The Rise and Fall of Laplacian Physics

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The Rise and Fall of Laplacian Physics

BY ROBERT FOX*

By means of these assumptions, the phenomena of expansion, heat, and vibrational motion in gases are explained in terms of attractive and repulsive forces which act only over insensible distances (distances imperceptibles). *In my theory of capillary action I related the effects of capillarity to such forces. All terrestrial phenomena depend on forces of this kind, just as celestial phenomena depend on universal gravitation. It seems to me that the study of these forces should now be the chief goal of mathematical philosophy. I even believe that it would be useful to introduce such a study in proofs in mechanics, laying aside abstract considerations of flexible or inflexible lines without mass and of perfectly hard bodies. A number of trials have shown me that by coming closer to nature in this way one could make these proofs no less simple and far more lucid than by the methods used hitherto.*[1]

1. INTRODUCTION

The period from Napoleon Bonaparte's assumption of power as First Consul in 1799 until his final overthrow in 1815 is generally recognized to have been one of the most glorious in the whole history of French science. It was a period when France led her European rivals in the quantity and in the quality of her scientific contributions, especially in the physical sciences. Great names, such as those of Laplace, Berthollet, Biot, Poisson,

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[1]P.S. Laplace, *Traité de mécanique céleste,* 5 vols. (Paris, 1799–1825), *5*, 99. Although the title page of the fifth volume bears the date 1825, the six books that made up the volume were published and dated separately. Book XII, in which this passage appears, is dated April 1823. For an earlier and slightly different statement see *Connaissance des tems . . . pour l'an 1824* (Paris, 1822), p. 323; also *Journal de physique, 94* (1822), 90. The translations throughout the paper are my own.

Gay-Lussac, Thenard, and Malus, abounded, and there were some remarkable successes, of which the most celebrated is perhaps the discovery and study of the polarization of light. It is not surprising, therefore, that both the "declinists" of Britain in 1830, such as Charles Babbage and David Brewster,[2] and those who complained no less bitterly about the state of French science in the 1860's and 1870's, notably Louis Pasteur and Adolphe Wurtz,[3] looked back to the years of Napoleon's rule as a truly golden age for science.

It is not difficult to identify at least some of the conditions that allowed French science under Napoleon to become a byword for excellence. As Maurice Crosland has shown, the supply of able graduates from the École Polytechnique, public recognition and encouragement given even by Napoleon himself, the attractive career possibilities that were available to young men trained in science, and the select research school centered on Berthollet's country house at Arcueil, just outside Paris, all played their part.[4] And equally, thanks above all to the work of L. Pearce Williams and Roger Hahn, we know something of the weaknesses of French science in the Consulate and First Empire. For example, although weaknesses were rarely acknowledged at the time, at least in public, Williams has identified grave deficiencies in education, especially at the elementary level,[5] and Hahn has pointed to the harm that was being done as the First Class of the Institute (the revolutionary successor of the Academy of Sciences) became increasingly a manifestation of Napoleon's *Kulturpolitik.*[6]

It is the purpose of this paper to take further the investigation of both the strengths and the weaknesses of Napoleonic science, with special

[2]In Britain admiration for Napoleonic science was naturally greatest some years after 1815 and was particularly strong about 1830. Babbage wrote glowingly of French achievements in his *Reflections on the Decline of Science in England* (London, 1830), especially pp. 25–27 and 30–36, maintaining that under Napoleon "the triumphs of France were as eminent in Science as they were splendid in arms" (*ibid.,* p. 26). An equally favorable view was given by David Brewster in his unsigned "Decline of Science in England," *Quarterly Review, 43* (1830), 313–317, although Brewster stressed that the enlightened policy toward science, which had been so characteristic of the First Empire, had continued under Louis XVIII and Charles X.

[3]See, for example, Pasteur's contribution of 1871 to the Lyons newspaper *Salut public,* quoted in R. Vallery-Radot, *La vie de Pasteur* (Paris, 1900), pp. 278–279, and C.A. Wurtz, *Les hautes études pratiques dans les universités allemandes* (Paris, 1870), pp. 5–6.

[4]M.P. Crosland, *The Society of Arcueil. A View of French Science at the time of Napoleon I* (London, 1967).

[5]L.P. Williams, "Science, Education and Napoleon I," *Isis, 47* (1956), 369–382.

[6]R. Hahn, *The Anatomy of a Scientific Institution. The Paris Academy of Sciences, 1666–1803* (Berkeley, Los Angeles, and London, 1971), pp. 310–312.

reference to physical science. In Section 2, I shall argue that the course and content of much of French physics and physical chemistry under Napoleon was determined by the zeal of the mathematician and physicist Pierre Simon Laplace and the chemist Claude Louis Berthollet for a program of research which they jointly sought to pursue. The program was seen, at least by them, as a natural culmination of eighteenth-century work in the Newtonian tradition, so that although it is described here as Laplacian, it was in reality not entirely the creation of Laplace himself, or indeed of Berthollet; it was Laplacian only to the extent that Laplace gave it a number of its characteristic features, stated it explicitly, and was its most brilliant exponent from the time he began to formulate it, probably in the 1790's, until his death in 1827.

In the years of its greatest success, from 1805 to 1815, the program both raised problems and laid down the general principles for solutions; and, by doing so, it gave French physical science a most uncommon unity of style and purpose. It also stimulated much good work; yet, as I hope to show, it owed its dominant position not merely to its merits, considerable though these appeared to be at the time, but equally to the effectiveness with which Laplace and Berthollet were able to control the scientific establishment of France in teaching as well as research. Once this control was lost (a process that I discuss in Section 3), the program became vulnerable, and, beset by the challenges of new discoveries and theories in heat, optics, electricity, magnetism, and chemistry and by a new generation of younger scientists who felt no allegiance to Laplace and Berthollet, it was abandoned, quite suddenly, between 1815 and 1825. By examining the downfall of the Laplacian program and its attendant doctrines, I hope to demonstrate the precariousness of what I see as the leading research tradition in physical science in Napoleonic France and to suggest that the successes of the period owed far more than has previously been recognized to Laplace's personal commitment to his program and to his ability to engage other men of exceptional ability in the same enterprise.

2. LAPLACIAN PHYSICS

Laplacian physics was a style of physics that depended on and was embraced by what J. T. Merz, with an acknowledgment to Maxwell, first called the astronomical view of nature.[7] It was a physics that sought to

[7] J.T. Merz, *A History of European Thought in the Nineteenth Century,* 4 vols. (Edinburgh and London, 1896–1914), *1*, 347–348.

account for all phenomena, on the terrestrial and, more particularly, the molecular scale as well as on the celestial scale, in terms of central forces between particles which, although treated by analogy with Newtonian forces of gravitation, could be either attractive or repulsive. Since attempts to "explain" gravitation had been generally abandoned long before, forces of this character could readily be accepted as "mechanical" and hence in need of no further explanation.[8] The forces were conceived as being exerted by and upon imponderable as well as ordinary ponderable matter; indeed, an essential and highly characteristic element in Laplacian physics was the system of imponderable fluids of heat, light, electricity, and magnetism. In accordance with beliefs that had come to be widely accepted by the end of the eighteenth century, each fluid was thought to consist of particles which were mutually repulsive but which in all cases were attracted by ponderable matter.[9] In the hands of the Laplacians, models of such fluids, founded on the assumption that the forces between imponderable and ponderable matter were effective only over "insensibly small" distances, were capable of being translated into systems of differential equations whose approximate solutions could "save" the phenomena already known and even predict new ones. And it was in the attempt to refine and quantify a theory of the imponderables which had hitherto been vague and qualitative that Laplacian physics found its main problems and had its most notable achievements.

The imponderable fluids, like the rest of Laplacian physics, did not originate with Laplace himself. As far as the basic model for their structure is concerned, they have their roots in Newton's speculations on the subtle electrical spirit in the General Scholium of 1713,[10] in his speculations on the ether which appeared in the second and subsequent editions

[8]However, the charge against the imponderables that they merely allowed the explanation of action at distance to be postponed, since their own properties presupposed the existence of such action, was raised in the late eighteenth and early nineteenth centuries, notably by Lavoisier and Davy; see R. Fox, *The Caloric Theory of Gases from Lavoisier to Regnault* (Oxford, 1971), pp. 17 and 118.

[9]Those who adopted the two-fluid theories of electricity and magnetism (see below, p. 93) made the additional assumption that there was attraction between the vitreous and resinous electrical fluids and between the austral and boreal magnetic fluids.

[10]Newton, *Philosophiae Naturalis Principia Mathematica,* 2nd ed. (Cambridge, 1713), p. 484. On the interpolation of the words "electric and elastic" to describe the spirit in Andrew Motte's translation of 1729, see A. Koyré and I.B. Cohen, "Newton's 'Electric & Elastic Spirit'," *Isis, 57* (1960), 337.

of the *Opticks*,[11] and even more clearly perhaps in what was generally recognized through the eighteenth century as the Newtonian view of gas structure—the view that the particles of gases were stationary and that repulsive forces between these particles accounted for gas pressure and the other characteristic gaseous properties.[12] This Newtonian model had been applied with increasing frequency in discussions of the properties of imponderable fluids since the 1740's, when Franklin used it in his widely read speculations on the nature of the electric fluid.[13] In fact, by about 1780 Franklin's belief that electrostatic phenomena could be explained in terms of the supposed repulsion between the particles of the electric fluid and the supposed attraction between the fluid and the particles of ordinary ponderable matter had become standard doctrine; and, as a result of the work of such men as Aepinus, Priestley, and Lavoisier, the same was true of the other "Newtonian" imponderables which had emerged by then, the fluids of magnetism, heat (or fire), and light.[14] There were divergences of opinion, of course, notably between the supporters of the "one-fluid" theories of electricity and magnetism, associated principally with the names of Franklin and Aepinus, and those like Coulomb who favored the "two-fluid" theories, in which a vitreous and a resinous electrical fluid and an austral and a boreal magnetic fluid were postulated.[15] But such differences did nothing to make the fundamental concept of the imponderable elastic fluid any less acceptable. By 1780 the

[11]Newton, *Opticks*, 2nd ed. (London, 1718), pp. 322-328 (Queries 17-24). In the *Principia* the subtle spirit was simply described as "electric and elastic" and no further details of its supposed structure were given. In Query 21 of the *Opticks*, however, it was suggested that the postulated ether might consist of mutually repulsive particles. Although the structure of Newton's ether was similar to that of the later imponderables, its function, primarily as the basis for an explanation of gravitation and optical phenomena, was quite different; see J.E. McGuire, "Force, Active Principles, and Newton's Invisible Realm," *Ambix, 15* (1968), 154-208.

[12]First stated (as no more than a mathematical hypothesis) in the first edition of the *Principia* (London, 1687), pp. 301-303 (Book II, Proposition xxiii).

[13]Most easily consulted in I.B. Cohen, ed., *Benjamin Franklin's Experiments* (Cambridge, Mass., 1941), especially pp. 213-215.

[14]For accounts of the rise of the imponderables between the 1740's and the 1780's see I.B. Cohen, *Franklin and Newton* (Philadelphia, 1956), pp. 365-554; R.E. Schofield, *Mechanism and Materialism* (Princeton, 1970), pp. 157-190; Fox, *op. cit.* (note 8), pp. 6-20; and J.E. McGuire and P.M. Heimann, "Newtonian Forces and Lockean Powers: Concepts of Matter in Eighteenth-Century Thought," *Historical Studies in the Physical Sciences, 3* (1971), 233-306.

[15]Also the Newtonians such as Gowin Knight, P.D. Leslie, Cadwallader Colden, Bryan Higgins, James Hutton, and Adam Walker, who worked in the predominantly British tradition discussed by Heimann and McGuire (see their paper cited in note

theory of the imponderables was recognized to be imperfect to the extent that it was still almost entirely qualitative, yet it was coherent, simple, and, above all, it had the merit of possessing what appeared to be a thoroughly Newtonian pedigree.

So for a Frenchman such as Laplace, whose interest in the experimental aspects of physical science, especially the study of heat, grew rapidly about 1780,[16] it was to be expected that the imponderable fluids would provide an entirely acceptable explanation of the phenomena of physics. This was a time when in a strongly Newtonian France few qualms were felt about accepting the possibility of forces acting at a distance and when in any case the physics of the eighteenth-century Newtonian tradition was seen as having had so many notable successes. Moreover, it was just at this time that the writings of Laplace's friend Lavoisier were beginning to attract favorable attention to the fluid or caloric theory of heat[17] and to give it the form it was to have for the next seventy years; and it was only shortly afterwards that Coulomb produced some of his most important work in electricity and magnetism,[18] work which did much to win

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14), held views on subtle fluids that differed appreciably from those of Franklin, Aepinus, Lavoisier, and Coulomb.

[16]The stimulus for this new interest came from Lavoisier, according to Laplace's letter to Lagrange, 21 August 1783, in *Oeuvres de Lagrange,* 14 vols. (Paris, 1867–1892), *14,* 124. In a letter to Lagrange of 11 February 1784 (*ibid., 14,* 130), however, he made no show of reluctance, writing, with reference to Haüy's recently published *Essai d'une théorie sur la structure des cristaux,* which he and Daubenton had read on behalf of the Academy of Sciences: "It contains an interesting application of mathematics to nature, and the hope that we may be able to extend the realm of geometry cannot be put too strongly. It is with this in view that I have devoted a little of my time to physics, and I am not without hope that I shall be able to grasp some other physical problems sufficiently well to be able to apply the methods of analysis to them."

[17]Notably in his papers in the *Mémoires . . . de l'Académie Royale des Sciences* for 1777 (pp. 420-432 and 592-600), and the joint "Mémoire sur la chaleur," written with Laplace, which appeared on pp. 355–408 of the *Mémoires* for 1780 (although it was not read until June 1783 and was published only in 1784). It is important to note that Lavoisier was always cautious on the question of the nature of heat, and in the joint paper with Laplace the view that heat consists in the motion of the particles of ordinary ponderable matter was stated to be no less plausible than the fluid theory. However, especially in his *Traité élémentaire de chimie,* 2 vols. (Paris, 1789), *1,* 4-27, he was easily taken for a convinced calorist.

[18]The seven papers describing this work, which all date from the period 1785–1789, are most easily consulted in *Collection de mémoires relatifs à la physique, publiés par la Société Française de Physique. Tome 1. Mémoires de Coulomb* (Paris, 1884), pp. 107–318. For a study of Coulomb's work in electricity and magnetism see C.S. Gillmor, *Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France* (Princeton, 1971), pp. 175–221.

support for the two-fluid theories that Coulomb himself favored.[19] But Laplace, of course, did more than merely accept the legacy of Newtonian physics as this was normally understood towards the end of the eighteenth century. It was Laplace's great achievement to build on the Newtonian tradition, to restate many of its principles in a mathematical form, to take up its outstanding problems, especially with regard to the short-range forces that were thought to operate on the molecular scale, and thereby to create a physics which, although Newtonian in origin, was unmistakably and characteristically Laplacian.

Signs of what was later to emerge as the true Laplacian program can be detected as early as 1796. In the first edition of the *Exposition du système du monde,* published in that year, Laplace stated that not only optical refraction and capillary action but also the cohesion of solids, their crystalline properties, and even chemical reactions were the result of an attractive force exerted by the ultimate particles (*molécules*) of matter, and he looked forward to the day when the law governing the force would be understood[20] and when, as he put it, "we shall be able to raise the physics of terrestrial bodies to the state of perfection to which celestial physics has been brought by the discovery of universal gravitation."[21] In Laplace's view there was good reason to believe that the molecular forces might themselves be gravitational in nature, even though they did not obey the simple inverse-square law, a complication that resulted from the effect, on the molecular scale, of the shape of the individual molecules.

But in all this he was saying no more than what so many eighteenth-century Newtonians, with an eye on the Queries, had already accepted as standard doctrine. Throughout the eighteenth century, molecular forces, usually assumed to be negligible except over a very short range, had been invoked as a standard element of Newtonian physics in treatments of optical refraction, capillary action, surface tension, and crystal structure,[22]

[19]*Collection de mémoires . . . Coulomb* (note 18), pp. 250–252.

[20]P.S. Laplace, *Exposition du systême du monde,* 2 vols. (Paris, an VII [1796]), *2,* 196–198.

[21]*Ibid., 2,* 198.

[22]The most important source for this acceptance was, of course, Query 31 of the *Opticks;* see Newton, *Opticks,* 4th ed. (London, 1730), pp. 350–382. For references to the work of Clairaut and Buffon, the leading exponents of the "molecular forces" tradition in eighteenth-century France, see notes 24, 25, and 26. Views on crystal structure are discussed in H. Metzger, *La genèse de la science des cristaux* (Paris, 1918), pp. 165–170, and J.G. Burke, *Origins of the Science of Crystals* (Berkeley and Los Angeles, 1966), pp. 34–35 and 78. Although Laplace did not discuss the matter in 1796, he almost certainly shared the belief of many eighteenth-century Newtonians that the molecular forces were operative only over a very short range. For

and they had been invoked also in a continuing tradition of work on chemical affinities.[23] Although molecular forces had been equally acceptable to British and to French Newtonians, it was probably the French, in particular Alexis Claude Clairaut and Georges Louis Leclerc de Buffon, who (next to Newton himself) exerted the greatest influence on Laplace, and it is by examining their work that we can see most clearly the continuity between Laplace and his Newtonian precursors in the eighteenth century. Clairaut, for example, had ascribed "the roundness of drops of fluid, the elevation and depression of liquids in capillary tubes, the bending of rays of light, etc." to gravitational forces that became large at very small (molecular) distances,[24] and Buffon had discussed similar forces, though in his case with special reference to the forces of chemical affinity.[25] Although both men were proud to call themselves Newtonians and stated explicitly that the molecular forces they postulated were of the same nature as those operating between celestial bodies, they disagreed fundamentally over the relationship between force and distance on the molecular scale. The debate in which this disagreement became apparent arose because of a discrepancy between the predicted and observed periods of the apogee of the moon.[26] To remove the discrepancy, Clairaut suggested, in 1747, that the force law should contain a term inversely proportional to the fourth power of the distance, $1/r^4$, in addition to the usual term proportional to $1/r^2$.[27] This additional term, he argued, would

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earlier expressions of this belief see, for example, John Keill, "Epistola ad Cl. virum Gulielmum Cockburn, Medicinae Doctorem. In qua leges attractionis aliaque physices principia traduntur," *Philosophical Transactions, 26* (1708–1709), 97–110, especially p. 100; F. Hauksbee, *Physico-Mechanical Experiments on Various Subjects* (London, 1709), pp. 157–160; and, for a contemporary French statement, by Coulomb, *Collection de mémoires relatifs à la physique . . . Mémoires de Coulomb* (note 18), pp. 125–126. For Newton's earliest comment on the matter, see his *Optice* (London, 1706), pp. 322–348 (Query 23), especially p. 335.

[23] For references to this tradition see note 32.

[24] A.C. Clairaut, "Du système du monde dans les principes de la gravitation universelle," *Mémoires . . . de l'Académie Royale des Sciences* for 1745 (published 1749), p. 338. The paper was read on 15 November 1747. Cf. his later comment on p. 547 of the same volume.

[25] G.L.L. de Buffon, "De la nature. Seconde vue" (1765), in his *Histoire naturelle, générale et particulière,* 44 vols. (Paris, 1749–1804), *13,* xii-xv.

[26] The debate is best followed in the papers by the two men that were published in the *Mémoires . . . de l'Académie Royale des Sciences* for 1745, pp. 329–364, 493–501, 529–548, 551–552, and 577–587. For accounts of the debate see P. Brunet, *La vie et l'oeuvre de Clairaut (1713–1765)* (Paris, 1952), pp. 82–88, and A.W. Thackray, *Atoms and Powers. An Essay on Newtonian Matter-Theory and the Development of Chemistry* (Cambridge, Mass., and London, 1970), pp. 157–160.

[27] Clairaut, *op. cit.* (note 24), pp. 337–339.

at once remove the anomaly in question and be consistent with the existence of molecular forces that were very large at insensible distances. Concerned at what he saw as a loss of elegance and simplicity and hence as a threat to the Newtonian system, Buffon upheld the familiar $1/r^2$ law, though some years later he admitted that such a law would be modified at short range by the shape of the particles of matter.[28] The details of the debate do not concern us here, and it is sufficient to note that it ended abruptly in 1749 when Clairaut found that, after all, he could reconcile the motion of the apogee with a simple inverse-square law.[29] However, he was careful not to concede victory to Buffon with regard to the short-range forces, and the form of the law obeyed by such forces remained undiscovered at the end of the century, having attracted little further attention.

Laplace's comment on the effect of the shape of individual particles on the force law suggests that he was especially indebted to Buffon, but even though he was reticent about his precursors, there can be little doubt that he owed quite as much to Clairaut, who had treated refraction and capillary action in a mathematical way of which he would surely have approved.[30] There is, then, evidence of continuity between Laplace and at least one kind of eighteenth-century Newtonianism which had a strong following in France, and the degree of continuity is so great that it could be argued that Laplace merely brought together a group of ideas which previously had been rather disparate. However, his statement of the range of phenomena that could be treated in terms of molecular forces, even as given in 1796, was certainly more comprehensive than any made previously, and it did direct renewed attention to the unification of terrestrial and celestial physics which, in his view, would result from further research. Above all, it looked forward to future work, in the manner of a research program.

It was not until the publication of the fourth volume of the *Traité de mécanique céleste* in 1805 that Laplace's brief and rather formal state-

[28] Buffon, *loc. cit.* (note 25).

[29] "Avertissement de M. Clairaut . . . sur le système du monde, dans les principes de l'attraction," *Mémoires . . . de l'Académie Royale des Sciences* for 1745, pp. 577–578. This is dated 17 May 1749.

[30] See Clairaut, "Sur les explications cartésienne et newtonienne de la réfraction de la lumière," *Mémoires . . . de l'Académie Royale des Sciences* for 1739 (published 1739), pp. 259–275, and (on capillary action) his *Théorie de la figure de la terre, tirée des principes de l'hydrostatique* (Paris, 1743), pp. 105–128. In both of these discussions he used short-range molecular forces. For a reference to one of Laplace's very few comments on his precursors, in this case to Clairaut, see note 40.

ment of 1796 was transformed into the basis for a truly Laplacian style of science. It seems crucial to our understanding of this transformation that by 1805 Laplace had gained a close and highly influential ally in Berthollet, his intimate friend since the early 1780's, his next-door neighbor at the village of Arcueil from 1806, and a man who saw chemistry in precisely those Newtonian terms that Laplace sought to apply more particularly in physics. In the *Recherches sur les lois de l'affinité* of 1801 and more fully in the *Essai de statique chimique* of 1803, Berthollet had expounded his view that chemical affinity was the result of attractive forces between the particles of matter. Indeed, he had gone so far as to begin the *Statique chimique* by declaring:

> The forces that bring about chemical phenomena all derive from the mutual attraction between the molecules of bodies. The name affinity has been given to this attraction so as to distinguish it from astronomical attraction.
>
> It is probable that both are one and the same property.[31]

Of course, in putting forward this idea, Berthollet was making no claim to originality. The idea was taken straight from the Newtonian tradition in the chemistry of affinites as this had come down through the English Newtonians such as the Keills and Hauksbee, and in France through Buffon and the French chemists Macquer, Guyton de Morveau, and Lavoisier, among others.[32] But in his lengthy writings on affinity Berthollet, like Laplace, did far more than reiterate a conventional view. Perhaps he did not succeed in answering many (or any) of the outstanding problems of the eighteenth century, for neither he nor his disciples carried through the systematic determination of chemical affinities that good Newtonian chemists saw as their goal. And his recognition of the difficulty of the tasks ahead may well have spread more despondency than encouragement. But by his rigorous, critical restatement of the Newtonian

[31] C.L. Berthollet, *Essai de statique chimique,* 2 vols. (Paris, an XI [1803]), *1*, 1.

[32] For accounts of this tradition, which was virtually unaffected by the innovations of Lavoisier, see M.P. Crosland, "The Development of Chemistry in the Eighteenth Century," *Studies on Voltaire and the Eighteenth Century, 24* (1963), 369-441, especially pp. 382-390; A.W. Thackray, "Quantified Chemistry—the Newtonian Dream," in D.S.L. Cardwell, ed., *John Dalton & the Progress of Science* (Manchester, 1968), pp. 92-108; and Thackray, *op. cit.* (note 26), pp. 199-233. The basic belief of eighteenth-century Newtonian chemistry was that chemical phenomena could be explained in terms of short-range forces which it was the goal of the chemist to quantify and systematize; hence the keen eighteenth-century interest in tables of affinity.

principles he succeeded in creating a coherent system and, what is even more important for our purpose, in laying down a program for future work, where before there had been a jumble of rather vague beliefs.

So when, between 1805 and 1807, Laplace published his theoretical studies of the refraction of light and capillary action in the fourth volume of the *Mécanique céleste*[33] and when he based these studies on the assumption that there were short-range attractive forces both between the particles of ponderable matter and between the particles of ponderable matter and those of light, he was using an approach which, in his own eyes and in those of his contemporaries, had the sanction not only of a strong and much admired eighteenth-century Newtonian tradition but also of the most eminent French chemist of the day. Berthollet's may indeed have been one of the last of all attempts to realize the "Newtonian dream" of a quantified chemistry based on the measurement of the forces between the particles of matter; it was also one of the most distinguished of all such attempts, and the fact that it was soon to be made obsolete by the new approach to chemistry initiated by the Daltonian atomic theory in no way diminishes its importance in helping to set the course and objectives of French chemistry in the Napoleonic period. Nor should its imminent rejection lead us to underestimate the influence that it must have exerted on Laplace in bringing him to publish his first detailed study of the molecular forces operating in physics just two years after the analogous forces of chemistry had been treated in the *Statique chimique.* Nor, indeed, should its influence on others be discounted, for it was no coincidence that the Abbé René Just Haüy developed his model for acid-alkali reactions in terms of short-range intermolecular forces in the three years following the publication of the *Statique chimique.*[34] We may be certain, of course, that by the period 1803–1806 Haüy was being influenced quite as much by Laplace as by Berthollet. But the precise circumstances of Haüy's work need not concern us; we need only note it as an important early product of the revival of interest in molecular forces that Berthollet and Laplace jointly fostered.

33 Laplace, *Mécanique céleste* (note 1), *4* (1805), 231–281 (on refraction). The two supplements (separately paginated) contained his theory of capillary action. On the date of their publication see J.B. Biot, *Journal de physique, 65* (1807), 88; also *Académie des Sciences. Procès-verbaux des séances de l'Académie tenues depuis la fondation de l'Institut jusqu'au mois d'août 1835,* 10 vols. (Hendaye, 1910–1922), *3*, 344 (28 April 1806) and 553 (6 July 1807).

34 See S.H. Mauskopf, "Haüy's Model of Chemical Equivalence: Daltonian Doubts Exhumed," *Ambix, 17* (1970), 182–191.

There can be little doubt that by 1805 Laplace already had a clear conception of his program for physics and for physical science as a whole. Certainly he had not yet set down his program formally, as he was to do rather sketchily some three years later[35] and again in 1823 in the definitive form of the passage quoted at the beginning of this paper; but the 1805 volume of the *Mécanique céleste* and its two supplements, published in 1806 and 1807, indicate clearly enough that Laplace had formulated the basic idea of the reduction of all physical phenomena to a system of densely distributed particles exerting attractive and repulsive forces on one another at a distance (albeit at a very short distance). In this earliest work on his program Laplace gave lengthy mathematical treatments of optical refraction and capillary action, basing both treatments on the supposed existence of short-range attractive forces of the type that had been first postulated by Newton and discussed so often through the eighteenth century.[36] In the case of capillary action these forces were assumed to exist between the particles of ordinary ponderable matter; in the case of refraction the attraction was between the particles of ordinary matter and those of the imponderable light.

Laplace's choice of optical refraction and capillary action as the subjects for his first *sorties* into the realm of molecular physics reveals clearly his caution at this stage in his work. For these were both manifestations of action at a distance on the molecular scale which had been of special

[35] This statement of his program appears in a note added to his "Mémoire sur les mouvemens de la lumière dans les milieux diaphanes," *Mémoires de la Classe des Sciences Mathématiques et Physiques de l'Institut de France, 10* (1809), 300-342. It dates presumably from between January 1808, when the main paper was read, and August 1810, when the volume was published. In the note (p. 329) he wrote, with reference to short-range molecular forces:

> In general, all the attractive and repulsive forces in nature can be reduced, ultimately, to forces of this kind exerted by one molecule on another. Thus, in my *Theory of Capillary Action,* I have shown that the attractions and repulsions between small objects floating on a liquid, and generally all capillary phenomena, depend on intermolecular attractions which are negligible except at insensible distances. Similarly an attempt has been made to reduce the phenomena of electricity and magnetism to intermolecular action. The behavior of elastic bodies also may be treated in the same way.

Later (p. 338), after discussing the relevance of short-range forces to the study of heat flow, he stated the purposes of his note as follows: "I have sought to establish that the phenomena of nature can be reduced ultimately to action *ad distans* between molecules and that the theory of these phenomena must be based on a study of such action."

[36] For references to this earlier work see notes 22, 24-26, 29, 30, and 32.

interest to eighteenth-century Newtonians, as well as to Newton himself.[37] Moreover, both phenomena had raised problems that remained unsolved even in 1805. The detailed mathematical treatment of refraction in particular had proved difficult, and the study of the molecular forces that caused refraction, although of such obvious interest to Newtonians, had scarcely begun.[38] Likewise, even Clairaut's treatment of the theory of capillary action,[39] which for Laplace was the only discussion worthy of serious consideration,[40] was brief and incomplete. By contrast, Laplace's treatments of both refraction and capillarity were lengthy and, to all appearances, comprehensive. Not surprisingly, the problem of discovering the law relating molecular force and distance, which remained in much the same state it was in after the inconclusive confrontation between Clairaut and Buffon nearly sixty years earlier, was one that the *Mécanique céleste* and its supplements did not solve, but Laplace's demonstration that the form of the law was unimportant at least made the situation appear less scandalous, and to that extent it was a minor triumph.

However, in Laplace's mind there was obviously far more to be achieved by a study of short-range forces than the mere tying up of loose ends, and between 1805 and 1807 he appears to have immersed himself totally in the problems of molecular physics. In this period he brought his writings on capillary action to the notice of the Institute on no fewer than four occasions[41] and engaged others in experiments designed to confirm and enlarge on his theoretical work. For example, Haüy, the Parisian engineer Jean Louis Trémery, and Joseph Louis Gay-Lussac, then a young *protégé* of Berthollet, were all asked to undertake experiments on capillary action,[42] while Jean Baptiste Biot, perhaps Laplace's closest disciple at this

[37]Newton, *Opticks* (note 22), 4th ed., pp. 324–327, 345–349, and 367–371 (Queries 19–21, 29, and 31).

[38]Although the existence of the molecular force that caused refraction was not in doubt for Newtonians, they could say little more than that it diminished rapidly with distance. This was true even of a quite detailed mathematical treatment like Clairaut's (cited in note 30).

[39]See note 30 for reference.

[40]See pp. 2–3 of the first supplement on capillary action in Laplace, *op. cit.* (note 33). Hauksbee was the only other authority mentioned.

[41]*Procès-verbaux, 3,* 293 (2 nivôse an 14; 23 December 1805); *3*, 344 (28 April 1806); *3*, 431 (29 September 1806); *3*, 553 (6 July 1807).

[42]Laplace, "Extrait d'un mémoire sur la théorie des tubes capillaires," *Journal de physique, 62* (1806), 120–128, and the first of the two supplements on capillary action that were added to the tenth book of the *Mécanique céleste* (vol. 4), especially pp. 52–55.

time,[43] was chosen to undertake the experimental investigation of refraction in gases which was proposed to the Institute by Laplace. The paper that resulted from this last investigation, read in March 1806,[44] is an important one in the history of the Laplacian program, since Biot and his young collaborator François Arago were convinced that their observations on the bending of light rays had yielded an accurate measure, indeed the first accurate measure, of forces on the molecular scale which had hitherto almost defied quantification. If, as I believe, Laplace had already conceived his program by this time, the work of Biot and Arago must have marked an advance of outstanding significance in his eyes; and its significance was certainly not lost on Biot and Arago themselves. Adopting what appears to have been standard Laplacian doctrine—that the short-range forces which caused the refraction of light were also the forces of chemical affinity—they maintained that their work, quite apart from its more obvious consequences in the field of optics, might also help to solve the great problems of chemistry, by which, of course, they meant Berthollet's chemistry of affinities and molecular forces. And it was in fact with some justification that they proposed the study of optical refraction as a more promising tool for the investigation of the forces that governed the course of chemical reactions than the direct observation of the reactions themselves, in which the complexities were quite daunting.[45] A table of affinities for light, they believed reasonably enough, would be easier to establish than a table of affinities for, say, oxygen, such as Berthollet had proposed.[46]

Laplace's great strength in the Napoleonic period was, of course, the

[43]However, later there emerged certain issues on which Biot and Laplace were apparently not in full agreement. For example, in two contributions to the *Mercure de France* in 1809 Biot took the opportunity of pointing to the dangers of accepting the physical reality of the various imponderable fluids. Such fluids, he wrote, were no more than "a convenient hypothesis to which [true natural philosophers (*physiciens*)] are careful not to attach any idea of reality"; see Biot, *Mélanges scientifiques et littéraires,* 3 vols. (Paris, 1858), *2*, 97–116, especially pp. 102–103 and 113–116, and (for the quotation) p. 114. There is further evidence of at least a temporary weakening of the bond between the two men in Laplace's decision to vote for Fourier rather than Biot in the election for the post of permanent secretary of the Academy of Sciences in 1822; see note 143.

[44]Biot and Arago, "Mémoire sur les affinités des corps pour la lumière, et particulièrement sur les forces réfringentes des différens gaz," *Mémoires de la Classe des Sciences Mathématiques et Physiques de l'Institut National de France,* 7 (1806), 301–387. Read 24 March 1806.

[45]*Ibid.,* pp. 327–330.

[46]Berthollet, *op. cit.* (note 31), *2*, 3–6.

influence that he was able to wield in the scientific community, an influence that was matched in France only by that of Berthollet, whose zeal for Newtonian science was in any case no less than his own. Laplace naturally did not hesitate to use his position to promote his beliefs, and direct patronage which he and Berthollet dispensed to promising young graduates of the École Polytechnique—Biot, Arago, Gay-Lussac, and Siméon Denis Poisson among them—was only one of his methods.[47] Laplace also wielded to extraordinarily good effect the system of prize competitions organized by the First Class of the Institute. Hence it is not surprising to find him serving on, and presumably dominating, the five-man committee which in December 1807 proposed a mathematical study of double refraction as the subject for the prize for mathematics to be awarded some two years later.[48]

Clearly the intention in setting the subject was that Laplace's treatment of refraction, given in the fourth book of the *Mécanique céleste,* should be extended to embrace double refraction as well. The prospect of success in the enterprise was all the more attractive from the Laplacian point of view because the explanation of double refraction had presented such notorious problems since the discovery of the phenomenon in crystals of Iceland spar in 1669.[49] Even Huygens himself had admitted that his wave theory was inadequate to explain all the observations associated with double refraction;[50] in particular, the behavior of the ordinary and extraordinary rays when passed through a second crystal defied explanation by anyone who, like Huygens, postulated a longitudinal wave motion, and it was only when Fresnel introduced the concept of transverse waves in 1821 and so made an understanding of polarization possible that this grave weakness in the wave theory was removed. Although Huygens' failure was seized upon in the *Opticks,*[51] Newton's own explanation, in terms of the "sides" of rays of light, was vague and difficult to reconcile with a corpuscular theory, and even by the end of the eighteenth century it had

[47] For an excellent study of the patronage system of Arcueil see Crosland, *op. cit.* (note 4).

[48] *Procès-verbaux, 3*, 632-633 (21 December 1807). The other members of the committee were Lagrange, Legendre, Lacroix, and Lazare Carnot.

[49] For accounts of earlier theories of double refraction see A.I. Sabra, *Theories of Light from Descartes to Newton* (London, 1967), pp. 221-229, and V. Ronchi, *The Nature of Light. An Historical Survey,* trans. V. Barocas (London, 1970), pp. 153, 188-190, and 205-206.

[50] C. Huygens, *Traité de la lumière* (Leiden, 1690), pp. 88-91.

[51] Newton, *Opticks* (note 22), 4th ed., pp. 332-336 and 336-339 (Queries 25, 26, and 28).

been little improved. So to Laplace the theory of double refraction presented the enticing challenge of a long-notorious anomaly; it was, moreover, an anomaly that was likely to become increasingly troublesome, since, for some years in France, Haüy had been using double refraction as a valuable exploratory tool for the investigation of crystal structure, and his work was still proceeding.[52]

Predictably, it was a confirmed supporter of the corpuscular theory of light and one of the most brilliant of Laplace's disciples at Arcueil, Étienne Malus, who was awarded the prize of 3000 francs on 1 January 1810. Indeed, it is probable that the subject for the competition was conceived by Laplace not simply in the hope of a glorious victory for the corpuscular theory but with Malus specifically in mind. For by 1807, despite some ten years of hard military service in the obscurity of Egypt and provincial France, Malus had already become closely associated with the style of physics that prevailed at Arcueil and appears even to have made contact with Laplace himself. In his earliest paper on light, which he read in 1807, he did not hesitate to assume the existence of short-range attractive forces,[53] and Laplace, in his own preliminary study of the theory of double refraction, which he read to the First Class of the Institute in January 1808, referred favorably to Malus' experimental work on the subject.[54] Presumably with the competition in view, Laplace, in this paper of 1808, also took the opportunity of pointedly laying down certain principles that he took to be established beyond doubt. Huygens' wave theory, he maintained, was inadequate for the explanation of double refraction,[55] so that the task ahead was clear: it was simply to devise a new explanation in terms of those short-range attractive and repulsive molecular forces that Newton had invoked in explaining ordinary refraction.[56] Since Laplace himself had already made inroads into the prob-

[52]See, for example, R.J. Haüy, *Traité de minéralogie,* 5 vols. (Paris, 1801), *1,* 229–235, and *2,* 38–51 and 196–229. For a brief account and further references see Burke, *op. cit.* (note 22), pp. 139–140. Haüy gave cautious support to Newton's theory of double refraction.

[53]See his "Traité d'optique," *Mémoires présentés à l'Institut National des Sciences, Lettres et Arts par divers savans et lus dans ses assemblées. Sciences mathématiques et physiques, 2* (1811), especially 265–266. Read 20 April 1807. Laplace was one of the referees appointed by the First Class for this paper; see *Procès-verbaux, 3,* 516 and 606–607 (20 April and 19 October 1807).

[54]Laplace, "Mémoire sur les mouvemens de la lumière dans les milieux diaphanes," *Mémoires . . . de l'Institut, 10* (1809), 300–342, especially pp. 302 and 309.

[55]*Ibid.,* pp. 301 and 303.

[56]*Ibid.,* p. 304.

lem, as he showed in his paper, and since he left no doubt as to his own belief in the physical reality of the molecular forces, it would have been an act of remarkable folly to attack the question set in December 1807 in any other way than Laplace's, and, as we should expect, Malus duly conformed, producing the vindication of the Laplacian position that was expected of him.[57]

As patron Laplace had been well served; specific problems had been answered for him, and the position of his program had been strengthened. And Malus had more than just his prize-winning paper to contribute to the Laplacian program. For example, after his discovery of the polarization of light in the autumn of 1808 he had given his now considerable authority to the view that the new phenomenon could not be explained by Huygens' wave theory, and instead had given an explanation in terms of the various forces which he supposed, in the best Laplacian fashion, to act on the particles of light.[58] In fact, Malus did so much for Laplacian physics in general and for the furtherance of the Laplacian program in particular that his premature death from consumption in February 1812, after some five years of intensive research, must have come as a grievous blow to Laplace.

However, there was no question of abandoning the program, and there were still outstanding issues to be settled. Foremost among these were the problems concerned with the behavior of elastic surfaces. Laplace had already stated that the theory of such surfaces might be established in terms of short-range intermolecular forces of repulsion,[59] and in 1809 a competition on the subject had been set by the First Class of the Institute.[60] Although the competition was stated to have been suggested by Napoleon, who had been greatly impressed by a demonstration of the experiments of Chladni, we may be sure that Laplace, whose zeal for his program was then at its height, had had a hand in the matter. But, presumably to his disappointment, by the closing date on 1 October 1811 no entry of sufficient merit had been received, and the competition had to be set twice more, with closing dates in October 1813 and October 1815, before the

57His paper was published in *Mémoires présentés . . . par divers savans, 2* (1811), 303–508, with the title "Théorie de la double réfraction." For Malus' attempt to explain double refraction in terms of short-range forces of the Laplacian type see especially pp. 489–496.

58Malus, "Sur une propriété des forces répulsives qui agissent sur la lumière," *Mémoires de physique et de chimie de la Société d'Arcueil, 2* (1809), 254–267, especially pp. 260–267.

59See the first passage quoted in note 35.

60*Mémoires . . . de l'Institut, 9* (1808), 240–241 of the "Histoire de la Classe" for 1808.

prize was eventually awarded to Sophie Germain in January 1816.[61] This award represented something of a defeat for Laplacian interests, since Germain's approach was fundamentally different from that of Poisson, whose paper on elastic surfaces, read to the First Class of the Institute on 1 August 1814, used short-range forces in the orthodox Laplacian manner.[62] And, insofar as it was a defeat, it has significance as a mark of Laplace's declining influence and reputation after the downfall of Napoleon. But this is a point I shall return to in Section 3.

Another outstanding problem that was settled in the last years of Napoleon's rule, though this time with rather more success from the Laplacian point of view, concerned the theory of heat. Again the issue was raised in a prize competition of the Institute, set in January 1811. As was made clear by the committee that proposed the subject, it was hoped above all that the competition, which asked for a detailed experimental study of the specific heats of gases, would lead to a decision on an important point in the caloric theory.[63] Since the point was one that had to be resolved before even the simplest mathematical treatment of the theory could be undertaken, it was of obvious interest to Laplace, and naturally he sat on the committee that set the competition.[64] The aim was to decide whether it was possible for some caloric to exist in a body in a latent, or combined, state (i.e., without being detected by a thermometer) or whether all of the caloric was present in its "sensible" form and therefore as a contribution to the body's temperature. The issue, which had been very much a live one since the 1780's, though never more so than in the first decade of the nineteenth century, had divided calorists into two groups; the supporters of the former view looked chiefly to Lavoisier, Laplace, and Joseph Black as their authorities, while those who advocated the latter, such as Adair Crawford and John Dalton, followed the Scottish pupil of Black, William Irvine. Predictably enough, victory went, in January 1813, to two young men, François Delaroche and Jacques Étienne

[61] On the history of this competition see I. Todhunter, *A History of the Theory of Elasticity and the Strength of Materials from Galilei to Lord Kelvin*, 2 vols. (Cambridge, 1886–1893), *1*, 147–149.

[62] Poisson, "Mémoire sur les surfaces élastiques," *Mémoires . . . de l'Institut*, Pt. 2, *13* (1812), 167–225, especially pp. 171–172 and 192–225.

[63] *Mémoires . . . de l'Institut*, Pt. 2, *11* (1810), xcv of the "Histoire de la Classe" for 1810.

[64] See *Procès-verbaux*, *4*, 399 (3 December 1810). He did not sit on the adjudicating committee but was well represented by Berthollet and Gay-Lussac; see *Procès-verbaux*, *5*, 105 (12 October 1812).

Bérard, who had performed their experiments at Arcueil and who, by upholding the distinction between latent and sensible caloric, vindicated the position long favored by Laplace.[65] One cannot help feeling that the only other competitors, Nicolas Clément and Charles Bernard Desormes, who deviated from the Laplacian view, simply had no chance and were fortunate to receive even the "honorable mention" that was accorded them.

Despite a certain slackening of corporate research activity from 1812, Laplacian physics was still, to all appearances, in a strong position in France toward the end of the Napoleonic period. Certainly the fact that regular meetings of the Society of Arcueil stopped in 1813[66] did not augur well for the future. But Laplacian influence was still just as great in French scientific education as it had long been in research and, because of strong administrative centralization, it was just as easily exercised. An examination of syllabuses, textbooks, and sets of lecture notes of the period shows clearly that pure Laplacian physics was being taught as standard doctrine both in science courses in the *lycées*[67] and, what is even more important, in the courses that mattered most for the future of French physical science, those at the École Polytechnique. Here at the École Polytechnique, as we know from Hachette's handbook for students of 1809,[68] from the annually published outline syllabuses,[69] and even more clearly from some notes taken (by none other than Auguste Comte)

[65]The paper by Delaroche and Bérard appeared in *Annales de chimie et de physique, 85* (1813), 72–110 and 113–182, with the title "Mémoire sur la détermination de la chaleur spécifique des différens gaz." For a detailed discussion of the competition and the issues at stake in the caloric theory see Fox, *op. cit.* (note 8), especially pp. 25–32 and 104–150.

[66]Crosland, *op. cit.* (note 4), pp. 2–3.

[67]See, for example, R.J. Haüy, *Traité élémentaire de physique,* 1st ed. (Paris, an XII [1803]); 2nd ed. (Paris, 1806); 3rd ed. (Paris, 1821); all three editions are in two volumes. This excellent textbook was written at Napoleon's direction for the use of teachers in the *lycées.*

[68]J.N.P. Hachette, *Programmes d'un cours de physique; ou précis de leçons sur les principaux phénomènes de la nature, et sur quelques applications des mathématiques à la physique* (Paris, 1809), especially pp. 1–8 and 49–220.

[69]See, for example, *Programmes de l'enseignement de l'École Royale Polytechnique arrêtés par le Conseil de Perfectionnement, dans la session de 1815–1816* (Paris, 1816), pp. 34–40, where it is clear that even in 1816 heat, electricity, magnetism, and optics were still to be taught in terms of the appropriate imponderable fluids. On the changes in the published syllabus that began to appear in the volume for 1817–1818 see Section 3 and notes 141 and 142.

at Petit's lectures of 1814–1815,[70] the existence of the imponderable fluids—caloric, light, electricity, and magnetism—went virtually unquestioned, and other aspects of Laplacian physics, notably the treatment of capillary action, were given great prominence.

So this was the situation in French physical science up to the end of the reign of Napoleon. Laplace and Berthollet stood for and fostered a unified program for their disciplines, based on a coherent set of traditional doctrines that had originated in Newton's comments on matter and molecular forces in the *Opticks* and had then come down to them through the eighteenth century. And by their zeal and power within the scientific community of France they had established a situation in which they were able to give at least certain of the main branches of physics and chemistry a remarkable degree of uniformity.

Now it must be stressed that the uniformity imposed by the program was by no means complete. In the first place, there were those, outside the Arcueil circle and usually outside the Parisian "establishment" of science, who opposed Laplace. I have referred already to the "outsiders" Clément and Desormes, and in the next section I shall argue that Fourier's mathematical treatment of the distribution of heat in solids appeared as a major challenge as early as 1807. Moreover, by the time of the Bourbon Restoration, Fresnel was already working, in almost total isolation, toward his critique of the corpuscular theory of light. And there were many more whose research was not opposed to Laplacian principles but independent of them. For example, the work of Gay-Lussac and Thenard (both members of the Arcueil circle) on the alkali metals and electrochemistry, Gay-Lussac's experiments on the combining volumes of gases (though they had subversive implications), and J. P. Dessaignes' study of phosphorescence (which won the Institute's prize competition for physics in 1809)[71] simply did not bear on the Laplacian program or, in any direct

[70]The notes, in six notebooks bearing the heading "École polytechnique—Cours de physique de M^{r}. Petit—Comte," cover all the main branches of physics with the exception of light. Comte was in his first year at the École Polytechnique in 1814–1815, and light was always studied in the second-year physics course. The notes are now kept at the Maison d'Auguste Comte, 10 rue Monsieur le Prince, Paris 6^{e}. I am grateful to the resident archivist, D. Cantemir, for allowing me to examine the notes and for providing me with a copy of them.

[71]For accounts of this work see, in addition to the standard histories, Crosland, *op. cit.* (note 4), pp. 354–365, and, by the same author, "The Origins of Gay-Lussac's Law of Combining Volumes of Gases," *Annals of Science, 17* (1961), 1–26. Dessaignes' prize-winning paper appeared as "Mémoire sur les phosphorescences," in *Journal de physique, 68* (1809), 444–467, and *69* (1809), 5–35.

way, on the theories on which the program was based. The same may be said of the brilliant crystallography of this period, although the fact that Haüy, like Romé de l'Isle before him, chose to concentrate on the description and measurement of crystals, rather than on the theory of crystallization,[72] which could have included a study of the forces binding together crystals and their constituent molecules,[73] may be seen as a missed opportunity for Laplace and his program.

But, despite these deviations and independent traditions of work and despite Laplace's own occasional vacillation,[74] the uniformity in the physical science of Napoleonic France is striking. Even if in chemistry there had been little progress, the program in physics had been pursued for a decade with vigor and, to all appearances, success. And certainly in France at the beginning of 1815 there seemed no reason why the dominant orthodoxy that had emanated from Arcueil since the early years of the century should, at least in the foreseeable future, be abandoned.

3. THE REJECTION OF LAPLACIAN PHYSICS

Yet within about ten years, by the mid-1820's, the intricate structure of Laplacian physical science had collapsed, leaving just a few increasingly isolated diehards to pursue the chimera that the program and its attendant beliefs were then generally recognized to be. In these ten years of revolt against Laplacian orthodoxy, the tradition that had gone almost unchallenged in the physical sciences in the Napoleonic period was abandoned. To the men who led the revolt it undoubtedly seemed that a new and more glorious era was dawning. Indeed, it may lead us to see Napoleonic science in a somewhat less favorable light if we accept, as I believe we must, that these men shared much of that feeling of exhilaration and liberation of the intellect which Guizot, Edgar Quinet, Lamartine, and so many

[72]On the style of Haüy's work see Metzger, *op. cit.* (note 22), pp. 170–206, and Burke, *op. cit.* (note 22), pp. 78–106.

[73]For example, Guyton de Morveau had discussed the cause of crystallization at some length in the "Essai physico-chimique sur la dissolution et la crystallisation," in his *Digressions académiques, ou essais sur quelques sujets de physique, de chymie & d'histoire naturelle* (Dijon, 1762), pp. 271–359, and in his *Élémens de chymie théorique et pratique,* 3 vols. (Dijon, 1777–1778), *1*, 49–78. With acknowledgments to the work of Clairaut and Buffon, he treated the problem in terms of the same intermolecular forces by which he accounted for other chemical phenomena.

[74]See below, pp. 111 and 127, and notes 93 and 143.

others associated with the Bourbon Restoration.[75] The men of science may not have gone so far as to see the Empire as an intellectual "desert," as Quinet did,[76] but, contrary to general belief, I do feel that most of them participated fully in the new optimism which so many Frenchmen experienced in those early Restoration years. Sadly, this enthusiasm and spirit of optimism, which was to be so fruitful in literature and the arts, in science came to nothing. For, as we can now see, what emerged from the ruins of Laplacian orthodoxy was not the new, revivified physical science that the early years of the Restoration had seemed to promise, but only a burst of creativity whose duration was no less brief, and whose decline was even more drastic, than that of Napoleonic physical science.

The men chiefly responsible for the revolt of the decade 1815–1825 were Joseph Fourier, Pierre Dulong, François Arago, Augustin Fresnel, and Alexis Thérèse Petit. Of these only Fourier, born in 1768, was over thirty in 1815, so that, with this one exception, they had learnt their science and performed their earliest work in the period when Laplacian principles had enjoyed their greatest success in France. Dulong, Arago, Fresnel, and Petit had all been thoroughly indoctrinated with these principles as students at the École Polytechnique;[77] and Dulong and Arago had even been members of the Arcueil circle (though, significantly perhaps, only since about 1810), and both had benefited in their careers from Arcueil patronage.[78] Petit, by contrast, was not a member of the circle, but his brilliant doctoral thesis of 1811 on capillary action[79] and his first lectures as professor of physics at the École Polytechnique in the winter of 1814–1815[80] were as Laplacian as they could possibly have been.

[75] F.P.G. Guizot, *Mémoires pour servir à l'histoire de mon temps,* 8 vols. (Paris, 1858–1867), *1*, 27–58; E. Quinet, *Histoire de mes idées. Autobiographie,* 7th ed. (Paris, 1895) [vol. 15 of the *Oeuvres complètes d'Edgar Quinet*], especially pp. 177–187 and 239–247; A.M.L. de P. de Lamartine, *Des destinées de la poésie* (Paris, 1834), in *Oeuvres complètes de Lamartine publiées et inédites*, 41 vols. (Paris, 1860–1866), *1,* 30–32. In certain ways, notably in matters of religion, the Restoration did not bring greater freedom, but generally the characterization of the period by Guizot, Quinet, and Lamartine seems just.

[76] Quinet, *op. cit.* (note 75), p. 241.

[77] In, respectively, 1801–1802, 1803–1805, 1804–1806, and 1807–1809. Because of ill health Dulong failed to complete the course.

[78] See Crosland, *op. cit.* (note 4), pp. 315–318.

[79] Petit, "Théorie mathématique de l'action capillaire," *Journal de l'École Polytechnique,* cahier 16, *9* (1813), 1–40. On his continued support of the Laplacian theory after 1815 see note 141.

[80] See note 70.

It is probably no coincidence that the challenge to the prevailing orthodoxy was first raised by the two members of the anti-Laplacian group, Fourier and Fresnel, who spent the greater part of the Napoleonic period in provincial obscurity far from Paris and hence far from the center of Laplacian control.[81] Fourier was nearly twenty years older than any other member of the group, and even when teaching at the École Polytechnique soon after its foundation in 1794 he had affiliated more closely with Monge than with Laplace and Legendre. With time taken from heavy prefectural duties at Grenoble (imposed on him by Napoleon in 1802 soon after his return from distinguished service in Egypt), he prepared a massive treatise on the distribution of heat in solid bodies, which he read to the First Class of the Institute in December 1807.[82] In this the entire Laplacian machinery of derivation of the basic equations by Newtonian principles was ignored by Fourier, who concentrated instead on his own methods for their derivation and solution. In 1811 he took his work further when he submitted a revised version of the 1807 paper for a prize competition set by the First Class of the Institute, and in January 1812 he was awarded the prize. But, despite evidence that he was receiving some favor in the eyes of Laplace himself,[83] the challenge was premature. His 1807 paper was published only in the form of an abstract, drawn up by a less than enthusiastic Poisson;[84] and even his great prize-winning paper was criticized by the judges (Lagrange, Laplace, Legendre, Malus, and Haüy) and, apparently as a result of this criticism (for which Lagrange was chiefly responsible), did not appear in print until 1824–1826,[85] when

[81] For the relevant biographical details of Fourier and Fresnel see the article by J.R. Ravetz and I. Grattan-Guinness (on Fourier) and that by R.H. Silliman (on Fresnel) in C.C. Gillispie, ed., *Dictionary of Scientific Biography* (New York, 1972), *5*, 93–99 and 165–171; also I. Grattan-Guinness (in collaboration with J.R. Ravetz), *Joseph Fourier 1768–1830. A Survey of his Life and Work, based on a Critical Edition of his Monograph on the Propagation of Heat, presented to the Institut de France in 1807* (Cambridge, Mass., and London, 1972), pp. 14–25 and 441–459.

[82] *Procès-verbaux, 3,* 632 (21 December 1807). On the 1807 paper see J.R. Ravetz, "Preliminary Notes on the Study of J.B.J. Fourier," *Archives internationales d'histoire des sciences, 13* (1960), 247–251; I. Grattan-Guinness, "Joseph Fourier and the Revolution in Mathematical Physics," *Journal of the Institute of Mathematics and Its Applications, 5* (1969), 230–253; and, for a detailed study and critical edition, Grattan-Guinness, *op. cit.* (note 81).

[83] Discussed in Grattan-Guinness, *op. cit.* (note 81), pp. 444–452.

[84] [Fourier], "Mémoire sur la propagation de la chaleur dans les corps solides," *Nouveau bulletin des sciences par la Société Philomathique de Paris, 1* (1807–1809), 112–116 (in the issue for March 1808).

[85] Fourier, "Théorie du mouvement de la chaleur dans les corps solides," *Mémoires de l'Académie Royale des Sciences de l'Institut de France, 4* (1819–1820 [published

the whole spirit of French physical science was very different and when Fourier himself was a permanent secretary of the Academy of Sciences. So it was only when the sympathetic Arago became one of the joint editors of the reorganized *Annales de chimie et de physique* in 1816 that the public had the opportunity of learning any details of Fourier's achievement (by means of a lengthy summary that appeared in the *Annales*[86]).

It was about this time that Fresnel, too, began to make his mark on the Parisian scientific scene. In October 1815, having recently gained some months of leisure for research (a leisure that he owed incidentally to his expulsion from office for his royalist sympathies during the Hundred Days), he deposited his first paper on the diffraction of light at the Institute.[87] In this he gave powerful support to the wave theory of light and in doing so exposed serious shortcomings in the rival corpuscular theory. Immediately he won over Arago and Petit, hitherto good Laplacians, and by December 1815 these two new converts had even performed some experiments on refraction in gases which they interpreted in such a way as to support Fresnel.[88] Ampère, whose commitment to the anti-Laplacian cause became really apparent only in the early 1820's, had

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1824]), 185-555, and *5* (1821-1822 [published 1826]), 153-246. On the criticism and delay in publication see G. Darboux's introduction to the *Oeuvres de Fourier*, 2 vols. (Paris, 1888-1890), *1*, vi-viii; also Arago's *éloge* of Fourier in *Mémoires de l'Académie des Sciences, 14* (1838), cxii-cxiii.

[86]"Théorie de la chaleur. Par M. Fourier. (Extrait)," *Annales de chimie et de physique, 3* (1816), 350-375. The volume from which the summary was made, described in a footnote as a quarto volume of 650 pp., never appeared. It would have been normal for Arago himself to prepare the summary, but it displays such familiarity with Fourier's work that Fourier himself was probably the author. This is the conclusion reached in Grattan-Guinness, *op. cit.* (note 81), p. 460n. If the summary was written by someone else, Sophie Germain may have been responsible, as Ravetz has suggested to me.

[87]Fresnel, "Mémoire sur la diffraction de la lumière, où l'on examine particulièrement le phénomène des franges colorées que présentent les ombres des corps éclairés par un point lumineux," *Annales de chimie et de physique, 1* (1816), 239-281. Presented to the First Class of the Institute on 23 October 1815 (*Procès-verbaux, 5*, 562).

[88]Arago and Petit, "Sur les puissances réfractives et dispersives de certains liquides et des vapeurs qu'ils forment," *Annales de chimie et de physique, 1* (1816), 1-9. Read to the First Class of the Institute on 11 December 1815. Two letters that illustrate Arago's enthusiasm for Fresnel's ideas during his period of conversion (both of them from Léonor Mérimée to Fresnel) are in *Oeuvres complètes d'Augustin Fresnel,* 3 vols. (Paris, 1866-1870), *2*, 831-833. According to Léonor Mérimée's letter of 20 December 1814 (*ibid., 2*, 830-831), Arago first learnt of Fresnel's work in December 1814. On the support that the work of Arago and Petit gave to Fresnel see Fox, *op. cit.* (note 8), pp. 202 and 233-234.

been won over by May 1816,[89] and even Berthollet's former *protégé* Gay-Lussac, now beginning to take on the mantle of his master as France's leading chemist, was sympathetic.[90] The interest in this challenge to the Laplacian position was enormous, and it was reflected most obviously in January 1817 in the decision of a committee of the Academy of Sciences, consisting of Laplace, Biot, Berthollet, Gay-Lussac, and the aged physicist J. A. C. Charles, to offer the prize in physics for a study of diffraction.[91] It seems clear that in this way the still powerful Laplacian party hoped to settle the issue finally in its own favor by bringing this important phenomenon of physical optics into line with polarization and double refraction, which had been explained so successfully in terms of the corpuscular theory. To Biot victory for a corpuscularian was an especially alluring prospect since, with the young C. S. M. Pouillet, he had recently been engaged in devising the corpuscular theory of diffraction which he described, with much other evidence likely to support the materiality of light, in his *Traité de physique* of 1816.[92] But the ruse—if such it was—backfired, for, despite the fact that among the five judges were Laplace himself and the two arch-Laplacians Biot and Poisson, Fresnel won the prize in March 1819 with a brilliant paper.[93]

[89]See Ampère's letter to Ballanche Fils, 19 May 1816, in *Correspondance du grand Ampère,* ed. L. de Launay, 3 vols. (Paris, 1936–1943), *2,* 511. Ampère was in no sense a typical or central figure in the anti-Laplacian group, but in his conflict with Biot in the early 1820's (see below, pp. 117) he attacked some of the fundamental beliefs of Laplacian physics. The central forces that were so important in his electrodynamic theory were decidedly, and significantly, not Laplacian in character, and a clear mistrust of Laplacian fluids can be seen in his *Théorie mathématique des phénomènes électro-dynamiques* (see below, pp. 117 and 128 and note 153). Moreover, in the 1820's Ampère experienced the direct opposition of Laplace concerning the possible identity of magnetism and electricity. In supposing the two to be identical Ampère was breaking with the view of Coulomb, which Laplace supported; see Ampère's letter to Davy, probably of 1825, in *Correspondance du grand Ampère, 2,* 680.

[90]See Arago, "Éloge historique de T. Young," *Mémoires de l'Académie des Sciences, 13* (1835), cii–civ.

[91]*Procès-verbaux, 6,* 138 (13 January 1817).

[92]Biot, *Traité de physique expérimentale et mathématique,* 4 vols. (Paris, 1816), *4,* 743–775. In this volume of the *Traité* Biot wrote at great length on polarization (pp. 254–600), which he felt to be adequately explained by his (corpuscularian) theory of mobile polarization. As Frankel has pointed out to me, the *Traité* has great importance both as a restatement of Laplacian doctrines, especially in optics, and as evidence of continuing work on the program after the death of Malus.

[93]Fresnel, "Mémoire sur la diffraction de la lumière," *Mémoires de l'Académie des Sciences, 5* (1821–1822 [published 1826]), 339–445. On the identity of the judges see *Procès-verbaux, 6,* 345 (27 July 1818). The other judges were Fresnel's good

However, on the question of the nature of light the Laplacians did not give up easily. In 1837 William Whewell raised the possibility that Laplace, Biot, and Poisson were chiefly responsible for a seven-year delay in the publication of Fresnel's prize-winning paper, and it seems not inconceivable that they even resorted to such underhand methods as mislaying some of his other papers.[94] But they were fighting a losing battle, as even Biot finally recognized when, in the early 1820's after some protracted and acrimonious debate with Arago, he retired from the Parisian scientific community (in particular, from the Academy of Sciences) for several years,[95] and so, at least in the eyes of his contemporaries, conceded victory to his adversary.[96]

The successful attack on the corpuscular theory of light helped to

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friend Arago and Gay-Lussac who, despite his closeness to Berthollet and Laplace, had already shown some sympathy toward the wave theory, as we have seen. It is interesting to speculate on the course of the discussions that gave Fresnel his victory. Arago and presumably Gay-Lussac would have supported Fresnel, while Poisson, always the most orthodox of Laplacians, and Biot, whose acrimonious public debate with Arago was now imminent, would surely have opposed him. Perhaps, therefore, it was Laplace himself who swayed the decision. It becomes less difficult to imagine Laplace supporting anti-Laplacians when we note how in 1822 he was to vote for Fourier rather than Biot in the election for a new permanent secretary of the Academy of Sciences; see note 143. And already he had shown some favor to the work of Fourier when he might have been expected to support Poisson; see Grattan-Guinness, *loc. cit.* (note 83).

[94]For Whewell's somewhat speculative account see his *History of the Inductive Sciences,* 3 vols. (London, 1837), *2,* 408–411; also his recollection in his paper "Comte and Positivism," *Macmillan's Magazine, 13* (1866), 355–356, where it is stated that Arago had told Whewell that the Laplacian domination of French physical science had been so effective about 1815 that he had actually been afraid to voice his early support for Fresnel. The lengthy delays in the publication of the prize-winning papers by Fourier and Fresnel should, of course, be compared with the delay of little more than a year in the publication of the winning paper of Malus (see note 57), but it should also be noted that it was not only members of the anti-Laplacian group who suffered in this way. Cauchy, for example, had to wait more than ten years before his prize-winning paper of 1815 on water waves was published, with additions, in *Mémoires présentés par divers savans à l'Académie Royale des Sciences, 1* (1827), 3-312.

[95]The *Procès-verbaux* for the period show that Biot's appearances at the meetings of the Academy of Sciences were infrequent from the autumn of 1822 until the early 1830's. Between the end of January 1823 and the end of 1824 he was present on only three occasions. About this time, however, he was not inactive and was engaged, for example, in preparing the third edition of his *Précis élémentaire de physique* (note 108).

[96]Arago was left, as Guglielmo Libri put it in the *Revue des deux mondes,* ser. 4, *21* (1840), 799, "maître du champ de bataille." See the passage quoted on pp. 123–124.

create an atmosphere in which it was natural that other Laplacian beliefs should be subjected to a new scrutiny. Once action at a distance was discredited in one branch of the Laplacian program, it became far easier to attack it in other branches; and the program in its strict and complete sense naturally collapsed completely. Moreover, the threat to Laplace and his school was heightened by another challenge that achieved success almost simultaneously. This was the challenge of Sophie Germain, whose victory in the Academy's prize competition in 1816 was, as I have already mentioned,[97] both a blow to Laplacian interests, as represented above all by Poisson, and a sign of diminishing Laplacian control. Like the criticism of the corpuscular theory of light, Germain's treatment of elastic surfaces stimulated a prolonged controversy, which lasted far into the 1820's and engaged Poisson in a bitter debate with a group of critics inspired by Fourier.[98]

In such conditions of mounting criticism it seems plausible to interpret the attack on the caloric theory which accompanied Petit and Dulong's announcement of their famous law of atomic heats in 1819[99] as a natural product of a questioning mood that had come to prevail in French science since 1815. In fact, such an interpretation seems necessary. For, despite the confidence with which Petit and Dulong stated their criticism of caloric,[100] the justification for their attack in terms of experimental facts was far from conclusive. They brought forward virtually no new evidence, and the attack was one that could have been made equally well ten years earlier.[101] However, as we know, it was not made at that time—and this in itself is strong evidence that the general intellectual atmosphere of the Napoleonic period was very different from that which existed within five years of Napoleon's downfall.

Another illustration of the changing atmosphere may be found in chemistry, where, at precisely the same time as the first attacks on Laplacian physics were being launched, there was a similar turning away from related

[97] See above, p. 106.

[98] See below, pp. 118–119.

[99] Petit and Dulong, "Recherches sur quelques points importans de la théorie de la chaleur," *Annales de chimie et de physique, 10* (1819), 395–413, especially pp. 396–398 and 406–413.

[100] A confidence that is especially apparent in Dulong's letter to Berzelius, 15 January 1820, in *Jac. Berzelius Bref,* ed. H.G. Söderbaum, 6 vols. in 14 parts (Uppsala, 1912–1932), Pt. 1, *2,* 13–14.

[101] For an account of the criticisms by Petit and Dulong see R. Fox, "The Background to the Discovery of Dulong and Petit's Law," *The British Journal for the History of Science, 4* (1968–1969), 1–22, especially pp. 9–16.

principles that had gone virtually unchallenged in the Napoleonic period. In this challenge the break with the past was manifested not so much by an explicit, open attack on Berthollet's chemistry as by the gradual acceptance of Dalton's atomic theory, which directed attention away from molecular forces to combining weights. Again it is 1815, when France renewed close contact with Britain, which seems to be the turning point, for until that date the atomic theory, opposed by Berthollet, had made little headway in France.[102] Incidentally, I need hardly say that I consider it significant that the same man who appeared as the leading French critic of the caloric theory from 1819—Dulong—was also known, from 1816, as the most enthusiastic supporter of the atomic theory in chemistry.[103]

So the attack on the caloric theory in 1819 and the new support for the atomic theory both seem to reflect the critical spirit that was abroad in French science during the early years of the Restoration, insofar as neither depended essentially on any startling new discoveries or observations. Yet discoveries and experimental evidence did contribute to the weakening of the Laplacian position. It was a great blow to the corpuscular theory when, in 1819, experiment confirmed the prediction based on Fresnel's wave theory that there should be illumination at the center of the diffraction pattern of a small opaque disc,[104] and the discovery of Dulong and Petit's law did much to strengthen the atomists' case.

Perhaps the best illustration of the importance of a discovery in weakening Laplacian physics is in electromagnetism. Following Oersted's observation of the magnetic effect of a wire carrying an electric current in 1820, French physicists zealously engaged in the investigation of the new phenomenon, and Biot and Ampère were quickly among the most prominent of them. There were, of course, problems for the Laplacians. For example, electromagnetism introduced a rotational force which had no obvious connection with the central forces of Laplacian physics; and Coulomb, whose views on the electrical and magnetic fluids had become part of the Laplacian orthodoxy, had denied the possibility of an interaction between

[102] The early history of the theory in France is well described in M.P. Crosland, "The First Reception of Dalton's Atomic Theory in France," in Cardwell, *op. cit.* (note 32), pp. 274-287.

[103] See Fox, *op. cit.* (note 101), pp. 16-18.

[104] On this crucial experiment, which was suggested by Poisson in his capacity as one of the judges for the competition on diffraction but which did little to shake his confidence in the corpuscular theory, see Ronchi, *op. cit.* (note 49).

electricity and magnetism.[105] But Biot, then at his most belligerent, was undaunted, and inevitably, it seems, there was conflict.

Ampère's theory of electromagnetic interaction contained much that Biot found objectionable.[106] In particular, he protested at Ampère's attempt to reduce not only electromagnetic phenomena but even the forces between magnets to interactions between current-carrying conductors; magnetic forces, in his view, had been explained perfectly well in terms of Coulomb's two fluids of magnetism. And the fact that Ampère retained fluids of electricity was no consolation, for Ampère's fluids were thoroughly un-Laplacian and apparently had more in common with Fresnel's ether than with the fluids of Coulomb.[107] Between 1821 and 1824 Biot put forward his alternative explanation, while pursuing a policy of faint praise, misrepresentation, and open criticism toward the work of Ampère.[108] By 1824, when he published the third edition of his *Précis élémentaire de physique expérimentale,* he had developed fully a theory in which the forces of electromagnetism were explained in terms of magnetic interactions between tiny magnets which he supposed to be arranged in a circular fashion around the current-carrying wire.[109] For Biot, convinced of the correctness of Coulomb's explanation, the cause of magnetic interactions was, of course, not in doubt, so that his electromagnetic theory appeared to him a highly satisfactory one. But the model did not withstand the scrutiny of Ampère, who quickly demonstrated its weaknesses, while defending himself against the charges that his own theory was un-Newtonian.[110] So discredited, Biot's theory was soon forgotten, and the attempt to treat the exciting new phenomena in accordance with Laplacian principles had failed.

105See L.P. Williams, "Ampère's Electrodynamic Molecular Model," *Contemporary Physics, 4* (1962), 113–114.

106For my treatment of Biot's response to the work of Ampère I am greatly indebted to E. Frankel, *Jean Baptiste Biot: The Career of a Physicist in 19th-Century France* (Univ. of Princeton Ph.D. thesis, 1972), of which the author has kindly allowed me to see the relevant chapters.

107See Williams, *op. cit.* (note 105), pp. 118–122.

108See especially the cursory treatment of Ampère's work in Biot's paper "Sur l'aimantation imprimée aux métaux par l'électricité en mouvement," *Journal des savants* (1821), pp. 221–235, and the overt criticism in his *Précis élémentaire de physique expérimentale,* 3rd ed., 2 vols. (Paris, 1824), *2,* 771–772.

109Biot, *Précis* (note 108), *2,* 766–771.

110See the many references to Biot in Ampère, *Théorie mathématique des phénomènes électro-dynamiques, uniquement déduite de l'expérience* (Paris, 1826), especially pp. 180–188.

By the mid-1820's, then, the position of the Laplacian orthodoxy had been gravely weakened. The attacks had been directed, for the most part, against established beliefs in various branches of physical science, but there is evidence of diminishing Laplacian authority in mechanics also. The relevant debate dates principally from the 1820's, although several of the issues had been raised some years earlier, notably in the prize competition won by Sophie Germain in 1816. It concerned the methods to be used in rational mechanics, particularly in the study of elastic media.[111] Underlying the debate, in which Fourier's *protégés* Navier and Germain were opposed most frequently by Poisson, there was a fundamental opposition between the "physical mechanics" (*mécanique physique*) advocated by Poisson and the style of the Fourier school, which Poisson described as "analytical mechanics" (*mécanique analytique*) and (incorrectly) associated with the name of Lagrange.[112] Although he had been using his style of mechanics as early as August 1814,[113] it was in a paper of April 1828 that Poisson stated its principles most clearly, when he supported it in the following terms:

> Let me add that it would be desirable for geometers to re-examine the leading problems of mechanics from this point of view, which is at once physical and consonant with nature. In order to discover the general laws of equilibrium and motion, it was necessary to treat them in a completely abstract manner; and, as far as treatments of this general and abstract kind are concerned, Lagrange went as far as anyone could imagine when he replaced the physical connections between bodies by equations relating the coordinates of the various positions they occupied. It is this that constitutes analytical mechanics. But besides this wonderful conception we can now establish physical mechanics, the sole principle of which is to reduce everything to the molecular actions which

[111]For a factual account see Todhunter, *op. cit.* (note 61), *1,* 133-160 and 277-285.

[112]For discussions of this opposition see P. Duhem, "L'évolution de la mécanique, *Revue générale des sciences* (1903), pp. 127-132; L. Brunschvicg, *L'expérience humaine et le causalité physique* (Paris, 1922), pp. 327-337; J.W. Herivel, "Aspects of French Theoretical Physics in the Nineteenth Century," *The British Journal for the History of Science, 3* (1966-1967), 121-125. In various ways Fourier and his associates, Navier and Germain, were related back to the Basel-St. Petersburg school of rational mechanics of the earlier eighteenth century, a school that embraced the Bernoullis and Euler. The line of descent to the nineteenth century touches Lagrange only incidentally and altogether bypasses Laplace.

[113]In Poisson, *op. cit.* (note 62).

> convey from one point to another the effects of the given forces and are the agents maintaining the equilibrium between these forces. If we proceeded in this way, it would no longer be necessary to draw up special hypotheses when one wanted to apply the general rules of mechanics to particular questions. Thus, in the problem of the equilibrium of flexible strings the tension that is introduced to achieve a solution would be the direct result of the actions of the molecules on one another when they are displaced slightly from their natural positions. In the case of an elastic membrane, the bending moment of elasticity would be a consequence of these same actions, taken throughout the whole thickness of the sheet, and the expression for it would be established without any hypothesis. And, finally, the pressures exerted by fluids both internally and on the walls of the vessels containing them would also be the resultant of the actions of the molecules on the surface under pressure, or rather on an extremely thin layer of fluid in contact with each surface.[114]

In seeking to explain phenomena in terms of the attractive and repulsive forces operating on the molecular scale, Poisson was of course declaring his allegiance to those principles which had dominated the *Système du monde* and the *Mécanique céleste,* but by the late 1820's, with Laplace now dead, he was increasingly isolated. He had no school to support him, and through the 1830's the advocates of Fourier's style of mechanics, led by Lamé and encouraged by Comte, carried all before them.[115]

Given this evidence, it is probably not too much to say that by the early 1820's there had emerged in France certain anti-Laplacian principles, not only in physics but also in chemistry and mechanics, to which all critics of Laplacian science could subscribe. Of these, skepticism toward the traditional imponderable fluids, sympathy for Dalton's atomic theory, the new rational mechanics of Fourier and his followers, and Ampère's electrodynamics were the most obvious. That those who sought to break with Laplace and his school had so many shared beliefs and operated on such a broad intellectual front is striking. For instance, Petit, as well as evidently sharing Dulong's skepticism toward caloric and his enthusiasm for the atomic theory, was among the earliest supporters of Fresnel's wave theory,

[114] Poisson, "Mémoire sur l'équilibre et le mouvement des corps élastiques," *Mémoires de l'Académie des Sciences, 8* (1829), 361-362. Read 14 April 1828.

[115] Brunschvicg, *op. cit.* (note 112), p. 331.

as were Dulong and Ampère.[116] Fresnel, for his part, was a critic of the traditional caloric theory and influenced Ampère in his work on electromagnetism,[117] and Arago not only championed Fresnel in his difficult early years after 1815 but also took a keen and highly favorable interest in the work of Petit and Dulong on heat.[118] Likewise, Fourier, whose role may be seen as that of a benign, influential, but rather detached patron of the new generation, expressed his support for the wave theory[119] and, by his extreme caution on the question of the nature of heat, notably in his *Théorie analytique de la chaleur* (1822), implied unmistakable criticism of caloric.[120] Such unanimity is, in fact, hardly surprising, for the members of the anti-Laplacian group were in close, almost daily contact in the scientific circles of Paris,[121] where they all lived and worked. Between some of them the relationship was especially close. Petit and Dulong were most intimate friends (until Petit's untimely death in 1820), and the same may be said of Ampère and Fresnel and of Arago and Fresnel. Arago, moreover, became Petit's brother-in-law when Petit married in November 1814.

Throughout the Restoration period the reaction of the Laplacian party to the growing criticism was complex. If we are to judge by Biot's response to Fresnel's wave theory, for example, the party felt the attacks keenly. But in their publications, at least, they gave the impression that little had changed. A comparison of the third edition of the *Précis élémentaire* (1824) with the *Traité de physique* (1816) shows that Biot was willing to make few concessions with regard to the imponderable fluids.[122] And

[116]See Petit and Dulong, *op. cit.* (note 99), p. 396; Dulong's letters of 15 January 1820 and 10 November 1825 to Berzelius, in *Berzelius Bref* (note 100), Pt. 1, *2,* 13 and 64; and his comment in *Annales de chimie et de physique, 31* (1826), 180-181. Also see above, pp. 112-113.

[117]See his "Complément au mémoire sur la diffraction," dated 10 November 1815, in *Oeuvres complètes d'Augustin Fresnel* (note 88), *1,* 59-60; also his letters of 5 July 1814 and 11 July 1814 to Léonor Fresnel, *ibid., 2,* 820-822 and 827-829. The connection between Fresnel and Ampère is discussed in Williams, *op. cit.* (note 105), pp. 118-120.

[118]See Fox, *op. cit.* (note 101), p. 2.

[119]In a report, written jointly with Ampère and Arago, on a paper by Fresnel on double refraction. See *Annales de chimie et de physique, 20* (1822), 337-344.

[120]The book began (on p. i) with the words: "First causes (*les causes primordiales*) are unknown to us; but they are subject to simple, unvarying laws which can be discovered by observation and the study of which is the object of natural philosophy."

[121]Fresnel is, in part, an exception since until 1818 his visits to Paris were restricted to periods of leave from the Corps des Ponts et Chaussées.

[122]Certainly, in the *Précis* (note 108), *1,* 466, and *2,* 2, Biot confessed ignorance

Laplace and the ever-loyal Poisson continued to work out their programs in accordance with the principles laid down two decades earlier.[123] Even in the fifth edition of the *Système du monde,* published in 1824, Laplace gave no indication that he had modified his view in any way,[124] and between 1821 and 1823 he confidently proceeded to devise and publish what was easily the most detailed of all versions of the caloric theory accounting for the physical and thermal properties of gases in terms of those short-range forces that he still supposed to govern all phenomena on the molecular scale.[125] Indeed, it was in a paper on the subject published early in 1822 that he first gave the classic statement of the Laplacian program, one version of which is reproduced at the head of this paper. In his work on caloric in the 1820's Poisson showed a similar disregard for the criticisms of Petit and Dulong (and of others, including that important ally of the anti-Laplacian cause Berzelius).[126] And even as late as

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of the true nature of heat, electricity, and magnetism, but he denied that the corpuscular theory had been discredited and maintained that the evidence still favored it (*2,* 130–132 and 452–463). In his *Traité de physique* (note 92), *1,* 66–68, he admitted that the existence of a fluid of heat was not certain, but the existence of fluids of electricity and magnetism was "very probable" (*1,* 7–8), and the materiality of light was "beyond doubt" (*3,* 148–149). This confident statement concerning light conflicts with his assertion, in the dedication of the *Traité* to Berthollet (*1,* xx–xxiii), that it was impossible to know its nature "with certainty." The caution that Biot displays, even in the *Traité,* is striking and it has to be compared with his criticism of fluids, referred to in note 43. Having regard to his optical work and his vigorous defense of the corpuscular theory, however, I feel (with Frankel, *op. cit.* [note 106]) that his caution was formal and that it does not convey the true measure of his conviction which is apparent in his major interpretative papers.

[123] On Laplace's changing attitude to Fourier, however, see below, p. 127, and notes 93 and 143.

[124] See, for instance, the "Avertissement" to his *Exposition du système du monde,* 5th ed., 2 vols. (8vo) (Paris, 1824), *1,* v, where Laplace wrote that he intended to make molecular forces the subject of a special supplement. Work on this project was never completed, as we see from the "Avertissement" to the quarto version of the sixth edition, published posthumously in 1835, and in this edition a chapter on molecular attraction which had appeared on pp. 315–357 of the (quarto) fourth edition (Paris, 1813) was simply reinstated (as Chapter XVIII of Book IV, on pp. 323–364). The chapter did not appear in the octavo versions of the sixth edition published in Paris and Brussels in 1827.

[125] This theory appeared first in a series of papers in the volumes of the *Connaissance des tems* for 1824 and 1825 (published respectively in 1821 and 1822) and was given its definitive form in April 1823 in Book XII of the *Mécanique céleste* (note 1), *5,* 87–144.

[126] See, for example, his "Mémoire sur les équations générales de l'équilibre et du mouvement des corps solides élastiques et des fluides," *Journal de l'École Polytechnique,* cahier 20, *13* (1831), 1–174, especially pp. 4–8. Read to the Academy of

1835 we find him publishing a lengthy work, the *Théorie mathématique de la chaleur,* in which the existence of caloric and its traditional properties were taken as no less axiomatic than they would have been, say, thirty years before.[127] But by 1835 Poisson's book, although its author does not seem to have recognized the fact, was a relic of a bygone age, an anachronism in terms both of its physics and of its laborious and inelegant mathematics. And, to judge by the almost complete silence in which it was received, it was seen as such by his contemporaries.[128] By the 1830's Poisson was a lone, almost pathetic figure, clinging vainly to an ideal of a "physical mechanics," based on Laplacian principles, which was unrealizable. When he died in 1840, the mathematician Guglielmo Libri wrote of his funeral, in a notice of singular warmth and affection:[129] "Never, since the death of Cuvier, had anyone seen such general sorrow nor a cortège accompanied by so many demonstrations of grief of every kind."[130] But in reality there were few men of note to mourn him. He had no official *éloge,* and even the biographical memoir by the permanent secretary of the Academy of Sciences, Arago, who had been for so long the scourge of the Laplacian school, was never read in full.[131]

Sciences on 12 October 1829. On Berzelius as an ally of the cause see Fox, *op. cit.* (note 8), pp. 241-243 and 246-248.

127 Poisson, *Théorie mathématique,* especially p. 7. The problem of heat diffusion in solids, which was treated in the *Théorie mathématique,* had been of great interest to Poisson for over twenty years. As is pointed out in Grattan-Guinness, *op. cit.* (note 81), pp. 466-470, his labored contributions on the subject are in sharp contrast with the elegant and incisive treatments of Fourier, and in a paper published in July 1823 Poisson did go so far as to acknowledge Fourier's priority with regard to most of his own results, though presumably with some reluctance; see his "Mémoire sur la distribution de la chaleur dans les corps solides," *Journal de l'École Polytechnique,* cahier 19, *12* (1823), 1-2. However, by way of justification of his own work, he stressed that his methods for deriving the results were different from Fourier's and that he had used Laplace's assumption that heat transfer within a solid was a short-range phenomenon (pp. 2-6).

128 The rare comments which have been found, such as that by J.D. Forbes in his *Review of the Progress of Mathematical and Physical Science* (Edinburgh, 1858), p. 154, and the anonymous review in the Swiss *Bibliothèque universelle, 59* (1835), 144-166, are generally critical.

129 [Libri], "Lettres à un Américan sur l'état des sciences en France—III. M. Poisson," *Revue des deux mondes,* ser. 4, *23* (1840), 410-437.

130 *Ibid.,* p. 429.

131 And even when extracts from the memoir were read at the public meeting of the Academy, on 16 December 1850, Arago was absent; see the *Comptes rendus hebdomadaires des séances de l'Académie des Sciences, 31* (1850), 840. The whole memoir was printed in the *Oeuvres complètes de François Arago,* 17 vols. (Paris and Leipzig, 1854-1862), *2,* 593-689.

Naturally enough purely intellectualist factors were not alone in bringing about the move from Laplacian science. This was not simply a case of new principles being measured against old ones and being found superior, although there was something of this in the situation, especially with regard to the debate over the nature of light. Other relevant factors include the weakening of the authority of the Arcueil circle after regular meetings had ceased in 1813.[132] And possibly even the personal unpopularity incurred by Laplace in the early years of the Bourbon Restoration played its part. On this unpopularity, in which political considerations seem to have loomed large, Libri wrote (with reference to the debate between Biot and Arago on the nature of light):

> M. Biot and M. Arago were among the first to participate. Unfortunately, instead of serving to strengthen the ties that bound them, the fact that they were engaged in the same field of study became the source of lively exchanges which culminated in a dramatic break between them; and the Academy was frequently moved by the strife between these two rivals who, in their heated debates, sometimes allowed themselves to be unduly carried away, especially when discussing questions of priority, which are always so delicate. Other *savants* joined in these discussions, and since Laplace, a man who wanted problems to be treated geometrically rather than in any other way, had appeared to take sides against Arago, enemies were raised to oppose him on every side; Legendre was put up deliberately as an adversary; the hand of friendship was offered to anyone who attacked the results contained in the *Mécanique céleste;* and all the liberal press was aroused and directed against those of whom we had once been so proud, men who, it was said, were now just old idols that had to be destroyed. Because the geometer Laplace had become the Marquis de la Place and on the pretext that some other academicians belonged to the Société des Bonnes Lettres,[133] these men were pro-

[132]On the decline of Arcueil, to the death of Berthollet in November 1822, see Crosland, *op. cit.* (note 4), pp. 395–428.

[133]The Société des Bonnes Lettres was a mainly literary society founded in the early years of the Restoration by Louis Fontanes and Chateaubriand, both champions of the Bourbons. Taking it as their aim "to revive the taste for good doctrines and good literature," most of the members supported traditional religion and monarchy, with the result that the society quickly became a byword for antiliberalism and hence the object of a good deal of popular suspicion. Under the influence of Chateaubriand and Charles Nodier, it helped to strengthen the early association between royalist sentiments and the new romanticism in literature. Among other leading members were the antiquaries Désiré Raoul Rochette and Quatremère de Quincy,

claimed ignoramuses, in the name of the Charter, in all the newspapers. It was then, as I have already said, that members of the public began to be admitted to the Academy,[134] and there they became the supporters of those who did not wish to excel solely by science. Laplace was put to silence, M. Biot stayed away from the Institute for several years, and M. Arago remained master of the battle field.[135]

Laplace's name, as Libri suggests here, seems to have become a byword for illiberalism in certain quarters (notably in the circle of the liberal writer P. L. Courier) in the early years of the Restoration, and it remained so until long after his death. Indeed, in the freer atmosphere of the Orléans monarchy, which did little to encourage restraint, criticisms of his "pliability" (*souplesse*) in political and personal matters and of his failure to defend the freedom of the press became common.[136] Perhaps the criticisms in his own lifetime were not so severe as to hasten his death, as the author of one standard biographical sketch maintained in 1834,[137] but there is sufficient evidence to dispel any image of Laplace living his last

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the orientalists Antoine Léonard de Chezy and Jean Pierre Abel de Rémusat, and the writer Eugène Destains. The activities of the society, which included poetry readings by Victor Hugo, are best studied in the thirty-three volumes of its official publication, the *Annales de la littérature et des arts,* which appeared between 1818 and 1829. For a brief account see C. Dejob, *L'Instruction publique en France et en Italie au dix-neuvième siècle* (Paris, n.d.), pp. viii, 210-225, and 441-444. Dejob points out that the society was established as a royalist answer to the Parisian Athénée, founded in 1781 by Pilâtre de Rozier, where ideas more in keeping with the traditions of the eighteenth-century Enlightenment were discussed. Libri's comparison of Laplace with Legendre, who suffered, for political reasons, at the Restoration, was an obvious one to make.

134 The ease with which journalists and the general public could gain access to the meetings of the Academy of Sciences remained a source of grievance long after this date; see, for example, Biot's comments in his *Mélanges scientifiques et littéraires* (note 43), *2,* 257-264 (first published in the *Journal des savants* for February 1837).

135 [Libri], "Lettres à un Américain . . . ," *Revue des deux mondes,* ser. 4, *21* (1840), 798-799.

136 See, for example, the entry on Laplace in A. Rabbe, V. de Boisjolin, and Sainte-Preuve, *Biographie universelle et portative des contemporains,* 5 vols. (Paris, 1834), *3,* 151-153, especially p. 151. Valentin Parisot was equally critical in his article on Laplace in the *Bibliothèque universelle, ancienne et moderne,* 83 vols. (Paris, 1811-1853), *70* (supplément), 237-260, especially pp. 239-244. A most unflattering description of Laplace's shifting political views appeared in the article on him by E. Merlieux in the *Nouvelle biographie générale,* ed. F. Hoefer, 46 vols. (Paris, 1855-1866), *29,* cols. 533-534. Poisson's behavior was subject to similar criticism in the article on him in Rabbe, *et al., op. cit., 5,* 591.

137 Rabbe, *et al., op. cit.* (note 136), *3,* 151.

years as a universally respected elder statesman of French science. By 1827 his reputation was severely tarnished.

And as Laplacian influence waned, so inevitably the leading members of the new anti-Laplacian generation were able, if only by virtue of age and seniority, to gain control of the still centralized scientific community of Paris. It was important, for example, that when the need for a wholesale reorganization of the *Annales de chimie* was felt, following the death of the secretary to the journal, Collet-Descotils, in December 1815, it was Arago (rather than, say, Biot) who became one of the two new editors.[138] Since this happened just after Arago's conversion to the wave theory of light and at a time when his relations with Biot were about to worsen rapidly, the appointment was crucial in the transfer of power from the Laplacian group. The publication of the work of Fourier and Fresnel, which followed with remarkable (and significant) rapidity,[139] soon gave a clear intimation of the changing allegiance of the *Annales* and, because of the established authority of the journal, did much to strengthen the anti-Laplacian position.

Almost as important as this new domination of the most prominent of the French research journals was the way in which critics of Laplace were able to exert influence at the École Polytechnique after 1815. Petit, for instance, had been made professor of physics there in 1814—as a good Laplacian and for reasons quite unconnected with the subsequent debate[140]—and he remained in the post until his death in 1820. Petit was followed in his turn by Dulong, who remained as professor until 1830, when he became Director of Studies for the École Polytechnique as a whole. The chief results of this sixteen-year tenure of the chair of physics by Petit and Dulong were, first, a marked rise both in the quality and the amount of physics taught and, second, some predictable changes of doctrine. In the published syllabus for 1817–1818, for example, the state-

[138] Crosland, *op. cit.* (note 4), pp. 404–406. Arago had special responsibility for physics, while Gay-Lussac, the other editor, was responsible for contributions on chemistry.

[139] Fresnel's first paper on diffraction appeared in the issue for March 1816 (see note 87), and the December issue contained the lengthy summary of Fourier's work cited in note 86. Moreover, the very first issue of the new series, in January 1816, began with an account of the recent experiments by Arago and Petit that strongly supported Fresnel; see note 88.

[140] The cause of Petit's promotion to the chair after some five years as a teaching assistant (*répétiteur*), first in analysis and from 1810 in physics, was the unsatisfactory standard of the teaching of J.H. Hassenfratz, who had been professor of physics since the foundation of the École in 1794; see Fox, *op. cit.* (note 8), pp. 231–232.

ment that light would be "treated as an emission from luminous bodies," which had appeared in earlier syllabuses, was omitted.[141] And in the syllabus for 1821–1822 all references to "caloric" (*calorique*) were eliminated and replaced by references to "heat" (*chaleur*).[142]

Changes that told against Laplacian interests also took place in the Academy of Sciences. Arago, then an orthodox Laplacian, had been elected back in 1809, and Ampère became a member in November 1814; but it was only after the final overthrow of the Empire that they were joined by the men who were to become their chief allies. Fourier, for example, was not elected to the place that he had so long deserved until 1816, and Dulong and Fresnel followed only in 1823, Navier in 1824. However, the really decisive election at the Academy dates from November 1822, when the anti-Laplacian cause gained its most glorious victory through the defeat of Biot by Fourier for the post of permanent secretary for the mathematical sciences.[143] The vote, thirty-eight to ten, was not overwhelming, but from that point Laplacian science was doomed, and the election of Arago to replace Fourier as permanent secretary in 1830 only sealed its fate, ushering in a period that sympathizers of Laplace seem to have resented bitterly.[144]

With the crumbling of the power of the Laplacian group, each of its members adopted his own strategy for survival. As has already been noted,

[141] *Programmes de l'enseignement de l'Ecole Royale Polytechnique, arrêtés par le Conseil de Perfectionnement, pour l'année scolaire 1817–1818* (Paris, n.d.), p. 35; cf. *Programmes . . . arrêtés . . . dans la session de 1815–1816* (Paris, 1816), p. 39. Naturally Petit did not effect a wholesale rejection of Laplacian doctrines, and in *Annales de chimie et de physique*, *5* (1817), 404–406, he even defended Laplace against criticism of his theory of capillary action.

[142] *Programmes . . . arrêtés . . . pour l'année scolaire 1821–1822* (Paris, n.d.), pp. 31–32; cf. *Programmes . . . arrêtés . . . pour l'année scolaire 1820–1821* (Paris, n.d.), pp. 31–32. References to the fluid of electricity were far slower to disappear. In fact, it was only in *Programmes pour l'admission et pour l'enseignement à l'École Polytechnique, arrêtés par la commission nommée en exécution de la loi du 5 juin 1850, et approuvés par le Ministre de la Guerre* (Paris, n.d.), p. 93, that earlier references to "electric fluid" (*fluide électrique*) were replaced by references to "electricity" (*électricité*); cf. *Programmes pour l'admission et pour l'enseignement . . . arrêtés . . . pour l'année scolaire 1849–1850* (Paris, 1850), p. 29, where "electric fluid" is used.

[143] Arago was also a candidate in this election but openly gave his support to Fourier. It is interesting to note that, despite a show of strict impartiality, Laplace appears to have voted for Fourier rather than for Biot; see *Oeuvres complètes de François Arago* (note 131), *1*, 100–101; see also note 93.

[144] Libri, for example, launched a violent personal attack on Arago on pp. 796–812 of the first of his (unsigned) "Lettres à un Américain" of 1840 (cited in note 135). His main charge was that, as a result of Arago's consistent abuse of his position, the activities of the Academy of Sciences had become increasingly trivial.

Biot chose to retire from the scientific scene of Paris in the early 1820's, but in the 1830's he seems to have mellowed and he returned to resume a valuable career in which his patronage of Pasteur in his early researches on crystals was perhaps his most important contribution. Poisson, by contrast, remained stubbornly loyal to the doctrine of Laplace until his death in 1840. In fact, he seems to have pursued the program with even greater zeal than the master himself, who, at least on certain issues, showed some signs of trying to adjust to the winning side. For example, after Fourier had produced an estimate for the age of the earth from geothermal considerations in 1819, Laplace wrote of his work in a decidedly complimentary manner.[145] And we must not forget that it was apparently Arago rather than Biot who had Laplace's vote in the momentous election at the Academy of Sciences in 1822.[146] Laplace's support for Fourier and his behavior in 1822 could, of course, only serve to alienate Poisson, and in view of the shifting allegiances of the 1820's it is remarkable only that so many personal friendships survived.[147]

So by the mid-1820's the style of science that had appeared so right and unassailable in the Napoleonic period had been abandoned by the leading figures in a new generation; and the peculiar organizational structure centered on Arcueil, which had provided essential support for the old science, had collapsed, leaving power in new hands.[148]

4. THE NEW AGE

It remains now to examine what was built on the ruins of Laplacian physics. The problem is a difficult one and simple statements are not possible. For, despite the solidarity of those who turned against Laplace and his disciples in the years after 1815, there emerged no single well-defined new style of science that was capable of filling the gap left by the

[145]See especially Laplace, "Sur la diminution de la durée du jour par le refroidissement de la terre," *Annales de chimie et de physique, 13* (1820), 416–417.

[146]See note 143.

[147]For example, when Dalton visited Arcueil in July 1822 there was every sign of friendship in a gathering for dinner that included Berthollet, Laplace, Biot, Fourier, and Arago; see the accounts in H.E. Roscoe, *John Dalton and the Rise of Modern Chemistry* (London, 1895), pp. 178–181. This, of course, was some four months before the election for the new permanent secretary.

[148]Ravetz has pointed out to me that a similar transfer of power took place in the early 1820's in the Société Philomathique of Paris, with Poisson and Biot giving way to non-Laplacians, notably H.M.D. de Blainville (soon to become a follower of Comte) and Fresnel; see Fox, *op. cit.* (note 8), pp. 272–273.

old and of yielding a clear program for the future. And this should not surprise us, since a certain diversity of approach was a natural enough product of a period of reaction against the Laplacian orthodoxy, and diversity in any case reflects a more normal situation in science than the one that had prevailed under Napoleon.

It has been suggested (with an eye on Poisson's distinction between "physical mechanics" and "analytical mechanics") that what has here been described as Laplacian physics was followed by a turning toward a positivist approach, and the emergence of the positivist strain of the 1820's and 1830's onward has been cited as the beginning of the end of French theoretical physics.[149] Now Fourier's program for the science of heat, modeled on the traditional rational mechanics in which the causes are taken as given, could indeed be interpreted as positivist in the sense the term later acquired in the philosophy of Comte (although the program was conceived by 1807 and so was not truly Comtean in any sense[150]). Moreover, a mathematical study of heat transfer based on the principles laid down most prominently by Fourier in his *Théorie analytique de la chaleur* of 1822[151] did continue through the 1830's, with Lamé and Duhamel as its most distinguished exponents.[152] More evidence for a positivist trend in French physics is to be found in Ampère's refusal to discuss causes in his *Théorie mathématique des phénomènes électro-dynamiques, uniquement déduite de l'expérience* of 1826[153] and in his decision to present his theory in terms only of observed phenomena; i.e., in terms of forces such as those that were known from his own experiments to exist between two current-carrying conductors. And in any discussion of positivist science it is obviously impossible to omit Comte himself, who expressed the prevailing skepticism toward the Laplacian imponderable fluids in an extreme form when he wrote on the subject in the mid-1830's.[154]

[149]Herivel, *op. cit.* (note 112), especially pp. 121–132.

[150]On Fourier's work in 1807 see above, p. 111.

[151]The style of Fourier's treatment is conveyed in the opening words of the "Discours préliminaire" quoted in note 120.

[152]See G. Bachelard, *Étude sur l'évolution d'un problème de physique. La propagation thermique dans les solides* (Paris, 1928), pp. 89–132.

[153]See especially pp. 4–8 of the book. It is hardly necessary to point out that Ampère did not adopt this positivist stand in all his work. On the two faces of Ampère see L.P. Williams, *Michael Faraday. A Biography* (London, 1965), pp. 143–144.

[154]I.A.M.F.X. Comte, *Cours de philosophie positive,* 6 vols. (Paris, 1830–1842), *2* (1835), 438–445.

But positivism, with its variants, was not the only, or even the dominant, philosophy to gain favor, at least in the early years of the attacks on Laplacian science. It is important to observe that of the men who were most closely involved in the revolt of 1815–1820, Dulong, Arago, Fresnel, and Petit were emphatically not positivists, although they appear to have been no less concerned at the errors perpetrated by the Laplacians than were Fourier and Ampère. Certainly they advocated caution, and they rejected many Laplacian doctrines; but they all wanted to substitute new theories for the ones they were criticizing, and they championed their theories—the wave theory of light, the atomic theory, the vibrational theory of heat—with enthusiasm and utter conviction. Moreover, they provided the basis for research traditions that were anything but positivist. Perhaps the best example of such a tradition, which grew from the work of Fresnel, was the search for a model for an all-pervading fluid ether that possessed at once the high elasticity of a solid and also the capacity to allow solid objects, such as planets, to pass through it unhindered.[155] This problem, which quite defied solution until the work of Stokes in the 1840's, together with a number of related problems engaged some of the great men of nineteenth-century physics from the late 1820's until the rise of electromagnetic theory in the 1880's. With mathematical physicists of the stature of MacCullogh, Green, William Thomson, and Maxwell involved, the tradition was by no means exclusively French, but major French contributions were made by Navier and, more particularly, Cauchy.

So positivism was certainly not the one philosophy that rose to take the place of the Laplacian principles prevailing in the Napoleonic period; and still less was it a *cause* of the rejection of these principles. Positivism, in fact, did not emerge formally as a recognizable strain in French physical science until after the short period of creativity which itself followed the discrediting of Laplace and his school. It was at most a symptom, and not a cause, of the state of physics in France after the mid-1820's. And in any case, as I have argued, it did not have the philosophical field to itself, even by the mid-century.

Despite the confusing diversity that characterized French physics for several decades following the abandonment of Laplacian orthodoxy, one thing is clear: the enthusiasm, zeal, and confidence of the decade 1815–1825 were quickly lost. And it is in this loss of intellectual impetus, how-

[155] On this work see E.T. Whittaker, *A History of the Theories of Aether and Electricity*, 2nd ed. (London, 1951), pp. 128–169.

ever caused, that I believe we must see one of the great turning points in the history of physical science in nineteenth-century France. In the last eighteen years of his life, for example, Dulong never again openly voiced his support for the modern vibrational theory of heat, despite the extreme confidence with which he had expressed it (and scorned caloric) in 1820.[156] There was no going back to caloric, of course, but, in the absence of the energy conservation principle, there was no going forward to a new theory of heat either. There was, in fact, widespread agnosticism, as is clear from the textbooks of the day.[157] By the mid-1830's the same was true of the atomic theory, which Dulong had championed so uncompromisingly as an anti-Laplacian doctrine some twenty years before. By this time and on this particular issue Dulong was not alone in his caution with regard to the physical reality of atoms, as we see from the guarded comments being made in the face of dauntingly complex problems concerning the determination of atomic weights by Jean Baptiste Dumas.[158] And there is a similar story of exasperation and unfulfilled hopes in the history of the wave theory of light, where the complex problems arose in the 1820's, 1830's, and 1840's in the search for a satisfactory model for the fluid ether.[159]

So the second quarter of the century in France seems to have been characterized by a failure to consolidate the gains of 1815–1825, and for a variety of reasons the men who once seemed so certain that they were retrieving the physical sciences from error and initiating a new golden age ceased to give French physical science the leadership it needed. Dulong, for one, was beset by frustrations and ill health and quickly lost heart for

[156]For evidence of Dulong's support for the vibrational theory see his letter of 15 January 1820 to Berzelius, cited in note 100 and quoted in Fox, *op. cit.* (note 101), p. 13, and *op. cit.* (note 8), p. 244.

[157]See Fox, *op. cit.* (note 8), pp. 275–277.

[158]J.B.A. Dumas, *Leçons sur la philosophie chimique* (Paris, 1837), pp. 231–290 (6th and 7th lessons). For accounts of the growing difficulties in the atomic theory and Dumas' mounting despair see G. Buchdahl, "Sources of Scepticism in Atomic Theory," *The British Journal for the Philosophy of Science, 10* (1959), 120–134, and Fox, *op. cit.* (note 8), pp. 282–295. As J.H. Brooke points out in his Cambridge University Ph.D. thesis "The Role of Analogical Argument in the Development of Organic Chemistry" (1969), pp. 61–68, the difficulties did not lead Dumas to positivism, even in the mid-1830's; indeed, Dumas can safely be described as a consistent realist with regard to chemical theory. But positivism did become prominent in French chemistry in the 1840's with the work of Charles Gerhardt; see Brooke, *op. cit.*, pp. 114–149.

[159]See note 155 for reference.

the struggle,[160] while Arago, like so many others, soon found the world of politics, in his case both academic and national, more alluring than the laboratory bench.[161] And, saddest blow of all, Petit, Fresnel, and Fourier were all dead by 1830; and Ampère died in 1836, just when Britain was beginning to assume European supremacy in the study of electricity and magnetism. Of these only Fourier had succeeded in establishing anything resembling a school of disciples to carry on the tradition of his work into the 1830's and beyond,[162] so that by the 1840's the thread of continuity with the early 1820's was tenuous indeed. It is significant in this respect that the most esteemed French physicist of the later period, Victor Regnault, had his intellectual roots not in the exciting years that followed the rejection of the Laplacian orthodoxy but in the subsequent period of diminishing impetus. In fact, the massive, dreary compilation of data which earned Regnault his high reputation and for which he is now best known[163] was begun in 1840 in answer to a plea from Dumas, who had urged a full experimental investigation of specific heats in an attempt to remove the notorious anomalies in current values for atomic weights.[164] Not surprisingly, the attitude to scientific investigation that Regnault's work reveals is strikingly similar to Dumas' at this time, being cautious but not truly positivist.

Regnault illustrates as clearly as any one man can the state of French physics at the middle of the century. Far from being an outsider like

[160]On Dulong's work between 1820 and 1838, when he died, see Fox, *op. cit.* (note 8), pp. 248-270.

[161]Arago was engrossed in his political activities, at the Academy of Sciences and as a liberal deputy, from 1830 until his death in 1853. Politics similarly enticed Dumas from science in later life, and Gay-Lussac and Thenard both gave much time to politics and public affairs. Among the less prominent men of science with strong political interests were Sadi Carnot, Charles Dupin, Galois, Desormes, Raspail, and Poncelet. The lure of public life in nineteenth-century France is discussed more fully in R. Fox, "Scientific Enterprise and the Patronage of Research in France 1800-1870," *Minerva, 11* (1973)

[162]See above, pp. 119 and 128.

[163]H.V. Regnault, *Relation des expériences entreprises par ordre de Monsieur le Ministre des Travaux Publics, et sur la proposition de la Commission Centrale des Machines à Vapeur, pour déterminer les principales lois et les données numériques qui entrent dans le calcul,* 3 vols. (Paris, 1847-1870). As well as being published separately in this form, the *Relation des expériences* also occupied almost the whole of three large volumes of the *Mémoires* of the Academy of Sciences (vol. 21, 1847; vol. 26, 1862; vol. 37, parts 1 and 2, 1868-1870). The pagination is identical in the two versions.

[164]On the work of Regnault and his debt to Dumas, see Fox, *op. cit.* (note 8), pp. 283-302 and 315-317.

Sadi Carnot, Galois, Laurent, or Gerhardt, he was in every sense a man of the scientific establishment, as the honor that was accorded him both inside France and, to a lesser extent, in the rest of Europe shows clearly enough;[165] and it is for this reason that a study of his career is unusually revealing. In nearly all respects he was ideally placed to participate in one of the most exciting developments in nineteenth-century physics—the discovery of the principle of the conservation of energy in the 1840's. Unlike many of his contemporaries in France, he had every material facility, in terms of laboratory equipment and assistance, that he could possibly have desired (because of generous government sponsorship), and by 1840, owing to his membership in the Academy of Sciences and his chair in chemistry at the École Polytechnique, he had the eminence and prestige to make his views felt. Moreover, he had the close familiarity with the operation of steam engines that seems to have been one of the most important elements in the intellectual makeup of the discoverers of the energy conservation principle.[166] Yet he failed; and he failed not only to make the discovery of the principle himself but also to appreciate its true significance when it had been made by others. Even by the mid-1850's Regnault recognized all too clearly the harm he had done himself by his preoccupation with experimenting, when he saw, sadly but too late, that the main course of physics had passed him by.[167] He was, I believe, a tragic figure, and he knew it only too well.

5. CONCLUSION

I have argued in this paper that between the end of the First Empire and the middle of the nineteenth century there occurred a highly significant change of style in French physical science. The change began, im-

[165]See Fox, *op. cit.* (note 8), pp. 299–300; also my article on Regnault in a forthcoming volume of the *Dictionary of Scientific Biography* (note 81). Regnault's was one of the few laboratories in France to attract students from abroad about the middle of the century. In addition to the students listed in Regnault, *op. cit.* (note 163), *2*, ix, the young William Thomson worked there in 1845; see S.P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1910), *1*, 122–133.

[166]See T.S. Kuhn, "Energy Conservation as an Example of Simultaneous Discovery," in M. Clagett, ed., *Critical Problems in the History of Science* (Madison, 1959), pp. 329–336.

[167]His disappointment can be observed in his comments in Regnault, *op. cit.* (note 163), *2*, iii and iv. Here, as also in vol. 1, p. 12, it is apparent that Regnault had seriously underestimated the magnitude of his task. His tone was certainly not that of someone content with his achievements.

mediately after 1815, with a sharp reaction against the leading tradition of the Napoleonic era. This reaction seems to have stimulated rather less than a decade of great creativity in physics, in which period scientists, along with many other French intellectuals of the day, apparently breathed more freely than they had done before 1815. But, in a way that remains to be analyzed in detail, enthusiasm and excitement were quickly dissipated and, despite the initial high promise, consolidation of the achievements of the first ten years of the Restoration period was not achieved.

This interpretation of the course of French physics in the first half of the nineteenth century leads naturally to a somewhat equivocal view of the achievements of the Napoleonic period. Of course, it is impossible to discredit completely an approach to physics that was capable of stimulating equally experimental work (such as Delaroche and Bérard's determination of the specific heats of gases, which remained standard until the 1820's), the highly sophisticated theoretical studies of Biot, Poisson, Malus, and Laplace himself, and at least one major discovery (polarization). Nor can it be doubted that these achievements owed much to the effectiveness of the highly centralized organizational structure that was developed in the period and that allowed Laplace, Berthollet, and their *protégés* to work together as one of the most closely knit schools in the whole history of science. But by the same token we must also observe certain grave weaknesses both in the content and in the strategy of Laplacian physics, as this was pursued under Napoleon. These weaknesses are to be seen in the excessively firm adherence to doctrines which, because their basic principles were open to such serious objection, were almost bound to be attacked and discredited once the influence of the Arcueil group waned. Certainly to scientists working during the First Empire, as to Pasteur a half century later, the years of Napoleonic rule were glorious ones for science.[168] But the historian, I believe, must take a different view. For he would merit no charge of writing Whig history if he were to assess many of the theoretical studies of the great mathematical physicists of the school of Laplace as little more than spectacular *tours de force* based on models that had largely outlived their usefulness. In fact, I would maintain that, far from being uniformly glorious, the period of Laplacian domination was

168The point cannot be missed in, for example, the annual reports on the work of the First Class of the Institute, published in the *Mémoires* of the Class. And it is explicit in J.B.J. Delambre, *Rapport historique sur les progrès des sciences mathématiques depuis 1789, et sur leur état actuel* (Paris, 1810), especially pp. 1–3 and 40–42, and in Cuvier's companion volume for the *sciences naturelles,* also published in 1810, especially pp. 389–394.

one in which French physics (and to a lesser extent chemistry) suffered from the imposition of theories, notably the theories of imponderable fluids, which were not only incapable of internal development but also quite inappropriate for the stimulation, or even the effective study, of the new experimental results that were transforming the physical sciences in the early years of the nineteenth century. It is worth noting, for example, that although, over a forty-year period that embraced the First Empire, Coulomb and Poisson between them were able to formulate the classic theories of static electricity and dipole magnetism, thereby completing a characteristically eighteenth-century research tradition, the exciting new field of electrochemistry had its origins not in France but in Britain, Germany, and Scandinavia. Significantly, too, it was in England that the freer, if less stimulating, intellectual climate allowed serious criticism of the imponderables of heat and light to get under way by the first years of the nineteenth century (in the writings of Rumford, Davy, and Young).

Hence I would suggest that the physical scientists of Napoleonic France had their notable successes when grappling with the outstanding problems of the eighteenth century. In the newer fields of research, by contrast, it is the paucity of their contributions that is remarkable.

In later life Arago recalled how the orthodoxy at Arcueil had been so rigid that at first he had not even dared to voice his support for the wave theory of light.[169] And in his biographical sketch of Gay-Lussac, read in 1851, he said of the Arcueil circle:

> For young men beginning in science it was a distinctly flattering situation to have, as the first judges and advisers in their work, men of European renown, such as Laplace, Berthollet, Humboldt, etc. But could one be sure that some preconceived ideas, which the best minds adopt more readily in what I may term an intimate gathering than before a large audience, were not such as to stifle the spontaneity of genius and to limit research to generally agreed problems? Also, was it not inevitable that the wish to display a fertile mind in the presence of the most famous *savants* of the day would sometimes lead men of lively intellect to commit themselves to rash theories?[170]

These comments, like much that Arago wrote, have a strong element of contentiousness and they cannot be accepted uncritically as reliable evidence. They ignore some of the great strengths of Arcueil. There were

[169] See note 94.
[170] *Oeuvres complètes de François Arago* (note 131), *3,* 33–34.

great leaders (albeit leaders steeped in the traditions of an age that was rapidly coming to a close), and under them there was a distinguished school of young men whose common objective gave their work at least coherence and momentum. But, in pointing to the way in which exclusiveness could lead all too easily to the perpetuation of "preconceived" doctrines that inhibited new research, Arago surely identified the Achilles' heel of Napoleonic science.

Of course, the weaknesses were not evident in the years of the Empire. The long-acknowledged distinction of Laplace and Berthollet, their influence, and their considerable personal fortunes (for which they had Napoleon, above all others, to thank[171]) allowed their control over teaching, research, and the careers of the rising generation to go unchecked and gave them every opportunity to pursue their own ambitions in science and to create an indebtedness and natural allegiance on the part of the young men who formed their school. But the situation in which they held such power, both directly (by the provision of research facilities at Arcueil) and indirectly (by their ability to manipulate the Institute and teaching institutions for their own advantage), was an unstable one, as was proved when, from 1815, the authority of the Arcueil school was seriously challenged for the first time.

In its suddenness and in its effectiveness the challenge of the years immediately after 1815 was remarkable, and it can only be explained by reference to a quite fortuitous combination of circumstances. I have referred already to Laplace's personal unpopularity following the Restoration; and the diminished income of the masters of Arcueil also had an effect, in that it made private patronage of research on a large scale impossible.[172] But perhaps the most important changes that accompanied

[171] On the incomes of Laplace and Berthollet, which exceeded 50,000 francs p.a. during the Empire, see Crosland, *op. cit.* (note 4), pp. 69–74. This figure should be compared with the 6000 francs paid to the permanent secretaries of the First Class of the Institute and to professors at the École Polytechnique. According to a letter cited in J.B. Morrell, "Science and Scottish University Reform: Edinburgh in 1826," *The British Journal for the History of Science, 6* (1972–1973), 51 (note 54), John Leslie was impressed to find Laplace and Berthollet with incomes of between £5000 and £6000 each when he visited Paris in 1814. At the current rate of exchange, this suggests an income in each case of over 100,000 francs p.a. Salaries paid to them as Senators (positions accorded them by Napoleon in 1799) were their chief source of income.

[172] By February 1816 Berthollet's income was reduced to 24,000 francs; see Crosland, *op. cit.* (note 4), p. 400. For a comment on the lack of research schools after the Restoration see note 115 of Fox, "Scientific Enterprise and the Patronage of Research" (note 161).

the Restoration were those that resulted simply from the passage of time. From 1815, when he was sixty-six, until his death in 1822, Berthollet increasingly felt the burden of old age and ill health;[173] and time had the even more important effect of bringing maturity, status, and the possibility of independent thought and action to men who once would have seen the favor and patronage of Arcueil as the surest way to success in research and in their careers as teachers.

However, even these changed conditions would not have sufficed to cause the sudden rejection of Laplacian orthodoxy. For this overthrow to come about, the old order had to be tested, and it was unfortunate for Laplace and his remaining disciples that so many major issues arose over such a short period of time. It was quite by chance, of course, that Fresnel emerged from isolation just when influential encouragement and a sympathetic audience were awaiting him in Paris. And the discovery of Dulong and Petit's law and Oersted's discovery of the magnetic effect of an electric current were likewise chance events which could easily have occurred before 1815 but which then would not have had the corrosive effect that they did have between 1815 and 1825.

So it was that one of the most distinguished schools in the history of physical science collapsed. As a research school it was impressive both for the boldness of the program that gave it coherence and purpose and for the enterprise with which its leaders sought to give it institutional strength. As an illustration of the power of "totalitarianism" in science, it is perhaps without equal, and for the historian seeking to explain, for example, why the French persisted so long and so keenly in their adherence to the theories of imponderable fluids, it displays the dark side of totalitarianism. For in the perpetuation of ideas that were kept immune from rigorous criticism at a time when reappraisal could have been beneficial there lay the dangers of the orthodoxy that Laplace and his followers tried to impose on French science.

[173]Crosland, *op. cit.* (note 4), pp. 398–401. See also Berthollet's letters to Berzelius, dated 20 December 1819 and 4 September 1820, in *Berzelius Bref* (note 100), Pt. 1, *1*, 70 and 73.