MICHAEL REDHEAD

Michael Logan Gonne Redhead

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elected Fellow of the British Academy 1991

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Michael Redhead FBA was the most distinguished and influential British philosopher of physics of the second half of the 20th century. After a degree in physics (1950) and some fifteen years running his family's business, he undertook a doctorate in physics (completed 1970) and thereafter became a philosopher of science, especially physics. He rose rapidly through the academic ranks at the University of London, and was Professor at University of Cambridge from 1987 to 1997, when he returned to the London School of Economics. Through his writings, his teaching and his academic leadership, he was the pre-eminent influence, from about 1980 onwards, in establishing the philosophy of physics as a discipline in the United Kingdom.

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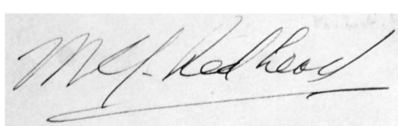

I. The life

Michael Logan Gonne Redhead was born in London on 30 December 1929. Following education at Westminster School, Redhead studied Physics at University College London, winning academic prizes and graduating with first class honours in 1950. He then pursued a career in the family property business, but in 1953 he published two papers on quantum field theory in the *Proceedings of the Royal Society A*. It seems that in the two decades following his BA at UCL, Redhead continued his self-education not only in physics but also philosophy and history of physics. Encouraged by his young wife Jennifer, Redhead returned to academia part-time in 1968 to write a PhD dissertation at UCL on the quantum theory of electron-electron and electron-positron scattering under the supervision of Leonardo Castillejo, building on his 1953 publications. The thesis was completed in 1970.

Redhead's subsequent rise in the academic world of philosophy was meteoric. In 1974, at age 44, he appeared unannounced in the Department of History and Philosophy of Science at Chelsea College London. At the time the Department was arguably the leading centre for philosophy of physics in the UK, with its Head of Department Professor Heinz Post and his colleague Jon Dorling being the prominent players in this field. (David Lavis, in the Department of Mathematics at Chelsea, also played a role.) In philosophy of science and logic, the department also boasted Donald Gillies and Moshe Machover, respectively. Redhead apparently impressed Post with his rapid solutions of various problems Post posed to him, and he became a regular presence in the Department. There followed a period of remarkable research output and astonishing rise in the academic ranks. He went from Honorary to Visiting Lecturer in the Department from 1978 to 1979, and was appointed to a Lectureship in 1981 and Senior Lectureship in 1983, having been elected Fellow of the Institute of Physics in 1982. In 1984 he was promoted to Professor of Philosophy of Physics. This achievement occurred within just a decade after setting foot in the Department as a visitor with no formal credentials in philosophy of physics. As early as 1977 he was widely regarded as one of the leading figures in the field in the UK. This was the year that he was invited to lecture and give tutorials for the Honour School of Physics and Philosophy at Oxford University, which he continued to do until 1983, commuting from his London home.

In 1985 the Department at Chelsea was absorbed into the corresponding Department at King's College London. A special Chair in the Philosophy of Physics was created for him at King's, and Redhead became Head of Department. In 1987, he left London for Cambridge, having been elected to the Chair in the Department of History and Philosophy of Science. The Department had grown up since the 1950s, teaching history and philosophy of science as a minor option within the Cambridge Natural Sciences Tripos. In view of the growth and success of the subject, and of the department, from the 1960s to the 1980s, the University agreed that on the retirement in 1987 of Mary Hesse FBA, who had been promoted to an *ad hominem* Professorship, there would be an established Chair in the subject. It was to this that Redhead was elected. A year after his arrival in Cambridge, Redhead was elected to a Professorial Fellowship of Wolfson College, where he served as Vice-President from 1992 until his retirement from Cambridge. In 1991 he was elected as a Fellow of the British Academy. By working closely with his colleague Jeremy Butterfield from the Philosophy Faculty, supervising a series of brilliant graduate students, and encouraging visits from both physicists and philosophers (such as James Cushing, Gordon Fleming and Paul Teller), Redhead turned the Department at Cambridge into the foremost centre for philosophy of physics in the UK until he reached his retirement age in 1997.

Still very energetic, Redhead then took on the role of Acting Director of the Centre for Philosophy of Natural and Social Science at the London School of Economics in 1998, a post he retained until 2001. In 1999 he was elected to a Centennial Professorship at the LSE: an honour bestowed on a small number of senior academics across all disciplines as a celebration of the centenary of LSE.

Amongst Redhead's other achievements were visiting fellowships at the Universities of Pittsburgh (1985), Princeton (1991) and Essex (2004), as well as All Souls College Oxford (1995). He acted as President of the British Society for the Philosophy of Science in the period 1989 to 1991. From 1991 to 1994, Redhead was the Tarner Lecturer at Trinity College, Cambridge. In 1995 he was elected Member of the Académie Internationale de Philosophie des Sciences. From 1992 to 2001, he was Joint Editor of what soon became the leading journal in the field, *Studies in History and Philosophy of Modern Physics*, of which he was one of the founders.

An important factor in Redhead's success was his personality. He was tall, calm, rather patrician in bearing, sure of himself but never arrogant. He had a keen sense of humour, and developed friendships with many of the academics and colleagues he came to know. Behind the affable exterior was a steely ambition which occasionally led to a modicum of ruthlessness. He had an extraordinary memory and quickness of thought. He excelled in and relished the atmosphere of research seminars; indeed he sought in Cambridge to emulate the famous Thursday seminars at Chelsea.[1] Those graduate students who received full attention from Redhead as supervisor remained devoted to him – as well as some that didn't. Many of his graduate students in London and Cambridge went on to successful careers in universities in the UK and beyond.

Redhead was supported to an extraordinary degree by his devoted wife Jennifer, whose liveliness and good cheer charmed all who knew her. They had three sons:

[1] For an account of the nature of the Chelsea seminar, and of the department generally, see Kamminga *et al*. (1993: xv–xvii).

Alexander, Julian, and Roland. Jennifer's sudden death in 2010, in her 68th year, was devastating for Michael. In her honour he endowed an annual prize at Cambridge for outstanding performance in the essay components for the department's MPhil degree.

II. The context of the work

Before surveying some highlights from Redhead's extensive publications, we will try to set them in context by first sketching how the discipline, the philosophy of physics, developed during the period of his career. In short, philosophy of physics became established during the 1980s as a distinctive sub-discipline of philosophy, as a result of two developments: one in philosophy, and one in physics.

In philosophy, as in all academic disciplines, the expansion of research and education in universities in the UK and abroad, from the 1960s onwards, made for sub-disciplines, each with its own community and activities such as conferences and journals. In philosophy of science, the main type of such a sub-discipline was the philosophy of a specific science, as against philosophical reflection on ideas occurring in every science: such as the confirmation of theories, scientific explanation and the nature of progress in science. And among the specific sciences, physics was a very natural focus of attention. For it had grown out of natural philosophy, i.e. the philosophy of nature: its basic concepts, such as space, time, matter and causation, were equally the subject-matter of metaphysics and epistemology.

A focus on these concepts was also natural because the first half of the 20th century had of course produced two revolutions about the concepts just listed: the quantum and relativity revolutions. These overturned – or at the very least, utterly transformed – almost every topic in physics. Quantum theory threatened established ideas about determinism, about measurement as revealing the pre-existing properties of the measured object, and about the distinction between wave and particle. And relativity theory questioned the distinctions between space and time, and between matter and empty space. We will discuss, first, quantum theory and then relativity.

Quantum theory, in the formulations it took in the mid–1920s onwards (after two decades of struggle, some of it philosophical), has of course had vast empirical success. It gives a quantitatively accurate description and explanation of countless phenomena. These phenomena occur across a vast range of scales. Nor are these 'just' scales in space and time: such as ranging from terrestrial experiments about very brief microscopic events, to vast, long-ago and very distant events such as the explosion of a star that has exhausted its nuclear fuel. Quantum theory is also confirmed across a vast range of scales pertaining to other physical quantities, such as energy or pressure. So much so that it remains unfalsified by any known experiment; while many experiments are impossible to explain satisfactorily using only the classical physics that preceded it. Furthermore, quantum theory underpins a great many technologies on which everyday life now depends, such as the transistor, the laser and the computer chip – with a boom of new technologies currently being developed.

And yet ... there remain conceptual problems about how to understand quantum theory: problems that are untouched by its quantitative empirical success. These problems were vividly presented during the debates in the 1920 and 1930s, by figures such as Einstein and Schroedinger. They concern two main issues, which were to become central in philosophy of physics: the nature of measurement in quantum theory (epitomised by Schroedinger's thought-experiment about his eponymous cat); and quantum theory's curious correlations between mutually distant events (called 'nonlocality').

But after 1935, these problems were largely set aside. Partly this was due to there being so much straightforward physics to do in developing quantum theory, both theoretically and experimentally. There was also a widespread consensus, which today seems shaky, that Niels Bohr, who in the debates had been Einstein's and Schroedinger's most distinguished and tenacious opponent, had come out ahead, if not 'won'. There was also the vast grim historical contingency: a world war, and quantum theory's being pressed into service to make an atom bomb. Winning that war, though at such terrible cost, led to an atmosphere of ebullient confidence in the power (and prestige) of physics in general, and of quantum theory in particular. Thus it was not until the 1960s that physicists' interest in these problems began to revive. In this revival, perhaps the strongest influence was the work of John Bell, especially his ground-breaking work on nonlocality.

Since then, this revival within physics has gone from strength to strength. A subdiscipline called 'foundations of quantum theory' has long been recognised; as has its large role in the rise since the mid–1990s of the new sciences of quantum information and quantum computation – for both of which, various ideas about nonlocality are central. Thus philosophers of quantum theory have for three decades enjoyed a convergence of their interests with those of quantum theorists. The result is that nowadays there is a seamless continuity between the two communities, foundations of quantum theory and philosophy of quantum theory.

There is a somewhat similar history about physics' other revolution, i.e. relativity theory. From the early 1960s, there was an extraordinary renaissance in relativity, especially in general relativity (Einstein's relativistic theory of gravitation), and in cosmology: as regards both theoretical and experimental, or observational, aspects. This inspired philosophers to work on topics in general relativity that were much more specialised than just the idea of spacetime (e.g. about the nature of gravitational energy, or two spacetimes being observationally indistinguishable, even in principle).

So to sum up: philosophy of physics got established during the 1980s, as a result of both philosophy and physics re-discovering natural philosophy: topics within physical theories that are not necessarily about either calculating an experimental result, or developing the theory's formalism – but are conceptual. Within philosophy, this interest was directed more or less equally at both relativity theory and quantum theory. But within physics, quantum theory got the lion's share of the interest: for physicists nowadays, the phrase 'foundations of physics' mostly connotes foundations of quantum theory.

Redhead's career fits very naturally into this landscape. Given his education and interests, he was indeed in the right place at the right time, as the saying goes. At the time he graduated (1950), physics (and especially quantum physics) enjoyed, in the aftermath of the Second World War, great public confidence and prestige. It was also a period of progress and confidence within quantum physics: for in the late 1940s, an enormously more successful quantum theory of electricity and light, called 'quantum electrodynamics' ('QED' for short), had been developed. (It was about this theory that Redhead wrote his two 1953 papers.) But as we mentioned, it was also a period when most physicists were unflinchingly practical in their outlook, with little interest in foundational questions.

But by the 1970s, when Redhead re-entered academic life after doing his physics PhD, such questions were again alive, both for quantum theory and for relativity theory. And fortunately for him, they were being addressed with great expertise, at his local department of history and philosophy of science, at Chelsea: a happy conjunction.

This sense that the time was ripe, in the 1970s but not the 1950s, for questions in natural philosophy is conveyed well in the opening words, indeed the title, of Redhead's Cambridge inaugural lecture, 'Physics for Pedestrians':

> When I first studied physics at University College London, everything seemed to go at a frenetic pace. We were kept so busy calculating answers to rather contrived problems in mechanics, thermodynamics, … that there seemed too little time to reflect on the general framework of what we were doing, to examine the fundamental concepts of space, time, motion, matter and force that seemed to be so much taken for granted on the first pages of the textbooks. ... What I needed was time to stop and stare, not the bustling physics of people in a hurry, but physics for pedestrians. … It was some years later that I discovered that there *were* people interested in these very broad foundational questions in the same way I was myself, but they operated in philosophy departments rather than physics departments. (1989: 1–2)

So much by way of placing Redhead's career within the development of philosophy of physics, as a sub-discipline. We will now turn to surveying Redhead's body of work in more detail.

This body of work naturally falls into four sectors; though the boundaries between them are of course vague. First, there is work in the philosophy of science that only in part relates to physics: see section III below. Then there is a great deal of work on quantum physics. This undoubtedly represents *the* central area of Redhead's research: unsurprisingly, in the light of his physics background having emphasised quantum physics, and physicists' renewed interest after 1970 in the foundations of quantum theory. So we will divide this work on quantum physics into two sectors. There is work about the two problems introduced above, that arise already for the formulation of quantum theory (called 'non-relativistic quantum mechanics') obtained in the mid–1920s: the problem of measurement and the problem of nonlocality. We report this work in our section IV. Then there is work about other aspects of quantum physics, and especially aspects that appear only in developments made after the mid–1920s, especially quantum field theory. This work will be the topic of our section V. Finally, there is work about branches of physics other than quantum physics: that will be our section VI.

III. The philosophy of science

Throughout his career, from the 1970s at Chelsea College to the mid–2000s at LSE, Redhead published papers in general philosophy of science. The range is wide. But we shall begin with papers from the Chelsea period, emphasising three papers that relate to physics.

In 1975, a year after his appearance at Chelsea College, Redhead published a lengthy paper in *Synthese* on the role of symmetry principles in understanding the formal relations between competing – usually successive – theories in physics (1975). The paper was partly inspired by Post's 1971 study of inter-theory relations, and indeed it attempted to improve on the use Post made of Curie's Principle in the context of pairs of theories standing in different sorts of correspondence. Redhead's paper was original in a number of ways. It introduced a new classification of symmetry principles. It articulated a detailed theory of theories that construed theories in terms of function spaces: and some regard it as a version of the 'sematic view' of theories, that was later influential. This theory also clarified the distinction between mathematical and physical symmetries, and introduced the influential notion of surplus structure in certain physical theories, such as gauge theories – to which we will return in section V(c). It provided a taxonomic treatment of heuristic symmetry principles approached from an empirical-historical point of view.

The richness, sophistication and scholarship of Redhead's 1975 paper reflect a wide knowledge, not just of modern physics, but also of advanced topics in the philosophy of science and the role of mathematics in physics. For readers who had no prior knowledge of his work, which probably means all philosophers outside Chelsea College, the paper must have seemed a revelation – and one which heralded the arrival of a new star in the philosophy of science with special reference to physics.

The year 1980 saw the publication of several papers that were based on talks given at various times in the previous five years. The most important were two papers on the philosophy of contemporary particle physics (1980a), and on the role of models in physics (1980b): each paper serving to illuminate the other. The first was an authoritative account of the emergence of elementary particle theory (EPT) and its various branches, but with special emphasis on their connection with age-old debates in natural philosophy such as that between atomism and an Anaximanderian type of fundamentalism. Although written when quantum chromodynamics was a recent addition to EPT and the quark theory of strong interactions did not have the paradigmatic status it has today, the paper surpassed anything of its kind in the philosophical literature to date in terms of detail and insight; and it is still compulsory reading in the subject. The second paper was based on the important realisation that scientific theorising is not 'the art of the soluble', but rather the art of approximation through models. It provided a careful classification of different types of models in physics; it stressed the unavoidable role of intuition in some cases; and it gave a gentle rebuttal of the claim by some philosophers of physics that the art of modelling can be a black one.

Other strands in Redhead's early papers concerned the logic of statistical tests (1974), the meaning of ad hocness in appraising scientific theories (1978) and a clarification of the role of Bayesianism in scientific methodology (1980c). Though characteristically insightful, these papers were of a lesser weight than those already mentioned. In the first and last cases, they were a response to work by Donald Gillies and Jon Dorling, respectively, who as mentioned were Redhead's colleagues at Chelsea College. It is clear that the lively intellectual atmosphere at Chelsea was a major factor in Redhead's early philosophical development.

The Chelsea period did not see the end of Redhead's work in the philosophy of science. Later papers appeared on inductive probability (1985), novelty and confirmation (1986), explanation (1990) and human cognition (1994). Perhaps the most controversial work is in a 2004 paper ('Mathematics and the Mind') attempting to show, without appealing to the complexities of Gödel's incompleteness theorem, that certifiable truth outruns provability. The argument rests on the alleged truth of the unprovable claim that numbers in 'Sorites' (Robinson) arithmetic – Peano arithmetic without the induction axiom – satisfy the commutative law of multiplication. Redhead saw himself as providing what is arguably a simplified justification of the famous claim by Sir Roger Penrose FRS and John Lucas FBA, both of the University of Oxford, that minds are not digital machines. Although it is safe to say that such justifications, simple or otherwise, have not convinced most philosophers of mathematics, the issue remains open.

IV. Non-relativistic quantum mechanics

The year 1981 saw the start of Redhead's publications on the foundations of non-relativistic quantum mechanics. In this year he published a paper, co-authored with Harvey Brown, a student at Chelsea College, criticising the famous quasi-classical Heisenberg 'microscope' thought experiment designed to justify the quantum uncertainty relations. Redhead's writings on quantum mechanics, for which arguably he was and is best known, were to span over two decades. The first phase of this work culminated in his 1987(a) book *Incompleteness, Nonlocality and Realism: a Prolegomenon to the Philosophy of Quantum Mechanics* (henceforth *INR*), and was largely motivated by important technical developments from the mid–1960s. These were due principally to John S. Bell (a physicist), and Simon B. Kochen and Ernst P. Specker (mathematicians).

John Bell's work, referred to in section II, was responsible for the feat of expanding discussions of quantum foundations from seminar rooms in philosophy departments into experimental physics laboratories. At the same time it had the effect of turning a paper of Einstein's that had had little impact for well over three decades into his most-cited paper. In 1935, Einstein and his collaborators Boris Podolsky and Nathan Rosen argued that if quantum mechanics is a *complete* theory, it must predict an instantaneous action-at-a-distance in the case of pairs of quantum systems that are 'entangled'. The possibility of such entanglement is uniquely quantum mechanical: it means that to completely describe the pair of systems it is not enough to describe them individually. There is something holistic, or non-separable in their joint description. Einstein did not approve of action-at-a-distance, sometimes called nonlocality (though this term today is used in a wider sense) and thus rejected the assumption that quantum mechanics is complete. This posed a serious challenge to the then-orthodox interpretation of the theory, namely the Copenhagen interpretation, whose leading authority was Niels Bohr. But in the wider physics community, the Einstein-Podolsky-Rosen (EPR) paper created barely a ripple.

Bell, a physicist working in the theory division of CERN in Geneva, had an interest in the possibility of hidden variable theories in quantum mechanics. These are roughly speaking the kind of theories Einstein was advocating – if quantum mechanics (QM) is incomplete, as Einstein argued, there must be ways of supplementing the wave function, i.e. the description given by the standard theory, with extra variables which, although not manipulable by the experimenter, would restore determinism to physics. Indeed such a theory, independently developed by Louis de Broglie and later David Bohm, already existed: the 'pilot-wave' theory. But this theory too suffered from action-at-a-distance for entangled systems, precisely what Einstein hoped to avoid! Bell asked himself whether the nonlocality of the de Broglie-Bohm theory is a *generic* feature of any such deterministic theory which agrees with the standard quantum predictions for entangled systems. In 1964, *Bell proved that this is the case*, contrary to Einstein's hopes and expectations. At the time, it turned out that the experimental evidence in favour of the correlation predictions involved in Bell's ingenious and simple argument was lacking. But eventually his paper led to hundreds of entanglement experiments being performed: which clearly corroborated the quantum predictions and not those of local deterministic theories. After Bell, discussions about the foundations of quantum mechanics became**,** in the view of many physicists**,** kosher. And as we reported in section II, the subject has since gone from strength to strength with the rise of quantum information science.

Also in the mid–1960s another result emerged that limited the scope of hidden variable theories. The most famous version of this result was due to Kochen and Specker in 1967, though Bell himself had already anticipated it some years earlier. Deterministic hidden variable theories are designed to yield definite predictions about the outcomes of measurements on the physical quantities of a quantum system, conditional on its quantum state and the value(s) of its hidden variable(s) – even if the latter are beyond the experimenter's control. What the Bell-Kochen-Specker theorem – often misleadingly called a 'paradox' – showed is that in order to be consistent with the standard quantum formalism, these predictions must be 'contextual'. That is: they must in general depend *on the way the quantity is being measured*, and so do not reflect an intrinsic value of the quantity. Bell himself was not surprised by this result: the de Broglie-Bohm pilot-wave theory had this feature built in.

In this initial phase of Redhead's work, he concentrated on spelling out precisely the assumptions involved in the Bell nonlocality theorem and its conceptual implications, especially in the light of Einstein's special theory of relativity, and also extending the analysis to stochastic (indeterministic) hidden variable theories. Then, in 1983, in a remarkable paper written jointly with his doctoral student Peter Heywood, he succeeded in connecting the issues of nonlocality and contextuality. Using the result of Kochen and Specker, the paper showed that for certain entangled quantum systems (not those in the 1964 Bell theorem) another demonstration of nonlocality for deterministic hidden variable theories is possible that does not depend on a key feature of Bell's argument, and its later variants – the so-called 'Bell inequality'. In the same year, Allen Stairs, a philosopher at the University of Maryland, independently provided a similar nonlocality proof: which proved to be considerably simpler as well as advantageous in other ways. However, in 1989, the physicists Daniel Greenberger, Michael Horne and Anton Zeilinger published another proof of nonlocality for deterministic hidden variable theories, again without using a Bell inequality (and without appealing to Bell-Kochen-Specker) – which attracted much more attention than the Heywood-Redhead-Stairs result. It is probably fair to say that this latter result has never received the recognition it deserves, particularly on the part of physicists. (In 1991, in collaboration with Rob Clifton (see below) and Jeremy Butterfield, Redhead proved a generalisation of the GHZ result for stochastic hidden variable theories.)

All of this early work on Redhead's part was incorporated into his 1987 *INR* book, referred to above. Though the literature on the subject was already extensive, the book arguably represented the best survey at the time, of the post-war advances in the foundations of non-relativistic quantum mechanics. It was both scholarly and clearly-written, and was a boon to students wanting to learn the subject in a rigorous manner. The book deservedly received the international Lakatos Award for Philosophy of Science in 1988, cementing Redhead's reputation as the leading philosopher of physics in the UK.

INR remains compulsory reading for students initiating themselves into the field, particularly in relation to its treatment of the Bell theorem and the Kochen-Specker result (Redhead does not refer to Bell's version of this result). The book also contains one of the first general proofs of the important so-called 'no-signalling theorem' in quantum mechanics (though a limited version dates back to Bohm in 1951).

Much progress has been made in quantum foundations since 1987, so that inevitably some aspects of the book now look dated. It was never really designed to be an introduction to the wider philosophy of quantum mechanics. As is made clear at the start, the book is about the question of what one can say about the values of quantities in quantum mechanics: whether they are sharp but unknown, unsharp or fuzzy, or undefined/meaningless. More specifically, it is about whether the first option is viable, and this makes the book essentially an analysis of hidden variable theories. These represent only one of several important approaches to the interpretation of quantum mechanics. (There are insightful comments in the book on Bohr's theory of complementarity, but they are brief. There is also a stand-alone chapter on quantum logic at the end of the book, but since 1987 this topic has faded in discussions of quantum foundations. The fact that there is no mention of the 1957 Everett ('many worlds') interpretation does not reflect adversely only on Redhead: regrettably, in the 1980s very few philosophers took it seriously.) The specific issue of under what circumstances quantities have values or not generally fails to resonate with the deep motivations behind the emergence of rival interpretations of quantum mechanics. Even restricted to hidden variables, the discussion in *INR* is rather abstract. It makes no reference to extant hidden variable theories; and so fails to capture the dynamical spirit, and the subtleties of the de Broglie-Bohm pilot-wave theory – as Redhead's students Rob Clifton and Constantine Pagonis later appreciated.

In a series of papers written after *INR*, Redhead further developed his views on the peaceful coexistence between quantum mechanics and (special) relativity theory. He also successfully defended the Bell nonlocality theorem against attempted refutations or misguided derivations. Some of his publications were the result of collaborations: principal amongst the co-authors were his students Rob Clifton, Constantine Pagonis and his colleague Jeremy Butterfield. Clifton, who went on to be appointed a full Professor at the University of Pittsburgh, died, tragically young, in 2002. He was widely recognised as a supremely gifted and productive researcher.

Two other strands emerged in this phase of Redhead's writings. The first had to do with the role of indistinguishable particles in quantum mechanics, and whether it represents a threat to Leibniz's Principle of the Identity of Indiscernibles. The latter question led to an influential paper (1988), co-authored with his student Steven French (who later became a Professor at the University of Leeds). It is noteworthy that this question would be taken up again many years later in the work of another of Redhead's students, Simon Saunders, after he became a Professor at the University of Oxford. Three further papers on the nature of indistinguishable particles were published by Redhead in collaboration with Paul Teller, a Professor at the University of California at Davis in the 1990s (1991, 1992, 2000).

The second strand involved detailed, constructive criticisms of the writings of two prominent philosophers on quantum mechanics – Hilary Putnam (Harvard) and Karl Popper (LSE) – which Redhead published in 1994 and 1995(a) respectively. In both cases, serious flaws in these ideas were revealed, though Redhead was careful to extol the 'abundantly benficial' nature of the overall influence of Popper's long-standing critique of the Copenhagen interpretation.

V. Quantum field theory

Non-relativistic quantum mechanics gets generalised in two principal ways: so as to incorporate special relativity's treatment of spacetime, and so as to allow a variable number of particles. Combining both generalisations, we get relativistic quantum field theory; often called, for short, 'quantum field theory'. This is nowadays a vast subject. It includes a very successful detailed model of the sub-atomic particles and the forces between them, called 'the standard model'. This model includes as one of its facets the quantum electrodynamics (QED) on which Redhead was trained in the early 1950s. (Another facet is the theory of quarks, i.e. the constituents of protons and neutrons; called 'quantum chromodynamics'.) Also, many ideas, methods and detailed results of quantum field theory have proved central in various branches of physics: even the physics of macroscopic systems. Besides, its intricate mathematical structure has stimulated much research in pure mathematics.

To the philosopher of physics, this vast subject offers three main areas of investigation; and Redhead worked in all three. First, one of course asks how the two problems of measurement and nonlocality look in the setting of quantum field theory. As we have seen, they are recalcitrant; so one naturally hopes for some insight, if not a knock-down solution, from quantum field theory's more advanced perspective. Second, setting aside these problems, one asks what becomes of other basic physical concepts such as particle and field in the transition to quantum field theory. Since quantum mechanics had already transformed these concepts from classical physics, one naturally asks what other lessons might be required of us, on the steep learning curve of quantum field theory. Third, quantum field theory introduces a handful of new concepts, without classical forebears, that merit philosophical assessment: for example, bootstrapping and gauge symmetry. Of these three areas, Redhead's research was mostly on the first and the second. We shall discuss them in order.

V(a). Nonlocality in quantum field theory

About the two problems of measurement and nonlocality, the first thing to note is that unfortunately quantum field theory (or more generally: quantum theory's other advanced formalisms) does not solve these problems. They recur. Indeed, the incorporation of special relativity's spacetime setting makes them in some ways more recalcitrant. This is especially true of the problem of measurement, in that two tenable solutions that have been developed for non-relativistic quantum mechanics prove difficult to adapt to a relativistic spacetime. (Namely, the pilot-wave theory mentioned before; and the revision of quantum theory's orthodox equations of motion to model 'the collapse of the wave packet' as a physical process.) But in any case, Redhead did not work on such adaptations. As for the non-relativistic theory, his main efforts concerned non-locality.

Starting in the early 1990s, Redhead wrote (sometimes with student co-authors) a series of papers about how nonlocality – quantum theory's curious correlations between mutually distant events – plays out in a relativistic spacetime. By way of example, we will give details about one of the main papers in this series, a paper called (with Redhead's gift for titles) 'Much Ado about Nothing': for it concerns how according to quantum field theory, even the vacuum exhibits these curious correlations (1995b).

This paper adopts an approach to formulating quantum field theory as rigorously as possible, called the 'algebraic approach'. This approach was launched in the 1940s and has been prominent since the 1960s; and in the early 1990s, Redhead (and other philosophers such as his ex-student, Rob Clifton. and John Earman in Pittsburgh) adopted it as their favoured framework for discussing quantum field theory. Accordingly, the paper builds on previous work about vacuum correlations in the algebraic formulation of relativistic quantum field theory.

About that previous work, the first thing to say is that the word 'vacuum' is used in a non-everyday sense. It does not mean 'nothing'. That is, 'the vacuum' does not denote the absence of the quantum system. Rather, it denotes the lowest energy state of the quantum system: i.e. in this discussion, of the quantum field that one's theory is about, such as the electromagnetic field. (Or the field that replaces a traditional particle, such as the electron; see section V(b) below.)

Classical physics leads one to expect the lowest energy state of any system to be utterly quiescent. The prototypical example is a classical particle at rest at the bottom of a well of potential energy. It has no momentum, and so no kinetic energy, and also no potential energy. But quantum theory's uncertainty principle forbids a quantum particle to have simultaneous precise position ('the bottom of the well') and momentum ('at rest' i.e. zero). And the situation is similar for a quantum field (or a system of several such, interacting with each other). The vacuum state is 'agitated' in that it prescribes non-zero probabilities to get outcomes of measurements (if one were to perform measurements on the system) that indicate 'activity', such as non-zero values for the field's momentum.

So what about correlations between mutually distant events, according to the vacuum state of a quantum field theory; or indeed, according to any other state? Work previous to Redhead's paper had shown that there is a rich structure to such correlations. (It suffices to consider the vacuum: the situation for other states is similar in relevant respects.) One seminal result (already in 1961) was by Reeh and Schlieder. Roughly speaking, it says that starting with the field in its vacuum state throughout all spacetime, an arbitrary state within some specified spacetime region R (even a very 'active' state such as the quantum-field-theoretic state underpinning, say, a performance of Beethoven's Ninth Symphony!) could be produced by acting on the vacuum state in *any* region R': even one that is so far from R that it cannot be connected to R even by light signals; even one that is tiny, such as a centimetre wide and a half-second long, like a finger-click. This is an amazing (and famous) result: though (as one might perhaps expect) the action that one needs to do on the vacuum in this distant and small regions R', so as to achieve the arbitrarily chosen state within the specified region R, must be very judiciously chosen.

We can see how this result sets the context for Redhead's paper, by noting that the Einstein-Podolsky-Rosen (EPR) argument invokes strict correlations, while Bell's theorem concerns non-strict correlations. In short, Redhead related results like the Reeh-Schlieder theorem to the EPR argument. For by the late 1980s, that theorem and others like it had led to precise results that the vacuum state violates the Bell inequality for appropriately chosen physical quantities associated to two mutually distant spacetime regions (where 'mutually distant' again means that they cannot be connected even by light signals). Redhead's innovation was to show – using a lovely analogy with the simplest possible system exhibiting the curious correlations, viz. two non-relativistic 2-level quantum systems, i.e. qubits – that such results, indeed the Reeh-Schlieder theorem itself, also imply strict correlations of exactly the type in the EPR argument.

This paper led to others. The most important were: a paper, co-authored with Rob Clifton and others (Clifton *et al.* 1998), exploring the structure of the EPR-like correlations in the vacuum state of any quantum field; and a paper, with his student Fabian Wagner (1998), giving a unified treatment of the EPR and Bell arguments within algebraic quantum field theory.

V(b). Particles, fields, localisation

We turn to the second area of philosophical investigation suggested by quantum field theory. Namely, how it treats concepts such as particle, field and localisation. Of course, the classical versions of these concepts had already been radically changed by the advent of quantum mechanics. There, the classical point-particle is replaced by a quantum 'particle' whose state is – not a position and momentum, but – a wave-function. That is: function on space assigning to each spatial point a number that is, roughly speaking, the (square root of the) probability for the 'particle' to be detected as located there – were a position measurement performed. So the question now is: what further changes, if any, in these concepts does quantum field theory demand?

The short answer, on which all agree, is: 'Yes, there are further changes. The erstwhile particle is to be treated "even more" as a field. A particle (and any finite number of them) is to be treated as an excitation – a state of agitation – of an all-pervasive field'.

To explain this, we first recall that the classical concept of field, such as the electric field, is that of a function on space: the assignment to each spatial point of an appropriate value (for the electric field, a vector). More generally: to specify the instantaneous state of a classical field requires infinitely many real numbers, while specifying the state of an assembly of particles requires only finitely many; (for a single particle in 3-dimensional space, just six: three for position and three for momentum). So 'field' (as against 'particle') is really short for 'physical system whose state can only be specified with infinitely many real numbers'.

So on this usage, even the quantum mechanics of section III replaces a classical particle with a field. For the state of the 'quantum particle' is a wave-function on space: requiring infinitely many numbers. The same goes for a pair of classical particles: the quantum mechanical replacement of their state is an assignment, to each pair of spatial points, of a number that is, roughly speaking, the (square root of the) probability for the two 'particles' to be detected as located, respectively, at the two points – if a joint position measurement were to be performed. And similarly for three classical particles: the state of the corresponding quantum system assigns to each triple of spatial points, the probability of three 'particles' being detected as located, respectively, at those points – if a measurement were performed. And so on, for any number, N, of quantum particles: i.e. of the quantum replacements of the erstwhile classical particles.

That may seem complicated enough. But quantum field theory adds more. For recall our saying at the start of section V that a principal idea of quantum field theory is to allow a variable number of particles, i.e. of quantum particles. So in quantum field theory, the state needs to encompass all the alternative possibilities for the total number of particles. This means that each state must prescribe (i) a probability for there to be one particle (and assuming that: prescribe a wave-function on ordinary space), and (ii) a probability for there to be two particles (and assuming that: prescribe a function on all pairs of spatial points), and so on up ... and finally also: a probability for there to be no particles (i.e. the vacuum, understood as in section V(a), as the lowest energy state of the field).

So the vision is: the underlying quantum system pervades all of space. At any time, its state includes, for any non-negative whole number *N* (0,1,2,…), a probability (in general non-zero) for *N* detectors designed to detect particles' locations – that we suppose we have distributed through space (and turned on!) – to register some number *n* of clicks (where *n* is 0 or 1 or 2 or ... up to *N*). This is summed up by saying that the quantum system is really a field, and that particles are – not fundamental, but 'just' – excitations of the field.

For example: for electrons, this means that instead of there being some definite though vast (and presumably contingent) number *N* of electrons with some presumably-complicated wave-function (assigning to each vast *N*-tuple of spatial points, a probability of detections at its respective components), there is 'only' (for each positive integer *N*) a probability for there to be *N* electrons, with some presumably-complicated wave-function.

This dizzying vision of the possible states of a quantum field as allowing any positive integer as a possible total number of particles is called 'Fock space'. It is a centrally important way of thinking of quantum field theory. Redhead's rich 1983 paper, 'Quantum field theory for philosophers', expounds it for a philosophical readership; together with associated ideas, such as the creation and annihilation of particles, and virtual particles. (A subsequent paper added some further comments and minor revisions: 1987b.)

Of course there is more, much more, to say: even about Redhead's own discussion of field and particle. But we will end this sub-section by mentioning his research about one aspect of the concept of particle that we have so far not stressed. Namely, the idea that a particle has a position: more precisely, that a point-particle can be localised at a spatial point. It is an infamous fact about relativistic quantum theory (even without adding the idea of variable particle number) that it cannot accommodate a concept of position, or localisation, with all the features that one would intuitively want. For example, one intuitively wants the following features:–

> (a) a state localised at a spatial point *p* should have no overlap with (in the jargon: should be orthogonal to) a state localised at a different spatial point *p*′;
> (b) if two instantaneous regions of space (in the jargon: two spacelike patches) are so far apart that they cannot be connected even by light signals, then it must be possible to accurately measure position for both the regions, i.e. to 'ask' the system 'Are you within this region?', for each region; and
> (c) in a relativistic theory, a system that is at an initial time confined to an instantaneous region of space, *R* say, will not propagate outwards from *R* faster than light; (in the jargon: it will at all later times have zero probability to be detected outside the future light-cone of *R*).

But one 'cannot have it all'. That is: it was known already in the 1940s that in a relativistic quantum theory, no concept of position can have all such desirable features like (a) to (c). This sort of impossibility or 'no-go' result was then developed in various ways: for example, in a famous 1974 paper by Hegerfeldt, emphasising the troubles about feature (c). Redhead's contribution (in a 2003 paper: again written with a student, Talal Debs) was to calculate some central details, that bring out how relativity makes the notion of localisation dependent on the frame of reference, i.e. the coordinate system in which we describe events. More precisely: a particle that according to one frame has a wave-function confined at some specific time to some finite region of space will, according to another frame in motion relative to the first, have at the corresponding time a wave-function spread throughout space. In short: here we glimpse the subtleties that beset the effort to combine relativity with quantum theory – even once we set aside the problems of measurement and non-locality emphasised in section IV.

V(c). Concepts without classical forebears

For quantum field theory, our third and final area of investigation is its use of some concepts that have no classical forebears. Even the most obvious list of such concepts is enticing, since most of the entries await, even now, a consensus about their overall philosophical significance. Here are some examples: gauge symmetry, boot-straps, the connection between spin and statistics, renormalisation, spontaneous symmetry breaking, and the origin of mass. Unlike the concepts we discussed in sections V(a) and V(b), each of these examples is not part of the common framework of quantum field theory. Each is used in one or another of the specific theories of sub-atomic particles, such as quantum electrodynamics (QED) and quantum chromodynamics, which we mentioned at the start of section V.

Fortunately for philosophers of physics, Redhead made several inroads into this territory; in both his early papers (from the mid–1970s) and in the 2000s, after he retired. Let us briefly mention papers about the first three entries in our list: gauge symmetry, boot-straps, and the connection between spin and statistics. We treat them in order.

Gauge symmetry is a powerful, multi-faceted concept which occurs already in quantum electrodynamics: on which, as we noted, Redhead was trained *c.* 1950. The general idea of symmetry has long been a very fruitful idea in physics, yielding countless methods and results: so Redhead's focus on it in his first major paper (1975: see section III) is unsurprising. But gauge symmetry is distinctive. For the main idea is to describe a system with more variables than one strictly needs: a redundancy (called by Redhead 'surplus structure') that at first seems inefficient, and so puzzling. Gauge symmetry gets more developed in theories, such as quantum chromodynamics, that were rapidly developed in the 1970s, and soon found to be empirically very successful. Indeed, that decade saw a revival, theoretical and experimental, of quantum field theories (with gauge symmetries), after a fallow period in the 1960s, when the physics of sub-atomic particles was dominated by another programme, the S-matrix programme. This programme downplayed fields: it proposed a 'boot-strap' conception of sub-atomic particles in which, roughly speaking, no particle was more fundamental than, or a constituent of, another, though they could transmute into each other. (This equality of status was jokingly called 'nuclear democracy'.)

Thus by the mid–1970s Redhead was well-placed to write about both: (i) gauge symmetry, both as it occurred in the already-established quantum electrodynamics and as it occurred in other then-novel gauge theories; and (ii) the S-matrix programme and boot-straps. His 1975 and 1980 papers mentioned in section III discussed (i); and he returned to it, mainly in his (2003) which records the 1970s' consolidation of gauge theories, and briefly discusses other developments such as BRST symmetry and gauge theories of gravitation. Besides, symmetry in general was an important theme for Redhead throughout his career. Indeed, it was the topic of a book (2007), co-authored with his student Talal Debs, which built on their joint papers: to which we will return in section VI.

As to the S-matrix programme and boot-straps: Redhead's (2005), written in memory of his friend James Cushing (University of Notre Dame), built on his (1980a)'s discussion, as well as on Cushing's very detailed historico-philosophical work, done in the 1980s.

Finally, we mention Redhead's work on the connection between spin and statistics. This was joint work (2003) with a student, Michela Massimi. The story begins with a brilliant though opaque proof by Pauli in 1940, that in a relativistic quantum field theory, systems whose spin (a quantum theoretic cousin of classical angular momentum) is an integer (0 or 1 or 2 etc.) must exhibit a certain kind of statistics (called 'Bose-Einstein' or 'boson'), while having a spin equal to one-half, or one-and-a-half etc. implies a different statistics ('Fermi-Dirac' or 'fermion'). Among the many subsequent analyses (and especially efforts to simplify the proof), Massimi and Redhead pick out a little-known one by Weinberg (in 1964). About this, they show, in a striking display of theoretical physics combined with historical analysis, not only the merits of Weinberg's approach over others, but also how it realised some intellectual ambitions of Pauli's, especially in relation to his disagreements with Dirac about interpreting negative energy solutions.

VI. Work independent of quantum theory

So far we have concentrated on quantum theory, and only mentioned relativity theory as being the 'other revolution' of early 20th-century physics. Thus we have so far neglected the third great pillar of modern physics, thermal physics. This comprises both: thermodynamics, which gives general relations between heat, work, temperature and other quantities independent of the system's microscopic constituents, i.e. its atoms; and statistical mechanics which (in both a classical and a quantum version) applies probability theory to the vast numbers of constituent atoms so as to explain macroscopic phenomena – including in good measure the results of thermodynamics.

Redhead also wrote papers about relativity and thermal physics. Of these papers, several are significant, both for their conceptual innovation and their historical and physical scholarship. Besides, some of the work on relativity done with his student Talal Debs led to the above-mentioned co-authored book (2007) on the nature of symmetry and related ideas about invariance and objectivity, not just in relativity but in physics as a whole. Again, we cannot present all the details of this material. We shall pick out just two topics: (1) as regards relativity, the work with Debs, which relates especially to the ideas of conventions and conventionality (and so also, in contrast: objectivity) in physical theories; and (2) as regards thermal physics, two papers (both co-authored) giving remarkable insights about the concept of entropy.

(1) At the heart of relativity's unification of space and time into spacetime is the idea that simultaneity depends on the frame of reference. Since the 1910s, this idea has engendered debate about the explanation of the different elapsed times along different worldlines – made vivid in the 'twin paradox', about the differential ageing of two twins with appropriately different worldlines in spacetime. (In short, the worldline of one is straighter than that of the other.) The idea of Redhead and Debs (1996) is to assess, indeed classify, the treatments of the paradox using the doctrine (stemming from the 1920s, especially the philosopher Reichenbach) that simultaneity is conventional. This is the doctrine that whether two events, that cannot be connected by a signal at most as fast as light, are simultaneous is a matter, not of fact, but of conventional choice. Redhead and Debs show how to reconcile different observers' having (appropriately constrained) choice about which simultaneity judgments they endorse, with the factual matter of the differential ageing. The themes here, of convention and objectivity, bring us back of course to the broad philosophical topics broached in section III. And as mentioned, Redhead and Debs worked up their views into a book (2007): which included their work on localisation – see the end of section V(b) – and also treated symmetry more generally.

(2) In thermal physics, the concept of entropy is central. Witness how everyone has heard of the second law of thermodynamics, about the remorseless rise of entropy. Or so it is said. For in fact, entropy is subtle; and in some respects it, and how to formulate the second law, is still controversial. (Certainly, the second law does not simply say that entropy rises.) Redhead wrote two strikingly original papers about entropy.

The first paper (1989: co-authored with his colleague Kenneth Denbigh FRS) is about a paradox that was first stated by the great 19th-century physicist Gibbs, and is therefore called 'Gibbs' paradox'. The paradox is about how mixing samples of two different substances increases the entropy, whereas mixing two samples of the very same substance does not. As one might expect, the contrast is usually thought to turn on the fact that for two different substances, the microscopic constituents, i.e. atoms, of the two samples are distinguishable, while for two samples of the same substance, they are indistinguishable. Thus the paradox is usually related to statistical mechanics; and also to the topic of indistinguishable particles discussed in section IV. But Redhead and Denbigh (who was an expert on physical chemistry) argue that the paradox is really resolved by ideas from physical chemistry and thermodynamics, i.e. independently of the samples' microscopic constituents.

The second paper (1998: co-authored with his student Katinka Ridderbos) is about a famous experiment that, as the phrase goes, 'reverses the arrow of time'. This means that given a certain process involving, say, particles with various initial positions and velocities, and ending some time t seconds later, with certain final positions and velocities: one engineers to make the process run backwards (like a movie film), by starting the particles off in their final positions, but with reversed velocities, so that after t seconds they arrive at what were (in the given process) their initial positions, with the reverse of their initial velocities.

The idea of such an exact time-reversal of a process was formulated in the late 19th century, as an objection to the great 19th-century physicist Boltzmann's mechanical version of the second law of thermodynamics, as it applies to dilute gases. Roughly speaking, Boltzmann's proved that when such a gas is isolated from its environment, its entropy is non-decreasing i.e. stays constant or rises. But since in Newton's mechanics of particles with positions and velocities, the time-reversal of a process which exhibits increasing entropy is equally possible – the assumptions in Boltzmann's argument were soon shown to be questionable. But apart from time-reversal's value as a thought-experiment for objecting to Boltzmann's derivation of the second law, one naturally asks: Can one in fact engineer such an exact time-reversal of a seemingly irreversible process? And if so, what are its implications for understanding the second law?

Indeed, in the early 1950s an experiment exhibited an exact time-reversal of a process involving the nuclei of hydrogen atoms, i.e. protons. (The analogue of the particle's velocity that gets reversed is the proton's spin: so the experiment is called a 'spin-echo' experiment.) Ridderbos and Redhead's paper opens with a simple model of the experiment, and then criticises a widespread proposal for how to understand the second law (in short: invoking a coarse-grained version of entropy). They show that this proposal is unable to explain the experiment. This critique leads into a more general defence of what is called 'interventionism'. Roughly speaking, this is the view that the second law can only be justified by recognising that the system in question is open, i.e. interacting with its environment, so that correlations with the environment are continuously generated.

VII

Let us conclude by summarising how Redhead's career fits in the landscape of the philosophy of physics. We have seen that from the mid–1980s to the mid–2000s Redhead was, together with figures such as Abner Shimony and John Earman in the USA, one of the 'Deans' of the subject. He was innovative and influential in all regards: in his writings, his teaching, his benevolent personality, and his enterprise in developing the subject. It has now become a thriving branch of philosophy, seamlessly in contact with physics itself. Thus, although nowadays graduate students and post-docs in the subject will not have known him – their generation is roughly that of his academic great-grandchildren – the community of people, as well as of ideas, in which they find themselves is very much of his making.

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