Professor Franco Sellerie 30 November 1983

My dear Franco,

I have just seen your Physics Letters - letter of 3 October 1983. I am very glad about it. Also, I have recently been reading your book again. I like it very much, but I disagree.

I am writing mainly to tell you that if you have an opportunity to come to England, I should love to have (not one but) several meetings with you.

I have done much work since I was in Bari: I have worked very hard, and when I should have had a little relief, I fell ill and had pneumonia for four weeks. I am slowly recovering from this. But throughout my pneumonia I have been working hard.

Just before my pneumonia I was in Trieste and gave, on 22 September, a ~~lec~~ lecture under the chairmanship of Abdus Salam. I am sending you the 18 Points which I discussed there. Abdus Salam was largely or wholly in agreement. He was very nice indeed.

Now a few words on your paper.

(1) There is no reason to regard QM (by this I mean the formalism in propensity interpretation) as non-local. The Copenhagen interpretation is non-local, but not the formalism = Q.M.

There were 2 arguments (invalid) for non-locality: (a) the so-called "collapse (or the "reduction") of the wave packet.

(b) the Bell-inequality in so far as it asserts that

$$\underline{\underline{\text{All}}} \text{ local theories} \neq \text{Q.M.}$$

But your beautiful paper has shown that I was right that the assertion~~s~~ that <u>All</u> local theories differ from Q.M. is mistaken. There <u>cannot</u> be a proof of the <u>All</u> assertion. So there cannot be a proof of the non-local character of Q.M.

Coming back to (a), let there be a wave packet split, and the particle (~~ph~~ photon or electron or neutron...) be found on one side. Why should the empty wave "collapse"? Why should it not continue to exist? (And perhaps retain the power ~~m~~ of interfering?)

In my opinion it is a misunderstanding of probability theory (= propensity theory) to say that finding the particle in one wave-train (i.e. one-half of the wave-packet) has any influence upon the other half of the packet.

Physicists usually misunderstand the situation in probability theory.

Denote the ("relative", or "conditional") probability that a will happen under the condition b) by p(a, b), read: "the probability of a given b"; say, in an experiment with a semi-permeable mirror,

$$p(a,b) = \tfrac{1}{2}$$

for a to go to the left. Then let a be found on the right. It is simply false that the information that a is on the right changes

$$p(a,b) = \tfrac{1}{2} \;;$$

for $p(a,b) = \frac{1}{2}$ is the probability to find a on the left under the condition b ; and this remains $\frac{1}{2}$. The information that the particle has been found on the right is, obviously, different from the condititiou b. Call it c. then $p(a,b) = \frac{1}{2}$, $p(a,c) = 0$ or 1. Thes two equations

$$p(a,b) = \tfrac{1}{2} \quad \Big|\Big|\Big| \quad p(a,c) = 1$$

do in no way clash. Thus the old wave packet does not "collapse": the old probability (the propensity wave) remains as real as ever it was (provided it was ever real!).

Thus there is <u>no</u> reason to regard QM as non-local. It is a local theory.

It is, of course, incomplete. This goes without saying. I suspect that we ~~s~~ shall never have a complete physical theory (a theory that explains why distant electrons <u>all</u> have ~~all~~ the same charge, etc etc).

A prima facie "deterministic" looking theory (The 8 body problem is, I suspect, in principle is not deterministic.) like Newton's dynamic QM could become causally far more complete without ever becoming fully deterministic.

These are just one or two of the points I should love to discuss with you, more than with any other physicist ~~body~~, [illegible] living

All the best Karl

P.S. Which of my books have you by now?
I should wish you to have:

Logik der Forschung <u>or</u> The Logic of Scientific Discovery
The Open Universe
Quantum Theory & the Schism in Physics
Conjectures & Refutations
Unended Quest.

P.S. Could you please send me the address of H. Rauch?

Telephone: England [0]49481 – 2126

Penn, 18-4-84

My dear Franco,

I hope that Dr Slade has sent you

(1) a letter saying that I shall be with my wife in Rome at a Conference of the Accademia dei Lincei ('Cosa et Pensiero'[; I do not know how "cosa" is ~~ment~~ meant here: attention & thought ??), from May 8 to May 12. If you can (and wish to) come, I could be in Rome on May 6 or 7 and spend two or one day with you.

(2) A <u>60</u> page-long typescript "Realism and Quantum Mechanics" for the Report of your 1983 Conference in Bari. [It is possible that ~~to~~ in the place of this typescript a shorter typescript, pp 3[illegible] "Realism and a new version of EPR" has been sent; this is part of the 60 page MS.]

(3) Another typescript which is based on my last (public) lecture in Bari: "Evolutionary Epistemology". I intend to use this for the Conference "Cosa et Pensiero".

However, if you have (2) this should be the main basis of our discussion in Rome, if you can come to Rome. If you cannot come, I should of course love to have your opinion on (2).

With all the very best wishes,

Yours ever

Karl (Popper).

IIVERSITÀ DEGLI STUDI DI BARI
FACOLTÀ DI SCIENZE
Dipartimento di Fisica

Bari, Oct. 5, 1984

Ns. rif.:
(da citare nella risposta)

Dear Karl,

I was hoping to see you in Athens, but we could only hear a paper of yours read from the young and brilliant Chris Dewdney. During my talk I discussed your proposed experiment from the point of view of Heisenberg relations applied to the source, since there are many physicists saying that your experiment is in principle impossible because the collinearity requirement cannot in principle be satisfied. I consider such opinions wrong and I think I proved them so. This argument was presented in Athens and will be contained in a paper I am preparing, which shows that no difficulty exists in principle against your experiment. For the moment I send you a photocopy of the presented transparencies.

The editors of the spanish edition have given a positive answer for the publication of Die Debatte um die Quantentheorie. Their name is Alianza Editorial. Also my book is being translated in Slovak and Greek. The path of the english edition by Reidel is instead very slow, due to some troubles of van der Merwe. Now, as I told you some time ago, I would be extremely grateful if you could write a preface, which I could use immediately for the spanish translation of the german book. I know that I am giving you additional troubles which add to your many activities and I am sorry for that. Perhaps you could simply allow me to use the words you spent for Die Debatte at the Bari workshop.

Various versions of our "variable photon detection probabilities" are being investigated by Trevor Marshall, Emilio Santos, Augusto Garuccio and me. We are very excited, because there is perhaps a chance that something really important lies in such an idea. For instance, I am very impressed with the fact that in order to save realism and separability we are forced to become more seriously dualistic (I hope I use words well: what I mean is that we are forced to give a consistent picture of the photon with particle-like as well as with wave-like properties). The whole thing is very nice. Unfortunately the french people are mad at us, perhaps they would like that we do not discuss the limit of Aspect's experiment. d'Espagnat is particularly

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FACOLTÀ DI SCIENZE
Dipartimento di Fisica

Bari, ...

Ns. rif.: ....
(da citare nella risposta)

active against us and the thing does not make any sense since a scientific idea should be discussed only on the scientific ground. O tempora, o mores!

We are preparing a conference in the medioeval town of Urbino for sept. 1985 in order to celebrate the 50th anniversary of the E. P. R. paper. We need your help and suggestions in order to do a really good thing. Trevor Marshall, who is in the organizing committee, will talk to you about the whole matter.

After the discussion we had on telephone about my paper "Generalized EPR paradox": I reread it carefully and concluded that it is not clear in what it does, so much so to be almost unreadable. However, the result seems to me correct and the criticisms you made are probably due to some misunderstanding. You said that I mix quantum mechanics with other considerations, and in a way you are right, but I do it only in situations (angle of the two polarizers equal to 0° or 90°) where quantum mechanics gives simple and non paradoxical predictions which have a good chance to be correct.

That proof is my universality claim and I believe it to be the only correct probabilistic proof of an inequality violated by quantum mechanics. Tarozzi and I proved that the proof by Clauser and Horne is valid only within a subjectivistic approach to probabilities (of the type you dislike). If probability is defined as frequency in a statistical ensemble then it is possible to give counterexamples to the Clauser and Horne Ansatz of factorability. We do not know how to define separability within a probabilistic theory! That is, I did it in the paper you criticized, but in a too weak and general way: if a stronger definition were found many more results would follow. Dear Karl, I leave you now by sending you my very best wishes and also many thanks for the help you gave to Trevor for his career.

Friendly yours, Franco

Franco

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Via Postiglione, 44
70125 BARI Italy (Tel. (0)80 - 360754)

evasa 18/11/84

Penn 28-10-84

My dear Franco,

I just have sent a <u>Preface</u> for your book to my typist. I estimate it to be $2\frac{1}{2}$ pages long.

I do not think that it is good. If you don't like it, we just throw it away. If you like it but wish to <u>change</u> it, just make your proposals or suggestions.

Love

Karl

I am far from well — and so is Hennie.

Angelidis has, I think, successfully repaired his <u>new</u> paper (his model).

\errata 31/1/85/

Penn 2-1-85

Dear Franco,

Many thanks for your letter, and many thanks for the paper in which you raise objections to my EPR experiment. Of course there will be some scatter. But I think there are some bubble chamber experiments which look rather different from your figures. Of course, you may be right. But after all, the Aspect experiment also looks very different from your figures.

Angelidis will soon send you two papers: one by Angelidis and one by Angelidis & me.

I met Millikan in either 1939 or 1940. And when I told him that I don't believe in the ~~existing~~ official interpretation of Q.M. and especially not in the indeterminacy relations, he told me the following story.

When Lawrence in late ~~29~~ 1929 or 1930 had the idea of the cyclotron which is based on the idea that in a magnetic field, the radius r of the orbit times mass times angular velocity = r × charge × field intensity, so that r cancels, this was criticized

$$r \times m \cdot \omega = r \times q \times H \quad ?$$

Tuesday 26-2-85, 2 p.m.

Dear Franco,

Thank you very much for your visit: I was very happy about it. And thank you for your discussion from which I learned a great deal.

I think I was not very good in the discussion; but I know you will forgive me. I think I can now do better.

Your argument is that we can <u>test</u>

$$* \quad p_{12}(a,b) = \int d\lambda\, \rho(\lambda)\, p_1(a,\lambda)\, p_2(b,\lambda)$$

with the help of, or in connection with, the CH tautology.

My reply: we can, for this purpose, forget the CH tautology. In order to test * against QF (quantum formalism) we must have a testable <u>conjecture</u> which depends on *. But we can derive from *, with Angelides, ~~2~~ 3 quite different results: from

(1) ~~from~~ $p_1(\lambda,a)=\frac{1}{2}[1+\cos 2(\lambda-a)$ and $p_2(\lambda,b)=\frac{1}{2}[1+\cos 2(\lambda-b)]$ we can, together with *, obtain <u>at least</u> 3 different interesting values for $p_{12}(a,b)$: (i) $\frac{1}{4}[1+\frac{1}{2}\cos(2\phi)]$ (ii) $\frac{1}{4}[1+\cos(2\phi)]$ (iii) $\frac{1}{4}[1+\cos^2$

where (i) < (ii) < (iii). Of these, (ii) is the value of the QF.

<u>This proves that * is quite insufficient to be tested; least of all against (ii).</u> This definitely refutes your claim that from * alone, Bell's inequality can be derived: it cannot even be derived if, in addition to (1), we assume *.

The tautology is just a red herring. It does not add to the situation, no more than assuming

$$0 \leq p(a,b) \leq 1$$

I beg you to derive yourself Angelides's (8): then you will see how (ii) and (iii), which is <u>bigger</u> than (ii), can be derived, by perfectly reasonable <u>additional</u> assumptions: * just is <u>not</u> <u>enough</u>. The $\rho(\lambda)$ is needed, <u>and</u> the choice of a substitution for μ.

Kindest regards and all the best!

K. P.

Professor Franco Selleri 12 - 2 - 87 1
Univ. of Bari
Dipartimento di Fisica.

evasa 23/2/87

My dear Franco,
Your letter of December 2nd with your beautiful paper 'Coherence properties of Photon Amplifiers' reached me only today. I feel confident that it will decide in favour of empty waves.

Unfortunately I do not understand your Fig. 3 on S. 20 because I do not know Ref. 1.

However, the preceding results are convincing. I believe even more: I believe that wave-generated stimulation can give rise where particle-generated could not (because there may be no particles for seconds ~~...~~ on end, or for minutes on end). In other words, I believe that completely empty waves may be amplified: the wave-amlitude <u>is</u> a continuous magnitude, and so is its possible stimulating effect upon amplification devices. Or to put it still differently: ~~...~~ while amplification is of course an energy-conserving effect, stimulation — the act of stimulation — does not need to supply energy. The quantum theory tells quite clearly us that so little energy as would be ~~is~~ needed for stimulation <u>does not exist</u>. So let us take this seriously.

(The term 'photo-multiplier' is totally misleading: you cannot multiply zero and obtain a result.)

Why do I say so? Why do I conjecture this? Very simple: The pictures transmitted from the space ships — from Jupiter, from Saturn. I came to this conclusion — that the waves during this transmission must have been empty of photons during longer intervals of time than were needed for fixing a point of the picture — some considerable time ago. And when I met one of the physicists who ~~[illegible]~~ had been engaged in the transmission + amplification, I asked him. And his reply was "According to our calculations we should have got <u>no</u> pictures. We were most surprised and simply could not explain it." My reply was: The explanation is a simple conjecture: there is no lower limit to the wave intensity of empty waves."

(This encounter happened in Pavia when I gave a lecture there at the end of May or the beginning of June 1986. The Physicist was an Italian working at M.I.T.; I have forgotten his name, but I can find it out, if you are interested.)

In these cases it must be an amplification that reaches a state where the waves might be empty for, say, an hour. But a second would be enough to clinch the issue of the stimulation through empty waves.

Your experiment _will_ of course clinch the issue. _I should not wonder if you would get far better results than you expect._

I apologize for this long letter and I wish you luck. My best congratulations to your paper.

All good wishes to both of you.

Yours ever

Karl (Popper)

P.S. I recently had a letter from Ballentine on my "Propensity Interpretation", with which he agrees. If you do not mind I'll send ~~you~~ him a photo copy of this letter.

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24 November 1986

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Dear Franco,

I have just received your letter and abstract and read both. All seems to me very promising. Of course, everything will depend on the experiments of which you speak at the end of your letter. I shall write again when I have read the paper. As you may remember, I strongly believe in empty de Broglie waves.

I wonder whether my very short contribution to your last conference is going to be published. I know I should have expanded it but better short than not at all.

With all good wishes,

Yours sincerely,

pp. Karl Popper