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How it Means: Mathematical Theories in Physical Theories. With Examples from French Mathematical Physics of the Early 19th Century

1. Motivation and outline

> It is interesting thus to follow the intellectual truths of analysis in the phenomena of nature. This correspondence [,] of which the system of the world offers us numerous examples, makes one of the greatest charms attached to mathematical speculations.
>
> Pierre Simon de Laplace (1)

The history of physics is one of the strongest branches of the history of science today: a remarkable range of past developments is collectively studied, covering both many periods and a wide variety of topics. Yet a rather noticeable lacuna becomes evident, especially for the 18th century onwards: the modesty of the place assigned to mathematics in those parts of physics where it actually played a prominent rôle. It is common to find in historical writings on physics mere mention of mathematics here and there, and/or statement without discussion of a few formulae (often stated in anachronistic notations). This is a pity, since significant aspects of some parts of the history of physics are thereby left out of the story. Indeed, important *interactions* between mathematics and physics can be passed over, not only in those cases where the physical problem motivated the mathematics to be created or adjusted in the first place but even when it was available already.

Before proceeding to any historical examples, let us think about the kinds of physical problems which are raised by the presence of mathematics in physics. In § 2 I outline three general issues pertinent to this theme, and in § 3 I adjoin three physical aspects of physical theories, whether or not mathematics is invol-

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(1) P. S. Laplace, *Exposition du système du monde*, Paris 1799², 171.

ved in them. Then, § 4 contains brief notes on the historical time-period to which I have chosen to apply this philosophical scenario: the broadening of mechanics into mathematical physics in France during the approximate period 1800–1830. The results of this application are sketched in §§ 5-8.

2. Three mathematical issues

The first trio of issues are basically independent of each other. They may be summarised as follows.

2.1 – *A spectrum of applications*

It is not sufficient to think of mathematics used in physics without distinguishing different kinds of usage that may be made. At least these four are worth noting:

a) Mathematics executed *without* physical interpretations being made, but clearly with some in mind; they may even provide the motivation to the study.

b) Mathematics executed and applied to a physical problem but with the utility of the application somewhat lost. To take some examples, a mathematical expression is developed far beyond any means of experimental or observational test; or the expression is immune to numerical calculation, and may not even yield predictions of general behaviour; or the whole problem is so over-simplified at the start that the application is useless; or a condition is introduced which does nice things to the mathematics (setting two constants equal to each other, say) but has no significance for the physics whatsoever; or the units and dimensions are only vaguely specified.

c) Mathematics executed and applied to a physical problem but with guidance from the physical problem maintained. The traps listed in *b*) should then be avoided.

d) Mathematics executed in some relatively general way and then adopted and extended to cover particular classes of cases. The mathematics of engineering often has this character when some kind of machine or device takes the main interest.

Theoretical and experimental physics, where mathematics plays little or no role, could be seen as framing this quartet.

2.2 – *Structure-similarity*

A piece of mathematics *M* has a certain structure, and some outcome of it is interpreted in the physics *P*. *To what extent* do the components and the structure of *M* find interpretation in *P*? For example, does an integral represent some area or sum in *P*, or does it arise solely in *M* itself? Again, if an interpreted expression c is the sum of a and b, are a and b themselves so interpretable? The answers to such questions can vary enormously, not only from case to case, but within a case, even from line to line in a mathematical deduction. For example, the formation of a differential equation and its solution will, to some extent, reflect a supposed structure of the physical situation; but the derivation of the solution could involve manipulations of no significance for the physics at all.

The concern here is strictly between *theories as such*, mathematical and physical. The application of the resulting mathematico-physical theory to the world is a separate matter, raised in certain ways in § 3.

2.3 – *Mathematical Denkweisen*

I prefer this German word to the English " style ", because of its connotation of theories as such. There are many " styles " in mathematics; I shall confine myself to three important ones which are of especial prominence in the period under discussion later. Firstly, there is the *geometrical Denkweise*, in which geometrical thinking is given prominence; perhaps (though *not* necessarily) figures are drawn. Secondly, we have the *algebraic Denkweise*, in which not merely symbols are put down and manipulated — at a reasonably high level of study, this is always likely to happen — but a certain dependence *only* on algebraic forms is evident and avoidence of other styles is attempted. Thirdly, we consider the *analytic Denkweise*; and by " analytic " here I intend branches of mathematics closely allied to mathematical analysis, and the use (to some low or high level of rigour) of limits and limiting processes. This is to be distinguished from " analytic " as opposed to " synthetic ", which is a distinction in logic between types of proof-method which may occur within any of the *Denkweisen* outlined above (2).

While the distinctions between these three are not sharp — indeed, I shall point to two " mixed " positions in § 5 — they are still of substance. They reveal themselves most clearly in the principles laid down for a given (physico-) mathematical theory, and also in some of its consequences. They are *not* to be identified with the *subjects* of geometry, algebra and analysis, although they may well appear there severally.

Their importance arises partly from their tenacity on a holder's thinking. With very few exceptions, and no striking ones, a man will hold to his *Denkweise* throughout his career as much as posible, even if his research interests change; and he uses it in his manuscripts as well as in his publications (in those

(2) In fact, during the period to be discussed such a distinction *was* conflated, in that " analytic " proofs were associated with algebra and " synthetic " with geometry, despite developments such as the elevation of analytic geometry (as it was even then called) to a subject suitable for textbook treatment. For a delightful study of the confused situation thereby created, see J. D. Gergonne, " De l'analyse et de la synthèse dans les sciences mathématiques ", *Ann. Math. Pures Appl.*, *7*, 1816-1817, 345-372.

cases where I have found manuscripts, anyway), so that it is not adopted for some policy connected with going into print. The question of its *genesis* then arises; and here I have little to offer, I am afraid. Part of the answer may lie in educational influences; but then the question is posed of the competing versions adopted by contemporaries at the same time. (The period to be studied later is full of examples of this.) So we may have to move into psychological questions to pursue the matter further. Such an exercise, however, is not attempted here.

3. Three philosophical aspects

I note here three companion issues which can arise in any scientific theorising, and provide some special points of issue with the trio just described when mathematics is also playing a role; for they arise in mathematics itself.

3.1 – *Generality*

One feature of the history of physics is that from time to time a theory is put forward as a general "research programme" (as philosophers are wont to say) which encompasses a wide range of phenomena, at least in their basic essentials. This programme will then exert influence among its adherents upon the mode of theorising to be attempted within a given part of that range: conversely, the success or failure achieved speaks to the range which the programme can or cannot cover.

Within physics these procedures raise questions concerning the relationships between the various parts of this range; and also related issues, such as the status of an *analogy* and its possible conversion into range *membership* (so that two kinds of phenomena are held not merely to be *like* each other, but one is reducible to the other, or each reduce to something else). Within mathematics the same sort of situation arises, and is itself an interesting example of structure-similarity. The connection can increase if both a physical theory and its attached mathematical theory are each being subsumed under research programmes, especially if structure-similarity also obtains between them.

3.2 – *Experientiability*

Mankind learnt long ago that the multitude of effects in this world is greater and more varied than his sensory means can encompass; and certain kinds of philosopher hold that only the (so-called) directly experientiable components be admitted into scientific theories, with the rest assigned to the *façon de parler* or even banishment. Less hard-line positions grant some scope for the non-experientiable, and some will openly assert the need for non-experientiable elements in theorising. For some thinkers, experientiable facts constitute truths, while the trans-empirical must remain always hypothetical (3).

Mathematics not being an empirically refutable body of knowledge, the same concerns do not apply. However, similar ones can arise in connection with *Denkweisen*, for example (§ 2.3), if a geometrical thinker associates his preferences with an empiricist epistemology; or if the physical guidance of mathematised theories (§ 2.1) is held to constitute an empirical rule; or if structure-similarity (§ 2.2) is to be so restricted.

3.3 – *Simplification and desimplification*

However narrow or broad a view one takes of the scope of legitimate theorising, we are all agreed on the complications of the phenomena of the world, and the need to allow simplification to be made in order to facilitate the construction of theories. Now one way in which a scientific theory develops is by a process for which I have proposed the term " desimplification ". Here attempts are made to take account of simplifications already *knowingly* made; for example, to abandon assuming that a temperature is constant and allow it to vary, even if only by a simple law; or to take note of the rotation of the earth; or to replace the " ideal " smooth rope by something like a real one; and so on.

The consequences for mathematics are very interesting. Desimplification may require only using more terms in a series, say (although that could be hard to effect), but it could demand radically new mathematics, even if the physics is not much changed (for example, the new integral has no known evaluation). Again, the accuracy of a required numerical prediction will increase, and fresh numerical methods may have to be brought in.

Note that the level of simplification of a theory is *independent* of the extent to which structure-similarity may between the mathematics and the physical interpretation; a simplification could *raise* the extent, not lower. it. The level also bears upon whether the application is, or becomes notional, and in decrease as well as increase: numerical mathematics can manifest notionalism.

Lastly, and most important of all, is the greatest danger of simplification: it goes too far, and the theory is just wrong. Desimplification from there may be to no avail, unless it is also radical and lucky.

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(3) For the period to be studied the distinction between truth- and hypothesis- oriented theorising is quite noticeable, not least because there seems to have been a change *over generations* from the first position to the second. I shall not pursue the matter further here, partly as it is largely independetn of the mathematisation of of physical theories and partly because of the confusing (confused?) roles assigned to hypotheses (for example, whether or not they became truths if their consequences were verified by experiment).

4. The French community, 1800–1830

I shall apply the scenario of ideas (4) outlined in § 2 and § 3 to a case which I have studied: French mechanics and mathematical physics during the Revolution and the Restoration. Since various names will be used quite often, it will be valuable to provide here a brief account of the social and personal circumstances under which these studies were effected. The word " brief " is quite important: the paucity of detail masks a large numer of institutional modifications and social complications.

4.1 – *Teaching centres*

It is habitual, and also reasonable, to commence a survey of the educational establishments with the *École Polytechnique*. Upon its founding at the end of 1794, it gave a three-year basic training in science and mathematics (reduced in 1799 to two years) for the purposes of public service and military engineering. The school was militarised in 1804 by M. Bonaparte (soon to make himself Emperor Napoleon); but public service remained an important activity for the *polytechnicien* graduates, and almost all of those who will interest us followed this line and/or took teaching posts themselves (5).

After graduation, the *polytechnicien* usually spent three years in a specialist *école d'application* before beginning his career. In contrast to the interest taken by the historians in the *École Polytechnique*, these schools are largely unstudied; and yet as more advanced institutions, they could have exercised a greater influence on the *formation* of its students. The *École des Ponts et Chaussées* is worth an especial study, since several of the more mathematically minded students passed through there (6). It was in Paris; most of the other *écoles d'application* were provincial.

The other main half of the educational system centred around the *Université Impérial de France*, founded by the former M. Bonaparte in 1808. Intended to train doctors, lawyers, clergy, teachers, and administrators of all kinds for the country's needs, it was divided into *Académies* by region, under which *all* teaching fell: not only university-level (as we would recognise it) but also all the *lycées* and other such establishments. The university equivalent was provided by *Facultés*, of which *sciences* was one. The most important *Académie* was, of

(4) I make no claim for originality in the components of this scenario, although its use as an ensemble may be new. From the philosophical point of view, I have come to the position partly from writings of Polyà and Popper; but the principal influence has been gained from the primary sources themselves, both from the period discussed here and from other cases.

(5) Among a large literature, A. Fourcy, *Histoire de l'Ecole Polytechnique*, Paris 1828 is still useful. See also, for example, *École Polytechnique. Livre du centenaire 1794–1894*, 3 vols., Paris 1894–1897; [U. J. J. Leverrier], *Rapport sur l'enseignement de l'Ecole Polytechnique...*, Paris 1850; M. Bradley, " Scientific education versus military training... ", *Ann. Sci.*, *32*, 1975, 415–449; and J. Langins, " The École Polytechnique (1794–1804): From Encyclopaedic School to Military Institution ", 1979, University of Toronto Ph. D.

(6) de Dartein, " Notice sur le regime de l'ancienne Ecole des Ponts et Chaussées... " *Ann. Ponts Chaus.*, *22* (8) 1906, 5–143; M. Bradley, " Gaspard-Clair-François-Marie Riche de Prony; His Career as Educator and Scientist ", 1984 C.N.A.A. Ph. D.

course, in Paris, and on its opening some *polytechniciens*, or staff of the *École Polytechnique*, took the chairs (7).

Over and above the *Académies* was the *École Normale*, based in Paris as an elite establishment to provide accelerated education for gifted students, who were destined for teaching posts in quality *lycées* and perhaps, in due course, *Faculté* places around the country. However, while not imitating the four-month existence and collapse of its former namesake of 1795, it never met its aim during the period under consideration here, least of all in science (8).

In addition, the *Collège de France* continued as an institution of learning "for its own sake". While it enjoyed much prestige, the scale of its influence is hard to appraise, and perhaps easy to over-estimate, especially for physics and mathematics, where audiences were sometimes small and the staffing in fact not always high-powered (9). Certainly it did not match the nationwide system of the *École Polytechnique* and the *écoles d'application*.

Grandes écoles, *petite Université* (and the *Collège de France* on the side): there is no doubt where the priorities and prestige lay. Indeed, the French have been suffering the consequences of this system to this day, despite many reforms and some substantial changes.

4.2 – *Other institutions*

These teaching establishments (including the *lycées*) provided learning opportunities for the young, some of whom became professors in due course. But other institutions around town provided posts and prestige.

The nominal apex was the "mathematical" and "physical" class of the *Institut de France*, as the old *Académie des Sciences* became in 1795, only for the reversal to *Académie* to occur in 1816 after the Restoration. Membership here was very prestigious, as a member could both present his own papers to it and sit on commissions to report on those submitted by outsiders (that is, if he bothered to do so): influence could be exerted in other ways too (10). However, its *Mémoires* appeared somewhat erratically, and could not support the mass of material being presented; so publication often occurred elsewhere, at least in summary form, or even in full in the *Journal de l'École Polytechnique* or the *Annales*

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(7) See L. Liard, *L'enseignement supérieur en France*, 1789-1889, 2 vols, Paris 1888-1894, *passim* and with variable enthusiasm. Little has been done on mathematics on the *Université* during this period; some notes on Paris teaching are provided in C. Hermite, "Discours prononcé devant le Président de la République le 5 aout [1889]", in *Inauguration de la Nouvelle Sorbonne*... Paris 1889, 30 *ff*; also in *Bull. Sci. Math.*, *14* (2), 1890, 6–36; and his *Oeuvres*, vol. 4, 283–313.

(8) *École Normale* (1810–1883), Paris 1884. C. S. Zwerling has some useful information in his "The Emergence of the Ecole Normale Superieure as a Centre of Scientific Education in Nineteenth Century France", 1976, Harvard University Ph.D., ch. 2, but in general he does not give mathematics sufficient attention.

(9) L. P. E. A. Sédillot, "Les professeurs de mathématiques et de physique générale au Collège de France", part 4, *Bull. Bibl. Stor. Sci. Mat. Fis.*, *3*, 1870, 107–170.

(10) *Procès-verbaux des séances de l'Académie [des Sciences]* ... 1795–1835, 10 vols., Hendaye 1910–1922. Despite all the interest in science during the Revolution, there is no general study of the first class to continue beyond the account to 1803 given in R. Hahn, *Anatomy of a scientific institution. The Paris Academy of Sciences 1666–1803*, Berkeley 1971.

de chimie (after 1816, *et de physique*). Among other non-teaching institutions, the *Bureau des Longitudes*, with its journal *Connaissance des temps*, was the most important for our purpose (11).

Of scientific societies, the *Société Philomatique* stands out for the rapid publication of short papers in its *Bulletin*. As a result, this journal is one of the most important of the time, for major work in all area of science and mathematics made its first bow there, even if only in summary form (12). There were, of course, various other organs of publication available, including quite a few in civil and military engineering: even the official newspaper, the *Moniteur universel*, was used at times, and otherwise carried much information about science.

4.3 – *People and communities*

Within this framework our careerist *savants* lived and worked, and indeed enjoyed the novelty of careerism, of making a living as teachers and members of this and that, and often the other, organisation (13). For the Parisians devised the awful *cumul* system, whereby a man may hold, say, half a dozen posts at once, in the *écoles* system (as professor and/or examiner), in the *Université* (as professor and/or inspector), and in other public organisations such as the *Bureau des Longitudes*. Competition for these jobs could be very intense, and the literature of the time is clear with evidence of ambitioning and besting others.

Intellectually, it was a time of considerable change, *but not quickly*. In the mid and late 1790s the major institutional changes were made; but only from the mid 1810s on were comparable modifications in subject matter achieved, or at least publicised. But then two broadenings occurred: the calculus moded into mathematical analysis based on limits (the analysis of § 2.3), where function theory and convergence of series were also sited; and mechanics became mathematical physics, with the emergence of heat diffusion, electricity and magnetism, and a good deal more mathematised optics (14). The period 1815–1826 is crucial for these changes; and they were achieved only after some stout resistance.

On the research level, well-established research programmes were threatened, and so the opposition had to be strong to be convincing. Further, even if regarded as legitimate as research, the new theories could be inappropriate for effective teaching: too difficult, perhaps, or too far removed from the practical need of the *grande écoles*. Hence, along with the careerism and the ambitioning, there were major disagreements about the content of many subjects, at both

(11) G. Bigourdan, "Le Bureau des Longitudes. Son histoire et ses travaux de l'origine à ce jour", esp. part 1, *Ann. Bur. Long.*, 1928, A1–A72.

(12) P. E. M. Berthoulet, "Sur les publications de la Société Philomatique et sur ses origines", *J. Sav.*, 1888, 477–493; also in *Mémoires publiés par la Société Philomatique à l'occasion du centenaire de sa fondation* (1788–1888), Paris 1888, i–xvii.

(13) For a general suvey of the scientific community during the Revolution, see M. P. Crosland, *The Society of Arcueil...*, London 1967.

(14) I. Grattan-Guinness, "Mathematical Physics in France, 1800–1840: Knowledge, Activity and Historiography", in J. W. Dauben (ed.), *Mathematical perspectives...* New York 1981, 95–138.

the research and the educational levels, and including the relationship between the two. *Thus there was no " the French tradition " about these topics, as they were or as they broadened* (15).

One manifestation of the disgreements is social: the community of major figures divides almost equally into those at the more theoretical end of mathematical physics, who brought the two broadenings mentioned above but in general did not pursue the engineering background of the teaching and training in the *grandes écoles* very far; and those who served as professional engineers and/or engineering teachers, who put their main efforts towards the specific ends and aims of a research-level engineer scientist (16). The table names these figures, with, where appropriate, indication of the year of entry into the *École Polytechnique*. I have divided them into the three generations into which they

Table of principal figures in France, 1800–1830, divided in terms of main research interests

Prominent in research	Mathematical physics/analysis	Mechanics/calculus/engineering
By 1800	Lagrange (1736–1813) Laplace (1749–1827) Legendre (1752–1833)	Bossut (1730–1814) Carnot (1753–1823) Delambre (1749–1822) Monge (1746–1818) de Prony (1755–1839)
By 1815	Ampère (1775–1836) Binet (1786–1856), 1804 Biot (1774–1862), 1794 Cauchy (1789–1857), 1805 Fourier (1768–1830) Germain (1776–1831) Malus (1775–1812), 1794 Petit (1791–1820), 1807 Poinsot (1777–1859), 1794 Poisson (1781–1840), 1798	Dupin (1784–1873), 1801 Francoeur (1773–1849), 1794 Girard (1765–1836) Hachette (1769–1834) Puissant (1769–1843)
By 1830	Duhamel (1797–1872), 1814 Fresnel (1788–1827), 1804 Lamé (1795–1870), 1816 (17)	Clapeyron (1797–1864), 1816 Coriolis (1792–1843), 1808 Navier (1775–1836), 1802 Poncelet (1788–1867), 1807

(15) For some examples, see I. Grattan-Guinness, " Recent Researches in French Mathematical Physics in the Early 19th Century ", *Ann. Sci.*, *38*, 1981, 663–690; " Euler's Mathematics in French Science, 1795–1815 ", in *Leonhard Euler (1707–1783). Beiträge zu Leben und Werk*, Basel 1983, 395–408; and " On the Influence of Euler's Mathematics in France During the Period 1795–1825 ", in *Verhandlungen der Euler Ehrung, Berlin, September 1983*, Berlin 1985, to appear.

(16) The presence of engineers in this story is especially welcome for the general purpose of the article, since it seems to me that historians of physics pay insufficient attention to engineering aspects of their studies. This applies whether or not a notable component of mathematics is also present.

(17) Lamé's placing here is rather incorrect, up to 1830 his career had been dominated by engineering research. However, this was due to the fact of having been sent to Russia with Clapeyron (see M. Bradley, " Franco-Russian Engineering Links: The Careers of Lamé and Clapeyron ", *Ann. Sci.*, *38*, 1981, 291–312); his preference for the theoretical sides is evident in the most distinguished career which he pursued after his return to Paris in 1832.

roughly fall. The table is restricted to mathematically oriented savants, and excludes those such as Arago, Haüy or Sadi Carnot, whose contributions lay largely in experimental and/or theoretical aspects of physics (including astronomy); or the great textbook writer Lacroix, who had no time left (or, I suspect, capacity) for research. As for Coulomb, his research career ended soon after 1800, and so he is not included; and, interestingly, he would belong on both sides in a way which later figures do not need to.

I turn now to the selection of examples taken from the achievements of this community. As my main aim is to illustrate philosophical points, I have made no attempt to cover them either completely or uniformly, or to be strictly constrained by chronology, or to record niceties of historical detail. The footnotes are confined almost entirely to references to principal primary literature and (where they exist) important secondary sources.

5. Lagrangian mechanics and its alternatives

5.1 – *Lagrange's algebraic ideology*

Of all practitioners of the algebraic *Denkweise* (§ 2.3), Lagrange is the prince; for he tried to algebraise everything, especially the calculus and mechanics. For mechanics this meant a preference for the principle of least action, because of its expression in variational form; it also led to a reduction of dynamics to statics via d'Alembert's principle, an (over-?) emphasis on equilibrium, and often static equilibrium at that, and also a preference for the refined end of mechanics: theoretical astronomy, point-mass systems, hydrodynamics and the like (18). Its calculus was algebraic, too, or tried to be: the allegedly automatic convergence of the Taylor expansion of a function provided the base, for the basic notions of the differential and integral calculus were held to be definable from the series, and calculable by purely algebraic means. Power series also provided the principal form of solution for differential and related equations, although the closed functional form was preferred for exhibiting known relations with initial condition functions (19).

The most significant single advance in this tradition during our period was the introduction of the so-called "Lagrange and Poisson brackets", in which variational techniques, applied to the basic equations of motion of a system of point masses, produced new solutions from known ones. These results were produced in great intensity in the period 1808–1810 by Lagrange

(18) The *locus classicus* is J. L. Lagrange, *Méchanique analitique*, Paris 1788; 2nd ed. *Mécanique analytique*, 2 vols, Paris 1811–1815; also in *Oeuvres*, vols. 11–12. On the development of his position see C. Fraser, "J. L. Lagrange's Early Contributions to the Principles and Methods of Mechanics", *Arch. Hist. Exact Sci.*, *28*, 1983, 197–241.

(19) J. L. Lagrange, *Théorie des fonctions analytiques*, Paris 1797, 2nd ed. Paris 1813; also in *Oeuvres*, vol. 9. See A. P. Jouschkevitch, "Euler und Lagrange über die Grundlagen der Analysis", in *Sammelband der zu Ehren des 250. Geburtstages Leonhard Eulers*, Berlin 1959, 224–244.

and Poisson (his chief follower) (20); they grew out of Poisson's attempt to desimplify Lagrange's and Laplace's theory of the stability of the planetary system by extending the proof to second order of the masses of the planets relative to that of the sun (21).

5.2 – *Euler's geometrical tradition*

By contrast, Euler's methods in mechanics preserved the Basel preference for geometrical thought: bodies move in space and have shapes, and so on. Thus for example, Euler's equations for the rotation of a solid body invite the mind to think of a body rotating, whereas in Lagrange's treatment Euler's movements of inertia arise as constants of partial integration of the Lagrange equations (22). This example typifies nicely a major contrast between these two traditions: *by and large, Euler's geometrical approach is superior for finding new results, while Lagrange's algebrism shows its strength in reproving, and organising into frameworks, results already found.*

However, the differences are greater still: content is also at issue. For Euler, statics and dynamics were separate disciplines, neither reducible to the other; and while he affirmed the generality of the principle of least action, he never founded upon it the comprehensive research programme that Lagrange attempted, granting it little role in his fluid mechanics or engineering studies, for example (23). The calculus for Euler was similarly geometrical: literally a calculus of differentials, infinitesimal dimension-preserving differentials dx on a variable x, forming ratios dy/dx to indicate rates of change, summing $\int y dx$ as an area, and so on (24).

This geometrical base gave Euler's mechanics considerable educational attractiveness as well as research potential. The most significant advance in research within it in our period came in a textbook, Poinsot's *Elémens de statique* (1st edition, 1803); for in this work the young man exposed an oversight in the development of mechanics by developing a general theory of the couple (his word)

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(20) J. L. LAGRANGE, " Mémoire " and " Second mémoire sur la théorie générale de la variation des constantes arbitraires... ", *Mém. Cl. Sci. Math. Phys. Inst. France*, 1808: pb. 1809, pt. 1, 257–302, 363–364, and 1809: pb. 1810, pt. 1, 343–352; also in *Oeuvres*, vol. 6, 771–816. S. D. POISSON, " Mémoire sur la variation des constantes arbitraires dans les questions de mécanique ", *J. École Polyt.*, *8* (1), *cah.* 15, 1809, 266–344.

(21) See P. S. LAPLACE, *Mécanique céleste*, Paris 1799; also in *Oeuvres* and *Oeuvres complètes*, vol. 1, Book 2, arts. 55–57, somewhat as *déjà vu* from Lagrange's *Méchanique...*, (18), 241–258. On the theoretical astronomy of this period see A. GAUTIER, *Essai historique sur le problème des trois corps...*, Paris 1817.

(22) L. EULER, " Découverte d'un nouveau principe de mécanique ", *Hist. Acad. Sci. Berlin*, *6*, 1750: pb. 1752, 185–217; also in *Opera omnia*, ser. 2, vol. 5, 81–108. J. L. LAGRANGE, *Méchanique...*, (18), 338–371. In private Lagrange was surprisingly warm in his admiration of Euler. On this and some other career details in Lagrange see I. GRATTAN-GUINNESS, " A Paris Curiosity, 1814: Delambre's Obituary of Lagrange and its ' Supplement ' ", in M. FOLKERTS (*ed.*), *Mathemata. Festschrift für Helmut Gericke*, Munich 1984, 493–510.

(23) See C. Truesdell's editorial contributions to Euler's *Opera omnia*, ser. 2, vols. 11-13, and J. Fleckenstein's edition of papers surrounding the principle of least action in vol. 5.

(24) H. J. M. Bos, " Differentials, Higher-order Differentials and the Derivative in the Leibnizian Calculus ", *Arch. Hist. Exact Sci.*, *14*, 1974, 1–90.

and stressing the similarity in structure of its theory with that of the composition of forces. Not many produce a masterpiece in their first publication, but Poinsot's guidance of his algebra by geometrical envisioning here (§ 2.1c) cannot be bettered (25).

5.3 – *Mixed positions: de Prony and the numerical need*

Between contrasting positions, there is compromise, perhaps even muddle. de Prony, a leading engineer of this time, taught mechanics both at the *Ecole Polytechnique* and the *Ecole des Ponts et Chaussées* (of which he was the director from 1798 until his death in 1839); and while he stressed the engineer's need to envision the case, his textbooks show some disinclination to include diagrams, which in Lagrange's books was a strict rule (26). His algebra, however, was the more prosaic language of trigonometry — and this linked with his own principal methodological aim, *the need to tailor mathematics to produce numerical results*.

This passion was evident in many aspects of de Prony's career, especially in the 1790s. At the beginning of the decade, anxious not to be sent to a provincial engineer appointment, he had himself made Director of the *Bureau de Cadastre*, and in this capacity directed a vast project to produce large-scale logarithmic and trigonometric tables. "Large scale" is an understatement, for he wanted calculation up to fifteen decimal places in some cases. Following Adam Smith's principles of the division of labour, he divided his team into sections of mathematicians (to choose the best developmental and check formulae), assistants (to lay out the pages and execute the checks) and calculators (dozens of ex-hair dressers rendered destitute by the new Revolutionary philosophy of puritan hairstyles, who added and substracted day after day and put the answers in the places marked). When it was finished, two vast sets of tables were produced, eighteen large volumes each (plus an explanatory volume) — impossible to publish of course, although various attempts were made. The product was really notional in the sense described in § 2.1b); taxation may be an important science, but it hardly needs decimal expansions to such lengths (27).

As founder professor at the *Ecole Polytechnique* a few years later, de Prony was more interesting; for there he taught difference equations and their solutions

(25) L. Poinsot, *Eléments de statique*, Paris 1803, ch. 1: editions appeared until the 12th, 1877. On other aspects of his contributions to mechanics, see P. Bailhache, *Louis Poinsot. La théorie générale de l'équilibre et du mouvement des systèmes*, Paris 1975. Note also Poisson's tempering of his algebraic preferences to suit educational needs in the preface to his *Traité de mécanique*, 2 vols., Paris 1811. His virtual ignoring of the couple (vol. 1, 33-34) can be explained, but not excused, by the mutual detestation between Poisson and Poinsot.

(26) See especially G. Riche de Prony, "Mécanique philosophique...", *J. Ecole Polyt.*, *3* (1), *cah. 8 bis*, 1800; also published separately.

(27) See especially de Prony's notice and discussion, and Delambre's report, in *Mem. Cl. Sci. Math. Phys. Inst. France*, 1804, 49-93; a truncated version of the tables was eventually published as *Service géographique de l'armée. Tables des logarithmes...*, Paris 1891. For discussion of his methods, see the papers by E. Sang and F. Lefort in *Proc. Roy. Soc. Edinburgh*, *8*, 1872-1975, 421-426, 563-587.

as a complement to colleague Lagrange's more orthodox concern with differential equations (28). His motivation was clear: in science and engineering we often take measurements from time to time, and so need a calculus to express the differences of readings thereby taken. As an example for the students he took gas expansion, coming by some apparent induction to the form

$$z(x) = \sum_{r=1}^{n} a_r b_r^x \tag{5.1}$$

for the volume Z of given gas body in terms of its temperature x, where the a_r and b_r were to be determined from the data. His choice of [5.1] may have been helped by recognising it as the solution to a linear difference equation; the move helped him in the mathematics. But how well does [5.1] represent the *physics* of gas expansion? Are some of his equation systems even stable (in the mathematical sense)? Such basic questions tend to be lost beneath the masses of calculations in his paper (29). Ironically, one of his students of the time was Gay-Lussac, who was soon to criticise the experimental evidence of Guyton de Morveau and Prieur on which de Prony had relied (30).

Similar frailties are also evident in some of de Prony's engineering mechanics. For example, in his studies of arches of around 1800 he drew on the La Hire-Bossut tradition of treating an arch as a sequence of wedges (thus rendering it easily susceptible to treatment *via* difference equations (31)), and in 1804 he outlined a graphical method of joining up adjacent wedges of circular arcs to yield a differentiable profile. The method was modelled on the idea of a singular solution to a differential equation and amounted to a simple anticipation of a modern splining technique. All nicely feasible, and simplifiable down to the numerical: he even extended the theory from circles to conic sections. However, the underlying stability theory had already been criticised (by Coulomb, for example) for its danger of furnishing an incorrect thrust line (32). In other words, de Prony's desire for the practical brought him within the realm of the mistaken.

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(28) G. RICHE DE PRONY, "Suite des Leçons d'analyse", *J. École Polyt.*, *1*, (1) 1796, *cah.* 2, 1–23, *cah.* 3, 209–273, *cah.* 4, 459–569.

(29) G. RICHE DE PRONY, "Essai expérimental et analytique sur les lois de la dilatabilité des fluides élastiques...", *J. École Polyt.*, *1* (1), 1796, *cah.* 2, 24–76; als in his *Nouvelle architecture hydraulique*, vol. 2, Paris 1796, 152–196. This and the papers cited in the previous reference were reprinted as the book *Méthode directe et inverse des différences...*, Paris 1796.

(30) L. J. GAY-LUSSAC, "Recherches sur la dilatation des gaz et des vapeurs", *Ann. Chim.*, *43*, 1802, 137–175.

(31) G. RICHE DE PRONY, "Résultats des expériences faites au Panthéon français, pour mesurer les mouvemens de la coupole", *Mem. Soc. Sav. Litt. Rép. Franc.*, *1*, 1800, 28–33; also in *Bull. sci. Soc. Philom. Paris*, *3*, 1801, 70–72; *Mag. Enc.*, *7*, 1801, 413–417; and *Ann. Ponts Chauss.*, *1*, 1833, 305–312.

(32) G. RICHE DE PRONY, "Note sur l'application de la théorie des solutions particulières des équations différentielles à des questions qui intéressent la pratique de l'art de l'ingénieur", *J. École Polyt.*, *4* (1) 1810, *cah.* 10, 49–58; also in *Ann. ponts chauss.*, *1*, 1834, 97–108. On this and related topics, see J. HEYMAN, *Coulomb's Memoir on Statics...*, Cambridge 1972; and J. V. PONCELET, "Examen critique et historique des principales théories ou solutions concernant l'équilibre des voûtes", *Compt. Rend. Acad. Roy. Sci.*, *35*, 1852, 459–502, 531–540, 577–587.

5.4 - *Mixed positions: Laplace's theoretical astronomy*

Laplace's *Mécanique céleste*, which begin to appear in 1799 and continued do so in 1802 and 1805, is Lagrangian in a number of ways: some liking for the principle of least action, a reluctance to draw pictures, and also a variety of results " borrowed " with his usual failure to give references (the proof of the stability of the planetary system mentioned in § 5.1 is one). However, his desire to study the intricacies of the planetary motions led him inevitably to geometrical thought, and even the odd diagram.

Until his death in 1827 Laplace was the leading theoretical astronomer in France, and *de facto* leader at the *Bureau des Longitudes*. But his efforts to theorise seem to have pushed French work towards the notionally theoretical in the sense of 1.3b): the expressions really do get longer and longer, and one wonders as to the efficacy of it all. The state of affairs is well shown by his invitation in the late 1800s to the young Binet to calculate high-order terms of the perturbation function. The poor lad duly did his stuff up to the seventh order, in a paper which now survives as 109 pages of largely wallpaper mathematics, displaying terms in orderly rows of columns; but the *Société Philomatique* published only a one-page summary (33). If there was a chink in the glittering majesty of French mechanics at this time, it is perhaps here: the *excessive desimplification* of mathematical astronomy (34). The Germans rather took over the initiative here, with more compact and numerically feasible procedures: Soldner (geodesy), Bessel and Gauss (planets) and Olbers (comets) (35).

6. Laplace physics and its alternatives

6.1 - *The programme*

During the 1800s Laplace's research interests turned, to some notable extent, towards what I call " planetary physics ", and even to physics itself (especially heat, and some electrostatics and magnetism). Around mid decade, perhaps wishing to be the Emperor of science (§ 4.1), he conceived of a general research programme based on treating " all " phenomena in terms of short-range inter-molecular forces. It was expressly trans-empirical (§ 3.2), for it had to allow for the shapes of the molecules; and as these were not known, the

(33) J. P. M. Binet, " Mémoire sur le développement de la fonction dont depend le calcul des perturbations des planètes ", *Académie des Sciences, dossier personnel*, 109 pp.; summary in *Nouv. bull. sci. Soc. Philom. Paris*, *3*, 1812, 113. Otherwise Binet's mechanics showed a nice geometrical guidance of the algebra, noted at footnote 25 and text with Poinsot.

(34) See C. Wilson, " Perturbation Theory and Solar Tables from Lacaille to Delambre ", *Arch. Hist. Exact Sci.*, *22*, 1980, 53-304: 283-284.

(35) A comparison of French and German methods would be a most desirable contribution to the largely non-existent history of mathematical astronomy of this period. Significantly, Bowditch described some of Gauss's ideas on his translation of Laplace's *Celestial mechanics*, vol. 3, Boston 1834, repr. New York 1966, 761-791, 873-910.

force function was indeterminate. Structure-similarity was prominent, for the various physical actions were to be mathematised in terms of integrals (single or multiple) summing the action over pieces of space-time. Starting off with atmospheric refraction, he then carried out a brilliant exercise in capillarity, and inspired a tradition of molecular optics among his followers, Arago, Biot and Malus (36). This was probably its strongest area, and led to a durable contribution to the terms used in science: the word "polarisation", proposed by Malus (in a charmingly honest and nervous passage (37)) because the properties of the imagined molecules of light bore similarity to the action of the poles of a magnet.

The broadening of mechanics into mathematical physics, mentioned in § 4.2, is very much concerned with certain figures overcoming the successes of Laplacian physics. In the rest of this section I shall describe three of the principal areas (38).

6.2 – *Fourier and heat diffusion*

As a mathematician Fourier exhibited the geometrical *Denkweise* in a marked form, taking over a form of it from his hero Monge. However, he did not follow Monge into a practical engineering-education career: the accidents of history took him from teaching at the *Ecole Polytechnique* to join Bonaparte's Egyptian campaign in 1797, and upon his return in the new century the same person appointed him Prefect of a *département*, centered at Grenoble. While there he produced his heat diffusion theory in his spare time, much of it between 1804 and 1807 (39).

Fourier's liking for the geometrical is evident in all his writings: functions were treated as curves, integrals were areas, heat was thought of as really flowing. And this view mirrored into his philosophy of physics, for he held a stoically empiricist line and eschewed doctrines about the constitution of heat. If one looks at his derivation of the wave equation, it seems to be a substance (caloric in those days) which was shuffling about; it one looks at his trigonometric series solutions, then one is tempted to invoke structure similarity and invoke a wave theory (which held some currency at that time (40)). Fourier himself

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(36) P. S. Laplace, *Mécanique céleste*, vol. 4, Paris 1805; also in *Oeuvres* and *Oeuvres complètes*, vol. 4, Book 10, esp. ch. 1, and the two supplements Paris 1806-1807. See R. Fox, "The Rise and Fall of Laplacian Physics", *Hist. Stud. Phys. Sci.*, *4*, 1974, 81-136.

(37) E. L. Malus, « Mémoire sur le nouveaux phénomènes d'optique », *Mém. Cl. Sci. Math. Phys. Inst. France*, 1810, pt. 2, pb. 1814, 105-111: 106. See A. Chappert, *Etienne-Louis Malus (1775-1812) et la théorie corpusculaire de la lumière*, Paris 1975.

(38) On various aspects of the concerns of this and the next section of the papers, see *passim* in H. L. F. K. Burkhardt, "Entwicklung en nach oscillirenden Functionen...", *Jber. Deutsch. Math. Ver.*, *10*, part 2, 1908; and E. T. Whittaker, *A History of the Theories of Aether and Electricity. The Classical Theories*, London 1951.

(39) I. Grattan-Guinness, in collaboration with J. R. Ravetz, *Joseph Fourier* 1768-1830..., Cambridge, Mass. 1972, includes an edition of his first main paper of 1807.

(40) S. G. Brush, "The Wave Theory of Heat...", *Brit. J. Hist. Sci.*, *5*, 1970, 145-167; also in *The Kind of Motion we Call Heat...*, 2 vols., Amsterdam 1976, ch. 9.

refused to take either stance. As a corollary, therefore, he opposed Laplacian physics on the ground of the experientiability issue of § 3.2. So when Laplace produced an intermolecular derivation of the spatial term of the diffusion equation, Fourier continued to reason in terms of Eulerian differentials of the bodies (41) swopping heat and cold (whatever they were!) with each other (42).

Fourier's series solutions also raised hackles, on mathematical grounds. Series of terms with determined coefficients, they were far from the approved functional form of solutions to differential equations noted in § 5.1. The periodicity question also provoked much difficulty, although Fourier's geometrical explanation, complete with diagrams showing how different series differed from each other as well as from the function outside the interval of definition, was quite masterly (43). Here the geometrical *Denkweise* showed its mettle strongly, in contrast to the algebraic Poisson, who never managed to understand the point at all.

Poisson's own contributions to heat diffusion began to appear in 1815, but the bulk was published by 1823 (44), when most of Fourier's work was (at last) also in print. Laplacian in its physics, it exemplified well the complications of the procedures. For example, where Fourier posited internal and external conductivity as known constants, Poisson had to define them in terms of certain cumulative action integrals (over $[0,\infty]$, indeed) (45). As for the solutions of the equations, how Poisson thought that Fourier series could be added and subtracted together when their intervals of definition differed is hard to grasp after Fourier's account, but it seems to be based on his mis-association of sine and cosine series with odd and even functions, and it led him to a non-uniqueness theorem for series based on the "equation"

$$0 = 1 + 2 \sum_{r=1}^{\infty} \cos rx, \tag{5.2}$$

which was nonsense even at the time of writing (as $x = 0$ rapidly shows) (46). As for applications, his cases are mostly rather trivial desimplifications of Fourier's heat diffusion: in a straight bar made of two materials instead of one, for

(41) J. Herivel located a change in Fourier's mathematical modelling of around 1810 (see his *Joseph Fourier. The man and the physicist*, Oxford 1975, pt. 2). As is explained in detail in the essay review in *Ann. Sci.*, *32*, 1975, 303-312, this interpretation is rendered possible only by an extraordinary accumulation of mistakes. A more useful publication is the collections of manuscripts and texts in J. Herivel, *Joseph Fourier Face aux Objections Contre sa Théorie de la Chaleur*, Paris 1980.

(42) See R. M. Friedman, "The Creation of a New Science: Joseph Fourier's Analytical Theory of Heat", *Hist. Stud. Phys. Sci.*, *8*, 1976, 73-99.

(43) See I. Grattan-Guinness, J. R. Ravetz, *Joseph Fourier...*, (39), chs. 7 and 9 for Fourier's best explanations. Historians of mathematics continue to ignore this periodicity problem in discussion on the history of the notion of a function (see, for example, A. P. Youshkevich, "The Concept of Function up to the Middle of the 19th Century", *Arch. Hist. Exact Sci.*, *16*, 1976, 37-85.

(44) See especially S. D. Poisson, "Mémoire" and "Second mémoire sur la distribution de la chaleur dans les corps solides", *J. École Polyt.*, *12* (1), *cah.* 19, 1823, 1-144, 249-403.

(45) *Ibidem*, 8-13.

(46) S. D. Poisson, "Suite du mémoire sur les intégrales définies...", *J. École Polyt.*, *12* (1), *cah.* 19, 1823, 404-509 (pp. 507-509: compare pp. 407-410, 428-432).

example. A lovely piece of notional application (§ 2.1b) occurred with his study of a bar heated at one point and cooling an an environment whose temperature varied with time, and so itself had to be expressed as a Fourier series. The resulting solution to the modified diffusion equations was clever, but most complicated and quite useless for application. But what is this application? Strap thermometers to your astrolabe, he told his readers in the *Connaissance des temps*, and find out from the beautiful formulae how much the sun will bend it out of shape (47). The *problem* is genuine, and indeed had been known before; but is *this* mathematisation the way ahead to solve it?

6.3 – *Poisson and others on elasticity theory*

Chladni's visit to Paris in 1808 to demonstrate his musical instruments and the nodal patterns on vibrating surfaces aroused much interest: one consequence was a prize paper at the *Institut* for a mathematical treatment. The social side is tragicomic: Sophie Germain eventually won in 1815 with the third of her miserable contributions; Poisson, not a member of the *Institut* at its initial announcement, read a paper on the same subject in 1814, after his election but before the award of the prize; Legendre protested on ethical grounds, and a commission was appointed to investigate and did nothing (48).

The mathematical side is more complicated. Germain, like Fourier, thought geometrically, and so applied the calculus of curved surfaces to the problem (her incompetence in execution is not of concern here), following certain ideas of Euler (49). Poisson carried out a remarkable Laplacian analysis, thinking in terms of the short-range inter-molecular forces, and produced a remarkable fourth-order second degree differential equation which no one to my knowledge has studied in detail but which reduced to known forms for small vibrations; he also corroborated and extended known results on stored energy (50). However, the generality of his equations is hard to appraise, in that the forces seem to have to account for all elastic properties; but the types and statusses of the elastic constants were mystified by the Laplacian integrals. There was also dispute over the mathematical power of the term representing the thickness of the surface.

A similar difficulty attends the work of our next major contributor to elasticity theory, and the second from our corps of active engineers in the table

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(47) S. D. Poisson, " Sur la distribution de la chaleur dans un anneau homogène et d'une épaisseur constante...", *Conn. des Temps*, 1826: pb. 1823, 248–257.

(48) On the sociology of this story, see L. Bucciarelli, N. Dworsky, *Sophie Germain...*, Dordrecht 1980. For discussions of elasticity theory in this period, see I. Todhunter, *A History of the Theory of Elasticity...*, vol. 1, Cambridge 1886, chs. 1–5.

(49) S. Germain, *Recherches sur la théorie des surfaces élastiques*, Paris 1821. Compare the Binet-like (footnote 33) wallpaper mathematics in G. Plana, " Mémoire sur les oscillations des lames élastiques", *J. École Polyt.*, *10* (1), *cah.* 17, 1815, 345–395, 633–634; it may have led Germain to similar notional elasticity theory of the superpositional kind (*Bibliothèque Nationale*, ms. f.f. 9116, fols. 204–211).

(50) S. D. Poisson, "Mémoire sur les surfaces élastiques", *Mém. Cl. Sci. Math. Phys. Inst. France*, 1812, pt. 2 (pb. 1816), 167–225.

of § 4.3. The philosophical position of Navier is curious. He too subscribed to a molecularist philosophy of matter; but, unlike Laplace or Poisson, he rarely mathematised these actions (an unhappy example is noted in a moment), and much more often adopted a geometrical approach married to a positivistic epistemology which we shall see again in § 8.1. In a lithograph of 1820 on the equilibrium of elastic surfaces he did not appeal to molecularist arguments at all but used the geometry of the configuration to determine the action of these forces: then he applied variational methods to express the position of equilibrium in terms of zero total moments of internal and impressed forces. However, his elastic constant was curiously specified: while of the units of stress, it acquired from nowhere a factor 8/15, which nicely cancelled out later. He then solved the differential equation by double Fourier series, in a physical context where the wave patterns could reflect physical structures (51).

In a sequel paper Navier sought the equations for elastic bodies. This time he went Laplace/Poissonesque, with a cumulative action integral, of unintelligible units and this time adorned with a $2\pi/15$, to represent the *only* elastic constant which he felt necessary for the problem. However, he also made assumptions more congenial to his empiricist inclinations, such as the proportionality of the displacing force to the measure of displacement (52).

This work stimulated the attention of Cauchy, who presented to the *Académie* in 1822 a paper on the equilibrium and motion of bodies and fluids which contains the essence of the stress-strain approach (to use Rankine's later terms). Unusually for him, he presented only a prosodic form of his results (53); maybe his arguments use proof based only on Taylor-series expansions, which brought dangers of both non-convergence (contrary to the rigour of his new doctrine of mathematical analysis in his teaching at the *École Polytechnique*) (54) and of non-uniqueness (following his final demolishment of Lagrange's faith in Taylor series (§ 5.1) with his 1822 counter-examples (55), such as $\exp(-1/x^2)$ at $x = 0$).

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(51) C. L. M. H. Navier, "Mémoire sur la fléxion des plans élastiques", Paris 1820; summary as « Extrait des recherches sur la fléxion des plans élastiques », *Bull. Sci. Soc. Philom. Paris*, 1823, 92–102.

(52) C. L. M. H. Navier, "Mémoire sur les lois de l'équilibre et du mouvement des corps solides élastiques", *Mém. Acad. Roy. Sci.*, *7*, 1827, 375–393; summary as "Sur les lois de l'équilibre...", *Bull. Sci. Soc. Philom. Paris*, 1823, 177–181. See R. M. McKeon, "A Study [of] the History of Nineteenth Century Science and Technology; Engineering Science in the Works of Navier", in *Proceedings of the XIIIth International Congress of the History of Science*, 1971, section II, Moscow 1974, 321–327.

(53) A. L. Cauchy, "Recherches sur l'équilibre et sur le mouvement intérieur des corps solides ou fluides, élastiques ou non élastiques", *Bull. Sci. Soc. Philom. Paris*, 1823, 9–13; also in *Oeuvres complètes*, ser. 2, vol. 2, 300–304. C. Truesdell spoils a valuable report of the prehistory of this paper by giving a hopelessly anachronistic statement of its content (*Essays in the history of mechanics*, Berlin 1968, ch. 4); luckily one of Cauchy's original pages is reproduced.

(54) A. L. Cauchy, *Cours d'analyse*, Paris 1821; also in *Oeuvres complètes*, ser. 2, vol. 3. See I. Grattan-Guinness, *The Development of the Foundations of Mathematical Analysis from Euler to Riemann*, Cambridge, Mass. 1970; and J. W. Grabiner, *The Origins of Cauchy's Rigorous Calculus*, Cambridge, Mass. 1981.

(55) A. L. Cauchy, "Sur le développement des fonctions en séries...", *Bull. Sci. Soc. Philom. Paris*, 1822, 49–54; also in *Oeuvres complètes*, ser. 2, vol. 2, 276–282.

But the basic form was clear: isolate a part of the body, and consider the forces in its interior and across its surface.

Cauchy, the chief inventor and practitioner of the analytical *Denkweise* described in § 2.2, showed a marvellous geometrical insight here, bringing clarity to the undifferentiated, and thus unstructured, Poisson/Navier worlds of ubiquitous forces. A difference of motivation is also evident: the theoreticians Cauchy and Poisson liked elasticity theory because the differential equations were fourth order and hard both to form and solve, whereas Navier needed to study elasticity because he was a practising engineer and so wanted to know the elastic properties of wood and metal.

6.4 – *Fresnel and the nature of light*

In his seminal paper on elasticity Cauchy referred to Fresnel's work on optics, studies which had been disturbing the Laplacians since around 1815. In contrast to the Laplacian molecularism noted in § 6.1, Fresnel put forward two kinds of wave theory for light: firstly, one asserting longitudinal vibrations of the molecules of the aether from the equilibrium positions; then, from 1819, vibrations construed as transverse (56). This change in physical theory did not affect his mathematics: he could use the same expressions to refer to the newly different motions and so, for example, had no need to rework the diffraction theory already presented in detail (57).

Fresnel's *Denkweise* was strongly geometrical, and the reasons are not only because he was treating optics but also the general principle stressed in § 5.2: he was seeking new results. The contrast between Lagrange and Euler noted there applies here also; Fresnel's Lagrange was Hamilton, who derived his ellipsoids of propagation in the early 1830 by algebraic methods drawing on the principle of least action, succeeding in part because he knew the equations already (58). Fresnel's geometrisation of the world even extended, unusually for that time, to suppose a structure for the aether: in order to provide a rationale for transverse vibration, he endowed it in 1821 with a three-symmetry regular lattice of particles, and obtained transverse motion off equilibrium from mechanical principles (59).

In contrast to Fourier's expressed claim of independence of heat diffusion from mechanics (on the grounds of irreversibility), Fresnel was anxious to make his theory look like mechanics (although he never tried to express it in differential equations). One example has just been given; another occurred when he

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(56) See R. H. Silliman, "Fresnel and the Emergence of Physics as a Discipline", *Hist. Stud. Phys. Sci.*, *4*, 1975, 137–162.

(57) On this theory, see J. Z. Buchwald, "Fresnel and Diffraction Theory", *Arch. Int. Hist. Sci.*, *33*, 1983, 36–112.

(58) Compare A. J. Fresnel, "Sur la double refraction", *Mém. Acad. Roy. Sci.*, *7*, 1827, 45–176 (also in *Oeuvres*, vol. 2, 479–596) with W. R. Hamilton, « Theory of Systems of Rays », part 4, *Trans. Roy. Irish Acad.*, *17*, 1837, 1–144 (also in *Mathematical papers*, vol. 1, 164–293).

(59) A. J. Fresnel, "Sur la double...", (58), art. 17.

interpreted Malus's empirical intensity laws for the ordinary and extraordinary rays in terms of conservation of *forces vives* at double refraction (60).

So the wave theory of light was " mechanical " for its conservation property; and of course molecular theory of light was also mechanics, for its placing of the basis of the phenomena in motions of (ballistic-like) molecules. Does this situation constitute a resolution of the clash? The basic divisions still remain, of course: a shimmering Fresnelian punctiform aether cannot be substituted for Biot's world of flying oscillating molecules. Yet in 1819 Biot did make a gesture towards some reconciliation, in a way relating to the issue of experientiability raised in § 3.2: he proposed the acceptance of the principle of interference, on which Fresnel relied heavily, as an " experimental law " " detached from all foreign considerations ". Portions of light could be said to interfere with each other, without any assumptions being made of the constitution of those interfering portions (61). It was an interesting move, but it did not save Laplacian optics from decline, for Biot never supplied a molecularistic argument to explain how his supposed molecules interfered in the way described.

7. Fluids and/or particles

7.1 – *A choice of scenarios*

I mentioned in § 6.4 that Fresnel was unusual for his time in trying to impose a specific structure upon the aether in order to further his theorising. However, he was very much *en courant* (as it were) in speaking of fluids, for the scientific world of Paris in those days was full of them: not only wet ones to drink, but also insensible ones, unusually of high elasticity, to transmit heat, " electricity " and the like; and the still finer aether, which for Fresnel created the effects of light and for him and others was the medium in which continuous phenomena take place. Hence the question of analogy *vis-à-vis* generality, posed in § 3.1, became of particular importance: was one type of phenomena merely like another one, or did they stand in some reducing relationship?

The competition between Laplacian physics and its opponents was in part a fight between those for and against Laplace's programme of inter-molecular force modelling; but, as the cases of Navier and Fresnel show, the relation is not simple, since they too had molecules of their own. Indeed, non-Laplacians were not agree on any common strategy; for example, structuring the aether would not suit Fourier's empiricist philosophy. Further, the role assigned to

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(60) A. J. Fresnel, "Note sur le calcul des teintes que la polarisation développe dans les lames cristallisées", *Ann. Chim. Phys.*, *17* (2), 1821, 102-112, 167-196, 312-316; also in *Oeuvres*, vol. 1, 609-653: 638.

(61) J. B. Biot, "Additions à l'optique", in J. G. Fischer, *Mécanique physique*, Paris 1819[3], 414-416; reprinted unchanged in Paris 1830[4], 497-499. See N. Kipnis, "History of the Principle of Interference", 1984, University of Minnesota Ph. D., chs. 6 and 7.

mathematics complicated the situation a little more; for while we noted in § 6.1 that Laplace would express his cumulative forces as integrals there is no such structure-similarity if the Eulerian differential calculus is used for some purpose, for its infinitely-thin-slices-of-bread kind of representation is not at all the same as lines of attraction and repulsion between tiny crumbs. For fluids, however, such similarity might obtain if they were regarded as literally continuous rather than punctiform. Of course, some treatments might apply both to solids *and* fluids; Cauchy's stress-strain modelling of § 6.3 is an important case.

With this rather complex network of possibilities in mind, I take now three areas of Parisian research on fluids. The engineers begin with wet ones; then the theoreticians come in, treating wet ones also but in their own way; finally, Poisson surmises on the distribution of the two electric fluids.

7.2 – *The measurement of water-flow*

In § 5.3 we saw de Prony urging the practical engineer's case, with mixed luck, in the 1790s. During the next decade he gained greater success with studies of water-flow published in the military engineering journal *Mémorial de l'Officier du Génie* in 1804, and also in short books (62).

The measurement of water-flow had long concerned French engineers, especially those involved in the massive schemes of canal-building and water circulation in the 18th century. The rate of flow out of an orifice depended heavily on its size, position and direction; but how could one find a theory of sufficient simplicity to prove usable and yet at the same time not to be hopelessly against the penomena? One of de Prony's chief tools was the " hypothesis of the parallelism of slices ", a theory in which strict structure-similarity obtained since the fluid was held to move in exactly the slices-of-bread way described in § 7.1, and so could be analysed by means of the (literally) differential calculus (63). Other questions such as cavitation and contraction, however, still were treated by rules of thumb.

An alternative method of measuring water-flow was to note the exact time taken to fill and/or empty a given space. Here again de Prony had ideas, including design features to aid the achievement of stagnation, and he also proposed a quadratic interpolation formula to relate drop in water level on outflow with time. There was a discussion of this and other " hypotheses " in the *Mémorial*, with seven hypotheses for measurement being proposed: a known outflow formula through a rectangular orifice, measurement of drop of level during

(62) G. Riche de Prony, "Mémoire sur le jaugeage des eaux courantes. Extrait", *Mémor. Off. Génie*, *1*, 1804, 151-209, 1821, 48-103. This ensemble of pieces by de Prony and two colleagues was based on his book of the same title, Paris 1802.

(63) For a further typical use of this hypothesis, for flow out of the base of a vase, see S. D Poisson, "Sur le mouvement d'un fluide...", *Corr. Ecole. Polyt.*, *1*, 1804-1808, 289-294; "Sur l'écoulement de l'eau dans un cylindre vertical", *3*, 1814-1816, 284-290.

outflow, de Prony's formula and a generalisation from two to n observations, and so on. The debate shows very well the engineer's difficulties with fluids, especially in large quantities: they are complex masses, far removed from the theoreticians' differential equations, and their internal structure renders theorising perilously hard, even with the geometrical differential calculus to hand.

In 1804, shortly after preparing this study, de Prony took up the related question of the theory of water-flow, especially in canals. His approach was a melange of the empirical and the deductive. He generalised, without real argument, his predecessor Dubuat's linear law relating the surface, mean and bed velocities of water in a canal; and he tried to embed in a mathematical argument Coulomb's empirical quadratic law of the resistance of fluids to the passage of bodies in the relation

$$S = \alpha U + \beta U^2 \qquad [7.1]$$

between the (supposedly) constant velocity U in a canal of uniform cross-section and its slope S, where α and β were constants to be determined (64). These he estimated by means of Laplace's recent account of (basically Boscovich's) minimax theory of errors recently published in the *Mécanique céleste* (65); however, he replaced Laplace's "purely analytic" approach by "the geometrical considerations and constructions [which] are much more familiar to a large number of engineers than abstract analysis" (66). For him, then, once again the geometrical *Denkweise* married happily to the practical need.

7.3 – *Girard and the Ourcq canal project*

de Prony's interest in the problems of water flow was motivated in part by an engineering project of essential importance to the capital: the building of the Ourcq canal from the Ourcq river to a place to the north east of Paris (and now in the 19th *arrondissement*) where a port, *Bassin de la Villette*, was constructed and the *Canal de St. Denis* was run up to the Seine river to the north while the *Canal de St. Martin* ran south to the Seine through the city. The whole system, which was constructed between 1800 and 1825, still operates, although the port as such is idle and some of its buildings are now being converted into a library and museum for the history of science.

Many engineers worked on this project at various times; they included student Cauchy from de Prony's *École des Ponts et Chaussées* (he worked on an

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(64) G. Riche de Prony, *Recherches physico-mathématiques sur la théorie des eaux courantes*, Paris 1804. See M. G. Mouret, "Antoine de Chézy. Histoire d'une formule d'hydraulique", *Ann. Ponts Chaus.*, *1*, 1921, 165–269.

(65) P. S. Laplace, *Mécanique céleste*, vol. 2, Paris 1799; also in *Oeuvres* and *Oeuvres complètes*, vol. 2; see Book 3, arts. 39–42, somewhat as *déjà vu* from R. J. Boscovich, C. Maire, *Voyage astronomique et géographique...*, Paris 1770. oi

(66) G. Riche de Prony, *Recherches...*, (64), xv–xxix. See L. Tilling, "The interpretation of Observational Errors in the Eighteenth and early Nineteenth Centuries", 1973, University of Londo Ph.D., 113–150.

acqueduct on the *Canal de St. Denis*) and later engineer Fresnel (lighting the quais on the *Canal de St. Martin*). The director for most of the time was Girard (67), who also tried to theorise about water-flow. To the opposition of some of his positivistically inclined colleagues, Girard loved to use analogy as a means of theorising. For example, the best way to plan the path of a canal was, for Girard, to think of it like a chain lying on a rough surface and then calculate the equilibirum position, treating the chain first via an n-body discrete model. For measuring water-flow he needed among other things, Coulomb's law [7.1], and he used analogies to claim that αU was like linear friction and βU^2 was due to " asperities ", as in air resistance. The slope of the canal was an important feature, since the flow had to surpass a certain agreed rate so as to prevent stagnation and thus leave the water, after circulation within Paris, in a fit state for drinking... (68).

7.4 – *Cauchy versus Poisson on the motion of fluids*

While a student at the *École des Ponts et Chaussées* in 1809, Cauchy had written a long manuscript on water-flow in rivers, which included in appendices a general analysis of the motion of fluids (69). This work must have stood him in good stead six years later when the *Institut* proposed a prize problem on the differential equations and solutions for the flow of fluids in deep quantity. True to form, Poisson, *membre de l'Institut* and even of the judging commission, presented his own paper on this subject in 1815, before the prize was awarded, and quickly had it published in the *Mémoires* (70); Cauchy, not a member (this happened in 1816 by Royal decree when the *Académie* was restored), won the prize, but had to wait a decade for his paper to appear in the *Savants étrangers* (71).

The contrast between these two theoretical papers and the engineers' worry over water-flow could not be more marked. Even though flow in depth was asked for in the problem, there were no simple formula relating velocities a different levels with Poisson and Cauchy; instead, each man took the known basic equations for hydrodynamics (in surprising switches of habit, Poisson used Euler's form and Cauchy used Lagrange's). For solutions both men used integral forms, but of different kinds: Poisson loyally adopted Laplace's integral solution of 1809 to solve Fourier's diffusion equation for infinite bodies (where

(67) P. S. Girard, *Mémoires sur le canal de l'Ourcq*, 2 vols., Paris 1831–1843, is still the major historical source on this project.

(68) P. S. Girard, *Essai sur le mouvement des eaux courantes...*, Paris 1804; interestingly revised when reprinted in P. S. Girard *Mémoires...*, (67), vol. 1, 237–312.

(69) A. L. Cauchy, " Mémoire sur les moyens de perfectionner la navigation des rivières... ", *École Nationale des Ponts et Chaussées*, ms. 1892, 98 pp.

(70) S. D. Poisson, " Mémoire sur la théorie des ondes ", *Mém. Acad. Roy. Sci.*, *1*, 1816 (pb. 1818), 71–186.

(71) A. L. Cauchy, " Théorie de la propagation des ondes à la surface d'un fluide pesant d'une profondeur indéfinie ", *Mém. prés. Acad. Roy. Sci. div. sav.*, *1* (1), 1827, 3–312; also in *Oeuvres complètes*, ser. 1, vol. 1, 4–318; the published version included several notes added later. See H. L. F. K. Burkhardt, " Entwicklungen... ", (38), 429–463 on the matters described here.

Fourier's trigonometric series could not apply) (72), while Cauchy developed a type of integral which is now known as "Fourier integrals" from Fourier's discovery of them in 1811 after Laplace's 1809 signpost (73). Cauchy claimed ignorance of Fourier's work, and indeed found Fourier's integral theorem himself (74). His proofs, elegant uses of properties of analysis, contrast with Fourier's own derivations based on rather hair-raising tricks with infinitesimals.

Integral solutions were acceptable, since they served as a variant on the functional forms noted in § 5.1, with the initial condition functions relatable to the functions in the integrands. However, as with Fourier's heat diffusion, neither man regarded his integrals as reflecting any structure of the physical interpretation intended. However, this did not prevent the competitors in hydrodynamics from manipulating and approximating to their solutions in order to obtain numerical predictions (for Cauchy, for example, the positions of peaks of waves) (75); their most important theoretical result was to refute Lagrange's guess that deep waves were propagated with uniform acceleration to find that the *velocity* was uniform (76). In addition, Poisson came to a special case of the novel notion of group velocity (as we now call it); and soon afterwards, seemingly independently, Fourier gave a clearer and more general statement of this idea (77). Thus, even if the integral solutions did not look much like water-waves, they were still worth manipulating.

Theoreticians in hydrodynamics, engineers in hydraulics: even the difference of name reflects the difference of aim and purpose. Girard exhibited it himself in fresh work of his own, presented (as a member) to the *Académie* not long after the Poisson-Cauchy dance: the results of a long series of experiments on, and theoretical conjectures about, the capillary *flow* of fluids in narrow tubes (78), in contrast to Laplace's preference for the statics of capillary *equilibrium*

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(72) P. S. Laplace, "Mémoire sur divers points d'analyse", *J. École Polyt.*, *8* (1), *cah.* 15, 1809, 229-265; also in *Oeuvres complètes*, vol. 14, 178-214.

(73) J.-B.-J. Fourier, "Théorie du mouvement de la chaleur dans les corps solides", part 1 (of his 1811 prize paper), *Mém. Acad. Roy. Sci.*, *4*, 1819-1820 (pb. 1824), 185-555 (art. 66).

(74) A. L. Cauchy, "Sur une loi de réciprocité qui existe entre certaines fonctions" and "Seconde note sur les fonctions réciproques", *Bull. Sci. Soc. Philom. Paris*, 1817, 121-124, and 1818, 178-181; also in *Oeuvres complètes*, ser. 2, vol. 2, 223-232.

(75) A. L. Cauchy, "Théorie de la propagation...", (71), the later Note 16. See the massive calculations of these values, executed on literally wallpaper-wide sheets, in *Bibliothèque de la Sorbonne*, ms. 2057.

(76) J. L. Lagrange, *Méchanique*..., (18), 1st ed., 492, or 2nd ed., 322; A. L. Cauchy, "Théorie de la propagation...", (71), 83, 107-108; S. D. Poisson, "Mémoire sur la théorie...", (70), 63-75.

(77) S. D. Poisson, "Mémoire sur la théorie...", (70), 118-120; J. B. J. Fourier, "Note relative aux vibrations des surfaces élastiques et au mouvement des ondes", *Bull. Sci. Soc. Philom. Paris*, 1818, 129-136 (also in *Oeuvres*, vol. 2, 255-265). Fourier also stressed a mathematical structure-similarity: between the forms of the linear partial differential equations for heat diffusion, surface waves and elastic lines and the kernels of their Fourier integral solutions.

(78) P. S. Girard, "Mémoire sur le mouvement des fluides dans les tubes capillaires...", *Mém. Cl. Sci. Math. Phys. Inst. France*, 1813-1815 (pb. 1816), 249-380; "Mémoire sur l'écoulement linéaire de diverses substances liquides..." and "Mémoire sur l'écoulement de l'éther...", *Mém. Acad. Roy. Sci.*, *1*, 1816 (pb. 1818), 187-259 and 260-274; the latter also in *Ann. chim. phys.*, *6* (2), 1817, 225-239, 334-336. A propos of our current discussion of fluids, Girard was referring to liquid ether in his last title! On viscosity studies, see G. H. Knibbs, "The History, Theory and Determination of the Viscosity of Water by the Efflux Method", *J. Roy. Soc. New South Wales*, *29*, 1895, 77-146.

a decade earlier (§ 6.1). Again, in the late 1820s de Prony, as one of the graduation examiners at the *Ecole Polytechnique*, criticised his former student and now professor Cauchy for excessive difficulty in the teaching of both analysis (now, we recall from § 6.3, the new mathematical analysis based on limits) and mechanics, and in particular deplored the replacement of parallel-slice techniques by curved-vein theory in the teaching of fluid mechanics (79).

7.5 – *Navier on viscous fluids; Poisson on everything*

As was noted in § 6.3, Navier worked and thought as a practising engineer, although his creative mathematical ability was unusually high for his group. After his success of 1820 with equations for elastic bodies, he imitated the same approach in order to find in 1821 and 1822 the equations for flow of viscous fluids. The analogy with the earlier studies was pressed not only on the mode of the differential equation but also in the use of double Fourier series to solve it. However, he also adopted a constant to express elasticity on the surface different from that in the interior (80).

The relation between solids and fluids raised in § 7.1 is particularly well exhibited by these two studies by Navier. The basic equation is often now called the "Navier-Stokes equation", but in fact Navier's form differs from Stokes's (which was obtained in the late 1840s) for compressible fluids, since different models were used: Navier again entertained a panoply of undistinguished inter-molecular forces to obtain his result, while Stokes used differential parallelipipeds and the stress-strain approach which Navier's earlier study of solids had motivated Cauchy to introduce (§ 6.3) (81).

When Navier's main papers were eventually in print, by 1827, another friendly chat with Poisson ensued. One feature is of considerable interest: Poisson extended the structure-similarity pertaining to Laplacian physics by representing the cumulative action of the inter-molecular forces by *sums*, on the grounds that the supposed physical structure of molecules separated by distances was too discontinuous for proper mathematisation via integrals, as had occurred hitherto in this tradition (82). In order to modify his theory, he used the Euler-MacLaurin summation formula to relate integrals and sums, preparing the mathematical ground with his own proof based on approximating to an

(79) These criticisms are contained in a manuscript of 18 November 1826 in *Ecole Polytechnique* archives, section III (draft at *École Nationale des Ponts et Chaussées*, ms. 2806). Extracts from this and other reports are published in I. Grattan-Guinness, "Recent Researches...", (15), 684-690.

(80) The chronology is most complicated. Navier's main papers are "Sur les lois des mouvemens de fluides, en ayant égard à l'adhésion des molécules", *Ann. Chim. Phys.*, *19* (2), 1821, 244-260, 448; "Mémoire sur les lois du mouvement des fluides", *Mém. Acad. Roy. Sci.*, *6*, 1823 (pb. 1827), 389-440.

(81) G. C. Stokes, "On the Theories of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids", *Trans. Cambridge Phil. Soc.*, *8*, 1849, 287-319; also in C. G. Stokes, *Mathematical and Physical Papers*, vol. 1, 75-129, see art. 5. According to his preface, Stokes seems not to have known Cauchy's paper before doing his research.

(82) S. D. Poisson, "Mémoire sur l'équilibre et le mouvement des corps élastiques", *Mém. Acad. Roy. Sci.*, *8*, 1829, 357-570, 623-627.

integral by a sum (83). He then followed on with a sequence of massive and shorter papers of great claimed generality, developing further his assumption of attractive gravitational forces and repulsive thermal ones between the molecules, and trying to cover solids, fluids, gases, vapours and the aether (84). He obtained some of Navier's and Cauchy's equations, while of course using different assumptions about the action of the "intimate structure" of matter. His treatment is virtuosistic in mathematical execution and general in physical conception; his rederivation of Navier's and Cauchy's results place him in the relation of Lagrange to Euler noted in § 5.2, or of Hamilton to Fresnel in § 6.4. Further, it shows again the weakness of most Parisian studies of elastic solids and fluids: a pretension for generality but with insufficient sub-structure to make it really effective.

Another feature of these studies is worth stressing, for they apply also to several other features of the new innovations in mechanics and mathematical physics noted in this and the previous section: *the preference for linear models*. Structure similarity is very evident here; not just linear laws but especially linear differential equations, with or without solutions such as Fourier series interpretable (or, for Fourier in § 6.2, not so) in terms of superposition of simple states. For *critics* of this great linear tradition, the issue of excessive simplification is raised.

7.6 – *Poisson and the "electric fluids"*

For reasons which I have not yet determined, French interest in electricity and magnetism was patchy, even though their major contributions were indeed durable. The example I choose here is Poisson's two papers of 1812 and 1813 on electrostatics (as we now call it). They began, like his paper on elasticity theory (§ 6.3), as submissions for an *Institut* prize, but in the end he presented them there when the death of Malus granted him election to the section of the class on physics (a subject in which he rarely worked) (85).

Although written during the heydey of Laplacian physics, Poisson's papers do not belong to § 6 above because he did not attempt the full-scale mathematisations of inter-molecular forces noted there: instead, he worked "one stage" further back, as it were. Taking as established by Coulomb inverse-square laws of attraction and repulsion of these fluids, he imported Laplacian potential theory

(83) S. D. Poisson, "Mémoire sur le calcul numérique des intégrales définies", *Mém. Acad. Roy. Sci.*, *6*, 1827, 571-604. See D. H. Arnold, "The mécanique physique of Simeon Denis Poisson...", parts 6 and 8, *Arch. Hist. Exact Sci.*, *28*, 1983, 343-367 and 29, 1983, 53-77.

(84) Poisson's principal papers are "Mémoire sur les équations générales de l'équilibre et du mouvement des solides élastiques et des fluides", *J. École Polyt.*, *13* (1), *cah.* 20, 1831, 1-174; "Mémoire sur la propagation du mouvement dans les milieux élastiques", *Mém. Acad. Roy. Sci.*, *10*, 1827 (pb. 1831), 549-685.

(85) See R. W. Home, "Poisson's Memoirs on Electricity: Academic Politics and a new Style in Physics", *Brit. J. Hist. Sci.*, *16*, 1983, 239-260.

into the subject in order to study two cases: the distribution of "electricity" inside a spheroid; and various situations involving two spheres (86).

Potential theory is a very interesting example of structure-similarity; for while equipotential surfaces can be asserted to describe some physical state, the individual terms of the expansion of the potential function in (say) surface harmonics are *not* usually interpretable individually. Poisson made no such efforts in his paper, nor did he offer any interpretation of the integral solutions which he ingeniously found for the functional equation for the two-spheres problem; and yet he thought out the solutions in a physical way. Accepting the normal Parisian view of the time that there were two electrical fluids, he imagined them as literally deposited upon the physical bodies and sought mathematical expressions for their depth. However, in order to make potential theory work here, he had to grant one fluid positive depth but the other negative, so that the expressions obtained each time were only "net depth", as it were, the *differences* between the depth of the fluids at the point of the body in question, not their sum; genuine "potential differences", one might say. So here the mathematics handled the physics in a peculiar way: exactly similar in principle (given the acceptance of electricity as a fluid in the first place) and yet only partial, since relative, in its information.

8. Engineers' mechanics: the importance of work

8.1 – *Forces vives and their conversion*

So far the theoreticians have held the stage for major changes. However, the engineers made one basic contribution to mechanics at this time which rather upset and reversed the theoreticians' assumptions (87).

As was noted in § 5.1, the Lagrangian tradition in mathematics tended to emphasise equilibrium over motion, and gave some priority to statical situations. To some extent this was true of all treatments of mechanics; and one particular consequence was that the theory of machines, where motion was prominent, even discontinuous motion after the effect of impact and percussion, was rather brushed aside or relegated to a sequence of special cases.

Various engineers of the late 18th century realised that something more general was required to bring machine theory into mechanics. The principal figure was Lazare Carnot, who sought for general theorems about the loss of

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(86) S. D. Poisson, "Mémoire" and "Second mémoire sur la distribution de l'électricité à la surface des corps conducteurs", *Mém. Cl. Sci. Math. Phys. Inst. France*, 1811, pt. 1 (pb. 1812), 1–92, and pt. 2 (pb. 1814), 163–274.

(87) For more details on the material of this section, see I. Grattan-Guinness, "Work for the Workers: Advances in Engineering Mechanics and Instruction in France, 1800–1830", *Ann. Sci.*, *41*, 1984, 1–33. On the related physics and engineering, see W. L. Scott, *The Conflict Between Atomism and Conservation Theory 1644 to 1860*, London 1970.

forces vives in mechanical systems in general (88); and Hachette tried to develop his ideas further from the late 1800s on when he launched a course on machine theory at the *École Polytechnique* and published a textbook (89).

But the next major breakthrough came from Navier. In 1819, at the time of the commencement of his studies described in § 6.3 and § 7.5, he published an edition of Bélidor's 18th-century *Architecture hydraulique* and also was appointed to a *suppléant* chair at the *École des Ponts et Chaussées* (90). In editorial notes and in lectures he advocated a general approach to mechanics in which energy conservation was given prime status with loss of *forces vives* being appraised in terms of conversion to " quantity of action ".

During the 1820s this approach, and the ideas from Carnot and also Coulomb were taken up by Coriolis (Cauchy's *répétiteur* at the *École Polytechnique*) and Poncelet (newly appointed to a chair in mechanics at the military school at Metz). Setting aside some complications of influence, both men independently set to the position that " work " (the word introduced as a technical term by Coriolis) could be taken *on a par* with *forces vives*, thus strengthening the primacy granted by Navier to energy conservation (91).

8.2 – *Features*

This story exemplifies nicely all the issues raised in § 2 and 3. I shall run through them in turn.

The mode of application was kept firmly under control as required by § 2.1c); for the general theory was applied by these men, and by others, to specific types of machine, such as dynamometers, water wheels, turbines, and even to some emergent ergonomics. The formulae had to be able to deliver numbers, in order, for example, that machine efficiency could be appraised.

Structure-similarity guided the choice of work as a prominent concept. In the most general case, of continuous action, $\int Pds$ really was to be thought of as integral, the sum of the product of the value of the force P and its traverse ds.

The generality went to the extent of reversing the usual priority of statics over dynamics. In his teaching Poncelet even explicitly began with dynamics, treating statics as the special case provided by the case of zero velocities. As a technical consequence, all the writers in this new tradition explicitly *avoided*

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(88) L. Carnot, *Principes fondamentaux de l'équilibre et du mouvement*, Paris 1803. See C. C. Gillespie (*ed.*), *Lazare Carnot Savant*, Princeton 1971.

(89) J. N. P. Hachette, *Traité élémentaire des machines*, Paris 1811; editions appeared until the 3rd (sometimes called 4th), 1828.

(90) B. Forest de Belidor, *Architecture hydraulique*, vol. 1, 1737, ed. C. L. M. H. Navier, Paris 1819. Navier's lecture notes are kept at the library of the *École Nationale des Ponts et Chaussées* (call-mark 13548 J49).

(91) G. G. Coriolis, *Du calcul de l'effet des machines*, Paris 1829; J. V. Poncelet, *Cours de mécanique industrielle...*, Metz 1829. Coriolis's book was only a "complement" to the teaching of machines at the *École Polytechnique*, which was carried out principally by Arago.

assuming that the cumulative work function $\sum P_r ds_r$ (or $\int P_r ds_r$) admitted a potential.

The *Denkweise* was strictly geometrical, not merely for the needs of theorising about particular machines but even in basic details. In particular, to continue the question of the integral, both Coriolis and Poncelet explicitly said that the integral $\int Pds$ be regarded as an area (although neither went to the mathematical length of Coriolis's professor Cauchy and defined the area as the limit of a sequence of partition-sums): since impact could be involved, P might be a discontinuous function, so that the integral required a geometrical backing.

In common with other engineers (§ 5.3, § 6.3, § 7) and also with Fourier (§ 6.2), this geometrical *Denkweise* was linked with a positivistic epistemology, especially in Navier and Poncelet (92). On these grounds there was even some preference for work over *forces vives* itself, since it was the *simple product* of two experientable objects, force and distance, while *forces vives*, in involving the *square* of velocity, was rather further removed. For Navier this position sat rather uncomfortably with his atomism (§ 6.3).

The capacity of this approach to desimplification was impressive, for it could be applied in considerable detail to the workings of machines, with its principles applied even seriatim to successive parts of their working (93). The next stage of the story was, in effect, a further desimplification; for the famous energy/heat conversion story of the 1840s was the successor to this one, where only one-way consumption of material to produce energy (for work) was contemplated, and where steam and heat engines did not play the major role accorded to them in the thermodynamics of the later tale.

Finally, the approach was appropriate both for research and for teaching purpose. Indeed, in contrast to the excessive erudition of Cauchy's concurrent teaching of analysis and mechanics at the *École Polytechnique* (§ 6.3), Coriolis, Navier and Poncelet all developed their ideas with educational needs in mind, at the *École Polytechnique* and at two of the *écoles d'applications* to which the *polytechniciens* could pass (94).

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(92) One naturally thinks, therefore, of Comte's *Cours de philosophie positive*, 6 vols., Paris 1830-1842, and yet this prolix production has hardly anything to tell us about these developments in engineering; indeed, for mechanics he tended to draw on the Lagrangian approach (§ 5.1). I do not understand his policy here.

(93) The books of Coriolis and Poncelet cited in footnote 91 contain several examples. For a still more developed one, see Poncelet's analysis of the Fourneyron turbine in "Sur la théorie des effets mécaniques de la turbine Fourneyron", *Compt Rend. Acad. Roy. Sci.*, *7*, 1838, 260–282; also in *Mém. Acad. Roy. Sci.*, *17*, 1840, 3–34; and his *Cours de mécanique appliquée aux machines*, ed. X. Kretz, vol. 2, Paris 1876, 289–311.

(94) One should also count in the *Conservatoire des Arts et Métiers*, where Dupin gave generous attention to "geometry applied to [engineering] mechanics" (see especially his *Géométrie et méchanique des arts et métiers*, 3 vols., Paris 1825–1826); and the *École Centrale des Arts et Manufactures*, where this approach to mechanics was prominently taught (contrary to the totally mistaken account of mechanics teaching there given in J. H. Weiss, *The Making of Technological Man...*, Cambridge Mass. 1982, ch. 4, as is pointed out in the review in *Ann. sci.*, *40*, 1983, 305–306).

9. Concluding remarks

After outlining a trio of mathematico-physical questions and relating them to a trio of philosophico-physical issues, I applied the resulting scenario to a particular group of developments in classical mathematical physics. I preferred to concentrate on one period because it is especially rich in clashes, disagreements and alternatives pertaining to the scenario and so furnished a tighter texture than could be provided by a scattering of examples with no special connection with each other. However, there is no particular Frenchified character in the scenario, and examples of its interactions can easily be found in other periods of historical space-time — they could even be found in modern work, since, the scenario itself is *not* specifically historical (95). I hope that specialists in other areas will try to make use of it, for it seems to have utility in indicating, in reasonably general terms, at least some of the reasoning *why* the use of mathematical theories in physical theories is a complicated business.

If the scenario is taken out on any particular historical context, then questions of normation arise. A large number of combinations arise: *should* the mathematising physicist use one rather than the others? As it stands, the question is *under-determined*: criteria of preference cannot be offered until a specific purpose is indicated. To take two types of example given earlier, the choice could be different if one is seeking new results rather than systematising a collection of known ones, and different again if one desires a good teaching scheme instead of some research-oriented purpose. Thus the next question is: on what kinds of ground would one prefer one purpose over another? I doubt if a general theory of meta-preferment can be offered: indeed, remembering the wide range of combinations available at the base level, plurality at this place of metametatheorising may itself be the best answer of all.

> [...] the sciences are like a beautiful river, of which the course is easy to follow, when it has acquired a certain regularity; but if one wants to go back to the source, one will not find it anywhere, because it is every where; it is spread in some manner over all the surface of the earth; the same if one wants to go back the origin of the sciences, one finds only obscurity, vague ideas, vicious circles; and one loses oneself in the primitive ideas.
>
> Lazare Carnot (96)

(95) For example, for the 17th century the geometrical and algebraic *Denkweisen* are replaced by two geometrical types: with or without kinematics. In modern physics, the world of quantum mechanics involves forms of algebraic and geometrical *Denkweisen* in its mathematisation, and also (competing)

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probabilistic and statistical forms. Elsewhere one notices the axiomatising *Denkweise* applied to mathematical physics: it is mostly concerned with restructuring of known results, like the form of algebraic thinking described in this paper, and at times slips into the notional mode of § 2.1b.

(96) L. CARNOT, *Essai sur les machines en général*, Dijon 1783, repr. 1786, conclusion. The book was reprinted in his *Oeuvres mathématiques*, Basel 1797, 1-124. His book cited in footnote 88 is a revised version, which became much better known.