On the Probabilistic Compatibility of Special Relativity and Quantum Mechanics

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Abstract

In this paper we introduce the three main notions of probability used by physicists and discuss how these are to be used when invoking spacelike separated observers in a relativistic format. We discuss a standard EPRB experiment and concentrate upon problems of the interpretation of probabilities. We promote a particularly conservative interpretation of this experiment (which need not invoke an objective notion of collapse) where probabilities are, tentatively, passively Lorentz invariant. We also argue that the Heisenberg picture is preferable in relativistic situations due to a conflict between the Schrödinger picture and passive Lorentz transformations of probabilities. Throughout most of this paper we discuss the relative frequency interpretation of probability—as this is most commonly used. We also introduce the logically necessary notion of ‘prior-frequency’ in discussing whether the choice by an observer can have any causal effect upon the measurement results of another. We also critically examine the foundational use of relative frequency in no-signalling theorems. We argue that SQT and SR are probabilistically compatible, although we do not discuss whether they are compatible on the level of individual events.

KEYWORDS: Peaceful Coexistence, Bayesian Probability, Lorentz Invariance, Wavefunction Collapse.

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Motivation

In the literature it is often stated that there is some kind of peaceful coexistence between Special Relativity (SR) and Standard Quantum Theory (SQT). This is usually expressed by the statement that SQT cannot be used to send informative signals at faster than the speed of light. Sometimes it is also stated in stronger terms, with the probabilities of outcomes of experiments being manifestly Lorentz invariant.

With so many interpretations of SQT abound, it would seem prudent to analyse this notion of peaceful coexistence and see which interpretations express
it most cogently. For example, often the more realist interpretations of SQT (those that involve an objective notion of wavefunction collapse) are said to be inconsistent with SR. In this paper we shall take a very conservative stance; we shall only discuss the ‘probabilities’ of outcomes of experiments and thus this issue shall be skirted in the main discussion. All arguments below should be independent of any ontological stance one takes, as long as one accepts a notion of collapse. We shall not discuss no-collapse pictures in this paper.

The aim of this paper is to try and find out how well our operational notions of probability mesh with SQT and SR, and whether we can promote one interpretation of probability over another. We aim to analyse peaceful coexistence in the light of these foundational issues, and show causality to be ambiguously defined on a probabilistic level (this is of course almost trivial but it is useful to state it pedagogically). Recently Stapp has attempted a proof of nonlocality in SQT by only invoking logical relationships between individual events. Stapp’s approach has recently been criticised by Shimony. We argue, here, only that nonlocality cannot be proved or disproved using probabilistic concepts alone; we do this in a way that is pedagogically useful, through the introduction of a notion we call “prior-frequency”.

But how are we to interpret ‘probability’? There are a few different notions of probability that physicists use, most of which can be put somewhere within the following (surprisingly broad) three categories:

1. **Relative Frequency.** This is the usual notion of ‘probability’ discussed by physicists—if just for the simplicity of its formulation. Here, relative frequencies are defined relative to infinite ensembles of ‘identical’ experiments. The relative frequency of an event being the proportion of the ensemble in which this event occurred (in the limit of infinite trials). Within this interpretation, relative frequencies represent the unpredictability of the outcomes of a repeat of a single experiment. It is, however, ambiguous where the origin of unpredictability lies. It could either originate from an inherent unpredictability of the system under investigation, or an inherent unpredictability within the ‘identical’ setups used to probe that system, or both. For a cogent description of this view see Ref. 5.

2. **Bayesian Probability.** Here, probabilities are not necessarily related to frequencies. The Bayesian view is to take the theory of probability as a “logic of inference”. There are a variety of Bayesian standpoints but they all share the idea that probabilities are not objective properties of things or ensembles, but rather, are subjective quantities that depend on both prior-information and data. This is not to say that probabilities are considered wholly subjective—quite the opposite; they are sometimes (as in the formalism of Jaynes) considered uniquely defined for all observers with the same prior-information and data as long as they use all the prior-information they have. Some authors treat Bayesian probability in a slightly more, but still not wholly, subjective manner. For a classic introduction to the use of Bayesian probability in SQT see Fuchs’ recent treatise. Like relative frequencies, Bayesian probabilities are carriers of incomplete information, but in the Bayesian case they depend, either uniquely or in part, upon the prior-information and the experimental setup; else they would be entirely subjective. One argument that is
often given against the Bayesian viewpoint is that of circularity; we seem to be defining probability with a notion of ‘prior-probability’. Suffice to say, this may simply be a problem of etymology, but we shall not go into that here.

3. Propensity. Here, probabilities are considered objective, and represent the actual ‘likelihood’, or ‘propensity’, of an event. A propensity is the tendency of a possibility to realise itself upon repetition of the ‘same’ experiment. This view was proposed by Popper\(^9\) in order to be able to apply the notion of probability to objective single systems (where ‘system’ here includes a notion of the experimental setup). Although it is to be noted that, within this interpretation, propensities are only ever calculated via the relative frequency method.

In the remainder of this paper we shall introduce the Bohm version of the classic Einstein-Podolsky-Rosen (henceforth EPRB) thought-experiment, and we will show that one cannot argue for (or against) causal relations between events using these notions of probability alone. In doing so we will argue for the use of the Heisenberg picture and introduce the logically necessary notion of “prior-frequency”. This will also provide impetus to discuss the foundational use of probability in no-signalling theorems.

Peaceful Coexistence

If we are, tentatively, to discuss the peaceful coexistence of two theories we must, in some sense, ‘trust’ those two theories\(^10\). If we trust SR then two spacelike separated events cannot be causally connected. This, however, is very different to saying that two spacelike observers cannot be logically connected. We shall elaborate upon this as we go.

If we trust SQT then we can predict the frequencies (or probabilities or propensities) of outcomes of single-system measurements. And, if we also posit some kind of collapse (by whatever means or justification) then we can also, we believe, predict the frequencies of outcomes of entangled subsystems, even if they are spacelike separated.

To claim ‘peaceful coexistence’ is to suggest that, trusting both SR and SQT, neither are disobeyed in an overt manner. A naïve view would suggest that this is impossible since any change in frequencies (or probabilities or propensities) is often said to occur instantaneously—where ‘instantaneous’ is usually defined in whatever Lorentz frame we are discussing. However, it is usually agreed that no useful information transfer between spacelike separated observers can occur through this method. It is upon this fact that many take SR and SQT to be loosely compatible. We say ‘loosely’ exactly because we might, perhaps naïvely, take the two theories to be incommensurable. We say ‘compatible’ exactly because we feel the spirit of SR remains intact when no useful information transfer can occur at faster than the speed of light.

An EPRB experiment

In this section we shall set up a framework for the discussion of peaceful coexistence. We will use the standard EPRB setup, using an entangled initial
spin-state for two correlated electrons prepared at a source $S_0$, and two spacelike separated observers $S_1$ and $S_2$ that use the Hilbert spaces $H_1$ and $H_2$ to describe their respective subsystems. Any entangled state is discussed in reference to $H_1 \otimes H_2$. For the sake of argument, let us frame the discussion below in terms of relative frequency; we shall discuss the other interpretations of probability once we have set up the problem.

![Figure 1. Frame $\Sigma$, where $t_1 = t_2$.](image)

The entangled initial state represents two electrons that are released, say, in opposite directions. At a later time, $S_1$ and $S_2$ each have access to only one of the two electrons, and they each have a choice of, say, two different spin directions in which to measure their received electron. For clarity, let us call the spins in these directions $S_x$ and $S_z$ and assume that both the observers have communicated previously so as to be discussing the same directions.

Let us take a Lorentz frame $\Sigma$ (see Fig. 1) in which $S_1$ and $S_2$’s measurements are considered simultaneous. In this frame we discuss the following entangled initial state (where the notation that refers to different subsystems is assumed obvious):

$$|\psi\rangle = \frac{1}{\sqrt{2}}(|x+\rangle_1 \otimes |x-\rangle_2 - |x-\rangle_1 \otimes |x+\rangle_2)$$  (1)

Let us, for the moment, frame this discussion in terms of the Schrödinger picture and let us discuss the naïvest form of collapse that we can think of—where collapse is instantaneous in whichever frame of reference we are presently discussing. We consider $S_1$ and, without loss of generality, we look at the case where she chooses to measure the spin in the $x$-direction and $S_2$ measures spin in the $z$-direction. When $S_1$ measures $S_x$ she will, in a single run of the experiment, either receive the result $S_x = +1$ or $S_x = -1$ for particle one. Since, in a single run, she will only ever receive one or the other, let us discuss the case where she receives $S_x = +1$ for her electron. Regardless of what we take ‘state’ to

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1One could also frame this discussion in terms of non-identical particles if one wished.

2It doesn’t matter whether we consider whether this collapse process is ‘objective’ or ‘subjective’ because the only things we ever verify in SQT are the final probabilities—so it is only these probabilities that we should query philosophically.

3This step is perhaps dubious from a probabilistic standpoint due to the infinite nature of ensembles, and we reserve the right to query it later.
mean (i.e. whether it is defined physically or with respect to ensembles), $S_1$ believes she ‘has’ the state $|x+\rangle_1$ and that, by the entanglement property and the simultaneity of the measurements, $S_2$ must have ‘received’ the state $|x-\rangle_2$. $S_2$, however, does not know what $S_1$ has received and he will not use the state $|x-\rangle_2$ as his initial state; he will instead measure $\mathbb{I} \otimes S_z$ using the initial state $|\psi\rangle$. $S_1$ will use $|x-\rangle_2$ to predict the relative frequency of $S_2$’s results given her’s, yet $S_2$ will use $|\psi\rangle$. Thus, $S_1$ and $S_2$ use different methods to discuss the relative frequencies of the ‘same’ events, and there is no a priori reason why they should be the same, for they are calculated by different methods; they are not discussing the same ensembles. $S_1$ uses a pre-selected ensemble where she always receives the result $S_x = +1$, which is trivially different from the ensemble $S_2$ uses. It is also trivial that these two relative frequencies could have different limiting values. We shall discuss the possibility of $S_1$ invoking an unselective ensemble later.

On the Conflict Between the Schrödinger Picture and Passive Lorentz Invariance of Probabilities

We have argued above that the relative frequencies that are predicted for an ‘event’ by spacelike separated observers are not the same and, furthermore, that they need not be the same for logical consistency as long as one accepts that different observers are necessarily discussing different ensembles. If you do not agree with this statement then please continue as we shall be discussing more ‘objective’ relative frequencies below. But what about the frequencies predicted by observers discussing the same measurements in a different Lorentz frame? Here, we are discussing passive Lorentz transformations rather than active ones. $S_1$ can ask “if I were the observer $S'_1$ in frame $\Sigma'$ then what probability would I infer for $S'_2$’s results”. $S_1$ hopes that such inferences should be the same in all frames—else there may exist a way to physically differentiate frames of reference; observers in different frames would predict different probabilities for the ‘same’ measurements. If the Poincaré group induces a symmetry on the Hilbert space then a Lorentz transformation of states is given by a unitary operator (see chapter 2 of Ref. 11). In this case, due to the cyclic trace property of frequency calculations, frequencies are defined to be Lorentz invariant. But, if we wish to invoke any collapse postulate, the situation is more complicated than this; we must take into account any unitary evolution and collapse of states within each frame.

In frame $\Sigma'$—where $S'_1$’s measurement occurs before $S'_2$’s—the naïvest view possible is, again, to suggest that the collapse occurs at the time of $S'_1$’s measurement. Again let us fix that $S'_1$ measures $S_x$ and receives, in the Schrödinger picture, the result $S_x = +1$ at time $t'_1$, and that, at time $t'_2$, $S'_2$ measures $S_z$. The frequency which $S'_1$ predicts $S'_2$ to receive the result $S_z = +1$ given that she received $S_x = +1$ is:

$$|\langle z + |\tilde{U}(t'_2 - t'_1)|x-\rangle|^2$$

(2)

Here, we have dropped the subscripts on bras and kets as we are discussing states in $\mathcal{H}_2$ alone. The unitary operator $\tilde{U}(t'_2 - t'_1)$ is the evolution\(^{4}\) opera-\(^{4}\)We are implicitly assuming that there are no nonlocal interaction terms in the total hamiltonian of $\mathcal{H}_1 \otimes \mathcal{H}_2$.
tor in $\mathcal{H}_2$. However, frequency $2$ is not obviously Lorentz invariant. When discussing a particular measurement in the Schrödinger picture, the quantum state invoked collapses to one of a fixed set of post-measurement states at each use of that measurement (the eigenstates of the operator that represents that measurement)—presumably in whatever frame of reference we are discussing because, in the Schrödinger picture, the states which correspond to the outcomes of a particular measurement are independent of the time (and spatial position) of the measurement. At each measurement we are, in a sense, losing information about the fiducial time at which the initial state was prepared. Also, it seems impossible to make such a setup passively Lorentz invariant; if we change frames then states are not going to change, but the unitary evolution between them does. This ruins any passive Lorentz invariance that we might tentatively have. This naïve notion of “simultaneous collapse in all frames” is therefore not explicitly Lorentz invariant when discussed in the Schrödinger picture. This conflict between the Schrödinger picture and the Lorentz invariance of probabilities is independent of any ontological significance that one assigns to the wavefunction; we are dealing with probabilities alone and, accepting a collapse hypothesis, frequency $2$ cannot be made explicitly Lorentz invariant. We cannot transform post-measurement states in the Schrödinger picture and we thus cannot maintain any Lorentz invariance of frequency $2$. Thus this conflict comes about despite the formal equivalence of the Schrödinger and Heisenberg pictures due to the interpretation we must assign to post-measurement states within the Schrödinger picture.

Let us now try and describe the same situation in the Heisenberg picture. In the Heisenberg picture, the possible post-measurement states depend upon the time (and, presumably, the spatial position) of the measurement—hence, to put it in a trite manner, in $\Sigma'$ we measure $S'_x$ and $S'_z$ and not $S_x$ and $S_z$.

If collapse occurs at time $t'_1$ then the resulting Heisenberg state after measurement in subsystem one is given as $|x' + (t'_1 - t'_0)\rangle$; $S'_1$ then infers that, at time $t'_1$, the Heisenberg state of subsystem two is the collapsed state $|x' - (t'_1 - t'_0)\rangle$. $S'_1$ can then infer, she believes, that the relative frequency of $S'_2$’s result $S'_z = +1$ should be the frequency given by:
This is $S_1''$’s inference about the frequencies of $S_2''$’s results and has no bearing upon $S_2''$’s inferences—they are space-like separated. In relativistic situations, the transformation of states explicitly depend on the Lorentz transformation invoked; states transform unitarily like:

$$|\psi\rangle \rightarrow \hat{U}(\Lambda, a)|\psi\rangle$$  \hspace{1cm} (4)

where $\Lambda$ is a Lorentz transformation, and $a$ is a translation parameter\textsuperscript{[11]} . Both Heisenberg states in (3) are defined in $\mathcal{H}_2$ (note that this Hilbert space is not primed as we are presuming the Poincaré group induces a symmetry on $\mathcal{H}_2$) and the frequency (3) is explicitly Lorentz invariant. If one also wishes for the covariance of the collapse process itself, then one might also invoke the methods of Hellwig and Kraus\textsuperscript{[13]} .

Although the Schrödinger and Heisenberg pictures are formally equivalent in a non-relativistic format, the Schrödinger picture is ambiguous when used to invoke passive Lorentz transformations of probabilities. In the Schrödinger picture, post-measurement states are independent of the time of the measurement and it is exactly these states that we wish to transform when Lorentz transforming probabilities. It is by this that I suggest there is a conflict between the Schrödinger picture and the tentative Lorentz invariance of probabilities when discussing relativistic situations.

Discussion

We have not yet discussed, however, whether any of the above is consistent with the change in time ordering that can come about when discussing changes in frames of reference with respect to spacelike separated observers. If we Lorentz transform frequency (3) to a frame $\Sigma''$ such that $t_2'' < t_1''$ then, as we discussed above, the frequency predicted is the same as long as the Poincaré group is a symmetry of $\mathcal{H}_2$. We must ask, however, whether the change in temporal ordering is consistent with the interpretation of frequency (3). In $\Sigma''$, $S_1''$’s measurement occurs after $S_2''$’s. If we wish to discuss $S_1''$’s prediction for the frequency of $S_2''$’s result then, with the new temporal ordering, we have to invoke a collapse in the second subsystem that occurs in the future of $S_2''$’s measurement. This may seem a weird interpretation but it is completely consistent with a post-selection of a quantum state in an ensemble\textsuperscript{[14]} . An observer is allowed to reason like the following (as long as he has no information about the previous measurement): “What does my result suggest about the unknown result of a previous known experiment?”. $S_1''$ can discuss an ensemble where she post-selects her received state, and using this ensemble she can predict the frequency of results of $S_2''$’s previous measurement given hers. Although $S_1''$ will not ‘know’ which measurement $S_2''$ made, they can confer before the measurements take place and collaborate in their actions—and given that $S_2''$ doesn’t mischievously measure something else entirely then $S_1''$’s predictions for the frequencies of $S_2''$’s results in this post-selected ensemble will, presumably, be correct.

Thus, it seems, we can discuss the results of SQT in relativistic situations consistently. Frequencies predicted by any given observer can, tentatively, be passively Lorentz invariant in the Heisenberg picture and can be interpreted...
consistently with respect to different time orderings due to the ensemble nature of relative frequencies. This might suggest that SQT and SR don’t ‘peacefully coexist’: they might be more than loosely compatible. To put it gnomically, perhaps SR and SQT do not peacefully coexist, but, rather, perhaps spacelike separated observers do.

Peaceful coexistence is usually expressed through no-signalling theorems, where it is proved that if we ‘trace out’ one observer’s influence we get the same value of relative frequency as we would have received had that observer not made their measurement at all. One problem with such arguments is that it is not clear what ensemble, and equivalently what relative frequency, we are discussing. No-signalling theorems (see, for example, Ref. 2) tend to be proved by invoking only local measurements (which are automatically commutative due to the nature of the tensor product) and they prove that the relative frequency of one observer’s (say Bob’s) result given that the other observer (say Alice) made a certain measurement (but ‘tracing out’ Alice’s results) has the same value as the relative frequency of Bob’s result given that Alice did not make her measurement. During these proofs no use of spacelike separation is made, so these theorems prove that no two subsystems can signal between each other given local measurements. These theorems, however, need not prove anything about causality; regardless of the fact that the value of the relative frequencies are the same, the relative frequencies are defined by ensembles that are counterfactually distinct—one is defined in an ensemble where Alice always makes her measurement (in every element of the ensemble) and the other is defined in an ensemble where Alice never makes her measurement. These proofs only prove the happy coincidence that two counterfactually distinct ensembles happen to have the same limiting value of the relative frequencies of a certain event. In order to prove anything about causality we must model a choice, between these two counterfactually distinct cases, that is well-defined at a single trial and in a small spacetime region. So, strictly speaking, no-signalling theorems do not prove that relativistic causality is maintained; we may even require additional ‘common sense’ assumptions (like in Bell inequality arguments or the recent argument of Stapp) about the relations between individual events in order to even discuss causality.
No-signalling theorems suggest that two different relative frequencies must have the same value for local measurements, but they say nothing about the logical distinction of the two relative frequencies; in this sense the two relative frequencies may 'objectively' have the same value but they cannot 'objectively' be the same relative frequency. In order to even prove no-signalling theorems one must assume that there is no a priori reason, regardless of causality, for the frequencies to have the same limiting value in order to prove that they do, otherwise the theorem would be a tautology. No two equals are the same. Probabilities with the same value are not the same probabilities. So if we are to call relative frequencies 'objective', in the sense that all observers agree that they are correct, we must note that their very definition depends upon the ensembles used when invoking them; if we wish to discuss the 'actual' relative frequency we must invoke the 'actual' ensemble used.

‘Objective’ Relative Frequencies

One argument against the ‘relativity’ of relative frequencies might be that relative frequencies are ‘objective’ properties of ensemble experiments—could we not simply set up an experiment, let it run, and then discuss, retrospectively, the relative frequencies of each outcome completely objectively? In a given experiment (where no choices are made) we have a well defined notion of relative frequency because we have fixed the discussion to a single ensemble of results with a well defined sampling and limiting process. Returning to Σ', to get the ‘actual’ relative frequencies of ‘the’ experiment we simply fix what S'_1 and S'_2 measure and record the results of each subsystem upon each repetition. Thus we define an ensemble such that in each element of the ensemble the measurements for each observer are fixed, and are made in every element of the ensemble, and thus we can have a well defined relative frequency that all will agree is correct. Say S'_1 wishes to discuss the relative frequency of S'_2's result; what she will do, theoretically, is set up an ensemble made up of two counterfactually distinct subensembles; one subensemble being the one where S'_1 received the result S'_x = +1 and the other where she received S'_x = −1. Let us call the frequency that S'_1 predicts for each of her own results ν(S'_x = −1|ψ′) and ν(S'_x = +1|ψ′) and, presumably, these are the weights she applies to each subensemble respectively. In the Heisenberg picture (we drop the time labels however), the relative frequency she shall predict for S'_2 to receive the result S'_z = +1 is (assuming there are no problems with combining frequencies that are in the limit of infinite trials):

\[ |\langle \mathbf{z'} + |\mathbf{x'}-\rangle|^2 \nu(S'_x = +1|\psi') + |\langle \mathbf{z'} + |\mathbf{x'}+\rangle|^2 \nu(S'_x = -1|\psi') \] (5)

There is no a priori reason, regardless of issues about causality, why this should give the same relative frequency that S'_2 predicts given that S'_1 didn’t make a measurement, namely ν(S'_z = +1|ψ'). I use the term ‘a priori’ in the sense that there is no logical reason why they should be the same; regardless of whether they have the same value, they are different relative frequencies—they are defined on counterfactually distinct ensembles. If there is no logical reason they should be the same why claim that if they are different this is due to causal reasons? We are discussing different ensembles so it is a tautology that the relative frequencies could be different—there is no a priori reason why
causality should be invoked to explain any difference.

One should sample the results in the way that they ‘actually’ occur before one takes the limit—else, since infinities rarely follow common sense, one may get nonsense. There is nothing wrong with saying “if the world were different we get different frequencies of results”, and surely we should not claim that this is a causal influence. The frequencies might be different for reasons related to the logical distinction of the two distinct worlds or ensembles. And given that we cannot claim that there is a causal influence, we also cannot claim that there is none. This obviously applies to the classical use of relative frequencies at spacelike separation as well.

One might try and get around the above argument by discussing either of two ensembles (with the knowledge that these are the only two cases), one where $S'_1$’s measurement always takes place and one where it always does not take place; if the two relevant relative frequencies had different limiting values then $S'_2$ might be able to infer whether $S'_1$ made her measurement. In this case, would we be able to infer a causal influence? Should such signals, if they were to be possible, be considered causal or logical things? $S'_1$ and $S'_2$ have obviously communicated previously in order to set up the experiment, and $S'_2$ would have to decide right from the outset (but without $S'_2$ knowing) whether to make her measurement in every element of the ensemble or none. This the logical equivalent of a common cause. In the hypothetical situation where the two relevant relative frequencies did not have the same limiting values then some would say that $S'_2$ could infer whether $S'_1$ made her measurements or not, even though she is spacelike separated from him. Trusting that the spirit of SR should remain intact, many of us would rather the two frequencies had the same limiting values so that ‘information’ cannot travel at faster than the speed of light.

In this hypothetical world (where no-signalling theorems are false), in order to access the relevant information $S'_2$ has had to collaborate with $S'_1$ to ensure that she will always measure, or always not-measure, in each element of the ensemble (but not which choice she will make). He then must experimentally determine the relative frequency of his result and infer from that whether $S'_1$ made her measurements or not (even though this is in principle impossible, due to the infinity of experimental runs required, there would come a point where he is confident that he can infer $S'_1$’s choice with a given accuracy). But could we then infer that $S'_1$’s measurement causally effected $S'_2$’s results? We could definitely infer that $S'_1$’s measurement affected $S'_2$’s relative frequencies but in order to argue for causal relations we must model $S'_1$’s choice of measurement at a single trial, which is a different matter entirely. Note also that $S'_1$ and $S'_2$ have collaborated significantly so it is not clear how we can distinguish common causes from uncommon ones (and these causes from logical collaborative information that both observers have).

Lets say $S'_1$ can and does signal to $S'_2$ that she made the measurement in every element of the ensemble. $S'_2$ knew from the outset that she would either make the measurement in every trial or not make the measurement in every trial, for they have collaborated so as to complete exactly this experiment, and he can infer the relative frequencies of his results conditional on each possibility. In what way would $S'_2$ be informed by this signal? He cannot use this ‘information’ to predict anything useful happening around him locally. He would be ‘informed’ about a spacelike separated region, but not about his own. But
by its very nature he cannot know what is going on in a spacelike separated region without collaboration and trust. This highlights that we are discussing inference and not influence—an inference conditional on the fact that he has collaborated with $S'_1$ and knows exactly what she might do (and that she will do exactly the same thing in every repeat of the experiment while he works out the relative frequencies of his results). Again, this might be the logical equivalent of a common cause. He can infer what measurement she made because he collaborated with her and knew what his relative frequencies would be in each case, and he trusts that only these two cases are apt. Thus signalling need not obey SR in an a priori sense as we are discussing inferences and not material influences. Therefore no-signalling theorems prove something slightly different to SR. In the Bayesian approach this latter statement is trivial as probabilities are explicitly considered inferences and not objective properties of things.

Such an experiment as above would seem a lot of effort to go to to transmit a single bit of information; but anyway, no-signalling theorems prove that such a method could not be used to signal such information between spatially separate observers since the relative frequencies do have the same value. SR only proves that light and matter cannot travel faster than $c$ and thus no-signalling theorems go further and show that ‘information’ cannot be signalled, in the way discussed above, regardless of separation (even though it would be a particularly inefficient way to transmit information anyway as it requires many trials to differentiate relative frequencies). I am only arguing here that we cannot consider such signals as strictly causal in the first place.

In order to show this, let us look a little deeper at our model of this ‘choice’ that $S'_1$ makes. In order for the experiment to go as planned $S'_1$ needs to ‘choose’ the same ‘choice’ in each element of the ensemble. $S'_2$ must be sure that she has done the same thing each time the experiment is repeated. In each trial $S'_1$ must make her ‘choice’ when she becomes spacelike separated from $S'_2$, but she must choose the same ‘choice’ each time. This ‘choice’ is no choice. If we want to model a ‘real’ choice, where she has the possibility of choosing either option in each run of the experiment, then we come across an inherent ambiguity in the discussion. Does $S'_1$, say, choose to measure $S'_x$ half the time, or one percent of the time? We have to know the relative frequency of her choice in order to set up the model. Yes, $S'_1$’s choice of whether she will measure or not can, tentatively, be causally separate from $S'_2$’s measurement, but we must invoke two ensembles weighted by the frequency of $S'_1$’s choice—we will call this a super-ensemble and the frequency of $S'_1$’s choice a prior-frequency. This prior-frequency, if it is well-defined, can be chosen beforehand, or inferred retrospectively, but, either way, this is trans-temporal (or rather ‘trans-trial’) knowledge which must be taken into account. Each such choice for the frequency of $S'_1$’s choice is a counterfactually distinct case (in terms of ensembles). Either she measures with one prior-frequency or she measures with another.

$S'_2$ cannot know this prior-frequency of $S'_1$’s choice unless they decide together beforehand and in such a case $S'_2$ is not informed of anything when they complete the experiment. The case where she either measures in every element or in no element of the ensemble cannot be described by a single prior-frequency and this is the only case where $S'_2$ might learn anything. No-signalling theorems prove that he cannot learn anything at all in this case but, even if he could, we cannot prove that an event at a single trial causes anything to change in the spacelike separated region. To put it gnomically, if something that is not
defined at a single trial affects results then why call it a ‘cause’? Rather, this ‘something’ is related to the logical relations between trials and is non-trivial. In both of the two possible ensembles $S'_1$ knew from the outset which choice she would make in all trials. This might be the logical analogy of a common cause: perhaps we could call it ‘common knowledge’ in the sense that both observers know that $S'_1$’s choice is made in all trials. The only time we could prove anything about causality would be when $S'_1$ makes her choice at a single trial (and at spacelike separation to $S'_2$), but such choices are not well-defined using ensembles (which are in turn used to define relative frequencies) because they could occur with any given prior-frequency.

One cannot argue for cause without good cause—one must discuss the connections between individual events (like Stapp has attempted to do). The only other option is to define a notion of causality using probabilistic counterfactuals. Although ubiquitous, counterfactual statements are anything if not tricky (and often, here by example, ambiguous).

Of course, if no signalling theorems were false we could signal to each other using real frequencies (rather than relative frequencies). One could use, say, 10,000 of these experiments and work out the frequencies predicted by each such that $S'_1$ makes the same choice of parameter on all 10,000 sub-experiments; either she measures the $x$-spin on all the sub-experiments or on none. This is even more effort to go to in order to transmit a bit of information, but it could be done in principle; this is why we are glad no-signalling theorems are true. But still, $S'_1$ and $S'_2$ have collaborated significantly which might—we have not ruled it out—be the logical equivalent of a common cause.

So, we have the option to call the relative frequency of an event, given a fixed ensemble of results, an ‘objective’ relative frequency (or perhaps a propensity). When choices are being made, however, we do not even have a well-defined experiment. In order to make it well-defined we must fix the prior-frequency so as to fix the distinct super-ensemble we are discussing. In order to make relative frequencies ‘objective’ we must first know the prior-frequencies that are implicit in their definition. Why call something ‘objective’ at a single trial if you must first restrict it further than you can legitimately at a single trial? For an experiment at spacelike separation this notion of prior-frequency obviously introduces a fundamental ambiguity that we cannot remove. To summarise, in order to prove causality one would need to be able to define a notion of probability that is well-defined at a single trial and within a small spacetime region. We have shown that relative frequency (and thus also propensity) interpretations do not satisfy these requirements. Thus the only options we have left, bar particularly exotic examples, are subjective notions of relative frequencies (not well-defined at a single trial) or Bayesian probabilities (at least sometimes well-defined at a single trial).

This notion of prior-frequency is conceptually very similar to the Bayesian notion of prior-probability, and, ironically, frequentists often use this notion to try and attack the Bayesian viewpoint with claims of circularity. The same claim of circularity can be applied to the frequentist view. This analysis would suggest that frequentists and Bayesians are more similar than either might want to accept. Bayesian probability is subjective and is a form of logical inference that observers make. From this point-of-view the ‘real’ probability is a misnomer, but the probabilities predicted by each observer are apt. If we relativise proba-
bilities to being defined only with respect to a certain observer then we trivially
do not come across the problems discussed above. The problems only arise
when we try to define a probability objectively in each run of the experiment.
This cannot be done using relative frequencies due to the inherent ambiguity
expressed by prior-frequencies. It cannot be done for Bayesian probability since
such a global probability assignment goes against the very definition of proba-
bility in this framework. As we discussed above, the probabilities inferred solely
by each observer can be made passively Lorentz invariant, and in this case the
probabilistic compatibility of SR and SQT is made manifest.

On the Relations to Other Work

The above argument is similar in spirit to the non-relativistic situation discussed
by Anastopoulos\textsuperscript{16}. He notes that relative frequencies are \textit{necessarily} additive,
and if, by presumption, the actual results of experiments are to be given by
relative frequencies then we have a conflict between these two notions due to the
non-additivity of quantum frequencies. We have two choices; either we accept
relative frequencies and accept that the frequencies depend upon the physical
sampling used in an experiment, or we make drastic changes to our notion of
relative frequency. This argument also links in with the Consistent Histories
(CH) programme\textsuperscript{17,18,19,20} where only additive probabilities are discussed.
Thus, CH is not in conflict with the notion of relative frequency; although
rarely is the relative frequency of elements in ensembles actually discussed in
papers on CH.

A very interesting paper by Aerts\textsuperscript{21} has recently suggested a new form
of probability calculus that takes account of the different limits of relative fre-
quency (due to the change in observer, or context). If one wishes to use a
relative frequency interpretation then such a novel notion of frequency would
seem apt; however, it is not clear exactly how this ‘subset’ frequency is to be
interpreted in actual experiments. Khrennikov also discusses Bell inequalities
while taking such delicate issues into account\textsuperscript{22}.

In this paper we suggest that relative frequencies must be calculated ‘relative’
to the sampling used by observers, and that any other notion is nonsen-
sical. This, however, is slightly different to the way Rovelli uses the term in
his inspirational paper on relational quantum mechanics\textsuperscript{23}. Here, we have not
discussed the notion of observers which measure upon whole entangled systems
(i.e. the next ontological level up; someone who measures the measurements
of the subsystem observers). This is because it is not clear how to interpret
such an observer in regards to spacelike separated subsystems; such an observer
would necessarily be ‘nonlocal’. This may, however, be a misnomer in the sense
that nonlocal observers are easy to imagine and might be used pedagogically.

Conclusion

We conclude that the Heisenberg picture should always be used in discussing
spacelike separated events because any conflict between SR and SQT seems to
be manifest through the Schrödinger picture. Within the Heisenberg picture—
as long as we use relative frequencies and are willing to discuss pre- and
post-selection of ensembles—then the interpretation is consistent with changes
in time-ordering. We could also use Bayesian probabilities consistently also: we have only used relative frequencies here for pedagogical reasons. We would like to emphasise again the subtle distinction between the notions of causal and logical connections between events. These notions are, of course, interlinked—perhaps even complimentary—and it is very difficult to differentiate them cogently. In order to invoke causal relations one must discuss the choices between counterfactually distinct cases. But when invoking relative frequencies it turns out that each choice of a choice is invoked using a distinct super-ensemble. This blocks any inference of causal relations when just invoking relative frequencies.

As regards the three main notions of probability, we conclude that:

1. Relative frequencies are properties of ensembles and, if one wishes for consistency, one must always discuss the limits of such frequencies with respect to the ensemble or super-ensemble used. If one wishes to use relative frequency then one must note that this notion of “the very definition of relative frequencies depend upon the prior-frequency of measurements” is a tautology; it simply must be taken into account.

2. Bayesian probabilities are subjective (but, remember, not wholly subjective\(^5\)) representations of an observer’s knowledge. Prior-probability is taken into account naturally within this view, and we argue that this is the most cogent way to discuss probabilities because, although it is a lot more complicated than discussing relative frequencies, it is foundationally stronger. Also, there is no problem of using infinite trials in the Bayesian framework. A recent, but instantly classic, text on Bayesian inference is the Gospel of Jaynes\(^6\). Any issues with causality become nullified within this view as probabilities are updated by inductive inference (rather than updated objectively).

3. ‘Objective’ probabilities have been treated as a misnomer within this paper exactly because the relative frequencies of elements are taken to be logically different for different prior-frequencies of measurements. If there is to be a notion of ‘objective’ probabilities (i.e. propensities) then the ‘objective’ prior-frequency of measurements must be taken into account.

In order to invoke any tentative ‘inconsistency’ between SR and SQT one has to invoke further assumptions (like in Bell’s inequalities). SQT is a theory of probabilities so one must analyse the compatibility of SQT with SR only on a probabilistic level; and in this case SR and SQT do not peacefully coexist—they are completely compatible. It is rather observers (and their inferences) that peacefully coexist.

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\(^5\)Nor, in some interpretations, uniquely defined for given exhaustive prior-information.
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