Critical Notice: Tian Yu Cao's

"The Conceptual Development of 20th Century Field Theories"

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Tian Yu Cao has written a serious and scholarly book covering a great deal of physics. He ranges from classical relativity theory, both special and general, to relativistic quantum field theory, including non-Abelian gauge theory, renormalization theory, and symmetry-breaking, presenting a detailed and very rich picture of the mainstream developments in quantum physics; a remarkable feat. It has, moreover, a philosophical message: according to Cao, the development of these theories is inconsistent with a Kuhnian view of theory change, and supports better a qualified realism.

Covering so much ground, it would be churlish to criticize the book on points of detail; the remarkable thing is that it was written at all. I should give warning of some general deficiencies, however. The style of writing can be off-putting; a great deal of background is presupposed; and the ideas are treated historically, without the clarity of hindsight. The last may be appropriate for a work in the history of science, and sometimes it is a good idea when doing philosophy of science, or when studying the foundations of modern physics; but it is not I think in this case. The ideas are too difficult; one loses sight of the wood for the trees. Cao surveys an immense number of trees, especially in the second and third part of the book, devoted to quantum field theory and gauge theory respectively (the first is concerned with general relativity); as a guide and bibliography for historians and philosophers of physics it will, I think, prove invaluable; but as a philosophical and historical review of these theories it is much less satisfactory. All the same, Cao has something important to say.

I shall read him as making two principal claims. The first is what he says it is: these theories are intertwined, and provide clear evidence of continuity of development, preserving what he calls "structural" aspects of phenomena (or theories of phenomena). He speaks of this in terms of "ontological synthesis"; he supposes that this is at odds with Kuhn's views on the nature of scientific change.

The second claim is largely tacit. It is that the conceptual problems and foundational questions of quantum physics, including questions of *ontology, realism, and truth*, have nothing to do with the problem of measurement in quantum mechanics. In fact Cao completely ignores the problem of measurement, dismissing out of hand its modern treatment by physicists and philosophers of physics. In Cao's words:

> We shall leave aside metaphysical speculations and unsuccessful physical attempts (such as the hidden-variable hypothesis, or hydrodynamic and stochastic interpretations) and concentrate on the interpretations that acted as guides in the historical development of quantum physics.

When he comes on to consider the later history of quantum field theory and gauge theory, the problem of measurement drops out of sight entirely. It played (almost) no role in either area, so Cao feels he is entitled to ignore it. Yet he is saying something about realism all the same; these programs are supposed to be consistent with realism, and even to lend support to it, regardless of the measurement problem.

Both claims are interesting and, if true, they are important. Are they true? This is the question I shall pursue. I will not be able to do full justice to Cao's book, for it encompasses more than these two claims; but it seems to me that they are the most interesting ones for philosophers. I

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shall begin with the first, and with the examples cited by Cao in support of it. As we shall see these examples have their shortcomings; they are, I shall argue, the wrong kinds of examples. I think he is right to say that *something* structural is preserved across theory change, but this claim needs to be disentangled from any particular thesis (such as Cao puts forward) about how quantum mechanics and relativity theory are to be reconciled with one another. Later on I will give some different examples, that suggest better what this “something” might be. Unfortunately for Cao’s second claim, however, this does lead on directly to the problem of measurement.

Structural Realism

Cao is concerned with three theories, or clusters of theories: general relativity, quantum field theory, and gauge theory. Over and above them, he is interested in what he calls “research programs”, specifically the geometric program, the quantum field program, and the gauge field program (GP, QFP, and GF respectively). In each case Cao charts the development of the theories concerned, especially their early stages. The geometric program, in particular, is quite narrowly construed in terms of Einstein’s views on the nature of geometry. But they are all introduced by reference to “ontology”; in his final chapter, where Cao returns to the philosophical themes of the book, he concludes that the deeper and most interesting questions are all to do with ontology. He points to a process of “synthesis” and “transformation” of ontology, which he claims to show is “progressive”. In advance he says:

> ....the historical replacement of ontologies, at least in the context of twentieth century field theories, is not without a pattern and direction. That is, an old ontology always turns out to be an epiphenomenon and can be derived from a new and more fundamental ontology. And this certainly ...[lends] support to a realist interpretation of theoretical ontology.

Quite what he means by “epiphenomenal” is not so clear, but he does mean to say that the old ontology is contained, in perhaps modified form, in the new. In this way, he claims, one can defend a certain version of realism, according to which we are learning about the nature of reality and are making objective progress in our understanding of what it is. In this sense, Cao is saying, physics can progressively approximate the truth. Exactly this is what Kuhn consistently denied, from the *Structure of Scientific Revolutions* to the last of his writings so far available (Kuhn 1993 p.330).

Evidently this is a claim that will have to be held up to careful historical inspection. But Cao is not careful in his use of language. He talks of “ontological synthesis”, but also of “dialectical synthesis”, of “emergence”, and of “essential” properties. He talks repeatedly of “structural” properties. Early on, alluding to Cassirer, he talks of a “functional mode of representing reality”, but in the same breath he mentions Whitehead, Leibniz, and Meyerson. One is hard put to know which of several different philosophies he has in mind. He is no clearer when he talks of general physical principles.

It may be that he wants the history he lays out for us to speak for itself. When, in the final chapter, he returns to the philosophical theme, in a section devoted to this concept of “structural ontology” he remarks:

> Among the properties that are relevant for something to be such and such a particular entity are the so-called essential properties. These properties are defined in a theory, and their descriptions may change from theory to theory. Once a former essential property is found to be explainable by new essential properties, it stops being essential. As a consequence of this theory dependence, what a particular entity essentially is will never be finally settled as long as science continues to develop.

Whether or not they are essential, in all cases “property” is some kind of relational construction, defined in terms of physical and mathematical concepts and relations. So understood, Cao’s remark seems uncontentious. But he goes on to say:

> Thus a theoretical ontology in the sense of entity cannot be regarded as a true replica but only as a model of physical reality, based on the analogies suggested by what we know, ultimately by observation. (p.360)

Why not call it a conjecture about physical reality, rather than "only a model"? But let that pass. Why do physical concepts or theories have to be, ultimately, *suggested by observation*, the old inductivist picture? Let that pass too. The difficulty comes when he says:

> What such a model actually provides us with, therefore, is not literally true descriptions of the underlying entities themselves, but rather, by analogy, the assertions of the observable structural relations carried out by the hypothetical entity. (p.360).

If this is what Cao means by "structural realism", it is old-fashioned popular Kantianism, of the sort physicists like Helmholtz and Hertz were so taken with. Believing we only have access to our sensations, they concluded we can only know of the objects causing our sensations insofar as the relations among the sensations mirror the relations among the objects. The objects themselves are necessarily hidden. We find a similar view in Mach and Poincaré, and, in a somewhat different setting, in Russell's middle period and in the early Carnap[2]. The tradition is empiricism rather than realism.

But Cao does not hold to this line consistently. He also says:

> In fact, a stronger case can be argued that any ontological characterization of a system is ... structural in nature. That is, part of what an ontology is is mainly specified by the established structural relations of the underlying entities. Thus structural properties and relations are part of what of an ontology is.

The two positions are quite distinct. The first is that objects are unknowable in principle, because we can only know the structure of their observable effects. The second is that objects are structures, and that we have knowledge of objects (as opposed to the sensations or observations deriving from them). The second is the interesting view; the first is the old-hat, hand-me-down idealism. One might similarly insist that since we can never get outside of our language, or our thoughts, or our theories, we can never compare the world to our representation of it, so the world is forever unknowable in principle. That is another bad argument for idealism.

One can take up a more neutral position. There is Worrall's version of structural realism. Worrall (1989, 1994) argued that a large part of what is ordinarily called the "interpretation" of a physical theory can be abandoned, or radically altered, yet leave intact its more formal ("structural") elements. This formal structure is something over and above the structure of the empirical data, to which the theory had previously (and, by assumption, successfully) applied, and at this level we indeed find evidence of continuity and progressive development through radical scientific change.

Worrall illustrates his thesis by reference to Fresnell's wave theory of light and the luminiferous ether. This is one of the most clear-cut examples where, with the development of the Maxwell-Lorentz theory and subsequently of relativity, the ontology of a successful and mature physical theory is not so much modified as simply obliterated. The ether was supposed to be the fundamental stuff of the world, the ontology of the wave theory; there was a sophisticated body of mathematics about it; the equations were experimentally accurate; many experiments concerned it; but the bottom fell out of the program, since the ether, it was concluded, did not exist. But according to Worrall, the structure of the theory (here Worrall speaks of systems of equations) remains intact: we have lost the *content* of electromagnetism, what the theory is about (namely the ether), but we retain the *form*. And modest though it be, it is a form of realism. It gives us a world neither too close to experiment (which would serve only an instrumentalist view of scientific theories), nor too remote and metaphysical (the world that Kuhn found to discontinuously change). We can specify the way the world is, whether or not that is to specify its ontology.

But Cao makes much of *ontology*. On the elimination of ether, Cao supposes that actually it was just *replaced* by the electromagnetic field. The field is a little more abstract, more formal, more "structural", than the ether, but according to Cao it is commensurable with it; the field stands in place of it. Cao's response to Kuhn and the anti-accumulativists is not that what is lost in theory change is an interpretive super-structure, a kind of metaphysic (and that progress in science occurs not at this level, but at the level of equations and structural features of phenomena); he is rather saying that ontology can be viewed in more structural terms, and, *when* viewed in this way, that there is greater ontological continuity over theory-change than Kuhn and Laudan acknowledge.

Now why, exactly, should a more structural view of ontology lead to a more continuous view of history? Cao explains:

> Although structural assertions will also be modified when the theory changes and new structural properties are discovered, these properties, like observables, are both approximately stable and cumulative because they are translatable from theory in virtue of their recognizable identities. (p.361).

Presumably by "recognizable identities" Cao means that they have some distinctive feature - call it form or pattern - by which structural properties can be qualitatively distinguished, and which is preserved through theory change. Cao's clearest statement of how this is to work is:

> In scientific theories, the structural properties that have been discovered are supposed to be carried by theoretical entities. With the accumulation of structural properties, old theoretical entities will inevitably be replaced by new ones, together with a change of the ontological character of the whole theory. However, as far as older structural properties are concerned, the change of ontology only means the change of their functions and places in the whole body of structural properties. Thus the replacement of theoretical entities should be properly regarded as an analogical extension of the theoretical ontology, which is caused by the accumulation of structural properties, rather than a revolutionary overthrow. (p.361-2).

There will be a difficulty if the "function and place" of a structural thing is part and parcel of what the thing is (as it well might be, on a "structural role" theory of ontology, the partner to meaning holism). We had better suppose that that is being denied. Presumably, then, Cao is saying that these patterns or structures may be assembled in different ways, in novel but coherent relations to one another; but that they are recognizably the *same* patterns.

The picture is an attractive one. It is backed up to some extent by the first of the three examples of structural properties that Cao gives us, namely "external and internal symmetries":

> External symmetries (e.g. Lorentz symmetry) satisfied by laws of physical objects are obviously structural in nature. In fact, the foundational role of transformation groups and the idea of invariants with respect to transformation groups as a description of the structural features of collections of objects were realized and advocated by Poincaré, from the late 19th century..., and were subsequently built into the collective consciousness of mathematical physicists such as Einstein, Dirac, Wigner, Yang, and Gell-Mann. Internal symmetries (e.g. isospin symmetry) are connected with, but not exhausted by, intrinsic properties of physical objects (conserved quantities, such as charge, isospin charge, etc.) through Neother's theorem. (p.361).

To trace the pre-history a little further, one might mention Felix Klein's *Erlangen* program, and the interplay of physics and mathematics in the work of Wilhelm Killing and Sophus Lie, in developing the idea of a group as a differential manifold in its own right. Just as striking, there is the invariance group of symplectic geometry in Hamiltonian mechanics (the group of canonical transformations). It played a central role in most formulations of dynamics, from the Hamilton-Jacobi theory to the Poisson bracket algebra and to the rules of the "old" quantum theory, and it lives on to this day in quantum theory (particularly as defined by canonical and geometric quantization processes).

Cao is on solid ground with this material. Nobody who has taken a serious interest in the development of 19th century mechanics, and its role in the development of differentiable geometry and the theory of topological groups, will deny that there are core concepts here which have been *progressively* deepened. When we come on to relativistic theories it is the same. The standard model (of the strong, weak and electromagnetic forces) incorporates a number of remarkable symmetry groups which are natural extensions of the symmetries of classical electromagnetism. Moreover these gauge symmetries - what Cao is calling “internal symmetries” - more or less control renormalizability at the quantum level. Cao gives some very useful material on this, and on some of the other ideas feeding into the standard model: spontaneous symmetry breaking, the Higgs mechanism, and, in the case of non-abelian unbroken gauge groups, asymptotic freedom.

Whatever else might change, one feels, there is something here that we have learned; we know something about how to model dynamical processes, we have a progressively deeper understanding of them. What is not so clear is that this is a matter of *ontology*. What we were right about in the case of Newtonian mechanics was that configurations of particle positions and velocities can be used to encode their *subsequent* positions and velocities; we learnt something about encoding, and symmetries (much of it still true in QED); but we were wrong to suppose that there are impenetrable bodies, or hard spheres, or point particles, as the fundamental consituents of the world (false in standard QED). Was it really the *ontology* that we got right on at the beginning?

Cao can stick to his guns here and say: So much the worse for the traditional concept of ontology. But he is reluctant to take this step. He wants to retain the traditional notion: “ontology is believed to be the carrier of the general mechanism which underlies the discovered empirical laws”. He explains;

> ...and we know what sorts of things these carriers are, because the potential reference of an ontology is specified by its observable structural properties. For example, the reference of a particle ontology is specified in part by the inclusion of such structural properties as “physical objects have isolable constituents”, and “these constituents have a certain theoretical characterizable autonomy”. The reference of a field ontology is specified by, for example, the inclusion of such structural properties as “the superimposability between different portions of the entity” and “the impossibility of individualizing the entity”.

But the examples are disappointing. Cao says that non-relativistic quantum mechanics has a particle ontology, but then where are the ”isolable constituents”? Particles cannot be strictly isolated in NRQM, not unless one has infinite energy; and not in any theory can they be isolated from gravity. In the case of classical field theory, configurations certainly cannot be superposed, or not in general; the superposition principle only applies to *linear* theories, or to linearized approximations. As for the no-individualizing criterion, it is just when we do have superimposability that we can individuate definite parts to fields (the normal modes or harmonics of the field). Perhaps Cao has in mind the *indistinguishability* of field quanta instead? But we can have indistinguishable particles in NRQM as well; we can even have them in classical mechanics.

None of these criteria really stand up to scrutiny, yet he calls them “hard-core structural properties”, on the basis of which “we can always establish an ontological correspondence between the two theories, and thus make the referential continuity of the ontologies ... discernible”. But it is not at all clear that we can. He does not subject this claim to searching examination. He does not consider the hardest cases.

Looseness with this, and carelessness on the distinction between ontology as the bearer of structure, as opposed to ontology *qua* structure, will prove to be damaging. Losing sight of what is distinctive to the idea of things as structures, he will have little that is new to say to Kuhn.

A Theory of Everything?

Cao's two remaining examples of structural properties, that he considers central to his argument, are respectively "geometrizability" and "quantizability". The latter "is a structural property of a continuous plenum, which is connected with a mechanism by which the discrete can be created from, or annihilated into, the continuous." He offers little further clarification; presumably we are to take what we will from previous chapters of his book on the QFP. But it is not quite clear how much can be included; perhaps renormalizability, as a condition on a system - that it be quantized in a *consistent* way, without any cut-off? He has certainly given us a good deal of background in earlier chapters on the criteria for this, but he makes no special mention of it in the sequel.

Neither is Cao's definition of "geometrizability" very promising. According to Cao, "it is a structural property that is isomorphic to the structural characteristics of a spacetime manifold or its extension". Depending on how big the extension - to infinite dimensions? configuration space? phase space? - one is covering an awful lot of ground. One might include in it virtually any topological algebra and any Lie group; or, for that matter, classical phase space; they are all differentiable manifolds. The emphasis had better be on *spacetime*, on the sort of unity conferred on space and time by Minkowski's formulation of special relativity.

But unlike quantizability, here Cao's earlier chapters on the geometric program are quite helpful. Recall that Einstein considered general covariance to be a generalization of the relativity principle of the special theory of relativity. It certainly played an important role in the genesis of his theory of gravity. But Einstein's understanding of the principle underwent important shifts in this period. In 1918 he first annunciated an alternative principle that he imputed to Mach, and that by his own admission he had not previously distinguished from the relativity principle. The principle was this: "the g-field is completely determined by the masses of the bodies....[it] is conditioned and determined by the energy tensor of matter." (p.74). According to Cao, "Einstein held that satisfaction of Mach's principle was absolutely necessary because it expressed a metaphysical commitment concerning the ontological priority of matter over spacetime". Cao also considers that Einstein was led by the hole argument[3] to the view that "the physical reality of spacetime was constituted by the points of intersection of the world lines of material points"; that, indeed, general covariance can be seen as an expression of this principle. Cao concludes:

> Thus, from its genesis, general covariance was less a mathematical requirement than a physical assumption about the ontological relationship between spacetime and the gravitational field: only physical processes dictated by the gravitational field can individuate the events that make up spacetime. In this sense, [general covariance] is by no means physically vacuous. (p.74)

Against the criticism made by Kretschmann, and repeated often since, that any space-time theory, including Newton's theory, could be cast into generally covariant form, Cao objects that the absolute structures present in such theories "makes the apparent [general covariance] in their formulations trivial and physically uninteresting".[4] While Cao is prepared to grant that general relativity does not really give support to Mach's principle, and admits that Einstein drew back from it in its original form (call it MP_1), he is prepared to defend a modified version of it (call it MP_2), a view he attributes to Einstein as well:

> MP_2, held by Einstein in his unified field theory period, says that spacetime is ontologically subordinate to the physical reality represented by a total substantial field, among whose components we find gravitational fields, and that the structures of spacetime are fully determined by the dynamics of the field....with respect to the relation between spacetime and the field, Einstein's later position was still Machian in spirit. The only difference between this position (MP_2) and Mach's (MP_1) is that Einstein took the field rather than ponderable matter as the ultimate ontology that gives the existence and determines the structures of spacetime. (p.81)

The point of all this is that the ultimate ground of the geometrical program turns out to be the *field* concept:

> Space of [Minkowski] type ...is not a space without field, but only a special case of the *g* field, for which ....the functions *g* have values that do not depend on the coordinates. There is no such thing as an empty space, i.e. a space without field. Space-time does not claim existence on its own, but only as a structural quality of the field. (Einstein 1952a).

Cao can quote plenty of other comments of Einstein's, early and late, in support of this view. Cao's claim, as he succinctly puts it, is that Einstein did not geometrize the theory of gravitation; he gravitized the geometry of spacetime. On this basis Cao is entitled, in his closing arguments, to opt for the program that he attributes to Einstein: 4-dimensional spacetime geometry should emerge as an expression or aspect of the dynamical structure of a system of fields. Given this, the ambiguity as to what exactly counts as an *extension* of space-time is not terribly important; all that matters is that we *do* recover space-time from the fields, regardless of whether or not they bring with them some other kind of geometry as well. With that, indeed, he is in a position to tell a story very different from the anti-accumulativist one told by Kuhn and Laudan. For it is plausible that one might recover this geometry from a *quantum* field theory and only from a classical one; this is, after all, exactly what physicists were trying to do in the '60s and early '70s in perturbative quantum gravity. They were, moreover, treating gravity exactly as a kind of gauge theory, so there are obvious links with the GFP; the hope that we may find a synthesis of GP, QFP and GFP, of the indicated sort, is entirely reasonable. This would, very likely, make for an "ontological synthesis" in terms of which the structures important to each program - geometrizability, quantizability, and internal symmetries - find themselves newly organized, and thereby in part transformed, just like Cao said. This is Cao's "analogical extension of a theoretical ontology". It is a theory of everything, a single theory which unites gravity and the standard model.

But if *this* is Cao's response to Kuhn and Laudan, it misses the point entirely. He has altogether lost sight of the opposing view, that general relativity is actually quite *different* from other field theories; that it is *not* a field theory in any usual sense of the term. Perhaps general covariance is *not* like other gauge groups. Instead, perhaps general relativity is a dynamical theory of *geometry*. And indeed, we have no idea how to quantize the full theory. Hitherto quantization has always made use of special spacetime symmetries; for example, in algebraic local field theory, Lorentz symmetry is used to define the commutators; in path-integral approaches, it is used to define the Wick rotation (I shall come back to this later). What happens when the light-cone structure is itself changing, as a part of the quantum dynamics?

There are of course well-known strategies which circumvent or solve these difficulties in special cases (the ADM formalism, for example). Cao is pointing to a rosy future, in which they properly generalize (or in which we learn to make do with those special cases). Quantum theory and gravity, he is predicting, will be unified along lines sketched out in the '60s. But in truth nobody has had much success with this program. The proper marriage of general relativity and quantum theory remains to be found. The problems obstructing it have turned out to be much deeper than they had originally seemed. Think of the "problem of time" in the canonical formalism; think of the violation of unitarity in black-hole evaporation.

I have great sympathy with Cao's project. I hope it, or something like it, will eventually be brought to completion. It would represent a definitive victory for a universal conception of physics and of the goals of physics. It would give us a complete and final theory of everything. But this is *only* an aspiration. There remains the alternative view. Perhaps quantum theory and gravity cannot be unified in anything like their current form. Perhaps gravity is fundamentally unlike field theory, and, consistent with the history of failure to date, it may be that nobody is going to be able to quantize it. Perhaps these differences are so great that we will see in the journals increasingly wide-ranging and seemingly irresoluble disputes, typically *philosophical* disputes, that Kuhn always *said* we would see in the pre-revolutionary stage, in the prelude to a scientific revolution; of a sort that in fact we *are* starting to see. Maybe we have a scientific revolution in the making; maybe it will be as dramatic as the one leading from classical to quantum theory, and maybe a whole lot of dross will shortly be falling to one side (or what will then seem to us to be dross, once we have “gone native”, as Kuhn put it, and “converted” to the new theory of gravity).

The difficulty with Cao’s claims is not that the picture he is offering is not a plausible one. It is that he offers no arguments against alternative pictures. The alternatives are not, in point of fact, so much as considered. But there are any number of other ways that things could turn out, which would lend support to Kuhn’s epistemology instead. What is the point of speculating about it?

Realism Reconsidered

Cao’s structural realism is too strong and too weak. It is too strong because it depends on the convergence of *programs*; the structures that he identifies are immediately linked to research programs, so that to preserve them (the impression is created) these programs had better be brought to fruition. And it is too weak because he is not telling us *enough* about structural properties of dynamics. For example, he is not making enough of the fact that the geometrical concepts underlying the gauge principle apply equally to the quantum and classical theories; he does not make enough of the continuity, not with some unknown and final theory, but between the theories that we presently have.

This claim is less interesting when the theories are all of a piece. Focusing as Cao does on the history of quantum field theory, continuity is hardly unexpected: this is surely a history of *normal* science, in Kuhn’s terms; continuity here is the norm.

It is becoming clearer how Cao’s second thesis, the one that is entirely implicit, makes itself felt: Cao thinks nothing has to be said about the interpretative problems of quantum theory; he thinks questions about the relationship between quantum theory and classical theory can go unexamined. Yet it is exactly here that Kuhn’s thesis of incommensurability comes most directly into play. Kuhn denies that classical and quantum theory can be systematically related; if it can be shown that quantum mechanics really can describe classical phenomena as a limiting case (or even that it has to be *modified* so that it can), Kuhn’s epistemology will have been found severely wanting.

But let us grant Cao his thesis that the problem of measurement had no influence on mainstream developments in physics, and consider, with Cao, only the mainstream. Consider further examples of purely formal correspondences between classical and quantum theories. Is it true - the problem of measurement to one side - that the question of ontology was really important to them? Here is an elementary example. Consider the classical field ψ, with complex conjugate $\psi*$; the 1-particle Schrödinger state-vector ψ (or “bra”, an element of the Hilbert space of square-integrable functions on R^3) with dual vector $\bar{\psi}$ (the “ket”); and the quantum scalar field ψ, with adjoint $\psi^\dagger$, all of them non-relativistic. I use the same symbol ψ for them all intentionally; likewise use the common symbol ψ^c for complex conjugate, dual, and adjoint. The theories involved then have a number of formal expressions in common. They are listed in Table 1.

Expression	ψ =Classical Field	ψ =!-Particle State	ψ =Quantum Field
$\int \psi^c(x)\psi(x)d^3x$	Total Matter	Total Probability	Total No. Operator
$\psi^c(x)\psi(x)d^3x$	Matter Density	Pos. Prob. Density	No. Density Op
$\int \psi^c(x)(-i\hbar\nabla\psi(x))d^3x$	Total Momentum	Total Momentum	Total Mom. Op.
$\psi^c(x)(-i\hbar\nabla\psi(x))$	Mom. Density	Probability Flux	Mom. Density Op.

Table 1

Another column could be added: in each case the same formal expression can be derived in the 1-particle theory, as expectation values of appropriate 1-particle operators, in any state ψ; and in quantum field theory, as expectation values of field operators in any 1-particle state ψ.

Evidently there is considerable ambiguity, from a formal point of view, as to just what the quantities in the left-hand column really mean. Cao devotes quite a lot of space to getting clear on the distinctions of the table, and in which column, if any, *material waves* are to be entered. He is more than a little critical of early practitioners of QFT because they confused them. But he recognizes that ambiguities on this score can actually help with the development of the theory, for "any clear-cut and unequivocal use of this concept [of material wave] would cause serious conceptual difficulties" (p.152). I think he is exactly right with this remark, but it needs explaining; why would it cause difficulties? Cao does not say; but I suspect it is because the question of ontology was *not* germane to these new ideas. It was important *not* to say just what these expressions really stood for. Physics, as so often in its developments, operates better with opportunistic reasoning than with systematic analysis.

In the relativistic case, in fact, formal expressions of these concepts - involving classical fields, 1-particle states, and field operators - no longer coincide. The 1-particle states obey different equations (insofar as they obey any) from the fields; there isn't a probability interpretation for particles in space (or spacetime); complex numbers at the level of the fields are different from the ones entering into the Fock space states[5]. But these distinctions are hardly heeded by physicists even today. And making these precise and clear distinctions, in relativistic quantum field theory, are we getting any clearer about ontology? Not really. In the relativistic case the Fock space formalism is quite unwieldy; the more flexible and fundamental tool is the path-integral. We are calculating propagators, Green's functions, correlation functions, quantities that make sense from the point of view of the statistics of fields. So what in this case are the fields? One is typically computing expressions of the form (the *generating functional*):

$$Z[J] = \int e^{i\int (L[\psi]+J\psi)d^4x} D\psi. \tag{1}$$

The integral is performed over the space of field configurations ψ (with measure $D\psi$ - I shall come back to the definition of this measure later). In effect one is adding the phases (as determined by the Lagrangian density L with external source J) evaluated along all possible field-histories; those far from the classical solution typically (but not always) cancel. What is this external source, and what are these fields? Do they really exist? Are they part of the ontology? Functional differentiation with respect to the external source J yields the quantities of physical interest; for example, the 2-point correlation function:

$$< 0 \mid T\phi(x_1)\phi(x_2) \mid 0 >= Z[J]^{-1}\left[-i\frac{\delta}{\delta J(x_1)}\right]\left[-i\frac{\delta}{\delta J(x_2)}\right]Z[J] \mid_{J=0} .$$

On the LHS I have written this quantity in the traditional way, as the expectation value of the q-number scalar field in the vacuum state $\mid 0 >$ (T is the time-ordering operator); the external source does not occur at all (after differentiation, it is equated to zero on the RHS). So presumably J is only a mathematical artifact? It is the traditional formalism which is genuinely, ontologically interpretable? The expression on the LHS is often interpreted as the probability for a particle to be created at x_1 and propagate to x_2 , where it is destroyed; now, surely, we have the real meaning of the correlation function. Not so: the concept of particle position is not well-defined in relativistic quantum theory; there is no position operator, and no eigenstates of position (not even in the generalized sense of "eigenstate")[6]. Physicists, then, routinely *misinterpret* the LHS, but apparently without any penalty.

The lesson, it seems, is that the correlations are what are physically significant, not the elements of the mathematical machinery used to compute them, whether states, operator fields, or classical fields. And what, precisely, the relata are, can be left open (I shall come back to this point later, in the context of the measurement problem). So long as one knows how to relate the correlations to measurement procedures, on the one hand, and to the more abstract and formal principles of quantum mechanics, on the other, it seems that the question of ontology that Cao raises - what are the *material waves* - can go by the board. And this ultimate, ontological question to one side, there is structural continuity aplenty.

We may guess at the ultimate constituents of dynamical processes, be they particles or field-values or quantum analogs of either or both; in terms of them the structures just considered will be interpretable one way or another; but questions of realism, of the progressive nature of physical theories, need not depend on stability at this deeper, underlying level.

Renormalization and Critical Phenomena

I have hinted at aspects to dynamics common to quantum and classical theories; if dynamical structures are what physics is really about, it is time I gave a serious example of one. I shall use the same material as does Cao, but I will organize it in rather differently.[7]

To this end consider renormalization theory in relativistic particle physics and critical phenomena in condensed matter physics. The latter, recall, involve thermodynamic variables like the viscosity and density of a fluid, or the magnetization of a ferromagnet, quantities which change abruptly across a boundary (such as the liquid-solid interface). These boundaries mark out different phases of the thermodynamic variables. As other parameters are varied, e.g. pressure or temperature, differences across the boundary vary as well. Usually they can be brought to zero, defining a (second-order) phase transition or *critical point*. Viewed from the other direction, the critical point is is a point at which a single homogeneous phase bifurcates into two distinct phases. Vewed from the microscopic up, it is a point at which small changes in local microscopic quantities are amplified up to the macroscopic level; we are seeing the sudden emergence of long-range forces. All statistical and thermodynamic properties in this neighborhood are called *critical*.

One of the most simple examples is the case of an isotropic magnetizable medium. For simplicity, suppose we have only a single axis of magnetizability, with total magnetization M along this axis. From general thermodynamic considerations, if we apply an external magnetic field H, we expect the Gibbs free energy G to vary with M at constant temperature T as:

$$\frac{\partial G}{\partial M} \;|_T \;= -H \tag{2}$$

As the medium is cooled below the critical temperature T_C it spontaneously magnetizes in the direction H; above this temperature these two phases (for the two directions of H) disappear. Given isotropy, indeed, the form of G will be as shown in Fig.1. Since for $T \approx T_C, M \approx 0$, then close to T_C we should be able to expand G as a Taylor series in M:

$$G(M) = A(T) + B(T)M^2 + C(T)M^4 + \ldots$$

where, to preserve G under the symmetry $M \to -M$, the coefficients of odd powers of M must vanish. For $H = 0$ the minima $M_\pm$ of G are found by solving:

$$0 = \tfrac{\partial G}{\partial M} = 2B(T)M + 4C(T)M^3.$$

Evidently the only non-trivial solution is if $C$and B have opposite signs, as shown in Fig.1. To a first approximation we can take

$$B(T) = b(T - T_C), \quad C(T) = c.$$

In that case the minima are given by:

$$M_{\pm} = \begin{cases} 0 & \text{for } T > T_C \\ \pm\sqrt{\frac{b}{2c}(T_C - T} & \text{for } T < T_C \end{cases} \tag{3}$$

In the case of non-zero H, to satisfy Eq.(2) we have

$$G(M,H) = A(T) + B(T)M^2 + C(T)M^4 - HM + \ldots \tag{4}$$

The minimum of $G(M,H)$ with respect to M at fixed H gives the value of M that satisfies Eq.(2) at that value of H.

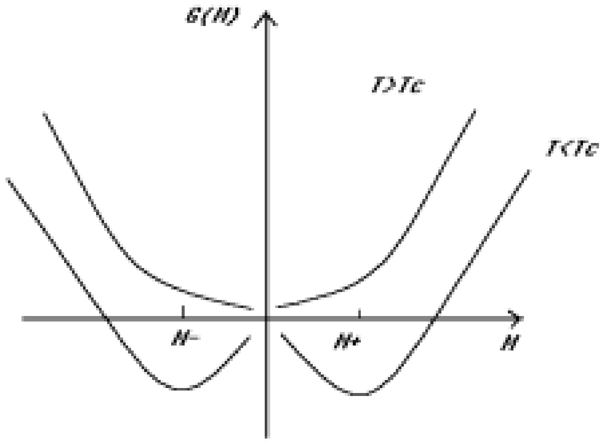

Fig.1

Eq.(3) shows how the magnetization *scales* with $T_C - T$, in this case going as the square root (so with *critical exponent* 1/2). Thinking of this in statistical mechanical terms, the *correlation length* ξ, the range of correlated spin fluctuations (which give rise to the magnetization), must also scales with $T_C - T$. In fact it is not hard to show that:

$$\xi = \frac{1}{\sqrt{2b(T - T_C}}. \tag{5}$$

ξ goes to infinity at the critical temperature. And now the really crucial point: the analysis is *just the same* for an enormous range of phenomena, not necessarily even involving magnetism. The coefficients *b*,*c* will depend on the particular medium, and the quantity involved (whether magnetism or mass density or viscosity or whatever), but the scaling laws (and specifically the critical exponents) all turn out to be unchanged. This theory, due to Landau, is *universal*; indeed, it depends on little more than symmetry, elementary thermodynamics, and the use of power-series expansions for the relevant functions.

It is all the more remarkable, then, that the critical exponents derived in this way, although in approximate agreement with observed scaling laws, depart from them systematically. Something is wrong with the Landau theory.

Consider now relativistic quantum field theory, an apparently entirely different theory with an entirely different subject-matter. As is well-known, calculations of non-trivial physical quantities invariably lead to dlivergent integrals (increasing without bound with the frequency). To remedy this it is usual to introduce a high-frequency cut-off Λ. Choose a real number r in the range $(0, 1)$, and introduce new variables as follows:

$$\psi = \begin{cases} \psi(k) \text{ for } 0 \leq |k| < r\Lambda \\ 0 \quad \text{otherwise} \end{cases}; \qquad \hat{\psi} = \begin{cases} \psi(k) \quad \text{for } r\Lambda \leq |k| < \Lambda \\ 0 \quad \text{otherwise} \end{cases}.$$

The ψ's are the low-energy configurations of the field, the $\hat{\psi}$'s the high-energy (in comparison to Λ); the ψ's are the ones we are interested in. Write the original fields in terms of $\psi + \hat{\psi}$, and compute the physical quantities of interest as functionals of the ψ's. One can then show that the same quantities can be obtained beginning from a new Lagrangian, containing additional interaction terms in the ψ's, but which does not contain the fields $\hat{\psi}$ at all; the influence of the latter are provided instead by the additional interaction terms in the ψ's. This new Lagrangian has new values for the mass and coupling constants. We can now continue this procedure, integrating over a another shell of momentum space (keeping Λ fixed); in the limit in which these shells are infinitesimally thin (for $r \to 1$), we obtain a continuous transformation of the parameters of the theory. This is the *renormalization group flow*, as formulated by Kenneth Wilson in the early '70s, following the earlier work of Gell-Mann, Low, Kadanoff, and Fischer. If the difference in scale (between the one of interest, and the one set by Λ) is very large, then the change in these parameters (the difference between the *effective* mass and coupling constant $\overline{\rho_m}$ and $\overline{\lambda}$, and the *bare* constants m and λ) becomes very large; in the limit of an infinite cut-off, these shifts are infinite.

And now much is explained. In perturbation theory, working with the bare parameters, the difference (an infinite difference!) shows up immediately at the 1-loop level, which seemed to invalidate the entire philosophy of perturbation theory. But now it is clear there is an alternative, namely, to factor in the influence of the high-frequency contributions explicitly, in the "effective" Lagrangian (for a given regime of energy). If we do this the 1-loop contributions to the perturbation theory (for the effective Lagrangian) remain small.

From its definition, the point at which the mass and coupling constant vanish is invariant under the renormalization group flow; this is a *fixed point* of the flow. In its neighborhood the effective mass and coupling constants (also called the *running coupling constants*) obey very simple equations. These quantities $\overline{\rho_m}$ and $\overline{\lambda}$, extracting the dimensional parameter Λ^2 , are dimensionless functions of the momentum k. If the mass parameter in the original Lagrangian is ρ_m , i.e.

$$L = L_0 - \tfrac{1}{2}\rho_m \Lambda^2 \psi^2 + \ldots$$

then, as we integrate down from the cut-off scale set by Λ, we expect the running mass coupling $\overline{\rho_m}$ of the effective Lagrangian to grow. One is integrating out degrees of freedom, irreversibly destroying information on the high-frequency short-scale correlations, retaining only their influence on the low-frequency components of the field. (The renormalization *group* is poorly named; one cannot recover the high-frequency behavior from the effective Lagrangian.) When we integrate down to the momentum scale of the effective mass, i.e. to k for which $\overline{\rho_m}(k) \approx 1$, the mass scale is acting as the cut-off, and sets the correlation length accordingly. Usually this will happen for k^2 close to Λ^2 ; but if the renormalization group flow leads sufficiently close to a fixed point, $\overline{\rho_m}$ will approach unity very slowly, after many repetitions, and correspondingly at much lower frequencies. The correlation length will then be very large in comparison to the original cut-off Λ, and the field becomes long-range with respect to it. Precisely at the fixed point, we can imagine the range becoming infinite - equivalently, that the cut-off distance can be taken to zero. This is what one traditionally tries to do in particle physics: to take the frequency cut-off to infinity. As we see, whether or not a theory is renormalizable in the usual sense (of power-counting, ensuring the terms of the perturbation expansion are all finite), it had better be defined at a fixed point of the renormalization group if we are really to obtain a cut-off free theory.

What does any of this have to do with critical phenomena in condensed matter physics? Think of it in slightly more general terms. Suppose we model a dynamical system on a lattice. We can imagine this as computational tool, either as a model for a continuum QFT or - and now comes the switch - for a system of molecules. The lattice-spacing gives a cut-off in length, and equivalently (because small lengths are probed with high frequencies) a cut-off in frequency. In particle physics, wanting to get rid of the cut-off is like wanting to get rid of the lattice (it is like taking the lattice spacing to zero). But in condensed matter physics, there is a natural scale to this cut-off, set by the size and separation of the molecules of the medium. Generally the statistical fluctuations of the media are all small-scale, evening out rapidly over clusters of tens of molecules or less. *Except in the neighborhood of a critical point*. There, the correlations become very large in comparison to atomic dimensions; indeed, they become macroscopic. But to reach a critical point we have to adjust the temperature and other parameters carefully, just as for the scalar field we have to adjust the mass parameter, so as to approach close to a fixed point of the renormalization group: we adjust an energy density, or mean kinetic energy, and hence the temperature. *Evidently the renormalization group ideas can be taken over mutatis mutandis to condensed matter physics.*

To be specific, the equation for the running mass constant as a function of frequency is:

$$\frac{\partial}{\partial \log k}\overline{\rho_m} = (-2 + \gamma(\overline{\lambda}))\,\overline{\rho_m}$$

where γ is a function that can be computed in perturbation theory. In the free-field case (in the neighborhood of the free-field fixed point) γ is zero and the solution is:

$$\overline{\rho_m}(k) = const.\Lambda^2 k^{-2}.$$

For k of the order of the mass m, $\overline{\rho_m}$ should equal 1, so the constant is $\rho_m = (m/\Lambda)^2$. For non-zero γ, the solution should go smoothly over to the free-field case as $\overline{\lambda} \to 0$; we therefore obtain:

$$\overline{\rho_m}(k) = \rho_m(\Lambda/k)^{2-\gamma(\overline{\lambda})}.$$

Concerning the correlation length ξ, assuming $\xi \approx k_0^{-1}$, where $\overline{\rho_m}(k) \approx 1$ we obtain:

$$\xi \approx \rho_m^{-\nu},\ \ \nu = \frac{1}{2-\gamma(\overline{\lambda})}. \tag{6}$$

The analogous formulae obtained by the replacement:

$$\rho_m = m/\Lambda \to (T - T_C)/T_C$$

yields the scaling law

$$\xi \propto (T - T_C)^{-\nu}. \tag{7}$$

Comparison of Eq.(5) with (6) and (7) shows that for non-zero γ, the critical exponents in Landau theory undergo systematic corrections.

There is moreover a refinement of this theory. The function γ (and hence the coefficient ν) can also be computed for systems of scalar fields with $O(N)$ symmetry. In that case we find:

$$\frac{1}{\nu} = 2 - \frac{N+2}{N+8}(4-d) \tag{8}$$

where d is the number of space-time dimensions. These exponents are in close agreement with the measured values of ν for magnetic media of N fluctuating spin components.

These results are spectacular. A highly mathematical and abstract theory of high-energy particle physics applies directly to low-energy macroscopic thermodynamic phenomena. Or perhaps we should put it like this: we have a new dynamical framework, different from the classical thermodynamic one, which accounts for the observed deviations in the “structural” properties, the truly universal ones (at both the quantum mechanical and quasi-classical level).

So what is it that we have learned - is it a matter of *ontology*? Or is it something about the nature of dynamics, its s*tructure?* Cao devotes a number of chapters of his book to RQFT; in one or another place are to be found most of the elements that have just been assembled. In Section 8.8, devoted entirely to the renormalization group, he summarizes the Wilson theory. But he does not remark on it subsequently; he does not relate it to Landau’s classical model. He does not ask the most obvious questions: Why should these scaling laws of RQFT apply to macroscopic thermodynamic systems? Why, indeed, do they apply to so great a range of systems, from magnets to fluids and binary alloys, yielding always the same critical exponents (for fixed order of symmetry N)? I presume Cao is knowledgeable on this subject; he does not neglect it out of ignorance. He neglects it because it does not serve his philosophical purposes. It puts in question his emphasis on *ontology*.

Here is a partial answer to these questions. In the neighborhood of a fixed point in RQFT only a small number of terms in the effective Lagrangian survive, under repeated iteration of the renormalization group transformation; and those that do survive grow very slowly. As a result the low-energy behavior (compared to Λ) will be extremely simple; in many cases indeed we have the *free* field theory, with negligible nonlinear interaction. The only term (apart from the mass) that might be non-zero in the limit is the coefficient of the ψ^4 coupling, what we have denoted $\overline{\lambda}$. This will be true *whatever* the original Lagrangian, and whether or not the cut-off can be removed to infinity (that is, whether or not the field theory is renormalizable in the traditional sense). The mass and λ^4 couplings are in this sense *universal*; likewise their scaling laws are universal.[8]

All well and good, but we have yet to explain why the same scaling laws should apply to condensed matter. Might one go further? The same law-like relations will hold whenever there is some long-range behavior, relative to the cut-off scale, irrespective of the detailed dynamics at the cut-off, granted; does it not follow that they will apply equally to atomic and molecular systems, just as for elementary quanta, since atoms and molecules are quantum mechanical too?

This sort of answer would lend support to Cao’s emphasis on ontology; we would have structural properties of the subject matter of a single theory (the ontology of RQFT). But this answer cannot in fact be correct. The value of the parameter d in Eq.(8) gives the lie: we only obtain corrections to the Landau exponents in *three* space-time dimensions. The reason that results in RQFT apply to condensed matter physics is not because atoms and molecules are at bottom composed of field quanta; rather, it is because of a correspondence - a *structural* correspondence - between RQFT and classical statistical mechanics. Specifically, there is a natural relation between the correlation functions of RQFT in *d space-time* dimensions, and the correlation functions of a classical field theory in *d spatial* dimensions.

We can see this directly in the case of the classical Landau theory. Eq.(4) translates readily into the language of classical fields; the magnetization M is given by the spatial integral of a local spin-density:

$$M = \int s(x) d^3x.$$

Then the Gibbs free energy corresponding to Eq.(4) will be of the form[9]:

$$G = \int (\tfrac{1}{2}(\nabla s)^2 + b(T - T_C)s^2 + cs^4 - Hs) d^3x \tag{9}$$

Consider again the generating functional of the correlation functions of a RQFT, Eq.(1). For ψ^4 theory, the exponent is:

$$\int (L[\psi] + J\psi) d^4x = \int \left(\tfrac{1}{2}(\partial_\mu \psi)^2 - \tfrac{1}{2}m^2\psi^2 - \frac{\lambda}{4!}\psi^4 + J\psi \right) d^4x \tag{10}$$

If we now transform the space-time coordinates according to:

$$x^2 = t^2 - |\vec{x}|^2 \to -(x_0)^2 - |\vec{x}|^2 = -|x_E|^2 \tag{11}$$

that is, we transform to *imaginary time*, yielding a *Euclidean* 4-*dimensional metric*; we then obtain in place of Eq.(10):

$$\int (L_E[\psi] - J\psi) d^4x_E = i\int \left(½(\partial_{E\mu}\psi)^2 + ½m^2\psi^2 + \tfrac{\lambda}{4!}\psi^4 - J\psi \right) d^4x_E.$$

We see, as before identifying the mass m with the deviation from the critical temperature $T - T_C$, that we have recovered the Landau expression Eq.(9) for the Gibbs free energy in a 4-dimensional space (multiplied by i). If we had started with a Minkowski space theory in 2+1 dimensions, we would have recovered the *three* dimensional Euclidean version of it. The transformation of Eq.(11) is called the *Wick rotation*. It had long been used as a calculational tool, since the additional factor i yields a new generating functional $Z_E[J]$ with *real and negative* exponent:

$$Z_E[J] = \int e^{-\int (L_E - J\psi) d^4x} D\psi. \tag{12}$$

The action as defined by Eq.(12) is bounded from below, so this measure over classical field configurations can be given a definite mathematical meaning.

The Wick rotation has proved to be a powerful tool in relating particle physics to thermodynamics (for example, in deriving the relation between the entropy of a back hole and the surface area of its event horizon). Using it we can apply the relativistic theory to (non-relativistic) critical phenomena, for Eq.(12) describes the generating functional of a classical statistical field theory with spatial dimensions equal to the spacetime dimensions. And we can explain why we arrive at a classical theory too, for the Lorentz group in $d - 1$ spatial dimensions becomes, on replacing t by it, the rotation group $O(d)$ in d Euclidean dimensions. But if relativistic fields commute in space-like directions, then, after the Wick rotation, they will have to commute in time-like directions as well, since the one direction can be rotated into the other under the group $O(d)$. Given microcausality, then, the Wick-rotated fields always commute; so we automatically obtain models for classical equilibrium systems in d (Euclidean) dimensions. We arrive at universal physical applications at a completely different level. Although there is no fixed point for $d = 4$ (as follows from Eq.(8)), the one for $d = 3$ (the Wilson-Fisher fixed point) still makes itself felt, but now in condensed matter physics.

Since there is no non-trivial fixed point in 4 dimensions, $\lambda\psi^4$ theory cannot be defined at arbitrarily high frequencies. We cannot take the cut-off to infinity. In the old, fundamental, ontological sense, there is no such quantum field. But there is a new sense in which there is such a fundamental field, and indeed a universal field: but this is surely to do with a structure to dynamics, rather than to a particular ontology.

Realism and the Problem of Measurement

Questions of realism can be distanced from the problem of measurement, but I do not think they can be divorced from it. If I am right and these structural features of dynamics are the ones which will be stable under theory change, then they had better not be sensitive to modifications of quantum theory (if any are needed) to solve the problem of measurement. The solution, supposing there is one, had better leave in place relativity theory, and it had better respect the distinction between quantum and classical as drawn in terms of commutators. It had better not make of the success of quantum mechanics and relativity theory a *mystery*. Best of all worlds, it will make use of just those structural features to phenomena, quantum and classical, that we have attended to. In fact, among the creators of the Euclidean theory, there are indeed those who thought that the Wick rotation held the key to the solution to the problem of measurement. In imaginary time, the Schrödinger equation goes over to the diffusion equation. Edward Nelson, who pioneered many of the connections between Feynman path integrals, the Schrödinger equation, and Markov processes, tried to interpret quantum mechanics as a classical stochastic theory at least implicitly in the light of this (Nelson 1967). Kenneth Wilson looked for links with critical phenomena for this reason.

I earlier remarked that Cao holds out the prospect of a grand unification of gravity with RQFT, but that this begs the question as a response to Kuhn *et al*. The lesser, more modest continuity that I am holding out for depends on much less. But it does, I suggest, require a conservative solution to the problem of measurement, which leaves in place the local Minkowski space symmetries, at least at the regimes of scale so far probed in particle physics. And this requirement is likely to have bite; a preferred frame of reference appears to be required not only of state-reduction theories but also of the one deterministic hidden-variable theory so far discovered (the pilot-wave theory of De Broglie and Bohm).[10]

Nelson's stochastic theory may not have been successful, but there are other, genuinely conservative solutions to the problem of measurement, which may yet be. These leave in place the principles of special relativity; they are, moreover, decidedly *structural* in flavor.

What is the problem of measurement? Cao does not comment on it directly, because he will only consider questions of interpretation "that acted as guides in the historical development of quantum physics". He does not avoid it entirely, for of course the problem of measurement certainly did have a part to play in wave-particle duality, to which Cao devotes quite a lot of attention; but he avoids one of the core questions, namely: in what sense can quantum mechanics describe the *actual outcomes* of experiments? The conservative approaches to the problem of measurement that I have in mind tend to downplay the significance of outcomes *per se;* the suggestion is rather that we can make do with a description of the *kinds* of outcomes - with their *structure* - either because the outcomes actually observed somehow go beyond what a physical description can really provide for (as, say, Omnès, or Mermin seem to claim), or because they are only a *part* of what a physical description provides (as in "many-worlds" interpretations). They have in common the appeal to decoherence theory - the theory, that is, of how classical (or "quasiclassical") equations can be defined in quantum mechanical terms - whether for "open" systems (sub-systems of the total system), or for certain ("decohering") bases.

As a solution to the problem of measurement decoherence theory has plenty of critics, for surely none of these structural properties tells us what we naively want to know: what quantum fields are *really* made of, what elementary particles *really* are, how they *really* move in space; what probability *really* is. And what it does tell us is tendentious: applied to closed systems, we learn when the pure state yields (almost) the same probabilities as does a mixture for a basis well-localized in space, albeit it is an "improper" mixture. We do not obtain one term or the other as the actual one, we obtain only the form or structure of events; as Mermin puts it, we obtain the correlations without the relata; the sort of outcome but not the outcome itself (Mermin 1997).[11] Is the recovery of the classical, as only form or structure, real enough for realism? But it may be real enough for *structural* realism.

Let me make clearer what this form or structure amounts to. An example is the result recently proved by Halliwell (1998), confirming a conjecture due to Gell-Mann and Hartle. He showed that locally-conserved densities are decoherent, in the sense of the decoherent histories theory, and that probabilities for their histories are strongly peaked about hydrodynamic equations.[12] Decoherence in this sense is in fact a condition for the interpretation of a history in probabilistic terms: that, for example, for mutually exclusive events A, A', the probability of "A, then B", plus the probability of "A', then B", is the same as the probability of "A or A', then B". This is a rather fundamental connection between probability and classicality. It is also consistent with Bohr's correspondence principle. Cao remarks on the latter principle in passing; he says it explains "the referential continuity between the quantum field and classical field in their structural properties" (p.363). This principle, like Ehrenfest's theorem and Mott's treatment of ionization trajectories, can be considered an early result in decoherence theory. But there is a difference: what Halliwell obtains is not so much the equations of classical electrodynamics as classes of equations which include thermodynamics and hydrodynamics as well. Once again, it is not at all clear that we have continuity at the level of ontology, so much as in terms of structure.

We can allow that there may be surprises in quantum gravity, of a kind that do not fit in with viewing it as a gauge theory, or even as a field theory, without jeopardizing the weaker version of structural realism that I think can be grounded on extant theory. Weak as it is, it shows that something is wrong with Kuhn's epistemology; it is an understranding of dynamics that well beyond the purely observational, and which incorporates both classical and quantum ideas. Viewed in this light, physics is clearly progressive. So much is obvious if we consider the progression from field theory to gauge theory, and on to canonical quantum gravity (if that theory were to make any sense); but my point is we do not need smooth changes like this to make sense of progress more generally. Realism does not hang in the balance in consequence.

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Endnotes

1. Philosophy Centre, 10 Merton St., Oxford, OX1 4JJ, http://users.ox.ac.uk/~0174/Saunders.html.
2. For a clear and very thorough review of the history of this tradition, see Gower (1998).
3. The argument concerned the physical significance of coordinates, and the individuation of points of space-time; it has been much discussed following the paper of Earman and Norton (1987).
4. Here Cao follows Stachel (1993), who denies that the hole argument (which can be formulated in any generally covariant theory) has in itself any significance for the individuation of space-time points. I disagree with Stachel on this (see my 2001), but I shall not labor the point here.
5. Only the former are locally defined; the latter involve the decomposition into positive and negative-frequency parts, which is a global operation. For further background, see my (1991, 1992).
6. See Malament (1996). For diagnosis and therapy, see my (1994).
7. I will also supply the equations, following the notation of Peskin and Schroeder (1995). Cao's book has very few equations, which may be one of its virtues; I did not find it so, however.
8. Cao has little to say on this in his book, although he has touched on it briefly elsewhere (Cao and Schweber 1993, p.60-61).
9. Since this is to be minimized to find the field *s*, the first term is a simple way to force nearby spins to align with one another.
10. Insofar as there is any good relativistic theory of this sort. See my (1999) for further discussion.
11. For an accessible review of the general ideas of decoherence theory, see Zurek (1991). But decoherence is not of course the whole story; for its shortcomings, see the exchange of letters which followed (Zurek 1993).
12. The result is well-known in the case of local equilibrium states, but its extension to certain non-trivial superpositions of the local densities is new.

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