation of gravity once known to the wise ancients. What he found there in antiquity he then incorporated into his cosmic system.

A Cause for Gravity?

In "A Treatise of the System of the World" probably written in 1685, when he still thought the *Principia* would consist of only two books and of which this treatise would be the second, Newton began with material from his work on the origins of gentile theology.[4]

> It was the ancient opinion of not a few in the earliest ages of philosophy, That the fixed Stars stood immoveable in the highest parts of the world; that under the Fixed Stars the Planets were carried about the Sun; that the Earth as one of the Planets, described an annual course about the Sun, while by a diurnal motion it was in the mean time revolved about its own axe; and that the Sun, as the common fire which served to warm the whole, was fixed in the center of the Universe.
>
> This was the philosophy taught of old by *Philolaus, Aristarchus* of *Samos, Plato* in his riper years, and the whole sect of the *Pythagoreans*. And this was the judgment of *Anaximander,* more ancient than any of them, and of that wise king

> of the *Romans, Numa Pompilius;* who, as a symbol of the figure of the world with the Sun in the center, erected a temple in honour of *Vesta,* of a round form, and ordained perpetual fire to be kept in the middle of it.[5]

Not, on this occasion, attempting to define the original religion, Newton nevertheless used his material from antiquity to validate heliocentrism, "the fire at the heart of the world."

But what had the ancients said about gravity? How were the motions of the celestial bodies performed as they moved about the central fire?

He had searched for but had as yet found no satisfactory answer. The most ancient had agreed that the motions "were performed in spaces altogether free, and void of resistance" (just as his own new system required) until the "whim of solid orbs" was introduced by Eudoxus, Callipus, and Aristotle "when the ancient philosophy began to decline." The motion of comets being incompatible with solid orbs, those bodies had been "thrust down below the Moon," but recent observations had restored them to their rightful place "among the celestial bodies" where the Chaldeans had correctly placed them before the Greek "fictions" prevailed. But, he regretfully noted, "we do not know how the ancients explained" the fact that the planets are "retained within any certain bounds in their free spaces" and "drawn off from the rectilinear courses, which, left to themselves, they should have pursued, into regular revolutions in curvilinear orbits." The ideas of later philosophers who "pretend to account for it" are all to be rejected, so he will treat the action of the force responsible for planetary restraint only "in mathematical way" and "call it by the general name of a centripetal force, as it is a force which is directed towards some center."[6]

Statements such as the above, and the famed "hypotheses non fingo" of the *Principia,* are often taken to exemplify Newton's positivistic approach, but that is rather to misread Newton.[7] One can hardly say he was uninterested in finding a causal explanation for gravity. He had for years thought he had had one, a material one, and, when that one failed, he wanted another. In the context of his statement in the "System," it is quite apparent that his "mathematical way" was a less than satisfactory alternative for him, put forward only because he could not determine what the ancients had thought about it and had not himself been able to formulate a good answer either.

There is much more evidence that Newton really did want a causal explanation for gravity; indeed there is a record of his continued search for one. He was even willing for a short time to give some credence to a

new hypothesis of mechanical causation propounded by Nicolas Fatio de Duillier, the young Swiss mathematician who was Newton's friend. Fatio's basic supposition was,

> That the Aether, or such like subtle matter, had a very violent agitation; so as to pass every-way, in great-Circles, about the earth at the rate as to surround the Globe in 1 h. 25'. By which he shewed that a Body driven by the impulse of this motion, would be carried towards the center.[8]

Much given to collecting the endorsements of famous men for his idea, Fatio later reported Newton's approval in these strong terms:

> Sir Isaac Newton's Testimony is of the greatest weight of any. It is contained in some Additions written by himself at the End of his own printed Copy of the first Edition of his Principles, while he was preparing it for a second Edition, And he gave me leave to transcribe that Testimony. There he did not scruple to say *That there is but one possible Mechanical cause of Gravity, to wit that which I had found out:* Thô he would often seem to incline to think that Gravity had its Foundation only in the arbitrary will of God. ...[9]

Although that copy of the *Principia* has never been found, there is reason to think that it did once exist and that Fatio quoted Newton accurately, for in a draft addition to the *Principia,* never used but probably written about 1690, Newton attempted to set the "necessary conditions" for a mechanical explanation of gravity, adding that the "unique hypothesis" by which gravity might be explained that way "was first devised by the most ingenious geometer Mr. N. Fatio."[10]

Gregory reported late in 1691, however, that "Mr Newton and Mr Hally laugh at Mr Fatios manner of explaining gravity."[11] For Newton, Fatio's hypothesis had probably foundered on the problem of the lack of retardation of celestial bodies moving in the subtle fluid. In the fragmentary and tentative addition to the *Principia* in which he praised Fatio, Newton tried to organize the particles of gross bodies into "networks" with pores large enough to allow "the gravitating cause" to operate just as much on the interior particles as on the exterior ones. Otherwise, "gravity would cease to be proportional to the [quantity of] matter." But even though he could create hypothetical "net-like figures" of any desired rarity to allow for the passage of "the gravitating cause" to the interior, still he knew "that in a heaven more filled with matter" solid gross bo-

dies, without pores, "would lose a larger part of their motion ... and even more if they were not solid bodies."[12] The "net-like figures" were required for the free passage of "the gravitating cause" to the interior of bodies. But porous bodies with that sort of structure would experience even more retardation than solid ones, whereas in actuality there was no evidence of retardation at all. The "necessary conditions" for Fatio's mechanical explanation of gravity thus were incompatible with each other. So Newton was left thinking that perhaps "Gravity had its Foundation only in the arbitrary Will of God." Could he find support elsewhere for that radical proposition?

If there were no material aether in the heavens, then what was there? God, of course. "It is true," Newton had written already in the 1660s in his student notebook, "God is as far as vacuum extends, but he, being a spirit and penetrating all matter, can be no obstacle to the motion of matter; no more than if nothing were in its way."[13] God, being spirit, constituted no obstacle to motion; in addition He penetrated all things.

The question of "penetration" had concerned Newton again about 1684 in his alchemical *Commentarium* on the *Emerald Tablet* of Hermes Trismegistus, the handwriting of which is indistinguishable from that of various papers associated with the early stages of the writing of the *Principia.* There, in his commentary, Newton made some revealing corrections in his text, especially revealing if the *Commentarium* was indeed written about the time that he was being forced to make a break with corporeal causality for gravity. Hermes had referred to the penetration of the alchemical active entity (it "penetrates every solid thing" in Newton's translation; *omnemque solidam penetrabit* in his Latin text), and in commenting on that Newton revealed the direction his thoughts were taking. Beginning to write *ob subti* – probably "on account of its subtility" – he cancelled those words (which of course would apply to a subtle corporeal fluid), and, after several attempts at a new formulation, ended by saying *vim penetrantem spiritus* – "the penetrating force of spirit."[14] Only spirit could penetrate and act in the way the evidence seemed to require. And so it was to be for the next three decades that Newton gave preferential consideration to a spiritual cause for gravity. As he summed up the problem in the General Scholium, "the power of gravity"

> must proceed from a cause that penetrates to the very centres of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes used to do), but according to the quantity of the solid matter which they contain. ...[15]

The cause must penetrate, and he had tried to devise a net-like structure for matter in the 1690s to allow for the penetration of Fatio's mechanical aether, but had been forced to conclude that only a spiritual cause could penetrate adequately without constituting a frictional drag on the motions of the planets and comets and producing a slowing down of their motions that was contrary to the phenomena.

Gravity and Ancient Wisdom

The first tentative solution to the problem of a cause for gravity to emerge in Newton's later years, then, was that the omnipresent supreme Deity subsumed gravity directly. There can be no doubt that Newton thought God to be literally omnipresent. As Gregory recorded on 21 December 1705,

> The plain truth is, that he [Newton] believes God to be omnipresent in the literal sense; And that as we are sensible of Objects when their Images are brought home within the brain, so God must be sensible of every thing, being intimately present with every thing: for he supposes that as God is present in space where there is no body, he is present in space where a body is also present.[16]

Henry More and other contemporaries were already treating the Deity as an incorporeal yet three-dimensional Being whose immensity constituted infinite three-dimensional space, and it is possible to see Newton's ideas as the "fruition of a long tradition" extending from Aristotle through Newton, a tradition in which Aristotle's finite plenum was slowly and by painful steps converted into the void, infinite, three-dimensional framework of the physical world required by classical physics. Newton's God-filled space was the penultimate development in the process by which concepts of space were developed by attributing to space properties derived from the Deity; after Newton's time, the properties remained with the space while the Deity disappeared from consideration.[17]

But there were theological complexities of which Gregory seemed unaware when he reported Newton's conviction of God's omnipresence. Simply to describe God as "present" wherever there is no body and also wherever there is body is to give a priority to body and the void that is theologically unacceptable. The Deity must be conceived as prior to the creation, and Newton's conception surely owed something to the theologi-

cal construct of God "as the ground of all being" in which God is the place of the world but the world is not His place. Newton sometimes used the Hebrew word *māqôm* (place) as an expression for God's omnipresence, and in the General Scholium cited many of the Old Testament texts upon which Jewish theologies of space were based.[18] "In him are all things contained and moved," Newton said, citing the passages from Kings, Psalms, Job, and Jeremiah that the rabbis and Jewish philosophers had pondered.[19] In addition he cited texts from "Moses" in Deuteronomy, from John, and from Acts, thus bringing in the "sacred writers" from the most ancient Hebrew authority through Christ himself and earliest Christian antiquity.[20]

The force of ancient wisdom drawn from the sacred writers thus helped form and also helped validate Newton's conception of God's omnipresence, as the force of ancient wisdom drawn from the most ancient natural philosophers helped validate his heliocentrism. But the old knowledge had been fragmented, corrupted, and partially lost: Newton concluded his validating footnote in the General Scholium with a comment on corruption that in effect explained that *all* of the ancients were not to be trusted. "The Idolaters supposed the sun, moon, and stars, the souls of men, and other parts of the world, to be parts of the Supreme God, and therefore to be worshipped; but erroneously."[21] This caution, this qualification, this selectivity with respect to ancient wisdom, accords with and reinforces the impression scholars have long had of a sane and cautious balance in Newton's general methodology: it was only when the various lines of his many investigations converged that he was convinced of the relative validity of his findings. After all, great matters were at stake.

Although undoubtedly Newton's conception of God's omnipresence was richer than that required for a conception of physical space, enriched as it was by a profound appreciation of the religious awe of God's immediacy, ubiquity, and plenitude that the ancients had expressed, the sacred writers had given Newton no assistance in the matter of gravity. It was one thing to let them speak for him and reinforce his own sense of God's presence everywhere. It was quite another thing to ask them *how* God's omnipresence subsumed the mathematical laws of universal gravity he had found. On that point the sacred writers were silent.

But perhaps the ancient natural philosophers were not entirely so. Newton had been talking with Gregory about God's omnipresence in 1705 with respect to possible publication of his ideas in the *Optice* of 1706. "But if this way of proposing this his notion be too bold," Gregory recorded immediately after the passage on omnipresence, "he [Newton] thinks of doing it thus."

> *What Cause did the Ancients assign of Gravity.* He believes that they reckoned God the Cause of it, nothing els, that is no body being the cause; since every body is heavy.[22]

What did Newton mean when he told Gregory that the ancients thought God was the cause of gravity? By late 1705, when he spoke to Gregory about it, he had been studying the issue for over twenty years, for already early in 1685 he had indicated in "A Treatise of the System of the World" that "we do not know how the ancients explained" it. What had he found between 1685 and 1705?

Much of Newton's work on the problem was done in connection with the abortive second edition of the *Principia* that was to have been edited by Nicolas Fatio de Duillier or David Gregory in the 1690s.[23] Fatio first mentioned Newton's efforts in a letter to Huygens in 1691/92,[24] and Gregory noted them in 1695.[25] The principal fruit of Newton's work, the so-called Classical Scholia, were never published by Newton himself. They were, however, designed as scholia for Propositions IV-IX of Book III of the 1687 *Principia.* The manuscripts, in Newton's hand, passed to David Gregory (who used them freely in composing the Preface to his own *Astronomi, physic, & geometric, elementa* of 1702, and they remain now with the Gregory manuscripts in the Library of the Royal Society.[26] Of primary interest here is the scholium for Proposition IX.

"So far I have expounded the properties of gravity," Newton said, referring to the propositions and scholia prior to Proposition IX. "Its cause I by no means recount." The latter statement seems to indicate that in the early 1690s Newton was by no means convinced that the explanation he had by then extracted from the ancients was correct. "Yet I shall say what the ancients thought about this subject," he continued.

> Thales regarded all bodies as animate, deducing that from magnetic and electrical attractions. And by the same argument he ought to have referred the attraction of gravity to the soul of matter. Hence he taught that all things are full of Gods, understanding by Gods animate bodies. And in the same sense Pythagoras, on account of its immense force of attraction, said that the sun was the prison of Zeus. ... And to the mystical philosophers Pan was the supreme divinity inspiring this world with harmonic ratio like a musical instrument and handling it with modulation, according to that saying of Orpheus 'striking the harmony of the world in playful song'. Thence they named harmony God and soul of the world composed of harmonic numbers. ... But the souls of the sun and

> of all the Planets the more ancient philosophers held for one and the same divinity exercising its powers in all bodies whatsoever. ...[27]

As with his work on the origins of gentile theology, Newton's search for ancient opinions on gravity led him to make eclectic use of authorities and to mold them into a coherent whole that spoke to his own purposes. Behind the ancient play on harmony was the Pythagorean-Platonic metaphysics of a recalcitrant world of matter, a chaos, that was divinely ordered and maintained by a "soul of the world composed of harmonic numbers." But the ancient doctrine had passed through many hands, often Christian or Stoic ones, on its way to Newton; thus, the supervisory harmonic function of the pagan demiurge could be read by Newton as "the providence and preservation of God," as Newton's contemporary Thomas Stanley called it.[28] Stanley was actually quoting the pseudo-Plutarchian *De placitis philosophorum,* where the statement was ascribed to Pythagoras himself,[29] and Newton probably read it in the *De placitis philosophorum,* from which he drew material for some of the Classical Scholia.[30]

Since Newton was later to cite Pythagoras in his validating footnote in the General Scholium, it is of some interest to trace the Pythagorean tradition as Newton received it. Justin Martyr had already moved in the direction of a Christianization of Pythagoras in the second century A.D. in a text that became firmly established in patristic literature and continued to be repeated through the eighteenth century. "God is one," Justin had Pythagoras say in what purported to be a direct quotation.

> And He is not, as some think, outside the world, but in it, for He is entirely in the whole circle looking over all generations. He is the blending agent of all ages; the executor of His own powers and deeds; the first cause of all things; the light in heaven; the Father of all; the mind and animating force of the universe; the motivating factor of all the heavenly bodies.[31]

In addition to having the works of Justin himself,[32] Newton had several other works that repeated Justin on Pythagoras.[33] Although we do not know Newton's exact source(s) for his conception of the Pythagorean philosophy, Justin's interpretation may well have informed Newton's views. As Heninger has observed, two conceptions of the Deity are implicit in Justin's words: one, the Deity as all-pervasive spirit infusing the universe; the other, the Deity as workman building and ordering the cosmos.[34] Both were important to Newton.

In conjunction with the Pythagorean-Platonic emphasis on number and world harmony, the vision of God as cosmic worker led to the ancient concept of "the music of the spheres" so fully assimilated later to the dictum from the Wisdom of Solomon that God had ordered everything by number, weight, and measure, and so thoroughly absorbed into Christian aesthetics through Augustine's *De musica.* It was the concept that undergirded Newton's conviction that ancient *prytanea* and temples were "true" geometric representations of God's cosmos;[35] it was the concept that in the 1690s came to support his tentative belief that the ancients had recognized the mathematical laws of gravity and hidden their knowledge in musical "figures." By the early 1700s, in an unpublished draft associated with Query 23 of the *Optice* of 1706, Newton asked his question about the cause of gravity again, but this time gave a somewhat firmer answer.

> By what means do bodies act on one another at a distance? The ancient philosophers who held Atoms and Vacuum attributed gravity to atoms without telling us the means unless in figures: as by calling God harmony representing him & matter by the God Pan and his Pipes, or by calling the Sun the prison of Jupiter because he keeps the Planets in their Orbs. Whence it seems to have been an ancient opinion that matter depends upon a Deity for its laws of motion as well as for its existence.[36]

The cause of gravity was God, acting by harmonious mathematical laws to order and maintain cosmos.

When Newton later cited Pythagoras in his validating footnote to the General Scholium, he thus drew upon a many-textured tradition of thought that stemmed from remotest antiquity and was an integral part of Christendom. "In him are all things contained and moved," he said.

> This was the opinion of the Ancients. So *Pythagoras,* in *cicer. de Nat. Deor.* lib i. *Thales, Anaxagoras, Virgil,* Georg. lib. iv. ver. 220; and Aeneid, lib. vi. ver 721. *Philo Allegor,* at the beginning of lib. i. *Aratus,* in his Phaenom. at the beginning. So also the sacred writers. ...[37]

We have already reflected upon the concepts Newton found in the "sacred writers" regarding the omnipresence of God; we have seen that they gave him no insight into the question of a cause for gravity. We have also found via the draft Classical Scholium to Proposition IX of Book III

of the *Principia* and the draft Query for the *Optice* of 1706 that Newton came to think it probable that one line of ancient thought (associated with Pythagoras) had encoded its knowledge of gravity in musical myths and in talk about divine and celestial harmonies. In their own way, Newton assumed, the mathematical and musical ancients had stated "that matter depends upon a Deity for its laws of motion." But that line of ancient thought had still not explained to Newton exactly *how* the Deity put the laws of motion into effect. There is reason to believe that Newton was not fully satisfied with the musical/mathematical analogy, and when he cited Pythagoras in the Central Scholium (as described in Cicero's *De natura deorum),* Newton was in fact pointing to the other side of Justin's interpretation of the Pythagorean Deity – the Deity as all pervasive spirit (or soul or mind) infusing the universe.

In Book I of Cicero's dialogue *On the Nature of the Gods,* the speaker is Velleius, the proponent of Epicurean theology, and the beliefs of Thales, Anaxagoras, and Pythagoras are cited by Velleius simply in order that he may refute them. Newton, however, apparently ignored the Epicurean critiques of the older views and appropriated instead the reports of the ancient theologies that served his own purposes.

> Thales of Miletus, who was the first person to investigate these matters, said that water was the first principle of things, but that god was the mind that moulded all things out of water. ... Then there is Anaxagoras, the successor of Anaximenes; he was the first thinker to hold that the orderly disposition of the universe is designed and perfected by the rational power of an infinite mind. ... As for Pythagoras, [he] believed that the entire substance of the universe is penetrated and pervaded by a soul of which our souls are fragments. ...[38]

Similarly, in Virgil, in both the *Georgics* and the *Aeneid,* Newton's references point to divine mind, spirit, rational intent, life – all permeating the cosmos. In the *Georgics,* Virgil had this to say:

> Observing these patterns, their intricacy, their justness,
> Some men conclude that bees, as they drink the air,
> Draw in with it a trace of divine intention.
> They say that a spirit fills earth and sky and sea;
> That man and his fellow-creatures, the wild and the tame,
> Take from it the delicate stuff of their life.

> Their is no dying, they believe, when the body fails;
> But, released, the life leaps lightly away to its place
> In the depth of heaven, a brightness pointing the stars.[39]

In the *Aeneid,* the following:

> "First, know, a soul within sustains the heaven
> and earth, the plains of water and the gleaming
> globe of the moon, the Titan sun, the stars;
> and mind, that pours through every member, mingles with that great body."[40]

In both of Newton's references to Virgil, then, one finds that the Deity is everywhere and mingled with everything, and it is of considerable interest for our line of analysis that Virgil is known to have come under Stoic influences,[41] for in a draft intended for the unimplemented edition of the *Principia* in the 1690s Newton himself called attention to "the Stoics" as having had the right ideas.

> ... [T]hose ancients who more rightly held unimpaired the mystical philosophy as Thales and the Stoics, taught that a certain infinite spirit pervades all space *into infinity,* and contains and vivifies the entire world. And this spirit was their supreme divinity, according to the Poet cited by the Apostle. In him we live and move and have our being.[42]

Stoicism had a systematic physics, and of fundamental importance for a fresh understanding of Newton's cosmos is the Stoic concept of *pneuma,* for the *pneuma* as Newton used it proved to be the essential factor linking the divine will with the world of matter. As he indicated in this draft, the Stoics taught him about an "infinite spirit" pervading everything, a spirit that "contains and vivifies the entire world."

One Stoic influence on Virgil, as on Newton himself, was the Stoic poet Aratus, whose didactic poem on the heavens opened with a hymn to Zeus as the all-pervasive, providential Deity.

> The sky is our song
> and we begin with Zeus; for men cannot speak
> without giving Him names: the streets are detailed
> with the presence of Zeus, the forums are filled,

> and the sea and its harbors are flooded with Zeus,
> and in Him we move and have all our being.
> For we are His children, and He blesses our race
> with beneficent signs, and wakes man to his work,
> directing his mind to the means of his life.[43]

Aratus studied under Zeno the Stoic in Athens around 227 B.C., and, though this prologue to his astronomical work is thought to be his own, it is similar in some ways to the Stoic Cleanthes' "Hymn to Zeus."[44] The famous Pauline statement on God as the ground of all being in Acts 17: 27-28 was probably drawn from the prologue of Aratus, for the poem was immensely popular in Hellenistic and Roman circles, and Newton seemed to indicate his knowledge of that filiation in his draft comment of the 1690s when he mentioned "the Poet cited by the Apostle." In the *Principia*'s General Scholium, of course, Newton cited the prologue of Aratus directly: "*Aratus,* in his Phaenom. at the beginning."

An examination of Philo will complete our survey of Newton's tapping of ancient sources to discover and to justify the ideas of the General Scholium. The reference was to Philo's *Allegorical Interpretation* of the Mosaic account of creation. At the beginning of Book I, Philo treats the words of Moses in Genesis 2:1 – "And the heaven and the earth and all their world were completed." Again, Newton selected a passage that focused on mind, for Philo argued that Moses called the mind "heaven." Moses "had already told of the creation of mind and sense-perception," Philo said;

> he [Moses] now fully sets forth the consummation of both. He does not say that either the individual mind or the particular sense-perception have reached completion, but that the original have done so, that of mind and sense-perception. For using symbolical language he calls the mind heaven, since heaven is the abode of natures discerned only by mind, but sense-perception he calls earth, because sense-perception possesses a composition of a more earthly and body-like sort. "World," in the case of mind, means all incorporeal things, things discerned by mind alone: in the case of sense-perception it denotes things in bodily form and generally whatever sense perceives.[45]

Not only did Philo identify "mind" with "heaven," but also he indicated that "world" (κόσμος, cosmos) "in the case of mind" meant "all incorporeal things," and those are almost certainly the passages that attracted

Newton's interest, for what one finds in Philo is a Platonizing Stoicism in which the traditionally material Stoic Deity has been dematerialized – made "incorporeal."

The more orthodox of the ancient Stoics had certainly held everything to be material, including the *pneuma* as Deity, probably in conscious reaction to the philosophical inadequacies of Aristotle's Unmoved Mover.[46] But the cosmic *pneuma,* as active principle, permeated and blended completely with body (conceived as passive principle), the tensional powers of the *pneuma* holding the body together. Stoic insistence upon the corporeality of the cosmic divine *pneuma* had always made the Stoic theory of total blending somewhat problematic. Are corporeal *pneuma* and corporeal body to fuse completely and so become something that is neither God nor matter? Are two corporeal bodies to occupy the same space at the same time? In their origins Stoic arguments on blending more or less avoided those issues by treating the *pneuma* in its motion through matter as a shaping, cohesive force,[47] but another solution to the difficulty was to create a dualistic system of matter and spirit, a Platonizing Stoicism in which the corporeal (but active and divine) Stoic *pneuma* was made incorporeal but still mingled and blended with every body. That was the solution chosen by Philo, by many of the Church Fathers, and by the prominent sixteenth-century Neostoic Justus Lipsius. It was also the solution Newton needed.

Philo Judaeus of Alexandria (c. 25-20 B.C. - c. 50 A.D.) was in some ways probably quite central to Newton's understanding of ancient wisdom. Influenced by both Platonism and Stoicism, Philo provided a synthesis of Hellenistic and Judaic thought by writing commentaries on many Old Testament books and characters (e.g., *On the Account of the World's Creation Given by Moses; On Abraham; On Noah's Work as a Planter)* and on various theological topics (e.g., *On Providence; On the Unchangeableness of God),* and also by allegorical exegesis of Scripture (e.g., *Allegorical Interpretation of Genesis II,* which Newton cited). Modern scholarly opinions on the originality and philosophic importance of Philo have been sharply divergent and contradictory, but there can be no doubt of his historical significance since his writings became a prime source for patristic exegesis and he influenced, among others, Justin Martyr, Clement of Alexandria, Origen, Eusebius, Ambrose, St. John Damascene, Basil the Great, and St. Augustine.[48] Newton owned Philo's *Opera*,[49] and, when he read the works in that volume, he would have been enabled to find Plato and the Stoics in remotest Jewish antiquity. For, although modern scholars understand Philo to have read Platonic and Stoic ideas *into* Scripture, Philo himself and also his Christian followers understood matters quite differently, insisting that the Platonism

and Stoicism came *out of* Scripture itself.[50] Newton, believing in an original pure wisdom, through his reading of Philo thus projected the knowledge of an incorporeal heavenly *pneuma* and divine mind back onto Moses himself.

There were almost certainly other influences in Newton's mental world pushing him toward a Philonic interpretation of Stoicism. Except for Tertullian, all of the early patristic writers had tended to spiritualize the material Stoic Deity (as Supreme God) even while evincing a certain tendency toward materialism with respect to other spiritual beings (such as angels and demons) and allowing for the mingling and penetration of spirit and matter in a way that indicated no sharp dualism in their thought between the spiritual and material realms.[51] Their ambiguity was similar to that of the seventeenth-century alchemists and physicians.[52] But a sharper dualism was present in Neoplatonic and Hermetic writings, and, in the revival of Stoic thought in the Renaissance, Stoic materialism was curtailed as it was Christianized. Most of the interest in Stoic thought in the Renaissance was focused on ethics and moral philosophy, but with Justus Lipsius there was also a full-scale effort to Christianize Stoic physics and metaphysics, Lipsius even using some of Philo's Christian followers as authorities.

Lipsius argued that the supposed materialistic monism of the Stoics was simply a disguised dualism. Drawing upon a distinguished line of Stoic reasoning regarding the two universal principles – active and passive – he made his point quite effectively with quotations from Diogenes Laertius, Seneca, and Cicero. The two principles are variously called, he said: *Efficiens* and *Patiens,* Reason and Matter, Cause and Matter, God and Matter, Force and Matter. But only the active principle should be conceived as eternal. The Stoics had made an error, Lipsius thought, in conferring eternity upon matter itself, and, their material Deity being of necessity joined with passive matter in all of nature, they had thus been led into a materialistic pantheism. Lipsius proposed to resolve these problems by de-materializing the Deity: God was to be conceived as present to the world as an eternal spirit, vivifying, creative, providential; as the eternal Cause and the ground of all being. Lipsius identified the Cause with the seminal reasons or spermatic logoi of the Stoics, that act to inform the passive, sluggish material principle, that from the latter generate the four formed elements of earth, air, fire, and water. Following the *Corpus Hermeticum* and one of the Christian writers influenced by Philo, St. John Damascene, Lipsius argued that God, the Divine Fire, *contains* the forms of all things, the Stoic forms thus becoming very similar to the Platonic Eternal Ideas. Lipsius further interpreted the Stoics who had denied form to the Fiery Breath to have meant only that god

has no *visible* form, and, again basing his argument on the *Corpus Hermeticum,* concluded that God reveals no form to human vision because He is incorporeal. God reveals Himself, however, in the forms of all corporeals. That is what is meant by Christians, Lipsius went on, when they say no one sees God but yet God appears in all things. That is also what the Stoics meant, he thought, when they identified God with nature or with the world. The pantheistic interpretations of Stoic intentions in equating God with the world being unacceptable to Lipsius, he argued away Stoic materialism, and, relying on Augustine to criticize Tertullian's materialism, insisted that God, being incorruptible and incorporeal, cannot be mixed with matter in any real sense (as Stoic theory on total blending had required). The all-pervasiveness of God was to be understood as forming and animating all things, but not as the form or soul itself; as ruling principle of all but blended with nothing. For Lipsius, God is in the world but more than the world. Conversely, the world is not by its own nature God, although the world communicates with and participates in the Divine Intelligence. God is Spirit. Lipsius thought there was little or no difference between the meaning of Spirit and the Stoic terms Reason, Soul, Mind, Intellect – if, that is, the Stoic terms were correctly interpreted – and he expressed his Christianized Stoic convictions in a passage reminiscent of Aratus and the Apostle Paul: *quia nos & omnia vivimus, et movemur, et sumus in Deo.*[53] Or, in King James' English, "In him we live and move and have our being" – the very point for which Newton cited Paul and "the Stoics" in the 1690s and Paul and the Stoic Aratus in the General Scholium.

On more than one specific point Newton seems to follow Lipsius. Some Stoics placed the center of the divine creative fire in the sun itself, "the fire at the heart of the world." Later Stoics often placed the divine creative fire in the *pneuma* or aether, however, and in the sixteenth century Lipsius said that the distinction between sun and aether as the seat of the divine active principle was not a significant one.[54] Since the *pneuma* was conceived as penetrating all things in any case, the Lipsian point of view had much to recommend it, and Newton seemed to echo it in the draft Scholium to Proposition IX when he attributed to "the more ancient philosophers" the idea that "the souls of the sun and of all the Planets" were "one and the same divinity exercising its powers in all bodies whatsoever."[55]

The Philonic and Lipsian points of view evidently recommended themselves to Newton in general, and Newton's own views on the divine *pneuma* in the post-*Principia* period resemble their approaches much more than they do those of most of the ancient Stoics. We cannot be certain that Newton read Lipsius' *Physiologiae stoicorum,* but he did read

Philo. And since the work of Lipsius depended at least in part on Philonian influences, perhaps a direct reading of Lipsius would have been superogatory. Newton would in any case have preferred the more ancient authority as being closer to the fount of truth. The tenor of Philonic Stoicism was to shift from a materialistic monism toward a Platonic dualism, within which there existed a distinction between spirit and matter that was quite foreign to most of the ancient Stoics. Under the dispensation of a Platonizing Stoicism with its incorporeal Deity as the active principle, Newton was able to use the Stoic concept of the Deity as a tensional force, binding the parts of the cosmos together, penetrating and mingling with all bodies, and that without the inconveniences attendant upon the corporeal *pneuma* or aether.

Conclusion

In the matter of gravity, Newton had recognized that no material, mechanical cause would serve and had been forced to make a break with corporeal causality. The evidence he had in hand denied the presence of the corporeal aether. Yet gravity acted, and it seemed to act as if it penetrated to the very centers of bodies. Only spirit could penetrate in that way without constituting a frictional drag by acting on the surfaces of bodies. In the context of Stoic thought, as interpreted by Philo and Lipsius, the all-pervasive spirit was of course the active principle, acting everywhere to penetrate and bind the passive principle of matter. The conceptualization of gravity as active principle, as subsumed by the literal omnipresence of God, as a spiritual force binding all together, was to serve Newton for many years. Stoic the idea was certainly, but Newton used it in its Platonizing version, in which the Deity was wholly immaterial, non-corporeal, yet all-pervasive.

With the important exceptions of Sambursky and Kubrin,[56] historians of science have largely ignored the affinities between Newton's doctrines and Stoic thought, but classicists have not been so negligent. Cicero made explicit the concept of the Deity as tensional force at the heart of the world and of all the matter in the world as he described Stoic philosophy in Book II of his *De natura deorum,* a work Newton certainly read.

> For all its [the world's] parts in every direction gravitate with a uniform pressure towards the centre. Moreover all bodies conjoined maintain their union most permanently when they have some bond encompassing them to bind them together;

> and this function is fulfilled by that rational and intelligent substance which pervades the whole world as the efficient cause of all things and which draws and collects the outermost particles towards the centre.[57]

In the modern Loeb Classical edition, from which this translation is drawn, the concept is considered to be so explicit, in fact, that the word Newton coined for his central attractive force, "centripetal," is used to gloss Cicero's words. Indeed, Newton's word has frequently been used to gloss the Ciceronian text at least since the middle of the nineteenth century,[58] and the concept, if not the word "centripetal," had a lengthy pre-Newtonian history. Hunt has argued that Zeno, the founder of Stoicism, probably explained the coherence of the cosmos by "centripetal force" (Hunt using Newton's word anachronistically) and that Stobaeus recorded a similar idea.[59] In his commentary on Lucretius, *De rerum natura,* I, 1052-82, Bailey attributed similar views to Parmenides and Plato as well as to the Stoics (including Chrysippus and the Ciceronian passage quoted above). Lucretius had undertaken to refute the idea, since his own infinite universe could have no center, and Bailey somewhat sadly observed, "This doctrine contained of course the germ of the theory of gravitation, and it is rather pathetic to see Lucr. rejecting an idea which is now universally accepted. ..."[60] There is, in addition, considerable evidence that the concept continued to be a living force in European intellectual life from the classical period until Newton's own time.[61]

In 1706, in what was then Query 23 of the *Optice* and what became Query 31 of the 1717/18 *Opticks,* Newton openly designated gravity as an active principle in his most general statement on the forces associated with matter, the particles of which he described as "solid, massy, hard, impenetrable, [and] moveable."

> It seems to me farther, that these Particles have not only a *Vis inertiæ,* accompanied with such passive Laws of Motion as naturally result from that Force, but also that they are moved by certain active Principles, such as is that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies.[62]

Within the context of Stoic doctrine, the divinity of Newton's active principles now seems perfectly obvious. Defined against the passive principle of matter, they are the eternal Cause, *Efficiens,* Mind, Reason, Force.

Although both Westfall and the present writer have argued that Newton came to view gravity as an active principle by analogy with the active alchemical agent,[63] that story no longer seems quite so straightforward. Active and divine the alchemical agent was certainly; in the *Emerald Tablet* Hermes Trismegistus even spoke of its quality of "penetration." Indeed, alchemical thought carried much Stoicism into the seventeenth century, but the alchemical agent was focused on micromatter. It was "that which causes Fermentation, and the Cohesion of Bodies," Newton said in the *Opticks,* and during most of the post-*Principia* period he apparently did not think he could make a direct analogy between it and the cosmic gravitational cause because he conceived the alchemical vegetable spirit as at least quasi-material and so subject to the same disabilities as his early mechanical versions of a material aether as far as gravity was concerned. He was later to change his mind, but not until after Hauksbee's electrical experimentation suggested a new interpretation to him about 1710.[64] Between 1684 and 1710 Newton's conceptualization of the "cause" of gravity was probably more directly mediated by ancient cosmic thought and especially by the Platonizing version of ancient Stoicism than by alchemical doctrines of an active principle at work in the realm of micromatter. The analogies at work in the evolution of Newton's thought on the cause of gravity would seem to have been cosmic ones based on the musical harmonies of ancient Pythagorean-Platonic metaphysics and on the Divine Mind of Presocratic and Stoic thought.

When Newton spoke to Gregory about these matters late in 1705, Gregory recorded Newton's analogy: "as we are sensible of Objects when their images are brought home within the brain, so God must be sensible of every thing, being intimately present with every thing."[65] As Newton paraphrased the ancient ideas, God is omnipresent and in him all creatures live and move and have their being. Newton's Deity can be aligned easily enough with the God of Judaeo-Christian tradition, and indeed must be in many ways, but His intimacy, the fact that He is mingled or mixed with everything – as in the Virgilian and Aratean passages Newton cited – is really not traditional and establishes that Newton's God had affinities also with the divine rational principle of the Presocratics, Philo, and the Stoics: the active principle as Mind. It was that aspect of Newton's Deity that at last seemed to him to offer, by analogy, an explanation of the "how" of gravitation. As we sense objects indirectly when their images are brought home to our little sensories, the centers of perception in our brains, so God perceives objects *directly,* "being intimately present with every thing." Then in the *Opticks* Newton established the correlative analogy of willed activity, all space being as it were God's "boundless uniform Sensorium." If we will to move objects, we can do so

only indirectly, sending messages from brain through nerves to bodily parts, which are thereby enabled to put our wills into effect. But God is "void of Organs, Members or Parts" and so acts directly. He is

> a powerful ever-living Agent, who being in all Places, is more able by his Will to move the bodies within his boundless uniform Sensorium, and thereby to form and reform the Parts of the Universe, than we are by our Will to move the Parts of our own Bodies.[66]

Thus, by analogy with the indirect willed activity of the human mind, the Divine Mind wills directly "to form and reform the Parts of the Universe," and that is what we call gravitation.

So when Newton referred to his "active principle" of gravitation and compared it to the "active principle" of fermentation, the latter concept was quite clearly based on alchemical ideas. But it is doubtful that Newton came to treat gravity as an "active principle" simply by analogy with the "active principle" of alchemy. Rather, Newton's new conceptualization of the gravitational principle was almost certainly mediated by the rigorous distinctions he had found in Philo and in Renaissance Neostoicism between active and passive principles, between Divine Mind and Matter.

Northwestern University, U.S.A.

NOTES

1. J.E. McGuire and Martin Tamny, *Certain Philosophical Questions: Newton's Trinity Notebook* (Cambridge/London/New York/New Rochelle/Melbourne/Sydney: Cambridge University Press, 1983), esp. pp. 362-65, 426-31.
2. Isaac Newton, "Of Natures obvious laws & processes in vegetation," Dibner Library of the History of Science and Technology of the Smithsonian Institution Libraries, Washington, D.C., Dibner Collection MSS 1031 B (written c. 1672); *idem,* "An Hypothesis explaining the Properties of Light, discoursed of in my severall Papers," in *The Correspondence of Isaac Newton,* ed. by H.W. Turnbull, J.P. Scott, A.R. Hall, and Laura Tilling (7 vols.; Cambridge: Published for the Royal Society at the University Press, 1959-77), I, 362-82 (Newton to Oldenberg, 7 Dec. 1675), *idem,* two different systems in a letter to Boyle, *Correspondence,* II, 288-96 (Newton to Boyle, 28 Feb. 1678/79); *idem,* use of gravitational vortices in correspondence with Burnet, *Correspon-*

dence, II, 319-34 (Newton to Burnet, 24. Dec. 1680; Burnet to Newton, 13 Jan. 1680/81; Newton to Burnet, ? Jan. 1680/81); *idem,* use of gravitational vortices in correspondence with Flamsteed, *Correspondence,* II, 340-56, 358-67 (Newton to Crompton for Flamsteed, 28 Feb. 1680/81; Flamsteed to Crompton for Newton, 7 March 1680/81; Newton to [? Crompton] ? April 1681; Newton to Flamsteed, 16 April 1681); *idem,* reference to the mnaterial fluid of the heavens that carried the planets in their course, in University Library, Cambridge, Add. MS 3964.14, f. 613r, cited in Derek T. Whiteside, "Before the *Principia:* The Maturing of Newton's Thoughts on Dynamical Astronomy 1664-1684." *Journal for the History of Astronomy 1* (1970), 5-19, esp. pp 14 and 18, note 42 (dated by Whiteside c. 1682). In the first two of these references the mechanical gravitational aether is most closely associated with the "active (non-mechanical) principle" of alchemy.

3. Cf. B.J.T. Dobbs, "Newton's Rejection of a Mechanical Aether for Gravitation," in *Scrutinizing Science: Empirical Studies of Scientific Change,* ed. by A. Donovan, L.Laudan, and R. Laudan (Dordrecht/Boston: Reidel, in press); *idem,* "Newton's Corpus: An Integrated View," in *Proceedings of the Tercentenary Celebrations of the Publication of Newton's Principia of the University of Maryland and the Smithsonian Institution* (in press).
4. Isaac Newton, *A Treatise of the System of the World Translated into English,* intro. by I. Bernard Cohen (facsimile reprint of the 2nd London ed. of 1731; London: Dawsons of Pall Mall, 1969), "Introduction," pp. vii-viii; Richard S. Westfall, "Newton's Theological Manuscripts," in *Contemporary Newtonian Research,* ed. by Zev Bechler (Studies in the History of Modern Science, 9; Dordrecht/Boston/London: D. Reidel, 1982), pp. 129-43, esp. p. 137. For a similar version of the same material, see Paolo Casini, "Newton: the Classical Scholia," *History of Science 22* (1984), 1-58, quotation on pp. 1-2. For Newton's work on the origins of gentile theology, see Richard S. Westfall, "Isaac Newton's *Theologiae Gentilis Origines Philosophiae,"* in *The Secular Mind. Transformations of Faith in Modern Europe. Essays presented to Franklin L. Baumer, Randolph W. Townsend Professor of History, Yale University,* ed. by W. Warren Wagar (New York/London: Holmes & Meier, 1982), 15-34.
5. Newton, *System* (note 4), pp. 1-2.
6. Ibid., pp. 3-5.
7. Cf. I. Bernard Cohen, "Hypotheses in Newton's Philosophy," *Physis 8* (1966), 163-84.
8. From the Journal of the Royal Society for 4 July 1688, quoted in Bernard Gagnabin, "De la cause de la pesanteur. Mémoire de Nicolas Fatio de Duillier, Présenté à la Royal Society le 26 février 1690. Reconstitué et publié avec une introduction par Bernard Gagnebin, Conservateur des manuscrits à la Bibliothèque publique et universitaire de Genève," *Notes and Records of the Royal Society of London 6* (1949), 105-60, on p. 115.
9. Bibliothèque publique et universitaire de Genève, Papiers Fatio No. 3 (Inv. 526), f. 66, quoted in Gagnebin, "De la cause" (note 8), p. 117.
10. I. Bernard Cohen, *Introduction to Newton's 'Principia'* (Cambridge, MA: Harvard University Press, 1971), pp. 184-85; Isaac Newton, *Unpublished Scientific Papers of Isaac Newton. A Selection from the Portsmouth Collection in the University Library, Cambridge. Chosen, edited, and translated by A. Rupert Hall and Marie Boas Hall* (Cambridge: At the University Press, 1962), pp. 312-17, quotations from pp. 313, 315.

11. Memoranda by David Gregory, 28 Dec. 1691 (From the original in the Library of the Royal Society of London) in Newton, *Correspondence* (note 2), III, 191-92.
12. Newton, *Unpublished Papers* (note 10), pp. 312-17.
13. McGuire and Tamny, *Certain Philosophical Questions* (note 1), pp. 408-09.
14. King's College, Cambridge, Keynes MS 28, f. 6v. quoted by permission of the Provost and Fellows of King's College, Cambridge.
15. Isaac Newton, *Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World* (1729, trans. Andrew Motte), ed. by Florian Cajori (reprint of the 1934 ed.; 2 vols.; Berkeley/Los Angeles: University of California Press, 1962), II, 546; *Isaac Newton's Philosophiae naturalis principia mathematica. The Third Edition (1726) with Variant Readings,* assembled and ed. by Alexandre Koyré and I. Bernard Cohen with the assistance of Anne Whitman (2 vols.; Cambridge, MA: Harvard University Press, 1972), II, 764.
16. *David Gregory,Isaac Newton and Their Circle: Extracts from David Gregory's Memoranda 1677-1708,* ed. by Walter George Hiscock (Oxford: By the University Press for the Author, 1937), p. 30.
17. Edward Grant, *Much Ado about Nothing. Theories of space and vacuum from the Middle Ages to the Scientific Revolution* (Cambridge/London/New York/New Rochelle/Melbourne/Sydney: Cambridge University Press, 1981), esp. pp. 182-264.
18. Brian P. Copenhaver, "Jewish Theologies of Space in the Scientific Revolution: Henry More, Joseph Raphson, Isaac Newton and their Predecessors," *Annals of Science 37* (1980), 489-548.
19. Newton, *Principia* (note 15), (Motte-Cajori), II, 545; (Koyré-Cohen), II, 762: I Kings 8: 27; Psalms 139: 7-9; Job 22: 12-14; Jeremiah 23: 23-24. For the references to the prior Jewish tradition, see Copenhaver, "Jewish Theologies" (note 18), p. 546, note 220.
20. Newton, *Principia* (note 15), (Motte-Cajori, II, 545; (Koyré-Cohen), II, 762: Deuteronomy 4:39; Deuteronomy 10: 14; John 14: 2; Acts 17: 27-28.
21. Newton, *Principia* (note 15), (Motte-Cajori), II, 545; (Koyré-Cohen), II, 762.
22. *Gregory's Memoirs* (note 16), p. 30.
23. Cohen, *Introduction* (note 10), pp. 177-99.
24. Newton, *Correspondence* (note 2), III, 193-95 (Fatio de Duillier to Huygens, 5 Feb. 1691/92).
25. Ibid., III, 335-36, 338 (Gregory's Memoranda, 5, 6, 7 May 1694).
26. J.E. McGuire and P.M. Rattansi, "Newton and the 'Pipes of Pan,'" *Notes and Records of the Royal Society of London 21* (1966), 108-43; Casini, "Newton: the Classical Scholia" (note 4); Gregory MS 247, Library of the Royal Society of London. Drafts of several items exist in the Portsmouth Collection, University Library, Cambridge.
27. Isaac Newton, draft Scholium to Proposition IX, as translated in McGuire and Rattansi, "Newton and the 'Pipes of Pan'" (note 26), p. 119; original Latin in Casini, "Newton: the Classical Scholia" (note 4), p. 33.
28. S.K. Heninger, Jr., *Touches of Sweet Harmony. Pythagorean Cosmology and Renaissance Poetics* (San Marino, CA: The Huntington Library, 1974), pp. 201-14, quotation from Stanley's *History of Philosophy* (2 vols.; London, 1656-60) on p. 204. Newton once owned Stanley's *Historia philosophiæ Orientalis* (Amstelodami, 1690) and the third edition of his *History of Philosophy* (London, 1701): John Harrison, *the Library of Isaac Newton* (Cambridge/London/New York/Melbourne: Cambridge University Press,

1978), p. 242, nos. 1551-52. Both items are, however, missing from that portion of Newton's library that now survives at Trinity College, Cambridge.

29. Heninger, *Touches of Sweet Harmony* (note 28), p. 230, note 18.
30. Casini, "Newton: the Classical Scholia" (note 4), e.g., pp. 39-40, notes 4-5, 17-18.
31. Justin Martyr in his *Exhortation to the Greeks,* as quoted in Heninger, *Touches of Sweet Harmony* (note 28), p. 202; for a selection of later authorities repeating Justin's rendition of Pythagorean thought, see ibid., pp. 202-203 and 229, note 9.
32. Justin, *St, martyr, Opera...* (Coloniæ, 1686): Harrison, *Library* (note 28), p. 170, no. 868, surviving as Newton Library NQ. 18.25[1], Trinity College, Cambridge, with some pages dog-eared, presumably by Newton himself.
33. E.g., Clemens, *Alexandrinus, Opera...* (Lutetiæ Parisiorum, 1641); Cyril, *St, Abp of Jerusalem, Opera...* (Lutetiæ Parisiorum, 1631); Hierocles, *Alexandrinus, Commentarius in aurea Pythagoreorum carmina...* (Parisiis, 1583); Stanley, *History of Philosophy* (note 28): Harrison, *Library* (note 28), respectively, p. 120, no. 398; p. 128, no. 476; p. 159, no. 762; p. 242, no. 1552. The *Opera* of Clement of Alexandria is now missing, as is the work by Stanley, but Cyril's *Opera* and Hierocles' *Commentarius* are extant as, respectively, Newton Library NQ. 11. 48 and NQ. 9. 78, Trinity College, Cambridge, both with some signs of dog-earing. One should also note in Newton's library the presence of several other works relevant to Pythagorean-Platonic thought and ancient music: Iamblichus, *Chalcidensis, De mysteriis liber...* (Oxonii, 1678) (Harrison, *Library,* p. 166, no. 827, now Tr/NQ.IL 26), *idem, De vita Pythagorica Liber...* (Amstelodami, 1707) (Harrison, *Library*, p. 166, no. 818, now Tr/NQ. 8. 75, with dog-earing); Marcus Meibomius, *Antiquæ musicæ auctores septem...* (2 vols.; Amstelodami, 1652) (Harrison, *Library,* p. 190, no. 1060, now Tr/NQ. 8. 36 & 37, including the musical works of Aristides Quintilianus, Nicomachus Gerasenus, and Martianus Capella, among others); Ficino's edition of Plato, *Opera...* (Francofurti, 1602), *Library*, p. 218, no. 1325, now missing); Claudius Ptolemaeus, *Harmonicorum libri III...* (Oxonii, 1682) (Harrison, *Library,* p. 222, no. 1357, now Tr/NQ. 10. 36). See also Penelope Gouk, "Newton and music: From the microcosm to the macrocosm," *International Studies in the Philosophy of Science 1* (1986), 36-59.
34. Heninger, *Touches of Sweet Harmony* (note 28), pp. 209-10.
35. Otto von Simson, *The Gothic Cathedral. Origins of Gothic Architecture and the Medieval Concept of Order* (2nd ed., revised; Bollingen Series XLVIII; New York: Pantheon Books, 1962), pp. 8-38; Helen Rosenau, *Vision of the Temple. The Image of the Temple of Jerusalem in Judaism and Christianity* (London: Oresko Books, 1979); R.J. van Pelt, *Tempel van de Wereld, de kosmisch symboliek van de tempel van Salomo* (HES Uitgevers/Utrecht: H & S, 1984), esp. pp. 323-29 and (English Abstract) pp. 397-98. For a generalized statement of the symbolism of the center, including fire altars and temples, as *imago mundi,* see also Mircea Eliade, *Images and Symbols. Studies in Religious Symbolism,* trans. by Philip Mairet (London: Harvill Press, 1961), pp. 37-56. For Newton's work in this area, see Westfall, "Isaac Newton's *Theologiae Gentilis Origines Philosophicae*" (note 4); Isaac Newton, *The Chronology of Ancient Kingdoms Amended. To which is Prefix'd, A Short Chronicle from the First Memory of Things in Europe, to the Conquest of Persia by Alexander the Great* (London: Printed for J. Tonson in the *Strand,* and J. Osborn and T. Longman in *Paternoster Row,* 1728), pp. 332-46 and Plates I-III. Plate I has been reproduced in Rosenau, *Vision of the Temple,* p. 123.

See also Newton's section of the Temple altar reproduced in Joseph Rykwert, *On Adam's House in Paradise. The Idea of the Primitive Hut in Architectural History* (2nd ed.; Cambridge, MA/London: The MIT Press, 1981), p. 217 (Babson MS number not given but probably the reproduction is from the Sir Isaac Newton Collection, Babson Institute, Babson Park, MA, Babson MS 434, written by Newton on the Temple of Solomon in the 1680s). I have explored this topic in more depth in my forthcoming *The Janus Faces of Genius: the Role of Alchemy in Newton's Thought.*

36. Isaac Newton, draft Query 23, University Library, Cambridge, MS Add. 3970, f. 619r, as quoted in McGuire and Rattansi, "Newton and the 'Pipes of Pan'" (note 26), p. 118.
37. Newton, *Principia* (note 15), (Motte-Cajori), II, 545; (Koyré-Cohen), II, 762.
38. Cicero, *De natura deorum,* I, x-xi (Loeb Classical Library, trans. by H. Rackham).
39. Virgil, *Georgics,* IV, 219-27 (trans. by Robert Wells, 1982).
40. *Idem, Aeneid,* VI, 724-27 (verse trans. by Allen Mandelbaum, 1971).
41. Cf. *Virgil: Georgics I & IV,* ed. by H.H. Huxley (London: Methuen & Co., 1963), pp. 162-63; Jean Martin, *Histoire du texte des Phénomènes d'Aratos* (Etudes et Commentaires XXII; Paris: Librarie C. Klincksieck, 1956).
42. Issac Newton, draft Scholium, University Library, Cambridge, MS 3965.12 f. 269, as quoted in McGuire and Rattansi, "Newton and the 'Pipes of Pan'" (note 26), p. 120.
43. Aratus, *Phaenomena,* ll. 1-9 (trans. by Sanley Frank Lombardo, 1976); cf. Stanley Frank Lombardo, "Aratus' *Phaenomena:* An Introduction and Translation" (Ph.D. Dissertation, The University of Texas at Austin, 1976), esp. pp. vii, 1-3, 30-32, 54-57, 102-03, quotation from p. 102.
44. Lombardo, "Aratus' *Phaenomena*" (note 43), pp. 1-3, 102; Cleanthês' *Hymn to Zeus,* in *the Oxford Book of Greek Verse in Translation,* ed. by T.F. Higham and C.M. Bowra (Oxford; At the Clarendon Press, 1938), pp. 533-35, no. 483 (trans. by Michael Balkwill).
45. Philo, *Allegorical Interpretation of Genesis II., III (Legum Allegoria),* I, 1 (The Loeb Classical Library, trans. by F.H. Colson and G.H. Whitaker).
46. David E. Hahm, *The Origins of Stoic Cosmology* (N. p.: Ohio State University Press, 1977), pp. 3-28, esp. pp. 14-15.
47. H.A.K. Hunt, *A Physical Interpretation of the Universe. The Doctrines of Zeno the Stoic* (Melbourne: Melbourne University Press, 1976), pp. 47, 50; Robert B. Todd, *Alexander of Aphrodisias on Stoic Physics. A Study of the De mixtione with preliminary essays,* texts, translation and commentary (Leiden: E.J. Brill, 1976); Paul Hager, "Chrysippus' Theory of Pneuma," *Prudentia 14* (1982), 97-108.
48. David Winston, *Logos and Mystical Theology in Philo of Alexandria* (Cincinnati: Hebrew Union College Press, 1985), pp. 9-12, provides a recent survey of modern opinion; see also Samuel Sandmel, *Philo of Alexandria. An Introduction* (New York/Oxford: Oxford University Press, 1979); Max Pulver, "The Experience of the Pneuma in Philo," trans. by Ralph Manheim, in *Spirit and Nature. Papers from the Eranos Yearbooks,* ed, by Joseph Campbell (Bollingen Series XXX.1; Princeton: Princeton University Press, 1982), pp. 107-21, originally published in German in *Eranos-Jahrbuch 13* (1945).
49. Philo, *Judaeus, Omnia quæ extant opera...* (Lutetiæ Parisiorum, 1640); Harrison, *Library* (note 28), p. 216, no. 1300, now missing.

50. Sandmel, *Philo* (note 48), p. 28.
51. M. Spanneut, *Le Stoïcisme des pères de l'Eglise: de Clément de Rome à Clément d'Alexandrie* (Paris: Seuil, 1957),pp. 288-91, 394-96, 426.
52. Cf. B.J.T. Dobbs, "Newton's Alchemy and His Theory of Matter," *Isis 73* (1982), 511-28, esp. p. 523 and the references cited there.
53. Léontine Zanta, *La renaissance du stoïcisme au XVIe siècle* (Bibliothèque littéraire de la renaissance, nouvelle série, tome V; Paris: Librairie ancienne Honoré Champion, Édouard Champion, 1914), esp. pp. 225-30; Jason Lewis Saunders, *Justus Lipsius. The Philosophy of Renaissance Stoicism* (New York: The Liberal Arts Press, 1955), pp. 121-37; Justus Lipsius, *Physiologiae stoicorum libri tres: L. Annaeo Senecæ aliisq. scriptoribus illustrandis,* in *Justi Lipsi V.C. Opera omnia, postremum ab ipso aucta et recensita: nunc primum copioso rerum indice illustrata* (4 vols.; Vesaliæ: Apud Andræam ab Hoogenhuysen et Societatem, 1675), IV, 823-1006, esp. *Physiologiae stoicorum,* I, iii-ix, in *Opera,* IV, 837-53, quotation from IV, 851. Newton is not known to have owned Lipsius' *Physiologiae stoicorum* even though two other of Lipsius' works were in his library: *Epistolarum selectarum chilias...* (Genevæ, 1611) and *Roma illustrata, sive Antiquitatum Romanarum breviarum... Ex nova recensione A. Thysii... Postrema ed.* (Amstelodami, 1657): Harrison, *Library* (note 28), p. 180, nos. 959-60, now, respectively, Tr/NQ. 8. 135 and missing.
54. Lipsius, *Physiologiae stoicorum,* II, xiii, in *Opera* (note 53), IV, 927-30.
55. Cf. *supra* at note 27.
56. Samuel Sambursky, *Physics of the Stoics* (reprint of the 1959 ed.; A Radius Book; London: Hutchinson, 1971), esp. pp. 33-40; David Charles Kubrin, "Newton and the Cyclical Cosmos: Providence and the Mechanical Philosophy," *Journal of the History of Ideas 28* (1967), 325-46; *idem,* "Providence and the Mechanical Philosophy: The Creation and Dissolution of the World in Newtonian Thought. A Study of the Relations of Science and Religion in Seventeenth Century England" (Ph.D. Dissertation, Cornell University, 1968). Cf. also B.J.T. Dobbs, "Newton and Stoicism," in *Spindel Conference 1984: Recovering the Stoics, The Southern Journal of Philosophy 23 Supplement* (1985), 109-23.
57. Cicero, *De natura deorum* (note 38), II, xlv, 115-16.
58. I have traced it as far back as 1850: M. Tullii Ciceronis, *De natura deorum libri tres,* erklaert von G.F. Schoemann (Leipzig: Weidmanssche Buchhandlung, 1850), p. 157.
59. Hunt, *Physical Interpretation* (note 47), pp. 57-58.
60. *Titi Lvcreti Cari De Rervm Natvra Libri Sex,* ed. with Prolegomena, Critical Apparatus, Translation, and Commentary by Cyril Bailey (3 vols.; Oxford: At the Clarendon Press, 1947), II, 782-83.
61. Michael Lapidge, "A Stoic Metaphor in Late Latin Poetry: the Binding of the Cosmos," *Latomus 39* (1980), 817-37; James Hutton, "Spenser's 'Adamantine Chains': A Cosmological Metaphor," in *The Classical Tradition. Literary and Historical Studies in Honor of Harry Caplan,* ed. by Luitpold Wallach (Ithaca: Cornell University Press, 1966), pp. 572-94, esp. pp. 583-88.
62. Isaac Newton, *Opticks, or A Treatise of the Reflections, Refractions, Inflections & Colours of Light,* foreword by Albert Einstein, intro. by Sir Edmund Whittaker, preface by I. Bernard Cohen, analytical table of contents by Duane H.D. Roller (based on the 4th London ed. of 1730); New York: Dover Publications, 1952), pp. 400-01.

63. Richard S. Westfall, *Force in Newton's Physics. The Science of Dynamics in the Seventeenth Century* (London: Macdonald; New York: American Elsevier, 1971), pp. 323-423; *idem,* "Newton and the Hermetic Tradition," in *Science, Medicine and Society in the Renaissance. Essays to honor Walter Pagel,* ed. by Allen G. Debus (2 vols.; New York: Science History Publications, 1972), II, 183-98; B.J.T. Dobbs, *The Foundations of Newton's Alchemy, or "The Hunting of the Greene Lyon"* (Cambridge/London/New York/Melbourne: Cambridge University Press, 1975), pp. 210-13.
64. Cf. Dobbs, "Newton's Corpus: An Integrated View" (note 3), and *idem,* "Newton's Alchemy and His Theory of Matter" (note 52).
65. Cf. *supra,* at note 16.
66. Newton, *Opticks* (note 62), p. 403. Cf. also Alexandre Koyré and I. Bernard Cohen, "The Case of the Missing *Tanquam.* Leibniz, Newton, and Clarke," *Isis 52* (1961), 555-66.

RICHARD H. POPKIN

NEWTON'S BIBLICAL THEOLOGY AND HIS THEOLOGICAL PHYSICS

In the vast literature about Newton, little is devoted to explaining his religious views, except as personal aberrations, infantile views, or premature signs of senility. In recent years R.S. Westfall, Frank Manuel, James Force and a few others have tried to give some more impressive explanations of why one of the world's greatest scientists should have spent so much time thinking and writing about religious matters.[1] In this paper I should like to turn the problem around, and ask why did one of the greatest anti-Trinitarian theologians of the 17th century take time off to write works on natural science, like the *Principia Mathematica?*

Newton wrote on religion and theology from his college days down to the end of his life. Almost half of the pages that he physically wrote, most still unpublished, deal with explicating the Bible, interpreting it, and developing a theory of Scriptural and natural revelation. To appreciate Newton's accomplishments in these areas he really has to be compared with the other great theologians of the 17th century, such as his teachers and associates, Henry More, Ralph Cudworth and Isaac Barrow. In this paper I will deal primarily with Newton's views, with some indications as to how he went beyond the others of the time.

Sir Isaac Newton's views on the Bible are an intriguing mixture of modern Bible scholarship, of the application of modern science to the Bible, and of a conviction that in the proper reading of the revealed text, God's plan for human and world history can be found. Newton wrote a great deal about the Bible as an historical document, about the accuracy of the Bible, about the chronology of the Bible, and about the message of the Bible. He wrote on these matters from his student days at Cambridge until his death. For many years, including the central ones in his intellectual career, he was preparing manuscripts on these subjects for publication, and withheld them because his views were so heretical for his time.

Four items appeared posthumously; *The Chronology of Ancient Kingdoms Amended* (1728), *Observations on Daniel and Revelation* (1732), an essay on the Cubit of the Hebrews, 1737, and two letters to John Locke dealing with the Doctrine of the Trinity, (1743).[2] Besides the published material a vast amount of still unpublished manuscripts exist in

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libraries from Jerusalem to Japan.[3]

When Newton died a large box of papers on religion and theology were given by his family to the Royal Society, of which he had been the President. The Royal Society looked through the material and returned it, telling the family not to show it to anyone. Years later they showed it to their minister, and were given the same advice. In the early 19th century some of the material was used by David Brewster in his biography of Newton, and he again advised the owners not to show it to anyone. Late in the 19th century Cambridge University sent some people to go through the papers, looking for mathematical or scientific treasures, and returned the rest, with the same admonition. The Earl of Portsmouth, Lord Lymington, became the owner in the early 20th century, and tried to give the papers to Cambridge University which refused them, and to the British Museum, which did likewise. The manuscripts were finally put up for auction at Sotheby's in 1936. The largest purchases were made by Lord Keynes and Prof A.S Yahuda, an eminent Arabist. Smaller units passed to libraries in Europe, and America, and various pieces are still in private hands.[4]

The theological manuscripts in the Keynes collection, which was given to King's College, Cambridge, have been published in a slim volume.[5] The largest collection, well over half of what was auctioned off, was bought by A.S. Yahuda, a wealthy Palestinian Jew, who took his degree in Arabic studies in Germany, became Royal Professor of Medieval Rabbinics in Spain, then professor of Arabic in Germany, a lecturer in England in the 1930's, and a refugee scholar in America from 1940, until his death in 1951. Yahuda had an enormous collection of manuscripts, of which the Newton holdings were one of 1,500 collections.[6] When he brought the collection to America, he tried to get his close friend, Albert Einstein, to aid him in placing the Newton manuscripts at Harvard, Yale or Princeton. Harvard refused them on the grounds that a war was going; Yale on the grounds that they had no space; and Princeton on the grounds that the material was not scientific.[7] Yahuda was forced to keep his Newton collection in his house in New Haven. As death neared, he debated what to do. A friend and disciple of his has told me that it was only on his death-bed that he willed all of his manuscripts to the Jewish National Library in Jerusalem. Yahuda had early on been a Zionist, but broke with the movement at the time of the Balfour Declaration in 1917. He became a leading opponent of the idea of a Jewish state, and refused to have anything to do with the state of Israel. However, on his death-bed, he was convinced to send his precious manuscripts to his homeland.[8] After his death a law suit followed, when the family tried to break the will. The manuscripts only got sent to Jerusalem in 1969, and have only

been studied there by a few scholars. So far only part of one of the Yahuda manuscripts has been published.[9]

Since Newton drafted his views over at least sixty years, and since most of the manuscripts have not been published, it is not yet possible to give a succinct statement of Newton's views on religious matters. Newton's views change, are reconsidered, amended over the years. Early on Newton was close to being a Calvinist literalist. In one of the Yahuda manuscripts, 1.1, (which was partially published by Frank Manuel), Newton wrote, "To chose those interpretations which are most according to the literall meaning of the scriptures unles where the tenour and circumstances of the place plainly require an Allegory."[10] A few pages later, after the text published by Manuel, Newton, said that it "is ye wisdom of God that he hath so framed ye Scriptures as to distinguish between ye good & ye bad, that they should be demonstration to the ye one & foolishness to the other."[11] This manuscript has been dated by Westfall as written in 1671.[12] Later on, apparently through the influence of Richard Simon, or maybe Spinoza and/or John Locke, Newton developed a rather critical view about any existing texts of the Old or New Testaments. Newton had several works of Father Simon in his library.[13] He could have learned of Spinoza's views from Simon's writings, or from Henry More's critique of Spinoza, or from the copy of Spinoza's *Tractatus* in Isaac Barrow's library, which Newton catalogued.[14]

In both the manuscripts (New College, Oxford Mss II, fol.192 and Yahuda Var. Ms. 1.7, 1.,1.9 and 10B11v)[15] and in the published version of the *Observations on Daniel and Revelation,* Newton offered a picture of how the text of the Old Testament got to its present state. He presented a theory of how the texts were composed, mixed up, and corrupted. Newton tried in some detail to describe how various Biblical documents got into their present confused state. He held that we have no texts that date before Talmudic times.[16]

On the historical side, using internal evidence, and the historical events mentioned in the Biblical texts, Newton worked out a detailed account of how the books of the Old Testament were compiled. A version of this account appears in the first chapter of the published *Observations.*[17] A much more detailed account of this appears in Yahuda Ms. Var 1.7:3, 9:2 and 10B. Newton did not claim Moses was the author of the Pentateuch. The history of God's people that is contained in it "hath been collected from several books" as was the history of the Creation. Newton named some of these lost books "the book of the generations of Adam," "the book of the wars of the Lord," etc. The books which have come down to us were written with the authority of Moses and Joshua. Samuel put them in their present form, inserting additional material as

well in *Genesis*.[18] Newton went on systematically constructing a theory about when each book was written. *Isaiah,* Newton said, was written at several different times, and collected together by the prophet.[19] "The book of Daniel is a collection of papers written at several times." The last six chapters Newton claimed were by Daniel himself, written at various times; the first six were written by others, some after Daniel's death, and collected together, possibly by Ezra. The texts were partially destroyed by Antiochus Epiphanes, and an effort of restoration was made by Judah Maccabeus.[20] The text of Yahuda Ms. Var. 7:3 and 10B indicates Newton thought some books were entirely lost, others jumbled together. Papers from *Samuel* got into *Nehemiah,* and other disorders occurred.[21]

It was only after the Roman captivity that, as the Jews tried to preserve their traditions by writing the Talmud, and to preserve their scriptures, they agreed upon an edition, and added in the vowel points, and counted the letters in each book.

> by preserving only this Edition, the antientier various lections, except what can be discovered by means of the Septuagint version, are now lost, and such marginal notes, or other corruptions, as by the errors of the transcribers, before this Edition was made, had crept into the text, are now scarce to be corrected.[22]

Thus, Newton was quite avant-garde in his view about the Biblical text, and did not subscribe to the view of the Westminster Confession, or of later Fundamalism, that God, in His Providence, had preserved the Word entire through all of the vicissitudes of human history.[23]

With regard to the text of the New Testament, Newton was much more critical. He was concerned to argue for the primacy of the book of *Revelation,* and for deliberate corruption of the New Testament texts by wicked characters like Saint Athanasius. Because, as we shall see, the core of Scripture for Newton, were the two books, *Daniel* and *Revelation,* he wanted to ward off the challenge to the latter work by those who from early Christian times to the present have tried to see *Revelation* as a late work, not connected with Jesus's life on earth. Newton marshalled much evidence from early Christian writers to support his contention that *Revelation* was written before the destruction of the Temple, and the expulsion of the Jews from Jerusalem. Its author, John the Evangelist, received his revelation that he recorded from Jews, and wrote it in a style that contains more Hebraisms than the Gospel of John (which Newton thought was written by the same person). Therefore, Newton argued, *Revelation* was written when John "was newly come out of Judea, where

he had been used to the Syriac tongue." John wrote his gospel later when he had spent much time with Greeks, and had dropped his Hebraisms.[24]

Further, Newton claimed, *Revelation* is alluded to in the epistles of Peter and in Paul's letter to the Hebrews, so it must precede them. All of this, for Newton, established *Revelation* as the earliest written work in the New Testament, written by a Hebrew or Aramaic speaking disciple of Jesus before the Jews were driven out of Judea.[25]

Newton felt that there was no need to argue about the truth of the book of *Revelation,* or that it was the statement of the very truth that Daniel was commanded to shut up and seal. He did argue that the text of the Gospel according to John and of *Timothy* was corrupt, and was deliberately corrupted in the parts dealing with the doctrine of the Trinity.[26] Newton examined as many manuscripts and manuscript readings as he could of the crucial texts. In one of the Jerusalem manuscripts, Yahuda Var.1.4, he listed pages and pages and pages of variant readings.[27] Although he made no effort to decide which manuscript was accurate, Newton, in two letters he sent to John Locke in 1690, put together a most forceful case that the doctrine of the Trinity was not in the original or early texts, and was not the view of the early Christians.[28] In three different unpublished manuscripts, one at the Clark Library of UCLA, another at Cambridge, and a third in Jerusalem, Newton set forth his indictment against Saint Athanasius.[29] In the longest unpublished manuscript, at the Bodmer Library in Geneva, Newton presented his grandiose theory of how the Church became corrupt, how it falsified the true doctrine of Christianity, and in part, how it accomplished this by tinkering with the texts of the New Testament.[30]

Although it is evident that Newton was outraged by the conspiracy he had uncovered in the grand mystery of the iniquity of the early church (that he believes still going on in his own day), he was also extremely anxious not to reveal that he himself was an Arian, and denied the doctrine of the Trinity. He had originally written the letters to Locke for publication. They are quite impressive as examples of critical Bible scholarship of the time. The letters were to be published by the Dutch liberal Remonstrant theologian, Jean Le Clerc. Newton apparently panicked when he realized the letters, though anonymous, would be recognized as his, and then he would be revealed to be an anti-Trinitarian, and could not hold any state office. Newton got LeClerc to halt the publication,[31] and two of the letters were only published in 1743, after Le Clerc died. A third letter to Locke drawing out the full anti-Trinitarian consequences of his researches has only been published for the first time in 1961.[32]

So, with regard to the Scriptural text that has come down to us, Newton accepted the Hebrew text as corrupt, but usable. The Greek New Testament text he felt had to be rescued from the Trinitarians, and restored to its original view, that Jesus was the lamb of God, but was not co-substantial or co-eternal with God. Because Newton, unlike Spinoza, was convinced that the essential message of the Bible survived in the prophetic texts in *Daniel* and *Revelation,* he did not become a sceptic about religious knowledge. He felt that a good deal of the Old Testament could be studied as an early historical writing, and evaluated in terms of our other historical data, as to its accuracy and import.[33]

The work that was published as *Te Chronology of Ancient Kingdoms Amended* (1728) shows Newton using newly discovered scientific knowledge to contend that the Bible is historically accurate, is the oldest historical record that we possess, and is more accurate than Greek, Phoenician, Babylonian or Egyptian records. Newton argued that the oldest chronologists, Manetho and Erastostenes were in contradiction both with Scripture and with the new astronomy. Since we do not possess records older than the Bible, and there are reasons to question the claims of great antiquity in some of the early pagan writers, Newton felt that one should start where one can have reasonable confidence in the data.[34] For him this involved accepting the histories and chronologies in the Bible up to the books of Ezra and Nehemiah, as well as accepting the astronomical records mentioned by Thucydides and Ptolemy. From the description of the stars in the constellations in the Zodiac given in accounts about the mission of Jason and the Argonauts, and about the Trojan War, one can now figure out when these events took place. The procession of various stars in these constellations is measurable and follows a uniform law. From present observations one can figure out backwards where these stars were previously, and date when the stars were in the positions described in early Greek history.[35]

The dramatic result of using this astronomical method to figure out the date of previous events was, for Newton, that it showed that the early events described in the Old Testament took place *before* the early events in Greek history. Jason's voyage took place, according to Newton, in 937b.c. The earliest known events in Egyptian history also postdated Biblical events. Hence, our earliest reliable knowledge of human history comes from the Bible. The ancient Israelites were the first civilization and had the first monarchy. All other cultures and kingdoms are derivative from the ancient Hebrew one.[36]

The point in Newton's elaborate astronomical argument, and his debunking of pagan chronological and historical claims was to point up the accuracy of the Bible as history, and the presumption that the message

that got conveyed in the Bible is of the greatest importance to mankind. If the Bible can be accepted as historically accurate, then God presented His Message from the beginning through the Hebrews, and through their history, and through the prophetic insights given to them.[37]

Before turning to this side of Newton's interpretation of the Bible, it should be mentioned that the scholar who collected most of Newton's religious writings that are now in Jerusalem, A.S. Yahuda, a renowned Arabist, wrote a book called, *The Accuracy of the Bible,* a few years before he acquired the Newton manuscripts. Yahuda tried to show that the story of the Exodus had to be written by an eyewitness because so many Egyptian terms appear in the account. Israelites of subsequent generations would no longer have known Egyptian. Q.E.D.[38] I have been told by a student and friend of Yahuda's that Albert Einstein was present when Yahuda first stated his theory in a lecture, and that Einstein wept with joy when he realized that one might be able to prove that the events in the Bible were accurately and factually described.[39] Unfortunately Egyptologists of the time sharply challenged Yahuda's contention, and it was quickly set aside by the scholarly world.[40]

It seeems likely that Yahuda collected Newton's manuscripts to see if Newton had more evidence on "the accuracy of the Bible." Yahuda's notes on the manuscripts, and his own unfinished essay on Newton's religious views, showed that Yahuda, a 20th century scholar, trained in the German world of Higher Criticism and in the secular methods of scholarship, felt a strong affinity with Newton in the attempt to establish some meaningful sense in which it could be said that the Bible is accurate.[41]

For Newton the accuracy claim reinforces the contention that God has revealed himself in Jewish history. The historical Biblical materials help explain the origin of mankind, of human institutions, and human cultural abilities, such as being able to write. And Newton left a lot of manuscripts on the development of political societies from the Israelite one onward.[42] He also was convinced that in the units God gave the Hebrews to measure (the sacred cubit of the Hebrews) and in the plans for Solomon's Temple, very important revealed information could be discovered.[43] But the most important thing to be discovered in the Biblical records is that God has laid down the plan of human history, as well as the plan of natural history. The latter is to be studied primarily in the Book of Nature, through scientific researches. The former is to be studied in the central prophetic statement about the course of human history, the books of *Daniel* and *Revelation*. Textual problems aside, these two works are continuous and united, and present, in deliberately difficult manner, what will happen to mankind up to the apocalyptic end of human history.

To those who are sceptical about the core of God's message being in *Daniel* and *Revelation,* Newton offered three answers. One, why would God have provided us with so many clues in the form of the cryptic symbols in these books, if we were not supposed to try to figure them out? Second, we are told in *Daniel,* that the wise will understand and the wicked will not. If one sees no message, that shows one's moral deficiencies, but is no comment on the message. Third, and most important, the prophectic character of the message is confirmed by carefully examining human history from the time of the writing of *Daniel* onward, and discovering how much of what has happened is an exact fulfillment of the prophecies set forth in *Daniel* and *Revelation* .[44]

The third answer then lays down the project Newton dealt with all of his adult life, writing the definitive explanantion of *Daniel* and *Revelation.* He sided with several great Bible scholars. He was very influenced by Henry More, the Cambridge Platonist, but apparently came to a basic disagreement with him about interpreting the symbol of the seven churches at the beginning of the text of *Revelation.* We know that Newton delayed publishing his scientific works because he was too engrossed in working with More on a commentary on *Revelation.* A letter of 1680 of More's indicates that they had a serious falling out.[45] In the Newton manuscripts More is occasionally directly criticized, but is usually obliquely attacked.[46] Newton presented himself as the true heir of More's teacher, Joseph Mede, the author of *Clavis Apocalyptica* (1627).[47] This work offered a synchronic way of correlating predicted events in *Daniel* and *Revelation* and introduced non-Biblical sources, like the medieval Arabic writer, Achmed, for understanding what ancient Israel could have been like, what they and other ancient Middle Eastern peoples could have believed in, etc. Mede also was very concerned to show how post-Biblical history conformed to deciphered predictions in *Daniel* and *Revelation.*[48] Newton over at least fifty years sought to figure out workable and reasonable rules for interpreting prophecies, as distinguished from fanciful ones used by so many of his predecessors and contemporaries. Newton studied the history of the late Roman Empire, the European Middle Ages, the rise of Islam in the Middle East, and sought to identify what happenned with what was predicted.

Newton, unlike too many interpreters of *Daniel* and *Revelation,* was not trying to deduce exactly when the world would come to an end, exactly when Jesus did return, and exactly when the Jews would return to Palestine and rebuild Jerusalem. His energies went into post-facto analysis of historical events, to show that, after they occurred, the wise can see that they are the very events predicted in passages in the prophetic works. Insofar as they are the predicted events, this shows that history

is Providential history. God has laid out the whole sequence, and we, post-facto, can realize this. In realizing it, we then should be in awe of God's dominion over our history as well as over nature.[49] (Something like this had been claimed by Nostradamus, whom Newton rarely mentions in the manuscripts.) Nostradamus said that God had given him the power to see the future. He saw it. If he told people, they would be unable to understand it. He then wrote out what would happen in such a disguised way that people would only see that he had forecast given historical events *after* they happened. In recognizing this, people would realize that everything that happens is part of God's Providence.[50] Newton's post-facto method of analysis became important in the later history of prophetic interpretation that became part of the fundamentalist movement.[51] Before exploring what is involved here, one other crucial element of Newton's prophetic interpretation needs to be mentioned. The realization that actual history is the fulfillment of predictions in the Bible involves realizing that Jewish history is the core of world history. The march from ancient Israel to Babylonia, Persia, Greece, Syria, Rome and onward is the development of God's relations to the Jews, and will culminate in the return of Jesus, and he and "the mortal Jews" establishing dominion over the world from Jerusalem. The Bible, especially *Daniel* and *Revelation* project what will happen to the Jews. The study of prophetic fulfillment shows that the predicted has happened in exact detail over and over again. Hence, in studying the Scriptural text, one is also studying what remains to be fulfilled. Newton got his erstwhile disciple, William Whiston, to spell this out in his Boyle lectures on the accomplishment of Scripture prophecies, in which a huge induction was offered from the vast number of Biblical prophecies already fulfilled, to the reasonable scientific expectation that the rest, including the end of the natural world, would be fulfilled in the not too distant future.(52)

Newton's aversion to futuristic predictions is understandable in a century where all sorts pf exact predictions by English Millenarians, Dutch Chiliasts, the great Jan Amos Comenius, the Sabbatian Jews, had not turned out to be true. The Millenium did not begin in 1656, 1666 or other predicted years of great expectation.[53]

Newton, in explicating *Daniel* and *Revelation* offered a theory of progressive development in understanding the prophecies. As we approach the end of this, "Then, saith Daniel, many shall run to and fro, and knowledge shall be increased." But, "Tis therefore part of this Prophecy, [Daniell0:21, l2:4,9] that it should not be understood before the last age of the world; and therefore it makes for the credit of the Prophecy, that it is not yet understood."[54] The great succession interpreting *Daniel* and *Revelation* by Mede and his followers, indicates we may be

approaching the time of the end.

At this point, Newton set forth his post-facto theory of interpreting prophecy.

> The folly of Interpreters has been to foretell times and things by this Prophecy, as if God designed to make them Prophets. By this rashness they not only exposed themselves, but brought the Prophecy also into contempt.

God did not make post-Biblical interpreters into prophets (though Newton sometimes speaks as if he were one, the only one.) God set forth the prophecies not so that curious people could know the future. God's purpose was that people would realize after the prophecies were fulfilled

> they might be interpreted by the event, and his own Providence, not the Interpreters, be then manifested thereby to the world. For the occurrence of things predicted many ages before will then be a convincing argument that the world is governed by Providence.[55]

Until Jesus' return and the actual Apocalypse, and the setting up of the Kingdom of Righteousness, we will not be able to understand all of the prophecies. But when it all does occur, we will know the true religion. Until then we should recognize that we can only understand the prophecies imperfectly, while realizing those prophecies that have been fulfilled show us evidence of God's Providence. The interpreters of prophecy in modern times have made so much progress, presumably from Mede to Newton, that, as Newton remarked, "I seem to gather that God is about opening these mysteries."[56] So, the world predicted in the Bible is near fruition.

Newton's conception of the Bible is that it is a historical document and a cryptogram telling us of God's historical plan. The historical document is open to examination, as are other documents. Critical investigation helps us realize the defects in the documents, and enables us to assess them. Using historical standards, plus modern scientific information about the movements of the stars in time, we can determine that the Bible is the oldest historical document we possess, and, except for parts of the New Testament, has been better preserved than other ancient documents. Further, from what we now know, the Bible is the most accurate account we have of human history.

But, for Newton, the Bible is much more than a source of historical data. It is the way God has communicated to us in words, just as Nature is God's communication in things. The natural and the verbal messages require tremendous effort, insight, and pious attention to understand. Science and the study of Biblical prophecy go together as ways of comprehending God's message. The progress in science and the progress in understanding *Daniel* and *Revelation* indicate that God is opening the seals, and letting us know the nature and destiny of man. Newton, as decoder of Nature and Scripture, may be the closest to a genuine prophet that remains.

Newton's view of the Bible, though mainly buried in still unpublished manuscripts, became sufficiently known through the items published in the 18th century. Most of the early fundamentalist interpreters, who saw the American and French Revolutions as fulfillments of prophecies in *Daniel* and *Revelation,* used Newton both as a source,[57] and as a theorist explaining why exact predictions often failed.[58] Newton's post-facto method of interpretation allowed for reconsideration and restudying of prophecies when prediction failed. The last printing of Newton's *Observations upon the Prophecies of Daniel and the Apocalypse of St. John* was in 1922! The head of the British Medical Association, Sir William Whitla, put out the work as an answer to German Higher Criticism of the Bible, and dedicated it to the General of the Salvation Army who was doing so much "towards the hastening of the coming of the kingdom predicted in the Book of Daniel."[59] So, Newton's influence as a Bible interpreter has continued into this century. Perhaps, when his theological manuscripts have been published, we will be able to assess more accurately his entire theory and his originality and his stature as a commentator on the Scriptures, and be better able to assess if he was as great in this area as he was in the physical sciences.

I should like to suggest that there may be some relation of Newton's Biblical theology to his metaphysical theology. A kind of metaphysical theology appears in some of Newton's scientific and philosophical writings: in his proof of the existence of God, his discussion of God's nature, his thesis that space is the sensorium of God, his emphasis on the importance of God in physics in his letters to Bentley, and so on. These various scattered comments have led to many interpretations. It is clear that Newton held that God was the creator, designer and sustainer of the world. And he clearly held that the study of nature was the study of what God had created, and the laws by which He governed his creation.

These views, taken by themselves, do not entail any particular historical religious view, and many 18th century deists built on just the metaphysical theology of Newton without putting it into a Biblical con-

text. However, some of Newton's close disciples, such as William Whiston and Samuel Clarke, presented Newtonianism in science as part of the Christian world view. They contended that God had created the world in Newtonian terms, and that it persevered and would end according to Newton's laws. They contended that the natural and historical world operated to fulfill the prophecies in the Bible, and that the finale of world history would be that described in the Book of Revelation. Both Whiston and Clarke presented Newton's physics and his physical theology as an outgrowth of his Biblical theology. God had created the physical world and the historical world. Both were to be understood by some kind of religious-scientific activity, as God progressively revealed both the natural clues and the Scriptural clues. As the end approached, both the world physically and the world spiritually would be seen as the twin message of God.[60]

Newton's statement in the general scholium of the *Principia* certainly suggests a Biblical interpretation of the God of his physics, and a Judaic one at that. After offering his version of the deistic argument from design purporting to prove that an intelligent and powerful Being exists and governs the world, Newton asserted,

> This Being governs all things, not as the soul of the world, but as Lord over all; and on account of his dominion is wont to be called Lord God, or *Universal Ruler;* for *God* is a relative word, and as respects to servants; and *Deity* is the dominion of God not over his own body, as those imagine who fancy God to be the soul of the world, but over servants. The Supreme God is a Being eternal, infinite, absolutely perfect; but a being, however perfect, without dominion, cannot be said to be Lord God; for we say, my God, your God, the God of *Israel,* the God of Gods and Lord of Lords, but we do not say, my Eternal, your Eternal, the Eternal of *Israel,* the Eternal of Gods; we do not say, my Infinite, or my Perfect: these are titles which have no respect to servants. The word God usually signifies *Lord;* but every lord is not a God. It is the dominion of a spiritual being which constitutes a God: a true, supreme, or imaginary dominion makes a true, supreme, or imaginary God. And from his dominion it follows that the true God is a living, intelligent, and powerful Being; and, from his other perfections, that he is supreme, or most perfect. He is eternal and infinite, omnipotent and omniscient; that is, his duration reaches from eternity to eternity; his presence from infinity to infinity; he governs all things, and knows all things

> that are or can be done. He is not eternity and infinity, but eternal and infinite, he is not duration or space, but he endures and is present. He endures forever, and is everywhere present; and, by existing always and everywhere, he constitutes duration and space ... It is allowed by all that the Supreme God exists necessarily; and by the same necessity he exists *always* and *everywhere*. Whence also he is similar, all eye, all ear, all brain, all arm, all power to perceive, to understand, and to act; but in a manner not at all human, in a manner not at all corporeal, in a manner utterly unknown to us. As a blind man has no ideas of colors, so we have no idea of the manner by which the all-wise God perceives and understands all things. He is utterly void of all body and bodily figure, and can therefore neither be seen or heard, nor touched; nor ought he to be worshipped under the representation of any corporeal things... We know him only by his most wise and excellent continuance of things and final causes; we admire him for his perfections; but we reverence and adore him on account of his dominion: for we adore him as his servants; and a god without dominion, providence, and final causes, is nothing else but Fate and Nature.[61]

Newton's stress on God's dominion, his omnipresence and transcendence, and his being beyond all human conceptions has a Judaic ring to it, rather than a deistic conception of a supreme architect. "He is not eternity and infinity, but eternal and infinite, he is not duration or space, but he endures and is present. He endures forever, and is everywhere present" is out of the Judaic attempt to express the inexpressible. Perhaps it is here that the influence of Maimonides on Newton really shows.[62]

As a closing note on this matter, let me just add that the German philosopher, Friedrich Schelling, commented that Newton's philosophy was better than his science, and his philosophy was better than that of his successors because he saw that *Deitas est dominatio Dei.* The essence of God is not his substance but his *dominatio* in the act of his sovereignty.

> Finally Newton says the decisive word. *Deus sive dominio, providentia et causis finalibus nihil aliud est quam Fatum et Natura:* Providence and final cause, that is intentions executed in nature are only consequences of the dominion. One insight is enough. God without dominion, or, as I shall say from here on, because it is the true and original meaning of the word:

> God without *Herrlichkeit* would be a mere *Fatum* or a mere Nature.[63]

What saves scientific deism from Spinozism is the insistence on God as dominator.

Before drawing this discussion to a close, a further comment should be made about Newton's Arianism. Newton firmly believed, in his Biblical writings, that the true message of Scripture had been deliberately and maliciously hidden by the institutional church from at least the time of the villanous St. Athanasius, and his doctrine of the Trinity to the present day Church of England. There was, for Newton, a conspiracy on the grandest scale to deceive. Hence, one had to be extremely cautious in letting the world know. This in part accounts for Newton's reluctance to publish his scientific views, and his fear of publishing his religious ones. He knew the lengths the villains of history had gone to keep the truth hidden. Newton sought for almost sixty years to find a formulation of his overall discovery, Biblical and physical, that he could publish. He was unable to do so, and reacted ferociously when Whiston let the cat out the bag, and stated the Newtonian scientific and Biblical theology in public.[64]

The scientists found by the end of the 18th century they did not need Newton's theological physics. Laplace could tell Napoleon that he had no need of the God hypothesis in his completed Newtonian physics. The fundamentalists saw, from the little of Newton that was available, that he had not only seen the physical world in its entirety, but he had provided a very important formulation of their historical vision, that the end of days was at hand, as the American and French revolutions unfolded, the Napoleonic wars, and on to the present scene. Newton as fundamentalist has been separated from Newton as scientist so that it now seems also incomprehensible that they could have been the connected vision of one sane man. I think if one starts from Newton's religious views, then perhaps it is not so strange that one with his prophetic vision could see nature and history as unfolding to the same climax.

University of California Los Angeles

NOTES

1. Gale E. Christianson, *In the Presencee of the Creator, Isaac Newton and his Times,* (New York 1984); James E. Force, *William Whiston, Honest Newtonian,* (Cambridge 1985); Frank E, Manuel, *Isaac Newton, Historian,* (Cambridge, Mass. 1963), and *The Religion of Isaac Newton, (Oxford 1974);* and Richard S. Westfall, *Never at Rest. A Biography of Isaac Newton,* (Cambridge 1980).
2. Isaac Newton, *The Chronology of Ancient Kingdoms Amended,* (London 1728), and *Two Letters to Mr. LeClerc,* (London 1754).
3. No inventory of Newton's theological manuscripts has been published. Westfall, in *op.cit.,* gives a survey of where manuscripts are located. I have found others at the University of Kentucky, at the Seventh Day Adventist Seminary at Barien Springs, Michigan, and in private hands. There is a sales catalogue of Sotheby's for the manuscripts that were auctioned off in 1936.
4. On the history of Newton's manuscripts, see Westfall, op.cit., pp. 875-877.
5. Isaac Newton, *Theological Manuscripts,* selected and edited with an introduction by H. McLachlan, (Liverpool 1950).
6. There is an inventory written by Yahuda at the Jewish National Library in Jerusalem. The Newton manuscripts are collection #1. #2 is a very large group of Napoleon items; #3 is the holograph manuscript of Einstein's *Physik und Realitat.* Over 95% of the other items are in Near Eastern languages.
7. The correspondence is in Yahuda Ms. Var. 1, Box 42.
8. This information was acquired in private conversations with friends of Yahuda. He is still a very controversial figure in Israel. Shortly after his death his widow privately published a work of Yahuda's entitled, *Dr. Weizman's Errors on Trial,* an attack on what Chaim Weitzman said about Yahuda in his work, *Trial and Error.*
9. This appears as an appendix to Frank Manuel, *Religion of Isaac Newton,* "Fragments from a Treatise on Revelation," pp.107-123.
10. Yahuda Ms. Var. 1.1, fol. 12r; Manuel, *Religion of Newton,* p.118.
11. Yahuda Ms. Var. 1.1., fol. 19v.
12. In the chronological order of Newton's theological writings prepared by R.S. Westfall, unpublished.
13. Cf. John Harrison, *The Library of Isaac Newton,* (Cambridge 1978), p.239.
14. See Manuel, *Religion of Newton,* pp. 84-85.
15. Newton manuscripts, New College Oxford II fol. 192; Yahuda Var. Ms. 1.7, 1.9 and 1.10B, fol.11v.
16. Newton, *Observations upon the Prophecies of Daniel and the Apocalypse of St.John,* (London 1733), chap. I, pp.1) 15.
17. *Ibid.,* loc,cit. A much more detailed account of this appears in Yahuda Ms. Var. 1.7:3; 1.9:2 and 1.10B.
18. Newton, *Observations,* pp.4-5.
19. *Ibid.,* p.10.

20. *Ibid., pp. 10-11.*
21. Yahuda Ms. Var. 1.7:3, and 1.10B, fol.11v-12r.
22. Newton, *Observations,* pp. 11-12.
23. *The Westminster Confession of Faith,* (London 1658), chap. 1,sec. viii, p. 6, states, The Old and New Testaments "being immediately inspired by God, and by his singular care and Providence kept pure in all Ages, are therefore Authentical."
24. Newton, *Observations,* p.238.
25. *Ibid.,* p. 239.
26. See Newton's letter to Locke, Nov. 4, 1690, (full reference given in note 28); and Westfall, *op.cit.,* pp. 312-313, for further references.
27. Yahuda Ms. Var. 14.
28. The first time two of these letters were published was inLondon in 1754. The full texts, plus a third letter are printed in *The Correspondence of Isaac Newton,* edited by H. W. Turnbull, (Cambridge 1961), Vol. III, pp. 83) 146.
29. William Andrews Clark Library Ms., Newton, *Paradoxical Questions concerning ye morals and actions of Athanasius and his followers;* Keynes Ms. 10, King's College, Cambridge, *Paradoxical Questions...* See also Yahuda Ms. 1.14.
30. Martin Bodmer Library, Geneva, Newton Ms. on the Mystery of the Grand Iniquity of the Church.
31. On this, see Westfall, *Never at Rest,* pp. 390-91.
32. See note 28.
33. Cf. Manuel, *Isaac Newton, Historian,* chaps. VI and IX.
34. Newton, "A Short Chronicle of the First Memory of Things inEurope," in *The Chronology of Ancient Kingdoms Amended,* pp. 1-42; and Manuel, *Newton, Historian,* chap. III.
35. Newton, *Chronology,* chap. I and p. 358; and Manuel, *Newton,Historian,* chaps. IV-VI.
36. Newton, "A Short Chronicle," pp. 25-26; and Manuel, *Newton, Historian,* pp. 85-88.
37. Manuel, *Newton, Historian,* chap. VI.
38. A. S. Yahuda, *The Accuracy of the Bible,* (New York 1935).
39. From a private conversation in 1985.
40. Yahuda was challenged by the Egyptologist, Wilhelm Spiegelberg.
41. Yahuda's essay is with his Newton manuscripts in Yahuda Ms. Var. 1, Box 43.
42. For example, see Ms. Keynes 146, New College Ms. 361.1, and Yahuda Ms. Var. 1.7.
43. Newton's essay, "A Dissertation upon the Sacred *Cubit* of the Jews and the *Cubits* of the several Nations," was published in John Greaves, *Miscellaneous Works,* (London 1737), Vol II, pp.405-433. Yahuda Ms. Var. 1.2:4, 1.2:5, 1.13:2 and 1.28:5 are on The Temple and the sacred cubit. The most complete statement by Newton on the structure of Solomon's Temple (with illustrations) is in Babson College Ms. 434, "Prolegomena ad Lexici Prophetici partem secundam, in quibus agitur De forma Sancturarii Judaici."
44. Newton, *Observations, pp. 13-15 and 249-50;* and Yahuda Ms. Var. 1.1, fols 1r and 19-20.
45. Cf. letter of Henry More to Dr. John Sharp, August 16, 1680, printed in Marjorie Hope Nicolson, *Conway Letters,* (New Haven 1930), pp. 478-79.

46. See for example Yahuda Ms. Var. 1.9:22, sec.ix, fol.87 and 10B. See also, Maunel, *Religion of Newton,* pp.90-91 and 100.
47. See Newton, *Observations,* p. 278, and Yahuda Ms. Var.1.1, fols. 84 and 28.
48. On Mede, see Katherine R. Firth, *The Apocalyptic Tradition in Reformation Britain, 1530-1645,* (Oxford 1979), chap. VII; and R.H. Popkin, "The Third Force in Seventeenth Century Thought: Skepticism, Science and Millenarianism," in E. Ullmann-Margalith,ed., *The Prism of Science,* (Dordrecht 1986), pp.21)50.
49. Newton, *Observations,* pp. 252)53.
50. See Michel de Nostradamus, "Préface de M. Nostradamus à ses Prophéties, Ad Caesarem Nostradamus filium," in E. Leoni, *Nostradamus, Life and Literature,* (New York 1965), pp. 120-131.
51. On this, see R.H. Popkin, "Newton and the Rise of Fundamentalism," forthcoming.
52. William Whiston. *The Accomplishment of Scripture Prophecy,* (London 1708).
53. Newton was obviously interested in some of these prophecies. See for example, Yahuda Ms. Var.1.10B, fol.14, where Newton referred to the prophecies of Cotterus, Christina and Drabnicius, published by Comenius.
54. Newton, *Observations,* pp. 250-251.
55. *Ibid.,* pp. 251-52.
56. *Ibid.,* pp. 252-53.
57. Popkin, "Newton and the Rise of Fundamentalism."
58. This theory of Newton is expounded in Leroy Froom, *The Prophetic Faith of our Fathers,* (Washington 1948), Vol. II, pp.659-669. Newton's text is quoted on pp. 665-666. Froom was the leading historian of the Seventh Day Adventists.
59. Sir William Whitla, *Sir Isaac Newton's Daniel and the Apocalypse,* (London 1922).
60. Force, *Whiston,* chaps. 1, 4 and 5.
61. Newton, *Principia Mathematica, Mathematical Principles of Natural Philosophy,* Motte translation, (Berkeley 1936), pp. 544-546.
62. Cf. R. H. Popkin, "Newton and Maimonides," in *Essays in Honor of Arthur Hyman,* forthcoming.
63. Friedrich Schelling, *Darstellung des philosophischen Empiricismus,* in Schelling, *Werke,* Band 10, (Stuttgart and Augsburg 1861), p. 261, translated by Dr. Fritz Marti. I am most grateful to Dr. Marti for bringing this discussion of Schelling's about Newton to my attention.
64. Force, *op.cit.* pp. 23-25.

C. HAKFOORT

NEWTON'S *'OPTICKS'* AND THE INCOMPLETE REVOLUTION

1. *Introduction*

Newton's *Principia* is generally considered to be the focus or synthesis of major developments in 16th and 17th century science. Interpretations of the nature and outcome of the Scientific Revolution therefore heavily rely on the historians' view of the *Principia.* In this paper the question is raised how the picture of the Scientific Revolution changes when one tries to make sense of Newton's second masterpiece, the *Opticks.*

The *Opticks* was published in 1704 and ran through several new editions during Newton's lifetime. It contained experiments and propositions regarding "the reflexions, refractions, inflexions [i.e. diffraction] and colours of light," including a version of the famous *experimentum crucis* on the heterogeneity of white light.[1] Whatever its great merits – it probably is the best work on optics ever written – it was in two related ways an incomplete work.

In the first place, there is an incompleteness which is all the more obvious when the work is seen within the age of the corpuscular philosophy. It concerns Newton's view of the cause or nature of light. In the first edition of the *Opticks,* there was not a single statement on this topic. Newton's well-known particle or emission hypothesis appeared only as an addition at the end of the main text of the *Opticks* in the first Latin edition of 1706, and even then in a special way, viz. as one of a set of questions, or 'queries,' to be investigated by further research.[2] In the Queries Newton discussed all kinds of speculations, especially concerning the nature of light. In his eyes, these hypotheses on light and other phenomena had a much lower level of certainty than the experimental conclusions in the main text. Thus, the *Opticks* was for Newton, as well as for his contemporaries, an incomplete work.

The second incompleteness of the *Opticks* was primarily methodological in nature. Yet, the *Opticks* was praised, even by contemporary continental scholars, as a model of experimental natural philosophy. Hence,

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the second incompleteness may be merely relative, an accident compared with the supreme methodological quality of the work. In this paper, an attempt is made to define this relative methodological weakness of the *Opticks* in relation to its incompleteness in substance.

As Newton's work is considered to be the culmination of the Scientific Revolution of the 16th and 17th century, a discussion of these points is more than a simple comment on one particular work of an arbitrarily chosen man. It is part of a larger problem which may be formulated as follows: Why was the Scientific Revolution successful in celestial and terrestrial mechanics, yet at the same time relatively less successful in physical optics? Or, if one involves other fields touched upon in the Queries: Why was the 17th century relatively unsuccessful in dealing with topics such as magnetism, electricity, heat and chemistry? In short: Why was the Scientific Revolution an incomplete Revolution?

In what follows, I shall first give a preliminary discussion of key concepts used in the subsequent analysis. This will be done by introducing relevant earlier discussions of the Scientific Revolution and the *Principia,* more particularly, those by Thomas S. Kuhn and I. Bernard Cohen (section 2). This will be followed by an analysis of the methodological incompleteness of the *Opticks,* which will be based on a comparison between the *Principia* and the *Opticks*. In this comparison, I shall focus on the use of physical models and related metaphysical issues. My claim is that a discussion of Newton's attitude toward models and metaphysics – inasmuch as it can be inferred from his published work (his private attitude is of course quite a different matter) – helps to understand his relative lack of success in the field of physical optics as well as the more general incompleteness of the Scientific Revolution (section 3). In conclusion, I shall point out an incompleteness in the analyses of both Cohen and Kuhn, and sketch a somewhat different perspective upon the Scientific Revolution and its aftermath (section 4).

2. *Preliminary Clarifications*

The first concept which needs elucidation is the concept of the incompleteness of the Scientific Revolution. To my knowledge, the clearest view of this subject was sketched by Kuhn in his essay on "Mathematical versus experimental traditions in the development of physical science" (in which, incidentally, he does not make use of the well-known terminology associated with his paradigm-view of science as exposed in *The Structure*

of Scientific Revolutions).[3]

According to Kuhn, the Scientific Revolution is not a single, indivisible event. To him, the 16th and 17th centuries witnessed two rather different events which, when taken together, may be called the Scientific Revolution.[4] The first event consisted in a conceptual transformation in sciences which already existed as relatively autonomous and specialized areas of study in Greek antiquity. Kuhn calls these sciences the classical, or mathematical, physical sciences. They include astronomy, statics, hydrostatics, mathematical (i.e. geometrical) optics, and parts of physical optics. The conceptual transformations in these mathematical fields (with the exception of optics) culminated during the 16th and 17th centuries in the *Principia* of Newton.[5]

The second event which constituted the Scientific Revolution was of a totally different nature. This was not a conceptual transformation of an existing cluster of fields, but the emergence of a cluster of new fields that had been almost non-existing before the 17th century. They included the study of magnetism, electricity and heat, as well as parts of physical optics which were related to newly discovered phenomena which we now know by the names of interference, diffraction and polarization. These fields Kuhn calls the Baconian, or experimental, physical sciences. Gilbert's book on magnetism and Boyle's *Experimental history of colours* are typical examples of the experimental tradition. As we have seen, physical optics is, in Kuhn's view, a mixed field, with two more or less separate parts.[6]

The important point in Kuhn's analysis is that he draws attention to the fact that practitioners of the mathematical and experimental traditions failed to integrate the two clusters of fields and the approaches which these implied, and which were often even hostile toward each other's traditions. Kuhn even claims that, in the two centuries between 1600 and 1800, not more than three people combined the two traditions in their scientific studies. Not surprisingly, one of these was Isaac Newton, another being Christiaan Huygens (to whom I shall come back later).[7]

In Kuhn's view, the two traditions existed side by side, most often without communicating with one another, until in the nineteenth century the Baconian fields were mathematized, and merged with the classical physical sciences.[8] One could say that this event marked the completion of the Scientific Revolution, at least in the physical sciences.

My opinion regarding the picture presented by Kuhn is mixed. I think he is partly right and partly wrong. He is right in stressing the existence of different, almost non-communicating traditions and in dividing the Scientific Revolution into different processes: the conceptual transformation of an existing tradition and the emergence of new fields. Howev-

er, he is wrong, I believe, in the identification of the traditions concerned. I have argued elsewhere, using examples taken from 18th century optics, that the existence of a third tradition should be acknowledged.[9]

This tradition can be distinguished from, and actually was often separated from the mathematical and experimental traditions. It may be called the natural philosophical or metaphysical tradition, and its products include works like Descartes' *Principia philosophiae* (Principles of Philosophy), Leibniz' *Monadologie* (Monadology), or indeed, most textbooks on natural philosophy or physics during the 17th and much of the 18th century. The third tradition can be traced back to Greek antiquity and the works of Plato, Aristotle and other philosophers. Its goal is to give a global and complete account of nature. Though epistemology and methodology varied considerably in the metaphysical tradition, they always included some *a priori* elements. Kuhn gives the impression that the tradition of global, *a priori* philosophy, inasmuch as it was a relatively independent tradition, suddenly disappeared in the 17th century leaving only a few traces in the Baconian and classical traditions.[10] He may be right for the English situation where, for example, Boyle held that his mechanistic explanations are strictly *a posteriori*. However, Kuhn's account fails to cover the work of continental scholars like Huygens, some of the Bernoulli's and Euler, who all in some degree *a priori* subscribed to mechanistic metaphysics or epistemology as well as to the goal of constructing a global world view upon these foundations.

I shall not try here to substantiate this modification of Kuhn's scheme, nor do I claim to have done this in my comments on 18th century optics. However, the modification is implicit in the formulation of the present paper and in the main points I wish to make about the *Opticks* and the Scientific Revolution. I shall come back to it in the conclusion.

The second concept to be clarified is that of the success of the *Principia*. What is it that made this work successful and revolutionary?

Many historians attribute the success of the *Principia* and its exemplary function with regard to modern science not primarily to its actual doctrines or theories, but to the method by which these results were achieved or, better yet, presented. I.B. Cohen, amongst others, subscribes to this view. In his book *The Newtonian Revolution,* he has given us an enlightening study of Newton's method, or of, as he calls it, the Newtonian style.[11] Cohen's account is mainly based on a careful study of the published text of the *Principia,* and not on Newton's unpublished work which is characterized by the use of many kinds of styles. Moreover, Cohen not merely summarizes or interprets Newton's scattered remarks on method. His account of Newton's style in the *Principia* is a reconstruction of the methodology implicit in the presentation by New-

ton. If one focuses upon Newton's physical and mathematical modeling, the Newtonian style may be described as follows.

The style consists of four different phases. In the first phase an imaginary 'mathematical construct' (Cohen's term) is investigated mathematically. For example, the construct of a mass point in a central force field. It is important to note that at this point of Newton's presentation no claim is made about the physical existence of forces or anything else. In phase 2 the construct is confronted with experiments, observations and mathematical laws which have an empirical backing, e.g. Kepler's area law. As a result of this, a second, more complicated, construct is studied in a new phase 1. And thus the process repeats itself. It should be clear to anyone that this mathematical game will eventually be used to describe the real world, and that the mathematical constructs are simplified and idealized descriptions of this world. However, in phases 1 and 2, no explicit claims are made about the relation between the world and the mathematical constructs.

The descriptive content of the constructs is made explicit in the following phase, phase 3, of the Newtonian style. In this phase it is proved that the mathematical constructs provide an accurate description of the system of the world, i.e. our solar system. The imaginary mathematical force of phases 1 and 2 now is identified with terrestrial gravity, and generalized to universal gravity. Because of its success as a descriptive concept, universal gravity is not merely a mathematical fiction but part of the world. In the words of Newton, "gravity does really exist."[12] However, though the existence of gravity is a fact to Newton at the level of description, its cause is unknown to him. To search for the cause of universal gravity, or to search for a causal model as I would like to call it, is a task which belongs to an optional phase 4 in the Newtonian style (Cohen calls phase 4 the sequel to phase 3).

It is important to note that phase 4, i.e. the framing of causal models or hypotheses, is optional for Newton. The reason is not that he is not interested in, or even fascinated by, the framing of causal models for universal gravity. After all, he hinted at such a model in the second edition of the *Principia,* and provided a more elaborate exposition of it in the Queries. The reason for making the framing of causal models optional is of a different nature. Newton's view of causal models may be seen as a reaction to, or a way of dealing with, the natural philosophical tradition, especially the mechanistic philosophy of Descartes, Huygens and others. In Newton's eyes they had made phase 4, the framing of the causal model, obligatory instead of merely optional. In addition, they had reversed the order of phases: for them the causal model came first, and then the mathematical descriptive model followed. That was the rea-

son why Huygens could not accept the concept of universal gravity in a mathematical description as long as Newton could not provide a causal model of it, which was compatible with the metaphysical and epistemological standards that were prescribed by mechanicism. Newton, however, wanted metaphysics and causal models to be separated from empirical evidence and descriptive models. Moreover, in his view the causal model was not a necessary condition for the introduction of a descriptive concept like that of universal gravity; and finally, a causal model should be introduced, if at all, only *after* the empirical facts had been established and the mathematical descriptions had been provided.

It should be noted here that a critique of the distinction between descriptive and causal models is not relevant at this point because Newton, as well as the mechanistic philosophers and indeed, virtually everyone in the 17th and 18th century, accepted the validity of that distinction. However, as it will be seen below at the end of section 3, the critique may become relevant if the difference between the *Opticks* and the *Principia* is to be understood.

Let us summarize. The use of models in the *Principia* is (almost entirely) restricted to the use of fictional mathematical models (or constructs) in a specific relation to empirical mathematical descriptions, i.e. to the first three phases of the Newtonian style. This style, with its acceptance of descriptive models without causal models is a distinctive feature of the *Principia,* the conceptual transformation of 17th century astronomy and mechanics, and thus, of the Scientific Revolution in general.

3. *The 'Opticks' and the 'Principia' compared*

Having clarified the incompleteness of the Scientific Revolution and the revolutionary character of the *Principia,* we are finally able to make a comparison between the *Opticks* and the *Principia.*

The obvious questions to ask now are: Did Newton apply the Newtonian style in the *Opticks,* and if not, does this help to explain his relative lack of success in physical optics?

Cohen has discussed these questions in his study on the Newtonian revolution.[13] His answer to the first question is: Newton used the Newtonian style in the *Opticks* in one instance only. He also informs us that Newton tried to use the special style for optical problems in the *Lectiones opticae* (deposited at Cambridge university library in 1674, and

published only after Newton's death), in the *Principia,* and in several manuscripts. According to Cohen, "Newton had no real success in this goal for physical optics."[14] Cohen does not say so explicitly, but his view leads to the conclusion that Newton's relative lack of success in physical optics was due to his failure to successfully apply his style. This is an answer to our second question.

I disagree with both answers. First, on closer inspection the single positive instance in the *Opticks* turns out not to be an example of the Newtonian style, whereas Cohen does not consider the references to a modified Newtonian style in the Queries. There we learn that Newton was successful, though only partially so, in applying the style to physical optics. Why then, did he not use these results in the main text of the *Opticks?* The reason is, in my view, that Newton's methodological doctrine prevented this.

The alleged positive instance of the Newtonian style in the *Opticks* is a passage in Newton's discussion of refraction in book I. Here Newton reports an experiment on the refraction of (monochromatic) light rays in order to demonstrate the proposition that every ray considered separately follows the law of sines (now known as Snell's law) "so far as appears by Experiment." To this he adds: "And that it is accurately true, may be demonstrated upon this Supposition. *That Bodies refract Light by acting upon its Rays in Lines perpendicular to their Surfaces.*"[15] Next, Newton gives a mathematical demonstration of the sine law. He had given this proof before in the *Principia,* and he would refer to it again in a query added in 1706 to the first Latin edition of the *Opticks.* Though the mathematical content of the demonstration is essentially the same in all three cases, the relation between the mathematical model and the theory of light varies considerably.

Let us first consider the argument as it appears in the *Principia.*[16] Here the system under consideration is an imaginary particle (or "body," in Newton's terminology) approaching a region where an imaginary attractive force is acting perpendicularly to an imaginary surface. The velocity of the particle is decomposed into two components, one horizontal, one vertical to the surface. The horizontal component does not change during the passage of the force region, while the vertical component increases. It is shown that, for an arbitrary angle of incidence, the ratio of the sine of incidence to the sine of refraction is constant.

The important point is that Newton, in a scholium following the discussion of the mathematical model, carefully discusses the status of the refractive force and the status of the entity the force acts on: the particle. Here he points out something which before had not been made explicit: that there is a mathematical analogy between the path of a light ray in re-

fraction and the path of the moving particle in his imaginary mathematical model. It should be noted, however, that the analogy regards two mathematical curves, not two physical entities describing these curves. Newton explicitly declares that from the mathematical analogy he does not conclude anything "about the nature of the rays of light," especially not about the question "whether they are bodies or not."[17]

In book I of the *Opticks* essentially the same status is given to the mathematical model. There are, however, two differences. First, probably in an effort to avoid creating any wrong impression regarding the status of the model, he now replaces "a body" by "any Motion or moving thing whatsoever."[18] Again, there is no positive statement about the nature of light, either at the beginning or at the end of the mathematical proof. Indeed, at these places his term is neither 'body' nor the less compromising, but cumbersome expression 'any motion or moving thing whatsoever,' but the descriptive term 'ray.' The second difference is that in the *Opticks* he made several statements about the real existence of an attractive force in the refraction of light, which, in his view, had been demonstrated by experiments.[19] (In the *Principia* he had been somewhat more cautious about this. There, he gave a short account of inflection, which suggested the existence of an attractive force acting upon light.[20]) As in the case of universal gravitation, the claims in the *Opticks* did not imply that the refractive force was an ultimate cause. Moreover, Newton held in the optical case (though, as in the case of gravity, he was not always clear on this point) that the cause of this force was as yet to be found.

In Newton's view the undeniable success achieved by the analysis of refraction (and internal reflection which was treated along the same lines in the *Principia,* though not in the *Opticks)* did not warrant the conclusion that light particles exist, either in the sense of a descriptive concept (like universal gravity or the refractive force) or in the sense of an ultimate cause. (I shall come back to these two possibilities below.) This explains why, in book I of the *Opticks,* the co-concept of the experimentally discovered refractive force was not the light particle, but the light ray. Moreover, this is why it is only in the Queries that he suggested that light consists of particles, different colours corresponding to differently sized particles. And finally, it explains why it is only in the Queries that he used his results from the *Principia* as supplementary evidence that light consists of particles.[21]

The identification of mathematical concepts with really existing entities remained incomplete in the *Principia* discussion of refraction, as well as in the passage invoked by Cohen in book I of the *Opticks.* Only in the Queries was the identification between imaginary particle and light parti-

cle asserted (of course, as everything in the Queries, with a question mark).

Let us try and understand in some detail why, in view of his successful application of the Newtonian style to mechanics, Newton decided to limit the unrestricted use of that method in his physical optics to the Queries only. Let us consider the essential features of the basic descriptive model in the *Principia* and the basic causal model in the Queries. These models, though quite different in status, show striking parallels. In the *Principia* the simplest mathematical construct is the masspoint in a field of force. In the Queries the simplest model is the light particle affected by a short-range force, e.g. the attractive force of refraction.

Now consider the relation between the models and the real world. In the *Principia* case the connection with the world is established in phase 3 of the Newtonian style. Though mechanistic philosophers like Huygens did complain about gravity existing without being explained, they did not object to the supposition of masspoints (or extended masses), as an idealization or simplification of planets and apples. If planets and apples exist, and they certainly do, then masspoints too do exist. Analogously, in the case of physical optics, a refractive force can be seen as a descriptive concept, analogous to that of gravitational force, to be further explained by a causal model. In the main text of the *Opticks* Newton certainly wished to consider this part of the causal emission model of light as a descriptive element. But the identification of light rays with particles (as proposed in the Queries) entailed the use of a causal model. In short, Newton's critics as well as his defenders and indeed Newton himself, all agreed that using the concept of corpuscles of light was using a causal model.

However, to benefit more from the Newtonian style than he actually did in the field of physical optics, Newton 'merely' needed to use the causal particle model in the several phases of the style. But, unfortunately for Newton and for the development of physical optics, the method breaks down in that field. Using a causal model in phases 1 and 2 would violate the rule that one should not put phase 4 (the framing of a causal model) ahead of the mathematical and experimental phases 1 to 3. Thus, in the *Opticks* the successful results of the Newtonian style were not given their full weight in the main text, while they were only alluded to in the Queries.

In conclusion, it may be said that the incompleteness in substance of the *Opticks* cannot be explained by a lack of success of the Newtonian style in physical optics, but should be seen as a result of a methodological barrier consciously erected by Newton. With hindsight, Newton's methodological doctrine may be seen as too restrictive: he did not allow himself

to use unrestrictedly what may be called 'the causal Newtonian style,' in addition to the style discussed by Cohen, which may be called 'the descriptive Newtonian style.' Again with hindsight, it may be said that the restricted acceptance of the causal Newtonian style defines the relative methodological incompleteness or weakness of the *Opticks.*

Newton's methodological doctrine was not shared by everyone. Thus someone else, more tolerant in his methodological outlook might have used the Newtonian style in a causal context. A particularly important example of this is Christiaan Huygens.

Compare Newton's self-imposed strait-jacket with the method Huygens uses in his *Traité de la lumiere* (i.e. Treatise on light), published in 1690. Huygens' method in this work can, to a large extent, be characterized as the skillful and partly successful application of the Newtonian style to physical optics, but of course with the difference that Huygens uses a causal model from the very start.

In the *Traité* Huygens consciously adopts a medium or wave model of light, explicitly backed by an epistemology quite different from Newton's.[22] Huygens adapts and refines his model of light as he goes along. This is most obvious in his chapter on double refraction in Iceland spar (a phenomenon we now explain by the concept of polarized light) which Huygens looked upon as the touchstone of his model. Going backwards and forwards between model and exact experiments, he refined the medium model of light. I can not go into detail here, but my somewhat paradoxical claim is that it was Huygens, not Newton, who was the first major scientist to apply systematically and without methodological restrictions the causal Newtonian style in physical optics. However, the causal Newtonian style was buried with Huygens for more than a century. It was revived, partly under the influence of the *Traité,* partly as an independent rediscovery, in the hands of a group of French scientists at the end of the 18th and in the first half of the 19th century.

A final point, already touched upon, must be discussed here. It concerns the distinction between causal and descriptive models. We do not have to consider the critique of this distinction In order to understand that there is something definitely puzzling in the use of this disctinction by Newton and his contemporaries as far as the *Principia* is concerned. As it was seen above, in phase 3 Newton identified gravitation and, eventually, universal gravitation, with the fictional central force of phases 1 and 2. But he did more. He also identified mathematical 'bodies' with apples, planets and comets. This was taken for granted by his mechanistic opponents. But surely the latter identification was quite different from the first. The supposition of the existence of particles of matter sharing some properties, such as inertia, on earth and in the heavens, was

not challenged by the mechanicists, because it did not conflict with their view of matter as fundamentally the same in the entire universe.

Indeed, in the context of the categories used in his time, Newton's supposition was ontological in nature, and therefore, it was part and parcel of a causal model. Though in Newton's view the essential and universal properties of matter were discovered by experience and generalized by induction, whereas a mechanicist attributed properties to matter on *a priori* grounds, both ascribed to a property like inertia an ontological status.[23] Thus we are led to the conclusion that the success of the Newtonian style in the *Principia* was possible only because of the implicit and unchallenged backing of the corpuscular philosophy.

4. *Conclusion: the incomplete Revolution*

If one looks at Newton's methods as they were presented in the text of the *Principia* and the *Opticks,* the best way to understand these methods is not to stress the differences between them. Common to both methods is that, by using them, Newton reacted to the tradition of natural philosophy and metaphysics. From very early on in his career, Newton strictly separated experimental evidence and mathematical descriptions from concepts belonging to natural philosophy or metaphysics. Or, to put it in the terms we used before, he used a method with high walls between descriptive models and causal models. (Perhaps I should add here once more that I do not claim that Newton did not mix descriptive with causal models when he was at work. He did. But what is at stake here is that he did separate them very strictly, once he wrote down his results for publication.)

It appears that the strictures on the use of the causal Newtonian style in the main text of the *Opticks* is a symptom of a historical inadequacy of the descriptive Newtonian style for physical sciences other than mechanics. The descriptive Newtonian style was adequate for most of classical or mathematical physical science in Newton's days, but it failed for the mixed (classical and Baconian) field of physical optics, and for more purely Baconian or experimental fields of physical science. This, however, was not all. The *Opticks* and its Queries were perhaps not merely unsuccessful. Newton's work might have failed to encourage 18th century physical scientists to integrate causal models and experimental-mathematical descriptions; it certainly gave no clues as to how to go about in doing this. If this is true, the widely followed example of the

Opticks and its Queries was an obstacle to the progress of Baconian physical science, insofar as it was one of the factors that prevented the integration of Baconian science with the mathematical tradition and the tradition of natural philosophy. Whatever the case may have been, the least that can be said is that the *Opticks* and its Queries were symptions of the absence of a strong, fully integrated research tradition in early modern physical science.

Both Kuhn and Cohen fail to give in their view of the Scientific Revolution and its aftermath sufficient emphasis to the existence and the integration of the tradition of natural philosophy and metaphysics. Only by completing their interpretation with this element, does one gather a better understanding of the *Opticks,* its influence on 18th century science and the events in France around 1800. This may be learned from the attempt to fit the *Opticks* within the Principia-*dominated* picture of the Scientific Revolution.

As has been shown, due consideration of the metaphysical element in early modern science even changes our view of the *Principia* itself, inasmuch as it clarifies the reason for the succes of that work and of the Newtonian style. Cohen's otherwise enlightening account of the Newtonian style does not point out the role of an implicit metaphysical backing as one of the conditions for its success. This backing of the Newtonian style in the *Principia* also helps to solve one of the puzzles in Kuhn's view of the Scientific Revolution. His account leaves open the question why some of the classical fields of physical science were successfully transformed during the 17th century, while others (especially parts of physical optics) had to wait another century or two. Part of the answer might well be that, while the metaphysical backing could remain unchallenged in terrestrial and celestial mechanics, it could not in physical optics or in other Baconian areas of science. Newton himself was the most fanatic enemy of mingling mathematical and experimental certainties with ontological or causal uncertainties. This is why the kind of success achieved in the *Principia* almost necessarily implied some sort of failure in the *Opticks.*

The general acceptance of the causal Newtonian style as a legitimate scientific method, particularly in Baconian physical science, was apparently a crucial element in taking, around 1800, the Scientific Revolution beyond the point where Newton left it in the *Principia.* It also made the Scientific Revolution somewhat less incomplete.

University of Twente, The Netherlands

NOTES

1. I. Newton, *Opticks or a Treatise of the Reflections, Refractions, Inflections & Colours of Light* (London, 1704; ^{2}1717, ^{3}1721, ^{4}1730; reprint, based on the fourth English edition: New York, 1952), the quotation is from the title page of the first English edition. A Latin edition was published as *Optice* (London, 1706).
2. Newton, *Optice* (1706), 315 (qu. 21); *Opticks* (1952), 370 (qu. 29): "Are not the Rays of Light very small bodies emitted from shining Substances?".
3. T.S. Kuhn, "Mathematical versus experimental traditions in the development of physical science," in: id., *The essential Tension. Selected Studies in Scientific Tradition and Change* (Chicago etc., 1977), 31-65.
4. Ibid., 52.
5. Ibid., 35-41, 52-54.
6. Ibid., 41-48.
7. Ibid., 48-51; the third was Edmé Mariotte.
8. Ibid., 60-63.
9. C. Hakfoort, *Optica in de eeuw van Euler. Opvattingen over de natuur van het licht, 1700-1795* (Amsterdam, 1986), 168-181. See also id., "Torn between three lovers: on the historiography of 18th-century optics" (abstract), in: XVIIth International Congress of History of Science, University of California, Berkeley, 31 July – 8 August 1985, Acts, vol. I, section Pc.
10. Kuhn, "Mathematical," 43-44, 53-55.
11. I.B. Cohen, *The Newtonian Revolution. With Illustrations of the Transformation of Scientific Ideas* (Cambridge etc., 1980), 4-154, esp. 62-68. See also my essay-review of this book: C. Hakfoort, "De Newtonse revolutie en de wetenschapsfilosofie," *Kennis en methode 6(1982) 368-379.*
12. I. Newton, *Mathematical Principles of Natural Philosophy* (transl. A Motte, ed. F. Cajori, based on the third Latin edition (1726); Berkeley etc., 1934, ^{8}1974), 547 (book III, general scholium). The general scholium was added in the second edition of the *Principia (1713).*
13. Cohen, *Newtonian Revolution,* 133-141. He draws on earlier work by J.A. Lohne, D.T. Whiteside, Z. Bechler and others. See also A.E. Shapiro, "Experiment and Mathematics in Newton's Theory of Color," *Physics Today* 37(1984), no. 9 (September), 34-42. Shapiro focuses on what may be called the third incompleteness of the *Opticks.* This is the virtual absence of a mathematical theory of colours (as projected and attempted in the *Lectiones opticae* but later abandoned), i.e. a descriptive mathematical theory in terms of rays (not corpuscles), based on the sine law of refraction and a dispersion law.
14. Ibid., 136. The *Lectiones opticae* are preserved in two manuscript versions. These have been recently edited and translated; I. Newton, ed. A.E. shapiro, *The Optical Papers,* vol. I, *The Optical Lectures 1670-1672* (Cambridge etc., 1984).
15. Newton, *Opticks* (1952), 75-82 (book I, part I, prop. VI), quotation: 79.
16. Newton, *Mathematical Principles,* 226-228 (book I, section XIV, prop. XCIV-XCV).

17. Ibid., 229-231.
18. Newton, *Opticks* (1952), 79. The historical commentator may raise the point whether Newton's "any Motion or moving thing whatsoever," which formulation obviously is meant to include all kinds of medium or wave theories of light, is as tolerant as it seems. It is difficult, not to say impossible, to conceive how an attractive force on a light wave (or, more accurately, on the part of the light medium through which a light wave progresses) is able to influence the direction and speed of the wave. The point was actually raised by Euler in a letter to Lambert, 25 April 1961 (K. Bopp, ed., "Leonhard Eulers und Johann Heinrich Lamberts Briefwechsel," *Abhandlungen der Preussischen Akademie der Wissenschaften. Physikalisch-mathematische Klasse,* 1924, no. 2, 1-45, esp. 22).
19. E.g. Newton, *Opticks* (1952), 269 (book II, part III, prop. IX).
20. Newton, *Mathematical Principles, 229-230.*
21. Newton, *Opticks* (1952), 371-372 (qu. 29).
22. C. Huygens, *Traité de la lumiere. Où sont expliquées les causes de ce qui luy arrive dans la reflexion, & dans la refraction. Et particulierement dans l'etrange refraction du cristal d'Islande* (Leyden, 1690; reprint: Brussels, 1967), preface, and 1-3.
23. Newton, *Mathematical Principles,* 398-400 (book III, Rules of reasoning in philosophy, III). Rule III was added in the second edition of the *Principia* in 1713; see A. Koyré, "Newton's 'Regulae philosophandi'", in: id., *Newtonian Studies* (Chicago, 1965), 261-272, esp. 266-268.

ANDRIES SARLEMIJN

NEWTON'S PENDULUM EXPERIMENT AND SPECIFIC CHARACTERISTICS OF HIS SCIENTIFIC METHOD IN PHYSICS

The interpretation of Newton's methodology seems to lead to an inevitable dilemma: many texts are in favour of an inductivist and empiricist interpretation, others seem to imply a kind of rationalism. The dilemma cannot be solved, I think, by looking for agreements between Newton's methodological statements and our definitions of "inductivism" or "deductivism." More clarity will be achieved, in my opinion, by analysing his reaction to important experiments and by comparing this reaction with those of his predecessors or contemporaries.

Cohen, Dobbs and Westfall have shown a close connection between the pendulum experiment and Newton's scientific development.[1] Therefore, it is very probable that he had this experiment in mind when he wrote down his methodological statements.[2] It is also a fact that many other physicists of his time dealt with the pendulum or with the clock. This makes comparisons with respect to more or less the same subject possible. Therefore, we will start with some of these details from the historical context.

1. Clock metaphors and pendulum models in the 17th century.

The best known clock metaphor of the 17th centruy is that of the Cartesians who used it in order to come to their conception of nature and in order to separate mind and spirit from physical phenomena, in accordance with their dualism. Westfall (1983, p. 15) gives a splendid description of it.

> Although Descartes did not employ the analogy of the clock, later mechanical philosophers would make it the image of their conception of nature. ... One complex mechanism generated a multiplicity of operations that suggested the infinite phenomena of the infinitely complex machine we call nature, except that the cosmic clock made by the divine watchmaker required no winding up. ... The conservation of the total

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> quantity of motion, and the principle of inertia on which it rested, insured its eternal operation.

But there were other traditions. Kepler used a clock metaphor[3] 39 years before the appearance of Descartes' *Principia* (1644). His tradition is not only older than the Cartesian one but it also differs from it; Kepler conceived the planetary motion to be a magnetic process; therefore, he was not reckoned to be of the same tradition as that of the Cartesians. There is not only a difference but even an opposition between the two traditions: for the Cartesians, the clock was the starting point for understanding the Universe; for Kepler the Pythagorean harmony of the Universe was the starting point and the clock was a defective imitation of it. Kepler's tradition – as opposed to the Cartesian view – continued the Mediaeval spirit: priests were considered as mediators between God and man, urging people to live in an orderly manner and in accordance with the divine harmony shown by the cosmos. The clock of the Mediaeval idea was the symbol for the orderly way of living. From Baudoin's *Iconologia* (1644, Fig. 134) it becomes clear that this idea was continued into the 17th century.

Another tradition started with Galileo. It is true, however, that it does not use the clock but the pendulum as an instrument for understanding physical systems. From the methodological point of view, however, this difference is less important than the fact that Galileo's pendulum has – as we will see – a much stronger link with the reality which it represents than the Keplerian or Cartesian clock.

A remark in Galileo's early work *De Motu* (± 1590, p. 108) deals with the pendulum. It says, that the oscillation of a lead bob lasts longer than that of a comparable wooden one. The text shows the specific way in which Galileo dealt with the pendulum: it is considered to be a model, prototype and instrument for understanding other kinds of motion. This becomes evident from the fact that the subject of the text is not the motion of a pendulum but the trajectory of cannon balls. The analysis tries to make it clear that balls made of heavier material can be thrown farther than ones made of lighter material. This means that the different durations of the oscillation of pendula with different bobs serve as a model for understanding the behaviour of cannon balls.

The strong empirical link between pendular motion (as model) and other kinds of motion is already present in the context, in which Galileo (probably) gained an insight into the pendulum laws (isochronism, independence of the oscillation time from the bob's 'weight,' relationship between oscillation time and pendulum length and relationship between weight and duration of the oscillation). Since Viviani's story about the

swinging lamp in the cathedral of Pisa is not trustworthy, the time and the context of the discovery can only be guesswork. Viviani's story implies the year 1583. But Drake (1978, p. 21) showed that the discovery happened later (in 1588). The more interesting point in Drake's view is that he believes Galileo discovered the laws when he was watching the strings of his father's musical instruments. This intepretation is in accordance with the most important passage on the pendulum in the *Discorsi* (1638, p. 96-108) in which insights in pendulum oscillations are used to solve acoustical problems. Drake's interpretation implies that the pendulum must have been a model from the very beginning and that Galileo's pendulum represented *a stronger analogy* than Descartes' or Kepler's clock: *the same laws are valid for the model (the pendulum) as well as for the reality it simulates (strings, cannon balls, and so on).*

In the *Assayer* (1623, p. 281) only one passage is dedicated to the pendulum. Also, there it is used as a model to describe the differences between fluids and solid bodies:

> ... the impetus and the motility impressed are preserved much more strongly and for a longer time in solid and heavy bodies than in light and fluid ones. Thus we see a great weight suspended by a chord conserve for many hours the impetus and motion communicated to it by a single push.

The *Dialogo* (1623, p. 245) treats technical details of the pendulum – like the motion of a pendulum with two bobs. But in this book, the oscillation serves also to explain other motions: of cannon balls (p. 242), of the moon (p. 473), of water in a basin (p. 448), and even the ebb and the flow of tides (p. 469). This use of the model is continued in the *Discorsi;* but here the pendulum has also another methodological significance: it unites different sciences. In antiquity, pendula and strings were not and could never have been subjects of *physics* of natural motion, but only of arts (of mechanics); science and art were strictly distinguished (see Sarlemijn, 1985 for more details). The distinction between mechanics and physics was indeed respected by Galileo: the complete title of the *Discorsi* mentioned *two* sciences. But the borders between the two sciences are not important for the content of the book; although Galileo maintained a distinction between natural and forced motion, nevertheless he used the laws and concepts of a mechanical motion to explain natural ones. This unity of science in the *Discorsi* prepared the high level of abstraction in Newton's mechanics.

After the discovery of the pendulum laws (1588) and after the pendulum experiments (1602), Galileo did not immediately have the technical know-how to combine the pendulum oscillation with time measurement. Not until the final days of his life did he discover some rules for this combination. After Galileo's death (1642), Huygens' mathematical and practical genius were needed to get to a mathematically and technically satisfying description.

How did Huygens invent the pendulum clock? Whereas Galileo used the pendulum for understanding other motions, Huygens followed the opposite direction: he used Galileo's laws of falling bodies in his analysis of the pendulum. In this way he discovered (in 1657) and proved (in his *Pendulum Clock* of 1673) that only a cycloic and not an arc could ensure a regular oscillating pendulum. Huygens came to this practical insight by purely mathematical reasoning.[4] Newton must have seen this with great pleasure; because it reinforced his idea (expressed in the preface to the first edition of the *Principia)* about the unity of technical and mathematical mechanics.

In Newton's *Principia* (1687), a mathematical analysis of the pendulum is presented in the first book and a more empirical section in the second one. The first is a further development of Huygens' analysis, the second can be seen as a continuation of Galileo's way of dealing with the pendulum. Therefore, Newton's *Principia* stood, in a certain sense (which I will explain later), closer to the Galileo-Huygens tradition than the other traditions mentioned before.

2. Huygens' pendulum as a special case in the more general theory of Newton's Principia

Huygens' cycloid is a line whose path is "traced out in the air by a nail which is fixed to the circumference of a rotating wheel which revolves" on a flat plane (Huygens, 1673, p. 11). Newton not only introduced the cycloid, but also the epicycloid (constructed on the upper side of the surface of a sphere) and the hypocycloid (constructed on the under side of such a surface) and analysed the influence of gravity on bodies which follow those lines. This analysis enabled the prediction of the behaviour of pendulums rising in an air balloon or descending in a deep mine. Also, the behaviour of Huygens' pendulum can be deduced as a special case *(Principia,* p. 158). In this sense, the section of the first book must be seen as a continuation of Huygens' mathematical analysis of the pendulum. Differences between Newton's and Huygens' use of models will be shown in the next section.

3. Pendulum experiment and lack of evidence for a mechanical force of aether

The results of the mathematical analysis of the first book were used in the experimental analysis of the second. More important for our subject is the following difference between the books. The first treats of motions in an empty space without any resistance. This idealisation is left aside in the second book: its sections deals with several kinds of media which resist certain kinds of motion. Resistance is conceived here as a loss of velocity. This means that, *according to the method of the second book of the 'Principia,' a mechanical force is shown, if a loss of velocity is observed.*

In this way, loss of velocity and mechanical force became logically (by operational definition) connected. But the result of the experiment implies only a decision about the mechanical force of the medium and does *not* imply a decision about its *existence.* This is an important (and also a prudent) presupposition for the final result of the experiment: refuting the opinion

> ... that there is a certain ethereal medium extremely rare and subtile, which freely pervades the pores of all bodies; and from such a medium, so pervading the pores of bodies, some resistance must needs arise *(Principia,* p. 325).

we can distinguish two phases in the experiment. The first is a neutral (i.e. not polemic[5]) analysis of the behaviour of pendula in different media. From empirical data the specific loss of velocity in media like air, water and quicksilver was observed in order to establish a general law about the density and the resistance of a medium.

This insight was applied in the second phase which begins with a change of the pendulum: the solid bob was replaced by an empty box which was filled with different materials ("lead and other heavy materials that were near at hand"). The aim was to establish the specific influence of aether on the "internal parts" of this special bob. Although Newton compared the data from the motion of pendula with different bobs, he could not establish a significant mechanical action for aether. In order to come to a consistent interpretation of the *Principia,* it must be accepted that the result of the experiment was not the rejection of the existence of aether (because in several other texts the possibility of aether existing was presupposed or not explicitly rejected[6]), but only of it having a mechanical force. Newton's prudent conclusion says that the *mechanical* force and resistance of aether *"on the internal parts of the box will be either nil*

or wholly insensible."[7]

We can conclude that the Galilean way of dealing with the pendulum was continued in the *Principia.*[8] The first phase of the experiment can be interpreted – as I remarked before – as a neutral analysis: its aim is to obtain knowledge about the resistance of media on moving bodies 'on Earth' in general; in a similar way, Galileo used the pendulum in order to come to insights into other kinds of motion. That Newton indeed considered the pendulum as a model and instrument for understanding other kinds of motions becomes also clear from the fact that, in 1694, he did not continue to trust the pendulum as the right model to measure resistance; he then intended to experiment with bodies falling from the roof of Trinity Chapel *(Corr.,* III, p. 384-6; see also Westfall, 1983, p. 507). On the other hand, especially with regard to the first phase of the experiment, a slight difference must be accepted between Galileo's and Newton's way of dealing with the pendulum; this difference will be explained later in section 5.

In the second phase, the pendulum also functioned – like in Galileo's method of science – as a model: the study of the behaviour of the pendulum in aether served the aim of deciding the mechanical influence of aether upon the motion of heavenly bodies. The decision was negative. The experiment showed, first of all, that (a) ...

> the resistence of the empty box in its internal parts will be above 5000 times less than the resistance on its external surface. This reasoning depends upon the supposition (of the opinion mentioned before – A.S.) that the greater resistance of the full box arises not from any other latent cause, but only from the action of some subtile fluid upon the included metal *(Principia,* p. 326).

And with regard to the medium for heavenly motion the experiment justified the conclusion[9] that (b) ...

> the celestial spaces, through which the globes of the planets and comets are continually passing towards all parts, with the utmost freedom, and without the least sensible diminution of their motion, must be utterly void of any corporeal fluid, excepting, perhaps, some extremely rare vapors and the rays of light (id., p. 366).

At the end of his report, Newton remarked that he had to quote the date from memory, because he had lost his notes. For Newton scholars, this remark implies many questions such as: when precisely did Newton perform the experiment?[10] Did he produce accurate data from memory?[11] These questions are important from an historical point of view. But there is no doubt about the accuracy of the observation that "the resistance on the internal parts" was "nil." Therefore, these problems will be left out of consideration here in order to direct our attention to some specific methodological questions.

4. The pendulum experiment and Newton's scientific method

The following questions serve to bridge the distance between the detailed description of the pendulum experiment and the more abstract methodological statements of the *General Scholium* at the end of the *Principia:* (a) How could Newton have become convinced of the fact that Cartesianism in a broad sense had been refuted by the experiment? (b) How would we, in a modern language, express the result of the experiment? (c) Was the experiment a crucial one? (d) What was the position of Newton's opponents after the pendulum experiment?

(a) Influenced as he was by Descartes and Boyle, Newton himself had believed (until ± 1680) in the existence of extremely thin particles that could not be observed, but whose collisions with the observable heavenly bodies could explain mechanically the motion of these bodies. *This opinion was the most general Cartesian hypothesis; its refutation implied the refutation of any more detailed Cartesian hypothesis.* No objection could be made against this reasoning of Newton.

(b) Does the observation that the resistance of aether is imperceptible, indeed, *prove* that aether has no influence at all? This would not be a correct reading. A better formulation would be: Within the Newtonian framework *the experiment did not lead to the empirical evidence of the mechanical influence of aether,* although the truth of the opinion at stake would have implied it. It was indeed only this lack of evidence which Newton accepted, and he continued to *believe* in the existence of aether. But that aether was not a mechanical concept anymore. And this belief was not a physical hypothesis; it represented a non-physical (theological and alchemistic) program.[12] We can conclude that in the *Principia* the tasks and methods of physics *became distinguished from theology and alchemy;*[13] these two disciplines had to solve questions which were left open by the (so-called) "experimental philosophy."

(c) Was the experiment a crucial one? From a modern methodological point of view crucial experiments in a strict sense are almost impossible, because it is not easy to see how one and the same experiment could reject one hypothesis and confirm an other. But even a crucial experiment in a weaker sense is difficult (Lakatos, 1970, p. 16/7, splendidly described the different arguments which a fictitious Newtonian of the 19th century would have used in order to defend the mechanical theory against attacks based on empirical evidence; these arguments hinder a convincing crucial experiment). Let us apply this modern insight to our example: the defender of the opinion in question can object against Newton's conclusion that certain disturbing factors had eliminated the influence of the aether on the special bob. But *leaving aside methodological and psychological considerations,* we can say that experiment had a crucial significance for Newton: the refuted opinion was – as I remarked before – to him not only the most general one, it was also very much akin to his own thinking during his mechanistic period. The experiment and the refutation expressed a profound change in Newton's own thoughts.

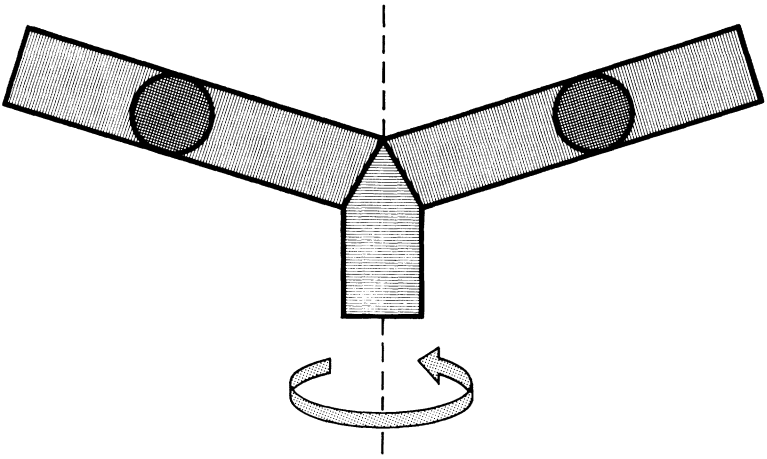

Fig. 1: Huygens' apparatus designed to demonstrate the mechanical action of gravity. Two bottles containing balls were placed obliquely upon a rotating spindle. In spite of the centrifugal force, the balls do not change place inside the bottles because these are filled with a fluid.

(d) After the pendulum experiment, Newton's opponents could try to show mistakes in Newton's data or in his conclusions, as I mentioned under (c). They could also look for other experiments, which would show in a different way the required empirical evidence. Did there exist other experiments? There were indeed many – performed by Borelli, Huygens, Hooks and others – which had to 'show' a mechanical explanation for the motion of the heavenly bodies (see Mach, 1974, p. 198/9, p. 230/1). *But these models showed but a weak analogy with the reality that had to be explained; they led to 'quasi-experiments' and 'quasi-experimental foundations.'* An example of such a quasi-experiment based on a weak analogy is the use of Huygens' apparatus for the mechanical explanation of gravity (Fig. 1). From a modern methodological point of view, we have to agree with Newton that these quasi-experiments could not produce any empirical evidence. These apparatus were based on a weak analogy; on the other hand, Galileo's and Newton's model had – as we saw before – a strong empirical link with the phenomena to be explained.

Now to the *General Scholium;* its abstract methodological sentences contain many paradoxes. Only two will be mentioned here.[14] (a) Whereas, the *Scholium* does not allow the introduction of hypotheses, Newton himself often introduced hypotheses without any empirical evidence. (b) Another paradox concerns Newton's causality postulate; whereas he required a causal explanation for the mechanical phenomena, he himself was not able to give such an explanation for gravity which formed a central part of his mechanics. Would consistency not imply that he should have waited before writing the *Principia* until he had found the required causal explanation?

The first paradox can be eliminated by a reduction of the meaning of the expression *hypotheses non fingo* and by translating it as follows: without empirical evidence I do not introduce a hypothesis in order to explain gravity mechanically with that hypothesis. This 'translation' presupposes that Newton had the pendulum experiment in mind when he wrote the *Scholium*. But this can – as we saw before – be accepted.

According to this interpretation, the hypotheses which "have no place in experimental philosophy" *(Principia,* p. 547) have two properties: (a) it is claimed that a causal explanation of mechanical phenomena can be deduced from the hypothesis, and (b) no evidence can be given. From a logical point of view, this interpretation admits all hypotheses which fail to have one of these two properties. It does not allow the introduction of an hypothesis with the first property when the second is also given (evidence fails). Therefore, no contradiction exists between the principle *hypotheses non fingo* and the fact (which we established before)

that Newton maintained the hypothesis of the existence of aether, whereas, the evidence of an aether having a mechanical force was rejected by him: he did not claim the deduction of a causal explanation from the existence of this aether.

This leads to the insight that empirical evidence and causal explanation are connected concepts and that there is a relationship between the two paradoxes. In order to obtain more clarity, it is necessary to study the causality postulate more carefully in the next section.

5. *Differences between Galileo and Newton; two characteristics of Newton's method*

The causality postulate is one specific characteristic of Newton's method and the analysis of the media of motions is a second. It becomes evident, that these characteristics are specific when we compare Newton's method with that of Galileo.

From a thorough analysis of his texts, it becomes clear that Galileo distinguished different kinds of experiments: (a) empirical, (b) Baconian, (c) thought experiments, (d) experimental tests of hypotheses, (e) nomological applications of laws in technological contexts (see Sarlemijn, 1985, 1985a and 1985b for this analysis). This list is not complete, but this is not required here. I shall, also, only explain the special meanings of these experiments as far as necessary for the comparison.

Empirical experiments are heuristic manipulations performed to obtain an empirical insight. Baconian experiments are guided by special rules which are formulated in Bacon's *Organon* and can be found in Galileo's works too. Thought experiments occupied a central position in Galileo's research. They led to the idealisation of phenomena in order to come to a satisfactory mathematical formulation for empirical connections. Galileo was very well aware of the fact that all disturbing factors were left out of consideration in the idealisation; therefore, he required for methodological reasons that these factors should not have a sensible influence on the process during the test of mathematical laws. Of course, this implied a break between thought experiments and tests on the one hand, and the technological application of the laws on the other; and the craftsman is again confronted with the 'disturbing' factors in his practice. It is a very characteristic point of Galileo's methodology that he did not try to eliminate this break. His plausible argument was the following: *the disturbing factors can be of an infinite variety and, therefore, they cannot be a subject of mechanics or any other science.* This means that the media of falling bodies is left to the practical insight of the craftsman. Whereas,

it was central to Galileo's physics in his younger days, the medium was no longer a subject of mathematical mechanics of the *Discorsi.*

This first specific point of Galileo's method is closely connected with the second one: for him the terminology of causality did not make sense in every context; that was especially the case in the context of the idealised reality of mathematical physics. Mathematical equations do not inform about things which are a cause or an effect. Therefore, Galileo did not search for a causal explanation for the motions of the heavenly bodies, nor for falling bodies: in science it is sufficient to have the required mathematical formulas. But, of course, in the context of the *Baconian* experiments causality can be introduced and Galileo has indeed done so: here cause and effect have an heuristic function. It also makes sense to ask in a *technological* context, why a body does not fall as fast as a law did make us believe. A craftsman can answer the question when he knows the specific properties of the medium; however, this answer is not a subject of science.

The two characteristics are connected in Galileo's method of science: in the context of the mathematical idealisation there is no place for causality[15] because, in Galileo's view, mathematical physics cannot deal with an infinite variety of disturbing factors.

Newton's method differs from that of Galileo on both scores: (a) the search for a mathematical formulation of the laws of resisting media formed a central part of the analysis in the second book of the *Principia,* as we have seen; (b) in the second book Newton looked also for answers to the question *why* bodies do not fall as fast as expected by a more general law.

His research was focused not only on the aether, but also on the properties of media, like air, water and so on. These were less (as we would say today) theoretical entities than aether. For the resistance of water, empirical evidence could be given; but that was not the case with aether. *Therefore, for the resistance of media like water, a causal explanation could be given, but for gravity it could not.* Thus the second book of the *Principia* justified Newton's right to say in the preface that he had found the bridge between mathematical and practical mechanics, between idealisation and 'disturbing' factors: it resulted from his search for the mathematical formulation of the relationship between the resistance and density of media.

Let us return to the paradox of causality mentioned in the preceding section: is there an ambiguity in Newton's scientific method? On the one hand, Newton discovered that the pendulum experiment could not give a mechanical explanation for gravity. On the other hand, he did not give up his quest for such an explanation. Westfall and Dobbs showed

Newton's search for an answer in theology and in alchemy (see the notes 1 and 12). Did Newton's predilection for these disciplines form the final motivation of his quest for causality in physics? Would not this imply a confusion between theology and alchemy with physics?

Newton's method of physics (I limited myself to this topic here) does not suffer from such a confusion. He interpreted empirical evidence in a strict way (as we have seen) and he had the courage to say that the quest for empirical evidence and the quest for causal explanation led to an open question in physics. He did not try to adapt his theory in order to attain an apparent harmony with the methdological quests in the physics of his time.

It is in that sense that the paradox of the preceding section must be solved: Newton's causality postulate led to an open question in physics. For Newton, the question was so urgent that he looked outside physics for answers. But this does not mean that his method of physics was inconsistent. As long as the mechanical explanation failed, the search for a non-physical explanation was, in Newton's opinion, rational.

6. Galilean and Newtonian method in the context of modern technology

The result of the preceding sections can be summarised in the following way. By paying attention to Newton's way of dealing with experiments it becomes possible to express the characteristics of his method in a modern language and show how they still can make sense. Furthermore, we saw the strong *similarity* between Galileo's and Newton's way of using models in their experimental methodology. But we also established that there was a great *difference* in their attitude toward the postulate of causality and in the manner of dealing with idealisations. The Galilean method prescribes that one must be aware that idealisations imply the abstraction of factors which are technologically relevant; this method leads to a separation between mathematical physics and technological know-how. The Newtonian method requires a new mathematical and fundamental analysis, if the influence of those factors is observable.

These two methods are different. *But that does not mean that they are necessarily opposed to one another,* provided the mathematical and technical insights are considered as complementing each other. *But in the history of science and technology these methods do often lead to antagonistic opposition.* One example is the evolution of organic chemistry during the 19th century; Leicester concludes his description of this evolution with the remark that

> A certain antagonism existed between those who followed the logic of mathematics and those who pursued the logic of organic chemistry (Leicester, 1971, p. 188).

A more recent example is the discussion between Shockley and Teal after the invention of the point contact transistor in 1947 by the scientists of the Bell laboratories. On the one hand, this invention was an important step in the evolution of microelectronics; on the other hand, this transistor was still an imperfect device. Teal worked on the development of other problems in the Bell laboratories, but he was a specialist in germanium (which was used for the transistor) and he wanted to be involved in transistor research. Therefore, he formulated some proposals in order to obtain the materials with the n- and p-conductivities which were necessary for the construction of the npn- and pnp-transistors (for more details see Sarlemijn, 1987). Afterwards he expressed his intuition in the followin way.

> I reasoned, that polycristalline germanium with ... its randomly occurring grain boundaries, twins, and lattice defects that acted as uncontrolled resistances ... would affect transistor operation in uncontrolled ways. ... It seemed to me essential to have a ... high-purity controlled composition semiconductor ... in order to ... understand the operation of the devices and thus to finally achieve an optimum use of them. ... Actually, it should be emphasized that these physical pictures with which we work and upon which theories must be built are highly idealised (Teal, 1976, p. 624, p. 627).

But Shockley totally disagreed with Teal and did not allow him to work on this program. The discussion took such a dramatic turn that Teal was afraid that he would lose his job. Whereas Teal's reaction may be regarded as Newtonian, Shockley's reaction was Galilean, since he thought that it should be left to the 'Fingerspitzengefühl' of the craftsman to find the materials with the required conductivities. Afterwards, in 1952, Shockley had to admit the correctness of Teal's ideas. This example shows how transitions from a Galilean to a Newtonian perspective also occurred in some special fields of technology.

But the impression that the 'Newtonians' are always the 'winners' is not correct. In the Sixties, physicists and chemists were working on semiconductor junctions at the Philips laboratories in Eindhoven. With their practical approach, the chemists regularly found new junctions. The physicists could describe these inventions theoretically in terms of wave

mechanics and Schrödinger equations; but they tried in vain to predict a new junction. This example shows that transitions from a Galilean to a Newtonian perspective can not always be enforced.

The historical transition from the Galilean to the Newtonian method cannot be seen as a methodological process, as if the later replaces the former. Both methods have their own justifications. The Shockley-Teal discussion shows that sometimes not even a brilliant genius is able to come to the right methodological decision. Such decisions are never without risks. We can learn from the history of science; but methodolgical prognostications cannot be deduced from past events.

Technical University, Eindhoven, The Netherlands

NOTES

1. A swinging pendulum slows down and comes to rest in an evacuated vessel as quickly as it does in ordinary air. From this observation Newton concluded in his *Notebooks*, which he wrote as a student in Cambridge, to the existence and the resistance of an "ethereal shower" which could explain the heavenly motions (Westfall, 1971, p. 336; Cohen, 1982, p. 57 and 1983, p. 114). Newton again cited this conclusion in his manuscripts of *De aere et aethere* (A.R. & M.B. Hall date these manuscripts in 1674, whereas Westfall, 1983, p. 374, and Cohen, 1983, p. 114, think that Newton worked on them in 1679; this discussion has an importance with regard to the question about the date of the pendulum experiment; see note 10). But a few lines after this citation the manuscripts break off. One of the possible reasons is that Newton "may have paused to reflect on the argument ... and realised that he was committing himself to an infinite regression in which a further aether would be required to explain the aether that explained the properties of air, a third aether to explain the second, and so on" (Westfall. 1983, p. 375). These reflections, perhaps, led to the experiment which is analysed in the *Principia*. Westfall was the first of the modern Newton scholars to call attention to the significance of this experiment (Westfall, 1967; see also Cohen, 1978, p. 104). But for some reason (see note 7), I prefer Cohen's (1982, p. 91) description of the conclusion ("the experiment did not prove that the aether does not exist, but only that the resistance of an aether (if any) is too little to manifest itself in pendulum experiments") above westfall's (1975, p. 223-4) description (Newton "convinced himself by an experi-

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resistance of an aether (if any) is too little to manifest itself in pendulum experiments") above westfall's (1975, p. 223-4) description (Newton "convinced himself by an experiment with a pendulum that the aether does not exist"; for a similar description see Westfall, 1971, p. 377; for the background of Westfall's description see note 7). The experiment plays a great part in the change of Newton's view on physics, theology and alchemy during the years 1675-90. One of the consequences concerns Newton's concepts of force and aether. But the Newton scholars do not agree on the formulation of this consequence. Dobbs (1975, p. 230) who at some points agrees with Westfall describes it as follows: "The original concept of the aether failing to stand further experimental testing, Newton abandoned it for a new concept of force which in fact incorporated the alchemical active principle into mechanism in a radically new way." Another opinion is that Newton never gave up the concept of aether, but that the mechanical concept was replaced by a Keplerian concept of the aethereal aura (Cohen, 1982, p. 92). The crucial point in the disagreement is the question: did Newton sustain the idea of an aether in 1686 (when he finished the first edition of the *Principia)* or not? The question is discussed in note 7.

2. Newton worked on both at the same time: in the second edition of the *Principia* he changed the conclusions about the experiment *and* he added to this edition the methodological *Scholium Generale.*
3. See e.g. Kepler's letter to H. von Hohenburg of February 10 1605 *(Werke,* XV, p. 146). My metaphor concept stands for the insight which is expressed by W. Pauli (1952, p. 129) with the statement "die symbolischen Bildern und archetypischen Vorstellungen sind das, was ihn (Kepler) zum Suchen nach den Naturgesetzen veranlasst." But, of course Kepler would not have used my term "metaphor" or Pauli's term "symbolisches Bild," because Kepler considered the "archetypi" as "(platonische) präexistenten Bilder" (Pauli, 1984, p. 92).
4. "... by a geometrical method we have found a different and previously unknown way to suspend the pendulum"; Huygens (1673, p. 11-2) mentioned also the two sources for his mathematical analysis of the pendulum clock: "the geometers of the present age" and "the doctrine of the great Galileo"; the mention of Galileo is interesting because of Viviani's attack on Huygens who claimed to have invented the pendulum clock in 1685. See also Dijksterhuis 1928/9.
5. Beth (1932, part I, p. 1) and Rosenberger (189, p. 198) suggest the whole content of book II has a polemic character.
6. In the scholium to the laws of motion, Newton uses the expression "terra tota in libero aethere" *(I-Principia, p. 71),* which in the *Principia* (p. 26) is translated as: the whole earth floating in the nonresisting aether. Already, the first edition contained that expression (for the importance of this remark see note 7; I quote another text of the first edition in note 13). Cohen (1982, p. 92/3) analyses some other texts which presuppose, or do not deny, the existence of aether.
7. "Quod cum ita sit, resistentia pyxidium in partibus internis aut nulla erit aut plane insensibilis" *(I-Principia,* p. 463). Newton omitted this crucial sentence – as Westfall (1971, p. 410) remarked – in the second edition and in subsequent editions. Westfall's explanation (Newton "began to admit the existence of an extremely rare aether at about the time of the second edition") is not satisfactory: already in the first edition, the existence of aether is accepted (see note 6). It is more probable that Newton never gave

during this period). He (1982, p. 91, his note 2) even thinks that the sentence "the resistance arising from any 'aethereal and exceedingly subtle medium that quite freely permeates the pores and passages of bodies is either nil or completely insensible'" can be found in all editions (only the place would have been changed). I could not confirm this; in this literal form the sentence fails in all editions (but see note 9). We can conclude that Newton must have had difficulties in formulating his insights: on the one side, the pendulum experiment showed him that aether did not have a mechanical force; on the other side, he wanted to ascribe a non-mechanical force to the aether; and this non-mechanical force should have physical effects (heavenly motions). In the second edition he decided to restrict himself to ascertaining that the mechanical opinion was refuted. And he judged that it was not opportune to say that no influence was sensible.

8. My thesis is that Newton's method in physics became very similar to Galileo's after Newton's change of mind (see notes 1 and 7). I do not deny, of course, that Galilei had only an indirect influence on Newton who, probably, never read the *Discorsi* (Cohen, 1964; Westfall, 1971, p. 402).
9. At the end of section 7 of book II, Newton summarized the results of sections 6 and 7. There it becomes clear (in accordance with Cohen's opinion, see note 7), that Newton, between the first two editions of the *Principia,* did not change his mind essentially about the physical or mechanical character of aether.
10. This question led to a new dilemma. Cohen (1982, p. 94) placed "the date of the experiment later in 1685, during the actual writing of the *Principia";* one of his arguments being (p. 59) that a new scholium to *De Motu* (originally written in November 1684) invoked still a poor example against the acceptance of a mechanical force of aether: the fact, "that sailors in the open seas when protected from the winds feel nothing at all of the continuos flow of the aether." For Westfall it must be placed about 1679, when Newton broke off his writings on *De aere et aethere* (see note 1); "if Newton performed it as late as 1685 in connection with the *Principia*'s composition, it is difficult to imagine that he would have lost the paper" (Westfall, 1983, p. 376).
11. Rosenberg (1895, p. 203): "Newton bemerkt noch dazu, dass er aus dem Gedächtnis abgerundete Zahlen gegeben habe, ... In der That haben die Zahlen nicht allzu viel Überzeugendes; ... ."
12. One of the consequences of the doctrine of *De gravitatione* "lay in the possibility that God might replace the aether Newton had already learned to employ. The role he attributed to God was exactly the role he attributed to the aether – an invisible medium that moves particles of matter in the manners in which they are observed" (Westfall, 1971, p. 341). For Dobbs (1975, p. 230) "the whole of Newton's career after 1675 may be seen as one long attempt to integrate alchemy and the mechanical philosophy."
13. This thesis is not inconsistent with Dobbs' thesis (see note 12) but complementary to it. The contemporaries did not accept Newton's demarcation. Leibniz e.g. wrote to Huygens (Oeuvres IX, p. 523) about Newton's concept of attraction: "Il semble que selon luy (Newton – A.S.) ce n'est qu'une vertu incorporelle et inexplicable au lieu que vous (in *Discours de la Cause de la Pesanteur* of 1690 – A.S.) l'expliqués tres plausiblement par les loix de la mechanique" (for other reactions: see Dijksterhuis, 1975, IV, par. 313). It was Newton's intention that the demarcation would guarantee that the question about the materiality or immateriality of the agent of gravity would remain an open question. It is true that such a decision is not implied by the definition of attraction ("I

about the materiality or immateriality of the agent of gravity would remain an open question. It is true that such a decision is not implied by the definition of attraction ("I here use the word *attraction* in general for any endeavor whatever, made by bodies to approach to each other, whether that endeavor arises from the action of the bodies themselves, as tending to each other or agitating each other by spirits emitted; or whether it arises from the action of aether or of the air, or of any medium whatever, whether corporeal or incorporeal, in any manner impelling bodies placed therein towards each other," *Principia,* p. 192). But the result of the pendulum experiment (an action of a corporeal medium is insensible) gave occasion to the theological interpretation (for similar conclusions see Westfall, 1975, p. 223/4).

14. Other paradoxes are: (a) Newton postulated an inductive justification of theories, whereas his mechanics had a strictly deductive character. (b) Explanatory factors, Newton thought, should be deduced from phenomena, whereas in a strict sense, nothing could and can be deduced from phenomena. (c) Even if we accept 'deductions from phenomena' as a rough manner of speaking, then causes cannot be deduced from effects.
15. Is this kind of idealisation due to the fact that Galileo restricted himself to a kinematic description and did not concentrate on the evolution of the principles of dynamics? Mach (1960, p. 231) seems to think so. But this judgment is, I think, not free from anachronism. Galileo emphasized the distance between the content of the idealisations of mathematical physics and the concrete factors with which the craftsmen deal. This 'distance' can be met on every level of the scientific and technological evolution and is not a specific characteristic of Galileo's kinematics.

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PART II

NEWTON'S SCIENTIFIC HERITAGE

P.B. SCHEURER

THE SURPRISES OF NEWTONIAN DETERMINISM

On the fifth day of July 1987, it will have been exactly three centuries ago that Newton's celebrated *Philosophiae Naturalis Principia Mathematica* rolled off the press into the diligent hands of E. Halley. This work may truly be regarded as the temple of Newton's determinism, and, albeit more generally and more loosely, of Newtonian determinism. The distinction is obvious enough. It is indeed a fact that three hundred years after the publication of the *Principia,* we still describe motion essentially in terms of differential equations, thereby following Newton's original choice and decision.

As can be seen from the very recent and still restrained use of B. Mandelbrot's fractals (which, besides being non-differentiable entities, are not even continuous), for the study of some complex systems, dynamics continues by and large to be formalized in terms of differential equations. More particularly, and this in spite of a widespread but erroneous opinion to the contrary, Quantum Mechanics makes largely use of differential determinism. Thus, Schrödinger's equation is as deterministic as any other differential equation. In this respect, even the great Poincaré himself failed when predicting in 1912 that, according to Max Planck's hypothesis, a proper treatment of what are now called the quantum phenomena would necessarily entail the rejection of differentiability. Poincaré's article in the *Journal de Physique et le Radium* written after the First Solvay Congress of 1911 and issued a few months before his death, definitely caused the physicists to take seriously the new and seemingly foolish notion of quanta. But history has shown by and large that the development of Quantum Mechanics, far from leading to a total rejection of differentiability - and hence of determinism - brought about the broadening of classical analysis to functional analysis in terms of operators acting upon vectors of Hilbert spaces.

Here I shall follow the characterization of Newton's determinism as given in the, by now, standard book by the great Russian mathematician V.I. Arnold.[1] Very concise and elegant, Arnold begins with the Galilean notions of physical space conceived as three-dimensional Euclidean space coordinated by x, and of time t as parameter. He observes that, in the physical world, it is sufficient as well as fully efficient to describe motion and its changes by means of a functional relation $F(x, \dot{x}, \ddot{x}) = 0$, where Newton's very own notation is used for the derivative of position x relative to time t. This expression of the causal law must be completed by

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giving an initial condition, viz. a pair $(x_o, \dot{x}_o)$ of the position x and the velocity $\dot{x}$ at the same time t_o, in order to uniquely determine the solution.

It is precisely my thesis that this structural characterization by Newton of determinism, which is apparently so simple and, as it were, so very poor, nevertheless displays a highly surprising degree of flexibility, and enables considerable reorganizations of the physical discourse used for the description of motion and dynamics, including Quantum Mechanics and the present-day so-called Dynamical systems. This thesis will be developed in five sections which will be followed by a conclusion. The subjects discussed will be:

1. The method of double coding into discursive and structural concepts. This method, first used for the resolution of contemporary epistemological problems can be traced back to Newton's *Principia* and his "De gravitatione et aequipondo" published only in 1962 by Hall and Hall.[2]
2. The birth of Laplacian Determinism due to the false belief that all differentiable functions must necessarily be analytical, and its corresponding Double Death.
3. The First Death caused by the abandonment of analyticity which ultimately was to lead to the development of Poincaré's Qualitative Dynamics into the actual Theory of Dynamical Systems.
4. The Second Death caused by the development of Quantum Mechanics as a result of the *de facto* impossibility of carrying out Laplace's program.
5. From the Discursive Revolutions of (Special) Relativity and Quantum theories to their Structural Derevolutions.

Conclusion: Toward a Grand Synthesis in our view of the physical world.

1. The Method of Double Coding

In 1971, I could lecture as Privat-Docent at the University of Geneva on "Languages and Discourses of physical theories." The purpose of those lectures was to improve upon the epistemological treatment of the problem of what it is that constitutes a theory, particularly in the mathematically formalized sciences whose paradigm is modern physics. At that time, the most fashionable approach was to consider a theory as a formal system at the syntactic level, which was provided with a semantic of models. A good example of this approach was M. Bunge's *Scientific Research*.[3] As to myself, I preferred going my own way, and I forged the

concept of a "language of structures," which was fully developed in my book *Révolutions de la science et permanence du réel.*[4] The structuralism exposed in this work is totally different from structuralism as it was developed in the French human sciences during the fifties and the sixties. It is a structuralism of the hard variety, Bourbachic in nature because derived from the work of the famous multi-headed mythical French mathematician Nicolas Bourbaki. It appeared to me that the Bourbachic constructions of relations, the scales of sets and the species of structures could be used not only to express a mathematical discourse, which was the initial purpose of their authors, but that it could be integrated into a language which could serve the discourse of other sciences, at least inasmuch as those sciences were sufficiently formalized mathematically. It should be obvious from this brief characterization that this Bourbachic structuralism goes beyond the structuralistic approaches developed during the seventies, mostly on the logical level along the lines proposed by P. Suppes, and elaborated by D. Sneed and W. Stegmüller.[5]

In those Geneva lectures, and later, in *Révolutions de la science...,* I first explicitly showed that the language of physical theories always more or less consciously refers to this language of structures, and that the discourse corresponding to those structures is always organized in terms of a bundle of regulative concepts.[6] But I had not immediately recognized that, in so doing, I had introduced an imbalance, an asymmetry between language and concept. This inbalance was all the less acceptable in view of the fact that, on another level, I had fully recognized the dialectic which is inherent to language and discourse as they appear in the historically given expressions to be found in different theories which pertain to the same domain of physical reality. I was fully aware that different discourses regarding the same object could be expressed in one and the same (structural, mathematical) language, while the same objective discourse regarding different aspects of nature could be explicated in different mathematical languages.[7]

Two examples may illustrate my point. The first refers to one single language used to represent several discourses, the second illustrates the opposite: one single discourse expressed in several languages.

The first case is paradigmatically exemplified by the so-called Copernican Revolution, an expression which was coined by Kant. The mathematical language used by Copernicus in his *De Revolutionibus* is exactly the same as the language used by Ptolemy in his *syntaxis* (which later the Arabs would call the *Almagest).* It is a language of circles, deferents, epicycles, eccentrics etc., which, in the end, roughly displays the same complexity as that of Ptolemy. This explains why, for more than half a century, the professional astronomers were not at all convinced

enough to give up their beloved habits which they had acquired through the Ptolemaic system and to shift their allegiance to the system of Copernicus whose predictive powers were ultimately not decisively superior.[8] But clearly, the Copernican revolution is effective on the level of discourse. Essentially, it exchanges the respective roles of the Sun and the Earth, whereby the Sun becomes the center of the world (or, more precisely according to Copernicus, a body nearly at the center), and the Earth a planet revolving around this center. History has shown that this exchange proved to be extraordinarily difficult to accept, primarily for strictly scientific, i.e. physical reasons. Metaphysically, the change was even less palatable.

The second case concerns Maxwell's theory of electromagnetic phenomena as expressed in different mathematical languages (which, again, are fully reciprocally translatable, but sometimes at a very high cost in conciseness). Now, Maxwell's equations are almost universally presented in terms of vectors and vector operators. This is the standard presentation in the textbooks of physics. One must remind oneself that, originally, Maxwell had expressed them in terms of Hamilton's quaternions which at the time were relatively well known, certainly better than they are now. It was only one generation later that, through the efforts of Heaviside and Gibbs, the quaternions were clearly separated into their vector and scalar parts. In the language of vectors, the theory is contained in four equations. It is well known now that Einstein reduced this number to two, by using the language of tensors. (Today, we recognize in this reduction the power of multilinearity!). But it is less well known that, when seen in terms of quantum vector fields or of differential forms, the entire theory of Maxwell can ultimately be condensed in one single equation. For example, it suffices for physics to establish the differential one-form $\frac{e}{c}A_\mu dx^\mu$ in spacetime (where e is the unit of electric charge, c the velocity of light, A_μ the vector and scalar potentials, x^μ the spacetime coordinates, and μ the index running from 1 to 4). All the remainder is given by pure mathematics, when using the operation of external derivation; that is to say, it is purely structural. Thus it can better be understood why Einstein, when establishing his theory of special relativity, was forced to maintain the form of the Maxwell equations, all the while emphasizing the Lorentz transformations, and instead, to change the form of Galilean Kinematics and Newtonian mechanics. In his "Autobiographisches," he himself says how desperately he tried for more than a year to hold on to the traditional equations of mechanics, and how he experienced near insanity before discovering the issue which to us now is so clearly revealed by the structure: for light, for electromagnetic interac-

tion, the world is four-dimensional (and it could be even more-dimensional for other interactions such as e.g. gravitation).

One could easily find many other examples, and not only in physics. But for our purpose, the situation is sufficiently clear. When faced with the epistemological question of what a physical theory is, one must cope not only with the syntax and the semantics of a language. Indeed, two complete languages, each endowed with its own syntax and semantics come to the fore: a language of discourse, and a language of structure. Each one of these is articulated into concepts, so that one must consider two classes of concepts: the class of discursive concepts and the class of structural concepts.

Let us consider some examples of each. Usually, discursive concepts that are adequate for the description of a state of affairs, of nature or, as in physics, of a part of nature, are taken - albeit more or less refined and specialized - from the vernacular and common language. Here one finds concepts such as time, space, matter, motion, velocity. Structural concepts, however, are usually taken from mathematics. Among them are the e.g. the concepts of coordinate, parameter, vector, scalar, differential etc. Sometimes concepts are ambiguous. Symmetry, for instance, functions both as a discursive and as a structural concept. In such cases, the balance between language and discourse is entirely restored.

The term 'Coding' in the expression 'Double Coding' is used in the same sense as in information theory. In no way does it imply that a certain message is disguised. All that is meant is that, in a physical theory, entities are to be expressed in a given grid of expressions, i.e. in a code of relevant symbols. The Coding is a *double* Coding for the following reason. First, the data obtained by a measurement, an experiment or an observation are coded into a first grid: the numbers obtained are most often related to certain well-determined physical magnitudes. For instance, one measures a length of 10 meters, a weight of one Kilogram, an electric field of 8 volt per centimeter, a duration of 6 seconds, etc. This is discursive coding. Secondly, the coded discursive concepts just mentioned must be coded into appropriate structural concepts in order to enter into and function within an appropriate mathematical structure. For example, a velocity is coded into a tangent vector, a force into a vector, time as a parameter or as a coordinate. An essential feature of the Double Coding is indeed the fact that there exists no fixed one-to-one correspondence between a discursive concept and a structural concept. Various discursive concepts can be coded into the same structural concept (e.g. velocity and force are usually coded into vectors) and, similarly, the same discursive concept can be coded into various structural concepts. This is the case for the discursive concept of time, which Newton structur-

ally coded into a parameter, while Einstein coded it into a coordinate. The latter example is of extraordinarily paramount importance as will presently be seen. But first, it ought to be noted that two different kinds of problems related to Double Coding present themselves. Given the first, discursive, coding, the problem may be to find an adequate structural coding within the frame of a selected mathematical pattern. The reverse problem which is well known and which frequently occurs presents itself when one disposes of a mathematical formalism while looking for its physical possible interpretation.

From the vantage point of this contemporary epistemological insight, it becomes easy to trace back the origins of Double Coding to the beginning of modern science and, more specifically, to Newton and Galileo themselves. Thus we rejoin our main topic.

It is well known that Newton carefully chose the title of his treatise in counterpoint over and against Descartes' *Principia Philosophiae.* For Newton, these two terms are now precisely defined and qualified. His *philosophia* is only *naturalis,* i.e. he considers only the science of nature; and the principles of that philosophy are expressed mathematically with as little ambiguity as possible. The aversion of the mature Newton against Descartes is well documented: no wonder that Newton intended to surpass and rival his predecessor in the science of mechanics. Hence his celebrated *"Hypotheses non fingo"* in contradistinction to the fictions and falsehoods which Descartes accepted as working hypotheses.

But now a second interpretation of Newton's title emerges, which is methodologically even more fundamental. Newton is perfectly aware of the method of Double Coding, as I shall presently demonstrate. First, both discourse and structure are present in the title itself. In the expression "philosophia naturalis," one must of course disregard the actual meaning of philosophy and translate the expression into its modern English equivalent which is "discourse about Nature." But the principles of the theory are given mathematically, i.e. as coded into structural concepts and patterns.

Yet far more important is the explicit usage of the double method ("duplici methodo") in one of Newton's so-called "Unpublished Papers" which were published for the first time in 1962 by Hall and Hall.[9] The title of the relevant text was derived from the opening words: "De gravitatione et aequipondio." Hall and Hall, as well as Herivel,[10] attribute the work to the young Newton, and give as probable date of composition the timespan from 1664 to 1668 at the latest. This dating, however, is suspect as has been argued by Mrs. J. Dobbs who argues, convincingly to this author, for a more recent date, specifically for the time at which

Newton was preparing the *Principia*. An internal analysis of this essay, in respect of both content and composition, supports the argument. Let us then read the first lines of this essay which occupies an ever more relevant position in Newton's legacy.

A few lines from the original Latin text already speak for themselves:

> De gravitatione et aequipondio fluidorum et solidorum in fluidis scientiam duplici methodo tradere convenit. Quatenus ad scientias mathematicas pertinet, (...). Deinde cum haec doctrina ad philosophiam naturalem quemadmodum affinis esse censeatur (...).[11]

Hall's translation of the complete related passage, with the exception of my more faithful translation of *"duplici methodo"* as "a double method" rather than Hall's "two methods", runs as follows:

> It is proper to treat the science of gravity and of equilibrium of fluid and solid bodies in fluids by a double method. To the extent that it appertains to the mathematical sciences, it is reasonable that I largely abstract it from physical considerations. And for this reason I have undertaken to demonstrate its individual propositions from abstract principles, sufficiently well known to the student, strictly and geometrically. Since this doctrine may be judged to be somewhat akin to natural philosophy, in so far as it may be applied to making clear many of the phenomena of natural philosophy, and in order, moreover, that its usefulness may be particularly apparent and the certainty of its principles perhaps confirmed, I shall not be reluctant to illustrate the propositions abundantly from experiments as well, in such a way, however, that this freer ("laxius") method of discussion, disposed in scholia, may not be confused with the former which is treated in Lemmas, propositions and corollaries."[12]

Surely, there can be no doubt that the double method is explicitly mentioned and that it certainly refers to resp. structure and discourse. But moreover, the entire organization of the *Principia* is contained in this one passage: Newton refers to abstract principles (axioms), Lemmas, propositions and corollaries, all categories of structure, as well as to scholia which are related to discourse. If all this had been so clearly conceived by Newton in his youth, the span of a quarter of a century which Newton

would have needed to pass to the execution of his program would indeed be a dark mystery. But if the text is seen as an essay written in preparation of Book II of the Principia, and meant as a weapon armed against Descartes, the mystery completely dissolves itself. Indeed, the essay does contain a long digression which amounts to a refutation of Descartes, the longest passage with a really philosophical content ever written by Newton.

But there is yet another text, written by the old Newton, which also contains a reference to the double method of discursive/structural coding. It is the draft review which was anonymously published by him of the *Commercium epistolicum* (1712). Newton explicitly uses two different verbs, resp. "to conceive" and "to expose," both of which have the same direct object, time. Now, it can be said without any distortion, that the object of the first verb regards the discursive concept of time, while the second regards the structural concept of time. Newton writes

> I consider time as flowing or increasing by continual flux & other quantities as increasing continually in time & from the fluxion of time I give the name of fluxions to the velocities with which all other quantities increase. (...) I expose time by any quantity flowing uniformly & represent its fluxion by an unit. (...).[13]

The expression "I consider time" stands in contrast to "I expose time." And here resides the secret of Newton's difference between mathematical and absolute time. Mathematical time is any quantity x whose derivative in respect of itself must necessarily be one, i.e. any independent variable taken as parameter (although the latter term was coined by Leibniz privately in 1673). Structurally, however, a parameter is entirely independent of, and without any relation to the manifold it parametrizes. Thus, a parameter is always "absolutus," "detached from." Nothing less, nothing more. In Section 3, I shall come back to this structural coding of time, and make of it an element of the criterium of demarcation between Newton's and Einstein's dynamics, on which the notion of structural derevolution of Special Relativity Theory is firmly founded.

Before doing this, however, it is necessary to briefly examine the birth and the double death of Laplacian determinism which represents the most canonical, iron-clad and totalitarian version of classical determinism. It will be seen how, contrary to Laplacian determinism, Newtonian determinism is in fact flexible, and surprisingly capable of accommodating new interpretations which have the reputation of having exploded determinism.

2. *The birth of Laplacian Determinism*

As it was said already in the introduction, Newton's determinism is essentially contained in his famous Three Laws of Motion. Structurally, these laws amount to differential equations of the second order. Now we know with certainty that diffentiability necessarily entails determinism, albeit a determinism of an essentially local nature. Contrary to this, Newton's laws of motion, and particularly the first law, of inertia, aim at giving a global presentation of all motion. Inertial motion is presented not only as being motion along the tangent, but it is ascribed all along the entire straight line. This confusion was related to the false belief which initially prevailed, that differentiable functions are necessarily analytic. Analyticity entails the powerful process of analytic continuation. A first solution of a differential equation of an analytic function determines a germ of a trajectory in the vicinity of the initial condition in a local neighborhood. When selecting a point of this element of trajectory within the neighborhood, it is possible to repeat the process of solution, and furthermore to extend, to continue the segment of trajectory in a new neighborhood which is in intersection with the first one. This can be continued both to the right and to the left of the line so that, ultimately, the entire trajectory is obtained from the value $-\infty$ to the value $+\infty$ of the parameter. That is to say that, if this parameter is time, the trajectory is completely determined from the most remote past to the most remote future. This encompassing knowledge excludes any possible surprise that might come from the production of novel events as time evolves. As Prigogine says, a little too emphatically, there is no more real time, there is no time at all. Such is the doctrine of Laplacian determinism, the standard classical representation of a completely determined world.

3. *The First Death of Laplacian Determinism*

This iron-clad picture was forced to gradually soften as the result of successively new insights into the nature of differential equations. First, at the beginning of the 19th century, the scandal produced by Fourier gradually led to the recognition that analyticity is a rather rare property among all possible differential functions. Toward the end of that same century, the rarity of differentiability among continuous functions was discovered. Consequently, the impossibility of generalizing the process of analytic continuation forced the rejection of discursive determinism within the constraints of locality imposed by structural differential determinism itself. Under these conditions, it proved to be increasingly more difficult

to calculate the solution of the most general differential equations. But Poincaré who was a prodigious calculator (as it is clear e.g. from his treatment of automorphous functions), taking this difficulty into account at least in practice, searched for and found a way to obtain qualitative information regarding the global behavior of the solutions of differentiable equations without integrating them explicitly. This led to his invention, a little more than a century ago, of Qualitative Dynamics, the branch of science which, in the hands of three successive generations of talented mathematicians was to evolve into the present form of the Theory of Dynamical Systems. The same applies for the phenomenon of what is now called chaotic determinism. These relatively old structures were recently used by Prigogine and others for the representation of complex physical systems. This goes to show that the novelty of these more recent theories resides much more in the discourse than in their structure. Thus, it has now been fully understood that determinism itself can lead to a behavior which has all the characteristics of chaos, and that in the phenomenon of the sensibility of a system to its initial conditions, it is possible to ascertain, by structural stability, that the collective behavior of solutions is entirely determined while this is not at all the case for individual solutions. Laplacian determinism had received its first lethal blow.

4. *The second death of Laplacian determinism*

In the first half of this century, Quantum Mechanics had condamned Laplacian determinism for reasons which cannot here be dealt with. It suffices to point out that through this new brand of Mechanics the famous Heisenberg uncertainty relations in the last analysis forced the recognition that the program of Laplace could not be fulfilled due to the impossibility of determining initial conditions. Whereas the differential law of evolution of the state of a system continues to be fully deterministic, as is the case in the famous Schrödinger equation, it is impossible to totally determine the initial condition of the state because a simultaneous physical measurement of the values of all its variables cannot be obtained. In the simplest quantum phenomena, this is the case for the values of resp. velocity and position. Geometrically, these correpond to the duality of tangent and cotangent spaces in any differentiable manifold. In terms of operators, this also represents Leibniz' rule of the derivation of a product of functions, so that it is possible to define what I call a Geometric Quantum Algebra on any differentiable manifold. Thus, the usual Quantum Mechanics can be structurally extended to a more general Quantum Kinetics.[14] Although further details of these developments cannot be

given here, the pertinent point is the fact that Quantum indeterminism is closely dependent upon differentiability which itself demands local determinism. This presents a strongly dialectical situation indeed. In short, it represents the second lethal blow to Laplacian determinism.

5. *From discursive revolutions to structural derevolutions*

I hope that the three last sections have shown that the rigidity of Laplacian determinism which all too often is still considered to represent the one and only true form of determinism, was responsible for its demise. Newtonian determinism, however, was flexible enough to be able to survive all the vicissitudes of the past three centuries and to persist, albeit in a new garb.

All in all, our present view of the development of science, and particularly of modern physics from Galileo and Newton until now, is largely mythical, both historically and epistemologically. Throughout the various episodes of this development, the continuity of what really matters is far greater than is generally thought and accepted as a well established fact. The clever discussion by T.S. Kuhn of the phenomenon of scientific revolutions constitutes the culmination of the idea that the development of science, ordinarily continuous as long as it is involved in the exploitation by a given scientific community of a paradigm agreed upon by consensus - a phase which Kuhn characterizes as "normal science" - from time to time is abruptly, suddenly, and often without warning interrupted by deep, broad and definitive breaks, ruptures and unbridgeable gaps - the so-called scientific revolutions which bring about a change of paradigm. Indeed, it has now become almost universally accepted to consider the advent of the theory of Relativity and of Quantum theory as the two major scientific revolutions of 20th century physics.

My thesis is a little more subtle. Far from me to deny the existence of a revolutionary change in our vision of the physical world as a result of those two awesome theories. It is abolutely clear that our conception of time has radically changed since Einstein. Time is no longer absolute, it is relative. No longer can it be conceived as totally independent from space. The notion of simultaneity is necessarily frame-dependent etc. And it is equally superfluous to stress the validity of the usual arguments about quantum discontinuity, indeterminism and essential probabilism. But if one seriously takes into account the epistemology of Double Coding, one must agree that all those revolutionary changes are important primarily on the discursive level. No doubt, the revolution is real, but it concerns essentially our discourse about the world. It is my opinion that,

on the level of structure, the importance of the changes is far more modest: continuity prevails in the coding of dynamics into structural schemes and patterns which can hardly be called revolutionary.

Even in the case of the often claimed incommensurability between Newton's classical dynamics and Einstein's (special) relativistic dynamics, the use of the modern structural language of differentiable manifolds enables the production of an unique, even structurally identical dynamical scheme.[15] Of course, the theories themselves are not identical. There are real differences, but these concern the difference in structural codings of the discursive concepts entering into the dynamic scheme.

In such a situation, it becomes even easy to establish a simple criterium of demarcation between Newton's and Einstein's dynamics. The previously given discursive/structural analysis of time according to Newton simply allows us to observe that Newton codes time into the structural concept of a parameter, while Einstein codes it into the structural concept of a coordinate. This simple criterium enables the total reconstruction of both theories by using the same dynamical pattern.

Of course, some adaptations must be made in order to proceed in a uniform manner. For example, Newton's Three Laws of Motion must be rephrased in a simple local sentence. This is possible provided one ceases to worship the literal expression of those laws. One should bear in mind that Newton himself has taken different positions with regard to the number of laws to be admitted. In his early *Waste Book,* he allows for two laws (more or less the first two laws); in one version of his *De Motu* from around 1684, just before he writes the *Principia,* the number has increased to six. The first number has a historical reason, the second was inspired by epistemological critical reasons pertaining to the intended content of the Three Laws. The first law concerns one single body in the Universe, the second law regards a body and the action of the rest of the world upon this one body under the form of a force. The third laws resumes and assumes the former two by the consideration of one single system in the world, composed of two parts, each of which is conceived under the action of the other in the closed world of the system.[16] Hence it is almost trivially clear that such rephrasing of Newton's three Laws of Motion applies without any change to Einstein's relativistic theory. Therefore the laws as formulated in footnote (4) may, however strange this may seem *prima facie,* be called the Newton-Einstein Laws of Motion.[17]

It would be useless at this point to give a detailed account of how, in the case of the second of the so-called major scientific revolutions of 20th century physics, the advent of Quantum Mechanics, the revolution on the level of discourse goes hand in hand with a derevolution on the structural

level. But something must be said about the possibility of extending Quantum Mechanics to a more general Quantum Kinetics. A simple criterion of demarcation between Classical and Quantum Mechanics can be established upon the discursive notion of 'state', whereby a different structural coding is provided to these two theories. Here it is relevant to quote a magnificent page by G.W. Mackey,[18] written as early as 1968, but still virtually unnoticed by philosophers of science and even by physicists.

> In the classical mechanics of n interacting particles one learns how to compute the positions and velocities of the particles at a time t given the values of these positions and velocities at any time $t_o < t$. A set of possible values for these positions and velocities is said to define a "state" of the system; thus knowing the state of any one time determines it for al future time. Quantum mechanics is a refinement of a classical mechanics which is forced upon us by the realization that it is in principle impossible to assign any meaning to a statement of the form: At the time t the positions and velocities of the particles have [such and such] values. [...]
>
> Although we cannot speak of simultaneous *values* for all observables, analysis shows that we can speak sensibly of simultaneous *probability distributions* for all observables. We therefore redefine the notion of state to be an assignment of a probability measure on the real line to every observable and find that with this notion of state we recover the determinism of classical mechanics. (pp. 57-58)

From this page, it should be easily seen why I classify the scientific derevolutions among the revolutions themselves. It is a class completely unnoticed by Kuhn. But it is obvious that scientific derevolutions entail major changes in our discursive views of the world.

Conclusion: Towards a Grand Synthesis: the World as Transformation

I hope to have kept my promise of demonstrating the surprises of Newtonian determinism. In conclusion, a last surprise will be presented, although it is by no means the least.

When one surveys from the same point of view the three centuries of Newton's legacy until the present scientific work done in the realm of change and motion, one single central theme imposes itself to one's atten-

tion. Newton's achievement completing Descartes' endeavor regarding the conservation of (the quantity of) motion is actually the first noticeable tip of an iceberg: the cristalline mountain of physical symmetries. The principles of conservation of the quantity of motion, of angular momentum, then again of energy, have been recognized as the classic contributions to an universal principle of conservation of symmetries. Structurally, this principle is based on the now famous Nöether theorem which was demonstrated in the second decade of this century. At the present time, this principle has been enlarged as to also encompass abstract symmetries, as these manifest themselves in the present gauge theories and supersymmetries. For the first time, a reconciliation of the separate worlds of microcosmos and macrocosmos can seriously be envisaged. There is the promise of a grandiose synthesis in respect of matter, force and spacetime. It is well known that such synthesis was the object of Einstein's unfortunately premature dream of an unitary theory. It was also the secret dream of Newton who was publicly proud of his great synthesis of all large motions, as can easily be detected in his "System of the World." But Newton was also secretly aware of his failure regarding those

> innumerable other local motions which on account of the minuteness of the moving particles cannot be detected, such as the motions of the particles in hot bodies, in fermenting bodies, in putrescent bodies, in growing bodies, in the organs of sensation and so forth.[19]

In a conclusion to the *Principia,* which Newton never published and from which this last quote is taken, he confessed that he had "least of all undertaken the improvement of this part of philosophy." ("Philosophiam hac in parte excolere minime suscepi").[20] And he continues: "I may say, briefly however, that nature is exceedingly simple and conformable to herself" ("simplex et sibi consona").[21] Natura, simplex et sibi consona. These words sound like the announcement of the majestic ontological synthesis which is finally open to us, three centuries later. This synthesis will not here be further characterized. But it can be evoked in the expression: the World is Transformation.

Catholic University, Nijmegen, The Netherlands

NOTES

1. Cfr. V.I. Arnold, Mathematical Means of Classical Mechanics (Moskow: Mir, 1972 sqq.). By the way, much of the mathematical apparatus used by Prigogine and by present-day chaotic determinists is to be found in the numerous and ample appendices provided by Arnold who considers that apparatus as an integral part of classical mechanics.
2. See also his Draft of the Commercium Epistolicum of 1712.
3. M. Bunge, Scientific Research I and II (New York: Springer, 1967).
4. P.B. Scheurer, Révolutions de la science et permanence du réel (Paris, P.U.F., 1979). As it is the usual fate of epistemologists writing in the French language, this book is well known in France but virtually ignored in the rest of the world.
5. I have coined the expression of the "4 S View of Theories" to designate the Suppes-Sneed-Stegmulller Structuralist view of Theories.
6. I borrowed the expression "regulative concepts" from S. Toulmin.
7. Of course, their very mathematical nature always allows for the complete translation of one such language into the others, but their use by physicists can differ considerably, depending on the difference in their heuristic power.
8. It is amazing as well as ironical that this language is part of Apollonius' language of the conic sections, which Kepler would stumble upon when discovering the elliptic paths of the planets. From this perspective, even Copernicus's search for a greater Platonic purity which accepted only uniform circular motions, as well as his rejection of the Ptolemaic notion of the equans can be seen as a kind of regression. The existence of two relevant points in a circle, the center and the equans, was in a sense a prefiguration of the two foci of an ellips. But history did not take advantage of this structural opportunity.
9. A.R. Hall and M.B. Hall, Unpublished Papers of Isaac Newton (Cambridge: U. Press, 1962).
10. John Herivel, The Background to Newton's Principia (Oxford: Clarendon, 1965).
11. See A.R. Hall and M.B. Hall, op. cit., p. 20.
12. Op. cit., p. 121.
13. I.B. Cohen, "Newton" in C.C. Gillispie: *Dictionary of Scientific Biography* (New York: Charles Scribner's Sons, 1974), X, pp. 49-50.
14. P. Scheurer, "Quantum Kinetics; An Extension of Quantum Structure beyond Mechanics," in *Arch. Sc. Genève, 40* (1987), pp. 57-64.
15. P.B. Scheurer, "Reversible Motion and Irreversible Evolution. Quantum Kinetics and the Postulate TEI," *Arch. Sc. Genève 37* (1984), pp. 229-264.
16. This can be formalized in a self-evident way:

    Law I: $p = m\frac{dx}{dt}$, then p is conserved along the tangent: $\frac{dp}{dt} = 0$.

    Law II: $\frac{dp}{dt} = F$

    Law III: $p_{(I+II)} = p_I + p_{II}$; $\quad \frac{dp_I}{dt} + \frac{dp_{II}}{dt} = 0$

17. Another evident adaptation concerns the metric. As time, a parameter for Newton, completely without relation to the manifold space, becomes integrated by Einstein as a fourth coordinate of the manifold spacetime, something has to be said about the consequences of this incorporation for the metric, and for the physically dimensional relation between length and time (a constant of homogeneity has to be introduced; evidently, it is c , the velocity of light). See reference of footnote 15.
18. G.W. Mackey, *Induced Representation of Groups and Quantum Mechanics* (New York: Benjamin, 1968), pp. 57-58.
19. A.R. Hall and M.B. Hall, Unpublished Papers of Isaac Newton p. 333.
20. Ibid. p. 321.
21. Ibid.

DENNIS DIEKS

NEWTON'S CONCEPTION OF TIME IN MODERN PHYSICS AND PHILOSOPHY

It is today a commonplace remark that Newton's conceptions of absolute space and time were coloured by philosophical and theological ideas, partly through the influence of such thinkers as Henry More and Isaac Barrow. The emphasis that is often laid on the importance of influences of this kind however tends to obscure the fact that nevertheless there is a clear sense in which the Newtonian concepts are - at least in part - determined by empirical evidence; Newton himself provided important arguments to demonstrate this latter point. As I want to emphasize in the present paper, it even is the case that Newton's concepts of uniform time and absolute simultaneity are *uniquely* determined by empirical evidence (this in contradistinction to his concept of absolute space). I furthermore want to point out that variations of Newton's arguments appealing to empirical data can also be applied in the context of modern physical theories. It can be shown in a straightforward way that within the theory of relativity these arguments directly yield the standard simultaneity relation. This leads us to the conclusion that, from the standpoint of a "modern Newtonian," the standard simultaneity relations in classical mechanics and in the theory of relativity possess the same status.

The result just mentioned stands in contrast to the view (wide-spread in the contemporary philosophy of science) that the simultaneity relation in modern physics is conventional (in the sense of being undetermined by empirical data) in a way it is not in Newtonian mechanics. I submit that the physically interesting difference between simultaneity in classical and in relativistic physics is not a difference in empirical status, but rather a difference in the *structure* of time in the two theoretical contexts. Although time in relativity is empirically determined in the same way as in classical physics, it cannot be interpreted as the parameter of the developing history of one single entity. It may well be that the intuitive background to the philosophical thesis of conventionality consists exactly in the feeling that the relativistic notion of simultaneity cannot be considered as simultaneity in a *literal* sense, because it lacks the properties, expected by common sense, which the Newtonian concept possesses. Thus it is suggested that the relativistic notion is not *really* simultaneity, that consequently it cannot be said to correspond to anything objectively existing, and that, finally, it therefore must be a *conventional* element of

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the theory. The happy marriage that existed for Newton between physical argumentation based on empirical evidence on the one hand and metaphysical interpretation on the other, after many years has ended in a divorce with the advent of the theory of relativity.

Newton's empirical arguments for absolute space and time and their status from a modern perspective

In the Scholium to the Definitions of his *Mathematical Principles of Natural Philosophy,* Newton makes a distinction between "absolute, true and mathematical time" and "relative, apparent, and common time." As Newton points out, this distinction is not artificial; it is part and parcel of the daily practice of astronomy.

> Absolute time, in astronomy, is distinguished from relative, by the equation or correction of the apparent time. For the natural days are truly unequal, though they are commonly considered as equal, and used for a measure of time; astronomers correct this inequality that they may measure the celestial motions by a more accurate time. ... The necessity of this equation, for determining the times of a phenomenon, is evinced as well from the experiments of the pendulum clock, as by eclipses of the satellites of Jupiter.[1]

What is being said here is that in astronomical practice "absolute time" is fixed in such a way that certain events (e.g., the ticks of a pendulum clock, the eclipses of the satellites of Jupiter) occur at equal intervals. In other terms, it is stipulated that certain processes, when measured by absolute time, neither accelerate nor slow down. The idea clearly is to single out those processes which are free from disturbing influences and which can therefore be expected to maintain the same pace.

Indispensable for Newton's strategy here is the notion that it is possible to ascertain what forces are at work without a prior decision as to the measure of time. Apparently, Newton proceeds from a particular idea of causal efficacy: that forces are linked to sources – which must be material bodies – and that their strength is determined by the (relative) distances from those sources. If there is knowledge about the mechanism of a process, and consequently about the forces at work, then it is possible to judge whether or not the process is being accelerated or slowed down. Thus the right choice of the measure of time consists in choosing the measure which reflects this state of affairs.

The crucial point in Newton's argumentation is the idea that absolute time and absolute space are such that a description and explanation of motion in terms of these concepts reflects the "true causal order of the world" in the sense that only physical forces between material bodies figure in the explanation of accelerations. In his discussion of how to prove empirically the existence of absolute space (a discussion which culminates in the famous "bucket experiment"), he writes:

> The causes by which true and relative motions are distinguished, one from the other, are the forces impressed upon bodies to generate motion. True motion is neither generated nor altered, but by some force impressed upon the body moved; but relative motion may be generated or altered without any force impressed upon the body. ... Again, true motion suffers always some change from any force impressed upon the moving body; but relative motion does not necessarily undergo any change by such forces.[2]

Newton's concrete application of these criteria for absolute motion is well-known. In a rotating frame of reference material bodies undergo accelerations without a physically acceptable cause. This means, according to Newton, that those accelerations are not *really* there; they are an artefact born from the choice of the frame of reference. The bodies move uniformly in respect of a reference frame which rests in absolute space.

What can be said of the cogency of Newton's arguments? At first sight, their apparent circularity seems to threaten their validity. Evidently, Newton *presupposes* a particular type of explanation in physics: one according to which bodies move uniformly and in a straight line when no external causal factors influence their motion, and moreover one in which all causal influences (forces) are identifiable with material sources. *Given* this restriction regarding the form of mechanical laws, absolute space and time are found to be the coordinate system in terms of which the laws indeed do have the desired form. But, so it may be argued, if one starts with *another* form of the mechanical laws, another preferred frame of reference will be found. For instance, if it be required that a body which is not influenced by other bodies describes a spiral, a restframe will be determined, which rotates in respect of Newtonian absolute space. Now, the point a critic of Newton might make is that it cannot be maintained *a priori* that spiral-like motion is "unnatural," and that it requires an explanation in terms of forces; what requires explanation and what does not, depends upon the particular form of the explanatory scheme that is being used. But the very form of the appropriate explanatory scheme depends

upon the choice of a preferred frame of reference! Therefore, Newton's arguments do not succeed in showing that there is but one frame of reference in which causal explanation is possible. All frames of reference are equally possible in this respect; only the form of the causal explanation differs from one frame to the other. Even if it should be further stipulated that every causal explanation has the essential characteristic that it prescribes that the "natural motion" of material bodies must be uniform and take place along a straight line, the situation remains essentially the same. For even then every frame of reference, regardless of its state of motion and measure of time, can be used as a rest system; but now one must introduce what Reichenbach in an analogous situation has called "universal forces."[3] Such forces would accelerate all bodies in the same way, regardless of the chemical or physical constitution. For example, we might use rotating frames as restframes in this way if we accept centrifugal and coriolis forces (which have the required universal character) as bona-fide physical forces and as grounds in causal explanations.

But if Newton's arguments thus fail to establish that only one frame of reference is possible for the formulation of causal laws, what is the status of Newtonian absolute space and time? What is the content of the assertion that absolute space and time do exist?

Clearly, every frame of reference can be used to give a consistent account of the empirical data. But then again, the question Newton is addressing and answering is clearly not whether demands of *consistency* determine a particular frame. Rather, Newton maintains that the data *physically support* the notion of a causally passive space. Of course, this means first of all that it is *possible* to provide a treatment of motion, according to which there is complete spatial *homogeneity* and *isotropy* and in which all inhomogeneous features of physical processes can be linked by forces to material sources. The important additional point, however, is that this treatment of motion is *maximally physically supported* because it introduces nothing beyond what is already inherent to the observable phenomena themselves prior to the determination of, e.g., synchrony. Purely local (in space and in time) physical properties do not single out any preferred location or direction in empty space. The *empirical claim* Newton is making can now be construed as follows: "A treatment of the empirical material according to laws of the mentioned type (following the *Leges Motus)* is indeed possible." In other words, here we have an empirical existential claim: there exists a framework in which all departures from homogeneity and isotropy can be accounted for by forces emanating from material bodies. Newton's discussion of the bucket experiment involves an attempt to prove uniqueness: it is shown that rotating frames necessarily require the use of *another* form of the laws of motion.

As is now of course well-known, Newton's attempt to prove the empirical uniqueness of absolute *space* cannot actually succeed. Only the full class of inertial systems is singled out by the above requirements. But the situation is different for absolute *time*. The form of the Laws of Motion uniquely determines the measure of time *and* the simultaneity relation, as a simple calculation immediately shows. If the measure of time or simultaneity is chosen in a non-standard way, the motion of a freely moving body will generally have to be described as accelerating or decelerating, and at the very least as being direction-dependent. In the general case the acceleration will fluctuate wildly according to the position. Newton's argument concerning the "equation or correction of the apparent time" therefore *does* prove the empirical uniqueness of absolute time within the context of classical mechanics.

Of course, it may be retorted that demands regarding a particular form of the physical laws are relevant for matters such as simplicity of description, theoretical elegance, etc., but have nothing to do with truth. This is the line taken by many modern empiricist philosophers, for instance Reichenbach in his work on the Philosophy of Space and Time.[4] Following this line of reasoning one must say that Newton's arguments for absolute space and time only demonstrate that an especially *simple* form of mechanics is obtained if certain frames of reference are used; but this does not furnish objective grounds for thinking that, e.g., Newtonian absolute time is in any sense the "real" time. Since all reference frames can be used to give a consistent treatment of the empirical data, all such frames are in principle equivalent.

However, Newton does not argue merely for a choice from the many consistent possibilities, but for the determination of that treatment which is maximally *physically supported.* The empirical possibility of considering particles outside the sphere of influence of other material bodies as being in rest or as moving uniformly in a straight line can accordingly be seen as a significant *fact* about nature, indicative – among other things – of spatial homogeneity and isotropy. It must be remembered here that such homogeneity and isotropy can be established independently of any synchrony. Contrary to this, descriptions involving inhomogeneity and anisotropy do not receive independent empirical confirmation. The standard measure of time and standard simultaneity are chosen so as to make this fact *manifest* in the formulation of physical laws. Thus, simultaneity becomes a *physical relation,* reflecting a particular physical condition, rather than a merely conventionally chosen equality of time coordinates. From a Newtonian perpective, genuine *temporal relationships* must therefore be seen as opposite to mere *correlations between coordinates* which only serve as markers and whose choice is inherently conventional.[5]

Newton's arguments in the context of relativity theory

After this extensive discussion we may be brief about the implications of Newton's arguments for the theory of relativity. His reasoning can be carried through in an entirely analogous manner; the requirement that "every body perseveres in its state of resting or moving uniformly straight on, except inasmuch as it is compelled by impressed forces to change that state ..." is already sufficient to determine uniquely a measure of time and a relation of simultaneity within each inertial frame of reference. Of course there is an important difference with Newtonian mechanics: this standard simultaneity relation does not assign a unique time difference to any two events regardless of the inertial frame which is chosen. In this sense, Newton's arguments no longer single out an *absolute* simultaneity relation. But *within* every inertial frame simultaneity is uniquely fixed (in special relativity, this leads to a global relation of simultaneity; in general relativity the relation is defined only locally. The situation in general relativity needs additional discussion, e.g. in connection with Mach's principle. That would, however, outstep the bounds of this paper). Thus, to the same extent that the laws of motion determine the simultaneity relation in classical mechanics, they also determine the standard simultaneity relation in the special theory of relativity. The doctrine that simultaneity in relativity is more conventional than in classical mechanics (Reichenbach, Grünbaum) receives no support from an approach in the Newtonian spirit, which tries to invest the temporal relations with physical significance. We may add that Newton's standpoint here seems to be representative of that of the majority of working physicists.

Nevertheless, it must be admitted that simultaneity in relativity has something elusive; a comparison with classical simultaneity suggests that it has a certain "looseness" which in turn easily invites the idea of arbitrariness. But what is really involved here is the intuitively strange feature that relativistic simultaneity is not *absolute*; philosophical doctrines about "conventionality" are not at issue. In classical mechanics absolute simultaneity can easily be interpreted in terms of the *"flowing now"* we know from introspection. Just like our personal *"now"* is connected with our progressing history, the universal *"now"* determined by the absolute simultaneity relation may be regarded as related to one history, e.g. the history of the Universe as a whole. For Newton *God* endures, and thereby constitutes time. This naturally fits in with the conception that there must be a universal succession of moments, determining the existence of one entity. Without the theological overtones, one might say that for Newton *space* endures through time. In any case,

there is *one* history, with *one "now,"* dividing past and future. However, the picture that emerges from the theory of relativity is not one of a universe which is completely ordered with respect to earlier-later, unless one specifies a particular frame of reference. The only order which is defined *regardless* of a specific choice of inertial system is the partial order along world-lines. It *is* possible to interpret this partial order in terms of "flows of time"; but this can only be done if we abandon the idea that there is *one* "flow of time" representing the history of one single entity. But, as will be shown presently, we have many histories, each one of which is associated with a particular world-line and progresses independently.[6]

The central intuition behind the idea of a flow of time is that the future is ontologically different from the present and the past because it is "open," "not yet real," or "ontologically indefinite." There is a moving "now," continuously changing the future into the present and the past and thereby making events ontologically definite. It is clear that there exists a tension here with special relativity because of the *universality* of the "now." However, it is not difficult to provide a natural generalization of the doctrine, that makes it compatible with relativity.

To this end, let us first confine our attention to the history of one single particle, object or observer – one single world-line in a space-time diagram (we do not consider the complications brought about by quantum theory). The idea of a flow may be implemented in this case by singling out "now-points" on the world-line in question. Each point on the world-line has to occur exactly once as "now-point." Thus we obtain an infinitude of space-time diagrams, each of which is individuated by different locations of the now-point; these diagrams may be ordered according to the order of the linear continuum. This complete linear order of now-points is the theoretical representation which fits the Newtonian conception of time. Now, our actual universe of course contains more than one world-line; perhaps it contains an infinite number of them. The theory of relativity demands that all these world-lines be treated equally. This entails that it is not appropriate to define one universal "now"; instead, a different now-point must be assigned to every single world-line. In this way, one obtains a division of every world-line into a future part and a past part. An arbitrary assignment of now-points to the the world-lines will not do, however, for the following reason. The idea of a flow of time combined with the ontological definiteness of present and past requires that everything in the past lightcone of an ontologically definite event be also ontologically definite. This entails the demand that no now-point should lie within the conjunction of the past lightcones of the other now-points.

Provided the restriction just mentioned be respected, one obtains a consistent assignment of now-points to the world-lines. The part of the space-time diagram contained within the conjunction of the past lightcones of all now-points may be interpreted as the space-time region of ontological definiteness; the conjunction of all future lightcones may consistently be regarded as "containing physically, i.e. ontologically real alternative possibilities." As before, one obtains an infinitude of space-time diagrams, each of which is distinguished by its own assignment of now-points. But now it is no longer possible to order these diagrams according to the order of the linear continuum. Instead, our collection of space-time diagrams is a set that is only *partially ordered.* The partial order is generated by the usual linear order along world-lines; there is in general no order relation between diagrams that are generated from one original diagram by moving the now-points along two different world-lines (if the restriction mentioned above is respected). Although we do not achieve in this way the simple order usually associated with the "flow of time," we obtain a natural relativistic generalization that can be thought of as a "flow of time *per world-line.*" As was mentioned already, this reflects the fact that the theory of relativity does not impose a total temporal order upon the space-time continuum but only a partial order.

Thus we see that the happy agreement which existed for Newton between empirical arguments for a particular structure of time and the (partly metaphysical) intuition no longer exists in the theory of relativity. Newton's argumentation relying on empirical evidence can still be used in relativity; and it is suitable to combat claims about the conventionality of simultaneity within inertial systems. But the ensuing notion of simultaneity does not satisfy the intuition which asks for an interpretation in terms of a developing history. The relativistic space-time picture *can* be interpreted in terms of physically objective "flows of time"; but then simultaneity plays no part in this interpretation. Therefore, if one follows the intuition that the idea of a "flow of time" represents an objective and essential feature of the physical world, it may be concluded that the relativistic notion of simultaneity is insignificant, since it can have nothing to do with such a flow of time. On the other hand, if physical theory is taken as the only guide, there is arguably nothing that clearly supports the notion of a physically objective flow of time,[7] and simultaneity (although not absolute) retains its former role. In either case something of the attractiveness of the original Newtonian position is lost.

University of Utrecht, The Netherlands

NOTES

1. Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World (F. Cajori, ed.), Greenwood Press, New York, 1969; pp. 7-8.
2. Op. cit. note 1; p. 10.
3. H. Reichenbach, The Philosophy of Space and Time, Dover Publications, New York, 1957; section 6. See also: D. Dieks, "Gravitation as a Universal Force," Synthese (forthcoming).
4. Reichenbach, op. cit. note 3; section 27.
5. For more on this point, see: D. Dieks, "The 'Reality' of the Lorentz Contraction," Zeitschrift für allgemeine Wissenschaftstheorie 15 (1984) 330-342; esp. sect. 4.
6. See also: D. Dieks, "Special Relativity and the Flow of Time," Philosophy of Science (forthcoming).
7. A. Grünbaum, Philosophical Problems of Space and Time, D. Reidel Publishing Company, Dordrecht, 1973; chapter 10.

F.H. VAN LUNTEREN

GRAVITATION AND NINETEENTH-CENTURY PHYSICAL WORLDVIEWS

The reign of Newton's theory of gravitation spanned a period of roughly two centuries. Much has been written about the original resistance it encountered, its gradual acceptance and its overwhelming success in the hands of the continental mathematicians of the eighteenth century. Equally well-known is the eventual dethronement of Newton's theory by Einstein's general theory of relativity. The conception of this theory, stemming from the equivalence of inertial and gravitational mass, its striking confirmation, involving a solar eclipse, and the resultant worldwide celebrity it brought its originator make for a popular story. Far less attention, however, has been paid to the attitude of nineteenth-century scientists towards the Newtonian theory, in spite of the fact that proposals of explanatory mechanisms, sometimes involving modification of the force-law, abounded in the second half of the nineteenth century.[1] This neglect is probably due to the sterility of these proposals and the fact that gravitation as a separate field of reseach did not exist during this period. This paper does not aim at filling this gap by treating any of these theories in detail or by presenting a general survey. Instead it will focus on the nineteenth-century change in attitude towards the nature of gravitation.

But let us first address the following preliminary question. Why would one try to revise a scientific theory? Two obvious answers may be found in the empirical inadequacy of a theory or (perhaps less obviously) in its unaesthetic complexity. Still there may be other motives. For instance the urge to fit a theory into a favoured epistemological or ontological framework, in other words to incorporate it in a physical worldview. Thus one may model a theory after one or more related theories or even unify two or more theories in order to achieve an even stronger synthesis. A related motive may be the incompatibility of a theory with a general physical principle. These last two factors were manifest in the attempts to modify Newton's theory.

Around 1800 Newton's theory of gravitation was generally seen as the paragon of science. The original resistance it had encountered, largely due to its apparent incompatibility with the prevalent mechanical philosophy, had vanished under the pressure of its instrumental value in accounting for a large variety of phenomena.[2] The former problem of the

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action-at-a-distance character of the theory was either ignored in accordance with Newton's supposed dislike of hypotheses in this matter, or denied by allotting activity in nature to God's omnipotence.[3] Some even argued that forces should be considered epistemologically equivalent or even prior to matter.[4]

The instrumental attitude towards distance-forces flourished throughout Europe, but particularly in France. It was here that a group of scientists headed by Laplace cultivated a programme aiming at the mathematisation of the 'physique instrumentale.' An important pillar of this endeavour consisted in the assumption of a host of long-range and short-range central forces acting between the hypothetical particles of light, heat, electricity, magnetism and ordinary matter.[5] By transferring the Newtonian approach from celestial mechanics to physics they expected to 'be able to raise the physics of terrestrial bodies to the state of perfection to which celestial physics had been brought by the discovery of universal gravitation.'[6] They thus firmly established what the historian Merz aptly called the 'astronomical view of nature.'[7] When this approach came to be criticized, it was not for the instrumentalist attitude towards distance-forces, but because of its reliance upon the hypothetical imponderable fluids.[8]

In Germany the situation was somewhat different. Here physicists tended to be more suspicious of mathematics, the emphasis being upon qualitative experimentation.[9] Two opposing traditions may be distinguished. On the one hand there were those who assumed matter to consist of indivisible and impenetrable solid particles, remaining agnostic with regard to the nature of the interparticulate forces, on the other hand, those who, adopting Kant's 'dynamische System,' argued for the primacy of forces, matter resulting from the opposition of two 'Grundkräfte,' one repulsive, the other attractive (i.e. gravitational).[10] In Britain similar traditions can be encountered, though divided along less clear-cut boundaries.[11]

One thing practically everyone agreed upon was the deficit of the mechanical explanations of distance-forces that had been proposed during the two preceding centuries.[12] Even those who did not accept forces as legitimate physical causes dismissed the search for mechanical explanations as speculative and considered the goal unattainable, thus anticipating du Bois-Reymond's well-known 'ignorabimus.'[13] Apart from the profound change in physical worldview and methodology since the seventeenth century, reflected in the ritual character of this overall rejection, there were also a number of technical difficulties regarding mechanical explanation, which were considered to be practically insurmountable.

A frequently encountered physical objection consisted in the following paradox: any gravitational fluid had to be extremely rare to ensure the heavenly bodies of a free passage and at the same time powerful enough to move these massive bodies with such a tremendous force.[14] Another popular objection, already put forward by Newton himself, was furnished by the fact that the force is proportional to bulk of matter and not to surface-area.[15] This fact required an enormous porosity of matter for any gravitational fluid. From the absence of relevant discrepancies in the lunar motion Laplace had calculated a minimum-value for the porosity besides an incredibly high minimum-value for the velocity of propagation of any mechanical cause of gravitation, thereby rendering such a cause less probable.[16] These arguments may have had some importance, they certainly were not decisive, as later developments proved.

What was it that caused the revival of mechanical explanations of gravitation, what directed the attention of scientists toward the nature of gravitation? Perhaps the discovery of astronomical anomalies? This answer may appear seductive, given for instance Adam's proof of the inadequacy of gravitational action to account for the secular acceleration of the moon's mean motion in 1853 and 1859 and Le Verrier's enunciation in 1859 of an anomalous advance in Mercury's perihelion.[17] Unfortunately the answer proves not to be correct. Astronomers preferred to explain these anomalies by resp. tidal friction and the presence of intramercurial matter.[18] Besides throughout the century theoretical astronomers kept up an instrumentalist attitude towards the theory of gravitation. In a few cases they were prepared to try modifications of Newton's law in the face of emperical discrepancies, but seldom in the context of a deeper explanation of the gravitational force.[19] On the other hand physicists who did propose deeper explanations, rarely pointed to astronomical anomalies to support their attempts. In the realm of physics the search for nineteenth-century empirical anomalies proves even less fertile. The scarce terrestrial experiments concerning the force of gravitation only served to enforce the general belief in its universality.[20] Accepting the assumption that empirical factors played a negligible part in the revival of gravitational theories and given the fact that theoretical astronomers were far more interested in calculation than in speculation as far as gravitation is concerned, it is only natural to turn one's eyes to overall developments within physics.

As a physical agency gravitation occupied an isolated position, largely due to its universality. Light, heat, electricity and magnetism could be created and influenced by physical circumstances, gravitation could not. Unlike the other agencies that formed the core of separate subdisciplines of physics, gravitation made its sole appearance in physical textbooks in

the customary chapter on the general properties of matter.[21] This isolated position is reflected in the speculations flourishing in the first half of the nineteenth century, concerning the correlation of physical agencies. Newly discovered interactions between mechanical, thermal, optical, electrical and magnetic phenomena strengthened the belief in the fundamental unity of nature, already part and parcel of the dynamical worldview, thereby contributing to the establishment of the principle of conservation of energy in the forties. Though a close connection between these agencies was often adhered to, gravitation usually remained out of the schema.[22] Nevertheless it is clear that an ultimate unification of physics would have to encompass gravitation. The multifarious imponderable fluids hardly lent themselves to the purpose of unification.

However, the theoretical and experimental work of Fresnel, Oersted, Ampère, Melloni and others served to diminish the number of imponderable substances, notably those connected with light, heat and magnetism.[23] In return a new fluid appeared, but, as many physicists recognized, with great unifying potentialities. It was held responsible for the propagation of light and (later on) heat and suspected by some to be connected with the phenomena of electrodynamics.[24] What could be more natural than that sooner or later gravitation would be associated with this universal medium. Still it seems premature to rest one's case, because there was an additional, only indirectly related factor of comparable importance. During the second half of the nineteenth century the former scruples concerning action at a distance cropped up again and gravitation was of course the archetype of action at a distance. Though the aether provided the obvious means to eliminate distance-forces, its establishment did not necessarily entail the extinction of distance-forces. In fact the French aether-theorists fitted their aether-models into the astronomical view of nature. They composed their aethers of (unextended) particles, interacting through short-range distance forces.[25] Likewise continental theories of electrodynamics were almost invariably formulated in action-at-a-distance terms.[26] Then what caused the renewed distaste for distance-actions?

Important factors were of course both the seminal work and the ideas of Michael Faraday. He was the first notable physicist to publicly oppose the notion of action at a distance in its generality. Paradoxically, Faraday was not an adherent of the mechanical philosophy of the remote past, but rather an exponent of the British dynamical tradition. His empirical research had led him to the notion that electromagnetic effects were propagated along curved lines. Though Faraday often stressed the purely descriptive character of his lines of force, there can be no doubt about his faith in the physical reality and primacy of his force-lines.[27]

Matter he likewise conceived as being composed of forces filling all space.[28] To Farady such a force-plenum meant an extinction of action-at-a-distance.[29] In his later years he argued that the indestructibility of force, a principle he firmly believed in, was incompatible with the received notion of gravitation, based on the distance-action of two particles, an action that could be arbitrarily diminished by enlarging the distance between the particles, leading to a loss of both speed and force.[30] His conclusion was that the above-mentioned principle implied the impropriety of action-at-a-distance.

However, Faraday's ideosyncratical ideas were out of tune with the prevalent conceptions in the middle of the century, based on the idea of central forces acting at a distance and dressed up in mathematical language. The French approach had spread all over Europe.[31] In Germany, *Naturphilosophie* and with it, dynamism in its extreme form, had been chased from the stage though remnants can be recognized in the popularity of point-atomism both in Germany and France.[32] Two related developments nevertheless threatened the astronomical view of nature and served to discredit the notion of distance-forces. In the first place the imponderable fluids were gradually replaced by forms of motion. These motions concerned either the aether, or systems of particles of ordinary matter. In some extreme cases even the latter were associated with aetherial motions of a permanent character. In this process the role of distance-forces was more and more reduced. This resulted in a movement aiming at a mechanical reduction of physics, a programme cultivated most vigorously in Britain.[33] An important impulse in this direction was furnished by the emergence of the kinetical theory of gases, given prominence by Maxwell and Clausius in the late fifties and the sixties. For many a scientist their gas model echoed distant themes connected with a mechanical view of nature, or mechanical philosophy, that preferred collisions to interparticulate forces as explanatory means. Far more extreme were later models, like Kelvin's vortex atom and the vortex-sponge aether, explicity eschewing any distance forces.[34] A culmination of this programme may be seen in the publication of Hertz' "Mechanik" at the end of the century, a work that eliminated forces in favour of rigid connections and hidden motions.[35]

A development that supported this movement and partly preceded it, was the rise of theoretical work done in the field of continuum-mechanics, that is to say hydrodynamics and elastic-solid-theory, in part a consequence of the triumph of the wave theory of light that directed attention to the physical properties of the optical medium. In these fields of theoretical research the astronomical approach was not generally acclaimed. Some theoreticians, especially in Britain, preferred to treat

fluids and elastic solids as continuous media, restricting themselves to general mechanical principles.[36] The progress in these subjects led to the use of hydrodynamical and elastic analogies in branches of physics other than optics, enabling Kelvin and Maxwell to make sense of Faraday's ideas, with a future promise of mechanical reduction of electricity and magnetism. Such analogies were also searched for in the case of gravitation, a development that commenced in the fifties and sixties, usually in the context of a theoretical synthesis.[37] Thus the suggestion is not that physicists worked out hydrodynamical or other continuum theories of electricity, magnetism and gravitation as a result of a new awareness of the absurdity of action at a distance, but rather that the new rejection of action at a distance that accompanied most gravitational theories, not only British but also continental, was at least partly a result of new technical and physical means of producing a "konstruierbare Vorstellung" of distance-forces, with the additional promise of unification.

That most earliest opponents of distance-forces realized they were stemming the current may be seen from their repeated appeal to authorities, the prime authority being of course Isaac Newton. The phrase in the third letter to Bentley expressing the absurdity of bodies acting upon another at a distance without the mediation of anything else must have been Newton's most frequently quoted statement in the second half of the nineteenth century.[38] The following sentence noting the possibility of an immaterial agent was usually dropped. In 1885, however, the Scottish physicist Tait self-confidently dismissed distance-forces as a 'most pernicious heresy,' knowing the majority of British physicists to be on his side.[39]

This rising consensus, for the moment restricted to the Britons, did not extend to the probable mechanism involved in the gravitational attraction. The most promising hydrodynamical theories were based on rather exotic models of the interacting particles, being either pulsating spheres in an incompressible fluid or sinks or sources (or both) of such a fluid.[40] Neither of these theories gained real prominence. More popular was the theory the Swiss scientist Le Sage had proposed and defended in vain throughout the second half of the eighteenth century.[41] It was revived in the second half of the nineteenth century in the wake of the kinetic theory of gases to which it bore some resemblance. The theory was based on the mutual screening of material particles in a permanent shower of aetherial particles, moving rectilinearly in all directions, thus behaving more or less like Maxwell's gas particles. It is revealing that Le Sage's theory that had been ridiculed from the last decennia of the eighteenth century up to 1865, when John Herschel considered it 'too grotesque to need serious consideration,' was ten years later called 'brilli-

ant' and 'ingenious' by a sceptical British critic.[42] It attracted less attention on the continent, although it was passionately defended by some continental theorists.[43] The lack of veritable success of all these theories is not hard to understand. Their empirical content hardly surpassed Newton's law and they failed to account for other physical phenomena. Besides, as Maxwell remarked, they all involved a constant expenditure of work.[44] Such criticism did neither discourage the work on the above mentioned theories, continuing into the next century, nor did it shatter the almost generally shared belief in Britain in the existence of an underlying mechanism.

The British attitude was not shared by the majority of continental physicists. They were less inclined to mechanical reduction and stuck to the astronomical view of nature if any model was needed. Moreover, positivism held a firmer grip on the continent. This last point needs some elaboration.

Nineteenth century positivism showed many faces. We already pointed out the unwillingness to explain the nature of distance forces. A stronger version abandoned the astronomical model, based on a disputable molecular hypothesis, in favour of mathematical equations containing solely measurable quantities. This attitude became more and more popular in France after the example of the Frenchman Fourier and possibly under the influence of the philosopher Comte.[45] British alternatives to this radical solution were the use of the Lagrangian and Hamiltonian methods, based on generalized mechanical principles, and the framing of mechanical models, that in spite of denials of ontological commitments, pointed towards the possibility of mechanical reduction, positivism bridging the astronomical and the kinetic view of nature.[46] In German-speaking Central Europe Ernst Mach became the most important spokesman of a movement that stressed the descriptive character of science, theories being a matter of economy rather than having explanatory functions.[47] Mach emphatically rejected mechanical reduction as being based on a historical bias, that mistakenly esteems mechanics more fundamental than other branches of physics.[48]

Though the last decennium of the century witnessed a wide dissemination of a Machian-like attitude, termed descriptionism by the historian Heilbron,[49] one also noticed that the British predilection for mechanical reduction had spread to the continent.[50] Though these seemingly antagonistic approaches could in some cases be reconciled, as is proved by the work of Pearson and Hertz who both advocated mechanical reduction as a means of economy, in general the latter cannot be seen as a result of the former.[51] The eventual defeat of the astronomical view of nature was mainly the result of Hertz' epoch-making experiments in the late

eighties.[52] The vindication of the British continuum-approach towards electromagnetism, encompassing the unification of optics and electromagnetism, had immediate consequences for the continental attitude towards distance-forces. In no time, gravitation had changed its status from a theoretical model into that of a theoretical problem,[53] although not exclusively theoretical. For gradually, experimentalists started to turn their attention to the force, wondering whether it really could not be influenced by any physical means.[54] It could not. In spite of the growing urge for a solution of the problem of gravitation, preferably in terms describing the general behaviour of the electro-magnetic aether, the force refused to be encompassed in a more general theory in a natural way. Proposals abounded but failed to gain approval. The most likely candidate, Lorentz' revival in a field-theoretical context of the older assumption of a slight difference in magnitude between attractive and repulsive forces acting between the electrical particles that were supposed to constitute matter, was, because of its ad hoc-character, mainly important in that it proved the admissibility of a velocity of propagation of gravitation equal to the velocity of light, as Lorentz himself acknowledged.[55] In fact gravitation came to pose one of the greatest obstacles to the electromagnetic view of nature that partly replaced the attempts of mechanical reduction at the turn of the century.[56]

However, when Newton's theory was seen to conflict with the requirement of Lorentz-invariance, or with the demands of special relativity, physicists gave up their ambitions of a unified physics and started to develop independent field theories of gravitation.[57] As is well known the most ambitious of these theories, Einstein's general relativity, proved the most successful. Not only did it provide a quantitative solution to the Mercury problem which by now had become a touchstone for any gravitational theory, even more important was the confirmation of the sun's ability to bend lightrays from distant stars through the predicted angle.

Summarizing we may conclude that new theoretical models and associated views of nature, rising out of different developments within physics and continuum mechanics, undermined and partly replaced the astronomical model, based on interparticulate distance-forces, thereby throwing doubt upon Newton's theory. The eventual success of the continuum-approach caused a rapid proliferation of these doubts. Moreover, the unification of radiant heat, light, electricity and magnetism asked for an explanation of gravitation in terms of a generalized theory of the electromagnetic medium. Unable to provide such a theory, physicists were nevertheless forced to confront the problem of gravitation by the conflict between Newton's theory and new physical principles. Experimental evidence came to play a crucial role, but only in the final stage.

An interesting parallel can be seen in the current attempts to unify Einstein's theory of gravitation with theories concerning the strong, weak and electromagnetic forces by means of an assumed rock bottom of knowledge, the latest fashion being the superstring model. Again, these attempts are not in the least motivated by empirical anomalies, but rather by a strong faith in the fundamental unity of nature, a faith that found its most spectacular justification in Newton's work and therefore may be considered an important part of Newton's scientific and philosophical legacy.

University of Utrecht, The Netherlands

NOTES

1. For a survey of these theories see: Woodward, J.F., "The Search for a Mechanism: Action at a distance in gravitational Theory," unpublished doctoral dissertation, Denver, 1972, ch. 4,5, and 6; North, J.D., "The measure of the Universe," Oxford, 1965, ch. 3; Roseveare, N.T., "Mercury's Perihelion: from Le Verrier to Einstein," Oxford, 1982, ch. 5.
2. Heilbron, J.L., "Elements of early modern physics," Berkeley, 1982, pp. 47-53; Hankins, T.L., "Science and the Enlightenment," Cambridge, 1985, pp. 37-41; the Dictionnaire de Physique (1793) lists fifteen different phenomena proving the existence of universal gravitation, Dictionnaire de Physique, Monge, G. et al., eds. vol. 1, Paris, 1793, pp. 327-328.
3. Dictionnaire de Physique (note 2), p. 326.
4. See notes 10 and 11.
5. Fox, R., "The rise and fall of Laplacian physics," Historical Studies in the Physical Sciences 4, 1974, pp. 91-92.
6. Laplace, P.S. de, "Exposition du système du monde," vol. 2, Paris, 1796, p. 198.
7. Merz, J.T., "A history of European thought in the nineteenth century," vol. 1, New York, 1965, ch. 4, especially pp. 346-348.
8. Fox (note 5), pp. 109-127.
9. Caneva, K.L., "Conceptual and generational change in German physics: the case of electricity, 1800-1846," unpublished doctoral dissertation, Princeton University, 1974, pp. 23-28.
10. Kant, I., "Metaphysische Anganfsgründe der Naturwissenschaft," Riga, 1786, esp. ch. 2; Mayer, J.T., "Anfangsgründe der Naturlehre," Göttingen, 1801, pp. 101-2; Kastner, C.W.G., "Grundriss der Experimentalphysik," Heidelberg, 1820, 2nd ed., vol. 1, pp. 56-57; Kries, F.C., "Lehrbuch der Physik," Jena, 1816, 2nd ed., pp. 8-9; Muncke, G.W., "Handbuch der Naturlehre," Heidelberg, 1829, vol. 1, pp. 27-34.

11. Heimann, P.M. & McGuire, J.E., "Newtonian forces and Lockean powers: concepts of matter in eighteenth-century thought," Historical Studies in the Physical Sciences 3, 1971, pp. 233-306.
12. Heilbron (note 2), pp. 64-65; Gehler, J.S.T., "Physikalisches Wörterbuch," Leipzig, 1798, vol. 1, pp. 169-170; Brisson, M.-J., "Traité élémentaire ou principes de physique," Paris, 1789, vol 1, p. 166; Vince, S., "Observations on the hypotheses which have been assumed to account for the cause of gravitation from mechanical principles, " Cambridge, 1806. Notable exceptions are the Swiss scientists Deluc, Prevost and L'Huilier, each of whom gave support to the mechanical theory of their compatriot Le Sage; Deluc, J.A., "Idées sur la météorologie," Paris, 1786, vol. 1, pp. 132-133; Prevost, P., "Notice de la vie et des écrits de George-Louis Le Sage de Genève," 1805, p. 563; L'Huilier, S.A.J., "Exposition élémentaire des principes des calculs supérieurs," Berlin, 1787, p. 187.
13. Heilbron (note 2), pp. 64-65; Du Bois-Reymond, E., "Ueber die Grenzen des Naturerkennens," 1872, in "Reden von Emil du Bois-Reymond," Leipzig, 1912, 2nd ed. vol. 1, p. 448.
14. See Le Sage, "1.er Traité. Physique Mecanique," in "Deux traités de physique mécanique," Prevost, P. (ed.). Genève and Paris, 1818, pp. 91-93.
15. Newton, I., "The mathematical principles of natural philosophy," reprint of Motte's translation of 1729, London, 1968, p. 392. Dictionnaire de Physique (note 2), p. 328.
16. Laplace, P.S. de, "Traité de mécanique céleste," vol. 4, Paris, 1805, pp. 325-326; ibid., vol. 5, Paris, 1825, pp. 403-407.
17. See Roseveare (note 1), pp. 1-4.
18. Ibid., pp. 4 and 20-32.
19. Ibid., p. 44 and 50-52.
20. The only notable experiments before the last quarter of the century were those performed by Bessel (pendulum-experiments with different materials) and Faraday (looking for a relation between electricity and gravitation); Bessel, F.W., "Versuche über die Kraft, mit welcher die Erde Körper von verschiedener Beschaffenheit anzieht," Astronomische Nachrichte 10, 1833, p. 97ff.; Faraday, M., "On the possible relation of gravity to electricity," Philosophical Transactions of the Royal Society of London, 1851, pp. 1-6.
21. Apart from an occasional chapter on astronomy, no rubric was devoted to it in any of the major subject classifications or abstracting journals. In the Fortschritte der Physik, for instance, gravitational theories were usually noticed under 'Mechanik.'
22. See for instance Grove, W.R., "On the correlation of physical forces," London, 1846.
23. Harman, P.M., "Energy, force, and matter. The conceptual development of nineteenth-century physics," Cambridge, 1982, pp. 19-42.
24. Ibid., p. 31 and 104.
25. See for instance Buchwald, J.Z., "The quantitative ether in the first half of the nineteenth century," in: Cantor, G.N. & Hodge, M.J.S., eds., "Conceptions of Ether," Cambridge, 1981, pp. 222-225.
26. Hesse, M.B. "Forces and Fields, The Concept of Action at a Distance in the History of Physics," London, Edinburgh, etc., 1961, pp. 216-222.

27. See Gooding, "Faraday, Thomson, and the concept of the magnetic field," British Journal for the History of Science 13, 1980, pp. 105-112.
28. Faraday, M., "A speculation touching Electric Conduction and the Nature of Matter," The London, Edingurgh, and Dublin Philosophical Magazine and Journal of Science 24, 1844, pp. 136-144.
29. Ibid., p. 144.
30. Faraday, M., "On the conservation of force," The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science 13, 1857, pp. 225-239.
31. This concerns especially the French analytical methods. See Crosland, M. & Smith, C., "The transmission of physics from France to Britain: 1800-1840," Historical studies in the Physical Sciences 9, pp. 1-61; Caneva, K.L., "From Galvanism to Electrodynamics: The Transformation of German Physics and Its Social Context," Historical Studies in the Physical Sciences 9, p. 123. The mathematics combined with already existing traditions of attractive and repulsive forces and (in Germany) with a revival of the particle as force-carrier. A classical expression of the astronomical view of nature with Kantian overtones is Helmholtz, H. "Ueber die Erhaltung der Kraft," Berlin, 1847.
32. In Germany notably Fechner, Redtenbacher, Wilhelmy and Robert Grassmann, see Snelders, H.A.M., "Point-atomism in nineteenth century Germany," Janus 58, 1971, pp. 194-200. In France adherents of this atomic conception were Ampère, Cauchy, Moigno, Seguin and De Saint-Venant, see Moigno, F., "Recherches de la cause qui maintient les molécules des corps à distance; etc." Cosmos. Revue encyclopédique hebdomadaire des progrès des sciences 2, 1853, pp. 371-382; Jammer, M., "Concepts of Force," Cambridge, MA, 1957, pp. 183-184.
33. See Rosenberger, F., "Geschichte der Physik," vol. 3, Braunschweig, 1887-1890, repr. Hildesheim, 1965, pp. 528-577; Merz (note 7), vol. 2, pp. 3-94, esp. pp. 34-35. Mechanical reduction is usually understood as representation in terms of the basic concepts of mechanics. These may include distance-forces in which case the astronomical view of nature is intrinsically mechanical. Here, however, it must be understood in a stronger sense, that is, as a reduction that encompasses distance-forces thus leading to what Merz calls a 'kinetic view of nature.'
34. See Whittaker, E., "A History of the Theories of Aether and Electricity," new ed., London and New York, 1951, pp. 294-303.
35. Hertz, H.R., "Die Prinzipien der Mechanik, in neuem Zusammenhange," Leipzig, 1894.
36. Harman (note 23), pp. 25-27.
37. For instance Riemann in Germany, Bjerknes in Scandinavia and Challis in Britain; See "The collected works of Bernhard Riemann," New York, 1953, pp. 528-538; Bjerknes, C.A., "Hydrodynamische Fernkräfte," Korn, A. & Bjerknes, V. eds., Leipzig, 1915, pp. 212-223; Challis, J., "A theory of the force of Gravity," London, Ediburgh and Dublin Philosophical Magazine and Journal of Science 18, 1859, pp. 442-451.
38. For instance Riemann (note 37), p. 534 and 553, Faraday (note 30), p. 232, Keller, F.-A.-E. & Em., "Mémoire sur la cause de la pesanteur et des effets attribués à l'attraction universelle," Comptes Rendus Hebdomadaires des Scéances de l'Académie des Sciences 56, 1863, p. 530; Thomson, W., "On the Ultramundane Corpuscules of Le Sage," London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science 45, 1873, p. 321; Isenkrahe, C., "Das Räthsel von der Schwerkraft," Braunschweig,

1879, p. 125, etc.

39. Tait, P.G., "Properties of Matter," Edinburgh, 1885, p. 6.
40. See North (note 1), pp. 32-38; Roseveare (note 1), pp. 101-108.
41. Aronson, S., "The Gravitational Theory of Georges-Louis Le Sage," The Natural Philosopher 3, 1964, pp. 51-74.
42. Herschel, J., "On the Origin of Force," Fortnightly Review 1, 1865, p. 438; Maxwell, J.C., draft of "Atom" Article for the Encyclopaedia Britannica and published version of "Atom" respectively, both in Garber, E., Brush, S.G., and Everitt, C.W.F. eds., "Maxwell on Molecules and Gases," Cambridge, MA and London, 1986, p. 173 and 205 resp.
43. See for instance Leray, P. "Constitution de la Matière," Paris, 1869; Isenkrahe (note 38).
44. Maxwell, J.C., "Attraction" in the Encyclopaedia Britannica, republ. in "The Scientific Papers of James Clerk Maxwell," Niven, W.D. ed., 1965 (1890), New York, vol. 2, p. 490.
45. Herivel, J.W., "Aspects of French theoretial physics in the nineteenth century," The British Journal for the History of Science 3, 1966-1967, pp. 123-125.
46. Kargon, R, "Model and analogy in Victorian science: Maxwell and the French physicists," Journal of the History of Ideas 30, 1969, pp. 423-436; Harman, P.M. (note 23), pp. 69-71. The term 'kinetic view of nature' again derives from Merz.
47. Heilbron, J.L., "Fin-de-siècle physics," in "Science, Technology and Society in the Time of Alfred Nobel," Bernhard, C.G., Crawford, E. and Sörbom, P., eds., Oxford, New York, etc., 1982, p. 53.
48. Mach, E., "Die Mechanik in ihrer Entwicklung, historisch-kritisch dargestellt," Leipzig, 1883, p. 472.
49. Heilbron (note 47), p. 51.
50. See, for example, McCormmach, R., "H.A. Lorentz and the Electromagnetic View of Nature," Isis 61, 1970, p. 461.
51. Though both Pearson and Hertz can be seen as Mach's allies in his battle against anthropomorphic conceptions in science such as 'forces,' Hertz and Pearson tried to eliminate physical forces by means of hypothetical mechanisms, a solution that Mach certainly would not have applauded. See Hertz (note 35), pp. 30-31; Pearson, K., "On a certain Atomic Hypothesis," Transactions of the Cambridge Philosophical Society 14, 1889, p. 71 and his "The Grammar of Science," London, 1892.
52. See note 50.
53. Many scientists noticing the dramatical change in electrodynamical conceptions mentioned the problem or riddle of gravitation. Thus Hertz, H.R., "Ueber die Beziehungen zwischen Licht und Elektricität," Tageblatt der 62. Versammlung Deutscher Naturforscher und Aerzte, 1889, p. 149; Sohncke, L, "Die Umwälzung unserer Anschauungen vom Wesen der elektrischen Wirkungen," Himmel und Erde 3, 1891, p. 171; Witz, A., "De la méthode et des théories de la physique moderne," Revue des questions scientifiques 30, 1891, pp. 57-59; Boltzmann, L., "Vorlesungen über Maxwell Theorie der Elektricität und des Lichtes," vol. 1, Leipzig, 1891, p. 3; Heaviside, O., "On the Forces, Stresses, and Fluxes of Energy in the Electromagnetic Field," (1891), Philosophical transactions of the Royal Society of London A 183, 1893, pp. 426-427; Lord Kelvin (Thomson, W.) preface to the English edition of Hertz, H.R., "Electric

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57. See Girard, P.R., "The conceptual development of Einstein's general theory of relativity," unpublished doctoral-dissertation, University of Wisconsin-Madison, 1981, pp. 85-109; Roseveare (note 1), pp. 148-152.

W.D. HACKMANN

ELECTRICITY IN EIGHTEENTH-CENTURY HOLLAND: A NEWTONIAN LEGACY

A follower of Francis Bacon, Robert Boyle in his empirical approach was largely responsible for the transformation of Dutch Cartesian rationalism into the experimental philosophy of the late seventeenth century. The next transformation was into Newtonian physics. Isaac Newton's *Principia* (1687) and the *Opticks* (1704) were studied in the Dutch Republic as soon as they became available by such influential natural philosophers as Christiaan Huygens, Buchardus de Volder and Bernard Nieuwentyt, but the first comprehensive textbook based on the Newtonian methodology was Willem Jacob 's Gravesande's *Physices elementa mathematica, experimentis confirmata. Sive Introductio ad Philosohiam Newtoniam* (1720-1). He wrote that he was proud to follow in the footsteps of his master. This is not the place to delve into the complexities of the philosophical and scientific background of Newton's two seminal works, apart from the broad generalization that they united two approaches to the study of natural mathematical analysis (abstraction) and experimentation.[1]

's Gravesande and his successor Petrus van Musschenbroek emphasized different aspects of Newton's methodology. 's Gravesande was more strictly phenomenalistic and tried to adhere to Newton's stricture of "hypotheses non fingo," while by the 1730s Van Musschenbroek was far less cautious and more willing to speculate on the mechanical causes underlying natural phenomena in terms of, for instance, such essentially untestable entities as atoms or sharp acid particles. Van Musschenbroek's approach, very much in the spirit of the *Opticks,* came to characterize the study of electricity in Holland (and elsewhere) during the second half of the eighteenth century.[2] All the major Dutch natural philosophers of this period were in this sense 'Newtonian,' a rather vague historical term which has become all but meaningless. Within this framework their electrical experiments and theories were primarily influenced by the ideas of Benjamin Franklin and Joseph Priestley. The hallmark of their research was a strong experimentalism with its concomitant strengths and weaknesses.

The earliest example of an electrical research programme following the model of the *Opticks* was Francis Hauksbee, the elder's *Physico-Mechanical Experiments on Various Subjects, containing an Account of Several Surprising Phenomena Touching Light and Electricity* (1709). As

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we have come to realize, Newton's ideas on the nature of electricity were largely shaped by Hauksbee's experiments.[3] 's Gravesande closely followed Hauksbee in his *Physices elementa,* which not only contains the earliest known illustration of electroluminiscence produced by Hauksbee's 'chafing machine,' but also a succinct summary of the latter's experiments in a short chapter entitled "The fire adherers to Bodies, and is contain'd in them: where we shall also speak of Electricity." He agrees with Hauksbee by suggesting that electrical attraction and repulsion were caused by the 'vibratory motion' of air and a very active and subtle matter, possibly fire.[4] Fire became commonly linked with electricity and this was to be of particular significance, as we shall see, in the 'electrochemical' theories of the nature of electricity in the latter quarter of the eighteenth century. 's Gravesande's substantial version of Hauksbee's 'chafing machine,' the progenitor of the frictional electrostatic generator, was widely disseminated by this popular textbook.

When Petrus van Musschenbroek became interested in electricity, he repeated the entire corpus of electrical experiments in about three weeks, after which he planned to make discoveries – a supreme optimism rewarded by the discovery of the Leyden jar in February 1746.[5] His earliest textbooks, such as the *Elementa physicae* (1734), has very little on electricity; the Dutch version, the *Beginselen der Natuurkunde* (1736), contains more, including the experiments of Gray and Du Fay, while his account of the discovery of the Leyden jar is added to the enlarged version of his textbook, the *Institutiones physicae* (1748). An exhaustive survey of electrical experiments is contained in his *Introductio ad philosophiam naturalem,* completed by J. Lulofs and published posthumously in 1762.

Apart from the enormous impact of the Leyden jar, Van Musschenbroek contributed little to electrical theory, but his eclectic textbooks disseminated what was known, popularized the subject, and became a model of how to conduct experiments for several generations of natural philosophers. Of great importance – and didactic value – was the development by 's Gravesande and Van Musschenbroek of novel experimental apparatus for laboratory work, and for lecture-demonstrations. By the 1740s, the frictional electrical machine had ousted the vacuum pump as the most popular instrument. Twenty years later England led in the design of the electrostatic generator, partly because of the innovations wrought by the large London-based instrument-making trade, and the subject was dominated by 'English' electrical theory based on Benjamin Franklin's single-fluid concept of electrical action, and popularized by Joseph Priestley's *The History and Present State of Electricity* (1767).

When the London instrument maker and natural philosopher John Cuthbertson (1743-1821) settled in Amsterdam in 1768, he became an obvious focus for Dutch interest in electricity.[6] There are numerous references to him in the contemporary Dutch electrical literature. He introduced the English plate (or disc) machine, and developed it into its most sophisticated form. In March 1783 he was charged by Martinus van Marum (1750-1837), the Director of the 'Cabinet of Physical Curiosities' and Library of the Teyler's Foundation (Teylers Tweede Genootschap) in Haarlem, to construct the largest electrostatic generator then technically feasible. This device took a year to complete and began their fruitful partnership which lasted until 1789, when they parted as a result of a controversy over a design detail. The impetus behind this instrument – the eighteenth-century equivalent of the modern 'atom smashers' – was the need expressed in Priestley's *History* for ever more powerful laboratory devices which were capable, like larger microscopes and telescopes, of 'magnifying' natural phenomena. Van Marum's aspirations were fully realized with this machine (Fig. 1).

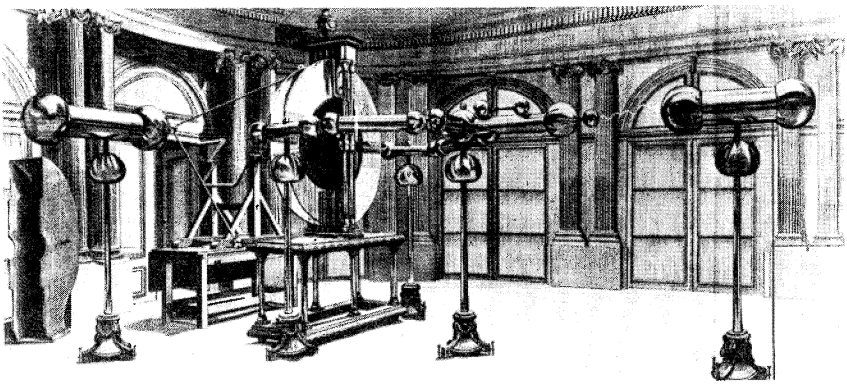

Fig. 1: Teyler's huge electrical machine when completed in 1784. From Van Marum, *Verh. Teyler's Tweede Gen.*, vol. 4 (1787), Pl. I.

It produced 24 inch (almost 70 cm) discharges, equivalent to about 300,000 volts, and with its large Leyden battery had an energy output of about 600 joules, which made it by far the most powerful electrostatic generator constructed in the eighteenth century, not to be surpassed for a century. Teyler's huge generator was greatly admired by the European natural philosophers. It helped to place Holland on the scientific map as

Van Marum was often asked to use his machine as a kind of instrumental Newtonian 'experimentum crucis' to arbitrate between contending theories.[7]

Cuthbertson was quickly assimilated into the Dutch scientific community. The first volume of his *Algemeene eigenschappen van de electriciteit* appeared in the early 1770s, and the third and final volume in 1793. Shortly afterwards he returned permanently to London probably because of the unsettled political situation in Holland. His motives for writing this book were "to diffuse more generally the science of electricity in Holland, the knowledge of which was at so low an ebb that most experiments were known but to a few individuals, as well as to introduce the plate electrical machines, which were at that time made in London..."[8] Judging by the contemporary scientific literature, his pessimism about the state of electrical science in Holland was probably exaggerated perhaps for commercial reasons, but Cuthbertson did create a ready market for his improved electrical instruments, and he became a key member in a small network of those intensively involved with electrical experiments, centred on Amsterdam and Haarlem. Besides Van Marum, the other prominent members were Jean Henri van Swinden (1746-1823), professor of philosophy, mathematics, natural philosophy and astronomy at the Athenaeum Illustre in Amsterdam, Cornelis Rudolphus Theodorus (later Baron) van Krayenhoff (1758-1840), a distinguished military officer, and the two best known members of the Dutch Chemists (an unofficial group specializing in the chemistry of gases), the physician Jan Rudolph Deiman (1743-1808), and the wealthy merchant Adriaan Paets van Troostwijk (1752-1837).

Van Swinden became best known for his extraordinarily accurate meteorological observations, but he was also interested in all aspects of magnetism and electricity. In 1785 he experimented with Van Marum on the effects of electrostatic discharges on permanent magnets. The relationship between these two mysterious forces had for long been of great interest. He also translated into French a book on medical electricity, the *De l'application de l'électricité à la physique et à la médécine* (1788), by Van Krayenhoff and Van Troostwijk. Van Krayenhoff in turn, translated into Dutch a popular French work under the title *Proef eener Elektrische Natuurkunde in 't Fransch van den Abt Jacquet* (1783), embellished in several appendices with his own observations, including those on the lightning conductor. He also experimented with Van Swinden on the "Lichtenberg figures," on which they published a joint paper in 1787. They demonstrated the electrical nature of these pretty patterns produced by sprinkling a light powder (such as red lead and flour of sulphur) onto an electrified metal or resinous plate, which they described in Franklinian

terms. Cuthbertson assisted Deiman and Van Troostwijk with their 'Amsterdam experiment' of 1789 when, for the first time, the electrolysis and synthesis of water in the same glass tube was achieved by means of a series of electric discharges. This classical experiment at the boundary of electricity and chemistry, was important for the early acceptance of Lavoisier's antiphlogiston (combustion) theory into the Netherlands.[9] Van Troostwijk was the ablest experimenter of the Dutch Chemists, and the one who worked most closely with Van Marum. Their first scientific paper, on the electrophorus, appeared in 1783, and was awarded the gold medal by the Batavian Society, but their most important work, performed a few years later, was in the field of electrochemistry (the behaviour of gases and metals subjected to electric discharges).

The intellectual framework of this closely-knit scientific network was 'Newtonian' experimentalism as it had evolved by the second half of the eighteenth century, while the cornerstone of their electrical research was Franklin's single-fluid theory. During the 1740s, fluid theories of electrical phenomena were replacing the earlier effluvia (or emanations) theories. This process crystallized around the ideas put forward by Franklin at the end of this decade. One of the notable early successes of his theory was the apparent ease with which it could explain the action of the Leyden jar, while the popular imagination was caught by Franklin's experimental 'proof' of the electrical nature of lightning and the subsequent development of the lightning conductor.

Franklin's theory suffered from several severe drawbacks (or inconsistencies) such as the problem of the repulsion of negatively charged bodies, and even more basically, how macroscopic attraction or repulsion followed from his propositions. Several solutions were proposed in the 1750s, notably by Ulrich Theodisius Aepinus (the interaction of the attractive and repulsive forces between particles of matter and of the electric fluid, and the repulsion between particles of matter), and by Giambatista Beccaria (mutually interacting electrical atmospheres), but these could not reconcile the difficulties. Indeed, in the 1770s, Beccaria the foremost Franklinian, followed Aepinus in denying the existence of these atmospheres around electrified bodies. Another solution was to postulate two electric fluids: one for vitreous (positive) and the other for resinous (negative) electricity. Each one behaved as Franklin's single electric fluid. The two-fluid theory became popular at the end of the eighteenth century, favoured by electrochemists and by the emerging breed of mathematical physicists, notably Coulomb.[10]

Van Marum's explanation of the action of the electrical machine was in essence the same as Franklin's of twenty years before: electric matter was 'pumped' up from the earth by the friction of the cushions against the

revolving glass electric, and his improvements in generator design were in accordance with this principle. In their 1783 prize essay on the electrophorus, he and Van Troostwijk accepted the modification by Aepinus of Franklin's theory, but later Van Marum followed Van Swinden in rejecting this because of its incompatibility with Newton's law of gravitation. Instead he now accepted Beccaria's concept of 'contrary electrical atmospheres,' and he never relinquished it even after this concept had been generally abandoned. Late eighteenth-century Dutch electrical theory suffered, as in other countries, from such basic inconsistencies which could not be resolved, steeped as this subject was in the Newtonian tradition, and the inability of the Dutch experimentalists to think in terms of unexplained electrical forces acting at a distance on the instrumental *macroscopic* level. As Roderick Home has pointed out, the transition from the older effluvial theories of electricity to the classical action-at-a-distance fluid theory constitutes one of the most important (late) eighteenth-century 'revolutions.'[11] These two conceptual frameworks or attitudes of mind – electrical atmospheres and action-at-a-distance – can be traced back to two traditions: the mechanists who required a physical medium for the transmission of force, and the empiricists who did not.

The Dutch natural philosophers were predominantly mechanists. This attitude influenced their approach to the study of electricity, which was qualitative and descriptive rather than quantitative and abstract. It would not seem unfair to characterize Deiman, Van Marum and Van Troostwijk as in the main chemists with an interest in electricity. One of their prime concerns was to discover the *nature* of electricity (similarly to the way they would investigate a chemical substance); a concern which fitted in well with their use of the electric discharge as a tool of analysis in their chemical research. Electricity was first identified with phlogiston, but later on with Lavoisier's caloric (warmtestof). However, here too there occurred an unresolved inconsistency, for in experiments performed with Cuthbertson and Van Swinden, little correlation was found to exist between length of wire fused by the electric discharge and melting point, which seemed to indicate that fire and electricity did not have identical natures.

The hallmark of Dutch electrical research was its focus on qualitative, descriptive laws at the *macroscopic* experiential level. The experimenters remained very close to their instrumentally-induced phenomena, placing great value on the 'visual' aspects of these processes. A case in point was Van Marum's 'proof' of the existence of a single electric fluid by the appearance of the large spark produced by Teyler's huge generator, in which the side branches were all pointing in the same direction, from the positive to the earthed negative conductor. An engraving of this

spark he submitted for approval to Franklin in 1785 (Fig. 2).

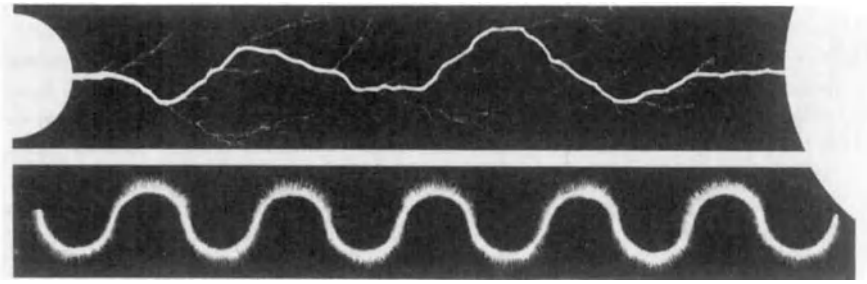

Fig. 2: Engraving of the 24 inch (70 cm) discharge with its side branches produced by Teyler's huge generator which was Van Marum's visual 'proof' for the existence of Franklin's single electric fluid. The lower engraving depicts the lateral rays of a discharge passed through a thin wire. From Van Marum, *Verh. Teyler's Tweede Gen.*, vol. 3 (1785), Pl. III.

Two basic types of electrical experiments were performed: laboratory procedures isolating certain electrical properties, very much in the manner of the *Opticks,* and model experiments based on the analogous argument, in which phenomena were recreated in the laboratory by means of scale models. These were generally of phenomena that could not be isolated in a laboratory, such as Franklin's, Cuthbertson's, or Van Troostwijk's experiments on the effects of lightning, or Van Marum's analogous experiments on atmospheric electricity or on the structure of plant vessels.[12] These models were generally regarded as real rather than as useful fictions. Their main weakness was that they remained too close to their experiments, and were unable to formulate mathematical idealizations of the observed phenomena at the deeper philosophical *microscopic* level. The data-gathering activities of these Dutch natural philosophers were indispensable for progress in the science of electricity, and eighteenth-century electrical studies in the Netherlands provided a fruitful Newtonian legacy.

Museum of the History of Science, University of Oxford, England

NOTES

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GUIDO J.M. VERSTRAETEN

RECONCILIATION OF THE NEWTONIAN FRAMEWORK WITH THERMODYNAMICS BY THE REPRODUCIBILITY OF A COLLECTIVE PHYSICAL QUANTITY

1. *The Hollinger-Zenzen's Framework*

Attempts to reduce irreversible processes to the scope of Newton's mechanics are particularly challenging topics for both physical and philosophical research. Hollinger and Zenzen,[1] for instance, claim that macroscopic irreversibility has a mechanical origin, and they explain this within the Newtonian framework.

It will first be explained what these authors understand by irreversibility. Then an examination of their ideas about entropic irreversibility will be made, and finally their interpretation of macroscopic irreversibility within the Newtonian framework will be scrutinized.

The first part of their work consists of an analysis of the distinction between *inertial, temporal* and *exclusion* irreversibility.

– A process is *inertially reversible* if it occurs so near to equilibrium that it can be reversed quickly by changing the external influences.

Temporal irreversibility stands in contrast to the temporal reversibility as found in the Newtonian equation of motion (and extended in general dynamics). Both authors emphasize the intimate connection between Newtonian time and motion (reversing one is reversing the other) and the double, i.e. both microscopic and macroscopic, level of time reversibility. Moreover, the transport equations, when formulated with macroscopic transport coefficients, become time reversible provided the appropriate transport coefficients are explicitly derived from particle equations. The same applies to the kinetic equation for gases or for Boltzmann's equation (see Penrose)[2] and its corollary: the H-theorem (see Penrose).[3]

The molecular chaos assumption and Bogolyubov's hypothesis modify the collision term in the Boltzmann equation and involve a time reversible equation (see Hollinger-Curtiss[4] and Hollinger).[5] Both assumptions are used to express the N-particle distribution function in terms of one-particle distribution functions with *appropriate initial boundary* conditions situated in the past.

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Finally, Hollinger-Zenzen situate *exclusion* irreversibily on the macroscopic level only. This form of irreversibility is best known in terms of the behaviour of entropy (according to its non-decreasing property expressed by the second law of thermodynamics). A more detailed survey reveals that there are processes of which the reverse is excluded by *natural means* and that they are *a priori irreversible* in the sense that their reverse is conceptually impossible.

B. *Entropic irreversibility* is not a priori irreversible nor can it generally be explained by the contingency of initial and boundary conditions *(de facto* irreversible). Hollinger and Zenzen place the entropic exclusion principle in the underlying particle dynamics which is ruled by laws characterized by temporal reversibility.

In order to substantiate their claim, they try to establish why the statistical description is so successful. The entropy of the observed processes is compared with *statistical entropy*. Because the increase of predictibility of the macrostate is an average of initially accessible microstates, they consider *increasing entropy* as a sign which ensures reproducibility.

They identify a macrostate with a set of microstates; if time evolution holds the set together, then the evolution of the macrostate can be expected to be reproducible. Since the drifting apart of the initially accessible microstates implies that they become accessible to a growing variety of macrostates, they conclude that the macroscopic evolution would not be reproducible.

In what follows it will be argued that the claimed connection between increasing/decreasing entropy and the reproducibility/non-reproducibility of the macro system is not generally tenable. In the first place, it can be objected that Hollinger and Zenzen compare a macro system and a set of microstates on the same epistemological level. Yet this assumption requires a careful investigation of the theorem of Birkhoff, which demands that the time evolution of a macrosystem can only be equated with an average of microstates. Secondly, reproducibility is a concept regarding the final hypothetical symmetric time evolution (which is not necessarily actual), this in contrast to the asymmetric time evolution of some finite physical systems obeying the second law of thermodynamics. Hence, the assertions of Hollinger-Zenzen regarding entropy should be regarded as a hypothetical statistical extrapolation of the entropy concept. It does not give us any further insight into the paradox of irreversible evolution of macro systems nor into the reversible equations of motions of the underlying particle dynamics.

C. Their interpretation starts from an equilibrium plateau where a collective property P has a determined value. A forced "initial" withdrawal at t_0 is "followed" by a relaxation of the mechanical system to an equilibrium plateau which starts at $t_1(t_0 < t_1)$.
After a certain time $t_2(t_0 < t_1 < t_2)$ there will appear spontaneous withdrawals from equilibrium, which are guaranteed by the periodicity of the mechanical model.

Hollinger-Zenzen allude to the well known discovery by Poincaré in 1890 that a Newtonian system of any number of particles, with or without interactions, is periodic if the system is confined by fixed external forces and isolated from changing external forces. It may be assumed that the latter condition is related to their definition of *"equilibrium state."*

If at time $t_3(t_1 < t_3 < t_2)$ an external influence (another external forced withdrawal) brings the equilibrium plateau "between" t_1 and t_2 to a new equilibrium plateau between $t_3 < t_2 < t_4$, the spontaneous processes will be excluded. If the external influences change with a frequency smaller than the frequency of recurrence of the Newtonian periodicity, then the system will appear to be mechanical. On the other hand, if the frequency of such changes in external influences exceeds the frequency of recurrence, then there will be spontaneous withdrawals from the old equilibria, and therefore these will be excluded.

Hollinger and Zenzen restrict statistical mechanics to young equilibria which ensure a continued randomness, and to early forced withdrawals from such equilibria. The creation of entropy is situated in the above mentioned young equilibria, and entropy irreversibility is completely explained in terms of the exclusion, resp. the admission, of certain processes. Entropy is considered as a metaphor, not as an essential, physical function.
Moreover, the role of the forced withdrawals is not clear. By locating Boltzmann's *Stosszahlansatz,* or the molecular chaos assumption, in the young equilibria area, Hollinger-Zenzen, while explaining the H-theorems, consider a merely statistical interpretation of entropy.

Finally, the time order is presupposed (e.a. the points of time t_o, t_1, t_3, t_2, t_4), which implies non-hypothetical, yet actual points of time,[6] which have to correspond to actual events. In the following sections more details will be given concerning the above mentioned restraints. At the end, the question will be raised whether it is possible to conceive of a "microscopic" interpretation of entropy in terms of the mechanical framework.

2. *The requirement of reproducibility*

As it was already mentioned, the mechanical equilibrium of the system examined is defined by the absence of changing forces. The system is characterized by a certain mechanical collective quantity P. Thus, the set of elementary particles may be considered as a physical system.[7]

Time duration is implied by the already mentioned equilibrium and the points of time t_0, t_1, t_2, or t_0, t_1, t_3 become actual (i.e. necessarily to be ordered) by identifying each of these points of time with corresponding values of P. If the irreversibility which appears after the withdrawals is to have a phenomenological signficance as is the case for a *thermodynamic* isolated system, the values of P must be *measurable* and *reproducible*.

If the external forces are represented by the constraint parameters $\lambda_1, \ldots, \lambda_n$ the mechanical system may be described as a thermodynamic system characterized by λ_1 1, $\ldots, \lambda_n$ and P which incorporates the role of θ in classical thermodynamic functions[8] (where θ is proportional to the absolute temperature). Thus a thermodynamic description will be allowed instead of a mechanical one. The thermodynamic entropy may be defined as a total differential of some other thermodynamic function.[9] This means that the entropy variation depends only on the final and initial state of the system.

The non-decreasing character of the thermodynamic entropy and its additivity properties follow from the reproducibility of P_1, resp. P_2 in both systems or both points of time,[10] t_1 and t_2.

Thus, and contrary to Hollinger-Zenzen, entropy is emphatically not "a sign of assurance of reproducibility of a macro system." It is the reproducibility of a quantity P, characterizing the system, which allows for the thermodynamical description and which involves the entropy function. The reproducibility of P guarantees the actuality of the events and hence also the order of time, as it was suggested by Grünbaum.[11] Thus one avoids the mere hypothetical approach of irreversibility of Hollinger-Zenzen as well as their statistical treatment which entails epistemological problems.

The following remark must be added. Both Boltzmann's *Stosszahlansatz* and the molecular chaos assumption are based on the reproducibility of initial conditions, which makes possible the thermodynamic description.

Finally, the Hollinger-Zenzen thesis fails to explicitly explain the role of the young withdrawals. In the interpretation which is defended in this paper, it is concluded that these withdrawals do not cause irreversible behaviour, because that would imply that irreversibility comes from

without the system, which would entail an infinite recurrence of the problem. In fact, the withdrawals form an additive constraint or they can be considered to be the result of the contact of the first system with another one. Thus the exclusion of these withdrawals is explained by a *collective* interaction of the "mother system" and the forced withdrawals involving the non-decrease of the entropy function of the mother system.[12]

But what about the periodic spontaneous withdrawals with higher frequencies than those of the forced withdrawals? This purely mechanical case results in a periodic movement where time is intimately related to an absolute mathematical parameter. The collective quantity P would not have a measurable, but a merely hypothetical value. To measure P a *duration of time* is needed, in which P is reproducible. This is not the case for periodic motion.

Indeed, one could take the time average of P during one mechanical period. If P is a random variable, summable over a finite volume in the phase space which represents all dynamical states of the system, then, according to Birkhoff's theorem, the average value of P exists,[13] if the volume V is metrically indecomposable ("invariant" under the natural mechanical motion), Then nearly everywhere within V the time average is equal to the phase average.[14] In that case the average has a statistical significance and there would be no evidence to connect a particular value of P to an *actual* point of time.

According to the last remark, Boltzmann's *Stosszahlansatz* starts from the *actual* current average variation of the numbers of particles in the collisions and not from the expected value.[15] Thus it may be concluded that a purely mechanical framework and a thermodynamical description are complementary. When collective properties of a system of particles become actually measurable, the thermodynamic description is valid.

This is germane to Prigogine's assertion[16] that irreversibility is inherent to microscopic nature and that it starts where the physical quantities of classical and quantum mechanics cease to be observable. Because measurable quantities do not need to be observable, it is preferable to use the term "measurable," which, according to Bunge,[17] is more general. In the next section, Prigogine's assertion will be investigated as it applies to mechanical systems with asymptotic Lyapounov stability.*

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* This stability is due to the existence of a positive function W around the mechanical state, which has a first derivative with respect to the time $\frac{dW}{dt}$ with the condition $W \cdot \frac{dW}{dt} \leqslant 0$.

3. *Complementarity of mechanics and thermodynamics*

In a series of articles, Lochak[18] deduces the relations between dynamics systems (which are stable in the sense of Lyapounov) and the second principle of thermodynamics. In addition, he examines the effect of limited forced withdrawals on such systems. Lochak's claims may be summarized as follows:
a) A perturbation which is limited at nearly any time does not disturb a movement which is asymptotically stable in the Lyapounov sense.
b) Periodic withdrawals of high frequency do not disturb the asymptotic stability, in Lyapounov's sense.
c) For a dynamic system with asymptotic stable equilibrium states in the sense of Lyapounov, a Lyapounov function W can be defined, which is non-decreasing and additive, as is the entropy function itself.

The first and second assertion specify the form of the forced withdrawals. The third assertion allows for the derivation of a random function which guarantees the stability of the dynamical system in Lyapounov's sense, and exhibits the principal features of the thermodynamical entropy function.

There is no evidence of the identification of both these functions, because both are dependent upon different variables. Nevertheless, some dynamic systems such as Mixing and K-flow give more insight into the possibility of a reconciliation between the mechanical and thermodynamical point of view. Mixing and K-flow are particular resolutions of the Liouville equation. The latter governs the natural motion in phase space. If $\varrho(t)$ represents the density of states in the phase space, obeying the Liouville equation, the functional Ω may be defined as follows:

$$\Omega = \infty \varrho^{\dot{t}}(t) M \varrho(t) dpdq \qquad (1)$$

The Poincaré-Misra theorem[19] points out that Ω can only be identified with the Lyapounov function of the system, if M appears as the functional of an operator and not of some ordinary phase variables. Misra[20] showed that, according to the Lyapounov condition, Mixing is a *necessary* condition and flow a *sufficient* condition for the existence of the operator M. Prigogine[21] identifies M with the microscopic entropy density operator and represents $M = T^t T$ where T is the time operator. But a restriction on Prigogine's identification is necessary. The principal argument against his view concerns the introduction of irreversibility from without and his fallacious derivation of a new complementarity between mechanics and thermodynamics.[22]

Nevertheless, the thermodynamic description of the systems mentioned becomes possible provided the systems can be represented by a microcanonical ensemble at an *actual* point of time. If the eigenvalue of the Liouville equation becomes *actual in the sense as explained in section 2,* the thermodynamical description appears. The *actual* point of time is the eigenvalue of the time operator T. If the time shift is represented by the mechanical Liouville operator L and by the unitary operator $U = e^{iA}$ (A hermitian), then the Liouville equation:

$$\frac{\delta A}{\delta t} = -iLA \equiv L \qquad (2)$$

and the theorem of Stone[23] yield the modified new complementarity:

$$[L,T] = i \qquad (3)$$

L represents the mechanical state, completed by "particular" initial boundary conditions which guarantee the actual reproducibility of the collective property of the system (in the case mentioned represented by the eigenvalue of L and confined in the indecomposable volume V of the phase space). T represents the time operator and is linked with the entropy operator $M = T + T$ which refers to the thermodynamic description of the system. Prigogine only demonstrated a complementarity between the "bare" mechanical state and the T operator, while in the complementarity which is suggested here, the prerequisite of the *actual* reproducibility of a *collective quantity* which is represented in phase space by the hermitian operator A, is essential to reconcile time reversal mechanics with irreversible thermodynamics.

Conclusion

Macroscopic irreversibility is not directly generated by the Newtonian framework but by the *actual* reproducibility of "initial" *collective* physical quantities involving the thermodynamic description which complements the mechanical framework.

Catholic University of Leuven, Belgium

NOTES

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T. GRABIŃSKA AND M. ZABIEROWSKI

NEWTONIAN GRAVITATIONAL THEORY AND GENERAL RELATIVITY IN THE LIGHT OF THE CORRESPONDENCE BETWEEN THEIR MATHEMATICAL MODELS

1. Introduction

The correspondence relation between two physical theories T' and T reflects their mutual kinship. It enters into consideration whenever the new theory T' is formulated and its formal and substantial ties with the previous one T are studied. In the present paper it is assumed that two theories T' and T satisfy the correspondence relation[1] if:

1. The new theory [T'] is empirically consistent with the older theory [T] in their common range of application to the description of physical reality. This requirement is often called the cumulativity of knowledge.
2. Both theories can be formulated in uniform language and it is possible to proceed from T' to T by the reduction of the denotation of the respective linguistic expressions. This requirement is often called the formal correspondence postulate.

This formulation of the correspondence relation is neither unique nor universal. Yet, it is very useful in order to elucidate the relation between two gravitational theories, viz. the general relativity theory [GR = T'] and the Newtonian theory of gravitation [NT = T]. This paper will focus on the properties of the two theories as well as on the validity of the formulation of the correspondence relation which they imply.

In the case where T' is the special relativity theory [SR] and T is the Galilean kinematical theory [GT] the two requirements of correspondence seem to be fulfilled.[2] The cumulativity condition is satisfied because SR reflects all the kinematical phenomena which are peculiar to the GT description. The formal correspondence between the mathematical models of the theories, i.e. the space-time with the Lorentz group of automorphisms for SR and the space-time with the Galilean group of automorphisms, is realized for $\frac{v}{c} \to 0$, where c is the universal constant of the velocity of light and v stands for the velocity of rectilinear and uniform motion.

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The Galileo space-time automorphisms are well known and given by the formulae:

$$x' = x - vt \ , \ y' = y \ , \ z' = z \ , t' = t,$$

where x', y', z', t' and x, y, z, t denote space-time coordinates of two relatively moving frames of reference. The Minkowski space-time automorphism group [the Lorentz group] is given by the following transformations:

$$x' = \frac{(x-vt)}{1-(\frac{v}{c})^2)^{\frac{1}{2}}}, y' = y, z' = z, t' = \frac{(\frac{vx}{c^2})}{(1-(\frac{v}{c})^2)^{\frac{1}{2}}}.$$

In the Galilean approximation for $|v| \ll c$ and $|x| \ll |ct|$ the Lorentz transformations are replaced by the series:

$$x' = (x-vt)(1+\frac{1}{2}(\frac{v}{c})^2 + \frac{3}{8}(\frac{v}{c})^4 + \cdots),$$

$$t' = t(1-\frac{vx}{c^2})(1+\frac{1}{2}(\frac{v}{c})^2 + \frac{3}{8}(\frac{v}{c})^4 + \cdots).$$

It can be claimed that the correspondence between GR and GT is formulated satisfactorily on the level of global symmetries of the respective space-times for the limit $\frac{v}{c} \to 0$.

One might question whether there is a similar form of correspondence between the gravitational theories GR and NT. However, the simple generalization of the correspondence between the kinematical theories does not obtain for GR and NT, because the two pairs of theories are concerned with different physical phenomena – kinematical and dynamical phenomena respectively. A careful study of the correspondence between gravitational theories requires a brief presentation of GR as a generalization of NT.

2. *The Newtonian theory of gravitation in the framework of general relativity*

The GR equations for gravitational potential $g_{\mu\nu}(x)$ are built in a similar manner as Poisson's equation in NT. The left hand side reflects the geometrical properties of space-time [i.e. the properties of the gravitational field] whereas the right hand side expresses the distribution of matter and fields

$$G_{\mu\nu} = \mathcal{K} T_{\mu\nu}, \qquad \mu,\nu = 0,1,2,3, \qquad [1]$$

where $G_{\mu\nu} = R_{\mu\nu} - (\frac{1}{2})g_{\mu\nu}R$ and is called the Einsteinian tensor, $R_{\mu\nu}$ is

the twice contracted Riemannian tensor depending on $g_{\mu\nu}$ and its derivatives, $R = g^{\mu\nu}R_{\mu\nu}$, and $T_{\mu\nu}$ is the momentum-energy tensor, font 7 H $\varkappa$ is a constant. The simple transformation of eq. [1] gives the following form of the GR gravitational equations:

$$R_{\mu\nu} = \mathcal{H}(T_{\mu\nu} - (\frac{1}{2})\, g_{\mu\nu}T) \ , \qquad [2]$$

where $T = g^{\mu\nu}T_{\mu\nu}$.

The Newtonian approximation of the gravitational field description needs the weak fields which cause the velocities $|v| \ll c$. Then all non-zero components of $T_{\mu\nu}$ are reduced to a single one: $T_{00} = c^2\varrho$, where ϱ stands for matter density. Consequently, there remains but one single equation [2]:

$$R_{00} - (\frac{1}{2})g_{00}R = \mathcal{H}c^2\varrho \ , \qquad [3]$$

If one takes into account the stationary nature of the gravitational field in the Newtonian approximation, then GR gravitational equations too are reduced t0 0ne single expression:

$$c^2\Delta(\frac{g_{00}}{2}) \approx \frac{(\mathcal{H}c^4\varrho)}{2} \ , \qquad [4]$$

where Δ is defined as the three-dimensional Laplace operator $\Delta = \frac{\delta^2}{\delta x_1{}^2} + \frac{\delta^2}{\delta x_2{}^2} + \frac{\delta^2}{\delta x_3{}^2}$. Compared with the Poisson equation for the Newtonian gravitational potential ϕ

$$\Delta\phi = 4\pi k\varrho \ ,$$

Eq. 4 implies the following identification of GR quantities with Newtonian quantities:

$$g_{00} = (\frac{2\phi}{c^2}) + const, \qquad [5]$$

$$\mathcal{H} = \frac{8\pi k}{c^4},$$

where k stands for the gravitational constant.

Formula [5] can be presented as

$$g_{00} = (1+\frac{2\phi}{c^2}) + const. \qquad [6]$$

The constant term in [6] is actually equal to 0 because in NT the field intensity is negligible at great distance from its source. Hence one should expect the Galilean metrics for three-dimensional space of the Newtonian approximation

$$g_{ij} = \delta_{ij}, \quad g_{0i} = 0, \qquad i,j=1,2,3.$$

Unfortunately, in this approximation the metrics is reduced[3] to

$$g_{ij} = \eta_{ij} + h_{ij}$$

where $\eta_{\mu\nu}$ is the Minkowski metric tensor and $h_{ij} \sim |\frac{2\phi}{c^2}|$.

Thus, it is impossible to obtain all the elements of NT from GR.

3. *The possible forms of correspondence relation between GR and NT*

The cumulativity requirement is fulfilled in both GR and NT. The latter is applicable only if motions caused by the gravitational field are slow $[|v| \ll c]$. GR is more general than NT because it describes all kinds of motion in both the static or changing gravitational field. In the common range of description both theories provide the same numerical values of physical parameters: The GR results differ from NT only in the terms $(\frac{v}{c})^2$ which are negligible in NT.

The appraisal of the formal correspondence between GR and NT appears to be harder than it is in the case of the cumulativity requirement. As it was shown, the derivation of all the formulas of NT from GR under the assumption of weak static gravitation field is not conclusive. Thus, the transition from GR to NT is not fully realized if the limiting transition between their individual formulas is taken into account. Such a requirement for the formal correspondence is met only inasmuch as the Newtonian theory treats space-time as an unchangeable arena for physical interactions, i.e. for dynamics. Newtonian heuristics distinguished the space-time geometrical expressions from the formulas of a dynamical theory. In fact, from the point of view of the Cartanian geometrical reconstruction of NT such an assumption is inadmissible. The example of the lack of formal correspondence between GR and NT points out the failure of the Newtonian concept of space-time within his gravitational theory.

4. *The reconstruction of kinematical theories and gravitational theories in a geometrical language*

The comparison between the four theories under consideration, [SR,GT] and [GR,NT], may be presented very clearly if they are formulated in the language of Riemannian geometry.[4]

The mathematical models of both kinematical theories SR and GT are four-dimensional affine spaces [E,V] supplied with an affine connection Γ which in these cases is trivially defined, i.e. its torsion tensor Q as well as its curvature tensor R are equal to 0 [E is a set, V is a vector space, and R here means $R_{\mu\nu\varrho\sigma}$]. They differ only in the form of their metrics. In SR it is simply the metric tensor $g_{\mu\nu} = \eta_{\mu\nu}$ [the Minkowski tensor] satisfying the condition

$$\nabla g_{\mu\nu} = 0 \,, \qquad [7]$$

[∇ is the ordinary differentiation operator] while in GT the metrics splits into two tensors χ and τ , where $\tau : V \to \mathbb{R}^1$ is the form of absolute time, and χ is the scalar product defined in space $S = \left\{ v \in V : \tau(v) = 0 \right\}$ of spatial vectors. Because the formal correspondence between SR and GT is complete, the following conditions must be satisfied according to eq. [7]:

$$\Delta\chi = 0 \,, \quad \nabla\tau = 0 \,.$$

In the case of the presence of a gravitational field the models of both theories [GR and NT] can no longer be the affine spaces: the affine connection is no longer trivial –

$$R \neq 0, \quad Q = 0 \,.$$

The four theories may be symbolically be represented as follows:

$$SR \equiv \left\{ E, \Gamma, g : Q=0, R=0, \nabla g=0 \right\}$$

$$GT \equiv \left\{ E, \Gamma, \chi, \tau : Q=0, R=0, \nabla\chi=0, \nabla\tau=0 \right\}$$

$$GR \equiv \left\{ E, \Gamma, g : Q=0, \nabla g=0 \right\}$$

syntax error file -, between lines 348 and 348

$$NT \equiv \left\{ E, \Gamma, \chi, \tau : Q=0, R_{00} \sim \varrho \,, \nabla\chi=0, \nabla\tau=0 \right.$$

The transition from SR to GT consists simply in the splitting up of the metric tensor $g \to (\chi,\tau)$ whereas the transition from GR to NT means $g \to (\chi,\tau)$ and $R_{\mu\nu\varrho\sigma} \to R_{00} \sim \varrho$ as in the weak static field limit. [The transition from GR to SR and from NT to GT are trivial because they only reflect the very definition of the theory of gravitation.]

The formal correspondence between GR and NT is understood here as a simple reduction of two geometrical objects, i.e. the metric tensor $g_{\mu\nu}$ and the curvature tensor $R_{\mu\nu\varrho\sigma}$.

5. The open questions of correspondence relation and some methodological remarks

In the Cartanian reconstruction of theories here presented, the transition from one kinematical theory to another is described in the same language as the transition from one dynamical theory to another. One may question the validity of such "symmetry" of transitions if the formal

correspondence is considered as the transition between groups of automorphisms of the respective theories. Unfortunately, the "symmetry" does not work because there are no distinct global space-time automorphisms in gravitational theories. It might be suggested that there should be a formal correspondence between local [gauge] automorphism groups. However, such gauge transformations are yet to be discovered for GR and NT. Such a discovery might provide a new and more general formulation of gravitational phenomena. Thus, on this point the goals of physical theoretical research and those of methodological investigations do converge.

The example considered not only sheds light on the problem connected with the mutual relations between GR and NT, but it also allows us to formulate more general methodological conclusions:

1. The role of the correspondence relation is not only descriptive but also normative. As it was shown in section 2., the construction of GR gravitational equations was modelled after the Newtonian gravitational theory: the consistency of the theories governed the choice of the $G_{\mu\nu}$ form which otherwise could be defined in more general way. Moreover, the investigation of the correspondence between automorphism groups opens a new perspective upon the development of gravitational theory.
2. It is not possible to provide a general clue as to how to understand the formal correspondence: is it a correspondence between individual formulae or between respective mathematical models of theories? In every case the form of correspondence depends upon the formalism of theories. In a more advanced mathematical formalism the correspondence between the structural elements [e.g. geometrical objects in space-time as in the case of GR and NT] appears to be the proper way of comparing both theories.
3. The present discussion of the correspondence relation confirms the claim already made by Amsterdamski[5] concerning the superiority of the formal correspondence requirement in respect of the cumulativity postulate. The second one is weaker, for it is automatically satisfied if the first one is, but not vice versa.

Wroclaw Technical University, Poland

NOTES

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W. HORNIX

CHEMICAL AFFINITY IN THE 19TH CENTURY AND NEWTONIANISM

Newton's influence on the development of chemistry did not originate in his chemical work. It is mainly the general impact of the "mechanical philosophy" of the *Principia,* and the specific mechanical explanation of the chemical displacement-reaction, given in the "Queries" of the *Opticks* that resonates in the theories of the chemists of the post-Newton era. For the eighteenth century this is set out in detail by Metzger, Schofield and Thackray. There are only fragmentary studies on Newtonianism in modern chemistry (since Lavoisier).[1] Particularly, two subjects are important: atomic theory and chemical affinity. For this Paper I choose the theory of affinity and I will concentrate on the contribution of three researchers: Faraday, Joule, and Helmholtz. This choice is suggested by the undervaluation of their influence on nineteenth century affinity theory. In the discussions of the history of this subject, which covers the time between Berthollet's law of mass action of 1801 and that of Guldberg and Waage as formulated in 1879, one usually forgets to mention the electrochemical affinity theory which dominated that period.[2] Thermochemistry, although not neglected, is historically closely related to electrochemistry, as I hope to show. Chemical thermodynamics started its triumphal career with the solution of electro-chemical problems.

Volta's discovery of the galvanic battery started a long and succesful research program in chemistry. Voltaic electricity appeared to be able to decompose many chemical compounds. The electrolysis of the alkalis, by Davy in 1807, fulfilled the high expectations of the chemical world, and spurred many to join in electrochemical research. Davy and Berzelius explained the affinity forces between the component particles of a compound as electrical forces. In electrolysis these forces are overcome by the electrical force of the poles. The latter force can be increased at will through series connection of the cells of a battery.

Faraday's contributions to electrochemistry are registered in the memory of the chemical community by the two laws about electrolysis which bear his name. The first, announced in 1832, said "The chemical power (i.e. the quantity of matter decomposed (329)) is in direct proportion to the absolute quantity of electricity which passes." The second law is a later combination of two of Faraday's statements, announced in 1833:

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> for a constant quantity of electricity, whatever the decomposing conductor may be ..., the amount of electro-chemical action is also a constant quantity, i.e. would always be equivalent to a standard chemical effect founded upon ordinary chemical affinity.

I think Faraday here intended only to introduce "electrochemical equivalents" and to prepare for the second law, which states for the products of electrolysis that "their electrochemical equivalents are the same as their ordinary chemical equivalents."[3] The maximum content of the first statement is, that it is identical with the second.

Pearce Williams and Levere, probably deceived by the last phrase of the first statement, suggest that the two laws also give a measure for chemical affinity.[4] This, however, is clearly denied in the 8th series of the Experimental Researches, which investigates the mechanism of the galvanic cell. Faraday concludes:

> The electricity of the voltaic pile ... is not dependent, either in its origin or its continuance upon the contact of the metals with each other ... It is entirely due to chemical action (882) *and is proportionate in its intensity to the intensity of the affinities concerned in its production* (1908); and in its quantity to the quantity of matter which has been chemically active during its evolution (869) ....[5]

The "intensity of the electricity" (or "current") of a battery is equal to the sum of the "intensities" of the separate elements. If the intensity of a battery exceeds the "intensity of the chemical affinity" of an electrolyte this can be decomposed. The electrolytes now can be ordered according to the intensities of their chemical affinities, and although Faraday does not explicitly propose a quantitative measure, in principle it is already given.

Faraday's ideas of the mechanism of the generation of current in the galvanic cell, and of electrolysis, are succinctly expressed:

> Volta-electro generation is a case of mere chemical action, so Volta-electro decomposition is simply a case of the preponderance of one set of chemical affinities more powerful in their nature over another set which are less powerful ... all the facts show us that the power commonly called chemical affinity, can be communicated to a distance through the metals and certain forms of carbon; that the electric current is only another form

> of the forces of chemical affinity, that its power is in proportion to the chemical affinities producing it ... The forces termed chemical affinity and electricity are one and the same.[6]

The "Experimental Researches in Electricity" seem to be a classic example of "Experimental Philosophy," giving an accurate report of the experiments and the considerations guiding these and the conclusions to which they lead. However, Pearce Williams has convincingly demonstrated that the Experimental Researches give the context of justification, rather than the context of discovery. Faraday's research was directed by a vision upon nature related to that of Boscovich, who envisaged the properties of matter as the resultant of the force fields of point atoms. Well aware of the resistance and incomprehension towards this abstract theory in the scientific and philosophical climate of Britain, Faraday tactically decides not to show his convictions before these could be supported by a convincing 'experimental' argumentation. Not until 1844 did Faraday discuss these ideas openly.

In sofar as Boscovich's theory can be called Newtonian, Faraday's thought was under the influence of Newton. Faraday's affinity theory found its inspiration in a sophisticated form of Mechanical Philosophy, it got its final clear and convincing form through the methods, and in the language of Experimental Philosophy.

James Joule's electrochemical work can be considered as the continuation of Faraday's investigations on chemical affinity. Its starting point was Joule's discovery in 1840, that the heat production, resulting from the passage of an electric current i, during a time Δt, through a metallic resistance R , is equal to $Ai^2R\Delta t$.[7] For a circuit consisting of a galvanic cell, with an internal resistance r, and an external resistance R, a similar law obtains $Q(\Delta t) = Ai^2r\Delta t + Ai^2R\Delta t$; however, one has to take into account, that there is an extra heat effect in the galvanic cell, caused by other reactions than the current generating reaction of the cell, which effect has to be added to the heat effect of the current. In case of the Smee-cell: Amalgamated Zn; diluted sulphurid acid; Platinized silver, Joule distinguished, with Faraday, the current generating reaction $Zn + HO \rightarrow ZnO + H$, and the subsequent neutralization reaction $ZnO + SO_3 \rightarrow ZnO.SO_3$. The heat effect of the neutralization reaction has to be added to the heat, associated with the current i, and the resistances r and R in the circuit, the so-called "voltaic heat of the current generating reaction." Because $i(r+R)$ is equal to the "intensity of the galvanic cell" (E) and $i\Delta t$ is according to Faraday's laws of electrolysis, proportional to the number of atoms n ("whether, water or zinc") con-

cerned in generating the current, the "voltaic heat" can be written as $Q_{voltaic}$ $E_{galv.}n$. In case there is also an electrolytic cell in the circuit, an extra complication arises, because beside the normal "resistance of conduction" of the electrolytic cell (r_c), responsible for the heat effect $Q_{el.cell} = Ai^2r_c\ \Delta t$, there is a "resistance to electrolization" (r_e), which does not affect the heat effect in the cell, but complicates the measurement of the "resistance of conduction," causing an electromotive force $E_{el.cell} = ir_e$ counteracting the e.m.f. of the battery of the circuit. For the complete circuit:

$Q_{voltaic} = Ai^2(r + r_c + R)\ \Delta t = B\ (E_{galv.cell} - E_{el.cell})\ n.$

At the end of 1840 Joule thought that these results justified the following conclusion:

> Berzelius thinks that the light and heat produced by combustion are occasioned by the discharge of electricity between the combustible and oxygen with which it is in the act of combination; and I am of opinion, that the heat arising from this ... is the consequence of resistance to electric conduction. My experiments on the heat produced by the combustion of zinc turnings in oxygen ... strongly confirm this view.[8]

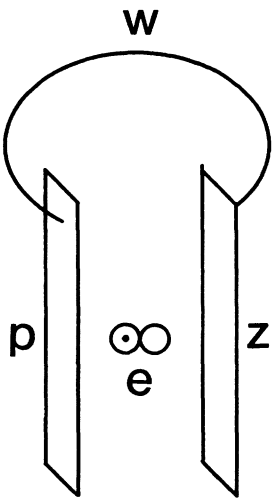

Fig. 1.

This confirmation consisted in the demonstration that the "chemical heat" evolved in the immediate oxidation-reaction is equal to the "voltaic heat" of the corresponding current generating reaction in a galvanic cell. An extended report of the researches, justifying the statement "chemical heat" = "voltaic-heat," was given in Joule's paper "On the Electric Origin of the Heat of Combustion," read before the Manchester Literary and Philosophical Society in November 1841.[9] We find in this paper a more refined interpretation of the electro-chemical phenomena, now closely related to Faraday's thoughts (See Fig.1):

> Let p represent a plate of platinum, z a plate of zinc, or other electropositive metal; and e, one of a series of atoms of water, extending from p to z. The intensity of the current along the

wire w is proportional to the affinity of oxygen for the positive metal, minus the affinity of oxygen for hydrogen.[10]

Joule understands under "the intensity of the current" the tension between p and z, because he measures the 'intensity' with a galvanometer of high internal resistance, "in order that the deviations of the needle may be depended upon in calculating the intensity of the current." It is clear that the electromotive force of a galvanic cell measures the affinity of the current generating reaction. In the case of the electrolytic cell it is the electromotive force necessary to overcome the "resistance of electrolization" which measures the affinity of the electrolysis reaction. Joule used the circuit of the following figure (Fig. 2):

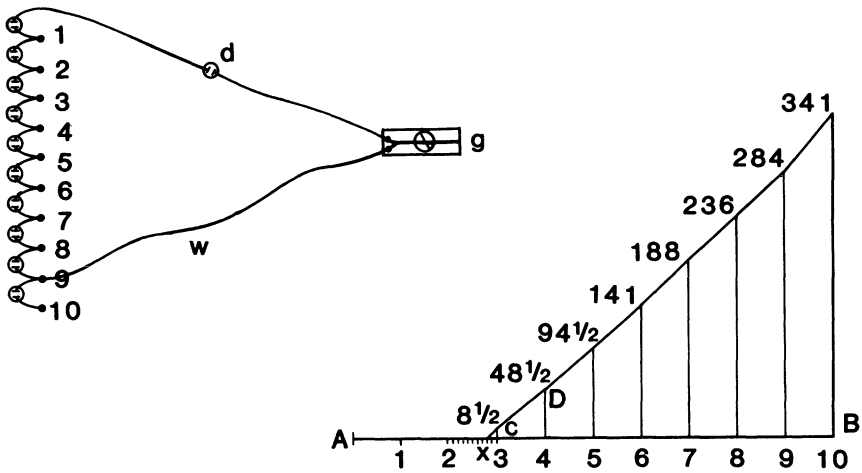

Fig. 2

The deviation of the needle is plotted against the number of Smee cells in series with the electrolytic cell d and the galvanometer g. The intersection of the straight line through the plotted points, and the abscissa, gives the quantity of the electromotive force expressed with the Smee cell as unit, and measures the affinity of the electrolysis reaction.

The measured "intensities" can be turned into "voltaic heats," using the result that the Smee cell (i.e. the unit of electromotive force) generates per unit of resistance, in one hour, a quantity of heat, giving an increase of 15.12° F to one pound of water, and oxidizes a quantity of 3.234

equivalents of zinc.

TABLE 1

Joule's first confirmation of "voltaic heat = "chemical heat"

oxidation of	"voltaic heat"	"chemical heat"
zinc	13°83	10°8
iron	12°30	9°48
potassium	21°47	17°6
hydrogen	10°47	8°36

Joule states that the comparison of the resulting values of the "voltaic heat" of the reactions considered with the results of the calorimetric determination of the "chemical heat" confirms their equality. Differences up to 30% are accepted: "Considering the difficulty of preventing some loss of heat (in caloric measurements on combustion reactions W.H.) it will, I think, be admitted that experiment agrees with the theory as well as could have been expected."[11]

The next important paper, read in January 1843, announces, in the 5th concluding general observation, the possibility of the conversion of work into heat by means of the magnetic electrical machine. The immediately following 6th observation can still be considered as consistent with the caloric theory of heat: "Electricity may be regarded as a grand agent for carrying, arranging and converting chemical heat."[12] I suppose that, at the beginning of 1843, Joule was changing his mind and that this is reflected in the text of this article.

Joule's discovery of the equivalence of heat and work presumably resulted from the extension of his research to circuits in which a revolving electromagnet is introduced.[13] In a coil rotating between the poles of a magnet a current is induced. Joule demonstrated the heat effect of the induced current follows the same law as that of a voltaic current $Q\ (\Delta t) = Ai^2R\Delta t$.

> I now proceeded to consider the heat evolved by voltaic currents when they are counteracted or assisted by magnetic induction. For this purpose it was only necessary to introduce

> a battery into the magneto-electrical circuit. Then by turning the wheel in one direction, I could oppose the voltaic current; or, by turning in the other direction, I could increase the intensity of the voltaic by the assistance of the magneto-electricity.[14]

It appears that again the measured, resultant, current, and measured heat effects are related through $Q\ (\Delta t) = i^2\ R\ \Delta\ t$. This result, although perhaps expected, has a completely unexpected consequence. Joule realized that it is impossible that at the same time for the whole circuit $Q(\Delta t) \sim i^2(R+r)\Delta t$ and $Q(\Delta t) \sim \frac{\Delta n}{\Delta T}\Delta t \sim \Delta t$ (according to Faraday's laws). He concludes that there is another source of heat, which is responsible for the difference between the two calculated $Q's$ for the same i. "We have therefore in magneto-electricity an agent capable by simple mechanical means of destroying or generating heat."[15] Joule's attempt "to connect heat with mechanical power in absolute numerical relations" then leads to the first determinations of the mechanical equivalent of heat. He calculates the 13 values of Table 2, resulting in a mean value of 838 lb foot, for a temperature increase of 1°F of pound of water.

TABLE 2

Joule's first determinations of the mechanical equivalent of heat

896	lb foot
1001	lb foot
1040	lb foot
1026	lb foot
910	lb foot
587	lb foot
742	lb foot (Mean value of 5 determinations)
860	lb foot (Mean value of 2 determinations)

Mean value 838 lb foot for a temperature increase of 1°F of 1 pound of water.

Joule does not suspect a fundamental flaw in his method.[16] He admits "that there is a considerable difference between some of the results, but not, I think, greater than may be referred with propriety to mere errors of experiment." He concludes his paper in August 1843 in a P.S.:

> I shall lose no time in repeating and extending these experiments, having satisfied that the grand agents of nature are, by the Creator's fiat, *indestructible;* and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained.[17]

And somewhat further, he adds:

> I now venture to state more explicitly, that it is not precisely the attraction of affinity, but rather the mechanical force expended by the atoms in falling towards one another, which determines the intensity of the current, and consequently the quantity of heat involved...[18]

Again, on February 20th 1844, he adds:

> I have lately been at some pains in framing the following theory of heat. Setting out with the discovery of Faraday that each atom is associated with the same absolute quantity of electricity, I assume that these atmospheres of electricity revolve with enormous rapidity round their respective atoms; that the momentum of the atmospheres constitutes "caloric," while the velocity of their exterior circumferences determines temperature...[19]

And some years later in his prize essay "On the Heat disengaged in Chemical Combination" published in 1846:

> The fact that the heat evolved in a given time by a metallic wire is proportional to the square of the quantity of transmitted electricity, proves that the action of the current is of a strictly mechanical character; for the force exerted by a fluid impinging against a solid body obeys the same law.[20]

Finally, in his 1847 lecture "On Matter, Living Force, and Heat":

> Indeed the phenomena of nature whether mechanical, chemical, or vital, consist almost entirely in a continual conversion of attraction through space, living force, and heat into one another. Thus it is, that order is maintained in the universe - nothing is deranged, nothing ever lost, but the entire machinery, complicated as it is, works smoothly and

> harmoniously ... the whole being governed by the sovereign will of God.[21]

As I have shown, Joule's philosophical ideas did not direct his research. They are clearly afterthoughts, used to justify his discoveries, pretending that they are the consequence of that strange mix of mechanical philosophy and natural theology, which was so popular in Britain since Newton. His ideas about the nature of electricity are vague. Those about the nature of the affinity forces unfounded, and his mechanical world view is only superficially related to the physical laws he discovered.

It was Helmholtz who, in 1847, presented a much more consistent elaboration of a mechanistic world-view in "Ueber die Erhaltung der Kraft."[22] He defined the energy of a mechanical system, and derived the principle of conservation of energy for conservative systems, starting from the laws of mechanics. With respect to the conversion of heat and work he is aware of the hypothetical and provisional character of the mechanical theory of heat, and proposes a more elaborated theory as an attempt to relate the physical and chemical phenomena to the "vis viva" (kinetic energy) and the "tension" (potential energy) of the atoms. Helmholtz sees Hess' law as a consequence of the principle of energy conservation:

> According to our way of viewing the subject, the quantity of heat developed by the chemical processes would be the quantity of vis viva produced by the chemical attractions, and the above law would be the expression for the principle of the conservation of force in this case.[23]

Also Joule's equality "voltaic heat = chemical heat" is considered as an implication of energy-conservation: for a circuit consisting of a galvanic cell, without polarization, and a resistance applies according to Helmholtz:

> During a given period of time there is no other change than that of the chemical proportions, and the quantity of heat. therefore the law of conservation of energy would require, that the amount of heat gained by the chemical reaction must be equal to the heat actually obtained (i.e. the voltaic heat W.H.).[24]

With these arguments Helmholtz offered a new mechanistic foundation to thermochemistry which was based on Hess' law and Joule's equality.

In retrospect this may be considered a fatal development. It took at least thirty-five years before the thermochemists became aware that both laws were mistaken. One of the greatest of them, Berthelot, tried till the end of the century to reconcile the laws with inconsistent results of improved measurements. It was Helmholtz himself who, in 1882, broke at last the spell of the equality chemical heat = voltaic heat, and offered a better foundation to thermochemistry, and a new view on chemical affinity. In his famous article "Die Thermodynamik chemischer Vorgänge,"[25] Helmholtz proved that the two fundamental functions of the thermodynamics of Clausius, U and S could be replaced by a new function the "free energy" $F(T,V) = U - TS$ via $S = -\frac{\partial F}{\partial T}$ and $U = F - \frac{1}{T}\frac{\partial F}{\partial T}$. The external forces (e.g. pressure, e.m.f.) could be expressed as $P_a = -\frac{\partial F}{\partial p_a}$ reversible work and heat by $dW_{rev} = \sum P_a dp_a = -dF - SdT$ and $dQ = TdS$ respectively.

For a galvanic cell, working at constant temperature, he derived $dW_{rev} = Ede = -dF$ and $dQ_{rev} = dU + Ede = T\frac{\partial E}{\partial T}de$, or

ΔU ("chemical heat") $-\Delta F$ ("voltaic heat") $= Q_{rev} = T\frac{\partial E}{\partial}T\ nFar$,

where Far is the charge of one equivalent. Consequently "chemical heat" $\neq$ "voltaic heat," and the difference can be measured with great precision via $\frac{\partial E}{\partial T}$. The e.m.f. E is, for the general case, replaced by ΔF as the measure of chemical affinity.

Helmholtz's contributions to the development of the formalism of thermodynamics and its application to chemical problems are well known to students in chemistry. His explanation of the terms free and bound energy are only of historical and philosophical interest. Free energy (F) is that part of the total or internal energy (U) that can be transformed completely into heat, electrical energy, mechanical energy etc.. The transformation of bound energy (TS) is subjected to the second law. Bound energy is present in the form of heat. This is specified in a draft for an *Introduction to Thermodynamics* of 1883:[26] heat taken up in phase transitions and becoming 'latent' belongs, with the free heat, to the bound energy of the system; heat taken up in isothermic expansion of a gas ($T\Delta S$) has also become latent, and is added to the bound energy. The work done decreases the free energy; in reversible adiabatic compression of a gas, the work done is transformed partly into bound energy ($S\Delta T$) and partly into free energy (ΔF); the difference between ΔF

and ΔU in isothermic chemical reactions is due to changes in bound energy connected with phase transitions. All this remains within the framework of phenomenological thermodynamics. One paragraph of Helmholtz' 1882 article is however written in a different language. He calls the work equivalent of heat, "which might for the most part, be considered as the vis viva of invisible molecular movements," the vis viva of the disorderly molecular movements. This expression, in the language of particle mechanics, connects the article with the papers on monocyclic systems, that Helmholtz published between 1884 and 1886.[27] These systems form a mechanical model with properties analogous to those of thermodynamic systems. This led to a mechanistic interpretation of thermodynamic variables and of the second law of thermodynamics.[28] In 1886 Helmholtz extended the basic equations of thermodynamics by introducing the time t, and the vis viva of visible (orderly) movements" L_0 by writing:

For the external force P_a and the associated variable p_a, q_a being $\frac{dp_a}{dt}$:

$P_a = -\frac{\partial}{\partial p_a}[F-L_0] - \frac{d}{dt}(\frac{\partial L_0}{\partial q_a})$, as an extension of $P_a = -\frac{\partial F}{\partial p_a}$,

$\frac{dQ}{dt} = -T\frac{d}{dt}(\frac{\partial F}{\partial T})$, as an extension of $dQ = -T(\frac{\partial F}{\partial T})$, and $U = F - T\frac{\partial F}{\partial T} + L_0$, as an extension of $U = F - T(\frac{\partial F}{\partial q_a})$.

The first equation is analogous to the Lagrangian equation:

$$P_a = -\frac{\partial}{\partial p_a}[\phi - L] - \frac{d}{dt}(\frac{\partial L}{\partial q_a})$$

and the third to $U = \phi + L$, F being analogous to the potential energy ϕ, and $-T\frac{\partial F}{\partial T} + L_0$ analogous to the kinetic energy L of mechanics; $-T\frac{\partial F}{\partial T} = TS$ being the kinetic energy of the disorderly movements.[29]

The object of the articles on mono-cyclic systems was to define special properties of mechanical systems so as to make the analogy complete. If Helmholtz, already in 1882, consciously introduced free and bound energy (and the formalism describing it) as the thermodynamic analogues of potential and kinetic energy, just to establish the relationships with Hamiltonian mechanics, then the influence of Newtonianism on the development of phenomenological thermodynamics is convincingly demonstrated.

University of Nijmegen, The Netherlands

NOTES

1. See Metzger (1923 and 1930), Schofield (1970), Thackray (1970), Williams (1965) and Levere (1971).
2. For instance Hiebert (1962), Holmes (1962) and Schelar (1966). An exception is Levere (1971), that treats the electrochemical affinity theory of Davy and Faraday extensively.
3. Faraday (1839) p. 107 (no. 377), p. 145 (no. 505), p. 245 (no. 839).
4. Williams (1965) p. 255 writes: "the first law stated that chemical action was exactly proportional to the quantity of electricity which passed through the solution undergoing decomposition. Faraday's theory attributed the decomposition to the distortion of the forces of affinity. The laws of chemistry, as Faraday understood them, correlated the forces of affinity with the combining or equivalent weights of the elements." Levere (1971) p. 87 comments on Faraday's first law "Since the results held generally for all electrolytes, the latter could be arranged into a single series, where the individual substances occupied definite positions, corresponding to the precise degree of their chemical affinities."
5. Faraday (1839) p. 272 (no. 916).
6. Faraday (1839) p. 272, 273 (no. 917, 918).
7. Joule (1840); Joule (1841).
8. Joule (1841): (1887) 1, p. 78.
9. Joule (1842).
10. Joule (1842): (1887) 1, p. 90-91.
11. Joule (1842): (1887) 1, p. 101-102.
12. Joule (1843): (1887) 1, p. 120.
    It cannot be excluded that Mayer's 1842 paper, which Joule translated – according to Steffens (1979) - in 1843 or early 1844, played an important role in Joule's renunciation of the caloric theory of heat. Kuhn's argumentation for simultaneity is not convincing (Kuhn 1959).
13. Joule (1843).
14. Joule (1843): Joule (1887) 1, p. 139.
15. Joule (1843): Joule (1887) 1, p. 146.
16. Joule was not aware of the insufficiency of the concept of electrical resistance, in case of circuits with a revolving electromagnet. The behavior of these circuits can be described with the concept of electrical impedance; the latter is dependent on the speed of rotation of the electromagnet.
17. Joule (1843): Joule (1887) 1, p. 157-158.
18. Joule (1843): Joule (1887) 1, p. 158.
19. Joule (1846): Joule (1887) 1, p. 122.
20. Joule (1852): Joule (1887) 1, p. 235.
21. Joule (1852): Joule (1887) 1, p. 273.
22. Helmholtz (1847).
23. Helmholtz (1847): translation (1853) p. 134.

24. My translation. Tyndall translates (1853): "During a certain interval the action of such a circuit will be changed in no respect save in its chemical conditions and quantity of heat ..." Date translates the same sentence in Ostwald (1896), translation (1980) p. 752: "During a given period of time there is no change in the action of such cells in regard to the chemical conditions and the quantity of heat ..."
25. Helmholtz (1882).
26. Koenigsberger (1903) bd2 pp. 298-303.
27. Helmholtz (1884, 1) (1884, 2) (1886): Helmholtz (1895) 119-222.
28. Klein (1972) pp. 63-67.
29. Helmholtz (1882) pp. 33, 34.

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PART III

NEWTON'S METHODOLOGICAL HERITAGE

ARTHUR DONOVAN

NEWTON, LAVOISIER AND MODERN SCIENCE

What is "modern" about "modern science"? This is a rather odd question, for in one sense the phrase "modern science" is redundant. Arguably the most distinctive features of modern culture, such as its progressiveness, rationality, objectivity and self-awareness, are exemplified by science itself. And if our concept of modernity is derived from science, than science is modern by definition.

Recent developments in cultural studies have rendered this cozy identification of science and modernity highly problematic. The advent of postmodernism obliges us to distinguish between "the modern" and "the contemporary," and modernity has come to refer to a particular period in the history of culture. Like the venerable terms medieval, renaissance, and baroque, "modern" now signifies a certain constellation of social, stylistic and metaphysical preferences that predominated during a particular period of history, one that had both a beginning and a end. Today we live in a postmodern age and we must struggle, as people always had to struggle, to understand what is fundamental and distinctive to our civilization. And yet, although the modern era has ended, science continues to evolve. We therefore can no longer identify science exclusively with the preconceptions of the modern age. It is this disjunction that makes the question what is "modern" about "modern science" a matter of some urgency. And because the disjunction is historical, the question falls within the domain of the history of ideas.

Political and cultural historians have traditionally divided the modern epoch into two phases. The first, the era of Early Modern History, refers to the period between the beginning of the Renaissance and the destruction of the Old Regime in France. The second, asymmetrically labeled simply the era of Modern History, refers to the nineteenth and twentieth centuries. I wish to suggest that these two phases also mark distinct periods in the history of modern science, the first being the era of natural philosophy, the second being the era of positive science. In the remainder of this paper I will explain why I believe Isaac Newton's achievement should be understood primarily as a contribution to the natural philosophy tradition, while Antoine Lavoisier's revolutionary transformation of chemistry should be seen as a contribution to the founding of the positive tradition of Modern, as opposed to Early Modern, science.

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Two preliminary cautions are in order, however. First, my suggestion that there were two phases in the history of modern science and that they correspond to the two phases of modern European history arises from the independent study of these two subjects, not from a theory of historical causation that sees science as derived from more general social and cultural developments, or *vice versa*. Of course the active mind, upon encountering correlations, strains to discover causal connections, and I have a few hunches of my own on this score, but for the present the question of causation, which I realize is of fundamental importance, will be left open.

Second, while Lavoisier developed a style of science I characterize as positive, he was not a builder of philosophic systems and certainly was not a positivist in the full Comtean sense of the term. The distinction is of some importance. Comte, like all philosophers of science, sought to capture in philosophical terms the methods and achievements of the leading scientists of his day, and fundamental to my interpretation is the claim that the approach to science he described was the approach pioneered and practiced by Lavoisier. As all students of nineteenth-century culture know, Comte's account of science had an enormous impact. Indeed, I believe it was precisely the positivist account of science, which was repeatedly refined, that came to define what is modern about modern science. While in a general sense scientific knowledge, like religious belief and political deliberation, is an aspect of all human cultures, specifically positivist theories of science predominated during a particular historical epoch, the era we call modern. In this sense, then, the modern era of science ended when positivist accounts of science ceased to be considered either accurate or adequate. Today the construction of postpositive theories of science, while still a lively area of research, is no longer a radically innovating undertaking. And from the perspective of history, it is becoming increasingly evident that in science studies, postmodernism began with the demise of positivism.

Historical evidence never speaks unambiguously, and those who wish to find anticipations of positive science in Early Modern Europe and residues of natural philosophy in nineteenth and twentieth century science can certainly do so. And yet historians have a professional duty to resist facile identifications of past and present, for it is only by emphasizing how the past differs from the present that we can learn from the record it offers us. If today we have a richly informed historical understanding of Newton's achievement, it is largely because several generations of scholars have concentrated on explicating the origins and nature of Newton's science in the context of his time rather than in that of our own. Historical studies of Lavoisier's achievement are also beginning to reveal in de-

tail how his revolutionary reformulation of chemistry grew out of the assumptions and problems of his own age. And the more we learn about these two titans of science, the more evident it becomes that, despite the very great influence of Newton's achievement in the eighteenth century, Lavoisier was not a Newtonian.

So far as the programmatic "guiding assumptions" of Newton and Lavoisier are concerned, it has long been recognized that they worked in different research traditions and employed very different explanatory strategies.[1] Arnold Thackray and Robert Schofield have both written extensively on this point, and I.B. Cohen summarized informed scholarly opinion when he observed that "Newton had a number of brilliant insights into the structure of matter and the process of chemical reaction, but the true revolution in chemistry did not come into being until the work of Lavoisier, which was not directly Newtonian."[2] But this conclusion leaves open the question as to whether Newton and Lavoisier shared common understanding of the aims and methods of science. I believe they did not. Newton was very much a seventeenth-century natural philosopher, whereas Lavoisier, for a variety of social and epistemological reasons, avoided taking on all the baggage of traditional natural philosophy and concentrated instead on forging a more restricted program of positive science.

To give some substance to this interpretive claim, I will briefly mention three features of each of the two conceptions of science, the Newtonian and the Lavoisieran, that distinguish them from one and other. As you will see, the features selected are also central to the more general distinction that I am trying to establish as well, that is to say the distinction between the natural philosopy of the Early Modern era and the positive science of the Modern era.

The three features of Newtonianism I wish to focus on are 1) the centrality of matter theory, 2) the search for natural laws and their causes, and 3) the presumed parallelism between the order of nature and social and political order. So far as matter theory is concerned, Schofield and others have argued that the Newtonian research program can be explicated in terms of theories of matter elaborated to explain the phenomena examined by natural philosophers. Clearly one must not go too far in this direction, for the Newtonian tradition was also shaped by epistemological, methodological, social and political considerations as well, yet formulating an adequate theory of matter was at all times one of the central aims of Newtonian science.[3] Lavoisier on the other hand, could not have been less interested in ontological questions, and unlike Newton he was totally unconcerned with solving the problem of transdiction.

Of course both Newton and Lavoisier were eager to discover natural laws, and Newton will always be remembered for his law of gravitational attraction. But for Newton a law was a mere description, not an end-point of scientific investigation. Laws require metaphysically-adequate causal explanations, and Newton was tormented by his inability to explain gravitation. As I.B. Cohen has written,

> Newton's insistence that it is enough to be able to predict the celestial and terrestial motions and the tides of the sea was, in fact, less a battle-cry of the new science than a confession of failure. For what Newton was saying in essence is that the system should be accepted in spite of his failure to discern the cause or even to understand universal gravity, because its results accord so well with the data of observation and experiment.[4]

Lavoisier, while also eager to explain the regularities and relationships he discovered in nature, took a much more heuristic and even opportunistic approach to theory than Newton would have considered acceptable. Experimentally demonstrated regularities, not their putative causes, were the core of science for Lavoisier.

The meaning of the Newtonian quest for natural knowledge cannot be fully understood when isolated from natural theology and natural law. Newtonian natural philosophy was part of a more comprehensive philosophical enterprise, and while Newtonian scientists focused their attention on particular phenomena and formulated theories to explain them, their investigations were intimately connected to this larger philosophical undertaking as well.[5] With Lavoisier, on the other hand, science appears to achieve that kind of cultural autonomy that has characterized it ever since. Natural theology simply makes no appearance. And by the latter decades of the eighteenth century the Enlightenment campaign for social reform, in which Lavoisier was so deeply involved, no longer depended on scientific findings about the intrinsic order of nature. This, I believe, brought about a dramatic unburdening of the investigation of nature, one that freed science from the multiple entanglements of natural philosophy and enabled it to define itself in the disciplinary and instrumental terms so characteristic of modern science.

Three central features that distinguish Lavoisier's science from the Newtonian tradition are 1) its emphasis on experiment, 2) the centrality of language to theory construction, and 3) the way in which science was thought to impinge on social and political reform.

The relationship between Lavoisier's experimental investigations and his reconceptualization of chemistry was both novel and of immense historical consequence. Unlike Joseph Priestly, who was unrivaled in the discovery of new phenomena, and unlike Newton, whose prism experiments are classic "crucial experiments," Lavoisier designed and performed analytic and synthetic experiments in such a way that they formed a chain of demonstrations that parallel and verified every step in the chain of reasoning that led him from observed facts to laws and theories.[6] It was a new form of experimental method, one suggested by the practice of experimental physicists in the 1760s and one that Lavoisier refined and made central to the science of chemistry.[7] And it was a method that came to dominate the paradigmatic disciplines of positive science in the nineteenth century, that is to say the sciences of chemistry, physics and biology.

For Lavoisier, language rather than ontology was the key to successful theory formation. As is well known, he used the linguistic theory of knowledge developed by the Abbé Condillac both in the collaborative enterprise that led to the establishment of a new nomenclature for chemistry and in his *Elementary Treatise on Chemistry,* the book that codified the triumph of the Chemical Revolution. For Lavoisier, as for subsequent positivists, it was language rather than the theory of matter that provided the link between what we observe and how we explain it.

It is not easy to determine how Lavoisier thought about the relations between science and social and political order, for he treated the two as quite separate. Yet there is reason to think that unlike earlier natural philosophers and later social Darwinists, he did not think that the structure of nature revealed by science provides an immediate model for the ordening of human society. It appears, rather, that like many later positivists, Lavoisier believed that the human virtues that lead to success in the acquisition of scientific knowledge are precisely those one should look for in those individuals chosen to serve as social and political leaders. It is not the content of science but rather the intellectual training it provides that links the pursuit of natural knowledge and the governance of men.

I have all too briefly and hurriedly suggested why I think the achievements of Newton and Lavoisier should be seen as belonging to two different phases of modern science. The first and better known of these phases occurred during the Early Modern era, was primarily concerned with problems in natural philosophy, and is normally studied under the rubric of The Scientific Revolution of the Seventeenth Century. The second phase, I suggest, occurred in the Modern period, was primarily concerned with the positive knowledge associated with the modern scientific disciplines, and can be thought of as having begun with The

Second Scientific Revolution, 1775-1830. Whether or not this notion of two phases in the history of modern science and the suggestion that there was a Second Scientific Revolution will take hold within the history of science remains to be seen. I offer them, however, as constructive responses to an historiographic problem that I do not think will simply go away, namely the perception that the modern epoch is now over. I believe this challenging conclusion in the history of culture is one that historians of science can only ignore at their peril. *

Virginia Tech, U.S.A.

NOTES

1. For the concept of "guiding assumptions" and its relation to several leading theories of scientific change, see Larry Laudan, Arthur Donovan, et al., *Scientific Change: Philosophical Models and Historical Research,* in *Synthese,* 1986, vol.69, no.2.
2. Arnold Thackray, *Atoms and Powers,* (Cambridge, Mass.: Harvard, 1970); Robert Schofield, "The Counter Reformation in Eighteenth-Century Science – Last Phase," in Duane H.D. Roller, ed., *Perspectives in the History of Science and Technology,* (Norman: Oklahoma, 1971), pp. 39-54; I. Bernard Cohen, *The Newtonian Revolution,* (Cambridge, Mass.: Harvard, 1980), pp. 9-10. I have examined this issue in greater detail in my "Newton and Lavoisier: From Natural Philosophy to Positive Science," paper read at the University of Maryland/Smithsonian Institution Symposium on the Tercentenary of Newton's *Principia,* forthcoming.
3. See Robert E. Schofield, *Mechanism and Materialism,* (Princeton, 1969); for a summary of critiques of Schofield's interpretation, see Simon Schaffer, "Natural Philosophy," in G.S. Rousseau & Roy Porter, eds., *The Ferment of Knowledge,* (Canbridge, 1980), pp. 55-91.
4. Cohen, *Newtonian Revolution,* p. 131.
5. See especially Margaret Jacob, *The Newtonians and the English Revolution, 1689-1720,* (Ithaca: Cornell, 1976), and her "Newtonianism and the Origins of the Enlightenment: A Reassessment," *Eighteenth-Century Studies,* 1977-78, *11:* 1-25.
6. For detailed studies of Lavoisier's experimental method, see Marcellin Berthelot, *La Révolution chimique – Lavoisier,* (Paris, 1890); Henry Guerlac, *Lavoisier – The Crucial Year,* (Ithaca: Cornell, 1961); and Frederic L. Holmes, *Lavoisier and the Chemistry of Life,* (Madison: Wisconsin, 1985).

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6. For detailed studies of Lavoisier's experimental method, see Marcellin Berthelot, *La Révolution chimique – Lavoisier,* (Paris, 1890); Henry Guerlac, *Lavoisier – The Crucial Year,* (Ithaca: Cornell, 1961); and Frederic L. Holmes, *Lavoisier and the Chemistry of Life,* (Madison: Wisconsin, 1985).
7. I have explored this connection in greater detail in my "Lavoisier and the Origins of Modern Chemistry," in Arthur Donovan, ed., *The Chemical Revolution: Essays in Reinterpretation,* vol. 4 of *Osiris,* second series, (Philadelphia: History of Science Society, in press).

ED DELLIAN

INERTIA, THE INNATE FORCE OF MATTER: A LEGACY FROM NEWTON TO MODERN PHYSICS

I. Introduction.

For many scholars, the publishing of Isaac Newton's *Principia* in 1687 marks the beginning of a period of physics which they call the classical one.[1] Yet it is questionable whether Newton's "Mathematical Principles of Natural Philosophy"[2] actually represent what is known as physics today. The *Principia* is a foundation for a mathematical philosophy of nature as viewed by Plato. If Physics is within the scope of this philosophy, then it also includes metaphysics, as a presupposed knowledge of the absolute, of space and time, of matter, force and motion, of cause and effect; read the Scholium that follows the eight definitions introduced at the beginning of the *Principia.*[3] That is why physicists of the positivistic school of thought have had their problems with Newton since the time of George Berkeley and Ernst Mach,[4] and why, as a result, many people are more familiar with the title than with the contents of the *Principia.*

In my view, this "bible of classical mechanics" (Max Jammer)[5] first and foremost represents the *metaphysical elements* (in the language of synthetic geometry) of exact science, "metaphysische Anfangsgründe der Naturwissenschaft," as Immanuel Kant would have said. For, the "Erklärungen" (definitions) and the "Lehrsätze" (propositions) which Kant formulates in his own work of this name in 1786[6] coincide with Newton's definitions and axioms both in their intent as a basic foundation, and partially also in their content; one only needs to compare the third proposition of Kant's main chapter 3 with Newton's First Law of Motion, the law of inertia.

In the present discussion, it will be established that the concept of inertia, among others, clearly shows that Newton's teaching not only goes beyond what we know today as classical physics, but also that it considerably differs from that subject. Afterwards, I will show that the Newtonian concept of *inertia force* could plausibly be used in solving certain open questions in modern physics such as the problem of the wave-particle dualism.[7]

P. B. Scheurer and G. Debrock (eds.), Newton's Scientific and Philosophical Legacy, 227–237.
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II. The Principle of Inertia in Classical Mechanics and Newton's "materiae vis insita."

1. The classical principle of inertia says that a body always continues in its existing state of rest or uniform straight-line motion as long as no external forces act upon it. This should result from a basic characteristic of matter which we call *inertia.* Yet Newton's philosophy rejects explanations of nature by such hidden qualities of matter.[8] It recognizes the ability of matter to continue in its existing state as an effect of its own implanted force which Newton calls *materiae vis insita,* or *vis inertiae,* or even *impetus.*[9] Because the word *vis* simultaneously conveys the concept of both *force* and *cause,* Newton views the force of inertia as the cause of uniform straight-line motion.[10] Classical mechanics lacks this approach; due to an arbitrary generalization of Newton's concept of centripetal force *(Principia,* def.5-8), it always equates force with acceleration, and considers the uniform straight-line motion to be without force, resulting in a "causal paradox" (C.F. von Weizsäcker).[11] Of course, such a limitation of the notion of force (which came about during the Enlightenment)[12] occurred without empirical foundation. It was rather the materialistic scientific ideology which, favoring the hidden quality of "inertia" of matter, eliminated Newton's spiritual *materiae vis insita,* and generally, wherever it seemed to be possible, all immaterial forces of nature (which were, however, the essence of the Neoplatonic Newtonian Philosophy, as preached from the pulpit by Samuel Clarke as late as 1704).[13] Immanuel Kant, in his commentary to the above- mentioned proposition, also turned against the theory that inertia is more than the mere lifelessness of matter. He remarks that everything else would be hylozoism. But Kant overlooked the possibility that force as a quantity, instead of being a necessarily active principle, might also be a passive principle, or even the power of continuing in a steady state. Newton, for his part, describes the force of inertia exactly in this way in the *Principia,* def. 3. According to his Third Law of Motion, this force becomes active and acts externally, only if and as long as an active external force is imposed on the body which interacts with the body's inertial force.[14]

The issue concerns the mechanism for the creation of a material change in the motion of a body through a non-material external cause, as well as the problem of psychophysical interaction (the matter-spirit relationship) which was a central theme of philosophy in the 17th century.[15] Newton solves this problem by assuming an interaction between external force *(vis impressa)* which changes motions, and internal force *(materiae vis insita)* which maintains motions, completely at the spiritual level. G.W. Leibniz, representing the antipode of that position, attempts to

solve the same problem by proclaiming *a priori* an equality between *causa* and *effectus,*[16] between force and change of motion, declaring this equality to be his "first axiom of mechanics."[17] Conceived in this way, however, the identity of cause and effect implies that force becomes materialized, thus completely shifting the problem to the material level, where it already had been placed by the materialistic Cartesian theory of impact. Indeed, ever since that time, this materialistic concept became the foundation of classical mechanics, as is shown by its paradigmatic principle that *force equals mass times acceleration.* Here, force is no longer a physical entity with an ontological status of its own, but only another expression for the rate of change of the material entity called *motion.*

2. In the question of Newton's force of inertia, nothing would be gained by showing that Newton merely gives the name of *force* to what was called *inertia* in classical mechanics. Galilean-Newtonian experimental philosphy requires that physical entities be quantified mathematically. This doctrine accepts only quantities, i.e. the measurable, as real and as a subject of exact science. To illustrate the point, Newton once again points to the mathematical realizability of the force of gravity as a sufficient proof of its reality, a proof which does not lose its validity even if the phenomenon of gravity itself is not understood physically.[18] Similarly, the force of inertia, if it is to be physically real, must have its mathematical measure. Now, this measure can be developed from the context of the *Principia,* with the Second Law of Motion pertaining to the external, motion-changing *vis impressa* serving as starting point.[19]

It is well known that scholars differ over the interpretation of this axiom in which Newton represents the external motion-changing force as proportional to the *mutatio motus* of the body. The controversy concerns the question as to whether *mutatio motus* is to be translated as *change of motion,* or whether Newton meant *the rate of* change of motion.[20] The wording of the Second Law as well as the contents of the commentary pertaining to it clearly favors the expression *change of motion* which could be translated into modern terminology by the expression $\Delta(mv)$, (were m is the mass, and v is the velocity of the moving body). This implies a finite change in the quantity of the measured motion (mv) . If so, the associated proportional force also takes on a finite quantity, somewhat analogously to the conventional notion of *impulse.*[21]

The controversy can be expanded to include yet another aspect: it concerns the *proportionality* (not equality) of force and change of motion stressed by Newton. Many scholars now feel that the force F should be considered to be proportional to the *mutatio motus* interpreted as *acceleration* a, whereby the quantity of matter or the mass m of the body would be the constant of proportionality. Ernst Mach, for instance,

thought that this formalistic approach could be used to define mass, because, as an opponent of the atomic theory, he mistrusted Newton's definition of mass.[22] Mach's interpretation, however, evidently fails because Newton utilizes neither the mere acceleration a nor the mere change of velocity Δv, but rather the change of *motion* (which is the quantity arising from the *product of mass and velocity,* our *momentum)* as the physical entity that is proportional to the force.

But if so, what then is the constant? Whether one symbolizes the *mutatio motus* by ma or by $\Delta(mv)$, the resulting *proportion* with a force F in any case presents the equations

either (with $F \propto ma$): $F = (ma)\cdot c$ (1)

or (with $F \propto \Delta(mv)$: $F = \Delta(mv)\cdot c$, (2)

both of which contain the hitherto unknown proportionality constant c.

This constant can under no circumstances simply be replaced by a "one" (without units) in order to remove it from the equation and perhaps obtain the formula $F = ma$.[23] For that would presuppose that the force F and its equivalent effect had identical measurements, or in other words, identical geometrical dimensions (units), which in itself would lead to the conclusion that they are one and the same.[24] In virtue of Leibniz's phrase *causa aequat effectum* his theory might appear to yield the identity between force as cause, and between change of motion as effect; but Newton's teachings completely forbid such an assumption. Here, Plato's principle of the *analogy* (not identity) of nature is valid.[25] But as to the principle of causality, this analogy of cause and effect means that, although nothing is in the effect that was not already contained in the cause (as Newton knows),[26] *one cannot reverse the sentence.* Cause and effect are entities of different measure, or *incommensurables.* Therefore, their mathematical relationship to each other cannot be identity, but only a proportionality.[27] Hence it follows that, the proportionality constant c being the quotient of these two entities of different measurement, it too has a mathematical measurement or dimension or units of its own. Therefore it cannot be removed from the equation without destroying the proportion. All in all, if seen from the Newtonian point of view, the *vis viva* controversy which led to quarrels among members of the learned world, starting with Leibniz's publication of 1686,[28] and continuing up to the supposed solution of the problem by d'Alembert in 1743 (upon which, however, Newton himself never commented), poses the question about the measurement of the constant c; knowledge of this constant yields knowledge of the measurement of the force *vis impressa.* The measurement of the *vis impressa* by the use of eq. (2) which is determined through the product of the change of motion $\Delta(mv)$ and the constant c, leads one to see also the equation of the condition of the *materiae vis in-*

sita. Indeed, if the uniform straight-line motion (mv) results from an interaction between *vis impressa* and *vis insita,* and its further cause after the completion of the effect of *vis impressa* is only the *vis insita* that maintains the motion,[29] then the measurement of this matter-implanted force, which from now on will be represented by the letter E, should be given by

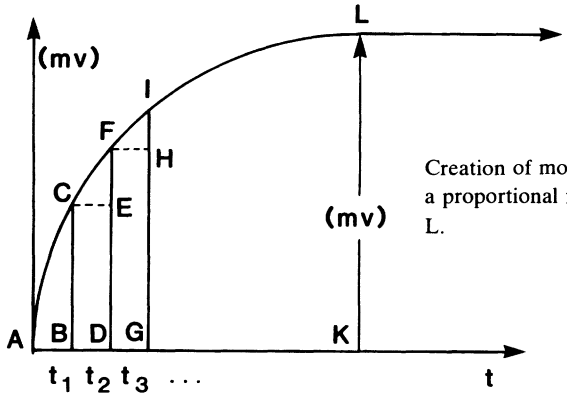
$$E = (mv) \cdot c \ . \qquad (3)$$

This mathematical-symbolic formulation reveals for the first time Newton's First Law of Motion in terms of a *causal* relation between motion (momentum) (mv) and the moving body's *vis insita* E. Everything else depends on the measurement or the dimension of the constant c.

3. Newton's concept of *vis impressa* as a *finite* quantity which is proportional to a *finite* change of motion developing as an event in a finite time, solves the puzzle. This concept clearly results from Lemma X of the "Method of first and last ratios of quantities" where Newton explicitly investigates the effect of a *vis finita.*[30] The development of the idea of a finite effect *(motion)* from a finite cause *(force)* as an event which takes place in time was an illuminating insight indeed. Everyone will concede that, when a billiard-ball is struck, it does not instantaneously gain the velocity of motion that is proportional to the striking force, but rather this will take a finite time because, as Galileo has demonstrated, the ball must first conquer all the smaller velocities. Nevertheless, classical mechanics offers no adequate description for this phenomenon. Instead, in virtue of the Leibnizian identity between *causa* and *effectus,* the effect *change of motion* should emerge instantaneously[31] from its "cause," that is, from the simultaneous and identical *impulse* of the same measurement. This identity of simultaneous, *functional* interrelated entities, which is the substructure of analytical geometry within the framework of Cartesian coordinates, yields no causal-creative, but a merely deterministic picture of nature. Here might perhaps be the deeper reason why Newton, but also Galileo and his disciple Evangelista Torricelli, preferred the *synthetic* geometry of the ancients. As a matter of fact, Albert Einstein's critique of the concept of simultaneousness in contemporary physics which led him to introduce an additional time-axis in the context of Special Relativity, might in some way represent a recovery of the virtues of *synthetic geometry.*

Let us then, for the moment, accept Newton's proposals and oppose Leibniz, and let us take it for granted that a finite force E yields a continually (i.e. in the course of time) developing motion, (mv), which is proportional to it. In this case, the event of the creation of this motion

will be characterized by a continual diminution of the increase of motion (or of the velocity of the creation of the motion) which starts from a maximum. The diagram below clarifies this description:



Creation of motion $mv = KL$ in time AK by a proportional force mvc decreasing from A to L.

The distances, BC, DF, GI represent the motions created during the times t_1, t_2, t_3, respectively; the distances BC > EF > HI represent the corresponding velocities of development (or the diminishing increase in the creation) of the motion. It is seen that, at the very beginning of the motion (mv) during the time interval t_1, the velocity of the development of motion, or the increase of motion, is equal to the velocity of the developed or created motion BC and has a maximum, inasmuch as it finally, when the motion in the time AK attains its size KL corresponding to the whole acting force, approaches zero. This first velocity, because it is a maximum, is apparently the same constant with different motions. This may be demonstrated by using Newton's "Method of first and last ratios of quantities" as follows: As the distances which bodies describe as an effect of some finite regular force are (according to Lemma X) proportional to the times squared at the very beginning of motion, so that the first velocities are proportional to the first times (distance : time squared = velocity : time), these first velocities during equal first times must be equal for different motions; henceforth they shall be represented by the symbol c.

Now this constant initial distance-time relationship c of the developing motion must obviously be seen as a factor of proportionality between the force E and the created motion (mv). This can be confirmed through a proportion given by Roger Cotes,[32] according to which the distances covered by a body at the very beginning of its motion[33] are proportional to the motive forces. In other words, these forces are to the distances as are (according to Newton's Lemma X) at the same time the first developed quantities of motion to the first times. This applies to the pro-

portion $E : l = (mv) : t$ (l is the initial distance, t is the initial time). From this statement, one can obtain a general relationship which no longer applies merely to the initial times (or to the very beginning of motion), but which, by transposing the proportion, unrestrictedly yields $E : (mv) = l : t$ or the proportionality factor of the relation of force E to created motion (mv) in accordance with Newton's Second Law of Motion. Indeed, in this case the creating forces are to the created motions as the initial distance l is to the initial time t; and with this constant ratio of the initial distance to the initial time, the emerging constant of proportionality is actually the initial developing velocity c established above, with dimension or units $[L/T]$. Since this argument rests upon Newton's concept of *absolute* (i.e. *real)* space and time, it should come as no surprise that his theory tacitly contains this absolute space-time relation c as a natural constant linking mathematics to physical reality.

Remarkably, the ratio $E : l = (mv) : t$ also provides the substructure of Leibniz's analytical theory of motion. If one solves for E and if it is assumed that the initial space-time relationship is not a constant quotient of absolutes, but a variable velocity v as a quotient of relative values, one obtains $E = mv^2$, which is the measurement of Leibniz's living force *vis viva,*[34] and within this relative frame of reference, the constant c vanishes. This method generalizes the proportion which Newton strictly confines to the very beginning of motion, and thus it indicates a linear increase in motion, or constant acceleration, as the essence of an analysis which is in flagrant contradiction to Newton's approach.[35]

III. Newton's Inertial Force and Modern Physics

Since the dimension of the constant c is known, eq. (3) which defines the inertial force is completely resolved. One should be able to equate this *Newtonian Constant*[36] with the constant c which dominates modern physics as the vacuum-velocity of light. In a consistent quantitative theory, the dimension of a physical entity characterizes its identity, and entities equal in dimension are identical in character.[37] Here this identity of character is confirmed since both constants are absolute maximum velocities. By virtue of this, there appears an exciting connection between Newton's inertial force and the elementary conceptions of modern physics, for the mathematical combination of the quantity of motion or the *momentum p* through the constant c with another variable E which in this case is called *energy,* is, for instance, to be found in the Photon Theory of Quantum Physics. And the above-mentioned measure $(mv)\cdot c$ of the inertial force E takes on the explicit form in the well-known non-relativist derivation of

Einstein's mass-energy proportion.[38]

Our $E = (mv)c$ also allows for a physical interpretation of the inertial force. Apparently it is an immaterial phenomenon that is associated with uniform straight-line motion proportional to that motion's measure (mv), namely the *force of unresisted motion* which in this case is not understood as the body's capacity for work or its *kinetic energy,* but rather as the real *cause* of motion, "the force of a body's motion" in the sense of Samuel Clarke who demonstrated that the measure of this force must stand in a linear relationship to the velocity of motion, and that it can by no means be given through the Leibnizian mv^2.[39]

Whoever seeks experimental proof for this real physical phenomenon must remember that the force of inertia appears as an external action only when a body undergoes *changes* of motion (including changes of direction due to its vector character). Modern physics actually provides this experimental evidence through the diffraction phenomena which are produced by a beam of electrons that passes through a diaphragm in the screen behind it (the beam being diffracted at the edges of the diaphragm), and which can only be related to immaterial wave phenomena. The dualism of waves and particles, a concept which has given rise to so much confusion and unsatisfactory speculation in both the physics and philosophy of the 20th-century, turns out to be evidence supporting the Newtonian idea of the reality of immaterial, motion-accompanying phenomena or *spirits,* which are called *matter waves* or *de Broglie waves* in modern physics, and are mathematically described through the equation $E = h \cdot \nu$ (h = Planck's constant, ν = frequency).[40] Endowed throughout with this spiritual force "energy," the universe (which the adherents of pure reason had conceived to be lifeless),[41] *lives again.*[42]

John Maynard Keynes, who in the 1930's studied unpublished papers from Newton's estate has said that Newton was certainly not the first harbinger of the mechanical age, but rather the last of the magicians.[43] Thus Newton could be perceived as a link in the chain that may have begun with Hermes Trismegistos who is considered to have brought man numbers, letters, science and art. It appears certain that Newton's legacy is not the deterministic godless mechanics of, for instance, Pierre Simon Delaplace. Rather, it is still waiting to be redeemed, and, as a daughter of time, it might perhaps construct the arch of western thought linking Plato to modern physics, and thus become the foundation for a realistic understanding of what occurs in quantum mechanics.[44]

Chiemsee, West Germany

NOTES

1. Cf. Eduard Jan Dijksterhuis, De mechanisering van het wereldbeeld, Amsterdam 1950; German trans. by H.Habicht, Die Mechanisierung des Weltbildes, Berlin 1956; reprint Berlin 1983, p.3.
2. Isaac Newton, Philosophiae Naturalis Principia Mathematica, in: Isaaci Newtoni Opera quae Exstant Omnia, Samuel Horsley ed., London 1779-1785; German trans.: Mathematische Prinzipien der Naturlehre, J.Ph. Wolfers ed., Berlin 1872; Mathematische Grundlagen der Naturphilosophie, Ed Dellian ed., Hamburg 1987, in preparation.
3. S.Horsley ed., ibid. Vol.II p.6-12.
4. George Berkeley, Schriften über die Grundlagen der Mathematik und Physik, W. Breidert ed., Frankfurt a.M. 1985 = Suhrkamp Taschenbuch Wissenschaft No. 496, p. 210-243; Ernst Mach, Die Mechanik in ihrer Entwicklung, repr. Frankfurt a.M. 1982, p. V.
5. Cf. Max Jammer, Concepts of Force, Cambridge/Mass., 1957, p.166.
6. Immanuel Kant, Metaphysische Anfangsgründe der Naturwissenschaft, in: Kant's gesammelte Schriften, Berlin 1911, Vol.IV p.468, 543.
7. This is still an open problem in quantum physics; cf. Gino Tarozzi/A. van der Merwe eds., Open Questions in Quantum Physics, Dordrecht 1985.
8. Cf. Roger Cotes's preface to the Second Edition of the Principia, London 1713, in S.Horsley ed., op. cit. Vol.II p.XIII-XXV.
9. Principia, definition 3; cf. S. Horsley ibid. p. 2. The problem that is discussed in this paper may have started in a sense from Andrew Motte's translating of *impetus* as impulse in the *Principia*'s first English edition in 1729, thus cutting the connection between Newton and the *impetus* theory (Galileo, Leonardo da Vinci, Buridan).
10. Also Wolfgang Breidert ed., op.cit. p.48-55, and Max Jammer, Der Begriff der Masse in der Physik, Darmstadt 1981, p. 74, 75.
11. C.F. von Weizsäcker, Aufbau der Physik, München 1985, p.234,243-45.
12. Cf. The banishment of the causa-effectus relation from mechanics ("Cet unique axiome vague & obscur") by Jean le Rond d'Alembert, Traité de Dynamique, Paris 1758, Préliminaire, p. XI-XII.
13. Samuel Clarke, A Demonstration of the Being and Attributes of God, London 1705.
14. Principia, definitions 3 and 4 with commentaries.
15. C.f. B.J.T. Dobbs, The Foundations of Newton's Alchemy, Cambridge 1983, p.193; see also W.Windelband, Lehrbuch der Geschichte der Philosophie, H.Heimsoeth ed., Tübingen 1980, p.343-365 (§ 31 "Substanz und Kausalität").
16. The phrase *causa aequat effectum* is actually a product of Leibniz's own; cf. H.J. Heß, Die unveröffentlichen naturwissenschaftlichen und technischen Arbeiten von Leibniz; Studia Leibnitiana Suppl. Vol. 17 (1978), p.183-205.
17. Ibid. p.203.
18. Cf. the Scholium Generale at the end of the Principia's book III (in S. Horsley op.cit. Vol. III, p. 174: "Et satis est quod Gravitas revera existat et agat secundum leges a nobis expositas ...").

19. Ibid. Vol. II p. 14. The axiom of the proportionality of cause and effect can be found in a little alterated formulation for instance in Jacques Rohault, Traité de Physique, Paris 1671, chapter V section 6-10; this book appeared in London in 1682 in a Latin translation, and was translated into English by John and Samuel Clarke in 1723.
20. Cf. Max Jammer, op.cit. (ref. 5) p. 130/1; Brian D. Ellis, Newton's Concept of Motive Force, Journ. Hist. Ideas (23) 1962, p. 273; I. Bernard Cohen, The Newtonian Revolution, Cambridge 1980, p. 172; Richard S. Westfall, Force in Newton's Physics, London 1972, p. 472; Werner Kutschmann, Die Newtonsche Kraft, Wiesbaden 1983 = Studia Leibnitiana Sonderheft 12; E.J. Dijksterhuis, op.cit. p. 525.
21. Cf. especially Werner Kutschmann, ibid. p. 35.
22. Ernst Mach, op.cit. p. 210.
23. But in this way it is treated by, for instance, Jürgen Mittelstrass, Neuzeit und Aufklärung, Berlin 1970, p. 288; Steven Weinberg, Teile des Unteilbaren, Heidelberg 1984, p. 139, and Brockhaus Enzykopädie 1970 under "Kraft."
24. Cf. Max Jammer, The Philosophy of Quantum Mechanics, New York 1974, p. 54: "The view that a formal identity between mathematical relations betrays the identity of the physical entities involved ... harmonizes with the spirit of modern physics. ... Physical entities which satisfy identical formalisms have to be regarded as identical themselves ..."
25. Cf. Principia book III, regula philosophandi No. 3 and commentary; in S. Horsley, op.cit. Vol. III p. 2-3; James E. McGuire, Atoms and the Analogy of Nature: Newton's third Rule of Philosophizing; Hist. Phil. Sci. I No. 1 (1970) p. 1.
26. Cf. James E. McGuire and Martin Tamny, Certain Philosophical Questions, Newton's Trinity Notebook, Cambridge 1983, p. 134, 135.
27. Cf. Principia book I, Scholium after Lemma X (S. Horsley, op.cit. Vol. III p. 36), "Si quantitates indeterminatae *diversorum generum* conferanter inter se ..." (my italics); cf. also John Wallis, Mechanica, London 1670, Proposition VII.
28. G.W. Leibniz, Brevis demonstratio erroris memorabilis Cartesii et aliorum ...., Acta Eruditorum, March 1686.
29. Cf. Principia, Newton's commentary to definition 4.
30. Cf. Principia in S. Horsley, op.cit. Vol. III p. 30,36.
31. So for instance Max Born, Die Relativitätstheorie Einsteins, Berlin 1984, p. 27; see also I. Bernard Cohen, Newton's Second Law and the Concept of Force in the Principia, in Robert Palter ed., The Annus Mirabilis of Sir Isaac Newton 1666-1966, Cambridge/Mass. 1970, p. 143.
32. Roger Cotes, Preface to the Principia's Second Edition of 1713, in S. Horsley ed., op.cit. Vol. III p. XVI.
33. At the very beginning of the motion = "sub ipso motus initio," as Cotes says, corresponding to Newton's own formulation in the Principia, Lemma X.
34. This concept corresponds to the *kinetic energy* of analytical mechanics, expressed by $mv^2/2$. The missing factor $1/2$ was arbitrarily added only in 1829 by G. Coriolis because of better usage for integrations; see Max Jammer, Concepts of Force, op. cit. p. 166 footnote 12.

35. Newton himself called the Cartesian-Leibnizian method the "analysis of bunglers"; see Richard S. Westfall, Never at Rest, A Biography of Sir Isaac Newton, Cambridge 1980, p. 380.
36. Cf. Ed Dellian, Die Newtonische Konstante, Philosophia Naturalis (22) No. 3 (1985) p. 400; and: Experimental Philosophy Reappraised, Speculations in Science and Technology (9) No. 2 (1986) p. 135.
37. Cf. footnote 24 above.
38. Cf. Ed Dellian, On Cause and Effect in Quantum Physics, Speculations in Science and Technology, in print.
39. Samuel Clarke, A letter to Mr. Benjamin Hoadly F.R.S., Philosophical Transactions Vo. 35 (1727-1728), p. 381.
40. The connection of this equation with our mathematical expression for the equivalent inertial force could contribute to a mastering of the formal problems of modern physics, since the eq. $E = (mv)c$ can very easily be demonstrated to be a desideratum in the foundation of quantum physics. Cf. also footnote 38.
41. Cf. Carolyn Merchant, The Death of Nature, German: Der Tod der Natur, München 1987.
42. B.J.T. Dobbs, op.cit. p. 212: "The universe lived again as Newton's thought swung on towards the *Principia* in the 1680's, for forces and active principles were everywhere."
43. Cf. B.J.T. Dobbs, op.cit. p. 13.
44. More thoughtful physicists concede that such an understanding of modern physics is still missing. Cf. for instance Murray Gell-Mann (Nobel Price 1969), whom I. Bernard Cohen quotes as follows: "All of modern physics is governed by that magnificent and thoroughly confusing discipline called quantum mechanics, invented more than fifty years ago... Nobody understands it, but we all know how to use it and how to apply it to problems; and so we have learned to live with the fact that nobody can understand it." (The Newtonian Revolution, op.cit. p. 147).

ANTONINO DRAGO

A CHARACTERIZATION OF THE NEWTONIAN PARADIGM

1. *The choice of the kind of mathematics.*

In previous papers,[1] I successfully introduced constructive mathematics into theoretical physics. Unlike classical mathematics, (Cauchy – Weierstrass - Dedekind) constructive mathematics does not make use of actual infinity. It extends rational numbers to a denumerable field of real numbers, which, although more restricted than the field of classical mathematics, includes all common real numbers, and moreover introduces some subtle differences which are extremely relevant for a student of the foundations of physics. Historically, constructive mathematics did not gain the attention of mathematicians until 1967[2], although its formal beginnings date from the first years of our century. It substantially fulfills Duhem's wish for an "approximation mathematics," Poincaré's idea of "physical continuum," Weyl's call for that "elementary mathematics" which is useful only to physics, and Bridgman's hints at an "operationalist mathematics."

Therefore, when constructing a theory of physics, a theoretical physicist may choose between two different mathematics. This was also the case for past theoretical physicists. For example, by introducing infinitesimal analysis in physics, Newton chose actual infinity so decisively that the number field he used was actually more extended than the real field of classical mathematics, thus approaching the modern hyper-real field of non-standard analysis.[3]

2. *The choice of the kind of theoretical organization.*

Furthermore, 40 years ago, E.W. Beth remarked that our scientific theories are organized by following one single model which had been suggested by Aristotle in his *Analytica Posteriora* and realized by Euclid's *Elements*.[4] Although Beth called for an alternative model, he was unable to find one. Nevertheless, my historical investigations of the scientific theories which were developed around the time of the French revolution, led to the identification of a number of theories which certainly do not follow an Aristotelian organization. These are: Lazare Carnot's mechanics (1782), L. Carnot's geometry (1803), classical chemistry, S. Carnot's thermodynamics (1824) and Lobachevsky's geometry (1840).[5] Each of

P. B. Scheurer and G. Debrock (eds.), Newton's Scientific and Philosophical Legacy, 239–252.
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these theories focuses upon one central problem; then it offers a method for solving any related problem. In the past an organizational model of that type had been rejected as being a typical engineering method, but now it is recognized that many well-known theories are organized in this manner and that these theories yielded theoretical results of a very high level.[6] Thus, a theoretical physicist must not only choose his mathematics, but he must also choose a particular type of organization of his theory. Newton too made this latter choice. His mechanics followed the Aristotelian model of organization and his work on optics represents a large attempt at organizing the laws of optics in the Aristotelian manner.[7]

3. *How much Newton was aware of such two choices?*

In the period preceding Newton's work there had been a very strong debate about the foundations of physics. I shall mention but two main participants in this debate: Galilei and Descartes.

After almost forty years of meditation, Galilei wrote his last two books in dialogue form, i.e. in a Platonic style which was the opposite of the Aristotelian style so familiar to him, as is testified by his commentary on Aristotle's *Analytica Posteriora.*[8] Indeed, he opposed the Aristotelian philosophy and its adepts in his time, particularly by supporting experimental evidence instead of the self-evident principles of Aristotle's scientific theories. The first of the two above-mentioned books *(Dialogo)* concerns the organization of the universe as the materialization of the organization of the entire universe, including the intellectual world;[9] its subject was the question which of two possible organizations of the world is the true one (Galilei's answer was not entirely definite). On the other hand, in his other book *(Discorsi),* Galilei spent a great deal of time unsuccessfully discussing the choice between actual and potential infinity. Thus Galilei was deeply concerned with two options.

Descartes too was aware of two options. In fact, he so resolutely opted for the Aristotelian model of organization that he deprecated Galileo's work as lacking "certain principles." Moreover, he was always opposed to the use of actual infinity in mathematics and physics.[10]

Consequently, we can be certain that Newton, when he constructed his theories, consciously opposed the choice made by previous scientists. Moreover, during his entire life, Newton was troubled by his optical theory which could not be submitted to the Aristotelian model of organization. When Newton finally published his *Opticks,* he included 31 problems concerning the foundation of his science, thereby showing how ineffective his organizational scheme really was.

4. Berkeley's critique (the "double fault").

Inasmuch as the two options are constitutive of physical theory, they must be considered as a part of physics. But the problem of these options is also philosophical inasmuch as the options are not subject to proof or disproof by experiment. Indeed, in the end, the issue at stake must adequately be treated by philosophers. Thus, Berkeley addressed an extremely pertinent criticism[11] to the option of actual infinity the use of which he opposed on the basis of the human incapacity to deal with anything but the denumerable. Although he did recognize the value of infinitesimal calculus in yielding exact results, he opposed it on philosophical grounds. "Berkeley's Lemma" states that no argument is valid if the content of the premise on which the argument is based changes in the middle of the argument. Now, as a matter of fact, the "Lemma" applies to Newton's infinitesimal calculus. At the beginning, Newton establishes that $dx \neq 0$, but then the calculus suppresses the infinitesimals of higher orders than dx, as if they were zero. The latter "fault" compensates for the former to generate correct results. Such criticism was very popular among those scientists (such as Lagrange, L. Carnot) who opposed Newton's views. It would take a long time to examine the deep implications of the choice of mathematics made by Newton (see Appendix). Within the context of this paper whose aim it is to characterize the Newtonian paradigm, it will suffice to examine the implications of Newton's choice of organization.

5. The "double fault" in theoretical physics.

To my knowledge, no criticism of the Aristotelian organization of Newton's *Principia* has ever been published. But Berkeley's criticism may in fact also be applied to this option of Newton. Let us inspect Newton's mechanics.

Its first principle postulates a situation (the equivalence between rest and uniform, rectilinear motion) which could be realized only if one were able to exclude all the effects of friction – a very idealized situation which may be compared with the petition of a dx as a non-zero infinitesimal. However, in the subsequent development of the mechanics, the equation of motion of any real body takes into account friction by means of a macroscopic force. This is a second mistake because, as a theory of isolated mass-points, mechanics could not be applicable to a macroscopic, collective phenomenon. Yet, in a curious way, it can both account for the real facts and yield the correct results. It should be noted here that the dou-

ble fault arises in an order which is the reverse of the order of calculus: while in infinitesimal calculus one creates dx and then suppresses its consequences, in mechanics one first suppresses the effects of friction only to reintroduce them later on. That Newton seems to have been aware of this doubt appears from the manner in which his version of the first principle is formulated. He incorrectly mentions force before defining it (only the second principle actually tells us what force and its effects are). Why? Force is mentioned in order to temper the all too idealized and therefore unrealistic situation described in the first part of the statement. The actual content of the first principle did not require the accelerated motion to be taken into account except in respect of that which the reader must at once be aware of: friction.

Due to the double fault, the first principle of Newton's mechanics includes both the idealized situation and its material correction. Such double fault, however, can be seen in other theories of classical physics. Electromagnetism as formulated in Maxwell's equation does not take into account entropy which is introduced as a correction of previous calculations ("physical realizability"). Thermodynamics too follows the same pattern, even though it is the science of heat, i.e. of the consequences of friction (and of chemical processes). Its first principle states an idealized situation: from $\Delta U = L + Q$ it follows that a quantity of heat possibly converts into a same quantity of work. Then, the second principle (a "mistake" from the point of view of the first one) imposes realistic restraints $\nu < 1$).

Now one can generalize. The double fault is actually the main feature of an Aristotelian theory. Its self-evident principles cannot represent a real process but only an idealized fiction; the second fault which is usually the second principle then redresses the first one in order to obtain correct results. It may be concluded that, while the reduced power of abstraction is a flaw inherent to the problematic way of organizing a theory, the double fault is the flaw inherent to the Aristotelian model of organization. Neither model of organization has an advantage over the other.

6. *The rooting of Newtonian paradigm.*

The "Newtonian paradigm" stood unrecognized for several centuries while the Aristotelian option was so deeply ingrained in the scientific community of physicists that they substantiated it in the form of a physical magnitude, viz. space. It can be shown that, in the work of the founders of physical theories, there exists a sharp correlation between the

choice of the Aristotelian model of organization and their use of the concept of absolute space (Descartes, Newton, Euler, Maxwell, Hertz, Boltzmann), and between the choice of the problematic model of organization and the use of relational space (Lagrange, L. Carnot, S. Carnot, the chemists, Faraday, Einstein).[12] Indeed, the Newtonian paradigm was contested by those theories which followed the problematic model and which were mentioned in section 1. The scientific community, however, either rejected these theories as too practical (L. Carnot's mechanics, classical chemistry) or judged them to be too difficult (the theories of S. Carnot and Lobachevsky).[13] Then, in the second half of the nineteenth century, a sizeable group of energetists actively opposed Newtonian mechanics as less basic for theoretical physics than thermodynamics. Unfortunately, their criticisms regarded a few physical concepts, and in particular, the concept of force. In order to favorably compare energy with the metaphysical concept of mechanical force, the energetists also featured the former concept as a metaphysical notion. Moreover, they based their scientific attitude on an Aristotelian formulation of thermodynamics – Clausius-Kelvin's thermodynamics – which presents the concept of energy in terms of a self-evident principle. Thus their effort to radically change the philosophy of physics was doomed to fail. Theretical physicists therefore confirmed the Newtonian paradigm as the only possible one.

7. *The crisis of the Newtonian paradigm.*

In the end, the Newtonian paradigm was shaken by the aether crisis, i.e. by the crisis of the surreptitious concept of space, which had arisen from the Aristotelian option.

In their search for a solution, physicists changed their basic concepts without being aware of the basic options inherent to the Newtonian paradigm. Nevertheless, the crisis forced them to change their paradigm. It so happened that classical mechanics was overwhelmed by a "problematic" theory, viz. the theory of special relativity which centers on the problem of finding covariant laws.

But, as has been mentioned in section 1, the Newtonian paradigm also included the choice of a particular mathematics. This choice was also substantiated by theoretical physicists in the form of their hypothesis of the continuity of all physical magnitudes (and, primarily, of time). However, eventually the quanta would oblige physicists to take discontinuities into account.

Thus a new theory was suggested: quantum mechanics, which radically differs from the Newtonian paradigm, at least within the permissible

realm of physical magnitudes.

In the end, two theories came to supersede the Newtonian paradigm. However, not one theorist deliberately changed Newton's basic options (with the exception of the advocates of a discrete space and time – a first rough approximation of a physical theory based on constructive mathematics). Of course, the two new theories appeared to radically differ from each other. As a result, the crisis in the foundations of physics is still very much alive. Its longevity clearly shows that its solution will require a novel point of view.

8. *Models of scientific theory, incommensurability.*

All the above provides evidence for the claim that the two basic options as described in sections 1 and 2 effectively characterize the main features as well as the main consequences of what is broadly called the "Newtonian paradigm."

These two options which are of interest to both the physicist and the philosopher constitute the most relevant and effective foundations of a physical theory.[14] Previous papers have supported this claim with much more elaborate historical evidence.[15] I have called the result of two such options a *model of scientific theory* and I have suggested that any two such models clarify what today is meant by *two incommensurable theories.*[16] Thus the entire historic period of modern science until now (1600-1980) may be characterized as a scientific development whose protagonists did not know what two incommensurable theories are, very much in the same way that Greek science was unaware of what two incommensurable numbers are. The Greeks, however, were able to formulate the problem and to somehow deal with it, whereas Western science, particularly as it was under the sway of Newton's authority, never formulated the problem except for the recent intuitive grasp of a prominent historian of science.[17]

9. *Radical variance of the meaning of concepts.*

The great difficulties encountered by a theoretical physicist, should he try to avoid the Newtonian model of scientific theory, may be illustrated by the radical variance of the meaning of concepts pertaining to two incommensurable theories (in section 6 the variance of the concept of space was already pointed out).

Here I shall summarize the extensive discussion that really ought to take place,[18] by means of an example pertaining to some theories which might represent the opposing models: the Newtonian model on the one hand, and L. Carnot's mechanics and S. Carnot's thermodynamics on the other (See Table 1). It should be easy to grasp the tremendous gap between the concepts at variance in the above theories. This gap was hardly noticed perhaps on account of the fact that L. Carnot's mechanics had not been recognized as being a very high level theory,[19] while now it is evident that, together with S. Carnot's thermodynamics, it represents a complete switch in scientific conception.

Newton	L.Carnot	S.Carnot
Principles	Main Problem	idem
Absolute space	Relational space	idem
Infinite space	Finite space	idem
Absolute time	Implicitly without time	idem
Isolated mass-point	Extended body, machine	idem
Motion as a property of a body	Communication of motions	Transformations suffered by a system
Acceleration	Work, velocity, interactions	Work
Idealized force	Energy	idem
$f = ma$	Shock's laws (energy conservation)	$PV = Nt$
Differential equations	Geometrical motions	Reversible transformations
All motions $(-\infty \leqslant t \leqslant +\infty)$	Limitations to the possible operations	idem
Machine as application of theory	Machine as universal subject of the theory	idem
Machine as generating unlimited power	Machine as generating a pluralism of options	idem
Perpetual motion	No perpetual motion	idem

Table 1

All this suggests that the history of physics is actually tripartite. It is i) an *objective history,* obtained by the scientific community by deleting all reference to the two options and to conflicts between incommensurable theories; ii) *a subjective history,* experienced by the individual scientist (and a few of his scientific friends), as the history of a scientific program of research operating by neutral techniques but appealing to metaphysical values; iii) *an effective* history, i.e. the history of the evolution, contestation, suppression etc., of the four models of development such as

they are actualized at a given time by the existing theories.

Some scientists may in fact unite subjective science with effective science; no one did it more than Newton did.[20] In the subsequent history of physics theoretical crises were so dramatic precisely because Newton strictly linked physics with metaphysics in a synthesis which seemed eternal and which obscured both the basic choices and the origin of the basic concepts.

10. Comparability between two incommensurable theories.

Today we are able to compare these different histories and their incommensurable theories because a "Gestalt phenomenon" has manifested itself: mathematical logic from the intellectual point of view, constructive mathematicians from the operative point of view and computer science from the social point of view have accorded to constructive mathematics the same theoretical dignity as to classical or non-standard mathematics. Consequently, the historical analysis could identify some theories that required no more than constructive mathematics as its own mathematics. Some of these prominent theories displayed their non-Aristotelian organization and thus, the two alternative basic options. These options constitute a model of scientific theory that is incommensurable with Newton' model.

In the past the scientific community, unaware of the pluralism of models of scientific theories, was unable to correctly compare two rival formulations of the same physical theory. It felt compelled to assess them as producing a case of inconvenient ambiguity or, worse, a case of contradictory opposition, both cases thereby requiring a decision as quickly as possible by means of an *experimentum crucis*. Yet, some scientists did manage to make correct comparisons. L. Carnot, no less, for instance, compared Newtonian mechanics with his own mechanics. In the last edition (1803)[21] of his book he devoted some pages (XI-XVII) to an equable comparison of Newtonian mechanics with his own formulation (although modified by the addition of seven principles). His conclusion was that any formulation presents some merits and some defects. A similar historical case is that of Lobachevsky. After editing his theory in a fully Aristotelian manner[22] he published a little book, recommanded by Gauss, in which he formulated as the central problem the number of parallels.[23] Finally, we might hazard the guess that even Newton came close to being capable of making a similar comparison when he edited the *Opticks*. The exposition of his theory was done after the Aristotelian manner, but the central part of the book expands upon experimental optics and, in the end, in the last part he formulates the previously mentioned 31 Queries. If one attempts to bring them under one heading, one finds it in the first Query: the action between light and bodies (including

the human body). Perhaps the *Opticks* is like a two-faced coin: Aristotelian on one side, problematic on the other.

APPENDIX: Newton's choice of mathematics and its alternative

In order to more precisely illustrate the implications of Newton's choice of theoretical physics, let us memorate that the three kinds of mathematics involved, viz. non-standard analysis, "rigorous" calculus, and constructive analysis, present three different interpretations of the concept of point. A point in non-standard analysis in general is an infinitesimal, in "rigorous" analysis it is a classical real point (but it should be noted that it is possible to pick out with exactness a single point from the continuum) and in constructive analysis one is restricted to the use of intervals only.[24] As a result, any physical concept whose definition derives either from the concept of point or from the concept of limit will be different in meaning. For instance, the Newtonian first principle of dynamics requires that we be able to decide when the force in space is exactly zero (an approximately zero force may yield unforeseeable effects, since first principles do not state its effects; these will be known by means of the second principles). In constructive mathematics, the above mentioned problem is undecidable. It appears that, among the many past versions of the first principle, L. Carnot's version agrees with such mathematics.[25]

Other examples of differences in the meaning of concepts are offered by the limit-properties which Newton imposed upon matter: the case of "hard body" (a body which is so rigid that it does not bounce at all when subject to shock) is well-known to historians of physics because the entire development of theoretical mechanics in the eighteenth century was befuddled by this odd concept. An other example is the case of aether, a concept whose features were obtained as limit-properties of the ideal concept of space. A successful example is the idea of an infinitesimal point-matter, i.e. an atom; but the experimental proof of its existence was obtained only two centuries after Newton's suggestion.

Let us now turn to experimental laws expressed by means of mathematical functions.

Evidently, any one of the three mathematics gives the same representation of an experimental law. Yet they eventually do differ in some mathematical functions belonging to theoretical physics. Some functions may exist in classical analysis while they do not exist in constructive analysis. The Dirichlet function provides a good example. In constructive mathematics, it can not be decided whether any given x is either a rational or an irrational number; therefore such function cannot be defined.

Are there examples of similar functions in theoretical physics? If there are, they must be defined by means of some property which cannot be decided by constructive mathematics. Now, generally speaking, actual infinity is one such property. Are there any Newtonian functions that are essentially based upon actual infinity? Clearly, absolute space and time are of this nature. Here one ought to remind oneself of Newton's impressive words whereby he defines these magnitudes in opposition to the experimental, vulgar magnitudes. More than two centuries later, the dramatic crisis of the concept of aether led Einstein to abandon such undecidable – and thus, inoperative – functions regarding any spatial, resp. temporal, instant.[26]

A further example regards the concept of reversible transformation. It is well-known that there exist several definitions of this concept.[27] Yet all these lack clarity because they all rely upon the "quasi-static" process. This odd idea (Is the process static or not?) is quite parallel to the infinitesimal (a zero which is not zero; it "tends to zero") was necessary in order to disregard during a dynamics process any secondary effect with respect of the instant values of some physical magnitudes.

Non-standard analysis presents no difficulty: a quasi-static process is a function defined upon hyper-real values. But "rigorous analysis" bans infinitesimals and must refer reversibility to a single point. But by doing so there is no longer a process and thus, no secondary effect can be disregarded without referring the concept of reversibility to the entire transformation which in turn would entail that all secondary effects must be taken into account, an unlimited task indeed. In order to escape from such difficulties, one does after all invoke an infinitesimal, albeit disguised as physical concept in the form of the "quasi-static" process. Constructive mathematics, for its part, rightly refers the concept of reversibiilty to an interval approximating the state of the system. Here, the reversibility of the transformation can be tested by going forwards and backwards, and one can decide which secondary effects are negligeable.

Let us now inspect the formulations of a physical theory by means of differential equations.

The three kinds of mathematics differ with regard to any differential equation, but the differences are particularly striking in the case of the problem of the exact differential. Today it is known that only non-standard analysis allows for infinitesimals and differentials. Inasmuch as it was ignorant of any theorem of impossibility, the original project of infinitesimal analysis was to obtain hard results – in our case, the primitive function of a differential form – by a clever use of infinitesimal teachniques. "Rigorous" mathematics, on the other hand, bans all infinitesimals and differentials; however, it does not renounce the original project of infinitesimal analysis, and its textbooks unscrupulously include the above mentioned problem as the problem of the "exact differential." Finally, constructive mathematics gives no meaning at all to differentials. Here, the problem of exact differential is changed into the problem of studying when an integral over any closed path is equal to zero. Therefore, the definition itself of the problem has changed.

But even the results are different. In constructive mathematics one can easily show that the problem is undecidable in general.[28] Now, many formulations of foundations of classical thermodynamics are of this type. By its very nature, constructive mathematics does not admit of these, while they are perfectly legitimate in "rigorous" mathematics. On the other hand, the oldest formulation of thermodynamics, as found in the S. Carnot-Clausius-Kelvin formulation, is considered adequate by constructive mathematics.

A similar result in theoretical physics can be obtained by scrutinizing the Newtonian differential equation for classical mechanics. Here, both non-standard analysis and "rigorous" mathematics incessantly look for new techniques to solve the hardest problems. But constructive mathematics simply states that such differential equation is decidable only in particular cases. Indeed, the differential equation $dx/dt = f(x, \dot{x}, t)$ can be solved generally only provided f is uniformly continuous and provided it satisfies the Lipschitz condition – i.e. conditions more restrictive than the classical ones.[29] Thus, a differential equation can no longer support an universalistic principle. Constructive mathematics therefore denies almost all of the traditional formulations of classical mechanics; the only theory in

generally only provided f is uniformly continuous and provided it satisfies the Lipschitz condition – i.e. conditions more restrictive than the classical ones.[29] Thus, a differential equation can no longer support an universalistic principle. Constructive mathematics therefore denies almost all of the traditional formulations of classical mechanics; the only theory in agreement with constructive mathematics is the mechanics of Lazare Carnot which is essentially aimed at solving the problem of the law of the conservation of energy in terms of velocity only (its basic problem is the problem of shock).

In conclusion, the differences presented by the three types of mathematics yield three different ways of formulating a physical theory. Conversely, a particular formulation of a physical theory possibly agrees with one kind of mathematics only. The theoretical physicist should be warned that his efforts to build a theory may encounter truly unsurmountable difficulties; contrary to what Newton's marvelous work led us to believe, one should no longer think that any problem can sooner or later be solved.

University of Naples, Italy

NOTES

1. A. Drago, Constructive mathematics and Carathéodory's thermodynamics, *Lett. N. Cimento 34* (1982) 52-56.
   A. Drago, Dimensional Analysis and Constructive Mathematics, *Lett.N.Cim.37* (1983) 409-12.
   A. Drago, Relevance of Constructive Mathematics, to Theoretical Physics, in E. Agazzi et al. (eds.), Logica e filosofia della scienza, *oggi,* CLUEB, Bologna, 1986, vol. II, 267-272.
2. E. Bishop, Foundations of Constructive Mathematics, McGraw-Hill, New York, 1967. For introductory literature, see: D.S. Bridges, F. Richman, What is constructive Mathematics? *Math. Intell. 6* (1984) 29-38.
3. A. Robinson, Non-standard Analysis, North-Holland, Amsterdam, 1960, ch.X.
4. E. Beth, Natuurphilosophie, Görinchen, 1948, and The Foundations of Mathematics, Harper, New York, 1970, ch. I, § 2.
5. N.I. Lobatchevsky, Geometrische Untersuchungen zur Theorie der Parallel-linien, Berlin, 1840 (Engl. transl. by Halsted, Austin, 1893). Geometry is included in the list because since Gödels theorem one may question the Hilbert's view; actually, Lobachevsky meant geometry as a theory about real bodies. For the remainder of the theories, see: A. Drago, C. Mordillo, Perchè l'ordine logico dei principi della termodinamica inverte l'ordine di scoperta storica?, Atti IV Congr.Naz.St.Fisica, Como, 1983, 113-121.
6. G. Berkeley first advocated an "instrumentalistic" view of scientific theories. Many authors claimed an organization opposed to the Aristotelian one: Nagel speaks of "instrumentalist theories," Hintikka of "goal-oriented process," Lakatos of "empiricist theories." The best illustration of the two organizations of a theory is sketched by A. Rapoport in R.L. Ackoff, Scientific Method, Wiley, 1962, pp. vii-viii.

choice between actual infinity and potential infinity contributed to fashion the scientific theories which different scientists would offer. The second premise is a consequence of giving historical relevance to the Jacobin program of recasting the foundation of all science (C.C. Gillispie, The *Encyclopédie* and the Jacobin Philosophy of Science: a Study in Ideas and Consequences, in M. Clagett (ed.), Critical Problems in History of Science, U. Wisconsin P., London, 1969, 255-289) and/or to the "odd" way in which S. Carnot and L. Carnot built their theories: L. Carnot's geometry and mechanics, and S. Carnot's thermodynamics present a problematic organization. Together with modern chemistry and with L. Carnot's new foundations of differential calculus – both of which are theories with problematic organization – constitute a new foundation for the whole of contemporary science. All in all, an accurate historical investigation of both the birth of modern science and the philosphy of science during the French Revolution can make apparent the two basic options of a physical theory. The two premises mentioned clearly show the few stereotyped statements which Koyré uses again and again to characterize the passage from ancient to modern science (A. Drago, La storia del concetto di spazio come rivelatrice delle due scelte fondamentali di una teoria fysica. II, A. Koyré e la metafysica della scienza moderna, in F. Bevilacqua (ed.), Atti VII Congr.Naz.St.Fisica, Padova (1986), 1987, 119-123) as well as Galilei's historical case (A. Coppola, A. Drago: Galilei ha inventato il principio d'inerzia?, in S. D'Agostino, S. Petruccioli (eds.), Atti V Congr.Naz.St.Fisica, roma, 1985, 319-327). In the study of the historical development of modern science, the next most relevant problem facing the historian is that of giving a full interpretation of Newton's scientific work. The present paper is aimed at accomplishing this task.

8. A. Drago, A. Coppola, op.cit. n.7.
9. A. Koyré, Études d'histoire de la pensée scientifique, Gallimard, Paris, 1966, p. 188.
10. A. Koyré, op. cit. n. 7, ch. 4.
11. G. Berkeley, The Analyst, London, 1734.
12. *A. Drago* : Storia del cocetto di spazio come rivelatrice delle scelte fondamatali della teoria fisica, I; Atti VII Congr. Naz. St. Fisica, Trieste, 1985, (in press); (Engl. abstract in *Not.SIF* suppl. 1986, p.38).
13. Let us note that the momentum of such new scientific theories generated new philosophical attitudes (mainly, positivism); but this fact did not greatly contribute to the acceptance of the theories by the scientific community.
14. It should be noted that the metaphysical content of two such options supports the traditional claim by physicists that metaphysics is irrelevant to clarify theoretical physics. However, some recent philosophers of science (Husserl, Burtt, Koyré, Feyerabend) did rightly suggest that a rigid metaphysics underlies modern science. The following historical appraisal of the whole scientific development by R. Burtt seems to me particularly penetrating: "Metaphysics, they tended more and more to avoid, so far as they could avoid it; so far as not, it became an instrument for their further mathematical conquest of the world." See R. Burtt, The Metaphysical Foundations of Modern Physical Science (1924), Routledge and Kegan, London, 1972, p,. 303.
15. A. Drago, Una definizione precisa di incommesurabilità delle teorie scientifiche, Atti VII Congr. Naz. St. Fisica, 1986, Padova (1987), pp. 125-129, (Eng. abstract in *Not.SIF* suppl., 1986, p. 212). A. Drago, Koyré e la metafisica della scienza moderna, ibidem, pp. 119-123.

16. This characterization of a "paradigm" differs from that offered by the structuralists because their "core" does not take any of the two basic options into account. Thus, their theory-nets would here become much more complex and split up. Instead, the present characterization is similar to the broad quadruple division of scientific theories suggested by V. Tonini, Le scelte della scienza, Nuova Universale Studium Roma, 1977, and to the four types of abstract productions relative to the four types of societies suggested by J. Galtung, Ideology and Methodology, Eijlers, Copenhaven, 1976, ch. I.
17. T.S. Kuhn, The Structure of Scientific Revolutions, U. of Chicago P., 1962. In fact one could re-read this work while identifying Kuhn's paradigm with Newton's model.
18. A. Drago, see the papers listed in footnote 15, as well as A. Drago and S.D. Manno, La meccanica di Lazare Carnot è una alternative alla meccanica newtoniana migliore di quella di Mach, Atti VII Congr. Naz. St. Fisica, 1986, Padova (1987), pp. 137-142.
19. C.C. Gillispie, Lazare Carnot Savant, Princeton U.P., Princeton, 1971. A. Drago and S.D. Manno, op. cit. n.18. One more example of a list of physical concepts at radical variance owing to the incommensurability of two physical theories is studied in A. Drago and V. Guerriera, Il primo caso di incommensurabilità tra teorie fisiche: ottica cartesiana vs. ottica newtoniana, in F. Bevilacqua (ed.), op. cit. n. 7, 131-136.
20. The famous Einstein confessed to being an opportunist on the metaphysical level.
21. L. Carnot, Principes fondamentaux de l'équilibre et du mouvement, Crapelet, Paris, 1803. A detailed study of its principles is to be found in A. Drago and S.D. Manno, Le ipotesi fondamentali della meccanica secondo L. Carnot (submitted to *Epistemologia).*
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23. I.N. Lobachevsky, Geometrische Untersuchungen ..., op. cit. n. 5.
24. Already H. Poincaré had perceived the distinction between the last two cases and called them resp. the "mathematical continuum" and the "physical continuum." (H. Poincaré, La Science et l'Hypothèse, Paris, 1907, ch. 3.
25. A. Coppola, A. Drago, Storia dei principi della dinamica alla luce della matematica costruttiva, Atti IV Congr. Naz. St. Fiosica, Como, 1983, 122-127.
26. Such result agrees again with Berkeley's criticism which thus reveals itself to be the most penetrating of all times. In this respect, see K.R. Popper, Conjectures and Refutations, Rougledge and Kegan, London, 1963, ch. 6. Although disagreeing with Berkeley, Popper declared himself "struck for its (Berkeley's philosophy of science) modernity" (p. 171). Only recently did some philosophers reconsider the problem of absolute vs. relational space; it is noteworthy that they reached results similar to that of the present paper. See K.L. Manders, On the Space-Time Ontology of Physical Theories, *Phil. Sci. 49,* (1982) 575-590 and B. Mundy, Relational Theories of Euclidean Space and Minkowsky's Spacetime, *Phil. Sci. 50,* (1983) 205-226.
27. P.T. Landsberg, Thermodynamics, Interscience, New York, 1961. It is worth noting that constructive mathematics offers a principle linking discontinuity to undecidability, which is very useful for the analysis of physical concepts; see O. Aberth, Introduction to Computable Analysis, McGraw-Hill, New York, 1980, p. 48.
28. A. Drago, Constructive Mathematics ..., op. cit. n.1.

29. O. Aberth, op. cit. n. 27, ch. 12.

P.A. KROES

NEWTON'S MATHEMATIZATION OF PHYSICS IN RETROSPECT

The publication of Newton's *Principia* marks the beginning of *modern* science. But what is so modern about Newton's *Principia*? After all, the content of the *Principia* is nowadays usually referred to as *classical* Newtonian physics. Physicists have 'abandoned' most of the ideas and theories contained in the *Principia*. Not Newtonian mechanics, but quantum mechanics and relativistic mechanics are the cornerstones of contemporary physics. However, in spite of the revolutionary changes in physics which took place around the turn of the twentieth century and which led to the overthrow of the Newtonian physical worldview, Newtonian physics is not completely outdated; it still has something in common with present day physics which definitively gives it a modern spirit.

According to Dijksterhuis [1986], this common element of classical Newtonian and 20th century physics consists of the crucial role attributed to mathematics. In his opinion, the mathematization of physics, which reached its culminating point in Newton's *Principia*, is one of the essential aspects of the great scientific revolution of the 17th century. In ancient physics mathematics did not play a comparable role. Ever since the publication of the *Principia*, mathematics has been the language of physics and, because Newton was the first to show how nature can be studied fruitfully and coherently in a mathematical way, he is rightly regarded as the founding father of modern physics. The *Principia* has become a paradigm for the mathematical treatment of physical problems, and in this respect it has lost nothing of its original 'grandeur'.

The purpose of this paper is to analyze and compare the role which mathematics plays in Newton's mechanics with the role it has in 20th century physics, and more particularly, in quantum mechanics. In both theories mathematics plays a dominant role, but nevertheless there appears to be a significant difference in the role of mathematical formalism in the two theories. My main thesis is that for Newton the use of mathematics is based upon a prior conceptualization of physical reality, whereas in quantum mechanics more or less the reverse appears to be the case. Newton starts from a given conceptual framework which includes his basic ontological commitments (e.g. the existence of absolute space and time) and defines the basic physical processes (e.g. only mechanical processes are allowed for, and action at a distans is excluded). Within

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the context of this framework he introduces mathematical constructs which are *mathematical representations* of his (idealized or simplified) conceptualizations of physical systems. Within quantum mechanics this is not the case. In its standard interpretation, the mathematical formalism of quantum mechanics is not a mathematical representation of a conceptualization of microscopic physical reality, i.e. of the physical structure of an atom. Its primary function is not to present a mathematical image of physical reality, but to correctly describe the correlations between observable physical quantities. The question whether or not this formalism allows for a coherent conceptualization of physical reality, becomes of secondary importance.[1]

Newton's mathematization of physics

Newton's mathematical approach to physical problems has been thoroughly analyzed by I. Bernard Cohen in his *The Newtonian Revolution*; he refers to it as "the Newtonian style." Let me summarize here the main results of his analysis.

According to Cohen, the essence of the Newtonian style consists in the separation of the study of nature into two parts. In the first part, Newton explores the mathematical consequences of imaginative systems which are mathematical representations of simplified or idealized physical systems. In the second, the mathematical results obtained are applied to the explanation of the phenomena [Cohen 1983, p. xii]. In Newton's own words [1973, p. 397]:

> In the preceding books I have laid down the principles of philosophy; principles not philosophical but mathematical: such, namely, as we may build our reasonings upon in philosophical inquiries. These principles are the laws and conditions of certain motions, and powers or forces, which chiefly have respect to philosophy; but, lest they should have appeared of themselves dry and barren, I have illustrated them here and there with some philosophical scholiums, giving an account of such things as are of a more general nature, and which philosophy seems chiefly to be founded on; such as the density and the resistance of bodies, spaces void of all bodies, and the motion of light and sounds. It remains that, from the same principles, I now demonstrate the frame of the System of the World.

So Newton approaches the study of nature by way of "mathematical principles." Cohen regards this as one of the most revolutionary aspects of the *Principia* [Cohen 1985, p. 236].

More particularly, Cohen distinguishes three phases in the Newtonian style, described by Newton himself in the following way:[2]

> In mathematics, the task is to investigate the quantities of forces and those ratios that follow from any conditions that may be supposed. Then, moving on to physics, these ratios must be compared with the phenomena, so that it may be found out what conditions of forces apply to each kind of attracting bodies. And then, finally, it will be possible to dispute more safely about the physical species, physical causes, and physical proportions of these forces.

The first phase is primarily of a mathematical nature and starts from a simplified or idealized conception of a physical system

> leading to an imaginative construct in the mathematical domain, a system in geometric space in which mathematical entities move in mathematical time according to certain set conditions which tend to be expressible as mathematical laws or relations [Cohen 1983, p. xii-xiii].

Starting from this mathematical construct calculations are made and the results of these calculations are translated back into the domain of physics. In the second phase, the outcome of phase one is compared with experimental data. Usually, this leads to an adaptation of the original mathematical construct, which starts phase one anew. In the final phase, the results of the first two are applied to the philosophy of nature. In the *Principia* this corresponds to the content of book III in which a new system of the world is described with the universal force of gravitation as an essential ingredient [Cohen 1983, p. 111].

According to Cohen there is still a fourth phase in the Newtonian style, which he calls the "sequel." In this sequel, Newton searches for the real causes and explanations of physical phenomena (such as the real causes of the force of gravitation).

For a better understanding of the relationship between mathematics and physics in the Newtonian style, it is necessary to have a closer look at the *origin* of Newton's imaginative mathematical constructs. Although he says, in the above quotation, that any mathematical construct whatsoever can be analyzed ("any conditions supposed"), he only considers those

mathematical constructs which are relevant for physics: "principles [...] such, namely, as we may build our reasonings upon in philosophical inquiries."[3] In fact, the mathematical constructs studied by Newton are based upon simplifications or idealizations of real physical systems [Cohen 1983, p. 37]:

> [...] he did not produce mere mathematical constructs or abstractions that were devoid of any content of reality other than "saving the phenomena," but he did create what he conceived to be purely mathematical counterparts of simplified and idealized physical systems that could later be brought into relation with the conditions of reality as revealed by experiment and observation.

Newton, for instance, carefully analyzes the mathematical construct corresponding to the motion of only one planet in a central force field. This physical system, as an approximation of e.g. the motion of the earth around the sun, is mathematically represented by the motion of a 'point particle' under the influence of a 'mathematical force'. The strength of this mathematical force is chosen so as to be equal to the force of gravity.[4]

With regard to our theme, the role of mathematics in physics, the most important conclusion from Cohen's analysis of the Newtonian style is that Newton's mathematical constructs are not arbitrary inventions. Their origin can be traced back to the kind of physical systems involved: they are mathematical representations of those physical systems. In passing from physical systems to mathematical constructs, idealizations and simplifications play an important role. Now, if we look more closely at how this representation is brought about, then we see that Newton's point of departure is his conceptualization of physical reality. For instance, Newton considers our solar system to be a gravitational system which he describes in terms of physical concepts such as 'absolute space,' 'absolute time', 'mass,' 'force', 'impulse' etc. Next he introduces mathematical concepts corresponding to these physical concepts, such as 'mathematical time' and 'mathematical force,' and an imaginative construct which is a mathematical counterpart of his conceptualization of this physical system. In other words, in Newton's case the mathematization of physics is based upon a prior conceptualization of physical reality.

Let us now briefly look back, from our 20th century perspective, at Newton's way of dealing with mathematics in physics. It is clear that Newton's attitude towards the role of mathematics in physics does not fit in with modern (20th century) positivism. For a modern positivist

mathematical formalisms are mere instruments for an adequate description of empirical data. Any mathematical formalism is allowed on condition that it satisfies Mach's principle of economy of thought and 'saves the phenomena'. But the difference between the *Principia* and the *Almagest* is precisely that, although mathematical constructs play a dominant role in both these works, Newton's mathematical constructs pretend to represent what really goes on in our solar system, whereas this is not the case for the *Almagest* [Cohen 1983, p. 31-2]. Moreover, there is the fourth phase in the Newtonian style, in which Newton inquires into the 'real' causes of nature. Such inquiries are irreconcilable with a positivistic attitude [op. cit. p. 113].

Might it be that Newton, in view of his emphasis on mathematics in his *Principia*, adhered to some kind of Pythagorean or Platonic doctrine in which the ultimate constituents of nature are identified with mathematical entities? According to Whiteside, Newton was indeed primarily a mathematician with an "inner vision which sensed the mathematicisable understructure in all things" [1982, p. 116]. But Whiteside also remarks that nowhere in his mathematical papers does Newton show a trace of a Pythagorean attitude.

Clearly, in retrospect it is not easy to grasp Newton's ideas on the relation between mathematics and physics. We have to keep in mind that at the time of Newton the bond between mathematics and physics was in a sense much tighter than it is now. Nowadays, mathematical theories are generally considered to be abstract formalisms which are free inventions of the human mind and which as such stand in no relation at all to physical reality. In the 16th and 17th century, however, most mathematical concepts were immediate idealizations and abstractions from experience. In the words of Kline [1972, p. 393], only "after 1700, more and more notions, further removed from nature and springing full-blown from the human minds, were to enter mathematics [...]."

With regard to geometry, this tight bond with physical reality continued to exist far into the 19th century. Geometry was considered to be the study of the structure of physical space [Kline 1972, p. 1028-9]. Newton was no exception; for him, Euclidean geometry was the structure of absolute space.[5] It is interesting to note that in his *Principia* Newton uses the language of synthetic geometry. This is remarkable, because his discovery of the calculus was made possible by the use of analytic geometry. To write the *Principia*, Newton went through the considerable trouble of restating his calculus of fluxions in the language of synthetic geometry. His revolt against analytic geometry is without doubt connected with his strong rejection of Descartes' philosophical and physical ideas [Westfall 1984, p. 379-381]. But there might be still another reason for

Newton's preference for synthetic geometry. He dislikes calculations in analytic geometry "for these computations [...] often express in an intolerably roundabout way quantities which in geometry [i.e. synthetic geometry, P.K.] are designed by the drawing of a single line" [Westfall 1984, p. 380]. It looks as if Newton objects to the rather formal character of calculations in analytic geometry; the 'real' meaning of these calculations in terms of line segments and points moving along lines is often not clear.[6] If this is indeed the case, then it is reasonable to suppose that Newton thought that analytic geometry, because of its rather formal character, was less suited for the description of nature than synthetic geometry, since the mathematics used in physics somehow had to 'match' nature. He expresses this line of thought in his controversy with Leibniz on the discovery of the calculus of infinitesimals, where he explicitly states that he prefers his own method of fluxions above the one of Leibniz because his "Method is derived immediately from Nature her self" [Westfall 1984, p. 380; Cohen 1983, p. 60]! In my opinion, this statement reveals an essential trait of Newton's attitude with regard to the role of mathematics in physics.

Mathematics and quantum mechanics

Changing the scenery to 20th century physics, we find ourselves in a different intellectual landscape. In the aftermath of the discovery of non-Euclidean geometries,[7] mathematics has freed itself completely from its ties to the physical world. This had far-reaching consequences for the foundations of mathematics; several schools of thought emerged like logicism, intuitionism and the formalistic approach. Within the present context, however, these are not of interest; we shall focus our attention on the role of mathematics in 20th century physics.

Here we will analyze the role of mathematics in the conceptual development of quantum mechanics, particularly Heisenberg's matrix mechanics. This theory is, of course, not in all respects representative for 20th century physics. But both Newton's classical mechanics and Heisenberg's matrix mechanics have had a strong influence on physical thought and, moreover, mathematics has occupied a dominant place in both these theories.

Quantum mechanics was not born all at once; it took a quarter of a century before many scattered elements were finally integrated into a coherent theory. Among these elements we find important conceptual innovations like the quantization of the interaction energy between matter and radiation (Planck, 1900), the quantization of the energy of the free

electromagnetic field (Einstein, 1905) and with it the wave particle duality of light, which in the twenties was followed by the wave particle duality of matter, the quantization of the orbits of electrons in atoms (Bohr, 1913), the quantization of the momentum of the electromagnetic field (Einstein, 1916), the discovery of electron spin (Goudsmit and Uhlenbeck, 1925), etc.

The origins of these conceptual innovations can be traced back to new empirical data, theoretical developments as well as to the use of mathematical formalisms and techniques. Concentrating upon the latter, we see that they often determined to a large degree the process of concept formation in physics. As an example of this, we shall have a brief look at the introduction of the notion of energy quantization into physics by Max Planck.

Schematically, the introduction of energy quanta came about in the following way [Kuhn 1978, Jammer 1966]. Measurements of the intensity of black body radiation in the far infrared, by Lummer, Pringsheim, Rubens and Kurlbaum, showed the restricted validity of Wien's radiation law; it had to be replaced. In October 1900 Planck formulated his famous radiation law which agreed with the known empirical data. However, the derivation of the law was strongly ad hoc.[8] So Planck faced the theoretical problem of finding a physically acceptable derivation and interpretation of his radiation law [Kuhn 1978, p. 97]:

> On the very day when I first formulated this law, I began to devote myself to the task of investing it with a real physical meaning, and that issue led me of itself to the consideration of the relationship between entropy and probability, and thus to Boltzmann's line of thought.

Thus Planck started to look for a new conceptual framework on the basis of which the validity of his new radiation law could be explained. He succeeded by using certain combinatorial techniques, developed by Boltzmann, for calculating the average energy of his resonators. From a classical point of view, the energy of these resonators is a continuous magnitude. Now, in order to apply Boltzmann's combinatorial techniques, Planck had to assume that the total energy of a resonator consists of a number of discrete energy elements [Jammer 1966, p. 19]. In this way he was able to derive the correct radiation law. At first, this idea of energy quantization had no serious physical meaning, but later physicists reached the conclusion that in reality the energy of the resonators, and thus also the interaction energy between matter and radiation, was quantized.[9] Note that Planck does not introduce combinatorial techniques be-

cause he is convinced of the physical reality of energy quantization. Rather the reverse is the case: the introduction of the idea of energy quantization is based upon the use of combinatorial techniques, which require a discrete distribution of energy. So, the use of a specific mathematical technique led to a drastic switch in the conceptual development of physics.

We shall now turn to the development of quantum-theoretical ideas with regard to the structure of atoms, starting with Rutherford's model. The problem of the atomic structure was one of the central topics in physics at the beginning of this century. It was finally 'solved' by Heisenberg's matrix mechanics and Schrödinger's wave mechanics. Especially the genesis of Heisenberg's matrix mechanics illustrates the changing role of mathematical formalisms in physical thought.

On the basis of his scattering experiments with alpha particles, Rutherford proposed in 1911 a model of the atom in which the atom consists of a small, but heavy nucleus with a positive charge, around which negatively charged electrons move in periodic orbits. According to Rutherford's model, the atom was a kind of microscopic planetary system. This planetary picture of the atom has dominated the conceptual development of atomic theory during the second decade and the early twenties.

A further elaboration of Rutherford's model was presented by Bohr in 1913 [Jammer 1966, pp. 69-88]. In his theory, Bohr tries to incorporate quantum ideas into Rutherford's classical conception of the atom. A serious problem with Rutherford's theory was the stability of atoms; from a classical point of view, a system consisting of electrons moving around a positive nucleus is unstable, because the electrons will continuously emit radiation due to their accelerations. Therefore they will lose energy which means that they will spiral into the nucleus. Bohr solved this problem by postulating the existence of *stationary* orbits in which the electron does not emit radiation. Radiation is only emitted when an electron 'jumps' from one stationary orbit to another. Bohr assumed that the laws of classical mechanics are still valid for the motion of electrons in stationary orbits but not for the transitions between stationary states [Jammer 1966, p. 81].

In Bohr's model the idea of an atom as a small planetary system is essentially maintained. In the later development of Bohr's theory, for instance by Sommerfeld, this idea played a strong heuristic role. Like planets, electrons move along, 'Kepler orbits' ('Kepler bahnen') and one of the main problems facing physicists was how to quantize these orbits [see, for instance, Sommerfeld 1921, p. 263 ff, p. 408 ff]. Bohr had assumed that electrons move in circular orbits, which means that they pos-

sess only one degree of freedom, corresponding to one quantum number (quantum condition). Sommerfeld added more degrees of freedom, and thus more quantum numbers, by supposing that the electrons move in elliptic orbits (two degrees of freedom) and that the plane of the ellipse revolves around the nucleus (three degrees of freedom). Treating the motion of the electrons relativistically, Sommerfeld also showed that the orbits are not elliptic but have the form of a rosette, analogously to the motion of Mercury around the sun. Thus in the Bohr-Sommerfeld theory quantum numbers are interpreted in terms of geometric properties of electron orbits (just think of the notion of 'space quantization'). In other words, the idea of an atom as a small planetary system provides the conceptual foundation for quantum numbers.

Another striking example of the heuristic power of the planetary conception of the atom is the use of the mathematical technique known as the separation of variables in the Hamilton-Jacobi differential equation. This mathematical method had been developed within astronomy for the computation of the periods of planetary orbits, but it also proved to be of great value for the calculation of atomic spectra [Jammer 1966, p. 102-3]. The use of this specific mathematical technique is intimately connected with the planetary picture of the atom, which constituted the conceptual basis of the Bohr-Sommerfeld theory. In this case, however, contrary to the introduction of the idea of quantization by Planck, the use of a specific mathematical technique was based upon a prior conceptualization of the physical systems involved.

The initial success of the Bohr-Sommerfeld theory strengthened the idea that in reality atoms possess a planetary structure. In the early twenties, however, problems began to accumulate. As a result of these, physicists slowly started to realize that this planetary picture had a merely 'symbolic meaning' (Heisenberg in a letter from the year 1923 to Pauli; Pauli [1979, p. 125]). In itself, this is not a remarkable development; many physical models have shared the same fate. What is remarkable about this case is that for very fundamental reasons the quest for a model of the structure of the atom was abandoned, at least by some physicists. With regard to this, the correspondence of Pauli during the years 1923-25, especially the correspondence with Heisenberg, is illuminating.[10]

In a letter to Bohr, dated Dec. 12th 1924, Pauli defends the idea that the basic concepts of classical kinematics need to be drastically altered. He questions whether the notion of trajectory or orbit of an electron is applicable to the motion of an electron in an atom: "We may not wish to cast the atoms in the chains of our prejudices (to which in my opinion the assumption of the existence of electron orbits in the sense of ordinary kinematics also belongs) [...]."[11] During this period Pauli adopts

more and more an operationalistic-positivistic view with regard to the use of quantumtheoretic concepts.[12] In a letter to Bohr, dated Nov. 17th 1925, he is very explicit on this [op. cit. p. 260]:[13]

> Now it is my firm conviction that a really satisfactory physical theory not only does not use unobservable quantities, but also has to relate to each other *all* observable quantities.

For the atomic theory this implies that notions like 'trajectory of an electron' have to be banished from physical discourse.

In the same period, Heisenberg adheres to similar ideas. He too rejects the classical kinematical concept of a trajectory of an electron; this trajectory is not observable and thus this notion is physically meaningless. His famous 1925 article, in which he lays the foundations of his matrix mechanics, begins in the following way [Van der Waerden 1968, p. 261]: "The present paper seeks to establish a basis for theoretical quantum mechanics founded exclusively upon relationships between quantities which in principle are observable." In his approach, Heisenberg abstains from the use of notions like 'position of an electron' or 'period of revolution of the electron,' because these notions do not correspond to physically observable quantities. In other words, Heisenberg proceeds in a strongly positivistic way.

Now, if we compare Heisenberg's matrix mechanics with the Bohr-Sommerfeld theory, we observe a significant difference. Heisenberg's mechanics is not based upon a conceptualization of the structure of an atom. It is solely a mathematical formalism for the computation of frequencies and intensities of spectral lines (i.e. of observable quantities of atoms). This mathematical formalism is not the outcome of an abstraction from or an idealization of physical reality or of a representation of physical reality in terms of a (visualizable) model; it is no more than an instrument for establishing quantitative relations between observables.

The abandonment of the search for a conceptual framework for describing the structure of atoms, is a characteristic feature of matrix mechanics. This development marks a new phase in the mathematization of physics: the use of mathematical formalisms moves away from the conceptualization of physical reality. In a sense, the conceptualization of physical reality becomes mathematical in nature; for instance, the spin of an electron is a two-valued operator on the wave function of an electron! Mathematical formalisms become an instrument to 'save the phenomena' instead of being a representation of physical reality in the mathematical domain. The so-called Kopenhagen interpretation of quantum mechanics, which is wide-spread among physicists, strongly favors this attitude

towards the role of mathematics in physics.

Finally, let us add a few words about Schrödinger's wave mechanics. The development of this theory, which is mathematically equivalent to matrix mechanics, followed a different path; the conceptual core of this theory is the notion of matter waves and the idea of stationary states as standing electron waves [Jammer 1966, p. 236ff]. For Schrödinger the physical interpretation of the mathematical formalism, especially of his wave function, was an important problem [op. cit. p. 267]; it was not just some mathematical symbol. A closer comparison between wave and matrix mechanics would undoubtedly show that in the former the bond between mathematical formalism and the conceptualization of physical reality was not severed as radically as in the latter. Nevertheless, the interpretation of wave mechanics still poses a challenge to many physicists.

Conclusion

In *The Newtonian Revolution* Cohen draws a parallel between the situations regarding Newton's gravitation theory and quantum mechanics [Cohen 1983, p. 147]:

> I believe that the outlook of post-Newtonian scientists, using a system based on the action of a universal force that they could not understand, was not wholly unlike that of present-day physicists with respect to quantum field theory. In a recent talk, Murray GellMann (1977) said: "All of modern physics is governed by that magnificent and thoroughly confusing discipline called quantum mechanics, invented more than fifty years ago. It has survived all tests and there is no reason to believe that there is any flaw in it. We suppose that it is exactly correct. Nobody understands it, but we all know how to use it and how to apply it to problems; and so we have learned to live with the fact that nobody can understand it." It was much the same in the age of Newton for gravitational celestial mechanics.

Indeed, in both situations physicists had serious problems with understanding the theories involved. Nevertheless, in my opinion the differences between the two cases are greater than the similarities.

In the first place, the problems regarding the interpretation of the mathematical formalism of quantum mechanics are of a much more fundamental character than those with regard to Newton's gravitation

theory. In the case of quantum mechanics, many physicists are convinced that a conceptualization of the micro-physical world, in terms of visualizable pictures, is in principle impossible. The same cannot be said for Newton's theory of gravitation. During his life, Newton himself examined different mechanical models for the explanation of the modus operandi of gravity [Cohen 1983, p. 111ff]. None of them proved to be successful, and since Newton was not prepared to 'frame hypotheses,' he postponed his judgement with regard to the nature of gravity. This, however, does not mean that for him, or for many physicists after him, a mechanical model of gravity was in principle inconceivable.

Secondly, although in both cases physicists had to rely heavily upon mathematical formalisms because of the absence of acceptable conceptual models, this should not mask the fact that in both cases the status of the mathematical formalisms is completely different. Newton arrives at his conclusion that "gravity exists" on the basis of a mathematical representation of his physical conception of our solar system. He deduces the existence of gravity from the phenomena by representing those phenomena mathematically. His mathematical constructs are mathematical representations of physical reality. This is not true for the mathematical formalisms of quantum mechanics. Within quantum mechanics mathematics is primarily an instrument to 'save the phenomena'. The use of mathematics does not have its foundation within a 'philosophy of nature,' as it did for Newton. Many contemporary problems in the foundations of quantum mechanics stem from the desire to complement the mathematical formalisms of quantum mechanics with a coherent conceptual framework in which the expression 'physical reality' refers to more than only the collection of observable quantities.

All in all, we may conclude that the attitude of a 20th century physicist with regard to mathematics is essentially different from the one of a 17th century physicist. An article of Wigner [1969] illustrates the difference; in it, Wigner discusses the "unreasonable effectiveness of mathematics in the physical sciences." Indeed, for a 20th century physicist the success of the use of mathematics, which he regards as a free invention of the human mind, cannot be but a miracle. For Newton there was nothing unreasonable about this success, because for him not only did mathematics find its origin in physical reality, but its function in physics was to represent that reality.

University of Nijmegen, The Netherlands

NOTES

1. Reformulated in Scheurer's terminology of 'double coding' [1984, 1987], my main argument is that a change has occurred in the relation between the conceptual and structural (mathematical) coding of physical phenomena.
2. Cohen [1983, p. 85]; this translation is somewhat different from the same passage in Newton [1973, p. 192].
3. In the preface to the first edition of the *Principia* Newton remarks: "[...] I have in this treatise cultivated mathematics as far as it relates to philosophy."
4. But Newton also takes the liberty to discuss mathematical constructs and forces which have no direct physical significance [Whiteside 1982, p. 117; Cohen 1983, p. 102-3].
5. The discovery of non-Euclidean geometries at the beginning of the 19th century finally led to a purely mathematical conception of geometry as a formal axiomatic system.
6. This is of course due to the fact that a geometric problem is translated into an algebraic one.
7. According to Kline "[...] the most consequential and revolutionary step in mathematics since Greek times" [1972, p. 879].
8. According to Jammer the derivation was a kind of mathematical interpolation; he remarks that [1966, p. 18] "never in the history of physics was there such an inconspicuous mathematical interpolation with such far-reaching physical and philosophical consequences."
9. Much to Planck's distress; see Kuhn [1978].
10. See Pauli [1979], particularly the letters numbered 47, 71, 74, 79, 93, 96, 97, 105 and 106; for the abandonment of visualizability within quantum mechanics, see also Miller [1984].
11. Pauli [1979, p. 189]: "Wir dürfen aber nicht die Atome in die Fesseln unserer Vorurteile schlagen wollen (zu denen nach meiner Meinung auch die Annahme der Existenz von Elektronenbahnen im Sinne der gewönlichen Kinematik gehört) [...]." The English translation is mine.
12. For the development of Pauli's operationalistic views, see Hendry [1984, p. 19ff].
13. The German original reads: "Es ist nun meine feste Ueberzeugung, dass eine wirklich befriedigende physikalische theorie nicht nur keine unbeobachtbaren Grössen mitverwenden darf, sondern auch *alle* beobachtbaren Grössen miteinander verknüpfen muss."

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BARRY GOWER

PROBABILITY, PLANETS, AND NEWTON'S METHODOLOGY

The early development of some aspects of probability was closely guided by experience, rewarding or otherwise, of games of chance. This association was fortunate so far as the origin of the 'doctrine of chances' in the seventeenth century was concerned; then, as now, simple games involving coins, dice or playing cards provided suitable models for understanding the relevant combinatorial arguments. But there were topics other than gambling which shared its characteristic vocabulary – 'chance', 'luck', 'fate', 'coincidence', 'random', etc. – and by the beginning of the eighteenth century a number of people skilled in calculating chances were finding opportunities to apply their expertise to some of these topics. John Arbuthnot's startling memoir for the Royal Society of London on the slight, and perhaps coincidental, preponderance of births of male rather than female children is one well known example of such thinking.[1] Another is the anonymous memoir, also published in the *Philosophical Transactions,* concerned with the credibility of testimony, where the chance of reports being false if corroborated by independent witnesses is examined.[2] But perhaps the best example is Jakob Bernouilli's clear and thorough account of the conditions necessary for extending the scope of the mathematical theory of chances. In the final part of his *Ars Conjectandi,*[3] we can learn of an ambitious but well founded plan for adopting the numerical methods of the doctrine of chances to calculate probabilities 'in civilibus, moralibus, et oeconomicis'. Although Jakob's famous treatise was published posthumously, recent studies have revealed that it was based upon work done in the 1680's when he was using the language of games of chance to explore and compute probabilities in practical affairs such as marriage contracts.[4]

One context in which reasoning about chance was thought to carry weight was astronomy. For, in England, Newton had created a theory which showed that some features of the solar system were governed by stated laws of nature. Thus, the Keplerian regularities in the geometrical properties of the planetary orbits had been shown to be no mere cosmic accident, but the result of the fact that the planets were subject to the same law of gravitation as other objects. But there were certain regularities in the behaviour of the planets and their satellites which had not been shown by Newton to be law governed. Is it, it was asked, no more than a coincidence, though a remarkable one, that the planets all rotate in the same direction around the sun? Is the fact that the planes of the orbits of

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the planets so nearly coincide with each other, or the fact that the eccentricities of these orbits are so small, to be attributed to chance? Or can these regularities, like Kepler's geometrical ones, be provided with an explanation?

One reason why these questions assumed significance in the early eighteenth century was that they featured in a version of the argument for God's existence based upon evidence of design in the inanimate world.[5] In particular, Newton and some of his followers were quick to realise that, because the new theory of gravity failed to entail that the planets rotate about the sun in a uniform directon or that the mutual inclinations of the planetary orbits should be so small, it made sense to inquire into the likelihood that these features were coincidental. If this likelihood was sufficiently small that coincidence could be safely ruled out, then these regularities could be treated as indicating the intention and design of a deity.

Evidence of Newton's approval of such reasoning was provided by William Derham who reported a

> peculiar sort of Proof of God wch Sr Is: mentioned in some discourse wch he & I had soon after I published my Astro-Theology. He said there were 3 things in the Motions of the Heavenly Bodies, that were plain evidences of Omnipotence & wise Counsel. 1. That the Motion imprest upon those Globes was Lateral, or in a Direction perpendicular to their Radii, not along them or parallel wth them. 2. That the Motions of them tend the same way. 3. That their Orbits have all the same inclination.[6]

Young Richard Bentley, convinced that Newtonian principles could be used to attack atheism and materialism, and encouraged by letters from Newton to think of celestial phenomena as being produced by 'choice rather than chance,'[7] had given public expression to the reasoning in his eighth and final Boyle sermon of 1692 as follows:

> Now, if the [planetary] system had been fortuitously formed by the convening matter of a chaos, how is it conceivable that all the planets, both primary and secondary, should revolve the same way, from the west to the east, and that in the same plane too, without any considerable variation? No natural and necessary cause could so determine their motions; and 'tis millions of millions of odds to an unit in such a cast of chance. Such an apt and regular harmony, such an admirable order

> and beauty, must deservedly be ascribed to divine art and conduct.[8]

Apart from Bentley's indication that the chance in question was actually *quantifiable,* there was nothing particularly novel about this kind of argument; ancestors to it had long been familiar to natural theologians. Nevertheless, despite its ubiquity and its importance in attacks on the atheistic Epicureanism of writers like Gassendi and Hobbes, the early probabilists were not specially careful to distinguish different senses of the word 'chance' or, indeed, to adopt a consistent meaning for it, even though some were aware of significant ambiguity. Bentley, for example, might be thought to have adopted a thoroughgoing epistemological or 'Laplacian' concept of chance when he wrote of chance events as those whose causes are unknown.[9] But he also claimed that of a chance event 'no further inquiry [is] to be made, no more difficulties to be solved, and no account to be demanded',[10] which suggests that he was tempted to think of it as cause-*less*. Most prominence, though, was given to yet another concept of chance elucidated in terms of 'design', 'foreknowledge' and 'intention'. Thus, Bentley explains that 'such effects as are commonly attributed to chance were verily produced by their true and proper causes, but without their designing to produce them,'[11] and that 'any effect among ... bodies ascribed to chance is really produced by physical agents, according to the established laws of motion, but without their consciousness of concurring to the production, and without their intention of such an effect.'[12]

It is this last conception of chance which was of most importance to Newton and Bentley, as well as to many of their eighteenth-century successors. Their concern was with circumstances where *order, regularity,* or *pattern* were apparent. They had no doubt that each event fitting into a pattern had some cause, even if it should be unknown. But that still left a question unanswered, which they expressed in the following terms: Is the pattern or regularity itself a product of design, or is it an unintended effect of independently operating causes? Thus, just as someone might discern an order in the arrangement of books on a shelf, and wonder whether this was the result of a chosen policy, or whether instead it was coincidental, so a Newtonian might detect a pattern in the positions or movements of celestial objects and inquire whether it was evidence of design or whether it was accidental. Clearly, this question cannot arise unless the existence of some pattern in events is recognised, for it is about the pattern, and not the events comprising it, that the question is asked. Moreover, this same question can be expressed in a way which throws helpful light on this concept of chance. For we may ask of any re-

gularity or pattern whether it is governed by a scientific law, or is no more than a coincidentally true generalisation. If the regularity is law-like then it will have some explanation; if it is accidental it will not. This way of putting the question plainly shows that the concept of chance being employed is non-epistemological. For, if the possibility that a regularity is accidental can somehow be eliminated, that does not imply that anyone knows of, or has reason to believe in, an explanation of the regularity. All that it implies is confidence that there is some explanation to be found.

A second reason for interest in these questions about planets was that during the first part of the eighteenth century Newtonian theory was struggling to make headway against the Cartesian theory of vortices which, whatever other difficulties it faced, did provide a natural explanation of the uniform direction of rotation of the planets. Once any planet had been captured by, or created in, the solar vortex, it would be constrained to circulate in the same direction as the vortex. Furthermore, this theory implied that the only problem about the inclinations of planetary orbits was why these existed at all. For whatever the original inclination of a planetary orbit may have been, a solar vortex would tend to make it coincide with the solar equator. Descartes himself had discussed 'latitudinal movements', i.e. variation in the angular distance of a planet from the ecliptic, though he was unable to do more than gesture at an explanation in terms of the effects of planets on each other.[13] But Newton, in the first edition of the *Principia,* had ignored the problem. His subsequent letters to Bentley show, it is true, that he was not oblivious to the implications of his theory for questions about whether certain features of the planetary orbits arose from 'choice rather than chance'. But in these letters, in the General Scholium written for the second edition of *Principia,* and in Query 31 of the *Opticks,* there is an ambivalence concerning unexplained features of the solar system. When attacking Descartes' theory he coupled the motions of planets with the motions of comets in order to make the point that although comets are subject to the same gravitational laws as planets, they 'are carried with very eccentric motions through all parts of the heavens indifferently, with a freedom that is incompatible with the motion of a vortex.'[14] But when Newton came to state his design argument in Query 31 of the *Opticks,* he invited his readers to *contrast* the motions of planets with that of comets:

> For while Comets move in very excentrick Orbs in all manner of Positions, blind Fate could never make all the Planets move one and the same way in Orbs concentrick, some inconsiderable Irregularities excepted, which may have arisen from the

> mutual Actions of Comets and Planets upon one another, and which will be apt to increase, till this System wants a Reformation. Such a wonderful Uniformity in the Planetary System must be allowed the Effect of Choice.[15]

Newton's anti-Cartesianism can, no doubt, be seen as exemplifying the methodological strictures against the use of hypotheses 'in experimental philosophy' which are a prominent feature of the General Scholium. The policy there stated tried to disarm not only questions about the cause of gravity but also, conveniently, those questions about the disposition and the direction of the planetary motions which are so conspicuously unanswered by the theory of gravity. Accordingly, there is no hint in the *Principia* of a probabilistic argument relevant to the evaluation of alternative answers to such questions. But in the 'Queries' to his *Opticks,* as in his private correspondence, there are indications of a less austere attitude towards speculative thinking. We may not be able to discern the mechanism responsible for the regularities in the disposition and direction of the planetary motions but we may be confident that there is, or was, such a mechanism. The alternative supposition, that there is no such mechanism to be discerned, can be rejected, for although it is *compatible* with observations, its likelihood of being true given those observations is very small.

Probabilistic arguments concerning planets acquired some currency after Newton's death. Daniel Bernouilli computed a likelihood in 1734,[16] and he was followed by Buffon, Lambert and Laplace, who each applied the probability calculus in various ways to justify the conclusion that there exists a reason for the order evident in the orientation of the orbits. In so doing, they implicitly adopted the methodological tone of the *Opticks,* even though the focus of their attention was a set of questions posed by the *Principia.* In fact, their reasoning was of the same form as that investigated by Thomas Bayes in his famous essay of 1763. Of the 'recurrency and order' to be found in the planetary motions, these scientists were asking 'what reason there is to think that such recurrency and order is derived from stable causes or regulations in nature, and not from any of the irregularities of chance.'[17] The credentials of Baysian reasoning are still a matter of dispute, and although it is not surprising to find a link between Newton's many-faceted methodology and the birth of that dispute, the example of the disposition of the planetary orbits aids us in understanding how the link was forged.

Durham University, England

NOTES

1. J. Arbuthnot, "An Argument for Divine Providence taken from the constant Regularity observ'd in the Births of both Sexes', *Phil. Trans. Roy. Soc. London, 27* (1710), 186-90.
2. Anon, "A Calculation of the Credibility of Human Testimony," *Phil. Trans. Roy. Soc. London, 21,* (1699), 359-365.
3. Jakob Bernouilli, *Ars Conjectandi,* (Basel, 1713).
4. See I. Schneider, "Why do we find the origin of a calculus of probabilities in the seventeenth century?" in J. Hintikka, D. Gruender, E. Agazzi (eds.), *Probabilistic Thinking, Thermodynamics and the Interaction of the History and Philosophy of Science,* (Dordrecht, 1981), pp. 3-24.
5. In 1691 a design argument based upon 'contrivancies' found in the animate world was published by John Ray in his *Wisdom of God manifested in the Works of Creation.* Newton seems to have thought that a design argument based on inanimate features of the world was superior.
6. Quoted in F.E. Manuel, *A Portrait of Isaac Newton,* (Cambridge, Mass., 1968), p. 127. Derham's *Astro-Theology* was first published in 1715.
7. A. Dyce (ed.), *The Works of Richard Bentley, D.D.,* (3 vols., London, 1838), v.3, p. 207.
8. *Ibid.,* 180. This is an elaboration of Newton's argument in his first letter to Bentley. Cf. Query 31 in I. Newton, *Opticks,* (4th ed., London, 1730).
9. Dyce (ed.), *op. cit.,* p. 97.
10. *Ibid.,* p. 98.
11. *Ibid.,* p. 100
12. *Ibid.,* pp. 101-2.
13. R. Descartes, *Principles of Philosophy,* (transl. V.R. Miller and R.P. Miller, Dordrecht, 1983), pp. 98-9, 177. First published in 1644.
14. "General Scholium" in I. Newton, *Mathematical Principles of Natural Philosophy,* (revised transl. F. Cajori, University of California Press, 1934), p. 543.
15. I. Newton, *Opticks,* (Dover Publications, New York, 1952, based on 4th edition of 1730), p. 402.
16. For details and discussion, see my "Planets and probability: Daniel Bernouilli on the inclinations of the planetary orbits," *Studies in the History and Philosophy of Science* (forthcoming).
17. R. Price, prefatory letter to T. Bayes, "An Essay Towards Solving a Problem in the Doctrine of Chances," reprinted in E.S. Pearson and M.G. Kendall (eds.), *Studies in the History of Statistics and Probability,* vol. 1, (Griffin, London, 1970), p. 135. Bayes' essay was originally published in *Phil. Trans., 53,* (1763), 370-418.

ALEXANDRU GIUCULESCU

ISAAC NEWTON'S LEGACY:

AN INSIGHT INTO RESILIENT PATTERNS OF THOUGHT

1. The anniversary of any major scientific event provides an opportunity for retrospective considerations regarding the reception the event has enjoyed in the learned world since it happened. On the one hand, the career of a scientific work reveals the psychological and social reactions to its value and scope and on the other hand it discloses what is perennial or, on the contrary, what is transitory or even evanescent in the spiritual life of an open society during a particular historical period.[1] Hence the various images of the event to be celebrated yield a standard by which to measure the degree of its insertion within the scientific tradition.

2. Isaac Newton is known mainly as the author of two books (the *Philosophiae naturalis principia mathematica* and the *Opticks)* and as the inventor of the infinitesimal calculus. His scientific work was a rich source of solutions to many difficult problems of mathematics and of natural science. It may be surmised that Newton might have explained his brilliant scientific achievements as the result of the exploratory power of analysis. But in fact, he used this method in combination with a tremendous erudition and an innate ability to perform practical experiments. The *Philosophiae naturalis principia mathematica* was praised ever since its publication as the expression of the highest performance ever achieved by the human mind in its struggle for discovering the laws of nature. No wonder that this work impressed Kant and urged him to analyse man's epistemic activity and to "codify" it in a manner which would have a deep and lasting influence on the thinking of our age.

At the present time, there is a strong interest for explorations of the incentives and the deeper motives underlying the choices and reticences of outstanding personalities. In Newton's case, the manuscripts have appeared to throw a significant light upon many obscure and unknown sides of his life and thoughts, even though they cannot be of much help in explaining his scientific reputation which has been and still is so overwhelming.

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3.1 Newton's *Philosophiae naturalis principia mathematica* may be considered as, chronologically, the last of a series of works that marked the acmes of scientific achievements in our culture, a series which began with Euclid's *Elements* and continued with Ptolemy's *Almagest,* the *De revolutionibus orbium coelestium* of Copernicus, and *La Géométrie* of Descartes. This selection of titles may seem arbitrary or perhaps too limited, but it may serve to illustrate two main types of posterity which can befall a scientific work. Each of the works mentioned fulfills the general criteria of scientific work: solidity or integrity, functionality, and coherence.[2] As long as a scientific work satisfies the above mentioned criteria of normality, the authority of that work remains undisputed and therefore it comes to play a paradigmatic role which does not exclude a continual process of updating as a result of minor modifications, improvements or internal developments. When this updating process loses its significance for the progress of knowledge, the work enters a phase of crisis, which ends either by the abandonment of its previously paradigmatic role or, to the contrary, by recovery.

The most interesting cases are, of course, those of recovery. Now, the history of science makes a distinction between total recovery, i.e. maintenance of supreme and exclusive authority, and mere recovery, i.e. maintenance of authority but without supremacy. Euclid's *Elements,* for instance, lost the privilege of authority only in the first half of the 19th century, while Ptolemy's *Almagest* lost it during the 15th century, and Newton's *Principia* in the first half of our century. However, it would be a mistake to evaluate the longevity of these works exclusively on the basis of the consideration of these dates. Indeed, in spite of the loss of their supremacy, each of them maintained its scientific authority in a newly shaped frame of thought which encompassed the status of the old, as well as of the new theory involved in the crisis. Non-Euclidean geometries did not replace Euclidean geometry, nor did the theory of relativity and the quantum theory imply the demise of Newtonian mechanics. There is an astonishing similarity between Euclid's and Newton's scientific longevities, and this might be explained by the form, the object and the scope of their principal works: the form is axiomatic and the object is the shape either of the space or of the motions in space. Both Euclid and Newton tried to provide an exhaustive description of their respective object by using as few as possible concepts and relations which were make explicit in a self-contained system of statements.

The axiomatic exposition proved to be well adapted to this aim, and, consequently, it was adopted afterwards as a standard form in mathematical, resp. mechanical works. The content of geometry and mechanics, however, consists of a conceptual net obtained from abstract translations

of perceptions of things and events. In the processes of abstraction it is assumed that the final result unequivocally corresponds to the source of perception, but there is no certitude in this assumption since there is no way to verify it. However, there are results of such process of abstraction, which are recognized as valid in themselves. Hence, the problem of unequivocal correspondence with the source of perceptions is transferred to the level of abstraction itself in the form of the problem of the unequivocal meaning of statements about perception. It must be emphasized that such development is not in the least the effect of a mere convention, as Poincaré, among others, supposed, but the consequence of a deeper and lasting process in which certain works, like those of Euclid or Newton, have played an essential role. Such process is similar to that discovered by Claude Bernard in living organisms, and the result of which he called the internal environment *(milieu intérieur)*.[3] Both Euclid and Newton created patterns of thinking which were "internalized" within the mind of subsequent generations through learning. Thus the question of accidental information became a matter of substantial formation. The spiritual internalization of certain scientific patterns is responsible for the longevity of some scientific theories, and moreover, the internalization explains the resistance to crises: the internalized pattern reacts like an elastic body or structure capable of withstanding a shock without permanent deformation or rupture. This property of resilience, as physicists call it, can explain the survival of certain theories such as Euclidean geometry or Newtonian mechanics in spite of severe criticisms and the emergence of antithetic theories such as resp. non-Euclidean geometries and the theories of relativity and quantum mechanics.

Much though internalization explains the property of resilience of some scientific theories, it cannot satisfactorily explain how their *validity* can be maintained, especially when they are held together with theories that implicitly oppose them. This persistent aura of validity may perhaps be attributed to the fact that both Euclidean geometry and Newtonian mechanics were highly mathematized theories. Indeed every mathematical theory possesses the property of mathematicity which may be defined in terms of three principles: (i) recursion, (ii) conformity, and (iii) apodicticity. The first principle is inherent to every process of counting or measuring, the second governs every act of mathematical thinking inasmuch as mathematical thought consists exclusively of compatible operations using compatible entities, and the third states the demonstrability of every sentence expressing a necessary truth regardless of any empirical verification or intuitive interference.[4] As mathematical theories, both Euclidean geometry and Newtonian mechanics were devices elaborated in order to describe certain natural objects and phenomena as ex-

actly as possible. But mathematicity is ambivalent since, on the one hand, it promises the highest precision in the investigation of reality while, on the other hand, its very abstraction emphasizes its distance from reality. Mathematical theories must cope with the discrepancy between their intrinsic formalism and the empirical content which they claim to describe accurately. The emergence of antithetic theories is, after all, an issue that justifies the strong tendency toward pluralism in the contemporary world. L. Wittgenstein proposed an analogy between several descriptions of nature and a collection of different nets, each with its particular form of meshes: some square, some triangular etc. Every such net can facilitate the observation, i.e. the "description" of a surface with black and white spots while covering the entire surface so that each spot appears in a certain mesh of the net. Although the nets, i.e. the descriptions, are different, their effect does not, since the form and size of the meshes do not essentially matter. Essential is instead the fact that no net can provide any idea of the nature of the surface itself. The conclusion of Wittgenstein's analogy obviously favours Kant's agnosticism.[5]

The idea that different mathematical descriptions of the same natural phenomena may be equivalent was expressed already four centuries ago when Copernicus proposed to replace Ptolemy's cosmological theory whose undisputed hegemony had lasted fourteen centuries. It is well known, if not always fairly acknowledged, that the Ptolemaic and the Copernican theories peacefully coexisted side by side for some time because, with the exception of a few passionate partisans, those who held them were impressed more by the similarity than by the difference of the theories in question. And indeed, mathematically speaking, the cosmological system exposed resp. in Ptolemy's *Almagest* and in Copernicus' *De revolutionibus orbium coelestium* was almost the same except for the name of the central body of the system they described. Stronger yet, the Ptolemaic-Copernican pattern which had been accepted many centuries ago was again adopted by Rutherford at the beginning of our century as an atomic model. This proves that this pattern too enjoys the property of resilience.

4. Isaac Newton was also the author of an unfinished treatise on optics. Whereas the *Principia* were similar to Euclid's *Elements,* Newton's *Opticks* show more similarity with Descartes' *La Géométrie* even though the topics of the two works are different. Both *La Géométrie* and the *Opticks* were unfinished works inasmuch as their respective authors urged their posterity to continue the research initiated by them. The "Queries" which Newton added to his *Opticks* played the same role as Descartes' *Discours de la méthode* for *La Géométrie:* both are the outcome of the

scientist's introspection revealing his opinions, beliefs, projects, failures, hopes and, last but not least, his methodological principles. Doubts and fears are also expressed in order to teach and convince posterity that the way to truth is no royal highway, as Euclid is alleged to have said long before. As to the exposition, neither Descartes in *La Géométrie* nor Newton in the *Opticks* chose the axiomatic form. It seems as if they intended to introduce the reader to the very workshop of the scientist in order to explain to him the way by which a solution to a problem can be found rather than to show the solution itself. It is moreover worth noting that *La Géométrie* deals with some optical problems while the *Opticks* contains many geometrical problems.

But neither work had the good fortune of the resilient patterns of thought found in Euclid's *Elements* or Newton's *Principia*. Nevertheless, posterity did find in both Cartesian geometry and Newton's optics a pattern of creative thinking inaugurating methods and fields of research which marked turning points in the development of modern science. As Newton's descendant, modern man is particularly privileged to have inherited a perennial and unique expression of human thinking.

Bucharest

NOTES

1. Alexandru Giuculescu, L'héritage leibnizien et la pensée scientifique contemporaine en France, *Studia Leibniziana,* Bd. VI., Heft 1, 1974, 76-92.
2. Alexandru Giuculescu, The architectonic of scientific knowledge: an essay on the dynamics of the sciences. *Diogenes 131,* July-Sept. 1985, 1-23.
3. Claude Bernard, *Introduction à l'étude de la médecine expérimentale.* Paris, 1865.
4. Alexandru Giuculescu, The mathematicity of scientific theories. In *Abstracts of the VIIIth International Congress of Logic, Methodology, and Philosophy of Science,* Moscow, 1722 August 1987. Volume 5, Part 1, 24-28.
5. Ludwig Wittgenstein, *Tractatus logico-philosophicus,* 6.341-6.343, Vienna, 1918.

EWALD VERVAET

NEWTON'S CONSTRUCTION OF THE LAW OF GRAVITATION

Newton's gravitational theory is generally regarded as the cream of the *Principia*. It is dominated by one law, the so-called law of gravitation, which states in a formal way that two mass-points at a certain distance tend towards each other with a force that can be expressed in the well-known law. Once one is in possession ot this law, it allows us to explain a wide range of phenomena: planetary motion, free fall, the tides etc.

However, the problem is not in the end, but in the beginning: How did Newton's law of gravitation come into existence? That question has often been raised, and in the following I shall suggest an answer, in which I have gratefully profited from the results of Cohen's very important research (Cohen, 1980).

Phylogenesis of a formal structure

Let us start with Kepler. Kepler conceived of the sun-planet system as a simple one in which the line of firce as it were ran from the sun to the planet. In his *Astronomia Nova* of 1609 he explicitly maintains "that the source of all motion rests (in the sun), is more problable than that the sun itself moves" (p. 24). In regard to terrestrial heaviness he remarks that two related bodies tend to a union with each other, in such a way that the earth attracts a stone more than that the stone seeks the earth; the same holds for high and low tide in connection with the gravitational force of the moon (p. 26). According to Kepler the attraction by a lighter mass is smaller than that of a heavier mass and the former decreases faster as the distance increases (p. 27 f.). In the case of the planets the solar force would be something immaterial that rotates because of solar rotation andf thereby drags the planets with it (p. 34).

Now, Newton introduced into this reasoning his famous law of action and reaction: "To every action there is always opposed an equal reaction: or, the mutual actions of two bodies upon each other are always equal, and directed to contrary parts" (Newton, 1687, 13). Thus, Newton transformed Kepler's one-directional mode of thought: The sun and the planet were supposed to have a two-directional interaction. Or, starting

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from Kepler's (sun → planet)-relation, Newton composed this relation together with its reverse, the (Planet → sun)-relation to a mutual and equilibrated (sun →← planet)-relation. And thereby Kepler's one-body-approach was transformed by Newton into a two-body-approach.

As we know now in 1987, such two-body-system is actually the smallest unit of analysis in the gravitational domain: in the above-mentioned reversal Newton struck upon the simplest, yet still meaningful gravitational relation. At the same time he realized that the relevant factor in the mutual attraction was 'mass' (or 'quantity of matter' in Newton's terms), for even in the one-body-problem Newton had supposed that the one body is not so much attracted to the *geometrical* location of the other body, but to that body itself, or to its *mass*.

The next logical step is that the *masses* of two planets should interact with each other, for in the sun-planet-system the factor 'mass' had appeared to be decisive. Of course, this idea can be tested in a much better way when the masses are greater and the distances between them shorter. Thus, in proposition XIII of book III, Newton calculates the gravitational action between Jupiter and Saturn near conjunction: Jupiter's influence on Saturn's motion should take measurable proportions (Newton,1687,421). In a letter dated December 30 1684 to Flamsteed stationed at the Observatory in Greenwich, Newton asks whether he "ever observed Saturn to err considerably from Kepler's tables about the time of his conjuction with Jupiter" (Turnbull,1960,407). He continues: "The greatest error I conceive should be either the year before conjunction when Saturn is 3 or 4 signes from the Sun *in consequentia* or the yeare after when Saturn is as far from the sun *in antecedentia*" (ib). Within one week Flamsteed answers that Saturn is 27' slower than one would expect on the basis of Keplerian considerations and that Jupiter is 14 or 15' faster (ib,408).

Thus, the relation which Newton presumed to exist between Saturn's positions and its conjunction with Jupiter appeared to really exist. In other words, the apparently meaningful gravitational relation between two masses could successfully be composed to a relation among three masses. Therefore a relation like the one between Jupiter and Saturn should also occur between the earth and the moon. This conclusion was indeed confirmed by observation.

Nothing stood in the way of Newton's extending his gravitational thinking to composite systems. First, it was extended to the attraction between a falling body and the earth, and then again to situations which (at least at that time) could not be controlled observationally: Any two bodies on earth should attract each other in the same manner. The simple and composite gravitational systems here described were concrete in

the sense that Newton could test his theory by referring to concretely observable bodies such as the sun, the planets and the moon. But henceforth his operations had become formal: Newton had to operate with *hypotheses* concerning the gravitational interaction of two terrestrial objects, so that the gravitational relation became *formal* by definition.

Thus, the line of his thinking is: *concrete-simple* → *concrete-composite* → *formal-simple*. At the same time Newton's operations went from the planets to free fall on earth and not the other way around, as has been thought previously. The classical example of this is Pemberton's version of Newton's discovery of the law of gravitation: "As [Newton] sat alone in a garden, he fell into a speculation on the power of gravity: that as this power is not found sensibly diminished at the remotest distance from the centre of the earth, to which we can rise ... , it appeared to him reasonable to conclude that this power must extend much further than was usually thought; why not as high as the moon? said he to himself." And further: "[He] considered with himself that, if the moon be retained in her orbit by the force of gravity, no doubt the primary planets are carried round the sun by the like power."

As we have seen, we have good reason to believe that it occurred exactly in the reverse order. Aand this is *supported* by the fact that the *form* (not the content!) *of Newton's thought* automatically followed a mode of thought and discovery that appears to be general amongst human beings. My thesis therefore is: The one-off-character of Newton's *mode of discovery* of the law of gravitation is subsumed in a broader class of the following general sequence: formal relations are discovered following the sequence *concrete-simple* → *concrete-composite* (Vervaet,1986,14-21/48-69).

In my demonstration of this general sequence I shall use evidence taken from experiments in the field of child cognition. However, a measure of fair warning is not out of place. I do not intend to compare Newton with a child any more than the expression '4:7=400:700' implies that '4' can be identified with '400.' The comparison concerns non-formal and formal operations in general, i.e. it concerns all such operations whether they occur in adults or in children.

Ontogenesis of a formal structure

Let us take as an example the seriation of 10 little sticks of unequal lengths (Fig 1b). Its formula is:

$$x_i > x_j \text{ if } i < j$$

i and j = 1, 2, 3, 4, 5, 6, 7, 8, 9, 10
x_i and x_j = *length of a stick.*

Children younger than 12 can not handle this formula and they are easily confused when they are confronted with a pseudo-formula for the seriation. One such a pseudo-formula is:

y = (10, 9, 8, 7, 6, 5, 4, 3, 2, 1) m
y = set of sticks
m = material (wood, iron etcetera).

The problem is: How does a child acquire the formula for the seriation of 10 sticks of unequal length? The research of the epistemologist Piaget (1942) (better, but less correctly known as a developmental psychologist; Bringuier, 1977) and replication research by myself both lead to the same sequence as the one we saw with Newton.

Children of about 5 or 6 take the longest and the shortest stick (say A and J) and place them next to each other; then they take the longest and the shortest stick from the remaining set (say B and I) and place them next to A-J, and so on. In this way, they reach the ordering of figure 1a.

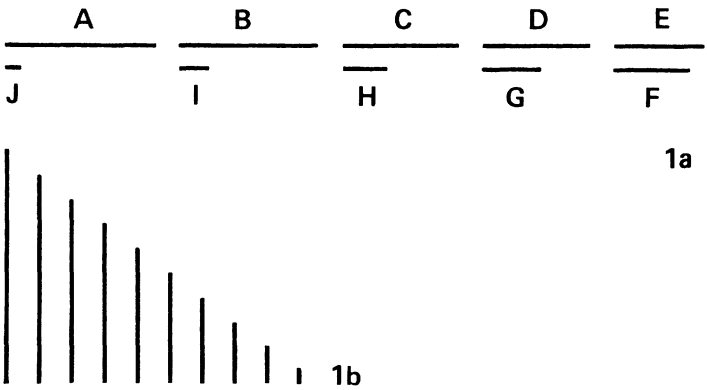

Fig. 1: Ten sticks of unequal lenghts are seriated in a 5x2-matrix (figure 1a) in a stage prior to the one in which they are seriated like in figure 1b.

Children of 7 and 8 are able to build the series like in figure 1b in one trial and without mistakes. The reason is that they can represent "X (e.g. B) is longer than Y (e.g. I)" and "Y is shorter than X" as the re-

verse of each other. Children of 5, 6 years of age, on the other hand, think here semi-relationally and semi-absolutely: that X is longer in this case is not a *relativum* with respect to Y, but a feature of X itself. Still younger children think of 'longness' as a characteristic of X, whereas 'longerness' *(sic recte)* is such for children of 5, 6 years of age. that is why children of the latter age (or better: 'stage') order X-Y correctly with respect to one another, not because "X>Y" and "Y<X" are each other's reverse, but because Y is associated unidirectionally with X's 'longerness' (or X with Y's 'shorterness').

We say that children of 7 to 8 have the ability to bring about *simple* systems (in this case, seriations on one variable).

On the other hand, children of 7 and 8 fail when confronted with the next problem (its solution is depicted in figure 2). I show them 16 different cards, randomly distributed on the table or on the floor. Each of them has one of these four lengths: 1, 2, 3, and 4 inches, and one of these four shades: white, light-pink, dark-pink and unmixed red. Each length-shade-combination occurs once. The question is: Put these cards in such a way that, when I peep through a slit between my hands I see four cards of the same colour, but changing from short to long or, when standing here (I turn 90°), four cards of the same length, but changing from white to red.

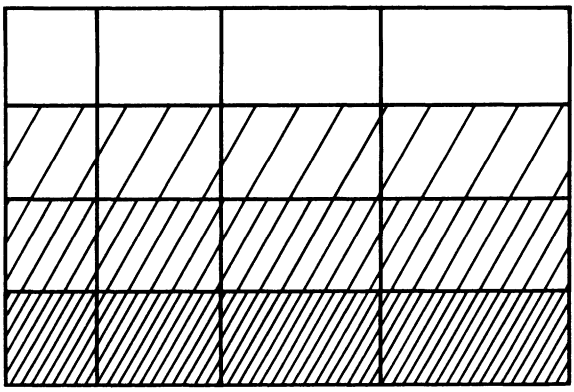

Fig. 2: The arrangement of 16 cards of four different lengths and four different shades of red (for more information, see the text).

Edward, aged 8, does not put the cards in a 4x4-matrix, but in one single row: first the white ones, then the light-pink ones and so on, within each shade always from short to long (figure 3a). I protest that I don't see four cards in a row then I look at them through a slit, but 16 cards when looking from one side and only one card when looking from the other side. Edward changes the positions of the cards. In fact he decomposes the row into two separate rows and partially reverses the order in ach of them: in one row the white cards run from short to long and the light-pink ones from long to short; in the second row the same with the dark-pink and the red cards (figure 3b).

Fig. 3: The 16 rectangular cards of figure 2 are represented as lines. First, Edward of 8 orders the 16 cards in one row (figure 3a) rather than in a 4x4-matrix (figure 2). Then he breaks up the row into two separate rows and reverses the order of the light-pink and of the red cards, so that he gets a 8x2-matrix (figure 3b).

Children of 9, 10 years old succeed in making the matrix of figure 2 and we say that they are able to bring about *composite* systems (in this case, seriations on more than one variable, simultaneously).

Only from 12 years onwards (but usually later in so far as they get no training in abstract thinking such as mathematics can children surpass *concrete* operations with simple and composite seriations in favor of *formal* operations. As has been said before, they choose the right formula out of a set of candidate-formulas for the seriation of 10 elements and their choice is not accidental, since it can be argued correctly. (Obviously, dealing with formulas is only one kind of formal operation.)

So, the sequence 'concrete, one variable → concrete, two variables → formal' fits in the general sequence that was seen with Newton: *concrete-simple → concrete-composite → formal.*

Discussion and Conclusion

In this paper I hope I have demonstrated a parallellism between Newton's discovery of the law of gravitation and the discovery of a formal law by children. Both sequences exemplify a characteristic of the human mind in general: Thinking goes through various stages: from operating reversibly and concretely with simple systems one acquires the competence to formalize them, but this competence is achieved by means of an intermediate stage of reversible operations with systems that are also concretely realized, but of a composite character.

Now, it is often pointed out that there is an essential incompatibility between the one-off-ness of history and the life of an individual on the one hand and the principle of replicability of science on the other. However, we have now seen that this gap can be bridged, viz. by focusing upon *structural features* rahter than upon particular *content.*

Other applications and consequences (e.g. the demarcation between pseudo- and authentic mathematizations, the synthesis of Piaget's genetic epistemology and Kuhn's paradigmatic theory) are discussed elsewhere (Vervaet,1986).

Amsterdam, The Netherlands

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I want to thank prof. B.J.T. Dobbs for directing my attention to a letter of Newton which serves as a pre-'concrete-simple' example of the general trend. For other reasons I had gone through some of the passages in note 14 in (Cohen,1980,314), but obviously not the one prof. Dobbs alluded to. However, the short span of time between the Newton Colloquium [of June 1987. Ed.] and the deadline of August 1 set by the editors of the *Proceedings,* did not allow me to check this.

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PART IV

NEWTON'S PHILOSOPHICAL HERITAGE

MORDECHAI FEINGOLD

PARTNERSHIP IN GLORY: NEWTON AND LOCKE THROUGH THE ENLIGHTENMENT AND BEYOND

Many names have been given to the eighteenth century: first and foremost the "Enlightenment," but also the "Century of Light," the "Age of Reason," and even the "Age of Newton." In view of the complex nature of the century, it is not surprising that such grand titles – and, more importantly, the unity of doctrine they imply – have been increasingly challenged. Which Enlightenment? What Reason? Whose Light? are just a few of the questions being posed by a new breed of historians determined to rid the eighteenth century of its traditionally one-dimensional interpretation. Certainly their probing has radically altered our perception of the period, giving it a depth and complexity missing a generation ago.

Naturally, such a shift has affected the historical role attributed to Newton – and John Locke – in the intellectual ferment that characterized the eighteenth century. Newtonianism, it is claimed, was not the sole ideology animating the period. Cartesianism remained very much alive – and influential – at least in the first half of the eighteenth century. Nor can the Leibnizians be ignored as a viable, competing "research program." Some recent historians have even denied that there existed a monolithic Newtonian party whose members shared a coherent, universally agreed upon philosophy of action. Instead, it is argued, the "Newtonians" were at best a loose assemblage of individuals whose writings and practices only vaguely resembled the canonical postulates of the Master.[1]

My purpose, however, is not to add to this growing body of literature that seeks to emphasize the eclecticism of the age and by so doing repudiate all claims for any significant unity. My on-going research on Newtonianism suggests that, despite the validity of much of the revisionist argument, with its emphasis on the diversity of allegiances and philosophies during the eighteenth century, there nevertheless existed an underlying cohesion. How can such a seeming contradiction be possible? The answer, I believe, can be found in the fact that we historians often confuse two levels of historical reality: our own "correct" appreciation of the ideas of Newton, and the way such ideas were perceived and assimilated during the eighteenth century. For this reason, regardless of the truth in

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much of the revisionist analysis, there nonetheless existed a powerful "image" of Newton – and to a lesser extent Locke – that informed the Enlightenment and inspired generations of scientists, philosophers and artists. For the English empiricists; for the Scottish Common Sense philosophers; for the French philosophes; and for the various members of the so-called "counter Enlightenment," Newton and Locke were first and foremost "emblems" of a new era. Whether they admired and sought to imitate the two great Englishmen or disagreed with and attacked them, the actual encounter was guided by their perception of all that they represented, which was a new point of departure for all future probes into the three major domains of human inquiry: man, nature and God.

Implicit in my argument is an amalgamated image of Newton and Locke. Such an image, it should be noted, runs counter to current revisionist Lockean scholarship, which rejects the common portrayal of Locke's "intellectual and philosophical attitude" as having been "molded by what he saw in the Newtonian achievement." Not only did Locke compose his *Essay* – and much else – independently of Newton, but, the argument continues, it was rather Newton who was the beneficiary of Locke's philosophy, proof of which can be found in the *Optics* and subsequent editions of the *Principia*.[2]

Again, for my purposes, this is hardly the point. The historical corrective is important and well taken, yet it is irrelevant to what the men and women of the enlightenment *thought about* the genealogy of ideas. And in this matter their beliefs are well-known. They had little doubt that Newton was the greater genius. Aware of the strong similarities between the ideas of the two Englishmen, they accepted Newton's earlier publication as indicative of Locke's indebtedness. Nor was Locke's effusive praise of the "incomparable Mr. Newton" lost upon a public that also noticed that such handsome compliments were never reciprocated by Newton. Thus, from the beginning there was general consensus about Locke's derivation of his methodology. Not that this was ever a cause for scorn. On the contrary. The popular perception concerning the congruity of their ideas became the basis of the very tradition eighteenth century men sought to perpetuate; they themselves could, and would, one day become the Lockes who would build on Newtonian foundations. Thus, for decades to come this perception would underlie what William Blake pejoratively termed the "single vision"; a compound of natural philosophy and metaphysics capable of serving as a universal doctrine for the attainment of truth. However, with the onset of the crisis of romanticism at the end of the eighteenth century, philosophers, scientists and men of letters began to consider anew this Newton-Locke relationship. As we shall see, when the time came for them to choose between the two, they

had little difficulty in siding with Newton, the person perceived as the originator of the new philosophy and the greater man.

The purpose of this article, therefore, is not to examine the actual content of Newton's and Locke's ideas, but rather to offer some preliminary reflections on the formation of such a powerful and pervasive image of a supreme paradigm, and the reasons for its eventual demise.

In a certain sense, it is possible to regard Locke as an early self-proclaimed recruit to the Newtonian camp. In the first edition of the *Essay*, Locke explicitly mentions Newton only once – in the "Epistle to the Reader" – but in a significant manner:

> The Commonwealth of Learning, is not at this time without Master-Builders, whose mighty Designs, in advancing the Sciences, will leave lasting Monuments to the Admiration of Posterity; But every one must not hope to be a BOYLE, or a SYDENHAM; and in an Age that produces such Masters, as the Great HUYGENIUS; and the incomparable MR. NEWTON, with some other of that strain; 'tis Ambition enough to be employed as an Under-Labourer in clearing Ground a little, and removing some of the Rubbish, that lies in the way to Knowledge.

By the next edition another important reference to Newton had been added by Locke: "Mr Newton, in his never enough to be admired Book, has demonstrated several Propositions, which are so many new Truths, before unknown to the World, and are farther Advances in Mathematical Knowledge." As Rogers points out, the reference was purely descriptive, intending only to illustrate a claim that had been made by Locke concerning maxims as early as the first edition of the *Essay*. Rogers proceeds to conclude that in no way was Locke interested in altering or accommodating his views to Newton's.[3] Without negating the validity of Rogers' textual analysis of the independent formulation of Locke's ideas from those contained in the *Principia*, a different conclusion may also be drawn. Locke, as is well known, read the *Principia* carefully within months of its publication, and read it again for the purpose of reviewing it for the *Bibliotheque Universelle & Historique*. There is no doubt that Locke was deeply impressed by the book, and not only because he found in it a validation of his own, similar, ideas. Rather, the astonishing success

achieved by Newton through his application of many of those shared principles deeply impressed Locke and made him fully aware of the immense potential impact of the *Principia* on men of science and the republic of letters at large. No doubt equally strong was the realization that readers of his own book – which was published two and a half years after Newton's – would also recognize the similarities in their respective methodologies. Hence, the flattering mention of Newton's name at the beginning of the first edition of the *Essay* may have been a conscious effort on the part of Locke to tie his star to Newton's, even if this meant sacrificing some of his own originality. In other words, by drawing explicit attention to Newton at the outset of his book, Locke was inviting his readers to draw the logical conclusion and grant his ideas the same reverence they had accorded to Newton's.

Obviously, such an hypothesis cannot be documented by chapter and verse. However, some circumstantial evidence lends credence to this argument. One may recall the similarity between the ideas of the two men, as well as the rapidity with which Newton's *Principia* was becoming to be considered a masterpiece. In contrast, an epitome of Locke's *Essay* met with a poor reception and widespread criticism upon its publication in 1688. To have a powerful ally in the war against "innate ideas" would certainly facilitate the reception of his epistemology. Be this as it may, the *Essay* did indeed become an instantaneous, and lasting, success. There were 20 separate English editions – in 29 printings – as well as 14 abridged versions before the end of the eighteenth century; 5 editions (in 15 printings) and 8 abridged versions in French – in addition to the six Latin editions during the same period. Certainly a most impressive record.

In the years following the publication of the *Essay*, Locke would not only become – at least for a time – a close friend of Newton, but he would continue to propagate Newton's ideas in his own works as well as to demonstrate the congruity of their world views. Quoted previously was Locke's mention of Newton in the second edition of the *Essay*. A few more instances may be cited. The most familiar appeared in the midst of Locke's controversy with Bishop Stillingfleet. Here he answered the Bishop's objection to the idea "that bodies can operate only by motion and impulses" with the claim:

> I am since convinced by the judicious Mr. Newton's incomparable book, that it is too bold a presumption to limit God's power, in this point by narrow conceptions. The gravitation of matter towards matter, by ways inconceivable to me, is not only a demonstration that God can, if he pleases, put into bo-

> dies, powers and ways of operation above what can be derived from our idea of body, or can be explained by what we know of matter, but also an unquestionable and everywhere visible instance, that he has done so.

Similarly, in his popular *Of the Conduct of the Understanding* Locke accepted the "admirable discovery of Mr. Newton, that all bodies gravitate to one another, which may be counted as the basis of natural philosophy" as one of the few fundamental truths that exist. In *Some Thoughts Concerning Education,* Locke again exalted "the incomparable Mr. Newton, [who] has shown, how far Mathematicks, applied to some Parts of Nature, may, upon Principles that Matter of Fact justifie, carry us in the knowledge of some, as I may so call them, particular Provinces of the Incomprehensible Universe."[4]

Apart from the specific references to Newton, Locke's repeated claims that mathematical certitude could be successfully applied to the domains of religion and ethics also facilitated the blending – in the public mind – of the Newtonian-Lockean philosophy. For example, having concluded in the *Essay* that he "doubt[s] not but it will be easily granted, that the *Knowledge* we may have of *Mathematical Truths,* is not only certain, but real *Knowledge*," Locke proceeds to argue that "hence it follows, that *moral Knowledge* is as *capable of real Certainty,* as Mathematicks." In the same manner, he would argue for our knowledge of the existence of God: "[T]hough this be the most obvious Truth that Reason discovers; and though its Evidence be (if I mistake not) equal to mathematical Certainty: yet it requires Thought and Attention."[5]

Such pronouncements drew much attention. As early as 1692 Locke's friend Molyneaux wished that he "would think of Obleidging the World, with a Treatise of Morals, drawn up according to the Hints [he] frequently give[s] in [His] Essay. Of their Being Demonstrable according to the Mathematical Method." Locke's reply is instructive:

> Though by the view I had of moral ideas, whilst I was considering that subject, I thought I saw that morality might be demonstratively made out, yet whether I am able so to make it out is another question. Every one could not have demonstrated what Mr. Newton's book hath shewn to be demonstrable.[6]

Newton, on the other hand, was less specific and made fewer direct references about the applicability of his principles to other domains. In his preface to the first edition of the *Principia* he merely stated: "I wish we

could derive the rest of the phaenomena of Nature by the same kind of reasoning from mechanical principles... I hope the principles here laid down will afford some light either to that or some truer, method of Philosophy." Two decades later, in the *Optics*, he was much bolder: "[I]f natural Philosophy in all its Parts, by pursuing this Method, shall at length be perfected, the Bounds of Moral Philosophy will be also enlarged."[7]

Yet Newton's disciples compensated generously for the master's reticence about how the principles could be introduced into domains other than natural philosophy. Both in their independent work and in their programmatic statements, these disciples would elaborate on the applicability of the principles in a manner that for contemporaries was representative of Newton's own beliefs. Furthermore, in giving voice to what remained just inferences in Newton, these disciples also incorporated into the Newtonian structure Lockean terms and concepts, thereby enhancing the perceived congruity of ideas. Thus, for example, Henry Pemberton's *A View of Sir Isaac Newton's Philosophy* was not only a lucid popularization of Newton's work, but also carried a distinct flavor of the epistemology of that "eminent philosopher" John Locke. Other Newtonians combined the two in the domain of religion. Samuel Clarke, for example, united "Locke's strong sense and Newton's piercing wit" – to quote Matthew Tomlinson's "The Trinity" – to create a new mathematical theology to combat deism and atheism. His friend Abraham de Moivre went a step further, inventing an ingenious "curve of error" according to which it would be possible to demonstrate that the irregularities in nature provide a high probabilistic level of certitude to the existence of God. Small wonder, then, that the Newtonian most faithful to Newton, Maclaurin, could pronounce in his *An Account of Sir Isaac Newton's Philosophical Discoveries* that "natural philosophy is subservient to purposes of a higher kind, and is chiefly to be valued as it lays a sure foundation for natural religion and moral philosophy."[8]

That contemporaries were quick to appreciate the parallels between Newton's physics and Locke's philosophy can be seen from the following two examples. In 1692, Richard Bentley began delivering his famous Boyle lectures. Even before he embarked on his celebrated correspondence with Newton, however, Bentley was utilizing ideas derived from both the *Principia* and the *Essay*. By 1697, when Locke became involved in various religious controversies, William Molyneaux used such Bentleian statements in order to demonstrate to Locke how his ideas had taken hold among scholars and churchmen. Both Bentley in his sermons, wrote Molyneaux, and another Newtonian, William Whiston in his dedication of *New Theory of the Earth,* adopted Locke's doctrine that "Idea Dei non est Innata." In addition, these authors agreed with Locke that "Phae-

nomenon of Gravitation cannot be Accounted for by Meer Matter and Motion, but seems an Immediate Law of the Divine Will so ordering it."[9] In view of Locke's own previously quoted statement on this issue, it is clear that by the later 1690s contemporaries had already begun to hopelessly mix up the ideas of Newton and Locke.

Not only Englishmen confused the ideas of one with the other. By the middle of the 1690s Leibniz also forged in his mind the inseparable link between the two which, as Jolly rightly observed, would be significant in shaping his later critique of Locke. It is also quite possible, as Aarslef claimed, that Locke's reluctance to enter into a philosophical correspondence with Leibniz – as well as his disparaging remarks about the German philosopher – was an early sign of the drawing of lines in the famous Newton-Leibniz controversy. At any rate, by the 1710's in his letters to Princess Caroline, Leibniz was expicitly accusing Locke (who was by now dead) and the Newtonian party of being the bearers of the decay of natural religion in England, an accusation that precipitated the famous Leibniz-Clarke correspondence.[10]

Finally, certain anecdotes also contributed their share to blurring Newton and Locke in the minds of eighteenth-century men of letters. One such anecdote was related by Pierre Coste, an intimate friend of Locke and Newton, and the translator of both men's books into French. According to Coste, Locke refused to reveal even to him the source of an intriguing passage in the *Essay* concerning the formation of matter. Only years later did he learn accidentally from Newton that it was he (Newton) who had provided Locke with the explanation. Such an anecdote, together with the public knowledge of the close personal ties between Newton an Locke, must have strengthened the perception of a collaboration between the two, as well as the notion of Locke's indebtedness.[11]

No less important for the creation of that "single vision" were the extensive efforts of popularizers and poets to spread the glory of the two great Englishmen. As early as 1698 the critic John Dennis exclaimed:

> What Proficients have we in Philosophy? What in Mathematicks? Let all Europe reply, who had read, and, reading, admir'd them. I shall content myself with mentioning Two of the living Glories of England, Mr. Newton, and Mr. Lock; the one of which has not his Equal in Europe, and neither of them has His Superior.

Two years later a less important literary figure, Timothy Nourse, provided a more passionate plea for their glory in his *Compania Faelix:*

> Now I explore, with Greedy Eye,
> The Depths of rich Philosophy;
> Where, with new Stores of Knowledge fraught,
> Locke paints the various Maze of Thought;
> Now mount above each Ancient's Throne,
> Where Phoenix Newton sits alone;
> Strange Truths unfolding in each Page,
> To the convinc'd, yet wond'ring Age,
> Of stated Laws for every Sphere,
> And Nature wildly regular.

Two decades later William Pattison acknowledged in his *Morning Contemplation* his gratitude to the poets for inspiring his love of nature, but it was Newton and Locke who "made plain to him all the secrets of nature's works."[12]

The transmission to the Continent of this legacy began long before Voltaire's celebrated apotheosis. Many foreign visitors – like Voltaire – carried their admiration for matters English back with them, or transmitted it through their letters, as did Nicholas Fatio de Duillier. One such early popularizer was Michel la Vassor who wrote a correspondent in 1714 that "the learned men of England... combine here the discourses of a famous mathematician of this country, named Sir Isaac Newton, and the views of a certain Mr. Locke, who has written on the nature and operation of the soul." Shortly thereafter there emerged another champion of Newton and Locke, the abbé Conti, who promoted the European knowledge of the two men both through his elaborate personal connections as well as through his writings. And to his efforts we may add the good services of the exiled Englishman, the Viscount Bolingbroke. Undoubtedly, however, it was Voltaire who – more than anyone else – became the greatest advocate of the English duomvirate on the Continent, both propagating their ideas as well as entrenching the public perceptions of their virtually single identity.[13]

Nor was such a message confined to the medium of print. The visual images of Newton and Locke – in the form of busts and paintings – adorned the libraries and embellished the gardens of both members of the aristocracy and men of letters. Queen Caroline's Hermitage at Richmond, for example, included the busts of Newton and Locke among the five worthies (the others being Clarke, Wollaston and Boyle), and an anonymous lady provided the following explication of these busts:

> Reason, that emanation of the mind,
> Breaks forth in Locke, diffusive, and refin'd...
> Newton, th' all-wise Creator's works explores:
> Sublimely on the wings of knowledge, soars;
> Th' establish'd order of each orb unfolds,
> And th' omni-present God, in all, beholds.

Lord Cobham's Grotto at Stowe and Pope's home at Twickenham were endowed with similar busts. Both D'Alembert and Jefferson displayed their portraits in their studies; busts of the two men were also to be found in Gibbon's study at Lausanne and in the study of the poet Edward Young. To each the poet attached a motto: Newton's reads "Hic Natura Clavis est," Locke's "Hic Hominis ostendit Tibi Te."[14]

The significance of this fusion of identities goes beyond a mere historical curiosity. As an image it permeated the entire structure of the Enlightenment and the reaction that followed it. It is a truism that the eighteenth century saw the inauguration of the new science of Man to which the two Englishmen contributed so much, directly or indirectly. Previously quoted are Locke's remarks concerning the possibility – and desireability – of modeling the moral sciences and religion on the firm foundations of the new [Newtonian] science as well as intimations to the same effect by Newton and his disciples. However, for Locke and for most men of the eighteenth century, moral philosophy took precedence over the sciences. This conviction of Locke is articulated in the celebrated couplet of Alexander Pope, the message of which served as the battle cry of the Enlightenment:

> Know then thyself, presume not God to scan;
> The proper study of Mankind is Man.[15]

Locke, in fact, anticipated David Hume's position that all knowledge derives, in the end, from the science of man:

> 'Tis evident that all the sciences have a relation, greater or less, to human nature, and that, however wide any of them may seem to run from it, they still return back by one passage or another. Even Mathematics, Natural Philosophy, and Natural Religion.

Diderot echoed the same sentiment in the *Encyclopédie*: "Man is the single place from which we must begin and to which we must refer everything."[16]

It is beyond the scope of this article to discuss the ways in which the study of Man – his soul, moral character, social institutions, economy and government – dominated the eighteenth century. For our purposes, it should be stressed that the new edifice was built on the foundation laid by Newton and Locke, in the sense that it was Locke's concerns that were set upon Newtonian footing. Nor does it matter whether they adopted the methodology – real or imagined – or the mathematics or the theology of Newton. Indeed, some only bothered to borrow his name. But this is precisely the point. The exact nature of the debt of the Enlightenment to Newton – with or without its Lockean aspect – can be evaluated only once it is realized that the ideas and reputation of Newton could be – and were – appropriated by most anyone seeking an authority to validate his speculations. The fact that someone like the Baron d'Holbach freely invoked the name of Newton despite his total lack of understanding of the Master in no way negates the import of d'Holbach's statements for modern historians. Just the opposite. It demonstrates how pervasive the Newtonian-Lockean influence turned out to be, making it impossible for anybody to escape the web of the "single vision." The men of the eighteenth and early nineteenth century could adopt such a vision, amend it, elaborate on it; they could pick parts from it or demean it; they could even reject it outright; but they could not ignore it. Even Goethe – one of the most violent opponents of Newton and all that he represented – still gave the Englishman the supreme compliment when he refused to view his greatness as a literary figure: "greater poets," he wrote a correspondent shortly before his death, "have lived before me; and others will come afterward. But that I am the only person in my century who knows the truth in the difficult science of colors – of that, I admit, I am not a little proud, and here I am conscious of superiority to others."[17]

By the beginning of the nineteenth century the symbiotic relationship between the two became something of a truism. In 1807, for example, Saint Simon fancied an imaginary dialogue between Newton and Locke in which the two conjured to divide among themselves the Cartesian empire, Newton the physical world, Locke the science of man. A decade later, Coleridge delivered in London a series of philosophical lectures, shrewdly observing in his penultimate lecture that Newton and Locke enjoyed the spoils of the mechanical philosophy and the metaphysics of Descartes: "Therefore one great man had overthrown the physical parts and another the metaphysical parts and to that circumstance we owe entirely the custom of talking of Locke and Newton."[18]

Such a harmonious view, however, should not obscure the fact that by the second half of the eighteenth century some major cracks had begun to appear in this facade. And since Locke's reputation owed a great deal to the perceived similitude of his ideas to those of Newton, once the "protective shield" of Newton was removed, Locke's fortunes began to decline. By the nineteenth century Locke's reputation was to reach a low ebb, while Newton's was to continue hardly affected. Three major factors contributed to the erosion of the appeal previously enjoyed by the so-called amalgamated philosophy, and the subsequent split in this powerful alliance. The first may be termed a natural process of cultural-intellectual maturation – the rise of a new climate of opinion that challenged many Lockean assumptions and theories. The second factor was the Romantic reaction, the revolt of men of feeling against the prophets of the age of reason. And the third factor, closely related to the Romantic rebellion, was a growing aversion to the materialistic and atheistic overtones of Newtonian-Lockean philosophy.

Perry Miller remarked shrewdly that "[m]en of the early eighteenth century were not so much the beneficiaries of Locke as they were his prisoners."[19] And indeed the half century following the publication of the *Essay* was conspicuous not only for the close alliance between Newtonian science and Lockean philosophy, but also for a virtual absence of any epistemology capable of challenging Locke's "way of ideas." However, the same fervor that made the study of man the battle cry of the eighteenth century would eventually produce an equally fervent reaction. The quarter of a century after 1739 – which marked the appearance of the firt two volumes of David Hume's magisterial *A Treatise of Human Nature* – witnessed the publication of a series of seminal works in the fields of epistemology, psychology and moral philosophy. Works by Condillac, Hartley, Bonnet, Reid and Kames – to name only a few authors – appeared in rapid succession, contributing in various ways to the radical changes that took place in such disciplines. And when at long last Leibniz' *Nouveau essais* was published in 1765, the stage was set for the rapid deterioration of Locke's reputation.

Such a turn of events, however, posed a major dilemma. After decades in which the names and ideas of Newton and Locke were intricately entangled, it was impossible to simply unravel the knot of ideas and personalities, and discard Locke. For as was mentioned previously, the alliance between the ideas of the Englishmen proved so successful precisely because it provided at least the impression that the science of nature and the sciences of man were part of the same edifice, and beneficiaries of the same methodology and modes of investigation. Put simply, then, given the seeming unity in their underlying principles, the problem be-

came, "How could Lockean epistemology be discarded without affecting Newtonian science?" This indeed was the quandary that faced Kant. His quest for the affinity between the "methodology of scientific knowledge and that of metaphysics" benefitted greatly from the publication in 1765 of the *Oeuvres philosophiques* of Leibniz – which included the *Nouveaux essais* – and Kant immediately sided with Leibniz against Locke. However, since Leibniz's philosophy also contained some fundamental incongruities with Newtonian physics and metaphysics, the question became whether it, too, would necessitate the abandoning of Newton.[20] In the end Kant managed to retain Newtonian science by elaborating upon Euler's solution. But the dilemma was genuine and one that many philosophers would grapple with as they proceeded to dethrone Locke from his position of supreme authority, leaving him only the distinction of being – at best – a pioneer. Thus, in 1784 Herder could boast: "Locke now concerns us only so far as he served the truth, and we have already for a long time, owing to Leibniz, been accustomed also to find weaknesses in his philosophy." Joseph de Maistre perhaps best articulated the new view: "In philosophy contempt for Locke is the beginning of wisdom."[21]

In England it may not have been necessary to wait for the publication of Leibniz's critique, for Hume had already anticipated Kant in suggesting a way of preserving Newton's physics while discarding, at least in part, Lockean philosophy. Nevertheless, Kant's "problem" bothered the English as well. Even more than the Germans, they desired to secure their adoration of Newton, and in view of the changing fortunes of Locke's reputation, they wished to dissociate the two. Coleridge – who disliked Newton as much as he did Locke – nonetheless wished that Sir Isaac might be given "a more worthy associate – & instead of Locke and Newton, we should say, Bacon & Newton, or still better perhaps, Newton and Hartley." His attitude was certainly shared by William Whewell who, unlike Coleridge, admired Newton, yet was a fierce opponent of Lockean ideas. Shortly after he was elected Professor of Moral Philosophy at Cambridge in 1839, Whewell wrote a correspondent in London that he had "already used [his] influence to introduce an Anti-Lockean philosophy, and intend[ed] to use it for other good purposes." Andrew Lang, on the other hand, perhaps best represented the more "popular" view. Commenting on a new printing of the *Essay* in 1828, he worte: "I never read one word of Mr Locke, but how did the dreary devil stagger like Crockett to a 26th edition?"[22]

The proliferation of alternatives to the Lockean rendering of "man" "ideas" and "society" that had set in by the second half of the eighteenth century also helped fuel the so-called Romantic revolt. Again, it is impossible to do justice to the many facets involved in the movement, and I

shall comment briefly on only two aspects of the revolt: th romantic concern with sensibilities and religion, both of which were deemed lacking in Newton and Locke.

Neither Newton nor Locke were celebrated for their appreciation of poetry or literary matters in general. Indeed, well-known was Newton's aphorism that poetry was "ingenious nonsense" as well as Locke's censure of the poetic enterprise. Charles Brown – one of the Keats circle – could freely rail against the "men of science" who were known to be against poets "from Sir Isaac Newton downwards." Similarly, in one of his imaginary conversations, Landor made the Earl of Chatham ask the Earl of Chesterfield: "And what would you think... if you discovered in Newton a sentence in the spirit of Shakespeare?" Chesterfield's answer was obvious: "I should look upon it as upon a wonder, not to say a miracle. Newton, like Barrow, had no feeling or respect for poetry."[23] Clearly, then, the suspicion of the imagination exhibited by Newton and Locke made them a perfect target for the romantics who accused their philosophy of having stifled creativity. And subsequently, when the romantics came to establish criteria for man in relation to his mind, imagination and nature, they challenged the restrictions imposed by rationalism.

However, most interesting for our purposes was the application of a double standard of sorts vis-a-vis their ultimate judgment of Newton and Locke. For as suspicious as they were of what clearly was the Newtonian heritage, the romantics still could not ignore Newton's genius, for they themselves created the cult of genius. Thus, although there certainly existed a general hostility toward the legacy of the Enlightenment, Newton escaped the brunt of the condemnation. For example, Wordsworth viewed Newton as an embodiment of "good" science (and in so doing brought down on himself the wrath of Coleridge). His reflection upon watching Newton's statue at Trinity College, Cambridge, is well known:

> The marble index of a mind for ever
> Voyaging through strange seas of Thought, alone.

The notable exception to this lenient attitude toward Newton was Blake, whose poetry and prose is filled with rage and hatred toward both Newton and Locke. For him the duomvirate – or, better yet, the unholy trinity of Bacon, Newton and Locke – was responsible for the destruction of England. In one place, he named the two as culprits who corrupted the rules of taste and style formulated by Sir Joshua Reynolds and the Royal Academy. In *Jerusalem* he expressed his feelings even more poignantly:[25]

> I turn my eyes to the Schools & Universities of Europe
> And there behold the Loom of Locke whose Woof rages dire
> Washed by the Water-wheels of Newton. black the cloth
> In heavy wreathes folds over every Nation; cruel Works
> Of many Wheels I view, wheel without wheel, with cogs tyrannic
> Moving by compulsion each other: not as those in Eden: which
> Wheel within Wheel in freedom revolve in harmony & peace

Blake's vision of evil, however, did not take hold or inspire his contemporaries; (It was left to Yeats to add that "Descartes, Locke, and Newton took away the world and gave us its excrements instead."[26]) And Newton's reputation survived the revolt of the poets. Clearly, in an age that came to attach almost mystical significance to genius, even Newton's opponents were trapped by their own admiration of his achievement. Not so poor Locke.

A similar double-standard was applied in the domain of religion as well. During the first half of the eighteenth century, the poets immortalized Newton and Locke for having produced the best possible defense of religion against deists and atheists. But there already began to emerge a minority opinion that doubted whether "demonstrative" religion might not better serve the enemy. Not that these men doubted the sincerity of either Newton's or Locke's beliefs in the deity. Rather, they feared that rationalism and empiricism would eventually dispense with plain faith. Moreover, the views of Locke and Newton equally lent themselves to the purposes of the Deists, who were quick to invoke their names in their support. For example, as early as 1698, in his "Life of Milton" Toland lavishly praised Locke

> Who in his Book of Human Understanding must be confest to be the greatest Philosopher after Cicero in the World; for he's perfectly acquainted with human nature, well vers'd in the useful Affairs of the World, a great Master of Eloquence (Qualities in which the Roman Consul excel'd) and like him also a hearty lover of his Country, as appears by his Treatises of Government and Education, not inferior in their kind to the divinest Pieces of Tully.

Six years later, in his *Letters to Serena* Toland continued to parade himself as a true disciple of the "incomparable Mr. Lock," while elsewhere he would paraphrase the familiar metaphor from Locke's preface to the *Essay*, ascribing to himself the role of one who "clear[s] away the rubbish that inhibits the attainment of clarity in knowledge."[27]

Nor was Locke alone to reap these double-edged compliments. As befitting the age, Toland appropriated Newton for his own purposes as well. In *Letters to Serena* he quotes Newton a few times, once admonishing philosophers to follow Newton's advice and acquaint themselves "with the Observations and Facts" of mathematicians prior to searching for physical causes. Elsewhere Toland claims that Newton's views on Time and Space were "favourable" to his interpretation, just as he (Toland) had elaborated on Newton's opinion concerning "the general or moving Force of all Matter." Finally, Toland paraphrases Newton when he denounces hypotheses: "Since PROBABILITY is not KNOWLEDG, I banish all Hypotheses from my Philosophy."[28]

Small wonder, then, that invariably the early critics of Newton and Locke were motivated – at least in part – by their fear of the fate of "true" religion. Such was the case with Bishop Berkeley whose searching criticism of the infinitesimal calculus, of optics and of epistemology, was directed at both Newton and Locke. Similarly, the attempt to defend religion was the motive animating the opposition of Cambridge scholar Robert Green to Newton and Locke – who along with Hobbes and Spinoza – were classed as inducements to materialism and atheism; certainly, such fear of the religious consequences stood behind much of the Hutchinsonian movement. Similar overtones can be found within the Methodist movement as well as in certain segments of the parallel revivalism of the Anglican Church.[29] By the turn of the nineteenth century, the religious bias against Newtonian-Lockean philosophy joined forces with the romantic rebellion to vindicate simple faith. Thus, it is no coincidence that Blake found in Berkeley a predecessor to be admired. But again, Newton's reputation was not seriously affected, even in the face of the religious onslaught. Certainly, by the mid-nineteenth century a much more "dangerous" enemy to religion had emerged – evolution – and it seemed counter productive to concentrate on Newton who, after all, defended a universe with an interventionist deity.

I hope I have been able to justify the title of this article. Whether or not John Locke can be said to have masterminded the formation of what has been referred to as a "single vision," the two Englishmen nonetheless became partners in glory. Together they were considered to be the rightful claimants to the ambitious empire that had eluded Descartes, and together they exerted an immense influence upon European intellectual thought for decades – if not centuries. But Locke was, ultimately, the weaker link in this partnership, for his domain of inquiry – human mind and nature – was far more demanding. Man, in the end, refused to be contained in orderly, clear-cut categories, as both men of feeling and proponents of revealed religion were rapidly demonstrating. And

fused to be contained in orderly, clear-cut categories, as both men of feeling and proponents of revealed religion were rapidly demonstrating. And when philosophers, romantics and defenders of revealed religion all discovered reasons to rise against the new dogma that was born in England c. 1690, they found it both possible and expedient to dissolve the holy alliance.

Boston University, U.S.A.

NOTES

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9. *The Correspondence of John Locke,* V. 702. See also VI. 38.
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MILIC CAPEK

WHAT SURVIVES FROM THE CLASSICAL CONCEPT OF ABSOLUTE TIME

The title of this paper may be understood as a form of question which is only in part a question of intellectual history. It cannot be answered by a mere outline of the post-Newtonian development of the concept of time; what is needed is also to contrast the modern concept of time with its classical counterpart and to analyze whether some residue of the latter is not perhaps present, no matter how disguised, in the concept which is generally accepted today. I do not mean only the time of relativity; there is a similar problem of time in thermodynamics, in quantum theory and in cosmogony, even though the one in relativity is probably the most fundamental.

The prevailing theory today is the *relational theory* in contrast to the previous, now abandoned, absolutist theory. The latter is usually connected with the name of Newton, even though its acceptance in the seventeenth century was really a culmination of a longer protracted struggle with the antagonist relational theory. A brief retrospective glance at this struggle will help us to understand better the nature of two different view of time.

The most conspicuous difference between the relational and absolutist theory is that the former insists on the *inseparability of time from its content,* i.e. from changes and/or motions. In this respect the relational theory had hardly any rival from the beginning of Western thought until the late Middle Ages. The Greek atomists, as Einstein correctly recognized, came very close to the absolutist view of space as an unchanging container of all material bodies, existing in itself and independently of them; but at the same time, as we know it from the famous and frequently quoted passage of Lucretius, they refused to regard time in a similar way. The author of *De rerum natura,* following on this point Epicurus, insisted that "time has no being by itself" ("tempus item per se non est"), i.e. it does not exist independently of the motions of the bodies. The cosmology of Aristotle which prevailed during the Middle Ages and which was so radically opposed to the atomistic theory, nevertheless shared with it the relational view of time; Aristotle agreed with Lucretius that time is "nothing in itself," being inseparable from changes (even though he did not believe that all changes are reducible to displacements). According to him, outside of the celestial sphere of the fixed

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stars, there is neither space nor time. Time was, so to speak, *embodied* in the regular rotational motion of the last celestial sphere which represented *the absolute cosmic clock,* the only truly uniformly running clock in the universe; its motion by its privileged character and its absolute uniformity was an empirical manifestation of the unity and the metrical homogeneity of the objective "flow of time." Like Newton's time, the time of Aristotle was *one* and *universal;* despite the enormous differences in their cosmologies, it was one point on which they agreed. It was the only point of agreement – but an important one.

There were difficulties in this view even before the celestial clock (i.e. the sphere of the fixed stars) was removed by Giordano Bruno at the end of the sixteenth century. The fact of the precessional motion, known already to the Greek astronomers, made it necessary to postulate another sphere beyond the eighth sphere; only that sphere – and not the sphere of the fixed stars – had the truly uniform motion. Since the Arabian astronomers doubted the uniform rotation of that additional sphere and thus were led to postulate still another – the tenth – sphere which would have the truly uniform rotation, the question was naturally asked whether this frustrating search for "the truly uniform motion" would ever end; in other words, whether such universal cosmic clock exists at all. The question was definitively settled with the acceptance of the infinite unbounded universe.[1]

But if there is no universal cosmic clock, is it still meaningful to speak of uniformly flowing time? Two answers were possible: one which was negative and which was unavoidable within the framework of a consistent relational theory; there is no single universal time since there are as many times as there are motions – *tot tempora quot motus* (Giordano Bruno); in other words, since there is no privileged uniformly rotating celestial body, there are as many times as there are stars each of which is rotating at a different rate: *tot tempora quot astra.* These words indicate how much Bruno was leaning toward the relational theory of time; but at the same time, he was aware of the second possible answer – to preserve the unity of time by making it *independent of any motion:*

> Now if it happened that all things are at rest, would this mean that they would not endure? Indeed, they would endure, *they would all endure by one and the same duration* (Italics added.)[2]

Obviously, Bruno hesitated in a manner characteristic of the transitional and intermediate periods, between the relational and absolutist theory of time. Similar hesitations are still present in Gassendi as long as he

remained under the influence of Lucretius; yet, as early as in his polemic against Descartes in 1644, he restated very clearly Bruno's anticipation of Newton's absolutist theory:

> Whether things exist or not, whether they move or are at rest, time always flows at an equal rate.[3]

This sentence occurs almost *verbatim* in the passage of Newton's tutor Isaac Barrow:

> Whether things move on, or stand still; whether we sleep or wake, Time flows perpetually with an equal tenor.[4]

Newton used the same words when, in his famous scholium, he characterized "absolute, true and mathematical time, of itself and from its nature" as flowing "equably without relation to anything external," i.e. independently of any concrete physical motion.

The expression "flow" has been much criticized recently. It is a visual metaphor, borrowed from kinematics; and to stress the obvious fact that it cannot be taken literally is one of the current fashionable pedantries. The fact, however, that it has been used again and again for more than two millennia – from Heraclitus to William James – must have some significance which cannot be ignored. It conveys, no matter how imperfectly, one essential feature of time: its *irreversibility,* the absence of symmetry between its successive moments. Furthermore, in the passage immediately following the quotation above, Newton proposed a far more appropriate term – *duration.* Every temporal interval by its own nature *endures* unlike spatial intervals of which all points are simultaneous or *co-instantaneous.* Newton obviously avoided a very common confusion of time with its static, geometrical symbol.

As mentioned above, Newton shared with the medieval cosmology the belief in one, universal, evenly flowing time; but, according to the latter, such universal time was inseparable from motion, being, so to speak, embodied in the real, uniformly running cosmic clock; while Newton, following in this respect Grazadei d'Ascoli and Nicolas Bonet, freed his time from the connection with any material clock. He had one simple reason for it: "the opening of the universe" – to use the expression of Alexander Koyré – swept away the last remnants of the celestial clockwork. Newton was very much aware of this difficulty; in his scholium he explicitly conceded that maybe "there is no such thing as an equable motion, whereby time may be accurately measured. All motions may be accelerated and retarded, but the flowing of absolute time is not liable to

any change. The duration and perseverance of the existence of things remains the same, whether the motions are swift or slow, or none at all..." In other words, we may say that he preserved the universal time by *dematerializing* it; his objective, physically empty and even-flowing duration is nothing but an immaterial cosmic clock. He assured its absolute character by making it one of the divine attributes; as he says in the concluding part of his *Principia,* time is a manifestation of God's everlasting duration: "this duration reaches from eternity to eternity." He had a similar approach to the problem of space which, like time, cannot be an object of sensory perception, but, though invisible and imperceptible, nevertheless exists independently of the material bodies which it contains; it is a manifestation of the divine omnipresence, *sensorium dei.*

Yet, Newton, because of his repeated emphasis on the empirical approach to physics, felt rather uncomfortable with his own acceptance of such extra-sensory entities such as space and time; this is why he tried to establish at least the existence of absolute space by his well known "rotating bucket experiment." It is equally well known that his alleged proof is generally viewed as unconvincing: it was criticized already by George Berkeley and more effectively by J.B. Stallo and Ernest Mach. All these critics agreed that the centrifugal forces arise *with respect to the great stellar masses* and not necessarily with respect to absolute space; Mach pointed out that we do not know whether they would not arise even during the so-called "relative rotations," if the walls of the vessel were made several miles thick.[5] Less known is the criticism of Roger Boscovich in 1755 when he pointed out that the experiment would give the same result in systems moving with a constant velocity with respect to the original one and thus would *not* show which of an infinite number of such inertial systems would coincide with absolute space.[6] Significantly, Newton did not perform nor did he propose a similar experiment intended to establish the existence of absolute time. He believed, however, that its existence, while not being directly perceived, can be *deduced* when he wrote in the same scholium: "therefore this duration ought to be distinguished from what are only sensible measures thereof; and from which we *deduce* it by means of the astronomical equation. The necessity of this equation, for determining the times of a phenomenon, is evinced as well from the experiments from the pendulum clock, as by eclipses of the satellites of Jupiter."[7]

There is then a reference to experiments after all, even though the emphasis was on *deduction* of absolute duration from them. At that time, when the questions of methodology were far from being clarified, and when Hume's analysis of "inductive leap" was still far in the future, it was natural that Newton was unaware that no absolutely certain gen-

eralizations can be obtained from empirical observations; consequently, that there is no way by which absolute uniformity of duration can be "deduced" from pendulum experiments. He failed to see that, in classical physics at least, the homogeneity of time is the precondition of measurements and not *vice versa*. Furthermore, there is one particular difficulty concerning the measure of time of which John Locke, Newton's contemporary and philosophical disciple, was perfectly aware. He pointed out that "two successive lengths of duration, however measured, can never be demonstrated to be equal"[8] for one simple reason that two temporal intervals – unlike two geometrical segments – can never be superposed upon each other because of their *successive* character; consequently, no abolutely reliable quantitative comparisons could be made. If Locke in the same paragraph wrote that "Duration in itself, is to be considered as going on in one constant, equal, uniform course" – it only shows how irresistibly the concept of homogeneous time imposed itself even on the critical minds. In Locke's own mind the critical doubts about the possibility of time-measurements existed alongside his Newtonian orthodoxy. By his doubts he anticipated the future criticism of such widely different thinkers as Bergson, Poincaré and Russell: Bergson, who pointed out that when we believe to measure time, we are really *counting simultaneities*, i.e. instantaneous coincidences of the clock-hand with a certain point on the dial; Henri Poincaré, who restated and expanded Locke's argument that we do not have a direct intuition of the equality of two successive intervals of time;[9] finally Bertrand Russell who put it in an elegant form in saying that "the axiom of free mobility," according to which any geometrical figure can be moved freely in space without being altered in size or shape, does not apply to the time intervals which are not superposable.[10]

In 1870 C. Neumann attempted to remove all ambiguity from the concept of absolute time by trying to define the equality of time-intervals: "Two material particles in inertial motion endure through the same interval of time when equal trajectories of one correspond to equal trajectories of another." In other words, Newton's absolute time would be thus embodied in what may be called "inertial clockwork." (This would apparently make Newtonian time relational in a sense; it would still be uniform, but not disembodied. But if we keep in mind that such "inertial clock" came into existence with the creation of the universe while time, being synonymous with the beginningless divine duration, existed *before* creation – as Gassendi in particular emphasized – the true significance of the absolutist theory of time and of its independence of its physical content will clearly stand out.) But there were several difficulties in Neumann's definitions. First, there are no purely inertial motions. Even if we would postulate hypothetically only two existing particles, their

gravitational interaction – no matter how small – would destroy the strictly inertial character of their motions. Second, even in the idealized situation of two inertial motions, what we would perceive would be the equality of *trajectories,* not the equality of times; the latter would be *defined* by the former, never directly perceived. Thus the basic objections of Bergson, Poincaré and Russell about the impossibility of superposing successive intervals of time – and therefore the immeasurability – has not satisfactorily been answered.[11]

Nevertheless, classical physics ignored these difficulties and retained the Newtonian concept of the universal, metrically uniform time which was challenged only by the coming of relativity. This was less the case for special relativity since the so-called "dilatations of time" are merely referential, perspective-like distortions which do not affect the identity of the local ("proper") times. But in general relativity such equality is not preserved – the fact strikingly illustrated by the actual lengthening of proper times in the gravitation/acceleration fields and in the asymmetrical aging in the so-called "paradox of twins." But does it mean that nothing whatever remains of the Newtonian concept of universal time?

In order to answer this question we must take into account that the relativistic revision of the concept of time was a part of a far wider conceptual change which affected classical physics. The significance of this revolutionary change lies in the contrast between the classical transformation of Galileo and the relativistic transformation of Lorentz; more graphically, in the difference between the classical concept of space-time and the relativistic (Minkowski's) space-time. It is true that the term "space-time" had hardly been used before Einstein; yet, there is no question that space and time were related in a definite way in classical physics and thus we can meaningfully speak of the *prerelativistic* concept of space-time;[12] furthermore, it may be said that an attentive analysis of this classical concept will bring into focus the contrasting features of its relativistic counterpart. The classical space-time can be defined as a continuous succession of instantaneous spaces. Each instantaneous space can be defined as a three-dimensional cross-section in the four-dimensional space-time continuum; it represents what Eddington called a "world-wide instant," i.e. a state of the world at one particular instant. In other words, each instantaneous space is nothing but a class of *absolutely simultaneous events* at this particular moment. The classical concept of absolute simultaneity was explicitly formulated by Newton when he wrote that "every indivisible moment of Duration is everywhere" *(unumquodque durationis indivisibile momentum ubique)* and, before him, by Gassendi insisting on the *ubiquity of every instant:* "A moment of time is the same in all places" *(Temporis momentum idem est in omnibus locis).*[13]

It is precisely on this point where the physics of relativity differs so profoundly from the physics of Newton. In the former there is simply no simultaneity of distant events as Einstein explicitly stressed in his *Autobiographical Notes* in 1949.[14] The expression "negation of simultaneity of distant events" is obviously stronger than the term "relativization" which is currently used in the textbooks of relativity; but what possible physical significance can a conceptual entity have which is *ex definitione* never perceived, but only constructed, and when construed, is different in different inertial systems? This negation of simultaneity of distant events follows from the peculiar structure of Einstein-Minkowski's space-time which is so radically different from the structure of its classical Newtonian counterpart.

Briefly, the difference is as follows: as mentioned above, in the classical space-time it was possible at every instant to separate a three-dimensional instantaneous space as a substratum of all objectively simultaneous events. In the Minkowski-Einstein space-time no such instantaneous cuts are possible; there is no objective "Everywhere-Now." It is probably this feature of contemporary physics which appears the most unacceptable to the traditional Newton-Euclidian habits of our thought. What apparently increases its paradoxical character is one conclusion which is often drawn from it: since the objective "Now" constitutes a definite boundary separating the past from the future, its negation apparently eliminates the distinction between the successive states of the world. Thus the relativistic space-time was – and still is – often seen as a static entity, a sort of hyperspace of which time is an additional dimension, basically not different from the other three dimensions. It is true that such a view is held more frequently by philosophical interpreters of relativity than by physicists; not infrequently it is used as an argument for the idealistic view of the unreality of time. But we find it even in Einstein when he wrote that it "appears more natural to think of physical reality as a four-dimensional existence, instead of, as hitherto, the *evolution* of a three-dimensional existence."[15]

Fortunately, such conclusion, which would run contrary to our most pervasive experience, is not valid. As already Eddington observed more than sixty years ago, in Einstein-Minkowski's space-time the past is separated from the future even *more effectively* than in classical physics; it is the whole four-dimensional region of "Elsewhere" – not merely a three-dimensional instantaneous layer – which separates the absolute causal past from the absolute causal future. As Einstein himself explicitly stated in a sentence immediately preceding the one quoted above, *"the concepts of happening and becoming are indeed not suspended, but yet complicated."* Especially in his later years his doubts about the adequacy

of a static interpretation of space-time gradually increased. According to the testimony of Carnap, he was becoming aware of the fact that theoretical physicists do not do justice to our immediately experienced difference between the past and the future. This was very probably a result of the proddings by Sir Karl Popper who two years before in a personal conversation teasingly called Einstein a "new Parmenides" and tried to convince him that time, evolution, becoming, cannot be summarily dismissed in the Eleatic fashion. It is equally possible that the last book of Einstein's own disciple, Hans Reichenbach, had a similar effect on him; for Reichenbach argued rather convincingly that the "Now" – which is always correlated with the experienced difference between the past and the future – is inescapable even for a physicist; for every act denying "Now" *implicitly reasserts it* since such act itself is located in its own "Now."[16] Whether Einstein became definitely converted to the dynamic and indeterministic view of reality as Karl Popper believes, remains uncertain; what is certain is that he definitely had some doubts about his former static interpretation. In this sense he was returning to his earlier statement quoted above that in the relativistic space-time becoming is "complicated, not suspended."

The paradoxical structure of space-time – which I, for obvious reasons, prefer to call "time-space" (I was told by the late Professor Giorgi Santillana that such term is used by the Italians – *il cronotopo)* – is due to the fact that it can be characterized by the following propositions:

a) There is no absolute universal "Everywhere-Now."
b) But there is the absolute Future not only for my Here-Now, but also for *all my causal contemporaries,* the term "contemporary" being redefined in a new relativistic sense. (The class of all contemporary events constitutes at given moment the "Elsewhere region" which could be equally well called "Elsewhen.")

These two propositions *appear* incompatible, but only because we are still subconsciously dominated by the Newtonian conceptual framework; when rephrased and clarified, they cease to be so. The proposition a) can be rephrased in the following meaningful way: the Newtonian allegedly universal "Now" is merely an artificial three-dimensional cut in the four-dimensional world-history; it is artificial since space is inseparable from time; it is, in a sense, arbitrary since the observers in different inertial systems will make such cuts differently. But under no circumstances do such cross-sections – their different "Now-planes" – intersect their own causal past and their own causal future. Thus no confusion of either the past or the future with the present can ever occur.

The proposition b) rephrased is as follows: my own causal future is intrinscially unobservable in my Here-Now since it is *ex definitione not included in my own causal past.* (It is obvious that the inclusion in my own causal past is a necessary condition of the observability in my Here-Now.) Furthermore – and that is rarely, if at all stressed – *neither is any event in my absolute future included in the causal past of any of my contemporaries; nor is any event in the causal future of any of my contemporaries observable Here-Now.* To deny it would mean to ignore completely the structure of the Einstein-Minkowski diagram, i.e. to deny the constancy of the velocity of light and Lorentz transformation; more specifically, it would mean to believe that the order of causally related events can be reversed by a mere change of the frame of reference. Thus my own causal future, being intrinsically unobservable not only in my own Here-Now, but in *all other conceivable frames of reference,* is unreal, and this unreality is in the true sense *absolute* and *universal,* and not merely local. Its alleged present existence or pre-existence belongs to the same category of fictitious and useless entities gratuitously postulated contrary to the accepted rules of the sound scientific methodology as I have pointed out in several of my previous writings.[17]

If we thus keep in mind that, from the standpoint of relativity, the universe is a dynamical framework of the irreversible world-lines (their irreversibility being a consequence of the constancy of the Einstein-Minkowski world-interval as Paul Langevin pointed out at early as 1911), we can clearly see what remains valid in Newton's concept of absolute time. We have seen what does *not* survive from this concept: neither the *metrical* uniformity nor its independence of its concrete physical content, nor the concept of absolute "Everywhere-Now." But the *irreversibility* of time in the light of what was said in the previous paragraph remains intact even in relativity. It is true that Newton himself did not use the word "irreversibility of time"; but the same concept is clearly implied in the statement of his scholium that *"the order of the parts of time is immutable."*[18] Now it is true that some relational theories of time have the tendency to define "the direction of time" – which is, by the way, a rather questionable metaphor, borrowed from geometry and kinematics – in terms of some all-pervasive physical change such as the growth of entropy or the expansion of the universe. In this respect the doubts of the late professor Bridgman about the adequacy of such approach are fully justified; he correctly pointed out that "the unidirectionality of time" is too basic and irreducible a feature of time to be interpreted in the terms of its contingent physical content; while the direction of motion in space can be reversed, the so-called "direction" of time cannot.[19] For the positions in the spatially inverted motion will *succeed each other* in the same way as

those in the original motion. Thus it is far more natural to say that entropy *decreases with time* rather than that time "runs backwards." (It is rather significant that entropy remains one of the invariants in the relativistic time-space.) The expression "backward running time" is nothing but a meaningless assemblage of words.

Center for the Philosophy and History of Science, Boston University

NOTES

1. Cf. Pierre Duhem, *Le système du monde* (Hermann, Paris, 1956), pp. 439-441; also J.L.E. Dreyer, *A History of Astronomy from Thales to Kepler,* (Dover Publications, 1953), p. 279.
2. G. Bruno, *Camoeracensis Acrotismus,* Art. XL. *Opera omnia conscripta,* (Fr. Fromann, Stuttgart, 1962.)
3. *Disquisitio Metaphysica seu Dubitationes et Instantiae Adversus Renati Cartesii Metaphysicam et Respona,* Opera omnia, (Lyon, 1658), III 347b.
4. *Lectiones geometricae,* Lec. I. All these texts (Bruno, Gassendi and Barrow) are reprinted in *The Concepts of Space and Time. Their Structure and Their Development,* ed. by M. Capek in Boston Studies in the Philosophy of Science, (Reidel, Dordrecht; 1976).
5. E. Mach, *The Science of Mechanics,* 9th ed. tr. by Thomas J. McCormack, The Open Court, 1942, pp. 284-85.
6. R.J. Boscovich, *Philosophiae recentioribus versibus tradita a Benedicto Stay Libri Decem,* Romae, 1755; translated and reprinted in *The Concepts of Space and Time,* pp. 289-90.
7. I. Newton, *The Mathematical Principles of Natural Philosophy,* The Citadel Press, New York, 1964, p. 19.
8. *Essay Concerning Human Understanding,* Ch. XIV, § 21.
9. H. Bergson, *Essai sur les données immédiates de la conscience,* Presses Universitaires de France, 1940, p. 80; H. Poincaré, *La valeur de la science,* Paris, Flammarion, 1940, p. 38.
10. B. Russell, *An Essay on the Foundations of Geometry,* Dover Publ., 1956, p. 156.
11. C. Neumann, *Über die Prinzipien der Galilei-Newtonschen Lehre,* Leipzig, 1870, pp. 16-19; transl. and reprinted in *The Concepts of Space and Time.* Cf. my critical comment on Neumann, *ibid.* p. XXXVII.
12. Regrettably, even some serious thinkers (e.g. Quine, Donald Williams, G. Schlesinger) believe they are discussing the relativistic space-time while dealing really with the classical space-time only.

13. *Newtoni opera,* ed. Horsley (London, 1779), III, p. 72; P. Gassendi, *Opera,* I, p. 224.
14. Cf. *Albert Einstein, Philosopher-Scientist,* ed. by Paul Schilpp, The Library of Living Philosophers, vol. VII (Evanston, Ill., 1949), p. 61.
15. A. Einstein, *Relativity. The Special and General Theory* (Crown Pub., New York, 1961), p. 150.
16. H. Reichenbach, "Les fondements logiques de la mécanique des quantas," *Annales de l'Institut Henri Poincaré, 13* (1952), p. 156. On Einstein's conversations with Karl Popper and Rudolf Carnap cf. my Introduction to the English translation of Emile Meyerson, *The Relativistic Deduction,* tr. by David A. Sipfle and Mary. A. Sipfle. *Boston Studies in the Philosophy of Science,* vol. 83 (Reidel Dordrecht, 1985), pp. XLIII-XLV.
17. Cf. in particular: *The Philosophical Impact of Contemporary Physics,* 2nd ed. (Van Nostrand, Princeton, 1969), esp. Part II and Appendix I and II; "The Myth of Frozen Passage; the Status of Becoming in the Physical World," *Boston Studies in the Philosophy of Science,* vol. II (New York, 1965), pp. 441-463; "Relativity and the Status of Becoming," *Foundations of Physics,* vol. 5, No. 4 (December 1975), pp. 606-616; "Time-Space rather than Space-Time," *Diogenes,* Unesco, Paris, 1983), pp. 30-49.
18. Newton, *op. cit.,* p. 19.
19. P.W. Bridgman, *Reflections of a Physicist* (New York Philosophical Library, 1955), p. 251.

H.H. KUBBINGA

NEWTON'S THEORY OF MATTER

Et haec de deo, de quo utique ex phaenomenis disserere, ad philosophiam naturalem pertinet.
Isaac Newton (1687)

1. *Introduction*

Until the beginning of this century, historians of natural philosophy used mainly three major sources in their study of Newton's theory of matter: the *Philosophiae naturalis principia mathematica,* the *Opticks,* and the minute treatise *De natura acidorum.* Thus, the great scholar of atomism, Kurd Lasswitz (1848-1910), whose monumental masterpiece *Geschichte der Atomistik vom Mittelalter bis Newton* published in 1890 is still the natural starting point of every investigation in this domain, erred, ostensibly in spite of himself, when he wrote:

> Thus Newton displays a great indifference toward the theories of matter, and he attempts neither to pursue any assumptions about the fundamental constituents of matter to their ultimate consequences, nor to expose them clearly or bring them into mutual agreement.[1]

Since that time, the Newtonian doctrine of matter has been the object of several extensive studies.[2] Most of these works deal with the general character of matter by emphasizing the physical, philosophical or metaphysical aspects of the problem.

The monograph of A. Thackray, *Atoms and Powers – an Essay on Newtonian Matter-theory and the Development of Chemistry,* which was published in 1970, deserves a special place in virtue of its breadth and depth.[3] Thackray gives a valuable picture of the development of Newton's ideas during the 18th century, not only in Newton himself, but also in his direct disciples (such as Gregory, Freind and John Keill) and his indirect followers (such as Boerhaave, 's Gravesande and van

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This contribution is dedicated to Miss H.G. Brinkman on the occasion of her fifth anniversary.

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Musschenbroek). Thackray defended the view that the Newtonian doctrine of the structure of matter was particularly deepened during the quarrel concerning the priority of the discovery of the integral and differential calculus, in which Newton and Leibniz confronted each other.[4] This process of deepening occurred at first in the works of some of his disciples. It was only after Leibniz's death in 1716 that Newton dared to express himself more or less freely, albeit rather prudently, in the second English edition of the *Opticks* (1717). Generally speaking, Newton's theory of the constitution of matter – the so-called nutshell theory according to the expression which Thackray borrowed from Joseph Priestley – can hardly be said to have advanced exact science during the 18th century.[5] Chemists and physicists, moreover, were particularly inspired by Newton's ideas concerning the reciprocal actions of the particles of bodies. Thus, it will be seen that Newton's influence on John Dalton (1766-1844), the inventor of a quantitative molecular atomism which is at the origin of modern molecular theory, concerns the inter-particle relations rather than the particles themselves.

Up to now, the sources on which the majority of the works mentioned earlier were based, contained only those publications that saw the light during Newton's life, and which were considered, according to 19th century terminology, the true foundations of so-called "positive" science. That Newton's activities had also a less "positive" and rather "obscure" side, at least if viewed from a triumphalist conception of science and its history, is a fact that has been recognized by historians only recently. In the thirties the British economist, J.M. Keynes (1883-1946), could prevent an important part of Newton's alchemical papers which function as key witness in this matter from being dispersed throughout the world. This gesture definitely focused the attention of the historians on this problem.

A first thorough study of Newtonian alchemy appeared in 1975, due to the care of B.J.T. Dobbs, under the title *The foundations of Newton's Alchemy or "The Hunting of the Greene Lyon."*[6] This work by Dobbs covered the period of 1668-1675 and yielded the conclusion that it was during that time that Newton discovered the foundation of a true alchemical philosophy and that he trusted that foundation to the very end of his life. More or less parallel to the important work of Dobbs was the effort of K. Figala to reveal the apparently very strong link between Newton's alchemical doctrine and his "nutshell" theory of the structure of phenomenal matter.[7]

In the present paper I propose to give a general view of the results of the research of Thackray, Dobbs and Figala. Moreover, I would like to project the Newtonian doctrine against the background of the theory

of matter as it had been developed during the 17th century. In my opinion, the great discovery of the 17th century was the concept of "substantial individual." It appears that this concept was at the heart of the doctrine of Georg Ernst Stahl (1660-1734), which dominated the entire science of matter during the 18th century. Now, it is precisely in this Stahlian tradition that the work of John Dalton is to be placed. As it will be shown, the work of Dalton represents the meeting point of the Stahlian and Newtonian threads of modern molecular theory.[8]

2. *The "nutshell theory"; Thackray, Dobbs and Figala*

The "nutshell" theory was first more or less clearly made explicit in the second English edition of the *Opticks,* more particularly in the third Part of the Second Book.[9] According to Newton, a sample of substance consists, as a first approximation, of particles of equal size, arranged so as to allow for empty interstices; the total volume of particles would be equal to the total volume of interstices. These particles are the cause of the color of the substance under consideration[10] and of its chemical behavior.[11]

Each of these particles is made up of smaller particles separated from each other by interstices, the volume of which is again equal to that of the secondary particles. The latter particles consist again of tertiary particles which are intermingled with pores amounting to a volume equal to that of the particles, so that the total volume is again half matter, half void. And so on, "perpetually."[12] It is not unequivocally clear what Newton means by the word "perpetually." In the strict sense the word would imply that the division never ends. This corresponds, if one believes Thackray,[13] to the extreme position which Newton took during his debate with Leibniz. Yet, according to the text of the *Opticks,* and in spite of the expression "perpetually," Newton emphasized that the divisibility is limited and that one is bound to come to ultimate particles. These would be really "solid particles, such as have no pores or empty space within them."[14] Newton calculated for each level the proportion of space occupied by the ultimate solid particles and by the space that is left empty. Thus, the ultimate solid particles have a proportion of matter:void of ∞:1. Somewhere else, the particles which are composed of these ultimate solid particles and the interstices with the same total volume are called "particles of the first composition";[15] these are characterized by a proportion matter:void of 1:1. This proportion appears rapidly to diminish: the "particles of the fifth composition" for instance, contain a quantity of matter which corresponds to only 1/63th part of

their volume. The proportion matter:void appears to follow the formula $1:(2^n - 1)$, where n represents the number of the level of composition, i.e. the relative complexity in respect of the ultimate solid particles. All this reflects the extreme porosity of matter, which is witnessed by many phenomena. Newton provides a good number of examples, from the transparency of water to light to the transparency of gold – which is nineteen times denser than water – to magnetism. The extreme void contained in phenomenal matter can be represented by saying, with Priestley, that all ultimate particles of the whole universe could be contained within a single nutshell. Thackray has retained this very eloquent expression to briefly characterize the Newtonian theory. Moreover, Thackray has developed a very ingenious model on the basis of cubic particles to illustrate Newton's idea (see Fig. 1).[16] It is to be remembered, however, that the ultimate solid particles are not identical for Newton. If one forgets for a moment the usual indirect style of the *Opticks,* this immediately follows from the famous passage of "Query 31" where Newton writes:

> All these things being consider'd, it seems probable to me, that God in the Beginning form'd Matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties, and in such Proportion to Space, as most conduced to the End for which he form'd them; [...].[17]

This image is further complicated by the statement in one of the English versions of the treatise *De natura acidorum* that the "particles of the second composition" can also result from "Aggregates" of "particles of the first composition".[18] The least that can be said is that Newton did not unambiguously take position on this point. For in the *Opticks,* he even admits that there are other ways of imagining the extreme porosity of bodies, and he adds that the real internal structure of bodies is not yet known.[19]

This survey of the "nutshell" theory can be brought to an end by considering what Newton said about the interparticle relations and about the stability of complex particles. In "Query 31" Newton stipulates that there exists a very strong attraction between the ultimate particles and that their stability diminishes as the complexity of the particles increases. The largest particles of a substance which are also called "particles of the ultimate composition" are, as will be seen, those upon which the colors of bodies and their chemical activities depend; they also determine the refraction of light in transparent media.[20] There are two forces which act between these "particles of the ultimate composition": one is attractive,

the other repulsive, Whereas the attractive force diminishes in some proportion to the distance from the particle, the repulsive force begins where the attractive force vanishes. Before investigating the precise epistemological status of these "particles of the ultimate composition," it is necessary to briefly focus upon the chemical implications of the "nutshell" theory.

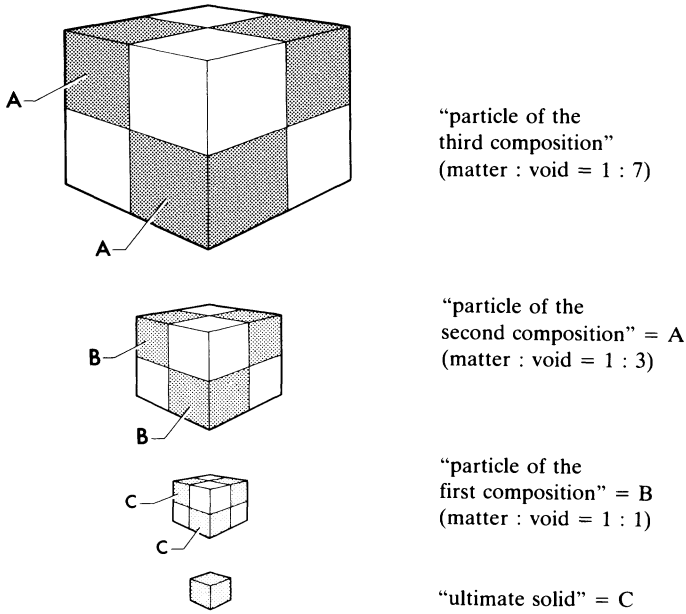

Fig. 1: The internal structure of matter according to Thackray's interpretation of Newton's nutshell theory (from: A. Thackray, reference 3, p. 64).

In the last chapter of her work on Newton's alchemy, Dobbs discusses the consequences of the "nutshell" theory for the interpretation of the chemical behavior of a body. She has paid particular attention to the treatise *De natura acidorum* in its different versions.[21] Her analysis yields the conclusion that, according to Newton in this particular treatise, the chemical reactions take place while the "particles of the ultimate composition" remain unchanged. Each substance would be composed of "particles of the ultimate composition" which are themselves composed of two kinds of subparticles, one earthy, the other acid, which are united in an arrangement which is characteristic for each substance under consideration. This would appear to be a revival of the sulphur-mercury doctrine of ancient alchemy.[22]

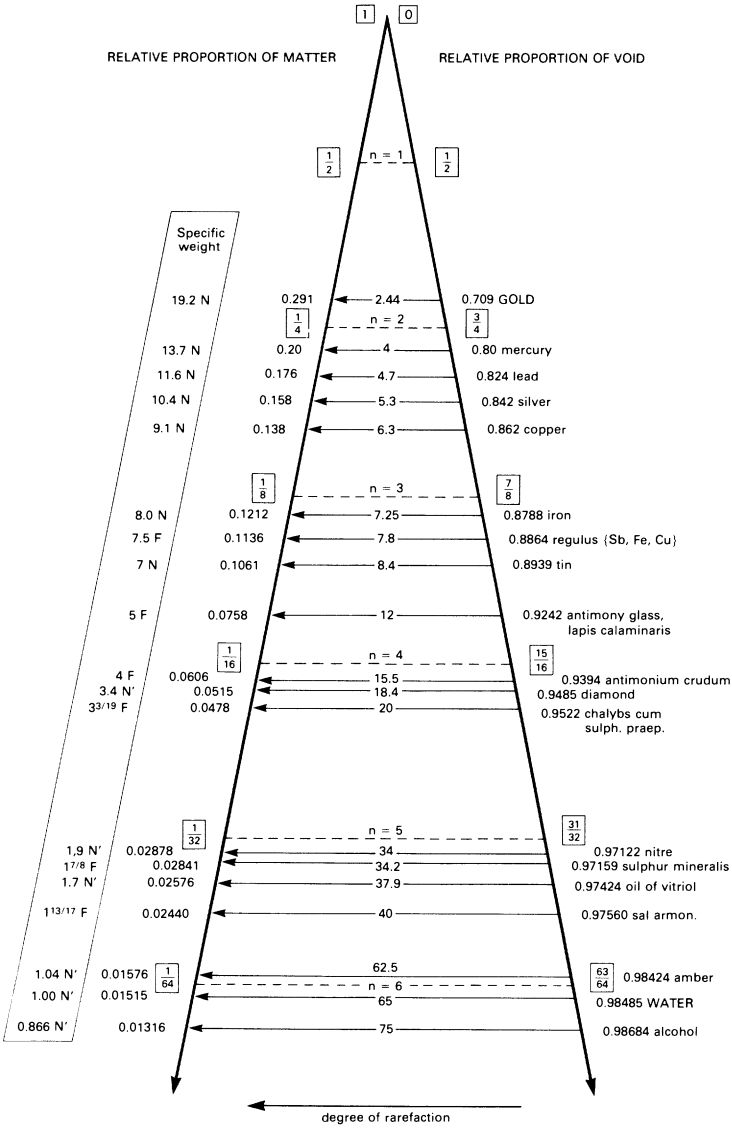

Fig. 2: "Genealogical tree" of all substances according to the nutshell theory and on the basis of their specific gravity (from: K. Figala, reference 7.*b*; with kind permission of the author).

On the other hand, Figala has been able to nicely connect the "nutshell" theory with the alchemical ideas and experiments which Newton mentions in his manuscripts (see fig. 2).[23] By using numerical data offered by both Freind and Newton, Figala has interpreted the "nutshell" theory in terms of relative density, whereby each value of the latter corresponds to a certain level of complexity. *Water* was chosen as a reference body in respect of which the relative density of a substance was determined. In his manuscripts Newton attributes to water a matter:void proportion of 1:65.[24] Figala's reconstruction allows us to easily calculate the matter:void proportion for any kind of substance, provided its relative density be known. Thus, since gold has a relative density of 19, this substance necessarily contains 19 times more matter than water does, and therefore it has a matter:void proportion of 19:65, i.e. approximately 1:2.5. This amounts to saying that gold belongs to a level of complexity between $n = 1$ (with the proportion 1:1) and $n = 2$ (with the proportion of 1:3). According to Figala, all chemical substances can therefore be ranged within a grand genealogical scheme (see fig. 2).[26] In this scheme the substance air, for instance, can be situated between $n = 15$ and $n = 16$, whereas the most complex and therefore least dense substance, i.e. the ether, must be situated near $n = 37$.[26]

By combining Figala's scheme with that of Thackray, it becomes easy to see how the "particles of the ultimate composition" of different bodies give rise to each other. To begin, one must imagine Thackray's scheme to be entirely empty. Following this, each cell which is to contain matter must – imaginatively – be filled with ultimate solids, and this must be done so as to place one single solid particle in each cell. This procedure shows some resemblance with the procedure which in modern atomic physics is contained in the so-called *Aufbau* principle. Since there are different types of ultimate solid particles in the Newtonian system, there will be just as many different sets of particles and, consequently, just as many series of phenomenal substances. At least, this follows from a method which is entirely *ab initio*. Now, if one follows Figala's analysis, Newton holds that both the ultimate solid particle and the "particle of the first composition" are the same in the particles of all bodies. Thus, by starting from the "particles of the second composition", it becomes possible to classify bodies in phyla, classes, orders, families, genera etc., if it should be permitted to make an anachronistic use of the vocabulary of biological taxonomy. This analogy moreover clarifies one of the key ideas of the Newtonian philosophy of matter, namely the idea that all substances are akin to each other, and furthermore, that these substances can be transformed into each other.[27] Such transformation could be brought about by the loss and/or the addition of one or more ultimate

solid particles of certain denominations. Thus the transmutation of metals arises by the successive removal of ultimate solid particles which inhabit the cells of the highest levels of the particles from the original product which, according to Newton, is the *antimonium crudum* (the mineral known in modern chemistry as stibnite, Sb_2S_3). The particles of this *antimonium crudum* are first converted into particles of "regula of antimony" which in turn are then successively converted into particles of iron, copper, silver, mercury and, finally, of gold. The progress of this transmutation can be seen in Figure 2.

By comparing the points of view of resp. Figala and Dobbs, certain discrepancies in the interpretation of Newton's theory of matter appear. The most important one concerns the modalities of the transformations of the bodies, i.e. the chemical reactions. According to Figala who, in our opinion, correctly emphasizes the position of the "nutshell" theory in Newtonian thought, a transformation results from the partial destruction of the particles of the original product, followed by the construction of the particles of the product on the common trunk. Every body therefore can, at least in principle, give rise to any other body.

Dobbs, on the other hand, stresses the permanence of the "particles of the ultimate composition" of the bodies which are combined during a chemical reaction. From this perspective, a chemical reaction would simply amount to the separation and/or the reunion of the "particles of the ultimate composition" of the bodies in question. The particles of sulphur and mercury, which are considered to be the only constituents of all substances, would stay in the shadow of the particles of these substances.

I shall abstain from making a choice between these apparently mutually exclusive interpretations. Let it suffice to say that Newton's doctrine of matter is far from being clear. Nothing can make us better see the ambiguities of Newtonianism within this context than the most simple and clear doctrine of Georg Ernst Stahl (1660-1734).

3. *Newton versus Stahl*

As I have shown elsewhere, the 17th century witnessed the birth of the concept of "substantial individual" as the necessary and sufficient condition for the existence of a "substantial species."[28] In that sense, the concept of "substantial individual" clearly prefigures the modern concept of "molecule." Here is without doubt a decisive step in the historical development of the theory of matter.[29] The so-called molecularization of the microcosm was indeed a very important aspect of what E.J. Dijksterhuis (1892-1965) has called the "mechanisation of the world pic-

ture."[30]

The first manifestation of the concept of "substantial individual" can be traced back to the scientific diary of the Dutch scholar Isaac Beeckman (1588-1637)(1620). Later, it is to be found, among others, in Sebastian Basso (end of the 16th-beginning of the 17th century)(1621), René Descartes (1596-1650)(from 1631 on), Daniel Sennert (1572-1637)(1636) and Christiaan Huygens (1629-1695)(about 1670).

In Leibniz, the concept of "substantial individual" appears at the start of his theory of monads, which corresponds to a metaphysics of the individual, with all its many different connotations. As with Newton, there exists in the work of Leibniz an intimate relation between the mathematical ideas and the physical doctrine. As it has been said above, Thackray has already stressed this capital point. He concluded, among other things, that the Newtonian theory of matter is opposed to the theory of Leibniz in exactly the same manner as his doctrine of "fluxions" is opposed to the doctrine of "infinitesimals."[31] This observation by Thackray has been confirmed by my own research which led to the conclusion that the "infinitesimals" of Leibniz' integral and differential calculus must be regarded as the epistemological complements in the domain of mathematics, of the concept of "substantial individual."[32]

From 1683 onward, the German chemist and physician Stahl developed a theory of matter which would dominate all of the 18th century, and which may rightly be considered as the direct precursor of modern molecular theory.[33] The foundation of the new doctrine appears to be the concept of "substantial individual," but now under the very significant name of "physical individual" *(individuum physicum)* (1700). Apparently, the concept of "substantial individual" had become toward the end of the 17th century an, albeit vague, received idea, since Stahl had been careful to explicitly formulate its status in some special treatises.[34]

Stahl's doctrine may be summarized as follows. Every *pure* body is considered an aggregate (Stahl was the first to use the word in its modern sense) of identical particles which are constituted by *atoms* of one and the same prime matter. Stahl distinguishes four species of *atoms* (water, vitrifiable earth, inflammable earth and liquefiable earth; inflammable earth is the well-known fire principle or phlogiston), characterized by a certain specific figure and size.[35] From these *atoms physical individuals* are formed, i.e. the *molecules* of the phenomenal substances. As is shown by laboratory experiments, these bodies exist, according to Stahl, in several degrees of complexity. Thus the *atoms* constitute the so-called "mixed" molecules *(mixta),* which in turn form the so-called "composite" molecules *(composita).* There exist also *corpora decomposita* and even

corpora superdecomposita, all this of course in an increasingly complex order.[36]

By means of this molecular interpretation of matter, it is evident that one must distinguish phenomena which merely concern the "aggregate" from phenomena which chiefly concern the molecules in the true sense of the word. Thus, the latter really consist in the disassemblage and the assemblage of individual molecules; now we would speak of "chemical reactions." The former operate under substantial permanence and would now be called "physical processes" (dilation, shrinking, evaporation, condensation, fusion, freezing, sublimation, extraction, dissolution, cristallization).[27] There is no need to get lost in the details of the particularly well thought out doctrine of Stahl. It will suffice to say that the theory of the three states of aggregation, which is in a way the spindle of the work of Antoine Laurent Lavoisier (1743-1794) as well as of the work of Dalton, was developed during the 18th century in the best Stahlian tradition.[38]

It is moreover illuminating to establish a parallel between Stahl's "physical individual" and Newton's "particle of the ultimate composition." In my opinion, one could argue that both the Newtonian particle and the Stahlian "individual" must be defined as "substantial individual." Like his Stahlian homologue, this "particle" is indeed the necessary and sufficient condition of the existence of a "substantial species." But this does not in any way take away the fact that Newton's definition, inasmuch as it can be deduced from the "nutshell" theory, is fundamentally different from Stahl's perception. For Newton, the "substantial individual" is defined by certain numbers of atoms of certain kinds, which are united within a spatial structure which is prescribed by the proportion matter:void of $1:(2^n-1)$.[39] Contrary to the position of Newton, Stahl says that the number of kinds of atoms is precisely limited to four. For Stahl too, there is no structural relation between the "physical individuals" of different origin except by chance. When Newton's doctrine is compared with that of Stahl, the most striking feature is the latter's pragmatism which so vividly stands in contrast to the apparently all too metaphysical character of Newton's position. Newton's all too cerebral doctrine not only has the tendency to feature all the aspects of the classical problem of matter but is also eager to meet the conditions imposed by alchemy, by chemistry in the strict sense, and by physics, philosophy, theology and mathematics. Besides giving a confused impression, it is also on certain points contradictory in its statements (this is for instance the case for the assertions concerning the limited or unlimited divisibility of matter). The theory of Stahl, on the other hand, which, although rather sober in its assertions and modest in its aspirations, is not only more practical, but it also has a very attractive dogmatic simplicity and, moreover, accounts for

most of the physico-chemical phenomena observed in the laboratory. In order to better appreciate the intrinsic value of Stahl's doctrine it may be useful to consider the fact that the physico-chemistry of phlogiston, the famous fire principle, and the molecular theory of which it was a part constitute so to say the mirror picture of the future chemistry of oxygen. That is why contemporary scientists had no difficulty whatever in making a choice. Most of them in fact opted for the doctrine of Stahl.[40]

Gabriel-François Venel (1723-1775), a distinguished representative of French Stahlianism, undoubtedly translated the general feeling when he praised the *Principia [..]* as the product of a "physicist's genius carried to the highest degree to be achieved by humanity," all the while recognizing Stahl's *Specimen Beccherianum [..]* as "the extreme pendant of chemical genius."[41] For Venel, physics was the science of the behavior of "aggregates" in relation to each other, while chemistry was the science of the behavior of the molecules and of intermolecular relations. There existed, according to him, an epistemological abyss between molecular mechanics and celestial mechanics, whereby physics stood to chemistry as mathematics stands to language, or the abstract to the concrete, or the absolute to the relative. But driven by zeal for the Stahlian cause, Venel failed to recognize the spell which mathematics cast over science in the 18th century and the influence of the astronomical success of Newtonianism. At the very time when the *Encyclopédie (..)* of Diderot and d' Alembert divulged Stahlianism in the interpretation by Venel, the scientific world was stupefied by the reappearance of Halley's comet in 1758; this phenomenon was considered to be the superb confirmation of the theory of universal gravitation in general and of the mathematical treatment of the relevant problems in particular.

In short, one could say that, in the 18th century, the science of matter rested upon two pillars: the theory of "physical individuals" of Stahl and the theory of interparticle forces (either attraction in combination with repulsion, or attraction by itself) of Newton.

4. *Newton, Stahl, Dalton and modern molecular theory*

In his *Traité élémentaire de chimie* (1789), Lavoisier accorded to the idea of the relative complexity of phenomenal substances, which was one of the pillars of Stahlianism, the privileged place which it deserves by defining the "chemical element" as "the last point which analysis is capable of reaching."[43] This position paradoxically implied that the notion in question was, if not abolished, certainly made superfluous, since in the new chemistry the existence of ever complex combinations was an obvi-

ous truism. Indeed, the notion of relative complexity had for Lavoisier become clearly tautologous.[44] Yet, while expelling the phlogiston – that hypostasis of combustibility – from the domain of chemistry, Lavoisier opened the way to the "caloric," i.e. to "the cause of heat, or that exquisitely elastic fluid which produces it."[45] Together with atmospheric pressure and a force of attraction, this "caloric" plays an important role in the interaction between the molecules. In Lavoisier's opinion, it is these three factors that determine the state of aggregation of the bodies:

> Thus the particles [the original text reads: 'molécules'] of all bodies may be considered as subjected to the action of two opposite powers, the one repulsive, the other attractive, between which they remain in equilibrio. So long as the attractive force remains stronger, the body must continue in a state of solidity; but if, on the contrary, heat has so far removed these particles from each other, as to place them beyond the sphere of attraction, they lose the adhesion they before had with each other, and the body ceases to be solid.[46]

Thus a body becomes solid, liquid or gaseous

> [..] according to the quantity of caloric by which it is penetrated; or, to speak more strictly, according as the repulsive force exerted by the caloric is equal to, stronger, or weaker, than the attraction of the particles of the body it acts upon.[47]

If there is no atmospheric pressure, the solid bodies are immediately transformed into gas at the very moment when, on account of heating, the repulsion were to exceed by the slightest amount the force of attraction.[48] It is curious to see how Lavoisier shows a complete indifference with regard to the epistemological status of the repulsive force which he attributes to the "caloric":

> It is probable, that the separation of the particles of bodies, occasioned by the caloric, depends in a similar manner upon a certain combination of different attractive powers, which, in conformity with the imperfection of our knowledge, we endeavour to express by saying, that the caloric communicates a power of repulsion to the particles of bodies.[49] (My translation).

Like Newton therefore, Lavoisier too interprets the behaviour of the molecules in relation to each other during dissolution and evaporation in two different ways. Either there is but one single force, viz. the force of attraction, or there are two, one attractive, the other repulsive, such that the former begins where the latter ends.[50] During the 18th century, and particularly since the studies by Herman Boerhaave (1668-1738) on heat and its dilating effect,[51] the repulsive force was most often identified with a sphere of repulsive acitivity around each particle. In that sense, the dilation of a body – whether solid, liquid or gaseous – amounts to the swelling of these spheres due to the addition of the matter of fire. This may be read in 1753 in the work of Venel, in the *Encyclopédie [..],* and, in 1808, in Dalton's *New System of Chemical Philosophy*. Certainly, for Lavoisier, the force of attraction is simply, like universal attraction, an *actio in distans;* the repulsive force is superposed upon this attraction. It is moreover not at all clear whether he considers that each particle has its own proper caloric or that the caloric is a sort of dissolvant for the bodies.[52]

Lavoisier says some very pertinent things on the nature of molecules, although they take a small place in the doctrine exposed in the *Traité élémentaire[..].* Apparently there is some mention of a true concept of "substantial individual," at least in what concerns the salts. Referring to René-Just Haüy (1743-1822), the great cristallographer who maintained an entirely Stahlian theory of matter, Lavoisier writes:

> Every species of salt cristallizes in a peculiar form, and even each salt varies in the form of its cristals according to circumstances, which take place during cristallization. We must not from thence conclude that the saline particles ('molécules') of each species are indeterminate in their figures: The primitive particles ('molécules primitives') of all bodies, especially of salts, are perfectly constant in their specific forms; but the cristals which form in our experiments are composed of congeries of minute particles ('molécules'), which, though perfectly equal in size and shape, may assume very dissimilar arrangements, and consequently produce a vast variety of regular forms, which have not the smallest apparent resemblance to each other, nor to the original cristal.[53]

Curiously, for Lavoisier, the molecular theory is nothing more than the core of the doctrine of the three states of aggregation; therefore, he can use it only in the domain of physics. Whatever the case may be, the little he says allows us to establish that the theory is entirely of Stahlian char-

acter.

As Thackray has shown in his beautiful monograph on the life and work of Dalton, one of the works by which the latter received his education was Lavoisier's *Traité élémentaire de chimie.*[54] Thackray also mentions the works of William Nicholson (1753-1815) and of Jean Antoine Chaptal (1756-1832) all of which were to be found in Dalton's library. Furthermore, Dalton himself confessed that he knew the chemical works of Robert Boyle (1627-1691), of Boerhaave and of an Anglican bishop whose name he does not give.[55] This statement dates from 1790, while the catalogue from which Thackray borrowed his information was made in 1800. All this allows us to establish that the most important work which Dalton studied during his chemical education was the *Traité élémentaire[..].* It is therefore not surprising to see that the weakening of the position of the molecular theory in favor of the theory of the three states of aggregation which I have pointed out in Lavoisier's doctrine finds its completion in Dalton. When reading *A new System of chemical Philosophy,* one sees indeed almost immediately that the theory of the three states is no longer considered the fruit of molecular considerations – as it was in fact from an historical point of view – but that, on the contrary, the molecular *hypothesis* was supported by the *observation* of three states. Thus Dalton writes in Chapter II of the first part (which appeared in 1808):

> [..] These observations [concerning the three states of bodies] have tacitly led to the conclusion which seems universally adopted, that all bodies of sensible magnitude, whether liquid or solid, are constituted of a vast number of extremely small particles, [..].[56]

A little further he demonstrates the identity of these particles by saying that the "specifick gravity" of any quantity of a "homogeneous body" such as water is always the same, whatever its origin:[57] if the molecules were not to have the same weight, one should expect to find a variation in the "specific density," which is not the case. What obtains for particles or water, for instance, should also obtain for those of the elements:

> Therefore we may conclude that *the ultimate particles of all homogenous bodies are perfectly alike in weight, figure, &c.* In other words, every particle of water is like every other particle of water; every particle of hydrogen is like every other particle of hydrogen, &c.[58]

The great step forward made by Dalton when compared to Lavoisier appears in this fragment where the concept of "chemical element" is linked – albeit not for the first time[59] – to the idea of specific atoms.

If it is presupposed that the atoms are preferably composed so as to form a "binary" combination - the molecules of which contain, according to Dalton's terminology, an atom of both species involved - the quantitative data of the chemistry of gases can be interpreted in terms of the new molecular theory. If the chemist knows but one single combination of two elements, he must conclude that the molecule of this combination should contain one single atom of each of the two denominations. Water, for instance, which at the beginning of the 19th century was the only known compound of hydrogen and oxygen, would therefore consist of molecules each of which would contain one atom of hydrogen and one atom of oxygen. According to Dalton, the so-called "binary" combination is the most probable one on account of the repulsive force which would come into play as soon as *two* atoms of the same species are combined with one single atom of another species.[60] If the combinations of the two elements are known, then one is "binary" and the other, somewhat less probably, "ternary"; the ternary combination would consist of two atoms of one of the two elements and of one single one of the other. And so on.

A study of the new system reveals some striking peculiarities of Newtonian inspiration with regard to interparticle relationships. There is, first of all, the hypothesis that atoms of the same species exercise a repulsive force upon each other. After all, it is strange that this idea should have survived during the interval development of the Daltonian doctrine. The precise course of this development as reported by Dalton himself was severely criticized in the past, but according to Thackray's recent monograph, Dalton is perfectly trustworthy. Now, it can be established that, when elaborating a doctrine of the mixture of gases, Dalton started from Proposition XVIII of Book II of the *Principia,*[61] where Newton stipulated that the particles of an "elastic fluid" *(fluidum elasticum),* i.e. of a gas, are repelled with a force which diminishes in inverse proportion to the distance between them. What Newton says and demonstrates with a certain reservation is for Dalton an "immutable law." In the beginning, Dalton thought that there were as many repulsive forces as there were gaseous bodies. This enabled him to explain that the gases in a gaseous mixture behave independently from each other (the law of partial pressures). In order to understand the chemical reactions within a gaseous state, he was also obliged to allow for an attractive force between the molecules of different species. The result of such reaction – the formation of water from the elements, for example – would then be an aggregate of

molecules of the same species which, therefore, exercise a repulsive force upon each other.

Later Dalto exchanged these, so to say, *elective* repulsions for the hypothesis of the "caloric," which states that each particle is surrounded by an atmosphere of the matter of heat, an atmosphere which was supposed to be responsible for the uniform repulsion of every molecule by every other molecule, without taking into account the species. This certainly is the view proposed by the *New system [...],* and makes the idea that the elective repulsion is maintained in order to support the gradation of preference in the formation of chemical combinations all the more remarkable.

Another interesting point concerns the interpretation of the gaseous state in itself. Following Newton, Dalton held the view that the gaseous state – like the dissolved state of a body in a liquid – is a *static* condition in which the molecules by their mutual repulsion keep themselves at a certain distance from each other. As a matter of fact, Newton had said in the Scholium following the above mentioned proposition that on the basis of that hypotheses one could *mathematically* deduce the behavior of "elastic fluids." Whether such a fluid would in fact consist of mutually repulsive particles would definitely remain a "physical" question.

A third and last point of interest regarding the influence of the Newtonian tradition on Dalton is the precise nature of the various species of atoms. Certainly, for Dalton the "atom" is not a more or less complex assemblage of ultimate solid bodies; nor does he know the distinction between "physical principles" (viz. those ultimate solid bodies) and "chemical principles," the latter particles being the products of chemical analysis. He rather compares his "atoms" with celestial bodies:

> No new creation or destruction of matter is within the reach of chemical agency. We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen.[62]

But, in order to justify his break with a tradition which required one single and same prime matter for all the species of atoms, Dalton is more Newtonian than Newton. Eager to astonish his adversaries, he quotes in 1810 the "Query 31" of the *Opticks,* and specifically the passage in which Newton suggests the possibility that God could create different species of atoms characterized by different densities and forces. Totally unashamed, Dalton omitted from his quotation the specification that the species of atoms would be destined to constitute different worlds in different places of the universe.[63] The slogan had become: Newton is dead,

long live Newton!

5. *Conclusions*

Newton's theory of matter is a "classical" theory in the sense that it attempts to fulfill all the conditions put forward by a tradition which went back to Antiquity. A special peculiarity of Newton appears to be his insistence, at least in the manuscripts, upon alchemy.

During the 18th century, the Newtonian doctrine had been forced to compete with the less pretentious but far more efficient doctrine of Stahl in a battle which it was bound to lose. Yet, one of the key ideas of Newtonianism, viz. the idea concerning the interparticle forces (either attraction alone or attraction combined with repulsion) infiltrated the Stahlian molecular doctrine.

The Newtonian-Stahlian doctrine was revived during the last decades of the 18th century by the "chemical revolution" brought about by Lavoisier. The "mariage à trois" which resulted from it, gave birth to the molecular theory of Dalton, which was a crucial event in the genesis of modern molecularism.

University of Groningen, The Netherlands

NOTES

1. K. Lasswitz, *Geschichte der Atomistik vom Mittelalter bis Newton,* Hamburg/Leipzig, 1890 (reprint Hildesheim, 1963), vol. II, p. 556 (my translation).
2. See the publications of H. Metzger, I.B. Cohen, M.Guerlac, A.R. and M.B. Hall, R.H. Kargon, R.E. Schofield, E. McMullin and B.J.T. Dobbs.
3. A. Thackray, *Atoms and powers – an Essay on Newtonian Matter-theory and the Development of Chemistry,* Cambridge, MA., 1970.
4. *Ibid.*, pp. 56-60.

I would like to thank Professor F. Figala (Technische Universität, Munich) for her generous cooperation.

5. *Ibid.*, pp. 23-24, 45, 187-188, and 204.
6. B.J.T. Dobbs, *The Foundations of Newton's Alchemy or "The Hunting of the Greene Lyon"*, Cambridge, 1975.
7. K. Figala,
   a) *Die "Kompositionshierarchie" der Materie – Newtons quantitative Theorie und Interpretation der qualitativen Alchemie,* Habilitationsschrift, Munich, 1977.
   b) Newton as Alchemist, *History of Science 15* (1977) 102-137.
   c) Die exakte Alchemie von Isaac Newton – Seine "gesetzmässige" Interpretation der Alchemie dargestellt am Beispiel einiger ihn beeinflussender Autoren, *Verhandlungen der Naturforschers-Gesellschaft Basel 94* (1984) 157-228.
8. The modern theory of phenomenal matter knows, in a first approximation, two complementary doctrines to explain substantial differences. Besides the *molecular* interpretation, there is the *lattice* theory. According to the latter, the atoms or ions of a compound are arranged in a specific manner into a three-dimensional crystal lattice. The "substantial species", then, is defined by the numeric proportion between the constituent particles and their spatial arrangement. Of course, the lattice theory only holds for *solid* substances. For molecular bodies in the solid phase, both theories coincide. One of the first lattice theories of matter can be found in the works of Joachim Jungius (1587-1657); see footnote 28., Section 3.2, p. 74-87.
9. I. Newton, *Opticks: or, a Treatise of the Refractions, Refractions, Inflections and Colours of Light,* 4th edition, London, 1730. Quotations are according to the reprint edition New York, 1952.
10. *Ibid.*, Book II, Part III.
11. *Ibid.*, Book III, Part I, Query 31, p. 394.
12. *Ibid.*, Book II, Part III, Proposition VIII, p. 269.
13. See footnote 3., p. 60.
14. See footnote 9., Book II, Part III, Proposition VIII, p. 268.
15. I. Newton, *De natura acidorum,* inserted in H.W. Turnbull ed., *The Correspondence of Isaac Newton,* volume III, Cambridge, 1961, pp. 256-257.
16. See footnote 3., p. 64. For a detailed analysis of the "nutshell" theory, see also: A. Thackray, "Matter in a nut-shell": Newton's Opticks and Eightteenth-Century Chemistry, *Ambix 15* (1968), pp. 29-53.
17. See footnote 9., Book III, Part I, Query 31, p. 400. Cf. footnote 3., p. 15.
18. I. Newton, *Some Thoughts about the Nature of Acids,* inserted in H.W. Turnbull ed., *The Correspondence of Isaac Newton,* volume III, Cambridge, 1961, pp. 257-258.
19. See footnote 9., Book II, Part III, Proposition VIII, p. 269.
20. Cf. C. Vilain, Le modèle mécanique de la réfraction dans les *Principia,* paper read at the colloquy *Les Principia de Newton: questions et commentaires,* Paris, March 7th, 1987. the text of this paper will be inserted in *Revue d'Histoire des Sciences 40* (3-4)(1987) (forthcoming).
21. See footnote 6., p. 217-222.
22. *Ibid*, p. 220. For a detailed analysis of the origin and intrinsic value of the sulphur-mercury theory, see R. Hooykaas, *Het begrip element in zijn historisch-wijsgerige ontwikkeling,* Ph.D. thesis, Utrecht (The Netherlands), 1983, p. 39-73. A xerox copy of an English translation of this work, entitled *The Concept of Element - its historical-philosophical development,* is obtainable from the author of the present article. See also

R. Hooykaas, the experimental origin of chemical atomic and molecular theory before Boyle, which appeared in R. Hooykaas, *Selected Studies in History of Science,* Coimbra, 1983, p. 285-307 (particularly p. 292-297).

23. See footnote 7b, p. 116.
24. See footnote 7c, p. 167. According to Professor Figala (private communication) Newton does not make clear in his manuscripts how he arrived at just this numerical proportion. Cf. A.R. Hall and M.B. Hall, *Unpublished Scientific Papers of Isaac Newton. A Selection from the Portsmouth Collection in the University Library,* Cambridge, 1962, p. 314 and 317. In *Opticks*, on the other hand, Newton deduces that "[...] water has above forty times more pores than parts," and this on the base of the relative density of gold (19 times that of water) and on the assumption that gold-foil would be permeable to water. See footnote 9., Book II, Part III, Proposition VIII, p. 267. With regard to this presumed permeability of gold to water Newton prudently adds "as I have been inform'd by an eye witness" *(ibidem).*
25. See also footnote 7c, p. 168.
26. Cf. *ibidem*, p. 170.
27. *Ibidem*, p. 169. See also footnote 6., p. 231 and compare footnote 9., Book III, Part I, Query 30, p. 374-375 and I. Newton *Philosophiae naturalis principia mathematica,* London, 1687, p. 402 (Hypothesis 3).
28. H.H. Kubbinga, *Le développement historique du concept de "molécule" dans les sciences de la nature jusqu'à la fin du XVIIIe siècle,* Ph.D. thesis, École des Hautes Études en Sciences Sociales, Paris, 1983. I am now preparing a monograph on *L'Histoire du concept de "molécule"* (until ca. 1925).
29. H.H. Kubbinga,
    a) Les premières théories "moléculaires": Isaac Beeckman (1620) et Sébastien Basson (1621) - Le concept d'"individu substantiel" et d'"espèce substantielle", *Revue d'Histoire des Sciences 37* (3-4) 215-233 (1984).
    b) La première spécification dite "moléculaire" de l'atomisme épicurien: Isaac Beeckman (1620) et le concept d'"individu substantiel", *Lias 11* (2) 287-306 (1984).
30. H.H. Kubbinga, Isaac Beeckman (1588-1637) en de molecularisering van de microcosmos. Een aspect van de "mechanisering van het wereldbeeld" [Isaac Beeckman (1588-1637) and the molecularization of the microcosm. An aspect of the "mechanization of the world picture"], *De zeventiende eeuw* (The Netherlands) *2* (2) 59-79 (1986) (with a summary in French).
31. See footnote 3., p. 56.
32. See footnote 29b, p. 300.
33. See footnote 28., Chapter IV, p. 121-156.
34. See for instance: G.E. Stahl, De differentia mixti, texti, aggregati, individui (1700), inserted in G.E. Stahl, *Observationis physico-chymico-medicae curiosae [..],* Halle, 1709, p. 47-63.
35. See footnote 34., p. 60.
36. G.E. Stahl, *Specimen Beccherianum [..],* a commentary on J.J. Becher's *Physicae subterraneae libri duo [..]* (Frankfort, 1669), added to the 1703 reprint edition of this work, that was prepared by Stahl.

37. G.E. Stahl, De divisionis et diffissionis differentia (1703), inserted in G.E. Stahl, *Observationes physico-chymico, medicae curiosae [..],* Halle, 1709, p. 63-76.
38. See footnoe 28., Chapter V, p. 157-196. See also M. Crosland, the development of the concept of the gaseous state as a third state of matter, *Proceedings of the 10th International Congress of the History of Science* (Paris, 1962) *II* p. 851-854 and R. Fox, *The caloric theory of gases, from Lavoisier to Regnault,* Oxford, 1971, Chapter I.
    More often than not Stahlianism has been judged from the sole viewpoint of the vicissitudes of the phlogiston theory. *E.g.,* Partington neglected Stahl's molecular theory almost entirely (J.R. Partington, *A history of chemistry,* volume II, London, 1961, Chapter XVIII). It is, moreover, very illuminating to see that neither Stahl's corpuscular theory nor his doctrine of the relative complexity of phenomenal substances have been dealt with at the symposium *Georg Stahl (1659-1734) und seine Zeit* (Halle-Wittenberg, 1984). For a better interpretation of Stahlianism, see H. Metzger, *Newton, Stahl, Boerhaave et la doctrine chimique,* Paris, 1930, part II, p. 91-188 (La doctrine chimique de Stahl et de ses disciples). See also H.H. Kubbinga, Hélène Metzger et la théorie corpusculaire des stahliens au XVIIIe siècle, contribution to the colloquy *Hélène Metzger (1889-1944): son oeuvre et son influence sur l'historiographie des sciences* (Paris, May 21-23, 1985); it will appear in the congress proceedings, entitled *Études sur Hélène Metzger* (Paris, 1988).
39. The proportion of matter:void $= 1:(2^n - 1)$ can be considered as something like a 'phlogiston' law, in the sense that it determines the spatial conditions that must be fulfilled in order that the 'particles of the ultimate composition' are transformed, the one successively into the other, starting from the common trunk.
40. See footnote 28., Chapters IV and V, p. 121-196.
41. G.-F. Venel, article 'Chymie' in D. Diderot and J. Le Rond d'Alembert (ed.), Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers, volume III (Neuchâtel, 1753), p. 416b.
42. *Ibidem*, p. 416a,b.
43. A.L. Levoisier, *Traité élémentaire de chimie,* Paris, 1789. We quote according to the translation of Robert Kerr (Edinburgh, 1790; reprint New York, 1965), entitled *Elements of Chemistry*. See especially p. xxiv.
44. Some traces of the Stahlian doctrine of the relative complexity are to be found in the terminology utilized in the *Méthode de nomenclature chimique, proposée par MM. de Morveau, Lavoisier, Bertholet [sic], & de Fourcroy,* Paris, 1787.
45. See footnote, 43., p. 5.
46. *Ibidem*, p. 3.
47. *Ibidem*, p. 7.
48. *Ibidem*, p. 8.
49. *Ibidem*, p. 25.
50. See footnote 9., Book III, Part I, Query 31, p. 395.
51. H. Boerhaave, *Elementa chemiae, quae anniversario labore docuit, in publicis, privatisque scholis,* Leyden, 1732, volume I, p. 175 ff.
52. See footnote 43., p. 15.

53. *Ibidem*, p. 383.
54. A. Thackray, *John Dalton - Critical assessments of his life and science,* Cambridge (Mass.), 1972, p. 66.
55. J. Dalton, *A new System of Chemical Philosophy* London, 1808 (volume I, part I), 1810 (volume I, part II), 1827 (volume II, part I). See especially volume I, part I, chapter II, p. 141-144.
56. *Ibidem*, volume I, part I, p. 141.
57. *Ibidem*, volume I, part I, p. 143: "If some of the particles of water were heavier than others, if a parcel of the liquid on any occasion were constituted principally of these heavier particles, it must be supposed to affect the specific gravity of the mass, a circumstance not known."
58. *Ibidem*, volume I, part I, p. 143.
59. Similar views were developed before by Joachim Jungius (1587-1657; see footnote 28., section 3.2, p. 74-87) and Georg Ernest Stahl (see footnote 28., section 4.4, p. 133-147).
60. See footnote 55., volume I, part I, chapter III, p. 211-216.
61. *Ibidem*, volume I, part I, p. 168.
62. *Ibidem*, volume I, part I, p. 212.
63. H.E. Roscoe and A. Harden, *A New View of the Origin of Dalton's Atomic Theory,* London, 1896, p. 112. On this point, I completely endorse the interpretation of A. Thackray. Cf. footnote 3., p. 274.

PETER FONG

ETHICS, POLITICS AND SOCIOLOGY AS NEWTONIAN SCIENCES

Newton's scientific and philosophical legacy can be brought out most clearly by studying the "Newtoniazation" of other scientific disciplines. Astronomy, chemistry and biology, starting from a completely different empirical basis, have been assimilated and have finally become, in principle, a subdivision of Newtonian physics. This remarkable power is based on the fact that Newtonianism is a logic system that is complete, exhaustive and exclusive. Once a system is identified as mechanical, the Newtonian control becomes effective all the way.

But the point that the universe is mechanical is not a matter of logic but one of empirical facts. And it took laborious scientific investigations over the centuries to eventually establish that the physical universe is indeed mechanical (for the sake of discussion in this paper defined as a system of particles interacting with forces). And the road of discovery is by no means smooth and straight.

In astronomy, it was once thought that the advance of the perihelion and the change of eccentricity of the planetary orbits were expressions of God's will that was beyond the control of Newton's laws. These phenomena were eventually explained by the very laws which Newton applied to the interactions among the planets. This established the planetary system as a purely mechanical system in which there was no role for non-mechanical entities.

Chemistery presented challenges. Things may disappear and reappear. Concepts such as those of phlogiston and caloric competed with the Newtonian concepts. But eventually it was learned that chemistry was entirely reducible to physics as a mechanical system.

Biology presented even greater challenges. There are phenomena in biology apparently not shared by inanimate material agencies: birth, growth, reproduction, and death. There is no sex in physics and chemistry. But through molecular biology most manifestations, including sex,[1] can now be explained on a mechanical basis. All previously supposed non-mechanical elements have been eliminated.

Since humans are a part of the biological system, it is logical that there be a Newtonian derivation of the social sciences. However, we cannot ignore the existence of apparently non-mechanical elements manifested in such notions as virtue, evil, liberty ang equality. These will have to be accounted for in a Newtonian framework. Then the social sciences can be developed as hard sciences - like physics and chemistry -

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that are well-established, complete, exhaustive and exclusive.[2] Such development would be one of the most important intellectual developments of this century. Right now, the existing social sciences are like the natural sciences before Newton, with conflicting concepts and principles that are competing with each other, without a body of firmly established knowledge for the solution of practical problems.

In pursuing such a program a problem immediately arises when morality is involved. Morality has always been thought of as beyond science and it has often been associated with non-mechanical entities. Whereas the abstract concepts of vitue and evil are indeed not scientific topics, morality is manifested in human behaviors which are legitimate subjects of scientific study. Instead of dealing with virtue and evil in the abstract, let us study virtuous behaviors and evil behaviors as happenings in the material world. This way we may gain insight into the issue of morality.

It will not take long to realize that there exist two types of behaviors (such as honesty and deceit) that are regarded as respectively virtuous and evil in *all* societies. Moreover, evil behaviors are mostly selfish and anti-social while virtuous behaviors are generally coopertive and altruistic. These terms are used merely as factual classifications without any sense of value judgment.

The ensuing scientific question is the origin of these types of behavior. It is quite obvious that selfish behavior, such as agression and violence, is necessary for a solitary individual trying to survive in the jungle (such behavior may therefore objectively be called solitary behavior). Such is the behavior of animals. And as such, its origin may be understood from a Darwinian evolutional point of view. In fact it should be regarded as a set of genetic treasures arduously accumulated over millions of years, enabling a solitary individual to eke out a living. However, such behavior goes against the norms of social living we currently pursue and its status has been changed from treasure to trash, and is regarded as evil (here we see the divergence of objective fact and subjective judgment). All seven capital sins are treasure to the solitary individual but trash to members of society.

It is also easy to see that the cooperative type of behavior, such as compassion and helpfulness, is beneficial to the *survival of society* (it may be designated by the neutral term of social behavior). Therefore its origin may also be explained from an evolutional point of view, albeit not of the original Darwinian kind. Here we consider society (instead of the individual) as an entity struggling for existence in the eco-system; and therefore its behavior is subject to the same evolutional mechanism of "mutation" and natural selection.

In order that the evolutional principle may operate to bring about the social (virtual) behaviors there must be an incipient society in which mutation and selection can take place. And here we encounter a serious problem regarding the origin of the ancient society. The difficulty is that the standard mechanism – random fluctuation – which works in physics, chemistry and other areas of biology, does not work here. Before the appearance of a society, individuals, having survived in the jungle for ages up to that point, have already accumulated a large body of genetic treasures – agression, violence, and so on – enabling them to persue a solitary living. These traits are bound to work against the formation and existence of any incipient society, just like they still function as destabilizing forces in our society today. If random fluctuation brings about an incipient society, no matter how rudimentary, such society will soon be destroyed by these human "evils" and it will not serve as a "seed" growing into a more complex society according to the evolutional mechanism.

However, the origin of the incipient society may be detected by observing a primate society. Apes and monkeys live in small groups that have a definite social structure. Their society is organized on the basis of the *principle of dominance.* When two monkeys meet, one will raise its tail indicating its dominant status and the other will lower its tail displaying submission. The dominant one has priority in the domain of food and sex. Social disputes are thus settled by dominance ranking without a fight, which is advantageous for the survival of the society. The ranks of all members form a linearly ordered sequence. The highest-ranking member, referred to as the alpha male by primatologists, is the monkey king in folklore. He is the king in every sense of the term.

The alpha-male-controlled society is a natural product of the law of the jungle – the pursuit of selfish goals by brute force, albeit in a slightly more civilized form (elimination of the internecine fights by dominance ranking). Under proper conditions it may be expected to emerge in evolutional history without need for, or the help of, supernatural intervention or sublime human qualities (it only needs the bestial qualities of selfishness and brute force). Once it has emerged, it may play the role of an incipient society, not on purpose but as a by-product. With this seed as the starting point the evolutional process can now operate to bring out the cooperative and altruistic behaviors leading to a higher form of society which is completely different in form, structure and purpose from the original incipient society. The important point is that it began in a *historical accident.*

Once some form of society has come into being, the advantage of social living over solitary living becomes evident. The group can forage and defend itself better than a solitary individual. Social living thus eventual-

ly wins out and eliminates solitary living from the evolutional process. Once the bridge has been crossed it is burned we are stuck with social living for better or for worse. It should be recognized that the advantages of social living are unpremeditated fallouts of a historical accident.

Already in some primate societies one sees the emergence to the sharing-food behavior, a break from the solitary animal's instinct of hoarding food for its own self. This is a cooperative behavior. It is beneficial for the survival of society although it calls for some sacrifice on the individual's part. Societies with this behavior can survive better and eventually eliminate other societies in the evolutional process. Human society, the top winner in evolution, may be expected to be possessed of this feature in a more complex form. Indeed, this is the *origin of the principle of social welfare.*

In such a society, individuals sharing food with others will be regarded as virtuous while those hoarding food for themselves will be regarded as evil. The concepts of virtue and evil now emerge as an abstraction of the social and solitary types of behavior. Morality can thus be studied on a behavioral basis instead of as a philosophical and religious topic.

In the same manner, other types of cooperative and altruistic behavior may be expected to emerge, which will be reflected in the appearance of the other virtues. Eventually humans become endowed with another set of genetic treasures, the cooperative and altruistic forms of behavior, over and above the first set of selfish forms of behavior.

But now the next serious problem arises. The two sets of behavior contradict each other, e.g. one consists in sharing food,the other in hoarding food. An individual can do only one of the two. If historically an individual has already developed the behavior of hoarding food, how can he develop a conflicting behavior? Once the two drives co-exist, how can the individual decide which way to go when coming upon a particular intersection?

The pursuit of a certain type of behavior is usually driven by a set of stimuli, positive and negative. The acquisition of food is driven, not by a consideration of nutrition and energy, but by the positive stimulus of gourmet pleasure and the negative stimulus of hunger pain. The pursuit of sex is not motivated by the perpetuation of the species, but by the positive stimulus of carnal pleasure and the negative stimulus of sex deprivation. The perpetuation of species is an accidental by-product. This is the way the biological system works.

The pursuit of cooperative behaviors are also driven in a simular way. The positive stimulus is the sense of *glory,* which is fulfilled by the physiological state of *exaltation.* The negative stimulus is the sense of *guilt* fulfilled by the feeling of *shame.* In developing a behavior pattern

the evolutional process must also develop these stimuli that drive the behavior. The stimuli driving the virtuous types of behavior can be expected to emerge in the same way as those driving the selfish types of behavior (lust and gluttony). Thus God's work and the Devil's work may be explained on the same scientific basis. They will emerge under the evolutional pressure. No supernatural intervention is needed to bring about the non-bestial qualities.

Once these higher mental qualities – honor, glory, guilt, humiliation – are developed, one may say *the soul has emerged* in the evolutional process. These are uniquely human qualities not shared by beasts. And when a novelist deals with the soul, all that is involved is these higher emotional activities. If the soul has a supernatural component, it is irrelevant in all practical considerations.

At this point, an entirely new feature of evolution makes its first appearance. The evolution of the individual and the evolution of society are two independent affairs, each of which is irrelevant and indeed contradictory to the other. But the results of both are all invested in the same individual, who begets two strands of genetic heritage that are contradictory to each other. How can the two co-exist in the same person and how can the individual cope with this conflict? The problem, in another form, is the long-standing and well known issue in philosophy and religion – the conflict between virtue and vice in human beings.

The solitary behavior, related to food and sex, is controlled by the old brain whereas the corresponding social behavior (table manners, sex etiquette) is controlled by the modern brain (the cerebral cortex). The cortex exerts effective control over the old brain by the mechanism of *suppression* through *inhibitive nerve wirings*. Under normal conditions, social behavior reigns supreme. But when its control relaxes, as in dreams, or when it is defective, as in psychosis, solitary behavior may become unleashed and manifest itself in the form of anti-social behavior. Freud attributed this mechanism to the *subconscious*. We have now derived its origin.

For more complex behavior both the selfish and cooperative motives wil exert their inluence and the individual is pulled in two opposite directions. Usually the stronger pull at the moment of confrontation wins out. But that does not imply a satisfactory final solution. St. Peter denied Jesus thrice, obviously prompted by the impulse of self-preservation. But the act was deemed cowardly, bordering on betrayal. And he was inflicted with a deep sense of shame which very likely prepared him for his eventual martyrdom. In the first battle against the Persians Urytus was cowardly. But the humiliation he experienced was so unbearable that he fought to death in the next battle. Both the virtuous and the evil type of

behavior are driven by the positive and negative stimuli. The "resultant force" decides which direction to take.

This decision process is the scientific basis of a discussion of problems of *moral upbringing, maintaining social order and administering criminal justice.* Yet it is entirely a behavioral problem subject to scientific analysis and control. Here, one faces the issue of *nature versus nurture.* Both virtuous and evil behavior have a genetic origin. But the decision making is also influenced by personal experience as it is evident in the last two cited examples. The problem regarding the question what kind of experience has what kind of influence is a behavioral problem. The scientific results provide the basis for the study of education, politics and criminology.

There are yet more complex issues for which even the combination of genetics and experience cannot provide a ready answer. The conflicting emotional drives often result in a heart-rending situation. The individual involved will have to go through a soul-searching trauma, a meditative process to sort out all the relevant elements in order to make a proper decision. At this stage moral behavior is neither nature nor nurture but a rational process. Here we come upon the *rational basis of morality.* Previously morality had been thought to be beyond reason.

The soul-searching involves a balance of all interests concerned. A decision that would benefit all parties would be the best. Unfortunately this may not be possible all the time and the best solution will require that someone's interest be sacrificed. The individual deciding to sacrifice his interest at the moment of confrontation so that others (including he himself in the future) may benefit will be considered to have committed a highly moral act.

An individual is driven to such a moral act not only by the positive and negative stimuli emerging from himself, but also from the attitude of his fellow human beings. In this respect the positive stimulus is *admiration and applause.* The negative stimulus is *contempt and denunciation.* These are the other higher emotional types of behavior that must be and can be developed in the evolutional process.

To balance the interests of all parties involved is actually to seek a compromise. Both the solitary (evil) and social (virtuous) types of behavior are genetically determined natural phenomena. There are no *a priori,* logical reasons to uphold and and deny the other. The conflict is the result of a historical accident (the formation of the incipient society) and the subsequent development (the burning of the bridge after crossing it). Under such circumstances the only possible solution is compromise. This leads to the *principle of tolerance* as a way to attain social tranquillity. Previously moral action was usually thought of as a holy war between

God and Devil beyond negotiation. Any compromise is regarded as a capitulation of principle. With these ideas in mind, one is ready to develop a new *science of ethics*.

Many soul-searching, heart-rending situations can arise without any fault on the individual agent's part. He can only blame the star-crossed fate that befalls upon him. When that happens, we are in the presence of high drama, the subject matter of novels, plays and operas. We have thus derived the origin of these creations in the humanities. A good story capitalizes upon the emotional clashes resulting from the conflicting interests. Aïda, Tosca, and so many others, are innocent by-standers of history *(Vissi d'arte, vissi d'amore)* who are accidentally dragged into the whirlwind of social conflict and eventually must take traumatic decisions in order to resolve their entanglement.

Having delineated the evolutional path to humanity, we now ask what distinguishes homo sapiens from the other primates so that he alone developed human qualities. Can some ape eventually develop compassion, sacrifice, honor, glory, guilt, humiliation, admiration, contempt etc. on the sole basis of the natural mechanism we have just explicated? If so, must one treat them as human beings? Are we still entitled to eat their meat, wear their skin and use them for medical experiments?

The emergence of uniquely human qualities (the soul), which did not exist in the genetic system before, does require a genetic basis to get started (a crocodile will not do so, no matter how favorable his evolutional environment). And such a basis can be found in the highly developed parent-child relations, particularly in the two-parent nuclear family, of homo sapiens. Here the ancestors of human beings branched off from the apes (male apes do not help in the raising of children). Male elders dominate, the young submit. The dominance-based social organization thus contains a genetic basis for the further development and greater sophistication of human beings. Many forms of social behavior, such as love, compassion and sacrifice originated in parent-child behavior. Eventually this development leads to the *plasticity of human nature,* the *ability of self-control* and so on.

The unique features of homo sapiens, the two-parent nuclear family, biped locomotion, and the use of language, seem to be the results of a set of rare natural occurrences combined with a singular evolutional path which cannot be elaborated upon here. The chance of duplicating this history is extremely small. The mathematical probability of a *second human emergence* on earth (and even in the galaxy) seems exceedingly low and may be excluded on account of the present human dominance. Thus man may be unique in practice, if not in principle.

We now turn our attention to political science. Historically, all human political systems of the past as well as most of the present ones are *despotic* – an illogical system sustained by raw power without inspiration or rationale. Genetically it is actually an extension of the alpha-male politics of the apes based upon the principle of dominance. The despot is the alpha male in every sense of the term. The principles and practices of the despotic system were already well developed by the apes.

From the scientific point of view the despotic system is a system that works. In fact, until recently it was the only one that worked in organizing a group of humans into a cooperative society. The reason is that solitary individuals are determined by genetics to be self-seeking. Generally, if not exclusively, they can set aside self-types of behavior and pursue cooperative types of behavior in an organized society only under the coercion of brute force. This feature is institutionalized in a rank-ordered society under the principle of dominance.

But there is a price to be paid for the forbidden fruit. The mass of people is suppressed and oppressed; they trade away their freedom in return for security. Even the alpha male must pay dearly. Peace and order can be maintained only as long as the dominance-rank structure remains intact, and this requires that the top rank be omnipotent. When a change in top leadership is made necessary (succession of the throne) by death or impotence, nothing can guarantee peace and order, and all violence may break loose. Conspiracy and intrigue, or extended wars, often accompany the succession of power in despotic systems. (Primogeniture in the feudal system was actually an enlightened improvement.) Most alpha males in ape societies are killed by young challengers fighting their way up the dominance rank to the top. Most despots die a violent death. The problem of succession in despotic systems is not just an unpleasant afterthougt. It comes from the very essence of the despotic system.

If human politics remains in the despotic stage, human beings may indeed be no higher than animals. But the emergence of the *democratic system* marks a drastically new development. It is indeed more humane and civilized; it is uniquely human and is not shared by animals. The study of its emergence is thus an interesting and important problem in political science. It is also important in promoting democracy in the rest of the world today.

The point of departure is that democracy substitutes the *principle of equality* for the principle of dominance. People in Western democracy tend to think of democracy as being an enlightened creation, if not a Divine gift, and to regard despotism as barbaric and inhuman. Science offers a different picture. In fact the principle of equality, just like the principle of dominance, emerges as a more sophisticated manifestation of

the law of the jungle, and can be expected to appear in the course of evolution without Divine intervention or sublime human qualities.

When two groups fight, usually the side with the larger number wins. When this fact is experienced over and over again, it will dawn upon reasonably intelligent beings that the outcome of a fight can be decided generally by counting the numbers instead of going through the gory details of tooth-claw engagement. Thus originated the practice of the raising of hands, not to fight, but to be counted; and the issues were decided by the majority of the contending sides. This is the origin of the *voting practice*. It saves life and energy, and is obviously advantageous from an evolutional point of view. By its adoption humanity advanced by a giant stride.

The majority principle and the vote counting is based on the notion of one-man-one-vote. This implies equal political power for each voter, and this is diametrically different from the rank-ordered vertical dominance system. The concept of *equality* thus emerged in the evolutional process as a historical product, not as a philosophical principle.

As a way of decision making the voting procedure replaces the previous practice of decision by coercion based on the principle of dominance. The dominance rank thus loses its essence and disappears as a social institution. Without the dominance rank the little people are freed from oppression. The concept of *liberty* now emerges as a historical product, not as philosophical principle.

With the transfer of power determined by majority vote, the alpha male is relieved of the trauma of certain death in the struggle for succession. The foremost beneficiary of democracy is not the little man at the bottom but the despot at the top. By virtue of the democratic system it is unanimously proclaimed that the democratic system is a better alternative than the despotic one, and this not on philosophical grounds but on the basis of the fact that the system guarantees the protection of every member of society from the top to the bottom.

The maintenance of social order amongst a group of self-seeking beings still needs brute force. But instead of coming from a historical accident – the alpha male – it now comes from the *delegation of limited power* from the voters to the elected officials who are subject to *checks and balances*. Political operations are still influenced by personal charisma. The "strong man" may still usurp the democratic process. The institutional safeguard is the *secret ballot* in virtue of which every voter can exercise his share of power without coercion. Even Gorbachev has now advocated the secret ballot.

These democratic institutions, evolving like an operon in genetics which consists of a series of genes working together to accomplish one definite mission, are created in an effort to retain the social structure which

historically descends from the alpha male system but which is now free from alpha-male-ism. (The sugar-oxidation operon, replacing the fermentation operon, still contains chemical steps that are distinctively of fermentation origin.) As such they are a haphazard patchwork not expected to be ideal and perfect. They are no God-ordained panacea; they work, albeit clumsily and sometimes ridiculously. Old and young, strong and weak are, of course, not equal in fighting ability. In fact, with the exception of identical twins, no two human beings are exactly equal. But, in virtue of the principle of equality, they are treated as equals, because only by such treatment and the attendant democratic institutions can there be a workable system to replace the despotic system based on dominance. Thus in the United Nations, there is the unavoidable absurdity of equating the United States and Albanian in voting power.

As philosophical principles, liberty and equality could be controversial. From the evolutional point of view, however, the change to democracy is a natural process similar to the change from fermentation to oxidation. Politics can be studied as a 'hard' science.

This scientific view, if novel, is nevertheless consonant with the ancient wisdom expressed in some of the major religions. We are now in a position to give a scientific interpretation of the Biblical tale of Temptation and Fall. Life in the Garden of Eden is solitary, without social institutions and therefore no social woes. Then a historical accident happened – the temptation of the benefits of social living. But human beings at that stage were genetically built to pursue solitary life. Thus the sweet fruit was forbidden. But the temptation was too great and humans succumbed to it. The result was original sin – the emergence of evil that man is born with. The conflict of the old individual interest and the new social interest is the source of all earthly woes. The significance of the concept of original sin consists in that it treats the virtue-evil dichotomy not as a philosophical contradiction but as a historical accident. In this respect it pre-dates our view by two thousand years. As a result, *monism* can be upheld without the pitfalls of *dualism* that beset all religions and philosophies.

Not the least of the fallouts of evolution was the successful development of Newtonianism by which the monistic idea that a single self-consistent set of principles entails all happenings in the universe was worked out completely (this paper represents the last segment of the program).

In Marxism the original sin takes the mutant form of dialectical contradiction. From the ministerial viewpoint the priest's original sin has become the contradiction of the 'cadre.'

Confucianism was unique in recognizing that social ethics is rooted in, and is an extension of, family ethics. It anticipated our view that the development of social types of behavior is built on the genetic basis of advanced parent-child relations. The important thing is that ethics now has a biological basis and that it can be built entirely on a human basis without Divine help. The only help needed is education and self-cultivation – the main concern of Confucianism. Thus originated Confucian *humanism*. Another unique feature which also anticipated our view was the advocation of compromise (the Principle of the Mean) for the resolution of conflicts, a doctrine which is anathema to philosophy.

Long ago, the ancient Hindu philosophers had contemplated the fact of *causality*. When Buddha came, he realized that all happenings in the universe are controlled by causality and that there is simply no room for any gods to play a part. Buddhism is the only religion that is completely free from any Deity. It anticipated the Newtonian view by two millennia. Our scientific view that the human world is not a rational design but an accidental juxtaposition of irrelevant elements is also recognized in Hindu philosophy in the notion of the *emptiness* of the world (no purpose, no destiny).

What is life then? Life is the result of the pursuit of food (and other material wealth), sex, power (dominance), knowledge, beauty, honor, glory, applause etc., and the avoidance of guilt, humiliation, contempt, etc. The social organization that makes possible the greatest fulfillment of these pursuits with the least conflicts would be the most desired society. The traditional approach to this goal, as expressed in Catholicism, Confucianism and Buddhism, is to suppress the selfish forms of behavior, thus taking a negative and aloof attitude to worldly affairs. The modern West is the only system that tries to reach this goal by exploiting the selfish types of behavior, encouraging the pursuits of wealth and power through individual initiatives and adversary procedures but tries to eliminate the conflicts by democratic processes and the rule of law.

We have shown how natural sciences, social sciences, the humanities and religion can be unified. The entire world can now be understood from the fundamental physical principles, and this understanding may well be regarded as the culmination of the Newtonian legacy.

Emory University, U.S.A.

NOTES

1. P. Fong, in *Biogenesis Evolution Homeostatis,* A. Locker, ed., Springer, 1973, 93-106.
2. The details will appear in: P. Fong, *East of Eden: the Evolution of Man and Humanity,* New Forum Press, P.O. Box 876, Stillwater, Oklahoma 74076, USA (in press).

GUY DEBROCK

ARISTOTLE WITTGENSTEIN, *ALIAS*

ISAAC NEWTON BETWEEN FACT AND SUBSTANCE

In spite of what many contemporary philosophers might claim, every human being holds certain metaphysical ideas. No more is meant by this than that every human being has some idea of what he considers to be real, as opposed to mere thought or fancy. Generally speaking, there are three sorts of entities which most people would at some point or other hold to be real: things, facts and events. Most people would say that this table is real, that Napoleon's defeat at Waterloo was real, and that the event of their birth was real. Indeed, our 'ontological commitment' to things, facts and events is so immediate and irresistible hat ordinary discourse tends to use the three terms indiscriminately. It seems to make little difference whether one believes in Napoleon defeated, or in the fact of his defeat, or, yet again, in the event of that defeat.

In this paper, it will be argued that, far from being indifferent, the choice of one of the three mentioned views indeed does make a difference, and an attempt will be made to show *what* difference it makes.

More specifically, the purpose of this paper is twofold. In the first place, it will be argued that there is good reason to believe that probably the most significant, though admittedly unrecognized, revolution marking the transition from ancient and medieval thought to the modern conception of the world has consisted in a reversal of emphasis regarding the relationship between thing and fact. Whereas the ancient and medieval world view was based on the conviction that the world was first and foremost a world of things, our modern world view is dominated by the conviction that the world is primarily the set of facts. Secondly, I would like to show that Newton's method of solving problems may have played a decisive role in this reversal, even though Newton himself was unaware of the transition, and in fact was caught between an implicit methodological commitment to facts and an ontological commitment to substance. The latter remark somewhat explains the otherwise mystifying title of the present paper.

In order to forestall any misunderstanding, it should be made clear from the outset that in this paper no preference for either one of these views will be defended. Indeed, it will be suggested that both world views are ultimately inadequate, because neither one leaves any room for the third sort of entity to which we all are committed, viz. the entities, or

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rather pseudo-entities which, for lack of a better word, we call events.

The paper will consist of three parts. In the first part, an attempt will be made to sketch the history of the tension between the worldview in terms of substance and the worldview in terms of facts, and to show how the predominance of the commitment to a world of facts came about. In the second part, a few remarks will be made regarding the comparative anatomy of substance and facts. In the third part, an attempt will be made to show *how* Newton may have contributed to the change in perspective.

1. Substance and Fact

To a large extent, Aristotle's metaphysics can be considered as an explicitation of the foundations of the ontological commitments of his fellow-men, and, more particularly, of the ontological commitments of the Greek philosophers in general. The core of his metaphysics may be found in texts such as the following:

> While 'being' has [several] senses, obviously that which is primarily is that 'what', which indicates the substance of the thing... Clearly then it is in virtue of this category that each of the others *is*. Therefore that which is primarily and *is* simply ... must be substance.[1]

More concretely, Aristotle tells us what sort of entities are good candidates for the rank of substance:

> Substance is thought to belong most obviously to bodies; and so are natural bodies such as fire and water and earth and everything of the sort, and all things that are parts of these or composed of them ..., e.g. the heavens and its parts, stars and moon and sun.[2]

Now let us compare this famous text with the probably equally famous opening of Ludwig Wittgenstein's *Tractatus:*

> 1. The world is all that is the case.

> 1.1 The world is the totality of facts, not of things.
> 1.11 The world is determined by the facts, and by their being *all* the facts.
> 1.2 The world divides into facts.
> 2. What is the case - a fact - is the existence of states of affairs *(Sachverhalten)*.
> 2.011 It is essential to things that they should be possible constituents of affairs.[3]

The opposition is clear. For Aristotle, the world is a collection of things; for Wittgenstein, the world is a collection of facts. Every age gets the metaphysics it deserves. Like Aristotle in his day, Wittgenstein too formulates the hidden assumptions of the world we live in. Indeed, if one looks at the world or at the culture of which we are a part, one cannot but notice that this world appears primarily as a world of facts. No self-respecting person can afford not to get his daily quota of facts through the newspaper or the television. Our educational systems are to a large extent tools for transmitting facts. Young scientists are taught how to collect facts, and politicians and businessmen are sent around the world on fact-finding missions. In fact, - no pun intended - we are so used to live in an environment of facts, that we can hardly imagine a world in which facts would not play an important role. Nor does it take much historical insight to see that the virus of facts invaded the world at the very same time when modern science developed - roughly during the seventeenth and the eighteenth century - gradually forcing us to adapt to it, so that now facts have become the pillar of our entire system of thought. The first newspaper in the modern sense of the word appeared during the 17th century, and the first encyclopaedias were published in the eighteenth century.[4] Their obvious success points to a need for facts which had not existed before, at least not to the same extent. It is true that it would have been unfeasible to publish works of such magnitude before the invention of the printing press, but books had been published for 250 years by the time the encyclopaedias made their appearance.

The point is not that until the seventeenth century there had been no interest for facts, but, more fundamentally, that facts simply had not been part of the world. People did not believe in facts, they believed in things. They believed in God, the stars, the angels, the devils. We no longer believe in things or persons. We believe in facts. When a modern person is asked whether or not he believes in God, he immediately translates the question into another question, viz. whether or not he believes in the fact of God's existence. For the ancient christians, the words "I believe in God" did not at all mean that they believed in the fact of God's ex-

istence, but that they put their trust in God. The martyrs were not put to death because they believed in the fact of God's existence but on account of their greater commitment to their God than to the Roman Emperor.

1.1 The Substance View of the world

The awareness that a historical reversal from a conception of the world in terms of things into the conception of the world in terms of facts had taken place, dawned on me when it struck me that even the best translations of Aristotle's work very frequently make use of the term 'fact,' whereas it never appears in the Stagirite's works for the very simply reason that the Greeks did not have the term.[5] Now, it seems fair to say that, if a certain term is not to be found in a particular language, it probably is because there was no need for it. And thus one may conclude that the world of the Greeks can never have been a world of facts.

Indeed, it is obvious that the majority of the Greek thinkers took it for granted that our world is a world of things. It is well known that, when the Pre-Socratic thinkers asked their first scientific questions regarding the world, those questions had invariably the same form: "What is the sort of thing the world is made of?" Although the various philosophers gave different answers to the question, they nevertheless all agreed that the question was right, and, moreover, they all agreed that whatever the answer was, the world which consisted of things had come forth from some one *thing*, such as water, or air, or fire, or little indivisible things called atoms. This conviction that the question was right was so strong that the members of the school of Pythagoras decided that numbers must be the *things* from which the universe was constructed. Similarly, Plato proposed a theory in which the real world consisted of immutable things which could be grasped only by the eye of the mind.

But it was Aristotle who would formulate the doctrine that was implicitly present in all those early doctrines. For better or for worse, he decided that it was high time to put some order in the often muddled discussions of his predecessors, and he considered it one of his basic tasks as well as one of his major achievements to classify whatever it is we consider to be real. His doctrine of the *Categories* states, as the passage quoted above clearly indicates, that 'being' is an analogous term, a term used in many different senses all of which must derive from one basic meaning. And so, by no more than a perspicacious analysis of ordinary discourse, he found that the basic meaning of 'being' always referred to that kind of reality to which we can ascribe all kinds of features but which itself can never be ascribed to anything else.[6] The basic constituents of reality are

things such as Socrates, Isaac Newton, this tree, this cow, this star, the sun. In short, Aristotle considers a substance whatever has properties of independence and self-identity similar to those we ascribe to ourselves whenever we say 'I'.

In itself, the theory is intuitively attractive because it corresponds to the way we generally perceive the world through the language in which we describe it. Each one of our sentences has a subject which signifies the reality we speak of. The relevant question, however, is whether the theory is tenable when all its consequences are worked out. Here I would briefly like to show at least one remarkable consequence of the theory. In one of the more fascinating passages from his logical works Aristotle discusses the famous riddle of the sea-battle.[7] The question is twofold. First: Can propositions concerning future events be said to be true before the events happen? Secondly, Does the truth of propositions concerning future events imply that these events necessarily will happen. Aristotle readily sees that an affirmative answer to the first question implies an affirmative answer to the second. If it is now true that it will rain tomorrow, then it must necessarily rain tomorrow.[8] But he rejects the view that propositions concerning future events may be said to be true before the events happen. Usually, his defense is interpreted as hinging upon the remark that if propositions concerning future events were true before the events happen, there would be no point in deliberation, or there would be no need for deliberation.[9] But it seems to me that such psychological argument is entirely unworthy of Aristotle, particularly, if one considers that the discussion occurs within a work of pure logic. His real argument is based on the premise that the truth of a proposition must be carried by something in reality. But, according to the ontological principle, the view that true propositions about future events would entail the necessity of these events implies that there must *now* be some substance, the structure of which would make it necessary that something should be happening tomorrow. The only source of necessity, according to Aristotle, lies in the constraints imposed by the form of things. For instance, it is by virtue of his form that every human being is necessarily rational. Thus, according to the same ontological principle, the sea-battle would necessarily occur if and only if there were something in the structure of some things, such as sailors, ships, weapons etc. that would make the actuality of the sea-battle unavoidable. But, since there is nothing in the structure of sailors or ships that either forces sea-battles to happen, or forbids them from happening, nothing can be said about the future sea-battle except that it is neither necessary nor impossible. Clearly, Aristotle solves the riddle of the sea-battle by staying within the limits of the Substance View of the world.

But there are a number of areas in which Aristotle seems to be less aware of the implications of the Substance View of the world. This is particularly the case in his theory of science. True, science is knowledge of the essence of things, i.e. it is knowledge of the answer to the question "What is this or that thing?" So far so good, science is interested in things. But, unfortunately, asking what a thing is amounts to asking what *sort* of thing a particular thing really is. Now, the one feature that separates Aristotle from Plato was that the Stagirite held the position that only *individual* things were real in the strong sense, and that sorts of things (universals) were real only in as much as they were instantiated in individuals.[10] If science asks what sort of thing a thing is, it must necessarily abandon every pretense at knowing *individual* things, and since individual things are the constituents of the world, science cannot know the world. And indeed, Aristotle held the paradoxical (but probably the correct) view that individual things *cannot* be known scientifically.

Aristotle's position already shows that it is probably impossible to simultaneously hold a Substance View of the world and a realistic philosophy of science that is compatible with such view. That the Substance View was not without problems was also clearly seen in other ancient philosophies. Indeed, that very tension explains that, already in antiquity, there are some clear indications of a preference for some sort of Fact View.

1.2 The primacy of facts

Probably the first announcement of the Fact View is to be found in Parmenides' dark saying that "Being and thinking are the same."[11] Without pretending to know exactly what he meant by that sentence, it is clear from the context that, according to him, there was a perfect symmetry between the content of a true proposition and the truth of the proposition. What is is thought in a true proposition, and a true proposition states what really is. Instead of saying what the world was made of, Parmenides in effect prescribed the conditions that had to be met by anything in order for it to be regarded as real. Parmenides does not say: the world is made of thing X or thing Y, but he says: "What is, is." In other words, he states what we would now call a *fact*, and then proceeds to describe this fact as if it were constitutive of a thing. But since he, together with this contemporaries, conceives of this fact within a discourse which is ontologically committed to substance, he treats the fact as if it were a substance with properties. What is is round, impenetrable, indivisible etc.

Whether or not the position of Parmenides is defensible, it undoubtedly contributed - sometimes directly, sometimes indirectly - to three major schools of thought that have influenced, and perhaps dominated, our Western culture ever since: atomism, Platonism and Stoicism. Of these three, Stoicism is the most important for our topic.

Although it was never a great favorite of the history of philosophy, perhaps because of its underdeveloped and ambiguous metaphysics, perhaps because of its emphasis on ethical matters, Stoicism nevertheless had an enormous influence. Whereas neo-Platonism and Aristotelianism were surface currents, Stoicism did its work in a mostly subterranean way, emerging now and then to the surface, at times when either people were less metaphysically preoccupied, or again at times of great moral and political crisis, when Stoicism was always ready to assure us long before Leibniz did, that we live in the best of all possible worlds.

Stoicism may very well have been the first movement in which the world was not thought so much in terms of things, as it was in terms of facts. Stoicism was not the child, but the grandchild of Parmenides. The link between the two was the thesis of the school of Megara, which held not only that true propositions are the faithful expression of reality, and that truth never changes, but also that true propositions, in virtue of their time-neutrality, implied the timeless reality of their content. Hence, they concluded that, given the principle of the excluded middle, i.e. that a proposition is always either true or false, it follows that to any proposition regarding any future event that would ever reveal itself to be true there corresponds a time-neutral state of affairs. It was already seen how Aristotle tried to escape from the implications of the latter belief by holding on to his Substance View.

Stoicism however adopted and adapted the Megarian doctrine. Not only is everything in the world given for all time, or rather regardless of time, but the world is now seen from the perspective of a Greek version of the Indian *pratityasamutpada,* the great chain of causation.[12] Whereas for the Megarians, there was no inherent reason why the time-neutral fact of a sea-battle taking place, say, in June of 1987 should in any way be connected to any other time-neutral fact, the Stoics made facts conditional upon the presence of *logos,* a principle of order, so that every single fact was concatenated with every other fact, and therefore, each single fact was but the expression of the total harmony of the world.

And so it happened that, when the Fathers of the Church took over what they considered to be a heathen instance of wisdom announcing the great truth of Christianity, they did not hesitate to incorporate that aspect of the doctrine of the Stoics.

The adoption of the Stoic doctrine of Providence grafted upon a theology which to a significant extent was influenced by the neo-Platonic version of the substance view, yielded a remarkable ambivalence of doctrine throughout the Middle Ages. On the one hand, the world was a world of things, of substances arranged according to a neo-Platonically inspired scale of Nature, with at the top of the scale, God, and at the bottom something not quite deserving of the term 'substance', matter, and in the middle, man as the hinge between the Spiritual and the material, between the rational and the irrational. The entire drama of all those things comes to life in the myriad sculptures and pictures that grace the cathedrals and the cloisters of medieval monasteries and churches: everywhere there are things, men, women, angels, devils, fish, lambs, doves, serpents, unicorns, gargoyles, all participants in the universal drama of God and his creatures. On the other hand, every constituent of the tragedy was a fact written in the eternal book of God's wisdom which could in no way be subject to change. No wonder that every theological treatise contained some astonishingly clever non-sense when it came to the discussion of man's freedom.

The tension between substance and fact came to a head in the great rationalist systems of the 17th century. Spinoza is most clearly the stepchild of ancient Stoicism, by combining the concept of a unique Substance with an unforgiving chain of mechanical causality. Leibniz most explicitly defended the Aristotelian substance view, but he undermined it by his theory that every substance is matched by its own totally adequate concept expressive of a content which simply consists of the concatenation of facts constituting not only the history of the substance but the substance itself.[13]

But the most fascinating case remains Descartes, whom Newton as a young man had read so well and from the beginning so strongly attacked. And to situate the position of Descartes and Newton, it is necessary to retrace our steps and ask the question: What exactly do we mean when we say that the world consists of facts rather than of things.

2. *Some anatomical remarks on things and facts*

Without pretending to fully discuss, let alone solve, the question which Heidegger so aptly called "Die Frage nach dem Ding,"[14] and the question regarding the structure of facts, it is imperative to at least touch upon the subject. Here we refer to the kind of problem that constitutes

the core of philosophy, questions so basic that every person who is caught up in the hustle and bustle of every day life or of scientific activity finds neither the time nor the energy to concern himself with them. Philosophical questions at first almost always appear slightly ridiculous.

Without reviewing theories of fact and substance I would like to concentrate on one particular aspect of the distinction between thing and fact, which will allow us to better see the specific role which Newton has played in the historic reversal which was mentioned at the beginning of this paper. It appears that in our perception of the world in terms of things, as well as in our perception of the world in terms of facts, abstraction plays an important role. Now, C.S. Peirce, the great, but unfortunately still terribly underestimated American philosopher, has pointed out that there are two modes of abstraction which are all too often considered as identical while they are indeed very different. Peirce makes the distinction between what he, perhaps a bit pompously, calls prescissive abstraction and hypostatic abstraction.[15] It will be shown that hypostatic abstraction is a very important ingredient of most, if not all facts. The meaning of these two expressions can best be elucidated within the context of an analysis of our experience of resp. things and facts.

2.1 Things and prescissive abstraction

Let us then begin with an example of something probably everyone would agree is a thing. On a clear day, I see a jumbo jet approaching a landing strip. I look up and see nothing but the blue sky and that huge thing miraculously hanging in there. We say that we *see* the plane, but in truth, we do no such thing, at least not directly. Our eyes merely register the color blue interrupted by some bluish grey. Somehow, the exclamation "Look at the jumbo" presupposes that we have carved out a part of what is presented to us and that we drop the rest or reduce it to a mere background. This activity of carving out relevant information illustrates something similar to, but not quite the same as what Peirce has called prescissive abstraction. Peirce used the expression to designate an epistemological act. I shall use it here more in the sense of a perceptual act. Our perception of things continually demands such prescissive abstraction.

Certainly, prescissive abstraction is not a sufficient condition for our perception of things, but it is a necessary condition. As Leibniz has so beautifully pointed out, probably the source of basic concepts such as substance, unity and causality is to be found in the basic experience each one of us has of himself, as a unified source of action. But the condition

that makes such projection of ourselves into the world possible is the activity of prescissive abstraction.

2.2 Facts and hypostatic abstraction

Let us now turn to facts. As it was already pointed out, the Greeks did not have the term, and therefore they can be excused from never having described them. 'Fact' is a Latin word and it was used to refer to something done, something achieved, and hence, more generally, to anything that had happened.

Let us list a number of facts.

> I am now standing here.
> $2 + 3 = 5$
> Napoleon was defeated at Waterloo in 1815.
> Gold is an element whose atomic weight is 196.967 and which has a melting point of 1.063 degrees Celsius, a boiling point of 2.966 degrees Celsius and a specific gravity of 19.3 at 20 degrees Celsius.
> Every body continues in its state of rest or uniform motion in a right line unless it is compelled to change that state by forces impressed upon it.

Different though these facts may be, what characteristics to they share? It would seem that the following features may be said to belong to each of them, although some are more obvious than others:

1. Facts are always related to universally *certain* propositions, and propositions that are universally considered to be certain are always related to facts.[16]
2. Facts are always related to *timeless* propositions. Even the fact that I am now standing here is related to a proposition the certainty of which can never be assailed. When we speak of hard facts, we mean that the propositions uttering those facts are timelessly unassailable. As such, all facts are equally hard.
   There are no soft facts.[17]
3. All propositions containing facts have a *conditional* structure, the general form of which is: "If A obtains, then B is to be expected with absolute certainty."

There can be no facts unless there are expectations.

4. The relationship of implication inherent to the conditional form of the proposition, is represented by a picture which is rougly isomorphous with the fact it represents. Every fact therefore is mediated by what Peirce has called an icon representing the fact.
5. The icon representing the fact is always the result of an hypostatic abstraction.
6. The higher the level of hypostatic abstraction, the more the fact will tend to be considered as explaining facts involving hypostatic abstractions at a lower level.
7. Every antecedent of a conditional expressive of a fact, is itself expressive of a fact.

The first of these properties is obvious. It is the basis of Wittgenstein's thesis that whatever we can not speak of, we must be silent about. That is indeed the consequence of the interpretation of the world as the collection of facts. To say that there are unknown facts is the same as saying that there are propositions of which we would be certain, if we were to be confronted with them.

It should be pointed out that things have no such inherent relationship to propositions. Indeed, the law of inertia certainly states a fact *in spite of* our knowledge that no thing exists which corresponds to the demand of that fact.

The second characteristic mentioned is closely associated with the irreducible pastness of a fact. There is no fact unless it is a *past* fact. Sure, the past may be an infinitesimally recent past, but still, once it is past, it is entirely past. Now the most prominent characteristic of the past is that it never changes. If there is any ontological foundation to the so-called logical principle of contradiction, it must be this property of the past. The apparent time-neutrality of facts is the result of the extrapolation of their pastness into an absolute timelessness, or better yet, the pastness of facts grounds their time-neutrality, in as much as only the past is truly time-neutral.[18]

When it is said that the world is made of facts, what else is said than that the world is immutable, past, and the object of whatever it is we talk when we are quite certain of what we say.

Here then, is one very basic difference between the world perceived as the collection of things, and the world perceived as the collection of facts. Things, like ourselves, are eminently subject to change, to growth, all the while remaining what they are. Facts on the other hand are static and immutable.

The third characteristic demands some explanation for it is far less obvious than the first or the second. Consider the first example given: It is a fact that I am standing here. It would appear that there is no conditionality involved in this fact. But this appearance is due to our old Substance View encroaching upon our Fact View. There is nothing conditional about the thing which I call I and to which I ascribe the property of standing here. But as soon as it is claimed that this is a fact, the implication is that if anyone takes the trouble of investigating, he will find out that there is an irreducible relationship between this time-place and my standing here, and that this relationship is such that if this place and time can be identified as being this place and time, then anyone who would investigate the matter would find his expectation fulfilled that I am standing here.

Probably the basic misunderstanding about facts is due to an underestimation of this feature of facts. The fact regards neither the antecedent nor the consequent, but it is expressive of the implication itself. Of course, we are misled into thinking that no conditionality is involved because my standing here is a fact of such a low level that in ordinary life no one would ever dispute its facticity.

In general, every fact has in principle the form: "If anyone were to investigate, he would have a certain expectation fulfilled."

The fourth feature, the iconic representation of a fact, can be seen to be closely related to the expectational aspect of the proposition expressing the fact. Because there can be no fact without expectation, and there can be no expectation without imagination, clearly the fact and the image presenting the implication constituting the fact are inseparable.

But a fact is related to an icon of a very special nature. Indeed, it is related to what Peirce has called the hypostatic abstraction. By this is meant that a particular aspect of our experience which has been carved out from the totality of the experience is raised to the level of a quasi-substance, so that new properties can be ascribed to it and propositions can be uttered about it. Any property ascribed to objects of our experience may be made into a hypostatic abstraction. E.g., the property of man having two feet may be raised into bipedicity. As Peirce has very well pointed out, hypostatic abstractions are very useful devices. In fact, mathematics would simply be impossible without them. Circles and squares and numbers are all examples of hypostatic abstractions. Because a hypostatic abstraction by definition discards all reference to time, it too reinforces the immutability and thus the hardness of facts.

It should be evident that every hypostatic abstraction can be the source of other, even higher sorts of abstractions. The roundness of a figure may be circular, and this feature lures us into forming the hypos-

tatic abstraction of circularity whose properties may again be raised to hypostatic abstractions. Indeed, the history of the progress of physical science was in a large measure due to a capacity for developing ever higher levels of effective *hypostatic* abstraction.

Finally there is the requirement that every conditional expressive of a fact have an antecedent which itself refers to a virtual fact.

In short then, of all the properties of facts, its relationship to hypostatic abstraction is probably the most important, for it is the level of hypostatic abstraction which allows for a hierarchy of facts. It remains to be seen how Newton forged his abstractions. It will be seen that his significance for the reversal from the Substance View to the Fact View was due to his very strong predilection for hypostatic abstraction, and particularly for the hypostatic abstraction from interactive properties. I think it is possible to show that when Newton referred to phenomena, he was concerned with low-level facts suggesting the low-level abstraction of interactivity.

3. *Newton and Facts*

To understand Newton's attitude toward facts, it is particularly instructive to compare him with Descartes. Let us return a moment to the examples of facts. Even a mere glance at them will have revealed that, in spite of their common feature of being a fact, they widely differed from each other. One of the most obvious differences is that some are analytical in the Kantian sense of the word, while some others are synthetic. But I prefer not to use the traditional opposition between analytical and synthetic or a priori and a posteriori for the very use of those terms would commit us to certain implications which probably ought to be avoided. The definition of gold is not a mere relation of ideas as Hume would say. Indeed, it is the outcome of observation and generalization. It may be more fruitful to order facts along a continuum with, at the one end, the limit of strictly individual facts, and, at the other, the limit of purely general facts. The best candidates for general facts are mathematical facts, i.e. facts containing a purely formal hypostatic abstraction and facts with a mathematical form, i.e. facts containing a very high-level hypostatic abstraction.

Now, although both Newton and Descartes were partial to a mathematical approach of reality, there is a world of difference in their outlook. Descartes insisted on deducing lower-level facts from higher-level (mathematical) facts. Newton, however, was convinced that the validity of the use of effective higher-level facts was conditional upon a

respect for lower-level facts. Moreover, while Descartes was methodologically handicapped by a substance view of the world which could not possibly provide the proper framework for a new science which was dependent upon a world of facts, Newton - and this *in spite* of an ontological commitment to substance - knew how to free his scientific method from the restraints imposed by his explicit metaphysics.

Descartes based his entire philosophy upon one single timeless fact: the Cogito. It is an unassailable proposition that if I think I am because there can be no I thinking unless there is an I that is thinking. From this analytical fact, Descartes infers the reality of a *substance* , and from the inherent properties of that substance he infers the existence of a perfect substance and of an extended substance. Then he attempts to infer all the properties of the physical world from the *inherent* properties of these substances.

Thus it can be said that, in Descartes' system, all facts are still basically derived from the *inherent* properties of substances. This is clear not only with regard to the properties of thinking substance, but especially with regard to the properties of *extended* substance. Now, medieval philosophy had amply shown that inherent properties have a tendency to hide themselves, and the new philosophy and science of men like Francis Bacon and Galilei had already tried to ban them from scientific investigation. The only inherent but non-hidden property of extended substance is precisely its property of being extended. It is that property which Descartes makes into the hypostatic abstraction of extension. Indeed, the so-called *res extensa* is nothing but extension conceived as substance. The material world is geometrical in nature, and only by *mere* deduction can the physical properties of the world be derived from extension injected with motion. Descartes is therefore the victim of a double deception. On the one hand, he thinks he is dealing with a substance while his whole conception is based on the hypostatic abstraction of extension. But that abstraction is still anchored in a Substance View of the world from which the mechanical philosophy which he defends is trying to escape. Moreover, the fact chosen by him was eminently unsuitable to provide an explanation of the lower-level facts, because he could not free himself from the demand imposed by the Substance View of the world that scientifically relevant properties be inherent properties. Indeed the dualism of Descartes is the direct consequence of this unsuitability. He defines a substance in terms of properties which belong to that substance exclusively. He is forced therefore to conclude that the two substances to which he reduces the world can in no way affect each other.

Newton, on the other hand, does not develop a theory of substance. Moreover, or perhaps because of this, like Galilei, he does abandon the Aristotelian strategy of asking for the essence of substances, and instead concentrates from the very start on *interactive* properties of whatever it is we experience. The following comment made by the young Newton to Descartes' theory of light in terms of pressure illustrates exactly both Descartes' and Newton's views:

> Light cannot be by pressure for y^n wee should see in the night a wel or better y^n in ye day we should se a bright light above us becaus we are pressed downewards... there could be no refraction since ye same matter cannot presse 2 ways... A man goeing or running would see in ye night.[19]

For Descartes light working by pressure is a hypothesis, an intelligent guess. Newton shows the hypothesis is unacceptable because it fails to account for some very basic facts regarding our interaction with the world we experience. He implicitly accuses Descartes of reversing the order of the hierarchy of facts. A hypothesis must contain a hypostatic abstraction which can be shown to be abstracted from lower-level facts. This is Newton's basic argument for rejecting any hypothesis which is not based on phenomena. In a letter to Father Ignace Gaston Pardies, the Jesuit who had enquired about his theory of colors, he writes:

> In answer to this, it is to be observed that the doctrine which I explained concerning refraction and colours, consists only in certain properties of light, without regarding any hypotheses by which those properties might be explained. For the best and safest method of philosophizing seems to be, first to enquire diligently into the properties of things, and to establish those properties by experiments and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be employed only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments. For if the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties. Hence it has been here thought necessary to lay aside all hypotheses, as foreign to the purpose...[20]

When therefore Newton constructs his physical theory, he does not go on the assumption that there are things, but he starts from one basic low-level fact, the fact of interaction. Whenever he speaks of the phenomena, he always means obvious facts of interaction. And thus, when he tried to formulate a universal physical theory, it was natural that he looked for one basic property of interaction. That property was raised to the hypostatic abstraction of moving bulk. Bulk is not body. It is not a substance, nor is it a property derived from a substance, but is an abstraction from the property in virtue of which the world of our experience appears. And that world is a world of interaction. There is no world without interaction. Interaction is what *first appears* to us, it is the first *phaenomenon.* The world does not appear to us as a world of things which we first perceive as existing and therefore interacting. No, we first perceive some interaction from which we then conclude to the existence of things. Whereas the continental rationalists continue in the belief that *agere sequitur esse,* acting follows upon being, Newton reverses the priority: *esse sequitur agere,* being follows upon acting, or better yet, *esse sequitur interagere,* being follows upon interacting.

Historians more versed in the many writings of Newton may disagree. Yet, the suggestion that Newton considered interaction to be the key to the understanding of the world and that this idea may well have been his fundamental intuition is almost irresistible. The evidence for the rightness of the suggestion can be found on several levels. There is first the circumstance that, although his major success was booked in the field of mechanics, he felt most at home in the arcane world of alchemy which was a world of interaction *par excellence.*[21] There is moreover the remarkable evidence of the Scholium in which he practically rejects the traditional idea of an absolute God and substitutes for it the powerful image of the *Pantokrator,* the Lord who is no Lord without dominion.[22] Finally, there is both his statement of universal gravitation and his statement of the third law which *is* the statement of universal interaction.[23]

If it be granted that Newton distinguishes himself by centering his view of the world on the hypostatic abstraction of interaction, and if it is kept in mind that one of the requirements of a fact is that it be expressible in conditional statements, it should be obvious that statements of interaction are ideal candidates to be statements of fact. Indeed, statements of interaction may very well be considered as the model of all conditional propositions. All conditionals, even purely logical conditionals, are idealized instances of our experience of interaction.

But although the interactions which Newton consistently called *phenomena* were the raw material of experience, he considered it the business of the scientist to formulate higher levels of interaction. One is tempted to use the alchemical language of purification and vivifaction. The raw material of the phenomenal interactions had to be purified into idealized interactions.

Now, the most prominent feature of any interaction is its reciprocity. And reciprocity raised by hypostatic abstraction to a quasi-substance is the mathematical equation. An equation is first and foremost an expression of perfect reciprocity. Therefore, it was natural that Newton thought it necessary to describe the phenomena or interactive facts in terms of mathematical equations.

One of the most astounding implications of his feat has been almost systematically neglected. Indeed, it is often said that mechanical philosophy gave a reductionist account of causality. Newton at least seemed to know better. For when he had formulated the law of gravity, he wrote that he had not yet disclosed the cause of gravity.[24] Somehow he knew that the category of 'cause' belonged to the Substance View of the world. To his 'natural philosophy', (which was elaborated in terms of the Fact View) the question of causation was irrelevant. When it is said that, given $x + 5 = 8$, x must be 3 because the difference between 8 and 5 is 3 and only 3, we do not thereby mean that the difference between 8 and 5 is the *cause* of 3 in the sense that 3 happens under the influence of that difference. The equation as such, i.e. the reciprocity of the two expressions literally contains the answer. That does not make the equation trivial, for - in the case of the equations used in physics - the reciprocity of mathematical equation enables the scientist to describe the interactive facts which appear at a lower level.

These equations then, are for the scientist the ultimate facts underlying all phenomena. The mathematical equations are the highest-level facts which, in virtue of their high level of hypostatic abstraction, contain the low-level facts that appear to us, the phenomena. In as much as Newton was able to elaborate one mathematical equation which contained all the phenomenal facts of motion, he was the first thinker to consistently work out a view of the world as a unified totality of facts.[25]

It would of course be foolish to argue that Newton deliberately set out to construct a world of facts, and that he deliberately rejected the Substance View of the world. Indeed, it is rather easy to show that, ultimately, he was ontologically committed to the Substance View of the world. Indeed, his entire work on alchemy which really fascinated him much more than the science which made him famous is steeped in a language of living substances, suffering, mating, and longing to be puri-

fied. But the most far-reaching example of his substance thinking is to be seen in his concept of absolute space as the place where God is at work. The absolute nature of space and, more inexplicably, of time, is still derived from a traditional view of the inherent properties of the divine substance. The immutable nature of God's Spirit demands an immutable place, but place is nothing if it is not the place of some substance.[26]

But once scientists had seen the enormous gain brought about by the *Principia,* they grabbed the part of Newton's heritage which worked and, almost shame-facedly dropped the other Newton. And it so happened that in the scientifically successful part of Newton's work the fact of interaction expressed in the mathematical fact of reciprocity played a central role. As so often, success breeds success.

But one might object that, even supposing that Newton, and natural science in his wake, did switch from a substance view to a thing view, what is gained by saying so? Does it really *make* a difference? Probably it makes no or little difference to scientists in their business of science. But it should be obvious that it must make a difference for the philosopher hidden within every scientist. Where then is the philosophical gain to be made? For a start, the unceasing flood of literature on a number of topics related to the philosophy of science would almost immediately come to a sudden stop, if it were seen that the muddle that so persistently characterizes many discussions might magically disappear as soon as it would be made clear that a particular question was asked within the Substance View or, as the case may be, within the Fact View. Three very brief references to fashionable topics should go some way to illustrate the point.

The question of determinism was already mentioned when it was pointed out that Aristotle formulates his view on propositions regarding the future in terms of the structure of substances. Now, in most modern discussions it is still assumed that it is valid to ask whether the *things* in the universe are such that certain outcomes are unavoidable. But the arguments which are usually maintained in defense of or against determinism are always arguments formulated in terms which make sense only within the Fact View of the world. Yet, it should be clear that the Fact View cannot be but deterministic. Indeed, given certain immutable facts, there are other immutable high-level facts which determine the low-level facts, very much in the same way that a mathematical function determines the outcome of certain values. The fault in reasoning consists in the interpretation of the word 'determine' as if it meant 'to make something happen'. But a function does not make anything happen; it is simply an equation, i.e. a fact of strict reciprocity. Asking whether this implies that a particular thing is thereby determined is asking a non-sensical

question, for the Fact View in which alone determinism can be formulated implicitly rejects or is at least indifferent to any ontological commitment to things.

Much the same remark applies to the whole so-called realism debate, in which everything turns around the question whether or not science in general, and, more particularly, modern science says something about the "real" world. But if the real world consists only of facts, it is just plain silly to expect science to say something about the reality of things such as for instance electrons or other particles. Real are the facts born from certain low-level facts constituted within the context of certain expectations that are fulfilled under certain conditions, in combination with a certain number of higher-level facts. Any reference to particles or any other entities as if these were individual things with properties is simply a residue of the Substance View of the world which still strongly survives in our ordinary discourse. The Fact View of the world commits us to the reality of *facts,* and nothing more.[27]

The third example is taken from the equally fashionable discussion about probability. As a layman, I shall limit myself to the humble dice. Assuming there is a thing such as a dice, then a dice must act according to its thingness. But there is nothing in the diceness of the dice that either demands or forbids its falling now on one side, then on the other. There is no question of probability here. For, if the dice shows itself with its six turned up, that has significance only if *I* expect it to turn up in this way. In other words, it is a question that is interesting to the thing which I call 'I', but it is of no interest whatever to the dice. However, if our ontological commitment is not to things but to facts, then it is a fact that there is a probability of 1 out of 6 that the dice will turn up a 6. Not 1 out of 5, or 1 out of 7, but precisely 1 out of 6. That is the relevant fact to which I am obliged to commit myself. And that fact does not concern the dice as an individual thing at all. The same must apply in all cases of probability. If my ontological commitment concerns things, then there is no probability. A thing is the way it is, there is nothing probable about it. On the other hand, there is no probability apart from facts of probability.[28] The problems arise only from the moment we waver in our ontological commitment.

4. *Conclusion*

In our world, every person has at least two names, a first name and a last name. The last name is what allows us to identify not only that one person but also the link between that person and his descendants. It used to be that last names often typified a certain characteristic of that person. I made an attempt to show that the characteristic of the work of Newton that had a lasting impact on his descendants was related to his tendency in what he called his natural philosophy to approach the world as a collection of facts. The philosopher Wittgenstein was the first to explicitly formulate that view. In that sense, it is justified to say that Newton's last name was Wittgenstein. But the first name is the name we like to be used by our friends and intimates. And if a first name also says something about a person, the name Isaac was rather poorly chosen, for the story of Isaac is really part of the story of his father, Abraham. The real Newton was the fiercely indivdualistic thinker who burned with the holy passion of discovering the real nature of things. In that sense, his name was Aristotle.

His failure was probably the failure of the last great attempt to defend the Substance View of the world. Our failure is that we do not see the limitation of the Fact View of the world. We do not really believe that there is nothing else than dead, immutable and timeless facts. Aristotle tried to discover nature through things, modern science tries to discover through facts. Both attempts are marked by the same defect: both refuse to recognize that the one fundamental feature of nature is that which is marked by the word itself. There is no nature without birth, without becoming, without happening.

There may be a science of nature which tells us the facts. But there can be no philosophy of nature that is ultimately based on mathematical principles. C.S. Peirce and A.N. Whitehead, both dedicated and esteemed mathematicians though they were, saw that a real philosophy of nature must begin with the beginning, and that is always the event that now takes place. Unfortunately, no sooner have I said this than it has turned into a dead fact.

Catholic University Nijmegen, The Netherlands

NOTES

1. Aristotle, *Metaphysics*, Book Z (VII), 1, 1028 a 13-15; 29-31. Translation by W.D. Ross as reproduced in *The Complete Works of Aristotle, edited by Jonathan Barnes* (Princeton U. Press, 1984), Vol. II, p. 1623. See also *Metaphysics,* Γ (IV), 2, 1003 a 33 ff.
2. *Metaphysics,* Z (VII), 2, 1028 b 9-13.
3. Ludwig Wittgenstein, *Tractatus Logico-Philosophicus* (London: Routledge & Kegan Paul, 1921 [1963]), p. 7.
4. The first newspapers (in the modern sense) were, on the continent, the *Mercurius Gallobelgicus* which first appeared in 1590, and, in England, the *Corante, or, newes from Italy, Germany, Hungarie, Spaine and France.* which appeared in 1621. See the article "Newspaper" in *Encyclopaedia Britannica* (Chicago, 1979), *Micropaedia,* Vol. VII. Denis Diderot's *Encyclopédie* which was inspired by the success of Ephraim Chambers' *Cyclopaedia or an Universal Dictionary of the Arts and Sciences* (London, 1728) appeared in the years 1751-1772, while the *Encyclopaedia Britannica* appeared in Edinburgh, Scotland in 1758. See *Ibid.*, *Micropaedia,* Vol. VI.
5. The closest Aristotle comes to using a term with a similar meaning is when he says that a proposition is true when it corresponds to the πράγμα, literally, the 'thing at hand'. See Aristotle, *De Interpretatione,* IX, 19 a 33. J.L. Acrill (in the Bollingen edition) translates the term by the expression "how actual things are", H. Tredennick in the Loeb edition by "facts." See *Aristotle in twenty-three Volumes,* Vol. II, *Posterior Analytics* by Hugh Tredennick [and] *Topica* by E.S. Forster (Cambridge, MA: Harverd University Press, 1976).
6. "A *substance* - that which is called a substance most strictly, primarily and most of all - is that which is neither said of a subject nor in a subject, e.g. the individual man or the individual horse." See Aristotle, *Categories,* V, 2 a 13-15.
7. Aristotle, *De Interpretatione,* 9, 18 a 28
8. It should be pointed out that the claim that a proposition uttered now concerning an event occurring tomorrow can be said to be true or false is already problematic on at least two grounds. First, it is not clear to which 'reality' the proposition refers. Intuitively one would be inclined to say that it refers to *events*. But saying that there are propositions 'about' events implies already that events are already treated as if they were 'entities.' Secondly, if a proposition by definition refers to facts, then the claim is dubious on formal grounds. For it rests on the mistaken assumption that the truth of the expression "X will take place tomorrow" as the truth of the proposition "X has taken place" which can be uttered only after X has in fact taken place. The mistaken assumption is implied in the claim that a truth never changes. The claim is correct if and only if the proposition to which the truth is ascribed remains the same.
9. Aristotle, *De Interpretatione,* 9, 18 b 31-32.
10. This follows from his conception of science: "We think we understand a thing *simpliciter* (and not in the sophistic fashion accidentally) whenever we think we are aware both that the explanation because of which the object is is its explanation, and that it is not possible for this to be otherwise." Aristotle, *Posterior Analytics,* I,2, 71 b 9-12. But paradoxically, even scientific knowledge must ultimately rely on νοῦς ("comprehension"

according to Ackrill, "intuition" according to Tredennick). Cfr. *Posterior Analytics,* II, 19, 100 b 15.

11. The doctrine is expressed in two fragments. In fr. 5, Parmenides writes: 'το#' γα#'ρ αθ#ητο#' νοειν εστι τε και#' ειναι.' Fragment 8.34 gives a different version: 'ταθτο δ'εστι νοειν τε και οθνεκεν εστι νοημα": thinking is the same as that about which there is thought. Cfr. Hermaan Diels and Walther Kranz, *Die Fragmente der Vorsokratiker,* 10th ed. Berlin, 1960.
12. The *pratītyasamutpāda* or doctrine of dependent origination is related to the second "noble truth" of Buddha, which says there is a cause of suffering. This causation is conceived as a chain of causation where one element as it were necessitates another. "*Pratītyasamutpāda* tells us that in the empirical world dominated by the intellect everything is relative, conditional, dependent, subject to birth and death and therefore impermanent. The causal formula is: 'This being, that arises,', i.e., 'Depending on the cause, the effect arises.' Contrary to the Western conception of causality, however, Buddhism sees causation as the proof of radical relativism. See Chandradhar Sharma, *Indian Philosophy: A Critical Survey* (New York: Barnes & Noble, 1962), p. 61.
13. The Leibnizian Predicate-in-Notion Principle (I borrow this expression from C.D. Broad. See his "Leibniz's *Predicate-in-Notion Principle* and some of its alleged consequences," in H.G. Frankfurt (ed.), *Leibniz: A Collection of Critical Essays* (Graden City, NY: Doubleday & Co., 1972), pp. 1-45.) is to be found in several works, but most notably in his *Discours de Métaphysique* and his *Correspondence with Arnauld.* The original text is to be found in *Philosophische Schrifte von G.W. Leibniz,* 7 vols. (Berlin, 1960-61), the former in Vol. IV, the latter in Vol. II.
14. The expression constitutes the title of a work by Martin Heidegger. See M. Heidegger, *Die Frage nach dem Ding* (Tübingen, 1962). No direct relationship between the thesis of this paper and Heidegger's work is intended.
15. My use of the distinction is somewhat more liberal than Peirce intended it to be. Perhaps the clearest exposition by Peirce is the following: "A decrease of supposed information may have the effect of diminishing the depth of a term without increasing its information. This is often called *abstraction*; but it is far better to call it prescission; for the word *abstraction is wanted as the designation of an even* far more important procedure, whereby a transitivie element thought is made substantive, as in the grammatical change of an adjective into an abstract noun." Cfr. C.S. Peirce, *Collected Papers,* 8 vols in 4 (Cambridge, MA/London: Harvard University Press, 1932...), 2.364.
16. That facts are expressed in *certain* propositions is a totally different claim from the almost universally held but nevertheless wrong belief that facts are expressed in *true* propositions.
17. This point might be contested on the grounds that scientific facts do not arise all at once but are often discovered gradually. But this observation regarding the *discovery* of facts, which in no way refutes the universally held conviction that, once a fact *has* been discovered, it must be recognized as a hard fact.
18. See G. Debrock, "The Future of Time," in P. Scheurer and G. Debrock (eds.), *Nature, Time and History,* 2 vols. (Nijmegen: Philosophy and Nature, 1985), pp. 129-147.

19. From the *Quaestiones* as found in the additional MS in the Cambridge University Library, quoted in Richard S. Westfall, *Never at Rest. A Biography of Isaac Newton* (Cambridge: Cambridge U. Press, 1980), pp. 91-92.
20. The letter, dating from 1672, is quoted in Westfall pp.242-2. See Cohen 106. the Original Latin in Corres 1, 164.
21. Obviously the most authoritative work on the importance of alchemy in Newton's work is: B.J.T. Dobbs, *The Foundatons of Newton's Alchemy: The Hunting of the Greene Lyon* (Cambridge, 1975).
22. See the "General Scholium" of *Isaac Newton's Philosophiae Naturalis Principia,* 3rd ed. with variant readings, 2 vols., edited by Alexandre Koyré and I. Bernard Cohen (Cambridge, 1972).
23. If I am not mistaken, the analysis which Cohen makes of the genesis of Newton's third law suggests that the major different between Newton's first two laws and the third law consists in the discovery of the importance of universal *interaction*, instead of mere action.
24. See e.g.: "I have not yet disclosed the cause of gravity, nor have I undertaken to explain it, since I could not understand it from the phenomena." Quoted in A. Rupert Hall and Marie Boas Hall (eds.), *Unpublisheed Scientific Papers of Isaac Newton* (Cambridge: Cambridge University Press, 1978 [1962]), p. 213.
25. As Prof. Drago pointed out during the Colloquium, my thesis might also apply to Galileo. Yet, there are two major differences between Galileo and Newton. First, Newton dared to make interaction the fundamental property of the physical universe, and second, his breakthrough alone led to the universal acceptance of Newton's method which was a method in which fact, not substance, became central.
26. For a detailed analysis of the importance of the issue of substance in Newton's concept of space, see I. Leclerc, *The Nature of Physical Existence* (London: George Allen & Unwin, 1972).
27. This was perhaps what Kant tried to express by insisting that scientific knowledge is possible only for the *phenomenal* world.
28. The increasing attention that has been paid since the nineteenth century to facts of probability may be an extremely significant change in the history of science. One could probably say that the laws of nature are generally probabilistic. But facts of probability are as immutable as any other facts. It is therefore a terrible mistake to think that facts of probabilistic facts generate probable facts. All fashionable claims to the contrary, science is not about to offer us a "new alliance" with the intuitive world of our experience.

A WORD ABOUT THE AUTHORS

M. Capek is Professor (emeritus) at Boston University. He studied philosophy (Prague and Paris) and physics (Chicago). He is the author of many works, including *The Impact of Contemporary Physics* (1961) and *Bergson and Modern Physics* (1971). He is now associated to the University of Delaware.

Gale E. Christianson is Professor in the History of Science and Biography at Indiana State University (U.S.A.), and Distinguished Professor of the College of Arts and Sciences of his University. He is the author of a biography of Newton, *In the Presence of the Creator: Isaac Newton and his Time.* Forthcoming is *Fox at the Wood's Edge: A Life of Loren Eyseley.*

I. Bernard Cohen is Victor S. Thomas (Emeritus) Professor in the History of Science at Harvard University. He served as President of the International Union of the History and Philosophy of Science. He is the author of many works about the history of modern Physical Science, most notably, his *Introduction to Newton's* Principia. Currently he is preparing a new English translation of the *Principia.*

Guy Debrock is Professor in the Philosophy of Nature at the Catholic University of Nijmegen. He is the Editor of the Nijmegen Studies in the Philosophy of Nature and its Sciences. He is currently preparing the publication of a book on C.S. Peirce.

Ed Dellian is a practicing lawyer who also studied Physics, Philosophy and the History of Science. He is the author of several publications on Newton. His translation of Newton's *Principia* is currently in Press under the title *Mathematische Grundlagen der Naturphilosophie* (Felix Meiner). He is also the author of *Playdoyer for Isaac Newton* (Forthcoming).

Dennis Dieks is associate professor in the foundations of science in the Department of History and Foundations of Science at the State University of Utrecht. His main publications are in the field of the foundations of physics.

B.J.T. Dobbs is Professor in the Department of History at Northwestern University (U.S.A.). She is well known for her many publications on Newton's alchemy. She is the author of *The Foundations of Newton's Alchemy, or "The Hunting of the Greene Lyon."*

Arthur Donovan, an historian of science and technology, is currently Professor at the U.S. Merchant Marine Academy, Kings Point, New York, where he is Head of the Department of Humanities. He is the author of *Philosophical Chemistry in the Scottisch Enlightenment* and other studies of science and engineering in the modern era.

Antonino Drago is Associate Professor in the History of Physics at the University of Naples (Italy). He has published in the areas of the Theory of Automata, Peace Research, the History of Classical Physics, the History of Mathematics and the Philosophy of Science.

Mordechai Feingold is Professor in History and the History of Science at Boston University (U.S.A.). He is the author of *The Mathematicians' Apprenticeship: Science, Universities and Society in England: 1560-1640* (Cambridge U. Press). Forthcoming from the same Publishers is *The Life and Works of Isaac Barrow.*

Peter Fong is professor of Physics at Emory University, Atlanta, Georgia (U.S.A.). He is the author of six books (including the forthcoming *East of Eden: the Evolution of Man and Humanity*) and about one hundred publications in nuclear physics, astrophysics, thermodynamics, quantum mechanics, molecular biology, geophysical sciences, philosophy, history and social studies.

Alexandru Giuculescu studied mathematics, philosophy, linguistics and law in Bucharest. He is Profesor of mathematics in Bucharest as well as a member of the staff of the Academy of the Socialist Republic of Romania. He did research in Paris and Bonn. He published on the philosophy and history of science.

Barry Gower is lecturer in the Philosophy of Science at Durham University, England. He has published studies of probabilistic reasoning in the physical sciences in *Annals of Science* and in *Studies in the History and Philosophy of Science.*

Willem D. Hackmann is the assistant Curator of the Museum of the History of Science at the University of Oxford. His interests include eighteenth- and early nineteenth-century physics, the interaction between instrumentation, experimentation, and scientific theory, and the diffusion of science between England and Holland. He is the author of *Electricity from Glass: the Development of the Frictional Electrical Machine 1600-1850* (1978), *Seek & Strike: Sonar, Anti-Submarine Warfare and the Royal Navy 1914-1954* (1984), and of the exhibition catalogue *Apples to Atoms, Portraits of Scientists from Newton to Rutherford* (1986).

Casper Hakfoort is a member of the History Department in the School of Philosophy and Social Sciences of the University of Twente (The Netherlands). He is the author of a book on the development of optics from Newton to Euler (1986). His present research concerns the shift from classical to modern physics as well as the history of industrial science. (Address: Universiteit Twente, Vakgroep Geschiedenis, Vrijhof, Postbus 217, 7500 AE Enschede, The Netherlands)

W.J. Hornix is lecturer in the history of chemistry at the Catholic University (Nijmegen, Holland). He has published several articles on the foundations of thermodynamics and the history of science (particularly thermodynamics and organic chemistry). He directed a research project on the history of the dyestuff industry. His publications pertain to the fields of the foundations of thermodynamics, the history of thermodynamics and of organic chemistry. He directed a research project in the history of dyestuff industry.

Peter Kroes (1950) studied physics and philosophy; he is lecturer in the philosophy of science. His major publications are: *Time: its Structure and Role in Physical Theories* (1985) and *Filosofie van de Natuurkunde* (1987). He also published several articles on the philosophy of physics.

H.H. Kubbinga is lecturer in philosophy at the University of Groningen (The Netherlands). He was recently awarded a research fellowship by the Royal Dutch Academy of Sciences. His Ph.D. thesis (É.H.É.S.S., Paris, 1983) concerned the history of the concept of "molecule" until the end of 18th century. He prepares a monograph on the history of the concept of molecule (until ca. 1925).

F.H. van Lunteren is instructor at the Institute for the History of Natural Science of the University of Utrecht (The Netherlands). He is currently preparing a work on gravitational theories in the 19th century.

Richard H. Popkin is Professor (emeritus) of Washington University (U.S.A.). He is co-director of the International Archives of the History of Ideas. He is the author of many works on the History of Ideas, and in particular of a *History of Scepticism*. He has a particular interest for Newton's theological writings.

Andries Sarlemijn (1938) studied social sciences (sovietology, economics), logic and philosophy in Fribourg (Switzerland). Since 1971 he teaches logic, method and the history of science at the Technical University of Eindhoven (The Netherlands). He published several books and articles on the history of science and technology.

Paul B. Scheurer, is Professor (Emeritus) at the Catholic University of Nijmegen (The Netherlands) and lectures at the University of Geneva in Philosophy and the History of Natural Science. His main interest concerns the discursive revolution and the structural derevolution of physical theories, especially Relativity and Quantum Physics, as well as the Critical History of the representations of motion. He is the author of *Révolutions de la science et permanence du réel* (P.U.F.) and of and *De l'homme, de la mesure et du temps* (Forthcoming).

G. Verstraeten was trained in both physics and philosophy. His doctoral dissertation in physics concerned second order phase transitions. His philosophical research focuses upon the complementarity of mechanics and thermodynamics. At the present time he is Senior Lecturer at the University of Malawi (Africa)

Ewald Vervaet is physicist and psychologist. He is an associate of Foundations Histos, an institution which propagates research in the field of the development of Insight. His research concerns gravitation, the Freudian unconscious etc. He is supervisor of a research-group involved in the study of psychotherapeutical processes, and he lectures in psychology. He is the author of a dissertation on "Structuralist explorations into Epistemology and Personology" (in Dutch).

Teresa Grabińska and Miroslaw Zabierowski are assistant professors at the Technical University of Wroclaw. They share an interest in particle physics, cosmology, astronomy, the methodology of exact sciences, and the philosophy of physics.

They developed appropriate methods for studying the intergalactic matter /JPII region and JPS phenomenon/. They were early advocates of teaching pragmatic logic and the logic of scientific discovery at technological institutes.