HALLEY'S ATHEISM AND THE END OF THE WORLD

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EDMOND Halley's views on theology and natural philosophy have often drawn puzzled attention both from his contemporaries and from subsequent scholars. There has seemed to be a contrast between some public statements he made when under pressure from ecclesiastical authority, and his continued, and privately-held, faith in the over-arching relevance of science (1). However, it now emerges from some unpublished papers which Halley read to the Royal Society in the 1690s that he made public his own debate over such issues as the eternity of the world. This new evidence gives us a much more consistent picture of Halley's work, and it refutes the view that there were two Halleys—the public orthodox face and the private heterodox one. It is true that the work of Edmond Halley presents us with a picture of considerable diversity. Nevertheless, throughout the 1690s he was primarily concerned with an investigation of Earth history independently of scriptural authority, and this gave some unity to his varied researches. However, there were both ideological and institutional problems with such a programme. The Anglican establishment of the period after 1688 was filled with a sense of threat. This led to a series of statements antipathetic to Halley's attitude, including a devaluation of the power of unaided reason and an emphasis on the power of God's Providence (2). Halley's failure to obtain the Savilian Chair of Astronomy in 1691/2 was due in part, perhaps, to this antipathy. Yet this failure was also precipitated by the personal antagonism aroused by Halley's jocular style, and the innate irascibility of Flamsteed. Because of these other sources of controversy the exact nature of Halley's atheism remains confused. Even his identification with the 'infidel mathematician' of Berkeley's *Analyst* is problematic (3). Yet the fact is that Halley took these charges seriously enough to spend several years working to show that one of them was unjustified. He had been accused of believing that the world would continue for eternity, and he was to try and show that it must, in the end, come to a halt.

In June 1691 the professorship of astronomy at Oxford fell vacant, and Halley applied for it. The other candidates were David Gregory and John Caswell. Flamsteed later told Newton that Halley had tried to dissuade Caswell from applying (4). The chair itself was in the control of the Archbishop of Canterbury, Tillotson, and the Bishop of Worcester, Stillingfleet, both of whom were much concerned with the threat of atheism, under the guise of natural philosophy, to true religion (5). Stillingfleet learnt that 'Halley was a skeptick, and a banterer of religion, and he scrupled to be concerned, till his chaplain, Mr Bentley, should talk with him about it, which he did' (6). Halley immediately wrote to his friend Abraham Hill that 'an affair of great consequence to myself calls me to London viz. looking after the Astronomy-Professor's place in Oxford' (7). He asked Hill to obtain a deferment from Tillotson, 'but it must be done with expedition, lest it be too late to speak. This time will give me an opportunity to clear myself in another matter, there being a caveat entered against me, till I can shew that I am not guilty of asserting the eternity of the world'. The interview, however, did not go well. 'The bishop began to ask him some questions. The Doctor told him, "My Lord, that is not the business I came about. I declare myself a Christian and hope to be treated as such" '(8). Thomas Hearne also reports that 'Dr Halley went to Dr Stillingfleet, and he told him y[t] he belieued a God and that was all' (9). According to Whiston, when Richard Bentley interviewed Halley 'he was so sincere in his infidelity that he would not so much as pretend to believe the Christian religion, tho' he thereby was likely to lose a professorship, which he did accordingly, and it was then given to Dr Gregory'. Gregory was scarcely notable for his piety, having refused the Test during the 'Presbyterian Inquisition' of 1690, and being accused of drunkenness and sloth in teaching (10). By contrast, Halley had received a strong recommendation from the Royal Society for his probity and ability in science.

Given the strength of opposition to Halley's candidacy, it is clear that the belief in the eternity of the world was seen as genuinely heretical, as it had been since at least 1277. Tillotson, in his *Principles of Natural Religion*, had condemned the Peripatetic belief in the eternity of the world (11). If Halley could give a *physical* demonstration of the end of the world he could safeguard himself against charges of 'atheism', on those grounds at least. Before examining the details of Halley's work between November 1691 and December 1694, however, it is essential to examine the ideological context of the period. The definition of atheism was relatively loose—a graphic example is the case of William Whiston himself. In 1704 he was expelled from Cambridge as an Arian. This may have led to some bitterness or envy between himself and Halley. When he found Whiston refusing a glass of wine because it was a Friday, Halley asked him whether he had 'a Pope in his belly'? (12). Clearly there is some distinction between the attack on 'banterers' such as Halley or Gregory, and on true, and devout, 'heretics' such as Whiston. The latter bitterly contrasted his own sufferings for the faith with Halley's easy attitude to religion: 'If it had not been for the rise now and then of a Luther or a Whiston, he (i.e. Halley) would himself have gone down on his knees to St Winifred or St Bridget' (13). We compare this suspicion of Halley's commitment to any religious viewpoint with the much more personal antagonism of Flamsteed. Much has been written about the priority disputes between the two men over the tidal and lunar tables which they produced. Quite apart from this, Flamsteed alleged that Halley had stolen the idea of the Earth having four magnetic poles (see p. 21) from his friend Perkins in the 1680s. In his allegations Flamsteed often questioned the morals of his enemy, and there can be little doubt that this antagonism was one source for the generally dubious reputation which Halley acquired (14).

Evidence for this emerged in 1703 when Halley successfully applied for another chair at Oxford, and Flamsteed wrote to Sharp that 'Dr Wallis is dead. Mr Halley expects his place, who now talks, swears, and drinks like a sea-captain'. Halley's success was not, in fact, uncontested even in 1703—all this makes his attempt in the 1690s to demonstrate the physical impossibility of theologically unacceptable facts the more crucial. Most of Halley's contemporaries were also searching for 'un lieu entre la science des savants et la revelation de la Bible' (15). There was a conscious effort to appeal to a 'reasonableness' in the audience which was essentially identical with that of the new philosophy, whilst at the same time retaining that quality of 'reason' which Anglican theology would demand. Ironically, the strictest followers of Newton were the ones most willing to leave miracles an important place in the world. Keill, for example, maintained that 'we are not to detract from the value of true [miracles] by pretending to deduce them from Natural or Mechanical causes, when they are no way explicable by them'. By 1700 this was a relatively common view. Thomas Baker, in his *Reflections upon Learning*, argued that it did seem that the cause of the heaviness of bodies would probably never be discovered, and that therefore his contemporaries should be satisfied with relying on the action of the divine: 'If we spend a thousand years upon these researches, may it not even then be necessary to come back to "attraction", or to content oneself with ascribing all to the power and providence of God? Why not then take that step now?' (16). The dividing line between the physical and the miraculous was not, therefore, clearly drawn—some natural philosophers were attempting to examine the Biblical record from a scientific point of view, whilst others attributed apparently physical events to the action of God.

Attempts such as those of Burnet in his *Sacred Theory of the Earth* to bridge this gap between physical law and divine action, would therefore be examined very critically. 'It seems to me very reasonable', wrote Burnet, 'to believe that besides the Precepts of Religion, which are the Principal Subjects and Design of the Books of Holy Scripture, there may be providentially conserved in them the Memory of things and times so Remote as could not be retrieved, either by History, or by the Light of Nature'. Burnet suggested that Moses spoke so as to avoid explaining natural philosophy to the people, who were, after all, especially stupid (17). This theme, the misleading and untrustworthy data preserved in ancient texts, was one which came to acquire great importance for Halley. Burnet, on the other hand, was worried by such statements as that of Moses that during the Creation God made light appear before the creation of the Sun. This, Burnet explained, was because the Children of Israel would have thought it undignified for God to work three days in the dark. The details of Burnet's account of the structure of the Earth owed more to that of Descartes. It was also very influential on Halley's work, in particular on his idea of a cavity within the Earth. Both Burnet and Halley also discussed a change in the orientation of the Earth's magnetic poles due to a shift in its axis. Although it was from this source that Halley drew the inspiration for his own view of the structure and history of the Earth, there is a sharp contrast with Burnet. Where Halley attributed the change of place of the magnetic poles to the rotation of a sphere inside the hollow Earth, Burnet saw the displacement of the magnetic poles from the true poles as a mark of the imperfection and fallen state of the Earth (18). The relative priority of Scriptural and physical considerations was completely reversed.

Most natural philosophers found problems in the physics of Burnet's 'unlikely story', but some objected to the very nature of his attempt (19). Newton described his physics as 'plausible', but insisted that no-one 'could mend that description which Moses has given us' (20). This problem of the feigned nature of Scripture was worrying—Erasmus Warren pointed out that 'should God deceive in one place he might do it in more'. Similarly, John Beaumont wrote in 1692 comparing the characters of Scripture with modern scientists: 'That the Antediluvian Patriarchs, as well as the Postdiluvian, were in their respective times, the most absolute Masters of the aforesaid Science (astronomy) of any Men on the Earth and that from them it has been convey'd down in its Pureness to us, is what I do not know how to disbelieve.' (21). We can therefore see that in an attempt to protect their work from 'philosophical' criticism, the Scriptural authorities were turned into the scientists of the ancient world. Even in Stillingfleet's *Origines Sacrae*, the complaints against the confusion of religion and philosophy were accompanied by the statement that both Adam and Moses were blessed with perfect reason—Adam, for example, had been able to name all the animals correctly (22). For Halley, always fascinated by ancient scientific observations, this was an open invitation to try to verify Scripture by the physical techniques he used so effectively eleswhere (23). Another eminent Newtonian, John Keill, used physical arguments *against* Burnet's hypothesis, but he realized the danger of the precedent created thereby: 'These contrivers of Deluges have furnished the Atheist with an argument, which upon their supposition is not so easily answer'd as their theories are made'. Keill wanted any reconstruction of Scripture to be physically plausible, of course, but it was just this argument which Halley used so powerfully to justify his own, very different position (24).

By the end of the century it had been conceded that 'the design of the holy Writings is not to instruct men in Philosophical but in Divine matters'. The way to 'prove the Christian Religion against notorious infidels' was to show that there was no contradiction with science (25). Halley merely put the onus of proof on religion; he argued that if at any stage physical considerations could be invoked in theological discussions, as they were by Whiston, Keill, and Burnet, then they could obviously be used generally for all investigations of the past. If the 'holy Writings' had little to say about the physics of their own times, they would have even less relevance to earlier times: 'What I have advanced', wrote Halley in his only explicit discussion of these issues, 'I desire may be taken for no more than the Contemplation of times whereof we have no manner of tradition, as being before the first production of man, and therefore not knowable but by revelation, or else a posteriori *by induction from a convenient number of experiments*' (26) (my stress).

There were, therefore, many obstacles in the way of any attempt by Halley to invoke physical arguments, even if they were in defence of theology. The context in which these arguments were to be presented was therefore crucial. Halley produced his first suggestion of his proof that the world must come to an end in the final section of a paper he read to the Royal Society on 25 November 1691, just one month after the Society had given him a glowing recommendation for the Oxford chair. This paper was the second of two he published on terrestrial magnetism, and outlined a theory which explained how the Earth came to have four magnetic poles, two in the northern hemisphere near the Bering Strait and near Spitzbergen, and two in the southern, in the Southern Ocean and south of Australia (27). Halley suggested that inside our Earth, which has its normal pair of poles, there was a second concentric sphere with its two poles, and the combined effect was to produce four poles on the surface which, because of the different speeds of rotation of the two spheres, were moving (28). Halley claimed thereby to have solved some problems which, as he confessed, 'I found not easie to surmount, . . . these difficulties had wholly made me despond and I had long given over an inquiry I had so little hopes of' (29). One proof of this theory came with Halley's account of the auroras of 1716, when he invoked this earlier paper to show that the 'northern lights' had a magnetic origin. On its publication in the *Philosophical Transactions* on 27 January 1691/2, however, Halley offered a different demonstration of the existence of an Earth inside ours, and this led immediately to his proof of the retardation of the Earth and the other planets.

Halley pointed out that if the Earth and the moon had the same overall specific gravity, then, since they are both moving through a resisting medium, the larger body, the Earth, would accelerate away from the moon since it would receive less retardation. 'I think I can demonstrate that the Opposition of the Ether to the motions of the Planets in long time becomes sensible, and consequently the greater body must receive a less opposition than the smaller, unless the specifick gravity of the smaller do proportionately exceed that of the greater in which case only can they move together, so that the cavity I assign to the Earth may well serve to adjust its weight to the Moon. For otherwise the Earth would leave the Moon behind it and she become another primary planet.' (30). The crucial point is that if, as Halley claimed, the companionship of the Earth and moon were a puzzle, then it would have been surprising if it had been confined to the end of a paper on quite another topic. In any case, what was startling about Halley's argument was that his solution implied not merely that the Earth and moon would now move comfortably side by side, but that they would be slowed down equally, and presumably, come to a halt at the same moment indefinitely in the future. Halley could not leave this important argument there, of course, but he did seem to be well on the way to a genuine argument against the eternity of the world.

Halley raised the issue in much more detail later in the year in a crucial paper entitled *Concerning the Motion of Light* which he read to the Society on 19 October 1692 (31). It is extremely difficult to sustain the view that has been put forward that this was merely a mask to deceive his orthodox Anglican critics. Halley argued in all seriousness that since the speed of propagation of a disturbance through a medium is a measure of the density of that medium, it must follow that the luminiferous aether must be eight million times rarer than the air, which carries sound. Nevertheless, because it does carry light, however fast light travels, the aether must have a nonzero density. This was the important conclusion which Halley drew from the refutation by Rømer of the Cartesian theory that the speed of light is infinite. Any medium which has a definite density will sensibly resist the motion of bodies through it, and so the planets, moving through the aether, will demonstrably be retarded.

In these two papers Halley had shown how the planets would be slowed down. He had shown that it would still be possible for satellites to keep up with their primary planets, and he had argued that the aether must have a real density. One principal source for these ideas was Christiaan Huygens, who had argued strongly against Newton in a discourse given at the Royal Society on 22 June 1689 (32). It was a premise of the Newtonian theories that any material with a real density would necessarily be a resisting medium. Newton had therefore stated in the *Principia* that the aether must be rare enough to offer no resistance to the planets, for otherwise it would be impossible for these bodies to 'continue their motion through (space) for an immense tract of time' (33). By contrast, Huygens did not share the fundamental premise. So in the *Discours sur la Cause de la Pesanteur* Huygens could argue that the aether must consist of particles in contact with no void in between, for otherwise it would be impossible to account for gravity or the 'prodigious' speed of light. Even if his audience accepted this mechanism to account for gravitation, or agreed that the speed of light demanded that the aether particles touch each other, there was still the problem of resistance to planetary motion. Here Huygens argued that if the particles themselves were made of a matter of a different and much rarer consistency, they would offer no resistance at all. 'Les particules s'y peuvent toucher, . . . & toutefois, à cause de la legereté de leur tissu, resister fort peu au mouvement des planetes . . . Ne faut-il pas qu'une matiere plus subtile & infiniment plus agitée, soit aussi d'autant plus aiseé à penetrer?' (34).

Halley's reaction to this view was ambiguous. The year before, on 26 February 1689/90, Fatio de Duillier had also read a discourse on the cause of gravity which Halley had attended and, apparently, approved of. Fatio, like Huygens, envisaged the particles as being very rare, that is, as having a different intrinsic density from other matter, but used this not to account for the absence of fluid resistance but instead to demonstrate the equality of gravitational and inertial mass (35). Halley's position was defined by two crucial factors—he *did* share Newton's premise that all matter has the same intrinsic density, and he did *not* share his premise that the planets were not slowing down. This meant that he could not accept the saving clause in the Huygens-Fatio position on the rarer constitution of the aether particles. Consequently, he found, in these discourses on the cause of gravity, eloquent support for his idea that the aether would resist the motion of the planets. Halley had, after all, already conducted a lengthy correspondence with John Wallis on the subject of the resistance of such fluids to motion, and had sent him some of the theorems which appeared in Newton's *De Motu* before 1687 (36). But to support a direct criticism of some of Newton's ideas, Halley still needed stronger evidence than he had found in the work of Rømer and Huygens, and in his own work on fluid resistance and terrestrial magnetism.

Halley found such support in his analysis of that great concern of the natural philosophers of his time—the trustworthiness of ancient scientific observations. In particular, Halley looked at ancient records of solar and lunar eclipses. If it was true that the planets were slowing down, then we would expect that the length of the solar year as observed from Earth would increase. Halley claimed that a detailed comparison of observations reported by Hipparchus, Ptolemy, and the Arab al-Battáni all revealed such a change in the length of the year. However this change had not been noticed by the ancients, and they had had to doctor their figures to keep them consistent. This was not, as we have seen, the first time the reliability of such remote data had been challenged by the natural philosophers. In 1662, for example, Stillingfleet himself had tried to completely discredit the ancient, preChristian chronologists. No doubt this was because they failed to mesh satisfactorily with accepted Biblical time-scales. Stillingfleet was convinced that there was 'no small ground to question the credibility of their Histories' (37). It is, by contrast, not surprising to find that Edmond Halley initially trusted such records. What is significant is that between 1687 and 1692 Halley changed his ideas on the validity of Greek positional observations. On 15 February 1686/7 Halley told Wallis of a paper Hooke had read which attributed the Deluge to a change in the shape of the Earth, and citing as evidence an alleged change in the values of latitudes observed by the Greeks. ''Tis his assertion', Halley wrote, 'that there are not extant any authentick records of the latitudes of places sufficient to evince the fixation of the poles, but that the observations of the ancients seem very rude and uncapable of giving any information in this matter' (38). There were two problems with Hooke's idea: Firstly, as we have already seen, many contemporaries objected to a physical analysis of the Bible. Wallis wrote to Halley on 4 March that he and his friends at Oxford 'seemed not forward, to turn y^e world upside down . . . to serve an hypothesis, without cogent reason for it'. But Halley and Wallis were more impressed by Hooke's claim that there had been a change in latitudes. As Wallis stated, though Hooke argued that 'we have no certain evidence in History from accurate Observations that the latitude of places was always the same that now it is; It is replyed that sure we are, there is no evidence in history that the top of the Alps was ever sea. . .' (39).

Halley's attitude to this exchange, for which he had acted as intermediary, was quite clear. On 9 April he told Wallis that 'the latitudes of places (have) been ever since wee have accounts of observations much the same, Alexandria being laid down by Ptolemy in the same lat: that Mr Greaves found it . . .'. Similarly, he had already written a paper on variations in latitude which repeated that the ancient observations were perfectly trustworthy, and that there was no evidence for the mechanism Hooke had offered, not even, as he wrote, for a change in the earth's year (40). It is all the more interesting, therefore, to find Halley using an argument similar to Hooke's in the defence of his claim that the Earth's year was lengthening. In the 1692 paper Halley first noted that Ptolemy had been forced to change the position of Alexandria in order to account for that change in the year which he had failed to notice. When Halley came to publish al-Battáni's Syrian observations in 1693, he told the Society that the 'numbers were so vitiated as not to be understood' (41). He insisted, however, that to reconcile Ptolemy with the Arabic and the modern observations of eclipses it was necessary either to assume a change in the year, or to falsify the positions of Alexandria, Antioch, and so on. Thomas Streete, so Halley claimed, had taken the latter course in his *Astronomia Carolina* (42).

The use of these ancient data illustrates Halley's powerful use of what Cecil Schneer has called 'painstaking historical scholarship coupled with natural history' (43). Schneer points out the way in which the revival of antiquarianism by such figures as Hooke and Lhwyd produced a corresponding revival in the science of geology in the seventeenth century. Halley was particularly interested in ancient astronomical data. It is therefore inappropriate to see this paper of 1692 as an opportunist effort to allay suspicion—it falls squarely in the tradition of Halley's work. In 1695, for example, Halley used exactly the same observations which he produced in the paper on light, this time to demonstrate the secular acceleration of the moon. In *The Ancient State of the City of Palmyra*, Halley wrote that any traveller in that part of the world could not do better than observe the phases of the moon, 'for in and near those places were made all the Observations whereby the middle motions of the Sun and moon are limited. And I could then pronounce in what proportion the Moon's motion does accelerate, which it does, I think I can demonstrate' (44). It seems to me to be a mark of Halley's commitment to his demonstration that the Planets 'should not be capable of eternity in the state they now are', that he was prepared to change his views on the trustworthiness of ancient observations, and also to show that the Earth was *de*celerating, instead of deriving immediately the momentous result of the acceleration of the moon.

Halley knew by 1692, however, that he had failed in his attempt to obtain the Savilian chair. One year later, in November 1693, he read a paper to the Royal Society which was again based on the figures he had derived from Ptolemaic and Arabic observations. This paper, *Some Observations on the Motion* *of the Sun*, has been the object of considerable confusion, mainly because the report of its content in the *Journal Book* for 18 October 1693, on which all previous analyses of this paper have been based, does not in fact completely tally with the manuscript version I have reproduced (45). In particular, although it is the case that Halley does announce that the length of the year is getting less rather than greater, that is, that the Earth is accelerating in a spiral towards the sun, he does *not* conclude that the world must therefore come to an end. The final words of the paper deserve some emphasis: 'There [is] still wanting a valid argument to evince from what has been observed in Nature that this globe of the Earth ever did begin or ever shall have an end.' Since Halley did read this paper to the Royal Society, this demonstrates that Halley did *not*, after 1692, sheepishly toe the orthodox line on the age of the Earth. We have now at least two occasions (the other being his paper on the Deluge read in December 1694) (46) when Halley was prepared to question the finite age of the Earth in public. This is crucial in a reassessment of his position on the relation of theology and natural philosophy.

In detail, then, Halley confessed that it was only *after* re-examining his figures, and finding that the Earth's motion round the Sun became more swift, that he realized why this must be the case. As the Earth experiences a resisting force from the aether, it will move towards the sun so as to travel through a shorter orbit. But in this shorter orbit it will be moving faster, and will therefore gradually spiral towards the sun in an accelerated motion. Halley was also able to explain why the moon, in its orbit round the Earth, had not been moving into the Earth as fast as the Earth had been moving into the sun. There were two reasons for this: firstly, the speed with which the moon orbits the Earth is less than that with which the Earth orbits the sun, and since (*Principia*, Book 2, Prop. 35) the resisting force varies as the square of the velocity of the moving body, this means that the moon is not so resisted as the Earth. As a result, the moon will not shorten its orbit so rapidly, and so will not be accelerated so much as the Earth. Secondly, Halley cited the passage from his 1691 paper, in which he had first discussed the opposition of the aether to the planets (47). As we have seen, he had explained that if the moon were denser than the Earth in the same ratio as the Earth is larger than the moon, the two bodies will keep together. But by assigning a greater density to the moon, Halley had also implied that it was more solid, since he shared the premise that all matter had the same intrinsic density. This would also mean that the moon would receive less resistance, and hence that it would accelerate into the Earth more slowly than the Earth was accelerating into the sun.

The fact that Halley was able to successfully account for the acceleration of the Earth and the moon makes it all the more startling that he resolutely refused to endorse the view that there was proof of the finite age and impending end of the world. In 1692 he had argued that planets which were now slowing down could obviously not have been slowing down over an infinitely long period, for otherwise they would at some time in the past have been travelling infinitely fast; and similarly they could not keep going for ever because in the end they would come to a halt. His pessimistic conclusion to the paper of 1693, however, is scarcely the statement of a man worried by an accusation of Aristotelian heresy.

Halley's attempt had ended in failure, though he may not have seen it as such. He had by now been turned down by the Oxford assessors, and was in a position to publish his discovery of the secular acceleration of the moon, along with other of his researches in Earth history. His work in hydrology (48) provided evidence that the geological time-scale should be lengthened rather than given a definite period. Here, too, his work of the 1690s can scarcely be characterized as rigidly orthodox. Using the observation that the salinity levels in lakes were increased by the process of evaporation, it became possible, with some confidence, to extrapolate back to the moment when evaporation had first begun. This would give an estimate of the minimum age of the Earth. This paper, based on work done at Gresham College in the early 1690s, was 'chiefly intended to refute the ancient notion, some have of late entertained, of the eternity of the world'. Despite this disclaimer, it was far more dangerous to prove, as Halley had done, that the 'world may be found much older than many have hitherto imagin'd', than it was helpful for orthodox to show that at least it was not eternal (49).

Halley insisted, in this paper on salinity, that the account of Creation offered in Genesis was allegorical, and was even prepared to speculate about the pre-Adamite world: ''Tis no where revealed in Scripture how long the Earth had existed before this last creation', he observed (50). Similarly, the other paper which Halley was working on at this period, and delayed in its publication for as much as thirty-one years, was concerned with the subject of the Deluge (51). It was read on 12 December 1694, and predated Whiston's very similar account in his *New Theory of the Earth* by at least two years. On at least three counts it demonstrates Halley's attitude to the relevance of theology in the history of the Earth. Firstly, he adopts an allegorical interpretation of Scripture, for the Bible story 'seems too imperfect to be the result of a full Revelation from the Author of this dreadful Execution on Mankind, who would have spoken more amply as to the Manner thereof, had He thought fit to lay open the secrets of Nature to the succeeding Race of Man' (52). Halley went on to reject the similar accounts of Burnet and Hooke: Burnet because he failed to offer a *consistent* physical mechanism, and Hooke because he had invoked a physical cause never found elsewhere, however consistent with the principles of physics. No 'philosophical' account of the history of the Earth could invoke any 'preternatural Digitus Dei'. It was clear to Halley that any unprecedented event in the physical world was just as 'miraculous' as direct intervention by God. Finally, as we have seen, (p. 20) Halley considered that induction from known and observed effects would act as a completely satisfactory substitute for revelation in our research into events 'before the first production of man' (53).

This discussion defines quite precisely the characteristics which Halley attributed to an adequate model of Earth history. In direct contrast to Newton's strictures, such a model would exclude not merely any reference to divine intervention but even any physical phenomenon not actually observed elsewhere. Instead, the account would consist of well-understood and predictable physical events all designed to supplement a totally deficient account provided in Genesis. None of this could have been particularly palatable to Halley's critics of the 1690s. It does not confirm the picture of a contrite and penitent Halley shocked by his failure to obtain the Savilian chair in 1691. The sole conditions of physical plausibility and physical familiarity would be damaging for the system of orthodox belief. On the other hand the details of Halley's work in hydrology and astronomy make the issue more complex. In the work of Derham, for example, we find arguments very similar to those used by Halley also employed in the defence of the faith (54). The *plausibility* of Christianity is stressed (55). This was just what Halley was doing in the papers of 1691–3, in which he attempted a physical argument against the eternity of the world. In that sense he could scarcely be accused of heterodoxy. But Halley was perhaps guilty of a misunderstanding. He had been accused of an unorthodox belief, and attempted to prove the orthodox view by scientic arguments. What was then objected to was not Halley's unorthodoxy, but his use of physical considerations in theology. His papers of the early 1690s were no defence against his critics.

The other complexity follows from the fact that many of the writers most strongly committed to a separation of purely physical and religious accounts were themselves the most staunch Newtonians. Keill and Newton himself resisted attempts to delve too deeply into the story contained in Scripture with natural philosophical tools (56). Is it not strange to find Burnet, the scientifically ill-educated divine, campaigning for a physical account of all cosmology, while Keill, the devout Newtonian, wants to give the miraculous a central place in Nature? The seventeenth century would not have seen this as any kind of irony—similarly, the apparent inconsistencies in Halley's position *vis-à-vis* what we see as the orthodox view show clearly that his own view of what he was about was based on completely different presuppositions of orthodoxy and of the role of natural philosophy. In this paper I have tried to argue for a more self-consistent picture of Halley's work based on his concern with the universal applicability of scientific criteria to any and all data recoverable from the historical record.

Finally, there remain the other sources for Halley's alleged atheism. Thomas Hearne, for example, reported that Halley was investigated in 1690 on an accusation of disloyalty to William III (57). 'This gentleman [Halley] is for confusion, and if all were of his mind, all Government would soon be at an end'. For some of the Anglican establishment Whiggery was often the same as irreligion: 'The said Sir Isaac Newton is a great Whig, and so is Dr Halley, tho' he pretends to be a Tory. In short, Dr Halley hath little or no religion'. There are obviously many problems with this account, and, in fact, even more with Joseph Stock's story of Berkeley's identification of Halley as the 'infidel mathematician' to whom the *Analyst* was directed (58). The critical factor, however, is not the accuracy of such reports but the fact that they were taken seriously, not least, as we have seen, by Halley himself. The debate which we have traced was not conducted between 'reactionary' ecclesiastics and 'progressive' scientists, but within both groups themselves. With this complex pattern of belief went a developing mood which rejected the relevance of science to the concerns of faith, and, indeed, to common life in general. It is not extravagant to claim that Halley was concerned by this separation more than most of his contemporaries. This was, as I have tried to show, just because he saw natural philosophy as a universally relevant system of belief, rather than because he was committed to some fashionable series of accounts which wedded Genesis and Newtonian physics (59). The underlying unity in Halley's work is to be found in its unrelenting pursuit of scientific consistency rather than in worries about religious orthodoxy. This is why when Halley did encounter serious theological opposition it was so particularly difficult for him to respond.

Concerning the motion of light by Mr Halley

Royal Society RBC 7.391 — Read 19 October 1692

Among the Discoveries of this present Age that of the motion of Light being propagated in time is perhaps one of the most Considerable: it is made out by the Experiments of the Eclipses of 2 Satellites which are always found to happen sooner when ♃ is near the Earth than when he is more remote, and without this allowance these appearances are not to be reconciled. Mr Reemer the Author of this Discovery hath Determined from many Observation of the Velocity of light and concluded that it passes the whole interval between the Sun and the Earth in about 10 minutes of time which is at least 10 000 Semid of the Earth that is 1000 Semidiameters or 4 000 000 miles in a minute, an incredible velocity if it were not yet more conceivable than the motion which makes light propagated in an instant as the Cartesians have it; this brings in a sort of analogy between light and sound as being both a tremor of the fluid medium, (viz.) Sound of the Air and Light of the most infinitely Subtil and rarified Æther to which add the propagation of the Circular Waves of Water visible at its surface when anything is cast therein, now that of Water it is easy to see is a very slow motion and perhaps doth not exceed above half a mile in an hour; Sound again is more swift and amounts to about the rate of 300 miles in an hour; and by what has been observed light advances $60 \times 4\,000\,000$ or about 240 000 000 miles per hour.

If then it may be used as an Hypothesis till a better be made out I shall suppose the densities of the Media to be reciprocally proportional to the velocity of the propagation of their tremors, and hence it will be that the Æther or fluid medium universally dispers'd thro' the whole Abyss of Space will be more rare than our air by about 8 000 000 times, and this being the vehicle of light is visibly seen throughout and amongst the orbs of the planets and that they make their way thro' in their seemingly perpetuall motions. Now if we come to consider how great a quantity of this Æthereal matter they penetrate and with how great a velocity it will notwithstanding its great subtility seem reasonable that some part of their motion should be taken off by the opposition of this medium, which tho' it be to be expected but a very small matter yet in Multitudes of years it ought to become sensible. This is what I think to have discovered by a long carefull comparison with all that antiquity has left us relating to the Sun and the Moons motion, and I doubt not but to make it appear that the length of the year grows longer and longer and that in that supposition it will be impossible to reconcile the undenied observations of the Ancients with the curious accounts we have of these motions from Tycho Braehe's time downwards.

The most ancient account we have of the Sun and Moon excepting the fabulous ones of the Chinese do not exceed 2400 years beginning at the days of Mardo Kempadi or as the Scriptures stile him Maradoc baladon being eclipses observed by the Chaldeans in Babylon these eclipses were used by Ptolemy as being about 800 years before his time but in order to reconcile them to the intermediate observations of Hipparchus he was obliged to suppose Babylon nearer to Alexandria by about half an hour than the same author in his Geography hath placed it and as latter Discoveries have made it nearly so much more Westerly than it ought to be.

This for some time made me conclude that they were differing authors, but the reason was that Hipparchus and he having defined the motion of the Moon from the Sun by observations made long after they found they could not solve the Babylonish Eclipses without half an hour's error which they threw upon the difference of the Meridians tho' it was really in the Sun's motion, and these Eclipses are not set down with all the preciseness that were to be used, yet they conspire in the same thing some more some less.

The Almagest of Ptolemy obtained for above 700 years 'till Albategnius an Arab under the Saracen Monarchy by more Curios observation found such errors in Ptolemy's Calculations as were not to be tolerated, and having rectified the Suns motion by the Equinoxes then observed, he wrote his book De Scientia Siderum where he gives 4 Eclipses two of ○ and two of ☾ observed at Antioch and Aracta in Syria, and the Astronomer is such that there is no room to doubt of his skill or fidelity. Now these Eclipses of Albategnius are such as are no means to be solved by the same Hypotheses with the present, but in all of them it is necessary to suppose either the Sun moving unequally faster before and slower since to reconcile them to Ptolemy's time and the present, between which they fall much about the middle, or else the Meridians of Antioch and Aracta are to be made 9 or 10 degrees more easterly than undoubted observations confirmed all manner of ways will allow them.

This is a truth that to reconcile them our Astronomers have been forced to remove these places much more easterly than they are, and particularly our Mr Street (a man whose skill and industry hardly allowed him superior in this art) has been forced to commit a very great Absurdity in his Caroline Tables making Antioch of Syria more Easterly considerably than Babylon itself, tho' by the Judgment of Travellers that have gone it to be 12 Degrees more Westerly, whereas Street makes it half an hour more easterly.

This is the principall Argument on which I would found my conclusion and without the position of the retardation of the ⊙ ⊖ real in a part moves. I am assured that there is no way to make these observations. Now if the Sun's motion be retarded the consequences are very great and considerable, for the Ether obstructing the progressive motion of the Earth will not allow eternity to it or any of the Planets, but according to Mr Newtons prop 15 lib 2 they must move in helicall lines nearer and nearer the Center, and at length must be swallowed up in the Sun. And tho' this Difference be exceeding small and occasioned by the opposite of a medium next to nothing, yet if it be anything it follows that in long time it must have the same effect as of a more dense medium in a shorter time and that how long soever these Globes may last they cannot be Aeternal neither could they have been so (illegible) everything that must perish in time having undergone an aeternity of time upon that Supposition which is therefore absurd. hence will necessarily follow the necessity of that Act of Creation and that these Globes of the Planets were not only formed with a wonderful and incomprehensible designe and contrivance as well as power to Execute, and that the motion they now have was impressed upon them at first in much more proportionate Distances and with such Degrees of Swiftness as may Enable them to subsist many millions of years but that they should not be capable of eternity in the state they now are, is what I presume may no way so well be demonstrated as by this argument, which if it seem to the Honble Soc of the weight I conceive it, at their command I shall more at large explain it and finish the Demonstration thereof.

Ptolemy makes Babylon too near Alexandria by $\frac{3}{8}$ of an hour therefore in reducing the Babylonish observations to Alexandria he makes all their times later than they were, and the interval between them and the observations made at Alexandria too little so that he makes the ☾ revolve in less time than (it) really did.

Some observations on the motion of the sun

Royal Society RBC 7.364 Read October 18 1693

About this time twelvemonth I proposed a sort of Demonstration that the length of the year did change and I supposed that I proved that without that Supposition it was impossible to Reconcile the observation of Hipparchus and Ptolemy with the present motions of the Heavens and to take in those of Albategnius and Itumen Aegyptius who lived about eight or nine centuries after Christ I was then ordered to insert in the Transactions as a thing of some consequence a Discourse about it, but coming more nicely to consider it I found that instead of a slower motion in the Sun it became more swift, and that to solve their immediate observations it was necessary to suppose the year shorter and shorter which not being at that time able to make out to my satisfaction I forebore to publish anything about it.

Since having further considered it I do find that the Orb ought to grow less and less, and the Earth to round the Sun in a spiral approaching him, and the revolutions to grow shorter and shorter as the orb grows less and less which will perfectly render an account of the Phenomenon, and whereas the moon in her motion about the earth seems not to have accelerated proportionately so much as the Sun is chiefly to be attributed to the slowness of the moons motion about the earth a half of the velocity receiving but a of the opposition and in the next place to the greater solidity of the Moon, which for a reason I rendered in Transact. n.195 viz[t] that the Annual Motion about the Sun might receive an equal obstacle both in the Moon as well as in the Earth. That so they may ever keep together, for that the obstacle to motion of any medium is reciprocally as the diameeter of the bodies if of equall Density, but if of unequall Density and the same Magnitudes reciprocally as the Densities, and generally the opposition of the Medium to bodies is reciprocally as the Density to the diameter, so that if the Moon were much denser than the Earth as the Earth is (in) Diameter bigger than the Moon they must needs keep moving legaliter. If the Honble Society shall command me to explain this matter as difficult as it is, and requiring the greatest both of and Geometry to make it out I shall endeavour if possible to make it intelligible, there still wanting a valid argument to evince from what has been observed in nature that this Globe of the Earth ever did begin or ever shall have an end.

Notes

(1) For example, R. S. Westfall, *Science and Religion in Seventeenth Century England*, Ann Arbor, 1973, pp. 113–114; David C. Kubrin, *Providence and the Mechanical Philosophy*, unpub. Ph.D. thesis, Cornell, 1968, p. 234 and pp. 251–252.

Westfall's idea that Halley was 'hasty and impetuous', and Kubrin's view that Halley made 'deliberate attempts to mislead', and that all his statements after 1691 supported the orthodox 'notion of the world's having had a beginning' must now be considerably modified. The paper of 1693 is crucial in this re-assessment.

(2) The most graphic example is the position taken up by Clarke in his correspondence with Leibniz. *The Leibniz-Clarke correspondence*, ed. H. G. Alexander, Manchester, 1956, II, 6 ff.; III, 13 ff. Compare also Clarke's other published views:

Demonstration of the Being and Attributes of God, 1705, p. 206: 'The course of nature is nothing else but the will of God, which course, or manner of acting, (is) in every moment perfectly arbitrary'. Clarke, in R. Watson, *Collection of Theological Tracts*, 2nd edn., 1791, iv, p. 189: 'The generality of men must not be left to the workings of their own minds, to the use of their natural faculties, and to the bare convictions of their own reason'. See M. C. Jacob, *The Newtonians and the English Revolution*, Hassocks, 1976, 96 ff. & 185–186. For the similarities between the position of the latitudinarians and that of the Cambridge Platonists on this see J. E. McGuire, 'Neoplatonism and Active Principles' in *Hermeticism and the Scientific Revolution*, Clark Memorial Library Seminar Paper, Los Angeles, 1977, pp. 97 and 101.

(3) T. E. Jessop, *Bibliography of George Berkeley*, Oxford, 1934, p. 10; See Berkeley's *Analyst* and his *Defence of Free-Thinking in Mathematics* in *Works*, Oxford, 1871, iii, p. 257 n. 1 and p. 305.

(4) H. W. Turnbull, *Correspondence of Isaac Newton*, Cambridge, 1961, iii, p. 199. Fatio told Huygens about the election on 8 September, ibid., iii, p. 168.

(5) Edward Stillingfleet, *Origines Sacrae, or a Rational Account of the Grounds of Christian Faith*, London, 1662; John Tillotson, *The Wisdom of Being Religious*, in *Works*, i, 3 ff. By the Peripatetic atheist, Tillotson explained that he meant 'those . . . who proceed upon Aristotle's supposition of the eternity of the world, but yet deny it to be from God, which he expressly asserts'. (p. 6). See R. Popkin, 'Philosophy of Bishop Stillingfleet', *J. Hist. Philos.*, **9**, 303–319 (1971).

(6) William Whiston, *Memoirs of the Life and Writings of Mr William Whiston*, 1749, p. 123.

(7) E. F. MacPike, *Correspondence and Papers of Edmond Halley*, London, 1932, p. 88.

(8) *Table Talk of Bishop Hough*, in MacPike, p. 264, and Rigaud, *Defense of Halley against the Charge of Infidelity*, Oxford, 1844.

(9) MacPike, p. 269; David Brewster, *Memoirs of the Life, Work and Discoveries of Sir Isaac Newton*, Edinburgh, 1855, ii, pp. 459–460. See also Kubrin, 1968, p. 248, n. 27.

(10) See P. D. Lawrence & A. G. Molland, 'Gregory's Inaugural Lecture at Oxford', *Notes & Records R. Soc. Lond.*, **25**, 145, (1970).

MacPike, p. 265, notes the story of a Scot who travelled to London just to see 'a man with less religion than Dr. Gregory'. See also Agnes Stuart, *The Academic Gregories*, Edinburgh, 1901, p. 58. Gregory assumed that Caswell would defeat both Halley and himself. (Turnbull, iii, p. 181).

(11) Pierre Mandonet, *Siger de Brabant*, Louvain, 1908, vii, pp. 175–191, for an analysis of the Paris condemnation of 1277. Tillotson's attack is summarized in John Ray, *The Wisdom of God . . .*, 1691, 40 ff., and Kubrin, 1968, ch. 2. M. C. Jacob suggests that in the 1680s Halley and Tillotson were on very good terms. (Jacob, 1796, pp. 30–31).

(12) Colin Ronan, *Edmond Halley: Genius in Eclipse*, London, 1969, p. 120.

(13) Whiston, *Memoirs*, pp. 242–243; R. S. Westfall, 1973, p. 134, notes a suggestion that Halley was also an anti-Trinitarian.

(14) E. F. MacPike, *Hevelius, Flamstead & Halley*, London, 1937, pp. 90–93; F. Baily, *An Account of the Reverend John Flamsteed*, London, 1835 ff., p. 132 and pp. 193–195; Ronan, p. 125 and p. 127. For the specific issue of Flamsteed against Halley's appointment to Oxford, see Bodleian, Rigaud Mss. 7 f. 63, and 8 f. 17, where it becomes clear that Flamsteed had been promised the position himself by Bernard, but being a Cambridge man was prevented, and expected Halley to get the place. On the Perkins theory of six magnetic poles in the earth, see T. Birch, *History of the Royal Society*, iv, pp. 18–19.

(15) Hélène Metzger, *Attraction Universelle et Religion Naturelle chez Quelques Commentateurs Anglais de Newton*, Paris, 1938, ii, pp. 99–102.

The problem of reconciling the two 'Books' of Nature and of Revelation is epitomized by Kenelm Digby's letter of 1635 in which he tries to show how the Flood came by natural causes at that moment when man's iniquity reached its worst: 'It belongeth to the wisedome of God to make naturall and morall regardes to keepe even pace together'. V. Gabrieli, *Sir Kenhelm Digby*, Rome, 1957, p. 281.

(16) John Keill, *An Examination of Dr Burnet's Theory of the Earth*, Oxford, 2nd edn., 1734, p. 28; Metzger, p. 23n. 3; also Westfall, p. 193, on Newton's correspondence with Bentley.

(17) Thomas Burnet, *Sacred Theory of the Earth*, London 1691, repr. Fontwell, 1965, i, pp. 5–6, and ii, pp. 387–390.

(18) K. Collier, *Cosmogonies of Our Fathers*, New York, 1934, pp. 41–42 and p. 75 n.2.

(19) Erasmus Warren, *Geologia or a Discourse Concerning the Earth before the Deluge*, London 1690, pp. 73–80 and pp. 189–200. See also Keill, *op. cit.*

(20) Brewster, ii, pp. 452–453.

(21) See Warren, 1691, pp. 2–3, and p. 98 ff. Also John Beaumont, *Considerations on a Book Entituled the Theory of the Earth*, 1692/3, p. 86.

(22) Edward Stillingfleet, *Origines Sacrae*, ii, p. 9; compare this expression of his main concern:

'The great gullery of this world hath been, taking philosophical Dictates for the standard of reason, and unprov'd Hypotheses for certain foundations of our discourses to rely upon. And the seeking to reconcile the Mysteries of our Faith to these hath been that which hath almost destroy'd it, and turn'd our Religion into a mere Philosophical speculation'.

(23) See Halley's use of ancient observations of comets, to determine their period of return, *Phil. Trans.*, **24,** 1886 (1704/5), or of the positions of stars to detect their proper motion, *Phil. Trans.*, **30,** 736–738, (1717–1719). I reject Kubrin's claim (p. 254 n. 40) that this kind of work was seen by Halley as an 'antidote' to Hooke's alternative cosmology.

(24) Keill, p. 17, writes that 'of all philosophers those have done religion the least service who have not only asserted that the world was made by the laws of mechanism without the extraordinary concurrences of Divine power, but also all the great changes which have happened to it'.

Against Whiston, *A New Theory of the Earth*, 1696, 6th edn. 1755, p. 49, Keill replied that 'although Mr. Whiston has been pleased to ridicule my fondness for Miracles, yet since all the natural causes he has assign'd are so vastly disproportionate to the effects produc'd he may be at least perhaps be convinc'd that the easiest, safest, and indeed only way is to ascribe 'em to miracles'. (p. 347).

(25) William Derham, *Astro-Theology: or a Demonstration of the Being & Attributes of God from a Survey of the Heavens*, 1714, xx; the institution of the Boyle Lectures is in Robert Boyle, *Works*, i, pp. 100–108. See also Metzger, iii, 156, on Derham.

(26) Edmond Halley in *Phil. Trans.*, **33,** 123–124, (1724–1725).

(27) Halley's first published paper on terrestrial magnetism, critical of Descartes, Gilbert, and Kircher, was given at the Royal Society on 23 May 1683. (*Phil. Trans.*, **13,** 208–221 (1683)). After a survey of all the reliable observations of the magnetic variation throughout the world, Halley concluded that the irregular pattern of isogonic lines could be accounted for by 'four ... magnetical poles which occasion the great variety and seeming irregularity which is observed in the variations of the compass.' Halley followed this with an exchange of letters with J. C. Stürm in 1684–1686. Stürm was the founder of the Collegium Curiosum sive Experimentale (1672) and he proposed an international solution to the problems of terrestrial magnetism. (M. Ornstein, *The Role of Scientific Societies in the Seventeenth Century*, Chicago 1928, p. 177.) See also Henry Oldenburg's *Correspondence*, ed. A. R. Hall and M. B. Hall, ii, p. 488. The details of the correspondence are given in MacPike, p. 55; and Royal Society Library LBO 9.297. Compare Turnbull, ii, p. 433 n.5. Stürm suggested that the poles could be explained by an influx of the particles of a magnetic fluid, but Halley thought that this was too similar to the ideas of Descartes. Halley considered the idea of heterogeneous magnetic rock 'maxime probabile'. This discussion culminated in 1691 with his second paper on the problem. On other ideas of a hollow earth, see C. Zirkle in *Isis*, **37,** 156 (1947).

(28) Sidney Chapman, 'Edmond Halley and Geomagnetism', *Nature, Lond.*, **152,** 231–237 (1943), points out that two concentric magnetic spheres, each with two poles, would still only produce a field with two overall poles, and not four. Therefore Halley's entire scheme is intrinsically mistaken.

29) *Phil. Trans.*, **17,** 564 (1691–1693).

(30) *Phil. Trans.*, **17,** 577–578 (1691–1693), and A. Armitage, *Edmond Halley*, London, 1966, p. 74. As to a demonstration of the hollow earth, Halley attributed the auroras of 1716 and 1719 to the escape of luminous gas from between the two shells of the earth. 'If such a Medium should be thus inclosed within us: what should hinder but we may suppose that some parts of this lucid Substance may, on very rare and extraordinary Occasions, transude through and penetrate the Cortex of our Earth, and being got loose may afford the matter whereof our Meteor consists'. (*Phil. Trans.*, **29,** 428 (1714–1716)). The escape would be at the thinnest section, which, as Newton showed, is at the poles. (*Principia*, iii, Prop. 19). Since Halley assumed that the effluvia were the result of magnetic action if of a very peculiar intra-tellurian type, Halley made the original magnetic observations of the Aurora Borealis. (Chapman, p. 236). Neither Ronan, who talks of 'another hypothetical medium' (p. 199), nor Armitage (p. 182 ff.) emphasize this irony.

(31) 'Concerning the Motion of Light', Royal Society Library RBC 7:391; MacPike, p. 229. In this paper Halley makes at least two numerical errors—he gives 4 million mph instead of 11 million mph as the speed of light Rømer derived, and he then obtains the result that the aether is 8 million times rarer than air where he should have 800 000.

(32) Brewster, i, p. 215, discusses this journey to England by Huygens. See Christiaan Huygens, *Oeuvres Complètes*, The Hague 1944, ix, p. 333, and xxi, p. 435 n. 31.

W. R. Albury discusses Huygens' influence on Halley in his 'Halley and the Traité de la Lumière of Huygens: New Light on Halley's relationship with Newton' *Isis*, **62,** 445–468 (1971).

Analysing Halley's optical queries in *Phil. Trans.*, **17,** 998–999 (1691–1693), Albury concludes that in 1690–1691 Halley firmly defended the theory of light put forward by Huygens against Hooke's attacks. Halley agreed with the Dutchman that light was a vibration in the aether, that it moves more slowly in denser media, and that this explained the phenomena of refraction. This all suggests that he was probably ignorant of Newton's current optical work. This is confirmed by the opening paragraph of his paper *On the Motion of Light* in 1692. By contrast, there seems to be a radical difference between the two papers (that of 1690 and that of 1692) over the issue of the uniform density of matter. I do not accept Albury's view that Huygens accepted this principle—instead, I find that in 1690 Halley definitely agreed with Huygens that the Universe was *not* 'constituted by the Various Texture and Coalition of the same sort of Atoms', while in 1692 Halley agreed with Newton that it *was*. This suggests that Halley was even more firmly committed to Huygens in 1689–1691 than even Albury argues.

(33) *Principia*, iii, Prop. 10, 'That the motions of the planets may subsist an exceedingly long time': 'Therefore the celestial regions being perfectly void of air and exhalations the planets and comets meeting no resistance in those spaces will continue their motions through them for an immense tract of time'. On February 18 1692/3 Newton also wrote to Bentley on 'wt proportion ye void space in our system may bear to ye solid mass'. (Turnbull, iii, pp. 246–252.) An unpublished manuscript of the 1690s states that 'in a heaven more filled with matter (the planets) would lose a large part of their motion

in proportion to the density, and even more if they were not solid bodies, . . . unless the spaces of the heaven and of air were nearly vacuous'. (A. R. Hall & M. B. Hall, *Unpublished Papers of Isaac Newton*, pp. 312–315).

Finally, Cohen and Koyré note an addition to the second edition of the *Principia* at Bk. 3 Prop. 6 Cor. 3: 'if the quantity of matter in a space can by any rarefaction be diminished, what should hinder a diminution to infinity?' It is quite evident that Newton insisted on an indefinitely rare aether which offered no resistance to planetary motion, at this period in his development.

(34) Huygens, *Oeuvres*, xxi, p. 473. All Newtonians, including Halley, shared the premise that all matter is intrinsically the same density. See Arnold Thackray, *Atoms and Powers*, Harvard, 1970, ch. 2. This is why Halley saw the fact that the moon was denser than the earth as a puzzle.

(35) See Bernard Gagnebin, 'Discours sur la Cause de la Pesanteur of Fatio de Duillier', *Notes and Records R. Soc. Lond.*, **6**, 106–160 (1949). Compare Turnbull, iii, 69 n. 1 and iii, 191, for Gregory's comments.

(36) See *Principia* ii, Prop 35 on resisted motion. Notes on the Wallis correspondence can be found in MacPike, p. 77 and pp. 80–82; A. J. Turner, 'Hooke's Theory of the Earth's Axial Displacement', *Br. J. Hist. Sci.*, **7**, 166–170 (1974); R. Gunther, *Early Science at Oxford*, Oxford, 1932 ff., iv, 199.

(37) *Origines Sacrae*, pp. 103–104: 'The high priests in Egypt prohibited all prying into their mysteries by any but those who had the same interest with themselves, and therefore were unlikely to discover any thing that might lessen their reputation. Whereas had there been nothing but Truth in their records, or that Truth had been for their Interest, what need had there been of so great reservedness and privacy?'

(38) MacPike, p. 78. For discussions of Hooke's theory see Turnbull, iii, pp. 43–44 n. 6; Birch, iv, pp. 511–513, 516, 521–525, 528; Royal Society Library RBO 6:53.

(39) Turner, pp. 166–170. Pierre Petit followed Tycho in proposing the idea that there was a change in the meridians, attributing to this cause the secular variation in the magnetic variation. (Oldenburg, iii, p. 382). This was difficult to establish since even the latitude was not accurately known, but it seemed simpler than invoking a magnetic cause for the secular variation. For investigations see Gilbert, *De Magnete*, tr. Mottelay (New York, 1893), pp. 315–317; Oldenburg Letter 1475 for Hevelius (1670), Gunther, vii. p. 554, Wallis and Hooke (1680), and E. G. R. Taylor, *Mathematical Practitioners*, p. 404, for Würzelbauer (1687) and Wallis (1701).

On 20 November 1684 Hooke lectured at Gresham College on the change in latitudes between ancient and modern times, arguing that such an alteration in the poles was just as likely as the known change in the magnetic poles. However, he did criticize Petit's similar idea, since it was based on a mistake Scaliger had made as early as 1604, in commenting on Gilbert's theory of magnetic variation. See R. Waller (ed.) *Posthumous Works of Robert Hooke*, 1705, p. 487.

(40) Unpublished paper, Royal Society Library RBO 9.193; MacPike, p. 210. Read February 1st 1687/8. Compare the demonstration that the latitude of Nuremburg had not changed since 1480. (*Phil. Trans.*, **16**, 403–406 (1686–1687). Ronan, 'Edmond Halley and Early Geophysics', *Geophys. J.*, **15**, 241 (1968). On Newton's reaction see L. Trengrove, 'Newton's Theological Views', *Ann. Sci.*, **22**, 277 (1966).

(41) MacPike, p. 232, and *Phil. Trans.*, **17** 913–921 (1691–1693), 'Emendationes ac notae in

vetustas Albatenii Observationes astronomicas, cum restitutione Tabularum Lunisolaris ejusdem authoris'. Halley continued his new tone of criticism of ancient observations in *On the Geography of the Ancients and Moderns* (1696), MacPike, p. 166. Halley wished the ancients 'had been a little more carefull in noting the latitudes of their places and their positions in respect of the Meridian: wherein perhaps they found some difficulty...'

(42) See appendix.

(43) C. Schneer, 'Rise of Historical Geology in the 17th Century', *Isis*, **45**, 256 (1954).

(44) *Phil. Trans.*, **19**: 174 (1695–1697). Early references to Halley's discovery can be found in Henry Pemberton, *A view of Sir Isaac Newton's Philosophy*, repr. London, 1972, p. 203; and as an addition at the very end of the second edition of the *Principia* (ed. Cohen & Koyré, Harvard, 1972, pp. 758–759). Both authors observe that the moon's acceleration may be due to an *increase* in the size of the Earth.

(45) The Journal Book entry reads: 'Halley read a paper of his own, concerning a Demonstration of the Contraction of the Year, and promising to make out thereby the necessity of the world's coming to an end, and consequently that it must have had a beginning, which hitherto has not been evinced from anything that has been observed in Nature.' (MacPike, p. 232).

(46) The paper on the deluge is in *Phil. Trans.*, **33**: 118–125 (1724–1725). See note 51.

(47) *Phil. Trans.*, **19**: 577 (1695–1697). The dynamics here are quite complex. In 1692 Halley had assumed that if the planets received a constant opposition from the aether they would be uniformly slowed down in their orbits. However, as he realized in 1693, the opposition of the medium to an orbiting body forces it into a shorter orbit, in which it travels closer to the centre but with an *increased* velocity. To transpose this into more modern terms, the planets lose potential energy at a faster rate than they gain kinetic energy by being in a shorter orbit, and so gradually spiral into the sun. (*Principia*, Bk. 2, Section 4, especially Lemma 3 Cor. 9). This problem was, of course, much argued between Hooke and Newton in 1679–1681. (See, for example, F. Centore, *Hooke's Contributions to Mechanics*, Hague, 1970).

Secondly, it is simple to derive the relationship Halley uses to account for the greater density of the moon and its effect on lunar motion: Let the density of the body be D, the diameter d, and a, b are arbitrary constants. Then we have:

Mass of body $= aD.d^3$ and Retarding force $= bd^2$,

so we have that deceleration (which Halley calls opposition) is $\frac{b}{aDd}$, and *is* reciprocally as the density and the diameter.

Note that there seems to be a mistake in Halley's final statement of this relation. (Paper of October 1693).

Note also this interesting comment by Lalande on the possibility of Halley's comet of 1759 being retarded by the aether; 'Cette resistance de la matiere etherée qu'on crut d'abord apercevoir dans tous les corps celestes, sembloit déja annoncer ses suites funestres pour l'humanité.... J'ai demontré dans un Memoire... que ce systeme de L'acceleration universelle n'avoit rien de réel.' (J. J. de Lalande, *Tables Astronomiques de M. Halley*, Paris, 1759, p. 108).

Finally, W. R. Albury (*Isis*, **62**, p. 462 (1971)) mistakenly assumes that in this paper

Halley demonstrated the retardation of the planets. He ignores Halley's change of position between this and the earlier paper of 1692.

(48) See A. K. Biswas. 'Edmond Halley, Hydrologist Extraordinary', *Notes & Records R. Soc. Lond.*, **25**, 47–57 (1970).

(49) *Phil. Trans.*, **29**: 299 (1715). The other papers Halley produced in this field were in 1687 (**16**: 366) on water balance in the Mediterranean, in 1691 (**17**: 468) on the origin of springs, and in 1693 (**18**: 183) on the rate of evaporat on from a water surface. Biswas estimates that from salinity measurements Halley might have obtained a minimum age of the earth of 90 million years!

(50) *Phil. Trans.*, **29**: 296 (1714–1716). The idea of extrapolating back to the creation from current observations can be found as early as the work of George Goodman, who in his *Fall of Man* (London, 1616, 412) states that if 'yee consider the daily decay of nature, and have relation to the seuerall degrees of this decay, you may in some sort gesse at the birth and beginning of nature'.

(51) *Phil. Trans.*, **33**: 118–125 (1724–1725). This paper was not published until 1725. My interpretation differs from that of Westfall (pp. 100 and 113–114). I agree that Halley 'ended the compromise with Biblical miracles'. This paper has been stressed just because it is one of his few statements on science and religion. I do not see it as 'impetuous', since we can now see the long tradition of such speculation which stressed back into the 1680s. To describe his addition to the paper, in which he claims that he is merely referring to an event in earth history before Adam, as a 'hasty and humiliating retreat' is to misunderstand its importance. Halley is, after all, explicitly separating science from the concerns of Scripture—Kubrin (p. 241 n. 10) notes Halley's very ambiguous use of 'authority' in this debate. He can scarcely both be 'impetuous' and 'fearful', and indeed that qualifying clause which places 'a posteriori induction' on the same level as 'Revelation' is one of the most dramatic affirmations of this principle. Who was the person 'whose judgment I have great cause to respect' who pointed out the problems with the paper? In view of his other criticisms, this may well have been Isaac Newton. (MacPike, p. 264. See Jacob, p. 136 on Newton's and Halley's views on the Deluge).

(52) *Phil. Trans.*, **33**: 120 (1724–1725).

(53) *Phil. Trans.*, **33**: 122 (1724–1725).

(54) Derham, *Astro-Theology*, xx. See also p. 190, and Whiston, p. 72.

(55) H. Guerlac & M. C. Jacob, 'Bentley, Newton and Providence', *J. Hist. Ideas*, **30**, 307–317 (1969); see also the discussion in Jacob, 1976, pp. 214–215. The consequences of the ideas put forward by Locke, Bentley, Derham, and Toland were clearly widespread. This seems to make Jacob's latitudinarian-freethinker distinction difficult.

(56) Keill, 28; Brewster, ii, 452–453. Whiston, preface (Discourse) p. 2, criticizes those 'so sensible of the wildness and unreasonableness of (the Biblical account) that they have ventur'd to exclude it from any just sense at all, asserting it to be a meer Popular, Parabolick, or Mythological Relation'. See Westfall, 92 ff.

(57) MacPike, pp. 268–269.

(58) Joseph Stock, the first biographer of Berkeley (*Life of George Berkeley*, Dublin 1776) states that in 1719 Addison told Berkeley of a deathbed conversation he had had with their mutual friend Dr Garth. Garth had refused the sacraments, telling Addison that 'I have good reason not to believe these trifles, since my friend Dr Halley . . . has assured me that the doctrines of Christianity are incomprehensible and the religion itself an

imposture'. (p. 28) Berkeley apparently then wrote the *Analyst* to show that the 'mathematicians' had accepted worse 'incomprehensibles' in the calculus. Yet Garth died in January 1719, and Addison in the following June, and Berkeley was out of the country between 1716 and 1720. (Jessop, p. 10). This is not, perhaps, as important as the fact that the story might have been believed and Halley identified as Berkeley's target. Ronan (p. 121) confusingly names Richard Bentley as the author of the *Analyst*.

(59) 'Espinasse, 'Decline and Fall of Restoration Science', in C. Webster (ed.), *The Intellectual Revolution of the 17th Century*, London, 1974, p. 351.