A PASSION FOR SCIENCE

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CHAPTER 4

THE ELECTRON AT THE END OF THE UNIVERSE

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PHYSICS is regarded as the hard edge of science. For most non-physicists the paradigm is still that of Newtonian physics, operating in a Universe whose every motion is theoretically predictable according to known laws. Yet in reality, an essential feature of modern physics is *uncertainty*. In 1927 Heisenberg deduced his celebrated uncertainty principle—that it is inherently impossible to measure exactly and simultaneously both the position and the momentum of a quantum particle. This has profound implications for the behaviour of matter at a sub-atomic level and effectively destroys the notion of a simple deterministic account of the universe. Nevertheless, outside this exotic realm of quantum mechanics, we still think of the physical world as an orderly and predictable place. It comes as something of a shock, therefore, to learn that a part of modern physics is preoccupied with the chaos of everyday situations. A major topic of study, for example, is the chaotic behaviour which occurs when a fluid pouring down a channel or pipe changes from a smooth, orderly pattern of flow to a turbulent one, characterized by swirls and eddies whose fluctuations are irregular and unpredictable. Chaos is mathematically respectable but, to the non-physicist, as difficult a concept as that of infinity. It makes one wonder whether physics is now losing that reliability and predictability which was always so reassuring.

Michael Berry, who is Professor of Physics at the University of Bristol, works on wave problems. It is a field which is concerned with many physical phenomena, ranging from the behaviour of radio waves and light to the motions of sub-atomic particles. Berry has addressed problems as diverse as the twinkling of stars and how it is that male moths can follow the trail of a female pheromone as it diffuses over long distances, perturbed by wind currents. His major tool is mathematics, which has always been the means by which physicists encode and understand the wŏrld. Newton's second law of motion, for example, which defines the force applied to an object in terms of its mass and acceleration, is expressed as $F=ma$. If known values are substituted for any two of the terms, the equation can be used to discover the third in an infinite number of situations. Michael Berry applies some of the newest ideas in mathematics to wave problems, and it was by using a type of mathematics known as catastrophe theory that he found it possible to describe the complexity of patterns made by sunlight shining down on to the bottom of the swimming pool. I had always thought that every school-child knew—and had known for several centuries—about the refraction of light, and the way it changes direction at the boundary between, say, air and water. What, I wanted to ask him, has become of physics? What has happened to all these laws and equations that once seemed to have the Universe sewn up?

In the past, a theoretical physicist who studied waves would have been conceived of as a person who found the solution to certain types of equation. The equations were well known, long known, and very simple to write down. There are certain standard mathematical representations of a wave which is travelling in empty space and which has come from a point source, or of a wave which has plane wavefronts, so that the rays of energy travel in straight lines and all the different rays are parallel to one another. Formulae of this kind tell what happens if a wave hits a diffraction grafting or passes through a lens. But all of those solutions have the property that they are possible only because of some simplifying circumstances in the physical situation—the cylindrical symmetry of a lens for example. Now it's been realized recently that in what's called the generic case—the case that has nothing special about it, and which people previously thought they could say nothing about without big computers to solve the equations—that this, actually, is the case to which a wonderful new regime applies. There are universal forms which emerge in these generic cases. An example of a waves problem which is a generic case is the refraction of sunlight by the wavy water of a swimming pool, where you see the bright lines of refracted light focused on the bottom of the pool. If you had asked an optical scientist ten years ago 'How do you explain those lines? I want to know what I actually see down there', then he could have said one of two things. Probably he would have begun by saying 'Oh! That's a rather trivial problem, it's just refraction of light, that's Snell's law, Snell sixteen hundred and something. We all understand the law of refraction'. But if you'd pressed him and said 'No, no, that's not enough, I want to know how the law of refraction gives rise to those morphologies that I see on the bottom there.' 'Ah', he would say, 'that's very difficult—you need a computer for that. But then it's trivial. With a big enough computer you can work out what those patterns are.' A computer would, indeed, provide you with a simulation, and you would, indeed, find that with quite simple patterns of waves on the water surface you would get quite realistic looking patterns of focused lines on the bottom of the pool. But still you would be missing the understanding. You wouldn't be able to answer the question 'Why is it I always see, for example, junctions of lines of this sort, and not that sort?' He wouldn't have been able to answer that. Now one *can* answer it because precisely this kind of morphology is classified by catastrophe theory. This means that one has a sort of library—a very small library—of universal forms out of which such short-wave patterns come. They are short-wave patterns because the waves of light are very, very small compared with waves of water. And this is a very intensely developing subject. Now one can really hit the heart of what actually one sees with one's eyes, which one previously was unable to do.

'Is this a new kind of physics that you're dealing with, or does it fall within conventional physics?"

I would characterize it in the following way. The image of a physicist is often of somebody who's seeking to discover the laws of Nature. Now, as it happens, it does appear that those laws are mathematical and so one speaks of trying to discover the fundamental equations for elementary particles, or fundamental fields that they satisfy, and so on. That activity's going on and, indeed, is undergoing something of a revolution now. It's a very exciting time. But what's being realized now is that concealed in the old-fashioned equations, long known, that describe matter on more familiar scales, there are new solutions, new phenomena which can only be brought out by using modern kinds of mathematics. And that's how I would characterize the feeling of excitement that is occasioned by the solution of ancient problems of this kind. One of the most difficult—it's still resisting solution, and I would characterize it as being the most important problem in theoretical physics apart from the elementary particles—is understanding the problem of fluid turbulence. Why is fluid motion so often unpredictable? That's precisely a problem of understanding the solutions of the fluid equations which were worked out by Navier and Stokes 150 years ago. So it's certainly old physics from a particle physicist's point of view, but the problem is understanding how these equations contain in them chaos. It's very easy to find solutions which aren't chaotic, but they're not the ones you observe very often. If the viscosity is small enough as it is, for example, with water and air, then one gets spontaneously developing chaotic motions. You can't, in fact, use catastrophe theory to solve that problem. It's the wrong sort of mathematics, and these are more difficult problems and they're being very very intensely worked on. But they still fit into this category of simple equations having complicated solutions. In a way that's a very satisfying answer to the problems which people who aren't scientists often bring up. They say 'Here you work with these few equations. You can write down on one sheet of paper all the equations of theoretical physics, but I see the world as a rich and complicated and beautiful place. Aren't you brutally truncating it in that way?' The answer is actually that simple equations have complicated solutions; it's a very, very compact encoding. Now one is able—with the aid of mathematics—to make very substantial steps in a whole new class of decodings.

'Chaos—that's not a concept that I would have associated with physics. My image of physics is that it gives us a highly ordered image of the world or of the universe. Does this concept of chaos imply that there's no predictability?'

Sometimes, strangely enough, it implies just that. We, as theoretical physicists, were brought up to believe that fundamental chaos only entered with quantum mechanics—you had the indeterminacy principle, and so on—and that before the advent of quantum mechanics the universe was predictable in the sense that you had for example, Newton's laws of motion, which, even as modified by Einstein, tell you that if you know the initial state of the universe—all the particles and their positions and velocities—you can predict for ever more its behaviour. We believed that. We swallowed that particular myth although it flies flagrantly in the face of anybody who has ever used a pinball machine. But in fact what's realized now is that unpredictability is very common, it's not just some special case. It's very common for dynamical systems to exhibit extreme unpredictability, in the sense that you can have perfectly definite equations, but the solutions can be unpredictable to a degree that makes it quite unreasonable to use the formal causality built into the equations as the basis for any intelligent philosophy of prediction. I give you this example. Suppose you've got lots of colliding particles. You can think of them as molecules of oxygen, let's say, in a gas and they're in a box. You believe that they obey Newtonian mechanics. They don't quite, but let's suppose they do. And you measure their initial position and velocity precisely. Of course, one couldn't do that, but suppose one can. Then one could predict their motion for all time. But wait a minute. You can only do that if the system is completely isolated and so you say, 'Isolate it as best you can according to the laws of physics as we now know them.' Well there's one force that you can't screen out, and that's gravity. So unless you know the position of every single external particle in the universe which would have a gravitational effect on your molecules, you couldn't predict the motion. So let's estimate the uncertainty that arises from this source by considering the gravitational effect of an electron at the observable limit of the universe. You agree it's not possible to think of a smaller perturbation than that.

'A single electron?'

A single electron. Just one. There it is at the observable limit of the universe, say ten thousand million light years away. It has its gravitational effect, but you don't know where it is *exactly*, so that's the uncertainty. Well, you ask, after how many collisions will the little uncertainty that's produced in a motion by that electron be amplified to the degree where you've lost all predictability, in the sense that you make an error in predicting the angle at which a particular molecule will emerge from a collision by say 90°. You could reasonably say you've lost predictability then. Well, the amazing thing is that the number of collisions is only about 50 or so, which is of course over in a tiny, tiny fraction of a microsecond. That means that it's really unreasonable in a large class of systems to consider Newtonian mechanics as being predictable. The more realistic case, actually, is if you think of the particles as billiard balls on a billiard table—an ideal, perfectly flat, perfectly smooth billiard table—and this time you're considering the uncertainties as being produced by the gravitational force of people moving about near the billiard table. There always are such people milling about. You want to know after how many collisions of the billiard balls will their motion be uncertain because of this. And the answer is six or seven, and that's why no billiard player, even the best in the world, can ever plan a shot which would have even three or four consecutive collisions and have some reasonable expectation that he'll be able to successfully carry it out.

'I always had the image of physics as an exact science. Do you think this undermines physics as an exact science?'

No, I don't think it undermines physics as an exact science because generally, when one can't predict something accurately, as in these cases, one then finds that actually one didn't really want to. It's statistical properties that one really is more interested in. But it has implications for other sciences which would seek to use physics as their model. I'm thinking particularly of economics. I don't know anything about economics but there are physicists who do, and they're now very intensely studying the possibility that the models that economists use to predict the future from the present are equations of this sort. They may be very pleased to get exact equations or accurate equations and they might think that therefore, like physicists, they can now predict the future. But it's likely to be the case that the equations are, in fact, of the unstable kind. They might find that knowing the laws doesn't enable them to predict the future. That's something which is just slowly filtering into other sciences which would seek to use mathematics. In other words, they have an outmoded paradigm of physics.

'How do you actually go about your work? Do you collaborate or do you work in isolation?'

A bit of each. Sometimes problems come rather naturally and internally as developments from what I've done before. Many of these morphological problems in waves are like that, but sometimes they arise quite randomly as a result of conversations I've had with people. The one about the male moth and the female moth arose from a conversation I had with a biologist and then very remarkably it turned out to have all sorts of links with things I'd done before. It's really a mixture of keeping my eyes and ears open and developing themes, if you like. It's themes I think of basically. Many practising theorists of my type—that is those who do a range of problems rather than one particular thing—develop a *style*. This means that they're alert to problems of particular sorts and look at them in particular ways. I'm aware that this is not at all the conventional notion of how scientists work. I tend to think of myself not so much as finding things out, although I do that, as of *making* things. I think of myself more as a carpenter than as somebody who is given a problem, solves it and then moves on to the next one.

'Could you explain what "making things" means?'

Yes, theories. The things are theories. I mean when you make a mathematical theory of some phenomenon, there's a large element of hewing it into shape. You polish a bit here and there and you chop off a bit here and there and then you think 'Well there's a bit here that needs refining' and so on. The resulting production has two aspects. It has an internal aspect, which is its coherence, whether one's pleased with its structure and so on, and the external aspect, which is its relation to the problem that stimulated it and to which, in the happiest cases, it can give something back in the sense of predicting something or explaining something. There's very much this element of making this sort of aesthetic and creative judgement which I guess isn't terribly widely appreciated.

'Now when you talk about hewing, are you hewing from mathematics in the first instance? Is it a mathematical chopping and changing, finding the right bit of mathematics to fit the phenomena that you're interested in?'

Yes, but it's worth saying that this is a very different kind of mathematics than the mathematics which mathematicians create. They're creating theorems. I think of those as raw materials. I use them to create theories.

That's really quite different, and there's a considerable difference, sometimes almost incompatibility, between these two modes of thought. I'm very much *not* a mathematician—I haven't studied mathematics in a formal way. I've taught myself, so much of what I write would, I guess, be quite horrific to some mathematicians. A mathematician is concerned with the logical rigour of every step, but as the physicist Richard Feynman said 'A great many more things are known than can be proved.' And while a physicist wants to be right, he doesn't want rigour to turn into rigor mortis. And if you're trying to create some rather elaborate edifice, and calling on lots of branches of mathematics, you just can't afford to be perfectly rigorous with every step. It's as though, for example, a printer would, in printing a book, have to insist that every letter was absolutely perfectly formed, that there wasn't a shadow of a smudge anywhere. You'd never get beyond the first line if you did that. That's really what I mean.

'So it's understanding via mathematics?'

Exactly so. Modelling and understanding.

'And you'll take any bit of mathematics from anywhere and use it in a way that will give you insight into your physical phenomena?'

That's right, and it's here that there's been quite a change in my and many other physicists' attitude to mathematics in the last decade. We took the view previously that modern mathematics was certainly logically very exciting and interesting and worthwhile under the terms of mathematicians' work, but as far as we physicists are concerned we knew all the mathematics we needed to know and all this modern stuff was of no assistance to us. Well I've changed my view totally on that. I now take the opposite view, that there's no piece of worthwhile mathematics that has been—or will be—invented which cannot and will not some day be of use in describing some aspect of the universe. I'm forced to this view because I've found it to be so. I've found the most surprising pieces of mathematics—topology and number theory, catastrophe theory and so on—to be just what I've needed for particular physical problems. That's the practical reason why I'm forced to this view. But even theoretically, when you think about it, it's fairly obvious. After all the whole universe, which it's the object of science to describe, is more complicated than the inside of one's head and that's where mathematics comes from. That's my rationalization for this change of view that I've had over the last few years.

'You see yourself as a theoretical physicist. What's your relationship to the experimental physicist?'

Very close, of course. Physics describes the real world. It isn't a sort of low level mathematics, which it would become if one lost contact. It's very important to always realize that there are phenomena, that there is a world outside our heads that we're trying to explain. Otherwise it's a curious game, a form of self indulgence which I think is intellectually not very worthwhile.

'But when do you interact with other physicists? When you're hewing your ideas, is that a very personal, isolated process?'

Again, this depends. I do sometimes work with other theoretical physicists, my research students, for example, and other colleagues. Indeed it's a valuable part of a research student's training. Any theoretical physicist has to learn how to get a theory into an aesthetically worthwhile shape. But mainly I work by myself.

'You keep talking about the aesthetics. Can you explain what you mean when you speak about the "elegance" of the theory?'

Very difficult. Taste is a difficult concept to define and it's something that's appreciated by people who know it and not by people who don't. I don't want to sound terribly aristocratic, I certainly don't feel that way. You can only explain it by analogy. A piece of music, let's say, can be ill-constructed or well-constructed, and you can after a while hear—learn to hear—the difference. It's like that with theories. I wouldn't like to give the impression that there could be no objective way of assessing the aesthetic value of a physical theory, but I don't know one.

'Is it something like when you hear a good bit of music, you get a nice feeling down your spine?'

Oh very much so. That happens, but that's part of the aesthetics. I don't call that objective. I mean it's objective in the sense that the tingling really happens and it's a psychically and, presumably, neurally real phenomenon. That's not what I meant. I meant that you could not communicate it to somebody who doesn't share the feeling. I don't think that's possible.

'To what extent, then, would you describe your choice of problems and your solutions to them as partly an exercise in self-indulgence? Doing things that please you enormously?'

When I said before that certain other kinds of activity were self-indulgent, I was implicitly defending myself against that charge on the grounds that theoretical physics of the kind that I and my colleagues do has some connection, however distant, with the real world. But there is, of course, a large element of play and that's very necessary to the successful development of theories. If you keep very narrowly in mind a particular practical goal, for example, you often won't solve the problem, at least at the level that we're speaking about. Some problems have been solved by this goal-oriented approach, but not the problems I've been speaking about. Of course one could say that that's only an excuse, and the fact that one's playing means that it's a form of self-indulgence. In defence against that I would only say that by that same criterion so is poetry, music, writing, and philosophy, and so has been much of previous science. I don't want to be dishonest and claim that the reason why I'm doing theoretical physics is because its productions are useful in industry. Occasionally they are, even the kinds of things that I do, but I don't want to claim that, because that would dishonestly represent my motivations.

'But when you say "play", do you mean having fun? What do you mean by play?'

Play means exploring every possible suggestion of an analogy or an allusion to some other part of mathematics. If one starts by solving one problem and suddenly finds an interesting by-way opening up, which looks as though it would be more fruitful and more rewarding, one isn't afraid to go along it. One does remember the original problem and come back to it, but one can spend a year sometimes going off and then finding that this unexpected pathway, this unexpected fork, takes us along a path which is very fruitful and surprising.

'Does it matter to you terribly whether what you do does have practical implications? Is that important to you?'

It's very pleasing when, as has happened a few times, something I've done has been useful. As I said, it would be dishonest of me to claim that that's my motivation for doing it. It isn't. But it's very nice when people from industry come and talk to me, and I'm always very happy to discuss the details of their problems and sometimes give minor technical help, such as solving equations and so on. But what is a much more difficult problem is the question of research which is used badly in my terminology. I'm thinking particularly of military research. There one always has to reckon with the possibility that what one does, or what one's colleagues do—which one is associated with as being a member of the whole enterprise—will be used in good and bad ways that one can't foresee. There's been a lot written about that, and a lot discussed on the question of responsibility of scientists for what they do, and my position is the following. In a complicated society such as ours, which is an interactive society, everyone's actions have effects which he or she can't foresee. I'm thinking, for example, of a man who makes steel in one of the steel works. He's certain that a fraction of the steel he makes, which, if he's mathematically minded, he can work out, will be used for purposes which he might not approve of. The steel will be used for making guns, knives and the like, and that's a mathematical certainty. Philosophically it's exactly the same as what we physicists do. We create something and then it becomes public property. What we make is part of our culture. We're making cultural artefacts, and we can have no control over what they do, after they've left us. But of course, that leads to the question of drawing lines and, in principle, it is impossible to draw a line which covers all circumstances. Since I do think that the application of science, especially of recent physics, to military purposes is a monstrous perversion of its aims and ideals, my personal line is not to do any work knowingly for the military or for immediate military ends. That's where I draw the line, but other people can draw lines differently.

'But it's very hard to predict what's going to happen in the future, so the same must be true in relation to the application of one's research work. For example, your work on how moths trail the scent of the female in wind currents obviously has military applications in connection with the spread of biological or deleterious agents. Doesn't almost anything one does have the chance of becoming used for, as it were, evil purposes?'

Yes, you're exactly right, and indeed the very example that you mention is well chosen. One of the papers I had to read in connection with this biological problem of the diffusion of pheromones was written in conjunction with somebody from Porton Down, and the interest that they have is obviously in what happens after the explosion of a chemical weapon. The distribution of death—and let's not put it any lesser way—the distribution of death will be governed by exactly this kind of mathematics. On the other hand, the distribution of pheromones is very important to the people interested in pest control who try to fool male moths with false scents and then catch them, and thereby save the crops. It's an immensely difficult problem. I think in every case, in every type of research that one does, one can rather easily and without too much imagination, find a military application. Every year the US Department of Defense publishes a list of aspects of fundamental physics on which it would like to see further research done. There isn't much left out. Modern warfare uses the whole range of science. In the battlefields, the oceans, the atmosphere, space, the deserts, the forest, everywhere you can think of there is an application for some kind of physics or another. I go back to what I said before, which was that if one wished to live a life which didn't in some way aid what I would call the forces of darkness, then I don't think one could do it in this society at all.

'That has the implication that you would not support the argument that has been used recently in relation to biology, that there are certain sorts of research that one shouldn't do in principle; that certain things should remain unknowable because of the danger of applying the knowledge.'

You're right. That isn't the view that I take. It's a very difficult question and one can clearly foresee such terrible things. Civilization is an uncertain enterprise and one of its uncertainties, which is also a tremendous adventure, is this intellectual journey into the unknown. I'm sorry to sound so melodramatic but that's what it is, and I think it won't work to place a moratorium on whole areas of research in this way. Although the mechanism would be extremely difficult, I could see and, indeed, would like to examine, the possibility that certain types of *applications* of research would be proscribed. Indeed, certain types of bacteriological warfare research were for a long time voluntarily not conducted by government, so it is possible. But I think one doesn't achieve that end by stopping whole areas of research.

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