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MATHEMATICS AND THE REAL WORLD

Inaugural Lecture of the Professor of Applied Mathematics delivered at the College on October 18, 1966

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I PROPOSE to begin by pointing out some of the limitations of the subject. Very often a layman will formulate a problem, ask us to 'work it out' and be disappointed by the answer that he gets! It is fairly obvious that in economics only a selection of the variables that matter can be measured numerically. We can assign numbers to prices and flows, but not to equally important variables like confidence and credibility. In politics we can count votes and record opinion polls, but many imponderable factors remain. If we were to confine the subject of music to what can be expressed mathematically, we should get but a poor shadow of the real subject. Even a specialist like an actuary is not all mathematician. He is usually thought to be a person who does nothing but calculate the probability that you will be alive next year – in reality this is quite a small part of his work.

There must be limitations even if we treat a purely mechanical problem. Consider the well-known laboratory experiment in which a pendulum is swinging on knife edges, and we want to predict its time of swing. Simple treatments are easily obtained, but then we find that corrections should be made because, for example, the knife edges are curved rather than being geometrical wedges, the surfaces on which they rest are not perfectly flat, air resistance affects the period, the motion of the pendulum pulls the supports which in turn pull on the room, vibrations from traffic can affect the pendulum – the list of possible corrections that might conceivably be important seems endless. Of a truth it is endless, but one must stop somewhere if one is to get a problem that can be set up and solved at all.

Therefore, we must expect some adaptations to occur. Certain aspects of a new problem may have to be ignored or simplified before it becomes tractable, and existing methods may have to be modified to cope with some new feature. I should like to describe this relationship between the mathematical methods and the problems studied as something like that between a machine tool and the material that it is working on. As this analogy suggests, some adaptation between methods and problem will always take place. One can, if one likes, describe this situation as one involving some stresses, but I most emphatically reject the idea that mathematics is a house divided against itself. I shall return to this point later.

Sometimes it does happen that pressure for quick answers leads us to use methods that are more rough and ready, or less rigorous, than could be wished. This may happen for reasons quite unconnected with the mathematics; calculations about an atomic bomb or a satellite may be useless if they are not available at the right time. A research worker has only one life and one research career and he may feel that he has to press on with his work even though there may be real doubt about the correctness of some of his steps.

These considerations all lead to the same sort of conclusion – that if one looks at applied mathematics as a whole, one may expect to find parts of it at all stages of development. Relatively few will be in an absolutely final form so that they can be accepted as a permanent part of knowledge. The vast majority will be at various earlier stages. One can compare the development of a piece of applied mathematics with the process of putting up a building. At a certain stage the scaffolding can gradually be taken down and the building can stand by itself.

I think I have talked enough generalities – perhaps I can illustrate these remarks by some actual examples.

An example of a piece of work that can now be regarded as a permanent part of knowledge is the celebrated calculation by Lord Kelvin of the maximum energy available to provide the heat of the sun, assuming that

it all came from gravitational attraction (the only source of energy then known). This calculation is quite easy; I suppose it could be done by a second-year undergraduate with a little help. The conclusion was that there was only enough energy to keep the sun going for about twenty million years at its present rate. This was in serious conflict with geological evidence – a combination of the known thicknesses of the sedimentary rocks with reasonable estimates of the rates at which they could be laid down pointed to a very much longer life for the earth. The puzzle was resolved by the discovery of radium, followed shortly by the realisation that matter can, in certain circumstances, be transmuted into energy. The point I want to make is that the validity of Kelvin's calculation is in no way affected by this discovery. It still remains a correct calculation of the total gravitational energy available to the sun. Strangely enough, this old result has recently been used once more. Certain recently discovered objects in the sky, known as 'quasars' have defied explanation so far. This old result has helped in the discussion of some of the models that have been suggested to describe them.

As an example of a field of work in which some of the scaffolding is still up I should like to mention the problems that have been thrown up by space research. These range from very simple problems, like the motion of a body in a circle under an attractive force that is dealt with at Ordinary level, through more sophisticated problems like the shape of the orbit due to the fact that matter is not distributed symmetrically in the earth, the effect of air resistance (which varies with height), the ionisation of the air, due to the fact that it is heated strongly by the passage of a solid body or missile, perturbations of the orbit due to the effect of the moon and planets and so on. We have everything from the trivial to the very complicated! For different problems the answers may be required to very different accuracies. In estimating the ionisation produced by a missile, which is important because it makes the air conducting and thus enlarges its effective target area to radar, an 'order of magnitude' calculation would be helpful, and the maximum requirement might be perhaps two significant figures. If we are trying to hit Mars with a satellite, a tiny error might be equivalent to a miss by millions of miles, so that the answer required might call for tables of several different quantities, all calculated to a great many significant figures.

In a field like this, it would be quite impossible to combine the solutions of all the above problems into one all-embracing formula, and no serious mathematician would even try. Even if such a formula could be found, it would be too complicated to use, and it would have to be expressed in the form of tables. It would be ludricrous even to combine the two problems of ionisation of the air (only important when a satellite is near the earth) and that of its motion at long distances into one formula – the two problems are completely unrelated.

The ordinary theory of elliptical and hyperbolic orbits and of perturbations of them due to small departures from ideal conditions is never likely to be modified, but treatments of some of the more complicated problems are still under development, though many types of highly impressive calculations can be done now. Here again, the development of the mathematics has been influenced by all kinds of non-mathematical factors, the two most obvious ones being time-scale and cost.

As an example of a field in which nearly all the scaffolding is still up, may I mention my own special interest of statistical mechanics? This is concerned with the relationships between the properties of a piece of matter like a lump of iron, a glass of water or a lump of ice, and the interactions between individual molecules. In the laboratory one can measure magnetic and electrical properties, changes of volume under pressure and so on. In the last ten years or so, considerable progress has been made towards the fundamental objective of relating these to the properties of individual molecules. I have to report that, so far, very few of the results have been established with proper rigour, the others are therefore still open to some doubt.

Often we are in a difficult position, because we may only know the interactions between molecules fairly roughly, and we always have to make plausible approximations in order to carry a theory through to the point at which macroscopic predictions can be made. A discrepancy between theory and experiment certainly means that the theory is wrong, but agreement between theory and experiment does not always mean that the theory is right! There is always the possibility that experimental errors may combine with the effect of faulty approximations in the theory and with incorrect assumptions about the interaction between molecules and leave us with a net discrepancy that is quite small. If this sounds rather absurd, let me assure you that it really does happen at times, as I know from bitter experience! We are not discouraged, but we do recognise that, in this particular field, most of the scaffolding is still up, and that only a few of our results are rigorously established.

May I conclude by returning to my point that I do not believe that there is any real conflict of pure and applied mathematics? Looking at the history of the subject, it is surprising indeed how often the results of pure mathematics have been, as it seems 'made to order', all ready to be used in actual problems! For example, the calculus, 'the study of rates of change', developed by Newton and Leibnitz, turned out to be the ideal instrument for the study of mechanics, both of single bodies and of continuous media. This led to the founding of the three oldest branches of applied mathematics; – mechanics, hydrodynamics and elasticity. Again, the attempts to generalise Euclid's geometry were at first carried out purely abstractly, but turned out to be the ideal tools for the development of electromagnetics and relativity. The study of matrices, the general theory of operators and of simultaneous linear equations, was also carried on abstractly for over half a century before it was found, in the late 1920's, to be the ideal instrument for the development of quantum mechanics.

With such examples before us, one is more inclined to forgive those cases in which an applied mathematician, faced with an urgent problem, adapts an old method or invents a new one, sometimes without proper justification! At least this fills the gap until something better is found.