THE DEVELOPMENT OF THE CONCEPT OF HYPOTHESIS FROM COPERNICUS TO BOYLE AND NEWTON

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Abstract

The period of history from Copernicus to Boyle and Newton covers a time of revolution in scientific method. The development of the concept of a hypothesis is part of this changing scientific method, and there are as many opinions on, and uses of, hypotheses as there are people involved in the revolution. Furthermore, there are many modern studies of these opinions and uses, studies which range from historical description to more philosophical analysis. The purpose of this paper is not to add to this vast literature, but rather to propose a coarse and simple tool by means of which the philosopher can begin to extract a general philosophical understanding of the development of the concept of hypothesis. The proposed strategy is to consider only the purpose with which hypotheses were put forward, and the type of justification which was considered suitable for hypotheses. The appropriateness of this strategy can be assessed by its effectiveness, and this paper is a first attempt to apply the strategy to the concept of hypothesis during the scientific revolution of the seventeenth century. The strategy is used to distinguish between different types of hypothesis, and to detect a general trend in the evolution of the concept. It is hoped that both these features allow a deeper philosophical understanding of the role of hypotheses in seventeenth-century thought.

Introduction

The period of history from Copernicus to Boyle and Newton covers a time of revolution in scientific method. The development of the concept of a hypothesis is part of this changing scientific method, and there are as many opinions on, and uses of, hypotheses as there are people involved in the revolution. Furthermore, there are many modern studies of these opinions and uses, studies which range from historical description to more philosophical analysis. The purpose of this paper is not to add to this vast literature, but rather to propose a coarse and simple tool by manes of which the philosopher can begin to extract a general philosophical understanding of the development of the concept of hypothesis. Applying this strategy to the concepts of hypothesis as employed in physics reveals a variety of importantly distinct concepts. Furthermore, while many of were current at the same time, a general trend can be detected. The strategy clearly highlights the way in which the purpose of hypotheses developed from fictional calculating devices, dominant in astronomy prior to Kepler, to the description of reality, while the justification of hypotheses developed from harmony with authority, through demonstration from an *a priori* metaphysical foundation, to a basis in the phenomena, preferably demonstration from the phenomena.

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Saving the phenomena, and justification by authority

The Aristotelian astronomical system is constructed from basic principles (assumptions) to agree with the phenomena. Harmony and reason are *a priori* guides to the basic principles. Even in Aristotle's time observations were available that are difficult to reconcile with his basic principles, and various mathematical systems were proposed to account for the observed motions of the planets in accordance with Aristotle's system. The two main presuppositions of Ptolemy's *Amalgest* (2nd century AD) are:

> (i) Circular motion with uniform speed is perfect, and therefore suited to the heavenly bodies.
>
> (ii) The Earth is at the centre of the universe, and therefore at the centre of the circular motions of the heavenly bodies.

Ptolemy's system is exceedingly complex. The concept of a hypothesis in astronomy became that of a fiction whose purpose was to provide the correct predictions for the observable phenomena whilst satisfying Aristotle's fundamental principles. Such hypotheses were not thought of as describing the universe; the job of description was still done by the basic Aristotelian system. This concept of a hypothesis was added to two others passed down from the Greeks: (1) a proposition assumed as the starting point of a deductive argument; (2) a conjecture to be tested by its consequences.

Despite Copernicus' professed deep admiration for Ptolemy, he was troubled by one particular feature of his system. In his manuscript *Commentariolus*, circulated many years before the publication of *De Revolutionibus* (1543), Copernicus' motivation is clearly revealed:

> These theories were not adequate unless certain equants were also conceived; it then appeared that a planet moved with uniform velocity neither on its deferent nor about the centre of its epicycle. Hence a system of this sort seemed neither sufficiently absolute nor sufficiently pleasing to the mind.
>
> Having become aware of these defects, I often considered whether there could perhaps be found a more reasonable arrangement of circles, from which every apparent inequality would be derived and in which everything would move uniformly about its proper centre, as the rule of absolute motions requires. After I had addressed myself to this very difficult and almost insoluble problem, the suggestion at length came to me how it could be solved with fewer and much simpler constructions than were formerly used.

Copernicus felt that Ptolemy had sacrificed the perfect circular motion in order to „save the phenomena", and he was unhappy with this. Instead, he chose to sacrifice the assumption that the Earth is at the centre of the universe, preferring to preserve the perfect circular motions. The titles of the early chapters of Book I of *De Revolutionibus* reflect this choice of the basic principles that Copernicus seeks to preserve in his system, at the same time as accounting for the phenomena. Chapter 1 is entitled „That the universe is spherical"; chapter 2, „That the Earth is also spherical"; chapter 4, „That the motion of the heavenly bodies is uniform, circular, and perpetual, or compounded of circular motions."

One purpose of his hypotheses is to fulfil the twin goal of preserving these principles and saving the phenomena. How well he succeeded is not relevant to our purposes here, and is beyond the scope of this paper. We are concerned with the purpose of the hypotheses he introduces and the justification that is considered suitable for such hypotheses.

The most important hypothesis he introduces is that the Sun is at the centre of the universe. This hypothesis was more to Copernicus than simply an attempt to save the phenomena and the

Aristotelian principle of the perfection of uniform circular motion. Copernicus believed that the hypothesis of a heliocentric system described the universe, it was not merely a calculational device (see his letter to Pope Paul III which prefaces *De Revolutionibus*). The physical harmony of a heliocentric system formed part of Copernicus' justification for the heliocentric system. He wrote (1543, Book I, Ch X):

> In the middle of all is the seat of the Sun. For who in this most beautiful of temples would put this lamp in any other or better place than the one from which it can illuminate everything at the same time? Aptly indeed is he named by some the lantern of the universe, by others the mind, by others the ruler... Thus indeed the Sun as if seated on a royal throne governs his household of Stars as they circle around him... We find, then, in this arrangement the marvellous symmetry of the universe...

Thus, in Copernicus we see the beginning of a move away from the concept of a hypothesis as a fiction whose sole purpose is accurate calculation of the phenomena within the confines of the Aristotelian principles noted.

However, 'saving the phenomena' using 'perfect circles' are the driving purpose and ultimate justification of the Copernican hypotheses, as is evident from the fact that, in the end, the centre of motion of the Copernican universe is not the Sun but an abstract point with no physical realisation.

From fiction to description, and the roles of descriptive and physical justification

The explanatory power of the Aristotelian description of the universe was considerably weakened by its divorce from the mathematical devices required to predict the phenomena, and its plausibility was threatened by Tycho Brahe's observations of a comet that appeared to pass through (what had come to be supposed as) solid celestial spheres. Kepler was deeply struck by Copernicus' attempts to describe and explain the world as a heliocentric system. As we have seen, Copernicus used powerful imagery to argue for the natural harmony of placing the Sun, the light of the universe, at the centre.

However, Kepler went beyond Copernicus in his insistence on a physical interpretation for astronomical hypotheses. Kepler was disappointed that on the Copernican system the Sun is not, in fact, at the true centre of the motions, and he sought to rectify this point. He looked for alternative hypotheses which, whilst being truly harmonious and rational in themselves, would (a) put the Sun at the true centre of the universe, and (b) save the phenomena. Thus, in *Mysterium Cosmographium* (1596), Kepler postulated that the heavens are formed from spheres (the perfect shape), and that there are six planets (by an analogy with the Holy Trinity). He used the five Platonic solids to postulate the distances between the spherical orbits. His hypotheses would be justified by their success in achieving their purpose.

However, the systematic observational data gathered by Tycho Brahe led Kepler to reconsider his basic hypotheses of the *Mysterium Cosmographium*, since his results did not agree satisfactorily with Brahe's observations. It is beyond the scope of this paper to discuss Kepler's motivations, how he came to make his truly revolutionary contributions to astronomy and, more generally, the conceptual framework of natural science. In this paper, we concentrate on the purpose and justification of the hypotheses that Kepler offered, and move straight to his approach to hypotheses in his *Astronomia Nova* (1609).

Kepler's insistence on extreme accuracy with respect to the phenomena derived from his emerging belief that the road to certainty lay in the phenomena. But Kepler (1609, p. 67) was also looking for physical explanation:

> in this work I treat all of astronomy by means of physical causes rather than fictional hypotheses.

And the phenomena are not enough to provide the physical account Kepler was looking for. In Part I of *Astronomia Nova*, entitled 'On the relationships of hypotheses', Kepler sets out to demonstrate the geometrical equivalence of the geocentric and heliocentric systems of Ptolemy, Copernicus and Brahe. Physical arguments are required to decide between hypotheses, and these introduce uncertainty. Kepler writes (1609, p. 47).

> [...] as is customary in the physical sciences. I mingle the probable with the necessary and draw a plausible conclusion from the mixture. For since I have mingled celestial physics with astronomy in this work, no one should be surprised at a certain amount of conjecture. That is the nature of physics, of medicine, and of all the sciences which make use of other axioms besides the most certain and evidence of the eyes.

Kepler was greatly influenced by Gilbert's study of magnetism, and between *Mysterium Cosmographicum* and the Astronomia Nova Kepler moved from an animistic conception of the motions of the planets to a mechanical conception, with the Sun as the motor of the universe. This was the physical justification of his hypothesis that the Sun is at the centre of the universe. The speed of the motions of the planets is related to their distance from the Sun, and this is similarly justified by the claim that the Sun is the motive force of the universe, and that its ability to move the planets depends on their distance from the Sun.

Kepler's method was to set up a hypothesis, to see what follows, to check these consequences against observations, and then to modify the hypothesis accordingly or select a new hypothesis. At the end of a long and tortuous search, the perfect Aristotelian harmony of the geometric circle was placed second to considerations of physical harmony and observational agreement: the circle was dropped for the ellipse. In other words, whilst geometric considerations were clearly of great importance, the choice of geometric figure was subordinate to the physical and observational criteria. The purpose of Kepler's hypotheses was to provide an accurate description of the phenomena which was both geometrically and physically harmonious. The justification of the Keplerian system in preference to other purely geometric systems was that it (a) agreed more accurately with the phenomena, and (b) provided a physical explanation of those phenomena.

Like Kepler, Galileo was convinced of the mathematical harmony of the universe, and he also sought to describe the phenomena mathematically and quantitatively. Thus, he introduces his study of motion in his *Third Day of the Dialogues Concerning Two New Sciences* (1638, p153) with the remark that:

> Some superficial observations have been made, as, for instance, that the natural motion of a heavy falling body is continuously accelerated; but to just what extent this acceleration occurs has not yet been announced.

He is famous for his concentration on the phenomena, rather than the causes of the phenomena, in his consideration of the terrestrial motions of bodies. He was insistent that hypotheses are to be tested and justified by observation. However, mathematical and physical considerations play an important role in justifying his abstractions of the 'ideal motions' from those actually observed in the presence of friction, air resistance, and so forth. Like Kepler, Galileo was interested in telling a physical story.

Underlying mechanisms: the problem of justification

Descartes was unimpressed by Galileo, because, unlike Galileo, he was interested in providing a new metaphysical foundation for philosophy. For Descartes, his fundamental metaphysical principles are known to be true *a priori* in virtue of being clear and distinct. His ideal for natural science, expressed in his *Discourse on Method* (1637) and the Author's Letter at the beginning of the French edition of the *Principles of Philosophy* (1647), is that everything should be demonstrated from these principles.

In Part II of the *Principles* Descartes derives from a 'clear and distinct' foundation that the world consists of matter in motion, and his challenge is to explain observable phenomena on the basis of these particles of matter so small that they are unobservable. The problem is (Part III, section 46) that

> we have not been able to determine in a similar way the size of the parts into which this matter is divided, nor at what speed they move, nor what circles they describe.

Therefore, hypotheses are introduced into the system: we need to hypothesise the sizes and movements of the particles and compare the consequences with observation:

> For seeing that these parts could have been regulated by God in an infinity of diverse ways; experience alone should teach us which of all these ways He chose.

Lauden (1981) contrasts this with Galileo's theory of the motions of macro terrestrial objects, where the phenomena that the laws address are there for all to see.

Descartes' system was intended to be descriptive, and so were the hypotheses which he used. Various forms of hypothesis are found in Descartes' writing, and all are descriptive. These include hypotheses of the unobservable causes of observable effects, hypotheses by analogy (such as in the first discourse of his *Dioptrics* where he explains the nature of light), and hypotheses that are idealisations (such as perfectly hard bodies, frictionless planes). His hypotheses are intended as explanatory descriptions. But how were they justified?

Explanation and *a priori* justification

It seems clear that for Descartes himself, the justification of hypotheses lay ultimately in their eventual integration into the demonstrative system built on the *a priori* foundation.

Descartes also suggests that the justification of hypotheses can involve agreement between their consequences and the phenomena. In places, he even claims that false hypotheses are to be 'accepted' if all their consequences agree with the phenomena, thereby further clouding the debate on the status of hypotheses. However, the contextual evidence indicates to me that Descartes was knowingly invoking the prominent conception of a hypothesis as a fiction to protect himself from the Church. Among his contemporaries who propounded the fictional 'save the phenomena' interpretation as opposed to the descriptive interpretation of hypotheses were Mersenne and Gassendi.

In the three essays which accompany the Discourse, it is only the discourse on the rainbow in the Meteors that Descartes claims as an example of his method. Tiemersma (1988) has examined the methodology displayed here, and finds that Descartes used experimental investigation based on hypotheses for discovery, rather than justification of the hypotheses. The purpose of Descartes' hypotheses is to explain various phenomena, such as the colours of the rainbow. The hypotheses are to be justified by their subsequent incorporation in the overall deductive system.

In practice some justificatory reliance on agreement with the phenomena is going to be inevitable due to the problem raised in Part III, 46. There is a tension in Descartes' philosophy between his ideal of justification by demonstration from first principles, and his realisation that the need for hypotheses introduces a reliance on confirmation by the phenomena. Those who were inspired by Descartes and who treated the purpose of hypotheses as explanatory description differ widely in respect of how they balance the two types of justification for hypotheses suggested by Descartes. Some concentrated on speculations concerning the unobservable mechanical causes of the observable effects, encouraged by Descartes' assertion (Part III, 46) that:

> we are now at liberty to assume anything we please, provided that everything we shall deduce from it is in conformity with experience.

Once it became clear that some restrictions on speculation were required, others employed these constraints as requirements for the justification of hypotheses, as we will now see.

The role of the phenomena in justification

The proposed constraints on a good hypothesis included the view that the hypotheses must be more general than the specific effects they were proposed to explain, and that they must fit into the framework of the metaphysical principles; these constraints were turned by the more experimentally inclined into requirements for justification. For example, Huygens (1690, pvi-vii) set out his requirements for the justification of a hypothesis as follows:

> whereas the Geometers prove their Propositions by fixed and incontestable Principles, here the Principles are verified by the conclusions to be drawn from them; the nature of these things not allowing of this being done otherwise. It is always possible to attain thereby to a degree of probability which very often is scarcely less than complete proof. To wit, when things which have been demonstrated by the principles that have been assumed correspond perfectly to the phenomena which experiment has brought under observation; especially when there are a great number of them, and further, principally, when one can imagine and foresee new phenomena which ought to follow from the hypotheses which one employs, and when one finds that therein the fact corresponds to our prevision. But if all these proofs of probability are met with in that which I propose to discuss ... this ought to be very strong confirmation of the success of my inquiry.

Jacques Rohault, an influential later Cartesian physicist, discussed the dangers of over-emphasis on either reason or observation, and endorsed a method of constructing hypotheses using experiment and reason, and testing these hypotheses by comparing their consequences with experimental findings. He writes (1728, Volume 1, p13-14):

> If that which we fix upon, to explain the particular Nature of any Thing, do not account clearly and plainly for every Property of that Thing, or if it be evidently contradicted by any one experiment; then we are to look upon our Conjecture as false; but if it perfectly agrees with all the Properties of the Thing, then we may esteem it well grounded, and it may pass for very probable.

After acknowledging that such a justification cannot render the hypothesis certain because different causes may produce the same effect, Rohault continues:

> And indeed there may be so many, and so very different Properties in the same Thing, that we shall find it very difficult to believe, that they can be explained two different ways. In which Case, our Conjecture is not only to be looked upon as highly probable, but we have Reason to believe it to be the very Truth.

Thus, for the more experimentally inclined Cartesians, if the phenomena can be deduced from the hypothesis then the hypothesis is confirmed.

In short, the purpose of hypotheses used by Descartes and his followers was explanatory description, and the justification of these hypotheses involved a balance between the place of the hypothesis in a system of demonstration based on *a priori* metaphysical principles and the agreement of the observable consequences of the hypotheses with the phenomena.

Experimental usefulness, and testing of hypotheses

Description and experimental testing were the primary purpose and mode of justification of hypotheses for physicists in England, as well as for some on the Continent. The interpretation of Bacon's methodology, and his influence on seventeenth century physics, is controversial. However, I am persuaded that the general features of Bacon's methodology are well represented by Horton (1973). As Horton argues, whilst Bacon's use of the term 'hypothesis' in *Novum Organum* (1620) is generally pejorative, relating to unjustified preconceptions, his use of the term 'axiom' applies to suppositions made on the basis of observation and open to further investigation. We have, then, two distinct and distinguished concepts of a hypothesis: an unjustified preconception, and an empirically grounded supposition. Due to space restrictions, I omit my discussion of Bacon, and simply note that the purpose of the latter conception is to explain the phenomena in terms of underlying mechanisms and to guide further observation and experimentation. Justification depends on agreement, and range of agreement, with the phenomena.

Bacon urged philosophy away from animistic explanations, towards explanations of the phenomena in terms of matter in motion rather than forms and qualities, and Boyle followed him in this approach. Boyle also followed Bacon in placing great importance on the gathering of a wide range of experimental data and using hypotheses to sharpen the power of the experimental method.

As Galileo had distinguished between describing the phenomena and discussing the causes of the phenomena, so Boyle distinguished between describing the phenomena and making hypotheses concerning the underlying mechanisms that might explain the phenomena. For example, in his discussion of the 'spring of the air', he was concerned to show that air has a spring, and to describe some of its effects, but he was not at the same time concerned to decide between potential explanations of that spring. However, he also sought to use the observations of the phenomena to support hypotheses concerning the underlying mechanisms.

Boyle's work was carried out within the framework of what he termed the 'corpuscular hypothesis', which was more general than the Cartesian or atomist mechanical philosophies, and was not regarded as a certain foundation for science. He wrote of its hypothetical status:

> that, which I need to prove, is, not that mechanical principles are the necessary and only things, whereby qualities may be explained, but that probably they will be found sufficient for their explication. (Boyle, 1675)

Hence, as with the more particular hypotheses of science, Boyle sought empirical support for his corpuscular hypothesis. In line with his general view of the justification of hypotheses, he sought a wide and varied range of support from experiments throughout physics and chemistry.

The purpose of a hypothesis was for the explanation of phenomena in terms of underlying mechanism, in order to guide and interpret experiments. Both Boyle and Bacon judged the value of a hypothesis by its utility in the experimental development and enlarging of natural philosophy. And, Sargent (1986) argues, for both Bacon and Boyle, this measure of the utility of a hypothesis is also a guide to how likely it is that the hypothesis is true.

Boyle (1675) writes:

> The use of a hypothesis being to render an intelligible account of the causes of effects, or phenomena proposed, without crossing the laws of nature, or other phenomena: the more numerous, and the more various the particles [i. e., particulars] are, whereof some are explicable by the assigned hypothesis, and some are agreeable to it, or, at least, are not dissonant from it, the more valuable is the hypothesis, and the more likely to be true. For it is much more difficult, to find a hypothesis, that is not true, which will suit with many phenomena, especially if they be of various kinds, than but with few. And for this reason, I have set down among the instances belonging to particular qualities, some such experiments and observations, as we are now speaking of, since, although they be not direct proofs of the preferableness of our doctrine, yet they may serve for confirmation of it.

Thus, a better hypothesis is a hypothesis which serves the above purpose for the widest range of phenomena, and this range of service also provides a criterion of justification for hypotheses. Boyle's view of the criteria for a good hypothesis, the role of hypotheses in experimentation, and the link with the justification of hypotheses, is clearly shown in the following remarks:

> The Requisites of a Good Hypothesis are:
> 1. That it be Intelligible.
> 2. That it Contain nothing Impossible or manifestly False.
> 3. That it Suppose not any thing that is either Unintelligible, Impossible or Absurd.
> 4. That it be Consistent with it self.
> 5. That it be fit and Sufficient to Explicate the Phenomena, especially the Chief.
> 6. That it be at lest Consistent with the rest of the Phenomena it particularly relates to, & do not Contradict any other known Phenomena of Nature, or manifest Physical Truth.
> The Qualities & Conditions of an Excellent Hypothesis are
> 1. That it be not Precarious, but have sufficient Grounds in the nature of the Thing it self, or at lest be well recommended by some Auxiliary Proofs.
> 2. That it be the Simplest of all the Good ones we are able to frame, at lest Containing nothing that is Superfluous or Impertinent.
> 3. That it be the only Hypothesis that Can explicate the Phenomena, or at lest that does explicate them so well.
> 4. That it enable a skilful Naturalist to Foretell Future Phenomena, by their Congruity or incongruity to it: and especially the Events of such Expts, as are partly devised to Examine it; as Things that ought or ought not to be Consequent to it.

In short, the purpose of a hypothesis is to be descriptive and to guide further experimentation to see how well predictions based on the hypothesis are borne out. This process amounts to testing the hypothesis, and so is its justification.

All the problems exposed: hypotheses in Newton's work

Cohen (1966) discusses the variety of concepts of hypothesis that he finds in Newton's work. The types of hypothesis appropriate to physics are all descriptive, but they vary in their additional purposes. For those hypotheses seeking a confirmed status as descriptions of the world, the only type of justification was demonstration from the phenomena. This is a significantly stronger requirement than that of Boyle, and results, as we shall see, from Newton's handling of the distinction between descriptions of the phenomena and explanatory descriptions of underlying mechanisms.

As Koyré, (1965) writes, in the first edition of the Principia (1687) Newton used the term 'hypothesis' to cover all premises from which conclusions were to be deduced: being a hypothesis depended on purpose, not justification. The changes between the first and second edition of the *Principia* reflect the confusion that the different concepts of hypothesis caused in physics at the time. They also reflect Newton's desire to distinguish his system from the hypotheses that he regarded as invalid. In the second edition, Newton uses the term to cover unproven assertions. In other words, the distinction is made on the basis of justification.

Using the purpose and justification to distinguish types of hypothesis yields a categorisation which differs a little from Cohen's. Clearly present in Newton's work are the following five conceptions of a hypothesis.

Firstly, there are his laws, the purpose of which is to describe the phenomena. In early manuscripts, such as 'On the Motion of Spherical Bodies', what are later termed the laws of motion are termed hypotheses. These hypotheses are used as unproved axioms on the basis of which discussions and derivations of phenomena are given. They are later termed laws in virtue of their status as having been proved by the phenomena, that is, demonstrated from the phenomena. Explaining his use of the term hypothesis in the proposed second edition, Newton writes to Cotes (March 28, 1713),

> as in geometry, the word 'hypothesis' is not taken in so large a sense as to include the axioms and postulates; so, in experimental philosophy, it is not to be taken in so large a sense as to include the first principles or axioms, which I call the laws of motion. These principles are deduced from the phenomena and made general by induction, which is the highest evidence that a proposition can have in this philosophy. And the word 'hypothesis' is here used by me to signify only such a proposition as is not a phenomenon nor deduced from any phenomena, but assumed or supposed — without any experimental proof.

Secondly, there are similar individual hypotheses which Newton regards as testable in principle, but which are not proven. There are those which Newton thinks are probable, but which he has so far been unable to prove. And there are those which are not thought to be probable, but which are used for the purposes of an argument, such as simplifying assumptions thought not to affect the outcome of the argument (for example, the hypothesis used in the attack on Cartesian vortices in Book II, Section IX of the *Principia*).

Thirdly, there are general systems, also describing the phenomena, such as the 'Copernican hypothesis', which are also evaluated with respect to the phenomena. The fact that Newton continued to call the Copernican system a hypothesis is evidence that he did not regard it as proven by the phenomena.

Fourthly, there are hypotheses whose descriptive content goes beyond the phenomena by, for example, attempting to describe underlying mechanisms. They are justified by appeal to their consequences for the phenomena. Such hypotheses cannot be justified to the same extent as those that describe only the phenomena, hence Newton's insistence on distinguishing between the two. Newton distinguishes very carefully between discussion of the phenomena, and hypotheses concerning underlying mechanisms, as is made clear by Newton's famous, not to say infamous, 'hypotheses non fingo':

> But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I feign no hypotheses; for whatever is not deduced from the phenomena is to be called a hypothesis; and hypotheses,

> whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction.

Newton's earlier exchanges with Hooke and Huygens on the nature of light are enlightening in this respect. Newton distinguished between the theory he proposed, which was restricted to the phenomena, and any hypotheses concerning underlying mechanism. His theory suggests the corpuscular nature of light, but does not rely on it. This important distinction was misunderstood by Hooke and Huygens, and by many of Newton's contemporaries. Newton replied that it was not his intention to provide such hypotheses.

This type of hypothesis is banned from the *Principia*, but is a fundamental part of Newton's work as a scientist, as is seen from the Opticks. The purpose of the mechanical hypotheses in the Opticks is primarily to aid experimental investigation of the phenomena, and any type of hypothesis may be proposed so long as it is a plausible description and provides a quantitative scale for investigating the phenomena.

At the end of the Opticks (Fourth Edition, 1730), after pages of rich and diverse hypotheses, 'saved' from being hypotheses by being presented in the form of queries, Newton dismisses hypotheses again, and once again the textual evidence is that by hypothesis he meant a proposition that is not answerable to experiment.

He writes (p404):

> As in Mathematicks, so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy.

He goes on to describe the justificatory role of experiments:

> And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phenomena, the Conclusion may be pronounced general. ... By this way of Analysis we may proceed ... from Effects to their Causes ... And the synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phenomena proceeding from them, and proving the Explanations.

Finally, there are those hypotheses that cannot be tested by appeal to the phenomena, and this includes deliberate fictions. Since demonstration from the phenomena is the only type of justification available, such hypotheses cannot be justified, and therefore they are not suitable for science. This type of hypothesis is the type that Newton regarded as one of the main failings of Descartes' approach to science, and that he wanted to dissociate himself from most strongly. Most of Newton's examples of hypotheses in this pejorative sense are taken from Descartes' physics. As Crombie says:

> Newton objects to the explanation of observations by supposing the existence, besides the observed phenomena, of something endowed with qualities or structure specially imagined for the purpose, and beyond the control of experiment.

In his Rule IV of the 'Rules of Reasoning in Philosophy' (Principia, 1713) Newton protects himself against opposing '*a priori* hypotheses', subjecting himself to the test of experiment only.

In the *Principia* Newton also wanted to dissociate himself from the fourth type of hypothesis, concerning underlying mechanisms, because the primary purpose of the Principia was his mechanics of the phenomena, his mathematical description of the phenomena, which was given in terms of hypotheses of the first type only; it was, Newton felt, demonstrated from the phenomena, and therefore not a hypothesis.

For Newton, the justification of a hypothesis lies not in its descriptive explanatory power, but in its demonstration from the phenomena.

Discussion and conclusions

Let us review the general picture that the strategy of concentrating on purpose and justification has brought into sharp relief.

The concept of a hypothesis as a useful fiction justified by appeal to authority was dominant in astronomy from Ptolemy to Copernicus, and continued to be advocated by such seventeenth century physicists as Mersenne and Gassendi. This conception was abused by Descartes in his ambiguous claims for hypotheses as descriptions as well as fictions, and his claim that true descriptions of the phenomena may be arrived at from false descriptions of underlying mechanisms.

It seems plausible that Copernicus inspired in Kepler a desire for hypotheses that described the world, and this purpose for hypotheses became dominant in physics as the seventeenth century progressed. Galileo sought a mathematical description of the behaviour of terrestrial macro-sized bodies. He also continued the move away from the justification of hypotheses by the testimony of authority towards justification by experimental evidence.

Descartes did not restrict himself to attempting to describe the phenomena, addressing himself also to the underlying mechanisms. The purpose of hypotheses was to provide an explanatory description of the mechanisms underlying the phenomena.

These hypotheses raised a new problem of justification, and Descartes himself preferred justification by demonstration from an *a priori* metaphysical foundation. However, there was also the need to appeal to the phenomena for the justification of hypotheses concerning underlying mechanisms. Philosophers in both the Cartesian-inspired tradition, such as Huygens and Rohault, and the empiricist tradition of Bacon and Boyle, sought to support hypotheses concerning underlying mechanism through investigation of the consequences of the hypotheses in the phenomena.

For Bacon and Boyle, the purpose of such hypotheses was in part explanatory description (a candidate for a true description of the underlying mechanism), as it was for Descartes, but it was also as a tool for the experimental investigation of the phenomena.

It was Newton who went back to Galileo's approach of a mathematical description of the phenomena, justified by the phenomena, which was independent of hypotheses concerning underlying mechanism. For Newton, a hypothesis is granted only when it is demonstrated from the phenomena, and this placed hypotheses concerning underlying mechanisms in a distinct class from those concerning the phenomena. The purpose of hypotheses concerning underlying mechanisms was usefulness for the investigation of the phenomena; plausibility was an essential requirement, but (officially) a hypothesis was not a candidate for true description since it could never be justified as such. Boyle was well aware of the hypothetical status of natural philosophy, but he did not seem to worry about it to the extent that Newton did.

In summary, I conclude that the development of the concept of a hypothesis in seventeenth century physics can be summarised in terms of purpose and justification. The purpose developed from fictional calculating device to description of reality, and the justification developed from harmony with authority, through demonstration from an *a priori* metaphysical foundation, to a sound basis in the phenomena, preferably demonstration from the phenomena. The concept of a hypothesis as a fiction was preserved, but became increasingly the subject of derision, eventually provoking Newton's famous outburst, 'Hypotheses non fingo'.

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