On Space and Time

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'Absolute, true, and mathematical time, of itself, and from its own nature, flows equably without relation to anything external'

Isaac Newton

'Already the distance-concept is logically arbitrary; there need be no things that correspond to it, even approximately'

Albert Einstein

'The future of our space and time / Is not gonna wither and die / The future of our space and time / Is not gonna say good-bye'

Vanessa Paradis/Lenny Kravitz

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Andrew Taylor is Professor of Astrophysics at the University of Edinburgh. He has made many major contributions to the field of cosmology, especially imaging the distribution of dark matter, studying the nature of dark energy and investigating the initial conditions of the Universe. He lives in Edinburgh with his wife and son.

Preface

What can more than two thousand years of human thought and several hundred years of hard science tell us finally about the true nature of space and time? This is the question that the philosopher Jeremy Butterfield and I posed to a unique panel of top mathematicians, physicists and theologians in a public discussion that took place at Emmanuel College, Cambridge in September 2006, and this is the book that grew out of that event. All four other panellists, myself and the astronomer Andy Taylor who spoke at a related workshop, now present our personal but passionately held insights in rather more depth.

The first thing that can be said is we do not honestly know the true nature of space and time. Therefore this book is not about selling a particular theory but rather it provides six refreshingly diverse points of view from which the reader can see that the topic is very much as alive today as it was at the time of St Augustine or of Newton or of Einstein. Our goal is not only to expose to the modern public revolutionary ideas at the cutting edge of theoretical physics, mathematics and astronomy but to expose that there is a debate to be had in the first place and at all levels, including a wider human context. Moreover, the reader will find here essays from leading figures unconstrained by peer pressure, fashion or dogma. These are views formed not today or yesterday but in each case over a scientific lifetime of study. So what I think this volume offers is six particularly coherent visions of lasting value at a time when serious thought on this topic is in a state of flux.

My own view as editor, and I think this comes across in all of the essays, is that right now is an enormously exciting juncture for fundamental science. There is real adventure to be had at a time in which pure mathematics, theoretical physics, astronomy, philosophy and experiment are all coming together in a manner unseen for almost a hundred years. You probably have to go back still further to the seventeenth-century Scientific Revolution to get a full sense of what I mean, to the era of 'natural philosophy', Copernicus, Galileo, Newton. I do sometimes hear some of my physicist colleagues lamenting the old days when physics was young, when there was not a vast mountain of theory or technique that a young researcher had to climb before even starting out. What I hope the essays in this volume will convey is a sense that actually *we are at the birth of a new such era right now*.

To explain this further I need first to explain where physics has been 'stuck' for so long. The sticking point – which is also the reason that no physicist can honestly say at the moment that they truly understand space and time – is what I call in my own essay 'a hole in the heart of science' and which the other essays refer to variously as 'the Planck scale' or 'the problem of quantum gravity'. Perhaps many readers will know that Einstein in his 1915 'General Theory of Relativity' provided what is still the current framework for how we think about gravity, as a curved spacetime. It is a theory that governs the large scale and among its subsequent solutions was the remarkable Big-Bang model of an expanding Universe which, with variations, is still used today. Einstein's earlier work also had some input into a theory of quantum mechanics which emerged in 1923, a theory which he, however, never fully accepted. Quantum mechanics evolved in the 1960s to its modern form 'quantum field theory' and this is our current best understanding of physics at the small scale of subatomic particles – quarks, electrons, neutrinos, and so forth – the resulting Standard Model well describes such matter *in* space and time as well as all fundamental forces other than gravity. But what still eludes us since those days is that these two parts of physics do not even now form a single self-consistent theory that could if it was known be called 'quantum gravity'. Without a theory of quantum gravity a theoretical physicist cannot pretend to 'truly' understand space and time. Without a true understanding we cannot, for example, say with confidence what happens at the very centre of a black hole (and modern thinking is that many galaxies have a huge black hole in their core) and nor can we say anything with certainty about the origin of the Universe, the very start of the 'Big Bang'. Let me stress, it is not only that we do not know, *we do not even have a theory to test* about this deepest layer of physics.

How then can a research scientist get up in the morning and go about their business if we do not understand such basic notions as space and time? The answer lies in two aspects of the way that science is done, something which nonscientists do not always realise. The first is that every bit of scientific knowledge generally has a domain of validity attached to it and in which it is supposed to hold. So you generally say something precise and accurate but under certain approximations where the things that you do not understand are estimated to be negligible. This is how we can have today what is a vast and marvellous body of accumulated knowledge accurate to many decimal places even if we do not understand the things that are most basic. Secondly, even if you want to explore the domain where all you really know is that you don't really know, or even if you just want to push the boundaries a little, you do have to put forth concrete ideas, postulates or specific exploratory models if you are going to get anywhere, sometimes even knowing full well that the model is not quite right but just hoping to get a bit of insight. This is the *theoretical* side of physics, that is to say you put forth bold or less bold hypotheses and see where they get you. In this case science, particularly at the fundamental end, does not have the certainty that the image of scientists in white coats often elicits in the public. It is more like exploring empty blackness armed with nothing more than a flashlight and a measuring tape.

Now, whereas Einstein was not content to 'give up' on the problem and spent the last years of his life looking for such a unified theory, the mainstream physics establishment for the most part *did* give up in a certain sense. I do not mean that physicists stopped talking about quantum gravity but in many ways it became the elephant in the room that one perhaps pays lip-service to and then goes away and solves something more tractable. Either that or it became a play area for often interesting but ultimately wild and somewhat random speculation slightly outside the mainstream and certainly divorced from the cardinal rule of serious physics – testability. I do not mean either that young physicists coming up through the system did not and do not dream of finding a theory of quantum gravity, most of us cherish this dream from childhood and something like that does not go away so easily. But the conventional wisdom until very recently was that we had *neither* deep enough ideas for a theory *nor* the hope of ever being able to test a theory if we had one. A double whammy! For example, many physicists will admit over a beer or a glass of wine that part of the problem is that spacetime is probably not a continuum, but they have no mathematical alternative, so they carry on building their theory on a continuum assumption. I include here string theory, which seeks to encode particles and forces as a quantum theory of small bits of string – but still moving in a continuum. On the other problem, even a few years ago it was inconceivable that quantum gravity could ever be tested in the laboratory in the foreseeable future. This was a simple matter of back-of-envelope estimates that gravity is so weak a force that its effects at a subatomic scale are absolutely tiny. I recall even three years ago a government grant application being angrily rejected because it implied that a certain quantum-gravity experiment *could* be done, as this would clearly be a waste of resources. Now I think this has all fed in recent decades into a certain malaise. It is a malaise that occurs when a long-standing problem is never really addressed and when most people believe that it is not even worth sincerely to try. I am not saying that nothing worthwhile has been done in the last two decades, far from it, rather my point is to analyse why the problem of an actual theory of quantum gravity has been so intractable and I think it is for the two very good reasons mentioned. And perhaps it could be said that in such a climate mainstream theoretical physics has in recent years lost a certain freshness of purpose, or if you like what has been acquired in the last two decades is a certain tiredness.

As editor of this volume I will be very happy if this reminder that *we do not really know* comes across more openly. This is an antidote to what I feel in recent decades has been a tendency in the media to give just the opposite impression. When one of my colleagues goes on the radio and provides an authoritative soundbite that spacetime is, for example, a 10-dimensional continuum (of which we are somehow constrained to live in 4) they give such an opposite impression. The correct statement is not that a version of string theory predicts that spacetime is a 10-dimensional continuum but that string theory *presumes* as a starting point that spacetime is a continuum of some dimension n in which extended objects called 'strings' move. String theory then turns out not to work in the desired case $n = 4$ due to some technical anomalies but one can fix these by taking $n = 10$, say. This 'fix' of course opens up much worse conceptual and technical problems about explaining away the unobserved extra dimensions and why this particular fix and not some other. What is lost to the public here is a sense of perspective, that this or that theory is just that, a theory that should be tested or if that is not possible then at least weighed for its explanatory power against its complexity and ad-hocness of presumptions. I should say that I have used the example of string theory here only because it has been so much in the media in recent years, the same criteria should be applied elsewhere in theoretical physics.

Now let me say why I think the conventional wisdom above about the unreachability of quantum gravity was unnecessarily pessimistic, and what else has quietly been going on behind the scenes. In fact both of the fundamental problems mentioned are to do with a dearth of imagination. First of all on the experimental side astronomers and Earth-based experimental physicists have made fantastic strides in recent years. We now have a convincing picture of the large-scale structure of the Universe from an observational side that throws up real puzzles that lack even one convincing idea for a theoretical explanation. These include issues such as the nature of dark matter and dark energy or as some would say 'the problem of the cosmological constant'. We also have new experimental technology such as very long baseline laser interferometry that will allow us for the first time to see the far and hence early Universe through gravitational waves and to test out radical new ideas for its origin. At the moment there are such detectors as LIGO and VIRGO based on Earth and another in development, LISA, to be based in space. Such technology could also be adapted to test for various quantum-gravity effects. Data from active galactic nuclei, from gamma-ray bursts hopefully collected by NASA's GLAST satellite to be launched in the near future, neutrino oscillations, all of these theoretically could now test different ideas for quantum gravity. So for the first time in the history of mankind we *can* actually put theories of quantum gravity to some kind of test. Some of this is fortunate happenstance but much of it is lateral thinking about how the tiniest quantum-gravity effects could in some cases 'amplify' to a measurable level. And all of this is *in addition* to certain long-standing puzzles such as why some fundamental particles have mass while others are massless. It was recently discovered that enigmatic particles called neutrinos do in fact have a small but nonzero mass, which was a surprise. In fact the Standard Model of particle physics has some two dozen free parameters and we have little idea why they take the values that they do, why elementary particles seem to be organised into mysterious repeating families, and so forth. Secondly, certain long-simmering but radical mathematical tools and concepts outside mainstream physics have also recently reached a critical level of maturity. One of them is quite simply abstract algebra which two decades ago was of no real interest to physicists but which is now accepted as a rich vein of structure that can replace continuum geometry. My own field has played a role here. Another is twistor theory in which geometry is built not on points but on light rays. I will say more about these topics later in this preface. So we live in an era where our schoolchildren should know that our most fundamental concepts are up for grabs, but that there are real theoretical and conceptual puzzles and real experimental data coming online that can test new and creative ideas, that the Universe is a totally fascinating, mysterious and yet scientifically knowable place.

Up to a point. It still seems unlikely that current scientific considerations alone can in fact provide the final answer, but perhaps they are elements of some kind of emerging new renaissance exactly centred on our understanding of space and time. What is already clear is that the true problems of quantum gravity also require deep philosophical input about the nature of quantum measurement, the nature of time. I think they force us to think about what physical reality itself is more generally. And I do not see this as a one-way street in which scientists only inform the public. Scientists' ideas have to come from somewhere too, from sitting in cafés, from art, from life. Let us also not forget that many of our great universities have their roots in the middle ages founded out of theological institutions. The 1347 statues of my own former Cambridge college, Pembroke, as recorded in 1377, precisely list the main topics of study as grammar, logic and natural philosophy at the BA level, philosophy and some mathematics at the MA level, and these so-called 'arts' and theology at the more senior level. Later on science largely won out as the fount of physical knowledge but if science is now short of ideas on the deepest issues we should not rule out a thoughtful wider dialogue.

And so this is how this volume is put together. Each of the authors brings their own unique expertise to bear on the problem. Andrew Taylor's work on gravitational lensing has helped establish the large-scale structure of the Universe. He is also part of a consortium with recent breakthrough results on mapping out dark matter which is a key unknown at the moment. His essay provides a full account of this current experimental picture of the Universe at a cosmological scale and some of Taylor's personal insights into the weak spots and fundamental problems. The Fields medallist Alain Connes is perhaps at the other end of the spectrum, a pure mathematician of the highest level who has seen deeply into the nature of geometry itself and into how it could be reinvented in terms of operator algebras. He is an acknowledged pioneer and world leader of this new field of noncommutative geometry. In his essay he shows how his 'spectral' vision of geometry elegantly encodes the plethora of elementary particles found by physicists as a finite extra bit of such geometry tagged onto ordinary spacetime. It provides a new geometrical way of thinking about matter itself. Remarkably, his formulation also predicts relations between the masses of certain elementary particles as well as an estimate of the Higgs particle mass, all of which could be tested in principle. At the end of his essay Connes also speculates on connections between quantum gravity and number theory. My own essay comes to a kind of noncommutative geometry, but down a slightly different road, namely from my work on 'quantum symmetry' at the pure end of mathematical physics. Such considerations lead to models of 'quantum spacetime' where exact points in space and time fundamentally do not exist due to quantum 'fuzziness'. Moreover, gamma-ray bursts and a modification of technology such as LISA could in principle test the theory (predictions include a variation of the speed of light). If quantum spacetime was ever observed experimentally it would be a new fundamental effect in physics. This vision and that of Connes have common ground and a synthesis is certainly possible. In the later part of my essay I speculate on deeper philosophical ideas about the self-dual nature of physical reality in which quantum symmetries naturally fit. A very different vision appears in the essay of Sir Roger Penrose, inventor among other things of 'twistor theory' in the 1970s. This says that light rays and not points should be the fundamental ingredients in a true understanding of spacetime. Due to time dilation a photon of light does not itself experience time but exists simultaneously from its creation to its destruction. An important part of twistor theory is the idea of conformal invariance and this can be used for the 'conformal compactification' of spacetime (or the method of Penrose diagrams as they have been called). Penrose uses this to present a radical new proposal for what happened *before* the Big-Bang origin of the Universe. In this theory if you look back far enough you should see remnant information from a previous universe, the infinite future of which is the origin of ours (and similarly our infinite future is the origin of a next universe). Although it is hard at the moment to quantify, gravitational wave detectors such as LISA could play a role in looking for such effects. Observational input such as the cosmic microwave background also plays a role in the arguments, while most profound is perhaps an explanation of how such a cyclic (endlessly repeating) vision of cosmology is compatible with the thermodynamic arrow of time in which entropy always increases. Penrose argues here that one must include the entropy of the gravitational field to balance the accounts. His essay also outlines some of his other ideas about gravity, quantum theory and causality, exposed further in his several books. The volume is rounded off by the essays of Michael Heller, philosopher, theologian and cosmologist at the Vatican observatory, noted for discussions on scientific matters with Pope John Paul II, and of John Polkinghorne, former Cambridge Professor of Physics and a distinguished Anglican theologian. Heller's article particularly explores the philosophical and theological consequences of the quantum nonlocality suggested by our emerging understanding of quantum gravity. Conversely he explains how many of the notions of Western science have their deeper roots in history and that it would be better to be aware of these past influences than to leave them hidden and unquestioned. Polkinghorne in his short essay focusses specifically on the nature of time and contrasts the atemporal 'block' concept in which we look down on time from the outside, which was also Einstein's view, and the need for a different doctrine of 'temporal becoming' that really expresses how time unfolds.

Finally, let me say that both my own and the essay of the astronomer Andy Taylor have pedagogical sections where some of the background material needed in other essays is explained at a less technical level. Hence the reader could consider starting with some sections of these essays. By contrast, the visions of Connes and Penrose are probably the most profound but will also require more work from the reader fully to grasp. The essay of Heller could be read directly but with backward reference for some of the science, while the essay of Polkinghorne is both a very accessible epilogue to the entire volume and an introduction to his own two recent books. In all the works, because we are talking about fundamental physics, the Universe, etc., there will be references to vast or to vastly small numbers. The kinds of numbers involved are necessarily mind-boggling even for a physicist and we will have to use scientific notation for them. Here a large number is expressed in terms of 1 followed by so many zeros. For example an American billion is 10^9 meaning 1 000 000 000 (1 followed by nine zeros). Similarly 10^{-9} means one American billionth, i.e. start with 1.0 and then move the decimal place 9 spots to the *left* to obtain 0.000 000 001. In terms of physical units, cm means centimetres, g means grams, s means seconds and K means degrees Kelvin (degrees centigrade + 273.15). Other units will generally be explained where needed. We will generally state physical quantities only to a level of accuracy sufficient for the discussion.

The cover is from Corpus Christi College, Cambridge, 'New Court'. Warm thanks to Ian Fleming for doing the photography for this, and to the college for access. The background is galactic cluster CL0024+17 overlaid by a fog representing the apparent distribution of dark matter, courtesy of NASA, ESA, M. J. Jee and H. Ford *et al.* at Johns Hopkins University.

Shahn Majid, Cambridge 2007