THE ROLE OF MATHEMATICS IN THE SCIENCES AND IN SOCIETY

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I should really talk about the probable developments of Mathematics in the not too distant future. During the presentation of the previous paper I greatly admired and envied Professor Spitzer who in his own field could do this; he could talk about the probable developments in Astronomy to a general scientific and scholarly audience, without getting into things, which have a strong appeal for astronomers, but do not yet have an appeal for the general public. In astronomy this is possible.

In mathematics this is very difficult. If one starts to talk about the substantive subject matter of mathematics, quite particularly when speculating about the future, one gets very quickly into things which will evoke response only among mathematicians. I will therefore orient myself differently and talk about the role of mathematics in intellectual life and in society.

Right at the beginning one has to answer a question that actually poses itself in all branches of science, and in all branches of scholarship. However, in mathematics it faces you in a particularly definite and extreme form. This is the question as to how useful mathematics is; how useful this usefulness is; how important usefulness is; whether science shall be pursued *per se* or whether it should be pursued in its relation to use in society. A great deal can be said about this subject. I think that the best that one can do in this regard in ten minutes is to point out how difficult it is, and how dangerous it is, to make snap judgments about it.

Let me quote you an epigram of the German poet Schiller. He describes a fictitious conversation between Archimedes and a disciple. The disciple expresses to the Master his admiration for science and wants to be initiated into "that

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divine science that had just saved the State," meaning the techniques which helped in the siege of Syracuse by the Romans. I mean they helped the Syracusan in a siege by a Roman army. Archimedes thereupon gives a somewhat stuffy speech in which he points out to the admirer that science *is* divine, but that she was divine *before* she helped the State; and that she is divine independently of whether she helped the State or not.

Now this position is quite important and pertinent. Science is probably not one iota more divine because she helped the State or Society. However, if one subscribes to this position, one should at the same time contemplate the dual proposition, that if science is not one iota *more* divine for helping society, maybe she isn't one iota *less* divine for harming society. The question is not at all trivial. A final point to consider in this conference is also that science is not one iota less divine, although she absolutely failed to save the State, because Syracuse was in fact taken by the Romans shortly afterwards.

So I shall talk on this question of usefulness—in spite of all the difficulties of evaluating in this context the importance of usefulness in every-day life, of usefulness to Society, without discussing where the place of mathematics in Society is, and what effects it has on us in general; and quite particularly, what effects it may have outside the group of professionals.

It is also quite interesting to consider what effects it has within the group of professionals. The effects within the group of professionals are quite different from what one might think. As far as the general and external effects are concerned, it is perfectly clear that mathematics furnishes something that is quite important, namely that it establishes certain standards of objectivity, certain standards of truth; and it is quite important that it appears to give a means to establish these standards rather independently of everything else, rather independently of emotional, rather independently of moral, questions. It is quite important to achieve this realization: That objective criteria of truth are possible, that such an aim is not self-contradictory, not in some sense inhuman.

This insight is neither obvious nor particularly ancient; and this very prestige of logic *per se*, of science *per se*, is probably connected with the role of science in our lives, and with the role of mathematics, in its completely abstract form, in science.

Again, the intrinsic truth of these propositions may even be debatable, but it is quite important that the propositions can be made at all, that one can make a precise and detailed picture of their content. This is possible, because one can form, with the help of mathematics, an image of what such a system would have to look like. In other words, quite apart from the question of whether these objective standards of truth given by mathematics are really objective, and whether or not these standards are really true, one can talk much more sense about this subject *after* one has experienced directly and *in vivo* what such a system would look like if it existed at all.

There are a number of mathematical examples to which we can refer for this purpose. How can these references be specifically implemented? Also: Even if the implementation is not immediately successful, exactly what kind of a system of ideas is it in which such extreme propositions are valid?

A great deal more can be said about this subject, and about this role of mathematics in establishing the possibility of objective standards. Let me say at once what the objections against this are. The objection, that even if absolute standards could be established by mathematics, they could not have absolute validity for the whole world, this has been discussed plenty; and I don't think that I can tell you much new about it. I think we have all faced this problem, and all have various methods to deal with it, whether we are satisfied with them or not. I want to point out, however, and this is a more technical matter, that the underlying propositions as to whether the standards of mathematics are truly objective, can also be doubted. In other words it is *not* necessarily true that the mathematical method is something absolute, which was revealed from on high, or which somehow, after we got hold of it, was evidently right and has stayed evidently right ever since. To be more precise, maybe it *was* evidently right after it was revealed, but it certainly didn't stay evidently right ever since. There have been very serious fluctuations in the professional opinion of mathematicians on what mathematical rigor is. To mention one minor thing: In my own experience, which extends over only some thirty years, it has fluctuated so considerably, that my personal and sincere conviction as to what mathematical rigor is, has changed at least twice. And this in a short time of the life of one individual! If you take the whole period, say from the beginning of the eighteenth century, there have been further serious fluctuations as to what constitutes a strict mathematical proof.

The great analyticists of the late eighteenth century accepted as mathematical proof things that we would absolutely not accept as such. It is true that they accepted these with a certain sense of guilt; but in many cases the sense of guilt was not overly evident. Also it is certainly true that in the nineteenth century there were *bona fide* disagreements as to whether a particular proof given by a very great mathematician, Riemann, was really a proof or not.

In my own experience, on two other occasions in the early twentieth century, there were very serious substantive discussions as to what the fundamental principles of mathematics are; as to whether a large chapter of mathematics is really logically binding or not. And in the nineteen-tens and -twenties a critique of these questions made it apparent, that it was not at all clear exactly what one means by absolute rigor, and specifically, whether one should limit oneself to use only those parts of mathematics which nobody questioned. Thus, remarkably enough, in a large fraction of mathematics there actually existed differences of opinion! Some mathematicians said that one need not question any part of what is in fact being used. There was also a body of opinion, that one should not use more than what the most exacting critics had approved. However, there was a further, large body of mathematicians, who felt that while there was some point in questioning certain areas of mathematics, it was all right to use them. This group was quite ready to accept something like this: Those portions of mathematics which had been questioned and which had been clearly useful, specifically for the internal use of the fraternity—in other words, when very beautiful theories could be obtained in those areas—that those were after all at least as sound as, and probably somewhat sounder than, the constructions of theoretical physics. And after all, theoretical physics was all right; so why shouldn't such an area, which had possibly even served theoretical physics, even though it did not live up to 100 per cent of the mathematical idea of rigor, why shouldn't this be a legitimate area in mathematics; and why shouldn't it be pursued? This may sound odd, as well as a bad debasement of standards, but it was believed in by a large group of people for whom I have some sympathy, for I'm one of them.

I do not want to go into the details of this critique; it is connected with the very difficult epistemological question as to whether it is legitimate to discuss collectives of entities which are not finite in number; or, if you are dealing with a collective of mathematical concepts which is infinite, exactly what it means to make a general statement about it, exactly what it means to say that you know that something is possible in such a collective. Does it mean that you have an actual example? Does it mean that you have some other methods to show that there is an example? In fact, is there any way to establish the existence of an example without exhibiting it specifically? One of the great surprises to all of us was, that it turned out that the generally accepted methods of mathematics were in fact such, that there were rather round-about tricks by which you could demonstrate the existence of, without exhibiting, an example. It is not easy to imagine how this can happen. But in fact it did happen, and it is normal mathematical practice.

So I would like to say that there are some very difficult and delicate questions here, and one cannot evade the conclusion that to some degree they are akin to those affecting the foundations of physics; that one may have a feeling of plausibility which however is tinged with convenience, and that there is no question of the absolute super-human reliability which is supposed to be one of the attributes of mathematics.

So there is a certain area of doubt there; and in evaluating the character and the role of mathematics one must not forget that doubt exists.

Let me now speak further of the functions of mathematics specifically in our thinking. It is commonplace that mathematics is an excellent school of thinking, that it conditions you to logical thinking, that after having experienced it you can somehow think more validly than otherwise. I don't know whether all these statements are true, the first one is probably least doubtful. However, I think it has a very great importance in thinking in an area which is not so precise. I feel that one of the most important contributions of mathematics to our thinking is, that it has demonstrated an enormous flexibility in the formation of concepts, a degree of flexibility to which it is very difficult to arrive in a non-mathematical mode. One sometimes finds somewhat similar situations in philosophy; but those areas of philosophy usually carry much less conviction.

This great flexibility, to which I allude, involves things like this: In normal terminology it is considered a problem, which has occupied philosophers greatly in discussing an area, whether the laws which control this area are of a following nature. Each event determines the event immediately following upon it directly. This is the *causal* approach. Alternatively, these laws might be *teleological*, which means that a single event does not determine the next event, but that somehow the whole process must be viewed as a unity, subordinate to a general law so that the whole can only be understood as a whole. If I say that this has beset the philosophers I am understating. This has played a very great role, and is still playing a very great role, for instance in biology.

Well, I don't say this is a bad question, or a meaningless question, but it is a great deal more subtle, at any rate, than it sounds; because a good deal of mathematical experience shows that unless you are awfully careful, the question has no meaning.

The classical example, the outstanding example for this, which I think deserves much more appreciation than it usually gets, is in an area between theoretical physics and mathematics, but is really mathematics, namely the mathematical treatment of classical mechanics. Classical mechanics is, of course, in theoretical physics; but once you agree to the principles of mechanics there remains the purely mathematical part of expressing these principles in mathematical terminology, and of investigating mathematically how one finds solutions, how many solutions there are, etc. Also, how one can state the same substantive principle in various mathematical forms, all of which are equivalent to each other, since they state the same thing, but which formally may look very different, and therefore give completely different technical approaches to problem-solving. These are then, generally speaking, different aspects by which one can understand the problem.

Now one of the simplest facts about mechanics is, that it can be expressed by any one of several equivalent mathematical forms. One of these is the Newtonian form where the state of the system is not only the position of every one of its parts, but also the velocity of every one of its parts at this moment. The state, thus defined, then uniquely determines the acceleration, and therefore the position and the velocity at the next moment. By repetition this can be used to derive the state of the system at any future, and in fact also at any past, moment. In other words this is strictly *causal*; if you know the system now, this determines it immediately thereafter, and by repetition also for all future times.

A second formulation of mechanics is by the principle of minimum effect, which I will not describe mathematically, but which says this: If you consider the complete history of a system (by a system I mean any mechanical entity, so it can be a planet floating in space, simplified to the extent where it is a point; or a system of a planet and a central body; or something of the complexity of the whole solar system; or of the complexity of a locomotive; or anything else you choose), if you consider its total history between two moments (it may be from now to five minutes from now, or between three billion years ago and now or any other combination of moments) then the total history permits you to calculate certain things, and specifically the integral of energy times time. And the actual history is that one which makes this quantity as small as possible. This is a clearly *teleological* principle. Indeed, here the history is not determined by anything that happens at one moment, but you must view the entire thing and minimize this particular numerical value of an integral extended over all of it.

The first approach is strictly causal, working from point to point in time. The second is strictly teleological, and defines only the total history by virtue of certain optimal properties, not any part of it. Yet the two are strictly equivalent; the actual history for movements that you derive from one is precisely that which you find from the other; and the question as to whether mechanics is causal or teleological (which in any other field would be viewed as an important substantive question calling for a yes or no answer) is manifest nonsense in mechanics, because it depends purely on how you choose to write the equations. I'm not trying to be facetious about the importance of keeping teleological principles in mind when dealing with biology; but I think one hasn't started to understand the problem of their role in biology, until one realizes that in mechanics, if you are just a little bit clever mathematically, your problem disappears and becomes meaningless. And that it is perfectly possible that if one understood another area the same might happen.

This is an insight which would probably never have been obtained without the purely mathematical trickery of transforming the equations of mechanics; it was purely mathematical skill and the flexibility characteristics of mathematical formulation and re-formulation, that produced this insight. It is not pure thinking at any abstract level, but is a specifically mathematical procedure.

Another thing that I would like to mention in this context is this. (I will again mix up theoretical physics with mathematics, in the same manner as before. The example belongs in theoretical physics, but the technical treatment which produces the results to which I refer is really mathematical manipulation. Hence it has something to do with the role of mathematics in insight, and not with the role of theoretical physics in insight, the latter being important enough, but something else than the former.) A statement which is frequently and freely made, especially before the matter was as well analyzed as it is now, is that there is some contrast between things that are subject to strict mathematical treatment, and things which are left to chance.

This is a plausible statement, and was very plausible up to about 200 years ago, at which time the theory of probability was discovered, which made possible a strictly mathematical treatment for undetermined and fortuitous events. And again it takes a mathematical treatment to realize that if an event is not determined by strict laws, but left to chance, as long as you have clearly stated what you mean by this (and it can be clearly stated) it is just as amenable to quantitative treatment as if it were rigorously defined. Of course what a quantitative treatment will tell you will not be what will happen, since this is not supposed to be possible in this particular case, but it will tell you whether that, for instance, if you try it a million times, how many times you are likely to get a positive result. Also how accurately this likelihood will be strengthened if you increase the number of tries. Also, which combinations of eventualities are those which you can disregard, which are absurd in spite of the uncertainty of the general laws.

The theory of probability furnishes an example for this, but an even more striking example of this is the modern form of quantum mechanics. It turns out that the elementary processes—the processes involving elementary particles, the atoms or possibly sub-atomic particles—are, in spite of everything known previously, apparently not subject to laws like those of mechanics, and most definitely not, because the laws of mechanics in their causal form tell you that if you know the state of the system now, you can tell exactly the state a short time afterwards, and by repeating this you can tell what it will be like at all times afterwards. It turns out that for the elementary processes it doesn't look as if it were this way. The best description one can give today, which may not be the ultimate one (the ultimate one may even revert to the causal form, although most physicists don't think this is likely) but at any rate the best we can tell today, is that you do not have complete determination, and that the state of the system now does not determine at all what it will be immediately afterwards or later. Of course, a state now may be incompatible with some further assumptions about what it will be an hour later; or some of them may be extremely improbable. But there will still be left many possibilities; and one might suspect that this is an idea which does not lend itself to description by precise mathematical means.

The fact is that this was discovered by the method of theoretical physics, and it was then crystallized, made precise, by mathematical means. In fact very sophisticated mathematical theories had to be applied; and the most peculiar things turned up. For instance: A system, like the one here referred to, is not causally predictable. You cannot calculate from its present state its state at the next moment. There is, however, something else which is causally predictable, namely the so-called wave-function. The evolution of the wave-function can be calculated from one moment to the next, but the effect of the wave-function on observed reality is only probability. That such a combination can at all be worked out, that it can decipher experience, and even be derived from experience, is something which again would have been completely impossible if the mathematical method had not existed. And again an enormous contribution of the mathematical method to the evolution of our real thinking is, that it has made such logical cycles possible, and has made them quite specific. It has made it possible to do these things in complete reliability and with complete technical smoothness.

Another thing about which we can't tell today as much as we would like, but about which we know a good deal, is that it might have been quite reasonable to expect a vicious cycle when one tries to analyze the substratum which produces science, the function of the human intelligence. The whole evidence of exploration in this area is that the system which occurs in intellectual performance, in other words in the human nervous system, can be investigated with physical and mathematical methods. Yet there is probably some kind of contradiction involved in imagining that at any one moment, an individual should be completely informed about the state of his nervous apparatus at that particular moment. The chances are that the absolute limitations which exist here can also be expressed in mathematical terms, and only in mathematical terms.

We have already had phenomena of this type. Theoretical physics has already indicated two areas in the physical world where absolute limitations to knowledge exist. One is relativity and the other is quantum theory. Here, by the best descriptions we can give today, there are absolute limitations to what is knowable. However, they can be expressed mathematically very precisely, by concepts which would be very puzzling when attempted to be expressed by any other means. Thus, both in relativity and in quantum mechanics the things which cannot be known always exist; but you have a considerable latitude in controlling which ones they are. In quantum mechanics, for instance, the statement is like this: You can never at the same time know what the position and what the velocity of an elementary particle is, but you can suit yourself as to which of these two you can find out. Any information you acquire about one, deteriorates the acquirable information about the other. This is certainly a situation of a degree of sophistication which it would be completely hopeless to develop or to handle by other than mathematical methods, or to talk sense about by other than mathematical methods; and much less to do what also has happened, namely to use it for predictions, with mathematical methods.

In coming to the evolution of mathematics I am fearful to be too specific. But I would like to make a few general remarks about it. I think the circumstances of its evolution are probably more instructive to a general scientific audience than the recital of exactly what happened; and even more than what anybody thinks is going to happen ten years from now. The circumstances of this evolution are very typical and very instructive.

Again, regarding the role of science in life or among other sciences, one thing is very conspicuous. There are large areas of mathematics which have been practically very useful. This practicality, however, is sometimes a rather indirect kind of practicality.

For instance, a mathematician usually means that a theory is directly useful if it can be used in theoretical physics. After which he still has to say that insight in theoretical physics itself is only useful if it is useful in experimental physics. After which you must say that a concept in experimental physics is, by ordinary criteria, useful if it is useful in engineering. Even after engineering you can make one more step. So all of these concepts of usefulness are rather limited, and we only mean by them, that each science should have applications outside its own area, and that there is some general direction in this sequence of applications towards practical ones for immediate social use. However, if one doesn't quibble about the definition of usefulness, and means, for instance, that by the standards of the mathematician anything is useful which is not mathematics, then one must say that large areas have been useful. Also, very large areas are really directly useful by the sum of all these criteria. Indeed, these things have really made a great difference in the world in which we live, usually somewhat indirectly, usually somewhat after the accession of some other area, but still in such a manner that the mathematical part is obviously quite vital.

Now it is very interesting that the majority of these things were developed with very little regard to usefulness, and very often without any suspicion that they might become useful later, for reasons of an entirely different character. It is a very characteristic situation. I might mention certain forms of algebra, in the field of matrices and operators, which were invented at times when there was no earthly reason to suspect that anywhere from twenty to a hundred years later they would play a role in (not yet existing) quantum mechanics.

It is equally true for the discoveries in the area of differential geometry, for which there was absolutely no reason to expect that some day there would be a theory of general relativity, and that the theory of general relativity would make use of this type of geometry. Yet these things are quite vital. The examples could be multiplied.

I must say, however, there are also examples to the contrary. One very important example is, that the calculus was certainly invented by Newton specifically for a specific purpose in theoretical physics.

But still a large part of mathematics which became useful developed with absolutely no desire to be useful, and in a situation where nobody could possibly know in what area it would become useful; and there were no general indications that it ever would be so. By and large it is uniformly true in mathematics that there is a time lapse between a mathematical discovery and the moment when it is useful; and that this lapse of time can be anything from thirty to a hundred years, in some cases even more; and that the whole system seems to function without any direction, without any reference to usefulness, and without any desire to do the things which are useful. Of course, one must also consider that this is really true for the entire course of science; in other words, that you should consider by what processes a large part of science got into the place where it impinges on society in everyday life: How most of physical science comes from mechanics, and how the original discoveries in mechanics were mainly connected with astronomy, and were absolutely not connected to the places where the applications today lie.

This is true for all of science. Successes were largely due to forgetting completely about what one ultimately wanted, or whether one wanted anything ultimately; in refusing to investigate things which profit, and in relying solely on guidance by criteria of intellectual elegance; it was by following this rule that one actually got ahead in the long run, much better than any strictly utilitarian course would have permitted.

I think that this phenomenon could be studied very well in mathematics; and I think everyone in science is in a very good position to satisfy himself as to the validity of these views. And I think it extremely instructive to watch the role of science in everyday life, and to note how in this area the principle of *laissez faire* has led to strange and wonderful results.