AMPÈRE, THE ETHERIANS, AND THE OERSTED CONNEXION

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IN 1826 André-Marie Ampère published the 'Mathematical theory of electrodynamic phenomena, uniquely derived from experiment', in which he showed how the mathematical law for the force between current elements could be derived from four ingenious equilibrium experiments.[1] He made a great show of following a Newtonian inductivist methodology, and his law, like Newton's for gravitation, was presented as a purely descriptive mathematical expression for a certain class of phenomena, one for which its author did not provide any causal or ontological justification. Ampère's electrodynamics would accordingly seem to have been a solid contribution to the Laplacian-Newtonian approach to physics so actively pursued in France during the first quarter of the nineteenth century.[2] It does not surprise us to read that his electrodynamic force law and his molecular-currents theory of magnetism were immediately and widely accepted by his French contemporaries.[3] Ampère was, in this view, just another of the many great French mathematical physicists of the period.

There are, however, serious problems with this picture. For one, Ampère's work was *not* warmly received by his contemporaries.[4] In fact, those who were the coldest were among those who should have been the warmest if the acceptability of a theory was simply a function of its mathematical-descriptive success. The three most prominent representatives of the style of physics which had been dominant in France during the first two decades of the nineteenth century, Laplace, Biot, and Poisson, were either hostile or silent. Many of those who did embrace Ampère's work did so because they shared his physical worldview and not because they were impressed by the success of his theories of electrodynamics and magnetism.[5] Furthermore, although Ampère presented his electrodynamics as a solid piece of Laplacian-Newtonian physics, he did not believe in action at a distance, and the strident positivism of much of his published work is glaringly at odds with many of his own deepest

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interests and beliefs.[6] In 1824 he wrote privately to Auguste de La Rive that 'I hardly doubt that the attractions and repulsions of electric currents are not, like [gravitational] attraction, a result of the motions of the fluid which fills all space'.[7] In 1825 he confided to Faraday that

> I think that everything which has happened in physics since the work of Dr Young on light and Oersted's discovery is preparing a new era for this science and that explanations derived from the effects produced by the motion of imponderable fluids will successively replace those accepted nowadays whose object is less to make known the true cause of the phenomena than to provide the shortest means for calculating them.[8]

At the same time that he was working on his grand memoir of 1826, where he argued for the scientific legitimacy of a severely Newtonian action at a distance formula, he wrote of his belief that one would soon know the 'true cause' of the phenomena of electricity, magnetism, and light, and that one would find that cause 'in the motions impressed on the fluid which fills space, formed by the union of the two electricities, and whose vibrations or a series of combinations and decompositions of these two elements constitute light'.[9] One wonders just what the connexion was between Ampère's mathematical theory and his physical world view. What did his phenomenological law of the interaction between current-carrying wires have to do with his conception of the current as a wave-like series of decompositions and recompositions of a neutral fluid, the same fluid which also underlay the phenomena of light, heat, and chemical activity?[10]

This short paper is part of a larger attempt to understand the diverse and often conflicting interests that resulted in Ampère's creation of electrodynamics. Existing accounts of his work have frequently overemphasized its logical character, as if the successive stages of Ampère's thought followed neatly one after the other.[11] Indeed, the idea that this work was coextensive with his 'thought' is already a gratuitous assumption. Similarly, although some writers have noted the similarities between Ampère's mature electrodynamics and the philosophy of science he had been developing during the foregoing twenty years, they have failed to see the primarily protective rather than directive role played by that philosophy.[12] Ampère's work must be seen as the result of a complex interaction between accidental discoveries, his physical world view, his philosophy of science, his mathematical ability, and his repeated frustration at not being able to get people to accept his views. There is no single or logically simple key to understanding the progress of his work on electrodynamics. Different thoughts and different preoccupations appear, disappear, and reappear in the course of his work, playing different roles at different times, as if Ampère's mind contained a collection of sometimes conflicting viewpoints which he could selectively draw upon or ignore when faced with a particular problem of public exposition or private speculation. This paper will explore the initial motivating force of one of the most persistent of those concerns, namely Ampère's attachment to a world view in which the modifications of an all-pervading ether provided a unifying explanation of chemical activity, heat, light, electricity, and magnetism. It cannot be overemphasized that this concern alone does not suffice to explain the full complexity of the development of his ideas over time. Nevertheless it does provide a plausible answer to a question which has scarcely ever been posed but which is one of the most glaring problems of Ampèrian historiography: Why was Ampère interested in electromagnetism in the first place? Nothing in his public career would have led one to expect the turn his energies took in his forty-fifth year.

It is well known that Ampère's imagination caught fire in September 1820, and that his work in electrodynamics possessed him for several years thereafter, but what had he been doing before then? At first blush the answer seems to be 'nothing'. Between his election to the Academy of Sciences on 28 November 1814 and his first post-Oersted paper of 18 September 1820, Ampère read just one paper before the Academy, a strictly mathematical treatment of the polarization of light.[13] The only other two papers he wrote and published during that six-year period were on the classification of chemical elements and on the integration of partial differential equations.[14] Otherwise his public scientific work was limited to the reading of an occasional commissioner's report on some mathematical or mechanical memoir presented to the Academy. Going further back, one finds that he had written a number of well-received mathematics memoirs in the years 1802-1805, when he was eager to acquire a Parisian reputation, and again in 1814, when he wished to strengthen his candidacy for the chair vacated at the death of Bossut.[15] Ampère did not for the most part like to do mathematics; he was just naturally good at it, and exploited his mathematical talents sporadically when he wished to advance himself professionally.[16] Despite his posts at the École polytechnique (where he was appointed Répétiteur d'analyse on 20 October 1804) and at the Academy, Ampère remained something of an outsider to the Parisian physical science establishment. Arago reported that Ampère's association with philosophers and psychologists such as Maine de Biran, Destutt de Tracy, Degérando, and Cabanis caused him not to be taken seriously by the scientists of the Academy.[17]

Ampère did harbour an abiding interest in chemistry, especially in the atomic theory and in Davy's electrochemistry and work on halogens.[18] His electrochemical views were an essential component of his physical world view, and in 1816 he was already publicly expressing the suspicion that there might be some underlying causal connexion between chemical affinity, electricity, and heat.[19] In 1814 he composed two important chemical memoirs, on 'Avogadro's' hypothesis and chemical combination, and on the relationship between Mariotte's law and the atomic theory of gases.[20] Yet it is only from Ampère's correspondence that one discovers his greatest lifelong interest to have been psychology and metaphysics, with his interest in chemistry a poor but important second. One finds strikingly few references to any *physical* phenomena before 1820—perhaps two or three. One of those was in 1813, when he cited Morichini's announcement of the influence of ultraviolet rays on magnetism.[21] It is no accident that his attention was caught by an instance of the interaction between two qualitatively different sorts of phenomena. Nonetheless, that was just once, and in 1813. It is still a problem to account for Ampère's sudden and passionate interest in electromagnetism in 1820.

The anomalous character of Ampère's reaction is heightened by the fact that most of his French scientific peers reacted with indifference or hostility to the very idea of electromagnetic interaction.[22] The first word of Oersted's discovery to reach Paris seems to have been Arago's report to the Academy on 4 September 1820.[23] A month later Dulong wrote to Berzelius from Paris that 'the news was initially received very coldly here. People thought it was another German *rêverie*'.[24] Ampère attributed people's hostility to Oersted's discovery to their believing in Coulomb's two-fluid theory of magnetism as a fact; their prejudice was such that when Arago gave his report on the new phenomena to the Academy, 'people rejected them just as they had rejected the stones fallen from the sky when M. Pictet at the time read a memoir on those stones at the Institut. Everyone decided it was impossible'.[25] Even Arago is said not to have believed a word of Oersted's discovery before re-doing it himself.[26] In contrast, Ampère seems from the start to have been open to the anomalous phenomenon. In a letter to his son of 25 September 1820 he apologized for not having written earlier.

> but all my time has been taken by an important circumstance in my life. Since I heard tell for the first time about the beautiful discovery of M. Oersted . . . I've thought about it continuously [and] I've done nothing less than write a grand theory about these phenomena and all those already known about the magnet.[27]

The 'problem of Ampère' which this paper addresses has two components: the relationship between Ampère's physical commitments and the positivistic tone of most of his published works, and the reasons for his immediate and positive response to Oersted's discovery of electromagnetism. To do full justice to the first question would require a longer excursus into Ampère's relationship with the Parisian scientific community than can be given here; for the present I would simply suggest that his public positivism was in large part an adaptation to what he believed community standards required, reinforced ultimately by certain features of his own philosophy of science.[28] Both questions, however, begin to make sense as soon as one appreciates Ampère's place in one of the central issues of the historiography of nineteenth-century French science: the revolt from Laplacian physics and its Newtonian programme of explaining phenomena in terms of attractive and repulsive forces acting at a distance between the smallest particles of matter.[29] Thanks especially to the work of Robert Fox, we know that something was going on between 1815 and 1825 that led to the fission of a major segment of the French scientific community into two opposing groups.[30] In this contest, in which personal and scientific rivalries reinforced each other, the established Laplacians—including especially Biot and Poisson—were challenged by a loose group composed of Arago, Fresnel, Dulong, Petit, and Fourier.[31] The chief protagonists in this feud were Biot and Arago, whose rivalry began in 1811 and had already become heated by 1813. Both men were working on the same problems in optics, and Arago thought Biot was unfairly encroaching on his territory and stealing his results.[32] In 1815 Arago became one of the earliest supporters of Fresnel's wave theory of light,[33] and the dispute reached its high point in 1821 when Arago sided with Fresnel against Biot.[34] Meanwhile Arago and Petit had mounted a direct attack on the Newtonian theory of emission,[35] and Dulong and Petit had come out in support of the chemical atomic theory and against the caloric theory of heat, which they suggested should be replaced by one based on the electrical nature of chemical combination.[36] Hence the battle lines had largely been drawn by late 1820, when Ampère enjoyed the quick support of Arago and Fresnel and encountered the equally quick opposition of Biot and Laplace. Arago recorded that the same (unnamed) people who had opposed Fresnel now opposed Ampère for the same (unspecified) reasons, though there can be no doubt whom he had in mind.[37]

My intention is to add Ampère's name to the group of anti-Laplacians. In addition, I would suggest that those men might also be referred to as the 'etherians' in order to underscore their attachment to a physical world view in which the ether played a unifying conceptual role. Indeed, the etherians' interest in connecting together disparate phenomena on the basis of a particular physical world view stood in sharp contrast to the phenomenological-positivistic stance of, for example, Biot, who was quite content to deal with the various classes of physical phenomena as if they had no connexions at all with each other.[38] Indeed, it seems that Biot and Poisson, beyond their positivistic public pronouncements, really were not as interested as the etherians in having a *physical* picture of phenomena and their possible interconnexions. Biot never offered any kind of physical explanation or plausibility argument in favour of his theory of a 'magnetism impressed on metals by electricity in motion', nor was Poisson apparently bothered by the fact that his 1824 theory of magnetism provided no imaginable explanation of electromagnetic interaction. Ampère *did* think and argue in terms of physical plausibility and he *was* bothered by the gap between reality (and one's image of it) and the more restricted domain of positive scientific knowledge. Misled, perhaps, by the positivist and 'Newtonian' tone of some of Ampère's writings, historians have not hitherto appreciated his central position in this anti-Laplacian movement nor his deep commitment to an etherian world view. It is my contention that Amprère's participation in this struggle on the side of the etherians provided an essential general motivation for his work on electromagnetism, and that his identification of Oersted with this outsider's view of science provided a further particular stimulus to that work. (The identification of specific historical antecedents to Ampère's etherian ideas lies outside the scope of this paper. Whatever the sources of these ideas, their prime appeal lay in the prospect of achieving a unified physical explanation of a broad range of phenomena.)

Let us look at Ampère's ties with the anti-Laplacian etherians. Arago—'general Arago', as Fresnel's uncle called him in 1817[39]—played a strategic role as co-editor of the *Annales de chimie* from 1816, and as a vigorous early supporter of both Fresnel and Ampère.[40] However, his role in these skirmishes seems to have been mainly strategic and supportive, and I have not found that he contributed significantly to the conceptual elaboration of the etherians' programme. We are told that Fourier was also an early supporter of Ampère, but his closer relationship with the etherians, and his commitment (if any) to their physical world view, are questions that still need to be investigated.[41]

The evidence for Ampère's close and consequential collaboration with Fresnel is substantial.[42] To begin with, Fresnel was interested early in his career in just those problems that concerned Ampère. In 1811 he was busy with psychology and chemistry.[43] We are told—though without elaboration—that Fresnel had a close relationship with Ampère and others in the psychological circle of Maine de Biran.[44] In 1814 his doubts about the caloric theory of heat and the emissions theory of light were connected with his concern with problems of chemical combination.[45] Reactions such as the burning of carbon, where the production of a gas is accompanied by the release of quantities of heat and light, presented a problem for the caloric theory but could be explained if heat and light were not substances but the motions of a fluid. Noting his inability to believe that light particles could be emitted with the same velocity under so many different circumstances, Fresnel wrote that he was

> very tempted to believe in the vibrations of a particular fluid for the transmission of light and heat. One could explain the uniformity in the velocity of light as one explains that of sound; and one could perhaps see in the disturbance of equilibrium of this fluid the cause of electric phenomena.

> One could easily conceive why a body loses so much heat without losing weight.[46]

In his first optics paper of 15 October 1815 Fresnel explicitly considered light to consist of 'vibrations of caloric', and discussed again some significant chemical reactions.[47] These passages also attest to Fresnel's profound concern with providing a unified explanation of diverse phenomena. A biographer has claimed that 'it was Fresnel's belief in the essential unity and simplicity of nature that conditioned his preference for a wave conception of light'.[48]

At least by 1814 Fresnel was in close contact with Ampère, and solicited Ampère's reactions to his ideas.[49] By May 1816 Ampère had been won over to the wave theory of light.[50] According to Arago, this was one of Ampère's three favourite theories, the others being the elementary nature of chlorine and the explanation of magnetism in terms of electric currents.[51] In August 1816 Ampère explained to Fresnel how the wave theory might account for polarization in terms of the non-interference of two orthogonal systems of vibrations.[52] By 1817 it was all-out war between Fresnel, Ampère, and Arago against the 'Biotistes' and 'émissionnaires', with Ampère urging Fresnel to enter the competition for the diffraction prize 'for himself and for the cause'.[53] Fresnel was in turn one of Ampère's earliest supporters in 1820, and the two did a number of experiments together in September and November, including one which looked for the induction of an electric current in a coil by means of a magnet.[54] They also exchanged ideas on the vibrational nature of light and electricity.[55] By January 1821, Fresnel had suggested to Ampère that the electric current loops responsible for magnetism might go around the individual molecules of the magnet, and not around its axis as Ampère had originally supposed.[56] Around June 1821, Fresnel announced his belief in the transversality of the vibrations of light waves, in which context he recalled his earlier exchange with Ampère over the same subject.[57] Sharing some of the same central concerns in philosophy, chemistry, and physics, Ampère and Fresnel enjoyed a supportive and fruitful interchange of ideas over an extended period of time (See Appendix, below).

The fact that Ampère's interest in chemistry fell chiefly between 1807 and 1816 made it natural for him to take keen interest in Dulong's discovery of nitrogen trichloride in 1811. Ampère believed this work would throw light on certain key problems of chemical composition. The two men shared the conviction that chlorine was an element and that nitrogen was an oxide of a radical which was combined with chlorine in Dulong's explosive liquid.[58] When Ampère wrote of Dulong's work to Davy in August 1812 he noted that 'a lot of light and heat is also produced in this explosion, where a liquid decomposes into two gases'.[59] Although Ampère did not explain why that was noteworthy, one may reasonably suspect that he, too, saw such a reaction as a problem for the received theories of heat and light. In a letter to Ampère of August 1816, Dulong wrote that his research on the oxides of phosphorus supported some of Ampère's theoretical ideas on chemical combination.[60] In the same letter he added: 'I have obtained some results which support one of your favourite hypotheses but I will keep them for you until you return. Petit and Arago are very well'.[61] Although it is not evident from the context what hypothesis he is referring to, it might well be to the atomic theory, of which both men were early defenders and which was one of the cornerstones of the anti-Laplacian programme. In a letter of Berzelius of January 1820, Dulong said his support of the atomic theory had earned him the opposition of Laplace, and he went on to express his belief that radiant heat and light were vibrations of a subtle fluid.[62] A defender of the mechanical theory of non-radiant heat, Dulong added his belief that 'the voltaic pile could only develop the phenomenon of heat if the electric current excited vibrations of the material particles'.[63] Although the exact connexions between the phenomena of heat, light, electricity, and chemical activity were often still vague, it is clear that Ampère, Fresnel, and Dulong—aided by Petit and supported by Arago—shared a world view in which these phenomena were seen as depending on the vibrations and combinations of an all-pervading ether.

These personal connexions and intellectual affinities are demonstrable. There is another connexion which may have been of even greater importance in determining Ampère's receptivity to Oersted's discovery of electromagnetism, and that is Oersted himself, or rather his earlier writings. In 1806 Oersted published an article in the *Journal de physique* on the electrical nature of chemical force in which he argued for the 'undulatory propagation' of both electricity and magnetism in terms of a series of expansions and contractions of otherwise unspecified nature.[64] In 1813 the French edition appeared of Oersted's 'Researches on the identity of the chemical and electrical forces', in which he sought to unify the phenomena of chemical combination, heat, light, electricity, and magnetism by relating them to two basic forces extended in space.[65] Oersted imagined the propagation of force to take place by means of continual rupture and re-establishment of the equilibrium of those space-filling forces, a mode of propagation he again called undulatory. Electricity, for example, does not flow like a liquid but is propagated

> by a kind of continual decomposition and recomposition, or rather by an action which disturbs the equilibrium at every moment, and re-establishes it in the following instant. One could express this succession of opposed forces which exists in the transmission of electricity by saying that *electricity is always propagated in an undulatory manner.*[66]

Oersted invoked the same representation to explain the propagation of radiant heat and light.[67]

These images are strongly reminiscent of Ampère's conception of both the electric current and of the mediated undulatory propagation of other forces through space.[68] Of course for Oersted force was something ontologically primitive, but the phenomenological tone of the 'Researches'—an aspect much more prominent in the French translation than in the German original—would have made it easy for the reader to interpret the phenomena in terms of subtle fluids.[69] Oersted's opposing forces filling all space look in practice very much like an ether composed of positive and negative electricity. Indeed, the tone of much of Oersted's book and the themes it dealt with recall a number of the other major aspects of Ampère's work. Both men represented science as consisting principally of phenomenological laws, while each retained a speculative interest in the physical causes and real interconnexions of phenomena.[70] Both were pre-eminently interested in unifying the several forces and phenomena of nature. Both saw electricity as the paradigm phenomenon in terms of which to explain chemical activity, heat, light, and (perhaps!) magnetism. And both were deeply concerned with problems of language and classification.

The conceptual parallels between Oersted and Ampère are very close. The question is, did Ampère know Oersted's work? The circumstantial evidence strongly suggests that he did. Oersted's book appeared just when Ampère was most intensely involved with chemistry, and it dealt with just those topics of greatest interest to him. Extracts from Oersted's book, which quoted some of the key passages on the undulatory nature of the propagation of electricity and light, were published in the May 1814 issue of the *Journal de physique*.[71] Whether or not Ampère saw this version, there is good evidence that he had read Oersted's book, possibly soon after its appearance in 1813. In a letter to Oersted of 3 November 1820, Blainville, the editor of the *Journal de physique*, wrote that Ampère had held Oersted's book in high esteem *before* he knew of the discovery of electromagnetism.[72] In a letter to Hachette of 14 November 1820, Ampère certainly meant to convey the impression that he was well acquainted with Oersted's views on the relationship between electricity and magnetism.[73] Blainville also mentioned that Ampère had obtained a copy of Oersted's book from their mutual friend, Chevreul.[74] Oersted had had considerable contact with Chevreul during his stay in Paris from December 1812 through February 1813, and had often read to him from the (apparently as yet unpublished) French translation of his book.[75]

It seems not unreasonable to suppose that Ampère became familiar with Oersted's theoretical views in 1813 or 1814, that he shared Oersted's concern with providing a unified causal explanation of chemical activity, heat, light, electricity, and magnetism, and that he saw this unity as being achieved in some vague way on the basis of an undulatory theory of an appropriate ether.[76] If this is so, then Ampère would have been primed both personally and intellectually to seize upon Oersted's discovery of electromagnetism in 1820 as a key phenomenon in the creation of a new unified physical world view. Ampère's 'Oersted connexion' is thus symbolic of his commitment to the etherian world view shared by a number of his contemporaries, and provides a badly needed clue to why Ampère should have been interested in electromagnetism in the first place. The fact that it has been ignored by historians has among other things contributed to an over-simplified picture of the logical development of Ampère's electrodynamics and thus to a serious misreading of the nature of his scientific creativity.[77] The transmutation of his initial interest into his mature theory of electrodynamics is another and more complex story involving the interplay of a battery of a diverse and changing concerns.

APPENDIX

Ampère's picture of the propagation of the electrodynamic action in terms of the vibrations of an ether and its connexion with Fresnel's theory of light.[78]

I communicated this law [on the vectorial analysis of the action of current elements] to the *Académie royale des Sciences* in a memoir read at the session of 6 November [1820]. I remarked in this memoir (1) that the attractions and repulsions whose existence I had recognized between portions of conducting wires cannot be produced like those of ordinary electricity, by the unequal distribution of the two electric fluids which mutually attract each other, and of which each repels another portion of fluid of the same kind as itself, because all the properties hitherto known of conducting wires demonstrate that neither of these two fluids is found in greater quantity in a body acting as a conductor of the electric current than in the same body in its natural state; (2) that it is difficult not to conclude therefrom that these attractions and repulsions could well be produced by the rapid motion of the two electric fluids traversing the conductor in opposite directions by means of a series of almost instantaneous decompositions and recompositions: a motion admitted since Volta by all the physicists who have adopted the theory given by this illustrious savant of the admirable instrument of which he is the author; (3) that by attributing the attractions and repulsions of conducting wires to this cause one can not help but admit that the motions of the two electricities in these wires is propagated on all sides in the neutral fluid formed by their combination, and of which all space must necessarily be filled when one explains as one ordinarily does the phenomena of ordinary electricity; in such a way that when the motions thus produced by two small portions of electric currents in the surrounding fluid mutually favour each other, there results a tendency for them to approach one another, which is in effect the case where one sees them attract each other; and that when the same motions oppose each other the two small portions of currents tend to move apart, as experiment shows; (4) that if one regards the attractions and repulsions in question here as in effect produced by this cause, the law according to which one small portion of electric current can be replaced by two others which are to it as are two forces relative to the resultant of these two forces, is a necessary consequence of this supposition, because velocities combine like forces, and that the motion communicated by the small portion of current to the fluid which fills space, represented in size and direction by the resultant, is necessarily the same as that which would result, in the same fluid, from the combination of the two small portions of currents represented in the same way by the two components.

At the time when I was occupied with these ideas, M. Fresnel communicated to me his beautiful investigations on light from which he has deduced the laws which determine all the circumstances of the phenomena of optics. I was struck by the agreement between the considerations upon which he relied and those which had occurred to me relative to the cause of the electrodynamic attractions and repulsions. He proved, by the totality of these phenomena, that the fluid that was extended through all space, and which can only be the result of the combination of the two electricities, was nearly incompressible, passed through all bodies like air through gauze, and that the motions excited in this field were propagated by a kind of rubbing of the layers already in motion against those which were not yet. According to which it was natural to think that the electric current of a connecting wire in part imparted its motion to the surrounding neutral fluid, and in part rubbed against it, in such a way as to give rise to a reaction of this fluid against the current, which could not tend to displace the latter as long as the difference in velocity was the same on all sides of the electric current, but which would tend to move it either on the side where this difference in velocity was less (that is, on the side where another electric current pushed the space-filling fluid in the same direction) or on the side opposite to the one where it was greater because there existed another electric current tending to push the same fluid in a contrary direction, according to whether the two currents which thus acted upon each other were directed in the same sense or had opposite directions.

These considerations lead one to admit attraction between currents travelling in the same direction and repulsion between those directed in the opposite sense, in conformity with the results of experiment; but I have never disguised from myself that fact that, in the absence of any means of calculating all the effects of these motions of fluids, they were too vague to serve as the basis of a law whose exactitude could be checked by direct and precise experiments. That is why I limited myself to presenting it as a fact uniquely based on observation, and only occupied myself with the construction of an apparatus with the help of which it could be verified by measurements so exact that there could be no doubt in this regard.

NOTES

[1]André-Marie Ampère, *Théorie des phénomènes électro-dynamiques, uniquement déduite de l'expérience*, Paris, November 1826; this was an earlier-published and slightly different separate printing of 'Mémoire sur la théorie mathématique des phénomènes électro-dynamiques uniquement déduite de l'expérience . . .', *Mémoires de l'Académie royale des sciences de l'Institut de France, 1823, 6,* (published 1827), 175-387, by which name it has become universally known.

[2]On 'Laplacian physics' see the references cited in n. 29, below. Herivel placed Ampère, Laplace, and Poisson together in a mechanico-molecular school in opposition to the analytico-positivists such as Fourier and Comte; see John W. Herivel, 'Aspects of French theoretical physics in the nineteenth century', *British journal for the history of science*, 1966-7, *3*, 109-132 (121). Comte, however, had in 1835 already assimilated Ampère's electrodynamics into the positivist tradition; see Auguste Comte, *Cours de philosophie positive*, 2 vols., Paris, 1975, i, leçon XXXIV, p. 548.

[3]R. A. R. Tricker, *Early electrodynamics: the first law of circulation*, Oxford, 1965, pp. 15, 98; Joseph Agassi, *Towards an historiography of science*, The Hague, 1963, pp. 22-3; L. Pearce Williams, 'Ampère's electrodynamic molecular model', *Contemporary physics*, 1962-3, *4*, 113-23 (122); idem, 'The physical

sciences in the first half of the nineteenth century: problems and sources', *History of science*, 1962, *1*, 1-15 (6); idem, *Michael Faraday, a biography*, London & New York, 1965, pp. 140, 161, 263; and idem, *The origins of field theory*, New York, 1966, p. 67. By 1970 Williams realized that Ampère's theory of electrodynamics 'was not immediately and universally accepted': see his article 'André-Marie Ampère', in C. C. Gillispie (ed.), *Dictionary of scientific biography*, New York, 1970, i, 139-147 (145).

Brown wrote that 'The *Académie's* attitude to Ampère's early papers were mixed, though generally magnanimous. According to Delambre's slightly retrospective summary of the scientific highlights of 1820, the new electrodynamic theory was widely and quickly accepted. He implies that most of the *Académie*'s members immediately adopted Ampère's views and found every recently discovered fact to be in perfect accord with his theory'; see Theodore M. Brown, 'The electric current in early nineteenth-century French physics', *Historical studies in the physical sciences*, 1969, *1*, 61-103 (89). Although favourable to Ampère, 'Delambre's' report (read on 2 April 1821) gave no indication of the reception of Ampère's work by others; see Jean-Baptiste-Joseph Delambre, 'Analyse des travaux de l'Académie royale des sciences, pendant l'année 1820. Partie mathématique', *Mémoires de l'Académie royale des sciences de l'Institut de France*, 1819-20, *4*, (published 1824), cxxvii-cciii (cxxxvii-cxlviii). Moreover, the relevant section of this report was almost certainly written by Ampère himself; see his letter of 25 March 1821 to Gaspard de La Rive in Louis de Launay, (ed.), *Correspondance du grand Ampère*, 3 vols., Paris, 1936-1943, ii, 568, and 'exposé sommaire des divers mémoires lus par Mr. Ampère à l'Académie royale des sciences de Paris, sur l'action mutuelle de deux courans électriques, et sur celle qui existe entre un courant électrique et le globe terrestre ou un aimant', *Bibliothèque universelle des sciences, belles-lettres et arts*, section 'Sciences et arts', 1821, *16*, 309-19, which is identical to the pages cited from Delambre's report. To be sure, Brown did recognize that 'Biot did not share in this enthusiastic applause, for . . . he remained the spokesman for the old electrostatic pile theory. And in Ampère's eyes, Biot's opinions, bound up as they were with Coulomb's theory of magnetism, still carried the day' (ibid., p. 90). Although proper clarification of the point would exceed the bounds of this paper, Ampère's differences with Biot had virtually nothing to with theories of the pile.

[4]This was recognized by his contemporaries; see Charles-Augustin Sainte-Beuve and Émile Littré, 'Illustrations scientifiques III. M. Ampère', *Revue des deux mondes* (Paris), 1837, *9*, 389-422 ('I. Sa jeunesse, ses études diverses, ses idées métaphysiques, etc.', by Sainte-Beuve) and 422-39 ('II. Physique', by Littré) (432); François Arago, 'Ampère. Biographie lue par extraits en séance publique de l'Académie des sciences, le 21 août 1839', in J. -A. Barral (ed.), *Oeuvres complètes de François Arago*, 17 vols., Paris, 1854-62, ii, 1-116 (58-9, 68-9). For Ampère's feeling that his work met with stubborn opposition see his *Correspondance*, op. cit. (3), ii, 571-2, 653, 678, 680; iii, 907, and the 'Réponse de M. Ampère à la lettre de M. Van Beck [sic], sur une nouvelle expérience électro-magnétique', *Journal de physique, de chimie, d'histoire naturelle et des arts*, 1821, *93*, 447-67 (447). This point requires a more extensive discussion than can be given here. Suffice it to say that no evidence for the contrary assertion has ever been adduced (see n. 3, above). My first awareness of this state of affairs came in conjunction with a paper I wrote in June 1969 for Prof. Charles Gillispie's graduate seminar at Princeton University ('On the reception of Ampère's electromagnetic theory in France').

[5]Perhaps the most important such case was Antoine-César Becquerel; see, for example, 'Du développement de l'électricité par le contact de deux portions d'un même métal, dans un état suffisamment inégal de température; des piles voltaïques construites avec des fils d'un même métal et même avec un seul fil, et de quelques effets électriques qui naissent dans les combinaisons chimiques', *Annales de chimie et de physique*, 1823, *23*, 135-54 (135-6, 146); 'Des effets électriques qui se développent pendant divers actions chimiques', ibid., 244-58 (248); and 'Considérations générales sur les changemens qui s'opèrent dans l'état électrique des corps, par l'action de la chaleur, du contact, du frottement et de diverses actions chimiques, et sur les modifications qui en résultent quelquefois dans l'arrangement de leurs parties constituentes', ibid., 1831, *46*, 265-94, 337-60 (268-9).

[6]The most famous example of Ampère's 'public positivism' is probably the first half-dozen pages of the work cited in n. 1 above. By 'positivism' I mean the view that objective knowledge of nature is limited to the formulation of mathematico-descriptive laws of phenomena which warrant no commitment to any 'underlying' image of reality. Newtonian action at a distance was the paradigmatic mathematico-descriptive law which said nothing about the physical cause of universal gravitation. The central role Biot played in propagandizing for this demarcationist ideology, both in his scientific and in his popular writings, remains to be studied.

[7]Ampère, letter of 2 July 1824, in *Correspondance*, op. cit. (3), ii, 658.

[8]Ampère, undated letter to Faraday, placed by de Launay at the beginning of 1825, ibid., p. 675.

[9]Ampère, letter of 5 August 1826 to Auguste de La Rive, ibid., p. 686. Ampère wrote 'today, 30 August 1826' in op. cit. (1), 196 (or p. 368 of the *Mémoires* eidition). For an extended account of Ampère's physical picture see the Appendix, above.

[10]I use 'phenomenological' to mean 'limited *de facto* to the description of experience', and to imply a less principled ideological stance than 'positivistic'. The earliest reference I have found to Ampère's conception of the current as a series of decompositions and recompositions of a neutral electric fluid is 'Extrait d'une lettre de Mr. Ampère au Prof. [Gaspard] De La Rive', dated 15 May 1821, *Bibliothèque universelle des sciences, belles-lettres et arts,* section 'sciences et arts', 1821, *17,* 192-4 (193). In October 1820 he described the current as consisting of a double stream of oppositely directed positive and negative electric fluids, although from the context it is possible that Ampère was describing what he took to be the generally received notion; see 'Mémoire présenté à l'Académie royale des sciences, le 2 octobre 1820, où se trouve compris le résumé de ce qui avait été lu à la même Académie les 18 et 25 septembre 1820, sur les effets des Courans électriques', *Annales de chimie et de physique,* 1820, *15,* 59-76 (64, 69, 72). There are several inconsistencies regarding Ampère's views on the electric current during that period. In general it should be borne in mind that his chief interest then lay in distinguishing between the effects of tension and current electricity, and in working out his theory of magnetism.

[11]Littré, op. cit. (4), pp. 427-8; Agassi, op. cit. (3), pp. 20-3 and 70; Williams, 'Ampère's electrodynamic molecular model', op. cit. (3), p. 117; Williams, in *DSB*, op. cit. (3), p. 143. At this last Williams said 'it would appear that Oersted's discovery suggested to Ampère that two current-carrying wires might affect one another', and referred to *Bibliothèque universelle,* 1821, *17,* 23; however, that reference—to an account of the Academy meeting of 25 September 1820—supports no such reconstruction. More reliable is Tricker, op. cit. (3), though even he has Ampère doing too much 'realizing' (cf. p. 27). The most sophisticated and successful account of Ampère's electrodynamics, though somewhat deficient in historical detail, is Jacques Merleau-Ponty, *Leçons sur la genèse des théories physiques: Galilée, Ampère, Einstein,* Paris, 1974, pp. 69-112.

[12]With this reservation, Williams's account of Ampère's views on the existence of a real world of *noumena* behind the phenomena is good; see *DSB*, op. cit. (3), pp. 441-2.

[13]Read on 27 March 1815 and published as 'Demonstration d'un théorème d'où l'on peut déduire toutes les lois de la réfraction ordinaire et extraordinaire', *Mémoires de la Classe des sciences mathématiques et physiques de l'Institut de France,* 1813-15, *14,* (published 1818), 235-48. On this paper see also n. 50, below.

[14]Ampère, 'Essai d'une classification naturelle pour les corps simples', *Annales de chimie et de physique,* 1816, *1,* 295-308, 373-94; 1816, *2,* 5-32, 105-25. On 24 January 1820 he presented to the Academy a mathematics memoir which had been written during 1818 and separately published in 1819: 'Mémoire contenant l'application de la théorie exposée dans le XVII^{e} cahier du Journal de l'École polutechnique, à l'intégration des équations aux différentielles partielles du premier et du second ordre', *Journal de l'École royale polytechnique,* XVIII^{e} cahier, 1820, *11,* 1-188; see also Ampère's *Correspondance,* op. cit. (3), ii, 537, 539, 544, 549-551.

[15]Institut de France, Académie des sciences, *Procès-verbaux des séances de l'Académie tenues depuis la fondation de l'Institut jusqu'au mois d'août 1835,* 10 vols., Hendaye, 1910-22, ii, 611, 616, 662-3; iii, 28, 88-90, 183, 325-7; v, 362, 398-400, 406, 417-9. Charles Bossut died on 14 January 1814.

[16]On Ampère's attitude toward mathematics see his *Correspondance,* op. cit. (3), i, 133-5, 225, 275, 281-2, 304-5; ii, 459, 470, 483, 486, 487; iii, 855.

[17]Arago, op. cit. (4), 34-5. On these connexions see François Picavet, *Les idéologues: essai sur l'histoire des idées et des théories scientifiques, philosophiques, religieuses, etc., en France depuis 1789,* Paris, 1891 (reprinted Hildesheim & New York, 1972), esp. pp. 467-76. Crosland noted that Ampère was associated with the Société d'Auteuil rather than the Sociéte d'Arcueil; see Maurice Crosland, *The Society of Arcueil: a view of French science at the time of Napoleon I,* London, 1967, p. 90. Herivel noted Ampère's 'somewhat uncertain reputation' prior to his work in electrodynamics, but identified only Ampère's interests in mathematics and chemistry; see Herivel, 'Ampère: pioneer of electrodynamics', *Endeavour,* 1975, *34,* 34-7 (35).

[18]On Ampère's work in chemistry see especially Trevor H. Levere, *Affinity and matter: elements of chemical philosophy, 1800-1865,* Oxford, 1971, pp. 113-22; Michelle Sadoun-Goupil, 'Esquisse de l'oeuvre d'Ampère en chimie', *Revue d'histoire des sciences et des leurs applications,* 1977, *30,* 125-41; and Seymour Mauskopf, 'The atomic structural theories of Ampère and Gaudin: molecular speculation and Avogadro's hypothesis', *Isis,* 1969, *60,* 61-74. Ampère's connexions with Davy need to be worked out in greater detail. His correspondence with Davy, begun in 1810, was by far his most important from a scientific standpoint before that with Faraday and Auguste de La Rive after 1820, and Ampère drew particular inspiration from his personal contacts with Davy during the latter's stay in Paris during November and December 1813.

[19]Ampère, 'Essai' op. cit. (14), *1,* 383-4. Brown recognized the general importance of Ampère's chemical sympathies, but there are serious difficulties with his claim that Ampère was originally a defender of the 'electrostatic theory' of the pile and that his main interest gradually shifted from the

pile to the 'reunions' in the wire; see Brown, op. cit. (3), pp. 84, 86. Ampère was never particularly interested in the pile.

[20]Ampère, 'Lettre de M. Ampère à M. le comte Berthollet, sur la détermination des proportions dans lesquelles les corps se combinent d'après le nombre et la disposition respective des molécules dont leurs particules integrantes sont composées', *Annales de chimie, ou recueil de mémoires concernant la chimie et les arts qui en dépendent, et spécialement la pharmacie,* 1814, *90,* 43-86; and 'Démonstration de la relation découverte par Mariotte, entre les volumes des gaz et les pressions qu'ils supportent à une même température' (read 24 January 1814), ibid., 1815, *94,* 145-60.

[21]Ampère, undated letter to Roux-Bordier, wrongly dated 'end of February 1806' by de Launay, *Correspondance,* op. cit. (3), *1,* 299. Ampère's mention of a certain 'Morosini' who magnetized steel needles by exposing them to ultraviolet rays can only have referred to the work of Domenico Morishini, published in France as 'Mémoire sur la force magnétisante du bord extrême du rayon violet; lu à l'Académie des Lincei à Rome, le 10 septembre 1812' *Journal de physique, de chimie, d'histoire naturelle et des arts,* 1813, *76,* 208-15, reprinted from *Bibliothèque britannique,* 1813, *52,* 21-35.

[22]This point was made by Levere, op. cit. (18), p. 115.

[23]Arago came back from Geneva with a French translation of Oersted's paper prepared by Marc-Augustine Pictet; only since then had other Parisian scientists received copies directly from Oersted; see *Annales de chimie et de physique,* 1820, *14,* 417. On 1 September 1820 a Danish correspondent of Oersted's who had been in Paris seven weeks wrote that he had not yet been able to find a published account of Oersted's discovery; see Henrik Gerner von Schmidten's letter of that date to Oersted in Mathilde Ørsted (ed), *Breve fra og til Hans Christian Ørsted,* 2 vols., Kjøbenhavn, 1870, ii, 8-9.

[24]Pierre-Louis Dulong, letter of 2 October 1820 to Jöns Jacob Berzelius, in H. G. Söderbaum (ed.), *Jac. Berzelius bref,* 6 vols., + 3 supplements Uppsala, 1912-1961, ii, part I, 18.

[25]Ampère, letter of 21 February 1821 to Roux-Bordier, in *Correspondance,* op. cit. (3), ii, 566; quoted by Brown, op. cit. (3), p. 90.

[26]Alexandre Marcet, letter of 15 September 1820 to Berzelius from Geneva, in *Berzelius bref,* op. cit. (24), i, part III, 210.

[27]Ampère, letter of 25 September 1820 to his son, in *Correspondance,* op. cit. (3), ii, 561-2. For a preliminary attempt to place Ampère's response within a theoretical/typological context, see K. L. Caneva, 'What should we do with the monster? Electromagnetism and the psychosociology of knowledge', *Sociology of the sciences: a yearbook,* 1980, *4,* forthcoming.

[28]Frankel correctly noted that 'Ampère's protestations of Newtonianism, which have received much attention in the secondary literature, were largely a response to Biot's criticism'; see Eugene Frankel, 'Jean Baptiste Biot: the career of a physicist in nineteenth century France', Princeton University doctoral dissertation, 1972, p. 345.

[29]Characterizations of this programme are given by Crosland, op. cit. (17), pp. 245-62, 299-308; Robert Fox, 'The Laplacian programme for physics', *Boletín de la Academia Nacional de Ciencias de la República Argentina,* 1970, *48,* 429-37; idem, 'The rise and fall of Laplacian physics', *Historical studies in the physical sciences,* 1974, *4,* 89-136 (91-2); and E. Frankel, 'J. B. Biot and the mathematization of experimental physics in Napoleonic France,' ibid., 1977, *8,* 33-72 (46-7). For a discussion of the methodological positions assumed by Laplace, Biot, Fresnel, Poisson, Fourier, and Ampère, see K. L. Caneva, 'Conceptual and generational change in German physics: the case of electricity, 1800-1846', Princeton University doctoral dissertation, 1974; to be published by Arno Press, pp. 331-63. The 'Newtonianism' of Laplace and Biot included a belief in the possibility of the non-hypothetical inductive derivation of certain fundamental physical laws—such as that of universal gravitation—directly from the phenomena. See also Roger Hahn, *Laplace as a Newtonian scientist,* Los Angeles, 1967.

[30]R. Fox, 'The background to the discovery of Dulong and Petit's law', *British journal for the history of science,* 1968-9, *4,* 1-22 (1); idem, *The caloric theory of gases from Lavoisier to Regnault,* Oxford, 1971, pp. 227-80; idem, 'The rise and fall', op. cit. (29), pp. 109-10.

[31]These last five individuals were identified as the chief anti-Laplacians by Fox, *Caloric theory,* op. cit. (30), p. 227; idem 'The rise and fall' op. cit. (30), p. 110; and by Jerome R. Ravetz, *Scientific knowledge and its social problems,* Oxford, 1971, p. 227. To this group E. Frankel added Ampère; see 'Corpuscular optics and the wave theory of light: the science and politics of a revolution in physics', *Social studies of science,* 1976, *6,* 141-84 (172). Frankel's work is particularly sensitive to the importance of personal clashes. Fourier certainly experienced the opposition of the Laplacians, but whether or not he shared the physical world view common to the others is an open question.

[32]Crosland, op. cit. (17), pp. 225, 322-3, 331-4. According to Crosland, Arago's ties with the Arcueil group were weaker than those of the others; see ibid., p. 316.

[33]Robert H. Silliman, 'Augustin-Jean Fresnel', in *DSB,* op. cit. (3), v, 165-71 (167); Frankel, op. cit. (31), p. 158. Fresnel's first diffraction memoir of 15 October 1815 (see n. 47 below) was presented to the Academy on 23 October and referred to Poinsot and Arago, the latter giving a long and favourable report on 25 March 1816; see *Procès-verbaux,* op. cit. (15), *5,* 562; *6,* 40. Arago's report was first

published in Henri de Senarmont, Emile Verdet, and Léonor Fresnel (eds.), *Oeuvres complètes d'Augustin Fresnel*, 3 vols., Paris, 1866-70, i, 79-87. Fresnel's polarization memoir of 7 October 1816 was referred to Arago and Ampère; see *Procès-verbaux, 6*, 84. Arago's report, not given until 4 June 1821, was printed in *Annales de chimie et de physique*, 1821, *17*, 80-102, and reprinted in Fresnel's *Oeuvres*, i, 553-68. See also n. 35 below.

[34]See the contributions of Fresnel, Arago, and Biot to the *Annales de chimie et de physique*, 1821, *17*, 80-102 (Arago), 102-111 (Fresnel), 167-196 (Fresnel), 225-258 (Biot), 258-273 (Arago), 312-315 (Fresnel), and 393-403 (Fresnel). On this feud see Frankel, op. cit. (31), pp. 165-8, and [Guillaume Libri], 'Lettres à un Américan sur l'état des sciences en France. I. L'Institut. II. L'Institut. III. M. Poisson', *Revue des deux mondes* (Paris), 1840, *21*, 789-818 (798-9); 1840, *22*, 532-54; 1840, *23*, 410-37. Libri noted that Laplace had taken sides against Arago. Schmidten reported that the battle between Biot and Arago during the summer of 1821 had led to the formation of two parties, the 'Ultra' and the 'Libérale', defending respectively the old and new theories of light, with Laplace belonging to the former; see Schmidten's letter of 21 March 1822 to Oersted from Paris, in Oersted's *Breve*, op. cit. (23), ii, 23.

[35]Arago and Alexis-Thérèse Petit, 'Sur les puissances réfractives et dispersives de certaines liquides et des vapeurs qu'ils forment' (read at the Academy on 11 December 1815), *Annales de chimie et de physique*, 1816, *1*, 1-9. On the relevance of this work to the authors' support of Fresnel's wave theory, see Fox, *Caloric theory*, op. cit. (30), pp. 202, 233-4. Fox elsewhere remarked that Dulong and Petit were among Fresnel's earliest converts, though hard evidence for this seems to be wanting (especially for Dulong) other than the probable intended support for Fresnel implied by Arago's and Petit's work; see Fox, 'The background' (30), p. 21. Petit, who died in 1820 at the age of 28, had married Arago's sister in November 1814; see Fox, *Caloric theory*, p. 233.

[36]Petit and Dulong, 'Recherches sur quelques points importans de la théorie de la chaleur' (presented to the Academy on 12 April 1819), *Annales de chimie et de physique*, 1819, *10*, 395-413. On this paper see Fox, 'The background' op. cit. (30), pp. 1-2. Dulong is reported to have shown strong sympathy for the chemical atomic theory as early as April 1816; see Fox, 'The Laplacian programme for physics', op. cit. (29), p. 435.

[37]Arago, op. cit. (4), p. 69. On Laplace's opposition to both Fresnel and Ampère, see an undated letter of Ampère's in his *Correspondance*, op. cit. (3), ii, 680. Though de Launay said the letter was to Davy, a comparison with letters from Ampère to Faraday of 3 July 1825 and from Faraday to Ampère of 17 November 1825 suggests that it was in fact written to Faraday after Ampère's receipt of the second of these; see Ampère's *Correspondance*, ii, 678-9, and L. Pearce Williams (ed.), *The selected correspondence of Michael Faraday*, 2 vols,. Cambridge, 1971, i, 153-4. Frankel suggested that Ampère's belief in an ether further alienated Biot from his theory; see op. cit. (28), p. 341.

[38]Jean-Baptiste Biot, *Traité de physique expérimentale et mathématique*, 4 vols., Paris, 1816, long sections of which were taken over virtually unchanged into his *Précis élémentaire de physique expérimentale*, 2 vols., Paris, 1817; 2nd edn., 1821; 3rd edn., 1824. See, for example, his handling of static electricity (*Traité*, ii, 211; *Précis*, 3rd edn., i, 466), and magnetism (*Précis*, 3rd edn., ii, 2); at this last, after running through some of the basic phenomena of magnetism, he asked: 'What is the nature of the principle which produces these phenomena? We do not know. But whatever it might be we will define it, for the sake of conciseness, by the name of *magnetism*; it is thus that one calls *electricity* the unknown principle of the electrical phenomena, and *caloric* the no less unknown principle of heat'.

[39]Léonor Mérimée, letter of 6 March 1817 to Fresnel, in Fresnel's *Oeuvres*, op. cit. (33), ii, 842.

[40]Cf. Frankel, op. cit. (31), pp. 154-68. Arago said that he and Ampère had been friends for thirty years—Ampère died in 1836—and that they had had an intimate correspondence which Ampère had asked him to destroy; see Arago, op. cit. (4), pp. 92, 100.

[41]Littré, op. cit. (4), p. 433. Many suggestive leads can be found in Robert Marc Friedman, 'The creation of a new science: Joseph Fourier's analytical theory of heat', *Historical studies in the physical sciences*, 1977, *8*, 73-99.

[42]See especially Jean Rosmorduc, 'Ampère et l'optique: une intervention dans le débat sur la transversalité de la vibration lumineuse', *Revue d'histoire des sciences et de leurs applications*, 1977, *30*, 159-67. Williams noted Ampère's belief in an ether and his friendship with Fresnel, although he missed the role the ether played for Ampère, who did not simply mathematize a physical model; see Williams, 'Ampère's electrodynamic molecular model', op. cit. (3), p. 119, and idem., *Michael Faraday*, op. cit. (3), pp. 148, 455.

[43]Léonor Mérimée, letter of 5 August 1811 to Fresnel, in Fresnel's *Oeuvres*, op. cit. (33), ii, 811.

[44]Silliman, in *DSB*, op. cit. (33), p. 167; idem, Fresnel and the emergence of physics as a discipline', *Historical studies in the physical sciences*, 1974, *4*, 137-62 (147). These connexions badly need to be worked out.

[45]Fresnel, letter of 5 July 1814 to his brother, in Fresnel's *Oeuvres*, op. cit. (33), ii, 820.

[46]Ibid., pp. 821-2.

[47]Fresnel, 'Premier 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', in Fresnel, ibid., i, 9-33 (12). See also his covering letter to Delambre of 15 October 1815 (ibid., pp. 9-10), and the supplement of 10 November 1815, 'Complément au mémoire sur la diffraction', ibid., pp. 41-60 (59), where he again presented heat and light as vibrations of caloric and discussed a few significant chemical reactions.

[48]Silliman, in *DSB*, op. cit. (33), p. 167; see also idem, 'Fresnel and the emergence', op. cit. (44), pp. 146-9.

[49]Fresnel, letter of 3 November 1814 to his brother, in Fresnel's *Oeuvres*, op. cit. (33), ii, 829; Léonor Mérimée, letter of 20 December 1814 to Fresnel, ibid., p. 830.

[50]Ampère, letter of 19 May 1816 to Ballanche, in Ampère's *Correspondance*, op. cit. (3), ii, 511; Fresnel, letter of 19 July 1816 to his brother, in Fresnel's *Oeuvres*, op. cit. (33), ii, 835. Costabel claimed that Ampère had supported the wave theory in his memoir of 27 March 1815 on the laws of refraction; see Pierre Costabel, 'L'activité scientifique d'Ampère', *Revue d'histoire des sciences et de leurs applications*, 1977, *30*, 105-12 (108). This is not borne out by an examination of the memoir in question (see n. 13 above) and is contradicted by a later statement of Ampère's; see Ampère, 'Mémoire sur la détermination de la surface courbe des ondes lumineuses dans un milieu dont l'élasticité est différent suivant les trois directions principales, c'est-à-dire celles où la force produite par l'élasticité a lieu dans la direction même du déplacement des molécules de ce milieu' (read 26 August 1828), *Annales de chimie et de physique*, 1828, *39*, 113-45 (114), quoted by Rosmorduc, op. cit. (42), p. 116.

[51]Arago, op. cit. (4), p. 103. If Arago's remark is correct, it is noteworthy that Ampère valued his theory of magnetism over his mathematical law for the interaction between current elements.

[52]MS note of 30 August 1816, in Fresnel's *Oeuvres*, op. cit. (33), i, 394.

[53]Léonor Mérimée, letter of 6 March 1817 to Fresnel, in ibid., ii, 841-2. On 15 September 1820 Alexandre Marcet wrote to Berzelius that 'Arago also demonstrated in Geneva a curious experiment on light which is considered to be a great triumph for *Messieurs les ondulateurs* at the expense of the poor *émissionaires* who are loosing ground every day'; see *Berzelius bref*, op. cit. (24), i, 211.

[54]Ampère, letter of 25 September 1820 to his son, in Ampère's *Correspondance* op. cit. (3), ii, 562. On 6 November 1820 Ampère deposited a sealed note for Fresnel with the secretary of the Academy, then asked for it to be opened and read; see *Procès-verbaux*, op. cit. (15), 7, 100. Fresnel's 'Note sur des Essais ayant pour but de décomposer l'eau avec un aimant' was published in *Annales de chimie et de physique*, 1820, *15*, 219-22, reprinted in Fresnel's *Oeuvres* op. cit. (33), ii, 673-6.

[55]Ampère, 'Exposé sommaire des nouvelles expériences électro-magnétiques faites par differens physiciens, depuis le mois de mars 1821, lu dans la séance publique de l'Académie royale des sciences, de 8 avril 1822', and 'Notes sur cet exposé des nouvelles expériences relatives aux phénomènes produits par l'action électro-dynamique, faites depuis le mois de mars 1821', in Ampère, *Recueil d'observations électro-dynamiques, contenant divers mémoires, notices, extraits de lettres ou d'ouvrages périodiques sur les sciences, relatifs à l'action mutuelle de deux courans électriques, à celle qui existe entre un courant électrique et un aimant ou le globe terrestre, et à celle de deux aimans l'un sur l'autre,* Paris, 1822 [-3], 199-206, 207-36 (214-15). This 'Exposé' was originally published as 'Notice sur les nouvelles expériences électro-magnétiques faites par différens physiciens, depuis le mois de mars 1821, lue dans la séance publique de l'Académie royale des sciences, le 8 avril 1822', *Journal de physique, de chimie, d'histoire naturelle et des arts,* 1822, *94*, 61-6. Three pages of the 'Notes' are translated in my Appendix, above. They probably date from July or August 1822.

[56]See two autograph notes by Fresnel found among Ampère's papers, 'Comparaison de la supposition des courants autour de l'axe avec celle des courants autour de chaque molécule', and 'Deuxième note sur l'hypothèse des courants particulaires', in *Collection de mémoires relatifs à la physique, publiés par la Société française de physique*, vols. ii-iii: 'Mémoires sur l'électrodynamique', Paris, 1885-7, ii, 141-3, 144-7. The second of these bore no title in MS, but was dated 5 June 1821. See also the editor's footnote, ibid., p. 140, and the 'Note sur deux memoires lus par M. Ampère à l'Académie royale des sciences, le premier dans la séance du 26 décembre 1820; le second dans les séances des 8 et 15 janvier', *Journal de physique, de chimie, d'histoire naturelle et des arts*, 1821, *92*, 160-5 (163). Ampère's first public discussion of molecular currents was on 15 January 1821.

[57]Fresnel, 'Considérations mécaniques sur la polarization de la lumière', *Annales de chimie et de physique*, 1821, *17*, 179-96 (179; cahier of June); reprinted in Fresnel's *Oeuvres*, op. cit. (33), i, 629-45 (629-30); quoted by Rosmorduc, op. cit. (42), 166-7.

[58]Ampère, letter of 12 February 1812 to Bredin in Ampère's *Correspondance* op. cit. (3), ii, 398.

[59]Ampère, letter of 26 August 1812 to Davy, in ibid., p. 417.

[60]Dulong, letter of 5 August 1816 to Ampère, quoted in English translation in Pierre Lemay and Ralph E. Oesper, 'Pierre Louis Dulong, his life and work', *Chymia: annual studies in the history of chemistry*, 1948, *1*, 171-90 (175-6).

[61]Ibid., p. 177.

[62]Dulong, letter of 15 January 1820 to Berzelius, in *Berzelius Bref*, op. cit. (24), ii, 12. Substantial portions of this letter are quoted in Fox, 'The background' op. cit (30), pp. 13, 18, and in idem, *Caloric theory*, op. cit. (30), p. 244.

[63]Ibid., pp. 13-14; also quoted by Fox.

[64]Oersted, 'Sur la propagation de l'électricité', *Journal de physique, de chimie, d'histoire naturelle et des arts*, 1806, *62*, 369-75 (371, 372).

[65]Oersted, *Recherches sur l'identité des forces chimiques et électriques*, Paris, 1813. This was a translation by Marcel de Serres of *Ansicht der chemischen Naturgesetze, durch die neueren Entdeckungen gewonnen*, Berlin, 1812; reprinted in Oersted, *Naturvidenskabelige Skrifter*, ed. by Kirstine Meyer, 3 vols., København, 1920, ii, 35-169. On the book's purpose see also the notice (by Oersted?), 'Ansicht der chemischen Naturgesete [sic], durch die neuern Entdeckungen gewonnen, ... c'est-à-dire, considérations sur les lois chimiques de la nature, fondées sur les nouvelles découvertes', *Journal de physique, de chimie, d'histoire naturelle et des arts*, 1813, *76*, 233-7 (233; cahier of March).

[66]Oersted, *Recherches*, op. cit. (65), p. 130; this portion was reprinted in the extract made by Jean-Claude Delamétherie, 'Des forces électriques considérées comme des forces chimiques', *Journal de physique, de chimie, d'histoire naturelle et des arts*, 1814, *78*, 338-74 (341). See also the notice in the *Journal de physique* cited in n. 65, above, p. 234.

[67]Oersted, *Recherches*, op. cit. (65), pp. 209-11. For an extensive discussion of Oersted's conception of light see Kirstine Mayer, 'The scientific life and works of H. C. Ørsted', in Oersted's *Skrifter*, op. cit. (65), i, pp. xi-clxvi (liv-lxi).

[68]See n. 10, and the Appendix, above. Cf. Ampère's letter to Albert van Beek, written between 12 January and 27 March 1822: 'M. Oersted has regarded the composition and decomposition of electricity, which I have designated by the name of electric currents, as the unique cause of heat and light, that is of the vibrations of the fluid diffused through all space and which, according to the hypothesis generally adopted of two electric fluids, one cannot but consider as the combination of these two fluids in the proportion where they mutually saturate each other. This opinion of the great physicist, to whom we owe the first experiments on the mutual action of conducting wires and magnets, accords perfectly with the totality of the phenomena . . .'; 'Réponse', op. cit. (4), pp. 449-50.

[69]Cf. Oersted, *Recherches*, op. cit. (65), pp. 78-9, 107-10, 130 with Oersted, *Ansicht*, op. cit. (65), pp. 81, 112-15, 140, 254-6. The French translation eliminated all talk of the Kantian construction of matter out of space-filling attractive and repulsive forces. In any event, Ampère's interest in Kant would have made Oersted's way of thinking more accessible to him than it apparently was to most of his French contemporaries, who would have nothing that smacked of German mystification.

[70]Missing from the German original, for example, was the claim that the dynamical theory of heat is 'based on a law drawn from experience'; see Oersted, *Recherches*, op. cit. (65), p. 200, compared with Oersted, *Ansicht*, op. cit. (65), p. 211; the former is quoted in the extract published in the *Journal de physique*, op. cit. (66), p. 374. See also the entire preface added to the French edition.

[71]See n. 66, above. Another possible source of Ampère's acquaintance with Oersted's views is the French translation of Davy's *Elements of chemical philosophy*, which included a précis of Oersted's *Ansicht der chemischen Naturegesetze* which stressed the unity of forces and mentioned the undulatory propagation of electricity; see Homfrede Davy, *Elémens de philosophie chimique*, trans. by Jean-Baptiste Van Mons, 2 vols., Paris & Amsterdam, 1813, i, 211-18. On Ampère's connexions with Davy, see n. 18, above.

[72]Henri-Marie-Ducrotay de Blainville, letter of 3 November 1820 to Oersted, in Marius Christian Harding (ed.), *Correspondance de H. C. Örsted avec divers savants*, 2 vols., Copenhague, 1920, ii, 271.

[73]Ampère asserted that Oersted had nowhere announced the identity of the magnetic and galvanic fluids 'dans tout ce que nous avons de lui'; see his letter of 14 November 1820 to Jean-Nicolas-Pierre Hachette, in Ampère's *Correspondance*, op. cit. (3), iii, 906. Ampère seems to have supplied Hachette with some of the information contained in the latter's note, 'Sur les expériences électromagnétiques de MM. Oersted et Ampère', *Journal de physique, de chimie, d'histoire naturelle et des arts*, 1820, *91*, 161-6, which was received by the Academy on 27 November 1820; see *Procès-verbaux*, op. cit. (15), *7*, 103. On p. 163 Hachette says that de Serres' translation of Oersted's book appeared in 1807; this was probably the source of François-Pierre-Nicholas Gillet de Laumont's similar error in [Gillet de Laumont and Ampère], 'Note sur les expèriences électro-magnétiques de MM. Oersted, Ampère et Arago, relatives à l'identitè de l'aimant avec l'électricité', *Annales des mines*, 1820, *5*, 535-58 (535). This published misinformation may have contributed to Ampère's own confusion, as when he referred in 1821 to Oersted's book as having appeared 'more than 15 years ago'; see his letter of 21 February 1821 to Roux-Bordier, in Ampère's *Correspondance*, ii, 567. Perhaps he was also confused by the date of Oersted's 1806 paper, op. cit. (64). This mistake of Ampère's presents a problem for my argument that he was personally acquainted with Oersted's book, but the evidence for that point still seems to me strong.

[74]See n. 72, above. Oersted reported during his third trip to Paris that Chevreul was one of Arago's

best friends; see Oersted's letter of 12 February 1823 to his wife, in Oersted's *Breve* op. cit. (23), ii, 48. Crosland said that Chevreul 'had crossed swords with the Arcueil group', and noted that when Davy visited Paris in 1813 'he received most attention not from the Arcueil group but from men on the fringe of French science such as Ampère, Clément and Chevreul'; Crosland, op. cit. (17), pp. 168, 323. A paper of Chevreul's on the relationship between mind and muscular movement was written in the form of a letter to Ampère, and attests to their long-term common interest in this and other questions of philosophical import; see Michel-Eugène Chevreul, 'Lettre à M. Ampère sur une classe particulière de mouvemens musculaires', *Revue des deux mondes* (Paris), 1833, *2*, 258-66.

[75]Oersted, letter of 25 February 1813 to his brother, in Oersted's *Breve*, op. cit (23), i, 312. Chevreul mentioned the book in a letter of 13 January 1819 to Oersted; see Oersted's *Correspondance*, op. cit. (72), ii, 294. In 1818 Chevreul gave an extensive and very favourable discussion of Oersted's and Ampère's systems of chemical classification in the article 'Corps. (*Chim.*)' in Frédéric-Georges Cuvier (ed.), *Dictionnaire des sciences naturelles*, 61 vols., Strasbourg & Paris, 1816-1845, x, 511-39 (530-2, 532-8, respectively).

[76]Referring explicitly to Arago and Ampère, Schmidten recorded his impression from Paris that those who best appreciated Oersted's ideas were those who sought unity in the phenomena of chemistry, electricity, light, and heat; see Schmidten's letter of 21 March 1822 to Oersted, in Oersted's *Breve*, op. cit. (23), ii, 23-4.

[77]Agassi, for example, explicitly rejected the possibility that Ampère had anything to do with Oersted's speculative belief in the unity of forces; see Agassi, op. cit. (3), pp. 21-2.

[78] Ampère, 'Notes', op. cit. (55), pp. 213-16.