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Dortous de Mairan and Eighteenth Century "Systems Theory"[1]

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Summary

Jean-Jacques Dortous de Mairan (1678–1771) was accused by d'Alembert of attempting to rehabilitate abstract metaphysical systems. However, Mairan makes clear in his preface to the *Dissertation sur la glace* (1749) that he distinguishes between good and bad systems. In this he takes a position which differs little from that of Condillac, whom d'Alembert so much admired. An examination of his published works and unpublished correspondence corroborates this and reveals a sophisticated and in some ways modern conception of scientific method. It seems likely that Mairan's pronouncements on systems were received with suspicion because of his reputation as a stubborn Cartesian.

Until comparatively recently, the reputation of Jean-Jacques Dortous de Mairan, Fontenelle's successor as Secrétaire Perpétuel of the Académie Royale des Sciences, was that of a stubbornly loyal Cartesian in a triumphantly Newtonian world. Recent work has modified this judgement considerably.[2]

1 I am using the modern term "Systems Theory" to apply to the eighteenth century debate about the importance for scientific method of the difference between *l'esprit systématique* and *l'esprit de système.* The word "system" was used in eighteenth century science to denote a wide variety of conceptual models to explain phenomena. See note 35.
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2 See J. Morton Briggs Jr., "Aurora and Enlightenment", *Isis* 58 (1967) 491–503; Daniel Roche, "Un savant et sa bibliothèque au dix-huitième siècle", *Dix-huitième siècle* 1 (1969) 47–88; Abby Rose Kleinbaum, "Jean-Jacques Dortous de Mairan (1678–1771): a Study of an Enlightenment Scientist" (Ph.D. dissertation, Columbia University, 1970); Henry Guerlac,

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Indeed, an examination of his published works, his contributions to the *Mémoires de l'Académie des Sciences* and his unpublished correspondence reveals a life-long fascination with Newton, and a predilection for what he thought of as the Newtonian scientific method.[3] It is in the context of this process of re-evaluation that we must assess the criticisms levelled at him by his contemporaries, and in particular by d'Alembert, for what they saw as his attempt to rehabilitate abstract systems.

What Mairan stood accused of was not distinguishing between *l'esprit systématique* and *l'esprit de système.* According to T. L. Hankins in his study of d'Alembert published in 1970:

> "Part of the 'confusion' which d'Alembert saw in Dortous de Mairan's work was his failure to differentiate between the *esprit de système* and the *esprit systématique.* D'Alembert carefully distinguished between them ... Dortous de Mairan tended to use the terms interchangeably."[4]

However, since the publication of Hankins' study, much work has been done on re-assessing Mairan's contribution to the development of eighteenth century science. It seems timely, therefore, to re-examine Mairan's position on the usefulness of systems to determine the nature and extent of his "confusion".

His preoccupation with method, which he shared with many of his contemporaries, can be seen in his early published works. He concludes, for example, in his *Mémoire sur la cause générale du froid en hiver et de la chaleur en été* (1719) that the method most likely to induce Nature to reveal her secrets is the constant interplay of experiment and reasoning. In his *Suite des recherches physico-mathématiques sur la réflexion des corps* in the *Mémoires ... 1723* (Paris, 1725), he contends that sometimes experiment, important though it is, is unable to verify demonstrated truths. In the *Avertissement* to his *Traité physique et historique de l'aurore boréale, Suite des mémoires de l'Académie des Sciences ... 1731* (Paris, 1733), he argues that we would miss certain phenomena if we were not alerted to their existence in advance.

His unpublished correspondence with Geneva scientists also reveals his abiding interest in scientific method. From his early correspondence with Fir-

"Some Areas for further Newtonian Studies", *History of Science* 13 (1975) 233–250; Guerlac, "The Newtonianism of Dortous de Mairan", in *Essays on the Age of Enlightenment in Honor or Ira O. Wade*, ed. J. Macary (Geneva, 1977) 131–141; Ellen McNiven Hine, "Dortous de Mairan, the 'Cartonian'", *Studies on Voltaire and the Eighteenth Century* 266 (1989) 163–179.

3 See Hine, pp. 172–176 Also, Mary Hamer, *Signs of Cleopatra* (London, 1993) 48:
"It was Newton's experimental method and the truth claims he made for it that historians of science now identify as his most crucial contribution to the development of Western thought."

4 Hankins, *Jean d'Alembert: Science and the Enlightenment* (Oxford, 1970) 80.

min Abauzit, for example, where he and Abauzit discuss the meaning of the word "metaphysics" in connection with Fontenelle's *Eléments métaphysiques et mathématiques de la géométrie de l'infini,* to his praise for the kind of "conjectures" that Jallabert utilizes in his description of electrical experiments, the fascination with methodological questions never lags.[5] In his letter of October 10th, 1718, he cautions Abauzit against taking the word "metaphysics" too literally.[6] He insists that Fontenelle conceives of the infinite only as mathematicians understand it, without entering into any philosophical discussion of it. In Mairan's opinion, Jallabert's conjectures on the cause of electricity are the kind of "system" which contributes the most to the advancement of the sciences.[7] Those who wish to banish the search for first causes from physics, Mairan believes, miss the mark just as much as those who are bent upon creating systems. One extreme is as bad as the other, and scientists have reason to be grateful to those who apply wise restrictions to the formation of systems, as does Jallabert in his treatise on the cause of electricity.

One of the books sent to Mairan by Jallabert shortly after its publication in 1744 and discussed in their correspondence was de Sauvages' translation of Stephen Hales' *Haemastaticks*.[8] In the author's preface, Hales avers that physics cannot be deduced from purely theoretical speculations or principles and that, like the mathematicians, we can only reason with passable certainty on given truths deduced from the evidence of numerous properly conducted experiments (vi). However, he argues, it is not unreasonable to venture beyond the evidence of observed facts since, from the outer edges of what we know, a kind of twilight lights up the areas that we do not yet know. Without this intellectual daring, continues Hales, progress would be slow, since new discoveries are often the result of bold conjectures and serendipitous flights

5 Jallabert had published his *Expériences sur l'électricité* in the same year as Mairan's *Dissertation sur la glace* (1749), outlining in the *avertissement* his method. He would describe electrical phenomena, which would then be arranged in an order that would facilitate the deduction of consequences. He argued that only from consequences can we proceed to causes, eventually arriving at a theory (iii–iv).
Such methodological questions continue to fascinate scientists. See, for example, Geoffrey Cantor's discussion of the role of experiment in "The Rhetoric of Experiment" in D. Gooding, T. Pinch, S. Schaffer, eds., *The Uses of Experiment* (Cambridge, 1989) 161, or D. Gooding, *Experiment and the Making of Meaning* (Dordrecht, Boston, London, 1990) 250 on the contributions of experiment to theoretical developments.

6 See Mairan à Abauzit, October 10, 1718 (Bibliothèque Publique et Universitaire de Genève, or BPU, ms. fr. 612, f. 83).

7 See Mairan to Jallabert, May 8, 1748 (BPU, SH 242, f. 105).

8 The volume of Hales' work entitled *Statical Essays: containing Haemastaticks* was translated by de Sauvages as *Haemastatique, ou la statique des animaux: expériences hydrauliques faites sur des animaux vivans* (Geneva, 1744), while the volume devoted to plants entitled *Statical Essays: containing Vegetable Staticks* was translated in 1735 by Buffon.

of the imagination. Even error in our initial tentative attempts can lead to the *experimentum crucis* that in turn may lead to an important discovery (vii). This is how progress in physics is made. Mairan no doubt read with great interest this description of Hales' understanding of scientific method which so closely resembled his own.

Like Jallabert, Mairan was a committed experimenter, whose letters are full of detailed descriptions of experiments that he himself had devised and performed, and also of those of other scientists which he had repeated.[9] An attentive reader of Newton, Mairan was in no doubt about the value of the empirical method.[10] However, his refusal to condemn all systems as chimera and his reluctance to reject all hypotheses as fraught with danger were viewed by such as d'Alembert with suspicion. Yet Condillac himself, whose ideas greatly influenced d'Alembert, makes the distinction between good and bad systems, as we see from the title of his work, *Traité des systèmes, où l'on en démêle les inconvénients et les avantages.*[11]

There is, then, as we have seen, ample evidence in Mairan's works and correspondence of his preoccupation with methodological questions. It is, however, the preface which he added to the 1749 edition of his *Dissertation sur la glace,* which serves as a mini *Traité des systèmes,* and which aroused the ire of d'Alembert. Mairan had read it in the public assembly of the Académie des Sciences in November 1748.[12] D'Alembert knew that Mairan had praised systems in this address, which prompted his comment in the *Discours préliminaire* of the *Encyclopédie* that times had changed and that it was too late for anyone to speak up in favour of systems.[13] In d'Alembert's opinion,

9 On Mairan's attempts to repeat Newton's optical experiments see A. Rupert Hall, "Newton in France; a New View", *History of Science* 13 (1975) 243.

10 An early example of his faith in the role of experiment and observation in the process of verification can be found in his *Instruction abrégée et méthode pour le jaugeage des navires, Mémoires ... année 1724* (Paris, 1726), in which he recounts how in 1723 he went to test several methods in the ports of Bordeaux and Agde, amassing information which confirmed him in his judgement, and returning to Paris to make his report to the Académie (p. 228).

11 Hankins, *Op. cit.* (n. 4) 78, documents "the close association between Condillac and d'Alembert".

12 The dissertation had won the prize of the Académie Royale des Belles-Lettres, Sciences et Arts of Bordeaux in 1716. It had been reprinted in Béziers in 1717 and in Paris in 1730.

13 "Discours préliminaire", *Encyclopédie, ou Dictionnaire raisonné des sciences, des arts et des métiers* (Paris, 1751) I: xxxi. He charged that a fondness for systems had as much place in physics as metaphysics had in mathematics, that hypotheses had to be rejected if they could not be verified mathematically, and that the principal merit of the scientist would be to cultivate *l'esprit de système* (by which he meant *l'esprit systématique*).
In a letter to Gabriel Cramer, Professor of mathematics at Geneva, dated September 21, 1749, d'Alembert wrote (BPU, ms. Supp. 384, f. 189):

> "Mes réflexions sur les systèmes ont été occasionnées par un ouvrage, lu à notre dernière assemblée publique, ouvrage qui paroîtra bientôt, et dont il me semble que l'auteur confond mal à propos les avantages réels de l'esprit de système, avec les avantages fort équi-

Mairan had confirmed his reputation as "yesterday's man" by undertaking the defence of systems at a time when the metaphysical systems of the preceding century were in the process of sustaining a telling blow from Condillac in the *Traité des systèmes,* published in the same year as Mairan's preface.[14]

The latter is divided into three parts in which Mairan discusses the dangers that systems present (iii–x), their usefulness for scientific progress (xi–xvi), and the hypothesis of subtle matter on which he bases his discussion of ice and which he compares to Newton's use of the term in the *Opticks* (xvii–xxix). He begins by admitting that if he were tackling his dissertation for the first time, he would not present it in the form of a systematic treatise "qui suppose tant de connoissances qui nous manquent, ou que nous n'avons qu'imparfaitement". Rather, he would confine himself to facts, observations and experiments, resorting to hypotheses only incidentally, in the process of induction and conjecture (iv–v).[15] In the light of this statement, it is not surprising that the *Journal des sçavans,* in its analysis of the *Dissertation sur la glace* in May 1750, described him as being almost more Newtonian than his opponents. Certainly, his reputation as an experimenter is unassailable, and there is little doubt that he regards experiment and observation as the linchpins of scientific methodology. It is true, also, that he condemns the unrestrained use of systems which lead to "des extravagances philosophiques" and "une infinité de rêveries stériles" (viii). However, he refuses to eschew the use of the term "system", although he is well aware that it will antagonise many of his fellow scientists. Despite the prejudice against systems, which, he claims, is perceived by his contemporaries as a sign of intellectual maturity,

voques des systèmes et des hypothèses vagues; et c'est pour répondre en deux mots à cet ouvrage, que j'ai dit que le meilleur usage de l'esprit de système est de n'en point faire, quand on ne sçauroit les appuyer par les calculs." (This letter is cited by Hankins, *Op. cit.* (n. 4) 79.)

By contrast, in a letter to Cramer on January 4, 1749, Mairan describes the reception that his reading of the preface received in November 1748 as follows:

"La petite Préface que je vous montrai, sur les systèmes, et sur la matière subtile, fut lue à la dernière assemblée publique, et elle a eu un succès auquel je n'eusse ôsé m'attendre en combattant les préjugés. J'ai sçu par voie non suspecte que mes idées et ma franchise sur ces deux sujets avoient été aprouvées de ceux-là même que j'aurois cru avoir pour adversaires." (BPU, ms. Supp. 384, f. 307.)

14 See Hankins' comment that Mairan was "one of the last supporters of Cartesian physics" *Op. cit.* (n. 4) 78.

15 It is not too difficult to find examples of Mairan's distrust of hypotheses. In the 17th *éclaircissement* added to the second edition of the *Traité physique et historique de l'aurore boréale* (Paris, 1754), for example, he refers to the claim that electricity is the cause of the aurora borealis as an unwarranted supposition, adding,

"Il est étonnant que dans un siècle où l'on ne cesse de crier contre les systèmes, on se hâte si fort d'en bâtir un sur la simple inspection de quelques expériences qui ne font que de naître, qui n'y ont qu'un rapport si éloigné, si équivoque et jusqu'ici de pure supposition." (p. 447).

he is prepared to argue that the hostility has gone too far. Systems are not, in fact, antithetical to the advancement of science. He is quite prepared to concede that systems can be abused, but, he argues, so can experiments if they are not performed systematically (viii). Besides, every experiment is inspired by "quelque idée générale", "quelque principe de spéculation", or "quelque supposition tacite" about the possible outcome (viii).[16] In this sense, "systems" are a fertile source of experiments and observations, which would never otherwise have occurred to the researcher (xv). Commenting on the Newtonian method, he distinguishes between the expository, or synthetic, method which he says Newton employs in the *Opticks* and the analytic method, or method of discovery, that he uses in the *Phil. Trans.* to describe the experiments which suggest themselves to him and which almost always have their genesis in "quelque réflexion systématique" (ix).

Sounding very much like Condillac, he defines *l'esprit systématique,* which is indispensable for scientific discovery, as a natural inclination, which becomes a habitual practice, to draw up "un plan raisonné" which will permit us to proceed slowly and methodically from what we know to what we do not know and would like to know.[17] The uninhibited creation of gratuitous systems and hypotheses is a denial of this essential *esprit systématique* (x). It is for this reason that almost all the great scientists have been, as he says, *gens à système.*

Again echoing Condillac, Mairan launches into a paean of praise for scientific imagination. Taking Kepler as an example, he maintains that there is a place for inspiration even in the exact sciences. Often, he says, what is required is "une espèce de verve" which sparks the creative process (xi).[18] Besides, what are now accepted as established "truths" started off as "systems" for which

16 While this strikes the reader as a fairly sophisticated and remarkably modern conception, it is interesting to compare what contemporary philosophers of science have to say about the relationship of theory to observation and experiment. Ian Hacking, for example, takes issue with Popper on the question of the role of theory in experimental work, maintaining that in Newton's work on the dispersion of light observations preceded any formulation of theory. *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge, 1983) 155–156.

17 Compare Condillac, *Traité des systèmes,* ed. Georges Le Roy, Corpus Général des Philosophes Français (Paris, 1947) I: 206:

> "Mais, pour ne laisser rien à désirer dans un système, il faut disposer les différentes parties d'un art ou d'une science dans un ordre où elles s'expliquent les unes par les autres, et où elles se rapportent toutes à un premier fait bien constaté, dont elles dépendent uniquement."

18 This was also the quality that Mairan admired in his *Eloge* of Edmund Halley. Referring to Halley's use of magnetism in his explanation of the Aurora Borealis, he wrote as follows:

> "M. Halley ne craignoit pas de heurter les opinions communes, et ne se faisoit pas un scrupule d'imaginer, de proposer des hypothèses, et de conjecturer d'après ses observations et ses idées particulières." *Histoire de l'Académie Royale des Sciences ... 1742* (Paris, 1745) 185–186.

the proof had not yet been found. Both the circulation of the blood and the Copernican system, which he dubs "un hardi paradoxe", "une hypothèse purement conjecturale" and "une simple analogie démentie par nos sens", fall into this category, and should be proscribed, if systems are to be rejected (xii–xiii). Resistance to a theory finally yields, as successive discoveries are made. The Copernican theory eventually led to the theory of universal gravitation, which, however it is understood, has become the foundation for Newtonian celestial physics, "un des chefs-d'œuvre de notre siècle" (xiv).[19]

Turning his attention to his own theory on the formation of ice, he expects, he says, to encounter opposition to his positing the existence of a subtle matter, which has been almost entirely banished from physics texts (xvi). He would condemn it himself if it were to be understood in the Cartesian sense, and if it involved the hard and inflexible particles with which Descartes filled the universe, and which were, he concedes, untenable (xvii). However, this does not mean that some kind of subtle matter which could account for a number of physical phenomena does not exist. How else, he asks, is it possible to explain action at a distance or electrical impulses? (xvii, xx).[20] To strengthen his argument, he enlists the help of "le sage et solide Newton", who made use of the principle of subtle matter in his 1678 letter to Boyle and again forty years later in the *Opticks,* "cet excellent ouvrage" (xviii–xix).[21] Only "ce principe actif et invisible", Mairan believes, could explain universal attraction and provide a mechanical model of the universe. Because he agrees that metaphysics should be banished from physics, he does not attempt to offer an explanation for the mechanism by which this subtle fluid operates, asserting that there is only one right way to "philosophise", and that is by the patient and thoughtful consulting of nature (xx–xxii). Indeed, it is impossible to try to explain such "abstract questions" as the vacuum and the plenum, space, the hardness and primitive cohesion of matter, or the origin of movement without seaching for the First Cause.[22] In this sense, the whole

19 In his correspondence with Gabriel Cramer he makes clear that he accepts without question the fact of universal gravitation, while admitting that he does not know the mechanical cause. See Mairan to Cramer, November 16, 1732 and August 31, 1738 (BPU, ms. Supp. 384, ff. 251, 268).

20 Compare Condillac's use of hypothesis to explain electrical phenomena. *Op. cit.* (n. 17) 203.

21 Kleinbaum, *Op. cit.* (n. 2) 38, states that "the rapprochement between Newtonian and Cartesian physics on the question of subtle matter was at best illusory". R. S. Westfall, however, in *Force in Newton's Physics: the Science of Dynamics in the Seventeenth Century* (London, New York, 1971) points out that subtle matter was a *sine qua non* of every mechanical philosophy, and that many passages in Newton's *Questiones quaedam philosophiae* admitted the existence of an ether (p. 336).

22 We can see from the following quotation that Mairan has encountered the obstacle of understanding the nature of matter and is struggling to define it. To do so, he resorts to the hypothesis of subtle matter:

of physics can be said to be but a corollary of nature (xxiv). However, it is possible for the scientist to regard such concepts as so many givens and to proceed as does "le Méchanicien [*sic*] ou l'Horloger", who is assumed to have done all that is expected of him when he explains how the clock works by going from the hand or pendulum to the weight or spring without bothering with the cause of either gravity or the spring (xxv).[23] To insist on more, and then to become discouraged because an exhaustive explanation is considered to be unattainable, would be to close the door on a thousand useful and infinitely satisfying facts (xxv–xxvi).

Rejecting Descartes' hard particles, and citing Boerhaave's experiments and reflections on fire, Mairan concludes by arguing that matter is composed of elastic particles, and that Newton himself, by not admitting either elasticity or gravity as an inherent quality of matter, thereby tacitly conceded Malebranche's *petits tourbillons,* without which no such fluid can exist in nature (xxvii).

The resemblances between Condillac's position and Mairan's are so striking that they even on occasion use the same words. Both, for example, allude to the *règle de fausse position* in mathematics to illustrate the necessity of hypotheses.[24] Both refer to the familiar idea that the clockmaker can explain how a clock works only by showing how each part of the clock affects the others until he arrives at the mainspring on which all the other parts depend.[25] Both discuss in similar fashion the role that imagination plays in physics. While Mairan admires the "génie de l'invention" and the "verve" of Kepler, Condillac recognises that "on ne doit pas interdire l'usage des hypothèses aux esprits assez vifs pour devancer quelquefois l'expérience".[26] In short, when Mairan was defending the value to the scientist of "systems" and hypotheses, which so exasperated d'Alembert, he was not occupying a position so different from that of Condillac, whose ideas on scientific method d'Alembert so much admired.[27]

> "En supposant l'existence de ce fluide, comme je fais par voie de demande et d'hypothèse, je n'entreprends nullement d'expliquer à son égard le méchanisme par lequel il m'aide lui-même à donner raison de la cohérence des élémens plus grossiers de la matière dure ou fluide qui tombe sous nous sens; car il n'y auroit plus de fin à une pareille recherche" (xxii–xxiii).

23 Cf. Condillac, *Op. cit.* (n. 17) 207:

> "Enfin ouvrez-lui cette pendule, expliquez-lui en le mécanisme; aussitôt il saisit la disposition de toutes les parties, il voit comment elles agissent les unes sur les autres, et il remonte jusqu'au premier ressort dont elles dépendent. Ce n'est que de ce moment qu'il connoît avec certitude le vrai système qui rend raison des observations qu'il avoit faites."

24 Compare Mairan, p. xv and Condillac, *Op. cit.* (n. 17) 196.

25 Compare Mairan, pp. xxv–xxvi and Condillac, *Op. cit.* (n. 17) 207.

26 Compare Mairan, p. xi and Condillac, *Op. cit.* (n. 17) 203.

27 Hankins described the preface as follows:

> "It was an eloquent and persuasive discourse that openly supported philosophical and

In the analysis of Lavirotte's translation of *L'Exposition des découvertes philosophiques de Newton* by Colin Maclaurin in the *Journal des sçavans* of May 1750, the journalist wrote:

> "Comment découvrir les secrets de la nature, si l'on ne joint l'esprit de raisonnement aux observations; l'esprit de raisonnement est-il autre chose que celui de système, autrement c'est entasser des faits sans discernement, sans en voir l'union et la dépendance; l'esprit de système est la réduction des expériences et des observations à des règles fixes et certaines.[28]

It is interesting in the context of the present essay to note that the journalist is thus using the term *l'esprit de système* to mean what Mairan means (p. ix) by *l'esprit systématique:*

> "En vain dira-t-on que *l'esprit systématique a fait tomber de tout temps les Philosophes dans les plus grandes erreurs.* Cet esprit n'en est pas moins tout ce qu'il y a en nous de plus précieux, de plus nécessaire pour arriver aux connoissances les plus sublimes, comme pour exécuter les plus grandes choses."

For Mairan, then, as for Condillac, there are good and bad systems, and good and bad hypotheses.

What Mairan has to say in the preface to the *Dissertation sur la glace* is consistent, not only with Condillac's views on systems and hypotheses, but also with those of Mme du Châtelet on the usefulness of hypotheses.[29] Moreover, Buffon had published a similar preface at the beginning of his translation of Stephen Hales' work, and it was in Buffon's memoir on generation that Mairan believed that he had found an admirable example of the usefulness of systems.[30] In short, it seems likely that Mairan's reputation in some quarters as a last-ditch Cartesian resistant to new ideas rendered suspect to opponents such as d'Alembert anything he had to say on the subject.

scientific systems, defended the theory of an ether, and carefully documented the fact that Newton believed in an ether as well. Some of these comments must have appeared to d'Alembert as a direct attack on his own ideas and those of Condillac" (*Op. cit.* (n. 4) 79).

28 In the dedicatory epistle to his translation of Colin Maclaurin's *Exposition des découvertes philosophiques de Newton* (Paris, 1749), Lavirotte states that it was Mairan who had urged him to do the translation. See *Journal des sçavans* (Amsterdam edition), May 1750, 84.

29 In the review of Mme du Châtelet's *Institutions de physique* in the *Journal des sçavans* of March 1741, the journalist compared hypotheses to scaffolding – of no use once the structure is complete, but without which it could never have been built (p. 312).

30 Buffon's preface (p. 5) contained the following comment:

> "C'est par des expériences fines, raisonnées et suivies, que l'on force la nature à découvrir son secret; toutes les autres méthodes n'ont jamais réussi, et les vrais physiciens ne peuvent s'empêcher de regarder les anciens systèmes, comme d'anciennes rêveries, et sont réduits à lire la plûpart des nouveaux, comme on lit les romans: les recueils d'expériences et d'observations sont donc les seuls livres qui puissent augmenter nos connoissances ... Amassons donc toûjours des expériences, et éloignons-nous, s'il est possible, de tout esprit de système." *Oeuvres philosophiques,* ed. J. Piveteau, Corpus Général des Philosophes Français (Paris 1954).

On Mairan's admiration for Buffon's system, see his letter to Cramer of January 18, 1749 (BPU, ms. Supp. 384, f. 310).

This was certainly not, however, a reaction shared by all of Mairan's contemporaries. Mairan himself commented, as we have seen, on the favourable reaction that the reading of the preface had received. Also, the abbé Nollet, in a letter written to Jallabert on May 9th, 1750, remarked that Mairan's new edition of the *Disseration sur la glace* had been well received, which, he declared, was not the case with the recently published three volumes of Buffon's *Histoire naturelle,* in which "systems" were piled one upon the other, and, added Nollet, "quels systèmes!"[31] Not, he continued, what one would expect from a man who for the past fifteen years had been constantly criticizing "les gens à système". It seems clear that Nollet approved of Mairan's "system" on the formation of ice while rejecting Buffon's "system", which Mairan himself applauded.[32]

Although, as Hankins says, Mairan tended to use the terms, *l'esprit systématique* and *l'esprit de système,* interchangeably, it is clear that he differentiates between two different *concepts.* In this respect, Mairan was neither unique nor anachronistic in his defence of certain systems and hypotheses. Jacques Roger was right when he wrote:

> "Sans rien ôter au rôle nécessaire de l'observation et des faits, la nouvelle pensée scientifique prétend donc aller au delà, et réhabiliter l'hypothèse et le 'système'. Il ne s'agit pas, sans doute, de ressusciter un passé périmé, et nous verrons en quoi les nouveaux systèmes diffèrent des anciens. Cependant, la nouvelle science va multiplier les systèmes, et les 'observateurs' fidèles à l'esprit de la période précédente le lui reprocheront amèrement."[33]

Roger shows convincingly that Mairan's voice was one of many to write in favour of "systems" between 1740 and 1750.[34]

Mairan was faced with two tasks in his *Dissertation sur la glace.* First, he had to try to explain his theory on the formation of ice, despite the obstacles represented by his inability to account for the hardness and cohesion of matter and his reluctance to relinquish a mechanical model of the universe. This necessitated the introduction of a subtle fluid which he attempted to persuade the reader to accept by alluding to Newton's use of it.[35] Secondly, he

L. Hanks states in "Buffon et les fusées volantes", *Revue d'histoire des sciences* 14 (1961) 137 that Buffon was accused by his contemporaries of being a "systématiseur", but concludes (p. 154), after examining the reasons for his contemporaries' criticisms, that it is the very unevenness of his scientific work which is interesting. His methods are sometimes effective and sometimes lead to error, while systems which seem arbitrary are found to have respectable scientific antecedents or successful consequences.

31 BPU, ms. Jall. SH 244, f. 186.

32 BPU, ms. Supp. 384, f. 310 (Mairan to Cramer, January 18, 1749).

33 Roger, *Les sciences de la vie dans la pensée française du dix-huitième siècle* (Paris, 1963) 468. See also Kleinbaum, *Op. cit.* (n. 2) 27.

34 *Op. cit.* (n. 33) 465–468.

35 The difficulty arose, as Hanks points out, from the many definitions which the eighteenth century gave to the term "system". The word was used to condemn a wide variety of scienti-

had to demonstrate what he understood to be the Newtonian method of scientific discovery. What he believes he is advocating in the preface is the Newtonian methodology with its preoccupation with mathematical relationships and its refusal to speculate on causes. While he is praising the empirical science that he so admired in Newton, he appears at the same time to be commending systems, which is what so incensed d'Alembert. Yet the systems that he supports are not the abstract systems of the preceding century. Because many of his contemporaries perceived him to be a Cartesian, any favourable comment on systems would arouse suspicion. However, as recent scholarship has indicated, it is no longer possible to regard him simply as a Cartesian. He was rather, as I have described him elsewhere, a "Cartonian", or, alternatively, a "Newtesian". Although it seemed like a courageous move in 1748 to acknowledge publicly his support for systems, it was not, as Hankins says, "a very unusual paper" that was read in the public assembly of the Académie des Sciences.[36] Mme du Châtelet, Bazin, Buffon, Hales and Condillac, among others, were all saying much the same thing about the same time, and Mairan's comments in the preface represented, as Roger says, "la nouvelle pensée scientifique".[37]

In conclusion, an examination of Mairan's published works and his unpublished correspondence reveals the complexity of his understanding of scientific methodology. The picture that emerges indicates a fascination with the Newtonian method, a wariness where speculative systems are concerned and an appreciation of the importance of the scientific imagination – all of which serve to throw light on the argument that he puts forward in the preface. The terms *l'esprit systématique* and *l'esprit de système* may be used by him interchangeably, but this is more a question of semantics than a confusion in his mind of two entirely different concepts.[38] It is true that d'Alembert stressed verification by calculation to establish the validity of a system

fic constructions in the form of speculative metaphysical systems, certain conceptual models involving mathematical equations, and hypotheses which resorted to an ether to explain phenomena. *Op. cit.* (n. 30) 140, n. 1.

36 Hankins, *Op. cit.* (n. 4) 78.

37 *Op. cit.* (n. 33) 468. Mme du Châtelet defended good hypotheses in the *Institutions de Physique* (1740), as did *Le Journal des sçavans* in Oct. 1742, 612–613. Also, Bazin's preface to his *Observations sur les plantes* (1741) is a manifesto in favour of hypotheses. See Roger, *Op. cit.* (n. 33) 465–466.

38 Indeed, an example of the confusion in his use of the two terms can be seen in his third letter of October 22, 1736 to Father Parrenin, a Jesuit missionary to China, in which he comments:

> "L'esprit de l'Académie des Sciences est de se tenir en garde contre ces vérités qui ne sont encore que systématiques, pour ne les recevoir sans restriction qu'après que l'expérience les aura mises au rang des vérités de fait les plus certaines." *Lettres de M. de Mairan au R. P. Parrenin* (Paris, 1759).

and equally true that Mairan's use of mathematics was different from Newton's.[39] However, Mairan describes his method in a letter to Charles Bonnet written on May 23rd, 1762 thus: He likes to begin, he says, by adopting an intellectual or "metaphysical" approach to a subject, followed by a mathematical analysis in order to verify his premise.[40] Mairan was quite clear in his mind about the fruitlessness of one kind of system and of the usefulness of another. In fact, his analysis in the preface is a moderate plea for not throwing the baby out with the bath water. In this, he reflects the view of the *Encyclopédie* itself, which, in the case of hypotheses, advocated neither placing too much trust in them nor proscribing them entirely.[41] Despite Mairan's difficulty with the concept of "action at a distance", and his initial reluctance to relinquish vortices and subtle matter in his mechanical model of the universe, his preoccupation with scientific method, his commitment to experiment and observation of the natural world and his familiarity with the *Opticks* resulted in a sophisticated understanding of scientific methodology which did not deserve d'Alembert's condemnation.

39 See Kleinbaum, *Op. cit.* (n. 2) 38.

40 In the postscript, Mairan refers to his discussion of the continuity of the solar atmosphere in the *Eclaircissements* at the end of his treatise on the aurora borealis. In the 4th *éclaircissement,* he says, he adopts a "metaphysical" approach to the subject, while in the fifth *éclaircissement* he adopts a geometric and algebraic calculation to verify his premise and to satisfy the mathematicians among his readers who would insist on it. BPU, ms. Bonnet 27, f. 55.

41 See Roger, *Op. cit.* (n. 33) 468.