An Applied Mathematician's Apology

by John Miles

I EXPECT that many of you will recognize my title as derived from G. H. Hardy's *A Mathematician's Apology.* But, whereas Hardy felt no need to define *mathematician,* the position is otherwise for an *applied mathematician.* Some mathematicians, I fear, might choose to borrow von Kármán's definition of an aerodynamicist and define an applied mathematician as one who "assumes everything but the responsibility."

The applied mathematician, naturally, might prefer a more flattering description. To this task I am unequal, but I believe that the spirit of applied mathematics is admirably conveyed by Lord Rayleigh's statement [from the preface to the second edition of his *Theory of Sound*]:

> In the mathematical investigations I have usually employed such methods as present themselves naturally to a physicist. The pure mathematician will complain, and (it must be confessed) sometimes with justice, of deficient rigor. But to this question there are two sides. For, however important it may be to maintain a uniformly high standard in pure mathematics, the physicist may occasionally do well to rest content with arguments which are fairly satisfactory and conclusive from his point of view. To his mind, exercised in a different order of ideas, the more severe procedure of the pure mathematician may appear not more but less demonstrative.

Now it may be objected that Rayleigh was a physicist, not an applied mathematician. Such distinctions are difficult. I am told that a physicist and a mathematician once disagreed on whether the late John von Neumann was a physicist or a mathematician. The physicist suggested that they resolve their argument by putting the following problem to von Neumann: *Two locomotives are approaching one another on the same track at a relative speed of 10 miles per hour. A deer bot-fly begins to fly back and forth between the locomotives at a constant speed of 100 miles per hour at a time when the locomotives are 5 miles apart. How far does the deer bot-fly fly before he is crushed between the two locomotives?* Now the idea here, or at least the idea held by physicists, is that a mathematician naturally will calculate the general term for the fly's *n*'th passage and then sum the series — whence, after some considerable time, he will arrive at the answer. The physicist, on the other hand, supposedly remarks to himself that the elapsed time between start and finish is 5 miles divided by 10 miles per hour, or 1/2 hour, and hence comes up very quickly with the answer that the deer bot-fly must fly 50 miles.

Well, our physicist put the question to von Neumann, and von Neumann answered instantly, "50 miles." The physicist started to exclaim, "Oh, Professor von Neumann, I am so glad to learn. . . ." "Oh, it was nothing," interrupted von Neumann. "I only had to sum a series!"

At this point, lest I be placed in the position of disbursing entomologically unsound information to a general audience, I think that an aside on the deer bot-fly may be in order. I therefore would like to exhibit what is perhaps both my shortest and my most widely read publication, a letter written to the Sydney *Morning Herald* on 21 January 1963.

Sir,

I refer to a recent series of letters in these columns . . . commenting on the speed of the deer bot-fly. Claims that this insect, also known simply as the deer fly, could achieve speeds up to 818 miles per hour [mph] have been made repeatedly for over a quarter of a century. E.g., the *Illustrated London News,* 1 January 1938, credited the female deer fly with a speed of 614 mph and the male with 818 mph (this alleged advantage of the male, presumably in the interests of biological necessity, appears to have been overlooked in other sources). *Newsweek,* November 15, 1954, stated, "Some naturalists claim that the little deer bot-fly, the fastest thing alive outside of winged man, can hit 400 mph." It would appear that, by 1954, naturalists had become aware of the difficulties of supersonic flight.

In fact, this question was dealt with decisively by Irving Langmuir, Nobel Laureate, in 1938 [*Science,* Vol. 87, pp. 233-234, March 11, 1938]. Langmuir identified the original source of such claims as Charles H. T. Townsend, who, writing in the *Journal of the New York Entomological Society,* stated that "on 12,000 foot summits in New Mexico I have seen pass me at an incredible velocity what were certainly males of Cephenomyia. I could barely distinguish that something had passed — only a brownish blur in the air of about the right size for these flies and without a sense of form. As closely as I can estimate, their speed must have approximated 400 yards per second." Dr. Townsend did not say how he identified the sex of the "brownish blur" and appears not to have realized that 400 yards per second is equivalent to 818 mph. . . .

Langmuir, on the basis of reasonable assumptions, estimated that, to achieve a speed of 818 mph, a deer fly would have to consume 1.5 times his own weight of food each second; at 25 mph, the corresponding figure would be 5 percent of his weight per hour. Langmuir also estimated, on the basis of experiments, that a deer fly would appear merely as a blur at 13 mph, would be barely visible at 26 mph, and would be wholly invisible at 64 mph. And, referring to the statement that deer flies "strike one's bare skin with a very noticeable impact," he commented that if the speed were 818 mph "such a projectile would penetrate deeply into human flesh." He concluded that "a speed of 25 miles per hour is a reasonable one for the deer fly, while 800 miles per hour is utterly impossible."

I remain, Sir,

Yours faithfully,

To return to the problem of defining an applied mathematician: The lines were not always drawn so sharply, either between physics and mathematics or between pure and applied mathematics. Indeed, applied mechanics as we know it today was largely the creation of Euler and the Bernoullis, and the vibrating string was the field on which such as Euler, Daniel Bernoulli, D'Alembert, and Lagrange waged their celebrated battles on the nature of a solution to a partial differential equation.

Von Kármán, in his 1940 address to the ASME on "Mathematics from the Engineer's Viewpoint," called the 18th century the "heroic period" of mathematics and the 19th century the "era of codification." Mathematics in the 18th century was largely, if not mainly, motivated by an understanding of the real, physical world. All of this had changed by the middle of the 19th century, at least on the Continent — although we should not forget that Gauss, surely the greatest mathematician of that century, was not above applied mathematics.

In England, this transition was delayed, and, as late as 1874, Maxwell declared: "There may be some mathematicians who pursue their studies entirely for their own sake. Most men, however, think that the chief use of mathematics is found in the interpretation of nature." And, if this seems too extreme, we have only to read the papers of Sir George Gabriel Stokes, the Lucasian Professor of Mathematics in the University of Cambridge through the end of the 19th century. But then I suppose that most mathematicians would regard Stokes as a physicist. (As far as I know, no one ever asked him about the deer bot-fly and the locomotives.)

I have, perhaps, dwelt too long on a bootless attempt to define an applied mathematician and should turn to what I originally promised — an apology for being one. This theme — of explaining why one does what he does — has been dealt with by both Hardy and A. E. Housman. Hardy began his 1920 inaugural lecture as Professor of Mathematics at Oxford by saying:

> There is one method of meeting such a situation which is sometimes adopted with

> considerable success. The lecturer may set out to justify his existence by enlarging upon the overwhelming importance, both to his University and to the community in general, of the particular studies on which he is engaged. He may point out how ridiculously inadequate is the recognition at present afforded to them; how urgent it is in the national interest that they should be largely and immediately re-endowed; and how immensely all of us would benefit were we to entrust him and his colleagues with a predominant voice in all questions of educational administration.

All of which sounds familiar today.

Hardy returned to this theme some 20 years later, and, in *A Mathematician's Apology,* says:

> A man who sets out to justify his existence and his activities has to distinguish two different questions. The first is whether the work which he does is worth doing; and the second is why he does it, whatever its value may be. The first question is often very difficult, and the answer very discouraging, but most people will find the second easy enough even then [and] the only answer which we need consider seriously [is] I do what I do because it is the one and only thing that I can do at all well.

And, as for the first question, whether the work he does is worth doing, Hardy concludes his *Apology* by saying:

> The case for my life, then, . . . is this: that I have added something to knowledge, and helped others to add more; and that these somethings have a value which differs in degree only, and not in kind, from that of . . . other artists, great or small, who have left some kind of memorial behind them.

But, to Hardy at least, mathematical creation had a special "character of permanence" which led him to declare: "Archimedes will be remembered when Aeschylus is forgotten, because languages die and mathematical ideas do not."

C. P. Snow, referring to this eloquent statement, questioned whether mathematical fame was not a little too "anonymous" to be wholly satisfying and pointed out that the work of Aeschylus carries with it a much more coherent picture of the writer's personality than does that of Archimedes. Another friend of Hardy's, when they were passing the Nelson column in Trafalgar Square, asked him whether, if he had a statue on a column in London, he would "prefer the column to be so high that the statue was invisible, or low enough for the features to be recognizable?" Hardy answered, "I would choose the first alternative, Dr. Snow, presumably, the second."

A. E. Housman, in his 1892 *Introductory Lecture* to the united Faculties of University College, London, attacks the question on a broader front. As he points out,

> Everyone has his favorite study, and he is therefore disposed to lay down, as the aim of learning in general, the aim which his favorite study seems specifically fitted to achieve, and the recognition of which as the aim of learning in general would increase the popularity of that study and the importance of those who profess it. . . . And accordingly we find that the aim of acquiring knowledge is differently defined by different people. In how many different ways, I do not know, but it will be sufficient . . . to consider the answers given by two great parties: the advocates of those sciences which have now succeeded in arrogating to themselves the name of Science, and of those studies which call themselves by the title, perhaps equally arrogant, of Humane Letters.
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> The partisans of Science define the aim of learning to be utility. I do not mean to say that any eminent man of science commits himself to this opinion; some of them have publicly and scornfully repudiated it, and all of them, I imagine, reject it in their hearts. But there is no denying that this is the view which makes Science popular. . . . And the popular view has the very distinguished countenance of Mr. Herbert Spencer. . . . The following, for instance, is the method by which he [Spencer] endeavors to terrorise us into studying geology. We may, any of us, some day, take shares in a joint-stock company, and that company may engage in mining operations; and those operations may be directed to the discovery of coal; and for want of geological information the joint-stock company may go mining for coal under the old red sandstone, where there is no coal; and then the mining operations will be fruitless, and the joint-stock company will come to grief, and where shall we be then? This is, indeed, to eat the bread of carefulness. After all, men have been known to complete their pilgrimage through this vale of tears without taking shares in a joint-stock company. But the true reply to Mr. Spencer's intimidations I imagine to be this: that the attempt to fortify man's estate against all contingencies by such precautions as these is in the first place interminable and in the second place hope-

less. As Sarpedon says to Glaucus in the *Iliad*, a hundred thousand fates stand close to us always, which none can flee and none avoid. The complexity of the universe is infinite, and the days of a man's life are threescore years and ten.

Turning to the humanists, Housman tells us that:

> While the partisans of Science define the end of education as the useful, the partisans of the Humanities define it, more sublimely, as the good and the beautiful. We study, they say, not that we may earn a livelihood, but that we may transform and beautify our inner nature by culture.

But he finds the partisans of the Humanities no more convincing than those of Science and concludes that:

> The two fancied aims of learning laid down by these two parties will not stand the test of examination. . . . And no wonder, for these are the fabrications of men anxious to impose their own favorite pursuits on others, or of men who are ill at ease in their conscience until they have invented some external justification for those pursuits. The acquisition of knowledge needs no such justification. . . . Curiosity, the desire to know things as they are, is a craving no less native to the being of man, no less universal through mankind, than the craving for food and drink.
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> For knowledge . . . is not merely a means of procuring good, but is good in itself simply: it is not a coin which we pay down to purchase happiness, but has happiness indissolubly bound up with it.

Returning to my own apology, I want to make some general remarks on what an applied mathematician does and how he does it. It is customary, in the first place, to distinguish between the attitudes and activities of the applied mathematician, on the one hand, and, say, the theoretical physicist on the other hand. In fact, as I have already said, this distinction is not a very sharp one — not nearly as sharp, for example, as the distinction between the applied mathematician and the pure mathematician.

It is sometimes argued that the primary interest of the theoretical physicist is the discovery of *new* physical laws, whereas that of the applied mathematician is the description of physical phenomena in terms of *known* physical laws. Now, to some extent, this is true; the modern theoretical physicist works, for the most part, in areas where the so-called laws are less securely established, usually because they fail in certain critical predictions, whereas the applied mathematician typically works in an area such as mechanics, where the basic framework goes back at least to Euler and Cauchy, if not to Newton. On the other hand, very few theoretical physicists are so fortunate as actually to discover new laws, and the adequacy of the accepted laws of mechanics for the prediction of next winter's rainfall, let alone the next ice age, is still open to question. Nevertheless, the foundations of fluid mechanics are pretty firm, and — even in areas where one expects them to break down, as in describing shock waves of only a few mean-free paths in thickness — the Navier-Stokes equations have been spectacularly successful. So much so that David Gilbarg once remarked that "equations have often been successful beyond the limits of their original hypotheses, and indeed this type of success is one of the hallmarks of a great theory."

This brings me to a theme that has, I believe, always been appreciated by both theoretical physicists and applied mathematicians but that has been espoused with special vigor by James Lighthill — namely, that the real issue for the applied mathematician is the interaction between mathematical analysis and physical ideas; and that, in particular, the primary job of the applied mathematician is the generation of new physical *ideas* through mathematical investigations. As Truesdell and Toupin put it, in their *Principles of Classical Mechanics and Field Theory*,

> The developments must illumine the *physical aspects* of the theory, not necessarily in the narrower sense of prediction of numerical results for comparison with experimental measurement, but rather for the grasp and picture of the theory in relation to experience. In this spirit do we pursue our subject, *neither seeking nor avoiding* mathematical complexity.

I believe that the central idea which I have been trying to convey in these last remarks — that mathematical analysis of physical problems frequently leads to physical ideas that become evident only after the formal analysis has been carried through — can best be expressed by another of von Kármán's favorite aphorisms: "Mathematics is sometimes more intelligent than the people who use it!"

In fact, I do not wish either to overvalue, or to undervalue, the role of mathematics in the understanding of nature. But I do say, quite unequivocally, that it *is* with the understanding of nature that applied mathematics is primarily, if not wholly, concerned.☐