In this paper, I propose a project of enlisting quantum information science as a source of task-oriented axioms for use in the investigation of operational theories in a general framework capable of encompassing quantum mechanics, classical theory, and more. Whatever else they may be, quantum states of systems are compendia of probabilities for the outcomes of possible operations we may perform on the systems: “operational theories.” I discuss appropriate general frameworks for such theories, in which convexity plays a key role. Such frameworks are appropriate for investigating what things look like from an “inside view,” i.e. for describing perspectival information that one subsystem of the world can have about another. Understanding how such views can combine, and whether an overall “geometric” picture (“outside view”) coordinating them all can be had, even if this picture is very different in nature from the structure of the perspectives within it, is the key to understanding whether we may be able to achieve a unified, “objective” physical view in which quantum mechanics is the appropriate description for certain perspectives, or whether quantum mechanics is truly telling us we must go beyond this “geometric” conception of physics. The nature of information, its flow and processing, as seen from various operational perspectives, is likely to be key to understanding whether and how such coordination and unification can be achieved.

“The subject...had in fact become an obsession and, like all obsessions, very likely a dead end. It was the kind of favorite puzzle that keeps forcing its way back because its very intractability makes it perversely pleasant.... I hoped that some new image might propel me past the jaded puzzle to the other side, to ideas strange and compelling.”

—E.O. Wilson
The Diversity of Life

I. INTRODUCTION

The central question quantum mechanics raises for the foundations of physics is whether the attempt to get a physical picture, from “outside” the observer, of the observer’s interaction with the world, a picture which views the observer as part of a reality which is at least roughly described by some mathematical structure which is interpreted by pointing out where in this structure we, the observers and experimenters, show up, and why things end up looking as they do to observers in our position, is doomed. The “relative state” picture that arises when one tries to describe the whole shebang by an objectively existing quantum state is unattractive, and many seek to interpret quantum states instead as subjective, “information” about how our manipulations of the world could turn out. Whatever else they may be the quantum states of systems clearly are compendia of probabilities for the outcomes of possible operations we may perform on the systems: “operational theories.” Quantum information science concerns what one can do with the operations available to us quantum mechanically, and has shown the power of the formal analogy between quantum states and classical information (probability distributions), an analogy which is also at the heart of the “subjectivist” attitude towards quantum states. Quantum computation and information theory are making the movement between “inside” and “outside” points of view of measurement processes and other interactions, formulated in a general and rigorous way, a commonplace of physical practice.

In this paper, I will suggest a particular project for harnessing quantum information-theoretic and computational ideas to the task of resolving the central tension in the foundations of quantum mechanics. This project has the virtue that even if it does not succeed in resolving this tension, the technical results obtained in the attempt will still be valuable. The idea is to view quantum mechanics as a framework for a particular type of empirical, operational theories. Such a theory is, roughly, a compendium of operations (such as measurements) that can be performed on a system, together with a specification of the probabilities of their outcomes, usually called a “state.” Actually, I will sometimes refer to as an operational theory, what should perhaps be called an “operational theory-type”: a specification of the possible set of operations on a system, together with a specification of a set of possible compendia
of probabilities for results of these operations. An “operational” or “phenomenological” theory itself arises when a particular probability compendium—a particular state—is chosen. Some who view quantum states as subjective and have a generally Bayesian view of probability as well, have expressed discomfort with the notion of starting with probabilities as “empirically given,” but of course one can and should view such “phenomenological” probabilities as themselves established via Bayesian inference. It is true that, in the case of quantum mechanics, for example, our actual subjective probabilities about the outcomes of experiments might not coincide precisely with the quantum-mechanical ones, because even after taking all the evidence into account we might have some very small subjective probability that quantum mechanics is wrong. I don’t think this causes any major difficulties for the approach taken below, however.

The viewpoint just sketched is the “operational” or “operational quantum logic” point of view, and it can be useful whether one wants to consider the empirical theory as for some reason all we can hope for, or as a description of how perspectives look within an overarching theory such as the relative state interpretation (RSI). Therefore, technical results obtained using this point of view are likely to be valuable to both camps, and of interest regardless of the ultimate disposition, if any, of the problem of quantum foundations. Nevertheless, they could also turn out to have implications for the disposition of this problem.

The new light being shed by quantum information science on foundations has, so far, served primarily to put the old questions in brighter relief, to pose them more clearly and in some cases to provide a framework, information theory, in which the peculiar features of quantum mechanics can be expressed quantitatively. In a way, this just illuminates even more clearly the stalemate reached early on by those attempting to understand quantum foundations. The fundamental issue is the tension between quantum-mechanical descriptions of measurement processes from the “inside” and “outside” points of view: from the point of view of the measuring system (observer) or from outside both observer and observed. Is there an acceptable quantum-mechanical outside view, and if not, must be content, perhaps even view ourselves as lucky, to live without such an outside view? Most commonly, this takes the form of controversy between those espousing a “relative state” interpretation of quantum mechanics [34, 35], in which the state vector is interpreted as a real physical object, extendible to include whatever observers may be involved in a measurement, and those espousing an “information” view of quantum mechanics, in which the statevector describes, roughly, the information about possessed by an observer who is in a position to do experiments on a system, about the possible outcomes of those experiments and should not be interpreted as having any more objective nature than this. While it has in my view as yet not made a decisive contribution toward resolving this tension, by focussing on the role of information held (through entanglement or correlation) or obtained (by measurement) by one system about another, it concentrates one’s attention on the practical importance of such measurements, and develops flexibility in moving between the inside and outside views of such information-gathering processes. It thus provides tools and concepts, as well as the ever-present awareness, likely to be useful in resolving this tension, if that is possible. In the best scenario, it might help provide a “new image” of the sort hoped for in the epigram from E. O. Wilson that heads this section, to propel us, in his somewhat Morrisonian words, “past the jaded [sic] puzzle to the other side, to ideas strange and compelling.”

Before discussing the project of combining the operational approach with the new tools, images, and analogies provided by quantum information science (QIS) in the task of trying to coordinate quantum perspectives into an overall physical picture, I consider, in Section III, some salient general implications of QIS for foundational questions (irrespective of its contributions to this project). QIS’s most impressive achievements are due precisely to the peculiarities of quantum mechanics that have impressed quantum physicists since the beginning. While it may not have achieved any interpretational breakthroughs, I illustrate how it provides tools for sharper analysis of interpretational questions, raises some specific and somewhat technical questions involving computability, and draws attention to the fruitfulness of formal analogies between density matrices and probability distributions.

In Sections IV and V expand upon the basic interpretational tension in the foundations of quantum mechanics; this provides a necessary basis for the discussion that follows, in general terms in Sec. VI and in more particulars in subsequent sections, of how the marriage of QIP and empirical quantum logic might contribute to its resolution. (However, those whose only interest is in technical aspects related to operational theories might skip to Section VI.)

Section VI turns to what I consider a good framework, centered around probabilities, for empirical operational theories. I use a notion of probabilistic equivalence with a long history, that identifies outcomes of different procedures (as far as “their effect on the observer” is concerned) if they have the same probability in all states of the phenomenological theory. I show it allows one to derive, from a phenomenological operational theory, a more abstract structure, a “weak effect algebra,” closely related to (and probably completable to) a structure well-studied in operational quantum logic: that of “effect algebra.” I review these and related notions of empirical quantum logic. In Section 7 I review “convex effect algebras,” emphasizing how the modern “positive operator valued measures” (POVMs) approach to quantum mechanics (in terms of POVMs’s and trace-nonincreasing completely positive maps, aka “quantum operations”) is a case of these, and how foundational results involving it are naturally expressed in these terms, sometimes as cases of general results involving convex effect algebras. I discuss probabilistic equivalence
In a setting, and relate Gleason-type theorems such as the recent ones for POVMs, to general results locating the states in the dual of the cone on which the convex effect algebra can be represented.

In Section VII I sketch work in progress on how such a framework might be extended to incorporate dynamics conditional on measurement results, exhibiting the quantum mechanics not only of effects, but also of “operations,” so familiar now to physicists through its ubiquity in the theory of QIP, as an example of a structure I call an operation algebra, basically a weakening (to have multiple top elements, corresponding to the many possible trace-preserved operations) of the notion of effect algebra, with the addition of a product of operations, interpreted as doing the operations in sequence, with the second conditioned on the first. In Section VIII I discuss the combination of subsystems in operational theories, and its relationship to dynamics in such theories and to the “coordination of perspectives.” In Section IX I discuss dynamics further, showing that there is a potential difference between “Schrödinger/Liouville/von Neumann” dynamics that act on states, and “Heisenberg” type ones that act on an effect algebra, arguing that only those Schrödinger dynamics that have a Heisenberg representation should be countenanced, at least in theories whose state space is maximal. In Section X I discuss how this framework may be used in the project of applying QIP ideas to foundational questions. This involves several thrusts. One is to develop, from a general operational theories approach, an understanding of the various ways subsystems may be combined to form larger systems, or individuated as parts of such systems. If quantum theory is viewed as a framework for “perspectives” of one subsystem on another, such a theory of subsystem combination is essential to the project of combining such perspectives in an overarching “geometric” picture. The category-theoretic tensor product and similar (but “non-minimal”) constructions are of particular interest as one such way of combining subsystems, probably leading (if applied thoroughly to quantum mechanics) to a standard “relative-state” view as the overarching picture; but the consideration of other possibilities is probably necessary if one is to avoid the grotesque features of that “many-worlds” theory, and interestingly, it is suggested by some other attempts to resolve quantum puzzles via taking a “global” point of view on quantum theory: Schulman’s [1992] symmetric boundary conditions proposal, and the Rovelli-Smolin (84) approach to unifying quantum mechanics and gravity with a “topological quantum field theory” type construction (and the related “relational” approach to quantum mechanics 76). It could also turn out that rather than, or along with, non-tensor combination of objects in some situations, the appropriate category could change, with a quantum description (as might also be the case with the tensor product combination law) only being appropiate to perspectives in certain situations or limits.

Even if no such overarching picture turns out to be possible and our “empirical operational theories” of how things look from various perspectives are viewed as all we are going to get, the consideration of subsystem combination is important in the understanding of empirical operational theories. One may reasonably want to require that the operations we perform on a system should be allowed to use auxiliary systems as apparatus, and we should be able to include those systems with the observed system, and describe our perspective on the system and apparatus in the same general terms (e.g. quantum mechanics) as the system itself. Within standard quantum mechanics, certainly, this works.

A major part of the project involving “empirical operational theories” is, as it has been since long before quantum computation came on the scene, “deriving quantum mechanics.” The hope is that if this can be done with axioms whose physical, or better yet information-theoretic or information-processing, meaning is clear, then one will have a particularly nice kind of answer to the question “Why quantum mechanics?” This has also been a preoccupation of many of the QI/QC researchers who delve into foundations (e.g. 4, 44, 54, 55, 82). I believe that QI/QC will provide a useful new source of axioms, with natural interpretations involving the possibility or impossibility of various information-theoretic tasks. But, one may ask, how might this contribute to resolving the basic tension? I think it is likely to contribute to whichever mode of resolution turns out to be right. Within the “geometric” or “objective overall picture” resolution, one might obtain the answer: Why quantum mechanics? “Because it’s the sort of structure you’d expect for describing the perspectives [or at least certain perspectives, of the sort beings like us wind up with in situations considered in standard quantum theory] that occur “from the inside point of view” within an overarching picture of this [fill in the blank] sort.” The blank might be filled in with a fairly particular overarching physical theory, or the quantum formalism might arise from quite general features likely to be common to many more particular proposals. A very similar answer might arise from the more “subjectivist” point of view on quantum states. Why quantum mechanics? “Because it’s the sort of structure you’d expect for describing the perspectives [or at least certain perspectives...] that occur “from the inside point of view” within a reality of this [fill in the blank] sort, which reality is however not completely describable in physical terms, so that these perspectives are as good as physics ever gets. [Explanation of why perhaps supplied here.]”

From this point of view, both those who hope for an overarching physical picture, or think it likely to emerge, including relative state-ers who think quantum mechanics already provides its general outlines, and those who think such an overarching physical picture unlikely to emerge, can nevertheless fruitfully pursue essentially the same project. This is the project of using axiomatic arguments involving the notion of “operational theory” to derive quantum mechanics, to understand, conceptually, how it differs from or is similar to other conceivable possibilities for such theories, and
the extent to which the quantum structure does or does not follow from elementary conceptual requirements on such theories (one way in which it could be “a law of thought”) or, in a more Kantian or perhaps “anthropic” way, from the possibility of rational beings like us (a different way in which it could be “a law of thought”). Of course, one’s view on how the basic interpretational conflict will be resolved might influence the axioms one chooses to investigate, with the subjectivists perhaps more inclined to axioms stressing the formal analogies between density matrices and probability distributions, quantum “collapse” and Bayesian updating of probability distributions \(11\). (But since on the “overarching physical picture with perspectives” view the probabilities are also tied to a “subjective,” perspectival element, the Bayesian analogy might be quite natural on this picture too.)

It thus doesn’t seem necessary to me to commit in advance to an opinion on how the resolution will be achieved. What does seem likely is that however it is achieved, the close link between “empirical operational theories” and perspectival information that one subsystem of the world can have about another, suggests that in the project of understanding quantum mechanics as a framework appropriate to such perspectives, the concepts of quantum information theory and quantum information processing will play a major role.

II. QIP: THE POWER OF THE PECULIAR

Virtually all of the main aspects of quantum mechanics exploited in quantum information processing protocols have been understood for decades to be important peculiarities of quantum mechanics by those concerned with foundations. Often this understanding goes back, sometimes in an incomplete form, to the founders of quantum mechanics itself. \(18\) coined the term entanglement, and emphasized its importance and its relationship to the nonuniqueness of the decomposition of mixed quantum states into pure states, after Einstein, Podolsky, and Rosen \(1935\) drew out its implications of nonlocality. The importance of the idea that obtaining information about a quantum system necessarily disturbs it was emphasized by \(56\) and \(18\) (though the Heisenberg light microscope proved to be a misleading illustration in some respects, suggesting incorrectly a need for exchange of energy/momentum as the source of the disturbance). \(18\) emphasized complementarity. The nonlocal correlations allowed by entanglement can be viewed as precisely what is exploited by certain better-than-classical communication complexity protocols. \(20\); the necessity of disturbance when information is gathered on a genuinely quantum ensemble \(1, 6, 7, 10, 14, 43\), which is closely related to the “no-cloning theorem” \(95\) and no-broadcasting theorem \(8, 62\), is the basis of quantum cryptography; the ability to obtain information complementary to that available in the standard computational basis, by effectively measuring in a Fourier-transformed basis to obtain information about the period of a function, is the heart of the historic series of algorithms due to Deutsch \(30, 31\), Bernstein and Vazirani’s \(1997\) algorithm (and efficiently!) measuring in a Fourier-transformed basis to obtain information about the period of a function, is the heart of the historic series of algorithms due to Deutsch \(30, 31\), Bernstein and Vazirani’s \(1997\) algorithm for “recursive Fourier sampling,” (the first superpolynomial quantum query speedup in the bounded-error model), Simon \(83\), and culminating in Shor’s \(1994, 1997\) polynomial-time factoring algorithm. \(,\) also uses complementarity via the quantum Fourier transform. The significance of these peculiarities of quantum mechanics is brought out by quantum information science: these are no longer curiosities, paradoxes, philosophers’ conundrums, they now have worldly power: to break public-key cryptography, speed up (though not to polynomial time) brute-force search for solutions to, say, NP-complete problems; to give unconditionally secure verification that one’s communication has not been overheard. At the same time, the concepts are made quantitative, analyzed more precisely. Sometimes their essential nature is somewhat better understood, but in my view this has not yet led to breakthroughs in dealing with the interpretational issues they raise (which is not to say such breakthroughs may not come).

A number of more specific and/or technical points on which QIP has contributed, or shows potential to contribute, something new to old debates can be identified. Some of the more salient ones are:

1. QIP provides tools with which to analyze much more precisely and algorithmically questions of what can and cannot be measured, or otherwise accomplished, either precisely or approximately, in quantum mechanics. Foundational work sometimes tends to take a rather abstract view of quantum mechanics: systems correspond to Hilbert spaces, states to density operators \(\rho\) acting on those Hilbert spaces, measurements to Hermitian operators \(A\) (or decompositions of unity) on those Hilbert spaces, probabilities of measurement outcomes are given by \(\text{tr} \rho E_i\), where \(E_i\) is a projector onto the subspace with eigenvalue \(\alpha_i\) in the spectral decomposition \(A = \sum_i \alpha_i E_i\) of \(A\) into orthogonal projectors (or by a positive operator \(E_i\) in a resolution of unity satisfying \(\sum_i E_i = I\)). The question “can any Hermitian operator be measured?” has been a longstanding one in foundations. The Wigner-Araki-Yanase theorem \(2, 90\) sheds some light on it (a necessary condition for a Hermitian operator to be measured precisely is that it commute with the system’s free Hamiltonian), and also sheds some light on the resources (notably interaction strength) needed to approximately measure operators that can’t be measured precisely. \(73\) established that any operator could be measured using passive linear optics (assuming arbitrary real parameters describing idealized passive components can be achieved, of course an idealization but a useful one). Their scheme required exponential resources in the number of qubits (or log of Hilbert space dimension)
of the system to be measured. The theory of universal quantum computation shows, for the first time, that arbitrary operators can be approximated arbitrarily well (and efficiently in the desired precision), using a basis of local gates of size polynomial in the log of the dimension of the Hilbert space (i.e., the number of qubits).

(To forestall confusion: the claim is not that any unitary can be approximated using a circuit containing a polynomial number of gates; rather, the basis set of gates used can be taken to be polynomial (say, a set of $g_1$ one-qubit and $g_2$ two-qubit gates, applied on all qubits or pairs of qubits, for a basis of size $g_1 n + g_2 n^2$ for expressing operators on $n$ qubits.)) Another point that becomes clear when a computational approach is taken is that, given a computational (quantum algorithmic) model of what a measurement is, there will be Hermitian operators that are impossible to measure in the same sense that certain functions are classically impossible to compute (cf. [71], for example). Indeed, prime examples in infinite-dimensional Hilbert spaces are operators whose eigenvalues encode the values of an uncomputable function, in the “standard” computational basis. In finite dimensional Hilbert spaces, the issue is less clear, but there may well be operators that are unmeasurable essentially because some of their eigenvalues or eigenvectors are uncomputable numbers (these would probably have to be numbers that are not even computably approximable, like Chaitin’s $\Omega$). Exactly what all this means for the foundations of the theory, which are often taken to include the abstract (and false) statement that all Hermitian operators can be measured, is not clear, but there can be no doubt that it is important. It raises the issue of the extent to which “operational” limitations, including basic and highly theoretical ones such as computability, should be built into our basic formalisms, and what it means for the interpretation of those formalisms and the “reality” of the objects they refer to, if they are not.

2. QIP techniques and concepts promise to allow a much more systematic approach than previously to experiments and thought-experiments suggested by foundational investigations. Error-correction, active and passive stabilization and control techniques may allow one to construct and quantum-coherently control much larger-dimensional quantum systems than heretofore, so that proposals that quantum coherence is lost when systems become too large or too complex, or other theories that postulate collapse as a real physical process occurring in a specified manner under specified conditions, may be tested.

Indeed, some (I am not among them) may suspect that the great difficulty so far experienced in getting even a few qubits of clean quantum computing power is a manifestation of such fundamental difficulties with large-scale quantum coherence. Alternatively, we may come to see, if there arise insurmountable practical obstacles to implementing QIP, which are in turn understood as involving some general theoretical principles, that macroscopic quantum coherence is in some sense a “non-operational” concept. Whether that has repercussions for the interpretation of quantum mechanics (do both branches of Schrödinger’s cat “really exist” in some sense) depends on how operational one is inclined to require one’s ontology to be. In my case, not particularly so, i.e. I am prepared to countenance the existence of theoretical objects whose existence might be completely impractical, for theoretically clear reasons, to operationally verify. However, if one can get an adequate picture of things without them, so much the better.

3. QIP, especially quantum information theory (that branch of the theory of QIP which is an analogue of classical information theory, i.e. concerned particularly with asymptotic rates at which tasks such as data compression and transmission can be performed, either perfectly or as a function of a tolerable distortion level) has demonstrated the power of taking the formal analogy between quantum density matrices and classical probability distributions seriously. Many things one does with probability distributions in classical information theory (e.g. transmit messages from a source which emits the messages with a given probability distribution), have natural quantum analogues when quantum states replace probability distributions. This may have implications for foundations, as it goes nicely with the view that “the quantum state is essentially information, and therefore collapse is not a mysterious, seemingly causality-violating physical process, because it is not a physical process, but a subjective one involving information.” However, I will argue below that this view of collapse is essentially shared by the two main candidates for “interpretations” of nonrelativistic quantum mechanics (the relative state interpretation and the “no-interpretation” interpretation). Still, QIP thereby focusses attention on these two approaches, which I view as corresponding to “inside” versus “outside” views of quantum measurement processes, thereby throwing into sharper relief the fundamental issue in quantum foundations, which is whether the outside view suggested by quantum mechanics is really acceptable, and if not, whether it is acceptable to live without such an outside view.

4. There is a long-standing philosophical/mathematical project of trying to understand quantum mechanics, and especially how it differs from classical mechanics, in an abstract and axiomatic way. A goal of such an approach may be to find mathematically natural characterizations of quantum theory and classical theory, from within a framework of “empirical theories” more general than either one. Another goal is to find such characterizations that have clear physical interpretations. QIP can contribute to this project by providing a source of natural
“operational” questions about whether certain information-processing tasks can or cannot be performed within the model theory, which may well be related to natural axiomatic properties of the theory. Also, QIP may be a natural source of examples of such empirical theories. These naturally arise when one considers attempts to perform quantum information processing with restricted means, which may correspond to those operations that are easy to do within a certain proposed implementation of quantum computing (say, linear optics operations on a multi-mode bosonic Hilbert space, with classical feedback [58]), or to a theoretically natural restriction on quantum operations (say, those needed to encode, correct errors in, and decode stabilizer quantum error correcting codes). For example, QIP considerations stimulated some of us [11] to generalize the notion of “entanglement” to a lie-algebraic situation more general than that of standard multipartite quantum entanglement, and then to a “convex cones” setting reminiscent in some respects of convex sets approaches to quantum foundations [28, 49, 63, 66].

In the remainder of this paper, I will consider in detail this last way in which QIP affects the study of foundations. Inevitably, though, it interacts with the other three points. I will be especially concerned with the relation to the the inside/outside dichotomy, and relative-state versus the “subjective density matrix” interpretations, which I will relate to a tension between a “geometric” and “dynamic” view of physics discussed, for example, in [17]. (I learned of this paper from a recommendation by Chris Fuchs.) For it is here, I think, that one finds suggestions of what else one needs from physics to resolve the tension between these points of view, and ideas for a research program aiming toward that goal.

In order to help understand how this goal might be realized I will first present my take on the basic tension between “inside” and “outside” views of quantum mechanics, in the next section.

III. IN WHICH DOCTOR SCIENCE MEETS SUBJECTIVEMAN IN HIS LASER LAB, AND TURNS QUANTUM MECHANICS INSIDE OUT

“Day or night he’ll be there anytime at all (Doctor Robert)
You’re a new and better man
He helps you to understand
He does everything he can (Doctor Robert)
He’s a man you must believe
No-one can succeed like Doctor Robert.”

— The Beatles
Doctor Robert (Revolver)

“Don’t pay money just to see yourself with Dr. Robert”

— The Beatles
Doctor Robert (Revolver)

“The probability of the actual is absolute. That we have no power to
guess it out beforehand makes it no less certain. That we may imagine
alternate histories means nothing at all.”

— Cormac McCarthy
Cities of the Plain

The central tension in interpreting quantum mechanics is between the idea that we are part of a quantum world, made of quantum stuff interacting with quantum stuff, evolving according to the Schrödinger equation, and the apparent fact that when we evolve so as to correlate our state with that of some other quantum system which is initially in a superposition, we get a single measurement outcome, with probabilities given by the squared moduli of coefficients of the projections of the state onto subspaces in which we see a definite measurement outcome. The relative state interpretation attempts to reconcile these ideas by taking the view that the experience of obtaining a definite measurement result is simply how things appear from one point of view, our subspace of the world’s Hilbert space, and the full state of the world is indeed a superposition. As I see it the correct way, on this view, to account for the appearance that there is a single measurement result, is the idea that the experience of a conscious history is associated with definite measurement results, so that consciousness forks when a quantum measurement is made. Just as there is no consciousness whose experience is that of the spacetime region occupied by you, me, Halley’s comet,
and the left half of Georges Sand, so, after a measurement has correlated me with the the z-spin of an initially x-polarized photon, there is no consciousness whose experience is that of the full superposition (or, once these branches of me are decohered, of the corresponding mixture). A more precise account of why would appear to await a better scientific understanding of consciousness, though there are probably some useful things to be said by philosophers, psychologists, biologists, and decoherence theorists. It is deeply bound up with the problem of choosing a “preferred basis” in the relative state interpretation (i.e., the question, “relative to what?”), and also with the problem of what tensor factorization of Hilbert space to choose in relativizing states, which appears in this light as the question of which subsystems of the universe support consciousness. One suspects the stability of phenomena and their relations enforced by decoherence may underly the ability to support consciousness, in contrast with a situation where “Dr. Science” is constantly reversing our seeming perceptions, memories, etc... Philosophers have devoted quite a bit of thought to the conditions necessary for personal and mental identity to persist through time (for things to be identified as manifestations of “the same person” or “the same mind” at different times and places), and this work is likely relevant here. Just why the branching should be described by a stochastic process with probabilities equal to amplitudes squared is somewhat mysterious; but I will not delve too far into it here; some further thoughts on this matter, including an argument based on Gleason’s theorem (but which I now believe did not sufficiently motivate the assumption of noncontextuality) and some remarks on arguments based on tensor products and relative frequencies and an implicit assumption that probabilities should be continuous in the state vector, can be found in [3], [5] also develops my basic splitting-minds view of the relative state interpretation (which, I argue there, is a plausible assumption of noncontextuality) and some remarks on arguments based on Gleason’s theorem (but which I now believe did not sufficiently motivate the amplitudes squared is somewhat mysterious; but I will not delve too far into it here; some further thoughts on this matter, including an argument based on Gleason’s theorem (but which I now believe did not sufficiently motivate the assumption of noncontextuality) and some remarks on arguments based on tensor products and relative frequencies and an implicit assumption that probabilities should be continuous in the state vector, can be found in [3], [5] also develops my basic splitting-minds view of the relative state interpretation (which, I argue there, is a plausible interpretation of what Everett himself had on the subject). Several authors, notably Saunders [77] and Wallace [87, 88] have given clear expositions of similar views.

Who is this “Doctor Science” I mentioned in the previous paragraph? He is the modern version of Doctor Robert. His powers are far greater than Doctor Robert’s; and he eschews Doctor “take a drink from my special cup” Robert’s crude methods, although he has refinements of them available when needed. To develop his methods, he had to perfect another crude tool from the era of Doctor Robert: John Lilly’s isolation tanks. He is able to isolate a human being completely from interaction with his or her environment; preserving his or her quantum state perfectly, and indeed, has universal, or at least extremely fine, quantum control over the state of that human being.

Just such an image was probably part of David Deutsch’s motivation for investigating quantum computation. He wondered what it would be like if an “information gathering and using device” (IGUS, to use Murray Gell-Mann’s term) was correlated with something quantum (“measured something on a quantum system”, “gathered some information”), and then, via Doctor Science-like quantum isolation and control, the two branches of the wavefunction describing it experiencing different measurement results were interfered in such a way as to reverse the measurement (in an interview in [29]). Supposedly, the IGUS’ report “I experienced a definite measurement result, but I can’t tell you what it was” would verify the many-worlds (relative state) interpretation. And just such an image has guided many a defender of (or, as in my case, devil’s advocate for) the relative state interpretation, when asked such questions as: “what do you mean “the different branches are all real”. Since we never see them, isn’t it just a metaphysical extravagance to suppose that they exist?” A thought-experiment like the one above involving Dr. Science provides a kind of answer to this question. We want to say we have a superposition for the same reason as we sometimes say this about atoms and photons: because, although in the case of a human being the experiment is impractically difficult, we could imagine having enough control over the system and its environment, including the observer, to interfere the different branches of the superposition, in such a way as to either destroy the definiteness of the measurement result (measure on the system and observer an observable complementary to the one describing definite measurement results, say) or—here is where control over the system’s environment beyond just the observer is crucial—disentangle the system from those degrees of freedom in observer and environment that constitute a record of the measurement result (possibly also making a joint system-observer-environment measurement to provide some confirmation that this disentangling has occurred). In the case of the relative state theory, though, the system to be controlled might well be the entire universe, through rapid decoherence. This rapid decoherence, of course, is one of the main things that makes Doctor Science’s task so difficult. And those opposed to the RSI might say “we can’t get outside the universe and interfere its branches, so the statement that it is in a superposition is meaningless.” Or if not meaningless, at any rate superfluous, and therefore should not be included in a scientific theory. Now, the claim that this statement would be meaningless for that reason seems to invoke a verificationist theory of meaning of a sort that, to put it mildly, is now considered somewhat problematic by philosophers. As to whether it is superfluous and therefore should be excluded from a scientific theory, that is certainly far from clear. The question, as the more sober advocates of this superfluity claim admit, is more one of judgement or taste. Rather than showing that the RSI is inconsistent, or impossible, it just displays features they don’t like, features they think are likely to prove impediments to scientific progress. I don’t necessarily disagree. I suppose my situation is that I don’t think the RSI is true; but I think pushing it to the limit can be a very useful exercise in dealing with the foundational problems posed by quantum mechanics: as useful as, and indeed in a strange way part of the same project as, pushing to the limit the view that “quantum states are subjective.”
One motivation for viewing superpositions in the universal state vector as real is the “slippery slope argument.” Probably by definition, doing interference experiments with the universal state vector is impossible. Indeed, when we don’t have quantum mechanics integrated with quantum gravity and cosmology, it is a bit of stretch to suppose that such a thing as a state vector (or density operator) of the universe makes sense (though one motivation for it was precisely to start combining quantum mechanics and cosmology). But on the other hand, one can sometimes reverse model measurements in microscopic systems, and if quantum error correction, noiseless subsystems, active noise suppression, and fault tolerant computing and control techniques which have been developed, mostly theoretically, within QIS, should prove implementable, then one may have available the tools to reverse measurement-like interactions involving far larger and more complex systems. Where does one draw the line? The subjectivist way is just to say, “include whatever is appropriate for this experimental situation, e.g. whatever you really have quantum coherent control over.” And, despite their willingness when pressed to sometimes concede that they can’t show the RSI is inconsistent, they also sometimes claim that it is inconsistent for an observer to view him or herself as described by quantum theory. Unfortunately, one doesn’t see (at least I haven’t seen) these vague allegations of inconsistency made into a rigorous argument. Even doing so within some toy model would be valuable. I think there is likely a valuable insight here, but its value will not be made manifest if no attempt is made to sharpen it. Also, from the relative states point of view, even if it is shown that it would be inconsistent for an observer herself to have a complete quantum-mechanical description of herself, the system she is measuring, and the part of the universe that decoheres her “in the pointer basis,” that of course does not show that such a description is itself inconsistent. The fact that it may be inconsistent, for instance, to assume that we could have a completely detailed quantum picture of the world (for one thing our brains are probably not big enough, even collectively and supplemented by available computing power), doesn’t seem to me to say much about why quantum mechanics is particularly ill-suited to play the scientific role, as a theory in terms of which, say, “God” could formulate a description of the universe and its dynamics, that people are supposed (on some accounts) to have ascribed to classical physics. Similar “self-referential paradoxes” seem just as threatening (and as potentially irrelevant) for a classical description.

Another beef with the subjective point of view is somewhat akin to Bell’s beef with those who would take “measurement” as an undefined primitive of quantum theory: there is too much vagueness, in my view, about when it applies. But I have hope that it can be made less vague. It could be that the difficulty, if it can be quantified or otherwise better understood theoretically, of reversing measurements made by complex systems such as organisms, is a deep insight into why the RSI is not the correct “overarching picture” for physics. Gravity could be involved: roughly because in current theory there is not negative “gravitational charge,” it may be theoretically impossible to shield things from gravitational interactions, and these may be sufficient to decohere any macroscopically different subspaces of a small subspace of the universe, like those corresponding to different positions of a pointer needle in a gauge. Statistical physics seems potentially relevant, too: quantum error-correction, and probably also active maintenance of noiseless subsystems, has a thermodynamic cost, and this cost may become prohibitive for macroscopic systems. But rather than just welcoming the ability to view quantum mechanics as only appropriate to describing an observer’s perspective on a system, revelling in the subjectivity of it all, the way it perhaps leaves room for mind, freewill, etc... as unanalyzed primitives, I think it is still promising to try to get a grip on these matters “from an outside point of view.” I think it rather likely that the relative state interpretation is wrong, and that quantum mechanics, as the subjectivists insist, indeed is not appropriate for describing the universe in terms of a universal state vector. But the attempt to understand its limitations in terms of a picture that coordinates local perspectives, some of which are appropriately described by quantum mechanics and some of which (because, perhaps, of in-principle or maybe “practical” operational limitations imposed by decoherence, the impossibility of gravitational shielding, or whatever), need to be described differently, seems promising, and related to serious physical questions involving statistical mechanics, cosmology, and perhaps quantum gravity.

An analogy might be special relativity. Here, an overarching picture was achieved by taking seriously the fact that position and time measurements are done via operations, from the perspective of particular observers. The heart of the theory is to coordinate those perspectives into a global Minkowski space structure. Certain aspects of the local operational picture had to change in order to do this. In particular, although one could still make the standard time and position measurements according to standard procedures, the overall picture implied limitations on the possible results of such measurements: notably, velocity measurements will never come out greater than the speed of light in a vacuum. (Indeed, this limitation was a motivation for the structure.) In a similar way, some assumptions of standard quantum mechanics may turn out to be invalid, though hopefully in a way as gentle as the limitation on velocities in special relativity, when the quantum-mechanical perspectives are coordinated into an overall whole. The alternative would be the RSI, which I view as repugnant and also (as I suspect do the subjectivists) as boring in something like the way that the Galilean coordination of local frames is boring compared to that of special relativity. But I don’t think that we should give up on an attempt at such coordination, perhaps celebrating the fact that quantum mechanics has shown us that it will be impossible to achieve under the aegis of physics, just yet.
A. Beyond Hilbert space?

An important point brought out by the attempt at a relative state interpretation of quantum mechanics is the need to bring in, in addition to Hilbert space, notions of preferred subsystems (“experimenter” and “system” perhaps also the “rest of the world”) or preferred orthogonal subspace decompositions (choice of “pointer basis” [96]). It seems unlikely, as Benjamin Schumacher likes to point out, that a Hilbert space, Hamiltonian, and initial state, will single out preferred subspace decompositions, hence the relative state interpretation points out the need for links to aspects of physics beyond Hilbert space. Schumacher has also criticized the RSI on the grounds that specifying a Hamiltonian evolution on a Hilbert space can be made to look essentially trivial (literally trivial, on a finite-dimensional space) by a time-dependent change of basis. If one takes the view that “the classical world” is supposed to emerge from this structure (Hilbert space, Hamiltonian, and initial state), then perhaps such transformations are legitimate. On the other hand, this transformation is not wholly trivial: if one specifies a dynamics on a Hilbert space, one is implicitly specifying a continuous canonical isomorphism at each time between a continuum of isomorphic Hilbert spaces parametrized by $t$. Then one can in addition specify a Hamiltonian evolution (essentially, another such canonical continuous identification). One can do this; if it were to lead to something interesting as far as picking out a set of subspaces that are special with respect to this structure, that would be interesting. I have doubts this will work; I also like Schumacher’s criticism that this specification of “two connections on a fiber bundle instead of just one” seems mathematically unnatural. But I am not wholly convinced by Schumacher’s criticism. For one thing, I view the RSI less as a way of getting the classical world of macroscopic objects and so forth to emerge from Hilbert space, and more as a way of giving a realistic interpretation to Hilbert space structure in the presence of additional structure such as preferred bases or subsystem decompositions that represent other aspects of physics, without necessarily trying to derive these aspects from a Hamiltonian on a Hilbert space. Schumacher views his argument against the RSI as also showing that one needs these additional aspects of physics—“nails in Hilbert space,” he calls them—to get a canonical identification of, say, bases from one time to the next (as, e.g., the spin-up/down basis). He interprets this as showing the appropriateness of the Hilbert space description as applying to subsystems where the special structure lies in relations to other systems (especially, for instance, macroscopic measuring devices, such as a Stern-Gerlach apparatus which distinguishes an “up/down axis” in space), and the inappropriateness of the Hilbert space structure for the description of the whole universe. There are, of course, plenty of such non-Hilbert space aspects of physics, involving symmetries, spacetime structure, and so forth. Often, however, these combine awkwardly with Hilbert space, as even quantum field theory suggests (I refer to the need for renormalization in standard Lagrangian quantum field theory approach), and as the difficulties with attempts at quantum gravity make very clear. The point is that some of these issues arise whether or not we take the relative state point of view on foundations. Yet, one hopes—at least I do—that this indicates where to look for a resolution of the tensions between inside and outside views—that it is this difficulty in squaring quantum mechanics with known physics, especially “geometrical,” “outside” aspects of physics, that suggests the distasteful aspects of the quantum-mechanical outside view may vanish once such a squaring, with whatever flexing is necessary from both sides, is accomplished.

B. Beyond spacetime?

I have said little about the Bohmian modification of quantum theory, but here is perhaps the place to remark that it is of interest as an example of one of those nonlocal hidden variable theories that Bell showed are the only non-conspiratorial way to realistically model the statistics of quantum measurements. Non-conspiratorial refers to the prohibition, implicit in Bell’s theorem and explicitly mentioned by him elsewhere, on explaining the statistics of quantum measurements by correlations between the hidden variables and what we “choose” to measure. In this regard, if one is thinking along the lines that the lack of unification of quantum mechanics with general relativity and gravitation may lie behind interpretational problems, locality seems potentially a petty restriction: when we are contemplating quantizing the spacetime metric or otherwise unifying gravity and quantum mechanics, perhaps it is not too farfetched to imagine that spacetime and causality will turn out to be emergent from a theory describing a structure at a much deeper level...if this structure turns out to contain things whose effects, at the emergent level of spacetime, can be interpreted as those of “nonlocal hidden variables,” this should hardly surprise. Of course, I do not know what such a theory might be like, and there is little reason to suppose its emergent description of induced “spacetime hidden variables” would resemble the specifics of the Bohmian theory, but it is useful to have at least one concrete example (and it might be useful, for those who seriously contemplate a unification project going “beyond spacetime”, to construct other examples of, or investigate general features of, nonlocal hidden variable theories).
C. Subjectivist and Objectivist views of the quantum state

The other main attitude to quantum mechanics is to abandon the idea that the quantum state describes reality, viewing it as a description of our beliefs or knowledge about the outcome of experiments that might be performed on a quantum system. Here, too, there are difficulties. There is a drive to understand experiments that one might perform as interactions between the system and some apparatus coupled to us; many aspects of experimentation require adopting this point of view, and then one could argue one is on a “slippery slope” leading towards viewing ourselves as quantum systems interacting with the system being experimented on.

One might characterize the subjectivist attitude as an “instrumentalist” view of quantum theory: it is a procedure for calculating the probabilities of measurement outcomes in experimental or other observational situations, and the objects referred to in this formal procedure should not necessarily be taken as “elements of reality.” Note that this does not mean that quantum mechanics tells us nothing about reality. It tells us that reality is such that this procedure works well, although not much about why it does. But it explicitly renounces the attempt to get a physical picture, from “outside” the experimenter, of how and why this procedure works, a picture which views the experimenter as part of a reality which is at least approximately, or well enough for our purposes, described by some mathematical structure, which is interpreted by pointing out where in this structure we, the observers and experimenters, show up, and why things end up looking as they do to observers in our position. It could be that the overall mathematical structure is very different in nature from the descriptions observers within the overall structure give of things from their perspective. Or it could be, as in the relative state interpretation, that the two kinds of structure are similar.

In my view, one should underestimate neither the importance of pragmatic, predictive procedure, nor of situating ourselves within a larger, at least partly comprehensible, structure, as goals of science. The subjectivist view might seem to give up on the latter, while in my view the RSI shows that, at a high cost, one can preserve both.

Despite the value I ascribe to situating ourselves within a larger scientific picture of the world, I am not yet willing to believe in the RSI. But I will be clear about why: I just do not like the picture it presents, of my consciousness constantly forking into perhaps infinitely many branches, all of which have as much claim to reality as the branch “I” who am writing this happen to be identified with. I don’t like it because it seems to devalue the particularity of the world as I experience it, instead injecting a disagreeable element of “everything goes” into reality. The negligible probability of bizarre quantum fluctuations rendering my world apparently uncontrollable and weird (while still allowing me to remain conscious) seems comforting, despite their theoretical possibility, within a Bohrian interpretation of quantum mechanics; with the RSI, all these bizarre possibilities are real, and the smallness of their amplitudes in the world’s state vector does not seem all that comforting. Perhaps to some my rejection of the RSI on these grounds will seem like wishful denial of the hard truths of science. This may be reinforced by my attitude towards Schulman’s proposed resolution of the problems of quantum mechanics \[79\]. As I understand it, he proposes to retain essentially a one-Hilbert space, state-vector evolving according to the Schrödinger equation, no-collapse version of quantum mechanics, interpreted realistically if you like, but to avoid macroscopic superpositions (Schrödinger cats) by bringing in cosmology and statistical mechanics, and arguing that symmetric consideration of final conditions along with the usual initial conditions (that the universe was once much denser and hotter than it is now) rules out macroscopic superpositions. There is a lot to do to make this persuasive, but perhaps it can be done; it is certainly an ingenious and appealing idea. And if it does work, I am fairly happy to retain the rest of the relative state metaphysics, now that I will not be committed to the disturbing existence of forking Döppelgangers in subspaces of Hilbert space decohered from me, but still real. So that my discomfort with the RSI can perhaps be summarized in a motto: “Enough of this forking nonsense.”

D. Mind and matter

A more philosophical objection to the RSI is made by \[17\]. In some respects this is just the same thing, but in others, perhaps, more substantive. “[The RSI’s] purpose, however, is to maintain the objectivity of a formally describable state of the universe as a whole. It does this by attributing the ambiguity that arises from the “relative” nature of the states of subsystems to the perceptions of the observer. In other words, the “objective nature of physical reality” (that is, the formal independently-defined quality of the systems described by physics, which are taken to be the ontological basis of all reality) is maintained by shifting everything we think of as objective physical fact (in the common intuitive sense of the specific details of the world as we experience it) over to the subjective side of the Cartesian split—that is, over to oblivion (as a materialist understands subjectivity).”

This contains the same distaste as my objections above: he doesn’t like to have the specific history of his experiences rendered merely one among many perspectives. But it also suggests a perhaps more substantive, metaphysical objection. The objection is that fundamental to the RSI is the notion that consciousness and other mental facts and things supervene on the physical. The “ontological basis of reality” is taken to be a quantum system described by a
state vector. A presumed philosophical and/or scientific psychology, of the sort I have already mentioned, and whose
details perhaps do not yet exist, is trusted to explain why the conscious perspectives on the world are as they are,
and fork the way they do. Even before any consideration of physical theory, it seems Bilodeau would object to the
idea that consciousness supervenes on the material world. This is the point of his remark that the RSI consigns this
subjectivity to “oblivion, for a materialist.” However, I think it is possible to maintain a version of the RSI which is
not materialist in this sense: the notion of supervenience of mind on matter, in contradistinction to ideas of identity
between them (or bogusness of one or the other), is around precisely to allow such moves. Bilodeau seems to view
supervenience as almost as bad as oblivion. But it seems to me that the RSI is also consistent with a “dual aspect”
teen theory of the mental: instead of saying the mental supervenes on the physical, arising like the froth on a wave whenever
certain physical conditions are met, we might more congenially say it is another, subjective, aspect of things, things
which can also have physical or objective aspects. The subjective aspect is what it is like to be those things ([69,
Chs. 12, 13], [71, esp. Chs. II, III]). In some cases, the physical system becomes complex enough, or whatever, that
what “it is like to be that system” becomes substantial enough to be called a mental phenomenon. This does seem
preferable to supervenience, which makes mental phenomena seem somehow superfluous, just along for the ride but
not part of the essence of things. But the difference between supervenience and “subjective aspect” may be less than
it seems. To make it substantive may require a move which Bilodeau wants us to make: to take the view that we will
not understand the subjective aspect of things in terms of physical theory, so that the mental aspect of things takes
on a life of its own, rather than just being an aspect of something adequately understood in terms of the physical.
However, I would not like to make too many assumptions in advance about the terms in which we will (if we will)
ultimately be able to understand things about consciousness and the mental. Almost certainly it will be in terms of
concepts, perhaps scientific concepts, at a much higher level than those of physics: this is familiar from sciences like
biology and chemistry. I just don’t want to assume that understanding enough about mind (which may not be very
much; it may even be compatible with the impossibility of a physicalist understanding of many important aspects
of the mental) to have a stylized psychology capable of explaining the perspectives, and the forking of perspectives,
required by the RSI, requires a thoroughgoing ontological committment to the “primacy of the physical” in some
sense. Indeed, even if physical terms (or higher-level terms no more “unphysical” than those of chemistry or biology)
suffice for a reasonable understanding of mental phenomena, or many aspects of them, I’m not sure this would rob
them of ontological independence, in a dual-aspect theory.

Of course, Bilodeau doesn’t very explicitly make the argument I have just made, that the RSI’s possible need for a
materialist psychology might be problematic enough to make us consider alternatives. In any case, I am arguing that
an RSI-like gambit can be run even by someone who believes in the reality and substantial independence of the mental
aspects of things; the relation of physical systems to consciousness will be subtler perhaps in such a theory, but it is
hard to imagine there won’t be some such relation capable of sustaining an RSI-like story. The bottom line, I think,
is really the matter of preference, even ego, I spoke of above. We just don’t like the fact that the RSI dethrones our
particular perspective from its privileged position, and establishes alongside it a possibly infinite number of variants
of it, some of which (although with negligible amplitude) involve bizarre and fantastically improbable events, along
with many other perspectives which are not even those of anything we think of as existing in our world. But again,
perhaps that’s just the way things are. Other branches of physics have suggested, with various degrees of plausibility,
that there is stuff outside our forward and backward light-cones, maybe even completely causally disconnected from
our forward light-cones, maybe even trapped in a bubble in spacetime called a “baby universe;” if so, there may well
be stuff there that is organized in such a way as to have consciousness, and that’s just how science tells us it could
be. But perhaps what is objectionable about the other Hilbert subspaces of the RSI and not about these inaccessible
parts of spacetime, is the wildly implausible nature of the things that happen in them.

What appeals to Bilodeau, I think, about the Bohrian interpretation is that it suggests that science is showing
us that the epiphenomenal (supervenient) “materialist” theory of mind I mentioned in connection with the RSI, is
untenable. One could, of course, adopt a wholly instrumentalist view of quantum theory, in which case it would
presumably not be telling us anything at all about mind. But Bilodeau wants to argue that the very fact that this
procedure works tells us something about reality. The fact that attempts to describe it, and our place within it, from
an overall point of view in a physical theory, supposedly fail just when we try to place ourselves, and our experiences of
definite quantum measurement results, within it, supposedly shows that our mental aspects such as capacity to have
experiences cannot be incorporated into an empirically accurate physical picture of the world, and so must be given a
certain degree of autonomy and fundamental role in an adequate picture of reality. Such a picture is fairly eloquently,
if necessarily vaguely, described on p. 400 of his paper. “There is Being. Being is aware. Being acts. The action of
Being (from our perspective as participants) represents itself (in part) as the physical universe in historical space and
time.” The main point is that “...consciousness must be closely related to existence itself, that it is vastly nearer to
the “basic level of reality” than anything signified by physical concepts.” I find these notions enormously attractive
both on philosophical grounds and at a pre-philosophical level. Yet I would not want to prejudge the potential of
scientific consideration of mental phenomena because of this attraction. And I also am not as convinced as he is that
quantum theory has decisively cast the die in favor of this kind of view, which may have to survive a bit longer on its own intrinsic merits. Indeed, I am not convinced, as suggested above, that the RSI does not provide a workable, even if unpalatable, way to do just what Bilodeau claims quantum physics forces us to renounce: view ourselves as part of a larger reality, about which reality it is not clear we need make, on physical grounds, a judgement about the degree to which it is “fundamentally physical,” or can be understood in physical terms. In other words, I think the RSI can be made to work to some extent independently of how tightly consciousness is linked to or subordinated to the physical, as long as the linkage is sufficient to get the various needed subjective perspectives working right. It thus seems possible to me that the unpalatability of the RSI’s picture of what minds exist is just one more sign of the unfinished scientific business of integrating quantum mechanics with the rest of physics.

We would like to take a view in which stuff happens to us, and this “historical” account of what happens, the incontrovertible stuff of everyday reality, is made sense of by finding regularities in it, regularities which turn out not to be tight enough to be describable by deterministic causal laws, but still tight enough to be describable by stochastic laws. The resistance to the RSI expresses a reluctance to dethrone this “historical” account of what happened to us, to a par with other experiences going on in other branches of Hilbert space. Yet, this is the sort of thing that science often forces us to do: acknowledge the existence of stuff—real, physical stuff, but also stuff which it is only reasonable to expect in some cases has as much “subjective,” “historical” existence as our experiences: other planets, other galaxies, perhaps wildly remote regions of the universe with which we will never be able to communicate in our lifetimes.

Further investigations from the point of view of the RSI seem to me just as justified, even as likely to be fruitful, as those from the “subjective” point of view. Indeed, I have argued that to a certain extent they may closely parallel each other, inasmuch as the RSI also takes the probabilistic interpretation of measurement outcomes and statevector collapse as “subjective,” appropriate for understanding how things are likely to look from a certain perspective.

IV. LAWS OF THOUGHT AND OVERARCHING STRUCTURES

Partly because in my view it has not yet resulted in significant substantive progress in understanding foundational problems of quantum mechanics, I think the most important impact of the emergence of QIS on the foundations of quantum mechanics is to provide a formal framework for analyzing and making quantitative those problematic or peculiar aspects of quantum mechanics, in the guise of the theory of information (broadly understood). The most significant potential impact of this information formulation lies in the fact that information is being recognized (and this simultaneously with, and to a fair extent independently from, the emergence of QIS) as critical to the understanding of many aspects of physics. This is most notable in statistical mechanics. The foundations of statistical mechanics have also been somewhat controversial; perhaps less so than those of quantum mechanics, but for some of the same reasons: it is difficult to sort out the subjective and objective aspects of the foundations of the theory. In my view, the foundations of statistical mechanics are probably best understood in at least partly subjective terms: a system has high entropy for an observer if that observer lacks information about it; information is power, in the sense that an observer having more information about a system, and hence a lower entropy for it, is likely (other matters equal) to be able to extract more useful work from it. This view emerges from the analyses by Landauer, Bennett, Zurek, Caves and probably others, of the Maxwell Demon paradox, and it shares with the subjective interpretation of quantum mechanics the idea that the theory (be it statistical mechanics or QM) is suited for describing one part of the world as seen by another, not necessarily giving a model for the world as it might be seen “from outside,” independent of any viewpoint within it.

Yet the issue of subjectivity seems somehow more pressing for quantum mechanics, because one has gotten used to the idea that statistical mechanics is not a “fundamental theory,” but an emergent aspect of a world governed by fundamental laws, indeed to the idea that certain aspects of statistical mechanics will likely be emergent from fundamental laws of many different forms, so that although in one sense less fundamental, they may actually be more universal than particular theories of the fundamental laws. Perhaps the information-theoretic view of quantum mechanics could be interpreted as saying that actually, QM is similar to statistical mechanics in this regard: QM should be viewed as a theoretical framework appropriate for the description of one part of the world by another, and not for the description of the world “from outside.” Yet this is compatible with the idea that quantum mechanics will be seen to emerge from a more fundamental theory... which smacks of hidden variables. It would be interesting if QM had some “universal” properties similar to those which statistical mechanics probably has...i.e., that quantum mechanics “must” emerge as a theory describing the perspective of one part of the world on another, whenever the “underlying” theory has certain properties. This is reminiscent of, though far from the same as, the idea that “quantum mechanics is a law of thought.” The two ideas may be brought into closer relation if we take this last statement not as obviously analytically true, (in the sense in which some might wish to say that it is analytically true that “Bayesian probability theory and/or decision theory is a law of rational thought”), but in a somewhat
Kantian sense, that the aspects of the world which give rise to quantum mechanics as an emergent description of perspectives within the world, are necessary for the existence of rational beings having perspectives (or in a weaker version, perspectives of a certain sort). Though I have no particular model of how this could be true, it seems quite plausible that a certain degree of regularity in the world is necessary for the emergence of rational, indeed probably even sentient, beings, and conceivable that a certain combination of regularity and irregularity, manipulability and uncontrollability of parts of the world by others, which is just right for a quantum-mechanical description, might be necessary for this emergence of rational and/or sentient beings.

Combined with an argument for the unknowability, even perhaps the pointlessness of attempting approximation via science, of the “underlying” description, this might begin to be a possible interpretation of the information-theoretic take on the foundations of quantum mechanics. More radically, one might wish to combine it with an argument for the “nonexistence” of this underlying description.

V. THE COMBINATION OF PERSPECTIVES

The viewpoint I am advocating, then, is that we should continue to maintain both the inside and outside view of quantum systems, and in interpretational matters to pursue a better understanding both of the possibility of viewing quantum theory as about the dynamics of information-like, perhaps subjective, states, and of the possibility of viewing it as about the sorts of entanglement and correlation relations that can arise between systems. A prime example of a worthwhile problem along the former lines is that being pursued by Caves, Schack, and Fuchs, of just how far one can push the analogy between quantum states and subjective Bayesian probability distributions, in particular how quantum measurements and “collapse of the wavefunction” can be viewed as similar to the updating of a Bayesian probability distributions upon gaining new information. A prime example of a worthwhile problem along the latter lines is understanding how the probabilities for collapse can be understood within the RSI, again as something like (or perhaps as exactly) a Bayesian updating process of “gaining more information about which branch of the wavefunction we are in.” As I just phrased it, this sounds exactly like Bayesian updating, but the key potential difference is that we are not gaining information about a pre-existing fact, but rather about a “perspectival” fact (such as “I am in the spin-up branch of the wavefunction”) that is not even true or false before the measurement procedure creates the branches.

But equally, or perhaps more, important problems can be posed without committing to either point of view, and it is this sort of problems, and the role of quantum information and computation in the project they suggest, that I want to emphasize here. I suggest that the “operational” or “empirical quantum logic” point of view is useful whether one wants to consider the empirical theory as for some reason all we can hope for, or as a description of how perspectives look within an overarching theory such as the RSI. Indeed, the similarity between the problems posed above is an example of how the operational approach is relevant to both: investigate quantum mechanics’ properties as a theory of perspectives of subsystems on other systems, without prejudging whether or not the perspectives will turn out to be coordinatable into an overarching picture—indeed, while trying to ferret out how this might happen or be shown to be inconsistent, and how this possibility or impossibility may be reflected in the operational, perspective-bound structures.

This general operational approach may turn out to be a limiting, or limited, way of viewing physical theories. It seems rather suited, however, to quantum mechanics, particularly Copenhagen-style or “subjective-state” quantum mechanics, and also adequate to treat classical mechanics in its information-processing aspect. The limitations arise because this sort of theory takes “operations,” such as making a measurement on a system, as basic terms in formulating a physical theory. It views a physical theory as a description of the behavior of a “system,” a part of the world viewed as susceptible to the performance of operations on it by, presumably, another “part of the world,” the observer or experimenter (32, p. 2). However, it need not necessarily be associated with a renunciation of the attempt to view the entire world as one structure, and physical theory as describing that structure. It might be possible to also view the entire world in terms of an empirical theory of the same kind, from the point of view of a fictitious observer. (I was somewhat surprised to hear David Finkelstein defend such a point of view a few years ago.) Classical theory may be such a case, while many believe that quantum mechanics is not. However, Everett’s “relative-state” interpretation (the inspiration for what is sometimes called the “many-worlds interpretation”) attempts such an “objective” interpretation of quantum mechanics. Indeed, Rovelli and Smolin [84] have proposed making the impossibility of such an external view a desideratum for an acceptable physical theory.

But (aside from quantum gravity aspects) the main interest the Rovelli-Smolin approach holds for me is, that it suggests a framework in which quantum mechanics is good for describing things from the point of view of subsystems, but not appropriate for the entire universe, but in which nevertheless there exists a mathematical structure—something like a topological quantum field theory (TQFT), spin network, or spin foam—in which these local subsystem points of view are coordinated into an overall mathematical structure which, while its terms may be radically different from
those we are used to, may still be viewed as in some sense “objective.” However, it is far from clear yet that this can be done while avoiding the more grotesque aspects (proliferating macroscopic superpositions viewed as objectively existing) and remaining conceptual issues (how to identify a preferred tensor factorization, and/or preferred bases, in which to identify “relative states”) of the Everett interpretation.

In TQFT’s or spin networks and generalizations, the description appropriate to “perspectives” is still Hilbert spaces, but only in special cases do these combine as tensor products. If we view a manifold as divided into “system” and “observer” via a cobordism, then as the “observer” gets small enough, while the “system” gets larger, we start getting, not the increase in Hilbert-space size to describe the system that we might expect as the system gets larger, but a decrease in Hilbert-space size whose heuristic interpretation might be that the observer has gotten so small that it no longer has the possibility of measuring all the operators needed to describe the “large” Hilbert space one might have expected. The Hilbert space does not describe the “large” rest of the world; it describes the relation between a small observer and the larger rest of the world. It would be nice if this could somehow be made to imply that the observer cannot get into a Schrödinger cat-like macroscopic superposition, but it is not clear this can be done. Indeed, when I sketched this viewpoint to Carl Caves, he asked how it differs from the Schmidt decomposition. View the entire universe as in a pure state (say 2^n dimensional). As we include more and more of the universe in one or the other of the systems, the local supports of the state become smaller and smaller: this can be interpreted precisely as I proposed interpreting the reduction in Hilbert space size in TQFT’s above: as a reduction in the local Hilbert space size needed to describe local operations, given the pure state that describes the system. However, here all the “apparatus” etc... needed to perform local measurements should be included on the observer side, so that in some sense, the “perspective” should be one in which a particular measurement is perhaps committed to, or perhaps better, in which we use a “decoherent histories” kind of approach to ask which histories are consistent with this pure state of the universe, this split into observer and observed, and whatever dynamics one introduces. And indeed, Rovelli and Smolin found themselves moving toward the decoherent histories point of view to interpret their theory, though Smolin at least felt that the Dowker/Kent objections to consistent histories were a serious problem. A more “operational” point of view, emphasizing a certain “freedom” to set up different local experiments, might need to divide the manifold into more parts, and specify the state less precisely than assuming a single pure state of the whole. Indeed, unless something like this is done one anticipates difficulties with the usual assumptions of the theory of quantum operations. If the entire system is in a pure state, or even certain types of mixed state, it may not make sense to assume that our local apparatus is in a state independent from that of the system. Understanding when this sort of assumption can be reasonable is a potential contribution of a theory of this sort. However, this also makes the interpretation of the local Hilbert space structure in the case where observer of system approaches being the whole universe, problematic. I do not view such potential difficulties, however, as reason to abandon the project of attaining a view of the world as an overarching structure which coordinates many perspectives. Rather, it suggests reasons, more physical than metaphysical, why the attempt to force that overarching structure into a mold derived from standard quantum mechanics may fail. But we need to try to patch up these failures, modifying how systems combine, and perhaps even introducing new categories of empirical theory, in order to make progress toward such an overarching structure. We may find, as some in the the “quantum states are subjective” crew might anticipate, that such progress is not possible, or not possible from the point of view of physics alone. This would be fascinating and significant, but I do not think we are yet near the point where one should give up on the project and accept its impossibility. But even if quantum states are subjective, this doesn’t mean they can’t be combined, as states from perspectives, in a coherent way. And whether or not we accept the the task of attempting to make progress toward such an overarching theory, we need to understand quantum mechanics, and classical mechanics, as examples of empirical-theory types: how the distinction between the two can be characterized, perhaps axiomatically; whether we can obtain such characterizations in which the axioms have an intuitive meaning, so that we understand the difference between the two in terms of that meaning; whether both share certain features, perhaps describable in terms of their power to process information, that are not necessary features of empirical operational theories in general; and so forth. Here we might see how the quantum description of certain perspectives could arise as a limiting case of some more general type of perspective, which necessarily also arises in an overarching structure that includes quantum-mechanical perspectives in a physically reasonable way. Or we might see how a non-tensor product law of combination of subsystems—quantum or not—could be relevant in some situations. This is just the sort of thing that operational quantum logic aspires to investigate. The role of quantum information and computation in this project could be of great importance: to suggest axioms with clear meaning in terms of information processing, and to elucidate their connection with other areas of physics such as statistical mechanics or cosmology. More mundanely but no less importantly, operational quantum logic could have a role to play in quantum information and computation proper: particular suggested implementations of quantum information processing, or other particular operational situations, which impose limitations on the measurements and other operations that can be performed, may correspond to structures of a kind already investigated in some branch or branches of operational quantum logic, which may supply mathematical tools and results with which to analyze them.
VI. FRAMEWORKS FOR EMPIRICAL OPERATIONAL THEORIES

A. Introduction: operational quantum logic and convex sets frameworks

In this section I will introduce frameworks, that I find particularly useful for thinking about empirical operational theories. The general area of mathematical descriptions of operational theories (“mathematical metascience,” as David Rüttiman, cited in [51], has called his (and collaborators’) version of it in an excellent introduction and overview) seems to me to exhibit some particular mathematical behaviors that it can help to take note of before plunging into the literature. One is that many constructions can be described in alternate ways, notably involving the relationship between order-theoretic and algebraic descriptions of the same thing, so that some of the most interesting categories have been independently defined in different ways. A notable example is “effect algebras,” introduced under this name by Foulis and Bennett [41], but, it turns out, introduced as “weak orthoalgebras” in [48], and independently, from a more order-theoretic point of view, as “difference posets” (D-posets, for short) by [50]. Also there are frequently nice functorial relations between different categories of empirical theory types, sometimes inducing equivalences under mild conditions, again providing different perspectives on the same structures. A notable example is the representability of “convex effect algebras” on ordered vector spaces [49, 51], and related results. The precise statement is that any convex effect algebra admits a representation as an initial interval of an ordered linear [i.e. vector] space. Long before this representation theorem was proved, effect-algebra-like structures had been noticed in the convex state-space approach to operational quantum mechanics and more general operational theories [28, 63, 64], and indeed, the term “effect” was introduced (so I have been told) by Ludwig, who worked within a version of the convex states approach. That some such relation existed has been suspected for quite awhile (see, for example [26] on the relation between the convex approach and a predecessor of the effect algebra approach); also, similar linearization theorems exist for other types of quantum structures (Rüttiman, cited in [51]).

Another notable general feature of the mathematics of this area is a certain “smoothness”: it is usually possible to make small changes to the axioms describing a category of operational theories, and the theory of the category changes incrementally rather than radically as a result of such changes. (No doubt someone will point to some welcome counterexamples; but there is a lot of such “smoothness,” overall.) This results in a profusion of results, often closely related but slightly different and sometimes stated in very different terminology.

The result of these general features is a body of mathematics which is rich and fascinating, but which also lends itself to duplication of terminology and results, and a certain amount of resulting confusion. Often a researcher will find it easier to develop results closely related to another’s, within his own framework, rather than make the effort necessary to learn yet another set of notations, results, and quirks. Yet one expects—and this is occurring—that in the end, it will be useful to develop a broad view encompassing many different approaches to quantum and other empirical “logics” or “structures.”

B. Frameworks for representations of empirical probabilities

My preferred approach to empirical or operational theories is to start from the compendia of probabilities that our phenomenological theory says are possible for the different possible results of different possible operations on a system (indeed, I will often call these compendia phenomenological theories), and attempt to construct various more abstract structures for representing aspects of empirical theories—effect algebras, classical probability event-spaces, $C^*$-algebraic representations, spaces of density operators on Hilbert spaces, orthomodular lattices, or what have you—describing types of operational theories, from these. With most such types of abstract structures, the possibility of constructing them in a specified way from compendia of phenomenological probabilities will impose restrictions on these compendia of probabilities, and the nature of these restrictions constitutes the empirical significance of the statement that our empirical theory is of this type (has this abstract structure). This approach promises to systematize our understanding of a wide range of empirical structures and their relationships, both mathematically and in their empirical significance. Of course, a large part of the existing body of work on empirical theories may be viewed as part of this project, since the relationship to the probabilities of experimental outcomes has always been a critical part of understanding these structures as empirical theories, and even as abstract mathematical structures, the space of “states” on such structures is often a crucial aid to understanding their mathematical structure. This is of a piece with the situation in many categories of mathematical objects. $[0,1] \subseteq \mathbb{R}$ is a particularly simple example of many categories of “empirical structure,” and a state is a morphism onto it; understanding the structure of some more complex object in the category in terms of the set of all morphisms onto this simple object is similar to, say, understanding the structure of a finite group in terms of its characters (morphisms to a particularly simple Abelian group, $S^1$).

In this project, I like to make use of an idea which has come in for a fair amount of criticism, but has been
with us from early in the game. It certainly appears in Mackey’s classic work on characterizing quantum mechanics
 axiomatically [67], but [27] even ascribe it to Bohr (an ascription which I have not independently checked). It is also
 fundamental to Ludwig’s convex approach to foundations, and I expect the use I make of it may turn out to be closely
 related to the role it plays in Ludwig’s work. One also finds it on p. 14 of [68], and doubtless in many other places. This
 is the notion of “probabilistic equivalence”: two outcomes, of different operational procedures, are viewed as
equivalent, “identified” in some abstract sense (best interpreted, perhaps, as “exhibiting the same effect of the system
on the observing apparatus,”) if they have the same probability “no matter how the system is prepared,” i.e., in
all admissible states of the phenomenological theory. The interpretation as “exhibiting the same effect of system on
apparatus,” is probably traceable to Ludwig, and is perhaps the motivation for his introduction of the term “effect
for the abstract entities that, in quantum mechanics, correspond to the equivalence classes of outcomes under this
relation. It also helps forestall the main objection to this notion, which is that two outcomes equivalent in this sense
may lead to different conditional probabilities (conditional on the outcome in each case) for the results of further
measurements. One may say that they are equivalent only as concerns the effect of the system on the apparatus
and observer, not vice versa. This discussion implicitly supposes a framework in which operations may be performed
one after the other, so that outcomes of such a sequence of
and observer, not vice versa. This discussion implicitly supposes a framework in which operations may be performed
one after the other, so that outcomes of such a sequence of N measurements are strings of outcomes a₁a₂...aN of
individual measurements. Then a stricter notion of probabilistic equivalence may be introduced, according to which
two outcomes x and y are equivalent if for every outcome a, b (which may themselves be strings), the probability of
a₁b is the same as that of ayb, in every state of the phenomenological theory. I believe that in the quantum case, there
is a bijection between the equivalence classes of this relation and the trace-nonincreasing completely positive
maps (quantum operations).

The main point, of course, will not be to exhibit another formalism with which to describe quantum mechanics,
but to exhibit quantum mechanics as a special case of a more general type of empirical structure. Then, within
the theory of such more general structures, one may investigate the effect of imposing additional axioms, the information-
processing power of empirical theories more general than quantum mechanics, the way in which such structures may
combine as subsystems of a larger system, and so forth. Before dealing with the probabilistic equivalence classes (often
also called “operations”) on phenomenological theories in which we take account not just of single measurements but
sequences of measurements made in succession I will deal with the case where the probabilistic equivalence relation
ignores sequential considerations (calling the resulting equivalence classes “effects”). The resulting structures are likely
still relevant to general empirical theories (just as POVMs are still useful in quantum mechanics even though in some
cases one needs the additional detail about conditional dynamics provided by “instruments,” collections of quantum
operations that sum to a trace-preserving one, terminology that may have been introduced in [28], and is used also in e.g. [57].)

C. Effect algebras and related structures

Before considering in detail the derivation of the structure of the set of probabilistic equivalence classes (“effects”)
of an operational theory, I will introduce some of the abstract structures we will end up with: effect algebras and
“weak effect algebras,” motivating them (in the case of effect algebras) with classical and quantum examples.

Definition 1 An effect algebra is an object ⟨E, 1, ⊕⟩, where E is a set of elements called effects, 1 ∈ E, and ⊕ is
a partial binary operation on E which is (EA1) strongly commutative and (EA2) strongly associative. The qualifier
“strongly,” which is not redundant only because ⊕ is partial, indicates that if the sums on one side of the equations
for commutativity and associativity exist, so do those on the other side, and they are equal. Explicitly, these equations are:

\[
\begin{align*}
\text{Commutativity} & \quad a \oplus b = b \oplus a \\
\text{Associativity} & \quad a \oplus (b \oplus c) = (a \oplus b) \oplus c.
\end{align*}
\]  

In addition,  
(EA2) \forall a ∈ E, ∃! b ∈ E \quad (a \oplus b = u). (The exclamation point indicates uniqueness. We give this unique b the name a′; it is also called the orthosupplement of a.)

(EA4) a \oplus 1 is defined only for a = 1. (We will often call 1′ by the name “0”.)

If we require that the equalities [1] and [2] hold only when both sides are defined, allowing the possibility that one is
defined while the other is not, we call these “weak commutativity” and “weak associativity.”

In the effect algebra of quantum mechanics (on a finite-dimensional Hilbert space, say), E is the unit interval of
operators e such that 0 ≤ e ≤ I on the Hilbert space, ⊕ is ordinary addition of operators restricted to this interval
(thus e ⊕ f is undefined when e + f > I), 1 is the identity operator I, and e′ = I − e. 0 turns out to be the zero
operator. Classical examples also exist. If we consider the set \( \mathcal{F} \) of “fuzzy sets” on a finite set \( \Lambda = \{ \lambda_1, \ldots, \lambda_d \} \) (which are functions from \( \Lambda \) to \([0,1]\)), and define \( \oplus \) as ordinary pointwise addition of functions (i.e. defining \( f + g \) by \((f + g)(x) = f(x) + g(x)\) except that \( f + g \) is undefined when \( f + g \)'s range is not contained in \([0,1]\)), and 1 is defined to be the constant function whose value is 1, then \((\mathcal{F}, 1, \oplus)\) is an effect algebra. This algebra is obviously isomorphic to the restriction of the quantum effect algebra on a \( d \)-dimensional Hilbert space to effects which are all diagonalizable in a particular basis. These “fuzzy sets” may be interpreted as the outcomes of “fuzzy classical measurements” in a situation where there are \( d \) underlying potential atomic “sharp” measurement results or “finegrained outcomes,” but our apparatus may have arbitrarily many possible meter readings, connected to these “atomic outcomes” by a noisy channel (stochastic matrix of transition probabilities, which are in fact the \( d \) values taken by the function (effect) representing a (not necessarily atomic) “outcome”).

If we add the requirement

\[(OA5) x \oplus x \text{ exists } \Rightarrow x = 0 \]  

then we have an orthoalgebra; the projection operators on a finite quantum system are an example.

Later, we will also be concerned with something I will call a “weak effect algebra.” This satisfies all the axioms above except that strong associativity (EA2) is replaced by weak associativity (WEA2). Obviously, every effect algebra is a weak effect algebra, but not vice versa. A strengthening of the effect algebra notion, called an orthoalgebra, is also of interest: it adds the axiom that \( x \oplus x \) exists only for \( x = 0 \). The projectors on a quantum-mechanical system, with the same definitions of 1, \( \oplus \) as apply to more general POVM elements, are an example (as well as being a sub-effect algebra of the full quantum mechanical effect algebra of POVM elements).

Other weakenings of the notion of effect algebra have been considered. Notably, Wilce considered a fairly general notion of “partial abelian semigroup,” (PAS) which is like an effect algebra except that we retain only (EA1) and (EA2); various combinations of additional requirements then give a remarkably wide variety of algebraic structures that have been considered in operational quantum logic, including effect algebras, test spaces, E-test spaces, and other things. In particular, an effect algebra is a positive, unital, cancellative, PAS. Since we weakened (EA2) to obtain the notion of WEA, WEA’s are not a subclass of PASes. However, later, when we introduce the notion of “operation algebra” we will want to generalize the notion of WEA analogously to the generalization of EA’s to PASes, by also weakening (EA3) and modifying (EA4) to reflect this.

A state \( \omega \) on a weak effect algebra \( \langle \mathcal{E}, \oplus, 1 \rangle \) is a function from \( \mathcal{E} \) to \([0,1]\] satisfying:

\[
\omega(a \oplus b) = \omega(a) + \omega(b) \quad ; \quad \omega(1) = 1 .
\]

A finite resolution of unity in a weak effect algebra (to be understood as the abstract analogue of a measurement) is a set \( R \) such that \( \oplus_{a \in RA} 1 = 1 \). So for a resolution of unity \( R \), \( \sum_{a \in R} \omega(a) = 1 \): the probabilities of measurement results add to one. A morphism from one WEA \( \mathcal{E} \) to another \( \mathcal{F} \) is a function \( \phi : \mathcal{E} \to \mathcal{F} \) such that \( \phi(a \oplus b) = \phi(a) \oplus \phi(b) \); it is called faithful if in addition, \( \phi(1_{\mathcal{E}}) = 1_{\mathcal{F}} \), where \( 1_{\mathcal{E}} \) and \( 1_{\mathcal{F}} \) are the units of \( \mathcal{E} \) and \( \mathcal{F} \). [0,1], with \( \oplus \) addition restricted to the interval, is an effect algebra, so a state on \( \mathcal{E} \) is a faithful morphism from \( \mathcal{E} \) to this effect algebra.

I will attempt to avoid issues involving effect algebras and WEA’s where \( \mathcal{E} \) is infinite and infinite resolutions of unity are defined, even though even finite dimensional quantum mechanics is properly done that way. To this end I will assume that EA’s and WEA’s are locally finite: resolutions of unity in them have finite cardinality. For example, for finite \( d \)-dimensional quantum mechanics, most things should work the same if we restrict ourselves to work with resolutions of unity into \( d^2 \) elements. (We certainly need \( d^2 \) to provide a basis for the Hermitian operators on the Hilbert space. Local finiteness, of course, is much weaker than the existence of such a uniform bound.) Those who are interested in structures that are not locally finite, \( \sigma \)-additivity issues and so on might start with [36] where test spaces (under the name of “manuals”) and orthoalgebras are discussed; they are included in more general discussions of convex effect algebras in Bugajski et al. [19] and of sequential effect algebras in Gudder and Greechie [50].

### D. Weak effect algebras from probabilistic equivalence

Now, I will relate this abstract structure to phenomenological theories, by showing that one can derive a natural weak effect algebra from any phenomenological theory (restricted for simplicity to “locally finite” phenomenological theories (for which each measurement has a finite outcome-set)), and mentioning how I think one may naturally extend any such weak effect algebra to an effect algebra proper.

The operation \( \oplus \) of the weak effect algebra will be the image, under our construction, of the binary relations “OR” (“\( \lor \)”) in the standard propositional logics (one for each measurement) of propositions about the outcomes of a given measurement. (This is one justification for calling effect algebras “logics”.)
1. Boolean algebras

In order to describe this construction, we first review Boolean algebras. A Boolean algebra is an orthocomplemented distributive lattice. A lattice is a structure \( (L, \lor, \land) \), where \( L \) is a set, \( \lor, \land \) total binary operations on \( L \) with the following properties. Both operations are associative, commutative, and idempotent (idempotent means, e.g., \( (a \land a = a) \)). In addition, together they are absorptive:

\[
\begin{align*}
    a \land (a \lor b) &= a, \\
    a \lor (a \land b) &= a.
\end{align*}
\]  

\( \lor \) is usually called join, \( \land \) is usually called meet.

These properties are satisfied by letting \( L \) be any powerset (the set of subsets of a given set), and the operations \( \lor, \land \) correspond to \( \cup, \cap \). For \( L = 2^X \) (the power set of \( X \)) we call this lattice the subset lattice of \( X \). The shape similarity of these sets of connectives, and the fact that the everyday meanings of “join” and “meet” are similar in meaning to “union” and “intersection”, respectively, provide a useful mnemonic.

An important alternative characterization of a lattice is as a set partially ordered by a relation we will call \( \leq \). If every pair \( (x, y) \) of elements have both a greatest lower bound (inf) and a least upper bound (sup) according to this ordering, we call these \( x \land y \) and \( x \lor y \), respectively, and the set is a lattice with respect to these operations. Also, for any lattice as defined above, we may define a partial ordering \( \leq \) such that \( \land, \lor \) are inf, sup, respectively, in the ordering. So the two characterizations are equivalent.

A lattice is said to be distributive if meet distributes over join:

\[
    a \lor (b \land c) = (a \lor b) \land (a \lor c) .
\]  

(5)

(This statement is equivalent to its dual (the statement with \( \land \leftrightarrow \lor \)).)

If \( L \) contains top and bottom elements with respect to \( \leq \), we call them 1 and 0. They may be equivalently be defined via \( a = a \land 1 \), \( a = a \lor 0 \) for all \( a \in L \).

We define \( b \) to be a complement of \( a \) if \( a \land b = 0 \) and \( a \lor b = 1 \).

Complements are unique in distributive lattices, not necessarily so in more general lattices. When all complements are unique, we write complementation as a unary relation (operation) \( \top \); this relation is not necessarily total even in distributive lattices with 0, 1.

A Boolean lattice, or Boolean algebra, is a distributive lattice with 0, 1, in which every element has a complement.

Any subset lattice \( L = 2^X \) is a Boolean algebra, with 0 = \( \emptyset \) and 1 = \( X \). An important theorem of Marshall Stone says that any complete, atomic Boolean algebra may be represented as a sub-Boolean algebra of a unique (up to isomorphism) subset lattice with a topological structure (now called the Stone space of the lattice).

Let us formalize the notion of phenomenological theory as follows.

**Definition 2** A phenomenological theory \( \mathcal{P} \) is a set \( \mathcal{M} \) of disjoint finite sets \( M \), together with a set \( \Omega \) of functions (“states”) \( \omega \) from \( \bigcup M \in \mathcal{P} M \) to \([0, 1]\) such that for any \( M \), \( \sum_{x \in M} \omega(x) = 1 \).

\( M \) are the possible measurements; taking them to be disjoint means we are not allowing any \textit{a priori} identification of outcomes of different measurement procedures. Any such identification will be at a higher level of abstraction, via, for example, identification of probabilistically equivalent outcomes. \( \Omega \) is the set of phenomenologically admissible combindia of probabilities for measurement outcomes. \( \mathcal{M} \) is an example of what Foulis calls a “test space”: a set \( T \) of sets \( T \), where \( T \) may be interpreted as operations, (tests, procedures, whatever you want to call them) and the elements \( t \in T \) as outcomes of these operations. We will assume each \( T \) is finite. (Without the interpretation, these are better known in mathematics as hypergraphs or set systems.) Call the set of all outcomes \( \Lambda := \cup T \). In general test spaces, however, the \( T \) need not be disjoint; here they are. Foulis calls such test spaces “semiclassical.” (Sometimes a weak requirement of irredundancy, that no one of these sets is a proper subset of another, is imposed on test spaces; it is automatic here.) We may sometimes call this test space \( \mathcal{M}(\mathcal{P}) \) (recalling, though, that it does not depend on the state-set of \( \mathcal{P} \).) States may be defined on test spaces also, in the obvious way, as functions \( \omega : \Lambda \rightarrow [0, 1] \) such that \( \sum_{t \in T} \omega(t) = 1 \) for any \( T \). It is only when a phenomenological theory is defined in such a more general context, where a given outcome may occur in different measurements, that the question of contextuality (does the probability of a given outcome depend on the measurement it occurs in?) arises at the phenomenological level. By not admitting such a primitive notion of “same outcome,” but distinguishing outcomes according to the measurements they occur in, the construction we make will turn out to guarantee noncontextuality of probabilities even at the later stage where the theory is represented by a more abstract structure in which the elements (effects, or operations) that play the role of outcomes may occur in different operations. We will return to this point in a discussion of what Gleason-type theorems mean in a setting such as ours. But it bears pointing out right now, since the rest of our discussion ignores
it, that the question of whether there can be convincing reasons for admitting a primitive notion of “same outcome” (based perhaps on some existing theory in terms of which the operations and experiments of our “phenomenological theory” are described) is worth further thought. A related point is that test spaces provide a framework in which we can implement a primitive notion of two outcomes of different measurements being the same, but we cannot implement a notion of two outcomes of the same measurement being the same (up to, say, arbitrary labeling). It is not clear why if we can do one, we should rule out being able to do the other. A formalism in which one can do both is that of E-test spaces (the E is for effect). These are sets, not of sets of outcomes, but of multisets of outcomes. Multisets are just sets with multiplicity: each element of the universe is not just in or out of the set, but in the set with a certain nonnegative integer multiplicity. In other words, where sets can be described by functions from the universe $U$ to $\{0, 1\}$ (their characteristic functions), multisets are described by functions from $U$ to $\mathbb{N}$. Note that the set of resolutions of unity in an effect algebra, shorn of its algebraic structure, is an E-test space (whence the “E” for effect in the name “E-test space.”). Not all E-test spaces are such that an effect algebra can be defined on them; those that are are called algebraic. Sufficiently nice E-test spaces are prealgebraic, and can be completed to be algebraic by adding more multisets without enlarging the universe (underlying set of outcomes).

To each phenomenological theory we may associate a set of Boolean algebras, one for each measurement. We will call this set of Boolean algebras the “phenomenological logic,” or even the “phenomenologic” of the phenomenological theory; note, though, that it is independent of the state-set $\Omega$. These are just the subset lattices of the sets $M$, or what I previously called the “propositional logics” of statements about the results of the measurements. We will distinguish them by subscripts on the connectives saying which measurement is referred to, e.g. $\wedge_M$ (although this is redundant due to the disjointness of the measurements).

The phenomenological states $\omega$ of $P$ naturally induce states (which we will also call $\omega$) on the logic of $P$, via $\omega(\{a\}) = \omega(a)$, $\omega(X) = \sum_{x \in X} \omega(x)$. They will satisfy $\omega(M) = 1$ for each $M$, and $\omega(\emptyset) = 0$.

We have, for example ($x$ and $y$ are now subsets of outcomes),

$$\omega(x \lor_M y) = \omega(x) + \omega(y) - \omega(x \land_M y),$$

(7)

(which is equivalent to its dual).

We call the elements of the Boolean algebras of a phenomenologic events, and we will refer to the set of events of $P$ as $V$.

**Definition 3** Events $e, f$ are probabilistically equivalent, $e \sim f$ in a phenomenological theory if they have the same probability under all states:

$$\forall \omega \in \Omega, \omega(e) = \omega(f).$$

(8)

$\sim$ is obviously an equivalence relation (symmetric, transitive, and reflexive). Hence we can divide it out of the set $V$, obtaining a set $V/\sim = : E(P)$ of equivalence classes of events which we will call the effects of the theory $P$. (We have dependence on $P$, rather than just $M$, because although $V$ depends on $M$ but not $\Omega$, $\sim$ depends also on $\Omega$. ) Call the canonical map that takes each element $a \in V$ to its equivalence class, “$e$.”

The images $e(M)$ of the measurements $M$ under $e$ are “measurements of effects.” Together they form an E-test space as defined above (a set of multisets).

We now define on this space a “logic” which is, at least as far as possible, the simultaneous “image” under the map $e$ of each of the Boolean algebras $M$, and show that this logic is a WEA. To this end, we introduce a binary operation $\oplus$ on the effect space.

**Definition 4**

$$e_1 \oplus e_2 := e(a \lor_M b),$$

(9)

for some $a$ such that $e_1 = e(a)$, $b$ such that $e_2 = e(b)$, and $M$ such that $a, b \in M$ but $a \cap b = \emptyset$.

If no such $a, b, M$ exist, $\oplus$ is undefined on the effect space. (If they do exist, we will say they witness the existence of $e_1 \oplus e_2$.) As part of the proof of Theorem [1] we will show from the definition of the map $e$ via probabilistic equivalence and the behavior of probabilities with respect to $\lor_M$, that this definition is independent of the choice of $a, b, M$.

Let $\omega^e$ denote the function from the effects to $[0, 1]$ induced in the obvious way by a state $\omega$ on the Boolean algebra: effects being equivalence classes of things having the same value of $\omega$, we let $\omega^e$ take each equivalence class to $\omega$’s value on anything in it.

One evident property not shared by general WEA’s is that the set $\Omega(E)$ of all states on such a WEA is separating:

**Definition 5** A set of states $\Omega$ on a WEA $E$ is separating if for $x, y \in E$

$$x \neq y \Rightarrow \exists \omega \in \Omega(\omega(x) \neq \omega(y))$$

(10)
Theorem 1 The set $E(\mathcal{P})$ of effects of a phenomenological theory $\mathcal{P}$ with state-set $\Omega$, equipped with the operation $\oplus$ of Def. 2 and the definition $1 = e(1_M)$ (for some $M$) constitutes a weak effect algebra. There exist phenomenological theories for which this is properly weak, i.e. not an effect algebra. For all $\omega \in \Omega$ the functions $\omega^e$ defined above are states on the resulting weak effect algebra. $\Omega^e := \{\omega^e | \omega \in \Omega\}$ is separating on $E(\mathcal{P})$.

The proof is a straightforward verification of the axioms and the statements about states from the definition, and an example for the second sentence.

Proof: We begin by demonstrating $\oplus$ is in fact a partial binary operation. This is done by verifying the independence, asserted above, of the definition of $\oplus$ from the choice of $a, b, M$ and of $1$ from $M$. Suppose $e_1 = e(a) = e(c), e_2 = e(b) = e(d), a, b \in M, c, d \in N, a \neq b, c \neq d, a \land_M b = 0, c \lor_N d = 0$. Consider any state $\omega$ on the set of Boolean algebras which is also in $\Omega$, the states of our phenomenological theory. By the definition of $e$,

$$\omega(a) = \omega(c) \quad \text{and} \quad \omega(b) = \omega(d);$$

therefore $\omega(a) + \omega(b) = \omega(c) + \omega(d)$. Now $\omega(a \lor_M b) = \omega(a) + \omega(b)$ because $a \lor_M b = 0$, and similarly $\omega(c \lor_N d) = \omega(c) + \omega(d)$. In other words, for any state $\omega \in \Omega$, $\omega(a \lor_M b) = \omega(c \lor_N d)$, so $a \lor_M b$ and $c \lor_N d$ are probabilistically equivalent, and correspond to the same effect.

Each Boolean algebra contains a distinguished element 1; by the definition of state on $\mathcal{P}$, these have probability zero, and one, respectively, in all states. Hence they each map to a single effect, and these effects we will call 0 and $\omega$ respectively.

$x, y$ are states, as claimed in the theorem. The set $\Omega^e$ is obviously separating. To be pedantic, suppose there exist effects $x, y$ having $\omega^e(x) = \omega^e(y)$ for all $\omega^e \in \Omega^e$. By the definition of $\omega^e$, $\omega^e(x)$ is the common value of $\omega$ on all $e$-preimages of $x$, and $\omega^e(y)$ is the common value of $\omega$ on all $e$-preimages of $y$. If these values are the same for all $\omega^e$, then the preimages of $x$ and of $y$ are all in the same equivalence class, so $x = y$. Hence, $\Omega^e$ is separating.

We now verify that $\oplus$ satisfies the weak effect algebra axioms.

(EA1) Strong commutativity: If $a, b \in M$ witness the existence of $x \oplus y$ as described in the definition of $\oplus$, by symmetry of $\lor_M$ and $\land_M$ (which enter symmetrically in the definition of $\oplus$) they also witness the existence of $y \oplus x$ and its equality with $x \oplus y$.

(WEA2) Weak associativity. Let $a, b \in M, e(a) = x, e(b) = y, a \cap b = \emptyset$, so that $a, b$ witness the existence of $x \oplus y$, and also let $c, d \in N$ and disjoint, $e(c) = z, e(d) = x \oplus y$, so $c, d$ witness the existence of $(x \oplus y) \oplus z$. Similarly let $b', c' \in P$ witness the existence of $y \oplus z$ and $a', f \in Q$ witness the existence of $x \oplus (y \oplus z)$, so that $e(a') = x, e(f) = y \oplus z$, and $a', f$ are disjoint. Then $\omega^e(x \oplus y) = \omega(a) \oplus \omega(b)$ and

$$\omega^e((x \oplus y) \oplus z) = \omega(a) + \omega(b) + \omega(c).$$ (12)

Also $\omega^e(y \oplus z) = \omega(b') \oplus \omega(c') = \omega(b) \oplus \omega(c)$, so

$$\omega^e((x \oplus (y \oplus z)) = \omega(a') \oplus \omega(f) = \omega(a) \oplus \omega(b) \oplus \omega(c).$$ (13)

But $\omega^e((x \oplus y) \oplus z) = \omega^e(x \oplus (y \oplus z))$ for all $\omega^e$ implies $(x \oplus y) \oplus z = x \oplus (y \oplus z)$ by the fact that $\Omega^e$ is separating.

(EA3) Define $e'$ to be $e(a')$, for any $a$ such that $e(a) = e$, and $a'$ is a's unique complement in the Boolean algebra of the measurement $M$ containing it. Since for any state, $\omega(a') = 1 - \omega(a)$ and this probability is independent of $a$ long as $e(a) = e$, $e'$ as thus defined is independent of which $a$ is chosen. Moreover, since $a \land_M a' = 0$ $e \oplus e' \equiv e(a) \oplus e(a')$ is defined and equal to $e(a \lor_M a') = e(1_M) = 1$, so that $f$ as we just defined it satisfies (EA3).

(EA4) Note that $x \oplus 1$ is equal to $e(a \lor_M 1_M)$, for some $M$ containing $a$ and with unit $1_M$, where $a \land_M 1 = 0$ and $e(a) = x$. But each $M$ has a unique $a$ such that $a \land_M 1_M = 0_M$, namely $0_M$. So an $x$ such that $x \oplus 1$ exists; it must be $e(0_M) = 0$.

This proves the first part of the theorem. We remark that $1' \equiv e(1') = e(0_M)$, so defining 0 as $e(0_M)$ for any $M$ coincides with the usual effect algebra definition as 1'. We now construct the counterexample required by the second part.

Consider an empirical theory consisting of states on the two atomic Boolean algebras:

$$M : \{ (a, b) \} \quad N : \{ (c, d) \} \quad (g)$$ (14)

with the indicated $a, ..., g$ being atoms of the Boolean algebras involved (“elementary measurement outcomes”). The vertical lining-up of parentheses in (14) visually indicates conditions we will impose on the theory: that all states of our phenomenological theory respect $\omega(a \lor_M b) = \omega(c)$ and $\omega(f) = \omega(d \lor_N g)$; further, let our theory contain states
with nonzero probability for each of \( a, b, c, d, f, g \). There are plenty of perfectly good empirical theories satisfying these constraints, but \( \oplus \) on the effect set of such a theory will not exhibit strong associativity: although \( e(a) \oplus e(b) \) exists and is equal to \( e(c) \), and \( e(c) \oplus e(d) \) exists and is therefore equal to \( (e(a) \oplus e(b)) \oplus e(d) \), no effect \( h \) exists with \( e(h) = e(b) \oplus e(d) \).

Although we have shown that the existence of witnesses for \( x \oplus y \) and \( (x \oplus y) \oplus z \) need not imply it, suppose these hold and also a witness for \( y \oplus z \) exists. Does this imply a witness for \( x \oplus (y \oplus z) \) exists? Again, the answer is no, for the same reason. Just because there is something \( h \) with \( \omega(h) = \omega(b) + \omega(d) \) for all \( \omega \), doesn’t mean something probabilistically equivalent to \( h \) is a possible outcome of a measurement which also has an outcome probabilistically equivalent to \( a \). We can illustrate this by adding a measurement to the previous example

\[
M : \begin{pmatrix} a & b \\ f & \end{pmatrix},
N : \begin{pmatrix} c & d \\ g & \end{pmatrix},
O : \begin{pmatrix} k \\ h \end{pmatrix}
\]

Note that the presence of \( k \) twice does not indicate two copies of outcome \( k \) (which is not allowed in our formalism for phenomenological theories’ test spaces) but rather just indicates that \( \omega(k) = \omega(a) + \omega(g) \). (The diagram also indicates the fact, which follows, that \( \omega(h) = \omega(b) + \omega(d) \).

The theorem indicates that we do not even have the semigroup part of “partial abelian semigroup”, though we have abelianity, a weaker associativity (associativity-where-defined), and some other special properties (the remaining effect-algebra axioms) that not all PAS’s have, but all effect algebras do.

An interesting question is whether the objects thus constructed share particular properties in addition to those of a weak effect algebra. We mentioned that the image of the state-set of the phenomenological theory from which \( \mathcal{E} \) was constructed, which in general could be a proper subset of \( \Omega(\mathcal{E}) \), is separating. As we will see in the discussion of convex effect algebras, this property has nice consequences.

There is a well-developed and attractive theory of effect algebras. It may therefore seem disappointing that our construction of a logic on effects yields weak effect algebras, rather than effect algebras. If the following conjecture is true (I suspect it is straightforward to show) then the theory of effect algebras may be quite useful in this more general context.

**Conjecture 1 (Completion conjecture for WEA’s)** Let \( \mathcal{E} \) be a WEA obtained from a phenomenological theory. A unique effect algebra \( \overline{\mathcal{E}} \), which we call the completion of \( \mathcal{E} \), can be constructed from \( \mathcal{E} \) as follows. Whenever one side of the associativity equation exists, adjoin new elements if necessary to make the other side exist, and impose the equation. This can also be characterized as the smallest effect algebra containing \( \mathcal{E} \) as a sub-weak-effect-algebra (with the latter concept appropriately defined).

There are some similarities between this conjecture and the fact that one can make a “pre-algebraic” test space into an algebraic one \([12]\), and similar results involving \( E \)-test spaces.

The adjunction of these new elements is an interesting theoretical move. In constructing theories, we often suppose the existence of things that do not, at least initially, correspond to things in the available phenomenology. The idea of including all Hermitian operators as observables in quantum mechanics is an example; as mentioned above, there has been much discussion of whether all of these actually correspond to anything observable. This has motivated the search, often successful, for methods of measuring various observables that had previously not been measured, and the development of a general theory, of algorithmic procedures for measuring certain observables. The situation with the elements whose existence is required by weak associativity could prove similar. Although the resulting effect algebra is not initially the WEA induced by probabilistic equivalence from the phenomenological theory, it could motivate the search for empirical methods of making measurements which would correspond to the additional resolutions of unity which were introduced to make the initial WEA into an effect algebra. In any case, it would be worth studying the nature of information processing, and information theory if this is possible, in properly weak effect algebras versus their completions.

In the next section, I will discuss the extension of these sorts of ideas to a framework allowing sequential measurements, and in the section following that, possible applications. However, we are now ready for a few remarks on the significance of Gleason’s theorem \([17]\) in this context.

**VII. GLEASON-TYPE THEOREMS IN LIGHT OF PROBABILISTIC EQUIVALENCE**

For quantum systems of finite dimension greater than two, Gleason’s theorem can be interpreted as saying that if mutually exclusive quantum measurement results are associated with mutually orthogonal subspaces of a Hilbert space, and exhaustive sets of such measurements to direct sum decompositions of the entire space into such subspaces,
and if the probability of getting the result associated to a given subspace in a given measurement is independent of the
direct sum decomposition (measurement) in which it occurs (probabilities are “noncontextual”) then the probabilities
for these results must be given by the trace of the product of the projector onto the given subspace with a density
operator.

A similar theorem with the resolutions of unity into orthogonal projectors replaced by resolutions into arbitrary positive operators has been obtained by [21], and independently by Caves, Fuchs, and Mannes [22, 41, 45]. In the next section we will see how this “B/CFRM” theorem is a case of a general fact about convex effect algebras.

Sometimes Gleason-type theorems are used in quantum foundations arguments to justify the quantum probability
laws. Then the question naturally arises: what justifies the two assumptions that the probability laws are noncontex-
tual, and that they are associated with orthogonal decompositions, or positive resolutions of unity, on a Hilbert space?
Although the construction above via probabilistic equivalence results in structures much more general than Hilbert
space effect algebras, or effect algebras of projectors on Hilbert spaces, it automatically results in noncontextual prob-
ability laws on the resulting weak effect algebras. This argument, of course, starts from probabilities, so it would be
circular to use it to justify noncontextuality and then turn around and use noncontextuality, via Gleason’s theorem,
to “justify” the probabilities. From our point of view, we have a result that we can fairly elegantly, conveniently
represent any empirical theory by a set of noncontextual probability assignments on a certain weak effect algebra
(and, if the completion conjecture is correct, embed this in an effect algebra). In the case of quantum theory, this
general recipe provides both the Hilbert space structure and the trace rule for probabilities, as a representation of the
compendium of “empirical” probabilities (perhaps somewhat idealized by the assumption that any resolution of unity
can be measured) of quantum theory.

The generalization of Gleason-like theorems to weak effect algebras, effect algebras, and similar structures are
theorems characterizing the full set of possible states on a given such structure, or class of such structures. The example of convex effect algebras will be discussed in the next section. In the particular case of a Hilbert space effect algebra, the import of the B/CFR theorem, from the operational point of view advanced above, is that the quantum states constitute the full state space of the “empirically derived” effect algebra. This observation is perhaps more interesting when one recalls that in other respects, the category of effect algebras probably does not have enough structure to capture everything we would like it to about quantum mechanics: for example, the natural category-theoretic notion of tensor product of effect algebras ([32]; see also [92, 93]), applied to effect algebras of finite dimensional Hilbert spaces, does not give the effect algebra of the tensor product Hilbert space (or of any Hilbert space), as one sees from a result in [14] (for a similar result involving projectors only, see [12]). Possibly relatedly, a natural category of morphisms for convex effect algebras, those induced by positive (order-preserving) linear maps on the underlying ordered linear space (see below), is larger in the quantum case than the “completely positive” maps usually considered reasonable for quantum dynamics. Nevertheless for a given Hilbert space effect algebra, its full state space (space of all possible states) is precisely the set of possible quantum states.

The role of Gleason-like results depends to some extent on point of view. In the project of exploiting the analogies
between quantum states and probabilities, their conceptual role is perhaps more obvious. Probabilities are, roughly,
“the right way” (nonarchimedeanity issues aside) to represent uncertainty, and to represent rational preferences over
uncertain classical alternatives. In this project, it would be very desirable to see quantum states as “the right way”
to deal with uncertainty in a nonclassical situation: the Hilbert space structure perhaps sums up the “nonclassicality
of the situation,” and the probabilities can be seen as just the consequence of “rationality” in that situation. For such
an interpretation of Gleason’s theorem, see for example [8] and [22]. This might seem to lay somewhat more stress on
formal analogies between quantum logic and classical logic than the operational approach I have been outlining; which
constructs the Hilbert space structure and the probability state-set simultaneously from the quantum probabilities.

But after the construction we can still make if we like the same interpretational division into “structure” (here, Hilbert
space effects) and “any state on that structure,” done after the fact, so that quantum mechanics (or whatever weak
effect-algebra theory we get) just looks like “any way to be rational in a certain situation,” though the structure
of the situation is ultimately empirically derived from probabilities as well. Interestingly, another point of view
from which the Gleason’s theorem derivation of quantum probabilities takes on a special significance is the relative
state interpretation. Here, one also takes the Hilbert space structure as given, and there is an implicit assumption
that orthogonal subspaces correspond to mutually exclusive statements about the system. Various arguments have
then been given by various authors, some based on additional assumptions, that the only consistent assignment of
probabilities is the standard quantum one. This suggests that the “structure of the nonclassical situation” mentioned
above might be described in terms of measurement outcomes (sometimes called “propositions” or “properties”) having
probability zero or one (at least in a framework where the outcomes are discrete); then Gleason’s theorem or analogues
for other “property” structures, might give the set of possible probability assignments for such a structure. This idea
need not be associated with the relative states interpretation. Indeed, this is my take on the “Geneva” approach to
empirical theories (rooted in the work of Jauch and Piron on “property lattices”).
A. Convex effect algebras

In an “operational” view of theories, it is natural to take the space of operations one may perform as convex. This represents mathematically the idea that given any two operations $M_1$ and $M_2$, we can perform the operation $(\lambda_1 M_1, \lambda_2 M_2)$ (where $\lambda_i \geq 0, \lambda_1 + \lambda_2 = 1$) in which we perform one of $M_1$ or $M_2$, conditional on the outcome of flipping a suitably weighted coin (or, in more Bayesian terms, arrange to believe that these will be performed conditional on mutually exclusive events, to which we assign probabilities $\lambda_1, \lambda_2$, that we believe to be independent of the system under investigation). A worthwhile project is to trace the implications of this assumption, or more general assumptions involving ways of combining empirical event logic and probability which might be made at the level of phenomenological theory, through the probabilistic equivalence derivation of weak effect algebras, arriving, one hopes, at a notion of convex weak effect algebra. We will not work this out here, but rather observe that one might expect that if we looked at the coin face and saw the index “i” and obtained the outcome $a$ of $M_i$, this should correspond to an outcome $\lambda a$ of $(\lambda_1 M_1, \lambda_2 M_2)$, and that any state should satisfy $\omega(\lambda a) = \lambda \omega(a)$.

Similar definitions may be made at the level of effect algebra or weak effect algebra (see below). For effect algebras constructed via probabilistic equivalence from such convex phenomenological theories, they are consequences of appropriately formalized phenomenological assumptions of convexity (details will appear elsewhere). It would be quite reasonable, also, to pursue the consequences of imposing a generalized convexity based on a more refined notion of “vector probabilities”, or other representations of uncertainty by nonarchimedean order structures. Such notions of generalized subjective probabilities (and correspondingly generalized notions of subjective utility functions) can result from Savage-like representation theorems for preferences satisfying “rationality” axioms but not certain technical axioms such as “archimedeanity” assumptions that make possible real-valued representations [37, 60, 61]. Such “probabilities” can do things like making statements about the relative probability of probability-zero events (which events even standard probability theory on continuous sample spaces cannot always contrive to consider impossible), conditioning on such events, and so forth. In such a construction, the states of the phenomenological theory would have to take these generalized probabilities as values, and “probabilistic” equivalence would have to be defined in terms of them. We will avoid such complications as much as possible here, but knowing about them may help one understand the role of some technical conditions in results to be discussed below.

The notion of convex effect algebra has been explored by Gudder and collaborators.

Definition 6 A convex effect algebra is an effect algebra $(E, u, \oplus)$ with the additional assumptions that for every $a \in E$ and $\alpha \in [0, 1] \subset \mathbb{R}$ there exists an element of $E$, call it $\alpha a$, such that (for arbitrary $\alpha, \beta \in [0, 1]$)

$$(C1) \quad \alpha(\beta a) = (\alpha \beta)a,$$

$$(C2) \quad \text{If } \alpha + \beta \leq 1 \text{ then } \alpha a \oplus \beta a \text{ exists and is equal to } (\alpha + \beta)a,$$

$$(C3) \quad \alpha(a \oplus b) = \alpha a \oplus \alpha b \text{ (again, the latter exists),}$$

$$(C4) \quad 1a = a.$$

The mapping $a \mapsto \alpha a$ from $[0, 1] \times E$ to $E$ is called the convex structure of the convex effect algebra.

Gudder and Pulmannová [51] showed that “any convex effect algebra admits a representation as an initial interval of an ordered linear space,” and in addition if the set of states on the algebra is separating, the interval is generating. To understand this result, we review the mathematical notion of a “regular” positive cone (which we will just call cone); it is basic in quantum information science, for example because the set of quantum states are the normalized members of a cone; so are the separable states of a multipartite quantum system, the (unnormalized) completely positive maps, the positive maps. It is also equivalent to the notion of ordered linear space, as we shall indicate.

Definition 7 A positive cone is a subset $K$ of a real vector space $V$ closed under multiplication by positive scalars. It is called regular if it is

- Convex (equivalently, closed under addition: $K + K = K$.)
- Generating ($K - K = V$, equivalently $K$ linearly generates $V$.)
- Pointed ($K \cap -K = \emptyset$, so that it contains no nonnull subspace of $V$), and
- Topologically closed (in the Euclidean metric topology, for finite dimension).

(In this definition, we adhere to the common mathematical convention that when sets are inserted into operations in place of operands, the expression refers to the set of things obtainable by inserting all combinations of elements of the sets into the operations: thus $K - L$ is $\{k - l : k \in K, l \in L\}$ and so forth.)

Such a positive cone induces a partial order $\geq$ on $V$, defined by $x \geq_K y := x - y \in K$. $(V, \geq_K)$, or sometimes $(V, K)$, is called an ordered linear space. The Hermitian operators on a finite-dimensional complex vector space,
with the positive semidefinite ordering induced by the cone of positive semidefinite operators, are an example. (A relation \( R \) is defined to be a partial order if it is reflexive \((xRx)\), transitive \((xRy & yRz \Rightarrow xRz)\) and antisymmetric \((xRy & yRx) \Rightarrow x = y)\). The partial orders induced by cones have the property that they are “affine-compatible”: inequalities can be added, and multiplied by positive scalars. If one removes the requirement that the cones be generating, cones are in one-to-one correspondence with affine-compatible partial orderings. In fact, the categories of real vector spaces with distinguished cones, and partially ordered linear spaces, are equivalent.

We pause to give motivations, some mathematical and some involving applications to empirical theories, of some of the seemingly technical conditions of regularity. Such a cone may represent, for example, the set of unnormalized probability states of a system, or a set of expectation values of observables. The normalized states may be generated by intersecting it with an affine plane not containing the origin. Convexity is fairly clearly motivated by operational considerations, such as those in the definition of convex effect algebra above, or in the desire to have a normalized state set given by intersecting the cone with an affine plane be convex. Topological closure is required so that the cone has extreme rays, and the convex sets we derive by, for instance, intersecting it with an affine plane, will have extreme points if that intersection is compact; then the Krein-Milman theorem states that these extreme points convexly generate the set. (An affine plane is just a translation of a \( d - 1 \)-dimensional subspace: for \( d = 3 \), a plane in the sense of high school geometry.) In “empirically motivated” settings such as ours, (in which the metric on the vector space will be related, via probabilities, to distinguishability of states or operations), one can argue that limit points can be as indistinguishable as you want from things already in the cone, so closing a cone cannot have empirically observable effects, and may as well be done if it is mathematically convenient. In the presence of some of the other assumptions, pointedness ensures that the intersection with an affine plane can be compact. Its appearance in the representation theorem for convex effect algebras (presumably essentially because the convex sets one gets via states tend to be compact intersections of an affine plane with such a cone) is one “operational” justification for pointedness. Pointedness also has a clear geometric interpretation: if \( K \cap -K \) is a vector subspace other than 0, then instead of a “point” at zero the cone could have an “edge” (if it is a 1-d subspace), which is why nonpointed cones are often referred to as “wedges”; of course the “edge” \( K \cap -K \), which must be a subspace, could have dimension higher than one, in general. The property of being generating is usually appropriate because any non-generating cone is generating for a subspace, and we may as well not drag around the extra dimensions. When several cones are considered at once, this might no longer be appropriate.

An initial interval in such a space is an interval \([0,u]\) defined as the set of things between zero and \( u \) in the partial ordering \( \geq \), i.e. \( \{ x \in V : 0 \leq_K x \leq_K u \} \). It is generating if it linearly generates \( V \) (i.e. anything in \( V \) can be written as a linear combination of things in \([0,u]\)). It can be viewed as a convex effect algebra, called a linear effect algebra, by letting \( \oplus \) be vector space addition restricted to \([0,u]\) and the convex structure be the restriction of scalar multiplication.

The representation theorem is to be interpreted as saying that any convex effect algebra is isomorphic (as a convex effect algebra) to some such linear convex effect algebra. In the case of finite-dimensional quantum mechanics the vector space and cone are \( H_d \) and the positive semidefinite cone mentioned above, and the interval referred to in the representation theorem is of course \([0,1]\).

In addition to the requirements for states on an effect algebra, states on a convex effect algebra must satisfy \( \omega(\lambda a) = \lambda \omega(a) \). The set of all possible states on a convex effect algebra may therefore be characterized via a version of Lemma 3.3 of [52], which describes it for linear effect algebras \([0,u]\). First, some definitions. The dual vector space \( V^* \) for real \( V \) is the space of linear functions (“functionals”) from \( V \) to \( \mathbb{R} \); the dual cone \( K^* \) (it is a cone in \( V^* \)) is the set of linear functionals which are nonnegative on \( K \). Then \( \Omega([0,u]) \), the set of all states on \([0,u]\) when the latter is viewed as a convex effect algebra, is precisely the restriction to \([0,u]\) of the set of linear functionals \( f \) positive on \( K \) and with \( f(u) = 1 \) (“normalized” linear functionals). Note that the restriction map is a bijection. Thus, viewing things geometrically, the states (restricted functionals) are in fact in one-to-one correspondence with the (unrestricted) functionals in the intersection of \( K^* \) with the affine plane in \( V^* \) given by \( f(u) = 1 \). Since any linear functional on the \( d^2 \)-dimensional vector space \( H_d \) of Hermitian operators on \( \mathbb{C}^d \) has the form \( X \mapsto \text{tr} AX \) for some \( A \), while the dual to the positive semidefinite cone in \( H_d \) is the set of such functionals for which \( A \geq 0 \) (i.e., the positive semidefinite cone is self-dual \((K = K^*)\)) this Lemma tells us that the states of a finite-dimensional Hilbert space effect algebra are precisely those obtainable by tracing with density matrices \( \rho \); in other words, the B/CFRM theorem for POVMs is a case of this general characterization of states on convex effect algebras. This illustrates the power and appropriateness of this approach (and probably other convex approaches, in which similar characterizations probably exist) to empirical theories, and to problems in quantum foundations. Gleason’s theorem itself cannot be established in this way, because the effect algebra (which is also an orthoalgebra) of projectors is not convex. However, there may be a natural notion of “convexification” of effect algebras according to which \([0,1]\) is the convexification of the effect algebra of projectors. Interesting questions are then, which effect algebras can be convexified, and for which of those (as for the effect algebra of quantum projectors) convexification does not shrink the state-space. Conversely, we
might ask for ways of identifying special subalgebras of effect algebras, composed of effects having special properties like “sharpness”, perhaps having additional structure such as that of an orthoalgebra.

B. Sequential operations

The operational approach I am advocating suggests that one consider what general kinds of “resources” are available for performing operations. Provided both system and observer are sufficiently “small” portions of the universe, it may be reasonable to suppose that the observer may use yet other subsystems (distinct from both observer and system) as an “apparatus” or “ancilla” to aid in the performance of these operations, that the apparatus may be initially independent of the system and observer, and that the combination of apparatus and system may be viewed as a system of the same general kind as the original system, subject to the same sort of empirical operational theory, with a structure, and a state, subject to certain consistency conditions with that of the original system. (Convexity is a case of this, the ancilla functioning as “dice.”) It may be that in some limits some of these assumptions break down, but it is still worth investigating their consequences for several reasons: so that we can recognize breakdowns more easily, so that we may even acquire a theoretical understanding of when and why to expect such breakdowns, and because we may gain a better understanding of why empirical theories valid in certain limits (say, small observer, small apparatus, small system) have the kind of structure they do.

One such structure, already mentioned, is convexity: provided one can imagine the observer conditioning the performance of various operations on events (say, “dice throws”) independent of the system, and provided there is a sufficiently rich supply of such events with different (say, subjective) probabilities that it is a reasonable idealization to assume one can get essentially any desired convex combination, then we should include all such operations involving conditioning on such “dice” among our possibilities. Other such elementary combinations and conditionings of operations should probably be allowed: essentially, the set of operations should be extended to allow including them as subroutines in a classical randomized computation. (Of course, this will not always be appropriate; for example, it would clearly not be in constructing examples of theories, that are not even classically computationally universal.) Among other things, this might get us the $\oplus$ operation previously obtained as the image of OR($\lor$) in Boolean propositional logics about each operation’s outcomes, “for free,” as we can use classical circuitry to construct procedures whose outcomes naturally correspond to propositional combinations of the outcomes of other procedures, and will have the same probabilities as those propositional combinations.

This leads us to the consider the possibility that the set of possible operations be closed under conditional composition. This means that given any operation $M$, and set of operations $M_\alpha$, $\alpha \in M$, there is an operation consisting of performing $M$, and, conditional on getting outcome $\alpha$ of $M$, then proceeding to perform $M_\alpha$. This assumption is extremely natural, but nevertheless substantive, in the sense that one could imagine physical theories that did not satisfy it. Some outcomes of procedures might destroy the system, or so alter it that we can no longer perform on it all the procedures we could before. Nevertheless, it is worth investigating the structure of theories satisfying the assumption (which certainly applies to the idealization of finite-dimensional quantum mechanics in terms of completely positive maps on a given Hilbert space). The structures obtained when conditional composition is not universally possible might turn out to be understandable as partial versions of those we obtain when it is always possible, or in some other way be easier to understand once the case of total conditional composability is understood. An operation in this framework, then, can be viewed as a tree with a single root node on top, each node of which is labelled by an operation and the branches below it labelled by the outcomes of the operation, except that the leaves are unlabelled (or redundantly labelled by the labels of the branches above them). The interpretation is that the root node is the first operation performed, and the labels of the daughters of a node indicate the operation to be performed conditional on having just obtained the outcome which labels the branch leading to that daughter.

From now on, we mean by phenomenological theory a phenomenological operational theory closed under conditional composition. If we extend a phenomenological theory by making this requirement, then the new outcome-set contains all finite strings of elements of the old outcome set. Given closure under conditional composition, a given string can now appear in more than one measurement. In order that the construction of dividing out operational probabilistic equivalence can work, we will have to require that the empirical probability of the string be noncontextual. We will also use a different notion of probabilistic equivalence: $x \sim y$ iff for any $a, b$, $\omega(axb) = \omega(ayb)$, where $x, y, a, b$ are all strings. In our context the noncontextuality assumption can actually be derived from the disjointness of “elementary” operations (those not constructed via composition) and the assumption that the choice of operation at node $n$ of the tree describing an operation constructed via conditional composition cannot affect the probabilities of outcomes corresponding to paths through the tree not containing node $n$. This is how one might formalize a generalization of the “no Everett phone” requirement suggested in Polchinski’s article: the probability of an outcome sequence cannot depend on what operation we would have done if some outcome in this sequence had not occurred.

With suitable additional formalization of the notion of phenomenological operational theory, and appropriate def-
inititions of $\oplus$ and a sequential product on the resulting equivalence classes, one can prove that dividing probabilistic equivalence out of such a set of empirical operations, in a manner similar to the construction of weak effect algebras via probabilistic equivalence, gives what I will call a weak operation algebra. The details will be presented elsewhere. Here I will exhibit the quantum-mechanics of operations as a case of a general structure, an operation algebra (OA), which I view as the analogue, for operations, of an effect algebra. The structure will be related to the notion of sequential effect algebra (SEA) studied by Gudder and Greechie \[50\], but differ from it in important respects.

Since this structure will be a partial abelian semigroup, with extra structure involving only the PAS operation $\oplus$, with a product meant to represent composition of operations, and additional axioms about how the two interact, we will discuss some more aspects of PASes (following \[93\]) before defining operation algebras. The reader might want to keep in mind the algebra of trace-nonincreasing completely positive maps (with $\oplus$ as addition of maps and the product as composition of maps) as an example to help understand this abstraction.

Recall that a PAS is a set with a strongly commutative and strongly associative partial binary operation $\oplus$ defined on it. Define a zero of a PAS as an element 0 such that for any $a$, $a \oplus 0 = a$. (Uniqueness follows.) If a PAS does not have a zero, we may freely adjoin one; we henceforth include its existence as part of a PAS. A PAS is cancellative if $x \oplus y = x \oplus z \Rightarrow y = z$, positive if $a \oplus b = 0 \Rightarrow a, b = 0$. The relation $\leq$ on a PAS is defined by $x \leq y \Leftrightarrow \exists z \ x \oplus z = y$. Part of Lemma 1.2 of \[93\] is that in a cancellative, positive PAS $\leq$ is a partial ordering. In such a PAS, we define $T$ as the set of top elements of the partial ordering (i.e. $T = \{t \in O|a \oplus t \text{ exists } \Rightarrow a = 0\}$). In a cancellative PAS we define $x \oplus y$ as that unique (by cancellativity) $z$, if it exists, such that $y \oplus z = x$. Define a chain in a partially ordered set $P$ (such as a cancellative, positive PAS) as a set $C \subseteq P$ such that $\leq$ restricted to $C$ is total.

**Definition 8** An operation algebra is a cancellative, positive PAS equipped with a total binary operation, the sequential product, which we write multiplicatively. With respect to the product, the structure is (OA5) a monoid (the product is associative) with (OA6) a unit $1$ (semigroup is sometimes used as a synonym for this unital monoid structure). The remaining axioms involve the interaction of this monoid structure with the PAS structure.  

(OA7) $0c = c0 = 0$.  

(OA8) $(a \oplus b)c = ac \oplus bc$.  

$ab \oplus c = ab + bc$ (distributive laws).  

(OA9) $1 \in T$.  

(OA10) Every chain in $O$ has a sup in $O$.

Note that the sup mentioned in (OA10) is not necessarily in the chain. The existence of an inf (again, not necessarily in the chain, though) for chains, indeed for sets, is guaranteed by the assumption of existence of a zero. Our structure is not an effect algebra because we do not assume it is (as a PAS) unital (i.e., has at least one unit). A unit of a PAS is an element $u$ such that for any $a$, there is at least one $b$ such that $a \oplus b = u$. In a cancellative, positive, unital PAS (equivalently, effect algebra) there is a unique unit, the sole element of the top-set $T$. Axiom (OA10) might need strengthening in order to obtain some of the results one would like. Notably, we would like to have a representation theorem in which the operations belong to a cone in a vector space (and thus belong to an algebra in one of the usual mathematical senses, the sense of a vector space with an appropriate product). Aside from belonging to a cone, the special nature of the convex set of operations in such a representation theorem would be expressed by an additional requirement, deriving from (OA10), which would specialize to the trace-nonincreasing requirement in the case of the quantum operation algebra (and generalize the initial interval requirement in the analogous (Gudder-Pulmannová) representation theorem for effect algebras).

We shall now show that quantum mechanics provides an example of this structure. We refer to the set of linear operators on $\mathbb{C}^d$ as $B(\mathbb{C}^d)$.

**Proposition 1** The set of trace-nonincreasing completely positive linear maps on $B(\mathbb{C}^d)$, with the identity map $I$ as 1, the map $M$ defined by $M(X) = 0$ for every $X$ as 0, ordinary addition of maps as linear operators, restricted to the trace-non-increasing interval, as $\oplus$, and composition of maps as the sequential product, forms an operation algebra. Its top-set $T$ is the set of trace-preserving maps.

**Proof:** The commutativity (OA2) and associativity (OA1) of $\oplus$ and the behavior of 0 (OA7), and the unital monoid structure (OA5 and 6) are immediate. Cancellativity holds for addition in any linear space, so since $\oplus$ is here a restriction of addition on a linear space of linear maps, it is cancellative (OA3). It is positive (OA4) because $A + B = 0 \Rightarrow A, B = 0$ for $A, B$ in a pointed cone (such as the cone of completely positive linear maps). (OA8) follows from the distributivity of multiplication of linear operators over addition of linear operators. The top-set $T$ is the set of trace-preserving operations, which follows from the easy observation that if you add any operation besides the zero operation to a trace-preserving operation, the result is not trace-nonincreasing. (OA9) follows since the identity operation is trace preserving. (OA10), and the statement about the top-set being the trace-preserving operations, may require a topological sort of argument, and needs to be worked out.  

\[\blacksquare\]
Let us note the interpretation of $\oplus$ and $\odot$ in terms of the HK representation of a map $\mathcal{A}$ in terms of operators $A_i$, so the map acts as $X \mapsto \sum_i A_i \{X\}^i$. Modulo irrelevant details of indexing, the HK representation sequence $A_i$ is a multiset $[A]$ of operators $A$ such that $A^\dagger A \leq 1$. $\mathcal{A} \odot \mathcal{B}$ exists if there are HK representations $[A], [B]$ such that $[B]$ is a submultiset of $[A]$. (Equivalently, there are standard HK representation sequences $A_i$ and $B_i$ such that $B_i$ is an initial segment of $A_i$, i.e. $\mathcal{B}(X) = \sum_i A_i \{X\}^i$ where $i$ ranges over the first $k A_i$. Thus it is obvious that $\mathcal{A} \odot \mathcal{B}$ will not always exist.

We define a weak operation algebra to satisfy all the above axioms except that associativity is replaced with weak associativity (whose statement is the same as in the definition of weak effect algebra). With suitable additional formalization of the notion of phenomenological operational theory, and definitions of $\oplus$ and sequential product on the equivalence classes, one can show:

**Theorem 2** The set of equivalence classes obtained by dividing the relation of operational probabilistic equivalence defined above out of a phenomenological operational theory, has a natural weak operation algebra structure.

A more formal statement of the theorem, and a proof, will be given elsewhere.

Note that if we have operational limits on conditional composition, as discussed above, we might accomodate that by modifying the notion of operation algebra (or WOA) to make the multiplicative monoid structure partial. It would then be interesting to investigate the conditions under which this partial structure is extendible to a total one (as well as the conditions under which a WOA can be completed to an OA).

We can add a convex structure to an OA with little difficulty. We just introduce a map of multiplication by scalars in $[0, 1]$ (i.e. a map from $[0, 1] \times \mathcal{O} \to \mathcal{O}$) such that the axioms (C1–C4) of convex effect algebras hold, and also

\[(COA15)(aa)b = a(ab) = a(ab) .\]  \[(16)\]

Again, we will expect such a structure to emerge from an operational equivalence argument applied to a suitable notion of convex operational phenomenological theory.

**VIII. DYNAMICS AND THE COMBINATION OF SUBSYSTEMS IN OPERATIONAL THEORIES**

The operation algebra approach sketched above implicitly includes a kind of dynamics, although without explicit introduction of a real parameter for time. Some operation algebras are probably extendible in natural ways to have a continuous semigroup structure related to their sequential product, that might allow for the introduction of a notion of time, although in this case conditional composition would probably be restricted by appropriate scheduling constraints. However, in the operation algebra sketched for quantum mechanics, the assumption is that any unitary—indeed, any completely positive—time evolution can be achieved. The time taken for the evolution is neglected, and the temporal element of the interpretation (which applies more clearly to the phenomenological operational theory than its image, the operation algebra) is only the primitive one that when one measurement is done conditional on the result of another, it is thought of as being done after the result of the first is obtained. This is the interpretation of the tree structure describing conditional composition, and since conditional composition is also basic to the motivation of this tree structure, it seems almost tautological. A stronger interpretation of the tree structure would be to assume the entire operation at a node is finished before any of the daughter node operations begin, so that time is associated with distance, in nodes, from the root. A more substantial notion of time might be introduced in many different ways by adding structure to the operation algebra, e.g. by some consistent specification of how long each evolution takes, or by the assumption that each evolution can be done in any desired finite amount of time. The latter is a very strong assumption. A realistic consideration of these matters would involve a much more detailed account of the interactions between apparatus and system that are actually available. This is an important part of the project I propose, but I will not pursue it much here. It reminds us, though, of one of the important lessons of QIP for foundations mentioned in Section [11] that which operations are possible may depend on the resources available, and that the beautiful structures one sometimes encounters as operational theories may be idealized. In particular, much of the attempt to implement QIP involves struggling with the limitations imposed by the limited nature of the subsystems, and interactions, physics makes available. It is probably important to try to incorporate these limitations within the structures used in the operational approach to characterize systems. [11] can be viewed as the beginning of an attempt to incorporate such restrictions, describable as a limitation of the resources available for control and observation in terms of a Lie subalgebra of the full Lie algebra $\mathfrak{s}(d)$ appropriate to arbitrary quantum operations, generalizing the notion of entanglement where the subalgebra is the "multilocal" one that is the direct product of local $\mathfrak{s}$'s. "The physics" may be though to include much more than just Hilbert space: preferred tensor product structures, symmetries, the whole business of representation theory. Another approach to involving this in operational theories has been the inauguration, in works such as [40] and [94], of a theory of group actions on empirical structures such as test spaces, orthoalgebras, and effect algebras.
It is virtually certain that when one attempts to coordinate operational theories into an overall picture, their idealized nature will have to be taken into account, and indeed perhaps explained in terms of the overall picture. Incidentally, this whole discussion raises the question of whether the operation algebra induced via probabilistic equivalence is going to wind up including operations which have strings of more than one elementary outcome as outcomes, but which are not conditional composition trees. The suspicion that it might is based on the following observations. While the tensor product of effect algebras is “generated” in a certain sense by one-round conditional composition, in either direction, of local measurements (it is the effect algebra of the algebraic closure of the $E$-test space formed by one-round LOCC measurements), it contains also measurements such that, although their outcomes are of course all product effects, they are not implementable by one-round LOCC. (In quantum information theory these are the measurements sometimes called “separable but not LOCC” measurements.) The proposed “operation algebras” seem to be related to the idea of viewing a system’s dynamics via a sequence of moments in time, say, with the outcome-set for a sequence of measurements being the Cartesian product of the outcome-sets at each moment, and then generating an operation algebra. The two-moment version of this seems very similar to the tensor-product construction, except that in the case of time, only one direction of conditional composition is allowed. (The corresponding “directional tensor product” has in fact already been constructed, for test spaces, by Foulis and Randall.)

Operations not arising from conditional composition trees would likely be difficult to interpret operationally. My guess is that the algebraic structure induced via an appropriate notion of probabilistic equivalence from an operational phenomenological theory will turn out to be a “weak operation algebra,” where, as with WEA’s, the PAS operation $\otimes$ is weakly rather than strongly associative. This might mean that, while it can be embedded in an operation algebra by including the “nonphysical” measurements that are not interpretable in terms of conditional composition, probably all the measurements in the WOA will be so interpretable. (On the other hand, since the quantum mechanics of operations is already an operation algebra, that suggests that this problem is less prevalent than one might have worried it would be. Maybe the unidirectional nature of the conditional composition actually helps here. The chief physical worry about the algebras derived from unidirectional composition is, in a combination-of-subsystems interpretation, that there exist states on the resulting effect algebras which exhibit “forward influence”: influence in the same direction as the classical communication required for composition. Usually theories have “too many” states because they have “too few” measurements. (In fact, allowing backward communication allows one to “detect” the bizarreness of such states, ruling them out as states on the two-way tensor product. Although, unlike the case of “ruling out” the nonpositive trace-one Hermitian operators that are still around in the tensor product, by introducing still more (“entangled”) measurements, we don’t even need to introduce additional outcomes here.)

**Problem 1** Does the directional tensor product of test spaces, $E$-test spaces, orthoalgebras, or effect algebras, contain measurements not implementable via one-way LOCC?

An important part of the project of combining operational empirical logic and QIP ideas to investigate whether or not physics can provide an overarching structure unifying perspectives, is to understand the operations available in an operational theory in terms of interactions with apparatus and/or environment. In particular, if we have a way, such as the tensor product in quantum mechanics, of describing the combination of apparatus $A$ and system $S$ as subsystems of a larger system $L$, we will probably want to require that the evolution induced on $S$ by doing an operation on the larger system is, under appropriate circumstances, one of the operations our theory describes as performable on the smaller system. “Appropriate circumstances” probably means that the apparatus should be initially independent of the system, which in turn requires that the notion of combination of subsystems have a way of implementing that requirement. Such assumptions bear close scrutiny, though, as they may be just the sort of thing that becomes impossible in certain limits. Some, for example O’Connell and Lewis [38], have argued for the physical relevance of some situations in which open systems are analyzed without the initial independence assumption. Independence works well in the case of completely positive quantum operations, though: indeed, all such operations can be implemented via a *reversible* interaction with apparatus. (It is worth occasionally pondering the possible operational significance, though, of the existence of positive but not completely positive maps.)

Direct consideration of categories, such as convex operation algebras and generalizations of these, that describe dynamics is probably the most promising way to investigate such questions in terms more general than quantum mechanics. Possibly the category-theoretic notion of tensor product will be defined for these categories. One could then examine, for example, whether when applied to construct the tensor product of two Hilbert operation algebras, it gives the operation algebra of CP-maps on the tensor product of the Hilbert spaces.

To define the category-theoretic tensor product in general requires the notion of a bimorphism. For “small” categories (whose objects are sets with additional structure, and whose morphisms are structure-preserving mappings), we can define a bimorphism of $A, B$ as function $\phi : A \times B \rightarrow T$, where $T$ is another object in the category, and $\phi$ has the property that for every $a \in A, \phi_a : B \rightarrow T$ defined via $\phi_a(b) = \phi(a, b)$ is a morphism, and similarly with the roles of $A, B$ reversed. In the category of vector spaces, for example, it is just a bilinear map.
Definition 9 The tensor product $A \otimes B$ is a pair $(T, \tau)$, where $T$ is another object in the category (also often called the tensor product) and $\tau: A \times B \to T$ is a bimorphism, and any bimorphism from $A \times B$ factors through $T$ in a unique way, and $T$ is minimal among objects for which such a $\tau$ exists.

To say $\tau$ factors through $T$ in a unique way is just to say that for any bimorphism $\beta: A \otimes B \to V$, there is a unique $\phi: T \to V$ such that $\beta$ is $\tau$ followed by $\phi$. Minimality in a set means not a subobject of any object in the set. Probably the uniqueness of the factorization is therefore redundant.

There is a partial “operational” motivation of this construction when it is applied to categories like effect algebras, operation algebras, etc:... the existence of $\tau$ and unique factorization implement the notion that the two structures being combined appear as potentially “independent” subsystems of the larger system, in a fairly strong sense that one can do any operation (or get any outcome) on one subsystem while still having available the full panoply of operations (outcomes) on the other. This is also probably linked to influence-freedom: it certainly is in the effect-algebra or test-space context, and the relation bears investigation in the general category-theoretic setting (for categories where the notion of state makes sense). However, these operational motivations do not impose the minimality requirement.

For a variety of operational structures one might use to describe quantum mechanical statics, including test spaces, orthoalgebras, and effect algebras, the category-theoretic tensor product (including the minimality requirement) has been constructed and it turns out not to be the corresponding operational structure for the tensor product of Hilbert spaces. This could indicate that the structure describing statics requires more specialized axioms, still consistent with quantum mechanics, and then the tensor product in this new category, call it $Z$, will come out right in the Hilbert space case. It could also be that the difficulty is the static nature of the categories. Indeed, the category-theoretic tensor product of test spaces or effect algebras includes measurements whose performance would seem to involve dynamical aspects. These are measurements describable as the performance of a measurement $M$ on system $A$, followed by the performance of a measurement $M_\alpha$, on $B$, where which measurement $M_\alpha$ is performed is conditional on the outcome $\alpha$ of the $A$-measurement. Indeed, the the tensor product of effect algebras must contain all product outcomes, and in fact it can be characterized as the effect algebra “generated” by requiring that it contain all the “1-LOCC” (local operations with one round, in either direction, of classical communication) measurements just described. Fuchs’ (2001) “Gleason-like theorem for product measurements” in fact proceeds by doing this construction for the case of Hilbert effect algebras. It turns out to be fairly elementary to show that it can also be characterized as the minimal “influence-free” effect algebra containing all product measurements (i.e. in which we can do all pairs of measurements one on $A$, one on $B$, with no communication). Freedom from influence of $B$ on $A$ means that for all states on the object, the probabilities of the outcomes of an $A$ measurement, performed together with an independent $B$ measurement, cannot be affected by the choice of measurement on $B$. Influence freedom means freedom from influence in both directions. Both of these provide strong operational motivations for the category-theoretic tensor product in this situation. The two motivations are closely related. Each of the characterizations is easily established starting from the other, and it also turns out that a similar construction of a “directed” product, in which 1-LOCC operations are allowed in one direction only, rules out “influence” in the direction opposite the communication. These things are also true, and were in fact first established for, test spaces and orthoalgebras.

I think it likely that the best way to resolve the problem of the effect algebra tensor product not giving the quantum mechanical effect algebra of the tensor product Hilbert spaces is to go to a dynamical framework like the operation algebras sketched above. Nevertheless, it is interesting to try to resolve it in a more static framework, by adding axioms beyond those of, say, convex effect algebras.

The difficulty, in the quantum case, is that the tensor product of orthoalgebras or effect algebras, while it must contain measurements of effects that are tensor products of Alice and Bob effects, and, through addition of effects, all separable effects, does not contain “entangled” Alice-Bob effects. The separable effects span the same vector space $B(C^d \otimes C^d) \cong H_{d^2}$ of $d^2 \times d^2$ Hermitian matrices (where $A,B$ both have dimension $d$) as the full set of effects on $C^d \otimes C^d$, but they are the interval $[0, I]$ in the separable cone, not the interval $[0, I]$ in the positive semidefinite cone. Consequently the available states, while they must be linear functionals of the form $A \mapsto \text{tr } AX$ for $d^2 \times d^2$ Hermitian $X$, are the normalized members of the separable cone’s dual, rather than of the positive semidefinite cone’s dual, so $X$ in the functional $A \mapsto \text{tr } AX$ is not necessarily positive semidefinite. The separable cone being properly contained in the positive semidefinite cone, its dual properly contains the positive semidefinite one’s dual, so that not only are we restricted to fewer possible measurements, but their statistics—even those of independent $A,B$ measurements—can be different from the quantum ones (although all quantum states are also possible states). Stated in more quantum information-theoretic terms: some nonpositive operators $X$ are nonpositive in ways that only show up as negative probabilities or nonadditivity when we consider entangled measurements: since in the effect-algebra or orthoalgebra tensor product we don’t have entangled measurements available to “directly detect” this nonpositivity, these are admissible states on these tensor products. Indeed, as observed in [91], they are isomorphic to the Choi matrices (block matrices whose blocks $M_{i,j}$ are $T(|i\rangle\langle j|)$) of positive, but not necessarily completely positive, maps $T$ (although the normalization condition (trace-preservation) appropriate for such maps is different from the (unit.
trace) normalization condition appropriate for states). Of course, the nonpositivity of the operator can be “indirectly detected” by tomography using separable effects, since these effects span the space of Hermitian operators.

One obvious solution to the problem would be to introduce axioms that would prohibit this divergence between the existence of entangled states and nonexistence of entangled measurements. Mathematically, this divergence reflects the important fact that the positive semidefinite versus separable effect algebras on $\mathbb{C}^d \otimes \mathbb{C}^d$ are differentiated by the properties of the corresponding cones: the former, but not the latter, being self-dual. Self-duality is a very natural and powerful mathematical requirement on cones. However, one might feel the requirement of self-duality to be too strong and/or not sufficiently motivated from an operational point of view. My view is that self-duality is an important part of the essence of quantum mechanics, and we should strive hard to understand its operational motivation. The cones for classical effect algebras can also be self-dual: e.g. the algebra of fuzzy sets of $d$ objects (equivalently, of effects on a $d$-dimensional Hilbert space diagonal in a fixed basis). The extent to which self-duality is a “regularity” condition or a basic conceptual assumption, and the meaning of such regularity or conceptual content, for information-processing tasks, should be understood in both quantum and classical settings.

An axiom, related to self-duality, violated by the tensor product of Hilbert effect algebras is the “purity is testability axiom.” We develop some concepts before formulating it.

**Definition 10** An effect-algebra theory is a pair $\langle \mathcal{E}, \mathcal{Y} \rangle$ where $\mathcal{E}$ is an effect algebra, $\mathcal{Y}$ a convex set of states on that effect algebra.

Here $\mathcal{Y}$ may be smaller than $\Omega(\mathcal{E})$, the set of all possible states on $\mathcal{E}$.

**Definition 11** An effect $t$ passes a state $\omega$ if $\omega(t) = 1$. An effect $t$ is a test for $\omega$ in theory $\langle \mathcal{E}, \nu \rangle$ if $t$ passes $\omega \in \mathcal{Y}$ and for no state $\sigma \neq \omega, \sigma \in \mathcal{Y}$, does $t$ pass $\sigma$. A state $\omega \in \Omega$ is testable in $\langle \mathcal{E}, \Omega \rangle$ if a test for it exists in $\mathcal{E}$.

Let us now assume our effect algebras are convex.

If two tests pass $\omega$, so does any mixture of those tests. Let $t$ be a test for $\omega$, then for $\sigma \neq \omega$, $(\lambda \omega + (1 - \lambda)\sigma)(t) = \lambda \omega(t) + (1 - \lambda)\sigma(t) < 1$, i.e. $t$ cannot test any mixture of $\omega$ with something else.

Although we just showed that a test tests a unique state, it is not necessarily the case that a testable state has a unique test.

Let $t$ test $\omega$; suppose $\omega = \lambda \sigma + (1 - \lambda)\tau$. Then $1 = \omega(t) = \lambda \sigma(t) + (1 - \lambda)\tau(t)$. This implies that $\sigma(t) = \tau(t) = 1$, hence by the fact that $t$ tests $\omega$, $\sigma = \tau = \omega$. In other words, only pure (extremal) states can be testable.

We will be interested in

**Axiom 1** All pure states are testable.

To study the consequences of this axiom, we introduce a basic notion in convex sets.

**Definition 12** A face of a convex set $C$ is an $F \subseteq C$ such that for every point $p \in F$, all points in terms of which $p$ can be written as a convex combination are also in $F$. In other words, for $\lambda_i \geq 0, \sum_i \lambda_i = 1$,

$$\sum_i \lambda_i x_i \in F \Rightarrow (\forall i, x_i \in F) .$$

(17)

Thus a face of $C$ is the intersection of the affine plane it generates with $C$. The set of faces, ordered by set inclusion, forms a lattice. This lattice characterizes the convex set (up to affine isomorphism, which is the proper notion of isomorphism for convex sets since affine transformations $y \mapsto Ay + b$ commute with convex combination).

**Theorem 3** The theory $\langle \mathcal{E}(\mathbb{C}^d) \otimes \mathcal{E}(\mathbb{C}^d), \mathcal{Y} \rangle$ violates Axiom 1 unless $\mathcal{Y}$ is contained in the set of separable states. In particular, $\langle \mathcal{E}(\mathbb{C}^d) \otimes \mathcal{E}(\mathbb{C}^d), \Omega(\mathcal{E}(\mathbb{C}^d) \otimes \mathcal{E}(\mathbb{C}^d)) \rangle$ violates it.

**Proof:** We begin by showing that the only states testable in $\mathcal{E}(\mathbb{C}^d) \otimes \mathcal{E}(\mathbb{C}^d)$ are pure product states. This is straightforward but we give details anyway. Let $\text{tr} X = 1$ and $\langle |\psi\rangle|\psi\rangle|\chi\rangle \geq 0$ for all product states $|\phi\rangle|\chi\rangle$, so that $A \mapsto \text{tr} AX$ is a state. Testability means there is a separable $A$ with trace between zero and one (separable effect) such that:

$$1 = \text{tr} AX .$$

(18)

The requirement on $A$ is equivalent to:

$$A = \sum_i \lambda_i |\chi_i\rangle|\psi_i\rangle\langle\psi_i|\chi_i| \ (\lambda_i > 0, \sum_i \lambda_i \leq 1, |\chi_i\rangle, |\psi_i\rangle \text{ normalized}) .$$

(19)
Thus \(18\) becomes \(\sum \lambda_i \langle \chi_i | \langle \psi_i | X | \psi_i \rangle | \chi_i \rangle = 1\), which can only hold if one of the \(\lambda_i = 1\), and for that \(i\), \(\langle \chi_i | \langle \psi_i | X | \psi_i \rangle | \chi_i \rangle = 1\). Then (dropping the subscript)

\[
X = | \chi \rangle \langle \phi | \phi \rangle | \chi \rangle + X_{\pi,\perp} + X_{\perp,\pi} + X_{\perp,\perp}. \tag{20}
\]

This is a resolution of \(X\) into components in four subspaces of the space of operators on \(\mathbb{C}^d \otimes \mathbb{C}^d\): the space \(\pi, \pi\) of operators on the one-dimensional Hilbert space \(\pi\) spanned by the pure product state, the space \(\pi, \perp\) of operators taking \(\pi\) to \(\pi^\perp\), the space \(\perp, \perp\) going the other way, and the space \(\perp, \pi\) of operators on \(\pi^\perp\). The middle two pieces are manifestly traceless, so the last one must be traceless for \(\text{tr} \ X = 1\) to hold. However, \(\text{tr} \ X_{\perp,\perp} = \sum_{ij} \langle i | \langle j | X | j \rangle | i \rangle\) in a product basis \(| i \rangle \langle j |\) for \(\perp\). Each \(\langle i | \langle j | X | j \rangle | i \rangle \) must be positive since \(\text{tr} \ X_{\perp,\perp} A = \text{tr} X A\) for \(A \in \perp, \perp\). So for \(X_{\perp,\perp}\) to be traceless, they must all be zero, and \(X = | \chi \rangle \langle \phi | \phi \rangle | \chi \rangle\) plus possibly some traceless stuff which does not affect the induced state.

Thus if Axiom 1 is satisfied, the extremal states of \(\mathcal{Y}\) are product states, so \(\mathcal{Y}\) is a face of the convex set of separable states.

Note that we can have a theory on \(\mathcal{E}(\mathbb{C}^d) \otimes \mathcal{E}(\mathbb{C}^d)\) satisfying the axiom of testability, but only if the state space is contained in the dual of the cone generated by the effect algebra. This suggests that the axiom, if required of the full state space \(\Omega\) of an effect algebra, is pushing us towards the idea that the cone be self-dual. Of course, if the state-set of a theory is smaller that the full set of all possible states, the testability axiom might be satisfied even for theories on non-self-dual cones: for example, the theory whose effects are all the separable ones, and whose states are also all separable.

The axiom of testability is very natural, and it turns out to have a long history in quantum logic. In the convex setting, \(68\) has certainly used it, I think Ludwig \(1983, 1985\) has too. Clearly theories which are the full state spaces of linear effect algebras that are initial intervals in self-dual cones satisfy it. Probably stronger things are necessary to get self-duality.

This axiom makes contact with the “property lattice” quantum logics of Jauch and Piron. See Valckenborgh \(85\) pp. 220–221 Their notion of property roughly corresponds to effects (or the analogues in other quantum structures, since most of their work was done before effect algebras were formalized in the quantum logic community) \(e\) which can have probability one in some states (in our terminology, effects that pass some states). Those states are said to “possess the property \(e\)”. Actually (and relevant to our observation that tests for a state are not necessarily unique), they define properties as equivalence classes of effects that pass the same set of states. They construct a lattice of properties for an empirical theory (set of states on some quantum structure).

Axiom 1 can be viewed as a statement about the relationship of the lattice of faces of a convex set of states on an effect algebra (i.e., the state-set of an effect-algebra theory) to the property lattice of that theory.

The extremal states are minimal elements of the face lattice, and the axiom says that there are “minimal properties” possessed by those states: minimal in the sense that no other state possesses them. I am not certain if this is minimality in the sense of Piron’s property lattice, however, it seems plausible that this would hold, perhaps under mild conditions. A generalization of Axiom 1 would assert, for each face of the state set, the existence of a “property” of being in that face, i.e. an effect passing precisely the set of states of that face. A similar axiom of \(2\) concerns “filters” for higher dimensional faces, but this also involves “projection postulate-like” dynamics associated with the filtering.

Araki also uses, as an assumption, the symmetry or “reciprocity” rule, satisfied in the quantum-mechanical case, that can be formulated once a correspondence \(\chi \leftrightarrow e_\chi\) between extreme states \(\chi\) and tests \(e_\chi\) for them has been set up:

\[
\chi(e_\phi) = \phi(e_\chi). \tag{21}
\]

It is not clear to me whether the extreme states \(\rightarrow\) effects correspondence must be one-to-one instead of many-to-one in order to be able to formulate the axiom, or whether one-to-oneness might be a consequence of it.

Faces play an important role in Ludwig’s work as well, as do statements reminiscent of Axiom 1, so it is quite likely Ludwig’s argument may turn out to be similar.

Araki credits Haag for emphasizing to him the importance of the reciprocity axiom. In the second edition of his book, \(53\) includes an informal discussion of the foundations of quantum mechanics based on the convex cones framework. He, too, uses Axiom 1, and a generalization associating faces of the state space (one-to-one!) with “propositions.” These “propositions” are effects passing precisely the states of the face, and minimal among such effects in the sense of a probabilistic ordering of effects

\[
e_1 \leq e_2 := \forall \omega \in \mathcal{Y} \left( \omega(e_1) \leq \omega(e_2) \right). \tag{22}
\]

This is a different strategy from the Jauch-Piron equivalence class one for getting uniqueness of the effect associated to a face, but it is closely related to it. Jauch and Piron were trying to get by with less reference to probabilities. Haag also uses the reciprocity axiom, which he argues imposes self-duality. (Haag uses uses the notion of self-polarity, but
for our type of cone, this is the same as self-duality. The polar of a convex body $C$ is the set of linear functionals $L$ such that $L(x) \leq 1$ for all $x \in C$; the polar of a cone is the negative of the dual cone, since whenever $L(x)$ is positive, $L(x')$ is greater than 1 for $x'$ a large enough positive multiple of $x$. Since the negative of a cone is isomorphic to that cone, a self-polar cone is self-dual.

He also gives some operational motivation for an additional assumption, that of homogeneity of the cone. This says that the automorphism group of the cone acts transitively on its interior. (This means that for any pair $x, y$ of interior points, there is an automorphism taking $x$ to $y$.) The operational interpretation of elements of the automorphism group is presumably as possible conditional dynamics. The operational interpretation of the assumption of homogeneity, at least for self-dual cones, is probably to be that any state is reachable from any other state by dynamics conditional on some measurement outcome. This is not a self-evident requirement, but seems natural. The motivation might be that if you can’t prepare any state starting from another state, with a nonzero probability of success, the state space might “fall apart” into pieces not reachable one from the other (orbits of the automorphism group). Or maybe while some pieces might still be reachable from all others, going the other way might not be possible... the theory would have intrinsically irreversible dynamics, even conditionally. A more detailed study of the potentially bizarre features of operational theories whose effects are naturally represented by a non-homogeneous cone, or whose state-space generates one, would be desirable, both with and without the assumption of self-duality. The “falling apart” into orbits of the automorphism group might be physically acceptable if the theory represented a perspective involving radical limitations on our ability to prepare states, say: going from one orbit to another might require a more powerful agent than the one whose perspective is being considered, but the consequences of such an agent’s actions might be observable by the less powerful agent. Quantum entanglement provides an example of such a situation: the perspective of the set of local agents, with the power to communicate classically, allows for pairs of states to exist, such that they have different statistics for observables implementable by local actions and classical communication (LOCC), but it is not possible, even conditional on some measurement outcome, to prepare one starting from the other via local actions and classical communication. Of course, the LOCC perspective of the local agents is not what is usually taken as a “subsystem” in quantum mechanics, so these sorts of perspectives can be taken as derivative rather than fundamental; but perhaps in some types of theories nonhomogeneous perspectives play a more fundamental role.

In finite dimensions, as Haag points out, homogeneous self-polar cones are known (e.g. [20]) to be isomorphic to direct products of the cones whose faces are the subspaces of complex, quaternionic, or real Hilbert spaces. (Extensions of these results to infinite dimensions are obtained in [25].) The factors in the direct product can be thought of as “superselection sectors;” classical theory would be recovered when the superselection sectors are all one-dimensional (at least in the complex and real cases). [2] obtains a similar theorem except the effects get represented as elements of a finite dimensional Jordan algebra factor. These are isomorphic to $n \times n$ Hermitian matrices over $\mathbb{R}, \mathbb{C}$, or the quaternions $\mathbb{H}$, or a couple of exceptional cases (spin factors and $3 \times 3$ Hermitian matrices over the Cayley numbers). (This is obviously closely related to the cone representation just described.) He also gives arguments for picking the complex case, based on the properties of composition of subsystems in the various cases. Araki’s argument is that “independence” of the subsystems should be expressed by $\dim V = ( \dim V_1)( \dim V_2)$ for the algebras. But, “essentially because the tensor product of two skew-Hermitian operators is Hermitian”, we have $\dim V > ( \dim V_1)( \dim V_2)$ except in trivial cases, when we take the $V$’s to be the algebras of Hermitian matrices over real Hilbert spaces $H_1, H_2$, and their tensor product. For $\mathbb{Q}$ there is not even a quaternion-linear tensor product. The bottom line is that “the complex field has the most pleasant feature that the linear span of the state space of the combined system is a tensor product of [the state spaces of the] individual ones.”

This, too, could use more careful “operational” study, but it is clear there are important operational and probably information-theoretic distinctions between the cases. For the real case, the key point is that in contradistinction to the complex case, states on the “natural” real composite system are not determined by the expectation values of local observables.

We discussed the possible operational motivation or interpretation for homogeneity, but said little about self-duality and reciprocity. I think these properties, too, may be related to the ability to coordinate perspectives into an overall structure, or the way in which they can be coordinated. In a “spin-network” type of theory, the edges of a graph with representations of a Lie or quantum group (su(2), for spin networks), which are Hilbert spaces. The vertices are associated to “intertwiners” between those representations. A state might be associated with, say, a partition of the graph by a hypersurface cutting it into two parts, “observer” and “observed.” If the hypersurface has two disconnected parts, the associated Hilbert space will involve will be the tensor product of the ones associated with the parts; otherwise, the representation is made out of the representations labelling the cut edges, in a way determined by the intertwinings at the vertices between them. One has the same Hilbert space whichever piece one takes as “observer” vs. “observed.” However, it is likely that the role-reversal between observer and observed corresponds to dualization, and the result that both correspond to the same Hilbert space will only hold in theories in which the structure describing a given perspective—here, the Hilbert space associated with the surface—is self-dual. To attempt to actually show something like this would involve a project of trying to construct “relational” theories like
the Crane-Rovelli-Smolin theories, but with other empirical theories playing the role of Hilbert spaces and algebras of observables on them. A simple first example might be “topological classical field theories,” if these can consistently be defined. In these general “pluralistic structures” coordinating perspectives, one might hope to find a role for self-duality and the reciprocity axiom, and perhaps homogeneity as well. For the different empirical structures associated with different surfaces to relate to each other in a “nice” way, it might be necessary that the structures be defined on self-dual cones, or exhibit reciprocity. Haag says, “[reciprocity] expresses a symmetry between “state preparing instruments” and “analyzing instruments” and is thus related to time-reversal invariance.” This suggestion, too, bears more detailed investigation, perhaps in the same context. Another relevant point is that quantum systems can roughly be defined as unital algebras (in the sense of vector space with linear associative product, and a unit for the product) with enough additional structure that their representations have natural notions of dual representation, and a monoidal product of representations which is close to being a tensor product, so that it could turn out that a broad class of network structures close to the class already considered by Crane, Rovelli, and Smolin, labelled with quantum group representations and intertwiners, are actually close to the most general structures one can build by coordinating operational perspectives. This is a line of inquiry that certainly deserves to be pursued further.

While I mentioned above the need to deal with dynamics and system combination in a structure, such as operation algebra, that is dynamical from the outset, it is also worthwhile to understand how the static structure that is available even in dynamical theories (e.g., an effect algebra can probably be derived from the same kind of operational phenomenological theory that can give us an operation algebra, and the two be closely related) is related to the dynamics. This suggests that we might ask what kind of “conditional dynamics” we can introduce on an effect algebra. For me, at least, the project of understanding dynamics in this way is farther along than the more natural project in terms of operation algebras, so I will discuss it at more length.

IX. DYNAMICS ON EFFECT ALGEBRAS: “HEISENBERG” AND “SCHRÖDINGER/ LIOUVILLE/VON NEUMANN” PICTURES

We now consider dynamics on effect algebra theories. A similar treatment will work for other related structures, e.g. theories on orthoalgebras, test spaces, or E-test spaces. Initially we will consider “unconditional dynamics,” i.e. those preserving total probability. These are a special case of “conditional dynamics,” in which we condition on the unit of the effect algebra. We refer to resolutions of unity in the effect algebra as measurements.

In a “Schrödinger” picture (or perhaps we should call it a “Liouville-von Neumann” picture), a dynamical evolution on an effect algebra is represented by an affine mapping $\sigma$, possibly many-to-one, from states to states. The affinity of the mapping just means that it is compatible with the convex structure of the state-set in the following sense:

$$\sigma(\lambda \omega_1 + (1 - \lambda) \omega_2) = \lambda \sigma(\omega_1) + (1 - \lambda)\sigma(\omega_2).$$

We will define a Schrödinger dynamics $D$ on an effect algebra to be a semigroup (monoid, with identity) of such Schrödinger evolutions. These are the “possible evolutions” for a system. Making the set a semigroup just says that the composition of two possible evolutions is also a possible evolution. Unless explicitly mentioned, we will also assume that a dynamics is a convex set. Both of these requirements may be justified from our operational point of view: but it bears emphasis that this means thinking of dynamics as the set of evolutions we can, in principle, make the system undergo. As an example of a dynamics, take a finite classical system: its $D^*$ contains all the $\sigma_A$ defined by:

$$\sigma_A(p) = Ap,$$

where $A$ is a stochastic matrix. In finite-dimensional quantum mechanics, $D$ is just the set of (trace-preserving) completely positive maps on the space of linear operators on the Hilbert space. (More precisely it is the set of restrictions of such maps to the manifold of density matrices; or, even more precisely, it is the set of induced actions of such maps on the normalized linear functionals $\omega_p$ defined by $\omega_p(A) := tr \rho A$, since it is these functionals that are the states when quantum mechanics is viewed in terms of effect algebras.)

We will say that an evolution $\sigma \in D$ is reversible if $\sigma^{-1} \in D$. Thus, the reversible dynamics $R$ of a system, the subset of $D$ consisting of reversible evolutions, is a group and not just a semigroup.

**Proposition 2** If $\sigma$ is reversible, then it takes pure states to pure states.

*Proof:* Let $\sigma(\psi) = \sum i \lambda_i \chi_i$. Then $\sigma^{-1}(\sum i \lambda_i \chi_i) = \psi$. But by affinity, $\sigma^{-1}(\sum i \lambda_i \chi_i) = \sum i \lambda_i \sigma^{-1}(\chi_i)$. By the extremality of $\psi$, this requires $\sigma^{-1}(\chi_i) = \psi$ for all $i$, whence $\chi_i$ is independent of $i$. So $\sigma(\psi)$ is pure. ■

(In a quantum-mechanical effect algebra, such evolutions are given by $\rho \mapsto U \rho U^{-1}$, for unitary or antiunitary $U$, by Kadison’s theorem. In a finite classical effect algebra of functions on a set $S = \{1, ..., d\}$, they are given by letting
Let $A$ in (23) be a permutation acting on $S$ (so $\sigma$ acts on the functions (effects) by taking $f$ to the function $f^{\sigma}$ whose value on $x \in S$ is $f(\sigma(x))$). If we represent the functions by diagonal $d \times d$ matrices, the permutations act on these by conjugation.

Note that by affinity, any evolution is determined, on the full state space, by its action on the pure states.

Now consider Heisenberg-like dynamics on effect algebras. Here we take (unconditional) evolutions to be \textit{faithful endomorphisms} of the effect algebra. These are morphisms $\gamma$ of $\mathcal{E}$ into itself; recall from the definition of morphism that $\forall a, b \in \mathcal{E}, \gamma(a) \oplus \gamma(b) = \gamma(a \oplus b)$ and $\gamma(1) = 1$. Reversible evolutions should be \textit{automorphisms} of the effect algebra, that is, endomorphisms that are bijections. (Possibly, we will want to represent dynamics conditional on measurement results by not-necessarily-faithful endomorphisms (i.e. remove the $\gamma(1) = 1$ requirement.) Moreover we will now want to consider convex effect algebras; then as with the Schrödinger evolutions, all Heisenberg evolutions are required to be affine, that is, commute with the process of taking convex combinations. This is for similar reasons in both the Schrödinger and Heisenberg cases: if the state preparation procedure (for the Schrödinger case) or measurement procedure (in the Heisenberg case) is a convex combination of two procedures, this is interpreted as meaning that it involves conditioning aspects of the procedure on variables which are otherwise independent from both preparation and measurement and the system itself. We should be able to imagine learning the value of the “dice” variables either before, or after, evolution, with no different effect on the resulting conditional states. (Thus the assumption may not be appropriate to apply when such “external random variables” or “sources of ignorance” are not available.)

Immediately, natural questions suggests are suggested: for a given effect algebra, is the set of all possible Heisenberg evolutions effectively the same as the set of all possible Schrödinger evolutions? Or, for a theory $T$ on $\mathcal{E}$ whose state-set is not the full $\Omega(\mathcal{E})$, and with a specified Schrödinger dynamics, are the evolutions in this dynamics all representable as Heisenberg dynamics? The same questions could also be asked for reversible Heisenberg/Schrödinger evolutions. More precisely, is it the case that for every outcome $a$, state $\omega \in \mathcal{S}$, and Schrödinger dynamics $\sigma \in \mathcal{D}$, there exists a Heisenberg dynamics $\gamma \in \mathcal{H}$ such that

$$\sigma\omega (a) = \omega (\gamma a).$$

Note that the converse is guaranteed when $\mathcal{H}, \mathcal{D}$ are the full sets of Heisenberg and Schrodinger dynamics (the maximal sets satisfying the axioms) on $T$; for every $a$, $\omega$, and Heisenberg $\gamma$, the state $\omega^\gamma$ defined by $\omega^\gamma(a) = \omega(\gamma a)$ is guaranteed (by the fact that $\gamma$ is an endomorphism of $\mathcal{E}$) to satisfy the requirements to be a state. However, it is easy to find examples of effect algebras where the full set of Schrödinger dynamics includes some which are not representable as Heisenberg dynamics.

I will now argue that on the operational conception of measurement and preparation, a very natural assumption about a dynamical theory on an effect algebra is that only evolutions representable as Heisenberg evolutions be allowed. The reason is that from an operational point of view, we should take as tests any procedures performed on the system, which yield various outcomes. This includes the procedure allowing the system to evolve by one of its allowable (Schrödinger, say) evolutions $\sigma$, and then performing a test $T$. This is a procedure we could perform starting \textit{before} the evolution, and each of its outcomes $a$ should also be represented as an outcome $a^\gamma$ of a test $T^\gamma$, on the unevolved system, such that

$$\omega(a^\gamma) = \sigma\omega (a),$$

If our effect algebra is required to contain $T^\gamma$, then there is an endomorphism $\gamma_a$ which takes each $a$ to $a^\gamma$, i.e., a Heisenberg representation of the Schrödinger dynamics $\sigma$.

This justification could fail if, for example, the effect algebra corresponding to measurements performable after evolution were different from the effect algebra for measurements performable before. Such a situation would require dynamics to be maps from one effect algebra (or its state space) to another(s). Another (generally weaker, in my view) motivation for rejecting the argument would be a rejection of the operational conception of measurement taken here. If measurement, for example, were conceived of as some magic process taking place instantaneously, then one might not include the procedure of wait-and-measure in the set of “primary” measurements which can be made at time $t$, even if that set of primary measurements were the same for all $t$. In either case, the theory would exhibit a kind of lack of time-translation invariance. In fact, the assumption that the theory does not lack this weak sort of time-translation invariance seems closely related to the assumption behind our introduction of operation algebras: that any operation we can do now, we can also do conditional on the outcome of another operation which is done first (in particular, conditional on the outcome of a top element of the operation algebra, which just means, following some previous unconditional dynamics).

A potentially misleading point here is that an endomorphism of the effect algebra that is not an automorphism maps the full effect algebra onto a proper sub-effect algebra, so that it might seem to be “changing the effect algebra” and therefore violating the assumption used to justify endomorphisms as dynamics in the first place. However, the
Heisenberg picture is tricky, because it goes in a sense “backward in time”: it maps the full effect algebra after evolution, with its usual operational interpretation, into the algebra of effects before evolution. Every operation in the effect algebra is still performable after the evolution; to find out what the corresponding probabilities after the evolution are, though, we need to map it into itself, possibly onto a proper subalgebra, and then evaluate the original state on it. Whether a subalgebra or the full algebra is mapped into the original algebra depends on the particular evolution. By contrast, a Heisenberg-like picture for a theory in which the operations performable after evolution were represented by a different effect algebra, say a subalgebra, would just map that subalgebra into the original algebra. No evolution would ever map an algebra isomorphic to the original algebra onto itself: reversible dynamics would be impossible.

Note that the theory whose state-set is the full state-set of $E(C^n) \otimes E(C^n)$, or even the theory with that (separable) effect-algebra but the states restricted to the quantum-mechanical ones, have Schrödinger dynamics that are not Heisenberg-representable (e.g. those taking separable states to entangled states). This is probably related to their failure to exhibit other axiomatic criteria (such as the “purity is testability” axiom) mentioned above.

As with so many plausible assumptions about operational theories, this “time-translation invariance” is not quite a “law of thought”: we could certainly imagine a lack of time-translational invariance of this sort might crop up in physics. But it is useful to see how it corresponds with natural formal statements about operational structures. Moreover, a dynamical theory that does have this sort of invariance would probably exhibit features with a natural information-theoretic interpretation.

One possibility involves distinguishability. An important tool in quantum information theory, and QIP theory, has been measures of distinguishability of two, possibly mixed, quantum states. A copious supply of such measures may be obtained in a general operational setting using a strategy which has proved useful in quantum information theory. It starts by considering classical measures of distinguishability of probability distributions. In an effect algebra dynamical theory, we simply define the distinguishability of two states $\rho, \omega \in S$ as the maximum over effect-tests $\Sigma$ (sets of effects $e_i$ such that $\oplus_i e_i = 1$) of the distance between the classical probability distributions $p_{\Sigma, \rho}$ and $p_{\Sigma, \omega}$ induced by $\rho$ and $\omega$ on the outcomes in $\Sigma$:

$$D_F(\omega, \rho) := \max_{\Sigma} D_{cl}(p_{\Sigma, \rho}, p_{\Sigma, \omega}).$$

(27)

The question then arises: when $D_{cl}$ is nonincreasing under classical dynamics, is the induced $D_F$ also nonincreasing, under the notion of dynamical evolution incorporated into $F$? When the dynamics consist of effect-algebra endomorphisms, the answer would seem to be trivially positive. (The argument does not even use the assumption that $D_{cl}$ is contractive.) For all the measurements made after evolution map correspond, via the Heisenberg-picture endomorphism, to measurements made before the evolution; the maximization over measurements performed after evolution, then, is over a set of measurements no larger than that before evolution (smaller, if the evolution is not an automorphism). There has been extensive study of the distance measures which are contractive under quantum evolutions (i.e., unital completely positive linear maps on operators interpreted as observables, or, dually, trace-preserving completely positive linear maps on the state space of such systems). The contractiveness of such distances has proven a useful tool, e.g., in establishing impossibility results in quantum information processing. For example, the “no-broadcasting theorem,” a generalization of the no-cloning theorem to mixed quantum states, was first proved (for nonsingular density matrices) in this way by [9], although interesting and perhaps more natural C*-algebraic proofs [6] have now been found. It may well be that the contractiveness of an appropriate set of distance measures is a principle that (combined with a tensor product structure of system composition which may well, as it does in the cases of orthoalgebras and effect algebras, automatically prohibit instantaneous inter-system influence) render exponential speedup of brute-force search impossible.

In fact, violation of this sort of “time-translation invariance” may lie behind the fact that some versions of “nonlinear quantum mechanics” appear able to speed up NP and #P problems. The combination of a nonlinear evolution law with the usual rules for quantum measurement ensures that in these theories, measure-and-then-evolve is not the same as evolve-then-measure; and indeed, Lloyd and Abrams’ algorithms make use of the ability to increase distinguishability exponentially in time via nonlinear evolution. This violation is probably also at the heart of the theoretical objections to nonlinear quantum mechanics discussed below. It underlines that the question of how to count “resources” and “information” become very subtle when such a natural formal requirement is dropped, and keeping things consistent is difficult. This is not to say that, like other “natural” operational requirements such as the tensor product law of system combination, this sort of time-translation invariance can never fail; it might be an idealization that holds in the limit of perspectives of a certain type, and it might be necessary to transcend it in certain settings, perhaps involving cosmology or quantum gravity. And, our requirement that all evolutions of the “starting at time $t$, evolve and then measure” type be included among the measurements we can make at $t$, may give us a description of observation that has thrown away information important for complexity issues (notably, the time it takes to make measurements via a set of physically easy-to-implement evolutions). But the discussion focuses attention on the nature of the problems likely to arise in making a sensible theory of situations in which it fails.
QIP emphasizes the usefulness of the conceptual peculiarities of quantum mechanics to the performance of tasks not classically possible. This suggests a strategy of trying to formulate these tasks, or the associated concepts, in ways general enough that we might hope to characterize different operational theories by whether or not these tasks can be performed in them, or by the presence or absence of conceptual phenomena such as: superposition, complementarity (which may be essentially the same thing as superposition), entanglement, information-disturbance tradeoffs, restrictions on cloning or broadcasting, the nonuniqueness of the expression of states as convex combinations of extremal quantum states (versus the uniqueness classically).

The impossibility of bit-commitment is a candidate for the set of axioms about of information-processing limitations shared by quantum and classical mechanics. This was suggested by Gilles Brassard; Chris Fuchs and Brassard also speculated that the combination of no-bit-commitment and eavesdropping-proof key distribution might single out quantum mechanics. This intriguing suggestion is spun into a thought-provoking fantasy in [44]. In the setting of C* algebras, Bub, Clifton, and Halvorson [24] have obtained qualitative confirmation of this hypothesis. One might also want to investigate it in more general convex settings, if a good notion of, for instance, subsystem combination is developed for classes of these. Of course, even before the upsurge of interest in quantum information science, these conceptual peculiarities were being generalized and studied by empirical/operational quantum logic researchers. For example, superposition has been considered in a test space setting by [12]; the nonuniqueness of the extremal decomposition was studied in a convex sets framework by [12].

Assumptions and tasks involving computation should also be investigated; I will discuss these in a bit more detail. In particular, it would be interesting to establish linkages between complementarity, or superposition, and computational speedup in some framework more general than quantum and classical mechanics.

For information-processing or computation, both dynamical considerations and composite systems are of the utmost importance. Since the environment which induces noise in a system or the apparatus used by an information-processing agent must be considered together with the system, a notion of composite system is needed. And notions of composition are basic to computational complexity, where the question may be how many bits or qubits are needed, as a function of the size of an instance of a problem (number of bits needed to write down an integer to be factored, say) to solve that instance. Indeed, the very notion of Turing computability is based on a factorization of the computer’s state space (as a Cartesian product of bits, or of some higher-arity systems), in terms of which a “locality” constraint can be imposed. The constraint is, roughly, that only a few of these subsystems can interact in one “time-step.” The analogous quantum constraint allows only a few qubits to interact at a time. In general operational models, some notion of composition of systems, such as a tensor product, together with a theory describing what dynamics can be implemented on a subsystem, could allow for circuit or Turing-machine models involving “bits” or other local systems of a nature more general than quantum or classical systems. There may be much to learn from a study of computational complexity in such general systems.

One of the meatiest open problems suggested by QIS, in my view a very physical though also very abstract problem, is this: why do neither quantum mechanics nor classical mechanics allow a speedup to polynomial time of “brute-force” search for solutions to problems in NP. More precisely, we are asking whether there is something that QM and the classical description have in common (other than the fact that they are both embeddable in a quantum description) that prevents them, given a “black-box” which tells us whether a given string is a solution to a specified instance of a problem or not, and a space of candidate solutions whose description length is polynomial in the size of the problem, why no procedure using a quantum or a classical computer can, for every instance of the problem, find a solution to that instance using the black-box solution checker a number of times bounded by a polynomial in the size of the instance. (This result is due, for the quantum case, to [12].) To make the general question precise enough for rigorous investigation requires specifying what such a “black-box” would be in the class of empirical theories (more general than but including quantum and classical mechanics) in which one is going to ask the question. It is clear in the quantum case what such a “black-box” should be, but less so in more general settings.

This is an example of the kind of problem I believe operational quantum logic has much to contribute to. One can formulate the problem in terms of effect-algebras, or weak effect-algebras, with a model of dynamics, or operation algebras, or some subcategory of one of these. Most likely the set of possible dynamics will be either the full set of endomorphisms on the effect algebras in question, or a composition-closed, probably convex subset thereof. To formulate such a problem conceptually will require implementability of the solution-checking oracle as a dynamical evolution. This enables us to formulate a notion of query complexity in this operational model: an r-query algorithm for computing a function f on a black-box input x (x would be the solution-checking black box, in the special case of brute-force search for the solution to a problem with polynomial solution-checking routine) in the model consists of a sequence of r dynamical evolutions chosen from the set of possible evolutions, and its execution consists of preparing the system in a standard starting state, applying the first evolution D_1 followed by the query oracle, then D_2, then
the query oracle, and finishing up, after the last query, with an arbitrary measurement, interpreting the result via some fixed (polynomial-time classically computable, say) algorithm as the value of \( f \). In some models, of course, one can’t even compute in polynomial time—or at all—some polynomials of computable classical functions. This would restrict the nature of the inputs \( x \), and drastically alter the theory of query computation relative to the classical case.

In a more tractable situation, holding for example for quantum computation, all \( x \) would be black-box implementable as circuits in the operational model.

To go beyond query computation to explicit algorithms such as Shor’s factoring algorithm (to give a quantum example) requires a notion of computational resources required to perform a given dynamical evolution or measurement. One way of specifying such a notion is by specifying a set, possibly infinite, of dynamical evolutions to which we ascribe unit cost, and a specification, say, of a set of measurements viewed as computationally easy. More generally, we might specify a function on dynamical evolutions and measurements. Precisely what we will want to do here may depend on which of several variant computational models we want to implement, depending on how we allow ourselves to interface the given operational model with “classical” computation. Perhaps most generously, we might specify a set of measurements-with-conditional-dynamics (“instruments”) viewed as taking unit computational time, and allow the conditioning of further dynamics and measurement on the results of the measurement in question. Subtleties would arise in counting the computational cost of the classical manipulations required to condition in a specified way, though probably a satisfactory solution using a standard classical computational model and counting one elementary operation in that model as costing the same as one in the general operational model, would work (at least if the general operational model can simulate classical computation one-for-one, or at least polynomially). Most simply, we might just perform the algorithm in the general operational setting via evolution without explicit measurement and classical control, and specify a “standard” measurement to be performed at the end (along with a standard procedure, or set of allowable procedures, for mapping the measurement result to the set of possible values of the function being computed). For explicit algorithms, in non-query models, it is important that not just any measurement be allowed at the end, since if the dynamics consists of all effect-algebra endomorphisms, say, any computation can be done by making one measurement.

Using such models of query complexity and/or computational complexity in some fairly general class of operational theories, I think we are likely to find intuitively meaningful, very general properties of operational physical theories, shared by quantum and classical mechanics but also by a wider class of theories, which forbid, for conceptually clear reasons, polynomial-time brute-force search. These properties may turn out to be linked to other properties of theories. Some possibilities are the second law of thermodynamics (impossibility of a perpetuum mobile), or the impossibility of instantaneous signalling between subsystems of a composite system. Richard Jozsa has suggested that the impossibility of speedup of brute-force search (in fact, of NP-hard problems) to polynomial time could serve as a constraint on proposed new physics. In this regard, 

If we take a broad view of work on the foundations of quantum mechanics, I think we can recognize that important insights have been achieved by pushing the various points of view to see how much they can accomplish, how rigorously their insights can be formulated, what hidden assumptions may lurk. Equally important insights are to be had by stepping back from the project of promoting an individual point of view on quantum foundations as “the answer,” and looking at how the insights derived from different points of views may relate, acknowledging that none of us yet has the answer, and the path toward it may require combining insights from several points of view. Quantum information science provides an arena in which such putative insights and their relationships can be analyzed with appropriate quantitative tools, and in terms of information-processing concepts and tasks that promise to have real

XI. CONCLUSION

If we take a broad view of work on the foundations of quantum mechanics, I think we can recognize that important insights have been achieved by pushing the various points of view to see how much they can accomplish, how rigorously their insights can be formulated, what hidden assumptions may lurk. Equally important insights are to be had by stepping back from the project of promoting an individual point of view on quantum foundations as “the answer,” and looking at how the insights derived from different points of views may relate, acknowledging that none of us yet has the answer, and the path toward it may require combining insights from several points of view. Quantum information science provides an arena in which such putative insights and their relationships can be analyzed with appropriate quantitative tools, and in terms of information-processing concepts and tasks that promise to have real
worldly and/or physical significance.

In this paper, I have promoted a particular project for harnessing the concepts of quantum information science to the task of illuminating quantum foundations. This project is to generalize tasks and concepts of information science beyond the classical and the quantum, to abstract and mathematically natural frameworks that have been developed for representing empirical theories; and to use these tasks and concepts to develop axioms for such theories, having intuitively graspable, perhaps even practical, meaning, or to develop a better understanding for the operational meaning of existing axioms. Moreover, I have emphasized a particular strategy for this project, which begins with an “operational” approach to describing empirical theories, taking the probabilities for various outcomes of operations one may do on the system as primary, and, via probabilistic equivalence, making connections to the more abstract structures of convex effect algebras and convex operation algebras, which are closely linked to the convex frameworks used to good effects by the authors of many of the other papers in this volume (Bacciagaluppi, Busch, and Hardy, with spring to mind). These approaches are particularly relevant to the project because the structure describing an empirical theory depends on the agent doing the operations whose probabilities it represents: it is “perspectival.” It is precisely the possibility of coordination of different agents’ perspectives into an “objective” framework, or the impossibility of doing this in a “realistic” way that produces an “objective” framework, that is the key issue in the foundations of quantum mechanics. So these operational, perspectival structures, “operational quantum logics” or whatever you’d like to call them, are appropriate for studying these coordination issues. Since information itself involves relations between parts of a system, say between an information-gathering and using agent and another subsystem it is gathering and using information about, the transmission, preservation, and use of information, and the network of relations it involves between subsystems, is likely to be crucial for understanding the nature, and the possibility or impossibility, of such coordination between perspectives. That is why the generalization of tasks and notions concerning information from classical theory to quantum theory and beyond to the more general operational structures discussed here is likely to be useful in this foundational project, and why quantum information theory in particular provides a both a model for that part of the project, and a fertile source of particular axioms, ideas, and constructions, for use in it. Conversely, “integrability of perspectives into a coherent whole,” is also a possible source of axioms about the nature of perspectives (self-duality or homogeneity of the cones used to represent them?), how they combine (via tensor products or some other rule?), that may ultimately help illuminate the significance of the laws of physics, and the flow and uses of information in physical systems.

Acknowledgements

Discussions over the years with Carlton Caves, Dave Foulis, Chris Fuchs, Leonid Gurvits, Lucien Hardy, Richard Jozsa, Eric Rains, Rüdiger Schack, and Alex Wilce, among others, have influenced my thoughts on these matters. Chris Fuchs brought the work of Bilodeau to my attention. The epigram from Cormac McCarthy appears also on Carl Caves’ homepage, so that may be where I got it from although it also leaped out at me when I read the book.

[1] Abrams, D. S., Lloyd, S., 1998. Nonlinear quantum mechanics implies polynomial-time solution for NP-complete and #P problems. Phys. Rev. Lett. 81, 3992–3995.
[2] Araki, H., 1980. On a characterization of the state space of quantum mechanics. Commun. Math. Phys. 75, 1–24.
[3] Araki, H., Yanase, M., 1960. Measurement of quantum mechanical operators. Physical Review 120, 622–626, reprinted in Quantum Theory and Measurement, J.A. Wheeler and W.H. Zurek, eds., Princeton Univ. Press, 1983.
[4] Banaszek, K., 2001. Fidelity balance in quantum operations. Phys. Rev. Lett. 86, 1366.
[5] Barnum, H., 1990. The many-worlds interpretation of quantum mechanics: psychological versus physical bases for the multiplicity of “worlds”, hardcopy available from the author on request.
[6] Barnum, H., 1998. Quantum information theory UNM Doctoral Dissertation. Slightly corrected version available electronically at http://info.phys.unm.edu/papers/papers.html.
[7] Barnum, H., 2001. Information-disturbance tradeoff in quantum measurement on the uniform ensemble (abstract). Proc. 2001 IEEE Intl. Symp. on Information Theory, 277. Electronic version of full paper available from the author.
[8] Barnum, H., Caves, C. M., Finkelstein, J., Fuchs, C. A., Schack, R., 2000. Quantum probability from decision theory? Proceedings of the Royal Society of London A 456, 1175–1182.
[9] Barnum, H., Caves, C. M., Fuchs, C. A., Jozsa, R., Schumacher, B. W., 1996. Noncommuting mixed states cannot be broadcast. Phys. Rev. Letters 76, 2818–2821.
[10] Barnum, H., Hayden, P., Jozsa, R., Winter, A., 2001. On the reversible extraction of classical information from a quantum source. Proceedings of the Royal Society of London A 457, 2019–2039.
[11] Barnum, H., Knill, E., Ortiz, G., Viola, L., 2002. Generalizations of entanglement based on coherent states and convex sets, arXiv.org e-print quant-ph/0207149.
[12] Beltrametti, E. G., Bugajski, S., 1993. Decomposability of mixed states into pure states and related properties. Int J Theor Phys 32, 2235–2244.

[13] Bennett, C. H., Brassard, G., Bernstein, E., Vazirani, U., 1997. Strengths and weaknesses of quantum computing. SIAM Journal on Computing 26, 1510–1523.

[14] Bennett, C. H., Brassard, G., Jozsa, R., Mermin, N. D., Peres, A., Schumacher, B. W., Wootters, W. K., 1994. Teleporting an unknown quantum state via dual classical and EPR channels. J. Mod. Opt. 41, 2307–2314.

[15] Bennett, M. K., Foulis, D. J., 1990. Superposition in quantum and classical mechanics. Found. Phys. 20, 733–744.

[16] Bernstein, E., Vazirani, U., 1997. Quantum complexity theory. SIAM J. Comp. 26, 1474–1483.

[17] Bilodeau, D., 1996. Physics, machines, and the hard problem. J. Consciousness Studies 3, 386–401.

[18] Bohr, N., 1928. The quantum postulate and the recent development of atomic theory. Nature 121, 580–590, reprinted in Quantum Theory and Measurement, J.A. Wheeler and W.H. Zurek, eds., Princeton Univ. Press, 1983, and in N. Bohr, Atomic theory and the description of nature, Cambridge Univ. Press, 1934.

[19] Bugajski, S., Gudder, S., Pulmannová, S., 2000. Convex effect algebras, state ordered effect algebras, and linear spaces. Rep. Math. Phys 45, 371–387.

[20] Buhrman, H., Cleve, R., van Dam, W., 1997. Quantum entanglement and quantum communication Los Alamos ArXiV Preprint Archive quant-ph/9705033 New version in 2001.

[21] Busch, P., 1999. Resurrection of von Neumann’s no-hidden-variables theorem, arXiv.org e-print quant-ph/9909073 New version in 2001.

[22] Caves, C. M., Fuchs, C. A., Manne, K., Renes, J. M., 2003. Gleason-type derivations of the quantum probability rule for generalized measurements LANL ArXiV.org gr-qc/0306179.

[23] Caves, C. M., Fuchs, C. A., Schack, R., 2002. Quantum probabilities as bayesian probabilities. Physical Review A 65, 022305.

[24] Clifton, R., Bub, J., Halvorson, H., 2003. Characterizing quantum theory in terms of information-theoretic constraints. Studies in the History and Philosophy of Modern Physics 34, also arXiV.org e-print quant-ph/0211089.

[25] Connes, A., 1974. Caract`erisation des espaces vectoriels ordonn´es sous-jacents aux alg`ebres de von Neumann. Annales de l’Institut Fourier, Grenoble 24, 121.

[26] Cook, T., 1981. Some connections for manuals of empiric al logic to functional analysis. In: Neumann, H. (Ed.), Interpretations and foundations of quantum mechanics: proceedings of a conference held in Marburg 28-30 May 1979. Bibliographisches Institut, Zürich.

[27] Cooke, R. M., Hilgevoord, J., 1981. A new approach to equivalence in quantum logic. In: Beltrametti, E., van Fraassen, B. (Eds.), Current issues in quantum logic. Plenum, New York and London.

[28] Davies, E. B., Lewis, J. T., 1970. An operational approach to quantum probability. Communications in Mathematical Physics 17, 239–260.

[29] Davies, P. C. W., Brown, J. R., 1993. The Ghost in the Atom: A Discussion of the Mysteries of Quantum Physics. Cambridge University Press, Cambridge.

[30] Deutsch, D., 1985. Quantum theory, the Church-Turing principle and the universal quantum computer. Proc R Soc London A 400, 97–117.

[31] Deutsch, D., Jozsa, R., 1992. Rapid solution of problems by quantum computation. Proc R Soc London A 439 435, 553–558.

[32] Dvureˇcenskij, A., 1995. Tensor product of difference posets and effect algebras. Int. J. Theor. Phys. 34, 1337–1348.

[33] Einstein, A., Podolsky, B., Rosen, N., 1935. Can quantum-mechanical description of reality be considered complete? Physical Review A 47, 777–780, reprinted in Quantum Theory and Measurement, J.A. Wheeler and W.H. Zurek, eds., Princeton, 1983.

[34] Everett, H. D., 1957. 'relative state' formulations of quantum mechanics. Reviews of Modern Physics 29, 454–462, reprinted in De Witt and Graham (1973) op. cit.

[35] Everett, H. D., 1957. ‘relative state’ formulations of quantum mechanics. Reviews of Modern Physics 29, 454–462, reprinted in De Witt and Graham (1973) op. cit.

[36] Foulis, D. J., 2000. Representations on unigroups. In: Coecke, B., Moore, D., Wilce, A. (Eds.), Current research in operational quantum logic. Kluwer, Dordrecht.

[37] Foulis, D. J., 2000. Representations on unigroups. In: C devices, Moore, D., Wilce, A. (Eds.), Interpreted quantum mechanics: proceedings of a conference held in Marburg 28-30 May 1979. Bibliographisches Institut, Zürich.

[38] Foulis, D. J., Bennett, M. K., 1994. Effect algebras and unsharp quantum logics. Found. Phys. 24, 1325–1346.

[39] Foulis, D. J., Randall, C. H., 1981. Empirical logic and tensor products. In: Neumann, H. (Ed.), Interpretations and foundations of quantum mechanics: proceedings of a conference held in Marburg 28-30 May 1979. Bibliographisches Institut, Zürich.

[40] Fuchs, C. A., 2001. Quantum mechanics as quantum information (and only a little more), arXiv.org e-print quant-ph/0001002.

[41] Fuchs, C. A., 2001. Quantum foundations in the light of quantum information, arXiv.org e-print quant-ph/0106166; version in 2001.

[42] Fuchs, C. A., Peres, A., 1995. Quantum state disturbance vs. information gain: uncertainty relations for quantum information. Physical Review A 53, 2038.

[43] Fuchs, C. A., 2001. Quantum foundations in the light of quantum information, arXiv.org e-print quant-ph/0106166; to appear in Proceedings of the NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer, ed. A. Gonis.
Fuchs, C. A., Peres, A., 2000. Quantum theory needs no "interpretation". Physics Today 53(3), 70–71.

Gleason, A., 1957. Measures on the closed subspaces of a Hilbert space. Am. J. Math. Mech. 6, 885–894.

Greuling, R. G. H., 1989. Toward a formal language for unsharp properties. Found. Phys. 19, 931–945.

Gudder, S., 1999. Convex structures and effect algebras. Int. J. Theor. Phys. 38, 3179–3186.

Gudder, S., Greechie, R., 2000. Sequential products on effect algebras, preprint.

Gudder, S., Pulmannová, S., 1998. Representation theorem for convex effect algebras. Commentationes Mathematicae Universitatis Carolinae 39, 645–659.

Gudder, S., Pulmannová, S., Bugajski, S., Beltrametti, E., 1999. Convex and linear effect algebras. Rep. Math. Phys 44, 359–379.

Haag, R., 1996. Local quantum physics. Springer, Berlin, revised 2nd edition. (First edn. is Springer, 1992.).

Hardy, L., 2001. Quantum theory from five reasonable axioms, arXiv.org e-print quant-ph/0101012.

Hardy, L., 2001. Why quantum theory?, arXiv.org e-print quant-ph/0111068. Contribution to NATO Advanced Research Workshop "Modality, Probability, and Bell's Theorem", Cracow, Poland 19–23.8.01.

Heisenberg, W., 1927. Über der anschaulichen inhalt der quantentheoretischen kinematik und mechanik. Zeitschrift für Physik 43, 172–198. English translation in *Quantum Theory and Measurement*, J.A. Wheeler and W.H. Zurek, eds., Princeton, 1983.

Holevo, A. S., 1998. Radon-Nikodym derivatives of quantum instruments. Journal of Mathematical Physics 39, 1373–1387.

Knill, E., LaFlamme, R., Milburn, G. J., 2001. A scheme for efficient quantum computation with linear optics. Nature 509, 46–52.

Köpka, F., Chovanc, F., 1994. D-posets. Mathematica Slovaca 44, 21–34.

LaValle, I. H., Fishburn, P. C., 1992. State-independent subjective expected lexicographic utility. J Risk and Uncertainty 5, 217–240.

LaValle, I. H., Fishburn, P. C., 1996. On the varieties of matrix probabilities in nonarchimedean decision theory. J Mathematical Economics 25, 33–54.

Lindblad, G., 1999. A general no-cloning theorem. Letters in Mathematical Physics 47, 189–196.

Ludwig, G., 1981. An axiomatic basis of quantum mechanics. In: Neumann, H. (Ed.), *Interpretations and foundations of quantum mechanics: proceedings of a conference held in Marburg 28-30 May 1979*, Bibliographisches Institut, Zürich.

Ludwig, G., 1983a. Foundations of Quantum Mechanics I. Springer, New York, translation of *Die Grundlagen der Quantenmechanik*, Springer 1954.

Ludwig, G., 1985. An axiomatic basis for quantum mechanics, vol. I. Springer, Berlin/Heidelberg/New York.

Ludwig, G., Neumann, H., 1981. Connections between different approaches to the foundations of quantum mechanics. In: Neumann, H. (Ed.), *Interpretations and foundations of quantum mechanics: proceedings of a conference held in Marburg 28-30 May 1979*, Bibliographisches Institut, Zürich.

Mackey, G. W., 1963. The mathematical foundations of quantum mechanics. W. A. Benjamin, New York.

Mielnik, B., 1969. Theory of filters. Commun. Math. Phys. 15, 1–46.

Nagel, T., 1979. Mortal Questions. Cambridge University Press, Cambridge.

Nagel, T., 1986. The View from Nowhere. Oxford University Press, Oxford.

Nielsen, M. A., 1997. Computable functions, quantum measurements, and quantum dynamics. Physical Review Letters 79, 2915–2918.

Nielsen, M. A., Caves, C. M., Schumacher, B. W., Barnum, H., 1998. Information theoretic approach to error correction and reversible measurements. Proceedings of the Royal Society of London A 454, 277–304.

Peres, A., 1989. Nonlinear variants of Schrödinger’s equation violate the second law of thermodynamics. Phys. Rev. Lett. 63, 1114.

Polchinski, J., 1991. Weinberg’s nonlinear quantum mechanics and the einstein-podolsky-rosen paradox. Phys. Rev. Lett. 66, 397–400.

Reck, M., Zeilinger, A., Bernstein, H. J., Bertani, P., 1994. Experimental realization of any discrete unitary operator. Phys. Rev. Lett. 73, 58–61.

Rovelli, C., 1996. Relational quantum mechanics. Int J. Theor. Phys. 35, 1637.

Saunders, S., 1998. Time, quantum mechanics, and probability. Synthese 114, 373–404. quant-ph/0111047.

Schrödinger, E., 1935. Die gegenwärtige situation in der quantenmechanik. Naturwissenschaften 23, 807–823;823–848;848–849, English translation in Proc. Am Phil Soc. 124, 323–38, reprinted in *Quantum Theory and Measurement*, J.A. Wheeler and W.H. Zurek, eds., Princeton, 1983.

Schulman, L. S., 1997. Time’s Arrows and Quantum Measurement. Cambridge University Press, Cambridge.

Shor, P. W., 1994. Algorithms for quantum computation: discrete logarithms and factoring. Proc. 37th ann. symp. on the foundations of computer science , 56–65.

Shor, P. W., 1997. Polynomial-time algorithms for prime factorization and discrete logarithms on a quantum computer. SIAM J. Comp. 26, 1484–1509.

Simon, C., Bużek, V., Gisin, N., 2001. No-signaling condition and quantum dynamics. Physical Review Letters 87, 170405.

Simon, D., 1997. On the power of quantum computation. SIAM J. Comp. 26, 1474–1483.

Smolin, L., 1995. The Bekenstein bound, topological quantum field theory and pluralistic quantum cosmology LANL ArXiv.org gr-qc/9508064

Valckenborgh, F., 2000. Operational axiomatics and compound systems. In: Coecke, B., Moore, D., Wilce, A. (Eds.), *Current research in operational quantum logic*. Kluwer, Dordrecht.

Vinberg, E. B., 1965. The structure of the group of automorphisms of a homogeneous convex cone. Trans. Moscow Math.
Wallace, D., 2002. Worlds in the everett interpretation. Studies in History and Philosophy of Modern Physics 33, 637–661.
Wallace, D., 2003. Everett and structure. Studies in History and Philosophy of Modern Physics 34, 87–105.
Weinberg, S., 1989. Phys. Rev. Lett. 62, 485.
Wigner, E. P., 1952. Z. Physik 131, 101.
Wilce, A., 1992. Tensor products in generalized measure theory. Int. J. Theor. Phys. 31, 1915–1928.
Wilce, A., 1994. A note on partial abelian semigroups, university of Pittsburgh preprint.
Wilce, A., 1998. Perspectivity and congruence in partial abelian semigroups. Mathematica Slovaca 48, 117–135.
Wilce, A., 2000. Test spaces and orthoalgebras. In: Coecke, B., Moore, D., Wilce, A. (Eds.), Current research in operational quantum logic. Klüwer, Dordrecht.
Wootters, W. K., Zurek, W. H., 1982. A single quantum cannot be cloned. Nature 299, 802.
Zurek, W. H., 1981. Pointer basis of quantum apparatus: Into what mixture does the wavepacket collapse? Phys. Rev. D 24, 1516.

One wonders, though: suppose we have $2^n$ energy levels such that there is a “generic” set of positive reals $\epsilon_i$ such that the energies, indexed by subsets $K$ of $\{1, \ldots, N\}$, have the form $E_K = \sum_{i \in K \subseteq \{1, \ldots, n\}} \epsilon_i$; isn’t the most compact description of this situation just to say that we have $n$ two-state systems, with energies $\epsilon_i$? In the nongeneric case, say all $\epsilon_i$ equal, though the spectrum including degeneracies would be that of $n$ qubits, we would be unable to associate individual energy states with “which qubit is excited.” (Put another way, there would be many tensor product factorizations into qubits with energies $O, \epsilon$ compatible with the Hilbert space and spectrum.) With some degeneracy breaking, say of the form that would come from small, unequal local perturbations $\delta_i$ to the excited state energy levels of a bunch of qubits, perhaps we could associate a “best” qubit decomposition with the spectrum. If this is so, it suggests the nice idea that the existence of sufficient order requires a tiny bit of disorder, to serve as a foil. Perhaps in some theories, this is the role played by initial conditions, versus laws. The laws isolate the order; disorder is concentrated in the initial conditions.