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Jean Paul Marat and the scientific underground of the old regime

Conner, Clifford Despard, Ph.D.

City University of New York, 1993

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JEAN PAUL MARAT AND THE SCIENTIFIC UNDERGROUND OF THE OLD REGIME

by

CLIFFORD D. CONNER

A dissertation submitted to the Graduate Faculty in History in partial fulfillment of the requirements for the degree of Doctor of Philosophy, The City University of New York

1993

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Abstract

JEAN PAUL MARAT AND THE SCIENTIFIC UNDERGROUND OF THE OLD REGIME

by

Clifford D. Conner

Adviser: Professor Joseph W. Dauben

This study attempts to broaden the scope of historical investigation of the scientific culture of the French Revolution, which has traditionally been viewed from the perspective of elite contemporary scientists alone. Prominent academicians such as Lavoisier, Laplace, and Cuvier defined a scientific orthodoxy whose dominance has long been assumed due exclusively to scientific merit.

The present study instead examines the work of a number of unorthodox natural philosophers who constituted a scientific underground of the old regime. Although they are all remembered for significant political, literary, or other accomplishments, their works of natural philosophy have generally been ignored or dismissed as "charlatanism," "pseudoscience," or worse. Their neglected texts form the subject of this study.

Jean Paul Marat's scientific investigations into heat, light, and electricity are examined in detail. Other natural philosophers whose work is considered are Jean Louis Carra (a leading revolutionary journalist), Nicolas

Bergasse (chief theoretician of the Mesmerist movement), Jacques Henri Bernardin de Saint-Pierre (Rousseau's literary heir), Pierre Samuel Du Pont de Nemours (statesman, economist, and industrial patriarch), Nicolas Edmé Restif de la Bretonne (a major figure in French literature), Jean Claude Delamétherie (moving spirit of the most important scientific journal before the Revolution), the Comte de Lacépède (academician and Napoleonic politician), Jean Baptiste Lamarck (pioneer life scientist), and a pair of collaborators, the Baron de Marivetz and Louis Jacques Goussier (known primarily for their immense work, *Physique du monde*).

The conservative political reaction following the Revolution reinforced scientific conservatism as well; speculative natural philosophy -- the "spirit of system" -- was virtually banned from institutionalized science in France. The further development of French science can be better understood by taking this post-Revolutionary social context into account. The advance of knowledge might have been better served if the orthodox scientists had appreciated the potential value of the scientific underground's creative contributions.

An open-minded examination of the work of Marat and the other natural philosophers leads to the conclusion that their science was normal science in the eyes of their contemporaries, and even by more universal criteria. To label it "charlatanism" or "pseudoscience" is ahistorical.

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PART I:
The Spirit of System
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CHAPTER ONE

EXPLORING A COGNITIVE LANDSCAPE

The central core of this dissertation will consist of a critical examination of the scientific work of Jean Paul Marat. Marat is best known, of course, as one of the most important popular leaders of the French Revolution. His prerevolutionary career as an experimental physicist has frequently been noted by historians of science but has not been examined in detail.[1]

From the standpoint of traditional history of science, Marat's labors to unravel the mysteries of heat, light, and electricity are simply not worthy of attention. One of the most important American historians of science, for example, described his own approach to selecting a focus of study in these terms:

> The historical point of view that has been adopted throughout this monograph is that the true measure of any contribution to science . . . is the degree to which it can be proved to have exerted a beneficial influence on the progress of knowledge.[2]

By this criterion, Marat made no apparent contribution to science. His work, regardless of whatever intrinsic value it may or may not have had, did not lead anywhere. It was not picked up and developed by later generations of scientists. His books were read by some of his contemporaries, but after 1780 they were studiously ignored in the leading circles of French science and were almost completely forgotten after the Revolution. What, then,

could be the value of studying Marat's scientific endeavors? The rest of this chapter will address that problem.

The criterion of scientific contribution that was quoted above is valid within limits, and has guided the production of many important histories of science, not least of which was the study of Benjamin Franklin's science from which the quotation was taken. Nevertheless, it is no longer the only perspective from which the history of science is examined; the assumptions on which it is based have "been badly shaken by the criticisms put forward by Kuhn and other post-positivist philosophers of science."[3] New historiographical trends have emerged during the past two decades, utilizing broader conceptions of both history and science.

Historians have begun to pay more attention to the social context in which science has developed. Steven Shapin and Arnold Thackray have observed that "*science as people think of it and as they use it* is every bit as historically important as science as scientists conceive of it."[4] Roy Porter has suggested that the older "tunnel history" approach of "tracing the progress of modern scientific disciplines" should be replaced by efforts aimed at "exploring the cognitive landscapes of the past."[5] A great deal of work over the past twenty years or so has begun that process by exploring such diverse cognitive landscapes as those inhabited by Hobbesists,

Latitudinarians, continental Enlightenment radicals, transcendental anatomists, *Naturphilosophs*, and English Lamarckians.[6]

The quality and scope of the scholarship that has been produced by this historiographical revolution is impressive. It is all the more surprising, however, that much of the cognitive landscape of the greatest of social upheavals has remained uncharted.

The development of science during the French Revolution has traditionally been portrayed as a struggle won by partisans of modern science, symbolized by Antoine Lavoisier, over the forces of "Jacobin science," symbolized by Jean Paul Marat. Although Lavoisier suffered martyrdom, the story goes, his cause triumphed; the victory of his successors over pseudoscience was an essential step toward modern science.[7]

This interpretation is a product of the narrow "tunnel history" conception of science of which Porter complained. Tunnel history is the traditional practice that, in Jacob Burckhardt's classic formulation,

> considers such past forces and individuals praiseworthy on whose work our present existence and relative welfare are based. Just as if the world and its history had existed merely for our sakes! For everyone regards all times fulfilled in his own, and cannot see his own as one of many passing waves.[8]

Tunnel histories examine the scientific past through the narrow scope of the limited interests of modern science. The work of natural philosophers like Marat,

assumed to be off the path of direct development of present-day science, has for the most part been ignored as being of little value or interest. While tunnel history is of didactic value for science educators, its projection of present-day concerns onto the past is in general a source of anachronistic distortion.

For the most part, the focus of historians studying the development of science during the French Revolution has been on the accomplishments of such leading members of the Academy of Sciences as Lavoisier, Condorcet, Laplace, Fourcroy, Berthollet, and Monge, among others. Although this approach is certainly understandable, its potential distortions have been noted and criticized: "The focusing on successful pioneer work may lead to anachronistic historiography."[9]

In particular, the focus on these pioneers has led to the delineation of an "orthodox" science, with the consequence that alternative, unorthodox ways of seeking knowledge that were widely practiced in the Revolutionary era have been overlooked or dismissed as pseudoscience or quackery. A significant part of the cognitive landscape of the past has thus been left out of account.

Neither the orthodox nor the unorthodox scientists can be neatly categorized and defined into homogeneous groups. It is useful, however, to distinguish between them on the basis of a distinction made famous by an earlier leader of the Academy of Sciences, Jean d'Alembert, who contrasted

what he called the *esprit de système*, the spirit of system, with the *esprit systèmatique*, the systematic spirit.[10]

To d'Alembert the systematic spirit was the mark of a true scientist; it signified devotion to patient, rigorous, rational, empirical, analytical, quantitative methods of inquiry into the workings of nature. The spirit of system, however, was quite the opposite. It signified the impatient habit of leaping to conclusions on the basis of insufficient evidence. Those who were infected with the spirit of system were wont to create grandiose hypothetical explanations of everything in the universe. The practice of system building, as d'Alembert saw it, was not good science. The orthodox scientist, according to this view, is one who eschews speculation and does not deviate from the Newtonian ideal summed up as *hypotheses non fingo*.

The speculative habits of system building were by no means foreign to orthodox science in the eighteenth century. Newton himself, in his *Opticks*, had engaged in wide-ranging speculation as to the underlying causes of natural phenomena.[11] In France, few scientists had attained the prestige of the comte de Buffon, whose *Histoire naturelle* was a monumental example of a natural philosophical system that aimed at explaining everything.

Nonetheless, d'Alembert's point of view gained strength among the scientific elite in the years preceding the Great Revolution. In 1769 the abbé Nollet wrote to a collaborator who had asked his opinion of a speculative

paper: "I ought not hide from you that the Academy is getting more and more difficult about this way of philosophizing."[12] The defining characteristic of scientific orthodoxy in France became opposition to system building. Speculation in matters scientific became suspect.

Though strongly frowned upon in elite circles, however, system building flourished in the decade or two preceding the Revolution. The 1780s and early 90s were a golden age in France for speculative natural philosophy. Works inspired by the spirit of system proliferated. The authors and their readers perceived these works as a significant aspect of the scientific quest for knowledge about nature. If, as Shapin and Thackray stated, "science as people think of it" is as historically important as "science as scientists conceive of it," then these authors are deserving of attention.[13]

That is all the more indicated by the fact that many of these natural philosophers became prominent figures in French cultural and political history. At first glance, the authors in question seem to be a heterogeneous collection of social types indeed -- noblemen and bourgeois, journalists and scientists, doctors and lawyers, hack writers and literary lions, revolutionaries and counterrevolutionaries -- impossible to blend into a coherent focus of study. Yet all have one thing in common: each published volumes of natural philosophy in the 1780s or early 1790s that attracted contemporary attention. The

variegated group, in addition to Marat, includes Jean Louis Carra (a leading revolutionary journalist), Nicolas Bergasse (chief theoretician of the Mesmerist movement), Jacques Henri Bernardin de Saint-Pierre (Rousseau's literary heir), Pierre Samuel Du Pont de Nemours (statesman, economist, and industrial patriarch), Nicolas Edmé Restif de la Bretonne (a major figure in French literature), Jean Claude Delamétherie (moving spirit of the most important scientific journal before the Revolution), the comte de Lacépède (academician and Napoleonic politician), Jean Baptiste Lamarck (pioneer life scientist), and a pair of collaborators, the baron de Marivetz and Louis Jacques Goussier (known primarily for their immense work, *Physique du monde*).[14]

These natural philosophers shared the spirit of system, which consigned them to the ranks of the scientifically unorthodox; their voluminous works of natural philosophy have been forgotten and unread for two centuries. These neglected texts represent a cognitive landscape that has not been previously explored by historians of science. This dissertation will endeavor to begin mapping out that territory in two ways; first, by providing an overview pointing out the highlights of the entire landscape, and second, by examining one exemplary set of works in detail: Jean Paul Marat's investigations of heat, optics, and electricity.

The choice of Marat's work as the primary focus of this study was not based upon its being typical of or representative of that of the unorthodox natural philosophers as a class. Although Marat became politically more radical than any of the others, his spirit of system was perhaps the least radical of all. Because of his political notoriety, however, Marat has been the most maligned by historians as a charlatan, quack, or pseudoscientist. A critical examination of his work is necessary to evaluate the validity of those descriptions.

A major thesis of this dissertation is that Marat was neither charlatan, nor quack, nor pseudoscientist. In the context of the late eighteenth century, Marat's science was normal science. The central justification for this examination of Marat's physics, then, is to demonstrate that previous interpretations have been at least to some degree ahistorical.

The value of previous studies that focused solely upon orthodox scientists such as Lavoisier or Laplace is undeniable. This study of Marat's science is not intended as an effort to overthrow standard histories of French Revolutionary-era science, but to supplement them and enrich our understanding of the period. Recognizing the value of Marat's physics in its proper historical context can in no way diminish the appreciation of Lavoisier's contributions, or those of any other scientists.

Rehabilitating "Losers" of Famous Debates

The prerevolutionary tensions between orthodox and unorthodox scientists can be interpreted as a long-running debate pitting speculative natural philosophers against elite types whose watchword was *hypotheses non fingo*. That debate was rather abruptly terminated in favor of the latter by the outcome of the Revolution; Lavoisier's heirs did indeed triumph over Marat's. Social factors thus strongly influenced the course of the debate. The standard interpretation, however, assumes that the victory of the orthodox scientists over the system builders can be attributed entirely to the superiority of their scientific ideas and practice.

Thomas Kuhn's seminal study *The Structure of Scientific Revolutions* prompted looking at old debates in a new light: "Out-of-date theories are not in principle unscientific because they have been discarded."[15] Several recent works have taken specific scientific debates of the past as their starting points. Sepper's *Goethe Contra Newton*, Appel's *Cuvier-Geoffroy Debate*, Desmond's *Politics of Evolution*, and Shapin and Schaffer's *Leviathan and the Air Pump* all challenge traditional evaluations of important scientific debates of the past and serve, in different ways, as models for the present study. None of these revisionist accounts aimed at turning "losers" into "winners" or vice versa, but all show the science of the

"losers" to be far more interesting and worthwhile than has traditionally been acknowledged.

Goethe *versus* the Newtonians

Sepper reassessed the long historiographical tradition that dismissed Goethe's challenge to Newtonian optics as scientifically worthless and even foolish. Until recently, the verdict of historians of science has been virtually unanimous: in this conflict Newton was right and Goethe was wrong. Goethe, it is asserted, simply did not understand Newton's experiments and conclusions and stubbornly persisted in his wrongheadedness. This attitude toward Goethe and his massive treatise on color theory, *Zur Farbenlehre*, was stated most bluntly by C. C. Gillispie:

> It is impossible to read the *Farbenlehre* without an acute sense of embarrassment at the painful spectacle of the author, a great man, making a fool of himself.[16]

The idea of a thinker of Goethe's stature playing the fool is so anomalous that it signals a clear historical problem worthy of deeper attention. Sepper and a number of other scholars have recently investigated the dispute more thoroughly and have discovered that Goethe was not so foolish after all.[17] In fact, the root of the problem was quite the opposite: Goethe had not failed to understand Newton, but for almost two centuries Newton's defenders, Helmholtz included, had failed to understand Goethe.[18]

Sepper demonstrated that, while on a number of points Goethe was more scientifically sophisticated than Newton's

epigones, the debate between them does not lend itself to simple judgments that one side was right and the other wrong. He did not conclude, then, that Goethe was "right," but "that Goethe has an ampler conception of science than Newton."[19]

The rehabilitation of Goethe's *Farbenlehre* by Sepper and others has opened up new possibilities for appreciating Marat's scientific work in particular. Marat's optical experiments and anti-Newtonian ideas about color directly influenced Goethe, who favorably cited Marat's writings on the subject. As long as Goethe's color theory could be dismissed out of hand as the work of a fool, a fair appraisal of Marat as scientist was out of the question. Although Marat has traditionally been portrayed as an arrogant buffoon for having had the temerity to challenge Newton, could it be that he, too, had "an ampler conception of science than Newton"? This dissertation will not argue that he did, but it is important that the answer not be prejudged; it is at least a question worth asking. In general, recognizing this "ampler conception of science" as valid in its historical context -- not only for Goethe and Marat but for other natural philosophers as well -- offers in turn an "ampler conception of history."

Geoffroy *versus* Cuvier

Toby Appel's study of the famous debate between Georges Cuvier and Etienne Geoffroy Saint-Hilaire has

stressed the extrascientific factors that influenced its course and its outcome.[20] Cuvier's scientific outlook was strongly colored by his political conservatism and the social context of the Revolution and its aftermath. The social and political roots of his fierce opposition to speculative system building will be examined in more detail in Chapter Three.

Appel has sufficiently shown that the traditional portrayal of Cuvier as winner and Geoffroy as loser in their conflict is, once again, a distortion born of an ahistorical method, and is in need of serious revision. The close links between Geoffroy and one of the most important of the system-building natural philosophers of the Revolutionary era, Lamarck, makes Appel's monograph particularly relevant to the present study.

English Lamarckians *versus* antievolutionists

Lamarck's influence also figured strongly in a debate examined by Adrian Desmond. Once again, those associated with Lamarck's ideas -- pre-Darwinian evolutionists in nonelite English medical circles -- when not ignored altogether, have been traditionally designated as losers. Desmond's revisionist account identifies the Lamarckians as proponents of a "republican science" in Britain during the tumultuous Reform Bill era of the late 1820s and early 1830s:

> Geoffroy and Lamarck became symbols of resistance; they were the tricolor banners waved by the medical democrats massing outside the corporation porticos.[21]

Desmond's study again demonstrates the strong role of social factors that influenced the outcome of the debate and its traditional interpretation. His conclusion bears on the present study of unorthodox natural philosophers of a different time and place:

> If we are not to see science as a monolithic creation of the conservative elite, then we must get this dissident dimension back in the picture.[22]

Hobbes *versus* Boyle

Steven Shapin and Simon Schaffer have challenged the "standard historiographic strategy for dealing with the Hobbes-Boyle controversy" over the meaning of Boyle's air-pump experiments.[23] They have exposed the one-sidedness of accounts that previously dismissed Hobbes' natural philosophy as "a farrago of nonsense," "manifold absurdities," and a "contentious and splenetic outburst" against Boyle.[24] Despite

> the seriousness with which Hobbes's natural philosophical views were treated in the seventeenth century . . . by the end of the eighteenth century Hobbes had largely been written out of the history of science.[25]

While Boyle's side of the debate has traditionally been accorded an "aura of self-evidence," Shapin and Schaffer have shown Hobbes' objections to be "plausible, sensible, and rational."[26]

These authors note that historians' dismissal of Hobbes as natural philosopher resulted from his having lost the debate with Boyle in the eyes of his contemporaries:

> Within the tradition of "Whig" history, losing sides have little interest, and in no type of history has this tendency been more apparent than in classical history of science.[27]

It is necessary to extend the insights of these excellent studies to the period of the French Revolution by demonstrating that the neglected natural philosophers of that era were neither stupid nor charlatans. Their ideas, like Hobbes's, were often "plausible, sensible, and rational," and therefore deserve a more open-minded assessment than they have heretofore received.

A secondary thesis of the present study is that the subsequent course of French science was strongly shaped -- for better and for worse -- by the political outcome of the Revolution. It was post-Thermidorian social conservatism rather than purely scientific considerations that allowed the systematic minds to drive the spirit of system out of French science. This theme will be elaborated in Chapter Three. Before that, however, it is necessary to consider the traditional interpretation of the development of science during the French Revolution.

CHAPTER TWO

THE MYTH OF JACOBIN SCIENCE

The traditional account of how the French Revolution affected science formed the background of a discussion among historians that took place in 1957; their papers were published as part of the important collection entitled *Critical Problems in the History of Science*.[1] The discussion centered upon who was to blame for "Jacobin science." Henry Guerlac defended the position that Jean-Jacques Rousseau had inspired an out-of-the-mainstream current of scientific thought antagonistic to orthodox Newtonian science as represented by the Academy of Sciences.[2] C. C. Gillispie put forward an alternative thesis that placed the onus on Diderot rather than Rousseau.[3]

Both Guerlac and Gillispie took for granted the existence of a scientific current whose hostility to orthodox French science "was profound and utter."[4] A third participant in the debate, L. Pearce Williams, agreed that "the anti-Newtonian school existed during the Revolution," but suggested that the incoherent character of Jacobin ideology should "make one wary of equating Jacobinism with a large-scale anti-intellectual movement."[5]

The notion of Jacobin science implies a strong relationship between politics and science, or, more to the

point, between political radicalism and bad science. As Williams' argument indicates, however, a critical examination of what has been meant by Jacobin science leads to some paradoxical conclusions. Most scientists who were Jacobins were not "Jacobin scientists" thus conceived. And conversely, almost none of those who could be characterized as "Jacobin scientists" had any connection with Jacobinism.[6]

In other words, far from a correspondence between Jacobin politics and out-of-the-mainstream science, the evidence suggests no correlation or perhaps even an inverse correlation. With the notable exception of Marat, virtually all of the scientists who can reasonably be associated with the Jacobin movement in a political sense were *mainstream* scientists. The political left wing of French science included such prominent members of the Academy of Sciences as Antoine François Fourcroy, Gaspard Monge, and Louis Bernard Guyton de Morveau, as well as Jean Henri Hassenfratz and Pierre Auguste Adet. Furthermore, such pillars of academic orthodoxy as Condorcet and Bailly, while not Jacobins, were likewise high-profile revolutionary political leaders. The one exception, Marat, did indeed combine Jacobinism with a somewhat unorthodox approach to science. Others who can be categorized among the scientifically unorthodox, however, represented the full range of the political spectrum from non-Jacobin radicals to royalists.

"Jacobin science," then, in any meaningful sense, is a myth. Yet like all enduring myths, it is by no means devoid of historical significance or interest. While nothing that can fairly bear the name "Jacobin science" really existed, the label reflects the existence of a *concept* that has affected the development of French science and that deserves to be examined in more depth.

There were, indeed, out-of-the-mainstream currents of scientific thought, inspired by Rousseau and influenced by Diderot, that forcefully opposed the narrow orthodoxy defended by the leading lights of the Academy of Sciences. Though not Jacobin *per se*, they can be associated with social and political radicalism in a more generic sense. By falsely portraying the speculative natural philosophers as *enemies* of science, the myth of Jacobin science served as an ideological weapon that the elite academicians used against their antagonists. Succeeding generations of scientists and historians of science have generally viewed the dispute through the eyes of the elite and have thereby unwittingly perpetuated the myth.

The opponents of the mainstream scientists were not characterized by Jacobinism but by a fundamentally different approach to seeking knowledge. They were synthesizers rather than analyzers and theory spinners rather than fact gatherers. These bold hypothesizers and system builders were heroically stalking the answers to the

"big questions," disdaining the smaller pursuits that formed the orthodox ideal.

These French natural philosophers of the Revolutionary era -- or some of them, at least -- were the true heirs of Rousseau, Diderot, and Buffon in the realm of science. Although their respective systems embodied a broad spectrum of varied and conflicting notions, they tended to exhibit a number of shared characteristics. Among these was a romantic universalism and taste for "the individual, the dramatic, the paradoxical"[7] that led them to take up the challenge of deciphering nature's most salient anomalies, among which were light, gravity, fire, magnetism, electricity, chemical affinity, and life itself.

Many of them shared an organic conception of nature as a unified whole and of the fabric of reality as continuous rather than discrete: "Its metaphysics treats becoming rather than being. Its ontology lies in metamorphosis rather than atomism."[8]

The system builders generally rejected the reduction of natural phenomena to mathematical abstractions. And finally, most of them insisted upon the *moral* component of science that transcends purely physical questions and demands a true science of humanity with the goal of improving society.

A polarity is evident here that has often been portrayed as genuine science *versus* natural philosophy, but which is better understood as a clash between two different

kinds of natural philosophy. Although Newton's followers frequently claimed reliance upon fact and observation alone, they consciously or unconsciously, explicitly or implicitly, developed philosophies of nature based upon a full range of metaphysical assumptions. The orthodox scientists apotheosized the Newton of the *Principia*, with its mathematization of nature and its *hypotheses non fingo*, while the unorthodox system builders looked to the Newton of the *Opticks* and called attention to his frequent sallies into metaphysical speculation. "We are," a representative of the latter declared, "at least as Newtonian as those who call themselves disciples of that great man."[9] On the other side, the notion of a Jacobin science inspired by Rousseau is weakened by the fact that even the most orthodox scientists frequently paid homage to the genius of Jean-Jacques.

The remarkable surge of natural philosophical speculation that occurred in France in the decade prior to the Revolution and during the early years of the Revolution itself was evidenced, among other things, by a massive literary production of "systems of the world," not in the narrow Laplacean sense of technical astronomy, but in the earlier sense of works that promised integrated scientific explanations of all natural phenomena.[10] Although they certainly would not admit to being unconcerned with empirical facts, the system builders saw themselves as "idea" people rather than "fact" people. They believed

their ideas were based in fact but insisted on the right and duty of science to go beyond known facts and to speculate on possible and probable causes. As early as the end of 1781, the most important scientific journal, reviewing the first two volumes of Jean Louis Carra's *Nouveaux Principes de physique*, commented: "Never have so many systems, so many theories of the universe, appeared as during the last few years."[11] Ironically, that journal was soon to come under the editorship of Jean Claude Delamétherie, who would himself prove to be one of the most prolific system builders of the 1780s.

Examining the Myth

What is mythological about the notion of Jacobin science is not that a number of unorthodox scientists existed, but that they represented a coherent, unified current of thought that was opposed to the progress of knowledge. Henry Guerlac writes of "the persistent tradition that the spirit of the Revolution was detrimental, if not actually antagonistic, to science," and credits its origins to

> writers and propagandists of Napoleonic days, who left the impression that Bonaparte, almost singlehandedly, had saved French science, which the Jacobins had sought to destroy.[12]

While Napoleon's flacks certainly solidified the legend, it actually originated earlier, immediately following Robespierre's fall on 9 Thermidor. Roger Hahn attributes its creation to "post-Thermidorian legislators"

who "shared a desire to work for France's future, hoping that the turmoil of the Terror would never return to disturb it."[13]

Among the most influential of those who "helped to create and propagate the myth of Jacobin vandalism," he identifies three legislators: Boissy d'Anglas, Daurou, and Fourcroy. "Along with Grégoire, La Harpe, and dozens of other figures," these three "were quick to fix the blame for intellectual disorganization squarely on 'the tyrant Robespierre.'"[14] The charge of vandalism, of course, was meant to equate the Jacobins with the despoilers of the classical culture and learning of ancient Rome.

It is important not to lose sight of the social context. The legislators' fear of "the turmoil of the Terror" was not an aversion to turmoil in the abstract; it was opposition to a powerful struggle of the "lower classes" for social equality. The legislators' efforts for "France's future," then, implied their own counterstruggle for a future that did not include social equality.

It is more than a little ironic that Fourcroy, the mainstream scientist who had been a prominent Jacobin politician, emerged after Thermidor as one of the principal creators of the "myth of Jacobin vandalism."[15] Another irony is the leading role of the Abbé Grégoire, who on August 8, 1793, had proposed the decree passed by the Convention that abolished the Academy of Sciences.[16]

> But the most dramatic pronouncement on Jacobin vandalism was delivered by the journalist and critic La Harpe at the Lycée on December 31. . . . La Harpe's lecture was widely distributed and probably established the myth of Jacobin vandalism even more securely than Grégoire's scholarly and tedious speeches on that subject at the Convention. The rapidity with which the myth took hold and the near-uniform support given it by men of learning is an arresting fact . . . .[17]

Thus was created a cautionary tale, according to which the radical revolutionary leaders were implacably hostile to science, sharing Rousseau's view that the growth of scientific knowledge had not brought about an improvement in the human condition but had to the contrary caused modern society to become less virtuous and less happy. Then during the Revolution, the story goes, upon taking power the radicals acted on this premise by destroying the Academy of Sciences, driving its leader, Condorcet, to his death, executing the most important French scientist of the time, Lavoisier, and declaring that the Republic had no need of scientists.

Although Guerlac noted this "persistent tradition," he himself was one of the most important scholars of recent years to perpetuate it. He identified Bernardin de Saint-Pierre and the "charlatanesque" Marat as part of a group of Rousseau's followers

> who clarified his thought, and kept up a sustained drum-fire against the Academic science of which Newton was the symbol, against the Academy of Sciences itself, and against Lavoisier as its dominating figure.[18]

The inclusion of Marat and Bernardin -- considered scientists by many of their contemporaries -- among enemies

of science shows how "Jacobin science" became a corollary of Jacobin vandalism.

Elsewhere Guerlac has presented some of the elements of this story in a different light. Of Lavoisier, Condorcet, Bailly, and other scientists who perished, Guerlac writes: "It was as politicians, financiers, and public officials that they were executed, not as men of science." Furthermore,

> at no point can we simply affirm, as did the men who wished to blacken still further the men of the Revolution, that the Revolution felt it had no need of men of science.[19]

The leading modern historian of the Academy of Sciences, Roger Hahn, firmly denies that

> learned societies were destroyed as part of a program of intellectual vandalism in which both the Jacobins and the lower classes of French society reveled. . . . All the earlier critics of the Academy, including Bernardin de Saint-Pierre and Marat, were careful to limit their accusations to the personal or communal habits of men who practiced science. Individual scientists might be accused of being dishonest or avaricious and institutions despotic or obsolete, but knowledge itself was hardly ever maligned.[20]

Despite the evidence, however, the power of the legend to shape attitudes toward historical events has hardly diminished at all. Something called "Jacobin science" is still widely assumed to have represented a hostile challenge to the development of modern science. In the *Critical Problems* debate mentioned earlier, for example, the focus of disagreement was not upon the existence of such an antiscience radicalism, but upon where to assign responsibility for it.[21]

The distortions born of the myth of Jacobin science are a subset of the more general problems encountered in trying to come to grips with any aspect of the French Revolution (or, indeed, any social revolution that retains the power to stir human passions). Many people in the English-speaking world -- historians included -- have been conditioned to react emotionally to the French Revolution as an episode of gratuitous violence, a historical event to be deplored rather than celebrated. A free-association test with "French Revolution" or "Jacobin" as stimuli will more often than not produce "guillotine," "terror," or "violence" as primary responses -- even among people who on the conscious level consider themselves partisans of the Revolution. This is the legacy not so much of Marat and Robespierre as of Edmund Burke and Thomas Carlyle.[22] The internalized aversion toward the French Revolution makes an interesting contrast to emotional responses evoked by the American Civil War, an extremely violent affair that most Americans have nevertheless been conditioned to respond to in positive terms: as that which ended slavery, unified the nation, and so forth.

If the French Revolution was "the great turning point of modern civilization," the Jacobins and other radicals played an indispensable role in shaping those events.[23] In the postrevolutionary period, however, the word "Jacobin" acquired negative connotations, as did the word "leveller" after the English Civil Wars or the word "communist" after

the Russian Revolution. The attachment of this stigma to unorthodox science helped to anathematize it during and after the Thermidorian reaction, thus affecting the development of French science as well as the perceptions of later generations. It is necessary to disentangle the subject from these socially loaded associations.

In spite of the myth of Jacobin science, the traditional assessment of the Revolution's overall impact on science has been generally positive; *i.e.*, that the great social transformation was, in the final analysis, beneficial to science. This historiographical line was solidly established by 1803, as evidenced by the physicist Jean Baptiste Biot's *Essai sur l'histoire générale des sciences pendant la Révolution française*.

The destruction of the royal guilds, corporations, and monopolies (including the much lamented Academy of Sciences) did, after all, succeed in "opening careers to talent," allowing the sciences to draw their human resources from a considerably larger social pool than was possible under the old regime. Particularly notable in this regard was the breaking of the old academic stranglehold over the medical sciences. Elimination of the caste system that had separated physicians, surgeons, and pharmacists opened the way for a new kind of medical profession.[24]

The establishment of formal equal access to careers in science was important, but the Revolutionary governments went further by creating educational institutions to

guarantee the state an expanding supply of scientific talent, a commitment that was reinforced and expanded by the Napoleonic regime. Another frequently cited aspect of scientific progress brought about, or at least accelerated, by the Revolution, was the great leap forward in specialization in the sciences; "the age of the generalist was over in the eighteenth century."[25]

While a number of ways in which the Revolution affected science have thus been recognized, historians have perhaps omitted a significant part of the story by ignoring aspects of unorthodox natural philosophy that were possibly beneficial to science. The strong tendency toward specialization, for example, was directly related to the defeat of those natural philosophers who stood for generalism in opposition to divisions along disciplinary lines. From their perspective, which was not at all unreasonable, specialization was a mixed blessing: its benefits had to be weighed against the losses that resulted from shattering knowledge into "fields" sealed off from each other.

The Scientific Underground of the Old Regime

The authors of universal systems of nature, who saw themselves as latter-day Aristotles and Descartes, constituted a scientific underground of the old regime analogous to the literary underground described by Robert Darnton.[26] In fact, there was an overlap between the two

groups, since some of Darnton's Grub Street writers were also natural philosophers. Furthermore, many of the "Grub Street scientists," like their literary counterparts, rose to prominence as revolutionaries (although rarely Jacobins) during the Revolution. Since the orthodox science they combatted was thoroughly integrated into the institutions of the state, their antagonism toward it proved to have revolutionary implications.

This phenomenon presents an interesting historical problem. While many of the most prolific natural philosophers of the Revolutionary period are "historical figures"; that is, still remembered as important for one or another of their accomplishments, their writings on natural philosophy have been almost completely neglected. And yet these forgotten savants generally expected that their systems of the world would be the most enduring of their gifts to posterity. Their misperception in this regard provides a measure of the distance separating eighteenth-century *mentalités* from our own. A reexamination of their neglected texts -- based upon eighteenth- rather than twentieth-century criteria of what is scientifically important -- would contribute to a fuller understanding of the development of scientific thinking in this critical period. As Daniel Mornet observed in his classic *Les Origines intellectuelles de la Révolution française*, the ideas of "second-, third- or tenth-rate writers" are worthy

of study "since those who seem tenth-rate to us may have been first-rate to their contemporaries."[27]

The neglect of the system builders' collective efforts to understand nature has in some measure impoverished the history of science. The operative word is "collective"; Restif de la Bretonne's *Physique*, for example, is not likely to ever be hailed as a landmark of science.[28] It is not the specific content of individual works that was significant, but the creative spirit of speculative thought that they represented. Although Restif's *Physique* represents an extreme example of the genre, it is an integral part of the ensemble and to omit it would skew the picture.

Creative speculation was the essential contribution of natural philosophy to the development of science, and Restif's fecund and restless imagination made him one of the most creative of system builders. Given his prodigious production of hypotheses and speculations it is not surprising that much of it has been dismissed as worthless by some modern observers. Nonetheless, it is precisely those of Restif's notions that today appear most bizarre that merit the historian's closest attention, because they most clearly signal a radically different way of perceiving the world. Here the methods of anthropology might well be advocated for historical research as a way to "make contact with the otherness of other cultures":

> When we run into something that seems unthinkable to us, we may have hit upon a valid point of entry into an alien mentality.[29]

Among the more promising is Restif's claim that the heavenly bodies reproduce sexually. As absurd as this may seem today, it is but a short step from the notion, widely held among Restif's contemporaries, of the heavenly bodies as a species of animal. Du Pont de Nemours and Delamétherie, for example, are usually treated as more sober and respectable thinkers than Restif, yet the former argued in favor of planets as living organisms and the latter acknowledged it as a strong possibility.[30] To dismiss this notion out of hand as fantastic is to block the possibility of understanding how nature was perceived in late-eighteenth-century France.

The concept of living planets, of course, has ancient roots. Plato's *Timaeus* taught that "the world became a living creature truly endowed with soul and intelligence by the providence of God." The creator gave "the form of a globe" to "the animal which was to comprehend all animals."[31] This idea was central to the macrocosm-microcosm conception of a human being's relationship to the cosmos that dominated natural philosophy for centuries. By way of Paracelsus it entered into the thought of nonelite social milieux during the English and French Revolutions.[32]

Although the historical value of an idea need not be justified by invoking modern echoes, it is nonetheless interesting to consider this one in light of the "Gaia

hypothesis" now being advanced by ecologists who contend that the Earth should be understood as a living organism.[33] Apropos of Restif's notion, the medical scholar and popularizer Lewis Thomas commented:

> The one biological function the Earth does not yet perform to qualify for the formal definition of an organism is reproduction. But wait around, and keep an eye on it.[34]

Epistemological Value Systems

As for the speculative natural philosophers listed in Chapter One, the assessment of the value of their collective contribution to the development of scientific knowledge depends first of all upon the epistemological outlook of the assessor. On the positive side, the case in favor of their kind of natural philosophy as a creative element complementing the Newtonian kind was concisely stated by the Danish physicist and *Naturphilosoph* Hans Christian Oersted:

> One class of natural philosophers have always a tendency to combine the phenomena and to discover their analogies; another class, on the contrary, employ all their efforts in showing the disparities of things. Both tendencies are necessary for the perfection of science, the one for its progress, the other for its correctness. The philosophers of the first of these classes are guided by the sense of unity throughout nature; the philosophers of the second have their minds more directed towards the certainty of our knowledge. The one are absorbed in search of principles, and neglect often the peculiarities, and not seldom the strictness of demonstrations; the other considers the science only as the investigation of facts, but in their laudable zeal they often lose sight of the harmony of the whole, which is the character of truth. . . . This conflict of opinions keeps science alive, and promotes it by an oscillatory progress.[35]

In this Hegelian interpretation, science is a dialectical unity of the creative spirit of system building and a healthy skepticism, a dynamic balance of centifugal and centripetal forces, or of analyzing and synthesizing tendencies.

Traditional history of science, however, has adopted the epistemological value system of modern science, which has not been conducive to a positive appreciation of speculative natural philosophy. When not ignored as being of little value or interest it has been portrayed in negative terms. The views of two prominent modern observers are particularly relevant: those of the philosopher of science Gaston Bachelard and those of the historian of science C. C. Gillispie.

Bachelard has accorded the system builders serious attention and respect but has insisted that the significance of their writings is as poetry rather than science; as a source of psychoanalytic insights rather than as material for the historian of science. In his *Psychanalyse du feu*, for example, he characterizes them as "muddled thinkers" and examples of their work as "nonsense," "stupid observations," and "ridiculous statements." Marat is identified as a "prescientific mind of little intellectual power"; Carra, Bernardin de Saint-Pierre, and Lacépède are likewise cited as examples of the "nonscientific" or "prescientific" mind.[36]

Bachelard also interprets the relationship of natural philosophy to science in Hegelian terms, but arrives at a radically different conclusion. To Bachelard, science is not a unity of opposites *including* natural philosophy; science is the *dialectical negation* of natural philosophy. It should be noted that although Bachelard would thus portray natural philosophy as "pre-science" or "antiscience," by doing so he at least assigns it central importance as the soil from which modern science has grown.[37]

Gillispie's extensive studies of science at the time of the French Revolution have focused on the mainstream scientists. He mentions in passing "that eighteenth-century literature that took all nature for a subject and explained its course by the operation of some universal principle or substance of great subtlety," but dismisses it as of little value to the history of science.[38]

Gillispie's epistemological outlook is indicated by the title of one of his best known books: *The Edge of Objectivity*. His interpretation is based on a set of value judgments as to what kinds of knowledge are important and what kinds are not. Gillispie's portrayal of scientific progress as an "advancing edge of objectivity" implies a limitation of science to knowledge about those things that are external to and thereby independent of human observers (or, more abstractly but more precisely, independent of "mind").

Gillispie's epistemological value system aligns him with the mainstream scientists and puts him out of tune with the system builders, who would have considered his view of science far too narrow and restrictive. For although Gillispie's notion of what is objective seems broad enough to encompass the entire universe, the natural philosophers would maintain with Rousseau that it excludes precisely that which is most important to human beings. What is a man profited, they would ask, if he shall gain knowledge of the whole "objective" world yet understand nothing of his own moral, mental, political, and social conditions?

Since Gillispie looks at the world in very much the same way as the mainstream scientists did, it is natural that he would tend to survey French science in the Revolutionary era through their eyes. "The romantic view of nature," he declares, "is the wrong view for science."[39] It is remarkable, then, and a tribute to the quality of his scholarship, that he has examined the work of some of the unorthodox natural philosophers more thoroughly and accurately (one is tempted to say "objectively") than any other historian of science to date. His treatments of Marat, Lamarck, and the Mesmerists are especially noteworthy.[40] His otherwise perceptive examinations of Marat's and Mesmer's work seem somewhat diminished, however, by his having relegated them to a chapter on quacks and charlatans.[41]

In any event, the present examination of the worth of the natural philosophers owes a great deal to Gillispie's writings, both as an invaluable source of historical information and interpretation regarding French science at the time of the Revolution, and as a prime exemplar of the traditional interpretation that is being challenged here. Gillispie is particularly quotable because his work has the great merit of forthrightness; never one to shroud his assertions in vagueness, Gillispie always comes straight to the point.[42]

To Gillispie and Bachelard, the decline of speculative natural philosophy and the consignment of its productions to historical oblivion are a natural consequence of the "advancing edge of objectivity": Lavoisier, Laplace, and the other progressive standard-bearers of modern science justifiably triumphed over conservative natural philosophers mired in an old, erroneous way of thinking. "The attempt to revive the Stoics' nature," Gillispie writes, "was, of course, bound to fail. Nature is not like that."[43] But if some of the system builders were reviving the continuity theories of Stoic physics, the orthodox Newtonians were reviving Epicurean atomism, and nature is not like that, either.[44]

The polarity in the late eighteenth century was not between modern science on the one hand and archaic natural philosophy on the other, but between rival approaches to seeking knowledge about nature. The notion that the "good

science" of the orthodox elite won the day simply because it was true implies that the struggle took place in the rarified realm of ideas and leaves the decisive role of social factors out of account.

Science and Morals

Before the Revolution, most speculative natural philosophers had no doubts or hesitations with regard to the social uses of science. Influenced by Rousseau, their science aimed above all else at bringing about social change to improve the human condition.

Gillispie has emphasized that this was not only the program of Rousseau, but also of the premier *Encyclopédiste,* Diderot. Gillispie thus assigns partial responsibility to Diderot for "the romantic tradition" that refused to accept a science "empty of morals and lessons."[45] This, to Gillispie, definitively places it outside science: moral considerations are subjective, therefore not objective, therefore not scientific.

If the history of science were a logical progression of pure ideas, Gillispie's point would be well taken. Since science is a social activity, however, and scientists are social beings with social interests, no science has ever been "empty of morals and lessons." The case of Cuvier, which will be discussed below, illustrates this contention particularly well. Although the orthodox savants no doubt sincerely believed that their science was value-free, their

views were no less socially mediated than were those of Rousseau's and Diderot's followers. In this regard, those elite scientists are akin to those more recent ideologists who proclaim "the end of ideology," or to those who believe that only people who pronounce words differently from themselves speak with an accent.[46]

That science should include the consideration of moral and social questions is undeniably an ideological proposition. But so, too, is the converse assertion that science should be morally and socially neutral. Socially neutral science is a corollary of the laissez-faire ideology that so perfectly suited the bourgeois social order of legal equality and social inequality after Thermidor.

Rousseau's and Diderot's desire to introduce subjective moral concerns into science is attributed by Gillispie to "the aspirations of humanity to participate morally and through consciousness in the cosmic process."[47] This soaring abstraction leaves the social context of the *ancien régime* out of account. If the motivations of a Marat, Carra, or Bergasse -- not to mention a Condorcet, Fourcroy, or Bailly -- are to be understood at all, it is necessary to consider the context of the social crisis that engulfed prerevolutionary France. Their view of nature and their desire (as Karl Marx would later put it) to not simply interpret the world but to change it, derived not from abstract cosmic *angst* but from hatred and loathing of

the very concrete, day-to-day brutality, corruption, injustice, and hypocrisy of the society they perceived all around them.

Gillispie has put his finger on the element in the worldview of the system builders that most fundamentally connected it with radical social thought. The romantic natural philosophers went wrong, he says, because they were concerned not with what *is*, but with what *ought to be*. He blames their focus on "metamorphosis" and "becoming rather than being" for leading them away from real science.[48]

The system builders, in other words, were more interested in nature's changeability than in nature as it exists at any given moment. They agreed with Heraclitus that all things are in flux. The orthodox approach, by contrast, treated nature as essentially static, or dynamic only in the mechanical sense wherein all change is reduced to displacement; that is, to the motion of material particles through empty space.

The ideological utility of this latter outlook for defenders of the social *status quo* is obvious. To make "what *is*" the only legitimate focus of science implies that no reality other than the presently existing reality is possible. From this starting point a Cuvier or a Bonaparte could easily demonstrate that revolutionary social aspirations are foolish, irrational, and futile.

Limiting science to "being" and excluding "becoming" not only excludes the possibility of a meaningful science

of society, however; it also blocks the progress of the natural sciences. To take only the most salient example, ruling out "becoming" is to rule out evolution; the case of Lamarck will be discussed in Chapter Six below. Meanwhile, Gillispie's dictum that "being" is the proper concern of science while "becoming" is the province of pseudoscience must be rejected.

Feminist and Environmentalist Critiques

The challenges described in Chapter One to the complacent assumptions of traditional history of science reflect a growing recognition that the dominance of positivistic tendencies within modern science is problematic. It can no longer be taken for granted that the only real science was that which was associated with the orthodox tradition.

This recognition did not originate in the realm of pure ideas but in a changing social reality during the 1960s and 1970s. Feminist scholars could not but balk, for example, at Gillispie's attribution of inferior science to "a feminine dislike of precision."[49] Men and women alike can now appreciate that whether one likes or dislikes precision is beside the point; the issue is whether one is willing to restrict scientific inquiry to only those problems amenable to quantification.

Philosopher Sandra Harding, examining the role of gender in science, pointed to the mythological character

and social roots of traditional history of science. "All of us grew up on a well-known story about the birth of modern science," she writes.

> The story is elaborated in mainstream histories and philosophies of science and in scientists' accounts of their lives . . . . The story stresses that the scientific revolution's new method of inquiry would prevent the projection of political interests and values onto the natural order. Modern science, unlike medieval inquiry, seeks knowledge that is free of moral, political, and social values.[50]

We should regard this story, she says, as we do the *Iliad*, the *Odyssey*, the book of Genesis, and fourth-grade histories of the American Revolution:

> These are all official and elaborated versions of much older origins stories whose social meanings had already deeply permeated the self-understandings of the people who constructed and listened to them.[51]

The insistence "that science is analytically separate from social life" is based on "the assumption that physics should be the paradigm of scientific knowledge-seeking." Despite good historical reasons why physics attained this centrality,

> its paradigmatic status today should be regarded as anachronistic, and as a reflection of distinctively androcentric, bourgeois, and Western concerns.

Rather than physics, which is "atypical of scientific knowledge-seeking," Harding proposes instead that "a critical and self-reflective social science should be the model for all science."[52] *Self-reflection*, of course, is precisely the quality -- characteristic of Rousseauian natural philosophy -- that Gillispie's conception of objectivity would rule out of science.

As important as the feminist critique has been, the rapid rise of the perception of global environmental crisis has perhaps been even more influential. Rachel Carson's exposé of the dark side of chemistry's contribution to agriculture touched off an explosive growth of uneasiness over the goals and methods of modern science.[53] The implicit promise of science to conquer and control nature for the benefit of humankind had not been fulfilled, and was perhaps unfulfillable in principle. Every intervention into the processes of nature, it seemed, produced toxic side effects.

New attention was directed toward the concept of ecology and the idea of nature as a delicately balanced, interactive system. The ecological reorientation quickly exposed the limitations of the analytic approach to science. The method of reducing problems to simplest elements and studying them in isolation could no longer credibly claim to be the only path to knowledge of nature.

This revelation has prompted a fresh look at those tendencies of scientific thought in the past that perceived nature as a unified whole rather than as a collection of discrete entities (the Stoics as opposed to the Epicureans) or as organism rather than machine (the Paracelsians as opposed to the mechanical philosophers). The French natural philosophers of the Revolutionary era are among those who can be better appreciated in this context. Bernardin de Saint-Pierre's *Etudes de la nature*, in particular, has

already begun to attract the notice of predecessor-hunters searching for "the first glimmerings of the ecological point of view."[54] As early as 1930 Louis Roule called attention to Bernardin's insistence that the focus of natural history should not be upon individual organisms but upon the relationships connecting them.[55] By making a cliché of the phrase "living planet," the new environmental consciousness has weakened the automatic tendency to dismiss Du Pont's and Restif's natural philosophy as weird and fantastic.

After Thermidor, however, the collective reputation of the French system builders was tarred with the brush of Jacobin science. They suffered the traditional fate of losers whose history is written by victors. Their work was misrepresented and its historical legitimacy wrongly denied. As previously noted, its productions have been frequently characterized as nonsense, pseudoscience, or antiscience, and its practitioners -- when not totally ignored -- as quacks, charlatans, and even psychopaths.

Appreciating the unorthodox science of the system builders, then, requires transcending the limitations of traditional history of science. It is necessary, first of all, to illuminate the social factors that tipped the scales against them. While the proscription against system building was strong in France in the years leading up to the Revolution, it became virtually absolute after the fall of the Jacobin Republic on 9 Thermidor, Year Three. The

reputation of the unorthodox natural philosophers thus fell victim to the social circumstances created by the Revolution.

CHAPTER THREE

THERMIDOR AND THE DOWNFALL OF THE SPIRIT OF SYSTEM

The interpretation of the seventeenth-century scientific revolution has been enriched by considering it in the social context of the "world turned upside down"; that is, the English revolutions of 1640-1660 and 1688.[1] The insights thus gained can also be profitably applied to the study of how science developed in the French Revolution. In both the English and French cases, the tides of social conservatism that followed the revolutions pushed science in a conservative direction as well by favoring reductionist, empiricist, positivist and mechanical-materialist tendencies.[2]

James Jacob has concluded, on the basis of his investigation into the social relations of science in seventeenth-century England, that "modern science developed in part in response to popular disorder and elite perceptions of the threat from below."[3] Jacob and others have argued persuasively that the Newtonian version of mechanical philosophy became the dominant scientific paradigm in England in the late seventeenth century not primarily as a result of its truth but because it was an integral element of the ideology of the social class that came out on top in the long revolutionary struggle that

erupted in the 1640s and culminated in the Glorious Revolution of 1688.[4]

A "vogue of Paracelsus" swept England during those times of troubles.[5] Those radical revolutionaries who sought a levelling of social ranks were attracted to certain antihierarchical implications of the organic worldview. Vitalist notions of nature wherein all creatures democratically share in the life-spirit suggested the fundamental equality and kinship of all humans and posed a challenge to traditional defenses of social inequality.

The moderate Latitudinarians, desiring social and political stability above all, sought an ideological response to this challenge from below and found it in Newtonianism. This new scientific outlook provided an unemotional rationalism that aspired to explain all phenomena as resulting from impersonal, lawful interactions of lifeless particles.

By thus "sucking spirit out of nature"[6] the moderates hoped to counter the socially disruptive enthusiasm of the ranters, the seekers, the diggers, the true levellers and the other radical sects. To the popular passions they opposed logic and reason; to knowledge born of inspiration they opposed knowledge produced by patient and disciplined experimentation; to knowledge accessible to all individuals they opposed knowledge controlled by a scientific elite. The Newtonian worldview, then, with its strictures against

speculation, rose to dominance in scientific thought on the coat-tails of a new ruling elite after a revolution.

The total victory of Lavoisier's heirs in French science after the Revolution can likewise be attributed in large part to the social usefulness of their ideology as a set of legitimating ideas supporting Thermidorian and Napoleonic society. In both England and France, the leading spokesmen for orthodox science after their respective revolutions were above all concerned to separate science from political, social and moral issues. The *hypotheses non fingo* program, by discouraging speculative thinking, by restricting the scope of scientific investigation, and by abstracting phenomena from their context and studying them in isolation, was ideally suited for a dispassionate science intent on minding its own business and not threatening the social *status quo*. In France again the course of modern science continued to develop "in part in response to popular disorder and elite perceptions of the threat from below."[7]

Radicals and Conservatives in Politics and Science

Margaret Jacob has examined the relationship between scientific unorthodoxy and radical politics throughout the seventeenth and eighteenth centuries. "It would be mistaken," she cautions,

> to imagine that a continuous historical chain, yet to be discovered, exists between Bruno, through the seventeenth-century Rosicrucians, to the radicals of the early Enlightenment . . . . Rather, there exists

> an intellectual tradition that bore fruit at various critical times in European history.

Jacob describes this tradition, a variant of the organic worldview, as "pantheistic materialism" and identifies it as "the philosophical link that runs through European radicalism from the English Revolution to the French Revolution."[8]

While a number of scholars have investigated this relationship with regard to the seventeenth century and Jacob herself has pursued it through the first half of the eighteenth, it is remarkable, given the amount of historical attention devoted to the French Revolution and its preconditions, that no attempt to examine its possible connections with "Jacobin science" has previously been made.

Robert Darnton's *Mesmerism,* by focusing upon the most prominent variant of prerevolutionary natural philosophy, constituted a major step toward such a project. Darnton examined the connection between Mesmerism and the "anti-establishment kind of radicalism" of such figures as Marat, Carra, Brissot and Bergasse: "These outcast philosophers carried new truths in their breasts, truths that threatened to disrupt the social order."[9] Darnton argues that

> no one has ever taken mesmerism and the other forms of popular science seriously -- virtually no one, that is, since the French of the 1780s. They looked out on a world so different from our own that we can hardly perceive it; for our view is blocked by our own cosmologies, assimilated, knowingly or not, from the scientists and philosophers of the nineteenth and twentieth centuries.[10]

The "vogue of Paracelsus" that accompanied the earlier English Civil Wars seems to have resurfaced in new guise just before the French Revolution:

> Of the many systems for bringing the world into focus, mesmerism had most in common with the vitalistic theories that had multiplied since the time of Paracelsus.[11]

Darnton contends, then, that "an investigation of that remote mental universe should improve the understanding of prerevolutionary radicalism." The connection is most evident in the human material. "One should not be surprised," he writes,

> to find that Mesmer's partisans included several important future leaders of the Revolution -- Lafayette, Adrien Duport, Jacques-Pierre Brissot, Jean-Louis Carra, Nicolas Bergasse, the Rolands, and Duval d'Eprémesnil.[12]

The particular case of Nicolas Bergasse, who most thoroughly exemplifies the link between organizing for the Revolution and Mesmerist natural philosophy, will be discussed further in the Chapter Four.

Pseudoscience, Mysticism and Irrationalism

The present study is greatly indebted to Darnton's examination of the social roots of Mesmerism and will attempt to extend its scope to include the full range of unorthodox science of the Revolutionary era. A particular difference in approach must be mentioned, however. Although Darnton takes the natural philosophers seriously as historical actors, he nonetheless relegates the writings of Carra, Bergasse, Restif de la Bretonne and Marat to "the

pseudoscientific trends of the 1780s" and refers to "Mesmer's innocent quackery."[13] His references to mystical, occultist or spiritualist themes in the works of Bergasse, Restif, and Du Pont convey the impression that their natural philosophy was essentially aimed at shrouding nature in mystery, which is misleading. While no precise line of demarcation separating speculative system building from mysticism exists, there is nonetheless an important distinction to be made. An occultist, by definition, explicitly rejects the possibility of scientific knowledge. Whatever their ambiguities, and however peculiar their notions may seem to us, the natural philosophers in question were striving mightily to penetrate nature's secrets, not to deny their knowability.

In the case of Bergasse, Darnton implicitly acknowledges this distinction by recognizing that after the collapse of his revolutionary hopes "he *retreated* into the realm of the spirit."[14] That there had been an element of spiritualism in his Mesmerism all along is not to be denied; the point is that it became dominant only with his political transformation from revolutionary to reactionary.

It is likely that some confusion is due to the apparent incongruity of the "pantheistic materialism" of some of the natural philosophers. Twentieth-century minds are conditioned to think of pantheism, vitalism, and animism as absolutely inconsistent with materialism, thus classifying them by default with spiritualism or mysticism.

Bachelard alluded to the strangeness of this hybrid when he described it as "a special form of materialism for which a word would have to be created," which is "intermediate between materialism and animism."[15]

If this pantheistic materialism is recognized as a legitimate scientific worldview in the context of late-eighteenth-century thought, then clearly the "pseudoscience" label is inappropriate for much of the unorthodox science of that period. Furthermore, controversies such as those over whether Lamarck and Cabanis were materialists or vitalists can be superseded by understanding that these categories were not mutually exclusive.[16]

Another notion that is jarring to twentieth-century *mentalités* is that irrationalism could have any valid place in a scientific outlook. Again, an important distinction must be made: a total rejection of reason would of course be antiscientific, but the romantic reaction to the reason-worship of the Enlightenment generated plausible criticisms of scientific methodology. From Rousseau to Diderot to Goethe, the theme was not that reason is useless in comprehending nature, but that nature cannot be known by reason alone. Nature, they argued, is far too grand to be subordinated to fallible, limited human reason. Nature exists only as an interconnected whole that can never be fully known by analytical methods that consider the parts in isolation. Nature is real, not abstract, and cannot be

reduced to the logical formalisms of mathematics. In light of the analytical and mathematical path taken by modern science, tunnel historians may fault the romantic critics in retrospect as shortsighted, but the legitimacy of their fundamental arguments must be acknowledged.

Preromantic natural philosophies, then, were complex worldviews not easily pigeonholed according to twentieth-century categories. Labels such as occultist, animist, or irrationalist can be useful only if applied with many qualifications.

Radicalism and Science in Prerevolutionary France

The persistence of the myth of Jacobin science owes a great deal to the fact -- so persuasively demonstrated by Darnton -- that many of the speculative natural philosophers were indeed prominent participants in the Revolution in its early stages, although few continued to support its most radical phase of Jacobin rule: most, that is, were "89ers" but not "93ers."[17] That few were Jacobins does not argue against a link between radical politics and natural philosophy; it simply reflects an unawareness before the Revolution of the full social implications of their outlook. As Tawney observed, "So little do those who shoot the arrows of the spirit know where they will light."[18]

A connection between revolutionary politics, thus broadly defined, and the speculative scientific spirit did

exist and was familiar to contemporary observers. Condorcet, the primary spokesman for organized science in the prerevolutionary period, considered the relationship between social conservatism and scientific conservatism to be a necessary one. In his 1782 eulogy of d'Alembert he asserted that

> the philosophers who, with regard to speculative opinions, retreat into a position of almost absolute doubt, *as a necessary consequence* have very moderate opinions in the realm of practical matters.[19]

Conversely, those who were most inclined toward speculation in science were also apt to be those most willing to consider radically new political ideas. Condorcet criticized the conservatives for utilizing a sort of agnosticism -- the claim that moral and social phenomena are in principle unknowable -- as an ideological excuse for taking no political action, thereby supporting the social *status quo* by default.

Although Condorcet has traditionally been placed solidly among the "rationalists" as against the "romantics," his position illustrates a certain lack of precision in these categories.[20] The notion that science should eschew consideration of social problems was entirely foreign to Condorcet. The manifesto of the Society of 1789, drafted by Condorcet, declares:

> There should exist for all societies a science of maintaining and extending their happiness . . . . This science, to which all others are contributors, has not been treated as a whole.[21]

Condorcet, however, believed that a science of society could be modeled on the physical sciences, a reductionist conception developed further in the nineteenth century by the *Idéologue* current and taken to its extreme in the positivism of Auguste Comte. Nevertheless, Condorcet's eulogy of d'Alembert can be read as a partial critique of the positivist spirit and a defense of the value of speculative hypothesizing in science:

> M. d'Alembert, long engaged in the mathematical sciences, developed the habit of being impressed by only those truths that are susceptible of rigorous proof. Certainty, to him, was extended by the cumulation of simple ideas derived from pure geometry and rational mechanics.[22]

While d'Alembert, he continued, was justified in distrusting grandiose speculative systems based on questionable insights and vague reasoning, by rejecting them out of hand

> he took his rigorousness too far, because although those hypotheses, those bold insights, those interpretations do not form a true science, they do serve to expand the value of experiments and observations by revealing them in all of their various aspects. They guide us in our researches, they pave the way for our discoveries, and they give us a glimpse of the glorious dawn that the centuries to come can expect to enjoy.[23]

Condorcet also explicitly challenged the moral and political narrowness of the reductionist outlook:

> M. d'Alembert reduced to a small number of general truths, or first principles, the little that we can know with certainty regarding metaphysics, morality and political science. Perhaps he set overly narrow limits on the human mind. Perhaps, accustomed to truths demonstrated and formed from simple and precisely determined ideas, he could not appreciate

> another category of truths that involved more complicated ideas. . . .
>
> The problem with M. d'Alembert's point of view is that it overly restricts the field upon which the human mind can exert itself. . . . It ends up by surrendering to doubt, uncertainty, and consequently to vague and arbitrary principles, *those questions that are important to the wellbeing of the human race.*[24]

It is apparent, then, that Condorcet, the official representative of orthodox French science from 1776 to 1793, to some degree shared certain crucial aspects of some of the speculative natural philosophers' worldview.[25] Although his organizational role within institutionalized science was essentially a conservative one, he was not a social conservative. When the Revolution erupted he, too, played an important part as a political activist and revolutionary of the 89er variety.

Beginning as a prime mover of the Society of Thirty and the moderate Society of 1789, Condorcet became radicalized in the course of struggle. The king's abortive flight to Varennes led him to break with moderates like Lafayette and Du Pont de Nemours and turn toward radicals like Brissot and Thomas Paine. He was a leading agitator for the radical Cercle Social, which brought him into political collaboration with Nicolas Bonneville (a devotee of Restif's natural philosophy) and Jean Louis Carra.[26] As the social character of the Revolution continued to deepen, however, and brought the plebian masses to the fore, Condorcet -- together with the *Brissotin* current in

general -- redirected his enormous energy toward containing its radicalism.

Condorcet's ambivalence in the realm of natural philosophy is no paradox; it was representative of the state of French science in the period just before the Revolution. There had indeed been a tension between speculative system building and the *hypotheses non fingo* orthodoxy. Nonetheless, they had coexisted even within individuals, as the example of Condorcet indicates. This coexistence became exceedingly difficult, if not impossible, after the Revolution. The great social turning point of 1789-94 also marked a discontinuity in this regard.

As the revolutionary tide subsided after Thermidor and radical politics became strongly out of favor in influential circles, the spirit of system likewise became increasingly unfashionable in official scientific milieux. Speculation and system building were associated with socially dangerous habits of thought and were ruled out of French science. The conservative reaction thus decided the struggle, bringing *hypotheses non fingo* to the fore as the watchword of legitimate science and banishing system building to the realm of pseudoscience. This tendency was reinforced by the scientific policies of Napoleon I's empire.

The scientific ideas and writings of the system builders do not seem to have constituted a "radical

science" with strong links to revolutionary politics. In any event, that is not what is at issue here. The point is that unorthodox science was *perceived* as radical after Thermidor in the reaction that condemned system building as a social danger. Its radicalism could not be discerned solely on the basis of the political consciousness or political actions of its practitioners; by that standard a Du Pont or a Lamarck would fall far short of a Condorcet or a Fourcroy.

Nor is it possible to directly connect radicalism with the specific *content* of their natural philosophical ideas. The example of Marat will serve to show that for all of the political inspiration he drew from Rousseau, his physics was based on surprisingly orthodox foundations. As a further example, it is instructive to note that *Naturphilosophie* was put to opposite social uses by radicals and conservatives. Adrian Desmond has shown that, in Britain, in the 1830s, radical sciences inspired by Lamarck and the French *Naturphilosoph* Geoffroy were combatted by the right-wing *Naturphilosophie* of Samuel Taylor Coleridge and his disciples. Although Desmond describes how "artisan agitators" used "a social Lamarckian science" to "underwrite the change to a democratic, cooperative society," he warns that "the social meanings of these sciences were local and contingent, and not logically inherent in the theories themselves."[27]

The point to be made is that conservative social pressures shaped the course of French science after the Revolution. No conspiracy theory is necessary to make that case; the evidence for France is more direct than that for the earlier English experience. Given the high degree of centralization and state control over French science, an examination of the attitudes and actions of the primary policy makers is of great relevance:

> With the founding of a new set of institutions of science during the Revolution and the Empire, French science became establishment science. French science, unlike English science, was centralized and largely confined to government institutions.[28]

The most influential shapers of scientific policy in the post-Revolutionary period were Pierre Samuel Laplace, Georges Cuvier, and of course Napoleon Bonaparte.

Laplace as Newton's Successor

Laplace represented continuity with the old guard of orthodox science; with the martyred Lavoisier and Condorcet no longer on the scene, the prestige of the *ci-devant* Academy accrued above all to Laplace. Comparing the influence wielded by Newton and Laplace in their respective postrevolutionary eras, Roger Hahn writes:

> Newton not only presided over the English scientific community in the early eighteenth century, but also ruled over it like the scientific magnate he was. Likewise, in the first two decades of the nineteenth century, Laplace controlled the French scientific community, imposing upon his younger contemporaries his individual preferences, his scientific world-view, and his own special vision of the true nature of scientific progress.[29]

Laplace's stature in the scientific community derived in part from his fruitful experimental collaboration with Lavoisier in the early 1780s and from his development of the mathematical theory of probability. It was his contributions to the field of technical astronomy, or celestial mechanics, however, that most of all established Laplace as the "successor to Newton."

Although Newton's *Principia* had provided a powerful mathematical description of planetary motion, it had left certain key problems unresolved. Newton's calculations, applied to discernible perturbations in planetary orbits, led to the conclusion that the solar system was ultimately unstable. According to Newton, stability was preserved only by periodic corrections to the planetary orbits that were supplied by divine providence. This aspect of Newton's natural philosophy had been ridiculed by Leibniz and the Cartesians who charged him with portraying God as an incompetent watchmaker, incapable of creating an eternal universal machine requiring no rewinding.

Laplace was able to show that even the planetary perturbations could be explained by Newton's gravitational equations as part of a mechanical system in perfect balance; the clockwork universe would, after all, run forever without any supernatural intervention. By thus demonstrating the essential stability of the solar system, Laplace was considered to have completed and perfected Newtonian physics.[30]

Laplace had contributed to solidifying the Newtonian world picture in more ways than one, however. The ideological implications gained new vigor as well. Eliminating the need for the hand of providence strengthened Enlightenment anticlericalism, while at the same time making the universe seem ever more absolutely mechanical and devoid of spirit.

Laplace is famous for having developed these philosophical implications into a system of absolute determinism. Extrapolating the precise predictability of planetary motion to the fundamental particles of matter, he asserted that all motion, all change in nature past and future, is in principle perfectly knowable, calculable, and predictable. Here is his classic formulation:

> The present state of the system of nature is evidently a resultant of what it was in the preceding instant, and if we conceive of an Intelligence who, for a given moment, embraces all the relations of beings in this Universe, It will be able to determine for any instant of the past or future their respective positions, motions, and generally their affections.
>
> Physical astronomy, that subject of our understanding most worthy of the human spirit, offers us an idea, albeit imperfect, of what such an Intelligence would be. The simplicity of the laws that make celestial bodies move, the relationship of their masses and their distances allow us to follow their motion through analysis, up to a certain point, and in order to determine the state of the system of these large bodies in past or future centuries, it is enough for the mathematician that observation provide him with their position and speeds at any given instant.[31]

Laplacean determinism, then, was the ultimate reductionism; it would in principle reduce the laws of all sciences, including the biological, psychological, and

social sciences, to the laws of Newtonian physics. Despite his "avowed reticence to deal with metaphysical questions," Laplace was clearly indulging himself "in speculations that went far beyond phenomena immediately at hand."[32]

Laplace's stabilization of the Newtonian universe provided ideological ballast for the stabilization of French society after the tumult of the Revolution. More was involved than simply providing a metaphor of stability for the use of propagandists, as Gillispie would have it.[33] To the extent that Laplace's mechanical world-picture and his determinism trickled down from the scientific elite to the rest of society, it served to discourage revolutionary activity and thin the ranks of radical political organizations. If social change is ultimately a predetermined outcome of the physical interaction of atoms, then it will happen when it happens and no amount of agitating, no organizing, no calling of strikes or demonstrations, no political clubs or revolutionary parties can bring it a minute closer. That was the social message of Laplacean determinism: not that revolutionary activity is illegal or immoral, but a waste of time.

Laplace had almost no interest in politics *per se;* he played no political part in the Revolution. After the Revolution, however, he was drawn into political life as a means of career advancement and rapidly rose to the top. As Napoleon's favorite scientist, he was named to the powerful

post of Minister of the Interior after the coup of 18th Brumaire (November 9, 1799).

Laplace did not last long in this position; after just six weeks he was dismissed. Napoleon later complained that Laplace had proven inadequate to the task because he brought to it "the spirit of the infinitely little"; he paid too much attention to details to the exclusion of larger and more important matters.[34] Although this might suggest a fundamental incompatibility between conservative epistemological instincts and success as an administrator, the case of Georges Cuvier would prove otherwise.

Despite initial failure, Laplace continued to hold positions of honor and importance in the Napoleonic regime. He was among the first of those named to the Senate, where from 1800 to 1814 he served, at one time or another, as secretary-general, vice-president, president, and chancellor.

Napoleon Bonaparte, *Savant*

Napoleon's attraction to Laplace was part of the First Consul's general enthusiasm for science, and above all for the "hard" physical and mathematical disciplines. Napoleon had had a first-rate technical education in the field of military engineering. Problems of fortification and ballistics required a mathematical foundation; when he entered the Ecole Militaire in Paris in 1785, his examiner had been none other than Laplace.

In 1797 Bonaparte was himself elected to membership in the reorganized Academy of Sciences, in the section of Mechanical Arts. Ironically, the position he occupied was the one vacated by another mathematically inclined military engineer, Lazare Carnot. Carnot, who had been one of five Directors at the head of the government, had just been driven from power by General Bonaparte's coup d'etat of 18 Fructidor (September 4, 1797); by going into exile Carnot had forfeited his place in the Academy.

While Bonaparte's election to the Academy of Sciences was obviously not based primarily upon considerations of scientific merit, neither should it be dismissed as a purely cynical act of power politics. His election reflected a genuine passion for science on his part and a recognition by the Academy that he was in a position to advance the cause of science. His joining the Academy did not initiate the merger of political power with institutional science -- witness Carnot's position, for example -- but it certainly consolidated and extended it.

Napoleon's view of science was in accord with the dominant tone set for the Academy by Laplace; he shared the bias against system building, and his presence could only increase its dominance within institutionalized science.

Nicole and Jean Dhombres have explained that it would be an oversimplification to portray Napoleon's views in purely Newtonian terms. They argue that his scientific philosophy resulted from the confluence of three major

currents, which they identify as Newtonianism, Encyclopedism, and Rousseauianism. "It is not possible to underestimate the debt that *le pensée napoléonienne* owes to the influence of Rousseau," they contend.[35]

In particular, "his admiration for Buffon, his friendship with Cuvier, Geoffroy Saint-Hilaire and Lacépède" and "the exceptional financial support his regime provided to the Museum of Natural History" all bore witness to the Emperor's Rousseauian attraction to the natural sciences.[36] Indeed, while Napoleon promoted Laplace, the ultimate reductionist, to positions of prestige, he likewise appointed the erstwhile natural philosopher Lacépède as Grand Chancellor of the Legion of Honor.

Napoleon's intervention in post-Revolutionary scientific politics should not be interpreted simply in terms of the Great Man theory of history. Bonaparte did not create the conservative scientific orthodoxy, nor did he bring about its dominance by imperial fiat. By favoring it, however, he did contribute greatly to augmenting its social weight, thereby ensuring its dominance.

In politics as in war, Bonaparte's success was not based on crude attempts to obliterate enemies, but on more sophisticated strategies of winning them over or neutralizing them by balancing opposing factions. In scientific politics, too, Napoleon oversaw the triumph of the prevailing orthodoxy -- which he simply took for granted to be "real science" -- not by decreeing *hypotheses*

non fingo as official dogma but by a more subtle administration of rewards and punishments. While Lacépède responded to the carrot, Lamarck and Bernardin de Saint-Pierre would be beaten with the stick of imperial disdain and ridicule, as will be seen in the chapters to come.

In the conclusion to his *Science and Polity in France at the End of the Old Regime*, Gillispie posed the rhetorical question, "What is it that statesmen have generally wanted from science?" and gave this answer:

> From science, all the statesmen and politicians want are instrumentalities, powers but not power: weapons, techniques, information, communications, and so on.[37]

Keith Baker has taken issue with this view, responding directly to Gillispie that science offered statesmen

> more than a repertory of technology derived from scientific knowledge. It also held out the potential for a new source of legitimacy, a system of authority resting on principles of reason and nature.[38]

Napoleon Bonaparte, to be sure, was greatly attracted to the utilitarian benefits promised by science, but his all-consuming concern was the reconsolidation of civil order following a social revolution, and science had a crucial role to play in legitimating his efforts in that regard.

The Role of Georges Cuvier

The man who turned out to be Napoleon's most important ally in shaping the course of French science was not the physical scientist Laplace but a life scientist, Georges Cuvier. Cuvier became permanent secretary of the Academy of

Sciences in 1803. Roger Hahn has pointed out that the post-Revolutionary Academy was not the primary locus of French scientific activity that the pre-Revolutionary Academy had been.[39] The key research facilities were elsewhere, mainly in centers of scientific education such as the Collège de France, the Ecole Polytechnique, or the Muséum d'Histoire Naturelle. Nonetheless, the Academy was still "the central stage upon which all the activities of the community were displayed, disputed, and eventually judged."[40] As its titular leader, Cuvier can be considered to have acted as an official spokesman for French science.

Cuvier's positions at the Collège de France and the Muséum d'Histoire Naturelle and on the Council of the University gave him immense powers of patronage. Scientists in all fields recognized the long shadow that Cuvier cast over their careers. Cuvier's influence over French science remained virtually unchallenged until the 1820s when rival factions within the Academy of Sciences began to emerge.

Owing to Cuvier's powerful positions and his connections with Bonaparte, his attitudes toward science and society can for all practical purposes be considered synonymous with those of the "scientific community" as a whole during the period of Napoleonic power. In general, his biographers have stressed that he "was by temperament cautious and conservative."[41]

Although Cuvier argued against the extreme reductionism of mechanist biology, he was in the most

general sense a reductionist: his scientific thought was characterized by a "conviction that the phenomena of the natural world were ultimately reducible to simple and exact description and explanation."[42] In an official report to the Emperor, for example, he reported that Lavoisier's new chemistry would

> reveal to us the essence of life itself in the continually changing proportions among the rather few elements. A little more or less oxygen or nitrogen, and there you have it: in the present state of science, the only apparent cause of the innumerable products of organic bodies.[43]

"Cuvier's physical world, then, was a machine," Coleman explains;

> Cuvier's nature . . . was a conservative and static conception of nature and one which was certain to be attacked by the advocates of a more self-sufficient and dynamic view of the world.[44]

As for his social and political views:

> Cuvier was a political conservative. His greatest fear and, considering the chaos of life during and following much of the era of the Revolution and First Empire, a very understandable fear, was social disorder. . . . His efforts in public administration and in education were thus devoted to securing a strong social order and to training the youth of the nation to cherish and to preserve this order.[45]

The links between Cuvier's scientific outlook, his political views, and the social context of the Revolution have been recognized and described by Toby Appel in her study of the debate between Cuvier and Geoffroy, which was discussed in Chapter One. "Cuvier," Appel writes, "was sufficiently chastened by the experience of the French

Revolution to begin identifying unbridled scientific theorizing with social and moral chaos."[46]

> A political elitist who advocated government by expert administrators, Cuvier feared above all the return of revolution and rule by the uncontrolled mob. Scientific ideas, if unrestrained, might be exploited by unscrupulous men to weaken the social order. Thus, it was partly for political reasons that Cuvier insisted so strongly on limiting science to "positive facts."[47]

> His belief that ideas were at the root of the Revolution led him to fear the consequences of unbridled generalizations in the sciences, as well as in political or social thought. . . . Cuvier had little sympathy for "systems" or hypotheses in natural history.[48]

Appel notes that while Cuvier's anxiety over "the effects of 'systems,' whether strictly political or supposedly scientific, on the uneducated multitude" may have been exaggerated, it "was in some measure legitimate because many times before the Revolution and many times afterward, scientific theories had indeed been politicized in France."[49] But whether his perception of the social dangers of system building were justified or not, the reality of Cuvier's power and influence over the course of French science made it a significant historical factor.

As the primary spokesman for centralized French science under Napoleon, Cuvier was fully aware of the importance of the well-defined social function he was expected to perform. In his 1808 report to the Emperor, Cuvier carefully enumerated the social uses of science as he saw them:

> To spread healthy ideas among even the lowest classes of people, to remove men from the influence of prejudice and passion, to make reason the arbiter and supreme guide of public opinion: that is the essential goal of the sciences; that is how science will contribute to the advancement of civilization, and that is what deserves the protection of governments who want to insure the stability of their power.[50]

Cuvier's formulation lays bare the classic themes of the social uses of science; the operative phrases are "healthy ideas," "lowest classes of people," "reason as arbiter of public opinion" and "passion." It is to be inferred that, with regard to the lower classes of people, unhealthy ideas are revolutionary ones. Ideas that challenge the social *status quo* must be shown to be unscientific, and the dangerous classes must be educated to accept that kind of science. To be dissatisfied with the state of society, to desire social change, or even to think that social change is possible must be demonstrated to be contrary to nature and contrary to reason.

Most of all, "passion" must be understood as unhealthy, unnatural, unreasonable and unscientific. Passion leads to enthusiasm and from there to political unrest. Passion overstimulates the imagination and prompts unreasonable conjectures about what is possible. The intrusion of passion into science stems from Rousseau's admonition that science should above all focus upon the most deeply felt human concerns. Passion, in sum, is a source of social evils that must be definitively exorcised from responsible science.

It is noteworthy that Cuvier stressed the social uses of science not as a secondary concern but as the *essential goal* of science: not knowledge for its own sake or for the satisfaction of curiosity, not technological advance for the sake of material betterment, but the *ideological conquest of public opinion in the interests of stabilizing the state power*.

It can be argued, of course, that Cuvier was simply emphasing a theme designed as a sales pitch to the Emperor to insure a steady flow of state subsidies for the Academy of Sciences. His biographers leave no doubt, however, that his social concerns were deeply and sincerely felt and were in close accord with the Emperor's.

Intention aside, the real efficacy of scientific ideology as a primary tool of social policy has also been questioned. The ideology, it has been argued, was not used to shape policy, but merely to justify it after the fact. What would result, Gillispie muses, if metaphors of organism and mechanism were banished from reasoning about state and society? "Nothing would happen to political realities, of course, but at least political apologists would be deprived of the right to draw dogmas from the nature of things."[51]

Scientific ideology, according to this view, was not a genuine causative factor but simply a reflection of events, a superficial phenomenon. Cuvier's statement, however, calls attention to the role of public opinion as a

mediating link between ideology and historical action. The public's exaggerated respect for the authority of modern science could be used to demoralize revolutionaries and dampen mass enthusiasm. The ability to convince the politically active part of the population that proposals for social change are unscientific pipedreams doomed to failure is an advantage that no ruling power voluntarily relinquishes.

There is a certain sense in which major historical movements are ontologically prior to ideologies associated with them; as Goethe put it, in the beginning was the *deed*, not the *word*. But relegating ideology to a "superstructure" walled off from all causal interaction with social activity is a mechanical-materialist fallacy. The power of ideas translated into political action to alter the course of human events has been demonstrated by countless social revolutions, not least of which was the one that erupted at the Bastille in 1789. As previously noted, Cuvier and the Emperor were most conscious of that power since they had just experienced it at first hand; their aim was to keep it under control.

Cuvier's report made clear what would and would not henceforth qualify as science. "All the hypotheses, all the more or less ingenious suppositions," he wrote, "are today rejected by real scientists."

> Experiment alone, precise experiments with scales, measurements, calculations and comparisons of all the

> substances used and obtained: that is today the only legitimate path of reasoning and demonstration.[52]

There was more than a touch of the double standard evident in this kind of proclamation. His own scientific work "was far from empirical," but "Cuvier adopted and promulgated an empiricist ideology in order to combat opposing points of view."[53]

The ideological distance from the radical Condorcet to the conservative Cuvier provides a measure of the transformation that followed Thermidor. As part of the general reversal of revolutionary ardor that it signaled, Thermidor also brought about the decisive defeat of Rousseau's program to integrate moral and social values into science. This is not to suggest that Robespierre's execution resulted in the instant death of radicalism; almost two months later the entire Convention marched in public procession to conduct Marat's ashes to the Pantheon. But if the door did not slam shut on radicalism, it certainly began to slowly close. The moderates had regained the upper hand. The removal of Marat's ashes from the Pantheon by the same Convention only five months after they had been placed there symbolized the irreversible decline of the French Revolution's radicalism.[54] It would echo in Babeuf's conspiracy of equals and Bonaparte would construct his social balancing act partially upon an appeal to Jacobin tradition. In the final analysis, however, the moderates -- the '89ers, who fought for legal equality and against social equality -- came out on top in the French

Revolution, while those who pursued their radicalism to the end -- the '93ers, who fought for social equality as well -- were defeated.

The marginalization of speculative natural philosophy resulted in large part from the cooling of a social revolution, not solely from the inexorable march of scientific progress. Its decline should be understood as a corollary of Thermidor rather than as the natural triumph of good science driving out bad science. The traditional dismissal of the system building of the prerevolutionary period as bad or primitive science, then, deserves reevaluation.

PART II:
The Scientific Underground of the Old Regime

CHAPTER FOUR

MARAT, CARRA, DU PONT DE NEMOURS, AND BERGASSE

As noted earlier, it is significant that quite a few of the authors who wrote now-forgotten works of natural philosophy in the era of the French Revolution are nevertheless remembered for their other achievements in politics, literature, technology, and science. The recognition of their importance in one of these other fields by their contemporaries suggests that they were not completely out of step with their times. They did not constitute an exotic fringe element; nor was their natural philosophy wildly unrepresentative of the contemporary *Weltanshauung*. Ideas that today may be deemed fantastic or foolish were shared by many intelligent people. The open hostility between the system builders and the orthodox academicians has tended to obscure the broad base of fundamental assumptions about nature shared by both groups. Although, for example, Bernardin de Saint-Pierre's scientific writings have frequently been ridiculed for their teleological character, Cuvier, the pillar of orthodoxy, likewise built his work on a thoroughly teleological foundation.

Some of the prominent denizens of the scientific underground of the old regime were identified in Chapter One. In this and the next two chapters they will be more

fully introduced, with special attention to their published volumes of natural philosophy and their respective relationships to the political events of the Revolution.

For the sake of presentation, it is helpful to group them together on the basis of the accomplishments for which they are best remembered. Three groups can thus be discerned, albeit somewhat arbitrarily: political, literary, and scientific. This chapter will introduce the political group, consisting of Marat, Carra, Bergasse, and Du Pont de Nemours. Chapter Five will discuss the two-member literary group of Bernardin de Saint-Pierre and Restif de la Bretonne. The subject of Chapter Six will be the scientific group of Lamarck, Lacépède, Delamétherie, and the team of Marivetz and Goussier.

The four members of the political group happen, by coincidence, to neatly represent four rather distinct stages in the development of the Revolution. Bergasse represents the earliest, least radical phase, which historians have denominated the "aristocratic rebellion." Du Pont de Nemours represents the next stage, that of the 89ers who welcomed the democratic changes wrought by the Revolution but who would have been completely satisfied by an English-style constitutional monarchy. Carra represents a more radical stage, that of the initial experiments in republicanism. Marat, of course, represents the most radical stage of all, which culminated in the brief ascendency of the Jacobin Republic.

Some historians of the French Revolution have associated these four stages with the interests of specific social classes.[1] Although this sort of class analysis has gone out of fashion, especially among English-speaking historians, no harm can come from recalling these correlations as long as they are not applied mechanically or dogmatically. Bergasse, then, would be associated with the aristocrats; Du Pont with the bourgeois business class; Carra with underappreciated professionals and other petty bourgeois; and Marat with the plebian Parisian crowd. No attempt will be made in the present study to justify this schema by demonstrating specific links between natural philosophers and social class interests.

The two members of the literary group are also representative of qualitatively different social strata. Bernardin belonged to the literary elite, while Restif, before the Revolution at least, was strictly Grub Street.

Social differences divided the scientific group as well, but not so neatly. Lacépède and Marivetz were of the wealthy nobility; Lamarck was of the nonwealthy nobility; Delamétherie and Goussier were untitled commoners.

The following introductions to the neglected natural philosophers of the Revolutionary era will begin with the political group, in descending order of their political radicalness.

I. JEAN PAUL MARAT (1743-1793)

Marat is certainly the best known of the lot: a revolutionary saint to some and a bloodthirsty sociopath to others, depending upon the ideological outlook of the observer. "For historians," Gillispie observes, "reactions to Marat are a touchstone of feelings about the French Revolution."[2]

Marat was the only prominent natural philosopher who can legitimately be identified with Jacobinism in the fullest political sense. His fame, or infamy, derives from his role as the most influential of the radical journalists and agitators. Of the hundreds of competing journals that sprang into existence with the collapse of censorship at the onset of the Revolution, Marat's *Ami du peuple* most thoroughly succeeded in expressing the aspirations and mobilizing the rage of the downtrodden Parisian *sans-culottes* during the height of the revolutionary fervor.

In addition to his role as journalistic agitator, Marat was also a political leader in the clubs and the Convention as well as a mass-action organizer. Elected a Parisian deputy to the Convention on September 9, 1792, he was to become a central figure in the Montagnard faction. Marat's organizing activities are not as easily documented, since planning bodies for insurrections are necessarily secretive. Historians have debated the extent of Marat's

direct organizational responsibility for the notorious prison massacres of September 1792 and the great insurrectionary uprising of May 31-June 2, 1793, that sealed the fate of the Girondins. Whatever the depth of his involvement in directing these events, it is safe to conclude that Marat was far more than a sideline commentator.

Marat, then, is the archetypal Jacobin scientist who serves as the flesh-and-blood embodiment of the myth. He fulfills perfectly every criterion for the role: before becoming a high-profile political agitator, he had been a successful medical doctor, an experimental physicist, a prolific author of non-mainstream scientific treatises, and a vociferous opponent of the Academy of Sciences. As a physicist he performed hundreds of experiments investigating heat, light, and electricity, and advanced the claim that he had improved upon Newton's theory of colors.

Marat's bitter opposition to the Academy of Sciences was politically significant because to challenge the Academy was to challenge an important institution of the monarchy. It stood for mainstream science not only in the ideological sense, but also as the representative of the state's power of control over science.

The antagonism that developed between Marat and the Academy of Sciences, however, was not primarily institutional, but originated in differing approaches to

science. As Joseph Dauben has shown, their relationship began cordially enough, but came to grief over the issue of system building.[3] The Academy was not originally hostile to Marat's experiments, but was eventually alienated by his attempts to create theoretical frameworks that would explain them in ways that challenged orthodox Newtonian assumptions. Condorcet cited "the Marat incident" as the prime example of how the Academy should function as "a barrier against charlatanism of all kinds."[4]

Some academicians, however, including Lamarck, continued to look favorably upon Marat's scientific activity.[5] Marat's challenge to Newtonian optics directly influenced Goethe's study of the science of color.[6] Darnton cites evidence of "the fact that he was respected, if not lionized, as a scientist, even in 1785."[7] Marat, in short, stood solidly in the tradition of eighteenth-century natural philosophy, which was a legitimate current within contemporary science. The usual dismissal of his work as that of a scientific imposter is therefore an ahistorical judgment, as Part Three of this dissertation will seek to demonstrate.

The core of Marat's extensive writings on natural philosophy consists of a trilogy: *Recherches physiques sur le feu* (1780), *Découvertes sur la lumière* (1780), and *Recherches physiques sur l'électricité* (1782). These will be examined in detail in Chapters Nine, Ten, and Eleven, respectively. Marat's theoretical ventures into the human,

or moral, sciences also merit attention: his *Philosophical Essay on Man* (1773) and *Chains of Slavery* (1774), written and published in English while he lived in England, and his *Plan de législation en matière criminelle* (1790) will be considered in Chapter Eight.

II. JEAN LOUIS CARRA (1742-1793)

Although no longer as notorious as Marat, Carra was also a radical journalist, agitator, and political leader of primary importance during the Revolution. He scored a major success as a pamphleteer with his *Orateur des Etats-Généraux* in April 1789, a significant contribution to the political agitation leading to the explosion of July 14. In October 1789 he founded a daily newspaper in collaboration with Louis Sébastien Mercier, the *Annales patriotiques et littéraires*, and served as its editor-in-chief. The young Cuvier, not yet a well-known scientist, wrote to his friend Pfaff that "le journal de Carras *(sic)* . . . abounds far too much in democratic absurdities." He believed that "Mercier contributes nothing to it but his name."[8]

The *Annales*, Lamartine tells us, made Carra "un nom redouté"; it was the most influential of the radical journals, at least through 1792, in the Jacobin clubs throughout France.[9] In the elections to the National Convention in late 1792 Carra was elected deputy by eight departments -- almost twice as many as any other candidate. At that time, "Carra was one of the most popular men in France."[10]

Carra was a prominent activist in the Parisian Jacobin clubs in the early phase of the Revolution. He was a central instigator of the events of June 20, 1792, when the

people in arms invaded the Tuileries palace and forced Louis XVI to hear their grievances face to face. Carra was also part of the leadership, along with Danton, that clandestinely organized the massive insurrection of August 10, 1792, overthrowing the monarchy and leading to the creation of the Republic.

Carra is not remembered as a Jacobin, however, because historians, following Lamartine, have traditionally placed him solidly in the Girondin camp. Although Carra was accused of participating in the Girondin "conspiracy against the republican government," and was guillotined on October 31, 1793, together with Brissot and eighteen others so accused, he had not in fact been entirely aligned with any faction. As a deputy he cast some key votes for and some against Girondin positions and as a journalist he tried to walk a tightrope of neutrality between the Mountain and the Gironde.[11]

During his trial, Carra defended his republican virtue by citing his works of natural philosophy as evidence of the consistency and continuity of his political outlook over the previous two decades. He recalled, for example, his reply two years earlier to Alexander Lameth's contention that a nation as large as France was unsuited for the republican form of government:

> There is no doubt that France, as well as the other nations of the world, can and will rapidly become republics, since the great physical system of the universe that rules over the moral and political system of humankind is itself a

> veritable republic (as I demonstrated several years ago in my *Nouveaux Principes de physique*, 4 vol. in-8°).[12]

Carra's first work of natural philosophy, *Système de la raison*, was published in 1773, and he produced a number of smaller studies on ballooning, Mesmerism and the nature of light, fire, and electricity.[13] The above statement indicates that the work he was most proud of was his *Nouveaux Principes de physique* (1781-1783), a grand synthesis of all scientific knowledge. Although long forgotten, Carra's masterpiece was widely read before the Revolution and contributed to his contemporary reputation, in some quarters at least, as a serious thinker. Bachelard acknowledged that Carra was "an author often quoted at the end of the eighteenth century."[14]

Carra also figured prominently in Darnton's study of the literary underground of the old regime.[15] His class of Grub Street writers was defined by inclusion in Rivarol's satirical *Petit Almanach de nos grands hommes*. Rivarol's outlook was characterized by an extreme elitism; his *Almanach* was designed to ridicule writers who had not attained high social status. As would be expected, Rivarol despised the Revolution and wielded his pen against it at every opportunity. In 1790 he published a sequel to the *Almanach* entitled *Petit Dictionnaire des grands hommes de la Révolution*; Carra was lampooned as a "patriotic barker" whose newspaper rejoiced over violence and death.[16]

Carra, it is true, had gained neither wealth nor high social rank from his writing. Nonetheless, he had attracted influential sponsors throughout his career. In 1771 he had been hired, on Voltaire's recommendation, by Jean Baptiste Robinet to join the editorial staff of the *Suppléments* to the *Encyclopédie*. Robinet himself had earned a reputation as an important natural philosopher on the basis of his four-volume *De la nature*. After a bitter falling out with Robinet (leading Carra to publish a book calling him a "fake philosopher" in its title), Carra was apparently able to call upon d'Alembert for help in obtaining a position in Paris.[17] After Benjamin Franklin's arrival in France in March 1777, Carra visited him frequently at Passy. He also had as patrons King Frederick William of Prussia, to whom he dedicated the *Nouveaux Principes de physique,* and Loménie de Brienne, who helped Carra obtain a position at the Bibliothèque du Roi.[18] While Carra can legitimately be classified among the Grub Street writers of the old regime, then, that does not mean that he was perceived as an oddball or social misfit, as Rivarol's ridicule might imply.

Carra's scientific career was based almost exclusively on his writing; unlike Marat he was not a working scientist. He proposed experiments for others to perform but only rarely performed any himself.

His contacts with the scientific elite were minimal. On January 14, 1784, he read a paper on aeronautics at the

Academy of Sciences. The subject was an important one: the balloons of Montgolfier and Charles had inaugurated the era of human flight, but the means of controlling the direction of flight remained unsolved. Carra's paper was his contribution to the discussion of aerial navigation.

His proposal was conceptually simple. A pilot's compartment shaped like a rowboat is suspended below a gas-filled balloon. A long copper tube extends out from each side of the pilot's compartment. Attached to each end of the tube are three large, light, but rigid "wings," in a configuration analogous to a riverboat's paddlewheel. The tube, and therefore the wings, were to be turned by pedals operated by the pilot. A moveable rudder extends below and behind the compartment. By pedalling, the pilot could increase the balloon's speed; by manipulating the rudder it could be steered.

Carra's appearance at the Academy was duly noted in the minutes, but there is no indication of any response.[19] He published his paper as *Essai sur la nautique aérienne*, with an illustration that renders his conception of an "aeronautique machine" fully comprehensible.

Carra's primary work of natural philosophy, *Nouveaux Principes de physique*, can best be summarized by quoting from the prospectus he issued, which was printed in the September 1781 issue of the *Journal de Physique*:

> The grand purpose of this work is the demonstrated discovery and exact definition of a universal agent to which God has given the property of being able to

> modify matter in every way necessary to accomplish all the wonders of nature.
>
> The author explains by means of this discovery, in a clear and demonstrative manner, the causes of attraction, of impulsion, of the rotation of the planets on their axes, and of their rotation around the sun; causes that have by no means been studied in sufficient depth until now.
>
> He demonstrates, by the same principle, that everything in nature is connected; that the absolute void postulated by Newton in ethereal space is in fact a relative void. Newton's absolute void is nothing more than a metaphysical system that the English mathematician believed he had established since it conveniently fit his calculations, but in fact it is entirely contrary to the laws and causes of attraction, gravitation, electricity, and magnetism.
>
> The present author establishes a mathematical distinction between gravitation, attraction, electricity, and magnetism by means of which he demonstrates that each of these four powerful forces of nature has particular and independent laws, which concur in an orderly way and which all connect to the universal mechanism.
>
> He presents, from an entirely novel point of view, the theory of the sun, of the stars, of planets, of comets, and of our globe. He also explains, by means of equally new relationships, the origin and development of the three kingdoms of nature, of light and colors, of fire and heat, of air and sounds, of water and heavy fluids, of the element earth, of mineralization, of vegetation, of the animal system, of the specific development of the forces and faculties of the animal prototype, of generation, of the animal economy, of the heart and circulation of the blood, of the brain and the nervous system, of the human *sensorium*, of sight, of hearing, of the senses of smell and taste, of memory, of the mechanism of dreams and sleepwalking, and of the passions.
>
> The work ends with a history of humanity with respect to the development of morality.[20]

This brief synopsis cannot do justice to the contents of Carra's four volumes, but it does serve to give an indication of the scope and general orientation of the work. As indicated above, its central concern is the elaboration of the properties of a universal fluid that serves as the ultimate carrier of all physical phenomena.

Carra distinguished his system from that of other natural philosophers primarily by the strength of his emphasis on the singularity of the universal fluid. Most others considered heat, light, electricity, and magnetism to be products of distinct fluids, but Carra insisted that all are simply modifications or variants of the single universal agent. Although Marat's experiments with the subtle fluids of nature received generally favorable reviews from Carra, he criticized Marat's treatment of igneous fluid, luminous fluid, and electric fluid as distinct entities.

Carra contrasted his idea of the universal fluid to that of Marivetz and Goussier as expounded in the first volume of their *Physique du monde*.[21] He said he thought their work and his were remarkably similar, and that they reinforced each other. He listed some points of disagreement, however, and gently suggested that the discrepancies arose from inconsistencies in Marivetz's system.

Marivetz had made it clear that his universal fluid was made up of exceedingly small spherical particles. Carra's ether, by contrast, was a *pure fluid:* not made up of any kind of tiny pieces but of completely continuous matter. Unlike ordinary matter, the universal fluid does not have the property of impenetrability, which explains how motion can take place in a universe completely filled with it.

Carra was attracted to Mesmerism, although in his 1785 pamphlet on the subject he described Mesmer as "personally ignorant" and animal magnetism as a "meaningless phrase."[22] He had sympathized with the politicized faction led by Nicolas Bergasse that split with Mesmer in 1785, but Bergasse, too, was a target of Carra's criticism.[23] Carra believed that the Mesmerists had empirically stumbled upon some medically important discoveries, but had failed to grasp the physical principles underlying those discoveries. Carra's purpose, in short, was to teach the Mesmerists some physics.

Mesmer, Bergasse, and other theoreticians had claimed that animal magnetism has absolutely nothing to do with the ordinary magnetism that affects iron. Carra strongly dissented; the auras or atmospheres around living beings that Mesmerist healers could manipulate are products, he said, of the universal fluid and therefore cannot be dissociated from ordinary magnetism, electricity, or any other forces of nature.

A review of the first volume of *Nouveaux Principes de physique* in the *Journal encyclopédique* placed Carra among "the small number of those who have fundamentally contributed to extending the sphere of our knowledge of physics."[24] Later volumes were also appreciatively reviewed by the same publication.[25]

A review in the *Journal de Physique* was less positive, however. Every author of a system like Carra's, the

reviewer complained, asserts that it alone represents the whole truth and that all others are in error. "Should all such systems be absolutely rejected, then? No. . . . It is for the public to decide whether Mr. Carra has done better than his predecessors."[26]

In his 1785 treatise on animal magnetism, Carra made explicit his view of the connections between the physical and moral sciences:

> While Europe undergoes a violent spasm upsetting the political equilibrium of the great national powers, while the whole globe seems to be preparing itself for a revolution in the course of the seasons, in physical changes on its surface and in its atmosphere, the science of nature and philosophy make immense efforts to diffuse and propagate their beneficial wisdom everywhere. The social masses, always floating on the waves of uncertainty, demand, more than ever, to finally clear up the moral and legislative chaos. They continuously yearn for the happy calmness that the perfectability of human reason promises them.[27]

Carra's works of physics were aimed at fulfilling that promise.

III. PIERRE SAMUEL DU PONT DE NEMOURS (1739-1817)

Du Pont de Nemours is remembered as an important statesman and pioneer economist, but if truth be told he is probably best known for having initiated an American business venture that his son Irenée later developed into one of the world's most powerful industrial enterprises.[28] The younger Du Pont's success was based upon knowledge of the techniques of gunpowder manufacture, which he had gained under the tutelage of his father's friend, Antoine Lavoisier.

Du Pont de Nemours had been closely associated with Quesnay and Turgot in the development of the physiocratic school of economic theory, and was the publisher of the Academy of Sciences' official *Mémoires* before it was suppressed. After the Revolution he was elected to the Council of Elders in the legislative body constituted by the constitution of 1795 and, following his return from America, served as nominal head of the Provisional Government of France in the brief period between Bonaparte's first fall from power and his spectacular hundred days comeback.

Du Pont was a moderate reformer, and can even be considered to have been a revolutionary in 1789. He was elected to the Estates-General as a representative of the Third Estate and was later elected to the Constituent

Assembly. He joined with Bailly, Mirabeau, Sieyès, Condorcet, and others in the moderate Society of 1789, but as the Revolution radicalized he resisted its course and became bitterly anti-Jacobin.[29] Driven into hiding in 1792, he produced his *chef-d'oeuvre* of natural philosophy, *Philosophie de l'univers*, which he dedicated to his close friends the Lavoisiers.[30] (Du Pont later proposed marriage to the widow Lavoisier but his suit was rejected; she married the famous physicist Count Rumford instead.)

Du Pont's biographers have generally stressed those aspects of his writings -- the *laissez-faire* economics, for example -- that have a somewhat modern appearance. An impression is thus conveyed of Du Pont de Nemours as a "man who saw through time," a person with a twentieth-century way of thinking who was born two hundred years before his time. The *Philosophie de l'univers* stands as witness against any such anachronistic interpretation. In spite of its author's reputation as an orthodox Enlightenment rationalist and its dedication to the foremost symbol of mainstream science, it is one of the more extreme examples of speculative natural philosophy. Restif de la Bretonne commented on it at length and recognized a kindred spirit in Du Pont de Nemours.[31] In the *Philosophie de l'univers* Du Pont "continued, in the style of a Turgot, to develop the ideas of a Restif or a Carra."[32]

Historiographical tradition has accounted for the *Philosophie de l'univers* as a product of extreme distress

"during these years when Du Pont's world appeared to have collapsed around him."[33] "The old belief in the forward march of Reason could not sustain him; he drew back into spiritualism."[34] That the *Philosophie de l'univers* was no aberration born of a temporarily fevered imagination, however, is attested to by Du Pont's own estimate of its worth; it was "the literary effort which he considered his major work."[35] Furthermore, he expanded and republished it in 1796 and 1799 and was still working on a fourth edition (never published) as late as 1814.[36]

In spite of Du Pont de Nemours' expectation that the *Philosophie de l'univers* would be his most durable contribution to posterity, it has for the most part been ignored. Ambrose Saricks' biography, "probably the definitive" study of Du Pont's life,[37] devotes but a few paragraphs to the *Philosophie de l'univers* and these do not convey the spirit of Du Pont's approach to natural philosophy.[38]

The status of this work as a neglected text is evidenced by a 1949 letter about it from a rare book dealer to Denis Duveen, a noted bibliographer of Lavoisier's works:

> I knew only vaguely about the existence of this book from the catalogue of the Bibliothèque Nationale, but I have never seen it nor did I find it listed in any dealers' catalogues. . . . This Du Pont-Lavoisier item is of the greatest rarity.[39]

Du Pont de Nemours, close friend of Turgot, Lavoisier, and Jefferson, deserves attention as an important link

between thinkers traditionally categorized as orthodox and unorthodox. His *Philosophie de l'univers* likewise merits consideration as an unsurpassable opportunity to gauge the distance separating eighteenth- from twentieth-century ways of perceiving the world. In the Preface, Du Pont lauded Lavoisier as a

> savant of the first magnitude, especially devoted to establishing the physical sciences upon strict methods, and upon the kind of precise analysis with which the truth can be grasped.[40]

Du Pont claimed to have utilized the same rigorous method. Starting with physical truths, he had sought to discover *moral* truths, "which are always based in physics."[41] Number one on the list in his table of observations was: "MORAL SCIENCE IS NOTHING OTHER THAN A PHYSICAL SCIENCE."[42] It is significant that Du Pont perceived his own scientific procedures to be equivalent to Lavoisier's and believed he was emulating the latter.

"This *Philosophie*," he wrote, "brings together truths from three different levels."

> The first are completely physical, and are rigorously demonstrated. . . . They are sufficient . . . to serve as a very solid basis for moral science, as it is ordinarily understood.
>
> The second, which may seem conjectural, can nevertheless be regarded as that which is *necessary* to complete the organization of the universe. Those things that we cannot see should not be governed by laws other than those governing what we perceive with our senses. . . .
>
> The third are not susceptible to any proof other than their perfect analogy with indubitable truths, their accord with the general laws of natural history, and the credibility they derive from the profound morality they reveal in the beautiful entwinement of phenomena that we call *Nature*.[43]

Philosophie de l'univers is divided into three parts. In the first, Du Pont says, he has tried to indicate

> CERTAIN AND INCONTESTABLE LAWS of good and evil that derive from that area of natural history that our senses and our capacity for reflection are able to let us know about with perfect evidence.[44]

The second part is based upon the doctrine of the great chain of being for which he cites the work of the Swiss naturalist Charles Bonnet as an important source.[45] Du Pont believed, as did many chain-of-being theorists, that the beings we are familiar with constitute but the lower end of the chain. Studying the lower end, which is accessible to our inspection, leads to the conclusion that there must necessarily be another, higher end. The universe would otherwise be incomplete. There is, therefore, a rather extended chain above us of beings quite superior to ourselves. We are not *directly* aware of their existence any more than oysters are aware of ours.

The third part is the exposition of a system "conforming to the general laws of natural history" of rewards for good deeds and punishments for crimes that an individual can anticipate receiving after death.

Du Pont was not a professional scientist in the sense of trying to make his living from science, but he was an extremely enthusiastic and hardworking amateur. He submitted numerous memoirs on a large variety of topics to leading scientific societies in France, America, and elsewhere.

Du Pont's primary field of scientific endeavor was natural history. One of his most strongly held convictions was drawn from intensive observations of a wide range of animal species: that the difference in intelligence between various animals in the chain of being is merely quantitative, not qualitative. If the intelligence of a horse or an ant differs from that of a human only in amount, and not in kind, it follows that other animals could conceivably gain in their ability to think and eventually become the intellectual peers of humans.

He devoted considerable time and effort to observing the behavior of various insects, birds, and other kinds of animals. The honeycombs of bees seemed clear evidence of high intelligence. "The beavers of Canada are more advanced than humans in their architecture and sociability."[46] Wolves "understand very well the theory, the practice, and the sanction of contracts; they adhere to them religiously."[47] A special section of *Philosophie de l'univers* was devoted to "the morality, the intelligence, the sciences, and the social institutions of ants."[48]

Du Pont rejected the belittling of animals' accomplishments implied by attributing them to instinct. What others called instinct, he called rapid thinking. To believe otherwise would make animals -- and, by extension, humans -- into mere machines, a notion Du Pont could not accept.

He studied linguistics in order to make a comparative study of the ways people and animals communicate by means of sound. All human languages, he believed, could be reduced to fairly simple elements:

> The alphabets are composed of but a certain number of hieroglyphs that have not yet lost their form, and all the root words are definitions and descriptions.[49]

Through patient observation of the behavior and sounds of cats, crows, and other animals, Du Pont believed he had been able to at least partially decipher their languages.[50]

Neither special intelligence nor language, therefore, are exclusively human attributes. Why, then, have other animals not developed technologies as humans have?

> Because they have not learned to write. But who knows if the more ingenious of those species that live in societies might not one day learn something equivalent?[51]

These considerations formed the basis of Du Pont's pantheistic outlook:

> Everywhere that intelligence manifests itself, there is a GOD. There is one (and perhaps several) in the polyp. There is one in the oyster. There is a very considerable one in the elephant. There is a sublime one in Confucius, in Socrates, in Marcus Aurelius, in Locke, in Leibniz, in Newton, and even in Haller and in Bonnet. I would also include in this category two men whom I have had the good fortune of knowing, Quesnay and Turgot. All of these *Gods* in the universe constitute GOD.[52]

Du Pont's pantheism, however, was strictly materialistic. The intelligence inherent in all things was not considered to be ontologically superior, or prior, to matter. Both were believed to be necessary and "co-eternal" aspects of reality. The supreme intelligence, or God (it

would be a quibble, he said, to avoid that word) organizes matter, but cannot create or destroy it.[53]

As a basis of human morality, Du Pont replaced the traditional biblical system of rewards and punishments with one based on Leibniz's concept of the monad. The monad represented the irreducible atom of the supreme intelligence that animates each individual being; it is what is meant by the word "soul." The monad of a human being who has committed crimes will find itself reassigned, after the human's death, to a considerably lower rung on the chain of being. As examples of the very depths of human depravity, Du Pont listed Marat, Robespierre, Danton, Philippe Egalité, and Choderlos de Laclos.[54] (Du Pont apparently found Choderlos' depiction of aristocratic immorality in *Les Liaisons dangereuses* extremely offensive. As for the other four, in a footnote appended later, Du Pont noted with apparent satisfaction that although they had marked him for death, his persecutors had in the meantime themselves all suffered violent deaths.[55])

The monad of such a wicked person may find itself in a "grain of sand, filled with remorse" and obliged "to beg for and be granted as an act of grace, permission to cause the vegetation of a lichen" as the first step of a slow ascension through "all the grades of the vegetable kingdom, and then the animal kingdom, before finally regaining the rank it had deservedly lost."[56]

A lifetime of exemplary moral behavior, on the other hand, could earn the reward of promotion to a higher-than-human rank on the chain of being. Although we do not directly experience the existence of such higher beings, we nonetheless sense their presence. Why not refer to them as angels? Again, it would be trivial to avoid the word simply because it is part of the vocabulary of ordinary religion.

We frequently, do we not, seek the counsel of dear departed friends?

> How often in difficult situations and in the grip of conflicting emotions have I not asked myself: "What would Charles Granisson do in this case? What would Quesnay think? What would Turgot or Lavoisier advise me to do?"[57]

These mental images of deceased friends represent the intuitive knowledge of the existence of superior intelligences that, if we are attentive, observant, and reflective, are willing to help us.

Du Pont vigorously denied that the beings above us on the chain of being are pure spirits. Like other living beings, they are born, grow old, and die. They are real, material beings; as real to us as we are to the uncomprehending oysters. (Oysters are also, it must be remembered, intelligent beings.)

Du Pont devoted a special section to the question of whether the heavenly bodies, including the earth, are in fact "animated beings"; that is, living organisms.[58]

> We do not know and we cannot know whether these enormous masses are gifted with life; that is, whether each globe is a huge animal upon which all of its

> inhabitants are but species of insects that feed upon it. It is not at all impossible; it is not repugnant to reason.[59]

A microscopic insect (which possesses a certain share of the universal intelligence) could understandably jump to the false conclusion that the elephant it lives on is eternal and inanimate. We should not blithely jump to the same conclusion with regard to the earth.

> In the largest things and in the smallest we find nature always the same, and in those things that seem farthest removed from each other we are forced to recognize closely analogous laws.[60]

Du Pont utilized his vast knowledge of nature to point out numerous analogies between the earth and animals. These remarkable similarities, he admitted, are far from constituting proof, but it would be stupidity to dismiss out of hand the possibility that the earth is a living organism. "Let us avoid both the presumption of affirming it and the audacity of denying it."[61]

Du Pont's natural philosophy will certainly seem to the modern reader to have little to do with science. Yet he repeatedly insisted that all of these ideas were rigorously derived from a close study of nature.

> The more they are compared with the general order of the universe, with the physical and moral laws that are already known with certainty, and the more their interconnections are considered, the more they are found to be reasonable, true, simple, and natural.[62]

Although written in the form of a letter to the Lavoisiers, there was no sense that Du Pont was trying to "win over" people that he anticipated would be hostile to his views. There was no tone of defensiveness; if Du Pont

was afraid of inviting the ridicule of the orthodox scientific community, he did not show it. To the contrary, he believed he was making a contribution that would be appreciated. By utilizing the methods of orthodox natural science to investigate the realm of moral science, he hoped to provide humanity with a scientifically founded ethical code; in other words, a scientifically sound religion.

IV. NICOLAS BERGASSE (1750-1832)

The place of Nicolas Bergasse in this study is due to his dual role as an important political figure during the French Revolution and as the principal expositor of Mesmerist natural philosophy. Franz Anton Mesmer maintained a certain distance between himself and his public; the appearance of being above worldly concerns was an important element of his personal mystique. Most of the literature of Mesmerism, then, was produced by his followers, of whom Bergasse was at a crucial time the most prominent. Bergasse's writings certainly constitute the most authoritative statement of the Mesmerist natural philosophy of the 1780s, but more than that, Bergasse should be accorded a large share of the credit for creating rather than merely propagating that natural philosophy. It was he who took Mesmer's principle of a universal vital energy and "made of it a system."[63]

Bergasse was not Mesmer's only spokesman, but in the critical formative years of the movement he held first place in the official ranking.[64] In 1783, he and his close friend Guillaume Kornmann, a wealthy banker, founded the *Société de l'Harmonie Universelle*; Bergasse became its main theoretician and organizer.

The Universal Harmony Society proved financially and organizationally successful. Within a few years the

Parisian base had several hundred members and sister sections had formed in Strasbourg, Lyons, Bordeaux, Montpellier, Nantes, Bayonne, Grenoble, Dijon, Marseilles, Castres, Douai, Nîmes, and a number of other towns.[65]

By 1785, however, a split had occurred and Mesmer had expelled Bergasse, Kornmann, and other prominent members of the organization. Although no longer willing to serve as Mesmer's lieutenant, Bergasse's dedication to the theory and practice of animal magnetism was undiminished. He and Kornmann rallied their supporters and quickly set up a rival organization. Bergasse, again, was its principal leader, but since it was headquartered at Kornmann's home it became known as the "Kornmann group."

The split between Mesmer and the Kornmann group reflected the political tensions of prerevolutionary French society. Bergasse and Kornmann wanted to "extend the movement's original fight against the 'despotism of the academies' . . . into the larger battle against political despotism."[66] Mesmer, however, had no interest in alienating Parisian high society; his insistence that the movement remain apolitical made a parting of the ways inevitable.

While the new group was unable to mount much of an organizational challenge to the mainstream Mesmerist movement, it attracted the most politicized elements away from the Universal Harmony Society and soon emerged as an

important focus of revolutionary activity. "The Kornmann group," Darnton writes,

> represents the culmination of mesmerism's movement into politics . . . . The resistance to the government was led in the Parlement by d'Eprémesnil and Duport, in the Notables by Lafayette, at the Bourse by Clavière, and among the reading public by Brissot, Carra, Gorsas, and Bergasse.[67]

"By the time of its greatest activity, 1787-1789," Darnton explains, the Kornmann group "had neglected mesmerism in order to devote itself fully to the political crisis."[68] Its members successfully

> rallied popular support for the Parlement's opposition to the programs of the Calonne and Brienne ministries and the Parlement's call for the convocation of the Estates General.[69]

Jacques Pierre Brissot, who had joined the Kornmann group in the summer of 1785, later advanced the claim that Bergasse's advocacy of animal magnetism was primarily a cover for radical politics:

> Bergasse did not hide from me the fact that in raising an altar to mesmerism, he intended only to raise one to liberty.
>
> Brissot quoted Bergasse as saying:
>
> It is necessary to unite men under the pretext of experiments in physics, but, in reality, for the overthrow of despotism.[70]

Darnton warns that these recollections of Brissot's cannot be taken at face value since Brissot, writing while facing charges of counterrevolutionary conspiracy, had good reason to portray his past as completely devoted to the Revolution.

Brissot's testimony would suggest that Bergasse's adherence to Mesmerism was merely tactical, a pretext for revolutionary organizing. It is more likely that Bergasse perceived the two as intertwined means to a single end. Mesmerism was the expression of a Rousseau-inspired natural philosophy that provided an ideological basis and a social theory for revolutionary action.

Bergasse rose to national political prominence in 1787 as a lawyer in a scandalous high-society divorce case involving his friend Kornmann. Bergasse represented Kornmann, who had charged his wife with infidelity in a spicy affair involving police lieutenant-general Lenoir and Beaumarchais, the celebrated author of *The Marriage of Figaro*. In the course of the highly publicized proceedings, Bergasse's attack went well beyond Madame Kornmann and her attorneys to include a charge of moral terpitude against the royal government and, echoing Rousseau, against *ancien régime* society as a whole.

Although he eventually lost the divorce case, Bergasse's great talents as an orator and a pamphleteer had made him a hero to opponents of the royal regime. He had championed the Parisian Parlement's battle against the king's ministers; when that struggle resulted in the calling of the Estates General, Bergasse emerged as one of its leaders.

To appreciate Bergasse's complicated trajectory in the French Revolution it is necessary to comprehend the

historical phenomenon known as the "aristocratic rebellion." The French monarchy, in the 1780s, was in the grip of a deepening fiscal crisis. The crown's frantic search for new sources of revenue forced it to turn to the aristocracy, which had traditionally enjoyed extensive exemption from taxation.

In spite of their privileged social position, the aristocrats had long been excluded as a class from a direct political role in governing the country. The threat to their economic interests prompted them to begin agitating for political rights as a means of self defense. The Parlement's challenge succeeded in shaking the monarchy, but in doing so it opened the door for other social layers to begin raising demands as well. The great irony of 1789 is that the social class that initiated the Revolution was the one that was ultimately destroyed by it.[71]

The case of Nicolas Bergasse illustrates perhaps best of all that the radical social and political implications of speculative natural philosophy cannot be linked to any specific brand of radicalism such as Jacobinism. Bergasse can hardly be considered even an '89er; he turned against the Revolution at the first hints of its democratic character.

Bergasse withdrew from the National Assembly before the end of 1789 and was soon in the direct service of the crown. After the ex-king's execution he withdrew from

political activity altogether. He was arrested during the period of Jacobin rule but by chance survived.

Bergasse's transformation from revolutionary to counterrevolutionary was rapid, but his reactionary tendencies continued to deepen with age. During the First Empire he resurfaced as a champion of the demand that the property confiscated from the nobles during the Revolution be restored to them. He became a fervent supporter of "the sacred principle of legitimacy" according to which the historic dynasties of Europe were the only "legitimate" rulers of their respective lands. That included the Bourbons, of course, and with their restoration to the throne of France, Bergasse himself regained a modicum of influence.

Bergasse's political resurrection was due in large part to his relationship with another "legitimate" ruler, Tsar Alexander I of Russia. After marching into Paris at the head of his triumphant armies in 1815, the Tsar visited Bergasse at the behest of a famous mystic, the Baronne de Krüdener, and recognized in him an ideological soulmate. Alexander and Bergasse shared an obsessive fear of the return of revolution throughout Europe, an obsession that was by no means purely delusional.

> The Revolution, uneasily buried, was far from dead. Beneath the surface of repression, police and censorship, its believers were burrowing their shafts, meeting, talking, planning: secret societies and conspiracies flourished.[72]

Alexander was a mystic with messianic ambitions; he spearheaded the formation of a Holy Alliance of the thrones of Russia, Prussia, and Austria as a "Christian answer" to the French Revolution. Bergasse, as the Tsar's confidant and spiritual advisor, contributed to shaping the ideology and policies of that counterrevolutionary crusade.

It was not this older, reactionary Bergasse who composed a grand synthesis of the laws of nature, but rather the Bergasse of the 1780s who was still in his early thirties and full of revolutionary optimism. His system was perhaps the most influential of all since it had the force of an organized movement -- the Mesmerist movement -- behind it.

Mesmer's Animal Magnetism

Bergasse's contributions to scientific culture cannot be appreciated in isolation from the remarkable social phenomenon that Mesmerism represented. The theory and practice of animal magnetism, a pure product of natural philosophy, took Paris by storm in the early 1780s. The interest it generated among the upper classes obliged the Academy of Sciences and the Royal Society of Medicine, after some hesitation, to give it a respectful hearing. The majority of the commissioners appointed by the royal government to evaluate it -- including Lavoisier, Franklin, Le Roy, and Bailly -- decided that Mesmerism had no scientific basis.[73]

Lavoisier's bibliographers, however, have demonstrated that some of the royal commissioners approached the task with less than open minds:

> The investigation of animal magnetism thus started with two of its chief members, Franklin and Lavoisier, having to some extent already passed judgement. The results obtained by the commissioners along these lines were exactly what Lavoisier had foreseen.[74]

Indeed, the commission's final report acknowledged that its members, before undertaking their investigation, had "armed themselves with that philosophic doubt which ought always to accompany enquiry." Thus armed, they "felt none of those sensations, which were experienced by the three patients of the lower class." Their conclusion that those patients' sensations were "the fruit of anticipated persuasion," then, might equally apply to their own lack of perceived sensations.[75]

The commission's report was not, however, a purely subjective dismissal of animal magnetism. Its success in countering the Mesmerists' influence derived from its experiments with blindfolded patients who behaved as if magnetized when they had not been and behaved as if not magnetized when they had been. The claimed effects of animal magnetism, then, were attributed to the power of suggestion upon the patients' imaginations.

Bergasse's Reply to the Royal Commission

Although the commission's report constituted a completely convincing exposé of charlatanism to many

influential opinion-makers, many others remained just as completely convinced of the validity of Mesmerism. The conflict of opinion was based upon two fundamentally opposed natural philosophies. Bergasse's *Considérations sur le Magnétisme animal, ou Sur la théorie du monde et des êtres organisés* was both an exposition of the natural philosophy of Mesmerism and a defense against the attack of the royal commission.

The commission's report had been issued in August 1784; Bergasse's foreword is dated 15 October, 1784. Although he did not explicitly state that the hostile critique had prompted him to rush an answer into print, he apologized for the haste with which it was produced. Since he had already lectured extensively on the subject, however, it was probably just a matter of putting his lecture notes into publishable form.

While his primary attention was directed against Mesmerism's external enemies, he lamented the "clash of opinions" and "partisan spirit" that had arisen within the movement in the previous months.[76] The internal struggle would break into open warfare a month later and the following summer Bergasse and his faction would be formally expelled. All of the distractions prevented him from being as thorough as he would have liked:

> I have thrown several new ideas together in haste in this work; someday, if I can find myself in tranquil circumstances, I will assemble these ideas and many others that I think are equally novel into a single system. Then it will perhaps be seen that it is

> possible to base the entire order of our knowledge upon a single principle and fact.

Such a "theory of the world," he added, would "provide our legislation, up until now so uncertain, with a solid base."[77] The single principle and fact that lies at the base of the system is the universal magnetism whose discovery Bergasse dutifully attributed to Mesmer, but the system as a whole is Bergasse's creation.

Bergasse also apologized to his readers for only being able to give a general outline of the system because he was bound by his word of honor to Mesmer not to reveal the specific details of the latter's discovery.[78] In spite of this consideration, when the split occurred Mesmer blasted Bergasse in public print for revealing secrets he had sworn to conceal.[79]

Bergasse had originally been attracted to Mesmerism by the therapeutic benefits it offered. Having been "a victim of doctors myself from infancy," and "noticing how their disastrous art has been a terrible scourge," Bergasse was overjoyed when, in 1781, he finally found relief at the hands of those practicing Mesmer's doctrines.[80]

But Mesmerism soon became for Bergasse much more than a medical theory. Its truth was confirmed by its cures, but could only be guaranteed by fully understanding its place in the lawful operations of nature.

Bergasse was especially anxious to refute those representatives of orthodox science who, like Lavoisier and Franklin, had attempted to demonstrate that Mesmerism was

unscientific. He explicitly rejected any imputation of mystical or occult knowledge and insisted upon the scientific truth and demonstrability of animal magnetism, which "has wrought a revolution in our entire system of physical knowledge."

> One feels that a doctrine that has as its object the theory of the world and the knowledge of the relationships of the individual human economy with the general economy of all beings, which demonstrates not only the truth of these relationships but in fact can point out the means (or the *mechanism*, if I may be permitted to use that word) -- one feels that such a doctrine must act in the still too little understood regions of advanced physics.[81]

"This is a profound revolution," he added, "in the domain of the natural sciences."[82] The system as a whole is presented as deducible from fundamental axioms of Newtonian physics:

> If there is one truth that is no longer challenged in physics, it is that all objects moving through space at some distance exert a mutual force on each other, that this force is stronger or weaker according to how close or far they are apart and to how more or less considerable their mass is. . . . Of all of nature's forces, this one is the most profound, invariable, and universal.
>
> Now, nature's most profound, invariable, and universal force must be that force through which nature develops, maintains, and conserves all beings.[83]

> The existence of these forces between beings necessarily implies that there exists between them a milieu or a medium that reciprocally transmits their forces. Certainly it is impossible to conceive of the action of one object upon another, whether close together or far apart, through an absolutely empty space. . . . How can a void (which is to say: "that which does not exist") transmit an action, a force, or a movement?[84]

The preceding passage reveals that Bergasse's outlook, at this stage of his life, had much more in common with post-Cartesian mechanical materialism than with the mysticism that would later attract Madame de Krüdener and the Tsar of all the Russias to his hearth. Returning to the overview of his system:

> The beings that populate the universe exist, therefore, in a common medium that receives all of their impressions and transmits them from one to another.
>
> But this medium can be nothing other than a fluid, and the most subtle of all fluids at that. If it were not a fluid, the medium between objects would be solid and the world would be nothing more than one huge solid wherein nothing could move.[85]

This fluid must be the most subtle of all fluids since it is the instrument and executor of all of nature's laws, from "that which determines the harmonious march of the celestial spheres" to "that which carries sensations and life to the most insignificant insect."[86] To be able to do this the fluid must penetrate all objects with ease and therefore must be exceedingly subtle.

The uniformity of natural law must be accepted as a fundamental truth. If causes simply produced their effects randomly, "we would see all around us nothing but disorder, confusion, and sterility."

> If the largest and smallest objects, from the celestial spheres to the creatures that inhabit their surfaces, all obey a uniform gravitational law, they must all therefore share a common property. . . .
>
> Now, how do the celestial spheres affect each other? Absolutely like two magnets that are brought into each other's presence. The closer two magnets are brought together, the more magnetic fluid flows from the poles of one and impetuously enters the poles of

> the other, and the stronger the attraction between the two magnets becomes.[87]

The same is true of two planets. The closer they are to each other, the more fluid flows between them and their attraction increases. Planets, like magnets, have poles. Mutual attraction implies fluid flow, and fluid flow implies points of introduction, or *poles*.

> Otherwise, what would happen? Contrary to observation, it would be impossible for the heavenly bodies to attract each other.[88]

Since all bodies gravitate toward each other, and since this gravitation operates by means of a fluid, it is absolutely necessary that poles exist in all bodies to receive and recycle that fluid. "Therefore all organic bodies, including all living beings and the human body itself, have poles."[89]

> Notice here an idea that is as far-reaching as it is profound. . . . In spite of all the efforts of Leibniz, Locke, Condillac, and others, there still was no theory of sensations that could be directly linked up with the general movement of the whole universe. Now, however, it is easy to see how a unified law can produce everything.[90]

The mutual influences that all beings have upon each other are effected by means of a fluid transmitting universal magnetism. Like all of nature's fluids, it tends toward the restoration of equilibrium, an aid to the maintenance, development, and conservation of organisms.

The forces that interact among organisms, however, are stronger or weaker depending upon how "analogous" their organic structures are. The animal magnetism between a dog and a cat, for example, would be greater than that between

a dog and a mosquito. The animal magnetism between members of the same species is strongest of all.

> Since it is through magnetism that all beings are conserved, and since they act magnetically on each other the more analogous they are to each other, the laws of their conservation can be discovered by studying the laws of magnetism and the analogies among organisms. The means necessary to restore them when their organization has been altered can thereby be determined with some certainty.

It is from this study, and this study alone, then, that "the true art of healing" can be derived.[91] Mesmerist therapy consisted in channeling the universal magnetism through the patient's poles in order to restore whatever internal imbalance had resulted in discomfort, pain, and illness.

Bergasse explained the theory by means of an analogy with a river flowing peacefully and tranquilly in its natural state. A bridge is built across the river, however, and loose gravel begins to accumulate around one of the bridge's supports in the water. A blockage develops that causes resistance to the river's natural flow.

> The profound and tranquil river represents the universal fluid obeying the harmonious and powerful forces of nature. The bridge represents an organic being, penetrated by the universal fluid. The gravel represents an illness.[92]

The aim of therapy, then, is to concentrate the natural forces of the flow of universal magnetism in order to sweep away the unnatural blockage. Specific therapeutic methods were not revealed by Bergasse; that was apparently precluded by the terms of his agreement with Mesmer.

Often, though not always, the magnetic power was directed into a patient by a laying on of hands. The patient would often report feeling an intense heat in an afflicted part of the body; in many cases patients trembled violently, went into trances, suffered seizures, or fainted dead away. These dramatic "crises" signalled the power of the magnetic force fulfilling its curative function.

Mesmerist Social Theory

To this point, Bergasse's exposition followed the general lines of Mesmer's teachings. Bergasse, however, went further and added a social dimension. The extension of Mesmerism from physical science into the realm of moral science was Bergasse's contribution.

The royal commission's report had warned the King that Mesmerism could entail dangerous social consequences. Noting that magnetizers were proficient at manipulating the imaginations of excitable subjects and stirring up their passions, it warned:

> The same cause is deeply concerned in rebellions; the multitude are governed by the imagination; the individuals in a numerous assembly are more subjected to their senses and less capable of submitting to the dictates of reason.[93]

The commissioners also worried about the moral implications of male practitioners laying their hands on excitable females and inducing what could only be described as orgasmic responses. This concern was omitted from the public report for reasons of propriety, but was

communicated to the King's ministers in a secret memorandum. Nonetheless, it was a frequent accusation raised against the Mesmerists, and Bergasse was well aware of it.

"It has often been said," Bergasse responded, "that M. Mesmer's doctrine is destructive of morals." But if animal magnetism and our morality are incompatible, "and yet animal magnetism is *a physically true* doctrine, then it follows that our morality must be false."[94]

Like Rousseau, Bergasse believed that civilized society had tended to lead human nature into depravity.[95] Modern technology had produced too many frivolous diversions, leading to a moral superficiality that renders people indifferent to each other. The plethora of artificial "needs" had whipped people into a frenzy, driving themselves and climbing all over each other to get things they only think they need. Moral breakdown results from the selfishness and envy that destroys human solidarity.

> Morals are good when the relationships among people are arranged such that the attractions and habits they produce render the solidarity among them more intimate and accustom them not to consider their own interests separately from the general interest.[96]

A social organism is analogous to an animal organism: moral corruption is to society what disease is to the human body. The difference between moral and immoral societies, then, is a *physical* one. That, too, results from a lack of equilibrium of the universal magnetism. Nature alone cannot

restore a corrupt society to moral health; nature's energy must be channeled and amplified.

Far from being destructive of morality, Bergasse concluded, the universal practice of Mesmer's doctrines is the only way a much-needed reform of morals could possibly be realized. By emphasizing the need to organize energy to sweep away social obstacles, this line of argument contains broad hints of action for radical social change, but Bergasse left it vague and abstract.

Bergasse's immediate concern was to refute the royal commission's conclusion that since the effects attributed to animal magnetism could be explained by suggestion, there is no evidence for the existence of animal magnetism. First of all, he said, there is no denying the reality of the power of suggestion, but the commissioners had gotten things backwards. "Suggestion" is not an explanation; it is precisely that which must be explained. Why is it, for example, that when one is in the midst of an emotional crowd it is difficult to resist sharing the crowd's emotion? This is a case of suggestion, to be sure, but is it not obviously a product of the overwhelming impact of the crowd's collective animal magnetism?

Along the same lines, Bergasse asks why it is

> that people who live in the same society are disposed toward receiving, as if involuntarily, the same opinions, the same prejudices, the same habits?[97]

The consideration of "involuntary opinions" could certainly have opened up a new and fertile field of

research had it been pursued further. In any case, Bergasse's epistemological point was well taken: attributing physical effects to suggestion raises more questions than it answers.

The physical proof of Mesmer's doctrines, Bergasse contended, lies in the many, many irrefutable cures that his methods had accomplished. These cannot all be attributed to suggestion and imagination. Although our imagination can certainly affect our state of health, more often than not it makes us feel worse, not better, because we tend more toward fear than toward hope. Furthermore, a significant category of Mesmerist cures cannot possibly have been brought about by imagination:

> Among all the scourges that afflict, deprave, and torment the human species, it seems to me that a large number of them are very little affected by the imagination. Teach me, if you will, how the imagination can cure blindness, deafness, wounds, or local paralysis.[98]

Finally, it is obvious that cures of infants and animals cannot be explained by imagination.

Bergasse's arguments are not those of a naïve and credulous person, but rather of one unwilling to accept reductionist explanations of complex phenomena. The royal commission's experiments with blindfolds were very clever, but the only explanation the commissioners could offer for cures that Bergasse had witnessed was to declare them fraudulent. From his point of view, to forego the immense

benefits of this science on the grounds of a sterile scepticism would simply be criminal.

Although widespread interest in animal magnetism made it a focus of a socially significant movement well into the nineteenth century, after the Revolution it was treated only with derision in official scientific institutions and dismissed out of hand as pseudoscience.

The words "Mesmerism" and "animal magnetism" today still evoke smiles and expressions of wonderment at the widespread gullibility they represented. When this condescending reflex reaction is set aside, however, a certain historical injustice is recognizable. The movement's initial impact and its staying power were based on the ability of Mesmerist practitioners to induce seizures in their patients or to put them in trances. The challenge for science was to explain those powerful effects. When the orthodox scientists determined to their satisfaction that no external physical force was responsible for them, they concluded that the phenomena of animal magnetism were unreal and Mesmerism was fraudulent.

After almost two centuries, leading historians of science continue to echo that judgment. Gillispie faults "the scientific community" for "its maladresse in handling Mesmer":

> At first, in refusing to examine his claims, it took the line that what everyone was tremendously interested in was not worth the attention of serious people, *which was true* but not tactful.[99]

Is it really true that the unprecedented effects demonstrated by the Mesmerists were unworthy of serious consideration? To the contrary, the academicians' myopia in this affair represented a monumental missed opportunity for the extension and advance of science.

As some partisans of animal magnetism argued, the inability to detect a cause cannot be grounds for absolutely ruling out the reality of a perceived effect. In the case of Mesmerism in particular, the absence of an external physical cause should not have ended the investigation of effects occurring in human subjects. The fact that Mesmer himself believed animal magnetism to be an external physical force did not absolve science of a responsibility to look deeper.

Some of the scientists who opposed Mesmer's claims dismissed the effects of animal magnetism as imaginary, by which they meant unreal, nonexisting, or at least beyond the province of scientific investigation. The effects produced by the Mesmerists upon their patients' imaginations, however, were real, existing phenomena.

> Justly it can be said that Mesmer's discovery of animal magnetism was a pivotal moment in the evolution of modern psychology and psychotherapy. It led to Puysegur's investigation of the consciousness manifested in magnetic sleep and the eventual discovery of a subconscious realm of mental activity.[100]

It would be anachronistic in the extreme to fault the academicians for failing to perceive the problem in modern psychological terms, but the point is that their

epistemological blinders prevented them from even acknowledging the existence of an important class of phenomena. The Mesmerist natural philosophy stimulated the development of a science of the mind; the scepticism of the orthodox academicians once again was a retarding influence.

CHAPTER FIVE

BERNARDIN DE SAINT-PIERRE AND RESTIF DE LA BRETONNE

V. JACQUES HENRI BERNARDIN DE SAINT-PIERRE (1737-1814)

Bernardin de Saint-Pierre was Jean-Jacques Rousseau's closest spiritual heir and is today regarded as a major figure in French literature; his novel *Paul et Virginie* has become a classic familiar to French schoolchildren. It has been suggested that a study of Bernardin de Saint-Pierre's words and deeds during the Revolution might provide insight into the perennial issue of how Rousseau would have reacted had he lived to participate in the events of 1789 and 1793.[1] Bernardin met Jean-Jacques in 1772, when the latter was sixty years old. They botanized together on longs walks on the outskirts of Paris; their close relationship lasted until Rousseau's death in 1778.

Although sufficiently prominent as a man of letters to win admission to the Académie Français, Bernardin de Saint-Pierre was equally well known to his contemporaries as a man of science. His monumental work of natural philosophy, *Etudes de la nature* (1784), received widespread public acclaim. In 1792, at the height of the Revolution, he was appointed Intendant of the Jardin des Plantes, an important administrative position that Buffon had developed into one of the most prestigious in the world of Parisian science. Although Bernardin's tenure was brief and unsuccessful, his

statue sits today in the Jardin des Plantes in acknowledgement of his general contributions to the botanical sciences. It has him seated, deep in contemplation, above a depiction of the arcadian innocents of his creation, Paul and Virginia.

Bernardin's relationship with orthodox science before and after the Revolution was particularly bitter. The academicians were alienated by the anthropocentrism of his *Etudes de la nature* and gave it the cold shoulder. Bernardin in turn wielded his mighty pen against them; his were "the most devastating of public utterances belittling the academic system."[2] The Academy of Sciences, he charged, was a closed corporation dedicated not to science but to preserving its members' privileges. He further distanced himself from mainstream science by challenging the Newtonian interpretation of data regarding the shape of the earth; our planet, he claimed, is not flattened at the poles but elongated.

A famous anecdote has it that Bernardin, in an interview with the Emperor, demanded to know why the scientific establishment refused to take him seriously. Bonaparte asked him if he was proficient in the differential calculus. When Bernardin acknowledged that he was not, the Emperor replied: "Then go learn it and your question will answer itself."[3] This tale is usually told to illustrate Bernardin's scientific irrelevance, but it illustrates equally well the role of the state's influence

in developing the hegemony of a narrow type of orthodox science. The refusal to take Bernardin seriously meant, among other things, rejecting his approach of studying natural phenomena in environmental context, a narrowness of scope for which science was to pay a heavy price. Napoleon's put-down of Bernardin suggested that as far as the pinnacle of state power was concerned, the only science that counted was mathematically oriented science.

After the Revolution Bernardin continued to gather material for another multivolume work of natural philosophy, which was to be published after his death as *Harmonies de la nature*. His posthumous reputation was considerably distorted by an adoring disciple, Aimé Martin, who sought to defend his master in conservative times by editing all of his writings to cleanse them of their anticlericalism and other radical implications. Martin was a legitimist before the Restoration; he integrated his own political and religious convictions into Bernardin's works.[4] Only the earlier, unbowdlerized editions of *Etudes de la nature*, then, can be considered trustworthy representatives of Bernardin's thought. The extent to which Aimé Martin considered himself Bernardin's continuator is indicated by his marriage to Bernardin's widow.

Like other works of unorthodox science of the same period, *Etudes de la nature* has been treated harshly by most modern commentators. Arthur Lovejoy cited it as a masterpiece of the teleological and anthropocentric genre

that stands as "one of the most curious monuments of human imbecility."[5] Nonetheless, the dismissal of Bernardin's work as scientifically worthless is unjustified, as will be shown below. As for its historical interest, if a Rousseauian or preromantic approach to science can be said to have existed at the time of the Revolution, it is most likely to be found with Bernardin in its purest form.

The connection between Bernardin's literature, science, and social theories is evidenced by the fact that his most famous novel, *Paul et Virginie*, was originally published as part of *Etudes de la nature*.[6] *Paul et Virginie* is a tale of an unspoiled boy and girl growing up on an idyllic island away from the corrupting influence of civilization. Thomas Carlyle said it was "read eagerly by all the world," and interpreted its message as: "Everywhere wholesome nature is in unequal conflict with diseased perfidious Art." Carlyle mistakenly characterized *Paul et Virginie* as "the swan song of old dying France"; it was much less a death rattle *of* the old regime than a Rousseauian death warrant *for* it.[7]

Bernardin's reputation as a scientist is succinctly stated in this description of "contemporaneous opinion" with regard to his scientific writings:

> There was a striking unanimity as to the value of his scientific or *soi-disant* scientific pronouncements and deductions, particularly the latter. The universal opinion was that with scarcely a single exception they are grotesquely wrong.[8]

This assessment, however, amounts to a considerable distortion. It would suggest that *Etudes de la nature* was not widely read and appreciated, which was far from the truth. A commentator writing in English in 1893 explained that although *Etudes de la nature* had been for the most part forgotten by that time,

> our grandparents read them greedily enough either in the original or in the excellent translation of Dr. Henry Hunter.[9]

In fact, *Etudes de la nature* was translated into several languages and attained considerable international popularity. The author who believed Bernardin's science had been completely unappreciated by his contemporaries fell into the common error of identifying "universal opinion" with that of the scientific elite. It was only the latter that with "striking unanimity" deemed Bernardin's scientific work worthless.

The journals of the scientific elite focused on two obvious but relatively insignificant errors as a means of discrediting the *Etudes de la nature* as a whole. The fact that orthodox scientists felt the need to do so was a reflection of the work's positive reception among the general reading public. Bernardin took the bait, however, and continued to vociferously defend his erroneous positions, thus further damaging his reputation among professional scientists. The two errors, which will be discussed more thoroughly below, involved classical

Newtonian problems: the cause of the tides and the shape of the earth.

The scientific elite, however, was guilty of a far more significant error in failing to recognize the potential value of *Etudes de la nature*. By choosing to ignore most of the work's scientific content, the defenders of scientific orthodoxy stifled an innovative line of inquiry in the life sciences that would only much later independently resurface and prove its worth.

Bernardin's Attempted Social Experiments

Bernardin had embraced Rousseau's philosophy long before the two men met. Believing as Rousseau did that human beings are born good but corrupted by society, the young Bernardin not only conceived utopian social schemes, but actually attempted to put them into practice. He traveled to Russia to request the assistance of Peter III in establishing an ideal republic on an island, but by the time he arrived the revolution of 1762 had brought Catherine to the throne and he found no sympathetic ear at her court.

In 1768 Bernardin set sail for the Ile de France (now Mauritius) five hundred miles east of Madagascar, with hopes of founding a state based on liberty and virtue, but was deeply disillusioned when he discovered that the officials he had counted upon for assistance in this endeavor were in fact engaged in the slave trade.

Upon his return to France he published a narrative of his two-year experience as *Voyage à l'Isle de France*. Although the book had a political argument to make -- that the colony was of little strategic value to France in protecting trade routes to India -- its picturesque descriptions of nature were what appealed most to the reading public and won its author a measure of literary success.

The infelicitous results of his practical attempts at social engineering apparently led Bernardin to conclude that he could best contribute to a Rousseauian reform of morals by means of his pen. He withdrew from governmental administration to devote his full attention to science and literature. The continuing integration of his scientific and social concerns is evident in the last two of the three volumes of *Etudes de la nature*, which were largely devoted to the elaboration of political and social reforms.

Politically, Bernardin was "a democrat, and rather an advanced one for the day for which he was writing."[10] His literary prominence, however, had earned him a royal pension, which influenced the political stance he took during the Revolution. He described his own ambivalent position during those turbulent times:

> Had I not been prevented by indisposition, I should still have been at a loss what side to take. I was bound by duty to the people, and by gratitude to the King, whose bounty had been for twelve years my support.[11]

Even after the political polarization had been radically exacerbated by the King's abortive flight to Varennes, Bernardin was still trying to preach moderation to both sides.

Bernardin continued, however, to inveigh against the injustices and cruelties of colonialism that he had witnessed at first hand. In *Etudes de la nature* he condemned European oppression of the rest of the globe:

> European ships . . . carry death and destruction to the peaceful nations of the south . . . The produce of our fields and our forests sail the seas for the purpose of desolating both hemispheres.[12]

Ecology and Morality

The reference to fields and forests -- as sources of hemp and timber for shipbuilding -- underscores the moral content of Bernardin's ecological scientific point of view: corrupt societies, he charged, turn the goodness of nature's bounty to evil purposes. In a particularly Rousseauian passage, Bernardin lamented: "The history of nature offers only blessings, that of man nothing but crime and rage."[13]

The criminality he deplored extended beyond colonialism's inhumanity of man to man to the destruction of nature itself:

> Our treatises on agriculture show us nothing in the fields of Ceres but sacks of grain; in the meadows of the nymphs, nothing but haystacks; and in the majestic forests, nothing but logpiles and bundles of kindling wood. What crimes pride and avarice have committed against nature! How many charming hills debased by our

> laws; how many majestic rivers reduced to slavery by our tariffs![14]

Bernardin's ecological approach to natural history led him to conclude that large-scale agricultural practices and industrial activity were detrimental to the natural environment that sustains human life. This was not a scientific ideology that the Napoleonic state and its elite scientific institutions were likely to find congenial. Bernardin's science appeared incompatible with economic progress. The orthodox scientists did not mount an attack on Bernardin's environmental ethic, however; they ignored it and instead focused on some of his more vulnerable positions.

Clarence J. Glacken, in his seminal study of the history of environmental thought, *Traces on the Rhodian Shore*, emphasized an important aspect of European science that historians have frequently left out of account:

> It is wrong to say that the Western tradition has emphasized the contrast between man and nature without adding that it has also emphasized the union of the two.[15]

Glacken listed Bernardin de Saint-Pierre with Linnaeus and Buffon as among the greatest of the eighteenth-century natural historians in whose hands organic themes flourished.[16] The success of Bernardin's elite critics in suppressing his reputation, however, is reflected in the fact that Bernardin receives no mention at all in Donald Worster's important recent history of ecology.[17]

Glacken's assessment is not based only on Bernardin's carefulness and thoroughness as an observer, but on the fact that he was among the first to insist that the proper study of nature is the interconnections between organisms and their environment. Bernardin began *Etudes de la nature* by explaining that he had started out several years earlier to write a "general history of nature, in the manner of Aristotle, Pliny, or Bacon."[18] He quickly discovered, however, that "not only the general history of nature, but even that of the smallest plant, is well beyond my ability."[19]

He had reached that conclusion by observing a strawberry plant in his window. To each of the enormous variety of tiny flies that visited it, it presented a different aspect. How could he ever hope to comprehend the plant from their perspective, or from that of the microscopic animals to which the strawberry plant was a world in itself?

> But even if, like them, I could have acquired an intimate knowledge of this new world, I still could not hope to know its history. It would have been necessary to study its relationships with the rest of nature, with the sun that makes it flourish, with the winds that spread its seeds, and the streams whose banks it fortifies and embellishes.[20]

Bernardin also recognized that no science of the strawberry plant could be complete without comprehending the environmental factors accounting for its remarkable geographic distribution:

> It would have been necessary to discover . . . how a fragile creeping plant could . . . spread over the globe from north to south, from mountain to mountain, . . . how it could have extended from the mountains of Kashmir to Archangel, and from the peaks of Norway to Kamchatka; and how, finally, it should be found in both Americas, even though an infinite number of animals make war on it, and no gardener bothers to sow its seeds.[21]

"Everything in nature is linked in a single chain," he declared.[22] One of the primary goals of botany should be to discover the factors that prompted nature to vary each plant's form

> and to create so many species of the same genus, and so many varieties of the same species, by coming to understand the wonderful interrelationships they have, in every latitude, with the sun, the winds, the waters, and the soil.[23]

A most important part of an organism's environment, of course, consists of the other living organisms surrounding it.

> If it now be considered that all these species, varieties, analogues, and affinities, in every latitude, have necessary relationships with a multitude of animals, and that we are completely ignorant of these relationships, it becomes obvious that the complete history of the strawberry plant would be enough to keep all the naturalists in the world busy.[24]

Even if we were acquainted in infinite detail with the parts of which plants are composed, he said, it would be a "useless science"; it is "the harmonies that they form" that are of real interest.[25] Bernardin qualified this harsh assessment of analytic/reductionist science by saying he "did not claim that no useful discovery has ever resulted from these partial methods," but he believed it very wrong

to wrap up all of science in them "and to reject in bad faith all that cannot be comprehended by them."[26]

As for the program of ecological investigation that he had proposed, "I do not know of anything of this sort that has ever been tried before."[27] Such a program could never be completed, but it was important to at least begin. Bernardin's *Etudes de la nature* represents an admirable beginning. His impressive erudition and his ecological insights equipped him to make an innovative and potentially important contribution to natural science. The contemporary scientific establishment, however, failed to recognize its value and chose instead to dismiss it with ridicule.

Extreme Providentialism

The ridicule was directed first of all against the anthropocentric framework that gave *Etudes de la nature* its structure. For although Bernardin disavowed reductionist systems based on single laws of nature, he too had ultimately produced a system of the universe founded on the single principle of providence. The supreme author of nature, Bernardin believed, had made nothing in vain, and nothing in nature exists other than that which is useful to humankind.

Providentialism was not uncommon in the late eighteenth century, even in the writings of respectable orthodox naturalists, but most merely tipped their hats to the concept by attributing to it such major benefits as

wheat or sunlight or air. Bernardin's providentialism was distinctive for its merciless consistency; he insisted that literally everything could be explained as gifts of a beneficial creator.

In the century of Voltaire this could be expected to invite ridicule, expecially considering the extremes to which Bernardin went. Volcanoes serve to purify the world's waters; earthquakes purify the atmosphere. Scorpions are useful because they frighten us away from damp and unhealthy places. The death of pets helps children learn to deal with grief. Cherries and plums are made to fit the human mouth; apples and pears to fit human hands. The melon is divided into quarters to be easily shared at family dinners; the pumpkin's huge size makes it a social fruit, designed to be shared with neighbors. Fleas are useful because they oblige the rich to give employment to the poor as domestics to keep their homes and clothes clean. There are no useless pests or harmful weeds in Bernardin's nature.

Humans are not the only beings that receive the benefits of providence; thorns, for example, protect birds from predators. But animals and plants are only protected insofar as they are ultimately useful to humankind; only humans are taken care of for their own sake.

Etudes de la nature is formed on the plan of systematic responses to objections against the existence of providence. His espousal of final causes in nature makes a

sharp contrast to the work of Buffon, who devoted much effort to denying final causes. Yet as Glacken points out, in the eighteenth century naturalists could operate productively "within a teleological framework or outside of it."[28]

To his detractors, the image of a melon designed for families became a favorite symbol of Bernardin's folly. Like Lamarck and the giraffe, Bernardin and the melon would be forever linked in the eyes of posterity.

Geophysical Controversies

Bernardin's critics, however, also targeted two prominent errors in *Etudes de la nature* as a means of discrediting the entire work. Their jibes prodded Bernardin to respond at length in the introduction to the second edition of *Etudes de la nature*. Never one to back away from an argument, Bernardin vigorously continued to insist that he was right on both points.

The first issue was the cause of the oceans' tides. In the early seventeenth century it had been possible for Galileo to believe that the tides resulted from the earth's rotation; by the end of that century the gravitational attraction of the moon was generally recognized as the dominant influence. Bernardin, late in the eighteenth century, disputed that explanation and instead attributed the flux and reflux of the sea to the partial melting and refreezing of the ice at the north and south poles. This

"alternate fusion of the polar ices" occurs on both an annual cycle (due to the heat differential between winter and summer) and on a daily cycle (due to the heat differential between night and day). Bernardin denied that the "astronomical attraction" of the moon on the equatorial waters is an important factor, because it "does not produce the slightest effect on the mediterranean seas nor even on the lakes of the torrid zone."[29]

To the Newtonians who scoffed at his theory, Bernardin replied that Newton himself had acknowledged the action of "some other unknown mixed cause" in the "periodic return of the tides."[30] This attributes a level of ambiguity to Newton's analysis that is not warranted. If any of Newton's defenders, however, were careless enough to suggest that lunar gravity alone could account for all tidal behavior, Bernardin was justified in pointing out their error. Although his alternative theory was widely perceived as erroneous, it was not universally condemned; he thanked Du Pont de Nemours, for example, for declaring his support.[31]

Bernardin's other contested assertion, having to do with the shape of the earth, was an extension of his theory of ocean currents and tides. The melting of ice masses *per se* would not impart a directional motion to the waters of the earth. "A multitude of meteorological, nautical, vegetable, and animal observations" had convinced him "that the waters of the polar ices are naturally impelled toward

the equator," but why?[32] The cause, he decided, was the gravitational force that makes water run downhill. Since the earth is slightly elongated at the poles, the water of newly-melted ice would naturally flow toward the equator.

Again, Bernardin had challenged a key element of the Newtonian world picture. Newton's theory of universal gravitation clearly required that the earth be flattened, not elongated, at the poles.

Whether Newton had been right or wrong on this question had been a hotly debated scientific issue in the earlier part of the eighteenth century. By the time *Etudes de la nature* appeared in 1784, however, it had generally been considered to have been resolved in Newton's favor.

Newton had deduced the flattened earth from the centrifugal force of its rotation on its axis. He sought to support his hypothesis with physical evidence, however, by citing the fact that a degree of latitude measured in England was found to be slightly longer than a degree measured in France. If the earth is indeed flattened, he reasoned, degrees of latitude are necessarily longer the farther they are from the equator.

Astronomers accepted Newton's reasoning but not all were convinced that the degrees of the meridian he cited as evidence had been accurately measured. A prominent father-and-son team of French astronomers, the Cassinis, defended the position that the earth was elongated at the poles. To resolve the dispute, in 1735 the Academy of Sciences sent

two distinguished research teams to measure degrees of the meridian close to the equator and north of the artic circle. An expedition headed by Maupertuis went to Lapland and another headed by La Condamine was dispatched to Peru.

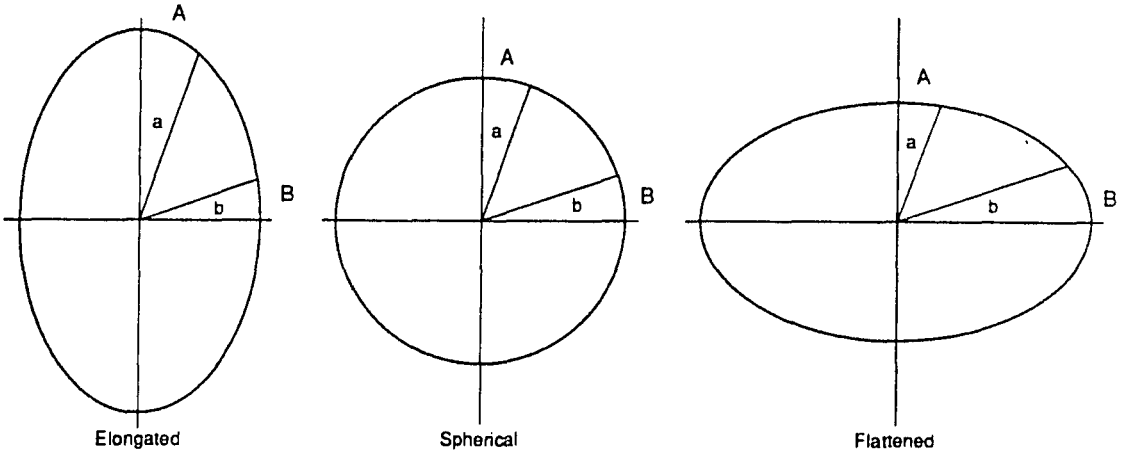
When the Maupertuis expedition reported its results from the far north, its measurement was compared with a carefully measured degree in France and found to be longer, in accordance with the prediction of Newtonian theory. The later return of the Peruvian expedition added further confirmation and the question was considered closed.

Bernardin, however, attempted to reopen it in *Etudes de la nature*. The measures of the degrees of the meridian, he said, do not lead to the conclusion that the earth is flattened at the poles, but just the opposite! If degrees of latitude in the north are longer than those close to the equator, then simple geometry demands that the earth must be *elongated* at the poles.

How could the great mathematician Newton have made such a fundamental geometrical error? He must have been led into carelessness, Bernardin suggested, by the oh-so-human desire to make the physical evidence fit into his metaphysical system. How could all of the mathematicians and astronomers since Newton have fallen into the same error? They were obviously misled by the overwhelming authority of Newton.

Since Bernardin clearly did not win this argument, a modern reader could understandably jump to the conclusion

that his quixotic challenge was purely irrational. Before doing so, however, the reader should take the trouble to consider Bernardin's argument. If a degree of latitude close to the north pole is greater than a degree close to the equator, does that imply an earth flattened or elongated at the poles? Consider the following diagrams:



In all three cases, the angles *a* and *b* are equal. In the case of the sphere, the arcs *A* and *B* are equal in length. In the other two diagrams, however, the arcs *A* and *B* are not equal. In the diagram representing an elongated earth, the arc near the "north pole" is longer than the arc near the "equator," and the converse is true for the diagram representing the flattened earth.

This is the simple geometric fact that Bernardin based his argument on. Does it not clearly refute the Newtonian position? If the reader is still convinced that Bernardin must somehow have been in error, is that conviction based

on anything more than authority (*i.e.*, the feeling that all of the modern textbooks simply cannot be wrong)?

Unless the reader is an astronomer, the assumption upon which the above diagrams are based will probably seem reasonable: that the angle defining a degree of the meridian originates at the geometrical center of the earth. In fact, however, degrees of latitude are determined by astronomical means in another manner, and therein lies the source of Bernardin's error. In actual practice, degrees of latitude were determined by sighting a fixed star and measuring the angle between the sightline and the horizon (the horizon being tangent to the earth's surface). An observer in the northern hemisphere can determine his or her latitude by measuring the angle between the horizon and a sightline to the north star, polaris. (It is left to the reader to figure out how this method of determining degrees of latitude accords with Newton's conclusion that the earth is flattened at the poles.)

Bernardin either did not know how degrees of latitude were astronomically defined or he may have considered it an arbitrary and erroneous contrivance on the part of astronomers.[33] In any event, his objections, though mistaken, were far from purely irrational.

The more important point to be made is that neither Bernardin's providentialism nor his erroneous geophysical theories are sufficient reason to dismiss the scientific content of *Etudes de la nature* as a whole. Yet that is what

the contemporary scientific elite did, and posterity has for the most part followed its lead.

In his efforts to reveal the details of the workings of providence, Bernardin explored such interesting biological phenomena as mimicry, symbiosis, and protective resemblances, which later theories would explain in terms of adaptation and survival value.[34] But the most significant aspect of Bernardin's work that was ignored by, and thereby lost to, orthodox science, was its antireductionist, ecological approach to the study of nature.

The fate of Bernardin's science was decided by social forces rather than scientific criteria. It was consigned to oblivion after Thermidor not because it was worthless but because of its Rousseauian associations. Bernardin shared Rousseau's critique of sterile Enlightenment rationalism, his contempt for academic science, and his insistence that science's ultimate mission is social reform. Though not at all politically connected with Jacobinism, then, Bernardin de Saint-Pierre fit the profile of the "Jacobin scientist," and his scientific reputation suffered accordingly.

VI. NICOLAS EDME RESTIF DE LA BRETONNE (1734-1806)

All of the natural philosophers of the prerevolutionary era were interesting characters in one way or another, but the most colorful by far was Restif de la Bretonne. His importance in literary history is attested to by the existence today of a *Société Rétif de la Bretonne* that publishes a scholarly journal, *Etudes Rétiviennes*, devoted to the study of his life and works.[35] Restif, played by Jean Louis Barrault, was the central character in the critically acclaimed film *La Nuit de Varennes*.[36]

Restif was a printer by trade who specialized in publishing his own writings. His sexually oriented novels -- *Le Paysan perverti* and *La Paysanne pervertie* were the best known -- won him fame but not fortune.[37] Restif has sometimes been described as a pornographer, but that is misleading. His "perverted peasants," for example, were not rural folk with exotic sexual habits, but innocent youth from the countryside who were lured into depravity by a morally perverted society.

Because of their sexual content it is not unusual to find Restif's works relegated to the *enfer* section of French libraries. "Enfer" literally means "hell," but it is also used to designate a library section where books of "doubtful character" are kept.

Restif's literary reputation, however, rests not on his erotic novels alone, but also on his utopian fiction

and his chronicles of Parisian life in the Revolutionary era. As a man of modest means, Restif associated with social classes that most other contemporary writers ignored; his accounts of his nightly wanderings among the ordinary people of Paris have been recognized as an invaluable source of sociological insights. Restif wrote of himself:

> Among all our men of letters, I am perhaps the only one who knows the common people and mingles with them. I want to describe them . . . . I have gone down into the lowest classes in order to see everything that is vile.[38]

It was Restif's habit of writing about the Parisian poor rather than his x-rated literature that earned him the sobriquet *Rousseau du ruisseau* -- the "Rousseau of the gutter." It must be remembered that Jean-Jacques, in spite of his constant drubbing of polite society, had chosen to rub elbows almost exclusively with its most privileged members.

Restif has also been compared to Rousseau because both were pioneers of the self-revealing autobiography. Restif's *Monsieur Nicolas* has often been treated as imitative of Rousseau's *Confessions*, but in fact Restif had planned and advertised his autobiography well before the publication of Rousseau's.

Because *Monsieur Nicolas* described his amorous adventures with some seven hundred women, Restif has also been called the French Casanova. The immense nineteen-volume *Monsieur Nicolas*, however, contains much more than

erotic reminiscences.[39] Six volumes are devoted to a *Philosophie de Monsieur Nicolas* consisting of a three-part *Physique*, a *Morale*, a *Religion*, and a *Politique*. The *Physique* is a detailed, 500-page elaboration of Restif's natural philosophy.

Although it may seem difficult to take seriously a natural philosophy embedded in an erotic autobiography, it must be recognized that sexuality was by no means incidental to Restif's worldview. The notion of copulating planets, mentioned above in Chapter Two, followed naturally from a metaphysics that placed sex at the center of all existence.

Sexual passion, after all, is certainly near the top of the list of human concerns, yet it was not considered a fit subject for orthodox science. Predictably, Restif's attempt to integrate sexuality into a scientific worldview was ignored in the halls of respectable science. From a less restrictive viewpoint, however, it can be appreciated as a laudable effort to broaden the scope of scientific inquiry.

In addition to the direct statement of his natural philosophy, Restif's utopian novels are of related interest, especially *La Découverte australe par un homme volant*. These works have been described as containing "one of the most detailed and meticulously elaborated systems in the history of utopia."[40]

Modern readers cannot help but be impressed by some of the technological and social innovations imagined by Restif. Others before him, of course, had envisioned a technology of human flight, but it is nonetheless interesting that Restif's "homme volant" appeared three years before the Montgolfiers' first balloon ascended. Among other things, he proposed a theory of microbes and elaborated a system of creative evolution. His social utopia consisted of a communist society based upon the abolition of private property, with collective farms and a five-hour work day.

Restif has long been a controversial figure in literary history. During his lifetime he was admired by many, including Beaumarchais and Goethe, and despised by many others. A famous anecdote concerns his proposed election to the National Institute in 1796, sponsored by Bernardin de Saint-Pierre and Mercier. La Harpe, president of the Institute, is alleged to have declared that Restif had genius but lacked taste, to which Mercier replied: "Ah, gentlemen, but which of *us* has genius?" Restif's candidacy, nonetheless, was rejected.[41] (La Harpe, it will be recalled, was among the primary creators of the myth of Jacobin science.) Restif, enraged at being blackballed from the Institute, bitterly accused his erstwhile friend Mercier of betraying him, but the accusation seems to have been unjustified.

When Restif died in 1806, an estimated 1,800 mourners attended his funeral, including members of the Institute. After his death, however, Restif's reputation declined precipitously as a consequence of the deepening conservatism of French society; by the time the *ultra* faction's influence had reached its zenith in the reign of Charles X, Restif had been virtually forgotten.

New revolutions in 1830 and 1848 stimulated a revival of social and literary criticism, however; Restif was slowly rediscovered and the controversy over the value of his work resumed. Restif's partisans are fond of quoting Paul Valéry's judgment: "I place Restif far above Rousseau."[42]

In spite of the recent attention devoted to Restif's writings, his natural philosophy has continued to be almost completely ignored. One scholar who did notice it described Restif's cosmology as

> pantheistic, since God as pure vital fluid was present in all things; materialistic, for no nonmaterial substance existed; evolutionary, as the form of beings was always in transformation; irrational, since reason had very little to do with the nature of men or with anything else; and above all, vitalist, for the whole universe was dependent on the mating of male and female beings who continued the life cycle. Pantheism, materialism, evolution, irrationalism, and vitalism -- what a peculiar combination of doctrines for the eighteenth century![43]

Except for the charge of irrationalism, this description is accurate enough, but the conclusion is wide of the mark. Restif was far from alone in holding views that could be characterized as materialistic pantheism. The

combination was more typical of than peculiar for the eighteenth century, although Restif's grand synthesis admittedly represented an extreme formulation.

That Restif's contemporaries detected radical political implications in his *Physique* is perhaps suggested by its publishing history. It was published in a separate edition, apart from the rest of *Monsieur Nicolas*, by the Cercle Social (formally the Confederation of the Friends of Truth). The Cercle Social had earlier counted Condorcet, Sieyès and Brissot among its leading agitators and had been one of the most influential of the radical organizations, with a membership of some 5,000 and a capability of mobilizing tens of thousands in insurrectionary action. It organized women's groups and workmen's circles and its main periodical, the *Bouche de Fer*, propagated feminist and socialist ideology. Marx and Engels credited the Cercle Social with initiating the tradition that evolved into Babeuf's Conspiracy of Equals and "gave birth to the Communist notions which Buonarotti . . . reintroduced into France after the Revolution of 1830."[44]

According to Darnton, "the ideology of the Cercle Social derived from a strain of occultism expressed most fully by Restif de la Bretonne."[45] The *Bouche de Fer* was edited by Mercier's friend Nicolas Bonneville, who, in addition to propagating Restif's ideas, "used the Cercle Social to promote Carra's works, which had much in common with his own."[46]

The Cercle Social's existence as a mass movement declined precipitously following the infamous Champ de Mars massacre in July 1791, and by 1796 its character had changed considerably. Bonneville continued to direct its publications program, however, and the Cercle Social was a leading publisher of works of popular science. In 1796, the same year Restif's *Physique* appeared, it was also the publisher of Laplace's very popular *Exposition du système du monde*. Both were undoubtedly aimed at a similar audience characterized by an interest in scientific topics. If so, Restif's work would not have appealed to that readership as "occultism," as Darnton suggests, but as another attempt -- different in style from Laplace's, to be sure -- at explaining how the universe works.

Restif later harshly condemned Bonneville and Mercier as swindlers who had taken financial advantage of him by underpaying him for his *Physique*. Perhaps it did not sell as well as Bonneville had hoped, but there is no evidence of fraud on his part. Bonneville's interest in publishing Restif's natural philosophy seems to have been more intellectual than pecuniary; he found Restif's system appealing and wanted to propagate it.

How did Restif's generally radical ideology translate into specific political affiliation? Was Restif in any sense a Jacobin sympathizer? It would be easy to make a case that he was, based on this statement:

> I believe that the true national representation is in the Mountain party; that the Jacobins and the patriotic clubs of the same sort, those who think as they do, are the true patriots . . . that Marat, Robespierre, etc., have saved the country; that the executions of September 2, 3, 4 and 5 were unfortunately necessary, above all as regards non-juring priests, the laity, the counter-revolutionaries, etc.; that the death of Louis Capet was just and necessary.[47]

But identifying Restif as a Jacobin on the basis of such statements would be extremely misleading. Restif was a political chameleon; as a later commentator observed:

> Under Louis XVI, Restif is devoted to the king; in 1789 he shares the ideas of the constituants; in 1792 he is a Jacobin; in 1794 a Terrorist . . . he always shares the dominant ideas with vivacity . . . Restif is a faithful mirror.[48]

Restif, while always politically cautious, was no simple opportunist, adapting his views to shifting circumstances for reasons of cowardice or convenience. Restif defended contradictory political positions with conviction and with little awareness of inconsistency. He was no detached observer of his social milieu; his immersion in it is what made him a "faithful mirror" of the dominant political moods at any given time.

Restif's exposition of his natural philosophy did not win him, as he had hoped, a scientific reputation surpassing Buffon's. His efforts were not wasted, however; by making explicit what others took for granted his *Physique* provides important insights into the ways many of his contemporaries perceived the natural world.

The neglect that Restif's *Physique* has suffered is witnessed by the fact that *Etudes Rétiviennes*, from its

first issue in 1985 to the present, has examined every imaginable aspect of Restif's life and works *except* his natural philosophy. Even more telling is the frequent omission of the *Physique* from successive editions of *Monsieur Nicolas*. The first republication was an 1883 "facsimile reprint edition" that simply left out the entire "Philosophie de Monsieur Nicolas."[49] A nine-volume *Oeuvre de Restif de la Bretonne* published in 1930-32 contained only brief excerpts of the *Physique*.[50] The first English translation, published in six volumes in 1930-31, was based on the 1883 abridgement.[51]

Not until 1959 was the entire text of the *Physique* finally included in a genuinely complete edition of *Monsieur Nicolas*.[52] The continuing neglect of Restif's natural philosophy, however, is indicated by the 1989 publication of a new, fully annotated edition of *Monsieur Nicolas* including hundreds of pages of scholarly notes, but once again omitting the *Physique*.[53] A long succession of nineteenth- and twentieth-century editors, then, had decided that Restif's natural philosophy was not of sufficient interest to justify its republication.

Restif, to the contrary, considered it the essential foundation of his autobiography. Its subtitle, "The Human Heart Unveiled," was meant to indicate that by fully baring his own thoughts and deeds to the world he intended to make an important contribution to the moral sciences. Since Restif considered moral science to be entirely based upon

physics, the *Physique* was the key to understanding *Monsieur Nicolas*.

The significance Restif attributed to his natural philosophy relative to his literary work as a whole is revealed by the fact that when in 1796 he appealed to the head of government, Lazare Carnot, for state financial support, he presented Carnot with a copy of his *Physique* as proof of his value to society. Carnot granted the request.[54] Restif looked upon Carnot as a fellow scientist and later praised him as one of the few people he had trusted who had not betrayed him.

According to Restif's own testimony, the formulation of his natural philosophy was begun in 1785 and completed before the Revolution, but he did not expect to publish it in his lifetime. The earth-shaking events of 1789 and after, however, put everything in a different light:

> Circumstances have changed. I feel today that it is necessary to come to the aid of my country and my nation by striking the final blow against the fanaticism and superstition of all times and places. That can only be accomplished by elaborating the true system of the world and of nature.[55]

Those few modern authors who have commented on Restif's natural philosophy have portrayed it as outlandish, bizarre, and mystical, and therefore as the very antithesis of science. Restif, however, did not consider himself an opponent of mainstream or orthodox science at all; he believed he could make a significant contribution to expanding its scope. He was a critic of

mystical and nonmaterialist views. "I do not acknowledge metaphysics," he declared. "Everything in nature is physical."[56] He admired and identified with orthodox scientists like Lavoisier, Laplace, and Condorcet. "Buffon and Lalande put the germ in my head," he stated, that grew into the *Physique*.[57] He drew upon the astronomical findings of William Herschel. Furthermore, after Thermidor Restif joined in the chorus of condemnation against the "Jacobin vandalism" of the likes of Marat, which had sought to "anhililate the sciences."[58]

While his *Physique* was not well received by contemporary professional scientists, it was criticized on its scientific merits and not treated simply as the fanciful productions of a poetic imagination. Laplace and Lalande, for example, appealed to the findings of mathematical astronomy to rebut Restif's assertion that comets are a natural stage in the development of planets.

To an anonymous critic who argued that Restif's "conclusions were at odds with those of Lagrange, Laplace, Lalande, Cassini, Brisson, etc.," Restif replied that, to the contrary, he had based his system on their doctrines and "differs from them only in the extent and multiplicity of my hypotheses."[59]

Nor were the reviews of the *Physique* unanimously negative; Pierre Bertrand gave an account of Restif's system in his *Nouveaux Principes de géologie* and asked, "Who is able to say that all of this is false?"

> Whatever one may say about it, this work deserves the attention of all those materialist philosophers who seek to submit everything to reason and to trace all things to their ultimate and eternal causes.[60]

It would be an exaggeration, however, to claim that Restif's system building was taken completely seriously by contemporary scientists; he complained of being laughed at by Lalande and others, whom he in turn then denigrated as mere "geometers" and narrow-minded astronomer-craftsmen. Nevertheless, it would seem that Restif did not have to endure the degree of ridicule with which Bernardin de Saint-Pierre had been confronted. This cannot be attributed to respect for Restif's literary stature since Bernardin, a member of the Institute, enjoyed more respect.

Restif claimed that he derived his system from observation of nature and a consistent application of a method based on analogy. He did not, he emphasized, get it from reading books. After working his system out, however, he felt he should familiarize himself with other philosophers' systems and with contemporary scientific research in all fields as a test of his ideas.

> I understood that it was necessary to read as much as possible to see if I could find a sensible and clear system that could overturn mine. . . . In the end, I remained firm in my opinion.[61]

> I admit that I would have had less confidence in the solidity of my ideas if, after reading the experiments of the unfortunate Lavoisier as reported by Delamétherie, I had not been convinced that the facts I had founded them on are true and the conclusions I had drawn from them are natural.[62]

Delamétherie, a chemist, geologist, and natural philosopher, was the editor of the *Journal de Physique*; his scientific activities will be discussed in Chapter Six. Restif drew heavily upon Delamétherie's main work on geology, *Théorie de la terre*. After describing Delamétherie's ideas on the origin of the earth, Restif stated:

> My observations with regard to this system, the best of all previous ones, will form my system, that of M. Nicolas.[63]

Although Laplace's mathematical astronomy was irreconcilable with Restif's notion of comets evolving into planets, Restif was nonetheless appreciative of Laplace's description of possible interactions between comets and planets in his popular *Exposition du système du monde*.

> Citizen Laplace's work is solid, fundamental, and full of certain facts; it has built upon all previous discoveries. Perhaps his title, *System of the World*, is too vast; the system he presents is incomplete, but nevertheless excellent. It was something we needed.[64]

Restif's critique of Laplace provides an insight into how Restif understood science and what he believed his own contribution would be. While acknowledging Laplace's "superior merit" as a mathematician, Restif maintained that not all physical truths -- the underlying unity of comets and planets, for example -- are discoverable by mathematics.

> A system of physics, to be complete, must take everything into account, must comprehend the origins of all things, and when demonstrative proof is lacking and beyond possibility, it must have recourse to the method of analogy.[65]

Direct observation can never provide the data necessary for a complete system of physics

> because the changes that take place are not detectable in ten thousand years, a length of time that exceeds the duration of all nations, all languages, and all human sciences.[66]

How, then, are mere humans to gain scientific knowledge about things and processes that cannot be observed, such as the origins or the ultimate fate of the universe? "We can only know by means of analogy," he declares. "Judge what is unknown by what is known."[67]

The basis of Restif's method of analogy had ancient roots, at least as ancient as the microcosm/macrocosm notions of Plato's *Timaeus* and the Hebrew scriptures' avowal that humankind was created in the image of God. "It cannot be doubted," Restif said, "that man is the image of the universe." Does not the voice of nature continuously testify that "all that is small is a repetition of what is large"?[68]

> My first axiom is: The world exists and I am part of it. The second: Everything changes and comes to an end, like me. The third: I can know everything by means of comparison and analogy. The fourth: The way to accomplish that is to study myself and to compare myself to all other beings, superior or inferior.[69]

Restif, then, did not claim that his system represented positive knowledge supported entirely upon observational or experimental facts. He did assert that his system was not contradicted by any known positive facts. He considered his contribution to science to be a method that

could penetrate those realms of material reality that are inaccesible to observation and experiment.

> If someone were to object to me that all this is mere conjecture, I would respond that conjecture is not the proper word. What I give are analogies, and they are of the greatest usefulness. . . . Long after I am dead some wise geometer like Laplace or a wise astronomer like Lalande will be searching for ways to prove these analogies by means of observation. But these conjectures might never have been thought of had they not been created by a fertile imagination such as mine (regardless of what certain dolts in the National Institute may say). These conjectures, these analogies were the result of long and profound meditation.[70]

To the petty calculators and astronomical craftsmen Restif says, "When I look at nature, I am looking at the big picture."[71]

> I concede you all your observations; I do not contest them; I agree with you on their correctness. But they are, as far as I am concerned, beside the point. . . .
> Oh, you enemies of every discovery that you yourselves have not made; you timid snails who crawl upon the earth -- pardon me, if you can, for having had more ideas than you![72]

Restif did not reject the orthodox "modern science" of the late eighteenth century; he embraced it, but only as a subordinate part of a much broader quest for knowledge of nature. His speculation was far-ranging and imaginative, but far from unbounded. He characterized the writings of his radical publisher, Bonneville, as mystical; he criticized Bernardin de Saint-Pierre's "supernatural optimism"; and he said of some of Du Pont de Nemours' notions that "such flights of imagination are not permissible in a work of physics, especially one addressed to Lavoisier!"[73]

Restif's methodological approach would not seem to place him outside the general framework of late-eighteenth-century natural philosophy, or even on its extreme fringe. Some of the specific conclusions he drew from his method of analogy, however, invariably strike modern commentators as peculiar and bizarre. Though his scientific imagination was, as he claimed, fertile and creative, his theories of the universe would not have appeared as jarringly strange to his contemporaries as they do today.

Restif's central principle, frequently repeated, is that "all is type and image in nature."[74] That is what assures the validity of his analogies.

The first analogy is the commonplace one that humans, animals, and plants are to the earth as the tiny mites and fungi that inhabit our skin are to us. The earth is not *like* an enormous animal; it *is* an enormous animal. That the earth is continuously producing a large variety of materials -- coal, metals, sulphurs, salts -- is "incontestable proof that it is not a cadaver but a living body."[75]

The second analogy is that the other planets are of the same type as the earth; they, too, are living organisms upon which smaller organisms live and feed. All of the planets, then,

> are in general what I am: they were born, they grew, they endure, and they will decline and die. I know them because I know myself. Therefore the planets are not eternal; all of this is further demonstrated by the fact that their surfaces continuously change.[76]

> Since the rise of Christianity, humans have become accustomed to think of the earth as a mass of dead matter (an absurd idea, since the earth produces everything that is living, and since its juices are homogeneous with those of humans and animals, which are nourished by them).[77]

Plants and animals, including humans, represent one level of living organism; the planets represent the next highest level. Above the planets are the suns; the planets are to the suns as we are to the planets. The chain of analogy comes to an end at the level above the suns; that is the Primary Being, or God. The suns revolve around God, the center of the universe, just as the planets revolve around the suns. The ladder of existence, then, is as follows: microscopic organisms draw their sustenance from animals like us; we draw ours from planets; planets draw theirs from suns; and suns draw theirs from God.

Those beings above ourselves on the ladder are not merely living, but *intelligent* as well. The planets are far more intelligent than we are; the suns still more intelligent, and God, of course, is the supreme intellect.

The English astronomer William Herschel, Restif noted,

> has just discovered the march of the heavens (*i.e.*, the ensemble of all the stars). The whole ensemble is turning. In order to turn, there must be a center, and Herschel, by that beautiful discovery has proved (perhaps without suspecting it) that there is a God.[78]

God, for Restif, is not a supernatural being; "nothing exists outside nature; everything is matter."[79] God is the ultimate gravitational attractor around which everything

revolves. But like ourselves, like the planets, and like the suns,

> God, or the Everything, is a living individual. Is he subject to change, as all that live must be? Science cannot teach us this, but it is within our power to consult analogies. According to analogy, God cannot be eternal, any more than the suns or the planets.[80]

The generation of living beings involves both the production of the lower orders out of the higher ones and the reproduction of like organisms. In the former case

> God produces the suns and the suns produce the planets by the same means that the earth produces its animals and vegetables -- they are excreted at the surface, or from the skin.[81]

The life cycle of a planet, for example, begins with the "secretion" of a mass of fluid from the surface of a sun. This mass of fluid, expelled from the sun by powerful electrical forces, begins life as a comet with a highly elliptical orbit. As it ages, it slows down and its orbit becomes more and more circular, spiralling slowly, slowly toward its ultimate destiny, which is to fall back into the sun.

> In the same way that my body will return to the earth, so the earth will return to the sun, and the sun will return to God.[82]

Extrapolating from data published by an unidentified astronomer, Restif calculated that the earth would fall into the sun in about 5,257,000,000 years.[83]

When the comet's orbit has become circular enough that it no longer crosses the orbits of planets, it has itself become a planet. Meanwhile, it has also been undergoing

processes of crystallization and dessication; its fluids have slowly turned into solids. As a planet ages, its seas diminish and it dries out just as we do as we grow older. Its final plunge into the sun is both its death and an act of copulation from which new "cometoplanets" will issue.

This is the "type" of the suns' life cycles as well; they, too, are destined to spiral slowly toward a final encounter with the Primary Being. When the last sun has fallen into the Primary Being, the entire universe will have died, but in the following instant a new universe will rise out of the collapse of the old one. "Eternity is a continuous set of revolutions."[84]

It follows that in any solar system the planets closest to the sun are the oldest. The earth, Restif estimated, is about eighty percent of the way through its life expectancy.[85]

The life forms on planets vary according to the age of the planet. Infant planets have primitive life forms; planets in the full vigor of youth have the most highly developed life forms; as planets get old and decrepit their life forms tend to degenerate. A very old planet like Mercury has most likely passed way beyond the time when intelligent animals ruled its surface; "the reigning species is probably the rabbit, the rat, or the mouse."[86]

As for the earth, since it, too, is well past its prime, the present dominance of the human species most likely represents a decline from an earlier epoch when a

more perfect, more intelligent, and more beautiful species -- perhaps a race of giants -- was in command. As for the future,

> after humans have died off, then monkeys will rule the earth, then a whole succession of lower and lower animals, finishing with the marine insects. By then the earth will have almost completely crystallized into solid rock, and will either have fallen into the sun or be very close to doing so.[87]

Restif proposed two alternative explanations for the origin of species; one was organic evolution based upon sexual selection and the other was a process of increasingly complex "crystallizations" of separate species. Both of these are scientific possibilities, he said, and he presented arguments in support of both alternatives.

As for evolution, "everything points to the conclusion that man developed from other animals."[88]

> If all animals are examined, it is found that their forms, however extremely varied, nevertheless have a common type.[89]

There are only slight differences between monkeys and humans.

If a very superior monkey were to give birth to a being slightly superior to itself, that offspring could be a primitive human, a "new man."

> A new man would love the beauty of his form; he would prefer the female that came closest to matching it; he would feel moral love for her and they would become a couple; they would have offspring more perfect than the rest of the species; they would form a little group separated from the nonperfected monkeys and would choose mates among each other.[90]

In spite of the persuasiveness of his arguments in favor of organic evolution by sexual selection, however, Restif opted for the alternative hypothesis of successively more complex crystallizations. "That is my opinion, reader; you are welcome to your own."[91]

Restif's evolutionary ideas should not be taken to mean that he was a "precursor of Darwin"; Darwin probably never heard of him and certainly never read the *Physique*. On the other hand, they do place Restif in an important milieu of scientific thinkers that included Lamarck, Lacépède, Geoffroy, Delamétherie, Blainville, Oken, and Goethe, who began seeking homologies (analogies) throughout nature and interpreting its history in developmental terms.

Perhaps the most startling of the conclusions that Restif drew from analogy is that like ourselves and the microbes that inhabit us, the larger living organisms that we inhabit also reproduce sexually. Planets and suns copulate, and they all experience sexual enjoyment: "The act of generation is, for all beings, the first of pleasures."[92] Nor is the Primary Being excluded: "Even God participates in the pleasures of love," since "all individuals are part of the whole."[93]

Restif knew that the idea of God enjoying orgasm would scandalize prudish Christians, but it couldn't be helped. The method of analogy leads where it will, and he simply deduced the consequences. If "all is type and image in nature," then the Primary Being must, in common with all

other living beings, be born, eat, reproduce sexually, and die.

Restif admitted that some of his hypotheses were more speculative, and therefore perhaps less certainly true, than others. Among the less well founded were some of the ancillary aspects of cosmic reproduction. For example, he believed the Primary Being, the suns, and the comets all to be male and the planets all female. The transformation of comets into planets therefore is accompanied by a sex change. Comets and planets copulate when the elongated orbit of a comet "penetrates" the circular orbit of a planet; Restif cited Laplace's writings as evidence of the possibility that an "exchange of fluids" could occur if a comet passes close to a planet.[94] The offspring of such a union, of course, is a planetary satellite, like the earth's moon.

Only a small sample of the mass of detail with which Restif buttressed his system can be presented here. Its position on the spectrum of contemporary natural philosophies can be gauged by his critiques of many of the others, which take up a large portion of Part Three of the *Physique*. He is harshly critical of Buffon's "puerile system," but insists nonetheless that "no one respects him more than I do."[95] He finds Delamétherie's system greatly to his liking; it would seem that Delamétherie was the source of Restif's concept of crystallization as a

universal explanation for the origins of almost everything, animal and vegetable as well as mineral.

Restif also gave de Maillet's *Telliamed* a positive review. With regard to the origins of the earth, seas, and mountains, "no physicist has come so close to the truth as Maillet."[96]

Another central leader of the Revolution who had also been a natural philosopher was the comte de Mirabeau. Since his work was unpublished, its influence among contemporary savants was negligible, but Restif came across Mirabeau's "general physics" in a notebook that he was allowed to peruse for three days. Rather than summarizing its contents, Restif provided a lengthy excerpt, upon which he commented: "Absolutely new ideas are to be found there, and they are worthy of that great genius!"[97]

Mirabeau, like Restif, believed that the stars are all centers of planetary systems and that the entire ensemble of the heavens rotates around God. The points of similarity between Mirabeau's cosmology and Restif's were so extensive that Restif apparently felt the need to again insist at length that he had read Mirabeau's notebook only after composing the main part of the *Physique*.

Of all of Restif's comments on the systems of others, the most interesting are those devoted to Du Pont de Nemours' *Philosophie de l'univers*. He began by identifying Du Pont as "Lavoisier's friend" and said of the recently published book:

> I was astonished and very gratified to see how many points of agreement there were between us. When two beings have neither seen each other nor read each other's work, and yet are of the same opinion, it is (I repeat) a proof of truth.[98]

The agreement was far from complete, but there was much to be placed on the positive side of the ledger:

> One thing that gave me great pleasure was to see that Du Pont does not contemptuously reject, as some ignorant people do, either that the planets are living beings or that they are intelligent, and he admits the hypothesis that we are like microscopic insects on these enormous animals.[99]

On the other hand, Restif is sharply critical of certain "flights of imagination" on Du Pont's part.[100] First among these is Du Pont's contention that there are more perfect beings on the ladder of existence between ourselves and God that can function as our guardian angels:

> I do not, like Du Pont de Nemours, claim that there exist invisible beings intermediate between God and myself: that which is invisible is beyond the realm of analogy; it does not exist as far as we are concerned, since it is beyond the reach of our senses, and even of our reason.[101]

> Here are the only intermediate beings that can exist between God and us: our planet and (if one so desires) its moon, and our sun. It is through these two or three intermediaries that God interacts with man, and not directly, as all of physics proves.[102]

Restif also rejected Du Pont's hypothesis of reincarnation as the basis of a system of ethics:

> The idea that after a good life we are rewarded with a more pleasant state of existence is very morally pleasing, but that is all there is to it. There is no proof of it, either in experience or even in physical analogies.[103]

Death, according to Restif, is "a state of rest, a cessation of individuality; nothing more."[104]

Restif was particularly critical of Du Pont's physiocratic defense of private property as a right based in natural law: "Du Pont cranks out sophisms here, which I will not do him the dishonor of repeating."[105] Elsewhere, Restif condemns as "odious economists" Turgot, Quesnay, and "Utpond," the latter being an anagram of Du Pont's name.[106]

In Restif's opinion, "no natural right of property exists: everything belongs to everybody."[107]

> People are so strangely stupid that they never seem to see that private property, above all else, is the source of all vice, of all crime, of all corruption.[108]

Restif's advocacy of a radical abolition of private property is a central plank in the program of his *Morale*, which he claimed was directly constructed upon the foundation of his *Physique*.[109] His *Politique* then elaborated a blueprint for a future communistic society. Restif's utopian vision is a work of insightful social criticism but, like Plato's *Republic*, is not without its draconian aspects. The "strangely stupid" people, it seems, would have to be forced into virtue and happiness. This element of Rousseauian and Robespierrian ideology has frequently been noted.[110]

Restif did not labor under the illusion that social reform could be accomplished by the mere propagation of a truthful social program: "I am well aware how useless it is

to preach communism to people. Ha! Too many passions oppose it!"[111]

In spite of Restif's reputation today as a purveyor of wild ideas and strange notions, a case can be made that in the context of his own times he was the relatively sober and responsible natural philosopher he claimed to be:

> I am not one of those authors who dream in front of their fires during the day and at night crank out those dreams as facts. I am first of all a skeptic. I never write until I am convinced, and I never publish until doubly convinced.[112]

Restif's natural philosophy is not objectively weird or bizarre. The extent to which it strikes us as peculiar is primarily a reflection of the depth of the changes that have occurred, between the eighteenth and twentieth centuries, in the assumptions about nature that form part of the collective subconscious of European culture. A major shift was underway just as Restif was writing; the suppression of imaginative speculation in French science was becoming ever more evident. Of those academicians who collected little facts and wrapped themselves in the flag of *hypotheses non fingo,* Restif lamented:

> How tragic it is for the sciences and for the human species that mathematics, geometry, and astronomy have been abandoned almost exclusively to minds so narrow and limited as theirs![113]

CHAPTER SIX

LAMARCK, LACEPEDE, DELAMETHERIE, AND MARIVETZ

VII. JEAN BAPTISTE PIERRE ANTOINE DE MONET, CHEVALIER DE LAMARCK (1744-1829)

Lamarck may seem at first glance an inappropriate choice for inclusion in the category of unorthodox scientists since he was a member of the Academy of Sciences and has long figured prominently in standard histories of science as a "precursor of Darwin."[1] Yet his penchant for speculative system building set him permanently at odds with most of his academic colleagues. Their antagonism toward him was expressed by Cuvier's notorious eulogy that ridiculed the deceased Lamarck's scientific work, and in particular the evolutionary hypothesis that later became the basis of Lamarck's enduring reputation.[2]

Lamarck's early botanical work demonstrated his capacity for meticulous attention to detail; his *Flore françoise* sufficiently impressed Buffon for him to have it published at government expense, and with Buffon's sponsorship Lamarck won election to the Academy of Sciences in 1779.

Lamarck's passion, however, was never for the details, but for trying to comprehend nature as a whole; he, like Bernardin de Saint-Pierre, considered nature's interconnections and processes more important than any phenomena observed in isolation.

> Lamarck was interested in facts, he said, but there were 'big facts' and 'small facts,' and he was interested in the 'big facts,' the facts of the first order of importance. Hypotheses were not to be foresworn as the bane of scientific progress. Instead they were to be recognized for what they were, and then used judiciously. Was one to remain silent with regard to certain facts rather than offer some plausible hypothesis to account for them, even if the hypothesis was not immediately provable? Lamarck did not think so.[3]

Lamarck moved beyond botany to become a pioneer of invertebrate zoology; he coined the word "invertebrate" and is often credited with coining "biology" as well.[4] Whether or not he was the first to use the word "biology," Lamarck's speculative theories concerning the place of life in nature helped to bring the very concept of "life sciences" into being.[5]

Lamarck's interests went far beyond botany and zoology *per se*. He devoted as much or more energy to chemistry, meteorology, and geology, but most of these efforts were ignored or scorned by the mainstream scientific community. Among his first acts as a new member of the Academy of Sciences was the presentation, in 1780, of a volume of his chemical theories explaining all of the most important natural phenomena, *Recherches sur les causes des principaux faits physiques*. Condorcet acknowledged receiving it, but the Academy hardly took note of its existence, much to Lamarck's chagrin. Only after the destruction of the Academy during the Revolution did he finally publish it, in 1794, with a strongly republican preface, one of the few indications of Lamarck's political inclinations.[6]

Almost everything about Lamarck, including his political views, must be gleaned from his published writings. His biographers have noted that he left very little in the way of manuscripts or letters.[7]

Although Guerlac, Hahn, and Jordanova have all pointed to Lamarck's alleged membership in the *Sociéte de 1789* as evidence of his republicanism, Richard Burkhardt, author of the most comprehensive scientific biography of Lamarck, has shown that this was a case of mistaken identification.[8] Barthélemy-Madaule cites Lamarck's strong declaration in the dedication to the 1794 *Recherches sur les causes des principaux faits physiques* and concludes that there is "no good reason to doubt the sincerity of this ardent period."

> But the decisive argument in favor of Lamarck's profoundly republican ideas is the *Système analytique des connaissances positives de l'homme* published in 1820, during the reign of Louis XVIII.[9]

Whatever Lamarck's personal view of the Revolution may have been, "Lamarckism" as a scientific doctrine was associated with republican, democratic, and progressive political ideology at the time of the 1830 and 1848 revolutions, as well as during the Reform Bill period of agitation in England.[10] "The Lamarckians of the late nineteenth and early twentieth centuries," moreover, "thought of him as a republican with progressive ideas."[11]

Chemistry, Biology, and Meteorology

Because Lamarck's chemistry was opposed to the "new chemistry" of Lavoisier, Fourcroy *et al.*, it has

customarily been portrayed by historians as hardly worth mentioning. Nonetheless, Lamarck considered his chemical and biological theories as an inseparable whole, and his chemical notions were indeed integral to the structure and presentation of his evolutionary hypothesis. The latter cannot usefully be considered apart from the former. Burkhardt argues that

> it was in no small measure Lamarck's refusal to give up certain scientific and philosophical positions -- positions that were under fire and of questionable defensibility -- that led him to his belief in evolution in the first place.[12]

The nexus of Lamarck's meteorological research with his biology was the recognition of the crucial effects of climate upon organisms. He was also convinced that a predictive science of weather was feasible and he undertook to create it. The central idea was that the earth's atmosphere is probably analogous to its oceans; it most likely has a flux and reflux that is directly affected by lunar gravitation. Lamarck therefore sought to discover a correlation between the weather and positions of the moon. After much effort he managed to win the support of the Academy in 1800 for the establishment of a meteorological data bank and his volumes of weather prediction were published annually for more than a decade. But alas, this project came to grief when the Emperor himself publicly humiliated Lamarck by declaring the whole endeavor a waste of time unworthy of "his" Academy of Sciences.[13]

> Napoleon did not like Lamarck. He preferred men whose thoughts threatened neither the bible nor the "ruling classes."[14]

Here again, as in the case of Bernardin de Saint-Pierre, the restrictive and retarding effect of the intervention of the Napoleonic state upon a nascent field of science is evident. There is irony in the fact that the volume Bonaparte so rudely rejected was not, as he had mistakenly believed, one of Lamarck's meteorological annuals, but his masterpiece, *Philosophie zoologique*, one of the very few works of science that has remained in print in several languages for almost two centuries.

The value of Lamarck's approach to science lay in its breadth of scope:

> However speculative his ideas may appear to be in retrospect, they stimulated the imaginative drive for an integrated worldview. They appealed especially to those who opposed the analysis of organisms in terms of physics and chemistry (reductionism) and to those who sought meaningful patterns and purpose in nature.[15]

Lamarck's devotion to an antireductionist brand of natural philosophy never wavered during the entire course of his long career. The earliest statements of his comprehensive system of nature, produced during the Revolutionary period, were the two-volume *Recherches sur les causes des principaux faits physiques*, which included a lengthy appreciation of Marat's contributions to a scientific understanding of heat.[16] A companion volume, *Réfutation de la théorie pneumatique*, consisted of a 400-

page point-by-point comparison of his own theory with Fourcroy's *Philosophie chimique.*

Lamarck restated his theories in a series of lectures presented at the reorganized Academy of Sciences (the First Class of the Institute), which were published in 1797 as *Mémoires de physique et d'histoire naturelle.* The presentations were met with open hostility on the part of some of the leading academicians. In 1802 Laplace interrupted him with harsh criticism as he was reading a meteorological memoir; never again did Lamarck attempt to present a paper to the Academy.[17] He did not, however, abandon confidence in the value of his spirit of synthesis. In 1809 he published *Philosophie zoologique,* which included the most complete statement of his evolutionary hypothesis, and in 1820 he published his final work of natural philosophy, *Système analytique des connaissances positives de l'homme.*

Cuvier's eulogy condemned "less favored philosophers" like Lamarck for "allowing themselves to be led away by what is conjectural or doubtful," and for having "laboriously constructed vast edifices on imaginary foundations."[18]

After systematically belittling Lamarck's life's work, Cuvier disingenuously observed that his

> attachments to systems, so little in accordance with the ideas which prevailed in science, were not calculated to recommend him to those who had the power of dispensing favours.[19]

As if Cuvier himself had not been foremost among those who wielded that power, and who thus determined what ideas "prevailed in science"!

Lamarck had explicitly and eloquently challenged the narrowness of those prevalent ideas that Cuvier defended:

> It is at present a highly esteemed merit to occupy oneself with the gathering of facts. One must search after them on all sides. One must consider them wholly in isolation. Finally one must confine oneself everywhere to the smallest details. This procedure alone is said to be estimable.
>
> As for me, I think it can be now useful to bring together the collected facts, and to consider them as a whole, in order to obtain from them the most probable general results. Those who would conclude that in the study of nature we must always limit ourselves to amassing facts resemble an architect who would advise always cutting stones, preparing mortar, wood, iron-work, &c. and who would never dare to employ these materials to construct an edifice.[20]

Burkhardt has argued that it was prejudice against system building and not the contents of Lamarck's ideas that led to their lack of general acceptance. Some of Burkhardt's formulations, however, tend to imply that the criticisms leveled by Lamarck's orthodox opponents were justified, as if their scientific ideology were superior to his in some objective sense. Burkhardt seems to hold Lamarck himself ultimately responsible for "the failure of his views to gain scientific respectability in his own day":

> Lamarck's problem was that he was unable to relate his broad hypotheses to factual evidence in such a way as to cause his contemporaries to treat his hypotheses as profound insights rather than unfounded speculation.[21]

According to this view, it was "Lamarck's problem": he was unable to "cause his contemporaries" to take his science seriously. That would make the failure entirely his, then, and not at all theirs. This extended to the negative reception of his evolutionary hypothesis as well:

> When in 1800 he told his students at the Museum that the origin and diversity of living things and the faculties they displayed could be explained in a relatively simple and wholly naturalistic fashion, he seemed to be advancing another "system" of the sort that his contemporaries had come to expect of him -- and of which they did not approve.[22]

It cannot be said that Lamarck's contemporaries "did not approve" of his system building, unless only the elite scientific types represented by Laplace and Cuvier are counted as contemporaries. While Burkhardt is right to emphasize the role of prejudice against system building in causing Lamarck's difficulties, that prejudice should not be treated as so obviously justifiable that it requires no further explanation. Darwin's associate, T. H. Huxley, later identified the source and the arbitrary nature of the reigning biases: "The vast authority of Cuvier was employed in support of the traditionally respectable hypothesis of special creation and catastrophism," he wrote; "the wild speculations" of the catastrophe theorists "were held to be models of scientific thinking," while "the really much more sober and philosophical hypotheses" of Lamarck's *Hydrogéologie* were derisively rejected.[23]

The blame for Lamarck's failure to win over the scientific elite should not be laid entirely at his feet,

as if his orthodox detractors represented real science and he did not. At least part of the explanation for that failure lies in the conservative social forces mobilized by Cuvier and others against system building in general and Lamarck's system in particular.[24]

Lamarck's Transformism

Lamarck's natural philosophy has not been entirely ignored by historians of science, but it has previously been considered almost exclusively in relation to his evolutionary theory. The familiar notion of Lamarck as precursor of Darwin presents an interesting paradox. Lamarck was a system-building natural philosopher and therefore supposedly the very antithesis of the orthodox "modern" scientist. Yet it was he who most forcefully advanced the hypothesis of species mutability or transformism (later called evolution), an indispensible plank in the platform of modern science. To compound the paradox, Lamarck's transformism was met with unrelenting hostility from the majority of the mainstream scientists, with Cuvier in the fore.[25] A number of ways to resolve this contradiction are conceivable; Gillispie has shown that some are untenable and has offered a suggestion of his own.[26]

First is the possibility that Lamarck was in fact a "real" scientist who merely dabbled in speculative natural philosophy on the side, or in his youth. Any such attempt

to separate Lamarck's work into good biology and bad theorizing, however, would constitute a gross distortion. As Gillispie acknowledges, "It cannot be too much emphasized that Lamarck saw his own work as a single body of thought." Lamarck "aspired to nothing less" than "to explain physically the origin and mechanism of the universe."[27] Cuvier's notorious eulogy attests to the fact that Lamarck persistently, even obstinately, defended his comprehensive view of nature to his dying day.

Another possibility would be to show that Lamarck simply proposed the transformist hypothesis off the top of his head; that it was a product of facile speculation as opposed to Darwin's laborious efforts to provide evidence for the hypothesis. This, too, is inaccurate. First of all, Gillispie notes, whereas Darwin is usually considered to have relied upon Charles Lyell's uniformitarian philosophy,

> Lamarck had been his own Lyell. His *Hydrogéologie* prepared the ground for his theory of evolution with a uniformitarian earth history as uncompromising as Lyell's, if not as well founded.[28]

Gillispie also concedes the "greater formal elegance" of Lamarck's presentation of the theory of evolution in comparison to Darwin's:

> It might be argued -- indeed, I do argue -- that in the relative cogency with which the two theories organize actual biological information, Lamarck's presentation in the great *Histoire naturelle des animaux sans vertèbres* is the more interesting and elegant. It is analytical and informs a systematic taxonomy, whereas Darwin simply amassed detail and pursued his argument through the accumulated observations in a naturalist's commonplace-book.[29]

Gillispie's own solution is that Lamarck cannot have been a precursor of Darwin because that would imply a step *toward* Darwin, a step toward the future of science, when in fact Lamarck's work was solidly attached to a scientific tradition belonging to the eighteenth century and earlier:

> Darwin had no predecessor in Lamarck. Lamarck's theory of evolution belongs to the contracting and self-defeating history of subjective science, and Darwin's to the expanding and conquering history of objective science.[30]

Lamarck's hypothesis, according to Gillispie, was not a step forward at all but the last step in the dead-end of an old, false, nonscientific way of perceiving the world. That would explain why Lamarck's brilliant insight lay dormant until Darwin and Wallace, independently of Lamarck and of each other, brought the transformist hypothesis into the realm of real science.

This, of course, still begs the question of how a natural philosopher like Lamarck, working in an allegedly inferior scientific tradition, arrived at an important modern conclusion more than a half-century before any orthodox scientists. Gillispie suggests that chance was a crucial factor. With regard to Lamarck's uniformitarian conception that "prepared the ground for his theory of evolution," Gillispie declares that "only accident found him on the side of sobriety."[31] The invocation of chance as an explanation is somehow unsatisfying, especially in light of the evidence Gillispie has cited in favor of Lamarck's diligence and great breadth of knowledge of life forms.[32]

Gillispie's demonstration that Lamarck was a man of the eighteenth century and not of the nineteenth or twentieth is helpful as a refutation of the myth of precursorism.[33] This is a valuable addition to the body of literature aimed at correcting the error of traditional history of science portraying important thinkers anachronistically. Roger Bacon, Francis Bacon, Galileo, Newton and others were not, as the cliché has it, "men who saw through time," but, like Lamarck, were products of earlier scientific traditions and were grappling with the problems of their own time. Their accomplishments were not of less historical interest or significance for that.

While Gillispie argues against one ahistorical error, however, he falls into another, as Burkhardt points out:

> Presenting Lamarck as an object lesson in the difference between good and bad science is little better than treating him as a thinker far ahead of his time.[34]

Gillispie's dismissal of Lamarck's natural philosophical worldview as an inferior kind of science is based on a twentieth-century judgment of what does and does not qualify as real science. Lamarck's achievement need not be attributed to luck if his scientific outlook is recognized as at least as valid in the eighteenth-century context as that of his orthodox opponents.

The point here is not to marvel at Lamarck's prescience but to recognize that the resistance to his insights had a negative impact on the course of the

development of knowledge. The narrowness of scientific view of the orthodox academicians led by Cuvier -- and their firm organizational control over French science -- *retarded* the growth of scientific knowledge, at least on the crucial issue of the origin of species. Indeed, the "central theme" of Coleman's biography of Cuvier "is Cuvier's uncompromising opposition to the hypothesis of the transmutation of biological species," because of which

> Cuvier was to become one of the most significant figures in the development, or retardation, of the new doctrines later to become known as the theory of organic evolution.[35]

Nor was this an isolated instance:

> On several of the great controversies of the eighteenth century, Cuvier tried to close debate by declaring the issue outside the realm of positive science.[36]

Cuvier cast a shadow over French science that endured throughout the nineteenth century. According to J. W. Herivel,

> the French took it hard that the creator of the theory of evolution should have been an Englishman, remembering too late Darwin's predecessor Lamarck, and they certainly were very slow in accepting Darwin's theory of evolution.[37]

The dismissal as worthless of contributions to science by natural philosophers of Lamarck's ilk thus needs to be reconsidered. Eighteenth-century science should be understood not as a manichean battle between "real" science and pseudoscience, but as a dialectical unity of analyzing and synthesizing tendencies.

This tension between the *hypotheses non fingo* outlook and system building was a creative one, and the exclusion of the latter from the realm of respectable science after Thermidor did not represent a pure gain for the development of French science. A price was paid, as the case of Lamarck illustrates.

Lamarck's System in its Earliest Version

The 1794 publication of the *Recherches sur les causes des principaux faits physiques* was the first complete statement of Lamarck's comprehensive system of nature. The full title of the two-volume work gives an indication of its scope:

> RESEARCHES INTO THE CAUSES OF THE MAIN FACTS OF PHYSICS and particularly those of combustion; the rising of water in the vaporous state; the heat produced by rubbing two solid bodies together; the heat that is detectable in rapid decompositions, in effervescences, and in the bodies of many living animals; causticity; the taste and odor of certain composite substances; the color of objects; the origin of mixtures and all composite substances; and finally, the maintenance of life of organic entities, their growth, their vigorousness, their decline, and their death.

Lamarck said he did not want to utilize the forms of courtesy of the old regime, which he characterized as one of "crawling slaves," so rather than dedicating the work to anyone he would simply give a "tribute of admiration" to the French people who, "magnanimous and victorious over all their enemies," had "recovered the sacred and inalienable rights given them by nature."[38]

Although the scope of the work was extremely broad, it did not "purport to explain the formation of the universe or to discover first causes."[39] Nor did Lamarck claim to have written a totally new account of nature. His principles, he said, are in accord with those of numerous other authors; his task had been to synthesize their "scattered and isolated insights."[40]

Lamarck recognized some definite limitations to human knowledge. Of particular interest is the fact that in 1794 he believed that the origin of life was eternally beyond the reach of scientific investigation; how life came into existence in the universe was held to be unknowable in principle. Furthermore, he stated:

> I believe it is as impossible for man to know the physical cause of the first individual of each species as it is to know the cause of the existance of matter or of the entire universe. . . . While there do exist many varieties produced by the effect of circumstances, these varieties do not at all denature the species.[41]

The earliest version of Lamarck's natural philosophy, then, explicitly rejected the evolutionary hypothesis. He was not simply agnostic on the question. Lamarck believed at that time that there was an absolutely unsurpassable barrier between living and nonliving entities in nature. Life, he held, could not have arisen out of nonliving matter. Nature was incapable of producing anything organic without an organic being as agent.[42]

Within a few years his position on these issues had begun to shift. In the 1797 restatement of his system he at

least allowed for the possibility that the physical causes of life might be knowable, but he continued to insist that life could only come from life.[43] By 1800 he had clearly decided that simple living organisms could arise from nonliving matter, opening the way for his explanation of the origin and development of species.[44]

The radical dualism of Lamarck's earlier views absolutely separated living nature from nonliving nature. On the one hand, by postulating a "vital force" that must of necessity remain forever shrouded in mystery, Lamarck's natural philosophy appeared to be a variety of vitalism. On the other hand, however, by considering most of nature to be completely devoid of life -- absolutely dead -- Lamarck was clearly no ordinary vitalist or pantheist. By sharply separating life from the rest of nature, Lamarck helped create the focus for a science of life, or "biology."

At its most fundamental level, Lamarck's system was based upon a chemical theory. In 1796 he published another volume, entitled *Réfutation de la théorie pneumatique*, which could not, he said, be understood apart from the earlier *Recherches sur les causes des principaux faits physiques*; the two works "make an inseparable set."[45] To the "pneumatic theory" of Lavoisier and Fourcroy, Lamarck counterposed his own "pyrotic theory" that he had presented, albeit without giving it a name, in the earlier work.

The problem with the pneumatic theory, Lamarck said, was that it had been constructed only upon the newly discovered facts having to do with gases; its partisans had not taken all of the previously known facts of chemistry into account as well. Since they had allowed themselves to be unduly impressed by novelties, "their revolution will perhaps not be as favorable to the real progress of the sciences as they think."[46]

Lamarck admitted that his own chemical theory bore certain resemblances to ancient ideas, but said that it was really not the same. Although he opposed Lavoisier's new chemistry, he did not accept Stahl's phlogiston theory, either. The most nonmodern aspect of his pyrotic theory was its reversion to the Empedoclean idea that all matter is ultimately composed of water, earth, air, and fire.[47] Although Lamarck used these words, his meaning can perhaps be better understood by thinking of the four elements as the "principles" of liquidity, solidity, gaseousness, and heat.

According to Lamarck, these principles can exist by themselves in a natural state, but when they combine to make up composite substances, their particles are radically deformed and held under great pressure. The composite substances that make up almost everything in the world, then, consist of particles of the four elements in unnatural states. The tendency of all composite substances is to decompose, so that the particles can escape the

pressure holding them and return to their natural uncombined state.

If all composites tend by nature to decompose, how did composites come into being in the first place? That is where life entered into Lamarck's system. Life is what continuously synthesizes the elements into composites; nonliving nature just as continuously breaks them down. This pair of polar opposite creative/destructive forces accounting for all of nature is reminiscent of those posited by Kant, Schelling, Hegel and other German natural philosophers.

When Lamarck said that all composite substances were products of life, he made no exceptions: "Without organic beings, none of the composite substances that are observed on our globe would ever have existed."[48] All of the earth's minerals, for example, are but "the debris of living beings."[49]

Lamarck's belief that nonorganic composite substances naturally tend to decompose put him at odds with the doctrine of elective affinity that most contemporary chemical theorists utilized. The concept of affinities assumed that certain substances have natural inclinations to combine with certain other substances. Lamarck, however, believed that

> the word "affinity" has been given too broad a meaning in chemistry, leading often to error by confusing phenomena that are in fact independent of each other.[50]

In particular, he thought it necessary to distinguish between "affinity of aggregation" and "affinity of composition."[51] The former, which is the tendency of identical particles to cling together, is a simple matter of attraction. The latter, however, which is the tendency of *unlike* particles to unite, does not exist; there is no such tendency. Only vital forces can cause dissimilar particles to unite. Matter, he repeated, "does not have any possible tendency toward composition." To the contrary, "all the composites of nature tend to self-destruct."[52]

Of the four elements, the one that played the most important role in Lamarck's system was fire; it was for that reason that he named it the pyrotic theory. Fire and heat, he believed, were the most misunderstood of all physical phenomena and therefore those most in need of clarification.

First of all, although often confused, "fire and heat are very different things."[53] Fire is one of the four basic, or elemental, kinds of matter. Heat is the effect produced by fire when it is in a particular state.

Fire exists in three states: a *natural* state, a *fixed* state (or state of *combination*), and a state of *expansion*. It was the failure to recognize and distinguish between these three states of fire that Lamarck believed had led the pneumatic chemists astray.

In its natural, elemental state, fire is nothing but matter and mass; it is not active; it does not cause heat

or light or anything else.[54] In this state it is "a fluid of inexpressible tenuousness, easily penetrating bodies, and found, therefore, uniformly spread out everywhere."[55]

Fire exists in the fixed state when it has been forced into combination with other elements. The followers of Lavoisier call it "carbon," and the followers of Stahl call it "phlogiston."[56] Its particles have been severely compressed and have a strong tendency to revert to the natural state. When they do escape, combustion results as the fixed fire is transformed into fire in expansion.

It is in the state of expansion that fire causes heat. Fire in expansion is the same thing that Lavoisier called "caloric" and Marat called "igneous fluid."

> All the bodies that make up the mass of our earth are penetrated by a certain quantity of fire in expansion.[57]

Lamarck had been particularly impressed by Marat's use of a solar microscope to produce an ocular demonstration of the materiality of fire in expansion: with Marat's device, it could be *seen*.

Lamarck was by no means ignorant of the revolutionary chemical ideas that had rapidly become a dominant paradigm; he simply disagreed with them. Summarizing the new explanation of combustion, he wrote:

> The best modern physicists and chemists at present assure us that in all combustion it is the vital air that burns. This vital air, they say, is a part of the atmospheric air (itself composed of about three parts nitrogen to one part vital air), and it decomposes into two parts during combustion. One part is fixed in the residue that results from this operation; they

> name it *oxygen*. The other, which they call *caloric*, is disengaged in the form of heat and light.[58]

Lamarck agreed that some matter disengages during combustion to produce heat and light, but he argued that it (fire in expansion) is liberated from the object that is burning rather than from the air.

Experimentation had indicated, however, that air was necessary for combustion. Lamarck explained that it was the resistance of the air that was necessary rather than a combustible ingredient it contained. That resistance is needed to keep the fire in expansion in contact with the burning body. In a vacuum, the freed fire simply expands without resistance and therefore causes no violence.[59]

As for Lavoisier's demonstration that water can be analyzed into two components (signifying that water is not an element after all), Lamarck replied: "Several experiments are cited that are undoubtedly authentic, but the conclusions deduced from them have been, in my opinion, exaggerated."[60] He conceded that Lavoisier had brought vital air (oxygen) and inflammable air (hydrogen) together and had obtained water from their combustion. Lamarck argued, however, that the water had been a constituent of both gases; it was a residue of their combustion and not a product of their combination.

Lamarck's system was comprehensive and internally consistent. He believed that it accounted for the total ensemble of observed and experimental physical facts better than either phlogiston theory or the new chemistry.

Although he did not win any significant converts among chemists, his system should not be considered a dead end because its relevance extended beyond chemistry.

The scope of his scientific thinking was, as he claimed, much broader than that of most of his academic colleagues. His weaknesses as a scientist were balanced by compensatory strengths. As his natural philosophy developed from its earliest formulation in 1780 to the *Philosophie zoologique* of 1809 and the *Système analytique des connaissances positives de l'homme* of 1820, Lamarck's fertile scientific imagination represented a valuable but largely untapped resource for French science.

VIII. BERNARD GERMAIN ETIENNE DE LA VILLE-SUR-ILLON, COMTE DE LACEPEDE (1756-1825)

Lacépède's career paralleled that of Lamarck in many respects; both were prominent naturalists inspired and cultivated by Buffon; both advanced transformist hypotheses with regard to the origin of species at about the same time. Most pertinent to the present study is that Lacépède, too, published several volumes of speculative natural philosophy in the 1780s and yet was himself a member of the orthodox science establishment. Although not elected to membership in the Academy of Sciences until its reestablishment after the Revolution, Lacépède had worked closely with Buffon, who in 1784 arranged for him to be appointed to an official position at the *Jardin du roi*.

Lacépède was one of a number of talented young naturalists whom Buffon had asked to contribute to his immense project, the *Histoire naturelle*, but none worked more faithfully and productively than Lacépède.[61] As the primary continuator of the *Histoire naturelle*, Lacépède deservedly won the reputation as Buffon's scientific heir.

The breadth of Buffon's scientific outlook was also the inspiration for Lacépède's early works of natural philosophy, *Essai sur l'électricité naturelle et artificielle* and *Physique générale et particulière*. Although Lacépède never completely abandoned his personal taste for the broad view of nature, unlike Lamarck he was

apparently more susceptible to the social pressures of respectable academic science and ceased producing works of speculative natural philosophy. (From Gillispie's point of view, he "graduated from these indiscretions, at least in part.")[62] Lacépède's friendship with L. J. M. Daubenton undoubtedly influenced him in this regard; Daubenton had a low tolerance for system building and continuously urged Lacépède to stick to his detailed descriptive work.

Lacépède's reputation as an orthodox scientist was based above all on his five-volume *Histoire naturelle des poissons* (1798-1803) and similar *Histoires naturelles of quadrupèdes ovipares* (1788), *serpents* (1789) and *cétecés* (1804). After 1804 Lacépède "practically dropped out of the scientific community."[63] Although he had been one of the most prominent scientists of his time, modern commentators have been less appreciative of his abilities and talents. Gillispie says that "Lacépède did not have an important mind," and Burkhardt agrees that he "was not a great scientist."[64] Such judgments, however, are at least partially shaped by the modern tendency to devalue practitioners of speculative natural philosophy.

Meanwhile, Lacépède had forged a formidable political career for himself. Although born into a noble family, and a wealthy one at that, he was progressive-minded and enthusiastically greeted the onset of the Revolution. He was one of a number of scientists who shared membership in the *Neuf Soeurs* masonic lodge with such future leaders of

the Revolution as Brissot, Danton and Sieyès. Lacépède was elected to the Constituent Assembly and later to the Legislative Assembly, of which he rose to the presidency.

As a hereditary noble, he decided it would be prudent to retire to the countryside in 1793 as the radicalization deepened. Soon after Robespierre's fall he returned to Paris and reclaimed an honored place in the scientific community under the Directory. His friendship with Napoleon certainly benefitted his political career; he became a Senator in 1799 and in 1803 was named Grand Chancellor of the Legion of Honor, a ministerial-level position that he held until Napoleon's fall from power.

> Of all the scientific types involved in the world of politics, it was undoubtedly Lacépède whom Napoleon utilized most, and for the most delicate tasks.[65]

On top of his other honors, Lacépède was named *comte d'Empire* in 1808. Bonaparte's downfall led to Lacépède's retirement from politics, but it did not diminish his prestige; he was raised to the peerage after the Bourbon Restoration.

Cuvier's respectful treatment of Lacépède provides a significant contrast to his ridicule of Lamarck. Burkhardt noted the hypocrisy of Cuvier's selective attack on Lamarck's evolutionary notions. Lacépède, he writes,

> was a man of considerable stature, being one of the senators of the consulate and then grand chancellor of the Legion of Honor and a minister of state. When in 1800 the second volume of Lacépède's *Histoire naturelle des poissons* appeared -- the volume in which Lacépède advanced his own thoughts on species mutability -- Cuvier sent Lacépède a letter praising

> the book and reported favorably on the book to the first class of the Institute.[66]

Lacépède, it must be said, did not aggressively promote his natural philosophy as did Lamarck. Nonetheless, Cuvier could not have simply failed to notice Lacépède's transformist hypothesis, so his praise was at least disingenuous.

Lacépède's System

The earliest statement of Lacépède's comprehensive natural philosophy was his *Essai sur l'électricité naturelle et artificielle*, published in two volumes in 1780. Throughout the *Essai* he promised further clarification of specific points in a forthcoming *Physique générale et particulière*, which appeared in two volumes in 1781 and 1782. The primacy given to electricity was justified by Lacépède's judgment that it was the most powerful and yet the least understood of nature's forces.

The distinction between artificial and natural electricity contrasted the relatively puny forces that could be created in the laboratory to the awesome powers displayed by nature. The first eight of his *Essai*'s 16 parts were devoted to theoretical and experimental electricity and the last eight to such natural manifestations of electrical power as earthquakes, volcanoes, thunderstorms, and the motions of the planets. The scope of the work is indicated in this introductory statement:

> We will consider the electric fluid acting on a grand scale and set into motion by the powerful forces of nature. The fires that burn in the bowels of the earth, the force that makes the earth tremble, the faults that it digs in the earth's interior, the flashes that it spreads throughout the shadows of night, the storms it excites in the atmosphere, the winds it unleashes, the lightning bolts it hurls, the hail it forms: all these great things will be the subjects of our reflections. From there we will be led to speak of magnetism and its associated phenomena. We will seek to recognize the power that magnetism and electricity exercise upon animals; we will see the electric fluid come, each spring, to spread greenness on the land and to perfume the flowers. Finally, with the boldest of speculations we will contemplate the course of the celestial bodies; we will see that they are submitted to the influence of the electric fluid.[67]

Like Lamarck, Lacépède did not in any sense conceive of what he was doing as scientifically unorthodox. Although he was not simply restating Buffon's system of nature, he cited his mentor's *Histoire naturelle* and *Epoques de la nature* as the source of numerous theoretical conceptions. His references to Buffon were always respectful and never challenging, but, he asked, "cannot some ideas be added to the sublime views of that great man?"[68] Lacépède felt that the Buffonian system particularly required updating with regard to electricity.

In any event, the idea that following in the footsteps of that prestigious scientist could lead to pseudoscience or "fringe" science would not have occurred to Lacépède. In addition to Buffon, Lacépède's other sources were almost entirely contemporary scientists whom posterity has categorized among the orthodox: Franklin, Beccaria, Volta, Muschenbroeck, Aepinus, Wilke, Le Roy, Nollet, Lavoisier,

Guyton de Morveau, Lalande, Cassini, and Romé de l'Isle, to name but a few. The frequent appearance of the *Journal de Physique* in his footnotes further testifies to his orientation toward the vanguard of professional science. That Lacépède also cited natural philosophers less favored by posterity -- Marat and Marivetz, for example -- does not suggest unorthodoxy on his part but rather that Marat and Marivetz were likewise perceived as legitimate practitioners of normal science by many of their contemporaries.[69]

Lacépède, together with virtually all other scientists of the day, believed that electrical phenomena were based upon the flowing of one or two special fluids. Robert Symmer and Torbern Bergman had argued that two distinct fluids were necessary to account for positive and negative electrical effects, but Lacépède followed Franklin and Aepinus in the belief that the two kinds of electricity are caused by a surplus or deficit of a single fluid.

Humankind had been slow to recognize the immense power of the electric fluid, Lacépède pointed out, because where we live, on the surface of the earth, it tends to be so evenly distributed and its equilibrium so seldom disturbed that its effects are rarely seen. Those that are observed in nature are relatively unimpressive, such as the attraction of tiny bits of matter toward rubbed amber or glass.

Experimentation, however, had led first to the suspicion and then to the certainty that the power of lightning is in fact a natural manifestation of the electric fluid. Furthermore, Buffon had hypothesized that electricity was among the causes of earthquakes and volcanoes. Lacépède sought to synthesize everything that was known about the electric fluid into a comprehensive philosophy of nature. He did not consider himself an armchair philosopher, but as an active experimentalist. While he did not enumerate and carefully describe his experiments, he claimed to have replicated those of other investigators whose results he reported and to have added a few original experiments of his own.

In spite of its centrality to his system, Lacépède did not consider electric fluid to be among the most fundamental of substances; in fact, he assigned it to a tertiary level. In Lacépède's matter theory, the ancient four elements -- air, water, earth, and fire -- were almost fundamental, but not quite. Fire was indeed considered fundamental, but the other three "elements" were really only modifications of something even more elemental, namely "matter in general."[70]

In the final analysis, then, there are only two really primary substances that make up all other, composite substances: fire (the active principle) and matter-in-general (the inert principle). Fire, to Lacépède, was "that living matter, the soul of the universe."[71]

The ordinary substances that we see around us are complex combinations of inanimate matter in its earthy, watery, and aeriform states; fire is there, also, in its fluid form. This was known even by the ancients. What Lacépède had determined that had not been previously known was the composition of the all-important subtle fluids of light, electricity, and magnetism.

Elemental fire adds the property of elasticity and expansibility to inert matter, and "without elasticity, dead nature would be reduced to a heap of heavy matter piled upon itself, devoid of motion."[72] Each of the secondary states of inert matter -- air, water, and earth -- combines in its pure form with elemental fire to form one of the subtle fluids. The combination of fire and air yields luminous fluid, or light; that of fire and water yields electric fluid; that of fire and earth yields magnetic fluid.

Lacépède included phlogiston in his system as a variant form of the luminous fluid:

> Light, as I conceive of it, is nothing other than phlogiston, differing only in the proportion of the substances that compose it. Phlogiston is in fact, as Mr. Buffon said, nothing but pure fire combined with air.[73]

Of more direct concern to Lacépède, however, was the origin of the electric fluid:

> A great number of the particles of that active matter, fire, combine in the earth's interior with those of water and by their intimate and immediate union form that fluid that has been named *electricity*.[74]

The "bowels of the earth," then, are "not only our principal reservoir of heat and fire," but also of electric fluid.[75] That is evidenced in the laboratory by the fact that electric fluid is pumped out of the ground by electrostatic generators and, whenever given the opportunity, will flow back into the ground.

The electric fluid formed in the earth's interior escapes through its crust at the weakest points and rises into the atmosphere, where it attaches to floating clouds. After discussing how the Leyden jar stores and releases electric fluid in violent discharges, Lacépède explained how a similar phenomenon could occur on a much grander scale in the atmosphere.

A Leyden jar, after all, can be reduced to a thin plate of glass where a surplus of fluid builds up on one side and a deficit on the other. Connecting both sides by a conducting material will give rise to a violent restoration of equilibrium. Alternating layers of air might, he proposed, be charged with electric fluid in such a way as to act like a huge natural Leyden jar. The presence of negatively and positively electrified clouds in conjunction with electrified layers of air also plays a role in storm creation.

Lacépède considered Franklin's suggestion that clouds are charged by the friction of cloud molecules rubbing against mountains, treetops, and ocean waves.[76] While accepting this as a partial solution to the problem,

Lacépède believed that it was not the primary mechanism. In addition to Franklin's hypothesis, he also discussed Wilke's theory of storms and Beccaria's notions of how clouds are electrified.[77] It is evident that this wide-ranging speculation was not being conducted among an isolated fringe of marginal scientists. The various hypotheses were aimed at resolving an important practical and theoretical question. Lacépède's participation in this process consisted of offering a theoretical contribution supported by experimentation.

The electrification of clouds and atmospheric layers, however, was all secondary in Lacépède's system to the original creation of electric fluid, which occurred elsewhere:

> The interior heat of the globe, the *element of fire,* combines with that of water in the depths of the earth and gives birth at each instant to a new quantity of electric fluid.[78]

Also within the earth are alternating layers of what he called idioelectric and anelectric materials. Idioelectrics are those substances such as glass and amber that are electrifiable by friction; anelectrics are those such as metals that act as conductors. Lacépède pointed out that "quartz, sandstone, the hard and vitrifiable rocks that our globe is composed of are incontestably idioelectric substances."[79]

Layers of these materials interlaced with veins of metals amounted to gigantic natural electrophores analogous

to the laboratory apparatus invented by Volta. These could accumulate huge quantities of the earth's electric fluid and occasionally discharge powerful strokes of underground lightning in large subterranean caverns. These great sparks would suddenly rarify the air, greatly increasing its expansive force and resulting in a gunpowder-like explosion that shakes the earth. The air is robbed of its elasticity in this process, leading to a partial vacuum and the implosion of the underground caverns, producing a full-blown earthquake. All of this activity also whips up winds in the subterranean tunnels, acting like a bellows on the earth's internal fires, causing volcanic action.

Lacépède also took a stand on another widely discussed issue: the relationship between electricity and magnetism. Many natural philosophers had noted strong analogies between the two forces, but many differences were obvious as well. An often-cited indication of their interaction was the testimony of mariners that electrical storms cause strong deflections in magnetic compass needles. Lacépède had seen for himself how lightning could agitate magnetized needles and concluded that "these great irregularities should be attributed to the electric fluid."[80]

Magnetic fluid, it will be recalled, was thought by Lacépède to be a composite of pure fire and elemental earth, while electric fluid was believed to be composed of fire and water. The two subtle fluids were not the same

thing, he said, but they obviously have a great affinity for each other.

The assumption of an interaction between electricity and magnetism was a significant one held by many natural philosophers. Coulomb, however, denied the association and his great authority retarded investigation into the relationship between the two forces. From the time of the Revolution until the announcement of Oersted's famous experiment in 1820, curiosity about mutual effects of electricity and magnetism was treated by institutional French science as a pursuit fit only for crackpots.

Lacépède, citing an article he had published in the *Journal de Physique* in 1789, reported a practical innovation he had derived from his ideas about electromagnetism.[81] Since glass is an idioelectric material it does not allow passage to electric fluid. Tightly sealing a pivoting compass needle inside a glass capsule, then, should shield it from the effects of lightning, and comparative experiments confirmed that expectation. The experiments utilized an unshielded magnetized needle as a "control" for comparison purposes. Perhaps if Lacépède's attention had been more focused upon the unshielded needle his observations might have helped to forestall Coulomb's denial of electromagnetism.

Lacépède also had practical ends in mind in considering the effects of the electric and magnetic fluids on animals. In addition to surveying the extensive

literature on medical applications of electricity and magnetism, he collaborated with the academician Baltazar Sage in experiments that subjected birds to virtual electrocution.[82] The birds given a drop of "volatile alkali" in their beaks, however, revived; those not so treated died. An antedote for an overdose of electric fluid was thus determined with possible applications for human lightning victims.

Early reports of Mesmer's claims had attracted Lacépède's interest. Speaking of his own speculations on the subject of magnetic effects upon living organisms, he said:

> Perhaps it will be recognized before long that these conjectures have already been confirmed by the facts reported concerning an *animal magnetism*.[83]

The final section of Lacépède's *Essai* occupies almost 200 pages, about a fourth of the entire work. It presented, by his own description, "the boldest of speculations" having to do with electrical phenomena in the heavens.[84]

The earth's internal heat not only produces but overproduces electric fluid; the vast surplus escapes through the earth's crust and forms an "electric atmosphere" that extends well beyond the atmosphere of breathable air.

> There it continuously produces remarkable effects upon which the order of our universe, and the various movements of the celestial bodies, depend in part.[85]

All planets, moons, suns, and comets, insofar as they are warm and not cold, are surrounded by similar electric

atmospheres. In order to estimate their magnitude, or at least establish reasonable limits, Lacépède investigated the electrical properties of small metallic globes in his laboratory. He measured the diameters of the electric atmospheres enveloping them and found them to be four times the diamenters of the globes. Those measurements, however, were affected by the presence of air; other experiments suggested that in a vacuum the diameters would be increased by a factor of 18. Since 4 X 18 = 72, Lacépède estimated that the electric atmospheres of planets and suns extend to at least 72 times their respective diameters.

The electric fluid is not absolutely stable; it tends to dissipate. The electric atmospheres around heavenly bodies retain their dimensions, however, because they are continuously resupplied by internal production of electric fluid. That means that the earth (for example) is continuously losing mass. As a consequence, according to Newtonian mechanics, the earth's orbit must be slowly but steadily increasing. Lacépède believed that astronomical data supporting this contention constituted the strongest evidence for his views:

> I admit that the totality of my hypothesis is not founded on incontestable phenomena, but we cannot, it seems to me, avoid admitting that the astronomical facts reveal an increasing distance of the earth from the sun, and by analogy we must admit a similar increase for the other planets.[86]

Lacépède's cycle of the universe was almost exactly the opposite of the one that would later be published by

Restif de la Bretonne, which was described in Chapter Five. Both begin with a violent expulsion of matter from suns. For Restif this matter begins life in the form of comets that continuously spiral closer and closer back toward the sun from which they originated, transforming into planets and ultimately falling into that sun. In Lacépède's system, the expelled matter begins in planetary form close to the suns and continuously moves away, transforming planets into comets around *other* suns with which they eventually collide and merge, completing the cycle.

The electric atmospheres play a small but critical role in this cosmic drama. The electric atmospheres do not have the shape of simple spheres centered on their planets. Light (*i.e.*, the luminous fluid) has a strong affinity for the electric fluid, as is evident in the luminosity of sparks. That means that luminous fluid is an anelectric material. It follows that there will be considerably more electric fluid on the dark side of a planet than on the bright side, where sunlight will have conducted much of it away. However subtle it may be, the electric fluid nonetheless has a certain amount of mass and the displacement of its electric atmosphere will shift a planet's center of gravity. This destabilizes the orbits and spins of planets and accounts for (among other phenomena) the precession of the equinoxes.

Lacépède, as we have seen, was candid in his admission that his universal system was not based on the absolutely

solid foundation of verifiable fact. He was aware of its hypothetical character. Nonetheless, he attempted to derive his cosmological speculations from up-to-date astronomical and physical data published by reputable scientists, as well as from his own laboratory investigations. This kind of system building, however, was rapidly gaining a negative reputation among the scientific elite and would be completely ruled out of French science after Thermidor. Lacépède got the message and did not emulate Lamarck's efforts to swim against the tide.

IX. JEAN CLAUDE DELAMETHERIE (1743-1817)

Delamétherie stood with one foot in the orthodox science camp and the other in that of the system builders. His career, like those of Lamarck, Lacépède, and Du Pont de Nemours, illustrates the fact that the two categories were by no means absolutely mutually exclusive. Although he made numerous contributions to many sciences, his principal role was as a latter-day Henry Oldenburg or Marin Mersenne; that is, as a scientific communicator. In 1785 he took over the editorship of the most important scientific journal of the time, the *Observations sur la physique* (later, the *Journal de physique*), and continued in that position until his death more than thirty years later.[87]

The *Journal de physique* had come to be known familiarly as "Rozier's Journal" after the abbé François Rozier who founded it in 1771. James McClellan notes that

> a succession of investigators have singled it out as being on the leading edge of change in the scientific press at the time and a landmark in the development of the scientific periodical.[88]

McClellan has shown that in spite of being to some degree in competition with the official publications of the Parisian Academy of Sciences and similar bodies, Rozier had nonetheless forged a fruitful working relationship with the institutions of orthodox science. The *Journal de physique* served as a central clearinghouse of scientific information in Europe before the Revolution.

McClellan and other scholars have suggested, however, that the journal's importance "seems to drop off" after being taken over by Delamétherie in 1785.[89] The basis of this assessment is twofold: first, that Delamétherie, unlike Rozier, was bitterly hostile toward the Academy of Sciences; and second, that whereas Rozier had facilitated Lavoisier's chemical revolution, Delamétherie fought against it. Henry Guerlac, for example, says that Delamétherie

> was violently opposed to Lavoisier's theories; the *Observations*, therefore, which had once been so hospitable to Lavoisier, became -- to a degree -- the journal of the opposition.[90]

The implication that the journal's significance rapidly deteriorated when Delamétherie assumed the editorship does not hold up under examination, however. It is certainly true that its role after the Revolution was considerably diminished, but that should not be attributed to alleged shortcomings of Delamétherie. The journal's decline was not absolute but relative; the changes in scientific practice wrought by the Revolution brought more specialized periodicals to the fore, such as the *Annales de chimie* that began publication in 1789.

Contemporary testimony indicates that the *Observations'* prestige was still undiminished after the Revolution had begun, more than four years after Delamétherie had become editor. The English commentator Arthur Young, writing in December 1789, praised Rozier for

establishing the *Journal de physique*, "which, take it for all and all, is by far the best journal that is to be found in Europe."[91] Young's accolades were undoubtedly based largely on the journal's earlier accomplishments, but they indicate that its reputation was still intact at the end of the 1780s.

When Delamétherie died in 1817, his replacement as editor was H. M. Ducrotay de Blainville, who had been assisting him for the previous five years. Blainville had begun his career as a protégé of Cuvier, but their collaboration ended in a bitter break in 1816. Blainville became a prominent proponent, second only to Geoffroy, of the transcendental anatomy that Cuvier detested. His election to the Académie des Sciences in 1825 was the focus of a harsh struggle; Blainville was the candidate of François Arago, who was then challenging Cuvier for leadership of the Academy. The rival candidate whom Blainville defeated was none other than Cuvier's brother, Frédéric.

The weakening of Cuvier's iron grip on French science is reflected in Blainville's rise; the culmination came in 1832 when he succeeded to the late Cuvier's chair of comparative anatomy at the Muséum d'Historie Naturelle. Blainville later published a history of science designed to highlight the "great men" of each era, in which he ignored Cuvier and celebrated Lamarck.[92]

Despite a certain eccentricity, Blainville was widely recognized as a first-rate scientist. His assumption of the editorship of the *Journal de physique* is another indication that the periodical had not become completely estranged from the scientific community during Delamétherie's three decades at the helm.

As for Delamétherie's attitude toward the Academy of Sciences, the most thorough history of that institution notes that although he "was particularly articulate in his tirades against all official learned societies," he did not launch a public attack until well after the initial upsurge of the Revolution:

> Delamétherie's first opportunity to lash out at the Academy came in the eulogy of the deceased crystallographer Romé de l'Isle, which he inserted in the April 1790 issue of his periodical, the *Journal de Physique*.[93]

Delamétherie's attitudes toward the Academy, then, cannot be accurately assessed in isolation from the political context of the Revolution. He radicalized along with his contemporaries and his anticorporate critique of official scientific institutions developed alongside his republicanism. On the specific issue mentioned above, it is difficult to fault Delamétherie in retrospect for arguing that Romé de l'Isle, a pioneer of crystallography, had been arbitrarily excluded from the Academy.

The charge that Delamétherie turned the *Journal de physique* into an organ of hostility toward Lavoisier's new chemistry also requires some qualification. Lavoisier's

bibliographers cite an article in the 1787 volume of the journal which

> shows that de la Métherie personally rejected the new nomenclature. He was impartial enough, nevertheless, to allow everybody the right to his own judgment and belief regarding the old and the new chemical nomenclature.[94]

The new nomenclature was much more than a matter of new words, of course; new terms like "oxygen" implied new concepts that were inseparable from Lavoisier's antiphlogiston theories of combustion, respiration, and chemical reaction.[95]

One of Delamétherie's most important contributions was the "Preliminary Discourse" he wrote at the beginning of each new year for the *Journal de physique* to summarize the scientific developments of the preceding year. Beginning in 1786 and appearing almost every year until 1817 (excepting only a brief period around 1794 when political events intervened and publication was suspended), Delamétherie's detailed commentaries represent a synoptic contemporary view of the entire scope of international science over three critical decades.

Delamétherie was perhaps the most prominent French scientific opponent of the new chemistry. An entry in the minutes of a session of the Academy of Sciences dated 16 Ventose An 6 (1798), signed by Fourcroy and Bertholet, explains that "those who concern themselves with chemistry can be divided into two classes: partisans of phlogiston and antiphlogistonists." The international leader of the

prophlogiston contingent was Joseph Priestley, who "undoubtedly is pleased to know that he can count among his cothinkers in France the citizens Lamétherie, Sage and Baumé." Everyone else, they add, belongs to the class of antiphlogistonists.[96]

Listing Delamétherie ahead of two veteran members of the Academy of Sciences is an indication of his standing in the scientific community. The opposition of academicians and foreign scientists of Priestley's stature meant that the orthodox majority could not end the debate by simply reading their opponents out of "real science." Nor were Fourcroy and Berthollet quite accurate in their claim to have all the others on their side; Lamarck, as we have seen, was no phlogistonist, but he had published lengthy works attacking the new "pneumatic" chemistry.[97]

Lavoisier's quantitative methods were clearly aimed at transforming chemistry into an analytic-empiricist science, so it is not surprising that irrepressible system builders like Lamarck and Delamétherie would oppose it. Like Lamarck, however, Delamétherie was more than a system builder. His scientific training was in medicine and he practiced as a physician for ten years before switching to full-time scientific journalism. During his long career he published a number of works on geology, mineralogy, and physiology, among other sciences, that were well received by his contemporaries.[98]

Official recognition of Delamétherie's scientific abilities was slow in coming. Although he was a member of a number of provincial and foreign academies, he was never elected to the Parisian Academy of Sciences, at least partially because unlike many of his peers he refused to campaign for admission. Only in 1801 was he finally awarded a teaching position, as a colleague of Cuvier's, at the *Collège de France* as professor of natural history.

Standard histories have recalled him primarily for his place on the losing side of the debate among chemists over phlogiston, but Delamétherie also made significant contributions to chemistry. One in particular had important consequences for nineteenth-century industrial chemistry. In 1789 Delamétherie demonstrated a means of producing soda from common salt in combination with vitriol (sulfuric acid) and charcoal. Further discoveries by Nicolas Leblanc later made the manufacture of soda from sea salt feasible on an industrial scale, but the Leblanc process, patented in 1790, was partially based on Delamétherie's original work.

It is not, however, as a practicing scientist but as a system builder that Delamétherie is relevant to the present study. Early in his career he published a preliminary version of his natural philosophy as *Essai sur les principes de la philosophie naturelle*. In the 1780s he revised and expanded it considerably and brought it out in two volumes as *Principes de la philosophie naturelle*. After

the Revolution he revised it once again -- separating the moral sciences from the physical sciences in partial deference to the post-Thermidorian mood -- and republished it under two new titles.[99]

Before and during the Revolution, Delamétherie acted on his conviction of the connection between politics and science. He had no qualms about utilizing the columns of the *Journal de physique* as a vehicle for his political opinions. His republicanism appeared radical at the outset, but the Revolution overtook him and left him defending a moderate position against Jacobinism. In both cases, his against-the-stream political pronouncements testified to a certain degree of courage on his part.

Delamétherie's mixing of politics and science took organizational form as well. Like Lacépède, he was a member of the masonic lodge that attracted so many progressive, reform-oriented scientists, the *Neuf Soeurs*. Furthermore, Delamétherie's participation in the *Société d'Histoire Naturelle* had a political character. "The naturalists who regrouped on 27 August 1790," Roger Hahn says, were "eminently aware of the political implications of their association."

> From the very beginning, they seem to have attracted naturalists of a republican stripe who felt it essential to introduce a political dimension into their carefully articulated constitution. Delamétherie, the editor of the *Journal de Physique*, Creuzé-Latouche, a member of the National Assembly, and the academician Fourcroy, who joined the naturalists before the year was out, were all enthusiastic associates of the reform movement and

> operated in this voluntary association almost as if it were a political club.[100]

Delamétherie's historical reputation has been diminished because his system-building proclivities and his position on the losing side of the phlogiston debate combined to give Cuvier a low opinion of his scientific worth. Since Delamétherie had not been a member of the Academy, Cuvier was not required to write an official eulogy, but he attempted nonetheless to pronounce the verdict of French science by writing the entry on Delamétherie in Michaud's biographical dictionary. His primary source was a biographical sketch by Blainville in the *Journal de physique* that was based on some of Delamétherie's own autobiographical notes.[101]

Although Cuvier admitted that Delamétherie's journalistic activity was "always very useful," he added:

> It would have been more so if the editor had been more knowledgeable and impartial, but La Métherie was rather ignorant of mathematics and very uneducated in everything having to do with the history of animals and plants. Furthermore, with regard to those things that he knew best, such as chemistry and mineralogy, his preconceptions clouded his judgment.[102]

Cuvier listed Delamétherie's major published works, but complained that

> the style of all these writings is dry; there are few original ideas, even among those that the author puts together to create his systems.[103]

Whatever the merit of Cuvier's critique, there is a great deal of irony, if not hypocrisy, in his charge that Delamétherie's scientific judgments were not impartial. The attack on Delamétherie's ignorance of the history of

animals and plants reflected Cuvier's obsession with his own *bête noire*, the idea of evolution. Delamétherie, he says,

> has tried, as have many other self-styled scientists, beginning with Maillet and Robinet, to derive the diverse forms of animals from their habitual activities, and to account for their origins, along with plants and even the planet itself, as a process of rising out of a primitive liquid.[104]

Cuvier was an incorrigible practitioner of the indirect polemic. His targets here were not De Maillet and Robinet, nor even Delamétherie, all of whom were dead, but rather his living antagonists Lamarck and Geoffroy, whose evolutionary theories he labored to discredit at every opportunity.

Delamétherie's activities and writings featured an intertwining of political radicalism, evolutionary biology, and opposition to the new trends in chemistry that were being promoted by Lavoisier. This combination, so typical of those who possessed the spirit of system, was bound to meet with Cuvier's disapproval. As a result, Delamétherie's posthumous reputation as a scientist was weaker in elite circles than it might otherwise have been.[105]

Delamétherie's Principles of Natural Philosophy

The 1787 editon of Delamétherie's *Principes de la philosophie naturelle* does not include his name on the title page or anywhere else in the two volumes; it also states that it was published in Geneva. It could not have been expected to pass the French royal censorship, because

Delamétherie's principles went well beyond physics, biology, geology, and cosmology to encompass the moral sciences as well. His conclusions, two years before the uprising at the Bastille, harshly condemned despotism and predicted social revolution.

Delamétherie admitted that the quest for knowledge has its pitfalls:

> It is perhaps arrogant for a being as puny as man to want to extend his vision so far. Consulting less the feeble resources he possesses than the depths of his imagination, he invents systems that he takes for reality and thinks he has grasped nature's secrets.[106]

He denied, however, that he had fallen into that trap:

> I have not in the least given in to this temptation, the result of a frivolous curiosity. . . . Nor have I abandoned myself to any leaps of imagination. All that I have to say are simple corrolaries of facts that we observe every day.[107]

Delamétherie's primary method was based upon analogy,

> the only means we have of extending our knowledge beyond what our senses directly present to us. It could be that we will be fooled, but we have no other path to take.[108]

Analogy is a form of reasoning "by which I can make judgments about what was or will be on the basis of what is."[109] Although it cannot be considered a source of certain knowledge,

> this kind of reasoning is of the greatest importance. It is simple and we know of no way to do without the truths it provides.[110]

The only alternative, as he saw it, was to ignore the most significant problems by hiding behind the *hypotheses non fingo* slogan:

> This stance that Newton took may perhaps be the wisest to emulate in all of the difficult questions, but that would not advance science at all.[111]

Delamétherie noted the increasing tendency in French scientific circles to denigrate the role of speculative thinking. Like Condorcet in his eulogy of d'Alembert, Delamétherie identified that tendency as skepticism: "A universal skepticism has taken over with regard to the most interesting topics."[112] Skepticism, he added, "has always been particularly annoying to me."[113] Among the "most interesting topics," of course, are those that have the most to do with human well-being, namely those that come under the heading of the moral sciences.

> Newton established the principles of the laws of physical bodies in his immortal work: I dare to believe that I have established those of metaphysics and morality in this one.[114]

The fundamental principles of moral science, Delamétherie asserted, "can be entirely submitted to calculation and thus be placed in the category of applied mathematics."[115]

With regard to such things as intelligence, judgment, will, love, hate, hope, and so forth:

> These qualities are magnitudes that can be "more" or "less"; therefore they can be treated mathematically. Calculation can be utilized in this field of philosophy as has been done in developing various branches of physics, such as mechanics, optics, astronomy, etc. This field of knowledge, for that reason, can achieve the same degree of certainty that they have, by means of which they have made immense progress.[116]

As strong as these claims appear, Delamétherie was quick to admit that "there is plenty of inexactitude in this calculus."

> Nevertheless, I do not consider this calculus to be useless, in spite of all its imperfections. It can give us a more exact idea of the variety of existance.[117]

Delamétherie's moral science was to be founded on the premise that all knowledge is derived from sensory inputs, a contention that

> is hardly doubted any more. Locke has shown that all the so-called innate ideas are in reality transmitted to us by our sense organs.[118]

The desire to quantify led Delamétherie to construct a pleasure-pain calculus based upon the formula

$$P \text{ or } D = xS \text{ times } n \text{ times } d$$

where **S** = sensory input,
x = intensity,
n = number,
d = duration,
P = pleasure,
D = pain.[119]

"This method may seem dry and arid to some people," Delamétherie wrote,

> but I believe that it is only through this path that metaphysics can attain the same certitude as the other mathematical sciences, and to raise it above the kind of chaos it has been mired in up until now. All those leaps of imagination called systems have been banished from physics. Nothing is allowed but facts and analogies derived directly from facts. In the moral sciences, the friend of truth must likewise distance himself from empty phrases, those products of brilliant imaginations that writer-orators so often substitute for reason.[120]

Delamétherie's moral calculus, however, was rather like Descartes' mathematical physics: an ideal to aspire to rather than a practical technique that could really be implemented. There is no indication that the pleasure-pain formula yielded any significant results.

Although the method of analogy proved more fruitful, its epistemological limitations were also inescapable. To the question "how do we know what we know?" Delamétherie was forced to admit:

> My answer will often be: I do not know. We are not knowledgeable enough about the order that exists in the universe. . . . Let us collect the greatest number of facts possible, let us draw conclusions from them by means of analogy, and let us declare our ignorance about everything that is impossible to know.[121]

Like it or not, then, we are stuck with probabilities as the basis of practical activity. In spite of the disclaimers, Delamétherie proceded to tackle such profound problems as the origin of the universe and the origin of life. Of the matter that composed the universe,

> earth, water, air, and fire are the four primary principles that we know. In all of chemical analysis, no others have ever been discovered.[122]

> At first spread out throughout space, the atoms of this material moved more or less rapidly according to their respective forces and soon united according to the laws of their affinity. It was primarily the earth and water elements that formed the great masses. Then they were joined by air and fire in the form of heat, fixed air, inflammable air, and acid. All of these fundamental materials in combination crystallized and formed into enormous globes. Meanwhile, the most subtle part of the air, together perhaps with a bit of water and the part of fire that was left uncombined, conserved their liquidity, and in this form constituted the ethereal matter as luminous fluid and fluid in the spaces between great globes. The part of

> these fluids that touched the globes, the densest part, made up the atmospheres.[123]

The difficulty of accounting for the origin of life was illustrated by the chemical fact that "oil," a necessity for life, does not exist at all in the mineral world. "If oil is necessary for life and there was none before life existed," Delamétherie asked, "how did life originate?"[124] His answer followed from a concept of generation strongly influenced by the new science of crystallography:

> I regard generation as a kind of crystallization. The seeds of male and female, in mixing, produce the same effect as two salts: the result is the crystallization of the fetus. All substances adopt a particular form: each salt, each metal, each stone has its crystallization; each plant, each animal has its appropriate form, which does not vary. In a word, everything in nature crystallizes.[125]

Experiments had shown, Delamétherie pointed out, that even the purest water in a tightly sealed container sooner or later will be teeming with simple living organisms. Apparently the crystallization of life does not absolutely require "the organs of already organized beings."[126] In the beginning, then, the materials necessary for life could have united spontaneously:

> Some waters began to stagnate in lakes and ponds, in which were mixed airs, acids, and various earths. The action of the sun corrupted the mixture. . . . First of all, some green spots appeared, which then developed into mosses; then new plants developed, then insects, which multiplied prodigiously. The tiny species managed to easily develop into the larger ones, because nature has no more difficulty producing an elephant than a mite. This work probably took some centuries; we know how slow this march is.[127]

Delamétherie devoted considerable attention to whether or not the earth and other planets are living organisms. "Many insects," he noted, "crawl on the surface of our bodies." Lice, fleas, and mites of all descriptions are found on all large animals. The microscope has discovered that on insects "even smaller animalcules abound."[128]

> This prodigious quantity of living beings has led Mr. Réaumur -- that profound investigator of nature's secrets -- to say: "There is, perhaps, no animal that is not, for smaller animals, what the earth is for us; and there may be here, as everywhere in nature, a progression where the end is unknown." This analogy is certainly well founded and it would be difficult to deny it. Our insects, our animalcules, then, perhaps consider our bodies as their world.[129]

This idea, Delamétherie pointed out, leads directly to the earth being regarded

> as a kind of living being, with all of the organized beings on its surface playing the role of its insects. . . . Pursuing the same analogy, all of the celestial globes may be insects on an even larger animal. Finally, then, the universe itself is considered to be a large animal.[130]

Delamétherie was by no means prepared to uncritically endorse such hypotheses:

> These analogies, however well founded they may seem to those accustomed to meditate on the whole of nature's works, will only be admissible with difficulty. It is hard to believe that the earth and the other heavenly bodies are alive. Their regular movements seem quite different from those of animals that are entirely free. . . . However, we have seen that the movements of animals follow necessarily from their sensations, and so are really no more free than those of inorganic bodies. Moreover, the movements of the heavens are not as uniform as has always been supposed.[131]

Furthermore, the idea that the planets are alive cannot be dismissed simply because they are so much larger than we are. The microscopic animalcules are

> far, far smaller, relative to the whale, than the whale is relative to the earth. The latter relationship is expressed by a ratio of ten-to-the-seventeenth-power to one; the former by a ratio of ten-to-the-twenty-second-power to one. It is therefore demonstrated that one cannot use the size of the earth as a decisive argument that it is not a living being.[132]

It is evident that if mites reason about the universe they know, they would have to have the same ideas about our bodies that we have about the earth. In the final analysis, however, "all of these analogies are very weak and we willingly admit that they are not perfectly conclusive."[133]

It is significant that Delamétherie, a geologist who was fully conversant with the most advanced scientific developments of the day, would not rule out the possibility of planets as living organisms. It was not a "fantastic" idea. Those who, like Du Pont and Restif, took an affirmative stand on the issue would not have been regarded by most of their educated contemporaries as foolishly credulous for doing so.

Delamétherie believed that the truths of natural philosophy are valuable because they "teach us what place humans must occupy" in the greater scheme of things.[134]

Unlike Bernardin de Saint-Pierre, who believed that humankind is the focus of all creation, Delamétherie arrived at an ecological outlook similar to Bernardin's by assuming that humans *do not* occupy a privileged rank among species:

> Nature has worked according to a plan that is little known to us, but the greatest harmony reigns among the existing beings that we can perceive: each part of the grand assemblage is made for the whole, and the whole for each part. Humans serve insects, as insects are useful to humanity. It could just as well be said, then, that the universe was made for the insect as that it was made for humans. Both have their place, as do all the other beings.[135]

No matter how broad, long, and objective a view the natural philosopher tries to take, however, he is human and his work is incomplete if it does not address itself to the most urgent human concerns. By drawing political conclusions from his principles of natural philosophy, Delamétherie acted upon his conviction that moral science should not be confined to pure abstraction but should be brought down to earth.

Delamétherie shared Rousseau's concept of the social contract. Instead of the freedom and independence that should ideally prevail, however, the executive arm of the state had become too powerful and had turned despotic.

> That is what happens every time when the executive power or the government is not obliged to be accountable to an assembly representing the leadership of the nation. All governments where sovereign national assemblies do not exist will be more or less disguised despotisms, more or less absolute and cruel.[136]

Delamétherie warned that this intolerable situation could not continue much longer:

> Despotism is rapidly leading us to the kind of abasement that has brought about the fall of the greatest empires. We are, perhaps, not very far from that at present.[137]

The political role of the moral scientist, as he saw it, was twofold:

> On one side, the philosopher cries to the despot whose hands are covered with the blood of his fellow men: "Monster! Do you want to reign over deserts?" On the other side, he reassures the fearful people that the cause of their troubles is in fact weaker than they are and would be easy to get rid of.[138]

But overthrowing despotism is not, by itself, a sufficient social program, so Delamétherie also presented some proposals for social reform. First of all, he observed, "the question of property rights is one of the most delicate in all of philosophy."[139] Changes in property arrangements, however, were necessary to remedy the "inequality of conditions," which had "reached the highest level."

> The immense disproportion in the division of the land is contrary to the right that each person has to that which is necessary for his existence. . . . The immense fortunes promote idleness, luxury, and all the vices that follow from them. . . . Everything therefore points toward the need to remedy these revolting abuses. The greatest of these is the state of the tiller of the land, who usually owns no property at all. A poor peasant works all year without missing a single day, a single hour, and yet he is often in need of bread. He has only poor clothing, all pleasure is forbidden him, so that the fruit of his labors can be absorbed by a few individuals who live lives of the greatest dissipation, thereby adding insult to injury.[140]

Delamétherie wanted to see "hand-labor remunerated at a price high enough" that the peasant could have "not only the bare necessities of life, but an honest comfort that would permit him some pleasure."

> It might be objected that it would be impossible to limit the big landlords without hindering productivity. But nothing seems simpler to me. The extension of landownership could be limited in such a way that each citizen might possess a certain amount according to his social rank. The rest of his fortune can go into movable property: townhouses, factories, commerce, etc. By these means, fortunes need not be limited, but the number of owner-cultivators could be considerably increased.[141]

Delamétherie's land-reform proposals were by no means as radical in conception as Restif's call for the complete abolition of private property. In fact, however, the peasants' land hunger proved to be among the most powerful sources of revolutionary energy. It took a social revolution, including the radical phase of the Jacobin Republic, to begin to resolve the inequality of land distribution that Delamétherie lamented.[142]

The scope of Delamétherie's conception of natural philosophy is evident in the preceding summary. As editor of a journal that stood in the vanguard of European science, he had the materials at hand to create a comprehensive synthesis of scientific knowledge. He would have rejected the epithet "system builder," which had gained negative connotations in the scientific community, but his two-volume *Principes de la philosophie naturelle* was nonetheless an admittedly hypothetical, speculative

attempt to give a complete account of the workings of nature. Its historical interest lies in its identification and discussion of those questions for which contemporary science could provide no positive answers.

X. ETIENNE CLAUDE, LE BARON DE MARIVETZ (1728-1794) and LOUIS JACQUES GOUSSIER (1722-1799)

All of the natural philosophers discussed above were better known for their political, literary, or other activities than for their published works of scientific speculation. Marivetz and Goussier were exceptions in this regard; their primary claim to public notice was the seven-volume, 4,000-page *Physique du monde* on which they collaborated and for which they were still gathering material when the Revolution intervened.[143] Their inclusion in Michaud's *Biographie universelle*, an early-nineteenth-century "who's who," is an indication that they had achieved reputations as "esteemed savants" in the eyes of at least some of their contemporaries.[144]

Although the Baron de Marivetz was a scientific amateur and Goussier was a professional, Marivetz was the leader of the team, partly by virtue of his privileged social position under the old regime. In addition to his hereditary title, his family was wealthy and he had connections at court. Like Lacépède, Marivetz was a progressive-minded aristocrat with a strong inclination toward scientific pursuits.

His older friend Goussier, a member of a number of provincial academies, had studied mathematics and physics and had collaborated with La Condamine on the latter's memoirs on measuring degrees of the meridian. He was also

an inventor and had been commissioned by d'Alembert to write a number of articles on the mechanical arts for the *Encyclopédie*. During the early phase of the Revolution he was named by Roland, then Minister of the Interior, to a post in the *Division des arts et metiers*.

Marivetz and Goussier began working together on a study of the physical geography of France. The collaboration blossomed into the much more extensive project that resulted in the publication of *Physique du monde*, an exhaustive system of cosmogeny and cosmology incorporating a unified theory of planetary motion, light, fire, electricity, and chemical reactions. Unlike other contemporary natural philosophers, Marivetz and Goussier showed little interest in extending their system to include the moral sciences. There is no hint of Rousseauian social criticism in *Physique du monde*, which was dedicated to the king.

The work on physical geography, though more limited in scope, was not insignificant; it included a study of interior navigation in France with specific proposals for the construction of new canals[145]:

> The hydrographic map that accompanied the work was very carefully prepared, and the majority of the canals shown on it and proposed by Marivetz were in fact constructed in the nineteenth century.[146]

According to Lalande's *Bibliographie Astronomique*, Marivetz's fortune had been previously diminished by bad investments, but the publishing costs of the expensively

produced *Physique du monde* were the final blow that ruined him. Lalande, an orthodox astronomer whose ideas Marivetz had directly challenged, concluded with the unkind assessment that "his ideas, his zeal, and his fortune were thus rendered equally useless."[147] Although Lalande had been a target of Marivetz's polemics, he had also served as a primary data source; his works were cited throughout *Physique du monde* far more often than those of any other astronomer.

Marivetz and Goussier -- defenders of a modified version of Cartesian vortex theory -- were obviously not in the mainstream of French science in the 1780s. It is fair to say, however, that they were not absolutely cut off from it, either. Reviews of *Physique du monde* were generally not as appreciative as Marivetz had hoped they would be. Some were polite but noncommittal; others were negative and critical.[148] Marivetz devoted considerable energy to answering the latter. Orthodox academicians tended to be dismissive, but the *Physique du monde* was not entirely ignored. Bailly, for example, briefly criticized it in his *Histoire de l'astronomie moderne*, provoking a lengthy rejoinder from Marivetz.[149]

The work received better treatment from some rival system builders. Carra devoted considerable space in his own *Nouveaux Principes de physique* to a friendly exchange of views with Marivetz and Goussier.[150] Lacépède praised them as "two ingenious authors" and said that in their work

"these two great physicists have deployed all the resources of science and genius."[151] Later, however, Lacépède criticized their central proposition and Marivetz felt obliged to answer the challenge.[152]

Goussier survived the Revolution, but Marivetz was not as fortunate. As a titled aristocrat, and especially one whose family fortune had been derived from collecting the hated salt tax, the Baron tried to keep a low profile as the anger of the oppressed mounted. He retired to his country home but was arrested in December 1793, charged with having been affiliated with the ex-Monarch's traitorous court, and executed near the end of February 1794. He had indeed held an official position at court, but there is no indication of counterrevolutionary activity, or even political interests of any kind, on his part.

The beautifully printed and bound volumes of the *Physique du monde* into which Marivetz had poured his soul and his fortune also met a tragic fate, providing a dramatic image of a truly neglected text:

> This work has become very rare, one portion of the copies having been bartered to the grocer and those remaining in the bookstore after the author's death having been transported to the Arsenal to be used as cartridge papers.[153]

The Counterposition of Systems

Marivetz was strongly committed to the proposition that science could best be advanced by the fair and open combat of opposing ideas. Whole volumes of *Physique du*

monde were therefore devoted to careful expositions of the systems of other natural philosophers, ancient and modern, on the critical scientific issues. Almost 200 pages of the first volume were given over to an epoch-by-epoch analysis and refutation of Buffon's *Epoques de la nature*.[154] The first 62 pages of Volume IV are a polemic with Lacépède on the underlying cause of elasticity.[155] A detailed discussion of Marat's *Decouvertes sur la lumière* occupies 50 pages of the same volume.[156] Of the three volumes on heat, the first two consist entirely of detailed summaries of the doctrines and discoveries of 46 dead and living philosophers, including Bacon, Descartes, and Newton. Last but not least of the 46 was Marat, who again was the beneficiary of a 50-page exposition of his *Recherches physiques sur le feu*.[157] In the final volume, Marivetz added a supplement to include a 35-page discussion of Delamétherie's views on fire, which had recently appeared in print.[158] Of Delamétherie, Marivetz writes: "With the exception of the celebrated Euler, it is he who most closely approaches our principles."[159]

Marivetz categorized physicists into two groups: in one were the "etherians" or "impulsarians"; in the other, the "attractionists." The former, of which he counted himself a member, believed that interstellar space is filled with "a rare, subtle, and resistant fluid called ether."[160] The others held that God endowed all matter with an inherent force of attraction. Although latter-day

followers of Newton were virtually all in the attractionist camp, Marivetz never tired of pointing to their master's frequent usage of the ether concept.

Marivetz's belief that he and Delamétherie shared common principles was undoubtedly based on Delamétherie's solid rejection of attractionism.[161] Being in philosophical accord with the editor of the *Journal de physique* was but one reason why Marivetz might justifiably have considered himself a legitimate -- indeed, an important -- participant in contemporary scientific discussions. He was well aware, however, that he represented a minority viewpoint against the dominant faction of Newtonian epigoni, but he refused to be intimidated by their accusation of system building:

> The word *system* is the warcry of our adversaries, and it is as authors of a system that we have been denounced, often by those who have not read our work. We will accept that designation and will be so bold as to declare that it is indeed a system that we are presenting. The abuse to which that word has been subjected, however, forces us to recall its real definition. We define it the way it was defined by that man renowned for the greatest genius and wisdom in analyzing ideas.[162]

"That man" is Condillac, whose *Traité des systêmes* was the primary inspiration for d'Alembert's and all subsequent condemnations of system building. Marivetz correctly pointed out, however, that Condillac did not rule out all systems, and in fact held certain kinds of systems to be beneficial for the advancement of knowledge. While Condillac rejected the "abstract systems" of Descartes and

Leibniz, he praised "real systems" based upon "well established facts" and "genuine scientific principles."[163]

D'Alembert later summarized Condillac's conception of a good system as one that "reduces, as much as possible, a large number of phenomena to a single one that can be regarded as their principle."[164] Real sciences, he declared, must be "confined to facts and to consequences deduced from facts."[165]

Marivetz wholeheartedly agreed. The purpose of *Physique du monde* was precisely to reduce all of the most important physical phenomena -- heat, light, gravitational attraction, and more -- to the single principle of impulsion. To those Newtonians who refused to accept impulsion as a fact, Marivetz replied that it was certainly and undeniably a consequence deduced from facts. The facts were the well defined motions of physical bodies that must have a physical cause. The Newtonians' invocation of action at a distance was certainly no explanation and their *hypotheses non fingo* was but an expression of agnosticism. Whenever the state of an object's motion changes, it can be inferred that some material entity has impinged upon it. That, Marivetz maintained, is simply deducing a necessary consequence from facts, which Condillac and d'Alembert agreed was scientifically legitimate.

By insisting that there must be a material entity pushing planets through their orbits and causing heavy objects to fall to earth, Marivetz was not, of course,

claiming discovery of a new truth. He and Goussier were trying to bring an old one back into favor:

> We resusitate the system of the plenum. We fill all space with a fluid that causes the rotation of the celestial spheres, which are driven by the fluid and obedient to its laws of motion.[166]

All motion in our solar system, according to these authors, originates from two fundamental facts:

> *Space is filled with an elastic fluid* and *the sun, turning in the midst of that fluid and rubbing against it, agitates all of the particles of which it is formed.*[167]

The rotation of the sun and the friction of its surface against the elastic molecules of the fluid create "a general and constant state of vibration" throughout the entire universe.

> These vibrations are the determinant causes of all of the modifications of matter; it is to this alone that all the states through which matter passes, all the phenomena of nature, should be attributed. Without it there would be no motion and no heat.[168]

Light, also, "is nothing other than the effect of vibrations of an exceedingly elastic fluid that fills all space."[169]

The consequences of the solar friction for planetary motion were spelled out in nine propositions that the authors said "contain our whole system."[170] The propositions laid the basis for a mathematical treatment of great whirlpools of subtle fluid carrying the planets around the sun. Marivetz's critics could hardly be blamed for suspecting his system of being a throwback to the discredited cartesian vortices.

The prominent astronomer and historian of astronomy Jean Sylvain Bailly had drawn that conclusion. Commenting on the *Physique du monde*, Bailly wrote:

> The fruitless efforts of the great names among the partisans of the vortex hypothesis have demonstrated its falsity. Vortices deserve to be forever forgotten.[171]

Marivetz, however, denied the charge of cartesianism. Noting Bailly's faint praise of the *Physique du monde*'s clarity of style, Marivetz responded:

> We must not have been very clear, sir, if you were able to confuse our concept of impulsion with cartesian impulsion, and our vortices with those that had previously been proposed, with which you seem to want to equate ours. The differences could not have escaped you; should you not have made them known?[172]

While hailing Descartes as "perhaps the greatest of all those whose names are inscribed in the annals of science," Marivetz took pains to distance himself from Descartes' "justifiably criticized errors."[173] He rejected the doctrine of the absolute plenum, for example, and acknowledged that the Newtonians' proof of its impossibility was "decisive."[174] For Marivetz, the elastic fluid spread throughout space was made up of tiny spherical particles. It follows geometrically that no matter how closely they are packed together there must be a considerable amount of "void" in the interstices. Free movement is therefore not impossible, as it would be in Descartes' absolute plenum.

It should be noted that orthodox academicians like Bailly and Lalande were not the only attractionists with

whom Marivetz debated. The long polemic against Lacépède at the beginning of Volume IV centered on that point.[175] Lacépède had accepted the concept of an elastic universal fluid, but argued that its elasticity could not be explained on the basis of impulsions between its particles. To the contrary, the particles must have an inherent power of attraction for each other independent of all external impulsion. The definition of elasticity, after all, is the tendency of an entity's particles to return to an original configuration after having been displaced. How can such a restorative action be envisioned without mutually attracting particles? Marivetz responded: "You have attacked me vigorously, my dear Comte; I will try to mount a defense worthy of an adversary such as yourself."[176]

Marivetz did not deny the existence of attraction, but insisted once again that it was not an explanation but rather something that needed to be explained. "In the final analysis," he said,

> attraction is nothing other than impulsion, as Newton suspected, as well as 'sGravesande (one of his most illustrious disciples), the Royal Society of London (the ministers of his temple), his worthy rival Euler, and so many, many others.[177]

As for Marat, Marivetz evidently held him in high esteem as a physicist in spite of Marat's adherence to attractionism. Marivetz attended Marat's public experimental demonstrations, corresponded with him on scientific matters, and utilized more than a hundred pages of *Physique du monde* to give a faithful account of Marat's

work on heat and light, adding very little in the way of criticism.[178] Marivetz could not, however, entirely avoid disagreeing with Marat's attractionist explanation of the diffraction of light, according to which light rays are attracted by the edges of objects.

Marivetz recognized that all theories of light transmission could be divided into two categories: those that treated light as particles in a void and those that treated it as waves in an ether. He was a partisan of waves, of course, and cited the opposing viewpoint as an example of the misapplication of mathematics to physics. Mathematicians, he charged, had drawn false conclusions from the proposition that light is propagated in absolutely straight lines, which is disproved by diffraction phenomena.[179]

Among the prominent system builders of the prerevolutionary era, Marivetz and Goussier were by far the most mathematically proficient. They were capable, therefore, of engaging Bailly, Lalande, and others on their own ground. The orthodox astronomers and mathematicians, however, rarely responded to Marivetz's challenges. Bailly's critique, noted above, was brief and suggested that he had based his negative opinion of *Physique du monde* on an *a priori* rejection of the vortex hypothesis.

Physique du monde represented perhaps the last major effort to stem the irresistible tide of action-at-a-distance metaphysics. Its failure was not determined by its

arguments being refuted but by their being ignored. Action at a distance won total acceptance as a fundamental fact of nature needing no further explanation, and the concerns of Marivetz and Goussier were declared to be outside the realm of scientific inquiry. The problem they had so heroically struggled with was never resolved; it faded away.

CHAPTER SEVEN

POSTSCRIPT: POST-THERMIDOR

Although the spirit of system was effectively banished from institutionalized science in France after the Revolution, it did not simply pass out of existence. As has often been noted, paradigm shifts rarely occur by the cumulative changing of individuals' minds; usually an old generation must die out and be replaced by a new one before the transformation is complete.[1]

Marat, Carra, and Marivetz did not survive the Revolution. Of the others, most continued, with varying degrees of steadfastness, to promote their natural philosophies. The most unyielding was Lamarck, who continued his assault within the Academy at a terrible cost to his personal welfare (his transformist hypothesis, of course, was advanced in the post-Revolutionary period). Du Pont de Nemours continued supplementing and revising the *Philosophie de l'univers* until his death in 1817. Bernardin de Saint-Pierre worked on a sequel to the *Etudes,* which would be published posthumously as *Harmonies de la nature.* Delamétherie revised his *Principes de la philosophie naturelle* to give separate treatment to the moral and physical sciences; he published it under two new titles in 1802 and 1805.[2] Lacépède played down his speculative views

in deference to his friend and patron, Bonaparte, but he never renounced them.

Although Carra was not among the survivors, his friend and collaborator Mercier published *De l'impossibilité du systême astronomique de Copernic et de Newton* in 1806; Mercier's love of paradox and attacks on rationalist philosophy earned him a reputation as "Rousseau's ape."

Furthermore, the system builders of the earlier era were not the only continuators of the practice. Some new authors of works of broad scientific generalization also appeared on the scene. Georges Cabanis's *Rapports du physique et du moral de l'homme* exemplifies a post-Thermidorian attempt by a respectable scientist to tackle some of the "big questions." As a medical man, Cabanis aimed at laying the foundations for a synthesis of physics and physiology.

Cabanis was a member of the Institute's Second Class. When the Academy of Sciences was reconstituted in 1795 it was as the Institute's First Class. A Second Class was established in posthumous tribute to Condorcet's call for a "science of man": the Class of Moral and Political Sciences. While in one sense legitimizing the existence of such sciences, it also established an institutional barrier separating them from the "hard" sciences. The moral and political sciences were thus quite literally assigned the status of second-class science.

Cabanis was a central figure in the *Idéologue* tendency along with Antoine Destutt de Tracy and D. J. Garat. Politically the *Idéologues* represented a conservative liberalism no less committed than Bonaparte to maintaining social order, but they resented the Emperor's dictatorial methods. The *Idéologues* became the primary focus of democratic intellectual opposition to Bonaparte, and since the Second Class of the Institute was their vehicle, the Emperor abolished it in 1803. Thus it was that even this tame and segregated center of "moral and political sciences" came to be perceived by Napoleon as a threat to the social order he had achieved.

Cabanis intended to keep his *Rapports du physique et du moral de l'homme* within the limits of mainstream science. Like Condorcet before and Comte after, Cabanis held the reductionist view that the social sciences would ultimately yield to the analytic-empirical methodology of the physical sciences. Even so, his book came under attack for daring to speculate about a moral-physical nexus. In the introduction to the second edition in 1803, Cabanis protested that his critics had unjustly portrayed him as a metaphysician "trying to overturn some doctrines and establish others with regard to the nature of first causes." In solid Newtonian fashion, however, he denied framing hypotheses: "We consider such causes to be outside the sphere of our research and forever hidden from the means of investigation available to mortals."[3] Nonetheless,

Cabanis's attempt to locate a "principle" in the cerebral system that would mediate the influence of the *morale* on the physical body could certainly be expected to arouse suspicions among the orthodox.

Their suspicions were not totally unjustified. In spite of his historical reputation as a pure mechanical materialist, Cabanis's scientific outlook did indeed have something in common with the speculative natural philosophers of the earlier era. Martin Staum says that Cabanis's

> metaphysical convictions . . . in fact resembled the ancient Stoic belief that the world is one, rational, and animated by a single binding force, or pneuma.[4]

Although these views were fully expressed only later in unpublished correspondence, Staum argues that one letter in particular "made explicit attitudes already apparent in the *Rapports*."[5] But while Cabanis's *Rapports* in a sense reflected continuity with the system builders of the 1780s, the reflection was considerably distorted by post-Thermidorian social pressures.

The Scientific Underground's Legacy to Social Thought

Especially in the area of social thought, the spirit of system continued to make important contributions in both France and Germany. The impact of *Naturphilosophie* upon German social thought is most evident in the Schelling-to-Hegel-to-Marx progression. The Marxist concept of scientific socialism is itself a further development of

Rousseau's insistence that science not be divorced from human affairs.

In France the *Idéologues* reflected, somewhat schizophrenically, the conflicting influences of the system builders and their orthodox opponents. Utopian socialism, however, and particularly Charles Fourier, stood more directly in the tradition of the speculative natural philosophers of the French Revolution. Fourier's contributions to social criticism were among the most important of the early nineteenth century; "he was among the earliest to advocate the absolute equality of women with men" and was "the real creator of nineteenth-century socialism."[6]

Fourier shared Rousseau's fundamental criticism of the sciences: "Our progress in a few of the natural sciences -- in mathematics, physics, chemistry, etc. -- is useless, for it has not provided us with a remedy for any of man's ills."[7] Although Fourier was more prone to debate his predecessors than to acknowledge learning from them, his frequent citations of Bernardin de Saint-Pierre's *Etudes de la nature* attests to the latter's influence. Also, Darnton calls attention to the "many similarities" between Fourier's *Théorie des quatre mouvements* and Bergasse's *Théorie du monde et des êtres organisés.*[8]

The parallels between Fourier and Restif de la Bretonne are particularly striking: their communistic social theory, their radical ideas for reforming human

sexual relations, their "penchant for neologisms and typographical experimentation,"[9] and above all their speculations concerning copulating planets. "A planet," Fourier wrote, "is an androgynous body, provided with both sexes and functioning as a male through copulations at the north pole, and as a female by those of the south pole."[10]

A scholar who has studied the work of both Fourier and Restif has written that Fourier's

> cosmogeny is weird and fantastic. The galaxy was a vast field of copulation with planets engaging in sexual relations . . . . All matter was permeated by the stuff of life, the passions, rendering the galaxy fundamentally biological. In [Fourier's] Romantic imagination . . . the universe lost its dead, mechanical image acquired since the intellectual triumph of the Newtonian world-machine, and became organic, pulsating with life throughout. Biological more than physical in nature, the universe . . . had a life of its own, and history was characterized by the cycle of its birth, childhood, maturity, and eventual death.[11]

It is clear, then, that the system builders' creative worldview did not reach a dead end at Thermidor but continued to stimulate important areas of French intellectual life well beyond the Revolution. In scientific thought *per se*, however, the *Naturphilosophs* exerted an ongoing beneficial influence in Germany that was denied to their French counterparts. The significant impact of *Naturphilosophie* on the development of electrical theory will be discussed in a postscript to Chapter Eleven.

It is noteworthy that the late appearance of *Naturphilosophie* on the stage of official French science, when it finally did occur, was stimulated by the renewed

social ferment leading up to the 1830 explosion. In the debate between Cuvier and Geoffroy, Geoffroy was widely perceived to be carrying the banner of *Naturphilosophie;* his disputed views on the metamorphosis of plant and animal parts had much in common with Goethe's.[12] Although the debate simmered beneath the surface for a number of years, it only boiled forth into public confrontation in 1830. The historian of this debate links Cuvier's willingness to bring his battle with Geoffroy into the open "to his anxiety in the months preceding the Revolution of 1830." Furthermore:

> The considerable support Geoffroy received at that time can be attributed in part to the association in the public mind between Geoffroy's view of nature and the liberal ideology of the 1830 Revolution.[13]

Lamarck, Geoffroy, and Blainville's transcendental anatomy, which they counterposed to Cuvier's conservative teleological anatomy, represented a significant legacy of the scientific underground of the old regime.[14] It directly influenced the important work of the dissident scientist-politician François Raspail in France,[15] and it struck a responsive chord among radical anatomists in Britain as well, as Adrian Desmond has demonstrated.[16]

Both French science and German science flourished in the nineteenth century. The influence of *Naturphilosophie* did not destroy German science by driving it into obscurantism. Nor did strictures against the spirit of system destroy French science by overly narrowing its

focus. It is valid to note, however, that these factors rendered French and German science significantly different, with differing styles and emphases.

"Better-or-worse" judgments made from the perspective of the late twentieth century should not be of primary concern when trying to understand how knowledge was sought in the late eighteenth century. If such judgments have been made here, the purpose has been to correct an imbalance; to demonstrate that the traditional dismissal of the work of the system-building natural philosophers as worthless is unjustified even by the criteria of tunnel history.

PART III:
Heat, Light, and Electricity according to Jean Paul Marat

CHAPTER EIGHT

THE PHANTOM AND THE SCIENTIST

Jean Paul Marat's role in the Revolution has left him, after two centuries, still one of history's most controversial characters. Revolutions divide societies with a line of blood and engender enduring loyalties on one side and hatreds on the other. Marat, more than any other individual, more even than Robespierre, has remained the focus of passionate emotions unleashed by the French Revolution.

What Marat symbolizes has never been expressed more poetically than in these lines by Victor Hugo:

> Marat: that is the great fearsome specter of the ages. If you want to know its true name, cry down into the abyss the word *Marat!* The echo from the depths of infinity will answer you: *poverty*! . . .
>
> They guillotined Charlotte Corday and they said *Marat is dead*. No. Marat is not dead. Put him in the Pantheon or throw him in the sewer; it doesn't matter -- he's back the next day. He's reborn in the man who has no job, in the woman who has no bread, in the girl who has to sell her body, in the child who hasn't learned to read; he's reborn in the garrats of Rouen; he's reborn in the basements of Lille; he's reborn in the unheated tenement, in the wretched mattress without blankets, in the unemployed, in the proletariat, in the brothel, in the jailhouse, in your laws that show no pity, in your schools that give no future, and he reappears in all that is ignorance and he recreates himself from all that is darkness. Oh, beware, human society: you cannot kill Marat until you have killed the misery of poverty.

The author of *Les Misérables* concluded by warning that "as long as there are *misérables* there will be a cloud on

the horizon that can become a phantom and a phantom that can become Marat."[1]

The persistence of this powerful phantom, so loathed by haves and yearned for by have-nots, has made a dispassionate evaluation of the historic Marat all but impossible. Fear of the phantom, and of its reappearance, has led countless polemicists and historians to seriously distort their portrayals of Marat the man, the politician, the doctor, and the scientist. Some of the distortion has undoubtedly been conscious; most was probably unconscious, and a great deal is attributable to the practice of recycling history, wherein historians have simply transmitted false evidence and fallacious conclusions of earlier authors upon insufficient examination.

In general, conservative and liberal historians alike have made Marat out to be a villain with few if any redeeming qualities. This attitude has negatively affected all aspects of their collective portrait of Marat, including his physical appearance (hideous), his character (criminal), his psyche (pathological), his medical practice (charlatanesque), and his experimental physics (pseudoscientific).

Marat's appearance and personality are of no concern here; it will simply be noted that the descriptions making him out to be repulsively ugly do not accord well with the charge against his character that he habitually seduced the beautiful wives and fiancées of his friends and patrons.

Phipson, for example, reports without comment the depiction of Marat as a "sallow man with pockpitted countenance, black flat hair, blood-shotten blinking eyes and spasmodically twitching mouth -- the incarnation of the repulsive," while also repeating the allegations that he seduced the artist Angelica Kauffmann and the Marquise de l'Aubespine.[2]

The present study is concerned primarily with Marat the scientist and secondarily with Marat the medical practitioner, but neither of these can be completely separated from Marat the revolutionary politician. A fairly sharp chronological line can be drawn between his scientific/medical practices and his political activities: the latter occurred almost exclusively from 1789 to the end of his life. Nonetheless, insofar as the scientist was father to the politician, the historic Marat resists dichotomization.

The objective determination of Marat's place in history is beyond the scope of this study; the present goal is the more modest one of placing his scientific activities and ideas in the context of the science of his contemporaries. As opposed to previous studies that have portrayed Marat's science as pseudoscience or quackery, or, conversely, have tried to justify it in terms of modern science, the point to be made here is simply that Marat's scientific practice was fully legitimate as science was understood in the 1780s. To put the argument bluntly: not

that Marat's investigations and theorizing were either good science or bad science, but that they *were science*. Marat was not, it will be argued, on the "fringes" of science in the eyes of his contemporaries, but was at the heart of it (although he can, indeed, be considered to have been on the fringes of the scientific elite).

Marat's character, personality, and temperament are not a major consideration here, but the extreme charges that he was a criminal, a psychopath, or both will be addressed, since it would be difficult to reconcile either with the picture of Marat as a solid, serious, and rather ordinary participant in the medical and scientific fields.

The Allegations of Criminality

The modest material conditions in which Marat chose to live at the pinnacle of his political influence would seem to support his claim to have been, like Robespierre, incorruptible. There is very little in the historical record that would suggest any venality on his part. A long-standing rumor, however, accepted as truthful or at least plausible by a number of important historians, charged Marat with having committed a museum robbery in England in 1776. Phipson assembled all of the supporting evidence, analyzed it at book-length, and concluded that the revolutionist Marat had indeed been a common thief. Phipson's book claimed that the charges against Marat "have

been very carefully examined and are here presented without, at least, any conscious bias."[3]

The moral lesson that Phipson drew from this tale was stated explicitly:

> We thus find that Jean Paul Marat was by no means the irreproachable figure he is so often depicted, but belongs, rather . . . to that more questionable class of politicians who qualified for revolutionary triumphs by early infractions of the criminal law. But nations have never been saved, although thrones may be overturned, by patriots of this stamp.[4]

Phipson's core case is that Marat, using the alias "Jean Pierre Le Maître," stole some rare coins from an Oxford museum, was caught, convicted, and sentenced to prison, but escaped and returned to France. That such a theft occurred is documented and undisputed; the matter of controversy was whether Le Maître (who also used the name "Mara") and Jean Paul Marat had in fact been the same person. (The previous generation of Marat's family had spelled their name "Mara.")

The earliest version of the rumor equating them appeared in an English publication in 1793 and was thereafter buttressed by "eyewitness testimony," all of which Phipson weighed and accepted as probably true. By systematically validating all of the rumor material and systematically denying the authenticity of contrary evidence, he constructed an apparently plausible scenario leading to the conclusion that Marat had committed the crime.

Phipson admitted at the outset that the charge was, on the surface, rather implausible. For one thing, Marat definitely appeared back in France in respectable circumstances just three months after Le Maître was sentenced to five years hard labor in England. Phipson cited evidence, however, that a month after Le Maître's conviction a prison break occurred in which six unnamed convicts escaped; was Le Maître among them?

> Assuming an affirmative answer to the question, which the present theory necessarily requires, we have now reached the close of the first phase of Jean Paul's enigmatic career.[5]

The most important piece of evidence that Phipson denied was a letter written by Marat datelined "Douvres, 11 avril 1776," in which he says he is leaving England.[6] That date falls between Le Maître's arrest (February 1776) and trial (March 1777); Phipson granted that Le Maître was definitely in prison at that time. Since the origins of the letter and even the addressee are unknown, however, Phipson judged that it was deliberately written after the fact by Marat to cover up the crime.[7]

The evidence for Phipson's case, then, hardly rises to the level of circumstantial; it is based on a long chain of implicit links to Marat deemed "probable" or "not impossible." Nonetheless, it was judged at least plausible by those who, like Phipson, perhaps felt a need to believe the worst about Marat, including such important historians of the French Revolution as J. M. Thompson.[8]

Fortunately for historical accuracy, more evidence surfaced later to definitively resolve this issue. In 1966, Robert Darnton, examining the archives of the Société Typographique de Neuchâtel, discovered a letter from Marat to F. S. Ostervald, one of the Société's founders.[9] The letter is dated May 14, 1776, and places Marat in Geneva at that time. Marat and Le Maître were therefore undeniably two different individuals; Marat was not a thief. The episode is of historiographical interest not for what it reveals about Marat, but as a case study in how historians can allow their social prejudices to sway their judgment. It would be interesting, though an unjustifiable digression from the purpose of the present study, to go through Phipson's argument point by point to assess the probability of each element in retrospect.

Before leaving Phipson, his treatment of Marat as scientist should be noted, since his study purports to cover Marat's entire prerevolutionary career. Although he says "the validity of Jean Paul's scientific claims" is "a question which does not concern us here," he observes that

> when, session after session, he pressed himself, his experiments, and his discoveries upon the Academy of Sciences, it would have none of them.[10]

As will be shown below, this is a demonstrably inaccurate interpretation. From the documents appended to Cabanès's study of Marat's science, which he repeatedly cites, Phipson should have been aware that Marat's

relationship to the Academy of Sciences was considerably less one-sided than this statement suggests.[11]

Phipson made the interesting (and for the most part accurate) observation that

> while the historians of the Revolution, with very few exceptions, unite in execrating the name of Marat, his biographers, with still fewer exceptions, laud him almost to the skies.[12]

Is it that Marat's biographers, upon gaining intimate knowledge of their subject, found him admirable after all, or is it that they are a self-selected group of individuals who were attracted to Marat in the first place because of their own prior radical ideological proclivities?[13]

In any event, the testimony of Marat's only American biographer provides a revealing insight into the problem of subjectivity. Louis Gottschalk's life of Marat was first published in 1927; it was reissued four decades later, in 1967, and his foreword to the later edition struck an apologetic tone for a "youthful endeavor" that had offered a rather positive portrait of Marat.[14] Although Gottschalk says that he found himself disliking Marat more and more as the years went by, he acknowledged that the cause of the change was not to be found in Marat but in the evolution of his own political outlook:

> What does candor require that a professor emeritus of nearly sixty-eight say about his first full-length book, which was essentially completed when he was then one third his present age? . . . If he had grown more conservative with the years, he might confess that he found it more difficult than in his youth to claim an empathy with so eccentric a personality as Marat's.[15]

The Allegations of Insanity

Although Marat's political polemics were hardly out of the ordinary for revolutionary Paris of 1789-1793, removing them from that context can make them appear to be the irrational ranting and raving of a lunatic.[16] In the hands of such bitterly antirevolutionary authors as Hippolyte Taine, the portrayal of Marat as a violence-obsessed madman served to damn by proxy the demands and aspirations of plebian Parisians -- the "mob" -- as unreasonable, irrational, and even insane.

In the case of Marat, historians of science have frequently followed the lead of the hostile political commentators. Douglas McKie, for example, in his biography of Lavoisier, offered this typical assessment:

> Jean Paul Marat was a fanatic; he was sincere; he was violent and bloodthirsty; he was enthusiastically and lyrically devoted to the Revolution; and he was probably insane.[17]

At issue here is not whether Marat was eccentric, moody, difficult to get along with, or suspicious by nature (none of which would be incompatible with being a scientist, as the example of Isaac Newton illustrates), but whether there are grounds for arriving at a clinical diagnosis of psychosis. An article by Charles W. Burr, M.D., frequently cited by later authors, concluded that Marat, judged by his deeds and writing, was a certifiable psychopath.[18]

Burr's article is by no means typical; the class bias animating his psychoanalysis of Marat is so blatant as to preclude the need for extensive commentary to deconstruct the text: Marat's "creed was simple -- all that the rich own belongs to the poor because they stole it from the poor," Burr wrote. "He belongs then among the insane, and is an example of paranoia of the political type."[19]

Doctor Burr, writing in 1919, had evidently been shaken by the Russian Revolution, in which he must have perceived Marat's phantom. In his distress he revealed the political component of his definition of insanity:

> The gentlemen who regard [Marat] as a political genius, e.g., the sincere members of the Bolshevik party of to-day, not only in this country, are themselves mentally abnormal. He is not the only lunatic in history who has had a following during life and after death.[20]

This professional man's social fears extended from communism to the feminist movement, reflected the generation gap separating parents and children, and included an element of xenophobia, as his comment on the roots of Marat's mental problems reveals:

> Mentally, in his earlier life in many ways he resembled the sentimental sympathizers with Bolshevism who are to-day making so much noise in America. It is noteworthy that almost all the Americans born among them have led shielded lives, have never been in contact with the realities of life, have never had to work (their fathers did that for them); the women advocates have failed in woman's first and natural function. Among the foreign born are internationalists, parasites, and those who left the countries of their birth for their countries' good.[21]

Burr's pseudofreudian screed, going beyond bias to bigotry, is an extreme example of the difficulties inherent in finding a genuine basis for judging a revolutionary leader insane. Marat and the Parisian people whose mentality he reflected were not crazy; they were *angry*, and their anger was far from irrational. The state of Marat's psychological health is impossible to evaluate with any degree of precision, but there is no indication that he crossed the qualitative line into psychosis.

Historians have cited Marat's suspiciousness and fears of plots against his reputation and life as evidence of paranoia. Whether or not his suspicions and accusations were exaggerated, they were certainly not completely unfounded. His complaints of a conspiracy of silence against him in certain scientific circles, for example, are corroborated by the correspondence of his bitter rival, the Abbé Bertholon.[22] Nor was his perception of being persecuted by his political foes imaginary; he was repeatedly hounded into hiding and exile, arrested, and ultimately assassinated.

Marat's Stature as a Physician

Marat's first profession was that of medical doctor. He practiced medicine in England from approximately 1765 to 1776, and then in France until he made the transition to full-time experimental physicist sometime in the early 1780s. Although he has frequently been portrayed as an

itinerant quack without credentials, a patent-medicine huckster, or a horse-doctor, he did in fact possess a valid medical degree and practiced as a high-society physician with a good reputation among the rich and famous, including high-ranking members of the royal court.

Marat's M.D. degree was awarded to him by St. Andrews University, Scotland, on June 30, 1775. Historians have often alleged that since Marat apparently attended no classes at St. Andrews he must have simply bought the degree. The diploma was signed by two legitimate medical examiners, however, who were apparently convinced of Marat's competence as a doctor. Their judgment is corroborated by two medical pamphlets Marat published a few months later, of which more will be said below.[23] Although this may seem insufficiently rigorous by modern standards of medical education, such comparisons are anachronistic. Marat's degree was legitimate by contemporary standards.

Evidence of hostility to Marat on the part of academic doctors of the University of Paris's Faculty of Medicine, and of Vicq d'Azyr and other members of the Academy of Medicine, has often been cited to signify that Marat had been shunned by the official medical profession and was not part of it. It must be remembered, however, that the official medical profession was in the preliminary stages of creation at the time Marat was practicing. The traditional Faculty of Medicine and the newer Academy of Medicine were in fact battling each other for the right to

determine, by means of control over licensing procedures, who was an official physician and who was not. Before the Revolution, of course, that was ultimately a matter of royal prerogative; both the Faculty and the Academy looked to the court to sanction their authority.

Meanwhile, Marat already clearly enjoyed the court's official approval and needed neither the Faculty nor the Academy of Medicine to ratify it. From 1777 to 1783 Marat held a position as physician in the household of the Comte d'Artois, a brother of King Louis XVI.[24] In the context of old-regime society, Marat was incontestibly a member of the medical profession, and a relatively high-ranking one at that.

The nature of Marat's employment by the Comte d'Artois has frequently been distorted, even by such influential historians as Thomas Carlyle and Jules Michelet, both of whom accepted for good coin the claim that Marat had been merely a horse-doctor. Although long known to be false, the historian of science J. L. Heilbron repeated it as recently as 1979.[25] The original motive behind this canard was to expose Marat as a faker for claiming more prestige than he really deserved. On the title pages of his major publications, Marat had identified himself as "Physician to the Comte d'Artois's Company of Bodyguards." According to his detractors, Marat was nothing more than a lowly veterinarian in Artois' stables.

Marat may in fact have served for a while as physician to Artois' squires as well as to his bodyguards; this seems to be the only real link between Marat and the stables. That would still not make him a veterinarian, of course, since squires are not horses.

It should be noted in passing that veterinary medicine was not held in the low esteem that Marat's detractors seem to think it was. Its utility to agricultural science was perceived as having highly beneficial economic implications. L. J. Daubenton's prestige as a scientist was based in no small part upon his contributions to veterinary medicine.[26]

In addition to his official position, Marat maintained a private practice on the side. Both the positive and negative aspects of Marat's reputation in Paris as a healer stemmed from his success in treating one particularly influential patient, the Marquise de l'Aubespine. She and her husband, the Marquis, credited Marat with curing her of a chronic respiratory ailment that other doctors had told them was uncurable. The case was publicized in the *Gazette de Santé*, increasing Marat's prestige.[27]

The "other doctors" struck back, however, by submitting the medicine Marat used to cure the Marquise to chemical analysis. The Abbé Tessier, doctor-regent of the Faculty of Medicine, issued a report stating that Marat's "eau-factice-pulmonique" was simply chalk dissolved in water; this was the origin of charges of quackery and

charlatanism against Marat.[28] Again, however, the context must be taken into consideration. Techniques of chemical analysis were in their infancy. The therapeutic efficacy of spa waters was routinely evaluated on purely empirical grounds. Marat's pharmacology fell within that tradition; there is no evidence of fraud on his part. The upshot of the affair was, on the one hand, that Marat attracted an upper-class clientelle and landed a position with a prince of the blood; on the other, that he gained the enmity of important segments of the medical profession.

Finally, mention should be made of Marat's published writings on medical subjects. One of his first published works, *A Philosophical Essay on Man* (1773), was primarily a metaphysical treatise, but he based his argument on anatomical and physiological data. In this book Marat joined the company of those who, like Descartes, attempted to demonstrate a specific physical link between the human body and the human soul. Although Marat's proposed solution to the old mind-body conundrum -- locating the physical link in the meninges -- proved to be of little consequence, the work serves to show that Marat had a command of contemporary anatomical knowledge.

The *Philosophical Essay on Man* also appeared two years later as *De l'homme*, a three-volume French edition published by Marc Michel Rey in Amsterdam. Rey, a major publisher of Rousseau's works, was "among the most powerful publishers of the Enlightenment."[29] That Marat was already

playing in the major leagues, so to speak, of French intellectual life, or was at least on their fringes, was further indicated by the rejoinder *De l'homme* provoked from Voltaire.[30] Some years later, during the Revolution, Marat remarked that Voltaire's irony might have wounded him if it had been deserved; in any event, it put him in good company since Voltaire "had taken the same liberty with Montesquieu and Rousseau."[31]

Marat also wrote two smaller, more specific medical treatises; one having to do with the erupting sores associated with gonhorrea, and another on a particular disease of the eyes that he called "accidental presbytopia."[32] Both papers present clear descriptions of his prescribed treatments and recount several case histories to illustrate their successful implementation.

After he had left medical practice to concentrate his efforts on experimental physics, Marat drew on both areas of his expertise to produce an essay on the medical uses of electricity. His *Mémoire sur l'électricité médicale* won first prize in a provincial academy's competition, as will be discussed further below. In the essay, he described his own clinical applications of electricity, surveyed the various ways other doctors had utilized electricity as a healing agent, and attempted to distinguish between valid and invalid therapies. He was sharply critical of the electrical cures claimed by his rival Bertholon, as well as of Mesmer's animal magnetism.[33]

To sum up the evidence concerning Marat's practice of medicine: His credentials were genuine, his clientèle was socially substantial, his writings demonstrated medical knowledge, and the testimony of his patients suggests that his healing powers were commensurate with contemporary standards. Without advancing claims as to the efficacy of his cures or as to any enduring contributions he may have made to medical science, it is reasonable to conclude that Marat was at least justified in presenting himself as a professional physician in prerevolutionary France. The charges of quackery, charlatanism, or fakery arose naturally from rivalries within the nascent profession, and were amplified after Thermidor as a weapon in the ideological war against Marat's revolutionary phantom.

Marat as Experimental Physicist

In the late 1770s Marat began to undertake a career change. Although he would remain in the medical service of Artois until 1783, he began to devote a considerable amount of time to the study of physical phenomena. Eventually this would become his full-time occupation and his transformation from physician to physicist would be complete. Throughout the 1780s, up to the eve of the Revolution, Marat dedicated himself to investigating and writing about the physical properties of heat, light, and electricity.

The numerous volumes that he published on these subjects describe literally thousands of experiments and observations. These works are more than empirical reports of experiments, however; by also attempting to offer a broad theoretical account of the phenomena they enter the realm of natural philosophy.

Marat's stated motives for changing professions and becoming a scientist have frequently been misconstrued, partially as a result of certain semantic shifts. Marat often claimed that he was motivated by "amour propre" and a quest for "gloire." To the modern ear these phrases, especially when translated into English, carry an uncomfortable connotation of narcissism and insufferable egotism.

Although "amour propre" can be translated literally as "self-love," its spirit is better rendered by "self-esteem," a rather more socially acceptable sentiment. As for "gloire," Marat's usage was linked with Rousseau's and was a commonplace of the era; its meaning had more to do with wanting to be of service to humanity than wanting to be an object of veneration. None of this has much to do with what Marat's real motives may have been, of course, which are unknowable, but putting his words into proper context softens the implication of disproportionate self-importance that has been used to suggest mental imbalance. In particular, critics have often cited Marat's challenge

to Newton's optical theories as an indication of megalomania.

The reception of Marat's scientific productions by his contemporaries has been a matter of historical controversy. Since Marat's relations with the Parisian Academy of Sciences ended in bitter recriminations, most historians have assumed that his scientific work was poorly received in general. This conclusion is flawed in three regards: (1) it overlooks the earlier, more positive reception accorded him by the Academy; (2) it treats the Academy as if it were the entire scientific community rather than the elite of that community; and (3) it exaggerates the monolithism of the Academy in the prerevolutionary era.

Marat's stature as a scientist was in fact analogous to his earlier medical reputation, as the following observations suggest:

(1) *His books on physics were taken seriously enough to be published in German translation, to be favorably reviewed by prominent journals, and to be cited in the works of other natural philosophers and scientists, including members of the Academy of Sciences.*

Christian Ehrenfriend Weigel (1748-1831) of the medical faculty of the University of Greiswald translated Marat's three main works on fire, light, and electricity into German; they were published at Leipzig by S. L. Crusius in 1782, 1783, and 1784, respectively.[34]

Favorable reviews or announcements of Marat's works appeared in the *Journal de physique*,[35] the *Journal de Médicine*,[36] the *Journal de littérature, des sciences et des arts*,[37] the *Courrier de l'Europe*,[38] the *Journal de Paris*,[39] the *Journal Encyclopédique*,[40] *L'Année littéraire*,[41] the *Nouvelles de la république des lettres*,[42] and numerous others.[43]

Authors who cited Marat's work favorably, at least with regard to certain aspects, included Romé de l'Isle, Sage, Van Swinden, Lamarck, Lacépède, Marivetz, and Carra.[44]

(2) *Marat submitted numerous essays on scientific subjects for competitions sponsored by provincial academies, some of which won prizes and honorable mention.*

Between 1778 and 1787 Marat entered essay competitions sponsored by the academies of Dijon, Rouen, Lyon, Bordeaux, and Montpellier.[45] In 1783 his entry on medical uses of electricity won the first prize, a gold medal valued at 300 livres, in a competition set by the Academy of Rouen.[46] His two submissions on Newtonian color theory to a competition set by the Lyon Academy in 1784 did not win, but were cited by the judges as the best of those that had opposed the Newtonian view.[47] In 1785, Marat's entry in another Rouen competition won honorable mention. Then the following year he again took Rouen's first prize, this time for an essay challenging Newton's explanation of colors in soap bubbles and other thin films.[48]

(3) *Marat was a serious candidate for the directorship of a national academy of sciences that was to be established in Spain.*

In 1783 one of Marat's wealthy backers, Philippe Roume de Saint Laurent, represented Marat's interests in negotiations with the government of Spain to win him the presidency of a projected Spanish Academy of Sciences.[49] Spain's chief minister, Floridablanca, directed his ambassador in Paris, the Comte d'Aranda, to meet Marat and sound him out. D'Aranda reported back to Floridablanca that he had gone to see Marat

> and asked him his intentions. He told me he wanted 24,000 livres while he was in Spain and 12,000 livres when he retired and that St. Laurent was empowered to arrange the conditions. I went to see some of his experiments. He knows that he has genius and talent. . . . He is a doctor by profession and has a good reputation.[50]

In a later letter to Floridablanca, d'Aranda suggested that Spain might be able to bargain Marat down to an 18,000-livre annual salary. As the negotiations progressed, however, it became apparent that Marat's reputation was not universally good; that in fact some influential figures in the world of Parisian science considered Marat to be a charlatan. In the end, Marat was not offered the job, but it is undeniable that he had been seriously considered.

(4) *Members of the Academy of Sciences, together with Benjamin Franklin, observed, participated in, and expressed appreciation of some of Marat's experiments.*

The Academy of Sciences twice appointed commissions to witness and report on Marat's work. In the first episode, Franklin joined academicians Le Roy and Sage in a series of visits to Marat's laboratory for demonstrations of his solar microscope.[51] The report of this first commission was highly favorable to Marat.[52]

In the second episode, academicians Le Roy and Sage undertook a long series of observations of Marat's optical experiments. This time, however, their official report was pointedly brief and noncommittal.[53]

Meanwhile, Marat's public demonstrations were attracting widespread interest. Allesandro Volta, "so famous for his experiments on electricity," had reportedly been "curious to see those announced by Marat."[54] According to Brissot's account, however, Volta's visit to Marat's laboratory occasioned a sharp dispute between the two electricians, leaving mutual enmity in its wake.

(5) *In addition to Marat's own public demonstrations of his experiments, a number of other savants offered public lecture courses and university physics courses based upon Marat's discoveries.*

In February 1780, a Mr. Paté, professor of physics at Châlons-sur-Marne, made a trip to Paris to meet Marat and discuss his experiments.[55] In June, Paté wrote to tell Marat that he had already announced the project of "making your precious discoveries known in the schools of Reims and

Châlons," and that the announcement had met with an enthusiastic response.[56]

In April 1780, the Abbé Jean Jacques Fillasier, member of several provincial academies, advertised a series of eight lecture-demonstrations based on Marat's *Découvertes sur le feu*; he announced further lecture series in May and June.[57] In July 1782 Pilâtre de Rozier announced a physics course intended to "make known all the experiments that Mr. Marat has demonstrated in the presence of the members of the Academy."[58] (The following year, 1783, Pilâtre de Rozier would earn enduring fame as the first aeronaut for his ascension in a balloon over Metz. In 1785 he became a martyr to the new technology of flight, perishing when his balloon exploded in fire during an attempted crossing of the English Channel.) In March 1783, the Abbé Miollan advertised a course on Marat's heat, light, and electrical experiments.[59]

Marat's physics attracted devoted followers. Among them were two of his future political enemies: Jacques Pierre Brissot de Warville and Charles Barbaroux, both of whom were to play important leadership roles in the Girondin tendency.[60]

(6) *A number of socially prominent personages admired Marat's scientific work and helped advance his career as a physicist.*

The Marquis de l'Aubespine was among Marat's most important patrons; it was at his residence in Paris that

Marat established his laboratory. The Comte de Maillebois served as an intermediary for Marat with the Parisian Academy of Sciences. The Duc de Villeroy, governor of Lyon, was his intermediary with the Lyon Academy. The Chevalier de Joubert, treasurer-general of the estates of Languedoc, was his go-between with the Montpellier Academy; Joubert also provided laboratory space for Marat in his home in Paris. The Baron de Feldenfeld represented Marat in correspondence with the Rouen Academy. The Comte de Wallis wrote to the *Journal de Paris* insisting that its editors publish reviews of Marat's work; they complied. Likewise, the celebrated Beaumarchais wrote a strong letter to the *Mercure de France* protesting that journal's failure to review Marat's *Découvertes sur la lumière*.

The Comte de Tressan, lieutenant-general of the King's armies, was a warm supporter of Marat's research. Tressan himself wrote a two-volume treatise on electricity; his views on the subject had much in common with Marat's.[61] Tressan's attraction to Marat is particularly interesting since Condorcet, in an official Academy of Sciences eulogy, praised him for a "philosophic spirit" that made him "a barrier against charlatanism."[62]

Taken together, this evidence should suffice to establish that Marat was a recognized participant in French scientific activities of the 1780s, and that he enjoyed a good reputation as a scientist in important intellectual milieux. While his contributions never became a central

focus of scientific discussion in the fields in which he participated, his work was widely known and discussed.

Nevertheless, Marat's life has been subjected to intense scrutiny and no fact that appears to put him in a positive light has gone unchallenged by his detractors. It has been alleged, for example, that some of the positive journal reviews were written by Marat himself, and that the recognition accorded him by the academies of Rouen and Lyon was less than spontaneous; Marat is said to have manipulated their award committees.

There is no doubt that Marat was an aggressive self-promoter; it is quite likely that he was indeed the anonymous author of some of the glowing journal reviews of his own work. In an exchange of letters with Delamétherie, for example, Marat appears to be complaining that Delamétherie had altered a review of *Mémoires académiques* that Marat had submitted to the *Journal de physique*.[63] The edited version was not hostile but it had omitted an explicit endorsement of Marat's anti-Newtonian optical conclusions. One would think that as editor Delamétherie's right to determine the content of unsigned material would be taken for granted; his answer to Marat, however, reveals that he felt the need to justify his action. Marat's attitude would suggest that these journal items were perceived by readers not as critiques by independent reviewers but rather as abstracts for which the author of an abstracted work bore responsibility. In either case, it

is evident that in the final analysis the various journals were under the control of their respective editors, and praise of Marat's work could not have appeared against their will.

As for the provincial academies, the charge is that some of their leading lights were secretly in cahoots with Marat, even to the point of allowing him to formulate some of their prize-essay questions himself, giving him opportunities to smuggle some of his pet theories into the competitions.[64] The evidence of such collusion is circumstantial, but nonetheless compelling. What it seems to reflect, however, is not conspiratorially rigged contests but a sort of run-of-the-mill factionalism wherein Marat's partisans labored to promote their man's ideas. This sort of cronyism, which is also evident in the episode of Marat's candidacy for the directorship of the Spanish Academy, certainly conflicts with the disinterestedness that characterizes the ideal of science. The issue, however, is whether it was atypical or whether it represented academic politics as usual.

The activities of Marat's provincial friends on his behalf have attracted the close scrutiny of historians primarily because of the notoriety he gained during the Revolution. The general extent of such cronyism in the academies is a problem awaiting further investigation. Nonetheless, one conclusion can be drawn from these episodes: the fact that Marat had cronies, collaborators,

or supporters in the provincial academies indicates that he was part of a scientific community rather than an isolated crank. It is particularly revealing that his friends in the provinces were not marginal characters, but included such powerful figures as the Duc de Villeroy and the Chevalier de Joubert.

Historiography

For almost two centuries the general historiographical line with regard to Marat's science has been, with a few notable exceptions, one of unremitting hostility. Fayet's *La Révolution française et la science* reduced Marat's scientific aspirations to a vendetta against the Academy. Maury's description in his *L'Ancienne Académie des Sciences* is typical:

> Marat, that charlatan, that bloodthirsty fool, who threw himself into revolutionary excesses out of an insensate pride and a hatred of his betters, imagined himself to be a physicist capable of dethroning Newton.[65]

Many of the most familiar denigrations of Marat's scientific endeavors were summarized by the popular science writer Isaac Asimov. His assessment is of interest not as original scholarship but as a faithful reflection of the standard treatment Marat has been accorded by historians of science. Asimov identified Marat as "a journalist who fancied himself a scientist" and stated that "the papers he offered the Academy (containing some foolish home-grown notions on the nature of fire) were worthless."[66]

There have been exceptions to these completely negative depictions. Augustin Cabanès published a study in 1891 (and revised it in 1911 and 1925) that attempted to resurrect Marat's reputation as a scientist.[67] Cabanès' version of Marat's career is a Jekyll-and-Hyde story: a cerebral scholar transmuted into a terrible beast by the passions of the Revolution. Hugo Rozbroj published, in German, a rather different defense of the value of Marat's work, noting its Rousseauian influences and comparing it to that of Lamarck and Goethe.[68]

Among English-language historians, an article by J. W. Dauben represented a first step toward a balanced appreciation of Marat's science.[69] While sharing some aspects of the traditional assessments, it recognized that their condemnations of Marat were grossly exaggerated, that Marat was not an enemy of science, and that the leading academicians' opposition to Marat was not to his experimentation but to his speculative theorizing.

N. B. Mandelbaum's extensive study concluded that Marat's science "was quite acceptable given the standards of the time" and that "though second-rate, his work deserved to be taken seriously."[70] It is significant that Mandelbaum included the important pioneer of crystallography Romé de l'Isle among "second-rank scientists."[71] Nonetheless, Mandelbaum characterized Marat as a "neurotic" whose patterns of life and thought are best explained by his tendency toward paranoia.[72]

Gillispie on Marat

The most remarkable treatment of Marat's science in any language, however, is a generally fair account from a surprising source: C. C. Gillispie. Gillispie can in no sense be considered partial to Marat; in the final analysis he views Marat as a psychopathic enemy of science. Nonetheless, Gillispie has weighed certain aspects of Marat's scientific career and found, to his evident surprise, more substance than he expected.

Although Gillispie groups Marat with Mesmer in a chapter devoted to examining "examples of the breed" of charlatans, he begins by pointing out that, unlike Mesmer, "Marat was no mystifier."[73]

Aside from certain matters of style, Gillispie deems Marat's early pamphlets on gleets and eye disease to be a "pair of clear and otherwise creditable medical papers."[74] With regard to the one on gleets, Gillispie dismisses later charges of plagiarism as unfounded. He also notes that the paper on presbyopia reveals that Marat "was already well versed in optics" as early as 1776.[75] As for the prize-winning essay on medical electricity, Gillispie says that "the actual content of Marat's memoir is marked by good sense and skepticism."[76]

Gillispie's account of Marat's scientific work accords well with the central contention of the present study; *i.e.*, that Marat's natural philosophy was not alien to

contemporary science but was fully in harmony with it. Discussing Marat's *Recherches physiques sur le feu*, Gillispie observes that

> if a browser knowledgeable about eighteenth-century physics were to happen upon this treatise, or its companion on light, on the shelves of some large library, and were to open it and begin reading *in medias res* without knowing the identity of the author, he would be unable to distinguish the work from any of scores of other titles in the literature of minor experimental science.[77]

Marat was no dilettante dabbling in physics in his spare time; "he had assembled a considerable physical laboratory."[78] Of his experimental work, Gillispie says: "The labor he performed is staggering."[79] Nor was Marat's knowledge superficial: "As always, he knew the literature."[80] This was particularly evident in his investigations of electricity. "However far-fetched his theory," Gillispie writes, Marat "had mastered the electrical properties of bodies and the various methods of electrifying them."[81]

Most significant is Gillispie's assessment of Marat's experimental practice relative to that of his contemporaries as well as to more universal standards. According to Gillispie, Marat's experiments

> are impossible to distinguish in principle from others by means of which persons then and now recognized to be scientists have explored the world.[82]

At the risk of belaboring the point, it must be emphasized that the present study makes no claims on behalf of Marat's science that go beyond this point.

Gillispie's most surprising revelation has to do with Marat's translation of Newton's *Opticks*. Brissot claimed in his *Mémoires*, written after he and Marat had become bitter political enemies, that Marat had falsified the translation and had snuck his own theories into the notes as a weapon in his war against Newtonianism. Then he allegedly tricked a gullible old scholar, Nicolas Beuzée, into endorsing it as a ploy to win the approbation of the Academy of Sciences.[83] As would be expected, later authors hostile to Marat have repeated these charges.[84]

Gillispie notes, however, that a number of authors "largely favorable to Marat," including Cabanès, also took Brissot's allegations for good coin:

> On this score, it will appear, he needs protection from his friends among historians no less than from his enemies among philosophers. For his is an excellent translation. A careful comparison with the fourth edition of the *Opticks*, from which Marat worked, requires it to be said in all fairness that he was nowhere unfaithful to Newton's meaning . . . . In regard to Newton himself, the tone is impeccable, respectful without being adulatory.[85]

Furthermore, he adds, "Marat did append a critical apparatus to his translation, but it is not true that he smuggled his own theory into the notes."[86]

Finally, some of Gillispie's observations aim at putting Marat's critique of Newtonian optics into historical perspective. Gillispie, it will be recalled, elsewhere expressed the opinion that Goethe made a fool of himself with his *Zur Farbenlehre*. [87] His general assessment of Marat's optical theories is no less harsh. Nonetheless,

he holds that the Academy's negative report on Marat's optical experiments failed to give some of his contentions

> the attention that others like them have turned out to merit. For the matter of primary colors and the question of Newton's measurements of specific refrangibility have proved far from empty, though the discussion has owed nothing to anyone's ever having read Marat.[88]

By appealing to what has "turned out," Gillispie weighs his subject on the scales of later developments in the science of optics, which shifts the focus of historical inquiry away from the period under consideration. Whether or not later developments owed anything to Marat has no bearing on the intrinsic validity of some of his criticisms of Newton, which Gillispie acknowledges.

Another aspect of Marat's critique of Newton deserves mention:

> That Newton's published accounts of how he had reached conclusions is often idealized to the point of misrepresentation is one of the findings of recent Newton scholarship. . . . In general, I should say that Marat's suspicions of the published work come closer to an accurate intuition about the kind of window-dressing that Newton sometimes put around his findings than does anything else known to me in the eighteenth-century literature.[89]

The positive appreciations of Marat's work cited above represent a selection of Gillispie's views on the subject, of course; he concluded that Marat was a "would-be physicist" and a charlatan. Nonetheless, it should be evident from what has been quoted here that the conflict between Gillispie's final verdict on Marat's science and that of the present study is rooted not in Marat's

scientific practice, but in the authors' respective conceptions of history and science.

In addition to its honesty, Gillispie's discussion of Marat is noteworthy for being based on an actual reading of the texts. His charge that previous commentators, in France and elsewhere, had not bothered to read Marat's scientific works is apparently well founded.[90] That Gillispie has recently examined them does not entirely disqualify them as neglected texts; his attention was directed more toward their positive (as in "positivist") content and less toward the "far-fetched theories." The present concern is more with their theoretical aspect, which is to say with Marat's natural philosophy. Before turning directly to the texts, however, consideration must be given to the relationship between Marat's science and his politics.

Marat's Turn to Politics

Marat's angry correspondence with Delamétherie over the review of *Mémoires académiques* occurred in March 1788. Early in 1789 his first contribution to the political discussion preceding the Revolution came off the press: the *Offrande à la patrie*.[91] This pamphlet was an indication of Marat's final career change, this time from physicist to revolutionary journalist and politician.

There had been earlier intimations of the political passions that were to raise Marat to the forefront of Revolutionary events. As a young man in England he had

participated in the "Wilkes and Liberty" movement of the 1760s and 70s, and he had published a political book entitled *The Chains of Slavery*. Soon after his return to France, he published another political work, the *Plan de législation criminelle*.

The *Chains of Slavery* was a sustained polemic against tyrannical government in general and against the current British regime of Lord North in particular. Although Marat railed against all kinds of injustice, there was no advocacy of republicanism or democracy, and he urged his readers to be obedient to the laws of their country.

His *Plan de législation criminelle* revealed in embryo the compassion for the poor that would later animate his revolutionary activities. He argued, for example, that the right to sustenance was an inalienable human right; that stealing food for survival was not immoral and therefore should not be illegal. If the moral content of this sentiment seems rather trivial, it is worth recalling that criminal law in England at that time prescribed capital punishment for theft of property valued at five shillings. Grisly public executions designed to strike terror in the hearts of onlookers were not uncommon occurrences in England and France.

Marat's early efforts at social commentary were progressive-minded and humane, but neither work could be considered particularly radical. He nowhere suggested that the oppressed masses themselves could be the agency of

their own liberation; his program was one of social reform rather than revolution.

His first salvo of the French Revolution, the *Offrande à la patrie*, appeared in February 1788. It likewise was reform-oriented, praising the good king and his wise minister, Necker, but calling for their executive power to be balanced by a legislative power exercised by elected representatives of the Third Estate. He also proposed a program of civil liberties, including the rights to a free press, trial by jury, and so forth. Although none of this seems very radical, it is obvious in retrospect that the movement for the convocation of the Estates General had a powerful revolutionary dynamic, regardless of the conscious intentions of those who initiated it. Marat's first pamphlet was not particularly influential, but it constituted a small contribution to that movement.

In September 1789 Marat began to publish the periodical that after four days took the name ever afterward associated with his own: the *Ami du peuple*. Its publication would suffer brief disruptions due to police repression and it would change its name to fulfill particular political needs, but the periodical maintained an essential continuity until Marat's assassination. He was correcting proofs for the next day's issue when he was murdered on July 13, 1793; that final issue appeared the following day, on the fourth anniversary of the storming of the Bastille.

During the last four politically charged years of his life, his earlier scientific activities do not seem to have been at the forefront of his concerns, although he did not ignore them entirely. A comprehensive view of his journalistic production refutes the frequent contention that he was obsessed with hatred or jealousy of the Academy of Sciences, and with Lavoisier in particular. His writings on this subject constitute a minute fraction of his total output. Insofar as there was an obvious connection between his scientific and political lives, however, it can be located in his relationship to the Academy, as many commentators have noted. The Academy represented the scientific elite of the old regime and would undoubtedly have been among Marat's political targets even if he had never engaged in scientific pursuits himself. His prior experience with that institution certainly shaped the specific form of his attack, which was expressed most fully and directly in a long pamphlet published in September 1791 entitled *Les Charlatans modernes*.[92]

As previously noted, Marat's initial contacts with the Academy were generally favorable from his point of view. There was, however, an element within the institution that seems to have opposed him from the start; he later identified it as "the class of geometers and astronomers, which has formed a terrible cabal against me."[93] Among the leading lights of this grouping were Laplace, Lavoisier, Condorcet, Lalande, and Bailly.

Before the Revolution, Marat generally confined expressions of his antagonism toward the leaders of the Academy to private correspondence. His public demeanor was more discrete; polemics were for the most part conducted by proxy.

Brissot de Warville

Among Marat's friends and admirers who wielded their pens on his behalf was a prominent young journalist who was to become one of the Revolution's most important political figures: Jacques Pierre Brissot de Warville. In 1782 Brissot published a work entitled *De la Vérité* that included an account of a lengthy argument between Brissot and Laplace over the merits of Marat's scientific work.[94] Although Laplace was not identified by name, Brissot later acknowledged that the dialogue's "geometer" and "skeptic" were, respectively, Laplace and himself.[95]

Marat was pleased with what Brissot had written, and, according to Brissot's *Mémoires*, tried to recruit him as a spokesman and publicist for Dr. Marat. Although Brissot says he declined because he felt Marat was seeking a worshipful disciple, their intellectual camaraderie seems to have lasted until irreconcilable political differences drove them apart after 1789. The view Brissot presents of Marat in his *Mémoires* is interesting but cannot be taken at face value since it was necessarily influenced by the political bitterness that had developed between them.

Their earlier rapport was based on the compatibility of their scientific outlooks. Brissot was another political radical with the spirit of a natural philosopher. His *De la vérité* had the character of a prospectus for a multivolume work synthesizing all human knowledge of nature, from physical to social.[96] If he had actually carried out that project, he would have joined the company of the natural philosophers whose works are under consideration here.[97] As a man of too many projects, however, his prodigious energies were channeled in other directions. His scientific interests led him to the forefront of the Mesmerist movement and from there into the Kornmann group where Bergasse had assembled some of the most important publicists of the coming Revolution.[98]

Brissot's political path diverged from Marat's during the course of the Revolution. Brissot's influence was such that his political faction in the Convention was known as the *Brissotins*. This was the grouping -- later named *Girondins* by the historian-poet Lamartine -- that dominated the revolutionary government in the crucial period between the fall of the monarchy and the rise of Robespierre's Jacobin republic. Although ultraradical in its call to extend the Revolution by means of a quixotic military crusade against the crowned heads of Europe, the Brissotin tendency ultimately took a conservative stand on the social question: its idea of revolution stopped well short of social equality for the propertyless classes. This, of

course, led Brissot and Marat to opposite sides of the barricades.[99]

On May 31-June 2, 1793, an insurrection occurred in Paris that ushered in the Jacobin government of the Year II. The Girondins were driven from power and their leaders arrested. Regarding the debate in the Convention over the fate of the Girondins, Marat was said to have had "the predominant voice in the decisions that followed."[100] Brissot was executed with other Girondin leaders and collaborators on October 31, 1793.

In his *De la vérité*, Brissot had explicitly defended Marat's scientific claims against Newton:

> Newton examined light with a prism; he believed that its decomposition was only caused by the prism itself. He did not take into account the preliminary decomposition that occurs at the edges of the hole through which the light enters the darkroom. He failed to grasp the great principle of the decomposition of light at the edges of all objects.[101]

He also lambasted the Academy of Sciences' persecution of those who "dare to replicate Newton's experiments," who "find them erroneous and dare to attack his theory." In a burst of passion, Brissot exclaimed:

> But do not allow their shouts, their declamations, nor their persecution to discourage you in the least, O you who nature has gifted with a genius for observation and an unflagging ardor for research and truth! You who trusts only in experiment and not at all in the authority of great names! You have courageously overthrown the idol of the academic cult and have substituted for Newton's optical errors a system of well-proven interconnected facts.[102]

"I am writing," Brissot concluded, "the story of that famous physicist Mr. Marat."[103]

Brissot's defense of Marat encapsulated several themes of the preromantic revolt against the narrowness and emptiness of the Newtonian worldview. In his dialogue with Laplace, Brissot revealed some of the common elements uniting the natural philosophies of Marat, Carra, Bergasse, Bonneville, Mirabeau, and other political activists[104]:

> The academicians are so presumptuous, so pigheaded in their opinions, that they rarely condescend to examine the new ideas with which they are confronted.[105]

When a geometer (Laplace) was heard to dismiss "a physicist who had announced new experiments on light entirely contrary to Newton's system" (Marat) as an imbecile, a skeptic (Brissot) lept to the defense:

> *Skeptic:* But Mr. Geometer, an imbecile is capable of neither ideas nor the aptitude to utilize them. How can you believe that a physicist who has done more than six thousand new experiments, written several volumes on physics, and so forth has neither ideas nor the capacity to put them into practice?
>
> *Geometer:* What! He dares to doubt the infallibility of Newton and he is not an imbecile?
>
> *Skeptic:* But Newton was human and subject to error . . . .
>
> *Geometer:* Newton enjoys universal support; his detractors are obscure writers.
>
> *Skeptic:* And were Newton and Descartes not obscure before they became famous? . . .
>
> *Geometer:* But suppose I prove to you mathematically that Newton's system is true. Then will you admit that his detractors are imbeciles?
>
> *Skeptic:* But those detractors wave around their books full of figures, too. What can be done with this chaos of numbers? Appeal to nature! Look at the facts, because calculations must be based on

> facts . . . . I would rather trust my senses and nature than your volumes of numbers.

The skeptic then proceeded to enumerate some physical facts, all key elements of Marat's theory of colors:

> That the number of primitive colors is three rather than seven; that the solar spectrum is composed of no more than three colors; that colors are formed simply by decomposition of light at the edge of the hole through which the rays pass.

Geometer: These facts are false, absurd, impossible.

Skeptic: You remind me of the theologians of the sixteenth century who acted as if words were reasons. Have you seen these experiments?

Geometer: No.

Skeptic: Have you read the author's books?

Geometer: No.

Skeptic: Have you heard him speak?

Geometer: No.

Skeptic: You have neither seen, nor read, nor heard him, and yet you pass judgment! And you say *he* is an imbecile!

Geometer: I do not need to see, read, or hear him. These ideas contradict Newton, the Academy, and my calculations; therefore they are absurd and I have no need to examine them further.

Skeptic: That is precisely how the Aristotelians reasoned against Descartes. . . .

Geometer: Good God! What will become of us if we have to examine everything?

Skeptic: I agree that the task can be burdensome, but if you want to claim the Academy as the sovereign arbiter of the progress of human knowledge . . . then should it not listen before it condemns?[106]

The skeptic was unable to convince the geometer, who continued to insist that the physicist in question was a

fool and his defender devoid of logic. Although this dialogue was written by a master of the publicist's art and from a strongly partisan viewpoint, it convincingly depicts the irreconcilability of Marat's views with those of his orthodox opponents. Marat himself later carried these themes through in his own polemic against academic science, *Les Charlatans modernes*.

Marat and Lavoisier

Marat's polemics have frequently been blamed for sending Lavoisier to the guillotine. As the most often cited individual crime attributed to Jacobin science, it deserves some scrutiny.

Marat cannot be accused of having had a direct hand in Lavoisier's death, of course, since he himself had been murdered ten months earlier.[107] The usual allegation is that his attacks against Lavoisier in *Les Charlatans modernes* and in the *Ami du peuple* formed the indictment that sealed Lavoisier's fate. Marat did not single out Lavoisier for special attention, however. While the accusations he levelled against Lavoisier were harsh, they were no more so than those he directed at dozens of other public figures. Another famous scientist and member of the Academy whom Marat targeted, for example, was Jean Sylvain Bailly. Marat held Bailly responsible, as mayor of Paris, for giving the order to fire on demonstrators that resulted in the Champ de Mars massacre.[108]

In his polemics Marat referred to Lavoisier's position in the Academy, but rarely if ever alluded to his scientific views *per se*. Marat knew that his readers were not greatly concerned with debates over phlogiston or Newton's optical theories or even animal magnetism. His attacks centered on Lavoisier's public functions, particularly his role in the General Farm. Lavoisier had been part of that institution for 25 years and it was the source of his substantial fortune.

The General Farm was not an agricultural entity but a tax farm. Louis XVI's government financed its activities by legislating taxes, but it did not have its own tax-collecting apparatus. Instead, it sold the exclusive rights to collect taxes to a corporate body consisting of some of the richest men in France. These Farmers General paid the government a large sum of money, which constituted the state's revenues. To recoup their investment, along with a profit, the Farmers General organized the collection of taxes. The social implications are evident. Tax collection cannot be accomplished without police powers to force compliance. The General Farm, then, was seen primarily as an agent of repression and therefore was among the most hated of old-regime institutions; its members were commonly perceived as "bloodsuckers."[109]

It was as a member of the General Farm, and not as a scientist, that Lavoisier was sentenced to death. He was not singled out here, either; the entire group of 28

Farmers General was arrested, tried, and executed at the same time. Although Lavoisier had not been among the wealthiest of the tax farmers, he had taken on an administrative role that made him one of the most visible.

Most significantly, he was identified in the public mind with the construction of a wall around Paris that was designed to enforce the collection of tariffs on commodities brought into the city. Parisians hated the wall because the tariffs significantly raised the prices of food, wine, and other necessities. Furthermore, those caught trying to smuggle goods into Paris without paying the tariffs were routinely executed. This repression was perceived by ordinary Parisians as an outrage; the subsequent execution of the tax farmers was thus considered simply a matter of retributive justice. Lavoisier in particular, as the man held directly responsible for building the wall around the city with its 54 watchtowers staffed by armed police, could expect little sympathy from those who had suffered as a result of his policies. Earlier, during the insurrection triggered by events at the Bastille on July 14, 1789, 40 of the 54 watchtowers had been demolished by the angry Parisian populace.

Marat, in short, did not fabricate the charges against Lavoisier, although his writings undoubtedly contributed to the wave of indignation that engulfed Lavoisier and his fellow tax farmers. It was certainly not a decisive

contribution, however; Marat had been removed from the scene long before the wave crested.

Marat's personal relations with Lavoisier had been minimal. Brissot claims that Marat, early on, had pressed him for an introduction to Lavoisier. Brissot says he declined on the grounds that while he had made Lavoisier's acquaintance, he really did not feel that he knew him well enough to arrange such an introduction. It seems that Marat did meet Lavoisier in 1780, however, and borrowed a book from him. An inscription by Marat found in a book from Lavoisier's library reads: "This book of Lavoisier's was lent to me for my researches on medical electricity in 1780."[110]

Later, however, Lavoisier became known to Marat as one of the important members of the Academy of Sciences who opposed his scientific claims. Marat thereafter counted Lavoisier among his personal enemies. Marat could not accept having his work rejected by the Academy, which he thought to be a self-appointed elite restraining the development of science in order to protect its monopoly. The mutual interactions of his scientific and his political passions are evident here. Other natural philosophers, including Bernardin de Saint-Pierre and Delamétherie, also rebelled against the Academy, and gave similar reasons for doing so, but none evolved along the same political lines.

Marat's ultimately antagonistic relationship with the Academy was not a result of irrational professional

jealousy on his part, as many commentators have argued, but was a case of mutual incompatibility. On the one hand, Lavoisier, Condorcet, and Laplace, among others, were fully convinced that Marat was simply a charlatan, not to be taken seriously. Marat, of course, was no less convinced that Lavoisier, Condorcet, and Laplace were charlatans, using their institutional authority as a shield against the scrutiny of their science by independent scientists such as himself. There was no common ground on which their differences could have been resolved. The Academy attempted to read Marat out of the scientific community, but the Revolution instead drove the Academy out of existence.

The political tide that swept the Academy away cannot be attributed to Marat's influence. The call for abolishing it was broad and strong; Marat lent his voice to the campaign, but his was only one among many. Closing the Academy of Sciences was but a particular manifestation of the Revolution's general elimination of privileged corporate bodies.

With regard to the reciprocal charges of charlatanism that Marat and Lavoisier levelled against each other, historians have often unconsciously assumed that since Marat's was false (witness Lavoisier's enduring contributions to chemistry) then Lavoisier's must have been true. The two propositions are not logically connected, however; neither was correct in his characterization of the other as a charlatan.

There is no evidence that Marat ever tried to use his scientific activities to intentionally hoodwink anyone for financial gain. It might be argued that a charlatan could be motivated by the desire for *gloire* rather than money, but there is still no indication that Marat was trying to convince others of anything that he himself did not believe. In any event, the charge of charlatanism was inappropriate. As was suggested above and will be demonstrated further below, Marat's scientific ideas were not the *sui generis* productions of a crank but were firmly grounded in contemporary natural philosophy. The real reasons that Lavoisier *et al.* found Marat's claims exasperating and unacceptable are to be found in the nature of his work.

Marat published eight large and small works on physical phenomena throughout the decade before the Revolution. Some of these were prospectuses for or summaries of more extensive treatments of the same subjects. The main body of his scientific work is to be found in a trilogy: one substantial volume devoted to each of his three main concerns in physics: heat, light, and electricity. They were, respectively: *Recherches physiques sur le feu* (1780), *Découvertes sur la lumière* (1780), and *Recherches physiques sur l'électricité* (1782). Each of these will be examined in turn in the next three chapters.

CHAPTER NINE

MAKING HEAT VISIBLE: *RECHERCHES PHYSIQUES SUR LE FEU*

The earliest natural philosophers identified fire as a central concern. Heraclitus of Ephesus concluded that fire was the underlying unifying principle of all of nature:

> All things are an equal exchange for fire and fire for all things, as goods are for gold and gold for goods.[1]

Empedocles' four-element theory, which included fire as one of the four fundamental materials of nature, was taken up by Aristotle and dominated natural philosophy for more than two millennia. Although an alternative concept of element championed especially by Robert Boyle began to challenge this notion in the seventeenth century, the four-element theory retained a strong position among natural philosophers even at the end of the eighteenth century. As was seen in Chapter Six, above, for example, Lacépède took the four-element theory for granted and Lamarck wielded it vigorously against Lavoisier and his new chemistry.

It is understandable, then, that from earliest times "What is fire?" would have presented itself as an obvious focus of scientific research. Some historians of science have been unable to resist the temptation to call it a "burning question."[2]

The difficulties presented by the question can be appreciated by temporarily stepping out of the eighteenth-century context and considering it in the framework of the

present. What answer would be given to a modern high-school science student who, contemplating the dancing flame of a candle, asked "What is fire?"

Philosopher of science Gaston Bachelard stated that when he asked "educated persons and even eminent scientists" this question, he generally received "vague or tautological answers which unconsciously repeat the most ancient and fanciful philosophical theories."[3] Nor, generally speaking, are better answers to be found in published writings:

> In the course of time the chapters on fire in chemistry textbooks have become shorter and shorter. There are, indeed, a good many modern books on chemistry in which it is impossible to find any mention of flame or fire. *Fire is no longer a reality for science.*"[4]

The course of scientific development has treated Marat's other main concerns differently. A high-school student asking "What is electricity?" or "What is light?" would likely elicit confident, definitive answers that both teacher and student would find satisfying. The question of what fire is, however, seems to have simply faded away.[5]

For Marat and his contemporaries this was still a central issue of physics. The obvious connection between flame and hotness was taken to indicate that fire is in some sense a pure expression of heat, and therefore that the appearance of flame is a special case of the general phenomenon of heat, or at least that the two phenomena are intimately related. Marat was far from alone in assuming

that heat and fire are "two effects of the same cause."[6] To inquire into the nature of fire, then, meant investigating the nature of heat.

There were two broad generalizations current with regard to the nature of heat. The first held that heat is a material substance that, when added to another material substance, made the latter hot. The difference between a cold piece of iron and a hot piece of iron, according to this view, was the absence or presence of a substance that can be identified as the "matter of heat" or the "matter of fire." The fact that hot and cold objects put in contact with each other soon reach a state of temperature equilibrium suggested that heat must be a fluid that flows into and out of objects. A great body of observational and experimental data seemed to confirm this commonsensical conception.

The alternative generalization was that heat is not a substance at all, but a pure product of matter in motion. This idea has ancient roots and was given renewed currency in the seventeenth century by Francis Bacon in his *De forma calidi* (1620).

Bacon observed that the production of heat seemed always to require imparting motion to a substance, whether by the friction of rubbing, the blows of a blacksmith's hammer on iron, or the striking of flint on metal. Other seventeenth century natural philosophers, including Newton and Leibniz, developed Bacon's concept and attributed heat

to the vibratory motions of an object's constituent particles. A hot substance, by this hypothesis of "intestine motion," would be one the particles of which are in a state of relatively rapid motion.

Although this idea enjoyed a great deal of support among seventeenth-century natural philosophers, by the last decades of the eighteenth century the material theory of heat had become dominant. After all, when a hot object and a cold object are gently placed in contact with each other, heat apparently flows from one to the other even though no obvious motion is imparted.

The notion of a "matter of heat" or "matter of fire" was in harmony with the generally mechanical outlook of many eighteenth-century natural philosophers. Fundamental explanations of such physical phenomena as heat, light, electricity, magnetism, gravity, sound, and even mental processes frequently depended upon postulating the existence of a "subtle fluid" that could mechanically account for observed properties. The fact, for example, that objects generally increase in volume when heated, or liquified, or evaporated, was explained by the mechanical motion of particles of the heat fluid insinuating themselves into the spaces between the object's particles and pushing them apart.

The important discoveries of Joseph Black with regard to heat, especially latent heat, were apparently made about 1760 but were not published in his name until 1803, after

his death.[7] In spite of the publication delay, Black's ideas were credited with influencing an extremely important technological development that occurred before they were put into print: James Watt's improved steam engine. In any event, Black's findings and concepts with regard to heat were propagated, albeit imperfectly, in the works of other authors.[8] Black's discoveries seemed to favor the matter-of-fire theory over the intestine motion theory; of the latter, Black declared:

> I myself cannot form a conception of this internal tremor which has any tendency to explain even the more simple effects of heat.[9]

That the heat fluid was subtle to the point, perhaps, of being imponderable, or weightless, resulted from the observational fact that objects do not increase in weight, or increase very little, when heated. Whether heat is absolutely weightless or not was a matter of controversy, as will be seen below. Another adjective frequently used to describe subtle fluids was "elastic"; the concept of elasticity will also be discussed below.

Both the subtle fluid and intestine motion concepts, of course, were hypothetical. Their existence was inferred from their "consequences," no one could credibly claim to have observed either directly. Although the authority of Newton's *hypotheses non fingo* was often invoked to discourage this sort of speculation about the hidden causes of phenomena, Newton himself provided the best-known models for speculative subtle fluid theories. As was mentioned

previously, Newton's influence on his eighteenth-century successors was complex; it can be roughly described as two somewhat contradictory influences, one stemming from his *Principia Mathematica* and the other from his *Opticks*.[10]

The *Principia* was the primary source of Newton's immense prestige among scientists. By uniting the principles of planetary motion and terrestrial gravity, and by giving precise mathematical expression to their common attractive force, Newton was thought to have established a model for all subsequent scientific investigation. Following Newton, all natural phenomena might be reducible in principle to mathematically describable matter in motion. Those who were left unsatisfied with abstract mathematical formulae and who continued to demand to know the *cause* of gravitational attraction were told that such questions were inherently hypothetical and therefore unscientific. "What is gravity?" -- like "What is fire?" -- was turned into a nonquestion for science.

The *Opticks*, however, revealed another side of Isaac Newton. Rather than depending upon deduction from quantifiable data, Newton's optical investigations were primarily empirical and qualitative. It was the Newton of the *Opticks* that provided the primary inspiration for experimental physicists such as Benjamin Franklin and Marat, whose investigations were also qualitative rather than quantitative. "The *Opticks*," says I. B. Cohen,

> is the only one of Newton's two major books on physical science to have been read by every Newtonian scientist. . . . The *Opticks* is the more important of the pair for our understanding of Newtonian natural philosophy in the eighteenth century.[11]

Most significant was the lack of hesitation to feign hypotheses that Newton exhibited in his *Opticks*. In a series of queries appended to that work, he developed speculative hypotheses regarding subtle elastic fluids or aethers as putative first causes of light, gravity, and other natural phenomena. The Newtonian legacy was therefore more than a little ambivalent:

> The scientists of the eighteenth century often took a curious position with respect to hypotheses in the Newtonian philosophy. For instance, they would attack Cartesian philosophy as resting on hypotheses and quote Newton's phrase about *Hypotheses non fingo*. Then, without further ado, they would present Newton's own hypotheses as if they derived them from phenomena and were accepted principles of natural philosophy.[12]

Belief in hypothetical fluids, then, and in a heat fluid in particular, was not a source of disagreement between orthodox academicians and critics like Marat. It was part of the common ground upon which they stood.

The Problem of Combustion

The investigation of heat was not the only approach taken in the quest to understand the nature of fire. Rather than contemplating the continuity of increasingly hot objects with burning objects, some natural philosophers chose to consider the discontinuity represented by the act of combustion. The observation that some materials burst into flame when heated while others do so less readily or

not at all led to the concept of a material basis of combustion. Substances that share the property of being burnable, it was reasoned, must have a specific component in common, a *principle of combustion*. This idea was championed in the seventeenth century by Johann Joachim Becher and was most fully elaborated by Georg Ernst Stahl, who in a 1703 publication gave the name *phlogiston* to the principle of combustion.[13] Phlogiston was envisioned as the "food of fire" that was consumed in the burning process. It was not destroyed, however, but transferred in a chemical reaction from combination with one substance to combination with another.

The phlogiston theory developed largely out of attempts to understand the economically critical industrial process in which iron was produced from iron ore by heating it over a charcoal fire. This process could be convincingly explained as a transfer of phlogiston out of the burning wood, leaving an ash, into chemical combination with the ore, producing the metal.

The phlogiston theory provided a satisfying explanation of combustion and remained dominant until near the end of the eighteenth century when a rival theory proposed by Lavoisier challenged it. Lavoisier's explanation of combustion held that burning objects did not give up a substance named phlogiston, but instead gained a substance -- a sort of antiphlogiston -- that happened to be a component of ordinary air: he named this substance

oxygen. In the earlier theory, the air had played the role of a neutral but necessary medium of transmission for phlogiston.

Although Lavoisier began to work out the new theory during the years 1772-1775, it only began to mount a serious challenge to the old paradigm after 1785, when such members of the French scientific elite as Berthollet, Fourcroy, Laplace, and Monge declared their allegiance to the new chemistry.

It had not completely conquered the field even at the end of the eighteenth century, however; important pockets of resistance remained in France and abroad. The most important defender of phlogiston theory, Joseph Priestley, continued to oppose Lavoisier's innovation to the end; his last scientific work, published in 1800, was entitled *Doctrine of Phlogiston Established*. In France the banner of phlogiston was held aloft by Delamétherie who, although not a member of the Academy of Sciences, was nonetheless an important scientific opinion-maker by virtue of his editorship of the *Journal de Physique*.[14] Lamarck's massive *Réfutation de la théorie pneumatique*, published in 1796, was directly aimed against the new chemistry, but it rejected phlogiston as well and tried to stake out a third alternative.[15]

While Lavoisier was investigating combustion, he was also studying the related subject of heat. In 1782 and 1783 he collaborated with Laplace on a series of classic

experiments utilizing a device invented by Laplace to measure heat, the ice calorimeter. Their resulting monograph, *Mémoire sur la chaleur*, suggested that the intestine motion and matter-of-heat doctrines were fundamentally equivalent. Lavoisier, nevertheless, preferred to consider heat as a material substance that flows back and forth among other substances. In the famous work that codified the chemical revolution, along with the new terms "oxygen," "hydrogen," and others, Lavoisier gave the name "caloric" to the matter of heat. In fact, caloric was the second entry in Lavoisier's "Table of Simple Substances"; only light was higher on the list.[16]

Lavoisier had earlier concluded that all aeriform fluids are composite substances containing caloric. The papers that he read before the Academy of Sciences could contain hypotheses as speculative as any of Marat's theoretical musings. In one, for example, he explained that the matter-of-heat and matter-of-light produced by ordinary combustion, as well as the matter-of-electricity produced by another kind of combustion, are all normally components of the air. "It is astonishing," he said, "to see how much this new theory adds to the explanation of a very great number of phenomena."[17]

Marat's *Recherches physiques sur le feu*

The preceding comments give a general idea of the state of knowledge and some of the debated issues

concerning heat, combustion, and fire when Marat's *Recherches physiques sur le feu* appeared in 1780. Marat sought to contribute to the discussions by reporting and interpreting a large number of experiments he had performed with the aim of developing a general theory of the subject.

Recherches physiques sur le feu was published in Paris with the approval of the official censors. There are 166 numbered experiments described in the book; the descriptions are careful enough in most cases to permit replication. Many of them required the use of sophisticated apparatus that Marat described in detail, and a M. Sikes, optician to the King at the Palais Royale, was identified as the master craftsman who had constructed the equipment according to Marat's specifications. The number of experiments may seem inflated by the fact that separate numbers were sometimes assigned to similar experiments in which the conditions were only minimally varied. On the other hand, some of the individually numbered experiments represented a dozen or more separate trials, and Marat reports that many were repeated numerous times.

Marat evidently put a great deal of thought and work into trying to unravel the mysteries of fire and heat. Although he was not always successful in preventing extraneous factors from influencing the outcome of his investigations, his carefulness as an experimenter appears to have been beyond reasonable reproach, at least by contemporary standards.

In the introduction, he stated that his book aimed to challenge "the opinion of the ages" with regard to fire:

> It was long ago believed, and is still believed today, that fire is a material element, dispersed throughout all material bodies and remaining hidden as long as it is not manifested by movement.[18]

Marat treated the notion of elemental fire as the virtually unanimous opinion of physicists and cited Hermann Boerhaave, "one of the most famous partisans of this system," as a specific example of someone identified with the doctrine he intended to argue against. "For the physicists," Marat said, "fire is a material destined to produce, by its very presence, light and heat." Since all material bodies contain it, why are not all bodies hot and luminous? "Because, they respond, this element is included with other basic elements in a mixture wherein it remains hidden until it manifests itself by means of movement."[19]

Marat contended that this doctrine leads to absurdities. "What?" he exclaimed, "Imperceptible atoms of fire are maintained for thousands of centuries in the midst of the least combustible materials?" If the sparks produced by striking two stones together is thought to represent the release of elemental fire, how is it, he asks, that this element can "lose all of the properties that it is known to have and then regain them by means of a simple collision?"[20]

Marat took strong issue with those who claimed that phlogiston, the matter of combustion, is equivalent to the matter of fire:

> Not content with holding that fire is material, they claim to be able to show it in the decomposition of mixtures. Open their books and you will see that this matter is the *phlogistic* part of bodies.[21]

Marat was by no means an opponent of phlogiston theory. In a footnote he disputed "a famous author" who "has strongly cried out against the nomenclature of chemistry." Marat agreed with this author, Buffon, that many terms used by chemists were empty of meaning, but disagreed that phlogiston was one of them. "By phlogiston, one understands the inflammable principle of bodies. This principle surely exists in nature."[22]

Marat conceded that the idea of fire being nothing more than phlogiston seems reasonable at first glance. Since "materials impregnated with phlogiston" burst vividly into flame, it is tempting to conclude "that they are almost completely made up of fire." The "true inflammable principle," on the other hand, "however pure it may be," is entirely unlike fire in that it is "never luminous, and is always found to be at the ambient temperature."[23]

Further insight can be gained by examining the concentrated acids, such as marine acid, vitriolic acid, and nitrous acid, "those liquors that the chemists regard as the mixtures most impregnated with pure fire." The action of nitrous acid, for example, which holds phlogiston

in solution, radically differs from that of fire: "Instead of making animal and vegetable material pass into the carbonous state in extracting their phlogiston, as fire does, it dissolves them entirely." On the basis of such considerations, Marat concluded that "phlogiston is absolutely distinct from fire."[24]

Having demonstrated to his satisfaction that "fire is not material," Marat believed he had destroyed the standard paradigm and was ready to put forward his own theory as an alternative.[25] Although he appeared to be preparing the reader for a conception not depending upon a postulated material substance, that was not the case. Fire is not a material fluid *per se*, he said, but a *modification* of a material fluid. He explained this by means of an analogy: colors are not themselves distinctly material entities; they are modifications of a material entity (*i.e.*, light). In fact, Marat said, he hoped his theory would do for the doctrine of fire what Newton's did for the doctrine of colors.

Although fire is not material, then, there exists a material substratum without which fire would not exist. Marat identified it as a subtle fluid and designated it by the name "igneous fluid." However full of igneous fluid bodies may be, in the absense of combustion they "are always at the temperature of the medium that surrounds them." From this fact he concluded that "it is therefore

the movement of this fluid, and not its presence, that produces heat and fire."[26]

By way of elaboration he added: "Heat and fire are two effects of the same cause that differ between themselves as more or less."[27] If heat is caused by a moderate motion of igneous fluid, fire is the result of a qualitative increase in that motion.

Marat had, in a sense, combined the "matter of heat" and "intestine motion" doctrines into a synthesis wherein the essential motion is not of the particles of the heated object itself, but rather of the corpuscles of a special material fluid.

Apart from whatever intrinsic scientific value this conception may have had, was it original? Marat claimed that it represented a genuine theoretical innovation, and there is no evidence that he or his readers believed otherwise. None of the critical commentaries of Lamarck, Marivetz, Carra, or others accused him of simply rehashing an old doctrine. It would appear, however, that the general notion was not new. A decade or two earlier, Joseph Black had reviewed all of the various theories of heat known to him:

> The greater number of French and German philosophers, and Dr. Boerhaave, have supposed that the motion in which heat consists is not a tremor, or vibration of the particles of the hot body itself, but of the particles of a subtile, highly elastic, and penetrating fluid matter, which is contained in the pores of hot bodies, or interposed among their particles.[28]

It would seem, then, that the essence of Marat's synthesis had earlier been considered by a perceptive English observer to be representative of continental physical thought in general. Was Marat ignorant of the literature on the subject, or was he falsely trying to ignore it? Neither of these alternatives is likely; he knew the subject well and he was writing for a sophisticated audience that he could not have hoped to so easily dupe. Perhaps the answer lies in differing emphases of interpretation on the part of Black and the authors whose work he was characterizing; perhaps the latter had not attributed as much importance to the motion of a special fluid's particles as Black thought they had.

Be that as it may, Marat stressed the originality of his contribution to heat theory. His primary innovation, however, was a methodological one, based on turning a little-known piece of apparatus to new use. Although the truth of his theory could be "deduced from the necessity of the facts," reliance on reason alone was unnecessary since it could now "be demonstrated to the eye itself."[29] The igneous fluid could be made visible by means of a solar microscope that Màrat had devised.

The solar microscope was an instrument used in a darkroom. A narrow beam of sunlight was let into the darkroom through a tube fitted into a hole in the shutters; one or more lenses were employed to focus the incoming light and to magnify images of shadows cast on a screen.

Marivetz included detailed illustrations of the apparatus in his *Physique du monde*.[30] Here is how Marat describes his initial efforts: "Trying one day to examine the flame of a candle in the darkroom, I put it at the focus of the lens, but no image was produced."[31] The incoming sunlight had been too strong; he decreased the amount of sunlight entering the room and found that "all matter less subtle than that of light can therefore intercept the light" and produce a shadow on the screen. "How surprised I was," he wrote,

> at seeing the image of the candle's flame in the form of a whitish cylinder, bordered by a white halo and crowned with a tuft of swirling jets that were less white. Recovering from my astonishment, I judged that this image was that of the igneous fluid, not of the flame. To assure myself of that, I substituted a piece of red-hot iron for the candle.[32]

Seeing a similar image, he continued to examine a number of other substances by the same method.

> Having become adept at rendering the igneous fluid visible, I undertook the reexamination of all the phenomena of fire in the darkroom; I performed a large number of experiments, and the truths that I had previously deduced from the necessity of facts were almost all confirmed.[33]

Marat's use of the solar microscope in the darkroom would be the central axis of all his future research in physics, and he believed that development of the technique would represent a major gain for science.

> This method of observing is absolutely new and I strongly recommend that physicists give it a try. To take it into certain branches of physics would be, I think, to open a source of new knowledge. I myself have already applied it to electricity, air, and

> light, and will soon communicate my discoveries to the public.[34]

This commitment was fulfilled with the publication of his *Découvertes sur la lumière* and his *Recherches physiques sur l'électricité*, which will be discussed in Chapters Ten and Eleven, respectively.

Observation of the candle flame, described above, was experiment number one; number two was the substitution of the red-hot iron for the candle. Before pursuing Marat's investigation of the igneous fluid, however, let us consider the question of his originality with regard to the solar microscope. Few innovations are absolutely unprecedented and this was no exception.

Joseph Priestley's 1772 history of optics, which was probably known to Marat, stated that "in 1738 or 1739 M. Lieberkuhn" invented a solar microscope. In 1739 Lieberkuhn was in England where "he showed an apparatus of his own making . . . to several gentlemen of the Royal Society, as well as to some opticians." One of the latter, a Mr. Cuff of Fleet Street, "took great pains to improve" the instrument.

> The solar microscope, as made by Mr. Cuff, was composed of a tube, a looking glass, a convex lens, and a Wilson's microscope. The tube is of brass, near two inches in diameter, fixed in a circular collar of mahogany; which, turning round at pleasure, in a square frame, may be easily adjusted to a hole in the shutter of the window, in such a manner, that no light can pass into the room, but through that tube. Fastened to the frame by hinges, on the side that goes without the window, is a looking glass, which, by means of a jointed brass wire, coming through the frame, may be either moved vertically or horizontally, to throw the sun's rays through the brass tube into

> the darkened room. The end of the brass tube, without the shutter, has a convex lens, to collect the rays, and bring them to a focus; and on the end within the room Wilson's pocket microscope is screwed, with the object to be examined applied to it, in a slider. The sun's rays being directed by the looking glass through the tube upon the object, the image or picture of the object is thrown distinctly and beautifully upon a screen of white paper, and may be magnified beyond the imagination of those who have not seen it.[35]

Priestley also mentions a later variation on this instrument that was described by a Mr. Brander in a 1769 pamphlet on "eines Sonnen Microscops." In this version, the solar microscope "has been introduced into the small, and portable, as well as the large *Camera Obscura*."[36]

Marat did not mention these predecessors, although he certainly had knowledge of them. While Marat did not refer directly to Priestley's history of optics, it would seem unlikely that he had not been familiar with it. In his later work on electricity, Marat repeatedly cited Priestley's *History and Present State of Electricity* as an authoritative source.

In any event, Marat did not claim to have invented the solar microscope, but to have devised an improved kind that he called a *helioscope*, and to have extended its use into new fields of observation:

> I did nothing more than use, in a certain manner, an instrument that had for a century been in the hands of all those who occupy themselves with physics.[37]

Marat drafted a codicil to his will in order to prevent members of the Academy of Sciences, and particularly a "M. Bettancourt," from stealing the credit for "my little helioscope," which was "capable of producing

the solar image in all its whiteness." Bettancourt, he said, is "a man above all others in knowing how to appropriate the results of others' work for his own use." In the event of Marat's death, the executor of the will was instructed to turn the design over to the Academy of Sciences. This would indicate that his hostility was not directed toward the Academy as an institution, but toward certain of its members.[38]

Marat had requested, through the good offices of his patron the comte de Maillebois, that the Academy of Sciences evaluate his experimental work.[39] The Academy appointed a commission to do so, and its report, dated April 17, 1779, stated that Marat's experiments

> were carried out by a new and ingenious means that opens up a great field of new research in physics. This means is the solar microscope. Until now this instrument has not been used except for small objects. [Marat, however,] has used it to show, in a perceptible manner, various emanations that without this means could not be seen at all, or at least not so clearly and distinctly.[40]

Accompanying the commissioners to Marat's laboratory to observe his experiments was one of the most renowned of contemporary scientists, Benjamin Franklin. Franklin injected a note of humor into the proceedings by volunteering his own bald head as one of the series of objects to be placed at the focus of the solar microscope. It, too, gave off the emanations whose shadows Marat interpreted as visible evidence of igneous fluid.

This first official encounter between Marat and the Parisian academicians apparently ended in mutual respect. The commissioners seemed genuinely impressed with what they believed to be a new and valuable experimental technique.

The commissioners would not go so far as to affirm that the shadowy emanations they had seen were indeed produced by igneous fluid, as Marat claimed. The wording of their report suggests that they differed among themselves on that point; to declare positively that they had witnessed igneous fluid "would get us involved in overly lengthy discussions." They did, however, state for the record that all of the experiments they had seen were consistent with the conclusion that some kind of fluid was seen emanating from heated bodies, and that this fluid was carrying heat with it. The report ended with what can only be described as a glowing tribute to Marat's set of "new, precise, and well-executed experiments, ingeniously and appropriately designed," which, they repeated, "open up a vast field of research for physicists."[41]

Marat could hardly have desired a more supportive statement. He published it in its entirety at the front of one of his short works, with the claim that it constituted the Academy's stamp of approval on the contents of the book.[42] Unfortunately for Marat, he had neglected to take the commissioners' apparently innocuous qualification into account. Lavoisier noticed Marat's claim and demanded that

the Academy publicly repudiate it, which is what occurred. Marat's relationship with the Academy rapidly deteriorated.

Condorcet would later refer to this episode as "the Marat incident," and would use it to illustrate the Academy's role as "barrier against charlatanism of all kinds." The Academy, he said in retrospect, had been "too soft" in its dealings with Marat; it should not have "accepted as new experiments which were already known and which were novel only through the jargon the author had given them."[43] Although not all of the academicians were opposed to Marat, the opposition of Condorcet, Lavoisier, and Laplace was sufficient to turn the institution as a whole against him. The response to Marat's next approach to the Academy, which had to do with his optical experiments, will be discussed in the next chapter.

That Marat's experimental techniques, at least, were considered to be of genuine value by many academicians was confirmed in 1784 by the report of the Franklin commission on the investigation of Mesmer's animal magnetism. While not mentioning Marat by name, the report cited the results of observations with a solar microscope as evidence against the existence of animal magnetism.[44]

One academician who considered Marat's theories worth discussing was Lamarck, who later wrote a lengthy critical appreciation (26 printed pages) of Marat's researches into the nature of fire.[45] This "Exposition of several very interesting experiments by means of which the matter of

fire (in expansion) is rendered visible" was published in 1794, after Marat had become famous primarily as a revolutionary martyr. Apparently wanting to keep the focus on the scientific issues at hand, Lamarck consistently refered to him only as "the author," identifying him in a footnote as "the patriot Marat."[46] Lamarck writes:

> Several interesting experiments that the author had occasion to perform by the ingenious means that he devised certainly revealed some things to him that other physicists knew nothing about.[47]

Lamarck carefully described and interpreted Marat's investigations, lauding his "beautiful experiments" and his "striking proofs." He concluded by counterposing Marat's work to that of "pneumatic chemists" such as Lavoisier and Fourcroy:

> I recommend, moreover, that the reader consult this interesting work, and above all to repeat the fascinating experiments that its ingenious author has performed. These experiments are very important for the true theory of fire, which the pneumatic chemists have assuredly not foreseen at all.[48]

Lamarck's comments testify eloquently to the seriousness with which Marat's endeavors were treated in important scientific circles. When this was published, Lamarck was already prominent for his botanical studies and had just begun his pioneering studies of invertebrate anatomy. His accomplishments had earned him entry into the Academy of Sciences in 1779, but his natural philosophy was not in harmony with that of its dominant faction, as his polemics against pneumatic chemistry attest. Lamarck's account of

Marat's work was not entirely uncritical; his criticisms will be discussed below.

Another important contemporary scientist and member of the Academy of Sciences, the comte de Lacépède, also expressed admiration for Marat's experimental innovation. He wrote that molecules of fire

> have been able to intercept rays of light and to project a shadow onto a surface; Mr. Marat has found an ingenious means to augment this shadow and render it more perceptible.[49]

He goes on to argue, as Marat had, that the heat fluid and the light fluid are distinct entities,

> and the series of experiments that Mr. Marat has made public prove it even better than all my reasoning.[50]

Carra's popular *Nouveaux Principes de physique*, in a section entitled "Theory of Fire and Heat," summarized the views of Newton, Musschenbroek, Boerhaave, the Cartesians, and Marat. He then declared:

> I therefore adopt, in relation to my own principles, the opinion of Boerhaave, that of the Cartesians, and that of M. Marat.[51]

It is evident that Marat's contributions to physics, and especially his solar microscope technique, attracted some attention in important contemporary scientific circles. While never a central focus of scientific concern, his work was certainly noticed, acknowledged, reviewed, commented upon, and discussed.

Let us now return to *Recherches physiques sur le feu* to find out what Marat was able to learn about the igneous

fluid. First of all, he anticipated a possible criticism of his interpretation of what the solar microscope reveals:

> It will undoubtedly be objected that the image on the screen may be the result of some light vapor escaping from these bodies.[52]

He disposed of this objection by experiments showing that the same shadows are produced by hot objects in the evacuated chamber of an air pump.

Marat admitted a limitation of the technique and expressed the desire for more powerful microscopes in the future:

> The fluid appears here only in its collectivity; perhaps dioptrics will some day be capable of allowing us to distinguish its individual atoms.[53]

Although Marat acknowledged being unable to see these atoms of the igneous fluid, he "deduces from facts" that they "must be of an astonishing smallness and spherical in shape," and therefore called them little globes, or "globules."[54]

Another property of the igneous fluid is "diaphaneity"; it never forms an opaque medium, as his experiments 10 through 12 demonstrate. In fact, it seems more than diaphanous; it could even be said to be lucid, since on the screen it shows up brighter than its surroundings.

A most significant property of the igneous fluid is one that saved Marat from a philosophical dilemma:

> Like all other bodies, this fluid has weight, for heated metals lose this weight when cooled.[55]

Watching, in the darkroom experiments, the prodigious quantity of igneous fluid that escapes from incandescent bodies "causes one to cease being astonished" at their weight loss. Experiment 27 consisted of heating a 16-ounce ball of fine silver to cherry-red, whereupon it registered a five-and-a-half grain weight gain. Experiment 28 similarly compared the weight of a piece of copper before and after being heated white-hot; again a weight differential is reported.

> Repeated four consecutive times, these experiments always gave the same results, from which it follows that the same mass of the same metal never requires other than the same quantity of igneous fluid to redden it to the same point.[56]

Marat, of course, was not the first to perform such experiments designed to ascertain whether or not heat has weight; Boerhaave, Buffon, and many others had long disputed the meaning of contradictory findings. Some investigators who found no weight differential between a cold object and the same object after it had been heated argued that this supported the intestine motion doctrine of heat. Others, likewise failing to detect a weight differential, refused to give up the concept of a material fluid, but declared it "imponderable"; *i.e.*, weightless by definition. Critics of the latter claim objected that weight is the *sine qua non* of materiality; a weightless material substance is a contradiction in terms.

Complicating this discussion even further was the attempt to explain why a metal apparently gains weight when

it loses phlogiston: phlogiston must possess *negative* weight (or an antigravitational property, which is the same thing). To those who proclaimed this notion absurd, its defenders replied that if the electric and magnetic forces can attract and repel, why, in principle, could gravitation not work both ways?

Marat's doctrine of heat, however, had no such difficulties, thanks to his conviction that he had measured the igneous fluid's weight. Thus there was no contradiction with his fundamentally Newtonian conception of matter: "Certainly all matter is extended, is divisible, has weight, is impenetrable, etc."[57]

"From my experiments," he continued, "it is certain that the igneous fluid weighs much more than the air it displaces." This fact was obscured by the igneous fluid's great subtlety. However,

> the igneous fluid, far from being the lightest of bodies, as one might think, is very heavy, considering its subtlety.[58]

"In spite of its weight," Marat said, "it is extremely mobile. And to this astonishing mobility is added a great expansive force," as can easily be seen by means of the solar microscope.[59]

"This fluid is compressible, but only when it is in action."[60] A footnote warns against confusing compressibility with elasticity. This is an important point for the internal consistency of Marat's natural philosophy. Although he does not say so explicitly at this point, in

his later book on electricity he rejects the standard Newtonian conception of elastic fluids as composed of particles endowed with a repulsive force. According to that standard conception, an elastic fluid -- of which air is the most familiar example -- expands without limit and resists compression because its constituent particles mutually repel each other. Marat held that no forces of repulsion exist in nature; all apparent repulsions can in fact be explained in terms of attraction. This would seem to reflect a metaphysical preference on Marat's part, but he believed he could substantiate it experimentally, as will be seen in his work on electricity, where repulsion presents itself as an unavoidable issue.[61]

Returning to the physical qualities of the igneous fluid: "It equals all others in the solidity of its globules. This solidity is extreme."[62] Igneous globules can pulverize diamonds; "thus are they as unalterable as the elements themselves."[63] Although denying fire the status of element, it would seem that Marat has here given his igneous fluid the essential characteristic of an element.

Finally, the igneous fluid has specific affinities; more for water than for air, for example, as is shown by boiling water, wherein the matter of heat hesitates to make the leap from water to air. Although in this work Marat nowhere mentioned Joseph Black's explanation of latent heat, this reference to the igneous fluid's hesitation

reveals that he was at least familiar with the phenomenon, if not with its implications for a theory of heat.

The igneous fluid's most pronounced affinity, according to Marat, is for light; *i.e.*, the luminous fluid, which, it will be recalled, Lavoisier placed at the top of his revised list of elements. The affinity between light and heat is suggested by the fact that "light always accompanies a vivid heat."[64] This had led some physicists to jump to the false conclusion that igneous fluid and the luminous fluid are one and the same. That cannot be true, however, because the converse is not true: "Heat does not always accompany a vivid light." Glowworms, for example, can be "as luminous as white-hot iron, while remaining at the temperature of their surroundings."[65] Furthermore, "heat penetrates all bodies, but not all bodies are permeable to light; that can only come from the difference in the fluids that do the permeating."[66]

These are but two of several plausible reasons Marat cites to refute the claim that the two fluids are identical. The clincher, however, is that in the darkroom the igneous fluid throws a shadow but the luminous fluid does not. "It is therefore proven that light and heat are not the same principle."[67]

Light is not the only thing that other physicists have confused with the igneous fluid; some have also asserted that the matter of heat is identical to the electric fluid. There are, Marat admitted, many points of analogy between

the two, but there are also a number of crucial distinctions. Igneous matter "always distributes itself equally in the entire substance of bodies it permeates"; electrical matter "often accumulates on the surface of the same whole."[68] Heat, moreover, penetrates a large body slowly, while electricity can do it with astonishing speed. Heat escapes from bodies over their entire surfaces, while electricity often escapes from a single point.

Again, their difference "can be seen by simple inspection when they are compared in the darkroom. Both are transparent, but electric fluid is much less so than igneous fluid."[69] Experiment 53 demonstrates this contention by comparing the shadow-image of a candle with that of the discharge from a Leyden jar.

Experiment 56 demonstrates that an electric discharge pushes and disrupts the shadow of igneous fluid emanating from a candle flame. Electric fluid -- insofar as it represents a positive charge -- repulses the igneous fluid. This seems to contradict Marat's later explicit disavowal of repulsive forces in nature, but perhaps he has simply neglected to make a distinction between apparent repulsion (which is an illusion) and real repulsion (which does not exist). Or it may be that he had not yet fully worked out this point in his own mind. In any event, this particular "repulsion" could have been attributed to mere mechanical interaction, with igneous fluid globules simply bouncing off of electric fluid particles.

A number of other distinctions between igneous and electric fluids were given, leading to a general conclusion:

> Since the igneous material differs so essentially from the electric material and from the luminous material, the only substances with which it can be confused, it constitutes therefore a separate fluid.[70]

All that has been stated above, Marat said, goes to show "that heat, fire, and flame are produced by a fluid in motion. . . . But what does this movement consist of?"[71]

After considering various means of producing heat, including friction, percussion, and the mixture of certain chemicals, Marat explained heat transmission in classical mechanical terms. Globules of igneous fluid apparently act like tiny spherical balls bumping into each other and rebounding according to the laws of Newtonian mechanics. "However hard one tries," he said, "one cannot conceive of intestine motion as other than the result of rectilinear movement broken up by some obstacle."[72]

What is the primary source of this motion? Where does it originate? "If it is asked what is the force that causes these molecules to interpenetrate, I will answer: the principle of attraction." Marat, unlike some of his contemporaries, was willing to accept attraction as a fundamental fact of nature requiring no further explanation: "This principle seems to me to be a property inherent in matter."[73]

This is a classical Newtonian stance, of course, in opposition to Cartesian efforts to explain gravitation by means of yet another subtle fluid. The position Marat defended was more characteristic of Newton's eighteenth-century followers than of Newton himself, who took various positions at various times. His well-known *hypotheses non fingo* represented a purely agnostic stance. In an exchange of correspondence with the Churchman Richard Bentley he warned Bentley against ascribing to him the position that attraction is inherent in matter.[74] But in the *Opticks* Newton seemed to accept exactly that position, even going so far as to speculate on a universal aether as a possible cause of gravity.

Marat argued that attributing attraction to a universal fluid simply "transposes the difficulty from the effect to the cause." In other words, if the motion of igneous globules is explained as resulting from the motion of particles of another fluid, what is the cause of this latter motion? Marat was not the only natural philosopher to recognize that this mode of explanation leads to an infinite regression of causes and effects. The only escape is to call a halt at some point and accept some basic force -- some "action at a distance" -- as inherent in natural law and thereby beyond further explanation.[75]

One of the greatest difficulties facing heat theorists was to explain how the sun's heat is transmitted. Some previous thinkers, Marat said, believed that the sun was

the only source of the heat that is felt when one is exposed to the sun's rays. Others, citing the fact that even in the tropics the tops of mountains are bitter cold, claimed that the heat emanates from the earth.

"To settle our ideas on this matter, let's consult nature."[76] If the sun's rays are focused with a strong magnifying lens, all combustibles at its focus catch fire. "On icy mountaintops and on burning plains these experiments always produce the same results. It would seem beyond doubt, then, that the principle of fire is in the sun's rays."[77]

The solar rays themselves, however, have no heat. They agitate the igneous fluid contained in bodies and set it in motion. "The sun's rays, then, are the *agent*, not the *principle* of heat."[78] If this be doubted, experiment 73 should remove all skepticism: the sun's rays are focused with a burning mirror and directed to the focus of the solar microscope. No evidence of igneous fluid is seen until an object is placed at the focus and begins to heat up.

> We conclude that the solar rays are nothing other than the matter of light itself, impelled in a straight line by the action of the sun, and if they produce heat it is only by exciting in bodies the intestine movement of the igneous fluid.[79]

As mentioned earlier, Lamarck applauded Marat's ocular demonstration of the igneous fluid, although he preferred to call this fluid *fire*, "the name by which it is most generally known."[80] While somewhat supportive, Lamarck was

by no means uncritical of Marat's theory. Responding to Marat's notion of igneous globules colliding and rebounding in accord with Newtonian mechanics, Lamarck said: "As far as I am concerned, this fine piece of reasoning is not as simple to follow as its author thought it was."[81] How can it be, he asked,

> that the motion communicated to these globules by collisions is not diminished proportionately as it is transmitted, as it must be?[82]

Fire does not behave according to this rule. "The smallest quantity of fire," Lamarck observed,

> such as the spark produced by striking a piece of flint, can produce an immense conflagration, burn a fleet of ships, destroy a large city, etc., etc. . . . What, then! Does this small spark, which in fewer than four seconds can produce the complete combustion of the most considerable powder magazine, all by itself communicate motion to the enormous quantity of igneous fluid that in an instant manifests an inconceivable force?[83]

Lamarck concluded that mere motion of the igneous fluid is an insufficient explanation.

Whether Marat had heard this objection directly from Lamarck or not is unknown, but he was certainly familiar with the argument because he gave an explicit answer to it.[84] "Someone will object," Marat wrote,

> that motion diminishes proportionately as it is transmitted. Fire, to the contrary, increases: a spark can become a conflagration.[85]

This is a point well taken, he acknowledged, but does the difficulty go away by considering fire to be a species of matter?

> How could a particle of this matter so quickly transform an enormous volume of heterogeneous substances into a substance identical to itself? Isn't this difficulty insurmountable?[86]

The objection to the motion hypothesis, however, is easier to resolve:

> It is true that motion generally diminishes proportionately as it is transmitted, but only when the thing that is moving receives but a single impulsion and when the objects involved are inelastic. In the opposite case, rather than diminishing, motion increases, and that is what happens with the igneous globules.[87]

Marat proposed a chain-reaction scenario, where each globule is involved in multiple collisions and "each new impulsion adds to the preceding ones, resulting in an augmentation of speed."[88] He compared the spread of fire to the way a storm develops: powerful winds that at first can create only the lightest ripples on the surface of the sea end up shaking the foundations of the earth. Although Marat was able to solve this problem to his own satisfaction within the framework of his own system, there is every reason to believe that his answer would not have satisfied Lamarck.

The question "What is fire?" was implicitly posed by the title of Marat's book. The long series of experiments was prologue to an attempt at a direct answer: "Fire consists of a violent intestine motion of igneous globules."[89] Fire, furthermore, "only occurs in combustibles by virtue of a specific attraction between phlogiston and the igneous fluid."[90] The form of the flame

is determined by air pressure, which always makes it take a vertical direction and the shape of an elongated cone.

In closing, Marat summarized the many manifestations of heat and fire that are accounted for by the intestine motion of igneous fluid.

> This principle has been established in an uncontestable manner, all carefully supported by a knowledge of physical phenomena, and the explanation of the phenomena are derived only from the laws of rational mechanics. Thus in comparing this doctrine to that of the so-called elementary fire, it will be found to clarify all the phenomena that the other cannot make sense of.[91]

CHAPTER TEN

THE BENDING OF LIGHT: *DECOUVERTES SUR LA LUMIERE*

The second volume of Marat's trilogy, published later in 1780, was devoted to the science of optics. In the time between the appearance earlier that year of the volume on fire and the publication in 1782 of the volume on electricity, a qualitative change for the worse had occurred in Marat's relationship with the French scientific elite. That change is reflected in the general tone of Marat's writing, which in the first two parts of the trilogy displays a more "professional" neutrality, whereas the third volume turns more polemical and antagonistic toward scientific opponents in general and members of the Academy of Sciences in particular.

Since the heightened hostility manifested itself in the third book of the series, the one on electricity, C. C. Gillispie attributes the change to a loss of psychological control on Marat's part that occurred while he was writing up his electrical experiments. A closer examination suggests, however, that Marat's change in attitude originated with his optical studies and derived less from his internal mental state than from the hostile reception the academicians gave his revisionist optical notions.

In *Découvertes sur la lumière*, Marat explicitly stated his intention to revise a fundamental tenet of Newton's theory of colors. Newton's authority in this field was so

great in the eighteenth century as to be virtually absolute. Marat's challenge was regarded as an act of effrontery and rejected without a real hearing. The leading lights of the Academy apparently deemed Marat's innovations to be worthless without conscientiously examining them. They closed ranks against him and his later writings reflected his anger at being rebuffed.

Later commentators who have defended the Academy's cold shoulder to Marat have done so on the grounds that subsequent developments in optics proved Marat wrong in his case against Newton. These judgments based on hindsight, however, do not address the question of whether dismissing Marat's contributions out of hand was justifiable in terms of contemporary knowledge of optics.

In fact, Marat's work targeted a problem in optical science that Newton had explicitly left unresolved in his *Opticks*. In reporting his investigations of diffraction -- or "inflexion" in Newton's terminology -- Marat was recalling attention to a neglected class of phenomena that would eventually prove to be of critical importance to optical science.[1]

Diffraction is not a readily apparent phenomenon; unlike reflection and refraction it is not part of everyday visual perception. It was never noticed by the natural philosophers of antiquity. The first to detect this subtle manifestation of light was the Jesuit natural philosopher Francesco Grimaldi, whose *Physico-Mathesis* reported it for

the first time.[2] Grimaldi's book was published posthumously in 1665; he had died two years earlier.

The commonplace observation that a strong light source casts a sharply defined shadow leads naturally to the assumption that beams of light travel in perfectly straight lines. In darkroom experiments, however, Grimaldi detected a very slight bending of light as it passes the edges of objects.

Imagine sunlight entering a darkroom through a very small hole in the window shutters and projecting a luminous disk upon the opposite wall of the room. A knifeblade placed between the hole and the disk of light produces a shadow within the disk. One would expect, if the commonsense assumption holds true, that a perfectly straight line would connect the hole, the edge of the knifeblade, and the edge of the knifeblade's shadow. A careful experimenter, however, will find this not to be so. The shadow will be slightly displaced from its anticipated position. Furthermore, the shadow will not be sharply defined, as would be expected, but will exhibit what appear to be a series of overlapping fringes.

Grimaldi gave the name "diffraction" to this unexpected behavior of light, and said it implied that light is a wave phenomenon. Perhaps even more surprising was his observation of barely perceptible bands of colors in the fringes of shadows projected onto the walls of his darkroom.

In 1672, seven years after the publication of Grimaldi's book, Robert Hooke detected this same peculiar bending of light and the colored fringes, but apparently without knowledge of Grimaldi's work.

Meanwhile, Newton had begun his optical investigations. His primary attention was directed toward the production of colors by the passage of sunlight through a prism. As a result of a series of classic darkroom experiments, he announced that the various colors of the spectrum were all originally present in the beam of sunlight; they were separated and spread out into a rainbow-like continuum by the prism because each color is refracted a little more or less than every other color as the rays pass through the prism. He called this phenomenon "differential refrangibility."

Although Newton thus placed refraction at the center of his color doctrine, he also showed an interest in the unexpected bending of light around objects. Instead of using Grimaldi's term, diffraction, he used the term inflexion to describe it.

Newton's earliest announcement of his optical conclusions, in 1672 in the Royal Society's *Transactions*, provoked heated opposition, which was not at all to Newton's liking. He did not accept criticism graciously. Among other things, Hooke charged Newton with stealing his ideas about diffraction/inflexion. Newton denied the charge

of plagiarism, saying he had given credit to Hooke for his observations.

Newton acknowledged the reality of inflexion, conceded that he was not sure how it could be reconciled with the rest of his optical doctrines, and left it as an unsolved problem for physicists of the future to probe. Nonetheless, the early investigations into diffraction by Grimaldi, Hooke, and Newton had had no significant sequel by the time Marat began studying the phenomenon.

A useful source for determining the specific content of optical knowledge at the beginning of the last quarter of the eighteenth century is Joseph Priestley's *History and Present State of Discoveries Relating to Vision, Light, and Colours*. Priestley's account of the invention of the solar microscope was noted in the previous chapter. Published in 1772, this work represented the conscientious effort of a leading physicist to survey and analyze the entire body of his predecessors' accomplishments in the field of optics. Priestley's personal prestige as a scientist assured a wide readership among practitioners in the field and thereby helped to establish a standard history to which the community of physical scientists and optical craftsmen could refer.[3]

Priestley introduces Grimaldi's discovery of diffraction by describing it as "such an effect as could hardly have been suspected without actual observation."

> Had Descartes, or any other philosopher before this period, been asked, what would have become of a ray of light, which should pass as near as possible to any body, but without actually impinging upon it; he would have replied, without hesitation, that it would proceed in a straight line, without being affected by its approach. But it was now found, that light coming within a certain distance of any body, will either be bent from it, or towards it.[4]

Aside from Robert Hooke's investigations, "little use had been made of Grimaldi's observations" before Newton took an interest in them. In the last section of his *Opticks*, Newton reported replicating Grimaldi's experiments, which Priestley says he did "with the greatest care, diversified them, and pursued them much farther." Priestley emphasizes that

> as these experiments were the last that [Newton] made, and are acknowledged by himself to be incomplete, it is the more necessary to be particular in relating the circumstance of them; that others may be enabled to repeat them, and carry them still further.[5]

This gives strong testimony to the legitimacy of Marat's work in the field of optics. First of all, it indicates that in Priestley's well-informed opinion, the understanding of the phenomenon of diffraction had not been advanced as of 1772; secondly, it is an explicit call for physicists to engage in precisely the kind of investigation that Marat was to undertake a few years later.

Newton's doctrine of color as a function of the differential refrangibility of individual rays of light achieved the status of an unchallengeable paradigm early in the eighteenth century. After 1728, Priestley says,

> no person, who chose to give his name to the public, or whose name is worth recording, made any opposition

> to it; so that, at present, no hypothesis in philosophy stands upon surer ground, or is more generally acquiesced in, than that of the differential refrangibility of light.[6]

While presented and widely accepted as a complete and satisfying explanation of the nature of color, disturbing anomalies remained unaccounted for. If light consists, as Newton believed, of "solid, massy, hard, impenetrable, moveable Particles,"[7] what force deflects them as they pass edges of objects? And if colors are made visible by the prism's power to deflect those particles at different angles depending upon their individual colors, then how can the colors produced in diffraction experiments be explained?

Newton attempted to suggest some preliminary answers in the queries appended to his *Opticks*. By relegating them to the queries, he acknowledged that they were provisional and hypothetical; in other words, that diffraction remained an unsolved problem for the science of optics.

Newton's *Opticks* is divided into three sections, or "books," which are in turn divided into "parts." The first two books are devoted to the reflection and refraction of light, and include consideration of the colors visible on the surfaces of soap bubbles and other such "thin transparent Bodies." Book III, Part I is devoted to inflexion. Book III has no Part II, however; this is an indication of the unfinished character of the work. In fact, Newton says as much, because after describing and discussing his investigations of inflexion, he states:

> When I made the foregoing Observations, I design'd to repeat most of them with more care and exactness, and to make some new ones for determining the manner how the Rays of Light are bent in their passage by Bodies, for making the Fringes of Colours with the dark lines between them. But I was then interrupted, and cannot now think of taking these things into farther Consideration. And since I have not finish'd this part of my Design, I shall conclude with proposing only some Queries, in order to a farther search to be made by others.[8]

The queries, eventually totalling 31 in number by the 1717 edition, appear directly after this apology and constitute the ending of the *Opticks*. Only the first few queries are directly concerned with inflexion, but it is clear that inflexion was what Newton was referring to when he said "I have not finish'd this part of my Design."

The title of Book III, Part I is "Observations concerning the Inflexions of the Rays of Light, and the Colours made thereby," and the problem is stated at the outset:

> Grimaldo has inform'd us, that if a beam of the Sun's Light be let into a dark Room through a very small hole, the Shadows of things in this Light will be larger than they ought to be if the Rays went on by the Bodies in straight Lines, and that these Shadows have three parallel Fringes, Bands or Ranks of colour'd Light adjacent to them.[9]

It should be noted that if the shadows of things in the light are *larger* than they ought to be, the implication is that the light is bending *away* from the objects it passes; if it were bending *toward* the objects, the shadows would be smaller. Grimaldi and Newton both recognized, however, that in fact some light rays appear to bend toward objects and some appear to bend away.

After carefully describing his inflexion experiments and apologizing for being unable to conclude the investigation to his satisfaction, Newton began listing his queries. The use of the question form was a bit of a ruse; as I. B. Cohen has pointed out, it "may have been adopted in order to allay criticism, but it does not hide the extent of Newton's belief."[10]

Newton's difficulty with inflexion is manifested in the way in which he directly contradicted himself in the queries. In Query 29 he asked:

> Are not the Rays of Light very small Bodies emitted from shining Substances? For such Bodies will pass through uniform Mediums in right Lines without bending into the Shadow, which is the Nature of the Rays of Light.[11]

Here Newton explicitly stated that light rays, by their very nature, move in straight lines and therefore do not bend into the shadow. He had already suggested in earlier queries, however, that light not only bends into the shadow; it bends away from the shadow as well. In Query 3 he asked:

> Are not the Rays of Light in passing by the edges and sides of Bodies, bent several times backwards and forwards, with a motion like that of an Eel? And do not the three Fringes of colour'd Light above-mention'd arise from three such bendings?[12]

What was to be made of this extraordinary eel-like behavior of light rays? Priestley reported that physicists heeded Newton's call to follow up on his investigations into inflexion: "We are much obliged to several ingenious foreigners who have made experiments similar to his." Among

these ingenious foreigners were Maraldi, Mairan, Du Tour, Le Cat, and Muschenbroek.[13] Nonetheless, Priestley concluded, these investigations "cannot be said to be completed."[14]

"The doctrine of the *inflection* of light seems to be capable of much farther illustration," Priestley continued. "The origin of the three coloured streaks, on each side of the termination of the shadows of bodies, has not been satisfactorily traced."[15]

Newton's conjecture "that light bends in its motion, like an eel . . . seems to be a kind of hypothesis that one would not chuse to have recourse to without necessity."[16]

Marat's choice of diffraction/inflexion, then, as a worthwhile focus of scientific inquiry was certainly reasonable by any contemporary standards. Marat's patron, the Comte de Maillebois, the honorary member of the Academy of Sciences who had arranged for a commission to evaluate Marat's experiments with heat and fire, once again approached the Academy and requested that Marat's optical work be examined as well. As a result, another commission was named to witness demonstrations of his experiments and to issue a report on them. According to Marat, the commission's first visit took place on June 22, 1779, and the last one on January 30, 1780:

> My experiments were performed during a seven-month period under the eyes of the academicians, and in that time they were all repeated a very great number of times.[17]

One reason the commission's work took so long to complete was that most of Marat's experiments could only be demonstrated on sunny days. Although the commission was officially composed of five members, most of the work devolved upon two of them: J. B. Le Roy and J. A. J. Cousin. Le Roy was assigned to write the final report.[18]

After the commission completed its observations at the end of January, Marat awaited their report. Hearing nothing but excuses from Le Roy and Cousin for several weeks, he began writing to Condorcet to ask when he might expect it to be issued. As time passed, Marat's letters became more impatient and persistent: why the inordinate delay?[19]

The report was finally issued on May 10, 1780, and consisted of three rather brief paragraphs. Marat had it printed in its entirety at the front of his *Découvertes sur la lumière* and followed it with a commentary of his own.

The report began by acknowledging that inflexion was a valid topic of optical investigation:

> These experiments, which are very numerous, touch upon various phenomena of light, and particularly with those having to do with its inflexion when it passes along the length of bodies.[20]

By careful wording and judicious use of phrases such as "according to the author," however, the report avoids saying anything that could be interpreted as praise for Marat's work or endorsement of his conclusions. The conclusion the commissioners were most concerned to dissociate themselves from was Marat's assertion that the

decomposition of colors attributed by Newton to refraction should in fact be attributed to inflexion. The separation of colors from sunlight that Newton observed, according to Marat, did not occur in his prisms but at the edges of the hole that admitted the sunlight into the room in the first place. The prism simply served to amplify the phenomenon. (This contention of Marat's will be discussed further below.)

This was a rather bold hypothesis aimed at eliminating a long-standing anomaly from the science of optics by incorporating reflection, refraction, and inflexion into a single explanatory model of color production. The Academy's report, however, explicitly avoided evaluating Marat's synthesis:

> But since these experiments are so very numerous, as we have said, that we have not been able for that reason to verify all of them (in spite of all the attention that we have paid to them) with the necessary precision; since, moreover, they do not appear to us to prove what the author believes they establish; and since they are generally contrary to what is best known in the field of optics . . . we do not regard them as being of the nature of that which the Academy could give its sanction or its approval.[21]

The report was signed by Le Roy, Cousin, and Sage, and certified by Condorcet. It is significant that the report does not explicitly deny the validity of Marat's theory; it simply refuses to take a position on the issue.

In reply, Marat said that he had neither asked for nor expected the Academy to endorse his conclusions; he had merely requested that the commissioners verify matters of

fact. "But I had the right to expect that this scientific society would state an opinion on the precision and novelty of my experiments."[22]

As for the commissioners' charge that his conclusions "are generally contrary to what is best known in the field of optics," Marat's response was, in effect, so be it; he had stated the conclusions his experiments had led him to, and he considered that to be a positive gain for science: "Is it necessary to defend the old opinions," he asked, if they can be shown to be erroneous? "There is no scientific society in the world whose judgment can make true what is false or false what is true."[23]

Although neither the Academy's report not Marat's response mentioned Newton's name, the authority of Newton was at the root of the dispute here. "What is best known in the field of optics" was a euphemism for Newton's doctrine of differential refrangibility. Marat's attempt to promote inflexion from being a mere curiosity to the central principle of optics was a clear challenge to Newtonian orthodoxy.

Although Marat's response reflects disappointment, it did not express the bitterness toward the Academy that he later displayed.[24] Apparently his resentment increased over time.

He later charged that the Academy had acted dishonestly in this affair. In his correspondence with Roume de Saint-Laurent about the directorship of the

Spanish Academy, he asserted that Le Roy had told a friend of his that in fact he, Le Roy, had written a 45-page, detailed report on Marat's experiments, but that it had been suppressed by the geometers' faction within the Academy.[25]

There is no independent evidence that such a report had ever existed, but it is not at all an implausible supposition. For the three-paragraph report that was actually released to have taken more than three months to prepare seemed preposterous to Marat. Had Le Roy not repeatedly assured him that the report was taking a lot of time and effort?[26] Had Cousin not told him that the reading of the report had required more than a single session of the Academy?[27] Why would so much time be needed to read three brief paragraphs?

Even if there had been no such report, the delay could well have been due to disputes between Le Roy, Cousin, and other academicians over how to formulate the report that did appear. Le Roy had prieviously complained openly to Marat that a "spirit of chicanery in the Academy" was making it difficult for him to write it.[28] It is conceivable that one or more members of the Academy recognized that Marat's exposition of diffraction phenomena did indeed raise questions that could not be answered within the confines of the standard Newtonian paradigm. If so, that was an admission that the central leaders of the Academy were unwilling to make. Their attitude was

represented by Condorcet's later reference to "the Marat incident" as a shining example of the way the Academy should deal with "charlatans," *i.e.*, dismissively.[29]

In any event, Marat could hardly be blamed for believing that he had been rebuffed by a conservative priesthood of science blindly defending its entrenched orthodoxy. Was the Academy's refusal to really examine the inflexion anomaly any different in principle from the refusal of Vatican astronomers to look through Galileo's telescope? Both seemed afraid to confront empirical evidence that threatened a cherished paradigm. The Galileo tale, however, had developed into one of the central myths underlying the ideology of modern science. Galileo's telescope symbolized Truth and those who refused to look through it represented ignorance and superstition. In Marat's case, it was the Academy of Sciences rather than the Catholic Church that proved unwilling to face up to empirical evidence.

To assess Marat's value as a scientist in terms of later developments in optics would be anachronistic. Nevertheless, the inappropriateness of the charlatan and pseudoscientist labels that have often been affixed to him by later writers can be appreciated by considering how the diffraction issue evolved in the following generation.

In England, about a decade after Marat's death, the following challenge appeared in the *Philosophical Transactions* published by the Royal Society of London:

> The experiment of Grimaldi, on the crested fringes within the shadow, together with several others of his observations, equally important, has been left unnoticed by Newton. Those who are attached to the Newtonian theory of light, or to the hypotheses of modern opticians, founded on views still less enlarged, *would do well to endeavour to imagine any thing like an explanation of these experiments, derived from their own doctrines, and, if they fail in the attempt, to refrain at least from idle declamation* against a system which is founded on the accuracy of its application to all these facts, and to a thousand others of a similar nature.[30]

The author of this blast, Thomas Young, was mistaken in his supposition that Grimaldi's discoveries had "been left unnoticed by Newton." His misperception in this regard is an indication of the state of neglect into which the study of diffraction phenomena had fallen. Grimaldi had been all but forgotten.

Young's optical investigations resulted in a theory able to account for diffraction by assuming that light is fundamentally wavelike in nature. Young's hypothesis did not have to ignore diffraction; it could explain it, and Newton's soapbubble colors as well, as patterns of mutual reinforcement and interference characteristic of interacting waves. At the same time, of course, Young's theory represented the ultimate challenge to Newton's concept of light rays as hard, massy particles.

Like Marat, Young did not receive an open-minded hearing from his contemporaries, as his angry rejoinder to their "idle declamations" against his system illustrates. Unlike Marat, however, he lived to see his ideas on diffraction vindicated, though not through his own efforts.

About 1814 a Frenchman, Augustin Fresnel, began studying the problem of diffraction without knowing of either Marat's or Young's previous work. His first contributions to the Academy of Sciences, reporting his observations on the fringes of shadows cast by opaque objects, received a thoroughly negative response, especially from Laplace, Biot, and Poisson.

Unlike their dismissive reaction to Marat, however, the academicians took the trouble to try to refute Fresnel and ultimately discovered they could not. The mathematician Poisson carefully studied Fresnel's explanation of diffraction as a consequence of the wave nature of light and demonstrated that it led to an obvious absurdity: that under certain circumstances the shadow of a small opaque disk would be brighter at the center than at the edges. Fresnel concurred with Poisson's inference, but denied that it was absurd. To the contrary, it is true; such circular shadows can indeed have bright spots at their centers. This physical fact was demonstrated in the laboratory of Fresnel's ally, Arago, lending strong support to Fresnel's contentions.[31]

When the wave theory became dominant in the 1830s, the pendulum of scientific prejudice that had blindly shielded Newton's *Opticks* from criticism swung in the opposite direction. Throughout the rest of the nineteenth century the *Opticks* was treated as an embarrassment and downgraded

to the status of a minor work representing a lapse in the great scientist's judgment.[32]

Again, the point of recounting the histories of Young and Fresnel is not to credit Marat with prescience or identify him as their predecessor in the march toward modern science, but to illustrate the degree to which nonscientific considerations intervened in the process of scientists coming to grips with diffraction phenomena. Until Fresnel's theory gained acceptance, the empirical facts of diffraction were subordinated for a century and a half to various social and personal biases that prevented their recognition and due consideration.

Marat, in fact, made no direct contribution to the later discoveries by Young and Fresnel, but not because his efforts were intrinsicly worthless. The blame for its lack of influence is not Marat's; he did not suppress his own work. The Academy of Sciences did indeed function as a blind defender of orthodoxy in this affair, and its actions represented, to some degree, a lost opportunity for science.

Découvertes sur la lumière

Unlike Marat's previous volume on fire, *Découvertes sur la lumière* does not bear the royal approbation that would have permitted its legal publication in France. According to the title page it was printed in London and could be purchased at the offices of the

bookseller/publisher Jombert, *fils aîné*. Either Marat could not get the official censor to approve it, or he did not want to spend the time and effort to do so. Although the book certainly represented no political challenge to the crown, it is possible that its anti-Newtonian contents had alienated certain academicians enough to put its approval in doubt. It is perhaps significant that the royal censor who accepted Marat's book on fire was Sage, who sat on the commission that reviewed Marat's optical experiments. On the other hand, his book on electricity seems to have had no problem gaining the censor's approval two years later. Whatever the case, it is possible that Jombert was the actual publisher and the claim of London publication simply the standard ruse employed during the old regime to evade censorship.

Marat did not intend for *Découvertes sur la lumière* to be his last word on optics. He stated that it was simply an "extract" of a larger work to follow that would include practical applications of his principles with regard to lens-making and other useful occupations. Although the Revolution intervened before he could make good on this promise, he left an unfinished revised and considerably enlarged version of *Découvertes sur la lumière*.[33]

Marat did not put forward a wave theory of light; although his work was entitled *Découvertes sur la lumière*, he was more directly concerned with the origins of colors than with the nature of light *per se*.

Marat had no need of the wave hypothesis, since he accounted for the bending of light at surfaces of objects by assuming the existence of specific atmospheres surrounding objects. Before adopting the wave theory, Thomas Young would first find it necessary to experimentally reject "the presence of such an inflecting medium in the neighbourhood of dense substances as I was formerly inclined to attribute to them."[34]

In *Découvertes sur la lumière*, Marat once again utilized the familiar format of numbered experiments; the total in this book was 202. As would be expected, given the nature of the inquiry, almost all make use of the darkroom. Marat's innovation consisted in bringing his solar microscope to bear on the subject.

Experiment number one identifies the central phenomenon that defines the focus of the work as a whole:

> When one exposes any body whatsoever to the solar rays, focused in the darkroom with the use of a simple objective lens, one sees its shadow surrounded by a luminous band that is more or less bright and more or less wide depending upon the distance that it is from the screen.[35]

Marat uses the word "aureole" to denote that luminous band. To what can the appearance of this aureole be attributed? Experiment two shows that it "does not depend upon a double refraction since it occurs no less when a steel mirror is used."[36] Experiment three eliminates the configuration of the ambient air, which he had first suspected to be the cause, since the aureole "is no less

perceived surrounding extremely thin bodies than it is surrounding spherical bodies."[37]

After having tested and rejected two hypotheses, Marat was left with one that he believed would prove successful. The aureole was to be attributed to "the principle of attraction that brings together at the surface of a body the rays of light that surround it."[38]

He reasoned that the rays cannot be brought together at the surface of an object unless they are subtracted from somewhere else, namely from the surrounding medium. The area farther away from the object should appear less bright, then, but in a large space the difference in brightness is generally imperceptible. Marat says that this difficulty can be overcome by making the field of light very small, which is a simple matter: "instead of forming it outside the body, it must be formed inside."[39] By this he means that rather than shining a light behind an object to produce a shadow surrounded by a field of light, he suggests instead directing the light through a small hole in a flat card. The spot on the screen will then be the field of light and the shadow will surround it. Marat did not claim this as an original idea; Grimaldi's classic experiments utilized a similar method, as did Newton's, among others.

In the next three experiments he utilized this technique and in each case found the shadow surrounding the hole bordered by a bright band. He varied the size of the

hole and concluded that "the rays that pass through the hole are principally attracted toward its edges."[40]

If the hole is made very small, the luminous band circumscribes a small area. One would expect this area to be a very bright point, but it is not; it is dark. "What is the cause of this privation of light?"[41] It indicates an attraction of the rays toward the edges of the hole. This counterintuitive fact that Marat demonstrated is the inverse of the one that helped Fresnel support his case against Poisson, Laplace, and Biot several decades later.[42]

"From these experiments and a thousand more like them, we conclude that bodies attract light."[43] Marat believed that the terminology of optics should acknowledge that this phenomenon was as important as reflection and refraction. To the recognized terms "catoptrics" (signifying reflection phenomena) and "dioptrics" (signifying refraction phenomena) he proposed adding "perioptrics" to designate "the part of optics that deals with the deviation of rays of light at the edges of bodies."[44] Marat's neologism never achieved the status of an accepted term, first of all because his optical work as a whole was suppressed, and secondly because the usefulness of such a term diminished in the nineteenth century with the rise of the wave theory of light. Nonetheless, at the time it was put forward by Marat it was a reasonable suggestion for a useful addition to the lexicon of optics.

In the course of introducing the new term, Marat stated:

> No doubt the deviation of the rays of light that I have just spoken of are nothing, in truth, but an external refraction, and it should be considered as related to the same principle.[45]

This was an echo of Newton's suggestion that inflexion may perhaps be but a special case of reflection and refraction. Priestley agreed with this view and stated it more positively:

> The same power that disposes the light which actually falls upon bodies to be reflected from them, or refracted into them, affects the light which only passes very near to them; some of the rays being thereby made to bend from them, which is similar to *reflexion*, and others to bend towards them, which is a kind of partial *refraction*.[46]

But whereas Newton and Priestley considered inflexion to be subordinate to refraction, Marat thought it should be the other way around: if anything, refraction was but a special case of inflexion. In either case, "however similar these principles [refraction and inflexion] may be, their laws are different and it is important not to confuse the phenomena."[47] Hence the need for the term *perioptrics* as distinct from *dioptrics*.

Marat devoted considerable attention to the force of attraction that he believed underlies this phenomenon. "Since all bodies attract light," he says, "each therefore has a particular atmosphere. This atmosphere becomes perceptible due to the aureole that appears to surround

their shadows."[48] As always, the ocular demonstration ranks high among the elements that count for evidence to Marat.

If the attraction that draws light to objects is at all akin to gravitational attraction, one would expect it to vary relative to the mass of the objects. Indeed, Marat believed this would be found to be so, "because it is a consistent law that the attractive force is deployed proportionately to mass."[49] This is not the only factor, however, because "it is no less consistent a law that this force is deployed proportionately to the affinity between bodies."[50]

Marat appeared to be making some assumptions about the attractive force that are not stated explicitly here. It would seem that he thought of it as a composite of gravitational force in the usual sense with the forces that cause the smallest particles of matter to bind together; *i.e.*, the forces of chemical affinity.

Of the two factors -- mass and chemical affinity -- the latter clearly seemed by far the more powerful to Marat. Experiment 31 revealed that

> after having introduced solar rays into a darkroom . . . if you present on the same line, at four feet from the screen, some objects of the same dimensions but different density, you will not necessarily find the most extensive aureoles around the denser objects; in fact, some of the denser objects have much less extensive aureoles.[51]

If mass were the only consideration, the denser objects would be expected to exert a greater attractive force and would therefore have broader luminous bands

surrounding them, but Marat found that not to be so. More important than mass is the kind of material the object is made of:

> After a multitude of experiments of this kind it was ascertained that certain materials, such as white wood, resin, paper, cotton cloth, wax, tallow, plaster, and above all the igneous fluid, attract light more than metals do.[52]

For example, Marat compared the shadows of objects made of lead and cork that were identical in size and shape. He found in both cases that the shadows were slightly smaller than would be expected if light traveled in perfectly straight lines past the objects. However, the amount of diminution of the shadows hardly differed at all between the two objects. This illustrated that neither the density of objects nor the specific characteristics of their materials has much influence on the attraction of light.

Marat discovered another variable, however, that proved to be a more significant factor:

> the size of the surfaces influences the attraction of light much more than either of the two aforementioned causes. . . . The deviation of rays tangent to the circumference of spherical bodies is related to the lengths of their circumferences, not to their masses.[53]

Regardless of the density of the material, "the attractive force always deploys itself in inverse ratio to the square of the distance."[54] Therefore, it is always at a maximum right at the surface of an object. Since it is evenly distributed over that surface, increasing the

surface area decreases the force per unit of area. "Thus the attraction of light is proportionately more intense in smaller as opposed to larger objects."[55] This assertion is supported by experiments 32 through 35.

Marat's emphasis on the attraction of light seems to place him somewhat at odds with earlier experimenters. If light is simply attracted to the objects it passes, then it would bend *toward* those objects and their shadows would appear smaller than if light travelled in perfectly straight lines. Marat reported that this is what he saw in the darkroom: smaller shadows.

Grimaldi's initial reports were quite the opposite: the diffraction of light produced *larger* shadows, and therefore indicated that the light was somehow deflected *away* from the edges of objects.

Newton, it will be recalled, suggested that light is deflected both ways; that in passing close to edges of objects rays of light are "bent several times backwards and forwards, with a motion like that of an Eel."[56]

The discrepancy between Marat's smaller shadows and the larger shadows reported by others can be accounted for by their differing definitions of the shadow. Marat did not include what he called the aureole as part of the shadow while the other investigators did. Some of the alternating light and dark bands or colored fringes lie inside and some lie outside the ideal shadow that would be made if the

light's trajectory were perfectly straight; that is why Newton conceived of light bending backward and forward.

Marat, however, treated the bending of the light and its dissociation into colors as distinct phenomena -- associated, to be sure, but not identical. To him, only the completely solid dark area counted as part of the shadow, and by that definition his claim that light bends always into the shadow and never away from it is comprehensible.

According to Marat, the colored bands outside the shadow (and even those that appeared black were really very dark blue) resulted from the decomposition of light. "All known bodies decompose light in attracting it," he declared, offering experiment number 42 into evidence:

> Place some of these bodies four feet from the screen and in the cone of light; their aureole will appear distinct. Fix this aureole carefully; it will appear to be divided into three small bands, an inside one of dark blue, an outside one of pale yellow, and a white one in the middle.[57]

He found that he could increase color production by shining light through a solid object cut in such a way that it became a grid of narrow openings (*un solide découpé en réseau*):

> But it is important that the little slits that make it up be parallel and separated only by small intervals. Then when at three feet from the screen you expose to the solar rays a card, a metal plate, or an ivory plaque cut up in this manner, the shadow of the grid is plainly seen bordered by bands of various colors. The colors that are consistently distinguishable are blue, straw, and pink.[58]

Marat had devised a simple diffraction grating. Priestley's history of optics relates that a number of

investigators had found colors in the shadows of very narrow objects such as needles, wires, or thin openings between straightedges, but none are reported to have arranged such objects to form a series of parallel slits, as Marat did. If color production were believed to be a function of light passing close to edges, it was a logical step to multiply the effect by multiplying the number of edges, which is what Marat accomplished with his *solide découpé en réseau*.

An interesting variant is described later in the book; experiments 118 and 119 utilize "a small disk with several very thin concentric circles cut out of it." The shadow of this device shows "each circle covered with three colored bands of equal width and intensity."[59]

While the color phenomena brought forth by most diffraction demonstrations in the eighteenth century were modest and no doubt unimpressive to skeptical observers, Marat's diffraction gratings could potentially have produced a more dramatic display of colors that would be harder to ignore. Since Marat did not give the dimensions of his gratings, the impact of the images they produced cannot be estimated. If the Academy's commissioners had been prone to discount Marat's claims, however, the colors produced by these apparatuses may have given some of them pause. If Le Roy did indeed struggle over preparing his report, his difficulty might well have arisen from trying to interpret effects produced by the diffraction grating

according to orthodox Newtonian principles. This, however, is a matter of speculation.

On the other hand, Marat was not given to understatement, and never underestimated the value of a striking visual demonstration. The fact that he did not emphasize his diffraction gratings suggests that their slits may have been too wide or far apart to really produce dramatic displays of color.

Rather than pursuing the diffraction-grating demonstrations, Marat turned instead to showing how "the aureoles of objects can be perceived by the naked eye." Experiment 82 gives one example:

> At six inches from the eye and several feet from a large jet of white flame, interpose a key, a little box, a pencil; their edges will appear transparent and bright red.[60]

A modern reader can substitute a lightbulb for the jet of white flame and try to see what Marat saw. Under appropriate conditions, the bottom of a straightedge against a bright light will appear to have a faint red or red and yellow fringe; viewed along the top edge, the fringe will appear blue. Patience is required to attain the necessary conditions -- it is particularly important that the eye be well adapted to the darkness -- but these colors are indeed perceptible to the naked eye.

The first of Marat's two frontal assaults on Newtonian orthodoxy had to do with the number of primary colors. Marat disputed the prevailing belief, based on Newton's

teaching, that there are seven primary colors. The truly primary colors, he said,

> are limited to yellow, red, and blue, because when the light rays to which any isolated body is exposed are decomposed, they always come to have only these three different colors, whether it is in the darkroom or outdoors, by the light of the sun or that of a candle.[61]

Marat believed, as Newton had, that different colors of light are composed of qualitatively different kinds of rays. But whereas Newton believed that there could be different kinds for every perceptible color, Marat held that there could only be three: one kind for yellow light, another for red, and another for blue. All other hues are composites of these, he said, including the indigo, violet, orange, and green that the Newtonians considered primary. "Even the prism gives no more than three inalterable colors; all the others are decompositions of those."[62] This is shown by setting up a prism demonstration in the usual way, but moving it back and forth, toward and away from the screen. As the prism moves, the green band (for example) can be seen to weaken and finally disappear into the encroaching yellow and red bands.

Continuing to move the prism in the same direction results in even the yellow band fading away into the encroaching blue and red bands. This seems to imply that among the three primary colors, two of them, red and blue, are more primary than the third.

Gillispie, although critical of Marat's contentions regarding the primary colors, acknowledges that they merited more attention than the commissioners of the Academy of Sciences gave them.[63]

> Newton's treatment of primary colors was certainly vulnerable, but Marat never gave his criticism any development. There is no hint of the distinction Goethe called for between the study of color as produced in light and as perceived in the eye.[64]

In fact, however, Marat did call attention to the physiological basis of color perception:

> Properly speaking, the colors consist of the elemental impressions of light on the organ of sight, because the light *per se* does not include any color within itself.[65]

The subtle fluid of light, he continued,

> is composed of parts that differ essentially among themselves, since they consistently affect the organ of sight in different manners. Thus, each ray of light is composed of three others, one of which produces the sensation of yellow, another that of red, and another that of blue; from their various combinations result a great number of composite sensations.[66]

Marat did not dwell on these points; *Découvertes sur la lumière* was, after all, a book about physics rather than physiology. It is worth recalling, though, that Marat's earlier publications in England included a tract on eye disease and revealed considerable knowledge (by contemporary standards) of the physiology of vision.

Marat's second major challenge to the Newtonians concerned the role of the prism in producing color. Newton's most admired experiment was the one where he showed that white light refracted through a prism divides

up into a spectrum of light of various colors. He clinched his argument by showing that the beam of colored light could be refracted in an inverse manner to reproduce the original white light. This experiment was so straightforward that Newton's interpretation seemed self-evident to later generations.

Although the Newtonian explanation had attained the force of dogma and had remained virtually unchallenged for more than half a century, Marat was not convinced. The appearance of color, he contended, owes absolutely nothing to refraction; it is entirely due to the decomposition of light at the edges of objects. The colors that Newton saw on the wall did not originate in his prism but in the hole in the window shutters where the light entered the room. The light was decomposed into colors as it passed the edges of the hole; the colors already existed before the light reached the prism. (See the diagram on the next page.)

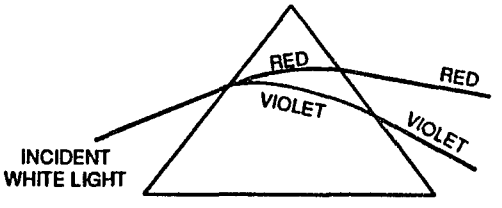
Marat anticipated some objections to this hypothesis. Admitting the light by means of a larger hole can allow white light -- light that has not been decomposed -- to pass through the prism; when that light reaches the screen it, too, will be seen to have separated into a spectrum of colors. Were those colors not produced by the prism?

Yes, Marat replied, but not in the way Newton thought; not by means of refraction. In this case the colors are decomposed by diffraction at the edge of the prism itself. As the red, blue, and yellow light then travels through the

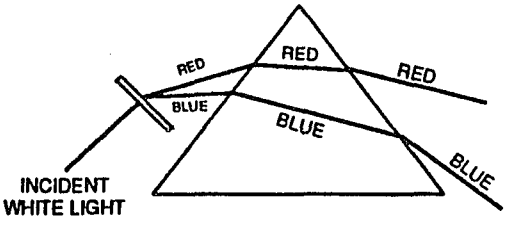
prism, it is refracted, but not differentially, as the standard view would have it; all three are refracted exactly the same amount.

> If some of them appear afterwards to have been refracted more than others, it is because their angles of incidence were unequal.[67]

Marat also used this explanation to account for why the images produced by prisms appear far more colorful than those produced by edges of objects. The light in the prism experiments is decomposed twice; once at the edges of the window hole and again at the edges of the prism. The diagram below illustrates the differences between Newton's and Marat's theories of how colors are produced.[68]



NEWTON:
Colors produced
by refraction



MARAT:
Colors produced
by diffraction

As proof of his contention that Newton was wrong about differential refrangibility, Marat proposed to measure and compare the focal distances of "the various rays of which light is composed." The ideal way to do this, he acknowledged, would be to use pure rays of blue, red, or yellow light. Unfortunately it is not feasible to do so, "since it is extremely difficult to gather a bundle of rays

of each kind large enough to cover the entire surface of a lens."[69] As an alternative to "rays decomposed at the edges of objects" he suggests using "rays decomposed at their surface; that is to say, the heterogeneous reflected rays."[70] Red reflected rays, for example, are those that are seen when looking at a red object, such as an apple.

For experimental simplicity, Marat utilized colored disks; one red, one blue, and one yellow. The images of these disks were projected through a lens onto a plane parallel to the disk. "Since the focal distances are the same no matter what the color of the bodies, the heterogeneous rays are equally refrangible."[71] Q.E.D.

Marat noted that Newton had likewise tried to measure focal lengths of light of different colors, but reported an exactly opposite result: that the focal lengths were not the same. "I am very far from suspecting that great man of untruthfulness," Marat declared, "but I cannot fail to point out that the manner in which he proceeded was very defective."[72]

It is here that Newton's partisans must have found Marat's presentation most offensive. Marat expressed wonder that Newton could have repeated his prism experiments for thirty years, and that other physicists had repeated them for another fifty or so, without understanding where the colors were coming from: "It is regrettable that such a great genius wasted so much time in vain in this research."[73]

As for his own theory attributing all color separation to diffraction rather than refraction, Marat stated:

> I know that this assertion is entirely opposite to that of all authors who have written on this subject, but it is founded on a multitude of simple, clear, and consistent experiments, whereas the other authors have established their opinion only on the results of experiments that are complicated, illusory, and based upon the most superficial understanding of a principle that plays a great role in nature.[74]

These were strong words, and the "other authors" could not have been expected to find them congenial. Nonetheless, Marat's demeanor should not be considered sufficient cause for other scientists' refusal to come to grips with his critique of Newtonian optics. His experiments did indeed demonstrate significant anomalies for the accepted paradigm; Marat was justified in claiming that the apparent bending of light at the edges of objects is "a principle that plays a great role in nature." The rejection of Marat's ideas seems to have been based more upon personal antipathies than upon rational scientific criteria.

Addendum: Goethe and Marat

Wolfgang von Goethe devoted some thirty years to developing and defending a theory of colors in opposition to the received Newtonian doctrine on the nature of color. The explanation of color phenomena that Newton had advanced in his *Opticks* was so impressive to most scientists that it had tended to discourage further inquiry. Goethe, however, challenged the Newtonian orthodoxy in a long ideological struggle that culminated in the publication of his massive

Zur Farbenlehre in 1810. As was noted in Chapter One, above, historians of science have traditionally dismissed Goethe's color theories as bad science, but recent studies have begun to force a new appreciation of *Zur Farbenlehre.*[75]

A central focus of Goethe's critique was the Newtonians' erroneous treatment of color as solely a physical property of light. If that were true, then color would be perfectly determined by some measurable quantity, such as Newton's "differential refrangibility" or, later, wavelength. To measure that property of light would be to know all there is to know about the color it produces. The reduction of color to a quantifiable aspect of light is seductive in its simplicity, but Goethe demonstrated that it simply does not accord with a great body of observational and experimental fact.

The Newtonian reduction leaves the undeniable facts of color *perception* out of account. Goethe's critique pointed the way toward understanding that the study of color cannot be exclusively restricted to the science of physics, but must be extended to the sciences of physiology and psychology as well. While light of varying measurable characteristics does exist external to the eye and brain, *color* does not.

The limitations of the Newtonian color doctrine exemplify the more general fallacy inherent in reductionism: the habit of declaring that a complex

phenomenon is "nothing but" an aggregate of simple parts. Much of the anger that Goethe expressed toward the Newtonians derived from his understandable irritation at seeing the mantle of science being claimed exclusively for the narrow metaphysics of the "nothing-but."

It is worth noting a common theme in the traditional dismissal of both Goethe's color theory and the Mesmerists' animal magnetism. In both cases, scientific orthodoxy held that only objective phenomena qualify as worthy of scientific attention, and to be objective a phenomenon must exist "out there"; *i.e.*, external to the human observer. By this criterion neither the crises induced by the Mesmerists in their patients nor color *per se* are objective. The refusal to acknowledge their reality and the need to investigate them does not indicate scientific maturity, but conservatism and narrowness of view.

Goethe devoted considerable space in *Zur Farbenlehre* to critically examining the ideas of his predecessors on the subject of colors. He summarized the central theses of Marat's optical theories in five pages.[76] In addition to *Découvertes sur la lumière,* Goethe cited two of Marat's shorter works on optics as well.[77]

Goethe believed that Marat's work had constituted a significant challenge to the Newtonian hegemony in the field of optics. He hailed Marat's opposition to the "accepted teachings" of the Academy of Sciences. As for the Academy's commission that had passed judgment on Marat's

ideas, Goethe considered its "expert opinion" to be "an example of bad faith on the part of those confronted with something that cannot be totally denied, in an attempt to at least do away with it."[78]

In Goethe's opinion, Marat's writings revealed an admirable degree of "perceptiveness and insight" on his part:

> As far as we are concerned, we think that Marat has, with a great deal of intelligence and good observations, brought the doctrine of color . . . to a very critical point, worthy of more observations, the clarification of which we can hope will lead to genuine progress in the doctrine of color.[79]

Marat's and Goethe's ideas on colors were by no means identical, however. "Those who are familiar with our views," Goethe wrote, "know where we stand with regard to the work of this researcher."[80] One of Goethe's main criticisms of the Newtonians was their notion that white light is a composite that can be analyzed into rays of various colors by refraction through a prism. Goethe held that white light was primary and indivisible. Marat substituted diffraction for refraction as the agent of decomposition, but the very notion of decomposition was unacceptable to Goethe.

On the other hand, Goethe believed (in much the same way that Aristotle had) that color phenomena arise from a contrast of light and darkness, or black and white. Since Marat's perioptrics drew attention to color production at the edges of objects -- that is, at the juncture between a

dark object and the light -- Goethe felt that Marat's doctrine represented an improvement over Newton's.

Goethe was no more successful than Marat in breaking through the wall of academic prejudice that shielded the Newtonian doctrine and sustained its hegemony. The recent scholarship that has recognized valuable and sophisticated insights in Goethe's ideas about color puts Marat's scientific work in a more favorable light as well.

CHAPTER ELEVEN

THE ILLUSION OF REPULSION: *RECHERCHES PHYSIQUES SUR L'ELECTRICITE*

The third volume of Marat's trilogy was devoted to an area of physics with a history much different from that of fire or light. Fire and light must certainly have been among the first natural phenomena to arouse the curiosity of early *homo sapiens*. Consciousness of the simplest electrical phenomena, on the other hand, seems to have only arisen during classical antiquity with the recognition that amber, when rubbed, attracts tiny bits of other materials. This amber effect remained no more than a minor curiosity until the middle of the eighteenth century. Although it began to receive more systematic attention in the seventeenth century, it was only in the generation immediately preceeding Marat's that electricity was definitely recognized as a major force of nature.

Suspicions of its significance arose from obvious analogies between sparks and shocks, on the one hand, and bolts of lightning on the other, but the conjectures were not confirmed until the famous experiments of Franklin and others in the 1750s. Only then did electricity move from the periphery to the center of physical investigations and attain the status of a full-fledged branch of physics. Franklin and other pioneers of electrical science, then, were still living and contributing to debates over

electrical theory at the time Marat was investigating the subject.

Marat's *Recherches physiques sur l'électricité* followed the same general format as his two previous volumes: a narrative of experiments intended to illustrate and support a comprehensive theory. While the book reports 214 experiments, Marat believed his main contribution to electrical science would be theoretical, rather than simply augmenting the pool of empirical facts. It was his intention to bring theoretical clarity to the jumble of conflicting hypotheses.

The validity of Marat's assessment of the chaotic state of electrical theory in the late 1770s may not be immediately apparent in retrospect. The electrical theory of Benjamin Franklin that was published in 1751 had gained wide support and influence, and since some of its key elements are still considered valid today, the extent of its acceptance by contemporaries tends to be overestimated. J. L. Heilbron has demonstrated that the very influential "Franklinist history" promoted by Priestley exaggerated the success of Franklin's theory, especially with regard to continental physicists.[1] In fact, Franklin's single-fluid theory of electricity was confronted with some glaring anomalies that Franklin himself was well aware of. As important as Franklin's contributions to electrical theory were, then, no serious scientist could have disagreed with

Marat's contention that in 1782 some very fundamental questions remained to be answered.

The ongoing debates over electrical theory were certain to stimulate Marat's interest; he seemed particularly drawn toward disputed issues. It was in his role as a controversialist that Marat the scientist most resembled Marat the revolutionary politician.

The Phenomena

For centuries the mysterious hopping of tiny bits of matter toward amber or other materials supplied one of the key mysteries of "natural magic." Like terrestrial gravity, and even more like the magnetic action of the lodestone, an occult force seemed to be creating action at a distance, confounding mechanical and materialist explanations of natural phenomena.

At the beginning of the seventeenth century, William Gilbert systematically examined the amber effect with the primary motive of distinguishing it from magnetism. His investigation of the electrical properties of a wide range of materials led him to divide them into two categories: "electrics" and "nonelectrics." Materials designated as electrics were those like amber or glass that, when rubbed, would display the attractive power. Nonelectrics were materials such as metals that seemed not to exhibit that property.

The chronology of early electrical discoveries need not be recounted here.[2] By the early eighteenth century the extent of electrical knowledge could be summarized in the following propositions: that small electrical forces could be created by rubbing glass, resin, or other materials; that these forces could repel as well as attract; that sometimes they would attract, hold, and then repel the same small object; that the electric "virtue" could be transferred from one object to another by contact; that one electrified object could temporarily electrify nearby objects without contact.

The ability to transfer electrification by contact suggested that perhaps electricity, like heat, was a subtle fluid. This supposition was strengthened by the discovery that the electric virtue could be transmitted over long distances -- hundreds of feet! -- through cords made of hemp. On the other hand, the mysterious temporary influence in the absence of contact suggested otherwise. This latter phenomenon, later called electrostatic induction, posed particularly difficult theoretical problems.

In 1733 Charles François Du Fay discovered the existence of two qualitatively different kinds of electricity; he called them "resinous" and "vitreous" after the kinds of materials they seemed to be associated with. The two electricities were distinguishable by their behavior: two objects possessing resinous electricity would repel each other, as would two objects possessing the

vitreous kind. But a vitreously charged object and a resinously charged object would always attract each other.

A major advance in electrical investigations occurred in the 1740s with the introduction of two important experimental tools: the electrostatic generator and the Leyden jar. The generator had been invented earlier, but only came into widespread use about 1743, by a number of German and Dutch experimenters.

Experimenters had long been able to produce small electrical charges by vigorously rubbing glass rods. This process was mechanized by attaching a crank to a glass globe or cylinder. By turning the crank with one hand while pressing the other hand against the spinning glass, an experimenter could generate a significant charge with minimal effort. Further innovations followed: a conducting wire was added, one end brushing continuously against the spinning glass and the other leading to a "prime conductor," which was usually a long piece of metal such as a gun barrel. These electrical machines could produce big flashes and shocks, far more impressive than the little sparks and tingling sensations of ordinary static electricity. Touching the prime conductor of one of these machines could kill small animals and knock strong men to the floor.[3] The power of electricity began to gain respect and to be compared with the effects of lightning.

In 1745, two physicists independently invented what became known as the Leyden jar. A German, E. J. von Kliest,

and a Dutchman, Pieter van Musschenbroek, each discovered a surprising phenomenon when they attempted to transfer electricity from a generator's prime conductor to a bottle of water. To accomplish this, they attached one end of a conducting wire to the prime conductor; the other end was hung into the water in a glass container.

When Musschenbroek tried this, he turned the crank for a while and then paused to examine the bottle of water. When he touched it he received a most unexpected result. In a letter to the Parisian Academy of Sciences, Musschenbroek reported what happened with a grave warning:

> I am going to tell you about a new but terrible experiment which I advise you not to try for yourself. . . . I was making some investigations on the force of electricity. . . . Suddenly my right hand was struck so violently that all my body was affected as if it had been struck by lightning. . . . The arm and all the body are affected in a terrible way that I cannot describe: in a word, I thought that it was all up with me.[4]

This device, the Leyden jar, seemed to offer further confirmation of the idea that the electric virtue was a sort of fluid. A fluid seemed to flow from the generator, along the conducting wire, and into the jar, where large quantities could remain stored.

The strangeness and newfound strength of electricity as revealed by the innovations in experimental apparatus stimulated efforts to understand it. A number of experimenters exchanged ideas and debated theories, but the most successful explanation was put forward by Benjamin Franklin in a series of letters between 1747 and 1754.

Franklin's theory first appeared in book form in 1751 and was published in French translation the following year. Although the physicists of Europe were surprised to see a major theoretical contribution coming from the American colonies, Franklin's view of electricity rapidly gained adherents and attained a strong position among competing theories, such as that of the Abbé Nollet. Nollet explained electrical attraction and repulsion by means of subtle effluvia streaming into and out of electrified objects. Franklin wrote in his autobiography that his own theory "was by degrees universally adopted . . . in preference to that of the Abbé."[5] While never achieving the dominant status of an unchallengeable paradigm, Franklin's single-fluid theory became one to which all other hypotheses about electricity were referred and compared.

Franklin's theory offered the simplicity of a combined explanation of both kinds of electricity, vitreous and resinous. It did away with Nollet's cumbersome system of dual emanations. According to Franklin, unelectrified bodies all contain a certain natural amount of electric fluid. When the normal state of equilibrium is changed by a transfer of fluid from one object to another, the one that loses fluid is in a state of "negative" electrification; the one that gains fluid is in a state of "positive" electrification. The difference between vitreous and resinous electricity is not a matter of different fluids,

then, but of a surplus or deficit of a single electric fluid.

The authority of Franklin's theory was enhanced by his success in showing that lightning can fill a Leyden jar with electric fluid. Although his priority with regard to this discovery was challenged by others, Franklin justifiably retained the greatest share of fame for it.

In France, the Abbé Nollet criticized Franklin's theory and continued to oppose his own hypothetical emanations to it. Nollet's ideas had a strong Cartesian flavor that helped win them a following in France. Nevertheless, some leading French physicists broke with Nollet and supported Franklin; among them was J. B. Le Roy, the academician who was to lead the commissions investigating Marat's experiments. Internal Academy politics played a large role in these developments. Franklin's theory was promoted by Buffon and his faction as a weapon against another faction led by Réaumur, which included Nollet. Buffon had arranged for the translation and publication of Franklin's book in order to embarrass Nollet.

The single fluid that Franklin postulated as the carrier of the electric virtue was a classical Newtonian elastic fluid, but with certain unique characteristics. An elastic fluid, such as ordinary atmospheric air, was conceived of as being made up of tiny particles that repel each other. Franklin was unable to offer a physical

explanation for this repulsive power, but believed that some physical basis -- an aether of some sort, perhaps -- must exist. Franklin was apparently willing to accept attraction as a fundamental property of matter requiring no further explanation of its action at a distance, but not repulsion.

The power of the fluid's particles to repel each other would account for its tendency to spread out and flow away from places where a surplus has accumulated. The unusual property of these electric particles, however, is that while they repel each other, they attract (and are attracted by) the particles of all ordinary matter. That would account for their tendency to flow into objects and remain there in a state of equilibrium.

The ordinary exchange of electricity by objects in contact was not difficult to conceive of in terms of the flow of Franklin's single fluid. But how were the phenomena of electrostatic induction to be explained? How was it that simply bringing an electrified object very close to an unelectrified object could electrify the latter without touching it? This phenomenon was all the more puzzling for being only temporary; when the two objects are moved apart again, the one that was originally unelectrified returns to its neutral state. Without contact, through what channels could anything have flowed into and then back out of it?

One way of accounting for this was to postulate electric atmospheres surrounding electrified objects.

Franklin did not originate this concept but his theory utilized it. When an object is electrified, according to this notion, the surplus particles of electric fluid tend to spread out beyond the confines of the object itself, forming a special atmosphere of electric particles. If that is the case, however, what ultimately confines the electric atmosphere to a specific, rather small volume around the object? Franklin suggested that the pressure of the surrounding air could perhaps account for it. This was an *ad hoc* explanation that was not fully satisfying even to Franklin. Some other theorists, including John Canton, sought to abandon the idea of electrical atmospheres altogether.

The greatest difficulty for the single-fluid theory was its inability to explain certain phenomena involving negatively electrified objects. First of all, how is it that a negatively electrified object can induce a temporary state of electrification in a nearby object? This presented a glaring problem. It was possible to conceive of a surplus of a single electric fluid supersaturating an object and spilling out of it to create an atmosphere. But how could a *deficiency* of that fluid create an atmosphere? Two different electricities that create two different kinds of atmospheres implies two distinct electric fluids.

There was an even more fundamental problem:

> In terms of Franklin's theory, there is no reason why two negatively charged bodies should repel one

> another, although . . . Franklin reluctantly came to admit that such a phenomenon always occurs.[6]

How, indeed, could the deficiency of a fluid cause objects to mutally recede?

> This question could not be avoided in discussions of the cause of electrical phenomena and it became a focal point for evaluating the merits of the whole Franklinian theory. . . . But Franklin had no contribution to make to the problem of why two negatively charged bodies repel each other.[7]

The paradoxes of negative electricity were persistent anomalies for his system, not unlike that of diffraction for Newton's color doctrine. In 1759, Ulrich Theodor Aepinus published a modification of Franklin's single-fluid theory that could "save the phenomena," but at the cost of introducing a questionable theoretical innovation.[8]

Aepinus's proposed solution was a radical one involving the redefinition of the nature of matter. In Franklin's theory the particles of electric fluid, it will be recalled, were endowed with the property of mutual repulsion but were supposedly attracted to the particles of ordinary matter. The particles of ordinary matter in the solid and liquid states, of course, were assumed to attract each other as well as those of the electric fluid. Aepinus's theory required that the particles of ordinary matter not only attract each other, but at the same time repel each other.

Aepinus, in other words, universalized repulsion as an inherent property of matter. According to this conception, the particles of ordinary matter and electric fluid coexist

with respect to each other by the rules "like attracts unlike" and "like repels like." In a neutral state of electrification, the attractive and repulsive forces are in equilibrium. When some electric fluid flows out of objects the balance of forces is disrupted, bringing the repulsive forces of the particles to the fore, thus causing two negatively electrified objects to repel each other.

Aepinus was well aware that many physicists would find his theory impossible to accept; that they would find "a contradiction in my assuming that a bodily matter is endowed at the same time with two opposite forces, namely, the repelling and the attracting."[9]

Particles of ordinary matter obviously attract each other; the fact that they aggregate in solid or liquid form could not be explained otherwise. The repulsive force postulated by Aepinus seemed by comparison to be no more than an *ad hoc* explanation designed to arbitrarily provide internal consistency to a theory of electricity. Nonetheless, as counterintuitive as his suggestion appeared, it did win adherents who preferred accepting some theoretical oddities to rejecting the single-fluid theory. The preference for a single-fluid theory as opposed to a dual-fluid alternative was based on a metaphysical desire to simplify the workings of nature to the greatest degree possible.

At about the same time as Aepinus published his electrical theory, a general theory of matter was proposed

by the Jesuit Roger Boscovich that also utilized the concept of a combination of attractive and repulsive forces.[10] Boscovich's notion of matter as a dynamic unity of opposing forces strongly influenced Kant's natural philosophy and through him the school of *Naturphilosophie*.

In England and France, however, the trend of metaphysical thought was considerably more conservative. The idea of a particle that simultaneously attracted and repelled others like itself was generally considered to be a violation of formal logic. Such self-contradiction was considered intolerable and Aepinus's electrical theory was not well received. More than a century later, in 1873, James Clerk Maxwell still found it necessary to argue against the objection "that if particles of matter repel one another, the theory must be in opposition to universal gravitation."[11]

A revised two-fluid theory of electricity proposed by Robert Symmer in 1759 met with more success. In spite of Franklin's great prestige and authority, the two-fluid theory seemed to many to be more in accord with known empirical facts about electricity. The "height of the active controversy over whether there were two electric fluids or one" occurred "in the closing years of the eighteenth century and the opening years of the nineteenth."[12] Coulomb lent his great authority to the support of the two-fluid theory. Furthermore, the discovery of the process of electrolysis after 1807 strongly

suggested the flow of two separate fluids in opposite directions: how else could one explain the simultaneous motion of sodium toward a cathode and chlorine toward an anode in an elecrified solution of molten sodium chloride? The two-fluid theory became dominant and remained so well into the nineteenth century.

It is evident that the theoretical situation that Marat encountered in the field of electricity was far from settled, and was becoming even less so at the very time that he was performing his experiments.

> By about the time of Franklin's death the uncovering of many phenomena had introduced into electrical theory some new considerations and a deep range of complexity.[13]

Marat was certainly not out of step with the science of his times in striving to develop a new synthesis of electrical theory. This was one of the central issues agitating physicists and *Recherches physiques sur le électricité* was Marat's contribution to the ongoing debate.

The volume, like the first in the series, appeared with the censor's stamp of approval.[14] The author is identified on the cover page, as he was in both previous volumes, as M. Marat, medical doctor and physician to the bodyguards of Monseigneur le Comte d'Artois.

Marat's Preliminary Discourse began with a brief survey of the history of electrical science. He described and praised the contributions of Gray, Hauksbee, Du Fay, Nollet, and others, saying that while they had not been

able to make a science of electricity, at least they had prepared the ground well. It is to "the celebrated Franklin," however, that the new science owes its greatest debt:

> The big picture that he opened our eyes to made better known a kind of matter that had for such a long time escaped observation. The subtle fluid, spread out everywhere throughout the surface of the earth, stays peacefully hidden in the depths of objects as long as it is proportionately distributed there. But after accumulating in the skies, if it brusquely regains its abandoned space, it often becomes one of nature's most irresistable forces.[15]

While Marat expressed appreciation for the empirical discoveries of his predecessors, he was generally scornful of previous attempts to create a synthesis of the known facts:

> Most of the works published on this subject are hardly more than summaries of puerile observations, poorly done experiments, false inductions, random hypotheses and contradictory opinions.[16]

In support of this negative assessment, Marat cited Priestley's *History and Present State of Electricity*. Although this work was itself a theoretical synthesis of sorts, Marat did not condemn it as such but utilized Priestley's account of conflicting theories to illustrate the chaos in which the subject remained.

Marat's stated purpose in undertaking the study of electricity was to create order where there had previously been only confusion. He began his exposition by outlining some of the general principles of his own theory of electricity. First of all, "electrical phenomena are

dependent on the action of a fluid in motion." While other physicists had confused this fluid with that of fire, of light, or of magnetism, "I have shown that it is of a unique nature."[17]

As for the essential properties of this fluid, previous theorists had believed them to be twofold; first, that its atoms are attracted by those of all other materials, and second, that its atoms mutually repel each other -- "fecund principles from which all of its effects have always been made to flow."

> But in examining these principles carefully I have assured myself of the truth of the former and the falsity of the latter. Better than that, I have demonstrated that, far from the electrical globules repulsing each other, they reciprocally attract each other.[18]

This is a key element of Marat's system of natural philosophy. Like Franklin, he was willing to accept attraction as a basic force of nature requiring no further explanation, but could not accord the same privileged status to repulsion. He could see the same apparent repulsion that all experimenters had witnessed, but he insisted that it was only a superficial appearance. Unlike Franklin, Marat saw no need to ascribe apparent repulsion to a yet-to-be-discovered aether; he believed all such occurrences could ultimately be explained in terms of forces of attraction.

The prevailing concept of an elastic fluid, it will be recalled, depended upon its particles being endowed with a

repulsive force. Marat, of course, could not allow that in his theory:

> Elasticity is still counted among the properties of this fluid; I will prove that it is not at all elastic. From that it follows that attraction is the great principle that can shed light upon these phenomena. Thus attributed to a single cause, the theory of electricity becomes simpler, clearer, and brighter.[19]

The extent to which Marat had adopted the general outlines of Franklin's theory is apparent in his assertion that while other objects attract the electric fluid,

> none can receive a surplus quantity without another losing a proportional amount. These latter are called *less electrified* or *negatively electrified*; the former are called *more electrified* or *positively electrified*.[20]

A final major feature of Marat's theory sought to account for how objects sometimes maintain their state of electrification without their electric fluid simply dissipating into the environment:

> It has previously been believed that the fluid accumulated in objects is retained only by their attractive force; I will demonstrate that it is above all retained by the pressure of the ambient air.[21]

Although Marat seems to suggest that this was an innovation on his part, it was reminiscent of Franklin's speculations on the subject. Marat states it more positively, since it is more central to his system than it was to Franklin's. Furthermore, there were differences in their conceptions of the role of the air. For Franklin the air served to restrict the extent of the electric atmosphere surrounding charged objects, and it did so simply by means of its

nature as a nonconductor. For Marat, who denied the existence of electric atmospheres, the air kept the electric fluid within the objects themselves, and did so by means of its pressure as well as its nonconductivity.

This concept, it should be noted, was still defended decades later by such pillars of scientific orthodoxy as Biot and Poisson: "As for the vexed question of the agency that prevented the escape of fluids from the surface of conductors," Heilbron writes, "Poisson was content to suppose with Biot that the 'pressure' of the air clamped electricity to its carrier."[22]

New Terminology

In electrical science as in optics Marat found a terminological reform necessary. He ruled out the words conductors, nonconductors, electrics, and nonelectrics to indicate a material's ability or inability to allow passage of the fluid.

> Instead of these, I have substituted two terms that correspond perfectly to the subject: "deferents" is the name I have given materials that transmit all at once the surplus quantity of fluid necessary to give a shock; "indeferents" is what I call those that transmit that quantity only little by little.[23]

Marat argued that his terms reflected the real situation better than the old ones. "Conductor" and "nonconductor" were misleading because all materials allow passage of at least a little electric fluid.

> Those that had been designated as insulators had been regarded as impermeable: an unfounded opinion supported with regard to glass alone by some

> unconvincing arguments. I have shown by means of new and decisive experiments that various kinds of glass are not impermeable.[24]

Although no material can be considered to be a perfect insulator, however, some are suitable for use as insulators for most practical purposes.

Marat next described his experimental apparatus in great detail with the help of a set of excellent professionally drawn illustrations. His devices were based on the standard pieces of equipment used by his contemporaries in electrical research: the electrostatic generator, the Leyden jar, and so forth. In each case, however, he had introduced design innovations that he considered to be significant improvements, and most of the pieces had been given new names. Whether he exaggerated the importance of his innovations or not, his apparatus was certainly adequate, by contemporary standards, for the research purposes to which it was applied.

Marat's Philosophy of Science

Marat ended his Preliminary Discourse with perhaps the fullest statement of his philosophy of science, which strongly reflected the Newtonian spirit of *hypotheses non fingo*. In contrast to the "multiplicity of systems" he had encountered in each branch of physics, he declared that "all vain speculation will be banned from this work." Everything therein was to based only on "certain knowledge";

> not a single hypothesis, not a single intuitive leap; the theory always marches in step with the experiments, and everything here is rigorously deduced from consistent facts, from which I allow myself to extract only the immediate consequences.[25]

Marat not only professed the most thorough-going empiricism, but an explicitly analytical and reductionist method as well:

> It is not enough that each assertion be deduced from constant facts; in order to be worthwhile it is necessary that the facts be simple. I have therefore taken care to analyze those that are complex and to reduce them to their elements.[26]

Marat made it clear that he did not see his role as that of a popularizer of science: "It has been my desire to write only for the initiated."[27] A certain amount of prior knowledge of electricity would be necessary for a reader to fully appreciate this work. There is nothing in Marat's approach that could have served as a model for the notion of "sans-culotte science" that appeared in the wake of the Revolution.[28]

The first step taken in the experimental narrative was to make a clear distinction between the electric fluid and other subtle fluids, especially the magnetic fluid. Marat's rather contemptuous attitude toward rival theorists began to be expressed more openly:

> To distinguish different things that seem similar ought to be the primary concern of those who scrutinize nature, but the first operation of our modern philosophers has been to mix them all up.[29]

Marat harshly criticized a "modern academician" for confusing electric fluid with fire and identified him in a footnote as J. B. Le Roy, whom, it will be recalled, headed

the commission that had given short shrift to Marat's optical experiments.

Some physicists, Marat declared, had considered the electric fluid to be identical to the true principle of fire, and had likewise lumped electricity and magnetism together, leading to the bizarre conclusion that fire and magnetism share the same underlying cause. But "magnetism and fire have not a single phenomenon in common."[30]

As for electricity and magnetism, the only thing they have in common is a real attractive force and an apparent repulsive force. Their modes of action are entirely different. Touch a magnet; does one feel even the slightest tingle of a shock? "While magnetism indubitably depends upon a fluid, that fluid is certainly not homogeneous with the electric fluid."[31]

Marat had been able to render the electric fluid visible with his darkroom methods, but had not been able to do the same for the magnetic fluid. He did not believe it impossible in principle, however, and saw no reason to suppose that he would not be able to successfully do so in the future. Meanwhile, his attention was directed toward electricity, not magnetism.

Among a long list of differences in the modes of action of electricity and magnetism, an important one is that the most characteristic sign of magnetism is not exhibited by electrified objects: an electrified needle does not turn to align with the earth's poles or with

anything else. Marat concluded that "the electric fluid and the magnetic fluid therefore differ essentially, even though electricity can sometimes excite magnetism."[32]

As noted earlier, Marat cannot be counted among those natural philosophers who formed the advance guard of the *Naturphilosophie* school. He was not seeking the underlying unity of diverse phenomena; his view of the relationship between electricity and magnetism was every bit as orthodox as Coulomb's.

Ocular demonstrations in the darkroom were designed to illustrate the differences between light and electricity:

> The electric fluid does not manifest itself until it is put into action; then it appears in the dark in various luminous forms. It is not luminous itself, because it only appears with the help of the light that stimulates it.[33]

Explaining the Appearance of Repulsion

As was seen earlier, Franklin shared with Marat the metaphysical stance recognizing attraction but not repulsion as a fundamental force of nature. Nonetheless, Franklin, together with most other physicists, conceived of the electric fluid as made up of particles that repel each other. Marat was prepared to do battle against that opinion.

Franklin and others had found that the pointed extremities of objects displayed special electrical properties; surplus electric fluid seemed to be denser in

pointed parts of objects than in flat or rounded areas. "Who could not see," Marat asked rhetorically,

> that if the electric globules mutually repelled each other, their repulsion would be most extreme at the tip of the point where they are instead found most condensed.[34]

Marat then turned to the direct examination of electrical phenomena involving apparent repulsion. He replicated a standard experiment utilizing two light-weight cork balls suspended by conducting wires, hanging very close to each other but not touching. When the terminals of the wires were both brought into contact with the same terminal of a Leyden jar, the cork balls were electrified and moved apart from each other. Was this not a clear-cut case of repulsion?

Marat observed, however, that the apparent repulsion ceases before the Leyden jar is completely discharged; the balls return to their original positions. Furthermore, he noticed, even after the balls had ceased to repel each other, they still contained enough electricity to make them jump toward his finger when he brought it close.

Marat substituted small spheres made of thin glass and gold leaf for the cork balls and obtained the same results. He concluded:

> If the repulsion of the electrified objects had come from a force essential to the electric globules, the cessation of the repulsion would be impossible as long as the Leyden jar continued to be charged, because a constant cause necessarily has a constant effect. It is therefore true that the electric globules do not repel each other.[35]

Marat utilized an air pump to repeat this experiment in a vacuum. The absence of air, he found, diminished the apparent repulsion. Since the balls moved apart less as the vacuum was stronger, he concluded that "their apparent repulsion therefore is caused by the ambient air," which attracts the balls outward.[36] Cranking the wheel of an electrostatic generator to supply the charge, Marat found that

> if the vacuum is at a certain degree, no matter how vigorously you turn the wheel, the balls will not move apart at all for fifteen to twenty seconds.[37]

Why would they move apart at all in a vacuum if the apparent repulsion is normally caused by the ambient air? Marat concluded that the walls of the air pump's vacuum chamber become charged and attract the balls outward, making it appear that they are repelling each other. This, it seemed to Marat, sufficiently explained the phenomenon, but he offered further arguments to support his denial of repulsion, including this observation: "The mutual repulsion of the electric globules is incompatible with the celerity of their movement in very small wires."[38]

The Pivoting Needle

The investigation of attraction and repulsion led Marat to try some experiments that produced some surprises:

> It is necessary that I deal with some unknown phenomena here that not only seem to invalidate my theory but also to overturn all received notions.[39]

In experiment number 21 he balanced a needle on a pivot. Two inches from the needle, at the same level and parallel to it, he placed a conductor attached to an electrostatic generator. By turning the wheel of the generator, electricity could be made to flow into the conductor. Marat reported his findings:

> When the wheel begins to turn with force the needle remains immobile, but when the needle is not parallel, as soon as the wheel begins to turn, the needle presents a point to the conductor. Then if you approach that point from the side with the tip of your finger, a brass wire, a glass tube, or a wax stick, it will move away. If you approach the other end of the needle with any of these objects, the needle tip will come toward it.[40]

Marat was clearly taken aback by these results. "If I were one who loved marvels," he said,

> I would stop here to allow free rein to the imagination of my readers, for what phenomena of nature have ever offered a more curiosity-arousing contrast than that which I have just described in detail?[41]

Marat would not stop there, of course; he would attempt to explain what had occurred. The circle through which the needle turned seemed to have been "halfway within the sphere of activity of a repulsive force and halfway within the sphere of an attractive force."[42]

Strangest of all, the repulsion appeared to occur between objects that according to all previous experience should have attracted each other: "The needle is electrified positively and the bodies that are presented to it are electrified in a negative manner."[43]

In this case it seemed to him that it could not be the approaching object's electricity that caused the needle tip to move away; this reinforced Marat's conviction that the appearance of repulsion is purely illusory.

Why, then, did the needle move when the other objects approached it? Marat attributed it to the simple presence of the approaching object regardless of its state of electrification. The point of the needle had originally been motionless because it had been surrounded by a homogeneously attracting ambient milieu. When the object approached, it displaced that milieu on one side; the needle tip then swung toward the other side where the attracting milieu remained undiminished.

The conductor Marat was utilizing was most likely not a wire carrying a current along toward some repository of charge (such as a Leyden jar) but rather a prime conductor; that is, a piece of metal that could receive the electrostatic charge generated by turning the wheel.[44] It would seem that the needle's action could be easily explained, then, by a separation of charges on the needle induced by the conductor's charge. If so, it is difficult to understand Marat's surprise, because electrostatic induction was far from being an "unknown phenomenon" in the 1780s, and he certainly was familiar with it. Although his description of the experiment appears detailed and clear, there is not enough information to allow a definite

determination of what caused the needle to behave as it did.

Marat, however, was looking for an anomaly that would raise doubts about electrical repulsion, and he believed he had found one. His curiosity and experimental imagination led him in potentially fruitful directions. His theoretical imagination, however, limited the value of his investigations by allowing him to satisfy himself with explanations of phenomena that he might otherwise have pursued further.

If such blame is to be assessed, however, some of it should be shared by those who ignored Marat's work altogether. Once again, the extension of knowledge would have been better served had Marat's experiments been replicated and his theoretical conclusions challenged rather than disregarded by the scientific elite.

Although Marat's desire to completely eliminate the concept of repulsion from electricity may appear to be an extreme theoretical position, it was not unprecedented. In fact, it is likely that the source of Marat's idea was a published letter of Ebeneezer Kinnersley, although he does not say so.

Kinnersley had challenged the "doctrine of repulsion in electrised bodies" in a letter that was included in the fourth (1769) edition of Franklin's book along with a response from Franklin. In the letter, Kinnersley suggested that all of the electrical phenomena that seem to exhibit

repulsion "may be well enough accounted for without it."[45] In particular, he thought the separation of charged cork balls in the classical experiment might be explained by the balls being attracted outward by a natural quantity of electricity in the surrounding air. In his reply, Franklin expressed an appreciation of Kinnersley's position; he, too, he said, had once hoped to be able "to resolve all into attraction." There were some cases of apparent repulsion, however, that he was unable to "so easily explain by attraction."[46] Franklin, then, remained skeptical with regard to Kinnersley's position, which was apparently identical to Marat's.

Marat then turned to another disputed point. "From the fact that the electric fluid always tends to regain abandoned space," all those who have written on the subject "have concluded that it is an elastic fluid."[47] Marat disagreed, and performed some experiments with barometers to demonstrate that the electric fluid does not add to the elasticity of the air.

> If it is proven that the electric fluid does not add to the elasticity of the air, then it is proven that it is not elastic itself. And how could it be? In fluids, elasticity consists in a tendency to rarify, but the electric globules tend naturally to condense, since they possess a reciprocal attractive force.[48]

It is of interest that Marat did not reject the concept of elasticity *per se*; he implicitly accepted the notion that air particles repel each other. Although he does not say so directly, it is possible that he would have

agreed with Franklin's position that the repulsive forces ultimately derive from more deeply hidden forces yet to be discovered.

In Section Two (of the six that make up the book) Marat announced:

> We now come to the most important article of this work, and since the new facts that I have developed are diametrically opposed to the received wisdom on the subject, I beg the readers to redouble their attention.[49]

He began by defining more precisely the new terminology that he previously introduced. He rejected the old distinction between conductors and nonconductors on the grounds that

> all materials propagate the electric fluid, except for air, light, igneous fluid, and magnetic fluid, substances upon which I have not yet rendered judgment.[50]

"I have proven by direct experiment," he stated, "that glass, wax, resins, etc., conduct electric fluid just as do metals, but with this qualification: that they do so little by little rather than all at once."[51] This suggests, then, that a more useful distinction is between those materials that can transmit shocks (deferents) and those that cannot (indeferents).

> From whence arises this difference among materials? From their tissues being more or less suitable for refusing (or, alternatively, allowing) passage to a certain quantity of electric fluid. The incontestible proof of this truth is that at equal dimensions a given deferent material does not cease to be so in combination with a given indeferent material, such as vinegar with litharge.[52]

Combining more than two substances, however, leads to a more complicated situation that is less predictable.

Explaining the Leyden Jar

The use of the Leyden jar, Marat said, "is very restricted; it consists of giving shocks."[53] Nonetheless, "the marvels revealed by this apparatus have heretofore been poorly explained."[54]

> If the jar is suspended in a room by a silk cord and if the air is dry, it cannot be charged, no matter how many times the wheel (of the electric generator) is turned. From this particular fact a general principle has been derived: "It is impossible to charge an isolated Leyden jar."[55]

Marat cited an experiment reported in the *Journal de Physique*, 1775, of a M. de Lor.

To the contrary, Marat declared, an isolated jar could indeed be charged if only a needle a few inches long is attached to its external lining. This is a claim for which the Abbé Bertholon heaped ridicule on Marat.[56] It was not Bertholon, however, who Marat was challenging with his novel interpretation of the Leyden jar. His aim was higher; the target was Franklin:

> The educated reader will already have perceived that here I am combatting the principles of a great Master. It is not without regret that I find myself brought to this kind of discussion, but it goes to the heart of my subject. Moreover, I have too high an opinion of that wise man, a few of whose ideas I am challenging, to believe that he would object to not being followed blindly.[57]

Apparently still smarting from the reception his attempt to revise Newton had met with, Marat denied that he had "set

out to attack great men" as his numerous adversaries claimed.[58]

It is not true that Marat was uniformly hostile toward famous scientists, either predecessors or contemporaries. His challenges were to specific ideas of Newton, Franklin, and others; like most other authors, he paid homage to their general contributions to science. There is no reason to suspect that his frequent praise of "the great Franklin," for example, is anything other than sincere. His approval of Becarria and his work is evident, and although he criticized certain notions of Wilke and Aepinus, he acknowledged their status as "two celebrated physicists."[59] His strongest polemics -- and even here it would be an exaggeration to describe them as vilification -- were directed toward the Abbé Bertholon, as will be seen below. Marat's scientific writings reveal that he could be testy on occasion, but there is really very little hint of the fire-breathing radical journalist that he was to become during the Revolution.

Continuing his analysis of the Leyden jar, Marat reported:

> Finally, a modern author has shown that the water from a jar that has just been charged, if transferred to an empty bottle that is externally lined and not electrified, can give a shock. If the celebrated Franklin has found the opposite, it was because the water was too weakly electrified, or perhaps that the electric fluid it contained dissipated in the process of being transferred to the other bottle.[60]

The "modern author" was identified as Nollet, so although Marat was generally supportive of Franklin against Nollet in theoretical matters, in this particular case he found himself in agreement with Nollet against Franklin.

The question of whether the water in a Leyden jar was a repository of electrical charge was important to interpreting how the apparatus functioned. It is evident that either Marat and Nollet, on the one hand, or Franklin, on the other, must have been in error on this critical issue. With benefit of hindsight it is simple enough to know who was right and who was wrong in this matter. The point, however, is that at the time Marat was writing, even direct experimentation was unable to decisively resolve the disputed issue. The nature of electrical experimentation was such that even careful experimenters could obtain conflicting results.

Distinguishing Between Positive and Negative Charges

The existence of electric effects was recognized for milennia before Du Fay's 1733 distinction between vitreous and resinous electricity, later designated "positive" and "negative" by Franklin. The long delay in detecting the two different kinds of charge was due to the great similarity in their behavior. They could only be distinctly differentiated by the fact that what one attracted the other repelled, and vice versa. Given a single charged object, however, how could an experimenter determine with

certainty whether its charge was positive or negative? Or, given two objects of unknown charge that attract each other, how could it be determined which was positive and which negative? Marat listed a number of methods that had been suggested by various physicists:

> The signs by which they have been accustomed to recognize positive electrification are: the attraction of freely movable light objects from a reasonable distance, the impression that the electric current makes on the sense of touch, the angles of inclination of the jets omitted by electrified bodies, and the combustion of volatile spirits.[61]

These signs, Marat argued, are far from decisive. Attraction is ruled out because it is reciprocal and no final distinction can be made between what is attracting and what is attracted. As for the second point, a shock feels the same whether the current flows to or from the person receiving it. On the third and fourth points:

> The inclination of the jets that electrified bodies emit escape the sharpest eye, and finally, the combustion of volatile spirits takes place whether one directs a spark toward them or draws a spark from them. It is therefore manifest that these signs are equivocal.[62]

Marat noted in passing that the uncertainty of these signs was what led to "the absurd system of affluent and effluent matters," a strong blast against Nollet's Cartesian theory of electricity.

Another modern academician, Marat said, had attempted to put forward what he thought to be less equivocal signs. In a 1755 memoir Le Roy had suggested that the shape of the discharges as seen in a darkroom is different for positive

and negative electricity; a positive discharge produces a "plume" while a negative one is evidenced by a "luminous point." Marat did not dismiss Le Roy's method out of hand but he did not consider it to be completely trustworthy.

> Up to now physicists have not found definitive signs of the two kinds of electrification. Is it necessary, then, to infer that none exist? Not at all; there are infallible ones, as we shall see.[63]

Marat's suggested method was based upon acceptance of Franklin's single-fluid theory:

> To distinguish immediately between an object positively electrified and one negatively electrified, it suffices to perceive the direction of the fluid that is transported from the one to the other. But since the fluid has infinitely too much speed to be followed by the eye in such a limited interval, it is necessary to judge its direction by that of other corpuscles that are caught up in its motion. Nothing could be easier: by means of our method of observation in the darkroom, the electric fluid that emanates from a positively electrified object (equipped with a point) can always be seen to chase before it the igneous emanations of an incandescent object.[64]

A negatively charged object, by contrast, does not agitate the igneous fluid. All of this was plainly visible to Marat, of course, in the images projected onto the screen by his solar microscope.

Van Marum's Ocular Demonstration

These endeavors of Marat can be better appreciated by comparing them with experiments performed a few years later by Martin Van Marum, working at the Teylerian Museum in Leyden with the most powerful electrostatic generator then in existence.[65] In 1784 he put his huge machine to use to produce an ocular demonstration in support of Franklin's

single-fluid theory. The spark discharges produced by his machine gave the appearance of miniature bolts of lightning with multiple forked endings. The forks could be clearly seen all pointing away from the positive terminal and toward the negative one, indicating the direction of electric flow that would be predicted by Franklin's theory.[66]

In 1785 Van Marum went to Paris and described his experiments to leading members of the Academy of Sciences, including Lavoisier, Le Roy, Berthollet, and Monge. "They all agreed with me," Van Marum later reported,

> that, given the direction of the electrical discharge produced by the large machine, which had become so visible, it had been incontestably proven that, in conformity with Franklin's theory, a single electrical fluid exits from the positive conductor and passes into the next conductor, which receives it.[67]

Le Roy then took Van Marum to meet Franklin. According to Van Marum's account of the meeting, Franklin declared:

> This proves my theory of a single electrical fluid, and now, at last, the theory of two kinds of electrical fluids will have to be rejected.[68]

Volta was particularly enthusiastic over Van Marum's accomplishment:

> All orthodox electricians owe you a great debt of gratitude, sir, for having dealt the deathblow to the heresy of the "dualists"; *i.e.*, the new followers of Du Fay, Nollet, and Symmer.[69]

Marat's ocular demonstration was not as direct as Van Marum's since it was dependent upon the visible motion of igneous fluid. As was seen earlier, however, Marat's claim to have rendered the igneous fluid visible was

accepted by a number of academicians, and none explicitly disputed it. If that premise were to be assumed valid, his ocular demonstration of the flow of electric fluid should have been as convincing as Van Marum's. It also had the advantages of having been announced five years earlier, and of not requiring apparatus that only well endowed institutions could afford.

The fate of Marat's demonstration, however, was quite the opposite of Van Marum's. If any other prominent physicists had taken note of it, they did not reveal that they had. No commission of the Academy reviewed Marat's electrical experiments as they had his earlier investigations of fire and light; Le Roy had apparently been alienated from Marat during the controversy over Newton's theory of colors. Condorcet had drawn from "the Marat incident" the lesson that Marat was to be given the cold shoulder.[70] Volta had also expressed antagonism toward Marat.[71]

It would seem, then, that the difference between the receptions accorded Van Marum and Marat was not based on the scientific merit of their respective ocular demonstrations but on other, extrascientific grounds. In the context of contemporary electrical science, Marat's work seems to have been as deserving of serious attention as Van Marum's.

Two Kinds of Electricity

The dispute over whether there are two distinct kinds of electric fluid or only one had not been resolved by either Marat or Van Marum, of course. Marat acknowledged that "the question remains undecided in spite of the efforts of physicists" but added:

> It is important to resolve it by facts that leave no doubt. I believe that the solution will not be difficult. Let us examine these so-called electricities with care, demonstrate their falsity, and uproot once and for all these thorns that cover the field of science.[72]

Marat turned, then, to a discussion of "the system of so-called vitreous and resinous electricities."[73] Vitreous, he reminds us, is positive; resinous is negative.

Almost all eighteenth-century electrical experimentation utilized a spinning glass globe as generator. As a result, far more positive electricity was produced in the laboratory than negative. The properties of positive charge were therefore much more familiar to physicists than were those of negative charge. This most likely was a result of the fact that most "resinous" materials are far less durable than glass; continuous spinning and rubbing would cause them to crumble.

Whatever the cause, it was a fact that favored the survival of Franklin's theory for which the most troubling anomalies were phenomena deriving from negative electrification. Marat, to his credit, recognized the asymmetry of experimental practice and proposed to rectify it:

> All physicists being in agreement on the question of the electrification of glass, it is to the electrification of resin that we must confine ourselves.[74]

Although Marat acknowledged the conventional use of the term to denote negative electrification, he did not believe that substituting a resinous globe for the standard glass globe would really produce a negative charge.

> Among several facts that demonstrate the contrary, let us limit ourselves to these. A conductor successively electrified by a glass globe and by a sulphur globe presents some phenomena that are more or less sharply defined, but absolutely identical.[75]

His list of these identical phenomena culminates with their appearances in darkroom observations. In all cases, he concluded, the objects were *positively* electrified: "It is beyond doubt that glass and sulphur electrify the bodies they are in contact with in the same manner."[76] The hypothesis of two electricities is thus rejected as "false and absurd."[77]

Volta and the Electrophorus

Marat utilized this insight in his analysis of the electrophorus, a "modern instrument invented by M. Wilcke and perfected by M. Wolta."[78] The electrophorus, as described by Marat, consisted of two flat plates, a resinous *plan* and a glass *plateau*, that are very close to each other but not touching. This instrument, he said, is capable of storing a charge for several months in the manner of a Leyden jar. The standard interpretation of a charged electrophorus considered the *plan* and the *plateau*

to be oppositely charged, one positive and the other negative, but Marat doubted that this was true.

The experimental source of the traditional interpretation was as follows: A charged electrophorus is used to charge two Leyden jars, one from the *plan* and one from the *plateau*. Then, when the terminals of the two Leyden jars are brought together, the jars violently discharge, strongly suggesting that they had borne opposite charges. Marat, however, pointed out that two jars bearing vastly unequal amounts of the same kind of charge would also violently discharge. This is what he believed happens when jars are charged from both sides of the electrophorus: "The Leyden jar electrified by the *plateau* is much more charged."[79]

There is more to the electrophorus than meets the eye, however. Marat does not, in the final analysis, deny the presence of opposing charges, but locates them in the bases that support the *plan* and *plateau*.

> It seems beyond doubt that, due to having confused the resinous *plan* with its metallic base, even those who perfected the instrument had not understood how it really worked.[80]

According to Marat's earlier acknowledgment, foremost among "those who perfected the instrument" was Volta. Marat's reinterpretation of the electrophorus reveals a wide chasm of theoretical incompatibility between himself and Volta that could easily account for any ill will that they felt toward each other.

Although Brissot's *Memoirs* indicated that Marat acted extremely rudely toward Volta when Volta visited his laboratory, Marat's references to him in *Recherches physiques sur le électricité* are generally respectful. While there is none of the high praise accorded Franklin, neither is there the kind of angry polemics aimed at Bertholon.

The Resistance of Air

Franklin had attributed the containment of a charged object's electrical atmosphere to the pressure of the surrounding air. While Marat's theory denied the existence of electrical atmospheres, the ambient air still had a significant function, "opposing the electric fluid with a resistance proportionate to its density."[81]

> The prodigious resistance that the air opposes to the electric fluid can be judged by the smallness of the jet that is emitted from a strong, completely charged battery.[82]

The air's resistance is measurable and calculable in principle, according to Marat, but the necessary labor of quantification had not yet been performed. It should be noted that the battery he referred to consisted of several Leyden jars connected in series; the invention of the Voltaic pile was more than a decade in the future.

The Power of Points

Marat attibuted the remarkable electrical characteristics of the extremities of pointed objects to

their relative ability to counter the air's resistance. Points have an exceptional ability to draw the electric fluid and an apparent ability to expel it as well. That is why a Leyden jar's terminal must end in a knob; if it ended in a point the jar would rapidly lose its charge.

Marat demonstrated the extraordinary power of points by showing, in the darkroom, that a needle glows in the presence of a highly charged object to a distance of up to six feet. Making the needle's point less sharp decreases the minimum distance necessary to produce the glow. The electrical properties of a point result from the fact that its form "is more suited to diminish the resistance of the milieu that must be traversed" by the electric fluid; *i.e.*, the ambient air.[83]

Another of Marat's experimental strategies included a quantifiable variable. The time it takes a Leyden jar fitted with a pointed terminal to discharge could be measured by having a little ball of wax on the tip of the point while the jar is being charged. Then the ball of wax could be blown off with a puff of air from a glass tube to initiate the time period. Marat demonstrated this technique, but left it to others to utilize it systematically in order to develop useful quantitative data.

The Speed of Electricity

Marat took issue with the generally accepted idea, attributed to Franklin, that electricity travels at the speed of light. He also disagreed with an unnamed "modern academician" who determined in 1746 that electricity traversed 4,000 *toises* in an imperceptible time and concluded that its transmission was instantaneous.[84]

Electricity, Marat stated, behaves like fluid in a pipe. If pressure is exerted at one end of the pipe, one particle of fluid bumps the next one, and so on all the way to the other end where a particle is simultaneously forced out. If the transmission seems instantaneous, it is not due to the actual transmission of any fluid from the beginning to the end of the pipe.

The only valid way to judge the speed of electricity, then, is by trying to observe it in motion through a free medium. Marat suggested what he admitted was an inexact method: estimating the speed of lightning jumping between clouds in a storm.

> Among a thousand observations that I have made on this subject . . . I will limit myself to this one. On July 22, 1780, a violent storm presented me with the best possible opportunity.[85]

Observing a flash of lightning leaping between two distant clouds, he made the following estimates and calculations. The elapsed time was brief but perceptible; it was estimated to be 20 *tierces*, or a third of a second. The angular separation of the clouds was measured with a quadrant and found to be 30 degrees. The clouds' distance

from him was determined by measuring the elapsed time between lightning and thunder; he accepted the Academy of Sciences' value of the speed of sound as 173 *toises* per second. By trigonometric calculation, then, he could estimate the distance between the two clouds and from that the speed of electricity, which he determined to be 19,200 *toises* (37,420,800 meters) per second.

This figure, he cautioned, could not be interpreted as a constant rate of speed. The velocity of electric fluid cannot be constant because it must always be accelerating when in motion, for the same reason that objects accelerate when they fall to earth. It is a consequence of the inverse square law of attraction.

Marat's application of the inverse square law to electric force provides the key to understanding his critique of the electric atmospheres postulated by Franklin and others, a term that Marat believed "should be proscribed from science."[86] Instead of explaining electrostatic induction by means of a cloud of electric fluid particles surrounding a charged object, Marat utilized a theoretical construct that he denoted as a charged object's "sphere of activity." Although at first glance the idea of an electrical sphere of activity may seem similar to that of an electrical atmosphere, the two concepts were not at all identical.

Marat's treatment of the sphere of activity implied a complete acceptance of action at a distance. It depended

upon a force of attraction no different in principle from gravity.

> The sphere of activity of the electric fluid always extends to a certain distance from the objects in which it is found; often even around objects that it cannot penetrate.[87]

The distance to which the sphere of activity extends "is always in ratio to its mass"; *i.e.*, the mass of the electric fluid itself. The energy of the fluid's attractive force

> is always deployed in direct ratio of the quantity of accumulated fluid, and always in inverse ratio of the square of the distance -- the law common to all bodies that attract each other.[88]

Coulomb's Law

A modern reader cannot help but notice that this seems to be a straightforward rendering of Coulomb's law, if quantity of electric fluid can be equated with magnitude of charge. Coulomb, however, only formulated his law in 1785, several years after Marat's book was published.

In fact, an inverse square relationship of electrical attraction analogous to the Newtonian gravitational formula had been suspected by many physicists well before Coulomb substantiated it. The hypothesis did not originate with Coulomb; his achievement was to demonstrate its validity by rigorous experimental means.

Priestley, for example, had explicitly stated the inverse square law of electrical force in his 1767 book, which Marat knew well.[89] The basis of Priestley's

conclusion was complex. First of all, Newton, in his *Principia*, had mathematically demonstrated that the inverse square relationship implied that the force at any point inside a uniform sphere would be zero. Secondly, it had been experimentally shown that the force of electrical attraction at any point within a uniformly charged cylinder was zero. In spite of several logical difficulties, not least of which was extrapolating from spheres to cylinders, Priestley's intuition led him to conclude that electrical attraction followed the general pattern of gravitational attraction.

As elegant as Priestley's reasoning was, it did not establish the inverse square law of electrical force on a solid enough basis to satisfy skeptics. It remained to be demonstrated experimentally.

Perhaps Marat's scientific abilities can most fairly be measured by comparing his approach to this problem with that of his more successful contemporary, Coulomb. Here is Marat's proposal:

> In order to get an idea of this law, a little pith-ball can be used, suspended from the ceiling by a long flax thread, which can be approached at various distances by the knob of the terminal of the Leyden jar, charged to various levels. The more the jar is charged, the more the little ball will be promptly attracted, and the greater the distance over which it is attracted. This same method would suffice to demonstrate this law rigorously, if it were possible to determine in each trial the exact amount of fluid accumulated on the knob of the jar (or rather, on its internal surface).[90]

Marat, then, suggested a method for a quantitative determination, but acknowledged its incompleteness by doubting the possibility of measuring the key independent variable (charge, or amount of electric fluid). Furthermore, even if the independent variable could have been measured precisely, Marat's method could not have determined the quantitative value of the dependent variable (force of attraction) with accuracy.

Coulomb, by contrast, was able to "demonstrate this law rigorously," not only in principle but in fact. His new apparatus, the torsion balance, provided a supersensitive scale for the measurement of forces that allowed him to give a relatively precise quantitative demonstration of the inverse square law of electrical force.

In comparison with Coulomb's torsion balance, Marat's suggestion for a means of measuring the force of attraction between charged objects seems astonishingly crude:

> The force of their attraction can only be estimated by the size of the spark that they throw. The difficulty, or rather the impossibility, of determining with precision the size of each spark therefore renders all the results uncertain.[91]

But Marat was not as unsophisticated as this notion of spark measurement makes him seem. If anything, his view of the problem was oversophisticated: he considered it necessary to distinguish between *forces acting between charged objects* and *forces exerted by charges* (or electric fluid) *per se*. If spark sizes could be gauged, that would give a direct measure of electrical force, but since that

is impossible an indirect method would have to suffice. The indirect measure he suggested was the determination of the amount of mechanical force that charged objects exert on each other, which was the method Coulomb was to utilize. Whereas Marat believed this could only be done with "embarrassingly imprecise" results, Coulomb achieved a degree of precision with his torsion balance that was high enough to satisfy most of his contemporaries.

Such a comparison runs the risk of imposing twentieth-century values on eighteenth-century events. It is worth noting, however, that the two physicists were thinking about the same problem at almost the same time, that Marat concluded that it could not be solved in practice, and that Coulomb did devise a convincing solution.

Affinities

The reason Marat would not simply assume the equivalence of attraction between charges *per se* and attraction between charged objects was because another variable had to be taken into account; namely, the relative affinities of electric fluid for various materials. Aside from ordinary solids, Marat was concerned with the affinities of electric fluid for pure air and for the fluids of light, heat, sound, and magnetism.

Electric fluid, he found, has a very detectable affinity for the fluid of light; they always attract each other. Luminous fluid decomposes at the edges of electrical

jets formed by discharges from Leyden jars, for example. This can be seen with the solar microscope when the shadows of the jets are projected onto the screen. The affinity is greater for yellow rays than for red ones, and still greater than for blue ones.

Electric fluid has no affinity, however, for igneous fluid or for pure air. Blowing pure air through a glass tube at electric jets produced no deviations in the jets. As for igneous fluid, this was one of the very few of Marat's experimental accounts that was not described fully enough to allow replication. It required the use of a new instrument that he had devised, the "refrigerant," to bring electric and igneous jets together: "Someday I will give a description of it."[92]

There was more to be said of the relationship between the igneous and electric fluids:

> Although igneous fluid and electrical fluid do not attract each other, igneous fluid in motion can augment the attractive power of electrical fluid. It does so by expanding the pores of the objects they are both in and by rarifying the ambient air, thereby lowering resistance to the passage of electrical fluid.[93]

Since electrical discharges have been shown to set things on fire, Marat said, some investigators had wrongly jumped to the conclusion that the electrical and igneous fluids are the same thing. This, however, was a case of reasoning from identical effects and confusing separate causes. The electric fluid never burns except when it is found together with the igneous fluid. Furthermore, no

matter how much electric fluid a body may contain, it never makes the slightest impression of heat to the sense of touch.

Noting that alcohol ignites when a spark is drawn from it, Marat explains that

> the ignition is certainly produced only by the internal motion of the igneous fluid that was stimulated by the electrical fluid's impetuous escape.[94]

All of these facts revealed that "electric fluid is therefore the agent, not the principle, of fire."[95]

The Uses of Electricity

The last quarter of Marat's book on electricity turns from theoretical to practical matters. What are "the uses to which the electric fluid is destined to be put"?[96]

> It seems that electricity, so full of marvels, has finally exhausted the admiration that it had at first excited. It is still being cultivated by a large number of devotees, but the public, less attuned than in the old days to the singularity of the phenomena it presents, today complains that for all of the discoveries made in this branch of science, almost nothing has been produced for the good of society.[97]

There is just the faintest touch of a Rousseauist criticism of science here, and even that is presented indirectly, as coming from the public.

Marat discussed the applications of electricity to chemistry and seemed to conclude that there is not much to be hoped for in that regard, based on what had been accomplished up to that point. He added a caveat, however:

> Let us not jump to a conclusion too hastily. To find out what promise this field holds, let us wait until

> some enlightened mind has made it an object of research.[98]

As for medical applications of electricity, Marat believed the potential was great, but the surface had hardly been scratched with regard to specific knowledge. He had utilized therapeutic electricity in his medical practice, and it would form the subject of one of his prize-winning academic essays. It would also be the primary field of dispute in his bitter rivalry with Father Bertholon.

A number of investigators had tested the effects of electric fluid on vegetation; the work of Jallabert, Nollet, and others had "proven that electric fluid favors vegetation." Marat intended to go farther, "to prove that it is the *principle* of vegetation, conjointly with heat."[99]

> No one that I know, except M. de la Cépède, has yet advanced a similar assertion. But since the electric fluid is for that author *a composite of water and heat*, and even *an integral part of plant life*, it can readily be seen that our assertions have nothing in common but the similarity of the sounds that serve to express them.[100]

Marat's ideas about this subject were apparently influenced by experiments showing that electricity could stimulate capillary action.

> Congealed earth cannot produce anything; saps that have become solid cease to circulate. But when heat renders them liquid, it is the electric fluid that opens their passage in the channels of plants. It is that which begins and accomplishes the work of germination. [101]

"Such assertions should be backed up by unequivocal facts," he declared. "Now it so happens that there are

decisive facts."[102] As evidence, Marat reports a botanical experiment that he performed in December 1780. It was a controlled experiment utilizing two terrariums, each of which contained three pots. Seeds were planted in all six pots; all of the seeds had been generated from the same plant source. The temperature was kept constant at slightly above freezing throughout the course of the experiment. One of the terrariums was electrified by keeping it charged like a Leyden jar for 17 of every 24 hours over a fifteen-day period. The control terrarium was not electrified.

Vegetation appeared in the electrified terrarium after seven days; by the fifteenth day there were little plants comparable to those grown in a considerably warmer environment. Meanwhile, no vegetation at all appeared in the control terrarium. "It is manifest that the electric fluid is one of the principal agents of the fertility of the earth."[103]

Marat's discussion of the general utility of electrical science ended on a hopeful, if ambiguous, note. He insisted that it would certainly become useful, and cited the lightning rod and medical applications as harbingers of its promise, but no other specific uses were suggested.

The Lightning Rod

The sixth and final section of *Recherches physiques sur le électricité* -- nearly a hundred pages long -- was

devoted entirely to consideration of "lightning and the means of defense against its blows."[104] The electric kite experiments meant there could no longer be doubts as to the analogy between lightning and electricity: "From the identity of effects one can infer the identity of causes."[105]

On the subject of methods of protection against lightning, Marat began with high praise for Franklin's lightning rod. After describing the device and the theory behind it, he commented: "If I may be permitted to say so, the consequences may be true, but the premises do not seem to me to have been demonstrated."[106] Marat, in other words, believed the lightning rod to have been a practical success that could have been even more useful had the underlying phenomena been better understood.

> Without doubt, conductors raised above an edifice are an excellent means of preserving them. But is it really true that they peacefully extract from storm clouds the fluid of which they are formed?[107]

Franklin designed his device not to attract bolts of lightning in order to channel them harmlessly to earth, but to attract a slow, silent leakage of electric fluid from the sky to the ground. This notion was based on his laboratory observations of the power of points to draw off electrical charge from considerable distances. Marat, however, considered it an unwarranted inductive leap to extrapolate from the laboratory to the great outdoors. It could not be assumed that the same phenomena that operate

at distances of a few feet continue to operate all the way up to the clouds.

Despite the lightning rod's proven utility, he said, its record was far from spotless.

> How many examples there are of buildings struck by lightning in spite of being armed with conductors! And how many examples of conductors themselves struck![108]

Marat cited eleven specific case histories, stating the places and dates they occurred, and concluded: "It is well demonstrated that the sphere of attraction of conductors does not extend to the clouds."[109]

Nonetheless, Marat considered the electrical grounding of high objects to be a wise policy. "These devices are often useful; that is incontestable. But are there cases when they may be dangerous? Now, that is a good question!"[110] It is possible, he warned, that lightning rods sometimes expose rather than protect a place.

As a prelude to his own general proposal for protection against lightning, Marat discussed two controversial issues. First, while it is not true that lightning rods "expose neighboring houses by preserving the one to which it is attached," as some had argued, a single lightning rod could not be expected to protect several neighboring houses.[111]

The second issue, one that had been hotly debated for years, was over whether the ends of lightning rods should be pointed or blunt. Franklin's original design called for points, and he and his partisans vigorously defended that

choice. Benjamin Wilson had made a name for himself by arguing in favor of blunt-tip or knobbed rods. Marat, citing the *Journal de Physique*'s summary of the literature on this debate as of 1779, concluded that "the results have been in favor of points."[112] Most of the previous studies, however, had been based on the idea that the conductors could silently siphon electric fluid from the clouds, a premise Marat did not accept. His decision in favor of points was based on the belief that their sphere of activity is less limited.

Taking all of his theoretical conclusions into consideration, Marat suggested some design improvements for individual lightning rods as well as a comprehensive method for the protection of entire towns. Lightning rod efficacy could be optimized, he said, by constructing them with multiple points aimed in all directions, and by equipping them with very thick ground wires sunk deep into the earth, to the depth of underground water.

A town's protection could be maximized by placing rods on the houses at both ends of every street, however long any given street may be, and connecting all of their ground wires into a single grid. The rods should ideally rise to at least 40 feet, should be made of wood coated with lead, and should end in multiple copper points.

Putting such a plan into effect, of course, would be a social project. Should it be forced upon the populace against its will? This is the only place in all of Marat's

physics trilogy where social theory comes under consideration.

The superstitious nature of the people would be an obstacle to implementing the plan. The idea that ringing the church bells can ward off thunderstorms, for example, was very strong in the countryside in spite of the fact that bell-ringers were frequently killed by lightning.

> To abolish such a harmful practice, the voice of persuasion is surely best if the common people are able to heed the voice of reason. The more widespread the superstition, the more important it is to destroy it. Will it be necessary, then, to use force? Any violence in this regard would amount to real tyranny. How, then, should this be handled? Let the bells be rung, but put a lightning rod on the spire of the steeple and run the groundwire down through the priest's home in order to shield it from the worried, superstitious types who might try to cut it. This is the only appropriate way of stopping so many useful subjects from foolishly seeking their own deaths.[113]

All in all, there is not much in this prescription that gives any hint of the future development of Marat's political outlook. Marat's radicalism would only come to the fore after the hidden tensions in French society erupted into open conflict.

Bertholon and the "Earthquake Rod"

Marat concluded his work on electricity with a lengthy, sarcastic polemic against a pet project of his rival, the Abbé Bertholon. Citing an article by Bertholon in the August 1779 *Journal de Physique*, Marat jeered:

> In a work full of grandiose claims but little substance, a surefire method for preventing the terrible effects of volcano eruptions and earthquakes

> is announced. Sublime project! If only it were as solid as it is flashy.[114]

Bertholon had extended the concept of lightning rods by inventing earthquake rods and volcano rods. Although this scheme is devoid of all foundation, Marat stated, he would take the trouble to refute it because Bertholon's celebrity might blind his readers to its falsity. He quoted Bertholon:

> Earthquakes are nothing more than subterranean thunder, and since it has been demonstrated that thunder is in fact an effect of electricity, one cannot help but recognize that the cause of earthquakes is nothing other than the matter of electricity.[115]

This was not an uncommon notion; Lacépède, it will be recalled, held similar views about the origins of earthquakes.[116] Bertholon had set up a small model of an area of terrain and by applying electricity had made it "quake" and had caused miniature volcanoes to erupt. Marat declared that these "childish games" in no way added support to the false hypothesis that earthquakes and volcanoes are electrical in origin. In fact,

> all available evidence points to the conclusion that volcanic eruptions and earthquakes are caused only by the explosion of a prodigious quantity of combustible materials.[117]

Bertholon's earthquake-and-volcano rods were pointed conducters designed to be sunk deep into the ground like upside-down lightning rods. Insofar as they succeed in attracting electric fluid, Marat warned, they would be hazardous rather than beneficial.

It is on that note that the volume concludes. The sudden ending, with no summary or conclusion, is uncharacteristic of Marat's writing and suggests that perhaps he was in a hurry to get it to the printer.

Marat's experiments on fire had received a positive review from a commission of the Academy; his second endeavor, the optical investigations, met with a much less favorable response. The final episode of his trilogy of physical researches, the electrical experiments, attracted almost no notice: they were ignored. Specific events in the history of Marat's relationship with the Academy had generated the orthodox scientific establishment's hostility toward him, of course, but it was also part of the general movement to drive the spirit of system out of French science.

Postscript: Further Developments in Electrical Science

That the downfall of speculative natural philosophy in France was mediated by social factors -- *i.e.*, that it was strongly influenced by the outcome of the French Revolution -- is evident when it is contrasted with the fortunes of natural philosophy outside France. The case of German *Naturphilosophie* in the early nineteenth century is instructive. *Naturphilosophie* was recognized as a respectable scientific outlook in most of Europe; metaphysical speculation was not considered to be

absolutely incompatible with professional careers in science.

Perhaps partly because they were not socially ostracized by their contemporaries, German natural philosophers have not been ignored by traditional history of science. Faraday's biographer has cited the seminal influence of Kant's *Metaphysical Foundations of Natural Science*, which postulated a dynamic physics based on an essential unity of attractive and repulsive forces:

> From this it followed logically that all the forces of nature were convertible into one another; one need only find the proper conditions for accomplishing the conversion. This fundamental insight was to serve as the stimulus for two generations of scientists amongst whom may be numbered Ritter, Davy, Oersted, and Michael Faraday.[118]

The most acclaimed of the *Naturphilosophs'* achievements was Hans Christian Oersted's demonstration of the underlying unity of electrical and magnetic phenomena. It has frequently been acknowledged that Oersted's success was conditioned by a strong belief in the fundamental unity of nature: "It was *Naturphilosophie*, not chance, that led to the discovery of electromagnetism."[119] Furthermore,

> the stimulus Oersted received from Schelling's "beautiful and great ideas" and from *Naturphilosophie* in general and also the influence of both the speculations and the experiments of Ritter should be recognized as factors involved in a major discovery in physics.[120]

Traditional accounts of Oersted's revelation have often posed the delay between the discovery of current

electricity (1800) and electromagnetism (1820) as an unanswered historical problem:

> It remains a mystery why this phenomenon evaded the experimenters on both sides of the Atlantic for so long after Volta's discovery.[121]

The identity of electricity and magnetism, however, had long been a staple of French natural philosophy. Carra, Marivetz, and many others took for granted that the two were but different manifestations of a single force. On the other hand, orthodox French scientists prior to Oersted's breakthrough had explicitly and vehemently denied the possibility of such a connection. André Marie Ampère later wrote to a friend:

> It is inconceivable that for twenty years no one tried the action of the voltaic pile on a magnet. I believe, however, that I can assign a cause for this; it lies in Coulomb's hypothesis on the nature of magnetic action; this hypothesis was believed as though it were a fact . . . it rejected any idea of action between electricity and the so-called magnetic wires. This prohibition was such that when M. Arago spoke of these new phenomena at the Institute, they were rejected. . . . Every one decided that they were impossible.[122]

François Arago's report of Oersted's discovery had been delivered on September 4, 1820 at a session of the Academy of Sciences. The academicians seemed shocked by what they were hearing. The irreducible duality of electricity and magnetism had been an article of faith for official French science; to challenge that assumption was to risk being ostracised from the scientific community. Arago's announcement, however, removed the prohibition and opened the door for rethinking the electromagnetic

relationship. It is significant that the investigator who most quickly and successfully responded to the new opening was Ampère, who had a strong inclination toward metaphysical speculation and system-building. Ampère's initial scientific inspiration had come from Buffon, Diderot, and Kant.

If Ampère was a French *Naturphilosoph*, however, he had kept those inclinations under wraps. His creative instincts had previously been blunted by the fact that the institutions of French science had channeled him into a career as a mathematician, and he was expected to stick to mathematics alone. After Oersted's discovery, however, Ampère wrote papers on electrodynamics that led some of his colleagues to recognize that his hypothetical notions were far from worthless.

The reason that French investigators had not anticipated Oersted is no mystery, then; that had been virtually precluded by the institutionalized social pressures arrayed against system-building. An external stimulus was required to break the bonds of prejudice restraining the speculative inclinations of an Ampère. The orthodox hegemony is here again witnessed in the act of retarding the development of knowledge. Even *after* the work of Ampère, Faraday, and Maxwell, "the theory of electricity and magnetism . . . was only accepted very slowly and hesitantly, and even unwillingly, in France."[123]

Although the path taken by French science in the later nineteenth century is beyond the scope of this study, J. W. Herivel's discussion of why there was no "French Maxwell" is relevant because it is based upon developments in the post-Revolutionary period.[124] While the nineteenth century "was a great century for French science," he writes, "there was perhaps something rather disappointing about it."[125] The resistance to evolutionary biology is noted in passing, but Herivel's primary concern is with the stunted development of theoretical physics in France. He suggests that "two main traditions or schools of thought," the *mechanical* and the *positivist*, dominated French science in the first half of the nineteenth century and diminished scientific creativity with regard to "thermodynamics, the kinetic theory of gases and electricity and magnetism in the period around 1850-1870."[126]

The social context is absent here; Herivel is only interested in the influence of "the total *intellectual* climate of opinion" on science.[127] He does not associate the two philosophical tendencies with social ideologies. It is clear, nonetheless, that both are variants of the conservative outlook brought to the fore by the Thermidorian reaction. Both "had a measure of common ground corresponding to certain of the generally accepted scientific presuppositions of the period," including a preference for "Newton's method" over "speculative

procedures" and agreement "on the danger of a search for hidden causes."[128]

Herivel associates the mechanical tradition with Laplace and Ampère (we have seen how different in outlook they were), while the positivist tradition is represented by the mathematician and theoretical physicist Jean Baptiste Fourier and by Auguste Comte. With regard to speculation over causation the mechanical school is described as merely cautious while the positivists "placed an all but universal ban" on the practice.[129]

Herivel puts the primary onus on "the predominance of the positivist tradition and outlook."[130] Its exponent, J. B. Fourier, had been an 89er during the Revolution, a baron in the Napoleonic aristocracy of merit, and a recipient of honors after the Bourbon Restoration. He was

> a man who by his position as one of the perpetual secretaries of the Académie des Sciences from 1822-1830 had a great influence on science as a whole.[131]

This influence was widespread, long-lasting and consequential; it was manifested by

> the strongly positivist attitude of almost all French experimental physicists and chemists in the first (and for that matter the second) half of the nineteenth century. In case after case we find strong emphasis on observation and scientific law as opposed to theory or speculation.[132]

In considering whether "the absence of a *French* Maxwell was unavoidable," Herivel is trying to explain why the "creation of a successful theory of electro-magnetism" was not accomplished by a French scientist.[133] Maxwell's

success had depended upon "an open attitude of mind" and, specifically,

> the speculative element corresponding to the introduction of the Displacement Current, an imaginative leap in the dark based on analogy and the desire for symmetry and completeness, one lacking at the time any experimental evidence, and only justified much later.[134]

Herivel suggests that the dominance of the positivist tradition, being "totally incompatible" with "the speculative, hypothetical element of Maxwell's work," made unlikely the appearance of such a theory in France.[135] If this is indeed the case, it would seem that the impact of Thermidorian conservatism on French science endured well into the nineteenth century.

CONCLUSION

Jean Paul Marat's science was normal science for the eighteenth century. The historical record does not support the familiar charges that he was a charlatan, pseudoscientist, or psychopath. The ordinariness of Marat's physics may be disappointing to the reader who anticipated finding in it the very essence of an unorthodox, romantic, Rousseauian science replete with obvious links to revolutionary ideology.

Exploration of the cognitive landscape of "Jacobin science" suggests that if a connection between Revolutionary politics and unorthodox approaches to science existed, the correlation was not a strong one. The most extreme of the speculative natural philosophers were not the most radical politicians.

Conversely, although Marat was indeed a system builder, his spirit of system was not particularly radical. The assumptions upon which he founded his natural philosophy were for the most part those that formed the worldview of most orthodox scientists. Just as Lamarck, Lacépède, Delamétherie, and Du Pont de Nemours each in his own way bridged the gap between orthodox and unorthodox science, so did Marat.

This conclusion has at least two significant implications for the history of the scientific culture of the French Revolution. First of all, it tends to underline

the mythological character of the notion of Jacobin science. If the prototypical Jacobin scientist practiced essentially normal science, the idea that an alternative, politically radical method of investigating nature posed a threat to "real science" seems less sustainable. The alleged connection between revolutionary politics and bad science is thus further revealed as a postrevolutionary invention designed to serve a social purpose.

The second implication follows from realizing that Marat's science cannot fairly be stigmatized as having been utterly worthless. Although his theoretical imagination seems rarely if ever to have risen above the ordinary, his experimental creativity was more impressive. Some of the scientific issues he raised were certainly worthy of attention and discussion. When the scientific elite ignored him, it did not act according to the vaunted ideal of impartiality that supposedly regulates scientific inquiry.

Marat's science was potentially capable of making modest contributions to a fuller comprehension of nature. If that potential was not realized, the fault does not lie with him alone. The guardians of orthodoxy would have better served the cause of advancing knowledge by attempting to refute his claims than by ignoring them.

The treatment Marat's science received at the hands of the scientific elite was symptomatic of a narrowmindedness that was to afflict the practice of science in France throughout the nineteenth century. The social conservatism

of Thermidor engendered scientific conservatism; speculative system building was condemned and banished from the halls of science as illegitimate. The downfall of speculative natural philosophy, then, did not result from inability to contribute to the growth of knowledge or from failure to survive the rigors of scientific combat; its fate was sealed by its perceived association with political radicalism.

In making the point that post-Thermidorian social conservatism played a significant role in retarding the development of French science, there is no intention to overstate it or to emulate those historians who "wave some dimension of history in front of their readers, claiming that it is the forgotten grand arcanum, the key to Clio's mysteries."[1] It cannot be argued, for instance, that France became in any sense a scientific backwater after Thermidor. Paris was the undisputed capital of European science in the first decades of the nineteenth century. The achievements of analytic-empiricist science were impressive and tended to reinforce its claim to be the only sure path to knowledge. The context of social conservatism, then, did not retard science in an absolute sense, but served to guide it into narrow channels.

The loss of Marat's potential contributions to physics or physiology was small, perhaps, compared to the loss of some of Lamarck's potential contributions to biology, the Mesmerists' potential contributions to psychology, or

Bernardin de Saint-Pierre's potential contributions to ecology. By portraying the unorthodox natural philosophers' influence as purely destructive, the scientific elite overlooked its creative aspect.

The decisive defeat of the system-building natural philosophers has often been characterized as a key step forward in the development of modern science. This is valid in the historical sense that modern science *as it has actually developed* was strongly shaped by the ultimate victory of the Laplaces and Cuviers over the Marats and Lamarcks. But the value judgment implicit in the word "forward" is certainly arguable. If science is defined as the general process by which humans seek to expand their knowledge of nature, it is by no means clear that the interests of the fullest development of knowledge were served by assigning pariah status to natural philosophy.

While the suppression of system building has traditionally been hailed as indispensable for the progress of science, then, an opposing argument can be made for its having retarded the growth of scientific knowledge. These apparently conflicting statements are not really irreconcilable; the demise of unorthodox natural philosophy had genuinely contradictory consequences. The exclusive reliance of orthodox science on analytical and qualitative methods did indeed expand the boundaries of some areas of human knowledge while simultaneously restricting development in others. But both the expansion and the

restriction illustrate the historical significance of the struggle between *l'esprit de système* and *l'esprit systèmatique*.

Further ventures into uncharted areas of French Revolutionary science are indicated. The long neglected natural philosophies of Bernardin de Saint-Pierre, Restif de la Bretonne, Jean Louis Carra, and others also merit renewed attention. The examination of Marat's physics represents the beginning, not the completion, of the exploration of these interesting and important cognitive landscapes.

NOTES TO CHAPTER ONE

1. Some of the works that have discussed Marat's science are: Augustin Cabanès, *Marat Inconnu* (Paris, 1891); Joseph Dauben, "Marat: His Science and the French Revolution," *Archives internationales d'histoire des sciences*, 88-89 (1969); C. C. Gillispie, *Science and Polity in France at the End of the Old Regime* (Princeton, N.J.: Princeton University Press, 1980); and N. B. Mandelbaum, *Jean-Paul Marat: The Rebel as Savant* (Unpublished dissertation: Columbia University, 1977).

2. I. B. Cohen, *Franklin and Newton* (Philadelphia: American Philosophical Society, 1956), 548.

3. Helge Kragh, *An Introduction to the Historiography of Science* (Cambridge, England: Cambridge University Press, 1987), 92.

4. Steven Shapin and Arnold Thackray, "Prosopography as a Research Tool in History of Science," *History of Science*, 12 (1974), 21; emphasis in original.

5. The title of this chapter is derived from this quotation; Roy Porter, "The Terraqueous Globe," G. S. Rousseau and Roy Porter, eds., *Ferment of Knowledge* (Cambridge, England: Cambridge University Press, 1980), 285-286. See also the discussion of "historiographic tunnel vision" in Steven Shapin, "Social Uses of Science" in the same volume.

6. These unorthodox scientific types have been examined, repectively, in Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump* (Princeton, New Jersey: Princeton University Press, 1985); Margaret Jacob, *The Newtonians and the English Revolution* (Ithaca, New York: Cornell University Press, 1976); Margaret Jacob, *The Radical Enlightenment: Pantheists, Freemasons and Republicans* (London: Allen & Unwin, 1981); Toby Appel, *The Cuvier-Geoffroy Debate* (Oxford: Oxford University Press, 1987); Dennis Sepper, *Goethe Contra Newton* (Cambridge, England: Cambridge University Press, 1988); and Adrian Desmond, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (Chicago: University of Chicago Press, 1989). This list is by no means intended to be exhaustive; it simply correlates a single title with each of the categories cited in the text.

7. Traditional depictions of the history of science during the French Revolution, including the concept of "Jacobin science," will be the subject of Chapter Two.

8. Jacob Burckhardt, "On Fortune and Misfortune in History," Rolf Saltzer, ed., *German Essays on History* (New York: Frederick Ungar, 1969).

9. Kragh, *Introduction to the Historiography of Science*, 79.

10. Jean Le Rond d'Alembert, *Discours préliminaire de l'Encyclopédie* (Paris: Librairie de la Bibliothèque Nationale, 1872; originally published in 1751), 23. The opposition to system building did not originate with d'Alembert; he was restating the views of Fontenelle, Condillac, and others. "Esprit" can be translated either "spirit" or "mind"; the ambiguity of the French word served d'Alembert's purpose.

11. Isaac Newton, *Opticks* (New York: Dover, 1952; based on the fourth edition, London, 1730). See also Cohen's discussion of the speculative aspects of Newton's natural philosophy in chapters six and seven of *Franklin and Newton*.

12. Letter of 13 March, 1769, from Nollet to E. F. Dutour. Quoted by J. L. Heilbron, *Elements of Early Modern Physics* (Berkeley: University of California Press, 1982), 69.

13. Shapin and Thackray, "Prosopography as a Research Tool in History of Science," 21.

14. The works of these natural philosophers will be discussed in Chapters Four through Six.

15. Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970), 2-3. Kuhn acknowledged that "except in occasional brief asides, I have said nothing about the role of . . . external social . . . conditions in the development of the sciences." *Ibid.*, ix-x.

16. C. C. Gillispie, *The Edge of Objectivity* (Princeton, N.J.: Princeton University Press, 1960), 196; Johann Wolfgang von Goethe, *Zur Farbenlehre* (Tübingen, 1810).

17. Sepper, *Goethe Contra Newton*; Amrine, Zucker, and Wheeler, eds., *Goethe and the Sciences: A Reappraisal* (Dordrecht, Holland: D. Reidel, 1987).

18. For Helmholtz's view of Goethe's theory of colors, see "On Goethe's Scientific Researches," in Helmholtz, *Popular Scientific Lectures* (New York: Dover Publications, 1962; originally published in 1853), 1-21.

19. Sepper, *Goethe Contra Newton*, ix-x.

20. Appel, *Cuvier-Geoffroy Debate*.

21. Desmond, *Politics of Evolution*, 24.

22. *Ibid.*, 21.

23. Shapin and Schaffer, *Leviathan and the Air-pump*, 11.

24. Louis Trenchard More, *The Life and Works of the Honourable Robert Boyle* (London: Oxford University Press, 1944), 97; Leslie Stephen, "Hobbes, Thomas," in *Dictionary of National Biography* (Cambridge: Cambridge University Press, 1891), Vol. IX, 935; Douglas McKie, "Introduction" to Boyle's *Works* (Hildesheim: George Olms, 1965), Vol. I, xii-xiii; all quoted in Shapin and Schaffer, *Leviathan and the Air-pump*, 9, 11.

25. Shapin and Schaffer, *Leviathan and the Air-pump*, 8.

26. *Ibid.*, 13.

27. *Ibid.*, 8-9.

NOTES TO CHAPTER TWO

1. Marshall Clagett, ed., *Critical Problems in the History of Science* (Madison, Wisc.: University of Wisconsin Press, 1959), 255-320.

2. Henry Guerlac, "On the Papers of Charles Coulston Gillispie and L. Pearce Williams," Clagett, ed., *Critical Problems*, 317-320.

3. Charles Coulston Gillispie, "The *Encyclopédie* and the Jacobin Philosophy of Science: A Study in Ideas and Consequences," Clagett, ed., *Critical Problems*, 255-289.

4. *Ibid.*, 279; the phrase is Gillispie's.

5. L. Pearce Williams, "The Politics of Science in the French Revolution," Clagett, ed., *Critical Problems*, 304-305.

6. The meaning of the word "Jacobin," of course, varies according to context. It is used here to refer to the left-wing political tendency that governed the Republic from the middle of 1793 to the fall of Robespierre in July 1794.

7. Simon Shaffer, "Natural Philosophy," Rousseau and Porter, eds., *Ferment of Knowledge*, 77.

8. This is "the infallible touchstone of romantic tendencies" in natural philosophy. Gillispie, *Edge of Objectivity*, 199.

9. The Baron de Marivetz and Louis Jacques Goussier, *Physique du monde* (Paris: Quillau, 1780-1787), Vol. I, lxxxvii. For a discussion of Marivetz and Goussier and their *Physique du monde*, see Chapter Six below.

10. For example: "The totality and consequences of necessary laws constitute mechanics. We call the totality of the other laws the 'System of the World,' which we cannot know entirely without knowing all phenomena." Condorcet, *Lettre sur le Systême du Monde et sur le Calcul Intégral* (1768); quoted by Roger Hahn, *Laplace as a Newtonian Scientist* (Los Angeles: University of California Press, 1967), 16.

11. *Observations sur la physique*, Dec. 1781, 503. This journal is better known by its later name, the *Journal de physique*. On its importance and on Delamétherie's role, see Chapter Six below. On Carra's *Nouveaux Principes de physique* (which would eventually grow to four volumes), see Chapter Four below.

12. Guerlac, "Some Aspects of Science during the French Revolution," *The Scientific Monthly*, Vol. 80, No. 2 (February 1955), 97.

13. Roger Hahn, *The Anatomy of a Scientific Institution* (Berkeley: University of California Press, 1971), 289.

14. *Ibid.*, 290.

15. See, for example, Fourcroy, *Rapport Fait au Nom du Comité de Salut Public. Séance de la Convention, 14 Nivose An III* (Paris, 1795). This aspect of Fourcroy's political career is entirely overlooked in the standard biography by W. A. Smeaton, *Fourcroy: Chemist and Revolutionary, 1755-1809* (Cambridge, England: W. Heffer & Sons, 1962).

16. On Grégoire's role: "L'homme qui fit à la Convention, après thermidor, cette série de rapports tissus d'injures et de faussetés, qui ont fourni aux ennemis de la Révolution tout un arsenal d'accusations ineptes et odieuses, c'est l'évêque Grégoire." James Guillaume, *Etudes révolutionnaires*, Première Série (Paris: P. V. Stock, 1908), 155.

17. Hahn, *Anatomy of a Scientific Institution*, 293.

18. Guerlac, "On the Papers of Gillispie and Williams," Clagett, ed., *Critical Problems*, 319.

19. Guerlac, "Some Aspects of Science during the French Revolution," 100. The apocryphal nature of the famous anecdote was established by James Guillaume, "Un mot légendaire: 'La République n'a pas besoin de savants,'" *Révolution française*, 38 (1900); reprinted in Guillaume, *Etudes révolutionnaires*, 136-155. Guillaume traces the origin of this "légende haineuse et sotte" to Grégoire and ranks Fourcroy and Cuvier among its most effective propagators.

20. Hahn, *Anatomy of a Scientific Institution*, 223.

21. Clagett, ed., *Critical Problems*, 255-320.

22. The tradition of hostility represented by Burke's *Reflections on the French Revolution* and Carlyle's *French Revolution* has hardly abated, as can be seen by the most popular work in English issued to coincide with the bicentennial commemoration, Simon Schama's *Citizens* (New York: Alfred A. Knopf, 1989).

23. The quotation is from R. R. Palmer's preface to his translation of Georges Lefebvre's classic *Quatre-vingt-neuf*, but it is intended as a typical statement representing the general assessment of historians until challenged by a revisionist school of thought initiated by Alfred Cobban's "Myth of the French Revolution" lecture in May 1954; see Cobban, *The Social Interpretation of the French Revolution* (Cambridge: Cambridge University Press, 1964). Neither the Revolution as turning point nor the necessity of the Jacobin role are any longer universally accepted. One variant of the revisionist argument holds that insofar as there was a positive legacy of the Revolution, it was accomplished early on, before the radicals rose to power; the Jacobin dictatorship was not only not essential to the revolutionary process, but was in fact antithetical to it. This important debate over historical interpretation cannot be resolved in a footnote, of course, but it is necessary that the point of view informing the present study be explicitly stated; *i.e.*, that (1) the French Revolution was indeed "the great turning point of modern civilization," and (2) Jacobin power, "the Terror," was necessary to the consolidation of the Revolution, which could not have survived otherwise.

24. J. B. Biot, *Essai sur l'histoire générale des sciences pendant la Révolution française* (Paris, 1803), 74ff.; Donald M. Vess, *Medical Revolution in France, 1789-1795* (Gainesville, Florida: University Presses of Florida, 1975).

25. James McClellan, *Science Reorganized: Scientific Societies in the Eighteenth Century* (New York: Columbia University Press, 1985), 257; see also, Hahn, *Anatomy of a Scientific Institution*, 264, 285.

26. Robert Darnton, *The Literary Underground of the Old Regime* (Cambridge, Mass.: Harvard University Press, 1982).

27. Daniel Mornet, *Origines intellectuelles de la Révolution française* (Paris: A. Colin, 1933), 24.

28. Restif's *Physique* is a section of his *Philosophie de Monsieur Nicolas*, which in turn is part of his autobiography, *Monsieur Nicolas*. See the discussion of Restif de la Bretonne and his works in Chapter Five.

29. Robert Darnton, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York: Basic Books, 1984), 261-262.

30. The natural philosophies of Restif, Du Pont, and Delamétherie will be discussed in Chapters Five, Four, and Six, respectively.

31. Plato, *Timaeus*, in *Great Books of the Western World* (Chicago: Encyclopaedia Britannica, 1952), Vol. 7, 448-449.

32. "Seeing the earth as a living animal still claimed a few supporters -- like Thomas Robinson or William Hobbs -- at the close of the seventeenth century. Such literal anthropomorphic animism, however, was in retreat . . . . Yet conceptions of the environment as organism, a quasi-animal, with its own internal heat and energy, and with metabolic systems of change, restoration, circulation and equilibration, remained key metaphors through the eighteenth century." Roy Porter, "Terraqueous Globe," Rousseau and Porter, eds., *Ferment of Knowledge*, 289-290.

33. James E. Lovelock, *Gaia: A New Look at Life on Earth* (Oxford: Oxford University Press, 1979); James E. Lovelock, *The Ages of Gaia* (New York: W. W. Norton, 1988); J. E. Lovelock and Lynn Margulis, "Atmospheric Homoeostasis by and for the Biosphere: The Gaia Hypothesis," *Tellus*, 26, 2 (1973).

34. Lewis Thomas, "Beyond the Moon's Horizon -- Our Home," *New York Times*, July 15, 1989.

35. H. C. Oersted, "Thermo-electricity" (1830); quoted by L. Pearce Williams, *Michael Faraday* (New York: Da Capo Press, 1965), 137.

36. Gaston Bachelard, *The Psychoanalysis of Fire* (Boston: Beacon Press, 1968), 6, 8, 29, 47, 48, 50, 63.

37. See especially: Bachelard, *La Formation de l'esprit scientifique* (Paris: J. Vrin, 1938).

38. Gillispie, *Science and Polity*, 158.

39. Gillispie, *Edge of Objectivity*, 201.

40. On Lamarck, see Gillispie, "Lamarck and Darwin in the History of Science," Glass, Temkin, and Straus, eds., *Forerunners of Darwin: 1745-1859* (Baltimore: Johns Hopkins, 1959), 265-291. On Mesmer and Marat, see Gillispie, *Science and Polity*, 261-330.

41. Gillispie, *Science and Polity*, Chapter 4.

42. Noting that the "social uses of science" are not "traditionally regarded as proper for the serious study of historians of science," Steven Shapin writes: "These views are articulated with unusual clarity in some of the writings of Charles Gillispie . . . who eloquently verbalizes what seem to be the implicit operating assumptions of many historians of science." Shapin, "Social Uses of Science," Rousseau and Porter, eds., *Ferment of Knowledge*, 94.

43. Gillispie, "Jacobin Philosophy," Clagett, ed., *Critical Problems*, 281.

44. As Gillispie observes elsewhere, "the continuum is by no means always the wrong side." Gillispie, *Edge of Objectivity*, 198.

45. *Ibid.*, 155. However, *N.B.:* "Diderot cannot be assigned to either the rationalist or the romantic moods. He belonged to both and to neither." *Ibid.*, 181.

46. Daniel Bell, *The End of Ideology* (Glencoe, Ill., 1960).

47. Gillispie, *Edge of Objectivity*, 181.

48. *Ibid.*, 199; also, Gillispie, "Jacobin Philosophy," Clagett, ed., *Critical Problems*, 282.

49. Gillispie uses this phrase twice: in *Edge of Objectivity*, 192; and in "Jacobin Philosophy," Clagett, ed., *Critical Problems*, 280.

50. Sandra Harding, *The Science Question in Feminism* (Ithaca, N.Y.: Cornell University Press, 1986), 202, 205.

51. *Ibid.*, 207.

52. *Ibid.*, 44-47. In addition to Harding, see also Carolyn Merchant's feminist contribution to the history of the scientific revolution, *The Death of Nature: Women, Ecology and the Scientific Revolution* (New York: Harper & Row, 1983).

53. Rachel Carson, *Silent Spring* (Boston: Houghton Mifflin, 1962); Frank Graham, Jr., *Since Silent Spring* (Boston: Houghton Mifflin, 1970).

54. Guerlac, "On the Papers of Gillispie and Williams," Clagett, ed., *Critical Problems*, 320.

55. Louis Roule, *Bernardin de Saint-Pierre et l'harmonie de la nature* (Paris, Flammarion, 1930).

NOTES TO CHAPTER THREE

1. Christopher Hill, *The World Turned Upside Down* (New York: Viking Press, 1972). For the historiography of the "new contextualism," see especially Shapin, "Social Uses of Science," Rousseau and Porter, eds., *Ferment of Knowledge*, 93-139.

2. The word "reductionist" means many things to many people. It has even been misused in defense of traditional internalist history of science. As Bruno Latour has noted, "to those who reject 'social' explanations of science . . . all social studies of science are thought to be reductionist and are held to ignore the most important features of science." Latour, *The Pasteurization of France* (Cambridge, Mass.: Harvard University Press, 1988), 153. In the present study, "reductionist" is used to denote the tendency to reduce all of the laws of nature to the laws of physics.

3. James R. Jacob. "'By an Orphean Charm': Science and the Two Cultures in Seventeenth-Century England," P. Mack and M. C. Jacob, eds., *Politics and Culture in Early Modern Europe* (Cambridge, England: Cambridge University Press, 1987), 242.

4. See especially: Margaret Jacob, *The Newtonians and the English Revolution* (Ithaca, New York: Cornell University Press, 1976), and Margaret Jacob, *The Cultural Meaning of the Scientific Revolution* (New York: Alfred A. Knopf, 1988).

5. P. M. Ratansi, "Paracelsus and the Puritan Revolution," *Ambix*, 11 (1964), 24.

6. On the other side, the moderates also opposed those materialists like Hobbes who sucked *too much* spirit out of nature, thereby calling the existence of God into question. See James C. Jacob and Margaret C. Jacob, "Anglican Origins of Modern Science," *Isis*, 71 (1980), 257.

7. James R. Jacob. "'By an Orphean Charm': Science and the Two Cultures in Seventeenth-Century England," 242.

8. M. C. Jacob, *Radical Enlightenment*, 38.

9. Robert Darnton, *Mesmerism and the End of the Enlightenment in France* (Cambridge, Mass.: Harvard University Press, 1968), 91.

10. *Ibid.*, 42-43.

11. *Ibid.*, 14.

12. *Ibid.*, 44-45.

13. *Ibid.*, 133, 125.

14. *Ibid.*, 128. Emphasis added.

15. Bachelard, *Psychoanalysis of Fire*, 75.

16. "Cabanis's image as a materialist, long accepted by historians of philosophy, often seemed contradictory to the findings of historians of medicine, who noted his defense of vitalist physicians." Martin S. Staum, *Cabanis: Enlightenment and Medical Philosophy in the French Revolution* (Princeton, N.J.: Princeton University Press, 1980), 8. "Lamarck is often set over against mechanism as a vitalist." Gillispie, *Edge of Objectivity*, 262. "Lamarck was no vitalist. To the contrary, the whole of his biological thought was decidedly materialistic." Richard W. Burkhardt, Jr., *The Spirit of System: Lamarck and Evolutionary Biology* (Cambridge, Mass.: Harvard University Press, 1977), 151.

17. The distinction between '89ers and '93ers is between those who believed that the Revolution should have stopped with the reforms achieved in 1789 and those who supported the radical measures of 1793 as necessary to consolidate the Revolution's achievements. The variety of political opinion constituted a spectrum rather than a dichotomy, of course, but these two poles of opinion tended to stand out on the spectrum.

18. R. H. Tawney, *Religion and the Rise of Capitalism* (Gloucester, Mass.: Peter Smith, 1962; originally published in 1926), 227.

19. Condorcet, *Eloges des Académiciens de l'Académie Royale des Sciences* (Paris: Chez Vieweg et Fuchs, 1799), Vol. III, 337-440. Emphasis added.

20. As Gillispie has recognized: "Normally treated by historians as the Girondist *savant* in the Revolution and last of the philosophes," Condorcet "might better be taken for an intermediary figure between the encyclopedists and positivism . . . ." By "encyclopedist" Gillispie means a person "whose deepest and most natural interests bore upon the social process and the role of science in that rather than upon problems of science itself." Gillispie, *Science and Polity*, 37, 39.

21. Quoted by Guerlac, "Some Aspects of Science during the French Revolution," 95.

22. Condorcet, *loc. cit.*

23. Condorcet, *Ibid.*

24. Condorcet, *Ibid.* Emphasis added.

25. Gillispie is not unaware of this aspect of Condorcet's outlook. In fact, it is due to Condorcet's concern with social problems that Gillispie downgrades Condorcet's value as a scientist. See Keith Baker's response to Gillispie on this point: Keith Baker, "Science and Politics at the End of the Old Regime," *Inventing the French Revolution* (Cambridge, England: Cambridge University Press, 1990), 153-166.

26. See the section on Restif de la Bretonne in Chapter Five for a further discussion of the Cercle Social.

27. Desmond, *The Politics of Evolution*, 329, 378.

28. Appel, *The Cuvier-Geoffroy Debate*, 8.

29. Hahn, *Laplace as a Newtonian Scientist*, 3-4.

30. Pierre Simon Laplace, *Exposition du système du monde* (Paris: Cercle Social, 1796) and *Traité de mécanique céleste* (Paris, 1799-1825).

31. Quoted by Hahn, *Laplace as a Newtonian Scientist*, 17.

32. Hahn, *Laplace as a Newtonian Scientist*, 9.

33. Gillispie, *Edge of Objectivity*, 201; also, Gillispie, "Lamarck and Darwin," 279.

34. Quoted by J. W. Herivel, "Aspects of French Theoretical Physics in the Nineteenth Century," *British Journal for the History of Science*, III (1966), 113.

35. Nicole Dhombres and Jean Dhombres, *Naissance d'un nouveau pouvoir: sciences et savants en France, 1793-1824* (Paris, Editions Payot, 1989), 683.

36. *Ibid.*, 685.

37. Gillispie, *Science and Polity*, 549.

38. Baker, *Inventing the French Revolution*, 159.

39. Hahn, *Anatomy of a Scientific Institution*, 304.

40. Hahn, *Laplace as a Newtonian Scientist*, 5.

41. William Coleman, *Georges Cuvier, Zoologist: A Study in the History of Evolution Theory* (Cambridge, Mass.: Harvard University Press, 1964), 4.

42. *Ibid.*, 5.

43. Georges Cuvier, *Rapport historique sur les progrès des sciences naturelles depuis 1789, et sur leur état actuel* (Paris: De l'Imprimerie impériale, 1810), 99-100.

44. Coleman, *Cuvier*, 29.

45. *Ibid.*, 11.

46. Appel, *Cuvier-Geoffroy Debate*, 12.

47. *Ibid.*, 9.

48. *Ibid.*, 30.

49. *Ibid.*, 54.

50. Cuvier, *Rapport historique sur les progrès des sciences naturelles depuis 1789, et sur leur état actuel*, 387.

51. Gillispie, "Lamarck and Darwin," 279; also, Gillispie, *Edge of Objectivity*, 201.

52. Cuvier, *Rapport historique sur les progrès des sciences naturelles depuis 1789, et sur leur état actuel*, 389.

53. Appel, *Cuvier-Geoffroy Debate*, 41.

54. Marat's ashes were placed in the Pantheon on September 21, 1794 and removed on February 8, 1795.

NOTES TO CHAPTER FOUR

1. Historians associated with the Marxist tradition have stressed the importance of the conflicting material interests of social classes as a factor influencing the events of the Revolution. The thesis that the Revolution was a bourgeois revolution (*i.e.*, accomplished in the interests of a rising capitalist class) is frequently identified as a uniquely Marxist concept, although it originated well before Marx. More recently, it has been thought of as the essential proposition of the "Lefebvre synthesis" and has been the central target of revisionist criticism.

2. Gillispie, *Science and Polity*, 290.

3. Dauben, "Marat: His Science and the French Revolution."

4. Quoted by Hahn, *Anatomy of a Scientific Institution*, 158.

5. Lamarck, *Recherches sur les causes des principaux faits physiques* (Paris: Chez Maradan, 1794), Vol. I, 343-368.

6. Goethe, *Zur Farbenlehre*, Vol. II, 601-606.

7. Darnton, *Mesmerism*, 95.

8. Cuvier, letter of February 19, 1791, *Lettres à C. M. Pfaff sur l'histoire naturelle, la politique et la littérature* (Paris: Librairie Victor Masson, 1858), 221.

9. Alphonse de Lamartine, *Histoire des Girondins*, in *Oeuvres Complètes* (Paris, 1862-66), Vol. IX, 163.

10. Alain Nabarra, "Carra, Jean-Louis," Jean Sgard, ed., *Dictionnaire des journalistes, 1600-1789* (Grenoble: Presses universitaires de Grenoble, 1976), 67-71.

11. M. L. Kennedy, "L'Oracle des Jacobins des départements; Jean-Louis Carra et ses *Annales patriotiques*," *Actes du Colloque Girondins et Montagnards* (Paris, 1980).

12. Carra, *Annales patriotiques*, July 8, 1791; in *Articles, Notes et extraits d'articles de J. L. Carra, tirés des Annales Patriotiques, depuis juin 1791 jusqu'au 31 juillet 1793* (n.p., 1793), 5.

13. Carra, *Essai sur la nautique aérienne* (Paris: Eugène Onfroy, 1784); *Examen physique du magnétisme animal* (London, 1785); *Dissertation élémentaire sur la nature de la lumière, du feu et de l'électricité* (Paris, 1787).

14. Bachelard, *Psychoanalysis of Fire*, 63.

15. Darnton, *Literary Underground of the Old Regime*.

16. Rivarol and Champcenetz, *Petit Dictionnaire des grands hommes de la Révolution* (Paris: L'Imprimerie Nationale, 1790), 14.

17. Carra, *Le Faux Philosophe démasqué* (n.p., 1772).

18. Etienne de Loménie de Brienne (1727-1794) was Minister of Finances in 1787.

19. Académie des Sciences, *Procès verbaux*, Vol. 103, entry for January 14, 1784.

20. *Journal de Physique*, Vol. 18, Part II (Sept. 1781), 247-248.

21. "Lettre à M. le Baron de Marivetz sur le premier Volume de la Physique du monde," J. L. Carra, *Nouveaux Principes de physique* (Paris: Morin, 1781-1783), Vol. II, 186-194. This is followed by a response from Marivetz and then another response by Carra. See the discussion of Marivetz and Goussier's *Physique du monde* in Chapter Six, below.

22. Carra, *Examen physique du magnétisme animal*, 96-98.

23. See the section on Bergasse in this chapter, below.

24. *Journal encyclopédique*, 1782, Vol. V, 191-201.

25. *Journal encyclopédique*, 1783, Vol. IV, 205-228; 1785, Vol. III, 415-435.

26. *Journal de Physique*, Vol. 18, Part II (Dec. 1781), 503-504.

27. Carra, *Examen physique du magnétisme animal*, 1.

28. One author went so far as to identify Du Pont de Nemours as "l'arrière-grand-père du nylon." André Conquet, "Pierre Samuel Du Pont de Nemours 1739-1817: Un Réaliste visionnaire," *Communication à l'Académie de Dijon*, May 27, 1983.

29. See, for example, Du Pont de Nemours, *Avant-dernier chapître de l'histoire des Jacobins* (Paris: De l'imprimerie de l'auteur, 1796). Furthermore, "Du Pont took up the sword as well as the pen," according to Ambrose Saricks, *Pierre Samuel Du Pont de Nemours* (Lawrence, Kansas: University of Kansas Press, 1965), 213.

30. Du Pont de Nemours, *Philosophie de l'univers* (Paris, 1792). The date on the title page is 1792, but the date at the end of the text is "10 juin 1793."

31. Restif de la Bretonne, "Système du citoyen Dupont, de Nemours, ami de Lavoisier," *Monsieur Nicolas, ou le coeur humain dévoilé* (Paris: Publié par lui-même, 1794-1797), Vol. V, 541-564.

32. Darnton, *Mesmerism*, 136.

33. Saricks, *Du Pont de Nemours*, 224.

34. Darnton, *Mesmerism*, 137.

35. Saricks, *Du Pont de Nemours*, 220; see also 261 and 355.

36. The basic text of *Philosophie de l'univers* was virtually unchanged in the second (1796) and third (1799) editions, but a large number of supplementary notes were added. Extensive changes in the text were planned for the projected fourth edition that was never published; Du Pont's editorial changes are written in the margins of a Third Edition that is now in the collection of the Hagley Museum and Library, Wilmington, Delaware. The Hagley collection also includes a manuscript edition, beautifully handwritten and bound, dated 1792.

37. John Beverly Riggs, *Guide to the Manuscripts in the Eleutherian Mills Historical Library* (Greenville, Delaware, 1970).

38. Saricks, *Du Pont de Nemours*, 222-224. Pierre Jolly, *Du Pont de Nemours: Soldat de la Liberté* (Paris, 1956), while generally less comprehensive than Saricks' biography, devotes a chapter to the *Philosophie de l'univers*.

39. Unpublished letter from Herbert Reichner to Denis Duveen, 1949. It is inserted, loose, in a copy of *Philosophie de l'univers* that Duveen donated to the Lavoisier collection at Cornell University's Olin Library. The price of this rare volume was set by Reichner at $75.

40. Du Pont, *Philosophie de l'univers*, 10.

41. *Ibid.*, 11.

42. *Ibid.*, 331. The capitalization follows the original.

43. *Ibid.*, 13-14.

44. *Ibid.*, "Table Générale," 329. The capitalization follows the original.

45. *Ibid.*, 269.

46. *Ibid.*, 274.

47. *Ibid.*, 252.

48. *Ibid.*, 283-314.

49. *Ibid.*, 281.

50. *Ibid.*, 275.

51. *Ibid.*, 282.

52. *Ibid.*, 283. Two levels of emphasis in original.

53. *Ibid.*, 222. See also, 46.

54. *Ibid.*, 101-102. The last name on the list (actually listed first by Du Pont) was identified only as "L. C. . ."; Pierre Jolly identifies him as Choderlos de Laclos, author of *Les Liaisons dangereuses*.

55. *Ibid.*, 102.

56. *Ibid.*, 178.

57. *Ibid.*, 150. This passage was evidently written after Lavoisier's death.

58. *Ibid.*, "Sur la possibilité de l'animation des Globes célestes," 245-252.

59. *Ibid.*, 52.

60. *Ibid.*, 251.

61. *Ibid.*, 252.

62. *Ibid.*, 212-213.

63. Etienne Lamy, "Introduction," in Bergasse du Petit-Thouars, *Nicolas Bergasse* (Paris: Librairie Académique, 1910), x.

64. Mesmer presented diplomas to each of his lieutenants that "certified his place in the hierarchy of disciples: Bergasse was first . . . ." Darnton, *Mesmerism*, 75.

65. Darnton, *Mesmerism*, 51.

66. *Ibid.*, 78.

67. *Ibid.*, 163.

68. *Ibid.*, 79.

69. *Ibid.*, 88.

70. J. P. Brissot, *Mémoires*, Vol. II, 53-56. Quoted by Darnton, *Mesmerism*, 79.

71. That many individual aristocrats not only survived the Revolution but were to be found among the upper crust of the new society does not negate the proposition that the aristocracy *as a social class* ceased to exist after the abolition of the legal privileges that had defined it.

72. Eugen Weber, *Europe Since 1715* (New York: W. W. Norton, 1971), 145.

73. *Commission chargée de l'examen du magnétisme animal*. There were in fact two commissions; all references to "the commission" here refers to the more important one that issued a report in August 1784 over the signatures of Franklin, Lavoisier, Bailly, Le Roy, Guillotin, D'Arcet, Sallin, De Bory, and Majault. Quotations from that report are from the English translation published in London in 1785 as *Report of Dr. Benjamin Franklin and Other Commissioners Charged by the King of France, with the Examination of the animal Magnetism as Now Practised at Paris* (London, 1785).

74. Duveen and Klickstein, *Bibliography of the Works of Antoine Laurent Lavoisier, 1743-1794* (London: Wm. Dawson & Sons, 1954), 249-261.

75. *Franklin Report* (on animal magnetism), 54.

76. Nicolas Bergasse, *Considérations sur le Magnétisme animal, ou Sur la théorie du monde* (La Haye, 1784), 3.

77. *Ibid.*, 141.

78. *Ibid.*, 37; 59.

79. Mesmer's assault against Bergasse was entitled *Lettre de l'Inventeur du Magnétisme animal à l'auteur des Réflexions préliminaires*; Bergasse's reply was his *Observations sur un écrit du Docteur Mesmer.*

80. Bergasse, *Considérations sur le Magnétisme animal, ou Sur la théorie du monde*, 34.

81. *Ibid.*, 60. Emphasis in original.

82. *Ibid.*, 60-61.

83. *Ibid.*, 38.

84. *Ibid.*, 41.

85. *Ibid.*, 42.

86. *Ibid.*, 42.

87. *Ibid.*, 45.

88. *Ibid.*, 46.

89. *Ibid.*, 47.

90. *Ibid.*, 49-50.

91. *Ibid.*, 58.

92. *Ibid.*, 113-114.

93. *Ibid.*, 92.

94. *Ibid.*, 78.

95. *Ibid.*, 65.

96. *Ibid.*, 80.

97. *Ibid.*, 54.

98. *Ibid.*, 131.

99. Gillispie, "The *Encyclopédie* and the Jacobin Philosophy of Science," Clagett, ed., *Critical Problems*, 267. Emphasis added.

100. Adam Crabtree, *Animal Magnetism, Early Hypnotism, and Psychical Research 1766-1925* (White Plains, N.Y.: Kraus International Publishers, 1988), xi.

NOTES TO CHAPTER FIVE

1. Maurice Souriau, *Bernardin de Saint-Pierre d'après ses manuscrits* (Geneva: Slatkine Reprints, 1970; first published in 1905), 255.

2. Hahn, *Anatomy of a Scientific Institution*, 183.

3. E. de las Casas, *Le Mémorial de Sainte-Hélène* (Paris: J. de Bonnot, 1969), 22 January 1816. Quoted by Dhombres and Dhombres, *Naissance d'un nouveau pouvoir*, 685.

4. Maurice Souriau carefully documented and analyzed what he called "les méfaits de Aimé Martin." Maurice Souriau, *Bernardin de Saint-Pierre d'après ses manuscrits* (Geneva: Slatkine Reprints, 1970; originally published in 1905). Daniel Mornet commented: "Nous savons que Bernardin de Saint-Pierre est toujours victime d'Aimé Martin." Daniel Mornet, *Le Sentiment de la nature en France de J.-J. Rousseau à Bernardin de Saint-Pierre* (New York: Burt Franklin, 1971; originally published in 1907), 10.

5. Arthur O. Lovejoy, *The Great Chain of Being* (New York: Harper & Brothers, 1960; originally published in 1936), 186.

6. *Paul et Virginie* first appeared in Volume IV of the Third Edition of *Etudes de la nature*, published in 1788. The first separate edition of the novel was published in 1789.

7. Thomas Carlyle, *The French Revolution* (New York: Modern Library, 1934), 48-49.

8. John Nottingham Ware, "The Vocabulary of Bernardin de Saint-Pierre and its Relation to the French Romantic School," *The Johns Hopkins Studies in Romance Literatures and Languages*, Vol. IX (Baltimore, Maryland: The Johns Hopkins Press, 1927), 2.

9. Augustine Dirrell, "Preface" to Arvède Barine, *Bernardin de Saint-Pierre* (Chicago: A. C. McClurg & Co., 1893). Arvède Barine was the pseudonym of Cécile Vincens.

10. Barine, *Bernardin de Saint-Pierre*, 128.

11. Quoted in "Memoir of the Life of M. de St. Pierre," in Bernardin de Saint-Pierre, *Works*, Frederic Shoberl, tr. (London: James Cundee, 1807), xxix.

12. Bernardin de Saint-Pierre, *Etudes de la nature*, 1:20.

13. *Ibid.*, 1:115.

14. *Ibid.*, 1:36.

15. Clarence J. Glacken, *Traces on the Rhodian Shore* (Berkeley: University of California Press, 1967), 550.

16. *Ibid.*, 501.

17. Donald Worster, *Nature's Economy* (Cambridge, England: Cambridge University Press, 1977).

18. Bernardin de Saint-Pierre, *Etudes de la nature*, 1:1.

19. *Ibid.*, 1:2.

20. *Ibid.*, 1:10-11.

21. *Ibid.*, 1:11.

22. *Ibid.*, 1:60. It should be noted that Bernardin explicitly disavowed the doctrine of the Great Chain of Being.

23. *Ibid.*, 1:53.

24. *Ibid.*, 1:13.

25. *Ibid.*, 1:18.

26. *Ibid.*, 1:37.

27. *Ibid.*, 1:18.

28. Glacken, *Traces on the Rhodian Shore*, 548.

29. Bernardin de Saint-Pierre, *Etudes de la nature*, 1:vii.

30. *Ibid.*, 1:ix.

31. Letter from Bernardin de Saint-Pierre to Du Pont de Nemours dated 3 Ventôse, an 9; in P. S. Du Pont de Nemours' personal correspondence, Winterthur Collection, Hagley Museum. Bernardin and Du Pont were distantly related by marriage. Furthermore, Du Pont's second wife and Bernardin were philosophic friends. She was the widow of Pierre Poivre, a naturalist who had been Intendant of the Ile de France; during Bernardin's stay on that island he and she had engaged in an extensive philosophic correspondence.

32. Bernardin de Saint-Pierre, *Etudes de la nature*, 1:xv-xvi.

33. Jean Jacques Levallois believes that Bernardin "in all good faith" had "confused geometric angular amplitude with angular amplitude based on tangents to the surface, which are derived from astronomical measurements." Jean Jacques Levallois, "L'Académie royale des sciences et la figure de la terre," *La Vie des sciences: Comptes rendus de l'Académie des Sciences*, Vol. III (série générale), No. 2, Mars-Avril 1986, 287.

34. See Louis Roule, *Bernardin de Saint-Pierre et l'harmonie de la nature* (Paris, Flammarion, 1930).

35. *Etudes Rétiviennes: Bulletin de la Société Rétif de la Bretonne*, Librairie Clavreuil, 37 rue Saint-André des Arts, 75006 Paris. When Restif was born, his family spelled its name "Rétif." He contended that the original, authentic spelling had been "Restif," which is the varient he chose to use.

36. *La Nuit de Varennes*, jointly produced by Opera (Rome) and Gaumont (Paris); released in 1983.

37. Restif de la Bretonne, *Le Paysan perverti, ou les Dangers de la ville* and *La Paysanne pervertie, ou les Dangers de la ville*. The two epistolary novels were later rewritten into a single work as *Le Paysan et la paysanne pervertis, ou les Dangers de la ville* (La Haye, actually published in 1787 although the title page says 1784). An English translation of the combined version, together with bibliographical information, can be found in *The Corrupted Ones*, translated by Alan Hull Walton (London: Neville Spearman, 1966).

38. Restif de la Bretonne, *Les Nuits de Paris: A Selection*, L. Asher and E. Fertig, trs. (New York: Random House, 1964), 68.

39. Restif de la Bretonne, *Monsieur Nicolas, ou le coeur humain dévoilé* (Paris: Publié par lui-même, 1794-1797). The number of volumes and their order is a complicated bibliographic matter that need not be detailed here; suffice it to say that "nineteen" is a reasonable interpretation.

40. Frank E. Manuel and Fritzie P. Manuel, *Utopian Thought in the Western World* (Cambridge, Mass.: The Belknap Press of Harvard U. Press, 1979), 550.

41. The source of this anecdote is given by the notes to the Gallimard edition of *Monsieur Nicolas*, 2:1434, as: Cubières-Palmézeaux, "Notice sur Rétif" in Vol. I of *Histoire des compagnes de Maria*, 156-57.

42. "Je mets Restif fort au-dessus de Rousseau." This was placed, for example, on the title page of Marc Chadourne, *Restif de la Bretonne, ou Le Siècle prophétique* (Paris: Hachette, 1958), which gives the source as: Paul Valéry, *Lettre*, 4 mars 1934.

43. Mark Poster, *The Utopian Thought of Restif de la Bretonne* (New York: New York University Press, 1971), 39.

44. Karl Marx and Friedrich Engels, *The Holy Family, or Critique of Critical Criticism* (Moscow: Foreign Languages Publ., 1956), 161.

45. Darnton, *Mesmerism*, 132.

46. *Ibid.*, p. 133. Nicolas Bonneville, like his friend Thomas Paine, took the side of the Girondins against the Jacobins and narrowly missed sharing their fate. He survived but further persecution by Bonaparte's regime prompted him to send his wife and children to the United States where he later joined them. He returned to France in 1819, made a meager living as a book vendor, and is said to have died poor and crazy in 1828 (Philippe le Harivel, *Nicolas de Bonneville: Pré-romantique et Révolutionnaire*). His son Benjamin Bonneville (1796-1878) became a career officer in the United States Army, eventually rising to the rank of general. Benjamin Bonneville was immortalized by Washington Irving in *The Adventures of Captain Bonneville, U.S.A.* (Norman, Oklahoma: University of Oklahoma Press, 1986; originally published in 1837).

47. Restif de la Bretonne, "Declaration of the Author's Political Beliefs," *Nuits de Paris*, Asher and Fertig, trs., 349-350. "Louis Capet," of course, is the republican way of denoting the former king, Louis XVI.

48. F. Funck-Brentano, "Introduction," in Restif de la Bretonne, *Les Nuits révolutionnaires; impressions et récits contemporains* (Paris: A. Fayard, 1910), 38-39.

49. Paris: Isidore Liseux, 1883.

50. Henri Bachelin, ed., *L'Oeuvre de Restif de la Bretonne* (nine volumes, Paris: Trianon, 1930-32). See 2:297-346.

51. Restif de la Bretonne, *Monsieur Nicolas, or the Human Heart Unveiled.* Translated by R. Crowdy-Mathers; edited, with an introduction, by Havelock Ellis (six volumes, London: John Rodker, 1930-31).

52. Restif de la Bretonne, *Monsieur Nicolas* (six volumes, Paris: Jean-Jacques Pauvert, 1959).

53. Restif de la Bretonne, *Monsieur Nicolas* (two volumes, Paris: Gallimard, 1989.)

54. The Pauvert edition of *Monsieur Nicolas* includes a photographic reproduction of Restif's letter to Carnot, including Carnot's order to grant the request written in the corner (Vol. VI, facing page 320).

55. Restif de la Bretonne, *Monsieur Nicolas* (Pauvert edition), 5:109. Unless otherwise specified, all quotations from the *Physique* and from the other parts of *Philosophie de Monsieur Nicolas* have been translated from the Pauvert edition of *Monsieur Nicolas*, Volumes V and VI.

56. *Ibid.*, 5:252.

57. *Ibid.*, 5:116.

58. *Ibid.*, 6:337, 6:430.

59. *Ibid.*, 6:400.

60. Pierre Bertrand, "Post-scriptum sur la *Philosophie de Monsieur Nicolas*," in his *Nouveaux Principes de géologie*, 521-538. Quoted in the Gallimard edition of *Monsieur Nicolas*, 2:1716-1717.

61. Restif de la Bretonne, *Monsieur Nicolas* (Pauvert edition), 5:467.

62. *Ibid.*, 5:109.

63. *Ibid.*, 5:517.

64. *Ibid.*, 5:526-527.

65. *Ibid.*, 5:521.

66. *Ibid.*, 5:465.

67. *Ibid.*, 5:293, 5:288.

68. *Ibid.*, 5:288.

69. *Ibid.*, 5:465.

70. *Ibid.*, 5:449.

71. *Ibid.*, 5:462.

72. *Ibid.*, 5:466.

73. The critique of Bernardin is from the "Cinquième Epoch" of *Monsieur Nicolas* (Gallimard edition, 1:935): "So many other works -- the *Etudes de la nature* by Bernardin, for example -- edify us every day by their supernatural optimism. That is not my aim in *The Human Heart Unveiled;* the motive behind my work is to present the life of a man as it really is." The source of the criticism of Du Pont is *Monsieur Nicolas* (Pauvert edition, 5:556).

74. Restif de la Bretonne, *Monsieur Nicolas* (Pauvert edition), 5:262 or 5:429, for example,

75. *Ibid.*, 5:243.

76. *Ibid.*, 5:463.

77. *Ibid.*, 5:267.

78. *Ibid.*, 5:378.

79. *Ibid.*, 5:206.

80. *Ibid.*, 5:279.

81. *Ibid.*, 5:286.

82. *Ibid.*, 5:463.

83. *Ibid.*, 5:125.

84. *Ibid.*, 5:378.

85. *Ibid.*, 5:374.

86. *Ibid.*, 5:374.

87. *Ibid.*, 5:349.

88. *Ibid.*, 5:328.

89. *Ibid.*, 5:329.

90. *Ibid.*, 5:338.

91. *Ibid.*, 5:353.

92. *Ibid.*, 5:268.

93. *Ibid.*, 5:547.

94. *Ibid.*, 5:522-523.

95. *Ibid.*, 5:507, 5:474.

96. *Ibid.*, 5:507. The work in question was by Benoit de Maillet (1656-1738): *Telliamed, ou, Entretiens d'un philosophe indien avec un missionnaire françois sur la diminution de la mer, la formation de la terre, l'origine de l'homme, &c.* (Amsterdam: L'honoré & fils, 1748).

97. Restif de la Bretonne, *Monsieur Nicolas* (Pauvert edition), 5:534.

98. *Ibid.*, 5:541.

99. *Ibid.*, 5:545.

100. *Ibid.*, 5:556.

101. *Ibid.*, 5:463.

102. *Ibid.*, 5:556.

103. *Ibid.*, 5:556.

104. *Ibid.*, 5:550.

105. *Ibid.*, 5:554.

106. *Ibid.*, 6:55. Restif frequently anagramized names, especially of living authors.

107. *Ibid.*, 5:554.

108. *Ibid.*, 6:44.

109. *Ibid.*, 6:33, 6:50.

110. See, for example, J. L. Talmon, *The Origins of Totalitarian Democracy* (London: Penguin Books, 1986; originally published in 1952); also, the section on Restif in Manuel and Manuel, *Utopian Thought in the Western World*, 538-555.

111. Restif de la Bretonne, *Monsieur Nicolas* (Pauvert edition), 6:355.

112. *Ibid.*, 5:561.

113. Restif de la Bretonne, *Monsieur Nicolas* (Gallimard edition), 2:1052.

NOTES TO CHAPTER SIX

1. See, for example, A. S. Packard, *Lamarck: The Founder of Evolution* (London: Longmans, 1901) and Glass, *et al.*, eds., *Forerunners of Darwin* (Baltimore: Johns Hopkins, 1959). Challenging the "forerunner" notion is Barthélemy-Madaule, *Lamarck, ou le mythe du précurseur* (Paris: Editions du Seuil, 1979).

2. Georges Cuvier, "Eloge de M. Lamarck." The eulogy was originally read by Cuvier at the Academy of Sciences on November 26, 1832. The influential English translation appeared as "Biographical memoir of M. de Lamarck" in the *Edinburgh New Philosophical Journal*, 1836. The latter version is also appended to the 1984 English edition of Lamarck, *Zoological Philosophy*.

3. Richard W. Burkhardt, *The Spirit of System: Lamarck and Evolutionary Biology* (Cambridge, Mass.: Harvard University Press, 1977), 41; the reference is to Lamarck, *Hydrogéologie* (Paris: Agasse, 1802).

4. L. J. Jordanova, *Lamarck* (Oxford: Oxford University Press, 1984), 44; Burkhardt, *Spirit of System*, 22.

5. Michel Foucault argued that there could have been no biology, or science of life, until the concept of life itself had attained its modern form at the end of the eighteenth century. *The Order of Things: An Archaeology of the Human Sciences* (New York, 1971), 127-128.

6. "How ardent a republican Lamarck was is difficult to determine." Burkhardt, *Spirit of System*, 37.

7. Barthélemy-Madaule cites the claims of Alphaeus Packard, *Lamarck, the Founder of Evolution*, but also notes that "since Packard's work, several Lamarck manuscripts have been uncovered." Barthélemy-Madaule, *Lamarck the Mythical Precursor*, 1, 153. A volume of Lamarck's previously unpublished writings were published in 1972: *Inédits de Lamarck*, edited by M. Vachon, G. Rousseau, and Y. Laissus (Paris: Masson, 1972).

8. Burkhardt, *Spirit of System*, 37; Jordanova, *Lamarck*, 7; Guerlac, "Some Aspects of Science during the French Revolution," 95; Hahn, *Anatomy of a Scientific Institution*, 169.

9. Barthélemy-Madaule, *Lamarck the Mythical Precursor*, 10-11.

10. See Desmond, *Politics of Evolution* and Appel, *Cuvier-Geoffroy Debate*.

11. Barthélemy-Madaule, *Lamarck the Mythical Precursor*, 9.

12. Burkhardt, *Spirit of System*, 10.

13. *Ibid.*, 10; the source of the story is François Arago, *Oeuvres* (Paris and Liepzig, 1854-1862), 1:94.

14. Barthélemy-Madaule, *Lamarck the Mythical Precursor*, 5.

15. Jordanova, *Lamarck*, 2.

16. The section on Marat is an appendix to the first volume of Lamarck, *Recherches sur les causes des principaux faits physiques*, 1:343-368.

17. Burkhardt, *Spirit of System*, 10.

18. Cuvier, "Biographical Memoir of M. de Lamarck," 434.

19. *Ibid.*, 452.

20. Lamarck, unpublished manuscript of of 1801 or 1802. Quoted by Burkhardt, *Spirit of System*, 41, where the source is given as Muséum National d'Histoire Naturelle, MS 756, "Physique terrestre," 1st cahier, 3.

21. Burkhardt, "The Zoological Philosophy of J. B. Lamarck," introduction to Lamarck, *Zoological Philosophy* (Chicago: University of Chicago Press, 1984), xvi.

22. *Ibid.*, xxi. The lecture Burkhardt refers to was Lamarck's "Discours d'ouverture" that he delivered on May 11, 1800, at the Museum. See *Zoological Philosophy*, 407-433.

23. Thomas Henry Huxley, "Evolution in Biology," in *Darwiniana. Essays by T. H. Huxley* (New York: Appleton & Co., 1896), 212; quoted by Barthélemy-Madaule, *Lamarck the Mythical Precursor*, 18.

24. Burkhardt gives this estimation of Cuvier's role: "Precisely how much Cuvier's treatment of Lamarck's evolutionary ideas influenced contemporary naturalists is impossible to determine, but there is no question that it was a weighty example for a vast majority of the naturalists of the day." Burkhardt, *Spirit of System*, 201.

25. Coleman, *Georges Cuvier*, 1.

26. Gillispie, "Lamarck and Darwin in the History of Science."

27. *Ibid.*, 273, 274.

28. *Ibid.*, 266. Burkhardt has argued that this perhaps overstates the originality of Lamarck's contributions to geology; he suggests that Lamarck's friend Jean-Guillaume Bruguière deserves at least some credit for being Lamarck's "Lyell." Burkhardt, *Spirit of System*, 109.

29. Gillispie, "Lamarck and Darwin in the History of Science," 266-267.

30. *Ibid.*, 286.

31. *Ibid.*, 275.

32. Gillispie elsewhere acknowledges that the "romantic tradition" was not sterile: "Biological studies received a tremendous impulse from the romantic attempt to replace physics by biology as the ordering science." Gillispie, *Edge of Objectivity*, 156.

33. See also Barthélemy-Madaule, *Lamarck, ou le mythe du précurseur*.

34. Burkhardt, *Spirit of System*, 7.

35. Coleman, *Georges Cuvier*, 1.

36. Appel, *Cuvier-Geoffroy Debate*, 49.

37. Herivel, "Aspects of French Theoretical Physics," 109. This should not be taken to imply that hesitation in accepting Darwin's theory in the nineteenth century was necessarily an indication of wrongheadedness.

38. Lamarck, *Recherches sur les causes des principaux faits physiques* (Paris: Chez Maradan, 1794), 1:v-vi.

39. *Ibid.*, 1:10.

40. *Ibid.*, 1:12.

41. *Ibid.*, 2:214.

42. *Ibid.*, 2:313-314.

43. Lamarck, *Mémoires de physique et d'histoire naturelle* (Paris: Agasse, 1797).

44. Lamarck, "Discours d'ouverture."

45. Lamarck, *Réfutation de la théorie pneumatique* (Paris: Agasse, 1796), 8.

46. Lamarck, *Recherches sur les causes des principaux faits physiques*, 1:7.

47. *Ibid.*, 1:316.

48. *Ibid.*, 2:314.

49. *Ibid.*, 2:376.

50. *Ibid.*, 2:46.

51. *Ibid.*, 2:46.

52. *Ibid.*, 2:47-48.

53. *Ibid.*, 1:14.

54. *Ibid.*, 1:143.

55. *Ibid.*, 1:320.

56. *Ibid.*, 1:175.

57. *Ibid.*, 1:144.

58. *Ibid.*, 1:175.

59. *Ibid.*, 1:330-331.

60. *Ibid.*, 1:316-317.

61. The first three volumes of Buffon's monumental *Histoire naturelle* appeared in 1749 as *Histoire Naturelle générale et particulière avec la description du cabinet du roi*. He would live to see thirty-six volumes published; his collaborators continued the project after his death, bringing the final total to forty-four.

62. Gillispie, *Science and Polity*, 158.

63. Appel, *Cuvier-Geoffroy Debate*, 38.

64. Gillispie, *Science and Polity*, 161; Burkhardt, *Spirit of System*, 116.

65. Dhombres and Dhombres, *Naissance d'un nouveau pouvoir*, 761.

66. Burkhardt, *Spirit of System*, 199.

67. Lacépède, *Essai sur l'électricité naturelle et artificielle* (Paris: Impr. de Monsieur, 1781), 1:4-5.

68. *Ibid.*, 2:313.

69. References to Marat are in *Ibid.*, 1:10, 1:39-40; the reference to Marivetz is in *Ibid.*, 2:378.

70. *Ibid.*, 1:18.

71. *Ibid.*, 1:18.

72. *Ibid.*, 1:30.

73. *Ibid.*, 1:49.

74. *Ibid.*, 1:53; emphasis in original.

75. *Ibid.*, 1:53.

76. *Ibid.*, 1:315.

77. *Ibid.*, 1:318, 324.

78. *Ibid.*, 1:223; emphasis in original.

79. *Ibid.*, 1:221.

80. *Ibid.*, 2:91.

81. *Ibid.*, 2:93.

82. *Ibid.*, 2:144.

83. *Ibid.*, 2:158; emphasis in original.

84. *Ibid.*, 1:5.

85. *Ibid.*, 2:194.

86. *Ibid.*, 2:300.

87. *Observations sur la physique, sur l'histoire naturelle et sur les arts* was edited by the Abbé François Rozier from its inception in 1771 to 1780, when his nephew J. A. Mongez assumed the editorship. Mongez, who disappeared with Lapérouse's ill-fated final expedition, was succeeded by Delamétherie in 1785; in "l'an II" the periodical began to appear under a new name: *Journal de physique, de chimie et d'histoire naturelle et des arts.*

88. James McClellan, "The Scientific Press in Transition: Rozier's Journal and the Scientific Societies in the 1770s," *Annals of Science*, 36 (1979), 427.

89. *Ibid.*, 448.

90. Henry Guerlac, *Antoine-Laurent Lavoisier: Chemist and Revolutionary* (New York: Charles Scribner's Sons, 1975), 106. *Observations* is an abbreviated version of the earlier name of the *Journal de physique* (see note 87, above).

91. Arthur Young, *Travels During the Years 1787, 1788 and 1789* (Bury St. Edmonds, 1792), 261. This entry is dated December 28, 1789. Quoted by McClellan, "The Scientific Press in Transition," 435.

92. H. M. Ducrotay de Blainville, *Histoire des sciences de l'organisation et de leurs progrès, comme base de la philosophie* (Paris: Perisse frères, 1845). Sharp criticism induced Blainville to include Cuvier in later editions.

93. Hahn, *Anatomy of a Scientific Institution*, 181.

94. Duveen and Klickstein, *Bibliography of the Works of Antoine Laurent Lavoisier*, 129.

95. See Lavoisier, *Traité élémentaire de Chimie* (Paris, 1789).

96. Institut de France, Académie des Sciences, *Procès-Verbaux des Séances de l'Académie tenues depuis la fondation de l'Institut jusqu'au mois d'août 1835*. Vol. I (1795-1799), 359. "Lamétherie" is an alternate spelling of Delamétherie. Antoine Baumé (1728-1804) and Baltazar Georges Sage (1740-1824) entered the Academy of Sciences as *chimistes* in 1772 and 1770, respectively.

97. Lamarck, *Réfutation de la théorie pneumatique* (see the section on Lamarck in this chapter).

98. Some of them were: *Vues physiologiques sur l'organisation animale et végétale* (1780); *Essai analytique sur l'air pur, et les différentes espèces d'air* (1785); *Théorie de la terre* (1795); *Leçons de minéralogie, données au Collège de France* (1812); *Leçons de géologie, données au Collège de France* (1816).

99. J. C. Delamétherie, *De la nature des êtres existans, ou principes de la philosophie naturelle* (1805); *De l'homme considéré moralement; de ses moeurs, et de celles des animaux* (1802).

100. Hahn, *Anatomy of a Scientific Institution*, 179.

101. H. M. D. de Blainville, "Notice historique sur la vie et les écrits de J.-C. Delamétherie," *Journal de physique*, Vol. 85 (1817), 78-107.

102. Georges Cuvier, "Métherie (Jean-Claude de La)," in Michaud, ed., *Biographie Universelle*, Vol. 28 (Paris, 1854; reprinted, 1968; the first edition appeared in 1821), 122. This judgment was repeated almost *verbatim*, without attribution, in the entry on Delamétherie in the *Nouvelle Biographie Générale*, Vol. 29, col. 211 (Paris, 1859; reprinted, 1967).

103. *Ibid.*, 123.

104. *Ibid.*, 122.

105. To Cuvier's credit, when the elderly Delamétherie was impoverished and without family, Cuvier provided him some financial assistance.

106. Delamétherie, *Principes de la philosophie naturelle* (Geneva, 1787), 2:193.

107. *Ibid.*, 2:193-194.

108. *Ibid.*, 2:194.

109. *Ibid.*, 2:22.

110. *Ibid.*, 2:22.

111. *Ibid.*, 2:206.

112. *Ibid.*, 1:xiii.

113. *Ibid.*, 1:xv.

114. *Ibid.*, 1:xv.

115. *Ibid.*, 1:xviii.

116. *Ibid.*, 1:2.

117. *Ibid.*, 1:9.

118. *Ibid.*, 1:23.

119. *Ibid.*, 1:64.

120. *Ibid.*, 1:63.

121. *Ibid.*, 2:5.

122. *Ibid.*, 2:160.

123. *Ibid.*, 2:194-195.

124. *Ibid.*, 2:293-294.

125. *Ibid.*, 2:297.

126. *Ibid.*, 2:301.

127. *Ibid.*, 2:309-310.

128. *Ibid.*, 2:111-112.

129. *Ibid.*, 2:113. René Antoine Ferchault de Réaumur (1693-1757), physicist and naturalist, was elected to the Academy of Sciences in 1708 and became one of its most prominent members. An intense rivalry between Réaumur and Buffon loomed large in the Academy's internal politics.

130. *Ibid.*, 2:117-118.

131. *Ibid.*, 2:120.

132. *Ibid.*, 2:127.

133. *Ibid.*, 2:127.

134. *Ibid.*, 2:109.

135. *Ibid.*, 2:130.

136. *Ibid.*, 1:278.

137. *Ibid.*, 1:80.

138. *Ibid.*, 2:423.

139. *Ibid.*, 1:229.

140. *Ibid.*, 1:267.

141. *Ibid.*, 1:268.

142. That the Jacobin Republic was necessary to solidify the gains of the Revolution is a traditional interpretation associated with such historians as George Lefebvre and Albert Soboul, but not all historians of the French Revolution concur. "Revisionist" historians such as François Furet argue that the great seigneurs voluntarily renounced most of their privileges in the famous session of the National Assembly on the night of August 4, 1789, less than a month after the fall of the Bastille. The privileges or property rights that the nobles were giving up, however, had already come under sharp attack by the peasants'

revolt. The aristocrats were hoping by their action to win compensation for property rights they very likely stood to lose anyway. The National Assembly backed their requests; its decrees would have required the peasants to pay for their emancipation. Since few peasants had the resources to pay such compensation, they were faced with being in perpetual debt bondage. The peasants, understandably, were not satisfied and continued their struggle (often in open rebellion to the point of civil war) until 1793, when the Jacobin Convention declared the peasants' redemptive debts null and void.

143. The fifth, sixth, and seventh volumes were all on the same subject (heat), and therefore were designated as three parts of Volume V.

144. Michaud, ed., *Biographie Universelle*, "Marivetz (Etienne-Claude, baron de)," Vol. 26, 691-692; "Goussier (Louis-Jacques)," Vol. 17, 262-263. The phrase "savant estimible" is used to describe Goussier, but Marivetz is depicted in equally favorable terms.

145. They made a point of saying that those readers who could not accept their system or their physical principles should not dismiss the utility of their "physical exposition of present and future modifications of the surface of France and, consequently, all of the conclusions that we have drawn with regard to the interior navigation of the Realm." Marivetx and Goussier, *Physique du monde* (Paris: Quillau, 1780-1787), 1:cxv. See also: Marivetz, *Observations sur quelques objets d'utilité publique, pour servir de prospectus à la seconde partie de la "Physique du monde", ou à la "Carte hydrographique de la France", et au "Traité général de la navigation intérieure de ce royaume"* (Paris: Visse, 1786).

146. Michaud, ed., "Marivetz," 692.

147. J. J. Lalande, *Bibliographie astronomique, avec l'Histoire de l'astronomie depuis 1781 jusqu'à 1802* (Paris: l'Imprimerie de la République, 1803), 753-754.

148. For examples of polite but noncommittal reviews, see *Journal d'histoire naturelle*, Vol. VIII (1789), Nos. 11-12, 158-160, and *Journal de physique*, Vol. XVIII (Sept. 1781), 236-239. For examples of replies to hostile reviews, see the front matter to volumes III, IV, and V of *Physique du monde*.

149. Bailly, *Histoire de l'astronomie moderne* (Paris: Les frères de Bure, 1779-1782), 3:267-268; Marivetz, *Lettre de M. le B^on de Marivetz, à M. Bailly . . . sur un paragraphe du 3^e volume de l'"Histoire de l'astronomie moderne"* (Paris: Quillau, 1782); also included in *Physique du monde*, 3:11-24.

150. J. L. Carra, *Nouveaux Principes de physique*, 2:186-238.

151. Lacépède, *Essai sur l'électricité naturelle et artificielle*, 2:378.

152. "Lettre à M. le Comte de la Cépède sur l'élasticité," *Physique du monde*, 4:1-62, is Marivetz's reply to arguments contained in the first volume of Lacépède, *Physique générale et particulière*.

153. Michaud, ed., "Marivetz," 692.

154. Marivetz and Goussier, *Physique du monde*, 1:58-248.

155. *Ibid.*, 4:1-62.

156. *Ibid.*, 4:440-496.

157. *Ibid.*, 6:373-424.

158. *Ibid.*, 7:363-398. The work by Delamétherie that was cited was his *Essai analytique sur l'air pur et les différentes espèces d'air* (Paris, 1785).

159. Marivetz and Goussier, *Physique du monde*, 7:373.

160. *Ibid.*, 1:26-27.

161. Delamétherie, however, seemed to have completely dropped his opposition to attractionism by the time he published the revised version of his natural philosophy after the turn of the century. See, for example, his discussion of "the nature of force" in *De la Nature des êtres existans, ou Principes de la philosophie naturelle* (Paris, 1805), 45-47.

162. Marivetz and Goussier, *Physique du monde*, 3:xvi (emphasis in original).

163. Condillac, *Traité des systêmes* (Paris: Libraires associées, 1778), 5-6.

164. Jean d'Alembert, *Preliminary Discourse to the Encyclopedia of Diderot*, 22-23.

165. *Ibid.*, 95.

166. Marivetz and Goussier, *Physique du monde*, 1:lxxxviii.

167. *Ibid.*, 4:61-62 (emphasis in original).

168. *Ibid.*, 3:54.

169. *Ibid.*, 3:58.

170. *Ibid.*, 2:253.

171. J. S. Bailly, *Histoire de l'astronomie moderne*, 3:267-268; quoted in *Physique du monde*, 3:15.

172. Marivetz and Goussier, *Physique du monde*, 3:17. This "Lettre de M. le B^{on} de Marivetz, à M. Bailly . . ." was also published separately as a fourteen-page pamphlet.

173. Marivetz and Goussier, *Physique du monde*, 1:lxxxix-xc.

174. *Ibid.*, 1:xci.

175. *Ibid.*, 4:1-62. Marivetz was responding to the first volume of Lacépède, *Physique générale et particulière*; the second volume had not yet appeared.

176. Marivetz and Goussier, *Physique du monde*, 4:11.

177. *Ibid.*, 4:50-51.

178. Marivetz mentions, for example, a letter he received from Marat dated November 1, 1783, discussing an experiment Marat had performed "chez M. de Joubert." *Physique du monde*, 4:470. The expositions of Marat's work are in 4:440-496, and 5:373-424.

179. *Ibid.*, 3:168.

NOTES TO CHAPTER SEVEN

1. This is sometimes called "Planck's principle," after Max Planck, who observed: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it." Planck, *Scientific Autobiography and Other Papers* (1949). The same can be said for general attitudes toward science.

2. Delamétherie, *De l'homme considéré moralement* (Paris, 1802); *De la nature des êtres existans* (Paris, 1805).

3. P. J. G. Cabanis, *Rapports du physique et du moral de l'homme* (second edition, Paris: Crapart, Caille et Ravier, 1805).

4. Martin S. Staum, *Cabanis: Enlightenment and Medical Philosophy in the French Revolution* (Princeton, N.J.: Princeton University Press, 1980), 298-299.

5. *Ibid.*, p. 299. The letter was later published as *Lettre (posthume et inédite) de Cabanis à M. F. . . . sur les causes premières* (1824).

6. Roger Henry Soltau, *French Political Thought in the 19th Century* (New York: Russell & Russell, 1959), 151.

7. Quoted in Jonathan Beecher and Richard Bienvenu, eds., *The Utopian Vision of Charles Fourier* (Boston: Beacon Press, 1971), 157.

8. Darnton, *Mesmerism*, 143.

9. Beecher and Bienvenu, eds., *Utopian Vision of Fourier*, xiii.

10. Charles Fourier, *Harmonian Man: Selected Writings of Charles Fourier*, Mark Poster, ed. (Garden City, N.Y.: Doubleday, 1971), 51.

11. *Ibid.*, 8. Poster is also the author of *The Utopian Thought of Restif de la Bretonne* (New York: New York University Press, 1971). The famous French socialist Pierre Leroux attributed Fourier's system in its entirety to Restif in "Lettres sur le fourierisme," 2ª partie, *Revue sociale*, mars 1850, 103-108. Poster points out that although Leroux had a Saint-Simonian ax to grind, his charge of plagiarism against Fourier "has been reiterated by most scholars who have written on Restif." Poster

himself concludes: "It would be difficult to deny a direct line of intellectual transmission from Restif to Fourier." Poster, *Utopian Thought of Restif de la Bretonne*, 133, 134.

12. Arthur Lovejoy has pointed out that the "notion of an *Urbild*, upon which all organized and perhaps all natural forms are variations, which was . . . to become almost an obsession of Goethe" had first been elaborated by the preromantic French natural philosopher Jean Baptiste Robinet. Lovejoy, *Great Chain of Being*, 279-280. See Robinet, *De la nature* (Amsterdam: E. van Harrevelt, 1763-1766).

13. Appel, *Cuvier-Geoffroy Debate*, 9.

14. See Philip F. Rehbock, "Transcendental Anatomy" in *Romanticism and the Sciences*, Cunningham and Jardine, eds. (Cambridge: Cambridge University Press, 1990), 144-160. "Higher anatomy," "philosophical anatomy," "transcendental morphology," and "transcendental anatomy" were all more or less interchangeable terms. "The distinguishing characteristics of transcendental anatomy," according to Rehbock, "were (a) the presupposition that a single Ideal Plan or Type (or, at most, a few such plans) lay behind the great multiplicity of visible structures in the animal and plant kingdoms, and that this Plan determined an organism's functional capacities rather than being determined by them; (b) the further presupposition that the Ideal Plan acted as a force for the maintenance of anatomical uniformity, in opposition to the diversity-inducing (some would argue *degenerating*) forces of the physical environment; (c) the belief that this *a priori* Plan, though it had no physical existence in its pure state, was nevertheless discoverable; and (d) the aspiration to discover additional concepts ('laws') which would support and elaborate the Ideal Plan by specifying how apparent anatomical diversities may be seen as uniformities." Rehbock, "Transcendental Anatomy," 144-145; emphasis in original.

15. On Raspail, see Dora Weiner, *Raspail: Scientist and Reformer* (New York: Columbia University Press, 1968).

16. Desmond, *The Politics of Evolution*.

NOTES TO CHAPTER EIGHT

1. Quoted by Jean Massin, *Marat* (Paris, 1970), 11-12.

2. Phipson, *Marat: His Career in England and France Before the Revolution* (London: Methuen, 1924), 16, 48, 86.

3. *Ibid.*, v.

4. *Ibid.*, 134.

5. *Ibid.*, 83.

6. Marat, *Correspondance*, Charles Vellay, ed. (Paris: Charpentier et Fasquelle, 1908), 1-2.

7. There is no evidence that Marat was ever aware of being suspected of any such crime.

8. J. M. Thompson, *Leaders of the French Revolution* (Oxford: Basil Blackwell, 1988; originally published in 1929), 167.

9. Robert Darnton, "Marat n'a pas été un voleur," *Annales historiques de la Révolution française* 38 (1966), 447-450.

10. Phipson, *Marat: His Career in England and France Before the Revolution*, 106.

11. Cabanès, *Marat Inconnu: L'Homme privé, le médecin, le savant* (Paris, 1891).

12. Phipson, *Marat: His Career in England and France Before the Revolution*, vi.

13. Marat's biographers have included Bougeart (1865), Chèvremont (1880), Gottschalk (1927), Walter (1933), and Massin (1970).

14. Gottschalk, *Marat* (Chicago: University of Chicago Press, 1967; originally published in 1927), vi.

15. *Ibid.*, v.

16. Marat's periodical began publication on September 12, 1789, as *Publiciste parisien*, but changed its name four days later to the *Ami du peuple*; on September 25, 1792, it became *Journal de la République française*, and on March 9, 1793, *Publiciste de la Révolution française*. See also *Pamphlets de Marat* edited by Charles Velay. For comparison, see other contemporary radical papers such as the

Révolutions de Paris (Lousallot *et al.*), the *Révolutions de France et de Brabant* (Desmoulins), the *Annales Patriotiques* (Carra), and above all, *Père Duchesne* (Hebert). For a representative sample of these in English, see J. Gilchrest and W. J. Murray, *The Press in the French Revolution* (New York: St. Martin's Press, 1971).

17. Douglas McKie, *Antoine Lavoisier, Scientist, Economist, Social Reformer* (New York: Schuman, 1952), 318.

18. Charles W. Burr, "Jean Paul Marat, Physician, Revolutionary, Paranoaic," *Annals of Medical History,* Vol. III (1919), 248-261. For an example of a reputable historian uncritically accepting Burr's diagnosis, see Norman Hampson, *Will and Circumstance: Montesquieu, Rousseau and the French Revolution* (Norman, Oklahoma: University of Oklahoma Press, 1983), 119.

19. Burr, "Jean Paul Marat, Physician, Revolutionary, Paranoaic," 260.

20. *Ibid.*, 259.

21. *Ibid.*, 257.

22. See especially Bertholon's letter to Antoine Buissart, December 15, 1782. Reproduced in *Revue historique de la Révolution,* Vol. III, 1912, 294-297. Bertholon's advice with regard to Marat is: "I believe it punishes this man more not to be answered at all . . . . Please do not refute him."

23. Marat, *An Essay on Gleets* and *An Enquiry into the Nature, Cause and Cure of a Singular Disease of the Eyes.* Reproduced in *Reprint of Two Tracts by Jean Paul Marat, M.D.*, James Blake Bailey, ed. (London: Percival & Co., 1891).

24. Artois would later place himself at the head of the émigré army attempting to crush the Revolution. After the Restoration he headed the ultraroyalist faction and finally took the throne himself as Charles X. He was deposed by the revolution of 1830.

25. Heilbron said that Marat had been "doctor to the grooms, or perhaps the horses, of the Duc d'Artois." J. L. Heilbron, *Electricity in the 17th and 18th Centuries* (Berkeley: University of California Press, 1979), 429. In 1927, however, Gottschalk had already noted that "a glance at the *Almanac Royal*" serves to disprove the claim: "There he is listed as Doctor of the Body-Guard of the Count of Artois until the appointment of his successor in 1786, although he resigned in 1783." Gottschalk, *Marat*, 9.

26. See "Le Berger Daubenton: Encore une légende contre-révolutionnaire," in James Guillaume, *Etudes révolutionnaires*, 178-197.

27. See Charles Vellay, ed., "Supplément à la correspondance de Marat," *Revue historique de la Révolution française*, Vol. I (1910).

28. This report also appeared in the *Gazette de Santé* (January 1, 1778). Its author was Henri-Alexandre Tessier (1741-1837); he was admitted to the Academy of Sciences in 1783.

29. Robert Darnton, *The Business of Enlightenment: A Publishing History of the Encyclopédie, 1775-1800* (Cambridge, Mass.: Harvard University Press, 1979), 19. For Rey's relationship with Rousseau, see Maurice Cranston, *The Noble Savage: Jean-Jacques Rousseau, 1754-1762* (Chicago: University of Chicago Press, 1991).

30. *Gazette de politique et de littérature*, May 5, 1777.

31. *Ami du peuple*, May 11, 1791.

32. Marat, *An Essay on Gleets* and *An Enquiry into the Nature, Cause and Cure of a Singular Disease of the Eyes*.

33. See Bertholon, *De l'électricité du corps humain dans l'état de santé et de maladie* (Paris: Didot, 1780; second edition, 1786).

34. The three works Weigel translated were those that are discussed in Chapters Nine, Ten, and Eleven, below.

35. *Journal de physique*, Vol. XVIII, Part I (April 1781), 317-320; (June 1781), 459-465.

36. *Journal de Médicine*, Vol. LV (1781), 475.

37. *Journal de littérature, des sciences et des arts*, Vol. VI (1780), 192-204 and 247-258.

38. *Courrier de l'Europe*, March 15, 1782, 170; May 3, 1782, 286.

39. *Journal de Paris*, June 9, 1780, 659; November 12, 1780, 1290.

40. *Journal Encyclopédique*, June 1780, Vol. IV, 532-533; January 1781, Vol. I, 38.

41. *L'Année littéraire*, Vol. XXIX (1782), 283-288.

42. *Nouvelles de la république des lettres*, January 30, 1782, 35-36.

43. For an apparently exhaustive list of such reviews, see N. B. Mandelbaum, *Marat: Rebel as Savant* (Unpublished dissertation: Columbia University, 1977), 511-513.

44. Jean Baptiste Louis Romé de l'Isle, *L'Action du feu central* (Paris, 1779); Baltazar Georges Sage, *Institutions de physique* (Paris, 1811), Jan Heindrich Van Swinden, *Recueil de mémoires sur l'analogie de l'électricité et du magnétisme* (La Haye, 1784). For Lamarck, Lacépède, Marivetz, and Carra, see the sections on them in Chapters Four and Six, above.

45. Mandelbaum has listed 12 such essays (*Marat: Rebel as Savant*, 382), but an exact count is not possible because some are known only indirectly and some anonymous memoirs later attributed to Marat may not in fact have been his.

46. Published in 1784 as *Mémoire sur l'électricité médicale* (Paris: Méquignon, 1784).

47. Both were published in Marat, *Mémoires académiques, ou Nouvelles Découvertes sur la lumière, relatives aux points les plus importans de l'optique* (Paris, 1788).

48. This essay, too, was published in *Mémoires académiques*.

49. According to Mandelbaum, a Spanish Academy of Sciences had existed in name since 1774, but had been inactive. Mandelbaum, *Marat: Rebel as Savant*, 369.

50. Letter from the Comte d'Aranda to Floridablanca, September 19, 1783. Translated and quoted by Mandelbaum, *Marat: Rebel as Savant*, 374.

51. See Marat's correspondence with Franklin from 1779 to 1783 in *Revue historique de la Révolution*, Vol. III, 1912, 353-361, and *Correspondance de Marat*, 81-82. Marat's solar microscope will be discussed in Chapter Nine.

52. *Registres de l'Académie des Sciences*, 1779, 97-100. Marat published it at the beginning of his *Découvertes de M. Marat sur le feu, l'électricité, et la lumière* (Paris: Clousier, 1779). The report can also be found in the appendices to Cabanès' *Marat Inconnu*, third edition, 511-514.

53. Marat published the report, followed by his own comments, at the beginning of his *Découvertes sur la lumière* (Paris, Jombert, 1780), 3-6.

54. Brissot, *Mémoires, 1754-1793* (Paris: Picard & Fils, no date, *c.* 1912), 1:201.

55. Letter from Paté to Marat, February 23, 1780, in Marat, *Correspondance*, 73.

56. Letter from Paté to Marat, June 3, 1781, in Marat, *Correspondance*, 73-74.

57. See announcements in the *Journal de Paris*, April 6, 1780 (397-398); April 16, 1780 (441-442); May 2, 1780 (506); June 4, 1780 (638).

58. *Journal de Paris*, July 5, 1782 (761).

59. *Journal de Paris*, April 11, 1783 (420).

60. More will be said about Marat's relationship with Brissot later in this chapter.

61. Louis Elisabeth de la Vergne de Broussin, Comte de Tressan (1705-1783); see his letter to Marat, July 24, 1780, in Marat, *Correspondance*, 67-68. Tressan, *Essai sur le fluide électrique, considéré comme agent universel* (Paris: Buisson, 1786) was published posthumously.

62. Condorcet, *Eloges des Académiciens de l'Académie Royale des Sciences* (Paris: Chez Vieweg et Fuchs, 1799), 3:440-456.

63. The review was in the *Journal de physique*, February 1788. Marat's two letters to Delamétherie, dated March 18 and March 26, are in Marat, *Correspondance*, 93-95.

64. See especially Claudius Roux, *Marat et l'Académie de Lyon, d'après des documents inédits* (Lyon: M. Audin, 1923).

65. L. F. Maury, *L'Ancienne Académie des Sciences* (Paris: Didier, 1864), 174.

66. Isaac Asimov. *Asimov's Biographical Encyclopedia of Science and Technology* (Garden City, New York: Doubleday, 1982), 223.

67. Cabanès, *Marat Inconnu.*

68. Hugo Rozbroj, *Jean Paul Marat (1742-1793), ein Naturforscher und Revolutionär, sein Zusammentreffen in der Geistewelt mit Goethe, Lamarck, Rousseau, u.a.* (Berlin: Verlag dr. Emil Ebering, 1937).

69. J. W. Dauben, "Marat: His Science and the French Revolution," *Archives internationales d'histoire des sciences*, 88-89 (1969), 235-261.

70. Mandelbaum, *Marat: Rebel as Savant*, 179.

71. *Ibid.*, 444. On Jean Baptiste Louis Romé de l'Isle (1736-1790) and the significance of his contributions to crystallography, see John G. Burke, *Origins of the Science of Crystals* (Berkeley: University of California Press, 1966), Chapter Three.

72. Mandelbaum, *Marat: Rebel as Savant*, 356.

73. Gillispie, *Science and Polity*, 258, 291.

74. *Ibid.*, 296.

75. *Ibid.*, 296.

76. *Ibid.*, 319.

77. *Ibid.*, 311-312. *Recherches physiques sur le feu* will be the subject of Chapter Nine, below. The "companion on light" is *Découvertes sur la lumière*, which will be the subject of Chapter Ten.

78. *Ibid.*, 312.

79. *Ibid.*, 324.

80. *Ibid.*, 314.

81. *Ibid.*, 314.

82. *Ibid.*, 312.

83. Brissot, *Mémoires*, 203.

84. For example: "He did a new translation of Newton's *Opticks*, adding notes and altering the text in such a way as to overturn the great man's theory. Then he went to the grammarian Beuzée, knowing his weakness of character, and managed to get Beuzée to put his name to this weapon, hiding its purpose from him no doubt." Maury, *L'Ancienne Académie des Sciences*, 174-175.

85. Gillispie, *Science and Polity*, 320. It should be noted that Gottschalk, in his 1927 biography of Marat, also noted that his translation of the *Opticks* was "faithfully executed." Gottschalk, *Marat*, 28.

86. Gillispie, *Science and Polity*, 321.

87. See Chapter One above.

88. Gillispie, *Science and Polity*, 307-308.

89. *Ibid.*, 328.

90. Mandelbaum is an exception, but Gillispie does not cite his work, which was unpublished.

91. *Offrande à la patrie, ou discours au tiers état de France*, in Marat, *Les Pamphlets de Marat*, Charles Vellay, ed. (Paris: Charpentier et Fasquelle, 1911), 1-35.

92. *Les Charlatans modernes*, in Marat, *Pamphlets*, 255-296.

93. Marat, *Correspondance*, 31.

94. Brissot, *De la vérité, ou Méditations sur les moyens de parvenir à la vérité dans toutes les connaissances humaines* (Neuchâtel: Société Typographique, 1782), 333-340.

95. Brissot, *Mémoires*, 198.

96. "These ruminations are but an introduction to a larger work -- the first stone, the foundation, of an edifice so vast that even the blueprints are astounding." Brissot, *De la vérité*, 1.

97. Brissot's natural philosophy was clearly attuned to the Rousseauian ideal: "The welfare of humanity -- that is the only criterion that should distinguish true science from false." Brissot, *De la vérité*, 7. "*Morale*, or the science of man, seems to me the first of all sciences . . . . Political science comes next . . . . Physics is necessarily subordinate to those two primary sciences." *Ibid.*, 8.

98. See the section on Bergasse in Chapter Six, above.

99. The reference to barricades is figurative; the famous barricades that have come to symbolize insurrectionary risings in Paris did not make their first appearance until 1827. See Georges Duveau, "The Barricades," in *1848: The Making of a Revolution* (Cambridge, Mass.: Harvard University Press, 1984), 161-181.

100. Gottschalk bases that conclusion on "the memoirs of several persons who witnessed these events and contemporary opinion in general." Gottschalk, *Marat*, 165.

101. Brissot, *De la vérité*, 77. See Chapter Ten, below, for Marat's theory of colors.

102. Brissot, *De la vérité*, 173.

103. *Ibid.*, 174.

104. On Bonneville and Mirabeau, see the section on Restif de la Bretonne in Chapter Five above.

105. Brissot, *De la vérité*, 332.

106. *Ibid.*, 333-340.

107. Marat's assassination occurred on July 13, 1793; Lavoisier's execution on May 8, 1794.

108. See Gene Brucker, *Jean Sylvain Bailly: Revolutionary Mayor of Paris* (Westport, Connecticut: Greenwood Publishers, 1984).

109. See McKie, *Lavoisier*, 374.

110. The book was Jean Jallabert, *Expériences sur l'électricité* (Paris, 1749). It is now in the Lavoisier collection at Cornell University. There seems to have been no question as to the inscription's authenticity.

NOTES TO CHAPTER NINE

1. See *The Presocratic Philosophers*, edited by G. S. Kirk, J. E. Raven, and M. Schofield, second edition (Cambridge: Cambridge University Press, 1987), 197-198.

2. W. P. D. Wightman, "The Burning Question," Chapter XVI of his *The Growth of Scientific Ideas* (New Haven, Conn.: Yale University Press, 1953), 176-196.

3. Bachelard, *The Psychoanalysis of Fire*, 2-3.

4. *Ibid.*, 2; emphasis in original.

5. In the nineteenth century, Faraday considered "What is fire?" an appropriate question to stimulate the interest of young people in science; the topic became a staple of his Juvenile, or "Christmas" lectures. See *Faraday's Chemical History of a Candle* (Chicago: Chicago Review Press, 1958).

6. Marat, *Recherches physiques sur le feu* (Paris: Jombert, 1780), 17.

7. Joseph Black, *Lectures on the Elements of Chemistry*, edited and published by John Robison (Edinburgh, 1803).

8. For example, Adair Crawford, *Experiments and Observations on animal Heat and the Inflammation of Combustible Bodies* (London, 1779), and J. H. de Magellan, *Collection de différens traités sur des instruments d'astronomie, physique* (London, 1980).

9. Black, *Lectures on the Elements of Chemistry.* Quoted by Duane Roller in "The Early Development of the Concepts of Temperature and Heat," *Harvard Case Studies in Experimental Science*, J. B. Conant, ed. (Cambridge, Mass.: Harvard University Press, 1957), 1:152.

10. See especially I. B. Cohen, *Franklin and Newton* (Philadelphia: American Philosophical Society, 1956), Chapters 6 and 7, and Guerlac, *Newton on the Continent* (Ithaca: Cornell University Press, 1981).

11. I. B. Cohen, *Franklin and Newton*, 143.

12. *Ibid.*, 145.

13. Stahl first stated his phlogiston theory in a lengthy commentary on Becher's *Physica subterranea*, which Stahl republished in 1703.

14. See the section on Delamétherie in Chapter Six, above.

15. See the section on Lamarck in Chapter Six, above.

16. Lavoisier, *Elements of Chemistry*, 175.

17. Lavoisier, "Mémoire sur quelques fluides qu'on peut obtenir dans l'état aériforme, à un degré de chaleur peu supérieur à la température moyenne de la terre," in *Mémoires de l'Académie Royale des Sciences*, 1780 (actually published in 1784), 334-343.

18. Marat, *Recherches physiques sur le feu*, 1.

19. *Ibid.*, 3.

20. *Ibid.*, 4.

21. *Ibid.*, 8; emphasis in original.

22. *Ibid.*, 8.

23. *Ibid.*, 8-11.

24. *Ibid.*, 12-14.

25. *Ibid.*, 14.

26. *Ibid.*, 17.

27. *Ibid.*, 17.

28. Black, *Lectures on the Elements of Chemistry*, 31.

29. Marat, *Recherches physiques sur le feu*, 18.

30. Marivetz, *Physique du monde*, "Supplément," Plate XV, illustrations 115 through 120; the notes to these illustrations are on pages 10 through 15.

31. Marat, *Recherches physiques sur le feu*, 18.

32. *Ibid.*, 18-19.

33. *Ibid.*, 19.

34. *Ibid.*, 19-20.

35. Priestley, *History and Present State of Discoveries relating to Vision, Light, and Colours* (London: J. Johnson, 1772), 741-742. Priestley gives the source as "Phil. Trans. ab. Vol. 3, p. 127."

36. *Ibid.*, 801. Priestley cites Brander's pamphlet as *Kurtze Beschribung, &c: eines Sonnen Microscops*, Ausburg, 1769. Another account of the same device "may also be seen in Martin's Gentlemen's Philosophy, Vol. 2, p. 249."

37. Marat, Letter to Roume de Saint-Laurent, November 20, 1783, in *Correspondance*, 39.

38. The codicil to Marat's will is a signed, undated, four-page document that is part of the Olin Library's collection of manuscripts (Cornell University). "Bettancourt" may have been the Spanish engineer Agustin Bethencourt y Molina (1758-1824), alias Betancourt, but he was not elected a corresponding member of the Academy until 1809.

39. Yves Marie Desmarets, comte de Maillebois (1715-1791), was an honorary member of the Academy of Sciences from 1749.

40. "Report of the Commission named by the Academy of Sciences to examine Marat's discoveries," *Registres de l'Académie des Sciences*, 1779, 97-100.

41. *Ibid.*

42. Marat, *Découvertes de M. Marat sur le feu, l'électricité, et la lumière.*

43. Quoted by Hahn, *Anatomy of a Scientific Institution*, 158. The source is given as: Bibliothèque de l'Institute, MS. 876, fols. 95-96.

44. *Report of Dr. Benjamin Franklin and other Commissioners Charged by the King of France, with the Examination of the Animal Magnetism as Now Practised at Paris* (London, 1785), 31. The report was signed by Lavoisier and Bailly, among others.

45. Lamarck, *Recherches sur les causes des principaux faits physiques*, 1:343-368. This critique was not based on Marat's *Recherches physiques sur le feu*, but on his earlier synopsis, *Découvertes de M. Marat sur le feu, l'électricité, et la lumière.*

46. *Ibid.*, 1:344.

47. *Ibid.*, 1:352.

48. *Ibid.*, 1:367-368.

49. Lacépède, *Essai sur l'électricité naturelle et artificielle*, 1:39.

50. *Ibid.*, 1:40.

51. Carra, *Nouveaux Principes de physique*, 4:8.

52. Marat, *Recherches physiques sur le feu*, 21.

53. *Ibid.*, 23.

54. *Ibid.*, 39.

55. *Ibid.*, 27.

56. *Ibid.*, 31.

57. *Ibid.*, 42.

58. *Ibid.*, 34.

59. *Ibid.*, 34-35.

60. *Ibid.*, 37.

61. See Chapter Eleven, below.

62. Marat, *Recherches physiques sur le feu*, 39.

63. *Ibid.*, 39.

64. *Ibid.*, 43.

65. *Ibid.*, 43.

66. *Ibid.*, 43.

67. *Ibid.*, 46.

68. *Ibid.*, 48-49.

69. *Ibid.*, 53.

70. *Ibid.*, 56.

71. *Ibid.*, 56-57.

72. *Ibid.*, 59.

73. *Ibid.*, 60.

74. Letter from Newton to Bentley, January 17, 1693, in Bentley, *Works* (London: F. Macpherson, 1836-1838), 3:210.

75. Some twentieth century cosmologists have postulated an exchange of "gravitons" between other elementary particles as a means of pushing the explanation of gravity to a deeper level. Should the existence of gravitons be confirmed, of course, the question of the cause of their motion would be posed. And so it goes.

76. Marat, *Recherches physiques sur le feu*, 72.

77. *Ibid.*, 72-73.

78. *Ibid.*, 75; emphasis added.

79. *Ibid.*, 78.

80. Lamarck, *Recherches sur les causes des principaux faits physiques*, 1:349.

81. *Ibid.*, 1:361.

82. *Ibid.*, 1:362.

83. *Ibid.*, 1:362-363.

84. These writings do not constitute a direct exchange between Marat and Lamarck; the chronology is as follows: Marat's original publication, *Découvertes sur le feu, l'électricité, et la lumière*, which Lamarck was discussing, appeared in 1779. Lamarck's discussion was published in 1794, after Marat's death. But Marat had responded to the identical objection in *Recherches physiques sur le feu*, published in 1780.

85. Marat, *Recherches physiques sur le feu*, 84.

86. *Ibid.*, 85.

87. *Ibid.*, 85.

88. *Ibid.*, 85.

89. *Ibid.*, 182.

90. *Ibid.*, 183.

91. *Ibid.*, 193-194.

NOTES TO CHAPTER TEN

1. The terms "diffraction" and "inflexion" will be used interchangeably here. Its discoverer, Grimaldi, used the word "diffraction." Newton, Marat, and Priestley all preferred "inflexion." Fresnel went back to "diffraction," which then became the universally preferred term.

2. F. M. Grimaldi, *Physico-Mathesis de Lumine Coloribus et Iride* (Bologna, 1665).

3. I. B. Cohen says, "Joseph Priestley's value judgments are notable because he was a great scientist and a splendid historian." Priestley's *History and Present State of Electricity* "has the particular merit of expressing the point of view and the foci of interest of the third quarter of the eighteenth century." On the other hand, "It should be pointed out . . . that Priestley's history of optics is a poorer book than his history of electricity." Cohen, *Franklin and Newton*, 429-430.

4. Priestley, *History and Present State of . . . Vision, Light, and Colours* (London: J. Johnson, 1772), 171.

5. *Ibid.*, 317.

6. *Ibid.*, 351.

7. Newton, *Opticks* (Query 31), 400.

8. *Ibid.*, 338-339.

9. *Ibid.*, 317.

10. I. B. Cohen, "Preface" to Newton's *Opticks*, xxxv.

11. Newton, *Opticks* (Query 29), 370.

12. *Ibid.* (Query 3), 339.

13. Priestley, *History and Present State of . . . Vision, Light, and Colours*, 521. The spellings are Priestley's. The scientists he discusses are Giacomo Filippo Maraldi (1665-1729); Jean Jacques Dortous de Mairan (1678-1771); Etienne François Dutour de Salvert (1711-1789); Claude Nicolas Le Cat (1700-1768); and Pieter van Musschenbroek (1692-1761).

14. *Ibid.*, 520-540.

15. *Ibid.*, 780.

16. *Ibid.*, 780-781.

17. Marat, *Découvertes sur la lumière* (Paris, Jombert, 1780), 5.

18. The other members of the commission were Maillebois, Etienne Mignot de Montigny, and Balthazar Sage.

19. See Marat, *Correspondance*, 5-7 and 58-64.

20. Marat, *Découvertes sur la lumière*, 3. The commission's report was originally printed in the *Registres de l'Académie des Sciences*, dated May 10, 1780.

21. *Ibid.*, 3-4.

22. *Ibid.*, 5.

23. *Ibid.*, 6.

24. Most notably in *Les Charlatans modernes*.

25. Marat, Letter to Roume de Saint-Laurent, November 20, 1783; in *Correspondance*, 66-67.

26. Marat, *Correspondance*, 58-61.

27. *Ibid.*, 61.

28. *Ibid.*, 6. Letter of Le Roy to Marat, February 13, 1780.

29. See Note 43 to Chapter Nine, above.

30. Thomas Young, "The Bakerian Lecture: Experiments and Calculations Relative to Physical Optics," *Philosophical Transactions of the Royal Society of London*, Vol. 94 (1804), 11; emphasis added.

31. John Worrall has recounted this episode -- and debunked the traditional account -- in "Fresnel, Poisson and the White Spot: The Role of Successful Predictions in the Acceptance of Scientific Theories," *The Uses of Experiment: Studies in the Natural Sciences*, edited by David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge University Press, 1988), 135-157. According to Worrall, the demonstration of the white spot did not force a dramatic, immediate about-face on the part of the academicians in favor of Fresnel's wave theory of light, as is often suggested. It was simply another nail in the coffin, so to speak, of the Newtonian paradigm, perhaps no more important than the century-old problem of diffraction. See also, in the same volume, Geoffrey Cantor's comments on Worrall's paper in "The Rhetoric of Experiment."

32. See Cohen, "Preface" to Newton's *Opticks*, xi.

33. Bibliothèque Nationale, Manuscripts: "Nouvelles aquisitions françaises" 309.

34. Young, "Experiments and Calculations Relative to Physical Optics," *Philosophical Transactions of the Royal Society of London*, Vol. 94 (1804), 11, 12.

35. Marat, *Découvertes sur la lumière*, 1.

36. *Ibid.*, 1-2.

37. *Ibid.*, 2.

38. *Ibid.*, 2.

39. *Ibid.*, 2.

40. *Ibid.*, 3.

41. *Ibid.*, 4.

42. Fresnel predicted the black spot as well as the white spot: "Fresnel had found that his theory entails that if light from a point source is shone on an opaque screen in which there is a small circular *opening*, then at certain points beyond the screen but along the axis of the aperture (that is, the extension of the line from the source to the centre of the aperture) there is total darkness. This 'black spot' prediction is 'complementary' to the white spot case -- it is just as counterintuitive as the latter and its empirical success just as unexpected and remarkable." Worrall, "Fresnel, Poisson and the White Spot," 145.

43. Marat, *Découvertes sur la lumière*, 5.

44. *Ibid.*, 6.

45. *Ibid.*, 6.

46. Priestley, *History and Present State of . . . Vision, Light, and Colours*, 520; emphasis in the original.

47. Marat, *Découvertes sur la lumière*, 6.

48. *Ibid.*, 13.

49. *Ibid.*, 22.

50. *Ibid.*, 22.

51. *Ibid.*, 23.

52. *Ibid.*, 23-24.

53. *Ibid.*, 29-30; see experiment 41.

54. *Ibid.*, 24.

55. *Ibid.*, 24.

56. See note 12 above.

57. Marat, *Découvertes sur la lumière*, 30.

58. *Ibid.*, 31-32.

59. *Ibid.*, 66.

60. *Ibid.*, 48.

61. *Ibid.*, 51.

62. *Ibid.*, 52.

63. Gillispie, *Science and Polity*, 307.

64. *Ibid.*, 328. Gillispie was commenting on Marat's *Mémoires académiques*, not his *Découvertes sur la lumière.*

65. Marat, *Découvertes sur la lumière*, 133.

66. *Ibid.*, 133-134.

67. *Ibid.*, 67.

68. Source: Mme. M. A. Tonnelat, "The Spread of Newtonian Optics," in *The Beginnings of Modern Science*, edited by René Taton (New York: Basic Books, 1964), 459. The diagram has been slightly modified.

69. Marat, *Découvertes sur la lumière*, 67.

70. *Ibid.*, 67.

71. *Ibid.*, 69.

72. *Ibid.*, 70.

73. *Ibid.*, 77.

74. *Ibid.*, 99-100.

75. See especially Sepper, *Goethe Contra Newton*, and Amrine, Zucker, and Wheeler, eds., *Goethe and the Sciences: A Reappraisal*.

76. Goethe, *Zur Farbenlehre*, 2:601-606.

77. The other two works were *Découvertes de M. Marat sur le feu, l'électricité, et la lumière* (1779) and *Notions élémentaires d'optique*. Goethe cited the original French edition of *Découvertes sur la lumière* but also noted Weigel's 1783 German translation.

78. Goethe, *Zur Farbenlehre*, 2:605.

79. *Ibid.*, 605.

80. *Ibid.*, 605.

NOTES TO CHAPTER ELEVEN

1. Heilbron, "Franklin, Haller, and Franklinist History," *Isis*, Vol. 68, No. 244 (December 1977), 539-549.

2. For a concise account of the early history of electrical science, see Duane Roller and Duane H. D. Roller, "The Development of the Concept of Electric Charge," *Harvard Case Studies in Experimental Science*, J. B. Conant, ed. (Cambridge, Mass.: Harvard University Press, 1957), 2:541-639.

3. The electrical machines were "capable of generating about 10,000 volt when first employed in the 1740s." By 1785 an immense machine constructed at Leyden and utilized by Martin van Marum "probably gave over 100,000 volt." Heilbron, *Elements of Early Modern Physics*, 74.

4. Letter from Musschenbroek published by Nollet in 1746 in the *Mémoires* of the Academy of Sciences. Quoted by Roller and Roller, "Development of the Concept of Electric Charge," 594.

5. Franklin, *Autobiography and Other Pieces* (Oxford: Oxford University Press, 1970), 148.

6. Cohen, *Franklin and Newton*, 518.

7. *Ibid.*, 531, 534.

8. Franz Ulrich Theodor Aepinus, *Tentamen theoriae electricitatis et magnetismi* (St. Petersburg: Typis Academiae Scientiarum, 1759).

9. *Ibid.*, 39; quoted in Cohen, *Franklin and Newton*, 541.

10. R. G. Boscovich, *Philosophiae naturalis theoria redacta ad unicam legem virium in natural existentium* (1758).

11. James Clerk Maxwell, *A Treatise on Electricity and Magnetism* (Oxford: 1873); quoted in Cohen, *Franklin and Newton*, 560.

12. Cohen, *Franklin and Newton*, 373.

13. *Ibid.*, 515. Franklin died in 1790.

14. The censor's approbation is dated March 7, 1782; the privilege of the king is dated April 24, 1782.

15. Marat, *Recherches physiques sur l'électricité* (Paris: Clousier, 1782), 10.

16. *Ibid.*, 12.

17. *Ibid.*, 13.

18. *Ibid.*, 13.

19. *Ibid.*, 13.

20. *Ibid.*, 14; emphasis in original.

21. *Ibid.*, 14.

22. Heilbron, *Elements of Early Modern Physics*, 290.

23. Marat, *Recherches physiques sur l'électricité*, 15.

24. *Ibid.*, 15.

25. *Ibid.*, 23.

26. *Ibid.*, 23.

27. *Ibid.*, 25.

28. For an example of an attempt to write a work of "sans-culotte science," see Henri Decremps' *La Science sanculottisée*.

29. Marat, *Recherches physiques sur l'électricité*, 27.

30. *Ibid.*, 28.

31. *Ibid.*, 28.

32. *Ibid.*, 31.

33. *Ibid.*, 32.

34. *Ibid.*, 36.

35. *Ibid.*, 40-41.

36. *Ibid.*, 43.

37. *Ibid.*, 42.

38. *Ibid.*, 44.

39. *Ibid.*, 48.

40. *Ibid.*, 49.

41. *Ibid.*, 51.

42. *Ibid.*, 50.

43. *Ibid.*, 50.

44. If the conductor had been a wire carrying a current it would not have exerted the force of an electrostatic charge, but would have produced an electromagnetic field. Such a field would not have caused the needle to turn (even if the needle possessed some residual magnetism) to point toward the wire.

45. Franklin, *New Experiments and Observations on Electricity*, Fourth edition (1769). Quoted in Cohen, *Franklin and Newton*, 531.

46. *Ibid.*, 532.

47. Marat, *Recherches physiques sur l'électricité*, 55.

48. *Ibid.*, 61.

49. *Ibid.*, 63.

50. *Ibid.*, 64.

51. *Ibid.*, 133.

52. *Ibid.*, 77. Litharge is lead monoxide.

53. *Ibid.*, 116.

54. *Ibid.*, 122.

55. *Ibid.*, 122.

56. See the letter from Bertholon to Antoine Buissart, December 15, 1782, in *Revue historique de la Révolution française*, Vol. III (1912), 294-297. Bertholon edited a journal called *La Nature considérée sous ses différens aspects, ou Journal d'histoire naturelle* (Paris: Perisse; the first volume appeared in 1787). Bertholon was identified on the title page as professor of experimental physics, member of sixteen academies, "etcetera."

57. Marat, *Recherches physiques sur l'électricité*, 126-127.

58. *Ibid.*, 84.

59. *Ibid.*, 101 (on Wilke & Aepinus). Marat calls Beccaria a "highly distinguished physicist" and says of his meterological research: "Never has there been a more assiduous observer of more different states of the atmosphere." *Ibid.*, 378.

60. *Ibid.*, 128-129.

61. *Ibid.*, 145.

62. *Ibid.*, 146.

63. *Ibid.*, 150.

64. *Ibid.*, 150.

65. Martin Van Marum (1750-1837). On the power of Van Marum's electrical machine, see note 3 above.

66. Martin Van Marum, *Sur la théorie de Franklin, suivant lequel les phénomènes électriques sont expliqués par un seul fluide* (n.p., *c.* 1819.). The plates illustrating the forked spark discharges are particularly interesting.

67. *Ibid.*, 9.

68. *Ibid.*, 10-11.

69. *Ibid.*, 12. Van Marum was quoting from a letter Volta had written him, dated March 8, 1786. Volta's obituary of the two-fluid theory was premature.

70. See note 43 in Notes to Chapter Nine, above.

71. See Brissot, *Mémoires*, 201.

72. Marat, *Recherches physiques sur l'électricité*, 157.

73. *Ibid.*, 157.

74. *Ibid.*, 159.

75. *Ibid.*, 160.

76. *Ibid.*, 161.

77. *Ibid.*, 162.

78. *Ibid.*, 167.

79. *Ibid.*, 172-173.

80. *Ibid.*, 189.

81. *Ibid.*, 199.

82. *Ibid.*, 199.

83. *Ibid.*, 210.

84. A *toise* is equivalent to 1,949 meters.

85. Marat, *Recherches physiques sur l'électricité*, 225-226.

86. *Ibid.*, 257-258.

87. *Ibid.*, 269.

88. *Ibid.*, 272.

89. Joseph Priestley, *History and Present State of Electricity with Original Experiments* (London: J. Dodsley *et al.*, 1767).

90. Marat, *Recherches physiques sur l'électricité*, 273.

91. *Ibid.*, 275-76.

92. *Ibid.*, 282.

93. *Ibid.*, 316.

94. *Ibid.*, 319.

95. *Ibid.*, 320.

96. *Ibid.*, 354.

97. *Ibid.*, 355.

98. *Ibid.*, 357.

99. *Ibid.*, 358; emphasis added.

100. *Ibid.*, 358; emphasis in original.

101. *Ibid.*, 359.

102. *Ibid.*, 359.

103. *Ibid.*, 360.

104. *Ibid.*, 364.

105. *Ibid.*, 365.

106. *Ibid.*, 408.

107. *Ibid.*, 408-409.

108. *Ibid.*, 414.

109. *Ibid.*, 415.

110. *Ibid.*, 416-417.

111. *Ibid.*, 418.

112. *Ibid.*, 419.

113. *Ibid.*, 427.

114. *Ibid.*, 427. Bertholon's article had appeared in the *Journal de Physique*, August 1779.

115. Marat, *Recherches physiques sur l'électricité*, 437-438.

116. See the section on Lacépède in Chapter Six, above.

117. Marat, *Recherches physiques sur l'électricité*, 447.

118. Williams, *Faraday*, 62.

119. Robert C. Stauffer, "Persistent Errors Regarding Oersted's Discovery of Electromagnetism," *Isis*, 44 (1953) 310.

120. Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism," *Isis*, 48 (1957), 48. The grudging tone of some standard accounts should be noted; for example: "Although nature philosophy was generally characterized by mystical, anti-mathematical speculation of a type which has usually been of little benefit to science, in this case it did encourage the trend toward unification which had some outstanding successes in the nineteenth century." Gerald Holton, *Introduction to Concepts and Theories in Physical Science*, Second edition (Princeton, N.J.: Princeton University Press, 1985), 416.

121. Bern Dibner, *Alessandro Volta and the Electric Battery* (New York: Franklin Watts, 1964), 96.

122. L. de Launay (ed.), *Correspondance du Grand Ampère*, 2:566: Ampère to Roux-Bordier l'aîné, February 21, 1821. Quoted in Williams, *Faraday*, 142-143.

123. J. W. Herivel, "Aspects of French Theoretical Physics," 109.

124. *Ibid.*

125. *Ibid.*, 109.

126. *Ibid.*, 121, 125.

127. *Ibid.*, 110; emphasis added.

128. *Ibid.*, 121.

129. *Ibid.*, 122.

130. *Ibid.*, 129.

131. *Ibid.*, 123. Dhombres and Dhombres have pointed out, however: "In more than one sense, he was an isolated researcher with little influence upon the development of mathematical physics before his 1822 publication." Dhombres and Dhombres, *Naissance d'un nouveau pouvoir*, 518.

132. Herivel, "Aspects of French Theoretical Physics," 129.

133. *Ibid.*, 115-116, 126.

134. *Ibid.*, 126.

135. *Ibid.*, 127-128.

NOTE TO CONCLUSION

1. Roy Porter and Mikulas Teich, eds., *The Scientific Revolution in National Context* (Cambridge: Cambridge University Press, 1992), 6.

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