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## EXPERIMENTAL VERSUS SPECULATIVE NATURAL PHILOSOPHY

### 1. INTRODUCTION

This chapter discusses an undeservedly neglected distinction in the discussions of method in natural philosophy in early modern England. It is the distinction between experimental and speculative natural philosophy. The chapter makes no attempt to analyse the modes of deployment of this distinction within the method discourse and practice of early modern natural philosophy. Rather it merely seeks to establish its presence, importance and historical development within this discourse. It is evident that the distinction between experimental and speculative natural philosophy was deployed within rhetorical, heuristic and philosophical contexts during the period, but I do not discuss these uses here.<sup>1</sup> It will be enough in this paper to establish its widespread incidence and to trace its development.

A distinction between speculative and experimental natural philosophy is found in many different English writers in the latter half of the seventeenth century. For example, John Dunton's *The Young-Students-Library* (1692), which, as its title suggests, is addressed to a student audience, divides natural philosophy as follows:

Philosophy may be consider'd under these two Heads, Natural and Moral: The first of which, by Reason of the strange Alterations that have been made in it, may be again Subdivided into *Speculative* and *Experimental*.<sup>2</sup>

Very roughly, and a degree of imprecision is important here, speculative natural philosophy is the development of explanations of natural phenomena without prior recourse to systematic observation and experiment. By contrast, experimental natural philosophy involves the collection and ordering of observations and experimental reports with a view to the development of explanations of natural phenomena based on these observations and experiments. Needless to say, it was experimental natural philosophy that was favoured by almost all natural philosophers in early modern England.<sup>3</sup> Indeed, the distinction is normally invoked

<sup>1</sup> For general discussions of method discourse in natural philosophy in the early modern period see: the Introduction to Schuster and Yeo 1986, pp. ix–xxxvii; Schuster 1986; and Dear 1998.

<sup>2</sup> Dunton 1692, p. vi.

<sup>3</sup> Hobbes was a notable exception. See Shapin and Schaffer 1985, chap. 4. For an early critique of this preference for experimental natural philosophy see Margaret Cavendish's *Observations upon*

to affirm, to vindicate and to recommend the experimental methodology or to criticise those who indulged in speculative natural philosophy. Experimental philosophy was definitely ‘in’ and speculative philosophy was ‘out’. Quoting Dunton again,

We must consider, the distinction we have made of *Speculative* and *Experimental*, and, as much as possible, Exclude the first, for an indefatigable and laborious Search into Natural Experiments, they being only the Certain, Sure Method to gather a true Body of Philosophy, for the Antient Way of clapping up an entire building of Sciences, upon pure Contemplation, may make indeed an *Admirable Fabrick*, but the Materials are such as can promise no lasting one.<sup>4</sup>

Now this distinction is easily passed over or dismissed as one peruses early modern writings about natural philosophy. It could simply be just part of the rhetoric of those promoters of Baconian natural philosophy of the kind practised by the early Royal Society. However, I want to argue that not only is it a fundamental distinction in the characterising of natural philosophical method in the latter half of the seventeenth century, but that it is *the* fundamental dichotomy in discussions of natural philosophical methodology during the period. I will claim that these were the defining terms of reference for any practitioner of natural philosophy. In fact, I will argue for five strong claims regarding this distinction:

1. this distinction is in evidence, in some form or other, from the late 1650s until the early decades of the eighteenth century
2. this distinction provides the primary methodological framework within which natural philosophy was interpreted and practised in the late seventeenth century
3. this distinction is independent of disciplinary boundaries within and closely allied to natural philosophy
4. this distinction crystallised in the 1690s when opposition to hypotheses in natural philosophical methodology intensified
5. this distinction provides the terms of reference by which we should interpret Newton’s strictures on the use of hypotheses in natural philosophy.

## 2. THE EXPERIMENTAL/SPECULATIVE DISTINCTION IS IN EVIDENCE, IN SOME FORM OR OTHER, FROM THE LATE 1650S UNTIL THE EARLY DECADES OF THE EIGHTEENTH CENTURY.

The origins of the distinction are not entirely clear. Certainly it is adumbrated in some form by Francis Bacon in *De dignitate et augmentis scientiarum* where he distinguishes between speculative (*speculativa*) and operative (*operativa*) natural

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*Experimental Philosophy* (1666) and in particular the chapter entitled ‘Ancient Learning Ought Not to be Exploded, nor the Experimental Part of Philosophy Preferred Before the Speculative’ (2001, pp. 195–197).

<sup>4</sup> Dunton 1692, pp. vi–vii.

philosophy.<sup>5</sup> The former was further divided into physic (or physics) and metaphysics; physic being founded upon natural history and metaphysics being founded upon both natural history and physic.<sup>6</sup> Furthermore, the third of Bacon's tripartite division of the subjects of natural history, namely Arts, which is the history of artefacts and the manipulation of nature, is called '*Mechanical and Experimental*'. This is regarded by Bacon as the most important and yet the most neglected form of natural history and is naturally seen as an antecedent to the experimental philosophy.<sup>7</sup> But none of Bacon's distinctions is co-extensive with the experimental/speculative distinction as found later in the century, for, at least from the 1650s, it was 'physic' (a synonym for 'natural philosophy') which was divided into the speculative or the experimental. Bacon famously opposed idle speculation and promoted the derivation of natural knowledge from experiment. The idols of the theatre and the critique of the dogmatists who spin webs out of themselves are familiar themes in the *Novum Organum* and are picked up by the promoters of experimental philosophy.<sup>8</sup> (A discussion of Bacon's distinction between speculative and productive philosophers is found in Stephen Gaukroger's chapter.) There was also a distinction between speculative philosophy and other types of philosophy in some scholastic divisions of the sciences in the late Renaissance. Thus, Toletus claims that philosophy has three principal parts: speculative, practical and factive. The speculative part was further to be divided into physics, metaphysics and mathematics.<sup>9</sup>

So there was a precedent for the experimental/speculative terminology and for the making of divisions within natural philosophy. And we find the term 'Experimental Philosophy' used as early as 1635 in Samuel Hartlib's *Ephemerides*.<sup>10</sup> Yet one cannot claim that a definitive division of the science of natural philosophy was bequeathed to the first generation of serious English experimental philosophers in the mid-seventeenth century. It does appear to be adumbrated in the early writings and opinions of William Petty, who seeks to 'explode' the 'meerly phantasticall' and promote experimental learning.<sup>11</sup> But it is clearly not present in the eclectic methodological views of Hartlib. However, whatever its origins, once the distinction between experimental and speculative natural philosophy reached the form in which it is found in the sixth decade of the century, it is not hard to find.

<sup>5</sup> Bacon 1859, IV, p. 343 = *ibid.*, I, p. 547.

<sup>6</sup> 'And so of Natural Philosophy the basis is Natural History; the stage next the basis is Physic; the stage next the vertical point is Metaphysic', Bacon 1859, IV, p. 362.

<sup>7</sup> 'Description of the Intellectual Globe', Bacon 1996, p. 102/3.

<sup>8</sup> See *New Organon*, I, 61–65 and 95, Bacon 1859, IV, pp. 62–66 and 92–93. See for example Glanvill 1668, p. 5 where he denies that the aims of the Royal Society are not 'little Projects of *serv*ing a *Sect*, or *propagating* an *Opinion*; of *spinning* out a *subtile Notion* into a *fine thread*, or forming a *plausible System* of new *Speculations*'. See also Nedham 1665, pp. 234–235.

<sup>9</sup> See Wallace 1988, p. 210.

<sup>10</sup> Hartlib Papers 29/3/37B.

<sup>11</sup> Petty 1647, p. 2.

It is certainly evident in Boyle's methodological writings of the late 1650s and early 1660s.<sup>12</sup> Indeed some of Boyle's recently published manuscript notes for his *Usefulness of Natural Philosophy* furnish us with an explicit link to Bacon. Boyle tells us

I shall ... do what is requisite to commend Experimental Learning to you, if I be so happy as to make it out, that Experiments considered in the Lump, or one with another, may very much assist the speculative Phylosopher, that is sollicitous about the causes and reasons of Naturall things; and that the speculative Phylosopher so assisted, may (on the other side) very much improve the Practical part of Physick. And consequently, that both of them may very happily conspire to the Establishing & Advancement of a Solid usefull Naturall Philosophy.<sup>13</sup>

He then goes on, alluding to *De augmentis* and referring to *Novum Organum*,

before I proceed to handle these things distinctly, I must advertise you, that I forget not that our excellent Verulam has mentioned a *Scala ascensoria & descensoria*; the former from Experiments to Axioms, the latter from Axioms to Experiments, as designed parts of his *Novum Organum* ...<sup>14</sup>

Boyle even composed a work entitled 'Of Usefulness of Speculative & Experimental Philosophy to one another', though this is no longer extant.<sup>15</sup> The distinction is also evident in Boyle's statement of the rationale of his *Sprung of the Air* (1660). He tells us of this work that

It was not my chief Design to establish Theories and Principles, but to devise Experiments, and to enrich the History of Nature with Observations faithfully made and deliver'd; that by these, and the like Contributions made by others, men may in time be furnish'd with a sufficient stock of Experiments to ground *Hypotheses* and *Theorys* on. ... I propos'd my Thoughts but as Conjectures design'd ... to excite the Curiosity of the Ingenious, and afford some hints and assistance to the Disquisitions of the Speculative.<sup>16</sup>

Likewise it is implicit in the preface to Henry Power's *Experimental Philosophy* when he tells us that 'this I am sure of, That without some such Mechanical assistance, our best Philosophers will but prove empty Conjecturalists, and their profoundest Speculations herein, but gloss'd outside Fallacies ...'.<sup>17</sup> And in the preface to Hooke's *Micrographia* (1665) we find

The *real*, the *mechanical*, the experimental Philosophy, which has this advantage over the Philosophy of discourse and disputation, that whereas that chiefly aims at the subtilty of its Deductions and Conclusions, without much regard to the first ground-

<sup>12</sup> See for example Boyle 'Proemial Essay' in *Certain Physiological Essays*, Boyle 1999–2000, 2, pp. 23–25, which was written in 1657.

<sup>13</sup> Boyle 1999–2000, 13, p. 351, underlining added.

<sup>14</sup> *Ibid.*

<sup>15</sup> See Boyle 1999–2000, 14, p. 342.

<sup>16</sup> *Defence against Linus* (1662), Boyle 1999–2000, 3, p. 12, underlining added. See also *Experiments and Considerations touching Colours* (1663), *ibid.*, 4, p. 5.

<sup>17</sup> Power 1664, Preface [c3v]; Power calls Bacon 'that Patriark of Experimental Philosophy', *ibid.*, p. 82.

work, which ought to be well laid on the Sense and Memory; so this intends the right ordering of them all, and the making them serviceable to each other.<sup>18</sup>

We also find it in Sprat's *History of the Royal Society* of 1667.

*Experimental Philosophy* will prevent mens spending the strength of their thoughts about *Disputes*, by turning them to *Works* ... And indeed of the usual titles by which men of business are wont to be distinguish'd, the *Crafty*, the *Formal*, and the *Prudent*; ... The *Formal* man may be compar'd to the meer *Speculative Philosopher*: For he vainly reduces every thing to grave and solemn general *Rules* ... the *Prudent* man is like him who proceeds on a constant and solid cours of *Experiments*.<sup>19</sup>

Indicative of just how widespread was the appreciation of this distinction is the fact that it is found in the literary responses to the new science. In Shadwell's *The Virtuoso*, Sir Formal (the name, of course, alludes to Sprat's 'formal man') sings the praises of Sir Nicholas Gimcrack, who embodies both the speculative and experimental,

Trust me, he is the finest speculative Gentleman in the whole World, and in his Cogitations the most serene Animal alive: Not a Creature so little, but affords him great Curiosities.

Sir Nicholas, when later found emulating a swimming frog, tells us 'I content my self with the Speculative part of Swimming, I care not for the Practick'.<sup>20</sup>

It is also worth citing some critics and opponents of the new natural philosophy. Margaret Cavendish is clearly working with the distinction in her *Observations upon Experimental Philosophy* (1666). In her preface she claims that 'as I have had the courage to argue heretofore with some famous and eminent writers in speculative philosophy; so have I taken upon me in this present work, to make some reflexions also upon some of our modern experimental and dioptrical writers'.<sup>21</sup> Later in the century, John Sergeant, in his *The Method to Science* (1696), sets the problem of the method of science up as follows

The *METHODS* which I pitch upon to examine, shall be of two sorts, viz. that of Speculative, and that of Experimental Philosophers; The Former of which pretend to proceed by *Reason* and *Principles*; the Later by *Induction*; and both of them aim at advancing *Science*.<sup>22</sup>

Finally, the Scottish virtuoso George Sinclair compares his method in hydrostatics with that of Archimedes using what would have been a familiar trope, 'His way is more *Speculative*: this is more *Practical*'.<sup>23</sup>

Now, it would be going too far to say that this distinction is completely ubiquitous, but it is extremely common. The experimental philosophy quickly

<sup>18</sup> Hooke 1665, Preface [a3].

<sup>19</sup> Sprat 1667, p. 341, underlining added. See also p. 257 where Sprat claims the method of the members of the Royal Society 'to be chiefly bent upon the *Operative*, rather than the *Theoretical Philosophy*'. For Sprat on method see Wood 1980.

<sup>20</sup> Shadwell 1997, pp. 9 and 30. Gimcrack is really a composite character displaying all of the features of the virtuosi that Shadwell seeks to ridicule, including a keen interest in experiment.

<sup>21</sup> Cavendish 1666 (2001, p. 10). See also note 3 above.

<sup>22</sup> Sergeant 1696, Preface [b6r-v], underlining added.

<sup>23</sup> Sinclair 1683, Epistle to the Reader.

emerged as, far and away, the dominant form of natural philosophy. Not surprisingly then, the term ‘experimental philosophy’ became the key descriptor for the kind of natural philosophy that members of the Royal Society practised and promoted. It is important to note however, that the term ‘experimental philosophy’ was not co-extensive with ‘natural philosophy’ because natural philosophy could be practised in a speculative way. In fact, one of the consequences of the impact of the distinction was that the term ‘speculative’ in philosophical and natural philosophical contexts often had a pejorative connotation. Thus Henry Oldenburg could tell a correspondent that the Royal Society ‘aimes at the improvement of all usefull Sciences and Arts, not by meer speculations, but by exact and faithfull Observations and Experiments’.<sup>24</sup>

### 3. THE EXPERIMENTAL/SPECULATIVE DISTINCTION PROVIDES THE PRIMARY METHODOLOGICAL FRAMEWORK WITHIN WHICH NATURAL PHILOSOPHY WAS INTERPRETED AND PRACTISED IN THE LATE SEVENTEENTH CENTURY.

The distinction between experimental and speculative natural philosophy provided the terms of reference for virtually all methodological reflection and practice of natural philosophy in England from the late 1650s to the end of the early modern period. Yet we should not simply use the distinction to classify natural philosophers as belonging to one camp or the other without further analysis. For, there is a cluster of epistemological issues that underlie the terms ‘experimental’ and ‘speculative’ and as these are unpacked it becomes clear that there was actually a range of natural philosophical methodologies within experimental natural philosophy, some of which incorporated elements normally attributed to speculative philosophy. It is therefore imperative that we examine that cluster of epistemological issues which were associated with the distinction and use them to shed light on the distinction’s utility and the spectrum of methodologies which fell under this rubric.

The first point to stress as we unpack the distinction is that the epistemology of this period was in a state of flux.<sup>25</sup> Many notions that we now take for granted in the philosophy of science were emerging and some were receiving serious philosophical reflection for the first time. The notions of hypothesis, probability, induction, laws of nature, testimony, experimental replication and so on, were all being discussed and incorporated into accounts of natural philosophical method. Now what is important to stress here, is that this flux of ideas and notions is reflected in a certain vagueness or indeterminacy in the distinction between speculative and experimental natural philosophy. The distinction became a kind of demarcation criterion whose terms were never fully spelt out or clearly defined. Indicative of this is the fact that many natural philosophers—and those who reflected upon natural philosophical

<sup>24</sup> Oldenburg to Norwood, 10 February 1667/8, Oldenburg 1965–1986, IV, p. 168, quoted in Hunter 1989, p. 47.

<sup>25</sup> See B. Shapiro 1983, chap. 2.

methodology—in the latter half of the seventeenth century, upheld methodological precepts which were not always consistent or easily reconcilable. Thus we find the juxtaposition of a stress on Baconian natural histories and an ideal of a demonstrative science of nature with talk of probability, moral certainty. We even find criticism of hypotheses and the deployment of hypotheses in the same writer and sometimes in the very same work.

Indeed, there has been a tendency in some quarters to speak of natural philosophical methodology in this period as if it was constituted by a relatively coherent form of ‘probabilism’ and ‘empiricism’ and as if some practitioners were consciously employing a form of hypothetico-deductive method.<sup>26</sup> However, while the claim cannot be fully substantiated here, it seems rather that the natural philosophical methodologies of this period are better characterised as underdeveloped, tentative and sometimes internally inconsistent. To be sure, there are partial adumbrations of what was to come, but a careful perusal of, say, the methodological writings of Boyle, Hooke or Locke do not yield anything closely resembling modern scientific methodology.<sup>27</sup> This should not surprise us because some of the issues were relatively new (such as laws of nature) and those issues that had a lengthy genealogy in philosophical reflection were often in the process of reinterpretation in the light of the strongly polemical context in which the new philosophy was being forged.<sup>28</sup>

This brings us to a second point, namely that a strong polemical agenda underlies the origins and use of the distinction between speculative and experimental natural philosophy. It was the new natural philosophers, and in particular those aligned with the newly formed Royal Society and its precursor groups, who first used the distinction. They did this not simply to emphasise the fact that they were experimentalists or saw an indispensable need for experimentation, but also to distance themselves from the old speculative way of proceeding in physics or physiology (as natural philosophy was often called). And, of course, the old speculative way was that of the schools; that of the Aristotelians, who indulged in hypothetical and metaphysical speculations which were often untestable or which cluttered the ontological furniture of the world. Such entities as inexplicable occult qualities, substantial forms, virtual extension, sympathies and antipathies were paradigm cases of speculative indulgence in natural philosophy.<sup>29</sup> Thus we find

<sup>26</sup> See for example Laudan 1981, chaps 4 and 5.

<sup>27</sup> For example, Boyle’s manuscript notes on the evaluation of hypotheses (Boyle 1999–2000, 13, pp. 270–272, Boyle 1991, p. 119) need to be assessed in conjunction with the very important exposition of the method of natural history in his letter to Oldenburg of 13 June 1666, Boyle 2001, 3, pp. 170–175. For recent assessments of Hooke’s methodological views see: Lynch 2001, chap. 3; Hunter 2003; and for Locke see Anstey 2002 and 2003b.

<sup>28</sup> There is still no consensus as to the emergence and development of some of these notions in early modern natural philosophy, and, to my knowledge, they have never been interpreted in the light of the experimental/speculative distinction. For discussions of early modern notions of probability and certainty see: B. Shapiro 1983; Patey 1984; Daston 1988 and 1998; and Franklin 2001.

<sup>29</sup> There has been extensive work done on the critique of early modern Aristotelianism(s) and such notions as occult qualities and the theory of forms. See for example Grant 1987; Mercer 1993; Hutchison 1982 and 1991.

Joseph Glanvill's *Plus Ultra* (1668) is written as a defence of the experimental philosophy against the criticisms of an Aristotelian. He tells us that,

the *Modern Experimenters* think, That the *Philosophers* of elder Times, though their *Wits* were excellent, yet the way they took was not like to bring much *advantage* to *Knowledge* or any of the *Uses of humane Life*; being for the most part that of *Notion* and *Dispute*, which still runs round in a *Labyrinth of Talk*, but *advanceth nothing*. And the *unfruitfulness* of those *Methods of Science*, which in so many *Centuries* never brought the World so much *practical, beneficial Knowledge* as would help towards the *Cure of a Cut finger*, is a palpable Argument, That they were *fundamental Mistakes*, and that the *Way* was not *right*.<sup>30</sup>

Nor did this polemical tone diminish as the century wore on and as the experimental philosophy became more entrenched. William Molyneux's dedicatory letter to 'the Illustrious The Royal Society' in his *Dioptrica nova* of 1692 exemplifies this,

I cannot omit expressing my Sense of that excellent Method of Experimental Philosophy, which now, by your Example and Incouragement, does so universally prevail ... 'Tis wonderful to consider, how the Schools were formerly overrun with a senseless kind of Jargon, which they call'd *Philosophy*; ... The Commentators on *Aristotle*, ... have rendred *Physicks* an heap of froathy Disputes, managing the whole Knowledge of Body and Motion ... by Hypothetical Conjectures, confirm'd by plausible Arguments of Wit and Rhetorick, ordered in a Syllogistical form; and answering Objections in like manner: But never studied to prove their Opinions by Experiments.<sup>31</sup>

But the so-called speculative philosophers were not confined to the Aristotelians. Boyle lists Leucippus, Epicurus, Aristotle, Telesius and Campanella as 'speculative Devisers of new Hypotheses'.<sup>32</sup> Furthermore, there was a general sense that the experimental natural philosophy was, in contrast to the philosophy of the ancients, something novel or new. In particular, it was new in the way it emphasised the role of the senses in acquiring knowledge. Naturally this was often emphasised in the context of the deployment of the new instruments such as the telescope and microscope which had opened up new vistas of knowledge by extending human senses. Thus we find Hooke in the preface to *Micrographia* stressing how the members of the Royal Society 'have begun anew to correct all Hypotheses by sense',

And I beg my Reader, to let me take the boldness to assure him, that in this present condition of knowledge, a man so qualified, as I have endeavour'd to be, ... may venture to compare the reality and the usefulness of his services, towards the true

<sup>30</sup> Glanvill 1668, pp. 7–8. Glanvill's earlier defence of the Royal Society, *Scepisis scientifica* (1665), was also motivated by an anti-Aristotelian polemic. See also Boyle's *Excellency of Theology*, Boyle 1999–2000, 8, pp. 87–88 for the claim that, unlike the schools, some of the ancients did practice natural philosophy by experiment and observation. See also Boyle's *The Christian Virtuoso, I*, Boyle 1999–2000, 11, pp. 292 and 296.

<sup>31</sup> Molyneux 1692, dedicatory epistle [A1–3], underlining added.

<sup>32</sup> *Certain Physiological Essays*, Boyle 1999–2000, 2, p. 24.

Philosophy, with those of other men, that are of much stronger, and more acute speculations, that shall not make use of the same method by the Senses.<sup>33</sup>

Third, it is important to note the relation between the experimental/speculative distinction and the widespread emphasis on the construction of Baconian natural histories amongst English natural philosophers in the early decades of the Royal Society. Much has now been written on the importance, indeed the centrality, of natural history to the natural philosophical method of the early Royal Society.<sup>34</sup> Baconian natural histories were vast collections of facts pertaining to particular objects or qualities which were to be gathered by observation, experiment, travelers' reports and any other means. These were to be assembled and arranged by natural philosophers who would then use this data in order to develop explanations of natural phenomena. Much of the work of the Society's experimenters was conceived in terms of the development of natural histories and Henry Oldenburg, the Society's indefatigable first secretary, conceived of his role as intelligencer for the Society in terms of the promotion and realisation of this method.<sup>35</sup> The erection of these histories is quite naturally seen as the organising principle of the task of experiment and observation which constituted the method of the experimental natural philosopher. This is not to say that all English natural philosophers from the period saw the practice of experiment and observation exclusively in these terms. For there was much debate over the relation between the construction of natural histories on the one hand, and hypotheses, principles, causes and induction on the other. But there were very few active natural philosophers in England who did not conceive of their task in terms of the construction of natural histories or whose method does not reflect the influence of natural histories.<sup>36</sup> Nor was this methodological emphasis restricted to England. Oldenburg's championing of Baconianism had an immediate impact on many of his Continental correspondents. And some early members of the Académie Royale conceived of their natural philosophical method in similar terms. Thus the young Christiaan Huygens could say, 'The principal work and most useful occupation of this Assembly should be, in my opinion, to work on the Natural History, somewhat according to the plan of Verulam'.<sup>37</sup>

If natural history was a central component of experimental natural philosophy until the end of the century, all were agreed that hypotheses were the province of the speculative philosopher. And this brings us to the fourth and most important issue associated with the experimental/speculative distinction, namely the epistemic status of hypotheses. 'Hypothesis' in early modern natural philosophy could refer to a causal explanation, a metaphysical principle or maxim, what we would call an inductive generalisation, or even a theory or system of doctrines such as the

<sup>33</sup> Hooke 1665, Preface [b1]. See also Hooke 1661, pp. 41–43 and *idem.* 1705, pp. 4–18; Glanvill 1668, pp. 52–53; and Cavendish 1666, (2001, p. 196).

<sup>34</sup> Hunter 1989 and 2003; Anstey 2002; Findlen 1997; Levine 1983; Cook 1993.

<sup>35</sup> For a sampling see Oldenburg 1964–1986, II, pp. 143–144, 146; III, p. 537; IV, pp. 315, 422 and 451.

<sup>36</sup> While the point cannot be established here, I have argued elsewhere (Anstey 2004) that Newton's use of queries has its roots in the lists of queries and heads for the writing of natural histories.

<sup>37</sup> Quoted from Sabra 1967, p. 171. See *ibid.*, pp. 170–174 for a discussion of Huygens on Bacon's method of natural history.

corpuscular hypothesis or the Copernican hypothesis. The word was also used as a synonym for conjecture, speculation and so on.

The core epistemological issue relating to hypotheses was the extent and manner in which they were related to observation and experiment. It is important to note that observation and experiment were not normally conceived as standing in an evidential relation to hypotheses. Rather, hypotheses were subservient to experience; hypotheses illustrated, explained, were deduced from or shed light on experiments and observations. As Boyle says in his dialogue on ‘The Requisites of a Good Hypothesis’, ‘an hypothesis is a *supposition* ... that men have pitchd upon, or devis’d, as a Principle, by whose help the Phænomeno[n] wherto it is to be applyd may be *explicated*’.<sup>38</sup> It is this inversion of the salient epistemic relation between hypotheses and experience which can seem so foreign to a twenty-first-century reader. A speculative philosopher then, was one who indulged in hypotheses without recourse to observation and experiment at all, or only as an afterthought in order to save the phenomena or in order ‘to adapt them to their Hypothesis’.<sup>39</sup> Speculative philosophers either failed to admit any relation between hypotheses and experience or subordinated experience to the hypothesis at hand.

Now such was the disdain amongst some English natural philosophers for hypotheses of any sort that some practitioners who were honest enough to realise that they actually employed them, admitted this very self-consciously as a kind of confession. Hooke for example, after affirming in his *Micrographia* that the experimental philosophy aimed at ‘avoiding *Dogmatizing*, and the *espousal* of any *Hypothesis* not sufficiently grounded and confirm’d by Experiments’, confesses to the Royal Society that ‘I may seem to condemn my own Course in this Treatise; in which there may perhaps be some *Expressions*, which may seem more *positive* than YOUR Prescriptions will permit’ and that he desires ‘to have them understood only as *Conjectures* and *Quæries* (which YOUR Method does not altogether disallow)’.<sup>40</sup> Others, such as Glanvill and Sprat, who saw a role for hypotheses in experimental philosophy, were acutely aware that this case needed to be argued for and could not be taken as a given.<sup>41</sup> If the core issue was the relation between hypotheses and experience, there was a degree of systematic confusion as to the relation between hypotheses and causal explanations, hypotheses and inductive generalisations and hypotheses and metaphysical first principles. Indeed, one needs to take early modern English discussions of hypotheses, and they are legion, on a case by case basis because the term is not always used consistently, not even by the same author.

<sup>38</sup> Boyle 1999–2000, 13, p. 271. See also Newton to Oldenburg for Pardies, 10 June 1672, Newton 1959–1977, 1, p. 164.

<sup>39</sup> Quoting Hooke from ‘A General Scheme, or Idea of the Present State of Natural Philosophy’, Hooke 1705, p. 4.

<sup>40</sup> Hooke 1665, To the Royal Society [A2v]. Auzout soon took Hooke to task on an example of this and Hooke’s reply is telling. ‘I could wish that this worthy Person had rectified my mistakes, not by speculation, but by experiments’, Oldenburg 1965–1986, II, p. 383. See also Brouncker’s cautionary comment made upon licensing the work, ‘though they [the Royal Society] have licensed it, yet they own no theory, nor will be thought to do so: and that the several hypotheses and theories laid down by him therein, are not delivered as certainties, but as conjectures’, quoted in Hunter 2003 from Birch 1756–1757, I, p. 491.

<sup>41</sup> See for example Sprat 1667, pp. 107 and 257.

Part of the problem with hypotheses was the fact that some natural philosophers maintained the ideal of a demonstrative natural philosophy. Inevitably, as we can now see with hindsight, any form of proto-hypotheticalism in early modern natural philosophy was tied both to the newly emerging probabilism and an acceptance of a central role for inductive reasoning (in the modern sense). This ran counter to the goal of a demonstrative science of nature and, for those committed to such a goal, undermined the epistemological status of hypotheses. For example, this is clearly a problem for Locke in his critical discussions of hypotheses in the *Essay* and elsewhere.<sup>42</sup> Another issue lay in the manner in which many speculative hypotheses were contrived merely to save the phenomena. The underdetermination of hypotheses by observational data came to be regarded by many experimental natural philosophers as a fundamental flaw in the method of hypothesis. Thus we find Newton defending his view of colours in the following terms

For what I shall tell concerning them is not an Hypothesis but most rigid consequence, not conjectured by barely inferring 'tis this because not otherwise or because it satisfies all phaenomena (the Philosophers universall Topick,) but evinced by ye mediation of experiments concluding directly & without any suspicion of doubt.<sup>43</sup>

Now the disdain that some natural philosophers felt for hypotheses led not only to a kind of justificatory pose for those experimentalists who advocated their use in natural philosophy, but also to a whole vocabulary of dismissive and pejorative terms. Hypotheses were castles in the air, mere speculations, fancies, phantasms, chimeras, and so on.<sup>44</sup> Indeed, this kind of invective is even found amongst Continental writers. Thus Huygens, who was a mechanist and upholder of the Cartesian vortex theory, could still claim that ‘Descartes has only spread idle fancies’ and given out ‘conjectures in the guise of truths’.<sup>45</sup>

It is also important to appreciate that while there were experimental natural philosophers who saw the utility of hypothetical reasoning for natural philosophical methodology, there was always a vocal group who opposed their use except under the most stringent of conditions. Sir Robert Moray expressed this outlook when he wrote of the Royal Society that

This Society will not own any Hypothesis, systeme, or doctrine of the principles of Naturall philosophy, proposed, or maintained by any Philosopher Auncient or Moderne, nor the explication of any phaenomenon, where recourse must be had to Originall causes, ... Nor dogmatically define, nor fixe Axiomes of Scientificall things, but will question and canvas all opinions[,] adopting nor adhering to none, till by mature debate & clear arguments, chiefly such as are deduced from legitimate experiments, the truth of such positions be demonstrated invincibly.<sup>46</sup>

<sup>42</sup> See *An Essay concerning Human Understanding*, IV. xii. 13, Locke 1975, p. 648. For further references see below and for a recent analysis of Locke on hypotheses see Anstey 2003b.

<sup>43</sup> Newton 1959–1977, 1, pp. 96–97. See also Cotes’ Preface to the 2<sup>nd</sup> edition of Newton’s *Principia*, Newton 1999, p. 393. For an early expression of this complaint about underdetermination see Childrey 1661, Preface to the Reader [b2v–b3r].

<sup>44</sup> See for example Sydenham 1848, II, p. 173.

<sup>45</sup> Quoting from Westman 1980, p. 98. See Huygens 1888–1950, 10, p. 405.

<sup>46</sup> Transcribed in ‘The early Royal Society and the shape of knowledge’ in Hunter 1995, p. 173.

It should now be clear why we cannot simply take the dichotomy between speculative and experimental natural philosophy and use it to divide up early modern natural philosophers. If we take the dichotomy and group those natural philosophers, commentators and propagandists for the new science, we find a diversity of positions. There were those like Boyle and Sprat who were of a 'reconciling disposition' who conceived of their work within the dichotomy, but who sought to find a role for hypotheses in experimental natural philosophy. Then there were those who like Oldenburg repeatedly emphasised the centrality of natural history to the enterprise and whose stress on history as a foundation for natural philosophy effectively precluded any serious consideration of the epistemological problems associated with incorporating the methodological emphases of 'the other side'. There were those like Hooke who strongly identified with experimental philosophy, but who were conscious that they used speculations and that their causal explanations of phenomena could be branded as speculative. They were normally quite self-conscious in their hypothesising and at times even apologetic. Finally, there were those who regarded the experimental approach as the only legitimate way forward in natural philosophy and who were strongly opposed to speculation and the method of hypothesis. This diversity of 'Baconianisms' is not only evident amongst the members of the early Royal Society,<sup>47</sup> where it was sometimes expressed in a rather schematic and embryonic way, but also amongst its later established practitioners and theorists. What is striking however, is that this range of views is always expressed in terms that are consistent with the fundamental dichotomy of speculative and experimental methods.

#### 4. THE EXPERIMENTAL/SPECULATIVE DISTINCTION IS INDEPENDENT OF DISCIPLINARY BOUNDARIES WITHIN AND CLOSELY ALLIED TO NATURAL PHILOSOPHY.

It is important to emphasise that the distinction between speculative and experimental natural philosophy was not restricted to practitioners of the experimental philosophy or to those who reflected on natural philosophical methodology. It was not the special province of the members of the Royal Society and those within its ambit, nor indeed of the English. But neither was it more commonly found amongst those working in a particular branch of natural philosophy, say, mechanics or astronomy, chymistry or pneumatics, physiology or hydrostatics. It is a distinction which transcended disciplinary boundaries and which was applicable to any form of natural philosophy whatsoever. Thus we find it appears in apologists for the Royal Society such as Samuel Parker. Parker tells us,

The chief reason therefore, why I prefer the Mechanical and Experimental Philosophie before the *Aristotelean*, is not so much because of its so much greater certainty, but because it puts inquisitive men into a method to attain it, whereas the other serves only to obstruct their industry by amusing them with empty and insignificant Notions. And therefore we may rationally expect a greater Improvement of Natural Philosophie from

<sup>47</sup> See Hunter and Wood 1989.

the *Royal Society*, (if they pursue their design) then it has had in all former ages; for they having discarded all particular *Hypotheses*, and wholly addicted themselves to exact Experiments and Observations, they may not only furnish the World with a compleat *History of Nature*, (which is the most useful part of *Physiologie*) but also laye firm and solid foundations to erect *Hypotheses* upon, (though perhaps that must be the work of future Ages:) at least we shall see whether it be possible to frame any certain *Hypotheses* or no, which is the thing I most doubt of, because, though the *Experiments* be exact and certain, yet their Application to any *Hypotheses* is doubtful and uncertain; so that though the Hypothesis may have a firm *Basis* to bottom upon, yet it can be fastned and cemented to it no other way, but by conjecture and uncertaine (though probable) applications, and therefore I doubt not but we must at last rest satisfied with true and exact Histories of Nature for use and practice; and with the handsomest and most probable *Hypotheses* for delight and Ornament.<sup>48</sup>

One cannot help but note the anti-Aristotelian polemic, the reference to natural history, the talk of discarding all ‘particular hypotheses’ and being addicted to observation and experiment. Glanvill’s ‘An Adress to the Royal Society’ in his *Scepsis scientifica* repeats the same themes,

Nor are these all the *advantages* upon the Account of which we owe *acknowledgments* to *Providence* for your *erection*; since from your *promising* and *generous endeavours*, we may hopefully expect a considerable enlargement of the *History of Nature*, without which our *Hypotheses* are but *Dreams* and *Romances*, and our *Science* meer *conjecture* and *opinion*. For while we frame *Scheames* of things without consulting the *Phaenomena*, we do but *build* in the *Air*, and *describe* an *Imaginary World* of our own *making*; that is but little a kin to the *real one* that *God made*. And tis possible that all the *Hypotheses* that yet have been contrived, were built upon too narrow an *inspection* of *things*, and the phasies of the *Universe*. For the advancing *day* of *experimental knowledge* discloseth such *appearances*, as will not lye *even*, in any *model* extant.

Yet Glanvill and Parker are the natural places to look for this kind of experimentalist rhetoric. It is important to note therefore, that from the late 1650s a number of physicians were calling for reforms to medicine that would parallel the changes taking place in natural philosophy. Thomas Willis expressed the wish that the successors of Hippocrates ‘had betaken themselves to Observations only, and Experiments’ for

without doubt the Art of Physick had been advanced to a greater perfection and fineness, and with much more advantage to the sick. But that which presently shut out the light which had been at first set up, and dimmed the eyes of posterity, was the preposterous endeavour of those men, who hastily, and in a manner after their own Phantasie, framed the Art of Physick into a general Method, after the fashion of some Speculative Science.<sup>49</sup>

But what of progressive medical practitioners who had no direct association with the Royal Society and who were not natural philosophers as such? Marchamont Nedham, in his *Medela medicinæ* (1665), explicitly links the need for reform of

<sup>48</sup> Parker 1666, pp. 45–46.

<sup>49</sup> Quoting Nedham’s translation (1665, p. 238) of Willis 1659, *De febribus*, Preface [H3v–H4]. See also Starkey’s *Pyrotechny* (1658) which opens with the question ‘What profit is there of curious speculations, which doe not lead to real experiments? To what end serves Theorie, if not applicable unto practice’, p. 1. See also p. 3.

medicine with reform in natural philosophy, appealing to the authority of Bacon.<sup>50</sup> If we turn to the writings of, say, Thomas Sydenham our familiar themes are also in evidence. For example, in *On Dropsy* Sydenham asserts,

however much hypotheses based upon the speculations of philosophy may be wholly futile — and futile they will be until men become endued with such intuitive knowledge as shall enable them to find foundations for these superstructures — hypotheses directly derived from the facts themselves, and arising from those observations only which are suggested by practical and natural phenomena, are stable and permanent; so much so that, although the practice of medicine to one who looks at the arrangement of writers only, appears as if it arose out of hypotheses, the truer view is that the hypotheses themselves, so far as they are true and genuine, themselves originated in practice ... Had I begun with my hypotheses, I should have shown the same want of wisdom that a builder would show who began with the roof and tiles, and ended with the basement and foundation. But it is only those who build castles in the air [*Aere Castella*] that may begin at either end indifferently.<sup>51</sup>

The familiar characterisation of unfounded hypotheses as ‘castles in the air’, the building metaphor, the reference to the ‘speculations of philosophy’ are all indicative of the experimental/speculative dichotomy. As for natural histories, the construction of histories of diseases was one of Sydenham’s key *desiderata* for medicine and one for which he appealed to the authority of Francis Bacon.<sup>52</sup>

#### 5. THE EXPERIMENTAL/SPECULATIVE DISTINCTION CRYSTALLISED IN THE 1690S WHEN OPPOSITION TO HYPOTHESES IN NATURAL PHILOSOPHICAL METHODOLOGY INTENSIFIED.

Of course Sydenham’s influence on the Molyneux brothers and Locke in these matters is well documented: not least in Locke’s correspondence where we find Thomas Molyneux agreeing with Locke on speculative theories in medicine,

I perfectly agree with you concerning general theories, that they are for the most part but a sort of waking dreams, with which men have warm’d their own heads ... beginning at the wrong end, when men lay the foundation in their own phansies, and then endeavour to sute the Phænomena of diseases, and the cure of them, to those phansies. I wonder that, after the pattern of Dr. Sydenham has set them of a better way, men should return again to that romance way of physick. But I see it is easier and more natural for men to build castles in the air of their own, than to survey well those that are to be found standing [i.e. natural histories of diseases]. ... Upon such grounds as are the establish’d history of diseases hypotheses might with less danger be erected.<sup>53</sup>

In fact, it appears from a close reading of the sources during the 1690s that there was something of a ‘ratcheting up’ of the opposition to hypotheses and speculative methodology. Locke’s correspondence is revealing here as well, particularly with

<sup>50</sup> See Nedham 1665, pp. 234–235. See also Simpson 1669, Preface.

<sup>51</sup> Sydenham 1848, II, p. 173 = Sydenham 1683, pp. 165–166. See also *idem*. 1848, I, p. 14 and ‘De Arte Medica’ (possibly by Locke) in Dewhurst 1966, p. 81 and Royal College of Physicians of London MS 572, fol. 9b, Meynell 1991, p. 17.

<sup>52</sup> Sydenham 1848, I, pp. 12 and 21.

<sup>53</sup> 20 January 1693, Locke 1976–, IV, pp. 628–629. See also William Cole to Locke, *ibid.*, p. 91.

reference to his exchange with William Molyneux over Richard Blackmore's *King Arthur*. Locke writes to William on 15 June 1697,

I have always thought, that laying down, and building upon hypotheses, has been one of the great hindrances of natural knowledge; and I see your notions agree with mine in it. And though I have a great value for Sir R. Blackmore, on several accounts, yet there is nothing has given me a greater esteem of him, than what he says about hypotheses in medicine, in his preface to *K. Arthur*.<sup>54</sup>

Turning to Blackmore's preface we find the following,

the raising of an Hypotheses in Philosophy obtains little more Credit with me, than the erecting a Scheme in Astrology; and the Judgments and Decisions that are given upon them seem to me alike Precarious and uncertain. I was once enamour'd with the *Cartesian System*, but the warmth of my Passion is quite extinguish'd. It may indeed make a Man capable of entertaining and amusing others, but not of quieting and satisfying himself. All Knowledge is valuable according to it's degree of Usefulness, as it do's more or less promote the benefit of Mankind, and for this Reason 'tis a great mortification to consider how little the Pains and Time I have bestow'd in Philosophical Enquirys, have contributed to my knowledge in Curing Diseases. I am now inclin'd to think, that 'tis an Injury to a Man of good sense and natural Sagacity, to be hamper'd with any Hypothesis before he comes to the Practice of Physic. For this prepossession obstructs the Freedom of his Judgement, puts a strong Byass on his Thoughts, and obliges him to make all the Observations that occur to him in his Practise, to comply with, and humour his pre-conceived Opinions; whereas in Reason, his Observations on Nature should be first made, before any Hypotheses should be establish'd. A clear and penetrating Understanding, Cultivated and Matur'd by repeated, Diligent Observation, will in my Opinion, make a more able and accomplish'd Physitian, than any *Philosophical Scheme* that has yet obtain'd in the World.<sup>55</sup>

Note that the Cartesian system is used as an example of a hypothetical system. This is a point to which we will return below. The pertinent issue here, however, is that the occurrence of this opposition to hypotheses in a literary work, albeit in a preface and by a poet and a physician who had come under the influence of Thomas Sydenham,<sup>56</sup> is indicative of just how widespread this phenomenon had become in the 1690s. Not surprisingly therefore, we also find this anti-hypotheticalism in the writings of the theologian and polemicist John Toland who in a very poignant remark claimed, 'since PROBABILITY is not KNOWELG, I banish all HYPOTHESES from my PHILOSOPHY'.<sup>57</sup>

Now the two really explicit statements of the division in natural philosophy between the speculative and the experimental quoted above are also from the 1690s. Dunton's student manual and Sergeant's *Method to Science* may well reflect a

<sup>54</sup> Locke 1976–, VI, p. 144. See also Locke to Thomas Molyneux, 1 November 1692, 'I hope the age has many who will follow his [Sydenham's] example, and by the way of accurate practical observation, as he has so happily begun, enlarge the history of diseases, and improve the art of physick, and not by speculative hypotheses fill the world with useless, tho' pleasing visions', Locke 1976–, IV, p. 563 and John Baron to Locke, *ibid.*, VI, p. 471.

<sup>55</sup> Blackmore 1697, Preface, pp. ix-x.

<sup>56</sup> Dewhurst 1966, p. 49.

<sup>57</sup> Toland 1696, p. 15, quoted in Feingold 1988, p. 305 who wrongly claims that it alludes to Newton's *Principia*. The 'hypotheses non fingo' appeared in the 2<sup>nd</sup> edition of 1713. See also Toland 1696, The Preface, p. viii.

‘suring-up’ or crystallising of the methodological precepts of the new science, such that natural philosophy can now definitively be divided into two quite distinct types. But what is certain is that the discussion of hypotheses in this decade is filled with more invective and ridicule and is far less concessive than in previous decades.

It is difficult to explain the causes of this phenomenon. I have one suggestion as to the underlying process by which the distinction between speculative and experimental natural philosophy became so widely accepted and deployed and by which the anti-hypotheticalism in English natural philosophy strengthened. It is a striking change in the polemical context. As the century progressed Descartes’ natural philosophy, and in particular his vortex theory, (which is discussed in the chapters by Schuster, Dear and Hattab) came to be regarded as the archetypal form of speculative natural philosophy. It was not simply the substantial forms of the scholastics, but Descartes’ threaded screws and whirlpools which became objects of ridicule. And his identification of matter and extension and consequent denial of the possibility of a vacuum had come increasingly to be regarded as metaphysical speculations. One can plot an increasing discomfort with Descartes’ natural philosophy amongst natural philosophers in England from the mid 1660s.<sup>58</sup> Power and Glanvill both seem quite sanguine about Cartesian natural philosophy and methodology in the 1660s. But both Boyle and Hooke, while being influenced by the general cast of Descartes’ mechanism, were critical of particular Cartesian doctrines, either because they were untestable or because they were not founded upon observation and experiment.<sup>59</sup> To be sure, many English natural philosophers continued to speak of the solar system as ‘our vortex’,<sup>60</sup> however, increasingly Descartes’ views were used as examples of speculative natural philosophy. And by the early 1680s the tide had turned against Descartes and the Cartesians.

By 1680 Locke was sceptical of the vortex theory. He comments rather sarcastically in his correspondence on the size of giant hailstones, ‘I doubt whether the Cartesians can have any contrivances to help in this matter, and whether the occult qualities of the Peripatetics may not break under such a load’.<sup>61</sup> More significant though is the shift in Newton’s attitudes. Newton appears to have accepted Cartesian vortices until the early 1680s.<sup>62</sup> However, his ‘De gravitatione et aequipondio fluidorum’, which B. J. T. Dobbs has recently argued was composed in

<sup>58</sup> It should be noted that in the late 1640s William Petty attacked Cartesian natural philosophy on the grounds that it was too speculative and not founded on enough experiments. See his exchange with Henry More, who was quite sanguine about the experimental support that Descartes had rendered for his system. It is interesting to note, however, that Petty’s attack seems not to have had any significant repercussions for the early acceptance of Cartesianism in England and that More’s later rejection of Cartesian mechanism was made on independent grounds. See More to Hartlib 11 Dec 1648, Hartlib Papers, 18/1/38B and William Petty to Hartlib?, *ibid.*, 7/123/1A–2A. For further discussion see Webster 1969 and Gabbey 1982. For Isaac Barrow’s opposition to Descartes and its possible influence on Newton see Gascoigne 1985, p. 409.

<sup>59</sup> See for example Boyle’s *Origin of Forms and Qualities* (1666), Boyle 1999–2000, 5, p. 353 and Hooke 1665, pp. 46 and 54–61.

<sup>60</sup> See Boyle’s *Notion of Nature* 1686, Boyle 1999–2000, 10, p. 508, for a reference to ‘our Vortex’.

<sup>61</sup> Locke 1976–, II, p. 176. See also Keill 1698, pp. 11–18.

<sup>62</sup> See Wilson 2002, p. 206 and especially Dobbs 1991, pp. 122–129.

the mid 1680s,<sup>63</sup> is a strongly anti-Cartesian essay in metaphysics in which Newton attempts to do away with Descartes' fictions (*figmenta*).<sup>64</sup> Newton's attack on Descartes was to culminate in his demolition of the vortex theory in the *Principia*. And by the 1690s the casting of Descartes as the archetypal speculative philosopher was complete. Thus, in Locke's *Second reply to Stillingfleet* (1699) we find a typical example of this criticism of Descartes,

'That Des Cartes, a mathematical man, has been guilty of mistakes in his system.'  
Answ. When mathematical men will build systems upon fancy, and not upon demonstration, they are as liable to mistakes as others. And that Des Cartes was not led into his mistakes by mathematical demonstrations, but for want of them, I think has been demonstrated by some of those mathematicians who seem to be meant here.<sup>65</sup>

Similar criticisms are even found in Huygens who remained a mechanist and advocate of the vortex theory after reading the *Principia*. His notes on Baillet's *Life of Descartes* (1691) describe Cartesian hypotheses as 'conjectures and fictions'. And his reaction on reading the *Principia* was to claim 'vortices destroyed by Newton'.<sup>66</sup>

The point here is that Descartes was a modern. He was a contributor to the new science and a reflector on natural philosophical method, but increasingly he, like the scholastics, came to represent the wrong way to do natural philosophy. Perhaps a compounding factor here was the battle of books which raged in the early 1690s. For, one line of argument in favour of modern learning was that the moderns applied a new experimental method, unlike the ancients who indulged in gross speculation. It is the application of the new method which accounts for the recent gains in natural knowledge. Descartes however was in some respects an awkward exception, for he was in many ways just as guilty of speculative natural philosophy as the ancients. Compounding this was the fact that a steady stream of truly speculative Cartesian cosmological systems was flowing from the presses on the Continent and in England.<sup>67</sup> It was as if Descartes' principles had gone to seed. Very few of these works were related in any way to observation or experiment and many of them were speculative in the extreme. Indeed before long we find other offenders being singled out. Thus William Wotton claimed in his *Reflections upon Ancient and Modern Learning* (1694),

I do not here reckon the several *Hypotheses* of *Des Cartes*, *Gassendi*, or *Hobbes*, as Acquisitions to real Knowledge, since they may only be Chimæra's and amusing Notions, fit to entertain working Heads. I only alledge such Doctrines as are raised upon

<sup>63</sup> Dobbs 1991, pp. 143–146. McGuire (2000, p. 271) finds this redating of 'De gravitatione et aequipondio fluidorum' 'persuasive if not decisive', whereas Stein (2002, p. 303 n. 39) claims that 'uncertainty remains' and Hall rejects it (2002, pp. 412–421)

<sup>64</sup> Published with an English translation in Hall and Hall 1962, pp. 89–156.

<sup>65</sup> Locke 1823, IV, p. 427.

<sup>66</sup> Quoted from Koyré 1965, p. 117. For discussion of the status of hypotheses in French Cartesian natural philosophy from the 1660s see Clarke 1989, especially chap. 5.

<sup>67</sup> See for example Malleme de Messange 1679, Barin 1686, Fontenelle 1686 and Burnet 1681. For attacks on the English cosmogonies see for example Keill 1698 who dismisses Burnet's theory as 'a philosophical romance', p. 26. Of Burnet's *Telluris theoria sacra* (1681) Locke claimed 'I imagine, if I should trouble you with my fancies, I could give you an hypothesis would explain the deluge without half the difficulties, which seem to me to cumber this', Locke to James Tyrrell, 14/24 February 1687?, Locke 1976–, III, p. 140.

faithful Experiments, and nice Observations; and such Consequences as are the immediate Results of, and manifest Corollaries drawn from, these Experiments and Observations.<sup>68</sup>

I present this anti-Cartesianism as only one instance of the kind of forces which led to a heightened anti-hypotheticalism in the late seventeenth century and what was arguably a more rigid characterising of natural philosophical method as either speculative or experimental. It is in this context that we need to evaluate the most notorious of all comments on hypotheses in the early modern period, the ‘hypotheses non fingo’ of Isaac Newton.

#### 6. THE EXPERIMENTAL/SPECULATIVE DISTINCTION PROVIDES THE TERMS OF REFERENCE BY WHICH WE SHOULD INTERPRET NEWTON’S STRICTURES ON THE USE OF HYPOTHESES IN NATURAL PHILOSOPHY.

There are many studies of Newton on hypotheses, but few which deal with the broader context of the acceptance of hypotheses in early modern natural philosophy. For instance, Ernan McMullin asks of Newton’s letter to Oldenburg of 8 July 1672 ‘Why this opposition to hypothesis, which had, by the 1670s, become common coin in natural philosophy?’<sup>69</sup> But apart from noting Newton’s aversion to Cartesian-style speculative hypotheses, he ignores the ‘common coinage’ entirely. Yet a careful perusal of Newton’s many discussions of the place of hypotheses in natural philosophical method clearly reveals that his terms of reference are the experimental/speculative distinction. Perhaps the most explicit statement of this is in a draft of a letter to Roger Cotes from March 1713.

Experimental Philosophy reduces Phænomena to general Rules & looks upon the Rules to be general when they hold generally in Phænomena. It is not enough to object that a contrary phænomenon may happen but to make a legitimate objection, a contrary phenomenon must be actually produced. Hypothetical Philosophy consists in imaginary explications of things & imaginary arguments for or against such explications, or against the arguments of Experimental Philosophers founded upon Induction. The first sort of Philosophy is followed by me, the latter too much by Cartes, Leibniz & some others.<sup>70</sup>

Note the contrast between the experimental and hypothetical (speculative) philosophy and the claim that the latter is practised by Descartes. Note too the claim that the hypothetical philosophy consists of ‘imaginary explications’ and ‘imaginary arguments’. I. B. Cohen has characterised this kind of comment in Newton as his ‘insistence on maintaining a sharp distinction between empirical science and

<sup>68</sup> Wotton 1694, p. 244.

<sup>69</sup> McMullin 1990, p. 69.

<sup>70</sup> Newton 1959–1977, 5, pp. 398–390, underlining added. See also Newton 1715, p. 224 where Newton contrasts his experimental natural philosophy with Leibniz’ in the following terms, ‘The one [Newton] proceeds upon the Evidence arising from Experiments and Phænomena, and stops where such Evidence is wanting; the other [Leibniz] is taken up with Hypotheses, and propounds them, not to be examined by Experiments, but to be believed without Examination’.

philosophy'.<sup>71</sup> But a more nuanced interpretation arises from our foregoing discussion such that we can characterise it as an insistence on the distinction between experimental and speculative natural philosophy and a clear acceptance of the former.

Now, a comprehensive analysis of Newton's discussions of hypotheses in the light of the experimental/speculative distinction is beyond the scope of this chapter, however, we can get a taste for just how illuminating such a study would be by examining some of the claims made about Newton and hypotheses in the secondary literature. We will address claims made by Barbara Shapiro, Larry Laudan, Zev Bechler, George Smith and Mordechai Feingold.

Barbara Shapiro claims '[a] modest yet positive view of hypothesis thus was characteristic of the English scientific community at least until Newton's more critical views were proclaimed'.<sup>72</sup> Larry Laudan makes a similar claim to the effect that in the mid-seventeenth century there was a well-developed 'method of hypothesis' which emerged in the writings of Boyle and Glanvill under the influence of Descartes. However, Laudan regards Descartes' mechanism, as encapsulated in the clock metaphor, as sowing the seeds of the downfall of the 'method of hypothesis'.

The demand for a hypothesis-free science, which was widely circulated after Newton, could never have gathered such enthusiastic adherents if the probabilism of Descartes, Boyle, and Glanvill had not died such a quick and needless death at the hands of those who thought nature's clock had no secrets which man's instruments could not seek out and know with certainty. As it happened the method of hypothesis went into virtual eclipse after 1700 ...<sup>73</sup>

Both Shapiro and Laudan see Newton as derailing the advances made in the method of hypothesis earlier in the century. However, if we take account of the ever-present critique of hypotheses from the 1660s on and the ideal for a demonstrative natural philosophy in writers like Hobbes and Locke, and if we interpret these phenomena in the light of the experimental/speculative dichotomy, a different picture emerges. It becomes clear that rather than Newton causing the derailment of the 'method of hypothesis', his comments on the role of hypotheses in natural philosophy are entirely consistent with one committed to the experimental philosophy and opposed to the speculative philosophy with its conjectures and fancies. Thus, rather than instigating the decline of the 'method of hypothesis', Newton's comments are indicative of the pre-existing terms of reference by which the discipline of natural philosophy was understood. And if Cohen is correct in claiming that Newton's attitude hardened against hypotheses in the 1690s,<sup>74</sup> we should see this as typifying the trend in natural philosophical method of the last decade of the seventeenth century.

Newton made significant changes to the hypotheses in the first edition of the *Principia* in the 1690s when he was in relatively close relations with Locke and

<sup>71</sup> 'A Guide to Newton's *Principia*', Newton 1999, p. 62.

<sup>72</sup> Shapiro 1983, p. 54.

<sup>73</sup> Laudan 1981, p. 48.

<sup>74</sup> Cohen 1966, p. 179. See also McGuire 1970, pp. 28–29.

when, as we have seen, the fortunes of hypotheses in natural philosophy were taking a turn for the worse.<sup>75</sup> His changing of the famous hypotheses in the first edition to the rules of reasoning in the second edition is best interpreted as indicative of the hardening against speculative natural philosophy in this decade and not as a development initiated solely by his own internal ruminations on the nature of natural philosophical method.<sup>76</sup> See, for example, his elaborative comments to Rule III where he says, ‘Certainly idle fancies ought not to be fabricated recklessly against the evidence of experiments’.<sup>77</sup>

Furthermore, if my thesis about the consolidation of the experimental/speculative dichotomy in the 1690s is correct, Newton’s ‘hypotheses non fingo’<sup>78</sup> of 1713 is also better regarded as a response to a generalised and widespread denigrating of speculative natural philosophy, rather than as the cause of it. Thus, when Newton says in the General Scholium that ‘hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy’ we should be aware of the clear connotation of Newton’s first published use of the term ‘experimental philosophy’.<sup>79</sup> Newton here is identifying himself with the experimental philosophy in opposition to the speculative. His terms of reference are identical to those of other natural philosophers we have discussed above. His comments are entirely consistent with anyone who favours the experimental side of the experimental/speculative dichotomy and are consistent with other earlier comments by Newton himself. This is reinforced when we consider that in the third edition he changed the word ‘experimental’ to ‘natural’ in the General Scholium in his comment that ‘to treat of God from phenomena is certainly a part of experimental philosophy’. For Newton there was experimental philosophy and there was natural philosophy and the two were not co-extensive. For Newton, as for Locke,<sup>80</sup> the study of God pertained to natural philosophy, but not to experimental philosophy.

Indeed the whole General Scholium is something of a declaration of Newton’s commitment to the experimental philosophy and opposition to speculative philosophy. As such he begins by recounting his rejection of the archetypal

<sup>75</sup> See Smith 2002, p. 161.

<sup>76</sup> For the details of Newton’s changes see Cohen 1966.

<sup>77</sup> Newton 1999, p. 795.

<sup>78</sup> It is perhaps worth pointing out the Baconian origin of the notion of ‘feigning from nature’. Bacon uses the expression *neque enim fingendum, aut excogitandum, sed inveniendum, quid Natura faciat aut ferat* in the *Novum Organum*, Bk II, Aphorism X (Bacon 1859, I, p. 236) which appears on the title pages of Boyle’s *Colours* and *Cold*, works with which Newton was familiar from the mid-1660s.

<sup>79</sup> This has recently been stressed by Alan Shapiro who has found no use of the term by Newton before a draft of Query 23 for the Latin translation of the *Opticks* in 1706: Shapiro 2004, pp. 186–189. We should also be aware that Newton is using the term ‘hypothesis’ in a very specific sense to mean a proposition that is independent of observation or experiment. See for example Newton to Cotes, 28 March 1713, ‘the word Hypothesis is here used by me to signify only such a Proposition as is not a Phænomenon nor deduced from any Phænomena but assumed or supposed without any experimental proof’, Newton 1959–1977, 5, p. 397.

<sup>80</sup> ‘The end of this [natural philosophy], is bare speculative Truth, and whatsoever can afford the Mind of Man any such, falls under this branch, whether it be God himself, Angels, Spirits, Bodies ...’, *An Essay concerning Human Understanding*, IV. xxi. 2, Locke 1975, p. 720.

speculative system, Descartes' 'hypothesis of vortices' and ends in the penultimate paragraph with the famous claim that 'I do not feign hypotheses'.<sup>81</sup> This scholium is best interpreted as the culmination of a long struggle that Newton engaged in as he attempted to situate and understand his own natural philosophical methodology in relation to the distinction between experimental and speculative methods, a struggle in evidence as early as his first reports on the properties of light in 1672.

An awareness of the distinction between speculative and experimental natural philosophy also enables us slightly to recast Zev Bechler's interpretation of the controversy in the early 1670s between Newton, Hooke and Huygens over Newton's optical experiments.<sup>82</sup> Bechler finds the tension between Newton and the others as resulting from his inability to appreciate the fallibilism of Hooke and Huygens and his uncompromising commitment to a deductive, even dogmatic, approach to his experiments and their analysis. Bechler sees this as the first such incident which marked the ushering in of a 'period of the blind spot'; a period in which natural philosophers were unable to see the efficacy of hypotheses or the way of hypothesis. However, if we interpret the exchanges between Newton and Hooke and Newton and Huygens in the light of the distinction between speculative and experimental philosophy, it is clear that Newton saw himself very much as an experimental philosopher and that dogmatism (of which he is accused by Bechler) was the province of the speculative natural philosopher, as were hypotheses. Little wonder that he took umbrage at Pardies' description of his view of colour as an hypothesis.<sup>83</sup> We need also to fine-tune George Smith's recent claim that 'From the beginning of his work in optics in the 1660s, Newton had always distrusted the hypothetico-deductive approach, arguing that too many disparate hypotheses can be compatible with the same observations'.<sup>84</sup> Rather, it is better to say that Newton distrusted speculative natural philosophy, for it is clear that what this amounted to is not what we call the hypothetico-deductive method. That cluster of epistemological issues in natural philosophical methodology surrounding probability, induction and hypotheses had not yet congealed into what we might, somewhat anachronistically, call the hypothetico-deductive method.

What Newton was unequivocally committed to was the experimental philosophy, with its rejection of dogmatism and speculation.<sup>85</sup> Clearly he was not partial to the sort of ill-formed probabilism which was to be found amongst a number of the members of the Royal Society and which was to find its clearest articulation in Huygens' preface to the *Treatise on Light* and *Discourse on the Cause of Gravity*.

<sup>81</sup> The anti-speculative/Cartesian polemic is also present in Cotes' Preface to the 2<sup>nd</sup> edition of the *Principia*. See Newton 1999, p. 393 and Newton 1959–1977, 5, p. 391.

<sup>82</sup> Bechler 1974.

<sup>83</sup> See the exchange between Newton and Pardies, mediated by Oldenburg, that was initiated by Pardies' letter to Oldenburg of 30 March 1672, Newton 1959–1977, 1, pp. 130ff.

<sup>84</sup> Smith 2002, p. 154.

<sup>85</sup> Newton wrote anonymously in 1715 'The Philosophy which Mr. *Newton* ... has pursued is Experimental; and it is not the Business of Experimental Philosophy to teach the Causes of things any further than they can be proved by Experiments. We are not to fill this Philosophy with Opinions which cannot be proved by Phenomena. In this Philosophy Hypotheses have no place, unless as Conjectures or Questions proposed to be examined by Experiments', Newton 1715, p. 222.

Yet, as we can see with hindsight, a coherent scientific methodology would have to embrace some form of hypothetico-deductive or retroductive inference. The problem was that Newton's strengths and successes lay in the realm of mathematical natural philosophy that most closely approximated the demonstrative ideal of Bacon, and that others committed to the experimental natural philosophy, such as Locke, believed to be at least a possibility in the light of Newton's achievements.<sup>86</sup>

This brings us to the recent claims of Mordechai Feingold in his discussion of some of the internal tensions between members of the Royal Society over the question of correct method in natural philosophy. Feingold argues that Newton's assertion about the certainty of his theory of colour in his 6 February 1671/2 letter to Oldenburg was a 'bombshell' and that Hooke took the comments as 'disparaging of the naturalist and experimental tradition of the Society'.<sup>87</sup> Furthermore, Feingold claims that Newton withdrew from engagement with the Society after his early optical controversies because 'he refused to abide by its moratorium on theoretical pronouncements'.<sup>88</sup> He finds an 'incessant rebuke of theory' amongst the Baconians of the early Royal Society and he claims that the emphasis on the primacy of mathematics in natural philosophy as found in the Newtonian party in the Royal Society from the 1680s on led not only to the deprecation of natural history but of the experimental philosophy itself.

However, if we regard the distinction between speculative and experimental natural philosophy as the backdrop to the controversies which Feingold discusses, I suggest that we can arrive at a more nuanced interpretation of them. For this backdrop enables us to see that the opposition between promoters of natural history and mathematical natural philosophers is a conflict internal to the experimental philosophy itself. To be sure, Oldenburg may have found Newton's assertion of certainty too dogmatic or presumptuous for publication in the *Philosophical Transactions*, but part of Newton's motive in being so forthright was to reason from his optical experiments without reverting to hypotheses, a motive with which Oldenburg could concur. If we take the railing against hypotheses, so prevalent from the 1660s, as a central facet of the stance against speculative natural philosophy (and remember Newton himself was one of the most severe critics of speculative hypotheses), we can see that there was no moratorium on theorising *per se*, but on the empty conjectures of the speculative natural philosophers. Nor was there any deprecation of the experimental philosophy.<sup>89</sup>

<sup>86</sup> See for example, Locke 1975, IV. ii. 9–13, pp. 534–536; IV. vii. 11, pp. 598–603; Locke 1989, p. 248; and Locke 1823, IV, p. 427.

<sup>87</sup> Feingold 2001, p. 83.

<sup>88</sup> *Ibid.*, p. 84.

<sup>89</sup> William Wotton draws a distinction between hypotheses and theories in his *Reflections*, claiming that theories are raised upon experiment and observation whereas hypotheses are not. Now, this distinction in Wotton was his own and is not a widely held view, moreover, it does emerge some three decades after the founding of the Royal Society. However, it is illustrative of the point that there was certainly no moratorium on theorising but rather on speculative hypothesising. See Wotton 1694, pp. 235 and 244. Hooke had claimed in his *Micrographia* that axioms and theories are to be raised upon natural histories, Preface [b2]. While Glanvill claimed that the members of the Royal Society 'continually declare against the *establishment of theories*, and *Speculative Doctrines*' (1668, p. 89) it is clear from

Furthermore, the denigration of the method of natural history, which gathered force in the 1680s, should not be identified with, nor should it be thought to entail, a rejection of the experimental philosophy. For the method of natural history was not constitutive of experimental philosophy, but rather it was the most prominent early manifestation of it.<sup>90</sup> The fact that the method of natural history, in a sense, lost its way at the same time as an appreciation of Newton's achievement in mathematical natural philosophy was dawning on the natural philosophical community, may well have precipitated its decline. The sentiment against natural history is summed up in the comments of Steele in *The Spectator* of 24 March 1711, when he speaks of physicians who, for want of better things to do, only

amuse themselves with the stifling of Cats in an Air Pump, cutting up Dogs alive, or impaling of Insects upon the point of a Needle for Microscopical Observations; besides those that are gathering weeds, and the Chase of Butterflies: Not to mention the Cockle-shell-Merchants and Spider-Catchers.<sup>91</sup>

Interestingly too, this rivalry issued in a rather ironic twist in the deployment of the experimental/speculative distinction in the early eighteenth century. For, some of the defenders of the method of natural history began to accuse the emulators of Newton's methodology of indulging in speculation themselves. In particular this accusation was levelled against the mathematisers of medicine such as Archibald Pitcairne.<sup>92</sup>

## 7. CONCLUSION

It is clear then that the experimental/speculative distinction is an important way of demarcating different approaches to method in English natural philosophy in the latter half of the seventeenth century. It functioned as a kind of general methodological rubric from the late 1650s until the early decades of the following century and was deeply ingrained in the methodological discourse of many practitioners, promoters and even critics of the new science. In fact, it even transcended disciplinary boundaries in so far as it also impacted upon the medical methodology of Sydenham who was quickly to become an exemplar of a more Hippocratic approach to medical practice. As natural philosophers became disillusioned with speculative systems such as the Cartesian vortex theory in the final decades of the century, the critical attitude towards hypotheses hardened and in the 1690s the experimental/speculative distinction appears to have become more firmly entrenched. This is reflected in the writings of Newton, whose changes to the

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the context that he means speculative theories, for immediately preceding this comment he speaks of the need to raise axioms from experiments, *ibid.*, p. 87.

<sup>90</sup> Thus there is no need to follow Feingold (2001, p. 85 and p. 100 n. 18) who implies that William Molyneux privately criticised the experimental philosophy when he criticised the method of natural history but in public 'was a bit more circumspect'. Molyneux's endorsement of the experimental philosophy, including natural history, as we have seen, was unequivocal. See also W. Molyneux to Locke, 27 May 1697, Locke 1976–, VI, p. 134; and Molyneux 1686, Epistle Dedicatory.

<sup>91</sup> Steele and Addison 1888, p. 37.

<sup>92</sup> See Feingold 2001, pp. 88–90.

hypotheses of the *Principia* in this decade are indicative of the broader intellectual climate as reflected in the writings of theologians, poets and philosophers alike.

Now it may be tempting to view this experimental/speculative distinction as equivalent to the modern distinction between rationalism and empiricism. After all, the experimental philosophy emphasised the importance of the senses, constantly appealing to observation and experiment, and it decried the use of mere reason in generating hypotheses. However, while these are certainly tenets of empiricism, there are also marked discontinuities between the two. The wariness of, and at times outright opposition to, hypotheses as well as the preference of some for a demonstrative science of natural philosophy are features of early modern methodologies which are foreign to modern empiricist theories of knowledge. Some might still desire to foist the nineteenth century *historiographical* categories of rationalism and empiricism on the broad spectrum of approaches to natural philosophical knowledge in this period, but to my mind the early modern *historical* categories of speculative and experimental philosophy are more effective terms of reference for interpreting the diverse range of discussions of method in the period. These terms ‘save the phenomena’ of our historical data in a manner that is far more satisfactory than the ‘fancies’ of nineteenth and twentieth century historiographers. Indeed it may be that the very origins of the categories rationalism and empiricism are to be found in the philosophical deployment of this unduly neglected distinction.<sup>93</sup>

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