CWI Quarterly Volume 9 (1 & 2) 1996, pp. 41 – 50

Mathematics in 19th Century Physics

Jed Z. Buchwald
MIT and the Dibner Institute for the History of Science and Technology

A quarter century ago Morris Kline at the Courant Institute in New York published a monumental survey entitled *Mathematical Thought from Ancient to Modern Times*. In a section entitled the "The loss of truth" Kline characterized mathematics at the end of the century in the following way:

> By the end of the nineteenth century, the view that all the axioms of mathematics are arbitrary prevailed. Axioms were merely to be the basis for the deduction of consequences. Since the axioms were no longer truths about the concepts involved in them, the physical meaning of these concepts no longer mattered. This meaning could, at best, be a heuristic guide when the axioms bore some relation to reality. Thus even the concepts were severed from the physical world. By 1900 mathematics had broken away from reality; it had clearly and irretrievably lost its claim to the truth about nature, and had become the pursuit of necessary consequences of arbitrary axioms about meaningless things (page 1035).

Kline placed here the origins of a divide between pure and applied mathematics, a division which, he asserted, did not for the most part exist much before the beginning of the present century.

One might, and I don't doubt that many contemporary mathematicians would, question Professor Kline's sympathies in such matters, but it does seem to me that the present-day division between pure and applied mathematics did not exist in anything like its current form throughout most of the 19th century. In England, for example, many of the people whom we today think of as creative physicists, such as George Gabriel Stokes, James Clerk Maxwell, or William Thomson (Lord Kelvin), were in fact products of the Cambridge Mathematical Tripos system, which, in their days, placed intense emphasis on mathematical dexterity. Stokes thought of himself as a mathematician for much of his career. Yet he was equally, indeed, deeply involved with optical experiments, and these two aspects of his work were tightly bound together. The situation in France throughout most of the century was similar.

Towards the beginning of the 19^{th} century the distinction between mathematical and physical work would not have been so clear as it later became, not least because so many mathematical problems arose directly out of, and remained tightly bound to, physical problems. Most such problems arose in the mechanics of point masses and continua, and, after *circa* 1816 in optics as well. By the 1820s electricity and magnetism had given rise to new sorts of problems involving integral representations, and by the 1850s, with the advent of field theory, the connections between integral representations and partial differential equations had also become significant. Issues arose in optics concerning solutions to the wave equation produced a great deal of novel, and physically significant, mathematics at the hands of Helmholtz and Kirchhoff, among others. New problems arose during the century out of physics in which questions involving the conditions to be satisfied by vector functions under specific transformations and particular constraints, this during a period in which the concept of the vector field was itself just beginning to take shape. Helmholtz, for example, was the first to produce an expression, one that soon became an important tool in hydrodynamics, for the conditions to be satisfied under the requirement that the integral of a flux vector through a surface be preserved under quite general deformations. The first general considerations of the transformation properties of vector functions were developed by the Irish mathematician James MacCullagh in the 1840s in his attempt to probe the underlying equations for the optics of crystals.

One could go on, listing at some length the many mathematical novelties that arose during the century in tight connection with a physics which was quite tightly connected to the laboratory. Rather than doing so, I have instead chosen several episodes from physics during the century in each of which mathematical developments are very powerfully bound to novel physical discoveries. I have chosen these episodes with an eye both to their novelty, for each case represents a new way of treating a physical problem, or a new physical problem altogether, and also to the particular character of the mathematics involved, since each case required developing mathematical structures that were not in common use among physicists, and that in some instances raised specific mathematical problems.

Our first example concerns optics, and, in particular, questions that, we might say in retrospect, concern the propriety of certain solutions to wave equations; our second concerns the physical implications that arise when one has integral equations but not partial-differential analogs of them; our second case concerns the physical implications that arise when one has integral equations but not partial-differential analogs of them; and our third involves prototypical differences, clearly evident by the end of the 19^{th} century, between an eager young physicist anxious to get a solution by any means possible, and the rigor demanded by an older mathematical colleague (not, of course, that I wish to suggest all mathematicians who demand rigor are old, and all physicists anxious to get results are young!). Here, in these three concrete cases, we will be able to see just how tightly meshed physics and mathematics were

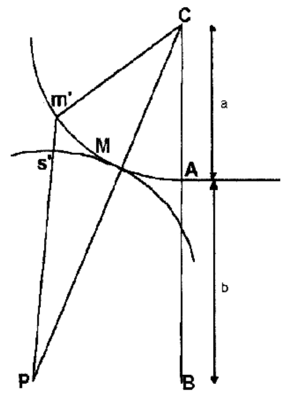

FIGURE 1

during this stunningly creative century. In 1815 a young graduate of the École Polytechnique named Jean Augustin Fresnel created a new optics based on the assumption that light is a wave form. Figure 1 is adapted from Fresnel's diagram. In it, C represents the source of a spherically- symmetric front AMm' that is intercepted by a screen AG. Adapting Huygens's principle, Fresnel conceived that each point on the front itself emits a spherical wave, albeit with an amplitude that decreases with inclination to the line joining that point to the source C. Introducing z as the distance along AM from the edge A of the diffractor, Fresnel could then represent the amplitude Q of a disturbance with wavelength 8 sent to an arbitrary screen point P in the following way:

$$\Psi = \sin\left[2\pi\left(t - \frac{CM + m's'}{\lambda}\right)\right]$$

$$m's' \approx \frac{z^2(a+b)}{2ab}$$

whence

$$\Psi = \sin\left[2\pi\left(t - \frac{CM}{\lambda}\right) - \pi z^2 \frac{a+b}{ab\lambda}\right]$$

which decomposes into

$$\Psi = \cos\left[\pi z^2 \frac{a+b}{ab\lambda}\right] \sin\left[2\pi\left(t - \frac{CM}{\lambda}\right)\right] +$$

$$\sin\left[\pi z^2 \frac{a+b}{ab\lambda}\right] \sin\left[2\pi\left(t - \frac{CM}{\lambda}\right) - \frac{\pi}{\lambda}\right]$$

Fresnel could at once conclude from this that the square of the resultant from all of the secondaries on the front, pairing up all terms with cosine amplitudes and all terms with sine amplitudes, can be computed from the following sum

$$\left(\int \cos\left[\pi z^2 \frac{a+b}{ab\lambda}\right] dz\right)^2 + \left(\int \sin\left[\pi z^2 \frac{a+b}{ab\lambda}\right] dz\right)^2$$

or, using a change of variables, to find

$$\int \cos\left(\frac{1}{2}\pi z^2\right) dz \text{ and } \int \sin\left(\frac{1}{2}\pi z^2\right) dz$$

There were two major difficulties with this result. One was how to establish an appropriately general coordinate system for calculation. This arises in the simplest case when the diffracting object, or aperture, has two edges, for then two limits are involved, and this in effect requires computing a surface. The other, which arises in all cases, including that of the semi-infinite plane, where only one boundary occurs (and where, accordingly, the surface just mentioned reduces to a line), is simply how best to calculate useful values for these integrals. Fresnel sought the quickest route to application, and that was by numerical integration (instead, e.g., through series, though Cauchy later developed divergent series for these integrals). In an astounding computational *tour de force*, Fresnel actually tabulated the integrals in steps of .1 from 0 to 5 by means of a method discovered in his posthumously published letters. Using Fresnel's own formulas, I have recalculated his tables with a machine, and I find that that his computational errors amount to a mean of only .0003. Further, the differences between his values and more accurate ones computed using the series later produced by Cauchy still amount to only .0006.

The appearance in physical equations of solutions that could only be evaluated by series expansions or by numerical integrations was by this time not altogether unusual. They emerged quite directly in astronomical problems, and they were soon also to appear in problems involving elasticity and heat flow. Nevertheless, what was unusual was the presence of such things as a fundamental expression of the underlying physics. For Fresnel's integrals, unlike, say, Legendre polynomials in astronomy, or, later, Fourier series in thermal processes, were not at all produced as solutions to given differential equations. On the contrary, they were asserted by him without his having had in hand the partial differential equation that they were meant to be solutions of, much less the methods and techniques necessary to solve such a thing under appropriate sets of boundary conditions. Fresnel, one might say, had discovered the solution to what would later be termed the reduced wave equation, or what was known after 1860 as the "Helmholtz equation", without having any idea at all what that equation was.

Let us now turn to our second episode, one that nicely illustrates, by means of a rather famous case, what may happen when only integral expressions,

$$\nabla \cdot \mathbf{E} = 4\pi\rho$$

$$\nabla \times \mathbf{B} - \frac{1}{c}\frac{\partial \mathbf{E}}{\partial t} = \frac{4\pi}{c}\mathbf{J}$$

$$\nabla \times \mathbf{E} + \frac{1}{c}\frac{\partial \mathbf{B}}{\partial t} = 0$$

$$\nabla \cdot \mathbf{B} = 0$$

FIGURE 2. Maxwell's equations

and not their partial-differential relatives, are available. Many readers will be familiar with "Maxwell's equations" for electromagnetism (Figure 2). These four, partial differential equations came to have something like their present form by the early 1890s, and had in fact been produced by Heinrich Hertz and Oliver Heaviside nearly a decade before that.

The second of these partial differential vector equations is often called the "Ampère Law" because it determines the magnetic field and, therefore, the mechanical forces that one closed, current-bearing circuit exerts on another one. As presented above the law also contains a term in the rate of change of the E (electric) field with time, indeed a very famous term, for it was the one said to have been introduced by Maxwell himself sometime around 1861–2. It represents the so-called "displacement current" (here *in vacuo*).

There are many interesting aspects to this story, but I want to concentrate on one only. Physicists often ask why it took so long for their 19^{th} century predecessors to realize that the "Ampère Law" actually had to have this extra term in it, for otherwise it would be in conflict with the long-known, elementary fact that open currents can produce collections of charge, i.e. with the continuity equation that links changing charge density to electric current. Specifically, if we write the "Ampère Law" without the extra term and just take its divergence, we see at once, that, in virtue of the continuity equation for charge, charge density could never change anywhere:

$$\nabla \times \mathbf{B} = \frac{4\pi}{c}\mathbf{J} \Rightarrow \nabla \cdot \mathbf{J} = \nabla \cdot (\nabla \times \mathbf{B}) = 0$$

and since the continuity equation requires

$$\nabla \cdot \mathbf{J} + \frac{\partial \rho}{\partial t} = 0,$$

it follows that ρ remains constant over time. But if we add $\partial \mathbf{E}/\partial t$ to the right hand-side of the Ampère law, then we have

$$\nabla \cdot \mathbf{J} = -\frac{\partial \nabla \cdot \mathbf{E}}{\partial t}$$

which, in virtue of the first Maxwell equation, is just the equation of continuity.

Clearly, inserting the "displacement-current" term into the Ampère Law solves our problem. Why then did it take so long for physicists to see the problem and to resolve it by this straightforward, essentially mathematical maneuver? The short answer is rather simple. In order to know that a problem of compatibility exists you must in the first place have the Ampère Law in this form, i.e. as a partial-differential expression. Neither Maxwell nor anyone else did until the late 1850s, specifically 1855, when Maxwell first began explicitly to produce integral forms of field equations for electrodynamics. Until then the prevailing view in Britain among the mathematically-minded held that Ampère's force- law, which had originally been developed for interactions between circuit elements, had no intrinsic physical meaning, and that the only physically-significant expression was obtained by integrating the elementary interaction about two closed circuits. That is, only integral expressions of the interaction were taken, in Britain, to be significant. Under such a view the problem of compatibility between the equation of continuity simply did not exist.

The issue arose only when, in 1855 and later, Maxwell began further to develop the field approach to electromagnetism, which considered all actions to take place through point to point influence, and therefore to have its true and proper representation in partial differential equations. Such expressions, and not their integral forms, were now taken to be fundamental. Under these conditions the compatibility problem did exist, and Maxwell even acknowledged a form of it in 1855 – but at the time he specifically limited his analysis to closed circuits, so that, even then, the problem did not exist in anything quite like its stark, modern form. In fact, recent historical work has shown, I think quite persuasively, that Maxwell's "displacement current" did not emerge out of considerations grounded in the formal structure of the emerging "Maxwell equations", but rather out of Maxwell's engagement in the early 1860s with a mechanical model for the medium whose behavior constitutes the electromagnetic field. Here, one might say, mathematical structures were bound to, and derived their significance from, physical considerations.

Mathematical were, as this case illustrates, hardly neutral in what they had to say about the physical assumptions of those who used them, and what they could and were used to do. Indeed, the particular mathematics used, and even the precise mathematical moves made, may be tightly bound to the underlying physical imagery. My final example, which concerns Heinrich Hertz, illustrates this and other points concerning the relationship between 19$^{\text{th}}$ century mathematics and physics rather well.

In order to follow what I want to say about Hertz we must begin with his mentor, Hermann von Helmholtz. This is not the place to go into details, but it is important for us to see that Helmholtz, particularly after 1870, based his physics and its correlated mathematics on the notion that interactions between objects are determined by only two things: first, the states of the objects at a given instant, and second, the distance between the objects at that same moment. These states are for the most part not considered to be reducible to anything else; they are qualities of objects that can be assigned numerical values. (Such, e.g., as a state of charge or a state of conduction, or even a state of strain.) Then, according to Helmholtz and his group, which included the young Heinrich Hertz after 1878, a particular interaction is represented by a so-called potential function that embodies (albeit not unproblematically) the energy stored in the bipartite system formed by the objects in particular states at a given distance from one another.

There are two aspects to this way of thinking and working that I want now to illustrate. First, the insistence on the bipartite character of all interactions carried with it the consequence that, in many instances, mathematical structures were not to be deployed in traditional ways even for problems that bore close similarities to ones that were thought already to be well understood. Second, the emphasis on finding appropriate potential functions, though constraining in certain circumstances, also provided mathematical tools that a resourceful young investigator like Hertz could deploy where others, as we shall see, could perceive no way forwards.

The Hertzian episode that we shall examine involves elasticity, and as formulated by him it was not a traditional one in this by then (the 1880s) highly-mathematical subject. Hertz, always particularly interested in bipartite interactions, wanted to find out what deformations occur, and what stresses arise, when two bodies are pressed together either by an external force or by forces of impact. Traditional problems in elasticity involved only one deformable body; what the body touched was considered to be immutable or simply to be given (as, for example., a wall with a flexible beam embedded in one end, or a sphere whose surface is subject to a given stress). The kind of problem Hertz had in mind was different, and it required him to invent some way to retrieve a simulacrum of the usual situation from his own in order to deploy the mathematics of stress that he had learned from the contemporary German master of elasticity, Gustav Robert Kirchhoff. This was not simple to do; and Kirchhoff, reading essentially with a mathematician's eye, did not like what he saw.

Hertz had first of all to create a system of coordinates that, on the one hand, would express the contact of the two bodies as they press together, and, on the other hand, that could also be used to calculate stresses and deformations. These two desiderata do not mesh nicely. The former (expressing bodily contact) demands a system that depends upon the relation between the two bodies; the latter (calculating stresses and deformations) demands a fixed surface for boundary conditions. These requirements are in apparent conflict because the one seems to demand a moveable system of coordinates, whereas the other seems to require a fixed one, and this was no doubt one reason for the comparative neglect of the problem among traditional elasticians.

Hertz's clever solution, which would not (and in fact did not) appeal to rigorous elasticians, was this: he made the system of coordinates itself approximate and mutable; he made it something that depended upon the physical character of the interaction. This was not an easy solution, as is amply evident from Kirchhoff's having required Hertz extensively to rewrite his initial explanation of it. The essential idea was this. When two elastic bodies press together, Hertz argued, the resulting deformation will usually be limited to a small region near their surface of contact. Far away from that region they will remain undeformed, but not unmoved: these far regions will move closer together very nearly as rigid bodies. So, Hertz decided, the appropriate thing to do is to introduce two systems of coordinates. Each system is rigidly connected to the undeformed region of one of the two bodies and moves with it. As the bodies press together, then, their respective systems of coordinates also move together through some distance, and one goal of the theory was to find a way to compute that distance.

It happens that the original manuscript for the article that Hertz wrote to do so exists today, and it is covered with emendations written by Kirchhoff, who examined the piece for possible inclusion in Borchardt's *Journal für die reine und angewandte Mathematik*. Much of the original passage in the MS was actually crossed out by Kirchhoff and then entirely rewritten by Hertz. Hertz's original wording had treated the new coordinate systems as conveniences for calculation and explained very little about them. The new one carefully explained them. Kirchhoff, with the late 19$^{\mathrm{th}}$ century mathematician's eye for rigor, had insisted on a much more careful explanation of what were, after all, entirely novel and admittedly approximate coordinate systems. It was not that Hertz introduced an approximation after having laid out the exact conditions of the problem, which was the traditional procedure. Rather, he began with an approximation for the problem's elementary mathematical structure. This was evidently sufficiently novel to be disturbing to a rigorous elastician like Kirchhoff.

Kirchhoff's emendations did not cease here. There was one other paragraph of this kind which Hertz did not find it easy to accept. For Kirchhoff insisted on very large alterations indeed to Hertz's mathematics, or, better put, he insisted on Hertz putting in mathematics that was missing, that Hertz had jumped over *via* qualitative argument. The nub of Kirchhoff's objection was, it seems, this. Aside from the complexity introduced by his novel coordinate-systems, Hertz's analysis had the following structure. He first provided a partial-differential equation and a set of boundary conditions. He then (following what was by this time becoming his common practice) introduced a possibly-suspect (because unexamined) potential function and went from that function directly, that is without analytical proof of any kind, to expressions for the elastic displacements, arguing along the way that the relevant conditions of the problem were ipso facto satisfied. Kirchhoff rejected almost all of Hertz's original argument here and himself wrote out several pages of direct analytical demonstration, but reaching in the end precisely the same results that Hertz had. Kirchhoff's emendations were used by Hertz verbatim in the printed version, without any mention that they differed considerably from what he had originally submitted.

This episode goes beyond the specifics of the Hertz-Kirchhoff disagreement and has something to tell us about relations in late century, in Germany at least, between someone focused on experiment and the generation of empirical novelty, and someone focused more on mathematical rigor. Despite the fact that Kirchhoff had himself done seminal work in producing the laws for radiant energy, by the 1880s he was preeminently concerned with the development of rigorous mathematical structures, particularly in the theory of elasticity. Indeed, Boltzmann wrote of Kirchhoff that he strove to "avoid bold hypotheses", to "build equations that correspond to the phenomenal world as truly as possible and quantitatively correctly, unconcerned with the essence of things and forces". This forms a striking contrast to Hertz's, and indeed to Kirchhoff's close friend and colleague, Helmholtz's, attitudes. For neither of them was at all concerned to avoid "bold hypotheses", and both had, if not a lax, nevertheless what one might characterize as an instrumental attitude toward mathematics.

Historians generally do not much like to draw lessons from history; the past is for the most part so different from the present that it is dangerous to make comparisons, and I will not violate this professional canon. The entire organization of science and mathematics, its scale, its connections with the broader social and cultural polity, are today considerably different from what they had been in most European countries in the 19^{th} century. Physics itself has evolved in ways that make the sorts of experimental discoveries and mathematical analyses produced by Hertz, working almost entirely alone, or Fresnel, who produced wave optics in still more primitive conditions, nearly inconceivable today outside, at least, of certain areas of mathematics itself.

During the 19^{th} century, as in the previous two, most mathematical work, as well as most work in quantitative natural science, was not inherently collaborative, and it tended also to be comparatively homogeneous. Although mathematicians and natural scientists have, since the origins of the modern way of thinking and doing in the 17^{th} century, always had other practitioners in mind, nevertheless they rarely worked together, though there are several notable exceptions. In experimental work, which first became widespread near the end of the 18^{th} century, the investigator usually had assistants, but he rarely had collaborators. Work on paper was even more isolated, at least in a punctual fashion, in that investigators as it were came into one another's individual spaces primarily through correspondence and the occasional formal meeting. This began to change, at first in France, towards the end of 18^{th} century, and the existence of departments and institutes in Germany by the middle of the 19^{th} century made close and ongoing contact between investigators much more common. Nevertheless, throughout most of the century fundamental work in both physics and mathematics still tended to be done in this punctually isolated fashion.

This is no longer generally so, at least in physics, and to some extent in mathematics as well. The near-instantaneity of electronic communications has displaced the old, stately world of preprint circulation; results are spread rapidly, and isolation has become comparatively rare. Fresnel would not today work alone at an optical bench; he would be part of a large, nicely-funded team, and his apparatus would no doubt cost several billion dollars, while he would certainly not have bothered developing clever numerical techniques for integrating his equations; he would have used a computer. Many of us have, and will continue to, benefit from these changes. In any case we live in this new world and cannot take ourselves out of it. It has clearly produced changes in the ways in which physics and mathematics are practiced, perhaps even in the meaning of what it is to make a novel discovery. Whether these changes are good or bad is not a question for a historian to answer, but I will say this much. For me, the world of the 19^{th} century physicist and mathematician has a deep, even profound sort of attraction. It was a world in which the individual stood a good chance of making a signal contribution to physics or mathematics without being a member of a heterogeneous team engaged in a mega-project of some sort. It was a world in which one person, working essentially alone, could create an altogether new way of thinking and working. That, I think, is an unlikely event today. There may be no Hertz or Fresnel or even Einstein for the 2^{st} century historian, though there may still be a Hilbert or Riemann or Klein.