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JEAN ANTOINE NOLLET AND EXPERIMENTAL NATURAL PHILOSOPHY IN EIGHTEENTH-CENTURY FRANCE

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THE UNIVERSITY OF OKLAHOMA

GRADUATE COLLEGE

# JEAN ANTOINE NOLLET AND EXPERIMENTAL NATURAL PHILOSOPHY IN EIGHTEENTH-CENTURY FRANCE

A DISSERTATION

SUBMITTED TO THE GRADUATE FACULTY

in partial fulfillment of the requirements for the

degree of

DOCTOR OF PHILOSOPHY

By

RAMEZ BAHIGE MALUF

Norman, Oklahoma

1985

JEAN ANTOINE NOLLET AND EXPERIMENTAL NATURAL PHILOSOPHY

IN EIGHTEENTH-CENTURY FRANCE

A DISSERTATION

APPROVED FOR THE DEPARTMENT OF THE HISTORY OF SCIENCE

By

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## PREFACE

In my first year as a graduate I had an argument with a classmate over whether or not Nobel prize laureates in science would be considered the great scientists of our age by future historians. The discussion revolved around these two points: If contemporaries were to decide what made for great scientific achievements and who was responsible for them, then historians would be deprived of the discretion of judging differently. But, on the other hand, if historians disregarded the judgement of contemporaries their narratives would run the risk of being anachronistic.

I have since then conquered most of my love for historical dilemmas, pseudo-dilemmas and "Great Scientists" theories of history. However, my initial interest in the Abbé Jean Antoine Nollet stemmed precisely from a desire to understand the reasons for the esteem he enjoyed in his lifetime, for I believed his physics to be too simple, his ideas commonsensical, and his diligence commendable but not the source of important scientific achievements. The more I looked into the matter the more I concurred with Voltaire's assessment of the Abbé: Nollet was not himself a great scientist but he knew more than great scientists of the past did. I hope that the following pages will justify this assessment.

The reading copy of this dissertation was presented to members of the faculty of the History of Science Department at the University of

Oklahoma in January of 1982 and defended the following month. I was asked then by the dissertation committee to make a few minor changes and corrections before submitting a final copy. One week later I flew back home to Beirut, Lebanon, believing I would send back the corrected version in a few days, or a few weeks. In fact, it would be a few years before I did so. While it had taken me about fourteen months to prepare the reading copy, it was to take me over three years to turn it into a final copy. Throughout these three years Beirut offered a sharply different environment from Norman, Oklahoma, and I had many occasions to sorely miss the tranquility and wealth of resources available to me at the History of Science Collections during my student days.

Because of these circumstances, literature relevant to the dissertation published since 1982 is not, for the most part, incorporated into the dissertation.

In translating passages from the French I have often opted for a literal rendering, preferring to sacrifice style rather than meaning. A recurring problem was the translation of the French word _expérience_. Eighteenth-century French writers did not have to distinguish between "experiment" and "experience" when they spoke of experimental physics, for the French word _expérience_ may refer to both concepts, or either. When the distinction is not made in French, the translator into English has to force a certain interpretation sometimes judging "experiment" was meant and at others, "experience." I hope that on the numerous occasions I have had to make this decision I was right more often than not.

One of the pleasures the completion of the dissertation offers me is the opportunity to thank those whose help made a difference. I

would like first of all to express my appreciation to the members of the dissertation committee: Professors John Biro, David B. Kitts, Duane H. D. Roller, Thomas Smith, and Kenneth L. Taylor. I am grateful for their interest, patience, and suggestions. Professor Smith read more than one draft of some chapters which profited greatly from his perspicacious editorial pen.

Professor Marcia Goodman, librarian in charge of the History of Science Collections, helped to solve many problems some of which appeared to me insurmountable. Her expert and kind help is acknowledged fondly.

The chairman of my dissertation committee, Professor Taylor, offered precise and clear suggestions, constructive and inspiring criticisms. His adroit guidance was instrumental in the development of the thesis offered here. Putting the finishing touches on the dissertation from Beirut would have been impossible without his patient help in Norman.

I wish also to thank Professor Mary Jo Nye for her kind assistance in checking some materials for my benefit at the Bibliothèque Nationale in Paris.

Valli Powell's willingness to type this dissertation to help me meet a deadline is appreciated.

My thanks also go to members of my family, Dalal, Zena and May for their financial and moral support.

TABLE OF CONTENTS

JEAN ANTOINE NOLLET AND EXPERIMENTAL NATURAL PHILOSOPHY

IN EIGHTEENTH-CENTURY FRANCE

RAMEZ BAHIGE MALUF

ABSTRACT

This dissertation examines the scientific career of the Abbé Jean Antoine Nollet (1700-1770) and attempts to throw some light on his work in the context of eighteenth-century physics.

A central theme of the dissertation is that Nollet enjoyed the esteem of contemporary scientists and savants because he preached and practised a type of physics that was considered beyond controversy, believed to be grounded on observation, experiments and those truths of science around which scientists were agreed.

Nollet also helped popularize experimental physics by building its instruments, designing experiments, and advancing theories based on them. His most important theoretical contribution was in the field of electricity--the eighteenth-century experimental science par excellence. The theory of electricity he presented in 1745 provides an illustration of his method and work. It was formulated to explain a vast array of experimental and observational data and it relied heavily on the senses; it also relied on Nollet's notion of a science built on non-controversial facts, a science of consensus. The theory can be seen as a methodical

arrangement of those ideas about electricity shared by a large number of students of the field and the many observations he performed.

Nollet saw his work as part of a collective process that presupposed standardization of instruments and procedures. He thus rejected anything that was controversial or that could not be settled in a *cabinet de physique*. He helped steer physics into the laboratory, keeping clear of controversies that engulfed much of French physics during the period of the introduction of Newtonian physics into the continent. Years later, as the *cabinet de physique* became more demanding and more precise Nollet's experiments appeared crude and his theories outdated.

JEAN ANOTINE NOLLET AND EXPERIMENTAL NATURAL PHILOSOPHY

IN EIGHTEENTH-CENTURY FRANCE

# CHAPTER I

## JEAN ANTOINE NOLLET (1700-1770)

Jean Antoine Nollet, popular French lecturer and demonstrator of experimental physics in the eighteenth century and one of its most respected students of electricity, Fellow of the Royal Society of London, member of the Institute of Bologna and of the Erfurt Academy of Sciences, Royal Professor of Experimental Physics at the Collège de Navarre, and one-time *directeur* of the Paris Académie des Sciences, was born on November 19, 1700 at Pimprez, a village about sixty miles to the north of Paris.[1] His parents were peasants. He was the only child of four by Charles Nollet and Geneviève Champenois to live past childhood.[2] Very little is known of Nollet's early life; recent biographies have added almost nothing of note to what Jean-Paul Grandjean de Fouchy (1707-1788) wrote in his "Eloge de M. l'Abbé Nollet."[3] One can still say today with this early biographer of Nollet that "we are absolutely ignorant of all details of his early years."[4]

At age fourteen Jean Antoine left Pimprez for Clermont in Beauvaisis to continue his studies at that town's *collège*.[5] This he did, according to one biographer, despite his father's wishes that Jean Antoine stay at Pimprez to help him cultivate the land.[6] Supposedly sensing Nollet's talents, it was the mother who pressed that he be allowed to continue his studies. The local curé was called in to arbitrate between the two parents and apparently was able to convince Charles that his son should continue with his studies and possibly prepare for an ecclesiastical career.[7] Jean Antoine left Pimprez for Clermont in October of 1714 and later went to the collège at Beauvais, an establishment near Primprez which taught the Humanities and prepared men for the priesthood.[8]

Nollet's pursuit of an ecclesiastical career required further studies that could best be done in Paris. He moved to that city around 1718 and once there was hired by the administrator and concierge of the Paris Hôtel-de-Ville as a live-in tutor to his children. At the age of twenty-two Nollet obtained the degree of maître ès arts. Two years later he was graduated a bachelor in theology and a year after that received the sous-diaconat and his license. He became a deacon in 1728.[9] Nollet never sought to pursue his career in the clerical hierarchy any further, although according to one biography he solicited and obtained a dispensation to preach and appears to have exercised this profession for a short time and with some success.[10] Thereafter he would devote his time to the study of physics and the mechanical arts.

Nollet's career in physics seems to have begun with his interest in the mechanical arts and the manufacture of instruments. While still at the Hôtel-de-Ville he built his own laboratory and his own instruments. There he also worked with Parisian émailleurs.[11] Although émail (enamel) was used to finish scientific instruments, Nollet's early interest was in the making of figurines and mechanical artifacts.[12] His reputation for mechanical adriotness resulted in an invitation to join the Société des

Arts in 1728.[13] This short-lived society was founded in 1726 with Louis Bourbon de Condé (1709-1779), Comte de Clermont and god-son of Louis XIV, as its patron. The Société had among its members men interested in letters, the sciences and mechanical arts. Among them were Fouchy, Alexis-Claude Clairaut (1713-1765), Charles Marie de la Condamine (1701-1744), the Abbé Jean Paul de Gua de Malves (1712-1786), the Académie's perpetual secretary Bernard le Bovier de Fontenelle (1657-1757), the musician Jean Philippe Rameau (1683-1764) and Pierre Polinière (1671-1734), lecturer and author of textbooks on experimental physics. The Société apparently limited its activities to social gatherings and, on occasions, the reading of memoirs.[14]

John Heilbron suggests that through contacts established at the Société, Nollet came to be associated with Charles François de Cisternay Dufay (1698-1739), although the historical record is silent on this question. Nollet worked with Dufay in the years 1731 through 1733.[15] Dufay, already a member of and a regular contributor to the Académie des Sciences, was then involved in experiments which were to result in his celebrated six memoirs on electricity of 1733 and 1734.[16]

In 1732 Nollet was entrusted by the renowned scientist René-Antoine Ferchault de Réaumur (1683-1757) with the responsibility for his prestigious laboratory.[17] Nollet collaborated with Réaumur on a number of projects, among them the improvement of the thermometer. The Abbé was primarily responsible for the construction of instruments for Réaumur's laboratory. It was during this period that Nollet's first contributions made their appearance in the records of the Académie. In 1733 two of his machines received the approval of the Académie: an improved camera obscura and a lens-grinding machine with a more convenient fixed base.[18]

In 1734 Nollet was invited by Dufay to accompany him on a visit to London.[19] Dufay had recently been appointed Intendant at the Jardin des Plantes and was on a mission to England to establish contacts and to research methods to revitalize that institution.[20] During that visit, undertaken with Henri-Louis Duhamel du Monceau (1700-1782) and Bernard de Jussieu (1699-1777), Nollet had his introduction to British scientific circles. He was made a Fellow of the Royal Society and met with John Theophilus Desaguliers (1683-1744), by then an experienced lecturer in experimental physics.[21] When Nollet returned to Paris he set up his own courses. He was later to acknowledge a debt to Desaguliers in the preface to his manual on experimental physics, published in 1738.[22]

In 1736 Nollet was once more offered the opportunity by Dufay to accompany him on another trip, this time to Holland. There Nollet met the brothers Jan (1687-1748) and Pieter (1692-1761) van Musschenbroek and Wilhelm Jacob 'sGravesande (1688-1742).[23] On his return to Paris Nollet resumed his lectures and the manufacture of scientific instruments. It was as an instrument maker and lecturer on experimental physics that Nollet was first to establish his reputation. His lectures attracted men and women of all ages from Paris, the provinces and abroad.[24] The Abbé Noël-Antoine Pluche (1688-1761), in his very popular Spectacle de la nature, recommended as early as 1739 that experimental courses be modelled after those offered by Nollet.[25] Nobles and princes requested private sessions. In 1738 he lectured to the Duc de Penthièvre and shortly after to the Duc de Chartres, and the following year he was called to the Court of Turin where he remained for six months offering physics lectures to the Duke of Savoy. When he left, his instruments stayed behind because the King of Sardinia, Charles Emmanuel III, wanted them kept at the University "afin que les Professeurs," Nollet wrote, "pûssent s'en servir dans la suite pour cultiver & pour enseigner la Physique par voie d'expérience."[26]

Nollet's services as instrument maker, too, made him sought by savants. The collecting of scientific instruments in the eighteenth century was an activity no longer limited to scientists and institutions but had become, in the words of Torlais, a passion of "grands seigneurs et riches bourgeois, hauts fonctionnaires et femmes du monde," as well as _philosophes_.[27] One of Nollet's clients was François Marie Arouet, or Voltaire (1694-1778). The machines of the Abbé, Voltaire wrote Jeanne Françoise Quinault in 1739, "remplissent ma galerie."[28] This eighteenth-century passion was costing the _philosophe_ a considerable amount of money. "Nous sommes dans un siècle," he lamented to another correspondent, "où on ne peut être savant sans argent."[29] The Abbé Bonaventure Moussinot, Voltaire's friend and _homme d'affaires_ in Paris, was indeed worried that the _philosophe_ might in fact become a "savant sans argent" and was never prompt or eager to pay Nollet for his services. Voltaire scolded Moussinot a number of times for his cavalier treatment of Nollet: "Ce n'est point un homme ordinaire avec qui il faille compter," he wrote Moussinot. "C'est un philosophe, c'est un homme d'un vray mérite qui seul peut me fournir mon cabinet de phisique et il est baucoup [sic] plus aisé de trouver de l'argent qu'un homme comme luy."[30] A few months later, in what may have been a more practical mood, he told his friend Nicolas

Claude Thieriot: "L'abbé Nolet me ruine."[31]

In April of 1739 Nollet received an accolade from the more professional segment of the scientific community when he was made a member of the Académie des Sciences, filling a position of adjoint mécanicien vacated by Georges Louis Leclerc, Comte de Buffon (1707-1788), who had been appointed adjoint botaniste. Three years later Nollet was promoted to associate rank.[32]

Up to the time he was admitted as adjoint mécanicien Nollet's only publication was his manual of experimental physics that appeared in 1738, the Programme ou idée générale d'un cours de physique expérimentale. This book, as the title indicates, was meant to serve as a general outline for his lectures in experimental physics. It was, in fact, a simple manual. The book was divided into three parts; the first two dealt with the subject proper and consisted of sixteen "Leçons," while the last was an enumeration of those instruments and materials--345 in all--Nollet believed necessary to illustrate and carry out the experiments that made up the lectures. The Programme, Nollet wrote, was meant in part to serve some of his "Auditeurs qui seroient bien aise de joindre quelques lectures à l'inspection des expériences, pour avoir des explications plus étendues que celles qui me sont prescrites par les bornes du tems."[33] It was also to provide a manual for those "qui n'étant point à portée d'y assister [i.e., attend his experiments], voudront les imiter ou se former un plan d'étude en les supposant."[34] It was then for these reasons, to allow others to repeat his experiments and to guide themselves through further readings, that he organized his Programme as he did. "On y indique en détail les matieres que l'on traite dans chaque Leçon, les opérations qui servent de preuves, les Phoenoménes qu'elles expliquent, les applications qu'on en fait ou qu'on en peut faire."[35] This was one of the reasons he included a list of instruments. If the public would continue to accord its approbation to the new school, Nollet promised to provide a larger work which would deal with the same materials but in greater detail. This promise was realized later in the six-volume Leçons de physique expérimentale that began to appear in 1743.[36]

The Programme was well received, and interest in Nollet's lectures continued to increase. The Programme, a reviewer in the Journal des sçavans wrote, is but a simple indication of what the Abbé has been doing for over three years before a public of all ages.[37] The success of the Abbé, the Journal predicted, would only be greater in the future:

> Le nombre des Disciples croît de jour en jour; & le Maître se perfectionne de plus en plus.
>
> Au reste, des Cours de Physique expérimentale ne pouvoient manquer de réussir. Cet établissement réunissoit l'utile & l'agréable . . . & les étrangers que l'amour des Sciences attire à Paris, étoient surpris de ne l'y pas trouver.[38]

The Mémoires de Trévoux reported the publication of the Programme in 1738 on two different occasions.[39] The first was a simple announcement, the second a review. In the review, which appeared in November, the reviewer wrote, "Quoique nous aîons annoncé cet Ouvrage, qui est déja fort connu, nous ne pouvons nous dispenser d'entrer dans le détail de ce qu'il contient & de ce qu'il promet."[40] While other physicists have spoken to the mind, ". . . M. Nollet a trouvé l'heureux secret de faire parler la Physique aux yeux. . . ."[41] The reviewer was very laudatory of the instrument cabinet that Nollet had assembled. The Abbé, he wrote, "est venu à bout de former un Cabinet très rare, qui manquoit à la France, & un Laboratoire où l'on construit tout ce qui est nécessaire pour la

Physique experimentale."[42]

The one-month course to which the Programme served as a manual was divided into two parts. The first eight lessons consisted of an exposition of general principles such as the divisibility, solidity and porosity of bodies; motion; gravity (pesanteur); equilibrium; and general mechanics. The remaining eight lessons considered more specific subjects, such as the weight, elasticity and other properties of air, water and fire; the relation of fire to light and of the latter to colors; the celestial bodies and their relation to the Earth; electricity and magnetism. Nollet indicated the approach he followed:

> J'expose en peu de mots l'état de la question; je prouve mes propositions par des opérations relatives; j'indique les applications qu'on en peut faire aux Phoenoménes les plus ordinaires, & les lectures qui conviennent à ceux qui voudront des explications plus amples; . . .[43]

It was Nollet's intention to make the lessons accessible to all those interested and he made it a practice to be clear and explicit in his exposition.

> . . . il a paru plus convenable de se rendre les expressions familiéres, de se former une habitude d'opérer en parlant, & même d'employer moins les paroles que exposition des faits pour se faire entendre, . . . .[44]

The emphasis on demonstration, in contrast to the use of "paroles," was meant to facilitate the understanding and also to emphasize the demonstrability of what was being taught. This was, after all, the new science.

> Cette science n'est plus comme autrefois un vain assemblage de raisonemens non fondés, ou de systêmes chimériques, les conjectures sont mises au rang qui leur convient; on ne croit plus que ce que l'on voit, & la raison ne prononce que sur le rapport & le témoignage de l'expérience.[45]

A result of this zeal for the explicit and accessible was a set of lectures whose contents were too elementary and innocuous to attract the interest of scientists. The lectures were not, of course, addressed to them. Among the public at large, Nollet's lectures, as I have indicated, were very successful.

Soon after returning from his stay at the Turin court in 1739 Nollet presented a series of memoirs to the Académie des Sciences on the construction of pneumatic machines, and a memoir on observations he had made on the vapors detectable in the receiver of a pneumatic machine after the air it contained was rarified.[46] In this last memoir Nollet argued that experiments of this nature would help identify the composition of the different substances present in the air.

In 1741 Nollet put his experimental prowess to work on a controversial issue concerning the Cartesian vortices and the mechanical explanation of weight they provided.[47] Descartes had explained the fall of objects toward the center of the Earth as being due to their displacement by the centrifugal motion of the subtile matter of the vortex surrounding the Earth. Particles of the rotating vortex, moving with greater centrifugal force than other bodies released above the surface of the Earth would tend to force them downward. Christiaan Huygens (1629-1695) criticized this explanation on the grounds that a spherical vortex rotating about an axis would cause "heavy" objects to fall toward the axis of rotation and not toward the center of the sphere. Huygens suggested that a possible alternative to this explanation would be to postulate a multitude of simultaneous motions to the vortex.[48] "Heavy" objects, according to this explanation, would then coalesce, i.e. fall, around the point of intersection of the many axes of motion. This explanation of gravity (pesanteur) received the attention of many physicists, both critics and sympathizers, and a number of different arguments and ways to put it to test were attempted.[49]

Again in 1728 the issue was revived by George Bernard Bulffinger (1693-1750).[50] That year the Paris Académie had made the subject of its annual competition the problem of providing a physical--and by that was meant mechanical--explanation of gravity (pesanteur).[51] Bulffinger attempted an experimental test of Descartes' explanation. He found that with one spherical rotation, "heavy" objects would coalesce cylindrically around the axis of rotation. Bulffinger suggested, however, that the Cartesian explanation could be salvaged by simply postulating that the vortex underwent not a multitude of motions, as Huygens suggested, but only two simultaneous motions around axes that intersected perpendicularly. The "heavy" objects would then fall toward the point of intersection, i.e. toward the center of the sphere. He suggested that an experiment to test this hypothesis would indeed confirm it, and he promised to perform the experiment himself in the near future.[52]

In 1740 the issue was once more brought before the Académie by one of its associates, Joseph Privat de Molières (1677-1742), a physicist and textbook author who adopted Descartes' explanation in his published works. Privat de Molières was attacked by the Newtonian physics teacher Pierre Sigorgne (1719-1809), who argued that reason and experiment contradicted the vortical explanation of gravity.[53] Privat de Molières performed a series of experiments before the Académie, experiments which he asserted contradicted Huygens' and Bulffinger's criticisms and validated Descartes' hypothesis. The Académie, however, judged his results inconclusive.[54] Nollet, picking up on the renewed interest shown in this matter, duplicated the machinery utilized by Bulffinger and with many carefully performed experiments reaffirmed the results arrived at in 1728.[55] Furthermore, Nollet constructed a device to test Bulffinger's suggestion that a simultaneous double motion to the vortex would force objects toward the center. With experiments conducted in the presence of Académie members, Nollet showed that this alternative, too, would not work. The "heavy" objects tended to coalesce around one of the axes or to move in unpredictable directions. A memoir describing the experiments, with plates of the instruments used, was published in the Mémoires for 1741.[56]

Privat de Molières remained unconvinced. On May 2 and 5 of the following year he performed his own experiments at the Académie and on May 9 presented that institution with a memoir summarizing his findings.[57] The Académie did not see fit to include it in its registers. A heated debate ensued and, according to an imaginative account of this episode, Privat de Molières left the Académie "tout bouillant."[58] On his way home he caught a chill and a few hours later suffered a violent fever and chest congestion. He died five days later and Nollet was appointed in 1742 to the position of associé vacated by his death.[59]

Some inferences may be drawn from this story--other than that Paris has treacherous May weather. One is that Nollet was apparently willing to bring his experimental expertise to bear so as to test, and consequently discredit, that which to many was a viable Cartesian defense in face of mounting Newtonian attacks against the vortex theory. Moreover, this was an indication that he indeed believed that a dispute of that nature could and should be decided experimentally.

In 1743 the first two volumes of Nollet's *Leçons de physique expérimentale* were published. The next two volumes appeared by 1748. The fifth volume came out in 1755, and the sixth and final volume was published only in 1764.[60] The *Leçons*, the extended version of the *Programme* Nollet had promised in 1738, was well received by the public and savants. Reviews and commentaries in the *Journal des Sçavans*, the *Mémoires de Trévoux*, and the *Histoire de l'Académie Royale des Sciences*, commended the work.[61] Some of the volumes were reissued as many as ten times before the end of the century, and Italian and Spanish translations appeared shortly.[62] The physics of the *Leçons* followed the same general outlines of the *Programme*, except for the treatment of electricity which was distinctly different. In the *Programme* the Abbé had adopted Dufay's resinous and vitreous electricity, but in the *Leçons* there was a full-fledged presentation and defense of his own "affluence and effluence" theory which he first presented in 1745.[63]

With the death of Dufay in 1739, Nollet, who had been associated with his electrical experiments, began his ascent to a position of eminence among French "electricians." In 1745 Nollet offered his own theory on the causes of electricity in a memoir he read to the Académie. The paper was subsequently published in the Académie's *Mémoires* for that year under the title "Conjectures sur les causes de l'électricité des corps."[64] The matter of electricity, Nollet argued, was the same as that of fire. The electrical phenomena of attraction and repulsion were explainable by the inward and outward flow of this matter from electrified bodies. Neither of these ideas was novel in its general terms. What Nollet brought to bear was greater detail in their exposition, and the respectability and prestige of years devoted to experimental physics.[65] The "Conjectures" were well received in France and elsewhere, and Nollet's theory reigned supreme among his countrymen in the field of electricity at least until the introduction of the works of Benjamin Franklin (1706-1780) early in the 1750's.[66]

After 1745 a large number of Nollet's publications were dedicated to electricity. In 1746 he published his Essai sur l'électricité des corps and in 1749 the Recherches sur les causes particulières des phénomènes électriques. These were followed by the publication of different Lettres sur l'électricité while in the pages of the Mémoires of the Académie a number of papers appeared upon a vast array of subjects concerning new discoveries in electricity, experiments examining its medicinal value, and discussions of competing electrical theories.[67] However, Nollet continued, as he had before 1745, to present the Académie with works on various other subjects.

In 1743 he had plunged into the Seine River to test the transmission of sound underwater as a preliminary to his study on the hearing faculty of fish.[68] In 1748, in between some electrical investigations, he read a rather lengthy memoir on experiments he had performed which, he claimed, disproved the then commonly held view that the cause of boiling in liquids was the dislocation of air present in them. Nollet demonstrated that the boiling was due to the escaping bubbles of steam, not air, through the surface of the heated vessel. In other liquids the boiling was due to the formation of vapors, other than air, seeking the surface of the vessel.[69] Nollet also contributed many other memoirs on such disparate subjects as the existence of luminous insects in sea water, an artificial way of cooling liquids by the addition of special salts, an observation of the Sun's perihelion, and one on "la vie et les moeurs" of an elephant Nollet had observed during his second trip to Italy.[70]

This second journey to Italy, in 1749--the first it will be remembered was in 1739--was motivated in large part by his desire to investigate reports originating in that country on the successful application of electricity in medicine.[71] Nollet himself had with the aid of the physician Sauveur-François Morand (1697-1773), chief surgeon at the Hopital de la Charité, attempted to induce cures for paralysis and injuries through the aid of electricity.[72] The results had not been very encouraging, and the experiments had even occasioned some harsh criticisms.[73] Meanwhile, news from Italy told of many successful cures induced through the use of electricity.[74]

Nollet spent about six months touring Italy, keeping a detailed account of his travels, beginning with his 4:00 a.m. departure on the Lyons coach on April 27.[75] His longest stay was at Turin where he was a guest of King Charles Emmanuel III for two and a half months. He once more lectured to the heir of the throne, the Duke of Savoy, and performed electrical experiments before the court.[76] Nollet visited a number of other cities and made detailed observations on a great variety of matters of interest to science, agriculture, and technology. Some of these were later reported to the Académie and appeared in the pages of the *Histoire* and *Mémoires*.[77] He also met with a number of savants, among them Maria Gaetana Agnesi (1718-1799), author of the very popular mathematical textbook *Instituzioni analitiche ad uso della gioventú italiana*.[78] From

Milan, Nollet continued on to Venice, Ferrara, and Bologna, where he was received at that city's famed Institute by savants who had come from different parts of Italy to welcome him. He went on to Florence and Rome where he had an audience with Pope Benedict IV (1674-1758).[79]

Nollet's observations were reported in the pages of the Histoire and Mémoires.[80] His findings on the use of electricity for medical reasons were unequivocal; the reports had been without any foundation. An investigation of these reports had been Nollet's primary objective in his visit, and he had sought out every electrician associated with the claims being made. He had either extracted admissions of failure or repeated the experiments in the company of the electricians themselves and reported on their inefficacy. Nollet also visited Pisa and reported his observations on the architecture of that city's inclined tower; he commented on the rock formations in the Piedmont and the use of limestone. With his thermometer at hand he concluded that the hot days in Italy were not really any hotter than in Paris. He also made a series of observations of interest to industry, such as on the use of myrtle for leather tanning, on the defoliation of mulberry trees, and the growing of hemp for rope-making. He commented in detail on his visit to Vesuvius and offered reasons for its eruption; and near Naples he visited the mysterious "grotte du chien," a cave supposedly filled with a poisonous air which had been the cause of death of many a wandering dog.[81]

Once back in Paris Nollet continued his lectures and experimentations. Almost fifty years old, he was now regarded by many as the doyen of experimental physics. His courses and demonstrations, especially in electricity, brought together the entertaining with the serious and continued to attract the interest of the educated public.[82] In 1753 a chair in experimental physics, the first ever in France, was created at the prestigious Collège de Navarre, and the King appointed Nollet to the position.[83] Four years later he was named physics professor at the royal military school at La Fère and in 1761 at the school at Mézières.[84] There, in addition to lecturing, he conducted experiments with artillery that resulted in his suggestions, published in the Mémoires, that some types of gunpowder being discarded as useless could indeed be utilized.[85]

He was appointed "Maître de Physique des Enfans de France" by Louis XV in 1758, a position which in fact made Nollet the official physics tutor to the royal grandchildren. Nollet, who had also tutored the Dauphin (d. 1765) "remplit les fonctions de ce poste honorable aupres les jeunes Princes," Fouchy wrote, "avec la même attention, & le même zèle, qu'il les avoit autrefois remplies auprès de leur auguste Père."[86] In 1757 he was appointed pensionnaire at the Académie, filling the position vacated by the death of Réaumur.[87] He was elected sous-directeur in 1761 and directeur in 1762.[88] While assuming these added responsibilities Nollet continued to devote time to the publication of new memoirs and the revision of old ones. In 1765 he produced an extensive article on the art of hat-making to the Académie's Descriptions des arts et métiers.[89] Nollet's contribution described in detail the state of the art of hat-making in France. It included a discussion of the materials used, their preparation, and the manufacture of hats proper.

The Descriptions des arts et métiers was a collection of works by Academicians meant to serve as manuals for the different French crafts. The initial intent of these descriptions, first formulated by a directive to the Académie in 1675 by the Minister Jean-Baptiste Colbert (1619-1683), was twofold. It was meant, first, to provide artisans throughout the kingdom with knowledge of the best methods employed in their craft, and second, with the aid of the Académie, to impose on these crafts higher standards of objectivity and precision. The directive remained lettre-morte until studies on the state of French metallurgy by Réaumur in the 1720's briefly revived the Académie's interest. It was not until 1761, under the direction of Henri-Louis Duhamel du Monceau, that the first volume of the Descriptions was published.[90]

Nollet seems to have maintained a full schedule until his last days. His three-volume Art des expériences, a detailed description of the construction and utilization of instruments, and the art of performing experiments, was published the year of his death.[91] At the beginning of the year he had been appointed, once more, sous-directeur of the Académie and on April 4, 1770 he was still attending its sessions.[92] He had even been appointed to a commission to look into the machines submitted to the Académie for approval for that year.[93] He died a few days later, on April 24, apparently from intestinal troubles, and was buried at Pimprez as he had requested.[94] His will, revised a few months before his death, indicates that while not rich, he had died financially comfortable.[95]

It is clear from this short outline of Nollet's scientific career that his interests ranged over a wide area. He was an experimental physicist, an instrument maker, a science popularizer, an electrician, a pedagogue and a scientific technician. To a large degree these many interests found unity in Nollet's notion of science and the scientist's role.

Nollet viewed the study of science, or more specifically physics, as an on-going process where advances were made by solidly basing and testing speculations against experiment and observation. He believed that it was the experimental method that had generated, and continued to generate, progress in science. Like many of his contemporaries he condemned the haste with which scientists build "systems" of thought. Modern physics, he believed, should salvage the 'kernels' of truth found in the works of the past, including those of systematists, but it should reject attempts to picture reality in a manner beyond the evidence of experiment and observation. The measure of good physics was its agreement with what could be demonstrated to the senses. The task of the individual scientist was to contribute his own share to this process. One of the purposes of this communal effort was to place knowledge at the service of man.

Flourishing at a time when French science was in large part characterized by a debate between Cartesianism and Newtonianism, Nollet advocated and attempted to pursue a type of physics that positioned him outside this debate. He believed that both of these philosophies had something positive to offer and that physicists should extract the viable elements from each. The following chapter is an examination of Nollet's views on this issue. Nollet, I argue, drew from both Newtonian and Cartesian tenets, constantly maintaining however his independence from either philosophy.

## CHAPTER I

## NOTES

[Throughout these citations, the following abbreviations are in use:

Histoire refers to Histoire de l'Académie Royale des Sciences de Paris;

Mémoires refers to Mémoires de l'Académie Royale des Sciences de Paris.]

[1]The main published biographical source for Nollet's life is still Jean-Paul Grandjean de Fouchy, "Eloge de M. l'Abbé Nollet," Histoir 1770 (1773), pp. 121-136; hereafter Fouchy, "Eloge." Among more recent works are Jean Torlais, Un physicien au siècle des lumières. L'Abbé Nollet, 1700-1770 (Paris: Sipuco, 1954); hereafter Torlais, Un physicien; V. Lecot, L'Abbé Nollet de Pimprez. Diacre, licencié en théologie, maître de physique et d'histoire naturelle des Enfants de France, professeur royal de physique au Collége de Navarre, membre de la Société des Beaux-Arts, de la Société Royale de Londres, de l'Académie des Sciences d'Erfort, de l'Institut de Bologne, sous-directeur de l'Académie des Sciences de Paris (Noyon: Imprimerie de Cottu-Harlay, 1856); hereafter Lecot, L'Abbé Nollet; G. Hector Quignon, L'Abbé Nollet, physicien. Son voyage en Piémont et en Italie (1749) d'après le manuscrit inédit de la Bibliothèque de Soissons. Extrait des Mémoires de l'Académie d'Amiens (Paris: Honoré Champion, 1905); hereafter Quignon, Nollet. There is some disagreement on Nollet's birth date. Fouchy, "Eloge," p.121, and Torlais, Un physicien, p.11, as well as Quignon, Nollet, p.2, all give November 19 as the birth date. However, the article "Nollet, (Jean Antoine)" in [Chaudon, Louis Mayeul, ed.], Nouveau dictionnaire historique, ou histoire abrégée de tous les hommes qui se sont fait un nom par le génie, les talens, les vertus, les erreurs, &c. depuis le commencement du monde jusqu'à nos jours; avec des tables chronologiques pour réduire en corps d'histoire les articles répandus dans ce dictionnaire. Par une société de gens de lettres (5 vols.; Paris: LeJay, 1772), IV, 737-739, at p. 737, gives November 17 as his birth date, while Lecot, L'Abbé Nollet, p. 3, gives December 19, apparently confusing the day of baptism with the birthdate.

[2]Torlais, Un physicien, p. 11; Fouchy, "Eloge," p. 121. Pierre Nollet, whom Lecot (L'Abbé Nollet, p. 3n) considered to be Jean Antoine's brother was, in fact, his cousin. See Torlais, Un physicien, pp. 11, 245.

[3]Both Lecot, L'Abbé Nollet, and Torlais, Un physicien, add some details about Nollet's early life but fail to document them. Some of these details are apparently taken from Alexandre Savérien, Histoire des philosophes modernes avec leur portrait (7 vols.; Paris: Brunet, 1760-1769), Vol. VI: Histoire des physiciens; hereafter Savérien, Histoire.

[4]Fouchy, "Eloge," p. 121.

[5]Torlais, Un physicien, p. 12.

[6]Lecot, L'Abbé Nollet, pp. 3-4.

[7]Ibid., p. 4.

[8]Torlais, Un physicien, p. 13.

[9]Ibid., p. 15; and Fouchy, "Eloge," p. 121.

[10]Nouveau dictionnaire historique, p. 737.

[11]Fouchy, "Eloge," p. 122.

[12]Ibid.

[13]Ibid.

[14]Fouchy, "Eloge de M. Clairaut," Histoire, 1765 (1768), pp. 144-159; and "Clermont (Louis de Bourbon-Condé, Comte de)" in Biographie universelle, ancienne et moderne (52 vols.; Paris:Chez L. G. Michaud, 1811-1828), IX, 86-88.

[15]J. L. Heilbron, Electricity in the 17th and 18th Centuries. A Study of Early Modern Physics (Berkeley, Los Angeles, London: University of California Press, 1979), p. 279; hereafter Heilbron, Electricity; Fouchy, "Eloge," p. 123.

[16]Four memoirs appeared in the pages of the Mémoires for 1733. Two more appeared in the Mémoires for 1734. Dufay later published two more papers on electricity. See Bibliography for full references.

[17]Jean Antoine Nollet, Programme ou idée générale d'un cours de physique expérimentale, avec un catalogue raisonné des instrumens qui servent aux expériences (Paris: Chez P. G. Le Mercier, 1738), pp. xiv-xv; hereafter Nollet, Programme. See also Jean Torlais, Un esprit encyclopédique en dehors de "l'Encyclopédie": Réaumur, d'après des documents inédits (Paris: Albert Blanchard, 1961, rev. ed.), p. 81; hereafter Torlais, Réaumur.

[18]"Chambre obscure de nouvelle construction, inventée par M. l'Abbé Nolet" (1733, n°405), and "Machine pour tailler les verres de lunettes, inventée par M. L'Abbé Nolet" (1733, n°406) in Académie Royale des Sciences, Machines et inventions approuvées par l'Académie Royale des Sciences, Vol. VI (Paris: Gabriel Martin, Jean-Baptiste Coignard, Fils,

& Hippolyte-Louis Guerin, 1735), pp. 125-6 and 127-8 respectively.

[19] Nollet, Programme, p. xvi.

[20] Yves Laissus, "Le Jardin du Roi," in Enseignement et diffusion des sciences en France au XVIII^e siècle, ed. by René Taton (Paris: Hermann, 1964), pp. 287-341;this volume hereafter cited as Taton, Enseignement; and Bernard le Bovier de Fontenelle, "Eloge de N. du Fay," Histoire, 1739 (1741), pp. 73-83; hereafter Fontenelle, "Dufay."

[21] Heilbron, Electricity, pp. 159-161.

[22] Nollet, Programme, p. xvii.

[23] Ibid.

[24] Nollet's success as a lecturer and instrument maker will be attested to on a number of occasions below. As we shall see in chapter three, public lectures in experimental physics had been performed in Paris at least since the middle of the seventeenth century and were quite popular.

[25] [Noël-Antoine Pluche], Le spectacle de la nature, ou entretiens sur les particularités de l'histoire naturelle, qui ont paru les plus propres à rendre les jeunes-gens curieux, & à leur former l'esprit (8 vols. in 9; 6th ed.; Paris: Chez la Veuve Estienne, 1737-50), Vol. IV: Contenant l'histoire de la physique expérimentale (1739), p. 452; hereafter Pluche, Spectacle.

[26] Jean Antoine Nollet, Leçons de physique expérimentale ([4th ed.]; 6 vols.; Amsterdam & Leipzig: Chez Arkstée & Merkus, 1754-1765), I, xiin; hereafter Nollet, Leçons. The following translation, and all others offered in the footnotes are mine: ". . . so that professors may use them later to cultivate and to teach physics by way of experiments."

[27] Jean Torlais, "La physique expérimentale," in Taton, Enseignement, pp. 619-645, at p. 631; hereafter Torlais, "Physique expérimentale."

[28] Voltaire, Correspondance. Texte établi et annoté par Theodore Besterman (2 vols.; Paris: Bibliothèque de la Pléiade, Gallimard, 1963-1965), II, 136; hereafter Voltaire, Correspondance.

[29] Idem, Correspondance, I, 1048. "A l'Abbé Moussinot. "We are in a century in which one cannot be a savant if one has no money."

[30] Idem, Correspondance, I, 1094. Voltaire expressed his dissatisfaction to Moussinot on a number of occasions; see Correspondance, I, 1078, 1086 and 1093. "It is not with an ordinary man at all that we are dealing. He is a philosophe, a man of true merit who alone can provide me with my laboratory instruments and it is much easier to find money than a man like him."

[31] Ibid., I, 1181.

[32] Fouchy, "Eloge," p. 123.

[33] Nollet, Programme, p. xxxvii. ". . . auditors who may want to add some reading to the inspection of experiments to have more extensive explanations than those afforded me by the limits of time."

[34] Ibid., p. xxxix. ". . . who, unable to attend [my experiments], may want to imitate them or form a plan of study where they suppose them."

[35] Ibid., p. xxxvii. "I here indicate in detail the subjects treated in every lesson, the operations that serve as proofs, the phenomena that they explain, the applications which are made or might be made."

[36] Op. cit.; see note 26, and the Appendix. The Leçons were republished many times.

[37] "Programme ou idée generale . . . ," Journal des sçavans, 1738, pp. 624-629, at p. 624.

[38] Ibid. "The number of disciples increases day by day; and the master perfects his art more and more. Moreover, courses in experimental physics could not fail to succeed. This establishment brings together the useful and the agreeable . . . and foreigners whose love of science attracts them to Paris are surprised not to find there an establishment like it."

[39] Mémoires pour l'histoire des sciences & des beaux arts [Mémoires de Trevoux], 1738: Vol. II [June], pp. 1145-46, and Vol. IV [November], pp. 2228-2236.

[40] Ibid., 1738, Vol. IV, p. 2228. "Even though we have previously announced the publication of this work which is already well known, we cannot but enter into the details of what it contains and what it promises."

[41] Ibid., p. 2229. ". . . M. Nollet has found that happy secret of making physics speak to the eyes. . . ."

[42] Ibid., p. 2231. ". . . has succeeded in forming a very rare laboratory, which France lacked, and a workshop where one can construct all that is necessary for experimental physics."

[43] Nollet, Programme, pp. xxiv-xxv. "I expose in few words the present state of the question; I prove my propositions by relevant operations; I indicate the applications to which the most ordinary phenomena can be put; and the appropriate readings for those who want more extensive explanations."

[44] Ibid., p. xxiii. ". . . it seemed more convenient to render expressions more familiar, to develop a habit of talking while conducting

demonstrations, and to limit the use of words and to rely on the demonstration of facts to make ourself understood."

[45]Ibid., p. iv, "This science is no longer, as in times past, a vain assembly of unfounded thoughts, or of chimerical systems; conjectures are given the rank they deserve; one now believes only that which one sees, and reason does not decide except on the basis and testimony of experiments."

[46]Nollet, "Mémoire sur les instruments qui sont propres aux expériences de l'air," Mémoires, 1740 (1742), pp. 385-432; "Sur les instruments qui sont propres aux expériences de l'air. Seconde partie. De la construction d'une nouvelle machine pneumatique de raréfaction à deux corps de pompes," Mémoires, 1740 (1742), pp. 567-585; "Sur les instruments qui sont propres aux expériences de l'air. Troisième partie. Des instruments qui assortissent la machine pneumatique de raréfaction," Mémoires, 1741 (1744), pp. 338-362; and "Sur la vapeur qu'on apperçoit dans le récipient d'une machine pneumatique, lorsqu'on commence à raréfier l'air qu'il contient," Mémoires, 1740 (1742), pp. 243-253.

[47]Nollet, "Mémoire dans lequel on examine par voie d'expérience, quelles sont les forces & les directions d'un ou de plusieurs fluides renfermés dans une même sphere qu'on fait tourner sur son axe," Mémoires, 1741 (1744), pp. 184-198.

[48]René Descartes, Les principes de la philosophie de René Descartes, quatriéme edition. Reveuë & corrigée fort exactement par Monsieur CLR (Paris: Chez Théodore Girard, 1681), part IV, chaps. 20-24. Republished in Charles Adam and Paul Tannery, eds., Oeuvres de Descartes, Vol. IX (Paris: Léopold Cerf, 1904), pp. 210-12; hereafter Descartes, Principes. Descartes had not explained fall toward the center by the simple action of one rotating vortex. In the Principes, part IV, chap. 27, he postulated that the reason bodies tend toward the center of the Earth was because parts of the sky (Ciel) move in many directions simultaneously so that they extend their motion in different directions. The Earth, "par sa dureté," repulses their movements so that they tend to move away from it in right lines from its center. See Huygens' explanation in Discours de la cause de la pesanteur (Leide: Chez Pierre Vander Aa, 1690), pp. 134-36, republished in Oeuvres complètes de Christiaan Huygens publiées par la Sociéte Hollandaise des Sciences (La Haye: Martinus Nijhoff, 1888-1950), Vol. XXI: Cosmologie (1944), pp. 454-455; hereafter Huygens, Pesanteur.

[49]Fontenelle gave a brief history of the issue in "Sur les tourbillons cartésiens," Histoire, 1741 (1744), pp. 1-10. See also E. J. Aiton, The Vortex Theory of Planetary Motions (New York: American Elsevier, 1972), esp. pp. 55-58, 75-85, and chapter seven, pp. 152-93; hereafter Aiton, Vortex Theory; and Brunet, L'introduction des théories de Newton en France au XVIIIe siècle avant 1738 (Paris: Librairie scientifique Albert Blanchard, 1931), pp. 153-202; hereafter, Brunet, L'introduction.

[50]George Bernard Bulffinger, De causa gravitatis physica generali disquisitio experimentalis (Paris, 1728). Not seen; hereafter Bulffinger, De causa. Mentioned in Aiton, Vortex Theory, pp. 155, 168-171, and Brunet, L'introduction, pp. 153-157.

[51]The question the Académie proposed was appropriately phrased: "What is the physical cause of weight?"

[52]Brunet, L'introduction, pp. 154-155.

[53]Fontenelle, "Sur les tourbillons cartésiens," Mémoires, 1741 (1744), p. 7.

[54]Ibid., p. 8.

[55]See note 47.

[56]Ibid.

[57]Savérien, Histoire, pp. 234-235. Torlais repeats the story in Un physicien, p. 68.

[58]Ibid.

[59]Ibid., and Torlais, Un physicien, p. 69.

[60]See the Appendix for a brief discussion of problems in dating the publication of Nollet's Leçons.

[61]"Leçons de physique de M. l'Abbé Nollet de l'Académie des Sciences & de la Société de Londres," Journal des Sçavans, 1744, pp. 17-23; "Leçons de physique expérimentale, par l'Abbé Nollet," Mémoires pour l'histoire des sciences & des beaux arts [Mémoires de Trévoux], 1744, Vol. III, pp. 1390-1418; "Leçons de physique expérimentale, tom. I & II", Histoire, 1743 (1746), pp. 27-28.

[62]Nollet, Lezioni di fisica sperimentale del sig. abate Nollet (3 vols.; Venice: Presso Giambatista Pasquali, 1746-1747); Lecciones de physica experimental (6 vols.; Madrid: J. Ibarra, 1757). (The latter translation not seen.)

[63]Nollet, "Conjectures sur les causes de l'électricité des corps," Mémoires, 1745 (1749), pp. 107-151; hereafter Nollet, "Conjectures". In the Leçons electricity was the subject of the last two lessons of volume six.

[64]Ibid.

[65]The "Conjectures" and what novelties they introduced in electrical theory will be discussed in Chapter Four.

[66]Heilbron, Electricity, p. 288; Roderick Home, "Historical

Background," in Aepinus's Essay on the Theory of Electricity and Magnetism. Introductory Monograph and Notes by R. W. Home. Translation by P. J. Connor (Princeton: Princeton University Press, 1979), pp. 65-106; hereafter Home, Aepinus; and Home, "Electricity in France in the Post-Franklin Era," Actes du XIVe Congrès International d'Histoire des Sciences (Tokyo, 1975), vol. II, pp. 269-72; hereafter Home, "Post-Franklin Era."

[67] See the bibliography for Nollet's works concerning electricity.

[68] Nollet, "Mémoire sur l'ouie des poissons, et sur la transmission des sons dans l'eau," Mémoires, 1743 (1746), pp. 199-224.

[69] Nollet, "Recherches sur les causes du bouillonnement des liquides," Mémoires, 1748 (1754), pp. 57-104.

[70] "Observations de physique générale, Histoire, 1765 (1768), p. 26; "Observations anatomiques, Histoire, 1754 (1759), pp. 66-70; and Nollet, "Recherches sur les moyens de suppléer à l'usage de la glace dans les temps et dans les lieux où elle manque, Mémoires 1756 (1762), pp. 82-106.

[71] Nollet, "Expériences et observations faites en différens endroits de l'Italie," Mémoires, 1749 (1753), pp. 444-488; hereafter Nollet, "Observations en Italie."

[72] Morand and Nollet, "Expériences de l'électricité appliquée a des paralytiques," Mémoires, 1749 (1753), pp. 28-39.

[73] Nollet, "Observations en Italie," p. 444.

[74] Quignon, Nollet, p. 26.

[75] Ibid., pp. 29-30.

[76] Nollet, "Observations en Italie."

[77] Quignon, Nollet, p. 36.

[78] Quignon, Nollet, p. 36.

[79] Ibid., p. 40.

[80] Nollet, "Observations en Italie;" and "Sur l'effet de l'électricité appliquée à la guérison de quelques maladies, Histoire, 1749 (1753), pp. 11-27.

[81] Ibid.

[82] Torlais, Un physicien, pp. 161-200.

[83] Fouchy, "Eloge," p. 133.

[84] Ibid.

[85] Nollet, "Expériences sur la poudre à canon employée en différens états," _Mémoires_, 1767 (1770), pp. 109-118.

[86] Fouchy, "Eloge," p. 133.

[87] Ibid.

[88] Institut de France, _Index biographique des membres et correspondants de l'Académie des Sciences du 22 Décembre 1666 au 15 Novembre 1954_ (Paris: Gauthier-Villars, 1954), p. 381. It was customary for a _sous-directeur_ to be elected _directeur_ the following year.

[89] Nollet, "Art du chapelier," _Descriptions des arts et métiers, faites ou approuvées par messieurs de l'Académie des Sciences de Paris. Avec figures en taille-douce. Nouvelle edition publiée aves des observations, & augmentée de tout ce qui a été écrit de mieux sur ces matieres, en Allemagne, en Angleterre, en Suisse, en Italie_, Vol. VII: _Contenant l'art de la draperie, l'art de friser ou ratiner les étoffes de laine, l'art de faire les tapis, façon de Turquie, l'art du chapelier, l'art du tonnelier, l'art de convertir le cuivre en laiton, & l'art de l'épinglier_ (Neuchâtel: La Société Typographique, 1777), pp. 225-322, plus 4 plates. Added to the article by Nollet are two letters by a M. Thierry, Nollet's reply to the first, and a résumé of a procès-verbal concerning the craft of hat making, pp. 322-344.

[90] For a bibliogrpahical and historical essay on the _Descriptions des arts et métiers_ see Arthur H. Cole and Georges B. Watts, _The Handicrafts of France as Recorded in the Descriptions des arts et métiers, 1761-1788_, Kress Library of Business and Economics, Publication 8 (Boston: Baker Library, Harvard Gradaute School of Business Administration, 1952).

[91] Nollet, _L'art des expériences, ou avis aux amateurs de la physique, sur le choix, la construction et l'usage des instruments; sur la préparation et l'emploi des drogues qui servent aux expériences_ (3 vols; 2nd ed.; Paris: Chez P.E.G. Durand, 1770). Hereafter, Nollet, _L'art des experiences_.

[92] Fouchy, "Eloge," p. 135.

[93] Torlais, _Un physicien_, p. 244.

[94] Ibid., and Fouchy, "Eloge," p. 135.

[95] Nollet's will was published by Quignon, _Nollet_, pp. 64-67, and Lecot, _L'Abbé Nollet_, pp. 74-76.

# CHAPTER II

## NOLLET'S POSITION IN THE CARTESIAN-NEWTONIAN DEBATE

Modern histories have identified Nollet variously as a Newtonian or Cartesian. One of France's leading lecturers and advocates of experimental physics, and also a leading student of electricity for a number of years, he has deserved the attention of historians of electricity and physics. Historians of electricity have tended to identify Nollet with the Cartesians. This was the opinion of I. B. Cohen, and also that of Roderick Home who, while critical of the overuse of the "Newtonian-Cartesian" categories, identified Nollet as a Cartesian and anti-Newtonian.[1] Historians who have looked at Nollet as an experimental physicist have tended to be influenced by Pierre Brunet's assessment of the Abbé as a Newtonian.[2] This was, for example, the opinion of K. M. Baker in his study of Marie-Jean-Antoine-Nicolas Caritat, Marquis de Condorcet (1743-1794), where he referred to Nollet as a Newtonian protagonist.[3] Brunet pictured Nollet as the man most responsible for the introduction of experimental Newtonianism into France. Nollet, Brunet wrote, learned this new method and approach to science through first-hand contacts, and later through correspondence, with Wilhelm 'sGravesande and Pieter van Musschenbroek during his visit to Leyden in 1735. Nollet carried back to France their Newtonianism without wearing the badge so that he could practice his Newtonian physics without incurring the wrath of the

Cartesians. Thus Nollet's repeated statements that he was neither a Cartesian nor a Newtonian were, according to Brunet, simple subterfuge.[4]

I will argue below that these categorizations of Nollet have been possible because in a sense he was both a Cartesian and a Newtonian. Nollet accepted and advocated many of the tenets associated with Newtonianism. He adopted Newton's optical theory from an early date; he accepted Newton's use of gravitational theory to explain the movement of the celestial bodies; and he looked favorably on what he considered to be the experimental nature of Newton's physics. On the other hand, Nollet's physics was basically one based on the mechanics of impulsion. He believed that gravitational attraction was reducible to impulsion, and he preferred Descartes' theory of light. If he was both a Cartesian and a Newtonian, one could argue that he was neither. More importantly, however, Nollet consciously pursued a type of experimental physics which he believed demanded an avoidance of commitment to either of these philosophies, or to any physical system bearing the character of a sect. In this chapter I will examine Nollet's position with respect to his Newtonian and Cartesian contemporaries and the issues that preoccupied them. In the following chapter I will examine his views on experimental physics.

Throughout most of the first half of the eighteenth-century, scientific activity in France was characterized in large part by a debate over method in general as well as specific issues about the natural world. Much of the background to this debate and much of the story has been told by Pierre Brunet, Alexandre Koyré, and others who have looked into the introduction of Newtonian theories into Cartesian France.[5] Nollet, I believe, owed much of his reputation among his contemporaries to the fact that he advocated and exercised a type of physics that positioned him outside this debate. The esteem he acquired resulted more from the manner in which he practiced physics than from any results he obtained. At a time when physics was perceived by many to have become, to use the words of Claude Buffier (1661-1737), "un amas de conjectures plus ou moins ingénieuses; ce qui fait d'une partie de la phisique, moins une sience [sic] qu'une sorte de vraisemblance,"[6] Nollet's ability to visibly and concretely demonstrate a vast array of scientific "facts" was appreciated by the public and the academicians. He himself, I will argue, regarded both Newtonianism and Cartesianism as "systems" that were to be appreciated only insofar as they agreed with experimental physics.

The classification of French scientists in the eighteenth century as either Cartesians or Newtonians has come under attack recently by historians who argue that these categories often prove misleading or uninformative. Home, in the work referred to above, showed that the application of the "Newtonian" label to scientists studying magnetism in that period does not guide us to their thoughts but, in fact, misleads us. Having selected a large "Newtonian" test-group interested in magnetism, Home showed how almost to a man they adopted a position the very opposite of what we might expect on the basis of this Newtonian labelling.[7] Schofield, in <u>Mechanism and Materialism</u>, while tracing the Newtonian legacy in British natural philosophy, made it clear that Newtonianism was, to a large extent, what "Newtonians" chose to make it. More recently he has gone further in his questioning of these categories and has, not without some humor, suggested a more evolutionary taxonomy to describe the activities and thoughts of scientists of the period.[8] Others who have looked at individual men of the French Enlightenment also found these descriptions too restrictive or uninformative. T. L. Hankins held this view in his study of Jean Le Rond d'Alembert (1717-1783), as did L. N. Marsak in his study of Fontenelle, and A. Vartanian in his study of Denis Diderot (1713-1784). Vartanian argued that Diderot is to be more easily understood as a Cartesian than a Newtonian. In a short but incisive criticism of the misuses of these terms, P. M. Heimann drew attention to the need for extra care in their use and for the incorporation of their significance within eighteenth-century thought in general.[9]

More recently, the approaches of both critics of and sympathizers with these categories have come under attack by Simon Schaffer.[10] He argued that even those "attempts to demonstrate rival, anti-Newtonian groups of natural philosophers remain defined by the contrast with Newtonianism rather than being seen as representative of a distinct philosophy in their own right."[11] Another point made by Schaffer is that as long as the discourse on eighteenth-century natural philosophy is limited to discussions of matter-theory, it will remain arbitrarily restricted to these types of discussions. What is needed, Schaffer suggested, is not a more careful use of the "Newtonian" and "Cartesian" categories, but a reevaluation of the historiography that generated them. The problem will remain, Schaffer argued, as long as natural philosophy in the eighteenth century is treated as if it were a distinct field of discourse.

I believe these criticisms to have been well made. They should serve to alert us to the use of broad and vague labels and categories that we, in applying them, further confuse. For these reasons it is desirable to make clear what is here meant by these labels. By "Cartesians" and "Newtonians" are meant those who identified themselves, however loosely, as such and who were identified by their contemporaries as belonging to one or the other group in the debate. Examples of Newtonians are Voltaire, Clairaut, Pierre Louis Moreau de Maupertuis (1698-1759) and Pierre Sigorgne (1719-1809). Examples of Cartesians are Fontenelle, Louis Bertrand Castel (1688-1757), Jean Jacques Dortous de Mairan (1678-1771), and Jean Banières (1700-?). Whether they were really Newtonians or Cartesians, and what that may mean, is not for the moment our concern. This is not to deny, however, that each group shared a communality of beliefs. However, these communalities tend to become elusive as we try to specify them. So, rather than attempt to lay new parameters for the Newtonian and Cartesian positions, I have let them unfold as they developed in this narrative.

Newton's works made their entrance into France soon after Cartesianism had just become comfortably installed after a long protracted war with the Peripatetic philosophy. In the 1720's, when Nollet studied in Paris, Cartesian mechanical philosophy was in the process of consolidating its victory over Aristotelianism in the colleges of the University of Paris.[12] This was a victory already secured in scientific circles, the Académie des Sciences and the Parisian salons. Textbooks, lecture notes, and scientific publications from that period reflect enthusiastic support for the fundamental principles of Cartesianism. From early on, however, Descartes' physics had not been above criticism from Cartesians who were against particular aspects of the theories of the founder of their school. Professors at the Paris collèges were aware of the constant challenges brought against some facets of Cartesianism on a number of grounds. The philosophy of Descartes, in fact, entered the University of Paris in the company of some of its major critics, among them Huygens and Edmé Mariotte (d.1684). Their views were discussed and incorporated into the lectures. When consensus on any specific point was lacking, professors chose to present the many different opinions on the subject rather than commit themselves to any one view. Where Descartes met with almost universal criticism, such as in his theory of colors, the views of his critics prevailed.

Nevertheless, it was the new mechanical philosophy of Descartes that dominated the scientific life of Paris--inside as well as outside the University. When criticisms were brought against it, they were offered to correct some facet of the new philosophy and almost never to challenge it. Popular lectures in experimental physics in the tradition of Jacques Rohault (1620-1675) enhanced and broadened the appeal of Cartesianism. While in this period experimentalism in the classrooms was limited mostly to textbook discussions of experiments, students had available to them lectures in experimental physics offered by private teachers.[13] During the period of Nollet's studies the most famous of these lecturers was Polinière, a man Nollet may have come to know personally through ocntacts at the Société des Arts.[14]

It is impossible to gauge to what extent Nollet, in his student years, became acquainted with the new philosophy and the many authors who discussed it. No helpful information of any kind, to my knowledge, is available on this matter.[15] What is known, however, is that there was accessible to him a wide range of courses and publications in the new science, including some aspects of Newtonian physics. Newtonianism, while not taught at the University of Paris until later, was known to many of the professors, and Newton's theory of colors was accepted by some at least as early as 1726.[16] However well Nollet may have become acquainted with the scientific knowledge available to him in his student years, certainly by 1738, the year of his first publication, he had ample opportunities to become immersed in the new philosophy. Outside the University Newtonian works were scarce but available and the subject of much discussion.[17]

As early as 1707 the Abbé Philippe Villemot (1651-1713) made it a point to clarify to the reader of his *Nouveau système, ou nouvelle explication du mouvement des planètes* that similarities between his work and that of Newton were coincidental.[18]

> Je suis bien aise de remarquer . . . qu'encore qu'il y ait déja quelque tems, que Mr. Nevvton ait publié des principes physiques d'Astronomie, je n'ai pû faire aucun usage de ses découvertes; puisque son livre, qui est trés rare en ce païs, ne m'est tombé entre les mains, qu'aprés la composition de mon ouvrage.[19]

The disclaimer was appropriate, for the new system Villemot was introducing was meant to deal with an important discrepancy he had discovered between Kepler's law establishing a relation of the distance of the planets to their periods and the Cartesian *tourbillons*. This discrepancy was, of course, a point of departure for Newton's criticisms of Descartes' mechanics. The similarities between the works of Villemot and Newton ended there. The *Nouveau système* was in fact an attempt to reconcile the *tourbillons* with Kepler's law.[20]

Until the 1730's practically all acknowledgements of Newton's works in France--except for his theory of colors--were by Cartesians defending the mechanical philosophy against the criticisms levelled against it in the *Principia* and the *Opticks*.[21] However, as Brunet has suggested, the vehement defense of Cartesianism is an indication of how much Newton's works and his criticisms of Descartes had become known and how much of a serious threat they were considered to be.[22] As Cartesians maneuvered to defend their vortices (tourbillons) and plenum against attacks, their world became filled with complicated new mechanisms. The attractive simplicity of the mechanical philosophy was lost in the complexities introduced to deal with apparent discrepancies.

The Newtonian alternative was not, however, considered very attractive, and for a time the more attractive option was to improve Cartesian mechanics. Jean-Baptiste Senac (ca. 1693-1770) was correct in stating in the introduction to his 1722 Nouveau cours de chymie suivant les principes de Newton et de Stahl that "Si M. Newton dit qu'il n'est pas content de la philosophie cartésienne, on ne doit pas en être surpris: il ne dit rien en cela que ne disent tous ceux qui ont [sic] examiné."[23] However, criticizing Descartes was one thing, accepting an alternative that to many was worse, was something else.

What Newton had done, in the eyes of many, was to abandon ship too early and return to the obscurantism of the scholastics. "Ils lui reprochent," wrote Antoine Augustin Bruzen de la Martinière (1662-1746) in 1731, "que malgré l'air de nouveauté qu'il a sçu donner à son systéme, il en revient aux principes obscurs d'Aristote, & qu'ils les rétablit sous d'autres noms."[24] While the Cartesian Castel was willing to accept some of Newton's criticisms of Descartes, he, like other Cartesians, believed Newton had gone too far. "Il auroit pu se contenter," Castel wrote, "de réfuter les Tourbillons de Descartes;" something Castel judged Newton had done "assés bien." However, "il a passé certainement le but, en réfutant les Tourbillons tout court. . . ."[25] Castel argued that without material vortices Newton's world would literally collapse, the moon would fall to the earth, "les Satellites sur Saturne, sur Jupiter, & toutes les Planetes sur le Soleil."[26] Castel elaborated, bringing geometry to his aid, on the argument already made by Leibniz that the Newtonian universe demanded the continuous intervention of the Clock Maker.[27] Without that intervention the happy equilibrium Newton described would eventually come to an end. This argument, in varied forms, would be a key criticism of the Newtonian world system. But more flagrantly repugnant to Castel, and to most Cartesians, was the notion of attraction at a distance. How could matter act where it was not, and through no intermediary?[28]

Cartesians did not deny, Castel pointed out, that there were problems in their system. There was a difference, however, between what Descartes had offered and Newtonianism. The problems in the Newtonian system did not make it worth salvaging. Throughout the third and fourth decades of the century a main concern of the Cartesians was to attempt to reconcile their physics with the objections raised against it.[29] However, by 1728 Fontenelle wondered if "l'ingénieux système des tourbillons de Descartes, & qui si présente se agréablement à l'esprit, tombera accablé sous les difficultés qu'on lui oppose;" and whether philosophers would be forced to adopt another system "qui a des difficultés aussi grandes, & plus frappantes, quoiqu'il ait des faces fort avantageuses."[30]

The fact that philosophers were confronted with these two imperfect choices had already been underscored by Fontenelle a year earlier. In his well-known "Eloge" of Newton published in the Histoire for 1727, Fontenelle contrasted the methodologies of the two great scientists bringing out the shortcomings of both the deductive-rationalist Cartesian approach and the inductive-experimentalist ideal of Newton.

> Les deux grand hommes, qui se trouvent dans une si grande opposition, ont eu de grand rapports. Tous deux ont été des génies du premier ordre, nés pour dominer sur les autres esprits, & pour fonder des empires. Tous deux géometres excellens ont vû la nécessité de transporter le géométrie dans la physique. Tous deux ont fondé leur physique sur une géométrie, qu'ils ne tenoient presque que de leurs propres lumieres. Mais l'un, prenant un vol hardi, a voulu se placer à la source de tout, se rendre maître des premiers principes par quelques idées claires, & fondamentales, pour n'avoir plus qu'à descendre aux phénomenes de la nature, comme à des conséquences nécessaires; l'autre plus timide, ou plus modeste, a commencé sa marche par s'appuyer sur les phénomenes pour remonter aux principes inconnus, résolu de les admettre quels que les pût donner l'enchaînement des conséquences. L'un part de ce qu'il entend nettement pour trouver la cause de ce qu'il voit. L'autre part de ce qu'il voit pour en trouver la cause, soit claire, soit obscure. Les principes évidens de l'un ne le conduisent pas toûjours aux phénomenes rels [i.e. tels] qu'ils sont; les phénomenes ne conduisent pas toûjours l'autre à des principes assez évidens. Les bornes, qui dans ces deux routes contraires ont pû arrêter deux hommes de cette espece, ce ne sont pas les bornes de leur esprit, mais celles de l'esprit humain.[31]

Although Fontenelle had skillfully maneuvered to bring Descartes into an éloge meant after all to pay tribute to Newton, French Newtonians had, in a sense, won a small victory. Newton and Descartes had been put on the same footing. Both of their methods had failed; although their failures were not due to either man, but to the "limitations of the human mind itself." While the Royal Society was incensed at the parallel Fontenelle had drawn between what it considered to be the defunct and bankrupt philosophy of Descartes and the philosophy of its former President, French Cartesians were themselves no less incensed. Banières years later expressed their sentiments in his Examen et réfutation des élémens de la philosophie de Neuton de M. de Voltaire:

> Nous avons entendu dire qu'on avait été choqué de la comparaison que M. de Fontenelle à [sic] fait de M. Descartes & de M. Neuton

> dans l'éloge qu'il fit de ce dernier, & qu'il prononça dans l'Académie Royale des Sciences, dont M. Neuton étoit membre. Peut-être qu'on n'a pas eu tout à fait tort de se récrier. Mais ce qui paroîtra suprenant, c'est que ceux qui devoient être naturellement choqués de la comparaison, n'ont rien dit, & que ceux qui devoient sçavoir bon gré à M. de Fontenelle de ce qu'il avoit élevé M. Neuton jusqu'à M. Descartes soient précisément ceux qui se sont récriés.[32]

The Newtonians, in other words, should have been thankful. Fontenelle had compared Descartes, who was a "grand Géométre & grand Philosophe," to Newton who was but a "grand Géométre & grand observateur."[33] Banières' Examen et refutation was written in response to Voltaire's Elémens de la philosophie de Neuton, a popular account of Newtonianism and it, in turn, was inspired, to an extent, by Voltaire's reading of Fontenelle's éloge of Newton.[34] This va-et-vient, the pitting of Newton against Descartes, and vice-versa, reflected two different concepts of the nature of science and the world, as well as a dispute over a whole array of specific issues on which agreement could not be reached. What caused the tides and what kept the Moon in its orbit? What was the nature of light and what differentiated colors? Why did heavy bodies fall and what was the shape of the Earth? And so on. Underlying most Newtonian answers to these questions was a conception of a scientific world distinct from the inaccessible and almost chimerical real world. The world the Newtonian scientist believed accessible to him, and hence the only one worth being studied, consisted of a nexus of interrelated phenomenological givens. It was the task of science to identify and codify these data of experience. Underlying most Cartesian answers was a conception of the scientific world as identical with the real one. Cartesians insisted that the only world worth knowing was the real one, and the pursuit of that knowledge the only worthwhile scientific endeavor.

However well Newtonian figures and numbers were brought into a harmonious whole they remained a construct of man's mind imposed on the world. It was the task of scientists to unveil the true construct of the world itself.[35] How these two perceptions of the scientists' role affected the specific issues will be discussed below, and in the following chapter, and only then can we hope to elucidate this debate on method further. However, while these general methodological discussions underlay the Cartesian-Newtonian debate, scientists were identified with one or the other side depending on the positions they took on the issues in dispute and not upon methodological discussions alone.

While Cartesians and Newtonians were both willing to admit shortcomings in their approach to science, both sects believed that the answer was to deal with the incongruities and to save the total structure. On the other hand, to Nollet, as well as to others whom I shall mention shortly, the better answer was to discard all conjectures and emphasize experimental observations. Bruzen de la Martinière spoke for them when he introduced his chapter on physics with the judgement that "Nous sommes encore bien éloignez d'avoir une Physique génerale universellement approuvée, il faudroit pour cela un plus grand nombre d'Experiences que nous n'avons."[36] If that meant that we should wait a century or two before we could discover the true nature of the world, so be it: at least we would then know for sure. Modern physicists, he judged, made the mistake of first constructing a system and then applying experiments to it.

> Les Physiciens tombent d'ordinaire dans un défaut, ils bâtissent un systéme, comme j'ai dit, & y appliquent les experiences. Descartes a fait cette faute. Il falloit au contraire rassembler les expériences, recueillir les veritez qu'elles démontrent, &

attendre qu'il y eût assez de véritez, pour en former un systéme.[37]

The advice for caution and reliance on experience was, together with the deprecation of systems, commonplace in prefaces.[38] Buffon, in the introduction to his translation of Stephen Hales' Vegetable Statics, expressed the common opinion: "C'est par des Expériences fines, raisonnées & suivies, que l'on force la Nature à découvrir son secret." All other methods, Buffon judged, "n'ont jamais réussi." The true physicist cannot but regard "les anciens systêmes, comme d'anciennes rêveries."[39]

The attack on systems was usually directed against Cartesianism by those whose sympathies leaned toward Newtonianism. The Encyclopedie's article "Systême"--based in large part on Condillac's Traité des systêmes--accused Cartesianism of making the liking for abstract, hasty system-building fashionable. "Le Cartésianisme qui avoit succédé au Péripatétisme, avoit mis le goût des systêmes fort à la mode."[40] The article was written sometime before 1760 and by then its author felt he could add the comforting thought that "Aujourd'hui, grace à Newton, il paroît qu'on est revenu de ce préjugé, & qu'on ne reconnoît de vraie physique que celle qui s'appuie sur les expériences, & qui les éclaire par des raisonnemens exacts & précis, & non pas par des explications vagues."[41]

Although the systematic, conjectural philosophy was often associated with the Cartesians, Newton and Newtonians did not escape similar accusations. Accusations of occultism and the use of unwarranted hypotheses levelled against him had already led Newton to delete the word "hypothesis" from the Principia in later editions, and to deny, in the 1713 edition, that he feigned any hypothesis.[42] However, that was not enough to quell the accusations of occultism and hypothetical reasoning. Banières, in his Examen et réfutation, derided the notion that the Newtonian attraction was, as the Newtonians claimed, more solidly based on observation than the Cartesian principles. For, after all, "le sistême de l'attraction n'est que le sistême de l'impulsion renversé. . . ."[43] Whatever merit attraction had was no surprise: ". . . on ne doit pas être surpris si tout ce qui a été démontré de l'impulsion s'accorde avec l'attraction," for the same effects will occur "soit qu'on suppose, qu'on pousse les corps de haut en bas avec un bâton, soit qu'on veuille que ces corps soient tirés en bas avec une corde."[44] But both of these approaches are suppositions, and Newtonians should stop telling Cartesians that they ought to treat attraction as a fact, "car il ne fût peut-être jamais de supposition plus gratuite."[45] Banières was repeating sentiments entertained by other Cartesians for whom Newtonianism was far from being free from the accusations of being a system built on suppositions and hypotheses.

Father Castel's "Soixante-douziéme problème" in his book appropriately entitled Le vrai systême de physique générale de M. Isaac Newton, was to address the question "Si l'Opinion de M. Newton sur les Couleurs, est un Systême, ou même une hypothese?"[46] Castel's answer was that it was very much a system and he was unimpressed with statements to the contrary.

> Monsieur Newton n'a point de Systême, dit-on tous les jours, & les Newtoniens, en effet, ne cessent de déclamer contre les sistêmes & les hypotheses des Cartésiens.
>
> C'est-à-dire que ces Messieurs veulent absolument que nous prenions pour des faits & pour des Expériences, tout ce qu'il a plû à leur maître de nous débiter sur les Couleurs, & sur toute la Physique en general.[47]

But the only difference between what the Cartesians were doing and what the Newtonians were doing was that "la maniere de Descartes & de ses Sectateurs, de donner ses opinions comme des Systêmes & des hypotheses, est plus modeste & plus philosophique." The manner in which Newton offers everything "pour des faits ou pour des Démonstrations géométriques, a quelque chose de trop fier, de trop imposant, & même de très-dangereux."[48] Castel was accusing the Newtonians of dogmatism, of not having the philosophical modesty of the Cartesians who at least presented their views as possibilities. Newtonians insisted that what they presented was fact. However, what Newton offered "dans son Optique" was "un Systême d'Expériences" and "dans ses Principes, un Systême de Géométrie."[49] In principle there was nothing wrong with that, Castel affirmed, for it was the business of science to offer systems. A system, after all, was nothing but "une liaison de pensées & de choses, qu'une tête ferme & géométrique sçait assortir & rapporter à un même but."[50]

What interests us here is what Nollet thought about all of this. What did he think was the role of experimental physics in the construction of systems and what in fact did he think of the debate between Cartesians and Newtonians? The answer, I believe, is that Nollet approached the debate between Newtonians and Cartesians as an argument between 'systematists'--maybe even enthusiasts. He believed that the enthusiasm with which each group adhered to its philosophy was detrimental to true physics. He shared with the Cartesians the view that physicists ought to seek mechanical, impulsionist explanations in their search for causes while in the field of planetary motions he was willing to admit to the worth of the Newtonian "physics of effects."

The task of the experimental physicist was to extract what was good from all philosophies. Physical knowledge progressed by continuously appropriating the elements of truth from different sources and developing them. Oftentimes, as we shall see, what he meant by physical truths was not more than those facts he regarded as least contested by the community of scientists. He believed that through correct reasoning, coupled with an adroit use of experiment, it would be possible to reach a "physics of consensus," which would be nothing less than the truth so clearly established so as to be beyond doubt.

Nollet's Programme offers us the first opportunity to examine his views on the disputes preoccupying his contemporaries. Although the Programme was meant to provide a manual to his lectures and as such is no more than an outline, it does give us some indications of his early interests. One of Nollet's intentions was to reach an audience of young men and women, even children, to educate them about the basic truths of science. Indeed, a central concern of the lectures, as expressed in the Programme was not so much the teaching of physics, as correct reasoning.[51] Teaching the young to think clearly, letting them recognize the laws which nature follows uniformly, would enable them to gain the notions they needed to fight off an infinity of popular prejudices.[52] In other words, the intent was, broadly defined, educational. The purpose was not to prepare students to follow either Descartes or Newton. Had he meant to do that "personne n'ignore qu'il ne fallût préparer l'esprit par des exercices préliminaires, le mettre en état de raisonner sur les choses difficiles, & de saisir les conséquences; . . ."[53] The study of nature can be undertaken on a number of levels, and while the most astute mind cannot understand the highest "la raison naissante est trés suscéptible des premiers."[54] To reach this "raison naissante" nothing more was needed than "le sens commun de la part du sujet, & l'attention de ne lui en point faire une étude trop pénible. . . ."[55]

These introductory remarks in the "Preface" are not so much a rejection of Newton's or Descartes' philosophies as an assertion that the physics presented in the *Programme* was of a simple enough level that it could remain free of association with either philosophy. Moreover, there is no attempt, as there would be later in the *Leçons*, and elsewhere, to contrast experimental physics, as a method, with the physics of Descartes or Newton. However, while he adopted Newton's theory of colors, something most of his contemporaries already did, Nollet's proclaimed independence from either the Cartesian or Newtonian physics in all other matters is maintained throughout the sixteen lessons that make up his course. Neither the question of vortices nor attraction is addressed anywhere in the text of the *Programme*. Neither the question of the fall of bodies nor that of planetary revolutions is dealt with from these perspectives. On a number of controversies during the period in which the *Programme* was published, such as those concerning the elasticity of bodies, the cause of the tides, the divisibility of matter, and magnetism, Nollet simply stated that he, in his lectures, reported and exposed "les opinions les plus probables" without mentioning what these were.[56]

Most of these issues, though not all, were addressed somewhat more extensively in the *Leçons*. The *Leçons*, it will be remembered, appeared over a period of over twenty years, with the first volume appearing in 1743 and the last in 1764. Thus passages throughout the six-volume text reflect preoccupations of different kinds and responses to situations which changed with the passage of time. Nevertheless, throughout the Leçons Nollet maintained his disassociation from both Newtonianism and Cartesianism. Moreover, different from his silence in the Programme was Nollet's clear assertion of the supremacy of experimental philosophy to both the philosophies of Descartes and of Newton. "Je ne me présente ici," he wrote, "sous les auspices d'aucun Philosophe."[57]

> Pénétré de respect, & même de reconnoissance pour les grands-hommes qui nous ont fait part de leurs pensées, & qui nous ont enrichis de leurs découvertes, de quelque nation qu'ils soient, & dans quelque tems qu'ils ayent vécu, j'admire leur génie jusques dans leurs erreurs, & je me fais un devoir de leur rendre l'honneur qui leur est dû; mais je n'admets rien sur leurs parole, s'il n'est frappé au coin de l'expérience. En matière de Physique, on ne doit point être esclave de l'autorité; on devroit l'être encore moins de ses propres préjugés, reconnoître la vérité par-tout où elle se montre, & ne point affecter d'être Newtonien à Paris, & Cartésien à Londres.[58]

In matters of physics, it is experience that must be consulted; it is the basis on which judgement on these matters should be formed. The Leçons would be confined to the subject-matter of experimental physics and for this reason Nollet had decided not to report on the different systems proposed by the ancients and moderns on the mechanism of the world. The best of these systems, he judged, could not hope to be anything but an ingenious "peut-être."[59] And while one could absolutely ignore these efforts of the imagination, he would have discussed those which had received greater attention, those of Descartes and Newton, had he not been "prévenu par un Auteur, dont l'Ouvrage est entre les mains de tout le monde, & qui a traité cette matière avec le même agrément qu'on rencontre dans tous ses Ecrits."[60]

The work Nollet was referring to, as he made clear in a footnote, was the second volume of the Histoire du ciel by Noël Antoine

Pluche (1688-1761).[61] This work first appeared in 1739, and, although not as popular as Pluche's very successful multi-volume Spectacle de la nature, the Histoire du ciel was well received by the public at large.[62] Since Nollet nowhere entered into a detailed critique of Cartesianism or Newtonianism as systems it may be worthwhile to look into Pluche's views in the Histoire in some detail.

A major thrust of Pluche's writing in the Histoire du ciel, as in the Spectacle de la nature, was to argue that the splendor of the world is a creation of God and only his revelation can yield total and complete knowledge.[63] Pluche restated here his opposition to the cosmologies of the physicists and reaffirmed the position he had developed in the Spectacle de la nature, that the cosmology of Moses was the only one that agreed with the findings of experimental physics and history.

Pluche was clearly critical of both Descartes' and Newton's attempts to establish systems of thought. He was much less sympathetic to Descartes than to Newton, but found the latter lacking as well. Pluche was willing to honor Descartes as a "très-grand génie: & encore plus, parce qu'il nous a le premier enhardis à secouer le joug d'Aristote. . . ." But he would have honored Descartes more if the latter, after realizing that the beaten track led nowhere, had not committed himself to another "aussi peu sûre, & peut-être plus dangereuse." Descartes' method of systematic doubt was a subject of ridicule for Pluche. After a sarcastic presentation of Descartes' laborious path to discover that he existed and had a body, Pluche derided this method "tant vantée," saying that there was not a peasant "si grossier qui, sans méthode &

sans méditation, ne sache très-bien qu'il est; qu'il a un corps; qu'il y en a d'autres autour de lui. . . ."[65]

Nollet shared Pluche's view that modern physics was obligated to Descartes for having freed it from the yoke of scholasticism. It was thanks to the method introduced by Descartes that students of physics were no longer subjected to that "langage inintelligible, qui déshonoroit la raison."[66] Although Nollet was often critical of Cartesian physics he commended Descartes' method, and the ridicule Pluche levelled against it is nowhere in Nollet's writings.

But it was another aspect of the method that Pluche believed more dangerous. The Cartesian method, he argued, is too presumptuous. There are no indications, Pluche wrote, that God wants us to know everything, to go "de connoissance en connoissance, jusqu'à pénétrer dans la structure de son monde. . . ."[67] The manifest intention of the Creator in creating us as He did was to help us obtain knowledge (<u>des connoissances</u>) through our senses and to help us regulate its use through reason. For men to attempt to use reason to obtain knowledge is to pervert the will of the Creator. Such a method is illusory and pernicious for it supposes that God expects us to know the foundations of his works and to know the reason for everything.[68] In knowing that quinquina cures fever must we, to be able to use it, know how it operates? The compass helps us reach India, need we know through what mechanism this happens? "Quelle témérité de demander ici que Dieu nous révéle le fond de son oeuvre. . . !"[69]

Pluche did not end his criticisms of Descartes here; he next looked at his physical system and strongly rejected it using a combination of scientific and religious arguments. Although he discussed, and not maladroitly, the criticisms levelled against some of Descartes' physical principles, especially where it concerned light, colors and laws of motion, his major criticism was directed against the assumption that there is a world that operates according to fixed laws and that man, through his reason, can come to know them. This regularity, this fixity of natural laws, which left God the role of the onlooker, was repugnant to Pluche, and he pointed out that not surprisingly atheists were conveniently served by it.[70] This criticism of Cartesianism was not new with Pluche. Samuel Clarke (1675-1729), of course, had brought it up in the correspondence with Leibniz.[71]

The strict utilitarianism of Pluche and his skepticism about the possibility of systematic knowledge may have had its sources in pyrrhonism.[72] Pyrrhonists, too, argued that natural philosophers who purported to explain the world by fixed laws were negating God's powers. They, like Pluche, believed that the business of natural philosophers was to accumulate knowledge of distinct, disparate and useful facts and not to attempt the impossible: to understand the laws of creation and the world.[73] Nollet himself strongly emphasized the utilitarian aspect of knowledge, as he made clear in his concluding remarks in the inaugural speech he delivered at the Collège de Navarre in 1753.

> Oui, je fais mille fois plus de cas de ces zélés Citoyens qui appliquent leurs lumieres et leurs talents à rendre potable l'eau qui ne l'est pas, à maintenir dans son état naturel celle qu'on embarque par provision, à purifier l'air dans les lieux où il est ordinairement mal sain, à rendre la Boussole d'un service plus sûr, à perfectionner la culture des terres, à conserver le produit des moissons, quoique tous ces objets ayent été entamés; que de ces Savants orgueilleux, qui cherchent à nous éblouir par la grandeur apparente, mais souvent imaginaire, ou par la singularité des sujets qu'ils entreprennent de traiter.[74]

It is clear from this passage, however, that Nollet viewed the utility of science in those efforts that scientists exercised as citoyens. In other words, while Pluche believed that the only knowledge possible was knowledge of the useful, Nollet was but a strong advocate of the need to make knowledge useful.

Pluche was less critical of Newton's physics, which he believed to be in accord with experience and the Mosaic scriptures.[75] His judgement in this work, which appeared in 1739, was that the Newtonian philosophy was "bien venue à présent dans les académies célébres. Elle y tient, en quelque sorte, le premier rang."[76] He identified three basic tenets of Newtonianism: the void, the laws of motion, and attraction. While he had no objections to the first two, it was the Newtonians' aptness to see attraction everywhere that he rejected. Pluche, like Nollet, was critical of the Newtonians' tendency to make attraction a real property of bodies, and to try to explain through it magnetism, electricity, capillarity, and worse yet, the figure of the earth.[77] ". . . le plus grand abus qu'on puisse faire de l'attraction . . . seroit sur-tout de se figurer que cette attraction, dont l'existence est plus qu'incertaine, ait été la cause formatrice de la terre."[78] The shape of the Earth and its creation, as well as each particular aspect of this world, could only be explained by the intervention of God who is free to create the Earth in whatever shape He pleases. The Newtonians' attempt to explain the shape of the Earth by independent laws was, in Pluche's view, similar to the sin of the Cartesians who believed that they could explain the world from natural laws leaving God without a role. While Newton's system was not as presumptuous as that of Descartes, it, too, attempted to explain too much.

Nollet, as we will see, was also critical of the Newtonians' attribution of the property of attraction to matter. He believed that the evidence on that score was far from conclusive. However, at least as early as 1743, Nollet had adopted Huygens' and Newton's views that the Earth was an oblate spheroid. And he explained the flattening at the poles as having been caused by the greater centrifugal forces at the equator counteracting the gravitational pull.[79] This was basically Newton's explanation. While the explanation was not dependent on making attraction a property of matter, it was essentially a physical, causal explanation for the shape of the Earth.[80] It was of this sin the Newtonians stood accused by Pluche.

After an expósition of over one hundred pages of the systems of Newton and Descartes, Pluche concluded with the following remarks:

> Ce que nous pouvons avancer hardiment, selon l'éxacte vérité, & conformément au but principal de cette histoire, c'est que malgré Aristote, à la honte des promesses de Descartes, selon tous les modernes les plus sensés, & de l'aveu de Newton même, nous ne connoissons point du tout le fond de la nature; & que la structure de chaque partie, comme de l'univers entier, nous demeure absolument cachée; d'où il suit qu'il y a bien du mécompte dans l'estime qu'on fait des systêmes de physique, quels qu'ils puissent être.[81]

Interestingly, Newton himself was spared the attack against the Newtonians. It was Newtonianism as a system that was being criticized. This would be repeated in Nollet. The rejection of systems in Pluche's Histoire went beyond the rejection of Cartesianism or Newtonianism, but it was, in addition, a denial of the possibility of systematic knowledge. The most that man could hope for, according to Pluche, was the accumulation of specific knowledge about particulars. This contrasted with the view shared by some of his contemporaries that systems built on experiments were permissible, possible, and desirable.

The extent to which Nollet agreed with Pluche cannot be ascertained solely from the above exposition. It is clear that the two men disagreed on some issues. However, the fact that Nollet chose Pluche's opinions on the two "systems"--Cartesian and Newtonian--to speak for him is significant. Their agreement lay, I believe, in a deeper affinity: their shared belief that both of these systems were overrated, as were all systems "quels qu'ils puissent être." That message, present in Pluche's Histoire and repeated elsewhere in his other writings, also known to Nollet, was unambiguously clear.[82]

A reading of the Leçons de physique will show that Nollet shared many of Pluche's views. The Leçons were the extended format of the Programme Nollet had promised in 1738. The six volumes covered twenty-one lessons--five more than in the Programme, but the overall nature of the subject matter remained the same. Volume one dealt with the extension, divisibility, solidity, elasticity and mobility (as contrasted with motion) of matter. Volume two dealt with centripetal and centrifugal forces, gravity and hydrostatics. Volumes three and four were dedicated to mechanics and the examination of the nature and properties of air, water, and fire; and volume five concerned optics and light. Half of volume six dealt with astronomy and magnetism and the other half with electricity. The Leçons, like the Programme, had a strongly utilitarian, pragmatic tone. Examples to illustrate the lessons were drawn from everyday life and often from industry and technology, and attempts were constantly made to relate the lessons to practical ends. Experiments were often followed by an account of their possible applications in industry, technology and everyday life. The intent was also to present a textbook that was free of conjectures. Only those principles least contested were to be included, and all metaphysical discussions avoided. The physics of the Leçons was to be "sensible & appuyée sur des faits."[83] Throughout the six volumes that claim was maintained.

Nollet credited Descartes with being the first to free physics from the hold of the ancients, but Cartesian physics would often be criticized in the text. Newton's physics was treated more sympathetically, and on a number of occasions his views were adopted explicitly. But this was always done with great reservations, for Newtonianism was clearly included in the list of "systems" Nollet wanted to avoid. This was not done, as Brunet interpreted it, as a tactical maneuver. Nollet drew clear distinctions between what he believed Newton had demonstrated clearly and experimentally and, on the other hand, the dangers involved in interpreting this for more than it was. His main objection against Newtonianism was the attribution to matter of an attractive virtue. But he also rejected explanations offered by Newton and Newtonians on such issues as the nature of light and the cause of the rise of liquids in capillary tubes. While willing to accept Newton's principles, he would not do it at the expense of a major principle of his own physics; viz., that unless it could be shown otherwise impulsion was to be regarded as the basic cause of motion.

The first opportunity Nollet had overtly to contrast the positions of Newton with those of Descartes came in volume two, where he dealt with "central" forces, or centrifugal and centripetal forces, and in the lesson immediately following it on gravity (pesanteur).[84] Nollet discussed Descartes' explanation of gravity and gave a brief history of the arguments and discussions this view had prompted. Volume two of the Leçons, it will be remembered, was published a year after the debate with Privat de Molières discussed in the previous chapter. Nollet gave a summary of the debate in which he had participated and the experiments he had performed concluding that Descartes' explanation of the cause of fall was "moins juste qu'ingénieuse."[85] However, he added that the explanation could still be salvaged in the future. While the Cartesian vortices had failed to explain the fall of sublunary bodies toward the center of the Earth, one could now say without any doubt, he wrote, that a circulating fluid-matter does cause bodies, both lighter and heavier than it, to precipitate. If this principle, which Nollet affirmed to be uncontestable, had not yet been applied wisely to fully explain the fall of bodies, this did not mean that it would not be applied some day. "Il me paroît plus raisonnable de croire que d'autres pourront faire ce que nous n'avons pas fait, que de regarder comme absolument impossible ce que nous avons tenté inutilement."[86] Nollet was placing himself clearly on the side of an impulsionist explanation of fall and echoing an attitude not uncommon among supporters of the vortical explanation who believed, to quote Bulffinger, "qu'il n'y a rien de plus simple que les tourbillons cartésiens; il faut donc . . . tout tenter avant de les abandonner."[87]

Lesson Six, immediately following the exposition of Descartes' explanation of fall, was a discussion on gravity (pesanteur) proper. Philosophers, Nollet wrote, do not agree as to what the cause of this force is, and their opinions can be separated into two groups. One group looks at gravity as a principle of nature, as an inherent and primordial quality of bodies which may have no other cause than the simple will of the Creator; and in this manner they cut short all difficulties. The other group argues that it is the effect of an invisible matter; however, the proofs on which this position is based have raised a number of important objections to which fully satisfactory answers have not yet been provided. Thus, Nollet believed, while the first group proceeded as if the problem of a causal explanation did not exist, the latter, who attempted to provide one, had so far failed. To say with the Aristotelians that bodies which fall down are obeying a principle that makes them fall is to say nothing that enlightens the mind.[88] To say with Newton that gravity is the natural consequence of the general gravity that we observe throughout nature is to abandon the search for cause and attach oneself to effects. And to pretend that attraction is a virtue of bodies which they all have for each other, as some Newtonians are prone to do, is to attribute to Newton a belief that he himself did not adhere to "s'il en faut croire ses propres paroles." However, neither the physical explanations of Pierre Gassendi (1592-1655), who explained that gravity was due to the "écoulemens d'une matiere qui agisse comme celle de l'Aimant," or Descartes' explanation, were, at this stage, acceptable.[89] Those who demand a physical explanation of weight, and demand that it be both satisfactory and intelligible, must not look for it in any of the works that are known at this time. Since causal explanations were not available, he suggested that for the present the study of gravity be limited to the study of the observed phenomena. For if the cause escapes our curiosity, we can console ourselves with knowledge of the effects, "autant celle-là est incertaine, autant celle-ci est bien constatée."[90]

In light of what Nollet had just said about Newton's attention to effects, this last statement is clearly favorable to the British scientist. And in fact Nollet included Newton among those he believed had contributed to our knowledge of the fall of bodies. But it was to Galileo Galilei (1564-1642) that the main credit was reserved. "[C]'est à ce Philosophe Italien que nous sommes redevables des plus intéressantes découvertes qu'on ait faites sur cette matiére."[91] It was on the foundation laid by Galileo's theory that "Huyghens, Newton & Mariotte ont travaillé depuis avec tant de succès & d'applaudissemens."[92] Indeed, the ensuing discussion of gravity is in large part an exposition of the accomplishments of Galileo regarding the laws of falling bodies. Newton's contributions are considered in the discussion on the apparent change in the force of gravity according to change in place.[93] The center toward which all heavy bodies fall, Nollet wrote, is that of the Earth. One might be then led to believe that as the distance from that center varies so does the gravity. However, no such change had been noticed and physicists had assumed gravity to be equal at all distances from the center of the Earth--until reasons to believe otherwise were found. Newton assures us, Nollet continued, that this secret power that makes bodies fall toward the Earth is weaker the further they are from it. The English _philosophe_ has done even more than that. As if he had carried a balance to the Moon, "il veut que l'on croie qu'une pierre qui commenceroit à tomber de cet astre, ne feroit pas plus de chemin en une minute, qu'elle en fait ici-bas en une seconde." In other words, this stone would fall "3600 fois plus lentement, qu'elle ne fait aux environs de la surface de la Terre."[94] Should the reader be astonished, that this philosopher spoke in such manner about things that appear to be beyond the reach of the human mind, what may surprise him even more is that Newton presented his views not as conjectures "mais qu'il ait appuyé tout ce qu'il a avancé, sur des preuves & sur des démonstrations qui tiennent contre l'examen le plus rigoureux." While he had not really shown that the centripetal force acting on the Moon is the same as that acting on other bodies of our globe, he has supposed it "avec tant de vraisemblance, que cela ne peut guéres passer pour une simple conjecture."[95] How could Newton speak with such assurance about what went on at the Moon? The answer was to be sought in the works of Newton, for what he said about gravity "est lié avec tout le systême général du Monde, qu'il a plus heureusement concerté qu'aucun autre Philosophe."[96]

After these unambiguous words of praise, Nollet proceeded to show how Newton had used this principle to explain the motion of the Moon around the Earth, and how it had served to explain the puzzling discoveries of Jean Richer (1630-1696) concerning the varying speed of pendulums depending on their location in respect to the Equator.[97] However, Nollet would come back in the same volume to the issue of attraction. Toward the end of Lesson Eight, in a discussion over the rise of fluids in capillary tubes, the issue is raised once more.[98] Here the enthusiasm for Newton's views is clearly moderated.

There are, Nollet wrote, two types of physicists that accept attraction between bodies as an explanatory device. Some, following Newton's intentions, see attraction as a fact that takes place throughout Nature and that could have a mechanical explanation worthy of investigating, though that explanation for the time being eludes us. (Nollet could very well have been describing himself here, for this was the position he took in Lesson Six.) Other physicists, Nollet continued, more daring than their own leader, pretend that the attractive virtue is a principle that has no other immediate cause than the will of the Creator. According to the first group, when two bodies approach or unite with one another, and the reason for this is not known, the fact itself is characterized by the word "attraction". This is done solely to distinguish it from other similar facts where the cause is known. The second group claims that all of this takes place in virtue of an innate force, a natural tendency through which of itself, and without any outside impulsion, a body moves toward another and acts on it without touching it directly or through other intermediary bodies.[99] Nollet did not believe the first group was doing anything out of the ordinary. Cartesians most loyal to the principle of mechanical causes, he wrote, refer constantly to phenomena whose causes remain obscure, and choose to give them names like "adhesion", "viscosity", "flexibility", "spring", etc. They should have no reason to be shocked at the use of the word "attraction".

But what about the attractive virtue considered as a principle of nature? The Creator, in establishing impulsion as the most common and ordinary cause of the motion of bodies, Nollet conceded, could have also established attraction as another cause. These two principles are not incompatible. But, he asked, are we to assume from the fact that God could have done it that He in fact did? Are we to assume that because we have not yet been able to explain attraction by impulsion that this cannot be done? Should we then hastily introduce a new principle into physics when we know that Nature affects as much simplicity in its causes as it does multiplicity in its effects? The human mind is limited in its knowledge (connoissances), and can never flatter itself with knowing all that there is to know, but it is never less enlightened than when it allows arbitrary explanations.[100]

The thoughts of Joseph Saurin (1659-1737) on these matters were, Nollet wrote, very wise and judicious.[101] This savant had had throughout his life ample opportunity to learn all that could be said in favor of the "Systême des Attractions, & en même tems tout ce qu'on peut reprocher à l'emploi qu'on a fait des Impulsions." His ideas on these matters, Nollet wrote, were worth repeating:

> "Il ne faut pas nous flatter, dit-il, que dans nos recherches de Physique nous puissions jamais nous mettre au-dessus de toutes les difficultés: mais ne laissons pas de philosopher toujours sur des principes clairs de Méchanique: si nous les abandonnons, toute la lumiére que nous pouvons avoir est éteinte, & nous voilà replongés de-nouveau dans les anciennes ténébres du Péripatétisme, dont le Ciel nous veuille préserver."[102]

This passage came from an article by Saurin that appeared in the Académie's Mémoires for 1709.[103] More than just a defense of impulsionism, this article was a defense of the Cartesian vortical explanation of gravity against difficulties proposed by Huygens and Newton. In the lines just preceding the passage quoted above, Saurin had accused Newton of treating attraction as a property inherent in matter itself.[104] Whether Nollet himself believed that this was what Newton had done is doubtful. He was prone, as we have seen, to interpret Newton's use of "attraction" as no more than the use of a word meant to identify a phenomenon the cause of which had not yet been fully explained. It was only some of Newton's followers, Nollet seemed to believe, who had interpreted it to be a principle of nature. Whatever the case may be, Nollet believed that Newton had only been able to demonstrate the usefulness of this principle in the study of matters well beyond the surface of the Earth.[105] Unable to apply it experimentally to a study of earthly physics he had carried the principle to the stars (astres) and there "il y trouva tant de conformité, qu'on est tenté de croire que ce Grand-homme a deviné le secret de la Nature." However, Nollet continued, whatever advantages might ensue from Newton's hypothesis--and it must be granted that it explains in a more complete manner than ever before the motion of the planets--"le fond de la chose reste toujours à juger." All of this could still be the effect of some physical impulsion and Newton himself did not dare pretend otherwise.[106]

It may be worth pointing out that 'sGravesande, a Newtonian who may have had an influence on Nollet, also entertained the view that gravitational attraction could be due to impulsion.

Nollet would return to the same issue twenty-one years later in volume six of the Leçons. At the end of Lesson Eighteen, after an exposition of the solar system, the motion of the planets, the Sun, Moon and the Earth, Nollet returned to the question of attraction.[107] What, he asked, is the nature of the two forces, centripetal and centrifugal, that keep the planets in motion without any sensible alterations in their elliptical orbits for so many centuries? The answer to this puzzle, Nollet wrote, has eluded philosophers for a long time, and their many efforts to explain it remain unsuccessful. They have been unable to produce anything but hypotheses, for and against which they argue interminably. However one such philosopher has approached the problem from a different perspective.

> Je ne sais si je me trompe; mais il me semble que Newton s'y est pris d'une maniere bien sage & bien raisonnable: au-lieu de s'amuser à chercher & à deviner les causes premieres, pour en déduire ensuite les phénomenes comme des conséquences, il a commencé, au contraire, par bien examiner ce qui se passoit sous ses yeux & autour de lui; il en a étudié les causes immédiates; il en a fait

> l'application à des effets plus éloignés, & en remontant ainsi du petit au grand, du plus connu à ce qui l'étoit moins, il est parvenu à expliquer d'une maniere trèsheureuse, les plus grands mouvements de la nature; & ce qui inspire une grande confiance pour la route qu'il a suivie, c'est qu'en marchant sur ses pas, en suivant sa méthode, on ramene tous les jours à ses principes des phénomenes de détail qui sembloient s'en écarter, des especes d'exceptions qu'il avoit laissées en arriere, ou dont on n'avoit pas encore connoissance de son tems.[108]

This passage appeared in 1764, by which time the debate between Cartesians and Newtonians had abated.

Nevertheless, after these flattering comments on the accomplishments of Newton and the Newtonians who had followed him and improved his theory, Nollet once more repeated his apprehensions against adopting attraction as an inherent quality of matter--a view he believed had recently gained more adherents. Modern physics, glorified for ridding itself of all occult qualities, was now seeing the painful reintroduction into matter of "une vertu abstraite, un être inconnu, & même inintelligible, & qui ne tient en rien au Méchanisme."[109] The possibility of finding a mechanical explanation for attraction should still be entertained, and he referred the reader to volume two, lesson eight for his opinions on this matter.[110]

Lesson Eight, as we have seen, was dedicated to hydrostatics, and the discussion of attraction had come in the context of an examination of explanations offered on the cause of the rise of liquids in capillary tubes. At issue was not only the explanation of this phenomenon but whether or not "attraction" should be used to describe the behavior of matter in physical processes where its explanatory value was questionable. The use of that principle to explain capillary rise, Nollet had pointed out, rather than simplifying matters had complicated them. Nollet referred to the experiments by the British physician James Jurin (1684-1750), a Newtonian who had concluded that inconsistencies would result if the principle of attraction alone was used to explain the capillary effect. Jurin, Nollet wrote, had been forced to have "recours à la pression d'un milieu assez subtil" to explain the phenomenon--a position, in fact, akin to that adopted by Cartesians and by Nollet.[111] Other Newtonians had taken different approaches. Thus Clairaut, "dans un savant Ouvrage qu'il vient de donner au Public" had done, Nollet contended, a better job than other Newtonians of applying attraction to the study of these matters, but not without disagreeing with Jurin both on what the effect of attraction on the rise of liquids was and where that attraction took place.[112] Those who were insisting that capillary rise should be explained by attraction, Nollet believed, were more concerned with endowing matter with an attractive virtue than with understanding true relations in the world. In stating this position in 1743 and reaffirming it in 1764 Nollet was being critical of some Newtonians whose opinions on these matters were well known.

Musschenbroek, in his Essai de physique, devoted an entire chapter to the argument that the attractive virtue was indeed a property of matter.[113] Musschenbroek argued that unless it could be shown otherwise, one should conclude that bodies attracted each other because they were endowed with an attractive virtue. Those who wanted to attribute attraction to some form of impulsion, Musschenbroek wrote, should have to prove their assumption "par de bonnes preuves & des observations éxactes" and show "qu'une telle cause est véritablement celle qui produit l'effet en question." No one should be expected to believe that impulsion is the cause of attraction as long as this fact has not been demonstrated. Musschenbroek stated his readiness to dismiss his belief in a real attractive virtue only if its effects could be shown to be due to another cause.[114] This view, of course, contrasted with that of Nollet, who argued that one could not assume an attractive virtue simply because impulsionist explanations had been unfruitful in some cases. Those who argue otherwise, Nollet explained, as if directing his comments at Musschenbroek, were lacking in logical reasoning: "car ce n'est pas raisonner en régle, que de dire, Ceci n'est point expliqué par les loix de l'impulsion, donc c'est un effet de la vertu attractive."[115] But Musschenbroek had even applied the attractive virtue to explain the capillary rise effect. It was this virtue, which he asserted to be "réellement dans les Corps," that caused the rise. The reason different liquids rose to different heights in glass tubes was that the degree of attraction varied with the material composition and density of the liquids and glasses used.[116]

In 1747, one of Nollet's compatriots, the Newtonian experimental physicist Pierre Sigorgne, expressed views similar to those of Musschenbroek. In a discussion of attraction over small distances, in his Institutions newtoniennes, ou introduction à la philosophie de M. Newton, Sigorgne stated that it was no longer possible to doubt that "les particules de la matiere [ont] une tendance mutuelle les une vers les autres." It was enough to open one's eyes to be convinced of that fact.[117] The rise of liquids in capillary tubes could be understood as the action of that tendency.[118] In a revised edition of this work that appeared in 1769, Sigorgne reiterated these views. The law of attraction was a true law to be regarded "comme loi originaire, primitive & universelle de tous les grains de la matière. . . ."[119] Those who tried to reduce attraction to impulsion were working in vain.

As Nollet pointed out, attempts to introduce attraction into the study of capillary effects did not help produce a clearer explanation of that phenomenon.[120] Jurin and Clairaut disagreed on the nature of its effects and the manner of its operation. Musschenbroek had offered an explanation for the different heights liquids achieved in the tubes; however, the explanation was too vague to carry any value. Sigorgne, in the 1747 edition of his Institutions, had tried to express this attraction mathematically, and had arrived at the conclusion that attraction over small distances operated in accordance with an inverse cube law.[121] In 1769 he discarded that idea and could only suggest that the attraction acted "dans une raison plus grande que l'inverse du quarré."[122]

It is worth underlining the point that Nollet was willing to adopt the use of "attraction" where he believed that concept to be helpful. He did so in his explanation of gravity and the planetary motions where he believed Newton "s'y est pris d'une manière bien sage & bien raisonnable." Nollet was willing to eschew causes and study effects where that had been shown to be fruitful, and as such he may be identified with that "philosophy of effects" usually associated with Newtonians. It is clear, however, that he was not willing to limit physics to that philosophy, nor was he willing to ascribe an attractive virtue to matter. In this last sense he disagreed with many of his Newtonian contemporaries.

In concluding his discussion of capillary tubes, Nollet's final judgement was, as it would be on a number of issues in dispute, very cautious. But it was clear that he rejected an explanation based on attractive virtues and inclined toward the position identified with the Cartesians, that the capillary rise was largely due to the pressure of a subtle fluid.

> De tout ceci il résulte que ces phénoménes, ou ne sont point encore bien expliqués, ou que les explications qu'on en donne, tiennent à des hypothéses qui ne sont pas généralement reçues. Peut-être cela vient-il de ce qu'on s'est obstiné à ne leur donner qu'une seule & unique cause . . . La pression inégale de quelque fluīde est probablement le point fondamental de l'explication; mais l'adhérence ou la viscosité naturelle des liqueurs, la grandeur & la figure de leurs parties, . . . &c. sont autant de moyens que la Nature peut employer pour ces sortes d'effets, . . .[123]

It was this cautious approach to issues under dispute, reflected in the above passage, which led the reviewer of the first two volumes of the Leçons in the Journal des sçavans to judge that "M. l'Ab. N. [est] fort retenu dans ses conjectures. . . ."[124]

The style of presentation of his ideas was less guarded in later volumes of the Leçons, but he maintained his claim to be neither a Cartesian nor a Newtonian. He continued to argue that physicists should extract that which was valid from both systems. Mention has already been made of his discussion of "attraction" in volume six. In volume five, dedicated to a study of the nature and properties of light, Nollet repeatedly contrasted the opinions of Descartes and Newton. According to the Cartesian view, Nollet explained, light is a material fluid that permeates the universe. The sensation of light is caused by a vibration of that contiguous fluid "semblable à celui qui fait le son dans l'air."[125] According to the Newtonian view, light is "tantôt une substance céleste qui part des astres, tantôt une matiére terrestre que l'inflammation développe."[126] Nollet objected to Newton's view because he could not accept the idea of a permanent, inexhaustible emanation of light rays crisscrossing through space.[127] On the other hand, he found Descartes' explanation "si naturel, si plausible, si commode pour rendre raison des phénoménes," that he was sure it would have been accepted by everyone "si des intérêts particuliers n'y eussent mis empêchement."[128] However, he believed that Newton had shown beyond any doubt that light was separable into parts distinguishable "par des propriétés constantes & des effets sensibles."[129] Nollet, in fact, had adopted Newton's theory of colors at least as early as 1738 in his Programme.[130] A reviewer of that work had judged that Nollet's treatment of that subject "fera peut-être plus de Newtonnienes [sic] èn France que les meilleurs Traités de la lumiere."[131] Nollet, however, drew a clear distinction between what he believed Newton had demonstrated--i.e., that light was composed of distinct and separate parts--and conjectures about the nature of those distinctions. What Newton had demonstrated beyond doubt could still, conjecture for conjecture, be explained by the Cartesian theory of light "sans inconséquence."[132] However, as long as experience could not offer anything to help us adjudicate between these conjectures, the best path was to sustain judgement; "je m'arrête", Nollet wrote, "avec le Philosophe Anglois aux effects sensibles, qui peuvent servir à expliquer les phénoménes de la vision qui ont rapport aux couleurs."[133]

It is time to bring this long chapter to a close. My intention has been to argue that Nollet regarded himself outside the Newtonian-Cartesian debate and that he believed that experimental physics should appropriate that which was valid from both systems. On one hand, he accepted Newton's theory of gravity and his explanation of the planetary motions. He did not believe that any of the attempts made to explain gravity mechanically had succeeded, and in fact he played a role, as we saw, in discrediting them. He also accepted Newton's work in optics at least as early as 1738. On the other hand, he adopted Descartes' explanation of the nature of light, rejected the idea of an attractive virtue inherent in matter, and believed that the principle of mechanical impulsion would eventually provide an explanation for attraction at a distance.

The view that Newtonian physics could be reconciled with Cartesian mechanics was not novel with Nollet. Nicolas Malebranche (1638-1715), Privat de Molières, and others, had attempted to bring Newtonianism into the domain of impulsionism.[134] Nollet himself, however, was not part of that enterprise, although he believed that in principle it could be accomplished. Malebranche and his disciples have been solidly placed in the Cartesian tradition by modern historians, while Nollet, as we saw, has eluded a unanimous designation. His identification as either a Newtonian or Cartesian could very well depend on the way in which one is using those categories. There are good reasons to call Nollet a Cartesian and good reasons to call him a Newtonian, but only if certain aspects of his physics are being emphasized. A distinction must be made between what Nollet and his contemporaries interpreted as a Newtonian or Cartesian position and what the modern historian, for whatever historical purposes, wants to so interpret. Brunet's assessment that Nollet, the Newtonian, was importing to France the physics of the Dutch Newtonians "en la simplifiant seulement sans la modifier," and that Nollet's claim to being neither a Newtonian nor a Cartesian was simple pretense is, I believe, erroneous.[135] It is questionable that Nollet would have had much cause to indulge in such subterfuge as late as 1755 and 1764 when he reiterated that claim. Reviews of Nollet's Leçons in the Journal des sçavans, Mémoires de Trévoux and the Histoire always took Nollet at his word. Castel, most probably the reviewer of the first two volumes of the Leçons in the Mémoires de Trevoux, in a favorable review contrasted Nollet's work with that of the Newtonians and Polinière "qui donnent constamment trop dans le détail des expériences recherchées & plus artificielles que naturelles, & ne les enchaînent guéres avec le raisonnement de la saine Physique."[136]

It may be argued that Newtonianism and Cartesianism are basically two different methodologies, with different views of epistemology which cannot be reconciled nor approached from a neutral position as Nollet claimed to do. Nollet, it may be said, must have adopted one or the other of these epistemologies. That argument, if valid, would only suggest that Nollet was logically inconsistent. In his own eyes he was neither Cartesian nor Newtonian. "Défions-nous sur-tout des Auteurs qui ont des systêmes à soutenir," he told his audience at the inaugural lecture for the chair in experimental physics at the Collège de Navarre; "défions nous de nous-mêmes, si nous les avons adoptés."[137]

> Hé! pourquoi vouloir être d'un ont décidé & en toute occasion, Cartesien, Newtonien, Leibnitien, &c? Quelqu'un de ces grands Hommes, dont l'autorité a tant de poids, a-t-il eu l'infaillibilité en partage? Ne peut-on pas respecter leur mémoire, admirer leur génie, profiter de leurs découvertes, sans s'attacher particuliérement à un seul, sans s'interdire la liberté d'examiner leurs opinions, de s'en écarter même, lorsque de nouvelles lumieres viennent nous éclairer sur ce qu'elles ont de défectueux? Pourquoi prendre indistinctement tout ce qui est renfermé dans un même trésor, quand il nous est permis d'en ouvrir plusieurs, pour nous enrichir avec choix?[138]

What gave Nollet's faith in experimentalism an important impetus, or rather, what was an important element of that method, was the eighteenth-century belief in the progress of man and man's knowledge. According to this belief, time was on the side of the scientist. Knowledge was increasing with the passage of time, and the search for truth was under no temporal constraint. The errors of the past were attributable, Nollet and others would point out, to the haste with which scientists had striven to build systems. Experimentalism, as practiced by Nollet, was a method of practicing physics in which the slow, careful accumulation of facts was essential. Nollet's insistence on accepting only solid, undisputed facts was regarded by many of his contemporaries as commendable neutrality at a time when elusive theories contended for the minds of physicists, with the consequence that much of the study of physics was seen to be in disarray. Pluche advised that Nollet's lectures be imitated everywhere, and Voltaire judged that one experiment by the Abbé was worth more than the whole Theodicée of Leibniz.[139] "Un simple mécanicien comme l'abbé Nollet qui ne sait autre chose que les expériences nouvelles," Voltaire wrote, "est meilleur phisicien que Démocrite et Descartes." He is not as great a man, "mais il sait plus et mieux."[140] But as the century proceeded and Newton's physics became more and more entrenched, that which in the 1730's and 1740's was regarded as commendable neutrality was increasingly regarded as sterile marginality. This may explain, in part, Nollet's fall from grace late in the century.

## CHAPTER II

## NOTES

[1] I. Bernard Cohen, Franklin and Newton. An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof, Memoirs of the American Philosophical Society, vol. 43 (Philadelphia: American Philosophical Society, 1956) pp. 386-390, and passim; hereafter Cohen, Franklin and Newton. R. W. Home, "'Newtonianism' and the Theory of the Magnet," History of Science, 15 (1977): 252-266; hereafter Home, "Newtonianism." Home argued that the boundaries of the terms "Newtonian" and "Cartesian" have been extended beyond the point where they can be useful and very near the point where they may be misleading. In this article Home expressed the view (p. 254) that Nollet was "anti-Newtonian." In his unpublished dissertation Home argued that Nollet was a Cartesian and as evidence of this fact pointed to his reluctance to accept Newton's laws of motion; Home, "The Effluvial Theory of Electricity" (Ph.D. dissertation, University of Indiana, 1967), p. 105.

[2] Pierre Brunet, Les physiciens hollandais et la méthode expérimentale en France au XVIIIe siècle (Paris: Librairie scientifique Albert Blanchard, 1926); hereafter Brunet, Les physiciens hollandais.

[3] Keith Michael Baker, Condorcet: From Natural Philosophy to Social Mathematics (Chicago: The University of Chicago Press, 1975), p. 4; hereafter Baker, Condorcet.

[4] Brunet, Les physiciens hollandais, p. 129.

[5] Ibid.; L'introduction; Alexandre Koyré, From the Closed World to the Infinite Universe (Baltimore: Johns Hopkins Press, 1957); hereafter Koyré, From the Closed World.

[6] Claude Buffier, Discours sur l'étude des sciences, 1733; cited in Ellen McNiven Hine, A Critical Study of Condillac's Traité des systèmes (The Hague, Boston, London: Martinus Nijhoff, 1979), p. 150n; hereafter Hine, Condillac. ". . . a mass of more or less ingenious conjectures; which makes a part of physics less a science than a kind of likelihood."

[7] Home, "Newtonianism."

[8]Robert E. Schofield, Mechanism and Materialism. British Natural Philosophy in an Age of Reason (Princeton: Princeton University Press, 1970); hereafter Schofield, Mechanism. Idem, "An Evolutionary Taxonomy of Eighteenth-Century Newtonianisms," Studies in Eighteenth-Century Culture, 7 (1978): 175-192; hereafter Schofield, "Evolutionary Taxonomy."

[9]Thomas L. Hankins, Jean d'Alembert: Science and the Enlightenment (Oxford: Oxford University Press, 1970); hereafter Hankins, D'Alembert. Leonard M. Marsak, "Bernard de Fontenelle: The Idea of Science in the French Enlightenment," Transactions of the American Philosophical Society, 49, pt. 7, new series (1959); hereafter Marsak, "Fontenelle." Aram Vartanian, Diderot and Descartes: A Study of Scientific Naturalism in the Enlightenment (Princeton: Princeton University Press, 1953). P.M. Heimann, "Newtonian Natural Philosophy and the Scientific Revolution," History of Science, 11 (1973): 1-7.

[10]Simon Schaffer, "Natural Philosophy," The Ferment of Knowledge. Studies in the Historiography of Eighteenth-Century Science, edited by G. S. Rousseau and Roy Porter (Cambridge: Cambridge University Press, 1980), pp. 55-91; hereafter Schaffer, "Natural Philosophy."

[11]Ibid., p. 71.

[12]L. W. B. Brockliss, "Aristotle, Descartes and the New Science: Natural Philosophy at the Unviersity of Paris, 1600-1740," Annals of Science, 38 (1981): 33-69; hereafter Brockliss, "Natural Philosophy." Most of the information on cartesian physics at the University of Paris presented below comes from this article.

[13]Ibid.; Charles M. G. B. Jourdain, Histoire de l'Université de Paris au XVIIe et au XVIIIe siècle (2 vols.; Paris: L. Hachette, 1862-1866); Torlais "Physique expérimentale;" and Paul Mouy, Le développement de la physique cartésienne 1646-1712 (Paris: J. Vrin, 1934); hereafter Mouy, Developpement.

[14]Brockliss, "Natural Philosophy," p. 65; and Torlais, "Physique expérimentale," p. 620.

[15]There are no indications in any of the published sources mentioned above of who Nollet's university teachers might have been. It is also unclear which college of the University of Paris he attended.

[16]Brockliss, "Natural Philosophy," pp. 57-58.

[17]Brunet, L'introduction. See also A. Rupert Hall, "Newton in France: A New View," History of Science, 13 (1975); 233-250. Hall argued that Brunet, in limiting his study to astronomy and to published material, neglected other, sometimes earlier contacts on a personal level between Newton, Newtonians and French scientists.

[18]Philippe Villemot, Nouveau système, ou nouvelle explication du

mouvement des planètes (Lyons: Chez Louis Declaustre, 1707), last page of preface.

19 Ibid. "I shall well remark . . . that while it already has been some time since Mr Newton published his physical principles of Astronomy, I have not been able to make any use of his discoveries; for his book, very rare in this country, did not fall into my hands until after the composition of my work."

20 Ibid. See Brunet, L'introduction, pp. 10-42, for a discussion of Villemot's work.

21 Brunet, L'introduction, pp. 7-9.

22 Ibid., p. 7.

23 [Jean Baptiste Senac], Nouveau cours de chymie suivant les principes de Newton et de Sthall [sic] (Paris, 1723); cited in Brunet, L'introduction, p. 122. Although the work is often attributed to Senac the identity of the anonymous author is in question. Cf. W. A. Smeaton, "Senac, Jean-Baptiste," in Dictionary of Scientific Biography, Vol. XII (1975), pp. 302-303. "If Mr. Newton says that he is unhappy with the Cartesian philosophy one must not be surprised; he says nothing here that is not said by all those who have examined it."

24 [Antoine Augustin Bruzen de la Martinière], Introduction générale à l'étude des sciences et des belles lettres, en faveur des personnes qui ne savent que le françois (La Haye: Chez Isaac Beauregard, 1731), p. 47; hereafter Bruzen de la Martinière, Introduction. "What they reproach in him is that in spite of the air of novelty that he has been able to give his sytem, he returns to the obscure principles of Aristotle, and he reestablishes them under different names."

25 Louis Castel, Le vrai système de physique générale de M. Isaac Newton, exposé et analysé en parallèle avec celui de Descartes; à la portée du commun des physiciens (Paris: Chez Claude-François Simon, 1743), p. 156; hereafter Castel, Le vrai système.

26 Ibid., p. 157.

27 Ibid., pp. 154-163. On Leibniz' views see the Leibniz-Clarke correspondence in Samuel Clarke, A Collection of Papers, which Passed between the Late Learned Mr. Leibnitz, and Dr. Clarke, in the Years 1715 and 1716. Relating to the Principles of Natural Philosophy and Religion. With an Appendix (London: Printed for James Knapton, at the Crown in St. Paul's Church-Yard, 1717); especially the first two letters by Leibniz, pp. 2-7 and 18-35; hereafter Clarke, A Collection of Papers. The letters or "Papers" appear in both French and English translations of the original Latin.

28 Castel, Le vrai système, pp. 194-195.

[29]Brunet, L'introduction, pp. 153-202; Aiton, Vortex Theory, pp. 209-243.

[30]Bernard le Bovier de Fontenelle, "Sur les mouvemens en tourbillons," Histoire, 1728 (1753), pp. 97-103, at pp. 97-98. ". . . the ingenious system of vortices of Descartes, which presents itself so agreeably to the mind, will collapse under the difficulties presented against it;" ". . . which has difficulties that are as large and more striking, even though it has some quite advantageous aspects."

[31]Idem, "Eloge de M. Neuton," Histoire, 1727 (1729), pp. 151-172, at p. 160. "The two great men, who found themselves in such great opposition, had much in common. Both were geniuses of the first order, born to dominate over other minds, and to found empires. Both excellent geometers saw the necessity of transporting geometry into physics. Both founded their physics on a geometry which they held almost entirely from their own efforts. But one, taking bold flight, wanted to place himself at the source of everything, to make himself master of the first principles through some clear and fundamental ideas, so as to have nothing more to do than descend to the phenomena of nature, as if to so many necessary consequences; the other, more timid or more modest, began his march by relying on the phenomena to rise through them to the unknown principles, determined to accept whichever principles he might arrive at through the chain of consequences. One starts from that which he understands clearly to find the cause of that which he sees. The other starts from that which he sees to find the cause, be it clear, be it obscure. The evident principles of the one do not always lead him to the phenomena such as they are; the phenomena do not always lead the other to principles that are sufficiently certain. The limits, which in these two opposing routes, may have halted the progress of two men of this kind, these are not the limits of their own minds, but those of the human mind in general."

[32]Jean Banières, Examen et réfutation des élémens de la philosophie de Neuton de M. Voltaire, avec une dissertation sur la réflexion & la réfraction de la lumière (Paris: Chez Lambert & Chez Durand, 1739), pp. xciv-xcv; hereafter Banières, Examen. "We have heard it said that some were shocked by the comparison that M. de Fontenelle made between M. Descartes and M. Newton in the eulogy he made of the latter; and which he read at the Royal Academy of Paris, of which M. Newton was a member. Maybe people were not totally wrong to protest. But what may appear surprising, is that those who should have been naturally shocked by the comparason did not say anything, and those who should have been pleased with M. de Fontenelle for having elevated M. Newton to the level of M. Descartes were precisely those who protested."

[33]Ibid., p. xcv.

[34](François Marie Arouet) Voltaire, Elémens de la philosophie de Neuton, mis à la portée de tout le monde (Amsterdam: Chez Etienne Ledet & Compagnie, 1738). On Voltaire's reliance on the éloge of Newton see Schofield, "Evolutionary Taxonomy," p. 182.

[35]Baker (Condorcet, pp. 85-128) presents an incisive exposition of this debate.

[36]Bruzen de la Martinière, Introduction, p. 43. "We are still very far from having a physics that is universally approved of, for that would take a greater number of experiments than we have."

[37]Ibid., pp. 44-45. "Physicists usually commit a mistake, they build a system, as I have said, and thereafter apply experiments to it. Descartes made this mistake. On the contrary, one must assemble experiments, collect the truths which they demonstrate, and wait until there may be enough truths to form a system."

[38]"C'est un système," wrote Dortous de Mairan in 1749, "fait souvent la critique entière d'un livre; se déclarer contre les systèmes, & assurer que ce qu'on va conner au public n'en est pas un, est devenu un lieu commun des préfaces." Jean Jaques Dortous de Mairan, Dissertation sur la glace, ou explication physique de la formation de la glace, & de ses divers phénomènes (Paris: Imprimerie Royale, 1749), p. v. For a discussion of the climate of opinion concerning systematic knowledge see Hine, Condillac.

[39]Stephen Hales, La statique des vegetaux, et l'analyse de l'air. Experiences nouvelles lûes à la Société Royale de Londres. Par M. Hales D.D. & membre de cette Société. Ouvrage traduit de l'anglois, par M. de Buffon, de l'Académie Royale des Sciences (Paris: Chez Debure l'Aîné, 1735), p. v. "It is by precise experiments, reasoned and followed up, that one forces nature to unveil its secret."

[40]"Systême, s.m. (Philos.)," Encyclopédie, ou dictionnaire raisonné des sciences, des arts et des métiers, par une société de gens des lettres (17 vols; Paris: Chez Briasson, David l'aîné, Le Breton, & Durand, 1751-1765 [Vols. 8-17: Neuchâtel: S. Faulche & Compagnie, 1765]), XV, 778a-779b, at p. 778b. The article is attributed to d'Alembert. If it was indeed prepared by him, it was written no later than 1758. D'Alembert resigned from the Encyclopédie that year and no longer contributed any articles. "Cartesianism, which had followed Peripatetism, had made the taste for systems quite fashionable."

[41]Ibid., p. 778b. "Today, thanks to Newton, it seems that we have rid ourselves of this prejudice, and that we only recognize as true physics that which is based on experiments and which clarifies them by exact and precise reasonings and not by vague explanations."

[42]I. Bernard Cohen, Introduction to Newton's 'Principia' (Cambridge: Harvard University Press, 1971), pp. 156-157, 240-245.

[43]Banières, Examen, p. xci.

[44]Ibid., pp. xci-xcii. "One ought not be surprised if all that has been demonstrated about impulsion agrees with attraction, . . ." "whether one supposes, that bodies are being pushed downwards with a stick, or that they are pulled from below with a rope."

[45]Ibid.

[46]Castel, Le vrai systême, p. 441. "If the opinion of M. Newton on colors is a system, or even a hypothesis."

[47]Ibid. "Monsieur Newton has no system, it is said every day, and Newtonians in effect, do not stop protesting against the systems and hypotheses of the Cartesians.

This is to say that these gentlemen demand absolutely that we take as facts, and for experiments, all that pleased their master to endow us with on the subject of colors, and on all of physics in general."

[48]Ibid., p. 442. ". . . the way Descartes and his partisans offer his opinions as systems and hypotheses is more modest and philosophical." ". . . as if they are facts or geometrical demonstrations, is somewhat too proud, too imposing, and even dangerous."

[49]Ibid., p. 443.

[50]Ibid., p. 442.

[51]Salomon-Bayet reports on a note in Nollet's own hand attached to his prospectus for a course in experimental physics in 1735; the note reads in part: "La superstition fondée sur l'ignorance diminuera dans le monde à proportion que plus de personnes connaîtront les causes physiques des effets de la nature, et l'on sait que la superstition cause bien des maux à la société." Claire Salomon-Bayet, L'institution de la science et l'expérience du vivant. Méthode et expérience à l'Académie Royale des Sciences 1666-1793 (Paris: Flammarion, 1978), p. 392; hereafter Salomon-Bayet, L'institution.

[52]Nollet, Programme, pp. xxxi-xxxii. ". . . pour concevoir la cause des effets les plus curieux, les plus communs, les plus intéressans, lorqu'elle est demonstrée d'une maniere sensible & agréable par des faits qui éclairent l'esprit en parlant aux yeux; pour reconnoître dans des cas préparés des loix que la nature suit d'une maniere uniforme dans toutes les occasions; pour acquerir quelques idées capables de fermer par avance toute avenüe à une infinité de préjugés populaires; faut-il autre chose que le sens commun de la part du sujet, & l'attention de ne lui en point faire une étude trop pénible. . . ?"

[53]Ibid., p. xxxi.

[54]Ibid., p. xxx.

[55]Ibid., p. xxxii. ". . . common sense on the part of the subject & and care not to make the study too painful."

[56]Ibid. See for example lessons XI, p. 75; XII, p. 81, XIV, p. 98; XVI, p. 108.

[57]Nollet, Leçons, I, xviii. "I do not present myself here under the auspices of any philosopher."

[58]Ibid., pp. xviii-xix. "Full of respect, and even gratitude for the great men who have shared with us their thoughts, and who have enriched us with their discoveries, from whatever nation they might be, in whatever time they may have lived, I admire their genius even in their errors, and I make it a duty to render them the honor that is their due; but I accept nothing on their word, if it is not struck on the die of experience. In matters of physics, one must not be at all the slave of authority; much less of one's own prejudices; one must recognize truth wherever it shows itself, and not feign being Newtonian in Paris, and Cartesian in London."

[59]Ibid., p. xx.

[60]Ibid. ". . . forestalled by an author, whose work is in everyone's hands, and who has treated this subject with the same felicity that one finds in all his writings."

[61][Noël-Antoine Pluche], Histoire du ciel considéré selon les idées des poëtes, des philosophes, et de Moïse (2 vols.; Paris: Chez la Veuve Estienne, 1739); hereafter Pluche, Histoire du ciel.

[62]Camille Limoges, "Pluche, Noël-Antoine," Dictionary of Scientific Biography, Vol. XI (1975), pp. 42-44.

[63]Pluche, Spectacle.

[64]Pluche, Histoire du ciel, II, p. 218. "very-great genius; and more so because he was the first to encourage us to liberate ourselves from the yoke of Aristotle. . . ." ". . . just as uncertain, and possibly more dangerous."

[65]Ibid., p. 220.

[66]Nollet, "Discours sur les dispositions & sur les qualités qu'il faut avoir pour faire du progrès dans l'étude de la physique expérimentale," in Leçons 7th ed., vol. I (Paris: Chez Durand Neveu, 1771), pp. xlv-xciv, at p. l. Hereafter Nollet, "Discours." This speech was delivered 16 May 1753 at the opening of the new Ecole de Physique Expérimentale established by Royal order.

[67]Pluche, Histoire du ciel, II, 220.

[68]Ibid., pp. 224-225.

[69]Ibid., p. 228 "What temerity to ask here that God reveal to us the essence of his work. . . !"

[70]Ibid., pp. 262-266.

[71]Clarke, A Collection of Papers, pp. 37-53, and 121-153. For a discussion of this issue see Koyré, From the Closed World, pp. 235-272.

[72]The suggestion that Pluche's writings may have roots in pyrrhonism is mine. The similarities between his ideas and those of contemporaneous French pyrrhonists are striking. See Richard H. Popkin, "The High Road to Pyrrhonism," American Philosophical Quarterly, 2, 1965: 18-32, and Popkin, The High Road to Pyrrhonism, ed. by Richard A. Watson and James E. Force (San Diego: Austin Hill Press, 1980).

[73]Ibid.

[74]Nollet, "Discours," pp. xcii-xciii. "Yes, I make a thousand times more of a case of those zealous citizens who apply their knowledge and their talents to render non potable water drinkable, to maintain in its natural state water which one takes along for provision, to purify the air in those areas where it is unhealthy, to render the compass of surer service, to perfect the culture of lands, to conserve the produce of harvests, although all of these subjects have been broached; than these proud savants, who search to dazzle us with an apparent grandeur, one which is often imaginary, or by the singularity of the subjects which they take upon themselves to deal with."

[75]Pluche, Histoire du ciel, II, 274.

[76]Ibid., p. 292.

[77]Ibid., pp. 294-324.

[78]Ibid., p. 314. ". . . the greatest abuse that one can make of attraction . . . would be above all to fancy that this attraction, whose existence is more than uncertain, was the forming cause of the earth."

[79]Nollet, Leçons, II, 150-154.

[80]Ibid., pp. 151-152.

[81]Pluche, Histoire du ciel, II, 322. "What we can boldly put forward, according to the exact truth, and in conformity with the main aim of this history, is that in spite of Aristotle, to the disgrace of Descartes' promises, according to the most sensible moderns, & to the admission of Newton himself, we have no knowledge at all of the essence of nature; & that the structure of each part, as of the whole universe, remains absolutely hidden to us; from which it follows that there is a lot of misjudgement in the esteem accorded to systems of physics, whatever they may be."

[82]Nollet was also acquainted with at least the first two or three volumes of the Spectacle de la nature and thought highly of them; Programme, p. xxxiv. It was volume IV of Pluche's work that contained the rather long history of experimental physics where much of the ideas discussed above are reiterated. Volume IV was published in 1739, one year after the Programme.

[83]Nollet, *Leçons*, I, xx.

[84]Nollet, *Leçons*, II, 75-76. Nollet distinguished between *poids* and *pesanteur*. The latter he identified as that "force" that pulls bodies toward the center of the Earth, hence my decision to translate *pesanteur* as "gravity."

[85]Ibid., p. 76.

[86]Ibid., p. 80. "It appears to me more reasonable to believe that others will be able to do what we were unable to do ourselves than to regard as absolutely impossible that which we have tried to do without success."

[87]Bulffinger, *De causa*; cited in Brunet, *L'introduction*, p. 153. ". . . there is nothing simpler than the Cartesian vortices; one must therefore . . . try everything before abandoning them."

[88]*Leçons*, II, 100.

[89]Ibid., p. 101.

[90]Ibid. ". . . just as the former is uncertain, the latter is equally well established."

[91]Ibid., p. 102. "It is to this Italian *philosophe* that we are indebted for the most interesting discoveries made about this subject."

[92]Ibid.

[93]Ibid., p. 141.

[94]Ibid., pp. 141-142. ". . . he wants us to believe that a stone that would begin to fall from this satellite, would not cover in one minute, the distance that it would cover here in one second." ". . . 3600 times slower than it does in the surroundings of the earth's surface."

[95]Ibid., p. 142. ". . . but that he has based all that he put forward, on proof & demonstrations that hold against the most rigorous test." ". . . with so much likelihood, that this can no longer be taken for a simple conjecture."

[96]Ibid., p. 143.

[97]Ibid., pp. 147-148.

[98]Ibid., p. 411.

[99]Ibid., p. 412.

[100]Ibid., p. 413.

[101]Ibid., p. 414.

[102]Ibid., pp. 414-415. "'We should not flatter ourselves, *he says*, that in our researches in physics we will ever be able to place ourselves beyond all difficulties: but let us not ever stop philosophizing over clear principles of mechanics: if we abandon them, all the light that we can have is extinguished, & there we would be drowned again in the ancient darkness of Peripatetism, from which heaven preserve us.'"

[103]Joseph Saurin, "Examen d'une difficulté considerable proposée par M. Hughens contre le systême cartesien sur la cause de la pesanteur," *Mémoires*, 1709 (1711), pp. 131-148.

[104]Ibid., p. 148.

[105]*Leçons*, II, 416-417, and *Leçons*, VI, 155-157.

[106]*Leçons*, II, 417.

[107]*Leçons*, VI, 151.

[108]Ibid., pp. 152-153. "I do not know if I am mistaken; but it seems to me that Newton has gone about it in a very wise & very reasonable manner: instead of amusing himself by looking for & guessing about primary causes, to deduce later the phenomena as consequences, he started, instead, by carefully examining what went on under his eyes & around him, he has studied the immediate cause; he has applied them to more distant effects, & by moving up in this fashion from the small to the large, from the better known to that which was less so, he was able to explain in a very happy manner, the largest movements of nature; & that which inspires great confidence in the route he has followed, is that by following in his footsteps, by conforming to his method, we are everyday incorporating into his principles detailed phenomena that seemed to elude us, apparent exceptions that he had left behind, or of which we were unaware in his time."

[109]Ibid., p. 156.

[110]Ibid., p. 157n.

[111]*Leçons*, II, 428.

[112]Ibid., pp. 428-429.

[113]Pieter Van Musschenbroek, *Essai de physique par Mr. Pierre Van Musschenbroek, professeur de philosophie & de mathématiques à Utrecht; avec une description de nouvelles sortes de machines pneumatiques, et un recueil d'expériences par Mr. J.V.M. Traduit du hollandois par Mr. Pierre Massuet, docteur en médecine* (2 vols. in 1; Leyden: Chez Samuel Luchtmans, 1739), I, 272-343; hereafter Musschenbroek, *Essai*.

[114]Ibid., pp. 273-274.

[115]Leçons, II, 418. ". . . for it is not to reason correctly to say, this is not at all explained by the laws of impulsion, therefore it is an effect of the attractive virtue."

[116]Musschenbroek, Essai, I, 337.

[117]Pierre Sigorgne, Institutions newtoniennes, ou introduction à la philosophie de M. Newton (Paris: Chez Jacques-François Quillau, fils, 1747), p. 376; hereafter Sigorgne, Institutions (1747).

[118]Ibid., pp. 378-379.

[119]Pierre Sigorgne, Institutions newtoniennes (2d ed.; Paris: Chez Guillyn, 1769), p. 358; hereafter Sigorgne, Institutions (1769).

[120]Nollet, Leçons, II, 410. See Hélène Metzger, Newton, Stahl, Boerhaave et la doctrine chimique (Paris: Félix Alcan, 1930), pp. 34-68, for a discussion of the inefficacy of the notion of attractive virtues in dealing with particles at small distances; hereafter Metzger, La doctrine chimique.

[121]Sigorgne, Institutions (1747), p. 382.

[122]Sigorgne, Institutions (1769), p. 357.

[123]Leçons, II, 429-430. "From all this it results that these phenomena are either not yet well explained, or that the explanations that are given rely on hypotheses that are not widely accepted. Maybe this comes from our obstinacy in giving them a one and only cause . . . The unequal pressure of some fluid is probably the fundamental point of the explanation; but adhesion or the natural viscosity of liquids, the size and shape of their parts, . . . &c. are so many means that Nature may employ for these kinds of effects, . . ."

[124]Review in Journal des sçavans, 1744, (Janvier), pp. 17-23, at 21. "M. L'Ab. N. [is] quite restrained with his conjectures."

[125]Leçons, V, 7.

[126]Ibid., p. 10. ". . . at times a heavenly substance that emanates from the stars, at times a terrestrial matter that inflammation develops."

[127]Ibid., pp. 10-11.

[128]Ibid., p. 9.

[129]Ibid., p. 320.

[130]Programme, pp. 86-90.

[131]Mémoires pour l'histoire des sciences & des beaux arts

[Mémoires de Trévoux], 1738 [November, Vol. IV], pp. 2228-2236, at p. 2233.

132 Leçons, V, 321.

133 Ibid., p. 322. "I stop, with the English philosopher, with sensible effects, that can serve to explain the phenomena of vision that have a bearing on colors."

134 For a discussion on the attempt to reconcile see Thomas L. Hankins, "The Influence of Malebranche on the Science of Mechanics during the Eighteenth Century," Journal of the History of Ideas 28 (1967): 193-210; Martin Fichman, "Privat de Molières," Dictionary of Scientific Biography Vol. XI (1975), pp. 157-158; Henry Guerlac, "Some Areas for Further Newtonian Studies," History of Science, 17 (1979): 75-101; Aiton, Vortex Theory, pp. 209-243.

135 Brunet, Les physiciens hollandais, p. 125.

136 Mémoires pour l'histoire des sciences & des beaux arts [Mémoires de Trévoux], 1744 [August, Vol. II], pp. 1390-1418, at p. 1392. Most likely by Castel.

137 Nollet, "Discours," p. lxiii. "Let us beware of all authors that have systems to uphold; let us beware of ourselves, if we have adopted them."

138 Ibid., p. lxi. "Hé! What need is there to be of definite voice & on every occasion a Cartesian, Newtonian, Leibnizian, &c? Did anyone of these great men, whose authority has so much weight, have infallibility as his quality? Can one not respect their memory, admire their genius, profit from their discoveries, without attaching oneself specifically to one of them, without denying oneself the liberty of examining their opinions, to distance oneself even, when new knowledge comes to show us what these opinions have that is defective? Why take indiscriminately all that is contained in a single treasure, when we are allowed to open up several, to enrich ourselves selectively?"

139 Pluche, Spectacle, IV, 452; Voltaire, Correspondance, II, 126, "à M. Des Alleurs," 13 March 1739.

140 Voltaire, Notebooks, ed. by Theodore Besterman (2nd ed.; Toronto: University of Toronto Press, 1968). "A simple mechanic like the Abbé Nollet who knows nothing other than new experiments. . . ." "is a better physicist than Democratus and Descartes." ". . . but he knows more and better."

CHAPTER III

NOLLET AND EXPERIMENTAL METHOD

> S'il falloit juger du mérite d'un homme par la réputation dont il a joui pendant sa vie et par le nombre d'éditions des ouvrages qu'il a publiés, personne n'auroit plus de droits que Nollet à la reconnoissance, peut-être même à l'admiration de la postérité. Cette manière de juger n'est pas exacte. Les réputations sont souvent le fruit du charlatanisme et de l'intrigue; . . .[1]

The author of the above passage, Antoine Libes (1752-1832), professor of physics at the Paris écoles centrales, believed that Nollet's merit should be judged not by his reputation in his time but by the substance of his real contributions to physics. These contributions, according to Libes, were Nollet's construction of improved pneumatic machines, his experiments on electricity--some useful, some entertaining--and, above all, his zeal in popularizing science. While his lectures to Parisian audiences were successful, he failed in his writings, for he lacked the necessary talent to synthesize experience and observation with mathematics (calcul) into a unified physics.[2] In an earlier work, Libes had been even less kind to Nollet, whom he characterized as symvolic of what had gone wrong with physics and its study in France.[3] Nollet had contributed to banishing the sterile systematic philosophy from French schools and instituting experimental physics in its stead, but this service rendered science would have had greater merit, Libes wrote,

> si son estimable Auteur eût su éviter le danger de l'enthousiasme si dangereux et si commun à l'époque des nouvelles découvertes; s'il

> eût su ne pas dédaigner les secours de la géométrie, donner à ses leçons une marche plus mâle et plus rapide, interroger avec plus de ménagement la nature, ou du moins ne jamais interpréter son langage lorsque ses réponses arrachées par un indiscrète importunité, étoient équivoques ou obscures.[4]

Had Nollet been able to do this he would have given his lectures a more vigorous character that would have saved them from the ravages of time and "sous le nom perfide de physique expérimentale, la physique ne fût point devenue le jouet de l'enfance, l'instrument du charlatanisme."[5] There is no physics without experiments, Libes wrote, but purely experimental physics does not offer the reflecting mind anything but a collection of toys amidst the rich furniture of nature.[6]

Libes' assessment of Nollet's work reflected a dissatisfaction with a lack of vitality in physics shared by others of his contemporaries. Jean-Baptiste-Joseph Delambre (1749-1822) commented that the noticeable decline of interest in the study of physics was caused, in part, by the increased interest in other fields closely related to it and which were impinging on its domain. Delambre believed that this was an inevitable consequence of the progress of physics which had now become "un champ presque épuisé."[7] Chemistry, which appeared a more fruitful field, was attracting greater interest. Some thirty years earlier Lavoisier had already registered the opinion that physics was being neglected.[8] Libes judged that this unfortunate turn of events was true because physics, as practiced by the likes of Nollet, had failed to incorporate geometry and chemistry into its domain. Physicists had reduced their field to the simple study of particular facts.[9]

Libes' attitudes have their counterparts in recent histories. To most modern historians Nollet is no more than what he was to Libes, a popularizer, or to use Burkhardt's term, an "impressario" of science.[10]

I. B. Cohen, commenting on Nollet's electrical theory, his most important theoretical contribution, judged that: "So far as the growth of scientific ideas is concerned, this theory might just as well never have existed at all."[11] More recently still, in a more sympathetic study of Nollet's electrical work, E. Yamazaki agreed with the Franklinian Jacques Barbeu Dubourg (1709-1779) in his assessment that Nollet's method was that of a simple botanist who "teaches us that trees have trunks, roots, branches, leaves, . . ."[12]

Why is the judgement of Nollet's merit by Libes and modern historians in such contrast with the esteem the Abbé enjoyed during his lifetime? What entitled Nollet to what Roger Hahn has called that "most coveted prize," election to the Académie des Sciences, or the appointment to the first chair in experimental physics in France and election to the major academies of Europe?

Part of the answer may be, indeed, the esteem the Abbé enjoyed as a popular public lecturer. In a period when science was one of the more serious pastimes of the educated public, the favorable reactions to Nollet's lectures and Leçons are understandable.[13] The Abbé, whose livelihood depended in large part on his success as a teacher, geared his lectures--and his physics--to attract an audience, of varied interests and backgrounds, infatuated with science. Moreover, his "useful and agreeable" course was attuned to contemporary developments in physics and he presented them in that most fashionable of modes, the experimental method. Nollet also contributed to the utilitarian domain of science. In chapter one I mentioned his contributions to the Mémoires and the Descriptions des arts et métiers on a variety of subjects of interest to industry and agriculture.

The eighteenth-century scientific community recognized a responsibility toward society at large. Scientists, and intellectuals in general, believed they had a social function to fulfill as educators and enlightened citizens. The spiritual and material reformation of society was to be led by the new learning of which they were the guardians and dispensers.[14] From that perspective, it is understandable that the works of the Abbé met with the approbation of the scientific commmunity. However, this same scientific community made a distinction between the broader, popular role of the scientist and his contributions to science. Popularity alone could not be the criterion of merit. While the scientist was expected and encouraged to engage in public responsibilities, whatever popular recognition and acclaim he derived from playing that role could not replace the more demanding judgement of his scientific peers in what was loosely called the "Republic of Letters."[15]

I hope to show in the next chapter that among Nollet's contributions to science was his work in electricity. There is no doubt that Nollet's electrical experiments and his electrical theory were highly regarded by scientists of his day. His explanation of the cause of electricity remained practically unchallenged in France at least until the introduction of Franklin's work in 1752 and, it has been argued, the more widely accepted explanation until the Abbé's death in 1770. However, electricity was only one of the Abbé's interests, and by 1745, the year he presented his "Conjectures," he was already sufficiently well known for his experimental natural philosophy to be enjoying the respected position of associate at the Académie.

In what follows I argue that Nollet's reputation was not a result of his electrical theory alone but was largely derived from the manner in which he practiced physics and not from any results he obtained. As a demonstrator of experiments, a lecturer in experimental physics and an instrument maker, Nollet participated in the reshaping of the field of physics in the middle decades of the eighteenth century. During Nollet's lifetime physics was brought into the laboratory, and it became increasingly defined as that enterprise carried out in the cabinet de physique, with a standard set of instruments and procedures.[16] This accomplishment was not the work of Nollet alone, nor was he solely responsible for its inception or introduction into France. It was the result of a process already apparent early in the seventeenth century in the works of such men as Robert Boyle (1627-1691), Robert Hooke (1635-1703) and Francis Hauksbee (c. 1666-1713) in England, Evangelista Torricelli (1608-1647) and the members of the Accademia del Cimento in Italy, and Blaise Pascal (1623-1662) and Edmé Mariotte (d. 1684) in France.[17] Physics in the seventeenth century was already becoming experimental, or, perhaps more precisely, the scope of physics was already being narrowed to that which could be carried out experimentally.[18]

This process resulted in a transformation of the field of physics which from its past definition as the study of the natural world in its many and varied facets became, toward the end of the eighteenth century, largely limited to the study of those topics we more readily understand as its particular domain, such as mechanics, optics, hydraulics, and electricity. This development also meant that mathematics remained outside the mainstream of physical studies.[19] Although forever lauded in physics textbooks and prefaces to physical treatises, mathematics remained throughout the first half of the eighteenth century confined to occasional appearances in treatments of celestial mechanics and optics. It was only in the latter half of the century that attempts at quantifying physics became a more predominant concern of physics. Once physics became comfortably installed in the laboratory, higher precision in its instruments was achievable.[20]

It was this process outlined above, the standardization of physics as a science of the laboratory, a science of instruments and rules for procedure, that Nollet helped carry to maturity in the eighteenth century. It was a process premised on a notion of scientific progress that saw knowledge about the physical world accumulating through successive generations of scientists. The individual scientist considered his work as part of a collective endeavor that assured gradual ascent to higher knowledge. Agreement among members of this scientific community became essential to the furtherance of this endeavor. And laboratory experimentation, in this context, received a special meaning: it was an operation amenable to standardization. Experimental physics in the eighteenth century thrived by the communal accord of its practitioners.[21]

The notion of scientific progress was already evident in the seventeenth century and modern historians of that period have drawn considerable attention to it. As Paolo Rossi has emphasized, it was a peculiarly modern notion which received its first and most celebrated exposition in the works of Francis Bacon (1561-1626).[22] The seventeenth-century believer in scientific progress, Rossi wrote, regarded science as "an edifice, constructed laboriously in slow stages, which is never really finished and to which each one can make his contribution to the limits of his powers and capacities."[23] According to Rossi, this view of science was distinct from any other known to antiquity or the scholastic period in at least three different ways. First, it was a view predicated on the belief that science grows through the successive contributions of scholars. Second, it acknowledged that even though the process is continuous and cumulative it is ever in need of revision and adjustment. Third, this view premised that there is a single scientific tradition, and not an assemblage of theories or "isms" set in opposition to each other. This tradition seeks to appropriate the kernels of truth acquired by previous generations and incorporate them into general theories in which the earlier ones are identified as particular stages.[24] Collaboration among scientists, their joint beliefs, and a communal acceptance of ideas become central to the scientific undertaking. Institutions are not only organized to promote science but also to institutionalize it. These ideas played an important role in Nollet's constant emphasis on agreement between physicists as a source for validity of his views.

The view that science was a communal effort, an edifice being built by a community of physicists, meant in fact that philosophical consensus was an essential element of the new science. Consensus or lack of it among scientists became a major preoccupation of Nollet. Issues in dispute were either dismissed from his physics entirely, his intention being, as he wrote, to limit himself to those facts least contested, or, when present, discussed as simple conjectures. He would often point to the consensus among physicists as an argument for the validity of an issue. He believed that only those disagreements that could be settled by experiment were worthy of being discussed, and, if possible, settled.[25] It is in this context that his dispute with Privat de Molières in 1741 is to be understood. If issues could not be thus settled they were to be subordinated to the status of conjectures. Physics could only accept those truths "frappés au coin de l'expérience."[26] This notion of collective assent to certain knowledge guided Nollet throughout his work.

Physics in the last twenty centuries, Nollet wrote in the preface to his Leçons, had been nothing but "un vain assemblage de systêmes appuyés les uns sur les autres, & assez souvent opposés entr'eux." Each philosopher believing himself "en droit d'élever un pareil édifice à sa mémoire, s'est efforcé de l'établir sur les ruines de ceux qui l'avoient précédé." The result of this disjointed individual work was that again and again a "vraisemblance en effaçoit cent autres."[27] In modern times "l'on se fit une loi de n'admettre au rang des connoissances, que ce qui paroîtroit évidemment vrai." Knowledge is now continuously on the increase. Physics textbooks are in constant need of being revised for physical science "se perfectionne tous les jours; les découvertes se multiplient, les erreurs se corrigent, les doutes s'éclaircissent."[28]

J. B. Bury, in the Idea of Progress, stated that before Francis Bacon only scanty references to the idea of progress are to be found in the literature.[29] Bury believed that before the concept could develop three necessary conditions had to emerge. First, the intellectual subservience to the thought of the classical philosophers had to be undermined. Second, man's life on this earth had to be valued for its own sake--Bury's concern was, after all, with the larger cultural notion of progress. Third, the conviction had to be formulated that the laws of nature were invariable and determinate. Certainty that knowledge would continually improve depended on placing it on sure foundations, and these had to be general, immutable laws of nature. Bury credited Descartes with providing the theoretical framework for this third condition.[30]

According to Bury, these conditions were inchoate throughout the sixteenth and seventeenth centuries until Europe was ready for its first theory of progress enunciated in the eighteenth-century works of the Abbé Eustache de Saint Pierre (1658-1743).[31]

However, scientific treatises of the seventeenth century carry ample expositions of the idea of progress. In addition to the works of Bacon, the view is clearly present in the works of Pierre Borel (1620-1671), Pierre Gassendi (1592-1655) and Joseph Glanvill (1636-1680).[32] It was clearly stated by Pascal in his Pensées, where we find the thesis that knowledge is the fruit of a social, co-operative enterprise extending through generations.[33] Our ability to learn from our ancestors, according to Pascal, distinguishes us from animals. Beasts have to learn all they know in each generation.

> Il n'en est pas ainsi de l'homme, qui n'est produit que pour l'infinité. Il est dans l'ignorance au premier âge de sa vie; mais il s'instruit sans cesse dans son progrès: car il tire advantage, non-seulement de sa propre expérience, mais encore de ses prédécesseurs; parce qu'il garde toujours dans sa mémoire les connoissances qu'il s'est une fois acquises, & que celles des Anciens lui sont toujours présentes dans les Livres qu'ils en ont laissés. Et comme il conserve ces connoissances, il peut aunsi les augmenter facilement; de sorte que les hommes sont aujourd'hui en quelque sorte dans le même état où se trouveroient ces anciens Philosophes, s'ils pouvoient avoir vieilli jusqu'à présent, en ajoutant aux connoissances qu'ils avoient, celles que leurs études auroient pu leur acquérir à la faveur de tànt de siecles.[34]

The analogy Pascal drew between knowledge learnt by the successive generations of mankind and the learning processes of a growing man was often repeated.[35] We know more than the ancients because we are older; it is we who are the ancients.[36] As the universe gets older "tous les hommes ensemble y font un continuel progrés," for the same thing happens "dans la succession des hommes, que dans les âges différents d'un particulier."

> De sorte que toute la suite des hommes, pendant le cours de tant des siecles, doit être considérée comme un même homme qui subsiste toujours, & qui apprend continuellement: d'où l'on voit avec combien d'injustice nous respectons l'Antiquité dans ses Philosophes; car comme la vieillesse est l'âge le plus distant de l'enfance, qui ne voit que la vieillesse de cet homme universel ne doit pas être cherchée dans les temps proches de sa naissance, mais dans ceux qui en sont les plus éloignés?[37]

Pascal was engaged in that quarrel between the "moderns" and the "ancients" known as the Battle of the Books.[38] This battle between the defenders of the ancients and those of the moderns was fought mostly over literary matters. At issue was whether the great poetry and eloquence of the ancients were or could be superseded or equalled by the moderns. The debate, which lasted for a good part of the seventeenth century, was later characterized by some as a malentendu over matters of taste, an unfortunate lack of recognition that, after all, les gouts ne se discutent pas.[39] But the debate over literary eloquence was only the medium through which other ideas were also being disputed. "Au fond du débat," wrote the historian Rigault, "il y avait une idée philosophique, une des plus grandes qui puissent être proposées à l'esprit humain, parce qu'elle intéresse la dignité de sa nature, l'idée du progrès intellectuel de l'humanité."[40] The debate had implications that ranged over the whole field of man's knowledge. So far as the sciences were concerned, most protagonists agreed that man's knowledge had progressed. Fontenelle, a commentator and contributor to the debate, expressed the common view when he distinguished between those intellectual fields that required a slow, cumulative development and poetic eloquence which depended solely on a lively and cultivated imagination.[41]

> . . . la physique, la médecine, les mathématiques, sont composées d'un nombre infini de vues, et dépendent de la justesse du raisonnement, qui se perfectionne avec une extrême lenteur, et se perfectionne toujours; il faut même souvent qu'elles soient aidées par des

> expériences que le hasard seul fait naître, et qu'il n'amène pas à point nommé. Il est évident que tout cela n'a point de fin, et que les derniers physiciens ou mathématiciens devront naturellement être les plus habiles.[42]

Fontenelle, who also used the analogy between the increasing wisdom of the maturing man and the increasing wisdom of successive generations of mankind, believed the analogy faltered on one point. For, while the individual wise man grows old and senile, the wisdom of mankind is forever on the increase.[43]

The "Battle of the Books" spanned the seventeenth century, and was carried out at the Académie Française, and in books, pamphlets and journals. One of its important consequences was the popularization of the idea of progress.[44]

Another aspect of this process, the growing belief in the development of man's scientific knowledge, was a concurrent transformation in the notion of history. To paraphrase Bury: the notion of human progress could not have flourished nor could it have survived on the slender foundation of abstract arguments. It would have to be judged by the evidence of history. And, according to Bury, "contemporaneously with the advent of this idea, the study of history underwent a revolution."[45]

The revolutionary development in the study of history Bury was referring to was that first glimpse of historical writing that interprets the present as a consequence of past processes. Often, this "historicist" conception appeared in eighteenth-century texts juxtaposed with its opposite, the idea of an almost abrupt break with the past, the bursting forth of Reason after centuries of darkness.[46] The latter attitude is evident in the works of a number of scientists who usually traced their tradition no further than the sixteenth or seventeenth century. Similarly, Nollet also believed that in the last one hundred years physics had progressed much more than in previous centuries.[47] Like many of his contemporaries Nollet credited Descartes with introducing the new experimental method into physics and of liberating it from the yoke of authority.[48] However, while the more fruitful period of the history of experimental physics was confined to the last century, Nollet pointed out that men had forever been "occupés, ou par goût, ou par état, à dévoiler & à contempler les merveilles de la Nature."[49] Contemporaneous histories of the sciences also traced the development of their fields from antiquity. One such work, by an author known and appreciated by Nollet, was Pluche's history of experimental physics in volume four of the Spectacle de la nature.[50] There Pluche traced the development of experimental physics back to the time of Creation, emphasizing the utilitarian nature of all knowledge. Pluche believed that the only knowledge available to man was that limited to particular discrete matters of fact which could help him cope with this life on Earth. As societies developed and man's needs varied his knowledge of facts grew.[51]

Another well-known history of the sciences was that of Antoine-Yves Goguet (1716-1758). His work, De l'origine des loix, des arts, et des sciences; et de leurs progrès chez les anciens peuples, was a model of careful scholarship.[52] Taking uncharacteristic care to annotate his three-volume work, citing his authorities by title and page, Goguet presented his history of the sciences as a developmental accumulation of knowledge. As the century advanced, many other histories appeared tracing the development of their fields as progressing toward present knowledge. The best known may be those by Jean Etienne Montucla (1725-1799) and Jean Sylvain Bailly (1736-1793), but there were others as well, including a large number of histories of electricity.[53] An early history of electricity written in just this "historicist" style by Nollet's mentor, Dufay, will be examined in the next chapter.

This belief in the progress of man's knowledge is a clear premise of Nollet's own work. It is in such a context that his work is to be appreciated. Physics was, to Nollet, a communal effort where each physicist was expected to contribute his own work to the single edifice of knowledge. In building this edifice the achievements of others had to be considered and incorporated. Nothing that was not solidly accepted by the community of physicists could, under these conditions, become part of the edifice. Cognizant of this fact, the experimental physicist, according to Nollet, should also perform his work so that others could, in turn, rely on it.

In his inaugural speech to the charter class in experimental physics at the College de Navarre, Nollet identified two things that anyone interested in applying himself to the study of physics should do:

> La premiere, & par laquelle il faut commencer, est de se mettre bien au fait de certaines vérités qui sont reçues comme principes, & de s'instruire de toutes les découvertes qui ont été faites avant nous. La seconde, est de travailler à augmenter ce premier fond de connoissances, par ses propres recherches, ou en profitant de celles des contemporains.[54]

The surest way for a physicist to help augment this "fond de connoissances" was to apply himself to experiments and observation. Nollet distinguished between the simple, or rather passive observation of the world and the consciously pursued, manipulative questionning of experience as a source of new knowledge. "Par la première on épie, pour ainsi dire, la Nature à dessein de lui surprendre son secret; par la seconde o[n] lui fait violence pour la forcer à le dire."[55] Nollet cautioned that both of these arts are difficult to perform; "il faut des dispositions naturelles, des qualités & des attentions particulieres, des secours qu'on n'est pas toujours en état de se procurer."[56]

Some of the advice that followed this counsel underscores the fact that experimental physics, as an _institutionalized_ activity, was still very much in a nascent stage:

> Un Observateur, dans quelque partie que ce soit de la Physique, doit avoir une patience à toute épreuve, une attention à laquelle il n'échappe aucune circonstance, une prompte & vive pénétration, une imagination sage & modérée. . . .[57]

The careful "observateur" must also observe with the utmost scrutiny "le temps, le lieu, l'état actuel de l'Atmosphere, la quantité, la durée, la forme, la couleur, l'odeur & les autres qualités sensibles."[58]

When possible, experiments should be performed simply and at little cost, with instruments that are neither elaborate nor cumbersome.

> . . . plus on y fera entrer préparations & de moyens, plus on aura à craindre de prendre le change sur la vraie cause des effets . . . Si l'on emploie une grande quantité de matieres, lorsqu'une moindre suffit; si l'on fait les frais de vaisseaux précieux, de machines bien fines, avant que d'avoir fait des essais qui en garantissent l'utilité, on se jette dans des dépenses superflues, & souvent on se met par-là hors d'état d'en faire d'autres qui seroient nécessaires, ou bien on en perd tout-à-fait le goût.[59]

This commonsensical, albeit judicious, advice could be found elsewhere in Nollet's works, and was not uncharacteristic of other texts on the art of performing experiments. André François Boureau Deslandes (1690-1757), in his 1736 "Discours sur la meilleure manière de faire les expériences," enumerated five essential rules for the experimenter: 1) he should be careful of the weather, and be aware that results of experiments conducted at night may differ from those conducted during the day; 2) the experimenter must be attentive to the fact that experiments conducted in different seasons of the year may have different results; 3) there may be occasions when the wind is a factor; 4) the experimenter's own physical condition, e.g. wet or cold hands, should not interfere with the experiment. The fifth rule was that the experimenter must have all the instruments that he needs, and must know how to use them.[60]

Concern over the instruments of experimental physics was a central point of Nollet's own work. His first published work, the Programme of 1738, detailed three hundred and forty five instruments and materials required for the practice of his lectures.[61] Later in life, he published a three-volume manual, the Art des expériences, dedicated entirely to the art of instrument making and the performing of experiments and meant to serve as an appendix to his Leçons de physique.[62] Nollet made clear the reason for his concern with the identification and enumeration of instruments. Reliance on the works of others was an essential aspect of the practice of physics. If the accumulation of scientific knowledge was to be possible each physicist had to perform, and describe, his work carefully.

> La vie & les facultés d'un homme ne suffiroient pas pour répéter généralement toutes les Expériences qui viennent à sa connoissance: on est souvent obligé de s'en reposer sur la foi d'autrui: mais, pour ne point donner sa confiance au hasard & trop légerement, il faut la régler suivant le mérite des Auteurs, & le soin qu'ils ont pris de nous motiver ce qu'ils nous proposent à croire. . . . Tout Physicien qui veut faire part de ces découvertes, doit donc exposer en détail de quelque maniere il a conduit ses Expériences, dans quelles circonstances il les a faites, & tous les effets qu'il a apperçus, avec leur nombre, leur grandeur, leurs différences, &c. & n'en supprimer que ce qui est visiblement inutile & capable de produire une fastidieuse prolixité.[63]

Only by this careful attention to detail could one hope to be taken seriously by other physicists; "il est important de ne souffrir dans son travail aucune négligence, aucune manipulation vicieuse, qui puisse le rendre suspect."[64] In addition to his zeal, the experimenter should possess considerable knowledge of machines and the resources to acquire them.

> La dépense qu'exige l'acquisition des Instruments nécessaires, & la difficulté de les faire construire dans les lieux où l'on manque d'Ouvriers capables, est sans doute un des plus grands obstacles que l'on ait à surmonter dans la Physique expérimentale.[65]

There was much truth to Voltaire's lamentation that without money to buy instruments one could not hope to be a savant in the eighteenth century.

Physics as practiced by Nollet was characterized not only by its emphasis on experiments but also by the use it made of them. Various approaches to the use of experiments are noticeable in the works of early eighteenth-century physicists. One approach, identified with Descartes and the Cartesians, assigned experiments the role of confirming or adjudicating between notions developed independently by the mind.[66] It belittled experiments done haphazardly and without direction. This approach was defended by Descartes and later by such famous Cartesians as Jacques Rohault (1620-1672) and Castel. A second approach, identified with Bacon, Galileo, Torricelli, and later with Newton, gave logical and chronological priority to experience rather than to reason. This approach was defended by the Dutch physicists Boerhaave, Musschenbroek and 'sGravesande.[67]

There were other experimentalists who either did not see matters from these two logical categories, or were simply content to emphasize recourse to experience and the tangible as a primary preoccupation of the scientific method. To Pluche and the pyrrhonists, for example, experimentation or simple observation of facts were ends in themselves. The role of experiments was neither to provide bases for theories nor to confirm them. Experiments were to be used to assemble knowledge about particulars. They approached science with a strong emphasis on its utilitarian aspect, and to them Cartesianism, Newtonianism or any system was anathema.

Nollet's own views of the role of experiments were closer to those of the Baconians. Experiments and observations were expected, in his method, to form the basis for a new physics. To elucidate his views let me contrast them with those of two earlier French lecturers in physics, Rohault and Polinière. While both of these men made much use of experiments we will see that each had a very different notion of what role they played in physics.

Rohault had a career similar in many ways to Nollet's. He was the best known of the French lecturers in experimental physics of the seventeenth century. His lectures and demonstrations--begun sometime around 1650--were Parisian social events; held weekly each Wednesday, they were attended by scholars and socialites, men and women of all ages who came from Paris, the provinces and even abroad. Rohault had in his time, and throughout the eighteenth century, a reputation that, according to Paul Mouy, paralleled that of Descartes.[68] His reputation was a result of his talent in popularizing Cartesian science among large segments of the public for the first time. He lectured on each of the major problems of natural philosophy beginning each session with a survey of the general nature of the subjects under consideration. From an exposition of the basic mechanical principles of Descartes' philosophy he moved to particular phenomena, confirming the explanation by experiment.[69] In

1671 Rohault published his Traité de Physique, a work that underwent twelve editions by the 1720's and became the leading textbook on natural philosophy of the time.[70] The Traité was heavily illustrated with experiments and demonstrations from Rohault's lectures.

However, Rohault's use of experiments and his belief in their usefulness for physics differed sharply from those of Nollet. Experiments, Rohault wrote, are necessary to physics, but "vouloir absolument rejetter le raisonnement pour ne faire que des experiences, c'est se jetter dans une extremité beaucoup plus prejudiciable que la premiere."[71] To shy away entirely from reason and rely solely on the senses would enclose our search for knowledge within narrow limits, for experience cannot serve but to acquaint us with gross and sensible things. To proceed correctly in the study of natural phenomena one must necessarily join together, in an alliance, reasoning and experience. That alliance would determine how wisely experience is used in physics.

There are, Rohault wrote, three ways in which experience can be used. "La premiere, à proprement parler, n'est qu'un simple usage des sens, comme lorsque par hazard & sans dessein, jettant les yeux sur les choses qui sont alentour de nous, nous ne faisons que les regarder. . . ."[72] Another way of using experience is that which, "lorsque de propos déliberé, mais sans savoir ni prévoir ce qui pourra arriver, l'on fait epreuve de quelque chose."[73] This is the manner in which chemists proceed, choosing one subject, then another, performing on each "toutes les tentatives dont l'on se peut aviser," and keeping a record of all of them so that they may in the future use the same means to arrive at the same ends.[74] When we observe craftsmen work and prepare their materials, we are in fact experiencing in this second manner.

Finally there are those experiences "que le raisonnement prévient, & qui servent à justifier ensuite s'il est faux, ou s'il est juste."[75] For example, if after considering the many attributes of a subject we arrive at an idea of its nature, we can test our conclusion by checking to see if under different circumstances the effects we expect will also follow. This third type of "experience" is of special utility to philosophers because it can help them discover the truth or falsity of their opinions. The two former types of experiments although not as "noble" as the third, are not to be rejected as useless by physicists. For in addition to helping physicists broaden their knowledge, they also serve to suggest initial conjectures, and keep physicists from falling into errors they would otherwise entertain. However, it was clearly the third, nobler kind of "experience" that Rohault considered it the task of physics to develop. These were premeditated experiments geared not to discover but to test a thought--to validate or invalidate notions arrived at rationally.[76]

This view of the role of experiment was very much that of Descartes, to whom particular experiments were useless "si on ne connoist la verité des choses. . . ."[77] Only after we are sufficiently equipped with a general knowledge of nature are we to indulge in particular experiences. Knowledge had first to proceed from thought itself, and to experiment or go after experience without knowing what one was after was a futile exercise. Experience, while important, could only help improve scientific knowledge in matters of detail after a general view of the nature of things had been established. Rohault's and Descartes' views contrasted with those of Nollet, to whom experiments could proceed from simple "soupçons" but not from a preestablished general view; for experiments were not simply meant to refine a given theory but, rather, to make possible its formulation. In the "Discours" Nollet wrote:

> Je dis qu'on a des vues, & qu'on doit en avoir quand on entreprend de nouvelles Expériences; mais ces vues ne doivent nous permettre que de simples soupçons, ou tout au plus des suppositions, pour lesquelles il ne faut prendre aucun attachement, aucune prédilection, afin qu'on soit toujours prêt à les abandonner, si les faits ne concourent point à les vérifier, ou du moins à les rendre très-plausibles.[78]

Another popular lecturer in experimental physics, with whom Nollet was acquainted through contacts at the Société des Arts, was Polinière. Polinière began to acquire a considerable reputation as a lecturer and demonstrator in experimental physics around 1690. His reputation was such that Fontenelle entrusted him with the education of his nephew, and the King appointed him tutor to the Duc d'Orleans. He was one of the first on the continent to adopt and advocate Newton's theory of colors.[79]

Polinière published his Expériences de physique in 1709, a work that grew out of demonstrations and lectures which he presented at the University of Paris upon request from the Faculty of Philosophy.[80] A second revised and enlarged edition came out in 1718, and three more editions were published, the last one posthumously in 1741.[81] Although Polinière made some original contributions to the theory of luminescence, his fame and prestige resulted from his activities as a lecturer and demonstrator of experimental physics.[82]

Polinière's views on the role of experiments in physics were not as clearly elaborated as were those of Rohault in the Traité. And the Expériences de physique was, indeed, as the title indicated, a simple collection of experiments and not a physical treatise. Like Rohault, Polinière expressed the opinion that experiments were to serve as checks on reason--to adjudicate between possible causal explanations postulated by the mind.

> En effet si les raisonnemens qu'on fait sur les proprietez des corps ne sont appuyez sur l'experience, ils ne peuvent passer que pour des conjectures incertaines, pour ne pas dire des pures imaginations. Car y ayant une infinité de choses possibles, il peut souvent arriver qu'on attribue des effets à d'autres causes qu'à celles qui les produisent. Pour choisir donc surement parmi ces causes possibles celles qui produisent veritablement les effets qui sont le sujet de nos meditations, nous ne devons fonder nos jugemens que sur les réponses que la Nature nous fait elle même dans les experiences, qui sont la seule voye par laquelle il nous est possible de l'interroger & de la contempler telle qu'elle est.[83]

However, other than their role in determining true causes, Polinière added that experiments also serve to suggest new discoveries and understanding which could not be arrived at otherwise.

> Souvent la connoissance d'un fait produit une autre connoissance. On se trouve qulquefoiss conduit comme de main en main à des lumieres que la plus subtile speculation & la meditation la plus profonde, n'auroient j'amais [sic] appreques sans le secours des experiences.[84]

These views clearly go beyond the tasks assigned to experimentation by Rohault. Experiments are no longer limited to being devices to check on reasoning but are also guides to causal explanations and to the discovery of new facts. Furthermore, the format of Polinière's book and the presentation of his experiments differed from those of Rohault's Traité. While Rohault presented an outline of the major physical subjects, illustrating and "confirming" them with experiments, Polinière's Expériences was a simple collection of experiments rather than a textbook in physics. In this sense, the presentation of Polinière's work was not in the Cartesian mold--experiments were here presented as subjects of interest independent of being part of an overall physical system. This value placed on experiments of and for themselves placed Polinière closer methodologically to Nollet and the type of experimentation advocated in the works of the Dutch experimentalists.

This approach to physics had come under attack by Cartesians who considered it contrived, directionless and meaningless. In a review, in the *Mémoires de Trévoux*, of the recently introduced French translation of 'sGravesande's *Elemens de physique*, Castel found occasion to attack this type of experimentation.[85] Castel derided the excessive attention given to experiments that seemed to be totally useless. For what, he asked, was "cet attirail d'experiences, de recherches penibles, de creusets & d'alembics, où sous prétexte que la nature veut qu'on lui arrache son secret, on la met sans cesse à la torture, l'alterant, la déguisant pour la mieux connoître."[86]

In later editions of his *Expériences*, Polinière added a prefatory "Réflexions sur ces experiences," that can be read as a reply to such criticisms. The experiments in his book, he told the reader, were not "un amas d'observations de differentes especes qui soit inutile, confus, amusant, sans dessein & sans consequences." These were "de materiaux recherchez avec choix, préparez & arrangez avec methode, & qui peuvent être considerez comme des fondemens d'une Physique exacte."[87] Experiments were the bases of physics and not just a tool for its perfection or demonstration.

> Les changemens continuels qui arrivent dans le langage, dans les goûts de differens siecles, dans le moeurs, & même les affaires du temps, rendent passagers, & font souvent mettre en oubli des ouvrages qu'on avoit estimez. Mais ce qui est contenu dans celui-ci, n'est point exposé à ces inconstances. Les sujets que j'y traite & les effets que j'y représente sont toujours les mêmes; le vrai que j'y annonce sera reconnu en tout temps & en tout lieu, sera toujours nouveau malgré sa vieillesse, & causera toujours de l'admiration à ceux qui commenceront à en avoir connoissance.[88]

Polinière, like Nollet, regarded his work as much more than just simple pedagogy. The experiments he performed were judged by him to be contributions to a true physics.

Nollet's views are much closer to Polinière's than Rohault's. However, while Polinière never went beyond performing and compiling experiments, Nollet attempted to create an entire physics based on them. This important fact was not lost on his contemporaries. Reviewing the first volumes of the Leçons de physique in 1744, the Journal des sçavans judged that they were "le premier Ouvrage où l'on trouve une Physique prouvée par une suite d'expériences qui se servent mutuellement de preuves."[89] De Mairan offered a similar judgement: ". . . cet ouvrage diffère-t-il de la plûpart de ceux de même espèce, en ce qu'il est moins un recueil d'expériences, qu'un assemblage méthodique de principes liez entr'eux, & prouvez par des faits."[90]

From Rohault to Nollet the approach to the use of experiments underwent different stages. To the former, experiments were means of refining or adjusting matters of detail in general theories developed by the mind. To Nollet, on the other hand, experiments became the bases and connecting links of physics. This shift in approach reflected and maybe helped occasion a transformation in the manner physics was practiced. To Nollet and others like him physics became a science of the laboratory, and instruments and materials the sine qua non of the practice of physics.

The impetus toward the standardization of physics developed concurrently with this transformation. If scientific knowledge was indeed to be--as Nollet believed it was--an edifice built gradually, and through a joint effort by successive generations of scientists, then the standardization of the field became a necessary element of that practice. Agreement among physicists, a consensus on views and principles, was essential to build the edifice. Even after the foundations of the edifice had been laid consensus would be required in adding new bricks to continue the construction. As we have already seen and as will become more evident in the next chapter, Nollet continuously emphasized the importance of consensus among physicists. To him, agreement among physicists often served as the equivalent of validity. In presenting his own views he took great care to show that they were built on notions which were shared by the community of scientists. In the joint effort to build a "consensus physics" the attractive thing about instruments was not their capacity for allowing quantification or higher precision--indeed, not until later in the century did quantification become an element of experimental physics; the attraction lay in the fact that they allowed for standardization and communication by making it possible for scientists to replicate the work of others.

Instruments and the _cabinet de physique_ also helped standardize the language of physics. Scientists communicated among themselves by referring to well-known or carefully described instruments and were expected to describe their work in a manner that allowed others to reproduce it in the laboratory. These considerations were reflected in the preoccupation with detailed lists and descriptions of instruments used, the appearance of manuals for the performing of experiments--such as Nollet's _Programme_ and the _Art des experiences_--and the concern with thorough description of experiments and material used. Nollet's role, as the _doyen_ of experimental physics and the author of textbooks and manuals, and as lecturer and instrument maker, was of primary importance in this development.

The emphasis on laboratory physics also meant a change in the subject-matter of physics. From the study of the world and nature in a wider sense, it became the study of the facts of the *cabinet de physique*. In other words, experimental physics addressed itself in its language and immediate concerns to the "facts" of the laboratory. The subject of the next chapter, and Nollet's major field of study, electricity, with its phials, revolving globes, pneumatic machines, and apparatus of all kinds, was the experimental science *par excellence*. As the century advanced, and maybe even to the distress of Nollet, the study of electricity became even more confined to the laboratory. Nollet's main rival in that field, Benjamin Franklin, constructed his electrical theory with basically one single laboratory experiment in mind, the Leiden experiment. Franklin was accused by Nollet, as well as others, of disregarding the more traditional and age-old problems of electricity.

It is in light of this process of the transformation of the subject-matter of physics, the transformation of the sphere of its study, and its installation in the laboratory, that Nollet's care to present only the most guarded and least controversial ideas should be evaluated. His search for a "consensus physics," confined only to those results he could produce in the laboratory and establish upon non-controversial facts, resulted in the seemingly non-innovative character of his work. Antoine Libes, reading Nollet's books decades after their composition and a half-century after the institution of the first chair of experimental physics, was probably unable to appreciate these transformations. To Libes, Nollet's guarded and copious study of apparently pedantic experimentation was too simple and uninspiring to be considered good physics.

In retrospect, and in defense of Nollet, one could argue that before laboratory physics could yield novel and interesting results it first had to become comfortably and solidly installed in its new settings. There first was needed an agreement among physicists that physics should operate from the laboratory; that is, that experiments were not only interesting and illuminating addenda to physics, but its very basis. Nollet was one of the men responsible for carrying out this transformation and helping make this process possible.

## CHAPTER III

## NOTES

[1]Antoine Libes, _Histoire philosophique des progrès de la physique_ (4 vols.; Paris: Chez Mme. Ve. Courcier, 1810-1813), III, 160; hereafter Libes, _Histoire_. "If the merit of a man should be judged by the reputation which he enjoyed during his life time and by the number of editions of his works that he published, no one would have more rights than Nollet to the recognition, maybe even the admiration of posterity. This manner of judging is not exact. Reputations are often the fruit of charlatanism and intrigue; . . ."

[2]Ibid., III, 161-165.

[3]Antoine Libes, _Traité élémentaire de physique, presenté dans un ordre nouveau, d'après les découvertes modernes_ (3 vols.; Paris: Chez Deterville, 1801), I, viii-ix; hereafter Libes, _Traité_.

[4]Ibid. ". . . if its esteemed author had known to avoid the danger of enthusiasm so dangerous and so common at the epoch of new discoveries; had he known not to disdain the help of geometry, to give to his lessons a manlier and quicker step, to interrogate nature with greater caution, or at least never to interpret its language when its answers, wrested out by an indiscreet importunity, were equivocal or obscure."

[5]Ibid., p. ix.

[6]Ibid., p. ixn.

[7]Jean Baptiste Joseph Delambre, _Rapport historique sur les progrès des sciences mathématiques depuis 1789, et sur leur état actuel. Présenté à Sa Majesté l'Empereur et Roi, en son conseil d'état, le 6 février 1808, par la classe des sciences physiques et mathématiques de l'Institut, conformément à l'arrêté du gouvernement du 13 ventôse an X_ (Paris: Imprimerie Imperiale, 1810), pp. 213-214; hereafter Delambre, _Rapport_.

[8]Antoine-Laurent Lavoisier, "Notice relative à l'Académie des Sciences," _Oeuvres de Lavoisier_, (6 vols.; Paris: Imprimerie Impériale, 1862-93), IV, 559.

[9]Libes, _Traité_, I, v-vi.

[10]Richard W. Burkhardt, Jr., the Spirit of System: Lamarck and Evolutionary Biology (Cambridge: Harvard University press, 1977), p. 65.

[11]Cohen, Franklin and Newton, p. 13.

[12]Eizo Yamazaki, "L'Abbé Nollet et Benjamin Franklin. Une phase finale de la physique cartésienne: la théorie de la conservation de l'électricité et de l'expérience de Leyde," Japanese Studies in the History of Science, 15 (1976): 37-64, at p. 44; hereafter Yamazaki, "Nollet et Franklin."

[13]On the popularity and social dimensions of science see Peter Gay, The Enlightenment: An Interpretation, Vol. II: The Science of Freedom (New York: Alfred A. Knopf, 1969).

[14]Roger Hahn, The Anatomy of a Scientific Institution: The Paris Academy of Sciences 1666-1803 (Berkeley, Los Angeles, London: University of California Press, 1971), pp. 35-57; hereafter Hahn, Paris Academy; Charles Coulston Gillispie, "The Natural History of Industry," Isis, 48 (1957): 398-407; idem, Science and Polity in France at the End of the Old Regime (Princeton: Princeton University Press, 1980).

[15]Hahn, Paris Academy, p. 39.

[16]There is only scanty treatment of this development in modern histories. See esp. Salomon-Bayet, L'institution, pp. 367-398; also Heilbron, Electricity, pp. 9-19, and 73-83; Maurice Daumas, Les instruments scientifiques aux XVIIe et XVIIIe siècles (Paris: Presses Universitaires de France, 1953); hereafter Daumas, Instruments (this work is mostly confined to the history of specific instruments and ateliers); and also Thomas Kuhn, "Mathematical versus Experimental Traditions in the Development of Physical Science," in Thomas S. Kuhn, The Essential Tension (Chicago & London: The University of Chicago Press, 1977), pp. 31-65; at pp. 43-39.

[17]Paolo Rossi, Philosophy, Technology, and the Arts in the Early Modern Era, trans. by Salvator Attanasio, ed. by Benjamin Nelson (New York: Harper & Row, 1970); hereafter Rossi, Philosophy. Idem, Francis Bacon: From Magic to Science, trans. by Sacha Rabinovitch (Chicago: The University of Chicago Press, 1968), pp. 1-35. W. E. Knowles Middleton, The Experimenters. A Study of the Accademia del Cimento (Baltimore & London: The Johns Hopkins Press, 1971). Martha Ornstein, The Rôle of Scientific Societies in the Seventeenth Century (3rd ed.; Chicago: The University of Chicago Press, 1938). See also Maurice Crosland, ed., The Emergence of Science in Western Europe (New York: Science History Publication, 1976), esp. Marie Boas Hall, "Science in the Early Royal Society," pp. 57-77.

[18]Heilbron, Electricity, pp. 9-10.

[19]Ibid.; Hankins, D'Alembert, p. 102 and passim.

[20]See Daumas, "Precision of Measurement and Physical and Chemical Research in the Eighteenth Century," in A. C. Crombie, ed., Scientific Change (London: Heinemann, 1963), pp. 418-430.

[21]Marie Boas Hall wrote: "There was another advantage to the experimental method, of peculiar importance for the organisation of the scientist as distinct from the organisation of science: it permitted co-operative endeavour, and it permitted various kinds of minds to contribute equally to the progress of science." Marie Boas (Hall), The Scientific Renaissance 1450-1630 (New York: Harper Torchbooks, 1962), p. 253. My argument differs from Hall's in a matter of emphasis. It was, I argue, more the pursuit of cooperation and the need for standardization that resulted in the emphasis on experimentation.

[22]Rossi, Philosophy, p. 64.

[23]Ibid., p. 63.

[24]Ibid., p. 64.

[25]Nollet, "Discours," p. 1.

[26]See Chapter II, note 58.

[27]Nollet, Leçons, I, vi.

[28]Ibid., p. ix.

[29]J. B. Bury, The Idea of Progress. An Inquiry into its Origin and Growth (London: Macmillan and Co., 1920), pp. 50-53.

[30]Ibid., pp. 65-66.

[31]Ibid., p. 128.

[32]Rossi, Philosophy, pp. 63-99; Edgar Zilsel, "The Genesis of the Concept of Scientific Progress," Journal of the History of Ideas, 6 (1945): 325-349.

[33]Pascal, "De l'autorité en matiere de philosophie," in Pensées, Oeuvres de Blaise Pascal (5 vols.; Hague: Chez Detune, 1779), II, 1-12; hereafter, Pascal, Pensées. See also Charles Frankel, The Faith of Reason. The Idea of Progress in the French Enlightenment (New York: King's Crown Press, 1948), pp. 13-38; and R. V. Sampson, Progress in the Age of Reason. The Seventeenth Century to the Present Day (Cambridge: Harvard University Press, 1956), pp. 13-38.

[34]Pascal, Pensées, pp. 8-9. "It is not so of man, who is produced for eternity. He is in ignorance in the first age of life; but he instructs himself endlessly in his progress: for he draws advantage, not only from his own experience, but also from his predecessors; for he always keeps in his memory that knowledge which he has acquired at one

time, & that of the ancients is always there available to him in the books which they have left. And as he preserves this knowledge, he can in this manner augment it easily; so that men today are, in a way, in the same position the ancient philosophers would have been, had they been able to go on living to the present, by adding to the knowledge that they had, the learning that their studies would have allowed them to acquire through so many centuries."

[35] Fontenelle used it, as I mention below, and so did Turgot much later; see Turgot on Progress, Sociology and Economics, translated, edited and with an introduction by Ronald L. Meek (Cambridge: Cambridge University Press, 1973), p. 41.

[36] Pascal, Pensées, pp. 7-10.

[37] Ibid., p. 9. "So that the whole series of men, during the course of so many centuries, must be considered as a single man who subsists forever, & who learns continuously: in this manner we see with how much injustice we respect antiquity in its philosophers; for as old age is that age furthest removed form infancy, who does not see that the old age of this universal man must not be searched for in the period close to his birth, but in that which is furthest from it?"

[38] H. Rigault, Histoire de la querelle des anciens et des modernes, in Oeuvres complètes de H. Rigault, Vol. I (Paris: Librairie de L. Hachette et Cie., 1859); hereafter Rigault, Histoire. Richard Foster Jones, Ancients and Moderns. A study of the Rise of the Scientific Movement in Seventeenth-Century England (reprint of 2nd edition of 1961; Berkeley and Los Angeles: University of California Press, 1965).

[39] Rigault, Histoire, p. 491.

[40] Ibid., p. xxx. "At the bottom of the debate there was a philosophical idea, one of the greatest that could be proposed to the human mind, because it concerns the dignity of its nature, the idea of the intellectual progress of humanity."

[41] Fontenelle, "Digression sur les anciens et les modernes," Oeuvres de Fontenelle (5 vols.; Paris: Salmon, 1825), IV, 235-254.

[42] Ibid., p. 242. ". . . physics, medicine, mathematics, are composed of an infinite number of views, and depend on the judiciousness of reasoning, which perfects itself extremely slowly, and perfects itself continuously. They must often be aided by experiences that chance alone gives rise to, and which it does not bring at a prescribed time. It is evident that all this has no end, and that the latest physicists and mathematicians should naturally be the ablest."

[43] Ibid., p. 249.

[44] Rigault, Histoire, p. 494.

[45] Bury, The Idea of Progress, p. 144. See also R. N. Stromberg, "History in the Eighteenth Century," Journal of the History of Ideas, 12 (1951): 295-304; hereafter Stromberg, "History."

[46] Stromberg, "History," passim. Pollard also discussed this "basic ambiguity of the Enlightenment view of history;" Sidney Pollard, The Idea of Progress. History and Society (New York: Basic Books, 1968), esp. pp. 18-95. For a more critical appraisal see Peter Gay, The Party of Humanity. Essays in the French Enlightenment (New York: Alfred A. Knopf, 1964), esp. pp. 270-274. Gay attacked the "myth" (p. 271) that acceptance of a theory of progress was a predominating idea of the Enlightenment. Gay, however, defines the notion too rigidly. Ehrard, on the other hand, emphasized that the dominant notion in the first half of the eighteenth century was that of Nature and not progress. Jean Ehrard, L'idée de nature en France dans la première moitié du XVIIIe siècle (Geneva & Paris: Slatkine, 1981), p. 741.

[47] Nollet, "Discours," p. li.

[48] Ibid., p. l. This reverence for Descartes, explained Mousnier, was due to the fact that Cartesianism was often confused with mechanism. Roland Mousnier, Progrès scientifique et technique au XVIIIe siècle (Paris: Librairie Plon, 1958), p. 46.

[49] Nollet, "Discours," p. li. ". . . occupied, either by taste, or by state, with unveiling & contemplating the marvels of Nature."

[50] Pluche, Spectacle, IV, 281-540.

[51] Ibid., pp. 532-540.

[52] [Goguet, Antoine Yves], De l'origine des loix, des arts, et des sciences; et de leurs progrès chez les anciens peuples (3 vols.; Paris: Chez Desaint & Saillant, 1758). The work appeared anonymously; later editions carry Goguet's name. See Bertha Bessmertny, "Les principaux ouvrages sur l'histoire des sciences parus en France pendant le XVIII[e] siècle," Archeion, 16 (1934): 325-328.

[53] Jean Etienne Montucla, Histoire des mathématiques, dans laquelle on rend compte de leurs progrès depuis leur origine jusqu'à nos jours (2 vols.; Paris: C. A. Jombert, 1758); and idem, Histoire des recherches sur la quadrature du cercle (Paris: C. A. Jombert, 1754); Jean Sylvain Bailly, Histoire de l'astronomie moderne depuis la fondation de l'école d'Alexadrie, jusqu'à l'époque de MDCCXXX (3 vols.; Paris: Chez les Freres De Bure, 1779-82). Histories of electricity will be discussed in the next chapter.

[54] Nollet, "Discours," p. xlviii. "The first, & the one by which one must begin, is to well acquaint oneself of certain truths that are received as principles, & to learn all the discoveries that were made before us. The second, is to work to increase this first basis of knowledge through one's own researches or by profiting from those of one's contemporaries."

[55]Ibid., p. lxvii.

[56]Ibid.

[57]Ibid. "An observer, in whatever part of physics, must have a patience against all odds, a capacity for scrutiny which allows no circumstance to escape, a live and quick acuteness, a wise and balanced imagination. . . ."

[58]Ibid., pp. lxx-lxxi. ". . . the time, the place, the present state of the Atmosphere, the quantity, the duration, the form, the color, the odor, and the other sensible qualities."

[59]Ibid., p. lxxxii. ". . . the more one introduces preparations & means, the more one would have to fear being misled on the true cause of effects . . . If one employs a large quantity of materials, when a lesser one suffices; if one undertakes the expenses of costly equipment, of very fine machines, before having undertaken trials that guarantee their utility, one is rushing oneself into superfluous expenses, & thus often placing oneself beyond the capability of conducting others that might be necessary, or one may altogether lose interest."

[60][André François Boureau Deslandes], "Discours sur la meilleure manière de faire les expériences," in Cours de physique expérimentale et mathématique par Pierre Van Mussenbroek, traduit par M. Sigaud de la Fond (3 vols.; Paris: Chez Bauche, 1769), I, xvii-xlvi, at pp. xxi-xxix.

[61]Nollet, Programme, pp. 123-190.

[62]Nollet, Art des expériences, I, iv, x-xii.

[63]Nollet, "Discours," pp. lxxxv-lxxxvi. "The life and faculties of one man will not suffice to repeat generally all the experiments that come to his knowledge: one is often forced to rely on the faith of others: but so as not to give one's confidence haphazardly & too lightly, one must regulate it according to the merit of the authors, & the care that they have taken to state the reasons of that which they propose for us to believe. . . . Every physicist who wants to inform others of his discoveries, must therefore expose in detail in what manner he conducted his experiments, under what circumstances he did them, & all the effects he noticed, with their number, their size, their differences, &c. & not suppress anything except that which is visibly useless & capable of producing a fastidious prolixity."

[64]Ibid., p. lxxxvi. ". . . it is important not to allow in one's work any negligence, any vicious manipulation, that might render it suspect."

[65]Ibid., p. lxxxvii. "The expense required for the acquisition of necessary instruments, & the difficulty of having them built in places where there is a lack of capable craftsmen, is no doubt one of the largest obstacles to overcome in experimental physics."

[66] On Descartes' views on experiment see Ralph M. Blake, "The Role of Experience in Descartes' Theory of Method," in Ralph M. Blake, Curt J. Ducasse, and Edward H. Madden, Theories of Scientific Method: The Renaissance through the Nineteenth Century (Seattle: University of Washington Press, 1960), pp. 75-103.

[67] Brunet, Les physiciens hollandais, pp. 51-74.

[68] Mouy, Le développement, pp. 108-132; esp. p. 113.

[69] Jacques Rohault, Traité de physique (2 vols.; nouvelle édition; Paris: Chez Guillaume Desprez & Jean Desessartz, 1723); hereafter Rohault, Traité.

[70] Ibid. See John A. Schuster, "Rohault, Jacques," Dictionary of Scientific Biography, Vol. XI (1975), pp. 506-509.

[71] Rohault, Traité, Vol. I, p. [9] of unpaginated preface. ". . . to want to absolutely reject reasoning and do nothing but experiments, is to fling oneself into an extreme much more prejudicial than the first."

[72] Ibid., Vol. I, p. [10] of unpaginated preface. "The first, properly speaking, is merely a simple use of the senses, as when accidentally & unintentionally, casting our eyes on things that are around us we are doing nothing other than looking at them. . . ."

[73] Ibid. ". . . when from a deliberate purpose, but without knowing or foreseeing what might happen, one tests something."

[74] Ibid.

[75] Ibid., Vol. I, p. [11] of unpaginated preface. ". . . which reasoning foresees, & that serve to justify afterwards whether it is right or wrong."

[76] Ibid.

[77] Letter to Mersenne, December 23, 1630, in Charles Adam and Paul Tannery, eds., Oeuvres de Descartes, Vol. I: Correspondance (Paris: Léopold Cerf, 1897), p. 196.

[78] Nollet, "Discours," p. lxxvii. "I say that one has views, & that one must have them when one undertakes new experiments; but these views must not allow more than mere suspicions, or, at most, suppositions, for which one must not feel any attachment, any predilection, so that one may always be ready to abandon them, if the facts do not concur to verify them or at least to making them very plausible."

[79] Torlais, "Physique expérimentale," pp. 620-621; also David W. Corson, "Polinière, Pierre," Dictionary of Scientific Biography, Vol. XI (1975), pp. 67-68.

[80] Pierre Polinière, Experiences de physique (Paris: Chez Jean de Laulne, Claude Jombert, & Jacque Quillau, 1709), p. vi; hereafter Polinière, Experiences.

[81] Corson, "Polinière, Pierre, Dictionary of Scientific Biography, XI, 68.

[82] Ibid., pp. 67-68.

[83] Polinière, Experiences, pp. vii-viii. "Indeed, if the reasonings one makes over the properties of bodies are not based on experiments, they cannot but be taken for uncertain conjectures, not to say for pure fancies. For there being an infinite number of possible things, it could always happen that one attributes effects to causes other than to those that produce them. To choose, then, with certainty among these possible causes those that truly produce the effects that are the subject of our meditations, we must base our judgements solely on the answers that Nature itself provides us in experiments, which are the only way we can possibly question her, & contemplate her as she actually is."

[84] Ibid., p. vi. "Often knowledge of one fact produces knowledge of another. One finds oneself sometimes being led as if by the hand, step by step to insights which the subtlest speculation & the most profound meditation would have never disclosed without the help of experiments."

[85] "Physices Elementa," Mémoires pour l'histoire des sciences & des beaux arts [Mémoires de Trévoux], 1721, IV [October], 1761-1796. This was a reprint of the same article that appeared in May.

[86] Ibid., p. 1766. ". . . this paraphernalia of experiments, of painful researches, of crucibles & alembics, where under the pretext that nature wants us to wrest its secrets from it, we torture it nonstop, altering it, disguising it so as to know her better."

[87] I had no access to the 1718 edition, but the "Reflexions" reappeared in 1728: Pierre Polinière, Experiences de physique (3rd ed.; Paris: Chez Charles Moette, Claude Prudhomme, & Guillaume Cavelier, 1728), pp. [7-10] among 12 pages of unnumbered prefatory material. At p. [7]: ". . . a heap of observations of different kinds that are useless, confused, amusing, without purpose & without consequence." ". . . material researched with discrimination, prepared and arranged methodically, & that can be considered as the bases of an exact physics."

[88] Ibid., p. [8] of unpaginated prefatory material. "The continuous changes that take place in the language, in the tastes of different centuries, in the morals, & even in the concerns of different times, render transitory, & often cause us to neglect, works that we once esteemed. But what is contained here, is not exposed to these inconstancies. The subjects which I here treat & the effects which I here point out are always the same; the truth that I here announce will be recog-

nized in all times & in all places, will be always new notwithstanding its age, & will always cause admiration in those who will begin to become acquainted with it."

[89] Journal des sçavans, 1744, p. 145.

[90] Histoire, 1743 (1746), p. 28. ". . . this work differs from most others of the same kind in that it is less a collection of experiments, than a methodical assemblage of principles that are interconnected, & proved by facts.

## CHAPTER IV

## NOLLET AND ELECTRICITY

In the first decades of the eighteenth century electricity was understood to be that property displayed by some bodies, after being rubbed, of attracting and repelling light objects nearby. Although other phenomena, such as sparks and heat, were also recognized to be associated with electricity, it was the phenomena of attraction and repulsion of light objects that most preoccupied students of electricity of the period.[1] Theories of electricity were primarily formulated to explain those phenomena. Most explanations resorted to postulating the motion of an effluvium triggered into motion by the rubbing of the body being electrified. The effluvium, identified as the ambient air, fire, or a special electrical matter, would cause the attraction and repulsion as it moved to and from the body.[2]

When the Abbé Nollet offered his "Conjectures sur les causes de l'éléctricité des corps" in 1745, the dominant electrical work in France, as in much of Europe, was that of Charles François de Cisternay Dufay.[3] Nollet adopted Dufay's electrical findings at least until 1738.[4] Between then and April of 1745 he developed his own views on the nature of electricity. Although Nollet published a number of books and memoirs on electricity after 1745, the theory he presented then remained basically unchanged.

Nollet's 1745 memoir rejected Dufay's vitreous and resinous electricities; it asserted that electrical matter was the same as that of Fire, and explained attraction and repulsion as the result of impulses by this matter flowing in and out of electrified bodies. "Effluence," or outward flow, was the cause of the apparent repulsion of light objects by electrified bodies, and "affluence," the inward flow, was the cause of the apparent attraction. According to his theory, electrification was actually the disturbance of Fire, a substance that pervaded all matter. This disturbance, brought about by rubbing or communication, caused small particles of Fire to flow out of the pores on the surface of the electrified bodies. These particles were replaced by other fire particles present in the surrounding atmosphere that rushed in through different pores. Thus, light bodies were "attracted" or "repulsed," depending on whether they were caught by an inward or outward flow.

The fact that bodies seemed first to be attracted and then, upon contact with the electrified object, repulsed was explained by the larger number of affluent streams. Surface pores that allowed an affluent, inward stream outnumbered those admitting an effluent stream. Hence, while the strength of the outward emanations was greater, small bodies had a greater chance to be caught by the more widespread affluent flow. However, as a small body approached the electrified object it, in turn, would have its fiery matter disturbed and become electrified. The atmosphere of fiery matter around it, created by its own emanations, meant that its "size" was increased, and it now became more likely to be caught by an effluent stream and be repulsed.[5]

Nollet's "affluence and effluence" theory was the dominant view of electricity in France at least until the introduction of the works of Benjamin Franklin (1706-1790) in the 1750's.[6]

My intention in what follows is to look into the theory from the perspectives presented in the preceding two chapters. I will argue that Nollet's theory is to be seen as a development of his experimental method. It was formulated to explain a vast array of experimental and observational data and it relied heavily on the senses. Also, it relied on Nollet's notion of a science built on non-controversial facts, a science of consensus. Indeed, the theory can be seen as a methodical arrangement of those ideas about electricity shared by a large number of students of the field. Nollet's "Conjectures" organized the ideas entertained by his contemporaries into a framework that seemed to make compelling sense.

Before proceeding I should point out that in a number of ways Nollet's work in electricity was not typical of his work in other fields. First, Nollet devoted more of his time to electricity than to any other subject. In addition to a number of works he wrote on electricity, the subject also commanded much of his attention in the form of public lectures and demonstrations. Second, it was on electricity that Nollet engaged in his most obvious theoretical work; it was only here that he offered what he was willing to refer to as a "system." Moreover, soon after offering this system he entered into a debate--one that sometimes turned sour--with proponents of differing electrical theories. These differences will not, I believe, affect the points I wish to make. On the contrary, they may even provide us with further insights into his scientific method.

Nollet's electrical work will be examined from three rather distinct perspectives. First, I will look into Nollet's apprenticeship with Dufay, and examine Dufay's work and note its influence on Nollet. Second, I will argue that Nollet's electrical theory relied heavily on experiment, on sense data and on the ideal of a professional consensus. Finally, I will suggest that Nollet's disagreement with Franklin's minus-plus electricity remained unresolved by their contemporaries for a number of years and that this was in part due to the fact that neither theory was clearly free of problems.

## Dufay's Electrical Work

When in 1734 at the end of his sixth memoir on electricity Dufay summarized his conclusions in sixteen points, it was clear that the definition of electricity had undergone an important change.[7] Most noteworthy among these conclusions was the assertion that all bodies could become electrified, either by rubbing or communication. Electricity was thus identified as a universal property or substance present in matter. Dufay also demonstrated that electrified bodies attract those that are not, and that there are two types of electricity: vitreous and resinous. Objects electrified resinously attract those electrified vitreously and *vice-versa*. Objects electrified with the same type of electricity repel one another. He also repeated experiments performed contemporaneously by Stephen Gray (1666/7-1736) that showed that electricity could be transmitted through long distances. Some materials facilitated that transmission while others hindered it. Dufay had no final suggestions on what the nature of electricity was. However, experiments he conducted *in vacuo* led him to dismiss the idea that electrical effects were due to the motions of the ambient air. Moreover, noticing that electricity could manifest itself in the absence of light and vice-versa, he rejected the possibility that they were effects produced by the same cause.[8]

Nollet collaborated with Dufay on some of the experiments that led to the memoirs of 1733 and 1734. Dufay acknowledged the Abbé's help in his third memoir. Referring to experiments he had conducted to determine which substances were more susceptible to electrical attraction and which substances best eased or hindered the passage of electricity Dufay wrote: ". . . M. l'Abbé Nollet . . . m'a infiniment aidé dans toutes ces expériences, & . . . en a imaginé plusieurs de celles qui se trouvent dans ce Mémoire."[9]

Nollet's close association with Dufay extended for two years, from 1731 to the fall of 1733; precisely those years during which Dufay was engaged in his researches on electricity.[10] Although the nature and extent, if any, of their collaboration on the other memoirs is not known, Fouchy made it clear in his éloge of Nollet that Dufay had secured the Abbé's assistance to aid him in his electrical researches. Fouchy also intimated that this collaboration with Dufay was one of the first opportunities Nollet had to be involved in the practice of physics proper.[11] Nollet and Dufay maintained a close relationship in the following years. In 1734 Dufay invited Nollet to accompany him on a trip to England and two years later he again invited him on a journey to Holland.[12]

The extent to which Dufay's approach to experimental physics affected Nollet is a matter for conjecture. However, similarities in their approaches are quite evident. Dufay's work was characterized by the concern and emphasis given to experimentation. "Dans ce que nous avons de lui, c'est la Phisique Expérimentale qui domine," wrote Fontenelle in his éloge of Dufay. "On voit dans ses opérations toutes les attentions délicates, toutes les ingénieuses adresses, toute la patience opiniâtre dont on a besoin pour découvrir la Nature. . . ."[13] Electricity was but one of Dufay's interests. Beginning in 1723 there are periodic reports of Dufay's contributions to the Académie on a variety of subjects.[14] He was the only one of whom it could be said, Fontenelle wrote, that he presented the Académie with contributions "dans tous les six genres des Mémoires que l'Académie a jugé dignes d'être présentés au Public. . . ."[15] Noteworthy throughout these contributions was Dufay's use of experiments, instruments and varied apparatus. It was Nollet's mechanical dexterity that seems to have led to his association with Dufay.

Apparent in Dufay's electrical work is that assumption of scientific progress which I have also identified with the work of Nollet. This is clearly manifested in Dufay's first memoir on electricity, which was dedicated to a history of the subject.[16] The memoir was a chronological exposition of those experiments and observations Dufay regarded as stages in the development toward the present state of knowledge. He made it clear from the outset that his history was not to be a recitation of all that had been said about the subject, but would be confined to the works of those men who had approached electricity "avec le plus d'intelligence, ou qui y ont fait quelque découverte considérable. . . ."[17] Were he to mention all those who had treated of electricity, Dufay told the Académie, he would have to write about all the authors who had written on physics; "il y en a peu qui ne se soient arrêtés à ce phénomene, & qui n'ayent tâché d'en trouver l'explication chacun dans son systeme."[18]

This very early history of electricity consisted of an exposition of works by William Gilbert, Otto von Guericke (1602-1686), Robert

Boyle (1627-1691), Hauksbee, Gray, and experiments performed by members of the Accademia del Cimento. Dufay completely neglected explanations offered by these or other men for the cause of electrical phenomena. He was also selective in his choice of their experiments and observations, neglecting to mention those he apparently did not believe significant.[19] He expressed surprise at the fact that the experiments of Guericke had escaped the attention of other electricians, failing to recognize, or at least to mention, that Guericke's experiments were not meant by their author to be electrical at all.[20] Dufay, in fact, chose to recount those points that were to be of importance to his own subsequent researches and was thus only interested in those works that he believed represented the "progrès qui ont été faits jusqu'à présent. . . ."[21] The history stressed the ever-increasing number of substances found to be amenable to electrification, the ability of electricity to act _in vacuo_, circumstances under which electricity was noticeably stronger--e.g. dry or cold weather--and the production and communicability of electricity. In concluding he once more repeated what his intention had been:

> Je ne répéterai pas que mon dessein n'a point été de parler de tous ceux qui en ont traité [de l'électricité], on voit assés que mon objet a été de ne faire mention que de ceux qui y ont fait quelque découverte singuliére, & qui ont contribué à porter les connoissances que nous en avons au point où elles sont aujourd'hui; . . .[22]

Dufay's short history set the stage for the remaining five memoirs in which he undertook to provide answers to six questions he believed encompassed "tout ce qui concerne l'électricité."[23] At the end of the sixth memoir he presented sixteen basic principles "ou, si l'on veut, les faits simples & primitifs auxquels se peuvent réduire toutes les expériences sur l'Electricité, qui sont connuës." He was sure the number of principles would diminish in the future "à mesure que l'on parviendra à une connoissance plus exacte de cette merveilleuse propriété de la matiére. . . ."[24]

This association with Dufay was Nollet's introduction to electricity and very likely to the practice of physics. It was a highly experimental, meticulous, approach to the study of a field Dufay believed to be progressing through time. Dufay brought together, clarified, tested, developed and ordered electrical knowledge and defined the character of subsequent studies of the field. In the words of one modern historian, Dufay "found the subject a record of often capricious, disconnected phenomena, the domain of polymaths, textbook writers, and professional lecturers, and left it a body of knowledge that invited and rewarded prolonged scrutiny from serious physicists."[25]

Nollet accepted Dufay's conclusions, at least in their general terms, in the Programme of 1738. These conclusions did not, however, include an explanation of the mechanism and cause of electrical phenomena.[26] It was not clear whether Dufay regarded electricity as a property of matter, or something separate but inherent in matter. At times he wrote as if he adopted the former view, at times the latter.[27] It was also unclear whether he regarded the nature of the vitreous electricity to be distinctly different from that of the resinous. These two electricities were constantly distinguished by Dufay who emphasized that they were "réellement distinctes, & très-differéntes l'une de l'autre."[28] The nature of the distinction, however, was not made clear; he sometimes described the two electricities as "deux genres d'électricité différents," and at others as "deux différentes natures d'électricité."[29]

Nonetheless, there were some general indications of what Dufay considered electricity to be. First of all, his conclusion that all bodies were susceptible of becoming electrified implied that whatever the electrical matter may have been, it was inherently associated with matter or present in all matter. There was also the suggestion that vitreous and resinous electricites were caused by effluvia of different kinds. While a body could become electrified either vitreously or resinously by communication, when it was rubbed its electricity was already determined by its composition. Glass as well as some other substances were invariably vitreously electrified, while resin, wax cakes and other specified substances invariably became electrified resinously.[30] All of these issues, and the many questions they raised, attracted the attention of students of electricity in the years following Dufay's memoirs.[31]

With the death of Dufay in 1739, his apprentice Nollet gradually became recognized as France's leading student of electricity. By 1743 the Académie recognized the subject as one of particular interest to the Abbé and turned over to him reports on electrical research it received from other scientists throughout Europe.[32] The Abbé remained thus well abreast of researches conducted elsewhere and his theory reflected substantial evidence of influence by other electricians.

## The "Conjectures"

Nollet's "Conjectures" was a methodical presentation of three different arguments on the nature and mechanism of electricity, all of which were already entertained by other electricians. The first was that electricity was caused by the motion of a material effluvium; the second was that the effluvium was Fire--that element Boerhaave had so convincingly argued permeates all bodies;[33] and the third was that this effluvium moved in and out of electrified bodies in converging and diverging streams. Nollet believed that experiments and observations had clearly established what he was about to present and, importantly, that these views were in their general terms accepted by a large number of other students of electricity. He organized these ideas into a systematic presentation that could, he believed, deal with the known phenomena associated with electricity.

Nollet first read his "Conjectures" to the Académie on April 28, 1745.[34] He introduced the theory cautiously, wanting to impress on his audience his awareness of the boldness of his undertaking. The more able physicists, he wrote, have refrained from offering an explanation of the cause of electricity "par la crainte de prononcer avec précipitation sur un sujet aussi obscur. . . ."[35] Of all people, he told the Académie, "Il me convenoit sans doute plus qu'à personne d'imiter cette sage retenue. . . ."[36] Careful to present his views only after he had examined them attentively, fearing the "reproche d'avoir osé les hasarder," Nollet put these ideas which he had conceived "depuis longtems" to the test of experience.[37]

> . . . attentif sur les faits, travaillant à les multiplier & méditant avec soin sur toutes leurs circomstances, j'attends depuis plus de dix ans qu'ils me conduisent eux-mêmes au principe d'où ils partent; je crois l'entrevoir enfin ce principe, & depuis plusieurs années je m'occupe à le concilier avec l'expérience.[38]

For the previous ten years--in other words since soon after his work with Dufay--Nollet had been trying to uncover the cause of electrical phenomena. In the last few years, after having come to grips with the underlying causal principle, he had been trying to "reconcile" it with experience. That is to say, Nollet had in his own view, appealed to experiment and observation to both originate and validate his ideas about electricity. Facts, according to this method, are expected to lead to an explanatory principle and then, once the principle is grasped, they can correct it and validate it.

New developments in the study of electricity in Germany, he told the Académie, had thrown further light upon his undertaking.[39] The developments he was referring to were successful attempts at igniting liquids through the use of electrical sparks.[40] Nollet interpreted these results as further proof of the similarity of Fire to the electrical effluvium.[41] These new developments, he wrote, strengthened his belief in the correctness of his views and encouraged him to present them.[42] While admitting that what he had to present was a system, he pointed out that in this case it was one based on fact; ". . . l'imagination en le formant n'a fait que mettre en oeuvre ce que l'expérience lui a fourni, & j'ose dire qu'on lui feroit tort en le prenant pour un assemblage de simples possibilités, ou de spéculations dénuées de preuves."[43] This was a system of the type Nollet believed possible and commendable. He would later emphasize that there was nothing "conjectural" about his theory, at one point even lamenting the use of the word "Conjectures" in the title.[44]

Nollet's first argument was to show that electricity was caused by the flow of a material substance. He began by distinguishing between electrical and magnetic phenomena. The distinction between magnetism and electricity was one commonly accepted by eighteenth-century physicists on the grounds that magnets act continuously and without preparation; that their action is limited to iron or matter containing that metal, and that they show none of the other characteristics identified with electricity such as luminescence or sparks.[45] After enumerating these distinctions he concluded that electricity and magnetism probably had nothing in common other than "l'obscurité de leur principe."[46]

Nollet next considered whether electricity was due to an attractive virtue inherent in matter, and he rapidly rejected that suggestion. Most physicists, he pointed out, did not entertain that view.

> On ne peut pas dire non plus que les effets de l'électricité viennent d'une attraction générale & commune à toutes les parties de la matière; outre que ce principe n'est adopté que par une partie du monde Physicien, qui n'est pas même la plus grande, ceux qui le soûtiennent avec le plus de chaleur sont obligez de convenir qu'on ne peut appliquer avec quelque vrai-semblance les attractions aux phénomènes dont il s'agit, sans faire une violence manifeste aux loix qu'on leur attribue, & selon lesquelles on suppose qu'elles agissent dans le méchanisme ordinaire de la Nature.[47]

Nollet was right in stating that not even the most avid Newtonians entertained attraction between particles of matter as an explanation for electrical phenomena. In France even so zealous a defender of attraction-at-a-distance as Voltaire believed electricity to be due to the motion of an effluvium. Voltaire asserted that electrical attraction "n'a rien de commun avec les lois découvertes par Newton."[48] Writing to Dortous de Mairan, Voltaire conceded that from all appearances electricity and magnetism act by an "écoulement de matière." Their effects were, indeed, within the "royaume de l'impulsion, mais l'empire de l'attraction," he told de Mairan, "non est hinc."[49] The distinction between the two types of attraction continued to be made well after the introduction of Benjamin Franklin's electrical works by such defenders of the Newtonian attraction as Buffon and d'Alembert.[50]

Musschenbroek, a strong believer in the existence of an attractive virtue inherent in matter, stressed that a distinction should be made between attractive and electric virtues. The two virtues, he believed, do not act in the same manner and their causes do not resemble each other "étant fort différentes les unes des autres." The electric virtue depends on "certains exhalaisons fort deliées, qui s'échappent des Corps. . . ."[51]

> Ces exhalaisons s'échappent des Corps, que l'on frotte, & y reviennent ensuite par des mouvemens tout-à-fait surprenans, comme on peut en juger par les Corps qu'elles mettent en mouvement. Elles meuvent & emportent avec elles tous les autres Corps légers qui peuvent être agités, de quelque espece ou nature qu'ils puissent être, & les repoussent ensuite. On peut-être assuré par ces effets & d'autres encore, que ces exhalaisons sont des véritables Corps qui agissent, puisqu'on peut les sentir, lorsqu'elles viennent frapper la joue ou la main. On peut aussi s'appercevoir, qu'elles repandent de la clarté pendant la nuit; & nous les entendons pétiller, craqueter, & s'échapper. Puisque tant de Sens en sont frappés en même tems, on doit être convaincu, que ce sont de véritables Corps, & qu'elles sortent des Corps électriques.[52]

Nollet would use the same argument, of the tactile sensual reality of electricity developed in the above passage by Musschenbroek, as a proof of the existence of the electrical matter.[53] He enumerated five different ways in which this materiality displayed itself. It was obvious to the touch in the form of a spider cobweb effect noticeable to the approaching hand. It crackled; making a noise similar to one made by running one's fingers through the teeth of a comb. It smelled or occasioned a smell similar to that of garlic or phosphorous. It was also visible in the dark; sparks could be seen to spurt out of electrified objects. Later, when a question was raised by a critic on whether these sparks actually left or entered the electrified object, Nollet in true observational fashion examined the sparks with a magnifying glass and observed that they indeed left the electrified body.[54] A fifth argument in favor of the materiality of electricity was that it was capable of igniting vapors and liquids. These last two points, luminescence and the ability to ignite liquids, Nollet also used to argue that the matter of electricity was the same as that of Fire.

Now, Nollet concluded, "qu'est-ce qu'une substance que l'on touche, qui se fait entendre, qui a de l'odeur & que l'on voit?" All these characteristics, "n'annoncent-ils pas incontestablement une matière?" This indeed was the opinion of "tous ceux qui jusques ici se sont appliquez à rechercher les causes de l'électricité. . . ."[55]

Having thus established the materiality of the electrical effluvium, Nollet turned to the examination of the two other questions; what could this matter be, and through what mechanism did it operate?

> Pour être en état de répondre à la première de ces questions, je cherche dans la Nature quelque fluide subtil & connu d'ailleurs, ou du moins supposé & admis par le plus grand nombre des Physiciens, un fluide qui ait des caractères semblables à ceux de la matière qui fait l'électricité, qui soit capable de brûler & d'éclairer, qui fasse néanmoins quelquefois l'un sans l'autre, qui éclate avec bruit suivant certaines circonstances, qui soit palpable & odorant, sinon par lui-même, au moins par les substances auxquelles il s'associe; car si j'en puis connoître un qui ait coûtume de s'annoncer par de tels effets, ne pourrai-je pas légitiment lui attribuer ces mêmes effets par-tout où je les recontrerai?[56]

The answer was obvious. These characteristics were those of "du feu proprement did."[57] Nollet once more appealed to the consensus of physicists and their agreement on the nature of Fire. It was also the fluid most commonly associated with electricity by contemporary electricians.[58] Johann Heinrich Winkler (1703-1770) believed that no fluid could possibly ignite anything unless it contained particles of Fire. Since electricity was known to ignite vapors Winkler had concluded that "toute matiere électrique contient des particules de feu."[59] Musschenbroek, in the Essai de physique entertained the possibility that the écoulement électrique consisted of an effluvium of Fire matter, or of particles from the electrified body accompanied by particles of Fire.[60] Only by assuming that Fire was somehow involved, Winkler and Musschenbroek argued, could one explain all the display of light and inflammation that accompanied electrical phenomena. Georg Matthias Bose (1710-1761) whose emphasis on the inflammatory and luminary power of electricity had made the association more vivid to Nollet, also equated Fire with electricity.[61] Here, too, Nollet was developing a view already widely entertained by electricians.

Nollet's views of Fire are discussed at great length in volume four of the Leçons de physique expérimentale.[62] As he acknowledged, his views were to a large extent those developed by Boerhaave.[63] Fire to Nollet was a distinct fluid present in nature "dès le commencement, & qui n'a besoin que d'être excité pour agir."[64] Whether it be the aether of the Newtonians or the first or second element of the Cartesians was not a concern of Nollet, "le nom n'y fait rien."[65] It was however a primitive substance and not one created by motion or friction. This substance was most likely that of light also. As nature only produces beings with great economy, while producing effects with profusion, he was "très-porté à croire que c'est la même matiére qui brule & qui éclaire, qui nous fait sentir la chaleur & voir les objets." In other words, "le feu & la lumiére considérés dans leur principe, sont une seule & même substance différemment modifiée."[66] Nollet invoked this same principle of the economy of nature to identify Fire with electricity. This was done more clearly in the Essai sur l'électricité than in the "Conjectures" and much more emphatically in his Lettres sur l'électricité.[67] In the Essai he argued that the main reason we may assume that fire and light are but one and the same thing is the simultaneity of their effects.

> . . . c'est que le feu éclaire presque toujours, & qu'il y a bien des cas où la lumiere brûle: la Nature qui économise tant sur la production des Etres, tandis qu'elle multiplie si libéralement leurs propriétés, auroit-elle établi deux causes pour deux effets auxquels il paroît qu'une des deux peut suffire?

> Cette raison est assurément bien plausible, & l'on peut en faire aussi l'application à la matiere électrique. Ceux qui en ont examiné la nature, & qui en ont jugé par analogie, ont presque tous prononcé que le feu, la lumiere & l'Electricité partoient du même principe.[68]

The manner in which fire, light and electricity differed was in the way in which the particles of Fire acted. In electrical phenomena, the particles of Fire, compressed inside particles of matter, are agitated by friction or, in the case of metals, by communication, and spring out of the electrified body accompanied by some matter from the body itself.[69]

Nollet compared the most commonly identified properties of fire to those of electricity and concluded that all indications were that those two materials were in fact one and the same.[70] He ran through seven properties he believed common to both fire, or heat, and electricity. The first was that bodies become electrified in the same manner in which they are made hot; "en les frottant on fait l'un & l'autre."[71] The second common property was that bodies that are denser and more elastic tend to be more susceptible of being made hotter and of being electrified. A third property common to both pheonomena was the rapidity and ease with which metals could communicate both heat and electricity. Fourth, Nollet pointed to the ease with which fire, when unhindered by any obstacles, dissipated without much sensible trace. When its free motion is retarded by obstacles, it grows more and more in strength by the force that continues to animate it, and may burst out of the body which contains it in a manner "semblable à une bombe qui éclate, il s'arme, pour ainsi dire, des parties de la matière qu'il a divisée, il heurte avec violence les corps qui sont exposéz à son choc. . . ."[72] The same happens with electricity as can be seen in the manner it dissipates through the air if its motion is unhindered while it discharges in explosive sparks if its motion is hindered in whatever manner. This analogy is more meaningful if one keeps in mind the fact that Nollet regarded the cause of both of these effects to be the particles of Fire compressed in the body being heated or electrified. When free to leave the body, the particles do so gradually and without much noticeable effect. However, when they continue to be agitated but cannot leave, they are forced to "explode" out of the body.[73]

Nollet's fifth analogy was that both electricity and fire move more freely in dense than in rare bodies. In other words, denser bodies carry heat or electricity more promptly. The sixth common property was the rapidity with which electricity, fire and light are transmitted. Finally, both electricity and fire, Nollet pointed out, were stronger in cold weather when the air is dry and dense.[74]

To conclude this argument, Nollet pointed out once more that the identity of Fire and electricity was accepted by most of those who have studied the matter and was a view that even the Académie had considered probable.[75] It was also, he claimed, an opinion entertained by Dufay, and to emphasize this point Nollet quoted a long passage from the latter's sixth memoir where Dufay had in fact considered that "c'est un feu réel ou une matière très-propre à le devenir qui sort des corps électriques. . . ."[76] He concluded this argument for the identity of Fire and the electrical matter in the following manner, appealing once more to the consensus of physicists and the evidence of the senses.

> Telle est donc l'opinion de ceux qui avoient le plus réfléchi sur la nature de la matière électrique, & qui avoient été le plus à portée de l'étudier, dans un temps où l'électricité n'avoit encore produit tout au plus que quelques étincelles piquantes, dans un temps où l'on avoit tenté cent fois, mais toûjours inutilement, d'animer le feu électrique jusqu'au point d'enflammer les autres

corps: à combien plus forte raison pouvons-nous maintenant embrasser le même sentiment, quand nous voyons des corps électrisez allumer réellement toutes les liqueurs & toutes les vapeurs inflammables, & les consumer comme elles ont coûtume de l'être par le feu le plus commun![77]

In a footnote added to the "Conjectures" when it appeared in print in 1748, Nollet indicated that the identification of Fire with the electrical matter was not an essential aspect of the mechanism of affluence and effluence.[78] Rather, he saw these as two independent arguments. However, evoking once more the principle of economy, he opined that it was very unlikely that the electrical matter was different from Fire. Later on, during his polemics against Franklin, Nollet affirmed the identity of Fire and the electrical matter in the strongest of terms.[79] He was in part motivated to do so to combat Franklin's assertion that glass was impermeable to electricity. If the electrical matter was indeed Fire, then Franklin could hardly argue that glass was impermeable to it; for glass clearly was transparent to light and obviously transmitted heat.[80]

After establishing that electrical phenomena were caused by a material substance and that that substance was Fire, Nollet next proceeded to his third argument which embraced the mechanism of affluence and effluence. His proofs for the existence of the affluent and effluent streams of Fire were strongly visual.

. . . j'ai vû presqu'autant de fois que je l'ai voulu, que quand un corps électrisé s'approche d'un autre qui ne l'est pas, il émane en même temps de chacun d'eux un courant de matière qui se fait sentir de part & d'autre comme un souffle léger, tant que les deux corps sont à une certaine distance l'un de l'autre, & qui devient une aigrette lumineuse & permanente, quand le degré de proximité n'est point assez grand pour le faire éclater en étincelles. . . .

Ayant fortement électrisé un globe de verre, pendant que je le frottois encore, on en approcha à quelques lignes de distance des corps solides de toute espèce, & je fus agréablement surpris de voir

> sortir par différens endroits de ces corps, & sur-tout pas les parties les plus saillantes, des jets de feu non interrompus, plus ou moins denses . . .[81]

These facts "paroissent assez décisifs," Nollet continued, "puisqu'ils nous mettent sous les yeux deux courans de matière électrique qui vont en sens contraires. . . ."[82] Furthermore, he added, if one accepts the fact that a material substance is judged by the (sensual) effects it produces, then there are a variety of other proofs for the mechanism of effluent and affluent streams. Here is one example, from many similar ones Nollet gave:

> Electrisez par le moyen du globe une verge de fer mouillée d'esprit de vin, vous sentirez tout autour une pluie imperceptible, causée sans doute par de petites gouttes de la liqueur que la matière électrique emporte avec elle en sortant, comme nous voyons qu'elle chasse devant elle la poussière du bois, le tabac, le sable, &c. mais pendant tout le temps que dure cet effet, la même verge de fer n'en attire pas moins tous les corps légers qu'on lui présente par quelque endroit que ce soit.[83]

The discharges of the electrical matter that caused the sprinkling could be seen to leave the electrified bar in divergent rays.

One is almost led to believe that Nollet indeed "saw" the electric effluvium leave the body in divergent rays. He spoke of the visibility of the rays so often, and described them so vividly in numerous experiments, that one senses that the electric fluid became no less "visible" to him than the motion of the air on a windy day is "visible" to us today.

While the major proof Nollet provided for the existence of the effluent streams was their visibility, the proofs for the affluent streams were less direct.[84] In fact, the gist of his arguments for the existence of the affluent streams was that they had to be assumed to explain the phenomena associated with electricity. Thus the phenomena to be explained became somewhat confused with the evidence for the theory. The affluent streams, Nollet explained, accounted for the endless supply of electricity present in bodies. If Fire left the body being electrified, as one could see it did, then an equal amount had to flow in to replace the loss. The affluent streams also explained why some specks of dust, powder, and the like, sprayed on an electrified body remained stuck to it. They were being forced down by the inward stream. More importantly, if the effluent streams were responsible for repulsion, as the many experiments Nollet performed had shown to be the case, then the affluent streams were responsible for attraction.[85]

The idea that electricity acted through divergent and convergent rays of electrical matter was also not novel with Nollet. Winkler, at least as early as 1744, had also explained attraction and repulsion very much in the same way Nollet did in 1745.[86] Both he and Winkler also considered that electricity was caused by rays of the electrified matter accompanied by Fire. Bose had also been led to believe that repulsion was due to divergent rays of matter and Fire. However, he believed attraction to be caused by the reaction of the ambient air. He changed his mind after receiving a letter from Nollet early in 1745 in which the Abbé, impressed by the similarities of their views, argued the advantages of assuming the return of Fire particles through converging rays. Bose made Nollet's letter an addendum to his Recherches.[87]

The strength of Nollet's theory was not, then, its novelty, but the thorough and methodical presentation of its arguments. To Nollet, some of the strength of the theory was the lack of novelty--the fact that its major tenets were already accepted by electricians, something he continuously emphasized.

After presenting the theory, Nollet drew from his large repertoire of experiments and observations to show how it could explain the many phenomena associated with electricity. It could explain the luminescence associated with electricity that appeared on the coats of lambs and other animals. It could also explain the appearance of sparks, electric shocks, ignition, as well as attraction and repulsion. Nollet was also able to show why Dufay had been misled into postulating two types of electricities. Since denser bodies could produce stronger electricity, that produced by glass overwhelmed the electricity produced by wax. This circumstance explained why "vitreous" electricity could attract bodies electrified "resinously."[88]

A few months after Nollet read his memoir, news was received in France of an experiment conducted in Germany and repeated in Leiden that occasioned electrical shocks of a strength unheard of before. The news arrived in a letter from Musschenbroek to Réaumur who passed it on to Nollet.[89] Musschenbroek, repeating experiments performed earlier that year in Germany, had received an electrical shock which, he told Réaumur, he would not care to experience again for all the Kingdom of France.[90] The experiment consisted of communicating to a gun barrel, or a simple iron bar suspended by insulating silk threads, the electricity from a rubbed, revolving globe. On the other side of the barrel a brass wire hung loosely and dipped into the water of a half-filled glass flask. If the experimenter held the flask with one hand and approached the barrel with the other he would receive a shock that could knock him off his feet. Musschenbroek described the sensation to Réaumur: "tout d'un coup ma main droite . . . fut frappée avec tant de violence, que j'eus tout le corps ébranlé comme d'un coup de foudre." J. N. S. Allamand, who also performed the experiment himself, warned Nollet: "Vous ressentirez un coup prodigieux qui frappera tout votre bras, & même tout votre corps, c'est un coup de foudre."[91]

Nollet reported on the experiment to the Académie in 1746 after having repeated it himself a number of times.[92] The experiment, he told the Académie, added further proof to his theory. It reinforced his argument that denser matter, where particles of Fire were more compressed (in this case the body of the hapless experimenter), occasioned greater electricity. Thus, as the experimenter's hand approached the barrel, the direction of electrical flow was directed toward his body since there it found a greater concentration of Fire particles. He explained that the strong commotion was due to the flow of electricity entering the body from both sides. The electrical affluences collided inside the body of the experimenter and their impact occasioned the shock.[93]

Nollet recognized that the experiment revealed a new and unexpected fact. The experiment seemed to indicate that glass was able to both communicate electricity and remain electrified.[94] Had glass been totally permeable to electricity, as most electricians believed it to be, the glass flask--which had to be grounded for the experiment to succeed--would not have accumulated electricity; and if electrified should have lost all its electrification upon being touched by the experimenter. However, experiments Nollet and others performed showed that the glass flask remained electrified for hours, even if left sitting overnight on a table. It was, Nollet concluded, this peculiar property of glass that made for this peculiar experiment.[95]

The Leiden experiment--the name was given it by Nollet--aroused the interest of the public and of electricians.[96] The novelty was the strength of the shock the vial could occasion, and a number of different entertaining ways were devised to demonstrate it.[97] Among electricians it raised a sentiment that there was much about electricity that they did not yet understand.[98] However, there are no indications that Nollet's theory was much the worse for it. The Abbé himself seems to have been convinced that the experiment was further evidence in favor of his theory and reiterated that opinion on a number of occasions.[99] Electricians, among them the Chevalier Patrick d'Arcy (1725-1779), Jean Baptiste Le Roy (1720-1800), Etienne François Du Tour (1711-1789), and Musschenbroek in Holland, continued to look favorably on the Abbé's "Conjectures" well past 1746.[100]

Of the few critics of Nollet's theory to surface before the introduction of Franklin's works, only the surgeon Antoine Louis (1723-1793), to my knowledge, criticized the Abbé's explanation of the Leiden experiment.[101] Louis was critical, in fact, of every aspect of Nollet's theory. He criticized Nollet's attempts to use electricity for therapeutic reasons; he believed that electricity was more likely due to the motion of the ambient air; and that it was the water, not the phial, that became electrified in the Leiden experiment.[102] Nollet responded to these and other criticisms in the Recherches. The other critics were Jean Morin (1705-1764), Jean Baptiste Secondat (1716-1796) and Nicolo Bammacaro (d.1778). The first two wanted to revive the theory that electricity was due to the ambient air, while Bammacaro remained unconvinced that the Abbé had fully demonstrated the mechanism of affluences and the similarity of Fire to electrical matter.[103]

Except for Bammacaro, whom Nollet later met in Naples, none of the other critics were men of significant scientific standing or any renown in electricity.[104] Their dilettantism was reflected in their criticisms. Nollet, however, answered all of them in detail. Louis' arguments were dismissed when it was shown that the glass flask in the Leiden experiment could be electrified even when empty of water; and his explanations that the shock was caused by the "air qui se debande" made to appear as nothing but pure opinion.[105]

However, while there were few critics and much praise of Nollet's theory, there are no indications that the theory provided a new direction or approach for electrical studies. Heilbron appears to have overstated the case in his assessment that "the opposition [Nollet's] system initially encountered was so little serious that it underscored the consensus."[106] French electricians continued to produce treatises on electricity, and four English and German treatises appeared in translation in 1748.[107] While Nollet's system did meet with approval, there was not a "consensus" in the sense of general adoption of any particular theory of affluence and effluence by others in their research. Nonetheless, none of Nollet's critics, or any of the new treatises, specifically addressed the new puzzling experiment from Leiden or Nollet's ineptness in dealing with it. It was only later, in the debate with Franklin, and primarily over the disagreement over the electrical permeability of glass, that the Leiden experiment became a key point of argument.

Before concluding this chapter with a review of some of the issues of that debate, let us summarize what has been argued so far. I have tried to show that Dufay was an important influence on Nollet both in the field of electricity and in the practice of physics proper. I have also argued that Nollet's electrical theory was strongly based on empirical confirmation and experimentation which relied heavily on sensual--and primarily visual--observation. Nollet's approach to electricity displayed that concern with which I have characterized the rest of his physics: to organize and refine the views shared by others. He regarded agreement among physicists as a sign of the validity of a view, and was very careful to frame his views with this consideration in mind. Although he presented his theory of affluences and effluences with great caution and, at least in 1745, with many prefatory apologies for offering a "system", there was very little in the "Conjectures" that was not already entertained by other--if not most--electricians. It is an irony of history that Nollet's theory became later identified with the alleged Cartesian tendency to philosophize.

## Nollet and Franklin

It is also an irony that Nollet should have become best known in modern times because of his rivalry with Franklin. When Thomas François Dalibard (1703-1779) first translated Franklin's Experiments and Observations at Buffon's urgings, Nollet was the pre-eminent electrician in France and in much of Europe. In fact, Nollet suspected, and correctly, that Buffon's intentions were simply to embarass him and his mentor Réaumur.[108] Nollet was not even sure that Franklin existed and for a time believed that Buffon, intent on carrying on his diatribe against Réaumur, had invented this savant from the far-off British colonies.[109] It soon became clear to everyone that while Buffon and Dalibard may have been motivated by polemical motives, Benjamin Franklin was very much alive and well and living in Philadelphia, and that he had many significant and interesting things to say about electricity.

Dalibard translated Franklin's Experiments and Observations in 1752, after receiving a short course on electricity from Delor (c. 1717-?). He prefaced it with a short history of electricity in which Nollet's name was conspicuously absent.[110] Soon after publication of the Expériences et observations, Dalibard, Delor and Buffon gave public expositions of the entertaining experiments performed by Franklin. Among members of the public to view them was Louis XV, who appeared to have been well entertained and diverted by the three savants.[111] In 1752, Franklin's sentry-box experiment, to test the analogy between thunder and electricity, was attempted near Paris. It was a success. As Franklin had predicted, a metal pole pointed at the skies collected the electrical matter from the passing clouds. This was verified when electrical sparks were drawn from the base of the insulated metal pole by an approaching brass wire. The experiment was soon repeated in different places throughout Europe. The utilitarian aspect of the pointed metal pole, capable, if grounded, of diffusing "thunderlightnings," quickly became a subject of much conversation. And Franklin's name was associated with it all.[112] The success of Franklin's Experiments and Observations and the popularity of his experiments delighted Buffon. Nollet was less happy.[113] The Abbé Nollet, Buffon wrote a friend, "meurt de chagrin de tout cela."[114]

Nollet, as we have seen, was not a newcomer to polemics. In the Recherches and to a limited extent in the Essai, he had entered into a diatribe against some critics of his theory. But these were criticisms by relatively minor scientific figures, men without any following and certainly not of the scientific stature of Nollet. Franklin's Experiments and Observations was introduced into France with great fanfare, and with the backing of Buffon. It soon acquired some following among electricians, some of whom, like Louis Guillaume Le Monnier (1717-1799) and Le Roy were members of the Académie.[115]

Furthermore, the manner in which Franklin's works were introduced into France seemed to be, as they were perhaps intended, an insult to Nollet. The Dalibard translation appeared prefaced with an "Avertissement" and an abridged history of electricity, both of which totally ignored Nollet and his theory of affluence and effluence while mentioning many other lesser contemporary electricians. The "Avertissement" quoted at length from Buffon's preface to his translation of Hales' Vegetable Staticks where Buffon had exalted the virtue of experimentation--"C'est (dit M. de Buffon,) par des expériences fines, raisonnées, & suivies, que l'on force la nature à découvrir son secret; . . ." But it derided experimenting by the incapable--(meaning Nollet?)--"il ne suffit pas de s'attacher uniquement à la voye de l'expérience, à moins que d'être, comme notre auteur, foecond en moyens, ingénieux en découvertes, & heureux en applications."[116] Following the "Avertissement" was a history of electricity, which Nollet believed written by Buffon, but which was based in large part on one written by Secondat.[117] The son of Montesquieu (who himself did not think much of the Abbé),[118] Secondat was one of the four critics of Nollet's electrical theory mentioned above. The choice of his history (not a particularly good one), the laudatory reference to it and its author as well as the exclusion of Nollet's name, were not lost on the Abbé. The history had been entitled "abbregée" [sic], Nollet wrote Dutour, "apparamment pour être en droit de ne me pas nommer;

cette affection dont je ne me plains point, a été remarquée de tous ceux qui ont vu cet ouvrage.[119]

Nollet responded to the Franklinists with a series of memoirs read at the Académie and a number of open letters on electricity. Published in three volumes over a fifteen-year period, most of these public letters were addressed to Franklin. Nollet also used his influence at the Académie to discredit some of the Franklinists' achievements and, Heilbron argues, may have succeeded in discouraging some of them from pursuing their work.[120]

There was very little in Franklin's theory or experiments that Nollet liked. He found the conduct of his sympathizers condemnable; they were more interested in amusing themselves with entertaining experiments than in the physical theories.[121] Ignorant of electricity, they hastily claimed discoveries for Franklin that were already known years earlier. Nollet believed the theory contained nothing interesting that was not already known--and nothing new that was not wrong.[122] But his attack on Franklin's theory centered basically around two issues. He believed Franklin's explanation of the Leiden jar to be gratuitous, ill-founded and not supported by experiment, and he pointed out that the theory was incapable of explaining the most simple facts of attraction and repulsion.

Nollet recognized that Franklin's minus-plus electrical theory had been primarily formulated to explain the Leiden experiment. "Il paroît, Monsieur," he told the Philadelphian in one of his "open letters," "que dans vos expériences sur l'Electricité, ce que vous avez eu principalement en vûë a été d'examiner à fond ce Phénoméne surprenant. . . ."[123] To Nollet, however, that experiment was but one of many others and its exceptional character could not be the basis of an electrical theory. In the "Eclaircissemens" he read to the Académie before 1752 he had made that clear.

> . . . je n'ignorois pas que dans l'expérience de Leyde, le vase de verre qui contient l'eau s'électrise fortement & conserve long-temps son électricité, quoiqu'on le tienne à plêines mains: . . . mais . . . j'ai laissé subsister la loi générale, & j'ai exposé cette particularité comme une exception qu'on peut regarder comme unique. . .[124]

In basing his theory on that one experiment Franklin, according to Nollet, had made two gratuitous assumptions. The first was that the electrical fire was other than Fire, and the second that glass was totally impermeable to electricity. And the theory failed to explain adequately the major phenomena associated with electricity, the attraction and repulsion of light objects.

According to Franklin's explanation of the Leiden experiment, for every amount of electricity acquired by the inner surface of the glass flask an equal amount was lost, or repelled, by the outer surface. Thus, while the inner surface accumulated more than its original quantity of the electrical fire and became electrified "plus", the outer surface, by losing electrical fire, became electrified "minus." The glass flask had to be grounded, Franklin explained, to allow for the loss because the electrical fire could not pass from one surface of the glass to the other; glass, in other words, is impermeable to the electrical matter.[125]

It was primarily to make this very unlikely hypothesis plausible, Nollet argued, that Franklin had distinguished between Fire and the electrical fire. Nollet haughtily dismissed Franklin's one other argument in favor of the distinction, the observation of "cold fusion," the melting of metal by electrical fire without any trace of heat or combustion remaining. That argument, Nollet told Franklin somewhat patronizingly, physicists would not receive.[126] The main reason Franklin had to distinguish between electrical fire and Fire "c'est que celui-ci se fait jour au travers de tous les corps, sans aucune exception, & que l'autre," according to Franklin, "ne traverse jamais que la demie épaisseur du verre le plus mince."[127]

Nollet's approach in the following years to the challenge of the Franklinists was to attempt a series of experiments to nullify the claim of the impermeability of glass. Every experiment that Nollet performed, the Franklinists rebutted with their own explanations and counter-arguments.[128]

Joseph Priestley (1733-1804), a supporter of Franklin's theory, wrote in his History of Electricity, published in 1767, that Nollet continued to support his "darling" theory when evidence indicated he should discard it.[129] The one salutary effect of Nollet's opposition to Franklin, according to Priestley, was that it helped increase Franklin's reputation and the zeal of his friends. Nollet, Priestley estimated, "never had any considerable seconds in the controversy, and those he had," he continued, "have all deserted him."[130] The arguments in favor of his theory Priestley found to be "very unsatisfactory," and the method Nollet devised to account for attraction and repulsion "more ingenious than solid." Priestley believed it a "great pity that this truly excellent philosopher had not spent more time in diversifying facts, and less in refining upon theory." Part of the problem was "the natural fault of a disposition to philosophize."[131] This view of Nollet as the stubborn antagonist to Franklin has not totally disappeared in more modern histories. Nollet's electrical work is too often evaluated simply in contrast to the more successful work of Franklin and seen as another example of the Cartesian love for systems that refused to die an honorable death.

I. B. Cohen, as we have seen, believed Nollet's electrical theory to be a total waste that did not achieve "any useful product."[132] "This theory," he wrote, "did not coordinate the observed data particularly well; it led to no predictions of new phenomena nor to practical applications in important devices." Nollet's theory even failed to "challenge scientists to produce a better theory to explain the phenomena which it was designed to serve." The reason this theory, which "might just as well never have existed at all," did not stimulate anyone to attack it "was the existence of a much better theory produced independently at about the same time: Franklin's."[133] Cohen criticized Nollet for stubbornly clinging to his opinions and attributed this to his excessive Cartesianism, the desire to explain electricity and everything else mechanically.[134] Cohen, like Brunet, did not believe Nollet was sincere when he stated that he was neither a Cartesian nor a Newtonian. While Brunet was sure the Abbé was a Newtonian, Cohen was sure he was a Cartesian.[135]

Whittaker in his History of the Theories of Aether and Electricity paid little attention to Nollet and simply pointed out that his theory lost its support soon after the introduction of Franklin's theory. As evidence he quoted the passage from Franklin's Autobiography where the American electrician himself stated so.[136]

Not everyone has been so unkind to the Abbé. Daujat acknowledged that Nollet did indeed secure and maintain a number of followers among electricians of the period and named Dutour, Jean Jallabert (1712-1767), and Laurent Béraud (1702-1777) among them.[137] More recently, both Home and Heilbron have argued that Nollet's theory maintained its set of supporters well past the time when Franklin's theory became known in France. Heilbron argued that Nollet's theory, formulated essentially to deal with the phenomena of attraction and repulsion and luminescence, remained the favorite theory of the old school electricians, such as Musschenbroek, Bose, Gordon, Dutour, Paulian, de Romas and others.[138] Franklin's theory, born in the age of the Leiden phial, was addressed, as Nollet and others soon recognized, to deal specifically with experiments associated with it and gained the adherence of the younger electricians more interested in this phenomenon.[139]

Indeed, a major difficulty that beset Franklin's theory was its inability to deal with some of the phenomena of attraction and repulsion. If the atmospheres around electrified bodies were the cause of attraction and repulsion, how did bodies electrified negatively attract other bodies? Why did they repel each other? This difficulty, which bothered Franklin himself, was one Nollet repeatedly pointed to and one that continuously preoccupied Franklin's supporters.[140]

To Nollet, the problem lay in Franklin's lack of recognition that inward and outward flow of electrical matter from the electrified object occurred simultaneously. Unless this fact was accepted--the simultaneity of an affluent and effluent flow of electrical matter--Franklinists would continue to be unable to explain a wide variety of phenomena. The more obvious of these was the fact that attractions and repulsions often occurred together:

> . . . le point capital, celui sur lequel se réunissent les partisans de M. Franklin, si divisés d'ailleurs, c'est que dans toute Electricité ils ne veulent reconnoître qu'un seul courant de matiere; & quand j'ai cité les attractions & répulsions simultanées, le premier, le plus infallible, le plus connu de touts les phénomenes électriques, comme un indice palpable des effluences & des affluences simultanées, M. Leroy m'a répondu: Nous ne savons pas comment se font les attractions & répulsions électriques.[141]

According to Home it was, in part, the inability of Franklin's theory to adequately explain attraction and repulsion that allowed Nollet's theory to remain dominant in France until the Abbé's death in 1770.[142]

Although Home has succeeded in debunking the view propagated by Priestley, and indeed by Franklin,[143] that the Abbé had few or no followers in France other than Mathurin-Jacques Brisson (1723-1806), he has not succeeded in making the stronger case, that the Abbé's theory predominated. In fact, Brisson's own testimony contradicts Home's opinion. Commenting on Priestley's assessment that Nollet had few followers, Brisson noted: "Ce n'est pas le nombre des partisans d'une opinion qui en détermine la valeur. La verité n'est pas toujours du côté du grand nombre."[144]

Brisson did not deny that Nollet had few followers, only that the worth of his theory could be judged by the number of its followers. One may wonder whether Nollet would have been happy with his loyal disciple's defense. For Nollet believed, indeed, that much of the worth of a theory was in the consensus that it rallied around it. Often, as we have seen above, Nollet blurred the distinction between the truth or validity of a theory and the consensus that it enjoyed. And as late as 1764, three years before the publication of Priestley's History, Nollet confidently reported that his theory enjoyed that consensus, as most electricians shared his views on electricity.[145] As Home has argued, Nollet may have been correct in this assessment. However, whatever the case may be, there is much truth in Priestley's statement that Nollet had no followers--in the sense that his theory was not being used and developed by others in the manner that Franklin's was. If indeed, as Home argued, Nollet's theory dominated in France as late as 1770, this was due in large part to the Abbé's position in French science and to the fact that the center of electrical studies shifted after mid-century to Italy.[146]

A history of the debate between Nollet and the Franklinists would involve a history of electricity to 1770 and would need to include the work of Giambatista Beccaria (1716-1781), Franz Ulrich Theodor Aepinus (1724-1804), and Roger Joseph Boscovich (1711-1787) as well as the early work of Alessandro Volta (1745-1827), among others. This is well beyond the intended scope of this chapter and, indeed, of this dissertation.

For our own purposes the debate with Franklin serves to underline the fact that Nollet's theory was formulated to deal with an amalgam of observations and experiments, while Franklin's was mostly geared to address the Leiden experiment and overlooked a number of other phenomena traditionally associated with electricity. Moreover, there are no indications that the Leiden experiment was perceived as a decisive anomaly by Nollet, although he did recognize in it an unexpected problem. However, to him, this problem was resolved by assuming glass to be semipermeable to electricity. Given the amount of observations and evidence that he had "reconciled" with his theory, it is not surprising that this one experiment, however unexpected, should play only a minor role in his considerations.

It was only with the increasing interest in the Leiden experiment on the part of later electricians that this experiment came to be seen as one of particular importance to any electrical theory.

CHAPTER IV

NOTES

[1]Heilbron, Electricity, pp. 209-305, and esp. pp. 301-305; and Jean Daujat, Origines et formation de la théorie des phénomènes électriques et magnétiques, Actualités scientifiques et industrielles, nos. 989-991 (Paris: Hermann & Cie., 1945); hereafter Daujat, Origines.

[2]Ibid., and Home, "The Effluvial Theory of Electricity," pp. 3-41.

[3]Heilbron, Electricity, pp. 250-260. Robert Moriston Sayre, "Charles Du Fay and Electricity" M.A. thesis (Norman, The University of Oklahoma, 1965); hereafter, Sayre, "Du Fay."

[4]Nollet, Programme, pp. 99-104.

[5]Nollet's "Conjectures" will be discussed in more detail below.

[6]R. W. Home, "Post-Franklin Era;" Heilbron, Electricity, p. 287.

[7]Dufay, "Sixiéme mémoire sur l'électricité," Mémoires, 1734 (1736), pp. 503-526, esp. 523-525.

[8]Ibid.

[9]Dufay, "Troisiéme mémoire sur l'électricité," Mémoires, 1733 (1735), pp. 245-246.

[10]Fouchy, "Eloge," p. 122. Fouchy does not give precise dates; 1731 to 1733 is my estimate.

[11]Ibid.

[12]Ibid.; and Nollet, Programme. p. xvi.

[13]Fontenelle, "Dufay," p. 76. "One sees in his operations all the delicate attentions, all the ingenious skills, all the judicious patience that one needs to discover nature. . . ."

[14]Ibid. Dufay's first report to the Académie is the "Memoire

sur les barometres lumineux," Mémoires, 1723 (1753), pp. 295-306. Sayre, "Du Fay," pp. 140-142, gives a list of Dufay's journal publications.

[15] Fontenelle, "Dufay," p. 75.

[16] Dufay, "Premier mémoire sur l'électricité. Histoire de l'électricité," Memoires 1733 (1735), pp. 23-35.

[17] Ibid., p. 23.

[18] Ibid. ". . . there are few who have not stopped at this phenomenon, & have not tried to find an explanation each in his system."

[19] For example, Dufay did not mention experiments reported by the Accademia performed with a Torricelian vacuum to test whether electricity operated in a void. A second set of experiments also neglected by Dufay had to do with the interposition of screens between the electrified object (amber, in this case) and light bodies. These last experiments, not novel with the Accademia, were designed to test whether screens intercepted or hindered attraction. Saggi di naturali esperienze fatte nell' Accademia del Cimento (Florence: Giuseppe Cocchini, 1667), p. 132. In his exposition of Hauksbee's work Dufay completely avoided mentioning theoretical considerations behind Hauksbee's experiments. These experiments were designed to test the behavior of the electric effluvium, to see where and how it passed through different bodies.

[20] Dufay, "Premier mémoire," p. 25. Through his interpretation of Guericke's works Dufay was ascribing a meaning to the experiments other than the ones their author had intended. Guericke's experiments were attempts to reproduce what he believed were the powers at play in the universe. The powers of the globe to attract or repel were demonstrations on a microcosmic scale of the wordly virtues to be found in nature. There were a number of these "mundane virtues" and the sulphur globe displayed only a few of them. Otto von Guericke, Experimenta nova (ut vocantur) magdeburgica de vacuo spatio (Amsterdam: Joannem Jansonium, 1672), pp. 125-151. See Heilbron, Electricity, pp. 215-219.

[21] Dufay, "Premier mémoire," p. 35.

[22] Ibid. "I will not repeat that my plan was not at all to talk about all those who dealt [with electricity], it is easily enough seen that my aim has been to mention only those who have made some singular discovery, & who have contributed toward bringing our knowledge to the point at which it is today."

[23] Dufay, "Second mémoire sur l'électricité," Mémoires, 1733 (1735), pp. 73-74. The six questions can be paraphrased as follows: 1) Whether all substances (corps) can become electrified by rubbing, and whether those that cannot acquire this virtue cannot do so simply because they cannot be conveniently rubbed? (Why Dufay should have

entertained the possibility that all bodies could be electrified is not clear.) 2) Whether or not all substances are susceptible of contracting the electric virtue by contact with, or the approach of an electrified object? 3) Which are the bodies that can hold (arrester) or ease the transmission of this virtue, and which bodies are more readily attracted to electrified objects? (The first part of this question is clearly inspired by Gray's researches.) 4) What is common, if anything, between the two virtues electrified bodies have for repelling and attracting? (If Dufay was indeed surprised--as he claimed he was in his fourth memoir--by the existence of electrical repulsion, then certainly these questions were drafted after he finished his researches.) 5) What circumstances can cause a diminution or augmentation of the electrical virtue (e.g. void, temperature, air)? 6) What is the relation between electricity and the faculty of producing light?

[24]Idem., "Sixiéme mémoire," p. 525. ". . . as there will be achieved a more exact understanding of this marvellous property of matter."

[25]Heilbron, Electricity, p. 260. See also Daujat, Origines, Vol. III,.esp. pp. 410-415. On page 413 Daujat wrote: "L'oeuvre la plus importante du début du XVIIIe siècle est de beaucoup celle de Gray et de Du Fay. C'est en effet avec ces deux auteurs que l'étude des phénomènes électriques entre dans une voie vraiment scientifique et que se constituent les premières notions fondamentales de la physique de l'électricité."

[26]Dufay's reference to tourbillons, in his fourth memoir (where he explains repulsion) cannot be interpreted as a causal explanation. Dufay introduced them almost in passing; he did not include them in his summary of that memoir and they are not in his sixteen concluding principles, or in the summary of his work that appeared in the Philosophical Transactions, 38 (1733-1734), pp. 258-266. Fontenelle, however, drew special attention to this aspect of Dufay's electrical work. Histoire, 1733 (1735), pp. 11-13. But Dufay never developed it further although it reappeared in his seventh memoir.

[27]Dufay referred to electricity both as a "propriété de la matiére" ("Sixiéme mémoire," p. 525) and as "une qualité universellement répanduë dans toute la matière que nous connoissons." ("Septiéme mémoire sur l'électricité," Mémoires, 1737 (1740), p. 86).

[28]Dufay, "Quatriéme mémoire sur l'électricité," Mémoires, 1733 (1735), p. 475.

[29]Ibid., pp. 465 and 466 respectively; emphasis added.

[30]Ibid., p. 472.

[31]For example: Musschenbroek's chapter on electricity in his Essai is heavily based on Dufay's work; pp. 254-272. The last pages of the chapter are devoted to an examination of issues Musschenbroek

believed Dufay had raised but left unanaswered; pp. 269-272. Christian August Hausen (1693-1743) tried to quantify Dufay's tourbillons; Hausen, Novi profectus in historia electricitatis, post obitum auctoris, praematuro fato nuper exstincti, ex msto eius editi (Leipzig: Apud Theodorum Schwan, 1746), pp. 53-56. Georg Matthias Bose (1710-1761) presented his work as an attempt to further perfect that of Dufay; ". . . quoique j'ai veritablement assez de penchant, pour le pirrhonisme en fait de sistémes, j'ai néanmoins tenté, de perfectionner celui, que Mr. Du Fay nous a donné sur cette matiére. Quelle superiorité de genie, que dans cet Academicien?" Bose, Recherches sur la cause et sur la veritable téorie [sic] de l'électricité (Wittembergue: De l'imprimerie de Jean Fred. Slomac, 1745), p. vi; hereafter Bose, Recherches.

[32]"Expériences sur l'électricité," Histoire, 1743 (1746), p. 45.

[33]Metzger, La doctrine chimique, pp. 191-198 and 209-246.

[34]Nollet, "Conjectures," p. 107. Nollet had already sent a sketch of his theory to his correspondent at Riom in Auvergne, Etienne François Dutour (1711-1789) one day earlier; Heilbron, Electricity, p. 282.

[35]Nollet, "Conjectures," p. 107.

[36]Ibid.

[37]Ibid.

[38]Ibid. ". . . attentive to facts, working to multiply them & meditating carefully over all their circumstances, I have been waiting for over ten years for them to lead me to the principle from which they come; I believe I now finally have a hold on this principle, & for a few years I have been busy reconciling it to experience."

[39]Ibid., p. 107n.

[40]Ibid., pp. 107-108n.

[41]Ibid., pp. 107-108.

[42]Ibid., p. 108.

[43]Ibid., p. 108. ". . . the imagination in forming it has done nothing other than put to work that which experience has provided it, & I dare say it would be unjust to consider it as an assembly of simple possibilities, or of speculations devoid of proofs."

[44]Nollet, Lettres sur l'électricité (2nd ed.; 3 vols.; Paris: Chez H. L. Guerin & L. F. Delatour, 1764-1770 [Vol. III publisher: P. E. G. Durand]), III, 181-182; hereafter cited as Nollet, Lettres. The first edition was published in three volumes, 1753-1767. The "Dix-neuvieme lettre" containing this refusal to consider his electrical work as

purely conjectural was addressed to Aimé Henri Paulian (1722-1801). Although sympathetic to Nollet's electrical work, Paulian had described them, in his Dictionnaire de physique, using Nollet's own words, as "conjectures." Recognizing that Paulian had nothing but good intentions in referring to his work in this manner, Nollet felt compelled nevertheless to respond (p. 132): ". . . pour fermer la bouche à certaines gens, qui ne m'ont peut-être jamais lû, & qui prennent encore plaisir à traiter d'imaginations, d'hypotheses, &c. tout ce que j'ai écrit sur l'Electricité." In fact in his Essai sur l'electricité des corps Nollet had clearly distinguished between those aspects of his theory he had established experimentally, and conjectures that he felt free to entertain. The "propositions fondamentales tirées de l'expérience" numbered thirty-three principles Nollet believed he had demonstrated experimentally; Essai sur l'electricité des corps (Paris: Chez les Freres Guerin, 1746), pp. 138-146; hereafter cited as Nollet, Essai.

[45]The distinction between magnetism and electricity is at least as old as William Gilbert (1544-1603); see Duane H. D. Roller, The De Magnete of William Gilbert (Amsterdam: Menno Hertzberger, 1959), p. 92. Gilbert apparently "took pains to emphasize the distinction" between electricity and magnetism, according to Duane Roller and Duane H. D. Roller, "The Development of the Concept of Electric Charge. Electricity from the Greeks to Coulomb," in Harvard Case Histories in Experimental Science, Vol. II (Cambridge: Harvard University Press, 1957), 541-639, at p. 548. Mottelay wrote that the "first explicit treatise upon the close relationship existing between magnetism and electricity was . . . written . . . by M. Laurent Béraud (1703-1777)" in 1748. Paul Fleury Mottelay, Bilbiographical History of Electricity & Magnetism Chronologically Arranged (London: Charles Griffin & Company Limited, 1922), p. 163. See also Daujat, Origines, pp. 408-424.

[46]Nollet, "Conjectures," p. 110.

[47]Ibid. "Neither can one say that the effects of electricity come from a general & common attraction of all parts of matter; other than that this principle is only adopted by a segment of the world of physicists, and not even the largest, those that uphold it most fervently are forced to agree that one cannot apply, with any likelihood, attractions to the phenomena involved, without doing blatant violence to the laws one attributes to them, & according to which one supposes that they act in the ordinary mechanism of Nature."

[48]Voltaire, Correspondence, ed. by Theodore Besterman (107 vols,; Geneva: Institut et Musée Voltaire, 1953-1965), IX, 37 (letter to Rolland Puchot Des Alleurs, 13 March 1739).

[49]Ibid., VII, 368 (letter to Jean Jacques Dortous de Mairan, 11 September [1738]).

[50]Heilbron, Electricity, pp. 60-61.

[51]Musschenbroek, Essai, pp. 254-255.

[52]Ibid., p. 255. "These exhalations escape from bodies that are rubbed, & return to them through altogether surprizing movements, as one can judge by the bodies that they place in motion. They move and carry away with them all the other light bodies that can be agitated, of any kind or nature that they might be, & repel them afterwards. One may be assured by these effects & others also, that these exhalations are truly bodies that act, for one can feel them, when they come to strike the cheek or the hand. One can also see that they spread light during the night, & we hear them sparkle, crackle & escape. Since so many senses are affected at the same time one must be convinced, that they are truly bodies, & that they exit from electrical bodies."

[53]Nollet, "Conjectures," pp. 110-112.

[54]Ibid., p. 112n.

[55]Ibid., pp. 112-113.

[56]Ibid., p. 113. "To be in a position to answer the first of these questions, I search in Nature for a subtle fluid & one known elsewhere, or at least supposed and accepted by the largest number of Physicists, a fluid which has characteristics similar to those of the matter that makes electricity, which is capable of burning and giving light, which nevertheless does sometimes the one without the other, which bursts with noise depending upon certain circumstances, which is palpable & fragrant, if not by itself, at least by the substances with which it associates; for if I can know one that is in the habit of presenting itself through such effects, could not I legitimately ascribe to it these same effects everywhere I find them?"

[57]Ibid.

[58]Home, "The Effluvial Theory of Electricity," pp. 68-103.

[59]F. [sic] H. Winckler, Essai sur la nature, les effets et les causes de l'électricité avec une description de deux nouvelles machines a electricité. Traduit de l'allemand (Paris: Chez Sebastien Jorry, 1748), p. 145; hereafter Winkler, Essai. The original German work appeared in 1744.

[60]Musschenbroek, Essai, pp. 269-270.

[61]Bose, Recherches, esp. pp. xxix ff. Nollet received reports of Bose's work as early as 1743; see n. 32.

[62]Nollet, Leçons, Vol. IV, Lesson XIII, esp. pp. 153-208.

[63]Ibid., pp. 154, 160-168, 184-187.

[64]Ibid., p. 155.

[65]Ibid.

[66]Ibid. ". . . very inclined to believe that it is the same matter which burns & which gives light, which allows one to feel heat & see objects . . . " ". . . fire & light considered in their principle, are one and the same substance differently modified."

[67]Nollet, Lettres, I, 39-59.

[68]Nollet, Essai, p. 121. Nollet included the similarity of Fire, light and electrical matter among those "propositions fondamentales tirées de l'expérience" he enumerated in the Essai. But although he claimed the similarity to be demonstrated experimentally, he phrased the proposition with some equivocation: "32. Il y a toute apparence, que la matière qui fait l'électricité, ou qui en opére les phénomenes, est la même que celle du feu & de la lumière;" p. 146. ". . . it is that fire gives light almost always, & that there are many cases where light burns: Would Nature, which economizes so much in the production of Beings, while it multiples so liberally their properties, have established two causes for two effects for which it seems that one of the two [causes] would suffice?

"This reason is surely quite plausible, & one can also apply it to the electrical matter. Those who have examined its nature, & who have judged thereof by analogy, have almost all pronounced that fire, light, & Electricity spring from the same principle."

[69]Nollet, "Conjectures," pp. 147-148; also Leçons, IV, 204-205.

[70]Nollet, "Conjectures," pp. 113-122.

[71]Ibid., p. 114.

[72]Ibid., p. 116.

[73]Nollet, Leçons, IV, "XIII. Leçon."

[74]Nollet, "Conjectures," pp. 118-121.

[75]Ibid., p. 122.

[76]Ibid. This passage appeared in Dufay's sixth memoir, p. 520. Nollet failed to mention that Dufay believed that he had good grounds to deny the identity of light with electricity because of experiments he had performed which showed that one effect could be occasioned without the other. By the time Nollet was presenting the "Conjectures," however, the experiments Dufay had performed on this subject had been put into question by Jean Nicolas Sebastien Allamand (1713-1787) and Christian Friedrich Ludolff (1701-1763); Home, "The Effluvial Theory of Electricity," p. 125.

[77]Nollet, "Conjectures," p. 122. "This is then the opinion of those who had reflected most on the nature of the electrical matter, &

who were in the best position to study it, at a time when electricity had not yet produced more than a few prickly sparks at most, at a time when it had been tried a hundred times, but always to no avail, to excite the electric fire to the point of inflaming other bodies: how much more justified we are to embrace the same sentiment, when we see electrified bodies actually kindle all the liquors & all the inflammable vapors, & burn them just as they are consumed usually by the most common fire!"

[78]Ibid., p. 138n.

[79]Nollet, Lettres, I, 39-59 ("Troisieme lettre").

[80]Ibid., pp. 48-49.

[81]Nollet, "Conjectures," p. 125. ". . . I saw almost as many times as I wanted that when an electrified body approaches another that is not, there emanates at the same time from each one of them a flow of matter that can be felt from one and the other side as a light breeze, as long as the two bodies are at a certain distance from each other, & which becomes a luminous & permanent aigret, when the degree of proximity is not large enough to make it burst out in sparks.

Having strongly electrified a glass globe, as I was still rubbing it, solid bodies of all kinds were brought close to it within a few lines of distance, & I was pleasantly surprised to see come out from different places in these bodies, & especially through the most jutting parts, uninterrupted jets of fire, more or less dense. . . ."

[82]Ibid., pp. 125-126.

[83]Ibid., p. 126. "Electrify using a globe an iron rod soaked in wine spirit, you will feel all around [it] an [almost] imperceptible rain, caused without doubt by small drops of the liquor that the electrical matter carries with it as it exits, as we also see that it drives away wood dust, tobacco, sand, &c. but during all the time that this effect lasts, the same iron rod does not attract any less all the light bodies presented to it from whatever place that that might be."

[84]Heilbron suggested that the divergent streams were the foundation of Nollet's theory. He pointed out that of the twenty-four experiments Nollet reported to Dutour in the 1745 letter sketching his theory, the first six related to discharges; Heilbron, Electricity, p. 283.

[85]Nollet, "Conjectures," p. 126. ". . . si l'on voit en même temps d'autres corps légers se précipiter de toutes parts sur le corps électrique dont il s'agit, n'est-on pas forcé de reconnoître deux courans de matière dont les directions sont opposées. . . ?" That Nollet did reason in this fashion is also evident from his letter to Bose: Bose, Recherches, pp. xliv-l; and Nollet's discussion of the circumstances that prompted it; Nollet, Recherches sur les causes particulieres des phénoménes électriques, et sur les effets nuisibles ou avantageux

qu'on peut en attendre (Paris: Chez les Freres Guerin, 1749), pp. 56-59; hereafter cited as Nollet, Recherches.

[86]Winkler, Essai, pp. 71, 83-84, and 107. Winkler was prone to believe that these divergent and convergent rays attracted and repulsed light bodies by cohesion, rather than impulsion.

[87]Bose, Recherches, pp. xlv-l.

[88]Nollet, "Conjectures," p. 147. He expanded on this argument in the Essai, pp. 118-120.

[89]Nollet, "Observations sur quelques nouveaux phénomènes d'électricité," Mémoires 1746 (1751), pp. 1-23; hereafter Nollet, "Observations."

[90]Nollet, who read excerpts from the letter to the Académie, deleted the reference to the Kingdom of France; "Observations," pp. 2-3. Jean Baptiste Secondat (1716-1796) apologized in his Mémoire for what he apparently regarded as Musschenbroek's malapropism explaining: "Il cite ce Royaume pour exprimer les grandeurs de l'Univers les plus flateuses." [Secondat], Mémoire sur l'electricité (Paris: Chez la Veuve David, n.d. [approbation is dated July 24, 1746]), p. 11; hereafter Secondat, Mémoire.

[91]Nollet, "Observations," pp. 2-3. ". . . all of a sudden my right hand . . . was hit with such violence, that I had all of my body shaken like a thunder bolt." ". . . you will feel a stupendous blow which will hit all of your arm, & even all of your body, it is a thunderbolt."

[92]Ibid., p. 4.

[93]Ibid., pp. 15-18.

[94]Ibid., p. 12.

[95]Ibid. Nollet discussed this "exception à la loi générale" in somewhat more detail in his Recherches, pp. 262-266, and in his Lettres, I, 83-128 ("Cinquieme lettre, sur différens faits concernants l'expérience de Leyde").

[96]Heilbron, Electricity, pp. 316-318. Nollet referred to the experiment as "l'expérience de Leyde" in the "Observations," and later reminded readers that he was responsible for its name, Lettres, I, 83.

[97]Heilbron, Electricity, pp. 316-321.

[98]Ibid., pp. 321-323.

[99]E.g., Nollet, Lettres, I, 83-128.

[100]Le Chevalier d'Arcy, "Mémoire sur l'électricité, contenant la description d'un électromètre, ou d'un instrument servant à mesurer la force électrique," Memoires, 1749 (1753), pp. 63-74; Le Roy had also participated in the preparation of this memoir; Histoire, 1749, p. 7. Le Roy later became one of Franklin's staunchest defenders. Fouchy in the Histoire for 1753 introduced his summary of the many papers on electricity read to the Académie that year in the following manner: "Jusqu'ici les Physiciens avoient été assez d'accord sur l'Électricité. La doctrine de M. l'abbé Nollet, proposée en 1745, n'avoit trouvé en Europe que peu de contradicteurs; l'Amerique vient de lui en fournir un. . . ." Histoire, 1753 (1757), "Sur l'électricité," pp. 6-39, at p. 6.

[101]Of course criticisms of his theory can be interpreted as criticisms of his explanation of the Leiden experiment. The Leiden experiment in the long run may have undermined support for Nollet's theory, but there are no indications that this occurred abruptly. Nollet undertook to answer critics of his theory in the Recherches and Essai. [Abbé N. de Mangin] Histoire générale et particuliere de l'électricité, ou ce qu'en ont dit de curieux & d'amusant, d'utile & d'interessant, de réjoüissant & de badin, quelques physiciens de l'Europe (Paris: Chez Rollin, 1752). The Abbé de Mangin (d. 1772) was also very critical of Nollet and others and had his own theory to offer readers. Mangin mentioned Franklin's work only briefly (pp. 174-186). It is clear from Mangin's special attention in combatting Nollet's theory that he considered Nollet's work dominant in France.

[102]Nollet, Recherches, pp. 32-56.

[103]Ibid., pp. 56-75.

[104]Ibid., and pp. 5-32, 76-102. (Jean) Morin, Nouvelle dissertation sur l'electricité des corps, dans laquelle on develope le vrai mecanisme des plus surprenans phénomênes, qui ont paru jusqu'à present, & d'une infinité d'experiences nouvelles, de l'invention de l'autheur (Paris: Chez la Veuve Estienne & Fils, 1748). Nollet mistakenly identified an anonymous 1746 memoir by Nicholas Antoine Boullanger (1722-1757?) as the work of Secondat. In a later work Boullanger identified himself as the author; [Nicholas Antoine] Boullanger, Traité de la cause et des phenomenes de l'électricite (Paris: Imprimerie de la Veuve David, et se vend chez Pecquet, 1750), p. iii; hereafter referred to as Boullanger, Traité. Nollet made the correction in subsequent editions of the Essai. I have not seen the 1746 memoir by Boullanger, however, in the Traité he mentioned Nollet only once in reference to an experiment. Boullanger believed that the electrical matter was nothing other than subtle particles of the atmosphere. Morin was professor of philosophy at Chartres and a correspondant of the Académie. He was also the author of Abregé du mecanisme universel, en discours et questions physiques (Paris: A. Cailleau, 1740). Louis was a surgeon at the Salpêtrière.

[105]Nollet, Recherches, pp. 39, and 50-56.

[106]Heilbron, Electricity, p. 288.

107 Boullanger, Traité. Translations of works by Winkler, Freke, Watson, and Benjamin Martin (1704-1782) appeared bound together in one volume under the general title Recueil de traités sur l'électricité traduits de l'Allemand & de l'Anglois.

108 On the animosity between Réaumur and Buffon see Torlais, Réaumur, pp. 239-245. For its repercussions on electricity see by the same author "Une grande controverse scientifique au XVIIIe siècle. L'abbé Nollet et Benjamin Franklin," Revue d'histoire des sciences, 9 (1956): 339-349; hereafter, Torlais "Controverse."

109 Torlais, "Controverse," p.341. Franklin, The Autobiography of Benjamin Franklin, ed. by Frank Woodworth Pine (Garden City, New York: Garden City Publishing Co., 1916), p. 292; hereafter Franklin, Autobiography.

110 Ibid. The translation of Franklin's Experiences and Observations, like those of the Recueil of 1748, appeared without the name of the translator; Expériences et observations sur l'électricité faites à Philadelphie en Amérique par M. Benjamin Franklin (Paris: Chez Durand, 1752); hereafter Franklin, Expériences.

111 Heilbron, Electricity, p. 348.

112 Ibid.

113 Ibid., pp. 348-351. See Nollet's "letter" to Marie-Ange Ardinghelli for an account by the Abbé of the sentry-box experiment, Lettres, I, 1-23.

114 Letter from Buffon to Président de Ruffey, 22 July 1752, in Correspondance inédite de Buffon à laquelle on été réunies les lettres publiées jusqu'à ce jour, ed. by Henri Nadault de Buffon (2 vols.; Paris: Librairie de L. Hachette et Cie., 1860), I, 56-57, at p. 57.

115 Le Monnier, "Observations sur l'électricité de l'air," Mémoires, 1752 (1756), pp. 233-243; Le Monnier, who apparently did not care to enter into a debate with Nollet, published only this paper sympathetic to Franklin's works. Le Roy published a number of them in the pages of the Mémoires for 1753, and later years. On Le Roy's position see Heilbron, Electricity, pp. 359-361.

116 Franklin, Expériences, pp. 7 and 11. "It is (says M. de Buffon,) by precise experiments, reasoned & pursued, that one forces nature to unveil its secret; . . ." ". . . it is not enough to attach oneself solely to the path of experience, unless one is, like our author, fecund in abilities, ingenious in discoveries, & fortunate in applications."

117 Nollet, Lettres, I, 5. Secondat, "Histoire de l'electricité lue à l'Académie de Bordeaux en 1748," in Observations de physique et

d'histoire naturelle sur les eaux minerales de Dax, de Bagneres, & de Barege, sur l'influence de la pesanteur de l'air dans la chaleur des liqueurs bouillantes, & dans leur congellation. Histoire de l'électricité, &c. (Paris: Chez Huart & Moreau Fils, David, Durand, & Pissot, 1750), pp. 125-170. Secondat himself did not mention the Abbé in this history.

118 Torlais, Un physicien, pp. 63-64.

119 Heilbron, Electricity, p. 348n. ". . . apparently to be in a position not to mention me; this affectation of which I do not complain, has been noticed by all those who have seen this work."

120 Ibid., p. 362.

121 Nollet, Lettres, I, 27.

122 Nollet, Lettres, I, 7, 34. Nollet was willing to acknowledge that Franklin had been the first to conceive of an experiment to test the analogy between thunder and electricity ("Discours," p. lxxix, and Lettres, I, 3-6). However, he emphasized that the analogy had been made much earlier by himself, and that Franklin never performed the experiment.

123 Nollet, Lettres, I, 83. "It seems, Sir, that in your experiments on Electricity, what you had mainly in mind was to examine in depth this surprising phenomenon. . . ."

124 Idem. "Eclaircissemens," Mémoires, 1747 (1752), p. 196. "I was not unaware that in the Leyden experiment the vase of water which contains the water electrifies strongly and conserves its electricity a long time, however full it may be kept: . . . But I have let the general law remain and I have exposed this particularity as an exception which one can consider as unique. . . ."

125 Benjamin Franklin's Experiments. A New Edition of Franklin's Experiments and Observations on Electricity, ed. by I. Bernard Cohen (Cambridge: Harvard University Press, 1941), p. 180.

126 Nollet, Lettres, I, 49.

127 Ibid., I, 50. ". . . is that the latter forces its way through all bodies without exception, & that the other . . . never goes but half-way through the thinnest glass."

128 For a history of the debate over experiments performed by Nollet to discredit Franklin's theory see Home, "The Effluvial Theory of Electricity;" also Home, Aepinus, esp. pp. 65-106.

129 Joseph Priestley, The History and Present State of Electricity, with the Original Experiments (London: Printed for J. Dodsley, et al., 1767), pp. 453-454 and 159-160.

[130] Ibid., p. 160.

[131] Ibid., pp. 453-454.

[132] Cohen, Franklin and Newton, p. 12.

[133] Ibid., pp. 12-13.

[134] Ibid., p. 389.

[135] Ibid., p. 388. Cohen wrote: "Although he [Nollet] claimed he was not a Cartesian, he boasted that he was not a Newtonian" (emphasis mine).

[136] E. T. Whittaker, A History of the Theories of Aether and Electricity from the Age of Descartes to the Close of the Nineteenth Century (New York: Longmans, Green, and Co., 1910), p. 41.

[137] Daujat, Origines, pp. 437-446.

[138] Heilbron, Electricity, p. 362; Home, "Post-Franklin Era."

[139] Heilbron, Electricity, pp. 358-362.

[140] Ibid.

[141] Nollet, Lettres, II, 63. ". . . the major issue, the one on which the partisans of M. Franklin unite, so divided [are they] otherwise, is that in all Electricity they want to recognize but one flow of matter; & when I cited the simultaneous attractions & repulsions, the first, the most infallible, the best known of all the electrical phenomena, as a palpable sign of simultaneous affluences & effluences, M. Leroy answered me: We do not know how the electrical attractions & repulsions take place."

[142] Home, "Post-Franklin Era."

[143] Franklin in his Autobiography (p. 293) wrote that Nollet "lived to see himself the last of his sect, except Monsieur B_________, of Paris, his eleve and immediate disciple."

[144] Brisson made an unsympathetic translation of Priestley's History of Electricity where he challenged Priestley's opinions on a number of issues. Histoire de l'électricité, traduite de l'anglois de Joseph Priestley, avec des notes critiques. Ouvrage enrichi de figures en taille-douce (3 vols.; Paris: Chez Herissant, 1771), I, 293n. "It is not the number of partisans of an opinion which determines its value. The truth is not always on the side of the larger number."

[145] Nollet, Leçons, VI, Lesson XXI.

[146] Heilbron, Electricity, p. 362.

## CHAPTER V

## CONCLUSION

This dissertation examines the scientific career of the Abbé Jean Antoine Nollet and attempts to throw some light onto his work in the context of eighteenth-century experimental physics. Before concluding it may be profitable to go over some of the themes developed in it.

Nollet's scientific activities covered a wide range of interests. He was an experimental physicist, a science popularizer, an instrument maker, electrician, pedagogue and scientific technician. These multi-faceted activities found unity in his view of science as useful and public knowledge acquired and developed through collective efforts of observation and experimentation in the scientists' laboratories. Already present in much of the science of the sixteenth and seventeenth centuries, this view of physics became more forcefully and widely expressed in the eighteenth century. Concurrent with this transformation in the way physics was viewed and practiced was a movement toward the standardization of the field of physics, a movement intrinsic to the growing belief that science was a collective endeavor. According to this belief, scientific knowledge advanced through the collaboration of men. Experiments, with set instruments and procedures for their operation, were one means through which this collaboration was made possible. Concomitant also with this transformation came a change in the immediate subject matter of physics. Once broadly identified with natural history, the physics of the experimentalists became by the end of the eighteenth century more and more understood to be that field practiced in the laboratory.

Nollet played an important role in this development. From early on in his career his interests led him toward experimental physics. His dexterity in the mechanical arts seems to have paved the way for his association with Dufay, one of France's prominent experimentalists and the person responsible for Nollet's meetings with Desaguliers, 'sGravesande and the Musschenbroek brothers. From 1733, the year he collaborated with Dufay on the latter's electrical experiments, to 1769, the year L'art des experiences was published, Nollet was a dedicated practitioner and advocate of experimental physics, and was thus recognized by his contemporaries.

Nollet's experimentalism was more akin to that practiced by his English and Dutch contemporaries than to that of earlier French experimentalists. It was sharply different in method and intent, as I argued in chapter three, from the experimentalism of Rohault. While Rohault believed that experiments were basically meant to illustrate or elucidate truths arrived at through "systematic" knowledge, Nollet regarded experiments as the necessary bases of physics. Rohault's experiments were expected to confirm an understanding already arrived at independently by the mind; the experiments of Nollet were to be guided by no more than simple guesses. Instead of confirming systems, experiments would help create them. It was only through the careful compilation and the use of observation and experimental facts that science progressed.

Physics as understood by Nollet was based on two tenets, the indisputable truths of the laboratory, and those truths of science on which there was a consensus among physicists. He believed that only by being based on these two solid, non-controversial tenets could the progress of science be guaranteed. Nollet's work in electricity serves as an illustration of the method he pursued; there he combined observation, experiments and those ideas he felt sure physicists were agreed on. He thus was certain that although he was offering a system, he was doing so using undeniable assertions and through steps about which there could be no dispute. To this end he worked toward the careful explanation of his experiments and description of instruments, advocating to his students that they do the same.

Thus, as practiced by Nollet and other eighteenth-century experimentalists, experiments became an intrinsic part of physics. While historians today may argue on whether or not, say, Galileo experimented, and if he did, whether his insights were occasioned by experimentation, there is no room for argument on whether Nollet, Franklin, Musschenbroek, or Boyle and Hauksbee experimented.[1] Their science cannot be understood without reference to their experiments.

Nollet, in fact, showed disdain for simple thought-experiments. He criticized Franklin for suggesting the sentry-box experiment but never bothering to perform it. He commented with sarcasm, that possibly the reason Franklin did not do it was that, maybe, it never thundered in Philadelphia.[2] He also criticized Descartes for simply assuming that an experiment would confirm his claim that the fall of bodies was caused by vortical motions.

> . . . voulant appuyer son hypothese sur quelque fait qui pût en faire sentir la possibilité, [Descartes] imagina de faire tourner

> sur son axe une Sphere creuse, de quelque matiére solide, & remplie de petits corps spécifiquement plus pesants les uns que les autres. Il prétendoit que ceux qui auroient le plus de masse, ayant, à vîtesses égales, plus de force centrifuge, obligeroient les autres à s'approcher du centre de leur mouvement, & qu'on verroit prendre à ces derniers la forme d'un noyau sphérique, qui indiqueroit par sa figure la direction des forces auxquelles ces petits corps obéissoient. Cette expérience ingénieuse ne fut alors qu'indiquée, c'est un Juge que ce Philosophe s'est nommé lui-même dans une affaire de Systeme.[3]

As I have indicated in chapter one, Nollet--engaged in a debate with Privat de Molières--performed the experiment which Descartes had only suggested and demonstrated to the Académie's satisfaction, that it refuted, and not confirmed, the claim made by the philosopher.

The new role assigned experiments in the physics of experimentalists met with resistance by some critics who believed that experiments were valuable only if used as auxiliary tools to the direct study of the natural world. Castel, one of those critics, expressed this view in his review of 'sGravesande's Elements of Physics, a work he found to be full of experiments but devoid of true physics.

> L'art est bon; il est bon de faire des experiences; mais lorsque je vois des livres entiers de Physique, . . . tout pleins de ces experiences rares, curieuses, ingenieuses, si l'on veut, que l'art fournit, dit-on, à l'Angleterre, sans presque aucune de ces observations simples, naïves, faciles que la nature fournit abondamment dans tous les pays, à tous les esprits; je me souviens alors que l'art altère tout. . . .[4]

Art may, indeed, alter everything, and in that sense, experimental physics was no longer the study of the natural world that interested Castel but the study of that world as reproduced in the laboratory. Not only the method, but also the subject matter of physics had changed.

Another critic of the new experimentalism was Dortous de Mairan. In a speech read to the Académie in 1748, Mairan defended the systematic method arguing that experiments could only be meaningful if suggested by a system. But systems and chimeras, he told the Académie, seem to have become synonymous words; "C'est un systême fait souvent la critique entière d'un livre."[5] It is, indeed, possible to abuse systems, Mairan conceded, but could not the same be said of experiments? "N'abuse-t-on pas des expériences si elles ne sont conduites par la méthode, & éclairées du raisonnement?" Meaningful experiments must carry with them "quelque supposition tacite de ce qu'elles doivent donner étant bien faites."[6] The experiments of Newton, "non comme on les voit rassemblées & rédigées dans son Optique en ordre de synthèse," but as they appear here and there in the Philosophical Transactions were, each one of them, the consequence of some systematic reflection.[7] Those to whom we owe the largest debt for increasing our knowledge of the world, all merit the title, "ou si l'on veut, le blame," of being systematists. It would have been a great loss had these men been more circumspect or more timid with their thoughts.

> Il est plus que probable que Képler n'auroit jamais pensé à la fameuse Règle qui l'immortalise, si elle n'étoit venue à l'appui, si elle n'étoit sortie comme d'elle-même de son système harmonique des Cieux, tout fondé sur l'inscription des orbes planétaires aux cinq corps réguliers des Géomètres, & sur je ne sais quelles perfections pythagoriques des nombres, des figures & des consonances.[8]

Mairan was speaking in praise of genius--and about men of a century often characterized as the century of genius. However, it is one thing to laud genius and another to advocate it as a method. With the emphasis experimental physics placed on collective work, on standardization, on instruments and procedures and on the need for careful work in the laboratory, this activity could not formally depend on, or exalt, the individual strokes of genius brought about by fortune. Experimental physics, one may say, de-emphasized individual genius by choice. It was the resulting pedantic character of experimental physics that led Libes to ridicule it, and modern historians to characterize its age as one of "normal" science or of simple "consolidation."[9]

Experimentalism also encountered criticism from other quarters. The mathematically-minded d'Alembert devoted a large portion of his article "Experimental" in the Encyclopédie, to advise Nollet and his students at the newly instituted chair of experimental physics at the Collège de Navarre on the true role of Experiments.[10] The use of experiments, d'Alembert wrote, was simply to confirm theory, and occasionally - to offer a suggestion for further study. Careful observation plus the ability to quantify sufficed to derive the basic and fundamental laws of physics. "Les phénomènes les plus simples & les plus orinaires," were enough, for example, to serve as a basis for a simple and illuminating theory of the laws of motion. Once experience yielded the essential fact experiments were no longer necessary. If physicists chose to occupy themselves with further experiments it would be as a "recherche de simple curiosité, pour reveiller & soûtenir l'attention des commençans;" simply to give new students the satisfaction of seeing with their own eyes that which "la raison leur a dejà demontré."[11]

This view of experiments, and their role in education, was very different from that of Nollet who believed that when he demonstrated experiments to his students he was transmitting ways of physical understanding "par la même voye que les Sçavans ont employée pour les acquerir."[12] Experiments were not addenda to theory and geometry, much less curiosities, they were the bases and foundations of physics. To Nollet, mathematics and physics were distinct fields. While he believed a basic knowledge of geometry essential to the practice of physics, he argued that those who were trying to introduce mathematics into physics were confusing the two fields. Mathematics was an exact science while in physics one almost never finds precision or certainty. In most works where physics is discussed in algebraic characters nothing of what is said would be lost if expressed in a language intelligible to all. These supposed physical treatises by mathematicians clearly show that "le peu de Physique qui s'y trouve a servi de prétexte à une autre Science, dont on a voulu faire parade."[13]

However, as the century advanced those fields that Nollet considered his particular domain came more and more to be treated quantitatively. While in the beginning of the century only optics, among the fields of physics, came under quantitative treatment, toward the end of the century mathematics seemed to widen its scope and impinge on much of the domain of physics. Delambre, in a work already referred to above, commented on this development.

> A mesure que les sciences font des progrès et que leurs limites s'étendent, on voit diminuer l'espace qui les séparoit, et la ligne de démarcation devient plus difficile à tracer. Si, d'un côté, elles font des conquêtes, elles peuvent aussi perdre quelques parties de leur domaine, qui passent dans celui de la science voisine: ainsi tout ce qui concerne la lumière, la pesanteur, le mouvement et le choc des corps, est aujourd'hui presque uniquement du ressort de la géometrie; on a même tenté de soumettre au calcul les phénomènes du magnétisme et de l'électricité.[14]

Nollet's "failure" to incorporate mathematics into the study of physics cost him Libes' strictures at the end of the century. Recently, Home has argued that it was the ability of Franklin's theory, as modified by Aepinus, to be rendered completely mathematical that resulted in the adoption of the Philadelphian's views in electricity and the demise of Nollet's.

In defense of Nollet one may point out, with Daumas, that before experimental physics could be made quantitative, before its instruments could be used for that purpose, physics had first to become a science of instruments. In the 1730's, when Nollet first began his scientific career, French science was to a large extent dominated by the systematic philosophy of Descartes. Although Newtonianism was making inroads into France during that period, to many what it had to offer was yet another system. Against this background Nollet opted for a type of physics with a somewhat different tradition--experimental physics, concerned with facts and truths that spoke to the mind, or rather to the eye, in a clear and straightforward way. Out of this experimentalism he expected to develop a physics built not on conjectures and hypotheses, but facts. He thus steered away from the debate between Cartesians and Newtonians, avoided adopting any position or view on which there was not a consensus, or of which the truth or falsity could not be determined in the _cabinet de physique_. His concern was with method and not with fine measurements or quantification.

From the 1730's to his death in 1770 Nollet continuously practiced and advocated the experimental method as he understood it, and was an important contributor to the development that led to much of physics becoming a science of instruments and laboratories. By the latter part of the century much of this process had taken place, and it was only then that much of laboratory physics began to be treated quantitatively. Nollet's work did not reflect this latter development. His approach to physics remained throughout his life much the same as it had been in 1745 when he published his electrical theory in the pages of the Académie's _Mémoires_.

The generation of experimentalists that followed Nollet, among them his disciple Brisson, concerned themselves with a more precision-oriented experimentalism and practiced their physics with much finer instruments. This allowed for physicists working only a few years after the death of Nollet to deride the lack of precision of his instruments. Sought after at the beginning of the century by the likes of Réaumur, Dufay and Voltaire, Nollet's instruments were regarded by the physicist J. A. C. Charles (1746-1823)--who bought a large part of Nollet's own _cabinet_--as curiosities of a distant past. The quantification of experimental physics having also engulfed electrical theory, where he had performed his major theoretical work, Nollet, who had been a leader in the introduction of experimentalism into France, had little to show for his reputation in 1810 when Antoine Libes was reading his works.

CHAPTER V

NOTES

[1]For a discussion of the importance of experiments to Galileo see Kuhn, "Mathematical versus Experimental Traditions," pp. 41-48 (Chapter III, note 16); and Stillman Drake, Galileo at Work. His Scientific Biography (Chicago: The University of Chicago press, 1978), pp. xix-xxii.

[2]Nollet, Lettres, I, 8.

[3]Nollet, "Mémoire dans lequel on examine par voie d'expérience, quelles sont les forces & les directions d'un on de plusieurs fluides renfermés dans une même sphere qu'on fait tourner sur son axe," Mémoires, 1741 (1744), p. 184. ". . . wanting to base his hypothesis on some fact which might make it look possible, [Descartes] thought of conceiving a hollow sphere, of whatever solid matter, filled with small bodies some heavier than others, and making it turn on its axis. He believed that those bodies with more mass, having, at equal speeds, more centrifugal force, would force the others to approach the center of their movement, & that one would see them taking the form of a spherical core, which would indicate by its shape the direction of the forces to which these small bodies respond. This ingenious experiment was then merely described, as a Judge that this Philosopher offered himself in an affair of system."

[4]Castel, "Physices elementa," Mémoires pour l'histoire des sciences & des beaux arts [Mémoires de Trevoux], 1721, 1761-1796, at pp. 1766-1767. "Art [i.e. technique] is good; it is good to perform experiments; but when I see whole physics books, . . . filled with these rare, curious, even ingenious, if one wants, experiments, that art provides, they say, in England, without almost any of these simple, naive, easy observations that nature provides abundantly in all countries, to all intellects; I am reminded then that art alters everything . . ."

[5]Dortous de Mairan, "Préface qui a été lûe dans l'assemblée publique de l'Académie Royale des Sciences, du 13 Novembre 1748," in Dissertation sur la glace, ou explication physique de la formation de la glace, & de diverses phénomènes (Paris: De l'Imprimerie royale, 1749), p. v. "It is a system often makes for the entire critique of a book."

[6]Ibid., p. viii. "Does one not abuse experiments if they are not guided by method, & enlightened by reasoning?" ". . . some tacit supposition of what they must yield if they are well done."

[7]Ibid., p. ix.

[8]Ibid., p. xi. "It is more than probably that Kepler would never have thought of the famous rule that immortalizes him, if it had not come in support of, if it had not come as if self-generated from his system of the harmony of the Heavens, all of it founded on the inscription of the planetary orbs of the five regular bodies of the Geometers, & on I do not know what Pythagorean perfection of numbers, of figures & consonances."

[9]T. S. Kuhn referred to eighteenth-century physics as a period of normal science, and A. R. Hall referred to it as a period of consolidation. Thomas S. Kuhn, The Structure of Scientific Revolutions (Chicago: The University of Chicago Press, 1962), pp. 39-40; A. Rupert Hall, The Scientific Revolution 1500-1800. The Formation of the Modern Scientific Attitude (London, New York, Toronto: Longmans, Green and Co., 1954), p. 341.

[10]D'Alembert, "Experimental," Encyclopédie, pp. 298-301, 300.

[11]Ibid. ". . . research of simple curiosity, to awaken and sustain the attention of beginners. . ." ". . . reason had already shown them."

[12]Nollet, Programme, p. vii. ". . . by the same path that the savants used to acquire this knowledge."

[13]Nollet, "Discours," pp. xci-xcii. ". . . the little Physics that one finds there has served as a pretext for another science which has been chosen for display."

[14]Delambre, Rapport, pp. 213-214. "As the sciences progress and their limits spread, one sees diminish the space that separates them, and the line of demarcation becomes harder to trace. If, on one hand, they make new conquests, they can also lose parts of their domain, which pass to that of the neighboring science: Thus all that concerns light, weight, the movement and collision of bodies, falls today almost uniquely within the jurisdiction of geometry; they have even tried to submit to computation the phenomena of magnetism and electricity."

APPENDIX

On the publication dates of Nollet's

Leçons de physique expérimentale

The historical literature on Nollet has perpetuated some confusion as to when Nollet's Leçons de physique expérimentale were first published. A good many authors, right down to the present (e.g., Heilbron),[1] have accepted that the six volumes were published between 1743 and 1748. Indeed, that information is printed in no less authoritative a place than the Catalogue général des livres imprimés de la Bibliothèque Nationale.[2] It is true that this is contradicted by Jean Torlais, who gave the publication dates 1743-1764.[3] But being unable to consult a full first edition of Nollet's Leçons, I thought I might not be able to find a definitive resolution to this puzzle.

The editions available to me were, like most sets of this often-reprinted work, mixtures of succeeding editions of the several volumes. The Bibliothèque Nationale's Catalogue, the National Union Catalog, and the Torlais bibliography show how the early volumes of the Leçons were reissued in advance of the appearance of some later volumes. It seemed possible, in view of the republication dates, that I had access to a set including first editions of volumes V and VI, mixed with later editions of preceding volumes. As my study of Nollet's work developed, this possibility took on some significance: it made a difference whether or not the views Nollet expressed in the fifth and sixth volumes of his Leçons dated only from the late 1740's, or instead from the 1750's and 1760's, respectively.

Nothwithstanding the authority of the Bibliothèque Nationale's Catalogue and of some worthy scholars, certain seemingly decisive clues pointed toward publication dates in 1755 for volume V and in 1764 for volume VI. First, there are the approbations of the Académie des Sciences in the volumes themselves. These official approvals are dated 15 March 1755 and 18 January 1764, for volumes V and VI respectively. How could approvals come so late for books published by 1748? In addition, I found original reviews of these two volumes in 1755 and 1764, respectively.[4]

Finally, through the kindness of Professor Mary Jo Nye, who examined the volumes of the Leçons at the Bibliothèque Nationale, it is now confirmed that the fifth volume was published in 1755 and the sixth in 1764. The original edition is therefore properly dated 1743-1764.

APPENDIX

## NOTES

[1]Heilbron, *Electricity*, p. 548; also Heilbron, "Nollet, Jean-Antoine," *Dictionary of Scientific Biography*, Vol. X (1974), pp. 145-148.

[2]*Catalogue général des livres imprimés de la Bibliothèque Nationale*. *Auteurs*, Vol. CXXV (Paris: Imprimerie Nationale, 1934), cols. 486-487.

[3]Torlais, *Un physicien*, p. 257.

[4]E.g., *Mémoires pour l'histoire des sciences & des beaux arts* [*Mémoires de Trévoux*], 1755 [Juillet], pp. 1954-1975; 1764 [Juillet], pp. 257-294.

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Anonymous. [Review of Nollet's] "Programme, ou idée generale d'un cours de physique expérimental [sic]. . . ." Journal des sçavans, 1738, pp. 624-629.

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