Preface

Physics appears to be the only source of fundamental knowledge about the natural world. No other system of thought or methodology has shown any of its systematic explanatory or predictive power. This success has been achieved by a continued attempt at minimalism and reductionism, and it appears that the greatest success has been achieved from the simplest possible foundations. Yet, because we have been obliged to approach the subject by an inductive method, working back from complicated observations to simple explanations, we have still to discover the ultimate foundations on which this whole conceptual scheme has been based. We know that the ultimate theory must be simple, probably extremely simple, but, because it must also be unique, we have no precedent which would help us to make the discovery. Yet the belief that the discovery is possible remains, and has led to many approaches towards a 'unified theory of physics' or a 'theory of everything', none of which seems to be close to success.

Obviously, no one expects to succeed instantly with a theory that will simply explain everything. What we would hope to do is to find a *process*, a systematic way of proceeding with strong indications that we were on the right track. This is what is being aimed at in this book. Positions that are rejected from the outset in the search include model-dependent theories of any kind; the aim of the work is resolutely abstract. One of the particular approaches avoided is the restructuring of particle physics in terms of multidimensional space-time strings or membranes. We can, of course, do this as a mathematical *representation*, and the procedure for doing so is sketched out in chapters 4, 15 and 18, but a string theory would not be a unified theory, even if we should chance to find out the 'correct' one from the many thousands of possible alternatives. A unified theory has to explain the *concept* of dimension, as well as the number (why 10? why 11?); it also has to explain space and time, their similarities and differences, and even the use of mathematics to explain physics. But there are also intrinsic difficulties with the approach which explain why it does not offer a true unity.

Even the first stage of combining space and time in the simplest way, by adding the single time dimension to space's three in special relativity, causes us problems when we look at quantum mechanics, a theory which appears to be an essential starting-point for all foundational work in physics. Time, unlike space, is simply not an observable in quantum mechanics, though the two quantities are assumed to have identical status in the relativistic 'space-time' concept. When we make the space-time part of an even-more complicated structure, as in general relativity, where 'curvature' is used to eliminate mass or gravity, the problems multiply, and we find singularities, nonlinearity, unrenormalizable infinities, and the violation of fundamental physical laws. We solve some problems, but create others.

Again, we must reject the idea that a single cosmic creation event has structured the laws of physics in a particular way, and that they could have been different in different circumstances. The idea could, in principle, be true, but then we would have no abstract subject of physics, no generality, no absolute mathematics, and no meaningful concept of conservation, the process which makes physics universal. The very idea that we could discover a *unified* theory of physics is impossible in such a context. Physics is fractured in the very act of creation. In addition, such explanations have the habit of becoming self-fulfilling prophecies. We simply refer difficulties to special conditions that occurred in the 'early universe', and deprive ourselves of understanding fundamental physical phenomena which ought to be valid at all places in all epochs. This does not, of course, mean that we cannot discover historical evolution over time for structures such as stars and galaxies, and galactic clusters. What it does mean is that physics, if it is to be a truly unified subject, should not be determined by cosmic history, whether or not this turns out to be true. The laws of physics cannot be the result of an accident.

Even the very successful approach to physics using symmetry groups, as employed extensively, for example, in particle physics, should be treated with caution. It is assumed that, if we find a group structure which accommodates all four known physical interactions, then we will have solved the problem of their relationship, and we will have a 'Grand Unified' theory of particle interactions. Of course, such a result would be a very significant step, and the idea is discussed in some detail in chapter 15, but we would not have solved the problem from a fundamental point of view unless we could explain *why* we have this particular group structure, and, indeed, why we have a group structure at all. Obviously, we have to proceed in understanding nature by stages, but a group structure will never be an end stage, nor will *any* structure. As long as anything complicated remains unexplained in our theory, we will not be able to describe it as a theory of *everything*.

So it is far from obvious how we would construct such a theory, but there is one important clue as to where we should start. One fundamental idea, and one only, has the necessary simplicity and intrinsic inexplicability to be the foundation for everything else. This is *nothing*, zero in mathematical terms. We could imagine creating a theory of everything if it was also a theory of nothing. The question is: can it be done? Can we start from zero, and use it to structure nature as we understand it today? The proposition would seem to be impossible, but, in fact, it is not, and it is the aim of the present research programme to justify this statement.

It would probably be impossible to do this by purely logical development from first principles, though, in a sense, mathematics attempts such an approach. Mathematics certainly provides a very powerful formalism, of which physics makes extensive use, but its own logical foundations, as Gödel proved, remain insecurely based on a seemingly empirical process of counting. Computing provides another alternative, and Wolfram and others have seen the development of complexity from simplicity in systems governed by cellular automata;[1] with the further assumption that the 'right' complex structures will somehow finally emerge, but, again, the empirical counting process is assumed, along with the idea that only discrete concepts matter. Why discreteness is to be privileged and what discreteness actually *is* remain unexplained. Physics allows us a very different route to the foundations, through the application of inductive methodology to masses of empirical data, and a ruthless Darwinian selection of the only formalisms which work, and it is in seeing what mathematical structures are essential to physics *at its very foundations* that we see what structures are also essential to mathematics and computing. By finding the common origin of mathematics, physics and computing and the way they deal with zero totality, using the dual processes of induction and deduction, we can finally track down the route through which 'everything' finally comes from nothing.

The structure of the book reflects this process. The first chapter develops a computing analogy to see how a zero totality can be used to create a *universal rewrite system*, which then allows us to structure mathematics without first assuming the number system or discreteness. This exercise leads us to a very definite mathematical structure, *with zero conceptual totality*, which we can then work towards in a deductive context. The next chapter is inductive, and takes physics as far as we can towards its ultimate foundations by analysing the most fundamental concepts that we are capable of imagining in a physical context. The procedures used in the mathematical structure can then be seen to correspond with the ones we have derived by induction as the basic components of physics. Further analysis of the physical context then shows us, in chapter 3, that the most convenient packaging of the mathematical structure is the one that provides the shortest route to zero totality, at the same time as presenting us with the fundamental equation that drives the whole of physics. Most of the remaining chapters then present the working out of the consequences of chapters 1, 2 and 3 in all the detail necessary to show that the structure is sufficient to generate the results which are considered foundational to physics, even to the point of numerical detail. Chapters 19 and 20, however, stand apart in showing that the kind of information processing structures that make physics and mathematics 'spontaneously' emerge in nature also apply (in a fractal sense) to biological and other large-scale systems. Only by creating the most efficient information processing possible could these large-scale systems be created against the natural tendency to disorder or increased entropy, and it is difficult to imagine that any information processing could be more efficient than the one produced by Nature's own rewrite code.

If this process is true to any considerable degree, then it will be of significance to everyone, scientist and nonscientist alike, with or without mathematical training. So, the book has been written in such a way that there are long sections of conceptual argument, which should appeal to the general reader as well as the professional scientist. However, there is no disguising the fact that mathematics lies at the heart of this book, and that credible results, in many areas, can only be achieved by using the full mathematical formalisms. So, these are also given in full detail where required. The idea has been to develop all the ideas from as foundational a position as possible, but the presentation concentrates generally on results which may be considered original in some respect, and only makes use of established work where it is absolutely necessary to the argument. Since the mathematics of quaternions and multivariate vectors is essential to the argument, an appendix is included at the end of the first chapter giving an elementary treatment of these algebras.

Despite the obvious novelty of the fundamental position, and some of the specific formalisms employed, most of the results generated certainly support 'orthodox' or 'mainstream' science, where existing work is available for comparison. Of course, there are new results and predictions, but there is no challenge here to the bases of quantum mechanics, classical physics, particle physics, or anything else now universally accepted. New ideas are certainly put forward in areas which are still highly speculative, such as certain aspects of cosmology, and some of the *physical* interpretations (for example, in relation to the emergent nature of fractional charges in quarks, or the gravitational-inertial explanation of general relativity) differ from the usual (though not exclusively accepted) ones, while retaining the overall mathematical formalisms which really define the theories. However, nothing here proposes deviations from the experimental evidence as now understood, though some new predicted results are available for testing, and some have already been confirmed since they were first predicted. In addition, many speculative concepts now in the literature would be ruled out by the analysis, while a few might be vindicated. A complete reading of the book should indicate that every position adopted is founded on the results incorporated in chapters 1, 2 and 3. The theoretical position is put forward as an organic whole, and every statement within it, in a sense, reinforces every other. Important links are shown through a system of cross-referencing.

To aid the reader, there is a synopsis of the contents of the chapters at the beginning of each, and a summary of the entire argument at the end. These can be used to get a general idea of the argument where details prove troublesome or appear to require too much specialised knowledge. As with all presentations of novel results, readers will have to make up their own minds about the thesis being proposed, but the book is intended to contain the minimum of 'speculation', in the ordinary sense of that word. Only a few of the mass calculations and the section at the end dealing with the derivation of the cosmic background radiation are consciously put forward as speculative proposals, and, in the latter case, it could be argued that Ockham's razor ought to favour an argument that leaves fewer unexplained facts than any known alternative. Many parts of the book (chapters 5, 6, 10 and 11, especially) are the working out of the consequences of new formalisms with the appropriate degree of mathematical rigour, the physical consequences following on directly from the mathematics, while the ideas on algebra and rewrite alphabets in chapters 1 and 3 leave plenty of scope for further development in the direction of practical application. Elsewhere, it is hoped that the sheer simplicity of the basic ideas, and their apparent ability to explain a great number of seemingly diverse facts, will recommend them to the reader's attention.

The project has a long history. I can write here in detail only of my own trajectory; those of my colleagues would, of course, be different and would have different emphases. The germ of several significant ideas began with student speculations.[2] The essential philosophy was developed between the last years at school and the first years at university. The group symmetry of space, time, mass and charge was in place by the mid-1970s, along with the first particle physics ideas based on charge structures, and some of the gravitational and cosmological ideas outlined in chapters 18 and 21. The first publications came at the end of the 1970s and the beginning of the 1980s, but success in 'respected' publication outlets was a long time coming.

Lee Smolin has described how the 'philosophical way of doing theoretical physics' of the 1920s 'gradually lost out to a more pragmatic, hard-nosed style of research', which was 'completed when the center of gravity of physics moved to the United States in the 1940s'.[3] By the 1970s, at the exact moment when the first ideas in this book were being developed, 'the transition was complete'. Smolin reports that: 'As a graduate student, I was told by my teachers that it was impossible to make a career working on problems in the foundations of physics. My mentors pointed out that there were no interesting new experiments in that area, whereas particle physics was driven by a continuous stream of new experimental discoveries.' The experiments, of course, led to the establishment of the Standard Model of particle physics, around 1973, but, since that time, no really new unifying principle seems to have been discovered, and the abandonment of research into the foundations of the subject has made such a discovery increasingly unlikely. The remarkable thing is that the pattern that has set in over the last half century or so has made many physicists seemingly unable to conceive of the *concept* of researching the foundations. Work of this kind seems to create bafflement in many and downright hostility in others.

Serious publication outlets were certainly minimal during the 1980s, but the situation improved towards the end of the decade when the PIRT series of conferences (Physical Interpretations of Relativity) were started in London, by Michael Duffy. I first attended in 1990, and, as a result of these meetings (which are also now held in Moscow, Calcutta and Budapest), I came into contact with the Vigier conferences (Toronto-Berkeley-Paris), organised from 1995 by Geoffrey Hunter, Stanley Jeffers and Richard Amoroso, the ANPA conferences at Cambridge, organised by Keith Bowden and Arleta Ford, which I attended from 1998, and the CASYS conferences at Liège, organised by Daniel Dubois, which I first attended in 2003. All these meetings, in their different ways, have been concerned with the foundations of the subject, and with tackling important questions in a freely inquiring spirit; and it is largely through contacts made through these and related events that I first met the collaborators, who are named on the title page of this book. During these years, also, I published three books, summarising my work from the 1980s, and consolidated the new view of relativistic quantum mechanics I had been developing as a result of my theories of symmetry. I managed to publish the first paper on this topic in 1994, and set about relating the particle physics consequences with my earlier work on this subject.

John Cullerne was my first real collaborator, and worked with me for many hours, principally during 1997-2000, on Dirac algebra and the derivation of the Standard Model and other aspects of particle physics. (See **5**, **6**, **14**, **15**.) Our intense weekly discussions were always a source of great and mutual intellectual stimulation. After this time, John's other commitments took hold and our collaboration became less intense, although it still continues on an occasional basis. In particular, John has acted as adviser on parts of chapters **10**-**13**, the orthodox making a potent combination with the unorthodox. A new departure was the universal rewrite system, which was the result of my collaboration with my computer science colleague, Bernard Diaz, from about 1997 (see **1**, **3**, **20, Appendix B**). Because of its extremely fundamental nature, this has proved a difficult area in which to work, one needing endless examination and re-examination of the concepts. Brian Koberlein, whom I met through ANPA, worked with me intensely for a few days in Cambridge, and then via email, on groups and dual systems, and, separately, on the comparison of the nilpotent and idempotent versions of quantum mechanics (see **4**, **7**, **15**). Presentations by Brian also stimulated the work which appears in **18.9**.

Peter Marcer (see **20**) was another ANPA contact and we have now had a wide-ranging collaboration for many years, on many subjects, beginning as informal discussion, and continuing under the auspices of the British Computer Society's Cybernetics Machine Group and the CASYS conferences in Liège. Through Peter, I have also had a fruitful interaction with Edgar Mitchell and Walter Schempp, our co-authors on the ground-breaking paper, 'Zenergy' (see **20**). Finally, through a London frontiers meeting, organized by Simon Daniel, as a result of an earlier Vigier meeting in Paris, I met the biologist Vanessa Hill, and we soon realised that we had a potentially powerful collaboration on applying algebraic and geometric concepts in biology (see **19**). Both of these collaborations (as recorded in chapters **19** and **20**) are developing rapidly and expanding into areas that we had not previously connected with the project.

Apart from these formal collaborations, I have had stimulating discussions and contacts with many other researchers, including Ruggero Santilli, Erik Trell, Stein Johansen, Jeremy Dunning-Davies, Clive Kilmister, Ted Bastin, Lou Kauffman, Dan Kurth, Mark Curtis, Sarah Bell, Cynthia Whitney, John Spencer, John Valentine, Mark Stuckey, Jose Almeida, Otto van Nieuwnehuijze, Tolga Yarman, Tuomo Suntola, Sergey Siparov, Vladimir Gladyshev and Tatyana Gladysheva.[4] Besides these there are a huge number of people to whom I and my collaborators are indebted. In particular, there are the organizers and participants of PIRT, Vigier, ANPA and CASYS, for many stimulating presentations and discussions; the Swansea / Bristol / Keele group (Viv and Mary Pope, Alan Winfield, Anthony Osborne); my colleagues David Edwards, Mike Houlden, Dominic Dickson, John Fry and Christos Touramanis, for their support and interest over many years. Mike, in particular, has been an endless source of new problems for me to challenge, and his advice and encouragement has been without parallel. Apart from my collaborators, he is the person of all to whom I am most indebted. The British Computer Society have been generous in their financial support for the Cybernetics Machine Group's activities; and I have been a beneficiary on several occasions, along with Peter and Vanessa. I am also grateful for funding to Dmitri Pavlov, and to the British Council, as well as to the University's Physics and Computer Departments.

Peter Rowlands
Oliver Lodge Laboratory
University of Liverpool