Cosmological measures without volume weighting

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Abstract. Many cosmologists (myself included) have advocated volume weighting for the cosmological measure problem, weighting spatial hypersurfaces by their volume. However, this often leads to the Boltzmann brain problem, that almost all observations would be by momentary Boltzmann brains that arise very briefly as quantum fluctuations in the late universe when it has expanded to a huge size, so that our observations (too ordered for Boltzmann brains) would be highly atypical and unlikely. Here it is suggested that volume weighting may be a mistake. Volume averaging is advocated as an alternative. One consequence may be a loss of the argument that eternal inflation gives a nonzero probability that our universe now has infinite volume.

Keywords: string theory and cosmology, inflation, gravity

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1. Introduction

One of the most serious problems of theoretical cosmology today is the measure problem, the problem of how to make statistical predictions for observations in a universe that may be so large that almost all theoretically possible observations actually occur somewhere. One would like to be able to calculate what fraction each possible class of observations makes out of all possible observations. However, this is problematic if the universe is infinitely large and if there are infinitely many observations of each class. Then one has the ambiguities of taking the ratios of infinite quantities.

This ambiguity of dividing infinity by infinity occurs not only for open universes that automatically have infinite volume (and hence presumably infinitely many observations, assuming a nonzero average density of observers), but also for closed universes that have eternal inflation to make them arbitrarily large. A huge effort [1]–[47] has gone into proposing procedures for making well-defined ratios of the resulting infinities of different classes of observations.

One particular challenge has come from a consideration of Boltzmann brains [48]–[74], which are putative observers that can apparently form from thermal and/or vacuum fluctuations arbitrarily late in a universe that lasts forever. If the spacetime (perhaps after being regularized to just a finite comoving volume if the actual total spatial volume is infinite) has only a finite four-volume where ordinary observers like us can live (e.g., during the finite lifetime of stars), but if it lasts infinitely long and if Boltzmann brains can form at a nonzero (even if extremely small) rate per four-volume, then it
seems there will be an infinite number of Boltzmann brains in comparison with us ordinary observers. Making the plausible assumption that only a very tiny fraction of Boltzmann brain observations are similar to our ordered observations (so that we can statistically exclude the hypothesis that we are Boltzmann brains), then since Boltzmann brains would infinitely dominate over ordinary observers, observations like ours would form only a very tiny fraction of the whole and so would be extremely unlikely.

Thus Boltzmann brains appear to be a reductio ad absurdum for present cosmological theories that allow ordinary observers in only an infinitesimally tiny fraction of spacetime and that allow infinitely more Boltzmann brains to form from fluctuations throughout a spacetime that lasts forever. It is not that cosmologists have lost their brains in considering Boltzmann brains [75], since few of us believe that Boltzmann brains really infinitely dominate over ordinary observers. Instead, we realize that Boltzmann brains form a paradox rather analogous to the ultraviolet catastrophe that plagued the classical thermodynamics of radiation before Planck’s introduction of the quantum that cured that problem. Now we need a suitable solution to the measure problem to cure the putative catastrophe of Boltzmann brains.

Since Boltzmann brains are expected to form at only extremely tiny rates, such as perhaps $\sim e^{-10^{42}}$ from vacuum fluctuations of a 1 kg brain for a time equal to the light travel time across it [55], one way to avoid the problem is for the universe to have a finite lifetime [53, 54, 57, 59, 71]. With a putative decaying universe, the problem might be solved (so long as what the universe decays into does not itself have too many Boltzmann brains). However, this puts severe restrictions on the decay rate.

The tightest restriction would come if the universe asymptotically expands exponentially, as would be the prediction from the currently observed cosmic acceleration if the dark energy driving the expansion has constant energy density (e.g., a cosmological constant, or the bottom of a potential well for a scalar field). If one imagined that such an asymptotically de Sitter universe decays deterministically at a fixed time, it would need to decay within about $10^{52}$ years, before the total number of Boltzmann brains per comoving volume would dominate over ordinary observers. However, if the universe decayed quantum mechanically with an uncertain time of the actual decay, one would need the expected decay time to be less than about 20 billion years [53, 59, 71] in order that the expectation value of the four-volume per comoving volume not be infinite and lead to an infinite domination by Boltzmann brains. Although such an astronomically rapid decay is not directly ruled out observationally, it would apparently require unnaturally fine tuning [53, 59, 71].

There are by now quite a number of other proposed solutions to the Boltzmann brain paradox [48]–[74], but all of them, including my own suggestions, seem to me rather unnatural. The main problem seems to arise from the rather plausible deduction that there should be an infinite number of Boltzmann brains that arise in any spacetime that lasts forever, especially if it also expands by an unbounded amount.

Here I propose that at least a major part of the Boltzmann brain problem could be averted if one abandoned the volume weighting that seems to imply that there is more weight for Boltzmann brain observations for spaces of larger volume. I suggest replacing volume weighting by the assumption that one should average over the volume of space, so that two spatial hypersurfaces with the same density of each class of observations, but with
different spatial volumes, would give the same weights for these classes of observations (if the two hypersurfaces have the same quantum amplitudes).

One would still need to sum or average over all spatial hypersurfaces in the quantum state the result that one gets for the volume-averaged weights for each class of observation on each spatial hypersurface. It is less clear to me what is the most natural way to do this sum or average over different hypersurfaces, so that there is still a potential Boltzmann brain problem from a sum or integral over an infinite number of different hypersurfaces, such as in a universe that lasts forever. This problem might be averted if the universe does have a finite lifetime [24,74], which with volume averaging rather than volume weighting might be very long and not requiring fine tuning, though there do remain potential problems with this that I shall discuss below. Another idea that I shall pursue below is in a classical approximation to restrict to closed spatial hypersurfaces of constant trace of the extrinsic curvature (i.e., three times the direction-averaged Hubble ‘constant’ $H$), and then to do the integral over $dH$.

Volume weighting has been the basis for the hypothesis that slow-roll eternal inflation [11], [76]–[78] makes the universe infinitely large (at least with some nonzero probability [79]), that not only did the universe undergo an early period of rapid quasi-exponential expansion, but also that quantum fluctuations produce an unboundedly large amount of inflation by today. Therefore, abandoning volume weighting in favor of volume averaging would apparently remove the argument for a nonzero probability for eternal inflation to give infinite volume at the time of observers. It would suggest that although the universe may have inflated by a very large amount, it would not have inflated by an infinite amount. As a result, if space is compact, it may well have a bounded volume with unit probability. Such a finite universe can more easily avoid the measure problem ambiguities of taking the ratio of infinite quantities that occur in infinite universes arising from eternal inflation.

2. The measure problem in quantum cosmology

A goal of quantum cosmology is to come up with one or more theories $T_i$ that predict the probabilities of observations, by which I mean probabilities for the results of observations. Here for simplicity I shall assume that there is a countable set of possible distinct observations $O_j$ out of some exhaustive set of all such observations. This set of possible observations might be all possible conscious perceptions [19, 80], all possible data sets for one observer, or all possible data sets for a human scientific information gathering and utilizing system [63]. (However, one should not mix different types of observations that are not distinct from each other, such as the data set for one person and the data set for all persons in some civilization, since they are not distinct observations; one can give rise to the other.) If one imagines a continuum for the set of observations (which seems to be logically possible, though not required), in that case I shall assume that they are binned into a countable number of exclusive and exhaustive subsets that each may be considered to form one distinct observation $O_j$. Then the goal is to calculate the probability $P(O_j|T_i)$ for the observation $O_j$, given the theory $T_i$.

As noted in [73], the mutually exclusive and exhaustive (complete) set of possible observations and the probabilities assigned to them by the theories should obey the following principles.
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(1) **Prior alternatives principle** (PAP): The set of alternatives to be assigned likelihoods by theories $T_i$ should be chosen prior to (or independent of) the observation $O_j$ to be used to test the theories.

(2) **Principle of observational discrimination** (POD): Each possible complete observation should uniquely distinguish one element from the set of alternatives.

(3) **Normalization principle** (NP): The sum of the likelihoods each theory assigns to all of the alternatives in the chosen set should be unity,

$$\sum_j P(O_j | T_i) = 1.$$  

One common strategy in cosmology is to calculate an unnormalized probability for each possible observation within a fixed finite region and then try to form a normalized sum of these over all regions of spacetime. The problem arises when there are an infinite number of regions and the sum of the unnormalized probabilities for each region diverges when being added over all regions. Then it is not clear how to normalize the divergent sum to get unambiguous finite ratios of the total probabilities for different possible observations. A lot of clever effort has gone into schemes for regularizing the total probabilities [1]–[47] but is seems fair to say that most of these schemes look rather *ad hoc* and unnatural.

One might think that once one has a quantum state for the universe, there would be a standard quantum answer to the question of the probabilities for the various possible observations. For example [73], one might take standard quantum theory to give the probability $P(O_j | T_i)$ of the observation as the expectation value, in the quantum state given by the theory $T_i$, of a projection operator $P_j$ onto the observational result $O_j$. That is, one might take

$$P(O_j | T_i) = \langle P_j \rangle_i,$$  

where $\langle \rangle_i$ denotes the quantum expectation value of whatever operator is inside the angular brackets in the quantum state $i$ given by the theory $T_i$ (which I am taking not only to give the dynamics, e.g. the Hamiltonian, or more generally the constraint equations of quantum gravity, but also to give the boundary conditions, e.g., the quantum state itself in the Heisenberg representation). This standard approach works in the case of a single laboratory setting where the projection operators onto different observational results are orthogonal, $P_j P_k = \delta_{jk} P_j$ (no sum over repeated indices).

However [73, 81], in the case of a sufficiently large universe, one may have observation $O_j$ occurring ‘here’ and observation $O_k$ occurring ‘there’ in a compatible way, so that $P_j$ and $P_k$ are not orthogonal. Then the standard quantum probabilities given by equation (2.2) will generically not obey the Normalization Principle equation (2.1). Thus one needs a different formula for normalizable probabilities of a mutually exclusive and exhaustive set of possible observations, when distinct observations within the complete set cannot be described by orthogonal projection operators.

The simplest class of modifications of equation (2.2) would seem to be to replace the projection operators $P_j$ with some other operators $Q_j$ normalized so that $\sum_j \langle Q_j \rangle_i = 1$, giving

$$P(O_j | T_i) = \langle Q_j \rangle_i.$$  

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Of course, one also wants \[ P(O_j|T_i) \geq 0 \] for each \( i \) and \( j \), so one needs to impose the requirement that the expectation value of each operator \( Q_j \) in each theory \( T_i \) is nonnegative. If one wanted the \( Q_j \)'s to be independent of the theory \( T_i \) and wished to allow different \( T_i \)'s to give all possible quantum states, then it would be natural to restrict the \( Q_j \)'s to positive operators. This is what I have usually assumed, but it would be well to remember that it is just \( \langle Q_j \rangle_i \) that needs to be nonnegative for each \( i \) and \( j \), and that in principle the \( Q_j \)'s themselves can depend on the theory \( T_i \).

The main point [73, 81] is that in cosmology one cannot simply use the expectation values of projection operators as the probabilities of observations, so that each theory must assign a set of operators \( Q_j \), corresponding to the set of possible observations \( O_j \), whose expectation values are used instead as the probabilities of the observations. Since these operators are not given directly by the formalism of standard quantum theory, they must be added to that formalism by each particular complete theory. In other words, a complete theory \( T_i \) cannot be given merely by the dynamical equations and initial conditions (the quantum state), but it also requires the set of operators \( Q_j \) whose expectation values are the probabilities of the observations \( O_j \) in the complete set of possible observations. The measure is not given purely by the quantum state but has its own logical independence in a complete theory.

In quantum terms, the measure problem is the problem (say for giving a complete formulation of a theory \( T_i \)) of choosing the operators \( Q_j \) whose quantum expectation values are nonnegative and normalized in the quantum state that is also to be given by the theory \( T_i \). The greatest challenge in a theory giving an infinite universe (or a superposition of finite universes for which the expectation value of the size is infinite) is the normalization.

For example, assuming a classical spacetime as a suitable approximation, one might hypothetically partition the spacetime into a countable set of disjoint regions labeled by the index \( K \), with each region sufficiently small that for each \( K \) separately there is a set of orthogonal projection operators \( P^K_j \) whose expectation values give good approximations to the probabilities that the observations \( O_j \) occur within the region \( K \). (However, I am assuming that each observation \( O_j \) can in principle occur within any of the regions, so that the content of the observation is not sufficient to distinguish what \( K \) is; the observation does not determine where one is in the spacetime. One might imagine that an observation determines as much as it is possible to know about some local region of spacetime, or even about the entire causal past of a local region, but it does not determine the properties of the spacelike separated region outside, which might go into the specification of the index \( K \) that is only known to a hypothetical superobserver that makes the partition.)

Now one might propose that one construct the projection operator

\[
P_j = I - \prod_K (I - P^K_j)
\]

and use it in equation (2.2) to get a putative probability of the observation \( O_j \) in the quantum state given by the theory \( T_i \). (For simplicity I assume that the \( P^K_j \)'s for different values of \( K \) all commute, so that \( P_j \) is indeed a projection operator.) Indeed, this is essentially in quantum language [73] what Hartle and Srednicki [63] propose, that the probability of an observation is the probability that it occurs at least somewhere. However, because the different \( P_j \)'s defined this way are not orthogonal, the resulting
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standard quantum probabilities given by equation (2.2) will not be normalized to obey equation (2.1). This lack of normalization is a consequence of the fact that even though it is assumed that two different observations $O_j$ and $O_k$ (with $j \neq k$) cannot both occur within the same region $K$, one can have $O_j$ occurring within one region and $O_k$ occurring within another region. Therefore, the existence of the observation $O_j$ at least somewhere is not incompatible with the existence of the distinct observation $O_k$ somewhere else, so the sum of the existence probabilities is not constrained to being unity.

If one were the hypothetical superobserver who has access to what is going on in all the regions, one could make up a mutually exclusive and exhaustive set of joint observations occurring within all of the regions. However, for us observers who are confined to just part of the universe, the probabilities that such a superobserver might deduce for the various combinations of joint observations are inaccessible for us to test or to use to predict what we might be expected to see in the future. Instead, we would like probabilities for the observations we ourselves can make. I am assuming that each $O_j$ is an observational result that in principle we could have, but that we do not have access to knowing which region $K$ we are in. (The only properties of $K$ that we can know are its local properties that are known in the observation $O_j$ itself, but that is not sufficient to determine $K$, which might be determined by properties of the spacetime beyond our local knowledge.)

A better proposal [73], though only one out of a large number of logically possible alternatives [81], would be to form the sum

$$R_j = \sum_K P^K_j$$

(2.5)

do the projection operators $P^K_j$ over all regions $K$ but for the same observation $j$, and then to normalize this in the theory $T_i$ to construct the positive operator

$$Q_j = \frac{R_j}{\sum_k \langle R_k \rangle_i}$$

(2.6)

to be used in equation (2.3) to give the normalized probability of the observation $O_j$ in the theory $T_i$ as

$$P(O_j | T_i) = \langle Q_j \rangle_i = \frac{\langle \sum_K P^K_j \rangle_i}{\sum_k \langle \sum_K P^K_k \rangle_i}.$$  

(2.7)

This procedure appears to be the quantum language for what is usually done more informally by adding up, over all regions, the unnormalized probabilities for each region and then normalizing the result. If one makes the approximation of a uniform density of observations per volume, this procedure gives volume weighting. Then if one has a quantum state with different amplitudes for different volumes (e.g., from stochastic inflation [11], [76]–[78]), the amplitudes for greater volumes (and hence a greater number of observers) would be more greatly weighted. Taking this to its logical conclusion [11], [76]–[78], this leads to infinite volume in eternal inflation, in which the probabilities for observations are dominated by the components of the quantum state in which the universe has undergone an unbounded amount of inflation.

Once one has a nonzero probability for the volume to diverge at the time of observation or at reheating, as one does with volume weighting in eternal inflation even for closed universes [79], one has the problem of how to produce well-defined normalized
probabilities, say by finding well-defined normalized operators $Q_j$ if one uses quantum language. This is a manifestation of the measure problem. Furthermore, there is the danger that the result might be dominated by Boltzmann brains, making our actual observations extremely unlikely in theories predicting such domination. Therefore, one is led to question the procedure of forming the operators $Q_j$ by summing over spacetime regions and then attempting to normalize the result.

3. Replacing volume weighting with volume averaging

Volume weighting has been advocated by proponents of eternal inflation [11], [76]–[78]. I have advocated it myself [82] to try to save the Hartle–Hawking no-boundary proposal from the fact that it gives the highest bare amplitudes for universes with a small amount of inflation. It has been invoked in a similar way more recently by Hawking [40] and by Hawking et al [42]. However, as we have seen, it gives the problem of infinite total unnormalized probabilities and the ambiguities of how to normalize the probabilities, as well as the danger of domination by Boltzmann brains.

To help avoid these problems from domination by arbitrarily large volumes, I propose replacing volume weighting by volume averaging. That is, when calculating the expectation value of $Q_j$ over the entire quantum state that is a superposition of different spatial geometries and matter fields, weight the absolute square of the amplitude for that space and matter configuration, not by the volume integral of the density of the occurrence of the observation $O_j$, but rather by the volume-averaged value of the density. Then, for spatial geometries and matter field configurations that have the same average density of the observation $O_j$, the weights contributing to $P(O_j|T_i)$ are just the squared amplitudes in the quantum state for the different spatial configurations, without any volume weighting.

This is the main new idea I would like to propose at present. It is somewhat ad hoc in that it cannot be derived from any basic underlying quantum theory [81]. However, at the level of what measure to place on observations occurring on a single hypersurface, volume averaging seems to be the simplest alternative to volume weighting. Therefore, if volume weighting leads to trouble, volume averaging might appear to be an attractive alternative.

Unfortunately, we are not done yet with the problem. To investigate the consequences of volume averaging, one needs to say how one will add up the measures from the various possible hypersurfaces. For this part of the problem, all the ideas I have come up with so far seem quite ad hoc, with many conceivable alternatives and with many problems for each idea. Therefore, I certainly cannot claim to have a complete natural solution to the measure problem. However, in the hope that volume averaging might be part of an ultimate solution when the problem of how to add the measures from different hypersurface is solved, let me make some initial attempts to say how to do that addition.

In canonical quantum gravity, one would ultimately like to do an average over all diffeomorphisms. However, since the group of all diffeomorphisms is an enormous infinite-dimensional set, I shall leave it as a future problem how to implement this averaging over all diffeomorphisms to give the probabilities of observations. Here I shall merely seek an implementation for the approximation that applies when the quantum state is considered to give an ensemble of classical spacetimes with certain properties. (However, there is no guarantee that the quantum state will give an ensemble of classical spacetimes, just as the
quantum state of a hydrogen atom need not give an ensemble of classical electron orbits, so ultimately we are likely to need a more quantum implementation.)

4. Hypersurfaces of constant mean extrinsic curvature $H$

For each classical spacetime in an ensemble, rather than looking at all possible hypersurfaces embedded in the spacetime, one might restrict attention to a certain subset of these. For example, one might look at a set of hypersurfaces that foliate the spacetime, so that each spacetime event lies on one and only only of these foliating hypersurfaces.

For globally hyperbolic spacetimes with compact Cauchy surfaces, York \[83,84\] has shown that the equations of general relativity simplify if one uses a foliation by hypersurfaces of constant mean extrinsic curvature, which one might call the direction-averaged Hubble ‘constant’ $H$. For three-dimensional spatial hypersurfaces, this is one-third the trace of the extrinsic curvature, which itself is the logarithmic expansion rate of the volume of a comoving region generated by comoving worldlines orthogonal to the sequence of hypersurfaces. Here I shall call these hypersurfaces of constant mean extrinsic curvature ‘constant-$H$ hypersurfaces’. Each of these hypersurfaces has the advantage of being determined just by the spacetime geometry in the neighborhood of the hypersurface (unlike, say, hypersurfaces orthogonal to a congruence of timelike geodesics, which requires that the congruence be specified throughout the spacetime and which also leads to problems at caustics of the congruence).

A lot of effort \[83\], \[85\]–\[96\] has gone into the analysis of the existence and uniqueness of a foliation by constant-$H$ hypersurfaces under the assumption of the timelike convergence condition \[97\] $R_{ab}u^au^b \geq 0$ for each timelike vector $u^a$ at each point of spacetime. This energy condition, which is equivalent to the strong energy condition for matter when the Einstein equations hold with zero cosmological constant \[97\], was thought to be ‘physically reasonable’ \[97\] in the days before the popularity of inflation and the observation of cosmic acceleration, both of which violate the timelike convergence condition.

For example, York \[83\] notes that in empty closed universes constant-$H$ hypersurfaces define ‘a definite slicing of space-time’. Marsden and Tipler \[87\] prove their theorem 3 that a nonflat spacetime having a smooth spacelike compact Cauchy surface, containing a compact smooth spacelike maximal hypersurface, and satisfying the timelike convergence condition, a hypersurface generic condition, and development uniqueness, then has a unique foliation by compact constant-$H$ hypersurfaces, with $H$ running from $+\infty$ at the big bang to $-\infty$ at the big crunch, so long as constant-$H$ hypersurfaces avoid singularities and don’t turn null. Gerhardt \[88\] and Bartnik \[89\] have proved the existence of such a foliation under less stringent conditions (though still assuming the timelike convergence condition), though Bartnik \[89\] also gives an example of a cosmological spacetime with compact Cauchy hypersurfaces that obeys the timelike convergence condition and yet does not have any constant-$H$ Cauchy hypersurfaces. Later Isenberg and Rendall \[90\] give an example that has a constant-$H$ Cauchy hypersurface but is not covered by a foliation of such constant-$H$ hypersurfaces.

Less seems to be known about constant-$H$ hypersurfaces in spacetimes not obeying the timelike convergence condition, such as inflationary spacetimes. For de Sitter spacetime,
Akutagawa [91] in three dimensions and Montiel [92] in higher dimensions showed that compact constant-$H$ hypersurfaces are all umbilical, meaning that all of the eigenvalues of the extrinsic curvature are equal. These hypersurfaces are all obtained by taking constant-time hypersurfaces in the $k = +1$ FRW representation of de Sitter and performing de Sitter symmetry transformations. (On the other hand [91,92,94], there are complete noncompact constant-$H$ hypersurfaces in de Sitter that are not umbilical and hence not isometric to the umbilical noncompact hypersurfaces of constant time in the $k = 0$ and $k = -1$ FRW representations of de Sitter.)

The sequence of constant-time $k = +1$ hypersurfaces of de Sitter gives a foliation of the entirety of de Sitter by compact constant-$H$ hypersurfaces, but since one can perform de Sitter symmetry transformations of each of these hypersurfaces, this foliation is not unique. I might guess that a generic small smooth perturbation of de Sitter would remove this ambiguity and lead to a unique foliation by compact constant-$H$ hypersurfaces, but I am not aware of any good evidence either for or against this guess.

For simplicity, here I shall initially assume that one restricts to ensembles such that each spacetime within the ensemble may be foliated by complete compact constant-$H$ Cauchy hypersurfaces. Let $t$ be a time parameter that is constant over each of these hypersurfaces and which increases monotonically as one goes to the future along a sequence of these hypersurfaces. Then one has $H = H(t)$, a unique value of $H$ for each $t$, though if $H$ does not change monotonically with $t$, one need not have a unique $t$ for each $H$ that occurs; there can be more than one hypersurface with the same $H$.

Now to avoid dealing with the entire infinite-dimensional diffeomorphism group, I would propose gauge fixing to this foliation. One could also fix the time coordinate $t$ (up to an overall shift by a constant) to be volume-averaged proper time by letting $dt = dV_4/V_3$, where $V_3$ is the three-volume of the constant-$H$ hypersurface at $t$ and $dV_4$ is the four-volume between that hypersurface and the one at $t + dt$. If there is a earliest infimum for $V_3$ (e.g., zero-volume at a big bang, or a minimum volume at some smallest bounce geometry), one could set $t = 0$ there to eliminate the shift ambiguity in $t$.

Once one has fixed the foliation and the time parametrization, the remaining diffeomorphism freedom is the coordinate freedom on the spatial hypersurfaces. In a neighborhood of any point on one of the hypersurfaces, one could always gauge fix to Riemann normal coordinates, thereby reducing the infinite-dimensional diffeomorphism group on that hypersurface to the six-dimensional group generated by translations of the point and rotations of the Riemann normal coordinates about that point.

Now suppose that instead of being a discrete index representing which region of spacetime, $K$ is now taken to be an element of the six-dimensional group of rotations and translations of the Riemann normal coordinates. Then we might imagine that $P^K_j$ corresponds to a projection operator onto some field configurations in the corresponding Riemann normal coordinate system that would be or give rise to the observation $O_j$. (For simplicity, I shall assume that the region needed for the field configurations does not exceed the applicability of the Riemann normal coordinates.) Since the observation should not depend on the orientation or location where it can occur (assuming that the fields are located and oriented appropriately there), one might expect that the contribution to $R_j$ from one hypersurface (say $R_j(t)$) would be an average over the six-dimensional group of elements $K$ of the $P^K_j$ on the hypersurface at that value of $t$. (The volume element of the group can be taken to be the volume element of the three-dimensional rotation
group multiplied by the volume element of hypersurface for the translations. Since each hypersurface of the foliation is assumed to be compact, the total volume of the group will also be compact, so the average over the group should be well defined.)

In this way with gauge fixing to compact Cauchy hypersurfaces of constant direction-averaged Hubble rate $H$, we can implement volume averaging over each such hypersurface. We still have to deal with the integral over different spatial hypersurfaces. It might be noted that by the preferred foliation of the spacetime, we have avoided the noncompact integration over boosts in the Lorentz group. It might seem somewhat artificial to have avoided this, since one might expect that in principle observations could be generated by configurations of fields (including the fields of the observer) at any velocity relative to that of the worldlines orthogonal to the preferred foliation by the constant-$H$ hypersurfaces. However, I shall leave aside that issue for future analysis.

5. Integrating over hypersurfaces

In the approximation of regarding a quantum spacetime as an ensemble of classical spacetimes, I have proposed gauge fixing to a particular foliation (e.g., by hypersurfaces of constant trace of the extrinsic curvature, constant logarithmic rate of the growth of three-volume) and then volume averaging on those hypersurfaces. It remains to say how to combine the effects of different hypersurfaces in the foliation of each classical spacetime in the ensemble, that is, how to integrate over the time parameter $t$ labeling the hypersurfaces in the foliation.

The simplest proposal seems to be

$$R_j = \int R_j(t) \, dt,$$

where, as given above, $R_j(t)$ is the group average of $P_{jK}$ over the rotation group and volume of the hypersurface of fixed $t$ and constant $H(t)$, and where $dt$ is the volume-averaged proper time between nearby hypersurfaces. This proposal might be called proper-time weighting of the hypersurfaces (in distinction to the volume averaging of observations made within each hypersurface that is my main proposal, though both parts are needed for a complete specification of the measure).

Another proposal would be

$$R_j = \int R_j(t) |dH/dt| \, dt,$$

the sum of $\int R_j(t)|dH|$ over all segments of the history where $H$ changes monotonically with the volume-averaged proper time $t$. This proposal might be called Hubble-constant-time weighting of the hypersurfaces.

A third proposal is

$$R_j = \int R_j(t) \, dV_4 = \int R_j(t) V_3(t) \, dt,$$

where $dV_4$ is the four-volume element between infinitesimally nearby hypersurfaces in the foliation, and $V_3(t)$ is the three-volume of the hypersurface in the foliation at $t$. This proposal might be called four-volume weighting of the hypersurfaces. It essentially restores...
the three-volume weighting. Because of the problems I have noted with volume weighting, here I shall not advocate this four-volume weighting procedure of the hypersurfaces, but I am pointing it out to show that it is not a priori obvious to me that one weighting is intrinsically far superior to another. This fact may just be a manifestation of what I have noted above [81], that for predicting probabilities of observations, a theory is not fully specified by just the dynamical laws and boundary conditions, but rather one also needs the detailed procedure for calculating the probabilities of observations. Therefore, it may be a mistake to expect this procedure to arise intrinsically from the formalism for the dynamical laws and boundary conditions. One needs more than just the quantum dynamics and the quantum state.

6. Problems with constant-\(H\) hypersurfaces

Although compact constant-\(H\) hypersurfaces seem to be the simplest prescription for foliating a spacetime when they work, there are problems with them. Firstly of course, they certainly will not work to foliate a spacetime that is not globally hyperbolic or that does not have compact Cauchy hypersurfaces. However, I have no good idea how to handle such cases at all, so for now I shall assume that such spacetimes can be excluded. (Perhaps they never arise from approximations to the quantum state of the universe, which conceivably could give only globally hyperbolic spacetimes with compact Cauchy hypersurfaces, if it gives spacetimes at all in some level of approximation.)

Secondly, there are known examples [89,90] mentioned above in which globally hyperbolic spacetimes with compact Cauchy hypersurfaces cannot be foliated by compact constant-\(H\) hypersurfaces. One might regard these examples as also rather pathological, but there is also a common example in inflation that apparently cannot be completely foliated by compact constant-\(H\) hypersurfaces: de Sitter with the formation of a thin-wall Coleman–De Luccia [98] bubble of Minkowski spacetime inside. Yang [99] has shown that a foliation by compact constant-\(H\) hypersurfaces covers only a small part of the Minkowski spacetime region inside the bubble and also avoids an infinite spacetime volume in the de Sitter region outside the bubble (though it also covers an infinite spacetime volume in the de Sitter region, up to infinite proper time along a large set of timelike worldlines that stay outside the bubble). Indications suggest that the similar behavior may occur if a Coleman–De Luccia bubble of our value of the cosmological constant formed out of a parent de Sitter universe of a larger cosmological constant. Then the compact constant-\(H\) hypersurfaces that cross the parent de Sitter region would not enter into our part of the spacetime nearly far enough to cover our existence.

If this behavior is indeed confirmed by a more careful analysis, and if indeed our part of spacetime formed as a bubble from some pre-existing spacetime with a much larger cosmological constant, it would suggest that our existence would not contribute to the measure defined in terms of a foliation by compact constant-\(H\) hypersurfaces. However, I am a bit skeptical of the usual picture in eternal inflation that our part of spacetime most probably formed from tunneling from some previous part of spacetime with a larger cosmological constant. I would prefer to explore the alternative that our part of spacetime formed rather directly, without having had a significant probability to have had ancestor regions of spacetime with different cosmological constants. (Although I am a creationist in the sense of believing the universe was created by God, I am not a recent-creationist
Cosmological measures without volume weighting in the traditional sense of believing that the universe was created within the past ten thousand years or so. But am I a recent-creationist in the sense that I suspect that the universe may most probably have started around 14 billion years ago instead of far earlier before some long series of tunnelings from some original ancestor’s spacetime to our pocket universe? My main worry with the latter popular scenario is not theological but whether the arrow of time would persist through an arbitrarily long sequence of bubble formations and decays, or whether the entire multiverse would tend toward some sort of heat death that would be inconsistent with our observations.

If the main contribution to the path integral for our present existence comes from our own spacetime region or pocket universe, without the need for a sequence of tunnelings from some ancestor region, then our existence might indeed be covered by some foliation of our region by compact constant-$H$ hypersurfaces. If our pocket universe decays into future bubbles of smaller or negative cosmological constant (e.g., terminal vacua), then the constant-$H$ foliation may indeed not penetrate very far into those regions, and it might not cover all of the future of our apparently asymptotically de Sitter region, but if it covers the existence of most observers in our part of spacetime, that should be sufficient to get at least approximately the right measure for our observations.

7. Alternatives to constant-$H$ hypersurfaces

Although constant-$H$ hypersurfaces presently seem to be the simplest known and most studied proposal for foliating at least some large class of spacetimes, I would certainly not claim that they are definitely the correct way to do things. If indeed they do not work (which does now seem rather likely if our pocket universe really did come from a long sequence of decays of previous vacua), one should look for alternatives.

A common alternative is to postulate some initial smooth spacelike Cauchy surface, construct timelike geodesics orthogonal to it, and then define foliating hypersurfaces to be the hypersurfaces orthogonal to this congruence of geodesics. One disadvantage of this procedure is the requirement of the choice of the initial hypersurface (or alternatively simply of the congruence itself), but this might be regarded as a small price to pay if the result overcomes any severe difficulties that might arise from other proposed foliations.

A much more severe problem with this proposed foliation is that generically the timelike geodesics will have conjugate points where they will begin to cross and form caustics. This will prevent the orthogonal hypersurfaces from remaining smooth and from being crossed only once by each inextendible timelike geodesic. Furthermore, there will be more than one hypersurface at many points of spacetime, where different timelike geodesics from the initial hypersurface intersect with different values of proper time. Within the solar system, timelike geodesics that remain within the sun or a planet will typically have conjugate points separated by only an hour or so, making the hypersurfaces orthogonal to the geodesics run into difficulty very quickly. Therefore, some modification of the procedure will be needed.

One modification that can partially ameliorate the problem is not to require that the hypersurfaces be orthogonal to the entire congruence of crossing geodesics, but instead to have constant values of a time function that is defined to be the maximum proper time of any causal curve back to the original hypersurface. The curves that maximize the proper time will of course be timelike geodesics that intersect the original hypersurface
orthogonally, but for later points intersected by more than one geodesic orthogonal to the original hypersurface, only the longest one will count for defining the time function. Thus one will get definite spacelike hypersurfaces of constant proper time from this procedure.

One disadvantage of this procedure for getting constant-proper-time hypersurfaces is that generically on the three-dimensional hypersurfaces of constant maximal proper time after the original hypersurface, there will be two-dimensional surfaces where two such maximal geodesics intersect. On one side of the two-surface, the time function will be given by one local congruence, but on the other, it will be given by a different local congruence. The two congruences will have different tangent vectors (four-velocities) on the two sides, so at the two-dimensional surface, the normal vector to the three-dimensional hypersurface will jump by some boost, giving the constant-time hypersurface a kink. There can also be further defects along one-dimensional lines within the hypersurface, as well as at isolated points. But if one can live with a foliation by hypersurfaces with kinks and other defects, and if one is willing to specify an original hypersurface, then the ones of constant proper time from the original hypersurface would seem to work in any globally hyperbolic spacetime with compact Cauchy hypersurfaces.

Another alternative worth exploring is what might be called ‘minimal-flux hypersurfaces’. For a given smooth compact Cauchy hypersurface with unit future-pointing timelike normal $n^a$, the energy density in the frame of an observer with 4-velocity $n^a$ is $\rho = T_{ab}n^an^b$, and the spatial energy flux in the frame of this observer is $J^a = -T^a_{\;b}n^b - \rho n^a$ \[100\]. Firstly, consider only hypersurfaces for which the volume average of the energy density $\rho$ (using the 3-volume element from the metric induced on the hypersurface by the spacetime metric) has a fixed value $\bar{\rho}$. Next, for such hypersurfaces, choose the one that minimizes the volume average of the square of the spatial energy flux, $J^a J_a$. Assuming the existence and uniqueness of such a hypersurface, it can be defined to be the minimal-flux hypersurface for the given value of the average energy density $\bar{\rho}$. Finally, consider the one-parameter sequence of such compact minimal-flux hypersurfaces as a function of $\bar{\rho}$. If this indeed foliates the spacetime, this will be a ‘minimal-flux foliation.’

In the case that the stress–energy tensor $T_{ab}$ has a unit timelike eigenvector field $u^a$ (obeying $u^a u_a = -1$ and $T_{ab} u^a = -\hat{\rho} u_b$ for some eigenvalue $\hat{\rho}$, which is the energy density in the frame with 4-velocity given by the eigenvector $u^a$, a frame in which there is no spatial energy flux) and in the case that this eigenvector field is hypersurface-orthogonal, then one may choose the hypersurfaces so that $n^a = u^a$, giving $\rho = \hat{\rho}$ and $J^a = 0$, so such hypersurfaces would be minimal-flux hypersurfaces. (For example, in $k = +1$ FRW spacetimes, the homogeneous isotropic hypersurfaces of constant $t$ would be such compact minimal-flux hypersurfaces.) However, generically the timelike eigenvector field (if it exists, which is also not guaranteed but seems to be the generic case) would not be hypersurface orthogonal, so that there would not be hypersurfaces with zero spatial energy flux $J^a$ everywhere. Minimal-flux hypersurfaces make one particular minimization of the deviation of their normals $n^a$ from the timelike eigenvector field $u^a$.

It would be worth exploring more the properties of minimal-flux hypersurfaces and the cases in which they do foliate part or all of a spacetime with compact Cauchy hypersurfaces. For example, do they foliate generic perturbations of FRW spacetimes? If a classical approximation to our quasiclassical component of the quantum state of the universe does have a minimal-flux hypersurface through our present location in this spacetime, what is the deviation of $n^a$ from $u^a$ at the surface of the earth? (I might guess
that would be of the order of the magnitude of the peculiar velocity of our galaxy from the mean Hubble flow, but one might ask whether the part of spacetime far beyond what we can see could possibly force the minimal-flux hypersurface to have a significantly greater variation of $n^a$ from $u^a$ at our location.)

Minimal-flux hypersurfaces would seem to have the advantage that their normals $n^a$ would presumably be close to the 4-velocity $u^a$ of the local matter frame (say given by the unit eigenvectors of the stress–energy tensor), at least for situations in which the peculiar velocities of the matter are small, as is generally the case in our part of the universe. A foliation by these hypersurfaces thus might avoid the objection Vilenkin [101] has raised to the constant-$H$ hypersurfaces, that inside new bubble universes their normals would typically be at high velocities to that of the matter there, even if the bubbles were locally close to open FRW universes. (As noted above, it now looks [99] as if the problem is even more severe for such bubbles, in that apparently the compact constant-$H$ hypersurfaces do not even go very far into the new bubble universes.) Although I am not convinced that the dominant contribution to the measure for observations will come from baby universes arising from bubble nucleation out of a parent universe (as opposed to the direct creation of our pocket universe without any ancestors), if one does want to investigate the measure inside baby bubble universes, minimal-flux hypersurfaces might be a better attempt for a foliation. However, this has not yet been investigated.

One difficulty in analyzing minimal-flux hypersurfaces is that they rely for their definition on the stress–energy tensor and so are not defined for vacuum spacetimes, but that should not be a problem for any spacetime region rather like our own, which certainly does have matter and a nonzero stress–energy tensor. There might conceivably be difficulties in an asymptotically de Sitter future part of spacetime with a cosmological constant as the non-vacuum part of the stress–energy tensor tends toward zero. (A purely vacuum part proportional to the metric, such as the cosmological constant, gives $J^a = 0$ for all hypersurfaces and therefore provides nothing nontrivial to minimize.) However, one might imagine that minimal-flux hypersurfaces would be defined wherever there is any reasonable nonzero stress–energy tensor, no matter how small. A bigger worry in my mind is whether the sequence of them with smaller and smaller $\bar{\rho}$ continue to foliate the spacetime, or whether these hypersurfaces might cross each other or else simply not go beyond some events in the spacetime.

Clearly constant-$H$ hypersurfaces, constant-proper-time hypersurfaces, and minimal-flux hypersurfaces are all rather ad hoc proposals. Although I am happy to propose volume averaging rather than volume weighting for observations within a hypersurface, I am not happy with anything I have thought of so far for which hypersurfaces to include and how to add up their contributions. This is definitely an ugly part that needs some elegant new ideas.

8. Violation of the correspondence principle

One objection [101] to volume averaging rather than volume weighting is that it violates the ‘correspondence principle’ that for a finite spacetime, ‘The probability of a given outcome of a measurement can be simply defined as the relative number of instances when this outcome is obtained’. For example, consider a $k = +1$ FRW spacetime with both a big bang and a big crunch, so that the scale factor $a(t)$ goes to zero at both $t = 0$
and $t = T$, where $t$ is proper time of the longest timelike curve from the big bang, and 
$T$ is the finite lifetime of this universe. In this simple case the constant-$H$ hypersurfaces 
will be the usual homogeneous, isotropic $S^3$ hypersurfaces of fixed proper time $t$ and 3-
volume $V_3 = 2\pi^2 a^3(t)$, and the volume-averaged proper time will be this same $t$, since 
$\text{d}V_4 = V_3 \text{d}t$. Therefore, let us use the foliation by constant-$H$ hypersurfaces and then 
compare four-volume weighting with proper-time weighting of the hypersurfaces.

Now let $n(t)$ be the density per 4-volume of some observation, for simplicity uniform 
over each constant-$H$ hypersurface. Then $N_{VW} = \int_0^T 2\pi^2 a^3(t) n(t) \text{d}t$ would be the total 
number of these observations in the spacetime, which is what one would get by volume 
weighting (equivalent to volume averaging over each hypersurface and then four-volume 
weighting for the integral over the hypersurfaces). On the other hand, volume averaging 
over each hypersurface and then proper-time weighting of the hypersurfaces would give a total weight for the observation proportional to $N_{VA} = \int_0^T n(t) \text{d}t$, simply the proper-time 
integral of the average density of observations. Thus volume averaging gives less weight 
to observations when the hypersurfaces have greater volume, violating the ‘equivalence 
principle’.

I readily admit that this is indeed a feature of volume averaging. However, it is not 
clear that it is in conflict with any observational or theoretical requirement. We have no 
strong evidence that the probability measures for individual observations are not actually 
going down as the volume of space goes up. However, if we use hypersurfaces of constant 
proper time since decoupling (or since reheating, assuming inflation occurred earlier) in 
our part of the universe, the volume has gone up by only a tiny fraction during the history of the entire human race, by a factor less than 1.000 05 during the 200 000 years of modern humans. Perhaps, for other things being equal, our present observations have 0.005% lower measure than those of early modern humans, but I do not see how we could possibly exclude this possibility observationally.

There would be a serious problem if our pocket universe had developed as a bubble 
in a parent universe with a much greater cosmological constant and if the hypersurfaces 
on which one does the volume averaging have the main contribution to their volume from the rapidly expanding parent universe region. Then the total volume of the hypersurfaces 
through us (most of the volume being in the rapidly expanding parent universe) would 
be growing at a rapid logarithmic rate, so with volume averaging the weight for our 
observations would rapidly decrease with time. This would then lead to the youngness 
paradox [3, 11, 16, 70]. However, as discussed above, I prefer to explore the alternative that 
our pocket universe formed rather directly, most probably without ancestors, so that the 
main contribution to the volume of the hypersurfaces lie within our pocket universe itself 
and is not growing very rapidly on human scales. Then the violation of the ‘correspondence 
principle’, although in principle present, would be very tiny during the past lifetime of 
human civilization.

9. Application to Boltzmann brains

Replacing volume weighting by volume averaging can greatly ameliorate the problem 
of Boltzmann brains, though whether it completely solves the problem depends on the 
procedure for the integration over hypersurfaces and on whether the universe lasts forever.
I shall here leave aside the four-volume weighting procedure, since that seems to leave the problem in the severe form it has with the usual volume weighting.

Suppose that the probability per Planck volume of a Boltzmann brain is very roughly $e^{-I}$, where $I$ is much, much larger than the value of the logarithm, say $J$, of the reciprocal of the probability per Planck volume of an ordinary observer in the present universe. For example, $I$ might be the instanton action for producing a Boltzmann brain. If one wants a brain of mass and size comparable to that of a human brain, say mass of the order of 1 kg and size of the order of 0.3 m, then $I \sim 10^{42}$ for a ‘brief brain’ [55]. If one wants a brain of mass 1 kg to last 0.1 s, which is what some people think may be necessary for it to make an observation, then $I \sim 10^{50}$ for a ‘medium brain’ [51, 55]. On the other hand, if one wants a brain of mass 1 kg to last for several Hubble times (or to be produced as a real thermal fluctuation in the future asymptotic de Sitter spacetime), then $I \sim 10^{69}$ for a ‘long brain’ [55].

Then in the universe up to the present time, the fraction of Boltzmann brains to ordinary observers is very roughly $e^{-I+J}$, so we can ignore their effect from just the past part of our spacetime. But now suppose ordinary observers mostly die out when the stars burn out and do not last enormously longer, whereas Boltzmann brains continue to fluctuate into and out of existence at their very tiny rates.

If we had used volume weighting, Boltzmann brains would dominate when the spatial volume is larger than today by a factor of roughly $e^{I-J}$. Since the volume asymptotically grows at a rate $e^{3Ht}$, with $H_\Lambda = \sqrt{\Lambda/3}$ in terms of the cosmological constant $\Lambda$, this time is of the order of $t \sim (I - J)/(3H_\Lambda)$, of the order of $10^{42}$ years if ‘brief brains’ can have observations (as I myself would think is most plausible), $10^{50}$ years if one needs ‘medium brains’, or $10^{69}$ years if one needs ‘long brains’ [51, 55]. Then if the universe is destroyed or decays deterministically before the appropriate one of those times, there would not be a Boltzmann brain problem. However, it is hard to see what is likely to make the universe cease to exist at such a time that is so short in comparison with the Poincaré recurrence time of the order of $e^{10^{123}}$ years for de Sitter spacetime with the presently observed value of the cosmological constant. One gets an even shorter expected decay time of the order of $10^{10}$ years to prevent Boltzmann brains from dominating with volume weighting if the universe decays quantum mechanically by bubble formation rather than deterministically [53, 59], since with a lower decay rate, the expectation value of the four-volume diverges, giving an expectation value of Boltzmann brains per comoving volume that is infinitely more than of ordinary observers in our universe. (This result may be modified by a consideration of ordinary observers and/or Boltzmann brains that may form in the next universe after the bubble decay [56, 57], but the answer is not clear because of the ambiguities of the measure problem with volume weighting.)

If we use instead proper-time weighting of hypersurfaces without volume weighting for observations on hypersurfaces, we need the expectation value of the total proper-time lifetime of the universe (rather than of the four-volume) to be less than roughly $t_0 e^{I-J}$. If we just try to get the order of magnitude of the exponent roughly right, this gives $e^{10^{42}}$ years for ‘brief brains’, $e^{10^{50}}$ years for ‘medium brains’, or $e^{10^{69}}$ years for ‘long brains’. Although these times are also enormously shorter than the Poincaré recurrence time for de Sitter spacetime, the logarithms of their logarithms are of the same order of magnitude, so it might be plausible that spacetime would decay within one of those timescales [24, 74]. But if the universe does last forever in a form wherein Boltzmann brains can continue to
fluctuate into (and out of) existence, then it appears that there is still a problem with Boltzmann brains even without volume weighting if one uses proper-time weighting.

Even if the universe does have an expected decay time shorter than say $e^{10^{42}}$ years, there still may be a problem [102] if the decay is by bubble formation of terminal vacua that leaves the bulk of our asymptotically de Sitter spacetime intact. Then presumably the constant-$H$ (or constant-proper-time) hypersurfaces will continue to evolve forward in the expanding part of de Sitter spacetime that remains outside the bubble formation, so that there would be an infinite time for Boltzmann brains to appear on it. One might think [99] that the volume fraction of the hypersurfaces that stay outside the bubbles would decrease fast enough to lead to a convergence in the integral over $dt$ of the average density of Boltzmann brains on the hypersurface (presumably only on the part that stays outside the terminal bubbles), but if the three-volume inside each bubble is small, whereas the part of the hypersurface outside keeps expanding with the asymptotically de Sitter universe, it seems to me more plausible that the fraction of the three-volume inside bubbles would always remain small. Then the density of Boltzmann brains, averaged over the entire hypersurface, would remain near its nearly constant value in the asymptotically de Sitter part, and so the integral of that over $dt$ would diverge along with $t$.

I would like to postulate that when terminal vacua are forming, somehow the quantum measure for the hypersurfaces decreases exponentially. If one imagined some sort of collapse of the wavefunction to possible nonexistence over the entire hypersurface with some probability proportional to the probability that a bubble forms on the hypersurface, this might be accomplished, but I am highly skeptical of any collapse of the wavefunction that would occur acausally over a hypersurface. I suppose one could still postulate a decrease in the absolute value of the existence amplitude of the hypersurface, as if it represented a probability for it to collapse out of existence, without invoking any actual collapse. However, for this to be caused by the amplitude for a bubble to occur at some location on the hypersurface smacks of some degree of acausality. On the other hand, a quantum state is a function or functional of an entire configuration space and so is certainly nonlocal. If the quantum state changes according to what may be happening on the hypersurface (e.g., by the potential for bubble formation), then since the quantum state is inherently nonlocal, it may appear as if the change is acausal.

If it turns out that the proper-time weighting of hypersurfaces simply does not work, one might instead turn to the Hubble-constant-time weighting of hypersurfaces. Then the integral at late times will not diverge at all, since $H(t)$ is expected to drop from its present finite value to a finite asymptotic value $H_\Lambda = \sqrt{\Lambda/3}$ if the dark energy driving the current cosmological acceleration is a cosmological constant or a minimum in a scalar field potential, or else to the finite asymptotic value of zero if the dark energy decays away. One might expect a divergence instead at infinite values of $H(t)$ if the universe started at a big bang with an infinite logarithmic expansion rate. However, if $H(t)$ has an upper cutoff at the Planck value, that would also keep the integral convergent and still sufficiently small when weighted by $e^{-I}$ that Boltzmann brains would not come at all close to dominating.

Even if one took $H(t)$ back to infinity at a classical big bang at $t = 0$, one might argue that in a region large enough for a Boltzmann brain, the energy density would be so high that the probability would be exponentially small that it would take the form of a Boltzmann brain. For example, suppose the entropy goes as a positive power $p$ of the
energy density, which itself is expected to go as $t^{-2}$ for small $t$ near the big bang, so that the entropy in a region of the volume of a Boltzmann brain goes as $S(t) \sim Ct^{-2p}$. Then if there are only a finite number of configurations in a given volume that would correspond to Boltzmann brains, then the probability that one of the $e^S$ configurations would be a Boltzmann brain would go roughly as $P(t) \sim e^{-S(t)} \sim e^{-Ct^{-2p}}$. Now if the scale size goes as some positive power $q$ of the proper time, $a(t) \sim At^q$, so that $H(t) = \dot{a}/a \sim q/t$ and $|dH/dt| \sim q/t^2$, then $\int P(t)|dH/dt| dt \sim \int e^{-Ct^{-2p}}q t^{-2} dt$ clearly does not diverge at $t = 0$. Almost certainly with the $|dH/dt|$ weighting factor, the probability of a observation by a Boltzmann brain in a big-bang universe with Hubble-constant weighting would be far below that of an ordinary observer. Thus the Boltzmann brain problem would be solved with volume averaging of observations on each hypersurface and with Hubble-constant-time weighting of hypersurfaces, even if the universe lasts forever.

On the other hand, in [103] Hawking and I argued that ‘In situations in which the wavefunction can be interpreted in terms of classical solutions by the WKB approximation, this choice of measure implies that the probability of finding the 3-metric and matter field configuration in a given region of superspace is proportional to the proper time that the solutions spend in that region’. This argument would tend to support the proper-time weighting of hypersurfaces.

10. Application to proposed quantum states of the cosmos

Volume averaging (rather than volume weighting) for observations on hypersurfaces thus seems to help solve the measure problem and the Boltzmann brain problem (at least if the universe does not last extraordinarily long, or if one uses Hubble-constant-time weighting of hypersurfaces). However, that is only the case if the quantum state of the universe is dominated by finite-volume spaces that evolve from a big bang (or at least from a region of high density) and then expand to give ordinary observers with high probability. There can still be problems explaining our observations if the quantum state is dominated by spaces without ordinary observers, such as large nearly empty spaces.

For example, the Hartle–Hawking ‘no-boundary proposal’ still seems problematic [55], because it gives an enormous amplitude for nearly empty de Sitter spaces, whose Boltzmann brains would greatly dominate over the ordinary observers that only exist in a part of the quantum state with much lower amplitude, even without volume weighting. The ‘tunneling’ proposals of Vilenkin, Linde, and others [76], [104]–[109] seem to fare better, though the ones of these that just reverse the sign of the Euclidean action give divergent amplitudes for arbitrarily large perturbations [110]–[114] (which is not what Vilenkin’s tunneling proposal does [76,104,108,109], though one might say that this proposal is not yet precisely defined, even at the level of minisuperspace).

The no-bang proposal [113] for the quantum state of the universe appears to avoid some of the problems of the no-boundary proposal and yet seems to be more precisely defined than the tunneling proposal. However, without volume weighting, the no-bang state appears to be dominated by thermal perturbations of nearly empty de Sitter spacetime, in which almost all observers would presumably be Boltzmann brains. Since this would almost certainly make our observations very unlikely, the no-bang proposal apparently is observationally excluded if one uses volume averaging rather than volume weighting.
Therefore, for volume averaging to solve the measure problem and avoid the Boltzmann brain catastrophe, one needs a quantum state that is not enormously dominated by nearly empty de Sitter spacetime. One would like a state that is dominated by spacetimes having a big bang or bounce at volumes much smaller than that given by the apparent cosmological constant observed today. Clearly more work needs to be done to find such a state.

11. Conclusions

The measure problem is a severe problem in theoretical cosmology that is logically independent of the question of what the quantum state of the universe is. With a suitable quantum state, replacing volume weighting with volume averaging in the cosmological measure appears to help avoid the ambiguities of infinite measures generated by eternal inflation and also avoid the Boltzmann brain catastrophe and the youngness paradox. It also appears to avoid the ‘Q catastrophe’ of exponentially preferring either huge or tiny primordial density contrasts [114, 115] and the analogous catastrophe for the gravitational constant $G$ [116]. However, even with this volume averaging measure, the result does depend on the quantum state, and one does still need to find a quantum state that would give sufficiently high probabilities for our observations.

One consequence of volume averaging rather than volume weighting would be a loss of the argument for infinite volumes today from eternal inflation [11], [76]–[79]. There would still be amplitudes for arbitrarily large amounts of inflation, but without volume weighting, the bulk of the probabilities for observations would occur for spaces with only a bounded amount of inflation. However, although there is a lot of indirect observational evidence for inflation itself, I think it is fair to say that so far there is not any observational evidence for infinite volumes from eternal inflation in particular. (It is hard to see how we can have any direct observational evidence about the volume of the spatial hypersurface containing us, since all of it outside ourselves is acausally related to us now.) It would be interesting to see whether there is any observational way to confirm or refute infinite volumes from eternal inflation, other than the Boltzmann brain catastrophe that often seems to accompany theories of infinite volume from eternal inflation. Volume averaging can help kill both Boltzmann brains and infinite volumes from eternal inflation, but it remains to be seen whether this solution can be observationally distinguished from solutions that kill Boltzmann brains but not infinite volumes from eternal inflation.

Many of the implications of volume averaging rather than volume weighting are qualitatively similar to those of the causal diamond or holographic point of view of Bousso and collaborators [23, 25, 31, 33, 54, 70]. They argue that one should restrict attention to the causal diamond region that is both in the past and in the future of an observer’s history or worldline. I would agree that perhaps the only testable predictions of a theory involve such restricted regions. (I would be even more radical in proposing that the predictions should involve only the data that has entered the observer and no external data at all.) However, to me it would seem that one should allow a theory making such predictions to give a more global description of the quantum state of the universe. (This is not a claim that there should be some classical global spacetime for the entire universe. Surely there will be a huge quantum superposition.) If volume averaging can avoid the problems of volume weighting that the causal diamond approach also avoids, and yet keep a global description, then it would seem advantageous.
It appears that yet another way to get rather similar results is the scale-factor cutoff measure [44, 46, 47]. It is not yet clear which of these various approaches is best. However, I would suspect that as an approximation to a prescription best given in a quantum analysis, it would be better to try to avoid properties of a global classical spacetime, such as any time parameters defined by some classical history of spacetime. For this reason I am not convinced that global time cutoffs are the best approach. This is my motivation for looking at hypersurfaces instead and trying to define them quasi-locally, since three-metrics and matter fields on spacelike hypersurfaces are the traditional configuration-space coordinates in canonical quantum gravity. (Of course, there are severe problems with canonical quantum gravity, including the fact that with fluctuations in the geometry it may not be definite that any hypersurface is acausal, allowing quantum xeroxing to occur and preventing a description of the quantum state as a wavefunctional of hypersurface geometries and matter fields.)

In any case, it may be premature to say which, if any, of the current approaches is most likely to lead to the ultimate answer to the measure problem. At present it seems best to investigate a variety of approaches, see what their consequences are, and try to find whether one can find a truly quantum implementation. It seems that volume averaging of observations on hypersurfaces, though by no means complete without a specification of how to do the sum over hypersurfaces, merits further investigation.

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