1 Introduction

1.1 Pedagogical and Real Physics

This book is for students who love physics and theoretical physics. It arises from a dichotomy which I believe pervades attempts to teach the ideal course in physics. On the one hand, there is the way in which physics and theoretical physics is presented in lecture courses and examples classes. On the other hand, there is the way we actually practise the discipline as professional physicists. In my experience, there is often little relation between these activities, which is a great misfortune.

There are, of course, good reasons why the standard lecture course has evolved into its present form. Physics and theoretical physics are not particularly easy subjects and it is important to set out the fundamentals and their applications in as clear and systematic a manner as possible. It is absolutely essential that students acquire a firm grounding in the basic techniques and concepts of physics. But we should not confuse this process with that of doing real physics. Standard lecture courses in physics and theoretical physics are basically 'five-finger' exercises, designed to develop technique and understanding. But such exercises are very different from performing Beethoven's *Hammerklavier* sonata at the Royal Festival Hall. You are only really doing physics and theoretical physics when the answers *really* matter – when your reputation as a scientist hangs upon being able to reason correctly in a research context or, in more practical terms, when your ability to undertake original research determines whether you are employable, or whether your research grant is renewed. These are quite different processes from working through drill exercises, with answers provided at the back of the book.

The major problem is that there is just so much material which lecturers feel they have to include in their courses that physics syllabuses become seriously overloaded. There is little time to sit back and ask 'What is this all about?' Indeed, the technical aspects of the subject, which are themselves truly fascinating, can become so totally absorbing that it is generally left to the students to find out for themselves many essential truths about physics.

It is important to stress at the outset that this book is *not* a textbook on physics and theoretical physics. There is *no substitute* for the systematic development of these topics through standard courses in physics and mathematics. This book should be regarded as a supplement to the standard courses, but one which I hope may enhance your understanding, appreciation and enjoyment of physics.

This book aims to redress the balance between pedagogical and real physics through seven case studies which span much of classical physics and the beginnings of quantum physics.[1] The subjects of the case studies are as follows:

I The origins of Newton's laws of motion and of gravity.
II Maxwell's equations for the electromagnetic field.
III Mechanics and dynamics – linear and non-linear.
IV Thermodynamics and statistical physics.
V The origins of the concept of quanta.
VI Special and general relativity.
VII Cosmology.

These topics have a familiar ring, but they are treated from a rather different perspective as compared with the standard textbooks – hence the subtitle of the book *'An Alternative View of Theoretical Reasoning in Physics*. It is not just the content of the physics which I am aiming to explore, but also the leaps of imagination involved in some of the greatest discoveries in physics and theoretical physics.

At the same time, we can gain important insights into how real physics and theoretical physics are carried out. These convey some of the excitement and intense intellectual struggle involved in achieving new levels of understanding. In a number of the case studies, we will retrace the processes of discovery which were followed by the scientists themselves, using only mathematical techniques and concepts available at the time. This has an added benefit in that many of the problems which students have in understanding physics are the same as those which challenged many of the greatest physicists.

1.2 Reflections on What Every Student Should Know

Let me list some of the lessons which I hope readers will take away from the book, based on my experience of practising and teaching physics for many years.

(i) It is only too easy to lose a *global view* of the physics and theoretical physics. Professionals use the whole of physics in tackling problems and there is no artificial distinction between thermal physics, optics, mechanics, electromagnetism, quantum mechanics, and so on. A corollary of this is that in physics any problem can normally be tackled and solved in a variety of different ways. *There is often no single 'best way' of solving a problem*. Much deeper insights into how physics works are obtained if a problem is approached from very different perspectives, from thermodynamics, from electromagnetism, from quantum mechanics, and so on.

(ii) How problems are tackled and how one thinks about physics are highly personal matters. No two physicists think in exactly the same way although, when they come to work out a problem, they should come up with the same answer. The *individual physicist's response to the discipline* is an integral part of the way physics is practised to a much greater extent than students or the lecturers themselves would often like to believe. But it is the diversity of approaches to physics which provides insight into the nature of the mental processes by which physicists understand their subject. I remember vividly a splendid lecture by my colleague Douglas Gough, summarising a colloquium in Vienna entitled *Inside the Stars* in which he concluded with the following wonderful paragraph.

> I believe that one should never approach a new scientific problem with an unbiased mind. Without prior knowledge of the answer, how is one to know whether one has obtained the right result? But with prior knowledge, on the other hand, one can usually correct one's observations or one's theory until the outcome is correct. . . . However, there are rare occasions on which, no matter how hard one tries, one cannot arrive at the correct result. Once one has exhausted all possibilities for error, one is finally forced to abandon a prejudice, and redefine what one means by 'correct'. So painful is the experience that one does not forget it. That subsequent replacing of the old prejudice by a new one is what constitutes a gain in real knowledge. And that is what we, as scientists, continually pursue.[2]

Douglas's dictum is the foundation of the process of discovery in research. All of us have different prejudices and personal opinions about what the solutions to problems might be and it is this diversity of approach which leads to new understandings.

(iii) It is often difficult to convey the *sheer excitement of the processes of research and discovery in physics*. Most of us spend many more hours pursuing our research than would be expected in any normal 'job'. The caricature of the 'mad' scientist is not wholly a myth in that, in carrying out frontier research, it is almost essential to become totally absorbed in the problems to the exclusion of the cares of normal life. The biographies of many of the greatest scientists illustrate their extraordinary powers of concentration. The examples of Newton and Faraday spring immediately to mind as physicists who, once embarked upon a fertile seam of research, would work unrelentingly until the inspiration was exhausted. All of us have experience of this total intellectual commitment at much more modest levels of achievement and it is only later that, on reflection, we regard these as among our best research experiences.

(iv) Key factors in these historical examples, which are familiar to all professional physicists, are the central roles of *hard work*, *experience* and, perhaps most important of all, *intuition*. Many of the most successful physicists depend very heavily upon their wide experience of a great deal of hard work in physics and theoretical physics. It would be marvellous if this experience could be taught, but I am convinced in fact it is something which can only be gained by dedicated hard work and perseverance. We all remember our mistakes and the blind alleys we have entered, and these teach us just as much about physics as our successes. I regard intuition as a distillation of all our experience as physicists. It is potentially a dangerous tool because one can make some very bad blunders by relying too much on it in frontier areas of physics. Yet it is certainly the source of many of the greatest discoveries in physics.

(v) Perhaps most important of all is the essential element of *creativity* in coming to new understandings of the laws of nature. In my view, this is not so different from creativity in the arts. The leaps of the imagination involved in discovering, say, Newton's laws of motion, Maxwell's equations, relativity and quantum mechanics are not so different in essence from the creations of the greatest artists, musicians, writers and so on. The basic difference is that physicists must be creative within a very strict set of rules and that their theories should be testable by confrontation with experiment and observation. In my view, the imagination and creativity involved in the very best experimental and theoretical physics result in a real sense of *beauty*. The great achievements of physics evoke in me, at least, the same type of response that one finds with great works of art. I think it is important to let students know when I find a piece of physics particularly beautiful – and there are many examples of these in this book. When I teach these topics, I experience the same process of rediscovery as on listening to a familiar piece of classical music – one's umpteenth rehearing of the *Eroica* symphony or of the *Sacre du Printemps*.

1.3 The Nature of Physics and Theoretical Physics

The natural sciences aim to provide a logical and systematic account of natural phenomena and to enable us to predict from our past experience to new circumstances. *Theory* is the formal basis for such endeavours. Theory need not be mathematical, but mathematics is the most powerful and general method of reasoning we possess. Therefore, we attempt to secure *data* wherever possible in a form that can be analysed *mathematically*. There are two immediate consequences for theory in physics.

The basis of all physics and theoretical physics is *experimental data* and the necessity that these data be in *quantified form*. Some would like to believe that the whole of theoretical physics could be produced by pure reason, but they are doomed to failure from the outset. As Volker Heine has remarked,

> No one starting with Schrödinger's equation would have predicted superconductivity.[3]

The great achievements of theoretical physics have been solidly based upon the achievements of experimental physics, which provides powerful constraints upon physical theory. Theoretical physicists should therefore have a good and sympathetic understanding of the methods of experimental physics, not only so that theory can be confronted with experiment in a meaningful way, but also so that new experiments can be proposed which are realisable and which can discriminate between rival theories.

A second consequence is, as stated earlier, that we must have adequate *mathematical tools* with which to tackle the problems we need to solve. Historically, the mathematics and the experiments have not always been in step. Sometimes the mathematics is available but the experimental methods needed to test the theory are unavailable. In other cases, the opposite has been true – new mathematical tools have to be developed to describe the results of experiment.

Mathematics is central to reasoning in physics but we should beware of treating it, in any sense, as the whole physical content of theory. Let me reproduce some words from the reminiscences of Paul Dirac about his attitude to mathematics and theoretical physics. Dirac sought mathematical beauty in all his work. For example, he writes:

> Of all the physicists I met, I think Schrödinger was the one that I felt to be most closely similar to myself. . . . I believe the reason for this is that Schrödinger and I both had a very strong appreciation of mathematical beauty and this dominated all our work. It was a sort of act of faith with us that any equations which describe fundamental laws of Nature must have great mathematical beauty in them. It was a very profitable religion to hold and can be considered as the basis of much of our success.[4]

Earlier, however, he had written the following:

> I completed my (undergraduate) course in engineering and I would like to try to explain the effect of this engineering training on me. Previously, I was interested only in exact equations. It seemed to me that if one worked with approximations there was an intolerable ugliness in one's work and I very much wanted to preserve mathematical beauty. Well, the engineering training which I received did teach me to tolerate approximations and I was able to see that even theories based upon approximations could have a considerable amount of beauty in them.
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> There was this whole change of outlook and also another which was perhaps brought on by the theory of relativity. I had started off believing that there were some exact laws of Nature and that all we had to do was to work out the consequences of these exact laws. Typical of these were Newton's laws of motion. Now we learned that Newton's laws of motion were not exact, only approximations and I began to infer that maybe all the laws of nature were only approximations . . .
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> I think that if I had not had this engineering training, I should not have had any success with the kind of work I did later on because it was really necessary to get away from the point of view that one should only deal with exact equations and that one should deal only with results which could be deduced logically from known exact laws which one accepted, in which one had implicit faith. Engineers were concerned only in getting equations which were useful for describing nature. They did not very much mind how the equations were obtained. . . .
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> And that led me of course to the view that this outlook was really the best outlook to have. We wanted a description of nature. We wanted the equations which would describe nature and the best we could hope for was usually approximate equations and we would have to reconcile ourselves to an absence of strict logic.[5]

These are important and profound sentiments which should be familiar to the reader. There is really no strictly logical way in which we can formulate theory – we are continually approximating and using experiment to keep us on the right track. Note that Dirac was describing theoretical physics at its very highest level – concepts such as Newton's laws of motion, special and general relativity, Schrödinger's equation and the Dirac equation are the *very summits of achievement of theoretical physics* and very few of us can work creatively at that level. The same sentiments apply, however, in their various ways to all aspects of research as soon as we attempt to model quantitatively the natural world.

Most of us are concerned with applying and testing known laws to physical situations in which their application has not previously been possible, or foreseen, and we often have to make numerous approximations to make the problems tractable. The essence of our training as physicists is to develop confidence in our physical understanding of physics so that, when we are faced with a completely new problem, we can use our experience and intuition to recognise the most fruitful ways forward.

1.4 Environmental Influences

It is important to realise not only that all physicists are individuals with their own approaches and prejudices, but also that these are influenced by the tradition within which they have studied physics. I have had experience of working in a number of different countries, particularly in the USA and the former Soviet Union, and the different scientific traditions can be appreciated vividly by the approach of the physicists to research problems. These experiences have added very significantly to my understanding and appreciation of physics.

An example of a distinctively British feature of physics is the tradition of *model building*, to which we will return on numerous occasions. Model building was an especially British trait during the nineteenth and early twentieth centuries. The works of Faraday and Maxwell are full of models, as we will see, and at the beginning of the twentieth century, the variety of models for atoms was quite bewildering. J.J. Thomson's 'plum-pudding' model of the atom is perhaps one of the more vivid examples, but it is just the tip of the iceberg. Thomson was quite straightforward about the importance of model building:

> The question as to which particular method of illustration the student should adopt is for many purposes of secondary importance provided that he does adopt one.[6]

Thomson's assertion is splendidly illustrated by Heilbron's *Lectures on the History of Atomic Physics 1900-1920*.[7] Heilbron gives this splendid example of Fitzgerald's approach to the structure of the aether:

> [FitzGerald had] in mind mechanical models, that is, detailed representations of physical phenomena, especially light and electromagnetism, in terms of the motions and interactions of hypothetical particles or media . . . The representations were not meant to be taken literally. To quote Fitzgerald:
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> To suppose that (the electromagnetic) aether is at all like the model I am about to describe (which is made from tennis balls and rubber bands) would be almost as bad a mistake as to suppose a sphere at all like $x^2 + y^2 + z^2 = r^2$ and to think that it must, in consequence, be made of paper and ink.

This approach is very different from the continental European tradition of theoretical physics – we find Poincaré remarking 'The first time a French reader opens Maxwell's book, a feeling of discomfort, and often even of distrust, is at first mingled with his admiration . . . '.[8] According to Hertz, Kirchhoff was heard to remark that he found it painful to see atoms and their vibrations wilfully stuck in the middle of a theoretical discussion.[9] It was reported to me after a lecture in Paris that one of the senior professors had commented that my presentation had not been 'sufficiently Cartesian'. I believe the British tradition of model building is alive and well. I can certainly vouch for the fact that, when I think about some topic in physics or astrophysics, I generally have some picture, or model, in my mind rather than an abstract or mathematical idea.

In my view, the development of *physical insight* is an integral part of the model-building tradition. The ability to guess correctly what will happen in a new physical situation without having to write down all the mathematics is a very useful talent and most of us develop it with time. It must be emphasised, however, that having physical insight is no substitute for producing exact mathematical answers. If you want to claim to be a theoretical physicist, you must be able to give the rigorous mathematical solution as well.

The influence of our environment applies equally to different physics departments, as well as to different countries. If we consider the term *theoretical physics*, there is a wide range of opinion as to what constitutes 'theoretical physics' as opposed to 'physics'. It is a fact that in the Cavendish Laboratory in Cambridge, most of the lecture courses are strongly theoretically biassed. By this I mean that these courses aim to provide students with a solid foundation in basic theory and its development and relatively less attention is paid to matters of experimental technique. If experiments are alluded to, the emphasis is generally upon the results, rather than the experimental ingenuity by which the experimental physicists came to their answers. We expect students to acquire most of their experimental training through practical experiments. The situation is in strong contrast to the nature of the Cambridge physics courses in the early decades of the twentieth century, which were very strongly experimental in emphasis.[10]

In contrast, members of Departments of Theoretical Physics or Applied Mathematics would claim rightly that they teach much more 'theoretical' theoretical physics than we do. In their undergraduate teaching, I believe this is the case. There is by definition a strong mathematical bias in the teaching of these departments. They are often much more concerned about rigour in their use of mathematics than we are. In other physics departments, the bias is often towards experiment rather than theory. I find it amusing that some members of the Laboratory who are considered to be 'experimentalists' within the department are regarded as 'theorists' by other physics departments in the UK.

The reason for discussing this issue of the local environment is that it can produce a somewhat biased view of what we mean by physics and theoretical physics. My own perspective is that physics and theoretical physics are part of a continuum of approaches to physical understanding – they are different ways of looking at the same body of material. In my view, there are great advantages in developing mathematical models in the context of the experiments, or at least in an environment where day-to-day contact occurs naturally with those involved in the experiments.

1.5 Final Disclaimer

Let me emphasise at the outset that I am *not* a historian or philosopher of science. I use the history of science very much for my own purposes, which are to illuminate my own experience of how real physicists think and behave. The use of historical case studies is simply a device for conveying something of the reality and excitement of physics. I therefore apologise unreservedly to real historians and philosophers of science for using the fruits of their labours in attaining my goals. My hope is that students will gain enhanced appreciation and respect for the works of professional historians and philosophers of science from what they read in this book.

Establishing the history by which scientific discoveries were made is a hazardous and difficult business and, even in the recent past, it is often difficult to disentangle what really happened. In my background reading, I have relied heavily upon standard biographies and histories. For me, they provide vivid pictures of how science is actually carried out and I can relate them to my own research experience.

My ambition is that all advanced undergraduates in physics should be able to profit from this book, whether or not they are planning to become professional theoretical physicists. Although physics can be carried out without a deep understanding of theory, that point of view misses so much of the beauty and stimulation of the subject. Remember, however, the case of Stark, who made it a point of principle to reject almost all theories on which his colleagues had reached a consensus. Contrary to their view, he showed that spectral lines could be split by an electric field, the Stark effect, for which he won the Nobel prize.

I particularly want to convey a real appreciation of the great discoveries of physics and theoretical physics. These achievements are as great as any in the field of human endeavour.

Notes

1 Those who want to go further and find out how the concept of quanta led to the theory of quantum mechanics may enjoy my ‘sequel’ to this book, *Quantum Concepts in Physics: An Alternative Approach to the Understanding of Quantum Mechanics*, Cambridge: Cambridge University Press (2013). A similar approach to the present book is taken, but now analysing the turbulent years from 1900 to 1930.

2 Gough, D.O. (1993). In *Inside the Stars*, eds. W.W. Weiss and A. Baglin, IAU Colloquium No. 137, p. 775. San Francisco: Astronomical Society of the Pacific Conference Series (Vol. 40).

3 Personal communication.

4 Dirac, P.A.M.(1977). *History of Twentieth Century Physics*, Proceedings of the International School of Physics ‘Enrico Fermi’, Course 57, p. 136. New York and London: Academic Press.

5 Dirac, P.A.M. (1977). *op. cit.*, p. 112.

6 Thomson, J.J. (1893). *Notes on Recent Researches in Electricity and Magnetism*, vi. Oxford: Clarendon Press. (Quoted by J.L. Heilbron in note 7, p. 42.)

7 Heilbron, J.L. (1977). *History of Twentieth Century Physics*, Proceedings International School of Physics ‘Enrico Fermi’, Course 57, p. 40. New York and London: Academic Press.

8 Duhem, P. (1991 reprint). *The Aim and Structure of Physical Theory*, p. 85. Princeton: Princeton University Press.

9 Heilbron, J.L. (1977). *op. cit.*, p. 43.

10 For many more details, see my book *Maxwell’s Enduring Legacy: A Scientific History of the Cavendish Laboratory*, Cambridge: Cambridge University Press (2016).