# On the Zigzagging Causality EPR Model: Answer to Vigier and Coworkers and to Sutherland

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The concept of "propagation in time" of Vigier and co-workers (V et al.) implies the idea of a supertime ; it is thus alien to most Minkowskian pictures and certainly to mine. From this stems much of V et al.'s misunderstandings of my position. In steady motion of a classical fluid nobody thinks that "momentum conservation is violated," or that "momentum is shot upstream without cause" because of the suction from the sinks! Similarly with momentum-energy in spacetime and the acceptance of an advanced causality. As for the CT invariance of the Feynman propagator, the causality asymmetry it entails is factlike, not lawlike. The geometrical counterpart of the symmetry between prediction and retrodiction and between retarded and advanced waves, as expressed in the alternative expressions  $\langle B | UA \rangle = \langle BU | A \rangle = \langle B | U | A \rangle$  for a transition amplitude between a preparation  $|A\rangle$  and a measurement  $|B\rangle$ , is CPT-invariant, not PT-invariant. These three expressions respectively illustrate the collapse, the retrocollapse, and the symmetric collapse-and-retrocollapse concepts. As for Sutherland's argument, what it "falsifies" is not my retrocausation concept but the hidden-variables assumption he has unwittingly made.

#### **1. INTRODUCTION**

According to Vigier and co-workers (V *et al.*),<sup>(1)</sup> there is an ongoing battle between two parties who aim at an explicitly relativistic formulation of the EPR correlations. Their party, which they see as belonging to the "realistic" Einstein tradition, uses the quantum potential concept and direct

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Costa de Beuregard

spacelike connections which are instantaneous in the center-of-mass frame. The other party, which looks more like a swarm of freelancers or partisans,  $^{(3,10-15)}$  uses the zigzagging causality model and discards any sort of interrelation other than the correlated particles themselves. V *et al.* summarize their model in their Sec. 5 and mine in their Sec. 2. This latter summary is faithful—except for a few misunderstandings which are crucial. They conclude the section with a terse condemnation of  $my^{(2-8)}$  theory much in the style of a sentence from the Holy Inquisition; so there should be little surprise if some of my rebuttal is equally sharp.

V et al. make a strong case of a recent article by Sutherland,<sup>(9)</sup> a former adherent of the zigzagging causality approach<sup>(16)</sup> who has "recanted." Sutherland says he backs his new argument on Bell's theorem, but I will show that, just the contrary, he is unwittingly making a hidden-variables hypothesis. So, what is falsified by his argument is not my retrocausation concept but his own conterfactual assumption.

### 2. "PROPAGATION IN TIME": AN INAPROPRIATE AND MISLEADING CONCEPT

The main source of V *et al.*'s misunderstanding of my model<sup>(2-8)</sup> is their acceptance of what I deem an inherently fallacious concept, which I never use (although a few others<sup>(17,18)</sup> do, certainly without my approbation): "propagation in time." This would imply the idea of a supertime, with the same adverse effects against clear thinking as had the elusive "luminiferous aether." It so happens that the three authors I have just quoted<sup>(1,17,18)</sup> use, in connection with this concept, a second one I deem no less obnoxious: *positive-energy particle propagating forward in time* and *negative-energy particle propagating backward in time*; this notion I will criticize in Sec. 4. Anyhow, when V *et al.* insinuate that "I shoot positive energies backward in time," and therefore that I "violate energy conservation" because I have "positive energy appearing from the future with no apparent cause," my answer is a flat *no.* Let me explain this.

The Poincaré–Minkowski space-time concept yields a static fourdimensional picture, analogous to a classical permanent regime, such as, say, a steady hydrodynamic flow. As is well known, in this case the velocity field is determined jointly by the pressure from the sources and the suction from the sinks. Nobody pretends that "momentum conservation is violated" or that "(forward) momentum appears from downstream with no apparent cause." But, by observing an emptying bath tub, one sees very well how forward momentum is drawn into the sink! V et al. are so committed to "efficient cause" (in Aristotle's wording) that they are simply blind to "final cause"—a concept well conveyed by Lamark's aphorism that "the function creates the organ."<sup>2</sup>

#### **3. REMARKS ON THE JORDAN-PAULI PROPAGATOR**

I am very surprised that V et al. apparently ignore that the Jordan-Pauli propagator

$$D = D_{+} - D_{-} = D_{R} - D_{A} \tag{1}$$

is the one showing up in the commutation relations of free fields and in the solution of the Cauchy problem, two questions closely related to each other, which Schwinger<sup>(22)</sup> has discussed covariantly. This is known since long ago; see, e.g., Wentzel,<sup>(23)</sup> pp. 20 and 115. I have given a compact covariant treatment of the D propagator in relation to reciprocal Fourier transforms.<sup>(5)</sup>

So V et al.'s remarks on this are simply pointless.

Let us incidentally remark that the two different spaces spanned by the  $(D_+, D_-)$  and the  $(D_R, D_A)$  pairs have a nonempty intersection, as is shown by formula (1).

#### 4. REMARKS ON THE FEYNMAN PROPAGATOR

Again, I am very surprised that I must come back to this.

First I emphasize that *there is no such thing as a particle having both a definite 4-momentum and a definite propagation state.* Being not Fourier-representable, such a concept simply does not exist. Its semblance of existence stems from an undue "reification" of the fact that the Feynman propagator  $D_F$  is such that  $D_F = D_+$  if t > 0 and  $D_F = D_-$  if t < 0. What this means is that a particle carried on it can be thought of as an *inseparable combination* of a positive-energy particle with a negative time coordinate and of a negative-energy particle with a negative time coordinate.

As for the Fourier representation of  $D_F$ , it can be expressed as (please do notice the irony!)

$$D_{\rm F} = D_{\rm R} + D_{-} = D_{\rm A} + D_{+} \tag{2}$$

<sup>&</sup>lt;sup>2</sup> This is the root of the frequent misconception that "retrocausation is absurd, as it would permit one to kill one's own grandfather in his cradle<sup>(19,20)</sup>"! Of course  $not^{(21)}$ : As there is only one world history, it is nonsense to think of rewriting it. In other words, retrocausation does not allow *re*shaping the past, but it *does* mean shaping the past.

meaning that it is a definite phase-coherent superposition of  $D_R$  and  $D_-$  or of  $D_A$  and  $D_+$ —something analogous to the elliptical polarization photon state obtained via a definite superposition of a linear and a circular polarization state.<sup>3</sup>

Everybody (including myself<sup>(21)</sup>!) knows that the Jordan-Pauli propagator cannot be used in the *S* matrix, where spacelike connections are needed both in the *x* and the *k* representations. So, everybody (including myself<sup>(21)</sup>!) uses the Feynman propagator in the *S* matrix.

Defining time reversal T and particle-antiparticle exchange C via<sup>4</sup>

$$T: D_{\mathbf{R}} \rightleftharpoons -D_{\mathbf{A}}, \qquad C: D_{+} \rightleftharpoons -D_{-}$$

we see that D is T- and C-invariant, but that  $D_F$  is only CT-invariant. Of course, all propagators are P-invariant. Therefore the S matrix is at best P- and CT-invariant.

## 5. LAWLIKE REVERSIBILITY AND FACTLIKE IRREVERSIBILITY IN THE USE OF THE FEYNMAN PROPAGATOR

V et al. write that "the Feynman propagator does imply an irreversible entropy increase in future particle evolution, and no particle evolution toward the past is allowed." Well, for one thing (and again) "evolution toward the past" has simply no meaning. Second, concerning irreversibility, an essential indication is lacking: The one contained in the title of this section.

It is well known (Ref. 24, p. 408) that use of the Feynman propagator in the S matrix automatically yields an exponential decay of higher energy levels in a predictive calculation but, symmetrically, an exponential buildup of these in a retrodictive calculation. This exactly conforms to the "lawlike and factlike situation" in classical statistical mechanics, as elucidated by Loschmidt and Boltzmann (Ref. 25, pp. 446–448.

Why the anti-Feynman propagator is rejected is because it has the wrong association between the time and energy signs and is, in this sense, "anti-Boltzmannian." This I have said (despite V *et al.*'s insinuations to the contrary) and have explained by "juggling with propagators."  $^{(5,26)}$ 

Of course, there is the same factlike asymmetry and lawlike symmetry in "the dispersion relations which have been checked experimentally" and in "spontaneous decays of the vacuum." No miracle can extract an objective asymmetry from basically symmetric formulas.

<sup>3</sup> Incidentally, these remarks are addressed not only to V *et al.* but also to other authors.<sup>(17,18)</sup> <sup>4</sup> I correct here two erroneous signs in the formulas of Ref. 6, p. 880.

The point is here that "realistic thinking" very often introduces unwittingly irreversibility in time as a hidden hypothesis—for example, by sticking to prediction or by assuming retarded causality. This happened with Boltzmann, who's first reaction to Loschmidt was "Well, do it!"—that is, reverse *exactly* at time t all the velocities. Later, however, he realized that a factlike answer is not appropriate to a lawlike question, and produced the correct explanation (Ref. 25, pp. 446–448)—in fact, one equivalent to Laplace's<sup>(27)</sup> earlier one using conditional probabilities; van der Waals<sup>(28)</sup> drew the connection.

So, when V *et al.* write that "Bohm and Cini have deduced factlike irreversibility by means of a *realistic* interaction process with the measuring device [my italics]," the same indication as above is sorely lacking.

Do I need say that my interpretation of arrows at Feynman vertexes is the standard one: For a particle crossing the frame of the graph, an arrow pointing into a vertex means "absorption of a particle or emission of an antiparticle," and the converse for an arrow pointing out of a vertex. Of course, no such distinction has meaning for the inner lines.

#### 6. ON THE PASSIVE AND ACTIVE ASPECTS OF CPT REVERSAL

Here again V *et al.* misunderstand me so completely that I must be pedantically explicit. They go so far as suspecting that I do not know the difference between the PT- and the CPT-operations and that I ignore the existence of T-violating transitions!

So let me state right away that the prediction-retrodiction symmetry  $\langle A | C \rangle = \langle C | A \rangle^*$  between a preparation  $|A \rangle$  and a measurement  $|C \rangle$ , to be discussed in the next section, is equivalent to the *CPT*, not the *PT*, reversal!

A passive *CPT* reversal is merely verbal: It reverses the arrows of the four axes and exchanges the labels "particle" and "antiparticle." Thus, for time, it is a "count down."

An active *CPT* reversal keeps the axes and the labeling but reverses the network of collisions; that is, it exchanges emissions and absorptions, preparations and measurements; also, it replaces particles by antiparticles. Geometrical reversal  $\Pi\Theta$  of all four space-time (or momentum-energy) axes precisely has these two effects: "covariant motion reversal" *PT* and (according to the Stückelberg–Feynman recipe) "particle–antiparticle exchange" *C*. Therefore, on the whole,

$$\Pi \Theta = CPT = 1$$

Thus elementary laws are invariant under the Lüders<sup>(29)</sup> "strong reflection"  $\Pi \Theta$ . Therefore, in the "microrelativistic" scheme, where there is

invariance not only under rotations, but also under strong reflections of the tetrapod, C and PT are two "relative images" of essentially the same operation.

CPT reversal is the legal heir of Loschmidt's T reversal, the quantal and relativistic expression of "microreversibility." This is because it entails the "principle of detailed balance,"

$$A + \overline{B} + \cdots \rightleftharpoons C + \overline{D} + \cdots$$

where (beware!) a bar means particle on the left-hand side and antiparticle on the right-hand side, and conversely for nonbar. So V *et al.*'s talk of T violation in K meson decay is just inappropriate.

### 7. ON PREDICTION-RETRODICTION SYMMETRY, RETARDED-ADVANCED WAVE SYMMETRY, AND *CPT* INVARIANCE

V et al. misunderstand the true meaning of the connection between the two symmetries mentioned in the title, which has been drawn (independently) by Fock<sup>(30)</sup> and by Watanabe.<sup>(31)</sup> And so their criticisms happen to be addressed, beyond me, to Fock and to Watanabe, missing all three, however, because, in fact, they are aimed at a mirage. The Fock-Watanabe symmetry is equivalent to CPT, not to PT, symmetry. Let me explain this.

With the unitary evolution operator U explicitly displayed, the transition amplitude between a preparation  $|A\rangle$  and a measurement  $|C\rangle$  has the three equivalent expressions:

$$\langle C | UA \rangle = \langle CU | A \rangle = \langle C | U | A \rangle$$

The first one, projecting the retarded preparation upon the measurement, illustrates "collapse" together with predictive reasoning. The second one, projecting the advanced measurement upon the preparation, illustrates "retrocollapse," together with a retrodictive reasoning. The third one displays preparation-measurement symmetry and illustrates what I call "collapse-and-retrocollapse," a synthetic wording for the elementary chance game played in quantum mechanics—and a wording unpalatable to V *et al.* This concept of symmetry between retarded and advanced waves has nothing to do with the *T* symmetry exchanging  $D_R$  and  $D_A$  (and preserving  $D = D_R - D_A$ ). But it has to do with the *CT* invariance of the Feynman propagator and of the *S* matrix!

As this transition amplitude has CPT invariance (either passive or active), the two-faced symmetry we are speaking of clearly is equivalent to the CPT, not the PT, symmetry.

# 8. THE QUANTAL STOCHASTIC GAME: WHAT IS BASIC, WHAT IS "ADDED BY HAND"?

V et al. write: "First of all, the quantal evolution is in fact a unitary evolution from the original preparation" (their italics). This is very far from fact, it is an interpretation, and one heavily influenced by our familiarity with macroscopic physics. Macroscopically speaking, in such experiments as, say, the Davisson–Germer one, the quantal wave looks like an ordinary macroscopic wave, displaying the sort of "factlike irreversibility" V et al. are speaking of. But the elementary phenomenon is fundamentally different, having the lawlike reversibility previously discussed.

A little later, V *et al.* add that "measurement as a spectral decomposition of the wave packet and the particle entering one  $\cdots$  subpacket accounts  $\cdots$  for the  $\cdots$  phenomena. It proves the inconsistency of the collapse concept and excludes the even more inconsistent collapse-andretrocollapse mechanism which in any case does not result from the formalism but is  $\cdots$  added by hand."

Again, what this means is *factlike irreversibility*—but expressed in the quantal probability calculus<sup>(32,33)</sup> instead of the classical one.

Therefore, as a follower of Bohr rather than of Einstein (V et al. dixit), I have exactly the opposite philosophy. What I deem basic in the quantum theory is the stochastic game using the transition amplitude  $\langle C|A \rangle$ . What I deem as ancillary is the unitary evolution from  $|A\rangle$  to  $|C\rangle$ , which is nothing more than a change of representation, of  $|A\rangle$  into  $|UA\rangle$  for a prediction, of  $|C\rangle$  into  $|U^{-1}C\rangle$  for a retrodiction. This so-called "evolution," being *CPT*-reversible, is fundamentally no evolution at all. It is not through it that timing enters the picture; timing enters via the factlike irreversibility of the preparation-and-measurement stochastic game. Not God, but the physicist, is "playing dice."

Of course, the S matrix is expressible as an infinite sum of Feynman graphs, which are concatenations linking the partial preparations and measurements. Feynman propagators act as alter egos of the U operator. Should I recall that the Fourier nucleus  $\langle k | x \rangle = \exp(ik \cdot x)$  follows as a necessary consequence (Ref. 34, p. 161) of: (1) The Born-Jordan wavelike algebra and (2) translational invariance in space-time (or momentum-energy space)? Feynman graphs are somewhat like the wiring of a computer, connecting the questions and answers.

# 9. ON CAUSALITY AS IDENTIFIED WITH CONDITIONAL PROBABILITY

V et al., arguing against me, write: "There is a basic difference between a retrodictive calculation and a retroactive propagation (their italics)".

Costa de Beuregard

First I interject that "retroactive propagation" is nonsensical. When they add: "The basic difference between retrodiction and retroinfluence is that the former concerns knowledge and the latter a physical occurrence," I think they misunderstand my proposal<sup>(7,8)</sup> that *the causality concept be identified with the conditional probability concept*. Is there anything more operational than a definition of causality based on this procedure: "If you do this, then the predictive probability that there will be such an outcome is..."; or: "If you find this, then the retrodictive probability that there has been such occurrence is..."? Is this not the *very meaning* of causality? And is not the *binding* between the two lawlike symmetries and factlike asymmetries, the objective one and the subjective one, thus clearly displayed?

As for my argument<sup>(3,5)</sup> that, while the reversed EPR correlations display the usual, retarded, aspect of causality, the EPR correlations proper display a (previously unsuspected) advanced one, it is quite clear, and I need not repeat it here. So, I briefly summarize my argument by saying that what counts, in the S matrix scheme, is the setting of the preparing and measuring devices while the particles go through them, and that what these are before or after is irrelevant; so, one can then play with the setting without changing the result. Straightforward thinking leads then to the above statement. This argument would fail in the case of classical correlations, due to the difference between the classical

$$(A \mid C) = \sum (A \mid B)(B \mid C)$$

and the quantal

$$\langle A | C \rangle = \sum \langle A | B \rangle \langle B | C \rangle$$

correlation formulas. In the former, the intermediate summation  $|B\rangle(B|$  is over real hidden states of the common source or sink, while in the latter it is over virtual states. That makes the difference and is the root of "the EPR paradox."

My argument<sup>(3,5)</sup> rests on the stochasticity of the answers at A and C, and definitely not on how the decisions are made concerning the questions asked at A and C. How these decisions are made does not show up in the correlation formula. While this does not forbid discussion of the matter, it does require consistency between the discussion and the formalism—which is not the case in Sutherland's<sup>(9)</sup> article to be discussed next.

#### **10. THE FALLACY IN SUTHERLAND'S ARGUMENT**

Section 4 of Vigier *et al.*'s article is entitled "A Specific Objection: The Sutherland Paradox." But it is better that I refer directly to Sutherland's<sup>(9)</sup> recent article.

Not aware that  $I^{(35)}$  had previously made a similar remark, Sutherland emphasizes that, in an EPR correlation, the separation between two distant measurements A and C performed upon subsystems issuing from a common preparation B need not be spacelike but could just as well be timelike. This being said, Sutherland and I each have our own strategy for explaining what the "paradox" looks like in this case. To be specific, let us assume that the AC 4-vector  $(M_1M_2$  in Sutherland's paper) is pasttimelike.

I had proposed<sup>(35)</sup> that the third Aspect experiment<sup>(36)</sup> be redone with one of the photon beams, BA, folded by means of a mirror, and the linear polarizer at A set after the paired photons had left their common source B. This would display the same (*sui generis*) sort of backward causation from C to A as of spacelike causation in the ordinary EPR experiments. Alas, I could not persuade Aspect to do this experiment.

Anyhow, Sutherland<sup>(9)</sup> proposes another scenario: Depending on the result found at C, an ordinary, macroscopic signal is sent from C to A, thus setting at A, according to a previously defined code, the measuring apparatus. Sutherland (who uses in this example a spin-zero fermion pair) then writes: "Bell's theorem tells us that the outcome of the  $M_2$  measurement is not independent of which spin component is chosen at  $M_1$ "—a loose wording, acceptable provided that distortions do not creep in later. He continues: "This means that the following must be true for some of the pairs of particles...: the result at  $M_2$  would have been different if a different direction had been chosen at  $M_1$ ."

We could stop here because, at this point, Sutherland's error is obvious: "counterfactual reasoning" (in the now current EPR jargon). But let us proceed. "In other words, for a pair of different directions [of the polarizers at  $M_1$  and  $M_2$ ] there must be some pairs of particles...for which the following is true (albeit without our knowledge): choosing the direction  $\omega_1 \cdots$  at  $M_1$  would yield [answer +1] at  $M_2$ , whereas choosing  $\omega_2 \cdots$  would yield [answer -1]."

The conditional tenses in Sutherland's article ("would have," "would be") testify to "counterfactual thinking," while his words "there must be, albeit without our knowledge" explicitly express a hidden-variables hypothesis. This is the basic fallacy in Sutherland's paper.

Another one has been alluded to at the end of my preceding section: As the connection between the question asked and the answer obtained at A is not a rigid, but a stochastic one, his objection against me does not hold.

#### **11. ADDITIONAL REMARKS**

The sort of criticisms raised by Vigier and co-workers against my EPR model in some of their earlier publications<sup>(17,19)</sup> has found supporter in Cramer.<sup>(18)</sup> His criticisms rest on the same sort of misunderstandings as those discussed in the present paper. Therefore this paper happens to be also an answer to Cramer's<sup>(18)</sup> Sec. 5, p. 685.

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#### The Zigzagging Causality EPR Model

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