

The State of Physics: ‘Halfway through the Woods’¹

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Abstract

I first celebrate the immense success of twentieth century physics, but then urge that the future may bring many surprises, even in the basic structures of physical theories.

Being the philosopher of physics among the panelists of the Springer Forum for the International Quantum Structures Association (IQSA), I believe I can best contribute by taking a broad view of the foundations of physics. So I shall celebrate the achievements of twentieth century physics, and then emphasise that physics remains open to conceptual upheaval—hence the significance of foundational studies as pursued by IQSA.

1 The Achievements of Twentieth Century Physics

It is a commonplace remark that the twentieth century saw two fundamental revolutions in physics—relativity theory and quantum theory. But nowadays, physicists, for whom these theories have become a daily tool, can easily lose their sense of wonder at these theories’ immense empirical success. So I propose, first, to emphasise how contingent, indeed surprising, it is that the basic postulates of relativity and quantum theory have proved to be so successful in domains of application far beyond their original ones.

Examples are legion. I pick out two examples, almost at random. Why should the new chronogeometry introduced by Einstein’s special relativity in 1905 for electromagnetism, be extendible to mechanics, thermodynamics and other fields of physics? And why should the quantum theory, devised for systems of atomic dimensions (10^{-8}cm) be good both for scales much smaller (cf. the nuclear radius of ca. 10^{-12}cm) and vastly larger (cf. superconductivity and superfluidity, involving scales up to 10^{-1}cm)? Indeed, much of the history of twentieth century physics is the story of the consolidation of the relativity and quantum revolutions: the story of their basic postulates being successfully applied ever more widely.

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The point applies equally well when we look beyond terrestrial physics. I have in mind, first, general relativity. It makes a wonderful story: the theory was created principally by one person, motivated by conceptual, in part genuinely philosophical, considerations—yet it has proved experimentally accurate in all kinds of astronomical situations. They range from weak gravitational fields such as occur in the solar system—here it famously explains the minuscule precession of the perihelion of Mercury (43” of arc per century) that was unaccounted for by Newtonian theory; to fields 10,000 times stronger in a distant binary pulsar—which in the last 20 years has given us compelling evidence for a phenomenon (gravitational radiation) that was predicted by general relativity and long searched for. But general relativity is not the only case. Quantum theory has also been extraordinarily successful in application to astronomy: the obvious example is the use of nuclear physics to develop a very accurate and detailed theory of stellar structure and evolution.

Indeed, there is a more general point here, going beyond the successes of relativity and quantum theory. Namely: we tend to get used to the various unities in nature that science reveals—and thereby to forget how contingent and surprising they are. Of course, this is not just a tendency of our own era. For example, nineteenth century physics confirmed Newton’s law of gravitation to apply outside the solar system, and discovered terrestrial elements to exist in the stars (by spectroscopy): discoveries that were briefly surprising, but soon taken for granted, incorporated into the educated person’s ‘common sense’. Similarly nowadays: the many and varied successes of physics in the last few decades, in modelling very accurately phenomena that are vastly distant in space and time, and/or very different from our usual ‘lab scales’ (in their characteristic values of such quantities as energy, temperature, or pressure etc.), reveal an amazing unity in nature. General theoretical examples of such unity, examples that span some 200 years, are: the ubiquitous fruitfulness of the field concept; and more specifically, of least action principles. For a modern, specific (and literally spectacular) example, consider the precision and detail of our models of supernovae; as confirmed by the wonderful capacity of modern telescope technology to see and analyse individual supernovae, even in other galaxies.

2 Clouds on the Horizon

And yet: complacency, let alone triumphalism, is not in order! Not only is physics full of unfinished business: that is always true in human enquiry. Also, there are clouds on the horizon that may prove as great a threat to the continued success of twentieth century physics, as were the anomalies confronting classical physics at the end of the nineteenth century. Of course, people differ in what problems they find worrisome. I myself find the various mysteries of interpreting quantum theory worrisome, and so I set great store by studies in foundations of quantum theory as pursued by IQSA. These studies are crucial to better understanding quantum theory—and they may lead to important new physics. But here I propose to leave this topic to other panelists, and instead to describe two other ‘clouds’: clouds which we in the foundations community tend not to focus on (though the first, at least, is recognized in the wider physics community).

First, general relativity and quantum theory are yet to be reconciled. More specifically:

while we have developed successful quantum theories of the other fundamental forces of nature (electromagnetic, weak and strong), we have no analogously successful quantum theory of gravity. Accordingly, finding such a reconciliation, perhaps unification, has become an outstanding goal of theoretical physics.

There are of course conceptual reasons why this goal is so elusive. The contrasting conceptual structures of the ‘ingredient’ theories and the ongoing controversies about interpreting them, make for conflicting basic approaches to quantum gravity. (For a review of these issues, see Isham (1997).)

But I want here to emphasise another reason why we do not yet have a successful theory, despite much effort and ingenuity: namely, a dire lack of experimental data! For there are general reasons to expect data characteristic of quantum gravity to arise only in a regime of energies so high (correspondingly, distances and times so short) as to be completely inaccessible to us. To put the point in terms of length: the value of the Planck length which we expect to be characteristic of quantum gravity is around 10^{-33} cm. This is truly minuscule: the diameters of an atom, nucleus, proton and quark are, respectively, about 10^{-8} , 10^{-12} , 10^{-13} , and 10^{-16} cm. So the Planck length is as many orders of magnitude from the (upper limit for) the diameter of a quark, as that diameter is from our familiar scale of a centimetre!

The successes of relativity and quantum theory, celebrated in Section 1, bear on this lack of data. That is: these successes work against us! For they suggest that we will not see any ‘new physics’ intimating quantum gravity even at the highest energies accessible to us. The obvious example is quasars: these are typically a few light-days in diameter, and yet have a luminosity 1000 times that of our galaxy (itself 100,000 light-years across, containing a hundred billion stars). They are the most energetic, distant (and hence past!) celestial objects that we observe: they are now believed to be fuelled by massive black holes in their cores. Yet suggestions, current 30 years ago, that their stupendous energies and other properties that we *can* observe, could only be explained by fundamentally new physics, have nowadays given way to acceptance that ‘conventional physics’ describing events *outside* the black hole’s event-horizon can do so. (Agreed, we expect the physics deep inside the black hole, in the ‘vicinity of its singularity’ to exhibit quantum gravity effects: but if ever a region deserved the name ‘inaccessible’, this is surely one!)

I turn to my second ‘cloud on the horizon’: namely, the cluster of problems surrounding the relation of quantum theory to *special* relativity. I admit that the physics community at large does not worry so much about this cluster (unlike the first ‘cloud’). Here, the successes of relativity and quantum theory work against us in a different sense from that noted above: their successes, especially when harnessed together in relativistic quantum field theories, make us forget that problems linger.

Again, people differ in what problems they find worrisome. Amongst philosophers, the best-known problem concerns the ‘collapse of the wave-packet’: all will agree that *if* this collapse is a real physical process, then we will need some account of how the process meshes with relativity. As to myself, I give some credence to the antecedent, and so I find this worrisome. But I also want to register disquiet at two more technical difficulties of relativistic quantum field theories. These difficulties are widely recognized ‘in principle’,

as probably arising from these theories' use of a continuum model for spacetime (and so, as related to my first 'cloud')—but for the most part, they are not worked on, nor *worried about*. The first difficulty is the need to subtract away infinities, even for the free field. The second is the fact that so far as we can tell (thanks to the efforts of the constructive quantum field theory school), no physically relevant interacting quantum field theory in four dimensions *rigorously* exists.

To complete this 'snapshot' of the present state of physics, I would like to endorse an analogy of Rovelli's (1997). He suggests that our present situation is like that of the mechanical philosophers such as Galileo and Kepler of the early seventeenth century. Just as they struggled with the clues given by Copernicus and Brahe, *en route* to the synthesis given by Newton, so also we are 'halfway through the woods'. Of course we should be wary of too grossly simplifying and periodizing the scientific revolution, and *a fortiori* of facile analogies between different historical situations. Nevertheless, it is striking what a 'mixed bag' the doctrines of figures such as Galileo and Kepler turn out to have been, from the perspective of the later synthesis. For all their genius, they appear to us (endowed with the anachronistic benefits of hindsight), to have been 'transitional figures'. One cannot help speculating that to some future reader of twentieth century physics, enlightened by some future synthesis of general relativity and quantum theory, the efforts of the last few decades in quantum gravity will seem strange: worthy and sensible from the authors' perspective (one hopes), but a hodge-podge of insight and error from the reader's!³

3 References

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