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Huygens's reaction to Newton's gravitational theory

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Much has already been written about the reception of and resistance to Newton's *Principia* – especially in France – and about Huygens's criticism of Newton's gravitational theory.[1] Such studies have usually focused upon the differences between Newton's and Descartes's conceptual world views that affected these reactions. Newton did not provide a mechanical explanation of gravitation, while in Descartes's theory gravity was an effect due to the motion of a vortex of subtle matter around the Earth.[2] Different conceptual views were, of course, instrumental in producing strong resistance to Newton's theory. Some papers have further pointed out that there were other aspects of Newton's work that also contributed to its slow acceptance – such as its mathematical difficulty.[3]

The part played by such factors cannot be ignored. None the less, we may ask whether it is possible to assert that people who did not accept Newton's gravitational theory at once were misled either by prejudice or by the weakness of their mathematics? Clearly this is not so. Other factors were also at work.

The present chapter proposes to analyse Huygens's reasons for rejecting Newton's theory, with the aim of showing that, in a sense, there were good methodological reasons for his resistance to accepting it.

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1 The classic reference is J. Brunet, *L'Introduction des théories de Newton en France au 18ème siècle tome 1 – Avant 1738* (Paris, 1931). See also: René Dugas, *La Mécanique au XVIIème Siècle* (Paris, 1954); Alexandre Koyré, *Newtonian Studies* (Chicago, 1968); I. Bernard Cohen, *The Newtonian Revolution* (Cambridge, 1980); A. Rupert Hall, 'Newton in France: a new view', *Hist. Sci.*, 1975, 13: 233–50; E. A. Fellmann, 'The *Principia* and continental mathematicians', *Notes Rec. Roy. Soc. Lond.*, 1988, 42: 13–34.

2 René Descartes's vortex theory was first fully described in his *Principia Philosophiae*, (Amsterdam, 1644). See Eric J. Aiton, *The Vortex Theory of Planetary Motion* (London, 1972).

3 Greenberg has recently stressed this aspect of Newton's theory of the flattened Earth: J. L. Greenberg, 'Isaac Newton et la théorie de la figure de la Terre', *Rev. Hist. Sci.*, 1987, 40: 357–66.

The choice of Huygens as a case study

Why study the reaction of Huygens, instead of someone else? Several circumstances determine our choice. Many of their contemporaries would have deemed Huygens a fit judge of Newton's theory.

Let us look at a letter from Fatio de Duillier to Huygens, written when the former was in England, on 24 June 1687:[4]

> I have been three times at the Royal Society where I have heard both very good proposals and some platitudes. Some of those gentlemen that compose it have an extremely favourable prejudice about a book from Mr. Newton that is now in press and will be issued in three weeks. . . . I have seen part of this treatise and it is certainly very fair and full of many valuable propositions, but I wish, Sir, that the Author had taken some advice from you about this principle of attraction that he assumes between celestial bodies.

So, according to Fatio de Duillier, Huygens might be a good judge of Newton's work. Were these no more than flattering words? I think not. Let us briefly recall Huygens's qualifications.

First: he was one of the few mathematicians of that time who could understand and master the *Principia*. Second: he was receptive to Newton's work, as his letters show. Third: he had proposed an earlier theory of gravity and discussed the flattening of the Earth independently of Newton. All these circumstances – which perhaps make Huygens unique in his time – contribute to our choosing him as a privileged expert whose opinions are worthy of attention.

Let us begin by clarifying the points just made about Huygens's reputation.

Most contemporaries would have been in agreement on the first point: Huygens was generally accepted as being a very good mathematician.[5]

Huygens is now best remembered for his wave theory of light. Indeed, his *Traité de la lumière* was a wonderful work. It did not merely put forward a suggestive theory of the nature of light. It was remarkable in expressing the theory in mathematical language and in being able to provide the extremely difficult proofs concerning double refraction in Iceland spar. Anyone who once tried to follow Huygens's demonstrations would certainly have been struck by his mastery of geometrical methods. From this point of view, his *Treatise on light* may be said to be far more advanced than Newton's *Opticks*. This is not the relevant point here, however, since Huygens's treatise appeared only after Newton's *Principia*. At that time, the basis for Huygens's eminence was his *Horologium oscillatorium*.

This work, published in Paris in 1673, represented an important step in the development of classical mechanics. Its geometrical style, stressed in its very

[4] Letter 2465, from Fatio de Duillier to Huygens, *OCCH*, 9: 167.

[5] According to Whiteside, 'in Newton's own lifetime only a handful of talented men . . . had, each in his own way, achieved a working knowledge of the *Principia*'s technical content' – and these few were Huygens, Leibniz, Varignon, de Moivre and Cotes – in Fellmann, *op. cit.* (note 1), 13.

title,[6] presents clearly stated propositions of increasing difficulty that are proved in what was recognized at the time as a rigorous way. It is this same style that was later used by Newton in the *Principia*. Although parallels can be traced with Galileo's *Due nuove scienze* (Leiden, 1638), there is a wide gap between the two works. Any high-school pupil can now understand Galileo's book. The same cannot be said of the *Horologium*. Like the *Principia*, Huygens's work includes several very difficult demonstrations – a challenge to mathematicians of the day.

It is also important to notice that Huygens's mechanics is very close to Newton's. It does not include all of Newton's ideas, of course. However, most of Newton's mechanics was already there – and all of Huygens's propositions are compatible with the *Principia* (a statement that does not apply, for example, to Galileo's works).

To drive this point home, let us compare Huygens with Descartes. There can be no doubt that Descartes was a good mathematician, but his natural philosophy was qualitative (except his geometrical optics) and his mechanics is weak and incompatible with that of Huygens and Newton. It is well known that there is a wide gap between Descartes's *Principia* and Newton's. On the other hand, Huygens and Newton are much closer. They belong to the same world. Newton would certainly have been proud to have written the *Horologium oscillatorium*.

This is the natural philosopher and mathematician that we propose as an expert assessor of Newton's gravitational theory. Very well: he seems to have the necessary competence. Is he sufficiently impartial?

One might think that the opposition between Newton's and Huygens's theories of light might predispose the Dutch scientist against the theory of gravitation – resulting in an attitude something like Leibniz's systematic opposition to Newton. That was not the case, however. Huygens praised Newton's optical researches highly. Besides, his travelling to England, in 1689, merely to meet the author of the *Principia*, is clear evidence that there was no personal prejudice of that kind. One may quote, for instance, Huygens's letter of late December 1688 to his brother Constantijn, who was then in England:[7]

> I think that the Royal Society is now on vacation. Nevertheless, you may have some chance of seeing Mr. Boyle and some other members. I would like to be in Oxford, only to meet Mr. Newton, whose beautiful inventions found in the work that was sent to me, I extremely admire. Maybe I shall send you a letter and you will find an easy way to deliver it to him.

Huygens's theory of gravity

The kind of evidence presented in the previous section allows us to accept that Huygens was a competent and unprejudiced judge of Newton's work. Let us now

[6] The full title is: 'Horologium oscillatorium sive de motu pendulorum ad horologia aptato *demonstrationes geometricae*' (my emphasis), (Paris, 1673). English translation in J. Yoder, *Unrolling Time: Christiaan Huygens and the Mathematization of Nature* (Cambridge, 1984).

[7] Letter 2529, of 30 December 1688, from Christiaan to Constantijn Huygens, *OCCH*, 9: 304.

briefly study his own work on gravity, to compare his method with Newton's, as applied to the same subject.

While he was living in Paris, in 1669, Huygens presented to the French Academy of Sciences a short work called 'On the cause of gravity'.[8] This small essay was intended to provide a mechanical explanation of the force that draws heavy bodies towards the Earth. The main ideas were explicitly Cartesian. About 18 years later, Huygens was led to think about the influence of the Earth's rotation upon gravity. The stimulus for this new work was information from Richer, published in 1686 by Mariotte.[9] In 1672 Richer had found that a pendulum had to be shorter at Cayenne (compared to Paris) to beat seconds. It was only natural that Huygens, the great pendulum authority, should try to explain this phenomenon. He attributed it to the centrifugal force caused by the rotation of the Earth. This also led him to suggest the Earth was flattened and to investigate the relation between latitude and gravity.

After the publication of Newton's *Principia*, Huygens wrote an additional final section to his essay. It was printed under the name *Discours de la cause de la pesanteur*, in 1690, together with his *Traité de la lumiere*.[10] At the end of this work, Huygens compares his own theory with Newton's. He also presents an analysis of the motion of bodies in resisting media – a subject also treated by Newton. In this instance, his results completely confirmed those of the *Principia*.

Like Descartes, Huygens states that there is no void in the Universe and there is a fluid invisible matter moving around the Earth. This fluid is described by Huygens as consisting of small particles circulating around the Earth, in all directions, with a very large velocity. On account of this motion the fluid matter endeavours to recede from the centre. It produces an opposite thrust, towards the centre, upon bodies that are at rest inside the fluid. Notice that Huygens does not accept Descartes's vortices turning about an axis.[11]

Huygens assumes that the particles of this fluid matter are able to pass undisturbed through solid matter. They are even able to move through and inside the Earth – otherwise, there could be no gravity inside a well.[12]

In this theory, there is no assumption about the force or mutual influence between two bodies at a distance from one another; there is no reference to the motion of planets around the Sun; and no assumption about a dependence of force on distance from the Earth. For Huygens, the Earth's gravity is constant, both inside and outside its body.

In the part of the *Discours* written before the publication of Newton's *Principia*, Huygens assumes at the outset that the Earth is spherical, and computes the changes

[8] This first essay and a discussion of its context can be found in *OCCH*, **19**: 617–45. The later development of Huygens's ideas on gravity is documented in vol. 21, p. 377–426.

[9] See *OCCH*, **9**: 130–1. See also: C. Wolf, 'Mémoires sur le pendule', in: Société Française de Physique, *Collection de Mémoires relatifs à la Physique* (5 vols., Paris, 1884–91), **4**: B-13.

[10] C. Huygens, *Discours de la cause de la pesanteur* (Leiden, 1690). This work was reprinted in *OCCH*, **21**: 427–88. All references in the present article are to the pages of the original edition.

[11] *Ibid.*, 131–7. [12] *Ibid.*, 139.

in the length of a seconds pendulum as a function of latitude. The only cause affecting the motion of the pendulum is assumed to be the centrifugal force due to the rotation of the Earth. From these hypotheses, he computes that the seconds pendulum should be shorter at the equator than at the pole by one part in 289. He also shows that, due to the rotation of the Earth, the surface of the seas cannot be spherical and there must be an increase in the radius of the Earth from the pole to the equator – but he does not compute the quantity of this polar flattening.

It has already been pointed out[13] that Huygens's theory is very weak compared with Newton's. The latter's treatment of these same problems is very sophisticated. From the hypothesis of forces varying inversely as the square of the distance, Newton first computes the gravitational properties of a homogeneous ellipsoid.[14] Under the assumption that this applies to the Earth, he studies the equilibrium of columns of liquid from the surface to the centre of the Earth.[15] By an ingenious process he computes the difference between the equatorial and polar radii as equal to 3/689ths of the polar radius. He also computes the variation of gravity with latitude[16] and obtains a polar gravity greater than the equatorial gravity by one part in 230. Measurements made in the eighteenth century led to results close to Newton's predictions.[17]

After reading Newton's *Principia*, Huygens wrote a final section to his *Discours*. He uses Newton's method of considering the equilibrium of water columns and computes the figure of the Earth. However, he computes no change in gravity caused by this new shape: he still assumes that gravity is constant. Indeed, he even states that gravity is not produced by the Earth but is in fact itself responsible for the shape of the Earth. He computes a small difference between polar and equatorial radii – just one part in 578. He compares his own work with Newton's:

> I have supposed that gravity is the same both inside the Earth and at its surface; . . . Mr. Newton . . . makes use of a completely different assumption – I will not examine it here, because I do not agree with a Principle that he assumes in this and in other computations. This is: that all small parts that one may imagine in two or more different bodies attract or tend to mutual approach. I could not admit this, since I clearly saw that the cause of such an attraction cannot be explained by any principle of Mechanics or by the rules of motion. I am also not convinced of the necessity of the mutual attraction of whole bodies; for I have shown that, even if the Earth did not exist, the bodies would not cease to tend to a centre by the so-called gravity.[18]

[13] F. Mignard, 'The theory of the figures of the Earth according to Newton and Huygens', *Vistas Astr.*, 1987, **30**: 291–311.
[14] I. Newton, *Philosophiae naturalis principia mathematica*, (London, 1687; 2nd edn, Cambridge, 1713; 3rd edn, London, 1726), Book I, Proposition 91.
[15] *Ibid.*, Book III, Prop. 19.
[16] *Ibid.*, Book III, Prop. 20.
[17] For the empirical confrontation between Newton's and Huygens's theories, see: I. Todhunter, *A History of the Mathematical Theories of Attraction and the Figure of the Earth – from the time of Newton to that of Laplace* (2 vols., London, 1873; New York, 1962); R. de A. Martins, 'Huygens e a gravitação Newtoniana', *Cad. Hist. Fil. Ci.*, series 2, 1989, 1: 151–84.
[18] Huygens, *op. cit.* (note 10), 159.

The contrast between Huygens and Newton

Part of the difference between Huygens's and Newton's views about gravitation (or gravity) is due to their conflicting methodologies. In the preface to his *Discours*, Huygens states his basic commitment to Cartesian principles:

> When Nature leads the so-called heavy bodies to the Earth, she acts by such secret and imperceptible ways that the senses have found nothing there, even though much attention and industry were employed. This constrained philosophers of past centuries to look for the cause of this admirable effect but in the bodies themselves and to ascribe it to some inner inherent quality that makes them to move downwards and to the centre of the Earth, or to an appetite of the parts to unite to the whole. That is not to expose the causes, but to suppose obscure and unknown principles . . . .[19]

After discussing some attempts to explain gravity, Huygens states:

> Mr. Descartes has recognized, better than those that preceded him, that nothing will be ever understood in Physics except what can be made to depend on principles that do not exceed the reach of our spirit, such as those that depend on bodies, deprived of qualities, and their motions.[20]

Any explanation of gravity by means of attraction was unacceptable to Huygens. Before reading Newton's book, he writes a letter to Fatio de Duillier in which we find the statement: 'I want to see Newton's book. I am glad that he is not a Cartesian, as long as he does not present to us suppositions such as that of attraction'.[21] After reading the *Principia*, Huygens gave a clear account of his opinion of Newton's ideas. He accepted that the following features of the gravitational theory had a solid basis.[22]

(a) The existence of forces between the Sun and the planets and among the planets themselves. Huygens supposed that he could explain these forces by an extension of his model for the Earth's gravity.

(b) The decrease of such forces in accordance with the inverse square law – a feature necessary to explain elliptical orbits and the relation between the Moon's orbital acceleration and terrestrial gravity.

(c) The non-existence of Descartes's vortices: this accounted for the planets moving freely, and was necessary to explain why the eccentricities and inclinations of the orbits were constant. This premise was also required for understanding the motion of comets.

Notice that all these features imply a considerable departure from earlier Cartesian ideas. So Huygens was clearly not a blind follower of Descartes, as one might perhaps have expected.

It might seem that rejecting Cartesian vortices and accepting the inverse square law of gravitation entails accepting the whole of Newton's gravitational theory. This is not

[19] *Ibid.*, 125. [20] *Ibid.*, 126.
[21] Letter 2746, of 1692, in *OCCH*, 10: 354. [22] Huygens, *op. cit.* (note 10), 159–60.

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so. There were two main points on which Huygens was in disagreement with Newton. The first one was that Huygens did not accept attractive forces acting at a distance. He conceived, as Descartes did, that forces must be transmitted by something material – some kind of aether. It was, in fact, possible to build a mechanical model for Huygens's aether compatible with the inverse square law. Leibniz and Fatio de Duillier were later to do so.[23]

The second difficulty was that Huygens did not accept that Newton's law of gravitation could be applied to the smallest parts of bodies. This was a basic premise used by Newton to compute the gravity of the Earth (both in the spherical and in the oblate spheroidal case) and its dependence upon distance. Accordingly, Huygens states that the inverse square law does not apply near the Earth and that gravity is constant.

There was, moreover, a third point that Huygens did not accept: Newton builds a huge edifice of mathematical propositions on the foundation of his theory. In doing so, he goes far beyond the domain where there was a reasonable basis for the gravitational theory – that is, in the field of planetary motion. In several of his letters, Huygens shows his dissatisfaction with this feature of Newton's work. Perhaps he thought that Newton was building on sand.

One may easily perceive some general methodological questions behind each of the points of disagreement. Such of these general questions as are not specifically related to any research field, but might be applied to any subject, can be put into the following form:

(1) Is it acceptable to construct a theory that describes effects derived from a cause that cannot be explained?

(2) Is it valid to generalize some results, obtained in a restricted domain, to all bodies, to all distances and to all circumstances?

(3) Is it valid to devote a large amount of work to the derivation of consequences of a theory whose basis is open to question, instead of applying oneself to elucidating the actual basis of the theory?

It seems as though Huygens were warning Newton: you should not do this, you are not following the correct method.

Why should Huygens and Newton be in disagreement about methodology?[24] Was it because Huygens was a Cartesian while Newton disliked the work of Descartes? It is not so simple as that. As we have seen, Huygens was not a blind follower of Descartes. Their methods were not exactly alike. And, as we have already seen, Huygens was certainly willing to change his ideas and accept several of the non-Cartesian features of Newton's work. Huygens was, indeed, a Cartesian – in some respects. However, this is

[23] Leibniz's work was published in 1689: 'Tentamen de motuum coelestium causis', in: G. W. Leibniz, *Mathematische Schriften*, ed. by C. I. Gerhard (Hildesheim, 1971), 6: 144. For Fatio's work, see: Bernard Gagnebin, 'De la cause de la pesanteur', *Notes Rec. Roy. Soc. Lond.*, 1949, 6: 105–60. A history of mechanical explanations of gravitation is presented in W. B. Taylor, 'Kinetic theories of gravitation', *Ann. Rep. Smithsonian Inst.*, 1876: 205–82.

[24] Any general discussion about what is *valid* in science – or, in general terms, any discussion about intrinsic scientific values – is a methodological debate.

not the cause of his attitude – it might be considered a symptom or description of his view, but not its cause. The question that needs to be asked is: Why did Huygens remain committed to the Cartesian method, instead of changing his mind? This question brings us to the general problem of the meaning of a 'scientific revolution'.

The Scientific Revolution of the seventeenth century

The process usually called *the* Scientific Revolution, that had its culmination in the period from Galileo to Newton, was both a revolution of concepts and of methods. The methodological revolution, like the conceptual one, did not stop with Galileo. Huygens, Newton and others developed new ideas and new research methods. The acceptance of both proved to be difficult – and this was particularly true as new methodological types[25] were successively introduced. Even those who took an active part in earlier stages of the revolution found it hard to accept new methodological rules: it proved easier to accept that some idea or theory was wrong than to accept that one could do research in new ways. The causes of this special difficulty were twofold: first, the creators of the Scientific Revolution regarded themselves as the founders of the *true* scientific method (as against the Aristotelian method); second, the scientific method was regarded as *restrictive*, prescribing some ways of doing research as correct and ruling out other ways as illegitimate. This difficulty in regard to methodology seems to be the reason for Huygens's negative reaction to Newton's theory.

What does a methodological revolution mean? It is the introduction of new ways of doing research. Those new ways are not incompatible with older ones – except if the former method is regarded as restrictive. Take as an instance the introduction of mathematical methods in some particular field. Quantitative reasoning is not incompatible with qualitative reasoning – except if one states that some particular field of study is, in principle, not subject to quantitative laws. Otherwise, the quantitative method can be introduced without any problems of compatibility. The same can be said about the introduction of other methodological types – different ways of doing research – such as the introduction of observation into a previously purely speculative field, or the introduction of experimentation where before only observation was used, or the building of special instruments for measurement or observation, and so on.

Galileo's work provides a clear example of the introduction of new methodological types in the early seventeenth century. However, one should not lay such stress upon his contribution to the new scientific method as to imply that the methodological revolution was also completed by him. There may well be as large a distance between Galileo and Newton as that usually recognized as separating the Aristotelians from Galileo.

Let us recall one instance: Galileo tried to prove, in the *Dialogo* (1632), that the

[25] Any description of a scientific method can be decomposed into statements about the intrinsic scientific value of 'elementary' procedures. The 'elementary' valuable scientific procedures are here called 'methodological types'.

rotation of the Earth could have no detectable influence upon the fall of a cannon ball from a tower. He also stated that the rotation of the Earth could not cause motion among bodies that are on its surface, but he did not compute numerical values for the effects of rotation of the Earth. Newton was also to agree that such effects were small. However, instead of dismissing them as negligible, he computed their size, and afterwards tried to observe their consequences.[26] What was he doing? To describe his activity in methodological terms, he was accepting the existence of small influences, computing their effects, predicting new phenomena and looking for them. He did this systematically in his work: he considered the effects of the resistance of air on the motion of the pendulum; he looked for small corrections in planetary motions due to forces between planets and due to the small motion of the Sun. Who else paid so much attention to secondary, perturbing causes, before Newton? This is just one of the new methodological insights that can be ascribed to Newton. Was he aware that this was a new and important step? We do not know. He does not theorize about it. He simply does it.

As to the methodological points of disagreement between Huygens and Newton, it is easy to understand Huygens's cautious, conservative position. Newton's step was a bold one. The lack of an explanation of gravitation did not prevent him proceeding with his work; he made what could be seen at the time as wild generalizations from laws arrived at by induction; and he devoted the greatest mathematical care to the development of detailed consequences of those laws. Huygens had good reason to inquire: How does Newton dare to introduce such new attitudes into Natural Philosophy?

Newton himself perceived (but not soon enough) that he had to present a defence of his method. This seems to be the reason why, from the second edition of the *Principia* onwards, he stated his methodological rules separately and explicitly and introduced the third and fourth of his *Regulae philosophandi*.

Newton's methodological rules

In the second and third editions of the *Principia*, the third book ('The System of the World') begins with a section called 'Regulae philosophandi'. It is a very short section (three pages). It presents four methodological rules.[27] The first and second were already present in the first edition of the *Principia* but were placed among the physical hypotheses of the System of the World. Between the first and the second editions, Newton had decided it was necessary to draw attention to his methodological rules and to introduce the third one (the fourth only appears in the third edition). It seems

[26] See, for instance: Angus Armitage, 'The deviation of falling bodies', *Ann. Sci.*, 1947, 5: 342–51.
[27] See the various versions of the 'Regulae' in the *variorum* edition of Newton's *Principia*: A. Koyré, I. B. Cohen and A. Whitman, *Isaac Newton's Philosophiae Naturalis Principia Mathematica, third edition (1726) with variant readings* (2 vols., Cambridge, 1972), 2: 550–6.

likely that this change is a response to the reaction of Huygens and other natural philosophers to Newton's work. It is easy to show that the content of the new rules does indeed provide an answer to Huygens.

The first and second rules are very short and are followed by just three lines of commentary. They state:[28]

> *Rule 1*: We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.
>
> *Rule 2*: Therefore to the same natural effects we must, as far as possible, assign the same causes.

These rules present a principle of simplicity in natural philosophy, and are methodological counterparts to the old metaphysical assumptions about the simplicity of Nature. They say nothing about induction. Now, the third rule, which first appears in the second edition of the *Principia*, and its latter complement – the fourth rule – can be described as principles for inductive argument:

> *Rule 3*: The qualities of bodies, which admit neither intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.
>
> *Rule 4*: In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions.

According to these rules, if experience has shown that gravitation applies to all bodies within our reach, we may accept that gravitation applies to all bodies in the Universe. And this assumption can be made notwithstanding any hypotheses about possible restrictions to such induction – only experiment can undermine experiment. If one accepts these rules, one cannot reject Newton's extrapolations and his tedious computations of so many consequences of the law of universal gravitation. If Huygens accepted that the planets, the Earth and the Sun do attract one another according to the inverse square law, why did he deny that all pieces of matter have the same property? Evidently, because he does not agree with the third and fourth rules.

These methodological rules, in the form in which they are stated and used by Newton, are indeed new. They establish that induction is independent of causal explanation, independent of theory and of necessary knowledge. They afford the natural philosopher a new freedom to explore the consequences of his assumptions, if these are based upon induction. Nowadays it seems natural that a physicist should be allowed to work for years and to publish many papers upon doubtful (or even wild) premises. It was not so in the seventeenth century. Natural philosophers were looking for the truth, and that truth had to be based on acceptable principles from its very beginning.

[28] We use here Motte's translation of the *Principia*.

Conclusion

One cannot conclude that Newton's proceeding was completely unjustified; but there were methodological problems in his work, and Huygens had good reason to criticize him: Newton was doing something new, deviating from the old standards and introducing 'dangerous' procedures.

We may note that Newton was not doing something incompatible with the old method. He would also have agreed that an explanation of gravitation was desirable.[29] However, he would not have agreed that all work should be suspended until this problem was solved. He would also have agreed that tests of laws obtained by induction are desirable, but not that research should stop and wait until no doubts remained concerning the results of induction. If earlier methodology is regarded as restrictive, Newton was indeed transgressing the rules; but if methodological rules are regarded as statements of non-prohibitive *desiderata*, Newton was not transgressing the rules: he was just adding new *desiderata* to the repertoire of methodological types.[30]

Since Huygens rejected Newton's theory, it seems that in his view the methodological rules were restrictive. This, after all, is the commonest view of scientific method, even now. According to this interpretation, Huygens criticized Newton because he assumed a restrictive interpretation of scientific method and because Newton's work represented a new step in the continuing methodological revolution of the seventeenth century.

[29] As is well known, Newton entertained several different explanations of gravitation at different times. At some time, he accepted Fatio de Duillier's theory. See the 'Draft addition to the Principia' (University Library Cambridge, Add. MS. 4005, fols. 28–9), in: A. Rupert Hall and Marie Boas Hall, *Unpublished Scientific Papers of Isaac Newton* (Cambridge, 1962), 313.

[30] For a systematic study of methodological *desiderata*, see: R. de A. Martins, *Sobre o Papel dos Desiderata na Ciência* (Campinas, 1987).