Workshop on Cosmology and Time

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Response to: *Three Merry Roads to T-Violation* by *Bryan W. Roberts*

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I. PREAMBLE

Dr. Roberts has provided a lucid account of the analytical arguments that underlie the study of T-violation [1]. His clear presentation makes my task rather easy. I will discuss T violation in a very general setting that incorporates quantum mechanics and quantum field theory, but is not tied to them. The focus will be on the conceptual aspects of the two approaches that have led to experimental proofs of T-violation in weak interactions.

Since Dr. Roberts mentioned the macro-world only in passing, let me begin a brief discussion of the manifest arrow of time we perceive in our everyday life and, more generally, in the physics of large or macroscopic systems. For simplicity, let me discuss this issue in the framework of classical physics because the core of the argument is not sensitive to the distinction between classical and quantum mechanics. Consider a large box with a partition that divides it into two parts, say, the right and the left halves. Suppose there is some gas in the left half and vacuum in the right. Once equilibrium is reached, the macroscopic state of this gas is described by the volume it occupies, V_i; the pressure it exerts on the wall. P_i and its temperature T_i, where i stands for 'initial'. If we open the partition slowly, the gas will fill the whole box and its macro-state in equilibrium will be described by new parameters, V_f, P_f, T_f. Thus, there has been a transition from the initial macro-state (V_i, P_i, T_i) to a final state (V_f, P_f, T_f). Our common experience tells us that the time reverse of this process is *extremely* unlikely.

However, we also know that the microscopic variables for the system are the positions and momenta of some 10^{23} molecules in the box. These are subject just to Newton's laws which are manifestly invariant under the time reversal operation T! Therefore, if we were to reverse the momenta $\vec{p}_{(\alpha)}(t)$ of each of the molecules (labeled by α) at a late time t, keeping the final positions $\vec{x}_{(\alpha)}(t)$ the same, time evolution would indeed move the gas from its final macroscopic state to the initial one. But it is very difficult to construct this time-reversed initial state. Thus there is indeed a macroscopic arrow of time but its origin is *not in the failure of the microscopic laws to be invariant under* T but rather in the fact that the initial conditions we normally encounter are very special. This is reflected in the fact that there are vastly fewer micro-states compatible with the initial macro-state (V_i, P_i, T_i) than there are compatible with the final macro-state (V_f, P_f, T_f).[1] Put differently, the entropy of the initial macro-state is much lower than that in the final macro-state.

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[1] This is primarily because the volume V_f allowed for *each* molecule in the final macro-state is twice as large as V_i, allowed in the initial macro-state.

To summarize the fact that there is a clear arrow of time in the macro-world does *not* imply that the microscopic or fundamental laws have to break T-invariance. Indeed, as Dr. Roberts emphasized in the beginning of his article [1], it was common to assume that the fundamental laws *are* invariant under the time reversal operation T. It was a major surprise that the weak interaction violates this premise.

II. WEAK INTERACTIONS AND THE CURIE PRINCIPLE

As Dr. Roberts has explained clearly, what the Cronin-Fitch experiment establishes directly is that the weak interactions are not invariant under CP, i.e., under the simultaneous operation of charge conjugation and parity. The parity operation, as normally formulated, is meaningful only if the underlying space-time is flat, i.e., represented by Minkowski space-time. This means one ignores curvature and therefore gravity. One further assumes that physics is described by a local quantum field theory on this Minkowski space, for which individual physical fields transform covariantly under the action of the Lorentz group and dynamics is generated by a self-adjoint Hamiltonian obtained by integration of a scalar density constructed locally from the physical fields. Then, one has the *CPT theorem* that guarantees that the product CPT of charge conjugation, parity and time reversal is an exact *dynamical* symmetry.[2] Therefore, as Dr. Roberts explained, if we assume that weak interactions are described by such a theory, then the observed breakdown of CP invariance implies that they violate T invariance as well.

Dr. Roberts describes the mathematical underpinning of the 'Curie Principle' in his section 2.4 using a linear transformation R on the Hilbert space of states, which is to play the role of a symmetry of interest. This formulation can be significantly generalized. The main point of my 'response' is to provide this formulation.

As Dr. Roberts emphasizes, his analysis has the advantage that it does not assume a specific Hamiltonian. Let us go a step further and consider a formulation that does not use even the mathematical structure normally used in quantum (or classical) *kinematics*. Both frameworks will be special cases of this *general mechanics*. What it assumes is:
i) We have a set $\mathcal{S}$ of states;
ii) There is a 1-1, onto dynamical mapping S —the 'S-matrix'— from $\mathcal{S}$ to itself. In practice is it convenient to consider two copies $\mathcal{S}_i$ and $\mathcal{S}_f$ of $\mathcal{S}$, representing initial and final states, and regard S as a map from $\mathcal{S}_i$ to $\mathcal{S}_f$;

$$S : \mathcal{S}_i \to \mathcal{S}_f; \qquad S(\sigma_i) = \sigma_f \quad \forall \sigma_i \in \mathcal{S}_i \tag{2.1}$$

and,
iii) A 1-1, onto map R from $\mathcal{S}$ to itself, to be thought of a potential symmetry. We will first consider the case in which R maps $\mathcal{S}_i$ to itself and $\mathcal{S}_f$ to itself. This is the case if R is, for example, the discrete symmetry represented by C, or P or CP.

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[2] This is a heuristic version one finds in quantum field theory text books (see, e.g. [2]). More rigorous versions based on Weightmann axioms [3] and the algebraic approach [4] are also available in the literature. However, one should note that we do not have a single example of a 4-dimensional, interacting quantum field theory satisfying either the Wightmann axioms or the axioms of the algebraic quantum field theory.

Now, in the spirit of the Curie principle, suppose that there exists some $\sigma_i \in \mathcal{S}$ such that

$$R\sigma_i = \sigma_i \qquad \text{but} \qquad R\sigma_f \neq \sigma_f \tag{2.2}$$

Then,

$$R\left(S\sigma_i\right) = R\sigma_f \neq s_f = S\left(R\sigma_i\right) \tag{2.3}$$

whence $SR \neq RS$. Thus, the dynamical map S does not commute with the candidate symmetry R: It is *not a dynamical symmetry*. We therefore conclude: *If there exists a state σ_i such that $R\sigma_i = \sigma_i$ and $S\sigma_i = \sigma_f$ but $R\sigma_f \neq \sigma_f$ then R is not a dynamical symmetry of the system.* Thus the Curie principle naturally extends to *general mechanics*. (It is straightforward to alter the argument to obtain the other desired conclusion of fact 1 in section 2.4 of [1].)

Since we assumed $\mathcal{S}$ to be only a set, we cannot speak of continuous evolution. But one could achieve this trivially by endowing $\mathcal{S}$ with a topology and replacing S with a continuous evolution map $E(t)$, where t is to be thought of as a time parameter. The argument given above will then imply that $E(t)$ will not commute with R.

Note that in this more general formulation one does not even assume that the space of states has a Hilbert space structure, whence, when applied to the quantum mechanical system, the argument does not require R or S to be linear mappings. In particular, they need not be unitary. Note also that this general framework enables one to discuss in one go all symmetries in classical mechanics and linear symmetries in quantum mechanics. More importantly, it should be useful also in non-linear generalizations of quantum mechanics (discussed, e.g., in [5]).

However, we did assume that R maps the space $\mathcal{S}_i$ of initial states to itself and the space $\mathcal{S}_f$ of final states to itself. This assumption is *not* satisfied by the time reversal operation T which maps initial states to final states (and vice versa): T is a 1-1 onto map from S_i to S_f. Therefore, in this case, T is a dynamical symmetry if and only if

$$S\sigma_i = \sigma_f \quad \Longrightarrow \quad S^{-1}(T\sigma_i) = T^{-1}(\sigma_f) \tag{2.4}$$

i.e., the time reverse of σ_i (which is in $\mathcal{S}_f$) is mapped by dynamics to the time reverse of σ_f.The generalization of the Curie principle discussed above does not have any implication in this case. In this respect, the situation is the same as in section 2 of [1].

Remark: While R invariance of dynamics is captured by the property $RS = SR$ of the S-matrix S while the T invariance is captured by $S^{-1}T = T^{-1}S$. The above argument shows that the difference arises simply because while R preserves each of $\mathcal{S}_i$ and $\mathcal{S}_f$, T maps one to the other. At a fundamental level, then, the difference is not because R is linear while T is anti-linear although, in standard quantum mechanics, one can use linearity of R and anti-linearity of T to arrive at the difference.

III. THE KABIR PRINCIPLE

Can we extend the arguments from *general mechanics* to encompass time reversal in the spirit of Kabir's argument discussed in section 3 of [1]? The answer is in the affirmative. However, to state Kabir's formulation, one needs to introduce additional structure in *general mechanics* which does not have a natural analog in classical mechanics. This is because Kabir's formulation refers to probabilities.

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Let us then introduce an *overlap map* $\mathcal{O}$ on the space of states $\mathcal{S}$ (and therefore on each of $\mathcal{S}_i$ and $\mathcal{S}_f$): $\mathcal{O} : \mathcal{S} \times \mathcal{S} \to [0, 1] \in R$, such that

$$\mathcal{O}(\sigma,\, \sigma') = \mathcal{O}(\sigma', \sigma), \qquad \forall \sigma, \sigma' \in \mathcal{S}. \tag{3.1}$$

$\mathcal{O}(\sigma, \sigma')$ is to be thought of as the overlap between states σ and σ', the generalization of the absolute value of the quantum mechanical inner product between normalized states $|\langle \Psi,\, \Psi' \rangle|$. The overlap map is part of kinematics. Therefore, to qualify as symmetry, the map R we discussed in section II has to satisfy

$$\mathcal{O}_i(R\sigma_i,\, R\sigma_i') = \mathcal{O}_i(\sigma,\, \sigma') \tag{3.2}$$

on $\mathcal{S}_i$ (and similarly on $\mathcal{S}_f$). Similarly to qualify as symmetry, the Time reversal map which maps $\mathcal{S}_i$ to $\mathcal{S}_f$ must satisfy

$$\mathcal{O}_f(T\sigma_i,\, T\sigma_i') = \mathcal{O}_i(\sigma_i,\, \sigma_i') \tag{3.3}$$

for all $\sigma_i \in \mathcal{S}_i$.

The dynamical map $S : \mathcal{S}_i \to \mathcal{S}_f$ should be compatible with this kinematical structure, i.e., satisfy

$$\mathcal{O}_i(\sigma_i,\, \sigma_i') = \mathcal{O}_f(S\sigma_i,\, S\sigma') \equiv \mathcal{O}_f(\sigma_f,\, \sigma_f')\,. \tag{3.4}$$

Given a dynamical map S, the *transition probability* between an initial state $\sigma_i \in \mathcal{S}_i$ and *any* given final state σ_f' is defined to be

$$P(\sigma_f',\, \sigma_i) := [\mathcal{O}(\sigma_f',\, S\sigma_i)]^2 \equiv [\mathcal{O}(\sigma_f',\, \sigma_f)]^2\,. \tag{3.5}$$

This is the additional kinematical structure we need on our *general mechanics* to formulate the Kabir principle.

Recall that T is a dynamical symmetry if and only if $S^{-1}T = T^{-1}S$. Suppose a dynamical map S satisfies this condition. Then,

$$\mathcal{O}_f(T\sigma_i,\, S(T^{-1}\sigma_f')) = \mathcal{O}_f(T\sigma_i,\, T(S^{-1}\sigma_f')) = \mathcal{O}_f(T\sigma_i,\, T\sigma_i') = \mathcal{O}_i(\sigma_i,\, \sigma_i') \tag{3.6}$$

where in the second step we have set $s_i' = S^{-1}\sigma_f'$ and in the last step we used (3.3). On the other hand, (3.4) and (3.1) imply

$$\mathcal{O}_i(\sigma_i,\, \sigma_i') = \mathcal{O}_f(S\sigma_i,\, S\sigma_i') = \mathcal{O}_f(\sigma_f', S\sigma_i). \tag{3.7}$$

The last two equations and the definition (3.5) of transition probability implies

$$P(\sigma_i', \sigma_i) = P(T\sigma_i',\, T^{-1}\sigma_f')\,. \tag{3.8}$$

Thus, we have shown that if T is a symmetry of the dynamical map S then the transition probability between the states σ_i and σ_f' must equal that between the two states obtained by a time reversal. Therefore if the transition probability between *any* two states and their time reversed versions differ observationally, then the time reversal symmetry is broken by dynamics.[3]

[3] It is worth noting that the actual transition rate is not determined solely by the transition probability. In the leading order approximation ("Fermi's golden rule") the transition probability has to be multiplied by the density of final states. But in practice one can easily take care of this issue and verify whether or not the transition probability is symmetric under time reversal.

As with the discussion of the generalized Curie principle of section II, this generalization of the Kabir criterion does not refer to the Hilbert space structure of the space of states or linearity (or anti-linearity) of various maps. In particular, in the case of quantum mechanics, while it incorporates the standard treatment neatly summarized in section 3 of [1], the results would hold even if, say, the S-matrix were anti-unitary. As with the Curie principle, this generalization may be useful to non-linear generalizations of quantum mechanics. However, in classical mechanics, there are no obvious structures corresponding to the overlap map and the subsequent notion of transition probability. Therefore, unlike our discussion of section II, the present discussion has no implications to classical mechanics.

IV. DISCUSSION

Apart from obvious advantages of inherent to a generalization, already in the context of quantum mechanics, the setting of *general mechanics* presented here serves to bring out the elements and structures that are essential in the discussion of CP and T violation. In particular, neither the linear structure not the details of the Hermitian inner product of the space of quantum mechanical states is essential. Secondly, the primary distinction between C, P, CP and T lies in the fact that while C, P and CP leave the space of 'in' and 'out' states invariant, T maps the incoming states to the outgoing ones. In standard quantum mechanics, this has the implication that while C, P and CP are represented by linear, unitary maps, T is represented by an anti-linear, anti-unitary map. However, from the perspective of *general mechanics* this difference is not primary to the distinction between the Curie and Kabir criteria.

Finally, Ref. [1] also provides a succinct summary of ideas for testing T violations through the measurement of the dipole moment of elementary particles, such as a neutron. I will just add a phenomenological remark. The electric dipole moment is not invariant also under the parity operation P. Therefore, even if one were to observe an electric dipole moment, one cannot *directly* conclude that there is T violation.

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[1] B.W̃. Roberts, Three merry roads to T-violation, talk at the Worksop on Cosmologogy and Time, held at Penn State, April 2013 (pre-print).

[2] S. Weinberg, *Quantum Theory of Fields* Vol 1, pp 244-246 (Cambridge University Press, Cambridge, 1995).

[3] R. F. Streater and A. Wightman, *PCT, Spin, Statistics and All That*, Chapter 4 (Benjamin, New York, 1964).

[4] J. Yngvason and H. J. Borchers, H. J. On the PCT-theorem in the theory of local observables, `arXiv:math-ph/0012020`.

[5] A. Ashtekar and T. Schilling, Geometrical formulation of quantum mechanics, in: *On Einsteins Path*, edited by A. Harvey (Springer-Verlag, Berlin, 1998), 23-66; also available at `arXiv:gr-qc/9706069`.

Response to Pashby: Time operators and POVM observables in quantum mechanics.

Gordon N. Fleming

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Abstract: I argue against a *general* time observable in quantum mechanics except for quantum gravity theory. Then I argue in support of *case specific* arrival time and dwell time observables with a cautionary note concerning the *broad* approach to POVM observables because of the wild proliferation available.

First, a terminological idiosyncracy of mine: I follow the admonitions (which will not be defended here) of Jean Marc Levy-Leblond [10] and Hans Christian von Bayer [22], to drop the term *particle* and call the bosons and fermions of the world, *quantons*.

1. Between Pashby and Hilgevoord

Back in 1998 professor Hilgevoord [9b], extensively referred to by Pashby [15], criticised a long paper I co-authored with Jeremy Butterfield [7], in which we discussed (among other things) Lorentz covariant 4-vector position operators, assigned to space-like hyperplanes, and with operator valued time components. Hilgevoord objected not only to the operator time components, but to the requirement of Lorentz covariance for the position operators as well! I did not then and do not now agree with these objections, for the time components were in no sense independent or general time operators, but supervened on the space components by being linear functions of them and this enabled the covariant transformation property. However, notwithstanding this episode, I think I am more sympathetic to Hilgevoord's objections [9c] to a *general* time operator in quantum mechanics (QM) than Pashby is. I will elaborate on this below.

On the other hand, I agree with Pashby's support of what I will call *case specific* time operators in QM, tentatively interpreted, when non self adjoint, as POVM observables. There are, however, delicate issues regarding the POVM interpretation of observables which I want to discuss in the context of such time operators. But first, my sympathies with Hilgevoord.

2. Time, observables and measurement

There are two brief arguments, other than Pauli's [16], that I would mount against a general, canonical, time operator in QM. They are first, and most importantly: In QM, space and time or space-time, are not, themselves, dynamical systems. QM is a theory of *temporally persistent dynamical systems*, indeed of *eternal* systems, which live in a fixed classical space-time. Unlike Quantum Gravity research or Quantum Cosmology, which seek a QM *of* space-time and must have general, operator valued, space - time observables *per se*, standard QM has no such need. The basic observables of standard QM, represented by self adjoint operators, are designed to answer questions about the possible values, or probabilities for values, of possible properties of persistent physical systems, *at specified times* (or, more relativistically, on specified space-like hypersurfaces). Even so-called unstable systems, which we normally think of as temporally transient, are included in this construal. We need only view the final decay products, the unstable parent quanton and the earlier formation progenitors as the final, middle and initial configurations, respectively, of a spontaneous internal transformation of *the* persistent system.

Second: I follow Ghirardi [8], Pearle [17], Penrose [18] and others in regarding primordial, stochastic state reduction (which we merely exploit in our measurements) as the really serious absentee in current QM. If and when this theoretical gap is filled, via improved versions of one or another of the already proposed schemes, or otherwise, I see it as only enhancing the special status of time in QM. For while state reductions (the exploited ones) can be tailored to specific observables and can have very varied relationships to spatial locations (think of reductions to near momentum eigenstates), they all occur at essentially *definite times,* either (the exploited ones) at times of our choosing or (the primordial ones) at wholly random times or, again, on space-like hypersurfaces. So there would be no question of measuring *when* the primordial reductions occur and trying to measure *just when* a measurement exploited reduction occurs (within the exploiting measurement) would be an instance of measuring a case specific time observable.

This conception of the reality of apparent state reductions may be wrong. If so, and *genuine* state reduction is replaced by an *illusion* induced by something like environmental decoherence [20]; well, that also is an ongoing

temporal process which would not, I think, alter the special status of time in QM.

The upshot is that I think Dirac, whether he miscalculated (as Pashby suggests) or not, was either lucky or wise in not sticking to his original guns [6a] of trying to formulate QM in the extended phase space with the extended Hamiltonian satisfying a constraint equation and with time emerging as an operator. For even without gravity to deal with, and notwithstanding the invaluable contribution of Dirac's later study of constrained dynamical systems [6b], I suspect that such an approach to QM in general would have encountered analogues to the kind of conceptual problems which plague quantum gravity research today. In quantum gravity research these conceptual problems must be faced; in the formulation of QM they would have been and were artificial.

3. Time-energy indeterminacy

While we do not have a general time observable in quantum mechanics, we do have a universal time-energy indeterminacy relation (TEIR) and it is striking how exactly opposite is our traditional interpretation of that relation from Heisenberg's early interpretation, as described by Pashby. While Heisenberg saw ΔT as an indeterminacy in a time of occurrence and ΔE was an *interval* between precise energy values, we now have ΔE as the standard deviation indeterminacy in the system energy while ΔT is the lower bound on the *intervals* defined by $\Delta T_X = \Delta X / |< \dot{X} >|$ for arbitrary observables, X. Derived by Mandelstam and Tamm [12] from the Robertson [19] general indeterminacy relations,

$$\Delta X \Delta E \geq (\hbar / 2) |< \dot{X} >| \ , \qquad (1)$$

the ΔT of their TEIR, $\Delta T \Delta E \geq (\hbar / 2)$, is the time one must wait for expectation values to change by amounts comparable to the corresponding standard deviations. This immediately yields the stationarity of energy eigenstates and, as Aharonov and Bohm [1] pointed out, it places no restriction at all on how quickly one can, in principle, perform an arbitrarily precise measurement of the energy of a physical system! However, for many states of interest, the standard deviation, ΔE, can be infinite and then (1) and the TEIR tell us nothing.

Accordingly, stronger indeterminacy relations have been derived with new time-energy relations among them [5]. Uffink and Hilgevoord [21a] have obtained one of the most interesting versions which I just mention here without further comment.

Let $\hat{\Pi}(E)$ be the projection valued spectral resolution of the Hamiltonian, $\hat{H} = \int E d\hat{\Pi}(E)$. For unit norm states let $W_\alpha(\psi)$, where $0 < \alpha < 1$, be the size of the smallest energy interval, *I*, such that,

$$< \psi \mid \int_I d\hat{\Pi}(E) \mid \psi > = \alpha \,. \tag{2}$$

Let, $\tau_\beta(\psi)$, where $0 < \beta < 1$, be the smallest time displacement such that,

$$< \psi \mid \exp\left(-\frac{i}{\hbar} \hat{H} \tau_\beta(\psi) \right) \mid \psi > = \beta \,. \tag{3}$$

Then it can be shown that,

$$\tau_\beta(\psi) W_\alpha(\psi) \geq 2\hbar \arccos\left(\frac{\beta + 1 - \alpha}{\alpha} \right). \tag{4}$$

In particular, for $\alpha = 0.9$ and $\beta = \sqrt{1/2}$, one obtains, $\tau_{\sqrt{1/2}} W_{0.9} \geq 0.9\hbar$ [9a].

4. Case specific time observables

Now I turn to case specific time observables where I agree with Pashby concerning both the possibility and the desirability of identifying and examining such observables in QM for various times of occurrence or durations.

Concepts of quantum observable times come in at least three forms: (1) times of occurrence (*arrival times*) of specified events, (2) intervals of time (*dwell times*) spent in specified regions or conditions or (3) (*relative times*) of occurrence of one event relative to a reference event. These are of a different nature from the 'property' observables for persistent systems. They acquire their objective indeterminacy from supervening on the property observables. They can be easily motivated within standard QM, beginning with the definition of case specific time operators. Until comparatively recent times such concepts have not received much attention, but are under

intense examination now [13], and, as Pashby suggested, usually lead to non-self adjoint operators.

Perhaps the very simplest (not to say simplistic) example, introduced by Aharonov and Bohm [1], and one of three examples considered by Brunetti et al [3a, c], among Pashby's sources, is the arrival time operator,

$$\hat{T}_0 = -\frac{1}{2}\left(\frac{m}{\hat{p}}\hat{x} + \hat{x}\frac{m}{\hat{p}} \right) . \tag{5}$$

With this operator one can, supposedly, calculate the average time of arrival, at the spatial origin of coordinates, of a free, non-relativistic quanton moving in one dimension. The position of the quanton at *parameter time*, $t = 0$ is represented by the operator, $\hat{x}$, and the momentum, by the operator, $\hat{p}$. Because 0 belongs to the spectrum of $\hat{p}$ and the 'inverse' of $\hat{p}$ appears in (5), $\hat{T}_0$, while symmetric, is not self adjoint. That 'inverse' restricts the domain of definition of $\hat{T}_0$. I think it is worth examining this toy model in some detail.

The motivation for the time operator construction, (5), is just the time dependence of the Heisenberg picture position operator for the free quanton,

$$\hat{x}(t) = \hat{x} + \frac{\hat{p}}{m}t . \tag{6}$$

The expectation value of position is zero at the precise time, $t_0 = -m < x > / < p >$, but this expectation value allows for contributing position eigenvalues that lie far afield from zero. There is no single, precise time for the quanton to arrive (be detected) exactly at $x = 0$, so the time operator, $\hat{T}$, that, hopefully, 'describes' the distribution of possible times is, perhaps naively, taken to satisfy the equation,

$$0 = \hat{x} + \frac{\hat{p}}{2m}\hat{T} + \hat{T}\frac{\hat{p}}{2m} , \tag{7}$$

where the symmetrized product allows for *momentum–time* incompatibility.

$\hat{T}_0$ of (5) is the solution to (7) and, indeed, it fails to commute with the quanton momentum and its position at $t = 0$.

$$[\hat{p}, \hat{T}_0] = i\hbar m\, \hat{p}^{-1}, \qquad [\hat{x}, \hat{T}_0] = (i\hbar m/2)(\hat{p}^{-2}\hat{x} + \hat{x}\,\hat{p}^{-2}) \tag{8}$$

The right hand commutator gives rise to the curious indeterminacy relation, in which the indeterminacy of the time of arrival at the spatial origin of coordinates, $x = 0$, competes with the indeterminacy of the quanton position at the parameter time, $t = 0$. Furthermore, the lower bound on the product of the standard deviations is governed by the expectation value of a function of position and momentum that could well diverge for many states!

From the left hand entry in (8), the momentum-time commutator, we do obtain the expected time-energy commutator,

$$[\hat{p}^2/2m, \hat{T}_0] = i\hbar\,, \tag{9}$$

but this does not conflict with Pauli's argument since the time operator is not self adjoint.

5. The POVM perspective

But just how shall we work with $\hat{T}_0$ in detail, given that it's not self adjoint? Brunetti et al tell us it is maximally symmetric with deficiency indices of 2 and 0. A more familiar account of the non-self adjoint character of $\hat{T}_0$ is provided by examining its continuous spectrum, generalized eigenstates. In the momentum representation they are given by,

$$\xi_t(p) = \sqrt{\frac{p}{mh}}\,\exp\left[\frac{i}{\hbar}\,\frac{p^2}{2m}\,t\right], \tag{10}$$

for the eigenvalue, t (that square root has to be handled carefully!). Notwithstanding the symmetry of $\hat{T}_0$, they are non-orthogonal, with the inner products,

$$<\xi_t \mid \xi_{t'}> = \delta(t-t') + \frac{i}{\pi}\frac{\mathrm{P}v}{t-t'}\,, \tag{11}$$

where $\mathrm{P}v$ denotes principal value. So $\hat{T}_0$ is not subject to a projection valued spectral analysis employing a projection valued measure (PVM). In its place a positive operator valued spectral analysis employing a positive operator

valued measure (POVM) is available. Why is this fact useful in physics and how much difference does it make?

There have been three main sources of the idea that POVMs comprise a valuable generalization of the standard concept of quantum observable. The earliest lies in the work of the physicist-philosopher, Gunther Ludwig [11], who anticipated the utility of POVMs in accounting for the probability distributions that could arise from innovative experimental procedures. Next came the recognition of POVMs as more adequately describing actual laboratory probability distributions due to technological limitations in attempts to implement ideal measurements of standard observables. The book, "Quantum Measurement" by Braginsky and Kahlili [2] is a good introduction to this source. Finally, there is a community of theorists who see in POVMs a vast source of valuable generalized observables that greatly extend our capacity for examining quantum systems. Paul Busch is a leader in this field and the books, "Operational Quantum Physics" [4a], which he co-edited, and "Time in Quantum Mechanics", to which he contributed [4b], are representative. The subject of POVM observables met with severe criticism in early days [21b] and calls for caution still occur [7b] (I will add to them shortly), but the field has weathered the criticism and is very active. The original mathematical work on POVMs is primarily due to Naimark [14].

A POVM for a single observable, X, is defined by a family of bounded, non-decreasing, positive operators, $\hat{P}(x)$, where, $-\infty \le x \le \infty$, satisfying the following conditions: for any, $x_1 \le x_2$,

$$0 = \hat{P}(-\infty) \le \hat{P}(x_1) \le \hat{P}(x_2) \le \hat{P}(\infty) = \hat{I} \,. \tag{12}$$

From these operators one can build the positive operator associated with any given member of a sigma field of Borel sets of the real line.

A PVM is the special case in which the $\hat{P}(x)$ are projection operators, $\hat{\Pi}(x)$, satisfying the further condition that,

$$\hat{\Pi}(x_1)\hat{\Pi}(x_2) = \hat{\Pi}(x_2)\hat{\Pi}(x_1) = \hat{\Pi}(x_1) \,. \tag{13}$$

If such a PVM comprises the spectral resolution for a standard observable, *X*, then the self adjoint operator, $\hat{X}$, for that observable, is just the first moment of the spectral resolution, i.e.,

$$\hat{X} := \int x \, d\hat{\Pi}(x) . \tag{14}$$

It follows from (13) that for any function, $f(x)$,

$$f(\hat{X}) = \int f(x) \, d\hat{\Pi}(x) . \tag{15}$$

In particular, for a unit step function,

$$\theta(x - \hat{X}) = \hat{\Pi}(x) , \tag{16}$$

and the PVM spectral resolution is recoverable from the first moment operator. Also any PVM provides the spectral resolution for some self adjoint operator.

Nothing of the kind holds for POVMs that are not PVMs! For such POVMs there is no condition analogous to (13). The positive operators in such a POVM need not even commute among themselves! Consequently, the POVM spectral resolution is usually *not recoverable* from the first moment operator!

Suppose we have a POVM, $\hat{P}_0(t)$, which provides a generalized spectral resolution for our time operator, $\hat{T}_0$. The mathematical meaning of this statement entails that two conditions must be satisfied. The first is that $\hat{T}_0$ and the first moment operator, $\int t \, d\hat{P}_0(t)$, have the same 'matrix elements', i.e.,

$$< \vartheta, \hat{T}_0 \psi > = \int t \, d < \vartheta, \hat{P}_0(t) \, \psi > , \tag{17}$$

for all ϑ and any ψ in the domain of definition for $\hat{T}_0$. The second condition for the POVM is that the squared norm of the action of $\hat{T}_0$ equals the expectation value of the *second moment* operator of the POVM, i.e.,

$$\left\| \hat{T}_0 \psi \right\|^2 = \int t^2 d < \psi, \hat{P}_0(t) \psi > . \tag{18}$$

In the QM application of POVMs , the probability for a measurement of $\hat{T}_0$ to yield a time lying between t_1 and t_2 would be given by,

$$\wp(t_1 \le t \le t_2) = < \int_{t_1}^{t_2} d\hat{P}_0(t) > = < (\hat{P}_0(t_2) - \hat{P}_0(t_1)) > . \qquad (19)$$

The broadest use of POVMs in QM is not to provide generalized spectral resolutions for observables identified with non-self adjoint operators, as we are now considering, but to *define* generalized observables in terms of a POVM directly via (19) alone.

But now consider the following *three parameter family* of POVMs, built upon some hypothetical $\hat{P}(t)$, where $0 \le a < 1$, $\lambda > a$ and τ, a time, is arbitrary,

$$\hat{P}_{a,\lambda,\tau}(t) := a\, \hat{P}(\lambda t + \tau) + (1-a)\, \hat{P}\left(\frac{\lambda(1-a)t - a\tau}{\lambda - a} \right) \qquad (20)$$

If the $\hat{P}_{a,\lambda,\tau}(t)$ are all arrival time candidate POVMs, they must be time translationally covariant, i.e.,

$$\exp[(i/\hbar)\hat{H}\tau]\, \hat{P}_{a,\lambda,\tau}(t)\, \exp[-(i/\hbar)\hat{H}\tau] = \hat{P}_{a,\lambda,\tau}(t+\tau) \qquad (21)$$

This can hold only if $\lambda = 1$. But both dwell time and relative time observables would be invariant under parameter time translation. Still, for dwell times we would want to require $\tau = 0$.

Regardless of the *kind* of time observable the POVM family, (20), is considered for, the first moment operators for *all* those POVMs are equal,

$$\int t\, d\hat{P}_{a,\lambda,\tau}(t) = \int t\, d\hat{P}(t) . \qquad (22)$$

Consequently, if $\hat{P}_0(t)$ belongs to the family (20), they all have first moment operators with the same matrix elements as our $\hat{T}_0$! For any given quantum state, they all give the same answer to the question, 'When is the average time of arrival at the origin?'.

The second moment operators of the various POVMs in (20) are not the same and their expectation values vary from the smallest, provided by $\hat{P}(t)$,

to larger values that increase without bound as $a \rightarrow 1$. So under our assumption about $\hat{P}_0(t)$, that it belongs to the family in (20), at most a few members of the family will satisfy (18) while all members satisfy (17). In fact, since our time operator is *maximally* symmetric, it is known that only the one member of (20), $\hat{P}_0(t)$, will satisfy (18). While this is good news for our time operator, because of uniqueness, it also means that all the other POVMs in the family can not provide generalized spectral resolutions for any symmetric operator, whatsoever!

To see that, let the state vectors, η_k be an orthonormal basis in the state space. Then satisfying both (17) and (18) requires,

$$\Sigma_k \left| < \eta_k, \int t \, d\hat{P}_0(t) \psi > \right|^2 = < \psi, \int t^2 \, d\hat{P}_0(t) \psi > . \tag{23}$$

All the members of (20) yield the same left hand side. Only $\hat{P}_0(t)$, among them, yields the correct right hand side. So only $\hat{P}_0(t)$ provides a spectral resolution of the time operator, $\hat{T}_0$, or of any symmetric operator. Note that if $\hat{P}_0(t)$ was a PVM instead of just a POVM, (23) would not be a *requirement* at all, it would be an identity!

Notwithstanding the fact that within the family, (20), only $\hat{P}_0(t)$ can, satisfy (23), we can still, tentatively, regard the POVMs as defining time observables, $T_{a,\lambda,\tau}$, in the broad sense. The squared standard deviation for these observables would be defined by,

$$(\Delta T_{a,\lambda,\tau})^2 := < \int t^2 \, d\hat{P}_{a,\lambda,\tau}(t) > - < \int t \, d\hat{P}_{a,\lambda,\tau}(t) >^2 \tag{24}$$

Setting $\lambda = 1$ for an arrival time observable, detailed examination of (20) results in,

$$(\Delta T_{a,1,\tau})^2 \geq (\Delta T_{a,1,0})^2 + a\tau^2 / (1-a) . \tag{25}$$

The POVM, $\hat{P}_{a,1,\tau}(t)$, yields no eigenstates at all for the observable, $T_{a,1,\tau}$ unless $a = 0$ or $\tau = 0$! Since $\hat{T}_0$ does have continuous spectrum eigenstates (see (10)) it follows that $\hat{P}_0(t)$, belonging to (20), must equal

$$\hat{P}_{a,1,0}(t) = \hat{P}_{0,1,\tau}(t) = \hat{P}(t) . \tag{26}$$

Regarding the others, $\hat{P}_{a,1,\tau}(t)$; is it physically reasonable to admit, as observables, POVMs that have *no* (generalized) eigenstates throughout the state space? For the affirmative, see three paragraphs below.

The dwell time case, in which λ may vary but $\tau = 0$, leads to a similar result in which $T_{a,\lambda,0}$ can only have an eigenstate for the eigenvalue, 0, unless $a = 0$ or $\lambda = 1$. The relative time case is more nuanced.

Before dismissing these examples as merely bizarre curiosities, bear in mind that I just cobbled (20) together for this workshop and, very probably, it just scratches the surface of ways in which one can build POVMs, all of which share the same first moment operator. If there are much more varied ways of doing that, it seems likely to lead to instances of the query, "Which one?". The definition of $\hat{T}_0$ via (7) was tentative, after all.

From PVM observables one can extract *everything* from the first moment operator, even the eigenstates. From a POVM observable, interpreted broadly, one can not even know, from the first moment operator, if there *are* eigenstates! This makes me wary of the broad approach to POVM observables. The POVM theoretical community, however, regards a particular subclass of POVMs devoid of eigenstates as very important. Called *informationally complete*, these POVMs yield probability distributions that distinguish between any two distinct quantum states [4a]. No PVM can do that.

Returning to Pashby and Brunetti et al: the latter, as indicated by Pashby, explicitly construct the POVM that corresponds to $\hat{T}_0$, according to (17,18)[3a], and their construction, while natural and physically plausible, would not be uniquely compelling, if they hadn't known what operator, $\hat{T}_0$, they were after (see Pashby's footnote 11 for differing interpretations of the POVM construction). Elsewhere they show [3b] that time translationally covariant POVMs lead to an indeterminacy relation for arrival time observables alone! Not a time-energy indeterminacy relation, but a *time* indeterminacy relation. The standard deviations of their time observables are never less than a universal constant divided by the *expectation value* of the system energy! Accordingly, the $\hat{T}_0$ generalized eigenstates, mentioned above, (10), are the limits of finite norm states with energy expectation values that grow without bound in the limit.

As case specific time observables gain importance from the POVM approach to time in QM, issues of the sort considered here will have to be further clarified. An approach to these issues may have recourse to Naimark's theorem [14] , which Brunetti et al exploit in their constructions, and Pashby mentioned. Naimark showed that every POVM is the projection from a larger Hilbert space of a PVM. This PVM would be the spectral resolution of a self adjoint operator in the larger Hilbert space which is then projected down to our first moment operator. Usually the larger Hilbert space is regarded as having mathematical significance only, but it can take the form of an Hilbert space for a supersystem containing the system of interest as a subsystem. I suspect that it would be advantageous to be able to interpret the supersystem and the larger Hilbert space, physically. Still, each of my $\widehat{P}_{a,\lambda,\tau}(t)$ POVMs would lead to different PVMs in (different?) larger Hilbert spaces and different, self adjoint, 'time' operators, all of which would project down to a first moment operator with the same matrix elements as our $T_{\mathrm{a},\lambda,\tau}$. So again the question looms: "Which one?".

For both case specific time observables and others, many theorists are enamored of POVMs because of the panoramic garden of delights they seem to offer. While delights there may be, the garden is not without weeds!

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Reference and Analysis in the Study of Time

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In his presentation, Alexis de Saint Ours often appeals to the writings of Carlo Rovelli and Julian Barbour, examining their relational, empirical, referential accounts of time. In Barbour's essay, "Relational Aspects of Time and Space," Barbour urges us to pay attention to "the discrepency between theory, in which change of variety is referred to a uniform standard, and actual practice, in which change and variety are in fact referred, not to space and time, which remain invisible, but to other variety…Ultimately, the reason why perfect uniformity cannot serve as the practical basis of a quantitative science is that perfect uniformity is nothing. As Leibniz said, 'Things which are uniform and contain no variety are never anything but abstractions, like time, space, and the other entities of pure mathematics.' You cannot measure change of variety against uniformity, because that is trying to compare something with nothing, whereas it is only possible to compare one thing with another thing." (p. 253) Here, however, Barbour is too severe. The abstractions of pure mathematics are constantly brought into rational relation with the referential discourse of mechanics concerned with clocks, atoms and large astronomical systems, despite the fact that the uniformities of pure mathematics are not empirically grounded. The issue, I think, is how scientists bring these two kinds of discourse into relation; good science needs them both. So Barbour is wrong to demand that referential discourse do the work of analytic discourse.

The parameter time, t, has haunted physical theories since Newton defined it, at the beginning of the Principia, as "absolute, true, and mathematical time, [which] of itself, and from its own nature, flows equably without relation to anything external," immediately distinguishing it from "duration: relative, apparent, and common time, … some sensible and external (whether accurate or unequable) measure of duration by the means of motion, which is commonly used instead of true time; such as an hour, a day, a month, a year." Thus it seems that there are two ways to investigate time: a priori as a condition of the intelligibility of things, and a posteriori in terms of the empirically determinable periodicity of things. (Another feature of time is its unidirectionality, apparent in the irrevocability of things; notably, Newton locates its asymmetry in his description of a priori time.)

In science as in ordinary life, in order to think well we must be able to refer successfully, so that we can show publicly and clearly what we are talking about. And we must analyze well (here I invoke Leibniz's definition of analysis as the search for conditions of intelligibility): we must discover productive and explanatory conditions of intelligibility for the things we are thinking about. In order to evaluate whether our means of analysis are really productive and explanatory, we need to be able to denote—publicly and clearly—what we are considering. And in order to check whether our ways of referring are really public and clear, we must set the object of investigation in a more abstract discursive context where we can study it deeply and broadly. Sometimes one task is more difficult, sometimes the other, sometimes both. The tasks themselves are very different, so it is not surprising that they generate different kinds of discourse. In referential discourse, we do our best to honor the extra-discursive world as what it is, with the best empirical means at our disposal; in analytic discourse, we treat the world abstractly, and totalize, simplify, idealize or infinitize it in the many ways that discourse allows.

The advantage of analytic discourse is that it is great for systematization and explanation, but tends to unfocus the specificity of things, to make them ghostly. The advantage of referential discourse is that it does better justice to the rich, specific variety of things, but it often loses its way in the forest of observation because of all those trees. In sum, science does its work best when we refer and analyze in tandem.

The kinds of representations that make successful reference possible and those that make successful analysis possible are not the same, so that significant scientific work typically proceeds by means of heterogeneous discourses that must be rationally reconciled without one collapsing into the other. The growth of scientific knowledge often stems from the work of reconciliation, whose fine structure has not received the attention it deserves. It has escaped the notice of many philosophers of science, perhaps because they are so deeply influenced by logic, which must impose homogeneity on the arguments it formalizes, so that they neglect the opportunities offered by, and the constraints imposed by, discursive heterogeneity in historically central reasoning. Philosophers need to pay more attention to this aspect of scientific practice, how scientists bring disparate discourses (and modes of representation) into rational, and productive, relation.

But what about the scientific investigation of time? Referential discourses about time include Leibniz's account of time as following upon—as posterior to—the relations of existing things, an account which (given his characterization of monads) guarantees the causal connectedness, continuity and lawfulness of physical processes, as well as their temporal asymmetry. In Leibniz's universe, everything strives, headed towards the future. Conversely, there is Boltzmann's referential definition of time, used in his argument that the apparent asymmetry of physical processes in time, suggested by the principles of thermodynamics, can be

explained away if the macroscopic description of the situation is replaced by a molecular description: heat phenomena (the unidirectionality of the increase of entropy) can be reduced to and redescribed as the collisions of large numbers of particles, governed by the laws of Newtonian mechanics. The second law of thermodynamics is a statistical regularity, not a universal and necessary law like the laws of Newtonian mechanics. We are misled by the fact that we apprehend events at the macroscopic level via the approximations provided by our sense organs. In the first case, the insistence on defining time referentially results in an account of time as asymmetrical; in the second case, an equally referential account leads to the conclusion that the asymmetry of time is only apparent.

Another example is the standardization of clocks in terms of ephermeris time (reckoned in terms of the movements of the sun, moon and planets, to avoid the unpredictable irregularities of the mean solar time scale), adjusted both with respect to standards developed in relation to the cesium atomic clock, and with respect to relativistic considerations. (Julian Barbour suggests that we could move from defining ephemeris time in terms of our solar system, up to the whole cosmos—at the least the whole observable cosmos.) Conversely, other modern cosmologists suggest that we use the curvature of the universe as an observable that can stand in for time. In the first case, again, the insistence on defining time referentially results in a definition of time in terms of a clock that is essentially periodic; in the second case, it results in a definition of time that is asymmetrical and unidirectional.

What if we treat time analytically, as a condition of intelligibility of everything? Unlike his opponent Leibniz, Newton invokes analytic time instead of referential time. In Newtonian mechanics time is geometrized first of all as a line. In the diagrams of the Principia, the line of inertial motion (a virtual motion un-deflected by a center of force, nowhere realized in the

universe) represents both the spatial displacement of a particle upon which no forces act, and the 'equable' unrolling of time. Because Newton presupposes that time is uniform and linear, and given prior to the creation of bodies and forces, he can let it play the role of independent variable with respect to which other physical parameters are differentiated and integrated. By assigning an origin and axes to the geometry of space, Newton also (as we would say now) identifies the Euclidean line with the real numbers; this convention guarantees the directionality of the line. So it seems as if Newtonian time is unidirectional. However, all the laws of Newtonian mechanics are time reversal invariant. That is, if there is an initial event A that must lead by the laws of Newtonian mechanics to another event B, then it is also the case that the event B′ (in which all particles are in the same configuration as in event B, but the velocities of all the particles are reversed) must lead to A′ (in which all particles are in the same configuration as in event A, but the velocities of all the particles are reversed). So Newton's formal treatment of time is ambiguous.

By contrast, Sadi Carnot treated time analytically in his Réflexions sur la puissance motrice du feu, and formulated his second law of thermodynamics to assert that the entropy of an isolated system not in equilibrium will tend to increase over time. Thus in Newton's analytical treatment of time, the temporality of mechanical systems seems to have no intrinsic direction, but in Carnot's treatment it does.

Recall that Boltzmann claimed that his referential reduction of (macroscopic) heat phenomena to (microscopic) Newtonian systems explained away the illusion of the arrow of time. But Leibniz's referential reduction of Newtonian absolute time to relational time asserted and indeed explained the arrow of time. In the theorizing of contemporary cosmologists, the referential reduction of the parameter time to the measurement of the changing curvature of the

universe, or to the ephemeris time of the great clock of the universe, is similarly non-commital: in the first case we get a unidirectional time, in the second case we do not. In sum, we find that scientists who wish to define time analytically do not agree about its fundamental nature, and neither do scientists who wish to define time referentially. The arguments of scientists who define time referentially (like Barbour and Rovelli) in order to clear up the confusions of scientists who wish to define it analytically do not in fact settle the disputes, and the conflicts among scientists who define time referentially are also not settled by their abstracter colleagues who deploy analytical definitions.

However, the philosopher of science need not counsel skepticism. Our best hope of understanding time may lie in looking at what happens when referential and analytical discourses fail to be wholly reconciled: to what extent are they unified (and how is that unification possible) and where and why does that unification fail? Thus we may learn something important about time by studying the debates between Leibniz and Newton, or the current attempts of scientists to integrate quantum mechanics and general relativity. Scientific language used in the study to elaborate and systematize abstract thought is, clearly, very different from language used by scientists working in the laboratory, field and observatory. Texts that announce important ideas, bringing two or more spheres of activity into intelligible relation, are therefore typically heterogeneous and multivalent, a fact that has been missed or misunderstood by philosophers who equate rationality with the kind of discursive homogeneity required by formal logic.

Philosophers of science need to ask new questions that bring the work of combination itself into focus. Is there a useful taxonomy of strategies of combination among heterogeneous scientific discourse about the nature of time? How does it contribute to the growth of knowledge? If the tasks of analysis and reference are often disparate, we may expect that, for

example, the records kept by astronomers (even when their work is informed by theory) will differ from the theorizing of physicists and natural philosophers (even when they are concerned primarily with celestial systems). The modes of representation and the idioms of mathematical and scientific expression will differ; and the explication and organization required of natural language will differ from one task to the other. What we can then expect are composite texts in both kinds of endeavor; and the nature of this composition can suggest a preliminary taxonomy of 'strategies of integration.' We need a better account of these strategies. The kinds of representations that make successful reference possible and those that make successful analysis possible are not the same, so that significant scientific (and mathematical) work typically proceeds by means of heterogeneous discourses that must be rationally reconciled without one collapsing into the other. The growth of scientific knowledge often stems from the work of reconciliation, whose fine structure has not received the attention it deserves.

So let us return once more to Julian Barbour. In his paper, "The Development of Machian Themes in the Twentieth Century," Barbour identifies three main strands in Mach's thought: a criticism of Newton's concepts of absolute space and time; a conviction that the task of science is not to set up theories about the world; and the suggestion that the universe can only be understood properly if it is treated as a whole. Thus Mach urges scientists to reformulate mechanics on a relational basis and to stay 'grounded,' establishing empirical connections among directly observable phenomena. These convictions go along with, oddly, a rather mystical commitment to holism. This combination led to Mach's principle (as Einstein called it) that locally observed inertial properties of particles arise not from absolute space but from "the combined effect of all the dynamically significant masses in the universe."

As the great dialectic of science and the philosophy of science proceeds from the mid-19th century to the present, Mach (and Poincaré) play a significant role, and so too do Rovelli and Barbour. I suggest, however, that all three, like Leibniz, cannot keep from elaborating an analytic discourse that stands in problematic (but productive) relation to the referential discourse they focus on, although they try to obscure the intervention of that analytic discourse. Such an account would take me beyond the bounds of this response, but one feature of Barbour's discourse mostly clearly betrays its hybridity. That is his insistence that his theory must apply to "the complete universe." (Recall his conception of ephemeris time calculated on the basis of the whole universe.) This is precisely the kind of theorization that analytic discourse offers, along with abstraction and uniformization, as Kant warned us: reason (which is about discourse, while the understanding is about perceived reality) seeks the full series of conditions and pretends that we can think them altogether. Barbour is here invoking the system of cosmological ideas, which Kant goes after in the First Antinomy, in the Transcendental Dialectic of the Critique of Pure Reason. In sum, this aspect of Barbour's theory, its insistence on the unity and organization of the universe, does not come from the patient measurement of stars by astronomers nor the careful recording of the ticks of the cesium atom; it comes from the speculation of analytic discourse which seeks the conditions, indeed the ultimate conditions, of the things of this world. So he too, like all good scientists, while insisting on the importance of empirical methods and the irreducible diversity of things, must also bring reference and analysis into rational relation.

This paper is the first draft of a response, and so far without a list of references, which will be added later as it is developed in tandem with the paper to which it responds.

Local and global definitions of time: Cosmology and quantum theory

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Abstract

I will give a broad overview of what has become the standard paradigm in cosmology. I will describe the relational (à la Leibnitz) notion of time that is often used in cosmological calculations and discuss how the local nature of Einstein's equations allows us to translate this notion into statements about 'initial' data. Classically this relates our local definition of time to a quasi-local region of a particular spatial slice, however incorporating quantum theory comes at the expense of losing this (quasi-) locality entirely. This occurs due to the presence of two, apparently distinct, issues: (1) Seemingly classical issues to do with the infinite spatial volume of the universe and (2) Quantum field theory issues, which revolve around trying to apply renormalization in cosmology.

Following the cosmological principle - an extension of the Copernicus principle - that physics at every point in our universe should look the same, we are lead to the modern view of cosmology. This procedure is reasonably well understood for an exactly homogeneous universe, however the inclusions of small perturbations over this homogeneity leads to many interpretational/ conceptual difficulties. For example, in an (spatially) infinite universe perturbations can be arbitrarily close to homogeneous. To any observer, such a perturbation would appear to be a simple rescaling of the homogenous background and hence, physically, would not be considered an inhomogeneous perturbation at all. However, any attempt to choose the physically relevant scale at which perturbations should be considered homogeneous will break the cosmological principle i.e. it will make the resulting physics observer dependent. It amounts to 'putting the perturbations in a box' and a delicate practical issue is that the universe is not static, hence the scale of the box will be time dependent. Thus what appears physically homogeneous to an observer at one time will not appear so at another.

This issue is brought to the forefront by considering the canonical (space and time rather space-time) version of the theory. The phase space formulation of General Relativity, just as for any other theory, contains the shadow of the underlying quantum theory. This means that, although

the formulation is still classical, many of the subtleties that are present in the quantum theory are already apparent. In the cosmological context the infinite spatial volume renders almost all expressions formal or ill-defined. In order to proceed, we must restrict our attention to a cosmology that has some finite spatial extent, on which our relational notion of time is everywhere definable. In particular, this would constrain the permissible data outside our 'observable universe'.

This difficulty is an IR or large (spatial) scale issues in cosmology, however in addition there are UV or short (spatial) scale problems that need to be tackled. There are the usual problems of renormalization, which are further complicated by the fact that the universe is not static. In the cosmological setting this leads to new IR problems which again prevent one from taking the spatial extent of the universe to infinity. The physical relevance of this problem, the consequence for defining a time variable, and the distinction of homogeneous and inhomogeneous IR issues will be discussed.

1 Physical cosmology

The modern view of cosmology is a profoundly phenomenological one. One interprets observational data in terms of approximate solutions to Einstein's equations assuming the matter content can be thought of as a fluid. The observations themselves are often only proxies for the desired observable, themselves containing various levels of approximations or assumptions. In addition, cosmology represents an enormous extrapolation of scales - 15 orders of magnitude or more - in energy density, acceleration, length and time from the more familiar laboratory scale physics that has been tested. Given these caveats, one should not expect that cosmological observations perfectly agree with our currently established understanding of physics, and indeed this is not the case.

Observations have shown that on galactic scales, evolution does not follow from Einstein's theory for any know matter content. Whilst there is some debate as to whether this Dark Matter may, in fact, be due to our lack of understanding of gravitation on these scales (MOND, TeVeS etc.) or perhaps a consequence of our approximations, it is generally believed that it is a phenomenological description of some, as yet undiscovered, types of matter. Indeed it would be rather more fantastic to think that all the particles in existence have properties that can be probed in the laboratory with current technology, and that there is nothing left to discover.

On much larger scales, approaching the size of the observable universe, it has also been shown that the expansion of space does not follow our naive expectations. The phenomenological description of this effect is Dark Energy and its underlying explanation is rather more open to debate, with possibilities ranging from a new fundamental scale, modifications to general relativity, misuse of averaging in cosmology, undiscovered types of matter with radically different properties to those we currently observe etc. In addition to these possible classical resolutions, there is also much work on the possibility that Dark Energy is a consequence of the under-

lying quantum theory.

At this point one may start to be concerned. We have a phenomenological description of our universe that can match observations only if we postulate two new sources of matter/ energy, that together account for approximately 96% of the total current matter/ energy content of the universe. However, whilst we may know little of the underlying physics of these new sources, there are a myriad of observations that all agree on their overall cosmological effect. These observations paint a coherent picture of the evolution of the universe over the last 14 billion years or so, a picture that is continually being tested by new observations and predictions. Whilst we may not know what it is made of, we can at least see the whole elephant!

This wealth of every more precise observations, and the remarkable success of the phenomenological approach, allows us to tackle the undelrying conceptual challenges inherent in cosmology, with the real possibility of testing our conclusions in the near future. An excellent example of this was the question of why regions of space that have not been in causal contact look (statically) the same? How could different regions 'know' the properties of the others? One explanation is that there was an earlier period Dark Energy-like expansion - known as Inflation - which ensures that the entire observable universe was, in fact, in causal contact. Of course there are other possibilities, such as a universe with an infinite past (for example a bouncing or cyclic cosmology), however different solutions to this conceptual difficulty led to different predictions for other observables. Whilst nothing has yet been completely ruled out, the current observations are able to distinguish between the most basic versions of these (and other) possibilities.

2 Spatial extent and time

Of the many important conceptual challenges facing any approach to cosmology I will concentrate on two: the spatial 'size' of the universe and the notion of time. Here I will argue that it is very likely that these two issues are intimately related, with the relation coming from the underlying quantum gravity formulation of cosmology (what ever it may be).

2.1 Time

Here, in line with the phenomenological under pinnings of cosmology, I shall consider a very practical notion of 'time', taking it to be given entirely by relative change. I will consider time as a relational concept, which can be defined for any two sub-systems. If one of these sub-systems is considered a 'clock', the state of the others evolve with respect to the internal time given by the evolution of this clock. For example, consider a system containing as sub-systems a swinging pendulum, a revolving wheel and an observer capable of observing both. This observer may choose to use the pendulum as a clock and ask questions such as "How many revolutions of the wheel are there in one cycle of the pendulum?" Of course, the observer may equally decide to use the wheel as a clock and

ask "How many cycles of the pendulum are there per revolution of the wheel?" The choice of which sub-system the observer chooses to use as a clock is irrelevant, only the relative change between them is important. Note that the inclusion of the observer here is important. If neither the pendulum nor the wheel were in motion, the observer would be unable to use either device as a clock, however the fact that the observer interacts with them (via observing), implies that there must be another sub-system that could be used for the observer to measure time. Namely the field propagating this interaction (i.e. photons traveling from the observer to the devices). Thus, should the clock be static, it may not be a useful means of measuring time, however the observer's act of seeing the clock means that time can still be defined, simply by using alternative degrees of freedom to register relative change. Consider the following example. A person observes a clock to read mid-day. A second, distinct, observation is subsequently made and again the clock reads mid-day. There are three conclusions that the observer might draw,

- $12N$ hours have elapsed between observations, for N a strictly positive integer,
- the clock is in fact broken and some indeterminate period of time has passed,
- no time has passed between observations.

The last case can be ruled out by the fact that the observer carries out *two distinct* observations. That is, other degrees of freedom - say the photon propagation from the observer to the clock - distinguish the two events. Similarly the first possibility does not present any difficult, since although this particular observation may produce in the same result - the clock reads mid-day - other observables would not (for example the charge in the battery). Finally the second possibility is the case in which the sub-system the observer has decided to use as a clock is static. Clearly this does not make for a useful method of measuring the passage of time, however, equally clearly, it does not mean that time is not definable. Other degrees of freedom that are responsible for the interaction of the clock and the observer could be used to define this relational notion of time. In practice this means that when we are choosing a particular degree of freedom of our system to be a clock, we want to ensure that its evolution is monotonic, at least for the period of our observation. If it is ever static, we must choose a different degree of freedom. In the event that *every* degree of freedom of the system is static, then, indeed, relative time would not be definable, however in such a situation the observer would also have to be static and evolution would have no meaning.

Typically we want to choose a clock whose evolution is largely unaffected by the state of the other sub-systems (we might prefer that our watches tick at a uniform rate despite swinging of our arms for example), however this needn't be the case. In cosmology for example, we may choose to use the expansion of the universe as a clock and ask "What is the state of some matter content when the expansion is such and such?" or "How does this state change when the expansion varies from x to y?". The evolution of the matter content is highly dependent on the expansion of the universe, however we can still describe the evolution of one

parameter in terms of another. In cosmological notation this amounts to writing $\phi(a)$, where a is the scale factor and ϕ is some particular matter content. This practical, relational notion is very typical for cosmological observations, where one regularly refers to time via red-shift (closely related to expansion) and considers the relative change of energy density, temperature, galaxy number density etc. with respect to red-shift. In early universe cosmology it is also common to consider the evolution of matter fields (in particular the inflaton) as defining a clock, thus writting (for example) $a(\phi)$.

2.2 The relation to spatial extent

To demonstrate the plausibility of a link between spatial extent and temporal evolution consider the following. General relativity provides us with local equations, the solution of which in any finite region can be calculated from data specified on the boundary of that region. Typically we consider foliating the manifold using 3-dimensional slices, labeled by a parameter t, in which case the required data can be specified at an 'initial' slice. The solution in any finite region on any other slice requires data to be specified only on a (different) finite region of this initial slice. For example, in order to predict the motion of an apple as it falls, one does not need to specify the data describing the sun at the time the apple is dropped (provided the apple hits the ground within 8 minutes - the light travel time from the sun to the Earth).

This heuristic explanation applies to any system that is 'well-posed' - of which general relativity is an example. It essentially says that solutions can be calculated by 'evolving' initial data in time and that if we are interested only in a finite temporal extent, then we need only specify a finite spatial extent of initial data (provided the speed of light is finite). The passage of time for some (finite) system can then be operationally defined by the expanding spatial region of initial data required to uniquely specify the evolution of that system. That is, given the state of an observable and a clock at some instant, to know how the observable will change in relation to the changing clock we need only specify the initial data required to calculate this evolution. The longer the evolution, the more initial data that is required.

Consider for example an observer and a clock on some slice Σ_t and suppose we provide initial data on a spatial slice Σ_i, prior to the slice containing the observer (i.e. some time before t). Both the observer and clock are a (presumably extremely complicated) solutions to the coupled general relativity and (classical) particle physics evolution equations, and they are given uniquely by the data contained within a certain (finite) region of the initial slice, $S_t \subset \Sigma_i$. Given this data, there is a unique solution describing the observer's evolution in time, as measured by the changing of the clock, from the initial slice to the slice labeled by t. Now we wish to understand what is required in order for this observer to evolve from the state of the clock given at t to the state at $t + \delta t$. The required information is simply the data specified in $S_{t+\delta t} \subset \Sigma$ and so we can consider the evolution in time of this particular observer, from t to $t + \delta t$, to be given by the additional data required in order to go from S_t to $S_{t+\delta t}$

i.e. $S_{t+\delta t} \cap (S_t)^c$, which is entirely defined on the initial spatial slice.

This transforming of the temporal extent of a solution to the spatial extent of the initial data is made possible by the existence of a fundamental velocity - the speed of light. This leaves aside issues such as singularities at which both spatial and temporal extent lose much of their meaning, however it does provide a useful definition of time, for all practical purposes, throughout most eras of cosmology. This entire discussion is a roundabout way of describing the very familiar concept of 'the observable universe' often used in cosmology - how much of the universe at some earlier time is in principle observable to us today? In the notation above, the current observable universe is just S_{today}, where the 'initial' data is specified on some slice given very early in the cosmological evolution (close to the big-bang singularity).

3 Infinite spatial extent

If we are interested only in evolution of a spatially finite system for finite times, only a finite region of the initial spatial slice is relevant, the rest is unimportant for this evolution and in particular the total spatial extent of the initial slice is not relevant. Of course, if we want to evolve a solution for infinite time, one must provide data on the entire spatial slice and in this case the total spatial extent of the universe is relevant. If our universe were exactly spatially homogeneous this question of spatial extent becomes redundant, even for evolution between infinite times. Since every point on the initial slice is equivalent, every point on any other slice must also be equivalent to each other (although there may of course be differences between slices). In this spatially homogeneous situation, all our solutions depend only on one function that relates any spatial region on different slices, the scale factor $a(t)$.

Before we move away from the classical theory it is worth noting that the above discussion also means that, for practical purposes, it does not matter whether the spatial extent of the foliating slices are infinite or not. Only a finite region of the 'initial' slice is relevant for the evolution of any particular observer (such as ourselves). Thus in classical cosmology there is essentially no difference between a universe that is spatially infinite and one that has some finite spatial extent, provided it is larger than the current size of the observable universe. Indeed observations can put limits on the minimum size of the spatial slice (and to some extent its topology), but these observational limits cannot extend beyond the observable spatial size of universe. There is of course a very important conceptual difference between a spatially infinite and a spatially finite universe, in particular when it comes to trying to define what constitutes a 'special' position or conditions. There are also some technical differences that effect the practical calculations typically used in cosmology such as Fourier series vs. Fourier transforms. However for cosmological observations the distinction is unnecessary.

A word on causality. The above discussion is very similar to the notion of causality, however it is not quite the same. Causality would usually be defined as follows. Consider a set of initial data on some slice and add to

it a localized disturbance. Causality now describes the fact that, under evolution, the consequences of this disturbance propagates slower than (or equal to) the speed of light. It is certainly true that causality implies that any disturbance outside our observable universe cannot effect us, and hence that there is essentially no difference between an (spatially) infinite and a (spatially) finite universe. However the converse is not true. As we will see, a theory may be non-local and hence 'know' if the universe is infinite or not, whilst still obeying causality.

Finally, note in particular that our relational definition of time can be defined for any finite evolution, given by a finite initial data S_t, completely independently of whether the spatial slice is finite or infinite (provided it is sufficiently large to encompass S_t). Indeed it can be defined completely independently of whether regions on the initial slice, outside S_t have any definable notion of time at all. In this sense, relational time, for finite evolution, can be defined 'locally' (i.e. it is insensitive to the global properties of the spatial slice).

4 The consequences of quantum theory: Hamiltonian formulation

So much for classical cosmology, what changes when we try to incorporate quantum theory? Quantum theory is not a local theory and any attempt at quantization fails when we have a spatial slice that is infinitely large. Indeed, this difficulty can be seen even before going to the quantum theory, by considering the Hamiltonian or action formulation of the classical theory. Both these frameworks contain the 'shadow' of the underlying quantum theory and already at this (classical) level the problem of an infinite spatial extent arises. The Hamiltonian generates evolution of the system via Hamilton's equations which we may hope to use as a practical definition of time, since they describe how the observables evolve with respect to one and other, one of which we may choose to refer to as a clock. However the Hamiltonian is defined as an integral over the entire initial spatial slice. In a sense, the Hamiltonian is defined so that it can generate the evolution of all possible regions of the initial slice for all possible times. The only ways for this to be well-defined are that the initial slice has a finite spatial extent with specified boundary conditions or that the functions giving the Hamiltonian vanish sufficiently quickly 'at infinity' (both possibilities also require that the initial data be suitably regular everywhere). The latter is the usual condition used in mechanical systems, where the spatial extent of the system being consider is obviously finite, however in cosmology this would mean that our position in the universe is somehow 'special' which breaks cosmology's guiding principle and in any case, precludes the existence of a solution which is exactly homogeneous. Thus we are forced to consider only cosmologies in which the spatial extent is finite. Provided this finite spatial extent is sufficiently large, it will not yet be observable, however this does alter our description of time. The evolution of some finite region may explicitly depend only on the initial data in some finite region of the initial slice, however for it to be defined at

all, initial data on the entire initial slice must be specified. The evolution of our falling apple may not depend on the state of the sun at the time the apple drops, *provided* the sun's contribution to the Hamiltonian is finite.

It is important to realize that although the Hamiltonian approach describes the classical evolution of the system, giving the same evolution described by Einstein's equations, it contains additional regularity conditions coming from the underlying quantum theory. These regularity conditions are non-local and we can no longer fully describe the evolution of a system independently of the evolution of other causally disconnected systems. It is not that the evolution of two causally disconnected systems are dependent on each other, however to be able to evolve one we must know that the evolution of the other exists and meets the regularity condition. Put another way, the existence of (this practical definition of) time for any system requires that the same definition exist for all systems. This additional requirement arises from the quantum theory, which then leaves its imprint in the classical theory via the Hamiltonian description. An exactly similar conclusion arises from the action formulation of the classical theory. In the language of quantum field theory, this phenomenon comes from the fact that the action contains additional 'off-shell' information, not present in the original classical equations of motion.

Thus we see that although the classical equations of motion allow one to define time via the evolution of a system, independently of causally disconnected regions, indeed independently of whether such regions exist at all or if they do, independently of whether time can be defined for systems inhabiting these regions, this is no longer the case once the quantum theory is considered. In the particular case of cosmology, if we wish to consider solutions that are (spatially) homogeneous, we are restricted to universes with a finite spatial extent and for inhomogeneous solutions the local existence of this practical notion of time i.e the evolution of our observable universe, implies that there exists a similar practical notion of time for all, causally disconnected regions on our spatial slice. That is, the existence of initial data sufficient to evolve into our universe implies that initial data exists everywhere on the initial spatial slice and most importantly that this slice is finite (or the data on it has compact support) and the initial data, when integrated over the entire slice, is also finite.

5 Consequences of quantum theory: renormalization

We have seen that there is a surprising relationship between the total spatial extent of our universe and the existence of a practical definition of time, that is now required to be globally defined. This is apparent already when the classical theory is written in terms of the Hamiltonian approach, however it arises from the quantum theory, and the requirement that evolution be unitairily implementable. The conclusion we have reached is that the total spatial extent must be finite, even if we wish to focus only on finite evolution of finite spatial regions. Previously the possibility of an infinite spatial extent was not relevant for finite evolution,

however now a divergence appears. Perhaps we should not be surprised that, in the transition from classical to quantum theory, previously finite expressions become divergent. This occurs also in the subtly of defining products of operators at coincident (spatial) points, which also lead to divergences in classically finite expressions. Since this is an issue to do the small separation scales they are usually referred to as UV divergences. These need to be regulated in a suitable manner, consistent with the basic axioms of quantum theory, through a process known as renormalization. The second source of divergence that we have discussed, due to an infinite spatial extent is usually referred to as IR divergences. Perhaps this too can be 'renormalized' in some suitable manner? Or perhaps the two divergences are related in some as yet unexpected way? The physical significant of our inability to define observables in a spatially infinte universe may well point to a failing of the renormalization scheme. It may be that the correct approach to renormalization, presumably arising from some full theory of quantum gravity, will show us how to regulate not only the usual UV divergences, but also the IR divergences that appear in cosmology. If this were indeed the case, it may allow the local notion of time that can be defined for classical systems to be harmonized with the global requirement that currently result from quantum theory.

In the following I will describe how once again, the spatial extent of the universe can enter the game when (one particular version of) the usual renormalization procedure is carried out in cosmology. Again this leads us to requiring that the total spatial extent of the universe be finite if physical quantities such as (expectation values of) energy density are to be well defined. At first sight this will suggest that we should alter our definition of renormalization, and indeed it may well be possible to do so in such a way as to ensure that all (finite) physical observables and their (finite) evolution are well defined, even in spatially infinite cosmologies. However, the point of the entire procedure of renormalization is that there is a universal method for removing UV divergences. This universality can be traced to the basic fact that all Riemannian manifolds are locally approximated by Euclidean space and hence that, in some loose sense, all UV divergences are the same. This universality is *not* present in considerations of IR divergences and without something similar it would be difficult to justify any particular approach.

5.1 Adiabatic renormalization

If one considers an almost homogeneous cosmology, then it is very natural to decompose the system into a homogeneous part (the spatial average) and the inhomogeneous part, which are considered small perturbations. This matches extremely well with all observations of the early universe and forms the basis of our modern understanding of the early universe. The next step is to consider these small perturbations to be a quantum field, now evolving on the homogeneous background, whose evolution provides us with a 'time' parameter with which to evolve the perturbations. This step is a particularly thorny one and it leads to many difficult conceptual issues that collectively go under the name of the 'quantum to classical transition', which will not be discussed here. Once this step has been

taken, our cosmological system contains the classical evolution of a homogeneous universe and a quantum field (the perturbations) evolving on the dynamical background. As with any quantum field theory, the definition of many important quantities require renormalization and in particular the definition of the energy density of the quantum field is formally divergent without this procedure. Fortunately there is a well defined method of performing this renormalization, even in for a system with a dynamical background, and one can calculate the (expectation value of the) energy density in the perturbations. For self-consistency of the basic approximations one needs to ensure that this renormalized energy density is indeed a perturbation to the homogeneous background.

One explicit method for performing this renormalization of products of operators, known as 'adiabatic renormalization' is the following. Perform a Fourier transformation on the perturbation field and calculate the formal expression for the desired operator (in this case the energy density). This formal expression contains an integral (or sum) over the values of the Fourier modes, k, which diverges. However, the integrand (or each term in the sum) is finite and well defined for every k, it is only the combination of all these finite contributions that diverges. Adiabatic renormalization then provides an explicit factor to subtract from the integrand (or from each term in the sum) *for all* k, that ensures that the resulting integral is well defined. In this scheme, the large values of k correspond to small spatial separations, and it is in this limit ($k \to \infty$) that the subtraction factor tends to a universial form. Indeed, the entire procedure for calculating this subtraction factor is based on the idea that, for large enough k (i.e. small enough scales), the spatial slice must approximate a flat slice and hence, on these scales, the quantum field should approximate the results of the well defined, flat space, quantum theory.

However, in a cosmology with an infinite spatial extent a new difficulty arises. This adiabatic renormalization scheme is designed to ensure that the large k (small scale) limit of the quantum field theory is well defined, however it does not guarantee that the explicit subtraction term is finite away from this limit. It may happen that the factor that needs to be subtracted from integrand at some low value of k (i.e. on some large scales) itself diverges, in which case the renormalization scheme may successfully remove the UV divergences of the theory, only to introduce new types of IR divergences. But should this be a concern? We have already encountered IR divergences in cosmology and have thus had to restrict our attention to cosmologies with finite spatial extent, is this more of the same? Unfortunately not. One can remove these new forms of IR divergences by considering a spatial slice that has a finite spatial extent, at the technical price of replacing the continuous Fourier transform with a discrete Fourier series, however this spatial extent cannot be arbitrarily large. If the renormalization procedure is to be well defined, not only must the spatial slice be finite, but it must have a spatial extent *smaller* than some specific value (given by the background dynamics).

But should this new IR divergence be considered physical? It arose from our attempt to remove UV divergences in some universal manner, but this universality applies only to the UV limit of the theory. If we choose to alter the subtraction terms for low k scales, whilst ensuring that the large

k limit remains intact, we will not have spoiled the universality property of the renormalization scheme. Indeed it is possible to perform an alteration in such a way that the IR divergences are removed, thus ensuring that the expectation value of all (relevant) renormalized operators are well defined. The difficultly is that there are many ways of doing so and unlike the removal of the UV divergences, there is no universality argument that can be appealed to choose one (class). One needs to use additional physical or conceptual inputs to choose the particular, 'natural', scheme.

6 Summary

As it has been presented here there appears to be two sources of IR divergences in cosmology, one coming from the homogeneous sector and an infinite spatial extend, the other from the renormalization scheme of the inhomogeneous perturbations, however the distinction is perhaps one only of mathematical convenience. Without splitting the cosmological model into an homogeneous background and inhomogeneous perturbations, it is difficult to formulate the quantum system, however physically it is perhaps not illuminating. In particular when one is forced to consider spatial regions larger than the currently observable universe. What an observer considers 'homogeneous' is tied to their observable universe. After all, we cannot say whether the universe is homogeneous or not on scales larger than our currently observable universe. As we saw in Section (4) quantum theory would seem to require us to restrict to a finite spatial slice if we wish to consider an homogeneous universe, whilst in Section (5) we saw that if we consider small perturbations over this background, then either one of the following must hold.

- Not only must the spatial extent be finite, it must be smaller than some specific scale or,
- Renormalization must be altered according to some, as yet unknown, new principles.

One possibility is that we may try to avoid this artificial decomposition into homogeneous and inhomogeneous sectors, defining renormalization in such a way as to ensure that (for example) the energy density in the zero-mode (the purely homogeneous sector) vanishes in some suitable manner. This may cure both forms of IR divergences (whilst not effecting the removal of the UV divergences of the theory) and *if* the resulting cosmological predictions were consistent with observations it would be a viable model. However, such an approach would impose our prejudice on the form of the universe on large (unobservable) scales. In such a case we would have succeeded in rendering quantum theory in an infinite universe well defined, however we would still have the fundamental discrepancy between the purely classical, local notion of time or evolution and the quantum requirement that this notion be applicable over the entire spatial slice. There is an essential non-locality built into quantum theory, which manifests itself at the classical level not by any change in causality, but rather by a requirement that our notion of time be globally definable.

These IR issues arise from the implementation of quantum theory to cosmology. In all other quantum systems, we do not need to consider the consequences of an infinite spatial extent, however in cosmology we must face this head on. There two possible resolutions to the discrepancy between classical cosmology, in which infinite and finite spatial extents are indistinguishable and the application of quantum theory, in which a difference appears:

- The classical locality is incorrect. We do need to constrain data outside our observable universe to be able to define evolution and hence relational time everywhere on a spatial slice, even for finite evolution of a finite region. In additional to the regularity constraints of the data, this requires either that the universe have a finite spatial extent or that the data defining the basics variables have compact support in a universe with infinite spatial extent (in particular it cannot be homogeneous).
- Quantum theory needs to be reformulated in a more 'local' way, so as to ensure that, when applied to cosmology, our observable universe is not dependent on unobservable data. However any reformulation must agree with the wealth of experimental data that quantum theory has provided on laboratory scales, whilst simultaneously allowing for independent definitions of evolution for causally disconnected regions *and* the compatibility between these evolutions if these regions overlap.

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The Burning Fuse Model of Unbecoming in Time.[1]

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In the burning fuse model of unbecoming in time, the future is real and past unreal. It is used to motivate the idea that there is something unbecoming in the present literature on the metaphysics of time, whose focus is merely the assigning of a label "real."

I

Please imagine a long fuse hanging down from the ceiling. It is a carefully woven tube of fabric that holds a core of gunpowder. We note that it is beautifully made, with brightly colored threads intertwined with the coarser bare cotton. It a masterpiece of the modern weaver's art.

We take a match, strike it and bring it to the end of the fuse that is dangling near the floor. It takes and emits a sputtering fire that shoots sparks onto the floor. We stand and watch as the fire gradually ascends. Our attention is held by the flaming point that slowly advances upward. Those parts of the fuse ahead of the fire take their turn to be consumed by it and to

[1] I am grateful to Mauro Dorato for discussion and guidance in the literature and for further discussion to Tom Pashby and Bryan Roberts.

disappear into the ashes. The beautifully woven fuse is reduced to a powder so light that it is scattered and disappears.

This is the burning fuse model. The advancing point of fire is the now. It consumes the future, the unburnt fuse, which is converted into the past, the scattered nothingness of ashes.

What is the relative ontic status of the past and future in the model? It is the inverse of the growing block model. In that model, the function of the present is to actualize mere possibility in the future into the realities of the past. In the burning fuse model, the future states carry the capacity of being able to come to be present. That capacity is realized in momentary fires of the present after which all reality is extinguished. The future is real; the past is not.

Which model should we prefer? The key novelty of the burning fuse model is the recognition that future events carry a property that is not carried by past events: they carry the capacity of being able to come to be present. Past events do not carry that capacity. Once they are past, they are spent. The capacity is lost. Indeed they carry no potentialities at all.

Let us grant that the carrying of a potentiality or a capacity endows something with a reality. Otherwise, what carries the potentiality? By that standard, future events have greater claim to reality than do past events. But we have not allowed degrees of existence, so the only way we can maintain the requisite ontic difference of future and past is to ascribe real existence to future events and none to past events.[2]

II

At this point, if I have done my job well, you are starting to wonder if I have taken leave of my wits. I am arguing for the reality of the future and the unreality of the past. Of all combinations that we might consider, that would appear to be the least credible. In spite of the appearance of caution and rigor, there is obviously something wrong in the burning fuse model that should not survive closer scrutiny.

Yes, creating that sense of unease is my purpose. It is how I feel about the burning fuse model. My real point is that I have the same feeling about the entire debate over presentism

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[2] For another defense of the reality of the future and the unreality of the past, see Casati and Torrengo (2011). They note that we believe it is possible to travel to future events merely by living, but we cannot travel to past events. They are gone.

against eternalism against growing block possibilism; or perdurantism against endurantism.[3] On the surface, we seem to be debating something concrete and important. Yet that sense evaporates when we probe beneath the surface. It is the same with the burning fuse model. I am using it as a foil to suggest that there is something unbecoming in this debate over becoming.

The old positivists are guilty of many excesses. That some proposition has no observable consequences does not render it meaningless. They were rightly chastised for overreach in suggesting otherwise. However they were on to something. If there are no observable consequences, then there can be no brake from experience for a runaway imagination. When we have a proposition with this unfortunate feature, we ought to take a second look and ask if the proposition indicates something real. Or is it a fictional invention in some fevered philosopher's dreams? Perhaps we are dealing with a pseudo-question, an artful use of language that appears to pose some deep problem but is really only tangling us in a labyrinth of our own invention.[4]

I am not the first to harbor such reservations. Mauro Dorato[5] has recently mounted a spirited assault on the issues debated in this literature. Among his many concerns is the lack of a contrast class to give meaning to the predicate "is real." The situation with temporal reality is unlike that of coffee, he notes. When we assert that the coffee is real, we are informing our listener that is it not a fake, ersatz coffee of burnt acorns. It is the real stuff. When a presentist asserts the unreality of past and future events, in just which way are they unreal or ersatz? It cannot merely be that they are not present. For that makes the presentist's view true by definition. To be asserting something more than a circularity, the presentist must provide some other sense in which future events are unreal. Eternalists hold that all events, past, present and

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[3] For an introduction to these notions, see Savitt (2008) and Hawley (2010)

[4] Another perennially recurring instance of this problem is the ever-popular question "What is time?" Its longevity is due precisely to its trivial unanswerability. See John D. Norton, "What is Time? Or, Just What do Philosophers of Science Do?"
http://www.pitt.edu/~jdnorton/Goodies/What_is_time/index.html

[5] Dorato (2006). For further discussion in a similar vein see Dolev (2006) and Savitt (2006).

future are real. They simply take the other side the definition and deny that a failure to be present is sufficient to deprive an event of reality.[6]

This difficulty, in my view, captures what is wrong in this entire debate. What is at issue is how a word, "real," is to be used. Consider three events. The earth one year ago; the earth today; and the earth one year in the future. At each event, the earth will be passing through the same position in its orbit around the sun. A myriad of facts now follow. The speed of the earth is momentarily the same. The distance to the sun is momentarily the same. The sun will appear from earth to be in the same place on the ecliptic; or, to use the older way of thinking, to be in the same house of the zodiac.

Presentists, eternalists and growing block possibilists will all agree on these facts and every other conceivable astronomical fact pertaining to the three events. They will disagree, however, just on one simple issue. How the label "real" should be applied. Presentists apply real to the present event only. Growing block possibilists will apply it to the past and present events; and eternalists to all three. That is all they disagree on. The whole debate reduces to a difference on how to assign a label.

The perdurantist-endurantist debate has a similar character.[7] Perdurantists conceive of the earth a year ago, the earth now and the earth a year hence as three temporal parts of the one thing, the earth, that perdures through time. Endurantists, however, regard the earth as wholly present at each instant. The earth now is not merely the present temporal part of the earth. It is the totality of the earth, which is an entity that endures through time. Once again, they both agree

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[6] Once we would have taken it as automatic that simultaneous events are equally present and thus equally real. Then special relativity brought us the relativity of simultaneity so that we replaced the unconditional relation of absolute simultaneity with a relative simultaneous-for-you and simultaneous-for-me, where you and I need not agree. This is a mismatch that the present literature has sought exploit, but to poor effect. We should cut off the problem at the start. We should not identify the simultaneity relation of relativity theory with the relation of co-present reality. For a notion of reality that is observer dependent is no notion of reality I recognize.

[7] See also Dorato (2012) for similar hesitations.

on all the astronomical facts just mentioned. They disagree only on how the word "part" is to be used. It is once again, a difference that makes no difference.[8]

The debate seems little different to me than the debate over Pluto's planetary status. It was known as a "planet" for the first three quarter century of our acquaintance with it. Then, in 2006, the *International Astronomical Union* declared it to be something less. It was not a planet, but merely a "dwarf planet." It was demoted since it failed to meet the third condition necessary for it to be a planet: it clears the neighborhood around its orbit. When Pluto was relabeled, there was an anguished response and a debate. There was a sense that something important had happened. However no new fact about Pluto had been discovered. The issue was merely how best to assign a label.

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[8] Here is a rejoinder I expect: while there may be no ordinary factual matter at issue in the debate, the distinction does matter for this or that other debate in metaphysics. My response: if this other debate depends on the mere question of how we assign an honorific label "real," then perhaps the new debate is correspondingly defective. I'm no more inclined to take seriously a castle built in the air when I'm told that there is another high tower built on top of the castle.

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Time and Quantum Theory: A History and Prospectus

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1 Introduction

The conventional wisdom regarding the role of time in quantum theory is this: "time is a parameter in quantum mechanics and not an operator" (Duncan & Janssen, 2012, p. 53). The reason for this is 'Pauli's theorem,' a collection of results that show that (subject to a mild restriction on the Hamiltonian operator) conventional quantum mechanics does not permit the definition of a time observable, i.e. a self-adjoint operator canonically conjugate to energy.[1] If one wishes to have time appear as a genuine observable of the theory, then this is obviously a problem, called by some "the problem of time in quantum mechanics" (Hilgevoord & Atkinson, 2011; Olkhovsky, 2011). Hilgevoord's (2005) attempted resolution of the problem rests on his rejection of a particular motivation that one might have for wishing to regard time as a genuine observable. Hilgevoord's argument is essentially this: there is nothing problematic about time being represented by a parameter rather than an operator since *space* is represented by a parameter rather than an operator as well.

In a recent historical survey, Hilgevoord (2005) contends that the demand that time be an observable can be traced back to a conceptual confusion common among the progenitors of quantum mechanics, in particular Dirac, Heisenberg, Schrödinger, and von Neumann. Hilgevoord claims that the expectation of the authors of quantum mechanics that time should be an observable was due to this confusion between space and position: led by the role of position in the theory as an observable, they were mistakenly led to the idea that time should be observable too. He traces the source of the

[1] See Srinivas & Vijayalakshmi (1981) for a rigorous derivation of this result.

confusion to the frequent use of the spatial co-ordinates (x, y, z) to denote the spectral values of the position observable of a single particle (q_x, q_y, q_z).

When presented with an operator whose spectral values appear to correspond to points of space, it is natural to expect also an operator whose spectral values correspond to instants of time. And given the expectation of these authors that quantum mechanics would ultimately be a relativistic theory, it is reasonable to demand of a theory set in space-time that time and space should appear on the same footing. However, as Hilgevoord points out, the spectral values of position are *not* identical with spatial points—this correspondence is only valid for a system comprising a single particle since in general the dimension of configuration space (and so the spectrum of the position observable) is $3N$, where N is the number of particles. Once this confusion is made manifest and it is realized that time t (a parameter) is to be contrasted with space (x, y, z) (also parameters) the apparent asymmetry is removed and so the justification for regarding time as an operator (i.e. an observable) is removed, or so Hilgevood claims. This leads him to dismiss later developments, such as the more recent use of POVMs (Positive Operator Valued Measures) to define (generalized) time observables, as conceptually confused for the same reason.

Now, with regard to this particular justification for regarding time as an operator, I agree that Hilgevoord offers an apt diagnosis: what is being contrasted here is not time and space, but time and position. But while I agree wholeheartedly that it would be a mistake to confuse space, time and position in this way, I am not convinced that this was a confusion to which many (or perhaps any) of the authors of quantum theory were prone. Indeed, Hilgevoord acknowledges that there were other reasons which led to the expectation that temporal properties were apt for representation by operators. It is my view that these other reasons for defining time operators were more important to those authors—I will claim that some remain compelling today—and thus are not so easily dismissed as resulting from a simple conceptual error.

In this paper I will be concerned with analyzing in more detail how ideas and expectations regarding the role of time in the theory arose and evolved in the early years of quantum mechanics (from 1925-27). The general theme will be that expectations which seemed reasonable from the point of view of matrix mechanics and Dirac's q-number formalism became implausible in light of Dirac-Jordan transformation theory, and were dashed by von Neumann's Hilbert space formalism which came to replace it. Nonetheless, I will identify two concerns that remain relevant today, and which blunt the force of Hilgevoord's main claim.

First, I point out Dirac's use of an 'extended' classical configuration space, which includes time and energy as conjugate variables from the get-go, is not subject to Pauli's theorem, and, moreover, his motivations for using this extended configuration space are not merely relativistic, as Hilgevoord claims. This indicates another way to avoid this 'problem of time:' by defining an 'extended Schrödinger equation' for functions of space *and* time we can have a quantum theory in which time and energy are represented by canonically conjugate observables, as Dirac had originally intended. Second, I contend that the temporal quantities under consideration concerned not time in the abstract, but the time of particular events (initially so-called 'quantum jumps'). If 'the' time operator concerns the location of an event in time, then it is no false contrast to draw an analogy with the position observable, which concerns the location of an event in space (the event in question being something like 'the particle's being here'). I will also attempt to show how these considerations are related, in that exploring the second claim (that the time of an event is an observable quantity) leads naturally to the first (that quantum theory should be defined on extended configuration space).

2 A Brief History of Time in Quantum Mechanics

2.1 Time in Matrix Mechanics

The expectation that energy and time must form a canonically conjugate pair arose from the close relation of the new quantum mechanics to the action-angle form of classical (Hamiltonian) mechanics that inspired it. In Heisenberg's famous *Umdeütung* paper of 1925, time plays an almost identical role in the description of the new quantum variables as it did in the specific classical cases he sought to re-interpret. The classical equation of motion, Hamilton's equation in action-angle co-ordinates (J, w), reads

$$\frac{dw}{dt} = \frac{\partial H}{\partial J} \quad ; \quad \frac{dJ}{dt} = -\frac{\partial H}{\partial w}.$$

The time dependence in action-angle form is particularly simple since the canonical transformation into these co-ordinates is chosen such that $\dot{J} = 0$ and $\dot{w} = v_0$, a constant. Thus J is time independent and w is linear in t. This being the case, a general solution $x(t)$ of these equations (for periodic systems) may be Fourier decomposed into a sum over components labeled

by amplitude and phase:

$$x(t) = \sum_{n=-\infty}^{\infty} \sum_{\tau \pm 1} A_\tau(J_n) e^{2\pi i \tau \nu_n t}.$$

So it was this special form of classical Hamiltonian mechanics, one in which time dependence takes an especially simple form, that provided the basis of the emerging quantum kinematics. The time evolution of these solutions was entirely confined to a complex phase, and so it was to be in the new quantum theory.

In the matrix mechanics of Born & Jordan (1925) kinematical quantities are represented by Hermitian matrices whose time dependence takes the same form,

$$\boldsymbol{p}(t) = p(nm) e^{2\pi i \nu(nm) t}; \boldsymbol{q}(t) = q(nm) e^{2\pi i \nu(nm) t}.$$

Having obtained a matrix representation of these kinematical quantities, it follows from the relation

$$\nu(nm) = W_n - W_m$$

that the time derivative of an arbitrary matrix function $\boldsymbol{g(pq)}$ may be written

$$\dot{\boldsymbol{g}} = \frac{i}{\hbar}[(W_n - W_m) g(mn)] = \frac{i}{\hbar}(\boldsymbol{Wg} - \boldsymbol{gW}), \tag{1}$$

where $\boldsymbol{W} = \delta_{mn} W_n$ is a diagonal matrix (pp. 288-9). Since the diagonal form of $\boldsymbol{W}$ was critical to the validity of this relation, the major practical difficulty of applying the new quantum mechanics to a particular system with a classical Hamiltonian of known functional form became essentially that of finding a representation in which the quantum mechanical energy took diagonal form.

By writing the Hamiltonian matrix $\boldsymbol{H}$ as a function of $\boldsymbol{p}$ and $\boldsymbol{q}$ Born and Jordan derived the following dynamical equations for quantum variables in the same form as Hamilton's equations in classical mechanics,

$$\left(\frac{\partial H}{\partial q} =\right) \dot{\boldsymbol{p}} = \frac{i}{\hbar}[\boldsymbol{Hp} - \boldsymbol{pH}] \quad ; \left(-\frac{\partial H}{\partial p} =\right) \dot{\boldsymbol{q}} = \frac{i}{\hbar}[\boldsymbol{Hq} - \boldsymbol{qH}].$$

They argued that this same relation holds true of a general function $\boldsymbol{g}(\boldsymbol{pq})$ as well, yielding the so-called Heisenberg equation of motion,

$$\dot{\boldsymbol{g}} = \frac{i}{\hbar}[\boldsymbol{Hg} - \boldsymbol{gH}], \tag{2}$$

which immediately gave the result that $\dot{\boldsymbol{H}} = 0$, i.e. that energy is conserved.

There is in this formalism no reason to suppose that time could not be represented by a matrix, and the fact that in classical mechanics w behaves very much like a time parameter is suggestive of the idea that there should be a matrix $\boldsymbol{t}(\boldsymbol{qp})$ canonically conjugate to $\boldsymbol{H}$. Indeed, if one demands that this matrix $\boldsymbol{t}$ vary linearly with time then (2) appears to imply that it is canonically conjugate to energy $\boldsymbol{H}$ since

$$\dot{\boldsymbol{t}} = 1 \Rightarrow [\boldsymbol{Ht} - \boldsymbol{tH}] = i/\hbar.$$

2.2 Time as a q-number: Dirac's Classical Analogy

Dirac, working in relative isolation in Cambridge, was led to the same dynamical equations by pursuing a structurally richer classical analogy. Like Born and Jordan he recognized the non-commutativity of the multiplicative operation as the key feature of Heisenberg's quantum variables, but rather than focusing on a particular representation of the variables, Dirac's approach led him to identify shared algebraic structures of the classical and quantum theories. We will briefly follow his development of the theory in his initial paper 'The Fundamental Equations of Quantum Mechanics' (Dirac, 1925).

Whereas Born and Jordan's derivative operation came for free from their use of matrix multiplication, Dirac sought to define his operation algebraically from the two basic conditions such an operation much satisfy: distributivity and the Leibniz law. He shows that the operation $ax - xa$, which is to say the commutator of two 'q-numbers' (a nomenclature introduced in a subsequent paper) satisfies these two conditions and so can be interpreted as a differentiation of x with respect to some parameter v. For a special case, Dirac let a be the diagonal matrix representing the energies of the allowed transitions, then v is the time and this returns $\dot{x}$, just as Born and Jordan had found.

But in contrast to Born and Jordan, who built up their dynamical equations from matrix operations acting according to the quantum condition, Dirac instead sought to establish a correspondence between classical and quantum *operations* by setting up a structural analogy between the two theories. He argued that as the quantum numbers become large the quantum commutator corresponds to the classical Poisson bracket (multiplied by a factor of $-i\hbar$).

$$\{x, y\} = \sum_r \left\{ \frac{\partial x}{\partial q_r}\frac{\partial y}{\partial p_r} - \frac{\partial y}{\partial q_r}\frac{\partial x}{\partial p_r} \right\}$$

The Poisson bracket is a canonical invariant, meaning that it takes the same value evaluated in any canonical co-ordinates. Moreover, the Poisson bracket expressions satisfy the two demands he placed on an operation of differentiation. This suggested to Dirac that the quantum commutator represented the same operation, valid for non-commuting 'q-numbers' — his own version of the correspondence principle.

Once this correspondence was established, the quantum equation of motion (2) followed immediately from the corresponding classical Poisson bracket by mere transcription according to the new quantum schema. The difference in Dirac's approach was manifest in his ability to import results from classical mechanics directly into his theory (although he was soon to see that his translation procedure led to ordering ambiguities). Since action-angle variables are classical conjugates with $\{w, J\} = 1$ (having been reached by a canonical transformation) the suggestion is very strong indeed that w and J, considered as q-numbers, must also be a canonical pair. Indeed, obtaining numerical results from Dirac's theory required transcription of the results of the corresponding classical problem, expressed in action-angle form.

2.2.1 'Relativity Quantum Mechanics'

When Dirac came to consider relativistic quantum physics his approach was, naturally enough, to define a suitable relativistic classical description in terms of Poisson Brackets, and then apply his quantum translation prescription (Dirac, 1926b).[2] It is worth quoting in full Dirac's description of this procedure and his view of the significance of defining suitable classical canonical variables.

> It will be observed that the notion of canonical variables plays a very fundamental part in the theory. Any attempt to extend the domain of the present quantum mechanics must be preceded by the introduction of canonical variables into the corresponding classical theory, with a reformulation of the classical theory with P.B.'s [Poisson Brackets] instead of differential coefficients. The object of the present paper is to obtain in this way the extension of the quantum mechanics to systems for which the Hamiltonian involves the time explicitly and to relativity mechanics. (pp. 406-7)

[2]Note that this work was done before he was aware of Schrödinger's wave mechanics.

In the following section, entitled "Quantum Time," Dirac immediately claims that "[t]he principle of relativity demands that the time shall be treated on the same footing as the other variables and so it must therefore be a q-number" (p. 407). In order to do so, Dirac defines a classical Poisson bracket that includes time t as a variable along with it's canonical conjugate $-W$ (i.e. , minus the total energy).

$$\{x, y\} = \sum_r \left\{ \frac{\partial x}{\partial q_r}\frac{\partial y}{\partial p_r} - \frac{\partial y}{\partial q_r}\frac{\partial x}{\partial p_r} \right\} - \frac{\partial x}{\partial W}\frac{\partial y}{\partial t} + \frac{\partial y}{\partial W}\frac{\partial x}{\partial t}$$

In defining this Poisson Bracket, the set of canonical variables is extended by two to include t and $-W$, and so the dynamics of the system now takes place in this *extended* phase space. The physical solutions are defined by the demand that the Hamiltonian (defined on the extended phase space) vanishes with the total energy W, what is called today a constraint equation,

$$H - W = 0. \tag{3}$$

So while t and $-W$ are variables conjugate on the extended phase space (leading to the quantum commutators detailed in Dirac's equation (7)) the dynamics of the system are confined to a subspace of the phase space defined by this constraint (the constraint surface).[3] As we have seen, Dirac is explicit here that relativistic considerations motivate his introduction of time as a q-number, but also he cites the time dependence of the Hamiltonian as a motivation. There is nothing about the use of extended phase space which implies that the system in question is relativistic, as it is just the fact that the Poisson Bracket is defined on the extended phase space which implies time and energy are conjugate variables, not the fact that the Hamiltonian is relativistic. Hilgevoord (2005) regards the use of relativistic arguments to motivate the demand that energy and time be canonical conjugates in quantum mechanics as misguided due to the limited role that relativistic particle mechanics plays in classical and quantum physics. Be that as it may (and Dirac's rhetoric here notwithstanding) it remains the case that the conjugacy of energy and time has little to do with the fact that a system

[3]In fact, Dirac notes that the conjugacy relations may be inconsistent with the constraint, but this is just to say that the relations don't necessarily hold for functions on the constraint surface. Without the Schrödinger equation in hand, it is not yet clear that the dynamical equation is in fact a wave equation satisfied by certain functions, whereas the relations of conjuagacy hold generally for functions of the extended configuration space. It is also worth emphasizing that, whereas position and momentum are conjugate variables on both the unextended and extended phase space, energy and time are conjugate variables on the extended phase space alone.

is relativistic, and everything to do with the fact that the system's dynamics are defined in the extended phase space. Although in the case of a relativistic system the use of extended phase space is unavoidable, it is clear that Dirac also viewed the (non-relativistic) Schrödinger equation as a wave equation defined on functions of space and time, i.e., as an equation in the extended configuration space.

2.2.2 The Time-Dependent Schrödinger Equation

Introduced by means of an optical-mechanical analogy, Schrödinger's wave mechanics was initially met with hostility from the matrix mechanics camp. However, as we have seen, Dirac's q-number approach was more general, and so more flexible for extension in other directions. Following Heisenberg's discovery of a connection between the solutions of Schrödinger's wave equation and the energy values that appeared in the time dependence of the matrix values for a dynamical variable, Dirac seized on Schrödinger's new approach, recognizing the means to free himself from the overly restrictive reliance on classical methods, and overcome the problems introduced by the degeneracy of energy levels arising in systems of many particles (Darrigol, 1993, pp. 329–333). Unencumbered by the conceptual baggage of Schrödinger's painstaking path through classical physics, Dirac's starting point was the realization that quantum systems could be described by functions obeying a linear wave equation, and he quickly moved to explore the consequences.

In a remarkable paper 'On the Theory of Quantum Mechanics' (Dirac, 1926a) he laid out the essentials of an approach which would serve as the basis for the later integration of wave and matrix mechanics. He demonstrated the power of this new formalism by deriving the Bose-Einstein and Fermi-Dirac statistics for an assembly of systems from elementary conditions on the permutation of the wavefunctions describing the individual systems. The foundation of this approach was the recognition that Schrödinger's theory allowed for the explicit representation of conjugate variables as differential operators. To write down the time-dependent wave equation, therefore, merely required him to make the substitutions

$$p_r = -i\hbar\frac{\partial}{\partial x}; \quad -W = -i\hbar\frac{\partial}{\partial t}$$

into the equation (3) above, treated as a wave equation, i.e.,

$$(H - W)\psi = 0. \tag{4}$$

Hence Dirac's derivation of the time-dependent equation depended on the extended phase space description described in (Dirac, 1926b). To explain:

the replacement of the conjugate variable $-W$ by the corresponding differential operator relies on the existence of a space of functions of t on which it acts. The implication is that t is also a q-number, an operator that acts by multiplication on this space of functions of *extended* configuration space.[4]

In order to set up a correspondence with the Heisenberg equations of motion Dirac is required to fix the value of the variable t, but in doing so he makes it quite clear that the functions (or superpositions of functions) that satisfy the general wave equation are functions of time and space.

> As an example of a constant of integration of the dynamical system take the value $x(t_0)$ that an arbitrary function x of the p's and q's, W and t has at a specified time $t = t_0$. The matrix that represents $x(t_0)$ will consist of elements each of which is a function of t_0. (Dirac, 1926a, p. 665)

Under the special condition that the Hamiltonian is time independent (i.e., a constant of integration), so that the energy W has a diagonal matrix representation, Dirac was able to derive the time dependence of the matrix elements of a q-number x (although, as he is at pains to point out, only for a Hamiltonian that does not involve time explicitly). This reverses the logical order of Born and Jordan's derivation of (1), which assumed the time dependence by means of Heisenberg's classical analogy. Dirac here instead shows how this time dependence arises *from* the dynamics of the quantum mechanics, that is, the Schrödinger equation.

However, he explicitly states that he views this separation of time and space as inessential, and describes the alternative (solving directly in terms of the extended phase space without considering variation in t) as more fundamental:

> It should be noticed that the choice of the time t as the variable that occurs in the elements of the matrices representing variable quantities is quite arbitrary, and any function of t and the q's that increases steadily would do. ... It is probable that the representation of a constant of integration of the system by a matrix of constant elements is more fundamental than the representation by a matrix whose elements are functions of some variable such as t ... (*ibid.* p. 666)

In summary, we can see that there was another motivation, independent from relativistic considerations, which led Dirac to regard energy and time

[4]Note again that the space of functions on which W acts is not identical with the space of wavefunctions that that satisfy the wave equation (4).

as conjugate variables: his expression for the time-dependent Schrödinger equation required that energy be represented by a differential operator d/dt, which was defined on a space of functions of time. Note also that Dirac did not yet have an dynamics that could apply to time-dependent Hamiltonians, since the treatment he had given assumed from the outset that the allowed energies were time independent.

2.2.3 Time in Transformation Theory

In Dirac's presentation of transformation theory, 'The Physical Interpretation of the Quantum Dynamics' (1927) it is apparently assumed from the outset that the theory involves the split between space and time that had been introduced in the earlier paper.

> These matrix elements [of a dynamical variable g] are functions of the time only. In the present paper we shall not take relativity mechanics into account, and shall count the time variable wherever it occurs as merely a parameter (a c-number). (Dirac, 1927, p. 625)

In the course of the development of the theory, which Dirac intends to to provide a generalization of matrix mechanics to address non-periodic systems and continuous observables, Dirac makes the (oft-quoted) crucial link with Schrödinger's wave mechanics:

> *The eigenfunctions of Schrödinger's wave equation are just the transformation functions ... that enable one to transform from the (q) scheme of matrix representation to a scheme in which the Hamiltonian is a diagonal matrix* (Dirac, 1927, p. 635; emphasis in the original).

These eigenfunctions are in fact the energy eigenstates, so what Dirac has found at this stage is the connection to Schrödinger's time-independent wave equation, which appears in the following form,

$$H(q'_r, -i\hbar\frac{\partial}{\partial q'_r})(q'_r/\alpha') = H(\alpha')(q'_r/\alpha'). \tag{5}$$

It remains for him to provide a link to the time-dependent equation that he had derived previously, i.e., the Heisenberg equations of motion. Remarkably, he does not do so: in his presentation of the theory the time dependence of the quantum variables is assumed (condition (ii) of p. 327, *ibid.*). Neither does he try to derive the time-dependent Schrödinger equation as we

would recognize it today. In fact, the time-dependent Schrödinger equation appears almost by accident; the only place Dirac explicitly considers time dependence of the solutions of the Schrödinger equation is in considering time varying Hamiltonians: first in general (p. 635) and then as a perturbation (p. 640).

Dirac had, at this first stage, only identified his transformations with energy eigenstates, and relied on the relation to the (extended) classical phase space to consider dynamics. The time variation of the quantum variables he considered—"constants of integration"—was particularly simple on this pseudo-classical picture, so long as the Hamiltonian was constant with time, and thus could be given by the Heisenberg equations of motion (2). However, if the Hamiltonian is time-dependent then the matrix scheme cannot have this simple time dependence. Dirac explains the problem as follows:

> For systems in which the Hamiltonian involves the time explicitly, there will be in general no matrix scheme with respect to which H is a diagonal matrix, since there will be no set of constants of integration that do not involve the time explicitly. (p. 635)

Yet the result of the derivation that he enters into to address this problem, is an equation for a Hamiltonian H that does not explicitly involve time—an equation that we immediately recognize today as the time-dependent Schrodinger equation for a wavefunction (q'/α'),

$$H\left(q_r, -i\hbar\frac{\partial}{\partial q_r}\right)(q'/\alpha') = H(q_r, p_r)(q'\alpha') = i\hbar\frac{\partial}{\partial t}(q'/\alpha'). \tag{6}$$

The form of this equation (numbered (12) in Dirac's paper) is inconsistent with his claim to have derived "Schrödinger's wave equation for Hamiltonians that involve time explicitly" (p. 636). In discussing this equation, (Darrigol, 1993, p. 341) presents an alternative derivation (not Dirac's) which follows Dirac's earlier paper in assuming that H is a constant of the motion. However, it is quite clear that this was not Dirac's intention. This is a puzzle. What could Dirac have meant by this claim?

It is very plausible that Dirac had just made a mistake in his derivation. Dirac's comment about the lack of energy eigenfunctions, and the dependence of H on t, indicates that he begins by considering solutions of the 'extended' Schrödinger equation that lie in the extended configuration space, for which there are no eigenfunctions. He begins the derivation (p. 635) by considering the Hamiltonian at an instant $t = \tau$, and the corresponding instantaneous variables q_τ, p_τ. Functions of q_τ and p_τ do not involve

time explicitly, and so we can regard the Heisenberg equation of motion (for variation in τ) as acting on these functions. From this it follows that

$$H_\tau(q'_\tau\alpha') = i\hbar\frac{\partial(q'_\tau/\alpha')}{\partial\tau}.$$

However, Dirac immediately suggests (p. 636) that we write t for τ and q for q_τ, which removes the time dependence of H_τ and gives (6). Thus does Dirac arrive at the time-dependent equation for a time *in*dependent Hamiltonian.

This mistake is actually quite telling: Dirac does not yet have a conception of a time varying state, and so he is compelled to interpret this equation as one in which the Hamiltonian varies. But, comparing (5) with (6), we see that what he had found was that the variation of the state (q'/α') in time is given by the Hamiltonian operator in the position representation (rather than the energy representation). As Dirac was aware, the space of instantaneous solutions was the only one in which his transformations could be defined, but it is clear that these instantaneous spaces are to be reached by fixing a particular value of the variable t in the larger space and considering variation with respect to a parameterization of that value.

The alternative was to consider a full blown four-dimensional wave equation applying to functions of time and space, a much more formidable problem. The middle ground that Dirac had found (apparently by accident) by dealing with the problem in this manner was taken by him to correspond to a time varying Hamiltonian, but he had instead derived the equation for a time varying *state*, where the Hamiltonian may (or may not) vary with time. Yet at no point in the paper does he entertain the thought that the wavefunction can vary in time without variation of the Hamiltonian.

Why did he not immediately recognize this? At this time—before he became aware of Hilbert space methods—Dirac did not possess the modern notion of a quantum state as a vector state. Moreover, the notion of a (Schrödinger picture) instantaneous state was one that he was to remain resistant to: specifying a time parameterization served to break relativistic invariance, and meant leaving the extended phase space.[5] It seems clear that, for Dirac, the state of the system was to be defined in terms of the extended configuration space, and from there the time evolution of the 'constants of integration' (here, quantum variables) could be specified. In essence, this

[5]The modern notion of state only appeared in the second edition of Dirac's *Principles of Quantum Mechanics*. See (Brown, 2006, p. 402-403) for more details. Also note that, contrary to Hilgevoord's claim that "this view disappeared from his later work" (Hilgevoord, 2005, pp. 36–37), the use of the extended phase space was essential to Dirac's work on constrained Hamiltonian Mechanics, e.g. Dirac (1966).

resembles the modern Heisenberg picture, but without the restriction that observable quantities (quantum variables) are evaluated at an instant. According to Dirac's interpretation, the theory gave information at the level of time averages.

2.3 The Time of a Quantum Jump

The question of whether time and energy are conjugate variables is closely related to the existence of a time-energy uncertainty principle. Reading Heisenberg's famous "*anschaulichen Inhalt*" paper today (Heisenberg, 1927), one is struck by the centrality of the time-energy uncertainty relation rather than the position-momentum uncertainty relation in his informal discussions of the "intuitive content" of the theory. It is clear that this relation is central to Hesienberg's attempt to articulate a physical interpretation of the theory. As he was to later put it: "I wanted to start from the fact that quantum mechanics as we then knew it [i.e. matrix mechanics] already imposed a unique physical interpretation" (from Duncan & Janssen, p. 5).

Looking more closely, we see that Heisenberg's concern is not with time in the abstract (i.e., on a par with "space") but rather the relationship between the energy of the system and the *time of a particular event*—a "quantum jump" regarded as a real physical process. Take the following passage:

> "According to the intuitive interpretation of quantum theory attempted here, the points in time at which transitions—the "quantum jumps"—occur should be experimentally determinable in a concrete manner, such as energies of stationary states, for instance. The precision to which such a point in time can be determined is $\ldots h/\Delta E$, if ΔE is the change in energy accompanying the transition." (p. 189)

This illustrates his faith that the observable content of the theory should be fixed by theory, but also seems to indicate that his view was that these quantum jumps took place at determinate moments of time, albeit times about which we have limited knowledge. Moreover, Heisenberg discusses how the possibility of measuring the energy precisely depends on performing a measurement between the moments at which jumps occurred.

> In quantum mechanics, such a behavior [of quantized periodic motion] is to be interpreted as follows: since the energy is really changed, due to other disturbances or to quantum jumps, each

> energy measurement has to be performed in the interval between two disturbances, if it is to be unequivocal. (p. 194)

The implication is that a quantum system is to be understood as having a determinate energy at all times, but that this energy fluctuates due to exchanges with the environment — quantum jumps. This was the view he had taken in his previous paper regarding energy exchanges between two coupled systems, which had inspired Dirac's transformation theory.[6] Given this view, we come to appreciate why the time-energy uncertainty relation has such a central role for the interpretation of the theory: since Heisenberg regarded the physical content of the theory as corresponding to discontinuous processes of energy exchange occurring at definite times, the energy-time relation was naturally of central importance to his project of providing an intuitive grasp of the physical content of the new quantum mechanics.

It is also of interest that Jordan's view at this time is very similar to Heisenberg's, and one can imagine that this is something that they had discussed together.

> What predictions can our theory make on this point? The most obvious answer is that the theory only gives averages, and can tell us, on the average, how many quantum jumps will occur in any interval of time. Thus, we must conclude, the theory gives the probability that a jump will occur at a given moment; and thus, so we might be led to conclude, the exact moment is indeterminate, and all we have is a probability for the jump. But this last conclusion does not necessarily follow from the preceding one: it is an additional hypothesis (Jordan 1927, from Duncan and Janssen p. 17).

It seems plausible that Jordan is taking here a similar view to Heisenberg, the view that quantum jumps are physical events taking place at the some definite time. In the first part of the answer, he seems to approach Dirac's opinion that "[the theory] enables one to calculate the fraction of the total time during which the energy has any particular value, but it can give no information about the times of the transitions" (Dirac, 1927, p. 622). But Jordan goes further to say that there is information here about the *rate* of occurrence of quantum jumps. He goes even further in suggesting that the theory might be considered to supply information about the *probability* that

[6] See (Duncan & Janssen, 2012, pp. 3-8) for more details.

a jump might occur at a particular time. This is distinguished from the view that the moment of time at which the jump occurs is *indeterminate*, leaving open the possibility that the probabilities involved could be interpreted epistemically rather than objectively.

The major technical contribution of Heisenberg's paper concerns the derivation of the position-momentum uncertainty relation from the Dirac-Jordan transformation theory. On the other hand, the time-energy relation (stated without proof on p. 177) followed intuitively from the quantization of action-angle variables in classical theory, assumed to form a conjugate pair in the quantum theory (presumably motivated by the classical analogy detailed in my Section 2.1). However, the early form of Jordan's transformation theory was particularly encouraging for the prospects of a parallel derivation of the time-energy relation along the lines of the position-momentum derivation, since it assumed (in modern parlance) that canonically conjugate observables have purely continuous spectra spanning the real line. This is a necessary condition for observables to allow the derivation of a standard uncertainty principle in modern quantum mechanics, but it does not hold of typical Hamiltonians (as Pauli later pointed out).

2.4 The 1927 Solvay Conference: The Fate of the Quantum Jump

The period of time during which this 'quantum jump' interpretation of the theory remained plausible was short-lived. By the time of Heisenberg's presentation with Born at the Solvay conference of October 1927 he no longer held this interpretation of the physical meaning of the time of a quantum jump. Born and Heisenberg's presentation contains the following passage,

> If one asks the question *when* a quantum jump occurs, the theory provides no answer. At first it seemed that there was a gap here that might be filled with further probing. But soon it became apparent that this is not so, rather, that it is a failure of principle, which is deeply anchored in the nature of the possibility of physical knowledge. One sees that quantum mechanics yields mean values correctly, but cannot predict the occurrence of an individual event. (Bacciagaluppi & Valentini, 2006, p. 420)

This seems to represent a retreat to Dirac's position that only the time average was a physically meaningful quantity. However, the rest of their

presentation reveals a more radical point of view. Bacciagaluppi and Valentini read their claim that "matrix mechanics deals only with closed periodic systems, and in these there are no changes. In order to have true processes ... one must restrict one's attention to a part of the system" (Bacciagaluppi & Valentini, 2006, pp. 205–6) to suggest that they shared the view of Campbell (endorsed by Heisenberg in a letter to Pauli) that time is a statistical phenomenon, absent in atomic systems but emerging at the macroscopic level like temperature or pressure.

Since the time-independent Schrödinger equation is solved by the stationary states corresponding to eigenfunctions of energy, if one makes the supposition that a system is always in such a state and the theory supplies probabilities for the 'jump' from one state to another, then it would be as if time did not exist except for these discontinuous transitions. Though Schrödinger introduces it only to reject it, his report contains a detailed analysis of this proposal, in which quantum systems considered as a whole involve no passage of time, and according to which time emerges from the theory as a macroscopic parameter related to the number of quantum jumps occurring between subsystems. According to this view, there is no change, and thus no passage of time in between quantum jumps (p. 207), and time rather emerges as a parameter related to the rate at which jumps occur.

> Limiting our attention to an isolated system, we would not perceive the passage of time in it any more than we can notice its possible progress in space. ... What we would notice would be merely a sequence of discontinuous transitions, so to speak a cinematic image, but without the possibility of comparing the time intervals between transitions. (p. 207)

According to Campbell's hypothesis, "one cannot regard the jump probability in the usual way as the probability of a transition calculated relative to unit time." (p. 451) On this view, the theory supplies probabilities for transitions between states and in terms of temporal information can only provide probabilities that one transition occur before or after another.

The alternative, says Schrödinger, is to regard the system not as occupying a single stationary state (along the lines of Bohr's earlier atomic theory) but rather as having a state that may be an arbitrary linear superposition of energy eigenstates. Taking this view—which was, of course, the view to win out—time appears in terms of the evolution of the relative phases of the eigenstates, decomposed relative to a particular basis. Now there is more to say here about the emergence of the modern notion of state, some of which is covered by Duncan & Janssen (2012), but we can see that already

these developments were fatal to the idea of the quantum jump as a discontinuous transition between stationary states, which relied critically on the hypothesis that a system remain in an energy eigenstate at all times.

3 In Defense of the Notion of the Time of an Event

It is clear that (with respect to von Neumann's Hilbert space formalism) these expectations regarding the role of time in the theory were false: time and energy are not conjugate variables, the Schrödinger equation is defined for functions of space alone, and there is no such thing as the time of a quantum jump (or collapse).[7] Nonetheless, I have shown that the motivations of the authors were not simply the result of conceptual confusion (although later physicists may have been misled along those lines), and so Hilgevoord's (2005) rejoinder that time already has an appropriate representation in the theory as a parameter is misguided. Taking a sympathetic reading of their motivations, I will show that these expectations can be physically motivated, and can in fact be met with minimal mutilation of the existing formalism. Thus the fact that the standard textbook presentation of quantum mechanics is inhospitable to the introduction of 'time' as an observable need not be read as a prohibition on the introduction of the *time of an event* as an observable quantity.

The initial motivation of Heisenberg to regard the time of a quantum jump as an experimentally meaningful quantity was the idea that a quantum system remains in a stationary state of definite energy, except when it instantaneously transitions to another stationary state. So if the energy of the system could be shown experimentally to have changed from one value to another, then a quantum jump must have occurred in the meantime (p. 191), and the time at which it had occurred could be experimentally determined (up to an uncertainty depending on the energy difference). It was thus natural for Jordan to regard the transformation theory as providing probabilities for such transitions to occur within a particular interval of time. I claim that, while the requirement that a system be in an energy eigenstate at all times was mistaken, the idea that the theory should provide probabilities for events to occur during intervals of time as well in regions of space was not.

To take a straightforward example, consider an experiment consisting

[7] Of course, the dynamics of the theory can be modified to bring about the stochastic collapse process at particular moments of time, but I will not consider these alternative dynamics.

of a single radioactive atom and a Geiger counter that fully surrounds it. If the half-life of the atom is 1 hour, then the probability that the counter clicks in the first hour is 1/2, the probability it clicks in the second hour 1/4, the probability it decays in the third hours is 1/8, and so on. Thus the probability that the counter clicks at some point in the future is given by an arithmetic sequence that tends to one as t tends to infinity. However, once the counter has been observed to click, the probability that it clicks in the future in zero. This is evidently a experimentally meaningful situation, and we should expect that this phenomenological law may be derived from a detailed quantum mechanical description of the decay process. However, within the standard account of measurement there is provably no way to implement this simple scheme.

This description would involve a (Heisenberg picture) quantum mechanical state ψ in a Hilbert space $\mathcal{H}$, a Hamiltonian H describing the time evolution of the system, and a series of operators T_1, T_2, T_3 such that $\langle\psi|T_1\psi\rangle = 1/2$, $\langle\psi|T_2\psi\rangle = 1/4$, and $\langle\psi|T_3\psi\rangle = 1/8$. Since the theory has a time translation symmetry implemented by the unitary group e^{iHt}, we have that $T_2 = e^{-iHt}T_1e^{iHt}$ and $T_3e^{-iHt}T_2e^{iHt}$, where t is one hour. Even this bare bones sketch is enough to tell us something interesting about the operators T_i: if H is a self-adjoint operator with spectrum bounded from below[8] then it follows that $\langle\psi|T_{i+1}T_i|\psi\rangle \neq 0$ and so these operators T_i cannot be projections onto mutually orthogonal subspaces of $\mathcal{H}$.[9]

Thus there is no mixed state decomposition in terms of states ψ_i such that $T_i\psi_i = \psi_i$, in which case the ψ_i would correspond to the system decaying during distinct intervals of time, and neither can the T_i together serve to define a self-adjoint 'time of decay' operator. The former implication indicates the von Neumann's collapse postulate cannot be applied to this situation; the latter than his identification of observables of the theory with self-adjoint operators is ill-suited to include the time of an event as an observable quantity. Yet there seems every reason to suppose that the theory should be able to answer questions like, "When will the Geiger counter click?" or in a diffraction experiment, say, "When will a dot appear on the screen?" In failing to answer these questions, the theory would be fail to be empirically adequate. This failure would constitute a real 'problem of time' for the theory. But this problem can be overcome, and without modifying the dynamics: The problem is not with the way that quantum mechanics

[8]This corresponds to the assumption that the is a value of energy for the system below which it cannot drop.

[9]For a proof this result see Unruh & Wald (1989).

defines the state of the system, but the way that probabilities are derived from the state.

The first thing to note is that in the identification of observables with self-adjoint operators it is assumed that these these operators act instantaneously. In the Schrödinger picture (in which the states vary with time) the expectation value of an operator A in the state ψ_t is $\langle\psi_t|A|\psi_t\rangle$, whereas in the Heisenberg picture (in which the observables vary with time) the expectation value of an operator is $\langle\psi|e^{-iHt}Ae^{iHt}|\psi\rangle$. (These return the same values since $|\psi_t\rangle = e^{iHt}|\psi\rangle$.) In the Schrödinger picture it makes very little sense to ask when a particular event occurs (in the sense of a probability for it occurring during some interval of time) since we may only interrogate the state at a moment of time. However, in the Heisenberg picture we may define operators that involve more than one moment of time by integrating over t.

Consider an instantaneous measurement of position. The existence of a self-adjoint position observable implies the existence of an assignment of projection operators P_Δ to regions of space Δ such that disjoint regions of space correspond to mutually orthogonal subspaces. In the Heisenberg picture, an instantaneous measurement of P_Δ at time t apparently corresponds to asking the question "is the system located in Δ at time t?" The probability of finding a positive answer is $\langle\psi|e^{-iHt}P_\Delta e^{iHt}|\psi\rangle$, and if the system is found to be in Δ then the state is updated accordingly by the projection operator $P_\Delta(t) = e^{-iHt}P_\Delta e^{iHt}$.

To consider a measurement that takes place over more that one instant, we can integrate these operators over t. The most straightforwardly defined of these operators is the dwell time operator,[10] whose expectation value corresponds to something like the proportion of time that a system spends within a region Δ,

$$T_d = \int_{-\infty}^{\infty} P_\Delta(t)dt.$$

But while this is a self-adjoint operator (albeit one whose measurement in a concrete experimental situation is questionable), it is not appropriate for describing the time of an event since it does not assign probabilities to times (or time intervals).

Consider instead a cloud chamber experiment where we set up a detector that is sensitive to the presence of high energy particles, with the chamber located in Δ. The presence of the particle will be registered by an ionization event, which is recorded by a photosensitive emulsion. For a given state ψ

[10] See Jose Munoz et al. (2010) for a recent discussion.

we want to obtain the probability that the detector fires during some interval $I = [t_1, t_2]$, given that it fires at all. The obvious candidate for an operator that corresponds to the presence of a particle within Δ during a time interval $[t_1, t_2]$ is

$$T_\Delta(I) = \int_{t_1}^{t^2} P_\Delta(t)dt,$$

but this operator is not normalized. Nonetheless, if we assume that the particle will be detected at some time (and exactly once) then the normalization is provided by the dwell time operator, which, being self-adjoint, has a unique square root $T_d = (T_d^{1/2})$. Using the inverse of this operator, we define

$$E_\Delta(I) = T_d^{-1/2} E_\Delta([t_1, t_2]) T_d^{-1/2},$$

which returns the identity when $I = \mathbb{R}$. With this 'operator normalization' the quantity $\langle\psi|E_\Delta(I)|\psi\rangle$ can be interpreted as giving the probability that the event occurs during I rather than at some other time.[11]

The operators $E_\Delta(I)$ together form a Positive Operator Valued Measure (POVM) which maps temporal intervals I to positive operators on $\mathcal{H}$.[12] However, these operators are not mutually orthogonal projections and cannot form a Projection Valued Measure (PVM); in general, a POVM that covaries with time translations $E_\Delta(I) = e^{-iHt} E_\Delta(I - t) e^{iHt}$ cannot be a PVM.[13] Since the self-adjoint operators on $\mathcal{H}$ are in one-to-one correspondence with the set of PVMs, there is no self-adjoint operator corresponding to the operators $E_\Delta(I)$ (in the same sense that the position operator Q corresponds to the projections P_Δ). Thus von Neumann's association of observables with self-adjoint operators would exclude these event time observables.

But it is not surprising that this is so since in von Neumann's measurement schema the measurement of an observable takes place at an instant, i.e. under the condition that the time is t. The usual probabilities given by an observable are *conditional* probabilities in the following sense: they are probabilities which are valid *given that the time is t*. On the other hand, the event time observable is 'measured' over an interval of time by leaving

[11] This operator normalization scheme is suggested by Brunetti & Fredenhagen (2002) and has been interpreted (Busch, 2007) in terms of the amount of time that the system spends in the region Δ during I, but to do so ignores the physical meaning of the normalization. The more natural interpretation, I claim, is in terms of the probability of a detection event, given that such an event will certainly occur.

[12] See Brunetti & Fredenhagen (2002) for a proof of this in general.

[13] See Srinivas & Vijayalakshmi (1981).

the experiment set up and waiting for it to register an event. The condition under which such a experiment takes place is that such an event will occur sooner or later, i.e., the probabilities provided by $E_\Delta(I)$ are also conditional probabilities, with the condition being that the event takes place exactly once over all of time. The incompatibility of these normalization conditions indicates that they correspond to incompatible measurements. But how many experiments take place at an instant of time? If we are judging by empirical relevance, I suggest that the conditionalization required by event time observables is more apt—only a poor detection experiment ends without a detection event.

These observables have a close relation to the screen observables of Werner (1986), which generalize the quantum time of arrival.[14] Screen observables apply to a typical diffraction experiment where an electron, say, is emitted and some time later detected at a photoluminescent screen. Since the screen is sensitive to the presence of an electron at all times, and electrons in an ensemble will be detected at different times, an instantaneous position observable cannot suffice to describe even the spatial distribution of detection events. For these screen observables, one assumes again that the detector will fire at some time t, and so the sum of the probability of detection over all times is unity. Again, very few experimental arrangements (if any) correspond to anything like an instantaneous position measurement, which would provide probabilities for a detector spread out through all of space which fires exactly once when switched on for an instant.

The main puzzle that is raised by event time observables such as these is, in my view, one of providing a suitable update rule. While POVMs play the same predictive role as PVMs[15], event time POVMs are particularly ill-suited to supplying the means to update the state, which is typically defined for projectors through Lüders Rule, which projects the state into the eigenspace of the measured eigenvalue (normalizing according to the trace). However, the very normalization of the POVM $E_\Delta(\mathbb{R}) = I$ which made it suitable for its role as an event time observable makes it ill-suited to provide probabilities for events that occur subsequent to detection. For example, it is unclear that we can obtain a definite answer to the question: what is the probability that the particle is first detected in Δ during I and subsequently in Δ'? To answer this question appears to require a new normalization scheme since now the particle is detected in both Δ and Δ',

[14] This has been a topic of much research. See Muga & Leavens (2000) for a review.

[15] As with a projection, a normalized positive operator E supplies probabilities through taking the trace of the density operator ρ, $Tr[\rho E]$.

i.e., twice. But if we normalize along those lines then we have lost the conditional nature of the probabilities desired: the question was, what is the probability of finding the particle in Δ' given that it was *already* detected in Δ?

For answering such questions, we must go beyond operator normalization and instead consider the *extended* Schrödinger equation, defined for functions of time and space as Dirac originally envisioned it,

$$(H - W)\psi(x, t) = 0.$$

The problem with this equation is that the operator $(H - W)$ has a continuous spectrum, and so there is no vector $\psi(x, t) \in L^2[\mathcal{R}^4] = \mathcal{H}_+$ which is an eigenvector with eigenvalue 0. However, as Dirac had claimed, energy $(-W)$ and time are conjugate variables on this space of functions of space and time. Without going into the details, recent work by Brunetti et al. (2010) has shown how solutions to this equation may be written in terms of linear functionals rather than vectors. While these physical solutions don't form a Hilbert space, and define non-normalizable 'weights' on the algebra of observables of $\mathcal{H}_+$ rather than algebraic states, there is a construction which, given an operator on $\mathcal{H}_+$ representing the occurrence of an event, leads to a GNS Hilbert space representation giving the expectation values for the algebra of observables on the condition that the event in question did occur.

Since time is a self-adjoint operator on $\mathcal{H}_+$ (which, remember, is not the space of solutions of the extended Schrödinger equation), the event time operators such as $T_\Delta(I)$ are projections in this space. By conditionalizing on these events, one can calculate probabilities for subsequent events, such as another detector firing elsewhere. This theory has a good claim to be regarded as a straightforward generalization of the usual Schrödinger dynamics. First, by setting up an appropriate map from $\mathcal{H}_+$ to $\mathcal{H}$, the projections $T_\Delta(I)$ become the operator normalized time POVM $E_\Delta(I)$. Second, and most importantly, the predictions of the usual Schrödinger picture description are returned in the instantaneous limit. But note that the differences between the instantaneous form and the extended form are significant: the solutions of the extended Schrödinger equation do not form a Hilbert space, and there is no meaning to the phrase 'the state of the system' without first specifying an event, the occurrence of which can be used to give a probability assignment to further events. Thus such probability assignments correspond to a *conditional* state.

4 Coda: The Philosophy of Time

What does this have to do with the metaphysics of time? The great debate over the nature of time in fundamental physics began with the correspondence of Gottfried Wilhelm Leibniz and Samuel Clarke. Against the Newtonian view of time as a substantive physical entity (substantivalism) — the "container view" of space-time — Leibniz argued that time is nothing more than the temporal relations between independently existing events (relationism). Recent discussions of the role of time in cosmological theories (e.g., Barbour (1999)) have tended to take the line that philosophical considerations demand the elimination of time from physical theories, or at least reduction of time to some other quantity with respect to which change may be defined. However, it is implausible that Leibniz intended to deny the existence of time. For example, he wrote that "time is the order that makes it possible for events to have a chronology among themselves when they occur at different times," which is inconsistent with a reductive or eliminativist account of temporal relations.[16]

Instead, Leibniz believed that time is nothing above and beyond the temporal ordering relations that exist between events: "times, considered without the things or events, are nothing at all, and ... consist only in the successive order of things and events." This view was in stark contrast to Clarke, who maintained that "time is not merely the order of things succeeding each other, because the quantity of time may be greater or less while the order of events remains the same." Thus what Clarke affirmed and Leibniz denied was the metrical structure of time; in a world consisting of successive events with a single linear temporal order, Clarke believed that one could (in principle) measure the amount of time between two successive events, whereas according to Leibniz this is a meaningless notion—once given the temporal orderings of events we have all there is to say about the temporal facts.

In ordinary quantum mechanics (the Dirac-von Neumann formalism) time features as a parameter that indexes the states (Schrödinger picture) or the observables (Heisenberg picture). In the Schrödinger picture, the natural way to view the time indexed states is as describing the same physical system at distinct moments of time. This seems to describe a world in which time is continuously valued, having the structure of the real line. It is hard to see how such a world could be made compatible with Leibniz's view of time as nothing but the temporal relations between events. But if we view

[16] All quotations are from translations at http://www.earlymoderntexts.com/.

the formalism as a means for providing probabilities for the occurrence of events in time, as I have suggested, we obtain a view of the world much more hospitable to Leibniz's contention that time is just the temporal ordering relations between (possible and actual) events.

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Three Merry Roads to T-violation

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Abstract. This paper is a tour of how the laws of nature can distinguish between the past and the future, or be *T-violating*. I argue that, in terms of the basic argumentative structure, there are basically just three approaches currently being explored. The first is an application of Curie's Principle, together with the CPT theorem. The second route makes use of a principle due to Pasha Kabir which allows for a direct detection. The third route makes use of a Non-degeneracy Principle, and is related to the energy spectrum of elementary particles. I show how each provides a general template for detecting T-violation, illustrate each with an example, and discuss their prospects in extensions of particle physics beyond the standard model.

1. Introduction

Unlike thermal physics, the physics of fundamental particles does not normally distinguish between the past and the future. For example, a typical classical mechanical system never makes such distinction, although one can imagine strange systems that do[1]. And there was a time in the mid-20th century when this "invariance under time reversal" or T-invariance was *demanded,* for example, even by Weinberg (1958), because of the great simplification it provided in the description of weakly interacting particles.

Well, a lot has changed since then, and a great deal of evidence has been accumulated which shows that, contrary to the early views of particle physicists, fundamental physics can be *T-violating* — it *does* distinguish between the past and the future! I do not wish to retell that story here. There are many sources[2], which are really much better than me, that will explain to you all about the gritty but ingenious detections

[1]For an overview of the classical case, see (Roberts 2013).

[2]For just one of my favorite recent book-length overviews, try (Bigi and Sanda 2009).

of T-violating interactions, the deep and beautiful theory underlying them, and how we can expect that theory to develop from here.

At this conference, I would like to attempt a different project, which is to draw out the basic analytic arguments underlying the various approaches to T-violation. I would like to cast these arguments into their bare skeletal form; to think about what makes them alike and distinct; and to ask how they may fare as particle physics changes is extended beyond what we know today. In sum, what I would like is to take a lighthearted tour – from a birds eye view, if you like – of the existing roads to T-violation.

What's helpful about this perspective, I think, is that it makes clear that there are really only three distinct roads to T-violation where we stand today. They can be summarized as follows.

(1) *T-Violation by Curie's Principle.* Pierre Curie declared that there is never an asymmetric effect without an asymmetric cause. This idea, together with the so-called CPT theorem, was the road to the very first detection of T-violation in the 20th century.
(2) *T-Violation by Kabir's Principle.* Pasha Kabir pointed that, whenever the probability of an ordinary particle decay $A \to B$ differs from that of the time-reversed decay $B' \to A'$, then we have T-violation. This provides a second road.
(3) *T-Violation by a Non-degeneracy Principle.* Certain kinds of matter, such as an elementary electric dipole, turn out to be T-violating whenever their energy spectrum is non-degenerate[3]. This provides the final road, although it has not yet led to a successful detection of T-violation.

In the next three sections, I will explain each of these three roads to T-violation. Some of these roads are very exciting and surprising, especially if you have not travelled down them before, and I will try to keep things light-hearted. My explanations will begin with a somewhat abstract formulation of an analytic principle, and then an illustration how it provides a way to test for T-violation. I'll end each section with a little discussion about the prospects for extensions of particle physics beyond the standard model, and in particular extensions in which the dynamical laws are not unitary.

Let's start at the beginning.

[3]A self-adjoint operator A in finite dimensions is *degenerate* if it has two orthogonal eigenvectors with the same eigenvalue. I will discuss this property in more detail below.

2. T-violation by Curie's Principle

The first evidence that the laws of a certain kind of "weakly interacting" system are T-violating, rather incredibly, was produced in 1964, *with little knowledge of the laws themselves*. How was this possible? It was through a clever mode of reasoning first pointed out by the great French physicists Pierre Curie, and adopted by James Cronin and Val Fitch in a very surprising discovery. Here is that story.

2.1. **Curie's principle.** In 1894, Pierre Curie argued that physicists really ought to be more like crystallographers, in treating certain symmetry principles like explicit laws of nature. He emphasized one symmetry principle in particular, which has come to be known as *Curie's principle:*

> When certain effects show a certain asymmetry, this asymmetry must be found in the causes which gave rise to them. (Curie 1894)

To begin, we'll need to sharpen the statement of Curie's Principle, by replacing the language of "cause" and "effect" with something more precise. An obvious choice in particle physics is to take an "effect" to be a quantum state. What then is a cause? A natural answer is: the *other* states in the trajectory (e.g. the states that came before), together with the law describing how those states dynamically evolve. So, Curie's principle can be more clearly formulated:

> If a quantum state fails to have a linear symmetry, then that asymmetry must also be found in either the initial state, or else in the dynamical laws.

This is a common interpretation of Curie's principle[4]. In fact it can be sharpened even more, and we will do so shortly. But first let's now see how it applies to the history of symmetry violation.

2.2. **Application to CP-violation.** In the study of symmetry violation, Curie's Principle appears to have first been used by Gell-Mann and Pais (1955). They did not refer to it in this way, but I think it will be clear that this is what they were using. Let's start with the example of *charge conjugation* (CC) symmetry, which has the effect of transforming particles into their antiparticles and vice versa. Suppose we have two particle states θ_1 and θ_2; their interpretation is not important for this point[5]. And suppose the state θ_1 is "even" under

[4] C.f. (Earman 2004), (Mittelstaedt and Weingartner 2005, §9.2.4).

[5] Gell-Mann and Pais used θ_1^0 and θ_2^0 refer to the neutral kaon states K_1 and K_2 discussed in Footnote 6 below.

charge conjugation, in that $C\theta_1 = \theta_1$, while the state θ_2 is "odd," in that $C\theta_2 = -\theta_2$. Then, Gell-Mann and Pais observed,

> according to the postulate of rigorous CC invariance, the quantum number C is conserved in the decay; the θ_1^0 must go into a state that is even under charge conjugation, while the θ_2^0 must go into one that is odd. (Gell-Mann and Pais 1955, p.1389).

Given C-symmetric laws, a C-symmetric state must evolve to another C-symmetric state. Or, reformulating this claim in another equivalent form: if a C-symmetric state evolves to a C-asymmetric state, *then the laws themselves must be C-violating*. That's a neat way to test for symmetry violation. And it's a simple application of Curie's Principle.

Although Gell-Mann and Pais were discussing C-symmetry, the same reasoning applies to any linear symmetry whatsoever. In particular, it applies to CP-symmetry, which is the combined application of charge conjugation with the parity (P) or "mirror flip" transformation. James Cronin and Val Fitch exploited Curie's Principle when they made the shocking discovery of CP-violation in 1964, for which they won the 1980 Nobel Prize in physics. In fact, Cronin later wrote that the Gell-Mann and Pais article "sends shivers up and down your spine, especially when you find you understand it," pointing out that it suggests a statement that is an unmistakable application of Curie's Principle (although Cronin does not call it that way):

> You can push this a little bit further and see how CP symmetry comes in. The fact that CP is odd for a long-lived K meson means that K_L could not decay into a π^+ and a π^-. If it does — and that was our observation — then there is something wrong with the assumption that the CP quantum number is conserved in the decay. (Cronin and Greenwood 1982, p.41)

When you create a beam of neutral K mesons or "kaons," the long-lived state K_L is all that's left after the beam has traveled a few meters[6]. It had been discovered eight years earlier in the same Brookhaven laboratory by Lande et al. (1956). And it was known that K_L is *not* invariant

[6] The study of strong interactions had led to the identification of kaon particle and antiparticle states K_0 and $\bar{K}_0$ that are eigenstates of a degree of freedom called *strangeness*. In the study of CP-violation, it is easier to study the superpositions $K_1 = (K^0 + \bar{K}^0)/\sqrt{2}$ and $K_2 = (K^0 - \bar{K})/\sqrt{2}$, since the lifetime of the latter is orders of magnitude longer. At the time, K_2 was identified as the "long-life kaon state K_L."

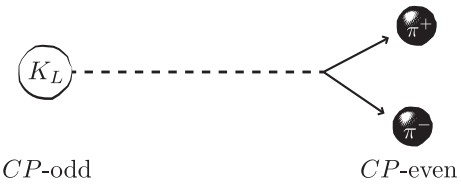

FIGURE 1. The $K_L \to \pi^+\pi^-$ decay. By Curie's Principle, this asymmetry between an initial state and a final state implies an asymmetry in the laws.

under the CP transformation, whereas a two pion state $\pi^+\pi^-$ *is* invariant under CP. The observation of such an asymmetric decay, Cronin suggests, could only be the result of a CP-violating law.

Amazingly, when Cronin and Fitch analyzed the photographs of a K_L beam in a spark chamber at Brookhaven National Laboratory, they found clear evidence that some of the long-lived kaons kaons were decaying into a pair of pions, $K_L \to \pi^+\pi^-$. Their conclusion, by a simple application of Curie's Principle, was that the laws must be CP-violating. They told Pais about their discovery at Brookhaven over coffee. Pais later wrote, "After they left I had another coffee. I was shaken by the news." (Pais 1990)

Of course, there were *many* deep insights that led to the discovery of CP-violation. They included the discovery of the strangeness degree of freedom, the prediction of kaon-antikaon oscillations, the discovery of the long-lived K_L state, the understanding of kaon regeneration, and many other things. But I hope to have shown here that, in skeletal form, the first argument for CP-violation is really a simple application of Curie's Principle.

2.3. **The conclusion of T-violation.** The final step to the conclusion of T-violation now follows from the so-called CPT-theorem. Virtually all known laws of physics are invariant under the combined transformation of charge-conjugation (C), parity (P), and time reversal (T). Of course, the precise law of unitary evolution governing the decay of the neutral kaon was not known in 1964. But there was a theorem to assure us that, at least for quantum theory as we know it — describable in terms of local (Wightman) fields, and a unitary representation of the Poincaré group — the laws must be invariant under CPT. This result, called the CPT *theorem*, was first proved in this form by Jost

(1957). And it straightforwardly implies that if CP is violated, T must be violated as well[7].

Thus, insofar as the CPT theorem applies to our world, the Cronin and Fitch application of Curie's principle provides immediate evidence for T-violation as well.

2.4. **Mathematical underpinning.** The statement of Curie's principle described above is not just a helpful folk-theorem. It can be given precise mathematical expression. Let me now try to make the mathematics more clear. I'll begin with a very simple mathematical statement of Curie's Principle in terms of unitary evolution, and then show how it can be carried over to scattering theory.

To begin, recall what it means for a law to be invariant under a linear symmetry transformation R.

Definition (invariance of a law)**.** A law of physics is *invariant* under a linear transformation R if whenever $\psi(t)$ is an allowed trajectory according to the law, then so is $R\psi(t)$.

In the standard model of particle physics, interactions are assumed to evolve unitarily over time, by way of a continuous unitary group $\mathcal{U}_t = e^{-itH}$, where H is the Hamiltonian generator of $\mathcal{U}_t$. In this context, the above definition of invariance is equivalent to

$$[H, R] = 0$$

where H again is the Hamiltonian and R is linear (Earman 2002). In these terms, we can give a first formulation of Curie's Principle as follows[8].

Fact 1 (Unitary Curie Principle)**.** *Let $\mathcal{U}_t = e^{-itH}$ be a continuous unitary group on a Hilbert space $\mathcal{H}$, and $R : \mathcal{H} \to \mathcal{H}$ be a linear bijection. Let $\psi_i \in \mathcal{H}$ (an "initial state") and $\psi_f = e^{-itH}\psi_i$ (a "final state") for some $t \in \mathbb{R}$. If either*

(1) *(initial but not final) $R\psi_i = \psi_i$ but $R\psi_f \neq \psi_f$, or*
(2) *(final but not initial) $R\psi_f = \psi_f$ but $R\psi_i \neq \psi_i$*

then,

(3) *(R-violation) $[R, H] \neq 0$.*

[7] CPT-invariance says that $(CPT)H = H(CPT)$, and thus that $(CP)THT^{-1} = H(CP)$. So, if we have time reversal invariance $THT^{-1} = H$, then we must also have CP-invariance $CP(H) = H(CP)$. Equivalently, if CP invariance fails, then so does time reversal invariance.

[8] A version of this fact was pointed out by Earman (2004, Prop. 2).

Proof. Suppose that $[R, H] = 0$, and hence (since R is linear) that $[R, e^{-itH}] = 0$. Then $R\psi_i = \psi_i$ if and only if $R\psi_f = Re^{-itH}\psi_i = e^{-itH}R\psi_i = e^{-itH}\psi_i = \psi_f$. □

This, again, is just a helpful first formulation. We have not yet arrived at a principle that is appropriate for the description of CP-violation. The claim of Cronin and Fitch was that in a neutral kaon scattering event, *there is a particular decay mode* $K_L \to \pi^+\pi^-$ that occurs only if the laws are CP-violating $[CP, H] \neq 0$. We have not yet given a rigorous formulation of *that* application of Curie's Principle.

To get there, we first observe that it is enough for CP to fail to commute with the S-matrix, $[CP, S] \neq 0$. For, if a symmetry R commutes with the "free" part of the Hamiltonian $[R, H_0] = 0$ (which is true of most familiar symmetries, including CP), then by the definition of the S-matrix[9],

$$[R, S] \neq 0 \text{ only if } [R, H] \neq 0.$$

Thus, by showing that the scattering matrix is CP-violating, one equally shows that the unitary dynamics are CP-violating as well. With this in mind, we can now state Curie's Principle in a form that is more appropriate for scattering theory.

Fact 2 (Scattering Curie Principle)**.** *Let S be a scattering matrix, and $R : \mathcal{H} \to \mathcal{H}$ be a unitary bijection. If there exists any decay channel $\psi^{in} \to \psi^{out}$ such that either,*

(1) *(in but not out) $R\psi^{in} = \psi^{in}$ but $R\psi^{out} = -\psi^{out}$, or*
(2) *(out but not in) $R\psi^{out} = \psi^{out}$ but $R\psi^{in} = -\psi^{in}$,*

then,

(3) $[R, S] \neq 0$.

Moreover, if $\mathcal{U}_t = e^{-it(H_0+V)}$ is the associated unitary group, and if R commutes with the free component H_0 of the Hamiltonian $H = H_0 + V$, then (R-violation) $[R, H] \neq 0$.

[9] One easy way to prove this is to just look at the explicit Dyson expression of the S-matrix,

$$S = \mathcal{T} \exp\left(-i \int_{-\infty}^{\infty} dt V_I(t)\right), \tag{1}$$

where V_I is the interacting part of the Hamiltonian $H = H_0 + V_I$, and $\mathcal{T}$ is the time-ordered multiplication operator (Sakurai 1994, p.73). If $H = H_0 + V_I$, then $[R, H_0] = 0$ and $[R, H] = 0$ implies that $[R, V_I] = [R, H - H_0] = [R, H] - [R, H_0] = 0$. Thus, since R is linear, we can pass it through the integral above to get that $RSR^{-1} = S$.

Proof. We prove the contrapositive; suppose that $[R, S] = 0$. Since R is unitary, $\langle \psi^{out}, S\psi^{in} \rangle = \langle R\psi^{out}, RS\psi^{in} \rangle = \langle R\psi^{out}, SR\psi^{in} \rangle$. So, if either the "in but not out" or the "out but not in" conditions hold, then,

$$\langle \psi^{out}, S\psi^{in} \rangle = \langle R\psi^{out}, SR\psi^{in} \rangle = -\langle \psi^{out}, S\psi^{in} \rangle.$$

Hence, $\langle \psi^{out}, S\psi^{in} \rangle = 0$, which means that there can be no decay channel $\psi^{in} \to \psi^{out}$. Finally, we note that if $[R, H_0] = 0$, then and $[R, S] \neq 0$ implies that $[R, H] \neq 0$ by the definition of the S-matrix. □

This, finally, is the precise mathematical statement of Curie's Principle that was applied by Cronin and Fitch. Taking $\psi^{in} = K_L$ and $\psi^{out} = \pi^+\pi^-$, they discovered a scattering event $\psi^{in} \to \psi^{out}$ that satisfies "out but not in" for the transformation $R = CP$. It follows that the laws are CP-violating. And given CPT invariance, it follows that they are T-violating as well.

2.5. **Advantages and limitations.** An obvious advantage of this approach to T-violation is that you don't have to know the laws to know that they are T-violating. At the time of its discovery in 1964, many of the structures appearing in the modern laws of neutral kaon decay were absent: there were no W or Z bosons, no Kobayashi-Maskawa matrix, and certainly no standard model of particle physics. All that came later. Nevertheless, Curie's Principle provided a surprisingly simple test that the laws are T-violating, even without knowing the laws themselves.

A more subtle advantage is that, as a test for CP violation, Curie's Principle will likely continue hold water in non-unitary extensions of quantum theory[10] Although unitary evolution is assumed in some of the background definitions, nothing about the argument from Curie's Principle requires the evolution be unitary. For example, the "scattering version" of Curie's principle in no way depends on the unitarity of the S-matrix; indeed, the conclusion that $[R, S] \neq 0$ holds when S is any Hilbert space operator whatsoever that connects ψ^{in} and ψ^{out} states. In this sense, the argument from Curie's principle is very general indeed.

The limitation is that it is an indirect test for T-violation, and one that we might not trust as we attempt to extend particle physics beyond the standard model. In particular, the reliance on the CPT theorem is troubling. It is not implausible that CPT invariance could fail as particle physics is extended beyond the standard model. For example, we might wish to consider a representation of the Poincaré

[10] C.f. Weinberg (1989).

group that is not completely unitary. In such cases, the CPT theorem can fail, and thus so would the link between CP-violation and T-violation. It would be preferable to have a direct test of T-violation instead.

One might respond to this concern by trying to apply Curie's Principle directly to the case of T-violation. Unfortunately, that doesn't work. Recall in the statement of Curie's Principle above assumed the symmetry transformation was linear. This turns out to be a crucial assumption; Curie's Principle fails badly for antilinear symmetries like time reversal[11]. So, this road to T-violation is *essentially* indirect. One can check directly for CP violation, but only recover T-violation by the CPT theorem. A direct test of T-violation will have to follow a completely different argument. That is the topic of the next section.

3. T-Violation by Kabir's Principle

New progress has recently been made in the understanding of T-violation. We now have evidence for the phenomenon that appears to be much more direct. The first such evidence began with an experiment by Angelopoulos et al. (1998), performed at the CPLEAR particle detector at CERN. Like the original T-violation experiment, this discovery involved the decay of neutral kaons. But unlike previous tests of T-violation, this experiment did not make use of Curie's Principle, and in this way managed a *direct* detection of T-violation. Things got even better when, just a few months ago now, yet another direct detection of T-violation was announced by the BaBar collaboration at Stanford (Lees et al. 2012). This experiment involved the decay of a different particle, the neutral B meson. It's an exciting time for the study of T-violation!

What I would like to point out is that both recent detections of T-violation hinge on a common principle. It is not Curie's Principle, for we have seen that this does not allow for the direct detection of T-violation. Let me call it *Kabir's Principle*, since it was pointed out in an influential pair of papers by Kabir (1968, 1970). Unlike the Curie Principle approach to symmetry violation, this one is really built to handle antilinear transformations like time reversal. Here is how it works.

3.1. **Kabir's Principle.**

To begin, let me summarize the simple idea behind Kabir's Principle somewhat roughly.

[11] See Roberts, "The simple failure of Curie's Principle," manuscript of July 21, 2012, `http://philsci-archive.pitt.edu/9249/`

> If a transition $\psi^{in} \to \psi^{out}$ occurs with different probability than the time-reversed transition $T\psi^{out} \to T\psi^{in}$, then the laws describing those transitions must be T-violating.

This suggests a straightforward technique for checking whether or not an interaction is governed by T-violating laws. We set up a detector to check how often a particle decay $\psi_i \to \psi_f$ occurs (called its *branching ratio*), and compare it to how often a the decay $T\psi_f \to T\psi_i$ occurs. Easier said than done, naturally. But if one occurs more often than the other, then we have direct evidence of T-violation.

In the next subsection, I will sketch briefly how such a procedure was first carried out at CERN. I'll then discuss the precise mathematical formulation of Kabir's Principle.

3.2. **Application to T-violation.** The first direct detection of T-violation involved the decay of our friend the neutral kaon. So, let's return to the strangeness eigenstates K^0 and $\bar{K}^0$, which have strangeness eigenvalues ± 1, respectively. It is generally thought that, if strong interactions were all that governs the behavior of these states, then strangeness would be conserved. So, by the kind of arguments discussed above, you could never have a particle decay like $K^0 \to \bar{K}^0$, because these states have different values of strangeness. However – and this is another thing pointed out in the remarkable article by Gell-Mann and Pais (1955) – when weak interactions are in play, there is no reason not to entertain decay channels that fail to conserve strangeness.

In fact, in the presence weak interactions, it makes sense to consider both $K^0 \to \bar{K}^0$ and $\bar{K}^0 \to K^0$ as possible decay modes. These particles could in principle bounce back and forth between each other, $K^0 \rightleftarrows \bar{K}^0$, by a phenomenon called *kaon oscillation*. This is a very exotic property, which applies to only a few known particles (one of them being the B meson), and it is part of what makes neutral kaons so wonderfully weird.

Now, the convenient thing about oscillations between K^0 and $\bar{K}^0$ is that they are very easy to time reverse. In particular,

$$TK^0 = K^0, \quad T\bar{K}^0 = \bar{K}^0.$$

This allows us to apply Kabir's Principle in a particularly simple form: if we observe $K^0 \to \bar{K}^0$ to occur with a different probability than $\bar{K}^0 \to K^0$, then we have direct evidence for T-violation! This is precisely what was found at the CPLEAR detector, in showing that there is "time-reversal symmetry violation through a comparison of the probabilities

FIGURE 2. Application of Kabir's Principle. If the decay $K^0 \to \bar{K}^0$ happens more often than the time-reversed decay $\bar{K}^0 \to K^0$, then the interaction is T-violating.

of $\bar{K}^0$ transforming into K^0 and K^0 into $\bar{K}^0$" (Angelopoulos et al. 1998).

At this level of abstraction, it was the very same strategy that was used in the Stanford T-violation experiment with B mesons. It turns out that neutral B mesons can also oscillate between two states, $B^0 \rightleftarrows B_-$. Bernabéu et al. (2012) pointed out that if these transitions were to occur with different probabilities, then we would have T-violation. And this is just what was recently detected by Lees et al. (2012) at Stanford. Thus, both the Stanford detection and the original CPLEAR detection T-violation were made possible by the abandonment of Curie's Principle, in favor of the more the more direct principle of Kabir.

3.3. **Mathematical Underpinning.** As with Curie's Principle, Kabir's Principle has a rigorous mathematical underpinning. But before getting to that, it's important to note the special way that unitary operators like the $\mathcal{U}_t = e^{-itH}$ and the S-matrix transform under time reversal. The point where many get stuck is on the fact that T is *antiunitary*. This means that it conjugates the amplitudes, $\langle T\psi, T\phi\rangle = \langle \psi, \phi\rangle^*$. It also means that it is *antilinear*, in that it conjugates any complex number that we pass it over:

$$T(a\psi + b\phi) = a^* T\psi + b^* T\phi.$$

As a consequence, the condition of time reversal invariance that $[T, H] = 0$ does *not* imply that the unitary operator $\mathcal{U}_t = e^{-itH}$ commutes with T. Instead, the complex constant picks up a negative sign. That is, for time reversal invariant systems, $T\mathcal{U}_t T^{-1} = e^{-(-itTHT^{-1})} = e^{itH} = \mathcal{U}_{-t} = \mathcal{U}_t^*$. Similarly, a unitary S-matrix describes a time-reversal invariant system if and only if $TST^{-1} = S^*$.

We can formulate a mathematical statement of Kabir's Principle. Note that, as discussed in Section 2.4, the failure of the S-matrix to be time reversal invariant ($TST^* \neq S^*$) implies T-violation in the ordinary sense ($T\mathcal{U}_t T^{-1} \neq \mathcal{U}_t^*$).

Fact 3 (Kabir's Principle)**.** *Let S be a unitary operator (the S-matrix) on a Hilbert space $\mathcal{H}$, and let $T : \mathcal{H} \to \mathcal{H}$ be an antiunitary bijection. If,*

(1) *(unequal amplitudes) $\langle \psi^{in}, S\psi^{out}\rangle \neq \langle T\psi^{out}, ST\psi^{in}\rangle$,*

then,

(2) *(T-violation) $TST^{-1} \neq S^*$.*

Proof. We argue the contrapositive. Suppose $TST^{-1} = S^*$. Since T is antiunitary, $\langle \psi^{out}, S\psi^{in}\rangle = \langle T\psi^{out}, TS\psi^{in}\rangle^*$. But $TS = S^*T$ by time reversal invariance, so,

$$\langle \psi^{out}, S\psi^{in}\rangle = \langle T\psi^{out}, S^*T\psi^{in}\rangle^* = \langle T\psi^{in}, ST\psi^{out}\rangle,$$

where the last equality just applies properties of the inner product. □

3.4. **Advantages and limitations.** Like Curie's Principle, Kabir's Principle provides a way to establish T-violation of the laws without assuming very much at all about those laws. But even better, it does so without recourse to the CPT theorem. In this sense, Kabir's Principle stands a better chance of remaining valid in CPT-violating extensions of the standard model.

A limitation is that, unlike the Curie's Principle approach, Kabir's Principle only seems to work when the dynamics is unitary. As in Section 2.5, suppose we consider some non-unitary extension of the standard model. Unfortunately, an essential part of the Kabir Principle argument involves the assumption that time reversal invariance has the effect,

$$T\mathcal{U}_tT^{-1} = \mathcal{U}_{-t} = \mathcal{U}_t^*.$$

When $\mathcal{U}_t$ is a *unitary* group, this is a simple mathematical fact. However, if we wish to consider a one-parameter group $\mathcal{U}_t$ that is *not* unitary, then the concept of time reversal invariance $T\mathcal{U}_tT^{-1} = \mathcal{U}_{-t}$ does not necessarily imply that $T\mathcal{U}_tT^{-1} = \mathcal{U}_t^*$. But this latter fact is (crucially) applied in the proof of Kabir's Principle.

Thus, although the Kabir Principle applied by Angelopoulos et al. (1998) and Lees et al. (2012) has the advantage of providing a direct test, they are not general enough to apply without modification to the context of a non-unitary dynamics.

4. T-violation by a Non-degeneracy Principle

I'd like to finish with one final road to T-violation. It is perhaps the most direct and yet the least well-known of all the approaches. In simplest terms, this route involves the search for exotic new kinds of matter. Let me begin with a toy model of how this can happen.

I'll then turn to the general reasoning underpinning this approach to T-violation, and finally show how this reasoning has been applied (unsuccessfully so far) in empirical tests.

4.1. **A toy example.** An electric dipole moment typically describes the displacement between two opposite charges, or within a distribution of charges. But suppose that, instead of describing a distribution of charges, we use an electric dipole moment to characterize a property of just one elementary particle. This particle might be referred to as an "elementary" electric dipole moment.

The existence of such particles has been entertained, though none have yet been detected. Let H_0 be the Hamiltonian describing the particle in the absence of interactions; let J represent its angular momentum; and let E represent an electromagnetic field. Then these "elementary" electric dipoles are sometimes[12] characterized by the Hamiltonian,

$$H = H_0 + J \cdot E.$$

Since time reversal preserves the free Hamiltonian H_0 and the electric field E, but reverses angular momentum J, this Hamiltonian is manifestly T-violating: $[T, H] \neq 0$. Therefore, an elementary electric dipole of this kind would constitute a direct detection of T-violation. No need for Kabir's Principle. No need for Curie's Principle. No need for the CPT theorem.

Like the T-violating $K_L \to \pi^+\pi^-$ and $K^0 \rightleftarrows \bar{K}^0$ decays, there are general principles underpinning this example of T-violation, too. In this case, they stem from the relationship between T-invariance and the degeneracy of the energy spectrum. The relevant relationship can be summarized as follows.

4.2. **A Non-degeneracy Principle.** A system is called *degenerate* if its Hamiltonian has distinct energy states with the same energy eigenvalue. An intuitive example is the free particle on a string, which is degenerate: the particle can either move to the left, or to the right, and have the same kinetic energy either way. Kramers (1930) showed that an odd number of electrons can be expected to have a degenerate energy spectrum, and for this his name remains attached to that effect: *Kramers Degeneracy*[13]. But it was Wigner (1932) showed the much deeper relationship between degeneracy and time reversal invariance.

[12](See Khriplovich and Lamoreaux 1997)

[13]The reason people were interested in the first place, it seems, is that degeneracy was a key part of knowing how to studying very low temperature phenomena using paramagnetic salts (Klein 1952).

For the purposes of understanding T-violation, the relevant relationship can be summarized as follows.

Fact 4 (Non-degeneracy Principle)**.** *If (1) time reversal acts non-trivially on states, in that $T\psi \neq e^{i\theta}\psi$ for some eigenvector ψ of H; and (2) the Hamiltonian H is non-degenerate; then we have T-violation, in that $[T, H] \neq 0$.*

We will see shortly how this fact has a simple prove deriving from the work of Wigner. But first, let me point out how it can be used to provide evidence of T-violation, if we were to detect a particular kind of electric monopole.

4.3. **Application to T-violation.** We observed above that an appropriate system can provide an explicit and direct example of T-violation. The properties that these systems tend to share, it turns out, are just the properties of the Non-degeneracy Principle above. There are various examples that one could study here to illustrate. But let me spare the reader and give just one that is rather important, the elementary electric dipole moment.

The thing that is not obvious is that the elementary electric dipole moment is that it always satisfies part (1) of the Non-degeneracy Principle. That is, time reversal always acts non-trivially on such systems, in that there is some eigenvector ψ of H that is transformed non-trivially, $T\psi \neq e^{i\theta}\psi$. We'll show why that is in the following. But to get from there to T-violation, notice that we need only make the plausible assumption that an elementary particle in a stable ground-state is non-degenerate. It then follows by the Non-degeneracy Principle that the system is T-violating.

To begin, let's introduce the elementary electric dipole moment[14]. It is normally taken to be a system characterized the following three properties.

(1) *(Permanence)* There is an observable D representing the dipole moment is "permanent", in that $\langle \psi, D\psi \rangle = a > 0$ for some eigenvector ψ of the Hamiltonian H. That is, the dipole is a permanent feature of the particle, like its charge or spin-type.
(2) *(Isotropic Dynamics)* Since it is an elementary particle, its simplest interactions are assumed to be isotropic, in that time evolution commutes with all rotations, $[e^{-itH}, R_\theta] = 0$. Note that if J is the "angular momentum" observable that generates the rotation $R_\theta = e^{i\theta J}$, then this is equivalent to the statement that $[H, J] = 0$.

[14]C.f. (Ballentine 1998, §13.3), (Messiah 1999, §XXI.31), or (Sachs 1987, §4.2).

(3) *(Time Reversal Properties)* Time reversal, as always, is an antiunitary operator. It has no effect on the electric dipole observable ($TDT^{-1} = D$), which is basically a function of position. But it does reverse the sign of angular momentum ($TJT^{-1} = -J$), since spinning things spin in the opposite orientation when their motion is reversed.

A system with these three properties turns out to satisfies condition (1) of the Non-degeneracy principle, that $T\psi \neq e^{i\theta}\psi$ for some eigenvector ψ of H. To see why, assume (for reductio) that it does not, and thus that for each eigenvector ψ of the Hamiltonian, there is a unit $e^{i\theta}$ such that $T\psi = e^{i\theta}\psi$. We will show that the assumption that the dipole moment is "permanent" then fails, contradicting our hypothesis.

Since $[H, J] = 0$, there is a common eigenvector for H and J, which we will denote ψ. By the Wigner-Eckart Theorem[15], each eigenvector of and J will satisfy,

$$\langle \psi, D\psi \rangle = c\langle \psi, J\psi \rangle \tag{2}$$

for some $c \in \mathbb{R}$. Now, an antiunitary operator T satisfies $\langle T\psi, T\phi \rangle = \langle \psi, \phi \rangle^*$ for any ψ, ϕ. And a self-adjoint operator satisfies $\langle \psi, A\psi \rangle^* = \langle \psi, A\psi \rangle$ for any ψ. Applying these two facts to Equation (2), we get that $\langle T\psi, TD\psi \rangle = c\langle T\psi, TJ\psi \rangle$. But T commutes with D and anticommutes with J, so this equation may be written,

$$\langle T\psi, D(T\psi) \rangle = -c\langle T\psi, J(T\psi) \rangle \tag{3}$$

Finally, we assume the distinct ray condition fails, so $T\psi = e^{i\theta}\psi$ for some $e^{i\theta}$. Applying this to Equation (3), we get

$$\begin{aligned} & (e^{-i\theta}e^{i\theta})\langle \psi, D\psi \rangle = -(e^{-i\theta}e^{i\theta})c\langle \psi, J\psi \rangle \\ \Rightarrow \quad & \langle \psi, D\psi \rangle = -c\langle \psi, J\psi \rangle. \end{aligned}$$

Combined with Equation (2), this implies that $\langle \psi, D\psi \rangle = 0$, contradicting our hypothesis that D is permanent.

So, the elementary electric dipole has at least one energy eigenvector ψ such that $T\psi \neq e^{i\theta}\psi$. That's premise (1) of the non-degeneracy argument. To get to T-violation, we need only convince ourselves of premise (2), that such a system is described by a non-degenerate Hamiltonian. Constructing such a system is part of an active search for T-violation.

There are many interesting things to say about this research; for a book-length treatment, see Khriplovich and Lamoreaux (1997). All

[15] A special case of this theorem states that the components of any vector observable are proportional to the components of angular momentum. (See Ballentine 1998, §7.2, esp. page 195).

I would like to point out for now is that this approach to T-violation hinges on a simple Non-degeneracy Principle, which is distinct from all the other approaches to T-violation discussed so far.

4.4. **Mathematical Underpinning.** As suggested above, Fact 4 basically arises out of Wigner's discovery of a connection between time reversal and degeneracy for systems with an odd number of fermions. Here is how that connection leads to a principle for understanding T-violation.

Wigner began by noticing a strange fact that two successive applications of the time reversal operator T. When applied to a system consisting of an odd number of electrons, it does not exactly bring an electron back to where we started. Instead, it adds a phase factor of -1. Only by applying time reversal twice more can we return an electron to its original vector state. This is a curious property indeed! But there is no getting around it. It is effectively forced on us by the definition of time reversal and of a spin-1/2 system (Roberts 2012).

This led Wigner to the following argument that the electron always has a degenerate Hamiltonian[16].

Proposition 1 (Wigner)**.** *Let H be a self-adjoint operator on a finite-dimensional Hilbert space, which is not the zero operator. Let $T : \mathcal{H} \to \mathcal{H}$ be an antiunitary bijection. If*

(1) *(electron condition) $T^2 = -I$, and*
(2) *(T-invariance) $[T, H] = 0$*

then,

(3) *(degeneracy) H has two orthogonal eigenvectors with the same eigenvalue.*

That's a fine argument for degeneracy, when we are confident about time reversal invariance. But what if we are interested in systems that are T-violating? No problem. We can just interpret Wigner's result in the following equivalent form.

Corollary. *Let H be a self-adjoint operator on a finite-dimensional Hilbert space, which is not the zero operator. Let $T : \mathcal{H} \to \mathcal{H}$ be an antiunitary bijection. If*

(1) *(electron condition) $T^2 = -I$, and*
(2) *(non-degeneracy) H has no two orthogonal eigenvectors with the same eigenvalue*

[16]Wigner's assumption of a finite-dimensional Hilbert space can be relaxed, as generalizations exist for Hamiltonians with a continuous energy spectrum as well (Roberts 2012).

then,

(3) *(T-violation)* $[T, H] \neq 0$.

This means that Wigner's result is actually a toy strategy for testing T-violation in disguise! Suppose we discover an electron described by a non-degenerate Hamiltonian. Then we will have achieved a direct detection of T-violation.

There is a more general sort of reasoning at work here. It turns out that the $T^2 = -I$ condition is stronger than is really needed to prove the result. The following generalization, which otherwise follows Wigner's basic argument, is available.

Proposition 2. *Let H be a self-adjoint operator on a finite-dimensional Hilbert space, which is not the zero operator. Let T be an antiunitary bijection. If*

(1) *(distinct ray condition) $T\psi \neq e^{i\theta}\psi$ for some eigenvector ψ of H, and*
(2) *(non-degeneracy) H has no two orthogonal eigenvectors with the same eigenvalue*

then,

(3) *(T-violation)* $[T, H] \neq 0$

Proof. We prove the contrapositive, by assuming (3) fails, and proving that either (1) or (2) fails as well. Let $H\psi = h\psi$ for some $h \neq 0$ and some eigenvector ψ of unit norm. Since T is antiunitary, $T\psi$ will also have unit norm.

Suppose (3) fails, and hence that $[T, H] = 0$. As we saw in the proof of Proposition 1, this implies that if ψ is an eigenvector of H with eigenvalue h, then so is $T\psi$. By the spectral theorem, the eigenvectors of H form an orthonormal basis set. So, since ψ and $T\psi$ are both unit eigenvectors, either $T\psi = e^{i\theta}\psi$ or $\langle T\psi, \psi\rangle = 0$. The latter violates non-degeneracy (2). And, since ψ was arbitrary, the former violates the distinct ray condition (1). Therefore, either (1) or (2) must fail. □

This simple generalization is now more than a "toy" experimental test. It is the mathematical grounds for the Non-degeneracy Principle stated in Section 4.2, and part of an active search for T-violation.

5. Conclusion

We have seen three routes to T-violation, of distinctly different forms. The first route, which employs Curie's Principle and the CPT theorem, is by necessity indirect. The reason is the curious result

that Curie's Principle fails for time reversal in quantum mechanics. As a consequence, one can only use this principle to test for linear symmetries like CP-violation. Insofar as the premises of the CPT theorem are correct, T-violation can then be derived as a consequence of CP-violation. But for a more direct test, one can take the second route and apply "Kabir's Principle," which restores the possibility of a direct detection of T-violation. For another direct test, one can take a third route and apply the Non-degeneracy Principle. This allows for a direct test of T-violation, which is not contingent on the premises of the CPT theorem, although it requires knowing more about the form of the Hamiltonian.

Curiously, the former two approaches (the only successful approaches) both ultimately rely, in their own different ways, on the assumption of unitary time evolution. The first approach does so not with Curie's Principle – it doesn't require unitarity – but in the application of the CPT theorem. The second approach does so in the application of Kabir's Principle. This suggests that, in extensions of the standard model that relax the assumption of unitarity, we may lose our best existing evidence for T-violation. Of course, there will always be a limiting case in which unitary evolution is justified, and so there will be a limiting case where we have T-violation. But moving forward, the question of whether T-violation will remain an explicit feature of the *fundamental* laws is, for the moment, an open one.

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Calling Time on Digital Clocks

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I explore two logical possibilities for the discretization of time, termed "instantaneous" and "smeared". These are found by discretizing a continuous theory, and the resulting structure of configuration space and velocities are described. It is shown that results known in numerical methods for integration of dynamical systems preclude the existence of a system with fixed discrete time step which conserves fundamental charges universally, and a method of avoidance of this "no-go" theorem is constructed. Finally the implications of discrete time upon quantum cosmology are discussed.

NB : At this point this paper is a draft, citations and formatting are not completed.

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I. INTRODUCTION

"The now is a link of time....for it links together past and future, since it is a beginning of one and an end of another." - Aristotle [1]

Whether physics can be described on a continuum or lattice is one of the oldest questions considered by philosophers in one form or another. The most famous paradox of Zeno argues against the infinite divisibility of a temporal interval - that is against continuous time[2]. Achilles is sent to chase a tortoise, which is given a head start. If we label the position of the turtle x_t for points in time t. Then in each instant that Achilles reaches x_t the turtle has moved on to x_{t+1}, thus it should seem logically impossible for Achilles to catch the turtle. Viewed externally, however, one can easily verify that Achilles does indeed catch the turtle at a finite time. Of course we now know the resolution to this apparent paradox is that there can be a finite sum of an infinite number of terms, as Archimedes found. To Zeno, however, this was not known, as it was assumed that an infinite sum cannot be finite, and thus it appeared that there could not be an infinite number of time points in an interval - time should not be continuous.

Tong makes a case against the a lattice reality based upon the problems with implementation in practical terms [3], stating " no one knows how to formulate a discrete version of the laws of physics." Futhermore he makes the compelling case that chiral fermions do not sit easily upon a lattice, and since the Standard Model is a chiral model, this means that it appears impossible to place known physics upon a lattice. Indeed lattice simulation models of chiral fermions in four dimensions seem to rely crucially upon treating the particles as living essentially on a five-dimensional lattice [4]. There is an important distinction to be noted here: Attempts are made to simulate the four-dimensional behaviour of the particles, for which the use of extraneous mathematical structure (in this case the extra dimension) is appropriate. If, however, one were to claim that physics in fact inhabits a lattice, rather than being simulated on one, this extra structure becomes unwelcome baggage whose existence must be explained. Tong goes on to argue that the appearance of the integers in physics is constructed from an underlying continuum, an argument which mirrors the duality between a particle which exists as the excitation of a field, and a field which is observed to be composed of particles.

There are three ways in which one can respond to arguments of this type. The first, more simplistic argument is to state that what has been shown is not a "no-go" theorem against lattice constructions in four dimensions, merely that we do not yet know how to construct one. As such it is plausible that a lattice construction may be achieved in the near future, at which point all such objections would be rendered null. A second point would be that the problem may come about from trying to force a continuum theory onto a lattice. If discreteness is fundamental, then a continuum theory should emerge at large scales, but features of theory at the lattice level may be radically different from those of the continuum. The final method of avoiding such problems in the context of this paper is to argue merely in favour of discrete time, not space. This argument may seem unnatural on some level, as the even-handed treatment of time and space is a guiding principle for modern theories such as relativity. However it is clear that there is a physical, substantive distinction between the two at least at the level of metric signature. In practical implementations of these theories a space-time splitting is employed regularly.

In this paper I will explore the effects of introducing a discrete tick to physical systems.

The paper is laid out as follows: In section III I will show how the effects of introducing this tick onto systems with continuous time parameter, establishing the kinematics of such treatments. This is followed by section IV in which I discuss the implementation of discrete time steps in numerical simulations. Section V shows one practical application of these techniques to circular motion, and a way of establishing dynamics which solve some the problems found is introduced in section VI. Finally I will note how this effects quantum gravity. But first, to clocks...

II. A NOTE ON CLOCKS

"They took away time, and they gave us the clock." - Abdullah Ibrahim.

The nature of physical clocks seems dichotomous at first glance. A clock is a timekeeping device, an instrument whose observation gives information used to define the interval between two events. A clock should contain a cyclic element, which describes the tick of the system. This role is performed by, for example, observations of the positions of shadows cast by the sun or the repeated dripping of water from a vessel (as was used in the earliest clocks of Egypt and Babylon) through to the oscillations of a caesium atom used in the atomic clocks of today. The clock must also be monotonic, defining unambiguously a separation of reality into past and future.

There is of course no contradiction in this. Although at first an individual system cannot be seen to be both globally monotonic and cyclic, a clock is not, in essence, a single system. Clocks consist of two distinct coupled systems, these being the cyclic and monotonic parts accordingly. The cyclic part triggers, at some point in its cycle, a distinct and discrete advance of the monotonic part, as the pendulum of a grandfather clock causes the second hand to tick upon reaching its escapement, advancing the second hand. Of course, a grandfather clock is cyclic in itself, but upon marking the end of each day, a calendar can be updated such that the overall observation of time remains monotonic.

As described thus far, the measurement of time may be refined by reducing the interval of a tick and classically there is no reason that this refinement may not, in principle, yield an arbitrary degree of accuracy. However, lurking in the small scales is the spectre of quantum mechanics and the Mandelstamm-Tamm uncertainty which effectively means that for any quantum clock there is an unavoidable minimum for the amount of time it takes for a wavepacket to move a distance equal to its standard deviation, for example. For a comprehensive review see [13]. This minimum is dependent upon the physical nature of the clock, so one might suspect that it is merely a practical problem to refine the tick indefinitely. However, it is conceivable that time is fundamentally discrete, with an indivisble tick.

A prime candidate for discretization is the Planck time - the unique time that can be formed from the dimensional constants of nature (Newton's constant, Planck's constant and the speed of light). The Planck time, around 10^{-42} seconds, is the time interval after the big bang on which quantum gravity effects are thought to be dominant, and the time-scale on which we would expect to see quantum corrections to Einstein's equations. The upshot is that the dynamics of the tick may in fact be unavailable as an observable, and thus the only reading of time one can get is that of the monotonic part, reading time as though from a digital clock.

Within Quantum Gravity, issues regarding implementation within the Hamiltonian framework are so severe that they have been dubbed "The Problem(s) of Time". Some state that

this consists of as many as eight separate yet connected issues [14]. The purpose of this paper is not to address such issues, but I will point out that even in the symmetry reduced mini-superspace models which are used ubiquitously in quantum cosmology time evolution is measured with respect to a scalar field. If one is even-handed in treating both geometrical and matter variables, one must apply the same "polymer quantization" [6] to both, and thus the universe is imbued with a discrete tick.

The role of a clock within a physical system is split into three parts by Busch [11, 12]. First, time as measured may be "external" or "pragmatic" - there is no coupling between the dynamical system being observed and the clock used to measure time within that system. In a classical sense, external time can be said to be measuring some aspect of Newton's absolute time on which dynamics takes place. Second, an "intrinsic" time is one which is measured as some quantity of the system itself, such as the readout of a digital display, or the position of the hands on the face of a watch. Third, "observable" or "event" time is a direct measurement of some physical quantity which is taken to be time itself, such as the position of a particle. Throughout this paper I shall always have the idea of intrinsic time in mind, as an external time can be made intrinsic simply by extending the configuration space of a system with external time by taking the product of a the configuration space with the configuration space of the clock. To those interested in relational observables, such as the cosmologist, intrinsic time is all one can work with - there is no external space on which a clock can live. In terms of quantum gravity, any physical clock must have a mass and thus interact gravitationally with all other components of the system through its action on space-time. Therefore cosmologically all observable time is intrinsic.

III. DISCRETE TIME

"God made the integers, all the rest is the work of man." - Kronecker.

In this section we will discuss the result of overlaying a discrete temporal structure onto a continuum dynamical system. The result of this will be to produce a system which contains only information about the discrete structure, removing all reference to the continuum. The background continuum is therefore used in the manner of Wittgenstein's ladder [7] - an external structure of convenience whose utility has no bearing on the resultant system. Constructions of this type are commonplace in physics. Prime examples include the use of a fiducial cell in cosmology against which to measure the size of the universe. Upon calculation, care must be taken to show that results are independent of the choice of cell. Similarly, General Relativity is a background independent theory, yet in practice calculations often involve coordinate choices. Again resulting physical quantities must be shown to be coordinate independent.

The mathematical structure of our background system shall consist of a manifold $\mathcal{M}$ of arbitrary dimension n which can be factlorized into configuration[1] states and a one-dimensional[2] temporal direction : $\mathcal{M} = \mathcal{C} \times \mathcal{T}$. The global topology of $\mathcal{M}$ will remain un-

[1] Here we shall work extensively in the position representation, but extension to the momentum representation of states is straightforward.

[2] It is conceivable that this is extended to multiple temporal directions, however this action seems to offer little to the discussion at hand other than to distract from the essential argument through mathematical

determined at present, however our underlying continuum mean that both $\mathcal{C}$ and $\mathcal{T}$ should be continuous. For reasons of convenience we will often think of $\mathcal{T}$ as being either S^1 (the circle) or $\mathbf{R}$ (the real line). Here we follow the construction of the "3+1 decomposition" of Arnowitt, Deser and Misner [8], however it is important to note that we are not splitting space-time but rather configuration-space-time. Physically time here is a further configuration variable, that of the system clock.

A. Treatment of position

The discretization of time can be performed in two distinct ways, which can be thought of as describing configurations instantaneously or over an interval. To illustrate this, let us consider two physical scenarios which each will describe a discretization of time on a particle moving in a circle. In the first situation the system is measured at fixed intervals with a stroboscopic light, whose flash is interpreted as being instantaneous[3]. This I will call "instantaneous measurement".

A second method would be to consider measurement to be smeared across an interval. This is effectively the complement of the above: a camera is set up to measure the system with its aperture open for an interval, closing instantaneously between intervals. The resulting measurement will not consist of a unique configuration but rather a trajectory across the interval which is to be considered a single measurement. Here the idea of an instant is used in analogy to that of Le Poidevin [16], in which an instant separates two intervals. Le Poidevin took this instant to be the "now" which separates past from future, I shall invoke it to separate temporal intervals. This treatment I will term "smeared measurement".

Both measurements of a system's position consist of replacing the time interval of our manifold by a discrete subgroup, for instance replacing a real interval $\mathcal{I}$ with a subset of the integers $\mathbf{Z}$ or replacing S^1 with S_n (the cyclic group of order n). Without loss of generality let us take each tick of the clock to be separated by an equal time interval with respect to the background time. This can be achieved by making a transformation of the background time, since an operational time obtained through any monotonic function of background time will serve will equal sufficiency. In replacing a continuous set with a discrete one we throw away an infinite amount of information about the system - the complete dynamics between "ticks of our clock.

To give an illustration of the differences between our two treatments consider the motion of a particle along a path. Suppose we describe the motion with background dynamics $x = x(t)$. One may ask for the relational observable X_T of a system, defined to mean the position of a particle at the time when the clock reads time to be T. In the instantaneous treatment X_T will be a single reading of a number, the unique position at that instant in background time. However in the smeared treatment the measurement will be a complete path, all positions occupied between background times T and $T + \delta T$ for temporal intervals δt.

"Instants are not parts of time, for time is not made up of instants any more than a magnitude is made of points" [9]

Some take instantaneous treatment of time is taken to induce a discretization of position

complexity.

[3] This could be the result of measuring a single photon reflected from a particle

space. Zeno's Dichotomy paradox [5] argues that one must occupy each point in a spatial interval whilst traversing the interval. In doing so, if time is discrete, one must take an infinite number of time steps, one at each position. This argument relies (unknowingly at the time of its inception) on the cardinality of any interval $\mathcal{I} \subset \mathcal{R}$ being larger than that of $\mathcal{Z}$ - there can be no mapping $f : \mathcal{Z} \to \mathcal{I}$ such that f is surjective. However in our treatment the motion is not taken to be a continuous one in discrete time - from one tick to the next one's position can change by an amount that is not infinitesimal.

The smeared treatment affects position space on a more fundamental level. Observations of position are no longer points, but rather (unordered) paths in the configuration manifold. In performing the smearing all information about the velocity through position space during a tick is lost - all that remains is a trajectory consisting of all the points occupied during the interval. As an example, consider a particle moving in one dimension, its position here during a tick would be the entire interval covered between its leftmost and rightmost positions covered by the continuum trajectory in this interval. Our observations will consist of a set of paths $\gamma_t : [0, 1] \to \mathcal{C}$ modulo an equivalence relation identifying two paths formed by any permutation of the unit interval. For continuous dynamics, this permutation should be smooth, but in principle there is no distinction for sudden leaps. Note that we have lost all sense of direction of the path - we cannot say from a single path whether the a particle moves left to right, or vice versa, or even starts and ends somewhere in the middle of the path, reaching ends at intermediate times. A path in which a particle revisits a previous point during a tick is permissable, and cannot be distinguished from one which does not.

If our background system is does not revisit the same point in position space during a two consecutive ticks there will be a unique point at which the paths are joined, and this may be taken to be the configuration of the background system at the instant between ticks. However if at any stage in these ticks a point in configuration space is revisited this reconstruction is no longer possible: As an example, consider a particle moving in a single dimension of which we make two observations. During the first tick we observe the path to cover the interval from 0 to 2. During the second, from 1 to 2. We cannot say, from these two observations whether the background dynamics had the paths join at 2, 1 or in fact any position in between, as all such dynamics would yield the same observations.

B. Treatment of velocities

Once position space has been defined, it is necessary to define velocities which will be fundamental to understanding dynamics. The usual method of defining a velocity is not available since defining

$$\dot{x} = Lim_{\delta t \to 0} \frac{x(t + \delta t) - x(t)}{\delta t} \tag{3.1}$$

relies crucially on this limit being accessible to the system. In a discrete time system, δt has a fundamental minimum, and furthermore, in the case of the smeared treatment, the action of subtraction of two intervals is not obviously defined. Let us first address the simpler case of instantaneous time.

The fletcher's paradox outlines a key difficulty in identifying velocity in instantaneous time. It can be formulated in the following way: Suppose one is given a set of instantaneous observations of a particle in motion (Zeno uses an arrow in flight). Then at each instant the particle has a fixed position and does not appear to move, and so there appears to be

no motion in sum. This is resolved by Russell's "at-at" treatment of motion [10] - that to travel between two points at different times is to occupy all intermediate points in the interval between.

In modern differential geometrical terminology, we describe the velocity of an object as a member of the tangent space to the position space at the point occupied by the system: $v \in \mathcal{T}_x\mathcal{C}$. One can define the obvious velocity in this instance by simply letting the removing the limit in 3.1. However, velocities defined this way are not necessarily members of $\mathcal{T}_x\mathcal{C}$. Recall that $\mathcal{T}_x\mathcal{C}$ is isomorphic to $\mathbf{R}^n$, where n is the dimension of the manifold $\mathcal{C}$. For a finite tick, $v\delta t$ must be a automorphism of $mathcalC$. In fact, the space of velocities at a point on $\mathcal{C}$, which we shall denote $\mathcal{V}_x\mathcal{C}$ must be isomorphic to $\mathcal{C}$, since velocity at a point is defined by the point in $\mathcal{C}$ at which one arrives after a single tick. Upon approach to the limit, the two points in the configuration space become close and once again we recover the tangent space. This naturally carries over to momentum which is no longer a member of the co-tangent space (the space of linear functionals of members of the tangent space) but in fact lives on the co-velocity space - the space of linear functionals of members of the velocity space.

Velocity in the smeared treatment is more complicated. Our observations consist of unordered paths in configuration space. In the case where any two adjacent paths overlap only in a unique point, one could define the velocity of a particle to be the length of the path covered divided by the length of the tick. However this gives only a minimum for the velocity of the particle in the background system, as the particle may not have uniformly covered this interval, but may have oscillated multiple times along the path during the tick. Consider the case of a man pacing back and forth on a boat - in a smeared observation he will appear to move with the approximate velocity of the boat, but on a closer inspection his speed at any time will not be this. Thus we obtain a lower bound for speed, defined to be the length of the path covered at any interval. This lower bound is the only reasonable candidate for a velocity in this setting, though we must assign a sense of direction to it. This is simple to do in the case that intersections are unique, but for multiply overlapping intervals the task becomes difficult. One possible assignment is to take the velocity as being oriented towards any part of a later path that is not contained in the prior one, or towards any point not contained in the subsequent path, but this of course does not deal with paths contained entirely within the both the preceding and subsequent path. In these cases the only seemingly logical thing to do is to set the velocity to zero.

Thus our space of velocities becomes the space of directed paths in $\mathcal{C}$. If $\mathcal{C}$ is compact, or indeed any configuration variable on $\mathcal{C}$ is compact, the space of velocities is again removed from $\mathcal{T}_x\mathcal{C}$. Those positions inhabiting a compact direction of position space must induce a finite dimension in velocity space.

Implementation of dynamics in the smeared system is considerably more difficult: The logical method would be to apply a similar form of numerical integration to the entire path, taking each point and moving it by the velocity of the path. In the case of a particle moving along a line, this is of course perfectly acceptable. However there is a question of accelerations, which are brought about by the application of force.

IV. DISCRETE TIME AS NUMERICAL INTEGRATION

""It always bothers me that, according to the laws as we understand them today, it takes a computing machine an infinite number of logical operations to figure out what goes on in

no matter how tiny a region of space, and no matter how tiny a region of time. How can all that be going on in that tiny space?" - Feynman.

The difference between velocity space and tangent space is known to those working on numerical integrators, though in different terms. These methods attempt to reproduce the continuum dynamics with a finite (though often variable) time-step. The Ströminger-Verlet method [17] (and improved velocity-Verlet [18]) uses the velocity and acceleration of the system both at an instant and in the preceding and succeeding instants to refine estimates of discrete motion to remain close to the continuum trajectory. The Runge-Kutta algorithm [19] is a more complex method involving the use of iterative methods to perform a time-step. One might be tempted, therefore, to state that a discrete system should run on a very accurate numerical integration method.

However, there is a problem with this system: The dynamics of the system of t_n depend upon an estimation of the configuration and hence accelerations of the system at t_{n+1}. Unless we allow for the (potential) future to act upon the present, this is not possible: A particle cannot know the accelerations it would feel if it were to move to a future location until it moves there. If one insists that all the information available to a particle is its current position and velocity (or momentum), none of these methods can be applied. Even if one were to allow it access to its complete prior history over discrete time, this would be insufficient, for one can construct two paths (using Hermite polynomials for a one dimensional example) each of which visit all prior configurations but differ on future locations. One is then driven towards describing the dynamics of discrete time using a method more akin to that of Euler for the numerical integration of a differential equation. This is clearly not optimal, as it will break conservation equations held in the background continuum (an example of which is given below). It has been shown by Ge and Marsden [15] that there is no fixed time step method which conserves symplectic structure, and Noether charges (ie all conserved quantities in the continuum system). In fact, all known numerical integrators which do conserve Noether charges require knowledge of the forces that the system will experience after the time-step has been implemented. In simulations this is perfectly acceptable, as one can tell the computer what model of physics to implement. However in reality this would require information about the future to be passed to a prior time, which is for most an unpalatable premise.

Thus one is forced into one of the following: We could drop the notion of discrete time as it appears incompatible with conservation laws. This is of course the most obvious route to take, but as I will argue in section VII there is good reason to believe that a discrete tick does exist. The second possibility is that a the continuum implications of Noether's theorem cannot apply in a discrete time setting, so physics breaks all conservation laws at some level. Since symmetry and conservation lie at the very heart of all modern physics, this is a very unwelcome path to take. Finally one could argue that dynamics must be altered in some fundamental way when one discretizes time. This I discuss further in section VI.

V. CYCLIC CONFIGURATIONS

The limitations of discrete treatments of time become apparent when one treats cyclic systems whose period with respect to background time is of the same order as the length of a tick. In such cases our ideas of basic physical quantities such as velocity can become ill-defined. We shall illustrate this problem by exploring a simple system: uniform circular motion. This systems has a long-standing association with the idea of time as for centuries

time has been displayed on a clock through the uniform circular motion of its hands. It is therefore fitting that this forms an exemplary model of issues in the discretization of time.

A. Uniform Circular Motion

Let us consider a simple system, the motion of a particle travelling with uniform angular velocity around a circle. The dynamics of our system is simple to describe: $\theta(t) = \theta_0 + \omega t$, in which ω is the angular velocity. For brevity of exposition let us choose a gauge in which $\theta_0 = 0$. Let us define the period of the system $\tau = 2\pi/\omega$.

In the instantaneous treatment of this system an observer makes a number measurements $\theta(t) = 0, \theta_1 ... \theta_n$ corresponding to the position of the particle at clock times 1 through n. However, upon being asked to reproduce the angular velocity of the system, such an observer runs into immediate ambiguities: She cannot determine whether or not the particle is moving clockwise with angular velocity $\theta_1/\delta t$ or anticlockwise with angular velocity $(2\pi - \theta_1)/\delta t$. In fact, to the observer all background dynamics of the form $\omega_n = \omega + 2\pi n/\delta t$ for $n \in \mathbf{Z}$ will yield indistinguishable observations. If δt is a multiple of τ the observer will not see the particle move at all. This is well known through the stroboscopic analogy as an optical illusion which has in the past been used to determine the frequency of, for example, cylinder firing in car engines.

This is a clear illustration of the difference between $\mathcal{V}_x\mathcal{C}$ and $\mathcal{T}_x\mathcal{C}$. The tangent space to the circle is the real line, but the topology of the space of velocities distinguishable with instantaneous time is that of the circle itself.

Let us consider dynamics, supposing that the circular motion of our particle is brought about by some central force. If one were to adopt a strict implementation of discrete time, taking as a basis for dynamics Newton's laws of motion, and updating both velocity and acceleration only once per time step (ie implementing the method of Euler) there is an obvious problem. Newton's laws of motion state that we must evolve motion along the tangent to the curve described (the velocity should be a member of $\mathcal{T}_x\mathcal{C}$. This immediately leads to the particle leaving the circle, breaking energy and angular momentum conservation. One could ask how bad this problem is: To first order in each of the variables involved the drift of the numerical integration increases as $\omega^2 r T \delta t/2$. Where T is the total time interval, divided into ticks δt, for a particle orbiting at radius r. For the orbit of the Earth around the sun since its formation this distance comes to around $2 \times 10^{-17} m$ if we assume the fundamental tick to be the Planck time. This is far less than the radius of a proton, for example, thus we are unlikely to be able to detect any effect of this granularity, even taking fast spinning objects such as pulsars into consideration.

One might argue that the implementation should involve some averaging or weighted average of an initial velocity and a final velocity, $v_f = v_i + a\delta t$ and so the implemented velocity for the tick should be $v_a = (v_i + v_f)/2$. However, this does not alleviate our problem - since it is simple to show that the final speed will be higher than the initial speed, thus the system has not conserved energy despite the presence only of a central force. One quickly runs into the theorem of Ge and Marsden[15] with such efforts.

At first sight the smeared treatment appears advantageous: It is indeed possible to determine the direction and angular velocity of the system for small values. For δt smaller than τ the measurement of angular velocity can be performed by simply taking length of the complete interval covered in each tick and dividing by the length of the ticks. However, once the tick length becomes greater than or equal to the period of motion, all observations

become indistinguishable, as each observation will yield the entire circle as the path covered in that tick. If the tick length is one half the period of motion or greater, we will be unable to distinguish in which direction the particle is moving without resorting to a trick: It will be moving counter to the direction in which the paths defined as the complete circle minus the path covered in a tick is moving. This again is ambiguous at exact division of the period into half.

VI. NEW DYNAMICS

If one wishes to keep both discrete time and conserved quantities in the manner of continuum systems, then one is forced to implement dynamics in a way that differs from the usual methods of Newton, Lagrange et al. Classical evolution is implemented through the use of a second order differential equation:

$$\ddot{q} = f(q, \dot{q}) \tag{6.1}$$

From this prescription one can find the trajectory of a system. However as has been noted above, if one implements finite time-steps the trajectory that one recovers differs from that of the continuum, since evolution through a finite length along a vector in the tangent space does not necessarily remain in the space compatible with conservation of Noether charges.

One way in which conservation can be implemented is to enforce "by hand" conservation which is brought about by symmetries of the system. I shall construct such a system to show that in principle such systems can exist, without making claims as to their accuracy or fidelity to known physics. This manner of ensuring that fundamental physical quantities such as energy and momentum are conserved is similar in spirit to that used in discrete Hamiltonian systems in engineering for example [20].

The method is implemented as follows: For a system, establish those charges which are to be conserved due to symmetry using the method of Noether. For an initial value problem, find the surface(s) within phase-space such that this conservation takes place, $\mathcal{E}$.

Dynamics is implemented by using finite time-step methods with the fundamental tick: $q'(t+\delta t) = q(t) + \dot{q}(t)\delta t, \dot{q}'(t+\delta t) = \dot{q}(t) + f(q,\dot{q})\delta t$. Finally one finds the point of $\mathcal{E}$ such that $q, \dot{q}$ are closest (with respect to some given metric) to $q', \dot{q}'$.

Implementing this method in the case of an inverse square central force yields approximately the correct dynamics: Conservation of the angular momentum and energy are implemented, and if one measures distances in configuration by $D := \sqrt{\tau^2 \Delta\dot{q}^2 + \Delta q^2}$ then one finds that the orbit recovers the clasical limit when $\dot{r}\tau$ is small compared to r.

This method has clear limitations. It depends upon the choice of measure used on configuration space to define a notion of distance from the conservation surface. Further the link between velocity and position become somewhat tenuous - velocity is no longer measurable directly as a change in position, but rather is an intrinsic quality of the system. Of course, this is the nature of any instantaneous velocity measured in a discrete system as we have seen, but nonetheless this remains aesthetically displeasing. The dynamics expressed here will differ considerably from continuum dynamics in the case of chaotic systems, as surfaces of configuration space compatible with conservation of fundamental charges may become close to one another, allowing a system to jump from one classical trajectory to the next.

Despite the limitations, what we have established is a method by which a discretized system may retain conservation laws mirroring those of the classical system on which it

is based. The method is contrived and arbitrary, relying heavily on implementation of conservation by fiat, but is proof that such solutions can exist without having to resort to retrodiction of motion in the manner used by numerical integrators, or varying of time step.

VII. DISCRETE TIME AND QUANTUM GRAVITY

There have been several attempts to quantize gravity through a discretization of geometry. Perhaps the most famous of these is Regge calculus [21] in which space-time is broken down into blocks which are internally flat (ie the inside is a section of Minkowski space) and whose curvature lies entirely at the interface between two blocks. This method has the appearance of breaking up a curve into a series of straight lines, with all the curvature being concentrated at the corners.

The causal dynamical triangulations programme [22] makes a splitting of space-time into space and time. Time is implemented in discrete steps, with the layer between being "triangulated" by a set of polyhedra. Once again, curvature lives on the interface between these polyhedra. The programme attempts to reconstruct a path-integral approach to quantum gravity by restricting the integral over geometry to being one over such triangulations. This has all the hallmarks of an instantaneous discretization of time - each layer of polyhedra is overlaid on the last at a discrete time interval. Sorkin's causal set model [23] follows a similar scheme, but rather than triangulating space, takes a discrete "sprinkling" of points into space-time to create its structure. Time here would again appear instantaneous for any finite number of points, though the interval of any time step would be arbitrary.

Collins et al. [24] show that when one takes into consideration the interactions of elementary particles a very tight bound is placed upon the breaking of Lorentz invariance. A granularity of space-time on the Planck scale does not necessarily induce a factor of E/E_{pl} in corrections to the continuum model for a system of energy E (wherein E_{pl} denotes the Planck energy). Does this rule out discrete time?

Rovelli and Speziale [25] argue that this is not in fact the case for discretization of length. Just as the discretization of angular momentum does not spoil rotational invariance, the existence of a minimal length (or in the case considered by Rovelli and Speziale area) does not break Lorentz invariance. A Lorentz transformation between two observers necessarily alters the operators used to measure length, which will not commute. It is entirely plausible that an analogous argument can be made for any measurement of time.

As I have previously noted, in treatments of cosmology, time must be intrinsic in the manner of Busch - there can be no external parameters which are used to describe time. Modern treatments of quantum cosmology, such as Loop Quantum Cosmology, employ a matter field as a clock. In the simplest models this is a massless scalar field, so its motion is monotonic, and observations of the field value give a time for the system [26].

In simplicial decompositions of space, such as those used in spin-foam theories, time will again appear to have a discrete nature. Here one decomposes four dimensional space into a set of simplices which fill a region, and a time slicing can be applied by cutting across these simplices at an instant. However, due to diffeomorphism symmetry, two slices are indistinguishable if the networks dual to the simplices have the same structure, so when viewed from this perspective, time is simply the evolution of networks, which are inherently discrete - only the topology of a network is important as there is no notion of geometry. Therefore each change to the network, such as adding a new link or node, is a discrete step, and must appear to happen instantaneously in the continuum theory, or in a "tick" in the

discrete theory.

There are two obvious issues that must be addressed when applying discrete time to cosmology. The first is that the Planck time is precisely the time at which quantum effects are thought to be dominant. However, by our definition there would only be a single tick between this point and the singularity itself. Within LQC, there may be as few as 20 ticks during which the density is high enough for quantum effects to be significant. Yet these differences are to be the time in which quantum gravity replaces GR. It appears unlikely that such a narrow set of points can have so large an effect on global dynamics.

The second problem to be addressed is that if one does have singularity resolution in a continuum theory due to some modified dynamics in which the universe "turns around" from contraction to expansion, the turn around could be missed in the discrete time step. As an analogy, consider a particle moving in a one dimensional potential well towards a thin yet high wall. In the continuum theory, the particle will reflect off the wall and its trajectory reverse, but in the discrete theory a high enough velocity will allow the particle in some cases not to "see" the wall at all - it will simply be on one side at one time and the opposite one tick later. In LQC this scenario is avoided as the singularity is disallowed by the constraint, but in other theories in which singularity resolution is brought about, this effect may be significant.

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