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HEURISTIC REASONING IN APPLIED MATHEMATICS*

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1. Introduction. About seventy years ago Ludwig Prandtl [1] initiated a rather remarkable contribution to applied mathematics. At the time he was trying to understand the two-dimensional flow of a viscous incompressible fluid past a rigid obstacle (a typical configuration is shown in Fig. 1), and his interest centered on situations in which U, L,

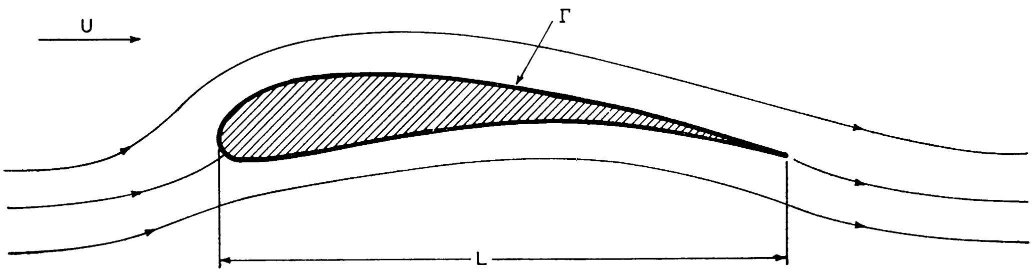

Fig. 1. Geometry of Prandtl's problem.

and the kinematic viscosity ν of the fluid were such that $\nu/LU \ll 1$.

The differential equations implying the conservation of mass and momentum could have been cast in the form

$$\nu\,\Delta\omega - \mathbf{v}\cdot\text{grad}\,\omega = 0 \tag{1.1}$$

and

$$\Delta\psi = \omega \tag{1.2}$$

where the velocity $\mathbf{v}$ is related to ψ by

$$\mathbf{v} = (u, v) = (\psi_y, -\psi_x), \tag{1.3}$$

ω is the vorticity, i.e.

$$\omega = \hat{z}\cdot\text{curl}\,\mathbf{v}, \tag{1.4}$$

and Δ denotes the Laplace operator. The boundary conditions required that $\psi = \psi_n = 0$ on Γ (see Fig. 1) and $\mathbf{v} \to \hat{x}U$ as $x^2 + y^2 \to \infty$.

The problem is nonlinear in a very nontrivial way, the geometry is messy, the domain

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is large, and methods conventionally available at the time were quite inadequate for the task in hand.

Prandtl's remarkably perceptive but equally simple observation was equivalent to the following. Eq. (1.1) describes the balance which must be achieved between the diffusion of vorticity from particle to particle and the carrying along of the vorticity "contained" by a particle as it moves at velocity $\mathbf{v}$. The time it takes most particles to traverse a distance L is of order L/U and the distance, normal to the object, over which vorticity can be transported by the diffusive process during that time is of order $\delta = (\nu t)^{1/2} \equiv (\nu/UL)^{1/2}L \ll L$. It followed readily that $\Delta\omega$ is very well approximated by ω_{nn} near the object (ω_{nn} merely denotes the second partial derivative in the direction of the local normal to Γ); furthermore, outside of a thin layer (of order δ) near the object and directly downstream of the object ω is zero for all practical purposes, simply because there is no mechanism by which vorticity could have been transported to such locations. An equivalent argument suggests that wherever ω differs significantly from zero, one can write

$$\omega \simeq \psi_{nn} . \tag{1.5}$$

The fact that $\Delta\psi = 0$ over much of the domain, the simplification of Eqs. (1.1) and (1.2) achieved by the foregoing approximations and the fact that ω need be described only in $n < \delta$ not only permitted Prandtl to solve his problem but have also given rise to a very large body of mathematical formalism commonly referred to as "singular perturbation theory" or "matched asymptotic expansions"; more importantly, they also have provided the basic foundations for the heuristically-reasoned, spectacularly successful treatment of many important problems in science and engineering. This success is probably most surprising to rigor-oriented mathematicians (or applied mathematicians) when they realize that there still exists no theorem which speaks to the validity or the accuracy of Prandtl's treatment of his boundary-layer problem; but seventy years of observational experience leave little doubt of its validity and its value.

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About sixty years ago Peter Debye made a rather different contribution. He found a very powerful technique for evaluating some integrals he needed. In essence his discovery was an ingenious extension to the complex domain of an idea attributed to Laplace which dealt with real integrands. For my purposes, it suffices to describe the latter and it requires fewer pages. The integrand under consideration is the product of two functions indicated in Fig. 2. One of the two functions, $f(x)$, is narrow in the sense

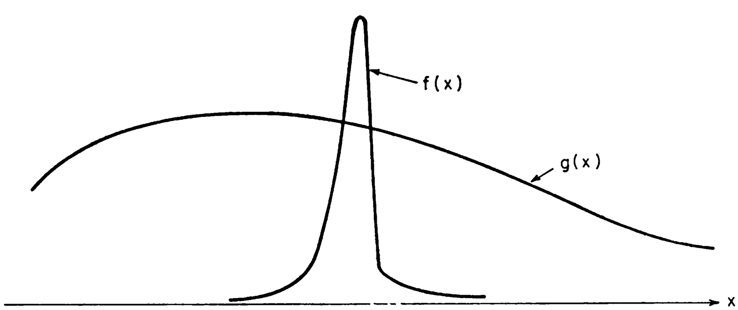

FIG. 2. The functions $f(x)$ and $g(x)$ of Laplace's problem.

that most of its area is associated with an interval on the x axis which is small compared to the distances over which there occur significant changes of the other function, $g(x)$. Fig. 2 provides plausibility for the argument that one might approximate the integral by $I \simeq g(x_0)F$, where F is the area under f and x_0 is the "center of gravity" of $f(x)$. Actually, Laplace's method was developed for functions $f(x)$ whose form typically is

$$I = \int_a^b g(x) \exp(-\lambda p(x))\, dx,$$

with $p'(x_0) = 0$ at some point $a < x_0 < b$, $g(x_0) \neq 0$ and $p''(x_0) \neq 0$. There are approximations beyond those described above which render the result even more simply explicit, but the important innovation has been described. Extensions of this use of the idea, which follow rather directly, note that it is sometimes very advantageous in subsequent manipulations and interpretations to replace one integral

$$I = \int g(x)f(x)\, dx$$

by another,

$$I^* = \int g(x)f^*(x)\, dx,$$

where f^*, which is much more convenient in the continuing analysis than is f, is chosen because it duplicates those macroscopic features of f which are important in determining the character of I (e.g. its area, its width in some appropriate sense, etc.).

The Laplace evaluation can be cast in the form of a formal asymptotic expansion in λ together with the error estimates which usually accompany such formalisms, but again, there seems to be no rigorous support for the direct extensions alluded to above, a fact which need not detract at all from the utility of the device and need not deter one from noticing that it frequently works beautifully even when f is not particularly narrow.

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In 1921, while studying the dispersion of particles in a moving fluid, G. I. Taylor needed some measure of the extent to which the velocity fluctuations at one location were statistically related to those at another. To him it was clearly plausible to adopt the simply-defined quantity, $R = \langle u(x, t)u(x + \delta, t)\rangle$, where $\langle\ \rangle$ denotes an average over time. The viability of the choice rests on its simplicity and the fact that the size of R at any given δ is clearly related to the extent to which statistical knowledge about the fluctuations at x implies statistical information about the fluctuations at $x + \delta$.

The *understanding* of turbulence still eludes us, of course, but the invention has played an enormously important role in our accumulation of *knowledge* about turbulent motions and it has been equally important in describing and understanding many other phenomena.

2. Applied mathematics. The studies outlined in the foregoing are three rather different examples of contributions which have had a profound influence on the mathematical treatment of scientific and other "real world" problems. I have outlined them, with considerable license, not to reveal the inaccuracy of my historical knowledge but because they have one common feature which I wish to emphasize: the invention, in each case, was not an abstraction—or a class of abstractions—or a class of all classes of abstractions! The first invention (the boundary layer concept) involved a simple quantitative idea which was based on a clear understanding of the underlying mechanisms and the nature of the phenomenon; beyond that, it required only a willingness to approximate in accord with the idea but without firm error estimates. The second required only a clear picture of the revelant features of the mathematical object (not its abstraction) and a willingness to discard that which was not essential to the task; the third required only a clear recognition of the role of the object (correlation function) to be defined and a willingness to go ahead without trying to choose among all possible alternatives. The investigator did not ask himself: find all possible approximations to Eqs. (1.1) and (1.2) whose solutions with the given boundary conditions differ from the true solutions by less than ϵ—or find all possible measures of the statistical correlation of the random variable $u(x + \delta)$ and $u(x)$. Rather, he asked himself: Can I find *one* simpler set of requirements on ψ which render its calculation tractable and which still retain enough of the underlying physics of the phenomenon to guarantee that the ψ so generated can be interpreted in the context of the scientific problem? or, Can I define *one* measure of the correlation of the velocity fluctuations which will aid me in trying to gain insight into the nature of this frustratingly irregular phenomenon?

There is still a multitude of worthwhile, unanswered questions in the world of natural science, in economics, in human physiology and other biological disciplines and in most of human endeavor, whose answers will ultimately be obtained (or rather evolved) with the help of mathematical tools and mathematical reasoning. Of these, I feel rather certain, many (perhaps most) will require the foregoing varieties of heuristic argument, the "find it before you prove it exists" attitude, and the determination to understand the real phenomenon; ordinarily, they will use, in no more than a peripheral way, the compounded abstractions and the concern with compounded abstractions which seem to have become the principal preoccupation of so many members of that community labeled (or merely identified as) applied mathematics. To repeat, most innovative, truly productive advances in the mathematization of disciplines with quantitative aspects will require the informal, opportunistic reasoning illustrated in Sec. 1.

Sooner or later, of course, these advances in knowledge and understanding will be carried out. But a question I can't answer asks whether applied mathematicians will make these activities their business or whether they will leave them to the engineers, the economists, the biologists and others. Another question which has more immediate importance arises from the following observation. To a large extent, the community of core mathematicians has decided that it is not its responsibility to provide instruction related to the *application* of mathematics; to the same large extent, much of the instruction in methodology has become the responsibility of the applied mathematics community. The important question is: Will this community include in the instruction it offers, illustrations of the heuristic, inventive, reasoning so necessary to progress in science, in technology, and ultimately in our whole society or will it retreat into a cloistered preoccupation with abstractions both in its research and its instruction? If it does retreat, then the present generation and possibly a few future generations of students will receive little instruction of this sort and those that enter the worlds of technology, of science, of environmental repair and of medical advancement, will do so severely handicapped by a grotesquely distorted education.

Our symposium is labeled "The future of applied mathematics." It is my view that if, in answer to the foregoing questions, we make the latter choice, applied mathematics has no viable future whatever—it could only become a small, rather sterile corner of mathematics, a discipline which despite its moments of greatness, already is populous enough to contain more sterility than it wants. But if we make the former choice, i.e. if among all of its other activities, the applied mathematics community provides innovative contributions to the mathematization of disciplines which are just becoming quantitative, if it concerns itself with finding approximate models for complicated phenomena (thereby sacrificing accuracy of detail for ease of interpretability), if it seeks to develop techniques whereby the implications of given mathematical models can more easily be inferred, then applied mathematicians will have inherited much of today's continuation of the challenges, the intellectual achievements, the contributions to society, and the fun which, before they abdicated, was largely the property of the mathematicians and the physicists.

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