Observations on Hyperplanes: I.
State Reduction and Unitary Evolution

by

Gordon N. Fleming
Professor Emeritus of Physics
Penn State Univ.
Univ. Park, PA

gnf1@earthlink.net

Abstract:

This is the first of two papers responding (somewhat belatedly) to ‘recent’ commentary on various aspects of hyperplane dependence (HD) by several authors. In this paper I focus on the issues of the relations of HD to state reduction and unitary evolution. The authors who’s comments I address here are Maudlin and Myrvold. In the second paper of this set I focus on HD dynamical variables and localizable properties and measurements and address comments of de Koning, Halvorson, Clifton and Wallace. Each paper ends with some reflections on the implications of HD for the ontology of Minkowski space-time. To set the stage for my responses, I begin this paper with some general position statements designed to correct what seem to be widespread and erroneous construals of some of my views. Two central points are argued for in this first paper. First, **dynamical evolution occurs not only within foliations of Minkowski space-time. Rather the transition from the physical state of affairs on any one hyperplane to any other, whether the hyperplanes intersect or are parallel, is always an instance of dynamical evolution between them, generated by some active combination of boost-like transformations and/or time-like translations and/or state reductions and ‘reconstructions’.** This point gives rise to a generalized conception of a history of dynamical evolution, allowing for the use of parameterized families of hyperplanes that multiply cover some portions of space-time. Nevertheless, and this is the second central point, **for any two generalized histories, H and H’, the quantum states for a system on all the hyperplanes of H from the asymptotic past up to some hyperplane, h, determine the quantum states for the system on all the hyperplanes of H’ from the asymptotic past up to any hyperplane, h’, such that h’ lies to the past of all those state reduction regions that lie to the future of all hyperplanes of H that are ‘earlier’ than h.** Consideration of these results should defuse concerns that have been voiced about the coherence and consistency of HD. A position closely related to the second point (albeit restricted to the comparison of histories confined to foliations) has already been argued for by Myrvold (2002).

1. Introduction and general position:

This is the first of two papers in which I try to clarify some of my views on Lorentz covariant quantum theory (LCQT) and the role of hyperplane dependence (HD) therein. The main body of the discussion is structured around a detailed response to 'recent' commentary in the literature concerning HD, state reduction and localization.

The specific topics addressed in this first paper of the set, in the order in which they will arise, are: (**§ 1a**) general considerations of the status and nature of the particles or quanta of quantum field theory (QFT) (here I try to shed a reputation for championing the fundamental status of point particles, a view I have never held), (**§ 2**) the causal analysis of statistical correlations between state reductions on intersecting hyperplanes (response to Maudlin's 1996), (**§ 3**) the part-whole relations that can hold within composite and/or spatially extended quantum systems and the different forms they can take on intersecting hyperplanes (response to Maudlin's 1998), (**§ 4**) the limiting case of hyperplane dependent state vector assignments in the presence of composite systems with constituents space-like separated by distances greatly in excess of their own space-like dimensions (response to Myrvold's 2002) and (**§ 5**) the concept of 'becoming' and dynamical evolution within LCQT (response to Myrvold's 2003). In the last section, **6**, I summarize the preceding discussion and offer some reflections as to where the preceding considerations may be pushing regarding the ontology of the spatio-temporal framework.

In the second and somewhat longer paper of this set the topics addressed are, (**§ 1**) the need for HD over ordinary time dependence of dynamical variables and states and the relationship between classical and quantum examples of HD dynamical variables (responses to anonymous queries), (**§ 2**) issues of frame dependence and frame independence resolved by HD and the relationships between localizable properties and localizable measurements (response to de Koning's 2001), (**§ 3**) the relationship between putative localization schemes, the relationship between local subluminal dynamics over a superentangled vacuum and non-local HD superluminal dynamics over a product vacuum, the distinction between localizing entities and localizing *properties* of entities and the status of universal microcausality and, again, localizable measurements (response to Halvorson's 2001), (**§ 4**) some of the same issues as in **§ 3** as well as the non-existence of strictly localizable objects and, consequently, strictly localizable measurements and, once again, the status of universal microcausality (response to Halvorson and Clifton's 2002), (**§ 5**) the relationship of field quanta and fields in QFT (response to Wallace 2001) and, (**§ 6**), some quantitative details on the comparison of localization schemes and the non-localizability of field quanta via the energy distribution of Newton-Wigner eigenstates. As in the first paper I devote the last section, **7**, to summarizing conclusions and reflections concerning spatio-temporal ontology.

In later sections, when I address specific differences with various authors, I will quote extensively from their writings, which seem to me either worthy of emphasis or in error, and will respond to the quotes pro or con. In the next subsection, **1a**, however, because of my perception of a widespread notion that I champion the fundamental status of point particles in LCQT (which misconception bears on the principal topics of these two papers), I will, without attribution of the sources of the misconception, simply deny the views which I do not hold and assert the corresponding ones that I do hold.

1a: the status of quantized fields and their particle-like quanta

First, I do not regard the particles of QFT as having any fundamental status. Within the framework of traditional QFT it is the fields that have the relatively fundamental status, since, among other things, it is only the fields that persist. However, for some time now, the fundamental status of QFT itself has been suspect, if not altogether abandoned. Even if Superstring Theory were not to live up to its current promise, the *Effective Field Theory* (Weinberg 1995, pp. 499, 523-5; 1996, **19.5-8**) attitude towards QFT renders QFT an essentially phenomenological treatment of the phenomena of high energy physics. But even this loss of fundamental status by the fields of QFT does not reinvest the particles of QFT with fundamental status. It just deprives the particles of any account that is based in fundamentals (modulo Superstring Theory).

In fact, I tend to be suspicious of the very concept of 'the fundamental', especially in science, but also in philosophy. That's a big topic that I don't wish to address here. I will just note that enough history of science has passed under the bridge to provide the lesson that anything that seems fundamental at some time is not likely to seem so for very long and long established useful theories usually have several ways of being formulated in terms of different sets of so-called 'fundamental' ingredients.

Second, I do not regard the particles of QFT or high energy physics as being point entities, i.e., without space-like extension. None of them! Not even the leptons, quarks and gluons that many of my high energy physics colleagues (at least those not yet committed to superstrings) commonly refer to as point entities. Indeed, since the mid 60's I have given arguments (1965a, b), here and there, on behalf of the space-like extension for all massive relativistic quantum particles. Nor does my position on this matter have anything to do with the Planck length order of magnitude extension that Superstring Theory demands for leptons, quarks and gluons. Without going into details, I claim that when my high energy colleagues declare leptons, say, as being point-like, as far as we know, what they mean is that these particles display *no internal structure* in scattering processes. But to infer pointhood from structurelessness involves an unwarranted reliance on a classical analogy. Some day our theories and our measurements may require us to recognize structured extension of leptons, quarks and gluons. For now, however, we can already argue for their *structureless extension*, on the order of, at least, their Compton wavelength, or so I would claim, fully aware, of course, that this view is not widely shared.

I believe the tendency to assume that a point-like entity is meant, when a worker calls a quantum entity a particle, is partly an unfortunate psychological consequence of the classical particle concept resisting partial transfer into the quantum domain. Paul Teller (1995) has argued for the consistent use of the term ‘quanta’ in place of ‘particle’ to avoid this and other undesirable tendencies. I have argued (2002) against this use of the term ‘quanta’ because of its already poly-semantic employment. But I agree with Teller that the term ‘particle’ has proved itself rather troublesome. Therefore, I would like to champion – as the *generic name of the members of the family* to which photon, electron, muon, tauon, lepton, gluon, neutron, proton, nucleon, pion, kaon, meson, hadron, and yes, - - - quark and neutrino, belong as instances or subsets – the term, **quanton**[1]. The term is not new, having been variously used for roughly the same purposes intended here since at least 1988 (Levy-Leblond, 1988; von Baeyer, 1997). In an effort to generate familiarity, I will use the term throughout these two papers.

So – a major focus of my work over many years – on localization and position operators, is not motivated by a concern for the treatment of fundamental, point-like particles ('though I often illustrate aspects of that work by considering single quanton quantum states). It is motivated by an interest in the problem of providing a manifestly covariant account for the position (as a quantum mechanical observable) of *anything*, in particular any **localizable property** of a physical system, especially localizable properties that are common to large classes of physical systems. Such properties include the center of energy (CE) or the center of spin (otherwise known as the Newton-Wigner (NW) position – suitably generalized to arbitrary massive physical systems). All massive systems in LCQT have such position observables, not just quantons. For example, any quantized massive Dirac field, as a whole, has a CE and an NW position operator as, in 'most' states, does the quantized Electromagnetic field (Fleming 1966).

2. Response to Maudlin (1996) on correlated state reductions on intersecting hyperplanes

In my (1996) I responded to Maudlin's earlier, excellent critique of hyperplane dependence, which appeared in his (1994). A major concern of Maudlin then was the "radically new ontological conception of the world" which hyperplane dependence embodied. Since it seemed to me that he regarded this radicality as stemming solely from the need to handle quantum measurement, or state reduction, in a Lorentz covariant manner, I stressed the fact that in purely deterministic, unitary evolution of composite quantum systems, precisely analogous hyperplane dependence would be seen in the various subsystem mixed states as a dynamical consequence of the evolving correlations between the subsystems. I then added, and will repeat here for emphasis (1996, p.19), "In this way we see that the hyperplane dependence of state vector assignments in the presence of state reduction is a consequence, required by consistency considerations, of hyperplane dependent correlations between microsystems and macrosystems established via unitary evolution in the absence of state reduction."

I also tried to assuage Maudlin's concern over the assignment, in the presence of state reductions, of distinct polarization states to a single quanton on intersecting hyperplanes, the intersection of which overlapped the localization region of the quanton within those hyperplanes. But my comments, in that response, on the existence of many HD observables, including polarization, were incomplete. I return to this matter in **§§ 3**, (pp. 13-15), and **4**, (p. 20), with a, hopefully, more adequate account.

In his (1996) Maudlin resumed commentary on hyperplane dependence and I now offer this severely belated response. This time, I think, Maudlin's characteristically penetrating observations are accompanied by a genuine misunderstanding of one aspect of hyperplane dependence and my comments will focus on correcting this.

Not unreasonably, following my (1996) comments, Maudlin (1996) challenges me to take state reduction seriously (p.299): "If Fleming thinks that collapses are objectively real, then he must take the collapse dynamics seriously, and see how the hyperplanes play a role there. And here there is, I think, a deep and difficult puzzle." Maudlin then considers the standard EPR-Bohm-Bell arrangement for two entangled electrons initially in the singlet spin state but with slightly misaligned polarization detectors to intercept the two electrons, (**Fig. 1**). The singlet spin state,

$$|\Psi_0\rangle = (2)^{-1/2}\ \{\ |e_L, \uparrow L\rangle |e_R, \downarrow L\rangle - |e_L, \downarrow L\rangle |e_R, \uparrow L\rangle\ \}$$

$$= (2)^{-1/2}\ \{\ |e_L, \uparrow R\rangle |e_R, \downarrow R\rangle - |e_L, \downarrow R\rangle |e_R, \uparrow R\rangle\ \}, \qquad (2.1)$$

holds on both hyperplanes, a and α, where the designations $\uparrow$L - - - $\downarrow$R refer to spin states along the directions defined by the slightly misaligned left (L) and right (R) detectors. Similarly, if the state reductions at L and R yield the results $\uparrow$L and $\downarrow$R, respectively, then the state vector,

$$|\Psi_f\rangle = |e_L, \uparrow L\rangle |e_R, \downarrow R\rangle, \qquad (2.2)$$

holds on both hyperplanes, c and γ.

Accepting the standard explanations of the correlations between the a – b and b – c collapses, on one hand, and between the $\alpha - \beta$ and $\beta - \gamma$ collapses, on the other, Maudlin asserts,

> - - - there is still an unexplained correlation. The a – b collapse is correlated not only with the b – c collapse, but also with the $\alpha - \beta$ collapse. That is, when the a – b collapse yields a down result on the R electron, 99% of the time the $\alpha - \beta$ collapse yields an up result on the L electron. What could possibly explain *this* ?(p.301)

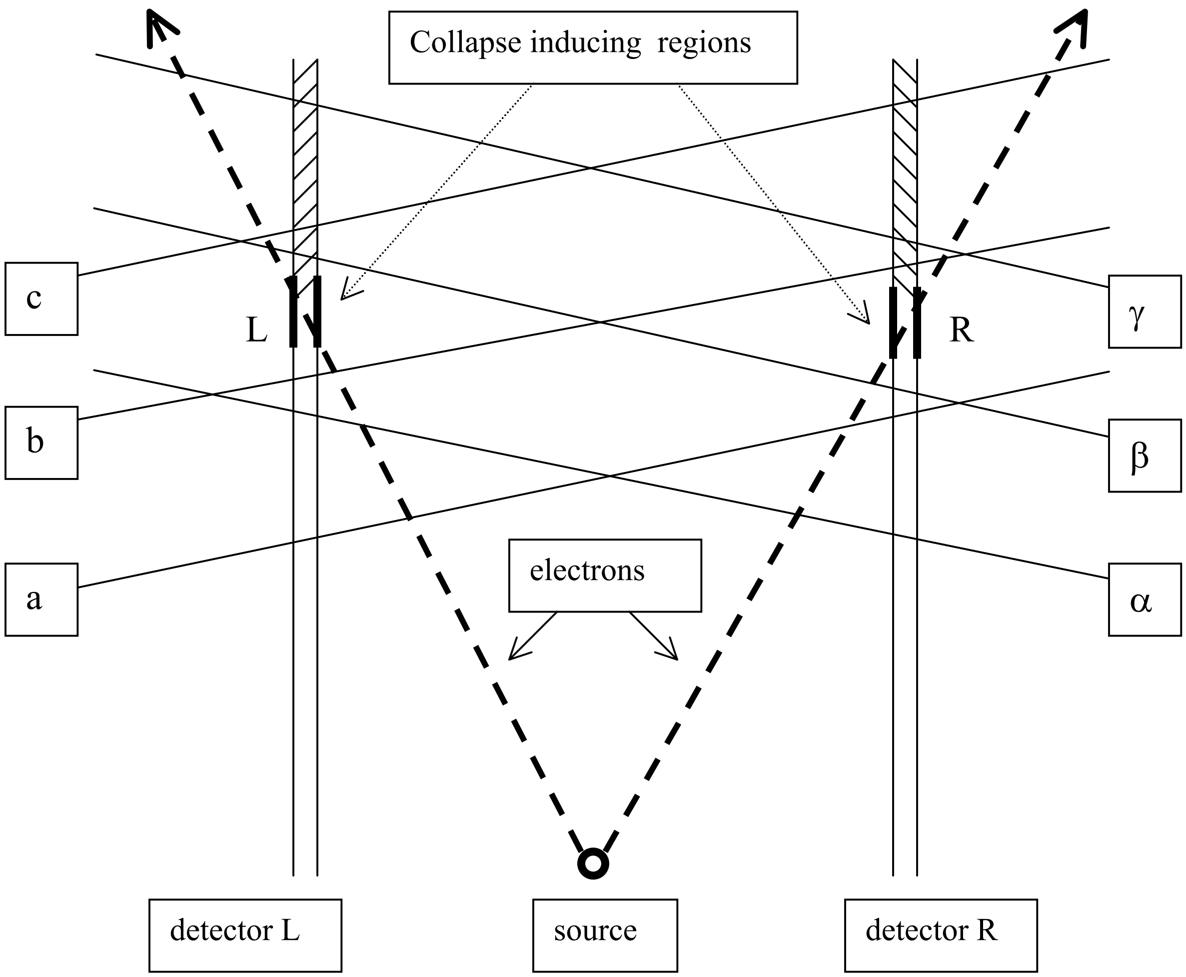


Fig. 1: EPR state reductions and hyperplane dependent state assignments

(1) In the Heisenberg picture the two-electron state <u>must</u> be the same on hyperplanes a and α since they are connected by unitary boost-like evolution.

(2) The hyperplane evolution from a to b includes the R state reduction changing the singlet state on a into a product state on b, say $|e_{L}, \uparrow R >|e_{R}, \downarrow R >$.

(3) The hyperplane evolution from α to β includes the L state reduction changing the singlet state on α into a product state on β, say $|e_{L}, \uparrow L >|e_{R}, \downarrow L >$.

(4) The b → c evolution including the L state reduction and the β → γ evolution including the R state reduction <u>must</u> yield the same Heisenberg picture state on c and γ since the physical states of affairs on c and γ are connected by unitary boost-like evolution and so both hyperplanes carry the same Heisenberg picture state vector.

He then goes on to question the adequacy of answering his query, as I might do, by insisting that for "consistency considerations" the result of the α – β collapse must be the same as the result of the b – c collapse since both collapses represent one and the same measurement event. As he puts it (p.301), "But the problem is to understand what, given the ontology of the theory, this last remark could possibly mean."

Maudlin's comments concerning "the ontology of the theory", however, indicate a misunderstanding of some of the dynamical aspects. Maudlin is exactly correct when he characterizes the move to hyperplane dependence as (p. 299), " - - - enfranchis[ing] *all* the foliations, embracing a radical democracy, and thereby demanding no new spatio-temporal structure." But it appears that Maudlin sees the dynamics holding within each foliation as somehow independent of all the others.

> Fleming must therefore accept a dynamics for *each* family of hyperplanes which uses that family to play the role of absolute time in the non-relativistic dynamics.(p.299) - - - These collapses are governed by a dynamics defined on a family of hyperplanes, and each family of hyperplanes, it seems, operates essentially as an independent universal time for the wave functions associated with them. So how are the collapses in different families coordinated?(p. 302)

But, as Myrvold (2002, p.450) has already pointed out, from a somewhat different perspective, the dynamics operating within single foliations of parallel hyperplanes are <u>not</u> independent. They are tightly correlated, and, I would add, *in the absence of state reduction* that correlation is implemented as a unitary dynamical process itself, being generated by the <u>active</u> action of the homogeneous Lorentz group generators, always including a contribution from the boost generators. Just as a **time-like** active action of the space-time translation generators functions as a Hamiltonian to change the physical state of affairs on one hyperplane into that on a distinct <u>parallel</u> hyperplane, so a **boost-like** active action of the homogeneous generators functions as a Hamiltonian to change the physical state of affairs on one hyperplane into that on a distinct reoriented (and, therefore, <u>intersecting</u>) hyperplane. For the general case this dynamical role requires a *dependence of the boost generators on the presence and nature of interactions* which is intimately connected with the interaction dependence of the time-like translation generators (Fleming 2003).

If we now return to **Fig. 1** we see that hyperplanes c and γ, both lying to the future of both state reductions, each harbor physical states of affairs involving two free electrons correlated with two post measurement detectors but not entangled with each other. Active, unitary, boost-like evolutions takes either of those hyperplanes and their associated physical states of affairs into the other. But, being free, the electrons can not have their spin states change within that evolution[2]. Thus consistency requires that both the b – c collapse and the α – β collapse yield the same result for the L electron. Similarly for the a – b and α – γ collapses for the R electron. There is only one L collapse and there is only one R collapse!

To be sure, in this case the unitary evolution from c to γ, or γ to c, does not require interaction dependence of the boost-like generators since the electrons are free for that evolution. But the general case would not be interaction free and arguments of the type employed here could succeed in the general case only if the boost-like generators carried the requisite interaction dependence, which they do. This issue will come up again in **§ 4**.

Maudlin found the consistency argument wanting because

> - - no such consistency is demanded elsewhere. In the region immediately below region L, for example, relative to the α – β – γ family, the L electron is still in the singlet state, while according to the a – b – c family it is in an up eigenstate for spin in the R direction.(p.302)

But the reason the electrons can be in a singlet state on α and a product state on b is that the state reduction at R prevents a unitary evolution from α to b or back. Similarly for the different states on a and β. But the unitary dynamical connections between a and α or between c and γ are not undermined by state reduction and boost-like generators will implement those connections.

Another way to emphasize that one does not have distinct reductions for each foliation evolving through a state reducing region is by noting that if the reduction is implemented in one foliation by applying a projection operator[3], Π, to the state vector associated with leaves of the foliation immediately to the past of the reducing region (a *precursor* state), then, for any other foliation, that same projector, Π, is similarly applied to obtain the reduced state (Fleming 1985, pp. 53-60). The reduced states may differ from foliation to foliation, but that's because their *precursors* already differed to the immediate past of the reducing region. Thus, from the distinct, Heisenberg picture, precursor states, $|\Psi_0>$ and $|e_L, \uparrow L>|e_R, \downarrow L>$ on the hyperplanes a and β, respectively, we obtain, from the R region state reduction, the distinct reduced states,

$$(2)^{-1/2}|e_L, \uparrow R>|e_R, \downarrow R> = (I \times \Pi_{\downarrow R})|\Psi_0>, \qquad (2.3a)$$

and

$$|e_L, \uparrow L>|e_R, \downarrow R><e_R, \downarrow R|e_R, \downarrow L>$$

$$= (I \times \Pi_{\downarrow R})|e_L, \uparrow L>|e_R, \downarrow L>, \qquad (2.3b)$$

on the hyperplanes b and γ, respectively.

In more complex instances in which more than two space-like separated state reductions are involved, the differences, for a given reduction, between reduced states associated with distinct foliations can be correspondingly more complex than here. For an example see my (1992). The generality of the considerations that have been brought to bear on this example may be summed up in the statement that,

the transition from the physical state of affairs on any one hyperplane to any other, whether the hyperplanes intersect or are parallel, is always an instance of dynamical evolution between them, generated by some active combination of boost-like transformations and/or time-like translations and/or state reductions and 'reconstructions'.

The term 'reconstructions' refers to the fact that reorienting a hyperplane results in 'half' of the hyperplane backing into the past of the original hyperplane and, possibly, backing through state reductions to the reconstruction of precursor states. Such reconstructions would not be calculable if the state reduction operator is not invertible, as in the above example. But that practical problem does not interfere with the resolution of the correlation problem we are addressing here. Further elaboration of these issues will be provided below in **§ 5** (p. 24).

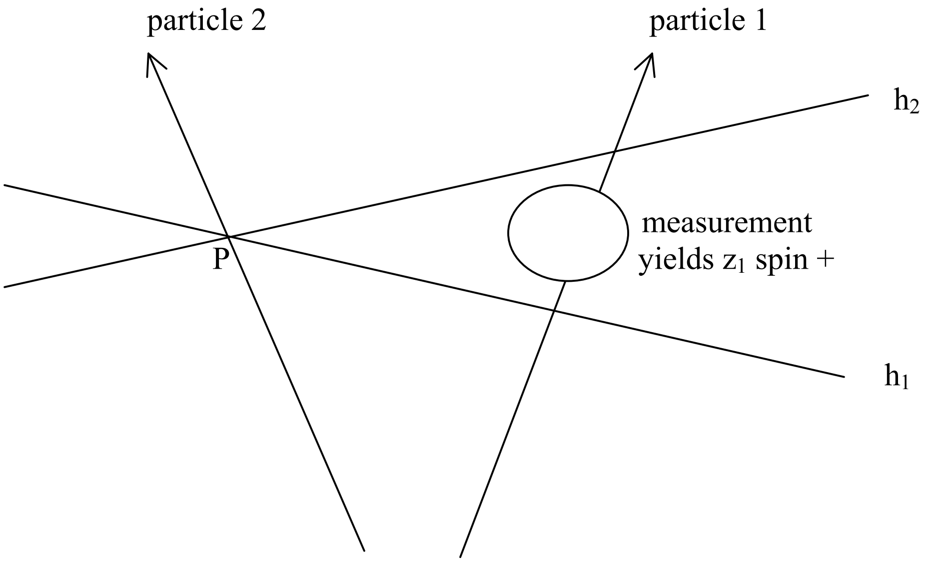
A last comment on a style difference, between Maudlin and myself, in discussing these matters, which may be confusing if not mentioned.

I refer to Heisenberg picture state vectors or density operators which, for closed systems, never change under unitary evolution, i.e., *such objects represent* (in the absence of state reduction) *the relationship of entire evolutionary histories to inertial frames of reference*. For a given frame of reference they change only in the presence of state reduction and then they correspond to those *portions* of entire evolutionary histories *lying between* the state reduction regions. As such they are associated with or are said to hold on every hyperplane similarly *lying between* the same state reduction regions. What changes unitarily in the Heisenberg picture from hyperplane to hyperplane, between state reduction regions, are the operators representing dynamical variables and their corresponding orthonormal eigenvector bases.

Maudlin tends to refer to state functions or wave functions. These are inner products between Heisenberg picture state vectors and Heisenberg picture eigenvectors. State functions almost always change from hyperplane to hyperplane, exclusively unitarily in the absence of state reductions, partly non-unitarily in their presence.

3. Response to Maudlin (1998) on HD part-whole relations in composite systems

In Maudlin's (1998) his concern is with the part-whole relations that can occur in the quantum world and the radical difference that emerges there compared to the non-quantum world. The most severe differences occur in the presence of both Minkowski space-time and state reduction. He considers a simpler arrangement than discussed in his (1996) and now the focus is not on the *correlations* between the results on intersecting hyperplanes but on the *differences* that can occur between the results on intersecting hyperplanes (**Fig. 2**). In particular the focus is on (1), the apparent need, in order to pin down the quantum state of one quanton (in this case an electron) at a space-time point, P, to choose one among the hyperplanes that contain P, and (2), the dramatic difference in the quantum states that can result from the choice, depending on such things as the existence of other quantons and state reducing regions and entanglements, etc.



$$|\Psi(h_1)> = (1/2)^{1/2}\{|z_1+>|z_2-> - |z_1->|z_2+>\}$$

$$|\Psi(h_2)> = |z_1+>|z_2->$$

Fig. 2: Distinct spin states for two quanton system on hyperplanes, h_1 and h_2, intersecting ‘at the world line’ of the unmeasured quanton.

The discussion that bears on HD occurs in the second half of Maudlin’s article. Summing up some of the results of the preceding discussion he writes,

> In quantum theory, then, the physical state of a complex whole cannot always be reduced to those of its parts, or to those of its parts together with their spatio-temporal relations, even when the parts inhabit distinct regions of space.(p.55)- - - But in the relativistic regime there is no unique way of carving up extended spatio-temporal objects into sets of interrelated parts.(p.56)

Turning again to the ever popular entangled singlet state of two spin ½ quantons, here chosen distinguishable (an electron and a proton, say) for simplicity, and with quanton 1 encountering a spin-z measurement in some bounded space-time region (**Fig. 2**), Maudlin assumes a space-like separation of their *world lines* that permits a *point*, P, on the quanton 2 *world line* that,

> - - - in one reference frame occurs before [quanton] 1 is measured and in another reference frame occurs after [quanton] 1 has been measured. - - - We have already seen that we cannot, on any view, capture all there is to say about the physical state of [quanton] 2 at point P without regarding it as a part of a larger whole. But which whole? Should we take [quanton] 2 at P together with [quanton] 1 before the measurement or with [quanton] 1 after the measurement? On any view the choices seem to be incompatible.(p.56)

Incompatible, of course, because the first choice leaves quanton 2 with no pure (or vector) spin state of its own while the second choice gives quanton 2 a definite pure spin state of its own, both, presumably, at *point* P.

Maudlin's reasoning here crucially employs idealizations which may be innocuous if kept in mind, but which, it seems to me, lead astray because they are not being kept in mind. The quantons in **Fig. 2** are portrayed via *world lines* intersecting hyperplanes at *points*, and Maudlin is inquiring about the state of a quanton at a *point* intersected by hyperplanes. Outside of Bohmian interpretations, however, real quantons do not have *world lines* or quantum states at space-time *points* at all. Even if, at some time, or on some hyperplane, a quanton has some **localizable property** sharply confined within a bounded region, that does not entail that the entire quanton, itself, is so confined. Furthermore, for any localizable property represented by a self adjoint projection operator, the property confinement immediately disperses, apparently partly superluminally (Fleming 1965b, 1985; Hegerfeldt 1974, 1985, 1998).

To be sure, there is a FAPP level of discourse within which we can often get away with talk of the evolution of a quanton as motion within a *world tube* to a good approximation. But even then it is a **localizable property** of the quanton (roughly determined by the detection process we have in mind) we're referring to when we engage in such discourse.

So the world lines of **Fig. 2** should, more correctly, be replaced by (probably expanding) world tubes and the intersection points with hyperplanes by intersection regions (**Fig. 3**). A real space-like hyperplane intersects a time-like world tube in a 3-dimensional bounded region and any two intersecting hyperplanes have an unbounded 2-dimensional plane as their intersection. So the only part of Minkowski space-time that the putative world tube of a quanton and two intersecting hyperplanes could have in common is a bounded portion of a 2-dimensional plane (**Fig. 3**).

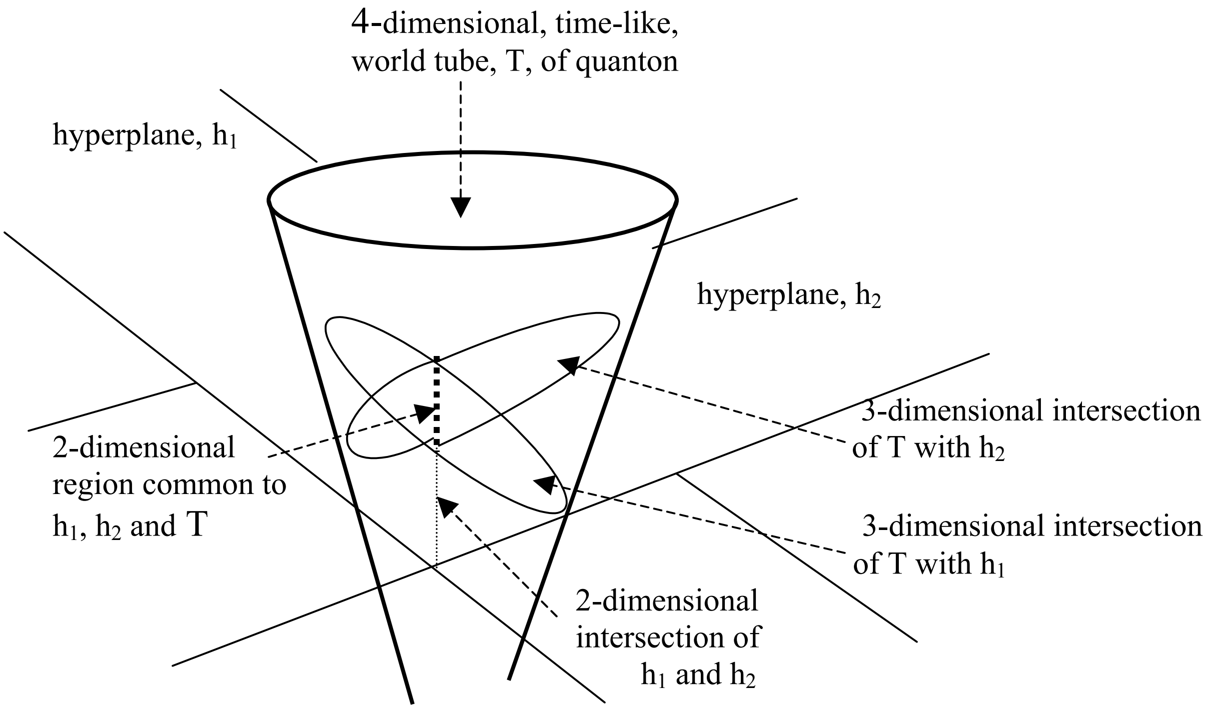


Fig. 3: Intersections of hyperplanes and a world tube

So, returning to Maudlin's account, the *parts of space-time* to which the two hyperplane choices attribute distinct states to quanton 2 are not one and the same point, P. There is no such point! The *parts* are, at best, distinct, bounded 3-dimensional space-like regions that share, at most, a 2-dimensional bounded planar region (**Fig. 3**). While this consideration may not eliminate the conceptual novelty completely, it certainly removes some of its bite. *The distinct states are associated with, at least, distinct 3-dimensional regions of different hyperplanes and those regions share, at most, a 2-dimensional overlap.*

Before Maudlin moves on to the consideration of the HD approach to this problem he mentions that this is not an instance of purely relational states

> - - familiar from Socrates being tall relative to Cebes and short relative to Theatetus. In that cast, Socrates has an unproblematic intrinsic state (his height) and all that changes are relational properties. But now the only possible intrinsic property (the spin state) itself changes in two wholes.(p.57)

I think it apropos here to mention that Socrates intrinsic height changes with the spatio-temporal whole characterized by his *age*. We tacitly understand the familiar example to refer to the adult Socrates but that simply means that we have quietly and in concert chosen the spatio-temporal whole. And it is a whole because Socrates was even less of a point entity than a single quanton and he was an open system besides. This dependence on a spatio-temporal whole is familiar and that fact has a lot to do with it not bothering us. But besides familiarity, the spatio-temporal wholes characterized by Socrates different ages do not *intersect* anywhere near Socrates location within them[4]. That eases the acceptance of the dependence. In the quantum case under discussion the spatio-temporal wholes we are considering not only intersect; their mutual intersection extends inside the locations of the quanton world tube within them. That is why the dimensional considerations spelled out above are important.

If all physical systems have non-zero, 3-dimensional space-like extension, as can certainly be argued for within LCQT and QFT (and will be argued for in more detail in my (2004), supporting conclusions to the same effect of Halvorson and Clifton (2002), albeit for my own idiosyncratic reasons), then the additional general considerations which have been brought to bear here (beyond those singled out on p. 10) can be stated as,

if any quantum system exists on each of two distinct hyperplanes, whether intersecting or parallel, the spatio-temporal regions occupied by the system on each of the distinct hyperplanes are always distinct 3-dimensional regions sharing, at most, a 2-dimensional intersection.

After mentioning the Bohm approach to this problem, which denies relativity at the fundamental level and assumes a preferred frame that determines the true simultaneity of wholes, Maudlin characterizes the HD approach as one in which,

> - - we won’t be able to say anything at all about [quanton] 2 - - - until we further specify the remaining parts of some whole. Since the candidate wholes all have parts that exist simultaneously in some referance frame, this means that we must specify a simultaneity slice through P. This idea, that all the properties of an object are relative to a hyperplane, has been championed by Gordon Fleming in his hyperplane dependent formulation of quantum mechanics.(p.58)

While I am uncomfortable with Maudlin’s repeated association of hyperplane choices with reference frames (see **§ 5**, below, p. 22) and would remove the reference to the point, P, and replace the word “object” by ‘system’ and the phrase “all the properties” by ‘many of the properties’ (since many systems have some properties that are not hyperplane dependent), this characterization is correct.

Maudlin then asserts (as in his 1994) that, “- - this radical hyperplane dependence is truly new.”, and he downplays my protests to the contrary (in my 1996 and lectures that led to my 2000). Presumably thinking of my (2000) account of HD in classical relativity he argues,

> In relativistic physics, lots of quantities are hyperplane dependent *locally*, how you describe the physical quantities at some point may depend on how you chop up space-time into slices *around that point*. But this new hyperplane dependence posits something else: how you describe the physical state at a point depends on picking a hyperplane through it and *on what goes on arbitrarily far away on that hyperplane*.(p.58)

Now I don’t know just what quantities Maudlin has in mind here since he doesn’t mention any, but he can’t be referring to the quantities I’ve used as examples from classical relativity. My most frequently mentioned example is what I’ve called the classical center of energy (CE) position variable for an arbitrary system on an arbitrary hyperplane. It is proportional to, as its name suggests, the position weighted integral, over the entire hyperplane, of an energy-like density constructed from the stress-energy-momentum field for the system. It’s a covariant analogue of the non-relativistic center of mass, into which, for *material* systems, it degenerates as the vacuum speed of light goes to infinity. If the system is itself a field, such as the EM field, or a space filling fluid, or even a multi-particle system with widely dispersed particles, then the location of the CE can, indeed, depend on what goes on far away on the hyperplane and can be very different on two intersecting hyperplanes, even two hyperplanes with an intersection that contains the location of the CE on one of them.

Be that as it may, Maudlin then goes on to indicate a feature of HD that is genuinely new in the quantum case, but which he fails to qualify adequately.

> - - no amount of information about how things are described on one set of hyperplanes (e.g., that the pair of [quantons] is in a singlet state before during and immediately after P) can give a clue about how they will be described on another set of hyperplanes - - (e.g., that [quanton] 2 has z-spin up).(p.58)

It is important to emphasize here that the situation Maudlin describes is crucially dependent upon the presence of state reduction. But we already know that the outcomes of state reduction can be inherently indeterministic from non-relativistic quantum mechanics. In the absence of state reducing regions lying *between* the hyperplanes under consideration, then, as mentioned in the previous section, the Heisenberg picture physical states associated with the hyperplanes are identical and the state *functions* are connected by unitary evolution; if you know the state functions on just one such hyperplane and the dynamics of the system, the state functions on the other such hyperplanes are determined.

But even in the presence of state reductions,

if one knows the evolution of the system for one foliation, F, from the distant past up to a given hyperplane, h, of that foliation, then the evolution on any other foliation, F', is thereby determined from the distant past up to any hyperplane of F' lying to the past of all state reductions that, themselves, lie to the future of h.

This is required by (1), the existence of unitary boost-like and/or time-like evolution connecting any two hyperplanes, intersecting or parallel, which lie between the same sets of state reducing regions and (2), the implementation of the outcome of a given state reduction by the application of the same (projection or effect) operator to each *precursor* state, regardless of hyperplane (**§ 2**, pp.9, 10).

In keeping with my declared view that all instances of physical change from any one hyperplane to any other are instances of dynamical evolution, it would be desirable to reformulate the italicized paragraph above to remove the dependence upon evolution within foliations of hyperplanes. This will be done below in **§ 5** (p. 24).

4. Response to Myrvold (2002) on HD as providing varied descriptions of objective occurrences.

Unlike Maudlin and most of the authors I consider in the sequel to this paper Myrvold is not a critic of HD. While carefully avoiding an assessment of the relationship between his perspective on HD and mine, he clearly regards instances of HD as necessary in LCQT and not at all in conflict with relativity, pace Einstein on separability. He confines his discussion within an approximation or limiting case – the situation in which the constituents of a composite system are space-like separated far in excess of their own space-like dimensions – whereas I am very concerned to go beyond that approximation. But within the limits of these conditions Myrvold's treatment is careful and, I think, convincing! I strongly recommend the careful reading of his paper.

It is not that I agree with everything Myrvold has written. I do have questions and challenges. But even my doubts are directed at matters rather clearly put.

Myrvold's concern is to show that arguments claiming the incompatibility of state reduction and relativity theory

> - - - do not succeed. Attention to the transformation properties of quantum mechanical states undergoing unitary, non-collapse evolution points the way to a treatment of collapse evolution consistent with the demands of relativity.(p.436)

He carefully spells out the domain of applicability of the analysis he will offer,

> - - - we will concern ourselves with - - - systems that, at least to a high degree of approximation, can be regarded as confined to bounded regions of space, and we will assume that the systems are confined to regions of space that are large compared to the Compton wavelengths of the systems in question but small compared to the distances between them. - - - The conclusions of this paper must therefore be regarded as limited to the scope of the approximation invoked.(p.438)

He then considers in considerable detail a variety of putative examples of state-reduction/relativity conflict taken from the literature, several of them initially presented by Aharonov and Albert in the '80s. They all involve space-like separated state reductions applied to composite, space-like extended systems. An intermediate stage in the analysis addresses the need to pay proper attention "- - - to the manner in which evolving states of spatially extended systems must be transformed." (p.443). Working with a Schroedinger picture (anathema to myself in a Lorentz covariant context[5]). Myrvold reaches an important conclusion with an historical pedigree.

> The instantaneous state of an extended quantum system is, therefore, defined only relative to a spacelike hyperplane (or, more generally, a spacelike hypersurface) of simultaneity. This is not a radically new postulate, but a simple consequence of the notion

> ‘state at a time’ plus the relativity of simultaneity. A state history of an extended system is defined only with respect to a choice of a foliation of spacetime into spacelike hypersurfaces. This was pointed out by Dirac (1933), and was elaborated by Tomonaga (1946) and Schwinger (1948). If one works within the Heisenberg picture, in which it is operators corresponding to fields that evolve rather than states, and concerns oneself with operators corresponding to local observables, then one need not deal with foliation-dependent states (though field quantities defined in terms of spatial integrals of local fields are, on such a picture, foliation relative). The language of foliation-relative state evolution is the translation of this Heisenberg-picture evolution back into the Schroedinger picture.(p.444)

I would protest here that the latter part of this statement neglects the evolution of the *eigenvectors* of observables in the Heisenberg picture, which, for extended systems, renders the complete orthonormal bases of joint eigenvectors HD, thus resulting in HD state *functions* , i.e., probability amplitudes, which are, as always, picture independent. One can also add (as has been discussed in the preceding sections) that in the presence of state reductions even the Heisenberg picture state vectors can differ on any two hyperplanes separated by such reductions. I would finally add that those “field quantities defined in terms of spatial integrals of local fields [which] are - - - foliation relative” are, in my judgement, the best approximations to what we actually measure in our experiments (this will be discussed further in my 2004).

The central conclusions of the paper are contained in the statements:

> - - - that with or without collapse, in a relativistic context the state of an extended system must be defined along a spacelike hypersurface of simultaneity, and that a state history must be defined with respect to a particular foliation of spacetime (p.455). - - - Nature has not been profligate with collapse events. There are not different collapse events corresponding to different foliations; the accounts given with respect to alternate choices of foliation are not descriptions of different events but different descriptions of the same events (p.457). - - - A complete state history given with respect to one foliation uniquely determines the state history with respect to any foliation - -(p.459). - - - reference frames may disagree on which collapse events are chance events and which have predetermined outcomes. This is a consequence of the fact that, though collapse events can be regarded as local events, the probabilities assigned to the various possible outcomes of this local event are calculated with respect to the globally defined state vector, and hence are foliation-relative (p.461). - - - Acceptance of collapse occurring as part of a foliation-relative state evolution entails regarding [our] intuiton, formed, as Maudlin points out, by aquaintance with relativity and *non-relativistic* quantum mechanics, as mistaken (p.461).

Within the context of Myrvold's approximation I am in essential agreement with these conclusions. With the exceptions of (1), the characterization of the collapse events as "local", a notion, as Myrvold uses it, with clear meaning only within his approximation and (2), the restriction of the concept of a *history* to a foliation (see **§ 5**, p. 22), I would go farther and argue for the general validity of the conclusions.

Prior to stipulating his approximation Myrvold distinguishes his position from some of my work by,

> - - - not assum[ing] that each detector is associated with a particular foliation of spacetime. I will also refrain from invoking observables that exhibit irreducible dependence on reference frames or spacelike hyperplanes. - - - A theory, such as Fleming's, that introduces irreducibly extended quantities faces an additional burden of reconciling these with the notion that different reference frames merely yield differing accounts of the same processes and events.(p.437).

While I believe it to be consistent for Myrvold to employ these restrictions within his paper, it follows from the considerations involved in the earlier discussion of **Fig. 3**, that it is only consistent in the context of his specified approximation. Like Maudlin, Myrvold has widely separated quantons on world lines intersecting hyperplanes at points and nothing like a dynamical interaction between them is permitted within his scheme. But if we allow the presence of an EM field, say, in the space-time region pictured in **Fig. 3**, the (charged) quanton within the world tube may very well have a different 4-momentum, or a different spin (precession), on the two hyperplanes, i.e.,

$$P^{\mu}(h_1) \neq P^{\mu}(h_2) \qquad (4.1)$$

and/or

$$S^{\mu}(h_1) \neq S^{\mu}(h_2) \qquad (4.2)$$

where the symbols may refer to Heisenberg picture operators or, if you prefer, to picture independent expectation values. Thus hyperplane dependent observables would be required.

I am also uncertain as to just what Myrvold means by the twice used term, "irreducible" in the preceding quote since, at least in the context of local QFT, all the HD dynamical variables I have considered have possessed their HD by virtue of being integral functionals of local fields over hyperplanes. Those functionals are, to be sure, almost always *incompatible* with the local fields of which they are functionals and so they can not be measured jointly with those fields. Perhaps <u>that</u> is the intended meaning of “irreducible” here. In any case, even when the observables being measured in collapse events are, themselves, HD, that specification is just part of the determination of *what* observable is being measured. It does not modify the *way* in which state assignments are rendered HD in the presence of the collapses. Thus I also fail to see the “additional burden” Myrvold refers to.

On the other hand, if one were to accept that *all physical systems*, without exception, were space-like extended systems, as I have claimed above (pp. 13 - 15), then Myrvold’s path (understanding extended systems by analysis into their localized constituents) to his conclusions is inherently approximate and not generally applicable. Nevertheless, Myrvold’s conclusions (pp.19, 20, and as qualified thereafter) <u>are</u> generally applicable.

There are two minor technical lapses in the paper. Fortunately, neither of them undermine the general argument. In discussing the Lorentz transformation of product states on p.443 Myrvold assumes a product form for the unitary transformation operator (eq.17). This would only be correct if no interaction existed between the subsystems whose states are represented by the factor states in the product. But Myrvold does not make the no interaction assumption until two pages later. The reader will recall similar comments on interaction dependence of boosts made in the context of my responses to Maudlin (p.8). The interaction dependence of Lorentz boosts (Fleming 2003) is often forgotten because, I suspect, Galilean boosts are <u>not</u> interaction dependent. Furthermore, in Myrvold’s case, one expects a <u>passive</u> boost to a new frame of reference not to modify the product character of a state. Nor does it. But in the presence of interactions the factor spaces corresponding to the subsystems *on a given hyperplane* change (in passive transformations) from frame to frame, just as they do in dynamical evolution (active transformations) from hyperplane to hyperplane *within a given frame*.

This lapse plays no havoc with Myrvold’s analysis because his systems do not ‘interact’ with each other except through state reduction and entanglement.

On p.449 Myrvold suggests that what he calls the *relativity of entanglement*, the presence and/or nature of the entanglement of one system with distant other systems varying with hyperplanes intersecting the one system in the ‘same’ space-time region, can occur only as a consequence of state reduction. In fact such variation can also be brought about via pure unitary evolution and Myrvold corrects himself on this point in his (2003)[6].

5. Response to Myrvold (2003) on relativistic quantum becoming

Myrvold’s (2003) is concerned to refute Albert’s (2000) claim that quantum theory is incapable of describing the world as *unfolding in Minkowski space-time.* Against this claim, Myrvold offers a specific construction of a concept of becoming in Minkowski space-time that is based upon an earlier local concept of becoming due to Stein (1991) but which is generalized to accommodate space-like extended systems and is, therefore, foliation relative, i.e., for foliations of hyperplanes, HD. Although I am not, myself, very concerned with whether or not a viable objective concept of becoming can be mounted within LCQT, I find myself sympathetic to Myrvold’s arguments and constructions, (modulo some concerns about an example involving a putative local observable)[7]. However, as a number of my comments above have indicated, I am very concerned to extend the notion of histories and dynamical evolution beyond the confinement within foliations, and it is at least questionable whether an objective concept of becoming can accompany that extension.

On p. 479 there is an important cautionary paragraph arguing a frequently ignored point (see the quotes from Maudlin on pp. 13 and 15):

> “Choosing a foliation is not the same as picking the perspective of some observer. The pedagogy of special relativity, with its tales of observers in trains and on spaceships, can lead the unwary to the impression that every observer is accompanied by a foliation consisting of hyperplanes orthogonal to the observer’s world line, each of which constitutes that observer’s infinitely extended present at some instant, and that accounts given with respect to different foliations correspond to the points of view of different observers. - - - Nor is anyone under any obligation to refer events to his or her own rest frame, and we often do not -------- ”

This warning should forestall any misconstrual of Myrvold’s foliation dependent construction as being observer dependent. Just as well informed observers, whether inertial or otherwise, will not disagree as to the physical state of affairs on any hyperplane, so they will not disagree on what has *become* relative to what.

Myrvold's extension of Stein's conception of becoming is expressed by declaring that of any two space-time regions, α and β, β is definite as of α iff β lies wholly within the causal past of α. Applied to a foliation history, every space-like hypersurface in the foliation is definite as of any 'later' hypersurface within the foliation. For Myrvold this suffices to implement a concept of becoming and, in the presence of quantum non-separability, can not be improved upon by being made more 'local'.

But the definition does employ the restricted concept of evolution, or history, to foliations, consistent with his practice in the earlier article discussed in the preceding section. Outside of foliations some transitions between hyperplanes involve 'half' of a hyperplane evolving into the 'past' while the other 'half' evolves into the 'future'. Nevertheless, I would argue that choosing *any set of hyperplanes parameterized by the real line*, *such that, each point of space-time is included in at least one hyperplane of the set and no hyperplane of the set lies wholly within the union of the pasts of earlier hyperplanes in the set* can be a useful evolutionary path to consider.

I am motivated to assert this addition because I see dynamical evolution (especially unitary dynamical evolution) as a more unified process than Myrvold or Maudlin. Dynamical evolution does not take place only <u>within each</u> of the equivalent possible foliations. It also occurs in the transition from a hyperplane of one foliation to a hyperplane of another foliation, i.e., from one hyperplane to an intersecting hyperplane. There is only one evolution in the HD scheme; that from any hyperplane on which the system of interest exists to any other such hyperplane. This point is very closely related to the interaction dependence (Fleming 2003) of the boost-like Lorentz generators. Unitary dynamical evolution consists of the complex of all possible combinations of <u>active</u> time-like translations and boost-like transformations. An account of the quantum state of affairs on any set of hyperplanes, such as described above in italics, is obviously just as informative as an account restricted to a foliation and may be more useful, even simpler, perhaps, for analysing particular systems, such as, e.g., Unruh's famous uniformly accelerating detector in the vacuum. If one develops the formalism for HD dynamical evolution (Fleming 1966) one finds that reorientation and parallel displacement of hyperplanes enter equivalently. Indeed, in a recent interesting application of the HD formalism (Breuer et al 1998) a natural merging of the two aspects of evolution into one has been achieved.

This is the appropriate place to fulfill the promise made at the end of **§ 3**, and formulate a precise concept of generalized histories (going beyond evolution within foliations) and articulate the relationships of mutual determination that hold between them in the presence of state reductions.

Accordingly, consider any continuous, invertible map from the real line to hyperplanes[8], such that (1) every point of Minkowski space-time occurs on *at least* one of the hyperplanes, (2) for any value, s_1, of the real variable, the hyperplane, $h(s_1)$, includes at least some 3-dimensionally open set of points that lies to the future of all hyperplanes, $h(s)$, with $s < s_1$. The first condition guarantees that all of space-time is covered (at least once) by the parametrized set of hyperplanes. The second condition guarantees that the parameterization preserves a future orientation throughout and never allows "later" hyperplanes to lie wholly in the already covered past. Any such parameterized family of hyperplanes defines a **generalized evolutionary path**. Taken in conjunction with the quantum states of a system corresponding to the hyperplanes of a generalized evolutionary path, this construction defines a **generalized history**.

The correlation between any two such generalized histories is then given by the following:

For any two generalized histories, H and H', if one knows the quantum states for a system on all the hyperplanes of H from the asymptotic past up to $h(s_1)$, then one can determine the quantum states for the system on all the hyperplanes of H' from the asymptotic past up to any hyperplane, $h'(s')$, such that $h'(s')$ lies to the past of all those state reductions that lie to the future of all hyperplanes, $h(s)$, with $s \leq s_1$.

The concept of evolutionary path introduced here allows for the hyperplanes within a history to sweep backwards (via reorientation) through a state reduction process as the real variable (playing the role of time) moves forwards. If such a state reduction is implemented by a projection operator then there is no possibility of calculating the precursor state from the state produced by the reduction. Thus for calculational purposes it would be desirable to have all state reductions implemented by invertible effect operators which can, after all, approximate projection operators arbitrarily closely. Nevertheless, with projectors or otherwise, there will always exist 'earlier' values of the real variable at which the reduction process in question is being swept over in the future direction and the precursor state is accessible in the normal manner.

Does any vestige of Myrvold's concept of becoming survive the extension to generalized histories? Well, at least this much: every hyperplane in a generalized history contains some space-time region that is not definite as of all 'earlier' hyperplanes in the history but which is definite as of some 'later' hyperplane and is 'permanently' definite as of some, possibly other, 'later' hyperplane in the history. Whether this can satisfy the champions of *becoming*, I do not know.

On p. 482 Myrvold is discussing the relevance of *local operations* to an account of the world unfolding in Minkowski space-time. He assumes the standard connection between *local operations* and the local observables of algebraic QFT. But then, Myrvold selects, as an example of his general discussion, the spin observables of a separated pair of spin ½ quantons in a singlet state. But *spin is not a local observable*! Not the spin of a quanton, nor the spin, understood as the internal angular momentum, of any system. It matters not how localized the system may be in whatever localization scheme one adopts (Localization schemes will also be discussed in my 2004). To see this, consider a single free quanton with 4-momentum observable, P_μ. Let O be a region of space-time within which one hopes to find the spin observable, let's call it $S^\mu(O)$, for the quanton, where some linear constraint allows only three independent components[9]. Suppose the displacement of the region O by the displacement vector, a^μ, is $O(a)$. Then, $O = O(0)$, and $S^\mu(O(a))$ would be a suitable spin operator for the correspondingly displaced quanton state. Now the 4-momentum, P_μ, is also the generator of displacements. So we must have,

$$[S^\nu(O(a)), P_\mu] = i\hbar\, \partial S^\nu(O(a))/\partial a^\mu \qquad (5.1)$$

But for a free quanton, spin is conserved and commutes with momentum. So our local $S^\mu(O)$ cannot be the spin. Nor can the matter be saved by setting, $\partial S^\nu(O(a))/\partial a^\mu\,|_{a=0} = 0$, since that would lead, from (5.1), to, $\partial S^\nu(O(a))/\partial a^\mu = 0$, for arbitrary a, and thence to, $S^\nu(O) = S^\nu(O(a))$, for arbitrary a. But then, for sufficiently large, space-like, a, we would obtain, from microcausality, $[S^\mu(O), S^\nu(O)] = 0$, and again our local observable is not the spin. If one argues that we should use a local observable for P_μ as well, one can show that for any *O'* that contains O within one of *O'*s open subsets, we must again have,

$$[S^{\nu}(O(a)), P_{\mu}(O')] = i\hbar\, \partial S^{\nu}(O(a))/\partial a^{\mu}\ , \qquad (5.2)$$

for the open set of a's such that *O*(a) lies within *O'*. With slight modification, the preceding argument again goes through.

The immediately preceding considerations occurred in Myrvold's **§ 3** where he is concerned with purely unitary evolution. The remainder of that section comprises a very clear set of arguments against Albert's claim of the *metaphysical incompatibility* of quantum theory and relativity and on behalf of the relativity of entanglement. As much of that discussion depends critically upon considering *curvilinear* hypersurfaces within foliation histories, it lies outside my primary concerns. I will only comment that if Albert defines special relativity tightly enough he can maintain his claim of metaphysical incompatibility. In that case Myrvold's position could be summed up by saying that quantum theory is not incompatible, metaphysically or otherwise, with Lorentz covariance. And Lorentz covariance is the only part of special relativity that counts in quantum theory. By all means Lorentz covariance should be subject to empirical test as Albert calls for and Myrvold embraces. But if the tests are passed, it is only Lorentz covariance that would be supported and not the classical accretions that comprise the rest of special relativity.

Myrvold's **§ 4** addresses the compatibility issue in the presence of state reduction. While I doubt the direct relevance of local observables to that which we actually measure (The observable, Ω, and it's spectral projectors, P^{Ω}_{k}, which Myrvold considers, would, by Reeh-Schlieder, have non-vanishing expectation values in *every* state of bounded energy spectrum, including the vacuum, as Myrvold is aware. For more on this, see my 2004), within the context of that presumption, Myrvold's discussion is illuminating and insightful.

The rest of the paper defends hypersurface dependent objective chance within the context of the usual eigenvalue-eigenvector link and the need for hypersurface dependence of position representation state functions. Needless to say, I am in agreement with all of this. We differ only in my exclusive focus on hyperPLANES, my suspicions about local 'observables' and my convictions on the non-approximate nature of position representation HD state functions, even for space-like extended quantons within QFT. All these matters are addressed at greater length in my 2004.

6. Conclusions and Reflections on Space-time ontology

In this paper I have, in **§ 2**, presented arguments and explanations which I believe should remove Maudlin's (1996) concerns about the correlations between quantum state assignments on intersecting hyperplanes due to state reductions. Those correlations are a consequence of the unitary, boost-like evolution between intersecting hyperplanes not separated by state reductions and the hyperplane independent operator action that produces any given state reduction. The unitary evolution is driven by the active application of the interaction dependent Lorentz boost-like generators in close analogy to the unitary time-like evolution between parallel hyperplanes, also not separated by state reductions, driven by the active application of the interaction dependent time-like generators (Hamiltonians). The general consideration involved is stated with emphasis on p. 10.

In **§ 3** I have addressed Maudlin's (1998) concern with the seeming incompatibility of overlapping intersections of quanton world tubes and distinct hyperplanes supporting distinct properties of the quanton and even distinct modes of the properties arising (as regards deterministically or indeterministically). I have argued that these matters can be understood when sufficient regard is taken of the space-like dimensionality of the intersections and of the unity of evolution between hyperplanes, whether intersecting or parallel, even in the presence of state reductions. The additional general considerations involved are stated with emphasis on pp. 15, 17.

In **§ 4** I largely accept Myrvold's (2002) analysis of HD for extended systems with widely separated constituents. Believing that all quantum systems are space-like extended, I champion the generality of Myrvold's central conclusions beyond his context while recognizing the need to modify his arguments towards those conclusions outside his context. I criticize his account of the relationship between the Heisenberg and Schroedinger Pictures and his comments on the non-introduction of HD dynamical variables. I argue, on the same dimensional grounds raised in **§ 3**, that, had he permitted dynamical interactions between the constituents of his systems, he would have required HD dynamical variables himself.

In **§ 5** I comment on Myrvold's 2003 and consider whether his notion of foliation dependent becoming can survive my extension of the concept of dynamical evolution and histories outside of foliations. The definition of generalized histories and the statement of their correlation is given on p. 24. I criticize his example of spin as a local observable and express my aversion to his reliance on local observables for his analyses.

6a: Reflections on the ontology of Minkowski space-time

In a section of the sequel to this paper I argue (criticizing an argument of Halvorson and Clifton 2003 for lack of consistency but, nevertheless, embracing and extending their conclusions) that no quanton, object, system, apparatus or measurement process can be *strictly* confined within any bounded region of any hyperplane on which it exists. If this is correct, then

all physical systems, not just traditionally composite ones, are space-like extended systems to which the conclusions discussed here for extended systems apply.

Such systems have their physical states and their dynamical evolution, unitary or non-unitary, associated with space-like hyperplanes or families of hyperplanes (and, presumably, in curved space-times, curvilinear space-like hypersurfaces) and not with points of space-time or even (strictly speaking) bounded regions of space-time. It is important to realize that by attributing space-like extension to **all** physical systems, the traditional analysis and reduction of extended systems in terms of their microscopic constituents is no longer feasible in principle; there are, strictly speaking, no microscopic constituents. This recognition does not conflict with the FAPP success of analysis of 'large' systems and processes into 'small' constituents or even 'pointlike' constituents. That FAPP success simply ignores the comparatively ephemeral space-like tails that all systems and processes possess.

This ubiquity of strictly unbounded space-like extension for all physical entities and processes, entails that the observables of physical entities are predominately strictly assignable only to whole hyperplanes and only FAPP assignable to bounded regions, let alone individual points. Even when the eigenvalues of the observables identify single points (as in the case of the

NW position operators), the points are very much *points on hyperplanes*, and not at all points of Minkowski space-time divorced from the hyperplane to which the diagonalized observable belongs.

The glaring exception to this characterization, it would seem, is provided by the local fields of QFT. But for these quantities, bounded space-time regions and space-time points are not the identified results of localizing efforts but, instead, are the *a priori* labels for the assignment of the putative observables of space-time itself; the energy, momentum, stress, charge and current densities and the bosonic force fields. Nevertheless, while some HD observables of general systems <u>can</u> be defined independently of the local fields, the claim for relatively fundamental status of the fields is enhanced by the fact that the HD observables are all expressable as functionals, over the hyperplanes, of the fields. But the physical identification of the significance of the space-time labels carried by the fields requires returning to the measurement of the HD observables of non-field-like physical systems, thereby generating an anti-fundamentalist vicious circle that is only FAPP defused by the macroscopic, semi-classical nature of the systems in question.

When the foregoing considerations are coupled with the holistic HD of state vector assignments for extended systems in the presence of state reduction (Heisenberg Picture) that we have discussed in this paper (a feature not only retained in the proposed dynamical theories of state reduction, but even in Decoherence Theory which, ultimately, treats state reduction as an effective illusion), the status, within Lorentz covariant quantum theory, of space-like hyperplanes (and the points they contain) seems to rival that of Minkowski Space-time itself (and the points it contains).

If local QFT could still be regarded as the fundamental underpinning of our theories in the Lorentz covariant quantum domain then, notwithstanding the fact that extended system observables usually do not commute with local observables, one could still make a case that the HD account required to deal with the physics of real systems is essentially derivative upon the underlying dynamical structure of local fields over Minkowski space-time.

But local QFT is no longer regarded as capable of providing a fundamental account. As mentioned in **§ 1**, the present standpoint, designated by the label, Effective Quantum Field Theory (EQFT)(see Weinberg 1995, pp. 499, 523-5; 1996, **19.4-8**), treats the QFT formalism as providing an adjustable scheme for the phenomenological analysis of the microscopic interactions of systems of quantons, at least insofar as those interactions can be manifested in scattering experiments. In particular, the parameters of the scheme may be adjusted to fit the energy scale of the experiments one intends to perform and the old constraint of renormalizability is no longer available to limit the putative couplings one may invoke for the data fitting. The pro-field theoretic, anti-quanton standpoint of workers in QFT in curved space-time of not so long ago now rings a bit hollow in the face of the present prosaic role of Lorentz covariant QFT.

As I said in **§ 1a** this demotion of the status of local QFT does not elevate the status of the quantons that QFT is used to describe. It simply deprives them of as ‘fundamental’ an account in terms of local fields as we once believed they enjoyed. At present the best prospect for a ‘fundamental’ account is offered by Superstring Theory, or, perhaps, M-Theory, whatever that might turn out to be. But the jury is still out on that.

Along with the advent of Superstring Theory (see Polchinski 1998), in which the basic entities are 1 or higher dimensional extended objects, and current research in Quantum Gravity (see Rovelli 1998 and Smolin 2003), which seems to be establishing the discrete quantization of volumes and surface areas, one may well be suspicious of the future status of the humble space-time point in physical theory. Of course, Superstrings and Quantum Gravity come into their own at the various Planck scales and one can question the wisdom of discussing their development as co-symptomatic with the emergence of the EQFT stance, which is applicable at vastly larger distance scales and lower energy scales.

Nevertheless, I find myself emboldened by this matrix of developments (including the work contributing to the entire discussion of this paper and my 2004) to suggest that perhaps, already at the level of LCQT, we may more accurately characterize the situation of our systems of interest as one of existing in a <u>seven dimensional world</u> of *points-on-hyperplanes* rather than a <u>four dimensional world</u> of *points-in-Minkowski space-time*. Thus, the point, p, with Minkowski coordinates, x^μ, on the hyperplane with normal vector, η^μ, (all relative to an inertial coordinate system, F), i.e.,

$$p \sim (\eta, x)_F, \qquad (6.1)$$

is *physically not the same point* as the point, p', with the same Minkowski coordinates in F, x^μ, but on a different hyperplane with normal vector, η'^μ, $p' \sim (x, \eta')_F$. Within such a conception the points of Minkowski space-time, M, are to be understood as equivalence classes of hyperplane points . Thus,

$$(p_M \in M) \Leftrightarrow$$

$$(p_M := \{p | \exists\, h \rightarrow p \in h \wedge (\forall F)(\exists x^\mu{}_F)(x^\mu(p)_F = x^\mu{}_F)\}\,)^{10}. \qquad (6.2)$$

A question provoked by this conjectured demotion of the status of the space-time points is why, in a seven dimensional world, a larger symmetry group than the inhomogeneous Lorentz group is not operative? Of course, perhaps such a group is operative but simply not yet recognized. On the other hand, the seven dimensional manifold in question, parameterized in a Minkowski-like coordinate system (the F in (6.2)) as the set of all ordered pairs of the form, (η, x) [with dimensionless 4-component, η^μ, satisfying, $\eta_\mu\eta^\mu = 1$, and $\eta^0 > 0$, and 4-component x^μ having the dimension of distance], is a *homogeneous space* of the (orthocronous) inhomogeneous Lorentz group, i.e., any point of the manifold can be reached from any other point by some transformation, (Λ, a), of the group, where,

$$(\Lambda, a)(\eta, x) = (\Lambda\eta, \Lambda x + a). \qquad (6.3)$$

In that sense, at least, there is no need for a larger symmetry group.

Alternatively, we may abandon points altogether as inputs and simply construct them as equivalence classes of 'intersecting' hyperplanes.

$$(p \in M) \Leftrightarrow (p := \{h|\ (\forall F)(\exists x^\mu{}_F)(\eta_\mu(h)_F\, x^\mu{}_F = \tau(h)_F)\}\,)^{11}\,. \qquad (6.4)$$

In such a scheme, while all points are derivative from hyperplanes, the Minkowski space-time points are identical with the points 'on' hyperplanes. This scheme employs only the 4-dimensional set of hyperplanes for the basic input, parameterized in a Minkowski-like coordinate system (the F in (6.4)) by the ordered pair, (η, τ), and the obvious symmetry group is, once again, the (orthocronous) inhomogeneous Lorentz group, where,

$$(\Lambda, a)(\eta, \tau) = (\Lambda\eta, \tau + a\Lambda\eta). \qquad (6.5)$$

However, as we must ultimately admit dynamical variables assigned to ordered pairs of an equivalence class of hyperplanes (a point) and one hyperplane from the class, e.g., HD fields such as the NW creation and annihilation fields (Fleming 2000, 2004), the effective seven dimensional world of the first scheme is not far away. In any case both schemes treat the points of Minkowski space-time as equivalence classes of (dare I say it) relatively more 'fundamental' entities, either points-on-hyperplanes or hyperplanes, themselves. It is conjectured that it is those latter entities that exist as spatio-temporal supports for more 'fundamental' properties and/or dynamical variables than those associated with Minkowski space-time points.

Further examination of these ideas will be pursued in the last section of the sequel to this paper (Fleming 2004).

Notes:

1.(from p. 4) To firm up the definition, let's reserve **quanton** for any particle-like quanta of a quantized field or any bound state of such quanta due to interactions which have, themselves, no macroscopic classical field manifestation, i.e., interactions other than electromagnetism or gravitation. This would include hadrons and nuclei among quantons and would exclude atoms, molecules, planets and stars.

2.(from p. 9) While not incorrect, this statement ignores the great variety of ways to describe spin and the varying forms of HD those different descriptions display in LCQT (Fleming 1965a, b).

3.(from p. 9) Or an effect operator if the state reduction is not exactly replicable.

4.(from p. 15) The Earth's spinning and orbital motions around a drifting Sun guarantee that the spatio-temporal wholes of Socrates different ages are not quite parallel hyperplanes.

5.(from p. 18) In the Heisenberg picture unitary evolution and state reducing evolution are cleanly separated between operators and states, respectively, and manifestly covariant notation is much simpler to implement.

6.(from p. 22) An instance of Myrvold's *relativity of entanglement* induced by unitary evolution is described in my (1996), pp 17-19.

7.(from p. 22) The relevance of local observables for describing actions we perform or quantities we measure is discussed at length in my forthcoming (2004).

8.(from p. 24) In any Minkowski coordinate system, the parameters, (η, τ), identifying the hyperplanes are continuous functions of the real variable.

9.(from p. 25) If $S^{\mu}(O)$ is the Pauli-Lubyanski spin operator the constraint is $P_{\mu}S^{\mu}(O) = 0$. If $S^{\mu}(O)$ is the HD generalization of non-relativistic spin, the constraint is $\eta_{\mu}S^{\mu}(O) = 0$, where η_{μ} is the unit, time-like, normal 4-vector for the chosen hyperplane.

10.(from p. 31) Here each coordinate system, F, assigns an ordered pair, (η, x), to each hyperplane point, p. See following paragraph.

11.(from p. 31) Here each coordinate system, F, assigns an ordered pair, (η, τ), to each hyperplane, h. See following paragraph.

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